DEVELOPMENTS IN GEOTECTONICS

10

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THE EXPANDING EARTH

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THE EXPANDING EARTH

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THE EXPANDING EARTH

PART I INTRODUCTORY REVIEW

PART II SOME PRINCIPLES

PART III REGIONAL

FACE OF THE EARTH

Suess

Das Antlitz der Erde, or "The Face of the Earth", was the title of the first great *magnum opus* on the gross surface features of the earth by Eduard Suess late last century. A recent British television documentary with the same purpose bore the same title. Suess' masterpiece was the product of one great mind. The TV show was produced by a very large team. Suess wrote for a scientific oligarchy. The TV documentary was directed at the man in the street. In Suess' day the only bird's-eye view was from a mountain-top. In this decade all have seen the face of the earth as viewed from the moon. From nineteenth-century elitism to twentieth-century mass communication, what has really happened to the science of the earth?

Early science was egocentric, and uniformitarian, in that it assumed that things have always been much as we now see them, at least since the earliest beginning too hazed in ignorance to discuss. A century ago geologists believed that the mass, volume, and diameter of the earth were fixed inheritances, that the axial obliquity to the ecliptic was immutable, that the earth was a dying body dissipating primal heat from a still molten core, that magnetic north was north and south was south, and always had been so, that physical constants had been and would remain constants, and that continents were fixed permanent features which heaved and sagged from time to time against an ebbing and flooding sea. The geologist's task was to describe and understand the details of a planet on which the really big things had happened eons ago as a prologue before his saga opened.

During the nineties Suess knew this had to be changed, for he recognized that 200 to 300 million years ago Africa, South America, India, and Australia had been a super-continent sharing the *Glossopteris* flora and a common ice age. But Suess assumed, not that Africa and the rest had moved, but that the parts between had foundered to form new oceans.

Wegener

Wegener took the first big unorthodox step by concluding that at the time in question all the main land masses of the world had been a single supercontinent, Pangaea, which had disrupted into separate blocks that drifted apart like great tabular icebergs, opening the Arctic, Atlantic, and Indian Oceans and reducing the area of the Pacific by a like amount (Fig. 1).

All this in the latest twentieth of the earth's life! Others indeed had preceded him (e.g. Snider, 1858, Taylor, 1910, Baker, 1911) but pioneers in science rarely make a home run. Wegener's distinction was his action across many specialist fields with sufficient errors to invite devastating counter attack -- successful but Pyrrhic. Wegener's thesis started violent controversy, but when the dust settled, most were cowed by dissenting physicists, whose edicts were valid enough for the simple models which for them represented Earth. So, few followed Wegener.

The orocline concept

As a student in the early thirties, it seemed to me that if continental shields had moved the long distances visualised by Wegener, then the orogenic belts between them, which were mobile and deforming at the relevant times, must record indelibly the relative motions. A rifting region where stable shields were separating could not be wholly obliterated by later events, and the pliable zones must still record the stretching and bending induced in them by the motion of the shields they separated. These deformations should be obvious in the plan view of the surface, where thousands of kilometres of displacement in bending and tearing and S drags, should be apparent. And so it was. Even the most primitive physical maps of the earth's surface confirmed these inductions. These disturbed belts certainly looked as though they had been bent and stretched and disrupted (Figs. 2 & 91). I knew by 1938 that if you straightened the obvious bends, restored the visible stretches, and reunited the dislocations, these processes alone reproduced a Pangaea essentially the same as Wegener had deduced from wholly different grounds. Most of what I published in the 1954 orocline paper was in the 1937 draft of my doctoral thesis. but had to be omitted at the eleventh hour because I realised that these concepts were

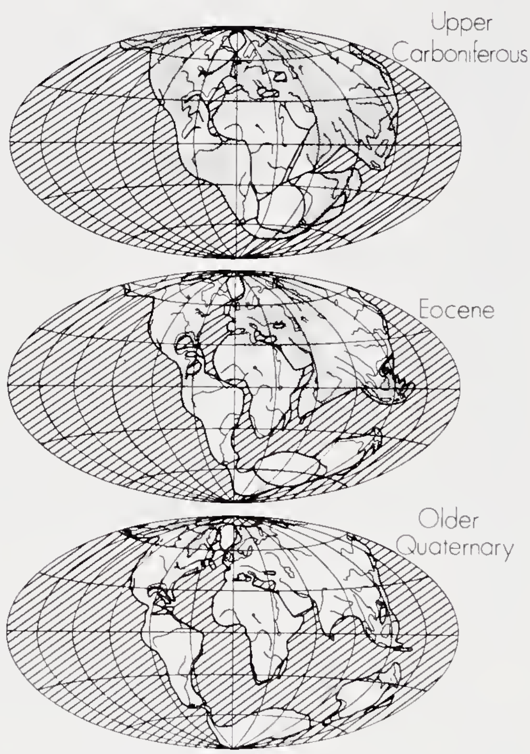

Fig. 1

Dispersion of Pangaea as visualized by Wegener.

too radical for acceptance then, and would have cost me my degree. Even in 1954, publication was refused by referees of the Geological Society of Australia.

In the late forties and early fifties came palaeomagnetism, led by Graham, Blackett, Runcorn, Gough, and others. Many rocks record the direction of the magnetic field at the time of their birth and hence the local magnetic latitudes and the magnetic meridian of that time. Such observations leave no doubt that the continents have separated and have relatively rotated to the extent claimed by Wegener. A third of the oroclinal bends I had described have been subjected to the palaeomagnetic test (Table 1). Every orocline tested has been confirmed.

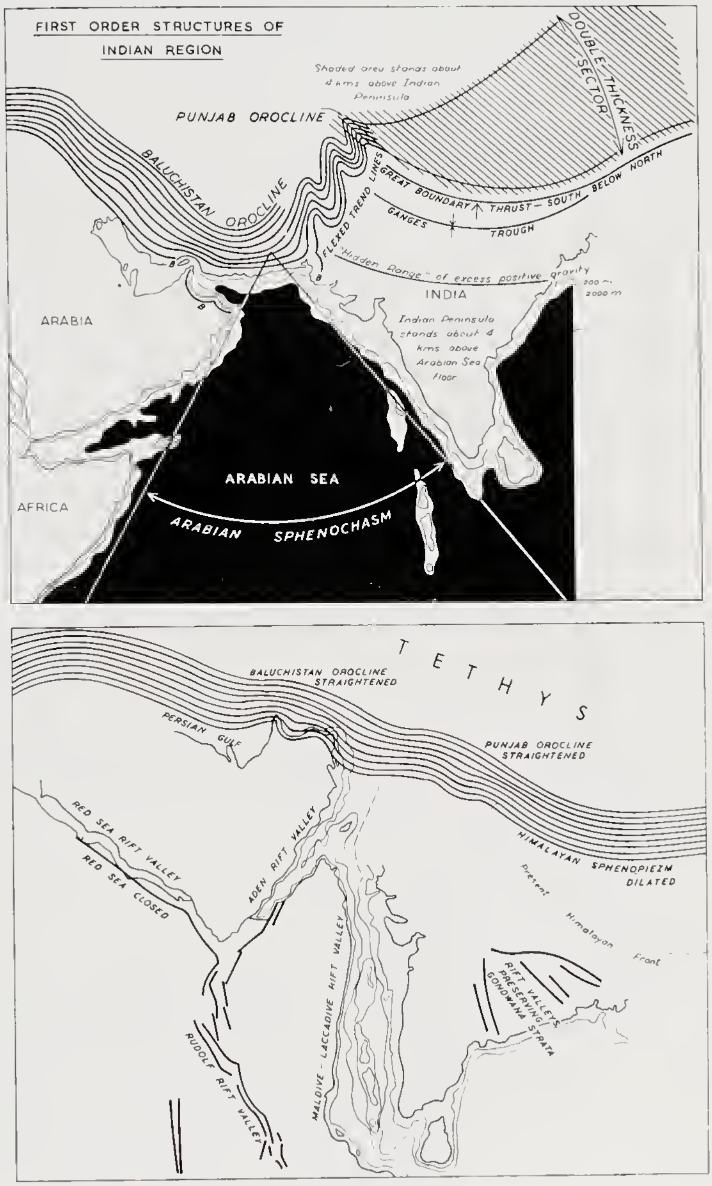

Fig. 2

The orocline concept (from Carey, 1955a).

TABLE I Tectonic Rotations Subsequently Confirmed Palaeomagnetically

Rotated Block	Rotation predicted by Carey	Palaeo-magnetic rotation observed	References
N. America to Europe	30°	30°	8, figs 5, 9, 10, 13, 14; 9, table 4; 35
Africa to S. America	45°	45°	8, figs 14, 17, 21, 24; 24, fig 5
Newfoundland	25°	25° s	10; 11; 12; 17, figs 6, 7; 18; 35
Spain	35° s	35° s	1; 5, figs 2, 3; 6; 8, fig 33; 13; 19, fig.10.10; 25a; 28; 30; 33; 35
Italy	110° s	107° s	5, fig 21; 8, figs 31, 32; 13, p.84; 17, figs 4, 5; 23; 35; 44; 48
Corsica and Sardinia	90° s	50° s	5, fig 21; 8, figs 31, 32, 51; 17, figs 4, 5; 22, fig 2; 29; 32; 35; 47
Sicily to Africa	0°	0°	5, figs 20, 21; 8, fig 32; 17, fig 32; 40
Arabia to Africa	3½° s	7° s	5, fig 12; 8, figs 1, 2, 36; 14; 17, fig 7; 25b; 35
India to Africa	70° s	70° s	5, figs 10, 12; 6; 8, figs 1, 2, 36, 51; 19; 31; 35
New Guinea	35°	40° s	3, figs 23, 25; 8, fig 38b; 27; 35
Honshu N. and S.	40°	58°	8, figs 40b, 40c; 15, fig 7; 19, p.249, fig 10.5; 32; 35, figs 1, 4;
Mendocino orocline	60° d	63° d	2; 4; 7; 8, fig 56; 11; 19, p.249; 21, figs 1, 3; 26; 32; 35; 37, p.93
Puerto Rico to S. America	45° s	53° s	17, figs 12-14; 36; 39
Jamaica to S. America	42° s	50° s	17, figs 12-14; 38; 39; 46; 49
Hispaniola to S. America	39° s	40° s	17, figs 12-14; 41; 46
Colombia	large s	80° s	8, fig 26; 39
Appalachian Arcs	20°-40°	29°	16, p.142; 20, p.165; 32
Malay Peninsula	Ca 70° d	70° d	3; 8; 34; 43, p.645; 45, figs 43, 53;

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1 *Argand, 1924*
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3 *Carey, 1938*
4 *Torreson, Murphy & Graham, 1949*
5 *Carey, 1955a*
6 *Clegg, Deutsch, Everitt & Stubbs, 1957*
7 *Cox, 1957*
8 *Carey, 1958a*
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12 *Nairn, Frost & Light, 1959*
13 *Van Hilten, 1960*
14 *Irving & Tarling, 1961*
15 *Kawai, Ito & Kume, 1961*
16 *Carey, 1962a*
17 *Carey, 1963*
18 *Black, 1964*
19 *Irving, 1964*
20 *Irving & Opdyke, 1964*
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23 *De Boer, 1965*
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27 *Green & Pitt, 1967*
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29 *Nairn & Westphal, 1968*
30 *Watkins & Richardson, 1968*
31 *Wensink, 1968*
32 *Tarling, 1969*
33 *Van der Voo, 1969*
34 *Kawai, Hirooka & Nakajima, 1969*
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43 *McElhinny, Haile & Crawford, 1974*
44 *Lowrie & Alvarez, 1974*
45 *Carey, 1975*
46 *Dasgupta & Vincenz, 1975*
47 *Orsini, Velluntini & Westphal, 1975*
48 *Lowrie & Alvarez, 1975*
49 *Vincenz, Steinhauser & Dasgupta, 1973*

s = sinistral rotation   d = dextral rotation

Decades in contempt

During the thirties and forties and early fifties Wegener's ideas were generally rejected as a fantasy - fascinating but false. "*Ein Marchen,* a pipe dream, a beautiful fairy story" chanted the American bandwaggon. During these decades of repudiation, arguments which denied continental dispersion passed without scrutiny or test. They were correct, *a priori,* because everybody knew that continental drift was wrong. Otherwise-great scientists like Harold Jeffreys got away with fatuous statements such as (1929, p.322): "the alleged fit of South America into the angle of Africa is seen on a moment's examination of a globe to be really a misfit of about 15°. The coasts along the arms of the angle could not be brought within several hundreds of kilometres of each other without distortion. The widths of the shallow margins of the ocean near the continents lend no support to the idea that the forms have been altered considerably by denudation and redeposition; and if the forms had been altered by folding there would be great mountain ranges at a distance from the angles with their axes pointing towards the angles, which is not the case."

I knew by 1931 that this was incorrect, because I had oblique stereographic projections to check it. I did not bother to publish this because anyone who checked Jeffreys' statement, would find it wrong, and it seemed

Fig. 3

Fit of Africa and South America at the 2000 m isobath. (From Carey 1955b).

to me that the *faux pas* would be generally recognized. On the contrary, this became one of the standard dogmas to discredit continental dispersion. Finally, when Dr. G.M. Lees, in his 1953 presidential address to the Geological Society of London, cited this reputed misfit as one of his three grounds for dismissing continental drift (Lees 1953, p.234), I sent him the

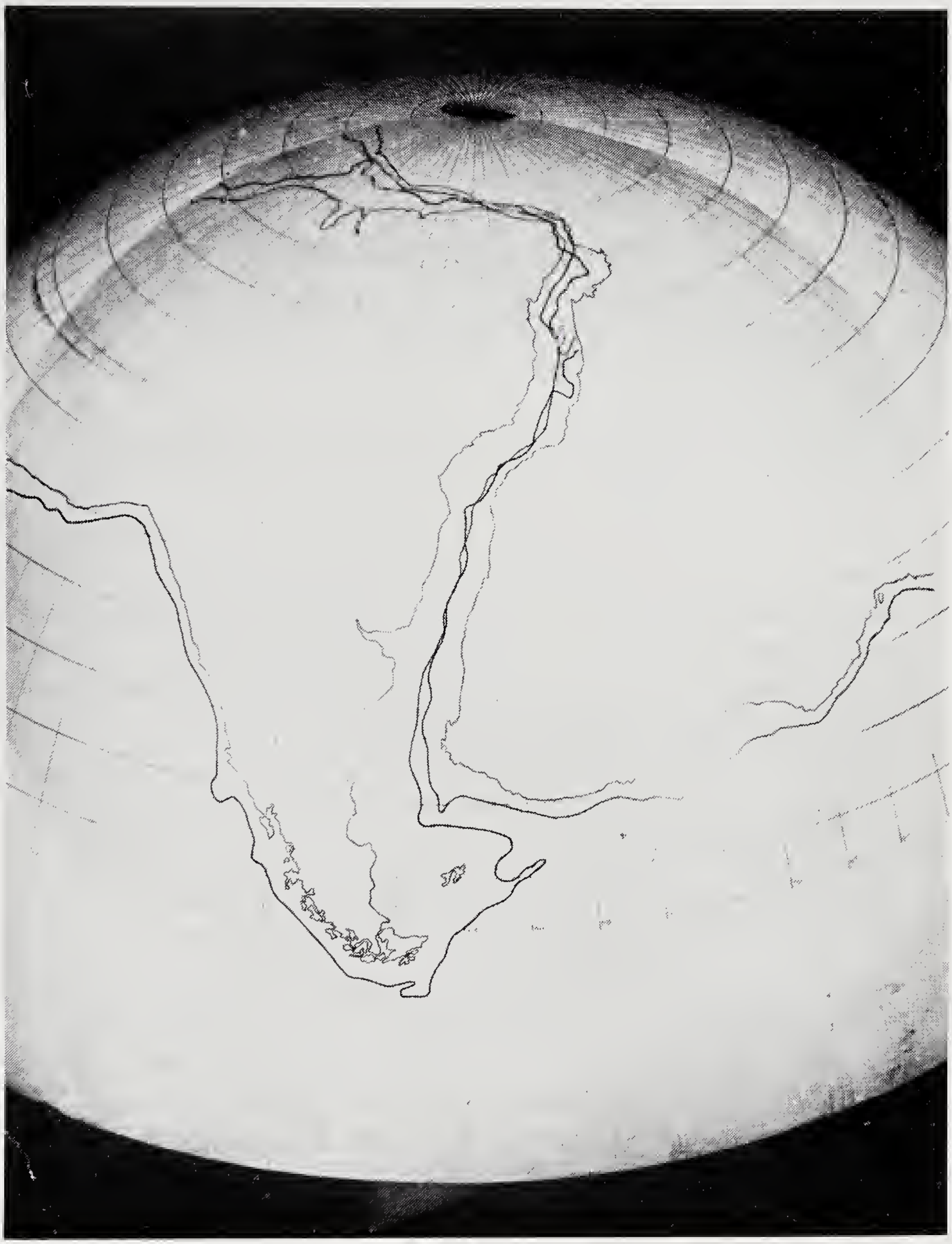


Fig 4

Segment of spherically moulded tracing foil on hemispherical drawing table, showing fit of Africa and South America at 2000 m isobath, as traced directly from a thirty-inch globe. (From Carey 1955b).

stereographic check (Fig. 3) and the check with spherically moulded plastic to fit my large globe (Fig. 4), and added that these figures did not express opinions or theories, but objective facts, which could be verified by anyone who cared to take the pains to do so by any valid means. Lees arranged publication (Carey 1955b). Subsequently Bullard checked the fit by computer (Bullard *et al.* 1965), so it became known as the "Bullard Fit". Recently Van Houten (1975) has recalled that Choubert (1935) had also made accurate comparison and reassembly of the coasts across the Atlantic. Choubert, no doubt, was as unaware of my work as I was of his.

Another among many similar solecisms, is Simpson's (1943, p.19) conclusion, quoted even by Holmes (1945, p.498) as "the chief adverse argument", that the Triassic reptiles of South Africa and South America have only a remote degree of kinship, consistent with their present degree of separation across a wide ocean:

	A	B	C
Families . . .	100	89	43
Genera . . .	82	64	8
Species . . .	65	26	0

A Percentage of recent Ohio mammals also occurring in Nebraska, 500 miles away

B Percentage of recent French mammals also occurring in northern China 5,000 miles away

C Percentage of known South American Triassic reptiles also found in the Triassic of South Africa, now 4,750 miles apart.

The naivete of such an argument is stark. The faunal lists for the recent Ohio, Nebraska, French, and Chinese mammals, are (for all practical purposes) complete. The probability of finding a new species is remote, still less a new genus, or a new family. Among the Triassic reptiles, by contrast, a whole genus may be known only by a single bone. The only Triassic reptile known from the whole Australian octant of the globe from Malaysia to New Zealand is a single specimen housed in the vault under my chair as I write this. It would be absurd to suggest that the Triassic faunal lists are even one percent complete. Now if we took a random one percent from the Ohio and Nebraska mammals, comparative percentages as in columns A and B would be immediately rubbed out. Also, column C deals with a whole geological period, whereas columns A and B cover only a moment of an epoch. If the one percent sample were taken, not from the faunal lists for recent time, but randomly for the whole of the Tertiary period, or at least for the Neogene, the comparative kinship would be further diluted.

But here's the rub. Although any loose statement denigrating or mocking continental dispersion got easy passage and approval for publication, any who was unwise enough to argue for displacement of continents was cold-shouldered by referees and editors, and became the butt for snide comments.

The tectonic confirmation of Wegener's continental dispersion acerbated its complementary difficulty:- If vast new oceans developed by widening rifts between continents, equal area had to vanish elsewhere. By the early fifties I prepared a paper which developed the transport of continental blocks on the back of convection cells, which advanced against the ocean, because the subcrust over the downgoing limb of the convection cell tended to be drawn into the down-flow, as a highly viscous fluid must. This continually sapped the foundations of the more brittle crust above, which therefore progressively subsided into the sink. In this way oceanic crust was steadily excised and swallowed, so that the sialic crustal block advanced against the ocean floor, notwithstanding equal or perhaps greater strength in the oceanic crust.

I sent this paper to the American Geophysical Union in 1953, but it was rejected by the referees on the grounds that it was naive. Twenty years later when "plate tectonics" had *arrived*, I dusted off the old manuscript and figures and sent them back to the American Geophysical Union with the letter reproduced on the next page.

Figure 5 is a reproduction of Figure 2 of the 1953 manuscript. It relates to the disjunctive seas of east Asia.

Meanwhile in 1959 I presented these concepts and illustrations in lectures in Yale, Columbia, Princeton, and a score of universities through North America - so perhaps some seeds fall on fertile ground. My 1953 naivete became the 1962 approbation of my friend Harry Hess as "a more acceptable mechanism whereby continents ride passively on convecting mantle instead of having to plow through ocean crust".

The primary rift system

According to my views right from the thirties the ocean floors, except for part of the Pacific, had to be newly developed crust, risen from the mantle step by step as the continents moved apart. The work of the Lamont group during the early fifties, particularly Heezen, crystallised with

12th October, 1971

The Secretary,
American Geophysical Union,
2100 Pennsylvania Avenue, N.W.
WASHINGTON, D.C. 20037 U.S.A.

Dear Sir,

Twenty years ago at a time when the gross separation of the continents was a heresy to be ridiculed, I prepared a note to answer one of Jeffreys' criticisms of continental drift and submitted it to your predecessor for publication. It was rejected with a note from a referee stating that it was naive and unsuitable for publication. . .

This note contained perhaps the earliest viable exposition of subduction, which is now the accepted dogma, particularly in America. You will also find in my continental drift symposium (published in 1957, although taught by me for many years previously), the first viable exposition of ocean floor growth at the mid-ocean ridges. This is reproduced on page 179 and figure 6 of Search, vol. 1, enclosed.

A visiting American geologist recently suggested that I should now send back to you the 1953 note with the suggestion that you might choose to publish it now as a historical document. I therefore enclose a copy of the original manuscript.

Although I worked with subduction models for more years than any of the new generation of subducers has yet done, I have since moved on to what I think are more probable models. American thinking has now arrived pretty much at where I was twenty years ago

Yours sincerely,

S. Warren Carey
Professor of Geology

(The figure 6 of *Search*, vol. 1, p.179, is reproduced here as Fig. 5).

The following reply was received six months later:

May 3, 1972

Dear Professor Carey:

RE: THE STRENGTH OF THE OCEAN FLOORS AND CONTINENTAL DRIFT
JGR MS #270202 (Carey)

The paper you submitted some time ago to Dr. Spilhaus came to my desk, and I sent it out to a number of members of the Editorial Board, for comments. I asked if we should change our policy and publish a version of a paper we had earlier turned down, however good it may be.

The response of the Associate Editors was that we should not change our policy. I regret that the JOURNAL OF GEOPHYSICAL RESEARCH cannot publish your paper.

Sincerely yours,
(sgd): Orson L. Anderson
Editor

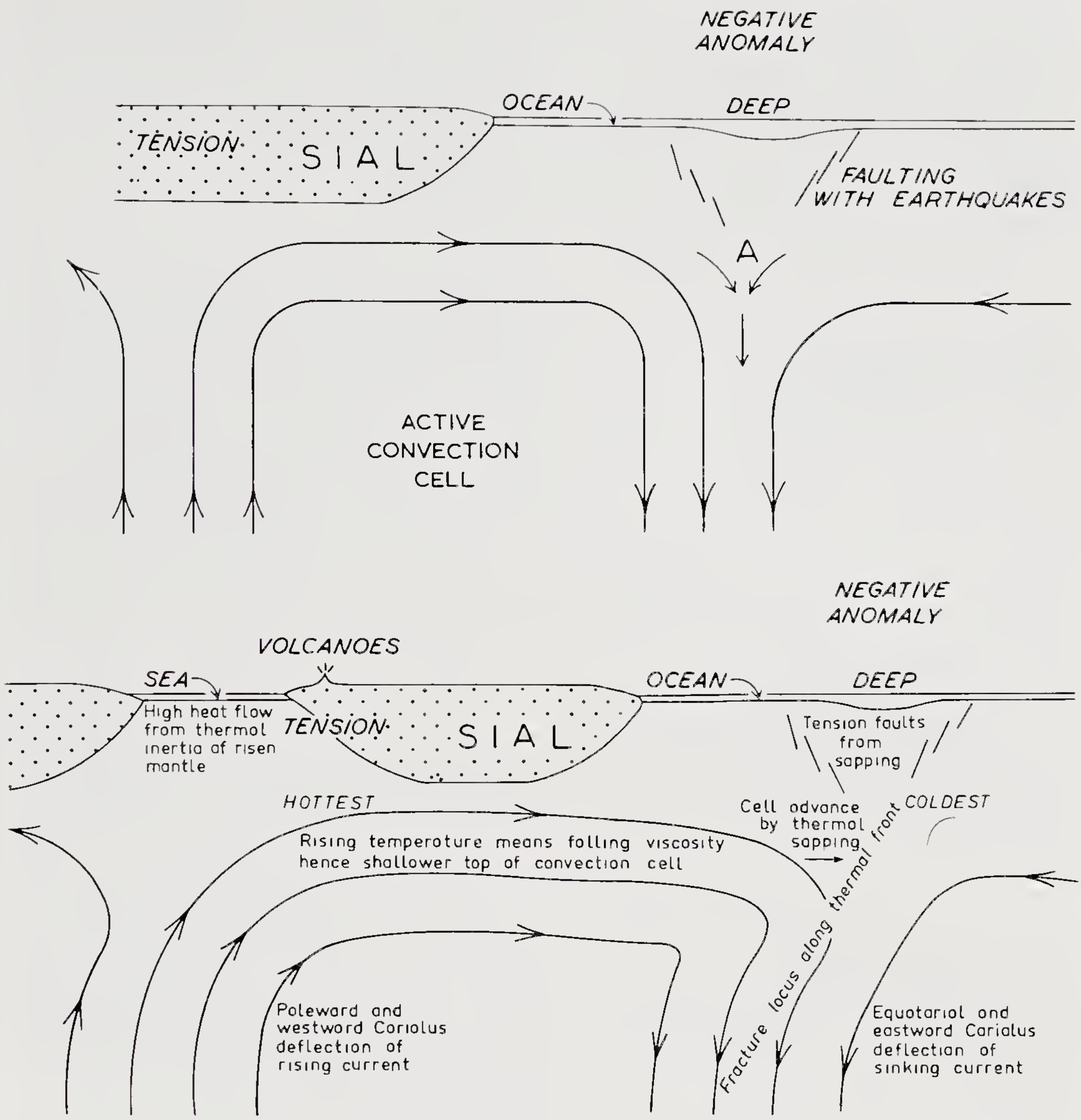


Fig. 5

Migration of continents through sapping and consumption of oceanic crust, as proposed by Carey in 1953.

increasing clarity the significance of the mid-oceanic ridges, as currently active rift zones, which formed a global system of hitherto unrecognised magnitude (Fig.6).

Ocean spreading, commencing as a rift valley system, and widening by addition of paired mantle slices to evolve through a stage like the Red Sea to the Atlantic Ocean stage, still with an actively spreading mid-oceanic ridge, and recognisable paired growth lines on the ocean floor, was first set out by me at the Continental Drift Symposium in 1957 (Carey 1958a, pp.

180-191), and elaborated at the Pan-Indian Ocean Science Congress in Madagascar in 1957 (Carey 1959), in lectures in many American universities in 1960, and in the Holland Memorial Oration in Calcutta in 1964 (Fig. 7). Here the concept was a global pattern of polygons growing and mutually separating by accretion along the mid-ocean ridges.

The last decade has yielded four exciting discoveries. First, that dated Quaternary lavas the world over were consistent in recording reversals of the earth's magnetic field, so that specific epochs of 'normal' and reversed polarity could be identified in chronological sequence. Second, that the same chronology of magnetic epochs, extended now through the Tertiary Period, was recorded in bore cores through ocean floor sediments. Third, that the ocean floor slices in sequence away from the mid-oceanic rifts recorded the same chronology of magnetic epochs in mirror image on both sides of the rift (Figs. 8 and 9). Fourth, that deep ocean drilling established that the bottom of the sediments on the ocean floors was Quaternary near the rifts, but became progressively older further away from the rifts.

As the first category above had been dated by radio-active isotopes, and the second category had been dated by fossils, the ages of the magnetisation of the strips were known. The time of magnetisation could be either the time of cooling of lava extruded through the widening rift, or the time of cooling through the Curie point of slices of already crystalline mantle rock as it ascended to occupy the extension zone. The conclusion that the ocean floors have been spreading slice by slice from the active medial rifts, at now measurable rates, has been confirmed and consolidated by snowballing independent data of several kinds. This avalanche of evidence wholly routed the earlier disbelief and scorn of gross continental separation.

Fig. 6

The oceanic rift system which divides the earth's surface into primary polygons, growing at their cambium-like boundaries. 1, Eopacific; 2, North America; 3, South America; 4, Africa; 5, Europe; 6, Siberia; 7, India; 8, Antarctica; 9, Australia.

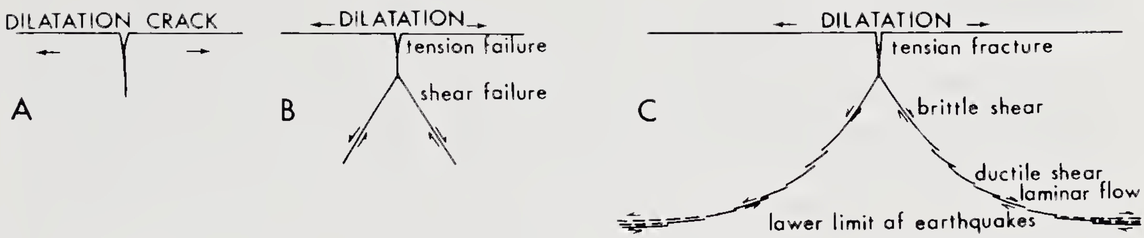


Fig. 7 Evolution from rift valley to mid-oceanic rift with cambium growth of new oceanic crust (from Carey in 1956 continental drift symposium p.184, and Holland Memorial Address, Calcutta 1964).

A Crustal failure in tension.

B Crustal extension by conjugate shears below depth where weight of over-burden exceeds shear strength.

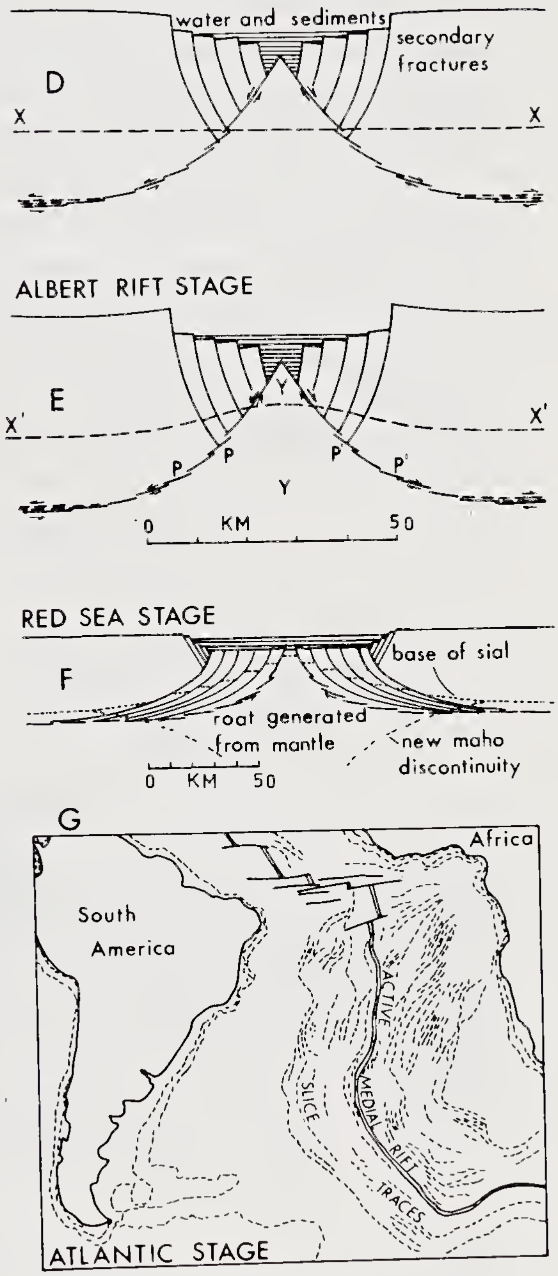
C Depth sequence through failure by tension, conjugate brittle shears, ductile rotation of shears, to laminar flow without fracture.

D Surface collapse, because if the blocks in B were simply pulled apart horizontally, crescent voids would appear along the fracture surfaces.

E Mass deficiency causes line XX to bow upwards isostatically causing raised rims to the rift valley. Phase changes caused by rising temperature through mantle out-gassing also causes a bulge.

F Shears en echelon (PP P'P'...) successively become the main shears, and stack paired slices as separation proceeds. In this process magma is not necessary to fill the gap as continents separate, although magma should also be expected. A "root" of lower density and lower seismic velocity than normal mantle is derived from mantle through thermal phase changes plus magma from partial melt.

G The zone showing the slice traces is the Tertiary crust generated during the Alphide expansion cycle. The rest of the Atlantic floor was generated during the Gondwana expansion cycle.



A shotgun wedding

This concept, now respectable, was married to concurrent swallowing of ocean crust in the deep ocean trenches by Dietz 1961, Hess 1962, Vine 1966, Le Pichon 1968, Heirtzler *et al.* 1968, and Isacks *et al.* 1968 and others, and has enjoyed meteoric rise to almost universal acclaim, and every aspiring author must jump the bandwaggon to gild another anther of this fashionable lily.

Today there are three competing theories: (1) this plate-tectonics conveyor-belt model, currently commanding overwhelming popular appeal; which is essentially what I taught my students in the forties, pooh-poohed then, revered now; (2) a small group led by Meyerhoff, Stehli, and Teichert in America, and quite a number in Russia, who believe that the continents and ocean basins remain now more or less where they have always been; and (3) a small third group - for which I will accept blame that any others may not wish to accept - who are convinced that the earth is expanding and that the separation of the continents by growth of new oceans is not extensively compensated by the swallowing of old crust elsewhere.

Gross expansion of the earth is not a new proposition. The Wegener bombshell of gross continental separation promptly triggered the concept of

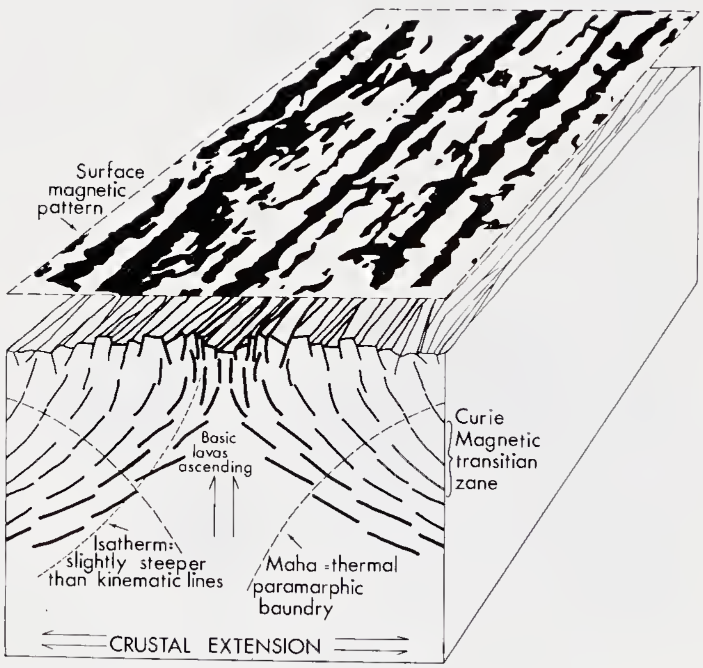


Fig. 8

Magnetic strips (positive anomalies black) parallel to the Reykjanes cambium rift southwest of Iceland. (Magnetic pattern from Heirtzler, Le Pichon, and Baron, 1966).

earth expansion as an alternative to drift, but books in German by Lindemann (1927), Bogolepow (1930), Hilgenberg (1933), and Keindl (1940) got little attention in the English literature. A second wave by Egyed (1956), Carey (1958), Heezen (1959), Neyman (1962), Brösske (1962), Barnett (1962), Creer (1965), Dearnley (1965), Jordan (1966), Steiner (1967), and Meservey (1969), ran against the orthodox tide, which in geology, is lethal.

Kuhnian revolution

Dr. Barrie Jones (1974) recognised that the episodic volte face, from fixism to gross translation of continental slabs, was a Kuhnian revolution, an impulsive switch from almost universal rejection of continental dispersion to an equally widespread acceptance of it. In Kuhn's book (1962) on the structure of scientific revolution, he postulates that "in any field of science there are periods of relative tranquillity separated by revolutions during each of which there is a shift from almost exclusive support of one theory to almost exclusive support of another theory that is incompatible with the former. Each such shift is characterised by a switch in gestalt - that is, there is a fundamental change in the way the *whole* field is com-

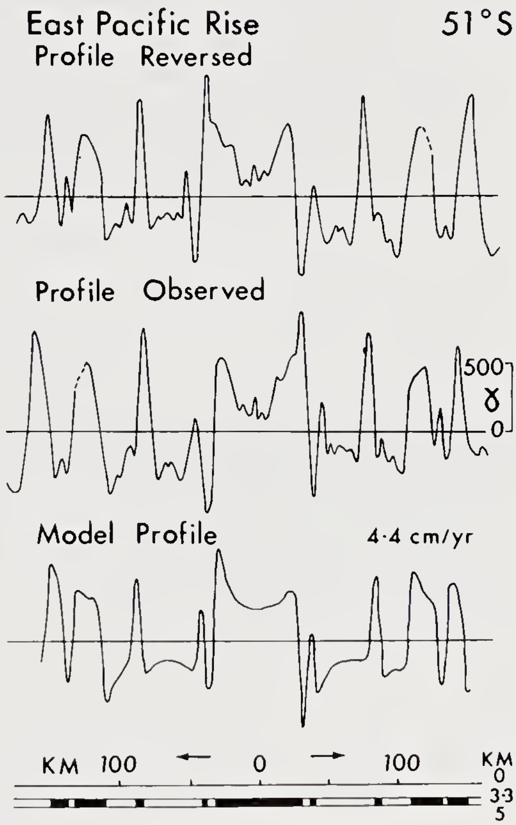


Fig. 9

Total intensity magnetic profile (centre) across the East Pacific Rise, for comparison with the same profile reversed (top), and with the standard model profile for all spreading ridges (bottom). Below the profiles is the succession of normal polarity (black) and reversed polarity (white) according to this model. (From Vine, 1966).

prehended. Each intervening period of relative tranquillity, each period of "normal science", is characterized by widespread adherence to whatever theory is current and by the engagement of almost all the scientists in the exploration and articulation of that theory and its ramifications."

Iconoclast or heretic, it falls to me to extirpate this so popular notion. Subduction exists only in the minds of its creators.

A new round of red-herrings

Just as highly respected scientists like Jeffreys and Simpson got away with quite false rebuttals of continental displacement when this hypothesis was ostracised by the establishment (pp. 6-9 above), so equally false rebuttals of earth expansion by respected scientists gain wide currency during the present subduction euphoria.

PALAEORADIUS

Palaeomagneticians (e.g. Ward 1963, 1966, McElhinny and Brock 1975) have "proved" that the radius of the earth has not changed significantly since the Palaeozoic, by combining in pairs all the magnetic data from a single continent over a single geological period, and calculating which palaeoradius produces the minimum scatter of pole positions. The answer always comes out at about the present radius. As these results emerge from an elaborate computer program, using sophisticated statistics, and produce convincing graphs (Fig. 10), and confirm accepted dogma, the con-

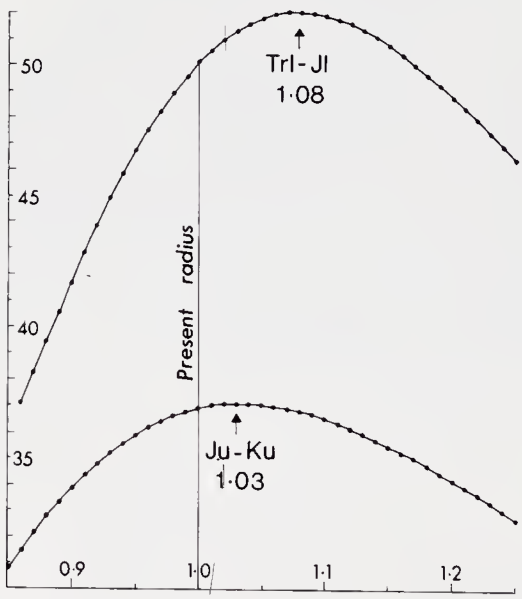


Fig. 10

Most probable palaeoradius deduced from minimum scatter of poles from Africa Mesozoic palaeomagnetic data using Ward's method. (From McElhinny and Brock, 1975).

clusion is clear -- the earth has not expanded, or at least not significantly. Unfortunately the method contains an inherent fallacy such that on an expanding earth, the minimum scatter would always appear at that radius on which the field measurements were made.

The method assumes, *inter alia* that the angle subtended at a central point on the continent by each pair of rock units remains constant (Ward 1966, p. 446). However, as the radius of a sphere increases, the sum of the angles of a spherical triangle on its surface declines from nearly six right angles (when the triangle is nearly a hemisphere) almost to two right angles (when the surface is nearly a plane). The statistics weight the results in favour of the most widely separated pairs of data points where the change in surface angles is greatest. The effect of expansion on the angle between the two great circles which intersect to give a pole position is statistically about the same as the change in geocentric angle on which the palaeoradius depends. Hence the effect of expansion on scatter cancels, and the method should always give minimum scatter at present radius, however much expansion has occurred.

In effect, the palaeomagneticians assume that the transformations from ancient configurations to present configurations occur according to the rules of an azimuthal equidistant projection. Unfortunately the earth's surface does not obey these rules. Cartographers know that in transforming part of the surface of the earth to a plane map, they may elect to conserve angles and distances from a central point (azimuthal equidistant), angles from a central point and all local shapes (stereographic, Mercator), angles from any point and all local shapes (Mercator), all areas (several), but they must hold one or two parameters and let the others change greatly. The same rules apply to transformations between spheres of different sizes, but here we do not have any choice. The earth started from a particular configuration and transformed to another configuration. It was not azimuthal, not equidistant, nor orthomorphic, nor equal-area, but a compromise between all of these. It is nonsense to choose to assume any one transformation. There is only the one reality - the one which actually happened. Because of the stepped hierarchy of adjustment between blocks (Figs. 6, and 17-21) the actual compromise depends on the scale we consider. It would be simpler if the whole of the expansion was adjusted on the scale of joint blocks (Figs. 22, 23), but adjustment of vertical, and adjustment of surface distance occur on all scales, with angle adjustment most on the joints (because of the dominance of gravity) and horizontal adjustment most on the first order polygons (where gravity is not involved).

Equatorial *v.* Meridional Extension

Le Pichon (1968, p. 3674) rejected earth expansion on the grounds that extension on spreading ridges indicated substantially greater increase in the equatorial great circles than meridional great circles. He found as much as 270 km difference between the average radius along extreme great-circle circumferences, and possibly as much as 500 km between individual radii during the last 10 million years, and still more serious discrepancies over the whole Cainozoic Era. As fashion believes subduction and rejects expansion, Le Pichon's statement has been repeated and reiterated as gospel, with gospel-immunity from verification.

What Le Pichon found unacceptable, was not the expansion model, but a Le Pichon model of expansion constrained by his "plate-tectonics" parameters. The only growth taken into his reckoning was the separations at those spreading ridges recognized by him (his Fig. 2), and the only motions were the rotations about the Euler poles of rotation determined on the plate model (his table 2). The Euler theorem, valid for the plate model, does not apply to an expanding sphere (a dominant part of separation is by radial movement outwards). Le Pichon does not recognize extension across orogenic zones, and carries this obsession into the expansion model to which he applies his tests. For example, there has been 8° of longitudinal elongation between Venezuela and Mexico, in addition to sinistral displacement (compare Fig. 16 with present positions), which is omitted from Le Pichon's model. This elongation is normal to the elongations indicated by the spreading ridges of his Figure 2. Likewise he has omitted the meridional elongation between Antarctica and South America shown on all reconstructions of Pangaea to the present positions. Le Pichon's calculations ignore at least half of the 66° meridional elongation between Antarctica through Australia to China (Fig.189), and the meridional extension across the Mediterranean indicated by the orocline tectonic analysis and by the double-equator paradox.

North Atlantic Ordovician Palaeogeography

Several plate disciples (Wilson, 1966; Dewey, 1969; Burrett, 1972, and others) have emphasised the close apposition of strongly contrasted Ordovician faunas across the Caledonian-Appalachian orogenic axis which divides Britain, slices the Atlantic coast of North America and possibly also sequesters Mauretania from Africa. Exacerbating this anomaly is the occurrence of Ordovician tillites over most of Africa (Fairbridge, 1969, 1970) in close apposition with the Ordovician limestones of the United States which carry rich tropical faunas.

Spjeldnaes (1973) has objected that "It may be necessary to give a warning that the fossils and sediments on these continents put certain constraints on the amount of movement you can construct. If you get coral reefs at the poles or widespread glaciations at the equator, this is a sign that there is something seriously wrong with the reconstruction. To put the record straight - the Bullard fit for North America and Africa is absolutely incompatible with the biological and sedimentological evidence from the Lower Palaeozoic. In order to reconstruct the Palaeozoic continent, it will be necessary to change their shape, and to rotate them so much that it will be a waste of manpower and machine time to try to make a reconstruction based on their present geometrical shape."

"Plate" tectonicists overcome these difficulties by postulating a wide Ordovician proto-Atlantic ocean which separated the non-neighbourly faunas and climates, and which vanished down a subduction dispose-all during the Appalachian orogenesis.

However these apparent anomalies stem from the assumption of constant earth diameter and vanish when allowance is made for earth expansion. Figure 11 shows the Bullard positions of Africa and North America which Spjeldnaes has criticised. On it is drawn the North American Ordovician equator as deduced palaeomagnetically by Irving, (1964). Also indicated are all the

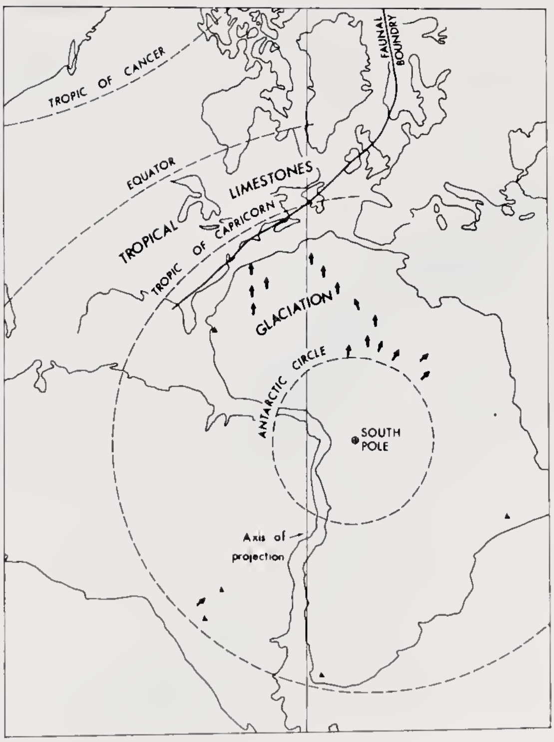


Fig. 11

Trans-Atlantic Ordovician latitudes and palaeogeography.

reported sites of Ordovician glaciation in Africa, which extend over a width of 60°. No Ordovician palaeomagnetic data have been reported from Africa, and the Cambrian and Silurian palaeomagnetic data for Africa are scant and ambiguous so they don't help fix the Ordovician pole either. The best we can do therefore is to assume a pole central to the distribution of the glacials, which extend over 60°. This is indicated.

The empiricism of earth expansion indicates that expansion rate has accelerated with time, and has pulsed. Surface area has doubled since the Palaeozoic, implying a late Palaeozoic radius of 0.7 times the present radius. A reasonable figure for the Ordovician would be 0.6 times the present radius. This is completely in harmony with the distance from the Ordovician equator to the centre of the glacials (assumed South Pole):

	Distance from Ordovician equator	
	Present degrees	Ordovician degrees
furthest south limestones	14°	23°
Africa-North America suture	20°	34°
Most northerly Ordovician glacials	26°	44°
Centre of glacial distribution	54°	90°

Thus the Ordovician limestones extended from the equator to some 23°S which is about the distribution of reef limestones today. The Ordovician glacials extended from the pole to latitude 44°. The waters of Bass Strait are today eroding Pleistocene tills in latitude 41°S, which is about the same as the latitude limits of the tills in Connecticut.

Ordovician red-beds also occur in the Appalachians just where they should be expected. It is pointed out in the later chapter herein on the rotation of the earth that circumpolar glaciation occurs during periods of large obliquity of the earth's axis to the ecliptic (as is now the case on Earth and Mars), and that during such periods seasons are sharply differentiated, as are the climatic zones, with an arid high-pressure belt between twenty and thirty degrees from the equator.

The North Atlantic Ordovician palaeoclimatic anomaly is simply the creature of the fixed-radius obsession.

We are still left with the apposition of distinctive faunal provinces. Ager, one of the most experienced of stratigraphers has commented (1973, p.18): "Mountains of paper have been piled up on the subject of faunal provinces in the fossil record; but very few of them stand up to critical examination and even the latest symposium on the subject has produced very little that can be regarded as concrete evidence. Almost all the differences that have been noted in contemporaneous fossil faunas and floras can be explained in terms of local environmental differences that are reflected in the sediments. Thus the famous 'Bohemian' (or 'Hercynian') and 'Rhenish' provinces of Devonian times are little more than the differences between a lime-mud and a sandy sea-floor".

Some (e.g. Williams, 1969) have sought to solve this faunal apposition by sequestration by two-way currents along the geosyncline. Burrett (1972, 1973) considered this improbable to the level of rejection, because of the distinctness of these provinces over such a long stretch of time. This assessment is certainly reasonable. However his next step in assuming that this implies that a wide intervening ocean has been subducted does not necessarily follow. There are several alternatives which may result in such Wallace lines. The most probable in this case seems to be sinistral shift along the Ordovician orogenic zone, which is near-equatorial.

The Tethyan orogenic zone experienced strong sinistral shift of tens of degrees during the Cainozoic (Figs. 91 and 172). The Caledonian-Appalachian orogenic zone was near-equatorial during the Ordovician, and similar inter-hemisphere transcurrency would not be unreasonable. The sense of offset and general trend is the same as those of the Great Glen fault and a bundle of cognate mid- to late Palaeozoic sinistral faults through Scotland. Moreover the Grenville core of the Appalachian zone has a gross sinistral S pattern, from the Grenville belt through the Adirondacks to Vermont thence to the Blue Ridge of Tennessee, which appears to be a sinistrally coupled orocline pair (Fig. 12), similar to the Mendocino-Idaho oroclines of the

Fig. 12

Sinistral coupling of possible Palaeozoic oroclines in Grenville correlates in eastern North America.

Pacific margin (Fig. 143). The Allegheny mio-geosyncline would be related to these oroclines in the same way as the Basin-and-Range depression is related to the Mendocino-Idaho oroclinal couple. This interpretation, if valid, would confirm gross early Palaeozoic sinistral shift along this belt.

Reversal of such a shift could bring the Lower Ordovician bathyurid fauna of America, northern Britain, Norway, and Spitzbergen opposite the bathyurid fauna of Siberia, instead of facing the asaphid faunas of the Baltic and the *Selenopeltis* fauna of England and Florida (Whittington, 1973, Fig. 1). Similarly in the Upper Ordovician the monorakid-remopleurid faunas would be brought opposite similar Russian faunas instead of facing trinucleid-homalonotid faunas (Whittington, 1973, Fig. 2).

Figure 11 of course suffers from the disability of attempting to reconstruct Ordovician palaeogeography on a projection which assumes the present earth radius. This discrepancy can be reduced to below threshold limits along the Atlantic, but only by producing yawning gores laterally. Siberia could not be closed against Canada on this projection, and the fifteen degree gap between Turkey and Egypt is purely an artefact of this projection. Hence in extending to Europe the North Atlantic palaeogeography depicted in figure 11, it must be remembered that southern Europe lay south of the tropic of Capricorn. For these reasons figure 11 is not intended to be a final statement of the palaeogeography, but rather a reconciliation of data so far available, to show the alleged Ordovician anomaly to be a fiction.

Burrett's provinciality index method begins with the assumption of the integrity of his plates. If plate tectonics is assumed *a priori* then his method is a useful guide for seeking to understand the relative movements of the plates. But the method can only be misleading when applied to non-plate models. The boundaries adopted for integral regions significantly affect the statistics of provinciality indices. The methods adopted by Waterhouse and Bonham-Carter (1975) are more reliable than Burrett's when the purpose is to test competing tectonic models. Burrett considers that oceanic barriers are the simplest explanation for the maintenance of faunal provincialism, and he assumes that orogens and ultra-mafic rocks identify sutures whence former oceans have vanished. On the expansion model by contrast, orogens and serpentinites are more likely to have been belts of extension. Ocean currents, latitudinal zonation, and available facies (originally and now outcropping) should not be ignored; broad epeiric seas separated by narrow rising rims from through-going deep rifts, with waters of different salinity temperature and depth, may result in proximity of faunas of sharply contrasted aspect through substantial time.

DEVELOPMENT OF EXPANDING EARTH CONCEPT

Bacon has been credited with the first suggestion that the earth might be expanding, but his only reference (in the 27th Aphorism of Book II of the *Novum Organum*) cannot be held to mean this:

> *"Verum his missis, etiam in ipsa configuratione mundi in majoribus non sunt negligendae instantiae conformes; veluti Africa, et regio Peruviana cum continente se porrigente usque ad Fretum Magellanicum. Utraque enim regio habet similes isthmos et similia promontoria, quod non temere accidit.*
> *Item Novus et Vetus Orbis; in eo quod utrique orbes versus septentriones lati sunt et exporrecti, versus austrum autem angusti et acuminati."*

Certainly *exporrecti* can be properly translated as "expanded" but here in a descriptive sense only.

About 1800 Humboldt (and probably others also) was struck by the matching shapes across the Atlantic (refer to Sabine's translation of the Kosmos, vol. 1, p. 280 *et. seq.*), but he did not conceive earth expansion as a cause.

Late in the nineteenth century, William Lowthian Green (1857, 1875, 1887), working alone in Hawaii remote from the conceptual pollution of orthodox establishment, conceived not only earth expansion, but also interhemisphere shear whereby the southern continents were displaced eastwards and separated from the northern continents by zones of profound crustal disturbance and vulcanism.

Towards the end of the century, one, Mantovani, is said to have suggested earth expansion to explain the similarity of the opposing Atlantic coasts (see Egyed, 1963). Later, an American Hiram W. Hixon (1920) rejected the contraction theory as inadequate to explain many geological phenomena, such as the African rift system, the Great Valley of Nevada, the Colorado Plateau, and the preponderance of normal faulting. He concluded that orogenesis and epeirogenesis were gravity-governed diapiric phenomena, caused by outgassing from an expanding earth.

Global tectonics developed primarily in the European schools fluent in German, fertilised no doubt by the great pioneers Suess and Wegener. English-speaking schools, with rare exceptions like B.B. Brock, remained side-tracked in the groove of compressional orogenesis, whereas in Europe, Russia, and Scandinavia many recognised that not only epeirogenesis but especially orogenesis is primarily a vertical phenomenon, with secondary superficial spreading. Bittner, Ampferer, Sederholm, Daqué, Haarmann, van Bemmelen, Beloussov (and many other Russians), to Krebs (1975) and the excellent model experiments by Ramberg, have emphasised the diapiric pattern of orogenesis. It is natural therefore that the idea of earth expansion was conceived and developed primarily in the German literature.

The controversy and ferment following the publication of Wegener's book, *Die Entstehung der Kontinente und Ozeane*, spawned not only the continental drift concept which took root also in the English language, but also at the same time the alternative explanation by earth expansion, but this latter line developed mainly in German. As Termier and Termier (1969) remarked later, "all palaeogeographic arguments that are, in general, in favour of continental drift are also in favour of the earth's expansion".

Lindemann (1927), in his book *"Kettengebirge, kontinentale Zerspaltung und Erdexpansion"*, argued that the dominating phenomenon of the earth's surface is rifting and extension. He accepted the validity of Wegener's disruption and dispersion of Pangaea, but attributed this to the expansion of the interior. He developed an integrated scheme of the evolution of an expanding earth, based on radioactive heating and analysed the causes of orogenesis with an insight which merits much wider currency and recognition than it has received.

Moschelles (1929) published an extended review of the general part of Lindemann's book, but stimulated little interest in the English-speaking world.

Bogolepow, in three Russian papers in 1922, 1925, and 1928, which are referred to in his 1930 work *"Die Dehnung der Lithospäre"*, probably preceded Lindemann in suggesting earth expansion. Caused primarily by radioactive heating, he proposed secular differential zonal motions in the mantle, which resulted in dextral eddy-like underdrag in the southern hemisphere and sinistral in the northern hemisphere.

Hilgenberg's *"Vom wachsenden Erdball"* (1933) was dedicated to Wegener, but he does not appear to have been aware until much later of Lindemann's book published six years before his, nor was he then aware of Bogolepow's contributions. Hilgenberg first assembled the continents on a basket-ball-sized papier-maché globe, the original of which I was privileged to handle when I visited him a decade ago. All the oceans had been eliminated and the sialic crust neatly enclosed the whole earth on a globe a little less than two-thirds of the diameter of the reference globe. He postulated that the mass of the earth as well as its volume waxed with time. To explain this he clung to the moribund aether flux concept of gravitation, and claimed that energy of the aether flux was continually absorbed in aether sinks associated with matter, and was transformed into matter. Hilgenberg still adhered to this interpretation at the 1967 Newcastle symposium, and in 1969. Hilgenberg (1973) again reaffirmed his 1933 model, now with the support of palaeomagnetic data, specifically for his Cambrian reconstruction.

Halm's presidential address to the Astronomical Society of South Africa in 1935 deduced an expanding earth from a theoretical analysis of the evolution of celestial bodies including stars and planets, tied to the variation of the effective size of atoms. He did not appear to have been aware of Lindemann's or Hilgenberg's books, though he adopted Wegener's Pangaea and explained the opening of the oceans in terms of his theory, along with other broad geological phenomena; but he did not consider Hilgenberg's model of the continents formerly enclosing the whole earth. Halm derived theoretically a radius increase of the order of 1000 km. He pointed out the implied progressive emergence of the continents through time simply through spreading over the increased surface area without consideration of the greater water depth in the disjunctive ocean basins. Halm explained the Red Sea as a proto-oceanic rift in the early stages, regarded the Mediterranean as disjunctive, and interpreted the Gulf of Honduras (p. 19) as a yawning gap, "the jaws being hinged in the neck occupied at present by Mexico".

Keindl, *"Dehnt sich die Erde aus?"* (1940) was, like the others, initially stimulated by Wegener, and he only discovered the works of Lindemann and Hilgenberg after he had independently reached the conclusion that the earth had to be expanding, in order to satisfy the first-order morphology of the earth. Nor did he seem to have been aware of Halm's contribution five years earlier. Like Hilgenberg, Keindl opted for an original sialic crust covering completely the whole earth, with subsequent tensional disruption, which gave rise to the ever-growing oceans. The source of the disruption and of all orogenesis had to be sought deep within the earth - at least in the core. The whole universe and everything within it is in a state of expansion. Normal stars differ from white dwarfs in that the luminous gaseous envelope has been stripped from the super-dense cores of the latter. Keindl argued for a small super-dense metastable core in the earth, and reached a conclusion similar to Halm but by a different route.

Shneiderov (1943, 1944, 1961) developed a theory of a pulsating earth wherein cataclysmic expansions produced the oceans, and slower contractions produced orogenic diastrophism; each contraction was less than the preceding expansion, yielding overall irreversible expansion. Shneiderov claimed that the earth has a nucleus of dense hot plasma, excited by a flux of cosmic sub-atomic particles (radions), the intensity of which is modulated by syzygies of the earth with the sun, moon, and planets.

Walker and Walker (1954), two "economic geologists, with a joint span of experience covering over fifty years of surface and underground observations, finding themselves confronted by more and more geological evidence, which could not possibly be reconciled with [the contraction] hypothesis, were slowly and reluctantly forced to the opposite conclusion that the Earth was increasing in volume, and that the cause of this phenomenon must be some expanding mass at the center of the Earth. This idea once adopted, the phenomena of vulcanism and orogeny, - heretofore inadequately explained - all fell into place like the parts of a jigsaw puzzle." They reached this conclusion it seems wholly independently of the earlier writers on the expanding earth, nor did Wegener or gross tectonics or global morphology play any part in their conception, which spawned from smaller scale conventional mineralogy, petrology, and orogenesis.

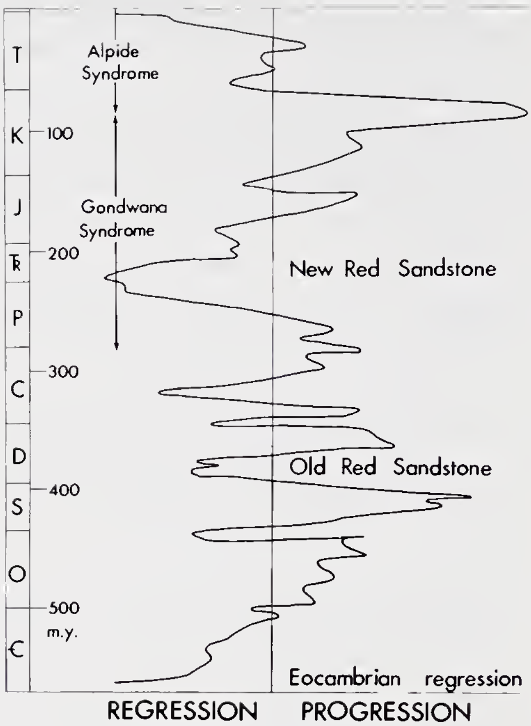
Egyed (1956) pointed out that although the total volume of ocean water had increased during geological time by not more than 4%, palaeogeographic maps of land and sea for the individual epochs since the Precambrian, compiled independently by the Termiers and by Strahov, showed a progressive decline in the proportion of submergence of the continents individually and collectively. This implied that the surface area of the earth had increased and that the relative proportions of ocean basins to continental platforms also steadily increased. Indeed, Darwin had concluded long ago (1883, p.288) that since the Precambrian, continents seem to have suffered a preponderance, during many oscillations of sea-level, of the forces of elevation, while in contrast, true oceanic islands showed no remnant of Palaeozoic rocks. Suess (1889) likewise had observed a secular emergence through geological time. Egyed calculated an average increase of 0.5mm per year in the earth's radius, which he assumed to be uniform, although Fairbridge (1964, p. 65) suggested that the indicated expansion may well have accelerated since the Mesozoic, as indeed much other evidence supports.

Armstrong (1969) pointed out that the major controls of sea-level (assuming constant ocean and crust volumes) are polar and glacial ice, orogenesis, epeirogeny, and erosion, and the oceanic rises which are elevated because of their transient higher temperature. All these effects are valid, and no doubt contribute to the strong fluctuations in Egyed's graphs, but could nevertheless amount only to modulations on a steady state. However,heat production (which is ultimately responsible for the rises) has declined by 20% over the last 500 million years, which, translated into volume of oceanic rises, would account for an 80m overall fall in sea level, a large proportion of Egyed's emergence. But is Armstrong's premise valid? This would imply a decline of tectonism with time, which is contrary to the general empiricism that the reverse is true. Hallam (1971) accepts the emergent trends recognised by Egyed, and also the second-order cycles, but offers the alternative explanation that continents have thickened by mantle-differentiation underplating more rapidly than they have been reduced by surface erosion, so that the continents have stood progressively higher through geological time. Egyed's conclusion has been criticised by Veizer (1971) on the grounds that his time units were systematically longer in the earlier periods so that, if continental platforms were randomly rising and sinking, longer time-samples would consistently reveal a larger area as having been submerged within that time-sample. However, maps carefully replotted with the best modern information still confirm Egyed's general result. Moreover they also reveal (as indeed do Egyed's graphs more crudely) that the general emergent trend is modulated by long-term second-order cycles. Nevertheless Wise (1974) considers that the residual emergent trend is still an artefact of sampling, which will gradually erode as data improve. For my part, I do not think the argument is very relevant to expansion. The total volume of seawater has increased with time from the outgassing of the earth, and the area and capacity of the ocean basins has also increased with time. Both stem from the same cause and might well both increase equally, though not being necessarily strictly in phase, short and long cycles of global transgression and regression modulate the growth (Fig. 13).

At the Hobart symposium on continental drift, March 1956, the present reviewer pointed out that when reconstruction of Pangaea is attempted by accurate plots using oblique stereographic projections, or by using spherically-moulded plastic tracing foil to transfer from a thirty-inch globe to a hemispherical compilation table, radial gore-like gaps invariably appear, which separate regions which the local geological history would tie together. This applies to every reconstruction that has been attempted by all authors, and arises from the fact that the assemblages have been made on an earth of present size. A coherent integral assembly is only possible on a globe of smaller radius, because the value of π (the ratio of the circumference to diameter) of a circular area on the surface of a sphere increases with increasing radius of sphere, to reach the conventional value of π when the size of the sphere becomes infinite. This led me to the finding of Hilgenberg's and Egyed's papers and thence to Keindl and Halm. Accordingly the hypothesis of an expanding earth was actively explored and found to

Fig. 13

Transgression and regression through Phanerozoic time - a qualitative plot compiled from various data (mainly from Schuchert, Stille, the Termiers, and Strachov).



solve many hitherto puzzling features of global tectonics.

Heezen (1959) stated to the Nice-Villefranche international colloqium of the CNRS in May 1958 that continental displacements can be effected in two very different ways - by continental drift, in which blocks float laterally across the upper part of the mantle, or by expansion of the interior of the earth, so that solid and differentiated crust breaks, and individual blocks become separated by insertion between them of newly derived mantle differentiate. In the case of continental drift one should find compression right along one side of a continent and extension structures on the opposite side. In the case of expansion of the interior one should find extension in all oceanic zones. Seismic refraction measurements in the upper part of the continental ramp along the east coast of North America revealed a sediment-filled trough of analogous dimensions to the present marginal trenches of Puerto Rico and the circum-Pacific girdle. The differences of morphology were probably the result of their different rates of sedimentation, and not of a difference of structural origin. The explanation of deep sea trenches by a great tectogene down-buckle of the crust was not tenable having regard to the geophysical data. The continental margins of all the coasts of the continents seemed to give evidence of extension.

Groeber (1959) argued that the surface area of the earth had increased by 27% since the Palaeozoic.

Wilson (1960) was stimulated by Dicke's support of Dirac's proposal that the gravitational constant diminished with time, which would involve some expansion of the crust. He rejected the concepts of a doubling of the radius from a primordial earth totally enclosed by sial, but found that the mid-oceanic rifts, the distribution of heat flux, and several other features were compatible with more limited expansion, "though this does not constitute a proof. Even if true, expansion at the rate here postulated could conceivably be due to phase changes in the Earth's interior or perhaps to differentiation of the core and mantle, but a decrease in G remains an inviting idea."

This idea of secular decrease of G with consequent earth expansion was taken up in Russia by Ivanenko and Sagitov (1961). Neymann (1962) also proposed an expanding earth from a study of relations across the Pacific. In this he followed Kirillov, who had reached the same conclusion but had assumed that the Pacific Ocean had formed by a widening rift between the Rockies and the Andes, which had originally been in apposition. Prophetically he stated that the parts of the Pacific Ocean adjoining North America have appeared only in the Cainozoic, which indeed is what the palaeomagnetic dating of the ocean floor strips has since established.

Brösske (1962), like Hilgenberg, reconstructed all the continents to encompass the entire earth. His assembly has a conventional but tighter relationship for Africa and the Americas, Greenland, Europe and Asia, but puts eastern Australia against the Peru re-entrant of South America, with the northern margin of the Australian block against California, and the East Indies packed between the Australian north-west shelf and the south-east Asian mainland.

Barnett (1962), unaware of earlier work other than Wegener's and the rigorous confirmation of the South Atlantic fit (Carey, 1955), cut rubber templates from a 4½ inch globe and reconstructed them on a 3-inch globe. The relations across the Arcto-Atlantic and Australia to Antarctica were conventional, and the Pacific was closed by bringing West Antarctica against the southern Andes, eastern Australia against Central America, and the northern margin of Australia against North America. Barnett remarked that "it is difficult to believe that chance alone can explain this fitting together of the continental margins". In a later paper (1969) Barnett recalled the resemblance of the southern continents to the petals of a flower, from a swelling bud. To quote Barnett: "A comparable pattern may readily be obtained by coating a rubber football bladder with a continuous crust of damp paper and then inflating it. Linear fractures are produced enclosing three or more petal-shaped forms 'aiming' towards the point of initial rupture. As the rupturing paper crust opens up like an expanding flower bud, each primary fissure extends and divides peripherally into secondary fissures to form smaller but still tapering patterns with occasional complete separation of large paper 'islands'." The bud-and-petal analogy, which had been developed fully by Hilgenberg (1933, p. 29), is useful because it incorporates the earth's hemihedral asymmetry, the antipodal relation of continents and oceans, the greater separation of the southern continents, and the northward migration of all continents with respect to the southward-moving parallels of latitude as the southern hemisphere (the opening calyx) expanded more rapidly than the northern (Carey, 1963).

Trapeznikov (1963) in a critical analysis of the expansion hypotheses, concluded that they were physically improbable - the perennial conflict between empiricism and doctrine.

Fairbridge (1964), like Heezen earlier, gave a comprehensive review of the literature and evidence for an expanding earth. Among many other things he pointed out that all the ocean basins are youthful, and that theoretical consideration of the gravitational constant, of mantle-core evolution, of geodetic consequences of mass displacements, polar shifts, and palaeogeographic development, all converge to support geologically youthful expansion of the globe. Subsequently (1965) Fairbridge, in a further review, could find no evidence that could justify ocean trenches as compressional phenomena, nor did orogenic belts call for primary crustal compression. On the contrary he interpreted the deep-sea trenches as the contemporary prototype orthogeosynclines, as tension gashes in a crust extending continuously at an increasing rate.

Creer (1965) prepared a set of perspex shell models of the continents on a 50-cm globe and remoulded them to the curvature of a 37-cm, and finally on to a 27-cm globe, and formed the impression that the fit of the continents on a smaller earth appeared to be too good to be due to coincidence, and required explaining. According to Creer the sialic skin first developed a U-shaped crack between Australia and America and between Australia and Asia, with subsequent expansion largely taken up in this initial crack which widened to form the Pacific Basin. Creer differed from all others in excluding expansion as the principal cause for drift or orogeny, but instead regarded it as a secular background phenomenon of cosmological origin, which was overprinted by a more rapid polar wander of planetary cause and by more rapid processes in the mantle which caused continental drift, and by regional crustal disturbances which produced orogeny.

Creer estimated the earth's radius as 0.55R in the early Precambrian, 0.94-0.96R at the beginning of the Palaeozoic, and 0.96-0.97R at the beginning of the Mesozoic.

Holmes (1965) reviewed the development of the expanding earth concept, and favoured decrease in the gravitational 'constant', coupled with phase changes through the inner and outer region of the core and the mantle as the prime cause, with convective circulation in the mantle as the probable mechanism.

Although the dual peaks on the hypsometric curve are obvious to the most casual inspection, Joksch (1955) pointed out that a rigorous statistical analysis of the frequency of altitudes discloses that the hypsometric curve is the combination of three primary distributions with median elevations of 4.5 km below sea level and 0.2 and 0.5 km above sea level. The two positive levels raise a hornet's nest of Davisian versus King canons of landscape evolution, with the possibility of one surface inherited from the Gondwana cycle and one from the Tertiary cycle. However Joksch, following Jordan, suggested that early expansion disrupted an early sialic crust, and that crustal differentiation and underplating continued, so that subsequent expansion disrupted a second layer which was in turn separated by the third (oceanic) layer. This involves an unsteady decline in G.

Jordan's approach (1966) was inspired by Dirac's philosophical proposal thirty years earlier that the gravitational constant G varied inversely with the age of the universe, which Jordan defended as the prime cause of earth expansion. Jordan systematically reviewed a large range of astronomical, geophysical, geological, and climatological contributions relevant to global tectonics, and concluded that the continents are remnants of an original entire sial layer which was ruptured by pan-global rift systems, along which new oceans developed from the underlying sima as the earth expanded. He emphasised that apart from expansion there was no single concept which explained why the earth has an intermittent blanket of sial while the hypsometric curve shows two crustal categories, not a distribution about a mean. However the plate tectonicists could counter this argument by postulating primordial twin polar sialic continents, produced by initial convection with subsequent disruption and redistribution by their conveyor-belt model.

Dearnley (1965a, 1965b, 1966) deduced an expanding earth model from a reconstruction of Precambrian orogenic belts. He assumed orogenesis to be the surface expression of mantle convection cells which form a multi-lobed jet stream with 2-, 3-, or 4-lobed patterns as proposed by Runcorn (1962), the transition being governed by the ratio of core and mantle radii. As the core grew the pattern changed from two to four lobes, with each pattern producing its consequential distribution of orogenic belts, and each transition resulting in crustal disruption, associated with continental drift and polar wandering. In accordance with this hypothesis Dearnley proposed that the earth radius was 4400 km 2750 m.y. ago, and 6000 km 650 m.y. ago, compared with 6378 today.

Steiner (1967) attributed a wide variety of first-order geological phenomena directly to the additive effect of the Dirac-Jordan secular decrease in G and a pulsation of G through the rotation of the galaxy, with a period of some 280 million years.

Waterhouse (1967) preferred "a model of an earth exploding in size since the Jurassic, with the sial never significantly detached from the sima, but carried apart by an expanding layer of sima much as Heezen (1962, p. 285) mentioned, coupled with considerable transcurrent shift". With all this I agree, except that the Jurassic is not the beginning. The expansion process is as old as the earth, but has accelerated with time. However the Precambrian expansion proceeded with extreme slowness but ever increasing with successive pulses of increasing magnitude. The late Proterozoic pulse about 800 million years ago was the first to achieve panglobal rifts (the eoPacific and eoTethys) but the early Mesozoic pulse and still greater Palaeogene pulse were the first to take on the "explosive" scale visualized by Waterhouse.

Meservey (1969) showed that the post-Palaeozoic movements of the circum-Pacific continents postulated by the plate-tectonic syntheses were topologically impossible unless the earth was expanding. The present perimeter of the Pacific is less than a hemisphere. Yet the perimeter polygon enclosing the Pacific has greatly increased by extension between the continental blocks, whereas in each of the plate-tectonic syntheses it should have greatly decreased. Meservey emphasizes that to transform from any of the configurations of the continents proposed for early Mesozoic to the present configuration, consistently with the ocean floor growth strips indicated by palaeomagnetism, is impossible on an earth of present size.

Deuser (1970), assuming constant earth mass, constant angular momentum, and rotation rates 150 m.y. ago of 380 and 395 days per year (based on Well's coral growth-line estimate), deduced equatorial radii between 6250 and 5990 km according to earth ellipticity assumed; these radii correspond to maximum increases of the equator of 804 and 2437 km respectively. Deuser compared this with the 2600 km of new crust on the last 70 million years alone, and concluded that expansion, if valid, must be quantitatively insufficient to match the observed crustal growth, hence there must be crustal sinks where crust disappears, hence the expansion hypothesis loses its *raison d'être*. The assumptions of constant mass (see later) and constant angular momentum are not necessarily valid. The South Atlantic expansion rates are exceptionally high, and do not represent the mean, either globally or since the Mesozoic, still less since the Palaeozoic, or since the Proterozoic. Deuser went on to interpret the Caribbean and Scotia arcs as evidence of foreshortening between North and South America and between the latter and Antarctica. But North and South America were very much *closer* in the relevant directions when fitted back against the African template, or by their palaeolatitudes, as also were South America and Antarctica. And even if this were waived and Deuser's interpretation of these arcs accepted, this would not satisfy Meservey's topological paradox.

Rodolfo (1971) claimed to escape Meservey's argument. He stated that where a moving continent is a significant fraction of the earth's circumference, its advancing front should suffer longitudinal extension until the front becomes a great-circle, and thereafter continued advancement would cause longitudinal shortening. He suggested that the Pacific front of the Americas had first elongated greatly, and thereafter had shortened, to produce the bowing of the Caribbean and Scotia arcs. Although Rodolfo in his preamble adopted the Euler theorem, that any translation on a sphere can be defined by rotation about a pole, his analysis and constructions (e.g. his figure 3) do not conform to this and are hence invalid. On the plate-tectonic model, the entire earth's crust, or any segment of it, however large or small, or however oriented, could rotate indefinitely about an Euler pole without extension or shortening, provided oceanic crust was consumed before it. Moreover the pattern of growth of magnetic strips, especially the Tertiary ones which are best identified and most relevant to Rodolfo's argument, do not permit the postulated post-Jurassic longitudinal shortening across the Caribbean and Scotia regions. Rodolfo correctly points out that, although the area of Meservey's spherical polygon enclosing the Pacific is only 35% of a hemisphere, its perimeter is actually longer than a great circle (contrary to the illusion caused by Meservey's azimuthal equidistant projection). However Rodolfo does not thereby rebut Meservey's essential point, that an oceanic *area*, equal to the combined area of the post-Jurassic Atlantic and Indian Oceans, must transfer from Panthalassa (Proto-Pacific) to Pangaea, during a time interval within which the intercontinental sides of the Pacific-bounding polygon (which contains no re-entrant angles) increase very greatly in length. Unless the earth has expanded greatly, within that time interval, this is topologically impossible. The situation is not altered by any amount of crust consumption within Panthalassa, nor by any extensions or contractions of these intercontinental links during the time interval. Meservey's argument stands inviolate.

Ranalli (1971) set up a theoretical model which would result in at most a few hundred kilometres of radial expansion since the formation of the oldest datable rocks. This would result in the formation of some new oceanic crust, but less than required for the development of all the young oceans. Nevertheless instability arising from this limited expansion is important as a trigger for tectonism generally. Expansion in this model slows down with time, which is contrary to empiricism. However Ranalli agrees that if factors other than radiogenic and gravitational energy are involved (such as changing G), very large expansion could occur.

In my presidential address to the Australian and New Zealand Association for the Advancement of Science (1970), I stated that the global distribution of the ocean trenches did not correlate with the distribution of oceanic rifts as the plate-tectonic model required. Each of the continental polygons had increased substantially in area by accretion of new crust since the Palaeozoic, and each continent had increased its distance from each of its neighbours. This universal dispersion was greatest from a point near the Scotia Sea and least from east Siberia (the earth poles for the early Mesozoic). Trenches were extensional zones analogous to the rifts at the head of landslides, or to the *bergschrund* of a glacier, or to the semi-circular arc of grabens and horsts which frame the Gulf of Mexico, whence departed Yucatan and Honduras.

Dooley (1973) reviewed several criticisms of the expansion hypothesis, such as palaeomagnetic data and available energy, which are discussed later. His main contribution concerns the topology of the transformation of continental cratons with substantial change of radius. For example, an early Mesozoic continent originally subtending 40° at the earth's centre should have an elevation of more than 100 km on the present earth, where it would subtend only 30°. Now the total change of curvature to be absorbed by the lithosphere in 10^8 years, if it remained always in gravity equilibrium, amounts to less than 1" per horizontal km, which is less than the cold crystalline crust of the Fennoscandian craton absorbed during the last 7000 years. Even if the rocks were not jointed (where 1" per km would be totally lost), and even if the stress did not disappear by an infinitesimal bias in the semi-diurnal elastic cycle of the body tides, the elastic stress so induced would relax with a half life of some 10^5 years. In fact the process is more complex than Dooley's simple terrella. The deformation of a first order continental block (which involves the whole mantle) is distributed first as basins and swells (which are some hundreds of km across and probably involve only the lithosphere above the asthenosphere); these deformations are distributed in turn among tilt-blocks and warpings some tens of km across (e.g. the tilt units recorded in Japan); these deformations are further dissipated among megajoints some hundreds of metres apart, and so on down through the hierarchy of lesser joints. Adjustment of the continent to its new curvature should certainly not be looked for in gross anomalies of the geoid, because adjustment occurred *pari passu,* nor in gross erosion, because there was never systematic elevation, nor in continental tilts, because they never reached the threshold of observation.

Dooley also raised the difficulty that, if the continental outlines fitted together on a smaller earth, they should not fit precisely on the present earth, and *vice versa.* This is not necessarily correct. For example if the whole surface crazed into small joint-block polygons, each of which moved out radially with small adjustments at all joints, the shapes would fit. Where there is an hierarchy of polygons (Figures 6, 17, and 20) shape discrepancies should appear on the first-order scale. This indeed is true, and it was this very misfit, which increased in magnitude as the size of the assembly increased, which first led me to suspect that the earth had expanded.

The behaviour of a cratonic sector settling from a smaller radius to fit an expanded globe had been discussed previously by Rickard (1969), who argued that, on an expanding earth, compression would occur along the margin of the continental craton, where a geosyncline and orogen would develop, complete with volcanic belt and Benioff underthrust zone, but without crustal consumption. Rickard's model assumes significant enduring strength in the continental crust. In this he is probably correct, notwithstanding the evidence of the post-Pleistocene uplift of Fennoscandia, Labrador, and Lake Bonneville, with a half life of some 10^3 years. Many discussions of crustal strength fail to appreciate the difference between isostatic and hydrostatic inequalities. In the former, the stress-difference persists to all depths (Fig. 58), and rapid adjustment occurs where the effective viscosity is 10^{21} or even less; and hence has a half life of 10^3 years where areas are large; crustal bending of a few seconds of arc per km (initially elastic or by joints) occurs in the shallow layers. Hydrostatic equality would only be attained when the continents have flowed out to cover the whole surface of the earth. In this case the stress-difference is confined to the upper 30 km where the effective viscosity exceeds 10^{26} poises, so that half life of the stress-differences is 10^{9} years; that is, even on an elasticoviscous model, continents endure through geological time, and can sustain significant stress-difference for extended periods.

However, for the very reasons just stated, Rickard's model fails, because it is founded on initial super-elevation of the central sector without any way of attaining that state. Because of the rapid adjustments in the asthenosphere, and the operation of Pascal's principle, the required super-elevation could never come about. Certainly the central sector must rise because of the megageotumour beneath it, but it would never depart far from isostatic equilibrium (Fig. 58), and there would never be any lateral gravitational force beyond that arising from hydrostatic equilibrium, which continents can sustain through geological time. Finally Rickard's model involves the common misconception that orogenesis is a compressional phenomenon.

Perry (1972) attributed the development of the Gulf of Alaska, Bering Sea and adjacent portions of North America to earth expansion, using the data of magnetic anomalies, seismic reflection profile and JOIDES drill holes: "During the Cretaceous the western margin of North America probably was south of the present Aleutian Island Arc area and the Gulf of Alaska existed only as a long narrow basin lying along a rift being opened in the continental block. As the Pacific grew in size during earth expansion, the Great Magnetic Bight of the Northeast Pacific was formed from spreading source areas near the present Aleutian Arc and the west coast of North America. The Gulf of Alaska has been opening since Early Tertiary as a result of rifting of the North American continent, with the spreading source remaining near the eastern side of the Gulf. The Aleutian Trench is proposed as a relatively young feature formed by tension rather than underthrusting. It is proposed that land areas surrounding the northeast Pacific underwent simultaneous and equivalent east-west expansion, with extensive mountain building and volcanic activity."

Walzer in a number of papers (1972, 1973a, 1973b, 1973c, 1974a, 1974b) is primarily concerned with the fluid dynamics of mantle convection, starting from the empiricisms of gross separation of continents and the approximate equality of continental and oceanic heat flux. His models assume Newtonian rheology.

He achieves episodic magmatism and orogenesis through continuous convection in the upper mantle and episodic convection in the lower mantle by virtue of the temperature dependence of viscosity: radioactive heating gradually decreases the viscosity until the critical Raleigh number is reached, convective overturn occurs but thereby drops the Raleigh number below threshold. His model yields linear roll currents rather than centrifugal cells and he derives a theoretical pattern of topographic harmonics in close quantitative agreement with empiricism.

Walzer concluded that the ocean spreading theory and the kinematic convection theory are compatible with each other in their main aspects and that they complement each other. However he goes on (1973c, p. 707) to list six contradictions in the plate theory, and points out that these objections refer almost exclusively to the subduction part of the concept. Accordingly, Walzer "considers it possible that the conception of the expansion of the globe, as introduced by Hilgenberg [1933], is correct in its fundamentals: If 23 palaeogeographic Eurasia maps of Sinitsyn [1962] are measured by planimeters, the same main result will be obtained as Egyed [1957] obtained from the world maps of the Termiers and Strahkov: From the Cambrian up to the present the area of the epicontinental seas has considerably decreased. When, in the first approximation, the continents may be treated as rigid spherical shells, and the volume of water in the water circuit has not diminished, then this is proof of the expansion. Moreover, up to now there is no acceptable suggestion for the explanation of the fact that the hypsometric curve has two distinctly separated maxima". Walzer contemplated a combination of expansion, with orogenic shortening, because the observed rate of growth of new crust exceeded the rate of circumference growth derived from Egyed's method. However, if juvenile accretion of water is incorporated Egyed's rates would have to be increased.

Owen (1973) found that the combination of the accepted ocean-floor spreading patterns of the last hundred million years with constant earth diameter leads to a series of major topological contradictions. Specifically, the Arctic, Caribbean, Pacific, and Antarctic regions, each independently but concurrently would have been grossly shrinking since the Palaeozoic, whereas in fact each has been greatly extending. In his conclusion, Owen was convinced that expansion of the earth has occurred. In his 1976 detailed study, Owen deduced that the Rhaetic earth radius was 80% of the present radius. However his Rhaetic Pacific Ocean was still 150 Rhaetic degrees wide, that is 120° modern distance. I think that very little of the present Pacific floor is as old as Rhaetic, which accounts for my significantly higher estimate of the post-Palaeozoic expansion.

Meanwhile the possibility of earth expansion had evolved among theoreticians, astronomers, and cosmogonists. Dirac (1937, 1938, 1974) deduced secular decline in the gravitational "constant" from his "large numbers" philosophy, which would in turn imply secular expansion of the earth. Dicke (1957, 1964, 1966) and Brans and Dicke (1961) also deduced diminishing G (and earth expansion) from consideration of the consequences of Mach's principle. Jordan (1966, 1969) applied the Brans and Dicke conclusion in considerable detail to earth expansion. Kropotkin and Trapeznikov (1963) proposed short-term fluctuation of G, annually and irregularly, and on longer pulsatory cycles up to the age of the earth. They invoke gravity variation as the prime mover in geotectonics primarily because gravity offers their only route to the implied rate of tectonic working (10^{15} megawatts). Hoyle and Narlikar (1971) recognised that the number of anomalous redshifts observed in nebular clusters had increased far beyond the probability that they could be explained by the chance occurrence of objects in the same line from Earth but at very different distances, and offered a possible explanation in the form of a particular modification of the cosmological models of Friedmann, Einstein and de Sitter, which involved secular decline in G. Hoyle and Narlikar recognised that this would imply earth expansion, and point out that the mounting evidence for large scale relative movements of continents implied exceeding large forces. That being so, they suggested that some such behaviour of G as is given by their model becomes essential for an understanding of the geophysical evidence.

Van Flandern (1974, 1975) found that the Moon's acceleration, determined by checking the times of lunar occultation of stars against atomic clocks, significantly exceeded possible tidal effects, and required either that the sun's mass was decreasing at an improbable rate or that the gravitational constant was diminishing.

The several approaches to the variation of the gravitational constant converge as a rate of change of G of the order of 10^{-10} per year. This is of the same order as the upper limit set by Shapiro, Smith, Ash, Ingalls and Pettengill (1971) from a study of the orbit of Mercury.

NECESSITY FOR EXPANSION

Nearly a quarter of a century ago, during the dark ages of global tectonics, Lester King wrote a paper which he titled "Necessity for Continental Drift" (King 1953). The equilibrium has now flipped, his plea has been granted, but the new establishment poses a new necessity - expansion.

Gaping gores

At an early stage in my investigations I went to some pains to ensure that shapes and sizes of the continental blocks were compared and transferred accurately. Tedious years were spent plotting large oblique stereographic projections about diverse centres, not only for Africa and South America (Fig. 3) but for every piece of the earth's surface. These were compared with spherical tracings (Fig. 4) from the globe, working on a spherical table. The reward for this zeal for accuracy was frustration. Again and again over the years I assembled Pangaea but could never attain a whole Pangaea without gaping gores ehich I knew to be false artifacts. I could make satisfactory *sketches* like Wegener's classic assembly (Fig. 1), but never accurately on the globe, or a rigorous projection. I could reconstruct satisfactorily any sector but never the whole. Starting from the assembly of Africa and South America, as in figure 3, a yawning gulf appeared between Indonesia and Australia (as in Fig. 14), although the oroclines indicated that Indonesia and Australia belonged together (as in Figs. 179 & 193). Starting from Australia and Indonesia there was no hope of closing the Arctic Sphenochasm as in figure 195, which appeared to be basically correct. A crucial link seemed to be missing from the global synthesis. I was tempted to abandon the quantitative assembly and resort to sketches which would show every block related, as I inferred they should be, even though I knew I could not bring them together that way with the rigour I sought. Even on a more limited arc, such as the South Atlantic, a small discrepancy emerged. In making the comparison in figure 3, although the fit was quite close enough to prove Jeffreys wrong, I was conscious of a second-order problem, in that it was necessary to bring the angle of Brazil tightly into the Gulf of Guinea, and even then the fit was more open at the extremities of the arcs. This was rationalized by assuming that the shelf slopes were steeper at the angle than towards the extremities, and that a somewhat deeper isobath (2.5 km instead of 2) would yield a more accurate fit. However it became clear later that this small discrepancy was due to earth expansion, as Owen (1973) has recognized.

But in the end the rigorous approach with accurate projection paid off. For it revealed a discrepancy, which had not been apparent, and could not be apparent in an approximation. It was not my method that was at fault, but my implicit assumption that the earth of Pangaea was the same size as the earth of today. The assembly of Pangaea was not possible on an earth of the present radius, but on a smaller globe, a globe such as is demanded by the orocline analysis, these difficulties vanished.

How then had Wegener managed to assemble Pangaea? A careful check disclosed that his Pangaea would not fit on the present earth either. π is the ratio of the circumference of a circle to its diameter when the circle is drawn on a plane. When a circle is drawn on a sphere "π" diminishes from its standard value 3.14159.... when the sphere has infinite radius, to 2.0 for a hemispheric surface, then drops to zero as the residual segment of

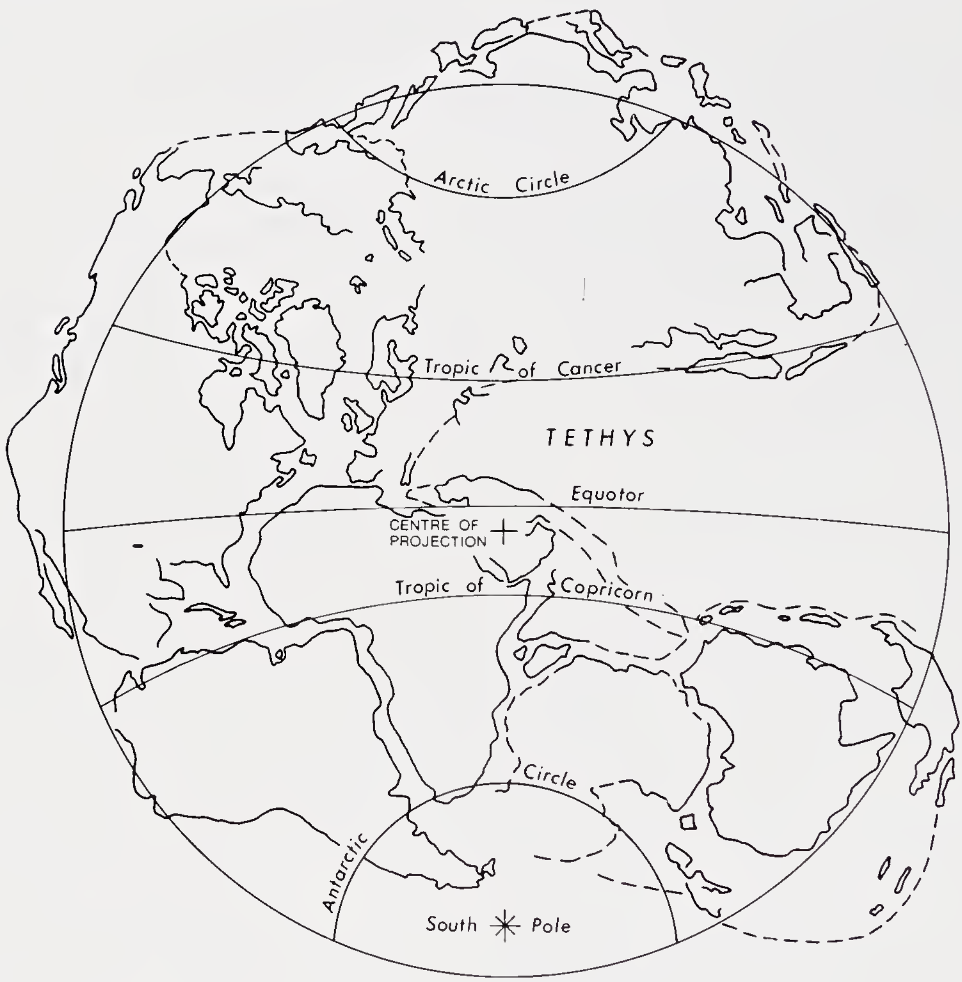
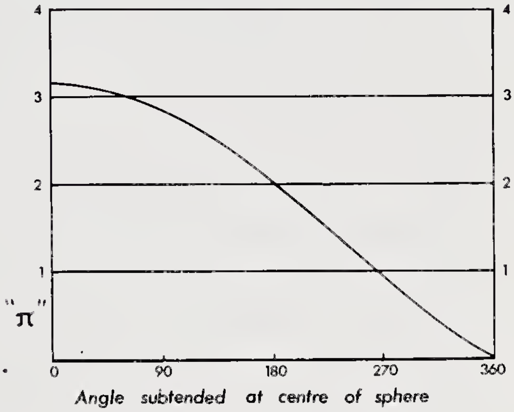


Fig. 14 Draft assembly of Pangaea submitted at the Hobart Continental Drift Symposium in 1956, to show the unacceptable result of making the assembly on an earth of present size (stereographic projection).

the sphere approaches zero (Fig. 15). Measurements across Wegener's Pangaea give a value of "π" of less than 1.9 subtending an angle of less than 160° at the centre, which is impossible. To be viable his Pangaea has to be placed on a smaller earth where it covers more than a hemisphere when "π" can be less than 2. When we close the open gores in his Pangaea between Australia and IndoChina, and in the Caribbean and the Arctic, the implied diameter of the earth diminishes to the value I have found by other routes.

Fig. 15

Change in the value of "π" (ratio of circumference to diameter of a circular segment of a sphere) as the angle subtended at the centre of the sphere increases from zero to 360°. On a plane surface the radius of the sphere is infinite, so the angle subtended at the centre is zero, and "π" reaches its conventional value π (3.14159...).



Everybody who has ever assembled Pangaea has left gapes. Bullard's computer fit across the Atlantic (Fig. 16) has already accumulated a highly improbable gape between Turkey and Arabia, and extension further towards Indonesia and Australia becomes absurd (see p. 436).

Fig. 16

The "Bullard Fit" across the Atlantic.

Hierarchy of extension

The earth's crust is made up of eight first-order polygons, each a few thousand km across, which meet each other in active tectonic zones (Fig. 6). These polygons enclose the continental nuclei of Africa, South America, India, Australia, Antarctica, North America, Europe and Siberia respectively. In addition the EoPacific polygon is oceanic, with a long history preceding that of the other oceans. The boundaries of the polygons have different ages around their perimeters. The oldest is the boundary of the EoPacific, which separated the *Urkontinent*, Pangaea, from the *Urozean*, the EoPacific, and which now corresponds roughly to the andesite line. Next came the Tethys which divided Pangaea into Laurasia and Gondwanaland. Most recently came the post-Palaeozoic mid-oceanic rift system which fragmented the primary continents. The present polygons are bounded by segments of each of these types of boundaries. I have included the Greenland block in the North American polygon and Arabia and Madagascar in the African polygon even though in each case there is already substantial spreading, but it would be quite reasonable to regard all of these as separate polygons. Likewise the Eurasian block is composite.

Each of these primary polygons in turn is made up of second-order polygons a few hundred km across separated by horst and rift zones (Figs. 17-20). In view of their dimensions the first-order polygons involve the whole mantle and the second-order polygons only the upper mantle and crust above the asthenosphere.

Fig. 17

Second-order polygons of Africa. (After Holmes 1945).

The hierarchy of polygons is no accident. Given a body consisting of a solid shell 3000 km in thickness enclosing expanding fluid 3000 km in radius, the primary fracturing pattern would inevitably be of the sort found in the first-order polygons. As gravity dominates the curvature, these major segments could not persist with increasing radius. But given a rigid and brittle crust a few hundred kilometres thick, below which the mantle behaves as a fluid on tectonic time-scales, the second order accommodation necessarily produces polygons a few hundred kilometres across. As new ocean floors increase in area, and decrease in viscosity through cooling, they too need such second order adjustment on a similar scale to the decreasing curvature of the crust (Fig. 19).

The second-order polygons, in turn, are broken into third-order polygons some tens of kilometres across (see figs. 20-21), which Japanese workers have found to jostle independently during earthquakes (Miyabe 1931 and 1935). Miyabe found that observed secular strains required the assumption of a series of independent blocks. ERTS imagery from satellites shows such polygons ranging down to the lower limit of resolution, and so to master joints, and the ordinary joint systems seen in outcrops of all rocks which have suffered epeirogeny (but not folding), i.e. two sets of joints, nearly at right angles with their intersections nearly vertical.

Billingsley and Locke (1939) found that the most important ore-bodies clustered on through-going fracture zones on the scale of the second-order polygons which they regarded as regional phases of the crustal movements of the continents. They concluded that these fundamental fractures penetrated the whole lithosphere and suspected that they expressed the adaptation of the earth's rigid crust to the equatorial bulge of the spinning globe.

Fig. 18

Second-order polygons of Western Australia. (After Glikson and Lambert 1972).

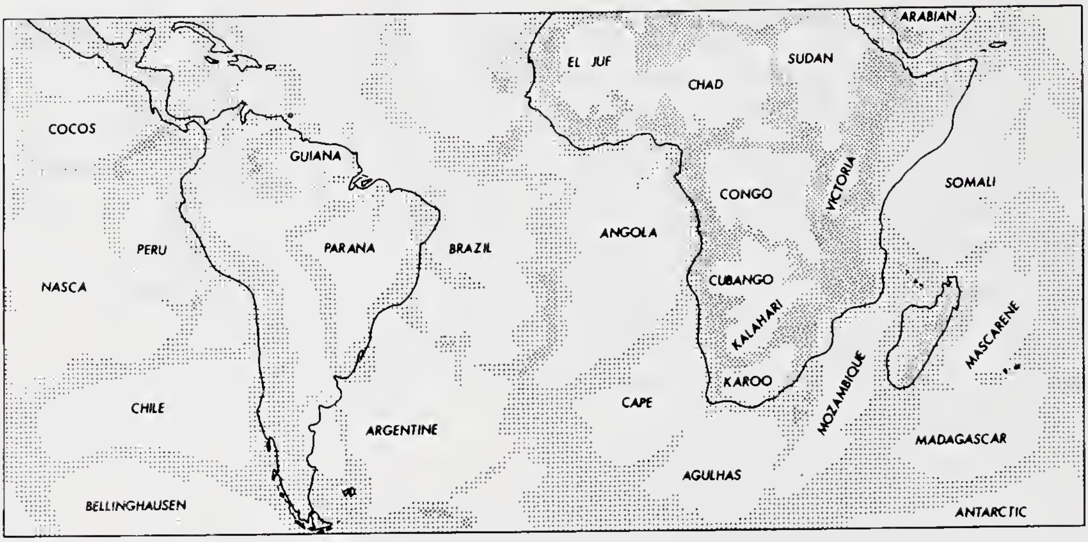


Fig. 19 Second-order polygon pattern extends through oceanic as well as continental crust.

Figure 22 is a vertical aerial photograph of part of the Tasmanian central plateau east of the Great Lake. The light tesselations are bare rock between sixth order master joints twenty to forty metres apart which carry low scrub. The larger polygons are fifth order tension fractures a few hundred metres apart. Where the hydroelectric aqueduct tunnel passed below these, zones of shattering and weathering were met. Each fifth order polygon tends to have consistent directions for the sixth order joint sets but adjacent polygons often have quite different orientation - like magnetic domains. Within the smallest tesselations visible in this picture, ground study shows the normal epeirogenic joint systems of the seventh order (see Carey, 1958b).

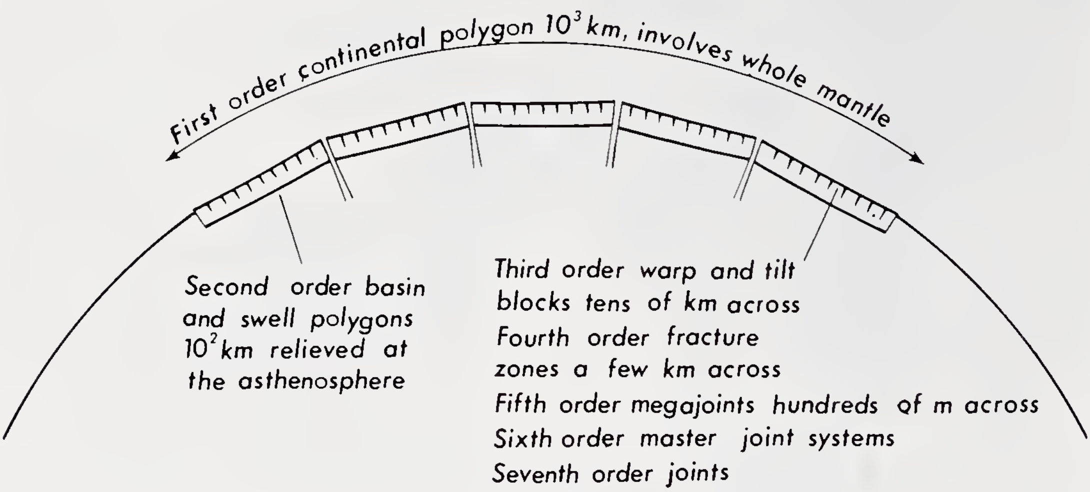


Fig. 20 Hierarchy of extensional polygons on the expanding earth.

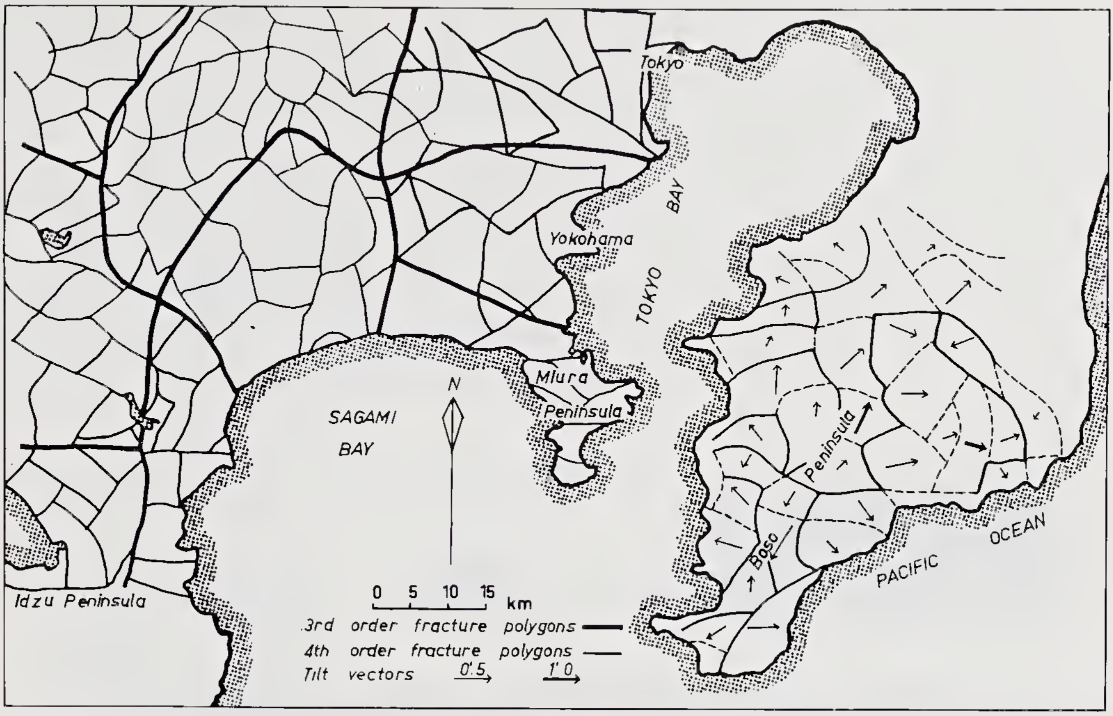


Fig. 21 Tilt blocks in central Honshu (after Miyabe 1931). These are the third- and fourth-order polygons in the hierarchy of crustal extension.

Fig. 22 Vertical aerial photograph of part of the central plateau of Tasmania, showing 5th order polygons some 500 m across and megajoints some 50 m apart. Ordinary field joints are below the resolution of this photograph.

Everywhere in the world where we find non-folded rocks which have suffered epeirogeny, they show two sets of joints nearly at right angles with their intersections nearly vertical (Fig. 23). Sometimes one of the sets may dominate. Sometimes there is a weaker third diagonal set. Such joints have commonly been interpreted as conjugate shears. But they belong to the tensional region of the upper crust and are the normal expression of tension failure where tensional stress is not symmetrical in the horizontal plane. They are modelled well by the fracture pattern in those parts of an automobile windscreen where tensile stress in the plane of the glass is unequal.

Fig. 23 Epeirogenic jointing in Permian siltstones. The Tesselated Pavement, Tasman Peninsula, Tasmania. (From Johnston 1888).

Brock (1956, 1957, 1972), Carr (1966) and Rickard (1967) have emphasized the polygonal pattern which dominates the surface of the globe. Rickard made histograms of the lengths and angles of the polygon boundaries: "The preponderance of near 120° intersections suggests that, as in the case of polygonal systems in other materials - e.g. mud, permafrost, and basalts, etc. - these polygons were formed in a tensional stress field." Brock (1972) has argued that the polygonal hierarchy, continental and oceanic, contradicts continental *drift*. This may be so, but on the other hand it is wholly consistent with continental *dispersion*.

Dispersion of polygons

When these first-order polygons are studied critically, each of them has increased greatly in area during the Tertiary and during the Mesozoic (Fig. 24). This is true irrespective of how much or how little crustal swallowing may have occurred at the trenches. This can only mean that if the crustal spreading at the rift zones is valid, then the total surface area of the earth has increased greatly since the Palaeozoic (Fig. 25). Let us study this in more detail. The areas of new crust added to the polygons are set out in Table II. The total increase in area for the surface of the earth (219 million sq. km) represents a 76% expansion of the earth's surface since the Palaeozoic or 33% increase in radius.

Fig. 24 Each primary polygon has increased in area since the Palaeozoic.

Every polygon has increased its distance from every other polygon on the surface of the earth. Let us adopt a central point on each polygon and measure the increase in distance between them by the major and minor arcs. The results in kilometres are set out in Table III.

Fig. 25

The black areas on these balls are identical. Each pentagon has increased by the same proportion as the mean increase of the earth's polygons (Table II).

Each of the great circles measured has increased but not necessarily by even approximately similar amounts. For example, if a crust expanded by opening on two sets of petal-like gores, it would be possible to take a great circle almost entirely along the mid-lines of gores which would show minimal new crust, and another along the gaps between the gores which showed anomalously high increase. But this effect is averaged out over twenty-eight great circles measured. The mean increase in circumference is 22,000 km. If you stand on any polygon, it has moved away from every other polygon, and if you face about, the distance to each polygon has also increased the long way round. The same applies also to the smaller blocks of the second-order polygons.

If I stand on Madagascar and look west, I see that Africa, which was nearby in the early Mesozoic, has moved away. If I face north-east, India, a Mesozoic neighbour, has receded more than 3000 km. If I face east, Australia has receded 5000 km. If I face south, Antarctica has receded nearly 4000 km. Even the small blocks, such as Agulhas, Kerguelen, the Mascarenes, Seychelles, Chagos, and Maldives, all formerly near neighbours, have receded across the horizon. Similarly, Antarctica formerly had near neighbours in Australia, New Zealand, South America, Africa, Madagascar, and India, which have boxed the compass in their flight through thousands of km. Africa was formerly in close contact with South America, North America, Europe, Anatolia, Arabia, India, Australia, Antarctica, and Madagascar, but all have moved apart. Greenland was much closer to Canada, but also to Scandinavia, and Siberia. This is the universal pattern, which can only mean an expanding earth. In an expanding universe, every random point sees all other points in recession from it. On an expanding closed surface the same pattern of universal dispersion appears in two dimensions.

The mean increase in distance of the polygon from all other polygons is set out in Table IV. This indicates a progressive increase in dispersion

TABLE II

Area of Growth of Polygons

(in millions of sq. km)

	Early Palaeozoic area	*Present area*	*Increase*	*Growth per cent*
Antarctica	15.2	62	47.2	311
Australia	21.1	80.6	59.5	283
India	7.1	21.6	14.5	203
S.America	25.5	65.6	40.2	158
Africa	47.4	99.4	51.9	109
Eurasia	61.2	122.4	61.2	100
N.America	32.5	57.5	25.0	77

As these boundaries involve interpretation, the numbers in tables II, III, and IV may interchange by several per cent, but the gross patterns remain valid.

TABLE IV

Mean dispersion of each polygon from all others along minor arc

(in hundreds of km)

Antarctica	55
Australia	55
South America	51
North America	37
Africa	33
India	30
Siberia	27
Europe	26

TABLE III

Dispersal of Polygons

(in hundreds of km)

	Minor arc	*Major arc*	*Great circle*
From Australia			
Africa	66	175	241
South America	66	131	197
North America	81	166	247
India	62	176	238
Antarctica	30	194	224
Europe	62	171	233
East Asia	36	148	185
From Africa			
South America	51	152	203
North America	45	220	265
India	16	221	237
Antarctica	38	188	226
Europe	11	183	194
East Asia	2	230	232
From South America			
North America	16	186	202
India	41	194	235
Antarctica	44	161	205
Europe	55	190	245
East Asia	85	113	198
From North America			
India	17	178	195
Antarctica	97	105	202
Europe	4	281	285
East Asia	2	181	183
From India			
Antarctica	13	192	175
Europe	1	206	207
East Asia	0	281	281
From Antarctica			
Europe	42	158	200
East Asia	62	138	200
From Europe			
East Asia	4	216	220

rate from a point near 66N 140E to a point near 66S 40W. These points correspond approximately to the poles of the Middle Mesozoic. This is what would be expected from the palaeomagnetic record from each polygon, which all show a progressive movement towards the north pole since the Jurassic, as Heezen pointed out long ago.

The conclusion that the oceanic areas have increased is consistent with Egyed's observation that each polygon shows progressively less marine transgression through geological time. An earth with continental crust over the whole surface would be entirely covered by some 2 km of water except where high mountains protruded. But any area of oceanic crust accepts more than twice the water volume of an equal area of continent. The implication is that the earth's surface has increased with time by progressive increase in the area of the ocean basins and therefore of the whole surface. Egyed's argument is elegantly simple and the necessary corrections (such as volcanic water accretion through time) only make it stronger. Moreover swallowing of continental crust with the mantle would not affect the verdict because in this analysis each fragment of continental crust potentially records the history of the whole.

The Pacific paradox

The Pacific Ocean is roughly circular, and less than a hemisphere in area. A common factor of all versions of continental drift and plate tectonics is that the Arctic, North Atlantic, South Atlantic, and Indian Oceans have all been created since the post-Palaeozoic disruption of Pangaea. Hence, irrespective of whatever subduction may be postulated within the Pacific, either the area of the Pacific within its bordering orogenic girdle must have been reduced by the combined area of these new oceans or the surface area of the earth must have expanded by this amount. Yet North America is now further from South America in the direction of the Pacific perimeter. South America is now further from Antarctica in the direction of the Pacific perimeter. Antarctica is further from Australia in the direction of the Pacific perimeter (Fig. 185). Australia is further from Asia in the direction of the Pacific perimeter (Fig. 185), and Siberia is further from Alaska. It is clearly impossible for the Pacific to reduce in area to about half, while its perimeter is increased by about half. The alleged subduction round the Pacific perimeter to swallow an area equal to the sum of the Arctic, Atlantic and Indian Oceans is obviously false. The Pacific subduction zones like all other subduction zones are myths.

I quote from my 1956 continental drift symposium (p.312): "The circumference of the Pacific Ocean is roughly a circle, a little smaller than a great circle. Proceeding round the circumference, the orocline analysis indicates a stretching of 19° of great circle arc between North and South America, stretching of 22° between South America and Antarctica, of 35½° between Antarctica and Australia, and of 23½° between Australia and Asia." Thus the circumference of the Pacific has increased by 100° from 240° to 350° or roughly 42%. This represents an increase of area by about 101%, or about double. These figures might be uncertain by 5% or perhaps more. The wholly unexpected, but quite rational result is that the Pacific segment has expanded since the Palaeozoic in approximately the same proportion as the Pangaean segment (which we found to be 44% linearly). Each has about doubled its area since the Palaeozoic.

Meservey (1969) repeated this argument and emphasised the topological impossibility of generating the present distribution of the continents from any of the early Mesozoic models derived or adopted by the plate tectonicists without gross expansion of the earth. Meservey adds:

"The topological argument given above is independent of possible downwelling of the ocean floor. It is also apparently independent of shrinkage of the continents associated with mountain formation since the important mountain regions of the perimeter could be included without significant increase in the perimeter. The argument is not very sensitive to the exact time scale or to variations in the rate of ocean-floor spreading, as long as these were reasonably monotonic in the period in question, and could be adapted to most proposed reconstructions of a single supercontinent. The only hypothesis that has been suggested thus far that resolves the paradox described above is that in the past the earth's interior has expanded considerably."

No escape is possible by appeal to stretching of the perimeter as it passed over the great circle from the Pangaean periphery on the other side of the earth. The present Pacific is only 35% of the earth's surface, so the latest changes must have been a 43% reduction in area, whereas the data leave no doubt that the perimeter has expanded grossly during the relevant time.

Examination of the Pacific radially repeats this conclusion from the elongation of its circumference. If, as the fashionable hypothesis requires, all the bordering lands of the Pacific have converged on it centripetally through thousands of km since the Palaeozoic, one would anticipate that the Pacific border regions would be overwhelmingly foreshortened radially. But the reverse is true. All agree that the Pacific margin between Australia and Samoa has stretched by some three thousand km since the Jurassic. All agree that along the Pacific margin of Asia (the Sea of Okhotsk, the Sea of Japan, the Yellow Sea, and East China Sea, the South China Sea, the Philippines Sea, the Celebes Sea, the Banda Sea, and the Caroline Basin, are all new extensions of the Pacific in the direction which should be foreshortened by many thousands of kilometres. In the Andes, Katz (1971) has shown that contrary to the expectation of subduction compression, the Pacific segment of South America has experienced extension radial to the Pacific, since the Miocene at least. Thompson (1960) has shown that Nevada has been extended and is currently extending radial to the Pacific. The ocean trenches which border the Pacific, superficially wear all the vestments of tensional rifts -- horsts and grabens, non-disturbed sediments, shallow dilatational earthquakes -- and a close scrutiny wholly confirms this preliminary examination.

The Arctic paradox

Every kind of study that has been made - palaeoclimatic, palaeomagnetic, tectonic, agrees that Australia, South America, Africa, India, Europe, North America, Greenland, are all in more northerly latitudes than they were in the Upper Palaeozoic or Mesozoic. This means that on an earth of constant radius, all these continents have converged on the Arctic. But the Arctic is also disjunctive, and no-one has claimed post-Palaeozoic subduction there. This is impossible unless the earth has expanded. This matter is elaborated later in the chapter on palaeomagnetism.

Owen (1973) reached the same conclusion a little differently: "In effect, on a modern diameter globe Pangaea reconstruction, the North American continental plate has to be rotated to a position some 10° further south of its current position. Unfortunately, this has the effect of expanding the area of the Arctic Ocean when proceeding backwards in time into the Late Mesozoic. This apparent opening out of the Arctic Ocean and the continental areas surrounding it is seen in the present authors' maps and in the other excellent map reconstruction drawn by Dr. P.L. Robinson (1971; text-figs. 5, 6) for the Permian and Triassic. There is no other remedy but to open out the Arctic Ocean if a modern diameter globe is used and thus a modern surface curvature for the Earth. However, in fact, the Arctic Ocean is expanding today as a result of sea-floor spreading at the mid-Atlantic ridge, and the bathymetry and movement of Greenland since the Late Cretaceous indicate that this process of expansion has been going on since the Late Mesozoic. The history of the development of part of the Arctic Ocean and its environs given by Harland (1969; 817-851) is apparently correct, but this reconstruction implies expansion and increased oceanic crustal area and not contraction and subvection of oceanic crust in this area from Late Cretaceous to modern times."

The young oceans

Palaeomagnetic surveys and deep-sea drilling has reached the stage where it is known that all the floors of all the oceans and seas are young. None has been found older than the Mesozoic and it has already become improbable that any will be found anywhere. The plate theorists claim that all the older oceans have been swallowed. With the changing patterns manifest in every other mode of tectonic behaviour, it is incredible that no sizeable block of old ocean crust would be left anywhere. On the expansion model, by contrast, the present distribution is wholly rational.

THE SUBDUCTION MYTH

Plate tectonics and expansion schools agree in respect to sea-floor spreading. They differ mainly in the interpretation of the trenches. This issue then is the crux of the debate.

Two false axioms

For thirty years after the 1928 AAPG symposium on the Wegener hypothesis, continental displacement was rejected in most places, but particularly in North America. When finally the rising tide of evidence established the growth of the oceans as widening rifts, this truth was wedded to two false axioms, which are quite independent of the ocean-floor spreading concept. First, that the earth had maintained approximately its present diameter, and second, that orogenesis and ocean trenches were compressional phenomena. These were adopted as self-evident "facts". Compressional orogenesis is essentially an English-language obsession. The Russians and many European geologists have long recognized that orogenesis is a diapiric, gravity-driven process, in a dilative environment, in which the upper part of the rising tumour spreads laterally under its weight, exactly as Ramberg's carefully scaled centrifuged models reproduce (1963, 1967). Nauss (1971) reminds us that this is simply Pascal's principle.

The compressional interpretation of the trenches began as pure supposition, presumably because of their seismicity and the prevailing acceptance of the contraction theory. Sea-floor spreading, combined with the "axiom" that the diameter of the earth was virtually constant, implied that crust be consumed at a rate equal to its generation. As new oceans are more than half the present surface of the earth, more than two hundred million sq. km of earth surface have to vanish since the Palaeozoic, and this elimination has to be guided, so that *all* Palaeozoic ocean floor is excised, because so far no one has found any ocean floor older than Mesozoic. The trenches were assigned this role, not because they showed any evidence of grand foreshortening, but because there were no other alternative sites to eliminate vast areas of crust. The trenches had to accept this role if the plate model were to be sustained.

Fairbridge (1964) wrote "There is now no reason to view the island arc-trench a compressional phenomenon at all. Could it not be a tensional feature? Many of the trenches are known to be faults with a strikeslip element (Hodgson, 1962) and the morphology of the least sedimented troughs suggests a graben-like character. The present oceanic trenches may thus be readily viewed as the modern equivalents of eugeosynclines of the past which often coincided with major geofractures".

Hatherton (1971) stated that "the special problem of the trench is the conflict between the theoretically compressional nature of plate boundaries and the apparently tensional nature of the trench itself. Indeed why the collision of two "rigid" plates should produce a trench is so far unexplained. Such earthquakes as are observed beneath trenches have a fault-plane solution best satisfied by normal faulting (Stauder, 1968; Kanamori, 1971). A sub-oceanic lithosphere must thus be fracturing and sinking through excess mass rather than by thrusting against its continental neighbour - but then the cause of seismicity and shortening in the continental-side crust is unknown."

Tanner (1973) has emphasized that the numerous writers on the trenches during the last decade have started with the "fact" that trenches were compressional structures on Benioff overthrust surfaces, and have interpreted their data within this framework, even though their own data were more amenable to a tensional environment. Tanner concludes:

"1. Compression is of little significance in creating and maintaining the major structures that underlie trenches, island arcs, and adjacent basins; instead, these features, in a strip up to 1,700 km wide, are caused by primary regional tension.

2. There is no 'down-going slab', whether driven by pushing from the rear or pulling from a sinking front edge.

3. The only important motions to be accepted for island-arc and trench areas are horizontal tension and strike-slip.

4. Many authors conclude that the 'down-going' concept is correct, in order to maintain the hypothesis, but this conclusion generally is contrary to their own data and should not be accepted."

Sychev (1973, p. 334) reached similar conclusions: "Unfortunately the objective examination of the data obtained and their interpretation is nowadays more and more often substituted for by a preconceived theory" . . . "The available data on island arcs and trench systems are in poor agreement with the concept of 'new global tectonics'."

Likewise Wilson (1974), in his review of Cretaceous sedimentation and orogeny in nuclear Central America, concluded that "the plate-tectonics advocates have produced a concept based upon well documented expansion criteria and complemented by a hypothetical subduction process".

Scholl and Marlow (1974b) find that "quite clearly the understanding we have of the tectonic setting of trenches comes more from concepts of global plate motions than from ideas generated by data gathered in the studies of trenches themselves". Concluding a review of the objective facts about the trenches, the same authors remark: "We remain genuinely perplexed as to why evidence for subduction or off-scraping of trench deposits is not glaringly apparent." In response to the suggestion that the evidence sought has itself vanished down the subduction zone, they add dryly: "You can't have your trench and eat it too!"

Brown (1974) asks: "What is the essential difference between a statement beginning 'Because the continents drifted 2.5 cm per year . . .'and one that might have begun 'Because the flood of Noah drifted the icebergs . . .'? I fail to see any difference; both are deductive arguments, arguing the specific from the general. Both arguments begin with conceptual principles. One could challenge this statement by remarking that there is "scientific" data in support of the first principle but not the second. But one could equally argue that within the *framework of the respective principles involved* there is as much support for one as the other; fundamentalists certainly find that to be the case."

Krebs (1975) has emphasized that "foredeeps and deep-sea trenches, crystalline belts and volcanic arcs, and intermediate furrows and interarc basins, respectively, are equivalent structures . . . which represent the top of diapir-like upwelling material from the asthenosphere. These subcrustal asthenoliths are characterized by crustal thinning, extension, inversion structures, high mean heat flow, deep earthquakes, positive gravity anomalies, extrusion of mantle-derived tholeiite basalts, and intrusion of ultra-mafic massifs. The forces caused by the rising asthenoliths are primarily vertical; horizontal stresses of secondary origin are gravity controlled . . . Global vertical tectonics explains the evolution of island arc-trench systems and mountain belts in a much simpler way than the hypothesis of plate tectonics."

In its fundaments, the case for subduction reduces to the faith that it is so.

Trenches are not where they should be

According to the currently fashionable hypothesis, new crust is generated continuously at the mid-oceanic rifts and the existing crust moves away from the rift to allow the new addition. At the same time equivalent crust is thrust down into the mantle in the trenches. If this were true, rifts and trenches would necessarily be complementary. But the trenches do not correlate with the rifts. On the contrary the trenches are a phenomenon of the Pacific margin, which includes the Caribbean and the Scotia arcs (as the Americas are moved back against the African template) and the Sunda arc (when Australia is fitted back against India). The trenches bow eastward (or less commonly equatorward), implying a rotation helm. Some of the most actively expanding rift zones have no corresponding trenches whatsoever. Take, for example, the African polygon. Africa is surrounded by its rift zone, shaped like an inflated caricature of Africa. The African polygon is more than twice the area of Palaeozoic Africa (Fig. 6). On all sides there is new crust, youngest at the rift, ageing from Quaternary through the Tertiary and Cretaceous and probably Jurassic towards the African coast. Somewhere within Africa the theory demands a sink which has swallowed an area of crust greater than the whole of Africa. Where is it? Such just does not exist! On the contrary, between the South Atlantic and Indian Ocean ridges is the great rift valley system. Africa is stretching latitudinally when the theory requires that the arc be shortening by nearly 3000 km.

Plate advocates have purported to escape this dilemma by claiming that African plate alone stayed put, while all the other plates and all the oceanic rifts moved away from it. But what about Antarctica? Its encompassing rift surrounds it on all sides enclosing more than twice the area of Antarctica, again Quaternary at the rift, giving place in turn to the successive epochs of the Tertiary, with still a wide zone to have been formed during the Mesozoic (Fig. 26). Somewhere within Antarctica the theory demands a central sink, which has swallowed an area equal to the area of Antarctica. Where is it? It does not exist. Escape may have been possible for Africa alone, but not for Africa and Antarctica. The plate theory is false.

The concept of a conveyor-belt spreading ridge and subduction sink is comprehensible in profile section. But the notion of a spreading ridge crabbing obliquely down its own subduction sink under California, while continuing to excrete towards the Pacific but swallow from the Atlantic; is as hallucinatory as a maw vanishing into its own anus, or really the reverse, if that is any easier to conceive.

The African subduction paradox, the Arctic convergence paradox, the Pacific perimeter paradox, and the anal-maw paradox, all contain the same divergent impossibility. In each case the divergence crystallises reality on an expanding earth surface, by the elimination of the mythical subduction.

Which side moves?

Seismically the arc side is vigorously active, while the Pacific side enjoys seismic peace. Continuous seismic profiles show regular sediments, monotonously undisturbed for thousands of kilometres on the Pacific side. But as soon as the trench is crossed tectonic violence of all kinds erupts. Heat flux on the Pacific side is consistently low; as soon as the trench is crossed heat flux more than doubles and even increases locally up to tenfold, and continues at double the normal rate right across the disjunctive basins. Surely it is the Pacific side that is pacific and static and the arc side beyond the trench that is mobile, orogenic, and active? Surely also the gross motion must be an *upward* tumour below the arc and disjunctive basin, bringing *up* the isotherms. The gross motion should not be down as claimed by the subduction model, which could only result in *reduced* total heat flux over the whole system of trench, arc, and disjunctive basin.

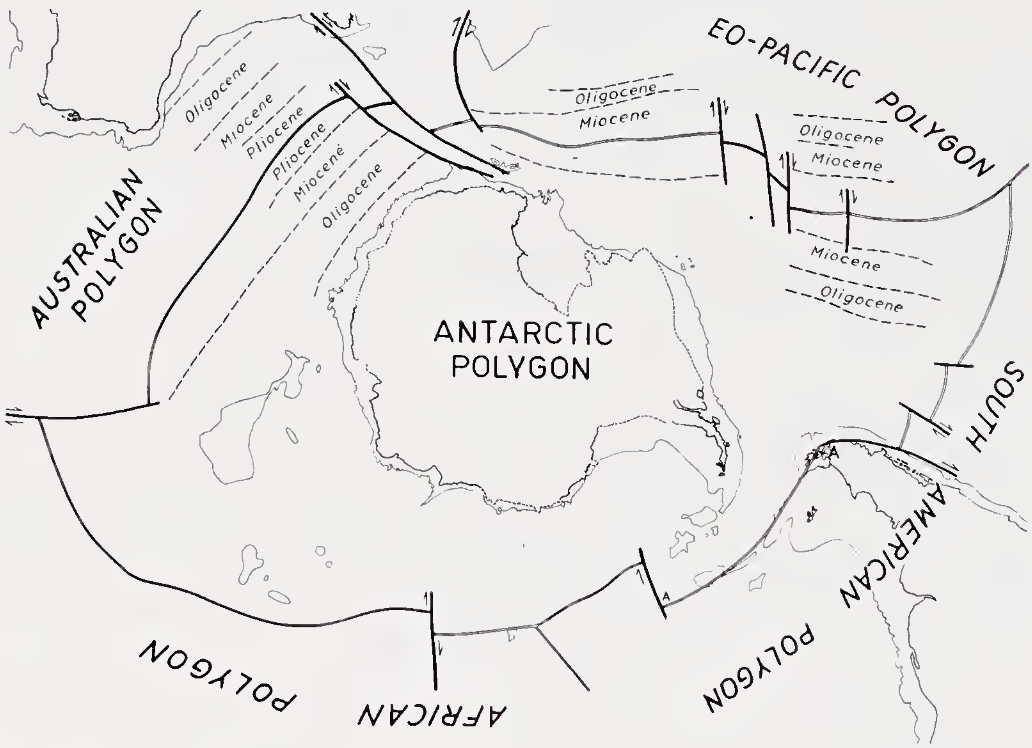


Fig. 26 The Antarctic polygon.

Trench Sediments are not deformed as they should be

According to the fashionable model the trenches are the outcrops of stupendous underthrusts with thousands of kilometres of relative movement. The underthrust plate bears a veneer of soft sediments a few hundred metres thick, intense crumpling of which would be inescapable. But this profound disturbance is not there. Some of the trenches are empty. Others show serenely slumbering sediments, quite undisturbed (e.g. von Huene and Shor 1969, Scholl *et al*, 1970). Others show horst-and-graben tensional movements, with the sediments borne passively on the dilating basement blocks (e.g. Ludwig *et al*, 1966). Occasionally there are the inevitable gravity slumps. Menard (1964, p. 103) has said: "Almost everyone who sees an echogram of the side benches and bottom troughs of trenches believes they are produced by normal faulting . . . the topography of the trenches suggests tension and supports the hypothesis that trenches owe their existence to tension rather than compression." This would be impossible if trenches were the outcrop of multi-thousand-kilometre underthrusts, such as the plate theory requires.

Scholl and Marlow (1974b) have observed that the "plate" tectonicists require that subduction of the Pacific "plate" has been proceeding frontally at several centimetres per year at the eastern end of the Aleutian trench south of the Alaska peninsula. Westwards along the trench the direction of the convergence becomes increasingly oblique until south of Attu island, the alleged motion is dextral straight along the trench, and at the extreme north-west end of the trench the plates should even be separating. Yet the transverse profiles remain much the same right along the trench, and in turn similar to those of the Chile-Peru trench, where rate of convergence is alleged to be much faster. Rather than appearing respectively strongly compressive and strongly transcurrent, the trenches everywhere appear as tensional rifts, which of course is their true nature in all cases. Walzer observed that similar anomalies have been raised by Andrusov (1968) and Tollmann (1966) with respect to the Carpathian arc.

The Aleutian paradox is deepened by the fact that the magnetic anomalies get older away from the trench as though it were a spreading zone (which of course it is!). Pitman and Hayes (1973) would escape this contradiction by postulating that a former spreading zone has coalesced with the trench by the swallowing of the whole of one flank, so that although this sequence approaching the trench from the south is indeed the sequence of approach to a spreading zone, it must not be interpreted as such.

Following initial amazement that some trenches were empty, many had kilometres of non-disturbed sediments, none had expected volumes of accumulated scrapings from the thousands of kilometres of underthrusting, and all looked like grabens, faith in subduction required that the zone of turmoil and chaos must be latent in the sediments of the landward slope. To fill this need a spate of papers (Beck, 1972, Figs. 13 and 14; Grow, 1973; Bunce, *et al.* 1974; Beck and Lehner, 1974, Fig. 20; Seely, *et al.* 1974; Beck, *et al.* 1975; and several oil-company restricted studies (which I have been permitted to see) interpreted ocean floor basement extending flatly back even tens of kilometres below the landward trench-slope, with underthrusting, faulting, contortion and chaos within the sediments. All of this work depends on interpretation of seismic reflection continuous profiles. Some of this interpretation is spurious, but I am satisfied some is valid.

Seismic reflection profiles look superficially so much like geological sections that there is an irresistable temptation to read them as such, although this they certainly are not. Quite apart from the large vertical exaggeration, and that apparent thickness is measured by travel times and not distances, and that the raw data have been processed in various ways, many (perhaps most) of the apparent interfaces are multiples, reflections, caustics, or hyperbolic diffraction traces. A specialised seismologist is needed to be sure that these seismograms are correctly read.

However when such artefacts are removed, I am satisfied that (a) basement can often be traced substantial distances below the toe of the landward trench-slope, and (b) nappes overfolds and overthrusts, all with vergence towards the trench, do commonly occur there. Indeed I would be astonished if that were not so. The landward slope must be characterised by extensive gravity nappes on the scale of the 1929 Grand Banks slide (Heezen and Drake 1963) and also glacier-like kinematics of the sediment pile, as in Beck, 1972, Fig. 2C. But, as the front of a regurgitating orogenic diapir, it should be expected to show structures similar in scale and complication to those of the Rocky Mtn. Front in Alberta or the Appalachian front in Tennessee (see Fig. 27). A drill-hole forty km behind the Rocky Mtn. Front passed through the Palaeozoic and Proterozoic sheet into flatlying autochthonous Mesozoic to basement. Ramberg's dynamically scaled models of orogenic diapirism show such decollements. Such overthrusts, adding up to tens of km of transport, should certainly be expected in the landward trench-slope, and the data, screened of spurious misinterpretations, are accumulating to confirm them, on the expected scale. But, - and this is a big but - there is nothing there to suggest the multi-thousand kilometre underthrusts the subduction models require.

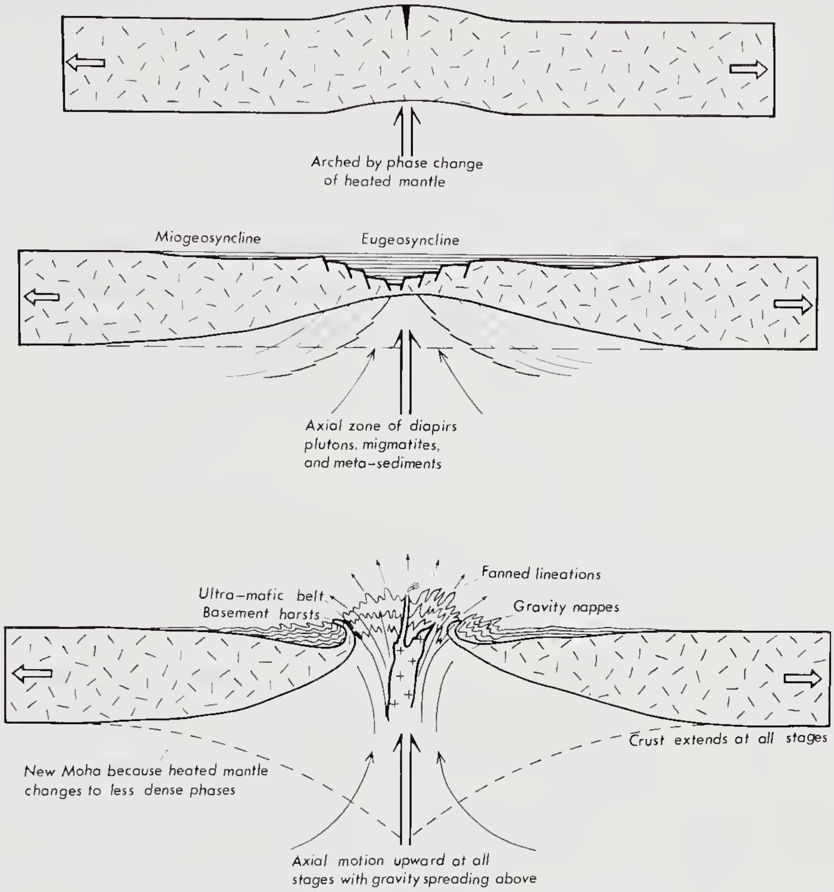


Fig. 27 Simplified symmetrical model of geosyncline and orogenesis in a dilating orogen. This pattern is commonly modified by crustal or rotational asymmetry.

Trench sediments do not go into orogens

Several plate disciples have tried to solve the paradox of the empty or non-deformed trenches by claiming that missing pelagic sediments now make up the deformed pile which outcrops in the orogens. But Scholl and Marlow (1974a) made a systematic study of the nature and volumes of pelagic hemipelagic and trench sediments, and conclude that "the volume of oceanic debris that should have been off-scraped in the trenches would form a very noticeable part of the circum-Pacific mountains if it were present there", and "the sequence of sedimentary deposits occurring in modern Pacific trenches does not support the idea that the circum-Pacific fold belt includes substantial amounts of uplifted trench deposits or tectonic scrapings." The two sets of sediments are not of the same kind. "The typical sedimentary fill of a trench includes a sequence of pelagic, hemipelagic, or pelitic hemiterrigenous deposits that are poorly represented volumetrically in the dominantly terrigenous graywacke beds and mafic volcanic rocks common to the fold belt."

Scholl and Marlow pose the question: "If the coastal mountains fringing the Pacific are virtually devoid of oceanic deposits, what, then, has happened to the tens of millions of cubic kilometres of pelagic and hemipelagic deposits that should have accumulated depositionally and tectonically in Mesozoic and Cenozoic trenches, and what was the depositional environment of the graywacke-dominated circum-Pacific eugeosynclinal assemblage? If the world is expanding, . . . then those questions are largely rhetorical."

No Moho bend oceanward from trench

According to the plate model, the lithosphere down to the asthenosphere (far below the Moho) bends down at forty degrees or steeper at the trench. Allowing for the crustal thickness the Moho should bend down well to the east of the trench, and this should show up unmistakably in gravity and seismic velocities, at least to a depth where the lithosphere condensed to a denser phase. The evidence is emphatically contrary (see, for example, Worzel and Shurbet, 1954; Gainanov *et al*, 1968; Galperkin and Kosminskaya, 1964; Walzer, 1973c). This discrepancy is exacerbated by Jordan (1975) who concluded that any decoupling zone beneath the plates must be at least 400 km down, perhaps much deeper.

Continents are welded to their own mantle

Beloussov (1967) and Walzer (1973c) have stated that the general equality of heat flux between continents and oceans contradicts the lithospheric conveyor-belt model of plate tectonics. Clearly the radiogenic heat yield of continental rocks exceeds that of oceanic lithosphere by an order. If continental lithosphere moved over pervasive mantle on a yielding asthenosphere, the heat yield below the asthenosphere would be generally uniform, so continental heat flux should be quite significantly greater than the oceanic. Whereas if the continents derived their radiogenic elements by differentiation of the mantle fixed beneath them as proposed by Ringwood, equilibrium heat flux would be more or less constant everywhere irrespective of the degree of differentiation. Variation would occur through erosive transfer of radioactive elements from rising orogens to adjacent geosynclines, and through the elevation of isotherms in all diapiric and stretching zones. Rocks have marked thermal inertia, and the relaxation time for elevated isotherms on tectonic scales by conduction dissipation is measured in hundreds of millions of years.

Jordan (1975) has established that the one-way vertical S-wave travel times through the upper mantle average five seconds greater below oceans than below continents, which, with other seismic data, implies significant differences between subcontinental and sub-oceanic mantle down to at least 400 km. Likewise he found that temperature differences between oceanic and continental shields also extend deep in the mantle. The thermal inertia and long thermal relaxation time implies that the mantle is bound to the crust at least to 400 km if not deeper.

Universal tension

Tanner (1971) concludes that "the sea-floor-spreading *hypothesis* may, for some geologists, require compression in the vicinity of trenches, but the *data* require horizontal tension." Tensional phenomena are not confined to the trench itself but extend back all the way to the continent behind. Between the Solomon Islands and the Queensland coast horsts and grabens recur all the way (Fig. 28), and this applies to the Tasman Sea generally. The disjunctive seas behind the arcs of east Asia have formed by extension since the Mesozoic, during the time that the allegedly westward-moving Pacific crust was to have been continuously driven against the arc and forced below it. This does not add, notwithstanding Karig's (1971) tectonic gymnastics to do so. Trenches are typically associated with thinning and necking of the crust, which implies extension, not compression, which could only produce thickening. Even the orogenic arc between the trench and the disjunctive basin, which many assume axiomatically to be compressional, is a diapiric structure. Diapirism normally indicates a dilational environment. Even these orogenic arcs have probably widened transversely during their evolution (Fig. 27).

Katz (1971) concluded that, contrary to the expectation of subduction compression, the Andes have experienced extension, across the trend, since at least the Miocene. Extensional stress dominates the upper crust there over an area at least 300-400 km wide. Likewise, Tanner (1974) has reported that "Carlos Ruiz, for years director of the national geologic sur-

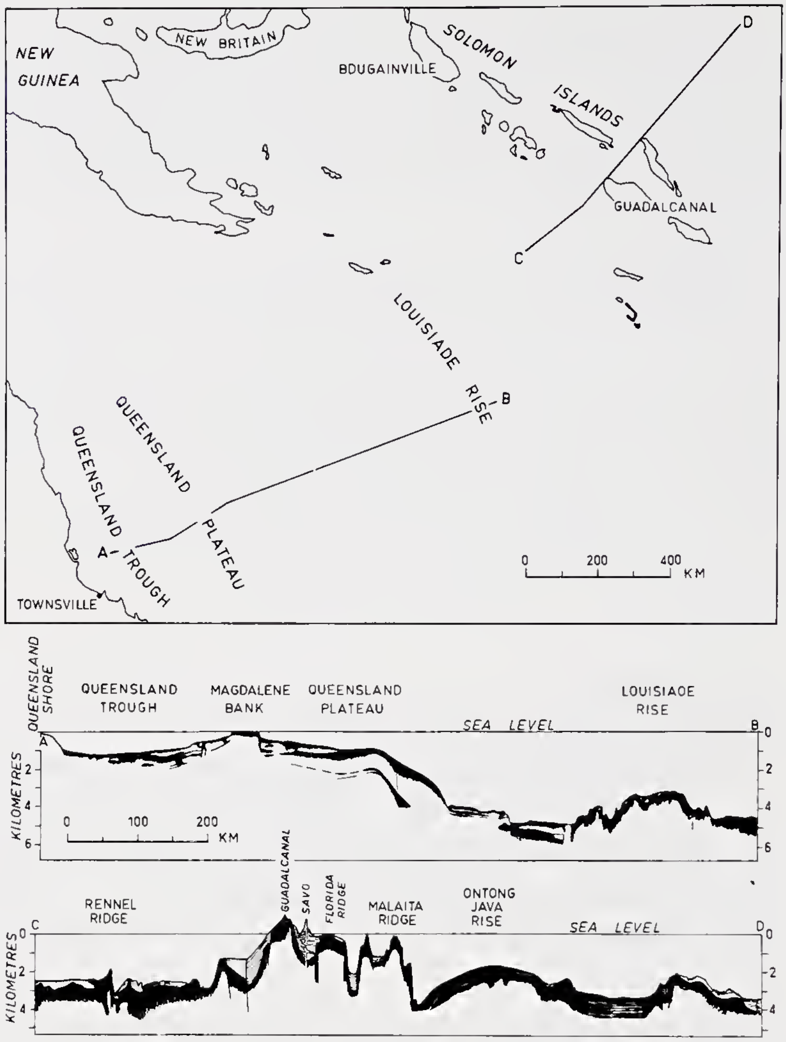


Fig. 28

Compilation from published sparker profiles from Townsville north-eastwards through the Solomon Islands, showing horst-and-graben structures throughout this belt.

vey of Chile, stated on more than one occasion that the Andes Mountains of Chile exhibit specifically an east-west tensional style. J.C. Vicente (1970) of the University of Santiago, Chile, has detailed a similar position. Ramirez (1971) pointed out that the main structure in west-central Colombia is a graben (oriented roughly north-northwest and south-southeast). Cobbing (1972) emphasized that the structure of Peru must be contrasted with the theoretical model of marginal thrusting, and concluded that the basic deformation was rifting. Carter and Aguirre (1965) described the horst-and-graben gross structure and the Cenozoic history of extension of Chile. Zeil (1965) stated that there has been no folding in the Chilean coastal range since the beginning of the Triassic; his structural cross sections show normal faulting and graben development. My own field work, primarily in Colombia and Chile, leads to the same conclusion: the fundamental tectonic style is extensional (east-west)."

Beck (1972) concluded that horst-and-graben tectonics (associated commonly with transcurrent faulting) dominate the pattern of all Pacific-type continental margins.

Evison and Calhaem (1974) find crustal extension across the New Zealand orogenic belt, where the plate theory requires several thousand km of crustal shortening: "The geographic distribution of the North Island calc-alkaline andesites with respect to age suggests that the subjacent Benioff zone migrated slowly through the region of the Northland and Coromandel Peninsulas during the period 18 my - 4 my BP, and rapidly through the Taupo Volcanic Zone thereafter. This migration was accompanied by crustal extension. In the slow phase the mode of extension was by volcanic intrusion into the Mesozoic sedimentary basement; the rapid phase appears to have involved active spreading with production of new lithosphere. The extension process in the Taupo Volcanic Zone has thus been essentially the same as in the Lau-Havre Trough. The spreading has occurred from near the andesite axis and is thus asymmetric. It accounts for the major bifurcation in the North Island Mesozoic basement structure; it provides a sufficient heat source for the widespread and persistent geothermal activity; and it may explain the production of very abundant ignimbrites by the heating of detached blocks of Mesozoic sediments."

Even the Tibetan highlands, the very paradigm of crustal foreshortening, are ill at ease in their role, for Terman's tectonic map of China and Mongolia (1974) shows Tibet as a region of horsts and grabens through the Tertiary, a picture more suggestive of extension rather than crustal compression. This complements the earlier summary by Wadia and West (1964) that the evidence so far obtained tends to show that large areas of the inner western Himalayas possess a comparatively simple type of mountain tectonics, and the piles of nappes, their complex refolding, virgations, digitations and inversions, such as those to which modern theory ascribes the formation of the Swiss Alps, have not yet been observed on the same scale of intensity or order of magnitude.

Commonly patterns interpreted as compressional are equally valid when the stresses are reversed, with compression replaced by tension. Thus in Scheidegger's analysis of Wilson's theory of compressional island arcs, he pointed out that the stress terms were squared, so that a tensional regime could mimic a compressional regime. Likewise Tokuda (1926-1927) reported his well-known simulation of ridges en echelon in island arcs by pasting a thin sheet of rice paper on to glass then puckering the paper by pressing with his finger. But the same pattern would result had he pulled his finger instead of pushing it, thus producing the reality of island arcs (Fig. 29). Similar studies were carried out by Fujiwara (1927), who made both compressional and tensional models, Carey (1938), Strel'tsov (1970), and Kaizuka (1975).

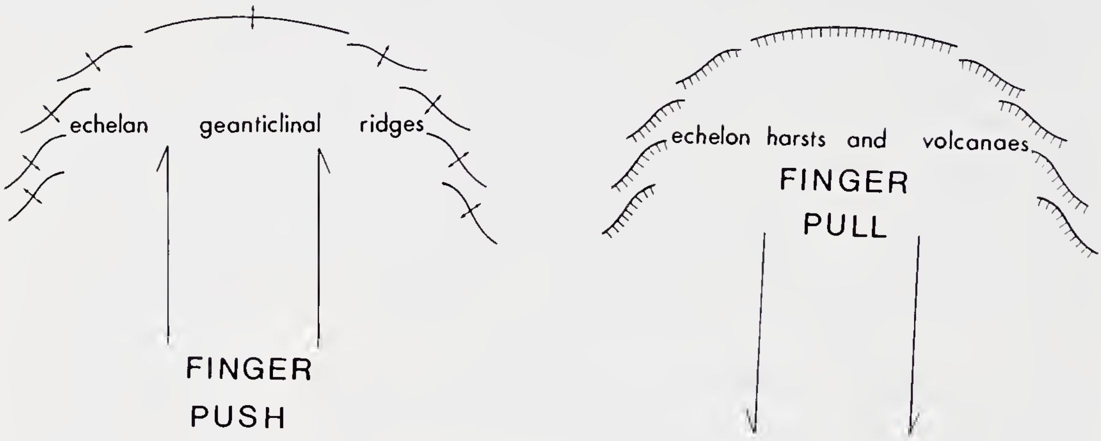


Fig. 29 Tokuda (1926-7) produced an arc of ridges en echelon by pushing his finger on rice paper glued to a plate. The same pattern emerges from tension instead of compression.

All motion is UP behind the trench

Within the orogenic arcs, the dominating motion is *up*, with lateral gravity spreading (Fig. 27). Overthrust surfaces, fold axial surfaces, and lineations are steep in the cores, flattening fan-like towards the outer trench side, with each inner side over-riding the outer side, reproducing the kinematics of the rising and spreading diapiric tumour. Diapiric mantled-gneiss domes and migmatitic concordant plutons form diapirs through flow-folded sediments, and through these, in turn, rise discordant magmatic plutons and volcanoes. The bodies pierce upwards in a viscosity peck-order (metamorphic tumours, *Migmatitpilze*, magmatic plutons, volcanic extrusions and *nuées ardentes*), and in a sequence from concordant convolution without fracture to increasing discordance, rupture and fragmentation. Metamorphic minerals in the core, such as kyanite, have come up more than twenty km from their high-pressure site of generation. The isotherms have risen with the general upward motion of the orogenic zone. The kinematic picture is a rising tumour fanning upward, and spreading under its elevated weight, with maximum upward motion in the core, diminishing to zero across the trench, the whole combined with overall dilation all the way from the trench across the diapiric orogen and volcanic zone to the disjunctive sea behind.

Sychev (1973) reviewed the seismic, thermal, and gravity data and concluded that the subduction model gave rise to difficulties and objections.

Only gravity differentiation of the mantle from depths of 700 km and more, possibly from the core-mantle interface, could, in Sychev's view, explain the complex of data observed. Zonenshain (1972) has crystallised the wisdom of many Russian tectonicists (indeed of most students of geosynclines) that, fundamentally, geosynclines are the sites where initially new oceanic crust is created, - which is the antithesis of the subduction concept. He concluded: "The origin of a eugeosyncline is accompanied by tension, by moving apart of lithosphere plates and by creation of a new oceanic floor. In some regions (South Mongolian, for example), one can restore ancient sea-floor spreading. Eugeosynclines appear to be analogous in their initial stage to ocean ridges, and in the mature stage to island arcs. On the whole, the evolution proceeds, possibly, from conditions resembling mid-ocean ridges to an island arc and further to an orogenic zone." Again, "geosynclines (and eugeosynclines especially) are deep-seated structures in which the energy and substance of the upper mantle rise up toward the earth's surface. In this process a new crust is created, including "basaltic" and "granitic" layers." The geosyncline-orogenic cycle is one of extension and upward diapiric regurgitation as indicated in figure 27, the direct opposite of the convergent, compressional downgoing process visualised in the plate tectonic hypothesis.

Kinematic contrast

The diapiric orogen model of figures 27 and 30 and the subduction orogenic model both yield thrusting and facings away from the orogenic axis, but their kinematics are very different (Fig. 31). In the orogenic diapir model the oceanic lithosphere is static, the orogen extrudes, and spreads under gravity. By contrast, on the subduction model the orogen is static and the oceanic lithosphere is swallowed down. The maximum motion in the diapiric orogen is upward, perhaps one km per million years. The maximum motion on the subduction model is downward at some 50 km per million years at the Benioff zone, diminishing to zero at the orogenic axis.

The mechanical contrasts between the diapiric and subduction models are shown qualitatively in figure 31c-d. Both models show the maximum stress-difference at the Benioff zone, the magnitude in each case being determined by the strength of the rock. Velocity of movement is maximum at the volcanic zone and upwards in the diapiric model, while in the subduction model velocity is maximum seawards of the Benioff zone, and downwards, at a rate of two orders faster than in the diapiric model. To a first order,

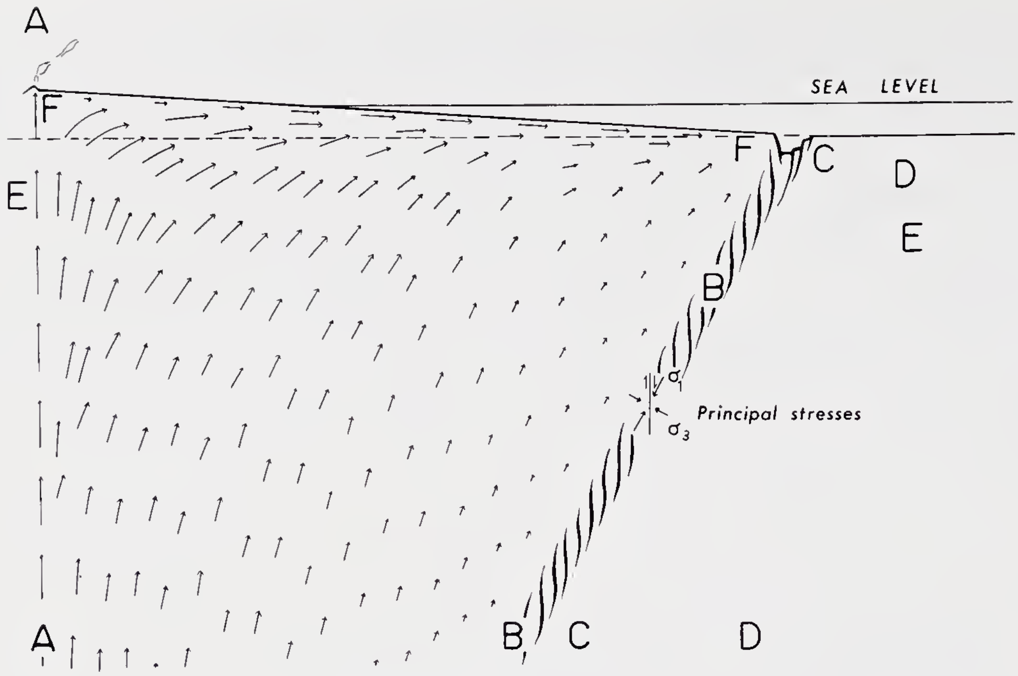


Fig. 30 Kinematics of orogenesis.

temperature differential against an average gradient must be closely sympathetic with velocity, since, on the time scale, involved most differential heat transfer must be by transfer of material, convection (*sensu stricto*), rather than by conduction. Modulation of this curve will arise from friction at the Benioff zone, by ascent of magma, and by subsidence of sediments. These corrections have been sketched qualitatively in the diagram, probably more generously than they warrant.

Which of these very different kinematic distributions fits the facts? The distribution of plutonism, vulcanism, *Migmatitpulze*, mantled-gneiss domes, metamorphic grade, heat flux, isostatic uplift, all fit the orogenic diapir but misfit the subduction model.

Orogenic and metamorphic energies do not fit

Oxburgh (1967), in a critical analysis of mantle convection as the primary tectonic engine, has followed Verhoogen (1960) in drawing attention to the marked disparity between thermal and dynamic phenomena in orogenic belts, the energy requirements of the former being two orders greater than the latter, even when the model is biased to minimize the disparity. "Thus in seeking some mantle process which could bring about orogeny in the overlying crust, it is desirable to find one which can supply substantial quantities

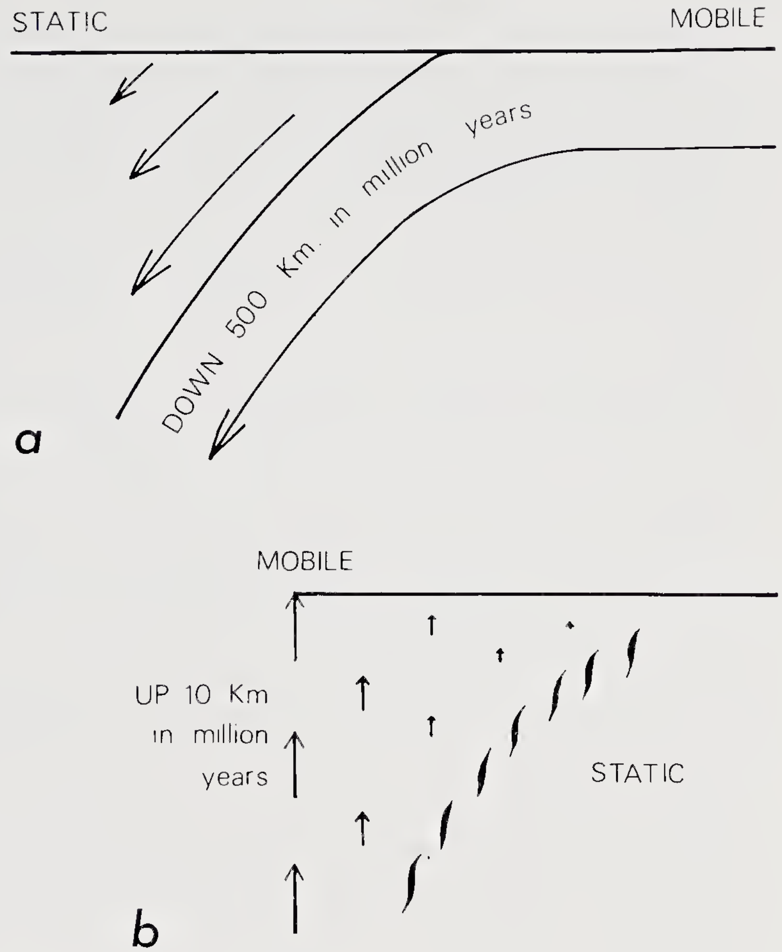
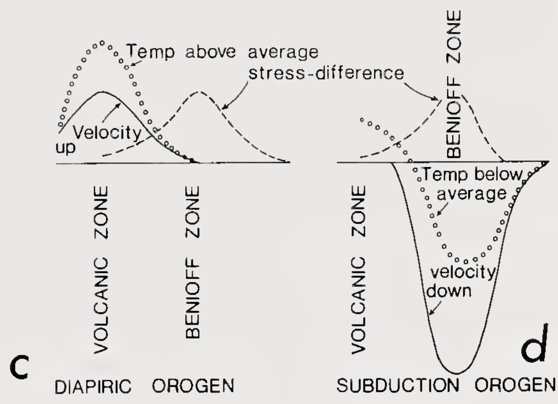


Fig. 31

Contrast in mechanics of diapiric and subduction models:
a-b, kinematic antithesis;
c-d, contrast of heat flux, stress-difference, and motion.



of thermal energy to narrow elongate belts of crust. It seems that the top of a descending convection current is most unsuitable in this respect; if anything the normal heat flow should be slightly less than the average." By contrast the diapiric orogen model of figure 27 does provide this thermal energy for metamorphism, and the subsidiary energy for lateral overfolding and dynamic spreading, in the observed proportions.

Blue schists and paired metamorphic belts

Miyashiro (1961) introduced the concept of paired metamorphic belts, and spawned a spate of studies (Takeuchi and Uyeda, 1965; Landis and Coombs, 1967; Minear and Toksoz, 1970; Dewey and Bird, 1970a and b; Oxburgh and Turcotte, 1970, 1971; Toksoz, Minear and Julian, 1971; Miyashiro, 1967a, 1967b; Landis and Bishop, 1972; Miyashiro, 1973; Forbes and Lanphere 1973; van Bemmelen, 1974), all of which set out from the premise of a subduction model.

The concept of paired metamorphic belts does express a reality, but in an over-simplified and over-compartmentalised form. In general, peri-Pacific orogens tend to have an "inner" belt (characterised by higher temperature metamorphism, with widespread granitic plutonism and andesitic vulcanism, steep-lineations, and metamorphic mineral assemblages through chlorite, albite, epidote, hornblende, biotite, andalusite, sillimanite, cordierite, staurolite, kyanite and almandite, with rock types which include greenschists, amphibolites, pyroxene-hornfels and granulite), and on the Pacific "outer" side nearest the trench a coeval adjacent belt (characterised by low-temperature metamorphism, little granitic plutonism, flatter, Pacific-facing lineations, and metamorphic mineral assemblages through prehnite, pumpellyite, thence to chlorite, hornblende, albite greenschists, and finally to lawsonite, glaucophane, aragonite, jadeite, and quartz blueschists, often associated with ophiolites and eclogites and other mafic and ultra-mafic bodies, which may represent tectonically incorporated sea-floor-type basement). The boundary between these two belts may be distinct, with marked contrast in facies, or more commonly transitional; in some cases, only one of the contrasting facies may outcrop. Indeed, the occasional sharp boundary and resulting term "paired metamorphic belts" may merely be an artefact of tectonic transport, giving juxtaposition of zones of different provenance.

Correlation with the experimental pressure-temperature stability fields of the mineral associations have assigned the inner belt to a high-temperature regime over a range of pressures, and the outer belt to high pressures at low temperatures. The former has been related to an environment of partial melting at depth, vulcanism, plutonism, and generally elevated isotherms. The latter has been interpreted according to the subduction model, according to which trenchfill sediments are dragged down to great depths by the cold down-going lithosphere slab to regions of high pressure and low temperature. Thereupon by some mysterious process they are shot back up again for ten kilometres or more against the down-going stream. This reversal is highly improbable kinematically.

Viscosities must be of the order of at least 10^{18}, and more probably much higher (this is a relatively low-temperature zone), and the density differential not greater than 0.3. A glacier has a viscosity of some 10^{13}, a millionth less viscous than the schists, yet I cannot imagine any part flowing back against the stream. A fault horse produced by dichotomy of the fault surface, will always be found between the dragged ends of its matching parts, never beyond either of them.

Any density differential of the blue schists could cause them to *sink* isostatically, not rise, because the density of glaucophane-schist exceeds that of granite, gneisses, and serpentine, and the associated eclogites are even denser!

It is true that the blue schists have risen, but, I suggest, as the marginal zone of a generally extruding geotumour, which has kinematic homogeneity. The original deposition of the sediments was in a trench, perhaps at depths of ten or twelve km, and this load may have been increased a further couple of kilometres by gravitational spreading of the regurgitating orogen, but their subsequent motion was ever upwards with, of course, differential diapiric lobes.

The crux of the paired metamorphic belt model is interpretation of the blue schists of the "outer" metamorphic belt as indicative of very high pressure with low temperature. This is not soundly based and I suspect that it is wrong. A given chemical combination yields diverse metamorphic products according to the synergism of at least four variables: temperature, fluid pressure, confining pressure, and stress-difference. Increasing confining pressure yields denser higher-energy paramorphs, while increasing temperature and fluid pressure tends in the opposite sense. Stress-difference below yield stress produces flow by stress-biased diffusion, and (by increase of maximum principal stress relative to minimum principal stress) favours schistophilic minerals whose lattices differ markedly in energy barriers in their respective axes, and these minerals crystallise at load pressures lower than they would otherwise require. Strain energy stored during active deformation greatly reduces the recrystallisation energy barriers (Newton *et al.* 1969, estimate by 15 kb per kc per mole for calcite-aragonite). Stress-difference above yield stress induces a variety of rheological modes, including migration of lattice imperfections, slip in low energy crystallographic planes, and recrystallisation of boundary zones, which collectively result in continuing flow with some work-induced rise in yield stress. These processes again favour schistophilic minerals in stress-governed orientation, and again these would crystallise at lower confining pressures than the same minerals would require under confining pressure alone. Although aragonite is orthorhombic, it commonly twins on the 110 prism to form pseudohexagonal twins with the carbonate groups causing dense packing in the basal plane and much less atomic density along the c axis.

The interpretation of the blue schists as high pressure rocks is based on the stability fields of the relevant mineral combinations (glaucophane, lawsonite, aragonite, quartz, etc.), determined by experimental work under hydrostatic pressure *without stress-difference*. Such experiments are not directly relevant, as the blue schists clearly belong to regions where strong stress-difference prevailed. Morover, the metamorphic grade in blue schist terrains correlates directly with the intensity of synkinematic folding (Coleman, 1967), that is, metamorphic grade for these schistophile minerals correlates with stress-difference, not hydrostatic stress, which cannot produce movement.

Vance (1968) reported the common occurrence of aragonite in phrenite-pumpellyite-phyllites in Washington, which have suffered large stress-difference but certainly not high hydrostatic pressure. Vance commented "The metamorphic facies of these rocks and the stratigraphic evidence indicate formation of the aragonite at low or only moderate pressures, not at the high pressures of the experimentally determined calcite-aragonite inversion curve . . . These relations raise fundamental questions as to (1) the reliability of metamorphic aragonite as an index of high pressure; and (2) the validity of the extreme pressures and especially of the unique geologic history assigned to certain rocks of the glaucophane-schist facies on the basis of the occurrence of aragonite". Chadwick (1974) found that the glaucophane facies in the southwest Swiss Alps developed at shallow depth but high rate of strain during the Mischabel backfolding. Blake, Irwin, and Coleman (1967, 1969) emphasised the association of blue schists with belts of regional shearing and increasing metamorphic grade from pumpellyite to lawsonite towards thrust surfaces. These authors sought to explain this association in terms of anamalous high water pressures in the fault zone, but I would invoke instead the high stress-difference which certainly existed in these regional shear zones.

The only relevant experiments I am aware of are those of Newton, Goldsmith, and Smith (1969), who found that, under conditions of stress-difference, calcite recrystallised to aragonite at 100°-300°C at confining pressures more than three kilobars less than required in hydrostatic conditions. In seeking the genesis of the blue schists by asking for the pressure-temperature stability fields of the observed mineral combination in hydrostatic pressure experiments, Miyashiro and his disciples have asked the wrong question, and have departed, content, with the wrong answer!

Coleman and Lee (1962) have considered stress-difference in relation to the stability fields of the blue schists, but only in the context of compressional tectonic hypothesis, i.e. of tectonic over-pressure.

The interpretation of the blue schists may be further complicated by tectonic and metamorphic non-homogeneity. For example, eclogites in this terrain may record equilibrium conditions established earlier than the metamorphism of the abyssal sediments which draped their horsts. Coleman and Lee (1963) emphasised that the Californian glaucophane-schist terrains contained disharmonious exotic schists (their Type IV rocks, including eclogites) which had recrystallised earlier in a deeper environment, and had been incorporated tectonically during the blue-schist metamorphism. They showed discordant petrofabric patterns and regressive metamorphism. In this they contrast with their host rocks, where the aragonite, for example, has petrofabric orientation concordant with the regional lineation. Again, deformation strain energy may have been differentially available in restricted zones. Likewise hydrothermal outgassing may be concentrated and produce environmentally-exotic hydrothermal veins. The blue-schist terrains are therefore a tectonic mosaic of several provenances, including (a) inherited metamorphic pre-orogenic facies, (b) syn-kinematic facies of low confining pressure, low temperature, low gas-pressure, high stress-difference, forming a suite from low grade to high grade (these are the blue schists *sensu stricto*), (c) local post-kinematic hydrothermal zones with high temperature, high gas-phase pressure, and low stress-difference.

Blue schists are rare in ancient terrains, perhaps not because they did not form, but because they are a relatively shallow facies which are much more prone to be eroded away during successive peneplanations than are the granulites, migmatites, and schists of intermediate depth. Reinterpreted in this way, the kinematic contradiction of the blue schists vanishes. The contrast of the "paired" metamorphic belts reduces therefore essentially to one of temperature. Both belts have high flow strain always upwards and outwards, though with greater travel in the hotter central zone. But as the temperature declines outwards, the stress-difference corresponding to flow rapidly increases. Therefore the central zone is dominated by high temperature, high water-gas-pressure, and a range of confining pressures. These variables wholly account for the metamorphic contrast in a homogeneous kinematic regime. The general gradient between these extremes is, of course, greatly modulated by local zones of higher outgassing, with consequent higher temperatures and lower stress-difference. Such second-order changes of metamorphic facies may be nearly symmetrical. Sharp discontinuities also occur along major tectonic lineaments which involve substantial differential transport; such tectonic discontinuities always have the higher temperature facies on the near side, as is required by kinematic homogeneity.

Seismic misinterpretation

Tanner (1973) has emphasised that the seismic data have been interpreted selectively on an initial assumption of subduction of a sinking slab, and that when examined without this preconception much is actually contrary to the subduction model, and much more admits other interpretation, but that the compressional solution has nevertheless been selected, often against the weight of evidence.

Several cognate but separate processes act together to yield a seismic symphony which is not simple. Fundamental to all is primary crustal extension, which is expressed by the universal horst-and-graben pattern. Tension faulting is dominantly along the structure, but the global extension is areal, so that oblique fractures also occur, which ideally would cut the trench at 60° as in columnar jointing. Such oblique faults along trenches are very common but are usually interpreted (probably incorrectly) as conjugate wrench faults.

Tensional failure can only occur in the shallower parts of the lithosphere where the effective load (overburden less fluid pressure) is less than the shear strength. At greater depths progressive extension from any cause results in a stress-difference greater than the shear strength, so shear failure must occur there before any of the principal stresses reduce to zero and become tensional (EE of Fig. 30, and BB of Fig. 48). Hence the only possible pattern in a stretching zone (e.g. over a geotumour or rising convection system) must be shallow tensional failure changing to shear failure below, at a depth determined by the weight of overburden less the fluid pressure. This is indeed the empirical pattern associated with trench-orogenic arc systems. Another important group of earthquakes are the shallow ones caused by the gravity spreading. These may be low-dipping overthrusts near the surface, but steep overthrusts also develop analogous to the steep gravity-spreading thrusts which border the basement horst blocks of Wyoming.

Still another group of earthquakes, which are transcurrent, stem directly from the global expansion, which must cause differential zonal motions because expansion along the equator increases the moment of inertia, whereas in high latitudes this effect rapidly diminishes. Hence we should expect rotation of large crustal blocks, sinistral in the northern hemisphere and dextral in the south. Transcurrent faults, to adjust differential expansion growth, and transform faults with strike-slip motion, may also occur.

A final category of deep earthquakes has been proposed by Evison (1967), due to impulsive phase change from a metastable state less dense than its pressure-temperature environment. Although denied by Jeffreys (1967) on the grounds that the S_H phases of these earthquakes indicate shear failure at the source and are inconsistent with implosions, Ringwood (1971) has pointed out that earthquakes generated by nuclear explosions show large shear-wave components which on Jeffreys' argument should imply that their source was a shear failure. Ringwood also argues that phase-changes could result in impulsive implosions under favourable circumstances.

An astonishing aspect of the plate-tectonic literature is the interpretation by Isacks, Oliver, and Sykes (1968) of the first-motion data from the Benioff zone, which has been repeated by so many others (Fig. 32) although Toksöz *et al.* (1971) have recognized the inconsistency. The shallow earthquakes are accepted by them as tensional, although they interpret these as due to the bending of the upper surface of the slab (which would be acceptable if the tension was confined to the belt of alleged bending, which it is not). At greater depth, they state that first-motion analysis yields

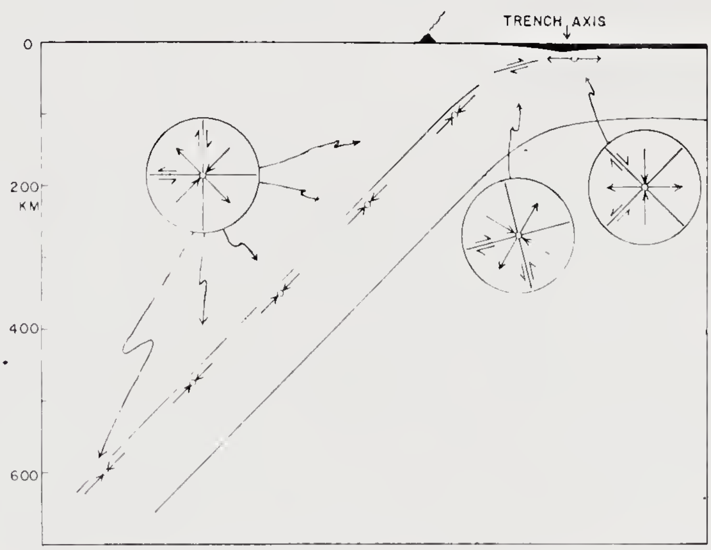


Fig. 32

Interpretation of stress distribution along the Benioff zone according to Oliver and Sykes (1968).

a maximum principal stress down the dip of the Benioff zone. Some, e.g. Tanner, 1971 and 1973, and Evison, 1967, would challenge this generalisation, but let us accept that some at least (in fact many) of the earthquakes do have this pattern. But Oliver and his co-authors go on to interpret this as being due to the lithosphere slab being forced down into the mantle in the direction of the Benioff zone. This could not be correct, because maximum principal stress along the Benioff zone can only mean that the seismic shear failure must be either dextral and near-vertical, or sinistral and near-horizontal. Yet on the plate-tectonic model the motion is *along* the Benioff surface, and the shear surfaces would have to be parallel to it. The maximum possible error is 45° and this is the error in their interpretation. Nor is the position altered by assuming that the "asthenosphere" into which the slab is pushed is weak, and offers little resistance to the penetration. In this case the maximum stress along the slab could not exceed the magnitude of this weak resistance, and must be well below the strength of the slab. You don't crack nuts by pushing them into butter. It would not help either to assume that excess weight of the sinking slab causes the downward motion and the earthquakes, because in that case the minimum principal stress would be vertical, and the shear failures would be along the Benioff zone, which is contrary to their own seismic data. The steep sinistral first-motion indicated by Isacks *et al* is precisely that required by the kinematics of a steeply rising tumour with fractures *en echelon* along the tumour boundary against the passive suboceanic crust (Fig. 30). Also the change from shallow tensional failures to shear failure at depths where the effective overburden load exceeds the shear strength, is precisely what must occur in a dilating zone (Fig. 48).

Seismic attenuation

Another matter which may have been misinterpreted is the low velocity zone and the attenuation factor (Q) of shear waves. It is commonly assumed that these are due to reduced viscosity with rising temperature.

The empirical distribution and thickness of the low velocity zone, and of low Q, suggests strongly a correlation with elevated isotherms: mid-ocean ridges, island arcs, orogenic zones, volcanic lineaments, and disjunctive areas such as the Basin-and-Range province. In my model all these categories are extensional and diapiric, and the elevated temperatures are due to outgassing. Both the higher temperature and the higher water content implied by the outgassing would be likely to lead to partial melting. But if partial melting is granted, the mechanism by which this could result in lower seismic velocities could not be through viscosity. The relaxation times are several orders too long.

Viscosity is only relevant if it causes significant stress relaxation during the elastic vibration cycle. Taking improbably high frequency for S waves (0.01 Hz) and a shear modulus of 10^{11}, the viscosity would have to be as low as 10^{14} poises to cause any detectable relaxation; for much more normal periods of one to 10 Hz viscosities as low as 10^{12} or 10^{11} poises would be necessary. Viscosities at the relevant depths are normally taken to exceed 10^{20}, although some have claimed viscosities locally as low as 10^{17}, but even this extreme figure is many orders too high. Seismic waves do not "see" bodies significantly smaller than their wave length. A crystal mesh with even 10% of interstitial melt (0.1% is a more common estimate) is seen by seismic waves with a frequency of less than one kiloherz as a composite, not as a liquid plus solid. The composite viscosity thus seen cannot be less than 10^{18}. Seismic waves through a glacier, most of which consists of crystal mesh with an interstitial melt phase, and sandstone saturated with interstitial ground-water, are analogous.

Two mechanisms (other than bulk viscous relaxation) do suggest themselves whereby the presence of a minor melt phase might cause the observed reduction in velocity and attenuation - excess fluid pressure, and stress-difference. Excess fluid pressure could perhaps reduce the effective elastic parameters - particularly the shear moduli, and hence reduce velocities, S being affected to a greater degree than P.

Stress-difference implies flow and it could be that the lower velocity and the higher Q may mean, at least in some cases, that the rocks are in a state of flow. Both diffusion viscosity (non-Newtonian) and plasticity (*sensu stricto*) are probably involved in the flowage of rocks. Rocks in a state of flow have a large stress-difference for viscous flow, and a stress-difference at least in excess of the yield stress for plastic flow, and this stress-difference is sustained while flow continues. If seismic waves traverse such a medium, P waves would be least affected, as the bulk modulus is more important than the shear modulus; but shear waves vibrating parallel to the flow laminae must lose energy rapidly, because in one half of the cycle the elastic shear stress adds on to a stress-difference (already in excess of the yield stress) so that additional slip occurs, absorbing energy. If this explanation be correct, S wave paths across the flow of a glacier should have lower velocity and more attenuation than paths along the glacier. The other half of the cycle is elastic, because it operates against a stress bias. In this context temperature may become relevant to attenuation, not by reducing viscosity, but because of its effect on reducing plastic yield stress. Such attenuation would be asymmetric - greatest in shear waves propagating normal to the rock flow. This interpretation of Q fits the tumour model, though in itself it is not contrary to the subduction model.

Green (1972) has argued that the weakness of the asthenosphere and the seismic properties of the low-velocity zone may be caused by the presence of exsolved CO_2 in a subsolidus asthenosphere, and Green and Radcliffe (1975) have found from high-voltage electron petrography of lherzolite xenoliths in alkali basalts, that the CO_2 was exsolved into microscopic (< 5μm) liquid CO_2 bubbles during intense solid-state flow. This again implies correlation of low Q and low velocity with large stress-difference rather than partial melting.

Higher velocity under Benioff zone

Early arrival residuals of a few seconds have been recorded for P-waves travelling along, but just below, the Benioff zone. These residuals have been interpreted in terms of a cold sinking slab. But is this the only interpretation? According to the diapiric geotumour model the Benioff zone is the boundary between a region in state of active upward flow where stress-difference exceeds the yield stress, and an inert region below it where the stress-difference is less than the yield stress. But this inert region is the medium pierced by the diapir, and the boundary zone is maintained in a state of elastic strain and corresponding stress-difference, below the yield-stress threshold (CC of Fig. 30). The maximum principal stress (σ_1) lies parallel to the dip of the Benioff zone. As the minimum principal stress (σ_3) cannot be less than the hydrostatic load, P velocity parallel to σ_1 would be higher (by up to 10%) than in similar rocks under the overburden load alone. This would result in early arrivals, by up to a second after 100 km of travel.

The velocity of a seismic wave is the root of an elastic function over the density. Fundamentally elasticity depends on the distance between atoms. Any process which packs the atoms more closely increases the relevant elastic parameters. Hence stressed rock shows different elasticity from non-stressed rock. The effect of an orogenic diapir on the elastically-strained immediately-adjacent lithosphere is to increase the maximum principal stress parallel to but below the Benioff zone (Fig. 30). Hence, earlier arrivals for seismic waves following this path should be expected.

The most relevant experimental work is that of Gupta (1973) who measured velocities on a triaxially loaded slab of Indiana limestone. Only the $\alpha(Z)$ data are relevant (that is P along σ), because in these unconfined experiments σ_2 and σ_3 fail in tension, whereas in the field problem all three principal stresses remain compressional right up to failure. Starting from a base P velocity of just under 4.5 km/sec, Gupta found the velocity along σ_1 to increase to 4.6 at 100 bars, then to 4.7 at 360 bars after which prefailure dilation dropped it slightly to 4.65 km/sec (Fig. 33). If a velocity increase of 0.1 km/sec is applied to the stressed region immediately below the Benioff zone, a P wave travelling say 500 km (i.e. from a depth of about 350 km) would arrive 3 seconds early. Clearly from greater depth and with greater velocity differential the arrival residual could be greater. Although these laboratory results cannot be applied numerically to the high confining pressures in the lithosphere, nevertheless it is clear that the observed early arrivals are of the right magnitude that should be expected from the stress-differences below the Benioff zone.

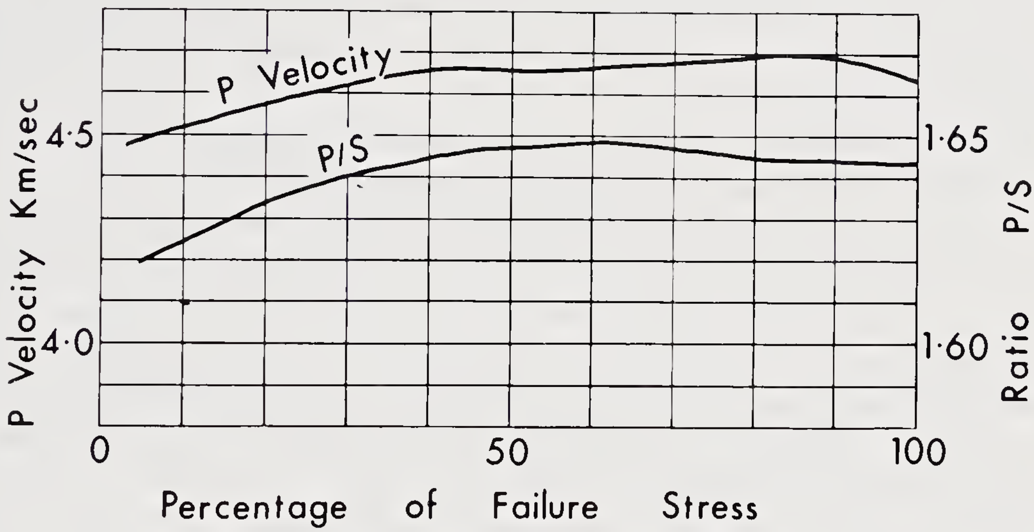


Fig. 33 Gupta's seismic velocities.

DEFINITIONS

This chapter collates some definitions and principles from my scattered earlier publications (1954, 1955a, 1955b, 1958, 1962a, 1962b, 1963, 1970), and from my class lectures.

Oroclines

An *orocline*, (Greek ορος, mountain, κλινω, I bend) is an orogenic belt with a change in trend which is interpreted as an impressed strain.

Examples of oroclines are:

(1) The bend in trends through 180° from the Sierra Nevada and Betic Cordillera of Spain through Gibraltar to the Riff of Morocco - the Riff Orocline (Fig. 91).

(2) The bend in trends from the Apennines of Italy through Calabria into Sicily - the Sicilian Orocline (Fig. 91).

(3) The bend in trends from the Apennines through the Ligurian Apennines into the Maritime Alps and the Pennine Alps - the Ligurian Orocline (Fig. 91).

(4) The bend from the Zagros of Persia through Baluchistan thence north-north-east to the Pamirs - the Baluchistan Orocline (Fig. 2).

(5) The bend in trend from the Canadian Rockies and Coast Ranges through the Alaska Range into the Alaska Peninsula - the Alaskan Orocline (Fig. 195).

(6) The right-angle bend from the Urals into the Pai Khoi Mountains (the Pai Khoi Orocline), continuing thence by another right-angle bend along the spine of Novaya Zemlya - the Novaya Zemlya Orocline (Carey 1958a, Fig.7).

These six examples, chosen from some thirty so far found on the surface of the earth, suffice to illustrate the meaning of the term. Each has been discussed in some detail before (Carey, 1955).

Albers (1967, p.145) has proposed the replacement of "orocline" by "oroflex" on the grounds "that 'orocline' suggests a feature inclined to the horizontal rather than a curvilinear feature". However the Greek verb κλινω means to bend or turn aside rather than deflect only from the horizontal. The verb κλινω was used with επασπιδα or επαδορυ for a soldier turning to his spear or shield side. Even in English, derivatives do not necessarily have any implication of deflection *downwards*. Thus the magnetic declination is a deflection in the horizontal plane, and in military usage "incline" means to move in a direction at an angle with the front. It so happens that the term I initially coined for this concept was "geoflex" (see Carey, 1954, 1954b), but as I pointed out in my formal definition (Carey, 1955), I replaced it by "orocline" to avoid the vulgarity of a Greek-Latin hybrid.

Coupled oroclines

Two oroclines in the same orogen forming an S shape are said to be coupled. They may have dextral or sinistral coupling, implying respectively right hand or left hand transcurrent movement. Examples of dextrally coupled oroclines are the Pai Khoi and Novaya Zemlya Oroclines (*loc. cit.*). the Sicilian and Ligurian Oroclines (Fig. 91), and the Baluchistan and Punjab Oroclines (Fig. 2).

Rhombochasm

Rhombochasm (from Greek ρομβος a rhombus, χαο I yawn) will be used for a parallel-sided gap in the sialic crust occupied by simatic crust, and interpreted as a dilatation. In dextral and sinistral rhombochasms, the blocks have moved apart with a right or left hand lateral component respectively. Examples of dextral rhombochasms are the Sea of Japan and associated north Pacific basins as far as the Gulf of California; the East Greenland Basin is a sinistral rhombochasm (Carey 1958a, figures 5 and 42).

Sphenochasm

A Sphenochasm (Greek σφην wedge, and χαο I yawn) is the triangular gap of oceanic crust separating two cratonic blocks with fault margins converging to a point, and interpreted as having originated by the rotation of one of the blocks with respect to the other. Within the boundary jaws there may be one or more radial rays which divide the general sphenochasm into two or more smaller sphenochasms.

Examples, several of which have previously been worked out in detail *(op. cit.)* are:

(1) The Biscay Sphenochasm between the Iberian block and south-west France, and apically opposed by the Pyrenean Compression (Fig. 91).

(2) The Corsican Sphenochasm between the south-east coast of Spain and the south-west coast of Italy. The Balaeric Islands, and the Corsica-Sardinia block are two radial rays which converge on the apex of the sphenochasm.

The Corsican Sphenochasm is apically opposed to the Ligurian Orocline (Fig. 91).

(3) The Arabian Sphenochasm separates the fault coasts of the Deccan peninsula on the one hand and the fault coasts of Arabia and Somaliland on the other. It is apically opposed by the Baluchistan Orocline (Fig. 2).

(4) The Honduras Sphenochasm between the fault coasts of northern Honduras and the east coast of Yucatan. The Cayman Islands and three other groups of islands form radial rays.

(5) The Arctic Sphenochasm, formed between the Siberian and Canadian shelves, has already been discussed in detail *(op. cit.)*. It is apically opposed to the Alaskan Orocline (Fig. 195).

These five examples serve to illustrate the term. Sphenochasms apically opposed to oroclines of more than 90° are so wide that they are less obvious. There are several such.

The floors of sphenochasms and rhombochasms stand 4 to 5 km. below sea level and are underlain by simatic crust. However they may rapidly fill with sediment, the weight of which causes further subsidence, so that the sediment thickness commonly exceeds 6 km. Such structures will be called filled rhombochasms (or sphenochasms). Examples are the Ob-Iriysh, Magdalena and Maracaibo basins.

Sphenopiezm

A sphenopiezm (Greek σφην̃, a wedge, πιεζειν to press) is the opposite to a sphenochasm. Instead of a wedge-shaped opening radially from a point, a sphenopiezm is a wedge-shaped squeezing together, radially from a point. Clearly a sphenopiezm and a sphenochasm may be apically opposed, although in this case the centre lines of the two need not be collinear. The Pyrenean sphenopiezm and the Biscay sphenochasm are examples of such a pair (Fig. 91).

Orotath

An orotath (Greek ορος, mountain, ταθεις, stretched) is an orogenic belt, which is interpreted as having been substantially stretched in the direction of its length.

Examples of orotaths are:

(1) The Andaman - Nicobar submarine ridge with its islands, extending from Lower Burma to the north-west of Sumatra, which is interpreted as having

originally been about half its present length - the Andaman Orotath (Figs. 178 and 179).

(2) Ryukyu ridge between Formosa and Kyushu and the Kurile ridge between Hokkaido and Kamchatka which are interpreted as having had formerly about half their present length - the Ryukyu and Kurile Orotaths (Figs. 170 and 194).

(3) The Kermadec ridge from East Cape of New Zealand to the Tonga Islands (the Kermadec Orotath) which is interpreted as having formerly been a fraction of its present length.

These examples, from the large number which will be cited, illustrate the kind of structure envisaged. Physically, if an orogenic zone in isostatic equilibrium is assumed to be stretched uniformly to twice its length without change of width, it will be in isostatic equilibrium when its mean height above the general ocean floor level is half its original mean height. It will become a permanent submarine ridge. If it also contracts uniformly in width as well as thickness, its mean height would be $1/\sqrt{2}$ of its original mean height - a higher submarine ridge. If the stretching is not uniformly distributed along the length, it would become a chain of islands with connecting submarine ridges. Such "boudinage" patterns are common.

Oroclinotath

An oroclinotath is an orogenic belt which is interpreted as having been subjected both to substantial bending and substantial stretching.

Examples of oroclinotaths are:

(1) The Lesser Antilles and associated islands between Puerto Rico and the Goajira Peninsula of the Maracaibo district. This is interpreted as having originally been very substantially shorter and nearly straight - the Antillean Oroclinotath (Fig. 164).

(2) The Scotia Oroclinotath (Carey 1958a, figure 38), the highly recurved ridge between Cape Horn, South Gorgia, the South Sandwich Islands and South Graham Island.

(3) The Banda Oroclinotath (Fig. 178), the strongly inflected ridge from the Lesser Sunda Islands, through the Banda Group to Salayas and the southwest arm of Celebes.

(4) The Sangi Oroclinotath (Fig. 178), the recurved northern arm of Celebes extending via the Sangi, Kawio, and Sarangani islands to southern Mindanao.

Since oroclinotaths merely combine the characters of orotaths and oroclines, these four examples suffice to define the group.

Nematath

There are many examples of submarine ridges across Atlantic type ocean floors, which mark the line of stretching across sphenochasms or rhombochasms, but which are not orogens. They join points which were originally closer, and mark the path of the separation movement. I suggest that they develop at those places where the limiting isotherm at which flow rather than fracture occurs is substantially shallower than is usually the case. Whereas the fracture boundary of the chasm normally dissipates into flow at a depth well below the Mohorovicic discontinuity, the level of transition will be higher where the rifting line crosses a zone of higher isotherms (i.e. a volcanic zone), perhaps even so high that the base of the sial will be involved in flow instead of fracture.

If I break a slab of toffee which is cold and brittle except for one warm spot, the slab will break cleanly except at the hot spot where a thread of toffee will be drawn out across the rift. The thread will be straight or curved according to the path of separation. If the isotherm at which fracture passes into flow is locally above the Mohorovicic discontinuity sialic material will rise into the rhombochasm along with the rising mantle material and form a thread of sial across the rhombochasm. In view of the density difference it will endure permanently as a submarine ridge on the ocean floor. For such threads the name nematath (Greek νεμα, a thread and ταθεις stretched) is proposed. In practice I find that such nemataths commonly join similar igneous centres across the rhombochasm, giving support to the above hypothesis of their generation. Where the transition from fracture to flow is below the Mohorovicic discontinuity no nematath results even if the isotherms are higher in some places than others. Nemataths may or may not bear intermittent volcanoes.

The following are examples of nemataths:

(1) The Lomonosov Ridge between the New Siberian Islands and Ellesmereland, interpreted as having had originally a small fraction of its present length, and as joining points formerly contiguous - the Lomonosov Nematath (Carey 1958a, figure 5).

(2) The Walvis Ridge between the Kaokoveld and Tristan da Cunha (The Walvis Nematath - Fig. 196) and the Rio Grande Ridge from Tristan da Cunha to the Santa Catarina area of Brazil (the Rio Grande Nematath - Fig. 196), which are interpreted as having originally been a fraction of their length and as now joining points formerly closely contiguous.

This 1956 definition, and the proposed genesis of a category of oceanic ridges through crustal movement relative to a mantle hotspot, led subsequently to a spate of papers on oceanic volcanic ridges and mantle "plumes".

Megashear

A megashear is defined as a strike-slip fault whose horizontal displacement exceeds significantly the thickness of the crust. It is inappropriate to use the term wrench fault for fractures of such magnitude. The latter term was brought into wide use by Anderson (1951), Moody and Hill (1956) and others. The wrench fault as used by these authors is defined in relation to the stress ellipsoid, and specifically in relation to pure shear. Now in structures of the magnitude considered here there can be no doubt that the fracture breaks the *whole* crust, and that the lower limit of fracture is the zone where plastic or viscous stress relaxation is sufficiently rapid to absorb the displacement by flow. The vertical stress is zero at the surface, and many times the unconfined compressive strength at the base of the crust. Over such a vertical range the stress field and the stress ellipsoid cannot even approximate to uniformity and to apply a stress ellipsoid or structures defined in terms of it to such a case is to apply it out of context.

Moreover Anderson and those who have followed him have assumed without argument that wrench faults are the result of pure shear produced by a non-rotational compression despite the fact that simple shear by rotational couple can produce identical strains. Also a pure shear failure can result from either compression (crustal shortening) or from dilatation (crustal dilatation) in all environments where the load of overlying rocks is greater than the unconfined shear strength of the rocks. Anderson was clearly aware of this, but in *applying* his theory to interpret faults and other structures, he tacitly assumed that a compressional pure shear is necessarily the causal stress system. Strain does not determine uniquely the causing stress system, and a pure shear (contraction) or pure shear (dilatation) or a simple shear are all possible solutions. The discrimination between these three cases is however often possible by an analysis of associated phenomena (e.g. rotations, brecciation etc.). It so happens, as shown later, that the very large transcurrent faults which concern us here must necessarily be the result of simple shear, if the orocline hypothesis is valid, even though many smaller wrench faults may be the result of pure shear as proposed by Anderson. The comments by Maxwell and Wise

(1958, pp. 927-8) are valid and significant.

For these reasons I propose to use the term megashear for those strike-slip fractures which displace by simple shear, not component parts of an orogen, but the orogen itself. Many examples of megashears will emerge as this analysis develops.

RESTRICTIONS

None of these structures can develop in isolation. An orocline implies a sphenochasm on one side of it,and this cannot go on diverging indefinitely. It must end against a megashear or perhaps another orocline. A megashear must either go right round the globe or begin and end at oroclines or rhombochasms. A single structure of this scale implies a chain of other structures to absorb the implied movement. Hence all such structures must be followed globally to the limit of their ramifications.

PHILOSOPHY OF OROCLINE CONCEPT

The emergence of these terms during the thirties has been mentioned on page 2. Philosophically, the coining of a term for such a class of structures was necessary and inevitable. For an orocline is the continuous deformation equivalent of a megashear, just as a fold is the continuous deformation equivalent of a fault. In each case, only flow deformation results if yielding occurs sufficiently rapidly that the stress-difference never reaches the shear strength of the material, whereas fracture occurs when the stress-difference exceeds the shear strength of the material.

Sir James Hall first made the induction that observed contortions in strata were actual deformations of beds originally flat, and therewith structural geology began. The next century and a half saw much progress, but most of it was towards increasing our understanding of the internal anatomy of orogens. Little was done about the deformation of the orogenic belt as a whole. Again, with the outstanding exception of Suess and his disciple Argand, the bias of structural nomenclature was towards the deformation of the profile cross-section of the orogen, not its shape in plan. There are adequate terms for the deformation and dislocation of strata in the vertical plane, and even for the further deformation in the vertical plane of such folded structures but there was no term for deformation of structures in plan, and still less for the impressed bending of the orogenic belt itself, or for its longitudinal stretching.

The order of the dimensions of a mountain system is 10^3 km. or more long by 10^2 km. wide by 10^1 km. deep. Reduced to a model we might think of it as a strip-lamina. Its plan and cross-section are shown to scale in A of figure 34. When such an orogen is folded, deformation in the vertical direction is closely confined by gravity and isostasy. Before and after orogenesis the locus of the centre of gravity of elements of the strip departs little from a gravitational isopotential surface, and the cross-section of the deformation of the orogen is still contained wholly within the lens B. It is quite impossible for the orogen to be deformed into the form C. The dimensions of length and breadth, on the other hand, are not confined or limited. Alpine geologists wedded to compressional tectonics speak in terms of 500 km., even 1200 km., of horizontal translation. Only 400 km. away on the strike of this alleged compression is the central plateau of France where the Alpine compression is nil or negligible, and 400 km. away normal to the strike of the Alps the Apennines disclose strong compression in a quite different direction, of an amount which has not been closely estimated but would on this model surely be of the order of 10^2 km.

When such phenomena are closely analysed, it will be found that their geometry and plan relations can only mean strong movements in different directions *with differential rotation through large angles* in the plan view. It is impossible to escape the implication of great change of surface shape, bending, shrinking and stretching in the non-limited dimensions of length and breadth (that is in plan). This deformation might reasonably be far greater than the change of shape in the end section, and there seems no

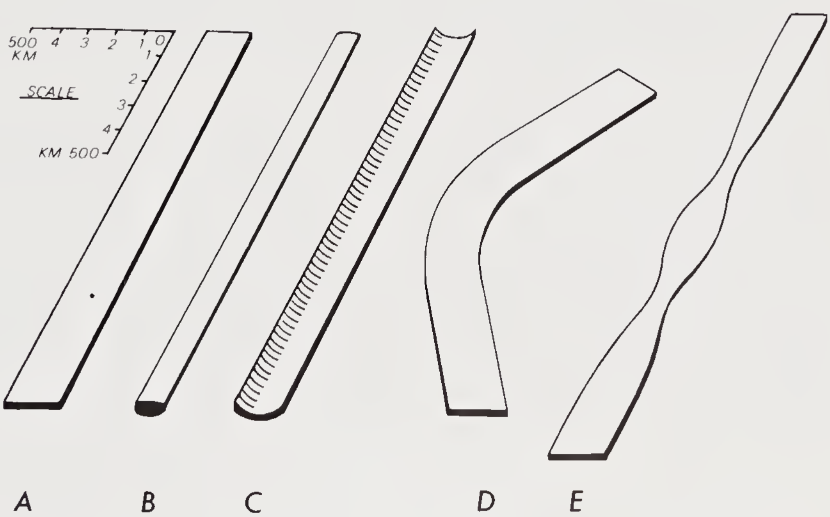


Fig. 34

The dimensions of an orogen.

logical reason why deformation by bending in plan to the form D of figure 34 might not occur, or stretching to the form E.

Diapiric orogenesis on a fixist earth escapes all these difficulties, but intercontinental movements either on a constant diameter or on an expanding earth necessarily yield oroclines. Cratonic blocks, because of their long relaxation times (hundreds of millions of years), may rift, separate, or shift along megashears. But when any such displacement reaches a currently active orogenic zone, bending or stretching occurs instead of rupture. This is necessarily so because such orogenic zones have some tens of kilometres of weak sediments in process of folding, and the crust and mantle below for a couple of hundred kilometres must be near melting temperatures, to produce the vulcanism and plutonism characteristic of these zones.

Plate tectonics theory regards the "plates" as rigid non-deforming sheets whose interactions occur at their mutual boundaries by separation (with growth of new oceanic crust), underthrusting (with elimination of crust), or transcurrent offset along megashears. Irrespective of earth expansion, plate tectonics has been deficient in not recognizing the deformation of the orogenic belts during the relative displacement of continental "plates". For example, the crust generation of the East Pacific Rise is taken only into the Gulf of California instead of making a triple junction along the Cocos Ridge and the Bartlett Trough; for assuredly the fit across the Atlantic shows that the Cainozoic crustal extension between North and South America is as great as the growth of new crust along the East Pacific Rise. But instead of a simple pattern of rigid blocks, the separation, and the large sinistral movement also implied by the Atlantic fit, yield oroclines, orotaths, sphenochasms, and megashears when crossing the orogenic belt. A similar pattern emerges in the other branch from the East Pacific Rise which passes south of Cape Horn to join the Atlantic and Indian ocean ridges.

The perimeter of each continental block may be extending or moving transcurrently as agreed by plate tectonicists, but it also deforms with oroclines and orotaths wherever the perimeter crosses an orogenic zone. The common perimeters between Africa and Europe have been widening and moving sinistrally, and have yielded several oroclines, orotaths, and megashears with intervening sphenochasms and rhombochasms. Likewise the common perimeter between Australia and Asia.

Euler's theorem

One of the theorems enunciated by Euler (1765), namely that any translation within the surface of a sphere may be wholly defined as a rotation about a specific pole on the surface, has become a cornerstone of plate motions in the theory of plate tectonics. However this theorem of Euler's does not apply to relative displacements on an expanding earth. For example, at one extreme, the surface could break up into first-order polygons, as in figure 6, or second, third, or fourth-order polygons as in figures 18, 20, and 21, and expansion and separation could occur, simply by radial movements outward. Similarly a sphenochasm could form by rotation about an apical point, but the arms, even if initially great circles, become small circles during the expansion.

SCALE OF TECTONIC PHENOMENA

Everyone would agree that in stepping from problems of geotectonics to problems of astrophysics, we must beware of the effects of the great change of scale on our mental models. Yet the whole range through global geology to astrophysics involves only three orders of magnitude, whereas *within* the field of rock deformation, structural geology and geotectonics, the linear scale varies through sixteen orders of magnitude (Fig. 35). The scale of time is as wide as the scale of size (Fig. 36).

Tectonic geology, in trying to understand the mechanics of orogenesis, necessarily relies on the mathematical relations established by the physicist and structural engineer. These relations are all empirical. Hooke's equation of elastic deformation, Newton's formulae of fluid flow, and the more complicated expressions for the deformation of the solid state, are no more than mathematical notations of observations on the deformation of laboratory materials. There is therefore no logical justification for assuming necessarily the validity of these relations outside the range of magnitudes of the observations on which they are based. Our concepts of solid, liquid and fluid, and the states of matter are like-wise empirical concepts. We may reason wrongly if we apply them far beyond the range of magnitudes of their empirical foundations.

Mechanics of geotectonics differ from experimental mechanics in the large magnitude of the loads, and what is even more important, in the very large magnitude of the duration of the loads. This neglected difference has, I suggest, been the Achilles' heel of geotectonic theory. Concepts based on experiments which last hours or days have been applied to phenomena whose duration is more than a thousand million times as long. Small discrepancies, which can be safely neglected in ordinary studies of deformation, prove to be direct functions of time, and in geotectonic phenomena become so large as to be the only significant terms in the result. Scrutiny of the time relations of the minor terms in experimental deformation leads to the induction that the earth behaves as a fluid for many geological phenomena where hitherto it has been regarded as an elastic solid.

Our thinking is done with models; concrete models such as a spheroid we can picture as the earth, but also by mathematical models; for when we write down symbols to represent the physical behaviour of the earth these symbols are also models, and however erudite our mathematical operations, the answer applies only to the model or terrella, and may have little relation to the behaviour of the real earth. Many false notions in geophysics

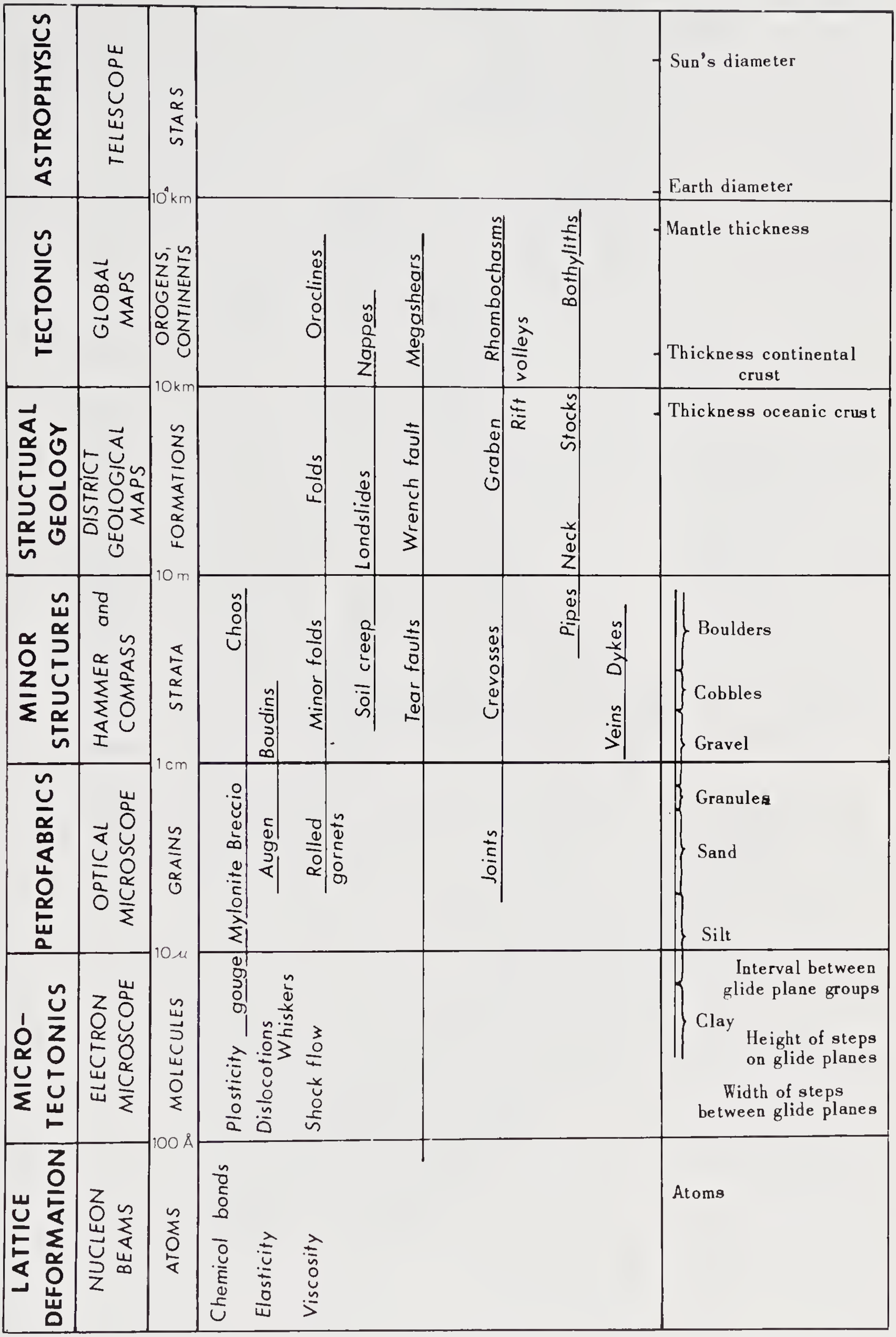


Fig. 35 Scale of geotectonic phenomena (from Carey, 1962).

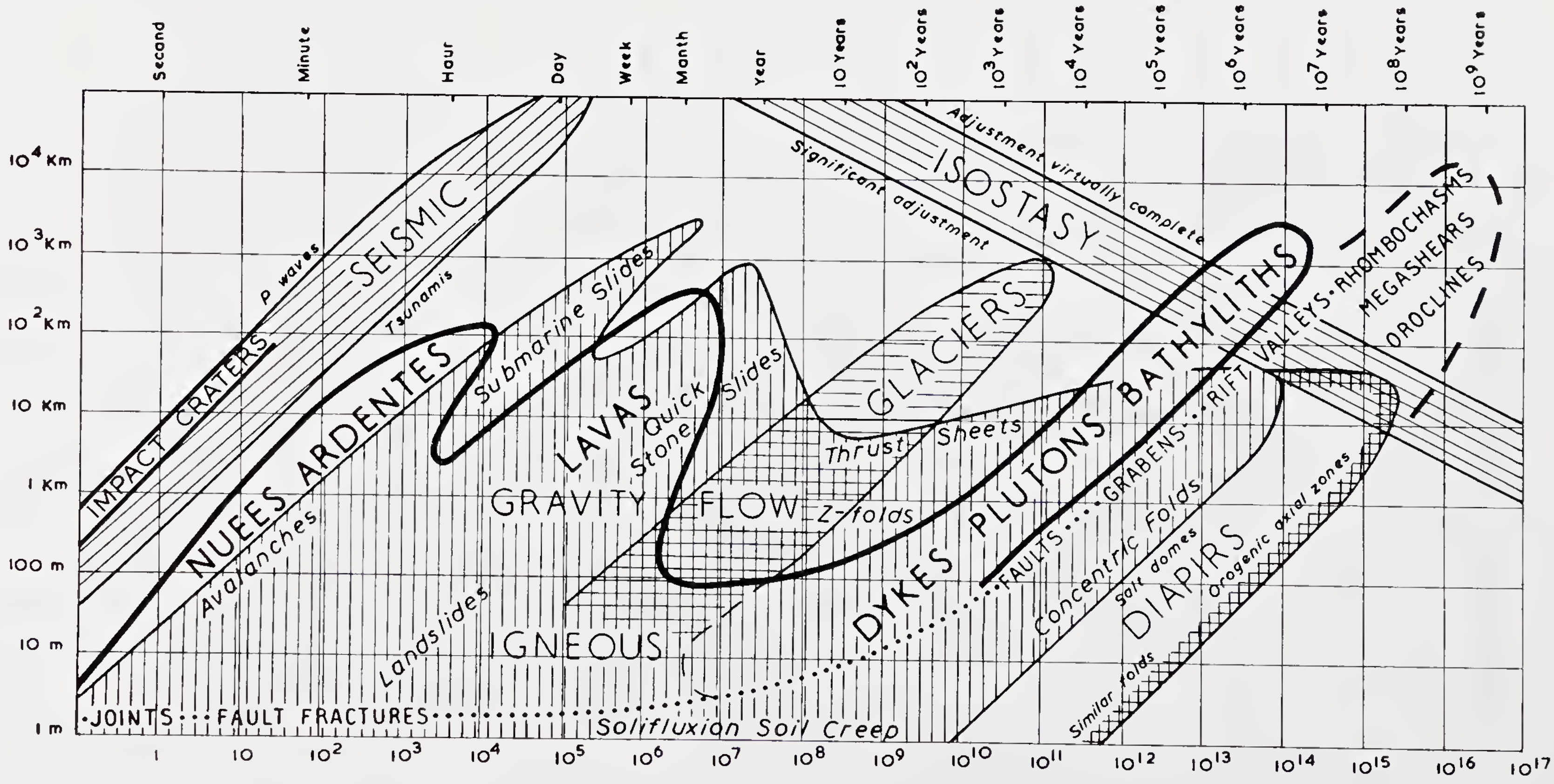


Fig. 36 Log-log length-time plot of geotectonic phenomena, from Carey 1962 (See also van Bemmelen, 1967 Table II).

have sprung from such validly derived mathematical conclusions, which would apply to an earth which possessed precisely and exclusively the properties represented by the adopted symbols.

Three kinds of error may result from applying empirical laws beyond their experimental range:

(1) In the first class a small correction is insignificant and perhaps undetectable unless the magnitude of some physical quantity, of which it is a direct function, becomes very large. Thus Newtonian mechanics gives correct results for ordinary velocities, but breaks down if the velocities approach the speed of light.

Two related comparable parameters may involve different powers of length or time, so that even with a mere thousand-fold scale range, one may increase by a million or a billion times the factor of the other. Such pairs are elastic and flow strain, or weight and strength. The most successful aeroplane, if faithfully reproduced on exactly twice the scale, could not fly. The weight would have been increased by eight and the wing area by only four. A double-sized aircraft can, of course, be built, but not to the same design. It is clear that hazards await us if we think of mountain deformation in terms of squeezing putty.

(2) In the second class a new type of phenomenon only appears when a threshold is passed by one of the physical quantities involved. Such are the melting and boiling points, the yield stress at which plastic deformation *(sensu stricto)* commences, the threshold of rupture, the sensitivity index of soils where a load-bearing stratum transforms to a slurry, the critical mass for the onset of fission, and the dimensionless numbers which determine the onset of turbulence, of convective circulation or magneto-hydrodynamic phenomena.

(3) In the third class the behaviour of an individual, or a number of individuals, contrasts with the statistical behaviour of large numbers of the same kind of individuals. Electrons, photons or locusts seen at close quarters behave as particles, but when the numbers are large, we see waves or swarms, and no individual photon or locust is identifiable in the group behaviour. If our experimental time-distance scale allows us to identify a particular electron or locust with significantly greater precision than the mean distance between individuals, the experiment reveals them as individuals, while any grosser analysis sees only the bulk phenomena. Crystal faces have hardness and lustre and many objective measurable properties, which have no existence when the individual atoms are probed with finer tools. The paradox of wave or corpuscular phenomena of light is not a fundamental conflict.

Which model is appropriate depends solely on the scale of our observations. Coarseness of time scale has similar bulking effect to grossness of scale of size. Thus the continuous rings in pleochroic haloes are long time integrations of single-particle collision stars which are recorded as individuals if the time-scale is short. Likewise flow of electric currents through conductive material, viscous flow and flow of fluids through permeable media are mathematically similar as stated by Ohm Newton and D'Arcy. But the phenomena may appear different because the relative magnitude of the analogous parameters may differ by orders, and in some we observe equilibrium states whereas in others we observe non-equilibrium transient states at leisure while flow proceeds towards equilibrium.

LENGTH-TIME FIELDS OF DEFORMATION

In view of these several sources of error - of behaviour discontinuities at thresholds, of the explosive expansion of importance of insignificant terms, the overwhelmingly different rates of change of related properties, and of statistical integration of random acts - our models, mental and mathematical, must be continuously selected to fit the particular scale of our deliberations at the moment. Our thinking would certainly be vulnerable if we tried to use the same kind of mental models through the fifteen orders of magnitude involved in the linear scale of geotectonics, which takes mass and volume through 10^{45}, a number too large to contemplate. In our past naivete we have often blissfully flitted through this range, in combination with as wide a scale of time, with little thought for the suitability of our models.

Brock (1972 p.11) has written: "Manifestly this is too drastic a jump for the nimblest mind. The intervening scales are the necessary steps in the ladder, and if a few rungs are missing there may be rude shocks in the shape of misunderstandings. Anyone brash enough to attempt to leap straight from the field to the global scale and back will almost certainly fail to find confirmation in the field of an alleged global feature. The failure proves nothing: the field scale features get lost on the globe and the global features get lost in the field. The difference is analogous to, say, a scale model of a large ore body and a microscopic section of the ore. The relationship becomes obscure. For the field man, who for one reason or another has not attempted global viewing, to dismiss a global feature as unreal is as wrong as disputing the evidence for atoms and molecules because one cannot see them."

How wide a scale span may be used? If we are brave enough, or rather sufficiently careful in our use of it, to consider in one span an interval as wide as the thousand-fold interval of astrophysics, there are *five* such intervals within the spread of structural geology and geotectonics. These are set out in figure 35 where the boundaries are drawn in arbitrarily. But usage has already produced some suitable categories, which may be extended into the following classification, each field of which embraces three orders of magnitude of size:

First order deformation: Continental and global structures. Tectonics.

Second order deformation: Structures seen on regional maps. Structural Geology.

Third order deformation: Structures seen on a field exposure. Minor Structures.

Fourth order deformation: Structures seen under the microscope. Petrofabrics.

Fifth order deformation: Structures as seen by atoms and lattices. Rock Mechanics.

In the fifth order, from 1Å to 10μ, we are concerned with the static and kinematic ideas of stress and strain, strength, the concepts of elasticity, viscosity, plasticity, deformation and flow, the forces between slip surfaces, single crystals and polycrystalline aggregates. This is the field of rock mechanics. The location of investigation is the laboratory, with theoretical analysis and induction. The electron microscope is our eye.

The next field, the fourth order, runs from 10μ to 1 cm. This is the field of petrofabrics, the study of the behaviour of grains, and the microscope stage is our arena. Here we are concerned with fabrics and textures, schistosity, foliation, lineations, augen, mylonites and gouges, as well as joints. We deal with the relations of individual mineral grains within rocks, rather than the relations between rocks. Here we must also concern ourselves with interstitial fluids, seepage pressures and flow nets, as well as with the consolidation of sediments, the behaviour of soils, and the phenomena of freezing of saturated materials.

The third order, from 1 cm. to 10 m. is the field of minor structures, which embraces everything we can see in a single exposure. The hammer, compass, clinometer and tape are our tools. Mylonites and gouges of the fourth order are replaced here by breccias, megabreccias and chaos - rock fragments instead of mineral grains. We study ptygmatic folds, boudins, rodding, and mullions. We abstract the results of the petrofabric scale and synthesize and interpret them here in terms of rock deformation structures.

We must recognize sedimentation structures and differentiate them from small scale tectonic structures. Solifluxion and landslides come in here, as well as outcrop-scale igneous bodies, such as veins, dykes, and pegmatites.

The second order, embracing structures from 10 m. to 10 km. is the range of structural geology in its narrow sense, and can only be studied by synthesis on regional geological maps and sections. The conclusions of our outcrop-scale observations are abstracted and symbolized on these maps, and from them regional structures are induced. Here we are concerned with folds, faults, and plutons, in their many combinations and permutations. Not rocks or beds, but stratigraphic formations are the deformed units. In the upper part of this range are horsts and graben, and the smaller nappes, and geanticlines.

The first order, from 10 km. to 10,000 km. is the field of tectonics, where our field of study is on continental and global maps. Structural geology and tectonics have been regarded as synonyms by many authors. In usage, tectonics has tended to denote structural geology on a large scale - the genesis and pattern of structures of a provincial scale. I therefore propose that this bias be crystallized, that structural geology be used for structures on the district map scale, for the relations of anticlines, synclines, faults and comparable structures, too large to be seen on a single exposure, but still on a scale on which beds and formations are represented. Tectonics should deal with still larger provincial structures, geosynclines, great transcurrent faults and thrust sheets, passing up to continental structures where the orogen itself is the deformed unit, the mid-oceanic ridges and rift systems, thence to the continents and ocean basins themselves.

In the smaller categories, horizontal and vertical are of little consequence in determining the pattern of behaviour. But in the second order gravity becomes increasingly significant, and in the first order it is dominant. Few structures can have vertical displacements of 10 km., and really large displacements depart little from geodesics. But there is little restriction in horizontal translations. On the tectonic scale proper, folds can only be about vertical axes (oroclines), as in figure 34.

So far as is practicable it is desirable to use distinct nomenclature, if not for each division, then at least to ensure that no term extends through too wide an interval. For thus the terms themselves would warn us of the change of scale and of different properties and patterns of behaviour, just as mound, village and clan evoke different images from mountain, city and race.

In general, analysis proceeds by abstracting the observations on the scale of the microscope slide, and then those of the scale of minor structures, and assembling these say in the scale of a mine, or of a single fold. Such compilations may be further abstracted and synthesized on the scale of the geological map and thence to tectonic scales of a province, a continent and the whole earth, and thence if you will to the solar system, and to the cosmos. However King (1973) has suggested "that we should reverse the conventional approach to the study of ore. Instead of starting with small pieces under the microscope, we should commence by contemplating the whole deposit as far as we know it, giving weight to shape and relationship to other rocks, and to other nearby deposits while reminding ourselves that possibilities which appear plausible on a microscopic scale may not bear thinking about on a scale of miles."

Within each scale range it is necessary to make a second spectrum of time, and to study independently impulsive and secular phenomena. The incidence of geotectonic phenomena with respect to both size and time is shown in figure 36 in which both scales are logarithmic. The maximum size and the maximum time in this diagram are limited respectively by the size and the age of the earth.

Most phenomena have fields which slope upwards to the right at 45°, the distance scale increasing in proportion to the time scale. In other words these fields express simply rates of propagation, or rates of deformation. The latter fields resolve therefore into an echelon sequence of viscosities, e.g. *nuées ardentes* with effective viscosities of the order of 10^{-3} poises, turbidity flows, 10^{1-4}, through various types of landslides to glaciers at 10^{13} poises, and on to salt and sediment deformation with viscosities through the range 10^{14} to 10^{21}, with an upper practical limit of flow at about 10^{27} poises, the viscosity of cold crystalline rocks.

The field of isostasy slopes downwards at 30° with increasing time, that is it is opposite in direction and flatter in slope than all the other fields. This expresses the fact that the wider the spread of a load, the more rapidly is adjustment reached. The 1 on 2 gradient is time against length squared. At the short time end, the isostatic field merges with the seismic field. The intersection represents the Maxwell relaxation time for earth materials. This is greater than a day (the semi-diurnal tides are entirely elastic), but less than a year (Fig. 121). Where these fields overlap and merge, both elastic and flow phenomena are significant, and elastic phenomena are partly relaxed and damped by flow.

In order to show how the diagram works, it might be useful to follow out a few examples. Following the upper edge of the isostatic field, and watching the time and size co-ordinates, it is apparent that a century would see virtually complete adjustments for loads involving the earth's figure. In a thousand years there would be complete adjustment of loads of the size of an ocean or a large continent, and noticeable adjustment of loads 2000 km is diameter. The recovery following the melting of the ice-sheets some 10,000 years ago is in accord with these scales. In 100,000 years adjustment would be virtually complete for loads 2000 km in diameter, and significant adjustment would be observed in loads 500 km in diameter. In a million years, adjustment of these latter loads would be virtually complete, and noticeable adjustment would have occurred in loads 50 km in diameter.

Phenomena whose fields overlap the isostatic field are accompanied by partial isostatic adjustment, *pari passu* as they grow, and those that extend above the field of isostatic adjustment, must remain substantially in isostatic equilibrium throughout their development. All of the first order tectonic structures fall within these categories.

Gravity flow of soils, sediments and rock is one great continuous field, with high speed avalanches, submarine slides and turbidity flows at one limit, and concentric folds, thrust sheets and nappes at the other. Flow of incoherent materials forms a lobe at the fast end. Continuous with the submarine slides is the field of quickstone flows where sensitive or thixotropic materials or sediments containing excess water pressures become quick and form large-scale flows either at the surface or below a nappe-sized raft of transported material.

The glacier field is continuous at the small scale end with solifluxion, which in turn grades into the field of soil creep.

The diapiric field includes salt domes, most similar folds, and the orogenic axial zones (see Carey, 1962a), and is continuous with the nappe field, as the nappes grow from the regurgitating axial zone (*loc. cit.*, Fig. 44) and hence the field of diapirically rising structures must pass without discontinuity into the lateral spreading of the nappes.

The igneous field has three lobes corresponding to gas-fluidized transport, surface flows, and sub-surface phenomena.

In general, the greater the scale the greater the dominance of gravity in producing flow phenomena. The driving force of weight increases with the cube of the linear scale whereas all flow and friction sliding depends on surfaces which increase by the square, and the flow and strength parameters of the materials do not change. Hence, when we consider orogenic belts and geotumours, gravity spreading by flow and nappes becomes universal.

GEOLOGICAL EXAMPLES OF SCALE FIELDS

Scale of global quanta

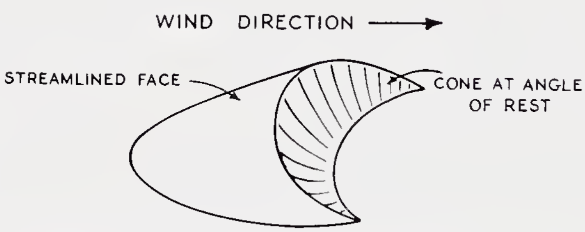
The earth has a crystalline mantle 3000 km thick (nearly half the radius), overlying a fluid core. Hence one should expect panglobal disruption to break the earth into chunks a few thousand km across involving the whole mantle. This is indeed the size of the several continental blocks (Figs. 6 and 20) and of the octantal rotations (Figs. 103 and 139). An earth with a thinner mantle would have produced more, smaller, continental blocks.

Tectonics on a smaller scale would see the earth as having a brittle crust a few hundred km thick and a yielding asthenosphere. Structural blocks within the primary continental blocks would therefore be a few hundred km across as in the second order polygons (Figs. 17 and 18) and the spacing of the Basin-and-Range, the Wasatch-Sevier, the Rio Grande, and the Cerralvo - Balcones - Luling rift belts (Figs.164 and 168), the mineralized lineaments of Billingsley and Locke (1939), and the deep faults of the Russian tectonicists.

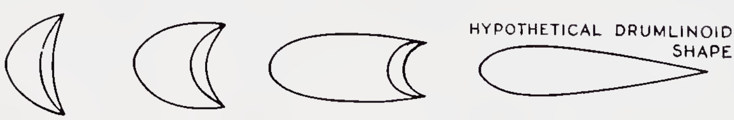
Dunes and drumlins

Sand dunes of the barchan type develop on plateau surfaces with such limited amounts of drifting sand that the wind scour is able to drive the sand into heaps which migrate slowly down wind. The dune is shaped by the intersection of two surfaces - a streamline surface like a hemi-paraboloid facing the wind, and a cone at the angle of rest where the sand tumbles from this surface on the lee side of the dune (Fig. 37). The air passes over the streamline surface by laminar flow. If the shape of the dune is not the precise streamline shape for that velocity there is either scour or turbulence until the dune is moulded to an equilibrium shape. If the wind velocity is increased the dune becomes more acute, lengthening in the direction of the wind, reducing the size of the conical portion at the angle of rest. Presumably, if the wind were fast enough, this conical section would vanish entirely leaving a completely streamlined shape with the broadest end facing the wind, tapering off to a point downwind (Fig. 37). A wind velocity of 200 miles an hour or more might be required to produce such a shape.

A drumlin is also a hill of loose material moved along on a rock platform under a moving fluid, namely, a thick glacier. It has a streamline shape with the broad end facing the moving ice and the tail end pointing down flow. Consider now the analogy between dunes and drumlins. Both consist of a deposit of incoherent material resting on a permanent platform and moulded to a streamline shape by the laminar flow of a fluid over the platform. Each structure migrates slowly in the direction of flow by the drag of the surface skin so that material is continually being stripped from the presenting face and added to the trailing face. Let us consider a dune in equilibrium with a wind of, say, 300 miles per hour, and assume that the air is replaced by another fluid having twice the kinematic viscosity. In order to preserve the equilibrium shape of the dune the velocity of the wind would have to be halved. Let us assume that again the viscosity is increased. Then, to keep the same equilibrium shape, the velocity would again need to be reduced. If this process were continued until the "wind" had the viscosity of glacier ice, the velocity of the "wind" might be comparable with the velocity of a glacier. The analogy can be extended to materials which accumulate behind projections - lee shadow dunes and "crag and tail moraines" respectively. However, the analogy should not be pursued too far for it is not perfect. The velocity and viscosity of the moving fluids are not the only properties which change. The surface velocities of glaciers have been measured, but the velocity at the sole of the glacier is not known with certainty. Limited data suggest that the sole velocity of a glacier is about 10^{-9} times that of the equivalent wind velocity for dunes, whereas the kinematic viscosity of the air is about 10^{-18} of that of glacier ice. Nevertheless, the analogy illuminates in a striking way the essentially fluid behaviour of crystalline glacier ice for phenomena of longer duration than a few weeks.



PERSPECTIVE SKETCH OF BARCHAN SAND DUNE



EFFECT OF INCREASING WIND VELOCITY ON SHAPE OF BARCHAN

Fig. 37 Parallel kinematics of dunes and drumlins.

Slow and slower tectonics

Consider a landslide onto the surface of a valley glacier (Fig. 38). The landslide movement commences, accelerates, peaks, and finishes, all in an hour or so. The glacial flow operates without interruption during this hour, but its total movement during this time is negligible. The gravity movement transverse to the glacier flow is the only significant factor controlling the landslide structures -- the tension gapes at the head, the lateral transcurrent tears, and the frontal bulges, folds, and overthrusts with longitudinal extension faults. However, during the ensuing decades the persistent perennial glacier flow deforms the landslide burden on its back, so that in due course the simple shear strain superimposed on the landslide flow-structures equals, then exceeds many times, the deformation imposed by the initial landslide movement. Faults, axial surfaces, and lineations, developed in the first structural system, are rotated through large angles, and a reference strain circle drawn on the landslide mass elongates into an ellipse, eventually of such eccentricity that it becomes stretched into a loop many kilometres long and only millimetres wide.

To the layman a time as short as a picosecond (10^{-12}) is virtually instantaneous. But to a particle physicist a duration a million times shorter is still a very long time interval in relation to his interactions. Likewise to the layman geological processes taking a million years are very slow indeed. But a tectonicist must differentiate such processes from other concurrent processes many orders slower.

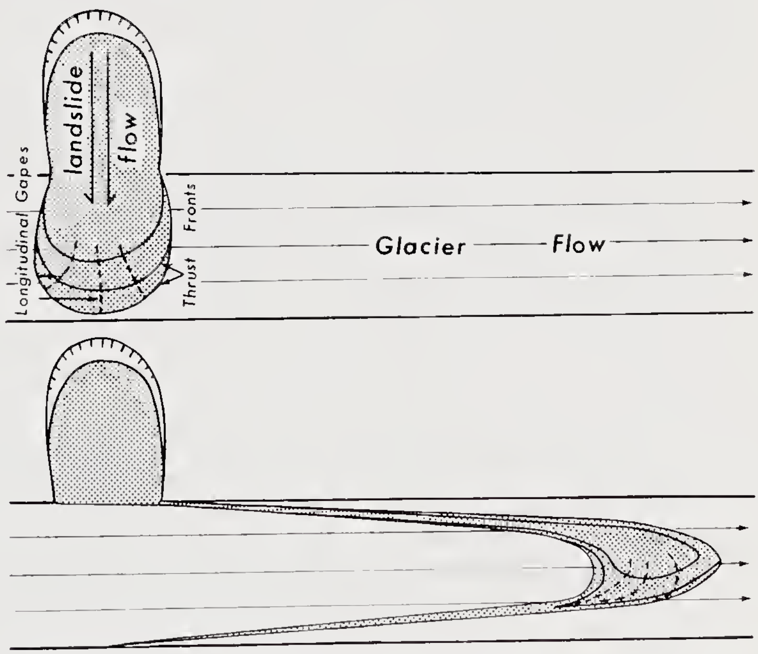


Fig. 38

Landslide on to glacier. Overprinting of slow and slower deformations.

For example, the diapiric extrusion and gravity outflow of orogenic nappes of the Laramide pulses each occupied part of an epoch. But superimposed on this, before, during, and after the orogenic pulses was the slow persistent dextral shear, which has dominated Cordilleran tectonics throughout the Phanerozoic (Figs. 139 and 140). Analysis of structures in and near the Rocky Mountain Trench is similar to analysis of the structures of the deformed landslide mass drawn out by the slower but cumulatively greater motion of the glacier (Fig. 38). The gravity flow of the Laramide nappes is the analogue of the landslide, and the secular dextral couple is the analogue of the glacial deformation.

Reynolds' number thresholds

Reynolds' Number, so named after Osborne Reynolds who demonstrated its significance in 1883, is an empirical ratio of fluid velocity (v) times kinematic radius (l) to kinematic viscosity (ν). It determines the threshold from laminar to turbulent flow.

Text-books of physics are vague concerning its real physical meaning and usually state intuitively that it expresses a balance of viscous and inertial forces, or of momentum. This is not correct. The relevant parameters are the viscosity and elastic rigidity. Moreover, Reynolds' criterion can be deduced directly from first principles. The failure to recognise the physical meaning of Reynolds' criterion has been due to the common false assumption that liquids have zero rigidity, which itself arises from failure to appreciate the scale time-fields of empirical phenomena (see below).

Laminar flow involves shearing couples between flow laminae. Couples which are not constrained rotate. Hence laminar flow is only possible if there is a constraining wall which prevents rotation. Reynolds' Number expresses quantitatively the condition that an applied shear couple must be transmitted to the wall before it relaxes. Put another way, the condition for laminar flow is that there shall be an effective constraining wall, and for the wall to be effective the shear stress must be transmitted to the wall in a time much shorter than the relaxation time for the substance.

Consider a substance of viscosity η, shear modulus μ and density ρ moving with velocity v at distance l from the wall.

The condition for the wall to prevent rotation is that the shear must be transmitted to the wall before it can decay significantly by viscous relaxation.

The velocity of transmission of a shear is $\sqrt{\mu/\rho}$, hence the time required for a shear to reach the wall at distance l is $l\sqrt{\rho/\mu}$.

The time required for a shear stress to relax to $^1/_e$ of its magnitude is η/μ.

Hence the shear stress actually reaching the wall will be relaxed to $^1/_e$ of its magnitude when

$$\eta/\mu = l\sqrt{\rho/\mu}$$

$$\text{i.e.} \quad \mu = \eta^2/\rho l^2 \qquad \ldots \quad \ldots \quad \ldots \quad (1)$$

The velocity difference across laminae unit distance apart is v/l where v is the velocity at distance l from the wall.

In viscous flow the stress required to cause this velocity difference between laminae unit distance apart is $\eta v/l$.

By Hooke's Law this stress equals μs where s is the elastic strain maintained across the laminae during flow *

$$\therefore \quad \mu = \eta v/ls \qquad \ldots \quad \ldots \quad \ldots \quad (2)$$

equating (1) and (2)

$$\eta^2/\rho l^2 = \eta v/ls$$

$$s = \frac{\rho l v}{\eta}$$

$$s = \frac{vl}{\nu} \qquad \ldots \quad \ldots \quad \ldots \quad (3)$$

where ν is the kinematic viscosity. ($\nu = \frac{\eta}{\rho}$ by definition).

The quantity $\frac{vl}{\nu}$ is a pure number (as is strain). By its derivation above it is the strain across laminae unit distance apart when $^1/_e$ of the axial stress reaches the wall. But if only $^1/_e$ of the stress reaches the wall one would not expect this to be sufficient to prevent rotation of the flow couples. To prevent rotation (turbulence) some factor (R) times this much reach the wall. If less than $^R/_e$ of the stress reaches the wall (through increase in velocity, or of distance to the wall, or decrease in viscosity) the wall cannot prevent rotation and rotational turbulence will occur.

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* Consider the mechanical analogue of a spring in series with a dash-pot in which flow can be maintained in the dash-pot by maintaining a constant stress in the spring, or the electrical analogue of a resistance in parallel with a condenser, in which a constant dielectric "stress" is maintained in the condenser while the current flows. The voltage across the condenser is the same as the voltage across the resistance during current flow. Similarly the positive shear stress (analogue of voltage) maintained across the fluid equals the Newtonian shear stress across the fluid during flow.

As $\frac{vl}{\nu}$ approaches zero (i.e. viscosity approaches ∞, or v or l approaches zero) the proportion of the shear stress to reach the wall approaches unity and deformation approaches perfect laminar flow, as the amount of rotation of the shear couple approaches zero.

As $\frac{vl}{\nu}$ increases (by reduction of viscosity or increase of velocity or of kinematic radius) the proportion of the shear stress reaching the wall diminishes towards zero and the constraint on rotation of the couple diminishes. At values of Reynolds' Number (R) in excess of about 2000 rotation is so advanced that the flow is turbulent and eddies form.

When the threshold of turbulence is passed, there are always zones adjacent to the walls where the flow is laminar. The velocity at the wall is zero, the distance to the wall is zero so the local Reynolds' Number at the wall is zero - hence no turbulence. As distance from the wall increases velocity also increases and, at a distance where the Reynolds' Number exceeds the turbulence threshold, eddy rotation occurs.

Laminar flow is a theoretical unattainable ideal. There is always some relaxation and hence some waviness of the flow-laminae but for practical purposes this may be too slight to be observable at low values of R.

Rigidity of liquids

Many textbooks state that liquids have zero rigidity. The velocity of propagation of an elastic compression wave (e.g. a sound wave) is $V = \sqrt{\frac{k+4/3\mu}{\rho}}$ where k is bulk modulus, μ is shear modulus (rigidity) and ρ is density.

For liquids μ is assumed to be zero, so that the velocity becomes $\sqrt{\frac{k}{\rho}}$. The fact that this usually gives the correct answer is interpreted to mean that the rigidity of fluids is actually zero. What this really means is that there is no *effective* rigidity because the shear stress relaxation time τ is very much shorter than the period of the propagated waves. If the period of the shear waves is decreased until it approaches the relaxation time of the fluid, the shear stress is incompletely relaxed within the cycle and the rigidity term starts to become significant. In fact rigidity is intrinsically related to specific volume so that we might expect the rigidity of water to be of the same order and perhaps a little higher than that of ice. But as the relaxation time $\tau = {}^{\eta}/_{\mu}$, the relaxation time of water would be less than a picosecond, so that in all experiments or oscillations longer than a picosecond, water would have no *effective* rigidity, not because water lacks rigidity but because the elastic shear stress is relaxed in much shorter time than the loading cycle.

Table V shows the approximate orders of magnitude of rigidity, viscosity, and relaxation times for upper mantle, salt, rubber, ice, silicone-putty ("sili-putty"), glycerine and water.

TABLE V

	Rigidity μ dynes/sq.cm.	Viscosity η poises	Relaxation Time τ seconds
Upper mantle	10^{11}	10^{22}	10^{11}
Salt	10^{11}	10^{18}	10^{9}
Rubber	10^{9}	10^{15}	10^{6}
Ice	10^{10}	10^{13}	10^{3}
Silicone-putty	10^{9}	10^{7}	10^{-2}
Glycerine 27°C	10^{10}	10^{0}	10^{-10}
Water	10^{10}	10^{-2}	10^{-12}

For any process or phenomenon where the duration of the stress-difference is an order less than the relaxation time, the substance will appear to be elastic. Thus with salt in loadings up to a year, ice with loadings up to a minute, rubber up to some months, "sili-putty" up to a small fraction of a second, and upper mantle up to hundreds of years, only elastic phenomena appear significant. A ball of sili-putty bounces just like a rubber ball because the elasticity is about the same as rubber, and the duration of the loading is something like one thousandth of a second. To observe the quite real elastic behaviour of water, the loading cycle would have to be completed in less than a picosecond. The stresses from the collapsing cavitation bubbles of a fast-turning propellor may achieve such short loading times, which may explain the cavitation pitting of metal propellors when the water-metal bonds are forcibly broken in times too short for the mutual stress to be relaxed by flow.

For any process or phenomenon where the duration of the stress-difference is an order or more shorter than the relaxation time, the behaviour of the substance will appear to be elastic and brittle (if the stress-difference exceeds the shear strength).

Thus for phenomena which maintain the loading for longer than an hour, ice behaves as a fluid - hence glaciers. For phenomena maintaining loads for a century, salt appears as a fluid, hence salt diapirs, and the salt "glaciers" of Iran. Sili-putty, which bounces like rubber, nevertheless flows when you squeeze it (loading greater than a fraction of a second). Continents appear to "float" isostatically in the upper mantle for loadings sustained for a few thousand years. So far as this criterion is concerned mantle convection is acceptable (see Figs. 121 and 122). We do not normally see any but fluid behaviour in water, for it is very difficult to conceive an experiment wherein the mechanical loading is applied, detected and unloaded in less than a picosecond.

However Raman and Venkateswaran (1939) found experimentally that supersonic P waves with a frequency of 10^7 hertz experienced no rigidity term when transmitted through glycerine at 27°C and had a velocity corresponding to $\sqrt{\frac{k}{\rho}}$ but hypersonic P waves with a frequency of 10^{10} hertz experienced an effective rigidity term and were transmitted with a velocity corresponding to $\sqrt{\frac{k+4/3\mu}{\rho}}$. They reported a rigidity modulus for glycerine (27°C) of 2.4×10^{10} dynes/sq.cm and a relaxation time of 350 picoseconds. As expected, this rigidity agrees closely with the reciprocal of the directly observed adiabatic compressibility of the glycerine. At higher temperatures the viscosity of glycerine falls very rapidly so that the shear stress relaxes within the period of the vibration cycle, and hence the velocity indicates no effective shear term. Raman and Venkateswaran comment: "Our observations thus seem to establish, experimentally, the thesis that for mechanical disturbances of sufficiently high frequencies, a liquid behaves essentially as a solid." The converse is also true, that for sufficiently long periods solids behave essentially as fluids (Table V).

Size limits of meteorites

Meteoritic impact illustrates the sensitivity of scale. Meteorites should not be found smaller or larger than a critical range of size. Those smaller than a critical size entering the earth's atmosphere at interplanetary speeds burn up and ablate as "shooting stars" in the upper atmosphere. Their vapour trails may dissipate as dust or condense as micrometeorites which fall at lower velocities determined by Stokes' law, and are found at specific horizons in ocean sediments. The smallest to reach the earth in direct descent are marble-sized, and after ablating their incandescent leading edges, their terminal velocities are reduced by air drag to speeds no greater than hailstones. They strike the earth with little damage to themselves, and lie on the surface where they fell, even on soft ground.

As the size of the meteorite increases, the atmospheric retardation of the entry velocity diminishes (Stokes retardation is proportional to diameter but momentum is proportional to its cube) so a size is reached where a stable terminal velocity is not attained before impact. In this range (up to a few metres) the meteorite body will be found in one piece or in fragments.

Beyond this size, atmospheric retardation becomes less significant, and the impact velocity rapidly approaches the initial entry speed. A meteorite 100 m in diameter reaches the earth with kinetic energy in excess of 10^{20} joules, which is at the upper limit of the most energetic earthquakes and the most powerful hydrogen bombs. The whole of this energy must be dissipated on impact, within seconds. The whole of the meteorite and a similar mass of the shocked earth is not merely melted, but volatilized and ionised. The ensuing physical phenomenon of the expanding fireball is identical with that of a megaton nuclear explosion (except for the absence of radiation and radioactive particles). There is no possibility of finding a piece of the intruding meteorite, although condensates from the ionised rock may perhaps form micrometeorite and impactite strewn fields.

The impact site would, of course, be indelibly branded by its astrobleme crater and peripheral zones, recording permanently the several degrees of melting, shock deformation, and fracture, but it would be futile to search there for fragments of the meteorite. A size range would also be reached where even an ocean impact could not dissipate the energy without a plasma explosion.

Reverting to the question of scale, it is clear that even if we assume a meteorite flux evenly distributed in size from microns to many kilometres, we would only find meteorite fragments in the size range from a centimetre to a couple of metres, and condensation products in *their* own size range.

Explosions

The scale of impulsiveness, or of rate of energy release per cubic centimetre, determines the kind of phenomena which ensue. Nuclear, chemical, volcanic, and impact explosions produce large impulsive pressures, which propagate as a shock, which degrades through six modes as the impulse diminishes. In the innermost zone the energy release is so rapid that atoms are ionized. Surrounding this the rock is volatilized, but not ionized. As this absorbs large amounts of energy in specific heat, and latent heats of melting and evaporation, and breaking of chemical bonds, and as the energy so absorbed increases as the cube of the distance propagated, this mode soon drops to the zone of flash melting. The energy absorption is still high, but when the front reaches the limit of melting, the energy being propagated is still higher than can be transmitted by elastic waves. Any given rate of strain in the transmission of an elastic wave corresponds to a level of stress. If this stress exceeds the yield stress, plastic flow ensues. If the stress exceeds the cohesion, pulverization occurs. Hence surrounding the zones of ionization, volatilization, and melting there is a zone of pulverization, which is surrounded in turn by a zone where energy transmission causes plastic deformation of the mineral lattices. This plastic shock front travels at higher velocity than the fastest elastic waves. Finally energy absorbed by the rock volume within the expanding shock envelope reaches the level where the maximum stress involved in the transmission can be borne elastically without lattice failure. From there out only elastic waves transmit the energy. At first, however, intermittent shear failures occur where reflected or refracted waves from local interfaces interfere with outgoing waves to produce local stresses exceeding the cohesion of the material. This is the zone of so-called shatter cones associated with astroblemes. The axes of such cones point back to the shock origin, and the cone angle is $90°-\phi$, where ϕ is the angle of internal friction.

The radius of the inner hyperelastic zones may be metres in nuclear explosions, or kilometres for large astroblemes. But even in quite small shocks, the zones may all be present, even if only at atomic distances. For example, the pale grey pulverized zone is visible for a few centimetres round a moderate gelignite explosion; even striking a rock sharply with a hammer causes volatilization of a few molecules, so that a sensitive nose may detect distillation products from petroleum residues in a limestone or the garlic odour of volatilized arsenicals from arsenopyrite, or only the "blasting smell" characteristic of shocked silicates.

Brittle or ductile failure

It appears that a parameter similar to Reynolds' turbulence criterion determines whether impulsive loading is relieved by plastic flow or pulverization. If the load can be relieved by lattice reordering before stress builds up to exceed the cohesion, then plastic flow occurs instead of powdering. Graphite, ice, salt, and gypsum have low thresholds, calcite intermediate, and silicates generally high. These parameters diminish with temperature and confining stress.

Plastic deformation of metals or gypsum is quite easy under moderate conditions. Marble can be extruded plastically under laboratory conditions, but the rock-forming silicates powder instead of deforming. However in the appropriate scale fields, which require either high confining pressure, high temperature, very high rate of shock loading, or very long time duration,the silicate minerals flow plastically, as do metals under normal surface conditions. It is simply a matter of the scale of the relevant parameters.

Hence, plastic slip lamelli, which are so abundant in the quartz, feldspar, and pyroxenes of intensely shocked lunar rocks, do not occur in volcanic explosions, even from deep diamond diatremes, but they do occur in astroblemes and they do occur in deformed marble.

EXPERIMENTAL TECTONICS

Problems of stress, strain and rupture are difficult and still far from rigorously understood even for static analysis,and still less so under dynamic conditions of large strain over long duration. Even where theoretical principles are well understood, the boundary conditions of real cases become too complicated to analyse even with the numerical capacity of a large computer.

Engineers, after exhausting their capacity for theoretical analysis, usually resort to scaled models with sizes, loads and durations reduced by many orders of magnitude. Engineers, whose structures may be measured in kilometres and relevant durations in centuries have had conspicuous success (and some failures) in using scale models to solve their problems in aerodynamics, hydraulics, and the stress distributions associated with large loads, and underground openings. As the equations of electrical current flow and potential distribution (e.g. Ohm's Law) are widely similar to the equations for fluid flow through porous media (e.g. D'Arcy's Law), to the flow of heat through thermal conductors, and to the deformation and rupture of materials under mechanical loads, electrical analogues have been made to predict behaviour in other systems. For these purposes resistivity is the analogue of viscosity, potential is the analogue of stress, capacity is the analogue of strength, dielectric constant is the analogue of elasticity, amperage of deformation rate, quantity (coulombs) the analogue of strain, and spark breakdown the analogue of rupture.

Tectonic geologists are concerned with structures three orders larger, loads nine orders larger, and times six to eight orders larger, than those of the engineer. They too have used models, from the earliest fiddling with paper, sealing wax, or plasticine, and wet clay, with varying degree of awareness of the kinds of hazards set out above (small unseen terms, different exponents of parameters, the possibility of unexpected behaviour thresholds, and statistical smoothing of individual behaviours) although many have been wholly blissful of the pitfalls. M. King Hubbert's classic paper (1937) systematically stated these problems and set out sound guidelines for all subsequent tectonic experimenters. His propositions have been refined in details by others since but his basic principles stand.

Sir James Hall (1815), Daubree (1879), Bailey Willis (1892), Cloos (1930), Nettleton (1934), and more recently Ramberg (1963, 1967), and some of the Russian institutes, stand out as leaders in experimental tectonics.

Mauna Ulu plate-tectonics model

Duffield (1972) has presented an excellent colour film and detailed description of the kinematics of the solidified skin on the surface of the lava column in the crater of Mauna Ulu, a satellite vent of Kilauea on Hawaii. Circulation of the lava column from convection, degassing, and other movements, transports the crust passively from rising areas to sinking areas. The crust, a centimetre or so thick, breaks up into rafts some tens of metres across, whose boundaries grow at the rising separating zones, converge, sink and disappear at the confluent zones, and show active triple junctions, transform faults, and some oblique shear faults, at other boundaries. All in all they make an impressive demonstration of the mechanism claimed for earth crustal plates.

Duffield goes on to use the methods of Hubbert (1937) to show that the scale ratios of length, velocity, and viscosity do correspond as closely as the parameters are known with the requirements of a valid model. Duffield is careful to add: "Given the present level of uncertainties in both global and lava-column plate tectonics, the two situations may not satisfy the more rigorous criteria of both mechanical and thermodynamic similarity (Hubbert, 1937). Nevertheless it seems unlikely that any actual scale model of global tectonics, as it is now understood, that is more nearly accurate than the naturally occurring one at Mauna Ulu could be constructed."

However, this statement is misleading. Duffield's comparative analysis applies to the lava column and the mantle. Further, he is quite correct in claiming that viscosities, velocities, distance, and time are reasonably scaled. But there is a misfit of 10^{24} in the Reynolds' numbers. The lava column is far on the turbulent side of the threshold, whereas the equivalent mantle is far on the laminar side of the threshold. It is quite invalid to suggest that a system which is certainly turbulent, even begins to model a system where flow is certainly laminar.

Duffield does not examine the modelling of the lithosphere by the thin basaltic glass skin, even though it is only the superficial skin that shows the patterns and structures analogous to plate tectonics. Here the scaling factors for thickness to surface length differ by about 10^7, and viscosity by 10^{14}, give or take an order or two in each case. There is a large difference in Reynolds' numbers but this is not material because both are on the laminar side of the threshold. But the hundred million million difference in scaling factor for viscosity *is* significant and the lava skin could not begin to model the lithosphere. Whereas the lava skin does bend down and subduct because of its lower scaled-viscosity, consumption at a convergent lithosphere boundary (if such exists anywhere on earth) would occur, not by bending, but by brittle fracture and sapping, as described by me long before plate subduction was conceived (Fig. 5).

The pattern of plates, accreting boundaries, convergent subducting boundaries, and transform faults in Mauna Ulu, are real and exciting. But they can not model lithosphere and mantle. Subduction, and lithosphere consumption, exists only in minds, minds which have transformed scales from mental models to their imaginary earth, without respect for the constraints of scale.

Laboratory models of mantle convection

Turner (1973) has modelled mantle convection using glycerine as his working fluid, because its viscosity varies through four orders of magnitude with temperature. This is an important advance, because the viscosity of the mantle must vary with temperature (and pressure) through four or five orders and by several orders more if the crust is included. Most mathematical and experimental models of earth convection have been irrelevant to reality because they could not provide for the wide variation in viscosity within the circuit. Turner's models have successfully reproduced the generation, translation, and subduction deep into the "mantle", of a thin skin of chilled glycerine. His models conform at least approximately to Hubbert's scaling factors and to the constraint of Reynolds' number. However his models are driven not by density differences from thermal cooling, but by pellets of carbon-dioxide ice which yield a stream of rising bubbles, which drag the system with them. So far as the kinematics and kinetics are concerned, the system is driven by overall density differences and it should be immaterial how these density differences are produced. However, the CO_2 complicates the system thermally in that the rising column, although warmer than the descending column, nevertheless is much cooler (and hence more viscous) than the "mantle" generally, which therefore certainly departs significantly from nature, not only thermally, but also mechanically.

Still another model of lithospheric plate motions has been produced by Oldenburg and Brune (1972), but their hot wax model is not driven by subjacent mantle circulation, but by mechanically applied surface extension at a controlled steady rate. Hence it would model either an expanding crust, or the expanding section only of the "plate tectonics" concept. The several scaling factors including Reynolds' number are reasonably satisfactory, except that the viscosity scaling in the "lithosphere" is somewhat too large, but the effect of this would appear in the subduction (if any) which is not involved in this experiment. This model reproduces excellently the orthogonal pattern of spreading ridge and transform faults, along with some interesting corallaries. Symmetry or asymmetry of growth at the spreading zone depends on the rate of spreading, and "spreading ridges may be formed under the influence of tensile stresses only, and forces from an active convection cell located beneath the ridge axis are not required."

OTHER PLANETS

Figure 35 is, of course, for earth phenomena. It has nevertheless implications for tectonic phenomena on the Moon, Mars, and Venus. Assuming similar materials in respect to deformation and strength parameters, the principal variable would be gravity. On a graph drawn for one of the bodies smaller than Earth the slopes of all fields would be the same as on figure 35, and fields (such as the seismic field) which do not involve gravity would remain unchanged in position, fields involving gravity and distance to the first order (such as rock flow) would be shifted to the right by the ratio of the new gravity to Earth gravity, and fields involving gravity and area (e.g. isostasy) would be shifted twice as much to the right.

Mars has a gravity 0.38 of Earth. The isostatic field would be expected to shift nearly one order to the right, gravity flow deformation, landslides etc., about one third of an order to the right, and the seismic field would not be displaced. Hence the time required for isostatic adjustment on Mars would be something less than one order longer than on Earth. The result is completely in accord with the conclusion of Lamar (1961, p.26) that the comparative ellipticities of Mars as determined optically and from the motion of the two satellites, indicate that the Martian equatorial bulge is isostatically compensated, and that fluid equilibrium must prevail at depths greater than 250 km. More recent data of Schubert and Lingenfelter (1973) support this interpretation.

Moon has a gravity field only 0.16 of the Earth, so the field displacements would be greater, e.g. the isostatic time would be one and a half orders longer than on Earth. Hence other parameters being equal, all major features on the moon would be in isostatic equilibrium, and loads 100 km in diameter would be substantially adjusted in 100 million years. This accords with the conclusion of Carey (1960, p.311) concerning the figure of the Moon.

This argument disregards an important variable - temperature, and would only be true if the temperature gradient from the surface was similar to that on earth. If the moon is really a cold body throughout, then time for adjustment would rise by several orders.

Planetary bodies suffer many scale thresholds. All phase transitions are governed by pressure and thus by gravity gradient and thus by total mass. Small planets, even of the same composition, could not have the same depth sequence of phases as more massive planets, nor would they go so far along the range. If core formation involves a phase change there would be a minimum diameter for planets with cores. Likewise some planets could establish convection cells whereas others could not depending on the rate of heat generation diffusisity and effective viscosity. If planetary magnetic fields are correctly interpreted as magneto-hydro-dynamic self-excited dynamos, a limiting dimensionless constant governs the onset because the scale must be large enough that the condensation of the magnetic field by fluid motions occurs more rapidly than the field can decay. Small asteroids in which the shear strength of their rock exceeds their own gravitational weight do not attain a spheroidal shape, and likewise there is a threshold size below which they would not be disrupted if they came close to a planet which would disrupt a larger asteroid if it transgressed the mutual Roche limit.

NON-UNIFORMITARIANISM

The concept of uniformitarianism goes back to the earliest beginnings of science; but interlarded with it repeatedly through the centuries were contrary ideas of unique events, catastrophies, and intervention of the gods. Thus to Archbishop Ussher, only a couple of centuries ago, the marine fossils which were inconveniently dug up from time to time high in the mountains, had been either deposited by Noah's flood, or inserted by the devil to tempt the faith of believers, or left there by itinerant fish-eating merchants or armies. Yet Xenophanes of the Ionian school five centuries before Christ, correctly interpreted the occurrence of sea shells and fish remains far from the sea as remains of extinct creatures. Likewise Herodotus understood the formation of deltas and estimated that it would take some ten thousand years to fill up the Red Sea with silt if the Nile opened into it. Robert Hooke, three centuries ago, concerned with the winning of coal, built up a sound frame of ideas on the origin of sedimentary rocks and the fossils in them, based on observations of currently observable processes. During the following century, the great Russian poet and scientist, Mikhael V. Lomonosov, and the German pioneer, C.C. Füchsel, also recognized that only the normal observable phenomena of nature could be taken as models for natural processes of the past; a Scottish contemporary, James Hutton, asserted that past geological phenomena should be interpreted in terms of currently acting processes. He could see "no vestige of a beginning and no prospect of an end" in the evidence of the rocks, and stated the canon that "the present is the key to the past", destined to become the fugue of uniformitarian polyphony.

The principle of uniformitarianism, thus spawned, has become the cornerstone of geology. And rightly so. But it has also ossified a lot of error. Tennyson has told us that each vice an elder virtue leads. The converse is also true. Each coin has two sides. We must also know the other side.

Overstatement of the principle was a product of the controversy of the times. Sir Charles Lyell, the accredited patriarch of the principle, faced the dogma of castrophism, proclaimed by Alcide d'Orbigny and Baron Cuvier. In this, as so often in that century, the savants of the French Academy and the fellows of the London Royal Society were jealous rivals in controversy Cuvier was extraordinarily brilliant over a wide field, and meticulously accurate and thorough in his observations and deductions from them; his work on *Le Règne animal* and his systematics of the tetrapods stand as monuments in the development of palaeontology. He scorned the speculative theories of his time but accepted without question the biblical dogma (he was later Grand Master of the faculties of protestant theology of the University of Paris). Hence, faced with the succession of faunas he saw in strata, he interpreted each assemblage as a separate specific creation, to be destroyed in totality at the end of each epoch by a divine catastrophe, of which Noah's flood was but the latest example. Each destruction was followed by a totally new and improved divine creation - like a sequence of Fords from T to energy crisis. Needless to say he totally rejected the emergent theories of evolution. Indeed anatomical study of a parade of Fords gives as good an evolutionary sequence as do the tarsals of five-toed *Eohippus* to farmyard Dobbin stilted on his middle toe.

Such catastrophism was the seed-bed of Lyell's uniformitarianism. He chose for the title of his great book *The Principles of Geology* (1830) with the subtitle *"being an attempt to explain the former changes of the earth's surface by reference to causes now in action"*. Thus he established the principle of uniformitarianism, recognised a generation earlier by Lomonosov, Füchsel and Hutton, although not so-named until later. "Actualism", derived from the French usage of *actuel* to designate currently existing things and states, is commonly used as a synonym for uniformitarianism.

In its historical context, the principle of uniformitarianism had to be, and had to be overstated, as an antidote to the entrenched false dogmas of catastrophism, fortified as they were by theological sanctions. Most geologists who have thought about it have realised that qualifications were necessary. Lord Kelvin, always ready to debunk geologists, commented that uniformitarianism was in essence perpetual motion, and therefore impossible. Newell (1967) defended Cuvier and d'Orbigny, pointing out that the staccato record of palaeontology is punctuated repeatedly by sudden extinctions, and that the record of life is in fact more episodic than uniform. In fact, right from d'Orbigny, palaeontologists have leaned more towards catastrophism, neocatastrophism, or episodism, through geological time, than have geologists in general.

Many analyses have distinguished between uniformitarianism in the application of natural laws and lack of uniformity in the environment in which they operate, between "methodological" and "substantive" uniformitarianism, or "immanent" and "configurational" uniformitarianism. But even to this day the principle of uniformitarianism is much too rigorously applied by the great majority of geologists, to the detriment of our science.

Uniformitarian beliefs form the foundations of our language. Thus *oxygen* means "life-generator." Yet at the ultimate source of life in the early Archaean, the direct opposite was true, for as Cloud (1974) has stated:

"Free oxygen in more than evanescent amounts would preclude the origin of life in several ways. Essential precursor molecules for living systems as we know them would not form, first because they are so readily oxidized, and second because high-energy ultraviolet radiation (UV), the most likely energy source, would not penetrate the ozone screen that results from the presence of much free oxygen (>1 percent present atmospheric level). If such molecules did somehow form, rapid oxidation would probably keep them from accumulating in sufficient quantities over enough time for more complex molecules to evolve, accumulate, and combine to form a bounded, negentropic, self-replicating, and mutable system of the type we would call living. If a living cell did, nevertheless, manage to evolve in some local reducing environment, it could not survive transfer to the larger oxidizing environment in the absence of suitable oxygen-shielding systems, O_2 being lethal to all forms of life in the absence of such systems. Additional evidence that life originated under essentially anoxygenous conditions is that the most basic metabolic processes of all organisms are anaerobic and that biochemical evolution has gone to great lengths to carry out most of its oxidations by removal of hydrogen rather than by addition of oxygen."

If we insist on interpreting the past in terms of the present, we will often reach quite false conclusions and miss many things which are most important.

A geologist of the Precambrian, who cannot escape his Phanerozoic cage, has no hope of seeing even obvious clues. His earth was much smaller, g was higher, as was also G, the relative radii of core and mantle were probably different, the balance of radioactive elements was different, the composition and geochemistry of sea-water was different, heat flux different, pressure gradient steeper, there was no land vegetation hence wind was the main transporter of rock clastics, the atmosphere lacked significant oxygen so weathering processes were different, there were no large oceans before the late Proterozoic, the obliquity of the earth's rotation axis was a vital variable, and Moon must have been very close in a high eccentric orbit (unless a quite different genesis be adopted).

Disenchantment with the restraints imposed by conventional uniformitarianism has led to the conception of a new counter-culture journal, *Catastrophist Geology*, to explore aspects of reality currently spurned because they transgress reigning preconceptions of the establishment.

KINDS OF CHANGE

With the passage of time a state or process may be:

Uniform - whether we assume a hot beginning from a gaseous nebula or a cold beginning from accretion of planetesimals, or some other model, the state of the earth cannot have been uniform.

A random walk - a process governed by change could yield a random walk.

A Trend - Radioactive decay, compaction under gravity, heating by either of these with generation exceeding dissipation, degassing, and many other processes exist which would bias the random walk to yield trends, with rates much more likely to be exponential or logarithmic than uniform. Thus the evolution of life, though intrinsically probably a random process, is biased by competition to a progressive trend; the evolution of the atmosphere, the degassing of the interior, decline in radioactivity, accumulation of surface water, salinity of the oceans, and gravitational attraction of the moon must be governed by trends. Reverse processes may establish balanced states.

Cyclic - Apart from astronomic cycles from the earth's rotation, orbital revolution, the gyration of the earth about the common centre of gravity of the earth-moon system, the wobbles, nutation and precession, the sun's cycles of energy output, and the complex cycles of the attractions of the other planets, many processes operating in the earth have restraints and feed-backs which produce instabilities and cycles. All of these find expression in geological processes.

A unique event, episodic or catastrophic - such as the birth of the earth, the capture or ejection of the moon (if either of these occurred), impact of comets or asteroids, initial onset of internal convection and discontinuous jumps to three- four- or five-lobed patterns (Fig. 42), episodic redistribution within the solar system from multi-body dynamic instability (see p. 312), and major changes in the condition of the sun. Some have regarded the onset of continental drift as a unique event, although for my part, I see no change of process from the earliest beginning, but merely waxing of intensity of the same process.

Simpson (1952), Wald (1955), Bullard (1966), and Gretener (1967) have drawn attention to the importance of rare improbable events in geology. Gretener points out that the chances of throwing six on all eight dice in a single throw is 1 in 1.5 million. In a couple of hundred throws it improves to 1 in 10,000. In 1.7×10^6 throws it is 63% and in 5×10^6 throws the probability of eight sixes turning up at least once is 95% - approaching a certainty. The improbable is bound to happen if the number of trials is sufficiently large. Wald observes that in geological probabilities "time is the hero of the plot .... What we regard as impossible on the basis of human experience is meaningless here. Given so much time the "impossible" becomes possible, the possible probable, and the probable virtually certain. One has only to wait: time itself performs the miracles". Bullard comments that "with a system as complicated as the earth, almost anything can happen occasionally". Nevertheless most of us tend to forget entirely the probability of unique events.

The great Sudbury body with vast volumes of mafic rocks and the lion's share of the world's nickel, has always been *known* to be an igneous intrusion - a lopolith. Not until 1964 did Dietz put forward the outrageous proposal that it was an astrobleme, the impact scar of an asteroid which hit the earth some 2000 million years ago, making a crater a hundred kilometres or so in diameter and 25 kilometres deep. Not until much more recently has almost everybody who has studied the matter agreed that he is right. Most geologists take it for granted that things have always been as they are. We have no right to assume that unique events have not occurred, but must be alert to recognise them if they are recorded. Unfortunately we see only what we know, so the probability is, that if faced with the evidence of a unique event in the geological record, we would fail to observe it. As Claude Bernard said, "It is what we think we know that prevents us from learning".

NON-UNIFORM PARAMETERS

Earth radius

That the diameter of the earth has increased with time at an increasing rate, is the theme of this book.

Fundamental constants

One of the surprises of the last few decades was to find that the north and south magnetic poles had swapped ends with astonishing frequency, and that the total intensity of the field was far from uniform.

When a physicist measures a fundamental empirical constant he knows its value - now. But can he assume that it always had that magnitude? Evidence from the rocks and of the cosmos, which record conditions existing aeons ago, is needed to answer this. The answer increasingly is that few of the fundamental "constants" are indeed constant. The gravitational constant, Hubble's constant, the ratio of electrostatic to gravity attraction between proton and electron, all seem to have varied with the age of the universe. The nexus of ephemeris time and atomic time, equated by definition today, may have converged thither during the past, and may diverge thence in the future. The conversion of energy to matter has probably been equally as important as the better known conversion of matter to energy. Such questions are discussed more fully in the final chapter of this book. Meanwhile they add another source of error when applying modern models to ancient aeons.

The atmosphere

Like the ocean waters, the atmosphere is of secondary origin and has evolved through geological time. Opinions differ as to the composition of the earliest exhaled atmosphere after the initial gross loss of hydrogen and the rare gases, but an atmosphere of carbon dioxide and nitrogen, with some water vapour, is as probable as any other. Sagan and Mullen (1972) concluded that ammonia was a necessary constituent for the first two billion years to close the existing thermal radiation window from 8 to 13μm; for otherwise the surface temperature of the earth should have been below the freezing point of water until about two billion years ago, which is denied by evidence of aqueous erosion and sedimentation, and by primitive life, for which liquid water was almost certainly necessary.

Like present-day Mars, the earth had very little free oxygen for the first seven-eighths of geological time. This means that all weathering and chemical precipitation processes must have been very different throughout the Precambrian, and organisms must have depended on fermentation processes for their oxygen. Perhaps we should recognise three stages of atmospheric evolution. First a hyper-hydrogen stage when nitrogen was present as NH_3, carbon as CH_4, sulphur as H_2S, and oxygen as H_2O, leading up to the first rains. Second, a carbonate stage when nitrogen was largely free, carbon was as CO_2, and oxygen was in CO_2, H_2O, and SO_3. Photosynthesis by ancestral blue-green algae contributed oxygen. This was an age of dolomites and the banded-iron formations which are discussed below. Third, an age when free oxygen increased, carbon went increasingly into living or dead organic matter, and oxygen appeared in red sandstones of the late Precambrian.

Clearly Sagan's and Mullen's ammonia shield must have disappeared before the initial appearance of free oxygen. It seems likely from their graph that this would have occurred before the CO_2-H_2O greenhouse effect had raised the surface temperature above water freezing temperature. This could explain a lot of facts about this part of earth history, such as the universal "Liparian erosion interval" more than 800 million years ago, the seemingly universal glaciation which appeared on all continents in the late Proterozoic, some of which seems to have been equatorial, and perhaps even the explosion of organic evolution which followed. Concurrent change of axial obliquity may also have been involved, as explained later in this chapter.

By the Cambrian the oxygen level may have reached 0.2%, allowing aerobic respiration for the first time, and perhaps this condition was primarily responsible for the sudden expansion of invertebrates. By the end of the Silurian the oxygen content may have reached 2% and was probably one of the factors which resulted in the rapid invasion of the land by plants and animals, where, incidentally, they were subject to gravity and desiccation for the first time. Earth viewed from Moon reveals a blue planet with about an eighth of the surface obscured by cloud. We assume this to be normal, and that Carboniferous dragonflies flitted in sunlight. But did they? The present is not the "normal" state of the earth. It is a time of high obliquity, of polar glaciation, and of strong seasonality and low free carbon-dioxide. The amount of water held in the atmosphere is a sensitive balance of several variables. Perhaps a globe wholly shrouded in cloud may have been a more "normal" state of the earth - a Venusian condition.

Radiation - The flux of electromagnetic radiation and of energetic particles reaching the top of the atmospheric envelope could not have been uniform, and even more variable was the penetration of such radiation to the surface of the earth. The many variables include the condition and output of the sun and cosmic sources, the form and strength of the earth's magnetic field, the composition and weight per unit area of the earth's atmosphere, and g and G. The critical importance of the absorption of specific radiation frequencies by ammonia, carbon-dioxide, ozone, and water have been outlined above.

Modern theory of stellar evolution, and of the present position and former path of Sun along the main-sequence zone of the Hertzsprung-Russell graph, implies that Sun's luminosity has increased through geological time. Estimates of the amount of increase vary from 30% to 60% according to different authors (see footnote 6 of Sagan and Mullen, 1972). The frequency and intensity of electromagnetic radiation in turn affects the rate of random genetic change, and hence the rate of evolution, for although most such changes damage or kill the organism, a statistical few are advantageous in some way. So the rate of evolution must have varied with time, probably combining a random fluctuation, periodic fluctuation, and a trend.

The hydrosphere

Volume of ocean water - The keynote of William Rubey's masterly presidential address to the Geological Society of America (Rubey 1951) was that the whole of the waters of the oceans had been exhaled from the interior of the earth, not as a primordial process, but slowly, progressively, continuously, throughout geological time. Whereas the amount of the common rock-forming oxides in known sediments balances reasonably with estimated products of rock weathering, the quantities of the more volatile materials - water, carbon dioxide, chlorine, nitrogen, and sulphur - are at least two orders too large to be accounted for by derivation from weathering of rocks. Furthermore, Rubey demonstrated that the proposition, that such quantities could have existed in the ocean or atmosphere from the Archaean, is flatly contradicted by many solid facts of geological and biological history. On the other hand, the relative balance of these "volatiles" is similar to that of known volcanic exhalations, and the current rate of such emissions, integrated over geological time, accounts adequately for the total quantity of the hydrosphere and atmosphere. Hence Rubey's firm conclusion that the ocean waters cannot have been an initial inheritance, but rather a life annuity, received throughout geological time from the deep interior of the earth.

Kulp (1951) confirmed Rubey's conclusion from a study of argon. The source of A^{40} is the radio-activity of K^{40}, but the total potassium in the crust could only have yielded one twenty-fifth of the A^{40} actually present in the atmosphere. Hence the A^{40} has been outgassing from the deep interior of the earth. Kulp used this index to estimate the sustained accretion of water via the same route, and found close agreement with the total water in the atmosphere, hydrosphere, and crustal rocks.

The total *volume* of seawater has steadily and episodically increased through geological time. This implies that the *capacity* of the ocean basins has also increased through time. Rubey, in a climate where continental drift was still scorned and global expansion scarcely conceived, pleaded that the increasing capacity might have been achieved by increasing *depth* of the ocean basins, although in this abbreviated section of his oration (pp. 1142-3), he was clearly insecure and in deep water. The alternative, that the *areas* of the ocean floors had increased through time on an expanding earth, had not occurred to him as a viable alternative.

As the generation of the ocean floors depends fundamentally on the same process as the outgassing of juvenile water it would be expected that the volume of sea water and the capacity of the ocean basins both increased in a related way. But not necessarily in phase. Several variables are involved, some with feedbacks and time-delays. There should be times when the capacity of the ocean basins increased more rapidly than the total volume of sea water, and vice versa. The former would result in general emergence and regression of the sea from the lands, the latter a transgression of the seas over the lowlands. This could happen on the gross scale of the order of whole geological periods, on epoch scale, and on progressively smaller scales to as short as a few years.

The Triassic Period, for example, was one of universal regression. Marine Triassic sediments are rare except in the deep troughs. There are few epicontinental seas of Triassic age. The Triassic is recorded on all continents, mostly as terrestrial or lacustrine sediments. The name "New Red Sandstone" expresses this fact. As might be expected, the "Old Red Sandstone" marks another conspicuous regression maximum straddling the Siluro-Devonian. By contrast, the Cretaceous and Cambro-Ordovician are represented everywhere by wide shallow epicontinental seas, where the ocean water spilled over far into the interior lowlands of the continents (Fig. 13). The Triassic was a period when the total capacity of the ocean basins had increased at a faster rate than the total volume of ocean water. The Cretaceous was a period when the reverse was true. Each period, each epoch, each minor unit has its own ocean-water/ocean-basin budget.

Secular variation of the temperature gradient in the mantle is another factor in this equation. When isotherms rise under a continent or megacontinent like Pangaea the depths of all phase transitions, from more dense to less dense paramorphs, descends to greater depths, and the surface bulges like rising dough, and there is general regression. Likewise as the mantle below a new ocean basin slowly loses its excess heat so that the isotherms retreat inwards, the phase transitions ascend so the floor of the basin sinks to greater depths. This increases the capacity of the basin, resulting in regression. Stratigraphers have long recognised the waxing and waning of ice sheets as modifiers of sea regressions and transgressions. But valid though they be, the glacial fluctuations are only modulators of the more important equation of the increasing total capacities of ocean basins and of ocean water.

FEEDBACK ON EXTINCTION RATES - The waxing and retreat of epeiric and shelf seas causes consequential feedback phenomena. The area of shallow and shoreline biotic environments fluctuates more sharply than does sea-level. Mayr (1965), Wace (1966) and Newell (1967) have emphasized "that a high correlation exists between biotic diversity and total area of habitat available for colonization". Hence rates of extinction should correlate closely with rapid regressions, which in fact they do, according to Newell (1967, Table 4). The highest known extinction rates per million years were between Upper Pennsylvanian and Middle Triassic, the time of greatest regression. Other very high extinction rates occurred in the Upper Cambrian, Upper Silurian and Upper Devonian, which were times of global regression.

COMPOSITION OF SEA-WATER - The assumption has commonly been made that the oceans were originally fresh and that salinity and composition of the oceans has steadily increased with time. Hence Joly's classical hour-glass for the age of the oceans by dividing the total mass of sodium in the sea by the annual increment of sodium, which gave an age of some hundred million years. Quite apart from the large correction for cyclic sodium and for high current rates of erosion, the argument has now the additional flaws that the total volume of sea-water has increased with time, and that the incremental water itself bears significant sodium.

This kind of proposition suffers the further fallacy that salts added to the oceans can stay there. Rubey (1951) has argued cogently (principally on biological grounds) that the composition of seawater has not changed markedly since perhaps the Archaean. An equilibrium between accretion of ions and their removal as carbonates or clay adsorbates was reached quite early, and has not changed greatly since.

The balance of composition of seawater has little correlation with balance of composition of the influx either from meteoric or juvenile sources. Nor has the balance of composition of connate waters now contained in sediments much correlation with the seawater originally occluded from the sea, for such waters remain near equilibrium with the pressure, temperature, and chemistry of their host rocks, as fluids are slowly squeezed by sedimentation and diagenetic over-pressure towards the surface.

Evolution of crust, mantle and core

The continuous progressive release from the deep interior of the "volatiles" to accumulate as hydrosphere and atmosphere is but the vapour from the cauldron, for these "volatiles" are nothing but the terminal members of a differentiation series, which involves the outer core, the mantle, the crust, the oceans, and the atmosphere. After 4000 million years of differentiation, we still have vulcanism, with no sign of waning intensity through time. The mantle has grown *pari passu* with the areal increase of the ocean floors. The model of a primordial grand differentiation into core, mantle, and sialic crust, with only relatively trivial activity since, is surely false (Engel, 1966; Patterson and Tatsumoto, 1964).

The model which emerges with sharpening clarity is that, following the initial big bang, the earth evolved from a super-dense metastable core, of composition similar to carbon chondrites (which satisfies the observations which lead Kuhn and Rittmann (1941) to their core of primitive solar material). Such a primitive composition has been proposed by Urey (1962) and Ringwood (1966). Differentiation of this metastable inner core has proceeded ever since, transforming at an increasing rate to a shell similar to iron meteorites, and a mantle similar to the stony meteorites; the mantle in turn excreted a thin basaltic scum, and exhaled residual volatiles. Initially thin, with steep thermal gradient, these shells have thickened with time, and still thicken. The presence now of a small residual superdense inner core would be difficult to detect. It would contribute little to the overall mass, volume, density, or moment of inertia of the earth, and would be difficult to identify seismically because waves do not "see" objects smaller than their own wave-length even as reflectors, and arrival time residuals add any errors in the assumed velocities along the rest of the path.

The crust and brittle top of the mantle has always been in tension because of the growing interior as the phase changes proceeded. Hence the crust failed mud-crack fashion with polygonal rifts surrounding lagging basins. These polygonal rifts started as whaleback arches (because the rising isotherms induced by the outgassing caused phase-changes there), similar to modern oceanic arches such as the Shatsky Rise, the Ontong Java Rise, the Troodos arch, and the swell that separates the East and West Caroline Basins. The primitive crustal arches developed rift horsts and grabens, and became channels for continued outgassing vulcanism, at first basaltic, which flowed into the basins yielding the first komatiites and layered complexes, followed later by sodic granites as differentiation proceeded. This is the kind of Archaean crust deduced by Glikson (1972) on very different grounds -- the petrology, geochemistry, and field relations of the oldest rocks.

This primitive crust must have been grossly modified by the period of intense asteroidal bombardment, so dramatically recorded on the surfaces of Moon, Mars, and Mercury. (See Green, 1972b). Metasediments nearly 3800 million years old have been dated in Greenland (Moorbath, O'Nions, and Pankhurst, 1973), but care is needed in deciding whether such sediments post-date the termination of the Hadean Aeon by the first rains. Mars has widespread and thick sedimentary deposits transported by dust storms. Such sediments would be loess-like and finely laminated rather than dune-like. Coarser clasts could be added by dunes or as volcanic tephra and meteorite bombardment, still without aqueous assistance. In their intensely metamorphosed condition positive identification of water-laid sediment might not be easy, except where gravels or cobbles are present. It is argued later in this chapter (pp.140-146) that banded iron formations are wind-transported, not chemically precipitated, but this still leaves open whether the accumulation was in water, or in the dry. It was such a primordial crust, which was stretched, mudcrack fashion, to form a mosaic of disjunctive polygons, some hundreds of kilometres across, ready to receive the first seas. This kind of expansion pattern still shows up in Figures 17, 19 and 20.

Evolution of the oceans

Most take it for granted that there have always been large oceans like those of to-day - at least since very early times. Such uniformitarianism seemed axiomatic. But is it? It is now known that *all* the floors of *all* the oceans, Pacific included, have been formed since the Palaeozoic, so it is assumed that equivalent other ocean areas have been "consumed". Maxwell's demon is needed again at the helm to ensure so clean a sweep that no stable remnant of old ocean remained anywhere! Oceans like the Arctic, Atlantic and Indian, in any case, date only from the Mesozoic and have doubled their area since the Eocene. I suggest that the Pacific too was a fraction of its present size before the Mesozoic. Of course, there were extensive ancient seas, but oceans of the modern type are a new phenomenon with an antiquity no longer than the reptiles.

The Archaean crust was, I suggest, panglobal, broken by a tensional rift, mud-crack fashion, over the slowly expanding interior. The outgassing of the deep interior concentrated along the polygonal boundaries, bringing up heat, so that at all depths rocks along these boundary zones tended towards less dense paramorphic phases, causing the polygonal outlines to continue to arch (undations of van Bemmelen or cymatogens of King, 1962); the continuation of the process led to rupture and rifting of the arches, giving at the surface a pattern of broad still-stand basins separated by rift troughs bordered by raised rims. Lake Victoria basin and its bordering troughs are a rather small present-day analogue (Fig. 39).

The polygonal rifts became primitive geosynclines which, with the continuation of the process, eventually regurgitated their contents with much volcanic and plutonic igneous activity and gravity outflow of nappes (of metasediments and plutonics, as well as of less altered sediments) on to the lower cratonic basins. Meanwhile the basins received sediments from the erosion of the swells (including orogenic detritus), as well as thick accumulations of dolomite and basic lavas, weights which further depressed the basins both by isostasy and the deep paramorphic increase of density under greater load. So we get the typical form of the ancient terrains - a mosaic of narrow troughs of intensely metamorphosed rocks often forking round "cratons", along with broad basins of ancient but little-altered strata. At this time there were no great oceans anywhere on the earth, but broad intra-continental seas (of the Witwatersrand type), always shallow during their development, and long narrow deep troughs, rarely as wide as the Red Sea.

Owing to the inherent feedback instability of the outgassing process, some undations inevitably developed more than others and, in due course, through-going pan-global tensional lineaments occurred. Such was the Eo-Pacific in the Upper Proterozoic which commenced some 800 million years ago

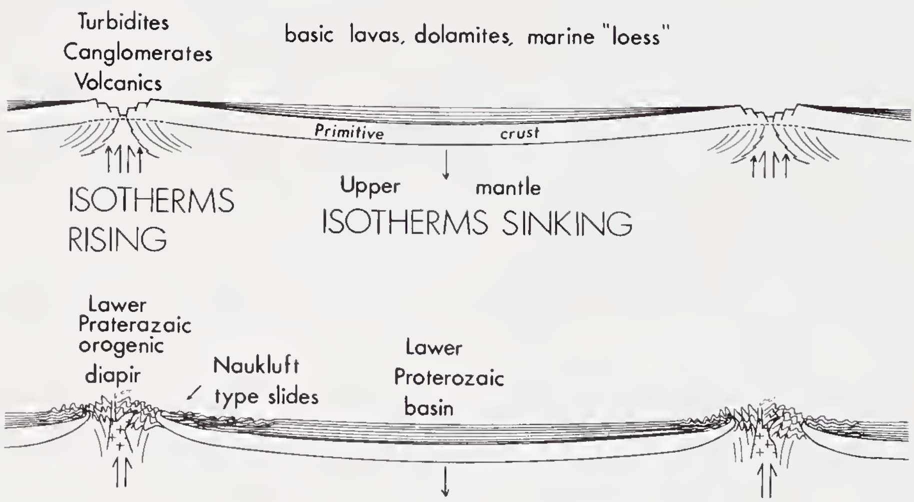


Fig. 39 Early Proterozoic sedimentary basins and orogenic troughs.

(see pp. 338-341). I suggest that at that time the Australian, Antarctic, and east Asian continents were separated from the Americas only by such a narrow seaway trough in which sediments accumulated. At a much later date this Eopacific geosyncline had widened into the Eopacific lune shown in figure 170.

Isaacson (1975) has shown that, during the Palaeozoic, the central Andes received their sediments *from the west*. "The size of the western land source that yielded the volume of sedimentary detritus in the Bolivian portion of the basin alone suggests that more land area existed in the western South American continent than exists today. Abundances of detrital mica in the sediments indicates that there was a provenance of highly micaceous rocks ..." This land source was northeastern Australia and Tasmantis - the now fragmented *easterly* source of Australian Palaeozoic detritus, now represented by parts of New Zealand and the Lord Howe ridge, and Antarctica.

Waterhouse (1967) reported from New Caledonia a Permian brachiopod species *Attenuatella incurvata* similar to New Zealand and Australian species but closest of all to a Mexican species *Attenuatella attenuata*. Avias (1953) had previously suggested the possibility of a large landmass east of New Caledonia. The Permian Cathaysian floral province straddled western North America and eastern Asia.

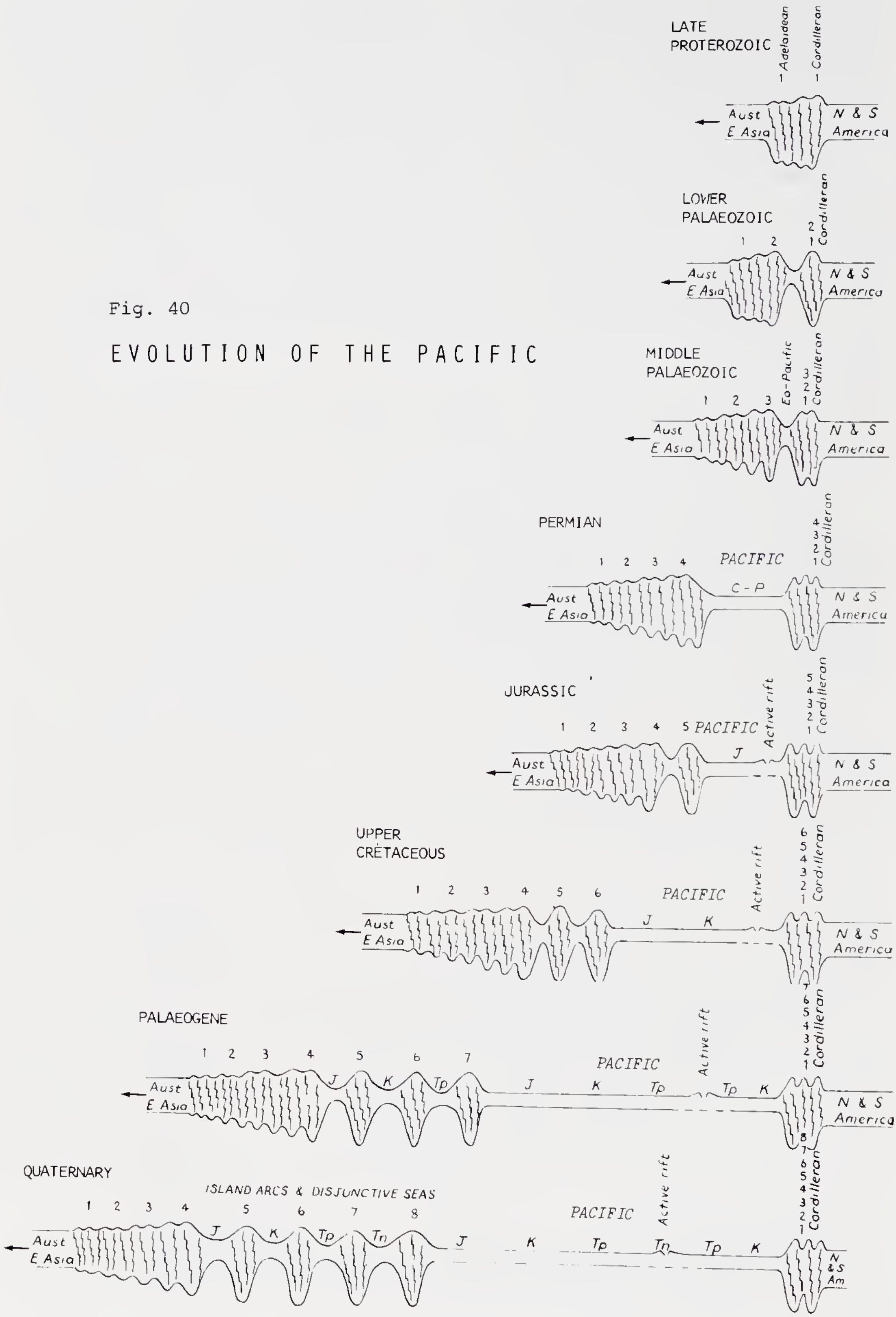
After the filled geosyncline became an orogen, further distension opened a new trough, generally along the eastern side of the older regurgitated zone and, in this, the Lower Palaeozoic orogenic sediments accumulated (Fig. 40). Likewise the Upper Palaeozoic trough opened mainly along the eastern side of the older orogens with ocean overflow flooding interior epeiric seas at appropriate times. By the Lower Mesozoic the tempo of expansion was increasing, so that wider disjunctive seas separated the active orogens on the American and Australasian sides - the beginning of the present Pacific Ocean floor.

In the Upper Mesozoic the first intra-oceanic rift zone appeared as a bilaterally spreading ridge, first in the south because expansion was greater in the southern hemisphere. By the early Tertiary disjunctive seas developed between the Australasian bordering orogen and the continents behind.

One of the articles of faith of "plate" tectonics is that ocean growth is necessarily symmetrical. But this is only known to be true for the youngest generation of oceans - those which disrupted Pangaea. Profiles across these Mesozoic oceans are broadly symmetrical topographically, structurally, seismicly, magnetically, petrologically, in thermal flux, and in the age of the ocean floor. The Pacific is not one of the Mesozoic oceans.

Fig. 40

EVOLUTION OF THE PACIFIC



It was born in the Proterozoic. It commenced equatorially, not meridionally. Profiles across the Pacific are asymmetric topographically, structurally, magnetically, petrologically, in arcs, trenches and disjunctive seas, in heat flux and in the age of the ocean floor.

"Plate" tectonicists set up their model for spreading ridges from Atlantic data, then force the Pacific, which differs from the Atlantic in every way it is possible to differ, to conform to this Procrustean strait-jacket. Because the young intra-Pangaean oceans grew symmetrically, the disciples knew that the Pacific must also have grown symmetrically. To match the Pacific floor which decreases in age all the way from Japan to California, there must have been originally a matching 7000 km of crust, which must have vanished under North America, because it is not there now!

On the contrary, the Pacific Ocean grew asymmetrically. The Asian and Australian blocks with their subjacent mantle migrated ever westwards, leaving wider and wider disjunctive seas just inside the currently-spreading orogenic ridge. By contrast the Cordilleran orogens, both in North America and South, fall on top of each other, right from the Proterozoic to the Recent, and lack disjunctive seas. The Pacific floor grew eastwards and only in the last hundred million years or so has there been a bilateral spreading ridge - and even now not in the far north. All island arcs bow eastwards (or equatorwards). There is also a north-south asymmetry. Because expansion has been greater in the south, all continents have migrated northwards. The dispersion of the successive orogens increases southwards. The bilateral intra-oceanic spreading ridge commenced first in the south and converges on to the coast of California.

The original Pacific seems to have been equatorial, and probably resembled the equatorial dilating fracture zone of Mars, which, like the embryo Pacific, was strongly developed only half way round its planet. By the Lower Palaeozoic polar wander had rotated the Cordilleran some 40° anti-clockwise so that the Caledonides-Appalachides were equatorial (Fig. 11). By the early Mesozoic polar wander had rotated the Cordilleran and the Caledonides a further 40° anticlockwise, so the Tethys was born as the encumbent equatorial rift zone. Subsequent polar wander has rotated all further anticlockwise, so that the Tethyan belt now lies some 40° north of the equator in the Mediterranean, crosses the equator in Indonesia, runs 15° S near Samoa, whence it sweeps north again to the Caribbean to complete the circuit to the Mediterranean. Prior to the advent of the Tethys, southern Europe was more closely related to Africa than to northern Europe, from which it was separated by the Palaeozoic seaway as Burrett (1973) has emphasised. This progressive rotation of the equatorial zone of extension and orogenesis is echoed by the secular ninety-degree migration of the north pole from its Proterozoic position normal to the cordillera to its present position (Figs. 138 and 139).

The symmetry of this pattern is modified by the greater expansion of the southern hemisphere and the northerly migration of all continents, and also by the recurrent sinistral drag along the equatorial zone, caused directly by the implications of this northerly movement on the movement of inertia of the earth. The accelerating growth of the Pacific had lead to a global asymmetry - an *Urozean* the Eopacific and an *Urkontinent*, Pangaea, now divided equatorially by the embryonic Tethys. This gross asymmetry spawned the Gondwana syndrome, which *inter alia* lead to the Gondwana rifting of the Arctic, Atlantic and Indian Oceans which was later rejuvenated in the Alpide cycle. The pattern differed from the Pacific, Caledonian and Tethys not only because it was younger, when global expansion rates were faster, but the rifts were essentially meridional, whereas the predecessors had commenced equatorially.

The youngest meridional rifts rupture and separate Late Palaeozoic floral provinces; the north Atlantic breaks and divides the Euramerican flora; the north Pacific breaks and divides the Cathaysian flora; the south Atlantic, Indian and south Pacific meridional sphenochasms break and separate the Gondwana flora. As discussed elsewhere, the boundary between the Gondwana flora and the northern hemisphere floras was not the Tethys but the narrow late Palaeozoic humid tropics zone, which had already tilted from the Tethyan great circle because obliquity change normally precedes the continental disruption.

Because expansion rates have increased with time, the Atlantic, Arctic and Indian rifts opened too rapidly to fill with sediments. The first-order processes of the earth are not uniform with time. Early seas and oceans are different in kind from those that developed later by the same fundamental cause. The present pattern of great oceans has no previous parallel in the history of the earth.

In summary, four stages emerge in the evolution of the hydrosphere: (a) the Hadean, without rain or water; (b) the Proterozoic polygonal rift-and-basin system; (c) the late Proterozoic and Palaeozoic throughgoing equatorial seas -- Cordilleran, Appalachian, and Tethyan with extensions into the polygonal system; (c) the Mesozoic birth of the great meridional oceans. The capacity of the ocean basins and the volume of water increased exponentially with time. The composition of sea-water stabilized early and has not changed much since.

Obliquity of rotation axis

In the later chapters on the rotation of the earth I argue that the obliquity of the rotation axis to the ecliptic (currently $23\frac{1}{2}°$) is the single most important variable for geological history. Because gravity is the dominant force, isostasy is general. But a continent, or a thermal geotumour, in isostatic equilibrium contributes significantly more to the moment of inertia than does an equivalent oceanic area. Hence large scale variations in heat flux (such as occurred under Pangaea at the end of the Palaeozoic) must produce gross axial wobbles, changes in precession, and (because of the rheological yielding of the earth) increase of obliquity. Internal friction (e.g. because the mantle on its own would precess at a different rate from that of the core on its own) gradually reduces obliquity to zero. So we must expect in the geological record, times of large obliquity, and times of minimal obliquity, with diverse consequences.

Magnetic Field

If the magnetic field is driven by the precessional coupling torque between core and mantle, as many have suggested, the intensity of the magnetic field would vary with the obliquity. As the obliquity wanes so does the differential precession couples of the mantle and core. The inclination of the magnetic axis to the rotation axis, the intensity of both dipole and non-dipole fields, and the westward drift of the magnetic field would all decline to low values during times of low obliquity. Studies of palaeo-intensities of the magnetic field (e.g. Carmichael, 1970 and Smith, 1970) do show significant variation, but so far results are too scattered to permit firm generalisations, and the elimination of possible intensity loss is far from satisfactory.

Major changes of obliquity would be expected to cause major changes in the migration of apparent palaeomagnetic poles. Irving and Park (1972) report two "hairpins" in the Phanerozoic pole path - one in the Pennsylvanian and one in the late Cretaceous. These correlate with the Gondwana and the Alpide tectonic syndromes described below which combine change of obliquity, climatic disturbance, mantle igneous cycle, and continental dispersion. Irving and Park also found four "hairpins" in the Precambrian and suggest that these divide the Precambrian into major eras. On the evidence currently available, this is certainly a plausible hypothesis.

Palaeoclimate

Since the obliquity is the direct cause of seasons, variation in obliquity should show up in the geological record, particularly in palaeoclimates. Periods of high tilt would have strong seasons. The long winter would produce extensive snowfields which, because of their high albedo, would not melt in the summer. So periods of high obliquity would be periods of polar glaciation. We are currently in a period of $23\frac{1}{2}°$ obliquity (induced by the Tertiary asymmetric expansion), and we are in a period of polar glaciation. Likewise Mars. At times of high obliquity the humid tropics (with coals) shrink to a narrow zone a few degrees each side of the equator, and the arid belt of deserts and evaporites occupies the rest of the tropics. A period of zero obliquity would have no seasons whatever, and the poles would receive permanent sunshine because the tangential rays would refract on to them. Without seasons there would be no polar glaciation. The belt of tropical cloud, at present extending some 10° from the equator might be expected to extend to 45° or 50° or more. The desert belt with evaporites would be very restricted. The balance between water in the oceans and in the atmosphere would shift. The earth would go towards a Venusian condition, with extensive humid cloud cover. The system is indeed very complex, with feed-backs and instabilities. The atmosphere is the working fluid of a heat engine which moderates the extremes between day and night, summer and winter, and the tropics and polar regions. Cloud conserves heat like a greenhouse. Frigid conditions remove cloud as ice crystals and increase heat loss by radiation and reflection.

The Carboniferous was such a period of low obliquity and spread of the steamy tropics to higher latitudes. Potonié (1953, 1968) observed that the well-known absence of annual rings in the coal forests permits the conclusion that on account of the universally uniform climate the plants experienced no annual rhythm. "The ferns are shade plants without photoperiodic responses. One can speak of a misty forest or of highly filtered light." Barghoorn (in Shapley 1960) wrote: "One impressive indication of uniform climate over great areas of the Carboniferous continents is the general absence of annual growth rings in coal-swamp trees. The entire question of ring development in woody plants is one fraught with botanical variables as well as climatic variables. However the consistent absence of any index of seasonal growth seems difficult to explain except on the assumption that winter cold and seasonality of rainfall were absent or at a minimum. In existing woody plants, annual ring development may occur under nearly uniform climatic conditions, as in equatorial rain forests. Nevertheless in climates with distinct seasons the seasonal effect is almost invariably reflected in pronounced annual growth rings". Kräusel (in Nairn 1961) agrees that the absence of annual rings "proves that there was no significant seasonal change". Thenius (1963) considered that the absence of resting buds makes it "certain that the whole year must have shown a rather uniform temperature and humidity".

The Permian, being a time of high obliquity is contrasted by the same authors. For example, Barghoorn *(loc. cit.)* wrote: "Fragments of wood from late Palaeozoic austral deposits frequently show pronounced ring growth as further evidence of seasonal periodicity, probably winter cold".

Polar glaciation was the exception rather than the rule in the past. In the Upper Devonian and Lower Carboniferous the earth was free from glaciation, the tropical cloud belt extended far each side of the equator, and a common *Lepidodendron* vegetation spread widely to northern and southern continents. In the Upper Carboniferous and Lower Permian, differential expansion caused strong tilt with the onset of glaciation, and the contraction of the steamy tropics to a narrow equatorial belt. Sharply different floras developed in the two hemispheres, the *Glossopteris* flora in the south, and the *Gigantopteris* flora in the north, separated by the tropical cloud belt into which tolerant members of the floras mingled. Through the Permian and the Triassic, the sharply differentiated climate showed up in red beds and salt.

A palaeoclimatic anomaly in the early Permian of eastern Australia has been progressively sharpened by Dr. F.C. Loughnan, as he finds more and more widespread distribution of flint clays and laterites within the early Permian succession, which appear anomalous in relation to the Gondwanaland Permo carboniferous glaciation. I quote one example from Loughnan (1975): "From a study of the association of flint clays and thick, lateritic palaeosols developed on Early Permian basalts at a depth of more than 1,200 m in the Quirindi Bore, it is concluded that during at least part of the Artinskian Stage in the Sydney Basin the climate was humid and tropical to subtropical. The concept however, is at variance with the popular belief that in accordance with other Gondwana countries, alternations of frigid and cold temperate conditions prevailed in Australia during the Early Permian and may have persisted through to the close of the period." On the present pattern of climatic zones (uniformitarianism), there is certainly a contradiction here, because both the glacial and palaeomagnetic data agree on high latitudes; but we are always at risk in transferring automatically present conditions to the past. Given a much greater axial obliquity in the early Permian, with more sharply contrasted seasons, the glacial, palaeomagnetic, and Loughnan's evidence of the climatical implications of the soils, may all be satisfied - as indeed they must, unless found to be misinterpreted.

The tilt gradually dissipated towards the Jurassic, with spreading of the tropical cloud belt to higher latitudes, through which very similar cycads, gingkos, and other gymnosperms spread to all lands, and the cold-blooded dinosaurs found their happy home.

This tilt cycle shows up as part of a quite general *Gondwana Cycle* which expresses itself in every aspect of the geological record from Carboniferous to Cretaceous.

Gondwana Syndrome

On the Carboniferous earth, all the present continents were part of a single continent - Pangaea. Asymmetric expansion caused a mantle tumour (the largest so far experienced by the earth) to swell under the southern part of this megacontinent - centred where modern southern Africa, southern India, southern South America, the south-western angle of Australia, and the Enderbyland corner of Antarctica, lay together. Isostatic imbalance produced by such a mantle tumour rapidly diminishes with a half-life of a few thousand years. But the tumour has a larger moment of inertia than the mantle it replaced. This remains after isostasy has been restored. Hence the rotational asymmetry caused by the asymmetric expansion led *pari passu* to an axial wobble, which the attraction of the sun and moon turned to an axial tilt. The wobble and tilt led to sharp climatic differentiation and the Pennsylvanian-Permian glaciation. The mega-tumour with raised isotherms caused several mantle phase-boundaries to move towards the less dense member, so that the whole of Gondwanaland and, indeed, most of Pangaea bulged higher with respect to sea-level, which accentuated the Permo-Carboniferous glaciation caused by the tilt and magnified the world-wide Triassic emergence due primarily to the changing balance of total ocean water and ocean basin capacity. This, combined with the sharp climatic differentiation with desert belts over what had been humid rain forest lands, led to the characteristic Permo-Triassic evaporites and red beds. As stated above, the narrowed humid tropic zone segregated the Gondwana and Laurasian floras, which therefore developed on different lines during the Permian and Triassic. Schopf (1968) has written "The ecotone distinguishing the Gondwana area from that occupied by northern floras is the most profound floristic boundary in the history of land vegetation". Naturally so, because the earth suffered then the largest axial obliquity, the sharpest differentiation of seasons, the sharpest differentiation of climatic zones, the most extensive glaciation, and a narrow belt of humid tropics forming a formidable barrier to non-tropical inter-hemisphere migration.

Meanwhile the higher isotherms in the mantle tumour led to widespread partial melting and the generation of enormous volumes of tholeiitic magma, the greatest irruption the earth had ever known - which invaded the sediments of shallow basins in all lands, and poured out on the surface as basaltic floods.

These stupendous phenomena have been integrated by K.G. Cox (1972) as the Karoo volcanic cycle, which he states began with the rise of a body of potassium-rich picritic magma from a depth of at least 500 km. Meanwhile, at about the peak of the igneous activity -- as pointed out by Vine and Hess (1971) and Scrutton (1973) -- the swelling tumour had rent Gondwanaland with radial rifts, which, with progressive widening, developed into the Atlantic and Indian Oceans and their off-shoot arms such as the Red Sea, the Persian Gulf and the Mozambique Channel; hence followed the disruption of Gondwanaland and the dispersion of the separated continents - Australia, Antarctica, Africa, India, and South America. Meanwhile the still differentiating residues of the great Karoo vulcanism produced alkaline intrusions, with increasing tenor of rare earth elements, carbonatites, and finally, in the Cretaceous, the widespread gas explosions, which formed the hundreds of kimberlitic diatremes and diamond pipes. By this time, 150 million years after the onset of the cycle, viscous drag had dissipated the tilt, the sharp contrast of seasons had abated, the humid tropics had widened towards the poles, and the common tropical flora had spread to progressively higher latitudes, and dissipation of the excess heat had allowed the mantle phase-interfaces to migrate upwards with the subsidence of the surface tumour, and allowed extensive invasion of warm shallow seas over the lands, characterised by the accumulation of chalk. Early in the Triassic Tasmania lay in high latitude. Yet "the overall aspect of the vertebrate fauna indicates a climate that was at least temperate, that lacked a marked cold season, and that was humid enough to maintain standing and flowing water during the entire year" (Cosgriff, 1974). Such a combination of limits is impossible on an earth with present climatic zonation, but is the predicted consequence of a low-obliquity earth, without polar frigidity.

It could be argued that habitat restrictions based on the ecology of modern descendents cannot with certainty be applied to ancient faunas and climates. Cosgriff examined this proposition carefully in the light of the anatomical features involved and concluded: "In summary then, it may be stated that Tasmania of the Early Triassic possessed a climate that was temperate in terms of temperature, and one that was not extremely arid. The only conditions of climate that are definitely excluded by the nature of the tetrapod fauna are aridity to the point of complete evaporation of surface water, and temperature drop to or below the freezing point." The Lower Triassic palaeo-latitude of Tasmania from remanent magnetism was 70° - 80°S.

Let us recapitulate this Gondwana cycle integration from the non-tilted times of the early Carboniferous to the non-tilted times of the late Cretaceous. First a mantle megatumour under Gondwanaland, causing increase in tilt, hence glaciation, and constriction of the humid tropics to a narrow equatorial zone separating independent hemispheric floral evolution, the universal Triassic regression, the disruption of Pangaea, the spreading to higher latitudes of the humid tropics as the tilt diminishes, with wide distribution of the Jurassic gymnosperms, the Cretaceous transgressions, and the final explosive diatremes of the diamond pipes - as the moribund megatumour gasps its last.

Alpide Syndrome

The Gondwana Cycle was not the only such cycle. The vigour of the continental dispersion initiated by the Gondwana cycle waned through the Cretaceous until, by the early Palaeogene, the earth enjoyed an epoch of comparative peace. But it was the calm before the storm. The later Eocene saw the onset of a new cycle, even greater than its predecessor. Tectonism erupted pan-globally on a large scale. Vast floods of basalts were poured out throughout eastern Australia, in the Deccan of India, in Greenland, the Rocky Mountain province, and in Siberia. Magmatic activity in the island arcs everywhere reached a crescendo. On the deep sea floors far from any land, every seismic profile and every deep-sea drilling shows this late Eocene unconformity (e.g. Nesteroff and von der Borch, 1973) and the universal siliceous cherts (seismic reflector horizon A) which recorded the excess silica content in the ocean waters, resulting from the stupendous submarine outpourings as the mid-oceanic rifts rapidly widened (Hermann, 1972). The dispersion of the continents during this Alpide Cycle since the Eocene, has been greater than in the Gondwana cycle, and so probably has been the total volume of basaltic extrusion (but in this case most of it was hidden under the new oceans). Antarctica, which had nestled into the Great Australian Bight until the Eocene, bid farewell, and the rift between has since widened by more than 3000 km. New disjunctive seas opened along the east coast of Asia and Australia, virtually from pole to pole, with progressively greater scale towards the south because of the asymmetry of the expansion. The latter, as expected, caused an axial tilt, now 23½°, with sharp seasonal differentiation and the onset of an ice age, which is still with us and will remain with us until the tilt dissipates. This Alpide cycle has run so far for 50 million years, but there is no sign yet that it is on the wane. The Gondwana cycle took three times as long to run its course. Maybe our diamonds are still to come!

Pan-African Syndrome

Further back in time we are beginning to identify another pan-global cycle originating in the deep mantle. Stewart (1972) has shown that, less than 800 m.y. ago, continental rifting athwart all earlier tectonic grain initiated the Cordilleran geosyncline and gave birth to the EoPacific separating North America and Asia, contemporaneously with the Pan-African tectono-thermal convulsion. The first Cordilleran sediments (Windermere Group correlates) were widespread glacial diamictites (Tindir, Rapitan, Toby, Pocatello and Kingston Peak Formations) correlated with the world-wide glaciation of that time, (Egan-Sturtian of Australia, Vendian of U.S.S.R., Varangar of Scandinavia, and several correlates in Africa), closely followed by basic volcanics, one to nearly two km thick (Irene, Leola, and Bannock Volcanics etc.). This association echoes the Late Palaeozoic glaciation, continental disruption, initiation of new oceans and basic irruptives of the Gondwana syndrome. Briden and Gass (1974) published a cumulative histogram of African radiometric age determinations, which shows up the Pan-African, Gondwana, and Alpide syndromes to the extent they affected Africa (Fig. 41). A longer series indicates a vista of such events back to the beginning.

Lower Palaeozoic Syndromes

Five to six hundred million years earlier, still another cycle is beginning to focus. Herz (1969) has pointed out that when Pangaea is

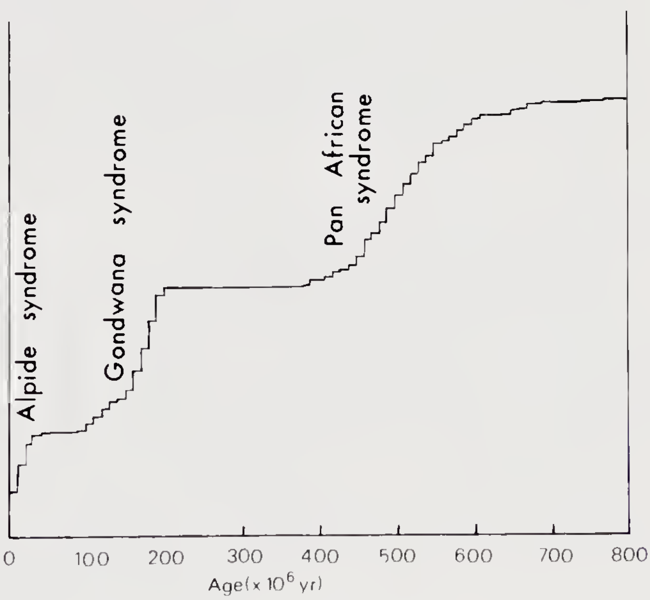


Fig. 41

Cumulative histogram of African radiogenic ages for the last 800 m.y., showing the Alpide, Gondwana, and Pan-African syndromes.
(After Briden and Gass, 1974).

restored, most anorthosites cluster in time and space into two belts, one in the north and one in the south. There was glaciation at about this time, and there were diamond pipes of about this age in Brazil, India, Africa, and Siberia; data are not clear enough to relate all these genetically, though it could be so. Perhaps two such cycles are merged in this generalisation. The Premier pipe near Pretoria is at least 1.2 b.y. old, possibly 1.7 b.y. The Roraima diamond placers of the Guyana shield are 1.7 b.y. old. Cratonic carbonatites and alkaline complexes are about 1.7 b.y. old. These dates seem to precede the anorthosite spread by three or four hundred million years.

Why anorthosites then, and tholeiites in the Gondwana cycle? Because of non-uniformitarianism. The temperature gradient was, to the best of knowledge, steeper. The pressure gradient could have been much steeper (as G appears to have been greater), and the stage of differentiation by degassing of the earth less advanced, all of which could modify the pattern of magma generation in the mantle. But each of the cycles does appear to represent the same kind of process - the inability of the heat generated in the earth to escape adequately by conduction or stable convection. Instead, an instability develops with the formation of a megatumour, gross wobble, and obliquity increase, and partial melting deep in the mantle, with the expulsion of the excess heat by magma floods, crustal disruption, and final diatreme explosions.

Symptoms of Early Syndromes

There must inevitably have been many other consequences. For example, the unusual climatic differentiation with unusual zonation of atmospheric transparency should modify faunas. Schinderwolf (1962) has emphasised three major discontinuities in faunal evolution (or *Faunenschnitte*) - EoCambrian, Permian and Eo-Tertiary. Is it coincidence that these were times of strong tilt, onset of glaciation, and rejuvenation of ocean floor growth?

Possible Causes of Syndromes

Many competing proposals have been made which might account for the succession of global syndromes. Einstein deduced that the universe should be expanding, and could be pulsating. Steiner (1967) proposed pulsating expansion correlated with the galactic year, and with first order geotectonic cycles. Runcorn (1962a) proposed that a major geological revolution would occur each time the number of lobes in the convection jet streams in the mantle increased in number (Fig. 42). This phenomenon could, of course, be another symptom of Steiner's cycle. Whatever other factors may be involved, asymmetry of heat flux and of rising isotherms seems to be the probable cause of the variation of obliquity.

Martian Parallels

Mars may have lost his initial atmosphere, and have been very moonlike for three-quarters of his history. Murray (1973) has suggested that the present CO_2 atmosphere came from late outgassing associated with the great volcanoes of Nix Olympia, Tharsis, Alba and Elysium. Water from the outgassing would have to charge the global watertable and hydrate the superficial rocks and dust before forming surface pools or ice-sheets. Some of the "chaotic terrain" has been interpreted as due to collapse of permafrost. The "laminated terrain" has been interpreted (e.g. Murray 1973) as produced by circumpolar deposits of snow and dust which record the migration of the pole after the associated CO_2 ice had sublimed. Rise of isotherms below a permafrost region would produce "lava" extrusions of water which could have eroded the sinuous channels and canyons which are currently the major mystery of Mars. Flow after flow of aqueous "lava" could have flowed down the same channel, not to fill a permanent lava basin as would normal lavas, but to sublime, leaving only their eroded channels and the collapsed chaotic terrain to indicate that they had ever existed. Very little of the erosion debris remains as braided channel deposits. The great bulk has long since joined the itinerant dust load of the red planet.

The parallels with the Gondwana syndrome are marked. First the asymmetry of the vulcanism. The Nix-Olympia-Tharsis group contains a large fraction of the total vulcanism, and all the vulcanism lies in one half of the "equatorial" belt (from 90°W to 90°E), and the other half has the greatest development of the "equatorial" shear zone. This combined "equatorial" zone is tilted 40° to the present equator, just as the Tethyan Shear zone is tilted 40° to the present equator, although along the late Palaeozoic equator. Thus just as in the Gondwana syndrome on Earth, so

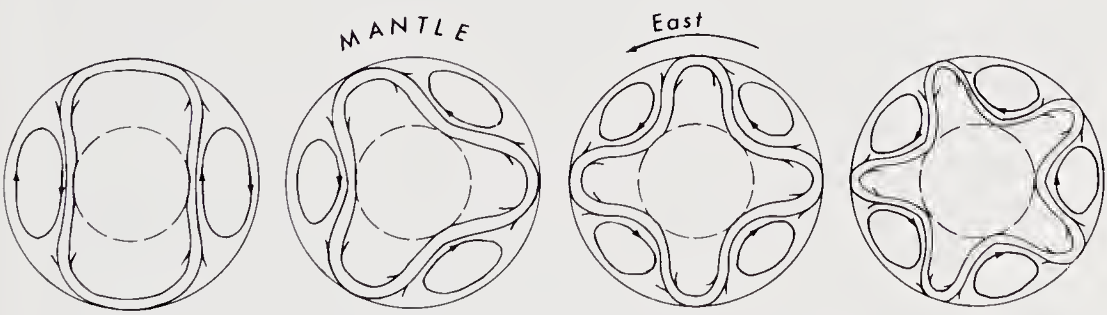


Fig. 42 Rotational jet-streams, with 2-, 3-, 4-, and 5- lobes round convection cells, as proposed by Runcorn (1962), based on the empirical observations of Hide.

the Nix Olympia syndrome on Mars has a great asymmetric exothermic magmatic tumour associated with crustal expansion, producing a global system of tensional rifts, a consequential large tilt of the rotation axis, and development of polar caps, which could not occur without tilt. Mars, like Earth, shows an increase of tectonic tempo with time. For Mars, the first rains are yet to fall!

NON-UNIFORM EXPRESSION

Uniqueness of every " now "— or the global persistence of facies

A central theme of Ager's book, *The nature of the stratigraphic record* (1973), is that geological epochs are not repetitive castings from common moulds, but on the contrary, each has its own individuality, and that particular phenomena are characteristic of specific points of time. Robinson (1971) remarked: "The degree to which certain lithologies become abundant only in certain geological systems almost causes one to lose faith in the principle of uniformitarianism."

For living matter this was early apparent, but for geological processes phenomena and products, the concept of repetitive uniformitarianism has often been taken for granted. But Ager emphasises that at particular times in earth history peculiarly individualistic types of sedimentary environment prevailed over vast areas of the earth's surface. He traces the Upper Cretaceous chalk, the particular limestone facies of the Urgonian and of the Tithonian, the Kimmeridge lithographic stones such as the Solonhofen, the New Red Sandstones with associated liver-coloured sediments and "brownstone" masonry, the Carboniferous coal measures, Mississippian limes, Frasnian reefs, Old Red Sandstone, Wenlock limestones, Arenig quartzites, and the basal Cambrian. There are several limestones and several quartzites in this list, but they are different. Each bears its birthmark and fingerprint wherever it occurs. Similar unique facies occur further back through the Precambrian.

Ager continues (p.20): "The greatest problems in the fossil record, however, are the sudden extinctions. Examples such as the disappearance of the dinosaurs have been chewed over and over *ad nauseam*, with every possible cause blamed, from meteoric impact to chronic constipation. For any one ecological group, such as the dinosaurs, it is comparatively easy to find a possible cause. It is much less easy when one has to explain the simultaneous extinction of several unrelated groups, ranging from ammonites to pterodactyls, living in different habitats at the end of the Mesozoic".

Which, in turn, led Ager on to his second proposition: *"Palaeontologists cannot live by uniformitarianism alone"*.

Proterozoic Banded-iron Formations

During the Lower Proterozoic, when oxygen was perhaps 0.1% of the atmosphere, widespread deposits of banded iron formations were laid down on all the continents - - the Animikie basin of North America, the Hammersley Yampi and Middleback regions of Australia, the Dharwars of India, in Rhodesia and the Transvaal in southern Africa, in the Krivoi Rog formations of the Ukraine, on the Sveccofennides-Karelides of the Baltic shield, and still others in the Brazilian shield.

A remarkable feature of these deposits is their cyclic stratification on an hierarchy of scales:

(a) Megacycles, on a thickness scale of several hundred metres, which alternate clastic formations and *iron formations* (e.g. in the Hammersley province we find the Jerrinah Shale and *Marra Mamba*, Wittenoom and *Mt. Sylvia*, McRae and *Dales Gorge*, Whaleback and *Joffre*, Yandicoogina and *Weeli Wolli*, Woongarra and *Boolgeeda*).

(b) Macrobands, on a thickness scale of metres, which alternate clastic bands with iron-rich bands within the iron formations italicised above.

(c) Mesobands, on a thickness scale of centimetres, yielding the conspicuous ribbon striping of these rocks in outcrop and hand specimen, with alternating zones dominated by chert and iron oxides.

(d) Microbands, on the millimetre scale, which are conspicuous in the cherty phases of the mesoband cycles and difficult to identify (if they are present) in the iron-rich phase of the mesobands.

This hierarchy of cycles and their extensive field distribution through the Hammersley region have been brilliantly synthesised by Trendall and his colleagues of the Geological Survey of Western Australia, backed by thorough mineralogical and petrological study. (See Bulletin 119 of the Geological Survey, and other papers by Trendall in the bibliography). Trendall has shown that even the paper-thin microbands can be correlated precisely over hundreds of kilometres (Fig. 43).

Huebschman (1972) reported similar persistent microbanding in the Proterozoic Pritchard and Aldridge formations of the Rocky Mountain region:

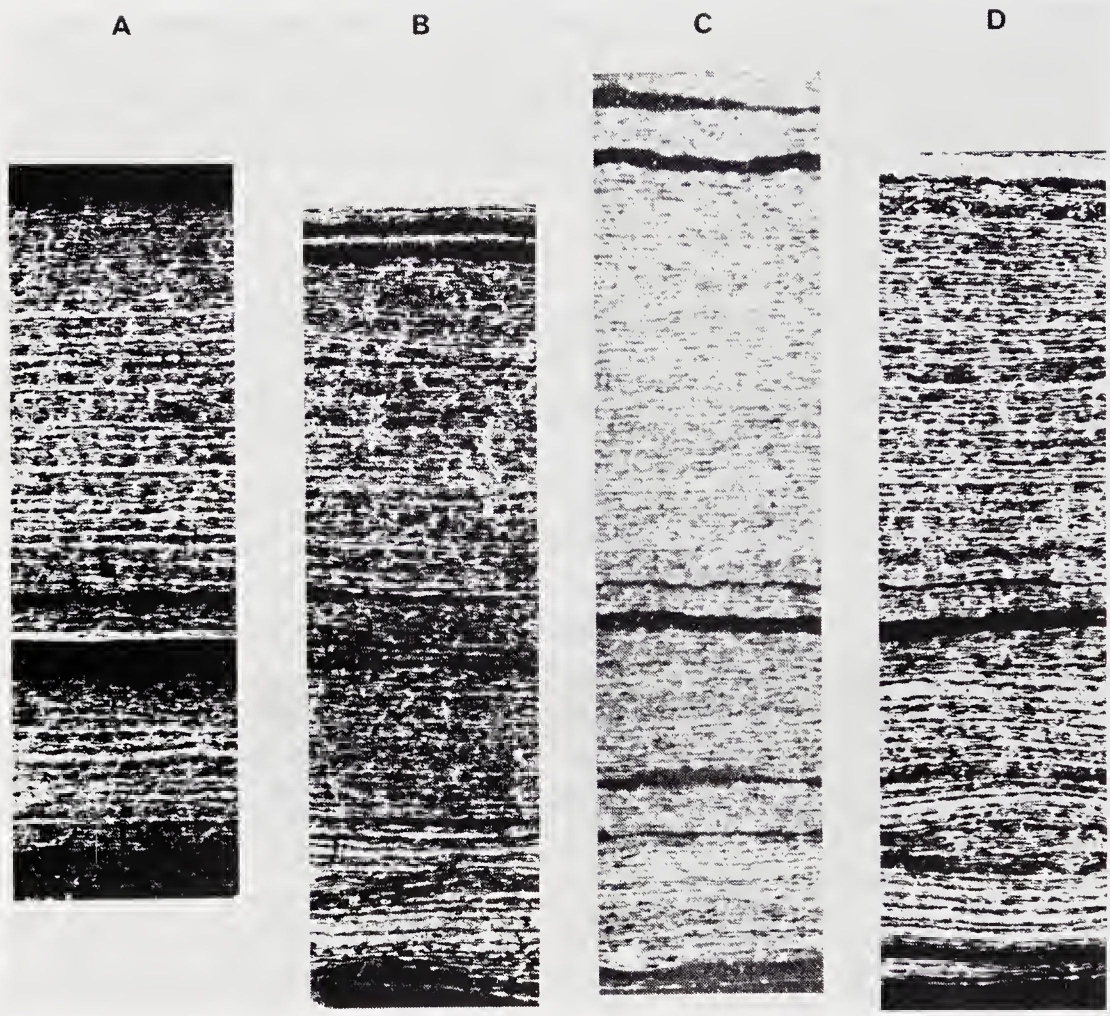


Fig. 43

Comparison of the chert-magnetite group at about 36.6 to 36.85 feet in the type section of the Dales Gorge Member at widely separated localities.

A Thin-section from Woongarra Gorge (lat. 22° 52' 30"S., long. 117° 07' 30"E).

B Thin-section from Point James (lat. 20° 58'S., long. 116° 10'E).

C Surface photograph of the type section core from Hole 47A at Wittenoom Gorge.

D Thin-section from Dales Gorge (lat. 22° 28'S., Long. 118° 33'E).

The localities A, B, and D form a triangle with sides of length 92, 145, and 185 miles. C, and D are, by comparison, only 19 miles apart. The lateral correlation of internal irregularities of the microbanding below mesoband scale is evident. (From Trendall and Blockley, 1970).

"The banded member of unit A is truly remarkable. It contains a sequence of light and dark gray bands 1 mm to 2 cm thick which can be matched, band-for-band, from one outcrop to another throughout the entire thesis area. Individual bands are strikingly uniform, and even the paper-thin bands maintain the same thickness in a single outcrop. A 13-foot 'type' section of the banded member was cored and photographed to scale. The photos of a unique six-inch sequence of bands within the banded member were then matched to the outcrop from one location to another. Over a distance of several miles the bands match perfectly ... Over tens of miles, each individual light and dark band varies in thickness proportionally to adjacent bands so that the same sequence of bands remains equally expanded or decreased ... The geographical range over which the banded member was traced is approximately 100 miles north-south and approximately 30 miles east-west, an area of about 3,000 square miles. Remarkably, each lamina down to 1 mm thick can be traced over the entire area".

Most students of banded-iron formations have explained them in terms of chemical or biochemical precipitation from the sea-water along uniformitarian principles. A difficulty is that solution in seawater appears incapable quantitatively of being the vehicle of transport from subaerial source to deposition (Kaplan, Emery and Rittenberg, 1963). A clastic carrier seems necessary. But to switch on and off the hierarchy of cycles synchronously over such large areas, in all continents of the globe seems beyond the capacity even of Maxwell's demon. But when we assemble the very different conditions of the Lower Proterozoic, a rational explanation emerges. Briefly, I suggest that the micro-bands represent individual dust-storms, which are pulsed by a lunar cycle, that the mesobands are seasonal and annual, and that the macro-bands and megacycles represent advance and retreat of the shorelines, caused by longer cycles. Consideration of a dust-storm origin for the microbands has been independently proposed by Haddon F. King, following extensive field experience in the Hamersley iron province of Western Australia and visits to iron provinces on other continents.

Before the advent of land plants, wind must have been the dominant carrier of fragmental materials on Earth. A modern analogy (but much less extreme) is the Great Stony Desert of central Australia, the so-called "gibber" plains, where everything moveable by wind has been moved. Sand-ridge country extends for hundreds of kilometres downwind from the ablated centre - - longitudinal dunes made entirely of sand-grade particles which skip and saltate before the wind. For hundreds of kilometres beyond the sand-ridge zone is the "bull-dust" country, mantled with tropical loess, consisting entirely of silt-grade material, which has been carried high into the atmosphere, and which advances as a dust-storm front, occluding the landscape, and blotting out the sky until the wind turbulence drops. Meanwhile the clay-grade material remains in the atmosphere haze until it is washed out by rain. When a dust storm is passing over land, dropped material may be picked up again by the next turbulent eddy; but over water loss continually occurs at the bottom of the storm. Thus a fine silt lamina is spread throughout the basin. Hence individual laminae may be correlated precisely throughout the basin, with the same certainty as the correlation of the wind-borne bentonite laminae through the Cretaceous of Wyoming and adjacent states.

Similarly the Pennsylvanian *tonstein* horizons of western Europe (Millot, 1970, p. 166-9) which are remarkable for their persistence and petrological consistency over hundreds of kilometres, appear to me to be wind-borne silts made up of illitic clots formed during a period of high axial obliquity, strong seasonality, and desert conditions encroaching towards the equator, while glaciers extended from the poles. In the same climatic cycle in Gondwanaland clays are reported implying a provenance from much lower latitudes than indicated by palaeomagnetism. These too may be wind-transported parna.

Under the conditions of the Lower Proterozoic, with a very much smaller earth, no large oceans of the modern type, an atmosphere without oxygen, and no vegetation, such dust-storms may have become panglobal, like the dust-storm which obscured the whole of Mars at the time of the arrival thither of spacecraft, Mariner 9. Such wide derivation satisfies the constraint of the narrow uniformity of composition of individual bands, the variation in composition between different bands, and the difficulty in providing so much iron to a single basin from a local source.

Linear projection back of the currently-observed recession of the moon (van Flandern, 1974) would bring the moon to close proximity of the earth by the late Proterozoic, but correction for diminishing G changes the regression from linear to exponential so that we may contemplate for the banded iron formations a near moon in highly eccentric orbit with a few days in a "month". The near moon, perhaps thermally in resonance with the sun, would profoundly effect the atmosphere regime with much stronger atmospheric tidal effects, and would also produce exceptionally large ocean tides at perigee, and still large but smaller tides at apogee. The high tides at perigee would attack the dunes at the shore and the coastal loess and produce clastic laminae interbanding with the purely wind-borne laminae.

Many have correlated the microbands with annual layers - a uniformitarian mental transfer from some modern finely-laminated Pleistocene varves back two thousand million years to the Middle Proterozoic, although Newell (1967) has reminded us that "there is no *a priori* reason to think that rates known today are representative for all geologic history", and Ager (1973) has emphasised that *average* rates, even for an epoch, are meaningless.

The annual mesobands imply strong seasons, and significant axial obliquity, at least as much as the present $23\frac{1}{2}^{\circ}$. Such seasonal changes could cause marked alternation in the transport regime - for example the present contrast between the northwest monsoonal winds and those of the southeast season. On Mars the seasons cause an annual migration polewards and equatorwards of the dust cover, which had been interpreted (before the nearby photography) as spring-fall vegetation cycles.

The macrobands and the megacycles seem best interpreted in terms of fluctuation of sea-level, whether this be climatic or eustatic. Transgression must significantly reduce the wind-transported fractions by covering the most susceptible areas and leaving the non-productive stony desert central regions as a much larger proportion of the land surface, and at the same time transgression may significantly increase the water-transported clastic fraction, because the waves attack large areas of dunes and coastal dust-plains.

We may also speculate on the nature of the dust. In the oxygen-deficient atmosphere rich in carbon dioxide, carbonates - siderite, ankerite, dolomite, calcite, rhodochrosite - may have been much more abundant weathering products from mafic rocks, and could be stable in this environment. In the gibber sandblasting stage, and the dune-transport stage, such carbonates would be susceptible to fracture to small silt-size cleavage grains, and at micron size could form a large proportion of the dust-storm minerals, notwithstanding its 3.89 density.

In this model the banded-iron formation starts as carbonate silt bands interlayered with clastics. Rising load pressure and temperature during burial and diagenesis transforms siderite to haematite and magnetite. The CO_2 would leave the system along with perhaps forty per cent by volume of water which is driven out during compaction. Many diverse processes, permeability paths, structural configuration (syncline enrichment), the dolerite dykes and sills, supergene enrichment during the Late-Proterozoic and Mesozoic peneplanations, etc. have caused differential redistribution of the iron to yield the large high-grade ore bodies.

Several geochemical problems, such as the replacement of calcite by silica in the chert bands, the source of graphite which forms the micro-banding in many quartz-sericite bands, and the conspicuous deficiency of aluminium in the iron formations as a whole, have been widely canvassed. If the aluminium found its way into phyllosilicates during the weathering stage, such minerals at micron size could have been separated into the finest fraction which remains in the atmosphere in Brownian movement until precipitated as rain far away.

Ore Deposits

Gorzhevsky and Kozerenko (1964), Salop (1964), and Pereira and Dixon (1965) have reviewed evolutionary trends in ore deposition, and concluded that with the passage of time there was an increase in the diversity of morphological types; that particular combinations of associated rocks or environments are significantly more characteristic of some geological periods than others; and that major deposits of particular metals tend to have a maximum abundance in definable geological periods, and that this pattern is related to contemporary geochemical and geobiological conditions and processes. Haddon King in his 1971 invitation lecture to the Society of Economic Geologists (1973) emphasised the same theme. Subsequently Dr. Janet Watson (1973) reviewed the influence of crustal evolution on ore deposition: "Most ore deposits older than 2.7 b.y. derived their material directly or through only one intermediate stage from sources in the mantle; they are deficient in those metals which are easily dispersed under unstable conditions. The evolution of the first stable and relatively cool cratonic crustal units at about 2.7 b.y. was followed by an increase in the variety of ore deposits. The first deposits concentrated by sedimentary processes date from this time, as do the first important concentrations of mobile elements such as uranium. During subsequent stages of earth history new mineralization was related both to the influx of magmas from the mantle and to the recycling of metals already dispersed in the crust. The transference of such metals as copper, lead and zinc to high levels in the crust was commonly followed by the appearance of concentrations of the same metals in sedimentary sequences of the adjacent cratons. The most constant styles of mineralization have been those resulting from the rise of partial melts generated in the mantle. The most variable have been those which involve concentration by sedimentary processes, which have been progressively influenced by the effects of organic evolution."

The relative abundance of particular types of ore at different periods has often been interpreted as resulting from progressively deeper erosion of orogenic roots. Certainly several cycles of peneplanation and isostatic rejuvenation are needed to expose orogenic hearths. Laznicka (1973) has developed this theme. However, although Laznicka's arguments are clearly valid, I agree with Watson that these effects, although real, are grossly swamped by evolution of the processes of ore genesis.

Reverting to the environment of the early Proterozoic discussed above in relation to the banded-iron formations, it is clear that supergene and detrital ore-forming processes must have been very different from those of the more recent periods. In the absence of atmospheric oxygen and of humic organic matter, sulphides and uraninite were transported non-oxidised to form deposits like the Khatanga and Zambia ores, which of course suffered subsequent diagenetic and metasomatic redistribution and enrichment, much as did the banded-iron formations. Such sulphides would have been transported largely by the wind in dunes, to be distributed finally by wave action at times of high sea-level. As wind was the principal transporting agent for the sand silt and clay grades, the streams would bear a much greater proportion of pebble and cobble load than do modern streams, along with heavy minerals such as gold, osmiridium and uraninite. Hence the Witwatersrand cobble conglomerates of banket-type characteristic of this age. Tin-bearing gravels did not form, because they needed the humic weathering of the post-Palaeozoic to disintegrate their host granites and free the cassiterite for rapid concentration.

This process does not directly explain the widely reported correlation between the carbon content of such bankets and their tenor of gold and uranium. I suspect that this correlation stems, not from the primary transport process, but from the subsequent processes of concentration and enrichment. Most observers think that contemporary organisms were confined by radiation to water depths of at least twenty metres (e.g. Plumstead, 1973), which implies that any contribution by organisms to concentration of gold or uranium occurred after the materials had been transported to their deposition site.

Igneous mimicry

Early in the uniformitarian debate, the identity was recognized between lavas seen to flow from a volcano and to crystallise, and prehistoric rocks cropping out in the field. The principle of uniformitarianism therefore ordained that the latter were the products of former volcanism, long since extinct. This principle was progressively extended until a very wide range of holocrystalline and porphyritic rocks were attributed to former igneous activity. However the ever-widening category began to overlap with empire-building from the opposite direction. Coarsely crystalline marble was obviously the product of metamorphism of sedimentary rock, as was quartzite, quartz-schist, phyllite, mica-schist, and thence to the garnet gneisses and foliated quartz-feldspar-mica gneisses of conformable diapirs which were already part of the igneous empire. Granite itself, which ruled supreme as the paradigm of orogenic igneous activity, was also cited as a metasomatic rock directly derived from the recrystallisation of former sediments in situ. The textures did not differ noticeably from those of granulites; the difference in mineralogy simply expressed different original material but, more particularly, appropriate combination of temperature, pressure, and gas-phase water; and field evidence of truncated and missing sediments, and the problem of accepting the overall volume of the granite into a compressing environment, appeared to favour the granitizers. This confrontation engendered heat and polarization in the eighties of last century and again in the thirties and forties of this.

A parallel suite involves on the one hand tuffs ignimbrites and quartz-feldspar-porphyries, which are observably derived from volcanoes, and on the other hand, the recrystallisation of sedimentary quickstones to quartz-feldspar porphyroids, which are interpreted by uniformitarianists as sheared tuffs and lavas. Here again granite of sedimentary origin is logically an ultimate product. Initial recognition and intensive investigation of this problem is due to brilliant work by J.N. Elliston, who encountered such rocks in the Warramunga geosyncline in the Tennant Creek region of the Australian Northern Territory (Elliston, 1963, 1968). His study involved three separable problems. First, the field problem of structural and tectonic mass movement in a geosyncline, on all scales from normal bottom traction, turbidity currents, surface slumps and slides, closed-cast quickstone transport and injection, to regional nappes of Alpine dimensions in non-orogenic environment (e.g. Heezen and Drake, 1963 & Korn and Martin, 1959). Second, the automatic recrystallisation of the quick sediments to quasi-igneous porphyroids with megacrysts of quartz and feldspars. This involved the full range of silica hydration, and the gel-sol inversion, and crystallisation with energy release. Third, the desorption from the phyllosilicates and other colloid-size minerals of heavy metal ions, which are then transported by the dewatering process to form ore bodies in adjacent structural or chemical traps.

Classical concepts of folding have been largely polarized through the filter of vice-like compression, although there have been persistent schools which granted a dominating role to movements towards gravity potential minima.

Every slope involves gravity-caused stress-differences which must cause secular creep towards the hydrostatic state at a rate depending on the "viscosity" and the magnitude of the stress-difference. Very slow movements produce very large strains when time is long. As weight (the accelerating force) increases by the cube of length and viscous forces (the retarding forces) by the square, large size also increases the relative importance of gravity flow.

Hence gravity deformation should be expected to be important in geological processes, for both time and size are great. Conversely, a laboratory model of a geosyncline scaled down 10^5 in size and speeded up 10^7 in time would need a sediment analogue reduced 10^{12} in viscosity.

On this basis gravity creep towards the trough should *a priori* be a significant phenomenon in the development of geosynclines. Turbidity flow and slumping are of course widely known and documented. But these are relatively small rapid superficial phenomena, complementary to larger-scale movements involving the main body of the geosynclinal filling.

Gravity movements at this stage are necessarily *towards the trough* - hence the term *syntaphral* which means just that, to distinguish them from the ensuing stage when the geosynclinal gut is extruded upwards several kilometres through the axial zones of the trough *(diataphral)* and the final stage when the rising welt spreads laterally under gravity, producing nappes and thrusts, and recumbent folds directed away from the axial zone *(apotaphral)*.

The viscosity of accumulating sediments increases with kind of material and with burial, from a few centipoises (say 10^{-1} poises) at the water interface to 10^{18} poises or higher in compact lithified sediments (crystalline limestone about 10^{21}). At 10^{18} poises (and many mudstones would be in this range) the pile would flow as much in 10,000 years as a glacier does in a year, and ten thousand years is really not long in this context and a year's deformation of a glacier is far from insignificant. The assumption, so often implicit in structural statements, that points now "above" other points in a geosyncline were originally deposited in that relationship, is clearly wrong. Competent layers inhibit such movement and, in converse, other processes operate to greatly increase them.

When sediments are deposited, the whole of the interstitial voids are completely filled with water. Superposition of other layers produces progressive compaction of the sediments. The addition of an extra metre of sediments at the top increases the load on *every* underlying stratum right to the base, and each tends to consolidate a little more. In this way the density, initially less than 2, increases at a diminishing rate with burial towards an asymptote at about 2.5. This increase in density is attained mainly by reduction of void space. As all voids are filled at all times with water, the reduction of void space can only occur by the driving out of water. The rate of water movement is determined by (a) hydraulic gradient, the excess pressure over hydrostatic at that depth divided by the path distance to the outcrop, (b) the permeability of the sediments along this path. Flow along bedding can follow the most permeable beds whereas flow across the bedding must cross the least permeable beds. As the permeability varies through many orders of magnitude there is a strong polarization of flow along the stratification. Since the rate of flow diminishes as the hydraulic gradient decreases it would take an infinite time for the excess fluid pressure in the sediments to diminish to zero, so there is always some degree of excess pressure in any sedimentary pile.

In an active geosyncline the rapid accumulation of sediments increases the excess pressure much more rapidly than it can be relieved by seepage to the outcrop, so that water pressures are normally well above hydrostatic and are further increased by gas generation and rising temperatures, so that they may approach lithostatic pressure. These excess pressures take long periods of time to dissipate, and it is normal to find significant degrees of excess pressure in drilling young sedimentary basins for oil.

Excess water pressure acts on the surfaces of the mineral grains tending to force them apart. This reduces the friction between grains and reduces the "viscosity" of the sediments (rate of shear under stress-difference). The effect of water pressure is to greatly reduce effective viscosity of some of the strata, so that gravitational flow on these beds is greatly increased.

Sands are affected much more than are pelites. Most of the pressure relief takes place by flow through the sandstones, because of their greater permeability. In addition, capillarity and the surface bonding between clay particles, and between clay surfaces and polar water molecules, yield significant forces which oppose the dilatant forces of the excess pressure. Hence as the excess pressure increases it is the sand rather than the clay which first becomes "quick", so that eventually we find sandstone seams and dykes and irregularly intrusive bodies much more commonly than we find equivalent intrusions of pelite.

The picture of our filling geosyncline now changes from the one suggested earlier of an ice sheet tending to creep towards the trough, to a more complex model with the viscosity of some strata being reduced in relation to others, and some even becoming quicksands. The whole mass creeps towards the trough but the rate of flow is differential. All beds - in addition to reduction of vertical thickness by consolidation, also thin through area increase as the whole mass creeps towards the trough, where piling up causes syntaphral deformation which may be of a substantial scale. The motion varies throughout the spectrum from distributed flow throughout the mass (like an ice sheet) to increased flow on some horizons, to actual quicksand flow and injection. Limestones and competent beds from early lithification may behave passively, or inhibit the process. Pelites between quicksand sandstones may stretch uniformly while the rate of strain is low, and a time comes when they neck or pull-apart like boudins.

So far we have been concerned only with excess water pressure in the sands. However, the claystones do not necessarily remain inert. Most pelites show some degree of "sensitivity" to deformation. This may result in part from the kind of packing of the clay flocks which is stable so long as it is not deformed. Deformation leads to structural change with collapse of "house-of-cards" flocks and drawing together of clay plates under electrostatic forces into a parallel packing, with forceful expulsion of water. Such processes are not reversible. Other sensitivities are thixotropic, for example undisturbed clay particles adsorb water in an orderly quasi-crystalline arrangement to form a stiff clay which on deformation breaks down with release of water. Both these processes (and others) release water, and in each case the effect of deformation of a stiff pelite produces a more fluid slurry.

Hence rapid filling of a geosyncline produces excess water pressures which in time may cause some of the sands to become quick, and resulting acceleration of syntaphral creep may cause some of the more sensitive pelites to break down to a slurry which in time increases still further the overall rate of movement. These creep processes are inevitable and self-generating, but may be triggered off by an earthquake shock to produce a large flow of nappe-like dimensions.

Tectonic consequences

The tectonic effects of these processes are:

(1) The axial zone of the geosyncline receives creeping masses, compressive foldings, nappe-like sheets, disintegrating slumps (chaos) and turbidity flows. As this is the axial zone of the later orogen, these early structures are normally over-printed by later deformation, and are likely to be interpreted as an earlier orogenic phase - perhaps correlated with an "important" orogenic phase elsewhere. In fact such foldings and overthrustings, even though of a large scale, have little tectonic significance, no more in fact than the outflow of the Malaspina glacier - moving likewise in accordance with its weight, its viscosity, and its profile.

(2) Nappe-like sheets of large dimensions may travel down slope and come to rest on sediment of different facies. For example, Heezen and Ewing (1952) and Heezen and Drake (1963) describe a sheet of more than 10,000 sq. km. in area, and approaching a kilometre in thickness which travelled forward many kilometres and came to rest on a sedimentary sequence covering the same time interval as the sheet. This is of comparable dimensions to many Alpine nappes and to the Taconic Klippe of New York and Vermont, and in due course when it has gone through the rest of its cycle of geosynclinal evolution, enlightened geologists of the future perhaps will discover this important nappe, wax eloquent about the Grand Banks Orogeny, and calculate the implied crustal shortening, and map in detail the "unconformity" between the sediments affected by this orogeny and those which cover it!

Does the Taconic Klippe have more tectonic significance than this important syntaphral slide? Similar phenomena on similar scale are known from the sea floor around Hawaii. They have been recognized in the geosynclinal sediments of Turkey, and in the Permian of Timor and from the Tertiary of Peru. I have no doubt that they are very common in all geosynclines and it is necessary that all geologists should recognize and identify these large scale syntaphral nappes and foldings which belong to this early "pre-orogenic" stage of the development of a geosyncline.

(3) The quickened sands and mudstones which form the mobile zones for these movements have fluid characteristics similar to magmas. They therefore form intrusive bodies which may be sheet-like along the decollement zone, or cross-cutting dykes and sills where the upper plate starts to break up. All kinds of breccia and intrusive contacts may develop.

Differential viscosity produces differential behaviour in a given stress field. The low viscosity materials flow more rapidly than the high viscosity materials, but as the different layers are bonded to each other they interact. A difference of viscosity of a couple of orders results in boudinage in the material with the higher viscosity. With several orders of magnitude difference in the viscosities, "brittle" pull-apart fractures appear in the higher viscosity material, and as the difference becomes wider, veins, dykes, apopheses and sills of the mobile material freely intrude the higher viscosity sediment with magma-like relations. An increasing proportion of the overall stress-difference is borne by the higher viscosity layers, which undergo elastic buckling, and fold disharmoniously with respect to each other. A fold which starts as a buckling phenomenon may develop rapidly by the inflow of low viscosity material, so that the bed must extend more rapidly than it could do by flow and so it breaks brittly around the curve of the fold - the tablets being separated by low viscosity material. Where the muds are sensitive they break down progressively to slurry as the movement proceeds, adding to the volume of mobilised material and to the large scale mobility of the mass. In this way large quickstone sheets and lenses develop with intrusive contacts.

While these large intrusive sheets are made of unmistakable sedimentary material, the resulting structures are strange but not deceptive. But if a process should ensue which gives these large sedimentary intrusions any of the other characters commonly associated with igneous rocks, the stage is set for hot controversy over the interpretation and genesis of the resulting outcrops. This is just what seems to have happened in the Tennant Creek outcrops described by Elliston (*loc. cit.*).

DIAGENETIC AND METASOMATIC CHANGES IN QUICKSTONES

Empirically quickstones seem to have been much more susceptible to diagenetic and metasomatic changes than their host sediments in the same environment. In some environments the change seems to be dolomitization in others silicification. Dolomitization, silicification, and replacement by sulphides seem to occur in particular quickstone environments, but elsewhere megacrysts of quartz and of potash feldspar seem to grow in the quickstone, resulting in porphyroidal and quasi-igneous rocks.

The well-known Italian building marble from La Spezia, known in the trade as "Portoro", or "Black and Gold", shows the dolomitization very well. The dense black calcilutite was separated by thin beds of calcarenite. When these become quick, distributed glacier-like creep spreads through the entire mass. The stratification is somewhat variable, but in one variety the main black calcilutite beds are 10 cm. or so thick, separated by groups of three or four alternating beds of calcarenite and calcilutite, a centimetre or so thick. The motion begins in the calcarenites in these thin-bedded zones and quickly leads to pull-apart cracks in the thin calcilutites. These then roll and rotate and suffer erosion, yielding their substance to the quickstone. At a later stage dolomitization occurs preferentially in the quickstone forming the "gold" layers with the dolomite growing at the expense of the black calcilutite olistoliths. The

Fig. 44 Development of quickstone brecciation in Potoro Marble, La Spezia, Italy. White areas are secondary dolomite replacing quickstone. On left side a pelite bed shows the beginning of parting. Elsewhere separation, rotation and erosion of the pieces are more advanced. (From Carey, 1967).

result is bands of black little-altered calcilutite several centimetres wide, separated by bands containing black fragments set in "gold" secondary dolomite. (Fig. 44)

As an example of sulphide replacement I would suggest the crenulated ore or the Racecourse lode at Mt. Isa, although the genesis of the pyrite and other sulphides there has been the subject of a number of interpretations and is still a matter of controversy. Figure 45 shows a tracing from a polished slab, in which a 10cm. thick pelite layer (A) goes right through without deformation, a 1½ cm. thick bed (B) goes through with one significant fold, a 3 mm. bed (C) suffers intense crenulation and pull-apart. The thick pelite (A) is free of sulphide, and sulphide increases with the degree of motion. Elsewhere the crenulated ore shows beautiful drag folds with consistent sense, the non-sulphide layers (a few mm. thick) fold disharmoniously with respect to each other, and suffer tablet-like pull-aparts, all as though the intervening sulphide zone was fluid slurry during the deformation. The sulphide forms miniature "sills" and dykes in the non-sulphide folded beds, just as though it were an intrusive sulphide magma (in fact this was an early interpretation). What is now sulphide was

Fig. 45 Crenulated ore, Mt. Isa, Queensland. (Tracing from a polished slab). Black bands are fine sediment not replaced by sulphide. Blank areas are rich in sulphide. (From Carey, 1967).

originally quickened sediment, and the crenulation occurred during syntaphral movement of the present footwall when both had the original gently-dipping attitude of their original deposition. The lode zone moves slightly transgressively across the bedding as one rises in level up the mine. I suggest that this is the normal transgression of a quickstone decollement. Whether the sulphides are replacement of original different quickstones or whether a sulphide-rich "sensitive" sediment became quick with little change of chemical composition, might be left open for the purposes of this discussion. The small spherical shells, which have been interpreted as organic, could reasonably be interpreted as colloidal structures, as Elliston has pointed out.

Cut and polished slices of "chert breccia" show a mass of disrupted sediment blocks in a base containing a variety of quickstones (Fig. 46).

Fig. 46

Quickstone chert breccia, Mt. Bischoff, Tasmania. Polished surface. (From Carey, 1967).

Here it would seem that silicification followed the quickstone movement. The white and light-tone areas are the sandy quickstone. The darker parts are pelitic fragments. The light bed about 5 cm thick near the top is a sandy quickstone with few small pelitic fragments. At least three successive quick movements are recorded, with some intervening consolidation, because some of the fragments are themselves quickstone microbreccias.

DISCRIMINATION BETWEEN SYNTRAPHRAL AND LATE-TECTONIC STRUCTURES

The structures in the Mt. Isa Racecourse lode have been interpreted as tectonic drag folds - in the sense of "hard-rock" deformation whereas it is now suggested that they were formed in very soft conditions. What criteria can be used to separate syntaphral from later structures?

In "hard-rock" deformation pelites are usually the less competent rocks, so that boudins of quartzite or amphibolite are found in a more yielding ground of mudstone, slate or schist. Likewise where disharmony occurs between beds it is the pelite derivative which usually forms the yielding accommodating material. In syntaphral deformation this is commonly reversed. *Sand*stone dykes, pipes, sheets, and sill-lets are common. Boudins and pull-aparts are common in *mud*stone and mudstone derivatives. It is of course possible to have pelitic quickstones of syntaphral origin derived from the liquification of sensitive pelites. It would seem that intrusive or incompetent behaviour in sandstone is indicative of soft sediment origin, but incompetent pelitic material might not always be indicative.

Cleavage is a problem. Many structural geologists take it for granted that cleavage (they may call it "true cleavage" or "axial plane cleavage") is evidence of "hard-rock" deformation. However this is essentially an article of faith, for I can find no evidence to compel this conclusion in all cases, and some to deny it. Penetrative cleavage seems mostly to involve crystallization of minerals with orientation related to the principal stress axes, but even this might conceivably result in part from colloidal crystallization during flow. For my part I would leave this door open.

The vergence of syntaphral folds and decollements are directed towards the orogenic axis. Apotaphral folds are directed away from the orogenic zone (e.g. the Valley-and-Ridge province of the Appalachian foothills, the Laramide folds of the Rocky Mountain Front, etc.).

Scale is not a criterion of "orogenic" as opposed to early deformation. I see no logical reason to expect syntaphral structures to be small, and much empirical evidence to the contrary.

Diagenesis

Empirically, quickstones are particularly susceptible to diagenesis, presumably because the inception of a quicksand involves significant dilatation and volume increase, specifically increase in the volume of the interstitial voids. Hence the quicksands become the most permeable zones of the geosyncline. Water, in amount equal to about a quarter of the total volume of the whole sedimentary pile, is driven out of the geosyncline during compaction. Very much more than its share of this passes through the quicksand zones. This water moves from higher pressures to lower pressures, from higher temperatures to lower temperatures, from high solute concentrations to lower solute concentrations. This water travels with juvenile waters, rising along the axis of the geosynclinal zone which becomes the orogenic zone (Fig. 28) and may bear *inter alia* silica and base metals (as in the Red Sea waters) and organic fluids and gases which elsewhere could accumulate as hydrocarbon concentrations.

These itinerant fluids are the instruments of diagenesis. Their journey is long (from the geosynclinal gut to the distant outcrop), but is secularly slow in any but geological time-scales. They remain close to geochemical equilibrium with their staging hosts, precipitating, dissolving, exchanging.

Whatever the local contemporary diagenetic process, that process will be more advanced in the quickstone than in the rocks intruded by the quickstone. In some places the dominant diagenetic process is dolomitization as in figure 44; elsewhere it may be silicification as in figure 46; or it may be the precipitation of sulphides as in figure 45; or it may be the crystallisation of quartz and feldspars as in the quartz-feldspar porphyroids described by Elliston. Laboratory experimental work on the stability fields of the minerals in such rocks use only pressure and temperature - not stress-difference, which may substitute for confining pressure, especially with schistophile minerals. The environment involved there is certainly one of flow. Another exothermic factor in the thermodynamics of the system is the change from the gel state to crystalline.

Such rocks are baited traps for uniformitarianists. Some lavas, ignimbrites, and tephra, observed to have issued from volcanoes are porphyries or fragmental rocks. Plausible extension from these include quartz feldspar-porphyries and quartz-feldspar-porphyroids, although no one has seen a porphyroid issuing from a volcano. Nevertheless such porphyroids and fragmental rocks are included in teaching collections for hand-specimen and microscope study by students as paradigms of volcanic variants. Naturally such rocks when met in the field by those graduates are recorded as "volcanic". This would be quite appropriate so long as "volcanic" were kept as a *descriptive* term defining the characters of the rock and not its *genesis*. To make matters worse, these rocks may be seen in the field to be clearly intrusive, to brecciate and include xenoliths of the host shales. But the conclusion "intrusive, therefore igneous" excludes the case of quickstone intrusion. The viscosity of quickstone overlaps with that of igneous magma, hence intrusive and brecciation phenomena at contacts mimic igneous intrusive contacts in every detail. Fragments of fine pelite partings, broken up by the quickstone movement, may mimic devitrified shards, even to curved shapes.

Lunar lavas

Petrological uniformitarianism is not confined to porphyroids and quickstones. Basalts (descriptive term) brought back from the moon are automatically interpreted as lavas, derived by partial melting of the lunar "mantle". But is this necessarily so? There are other routes to molten rock in large volume which should be considered as possible alternatives. Whenever an asteroid impacted the moon, its kinetic energy relative to the moon had to be converted to heat in a matter of seconds. The whole mass of the asteroid and a comparable mass of the moon would have been volatilized "instantaneously". Much of this would be lost to space - some to be recovered by the earth as tectites or micrometeorites. But Moon herself would experience lava rain, which could coalesce to form lava flows. Such flows could differ from equivalent lavas derived by partial melting, in that they would be impoverished in volatiles - first to go, first to escape, last to condense. Such impoverishment is the most conspicuous character of some lunar basalts. An enigmatic character of selenomorphology is that some pre-maria craters (e.g. Archimedes) have maria surfaces at similar levels both outside and inside the unbroken crater rim. I cannot imagine how a lava flow could achieve this, but a lava rain could easily do so. Similarly, around the margins of maria, the maria surface is sometimes seen to extend into the valleys and closed basins of the peripheral highlands, sometimes at a higher level. Again a difficulty for lava flows, a simplicity for lava rains

I do not suggest that all lunar lavas originated from lava rains, and not from partial melting of the subcrustal rocks. In fact the latter process should be an inescapable consequence of the thermal shock front of the impact, followed by the unloading of tens of kilometres of overburden in the impact explosion, as D.H. Green and others have pointed out. But

I do insist that each case must be tested against genetic criteria.

Moreover, the same applies to terrestrial lavas, especially in the early Hadean stage of earth history, but even lavas of later vintage should not be immune from such scrutiny.

Record of glaciation *

The study of glacial sediments has grown out of the study of modern glaciers and of the terrestrial deposits left by the Pleistocene glaciation. Until recently comparatively little attention had been paid to glacial sedimentation at sea. From the point of view of the palaeogeographer, however, marine glacial sediments may be more important in that they are much more likely to survive in the geological record. Today the most obvious and widespread glacial sediments are the terrestrial deposits - moraines, tills, and outwash. However, on the scale of geological time all these deposits are likely to be completely destroyed. But in areas where the glacial sediments are included in a continuing cycle of subsidence and sedimentation, the glacial record will be preserved, perhaps for several geological periods. Such areas occur in the Arctic, Alaska, and off the Ross Shelf. The most permanent records will be those where the glacial sediments have been enclosed in a currently active geosyncline, where strong folds will, in due course, take the glacial formations deep into the crust, and where several cycles of peneplanation will be necessary to erase the last vestiges of the record.

The remains of the "Proterozoic" glaciations are largely of this type - in folded geosynclinal piles usually resting on other marine strata, with rarely any sign of a glaciated bedrock pavement. There were of course contemporaneous moraines, tills, and outwash, but they were the first to be wiped out. No doubt there were shelf deposits, but they, too, have largely disappeared.

The record of the late Palaeozoic glaciation is partially geosynclinal but there are still many shelf deposits, often unfolded, often associated with glaciated bedrock pavements, but usually leading up into a following succession of marine or lacustrine sediments that served as a protecting blanket during the erosion cycles of the Mesozoic and Tertiary. Terrestrial moraines and tills are relatively rare in the Palaeozoic, depending for their preservation, as they would, on penecontemporaneous burial through tectonic subsidence of the shelf on which they lay.

* From Carey and Ahmad (1961)

In spite of this difference in the state of the record in Pleistocene, Palaeozoic, and Precambrian glaciations, there is still a strong tendency to look to terrestrial glaciation for analogies for ancient glacial sediments, rather than to marine environments. Some geologists have gone so far as to discredit ancient glaciations unless they find preserved a striated pavement or other terrestrial attributes of the icesheet. This limitation is clearly observed in the nomenclature, where rock names are carried over from terrestrial environments. There is a dearth of names to designate marine glacial sediments in spite of the frequency of their occurrence, and cumbersome circumlocutions are often used (e.g. Miller, 1953, p.26).

NON-UNIFORMITARIANISM—RETROSPECT

In summary, the principle of uniformitarianism, necessary as it was to counter catastrophism, has been over-played. Field geologists must be ever keenly aware of the non-uniformities between geological periods, as well as ever alert for the wholly unexpected unique event. It is fatuous to think of sediment transport in the Ordovician in similar terms to that in the Jurassic.

Geological periods are not the same. They, of course, differ in the stage reached by organic evolution and by the evolution of the atmosphere, but they also differ in axial tilt, differentiation of the seasons, volume ratio of ocean water to ocean basin capacity, temperature gradient, heat flux, petrological condition of the mantle, the flux of meteorites and asteroids, and of extra-terrestrial rays reaching the surface, and heat loss to space from changing albedo. There is much to suggest that the sun, which governs surface temperature, may be a variable star. Perhaps the earth's mass has changed - perhaps even the redness of light!

When the astronauts set foot on the moon they found a very different terrain. But Earth had its quasi-lunar stage when asteroidal infall dominated earthly petrology with impact melts and clastics and impact *nuées ardentes*. The earth had its primitive crust and cratered surface with no stable atmosphere or hydrosphere.

When the mariner probes orbited Mars they found a surface very different from what we know. Crater-pocked primitive regions, but others with immense shield volcanoes, an asymmetric equatorial rift belt, a tilted axis, glacial caps, a carbon dioxide atmosphere and pan-global dust storms transforming the whole surface aspect in days. But Earth too had its quasi-Martian stage.

When the space probes prodded Venus, Earth's sister planet, they found things surprisingly different - again a deep low-oxygen atmosphere from pole to pole and no glacial cap. The Earth too may have had its quasi-Venusian stage.

The price of complacency of doctrine is the comfort of a mutual admiration society of orthodox fallacy.

TECTONIC PROFILES

Mohr's stress diagram and the stress ellipsoid are both *general* figures, capable of representing a general stress field. In tectonics (in the sense of Fig. 36) we deal with *particular* stress fields which have the following restricted properties:

(i) The earth's surface is a surface of zero stress.

(ii) The tectonic stress field has a unique direction, the vertical.

(iii) It has a unique plane, the horizontal.

(iv) At any point there is an effective upper limit for loads, the weight of the overburden.

(v) No stress can exceed the strength of the material at that point.

If we use a more general stress diagram without limiting it with the boundary conditions of our particular problem, we are not using all data available to us. The tectonic profile proposed here is based on the Mohr stress diagram, but is modified to fit the limitations of the tectonic problem.

Draw a horizontal line representing the surface, and a line OO' along which depths below the surface are measured (Fig. 47). Draw OL to represent, by the horizontal distance from OO', the load of the overlying rocks at all depths measured along OO'. Draw TT' and SS' to represent, by their horizontal distance from OO', the tensile strength and shear strength respectively of the rocks at the corresponding depths. For any particular vertical column of the earth's crust, these lines are all fixed. From them, four other lines immediately follow:

(i) The line AA, drawn at the depth where $T_A L_A = O_A S_A$, is the maximum depth at which tensile failure may result from simple shear.

(ii) The line BB, drawn where SS' croses OL, is the maximum depth at which the rocks may be put into tension as a result of simple shear.

(iii) The line CC, drawn at the depth where $T_C L_C$ is twice $O_C S_C$, is the maximum depth at which tensile failure may result from pure shear or from extension of the crust.

(iv) The line DD, drawn at the depth where $O_D S_D = S_D L_D$, is the maximum depth at which tensile stress may exist in the crust. The validity of these limits will emerge from the ensuing discussion.

Since we are concerned with flow as well as fracture, it is necessary to add the line $\eta\eta'$, representing (by distance from OO') the variation with depth of the logarithm of the viscosity of the rocks.

The tectonic profile thus constructed from any given point on the earth's surface is unique, and expresses the conditions under which any deformation or failure may occur there. The profile is used by constructing, on the horizontal line representing any depth, Mohr stress diagrams expressing the conditions at that depth. We may now study this tectonic profile in a variety of tectonic regimes.

Local hydrostatic stress

Stresses in the earth's crust tend always towards local hydrostatic equilibrium. Mine openings invariably show noticeable closure within tens of years. The half life for stress differences in an average coal mine appears to be the order of 10^3 years. The post-glacial uplift of Fennoscandia indicates substantial flow towards equilibrium in 10^3 years. The half life for stress differences in an average mine in crystalline rocks appears to be about 10^4 years. On tectonic scales (Fig. 36), this equilib-

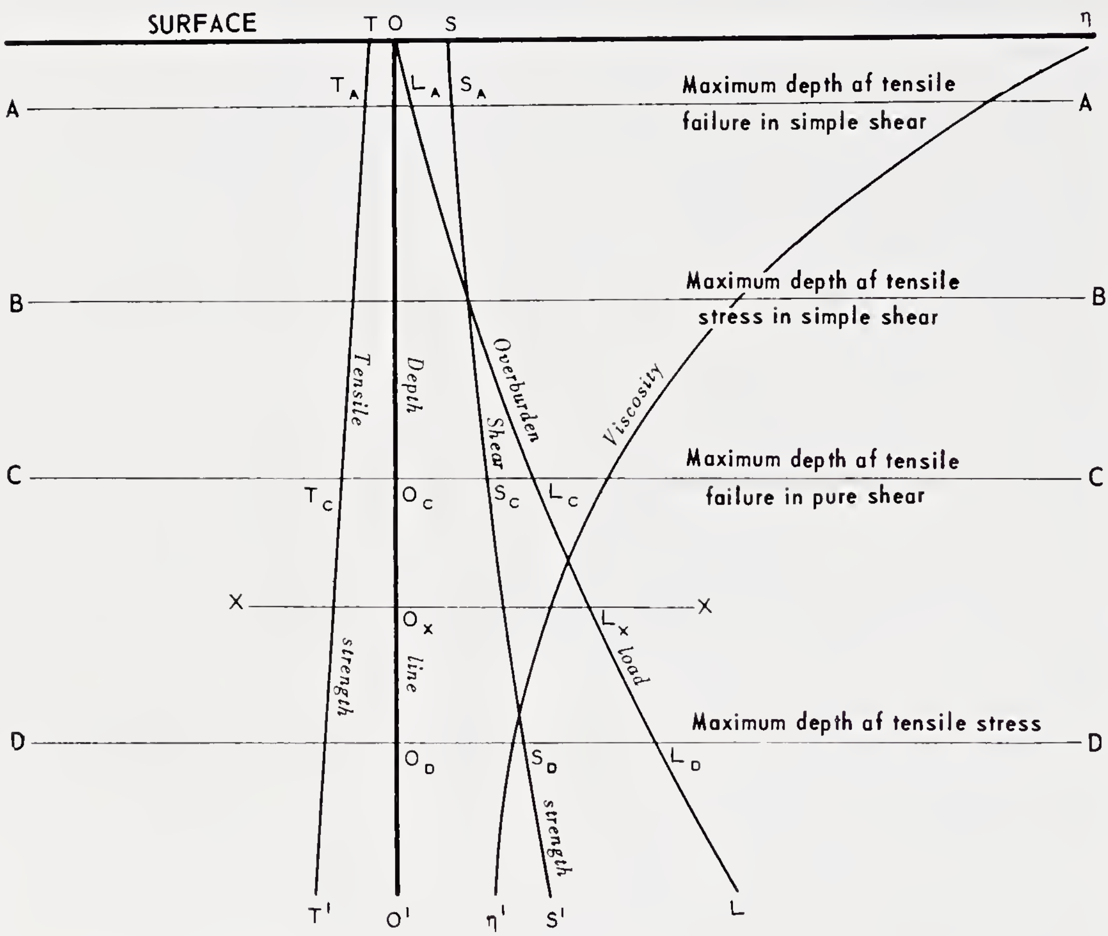


Fig. 47 Tectonic profile.

rium is virtually complete. In a state of local hydrostatic stress, all normal stresses are equal. Thus, in figure 47 hydrostatic stress at some random depth XX is represented by three equal principal stresses, each equal to the weight of the overburden and each represented by the line $O_X L_X$. Under these circumstances the Mohr's circles shrink to the point L_X, and shear stresses to zero.

Crustal extension

In figure 48, consider the stress relations at a random depth XX in an area which has been tectonically inactive for a prolonged period and which is about to suffer crustal stretching in one direction. Before the beginning of the new disturbance, the stresses at X approximate towards hydrostatic, and the Mohr circles reduce to the point L_X, all principal stresses being represented by the length $O_X L_X$.

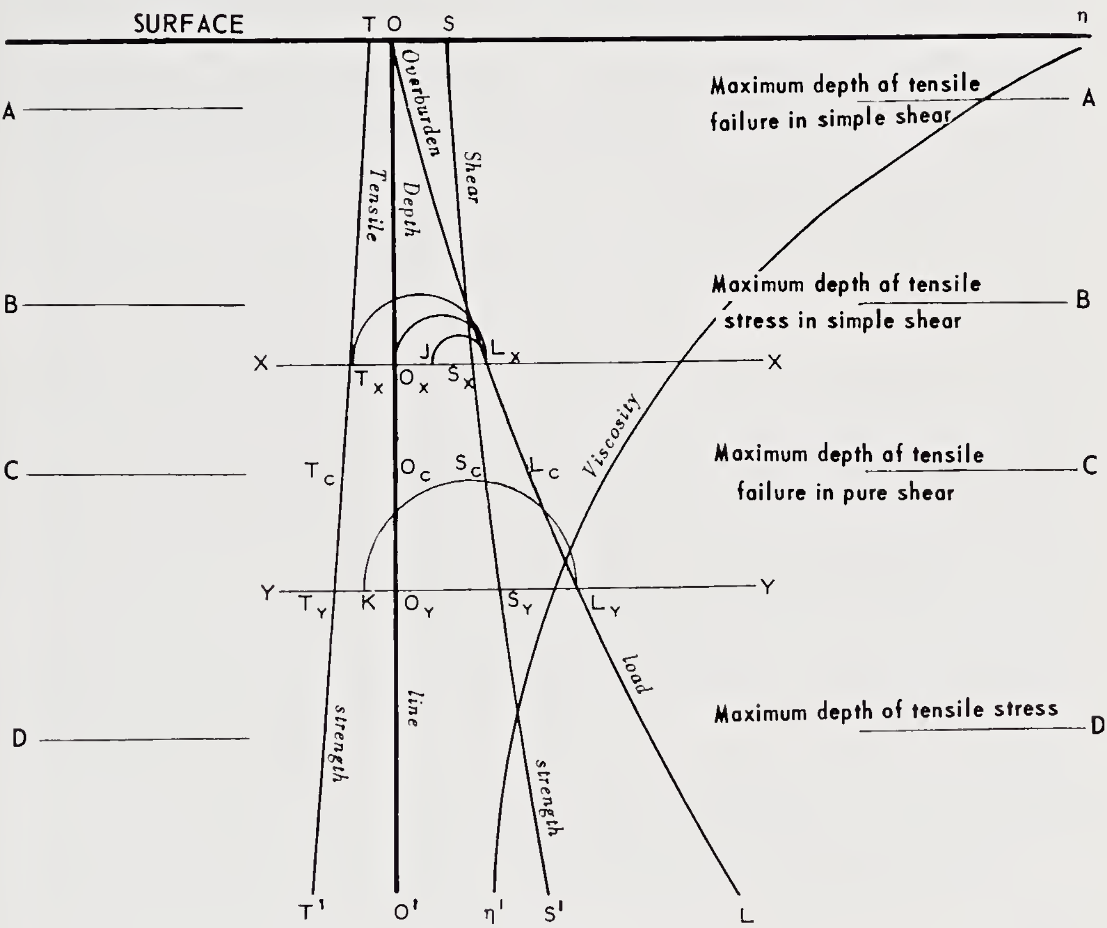


Fig. 48 Tectonic profile for simple extension.

When stretching commences, one of the stresses is reduced and is represented by O_xJ. The vertical principal stress remains at L_x, while the intermediate stress may be anywhere between L_x and J. The stress distribution is represented by the Mohr circle on JL_x as diameter. The maximum shear stress is $\frac{1}{2}JL_x$. This stress is below fracture level, but if sustained, flow deformation would ensue leading to subsidence at the surface as the load O_xL_x does work against the minimum stress O_xJ.

If the regional crustal stretching proceeds at a faster rate than can be corrected by this subsidence, the point J will move towards O_x, which it will reach when the stretching reduces the minimum principal stress to zero. At this stage, the stress field is represented by the Mohr's circle on O_xL_x and 2 other smaller circles within this circle. No fracture is possible at this stage, since the maximum shear stress ($\frac{1}{2}O_xL_x$) is still less than the shear strength O_xS_x.

As stretching continues, the minimum principal stress becomes a tension and eventually, failure in tension occurs when the minimum principal stress is O_xT_x. At this stage the maximum shear stress ($\frac{1}{2}T_xL_x$) is still less than the threshold for shear failure O_xS_x. We may conclude that at depth X, crustal extension may produce regional subsidence by warping if the extension is slow, or tensional faulting if the extension is sufficiently rapid, but failure in shear cannot occur.

Consider now the behaviour at depth YY, which is below CC, the limiting depth for tension failure. The development of the stress field will proceed as in the case of XX. When the minimum principal stress reaches O_Y, it becomes tensional. Before the minimum principal stress reaches T_Y (the condition for failure in tension), the threshold for shear failure is reached when the minimum stress is O_YK. At this point, the radius of the Mohr shear circle on KL_Y equals the shear strength O_YS_Y and failure occurs in shear. The boundary between failure in tension as at X and in shear as at Y is at CC, where T_CL_C is twice O_CS_C, so that the threshold of tension failure and shear failure are reached at the same point. Above CC failure is in tension, below CC failure is in shear. Similarly, below DD failure in shear occurs before the minimum principal stress reaches zero and becomes tensional, so that below this level tension stress never occurs. Tension failure cannot be induced below CC by applying the load impulsively, nor does secular application of the load induce tension failure, although the latter does favour flow deformation instead of or in addition to shear failure.

A consequence of this analysis is that rift valleys may break surface as normal faults, but these must pass into shears in depth (see Fig. 49).

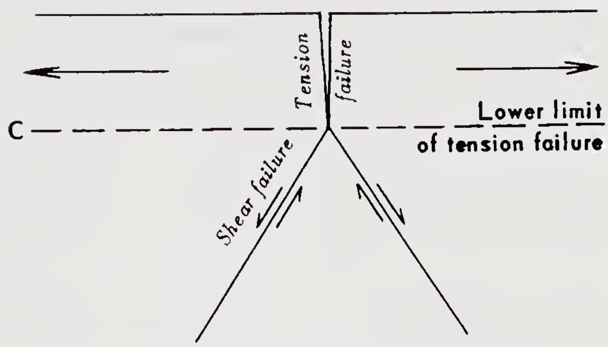


Fig. 49 Theoretical fracture pattern for simple extension with adjustment for internal friction.

Pure shear

The analysis of pure shear results in the same limiting conditions as simple crustal extension, which is indeed a particular case of shear. If pure shear is produced by increasing one or more of the principal stresses above the load of the overburden, then failure can only be in shear. If pure shear is attained by reducing one or two of the stresses then the case is identical with that just examined. One of the stresses at least must equal the weight of overburden, for otherwise this would be left without support.

These conclusions mean that tensional jointing only develops above the lower limit of tension failure CC.

Simple shear with vertical axis

Consider next simple shear with a vertical rotation axis. The strain rhomb in plan is shown in figure 50a. The couple EF, GH acts in the horizontal plane, and results in departure from pre-existing local hydrostatic stress by the addition of a compression EG (which now determines the direction of the maximum principal stress), and a tension FH (which now determines the direction of minimum principal stress). The weight of the overburden represented by OL (Fig. 50b) determines the intermediate principal stress at any depth.

Consider the stress field at a depth represented by XX. The introduction of the simple shear causes the stresses to depart from the hydrostatic state, where all three principal stresses plotted at L_x. The maximum principal stress now moves outwards to J_x, owing to the additional compression involved in the shear; the minimum principal stress moves to K_x, owing to the addition of a tension component of the shear, and the Mohr circles have diameters K_xJ_x , K_xL_x and J_xL_x. No rupture can occur at this stage, but flow deformation may occur, with the maximum principal stress O_xJ_x doing work against the minimum principal stress O_xK_x, thereby providing oblique folds oriented along the longer diagonal of the strain rhomb. If the shear stress is increased, a rupture threshold is reached where L_xK_x is equal to O_xS_x, so that failure occurs in shear. There is no possibility of tensile rupture or even tensile stress existing at this depth, since failure in shear necessarily occurs before any stress passes into tension.

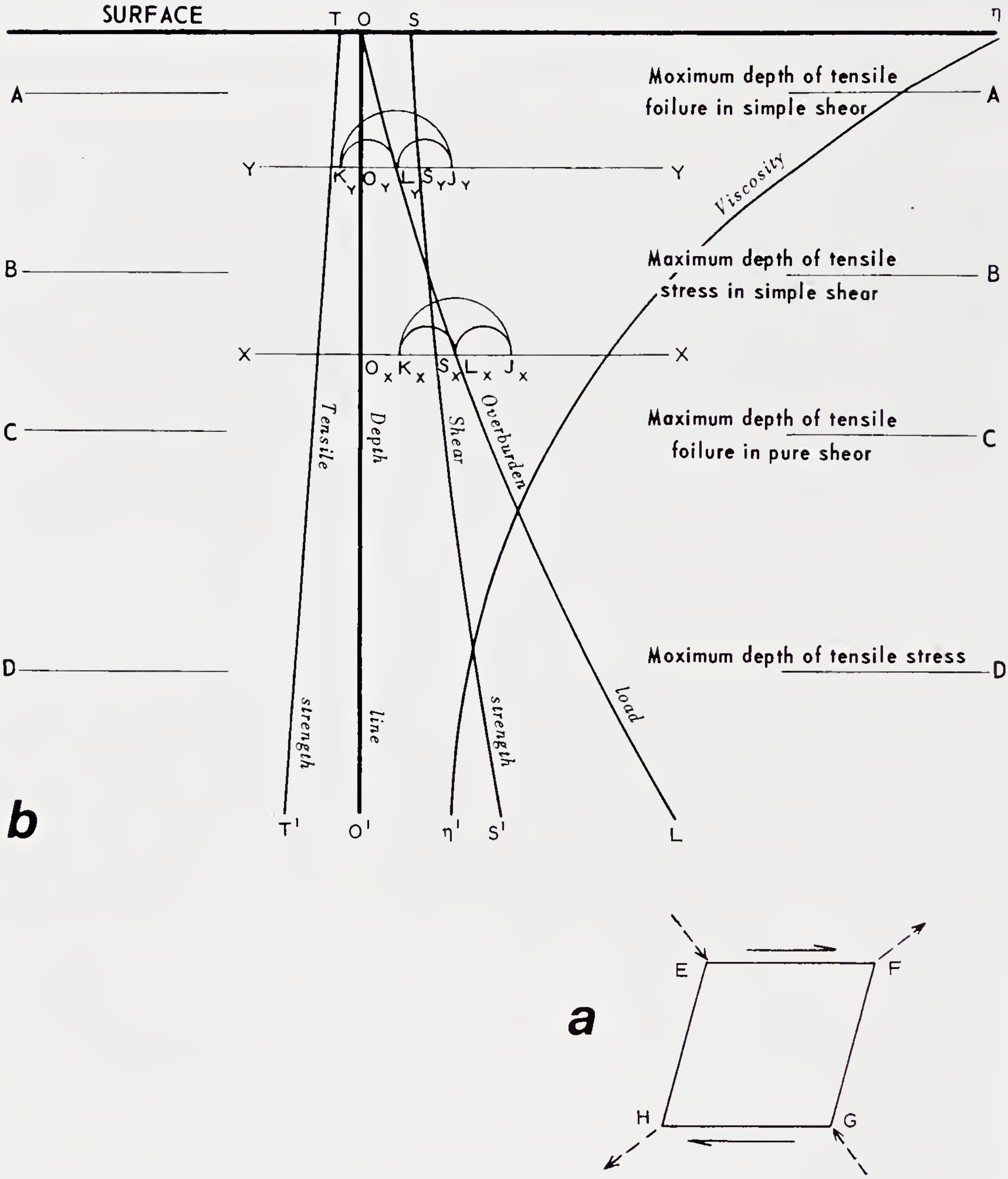


Fig. 50 Tectonic profile for simple shear.

Consider now stresses at a depth YY, shallower than the depth where the load of overburden equals the shear strength. Development is similar to that of XX, except that the minimum principal stress has to fall to zero and go into tension before shear rupture occurs when $K_Y L_Y = O_Y S_Y$. The boundary between these two types of behaviour is at depth BB, where the overburden load equals the shear strength and shear failure occurs just as the minimum principal stress falls to zero. This is the lowest limit at which tensile stress may be produced in the crust by simple shear.

Next consider simple shear for any depth above AA (the depth at which $T_A L_A = O_A S_A$ (Fig. 47). To save repeating the argument we will consider conditions right at the surface. The vertical load represented by OL is zero at the surface and this must remain the intermediate principal stress. Application of simple shear adds a compression and a numerically equal tension normal thereto, which become respectively the maximum and minimum principal stresses. Provided these stresses remain smaller than OT, no rupture can occur and the only deformation is folding along the longer diagonal of the strain rhomb. If the stress increases, the first threshold is reached when the minimum stress reaches OT, whereupon tension rupture occurs. Since the tensile strength of rocks is consistently less than the shear strength, shear rupture cannot occur at the surface. Although the compressive stress cannot itself cause rupture, it does produce flow at a rate determined by the stress differences and η. Hence, the resulting pattern at the surface is folding crossed by tension fractures.

The boundary between the fields of tension failure and shear failure is AA, the depth at which overburden load plus the tensile strength numerically equals the shear strength. Observe that the lower limit of tension failure is much shallower for simple shear than for extension.

Fluid pressures

Pore pressure of fluid reduces the effective stresses by the amount of the fluid pressure. This means that the effective pressure line OE (Fig. 51) replaces the overburden load line OL; failure in tension may extend to correspondingly greater depths. Under such circumstances tension openings fill immediately with pore fluid. The drop in fluid pressure has two consequences. Quartz, calcite, and other minerals, held in solution by the pressure, precipitate into such tension openings, producing mineralised gash veins, or mineralised boudin necks. The fluid pressure in the cusps of the tension openings causes these to burst upwards, bringing the deeper fluid pressures to shallow depths, where their contribution to the propagating failure is increased by shifting OE to the left at these depths. In this way tension openings normal to the regional tension may accelerate towards the surface, filled with high pressure water, and followed by magma, if the aqueous fluids were emanating from a magma source. In the upward ascent, one or both of two thresholds may be reached - the boiling pressure corresponding to the temperature of the water, and the depth at which the fluid pressure equals the lithostatic pressure. Either may result in a vertical dyke breaking upwards into either an explosive diatreme or a cone sheet. Such cone sheets dip steeply at first, but may flatten into shallowly dipping sheets, or even sills at depths where the density of the overlying sediments is less than that of the magma. A complete spectrum from dyke-tuffisite-diatreme to dyke-conesheet to dyke-conesheet-sill may occur according to the impulsiveness of the event, the water-to-magma ratio, and the density and layering of the sediment cover.

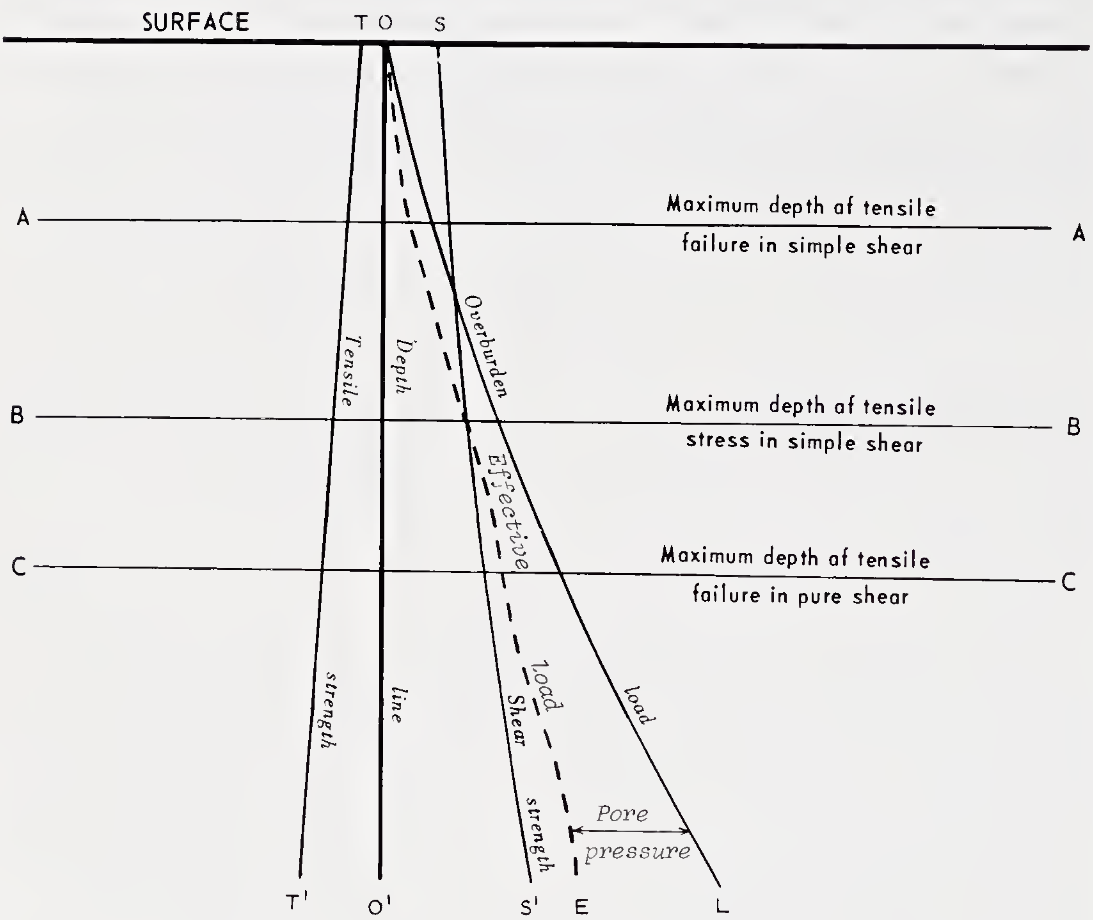


Fig. 51 Effect of pore pressure on tectonic profile.

Failure at the free surface

The surface itself necessarily possesses special properties. All fracture surfaces must be normal to the surface, since a free surface can only transmit normal hydrostatic pressures. This would still allow vertical normal faults, or vertical transcurrent faults, but the latter are ruled out by the fact that rocks are weaker in tension than in shear. Transcurrent movement may occur along a vertical shear surface at depths below AA, but it must change its strain pattern, in the uppermost surface layer, to a series of folds en echelon (Fig. 52a), a series of tension gashes en echelon (Fig. 52b), or a combination of both (Fig. 52c). This analysis assumes isotropic material. Pre-existing weaknesses in or near the direction of shear may cause the rupture threshold for shear in that direction to be reached before the rupture threshold in tension. The only theoretical limitations at the surface, then, are that the shear surface shall be normal to the surface and the shear movement shall have a vertical rotation axis. The relative development of folds and fractures depends on the rate of stress in relation to the deformation time-constants of the material, not the geometry of the stress field. Many examples of surface patterns of this kind occur in the field where young sediments bury active transcurrent faults.

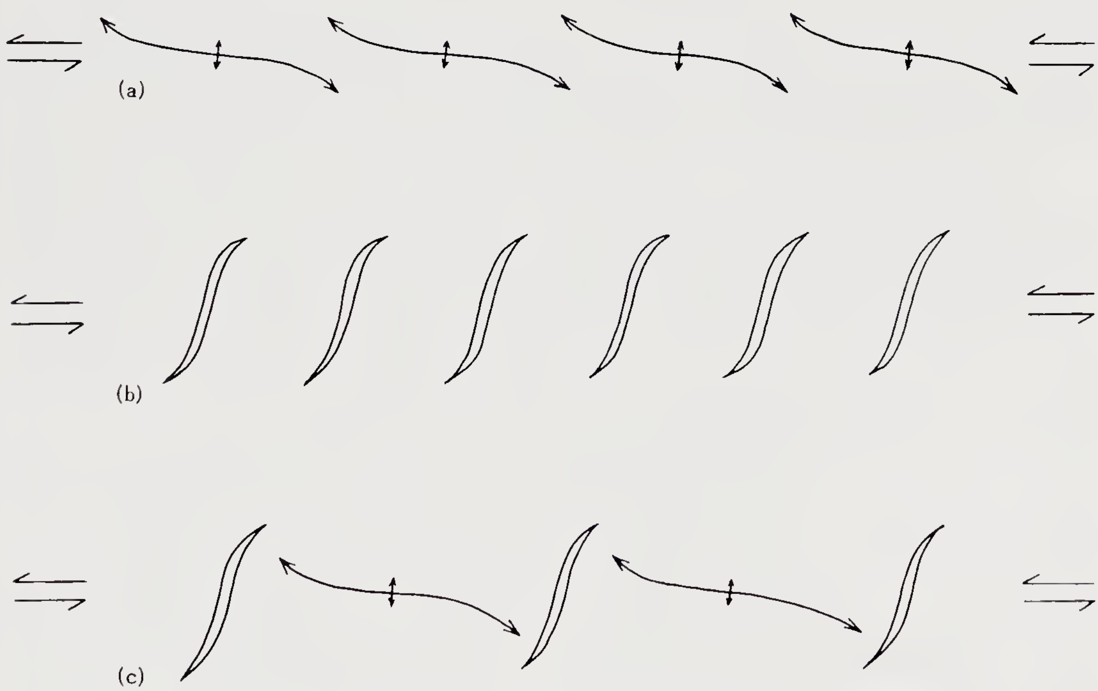


Fig. 52 Surface expression of underlying sinistral transcurrent movement
(a) folds en echelon, (b) tension fractures en echelon,
(c) combination of folds and fractures.

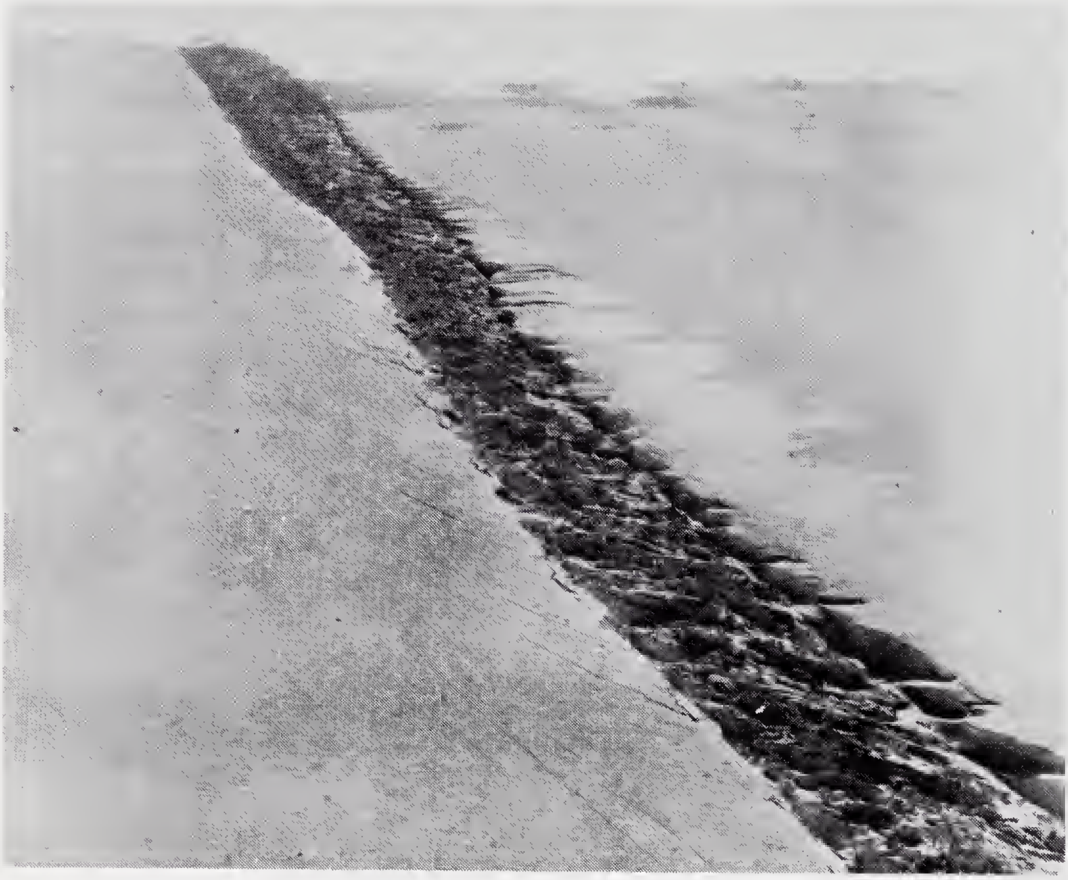
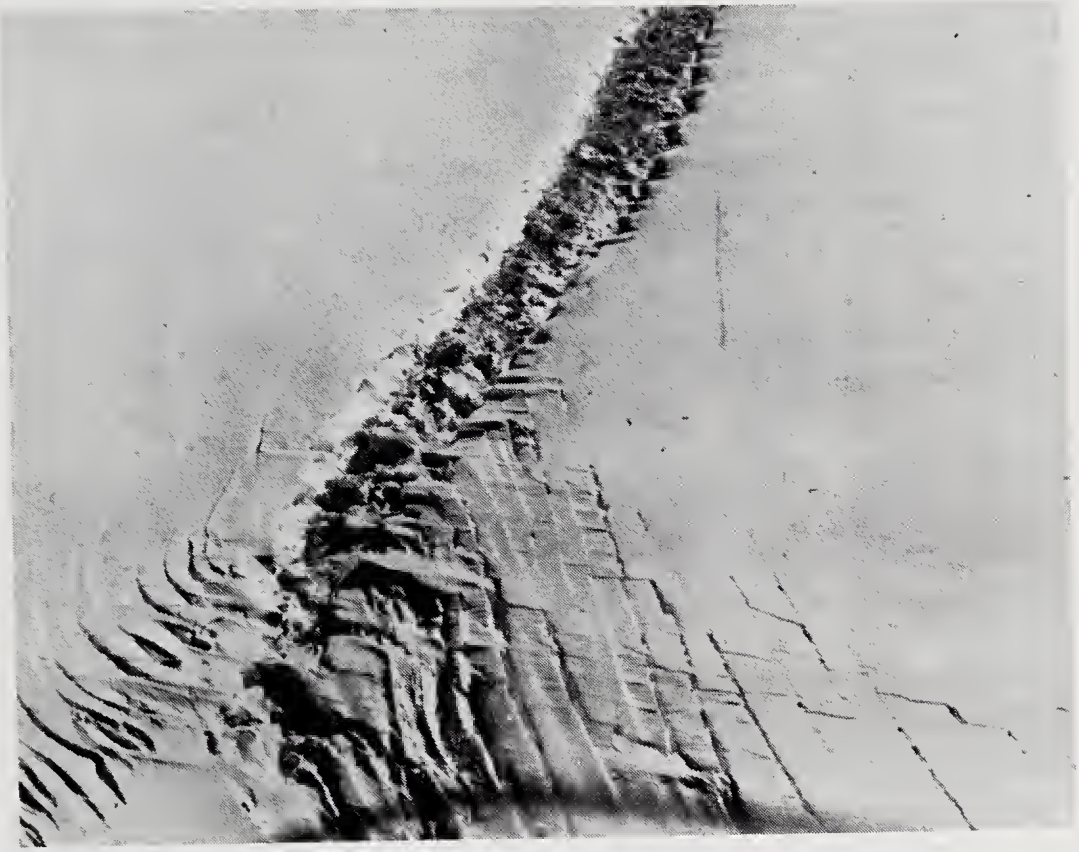


Fig. 53 Oblique aerial photographs of the Great Ice Chasm on the Filchner Shelf, Antarctica reported by Fuchs and Hilary, 1958, and analysed by Wilson 1960. This ice rift valley is reported to be about 100 km long, up to 50 km wide and 50 or more m deep. The photograph opposite shows the fraying fracture pattern at the headward (eastern) end.
(Reproduced with the permission of the Commonwealth Trans-Antarctic Expedition).

An excellent example of the surface expression of transcurrent movement was discovered on the Filchner Ice Shelf by aerial reconnaissance flights of the Commonwealth Trans-Antarctic Expedition 1955-8, and reported by Wilson (1960). The chasm (Fig. 53) is a zone of sinistral transcurrent movement with a vertical axis of rotation. The pattern of development is shown diagrammatically in figure 54. The initial surface effect of the simple shear is to produce en echelon tension fractures at 45° to the direction of shear (Fig. 54a). As the strain increases, these early-formed cracks rotate sinistrally, at the same time extending at each end. Since the extension is controlled by a constant stress system, the growth always tends to be at 45° to the shear, notwithstanding the steady rotation of the part already opened. Thus, the fracture acquires a sigmoidal shape (Fig. 54b), which becomes more pronounced as the strain increases (Fig. 54c). With increasing rotation of the gaping central zone, these fractures depart



further and further from the direction of tension fracture, so that new tension fractures develop between them, often linking successive echelon fractures (Fig. 54d). At this stage of strain, conjugate shear movement ensues on the rotated tension fracture, because substantial shear stress exists right to the surface, about a vertical axes of rotation, and is normally prevented from appearing because tension failure occurs at a lower threshold (Fig. 54c). The extreme lower left corner of the photograph of the Filchner Shelf chasm shows this stage of development. With further

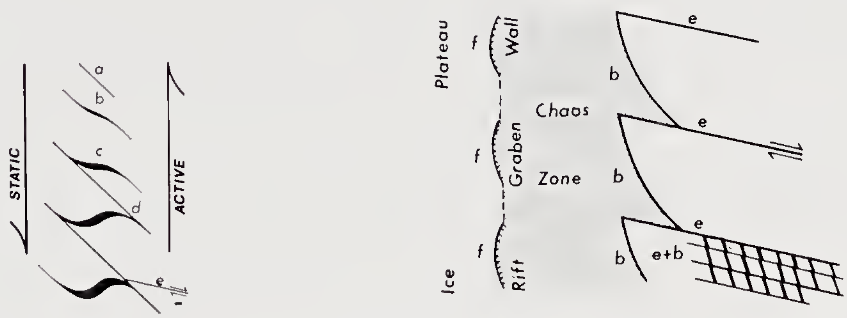


Fig. 54 Diagram showing development of fractures along the Filchner Shelf, Great Ice Chasm.

strain, the central structure breaks down completely, with rotation of the central blocks, and breaks through to the surface of the main walls of the primary transcurrent movement.

The marginal wall on the right shows dominant semi-sigmoidal tension gashes with subordinate shears of the dextral conjugate set. The cliff edge has a shark-tooth margin formed by the attenuation of these two fracture modes. The left wall of the rift valley, by contrast, forms a straight tension scarp, scalloped by occasional slip-circle slides (Fig. 54f) into the central graben. The floor of the graben becomes a chaos of rotated tension blocks and seracs.

Simple shear with horizontal axis

A great number of tectonic phenomena develop under conditions of simple shear with a horizontal axis, for example, most gravity tectonics, shear folding of axial zones, diapiric folding, most concentric folding, and most overthrusts and nappes.

Figure 50 and the discussion of simple shear with a vertical axis of rotation applies equally to simple shear with a horizontal axis of rotation. The intermediate stress, whatever its orientation, remains equal to the vertical load, and the critical boundaries are identical. In fact, figure 50 is applicable to all simple shear stress, irrespective of the plunge of the rotation axis. Depths below the surface are measured along OO', and the magnitude of all normal stresses, whether they are compressive or tensional, are plotted normal to this line from the point representing the depth; all shear stresses are plotted parallel to this line, and the Mohr's circles are drawn between the principal stresses. The same figure results, irrespective of what direction in space these stresses may have. Therefore to specify completely the stress field, it is necessary to write, on the stress line, the direction in space of the shear vector, and also the direction and plunge of the axis of shear rotation, looking along the axis in the direction such that the rotation is dextral (a right-hand screw).

Pure shear is completely specified if the directions and plunges of the maximum and minimum principal stresses are written on the stress line.

The foregoing discussion has involved only two-dimensional Mohr circles, the intermediate principal stress, σ_2, being assumed invariant. However variation of σ_2 does not materially affect the argument, as σ_1 and σ_3 are necessarily first to meet any threshold.

Explosions

At depths shallower than the maximum depth of tensile failure in simple shear (AA of Fig. 47), failure in shear will occur if the explosion is sufficiently energetic, but a less energetic shock may still cause failure in tension when the dilational phase of the wave follows the initial compression. This cannot occur at depths greater than AA. Further failure may occur below a free surface, where the reflected shockwave interferes with the advancing wave and produces double the amplitude of tension and compression. As this will be above AA, this failure will be tensional for the two reasons that the dilatational phase leads the reflected wave, and that, at these depths, the initial stress (overburden load) is nearer to the tensile than the shear load. (TL<LS on Fig. 47).

Implosions

Evison (1967) has suggested that deep-focus earthquakes may be caused by impulsive phase-change implosions, which would yield a dilatational shock front. Although such impulse is tensional, nevertheless failure at depths deeper than CC (of Fig. 47) must be in pure shear. Above CC, failure will be in tension. Tension failure will be parallel to the shockwave front.

Departure from isostatic equilibrium

In a region of negative isostatic anomaly, the theoretical spheroid GG lies above the local geoid (Fig. 55). The overburden load, represented by OL, is everywhere below the overburden load $O_G L'$ at similar depths below the spheroid surfaces in adjacent areas which are in isostatic equilibrium. At any depth XX, there is a pressure difference equal to $L_x L'_x$ on the level surface at that depth, between the area under consideration and adjacent areas. If the area of the isostatic anomaly is some hundreds of km. wide, the pressure gradient along any level is very small, as the total pressure difference $L_x L'_x$ must be divided by the distance measured in cm. between the two areas. But since the pressure difference extends down to depths in the mantle where viscosities are comparatively low, adjustment at these depths is comparatively rapid, with the result that the pressure difference in fact operates over a few tens of kilometres vertically, instead of some hundreds of kilometres horizontally. Hence, adjustment towards isostatic equilibrium is relatively rapid, the surface ST rising towards GG and the line OL moving towards $O_G L'$.

Global hydrostatic state

Isostatic equilibrium is not synonymous with hydrostatic equilibrium, as O'Keefe and others have tended to assume (O'Keefe 1960, p.979). In the former, the weights of all columns of equal area are equal; in the latter, the overburden loads on any geodesic are constant. A floating tabular iceberg is in isostatic equilibrium in that all columns of equal surface area have the same mass of water, or ice plus water, but hydrostatic equilibrium is not complete. The isopotential surfaces in the ice are at a higher level than the same isopotentials in the water (Fig. 56). If the ice behaves as a fluid, gravity can do work against its viscosity by spreading the ice into a thinner sheet, lowering its centre of gravity. Every buoyant ice sheet spreads and thins in this way at a very slow rate, with a form like figure 57 as a transitional state.

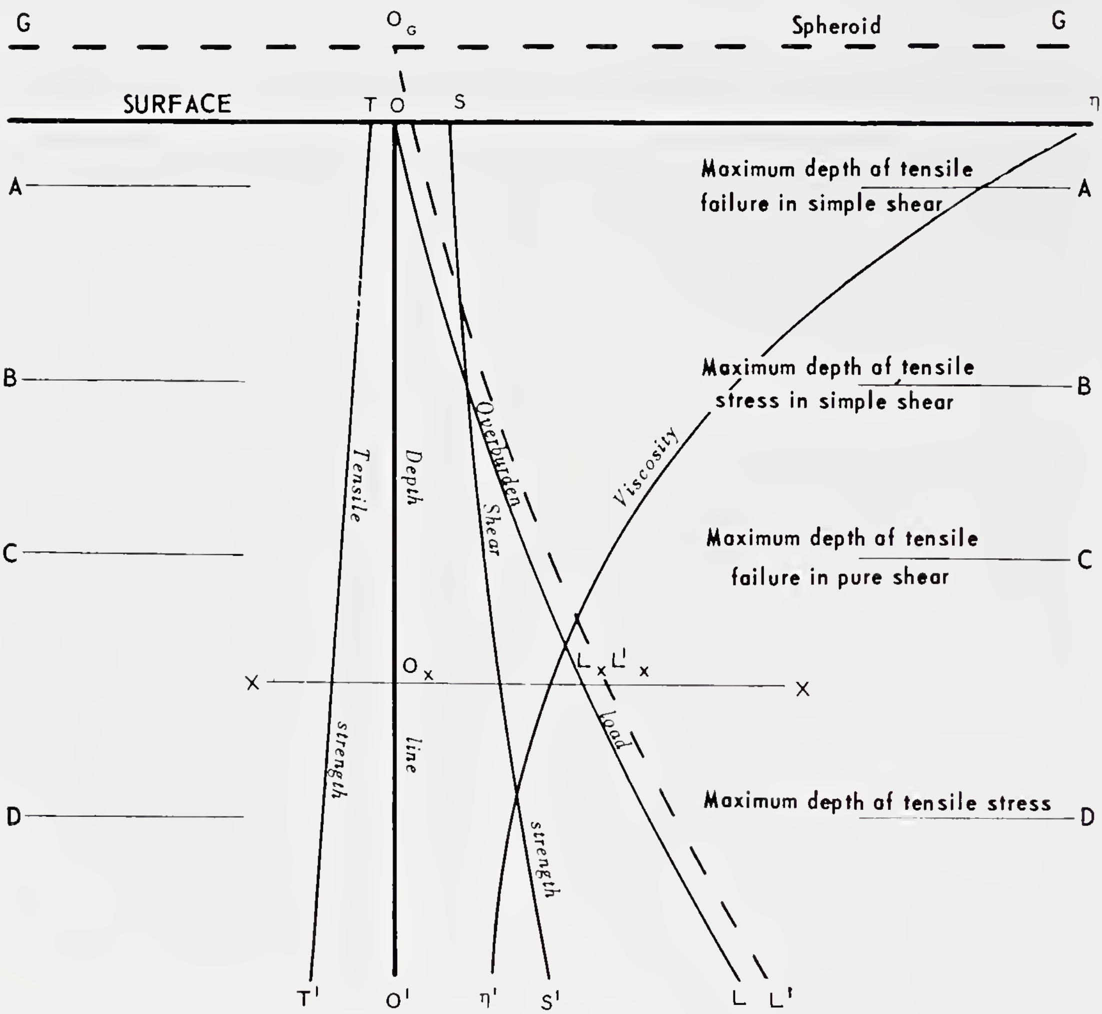


Fig. 55 Tectonic profile in region of negative isostatic anomaly.

In figure 58 the overburden load curve is drawn on the same profile for two areas in isostatic equilibrium, one continental and the other oceanic. At a depth equal to the base of the continent (C), and all greater depths, the two curves coincide, because the areas are in isostatic equilibrium, but, at all shallower depths below the spheroid, the weight of overburden at the same level is greater under the continents than at the same level under the oceans. At any such depth XX, there is a pressure difference equal to L_1L_2 tending to produce perfect hydrostatic equilibrium, which, if attained, would result in the sialic crust being uniformly spread above the simatic zone over the entire earth. The energy available to drive this process is gravitational potential energy, since, in this fully hydrostatic state, the centres of gravity of the whole crust would be lower than in the present continent-and-ocean distribution. However, the rate of progress towards this state is so slow that it has not been observed in times of hundreds of millions of years. The reason for this is apparent from comparison of figures 55 and 56. In departures from isostatic equilibrium, the pressure difference between complementary areas extends right down into the mantle, where viscosities are of the order of 10^{23} to 10^{21} poises. Hence, adjustment at this level is rapid geologically speaking so that the distances over which the pressure difference has to operate is reduced from the horizontal distance between anomalies (hundreds of km.) to the vertical distances of some tens of kms. In the hydrostatic anomalies, on the other hand, the pressure difference is confined to the crustal zone, and is greatest at the shallowest depths, where viscosities are not less

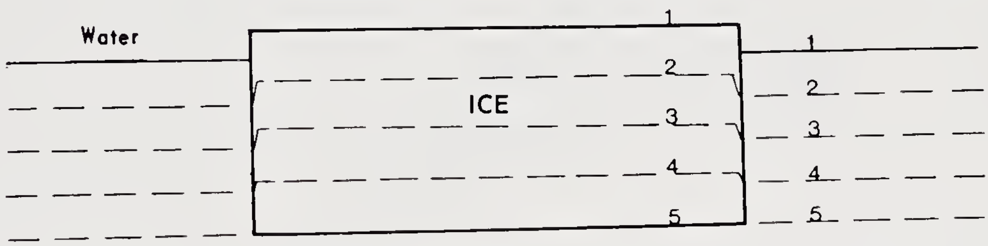


Fig. 56 Isostatic equilibrium in a floating ice sheet.

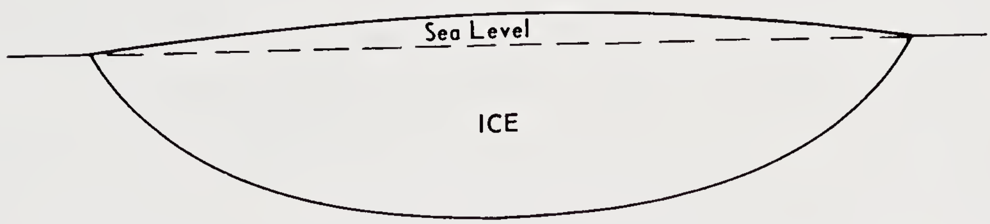


Fig. 57 Isostatic equilibrium trending towards hydrostatic equilibrium in an ice sheet.

than 10^{27} poises, and the pressure differences are distributed through horizontal distances of 10^3 km. Hence, we should expect that isostatic adjustment should take place in times some 10^6 to 10^8 faster than hydrostatic adjustment -- which is what we find empirically -- 10^4 years for substantial isostatic correction of Fennoscandia and no noticeable hydrostatic correction in 10^8 years or 10^9 years. Lester King (1953) has reconstructed the gross profile of the assembled Gondwana continents and shows that the great slab had spread towards the general form of figure 57 in some 10^9 years, but the separated continental blocks show little spreading in 10^8 years. These results are entirely consistent with the orders of time and size involved.

General summary of the tectonic profile

The general form of the tectonic profile for any area shows the sur-

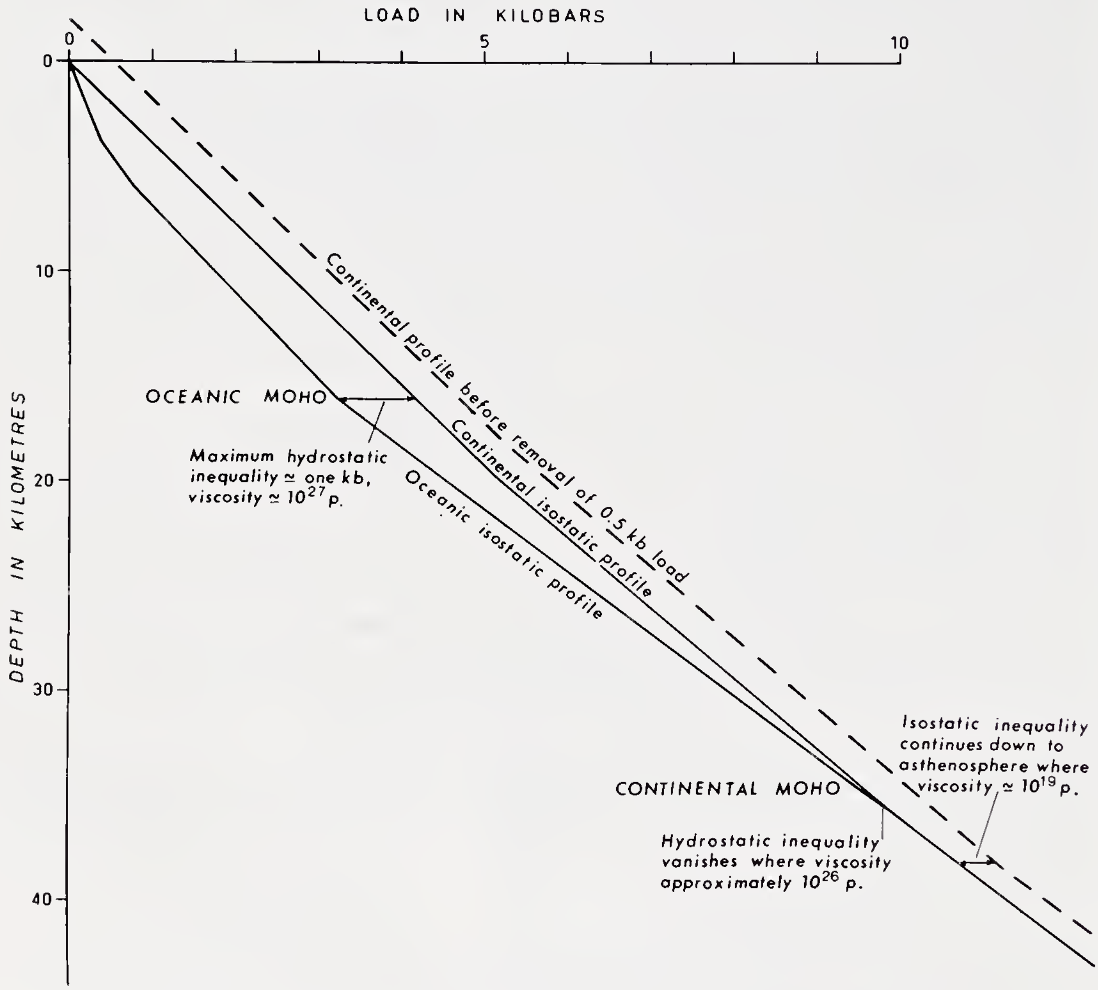


Fig. 58 Contrast of isostatic and hydrostatic inequalities.

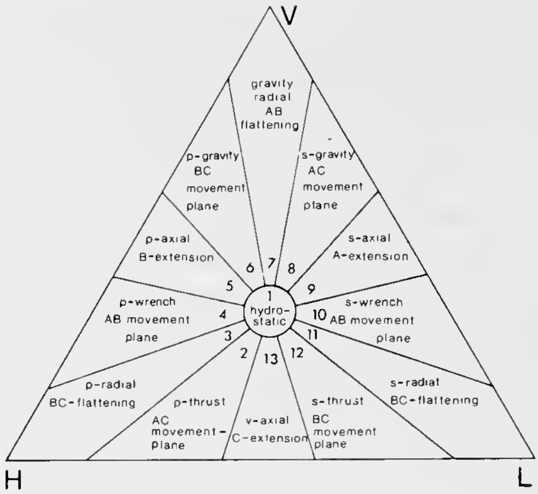
face, the depth line normal to the surface, and the variation with depth of overburden load, shear strength, tensile strength, and viscosity, all of which are predetermined by the particular rock column in that area. From these the lower limits of tensile rupture in simple shear, of tensile stress in simple shear, of tensile rupture in crustal extension, and of tensile stress in crustal extension all follow uniquely (Fig. 47).

Laboratory measurements show that with increasing depth, the stress difference which may be sustained by rocks increases uniformly to values such as 40,000 kg/cm^2 for quartz glass at overburden loads of 65,000 kg/cm^2. The depth of the lower limit of tension failure varies between wide limits for different materials. For moist clays it may be as low as a few metres; for many poorly consolidated sedimentary rocks it is less than a kilometre. For average sandstone, conglomerate, tuffs, and limestone with compression strengths from 200 to 750 kg/cm^2, the depth limit of tension failure varies from one to three km. These rocks grade continuously into slates, quartzites, marbles, schists, and gneisses, with strengths increasing from 810 to 2,000 kg/cm^2, and, as they do so, the limiting depth of tensile failure descends from 3 km to 8 km. The limiting depth for tensile failure in crystalline rocks is about 10 kms. Below 10 km. in depth, the overburden loads are consistently too high to allow any rocks to go into tension before failure.

The condition of stress at any specified depth may now be added, plotting the magnitude of the principal stresses and writing the directions of the shear axis and shear movement vectors in the case of simple shear, and the direction of the maximum and minimum principal stresses for pure shear. With this information, the deformation is completely specified. The elastic behaviour is known; the possibility of fracture and the kind of fracture and its direction are known, and the kind of flow deformation and its rate are known.

Harland and Bayly (1958) have given thirteen tectonic regimes according to the existing stress field and the pattern of ensuing movement (Fig. 59). All these regimes fall simply on the tectonic profile. All hydrostatic states plot with Mohr circles reduced to points. If this point falls to the right of the load line, transport upwards without deformation will ensue. If to the left of the load line subsidence follows. If the load line inflects towards a steeper slope downwards, a density inversion exists and diapiric overturn is likely, at a rate and with a pattern governed by the viscosity distribution. Wherever stress difference appears on the diagram flow deformation follows, at a rate determined by the viscosity, with con-

Fig. 59 "Stress triangle" of Harland and Bayly, depicting thirteen tectonic regimes.



traction and dilatation respectively in the directions of the maximum and minimum principal stresses. If the centre of the longest Mohr circle fall to the right or left or on the load line then deformation will be accompanied respectively by upward or downward movement, or by no translation.

An essential condition imposed by the tectonic profile is that the analysis of stress and associated deformation commences from the stressed state of the rocks as they exist under the enormous weight of their overburden even at shallow depth, whereas many discussions of deformation and rock failure commence by adding a load to a state of zero stress. As a result, many geologists intuitively think of folding and states of pure shear as implying the introduction of a compressive force. In fact, at all but comparatively shallow depths, the normal result of *extending* a crustal prism, is to produce stress difference, with all three principal stresses *inwards* in pure shear. Resulting failure by fold or fracture mimics completely triaxial compression.

PALAEOMAGNETISM

The beginnings

Late in the sixteenth century Gilbert entertained Queen Elizabeth of England with a globe containing an internal dipole, which modelled faithfully the dip of a magnetic needle as it was moved about the earth. In 1849 Delesse discovered that some recent lavas were magnetised in the direction of the earth's local field, and Melloni confirmed in 1853 that Vesuvius lavas were magnetised parallel to the ambient field. At the turn of the century Folgerhaiter extended this work to bricks and pottery, and showed that ceramics became magnetised parallel to the local field during firing, that this magnetisation remained stable even where pots had remained buried randomly for thousands of years, and that the history of the secular variation of inclination could be re-established from the study of ancient pots.

During the epi-Wegener ferment of the late twenties, which spawned so many new ideas (including earth expansion), Mercanton (1926) recognised that magnetic studies on the dispersed pieces of Pangaea could give objective tests of the Wegener concepts. This idea was supported by Gutenberg (1940, 1951), but it was not until the late forties and early fifties that Blackett and Runcorn and their students, stimulated by Graham's report (1949) from the Washington Department of Terrestrial Magnetism that at least some sedimentary rocks also retained fossil magnetisation directions through long geological times, seriously investigated this proposition.

At this time, a vertical, 300m bore into the Jurassic dolerite on the eastern shore of Great Lake had just been completed by the Tasmanian Hydro-Electric Commission, so I air-freighted to Blackett cores 30m apart, and predicted that they should prove to have been relatively near the pole of the time. In due course Blackett cabled that except for the surface sample, the magnetisation of each core was close to its axis, confirming the tectonic prediction. This led, through the co-operation of Professors Jaeger and Runcorn, to the invitation to E. Irving, then a graduate student at Cambridge, to take up a research fellowship at the Australian National University, whence he commenced a systematic study of the remanent magnetisation of the Tasmanian Jurassic dolerite. This resulted in Irving's paper (1956) which established a Tasmanian Jurassic palaeolatitude of 80°S.

Meanwhile, one of my staff (M.R. Banks), on study leave in the United States, was requested to collect oriented samples from the Rhaetic Palisades sill on the west bank of the Hudson River, and I asked Dr. Reinhard Maack of the Curitiba Museum to collect from the basalts of the Parana Basin of Brazil. As Gough and Hales had been investigating the palaeomagnetism of the Karoo dolerites at the Bernard Price Institute, Johannesburg, since 1950, the palaeomagnetic confirmation of the reality of Pangaea emerged (Carey, 1958a, p. 280-1).

While this was going on I had interested Blackett in the possibility of a palaeomagnetic test of the forty-degree sinistral rotation of Spain, which I had deduced tectonically, and Argand had proposed thirty years earlier on other grounds. As a result, a team collected oriented samples from the Bunter Sandstone, but because of unstable components, the results were inconclusive (Clegg *et al.* 1957). It was some years before the development of magnetic cleaning techniques enabled positive confirmation of this rotation to be established.

Ocean floor magnetic lineations

Before the discovery of the magnetic lineations of the ocean floor, I had already deduced on tectonic grounds that the growing mid-oceanic rift zones along the Red Sea and Atlantic Ocean should produce paired slices some kilometres wide, becoming progressively older away from the active central rift (Carey 1958, fig. 3f, pp. 181-191). It was also clear that the active cambium-like growth did *not* necessarily require magma to fill the dilating rifts. Ordinary tectonic process could achieve the growth of the whole Atlantic without any magma whatever. "The mechanism proposed ... fills the gap neither with sediments nor igneous intrusions, although both may be involved to a minor or major degree. The principal filling is crystalline sima .... There is no necessary concomitant vulcanism. The only requirement is crustal stretching. Nevertheless the isotherms in the rift zones are higher than at similar levels elsewhere so that the probability of vulcanism is rather higher in the rift zones" (Carey *loc. cit.* p. 189-190).

The discovery of the magnetic lineations in the early sixties immediately correlated with my paired slices, but in my tectonic lectures then the thermoremanent magnetisation was attributed to *both* magnetisation of the ascending crystalline slices of mantle rock as the temperature dropped through the Curie point for the relevant minerals, and magnetisation of basaltic flows and dyke swarms. Vine and Mathews (1963) attributed the magnetisation to the cooling magmatic rocks. Whereas I immediately agreed that their paired-magnetic-strip interpretation was, in general, correct, nevertheless I still believe that both my original processes are valid, and contributing. The two magnetisations in the one strip are not necessarily synchronous, and variations in the combination may account for individualities of successive strips.

The subsequent development of the interpretation of the palaeomagnetic lineations, their correlation, and the analysis and dating of the implied movements, has been most exciting and impressive (e.g. McKenzie and Sclater, 1971). I am convinced that most of this work is valid. This part of the so-called "new global tectonics" essentially adds details and precision to all the movements I have for so long argued. This fine conception thrives perfectly on an expanding earth without the hypothetical subduction in the trenches.

Nevertheless there are some reservations:

(1) All the geometrical work is being done assuming the validity of Euler's theorem concerning displacement on spherical surfaces. This theorem is not necessarily valid where the radius of the sphere changes. Misinterpretations are certain to occur where this theorem is held as a constraint.

(2) Whereas I have only praise for the skill of the filtering treatment and correlation of the magnetic anomalies, the analysts have sucked their information from the intermittent and noisy data to the extreme limit of extraction, and it is too much to hope that some misinterpretation and miscorrelation has not occurred. The final shapes which have to be matched do not carry many information "bits", and as with a stalagmite profile, one caverneer recognises Napoleon and another sees Queen Victoria and a third Pickwick; many of the confident correlations may change. However this will not alter the validity of the overall picture, although modifications of detail should be expected.

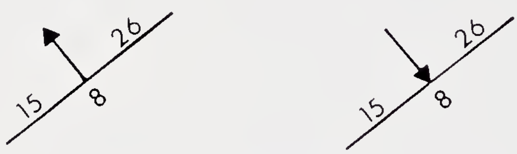
(3) All the spate of analysis so far has been done with the constraints of "plate" theory, particularly in projection between traverses. All such constraints should be removed so that the palaeomagnetic data comes through without bias. Otherwise, false diversions are inevitable and significant facts will be missed.

Palaeomagnetic notations

Palaeomagneticians, at the outset, adopted the practice of expressing palaeomagnetic data in the form of implied poles, and this has become ossified as a standard procedure, - regrettable, because it implicitly introduces the unwarranted assumption that the radius of the earth is constant. Palaeomagneticians refuse to be concerned about this because they *know* ex cathedra that the radius has not varied significantly. The correct way to express the data is to draw a short line through the sampling site normal to the magnetic meridian, with two numbers beside the tab pointing to the nearer pole, giving the spread of the implied palaeolatitude; a number on the bar gives the radius of the 95° confidence cone; the arrowhead indicates the direction along the meridian to the north pole.

In figure 60, the inclination I was 36° with a confidence cone of radius 5°. The left-hand symbol means that a northern hemisphere parallel between 15° and 18° passed through that point with an uncertainty of 8° in the bearing indicated, and that the north pole lay in the direction of the arrow. The right-hand symbol indicates a similar result in the southern hemisphere (with the south pole back long the shaft of the arrow). The arrow directed into the bar means that the north pole lay in the other hemisphere. Neither the rocks nor the symbols declare whether the field was reversely magnetized at the relevant time. This is a matter of interpretation, not objective fact. This notation incorporates *all* information contained in the data, and does not introduce any assumptions, other than the initial assumption of a dipole field. From a set of such symbols on a single block, probable latitude isoclines can be drawn within the area covered by the data, and the palaeomagnetic match or mismatch between juxtaposed blocks becomes apparent. Palaeolatitude lines can be extrapolated beyond the area covered by the data, with any desired explicit assumption concerning earth radius.

Fig. 60
Palaeomeridian notation.



Van Hilten (1962) was the first to emphasise the disadvantage of expressing palaeomagnetic data as palaeopole positions, and instead plotted his results as isoclines. He has been followed by some, e.g. de Boer, but the palaeopole procedure has become entrenched in the literature; and because almost everyone in this field does so, it has become respectable and normal. Nevertheless it is patently invalid, except on the implicit assumption of a constant radius earth, and palaeomagneticians should have the courage to abandon the fashionable bandwaggon and put their house in order.

In a similar vein (although he was concerned rather with discrimination between polar wandering and continental drift), King (1964) has written: "We should therefore insist that palaeomagnetists and others concerned with relative movements of poles and continents should no longer follow the lazy way of "plotting the position of the pole", which is wrong, but should adopt the more arduous procedure of resiting the locality with respect to a fixed pole and appropriate net of meridians and parallels."

ABORTIVE ATTEMPTS TO MEASURE PALAEORADIUS

Common meridian method

Egyed (1960) proposed that the ancient radius of the earth could be determined from two contemporary palaeomagnetic sites on the same palaeo-meridian, as the surface distance and the geocentric angle between them were both known.

Cox and Doell (1961) applied this test to Eurasia and reported that Permian palaeomagnetic latitudes of European and Siberian rocks showed no significant change in the earth's radius. Uncertainty in the data would not exclude (nor support) expansion at the rate (~ 0.6mm/year) proposed by Egyed, but made unlikely the radius increase of 1100 km proposed by Carey (1958a). This analysis involved explicitly the assumption that the Eurasian block had remained a stable unit unchanged since the Permian. However, my model (*loc. cit.* p. 203-4 and fig. 9) explicitly involved an extension of some 10° between the European and Siberian sampling areas since the Permian. Hence if Cox and Doell had applied their test to the model published by me in the reference they cited, the Permian radius would have been about 4500 km.

The formula for the palaeoradius R used by Cox and Doell was:

$$R = \frac{d}{\mathrm{Cot}^{-1}(\frac{1}{2}\tan I_1) - \mathrm{Cot}^{-1}(\frac{1}{2}\tan I_2)}$$

(d being the distance between the two sample points, i.e. ES in figure 61). They point out that this is geometrically the same as that used by Eratosthenes to measure the radius of the earth more than two millennia ago (see p. 223). In figure 61 palaeomagnetic measurements on Permian rocks at E and S showed that they were approximately on the same palaeomeridian, and differed in palaeolatitude by say 20°. Then the distance ES subtended 20° at the centre of the Permian earth which had a circumference 18 times ES. But this argument fails if the OB region is a disjunctive rhombochasm. Then it is the distance ES - OB which subtends 20° at the centre of the Permian earth, which was hence smaller than the present earth in the proportion (ES-OB)/ES.

Ward (1963) repeated the same error. Van Hilten (1963) recognized this possibility (which he called "orange-peel effect") though he did not use the specific separations shown in Carey (*loc. cit.*). Nevertheless he concluded "that the palaeomagnetic evidence . . . seems to indicate a noteworthy increase in the Earth's radius since the Carboniferous, the rate of which agrees roughly with the hypotheses of Carey and Heezen". Van Hilten (1964, p. 41 and 1965) carried this further and from palaeomagnetic data alone deduced that differential movements had occurred between the blocks of the Eurasian continent.

Creer (1967b, p. 386) explicitly, and van Andel and Hospers (1968) implicitly, notwithstanding their disclaimer to the contrary, also depended on the integrity of the Eurasian block; this cannot be accepted as a test, not only because of the Ob sphenochasm, but also because of the tectonically disturbed zone through the Black Sea, Caspian Sea, Aral Sea, Lake Balkhash, Lake Baikal, to the Sea of Okhotsk. Nor do any of the plate tectonicists regard Eurasia as a coherent block. They recognise severally the North European "plate", South European "plate", Siberian platform "plate", Jano-Kolymian "plate", Kazakhstan "plate", North Chinese "plate", South Chinese "plate", and Indian "plate", with geosutures and differential movements between them, as well as many small "micro-continents" with their own differential translations and rotations. Likewise Kropotkin (1971) pointed out that all the continents except Eurasia have only one nucleus or craton.

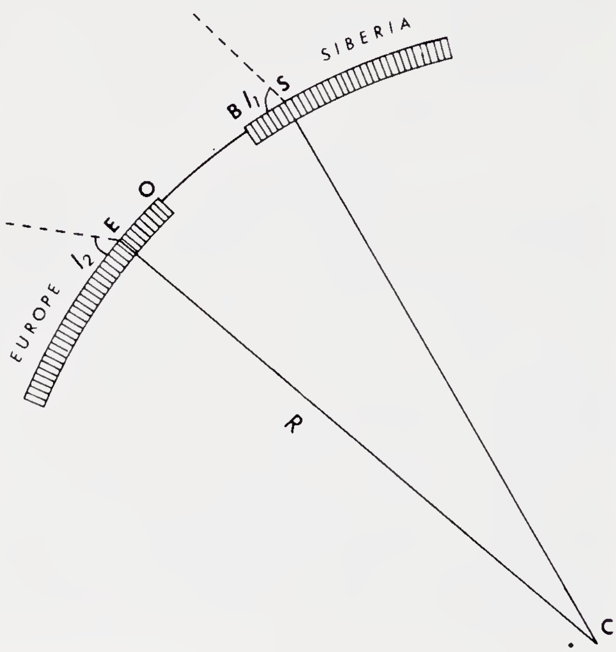


Fig. 61

Common meridian method for palaeoradius.

Eurasia includes four main Precambrian platforms, which have moved independently, as well as several smaller Precambrian nuclei. The Eurasian complex has dispersed substantially since the Permian.

Ward (1966) asserted that van Hilten's method of estimation of palaeoradius was erroneous, and hence that his conclusion that the earth has expanded was invalid. To quote Ward *verbatim:* "The model which both van Hilten and I explicitly use has the following two main properties: first, that as the Earth's radius changes, the size of the continental block remains constant, and thus the geocentric angles between the rock units will change, and second that, as the usual consequence of assuming a dipole field, *the geocentric angle between each rock unit and the palaeomagnetic pole calculated from it remains constant as the Earth's radius changes* [italics S.W.C]. Now in equation (1) of van Hilten's paper we see that the geocentric angle between a rock unit and the corresponding pole is considered to change proportionately to the ancient radius - contrary to the requirements of the model." In this Ward misunderstood van Hilten's notation. To quote van Hilten:

$$" \quad p = \frac{Ra}{R} \cot^{-1} (\tfrac{1}{2} \tan I) \qquad (1)$$

where p is the geocentric angle between sampling site and ancient pole position *adapted to the present-day radius of the Earth*" [italics S.W.C.]. In other words p is not the *ancient* geocentric angle but the geocentric angle *on the present-day earth*, from the sampling site to the palaeopole. This "angle between the rock-unit and the corresponding pole" must, of course, "change proportionately to the ancient radius". The italicised part of Ward's statement (above) is incorrect (Fig. 62). The inclination (I) of the remanent magnetisation to the horizontal remains frozen; and so does the position of the palaeopole on the continent. The linear distance from the sampling site to the palaeopole remains constant, but the geocentric angle (p) subtended by this distance varies inversely as the earth radius in accordance with van Hilten's equation (1). The criticism of van Hilten by van Andel and Hospers (1969, p. 115) is also caused either by following Ward uncritically, or by independently falling into the same error as Ward did.

Ward goes on to state that: "for a method such as the ones under discussion to be tenable, it should be possible to work entirely with the opposite poles - that is, those in the region of what nowadays is the S. Atlantic Ocean - and obtain the same results. This criterion holds good for my method, but not for van Hilten's; in fact, if he had used these opposite poles instead, none of his intersections would have occurred, and his result would have been that the Earth has *contracted* rather than expanded during the period under discussion." This criticism is again Quixotic. For the model which Ward proves absurd is not van Hilten's model, but Ward's incorrect version of it. In figure 62 the ancient complementary pole A becomes B (measured along the meridian from the sampling site as required by van Hilten) but becomes C according to Ward's version of van Hilten. Van Hilten's complementary pole does yield consistent results with those from the other hemisphere. Ward (1963, p. 225) misquoted my estimate (1958) of the expansion since the Palaeozoic as 45% in the radius (instead of 45% in the surface area) which had the effect of pushing my estimate well outside the uncertainties of his estimated expansion. However he later (1966, p. 466) acknowledged this misquote and reduced the 45% to 25%.

In discussing such estimates I should also emphasise that the data on which I based my 1956 estimate would not allow precision either of the geological period of the estimate or of the amount of expansion. The ratio of the total area of "new" ocean to "old" crust indicated an approximate doubling of the surface area since about the end of the Palaeozoic. Even with increased data available now (twenty years later) the boundaries to be assigned to any pre-Triassic oceanic crust are vague. Palaeomagnetic dating and correlation of ocean floor before the Tertiary is very uncertain, and the first basalt met in JOIDES drilling indicates a minimum age, but does not preclude older oceanic sediments below. There is indeed much to suggest

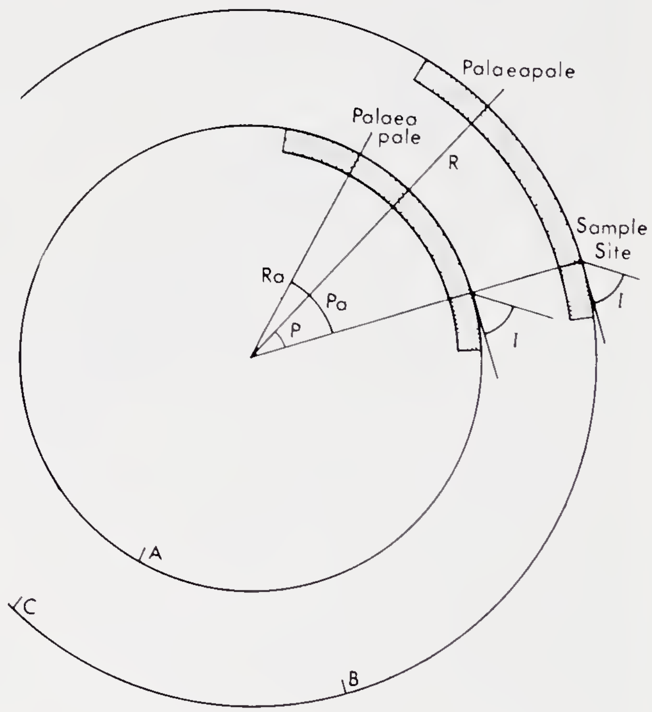


Fig. 62

Variation of geocentric angle p between sample site and palaeopole with change of earth radius R. p is proportional to $\frac{Ra}{R}$.

that the opening of the Arctic Basin and North Atlantic commenced, in part, as early as the Middle Palaeozoic.

Van Andel and Hospers (1969) next combine the palaeoradii reported by several authors, to derive statistically probable ancient earth radii. When the invalid Eurasian data are eliminated from these sources, nothing relevant or significant remains.

Finally, van Andel and Hospers (1970, p. 314) agree that validity of the Ob rhombochasm would destroy their calculation of former earth radii resting on palaeolatitude comparison between Europe and western Siberia. Hence in order to rebut the rhombochasm they set up their model of a rhombochasm, and proceed to prove it untenable - a Quixote procedure. Their model assumes an empty rhombochasm floor to be 5 km below sea-level, and the sediment-filled equivalent in isostatic equilibrium to be 16 km deep. This was not, and is not my model, nor could it be a valid one. Only a few older ocean basins, which have had time to lose their excess heat, are 5 km deep. In a young rhombochasm mantle material has risen 20-30 km and elevated the isotherms by almost this amount. The excess heat has an attenuation time of tens of millions of years, and meanwhile the equilibrium phase-boundaries within the mantle all lie at greater depths, with the result that the surface in isostatic equilibrium stands two to four km higher, and the Moho is depressed by ten to twenty km. The Moho is simply a seismic velocity discontinuity, which theoretically could be either a chemical discontinuity or a phase boundary, in this case the latter. This is the situation adjacent to the mid-Atlantic ridge, the Shatsky Rise, the Eauripik-New Guinea Rise, the Ontong Java Rise, indeed everywhere that the isotherms are sufficiently elevated in mantle rocks. That the isotherms are indeed appreciably elevated under the Ob rhombochasm is, of course, well known (e.g. Tamrazyan, 1964). Ryabov (1968) reports: "Some years ago, Soviet geologists searching for underground oil and gas in Western Siberia, were disappointed when the bores that they had sunk at great expense yielded only hot water. Today, however, it is recognised that the enormous artesian basin they had discovered has very valuable possibilities. The area of the basin is larger than the Mediterranean - stretching to the Urals in the west, to the Sayany Mountains in the south-east, to the Kazakhstan steppes in the south and almost to the Arctic Circle in the north. It is known as the 'West Siberian Plain'. A layer of sedimentary rock, two kilometres thick, separates the hot subterranean sea from the surface. The sea itself saturates porous rocks and sandstone and fills a huge depression in the earth, never less than six-and-a-half km in depth. The temperature of the water is 40 to 60 deg.C at its upper levels, but deeper down it reaches as much as 160 deg.C." The crustal thicknesses and depth to the Moho, cited by van Andel and Hospers in rebuttal of the expansion model, are thus what would have been predicted from my model which was set up before these data were known.

Van Andel and Hospers also cite the report by Mirchink *et al.* (1967) (of basement horsts under the West Siberian lowland composed of strongly deformed metamorphic geosynclinal formations cut by various intrusions) and state that "this is, of course, again in obvious disagreement with the composition of the basement following Carey's hypothesis, namely a composition similar to that of the ocean floor." But these data are in no way inconsistent with the rhombochasm model, any more than the Danikil basement horst is inconsistent with the Afar - Red Sea rhombochasm. If the Japan Sea were filled, as indeed the nearby Ordos and East China basins have been (Figs. 177 and 183),the resulting plain would resemble the trans-Uralian plain, and the Yamata horst would resemble those reported by Mirchink.

Minimum scatter method

Egyed pioneered also the minimum scatter method (1961), applicable to palaeomagnetic data generally, irrespective of relation to palaeomagnetic meridian. He proposed to determine the pole position for each site with a series of assumed values for the palaeoradius, and to examine these poles in pairs, to find which assumed radius gave the minimum scatter of poles. The technique was improved by Ward (1963, 1966) and van Hilten (1963, 1967) and was used by van Andel and Hospers (1969) and McElhinny and Brock (1975).

Ward realised that errors could arise from deformation of the continental slab in adjustment to changing earth radius. He wrote (1966): "The model assumes that the continental block remains constant in size as the Earth's radius changes, however the shape must change slightly, as the continent must accommodate itself to the changing curvature of the Earth's surface. The simplest form of this accommodation was incorporated in the model which I used: it was assumed that there is a central point in the continent at which the strain due to this curvature change is zero, and that the strain is axially symmetric about that point and entirely tangential with no radial component. Thus the distance from the central point to each rock unit will remain constant so that the corresponding geocentric angle is given by θ_i'/ρ where θ_i' is the present value of that angle. Further, the angle subtended at the central point by a pair of rock units $(\phi_i' - \phi_j')$ will remain constant. Finally the declination of magnetization of each rock unit D_i', measured with respect to the central point, will remain constant, as of course will the geocentric angle Ψ_i between each rock unit and the corresponding pole". In effect Ward adopted the centroid of the sample sites.

Van Hilten (1967) knew that the deformation problem was serious. He chose the centroid of the continent with the suggestion that "the central part of the continent will be hardly deformed; near its rims, however, we might suppose that radial tension faults develop." Van Hilten recalculated several sets with different assumed centres, and established that the pole position deduced was very sensitive to the position of the assumed centre. Unfortunately, adoption of the centroid of the continent or adoption of any other point does not remove the deformation difficulty.

All cartographers know that a part of the spherical surface of the earth cannot be represented on a plane map without distorting some parameters, so specific projections have been devised to preserve those parameters most relevant to the particular study. Thus equal-area projections conserve areas, but sacrifice angles, shape and length. Azimuthal projections conserve bearings from a selected point but sacrifice shape, usually all angles other than those from the origin, and, commonly but not necessarily, areas. Orthomorphic projections conserve angles and scales from any point, and hence conserve shape locally, but sacrifice area, and uniformity of scale. The Mercator projection is orthomorphic and also conserves bearings from any point on the map to any other point, but these are rhumb bearings not great circle bearings.

All these restraints apply equally to the transformation from the surface of a smaller sphere to a larger sphere and vice versa. The sphere-to-plane transformation is only a special case of a general relation.

Ward (1963, 1966), and those who followed him in purporting to determine the palaeoradius from palaeomagnetic data, arbitrarily assumed that an azimuthal equidistant equal-area transformation about a selected point of the continent validly represented a transferred continent (see Ward's specification quoted above). At the outset Ward's model is an impossibility. An azimuthal transformation may be equidistant or equal-area but not both, although Ward specifies both. In fact he does not use the equal-area presumption in his subsequent analysis (nor is it relevant to it), so we may delete the "constant in size" prescription from his specification. But Ward then proceeds to measure angles in the transform from points other than the projection centre, and, to make matters worse, these points must be at least a few tens of degrees apart to escape the uncertainties of the data. Angles measured from two such points other than the projection centre of an azimuthal equidistant transform are certainly incorrect.

Dr. W.D. Parkinson has shown the effect of the size of the area on such azimuthal equal-area projections, for transformations involving doubling the global area ($\sqrt{2}$ times radius), which is the kind of expansion inferred since the Permian. The following table considers a circle of radius given in the first column:

TABLE VI

Radius of circle considered	% change area	% change in parameter	% increase in length of chord subtending 90° at centre	Change of 90° angle at centre	Change of 90° angle not at centre
20°	0.0	0.0	0.5	0°.6	0°.6
40°	0.1	3.3	1.4	2°.6	2°.7
60°	2.6	9.3	5.8	6°.5	6°.9
80°	4.4	20.4	16.4	13°.3	13°.8

Geophysicists commonly set up a quantitative earth model, which they can handle mathematically, they perform certain mathematically valid operations on this model, from which they derive a conclusion. So far so good. Their error intrudes if they go on to affirm that this is how the real earth has behaved. This is only true if the model parameters and the operations performed on them are strictly true for the real earth. This is certainly not so for any of the palaeomagnetic calculations of palaeoradius.

Each of the transformations used or contemplated assumes that, area, inter-great-circle angle, geocentric angle, or shape of spherical triangle remains constant. None of these assumptions is valid for the transformation from the present earth surface to a former earth surface. There is only one transformation which is valid - and that is the single transformation which actually occurred. In that transformation *none* of the specified parameters (area, surface angle, surface distance, geocentral angle) held constant. *All* varied - though none of them so much as when only one or two varied. Van Hilten assumed that the centroid of the continent was a reasonable origin for azimuthal equidistant equal-area operations, and that the accommodation was met by radial gores which increased peripherally; but the earth did not behave like this; adjustment occurred throughout in a penetrative mode (in the sense of structural analysis); the centroid has no unique properties. Moreover equidistance and equal-area are mutually incompatible in all azimuthal projections. Ward's model was similar, except that he chose the centroid of his sites rather than the centroid of the continent. Van Hilten's method looked better to him geologically, Ward's looked better to him geometrically. Neither was valid. The departures from reality are only small locally, but they increase rapidly to dominance with increasing geocentric angle (see table VI) and these geocentric angles have to be large (several tens of degrees) before any palaeoradius differentials could show up.

The behaviour of the real earth had already been worked out without any reference to the present context (Carey 1958b, 1963, and pp. 42-46 herein). The simplest approach to the adjustment of the lithosphere to changing radius is to model the crust as being composed of vertical joint prisms of the order of a metre across (the seventh-order polygons of Fig. 20). If I painted across the tops of these joint-polygons the spherical triangle of two sites and the contemporary pole, and let the whole earth expand with adjustment distributed uniformly between all the polygons of the lithosphere, the painted spherical triangle would maintain both its surface angles and its geocentric angles, its sides would remain great circles, but there would be no point, centroid or other, possessing any unique properties. Simply expressed, this expansion mode is the same as increasing the linear scale of the globe, which does not alter any of the angles. However the requirement of linear constancy of the sides of the triangle assumed by Ward and Van Hilten has already failed. We could visualise a similar simple earth model where the polygonal tessellation was made up only of the sixth-order master joints, or the fifth-order, or fourth-order polygons of figures 20 and 21. The above presumptions would remain valid, provided the scale of the spherical triangle was large compared with the scale of the tessellation. As we move up to an earth model whose adjustment was entirely through the second-order polygons of figures 17 and 20, or entirely through the first-order polygons of figure 6, this scale limitation could no longer be held, as the angular change and linear separations between polygons would be large, and in any case the strength of the lithosphere could not sustain a crust consisting only of the first-order polygons, without concomitant adjustment on the polygons of higher order.

The real earth, in fact, shows a combination of tessellations of all orders, all playing their part. In such a symphony none of the fixed parameters required by the palaeomagneticians remain constant. Reverting to the painted spherical triangle, geocentric angles change; surface angles of the spherical triangle change; the spherical triangle is no longer a spherical triangle in that its sides become small circles, not great circles. The linear distances along the sides also change. The sum of the surface angles of a spherical triangle varies between the limiting values of two right angles (when the surface approaches a plane) and six right angles (when the triangle approaches a hemisphere). In the simplest polygonal tessera-expansion model above, these angles remained constant. In the compound real model these angles all diminish. Neither of the models assumed by Van Hilten and Ward is valid. Although intrinsically quite simple, and indeed predictable, the real-earth transformation is not simple mathematically. For easy calculation it would be nice to constrain the earth to hold at least some of the parameters constant. But, in fact, all parameters share in the adjustment.

Were it possible to devise an expression to accommodate variation of all parameters, and to master the computations, the data to feed in to such a computation would be too complex to gather. Hence, not only are none of the current methods of estimating palaeoradius valid, but I see no prospect now or in the future of doing any better.

From such false foundation Ward proceeds with his statistical hocus-pocus leading to his verdict backed with computer authority. Using his centroid azimuthal equidistant frame he feeds in all the coeval sites with their polar geocentric angles, and computer pole positions, assuming progressive steps in ancient radius (in terms of ratio of ancient radius to present radius, $\frac{Ra}{R}$). That assumed ratio which results in the least scatter of poles is for him most probably correct. As all of the parameters assumed to be constant vary with the radius, at a rate which increases rapidly as the size of the triangle increases, the minimum scatter inevitably occurs with the least change from this base radius (R), i.e. when $\frac{Ra}{R} = 1$. This indeed is what everyone has found who has used the method, and as everyone will find who uses the method in future. *This predictable result has nothing to do with the former radius of the earth.* If all the relevant arcs remained great circles, the rapid decrease of the sum of the angles of a triangle between the limits of six rightangles and two rightangles would of itself ensure that the least scatter would occur when the present radius was used. But the problem is even more complex than this, because none of the original arcs remain great circles, although they do adjust partly that way.

A blunt tool

Rezanov (1968) scathingly criticised the misuse of palaeomagnetic data, for example, to confirm the large latitudinal migration of India during the Mesozoic when other equally large latitude changes based on equally well screened data within one block are ignored. Palaeoradius has been determined from selected sets of data, yet other sets, from equally well screened data, are ignored, for the good reason that the result would obviously be false. Scatter of poles from a single continent for a single period is sometimes as wide as they could be on the earth's surface. Rezanov finally concludes "the palaeomagnetic data are still so unreliable and contradictory that they cannot be used as evidence either for or against the hypothesis of the relative drift of continents or their parts. Nor are they serviceable as proofs of an expansion of the earth."

Rezanov's criticisms have validity, because there are so many sources of large error that, even after all care, many contradictions and anomalies remain. Some of the sources of error are:

(1) Magnetic instability can for the most part be washed out thermally or electromagnetically, and sometimes fold, conglomerate, or reversal confirmations of stability give reassurance. Nevertheless some anomalies from this source probably remain.

(2) A large scatter results from the secular migration of the magnetic pole about the rotation pole, which can be resolved statistically provided sufficient cycles are covered by the samples. Nevertheless, with the limitations of outcrop and collecting diligence, sometimes quite large anomalies must remain from this source.

(3) As the earth's obliquity to the ecliptic now appears to be one of the most important geological variables, the angle between rotation axis and the magnetic axis precessing around it also probably varied substantially in the past. The relative magnitudes of the dipole and non-dipole fields also changes greatly at each reversal (Dunn, Fuller, Ito, and Schmidt, 1971) and perhaps more generally. Whereas the dipole assumption seems empirically to hold substantially through the observable geological record, nevertheless there are real departures from this model now, and the degree of departure may well have varied sufficiently in the past to inject significant errors into some of the palaeomagnetic results.

(4) Inaccuracies in field measurement of reference azimuth and inclination, in transfer to the bicylinder coring, and in the laboratory measurement, all add their bit.

(5) Remagnetization, through igneous activity, metasomatism, metamorphism, or lightning strikes, remains a potential source of large error, which may give a false age to the measured magnetization.

(6) Inaccuracy of dating, whether by radioactive isotopes or by fossils, may not be very significant when plotting continental movements, but can lead to large errors when data are combined across a continent for such purposes as palaeoradius estimation.

(7) Measurements from folded strata are normally corrected on the assumption that the folding was cylindroidal, so that the observed direction of magnetisation may be rotated through the dip angle about the direction of strike. This assumption may be invalid, and according to the relation of the direction of magnetisation to the pitch, may lead to large errors. Sampling several parts of the fold, or several folds, may reduce or eliminate this error statistically, but available outcrops rarely allow this.

(8) The earth's surface is broken up in an hierarchy of tensional polygons ranging from the primary continental blocks to various subordinate orders which have moved differentially (Figs. 6, 16, 19). We have already seen that sediment-filled rhombochasms give a false impression of integrity to a continent which may lead to large errors, but in the subordinate polygons the cumulative total of large numbers of very small angles in the same sense statistically, completely defeats attempts to measure palaeoradius.

(9) Most palaeomagneticians plot poles on the gratuitous assumption of a constant-radius earth, which may lead to large errors and scatter for the older periods.

With so many treacherous gremlins, Rezanov's defeatist conclusion is not surprising. But though palaeomagnetism *is* a blunt tool, it is not as useless as Rezanov would depict. Although Rezanov claims that increasing the number of observations increases the overall scatter, the statistical precision does usually improve, and where this is not so, one or more of the above errors remains to be eliminated. However palaeomagnetism is particularly sensitive to rotations of small blocks, and may reveal this reliably even when the palaeo-latitudes are affected by some of the above errors (Table 1, pp. 4-5).

Quite apart from the large departures of the real earth from the theoretical modes of geometrical transformation in adjusting to different curvature, palaeomagnetic measurements are not sensitive enough to detect earth expansion on a single continental block, and show no future prospect of doing so. For example, consider two stations giving palaeomagnetic inclinations of 85° and 20° (corresponding to latitudes 80°4' and 10°19') with a 5° cone of confidence about each of them. The implied latitude difference could be anywhere in the range 52° to 89° - that is a palaeo-radius range of 30%. This is the most favourable case for a block like North America, Asia or Europe which has remained in the northern hemisphere since the early Palaeozoic. In general, the localities on a single continental block are less than 70° apart, and then not necessarily in the direction of the palaeo-meridian; 95% probability cones are commonly greater than 5°; dating of sequences so far apart is rarely as close as ten million years. Each of these variables further extends the palaeo-radius uncertainty beyond 30%.

McElhinny and Brock (1975) claim to increase greatly the precision of pole determinations by combining many individual statistical poles, and they identify the whole Mesozoic pole for Africa at 149°.5E, 67°.4N with a 95% confidence of 4°.4. This of course is absurd. Certainly statistical combination of a large number of randomly imprecise measurements of a single entity sharpens the precision of the determination of that entity beyond the precision of any one determination. But the statistical combination of a large number of imprecise measurements of many entities whose variation is real does not sharpen the precision of any one of them, and the average has no real meaning, beyond a game with numbers, and its 95% confidence parameter is a fantasy.

For example, if we combine statistically a large number of positions of the south magnetic pole over the last million years, we do get a statistical position at or near the south *geographic* pole, with a high degree of confidence. But this is not a more precise position of the south *magnetic* pole with that high degree of precision. So far as we know, the south magnetic pole at no time occupied this position, in fact it may never have been within 20° of that position. Likewise McElhinny and Brock cannot get a valid position for the geographic pole for Africa for the whole of the Mesozoic by combining many determinations of the African Mesozoic pole. They could, of course, get the centroid of these poles, perhaps with a small confidence circle. But the real locus of the pole may never have entered this confidence circle, or even a circle several times larger. In this case the illegitimate statistical treatment hid still another kind of error; for, as Wilson (1970) pointed out, when these data are analysed by regions, the mean "pole" for each region overshoots the mean for all regions.

Does this then imply that palaeomagnetism is unable to determine palaeo-radius questions? Not so. But two conditions are necessary. First the method adopted must be applicable validly over large arcs covering more than one continental block, so that the answer rises well above the noise of the uncertainties involved. The second condition is currently refractory. Palaeomagnetism today is in a state similar to that of geology of the forties and fifties, when most geologists *knew* that large scale continental displacement was a physical impossibility. Most palaeomagneticians today *know* that large expansion of the earth is a physical impossibility. So the second condition is that they excrete their prejudice. This was virtually impossible for the geologists and geophysicists of the thirties, forties and fifties, good sound responsible scientists as they were. A Kuhnian revolution was necessary. It may well be impossible for most palaeomagneticians of today, good sound responsible scientists as they undoubtedly are. For one thing they must concentrate on palaeolatitudes of sites, not inferred poles which have no validity if the radius was different.

With this unpromising overture let us attempt the poly-continental symphony.

At an early stage, I found that Pangaea models gave too short a surface distance to correspond to the intervals between palaeolatitudes on the assembled blocks. This to me implied a smaller ancient earth. But this evidence was not crucial, because in some cases escape was possible by claiming removal of formerly intervening oceanic crust by subduction, and even when this was not possible, the conclusion could be side-stepped by disallowing the particular reconstruction of Pangaea, even on the circular argument that it involved the stated palaeomagnetic inconsistency.

However when palaeomagnetic data are examined globally there remain paradoxes which only an expanding earth can solve, and which do not allow escape through subduction or through any debatable reconstruction.

GLOBAL PALAEOMAGNETISM REQUIRES EXPANSION

Arctic paradox

Palaeomagnetic data agree that the Permian equator crossed North America roughly through Texas and Massachussets. The equator is now along the Amazon. Hence North America is now some 40° nearer the north pole than it was in the Permian. European palaeomagnetic data agree that the Permian equator passed close to the south of France about where latitude 40°N now runs. Hence Europe is now some 40° nearer the north pole than in the Permian. Similarly Greenland is about 50° nearer the north pole than it was in the Permian. The Siberian Permian data show a large change in direction of the meridian, but this was to be expected because translation in high latitudes causes change in direction of the meridian unless the motion is along or normal to the meridian. However it is clear that Siberia is now some 17° nearer the north pole than it was in the Permian, and the Pacific coastal region of Asia has moved about 23° nearer the north pole since the Permian. Inaccuracies of measurement and a statistical integration of the data leave some looseness in all these figures, but these uncertainties are only a few degrees. Hence the verdict is clear; since the Permian all the countries surrounding the Arctic have converged from all sides. Yet all but the fixists agree that since the Permian, the Arctic has been a disjunctive area. The fixists claim one significant change. None allege post-Permian subduction within the Arctic. The convergence towards the Arctic, combined with fixism or disjunction, is impossible except on an expanding earth.

Permian palaeomagnetic data from the Gondwana continents wholly support their Laurasian contemporaries. The Permian pole according to South American rocks was in the South Atlantic near the Falkland islands. South America is some 40° further north than in the Permian. The Permian palaeomagnetic pole for Australia was in the Great Australian Bight. Australia is some 45° further north than it was in the Permian. The South African Permian rocks put the pole in the southern Indian Ocean. Africa is some 40° further north than it was in the Permian. India is some 60° further north than in the Permian. Every continent on the globe except Antarctica has moved further from the south pole and nearer the north pole since the Permian, through a time interval when the Arctic itself was extending. Such a situation is impossible except on an expanding earth, and indeed on an earth suffering greater crustal expansion in the southern hemisphere than in the north. This rider is of course indicated independently by the much greater dispersion of the Gondwana continents than the Laurasian.

The only possible solution to this paradox is global expansion, which allows the parallels of latitude to sweep over the continental blocks (Fig. 105). This test is crucial and absolute. Even the Pacific Ocean floor has shared this northward march in latitude -- 30° since the mid-Cretaceous (Vine, 1968), and 15° since the Oligocene (Hammond and Theyer, 1974) -- but I have omitted this from the argument because escape from this part might be attempted by seeking sanctuary down the Aleutian trench (if its tensional character is set aside). Such escape is not possible for the Siberia, North America, Europe set. Let us therefore re-examine the evidence in detail, period by period, confining the study to the peri-Arctic blocks, where the absence of any alleged subduction leaves the issue beyond doubt.

TABLE VII

Permian periarctic palaeolatitudes

	Present Longitude	Present Latitude	Palaeo-latitude	Movement North	
N. America	112W	36N	6±6	30±6	37.1°±5
	110W	38N	4	34	
	99W	35N	1±5	34±5	
	81W	39.5N	4±4	35.5±4	
	64W	46N	8	38	
	23W	72N	21±7	51±7	
Europe	4E	47N	5±4	42±4	39.8°±8
	8E	50N	8±13	42±13	
	10E	60N	20±2	40±2	
	38E	48N	8	40	
	52E	58N	23±15	35±15	
Siberia	68E	52N	37	15	17.1°±5
	70E	44N	27	17	
	70E	41N	25	16	
	87E	54N	23.5±6	30.5±6	
	92E	53N	46±4	7±4	
Pacific Margin	102E	72N	51±5	21±5	22.8°±23
	103E	30N	2±40	28±40	
	117E	36N	16±38	30±38	
	132E	43N	31±10	12±10	

Table VII and figure 63 show the available Permian data. In this table and in those that follow, I have included all the data listed in the compilations by Irving and by McElhinny as satisfying acceptable conditions of sampling and laboratory treatment. The first two columns give the present longitude and latitude of the sampling site. The next column gives the palaeolatitude with the confidence limits. The fourth column gives the number of degrees the station has moved north since the Permian. Note that *all* stations indicate a northerly movement. The mean northward march for the eight North American sites is 37°.1, for the five European sites 39°.8, for the five Siberian sites 17°.1, and for four Pacific margin sites east of the Assam megashear (which could complicate these results) 22°.8. On an earth of fixed radius this means that North America and Siberia have approached each other across the Arctic by 54° (or by 44° if *every* result is biased to the extreme limit of its confidence circle), this during a time when the Arctic was a disjunctive region. The corresponding angular convergence for North America and Europe is 77°.

A superficial inspection might suggest escape from this conclusion by assuming that polar wander from a Permian position in the northwest Pacific together with Atlantic opening might achieve this result. For example, on the stereographic projection figure 64, wander of the pole 40° towards the points A and B moves both these points 31° higher in latitude while they remain fixed relative to each other. But such effects reduce to zero when they are separated by 180° longitude. In the Permian case we have four separate blocks distributed round 250° of longitude. Opening of the Arctic-

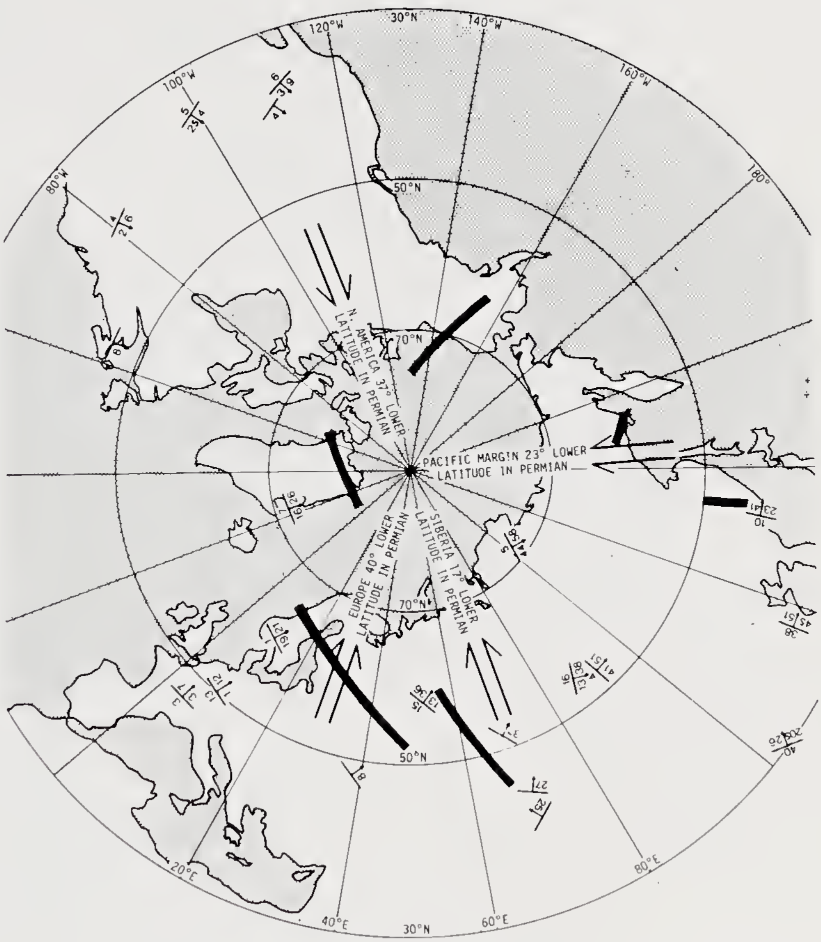


Fig. 63

Permian periarctic palaeolatitudes.

Atlantic gore by 30° and the Ob sphenochasm by 10° does not help. The situation is illustrated by drawing the 30°N Permian palaeolatitude on each of the blocks, North America, Greenland, Europe, Siberia, Sikhote Alin, and Kolyma, (shown by the thick lines on fig. 63). These fragments (which

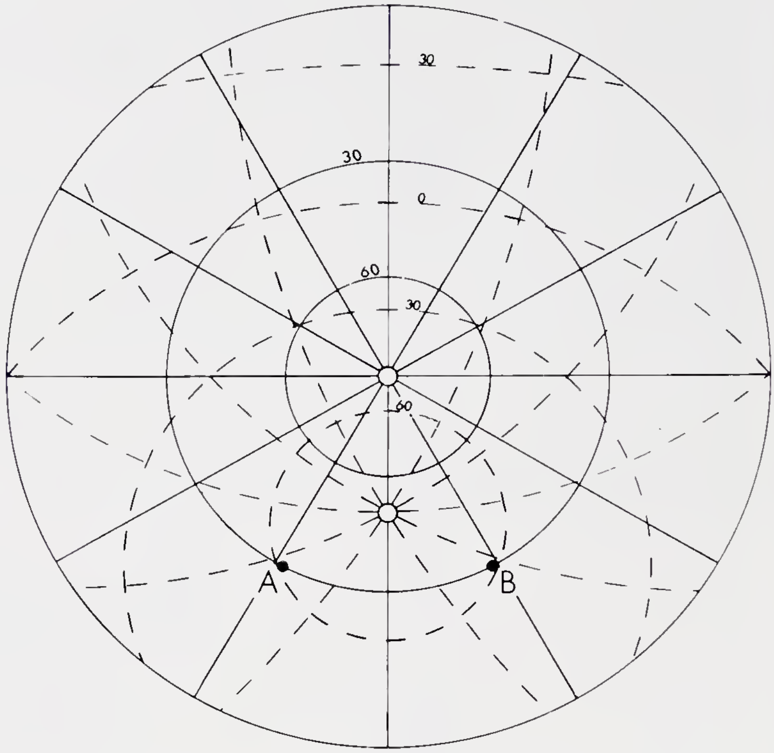


Fig. 64

Latitude change with polar wander (stereographic projection).

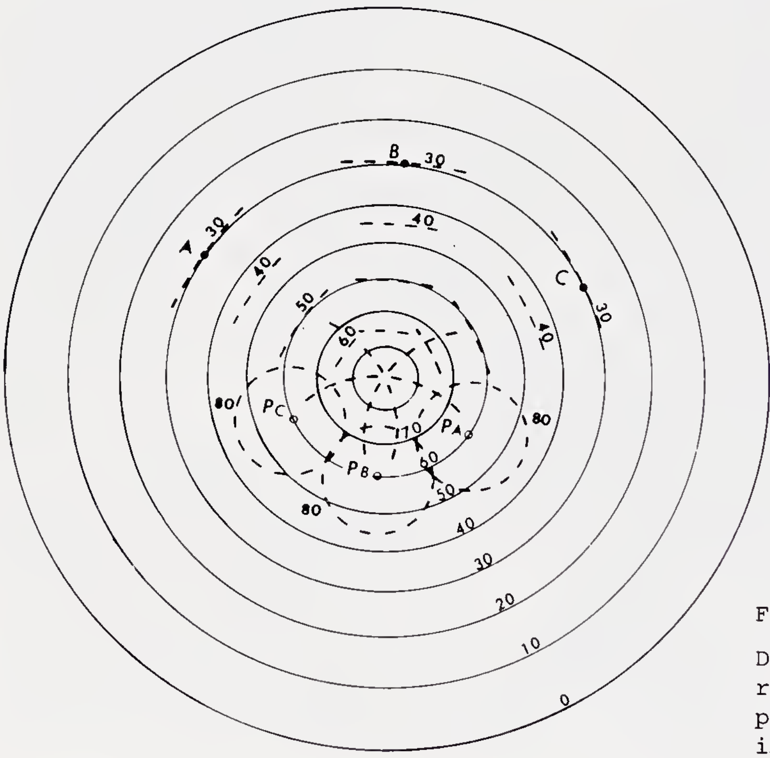


Fig. 65

Discordant azimuths result from incorrect palaeoradius assumption in high latitudes.

should fall on a circle within 60° radius), all fall within a circle with 32° radius. This requires that the length of a degree was much shorter in the Permian than at present. This difficulty is exacerbated by the patent disjunctive character of the post-Permian Arctic tectonics.

Note also that the segments of the projected 30°N palaeolatitude in figure 63 are not concentric. This, of course, is partly due to opening gores between them, but such non-concentricity is inevitable if the change in earth radius is not taken into account. For example, in figure 65 three hypothetical sample sites A,B,C, which lay on the same parallel (30°N) yield widely divergent palaeopoles P_A, P_B, P_C, and non-concentric parallels (broken) if the present radius of the earth is used instead of the palaeoradius. This becomes particularly confused near the projected palaeopoles.

Table VIII and figure 66 set out the Triassic data. Here the mean northward shift of nine North American sites is 23°.2, and the mean of four European sites 29°.2; the mean of four Siberian sites is 14°.2, and of five sites east of the Assam megashear 6°.4. This means that on a fixed radius

Fig. 66 Triassic periarctic palaeolatitudes.

earth, North America has approached Siberia across the disjunctive Arctic by 37° and Europe by 52° since the Triassic. Even if the disjunctive were disclaimed, this is impossible unless the earth is expanding.

TABLE VIII

Triassic periarctic palaeolatitudes

	Present Longitude	Present Latitude	Palaeo-latitude	Movement North	
N. America	121W	51N	32±12	19±12	
	112W	37N	10.5±13	26.5±13	
	108W	40N	9±19	31±19	
	107W	43N	12.5±7	30.5±7	
	75W	41N	12.5±8	28.5±7	23.2°±9
	68W	52N	23±10	19±10	
	65W	45N	27±8	18±8	
	63W	56N	51±2	5±2	
	23W	72N	51±3	21±3	
Europe	8E	47N	13±7	34±7	
	38E	48N	32±18	16±18	29.2°±12
	50E	50N	32	18	
	50E	59N	10	49	
Siberia	63E	56N	40±9	16±9	
	71E	41N	23±9	18±9	14.2°±9
	92E	67N	71±12	-4±12	
	101E	71N	44±5	27±5	
Pacific Margin	108E	75N	51±6	24±6	
	114E	63N	83±13	-20±13	
	116E	40N	10±32	30±32	3.3°±15
	132E	43N	54±10	-11±10	
	159E	63N	54±23	9±23	
	102E	3N	15±6	-12±6	

TABLE IX

Jurassic periarctic palaeolatitudes

	Present Longitude	Present Latitude	Palaeo-latitude	Movement North	
N. America	123W	38N	24±8	14±8	
	122W	37N	50±5	-13±5	
	112W	37N	30±8	7±8	1.4°±7
	109W	39N	44±9	-5±9	
	71W	44N	40±4	4±4	
Europe	13E	48N	31±6	17±6	17°±6
	55E	40N	23	17	
Siberia	88E	67N	60±6	7±6	7°±6
Pacific Margin	158E	64N	68	-4	
	163E	66N	67±15	-1	-4°
	166E	65N	72	-7	

Fig. 67

Jurassic periarctic palaeolatitudes.

Table IX and figure 67 set out the Jurassic data. The five North American sites give a mean northward displacement by 1°.4, the single Siberian site by 7°, the two European sites by 17°, and the three Pacific stations east of the Assam megashear -4° (i.e. relative movement south. This last is consistent with the dextral displacement of this zone, which reaches a maximum of 40° relative to the rest of Asia). Thus North America and Siberia have approached each other by 8° across the Arctic since the Jurassic, and North America and Europe by 18°.

Fig. 68 Cretaceous periarctic palaeolatitudes.

TABLE X

Cretaceous periarctic palaeolatitudes

	Present Longitude	Present Latitude	Palaeo-latitude	Movement North	
N. America	121W	49N	30±11	19±11	3°.5±9
	120W	38N	42±4	-4±4	
	109W	41N	52±13	-11±13	
	104W	79N	69±8	10±8	
	93W	35N	35±9	0±9	
	73W	45N	38±7	7±7	
Europe	18E	46N	28±19	18±19	19°.0±16
	38E	41N	19±14	22±14	
	55E	40N	23	17	
Siberia	67E	38N	36±6	2±6	4°.3±10
	71E	41N	29±15	12±15	
	92E	56N	57	-1	
Pacific Margin	126E	39N	50±4	-11±4	-6°.3±12
	129E	36N	34±10	2±10	
	132E	43N	72±11	-29±11	
	135E	35N	42±15	-7±15	
	139E	36N	31±2	5±2	
	141E	39N	10±22	29±22	
	142E	47N	51	-4	
	157E	62N	78±14	-16±14	
	163E	67N	76±22	-9±22	
	164E	62N	62±15	0±15	
	166E	65N	84	-19	
	103E	2N	17±9	-17±9	

Table X and figure 68 set out the Cretaceous data. Six North American sites indicate a mean northerly movement of 3.5°, three European sites indicate northerly movement of 19°, and three Siberian sites indicate a mean northerly movement of 4°.3, while eleven Pacific stations east of the Assam megashear indicate a mean northerly movement of -6°.3. These results are not significantly different from those of the Jurassic, with North America and Siberia approaching across the Arctic by 8° and North America and Europe by 22°.

TABLE XI Summary of circumpolar northward shift

	North America	Europe	Siberia	Pacific Margin
Permian	37°.1	39.8	17.1	22.8
Triassic	23°.2	29.2	14.2	3.3
Jurassic	1°.4	17.0	7.0	- 4.0
Cretaceous	3°.5	19.0	4.3	- 6.3

These results are summarised in Table XI. The Jurassic and Cretaceous results are not significantly different from each other. The overall pattern of global expansion is clear. Although the Arctic has opened, the much greater expansion on the southern hemisphere has caused the parallels to sweep south across North America and Eurasia (indeed also across Africa, India, Australia, and South America). The independent Gondwana and Alpide expansion cycles appear again here. The Gondwana expansion was strong in the Permian and through the Triassic, but died away through the Jurassic and Cretaceous. The Alpide expansion cycle is entirely Tertiary, and in the northern hemisphere about half the scale of the Gondwana cycle. The relative movement of the continents has not been precisely a dispersion from the present north pole. In the foregoing analysis only this component of the total movement, reflected in the change of latitude, has been used. Neither the longitudinal component, nor the concurrent polar wander can invalidate the conclusion of circumpolar convergence in latitude combined with physical dispersion, a combination only possible on an expanding earth.

In setting out this analysis I have been careful to avoid introducing the Pacific Ocean floor, not because similar northward convergence did not occur there, but because those who believe that trenches are subduction zones could side-step by claiming that the northward motion was swallowed in the Aleutian trench. In fact the palaeomagnetic data establish that the Pacific Ocean floor between Hawaii and Japan has moved north 30° since the mid-Cretaceous, and that Midway Island has moved north 15° since the Oligocene (Vine, 1968); the equatorial central Pacific has converged on the north pole by some 18° since the Oligocene (Hammond and Theyer, 1974), a rate of the same order as the convergence of the continental blocks. Further south, the Ontong Java Plateau (DSDP 289) has moved progressively northwards from 10°S in the Aptian (mid-Cretaceous) to its present latitude of ½°S (Hammond *et al.*, 1975). The mechanism of these latitude shifts is shown in Fig. 105.

Double equator paradox

The complementary paradox of the "double-equator" shows up in the Mediterranean. The Triassic equator, projected from European rocks, passes through the Mediterranean (Irving, 1964, fig. 9.8). But the African rocks put the Late Triassic to early Jurassic equator through the Cameroons and Aden, more than 30° further south (Irving 1964, fig. 9.47). Classical and currently fashionable tectonics would have the Mediterranean orogenic belts as compressional or subduction zones where hundreds of kilometres of crust have been swallowed as Africa advanced on Europe, squeezing out the great nappes of the Alps. Contrariwise, I have always maintained that the Mediterranean is a disjunctive zone where Africa has *retreated* from Europe by about 10° and that the orogens are diapirs which have been *widened* transversely somewhat during their evolution. If plate theories were correct, there would have to be lost parallels representing formerly intervening ocean crust, or continent underthrust below continent. But if I am right there should be repetition of parallels projected from the two blocks. The palaeomagnetic latitudes cited above are unequivocal, that the Mediterranean is dilatational, at first sight by nearly 40°. De Boer (1965) would escape from this dilemma by claiming a large dextral shift of Africa against Europe, whereas the relative movement can only be sinistral (see pp. 256-259).

The same paradox is repeated in the Americas. The Triassic palaeomagnetic equator crossed from Baja California to northern Florida (Irving, 1964, fig. 9.27). But according to the South American rocks, the Triassic palaeomagnetic equator crossed from Northern Peru to the mouth of the Amazon (Irving, 1964, fig. 9.72). Here again plate theory requires subduction zones swallowing crust and meridional foreshortening in the east-west orogenic belts of Venezuela, and the Greater Antilles. The expanding earth on the contrary requires these orogenic belts to be diapirs in a dilating environment, and the whole of the Caribbean region and the Gulf of Mexico to be disjunctive and extensional, producing precisely the result found in the palaeomagnetic observations. In this case there is objective evidence to establish the meridional elongation, because when the Atlantic is closed with South America fitted into the Gulf of Guinea and North America wrapping round the bulge of Africa, the central American region has to be grossly foreshortened.

Alaskan orocline paradox

The Alaskan Orocline was described by Carey (1955, figs. 13 and 14) and Carey (1958, figs. 5, 6, 9 and 14), according to which North America had rotated 28° against Eurasia about a pivot in central Alaska, thereby opening the Arctic and North Atlantic Oceans. This rotation was confirmed palaeomagnetically by Irving (1958, fig. 3 and table 4). Subsequently, Dr. David Stone went to the Alaskan Geophysical Institute, inter alia to investigate a palaeomagnetic rotation around the Alaskan Orocline. In due course Packer and Stone (1972) selected nine Jurassic sites along the Aleutian Range - Alaskan Range belt, which they accepted as palaeomagnetically stable and mutually consistent, for comparison with the North American block. Their samples did indeed disagree with the American Jurassic pole. But far from showing a sinistral rotation of 28° with respect to the American block, they showed a *dextral* rotation of 52°! Surprisingly this result is precisely what should have been predicted from the expanding earth (Fig. 69). For, not only the Arctic, but the North Pacific has opened as new crust since the Palaeozoic. The North Pacific sphenochasm heads in the Gulf of Alaska.

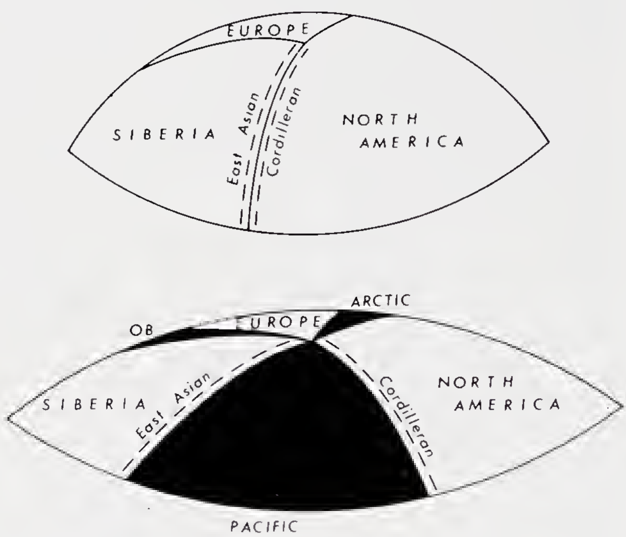


Fig. 69

Sense of interblock rotations about the Alaskan focus.

When a segment of a spherical shell is forced to adjust to a sphere of larger radius, radial gores must develop. This is what van Hilten (1963) called "the orange-peel effect" (Fig. 70). The focal point for the Arctic sphenochasm, the North Pacific sphenochasm, and several smaller sphenochasms, is, coincidentally, at Dr. Stone's base, the Alaskan Geophysical Institute. Hence, although the Arctic sphenochasm has indeed opened as indicated long ago by me, the Pacific has also opened concurrently. Hence the Alaska and

Aleutian Ranges (which extend south-west from the pivot into eastern Siberia), should show a large *dextral* rotation against North America, which is what Packer and Stone found. This paradox can only be resolved on an expanding earth. Some centrifugal dispersion occurred in combination with the opening of the radial gores shown diagrammatically in figure 69. The North Pacific sphenochasm heads in the Gulf of Alaska with one near-great-circle arm trending down the cordillera, and the other down the island arcs of the Aleutians, the Kuriles, Honshu, and the Ryukyus. The 55° dextral rotation of Guam since the Miocene reported by Larson and his co-authors (1975) is consistent with Packer's and Stone's findings in the Alaskan Peninsula, although more local dextral movements may have contributed, as suggested by the authors.

Fig. 70
"Orange-peel effect", suggested by van Hilten.

In my earlier papers, my concern was with the relation of the Alaskan Orocline to the opening of the Arctic and Atlantic (1958, figs. 6, 9, and 14). To test the Alaskan Orocline in this context, it would be necessary to compare magnetic azimuths of the Brooks Range with those of the North American block. In that publication I made it clear that there were further sphenochasms between the Brooks Range and the Aleutian and Alaskan Ranges, but the North Pacific sphenochasm, although suspected, had not been categorized because of the total lack of information about the sea floors in the early fifties. Perry (1972) independently proposed a course of evolution for the Gulf of Alaska and the Aleutian Trench, identical with my reconstructions, and attributed by him to earth expansion.

Malay peninsula paradox

Palaeogeographic data have long indicated that the Malay Peninsula and the associated archipelagoes of southeast Asia have been closely related to Australia throughout the Phanerozoic. Continuity between east Asia and Australia is required by the early Cambrian archaeocyathids, the Cambrian trilobites (*Redlichia and Mindyallan* faunas), the Ordovician cephalopods, the Llandovery corals, the *Monograptus-Tentaculites* faunas of the Yunnan-Malaya and Tasman geosynclines, the Devonian brachiopods, the Carboniferous foraminifera, the mingling of the *Glossopteris* and east Asian floras in New Guinea, Sumatra, and the Malay Peninsula, the early Triassic amphibians and the Jurassic *Buchia-Belemnopsis* faunas. Burton and Ridd have emphasized that, through the Palaeozoic, cratonic land lay to the west of the Malay Peninsula (south of it when the Mesozoic clockwise rotation is reversed), and this land could be scarcely other than India. The main Tethys seaway passed north of Indo-China.

Paradoxically, McElhinny, Haile, and Crawford (1974) concluded that "palaeomagnetic results from Malaya show that the Malay Peninsula lay 15 ° N during the late Palaeozoic, a result incompatible with the hypothesis that it once formed a part of Gondwanaland adjacent to India or Australia". However, this contradiction lies not in the data given by McElhinny and his co-authors, but in their interpretation of them. Indeed, far from being contradictory, the palaeomagnetic results in question agree precisely with the reconstruction of this region which I had deduced solely on tectonic grounds, without use or even knowledge of the palaeomagnetic measurements to come (Carey, 1958a, 1975).

McElhinny *et al.* measured the remanent magnetism of 60 specimens of red sandstones, rhyolites and basalts, ranging in age from Cretaceous to pre-Visean, and ranging along the Malay Peninsula from Singapore to the Thai border. Most were stable and results were fairly consistent. Two distinct groups emerged: the Cretaceous rocks, which were mutually consistent, and the pre-Cretaceous, which were consistent between themselves but quite distinct from the Cretaceous.

If we accept as valid all the palaeomagnetic data provided by these authors, and accept their interpretations, that they represent reversed polarity, with the Malay Peninsula lying latitudinally between 10° and 20° north latitudes during the Upper Palaeozoic, these results are in total harmony with my tectonic reconstructions of Pangaea (e.g. Carey 1975, Figs. 8, 9 and 11, which are reproduced as Figs. 170,179 and 185 herein) in which the Malay Peninsula is an integral part of Gondwanaland, and at the same time part of Pangaea. The Sundra shear and Melanesian shear in these figures becomes the boundary between the Asian polygon and the Indian and Australian polygons of figure 6. Note the position of the Malay peninsula with respect to this palaeo-equatorial boundary.

McElhinny and his co-authors assume an earth of present radius, which in compiling continents from a smaller earth would necessarily involve large apparent separations even if distributed, but which they concentrate entirely between Malaya and Gondwanaland. This artefact inevitably forced these authors to conclude that there was no possibility of Malaya having been contiguous with India or Australia.

What these authors prove is that Malaysia cannot be part of Gondwanaland *on a "plate tectonics" model*. Taken with the clear geological evidence cited above from the continuity of the western Pacific region throughout the Phanerozoic, their results imply that earth expansion is inescapable.

McElhinny and his co-authors go on to point out that between the Triassic and the Cretaceous the Malay Peninsula suffered a clockwise rotation of 70°, whilst maintaining much the same palaeolatitude. This again is precisely what I had long ago deduced on tectonic grounds (Carey 1938, 1958a, 1970, 1975).

Waterhouse's Yukon Permian anomaly

Waterhouse (1969) and Waterhouse and Bamber (1971) have emphasized the misfit between the projected Permian palaeomagnetic latitude of the Yukon region of northwest Canada and the Permian faunas there. They pose the paradox as follows:

"The Yukon brachiopod faunas also have important implications for continental drift and palaeogeography. Brachiopod and other faunas are sensitive to temperature and vary in both diversity and kind, with temperature as a major and also an indirect control. Massive coral reefs are presently most abundant in tropical or subtropical areas, and were probably most abundant in similar low-latitude areas in the past as well. Permian reefs are accompanied by numerous other equally stenothermal life forms, notably Fusulinacea. Fewer, but equally distinctive genera required cool waters and could not tolerate high temperatures. In addition, there is an overall diversity gradient in distribution of present life from a peak at the present equator to a pit at the geographic poles (Fischer, 1960; Stehli *et al.*, 1967). Using this analogy, the greatest diversity at any particular time in the past should indicate the likely position of the equator at that time, and the least diversity, or diversity pit, should indicate the probable geographic pole, or at least the nearest preserved place to the pole. Intervening faunas and distances can be graded also. It is possible, therefore, to use fossil life to calibrate a relative scale of temperature and possibly paleolatitude. In Australia, for example, the distribution of Productacean brachiopods, under close time-control, has been calibrated to show that

Tasmania probably lay almost at the South Geographic Pole and that Western Australia lay farthest from the South Pole (Waterhouse, 1969) . . . But this agreement between evidence from paleomagnetism and that from brachiopod distribution does not apply to the Canadian Permian faunas of the Yukon. As noted by Waterhouse (1967), the Yukon faunas are considerably out of phase with the paleomagnetic data, which suggest that the Yukon occupied a paleolatitude of about 21° North (Irving, 1964, Fig. 9.41, p.208). This pole position, plotted chiefly from results in Colorado, has been supported by further Colorado data (McMahon and Strangway, 1968) as well as by work in the Maritime provinces. Such data suggest that the well-known Texan faunas occupied a magnetic latitude of 17°S and should thus match the Yukon faunas to a considerable degree. But the faunas of these two areas are very different - as Stehli (1968) confirmed in questioning the reliability of paleomagnetic data. Judged from faunal diversity and also from the presence of certain genera such as *Wyndhamia*, *Tomiopsis*, *Yakovlevia*, *Licharewia*, *Spiriferella*, etc., and the absence of others such as *Eurydesma*, *Richthofenia* and *Lyttonia* (Waterhouse, 1969b), the Yukon Permian probably occupied a paleolatitude slightly lower (= warmer) than New Zealand and Western Australia, and slightly higher than Timor, in the neighbourhood of 40°N and more probably 45°N. This figure, calculated by analogy with Australasian distributions around a South Pole, is almost exactly the same as the distance from Yukon to Texas. The Texas faunas, judging by their astonishing diversity, almost total absence of cool-water genera (except during cool spasms), and abundance of warm-water life, lay at the Permian equator (Waterhouse, 1969, 1970b). In other words, the distribution of Permian life around a geographic pole seems out of phase with paleomagnetically based latitudes . . . The difference between the faunal and the paleomagnetic data remains a very important anomaly."

This paradox arises solely from the projection of palaeomagnetic latitudes from Colorado sites to the Yukon on the assumption that the Permian radius was the same as at present. Texas and the Yukon are now 40° apart. This surface distance would correspond to 57° apart on the Permian earth. The Texas palaeomagnetic latitude is 17°S which would imply 40°N from the Yukon which is what Waterhouse predicts from his faunas.

The Australasian Permian gradient described by Waterhouse, also fits this scaling. From polar Tasmania to the temperate northwest Australian faunas on the present earth-radius is 33° and to the Permian position of Malaya 49°, but with Permian earth radius these would be 49° and 90° respectively, which closely agrees with both the palaeomagnetic and faunal data.

Paradox of palaeopole overshoot

Several palaeomagnetists (e.g. Creer *et al.*, 1954, Cox and Doell, 1960, Irving 1964, Opdyke and Henry, 1969) have shown that the averaged palaeomagnetic observations for all Upper Tertiary rocks yield a mean pole which coincides closely with the geographic pole. This has become one of the pillars of palaeomagnetism. However, Wilson (1970) has re-examined the Late Tertiary mean poles, *region by region*, using all available data from continental igneous rocks and oceanic sediment cores, and has found that the mean poles from each source region overshoot their combined mean pole systematically (Fig. 71). This is precisely what should be predicted on an asymmetrically expanding earth, because the palaeo-geocentric angles (which is all that the data yield) correspond to a smaller number of kilometres than

Fig. 71 Overshoot of regional palaeomagnetic poles beyond global mean pole (after Wilson, 1970). In each case the open circle is the centroid of the sampling sites, and the filled circle joined to it is the mean pole corresponding to those sites.

on the present earth. On an earth of constant diameter or on a uniformly expanding earth, there would be no overshoot. But as on all relevant meridians the proportional expansion is greater along the major arc than along the minor arc from the sampling site to the palaeopole, all individual palaeopoles overshoot the mean palaeopole. Wilson's data would correspond to a linear expansion of some 8% in say the last 10 m.y. This again is consistent with my observation that the rate of expansion and of all tectonism has increased with time, and is now the fastest it has ever been.

A similar anomaly in the Permian has been reported by Waterhouse (1967), when palaeomagnetic latitudes measured in Texas (and in harmony with the faunas there) are projected to the Canadian Arctic. The faunal palaeolatitude (about 45°) conflicts with the projected palaeomagnetic latitude (about 20°), so sharply that Bamber and Waterhouse (1971, p.189) even suggested that Texas and the Yukon may have been different plates in the Permian and since fused! Admittedly faunal palaeolatitude estimation is a blunt tool in the sense of palaeomagnetic latitude estimation being a blunt tool (supra) but the discrepancy is indeed large. Latitude projection from Texas to the Arctic using an appropriate Permian radius reduces the anomaly to within the tolerances of the methods. It is perhaps significant that Permian palaeomagnetic latitudes determined from nearer rocks in north-east Siberia (Irving, 1964, p.197 and fig. 9.9) give much less anomalous latitudes when projected to the Yukon. This also accords with the expectations from the expanding earth model.

Watkins (1972), with more data than Wilson (1972), confirmed the pole overshoot found by Wilson for the northern hemisphere, but not for the southern hemisphere; he found long-term inter-hemisphere asymmetry to be real. This again is the direct prediction of the asymmetric earth expansion found empirically. The southern hemisphere crust is expanding more rapidly than the northern hemisphere crust, and the dispersion of the Gondwanaland blocks is much greater than that of Laurasian blocks (see table IV, p. 49). Hence the motion of the southern hemisphere blocks approximates more closely towards radial separation from the geocentre, with strong surface dispersion, whereas the Laurasian blocks, with much less dispersion, move northwards (Fig. 105).

Wilson and McElhinny (1974) re-examined the global data still more thoroughly and reduced scatter by using only sites based on twenty or more oriented samples. They confirm that the overshoot is real and increases with age (Fig. 72). By calling the longitude of each site zero longitude for the plot they found that the pole overshoot veered to the right as seen by the station (Fig. 72). But here again interhemisphere asymmetry shows up, for the Gondwana sites plot to the right, but the Laurasian sites show no significant divergence (Fig. 73).

Südpolflucht, Nordpoldrang

Wilson (*loc. cit.*) went on to examine the palaeomagnetic inclinations, following Runcorn's method (1959) of plotting mean inclinations against latitude and comparing the curve for an ideal dipole. He reported (p.426) that in every case the inclinations plot to the upper right of the dipole relationship. "One very important fact is that *this inclination is shallower*

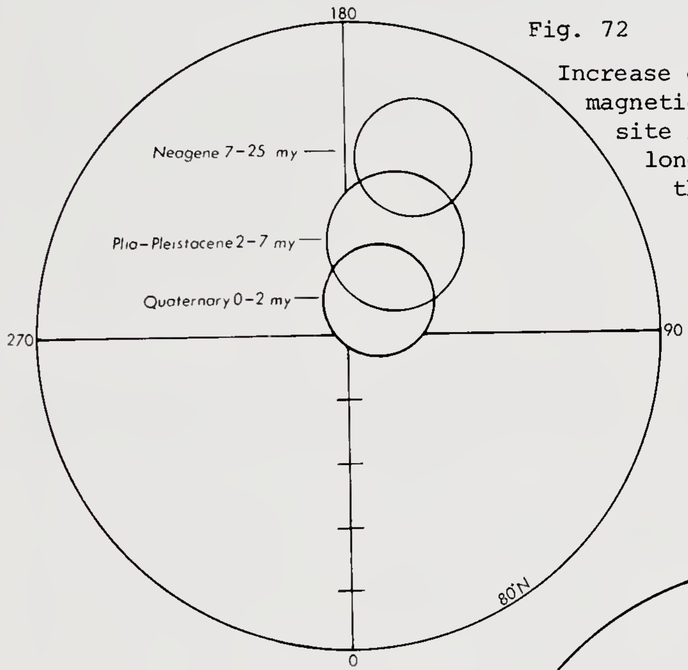


Fig. 72

Increase of overshoot of palaeomagnetic poles with age. Each site is plotted with its longitude as zero longitude, then each group is combined statistically to give combined poles and the 95% confidence circles shown. (After Wilson and McElhinny,1974).

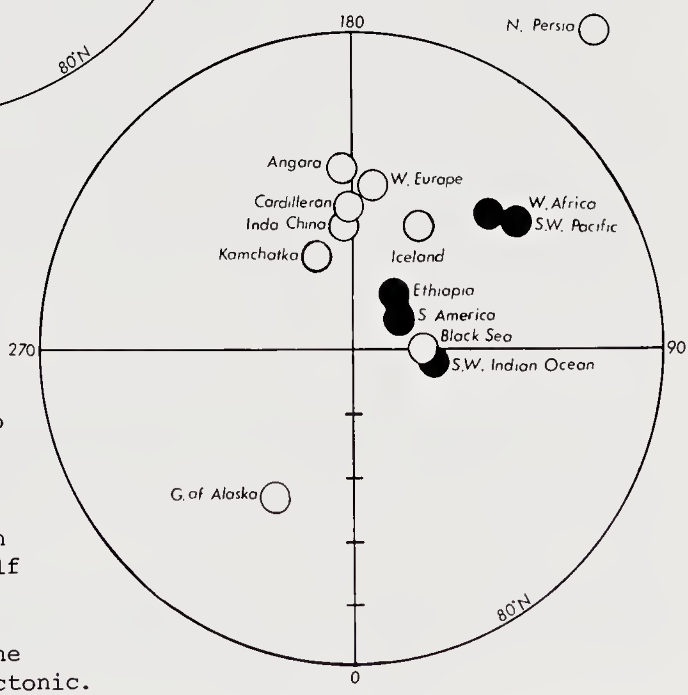


Fig. 73

Regional Neogene mean palaeomagnetic poles, each plotted with the site meridian as zero meridian. Open circles are plotted from Laurasian sites; filled circles are from Gondwana sites. (After Wilson and McElhinny, 1974). The Gulf of Alaska site is aberrent because of strong tectonic rotation. The aberrence of the N. Persia site may also be tectonic.

than the dipole inclination in the Northern Hemisphere, but steeper in the Southern Hemisphere" [italics S.W.C.]. This is precisely what should be predicted if the southern hemisphere has expanded more rapidly than the northern hemisphere, as I have consistently maintained. Wilson shows that the data negate a range of would-be explanations which require the rock to 'know' their hemisphere, and could suggest only two possible explanations: "(a) The field has been a centred axial dipole on time average. Then over especially the past 2 Ma., all continents and sea-floors, sampled over a very complete range of longitudes and latitudes, have had a northwards component of crustal movement amounting to three or four degrees (say 400 km on average). No region has shown a southward movement."
(b) A deviation from the central axial dipole field.

Wilson dismissed the first of these explanations because it is contradictory to the currently fashionable "plate" theory, which requires that the Arctic be opening out, and he therefore concluded that the centred axial dipole must be abandoned with all the consequences thereby implied. However, all the data, far from being contradictory, are precisely what should be expected on the expanding earth, without sacrificing the dipole model which had been confirmed by palaeomagnetic data generally.

The *Nordpoldrang* had been inferred, palaeomagnetically, earlier by Vine (1968), because magnetic anomalies associated with isolated seamounts southwest of Hawaii and east of Japan indicate northward movement of 30° of latitude since mid-Cretaceous with respect to the magnetic pole, and drill core from Midway indicated 15° northward movement since the Oligocene.

Complementary evidence has been given by Hammond and Theyer (1974) who report that "results based on a paleomagnetic investigation of more than 70 sediment cores show that in the western equatorial Pacific relative motion has occurred between the lithosphere and the earth's magnetic field. Assuming the earth's field to be a fixed axial dipole, these results indicate that throughout much of the Cenozoic there has been a continuous northward component of plate motion in the region of the Central Pacific basin, as well as net northward displacement of the area encompassing the Ontong Java Plateau since the Upper Cretaceous. Evidence for plate motion preserved in the cores consists of the following: (1) within cores containing sediments which span several million years recovered from the southern hemisphere, angles of paleomagnetic inclination vary directly with sediment age; inclination angles within analogous cores from the northern hemisphere vary inversely with age; (2) the mean inclination from a core from either hemisphere increasingly diverges from the present field inclination at the coring site as the mean age of the cored sediment increases; (3) hiatuses lasting several million years, where present within cores, abruptly separate sediments characterized by noticeably differing angles of inclination; and (4) several cores recovered north of the equator contain sediments which, based on both paleomagnetic and biostratigraphic evidence, were deposited in the southern hemisphere. In one such core, a paleomagnetic record of the transition between hemispheres is preserved." Hammond and Theyer report an average northward movement rate since the early Miocene of 8 cm/year for the central Pacific basin. The implications of these results differ on an expanding earth from a plate model with fixed radius. In the latter, the Pacific plate must move north relative to the fixed dipole field. In the expanding earth the Pacific block and blocks in general do not move relative to their underlying mantle; but if new ocean floor growth occurs anywhere in the world, the circumference increases and the co-ordinates (and the magnetic field lines) move a little relative to all surface points. As more new crust was inserted in the southern hemisphere than in the northern, all places show a northward movement in both geographic and geomagnetic latitude.

The *Nordpoldrang*, of course, shows up in many other ways, such as the progressive northward migration with antiquity of evaporites (Irving and Briden, 1962), oil fields (Irving and Gaskell, 1962), and the locus of bathyal carbonate sedimentation.

John K. Davidson has drawn my attention to an interesting corollary arising from Wilson's paper. If palaeomagnetics were a very accurate tool and the radius of the earth assumed constant, it would mean that in putting all, say Permian, palaeomagnetic north poles together from each continent, there would be gaps between the continents; conversely using south poles, the continents should overlap. Now taking fig. 2(c) on page 289 of Creer (1970), not only do the southern continents not overlap, they are in fact widely separated -- in accord with the asymmetric expansion implied by the tectonic evidence.

PALAEOMAGNETISM CONFIRMS OROCLINE TECTONICS

Rotations

Palaeomagnetism is sensitive in recording rotations, unless the axis of rotation lay near the relevant palaeopole. In previous papers I have figured many such rotations on tectonic grounds. Sixteen of these previously published rotations have been subsequently tested palaeomagnetically. *In every case* the tectonically predicted rotation has been confirmed (Table I). None predicted tectonically and tested palaeomagnetically has not been confirmed. This clean sweep gives strong support for the validity of the tectonic method. Few of these rotations have been predicted by any other tectonic theory. Kahli (1974) has commented: "In contrast with other sciences, the earth sciences have not been particularly noteworthy for their ability to predict." The exception is the orocline hypothesis.

Van der Voo (1968) has reported a large sinistral deviation of the Mesozoic palaeomagnetic meridians of Anatolia from those of Europe, although the declinations (palaeolatitudes) agree quite well. This confirms my view that the Hellenic orocline, veering the Dinarides into the Taurides, and the Anatolian orocline, veering the Taurides into the Zagros (Carey, 1958a, fig. 31a), are impressed strains which have to be straightened, closing the Black Sea and the reclaimed lowlands north of the Crimea. This would rotate the Turkish palaeomagnetic meridians to agree with the European in both inclination and declination.

Faced with such deflected azimuths, palaeomagnetists have, in some cases, regarded the rocks as in some way anomalous, or tectonically disturbed, or remagnetized by later processes, or as valid but transient magnetizations during a magnetic polar reversal (e.g. Guja and Vincenz, 1971). However, a few palaeomagneticians (e.g. van Hilten, Irving, Ashworth and Nairn, Watkins, and Green and Pitt) have recognised that the rotations found were, in fact, what I had predicted.

Megashears

Palaeomagnetism can confirm large tectonic offsets where the trend of the megashear is along or near the palaeomeridian. The largest offset proposed by me is the Tethyan Shear System (1938, p.62-77; 1958a, p.261-292; 1962, p.369-377, and 1970, p.185-6). However, as this offset was equatorial, palaeomagnetism is not capable of a significant test, although de Boer (1965) has attempted to do this. Anomalous, sinistrally rotated, and displaced palaeomagnetic orientations characterise this zone throughout.

The other first-order offset is the Bengal-Caribbean dextral zone (Fig. 176) which extends in a near-great-circle from the Antarctic, along the Ninety-east Ridge, through eastern Asia to the Rocky Mountain Trench, and the San Andreas rift to the Gulf of Mexico (Carey 1958a, fig. 38b, and

1970, p.185). This dextral offset intersects the sinistral Tethyan Shear zone nearly at right angles, so the two could be conjugate (1962, figs. 2 and 3). Along this belt also, palaeomagnetic rotations and displacements are characteristic, e.g. Irving and Yole (1972), and McElhinny (1973). Each sampling unit must be considered individually according to its tectonic setting. Azimuths from adjacent blocks cannot be safely combined and averaged. However, rotation of a small tectonic unit, which drastically alters the palaeomeridian and the projected pole positions, makes no significant change in the palaeolatitude. Hence in these disturbed zones palaeolatitudes may be averaged (as in Table V) for comparison with blocks on the other side of a megashear zone, irrespective of tectonic rotation. In general, the East Asian blocks tend to show higher palaeolatitudes than the Siberian Shield (implying equatorward shift), and the North American cordilleran region tends to show lower palaeolatitudes than the North American shield (implying poleward shift). Thus, Vancouver Island has been claimed to have translated northwards from the latitude of mid-California (Irving and Yole, 1972), and southeastern Alaska to have been translated some 15° northward from the vicinity of northern California (Jones, Irwin and Overshine, 1972).

Van Hilten (1963), Kropotkin (1962, 1967), Khramov and Sholpo (1967), Hamilton (1970), and McElhinny (1973) have all recognised palaeomagnetic discrepancies between Europe and Siberia across the Urals. Table XI shows that the European block has consistently moved further north than the Siberian block (by 23° since the Permian, 15° since the Triassic, 10° since the Jurassic, and 15° since the Cretaceous). Even allowing the imprecision of palaeomagnetic data, a continuing dextral movement along the Urals from the early Mesozoic onwards is indicated. Taken at their face value, the data suggest an eo-Mesozoic movement of some 8° (Gondwana syndrome), relative quiescence during the Mesozoic, then early Tertiary reactivation of the dextral movement of some 15° (Alpide Syndrome). However this probably reads more reliability into the figures than is justified. Nevertheless the validity of the overall dextral shift is clear. The new seas with ocean floors of the Mediterranean (including the Tyrrhenian, Adriatic, Ionian, and Argaean Seas), and the Black, Caspian and Aral Seas, and the sediment-filled Pannonian and Transylvanian basins, represent the extension zones at the rear of the European block moving northwards with respect to the Siberian block.

John K. Davidson has commented to me that there is excellent stratigraphic evidence to support large dextral motion along a zone from the north tip of Novaya Zemlya to west Mongolia, and for that matter continuing across Mongolia to the Fossa Magna and Korea, which could well be a continuation of the offset along the Urals. All this is consistent with an anticlockwise rotation of the whole Siberian block, (some 40° in diameter) and with similar anticlockwise rotations of the North American block, and the North Pacific block, recalling the octantal motion pattern of figure

ASYMMETRY OF THE EARTH

ASYMMETRY OF FIGURE

The spherical earth

Homer's picture, of a flat earth surrounded by *Oceanus* with *Chaos* beyond, was discarded by the Greek logicians when they contemplated the curve of the horizon, its recession as an observer climbs, and the shape of the earth's eclipsing shadow on the moon. By 540 B.C. Pythagoras, the first on record to affirm that the earth was spherical, had calculated the earth's circumference to be about 74,000 km, which is of the right order although eighty-five per cent. too large. Plato and Aristotle agreed with Pythagoras.

In the third century B.C., Eratosthenes, librarian of Alexandria, with a level of luck which does not always grace good reasoning, got within 15 per cent. of the correct circumference (or closer than one per cent. if an alternate meaning of a stadion is adopted). He had observed that the sun fully illuminated the bottom of a vertical well at Syrene (Aswan), at noon on the day of the summer solstice, whereas at Alexandria the sun was one-fiftieth of a circle from the zenith on that date. Camels, whose daily stage was one hundred stadia, took fifty days to go between these cities, so the circumference of the earth must be a quarter of a million stadia (or 46,250 km in modern measure).

The ellipsoidal earth

For two millennia the earth remained a sphere, at least to the wise, although flat-earth cults persist to this enlightened day! In 1669 Jean Picard measured by baseline and triangulation the distance from Amiens to Malvoisine, and the first of the Cassinis extended this arc northward to Dunkirk and south to the Pyrenees. The length of a degree north of Paris was found to be 267 m shorter than one south of Paris. This meant to Cassini that the earth must be prolate (Fig. 74G). But Jean Richer, a Paris clockmaker who had been commissioned to build a clock for an observatory in French Guiana, found in 1672 that his pendulum clock, which kept correct time in Paris, lost 2½ minutes a day in Cayenne, and was correct when returned to Paris. This suggested an oblate earth (Fig. 74H).

Newton (1687), prompted by Cassini's report, deduced that centrifugal force, in opposition to his gravitation, should make the earth oblate (Fig. 74H), and he argued that otherwise the ocean waters would run to the equator leaving all high latitudes bare. On the simple assumption of uniform density, the polar flattening (difference of polar and equatorial diameters divided by equatorial diameter) should be 1/230. Empirically the flattening of the real earth is 1/298.

Thus sparked off a controversy, destined to rage for half a century, between the French empirical egg and the English theoretical orange, a debate not unmixed with filial undertones as Cassini son followed Cassini father for four generations of directors of the Paris observatory. Finally, in 1735, to settle the matter, the French Academy of Science sent an expedition to Peru (now Ecuador) under Bouguer, La Condamine and Godin, and another to Lapland in 1736, under Maupertuis, to measure and compare equatorial and Arctic arcs. The degree in Peru proved to be 900 m shorter than the degree in Lapland and implied polar flattening between 1/179 and 1/266. Hence Voltaire's famous quip to La Condamine:

> *'Vous avez trouvé par de long ennuis*
> *Ce que Newton trouva sans sortir de chez lui.'*
>
> *You have found at great labour and care*
> *What Newton found without leaving his chair.*

The oblate ellipsoidal figure, as an equilibrium between centripetal gravitation and centrifugal rotational force, was rigorously computed by Clairaut (1743).

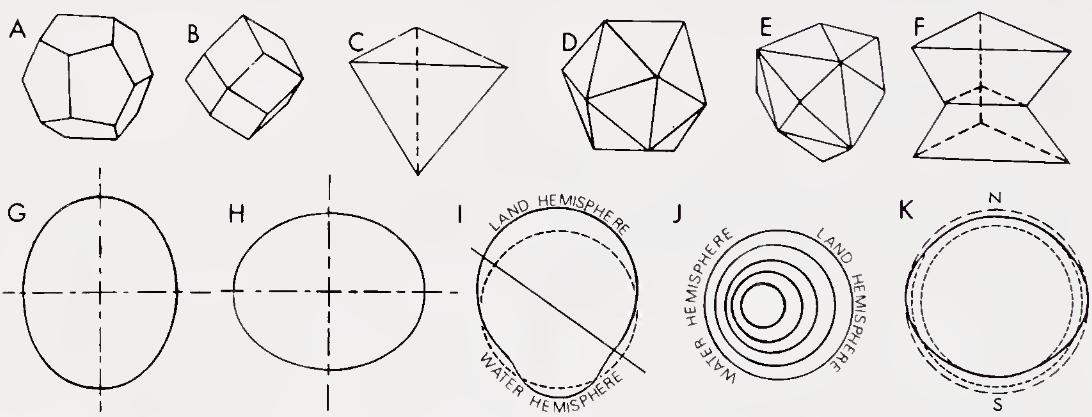


Fig. 74 Figures on which earth models have been based. A: Pentagonal dodecahedron (Elie de Beaumont, Spilhaus). B: Rhombic dodecahedron (Owen). C: Tetrahedron (Owen, Lowthian Green, Gregory, Woolnough, Spilhaus). D: Icosahedron (Havemann). E: Hexatetrahedron (Lowthian Green, Gregory). F: Twinned tetrahedron (Hobbs). G: Prolate ellipsoid (Cassini). H: Oblate ellipsoid (Newton, Huygens, Clairaut). I: Oblique pear (Jeans, Sollas). J: Eccentric spheres (Love). K: Hemimorphic spheroid (La Caille, Lapworth, O'Keefe).

The pear-shaped earth

Despite a few awkward observations, the earth was thought to be symmetrical until relatively recently. It was natural to assume that 'equator' means what it said - that north was equal to south. The first suggestion to the contrary came from La Caille, who reported in 1751 that the southern hemisphere was squatter than the northern. He measured a meridian degree at the Cape of Good Hope and found that not only was it longer than an equatorial degree, as required by the rotational flattening, but also that it was longer than a degree in equivalent northern latitudes, a result which was confirmed by Maclear.

Steinhauser published a map late last century (reproduced in Gregory, 1899) showing the number of millimetres by which a pendulum which beats seconds at the equator must be lengthened to beat seconds in other latitudes. In both hemispheres the pendulum must lengthen with increasing latitude (indicating increasing gravity and flatter figure polewards), but the rate of increase is greater in the northern hemisphere. On 50°S between 2 and 3 mm must be added, whereas on 50°N between 3 and 4 mm must be added, according to Steinhauser's map. This suggests more flattening in the northern than in the southern hemisphere.

Round the turn of this century, Lapworth (1892, in Sollas, 1903), Jeans (1902), and Sollas (1903), all considered that the earth was pear-shaped (Fig. 74I). Lapworth and Sollas worked empirically from morphological features; Jeans deduced that gravitational instability could produce an asymmetric body which might subsequently collapse towards a spherical shape, although relics of it might still be discernible. The three versions of the pear-shaped earth did not agree on what was the long axis of the pear nor which was the big end. Lapworth's axis coincided with the present axis, with the small end in Antarctica. Jeans and Sollas chose axes oblique to the present axis, but Sollas had the small end in Africa and the big end in the central Pacific, whereas Jeans placed the large end in the neighbourhood of England and the small end in Australia, with the Pacific, South Atlantic and Indian Oceans making the "waist" of the pear.

Jeans retracted his pear-shaped earth in 1916 because he found his physical assumptions untenable. Notions of a north-south asymmetry were then abandoned, although several geodesists (Helmert, 1915; Heiskanen, 1924 and 1928; Niskanen, 1945; and Uotila, 1956) found evidence of an ellipticity in the equator, the gravity anomalies being strongly negative in India and to a less degree in North America, whereas in Europe, the eastern Atlantic and much of the Pacific they were systematically positive. This triaxial ellipsoid was expressed (in terms of its surface gravity) by the general formula:

$$\gamma = C_1 + C_2 \sin^2\phi - C_3 \sin^2 2\phi + C_4 \cos^2\phi \sin^2 (\lambda + C_5)$$

in which γ is the value of gravity at latitude ϕ and longitude λ, C_1 is the gravity minimum at the elliptical equator, C_2 is the centrifugal acceleration at the equator, C_3 expresses a mid-latitudinal waist caused by the shelled distribution of density within the earth, C_4 is the gravity difference between the major and minor axes of the elliptical equator, and C_5 is the longitude at which the gravity maximum occurs. Later measurements did not support this figure, and Heiskanen and Vening Meinesz (1958) argued in favour of universal isostasy on an ellipsoid of revolution. By the time of the first sputnik, western geodesists had abandoned the triaxial figure, but leading Russian geodesists still stuck staunchly to a lopsided world.

In retrospect, geodesists defaulted in ignoring the direct evidence of asymmetry reported by La Caille, Maclear, and Steinhauser, and in taking for granted instead the equatorial symmetry implicit in even the triaxial figure of the last paragraph. But to venture from paths already fenced by the orthodox establishment has never been respectable. When the figure of the earth was expressed as a series of spherical harmonics, each bringing the approximation closer to the geoid, the third and fifth harmonics were intuitively assumed to be zero, because the effect of them would have been to produce an asymmetric earth. Geologists, faced with the gross asymmetry of the earth's morphology, of the late Palaeozoic glaciations, and of the land and water hemispheres, should also have demurred about this assumption of equatorial symmetry.

The sputniks finally proved the pear-shaped asymmetry of the geoid (O'Keefe, Eckels and Squires, 1959). Broadly, a satellite's orbit is an ellipse with the earth at one focus. There are many perturbations of this motion. One (caused by the attraction of the earth's equatorial bulge) is that the long axis of the ellipse slowly rotates so that the position of the perigee creeps round the earth in the plane of the orbit; a second effect (due to the atmospheric drag) is that the orbit becomes progressively smaller and less elliptical. Statistically this decrease of ellipticity is faster when the perigee is in the southern hemisphere than when it is in the northern, which can only mean that the earth's gravity differs between the north and south, that is, one hemisphere is more flattened than the other. Expressed quantitatively, the geoid is fifteen metres higher at the north pole than a mean symmetrical formula would indicate, seven metres lower in the mid-northern latitudes, correct on the equator, seven metres too high in the southern mid-latitudes, and fifteen metres too low at the south pole. In other words the earth is slightly hemimorphic (Fig. 74K), but the other way up from what Jeans and others had imagined (Fig. 74I).

Although the term pear-shaped was used by O'Keefe, Eckels and Squires (1959) and by others to describe this figure, this is not a good analogy, because pears are prolate (as was Jeans's pear, Fig. 74I) whereas the earth is an oblate hemimorphic spheroid. If we must pick fruit, let Mother Earth be a peach! And let even the peach be galled. For although satellites have proved hemimorphism, they are insensitive to longitudinal or quadrantal irregularities, inherited from the history of the earth's expansion.

The tetrahedral earth

Elie de Beaumont, one of the most fertile and original thinkers in the history of geology, proposed in 1829 that the shrinkage of the earth's crust tended towards a pentagonal dodecahedron (Fig. 74A), and that the edges of this figure determined the main mountain grain and gross lineaments of the earth's surface. A spherical crust of fixed surface area would have to buckle up into wrinkles if the volume within it contracted. De Beaumont argued that the shrinkage figure should be regular, with many planes of symmetry. De Beaumont saw in the pattern of relief of the earth's surface the elements of a dodecahedral framework, the faces corresponding to seas or basins, and the edges to ridges. Richard Owen (1888) substituted a rhombic dodecahedron (Fig. 74B) for the pentagonal dodecahedron, claiming that this gave a better match with the face of the earth. C.R. Keyes (1901) also used a rhombic dodecahedron but with a different orientation. Havemann

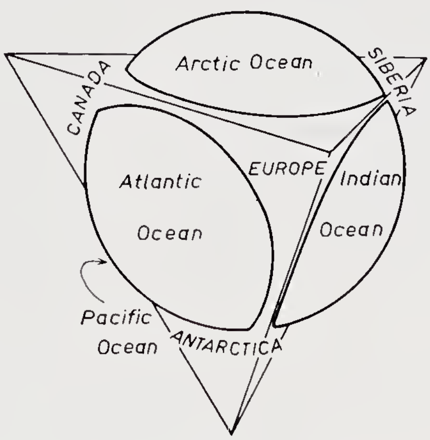


Fig. 75

The tetrahedral earth showing sphere and tetrahedron of same volume.

(1952) claimed that differential motion of core and mantle produced strains in the mantle and lithosphere forming a global icosahedral pattern (Fig. 74D), to which he tied the Mediterranean-Alpide ring of structures which were formerly equatorial, the origin of the Atlantic and Indian Oceans, and the crustal massif of central Asia. The weakness of these models is that in the absence of some rigorous statistical control of significance it is difficult *not* to find matching sympathetic features in most places where they are wanted.

All these models were equatorially symmetrical. Meanwhile a proposal that the tetrahedron should replace the dodecahedron introduced axial asymmetry so that north could no longer equal south (Fig. 74C). This tetrahedral idea originated with Richard Owen (1857), but it was William Lowthian Green (1875, 1887) who developed it (Fig. 75). He pointed out that a sphere has the minimum surface for a given volume, but of all regular figures the tetrahedron has the maximum surface for a given volume. Hence if the crust of the earth solidified and thereby froze its surface area, further shrinkage of the interior could be accommodated with least work if the shell collapsed towards a tetrahedral form. This argument overlooks work done against gravity in a large body like the earth where self-gravitation dominates.

Michel Levy, Marcel Bertrand (1887, in Gregory, 1899), and J.W. Gregory (1899) developed the tetrahedral theory, but the four versions differed in respect to the location of the axes. Emerson (1900), Hobbs (1921) and

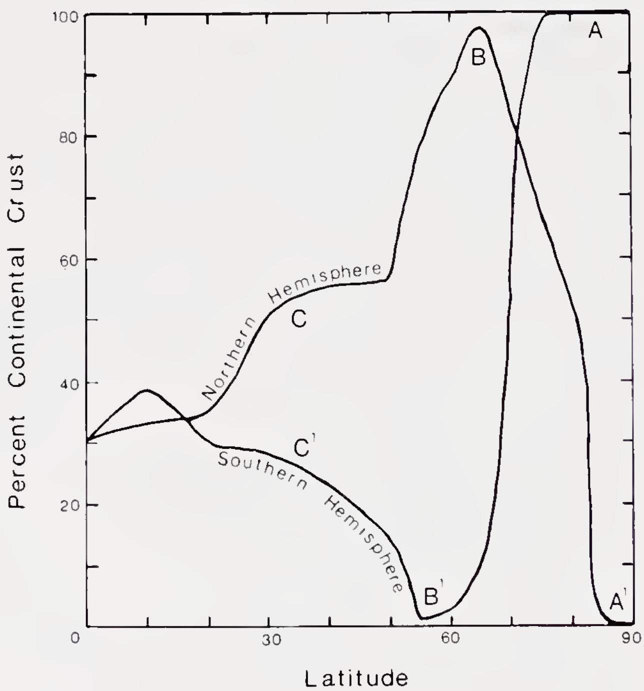


Fig. 76

Percentage of continental crust plotted against latitude. Seas shallower than 2000 m are plotted as continental.
AA': South polar continent, north polar ocean.
BB': Northern land girdle, southern ocean girdle.
CC': Lands taper south, oceans taper north.

Woolnough (1946) strongly supported Gregory's version. Woolnough's presentation is particularly clear. Briefly the evidence for a tetrahedral earth is:

(1) The face of the earth presents a north polar ocean surrounded by a continuous belt of northern lands from which protrude three meridional land rays which taper southward.

(2) A south polar continent is surrounded by a continuous belt of southern ocean from which protrude three meridional ocean rays which taper northward. The belts of land and sea are like a pair of cog-wheels with interlocking teeth.

(3) The equator is not a plane of symmetry but of antithesis (Fig. 76).

(4) Lands and seas are more or less antipodal (Fig. 77): Antarctica is antipodal to the Arctic Ocean; the rather small continent of Australia is antipodal to the rather small north Atlantic Ocean; North America is antipodal to the Indian Ocean. Europe and Africa are more or less antipodal to the Pacific, and with a little fudging, the South Atlantic would be antipodal to East Asia. The antipodal antithesis relates to the centre of the earth, not its axis, and hence on its own, it is independent of the position of the poles, although, taken with the inverse correlation with latitude, the pattern has the symmetry of an axial tetrahedron.

Gregory did not claim the antipodal relation to be perfect, but, as 95 per cent. of land is antipodal to sea, it is a lot better than random, and it is not difficult to rationalize the residual discrepancies with plausible explanations. For example, Woolnough (1946) explained the departures from a completely regular development of the shrinkage tetrahedron by adopting Darwin's resonance hypothesis for the birth of the moon from the earth, leaving the Pacific Ocean as the imperfectly healed scar. The earth never recovered from this event, and Woolnough sees in this deformity the reason for the departures from strict antipodal regularity. Thus also would he explain the facts that the northern land girdle has its maximum at 65°N (30° for a regular tetrahedron) and that the longitude interval between rays is 90°, 115° and 155° (120° for regular tetrahedron).

(5) If a tetrahedron of equal volume is centred within the earth and suitably oriented, one apex would emerge in the Antarctic shield and one each in the Angara, Canadian and Baltic shields. The flat faces of such a tetrahedron would correspond to the Arctic, Pacific, Indian and Atlantic Oceans, and the edges would correspond reasonably well with continental masses. In Gregory's model, the earth is like a tricorn peg-top (Fig. 75).

(6) Assuming a shrinkage towards a tetrahedral form, the rising

apices, and to a less extent the ridges, would emerge as land and be attacked by erosion; they would tend to be continuously rejuvenated as the shrinking continued through the aeons; they would be just as continuously reduced - reduced but not subdued; they would become profoundly eroded quoins - shields of Archaean rocks containing eroded granite which rose in their fractured axes during the early stages of the shrinkage before they became thick corns immune from further injection - shields, rarely if ever overrun by the sea during their long history - nuclei around which the wastage of the ages accumulated and grew as successively plastered prisms of continental growth. The permanence of these tetrahedral quoins was matched by the permanence of the ocean basins which occupied the antipodal tetrahedral faces.

Spilhaus (1973), after viewing the earth through variously oriented and constrained projections, found most virtue in a dodecahedron, with the central pentagonal face representing the African polygon (Fig. 6), with apices at the triple junctions of the spreading ridges, whence five other peripheral polygons complete the hemisphere. (Eurasia and Indo-Australasia are regarded as single polygons). Spilhaus added that three - tetrahedron, cube, and dodecahedron - of the five regular solids around which Plato built his cosmology have triple points, that the four triple points of the tetrahedron can be superimposed on four of the eight triple points of the cube, and that all eight triple points of the cube can be superim-

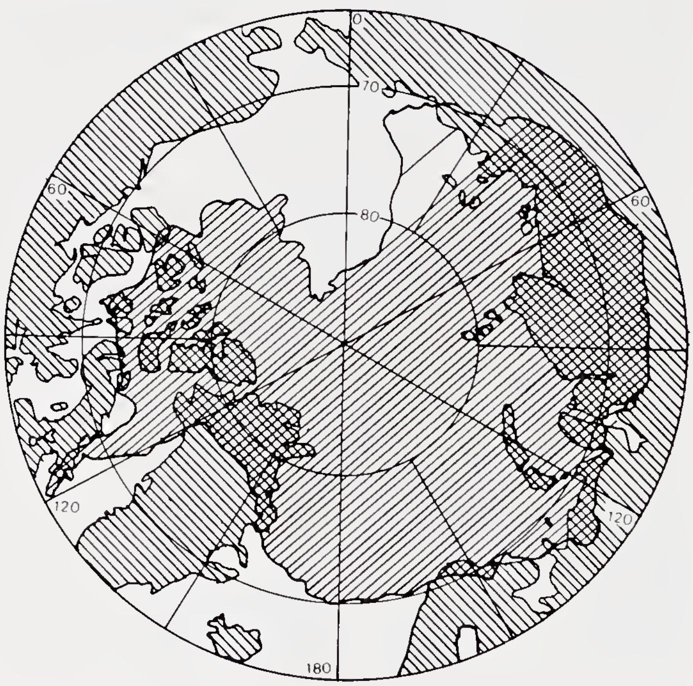


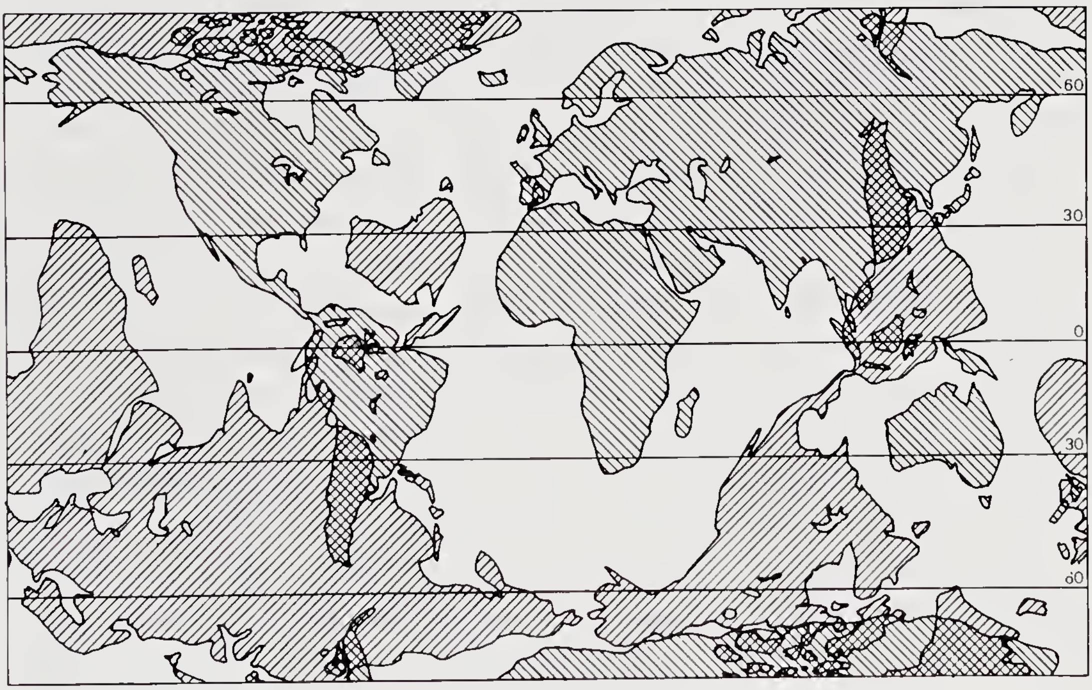
Fig. 77 Antipodal relations of lands and seas.
Right - polar azimuthal projections.
Opposite - Mercator projection.

posed on eight of the twenty triple points of the dodecahedron. Spilhaus speculated that the tetrahedron, cube, and dodecahedron may all find expression in the primary morphology of the earth, with four most significant triple points corresponding to the apices of the tetrahedron shared also by the cube and dodecahedron; the four next most significant triple points would correspond to the other four apices of the cube, which are also shared by the dodecahedron; and a further twelve significant points would correspond to the remaining twelve apices of the dodecahedron. Spilhaus asks: "Did a simple tetrahedral framework progress through a cubical, then to a dodecahedral framework, to an icosahedral framework today? If so, we should expect that the four triple points common to these three solids might have special significance and permanence."

Discussion of the Tetrahedral Model

(1) The tetrahedral theory has not been generally accepted. Yet it is commonly mentioned in most accounts of the morphology of the earth. It has indeed something more than historic interest. The tetrahedral theory may not give a true model of the earth, but it does vividly express a truth about the earth, which is absent from most contemporary earth-models, mathematical or mental: the earth is not symmetrical; it is axially hemimorphic, and radially tends to be tri-lobar.

(2) The tetrahedral model must not be pressed too far. There are other ancient shields which co-rank with the Canadian, Fennoscandian, Angara



and Antarctic shields, the chosen tetrahedral apical quoins. The African, Australian, and Brazilian shields are comparable, and there are other smaller but significant cratons. Hobbs (1921) met this problem by postulating tetrahedral shrinkage on Gregory's axes for the Archaean revolution, during which the Baltic, Angara, Laurentic and Antarctic shields were dominant, and a twinned reversed tetrahedron during the Palaeozoic revolution which gave greater prominence to the shields of Africa, Australia and Brazil which coupled with the earlier quoins, resulted in the medial sea of the Tethys. The Cainozoic movements rejuvenated the ancient shields. This model (Fig. 74F), which gets nearer to that of de Beaumont, still falls short. The shields of Brazil, Africa and Australia are as ancient as those of Canada, Angara and the Baltic. Indeed, according to Jacobs, Russell, and Wilson (1959, figure 15-8), the known really ancient nuclei are the Keewatin and Yellowknife provinces in Canada, Sierra Leone and the Tanganyika-Natal belt in Africa, the Guayana shield in Venezuela, the south-western Australian block, the Dharwar massif in peninsular India, and the Ukraine nucleus. This list, although admittedly incomplete, does not correlate well even in its positive aspects with either Gregory's or Hobbs' models.

(3) The argument, that a uniform spherical crust with a fixed surface area covering a shrinking interior would collapse towards a polyhedral geometrical figure, is valid, but only so long as the implied isostatic departure is less than strength, and duration is not long in relation to relaxation time. The number of faces in such a figure is determined primarily by the scale of size. A tetrahedron has the least possible number of faces and the greatest efficiency in accommodating excess surface to diminishing volume. The pentagonal dodecahedron has three times as many faces, has much greater symmetry and involves much less departure from gravity isopotentials. For a given material model, a size range exists where the contraction would take this form. In a pea, a prune, or a shrivelling apple, the internal volume shrinkage is accommodated by a large number of quite small figures usually with three sides meeting at a point. For any material model, a size range exists where this would be the pattern of collapse. The number of faces developed is a buckling phenomenon determined by elastic instability. Axial loading of a plate in its own plane shortens and thickens it, but if the load increases it becomes unstable elastically and buckles to a bowed form. The threshold of instability is determined by its elastic parameters and the ratio of length to thickness. In a plate of very large area compared to its thickness more than one flexure may develop between the edges, the number of buckles depending on the

relative dimensions of the model. On this basis the tetrahedral earth model is quite improbable. Bull's shrinking membrane (1932) is a nearer analogue. The threshold between buckling by flexure or by fracture is also determined by scale, in this case relative magnitudes of time-constants in rate of contraction and rate of relaxation of elastic stress by flow. Many contraction experiments have been made using a variety of materials. De Chancourtois (1878) coated a greased rubber balloon with wax. Daubree (1879) coated balloons with paint, wax, gum arabic, and gelatin. Avebury (1903) used interlayered carpet-baize and sand, Toula (1914) published photographs of his shrinkage experiments with coated rubber balloons, Quirke (1926) coated a rubber ball with wax and compressed it in water, and Bull (1932) stretched a rubber sheet over the top of a cylinder, coated it with various materials such as different waxes, collodion, gelatin and papier-Josef, each with a suitable non-sticking undercoat, and allowed the rubber to contract. All these experiments produced shrinkage patterns, usually of polygons with ridges tending to be concurrent in threes. Bucher (1924, in Bucher, 1933) carried out careful experiments of failure both in tension and in compression of thin glass balls, thin wax balls, and wax coated on sponge rubber balls. He produced a number of interesting patterns, some of them asymmetric. All these patterns are real for the model, but only for a particular range of scale, and none of them is necessarily at all close to the pattern the earth would adopt.

(4) The tetrahedral model is based on the present poles. A tetrahedral axis emerges through the centre of the Arctic Ocean and the Antarctic continent. This configuration of the poles is young. Palaeomagnetic data from all continents agree that prior to the Cainozoic the poles diverge further and further from these locations. Whatever truths are represented by the tetrahedral model, they, too, must be essentially Cainozoic.

(5) The tetrahedral model of Gregory, Hobbs, and Woolnough has the big end in the northern hemisphere. The 'pear-shape', now firmly established by geodetic analysis of satellite orbits, has its big end in the south. As so often happens in mechanics, reversal of plus and minus may give the same general result, perhaps in mirror image. Many problems of stress-fields and fracture behave thus. Gregory assumed an oversized earth-crust shrinking from a spheroid to a tetrahedroid shape. A similar result could follow from the stretching of a too-small brittle crust by an expanding interior. The greatest relief with least work should come from a tetrahedroid fracture pattern, and on a rotating earth this should have axial symmetry. This leads to the next point.

(6) The mid-oceanic rift system, also conforms to the tetrahedral pattern. The oceanic crack system rings the Antarctic at about the fiftieth parallel, antipodal to the northern hemisphere girdle of lands. This ring has three projections from which spring the Atlantic, Indian and Pacific crack systems, repeating the tetrahedral pattern, but this time in tension.

(7) The rate of growth of new crust, and rate of dispersion of the continental polygons (Table IV) are hemihedral, greatest in the south, least in the north. As there are three widening wedges in each hemisphere, the pattern is tetrahedral. The *Südpolflucht-und-Nordpoldrang* march of all the continents (p. 217) also reflects the north-south asymmetry.

The unique Pacific

Thus far we have considered only asymmetry about the equator. There are also marked asymmetries about meridians. The most outstanding is the Pacific Ocean - a structure quite unique on the face of the globe. Osmund Fisher in 1881 referred to 'the remarkable circumstance that one hemisphere of the globe is almost entirely covered by an ocean, the Pacific, while the other consists chiefly of land'.

Uniqueness may justify a unique cause. George Darwin, and many who have followed him, regarded the Pacific as the scar left by the departing moon. A modern fashion is the directly contrary proposition - an astrobleme, or impact scar, of a moon-sized asteroid. This proposition in turn has still another contrary - that it was the non-Pacific which was the impact site of a sialic asteroid one-third the size of the moon (Howell, 1959, pp. 276-278) or perhaps of a second smaller moon driven to fall into the earth by an inherent instability of the three-body system. The added mass could have provided the original Pangaea.

Richard Owen (1857), in the paper which spawned the tetrahedral theory, first conceived the birth of the moon from the earth and the origin of the Atlantic Ocean as a rift - truly a stimulating trinity! Sir George Darwin (1879) developed a physical theory to explain how the sun's tide in the earth resonated with the natural elastic vibration period of the whole earth, how this led to increasing triaxiality, until the distended equatorial ellipse developed a constriction, and finally, how a smaller body, the moon, detached itself from the equator. Osmund Fisher (1881) gave provisional support to Darwin's hypothesis but mentioned the difficulty of the new-born moon escaping intact from the immediate vicinity of the earth.

The lunar scar theory of the Pacific Ocean was also adopted by the astronomer W.H. Pickering (1907a, b).

A.E. Love (1907) in his Presidential Address to the British Association gave the Pacific a unique origin of a different kind. Love claimed that a homogeneous sphere of the same size and mass as the earth, made of material as nearly incompressible as granite, could not exist; it would be gravitationally unstable. The external form would be spherical but the centre of gravity would be eccentric (Fig. 74J). The ocean waters would take a spherical shape with the earth's centre of gravity as its centre. Thus, according to Love, the force which keeps the Pacific Ocean on one side of the earth is gravity, and there is no need to account for it as a scar left by the moon. Gravitational instability accounts for the existence of the Pacific Ocean. However, Love's theory is founded on unreal assumptions.

Kropotkin and Shakhvarstova (1965) recognized that the locus of maximum neotectonic activity passes near the great circle drawn through the poles along 120°E and 60°W. This zone essentially separates the Pacific from the non-Pacific. It is a zone of dextral rotation. The zonal agreement is even closer if the great-circle is related, not to the present poles but to the Mesozoic poles and the Mesozoic equator along the Tethyan torsion zone. These two pan-global shear zones appear to be conjugate, one sinistral and one dextral.

Woolnough (1946) again emphasized the uniqueness of the Pacific Ocean: "The most perfunctory inspection of the map of the earth, on any kind of projection, reveals the fact that the Pacific Ocean basin is much the most extensive individual unit of terrestrial structure. Its colossal area, the comparatively simple contours of its boundaries and the absence from it of any considerable land masses all require investigation and explanation. Its dominating importance as a terrestrial unit becomes ever more and more apparent as the nature and structure of its islands are examined, the individual characteristics of its shore lines are studied, and its relation to continental drift is made apparent . . . Whatever the origin of the depression, the objective fact is that the Pacific Ocean is an anomalous earth unit. If it is of very ancient origin it must have imposed an initial asymmetry on all subsequent development."

The Pacific should not be considered without its counterpart, the non-Pacific. Examined at present, the contrast is profound. Pacific coasts are concordant, the tectonic grain runs parallel to them. Non-Pacific coasts are discordant; the coasts cut across the fold grain - from the oldest Precambrian to the youngest Cainozoic. In places the grain is para-

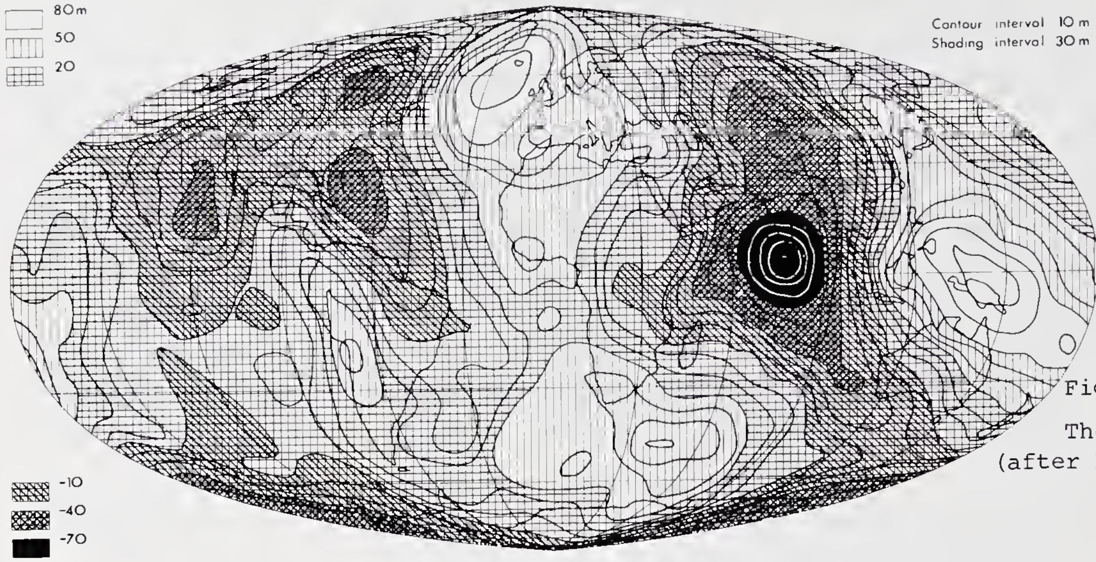


Fig. 78

The geoid.
(after Rapp 1974

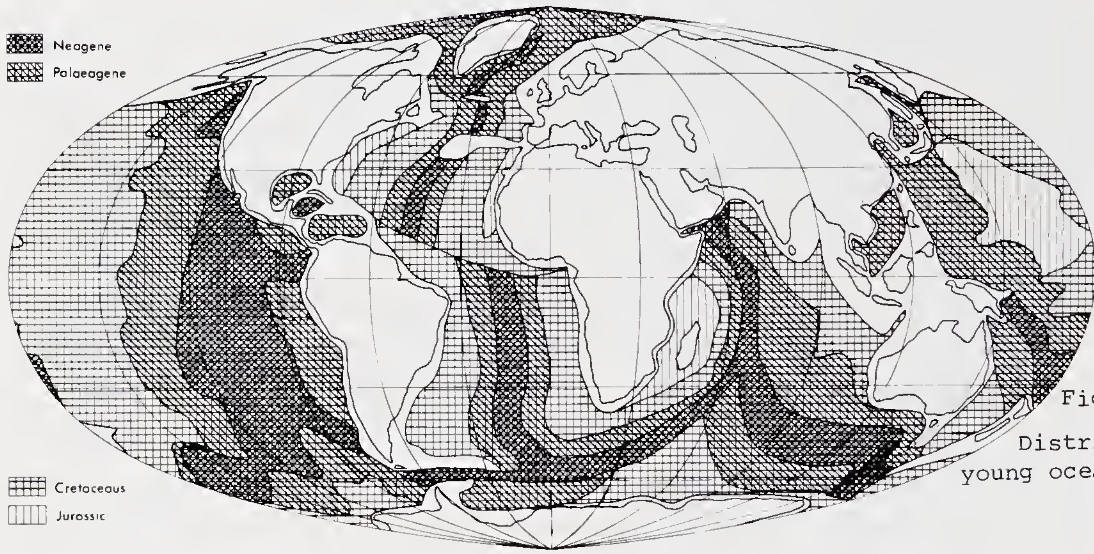


Fig. 79

Distribution c
young oceanic crus

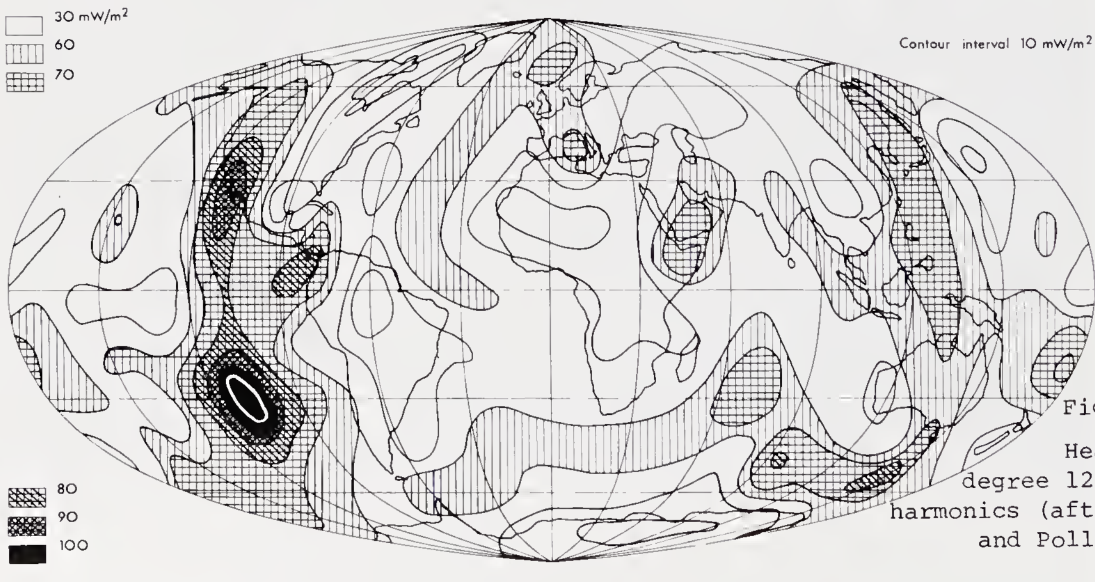


Fig. 80

Heat flux t
degree 12 spherica
harmonics (after Chapma
and Pollack, 197

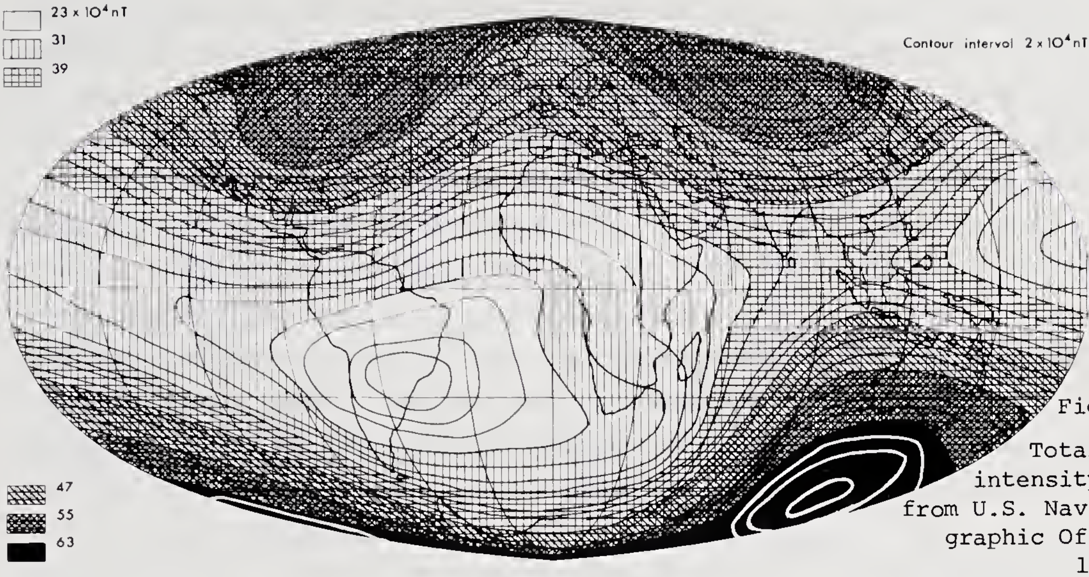


Fig. 81

Total magnetic intensity (plotted from U.S. Naval Oceanographic Office chart 1703, 1964)

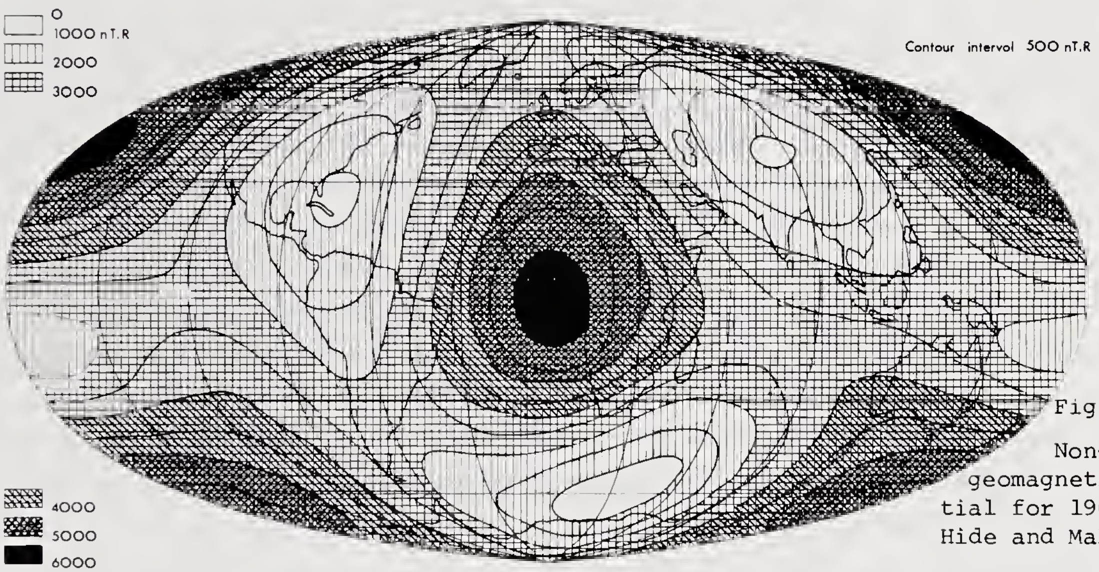


Fig. 82

Non-dipole geomagnetic potential for 1965 (after Hide and Malin 1971)

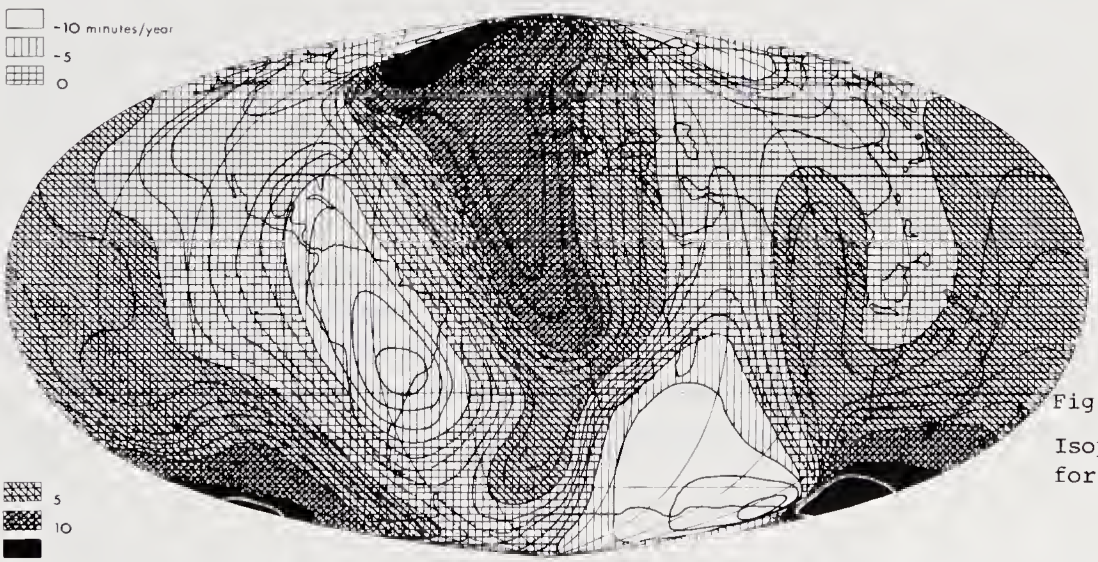


Fig. 83

Isopors for 1965

llel, but not more than would be parallel from random chance. This, taken with the associated mid-oceanic rifts, points to the dispersion of the non-Pacific from an original Pangaea, whereupon the primary asymmetry becomes clear, a *Urozean* and a great *Urkontinent* sharing the face of the earth. Although at first sight we find here a primary opposed pair, there is another element to the symmetry, an element which recurs - one of the opposed pair is itself double. There is only one Pacific Ocean, but Pangaea is double - Laurasia and Gondwanaland; Du Toit and Lester King would have them separate from the start.

The Pacific is also asymmetric within itself. The east-facing coasts all the way from the Aleutians to Antarctica show island arcs widely separated from their continents by disjunctive seas. Along the west-facing coasts of the Pacific Ocean, again all the way from Alaska to Antarctica, the orogenic belts are welded to their parent continents. There are small disjunctive seas (Gulf of California) but these are narrow and exceptional and do not match the broad Asiatic or Tasman Seas. Along the Cordillera the succession of orogenic belts from eoCambrian to Quaternary weave along on top of each other. Along the western Pacific the orogens step westwards with age, the youngest through the Marianas, Solomons and Samoa, the oldest far back in the cratons of Asia and Australia (Fig. 40).

So we are left with a pronounced east-west asymmetry of the Pacific. As east and west only come into being with the earth's rotation, this asymmetry springs ultimately from control by the earth's sense of rotation.

GEOPHYSICAL ASYMMETRY

Gravity

Gravity, expressed as the algebraic sum of the attraction to the earth's mass and the centrifugal acceleration due to the earth's rotation, dominates all other forces, and hence determines the figure of the earth (the geoid) which has already been discussed. Ten-metre contours on the surface of the geoid are shown in figure 78 from Rapp (1974). These contours represent departures of normal sea-level from the mean figure. First-order asymmetry is immediately apparent. There are four main highs and four main lows, roughly octants. The deepest low (near Ceylon) is adjacent to the highest high (in Melanesia). The geoidal swells and troughs are not circular, but are sinistrally sigmoidal with a northwest-southeast skew, the southern hemisphere axis being some tens of degrees east of the northern. The obliquity to the meridian is greater in the south than the north. This interhemisphere asymmetry recalls the Tethyan torsion. Indeed it may be more than coincidence that the largest anomalies (Melanesian high, Cingalese low, North Atlantic high, and Bahamas high) all fall close to the Tethyan shear zone.

The regional asymmetries of the geoid must be due to density differences in the upper half of the mantle because

(a) they show no correlation with the distribution of contents and oceans, nor with the Cainozoic behaviour of the lithosphere. For example, the Indian and Atlantic Oceans are both young rift oceans derived from the disruption of Pangaea, yet the former bears the deepest low, whereas the Atlantic is consistently high.

(b) the density differences cannot be shallow, in view of the broad regional gradient.

(c) they cannot be as deep as the core, or even the lower mantle, because the implied density variations at such depths would be unacceptably large (Bott, 1970; Higbie and Stacey, 1970).

The density differences are not likely to represent differences of composition because the consequent stress-differences should dissipate in times geologically short. This would seem to imply that the asymmetries are actively regenerated and maintained. This could be mechanically, by the vertical component of the viscous drag of a convection circulation, or thermally by the transformation to less dense phases owing to raised isotherms. Considered as convection cells, the highs are not where tectonicists would expect up-currents, and the lows are not where down-currents would be expected. The correlation with the global distribution of heat flux (Fig. 80) is reasonably good, and could well improve, because the heat flux compilation is still at a primitive stage. Outgassing on an expanding earth differs from the convection model in that rising diapiric zones may or may not be accompanied by a returning sinking zone. All active motion could be radially up.

Heat flux

The global pattern of variation of heat flux has been difficult to integrate and even now is far from an adequate synthesis.

Land areas as a whole have been investigated by a different technique from the seas, and there could well be a significant difference between the meanings of the two sets of measurements. Heat-flow measurements on land are made in boreholes, in rocks which have for the most part been substantially consolidated. By contrast, heat-flow measurements at sea have been made by driving a core barrel into soft sediments, the youngest sediments at the top of the current sedimentation column. All such sediments are in process of compaction, dewatering and diagenesis. The large molecules of included organic matter are cracking towards ultimately methane, carbon-dioxide, water, and carbon. Fully hydrated clay sols are transformed to gels, then recrystallise, with syneresis, desorbing metallic cations. These processes are at least in part exothermic. The heat flow being measured is of the order of one calorie per sq. cm. per year. A column of sediments 4 km deep has about one tonne of sediment below each sq. cm. I would be surprised if the exothermic processes in one tonne of sediments undergoing diagenesis could not yield a calorie per year.

Apart from such fundamental uncertainties, the heat flow data are still inadequate (vast areas have no observations) and are highly variable even in adjacent measurements. Chapman and Pollack (1975) have progressed towards meeting this problem by calculating a mean heat flow value to each 5°x5° grid area, where the data were sufficient to permit this, and then extending

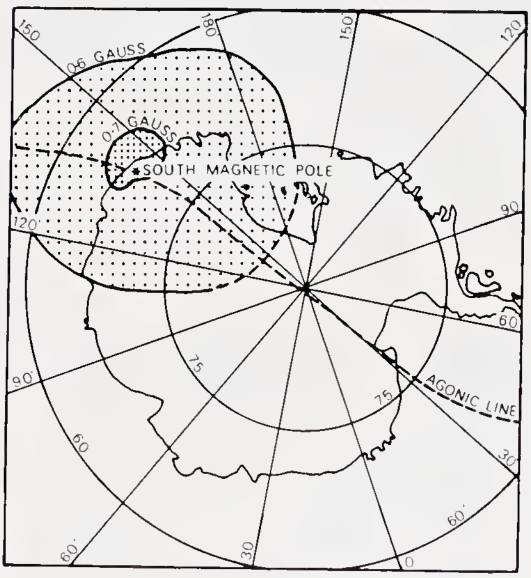


Fig. 84

Magnetic field near the South pole. Stippled areas are zones of maximum field strength.

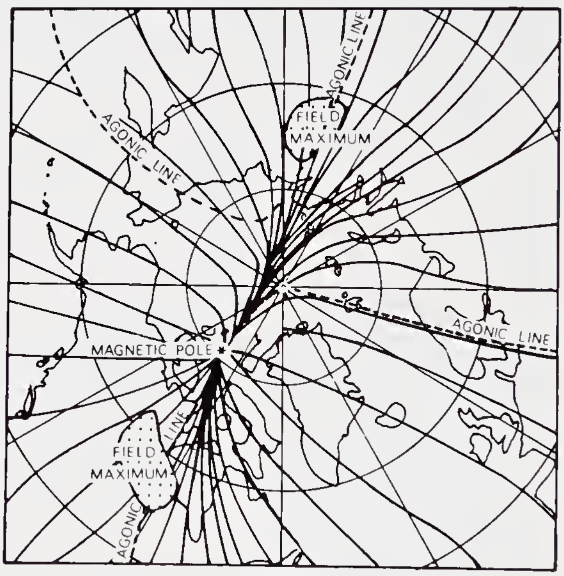


Fig. 85

Magnetic field near the North pole (after Hope, 1959). Heavy lines are magnetic meridian.

these data into the blank areas by assigning to each blank square a value predicted from those areas considered to be tectonically comparable. These grid values were then subjected to global harmonic analysis. Figure 81 shows the resulting degree 12 spherical harmonics, plotted on an equal area projection.

This map discloses a gross asymmetry. The heat flux from the Pacific hemisphere significantly exceeds the heat flux from the non-Pacific hemisphere, and the flux from the southern hemisphere exceeds that of the northern hemisphere. This is concordant with the suggestion that phase transition boundaries on average are currently deeper in the southern hemisphere than in the northern, and hence the "pear-shape" asymmetry of the geoid. A reasonable correlation emerges of high heat flow and young ocean floor, and this could improve with more heat data.

The degree of harmonic analysis is too coarse to show up second order features of tectonic interest. For example, the Fiji Basin has very high heat flow and the Coral Sea, between the Solomon Islands and the Queensland coast, has rather high heat flow (Heezen and Hollister, 1971), but these basins are lost in the general averaging process at this degree. Nevertheless, in this chapter we are concerned with global asymmetries, for which purpose the analysis goes far enough.

Seismic asymmetry

Earthquake distribution is grossly asymmetric.

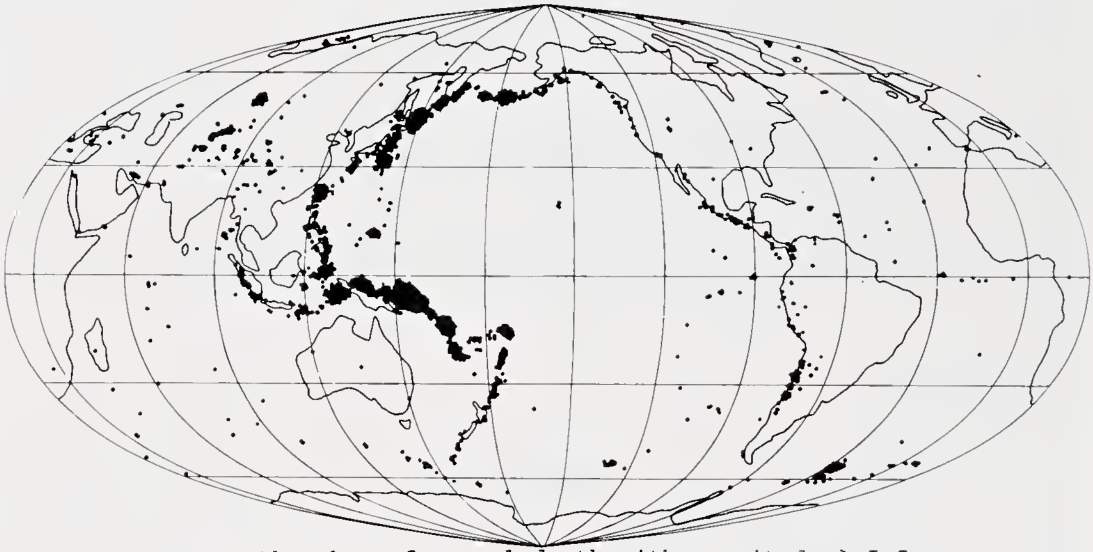
(1) Eighty per cent. of the world's earthquakes are closely associated with the Pacific margin as defined by the Andesite Line (Fig. 87). Thus modern seismicity reproduces the unique Pacific.

(2) The seismicity of the eastern margin of the Pacific is significantly weaker and shallower than seismicity of the western margin (Figs. 86-87). Modern seismicity thus reproduces the east-west asymmetry of the Pacific.

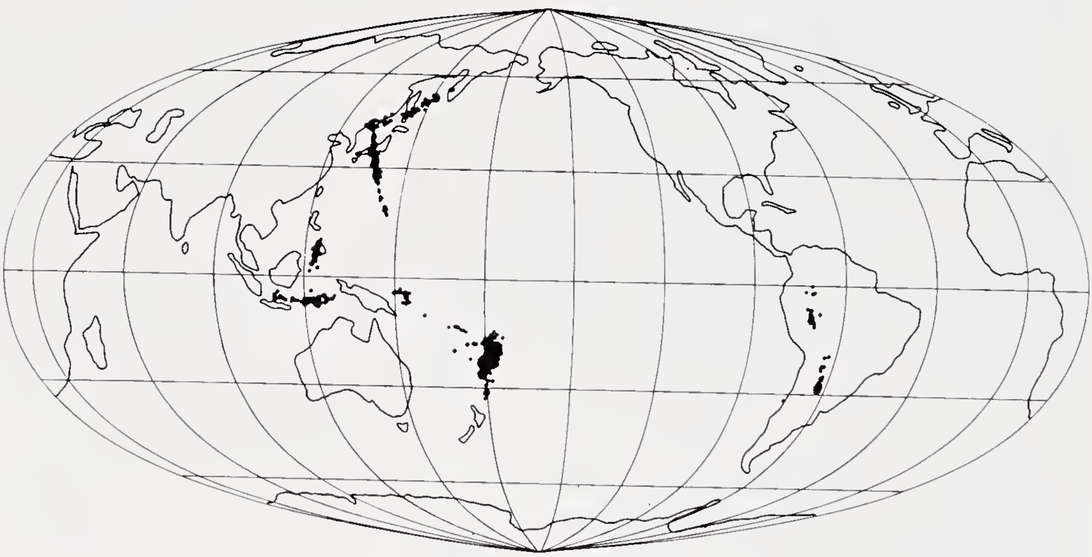
(3) The Pacific seismic belt shows clearly the gross westerly off-set of the northern hemisphere with respect to the southern along the Tethyan torsion zone (Fig. 86). Nevertheless the Tethyan torsion zone across the central Pacific is not seismic. On an expanding earth it is not necessary that it should be. The opening could be like a half transform with no growth east of the trenches, new growth west of the trenches and south of the equatorial torsion, and much larger new growth west of the trenches and north of the torsion zone.

(4) If the Pacific seismic girdle is eliminated, a large proportion of the remaining earthquakes are within the Tethyan torsion zone, and those that are still left are predominantly on the mid-oceanic rift system. Another equally valid way of looking at this distribution is to say that some ninety per cent. of the world's earthquakes fall within the tri-semi-circular orogenic margins (in the order I, II, III of Fig. 88) and nearly all the remainder fall in the mid-oceanic rift system. A plot of the active and recently active volcanoes of the world reproduces the same result, with a similar ranking between the above categories.

(5) Significantly more earthquakes occur in the northern hemisphere than in the south. Less than one *tenth* of the earth's quakes occur in the southern *quarter* of the crust (Talley, 1962). The great Antarctic conti-



A Earthquakes of normal depth with magnitude ≥ 5.5



B Earthquakes originating deeper than 300 km with magnitude greater than 5.5

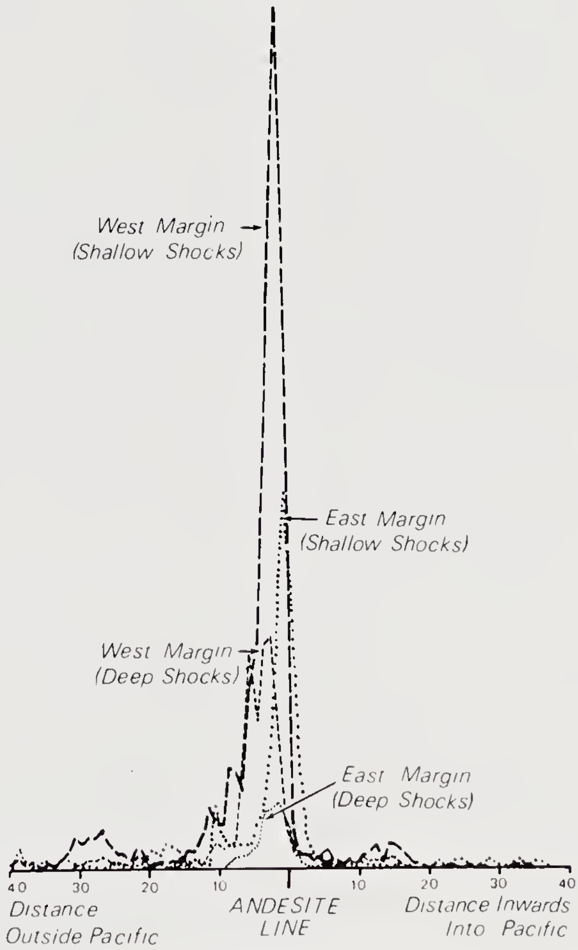
Fig. 86 World seismicity, 1961-76. Note 1. offset by Tethyan torsion
2. partly independent distribution of shallow and deep earthquakes
3. contrast in intensity of seismicity between west and east Pacific.

nent and its bordering ocean for several degrees is currently devoid of shocks. This is not an artifact of instrument distribution. During and since the International Geophysical Year a considerable number of high-grade stations have been maintained in the Antarctic region by seven countries. Not one Antarctic epicentre has been recorded. The southern mid-oceanic rift system which surrounds Antarctica in the southern Atlantic, southern Indian Ocean and southern Pacific produces a number of shocks, but within this girdle there is peace such as is not enjoyed by any other crustal block of comparable size. Australia, too, although not aseismic has a smaller ration of earthquakes than any other continent except Antarctica.

There is abundant evidence that Antarctica has been actively seismic in the past. But was it then polar? Palaeomagnetic evidence says no.

Contemporary seismicity, vulcanism, and geomagnetism disclose the *current* tectonic asymmetry of the earth, the first two in the crust and shallow mantle, the third in the deep core. These asymmetries are of the same kind and of the same scale as those which stem from ancient history. This is a matter of great significance, which leads away from unique acci-

Fig. 87 Histogram of earthquakes for three years (December 1958 to August 1961) plotted against distance from the Pacific margin (andesite line).



dents as causes for the observed asymmetries.

Magnetic asymmetry

The earth's magnetism is surprisingly asymmetric, with respect to axis, to centre, to hemisphere, and meridionally. The magnetic axis does not pass through the geodetic centre of the earth. It misses by some 300 km. The magnetic axis is inclined to the rotation axis, surprisingly enough at nearly the same angle as the earth itself is inclined to the ecliptic. Is this just chance, or is this coincidence significant? This would indeed be the expectation if the root cause of the core circulation, to which the field is attributed, is the couple between core and mantle necessary to constrain the two to precess together (see below). The total intensity of the earth's field increases to a single maximum at the south magnetic pole (Fig. 84). In the northern hemisphere there are *two* somewhat weaker magnetic maxima, one in Canada and the other in Siberia (Fig. 85) nearly 50° apart along a great circle and almost 180° apart in longitude. The south magnetic pole with its single magnetic maximum seems to gyrate with an epicyclic path round the rotation pole in a few thousand years. The north magnetic pole (i.e. the point where the magnetic inclination becomes vertical) seems to oscillate along a straight path between its two magnetic intensity maxima (Hope, 1959). At present the north magnetic pole is on the Boothia Peninsula about a third of the way from the Canadian intensity maximum towards the Siberian intensity maximum. The dichotomy of the northern magnetic maxima produces a strange pattern in the magnetic meridians, which converge, not to a point as in figure 84 at the magnetic south pole, but to a bar 2000 km long running between the two maxima (Fig. 85). These phenomena, like most other geomagnetic features, have no relation to crustal rocks, but are controlled deep within the earth, and point to a fundamental asymmetry.

In the southern hemisphere the agonic line runs from magnetic equator to magnetic pole and back to equator as a smooth curve approximating to a great circle, as one might expect a meridian to behave (Figs. 81 and 84). In the northern hemisphere (Figs. 81 and 85) the agonic line tends towards two great circles; one goes through the two maxima and the north magnetic pole, and the other passes through none of these points but cuts the first at right angles and runs through the rotation pole. The result is that the agonic line really writhes! Coming into the northern hemisphere from the well-behaved southern hemisphere it runs directly to the Canadian magnetic maximum, continues to the geodetic pole then swings back to the magnetic equator (cutting off a quadrant instead of a hemisphere) then turns back north-east to the Siberian maximum, whence it continues towards the geodetic pole, but veers east again before reaching it and then swings in a wide lobe southwards to join up with the other limb from the southern hemisphere.

In addition to these north-south asymmetries there is an equatorial asymmetry. Figure 81 shows the total magnetic intensity along the magnetic intensity equator. There is a minimum in Brazil and a maximum in Malaya. Making a geodetic analogy, the magnetic equator is elliptical with a difference in semi-diameters of some 44 per cent. The range in total field intensity is nearly twice as great in the southern as the northern hemisphere, and the southern magnetic hemisphere is much more active than the northern. In figure 83, which shows the rate of change of total intensity, more than half the southern hemisphere has an annual rate of change of field greater than 60 gammas. Very little of the northern hemisphere reaches this rate.

The non-dipole geomagnetic field shows the same scale of asymmetry as does the geoid: three potential minima, two northern - Bahamas and China, and one southern - south of Africa. The three potential maxima include one equatorial off the Gulf of Guinea, and two on the 180th meridian, one north, one south. Although the scale of the magnetic and geodetic asymmetries is the same, they do not correlate in position. The non-dipole field of course lies in the core and the gravity field has its source in the mantle.

The most plausible theory for the origin of the field is that fluid circulation of the mantle yields a magnetic hydrodynamic toroidal field because the mechanical motions compress or dilate the field more rapidly than the field can decay, the isoporic foci being the location of eddies in this circulation. The energy to drive the circulation and to induce the field has been attributed to thermal convective overturn from radioactive heating, which has been shown by Bullard to be probably adequate energywise. However a much more attractive alternative has been offered in the form of the precessional restraints on the core, the energy in this case ultimately coming from the recession of the moon.

Precession of the earth depends on the centrifugal ellipticity of its figure, and the fact that the plane of this ellipticity is oblique to the ecliptic. The ellipticity of the core differs from that of the mantle, hence the lunisolar torque acting on the core is less than that acting on the mantle. The core torque is only about three-quarters of the torque necessary to cause it to precess at the same rate as the mantle. Malkus

(1966, p.26) estimated that if core and mantle each precessed at their own free rates, the relative motion of core and mantle would reach a maximum of about a thousand miles per hour after half a precession cycle, when their rotation axes would be tilted in opposite phase, and would decline again to nearly zero nearly 13,000 years later. Clearly the core and mantle are coupled to precess together, so that the mantle precession drags the core precession along with it. There is much debate about the coupling mechanism, and Toomre (1966) has discussed the relative magnitudes of hydrodynamic (inertial), viscous, turbulence, and magnetic coupling models. Stacey (1973) has neatly combined the inertial and dissipative restraints. However Rochester, Jacobs, Smylie and Chong (1975) have estimated that between 10^9 and 10^{11} watts is the rate of working necessary to maintain the geomagnetic dynamo, and that the maximum power available from differential precessional torques cannot exceed 10^8 watts if the core flow remains stable. Nevertheless it would seem that even though the energy source to power the field may be primarily thermal (e.g. radioactivity) nevertheless the pattern developed could be largely determined by the precessional torque. Whatever the mechanism, clearly some energy dissipation is involved, which would contribute to the earth's secular retardation of rotation, supplementary to the normal tidal drag. Since precession depends on obliquity, which has varied through geological time, the magnitude of this differential precession dissipation would also have varied with time, as would the contribution of this drag to the acceleration of the moon.

These couples, excited by the mantle to accelerate the precession of the core, and by the core to retard the precession of the mantle, in each case tend to turn the rotation axes, but in opposite directions. Hence they must produce mutual precessions with several cycles to the primary precession cycle. As these torque interactions are mutual within the earth, the result should be wobbles of the mantle with respect to the core, and of the core with respect to the mantle. The periods should be the same but the amplitude of the core wobble should be much greater than the amplitude of the mantle wobble in proportion to their respective moments of inertia. As the kinematic viscosity of the core is at least as low as 10^5 cm^2/sec (Jeffreys, 1959), and probably as low as 10^{-2} cm^2/sec (Toomre 1966), which is of the same order as that of water, and as the period of the impressed wobble should be of the order of tens of thousands of years, the core must behave as a turbulent fluid in this torque. Hence the effect on the core should be, not to produce a precession of the core, but to drive large eddies whose axes should have an obliquity nearly as large as the earth's obliquity to the moon's orbital plane. These axes should precess about the earth's rotation axis.

The correlation between the westward drift of the non-dipole magnetic field and changes in the length of the day (Kahle, 1969) indicates that the core does indeed lag behind the mantle at about 10'.8 per year, so that if this rate is average the magnetic pole completes the circuit about the mantle rotation pole in about 2000 years, anticlockwise as viewed from the north, with a polhode of radius between 10° and 20°. Meanwhile the polhode of the mantle should complete a circle clockwise about 1° in diameter, because the moment of inertia of the mantle is so much greater than that of the core. This is consistent with the observed polar wander as shown in figure 116. On this model the mean magnetic field over a full cycle would necessarily coincide with the mean rotation axis. However this could suffer a longer secular wander. The core eddies needed to generate the magnetic field have been estimated by Bullard to have a period of about 40 years. These may correspond to the Markowitz wobble, which has a period of about 40 years and an amplitude of about 0".02.

The westward drift of the magnetic field was first reported by Gellibrand (1635), and by 1692 Halley deduced that the source of the field was at least partly located in the core of the earth, and that the core rotated a little more slowly than the outer shell of the earth. Bullard (1956) comments: "It is not too much to say that by reading these two papers of Halley's one can learn more about the origin of the earth's magnetic field and its secular variation than will be found in all that was written during the succeeding 250 years". It now seems that Halley's retardation of rotation of the core with respect to the earth's outer shell is a precession of one against the other caused by the precessional coupling torque between them.

The fluctuations in the rate of westward drift have been one of the most intractable enigmas in geophysics. Newcomb (1902) wrote that their explanation by any known factor is so difficult as to leave us with the belief that their cause lies in some hitherto unknown process. Pariyskiy (1955) calculated that the implied torques exceeded the tidal torques by two orders of magnitude. However the development of eddies in the core induced by the core-mantle precessional torques, interacting with through-going jet streams, could cause such fluctuations.

Vestine (1953) suggested that fluctuations in the rate of westward drift of the isoporic foci correlated with fluctuations in the rate of rotation of the earth as indicated by differences between universal time and ephemeris time, which implied transfer of angular momentum between mantle and core, in both directions. Munk and MacDonald (1960) and As (1967) have confirmed the probable validity of this interchange. Bullard (1950) has pointed out that if the differential motion of mantle and core were due to viscous drag of the core from tidal deceleration of the mantle, the drift should be eastward not westward as observed. Likewise, differential expansion would be expected to yield an eastward drift. Differential precessional torques between core and mantle do give the observed westward drift.

Bullard (1949) recognised that the differing precession periods of core and mantle offered more than adequate energy to drive the geomagnetic dynamo if the two precessions were not firmly coupled. But he assumed that there was no differential precession, and overlooked the facts that the coupling itself implied a mutual precession of mantle and core, and that in fact the axis of the core circulations did precess about the mantle rotation axis with a period of a few thousand years. Malkus (1963) and Stacey (1967) agree that precessional torques are an adequate and likely source of the geomagnetic field.

DYNAMIC ASYMMETRY

Orogenic belts

The orogenic systems of the earth are neither symmetrical nor random. All would agree that since the Palaeozoic the Pacific margin has been an orogenic ring, along with which there was a Mediterranean belt extending through Indonesia to Gibraltar, and most would extend this via the Atlas to the Caribbean. This may be represented in perspective by three semi-circles on a common diameter (Fig. 88a). This is a tri-lobed system, but its axes are not those of a tetrahedron. One axis (from Indonesia to the Caribbean) has similar ends, whereas all tetrahedral axes have dissimilar poles; also two lobes are land (Laurasia and Gondwana) and one ocean (the Pacific). In the tetrahedral model, all faces are similar. The tri-lobed symmetry is not complete (AAA), but partial (AAB). It is clear from figure 84a that the orogenic girdle of the Pacific is equally an orogenic girdle around Pangaea. Subsequently, Spilhaus (1973, p.53 and Fig.3) again emphasizes this point; it is, of course obvious that this would have to be so in respect of any near-great-circle on the globe - if it surrounds some category it must also surround the complement of that category.

Holmes (1933) saw the same frame as a figure-of-8 wrapped round the globe (Fig. 88b), with one lobe surrounding Laurasia and one Gondwana. The differences between figures 84a and 84b vanish when the figures are applied to the globe.

Sollas (1903), followed by Lake (1931) and Wilson (1949), saw the same features as a T wrapped round the globe (Fig. 88c). Wilson's Pacific ring is broken between New Zealand and Antarctica to form the cap of the T and his Mediterranean ring is broken between the Atlas and the Antilles. Adherents to continental drift or continental dispersion theories regard these gaps as superimposed and not significant, and when the T, which is a plane figure, is applied to a globe, there is little difference between the tri-semicircle, the figure-of-8 and T models. Under each of these models the orogenic belts make a tripartite division of the earth's surface, and this has AAB symmetry, not AAA. Wilson's T has less symmetry than the others because his does not equate the East and West Indies.

Eastward bias

Island arcs and trenches mostly bow eastwards, and overthrust eastwards. A few bow equatorwards; some combine both. This suggests a rotational filter, because eastwards and equatorwards equally involve acceleration with respect to rotation. Around the Pacific girdle overthrusting to the east far exceeds overthrusting to the west. This is equally true on both sides of the Pacific -- East Asian and Cordilleran, Rockies and Andes. Rotational factors must be at the helm.

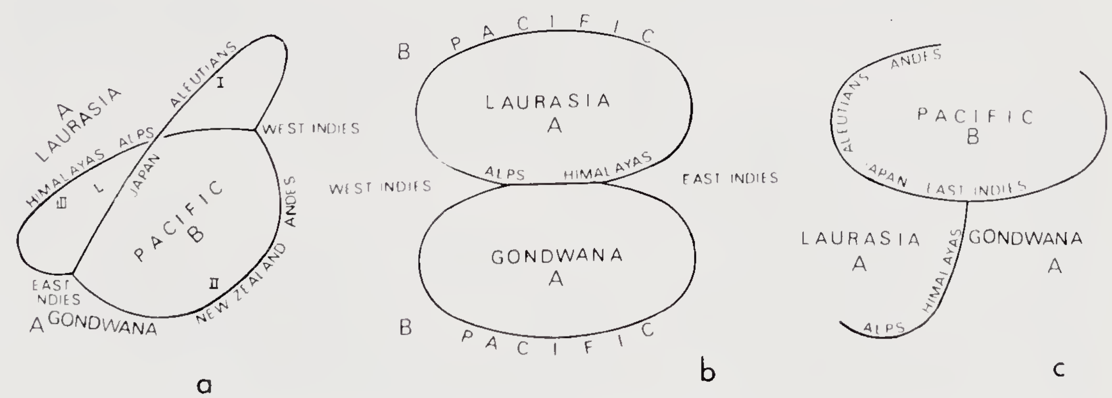


Fig. 88 Three models to represent the post-Palaeozoic orogens. (a) Three half great-circles on a common diameter. (b) Figure-of-eight of Holmes. (c) T-pattern of Wilson. (From Carey, 1963).

Asymmetry of contintental dispersion

Gross asymmetry is apparent in the post Palaeozoic enlargement of the surface area of the earth. More new oceanic crust has been inserted between the southern continental blocks than between the northern, so that they have separated from each other more. This has resulted in the so-called "water hemisphere" and "land hemisphere". Dispersion is maximum at a point south east of New Zealand and least in Siberia (see pp. 48-50).

The Tethyan Torsion—a north-south asymmetry

William Lowthian Green (1875, 1887) pioneered the idea that an equatorial torsional displacement had occurred between the northern and southern hemispheres, in order to explain the observation that the southern continents (Africa, South America, India and Australia) were consistently separated from the northern continents by zones of profound crustal disturbance and vulcanism. He argued that the shrinkage from the spherical to the tetrahedral form involved more increase in angular velocity for the three depressed faces of the tetrahedron on the southern hemisphere than for the three tetrahedral quoins of the northern hemisphere. The southern hemisphere therefore tended to drag further east than the northern - hence the zone of torsion. This theory was supported by Daubrée. The equatorial-torsion hypothesis was elaborated by the Belgian astronomer Wilhelm Prinz (1891), who also suggested that there were similar gross patterns on the surface structure of Mars, which might indicate a parallel development of the two planets.

The principle of an equatorial torsion affecting the whole earth, developed by Green and Prinz, did not take root, and was forgotten. Working

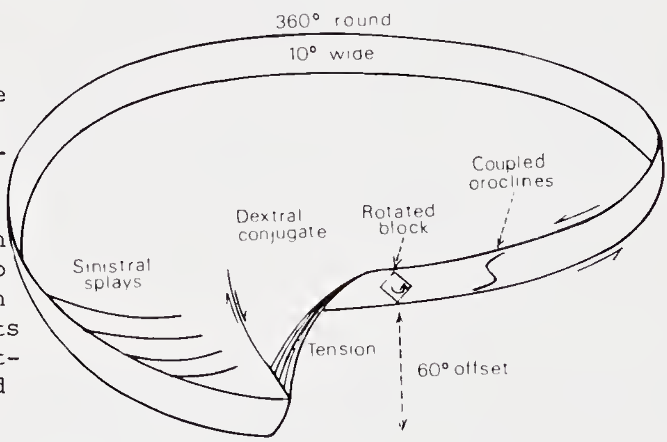


Fig. 89

Global pattern of Tethyan Torsion, showing (a) the scale relation of zone width (10°), circuit length (360°) and torsion offset (60°); (b) the relation to the main torsion of the linking structures such as sinistral splays (Mendocino type), dextral conjugates (San Andreas type) and tension rifts (North Atlantic type); (c) rotated blocks (Iberian type) and coupled oroclines (Sicilian-Ligurian types).

in New Guinea during the thirties, I quite independently came to the same conclusion and, until recently, I did not know I had been anticipated nearly half a century before. In 1938 I wrote: "New Guinea has been sheared westward under a colossal shear system on a scale grander than has been demonstrated anywhere else on the globe" . . . The stresses which are responsible for this great westerly displacement are of continental dimensions. They are probably related to the main architectural pattern of the globe". In the continental drift symposium held in Hobart in 1956 I showed how the zone of torsion extended right round the earth, north side displaced westward (Carey, 1958a).

The Tethyan Torsion zone has six gross characteristics:

(1) it marks the early Mesozoic equator.

(2) this equator has since been tilted some thirty degrees to the present

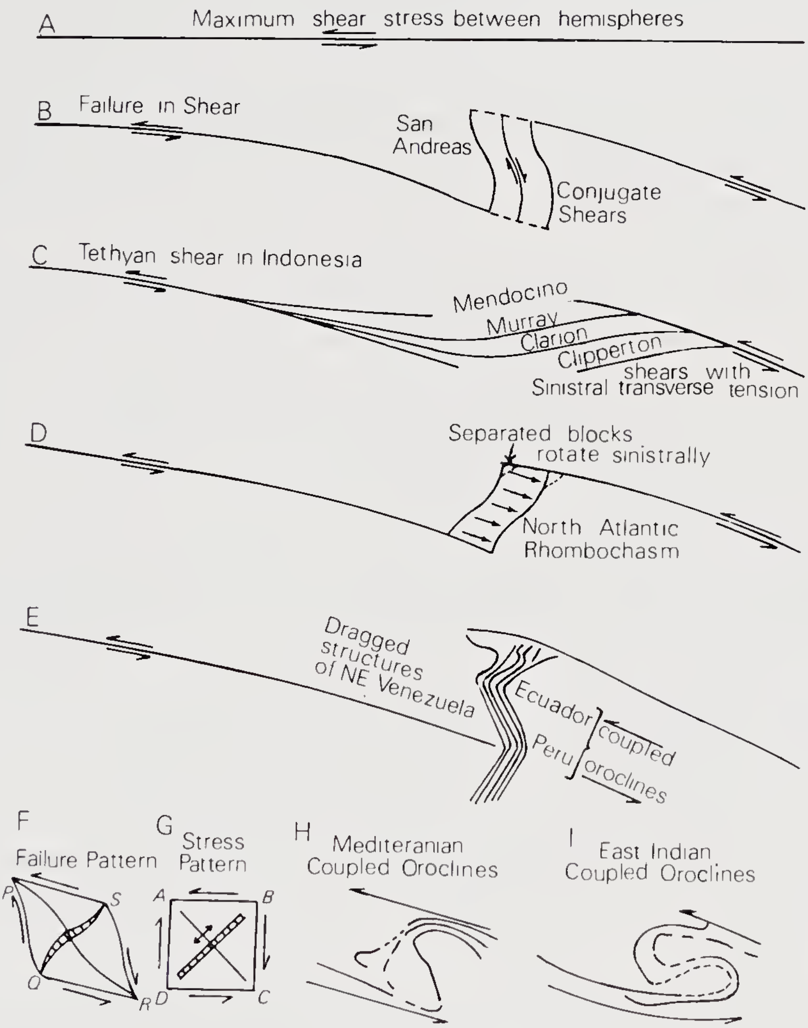


Fig. 90 Geometry of the Tethyan Torsion System.

equator, with the Mesozoic pole now in north-east Siberia, and the Mesozoic equatorial belt through the Mediterranean far to the north of the present equator, and in the central Pacific south of the present equator.

(3) the *Südpolflucht-Nordpoldrang* northward movement of all continental blocks (p. 217) has displaced the old equatorial belt northwards, so that even though the tilt remains, the belt reaches much higher northern latitudes through the Mediterranean than its extreme southern latitude in the Pacific.

(4) torsional *ruptures* are inclined to the shear stress, and are returned to the plane of shear via complementary tension rifts. The gross inequality of the earth's crust (with Pangaea and eoPacific) has influenced this pattern.

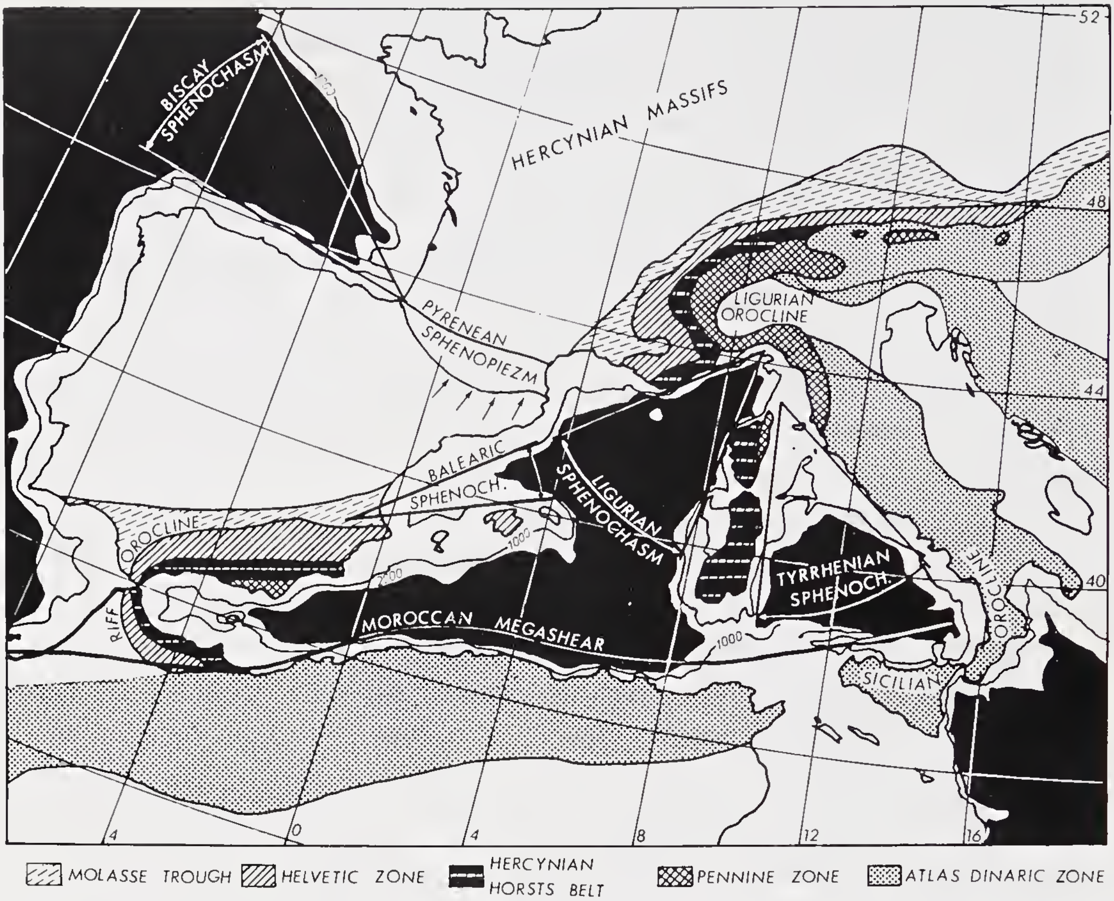
(5) as active orogenic zones were involved, bending, dragging, and stretching are common in addition to the fractures.

(6) as would be expected on an expanding earth, transverse widening accompanies the sinistral offset - about 600 km between Africa and Europe (Figs. 92 and 93), some 1500 km between North and South America (Fig. 16) and about 2000 km between Australia and southeast Asia (Fig. 185). Extension seems to have been least in the Mediterranean and most antipodally in the western Pacific.

Hence looked at as a single structure (Fig. 89), the Tethyan torsion zone is a belt generally 1000 km or more wide, and a total length of some 40,000 km, which creeps southwards as it is followed eastward round the globe, so that the zone miscloses its own beginning by about 60° (some 7000 km). This results in linking-structures of three types, (i) north-east-trending tension rifts like the North Atlantic, (ii) sinistral splays like the Mendocino, Murray, Pioneer and Clipperton Shear Zones, (iii) dextral conjugate structures like the San Andreas Rift. Linking fractures may of course develop with other trends. If their direction lies within the angle BDC of figure 90G they will have both sinistral and tensional motion, entirely the former where the direction coincides with DC and entirely tensional where the direction coincides with DB. The Mendocino, Pioneer, Clarion, Murray and Clipperton shear zones all fall within this category. The sinistral offset on the Pioneer zone (200 km) and the Mendocino zone (about 1100 km) has been demonstrated by the brilliant marine magnetic work of the La Jolla group under Vacquier (1959).

Since these words were written in 1962, these fracture zones have been interpreted as transform faults associated with the opening of the Pacific. This is probably largely correct, but supplements rather than contradicts the earlier interpretation. The motions and disruptions of the lithosphere are not unicausal. Certainly crustal extension is one of the primary processes. But so is the interhemisphere shear expressed in the Tethyan torsion, and octantal rotations (as in Figs. 103 and 140) and probably zonal motions (as in Fig. 103). Not only is the actual fracture pattern determined by the combination of all the active processes, but these may be synergistic. The strong sinistral equatorial torsion of the Tethyan zone is responsible for the pronounced sinistral bias superimposed on the fracture zones associated with the spreading ridges, where these two processes interact. If the direction of fracture falls within the angle ADB of figure 90G, the faults will be combinations of dilatation and dextral shear, with the dextral component varying from zero to total as the trend changes from DB to AD. The rift faults of the Basin-and-Range province of western North America fall in this category near DB (i.e. dominantly tensional). Fractures trending between AD and AC are dextral with thrusting. The San Andreas fault falls in this category near AD (dominantly dextral). The Gulf of California represents the dilatation component. Just as the Mendocino group of fracture zones could act both as transform faults and as members of the Tethyan torsion zone, so these Californian and Nevadan structures are related to the rotational drag of the Cordilleran margin of North America (Fig. 140) and to relief of the Tethyan torsion.

The Tethyan torsion zone is usually about 1000 km wide. Where this involves strong crust, blocks isolated by linking fractures may rotate sinistrally during the torsion movement (Figs. 89 and 90H). Examples of such rotated blocks are the Iberian Peninsula 40° (Figs. 91 to 94), Arabia 3°, India 70° (Fig. 2), Guatamala-Yucatan 85° and Honduras-Nicaragua 20° (Figs. 99 to 102) and Newfoundland 25° (Figs. 94 and 95). Where on the other hand, the shear zone involves more plastic crust which tends to bend or stretch rather than break, the shear movement may drag one end of a segment round into the direction of shear or form an S-shaped coupled orocline loop, or stretch the segment greatly in the direction of shear. Examples of sinistrally coupled oroclines are the Sicilian-Ligurian couple (Figs. 90H, 94 and 95), the Baluchistan-Punjab couple (Fig. 2), and the Sunda-Banda-Celebes loop (Figs. 90I and 98). Examples of sinistrally dragged geanticlines are the Riff loop (Figs. 90H, 94 and 95), New Britain, the Antilles (Figs. 99 to 102) and the Panama, Central Cordillera, Eastern Cordillera, Santa Marta, Periha, Trujillo and Falcon belts of Venezuela (Figs. 90E, 96 and 97). Every drag, every block rotation, every coupled pair of oroclines, and every coupled sphenochasm and sphenopiezm right



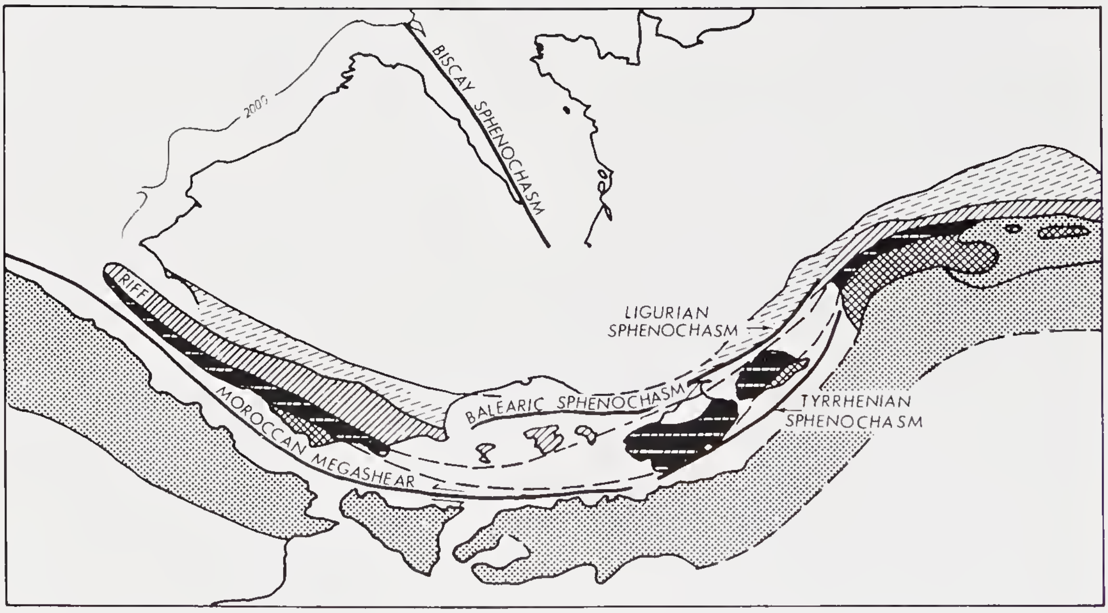
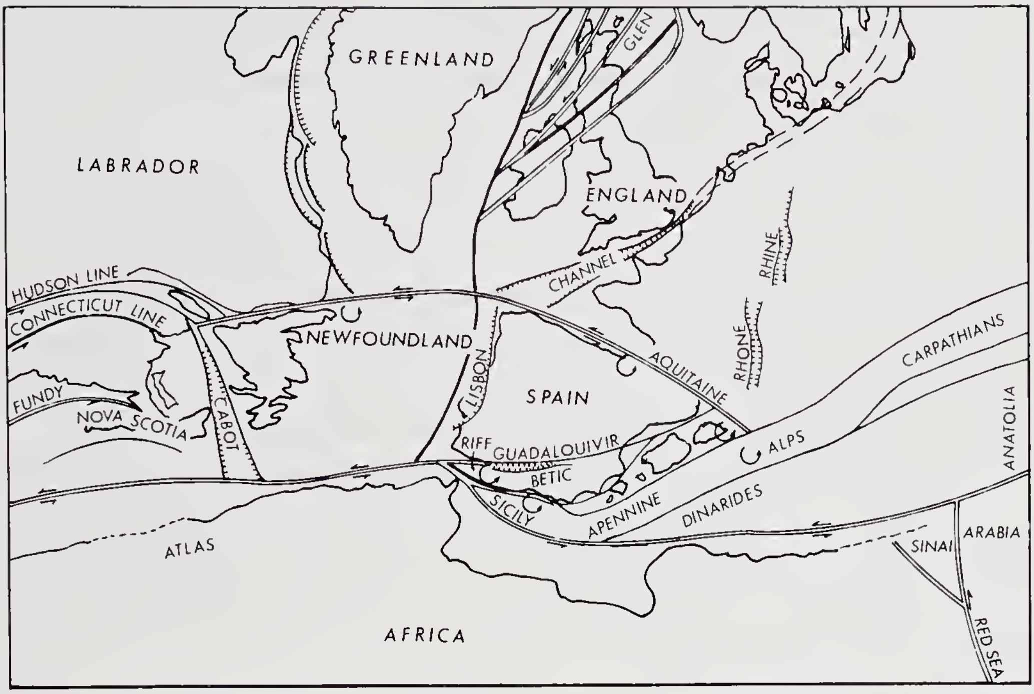


Fig. 91 (above) Tectonic development of Western Mediterranean by westward movement of Europe with respect to Africa across the Tethyan torsion zone.

Fig. 92 (below) Tectonic pattern of the western Mediterranean.



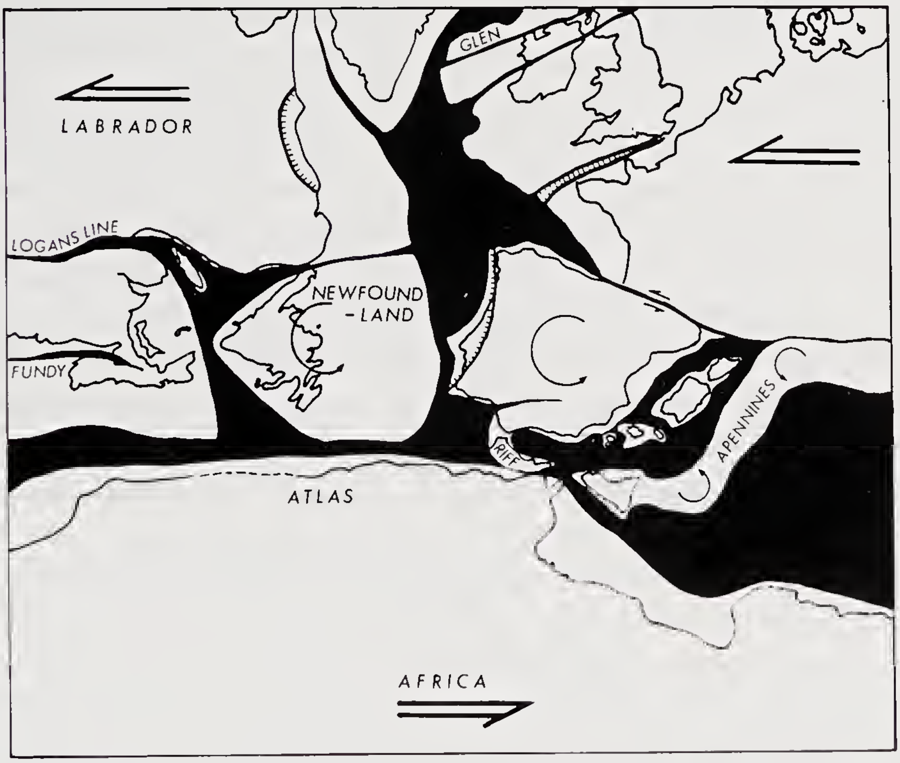


Fig. 93 (above) Tethyan torsion zone between Africa, North America and Europe.

Fig. 94 (below) Initiation of break-up of Afro-Laurasian block through Tethyan torsion movement.

round the Tethyan torsion zone indicates that the motion is sinistral.

Wherever palaeomagnetic azimuths are available they confirm the rotations (Table I). No contrary palaeomagnetic data are known to me.

Tethyan torsion across the Pacific

The Pacific Ocean is roughly circular, but the northern half is offset far to the west of the southern half (Fig. 98). The boundary of the Pacific may be defined in several ways. Perhaps the most fundamental is the andesite line, which fronts Kamchatka, Japan, Formosa, the Philippines and Halmahera with a trend veering progressively from south-west to meridional. At the Moluccas the andesite line jumps discontinuously eastwards for sixty degrees to Samoa, whence it resumes its southerly trend via New Zealand towards the Antarctic. This great offset is the Tethyan torsion zone. The eastern margin of the Pacific is equally offset but the displacement is distributed by the great series of linking shear zones already mentioned, which include the Mendocino, Pioneer, Clarion, Murray and Clipperton shear zones and extend as far south as Peru.

Tethyan torsion across Pangaea

Each of the Gondwana continents has been displaced east with respect to its Laurasian counterpart during the disruption of Pangaea. (1) When The Americas are moved back to fit against the African template (Fig. 16), the displacement of North America is substantially greater than the displacement of South America. Hence, there has been large sinistral motion between North and South America along the Tethyan torsion zone during the disruption of Pangaea. This is confirmed by the structures within the central American zone; this is amplified below. (2) When North America is fitted back against Africa as in figure 16, the continental shoals off Newfoundland extend along northern Africa well east of the Greenwich meridian. This is only possible if there has been strong sinistral movement between Europe and Africa during the disruption of Pangaea. This is confirmed independently by the structures within the Mediterranean region (see below). (3) Australia has also suffered large sinistral shift with respect to Asia since the disruption of Pangaea (Fig. 194).

Tethyan torsion through the Mediterranean

When the Mediterranean area is taken as a whole, the single movement eastward of Africa relative to Europe, with some transverse dilatation, produces all the complex tectonic structures of the region from the original Tethyan geosyncline (Figs. 92 and 93). The Algerian megashear drags the Riff loop round, and the movement is continued by the rotation of Spain with the opening of the Biscay sphenochasm and the closing of the Pyrenean sphenopiezm; at the same time the trunk of Italy is rotated westward, opening the Ligurian and Tyrrhenian sphenochasms, and forming the Sicilian orocline which takes up the megashear's displacement. The rotating blocks (Newfoundland, Spain, the Balearic Islands, and the Apennines) are enclosed between the two main megashears and the rotation axes for the several segments fall on these shears (Fig. 93). Widening tension rifts run between this pair of megashears.

The following generalizations are true of the Mediterranean zone, and are equally true of all the other deformed zones along the equatorial torsional belt.

(a) A topological homogeneity is maintained throughout the displacements. The reconstructions are not like jig-saw puzzles, where any block may be reassembled at any place which might seem to suit it. All blocks retain the same sequence and order and mutual relationship, in the same way as the skull of an ape is topologically identical with the skull of a man, or, more extremely, the entire skeleton of a snake is morphologically homologous to the skeleton of an elephant.

(b) The present configuration may be derived from the past configuration by slippage along megashears conforming to the sinistral pattern, together with progressive insertion of wedge-shaped slices of new simatic crust which separates surfaces formerly in contact. Once inserted, the new crust becomes permanent. No crust is swallowed or mysteriously eliminated. Crustal blocks do not move *through* the sima like rafts. Displacement is achieved by opening of rifts wherein new crust separates the old surfaces. If the opening is wedge-shaped, relative rotation occurs.

(c) The whole process is one of dilatation, together with the sinistral torsion.

Further east in Asia, the pattern of sinistrally dragged oroclinal couples is repeated in the sweep of the Anatolian ranges round the Black Sea, and of the Elburz Mountains round the Caspian (Fig. 95). Van Bemmelen (e.g. 1969) has interpreted the Alpine loop, the Sunda-Banda loop, and the Caribbean loop along the Tethyan zone, as the surface expression of mesoundations from rising asthenoliths. I think his general thesis is valid. The Tethyan zone is indeed a widening belt and hence one of generally rising mantle material and growth of new crust. Further, owing to the feedback effect of rising isotherms, the ascent at particular centres along the belt increasingly outstrips the rest, hence lens or ovoid anastomoses develop on the surface. At the same time the elevated isotherms cause more ductility, so that the concurrent interhemisphere torsion yields oroclinal drags rather than brittle block rotations, and bias these ovoids sinistrally. All the motions stem from the same cause (outgassing of the deep interior) and the resulting processes are synergistic.

Unfortunately there remains some prima facie contrary evidence. Gregor and Zijderveld (1964) reported that Permian samples from the Amasta region on the Black Sea coast of Turkey show *dextral* rotation of 80° and Pavoni has long argued for large dextral shift on the Anatolean fault and for this region generally on the basis of stream offsets and analysis of the fault and fold trend patterns. Certainly Ambrayseys (1970) documented conclusively dextral offsets during recent earth movements in this region. In addition the reversed-S bow from the Carpathians through Romania, Moravia and Bulgaria to the Dobrudja suggests dextral rather than sinistral motion. De Boer (1965) advocated dextral mega-translations to bring the African and European blocks into acceptable latitude relations. The palaeomagnetic and morphological data and arguments with respect to sinistral versus dextral shift across the Mediterranean have been summarized by Zijderveld, Hazeu, Nardin, and van der Voo (1970), who confirmed the validity of the large sinistral rotations of the several "loose" blocks in the Mediterranean zone, dismissed as invalid de Boer's argument on palaeomagnetic ground for large dextral shift, but refrained from declaring a firm decision. They concluded: "Until further Permian paleomagnetic data for Africa have become available, we are not in

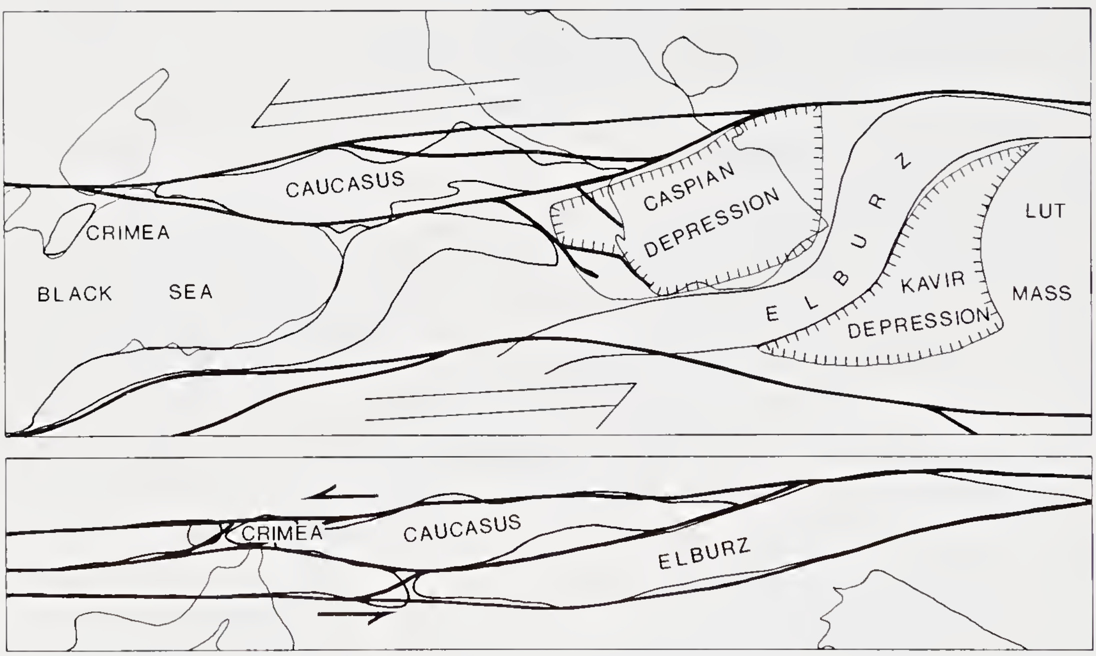


Fig. 95 Tethyan torsion in Asia Minor.

a position to make a choice on the basis of paleomagnetic data, but it seems that evidence from the structure of the Atlantic Ocean floor is in favour of the latter (sinistral-shear) hypothesis (Funnel and Smith, 1968)".

Clearly there remains some problems to be solved in this region. However the constraints of the gross continental relations round the entire Tethyan girdle cannot be satisfied except by substantial interhemisphere torsion. A number of models could be adopted to combine such first-order torsion with regions of dextral movement within it (e.g. interhemisphere shear combined either with easterly motions (Tanner, 1964); or octantal motions as in figure 103, or the surface expression of van Bemmelen's asthenolithic tumours).

Tethyan torsion through S.E. Asia

The anticlockwise block rotation of India, and the S-sweep of the Cretaceo-Tertiary orogen -- southeast through Iran, east-west through Baluchistan, thence nor-northeasterly to the Hindu Kush, thence round the Jhelum hairpin to the Himalayas -- record the gross through-going sinistral shear of the Tethyan torsion. The separation of Australia and India during the disruption of Pangaea implies an eastward shift of Australia relative to India of more than three thousand km (Fig. 172). The strongly dragged and stretched loop of the Sunda-Banda oroclinotaths (Fig. 173) indicates similar sinistral displacement there, which continues through the major shear zone, which smears New Guinea against the Pacific coast to the north, rotates New Britain, and opens tensional sphenochasms with a northeast trend.

Tethyan torsion in the Caribbean region

The Caribbean region lies within the track of the Tethyan torsion belt. Orogenic trendlines loop right round through 180° as in other strongly disturbed zones along the belt such as the Alps and Indonesia. The African template (Fig. 16) leaves no doubt whatever about both the sinistral movement of some 3000 km between North and South America, nor of the transverse stretching between them across the Caribbean and Gulf of Mexico of more than 1500 km. The former was equatorial, the latter meridional, when referred to the Mesozoic zonation.

Thus the relation of North America to South America is completely analogous with that of Europe to Africa. Hess and Maxwell (1953) previously deduced a first-order sinistral megashear through the region. The north-west coastal region of Venezuela and Panama shows (Figs. 96 and 97) strong sinistral shear through the region. The continental block has been opened up by the westward drag of the torsion to form sphenochasms and basins. Heezen and Tharp (1961) produced the first detailed map of the central Atlantic, in

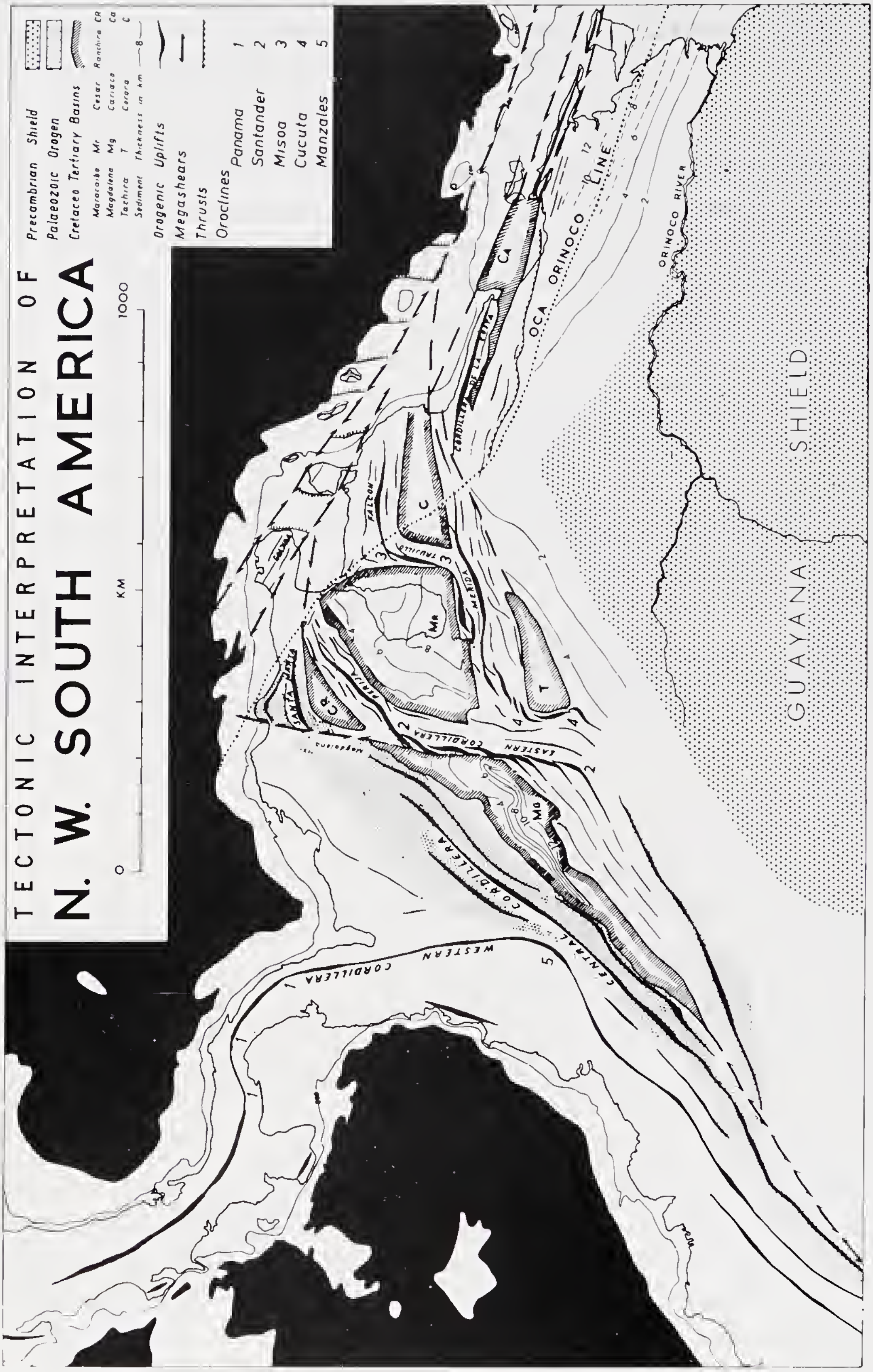

TECTONIC INTERPRETATION OF
N. W. SOUTH AMERICA
0
1000
KM
Precambrian Shield
Palaeozoic Orogen
Cretaceo Tertiary Basins
Maracaibo Mr Cesar Ranchiro CR
Magdalena Mg Cariaco Ca
Tachira T Carora C
Sediment Thickness in km
Orogenic Uplifts
Megashears
Thrusts
Oroclines
Panama 1
Santander 2
Misoa 3
Cucuta 4
Manzales 5
GUAYANA SHIELD
ORINOCO RIVER
OCA ORINOCO LINE
CORDILLERA DE LA COSTA
FALCON
TRUJILLO
MERIDA
EASTERN CORDILLERA
CENTRAL CORDILLERA
WESTERN CORDILLERA
SANTA MARTA
Magdalena
CA
C
Mr
CR
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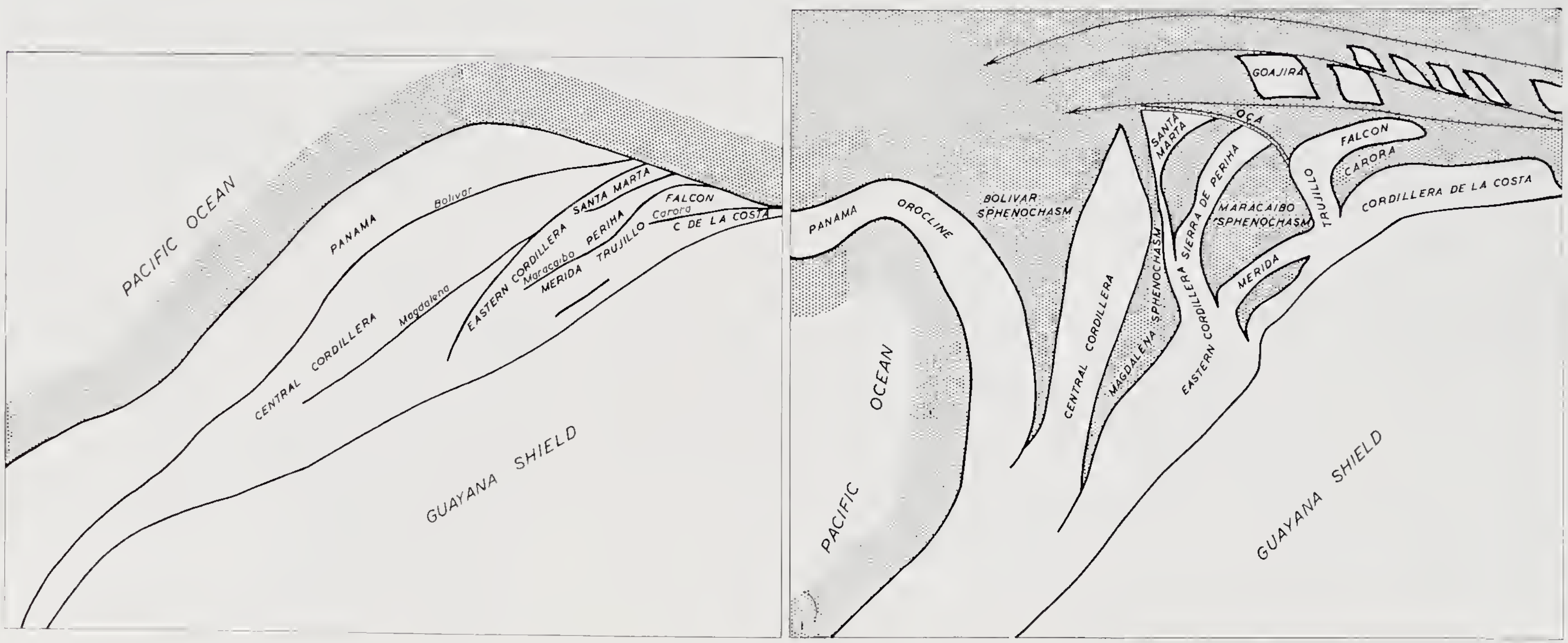
Fig. 96 (opposite) Tectonics of northwest South America.

Fig. 97 (below) Diagram showing mechanism of dragging open of northwest Venezuela, Colombia, and Panama by the Tethyan torsion movement.



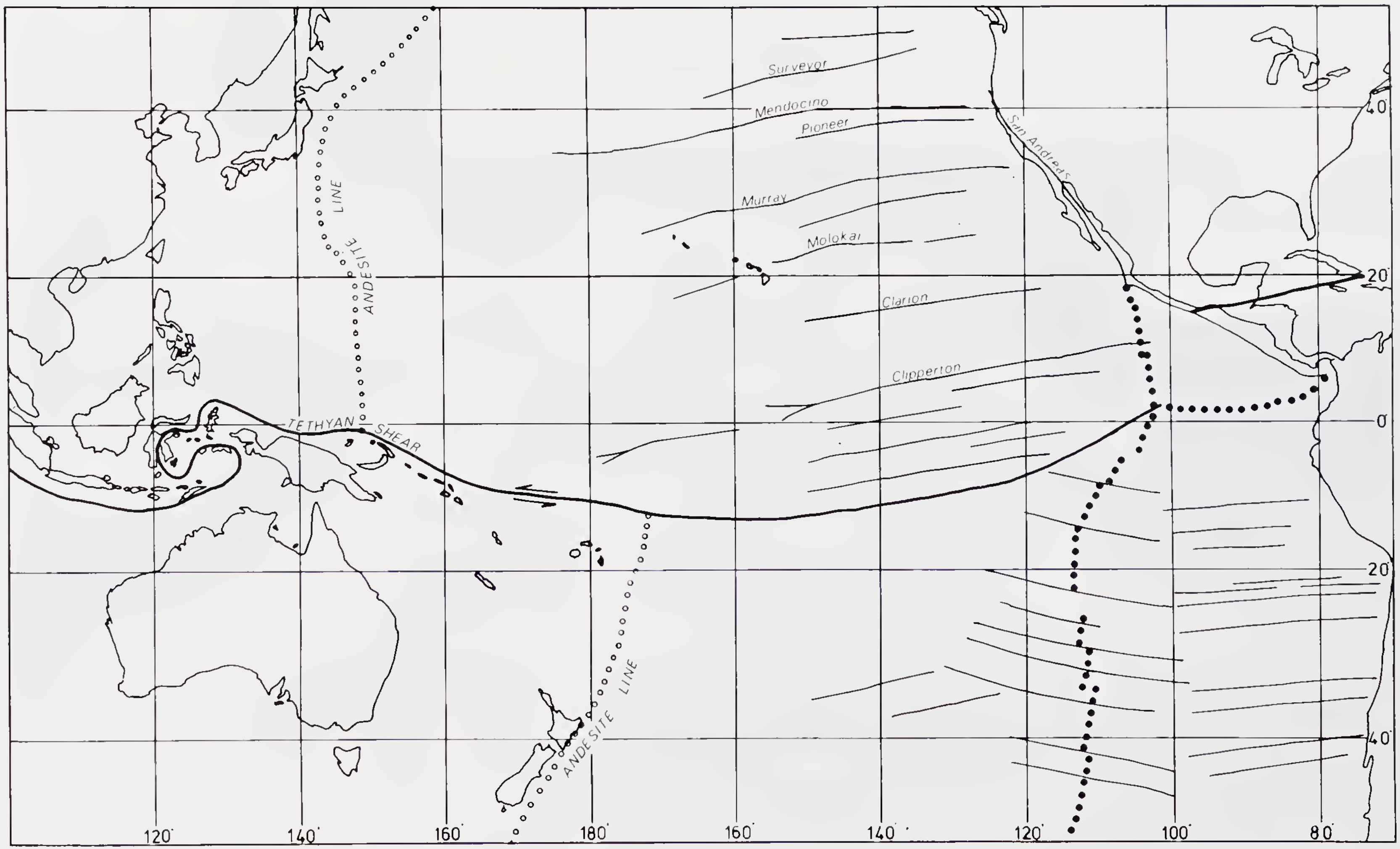


Fig. 98 Displacement of the Pacific Ocean by the Tethyan torsion zones.

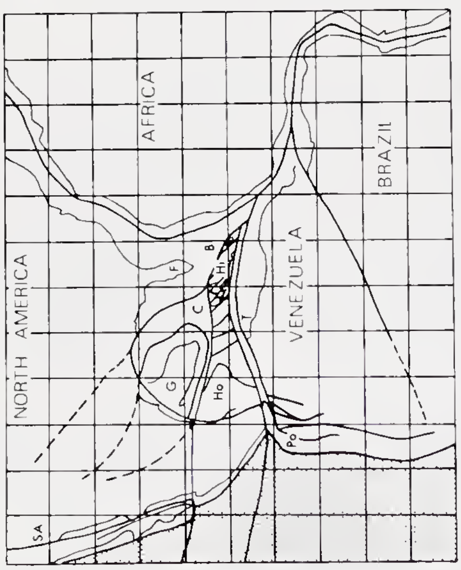
which they traced intense shear zones on the Atlantic floor from the Caribbean into the Gulf of Guinea, which have been fully confirmed and detailed by subsequent work. Viewed through the filter of plate tectonics theory, these fracture zones have latterly been seen just as transform faults. Nevertheless the overriding gross sinistral offset remains valid.

All of this agrees well with the general position of the Caribbean astride the Tethyan torsion zone. However, the area is so complex tectonically that there is still room for different versions of the reconstruction. All must bring North America nearer to South America meridionally and some 3000 km further east with respect to it. All must bring the Guatemala-Yucatan-Honduras-Antillean block to its former position filling the Gulf of Mexico. All must conform to the three general conditions of transformation and continuity set out above with respect to Europe (topological homogeneity, shift along megashears plus separation across rifts, and general dilatation by insertion of new crust). But while satisfying all these conditions, different reconstructions are possible involving different amounts of sinistral rotation. One such reconstruction with very large rotations was presented to the Hobart continental drift symposium. Another reconstruction involving minimal rotation presented in my 1962 presidential address is reproduced here in figures 99 to 102. In these figures an arbitrary grid and its boundary frame have been carried through the stages of the dispersion. This reveals the relationship of the Mendocino-Clipperton group of shears with the San Andreas, the Caribbean, the Venezuela-Guinea and the Amazon shears. It also shows the combination of sinistral torsion with general dilatation.

At present the Honduras-Nicaragua block (Ho) continues eastward as a submerged shelf to Jamaica (J), Hispaniola (Hi), and Puerto Rico (P). Following this back through figures 95 to 98, the evolution of this belt is clear. It is an imbricate zone between two major shears which has become progressively dragged or smeared out as the torsional movement developed. But the general relation of all the parts has remained the same throughout. Initially the imbricate faults within this slice had the trend and movement-sense of the conjugate shears of San Andreas type, but they became progressively rotated as the torsion proceeded.

The shear which forms the northern boundary of the Honduras-Puerto Rico belt just described continues eastwards across the Atlantic to the Gulf of Guinea along the line recently traced by Heezen, and westward it continues along the north coast of Honduras and into the Pacific where it becomes the Murray shear zone. This shear forms the south boundary of Cuba (C), and

99



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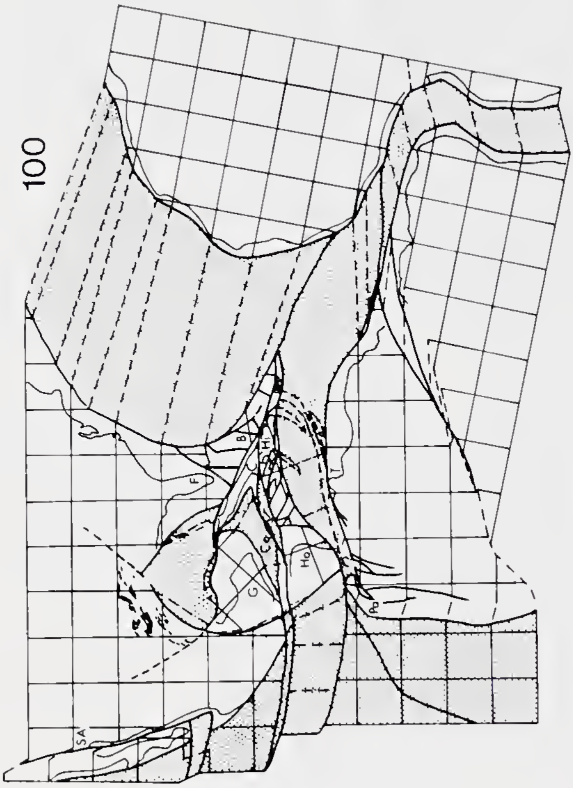

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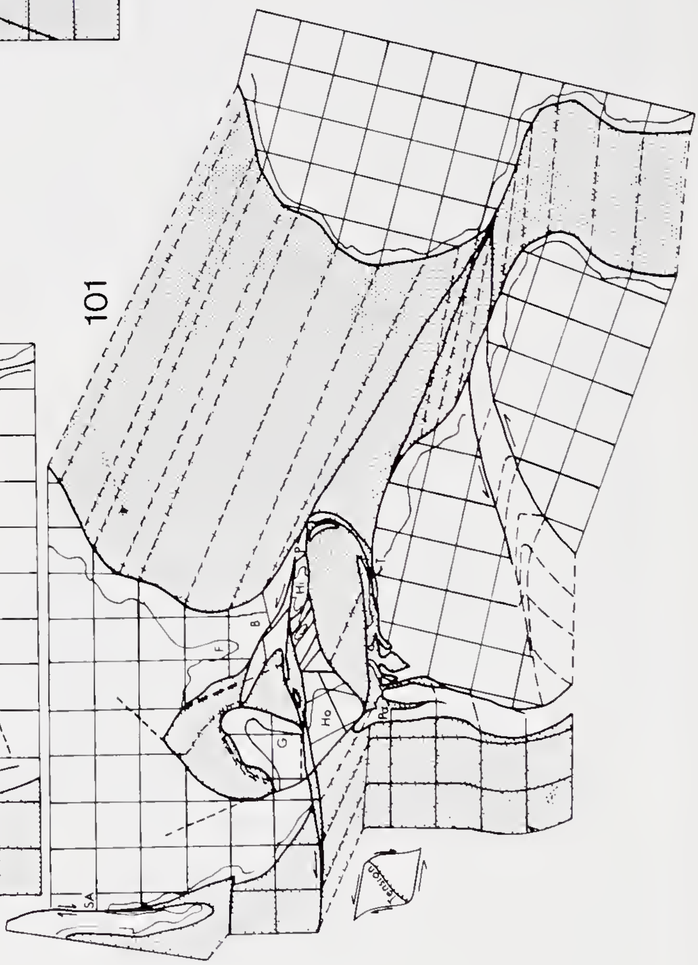

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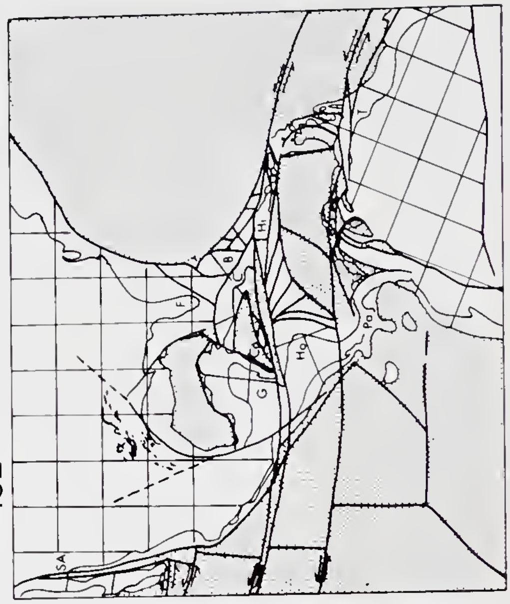


defines the Cayman Shelf (Ca) and the Gulf of Honduras. The diagrams show the development of this structure with progressive sinistral displacement and north-east to south-west extension and anticlockwise rotation (which opens the Honduras sphenochasm).

The Bahamas (B) develop from the southward extension of the Florida shelf with some imbrication in the shear zone and with some sinistral drag and rotation. The Bahamas retain essentially the same relationship to Puerto Rico and Cuba throughout except that there is progressive sinistral slip on the main shear zone north of the Honduras-Puerto Rico zone.

The block, which originally consisted of Guatamala-Yucatan, Honduras-Greater Antilles, and Cuba, originally filled the Gulf of Mexico. On the Texas side there is still a great series of tension faults which make up the Golden Mile, Cerralvo, Balcones, Luling, Mexia, and Tehuacana fault groups, which collectively look like the head of a great landslide scar (Fig. 102). The diothesis movements commenced in the Jurassic and first let the Jurassic sea into this area, gained momentum through the Cretaceous and Palaeogene, and slackened off into the Neogene and Quaternary, although the movements still continue.

A single cause,steady progressive sinistral torsion,applied to figure 95 would produce all the motions and translation shown in the present map. Viewed in this light the complex structure of the Caribbean becomes simple.

SUMMARY OF ASYMMETRIES

The shape of the earth has been compared with a disc (Homer), a ball (Pythagoras), an egg (Cassini), an orange (Newton), a peach *(infra)*, an eccentric sphere (Love), a pentagonal dodecahedron (Elie de Beaumont), a pear (Sir James Jeans, King-Hele), a tetrahedral peg-top (J.W. Gregory) and a potato (Sir George Darwin). Sir John Herschel said it was earth-shaped and it is now called a geoid (Listing), which merely begs the question by saying the same thing in Greek.

Figs. 99-102 (opposite) Stages in the dispersion of the Caribbean region in the Tethyan torsion. B, Bahamas; C, Cuba; Ca, Cayman Is.; F, Florida; G, Guatemala-Yucatan; Hi, Hispaniola; Ho, Honduras-Nicaragua; J, Jamaica; Pa, Panama; P, Puerto Rico; Sa, San Andreas Rift; T, Trinidad. An arbitrary grid drawn on Figure 101 is carried on to the other figures to show dispersion and sinistral rotations.

However her figure may be slandered, Mother Earth does have the following asymmetries:

HEMIHEDRAL

1. Ocean and land are generally antipodal.

2. A north polar ocean surrounded by a northern land girdle and three land rays directed southward, are contrasted with a south polar continent surrounded by a southern ocean girdle and three ocean rays directed northward.

3. A southern circumpolar mid-oceanic rift system sends three rays towards the Arctic.

4. The southern hemisphere is bigger than the northern and this, coupled with the stability of the current poles and general isostasy, indicates a slightly greater mean-density of mantle in the northern hemisphere.

5. The southern hemisphere shows greater post-Palaeozoic expansion and continental dispersion than the northern, and greater current heat flux.

6. Geomagnetic activity is currently significantly greater in the southern hemisphere than the northern. The southern hemisphere has one intensity maximum at the magnetic pole, the northern hemisphere has two weaker maxima 50° apart along a great circle and roughly 180° apart in meridian. The southern hemisphere has a single great-circle agonic line, the northern has two roughly at right angles. The south magnetic pole appears to have an epicyclic motion. The north magnetic pole appears to have a linear oscillatory motion.

7. The southern hemisphere currently has less seismicity than the northern.

EAST-WEST ASYMMETRIES

1. The Pacific Ocean is unique. There seems to have been an *Urkontinent* and an *Urozean*, perhaps a bi-lobed stage preceding the post-Palaeozoic tri-lobed stage in earth development. Heat flow from the Pacific lobe currently exceeds that of the non-Pacific.

2. East is not equal to west tectonically, nor in seismicity.

3. The earth's magnetic field is very far from symmetrical around the equator.

TRI-LOBED AAB ASYMMETRY

Several features old and new, show a three-lobed symmetry about an axis, where the lobes are two and one:

1. The *Urozean* (Pacific) and the *Urkontinents* (Pangaea, Laurasia and Gondwanaland).

2. The oceans: Pacific (concordant) and Indian and Atlantic (discordant rift oceans).

3. The post-Palaeozoic orogens (Fig. 84).

4. The present maxima of total intensity of the geomagnetic field.

HEMISPHERE TORSION

The southern hemisphere has been turned some 50° further east than the northern hemisphere since the Palaeozoic.

OBLIQUITY

1. The earth is at present asymmetric to the ecliptic by 23½°, for which there is no obvious explanation.

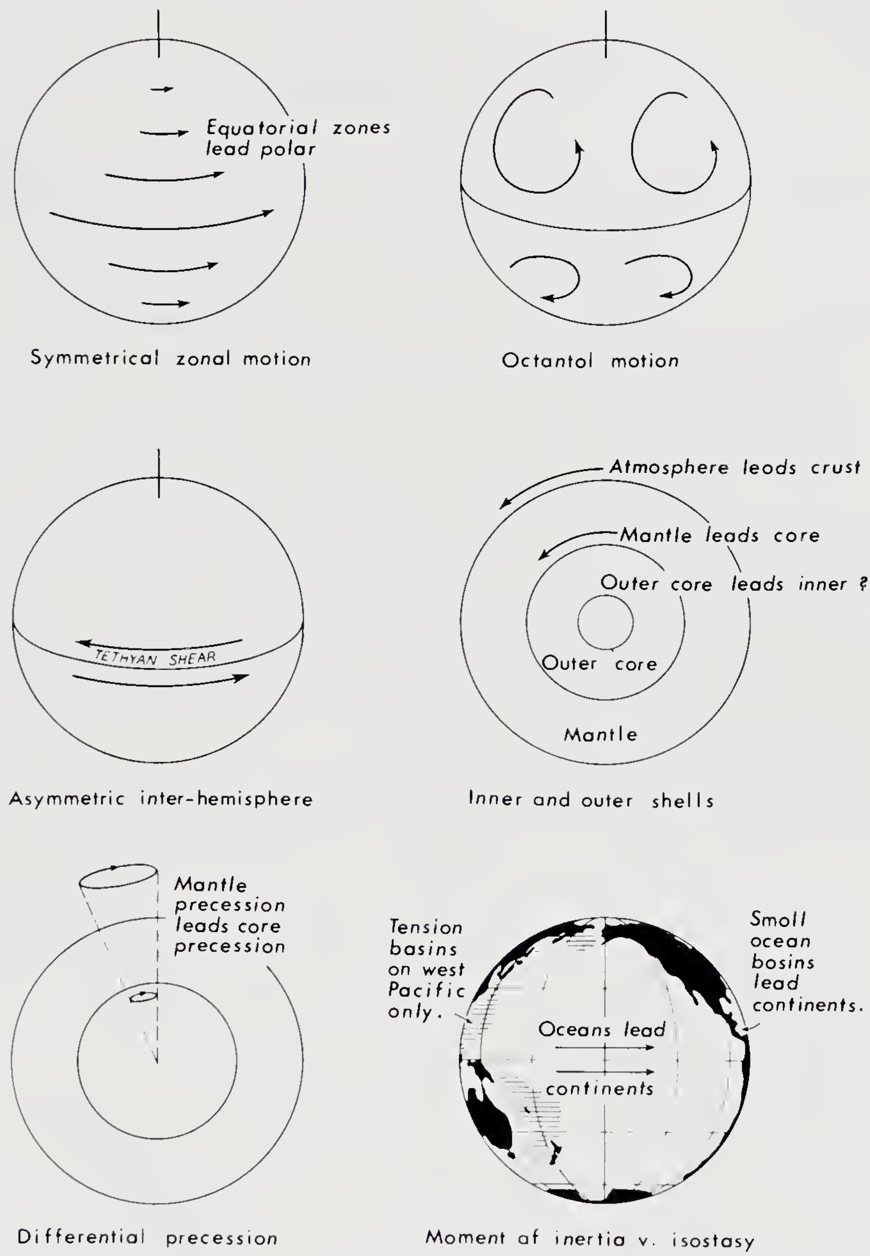


Fig. 103 Modes of differential motion with the earth.

2. The earth's magnetic axis is inclined to its rotation axis at nearly the same angle as the latter is inclined to the ecliptic.

All the asymmetries are of the first order, and involve the whole earth. The contemporary asymmetries of geomagnetism and seismicity are of comparable scale, wavelength, and kind to those which have developed in the near and distant past. This suggests a continuing process rather than an accidental heritage.

ASYMMETRY IS NORMAL

Obliquity of rotation axis to orbital plane is normal among the planets. So also is internal asymmetry about the rotation axis. Jupiter's Great Red Spot must represent such an asymmetry. Moon's figure bulges significantly towards the earth, which implies radial density inequality, because even on Moon, the relaxation time is short relative to age, so that near approach towards general radial *isostasy* must prevail, although significant departure from *hydrostatic* equilibrium is indicated (Fig. 58).

Mars has obliquity and seasons similar to Earth. His centre of mass is offset from the centre of figure, or put another way, Mars has a bulge in one direction. As large departure from isostasy could not persist, radial density difference is implied. This does not necessarily mean radial difference in composition. Radial asymmetry in temperature gradient would shift paramorphic phase boundaries, so that lower densities and surface bulges would follow steeper thermal gradients.

One half of the planet is completely different from the other half. The equator is not exactly the dividing line, but is approximately so. The northern hemisphere is only slightly cratered, the southern hemisphere strongly so. It is difficult to conceive that primordial cratering would have been other than random. The observed difference implies a subsequent asymmetric endogenous process.

The most conspicuous features of Mars are first, the great volcanoes, and second, the Coprates rift zone, a belt of tensional horsts and graben, 300 to 500 km wide, 3 to 6 km deep, extending more than a quarter of the circumference of the planet, on a great circle a little oblique to the present equator, like Earth's Tethyan zone today. Complementary to this rift belt is the region of the great volcanoes. Clearly the crust of Mars is in tension, from internal expansion, with outgassing and some partial melting. Equally clearly this expansion process is globally asymmetric, with steeper temperature gradients with volcanism on one side, and brittle fractures on the other. The present condition of Mars closely resembles an earlier stage of Earth, when the primordial Pacific rift zone, then equatorial, was bursting open half way round the globe to yield the *Urkontinent* and *Urozean*.

Global asymmetry of outgassing, of vulcanism, and of expansion, be it on Moon, Mars, Earth, or Jupiter was inevitable. How could it have been otherwise in view of the automatic feedback properties of heat transport? Radiation and conduction being inadequate, heat must be transported by pervasive gases, and eventually by bulk convection. As viscosity, plasticity and paramorphic phase transitions are directly coupled to temperature, any slight inequality in thermal gradient is automatically amplified at an accelerating rate, as is the rise of a salt dome. Sediments over salt, or syrup over grease are both gravitationally unstable, with much potential energy available to do the work of overturn. Without a slight inequality there are no differentials to start the overturn. But once an inequality appears, the driving force and rate of motion accelerate by feedback.

Hence the tectonic processes of the planets are inherently asymmetric on a global scale.

CONTROL AND CONSTRAINTS OF ASYMMETRY

On a static planet endogenous processes should be radial but random. However the empirical patterns of asymmetry are not random; east-west, north-south, equatorial, axial and inter-hemisphere asymmetries dominate. East, west, north, south, equator, and axis have no meaning except in terms of rotation. Hence rotation is implicated. Indeed in all first-order geotectonic processes, gravity is the power, and rotation is the helm.

Jardetzky (1954) has pointed out that "there is no trace of mechanical equilibrium in any known celestial body", and on the time scale of geotectonics, the earth is wholly fluid except for an eggshell-thin crust.

During the evolution of the planet the following modes of differential motion between its parts must be considered, and probably all have occurred and indeed still occur (Fig. 103): zonal motions, octantal rotations, inter-hemisphere asymmetric differential rotation, differential rotation of shells, differential precession of mantle and core, and differential rotation of continents and oceans.

Zonal motion

Zonal motions, accompanied by concentrated latitudinal shear between zones, have long been known in the patently fluid members of the solar system - Sun, Jupiter, and Saturn. Several authors (Jardetzky, 1929, 1935, 1948, 1949, 1952, 1954; Bogolepow, 1930; Tanner, 1964; Gilliland, 1964, 1973; Nelson and Temple, 1969, 1972; Bostrom, 1971a, 1971b, 1972; and Thomas, 1972), have proposed that, on the geological time-scale, on which the mantle behaves as a fluid (e.g. isostasy), such zonal motions should occur in the mantle, and should and did show up in global tectonic patterns.

On a simple model without variations of moment of inertia or of angular velocity, Jardetzky adopted a terrella involving zonal rotation in the fluid earth, with angular velocity increasing from the poles to the equator. In the early stages of evolution of the planet this would apply to the whole earth. During the present stage, on the time scale of daily rotation, this would apply only to the fluid core, but on tectonic time-scale, to all but the lithosphere. In the adjustment between these stages the thin solid crust would experience anti-clockwise torques in the northern hemisphere, and clockwise torques in the southern.

Most writers on zonal motions within the earth assume eastward motion of the equatorial regions with respect to the polar zones. This is consistent with the zonal motions on Sun, Jupiter and Saturn. On the other hand a rising convection current, or a rising geotumour, should deflect west, and bodies constrained to a shell and moving equatorward should deflect west, although these Coriolis effects may be quite small on the tectonic scale of the parameters involved. On a uniformly expanding earth, the equatorial zone increases the radius of its rotation, and hence its moment of inertia, whereas polar zones move out along the rotation axes. This should result in westward zonal motion.

Octantal Rotations

Rotations of large segments of the earth's surface of the kind suggested in the top right terrella in figure 103, have been inferred by several writers, - e.g. Fujiwhara (1927, p.14), St. Amand (1957b), Benioff (1958) Biq (1958) and herein (pp. 342 to 347, and Fig. 140).

Inter-hemisphere Torsion

Expansion of the earth without other compensation (e.g. differentiation) would increase moment of inertia and retard rotation. Differential expansion between hemispheres would produce zonal shear stresses in a rigid earth, zonal rotations in a fluid earth, and zonal rotations with crustal equatorial shear and block rotations in an earth with a fluid interior and brittle crust. This is indeed what has occurred. All northern continents are displaced west with respect to their southern neighbours.

The Tethyan shear involves a relative displacement of some 1500 km since the Jurassic, which would average about a centimetre a year. The displacement varies significantly along the Tethyan zone, because of differing separations of adjacent blocks in one hemisphere facing blocks in the other hemisphere. Also the rate was far from uniform, and the Tethyan shear was probably most active during the last 60 million years. However even ten times this rate would be reasonable. For example, on much simpler assumptions of zonal motions, Jardetzky (1949) estimated likely inter-zonal speeds along parallels of 19 cm/year and Bondi and Lyttleton (1948) estimated interzonal speeds of 22 cm/year.

At first sight the inter-hemisphere shear appears to be in the wrong sense. If a terrella of dough, with more leavening in the south, were allowed to rise while rotating rapidly, the expanding hemisphere would indeed increase its moment of inertia, and hence tend to slow down and drag *west* with respect to the north. But this terrella differs from the real earth in that its self-gravitation is infinitesimal with respect to its rotational forces, whereas in the real earth gravity is dominant. The half life of regional departures from isostatic equilibrium is a few thousand years. Hence on the time scale of the global expansion the departure from regional isostatic equilibrium remains negligible, except locally where tectonically maintained. Because the centre of gravity of a continent is two and a half km higher than that of an ocean in isostatic equilibrium with it, a continent has greater moment of inertia than an equivalent ocean. The effect of the greater expansion of the southern hemisphere has been to move all continents northwards, so that the northern hemisphere has an excess of continent and the southern hemisphere an excess of oceans, which implies that the northern hemisphere has increased its moment of inertia with respect to the southern as a result of the differential expansion. Hence the northern hemisphere as a whole has moved west with respect to the southern along the Tethyan shear.

The contrast of moment-of-inertia contribution of continental and oceanic segments shows up also on smaller scales. Thus, along the east coast of Asia and Australia the continents tend to retard (west), and the Pacific Ocean as a whole tends to accelerate (east) with respect to it. Hence there are disjunctive seas all the way from the Aleutians to New Zealand. In contrast, along the west coast of the Americas, the continents tend to retard (west) and the ocean tends to advance (east), so there are no disjunctive seas along these coasts. On a still smaller scale the Caribbean is a small new ocean area between the two continents of North and South America - it has therefore grown and migrated eastwards with respect to them. Likewise the new Scotia Sea between South America and Antarctica has moved east with respect to these continents. Similarly the individual disjunctive seas of East Asia have grown eastward with respect to Asia, and those of the Tasman Sea have grown eastward with respect to Australia. In these relative displacements, east (greater velocity) may be replaced by equatorward (greater radius of rotation). Thus the disjunctive seas of east Asia all have an equatorward as well as an eastward component until the equator is reached (Sunda-Banda arc) where only eastward motion occurs.

Because of its greater moment of inertia, a free shell of continental crust would precess at a different rate from a shell of oceanic crust isostatically equivalent to it. Hence precessional torques must operate differentially on continental and oceanic regions. Schweydar (1921) estimated that such torques exceed by two orders the tidal torques of the earth as a whole. On the time-scale of precession (Fig. 121) such torques should produce differential rotations of parts of the earth's crust.

Quantitatively, the excess moment of inertia of continental crust in isostatic equilibrium with oceanic crust is quite significant. The moment of inertia of the earth is $8 \times 10^{44} g.cm^2$. In a terrella with continental northern hemisphere and oceanic southern hemisphere in isostatic equilibrium, the excess moment of inertia of the continental hemisphere would be 1.25×10^{40}, that is 1.5×10^{-4} of the moment of inertia of the whole earth. This is certainly large enough to show up tectonically on an earth with rheological properties such as in figure 121.

The force considered here is related to, but not identical with, the

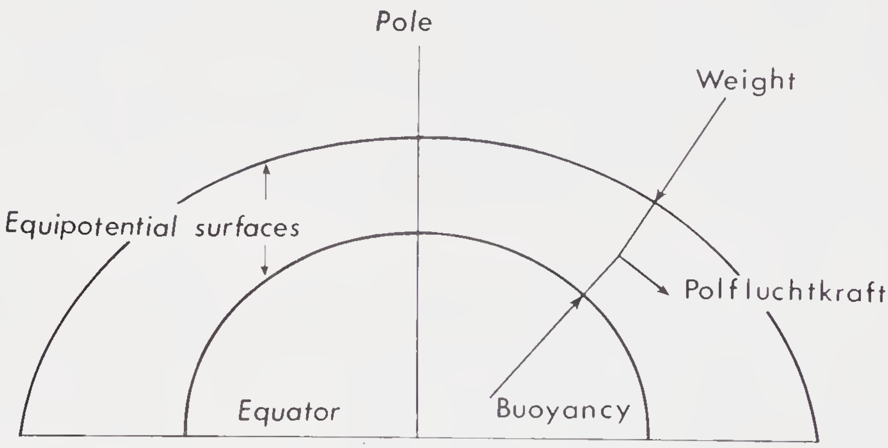


Fig. 104 Polfluchtkraft according to Eötvös.

Polfluchtkraft (flight-from-pole-force) which was first proposed by Eötvös (1913), who argued that, because the centre of gravity of a floating continent is above its base, the gravitational force through the centre of gravity, and the buoyancy force which supports the continent (both normal to the isopotential surfaces at the relevant depths) intersect at an angle (Fig. 104), and hence have a small resultant force tending to drive the continent towards the equator. Epstein, Lambert, Ertel, Milankovitch, and others have made analytical studies of the *Polfluchtkraft* and produced quantitative expressions for its magnitude. Prey (1936) however, has denied the reality of the force, arguing that equilibrium is attained more readily by a slight tilting of the continent and a minute displacement of the continent *towards* the pole. However Prey is only concerned with attainment of radial equilibrium. He does not consider rotational equilibrium and the contribution of the continent to the earth's moment of inertia, nor the effects of differential expansion on rotational momentum.

Migration of Latitudes

Greater expansion in the southern hemisphere implies that the parallels of latitude must sweep southwards across the continents, even though the continents have not moved with respect to their underlying mantle (Fig. 105). Post-Palaeozoic palaeomagnetic data from every continent shows this southward movement of the parallels relative to the continents. These latitude changes reflect insertion of new oceanic crust between them and the south pole, not movement of the continents with respect to their substratum. In the case of India this effect is amplified by its rotation in the Tethyan

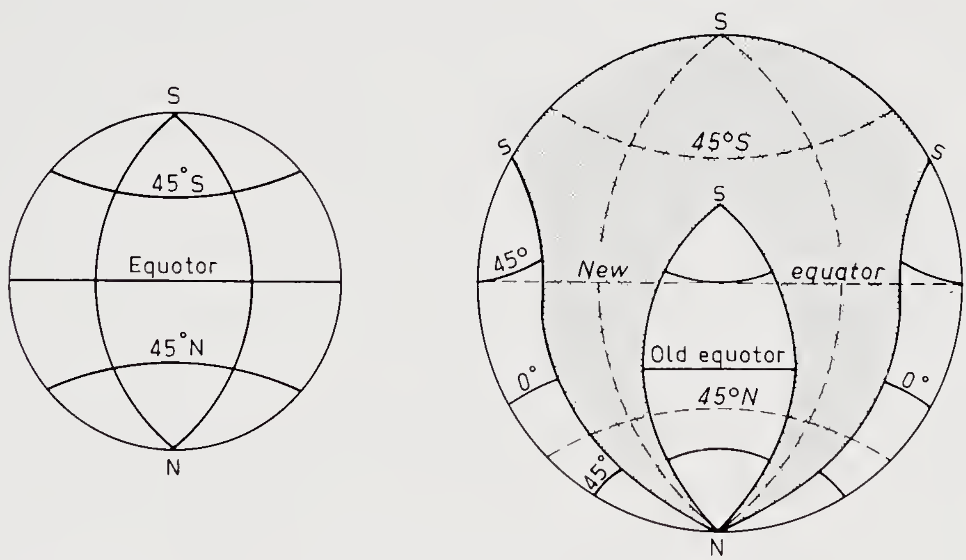


Fig. 105 Migration of parallels of latitude across continents during asymmetrical expansion.

shear zone, with insertion of new crust west and southwest of it. But India was never as far south of Asia as the palaeomagnetic latitude at first suggested.

Migration of Longitudes

Several workers have sought evidence in longitude measurements for the contemporary drift of Greenland away from Europe (see review by Du Toit, 1937, p. 299-301). Koch found that longitude observations made in 1823, 1870, and 1907 showed a progressive westward drift amouting to 1610 m in 84 years, (19 m per year). This result was criticised on the grounds that the early observations used the moon, whose position was not known with sufficient accuracy, that the early chronometers were not accurate, and that the observations were not made at the same point and the intervening survey tie was inaccurate. Jelstrup re-determined the longitude in 1932 eliminating all these criticisms and found an implied westerly shift of 615 m in 62 years (10 m per year). Another set of observations in 1863, 1883, 1922, and 1927 showed a westward displacement of nearly 17 m per year from 1863 to 1922, and of 30 m per year from 1922 to 1927. Tollner-Kopf compared the longitude of Jan Mayen Is, in 1883 and 1933, and found a westerly drift of 25 m per year with respect to Europe. Wegener has pointed out that it would be a most improbable coincidence if these consistent results were all in error and all in error in the same sense, and all within the span of average rate of separation of the respective continents since the Jurassic. Baracchi (1914) reported a consistent decrease in the longitude of Sydney in observations in 1883, 1904, and 1926, indicating an average westerly movement of 3 m per year. Littell and Hammond reported that the longitude of Washington had increased (westerly displacement) by 0.32 m per year over thirteen years. Stoyko (1942, 1959) has extended these investigations.

Torao and his co-workers (Torao *et al.* 1963, 1964, 1968, 1969) studied longitude determinations over thirty years from observations at Greenwich, Hamburg, Paris, Potsdam, Uccle (Belgium), Ottawa, Washington, Richmond (Va.), Buenos Aires, Tokyo, and Mizusawa (Japan). They found that the geocentric longitude angle between Tokyo and Buenos Aires had increased by 1.51 ms per year, between Washington and Tokyo by 1.05 ms per year, that between Tokyo and the European observatories showed no significant change, and that between Washington and Europe had decreased by 1.25 ms per year. These results, taken literally, would suggest that the Atlantic and Pacific Oceans are currently widening at similar rates, whereas Eurasia is not currently widening. However, although the results over the thirty-year interval were indeed large enough to be significant, Torao and his colleagues were reluctant to interpret their results in terms of relative displacement of continents, ostensibly because trends were not all consistent when taken over shorter intervals, there were some errors and uncertainties in time-keeping, and there were discrepancies between different star catalogues for the positions and proper motions of some stars used, but, I suspect, intrinsically because no-one would have believed such a conclusion in the climate of the time.

All such studies have involved the implicit assumption that the radius of the earth is constant. For example, if the earth were expanding uniformly, the Atlantic and Pacific would be widening and the continents would be separating without any change in their longitudes. If the Pacific sector were widening more rapidly than the Atlantic, the longitude difference between Greenland and Europe could be diminishing while the ocean between them was widening! The secular decrease in the longitude of Sydney could mean that the major arc crossing the Pacific and Atlantic Oceans from Australia to west Africa is expanding more rapidly than the complementary minor arc.

ROTATION OF THE EARTH

The earth rotates about an axis tilted to the ecliptic at an angle of 23½°, and this tilted axis precesses with a period of 25,700 years. Apart from some very minor nutations and wobbles, there, for most people, the matter ends.

One of the most profound mysteries of the universe is that everything in it rotates. So said Eddington. Jardetsky (1949, p.797) remarked that "the simple hypothesis that the planet rotates like a rigid body, should have been abandoned at the end of the nineteenth century". Munk and MacDonald (1960, p.15) commented: "The problem of stress and strain plagued Kelvin and George Darwin. Kelvin assumed the Earth could be treated as an elastic body even for long-period deformations, while Darwin assumed that the Earth behaves plastically, even for small stresses. Today the situation is identical to that of 1900, and proponents of the Darwin and of the Kelvin view can be found. There are few problems in geophysics in which less progress has been made." For Darwin (1908, p.117) the word "earth" was an abbreviation of the expression "a homogeneous rotating viscous spheroid." Both Kelvin and Darwin were empirically correct, in particular time-fields (see Fig. 121). For tectonic phenomena the Darwin terrella is more often appropriate.

Few theoreticians have studied the effects of secular yield of the earth to rotational perturbations, of the energy loss from even trivial internal friction, of asymmetric differentiation of the earth with expanding geotumours (such as led to the disruption of Pangaea), or of gross tectonic displacements generally, or of earth expansion in particular. Because continents in isostatic equilibrium contribute more to the moment of inertia than equivalent oceans (as explained below), the dispersion of continents (whether by "plate" or expansion models), must significantly perturb the rotation. It is necessary therefore to review theoretical terrellae to see what limitations are imposed by physical principles, then to appeal empirically to the geological record. Such a study, of course, leads to the conclusion that the rotation rate and the position of the pole on the crust have changed through time, but in addition the obliquity of the axis to the ecliptic becomes a fundamental geological variable, and the rotational consequences of expansion, and particularly of asymmetric expansion, lie at the root of first order geotectonic phenomena. Accordingly, a study of the rotation of the earth, the activation, periods, and damping of the many

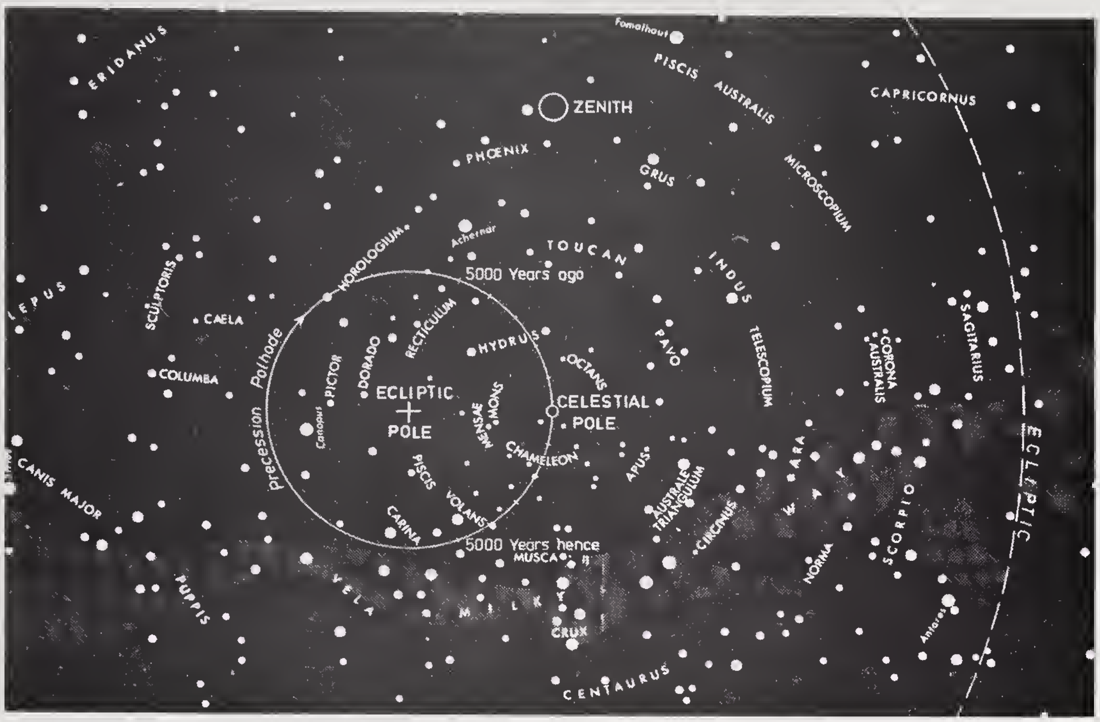


Fig. 106 All-night image of Hobart sky as with camera pointed up to the south pole with the shutter left open.

Fig. 107 Path of the pole through the constellations because of precession of the equinoxes.

short term and secular perturbations, the differential rotation of its parts (Fig. 103), and secular changes in moment of inertia, become fundamental to a course in geotectonics.

This chapter is certainly incomplete, and probably in parts wrong. I will correct it as I know where. The earth models used by the applied mathematicians are not the real earth. Secular behaviour of the real earth has not been solved. The yawning gulf between Kelvin and Darwin is as wide today as it ever was. Even their divergence concerned only deformation, and ignored other major variables such as expansion and asymmetric thermally-induced bulges like the geotumour which swelled under most of Pangaea during the early Mesozoic. Within these wide freedoms lies the actual rotation history of the real earth, whose consequences in the evolution of the earth's crust must have been profound.

Many tectonicists, notably Peyve (1961) and Kropotkin (1970) have proposed that the energy for tectogenesis and mass crustal displacement flows ultimately from gravitational perturbations of Earth's rotation by her neighbours.

NOMENCLATURE

Rotation is the spinning of the earth on its axis (Fig. 106). *Revolution* is the movement of the earth round its orbit.

Axes and Poles

At any instant a spinning body has an *axis of rotation*, but the position of this may change. The point where this axis of rotation pierces the constellations is the *celestial pole* (Fig. 107). The axis of rotation may, or may not, coincide with the *axis of maximum moment of inertia*. In a body such as the earth, which yields to stress differences, the centrifugal force causes the rotation equator to bulge, thus increasing the moment of inertia about the rotation axis; the rotation axis rotates about the inertial axis, and these two axes tend to approach each other through deformation of the earth. Each of the motions of the earth (rotation, precessions, nutations, wobbles, polar wander, sway, etc., such as are described below) has angular momentum. The sum of all these angular momenta defines an axis in space (relative to distant stars). This *axis of momentum* is fixed except in so far as some torque, external to the earth, may change it. Late last century a group of observatories along latitude 39°8'N made continuous measurements of their angular distances from the axis of rotation, and hence determined the position of the rotation pole. The distance of this pole from each observatory was found to move from month to month by a few metres. This would be most inconvenient to precise time-keepers, cartographers, navigators, and astronomers, and others who need stable coordinates for all points on the earth's surface, and stable time-marks. To satisfy these needs, the mean position of the pole, determined by the observatories of the International Latitude Service during the years 1900-1905, was adopted as the *geographic pole* for all such purposes, and is known as the *Conventional International Origin* (CIO). The problem did not arise until 1892 when the position variation of the rotation pole was proved. Had this point been determined a decade or a century earlier, or later, a different point would have been adopted, but having adopted it, this point remains permanently the geographic pole. The great circle through the CIO and Greenwich observatory was adopted as the zero meridian, and the great-circle normal to the CIO axis became the co-ordinate equator. The latitude service (Bureau international de l'heure) determines, from the latitude variation of some fifty observatories, the changing latitude and longitude of the instantaneous rotation pole and hence instantaneous equator. Thus sidereal time and true co-ordinates can be corrected for any particular time and place of observation whose geographic co-ordinates are known.

A stake could be placed at the north geographic pole (CIO). A second stake could be placed a few metres away which would mark the point about which the earth is rotating at that instant. A third stake could show the axis of maximum moment of inertia. A fourth stake could show the axis of total angular momentum. Except for the movement of the polar ice in the short term, and secular crustal movements in the long term, the first stake could be fixed and permanent. But it would have no intrinsic significance beyond being a definite point from which to measure. The second would have to be moved a little every day. In the course of a year, it would have almost completed a loop around the first stake (see Fig. 116), and in the course of a century it would also have wandered substantially away from the first stake. But it would always be significant, as it indicates the axial point about which the earth is actually rotating. The third stake would shift more slowly. It tends to move towards the second stake but is heavily damped by the deformation stiffness of the earth. Although the momentum axis is fixed where it intersects the distant stars on the celestial sphere, nevertheless the fourth stake would have to be moved about on the earth's surface as angular momentum is exchanged between the several perturbations of the earth's rotation.

The path of a pole with respect to the geographic pole (CIO) is called the *polhode* (from Greek ὁδός : route).

Ecliptic

The plane of the earth's orbit is the *ecliptic*. The point where a line from the earth's centre of mass normal to the ecliptic pierces the constellation is the *ecliptic pole* (Fig. 107). The orbit is a nearly circular ellipse (eccentricity 0.017) with the sun at one focus. At the ends of the long axis of this ellipse the earth is nearest *(perihelion)* and farthest *(aphelion)* from the sun (Fig. 112). Similar points for the moon with respect to the earth are called *perigee* and *apogee*, and for satellites in general are *periapsis* and *aphapsis*. The angle of inclination of the earth's rotation axis to the normal to the ecliptic is the *obliquity*. The two points of the orbit where the sun lies in the plane through the earth's axis and the normal to the ecliptic through the earth are the *solstices* (literally sun-stands, because here the sun reaches a position vertically over either the tropic of Cancer or the tropic of Capricorn, then moves back towards the equator and the other tropic). The points where the obliquity is along the orbit are the *equinoxes* (as the sun is vertically over the equator and day and night are equal). In other words, solstice is when the sun's declination equals ± the obliquity, and equinox when it is zero. The line of intersection of two orbital planes (e.g. moon's orbit and ecliptic) is called the *line of nodes*.

Precessions

The direction of the obliquity of the rotation axis to the ecliptic suffers a large number of precessions, nutations, librations and wobbles. In general, *precession* is used for the large amplitude motion, *nutation* for small angle nodding of the axis which modulates this large motion (Latin *nutare* : to nod), and *libration* is used for similar small motions (from Latin *libra* : a balance, referring to the oscillation of the beam of a balance). But precession is used by many writers for all such motions, and all terms have been used irrespective of whether the motion is free or constrained (see below), or caused by external torques or internal redistribution of mass. *Wobble* is usually reserved for motions of the latter type. The earth's rotation is clockwise (looking up from the north pole) and the main motion of the rotation axis is anticlockwise (see Fig. 110) so the equinox occurs three seconds earlier each year - that is, it precedes by this amount. Hence this motion of the axis was called the *precession of the equinoxes*, and the term precession was later extended to include all such motions of the axis of rotating bodies, e.g. tops, gyroscopes, etc.

Because of this precession, the *sidereal year* - the time for a complete revolution of the earth as seen from a star (Latin, *sideris* : of a star) is 365.2564 mean solar days, whereas the *tropical year* - the year as seen from the earth (from Greek τροπή : the turning - of the sun at solstice) is 365.2422 mean solar days. This small discrepancy was observed more than a century B.C., by Hipparchus of Rhodes and Alexandria, the founder of scientific astronomy.

Free and Forced Periods

Distinction is necessary between free periods and forced periods. If a number of rods hang tandem from one another, it matters not whether we belt them with a bat or pull them gently aside and let them go, the oscillation period will be compounded of the pendulum periods of the parts and of the whole. Likewise Earth has a series of natural periods governed by mass, moment of inertia, rotation rate, gravity field, deformation properties etc. The elastic oscillations of the whole earth after a large earthquake, and the Chandler wobble described below, are such free periods. However where the earth receives an impulse repeated at a fixed rate, forced wobbles or nutations occur at the period of the repetitive perturbation. The small tidal nutations, annual wobble, and the principal nutation (18.6 years forced by the regression of the lunar line of nodes) are examples of such forced nutations.

PRECESSIONS, NUTATIONS, LIBRATIONS AND WOBBLES, AT FIXED OBLIQUITY

Precessions

The earth's daily rotation, the celestial pole, local latitude, and local meridian can be measured simply by pointing a camera vertically and leaving the shutter open all night at the time of new moon (Fig. 106). Disregarding some other perturbations to be discussed below, repetition several thousand years later would yield substantially the same latitude and meridian, but the axis of the earth would point to a very different celestial pole (Figs. 107 and 109). (ω *Carinae* will be the south pole-star four thousand years hence, and γ *Doradinis* was the south pole-star eight thousand years ago). This movement of the celestial pole through the fixed stars is due to the fact that Earth precesses like a top (Fig. 108). A top without spin would fall over because of the couple made by its weight and the reaction of its peg, but the same torque applied to a spinning top causes it to precess (Fig. 108).

Synge and Griffith (1949, p.429) have written: "Why does a spinning top not fall down? How does its rapid rotation render it apparently immune to the force of gravity, which makes non-spinning bodies fall? It is difficult to give a simple answer to this question. The only way to explain the phenomenon is to construct the mathematical theory of the top". It is true that precessions and nutations are usually explained in terms of the product of vectors. Nevertheless precession does follow simply and directly from Newton's first law of motion - that every body continues in its state of rest or uniform motion in a straight line except in so far as some external force changes that state.

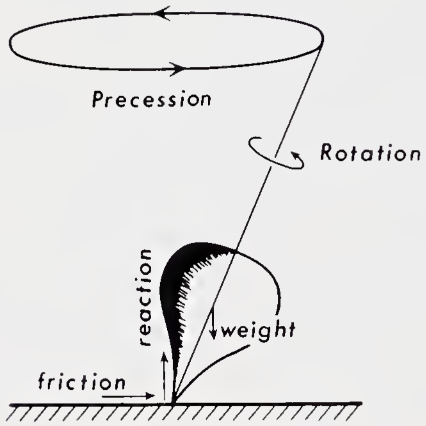


Fig. 108 Precession of a spinning top.

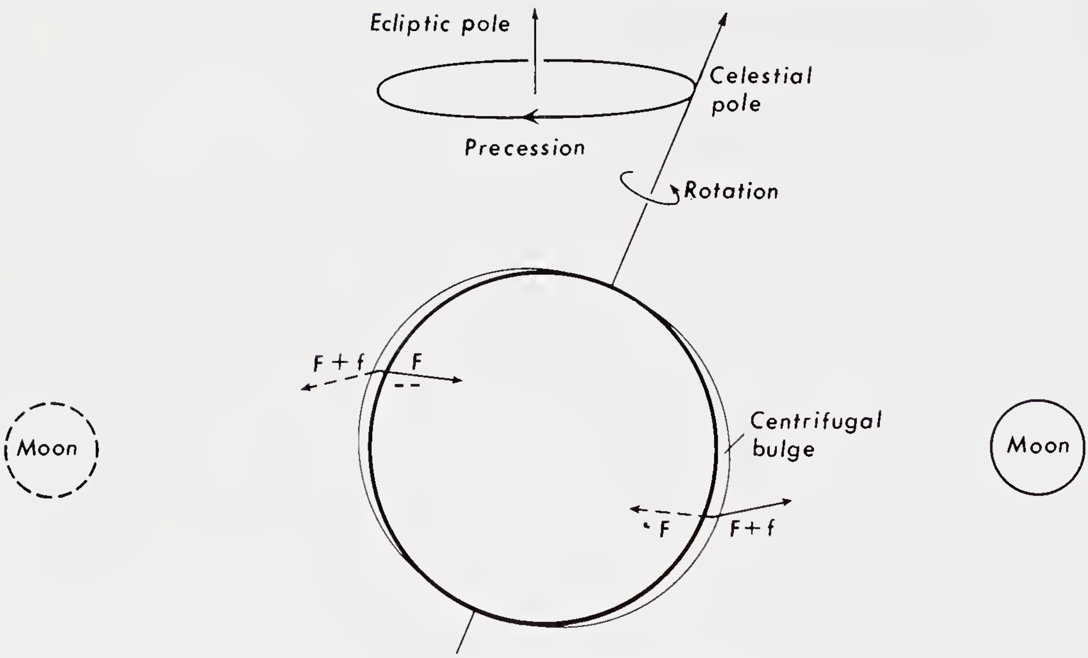


Fig. 109 Precession of the earth caused by the differential attraction of the moon for the near and far sides of the earth's equatorial bulge.

Consider a disc (Fig. 111) spinning about axis O normal to the page, and subject to an applied torque tending to lift the lefthand side and depress the righthand side. All parts of the disc in the bottom left quadrant are being carried by the spin further from the axis of the applied torque, where they would need to increase their velocity to turn with the torque and remain in the disc. As there is no differential force to help them do this, they therefore tend to be left behind with respect to the torque-induced motion. As the torque tends to lift this quadrant out of the paper, the spin and torque combination tends to make them go down. In the top left quadrant the spin is taking all particles closer to the torque

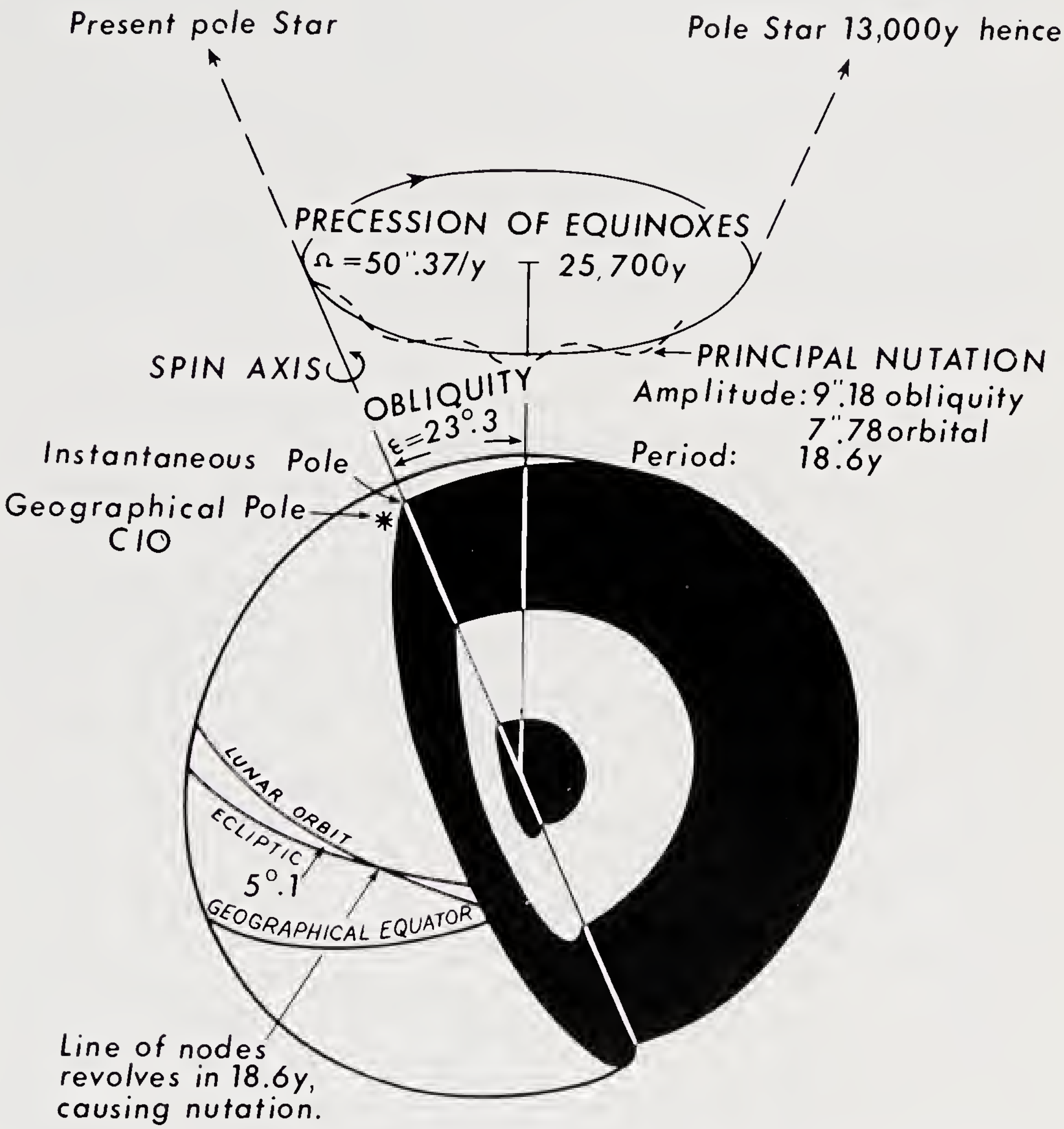


Fig. 110 Precession and nutation of the earth (after Rochester, 1973).

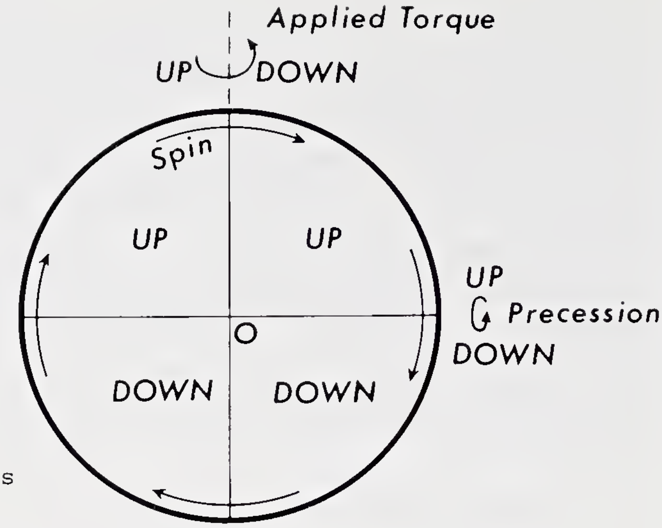


Fig. 111

Precession in terms of Newton's first law of motion.

axis where they find themselves travelling too fast with respect to the torque rotation. Hence they tend to overrun the torque. As the torque tends to lift them, over-running means that they tend to rise. Similar analysis shows that the top right quadrant tends to rise and the bottom right quadrant tends to go down. Hence the combination of the spin momentum and the torque is to cause the disc to rotate (precess) lifting the top half of the disc, and depressing the bottom half of the disc.

The centrifugal force of the earth's rotation causes an equatorial bulge, so that dynamically it may be likened to a sphere with an equatorial annulus. The attraction of the sun tends to turn this annulus into the plane of the ecliptic, and the moon similarly towards the plane of its orbit. This effect is illustrated in Figs. 109 and 112). At solstice (when the tilt of the earth's axis is towards the sun) the sun's attraction on the near side of the annulus is greater than its attraction on the far side of the annulus because the former is nearly thirteen thousand km closer to the sun. The difference is a small residual attraction along the line joining the sun's centre to the earth's near-side equator, which does not pass through the earth's centre of mass. If a body is pulled by a force not passing through its centre of mass, it tends to turn that body until the centre of mass lies in the line of the force. Therefore the sun's attraction tends to turn the earth's axis towards becoming normal to the ecliptic. In the same way at the other solstice the sun's greater attraction on the nearer side of the equatorial annulus still tends to turn the earth's axis towards normality to the ecliptic. The magnitude of this tidal torque varies as the inverse *sixth* power of the radius vector, and hence as the sixth power of the sine of the obliquity. The effect of this erecting torque on the spinning earth is to make it precess. The attraction of the moon acts similarly but is four-fold greater (Fig. 109). The erecting torque of the moon fluctuates fortnightly from a maximum when the earth's axial tilt is directly towards the moon to a minimum when the earth's axial tilt is in a plane normal to the moon-earth line of centres. The sun's erecting torque similarly fluctuates semi-annually. But the sense of the torques in all configurations is always to turn the rotation axis towards normal to the ecliptic. One precession cycle of the earth appears to take 25,700 years. But, during this time the long axis of the earth's elliptical orbit turns 8'8".3 owing to a relativistic effect predicted by Einstein mechanics but not by Newton mechanics (Fig. 113), so that when the precessing pole has, in fact, completed one revolution it has 15 years further to go before it appears to us that a revolution has been completed.

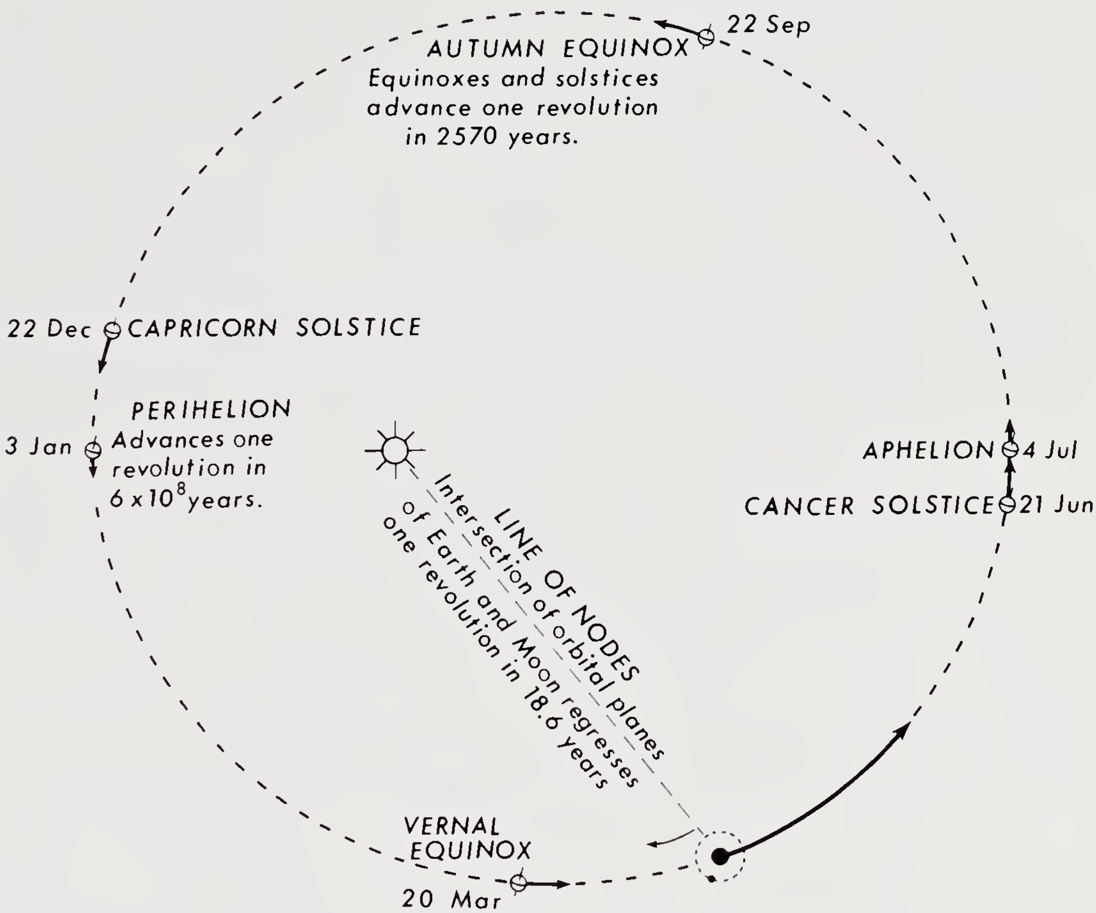


Fig. 112 Perturbations of the earth's orbit.

A top, set spinning on a frictionless surface, would precess about its centre of mass, as does the earth. On a rough surface a horizontal friction force acts on the peg, so that the top precesses about a point between the centre of mass and the peg, while the peg traces out a small circle on the surface. When the top is pivoted at the peg, the top precesses about that point.

Free nutations

In general, the spin axis of a precessing body such as a top does not trace out a simple circular cone with a fixed obliquity to the mean axis, but, under appropriate conditions, the angle of obliquity waxes and wanes between two libration limits, so that the rotation axis traces out an epicycloidal path (Fig. 114). The result is that the rotation axis appears to nod up and down (nutate) as it precesses. During this motion the total energy of the system is the sum of five parts: kinetic energy of spin (Eks), kinetic energy of precession (Ekp), kinetic energy of nutation (Ekn), potential energy against external couple (Ep), and energy dissipated to the environment (e.g. through friction); energy may be transferred between these components.

The relative magnitude of these energy components determines whether the spinning body (a) spins stably without precession, (b) spins with precession and without nutation, or (c) spins with both precession and nutation. According to Jeans (1907, p. 310-317) and Stephenson (1960, p.220) the conditions for states (a) and (b) are in each case that

$$\mathrm{Eks} \geqslant 2\,\frac{\mathrm{Ia}}{\mathrm{Ic}}\,.\mathrm{Ep}$$

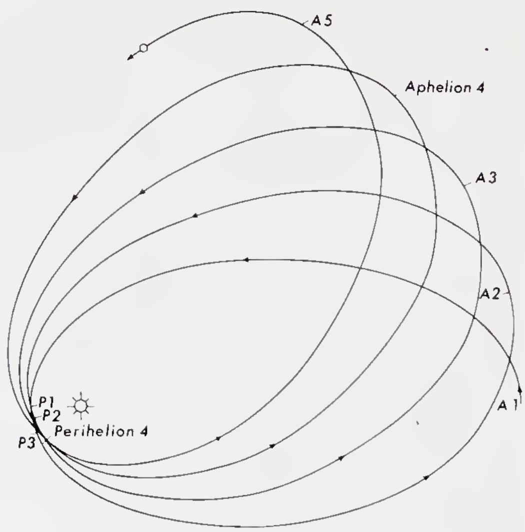


Fig. 113

Relatavistic rotation of planetary orbit.

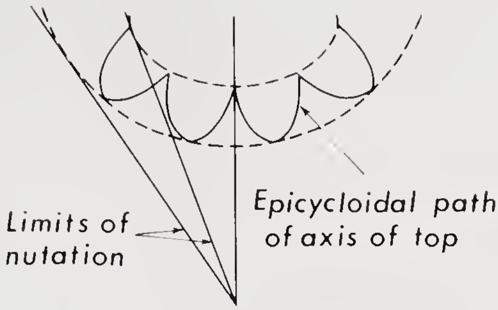


Fig. 114

Epicycloidal path of free nutation (after Scarborough).

where Ia and Ic are the moments of inertia of the body about the relevant axes. For a quasi-spherical body (such as the earth) Ia is nearly equal to Ic so the above condition reduces almost to the condition that the spin energy should be more than double the potential energy.

When first thrown, the kinetic energy of spin of a top far exceeds the potential energy, so that its motion is stable without precession or nutation, if it were set down carefully in an erect position. However it is normally set down at an angle, often with translation momentum as well, and the fast rotation greatly reduces the effective friction on the peg. (Actually the friction remains nearly constant but at high rotation rates the frictional losses are a very small part of the energy). Hence it travels bodily in a large circle, precessing at attenuating rates, and *without* nutation, while it erects itself towards the stable vertical position (Fig. 115a). During the first stage, spin kinetic energy is lost to friction on the peg, and against the air, and in doing work against gravity to increase the potential energy to a maximum in the erect "sleeping" position (the second stage) (Fig. 115b). So long as the kinetic energy of spin is more than twice the maximum potential energy, any small perturbations are attenuated by return to the "sleeping" position, just as the original obliquity was attenuated. In this stage spin kinetic energy is progressively dissipated by friction until it drops below twice the potential energy, so that the third stage commences. Now spin energy is insufficient to correct small perturbations, which therefore become precessions, in this case, *with* nutation. This stage continues with increasing obliquity of precession and increasing libration amplitude of nutation until the top falls over inert. The whole of the kinetic energy has been dissipated as heat and air molecular motion, and the whole of the top's angular momentum has been transferred to the earth as a minute increment to its polar wobble.

Several nutations are identified for the earth, but unfortunately the nomenclature is loose and none of these are free nutations in the strict

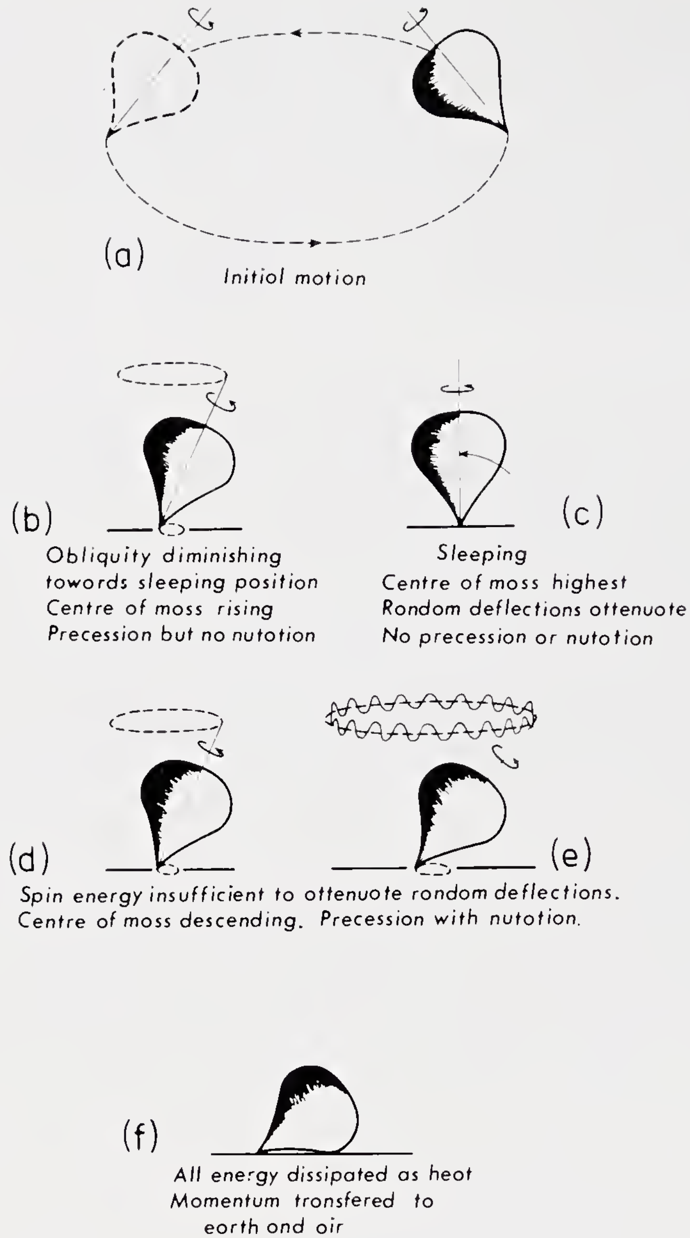


Fig. 115 Sequence of states of a spinning top.

sense just explained, although they are nutations in the etymological sense, in that they appear as minor nodding of the obliquity of the earth's axis. The Chandler wobble described later is often referred to as the "free nutation". This description is valid in the two senses that it is a "free" non-forced oscillation and that the rotation axis nods or nutates. But it is a wobble not a small precession modulating the primary precession. Clearly the rotational kinetic energy of the earth is many times greater than the potential energy of the lunisolar attraction of the equatorial bulge. Hence the earth cannot have a nutation in the sense just described for a top. The earth's "principal nutation" and its several smaller nutations are forced nutations.

Forced nutations

The principal nutation of the earth causes the obliquity (mean 23°.3) to increase and decrease by 9".18 with a period of 18.6 years. In the direction of the orbit the nutations appear to speed and retard the rate of precession, which results in a Doppler-like shortening of the wave-length of the nutations, so the amplitude of the principal nutation in this direction appears as 7".78 (Fig. 110). Other similar minor nutations with amplitudes less than 1" have periods of 9.3 years (half the principal nutation), annual and semiannual (which are caused by the variation of the solar couple at perihelion and aphelion), and fortnightly (due to similar variation of the lunar couple at perigee and apogee).

All these nutations are forced nutations caused by cyclic variations in the magnitude of the precessional torques exerted by the sun and moon. They are therefore all tied to the periodic motions of the sun and moon with respect to the earth. The principal nutation is caused by the regression of the line of nodes. The moon's orbit is inclined 5°.1 to that of the earth. The line of intersection of the orbital planes of Moon and Earth turns backwards (with respect to the sense of rotation of the earth and revolution of the moon) with a period of 18.6 years (Fig. 112). This regression of the line of nodes causes a small variation in the lunar torque as the perigee and apogee come into the plane of tilt of the earth's axis -- hence the principal nutation. The 18.6 year cycle is referred to as the *Metonic cycle,* after the Athenian (Μετων) who discovered it.

Wobbles

In addition to precession and nutations, which are caused by external torques, the earth has wobbles, which are caused by redistributions of mass, such as motions of atmosphere, water-tides, ice-caps, seasonal changes of water-table, erosion and sedimentation, vulcanism, continental drift, and tectonic movements. Reverting to figures 106 and 107, the latitude of the observer does not change during precession, although the axis of the earth migrates through the constellations, and the declinations of stars do change.

This means that a stake could be set up at the rotation pole, and the earth would continue to rotate faithfully about this mark, notwithstanding precession and nutations. But wobbles caused by internal readjustments cause the rotation pole to move, so that there does not, in fact, exist any fixed point where a stake could be placed to mark the rotation pole permanently. Such wobbles alter the latitude of all observatories, but not the declinations of the stars. By measuring the latitude of several observatories, the position of the rotation pole at any time can be determined. But repetition of the measurements a month later or years later does not yield precisely the same point. Strictly, conservation of angular momentum requires that a wobble also induces a nutation (and vice versa), as Rochester *et al* (1974) have emphasised, but their amplitudes are of different order.

Polhodes of the actual rotation pole with respect to the conventional pole (CIO), during two three-year intervals through half a century, are shown in figure 116; these reveal some significant phenomena:

(1) During each of the intervals 1916-1919, and 1969-1972 the actual rotation pole moved in a spiral path about the conventional origin.

(2) Only about five-sixths of a cycle is completed in a year, that is, the cycle takes seven-sixths of a year, or about fourteen months. This cycle is known as the *Chandler Wobble*.

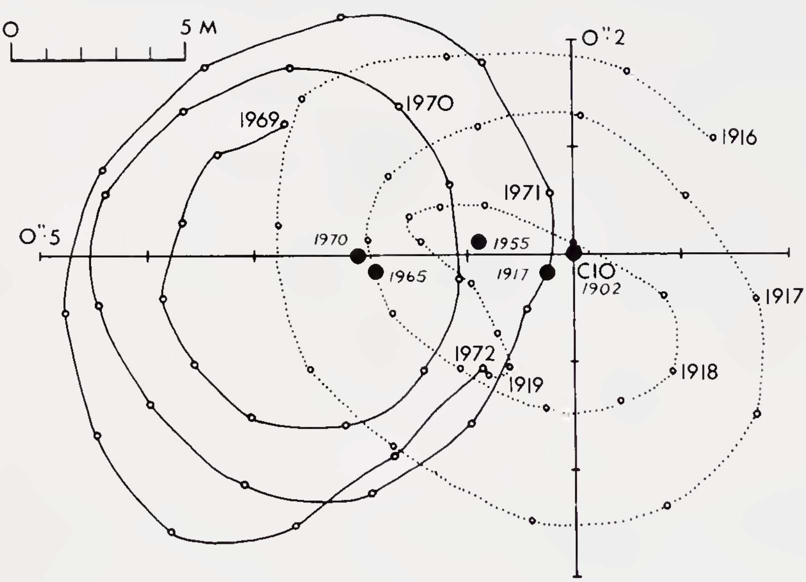


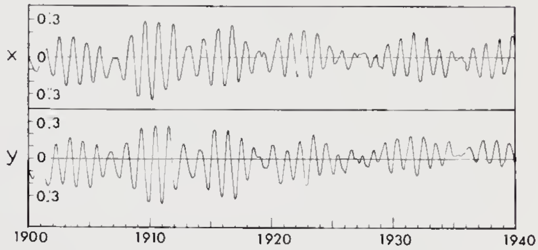
Fig. 116

Polhodes for two 3-year intervals 1916-1918 and 1969-1971 showing (a) the Chandler period, (b) the beat of the Chandler and annual wobbles (the amplitude was decreasing during the 1916-1918 interval, but increasing during the 1969-1971 interval), and (c) the slow pole migration of about 0".03 per year. The filled circles indicate 3-year means, which filter the Chandler and annual wobbles from slower secular polar wander.

(3) The amplitude of the polar motion is not constant. During the 1916-1919 interval the amplitude was diminishing at about 0".05 per year, whereas during the other interval the amplitude was increasing. However this waxing and waning of the amplitude of the polhode is due mainly to beating of the Chandler wobble with the seasonal forced wobble (Fig. 117). Munk and MacDonald (1960) computed the root mean square of the Chandler amplitude (after separation from the annual wobble) to be 0".14, with a period of 1.19 years.

Fig. 117

Beating of Chandler and annual wobbles. x is positive from CIO towards Greenwich y is positive from CIO towards Chicago (after H. and Y. Labrouste).



(4) Both polhodes are elliptical rather than strictly circular. For the 1916-1919 period the long axis was roughly 50°E. For the 1969-72 period the long axis was roughly 30°W. The cause of the ellipticity is not definitely known. Theoretically the pole tide should cause slight ellipticity, but less in amount than is observed in figure 116, and approximately constant in longitude. Asymmetry in the equatorial moment of inertia of the earth could cause ellipticity, though one would expect its orientation to be constant. Asymmetry of tectonic mass redistribution could cause ellipticity but hardly on this short time scale. Undirectional polar wander combined with a circular wobble path would yield an elliptical polhode. This interpretation would imply a secular motion of about 0".1 per year and the ellipticity would be consistent in orientation. A slower wobble superimposed on the Chandler and annual wobble would cause the orientation of the ellipticity to rotate with the period of the secular wobble. The *Markowitz wobble* (see later) which shows up when the mean rotation pole for each year is plotted, has too long a period and an order too small an amplitude to be responsible for this ellipticity. The time scale plus the sharp difference in orientation between the waxing and waning polhodes of figure 116 indicate that a beat phenomenon is involved -- the interference of the Chandler and seasonal wobbles. In subtracting the annual wobble from the Chandler wobble the mean amplitude of the seasonal wobble is used, and the seasonal wobble is assumed to be uniform, for which there is no evidence. The annual wobble, which shows up in the monthly means of the polar motion, does not vary much from year to year, is itself elliptical

with its major axis (coincidentally) on the Greenwich meridian (Rochester, 1970). This suggests that the Pacific-nonPacific asymmetry of the earth is involved.

(5) There is a secular shift of the axis of the Chandler wobble. During the three-quarters of a century covered by this figure, the mean position of the rotation pole has migrated 0".25. If the international conventional pole were determined now, an origin would be adopted 7.4 metres nearer Rangoon and further from Chicago than was adopted in 1905. This indicates a slower long-term wander of the pole than the Chandler wobble. A migration of a quarter of a second in seventy years may seem trivial, but if it were steadily maintained, the pole would migrate from its present position to the present equator in ninety million years. Such a migration of the rotation pole with respect to the adopted geographic pole may not differ in kind from a wobble; but where the scales of distance (> 10m) or time (secular, geological) are large the movement is referred to as *polar wander*.

The period of the Chandler wobble provides an example of the conflict of empiricism, with theory based on too simple a terrella. Euler in 1765 calculated theoretically that the earth would wobble with a free period of 305 days (ten months) if an asymmetric disturbance occurred; so, many astronomers, including Peters, Bessell, and Maxwell, searched in vain for a latitude variation with such a period. Finally in 1891 S.C. Chandler, a Boston actuary and amateur astronomer, found two wobbles, one of 12 months and one of 14 months. Chandler's result was not accepted -- after all, he was an amateur, and many distinguished professionals had failed to detect any such oscillation. A year or so later systematic latitude measurements at Honolulu and Berlin (180° apart in longitude) confirmed a latitude variation of 0".5, with the period determined by Chandler; whereupon Newcomb (1892) pointed out that Euler had assumed a rigid terrella, whereas the elasticity of the earth would extend the natural period from ten months to fourteen months. When a result is anticipated, geophysical theory manages to fit it!

Spectral analysis of the polhode shows a sharp period corresponding to the seasonal wobble, but rather a broad frequency spread over several weeks for the Chandler wobble (Fig. 118), which has stimulated much discussion, because there is no apparent reason for such variation. The periods should coincide either with a cyclic process which causes repetitive mass redistribution (such as the seasonal and tidal wobbles), or with the free period of the earth, represented by the Chandler wobble. However the apparent spread of the Chandler period may be an artifact, because the number of cycles

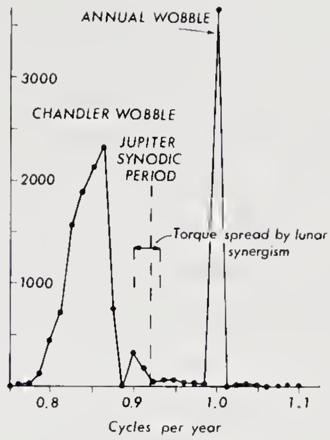


Fig. 118

Power spectrum showing frequency distribution of polar wobble through half a century. (After Munk and MacDonald).

available in the record is insufficient for really reliable spectral analysis. On the other hand neither the number of cycles nor the rheological relaxation should be markedly different (~15%) for the Chandler and annual wobbles, so their spectral peaks should be equally spread. Although the attenuations should be comparable, the excitation of the annual wobble is strong and regularly periodic, whereas the excitation of the Chandler wobble is probably more random.

So far the analysis has been in terms of the motions of rigid (Euler), or perfectly elastic (Newcomb) bodies. Even where redistribution of mass is the cause, consideration of the motion has not involved internal deformation. The analysis of excitation and damping, however, immediately involves the anelastic behaviour of the earth.

Damping or attenuation is caused by frictional losses where the wobble causes non-elastic deformation. The Chandler wobble is strongly damped (attenuation $\frac{1}{e}$ in 10-30 years), because, apart from the fluid core, some of the solid mantle has a relaxation time of less than the Chandler period (implying an effective viscosity of 10^{19} poises). The Chandler wobble is damped so rapidly that some process must reactivate it. The annual wobble is continuously reactivated by many seasonal changes of ice caps, sea ice, monsoonal changes, atmospheric heating, vegetation, etc., and is thus a forced nutation rather than a free nutation.

The activation of the Chandler wobble has been enigmatic. The most promising suggestion has been that large earthquakes trigger off the wobble, which then damps with a 10-30 year attenuation time until reactivated. This has been developed by Mansinha and Smylie (1967, 1968) who show jumps in the amplitude of the wobble (three or four per year) which correlate with large earthquakes (Fig. 119). The power needed to maintain the wobble is between 10^{13}-10^{14} joules per year which is only 10^{-4} of the seismic energy released annually; but the real energy applicable is much less than this because any random seismic impulse would be as likely as not to be in opposite phase with the wobble. A connection between earthquakes and the Chandler wobble is supported by the report by Whitten (1974) of a seven-year periodicity in the release of seismic energy, a period which corresponds to the beat period of the Chandler and annual wobbles. It is also true that the energy released in moonquakes is strongly correlated with tidal stresses at perigee and apogee. Pines and Shaham (1973) have suggested that the Chandler wobble causes earthquakes by triggering the release of elastic energy (estimated at 10^{25} J) stored in departures from an equilibrium figure. However their concept is inconsistent with isostatic empiricism, which indicates that regional stress-differences over distances greater than 10^2 km have attenuation times of the order of 10^3 years, five orders shorter than the 10^8 years assumed by them. This is fatal to their otherwise elegant analysis.

The capacity of earthquakes to excite the Chandler wobble is still disputed, particularly by Dahlen (see panel discussion by the leaders in this field, in the *Transactions of the American Geophysical Union*, v.54, No. 8, p. 795-6). Rochester and Smylie (1965) have dismissed excitation by electromagnetic core-mantle coupling as too small, although Runcorn (1970) still supports this mechanism, reinforced by topographic coupling by relief on the core-mantle interface, as originally proposed by Hide (1969). According to Munk and MacDonald (1960), the mass displacements involved in even the largest earthquakes are at least two orders of magnitude too small to account for a 0".5 wobble, although Mansinha and Smylie still argue that analysed globally, the magnitude is sufficient. In several cases the large earthquakes occurred after the jumps in the amplitude of the wobble

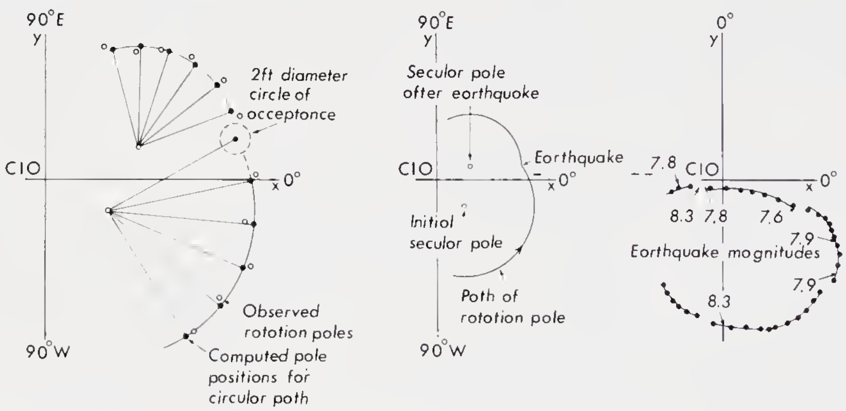


Fig. 119 Correlation of discontinuities in the Chandler polhode with large earthquakes. (After Mansinha and Smylie).

rather than before them (Chinnery, 1971). However Mansinha and Smylie point out that as the seismic energy release rate is four orders larger than that needed to maintain the wobble, a close correlation with large earthquakes should not necessarily be expected. They add that the seismic energy source could also possibly be the cause of the secular polar wander. They calculate a probable random walk of the rotation pole at an annual rate of $(\frac{1}{\tau})^{\frac{1}{2}}$ times the root-mean-square of Chandler wobble excitation (τ is the damping time), which would predict a probable polar shift of 0".005 per annum, the same magnitude as the observed shift for this century.

Because the beating of the Chandler and annual wobbles cause the rate of pole migration to vary from as much as 15 cm per day down to a few mm per day over a cycle of about seven years, Whitten (1974) plotted the rate of pole migration on to Benioff's curve for annual seismic strain release for this century. On "eye-ball examination" he claimed that there was "some correlation", but made no rigorous statistical check. He then plotted recorded large earthquakes through the last 350 years seeking a sabbatical period - but found none.

Many of the anomalies disappear if the large earthquakes and the excitation of the wobble are not cause and effect, but both are effects of a common cause - namely the mass redistribution of expanding geotumours. A geotumour is a complex diapir with an hierarchy of piercements-within-piercements driven by excess pressure in the deep interior. In addition, the elevation of isotherms by outgassing fluids causes phase changes to less dense phases. Even though isostasy be approximately maintained, the moment of inertia of the geotumour is substantially increased, which accordingly alters the axis of greatest moment of inertia of the earth. As a result, the spin axis of the earth describes a circle about the new axis of greatest inertia at the period of free wobble.

Possible Planetary Excitation of Chandler Wobble

Tidal influence on the planets appears at first sight to be orders too small to be significant in the excitation of the Chandler Wobble. But that is also true of the relation of the configuration of the planets in determining the sunspot cycle, discovered first by Wolf (1859), and actively pursued by several workers since, but widely discredited, until general acceptance in relatively recent years (for a review of this matter, see Williams, 1973). The tidal influence of the satellites is governed mostly by the tangential traction (which is not opposed by the primary's gravitation) rather than the direct attraction as in figure 120 (which is opposed by the primary's gravitation). The traction is a maximum when the declination of the satellite is 45°, and the resultant effect varies directly with the mass of the satellite and inversely as the *cube* of its distance.

The relative tidal influence of the planets on the solar corona has been found to be

Jupiter	1.9 - 2.6
Venus	2 - 2.2
Mercury	0.5 - 1.9
Earth	0.9 - 1.1
Saturn	~ 0.1
Mars	~ 0.03
Uranus	~ 0.02

The variation depends on the perihelion and aphelion of the satellite. Recapitulation and prediction of the sunspot cycle from this source has been so successful that the correlation is no longer in doubt.

But the magnitude of the tidal attraction has always been an embarassment. Thus Young (1882) commented: "It is very difficult to conceive in what manner the planets, so small and remote, can possibly produce such profound and extensive disturbances on the sun." Stetson (1947) suggested "It is possible that even the slight tide-raising forces of the planets could in the course of time set up a major oscillation in the sun's atmosphere very much in the way in which synchronized footsteps of a regiment may set a steel bridge asway". Johnson (1946) wrote: "These planetary influences cannot be gravitational but must be magnetic or electrical." Similarly Tchijevsky (1936): "One difficulty with all theories for explaining the appearance of sunspots is that the tidal action of the planets is too small to be significant in causing eruptions in the solar atmosphere on gravitational grounds. If one were to suppose, however, that the planets are at different electrical potentials then there is perhaps a fresh basis for attack on the sunspots theory from the planetary viewpoint." The

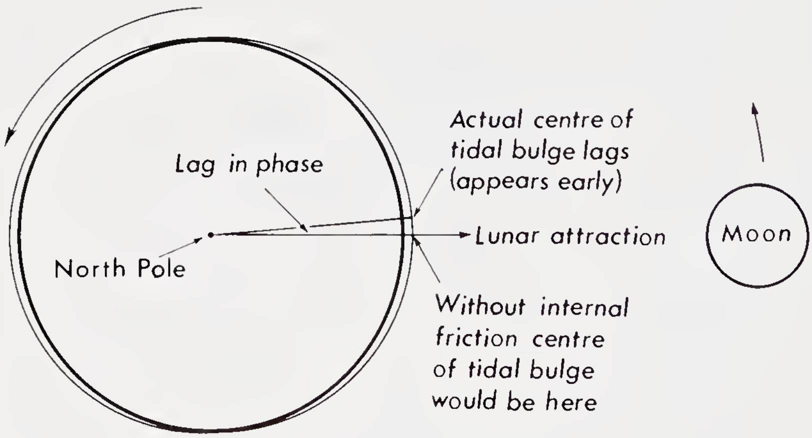


Fig. 120 Earth tides lag in phase because of internal friction.

viability of the electromagnetic proposals is suspect because the process must apply equally to Jupiter which is a powerful electromagnetic emitter and to Venus (and in another context probably to Moon) which lack significant magnetic fields.

However Luby (1940) points out that the "precessional action of a planet on the sun is enormously greater than is the tidal action, and its disturbance of solar equilibrium is correspondingly large." As reported by Williams: "This caused Clayton to re-examine his 1923 findings and he reported in 1941 that both for Earth and Venus the maximum of sunspots on a sidereal revolution is surprisingly close to that called for by Professor Luby's theory."

The precessional torque of Jupiter on the solar corona varies with perihelion and aphelion, an 11.86 year cycle, which is relevant in relation to sunspots. The precessional torque of Jupiter on the earth varies with Jupiter's synodic period of 399 days (which is close to the Chandler free resonance period) and with the configuration of the conjunction in relation to the direction of the earth's obliquity, which would produce a 12,800 year cycle. This long cycle would vary from zero torque at the Earth-Jupiter "equinox" to maximum at the Earth-Jupiter "solstice". The 399-day cycle changes by a factor of four at conjunction to one at opposition. Of the other planets, Venus, while offering equal precessional torque with Jupiter has a synodic period of only 225 days, much too far from the Chandler free period to be considered as an exciting source, and Mercury and Mars contribute torques more than an order smaller, as do the outer planets, which in any case have synodic periods close to the annual wobble, and could only have insignificant excitation in comparison.

Thus Jupiter is the only candidate for consideration. The effective period between conjunctions is broadened to between 406 days and 392 days because the torque would be synergistic with the moon's torque so that the peak might occur up to a week before or after the conjunction. At this peak, Moon, Sun, and Jupiter add their torques. The conjunction and the limits of the effective peak are plotted on the frequency distribution curve of the polar wobble (Fig. 118). It is immediately apparent that the 0.9 cycles per year peak between the Chandler wobble and the annual wobble falls right in the Jupiter band. Further, the shorter end of the Jupiter band is sufficiently close to the earth's free period to excite it, and the Jupiter peak would in turn be amplified by the free wobble.

Jupiter's precessional torque, acting on a rigid body, would of course produce only a nutation without polar wobble. However the atmosphere, the hydrosphere and the core would all respond differentially to the solid earth, and the tidal friction would result in wobble excitation.

Hence the question arises:- Does Jupiter excite the Chandler wobble? Assuredly the magnitude of the torque appears much too low. But this is also true of the planetary influence on sunspots. Moreover, as elaborated below, interplanetary resonances have certainly played a dominating part in the present configuration of the solar system.

Other Wobbles

Superimposed on the Chandler free wobble, the seasonal redistribution of ice, snow, water, atmosphere, vegetation, groundwater, winds, and currents causes a smaller annual wobble with an amplitude of about 0".09 and a semi-annual wobble with an amplitude of about 0".01. The lunar tides, which are oblique to the equator and vary with apogee and perigee, produce theoretical monthly and fortnightly wobbles with an amplitude of about 0".001, which is below the noise level of current instrumentation. The daily tides yield a wobble with a period nearly equal to the sidereal day and amplitude of about 0".02. In these cases there is redistribution of mass components of the earth, so that these wobbles are distinct from the small precessions ("nutations") with the same periods, which are due to variation of the couple applied to the equatorial bulge. A longer wobble (known as the Markowitz wobble, after the discoverer) with a variable period of about 24 years and an amplitude of about 0".02, has also been detected. This has been attributed by Busse (1970) to differential motions of the mantle and inner core, inertially coupled through the outer core.

Pole-tides

Kelvin (1876) pointed out that earth wobbles (such as the Chandler, annual and Markowitz wobbles) would induce oceanic tides. Calculated equilibrium amplitude for the pole tide induced by the Chandler wobble is only 0.5 cm, which is scarcely above detectable background. However, Haubrich and Munk (1959) found a peak in the tidal spectral analysis with a period of 1.19 years which corresponds to the Chandler Wobble. Miller and Wunsch (1973) after culling systematically a quarter of a century of tidal records could not find positive correlations except in the Baltic, where tides with the Chandler period showed amplitudes of 0.7cm per year in the south to 1.8cm at the northern end of the Gulf of Bothnia. This confirmed an earlier observation by Maksimov and Karklin (1965) that the pole tide is detectable in the Baltic. Such amplification could be a seiche-like resonance effect.

Rotational perturbations on geological time-scales

Theoretical studies of the earth's rotation have been confined almost entirely to the dynamics of a rigid body or of an elastic body. Where rheological properties have been considered at all they have been confined to short durations with respect to geological times. Our position therefore, is that whereas observational astronomy and theoretical analysis of the behaviour of eclectic terrellae are adequate for synthesis of the perturbations of the earth's rotation over periods of centuries and perhaps millennia, for the time scales of interest to geology, we have to fall back on consideration of the likely consequences of gross tectonic phenomena, and ultimately on our own empiricism in the evidence of the rocks.

Although the deformation of the mantle certainly involves plasticity *(sensu stricto)* as well as elastic and viscous deformation modes, the kind of time-dependence of whole-earth deformation may be illustrated by a simplified terrella (Fig. 121), in which the anelasticity is shown as

Fig. 121
Deformation of time-fields of the earth.

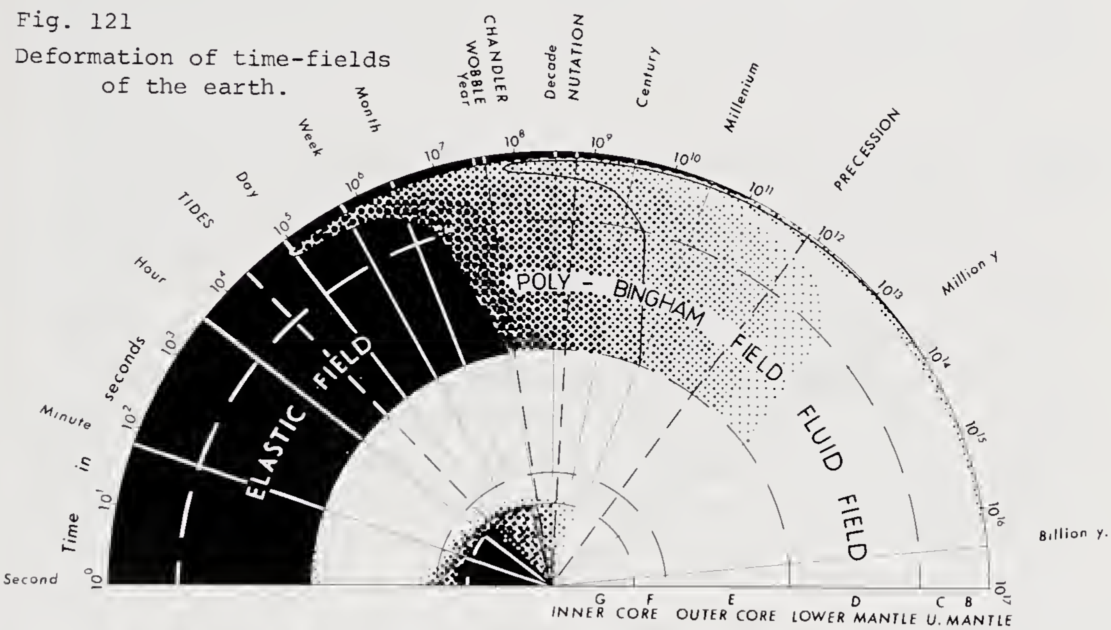


Fig. 122 The earth as seen by different tectonic phenomena. Black means elastic, and white viscous. In each case the elastic shells are constrained by the viscous shells to give a poly-Kelvin rheology.

Maxwell relaxation times corresponding to viscosities in excess of 10^{27} poises at the surface, diminishing to a minimum of 10^{19} in the asthenosphere and rising again at greater depth presumably because pressure overtakes temperature as the dominating parameter of viscosity. For any time-scale (radial lines) some layers are effectively elastic while some are effectively viscous so that the whole system always behaves as a poly-Kelvin body. Thus, on the time scale of earthquakes, seismic waves see the mantle as elastic and the core as a fluid with anelasticity in the F transition zone. The semi-diurnal tides also see an elastic mantle and a fluid outer-core. Calculation of the free-period of an elastic mantle and fluid core reveals a small discrepancy against the observed period of the Chandler wobble, because in this time-scale some anelastic correction is needed. On the time-line of principal nutation more significant anelastic correction is necessary. Indeed Jeffreys and Vicente (1957) found a 600 day discrepancy in the 19-year period. On the time scale of the precession, virtually complete fluidity is seen in the crystalline mantle, so that Clairaut, who assumed a thin rocky shell on a magma substratum, did use the appropriate terrella and found no discrepancy between computed and observed precession. Any changes in the polar and equatorial moments of inertia due to waxing and waning of ice caps, and the climatic mass-redistributions, sea-level changes, volcanic extrusions etc., attenuate within one cycle of the precession, so that the fluid model yields the correct result. Isostasy is also in this time-field of millennia to millions of years, so the appropriate terrella is a thin elastic crust on a fluid asthenosphere.

Modes of rotational perturbation which arise through the rheological yield of the earth under sustained stress-difference include gross polar wander, large variation of obliquity (which has profound palaeogeographic effects), change of rotation rate from tidal friction, from differentiation and from expansion, and differential motion of zones and sectors (which has the most profound geotectonic consequences).

Polar wander

The crucial significance of the scale of time is illustrated by the stabilising control of the equatorial bulge, which has been claimed by some to freeze the obliquity of the earth's axis. Munk and MacDonald (1960, p.57) quote the apprehension of Senator Kefauver that a 10^{17} joule hydrogen bomb might tilt the earth's axis by 10°, and they point out that, owing to the stabilising control of the equatorial bulge, the resulting tilt caused by the bomb would be about one micron. This is, of course, true because the time-scale of the stresses caused by the bomb falls at the lower time limit of figure 121, where the whole mantle is in the elastic field. However, similar loads maintained for thousands of years would see a fluid mantle, so that the equatorial bulge would migrate to the new equilibrium position required by the stress field. It might be argued that this assumes a Maxwell rheology with no stress threshold for deformation, but would not apply to a plastic body which has a yield stress. In fact both viscous and plastic terms are present (along with the elastic term), so slow yield would always occur; but the mantle is never without stress-difference from tidal, erosional, sedimentational and tectonic phenomena, so even plastic yield does not face its non-stressed yield-stress threshold.

Clearly asymmetric mega-geotumours such as postulated by van Bemmelen for the development of the Atlantic and Indian Oceans, should certainly result in large wobble and gross polar wander. Differently expressed, the Gondwana geotumour which yielded such vast floods of tholeiitic magmas and was followed by the dispersion of Gondwanaland, should certainly have resulted in gross polar wander. Likewise the asymmetric spreading of the Pacific, Indian and Atlantic Oceans should have produced large wobble and gross polar wander.

Gravity is the dominating force in the earth, so continents and oceans approach isostatic equilibrium to a high degree in times geologically short. But when isostatically balanced, or nearly so, a continent contributes significantly more to the moment of inertia than does an equivalent ocean. Hence any mode of relative movement of continents, irrespective of whether we assume moving lithospheric plates or dispersing polygons involving the whole mantle, must cause large perturbation of the rotation pole. Empirically, palaeomagnetic results confirm that gross polar wander with respect to each of the continents has indeed occurred in addition to continental dispersion and rotation of continents. Ahmad (1973) has summarised the unequivocal evidence that large scale and rapid polar wander has certainly occurred during the last few hundred million years. He attributes this to mass redistributions due to asymmetries of expansion, gross magmatic irruptions and dispersion of continents. Ahmad's model implies major wobble (with migration of the equatorial bulge) but not necessarily any change in obliquity.

A difficulty in discussing polar wander is to define polar wander with respect to what? There should be no polar wander of the axis of angular momentum. There is clearly polar wander with respect to individual continents but such movements could be complementary and cancel each other. On the other hand there could be residual polar wander of the whole crust relative to the mantle, or of the mantle relative to the core. Jurdy and van der Voo (1974, 1975) attempt the integration of the continental blocks and find no significant polar wander since the Upper Cretaceous. This is consistent with the empirical general *Südpolflucht* and *Nordpoldrang* shown by all continents and also the ocean floors. However, as these authors recognise, the movement of the ocean floors contribute heavily and could upset this conclusion. Also their analysis assumes subduction and a non-expanding earth, and their conclusions may not apply to an expanding earth.

VARIATION OF OBLIQUITY

Studies of the dynamics of the earth have assumed as axiomatic the inviolability of the obliquity. This is an ideal assumption, for it requires total absence of any non-axial frictions externally induced, an assumption which is highly improbable. No-one has asked - still less explained - why the obliquity in the first place? To what extent is the obliquity damped? And activated? At least five processes operate to change the obliquity of rotation axis to the ecliptic.

In the simplest case of the spinning top, whose rate of spin declines through the frictional resistance to its rotation, the obliquity to the axis changes systematically as the ratio of spin kinetic energy to potential energy varies. This should also apply to the earth, because like the top, its spin is continually retarded by lunisolar tidal drag (Fig. 120). If there were no internal friction, the tidal bulges would face directly the moon and the sun. The 3° lag in phase of the tidal bulge implies friction, the friction implies reduction of spin kinetic energy, and this in turn implies that the obliquity must vary. Because the kinetic energy of spin of the earth exceeds by many orders the potential energy against the lunisolar torque, the earth's axis should move to the "sleeping" position where this potential energy is a minimum - that is, the obliquity should increase until the rotation axis lies in the plane of the couple.

Again when a brake is applied to a spinning spheroid so that the braking torque is normal to the spin axis, the brake only retards the spin. But if the braking couple is oblique to the axis of spin, the application of the brake causes the spin axis to move towards the brake. The lunisolar tidal friction brake is applied more than twenty degrees from the equatorial plane, so its effect is to turn the rotation axis towards the brake, that is once again to increase the obliquity. The sense is the same at both solstices and at both parts of the lunar orbit. The magnitude of this effect has been estimated for a non-rheological earth and the present lunar distance and present value of G, as about one degree increase in obliquity in 10^9 years, which is trivial. However the assumptions involved leave this question wide open.

Of the two axial positions when the precessional torque becomes zero (in, or normal to the orbital plane) most satellites are near the former but three which do not possess moons (Uranus and Pluto and Venus) are in the latter category, but this may be coincidental.

These effects are complicated by the fact that moon's orbital plane is inclined 5° to the earth's orbital plane. The lunar torque is zero when the earth's axis is normal to the moon's equatorial plane, whereas the solar torque becomes zero when the earth's axis is normal to the ecliptic. The lunar torque, is four times the solar torque and varies from its maximum value to zero with a monthly cycle, whereas the cycle of the solar torque is annual. The lunar cycle is too short in relation to the rheological relaxation times within the earth, whereas the solar torque would cause greater cumulative migration of the equatorial bulge (Fig. 121). This leads again into the *terra incognita* (literally) of the non-understood rheological effects on the earth's rotation. Indeed, let us recall that Darwin's two-body calculation of the earth-moon relations as a result of tidal drag (1908) indicated that the inclination of the moon's orbit to the earth's rotation axis became much less when traced back into the past. Such interaction must have been reciprocal with a feedback on the earth's obliquity. The problem is further complicated when the third body (the sun) is introduced into the system. The inclination of the solar plane relative to the earth must also change - that is, the earth's obliquity.

The earth does not precess as a homogeneous body. The ellipticity of the core is significantly less than the rotational ellipticity of the lithosphere, which implies that the lunisolar torque on the equatorial bulge of the core would cause a slower precession than that of the mantle and lithosphere. Inertial, viscous and electromagnetic coupling between the core and mantle must lose energy at the rate of more than 10^{11} watts to maintain core and mantle precessing at the same rate. This dissipative loss should be debited to the kinetic energy of the precession, which would reduce the angle of the precession, that is, the axial obliquity. Aoki (1967) ascribes the observed but unexplained secular change in the earth's axial obliquity to differences between the angular velocity vectors of core and mantle.

Ward (1973) has reported large scale variations in the obliquity of Mars produced by the coupling between the motion of its orbital plane due to the gravitational perturbations of the other planets, and its precession. Sympathetic variations occur also in the orbital inclination (Fig. 123). The calculations yield a series of terms. The beating of the largest terms causes the axial obliquity and the orbital inclination to oscillate with a period of 120 thousand years, and the next terms modulate this with a period of 1.2 million years. The axial obliquity ranges from 14°.9 to 35°.5 while the orbital inclination ranges from 1° to 5°. Similar considerations must apply to the earth. Indeed a libration has been observed of 47" per century with a theoretical period of 40,000 years. The secular terms should be much longer. Ward (1974) has estimated that the variation of the obliquity of the earth from this cause would be about 2° - much smaller than that of Mars, because in the case of Earth, the natural frequency is more rapid than the forcing frequency, whereas for Mars these frequencies are close, with resonance producing large oscillation. Even the 40,000 years period is an order longer than the stress attenuation time in the mantle (see Fig. 121), so the equatorial bulge would follow the oscillation and cause feed-back effects on the gravitational perturbations. Ward has also pointed out (1974) that our ideas of uniform precession of a spin vector about the orbit normal at constant obliquity need to be drastically revised if we relax the constraint of a fixed orbit.

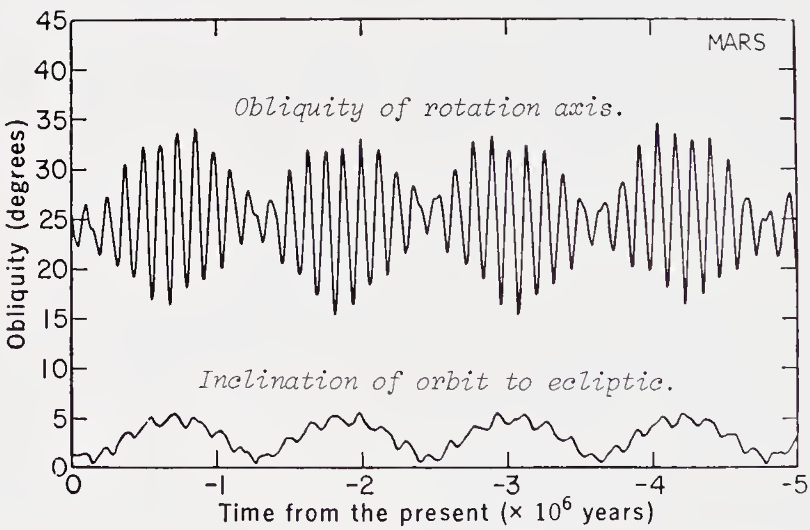


Fig. 123 Variation in obliquity of Mars (and orbital inclination) caused by attraction of the other planets. (From Ward, 1973).

Ward (1975) has retrogressed the Earth-Moon system and finds large variation of obliquity of both bodies. During the early stages while the Moon was less than 25 earth-diameters from the earth, the lunar axis was close to the ecliptic. Thereafter the motion rapidly became unstable until at 34.2 earth-diameters the lunar spin axis tipped to within 13° of the ecliptic (almost spinning "on its side"). However the motion stabilised at an obliquity of 49° which declined exponentially to its present value of 6.7° (Fig. 124). Implied but not quantified are conjugate variations in the obliquity of the earth's axis. The geological age to be assigned to the short period of great instability involves uncertainties, but it could correspond to the epiArchaean break.

Solar and lunar attraction of an asymmetric geotumour increases the precessional torque, thereby making the precessional angular velocity a geological variable, but it also displaces the axis of maximum moment of inertia, which causes a viscous migration of the equatorial bulge. Owing to internal dissipation of energy, rotation about a non-principal axis decays, with relaxation times of the order of 10^3 years, to a rotation about the maximum moment of inertia. The attraction of the sun and moon on the asymmetric geotumour provides external reactivation of the obliquity. The

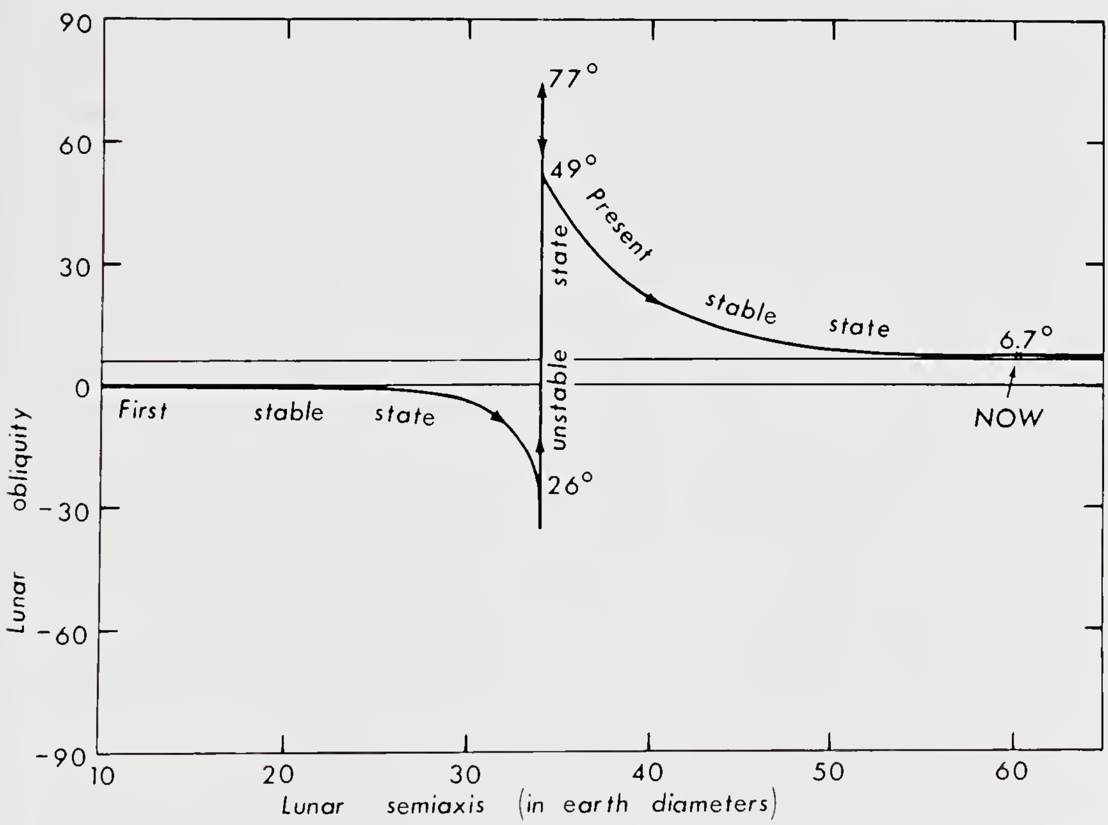


Fig. 124 Variation in obliquity of Moon, according to Ward,1975).

obliquity of the earth is continually damped towards zero, but is intermittently reactivated by asymmetric expansion on geotumours. The obliquity of the earth thus becomes an important geological variable.

Still another suggestion has been made by Kelly and Dachille (1953), Gallant (1963) and Dachille (1963), that obliquity changes could result from the impact of large meteorites. Certainly the principle is valid, but it is a question of scale, and geological evidence. Dachille correctly points out that even under the most extremely favourable conditions of impact with an asteroid the size of Juno (190 km diameter) the resultant axis change would be only 0'.02. There is no evidence of Phanerozoic astroblemes of sufficient size although such could have been hidden under the sea, or later sediments. Urey (1973) proposed that comet impacts would have energies of 10^{24} joules or more (sufficient to boil an area of ocean 200 km in diameter and 3 km deep) and that such impacts could explain the termination of geological epochs and periods, and associated sudden extinctions. Be this as it may, energy from such impulsive catastrophies would be converted almost entirely to heat rather than mechanical motion, and as the thermal pressure of the plasma fire-ball would act radially on the earth, momentum conservation would be orbital rather than rotational.

Williams (1972, 1975) catalogs much evidence supporting his conclusion that the obliquity of the earth has changed systematically throughout geological time. For example, Williams concludes that the Pennsylvanian and the early Permian were times of large obliquity and the Jurassic and early Cretaceous were times of near zero obliquity with uniform climate extending to high latitudes without marked seasons. Williams' model envisages the initial attitude of all planets as having their rotation axes in the plane of the ecliptic. Owing to a postulated rotation of the plane of the whole solar system with a period of 2500 million years, the rotating planets act like gyroscopes and maintain their momentum axes constant in space. They therefore turn in relation to the ecliptic. One and half cycles were completed 750 million years ago (at the time of very wide spread Eocambrian glaciation), with the axes in the ecliptic and with the other pole directed in the initial direction. One and three-quarter cycles were completed 125 m.y. ago (at the time of the Jurassic-Cretaceous equable interval) when the axes were nearly normal to the ecliptic. Earth, Mars, Saturn, and Neptune are accommodated into this scheme. The other planets show varying degrees of aberrance attributed to other disturbances.

Chandler, in his famous search for latitude variation, which led to his discovery of the annual and 14-month wobbles, also searched for secular variation of obliquity or "sway", but did not find any. This is not surprising because the rate of change of obliquity suggested by geological empiricism would involve changes at least an order of magnitude less than the present threshold of observation. If it is assumed that asymmetric geotumours cause both the activation of the Chandler wobble and change of obliquity, then sway would be expected to be two orders smaller than wobble (Munk and MacDonald 1960), and other changes of obliquity might also be of similar order. This however could eliminate the obliquity within a geological period. As Jardetsky (1949, p.797) and Munk and MacDonald (1960, p.15) have pointed out, theoreticians have not yet tackled the problems of the rotation of a non-elastic earth. There is an urgent need to study the effects on the secular perturbations of the earth's obliquity of rheological properties like those illustrated in figure 121.

VARIATION OF RATE OF ROTATION

Data relevant to variation of the rate of rotation and revolution of the earth come from astronomical observations, historical records of eclipses, concurrence of annual, sunspot and precessional rhythms in ancient sediments, and from growth-lines of fossil invertebrates. Theoretical causes of variation of rotation include tidal friction, expansion, differentiation, volume changes from change of temperature gradient, and variation of physical "constants". An objective review of several of these matters has been given by Scrutton and Hipkin (1973).

Evidence of rotation change

Astronomic Observations

The recorded motions of planets and their satellites appear to be somewhat erratic, but also accelerating. Both the erratic and accelerating changes vary sympathetically for bodies of all sizes and periods, such as the sun, the planets, and their satellites. This means that it is our time-keeper (the earth's rotation, or *universal time*) which is the variable. The rate of rotation of the earth diminishes by about 40 seconds of arc per century, hence the apparent acceleration of all other bodies timed with such a clock. The erratic behaviour is also the fault of the earth. The mantle overruns the core at about 10'.8 per year (as indicated by the westward drift of magnetic isopores), but this rate fluctuates, and angular momentum is exchanged between mantle and core; the surface accelerates while the core is losing momentum and decelerates while the core is gaining momentum. *Ephemeris time*, which uses the planetary orbits instead of the earth as time-keeper, gives steady motions for all planets and satellites (except the moon, which has real acceleration involving a recession of about 3 cm per year, which is attributed to the moon's tidal deceleration of the earth). Although ephemeris time is clearly a better standard than the earth's universal time, ephemeris time also gains if the gravitational constant diminishes, as modern data suggest.

Ancient Eclipses

Total eclipses of the sun have been recorded over 3000 years in Greece, Babylon, and China. As the angles subtended by the sun and moon are nearly the same, the *place* where a total eclipse was observed, gives a close measure of the rate of rotation of the earth. Edmund Halley, the second Astronomer Royal, first deduced (1695) that the moon had accelerated (or the earth slowed down) from the inscriptions recording a total eclipse in the ancient Aramaic city of Palmyra (Tadmor), 250 km northeast of Damascus, devoted to the worship of the sun. Again the indicated deceleration of the earth is about 40 seconds of arc per century. However this is 20% less than would be expected from the lunar tidal torque if this were the only variable, and the solar tidal torque increases the misfit by a further 20%. Clearly other variables (of which there are many) have a net accelerating effect on the earth.

Sedimentation Cycles

Tree growth-rings and varved sediments occur, which show modulation of the annual thicknesses by the sun-spot rhythm. Similar relations have been reported in Pleistocene glacial sediments, in the Cretaceous and Devonian, and in sediments as old as the Nama system of South Africa (Late Proterozoic). This suggests that either the earth's year and the sunspot cycle have both varied at the same rate, or that neither has varied significantly over 10^9 years. However, as the sun-spot cycle and its several modulations have been found to correlate with the variation in the combined planetary tidal attractions on the sun (Meldahl, 1949, Wood, 1972 and Gribbin, 1973), it is conceivable that the revolution periods of all the planets and the sun-spot cycles could indeed vary in phase with each other, e.g. by variation of the gravitational constant.

Invertebrate Growth Cycles

Rhythmic growth lines on invertebrate skeletons have been recognised as such for a very long time, but not the identification of the specific cycle recorded. The rosetta in the present context is the identification of two concurrent but independent cycles in ancient fossils, thereby promising the key to their relative rates through geological time. Unfortunately, noise in the record, causing suppression of some cycles, the obliteration of others, and spurious addition of unrelated signals, seriously impairs this attractive edifice, and reduces its foundations to quicksand.

Forty years ago, Ma identified seasonal growth-lines in the recent coral *Favia speciosa* and in fossil corals. He claimed that the annual growth rhythm was an expression of seasonal temperature rhythm in the sea, and that the banding was weak in the tropics but strong in the sub-tropics. He proceeded to make extensive measurements of them to determine the latitude zone on several continents at different periods, in order to test hypotheses of continental drift (Ma 1933, 1934 and several subsequent papers).

Krempf (1934) identified the 18.6 year Metonic cycle in the growth rate of *Favia* through 112 years (six cycles), the last cycle ending in 1931 or 1932, with a new cycle just started. The cyclic growth produces a "micro-atoll", with annuli representing the Metonic cycle. The marine tidal cycle caused by the regression of lunar nodes reached its maximum in 1932. Krempf suggests that maximum tidal range restricts coral growth while minimum tidal range allows corals to grow nearer to the mean sea-level. His explanation is plausible but not compelling, for there are no reported annual reference marks, so the correlation of the patent cyclic growth with the Metonic cycle is hypothetical, though reasonable. Krempf recognised the significance of his discovery for identifying the precession cycle in the past.

Wells (1937) found smaller bands, too numerous to be annual, and suggested that these might be caused by the lunar tidal cycle. Wells (1963) noted very fine growth-lines on the external epitheca of some recent corals, and since about 360 of these occurred between annual growth-ridges (which were a centimetre or more apart), he suggested that these were circadian (Lat. *circa die* : about a day) growth-lines, caused either by tidal or light fluctuations. Wells went further and reported (1970): "(1) A *Streptelasma* from the Upper Ordovician of Ohio showed about 412 daily rings per year, (2) A *Ketophyllum* from the Middle Silurian of Gotland has about 400, (3) A number of specimens of *Heliophyllum, Eridophyllum, Cylindrophyllum* and *Favosites* from the Middle Devonian of New York and Ontario gave an average of 398 with a range from 385 to 405, (4) A *Lithostrotion* from the Mississippian of Wales, 398, (5) Two specimens of *Lopho-*

phyllidium from the Pennsylvanian of Pennsylvania and Texas showed 380 and 390 layers per year."

Projecting back a retardation of the earth's rotation by lunar tidal friction of 2" per century for 400 million years, gave 399 days in the Devonian year, much too good an agreement, considering the variations and uncertainties on both sets of data! Continuing this projection back brings the moon to rendez-vous with the earth less than 2000 million years ago - much too bad an agreement, and certainly wrong. (Where was the moon parked for the two previous two billion years, and what brought about the Earth-Moon wedding, or parturition, at that time?)

Scrutton (1965, 1970) observed bands intermediate between the fine circadian lineations and the circannian (about annual) ridges, and like Wells (1937) he interpreted these as lunar. Unfortunately the "annual" bands were not clear on these specimens. Using ten Middle Devonian corals with a total of 113 bands, he found an average of 30.59 circadian lineations per band. Assuming Wells' estimate of 399 days in the Devonian year, this would imply 13.4 months in the Devonian year.

Berry and Barker (1968) found Lower Cretaceous pelecypods *Indonearca vulgaris* had a dozen or so prominent bands, each bearing some two dozen ridges and grooves, on which were finer lineations. As there were no specific lines to divide the prominent bands precisely, the ridges were counted over several specimens which gave a mean of 24.98 ridges per major band. Assuming the bands to be annual, this gave 24.98 fortnights or 12.49 lunar months per year. Another pelecypod in the same beds *Crassatella vadosus* bore sharply incised grooves with subordinate ridges and finer lineations. Eight such shells had 1095 lineations across 37 ridges. Assuming that the lineations recorded days and the ridges months would give an average of 29.59 days per month, which was adjusted by the authors to a "weighted" average of 29.65. Combining the *Crassatella* days in a month with the *Indonearca* fortnights in a year gave 370.3 days in the Late Cretaceous year, in nice conformity with 371 days obtained by linear interpolation between Wells' 400 day Devonian and the present. Except that the result would not fit nicely, it is not clear why the *Crassatella* ridges are months and the *Indonearca* ridges are fortnights, nor are we told how many lineations make up an *Indonearca* ridge, nor how many *Crassatella* ridges make up its incised annual grooves.

Mazullo (1971) counted circadian, circalunar and circannian bands on corals and brachiopods from Silurian to Pennsylvanian, and gave 421 days to the year for early Silurian, 419 for Middle Silurian, 410 for Middle Devonian, with the corresponding decrease from 32.4 to 31.5 in the days in the month from Silurian to Devonian.

What is the physical variable or variables recorded in the growth-lines? The Metonic cycle reported by Krempf (1934) and the lunar and semi-lunar cycles reported by Berry and Barker (1968) and by several others, could record scarcely any other variable than lunar tides. It is also clear that it would have to be the synodic and not the sidereal month, and the tropical year, not the sidereal year. The annual cycles could also be directly recording tides, especially if the earth's orbital ellipticity were greater in the past. Scrutton and Hipkin report that Barnes (1971) has identified a daily growth lineation in some living reef corals, which he interpreted as a light cycle perhaps due to the photo-metabolism of the symbiotic algae in the coral food chain. The annual bands and their thicknesses have been attributed by Ma to temperature. The total growth rate is maximum when the seasonal banding is least. Scrutton and Hipkin point out that this implies that interruptions in deposition might be expected to maximise in those regimes where the annual growth increments are most clearly developed. They observe that "causal relationship between water temperature and growth annulations, although strongly supported by circumstantial evidence, has never been confirmed by direct experimental observation". Again they state that "in the case of the supposed monthly banding, there are no satisfactory observations on modern corals, living or dead, to aid the interpretation of this rhythm"; and also that "the best evidence for the interpretation of the growth rhythms in fossil forms [as daily, monthly, or annual] is still at present the numerical relationship between them".

Dodge, Aller, and Thomson (1974) have established that the dominant factor causing growth variation in *Monastrea annularis* in Discovery Bay Jamaica, is the intensity of resuspension of bottom sediments. Corals can tolerate low sediment deposition and clean themselves by removing the sand with copious extrusion of mucus. However this consumes energy and slows growth. A diurnal rhythm results because, throughout the morning hours the lagoon is calm and clear, but by afternoon the northwest trade winds, which blow daily, have driven water turbidity to a maximum, and by evening the lagoon is again calm and clear, and remains that way all night. Clearly growth control through water turbidity variation could record circadian, tidal, annual and sun-spot, metonic rhythms. But it would be least reliable for daily rhythms and certain to give incomplete (too few) recordings.

Hazel and Waller (1969) point out that the average counts for days per lunar month on six Recent bivalves differs statistically by four times the standard error from the present month. Farrow (1972) found a complex pattern of growth rhythms in Recent bivalves, which in favourable circumstances record seasonal, lunar, semi-lunar, daily, and twice daily rhythms, and some correlating with opening and closing of the shell, but they also include gaps caused by storms, distinct breaks due to winter rest, and others due to spawning. The interpretation of counts in fossil shells would seem to depend on the answer hoped for by the investigator.

Whereas circadian lines may be definite and countable (provided none are missing from non-development or subsequent abrasion), and circalunar and circannian bands may be clearly recognisable as waxing and waning of the growth, there is no particular day-line by which the beginning of a month or year can be identified. Hence the only way to proceed is to count over several months or several years and divide by the number of longer cycles represented. This cannot be exact because beginning and end of the count are arbitrary, there are never many cycles on a single specimen, and the estimate must always be a minimum because there is no way of determining how many days have failed to record, or to remain preserved.

Causes of change of rotation rate

(1) Kant (1754) first pointed out that tidal friction must reduce the earth's rotation rate, and Delaunay (1865) first computed this effect. Darwin (1908) investigated the problem more intensively, and found that (a) the earth turned progressively faster in the past, (b) momentum is conserved by the moon's recession from the earth, and the moon must have been close to the earth between 800 and 2000 million years ago, with the earth then rotating more than four times as fast as it does now, and (c) the inclination of the moon's orbit to the ecliptic was much steeper in the past. Gerstenkorn (1955) confirmed these calculations, and accordingly proposed the capture of the moon by the earth in a highly inclined retrograde orbit, and that the tidal friction at first caused a reduction of the month to only nine hours, and the mean radius of the moon's orbit to 18,500 km, with increasing inclination until it became polar, then steeply inclined prograde. Thence the tidal friction caused the recession of the moon and the reduction of the inclination of its orbit, both rapidly at first, and both continuing today. Darwin (1879) had estimated that the tidal friction during the early stages would raise the temperature of the earth (and of the moon) by at least 1000°C. The capture proposed by Gerstenkorn would greatly increase

this. Munk (1968) points out that the ocean would evaporate or the earth would melt, or both. The geological evidence, both on the earth and the moon, clearly indicates that this could not have happened during the last 3.5 billion years. Hence the currently observed rate of recession of the moon cannot be projected back unless some other significant change also took place. This anomaly disappears on an expanding earth because the size of the tidal bulge would be less on a smaller earth. The moment (i.e. the torque) of the equatorial bulge would also be less for the same attractive force. Also Jeffreys (1920), and Munk (1968), have concluded that most of the tidal energy dissipation takes place in the seas, and not in the body tides. As already pointed out, the relative area of seas and the total volume of sea water have increased with time; however, as elaborated in the discussion of the evolution of the oceans, the relative proportion of epeiric seas and continental shelves to deep ocean basins has decreased with time, with strong modulation of the general decline curve. Jeffreys estimated the tidal retardation from the expression

$$0.002 \int \bar{v}^3 \, dA$$

in which v and A are the tidal velocity and the area of the seas. The cube of the velocity means that most of the retardation occurs in very shallow seas (50% of it in the Bering Straits!). Hence the rate of retardation based on eclipse or astronomic observations during the last few thousand years would have little relation to retardations as recently as 20,000 years ago when most of the present continental shelves were above sea level, and the proportion of the area of the earth occupied by shallow seas was a small fraction of its present value. Yet it is precisely these historical observations which are projected back for comparison with Devonian coral growth-rings. The proportion of shallow seas now is much greater than in the Triassic and much less than in the Cretaceous, so the contribution of tidal drag to retardation has been highly variable, and the present has no claim to being the norm.

(2) The different ellipticities of the core and mantle imply that the core, if free to do so, would precess in some 34,000 years as against 25,800 years for the whole earth. The mantle continuously drags the core along into phase with its precession. This dissipates energy which would slow the rotation of the earth. This question, with other implications is discussed later

(3) Holmberg (1952) has revived the observation by Kelvin (1882) that the gravitational couple on the atmospheric tides accelerates the earth's rotation, and that the natural period of resonance of the atmosphere coin-

cides with the diurnal heating cycle. This greatly amplifies diurnal barometric variation, and perhaps also the diurnal variation of the magnetic field. Holmberg suggests that this coincidence is not fortuitous, tidal friction would have retarded the earth's rotation until the period approached the natural period of the atmosphere, at which point resonant amplification of the atmospheric cycle would increase and balance the tidal retardation; this, he suggests, is the present state. Mechanical energy is extracted from the solar heat engine to balance the mechanical energy lost in tidal friction. If Holmberg's estimates are correct the cause of observed change in the length of the day and the acceleration of the moon must be sought elsewhere. This proposition is still debated, but if Holmberg be correct, both oceanic and atmospheric torques are eliminated as causes of the secular deceleration of the earth.

(4) Interaction of the solar wind with the magnetosphere retards the rotation of the earth. The viscous and magnetic torques have not been closely quantified but appear to be an order or more less than that due to meteoritic infall. The magnitude of this factor is itself a polycyclic variable, depending on the tidal attraction of the planets (Gribbin, 1973, p.453 and Gribbin and Plagemann, 1974), and (on a time scale of 200 million years) possibly cyclic nuclear activity in the Sun (Dilke and Gough, 1972).

(5) Impact of asteroids could vary the rotation rate. Dachille (1963) has estimated that under the most favourable equatorial impact conditions an asteroid 3.2 km in diameter could increase (or decrease) the rotation rate by 0.000001%, a 32 km asteroid by 0.001%, a 320 km asteroid by 0.9%, and a 640 km asteroid by 7.5%. An event of this kind cannot be excluded as impossible, even in later geological times, although in that case the astrobleme would have to be hidden under the sea or later sediments. The probability diminishes rapidly with the postulated size of the body, and at smaller sizes with higher probability there is increasing chance that the direction of change would be cancelled by other impacts.

(6) Progressive heating of the interior and steepening of the temperature gradient, which now has displaced the cooling earth model, implies increase in moment of inertia and decline in rotation rate, because of temperature-sensitive phase changes in the interior.

(7) Differentiation accelerates the rotation of the earth. A chemically homogeneous earth would of course increase in density towards the centre because of elastic compression and phase condensation of lattices, but chemical differentiation, transferring heavy elements towards the core and lighter elements towards the crust, reduces the moment of inertia.

The initial and continuing(?) segregation of inner and outer core and mantle decreases the moment of inertia. Every basaltic irruption involves differentiation of primary mantle into peridotite and basalt and decreases the moment of inertia. and increases the rate of rotation.

(8) Physical constants. Most discussions of rotation variation assume constancy of physical constants, such as G, $^{e}/_{m}$, and mass. Changes of any of these would affect the rotation rate (see discussion of possible causes of expansion).

(9) Expansion would cause decline in rate of rotation. The earth has been and still is differentiating, so the rotation rate of an expanding earth could be increasing, decreasing, fluctuating or remaining steady, according to the relative magnitude of these effects.

VARIATION OF PERIOD OF REVOLUTION

In all discussions of variation of the earth's rotation, it is tacitly assumed that the length of the year is constant. But is it? Variation of physical "constants" (e.g. G) would, of course, make the revolution period a variable. Several insignificant secular variations are known. For example the earth retreats from the sun for similar reasons of tidal drag that the moon retreats from the earth, but because of the very large angular momentum of the earth, this tidal torque would only have increased the length of the year by some four seconds since the beginning of the earth. Viscous and magnetic torques originating from the solar wind are probably a couple of orders greater but still insignificant geologically. Accretion of meteoritic material could have lengthened the year by several hours. These effects are all trivial. But even with classical assumptions variation of the revolution period is inevitable. The numerous resonances which have been established within the solar system imply that mutual interchange of momentum between the several bodies within the system has occurred, and is probably still occurring. These resonant interchanges are far from trivial.

Resonant periods in the solar system

Earth-Toro Resonance

Consider first the earth capture resonance of the small asteroid Toro, reported by Danielsson and Ip (1972). The orbit of Toro projected on to the plane of the ecliptic along with the orbits of Venus, Earth, and Mars is shown in figure 125, which indicates that Toro crosses Earth's orbit in January and August; but as the revolution period of Toro is 1.6 years, five Toro revolutions equal eight Earth years. The result is that every fifth revolution of Toro (eighth year) there is a close Earth-Toro approach in August, while on the third and eighth revolutions there is a close approach in January. The path of Toro plotted on a co-ordinate system rotating with Earth is shown in figure 126. At the August encounters (the ascending node), Toro is ahead of Earth and travelling faster, and hence his orbital velocity is retarded by Earth's gravitation, reducing his momentum and angular velocity, so that the subsequent January encounters are closer. In the descending node (January encounters), on the other hand, Toro is behind Earth and is accelerated, his period is increased, and again the closeness of the January approach is reduced. The result is that Toro has been captured by Earth in a five orbit to eight orbit resonance cycle. Toro's

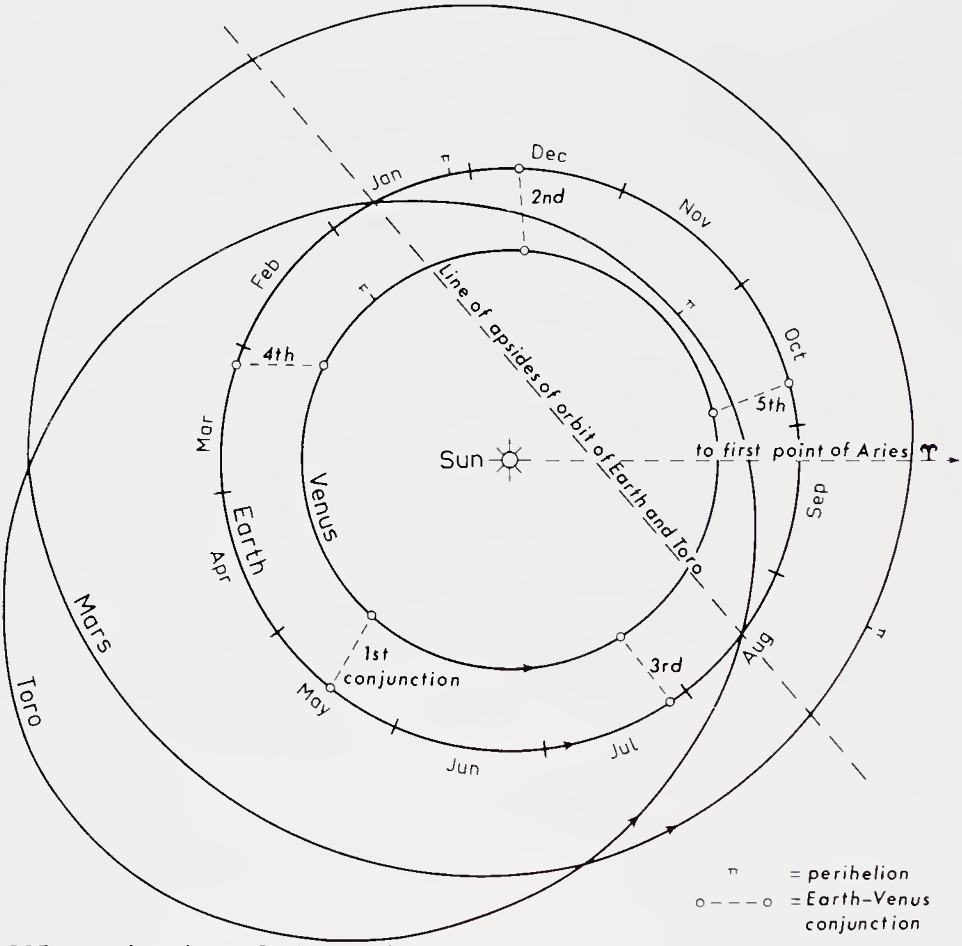


Fig. 125 Projection of the orbits of the terrestrial planets and Toro onto the ecliptic plane. The position of Venus and Earth when Toro passes its perihelion are marked. The data used are from the perihelion epoch of December 1967 and four following perihelions. (From Danielsson and Ip, 1972).

period oscillates between 1.598 years and 1.602 years over a cycle of 144 years, during which time he varies between a closest approach of 0.15 astronomic units in August to a similar close approach in January 72 years later. During this cycle Earth appears to Toro to oscillate between the two close approach positions as shown in figure 126. The point about this resonance capture is that as Toro is accelerated and retarded by Earth, so Earth is retarded and accelerated in proportion, and hence the length of a year fluctuates with the 144 year cycle. During the initial capture (when Toro first became a "male" asteroid, presumably through perturbation by Jupiter) there would be a unidirectional change in the length of the year, positive if the first approach was in the ascending node, negative if in the descending node. Two of Saturn's satellites, Titan and Hyperion, are in a stable 4/3 resonance, and like Earth-Toro conjunctions, their conjunctions librate about the aposaturnium of Hyperion with a period of 640 days.

Plotting Toro's orbit in this way is equivalent to our familiar concept of Moon in elliptical orbit about Earth, whereas in fact Moon's orbit is essentially an ellipse about Sun modulated by an Earth resonance near 13:1 (Fig. 127). It is just as real to show Earth in orbit about Moon, because they orbit as a binary couple whose centre of gravity orbits Sun. The path of Moon is always convex outwards.

Neptune-Pluto Resonance

Like Toro and Earth, Neptune and Pluto are in 3:2 resonance (Fig. 128), and although their orbits intersect, they should never collide because their nearest approach is 18 astronomical units (Cohen and Hubbard, 1965). Except

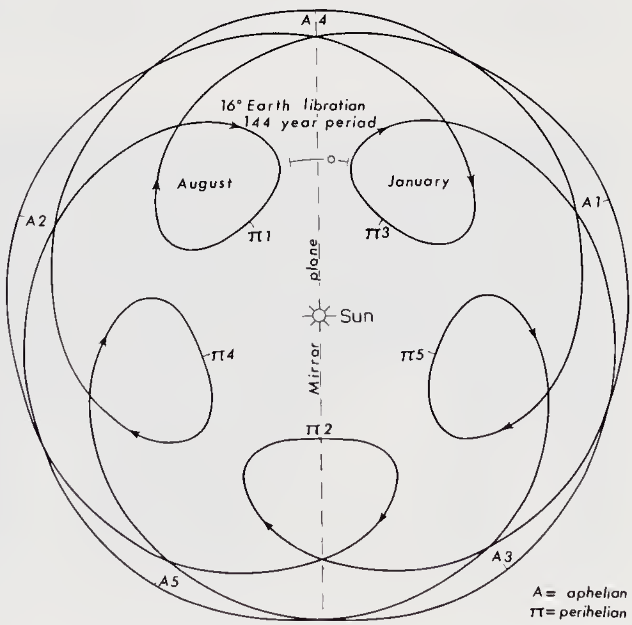


Fig. 126

Projection of Toro on the ecliptic plane in a co-ordinate system rotating with the earth. The fact that Toro's period oscillates around 1.6 years is shown by superimposing an oscillation on the rotation of the co-ordinate system, which makes Earth appear to oscillate along a 16° arc with a period of 144 years. The open circle indicates the position of Earth with respect to the orbital path of Toro in the year 2020. (From Danielsson and Ip, 1972).

Fig. 127 The orbit of Moon (shown by white dots, which are ten times larger than they should be for the scale of the orbit), and the orbit of the centre of mass of the Earth-Moon system (shown by white dashes).

that Earth-Toro is a 5:8 resonance and Neptune-Pluto is 3:2, the orbital patterns are very similar (Figs. 126 and 128). In each case the mirror symmetry line across a closed path implies that the sum of the mutual perturbations is zero, and that a stable capture has been achieved.

Resonances of Other Planets

Jupiter and Saturn are in 5:2 resonance and Jupiter and Mars in 1:12 resonance, and, of course, Earth and Moon in 1:13 resonance. Mercury has a stable three rotations to three revolutions resonance. Venus is in a rotation resonance with the earth in the sense that Venus always turns the same face to earth at inferior conjunction (Goldreich and Peale 1967). Sun is, of course, the focal body of this group and indeed the great solar tidal couple on Venus' deep atmosphere must have been largely responsible for retarding the rotation of Venus almost to its revolution period, which is just what Earth did to her satellite, Moon. This process lengthened the Venusian year. This solar control would result in Venus always presenting the same face to Earth, even without help from Earth. But Earth

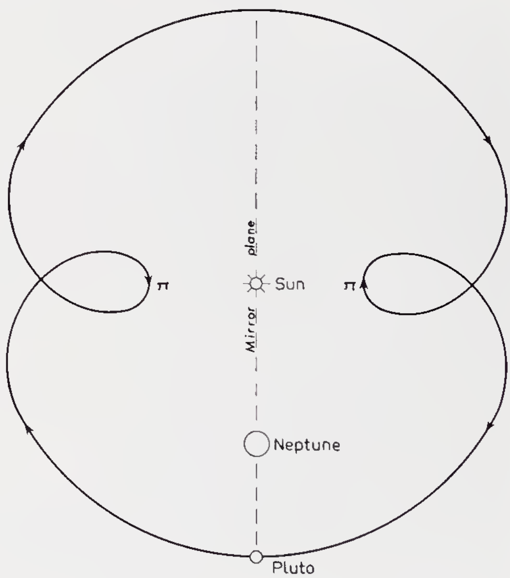


Fig. 128

Pluto's orbit projected on to Neptune's orbital plane in a co-ordinate system revolving with Neptune, to show the mutual resonance capture of these two planets.

does help lock this resonance because Earth's tidal attraction on Venus is forty times greater at conjunction than at opposition and at resonance the resulting tidal drag is zero. Evidence is growing that Earth, Moon and Venus are involved in a three-body resonance system. Indeed the tetrad, Venus, Earth, Moon and Mars, are close to resonance in their mean motions (Dermott 1973). Dermott goes further to point out that there is a significant preference for near-commensurability among pairs of relative mean motions throughout the solar system. Jupiter's sidereal period (11.86 years) is the main element in sunspot cycles, and in Sun's motion about the mass-centre of the solar system (Fig. 131). Jupiter's sunodic period with Earth (1.092 years) shows up in Earth's polar motion (Fig. 118), presumably through atmospheric effects.

Kirkwood Gaps

More than a century ago, the American astronomer, Daniel Kirkwood, concluded that the gaps between Saturn's equatorial rings (Fig. 129), were due to resonance perturbations by Saturn's three inner satellites, Mimas, Encelades, and Tethys (because they are so close and so frequent), and Saturn's largest satellite, Titan (because it is so massive - it is larger than Moon). The most conspicuous dark gap, nearly 5000 km wide (the Cassini division between the A and B rings), corresponds to orbital periods straddling eleven hours, which is half that of Mimas (22.62 hours), one-third of that of Encelades (32.88 hours), one-quarter of that of Tethys (44.30 hours), and one-fifth of that of Dione (65.68 hours). Franklin and Colombo (1970) have successfully reproduced all of the rings and their divisions by this resonant model. Similarly the asteroid orbits occupy a band 320 million km wide, but orbits corresponding to one half, one third,

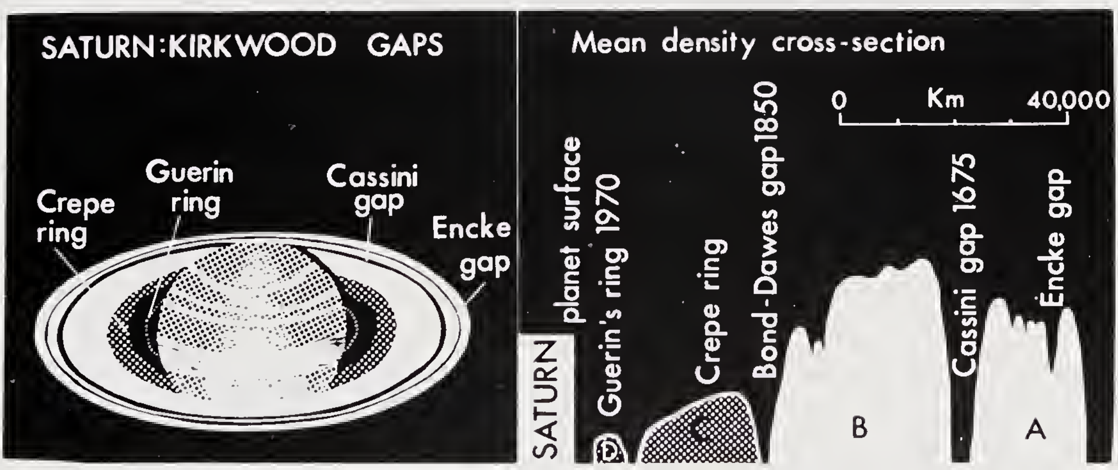


Fig. 129 Mass-density profile across Saturn's rings.

one quarter, two thirds, and other simple functions of Jupiter's period, have been swept clear of asteroids, leaving gaps like the Cassini and Encke divisions of Saturn's disc (Fig. 130).

Bode's Law

Pre-eminent over all these resonances is the primary resonance of the solar system, discovered in 1772 by the Prussian astronomer, Johann Daniel Titus, which became known as Bode's Law after Johann Elert Bode, founder and editor of *Astronomisches Jahrbuch*, who publicised Titus' empirical relation:

	Mercury	Venus	Earth	Mars	Asteroids	Jupiter	Saturn	Uranus	Neptune	Pluto
A	4	4	4	4	4	4	4	4	4	4
B	0	3	6	12	24	48	96	192	-	384
C	4	7	10	16	28	52	100	196	-	388
D	3.9	7.2	10	15.2	~28	52	95.4	192	307	395

which gives each planet an initial 4 (Row A), to which is added a geometric progression of 3 with multiplying factor 2 (Row B), and the sum of Rows A and B is Row C, which predicts the distances of the planets from the sun, taking the earth distance as 10. The "law" is moderately satisfactory. It correctly predicted the existence of Uranus (discovered in 1781), and the existence of the asteroids between Mars and Jupiter, discovered in 1801. For mathematical consistency, Mercury should have 1½ instead of zero in Row B; Neptune is a cuckoo in the nest (though it could be argued that Neptune-Pluto is a binary (Fig. 128) and should be combined like Earth-Moon). Several authors have subsequently proposed refinements of Bode's law to reduce the discrepancies, notably Wurm (1803), Gaussin (1880), Belot (1911), Reynaud (1919), Pierucci (1923), Penniston (1930), Mohorovicic (1938), and Melchior, 1947. Some of these make an inner and outer set of planets with different progression functions. These formulae and the extensive literature have been reviewed by Melchior (1947).

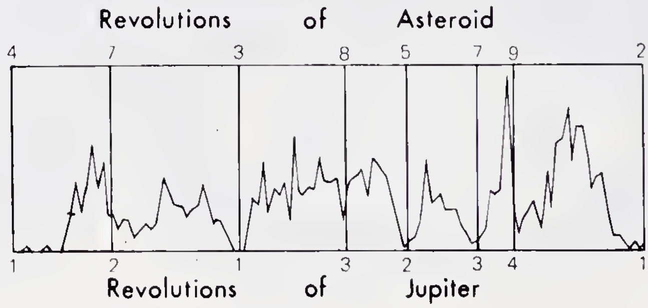


Fig. 130

"Kirkwood gaps" in the asteroid system.

However these defects really originate from two false notions incorporated in this law by Titus.

First, the solar distances are stated in *Earth* units, (in Row D, the distance of the earth is taken to be 10). Second, solar distances are used rather than orbital periods. Superficially neither of these should be material, as all data should be commutable between units, and periods are directly related to solar distances. However it is the synodic period with the reference planet (not the sidereal period) that is relevant, and that does depend on which planet is adopted as base. The difference is not large, so the Titus formula does work reasonably well. But in selecting a reference planet for the solar system Earth should be a minor candidate and Jupiter must prevail.

Sun-Jupiter Binary

To any but an earthling, the special status of Jupiter should have been obvious. But such is but another case of human egocentricity which has blurred and retarded progress of science. Man, who created his god in his own image (male and pale), found it axiomatic that Earth should pivot the solar system and the universe beyond, and adopted here-and-now as norm, in obliquity, in gravity, in the states of matter, and of all phenomena of nature.

McDonough (1974) reminds us that "Jupiter resembles the Sun more than the Earth. In composition, energy production, radio noise, differential rotation, field eccentricity, and the interaction of its plasma with its magnetic field, Jupiter displays truly stellar qualities". Sun-Jupiter is a binary couple, an average yellow star with a faint weak companion star.

Even starting with a globular distribution, the gravitational perturbations of the Sun-Jupiter binary couple would eventually redistribute all matter in the orbital plane of the binary couple - hence the ecliptic. (However, egocentric man has adopted Earth's orbital plane as the ecliptic, not Jupiter's, which differs by $1^{\circ}.3$). The repetitive perturbations would eventually sweep clear all nodal zones having simple functions of their mutual period of revolution - hence Bode's law.

Jovian Resonance Law

But just as there are instabilities, there are stable captive resonances such as the Earth-Moon, Earth-Toro, Neptune-Pluto systems, and it is these which are expressed in Bode's Law. However Bode's Law should be rewritten with Jupiter as origin and with Row C as the synodic periods as seen by Jupiter:

	Mercury	Venus	Earth	Mars	Asteroids	Jupiter	Saturn	Uranus	Neptune	Pluto	Trans-Pluto Planet?
A	$\frac{1}{48}$	$\frac{1}{24}$	$\frac{1}{12}$	$\frac{1}{6}$	$\frac{1}{3}$	-	3	6	12	24	48
C	$\frac{1}{49.2}$	$\frac{1}{19.3}$	$\frac{1}{11.9}$	$\frac{1}{6.3}$	$\sim\frac{1}{3}$	-	2.5	7.1	13.9	20.9	?

This procedure removes the two serious anomalies of the Titus-Bode version; neither Mercury nor Neptune are special cases outside the law.

A quite unexpected symmetry about Jupiter appears, - which should not be surprising, for what body other than Jupiter could be the fulcrum for solar period symmetry in a Sun-Jupiter binary system? Symmetry even suggests an undiscovered planet beyond Pluto. Indeed Rawlins (1970) has suggested such an "alien perturbation" to explain what he calls "the great unexplained residual" of seven seconds of arc in the position of Neptune, when its orbit is calculated back from the discovery sighting in 1846 to an earlier recorded sighting in 1795, and Brady (1972), to explain residuals in the reappearance of Halley's and other comets, deduced that this tenth planet X needed to have a mass only slightly less than Jupiter's at 60 astronomical units from the Sun (giving it a magnitude of 13 or 14) in a highly inclined orbit (120°). However an authoritative study by Duncombe (1973) established that, if these orbital elements were correct, the magnitude must be beyond 16 or surveys made would have observed it.

The numerical agreement is good but not perfect, probably because Row A relates each planet individually to the Sun-Jupiter binary without allowance for the mutual attractions of the other planets. This might be the reason why Neptune and Pluto, who are in resonance with each other (Fig. 128), have numbers in Row C drawn nearer to each other than the binary prediction in Row A. There may be other corrections.

The revised law suggested above has the form:

$$Tn = 3J2^{n-1}$$

where J is Jupiter's sidereal period, and Tn is the Jovian synodic period of the n^{th} planet outside Jupiter; the reciprocal of this expression applies to planets inside Jupiter.

A potential contradiction emerges here. On the one hand, simple fractions of Jupiter's period are unstable, as indicated by the Kirkwood gaps, but on the other hand, specific fractions involving 3 and 2^n are specially stable, as indicated by the Jovian resonance. Clearly an additional factor is involved. First, one is bound to wonder, what is special about 3, the initial factor, and about the systematic multiplier 2? These could scarcely be arbitrary, and presumably arise from multi-body dynamics. Lagrange (1772) found the three-body problem intractable except in some special cases. However he found that where the third body led the second body by one sixth of a revolution, or followed by this amount, a stable equilateral-triangle configuration resulted, hence the Jovian asteroids, which share Jupiter's own orbit, a potentially lethal situation among all such, but lead or follow Jupiter in Lagrangian resonance. The Jovian asteroids won't hit Jupiter, Toro won't hit Earth, Moon won't hit Earth, Pluto won't hit Neptune, even though each crosses the other's orbit; they are in resonant "troughs" where small perturbations are brought back to the trough as explained for Toro above (p. 312-314). What Lagrange found was a stable resonance. I therefore suspect that the modified law above expresses the fact that after all the reiterative cycles suffered by the solar system, only stable configurations remain, and that the recurring factor 3 is a condition for such stability for three bodies - Sun, Jupiter, and the test planet. The multiplier 2 is also to be expected.

Ovenden (1972) successfully explained the resonant distribution of the planets and of their satellite systems by his "Principle of least interaction". He explains that "a satellite (or planetary) system of N point-masses moving under their mutual gravitational attractions will spend most of its time close to a configuration for which the time-mean of the action associated with the mutual interactions of the satellites is an overall minimum."

Resonance of Planetary Satellites

Similar resonant systems have developed for the satellites of the planets. Dermott shows that the five satellites of Uranus are a near-resonant system close to a Bode's Law of the form:

$$Tn = ToA^{n+d}$$

	n	Tn calculated	Observed
Miranda	1	1.40	1.41
Ariel	2	2.49	2.52
Umbriel	3	4.43	4.14
Titania	4	7.78	8.71
Oberon	5	14.1	13.5

Likewise Jupiter's satellites Io, Europa, and Ganymede are in stable three-body resonance. In fact Ovenden (1972) found that Jupiter's inner five satellites form a resonant system in harmony with his "principle of least interaction action". The Trojan asteroids are in resonant Lagrangian orbits with Jupiter and are held captive in those positions. Saturn's satellites, Mimas, Enceladus, and Titan are also in stable three-body resonance. Orbits resonant with Titan, his largest satellite, are unstable, and are swept clear. Saturn's seventh satellite Hyperion, was sought and discovered in 1848 because the 4/3 resonance law with Titan suggested there should be a satellite there. Mimas-Tethys and Enceladus-Dione are respectively in 2/1 resonance.

Secular and impulsive variation in the solar system

To sum up, the solar system is vibrant with resonances, tones, and overtones, like a random tray of sand which has been perturbed by vibrators for long enough to develop complex systematic patterns. As every grain of sand has jostled its position in relation to its neighbours, so every body in the solar system has varied all elements of its motion in response to the perturbations of its fellows. The dominant directors have been the Sun-Jupiter binary, and if any such a binary started with a random field of associated bodies, reiterative perturbations through the aeons would have produced a resonant system such as we see, in the plane of the dominant binary. But every particle in the system has contributed its tittle in determining the motions of all the others.

These conclusions have great significance in the palaeotectonics of the earth. It is false to assume that any of the elements of motion of any of the bodies have been constant. All have been modulated by the symphony of the whole. Variation of the gravitational constant, G, adds another factor to the variation. Nor is it valid to assume that all the major changes occurred early, or at least during the first aeon, and that perturbations have progressively diminished with time. The mathematically intractable stabilities and instabilities of the multi-body gravitational problem pollutes with uncertainty retrospection of the history of the solar system. Rare unstable configurations seem probable, analogous to systematic instabilities in radioactive nuclei. Such an unstable configuration would cause significant regrouping. Perhaps the statistical diffusion relocation of atoms in an otherwise stable crystal lattice is a better analogue.

Such astronomic spasms may be the answer to some of our present enigmas. For example, all who have attempted to project back the course of the Earth-Moon system have found the moon close to the earth in the late Proterozoic, which is clearly contrary to the geological record. However an instability reshuffle somewhere in Phanerozoic time could have initiated the Earth-Moon courtship, first as distant resonant mutual perturbation, with progressively tighter resonant coupling from reiterative perturbations at each conjunction, and ultimately as the present permanent marriage (Fig. 127). Capture in a single pass would have produced drastic effects which would have been obvious in the geological and selenological records.

Ovenden (1972, 1973) concluded from his study of the evolution of the resonance of the Jupiter-Saturn-Uranus-Neptune tetrad, and independently from the resonance of the Mercury-Venus-Earth-Mars-Jupiter group, that an asteroidal planet (which he named Aztex) as massive as Saturn was implied right up to 16 million years ago. Ovenden pointed out that whereas the rubidium-strontium and uranium-lead ages of meteorites indicate ages around 4.5×10^9 years, the cosmic-ray exposure ages of chondrites and achondrites lie between 2.2×10^7 and 5.0×10^6 years, tailing off towards the younger end - wholly consistent with primary disruptions. However he was left with the enigma of what happened to Aztex so recently, and of the fate of 99.9% of its mass.

The resonance of asteroidal orbits under Jupiter's helm seems to be significantly less developed than that of the solar system as a whole, which accords with the recency of the disruption of Aztex. Professor Guskova (1975) has deduced from the remanent magnetization of meteorites that the asteroids are the remnants of a space catastrophe which destroyed the asteroidal planet which she called Phaeton, which had been a little smaller than Earth. (Tatsch, 1972, had called this asteroidal planet *Aster)*. However, many asteroid specialists discredit Aztex-Aster-Phaeton, and believe the asteroids to be primitive planetesimals, which have suffered some fragmentation by mutual collisions. (See, for example, the popular review by Chapman, 1975, and specific rebuttal of Ovenden by Napier and Dodd, 1973).

Sun's orbit about centre of solar system

A complete cycle from the approximate conjunction of all planets in line on the Sun-Jupiter axis takes about 179 years varying by a few years according to when the conjunction falls in relation to Pluto's perihelion, as Pluto's orbit has the highest eccentricity (0.249) of any planet. The 179 year cycle is less than one revolution of Pluto (248), for clearly, having started from alignment, the shorter period planets complete their cycle to conjunction and realign with Pluto before he has completed one revolution (see Gribbin and Plagemann, 1974). Neptune orbits in 164.8 years, which takes him back to the starting point, but he has to go on further before he catches up with Pluto. Saturn reaches the grand conjunction after six orbits and about a tenth, while Jupiter makes fifteen revolutions plus a tenth. The inner planets make several conjunctions with each of the others before the culmination in the grand conjunction.

Jose (1965) reported that the variation in the motion of the Sun about the centre of mass of the solar system has a periodicity of 178.7 years (Fig. 131). Indeed it would have been astonishing if a result had been found differing from the cycle from one grand conjunction of the planets to the next! The sunspot cycle has the same 179 year periodicity, and the dominant element in the sunspot cycle is, of course, Jupiter's period of 11.86 years.

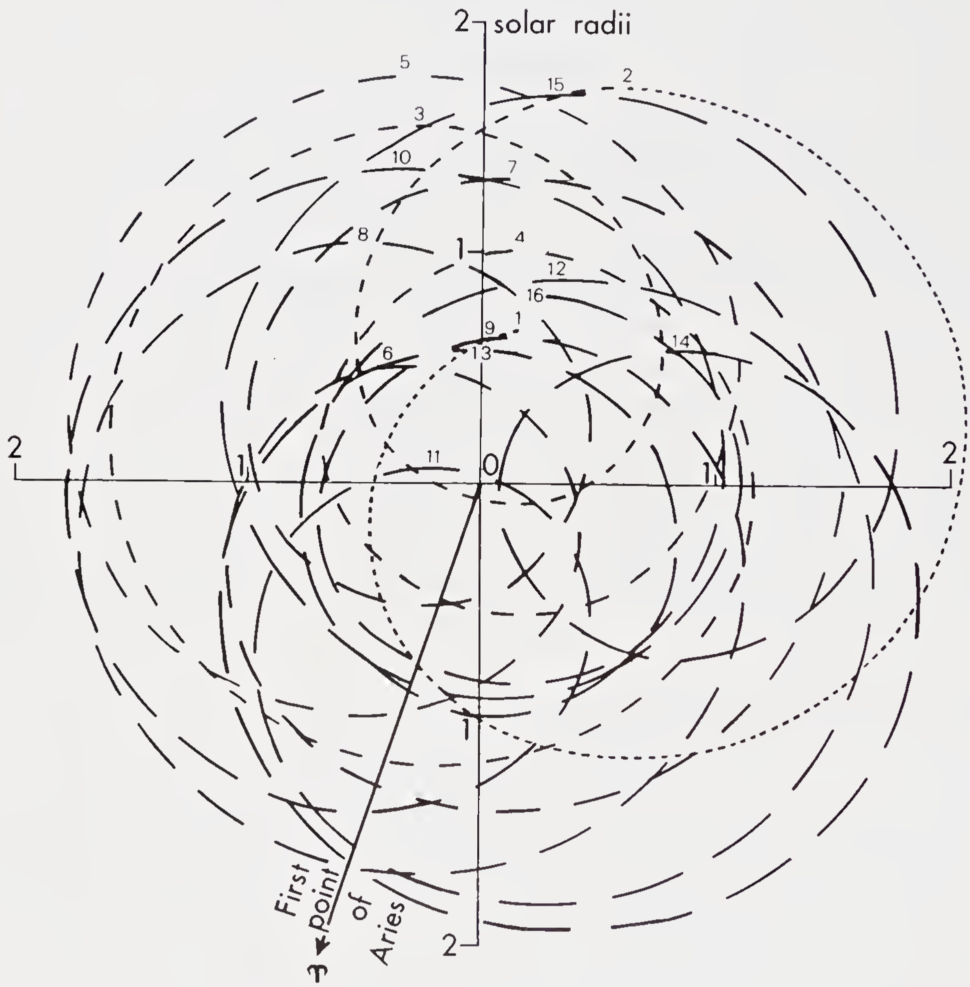


Fig. 131 Sun's motion about the mass-centre of the solar system. (After Jose, 1965).

MODES OF CRUSTAL EXTENSION

Cambium ridges

In the earliest speculations on continental displacement, new oceanic crust had to appear between the separating continental blocks. By 1956 at the Hobart symposium on continental drift, it was clear that median ridges such as the mid-Atlantic ridge were the locus of current active growth and that increments were in the form of paired slices progressively older away from the ridge (Carey, 1958a, fig. 3 and Fig. 7 herein). Rothé (1954) had already detailed the course of these fracture zones through the Atlantic and Indian Oceans, and my own seismic station in Hobart had traced this zone through the southern ocean into the Pacific. In the Holland Memorial Address (Calcutta, January 1964) I referred to these as "cambium ridges" to convey the concept that these ridges continuously begat paired ridges which moved apart for younger pairs to be produced between them. At the 1956 symposium I made it clear that this process could occur entirely in the solid state without the production of lava, but as the cambium ridge was necessarily a zone of greatly elevated isotherms, magma production was probable, in small or large amounts, and that this would take the form of minor or major lava extrusions, or of sheeted dykes. In such cases generation of transverse ridges was probable, forming permanent nemataths to mark the path of the separation (like medullary rays -- to extend the cambium metaphor).

Although the cambium ridges were accorded major significance, it was always clear to me that this was only one of the modes of crustal extension, and that several other modes (dyke swarms, trenches, disjunctive seas, sphenochasms, undation-swells of van Bemmelen, cymatogens of King, etc.) also made a large contribution to this overall expansion of the crust. However when the new breed of plate tectonicists usurped this already well-developed field, they grossly restricted these processes. Crustal regions became rigid plates which interacted only at their interfaces by separation, convergence or transcurrency - full-stop. Truly, the concept has been relaxed a little since to accommodate the "small-ocean" basins and a little intraplate activity - but the basic concept of new crust generation by paired ridges at the median ridges remains supreme, and all growing ridges are forced into this mould.

The wax model experiments of Oldenburg and Brune (1972) found that there was a range of combinations of rate of extension, viscosity, and thermal gradient, which yielded a pattern of spreading ridges and orthogonal transform faults very similar to observed patterns in the mid-Atlantic ridge. But with other combinations of these parameters, growth was asymmetrical, with accretion on one side only of the spreading ridge. Plate tectonicists have assumed gratuitously that because some spreading ridges grow symmetrically with paired slices, all must do so. When the North Pacific crust showed an extensive spread of growth slices in the west without their eastern twins, it was assumed that thousands of kilometres of mirror-image crust must have been emplaced because the model required it, but because it is no longer there, it must have been seduced and abducted to the netherworld. In fact, mirror region was never born. Its conception, birth, and destruction are all imaginary.

My thesis is that spreading ridges only grow symmetrically if the combination of parameters yield such behaviour. At any one time, growth could be symmetrical in one part of a long zone, unilateral along another part. Symmetrical and unilateral growth may succeed each other along a growing ridge if the combination of controlling parameters so demand.

Just as the orogenic girdle of the Pacific may also, quite correctly, be regarded as an orogenic girdle of Pangaea, so the cambium ridges, which have commonly been dubbed mid-oceanic spreading ridges, can also, and probably more appropriately, be regarded as peripheral accreting zones surrounding each continental polygon. Bias to the former conception may obscure the truth. Each continental nucleus is surrounded by a cambium zone which is a bloated caricature of itself. As the nuclei form a polygonal pattern on the globe, the cambium ridge must form triple junctions between each three contiguous polygons. The triple junctions in the Indian Ocean were apparent from the start, but the triple junctions in the south Atlantic, between South-America Antarctica and Africa, the triple junction in the south Pacific between South-America Antarctica and Australia, the triple junction in the north Atlantic between North-America Africa and Europe, and the triple junction in the equatorial east Pacific between the Americas and the rapidly growing southwest Pacific, have commonly been disregarded or overlooked.

Plate tectonicists usually depict the East Pacific rise as continuing north into the Gulf of California. Whereas this is essentially valid, this arm of the triple junction is the weakest of the three. The most active of the three arms separates North and South America and runs up the Cocas ridge, offsets along the Mid-American trench; crosses the Guatemalan isthmus to the Gulf of Honduras, thence along the Bartlett trench (Fig. 164). The transverse extension between the American polygons across this zone is of the same magnitude as the transverse extension across the East Pacific rise, and it forms an essential segment of the peripheral accreting girdles of both North and South America.

Many second order triple junctions occur along the cambium ridges, because the primary polygons are broken up into second order polygons, which are zones of secondary extension, outgassing, rising isotherms, secondary seismicity, and occasional vulcanism. In the simplest form they develop the whaleback rises described below. The Walvis and Rio Grande rises fit into this category, with triple junctions against the median cambium ridge of the South Atlantic. They have also been interpreted as nemataths. Such dual classification is not necessarily invalid, as the implied processes are compatible and synergistic.

Whaleback tumours

Rising from the normal Pacific oceanic floor more than four km deep are long non-seismic whaleback arches, for which the Eauripik-New Guinea rise serves as a type. This swell runs meridionally for 600 km dividing the Caroline Basin into two (Den and co-authors, 1971). It is a broad arch, about two km high and some 300 km wide (Fig. 132). Continuous seismic reflection profiling indicates that bathyal sediments are continuous from the lateral basins over the top of the arch. JOIDES drilling (J62) shows 570 m of Neogene to Upper Oligocene calcareous pelagic oozes, with increasing induration and silicification downwards to a flow of Oligocene basalt. The calcareous oozes thin towards the flanks; this has been attributed to increasing solubility of $CaCO_3$ with depth. Seismic refraction indicates that the Mohorovicic discontinuity plunges from a normal depth of 10 km to about 20 km under the rise, which means that the layers above the Moho increase from 6 km to 18 km.

Some rises of this type show a simple unbroken arch. Others like the Eauripik-New Guinea rise show a number of small grabens with occasional horsts on the flanks. With increasing development of such tensional rifting such rises progress with the normal pattern of a spreading ridge. It is only a matter of degree.

Rises of this type may develop anywhere on normal oceanic floor where extensional fracturing leads to outgassing from the deep interior. The emerging gases (mostly water - hence the oceans) raise the isotherms and depress the phase-transition levels for mantle minerals, so that material which was below the Moho transforms to layer-3 material, and the Moho moves down through the mantle. Hence the zone swells like rising dough, still with the same material in the column, and still in isostatic equilibrium.

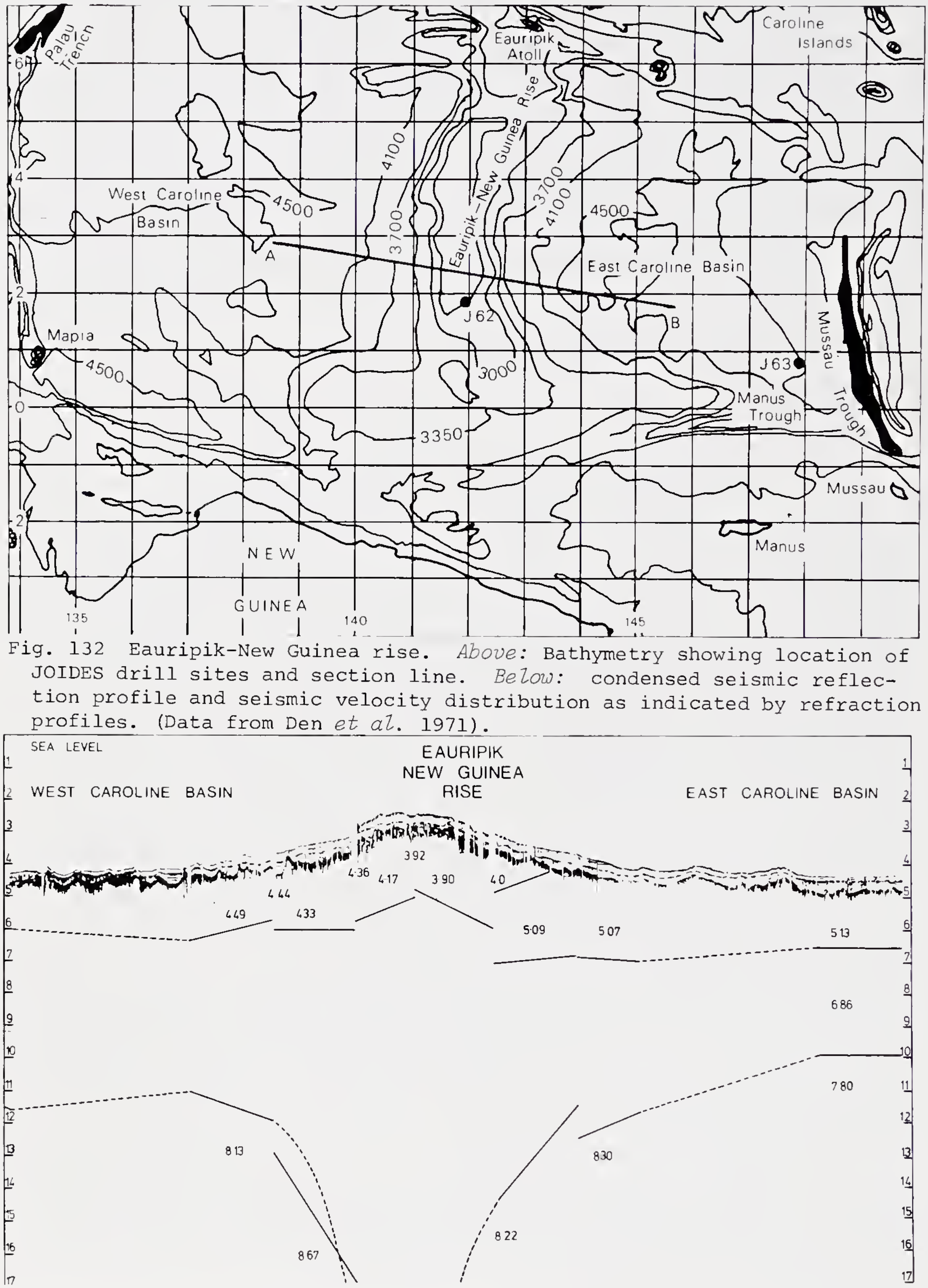


Fig. 132 Eauripik-New Guinea rise. *Above:* Bathymetry showing location of JOIDES drill sites and section line. *Below:* condensed seismic reflection profile and seismic velocity distribution as indicated by refraction profiles. (Data from Den *et al.* 1971).

Gravity and seismic profiles agree in the presence of a lower-density, lower-velocity "root" and "crust", thickened by a factor of three, if crust be defined as supra-Moho material. Hydration (serpentinization) may also be involved when the pressure-temperature conditions permit. At more advanced stages of the process partial melting below the root may contribute basalt lavas and dykes and later dioritic plutons to add to the crustal layer. Cessation of the outgassing at the stage a little earlier than now reached by the Eauripik-New Guinea rise could allow the isotherms to subside, the phase transitions to rise, and the Moho to return to normal depths, and hence the rise and root would cease to exist.

The Shatsky rise in the northwest Pacific repeats all the characters of Eauripik-New Guinea rise except that the sediments are thought to go down into the middle Mesozoic (Heezen and Hollister, 1971, and Ludwig, Nafe and Drake, 1970). Tensional horsts and grabens are more advanced. The crest shoals to 2 km from flanking basins deeper than 5 km, and the Moho descends from 14 km to 25 km, giving a crustal thickening from 8 to more than 20 km. The submarine arch immediately east of the Solomon Islands, trending north to break surface in the Ontong Java plateau, is also of this kind.

The Solomons archipelago itself commenced in just the same way, but is at a more advanced stage. Indeed this applies to the whole of the so-called "outer Melanesian arc" which includes the northern ranges of New Guinea (Bewani, Torricelli, Prince Alexander, Adellbert, Finisterre, Saruwaged), New Britain, New Ireland, Solomons, New Hebrides, Fiji, Samoa, Tonga, and the Kermadecs to New Zealand. Right along this line late Mesozoic pelagic limestones overlie a couple of kilometres of basic volcanics (lavas, breccias, etc.) resting on ultramafic rocks, now mostly sheared serpentinites. The whole complex arched upward and shoaled in the Eocene, to receive shallow foraminiferal limestones, which were eroded along with the underlying pelagic limestones, volcanics, and ultramafics, to yield detritus to the adjacent grabens. In the late Eocene and Oligocene igneous and tectonic violence erupted all along the belt, with intensified horsts uplifts and deepening troughs, extrusive basaltic and andesitic tephra, and dioritic plutons, disrupting and complicating the overall arching of the belt, which has continued without significant break to the present. The Troodos whaleback arch of Cyprus is cast in the same mould.

The inner Melanesian belt running through the main New Guinea highlands, the Louisiade archipelago, and New Caledonia, to the Auckland peninsula has the same pattern, one stage older. Still further on towards the Australian craton, the New England orogenic zone had the same history; the ocean floor commenced its arching in the Devonian, and the Pennsylvanian vulcanism and magmatism culminated in the Permian; and this zone has been rising ever since. Two multichannel reflection profiles across the Tonga ridge have revealed a similar genesis, a whaleback arch with numerous horsts and grabens. The basaltic basement is Middle Eocene(?) overlain by Upper Eocene and Lower Oligocene limestones with pockets of Middle Oligocene clastic limestones, Upper Oligocene carbonates admixed with detritus, then Lower Miocene tephra (Kroenke and Tongilava, 1975).

The whaleback arch of the sea floor, progressing through vulcanism, plutonism and orogenic regurgitation, is a fundamental mode of crustal extension, resulting from outgassing of the deep interior. All stages can be studied in age progression from the newborn whaleback arch to the old deeply eroded orogenic tumour. The young simple arch may progress either to the spreading ridge or to the orogenic tumour according to the rate of extension. Fast extension yields a broad rise with extensive horsts and grabens, large volumes of submarine extrusion, no subaerial erosion and little sedimentation. Slower extension leads to arching to sea level, reef limestones, extensive more differentiated vulcanism and plutonism, extensive sediments, greater vertical relief, and gravity spreading of the sediment overburden. Remove the sediment cover from the Cordillera and the Rocky Mountains, and there is little remaining difference in scale, relief, and geophysical parameters (seismic, gravity, or thermal) between the Cordillera and the Mid-Atlantic ridge.

Deckenschrund ridges

The tensional origin of the trenches has already been discussed (p. 63). Most now agree that the seas between the trenches, island arcs, and the continental craton, are disjunctive basins floored by young oceanic crust. The ridges themselves have all developed initially as whaleback arches in the manner just described. In general the basement rocks are ophiolites (*sensu lato*), serpentinites, and other derivatives of oceanic crust, overlain commonly by a kilometre or more of altered submarine lavas and tephra, then pelagic fine-grained carbonates and cherts, then commonly by shoaling facies. Next this complex which continues to rise sheds all these facies to lateral troughs as conglomerate breccias and turbidites, while vulcanism and plutonism develop through the broken arches. In all these ridges, the pattern is similar, although the age of onset ranges widely, but in ordered procession. This genetic process is not static, but migrates in accordance with the displacement pattern superimposed by the global expansion. In the western Pacific, both north and south, this kinematic pattern is illustrated diagrammatically in figure 133.

When the opening of the eoPacific reached a width such that a new accommodating zone was necessary, arch-ridges developed in the oceanic crust along the eastern margin of Asia and Australia-Antarctica. The continuing western migration of the continents (because of their greater moment of inertia with respect to equivalent oceanic lithosphere in isostatic equilibrium with them) caused crustal accretion along this front and the growing zone was left behind, just as the terminal moraine is left behind by the retreat of the glacier which fed it (Fig. 133b).

Several concurrent processes complicate this simple model:
First the Tethyan torsion superimposed sinistral offset across the Tethyan zone (Fig. 133c). This is clearly shown in figures 170 and 193.
Second, expansion was progressively greater in the south, so that the expansion between the Marianas and eastern Asia was greater than between Japan and eastern Asia, and the expansion between Australia and Tonga was still greater.
Third, the process was pulsed. After the earliest whaleback arch orogen had climaxed, and been left a little behind by the retreating continent, a new pulse spawned a new whaleback arch on the sea floor immediately east of the older orogen, and this new orogen was left behind, with a new disjunctive sea developing between it and the earlier orogenic arch. This cycle was repeated three or four times.

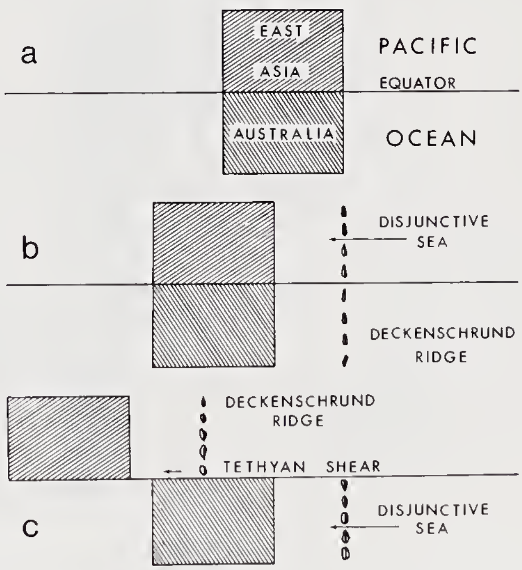


Fig. 133
Pattern of development of deckenschrund ridges of the western Pacific

Fourth, thermal feedbacks in diapiric processes always tend from the linear uniformity of a diapiric wall towards piercement through a line of domes. For salt domes, the spacing tends to be a few tens of kilometres, depending on the thicknesses of salt and overburden. For lithospheric processes, the scale is a few hundred kilometres, determined by the thickness of the lithosphere. Hence the disjunctive zones tend towards chains of ovoid

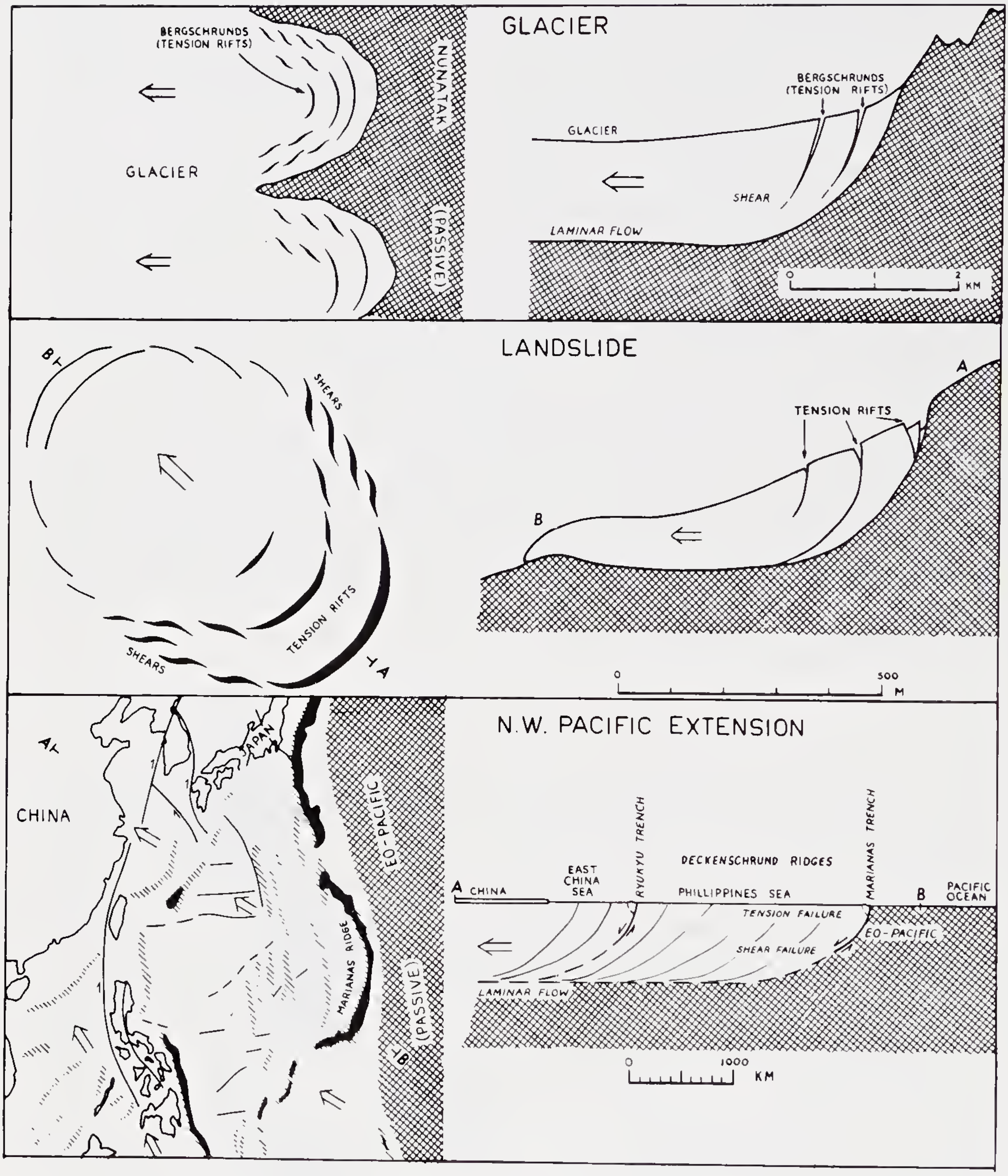


Fig. 134 Comparative morphology of Bergschrund, landslide and Deckenschrund

tumours with new oceanic crust, and the orogenic whaleback ridges become arcuate round the tumours. Another effect of the thermal feedback is that the whaleback orogens themselves break-up, boudin-like, as they stretch longitudinally, and hence inevitably become a chain of island boudins, like a string of beads.

The retreat of Asia away from the island arcs and trenches and the static Pacific, resembles the pattern of a glacier moving away from its headward nunataks opening *Bergschrunds*, or the pattern of a slab-type landslide, leaving tension gapes at the head against the stable base (Fig. 134).

Dyke swarms

Filling of extensional fractures with basaltic lava has always been an important mode of low key crustal extension. Most old cratons have suffered definite epochs of extensive dyke intrusion. Holmes (1965, p.250) reported that along a fifteen-mile stretch of the coast of Arran a swarm of 525 dykes have a total thickness of 5410 feet, implying a local extension of the crust of one in fifteen. Macquarie Island, the most distant island of the State of Tasmania, consists for a large part of its area of an intense complex of dykes, which imply extension (at least for some kilometres) of at least fifty per cent. Similarly the sheeted intrusive complex on Cyprus consists largely of meridional basaltic dykes which add up to ninety per cent extension, and imply a latitudinal extension of 130 km.

Categories of submarine ridges

This discussion of the modes of crustal extension leads to a genetic classification of the categories of oceanic ridges:

Continental splinters: Initial disruption of a continent along graben-and-horst zones, often en echelon, and often anastomosing, commonly results in splinters, slivers, and slices of continental material, which may remain adjacent to the continent (like buried horsts off the Atlantic coast of America) but which may be separated from the continent by new crustal growth. Such are the Maldive-Laccadive-Chagos line, the Seychelles and Mascarene ridges in the Indian Ocean and the Rockall splinter in the North Atlantic. The Ruwenzori horst and the Danikil horst show two stages of development. The Cayman ridge shows the same pattern in a sphenochasm.

Cambium ridges: These are synonymous with the "mid-oceanic" ridges, or "median" ridges. But they are not always mid-oceanic, and they cross orogenic belts where an orogenic zone had extended across two continents which have been separating (such as the Rockies-Andes orogenic zone, and the Southern Andes-West Antarctica orogenic zone). They also run into and merge into the widening belt along the Tethys. In both these cases the morphology of the cambium ridges changes substantially, because the crustal extension involves stretching (orotaths) and bending (oroclines), as well as horsts and grabens.

Whaleback arches: such as the Ontong Java, Shatsky, Eauripik-New Guinea rises, the Troodos arch, etc. Many orogens commence in this mode, but become increasingly complicated through strong rift-and-graben faulting, vulcanism and plutonism and complex folding and overthrust spreading of the superficial, but locally thick, sediment cover.

Polygon arches: These are the boundaries of the second-order polygons which must develop even in oceanic crust to allow adjustment of the spherical segment to increasing radius. Examples are the boundaries of the Cocos, Nasca, Peru, Chile, and Bellinghausen polygons shown in figure 19. The Walvis and Lomonosov ridges also appear to serve this function, but this does not preclude them from filling at the same time some other synergistic function (e.g. Tethyan torsion, nematath, etc), for the rocks of the crust do not know genetic categories, only the algebraic sums of thermal and stress conditions in the crust, to which they respond.

Nemataths: from which the concept of mantle "plumes" arose. However nemataths develop from the growth of oceanic crust by accretion from a hot-spot on a cambium ridge, not from motion of consolidated lithosphere relative to a fixed hot-spot in the mantle. "Plume traces" on continents (if they exist) could not be nemataths because lithosphere crust remains welded to its own chunk of mantle, according to my view. Examples of nemataths are the Lomonosov, Rio Grande, and Walvis ridges. In reconstruction of earlier continents the ends of nemataths linking across an oceanic rhombochasm must be brought together.

Orotaths: are grossly elongated sections of an orogenic zone. They are the submarine ridge links between the island "boudins" of such an elongated orogen. In reconstructions the orotaths must be greatly shortened along their lengths, so that the ends of the orotaths are brought close together. Examples are the Aleutians, Kuriles, Ryukyu, and Andaman-Nicobar submarine ridges.

Oroclinotaths: are inflected orotaths, such as the Banda, Scotia and Lesser-Antilles loops.

WESTERN NORTH AMERICA

TECTONIC SETTING

Major structures

Western North America presents a diverse and superficially contradictory combination of major tectonic features, which nevertheless did develop concurrently in space and time. They are cognate, they co-exist, and hence must arise from a coherent tectonic regime, albeit with independent warp, woof, and interweave.

Concomitant Structures of Western North America

Cordilleran truncation of Precambrian grain (Fig. 135)
The Rocky Mountain "bulge" (Fig. 164)
East Pacific fracture zones (Fig. 164)
San Andreas Rift system (Fig. 144)
Rocky Mountain trench
Basin-and-range province (Figs. 141-143)
Wasatch-Sevier and Rio Grande rift belts (Fig. 164)
Laramide thrust regime
Cascadan lineament (Fig. 143)
Mendocino and Idaho oroclines (Fig. 143)
Nevadan plutons
Plateau basalt floods
Snake River senkungsfeld (Fig. 164)
Las Vegas trend inflections (Fig. 152)
Colorado plateau (Fig. 164)
Wyoming-New Mexico block-and-basin province
Transverse zone near Canada-United States border (Fig. 162)
Californian transverse ranges
Franciscan belt
Gulf of California and the Sacramento-San Joaquin trough
The Florida-British Columbia lineament (Fig. 161)
Palaeomagnetic and palaeogeographic rotations (Figs. 301, 302, 310, 313)

Dynamics

Four pan-global processes co-operate and mutually interfere in the tectonic evolution of western North America: gravity, octantal rotations, circumferential elongation, and the Tethyan torsion.

Gravity is, of course, the dominant force in the development of this region, as it is for all other regions. Driving forces are vertical, but they fan by virtue of Pascall's principle, and nappe-scale outflow occurs in the higher zones. Superimposed on gravity is the counter-clockwise rotation of the North American block against the north Pacific block (Figs. 138, 139 and 140). Thirdly, the distance between Venezuela via the Cordilleran zone to the Arctic coast has extended by some thirty percent since the Palaeozoic, (see for example any of the reconstructions before the opening of the Atlantic). This extension is mainly expressed at the surface by brittle rifts (Fig. 164). Extension also continued across the Arctic basin. Contemporaneous latitudinal extension was taken up in the growth of new ocean crust as the Pacific Ocean sphenochasm widened (Figs. 69 and 184). Gravity operates on a short-time scale, as isostatic inequilibria of tectonic dimensions attenuate (to $\frac{1}{e}$) in less than 10^4 years, even in cold crystalline crust. By contrast, the block rotations and the meridional extension, are secular, with a time scale a thousand times as long (cf. Figs 38 and 138). The interplay between these three processes accounts for every tectonic lineament of western North America.

Cordilleran truncation of Precambrian grain

Up to about 800 million years ago the tectonic grain of North America was west-southwest. The cordilleran system cuts directly athwart this grain (Fig. 135), in the same way as the New Guinea cordillera cuts across the meridional pre-Mesozoic grain of the Australian craton. In each case the truncated grain predates the cordillera, which developed from sediment and igneous outgrowth along the ruptured continental margin, just as new geosynclinal outgrowths are now developing transversely to the Palaeozoic grain along the ruptured continental margins bordering the Atlantic. The youngest rocks with pre-cordilleran grain are the Beltian in the United States and the Purcell in Canada. They trend right into the cordillera until they are replaced by cordilleran facies resting on oceanic floor. The continuation of these rocks should be sought in east Asia. The first rocks after the truncation are the Windemere group and their correlates, which are not more than 850 million years old. This dates the initiation of the Pacific Ocean.

Stewart (1972) summarizes the initial separation (see Fig. 136):

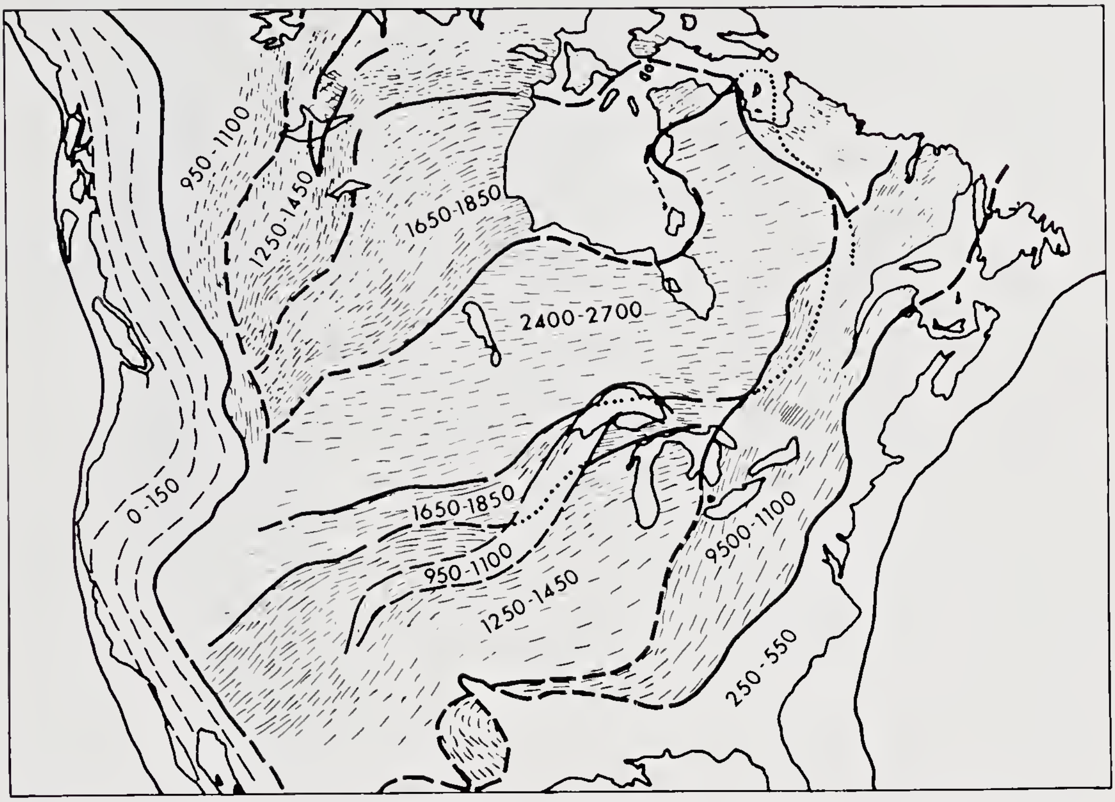


Fig. 135 Truncation of Precambrian grain by Cordilleran geosyncline, which was born less than 800 million years ago (after Gastil, 1960).

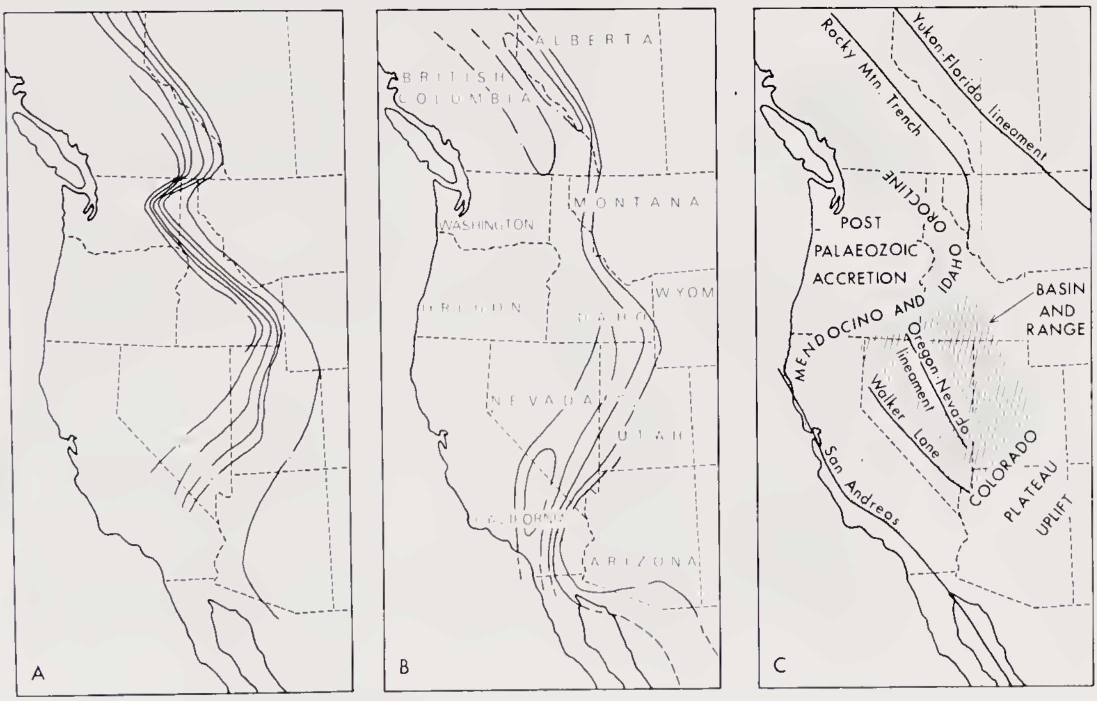


Fig. 136 Pacific margin of North America (after Stewart, 1972).
A: Isopachs of late Proterozoic and Lower Cambrian. B: Lower Proterozoic isopachs. C: Post-Palaeozoic distortion.

"Upper Precambrian and Lower Cambrian strata in western North America are exposed in a narrow slightly sinuous belt extending from Alaska and northern Canada to northern Mexico, a distance of 2,500 mi. Within this belt, the strata thicken from 0 ft on the east to 15,000 to 25,000 ft in areas 100 to 300 mi to the west. The basal unit of this sequence is a diamictite (conglomeratic mudstone) which is widely distributed but discontinuous and is generally considered to be of glacial origin. Overlying sedimentary rocks consist predominantly of siltstone, shale, argillite, quartzite, and conglomerate. Tholeiitic basalt forms thick and widespread units near the base of the sequence but is sparse higher in the section.
The distribution pattern and lithologic characteristics of the upper Precambrian and Lower Cambrian sequence fit the recently developed concept that thick sedimentary sequences accumulate along stable continental margins subsequent to a time of continental separation. The depositional pattern of the sequence is unlike that of underlying rocks, a relation consistent with the idea of a continental separation cutting across the grain of previous structures. The pattern is, on the other hand, similar to that of overlying lower and middle Paleozoic rocks, suggesting that the diamictite and post-diamictite rocks were the initial deposits in the Cordilleran geosyncline. Thick units of volcanic rock near the bottom of the sedimentary sequence indicate volcanic activity related to the thinning and rifting of the crust during the continental separation."

The facies and thickness sequence across this newly-formed continental margin are shown in figure 137. The Rocky Mountain trench marks the transition from continental to oceanic facies. The trench is a fundamental fracture along the continental border against the Pacific. Hughes (1975) has argued the case for the initial rifting of North America from eastern Asia at the dawn of the Phanerozoic, and for the gross separation of western North America from eastern Asia since the Palaeozoic through the rapid widening of the North Pacific. Hughes' arguments on these two matters are independent of his dilemma that in order to maintain these two relationships on an earth of constant size, he is obliged to shuttle the Cordilleran slice between Asia and America with an ocean east of it in the Palaeozoic and west of it in the Mesozoic and Tertiary. Johnson and Potter (1975) have shown that tensional grabens were still developing along this continental margin during the Silurian.

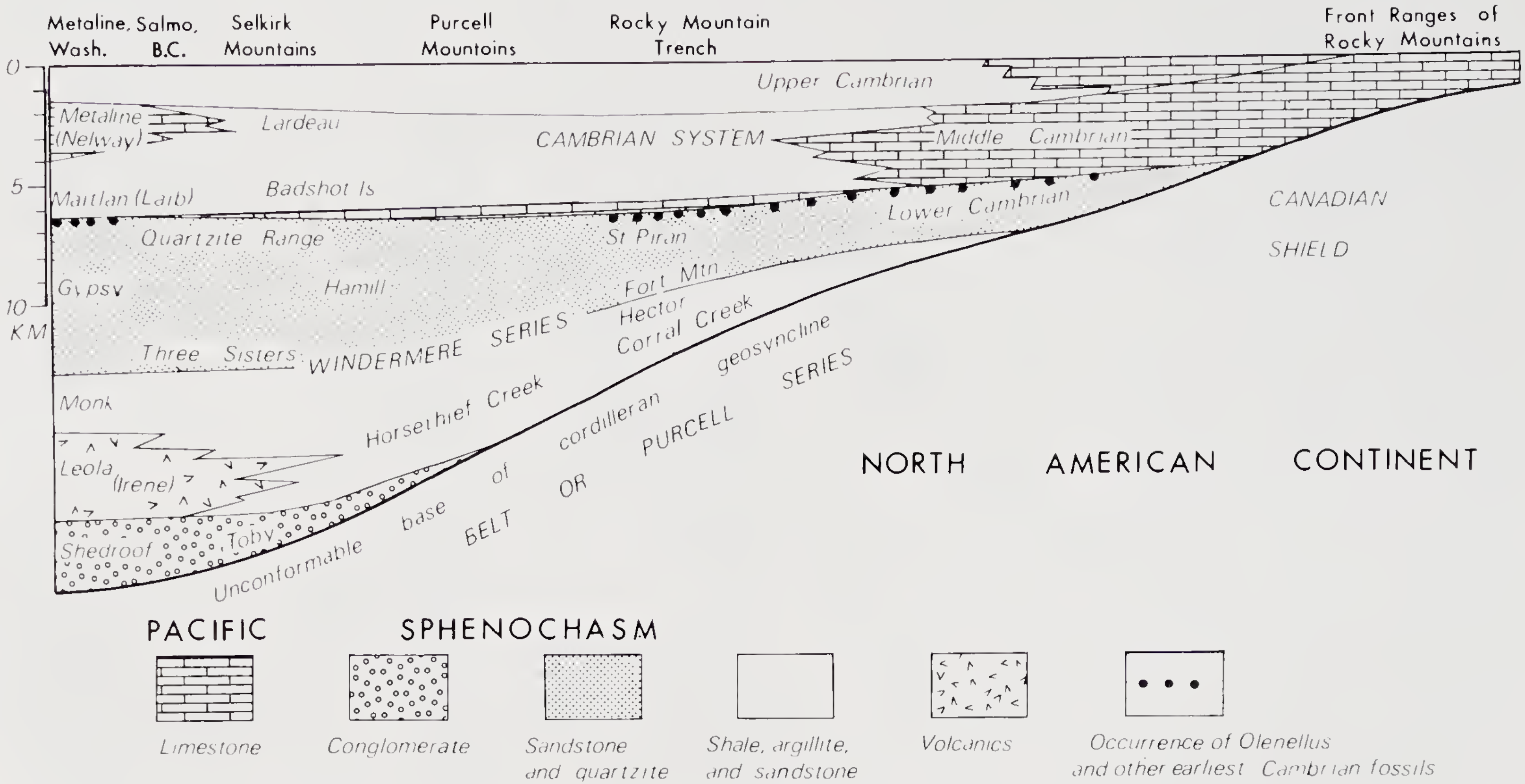


Fig. 137 Facies and thickness of the initial sedimentary cycle of the cordilleran geosyncline (after King, 1959).

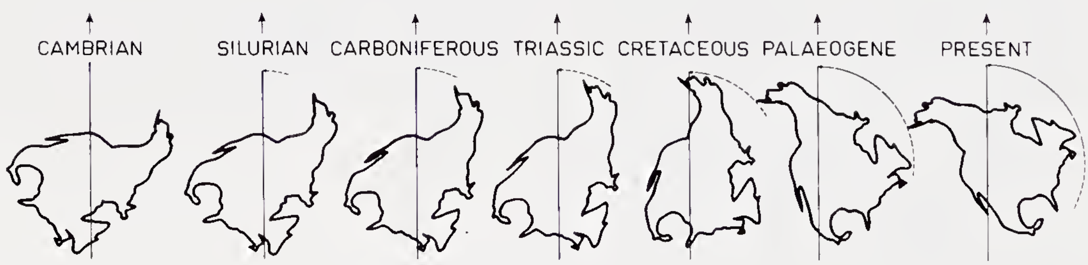
Octantal Rotations

The direction of the palaeomagnetic meridian as recorded by North American rocks shows a progressive change through 120° from south of west in the Cambrian to the present meridian (Fig. 138). This is due to the progressive counter-clockwise rotation of North America with respect to the pole (Fig. 139). Figure 138 also shows the rotation at successive intervals of 100 million years. The rate of the rotation has progressively increased.

The secular anti-clockwise rotation of North America is revealed quite independently from the rotation of the magnetic azimuths. When the Cordil-

Fig. 138 Migration of the north pole as seen from North America. Increasing spacing of 100 m.y. markers indicate secular acceleration of the movement.

Fig. 139 (below)
Rotation of North America as seen from the north pole.



leran rupture between North America and east Asia first occurred in the Upper Proterozoic, this rupture was near-equatorial. By the Ordovician the Appalachian-Caledonian belt was near-equatorial, implying an anti-clockwise rotation of some 50°. By the Permian, a line from Texas to Maine was near equatorial, bringing the total rotation to about 90°. The Tethyan torsion zone follows the Mesozoic equator, and the present equator is about 120° from the Upper Proterozoic direction which is about the same rotation as that indicated by the palaeomagnetic azimuths. Here again there is an acceleration of the rate of rotation with time.

All tectonic processes independently show this exponential increase in rate, from old to young. The North Pacific crust also shows a counter-clockwise rotation with respect to North America and with respect to east Asia. Likewise the Siberian block shows a counter-clockwise rotation against the North Pacific and against Europe. Such octantal rotations are a direct consequence of earth expansion (Fig. 103). In a strong rigid spheroid such forces would produce static stresses within the body. In a fluid spheroid, circulation cells would develop like the circulation of the oceans. In an earth such as the real earth, which is a tectonic fluid (Fig. 121) with a thin brittle crust, sub-crustal circulation would rupture and rotate crustal segments. The result is that the western rim of North America, especially in the mid-latitudes, suffers a strong dextral couple (Fig. 140). This has been present ever since the late Proterozoic birth of the Pacific, and has intensified progressively since then (Fig. 138) and is currently the strongest it has ever been. This dextral couple has dominated the tectonic evolution of the cordilleran region.

The sequence of emergence of this concept was for me: first, that the local and regional structures of North America implied a general dextral couple; second, that this was supported by the palaeomagnetic azimuths; third, that such gross octantal rotations were to be expected on an expand-

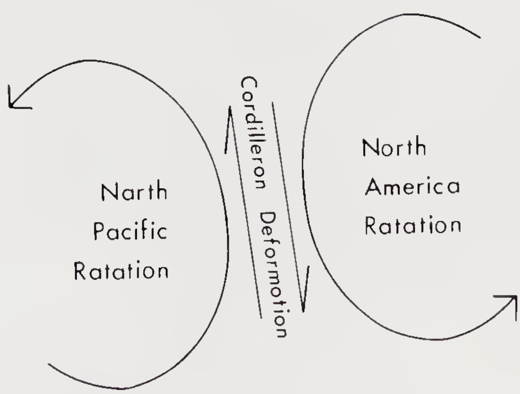


Fig. 140

Differential rotation of North America and north Pacific.

ing earth; and fourth, that some others had already published similar conclusions.

Sakuhei Fujiwhara concluded in 1926 that "it seems as if the bed of North Pacific Ocean is making a counter-clock-wise rotation. The angular velocity may not be the same as its various parts, so that some cracks, for example maritime Fuji Volcanic Zone, or vortices, for examples as in Hokkaido, Honsyn, Kusyu, Taiwan, and elsewhere, must necessarily be the result. The seismic generating force in Japan and in California seems also due to this rotation to some extent. For the present I reserve any theory for the origin of such a rotation. It is however, not a matter of improbability that everything belonging to the rotating earth is more or less pertaining also to some local rotation. There are also abundant echelons and earth-vortices on the other parts of our globe. By the investigation of them some more discoveries and consequently the advancement of our science will be made." (Fujiwhara, 1927, p.14).

Hans Cloos (1928), in his *Bau und Bewegung der Gebirge in Nordamerika, Skandinavien und Mitteleuropa*, suggested such a dextral couple dominating the Rocky Mountain region, which led Becker (1934) to write [SWC trans.]: "Through the northward-directed drag of the Pacific block the Great Basin suffers a rotational stress which causes the north-south-trending tensional faulting. These are the dislocations which cut up the horsts and tilted blocks of the Basin Ranges" (Fig. 141). Later, Locke Billingsley and Mayo (1940) reached the same dynamic picture by a different route (the pattern of granites and "lanes"): "If, in examining the tectonic picture, the observer allows the curves and overthrusts to fade into the background, he will see a remaining framework into which all can be properly fitted. Local

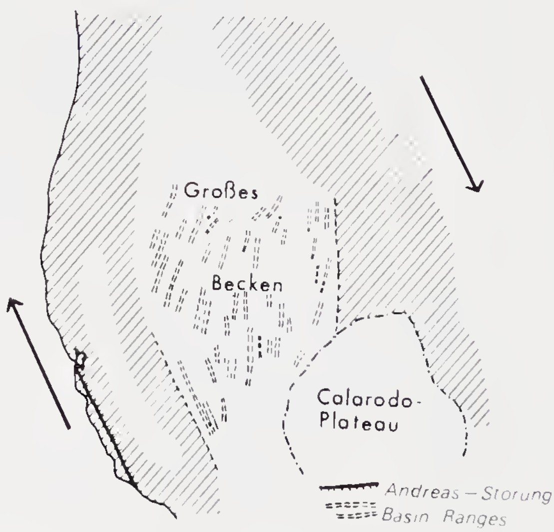


Fig. 141

Becker's explanation of the development of the Basin Ranges through the relative northward movement of the Pacific block (1934).

convolutions are merely drags in a current. The picture now becomes a glimpse of a great rock stream, a regional shear zone, making adjustment between the northwest-drifting Pacific terrane and the south-east-drifting terrane which borders this on the east." These authors also recognized another of the three kinematic patterns referred to above - the concomitant upward diapiric motion, at least of the plutons, which they saw as indigenous funnel-shaped bodies, flaring upwards, and resulting from upward and outward expansion into levels of less pressure.

St. Amand (1957), Benioff (1958), and Tanner (1964) suggested that the Pacific basin showed an overall dextral rotation, but Biq Chingchang (1958) thought that in east Asia the movements were sinistral. He based this conclusion on the offsets of the Izu (1930), Mino-Owari (1891), Tango (1927), and Taiwan (1951) earthquakes, and on the gross tectonic pattern in plan which he interpreted as sinistral simple shear. However the overall pattern is one of large areal extension which removes the pattern from the simple shear category. The gross offset of magnetic palaeolatitudes leaves no doubt that the marginal zone of the western Pacific has been displaced

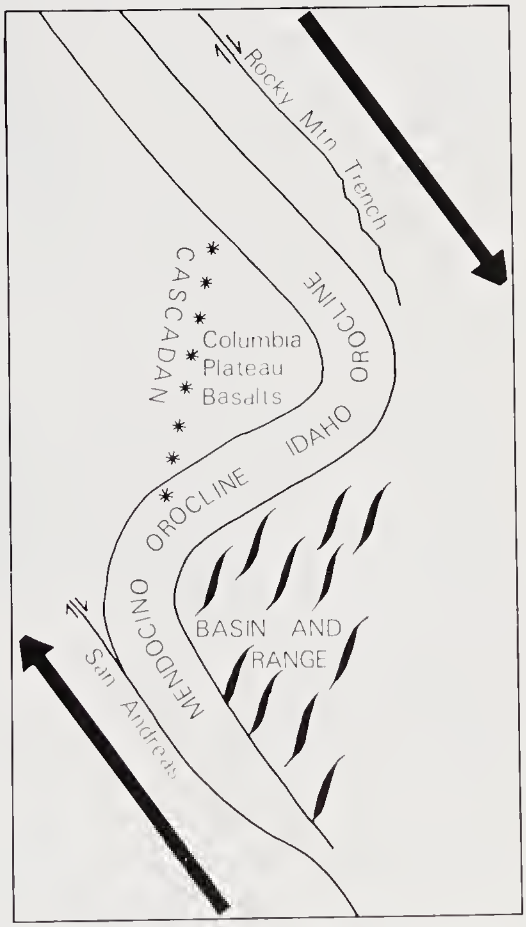


Fig. 142

Primary elements of cordilleran tectonics induced by the Pacific dextral drag.

southwards with respect to the Asian mainland, i.e. dextrally (see Table XI).

The tectonic pattern of the Rocky Mountains and Cordillera were summarized by me in the 1956 symposium on continental drift (Carey 1958, pp. 336-338), and in more detail in 1960 lectures at Yale and Princeton, and in an invitation address to the June 1960 meeting of the Rocky Mountain Section of the Geological Society of America in Banff. The San Andreas shear system, the Rocky Mountain Trench, the Mendocino and Idaho oroclines, the Basin-and-Range province, the Cascadan zone, and the Columbia Plateau basalts are a cognate group, whose relations are shown in simplified form in figure 142. The transitional mobile orogenic zone is dragged between the differential rotation of the North American and the Pacific blocks. The San Andreas dextral system and the Rocky Mountain Trench shear system end at the oroclinal bends. The Basin-and-range province bridges the concave sector of the Mendocino orocline just there, and nowhere else; the Cascadan zone with its associated Columbia plateau basalts similarly bridges the concave sector of the Idaho Orocline, just there, and nowhere else. The Cascadan is the oceanic counterpart of the continental Basin-and-range.

How can oroclinal bends occur *within* a continent without major radial folds within concave angles of the bends? This question arises only in the minds of those obsessed with notions of compressional orogenesis. The mechanics of such oroclinal drags is shown in figure 143. In this diagram A shows the prevailing shear system. B shows a simple offset by this shear. Because of internal friction, actual shear failure does not occur parallel to the direction of maximum shear stress, but on shears in echelon making an angle to the maximum shear stress of half the angle of friction, as in C. The whole of the displacement of shear I is transferred to shear II by accommodation of the tension gap shown in black. Neither shear I nor shear II exists beyond this tension gap. A less brittle belt (such as an active orogenic zone with elevated mantle and crustal isotherms associated with lavas and plutons) will tend to bend and stretch rather than fracture, producing a double orocline with flanking tension gaps in the concavity as in D, but again the adjustment for the orocline is confined to the tensional areas. There is no implied external deformation of either block. The tensional adjustment may occur as a single hole as in D or as a distributed tension as in E, where a series of grabens and horsts (basins and ranges) take up the extension. The direction of tension horsts and grabens in relation to the shear stress is shown in F. Warp or fault adjustments occur between the grabens. These structures are worked out in detail below.

Alternatively, plastic oroclinal bends can be taken up entirely by transcurrent shears in the brittle zones, as in G. In this case the shears persist far beyond the oroclinal zone. In western North America both patterns co-exist; dragged and flexed zones accommodate partly by tension grabens of the Basin-and-range type as in E, and partly by dextral shears of the San Andreas system; the dextral shears are replaced in part by disjunctive grabens (as in Fig. 146).

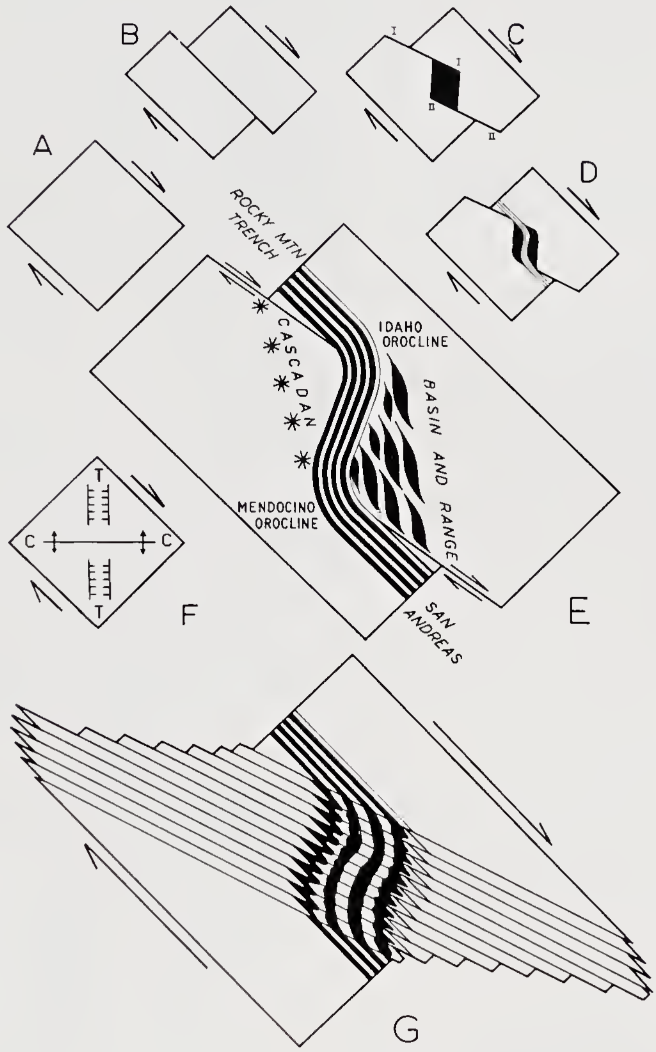
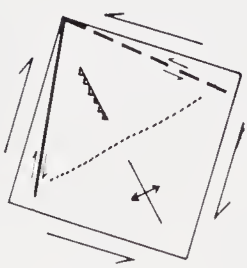


Fig. 143 Mechanics of Mendocino and Idaho oroclines. (See explanation in text).

Fig. 144

Right:
The San Andreas dextral fault family and sinistral conjugates.

Below:
The stress system (arrows external to the square) and consequent failure system (structures inside the square). In both map and diagram continuous fault lines indicate dextral shears, dashed lines indicate sinistral shears, and dotted lines indicate tensional faults. The sinistral system parallels the Tethyan torsion which here runs 30° anticlockwise to the latitude parallels.



THE SAN ANDREAS SYSTEM

Dextral displacement along the San Andreas fault was demonstrated impulsively by the 1906 earthquake by offsets of fences, roads, plantations, and structures. The creeping secular distortion is perennially confirmed by curbs and pavements, bending of pipes, and by repeat triangulation. Hill and Diblee (1953) demonstrated continuity for nearly a thousand km, and identified dextral displacements with varying degrees of certainty: 9 m in recorded earthquakes; 1-3 km in recent drainage; 11 km in terrace deposits; 18 km in Pleistocene gravels; 120 km in Upper Miocene facies; 320 km in Lower Miocene facies; 415 km since Late Eocene; 590 km since Cretaceous; and 650 km since Jurassic. Subsequently Smith (1959) estimated 25 km since the Pleistocene, and Hall (1960) something between 90 and 250 km since the Upper Miocene. Most authors agree that 300 km of dextral shift occurred on the San Andreas since the Palaeogene, but the amount of pre-Miocene shift is still a matter of controversy (see Crowell, 1973).

The San Andreas fault *(sensu stricto)* is an impressive fault zone, but it is a member of a family of faults (the Huasne, Suey, Little Pine, Palos Verdes, Sur-Nacimiento, Pine Mountain, Newport-Inglewood, San Gabriel, Elsinore, San Jacinto, Rose Canyon, Healdsburg, Jackson, Hayward, Calavaras, Riliz, Espinosa, San Antonio, Marcos and Rinconada faults), which Howell

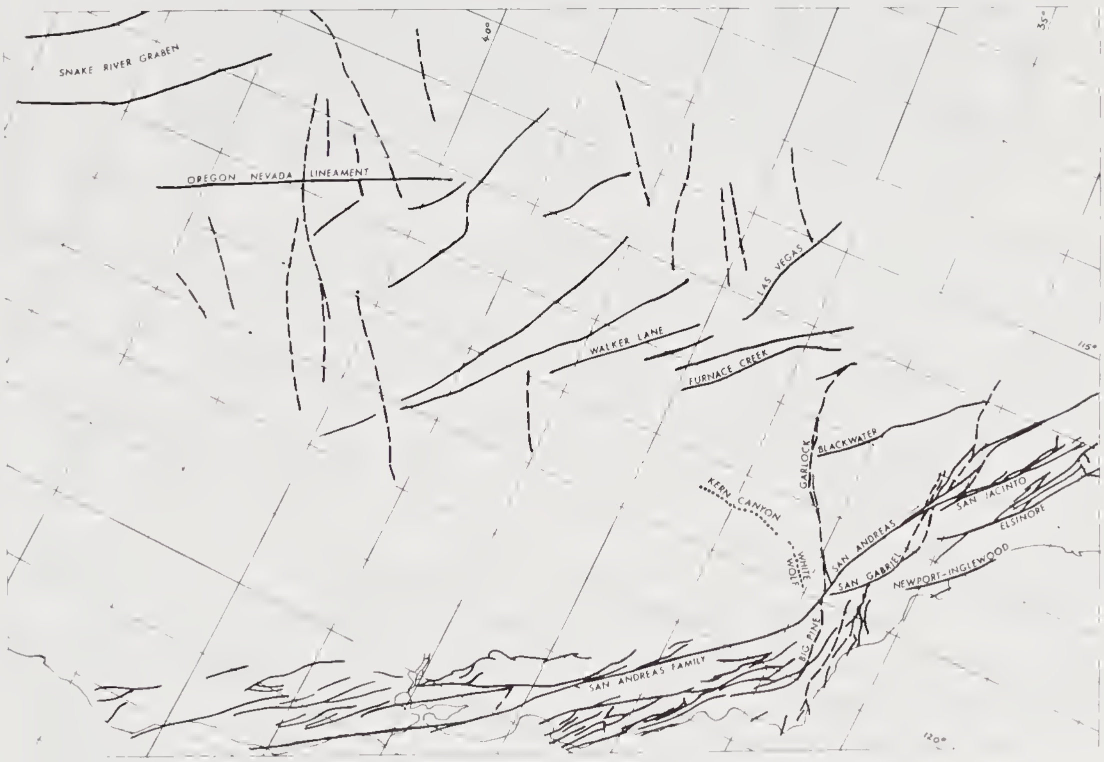


Fig. 145 The dextral group of fault families.

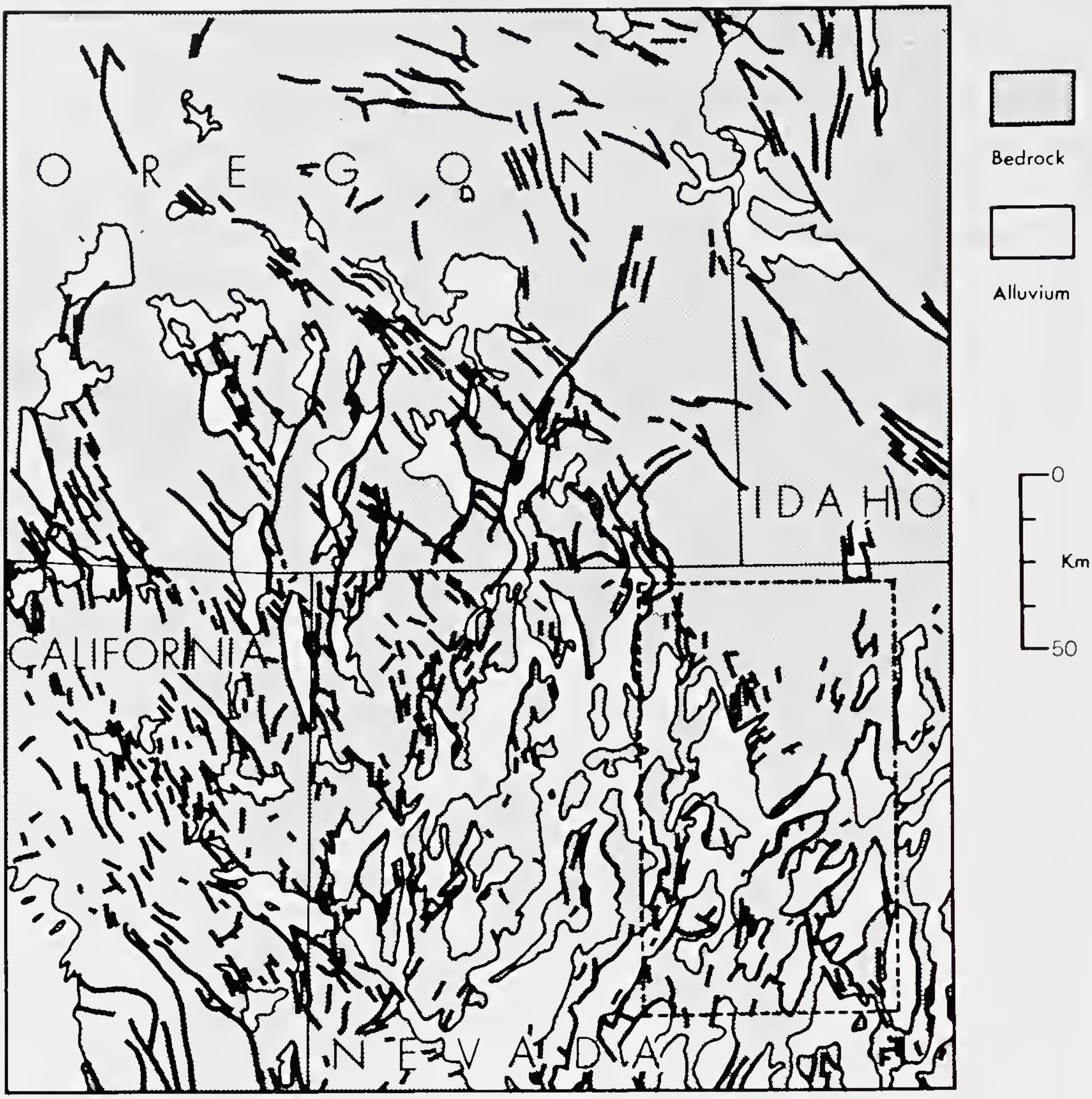
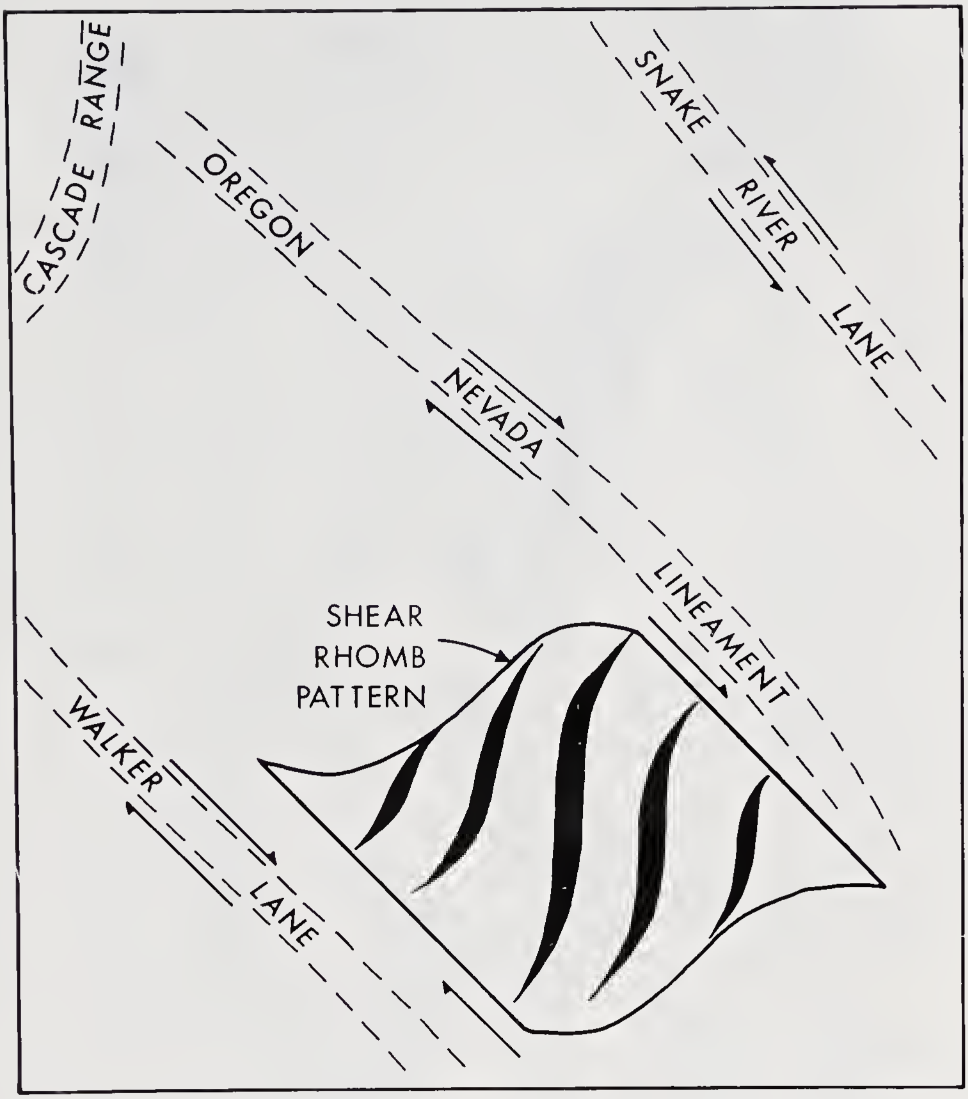


Fig. 146A (above)
Fractures associated with the Oregon-Nevada lineament, Walker lane, and Snake River lane. (After Stewart, Walker and Kleinhampl, 1975).
Fig. 146B (opposite)
Structural interpretation of figure 146, on the same scale.
Fig. 147 (next page)
Enlargement of the rectangle marked by broken lines in bottom left of figure 146A, to show the concentration of Neogene volcanic centres along the Oregon-Nevada lineament.

(1975) suggests contribute to a gross dextral displacement of 700 km since the late Cretaceous (Fig. 144). The movements interchange mutually and with conjugate sinistral faults, and with cognate tensional faults, and compressional thrusts.

This San Andreas family is part of a still larger system of dextral



families which include the Agua Blanca and San Clemente, the Furnace Creek fault zone, the Walker Lane, the Oregon-Nevada lineament, and probably others to be identified (Figs. 144 to 147). Stewart (1967) estimated about 90 km of dextral displacement across the Furnace Creek zone and about 60 km across the Walker Lane (Fig. 149), both of which are further increased by oroclinal bending.

Walker lane

The Walker Lane was first recognised by Spurr (1901) as the division between north-trending (Great Basin) and northwest-trending (Sierra Nevada) regions. In 1935 Billingsley found a through-going shear near Goldfield, and named it the Walker Lane after the explorer who had followed a route

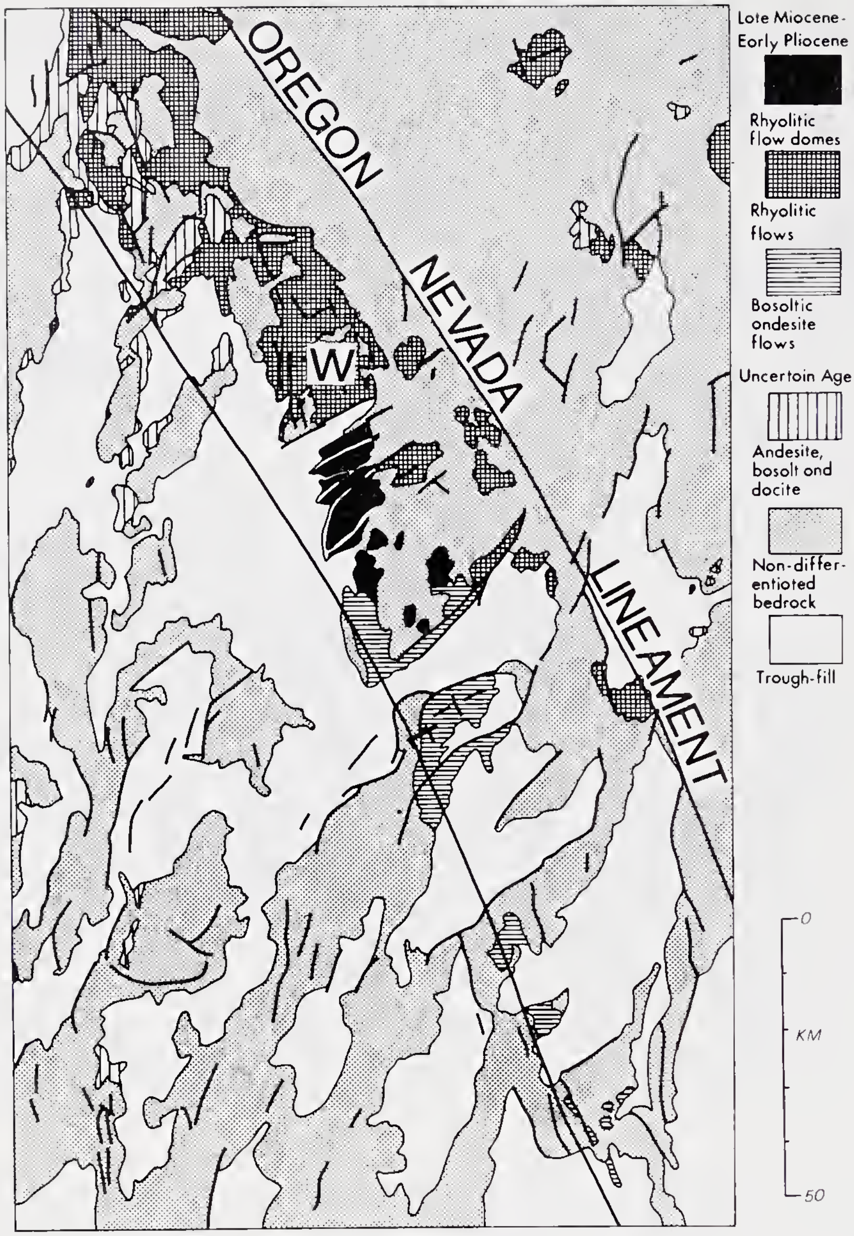


Fig. 147 See caption figure 146A.

determined by this structure. Billingsley considered it would prove no less significant than the San Andreas system, the two being complementary on either side of the Sierra Nevada block. Locke, Billingsley, and Mayo (1940) used the term "lane" for the northwest-trending valley lineaments which followed such dextral zones. Ferguson and Muller (1949), and Shawe (1965), confirmed the large dextral offset on the Walker Lane, and Shawe particularly emphasised the genetic relation between the dextral shear zones (San Andreas, Walker Lane), the conjugate sinistral shears (Garlock, Carson), and the Basin-and-range grabens. The Walker lineament is the axial trace of the

Mendocino orocline - hence the change in trends across this zone. The axial zone is sheared as in figure 143G. The amount of dextral offset on the Walker Lane is not clear; Ferguson and Muller (1949) did not find significant offset in the Hawthorne quadrangle of Nevada.

Oregon-Nevada lineament

"The Oregon-Nevada lineament is a 750-km-long northwest to north-northwest-trending belt of closely spaced, partly en-echelon faults extending from central Oregon to central Nevada. The lineament is also marked by centers of volcanic activity that yielded voluminous late Miocene lava flows in Nevada, and Miocene and younger volcanic rocks in Oregon. A conspicuous aeromagnetic anomaly is coextensive with the lineament in Nevada. The lineament is considered to be the surface expression of a deep-seated fracture zone that may have had a complex history of strike-slip and tensional movement" (Stewart, Walker and Kleinhampl, 1975). Figure 146A shows the regional pattern of fractures and intermontane flats associated with the Walker Lane, the Oregon-Nevada lineament, and the Snake River lane along the northwest-trending part of the Snake River valley. Figure 146B is a diagram of the same area as figure 146A to show the mechanical interpretation of this region. Figure 147 is an enlargement of the southeastern part of the Oregon-Nevada lineament, indicated by a rectangle in broken lines in the lower right of figure 146A. This shows the distribution of volcanic centres and of tensional fractures along the Oregon-Nevada lineament.

Sinistral conjugates

The best known sinistral conjugates are the Garlock-Big Pine Santa Ynez, and Malibu Coast faults, which collectively deflect the San Andreas family, and generate the transverse ranges (Fig. 144). However, sinistral conjugates are widely distributed through California and Nevada (Fig. 145), although topographically they are overshadowed by the dominant Basin-and-range tensional faults. Nevertheless they serve an important function in transferring overall movement between the tension faults en echelon. Smith (1960) found that distinctive dyke swarms were offset nearly 70 km by the Garlock fault, and Hamilton (1961) pointed to a similar offset of the east margin of the Sierra Nevada batholith.

A transform function?

Large dextral shifts on the San Andreas system of fault families have long been established. However, plate tectonicists, following Wilson (1965), reinterpreted the San Andreas fault as a transform, linking the northern fork of the East Pacific rise and the Gorda ridge. They also reduced its movement to the Neogene (Atwater, 1970), and reduced the total movement, disregarding the evidence cited for continuing movements since the Upper Cretaceous, if not the Jurassic, and for the total shift of the fault. Hill (1974) has objected to this reinterpretation on several grounds, which in turn were disputed by plate tectonicists. However, whatever genesis is assigned to the San Andreas family must also be assigned to the whole system extending across California and Nevada and beyond. The movement pattern is wholly consistent with the Tethyan torsion and with the sinistral rotation of both North America and the North Pacific (Fig. 144). Nevertheless this does not preclude the San Andreas also functioning as a transform fault during part of its life in the progressive expansion of the Pacific, although the model is complicated by the fact that it is here that the controlling parameters were such that the spreading ridge developed unilaterally at least for most of the time.

MENDOCINO AND IDAHO OROCLINES

The Z-shaped deflection in the trends from California to Canada is obvious on any structural map (e.g. King 1959, Fig. 89). The coastal plutons form a belt trending northwest to the Klamath Mountains, then they step 900 km eastnortheast to the Idaho batholith, whence they resume their northwest trend (Fig. 148). Moore's "diorite line" (1959) which divides the more potassic from the calcic plutons, and "is probably parallel to, and not far distant from the edge of the granitic (sialic) layer in the continental crust", confirms this offset. The belts of ultramafic rocks, in a west-progressing age sequence from Lower Palaeozoic to late Jurassic, all follow this same Z offset pattern (Maxwell, 1974, Fig. 1). So do the metallogenetic belt and trace-element abundances as reported by Burnham (1959) and Wise (1963). The grain of the pre-Cretaceous rocks runs northwest through California to Cape Mendocino, then wheels through 80° in the Klamath Mountains, and is heading towards the Idaho batholith before passing below the young volcanic cover. Nevertheless inliers in the Blue Mountains

Fig. 148
Mendocino
and Idaho
oroclines.

confirm that the easterly trend persists under the volcanics into Idaho, where the northwest trend is resumed. Yates (1968) reports that the eastern limit of Mesozoic alpine ultramafic rocks (which he interprets as the continent-ocean crust boundary) repeats this pattern, trending north-northwesterly through California to the Klamath Mountains, where it wheels northeast to the Idaho batholith, where it is offset northwest again to northern Washington.

Intermittently down the axis of the Mendocino orocline from the far northwest corner of Nevada to its extreme southeast corner, structural facies and isopach trends mirror these inflections, but with diminishing amplitude as the Basin-and-range grabens reduce the total inflection (Wise 1963, Burchfiel 1965, Albers 1967, Stewart 1967). The Z-shaped double wheel in trends is an objective fact. The only question is whether this shape is an original feature or impressed bends. For me this question was answered initially by the dynamic integration of these oroclines with the palaeomagnetic rotation of North America, the Rocky Mountain Trench and San Andreas shear system, and the Basin-and-range and Cascadan provinces. However this induction is amply confirmed by analytical data derived from palaeomagnetic rotations, petrofabric rotations, and rotation of sedimentary structures.

Petrofabric rotations

Greenwood and Reid (1969) have summarized their findings as follows: "Structural analysis of two areas in central Idaho indicates the presence of large, steep-plunging, sigmoidal folds. These structures bend schistosity and early fold axes, and one of the structures is cut by Cretaceous quartz-monzonite of the Idaho batholith. A large, synkinematic orthogneiss body which intrudes the core of one of these folds has a weak schistosity parallel to the axial surface of the fold. The sigmoidal folds may be from latest Precambrian to early Cretaceous in age. These large sigmoidal folds are similar in style, attitude, and direction of tectonic transport to the much larger Cordilleran Orocline proposed by Carey (1958). The presence of the folds in central Idaho supports Carey's concept of translational rotation of the northwestern United States in response to deep crustal movements."

Rotation of sedimentary structures

In Nevada, Stewart (1967) recorded the dip of cross-bedding at 20

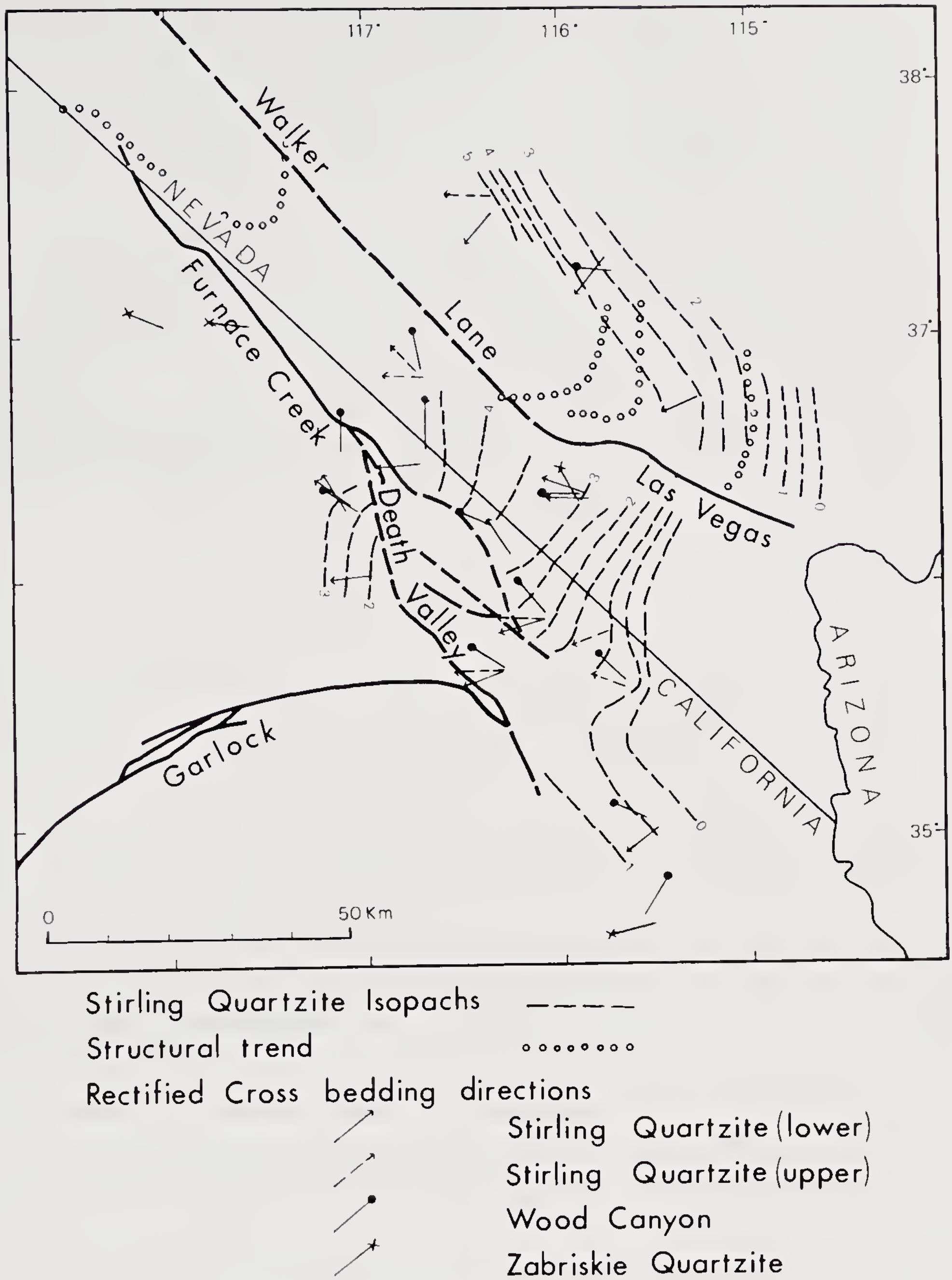


Fig. 149 Inflection of isopachs and rotation of cross-bedding current directions by the Mendocino and Idaho oroclines. (After Stewart, 1967)

localities (about 600 readings) in the Stirling Quartzite, 14 localities (about 700 readings) in the Wood Canyon Formation, and 4 localities (about 60 readings) in the Zabriskie Quartzite. After rectification of the structural dip, the means for each locality disclose that the cross-bedding directions have been rotated in those areas where tne oroclinal interpretation requires them to be rotated, and by the appropriate large angles;

further, that the isopachs for this formation have also been rotated; the cross-bedding remains normal to the isopachs both in the rotated and non-rotated sectors (Fig. 149). Sedimentary facies boundaries (volcanic to non-volcanic, carbonate to non-carbonate, shorelines, and neritic limits), all wheel in sympathy with the oroclines.

Professor B.C. Burchfiel has informed me that southeasterly trends continue in the area southwest of the junction of Death Valley and the Garlock fault of figure 149. Indeed, this should have been expected. Grabens of the pattern of 143E progressively cancel the oroclinal bending, whereas shear zones of the pattern of 143G do not. Hence a south-westerly trend should recur until the last of the San Andreas family has been crossed, with oroclinal drags and inflections corresponding to each family of shears.

Magnetic rotations

Idaho Batholith

Beck, Ellis, and Beske (1972) investigated the remanent magnetization of the Idaho batholith because, in view of its crucial position in respect to competing tectonic hypotheses, they reasoned that directions of remanent magnetization within it could be used to reconstruct its tectonic history, if the batholith had reacted to tectonic stresses as a coherent mass. They drilled 110 samples from 13 sites in road cuts or very fresh glaciated exposures (Fig. 150). They concluded: "the Idaho batholith has not reacted to tectonic stresses as a coherent rigid block, contrary to our initial assumption. Only sites 17, E2, E3 and E5 yield palaeomagnetic poles that fall close to the polar wandering curve for North America. Pre-Tertiary site I1 is displaced some 30° from the mean Cretaceous pole for North America, probably too far to attribute to normal dispersion. The remaining poles are widely scattered. These include the three sites with large uncertainties in their mean directions, but the divergence of these poles from any 'expected' position are much greater than their error limits. Mechanisms that might cause apparently stable site-mean directions within the batholith to diverge from one another and from any known North American post-Palaeozoic polar wandering path include partial remagnetization, as by high-temperature chemical remanent magnetization or partial thermoremanent magnetization, and internal deformation, as by block faulting, in response to tectonic stress. Partial remagnetization of older rock by later events probably cannot account for the divergent poles, for the following reasons. First, the reheating (or chemical activity) necessary to partially remagne-

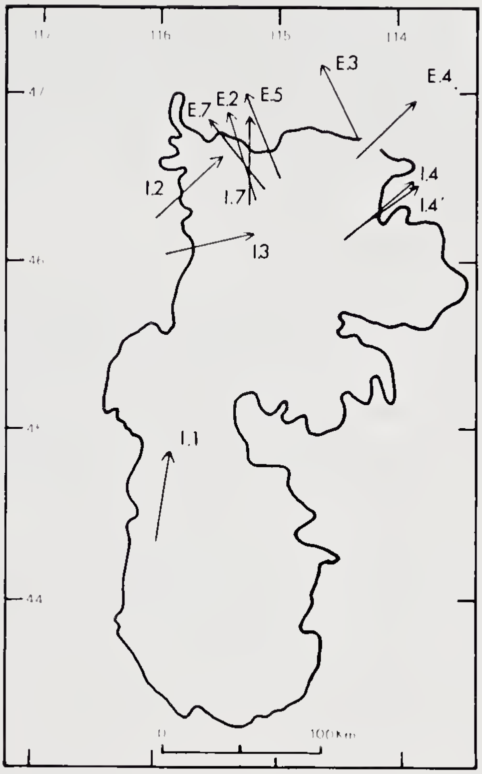


Fig. 150

Palaeomagnetic meridians from the Idaho batholith. (from Beck, Ellis, and Beske, 1972)

tize a site would increase diffusion rates and should result in argon loss, with consequent lowering of potassium-argon age. Divergent, highly stable sites I2 and I4, however, are respectively Early Cretaceous and Late Eocene in potassium-argon age; that is, they represent the extremes of age within the batholith. It seems unlikely that reheating could completely 're-set' the potassium-argon 'clock' at site I4 (and I4´), simultaneously causing a partial remagnetization, while, on the other hand, at site I2 (and I3) partial remagnetization could have occurred without any significant effect on the potassium-argon age. Second, most sites sampled in this study are lithologically and magnetically heterogeneous. Because the amount of remagnetization induced by reheating should be sensitive to the composition and effective grain size of magnetic minerals within the individual specimens, such sites might be expected to show a streaked distribution of within-site directions along a great circle path connecting the old and new directions. But a streaked distribution was encountered in this study only at site I2, where it is probably related to a marked gneissic texture. We conclude that the Idaho batholith has suffered some degree of internal deformation, at least part of which is post-Eocene."

It is clear from these results that possible causes other than differential rotation within the Idaho batholith have been excluded. The rotations are as large as 90° and are all towards or into the trend of the Idaho orocline. From the paradigm of figure 143, it is clear that integral rotation should not necessarily be expected, but rather sharp rotations between disjunctive fault blocks. This is the pattern which has emerged in the Idaho batholith, and in the Tertiary lavas.

NEVADA BATHOLITH

Locke Billingsley and Mayo (1940) found that the gross sigmoidal shape of the Sierra Nevada plutonic complex was repeated in the many cupolas, roof pendants, and so on through an hierarchy of smaller units down to hand specimen size. This confirms that the gross regional deformations are penetrative to units of all magnitude. Salt domes offer a useful analogy. To the petroleum geologist concerned with its surroundings rather than its internal anatomy, a salt dome is a simple integral intrusive stock. But mining within salt domes reveals a bewildering complex of successive injections of salt cupolas through salt cupolas, with tectonic rotations which box the compass, though most rotations have a vertical axis. Kinematics within a batholith are even more complex, because, whereas the salt flows as a plastic solid with not too wide limits of "viscosity", batholith emplacement involves a wide spectrum of intrusive flow modes from "solid" state through various intermediates to wholly liquid, with "viscosities" varying from time to time and from place to place through twenty orders of magnitude. Hence it would be naive to expect the Nevada batholith or the Idaho batholith to have behaved as a single unit in these tectonic rotations. All rotations should, however, be consistent in sense with the prevailing kinematics, though varying greatly in degree. This is precisely what has been found.

The overall sigmoidal shape of the Nevada batholith observed by Locke and his co-authors is identical in form and trend with the sigmoidal tension gashes in figure 146B. This is of course what should have been expected, because all batholiths are diapiric structures and, as Tanner has repeatedly emphasised, diapirism implies a tensional environment. The belt between the Oregon-Nevada lineament and the Walker lineament shows conspicuous brittle fractures forming horsts and grabens, whereas this is not so between the Walker and San Andreas zones. This has been interpreted by many as implying that the Sierra Nevada was a more rigid block between the more active shear zones of San Andreas system, and the more active system east of the Walker Lane. However the reverse is the case. The dextral shear and latitudinal extension was more pronounced in the Sierra Nevada strip, as evidenced by the grossly sigmoidal batholith, and the repetition of this pattern through smaller scales within the batholith, as reported by Locke *et al.*; but in this zone temperatures were higher, the rocks deformed plastically more than brittly, shears were welded as they formed, and grabens were filled by plutons.

Palaeomagnetic rotations in Tertiary lavas

Since palaeomagnetic anomalies began to show up in the Tertiary lavas of Oregon, Washington, and Idaho, several palaeomagneticians (Campbell and Runcorn, 1956; Cox, 1957; Irving, 1959; Watkins, 1963; Watkins, 1964a; 1964b; Heinrichs, 1964; Watkins, 1965; Beck, 1962; Beck and Noson, 1972; Beske, Beck, and Noson, 1972; Watkins and Baksi, 1974) have been attracted to this area. Figure 151 (mainly following Watkins) is a compilation of all these observations in comparison with the Tertiary palaeomagnetic meridian for North America derived from tectonically undisturbed rocks. From this it is apparent (a) that rotations only occur in the areas where they are expected as a result of the oroclinal bending, (b) the rotations are all in the sense required by the oroclines and may be as large as 90°, (c) rotations are not as an integral block but vary greatly from point to

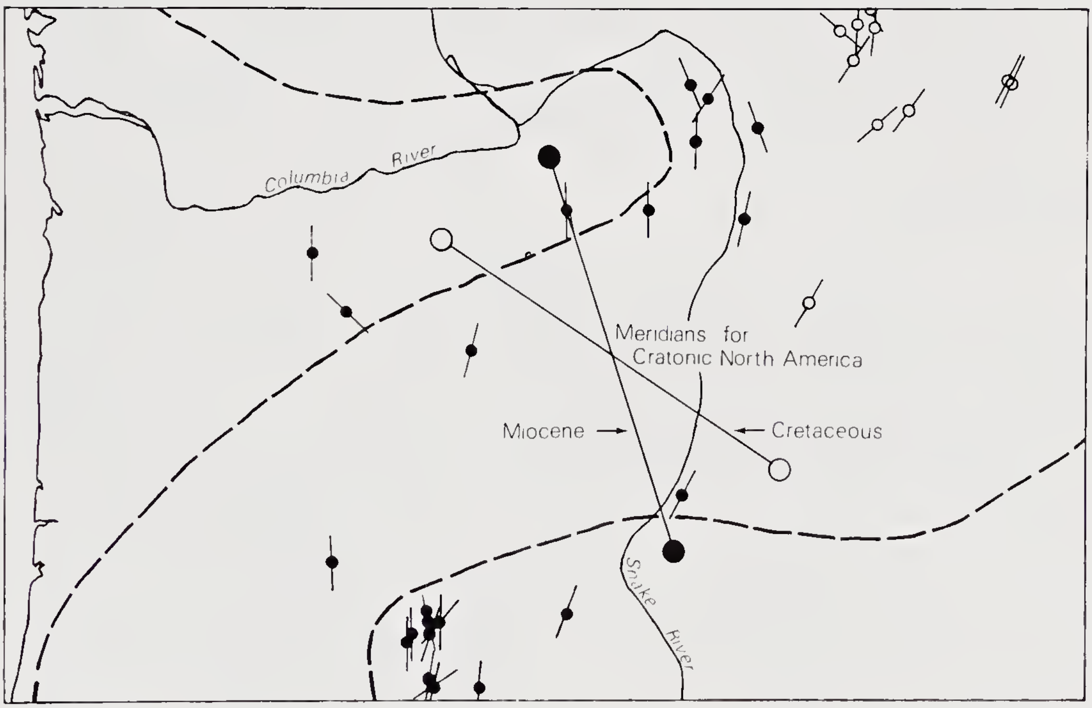


Fig. 151 Rotation of palaeomagnetic meridians by Mendocino and Idaho oroclines (mainly after Watkins).

point as would be expected from the dynamic model of figure 143.

Symons (1973) attributed to tectonic rotation the sinistrally discordant magnetic azimuths he found in Mesozoic plutons from the western cordillera of British Columbia. Taubeneck (1966) has argued that no post-Miocene tectonic rotation has occurred in the Washington-Oregon-Idaho region, primarily because he has found no rotation of the basaltic dykes of a "gigantic swarm" which runs for some 250 km along the Snake River valley from Huntington-Oregon to Almota, Washington, and because the great 600 km north-south lineament of the Cascade volcanoes, which persisted throughout the Tertiary has not been bent or rotated. Clearly Taubeneck has misunderstood the mechanics of the Mendocino-Idaho oroclines. Neither the nearly meridional extensions expressed in the Cascadan province nor the tensional faults of the Basin-and-range type (Fig. 143E), nor the dextral shears of the San Andreas type (Fig. 143G) rotate. They may continue to open, or others may open beside them, but they do not significantly change azimuth. The northwest-trending part of the Snake River Valley from Bruneau, Idaho, to Almota, Washington is not a typical Basin-and-range depression, but belongs rather to the same family as the Gulf of California, Furnace Creek, the Walker Lane and the Oregon-Nevada lineament - primarily dextral translation, but with some transverse extension. Sixty or seventy parallel dykes of the gigantic swarm would imply a kilometre of transverse extension. I have no quarrel with the data presented by Taubeneck, but they are quite compatible with the Mendocino and Idaho oroclines.

Las Vegas inflection

Between the Canadian and Mexican borders the grain of the Cordillera, as expressed by strikes, isopachs, facies and igneous activity, makes two southwesterly offsets from the general northwesterly trend (Figs. 136 and 149), the larger offset being the sweep of the Idaho and Mendocino oroclines. The amplitude of the inflections diminishes progressively southeastwards from the main axial zone of the oroclines, until at Las Vegas the length of the bar of the offset between inflections is only some 85 km (Fig. 152), but there are further inflections of this magnitude along the front. The axial bend of the Mendocino orocline runs close to the California-Nevada border. A series of dextral faults such as the Furnace Creek and Walker Lane follow the axis.

The amplitude of the offset of the Mendocino orocline diminishes with each successive tensional opening of the Basin-and-range system (Fig. 143E) and with each successive dextral shear (Fig. 143G) until the oroclinal offset dies out completely.

Thirty-five kilometres west of Las Vegas, the late Mesozoic Keystone thrust, which transports the full Palaeozoic section over the upturned and overturned Triassic and Jurassic (Aztec Sandstones), wheels 80° from its north-northwest strike to trend northeast towards the Muddy Mountains, where a north-northeast trend continues (Figs. 152 and 153 from Longwell, 1960). I was privileged to be conducted on this area by Longwell in 1959. This wheeling of the Keystone thrust is the axial bend of the Mendocino orocline, which is complicated by a series of dextral tears. However in the axial region, the Keystone thrust repeatedly overrides splays of its own self, each of which throws Palaeozoic carbonates over the Mesozoic sandstones. It is clear that the Keystone thrust has been folded. But is the folding and overriding of its own self part of the orogenic outflow, or is it the overprinting of the relatively rapid gravity nappes by the slower secular dextral strain of the Mendocino orocline as in the landslide-glacier model of figure 38? The facts that this occurs in the axial zone of the Mendocino orocline, and that several of the fault slices are bounded by dextral tears of the regional shear system, suggest the latter view. Longwell himself had reached a kinematic interference interpretation, concluding

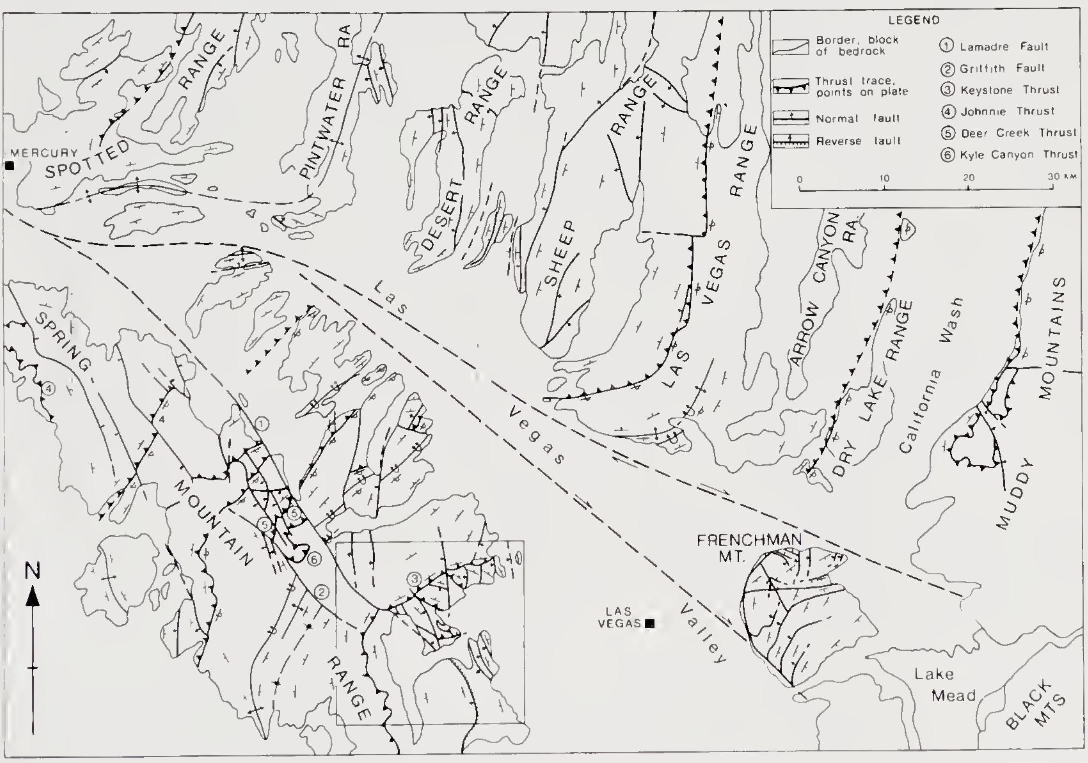


Fig. 152 The Las Vegas inflection. (Data from Longwell, 1960)

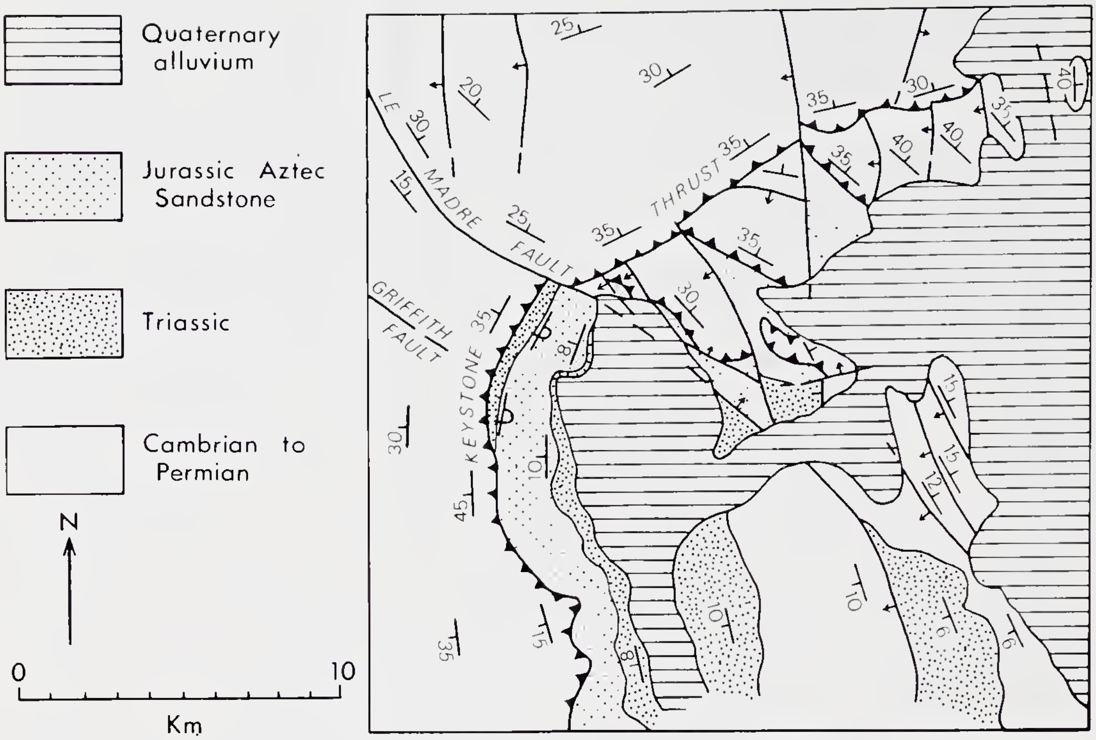


Fig. 153 The Keystone thrust over-riding its own self (from Longwell, 1960).

that the nappe movements and the dextral shearing were active concurrently.

The time inter-relation of the several movements was inevitably complex, and often apparently contradictory. The dextral couple persisted throughout the Phanerozoic (Figs. 138 and 139), but not uniformly. Overall, rates accelerated with time, but pulsed on several scales. Likewise the diapirism of the plutons and the outward spreading of the nappes recurred intermittently since the Palaeozoic, with spatial migration of intensity. Regurgitation pulses produced more rapid translation than did the secular dextral couple; hence the Nevadan and Laramide nappe translations showed no evidence of the concurrent dextral regime. The hare sees the tortoise as stationary, and the landslide ignores the sustained movement of the glacier on to which it spreads. Moreover, a particular structure may be quite restricted in time, and two such structures, both produced by the dextral regime, may respectively precede and succeed a third structure belonging to the nappe-regurgitation regime. I am informed by Professor Burchfiel that evidence is accumulating to restrict most of the movement of the Las Vegas shear to the Late Miocene. This may well be, although the regime which produced it was sustained. A particular crevasse group in a glacier may have developed almost impulsively, even though the relevant glacial flow preceded, induced, and succeeded by long intervals this specific impulsive event.

Further north, Gilluly (1960) has described strong folding of the early Mississipian Roberts thrust in the Shoshone Range of central Nevada, where he has demonstrated a series of splay slices extending into the upper plate, very reminiscent of those described by Longwell. Gilluly like Longwell concluded that the development of the splay slices occurred concurrently with the folding of the thrust, so that as the thrust surface bowed downward below the general plane of the translation, successive new thrust surfaces developed in the upper plate in the general translation plane, which did not vary much. Such an association is certainly to be expected commonly, and may well be correct in this case. On Gilluly's interpretation the folding and splaying of the Roberts thrust all occurred in the same diastrophic episode, namely the Mississippian folding. However, the fault surface on the western rim of the window seems to merge into a meridional Basin-and-range tensional fault, and the splaying of the surface is clearly shown by Gilluly to be related to the sinistral Trout Creek fault, which also appears to be one of the sinistral conjugates of the Basin-and-range - San Andreas system. Also this folding of the thrust occurs in the middle of the zone of the Oregon-Nevada dextral lineament (W on Fig. 147), much as the Las Vegas folding of the Keystone thrust occurs on the Walker Lane - Furnace Creek dextral lineament. Hence, whereas the folding of the thrust and the splaying of the thrust surface would still be cognate and contemporaneous, both of these processes may be later than the gravity outflow of the Roberts thrust, and bear a similar relation to each other as to the slow refolding by the glacier of the initial gravity outflow landslide of figure 38.

' The bulge '

The Pacific mountain belt of North America is less than 700 km wide through British Columbia and Mexico, but is more than twice this width through most of the United States. The explanation of this anomalous "bulge" is immediately obvious from figure 142. The root cause is the dextral drag fold of America's orogenic rind. This is amplified by the accretion of the Cascadan volcanoes and plateau basalts in the northwest, and by the Colorado tumour in the southeast. The Rocky Mountain Front in Alberta is synonymous with the front of the Laramide nappes. In Colorado the Rocky Mountain Front is the front of the Neogene tumour, but the nappe front is a long way further west. The nappe front sweeps back through the northwest corner of Wyoming across Utah to the Las Vegas inflection in southern Nevada. The position of the deformation front depends on the meaning applied to "deformation" and on which period is considered, but in all the belt has a southeast trend in Canada, wheels back southwestwards in the United States and resumes a southeast trend in Mexico. The deformation front is the front of the density tumour in the mantle, irrespective of whether nappes spread out superficially or not.

BASIN-AND-RANGE

First clear recognition of the graben-and-horst structure of the Great Basin region northwest of the Colorado Plateau was by Gilbert (1874), whose concept of Cordilleran folding, thrusting, cratonization, and widespread erosion, followed much later by disjunctive uplift of large blocks separated by gravel-filled troughs, was accepted by Powell (1876), King (1878), Dutton (1880), Russell (1884), and Diller (1886), but contradicted by Spurr (1901) who insisted that the high ranges were fold mountains, and the basins merely erosional valleys, heavily choked with alluvium. This led to a period of controversy over "The Basin-Range problem".

Davis (1925) reaffirmed the validity of Gilbert's interpretation, and with typical Davisian clarity of categorisation, recognised sequentially (1) the initial Palaeozoic and Mesozoic sedimentation, (2) the folding of the Cordilleran orogeny, (3) the Powell peneplanation (Late Cretaceous - early Tertiary), (4) the Louderback lava floods (Oligocene-Miocene) and (5) the Gilbert block faulting (Neogene), (6) regional uplift (Quaternary). This admirable crystallization is as valid now as when Davis stated it, but the rocks did not always know their pigeon-holes as clearly as did Davis. His categories overlap substantially in time, along with migration of regional incidences. Cloos (1928), Becker (1934), Locke Billingsley and Mayo (1940), and King (1959), all adopted the essentials of the Gilbert-Davis model.

Final clinching came from the geophysical work of Thompson (1959 and 1960) whose gravity and seismic surveys established great thicknesses of sediment in Nevada valleys which had sunk deeply while adjacent ranges rose. The displacements "produced not only the visible topographic relief, but also a buried relief of comparable magnitude." Death valley has more than two thousand metres of sediments below its floor, itself nearly a hundred metres below sea level. Thompson confirmed that the Basin-and-range region had suffered strong extension in an east-west direction since the Palaeogene.

King (1959, p.152) stressed the need for precision in nomenclature and to differentiate the different expressions of cognate features:
"(1) The *Great Basin* or region of interior drainage, from which no streams flow to the sea, and which occupies much of Nevada and adjacent parts of Utah, Oregon, and California.
(2) The *Basin and Range province*, which includes not only the Great Basin, but also extensive regions of exterior drainage on the south and southeast in southern Arizona, much of New Mexico, and parts of western Texas.
(3) *Basin and Range topography*, or the characteristic landscape of the Basin and range province - the peculiar, short, sub-parallel ranges and intervening desert basins whose appearance on the map suggested to Major Dutton 'an army of caterpillars crawling northward out of Mexico.'
(4) *Basin and Range structure*, or the later deformations of the crust that influenced or produced the Basin and range topography. To some extent the topography is a product of the erosion of a deformed terrane, but to a greater extent than in most other regions it appears to be a direct product of the deformation itself."

The genesis, and the explanation of all the features of the Basin-and-range province, are fully explained in figure 142 and figures 143 D and E. This graben-and-rift province lies precisely where it is required to exist in relation to the Mendocino and Idaho oroclines. Grossly, the region is near isostatic equilibrium, but the individual continental blocks stand higher than they would in local isostatic equilibrium, and the intervening floors below the sediments in the troughs stand lower than if local compensation were complete. These local departures from isostasy are borne by the strength or viscosity of the rocks. This pattern is similar to that of Ruwenzori and the contiguous Albert rift of central Africa.

Wasatch-Sevier rift belt

Cook (1969) has emphasized the importance of the belt of trenches, en echelon, which separate the Basin-and-range and Colorado plateau provinces (the Wasatch, Sevier, Tuschar and Hurricane faults). This zone is seismicly active, and has gravity anomalies, and high heat flux. Cook described the Wasatch trench as a series of great grabens downfaulted relative to the horst of the Wasatch mountains, extending for some 300 km, with a width of 20 km. This active rift system continues en echelon along the Sevier-Tuschar and Hurricane fault zones and the Cedar valley graben, in which Cook and Hardman (1967) report valley-fill more than a kilometre thick.

Rio Grande graben

The Rio Grande rift valley frames the Colorado plateau on the east, and is cognate with, and complementary to, the Wasatch-Sevier rift zone which frames it on the west. The trench crosses New Mexico from border to border, losing its identity in Mexico in the south and in Colorado in the north. Like the Wasatch-Sevier belt, the Rio Grande graben is characterised by current seismicity, markedly high heat flux (> 2.5 μcal/sq.cm/sec), Bouguer gravity deficiency of some 30 milligals, and more than a kilometre of Cainozoic sediment (Reiter *et. al.*, 1975, Joesting *et. al.*, 1961).

Colorado plateau

The Colorado plateau is an area of half a million sq km, which arched upward nearly two km since the early Miocene after half a billion years of still-stand and intermittent shallow-shelf transgression. Hess (1955) suggested that the uplift was caused by transformation of subjacent peridotite to serpentinite. Gilluly (1963) suggested subcrustal transport under this region of sialic material removed from under the Basin-and-range province. Although it is true that the fragmentation of the Basin-and-range and the uplift of the Colorado plateau seem to have been essentially concurrent (for the most part Middle Miocene), I do not agree with Gilluly that sialic material has been removed from the Basin-and-range region. Here continental crust has been sliced by steep faults and pulled apart concertina-fashion; the trend is towards a close array of continental slivers separated by ocean-type crust. I agree instead with Hess that the cause must be sought in changes within the prism below the plateau, as in the oceanic whaleback rises. Such processes, while due primarily to rising isotherms resulting from outgassing of the deep interior focussed into a tensional region, may involve pressure-temperature paramorphic transformations from temperature rise without change of pressure (e.g. eclogite-gabbro), or the pervasive fluids may have contributed significantly (e.g. peridotite - serpentinite). Both processes could have acted simultaneously at different depths.

The depth of the Moho below the plateau is reported to be some 45 km (Gilluly, 1963) quite significantly greater than "average" continental depth. This is consistent with the paramorphic change from 8 km/sec mantle material to 6.5 km/sec transformed mantle material, so that the Moho progressed downwards through the mantle as the isotherms rose. By contrast, under the adjacent Basin-and-range province the average depth to the Moho is reported to be less than 30 km. However, Cook (1969) has pointed out that anomalously low velocities (7.6-7.7 km/sec) continue below the Basin-and-range to as deep as 70 km, and has referred to this anomalous mantle as "mantle-crust mix". This term may be valid for moderate depths, when seismic refraction arrival times are interpreted on a layered model; a more appropriate model would be a series of continental slices separated on steep boundaries by denser mantle material which would appear on seismic refraction as a zone with intermediate velocity. At greater depths, the lower-than-normal velocities may represent thermally-induced phase changes.

The uplift of the Colorado plateau occurred without significant folding, and although many tension faults occur with the north to northnortheast trend characteristic of the Basin-and-range (e.g. the Grand Wash, Hurricane, Toroweap, Sevier and Paunsagunt faults and the east and west Kaibab monoclines) these faults are marginal to the plateau proper; the Colorado plateau has not suffered the gross extension of its neighbour, which has approached an oceanic condition with an array of large slices of embedded crust separated from each other (Fig. 143). At an early stage, before the intensive horst-and-graben rifting, the Basin-and-range province may have resembled the present Colorado plateau, and may have suffered severe erosion at that time of great elevation.

Basin-and-range *sensu-lato*

The Basin-and-range province *sensu stricto* is a rhomboided area bounded by the common limb of the Mendocino-Idaho orocline, the Walker Lane lineament and the Wasatch-Sevier active rift belt. However, Fenneman and Johnson (1946) define the Basin-and-range province much more widely to extend beyond the Colorado plateau to the Rio Grande graben in New Mexico.

Certainly this wider region shares the pattern of northnortheast tension rifts and northwest dextral tears, but not uniformly so on the scale of the horsts and grabens of the Basin-and-range *(sensu stricto)*. Rather we see here the scale of the second-order polygons of figures 17 to 20 which are determined by the thickness of the lithosphere. Hence the Tulsa-Laredo, Rio Grande, Wasatch-Sevier, and Mendocino-Idaho intervals of from 500 to 900 km are the second-order spacings, whereas the graben-and-horst intervals within these regions are the third and fourth order spacings, analogous to figures 20 and 21.

The second-order intervals continue beyond the Mendocino-Idaho axis as the Cascadan belt, and the active rift zone of the Gorda rise, still with the same northnorthwest trends and the same tectonic pattern.

CASCADAN PROVINCE

As the Coast Range belt of Baja California and California swings back into Idaho, and returns to the Pacific margin near Vancouver, it would follow that much of Oregon and Washington must have been reclaimed from the ocean. King (1959), after pointing out that in more than one hundred thousand square kilometres of Oregon and Washington no rocks older than Tertiary volcanics are exposed, asked: "Could the western edge of the Nevada arc have formed the continental border until late in Mesozoic time, before which its recess was floored by oceanic rather than continental crust? If so, the recess was made continental later by filling with sediments and volcanics, and the coast was straightened by growth of a new set of volcanic structures along the Cascade Range". I had already concluded that this was so at the 1956 Hobart symposium (Carey 1958, p.336-7). Trending nearly meridionally across this re-entrant is the Cascade province, which is the "oceanic" counterpart of the "continental" Basin-and-range province (Figs. 142 and 143G). When stable continental crust is stretched by a system of grabens as in figure 142, the deficiency of mass represented by the rifts is bridged by the intervening blocks, so that, regionally, there is general isostatic equilibrium. Disregarding thermally induced phase changes, if a section of continental crust is stretched, its average thickness is reduced, and hence the average depth of the Moho must be reduced. In the case of a brittle crust this stretching takes the form of horst blocks and grabens, and the average rise in the level of the Moho lifts the horst blocks to greater than normal elevation, while the intervening trenches fill with detritus from these blocks together with volcanic accumulation. Much the same occurs if an area of oceanic crust is stretched. The average depth before stretching is normally something over 4 km. Stretching produces graben trenches deeper than this so that regional isostasy lifts the intervening horst blocks to shallower than 4 km.

The Cascade province which occupies the Mendocino-Idaho-Vancouver re-entrant is the oceanic analogue of the Basin-and-range province. Four north-south-trending belts make up the province, oldest along the coast and youngest inland (Fig. 154): the coast range, andesitic volcanics, young volcanoes of the Cascade range, and the Columbia basalt plateau. Of these

Fig. 154 The Cascadan Province

the Cascade volcanics penetrate deeply into the older oroclinal zones at both northern and southern ends.

The coast ranges diverge from the Nevadan orogen at the Klamath mountains and trend a little east of north to the Olympic mountains, where they merge northwestwards into Vancouver Island to join the orogenic belt. This belt at the end of the Mesozoic was a whaleback oceanic tumour (p.326-9), resembling the modern Eauripik-New Guinea ridge except that it had formed near the coast to take up the local extension caused by the bowing of the Mendocino-Idaho oroclinal couple (Fig. 143E). During the early and middle Tertiary, the associated grabens filled rapidly with volcanic clastics, submarine lavas, and cherts, with turbidity current contributions from the land. There was much movement contemporaneous with sedimentation and repeated injections and extrusions of basic magmas. The isostatic uplift of the blocks mentioned above, together with the thermal whaleback arching and the accumulation on them of volcanic piles, enabled them to contribute to local sedimentation. There is accumulating evidence that the coast ranges, here and right along the North American coast from Baja California to Alaska, consist of slivers of marginal orogenic rock dragged northward in the first order shear between the Pacific and North American blocks which was responsible also for the oroclinal drags and for the birth of these ranges of the Cascadan province.

East of the coast ranges is the sigmoidal trough which extends through Vancouver, Seattle, Portland, Salem and Springfield. The main stem trends a little east of north, but the northern end veers northwest along Georgia strait and the southern end veers southeast along the Williamette valley in Oregon. This trough is genetically related to the Wasatch-Sevier trough and the Rio Grande trough further to the southeast.

The Cascade Range consists mainly of Tertiary andesitic volcanics along the western flanks, and lofty Quaternary volcanoes on the eastern side, which form a sigmoidal line for more than one thousand kilometres, from Lassen Peak in California to Mt. Baker near the Canadian border. From Lassen Peak to Mt. Shasta they trend northnorthwest, but wheel to trend a little east of north through Mt. McLoughlin, Devils Peak, Crater Lake (Mt. Mazama), Thielsea, Three Sisters, Washington, Jefferson, Hood, Adams, Rainier to Glacier Peak where the trend wheels northwesterly to Mt. Baker to complete the sigmoid. The pyroxene-andesite lavas, tuffs, and breccias of the older Cascades have been faulted, tilted, and deeply dissected to expose plutons of granodiorite and quartz-diorite, of late Palaeogene age.

Filling the re-entrant between the Cascade Range and the Idaho oroclinal bow is the Columbia basalt plateau of the Lower Snake River and Columbia rivers. These basalts have reclaimed this sequested segment of the Pacific Ocean. The basalts have also flooded over much of the orogenic belt of the southwestern half of Oregon to form the Malheur plateau, but inliers of basement rocks show through in structural continuity with the Klamath and Idaho mountains, and proclaim the difference between the Malheur and Columbia plateaus.

Tensional elongation, trending a little east of north, and dragging sigmoidally at the ends, dominates the tectonics of the Cascadan province. Vulcanism usually implies crustal extension (Brouwer, 1962; Scheinmann, 1962). Diapirism implies extension (Tanner and Williams, 1968) and granitic diapirs also imply crustal extension. The shared dextral sigmoid patterns of the several units of the Cascadan province, of the Sierra Nevada batholith, the Wasatch-Sevier belt, and the Rio Grande trough all originate from the same tectonic constraint, the tensional component of the prevailing dextral shear. The basalts of the Columbia plateau show some folds which trend from a little north of west, which is the corresponding compressional direction of the general dextral shear, to as much south of west. However, they tend to radiate, as though somewhat pinched in a sphenopiezm.

Finally, I can only agree with King (1959, p.163) that the "Cascadan Revolution" alleged to separate the Tertiary and Quaternary eras, for which this region is the paradigm, is a myth.

THE ROCKY MOUNTAIN TRENCH

The Rocky Mountain Trench *(sensu lato)*, which will be abbreviated herein to RMT, runs the full length of the Canadian Rockies, continuing northwest into Alaska and southeast into Montana. It dies out within the continent precisely at the inflection of the Idaho orocline and the beginning of the Basin-and-range, which together transfer the movement to the coastal region of California and Mexico.

The RMT consists of three segments in dextral echelon:

(a) The southern RMT, about 1100 km long, occupied in turn by the valleys of the Kootenay, Columbia, Canoe, Frazer and McGregor rivers;

(b) the northern RMT, 900 km long, occupied in turn by the valleys of the Parsnip, Peace, Finlay, Fox, Kechika and upper Liard rivers; and

(c) the Tintina lineament, more than 800 km long, occupied by the valleys of the St. Francis Lakes, and the Pelly, Stewart and upper Yukon rivers.

The nature of the RMT depends on the point of view. To a geomorphologist, the trench is a conspicuous topographic gutter which has been intensified by Pleistocene glaciation. The divides between the successive streams which occupy it in turn are all subdued, and overhung by the bordering ranges which continue through. But the line of the RMT has been a fundamental boundary for the last 800 million years, since North America rifted from east Asia (Fig. 135). Throughout the Late Proterozoic and Palaeozoic sedimentation, the line of the RMT has been the hinge where formations rapidly thickened, and changed facies from carbonates, mature sandstones, and shales, to greywackes and volcanics (Fig. 137). Throughout this time vulcanism and plutonism were confined to the west of the RMT line. The Nevadan granites are developed intermittently west of the trench but do not cross it. The flat Laramide nappes and thrust start to turn down towards their roots as soon as the trench is reached, indicating that here we have the boundary of a fundamental zone of tectonic diapirism and regurgitation (Carey 1962, pp.136-143). In our present context, the RMT is a great dextral megashear whose movement is transferred to the San Andreas family of shear systems via the coupled oroclines and the Basin-and-range extensions (Fig. 143). East of the trench is the North American shield, west of the trench is the orogenic rind of the continent, then the Pacific crust. The orogenic zone is the yielding zone accommodating the secular anticlockwise rotations of the continent and the ocean against each other, shearing along the trench, and dragging the orogenic rind into the drag folds now forming the oroclines.

The kinematics whereby a great megashear can terminate within a continent is shown in figure 143. The shear II of C terminates within the block, and progressive modulation of this basic pattern is shown in D and E. The pattern of the termination of the RMT is shown diagrammatically in figure 155. The trench is asymmetric. On the northeast wall the high ranges of British Columbia trend parallel to the trench; on the southwest side, by contrast, broad lake-filled valleys (like smaller editions of the

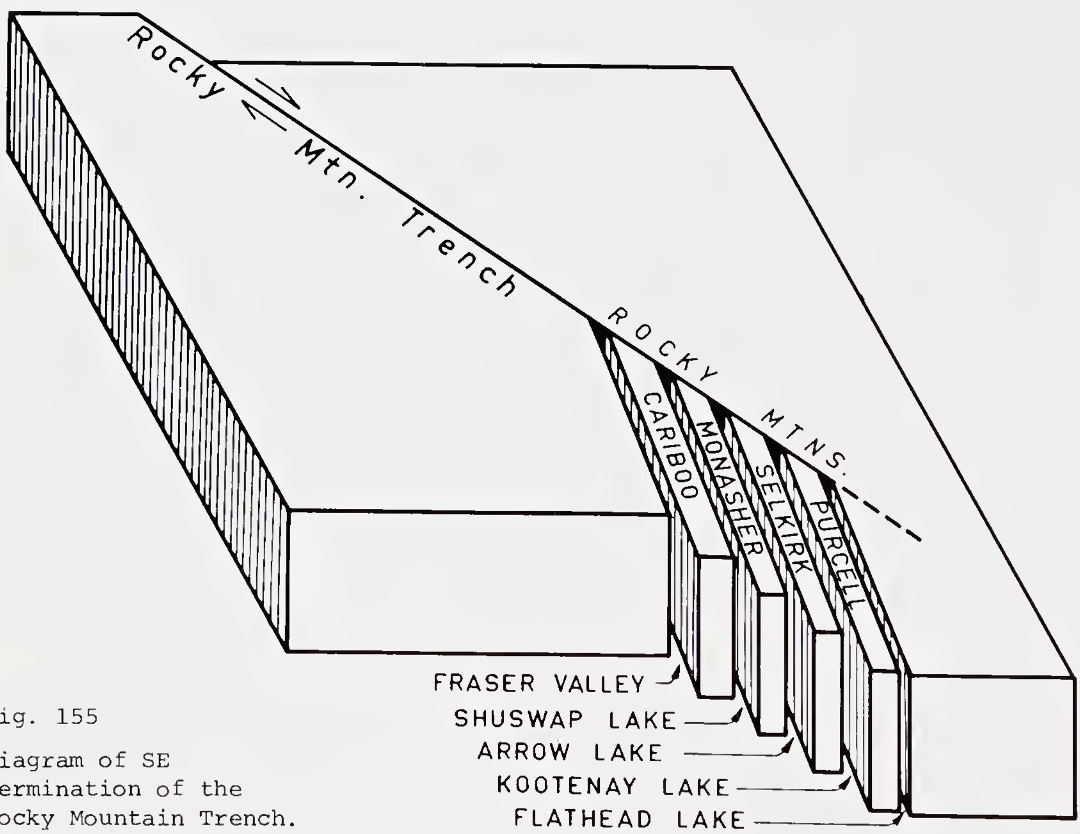


Fig. 155

Diagram of SE termination of the Rocky Mountain Trench.

trench itself) together with their separating ranges, impinge en echelon into the trench. These north to northnortheast-trending valleys (Flathead, Kootenay, Arrow, Shuswap, and Fraser) and their separating ranges (Purcell, Selkirk, Monosher, and Cariboo) have the same strike as the Basin-and-range grabens and horsts, with which they are cognate, and into which they merge. Figure 155 shows how a transcurrent fault can commence within a continent, and build up by increments from associated grabens, to become a megashear of the first order.

The southeastern end of the RMT follows a zig-zag path, made up of reaches trending alternately southeast (the dextral shear direction) and southsouthwest (the tensional rift direction) as in Flathead Lake valley (Fig. 156). This can be confusing when the nature of the trench is debated. For the rift legs are genuine rifts, whereas the transcurrent legs are genuinely transcurrent. Further north the dextral movements have added to a degree such that the transcurrent nature of the trench is not in doubt. Cook (1969) regarded the Sevier-Wasatch belt of grabens as the continuation of RMT. This is of course correct, but the kind of movement changes, just as it does between the zig-zags in Montana. Nevertheless the continuation of the RMT into the Wasatch-Sevier rift belt has special significance, for it does mark the final line of transfer of Cainozoic dextral movement from the RMT system to the San Andreas system. Between the Wasatch-Sevier belt and the Mendocino-Idaho oroclines lies the intensely sliced Basin-and-range province, southeast of the Wasatch-Sevier belt is the larger, less broken block of the Colorado plateau, separated by the Rio Grande rift belt from the next block which continues to the Tulsa-Laredo rift belt, beyond which is the disjunctive rhombochasm of the Gulf of Mexico.

The tectonic expression of the trench zone is complex, because we now

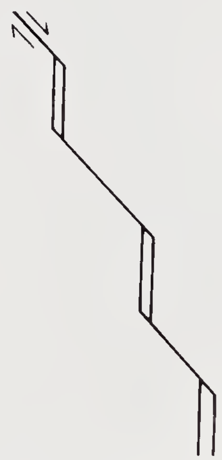


Fig. 156

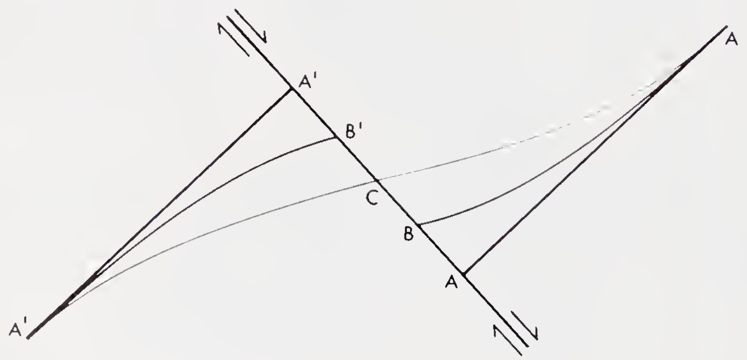
The southeastern end of the Rocky Mountain trench is a zig-zag alternation of dextral shear legs and tensional rift legs.

view the shear movement through a pile of Laramide nappes, which near the trench, have been deformed by the overprinting of the shear. There are two interfering processes. The Laramide orogeny resulted in the welling up and regurgitation of the gut of Cordilleran geosyncline, which extruded from a zone just west of the trench, to form a nappe-on-nappe pile of flat thrust-sheets spreading gravitationally eastwards. Tectonically this was a relatively rapid progress and was little affected by the regional dextral shear. Commencing eras earlier, and continuing ever since, has been the slower secular rotation of North America, with little vectorial addition to the faster movements of the nappes during their development, but adding up through the epochs to great total displacements. The slow secular distortion of the folds and thrusts formed by a relatively rapid gravity flow onto a glacier, is illustrated in figure 38. Similarly the secular megashear displacement in the floor under the nappes subsequently distorted the Laramide thrust sheets in the vicinity of the trench.

Flat-lying axial surfaces of recumbent folds trending oblique to the trench and facing away from it, first erect themselves as they approach the trench, then turn over the other way and face towards the trench -- thus forming "screw folds" (Leech, 1959). Deflections of thrust surfaces occur, similar to those reported by Longwell near Las Vegas, and Gilluly in the Shoshone mountains (see pp. 363-365). The "screw folds" are due to the interference of rotations, after the manner of precession (see Fig. 111). The shear movement in the basement may be wholly fault-offset with minimal drag. But the overlying sediment pile accommodates the offset increasingly by drag, so that high in the sediment pile, the rock boundaries may drag round continuously without fault offset (Fig. 157). Hence, the lower layers of a column of sediments are displaced more than the upper layers, which implies a rotational shear within the sediments which is clockwise about a *horizontal* axis normal to the fault. Looking down on the sediments, they are suffering clockwise rotational shear about a *vertical* axis.

Fig. 157

Basement offset AA' is partly taken up by drag in the sediment blanket BB', while high on the overburden the offset may be entirely accommodated by deformation A'CA.



These two rotations combine to yield a rotation about a horizontal axis normal to the other two, that is parallel to the fault trace, and clockwise looking southeast, or anticlockwise looking northwest. The hinges of the recumbent nappes generally converge towards the shear zones as they are traced northwest. Hence this anticlockwise resultant causes the axial surface of a recumbent nappe to steepen to vertical, then overturn towards the shear zone as the fold converges on the shear zone.

Thus several factors have emerged which confuse the recognition of the true nature of the RMT. The worst is perhaps the local secular overprinting of the slow regional shear on the fast gravity nappes and thrusts. But, particularly towards the southern end where the dextral shift progressively diminishes to near zero, there is the zig-zag alternation of rift and shear stretches, and the fact that, where the sediment cover is thick, structures may veer round and cross the trench zone without significant offset.

PRE-TERTIARY PATTERNS

The synergistic structures thus far described all developed during the last hundred million years or so. But the secular rotation of North America goes right back through the whole of the Phanerozoic, so similar kinds of structures should be expected in earlier times. This indeed proves to be the case.

Gilluly (1963, fig. 9) shows deep troughs of Upper Cretaceous sediments in Utah, Wyoming and Colorado up to six kilometres deep (Fig. 158). These

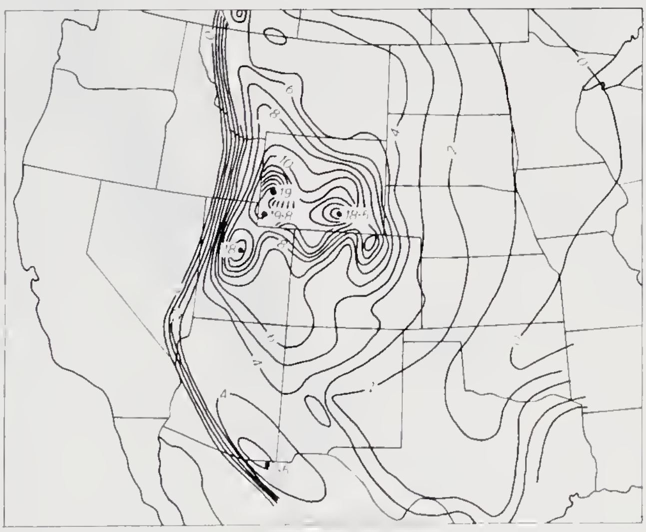


Fig. 158 An Upper Cretaceous Basin-and-range ancestor? (after Gilluly, 1963 and Reeside, 1944).

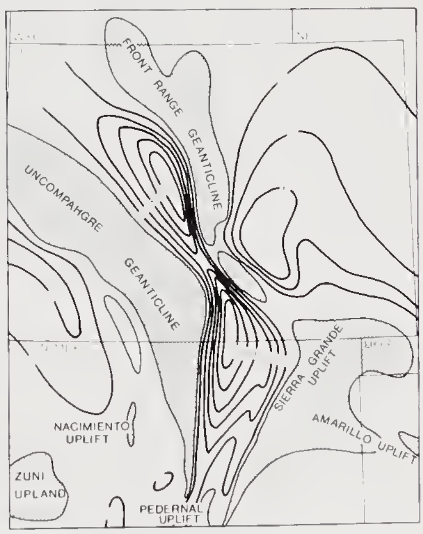


Fig. 159 A Permian Basin-and-range ancestor? (after King, 1959).

Isopach interval, one km

holes are oriented a little to the east of north, and have all the characters of a Cretaceous Basin-and-range province.

King (1959, Fig.57) shows deep troughs of Permian sediments in Colorado and New Mexico with more than three kilometres of sediment, which thinned out to zero within 15 km, which have all the characters of a Permian Basin-and-range province (Fig. 159). Here there is a combination of northnortheast and northwest troughs, corresponding to the tensional rifts and the dextral "lanes" of the Basin-and-range province. The combination of two trends at a large angle can only be produced by a wholly tensional or a transcurrent system.

Muehlberger (1965) claimed 300 to 400 km of dextral shift on the Texas lineament along the Mexican border during the late Palaeozoic (Mississippian to Permian) orogenic events. He based this estimate on an offset of more than 300 km of the Ouachita facies and an offset of some 400 km of the Precambrian "Grenville front". Muehlberger concludes that "Texas' original position was just west of Florida. This is a displacement of about 500 miles. According to this interpretation, the Ouachita system is the linear prolongation of the Appalachian system, the Grenville Front is nearly linear, and the Cambrian rhyolite and basalts of Florida, southern Georgia, and southern Oklahoma were originally part of the same volcanic terrane."

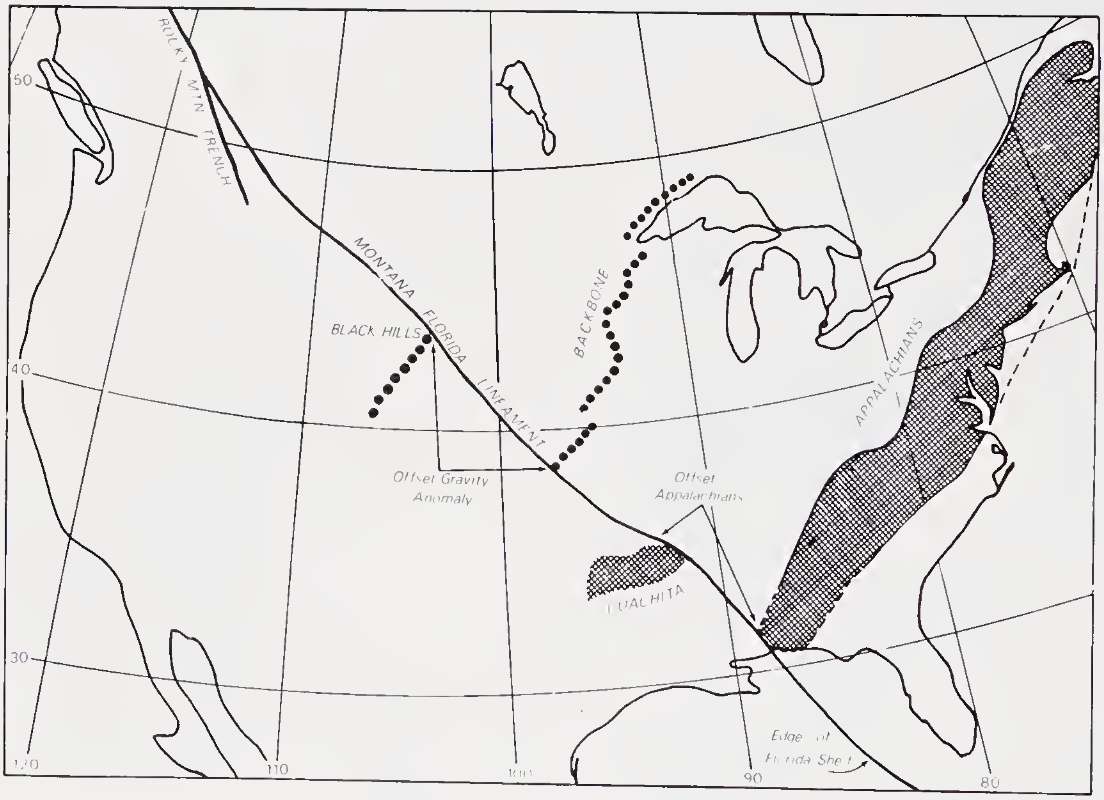


Fig. 160 Montana-Florida lineament. An ancestor of the Rocky Mountain Trench?

The Appalachians pass out of sight where the transgressive Cretaceous laps round them in Florida, but it is clear that continuation in this direction is out of the question because it is only a short distance to the oceanic crust of the Gulf of Mexico. Adopting Muehlberger's suggestion that the Appalachians have been offset dextrally and reappear in the Ouachitas, the cut-off line from the most southerly sub-surface indication of the Appalachians runs along the edge of the continental shelf west of Florida (Fig. 160). If this offsetting line is extended northwestwards, it truncates the southewstern end of the "Backbone of North America" the remarkable positive gravity anomaly, which extends from Lake Superior to central Kansas (Fig. 161). As the country has no significant relief this anomaly indicates a belt of exceptionally dense rock below the sediments. If we look for a dextral offset by the same amount as the offset of the Appalachians to the

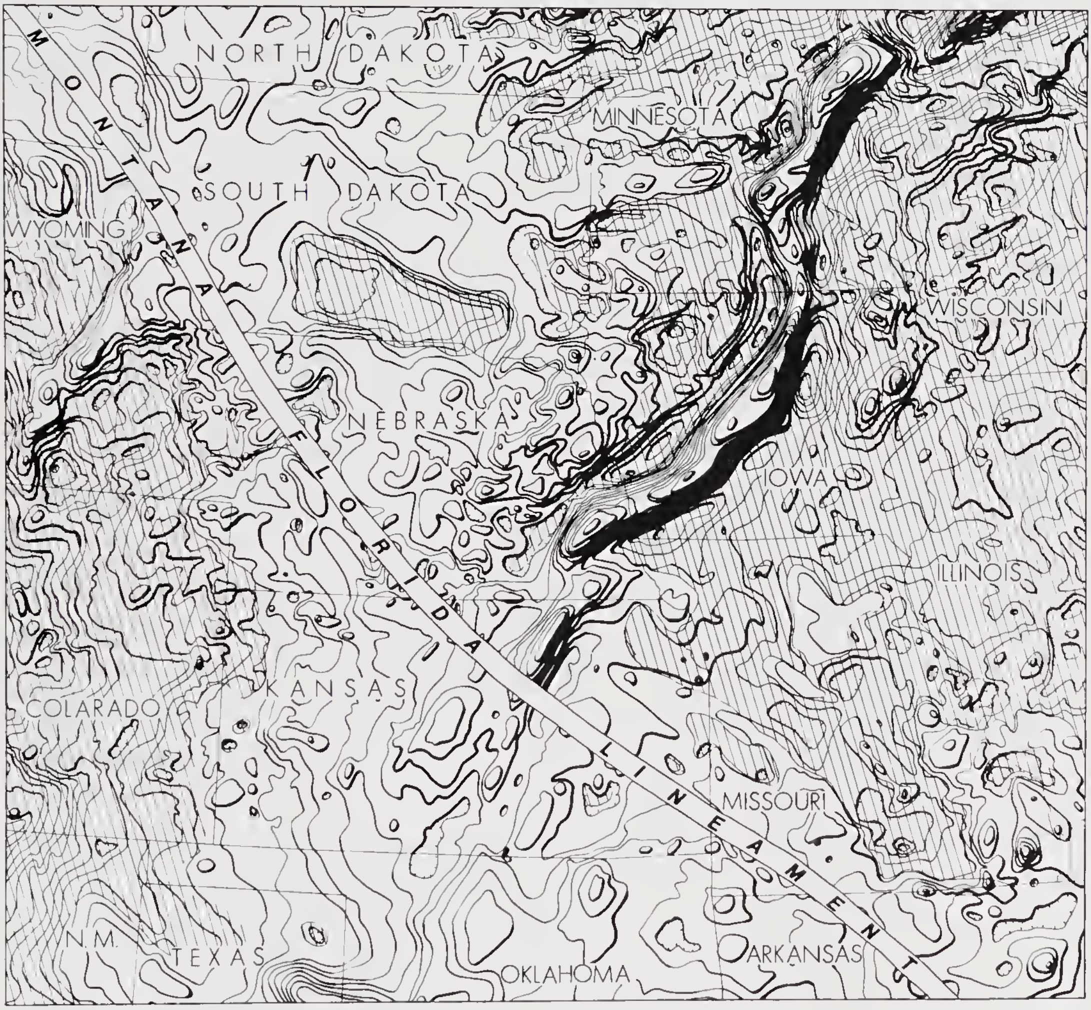


Fig. 161 Bouguer gravity relief map of Central United States showing the offset of the positive anomaly belt known as the "Backbone of North America" by the Montana-Florida lineament from central Kansas to the Black Hills of South Dakota. The shading indicates the region below the continental gradient.

Ouachitas, we find a similar gravity high trending in the same direction, from the Black Hills of South Dakota to Laramie, Wyoming. Here the positive anomaly becomes swamped by the steepening negative Bouguer anomalies of the Rocky Mountain Front. Further extension of this offsetting lineament is collinear with the Rocky Mountain Trench.

DEXTRAL SHIFT OF COASTAL FRINGE

The displacement of the Californian peninsula northwestwards with respect to the rest of North America is now well established. However several authors have suggested much larger northward displacements of coastal slivers.

Tozer (1970) reported a Triassic anomaly of more tropical faunas in the western eugeosynclinal rocks apposed to cold-water faunas in the miogeosyncline further east.

Monger and Ross (1971) found two contrasting foraminiferal facies and associated faunas ranging from Pennsylvanian to Permian in the Canadian cordillera, which could be interpreted in terms of contrast of facies transverse to the palaeogeographic grain, but could alternatively mean a gross dextral displacement along the grain.

Jones, Irwin, and Overshine (1972) reported an anomalous slice of lower Palaeozoic rocks in southeastern Alaska which appeared to be exotic, and matched similar rocks far to the south in the Klamath mountains.

Irving and Yole (1972) reported northward palaeomagnetic anomalies for several sites near the Pacific margin with respect to the main body of North America. They include: The Eocene Siletz volcanics of Oregon with a discrepancy of 57°; Upper Jurassic or Cretaceous Franciscan spilites, diabases, and ultramafics of California with discrepancies of 69°, 75°, 84°, and 86°; Upper Triassic Karmutsen volcanics of Vancouver Island with apparent northward shift of 55°. Irving and Yole state that these discrepancies may be in error by 10° but not by more than 20°.

All these discrepancies are concordant with the overall tectonic regime of the region whereby the coastal eugeosyncline has been the zone of dextral shear between the Pacific and North American blocks. However more work is needed to determine the details of the translations.

CANADIAN BORDER TRANSVERSE ZONE

Much has been written about transverse lineaments near the Canadian-United States border, which show up in so many ways that some fundamental feature must surely be implied. King (1959) has stated: "Between Helena and Missoula, Montana on the east, and Spokane, Washington on the west, the continuity of the Cordillera is interrupted by a series of west-northwest-trending transverse valleys . . . [which] furnished the route for the historic journey of Meriwether Lewis and William Clark in 1805 to the Pacific Coast . . . [which] today is followed by U.S. Highway 10 . . . The transverse valleys are followed by discontinuous high-angle faults of the same trend, of which the best known is the Osburn fault of northern Idaho. Along the same trend in the Eastern Ranges zones of abrupt flexing and belts of en echelon faults seem to express a draping of the sedimentary cover over faults in the basement rocks".

Projection of this line west-north-westward takes it along the northern edge of the Columbia re-entrant (suggesting perhaps that the offset of the orogenic belt from the northern end of the Idaho batholith to Vancouver Island may be in part a tear as well as an oroclinal bend) whence the lineament continues far out into the Pacific.

That the transverse lineament is quite fundamental is indicated by geomagnetic induction studies. The boundary of the North American craton against the Cordilleran orogenic diapir (which coincides with the Rocky Mountain Trench) shows up as a magnetotelluric boundary in deep-sounding traverses. Niblett, Whitham, and Caner (1969) reported that "the anomalous zone appears to be related to an upper mantle feature of major proportions. Its extension south of latitude 32°N and north of latitude 51°N is still undetermined, but between these parallels we have a structure which underlies nearly the whole of the cordillera". It is this fundamental boundary which is offset by the Canadian border transverse zone. Lajoie and Caner (1970) found "a sharp (near vertical) discontinuity in deep-electrical-conductivity structure, trending roughly east-west and located at the end of Kootenay Lake" which they interpreted as evidence of a sinistral strike slip below the Cordillera, striking 120°±20, with an offset of at least 50-75 km.

Kanasewich (1968) had previously reported a deep transverse graben structure south of Calgary which was detected independently by total magnetic field, by gravity anomaly and by deep seismic reflection. The Moho dropped by 9 km and the Riel discontinuity by 11 km.

An isopach map of the Cordilleran Upper Proterozoic and Lower Cambrian strata from Alaska to Mexico by Stewart, 1972, (Fig. 137) shows that the hinge, where these strata wedge from 25000 feet to zero, is offset 300 km dextrally along the Canadian border.

Although the evidence is strong that a deep transverse structure exists in the sub-continental foundation, the overburden also shows many odd features along this zone. The foothill front of the Rocky Mountains bends east by more than 200 km before resuming its trend. The Alberta syncline closes out. The Lewis thrust and McConnel thrust change trend eastward and plunge down into the Proterozoic from high in the Palaeozoic. The Fernie coal basin starts from this lineament with maximum width and depth and thins and

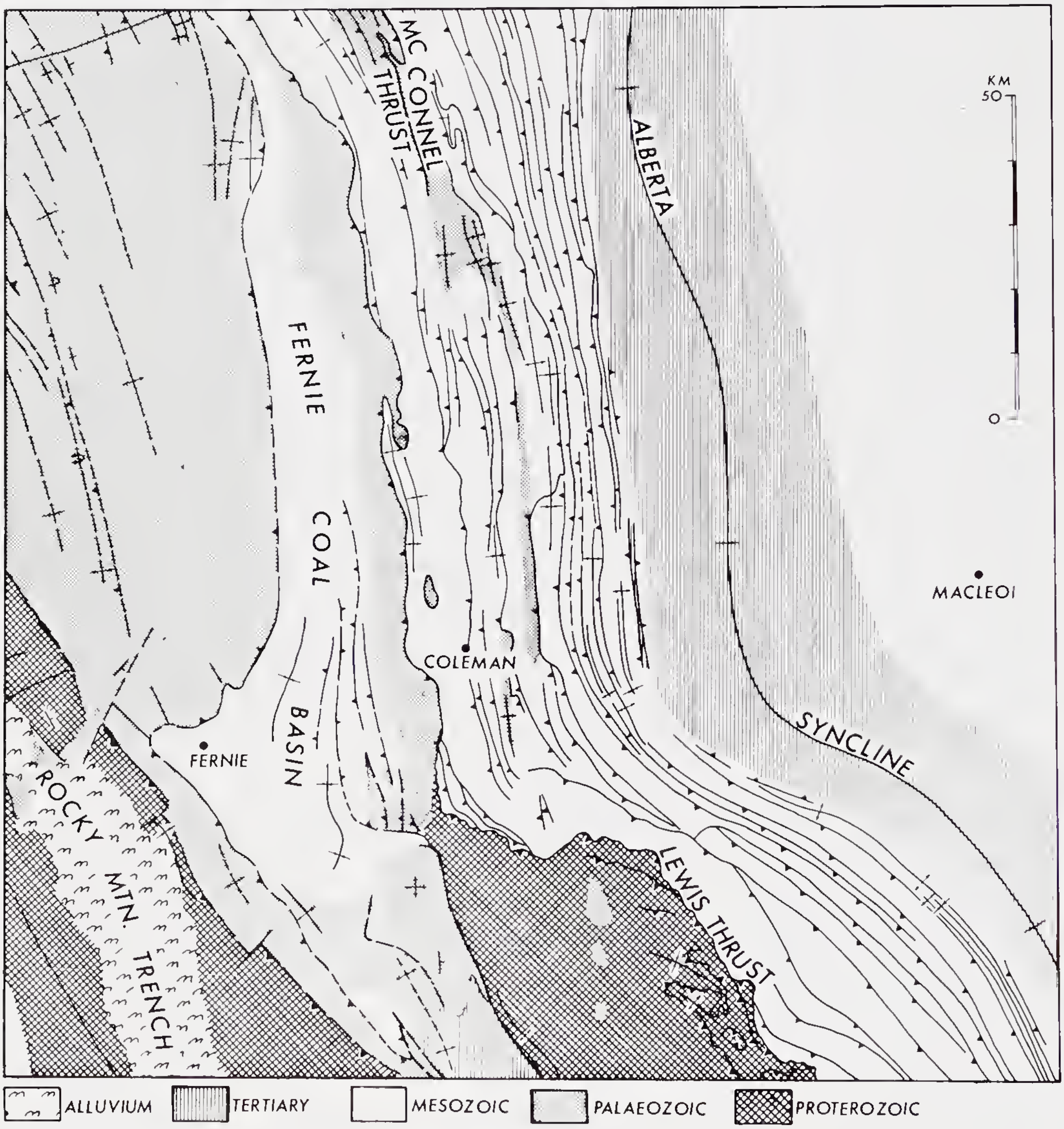


Fig. 162 Structural anomalies near the Canadian border transverse zone.

narrows away from it. This deep Cretaceous trough has the orientation of the tension zone associated with the regional northwest dextral shear and its sinistral conjugate. The Fernie trough suggests that a tension gape developed in the basement as a result of the transverse shear zone, and that the overlying sediment blanket sagged into it *pari passu* to form the Fernie lake, a Cretaceous analogue of Death Valley, with the same structural relation to the Canadian border transverse zone as Death Valley has to the Garlock fault and the transverse ranges of California.

CENTRAL AMERICA

TECTONIC CONTROL

Four primary processes dominate the tectonic evolution of the central American region: the general dispersion, the Tethyan torsion, the meridional elongation, and the peri-polygon cambium zones. The latter three flow directly from the general dispersion. The interaction of these processes is illustrated diagrammatically in figure 163.

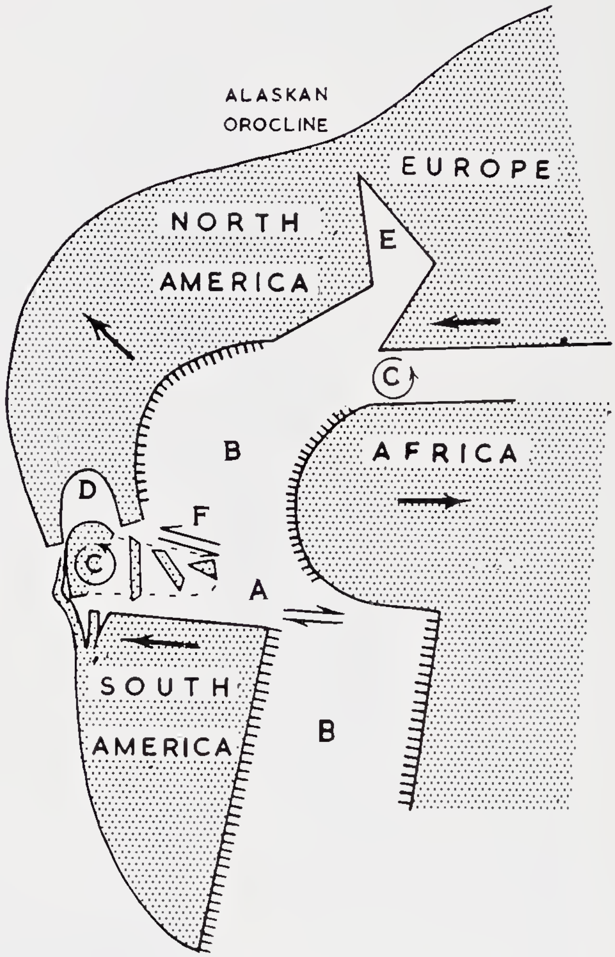


Fig. 163

Simplified and stylized diagram showing the relative displacement of the four Atlantic polygons:

- A Afro-Venezuelan dextral shear
- B North-Atlantic rhombochasm
- C Rotation of Tethyan torsion
- D Mexican diothesis
- E Arctic sphenochasm
- F Tethyan torsion

(from Carey, 1958a).

General Dispersion

Five first-order polygons are involved: Africa, North America, South America, the northern Eopacific and the southern Eopacific. Each has moved away from all others: North America from South America 2900 km, North America from Africa 6400 km, South America from Africa 4700 km, North America from Eopacific nearly 2500 km since the Mesozoic, and South America from

Eopacific 6000 km since the Mesozoic. The universal dispersion applies also to all the smaller units (compare Figs. 164 and 165). This is a property of an expanding surface on which every unit is a centre of dispersion for all other units.

Tethyan Torsion

The Tethyan torsion through central America has already been discussed (pages 257-263 and Figs. 96 and 97). In the Andean cordillera the torsion is expressed in a series of oroclinal drags with intervening sphenochasms (Fig. 97). In the Caribbean region the torsion is expressed by strong sinistral rotation of individual blocks, by large sinistral offsets on latitudinal megashears, and by the oroclinotath drag of the Lesser Antilles. The central megashear is the Clipperton shear zone which continued from the Pacific to the mouth of the Niger (Fig. 165) with a total offset of more than 3000 km. The sinistral displacement between Trinidad and Jamaica is 2000 km. Within the Pacific, the Clipperton distributes its distortion over its parallel system (including the Clarion, Molokai, Murray, Pioneer, and Mendocino), its dextral conjugates (the San Andreas, Texas-Las Vegas, and Montana-Florida families), and the NNE tensional associates (Fig. 164). The whole pattern forms a coherent integration of the synergistic processes (global dispersion, equatorial torsion, octantal rotations and cambium outgrowth with transform offsets) which have disrupted this region.

Meridional Elongation

Tanner (1971) has emphasised the meridional elongation across central America. Owing to the concurrent Tethyan torsion, the net effect is a series of latitudinal rhombochasms and grabens which are offset along a northwest-southeast array (Fig. 164). The three largest rhombochasms are the Gulf of Mexico, the northern Caribbean basin between Honduras and Cuba, and the main Caribbean basin between Venezuela and the Hispaniola-to-Nicaragua belt of shoals. However the Snake River graben from the Yellowstone Park to Twin Falls, and the New Mexico volcanic lineament, reflect the same extension. East-west normal faults belonging to this system have been reported by Ball and his co-authors (1971) from marine studies along the Caribbean shelf of Venezuela. South of the Caribbean the Orinoco depression (and its Tertiary predecessors, Fig. 166) and the main Amazon depression, dress with this array.

Concurrently, northnortheast-trending rifts were developing in the region (Laredo-Tulsa, Rio Grande, Wasatch-Sevier, Cascades, Seattle-Portland, Gorda, and Juan de Fuca). To this list, Schwartz *et al.* (1975) have added the Managua graben, 25 km across and at least 38 km long with possible

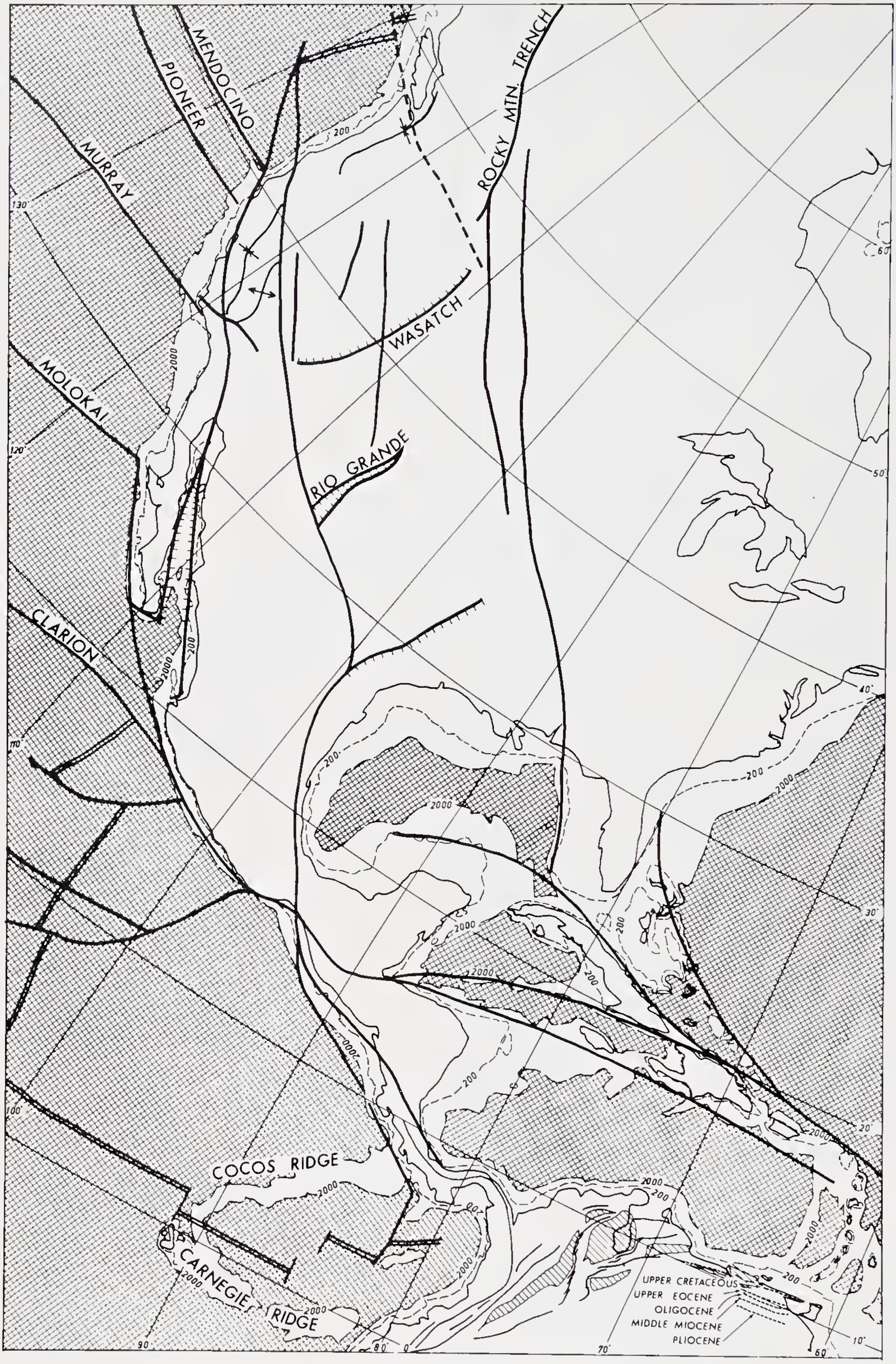


Fig. 164 Tectonic features of western America between Venezuela and Canada.

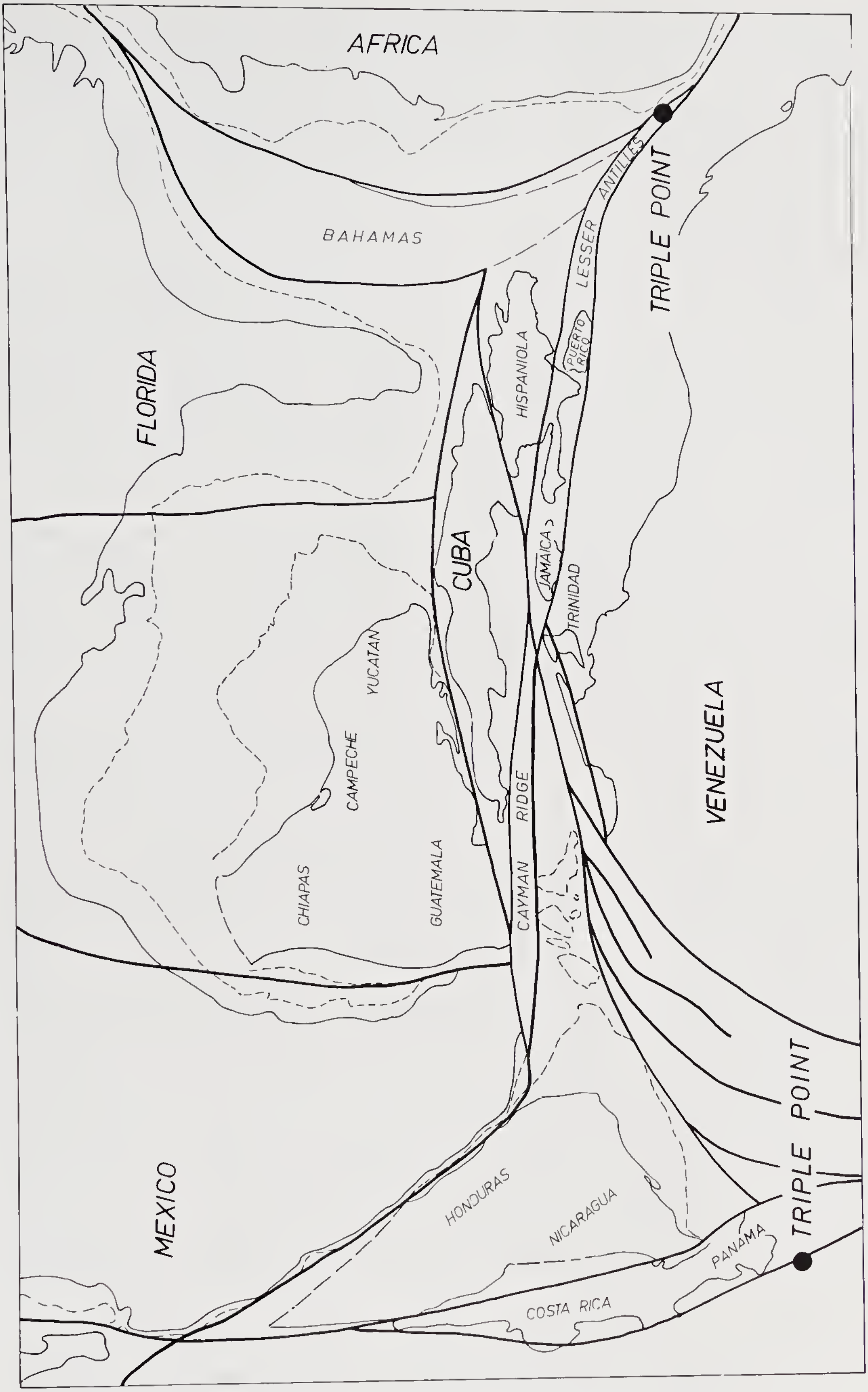


Fig. 165 Reconstruction of central American region.

continuation to the north. These near-meridional rifts are cognate with the sinistral latitudinal shears of the northwest Pacific and their dextral conjugates on the continent. In a uniformly expanding surface, three directions of extension may be expected (e.g. in columnar jointing), but in the non-uniform conditions with three or four synergistic processes, one or two commonly dominate.

Peri-polygon Cambium Growth

Raff (1968) recognized that an active spreading zone branched from the East Pacific rise west of the Galapagos islands, and suggested that it formed a triple junction with the East Pacific Rise, with the third branch extending to the Gulf of California. He also drew attention to the abrupt termination of the Carnegie and Cocos ridges, and asked whether all three could have been offset transcurrently along the trench line fringing the cordillera.

Van Andel *et al.* 1971, Holden and Dietz 1972, Forsyth 1972, MacDonald and Mudie, 1974, Klitgord and Mudie 1974, Williams *et al.* 1974, and Detrick *et al.* 1974 all recognized a spreading zone between the Cocos and Carnegie ridges, and from bathymetry, seismicity, and heat flux, variously identified spreading zones and transform offsets through this basin.

The triple junction recognized by Raff is indeed the junction of the cambium spreading ridges peripheral to the South American, North American and Eopacific polygons. The rate of spreading on the Panama basin leg is similar to that on the East Pacific rise (South America-Eopacific) and greater than that on the meridional leg (North America-Eopacific). The cambium spreading axis is offset by the transcurrent zone in the marginal trench, and shifts to the Motagua-Polochic zone, whence it divides the Gulf of Honduras, follows the Cayman trough (Bartlett trench), the trench flanking the south coast of Cuba, and the Puerto Rico trench. The transverse spreading across the zone between the two continents is nearly 3000 km.

The expression of the cambium spreading zone is necessarily different here from the standard spreading ridge in the oceans, because here it runs athwart young orogens, not-so-young orogens, and cratonized crust, so that a variety of bending, stretching, and rotating phenomena occur. This part of the peripheral cambium ridge is also complicated by the large offset of the Tethyan torsion, and naturally this integrates with the spreading movement.

Nevertheless a substantial bilateral symmetry has developed transverse to the cambium axis. The Yucatan block with the Campeche bank mirrors the Honduras-Nicaragua block and the Mosquito bank. The Jamaica-Puerto Rico

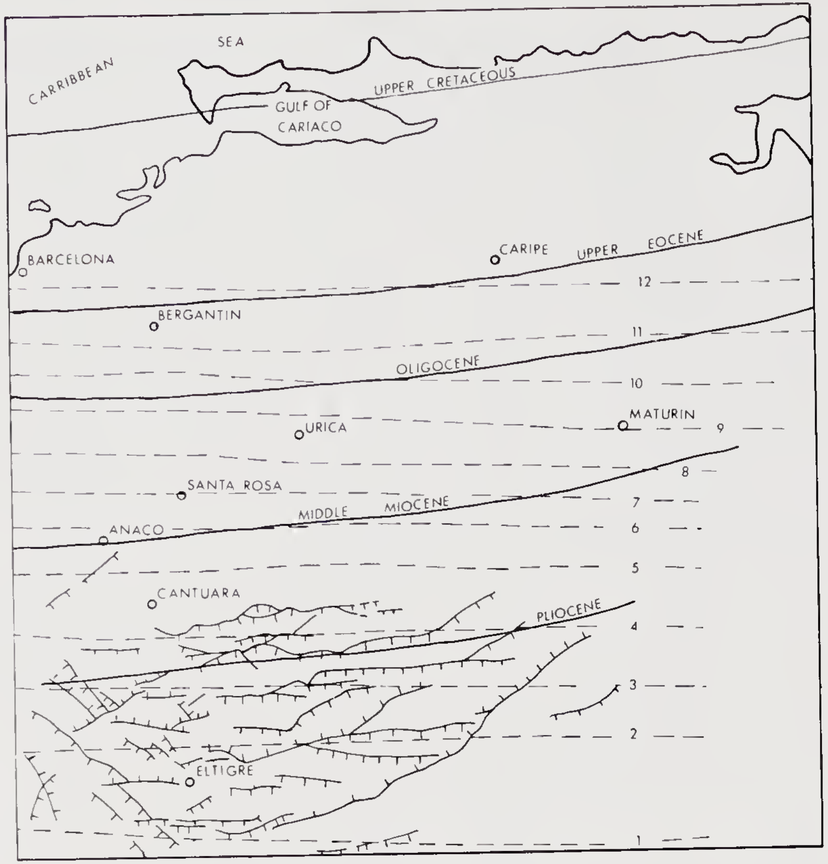


Fig. 166 Southward migration of trough axes in central Venezuela.

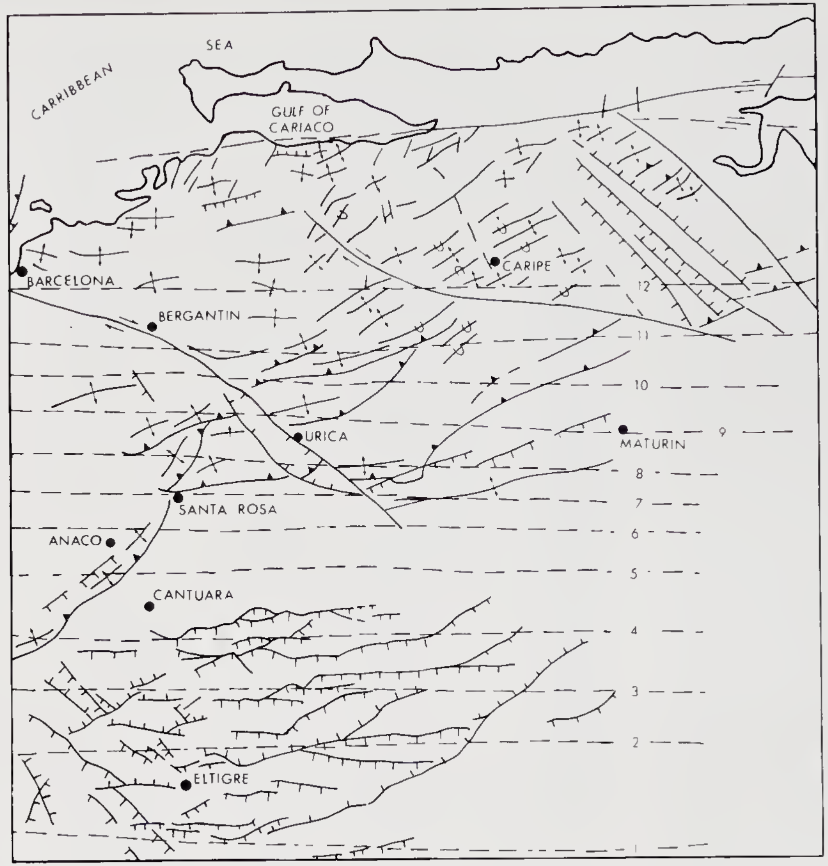


Fig. 167 Structures in central Venezuela (from Murany, 1972).

belt pairs with the Cuba-Hispaniola belt. The Gulf of Mexico pairs with the Caribbean Sea north of Venezuela. The Surinam-Guinea block mirrors the Sierra Leone block. The offset Pacific segment also has crude symmetry across the cambium axis, with the Cocos ridge pairing with the Carnegie ridge.

CONSTRAINTS ON RECONSTRUCTIONS

During the last twenty years I have offered three reconstructions of the Central American region - one as figure 24 presented at the 1956 continental drift symposium, one as figure 12 of my 1962 ANZAAS presidential address (reproduced herein as figure 99) and one presented here as figure 165. These differ primarily in the amount of sinistral rotation assumed within the disturbed zone of the Tethyan shear.

Nevertheless, there are constraints which severely restrict possible solutions (see pp. 252-256):

(1) First is topological homogeneity. All blocks must retain the same topological relation to other blocks. They may become separated by new oceanic crust, or be offset even large distances along megashears, or may be rotated, but they cannot leap-frog other blocks.

(2) Only new oceanic crust may be inserted to separate blocks, and reconstruction must be reached only by removing such oceanic crust and with shifting along megashears, or unwinding impressed oroclines.

(3) Continental crust cannot normally be destroyed or eliminated, nor created (except as new orogens). It must retain its size and shape except where visible strains are obvious. Stretching a continental zone by even a few kilometres could not occur without obvious rifts.

(4) Orogenic zones may bend (oroclines), or stretch (orotaths), or be interrupted by long distances, but in that case an orotath or oroclinotath would connect the former contiguous ends; such ends of orotaths must be brought into proximity in the reconstruction. Orogens are diapirs. They are *not* fore-shortened zones, but on the contrary they may have *widened* a little transverse to their strike, although not significantly so. The ends of nemataths springing from cratonic zones must be brought into close proximity.

(5) Magnetic growth strips, where clearly identifiable, give firm control on the movement pattern, but they are rarely identifiable unambiguously in the areas of large movements near orogenic zones, as in the Mediterranean, southeast Asia and central America. Palaeomagnetic rotations must also be satisfied.

(6) The reconstruction must be homogeneous tectonically, and in the geological integration in respect to petrology, palaeontology, palaeogeography, geochemistry, geochronology, and sedimentary provenance.
(7) The reconstruction, if it is correct, should yield unexpected integration and synthesis of previously non-related matters. For example, the through-going integration of the main sinistral megashear of the Tethyan torsion from the Clipperton zone through the Caribbean and the bundle of mid-Atlantic shears first reported by Heezen, to the Gulf of Guinea, was an unexpected synthesis of this reconstruction, as was the eastward extension of the Trinidad trend into Jamaica.

All three solutions which have been presented satisfy the first four criteria, but as more information has become available the fifth and sixth group of constraints have led towards the third reconstruction of figure 165. One important constraint is that the Sierra Maestra of southern Cuba runs out through Cape Cruz into the Cayman Ridge which is continuous into the open jaws of the Gulf of Honduras. Hence this relation must be maintained in telescoped form in the reconstruction. The fragmented blocks of the Bahamas and of the Mosquito bank stretching towards Jamaica must be held in the same topological relations as they are closed up. The former must retain their relation to Florida and Cuba and the latter to Jamaica except to the extent that they may have been offset along a megashear.

Within the constraints listed, there is still room for some permutation, but as more data become available these will be progressively restricted. For example the reconstruction at the 1956 symposium, while satisfying the general restraints, sought to find continuity of the Appalachean and Palaeozoic Andean orogens via central America, whereas the reconstruction of figure 165 gives continuity to the folds of Trinidad, which when last seen are striking east into the Atlantic, as though they were wholly independent of the Lesser Antilles which strike towards them from the north. Again, Jamaica (although different orogenically) looks as though it could be comfortably fitted back against the Pedro bank (which would be a normal relation across the grain) and thence to come to rest as an orogenic rim to the Mosquito bank off Honduras. This relation was held in the first two reconstructions, but relaxed in the third, but only by the intervention of the main megashear of the region. The trio, Cuba Florida and the Bahamas, could be simply closed up onto the central Cay Sal bank by narrowing the three-way straits between them, or Cuba may have lain further up into the Gulf of Mexico as in the first two reconstructions. The third is much the simpler topologically, and the most probable of the three tectonically. These several correlations, and several others which have been suggested, are the least reliable of the constraints, and have to be relaxed when challenged by non-equivocal data.

An important difference between the reconstruction presented in figure 165 and my 1962 version (Fig. 99) and my 1956 version (Fig. 24 of the continental drift symposium) is the position of the Honduras-Nicaragua block. The simplest solution is to regard the Gulf of Honduras as a simple sphenochasm, as Halm did long ago, and close the Mosquito Bank back against the Campeche Bank as in figure 99. This fit of the combined Guatemala-Honduras block into the Gulf of Mexico (less subsequent sedimentation) is possible (but a little tight). However, the Mesozoic vulcanism of Cuba has to terminate against the Ouachita fold belt, and a separate belt of Mesozoic vulcanism and plutonism along the present north coast of Honduras has to terminate against the Mexican cordillera. Such are possible, but at best suspect.

If the Honduras-Nicaragua block did not come out of the Gulf of Mexico, along with the Guatemala-Yucatan block, the only alternative is a large sinistral shift along the north coast of Honduras and along the Montagua-Polochic valley, where Hess (1938), Hess and Maxwell (1953), and Pinet (1972) have suggested large sinistral offset. Many authors subsequently have accepted this suggestion. Schwartz and Newcomb (1973) reported that: "a complex of regionally metamorphosed middle - upper amphibolite facies marbles, pelitic schists, gneisses, and metaigneous rocks borders the southern boundary of the fault zone. These belong to the Las Ovejas Group, contain the only known regionally developed occurrence of sillimanite in Central America, and are the highest grade rocks in the region. Las Ovejas rocks appear to trend into northern Honduras and may possibly continue out to sea as part of the Nicaraguan Rise. North of the fault zone and directly across from the Las Ovejas are highly deformed granite gneisses of the Chuacus Group. A melange of serpentinites, amphibolites, and red beds fills the fault zone. Preliminary structural, petrologic, and chemical comparisons between Chuacus and Las Ovejas indicate strong differences in the evolution and geologic history of these two basement complexes and suggests that the Motagua Fault zone represents a suture between two lithospheric plates."

Wilson (1974) confirmed intense tectonism along this zone with widespread crustal rupturing with lava flows and coarse conglomerates during early Cenomanian, and a very thick eugeosynclinal sequence of metamorphosed greywacke, limestone, radiolarian chert, and ultramafic rock in the early

Upper Cretaceous, which was deformed, plutonized, and metamorphosed in the Turonian-Santonian. Notwithstanding his clear recognition of tectonism, Wilson concluded that: "interpretation of the structural evolution of Nuclear Central America during the Cretaceous Period calls neither for subduction zones nor for catastrophic transcurrent faulting since the facies developments of Lower Cretaceous carbonate rocks north and south of the Montagua Valley neither support nor require such displacements."

The alternative position of the Honduras-Nicaragua block has several points in its favour. The main line of partition between the north and south American polygons emerges as a main equatorial sinistral shear at all stages, and the magnitude of the Tethyan torsion offset is certainly enough to displace the Honduras-Nicaragua block from its position in figure 165 to its position in figure 164. The opening of the south Caribbean basin between Nicaragua and Columbia-Venezuela is consistent in magnitude and direction with the meridional elongation which characterizes the region. The displacement of the Honduras-Nicaragua block is consistent with the fanning of the Venezuela-Columbia-Panama region as indicated in figure 97. It is simply another leaf to this system. The Mesozoic-meridional shears, equally dextral and sinistral, between Panama - Costa Rica and Mexico, Mexico and Guatemala, Guatemala and Florida, and Florida and Bahamas are the necessary tears as North and South America separated meridionally along interdigitated block salients. In this reconstruction all the Mesozoic vulcanism and plutonism of the Greater and Lesser Antilles and northern Venezuela contract into a single equatorial belt 300 km wide marking the initial rupture between the American polygons, and extending between the North America - South America - Pacific triple junction and the North America - South America - Africa triple junction. When this eugeosynclinal trench regurgitated, miogeosynclines developed on both flanks, and the trough of the miogeosyncline migrated away from the original arching orogen.

The reconstruction of figure 165 conserves the essential collinearity of the Appalachians - Ouachita orogenic belt into the Andes, and at no time from the Palaeozoic to the present has there been any substantial marine barrier separating North and South America. Connection between Pacific and the nascent Atlantic was only intermittent.

As this book was being proof-read Owen's competent analysis, *Continental displacement and expansion of the Earth during the Mesozoic and Cenozoic*, (Owen, 1976) was received. He had also placed Honduras west of Mexico in his reconstruction, from a different chain of reasoning.

REGIONAL TECTONICS

Sense of shears

The Tethyan zone generally, and this includes central America, is a zone of large sinistral offset, as indeed is established by the greater separation from Africa of North America compared with South America. Nevertheless important dextral offsets with the same trend also occur.

For example, the latitudinal shear between the north coast of Brazil and the Ivory, Gold, and Slave coasts of Africa was a *dextral* shift of a couple of thousand kilometres. If the opening of the north and south legs of the Atlantic was entirely contemporaneous, *pari passu,* such dextral shift could terminate at the cambium spreading ridge. However if the movements were not synchronous (and there is much evidence to suggest that they were not) then this dextral offset could extend much further west; and even though earlier or later parallel sinistral offsets occurred, they may have been a few degrees further north and the two sets of offsets would both be recorded, each in their respective domains. Such independence of movement of continental blocks is not unexpected theoretically, because the differential movements other than dispersion are caused by the differential moments of inertia of continental and oceanic crust, which in turn depend on the relative latitudes of the differentials. Herein lies another likely reason for the occurrence of both dextral and sinistral movements in the Caribbean region. For oceanic crust tends to go east with respect to continental crust (see pp. 268-270), so the Caribbean crust as a whole tends to go east with respect to both North America and South America, suggesting the expectation of sinistral offsets to the north of the Caribbean and dextral offsets on the Venezuelan side.

Whichever of these or other non-recognised causes may have been responsible, all geologists who have worked in Venezuela agree that dextral transcurrent shears and cognate structures are dominant there.

Venezuelan tectonics

The complex interaction of different tectonic processes in Venezuela is shown in figures 96, 97, 163, 166 and 167. The meridional elongation of part of Venezuela is shown in figure 166 by the thickening of the sediments, and the progressive southward migration of the trough axis as reported by Bell (1971) and Murany (1972). In the Upper Cretaceous the trough axis was through Caracas along the Araya and Paria peninsulas and the northern range of Trinidad. By the Upper Eocene the active trough had migrated 50 km further south, 80 km by the Oligocene, 120 by the Middle Miocene and 150 km by the Pliocene. Through the next 50 km young horsts and grabens break the surface and next south is the Orinoco depression. The initial trough was a trench (tensional) through the Antilles (Fig. 165), but as this regurgitated diapirically dragging upwards the lateral basement floor, a miogeosyncline developed south of the orogene. As the orogenic bulging continued, this trough axis migrated steadily south, away from the regurgitating orogenic zone.

The structural geology of the same region, adapted by Murany (1972) from official maps, is shown in figure 167. The Venezuelan dextral shear system dominates in the north, with 475 km of displacement on the El Pilar megashear along the axis of maximum sediment thickness (more than 12 km). This shear system continues through the 100 km wide belt south of the El Pilar megashear, not as transcurrent faults (the basement is too deep for that except on a large megashear like the El Pilar) but as southwest-trending folds and thrusts and southeast-trending tension faults.

Superimposed on this system are structures which I would interpret as induced by the gravity spreading towards the southeast and south from the regurgitating orogens along the axis of maximum sedimentation through the Merida, Cordillera de la Costa, and the Aruya-Paria. I suggest that the Amaco, Pirital, San Juan and Guanoco faults are the final emergences of bedding slides from the orogenic zones of the competent Merecure formation (Oligocene) on incompetent Caratas and Vidono formations (Eocene and Palaeocene). In this interpretation the Urica and San Francisco faults would be lateral lobe tears such as are developed in a similar situation in the Jura.

Oca-Orinoco line

A boundary of fundamental tectonic significance in Venezuela is the Oca-Orinoco line (Fig. 96a), which separates two different tectonic provinces and forms the northern limit of the Andes and of the Brazilian Shield. It might indeed be regarded as the northern boundary of South America. Along this line the Guayana craton, which is the northern extension of the Brazilian Shield, hinges down below more than ten kilometres thickness of Mesozoic and Tertiary sediments. South of this line the orogenic cores consist of Palaeozoic and Precambrian rocks. North of this line orogenic cores are Mesozoic metamorphics, Mesozoic basic igneous rocks and Mesozoic granites.

All the way from Cape Horn to the Oca-Orinoco line the Andes have cores of Precambrian and Palaeozoic rocks between troughs of Cretaceous sediments and Cretaceous granites. This persists into Columbian Central Cordillera and the Sierra Nevada de Santa Marta right to the Oca fault line. It is also true of the Columbian Eastern Cordillera, and the Sierra de Perija, where the pre-Mesozoic rocks again end sharply at the Oca fault. Still further east the easternmost virgation of the Andes, the Merida Andes, has a broad core of Precambrian and Devonian rocks and Palaeozoic granites right up to the Oca-Orinoco line. Beyond this line with a new trend are the Goajira Arch, the Falcon Uplift, the Paraguana Peninsula, the Cordillera de la Costa, the Paria Peninsula and the Trinidad northern range, none of which have any known Palaeozoic or Precambrian rocks, but instead all have Mesozoic metamorphics, Mesozoic ultrabasics, or Mesozoic granites. Thus both the Andes and the South American Shield end abruptly along the Oca-Orinoco line, across which is a transverse Mesozoic orogenic belt whose floor is simatic.

The Oca-Orinoco line also separates tectonic provinces. On the north, from Goajira to Trinidad the over-riding pattern is a dextral simple shear, first recognised by Bucher, which dominates the trends of folds, faults and powerful megashears - the Venezuelan Shear System. To the south are four pairs of sinistrally coupled oroclines, which separate a series of deep Tertiary basins.

The abrupt truncation of the Andes along the Oca-Orinoco line with no sign of gradual extinction would be surprising. But when we trace a structure to the brink of a later cross-cutting structure, surely we should look for the continuation beyond. We do not look in vain. Prior to the Mesozoic the Andes did not end here, but continued to a triple junction in central America with the Cordilleran and the Appalachians. But the early Mesozoic saw the beginning of the Atlantic, and the separation of the Americas from Africa and from each other. The Oca-Orinoco line is the southern margin of this Mesozoic rift which split the crust to its foundations, and allowed thick Jurassic sedimentation on the newly exposed simatic floor. The continued stretching and displacement was accompanied by simatic intrusions, strong metamorphism, folding and ultimately granitic intrusions.

Maracaibo sphenochasm

The Maracaibo sphenochasm has developed between the Trujillo and Falcon oroclines on two sides and the Santander and Perija oroclines on the other two sides. The Oca megashear is the bounding tear of the free limb. It is significant that the Oca megashear, which has a displacement of about 140 km where it cuts off the Sierra Nevada de Marta, does not cross the Merida-Trujillo-Falcon zone. On all sides the pre-Cretaceous floor of the sphenochasm drops sharply to a depth of 5 kilometres, where the floor continues to slope down to a depth of 10 km along the south-east side. There can be no question of depression of the ordinary sial crust to such depths because this would imply displacing 5 to 10 km thickness of sima in return for this thickness of sediment. This would involve a mass deficiency of from 8 to 20 million kilograms per square metre. The Maracaibo basin is a basin of dilatation, not of subsidence. The sialic crust has yawned open as the oroclines indicate, forming a sphenochasm with a simatic floor. Marginally the floor is sialic, but a rapidly thinning sial. Detached blocks of sial could of course occur within the basin. The stretching would have occurred along dilatational faults (Fig. 7) and was spread over many millions of years, the most active phase being in the Lower Tertiary (Middle and Upper Eocene).

In practice one would not expect the stretching at any time to be uniform but rather to be concentrated in relatively narrow rift zones or trenches which would fill up with very great thicknesses of sediment in a very short time. Synchronous sedimentation in other parts of the basin would of course be much less, and in a different facies, which might well be lagoonal, because the mass deficiency in the narrow zone of rifting would be distributed isostatically by regional shallowing of the sea, the total distributed uplift tending to equal the concentrated mass deficiency. Such filled fossil trenches might, in a composite sedimentary basin such as the Maracaibo depression, easily pass undetected or be not believed if encountered. However Sutton's description of the Trujillo Formation (1946, p. 1660 *et seq.*) fits this picture. The measured thickness of the Trujillo in the type section is nearly 5 km but the Angostura and Marcelina Formations which occupy the same narrow stratigraphic interval in the section in other parts of the basin have measured thicknesses of 400 m, 200 m, 75 m, for the former and 610 m for the latter, and in many places no deposition occurred at all. The facies of the Trujillo is believed to be deepwater partly marine, with scant fossils, while the Angostura and Marcelina are freshwater to brackish water shallow deposits. I quote verbatim Sutton's description:

"The Trujillo formation is composed of dark blue-gray to dark gray and black, locally micaceous and carbonaceous shale with subordinate amounts of gray and brown sandstone. The basal part of the formation exhibits marked induration almost to the point of metamorphism, and innumerable white quartz veins cut the shale and sandstone members at right angles to the bedding. These veins vary in width from less than one inch to 12 inches and are believed to represent secondary deposition along fractures by ascending mineral waters. Continuing upwards in the section, the quartz veins become progressively less in number and in the upper part are scarcely present. At the same time the shales become less hard, lose their slaty character and are generally soft and fissile. Occasionally present are ellipsoidal and discoidal, pyritic, sandy limestone concretions that weather to ironstone. Thin seams of sub-bituminous coal have been noted in the lower part of the formation. The sandstones, which are more pronounced in the upper half of the formation, are gray and brown, fine-to-medium-grained, micaceous, and locally carbonaceous. They are generally well stratified and vary in thickness from a few centimeters to 2 meters."

The equivalent Angostura and Marcelina Formations are composed of interbedded sandstone, shales, sandy shales and coal beds. The Angostura contains small ironstone concretions and the Marcelina sandy limestone nodules. The Trujillo and Marcelina Formations overly apparently conformably the Gusare Formations (Upper Palaeocene) and are followed with unconformity by the Misoa Formation (Upper Eocene). The Angostura transgresses unconformably on the Upper Cretaceous and is followed unconformably by the Misoa Formation.

My suggested interpretation of this association is that the Palaeocene erosion surface suffered stretching with a local Lower Eocene rift valley which deepened to nearly five kilometres. This was mostly filled with sediments as fast as it developed; the water was fresh, but the actual trench area was usually deeper than the broadly subsiding basin generally. The sea entered the deeper part during the later phase of the stretching, giving a scanty fossil fauna to the upper part of the Trujillo. Meanwhile the deepest Trujillo sediments now 4 km down were permeated by rising vapours ascending up the rift fractures through several km of earlier sediments. Through the active stretching period there was a negative gravity anomaly and hence sediments accumulated to greater thickness than could remain below sea level in isostatic equilibrium. When the rate of stretching eased in the Middle Eocene, the isostatic recovery caused the whole area to undergo some erosion before the Upper Eocene subsidence during the next active stretching.

There are likely to be other cases of sharp facies and thickness variation of this kind in the basin since at any one time the stretching might be largely concentrated in one or more trenches opened in other earlier parts of the sequence. The chances are perhaps against stumbling on them, since each such trough might be completely filled and the whole surface subjected to erosion.

The hinge belts described by Miller *et al.* (1958, p. 633) would be interpreted by me as buried rift lines. The slab between the trenches behaves like a platform carrying a reduced thickness of sedimentation, and even ages of non-deposition or erosion. The "Maracaibo Platform" described by Gonzales and Ponte (1951) and Miller *et al.* (1958, p. 615-8) fits this prescription, but my expectation would be that the reality is much less simple than these authors visualise. The conditions implied are described by Youngquist (1958, p. 699-702) in Peru: "The major highs and lows are outlined by large faults which reach basement and which have displacements of hundreds or even thousands of feet. These large faults generally increase in throw with depth, and are the zones along which the highs and lows have asserted their respective characters on a relative basis. There is a myriad of secondary faults with displacements of a fraction of an inch to several hundred feet." . . . "These highs, shelf areas, and lows, in general, have retained their respective high or low tendencies at least as far back as the very early Tertiary - though to different degrees at different times. Accordingly, they have influenced the nature and distribution of faunal and sedimentary facies during Tertiary depositional history. The influence of repeated movements of these highs relative to the lows is shown by the way in which the several unconformities follow the contours of the present structures, and by the fact that for almost any mappable Tertiary sedimentary unit, most structures can be outlined in general (and in some cases in considerable detail) by merely contouring the sand-shale ratios of that unit."

The folding in the Maracaibo basin does not necessarily imply regional compression. Some of the long folds such as the Petrolea-Tarra folds with long flat slopes basinward and steep to overturned limbs away from the lake seem to be gravity folds prior to the final isostatic rise. Others such as the Rio de Oro which are long, narrow, highly faulted, with flat slopes away and steep slopes towards the lake may be surface expression of the deep block faulting associated with the stretching. Others (such as the La Paz) could be diapiric, and the Oca transcurrent movements may have contributed local compression from the rotational couples. The general question of folds interpreted as compressional in an environment of regional dilatation will be discussed fully later.

The relation of the proposed Maracaibo reconstruction to the general pattern of the Caribbean is shown in figure 165.

Beata ridge

The Beata ridge (Fig. 164) which trends southsouthwest from Hispaniola seems to be a young structure of the whaleback arch type, similar to the Shatsky Rise and the Eauripik-New Guinea rise (see pp. 327-330). Like the Eauripik-New Guinea rise dividing the Caroline Basin, the Beata arch divides the Caribbean basin into two. Edgar, Ewing, and their co-workers in the deep-sea drilling project reported (1971) that the sedimentary column of bathyal sediments is similar on the crest of the arch to the section in the adjacent basins. This is characteristic of such whaleback arches. . The trend of the Beata ridge (like that in the Caroline basin) is that of extension structures associated with the Tethyan torsion.

Magdalena sphenochasm

The Magdalena sphenochasm is the recurrence *en echelon* of the same shear and dilatation movement as the Maracaibo sphenochasm (Fig. 96). Morales *et al.* (1958) report more than 12 km of Cretaceous and Tertiary strata in this narrow trough which is completely rimmed with Palaeozoic and Precambrian massifs which were positive through most of this time. Like the Maracaibo sphenochasm the isopachs indicate a progressive deepening of the floor towards the east-south-east side, where the basement rises sharply from 12 km below sea level to 3 km above sea level. Negative Bouguer gravity anomalies of 150 milligals suggest that the movements are currently continuing.

Gulf of Mexico diothesis

Framing the head of the Gulf of Mexico is a system of faults which include the Luling, Mexia, Balcones, and Curralvo fault zones (Fig. 168), all of which have been mapped in great detail on the surface and subsurface because of their role in trapping oil. Further south in Mexico is the Golden Lane. These fault zones reproduce on a continental scale the tension fracture pattern commonly found at the head of a landslide. Here they head the zone plucked out of North America during the dilatation between

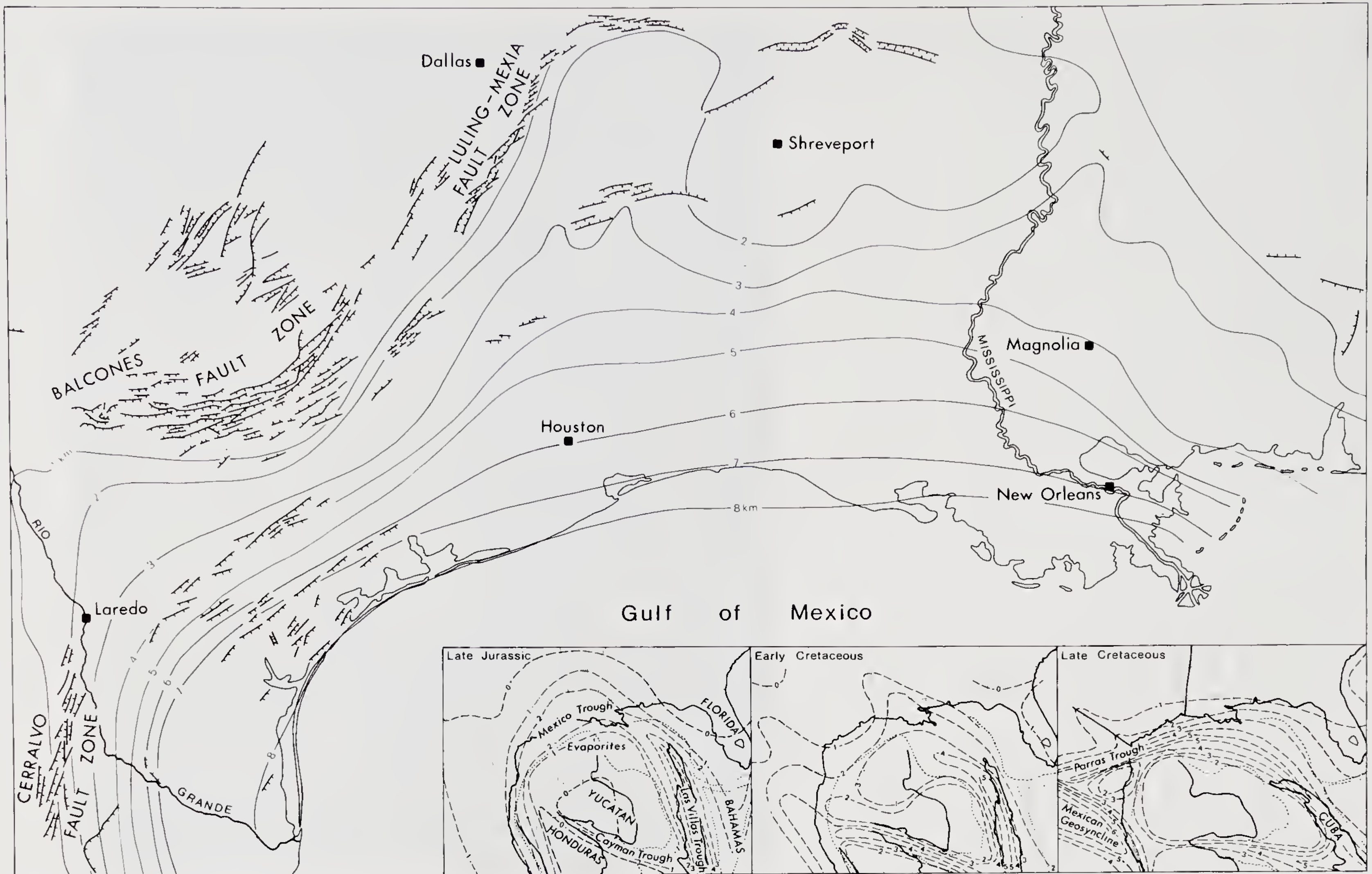


Fig. 168 Gulf of Mexico diothesis (after Carey, 1958a). The tensional fault systems frame the Gulf of Mexico like the head of a gigantic landslide. (Cf. Fig. 169).

North and South America. The faulting has continued intermittently throughout the Tertiary.

Cloos (1968) has modelled the fracture pattern of the Gulf coast (Fig. 169), using clay resting on horizontal floors, one of which was stationary and the other could be slid from under the first one to subject the clay mass to horizontal extension in its foundation. The pattern developed was astonishingly close to reality, as figure 169 has the actual faults superimposed on those experimentally produced. Cloos interpreted the genesis of this pattern to "regional gravity creep of the sedimentary blanket into the basin". Whereas this may be largely true, nevertheless his actual model reproduces accurately the pattern I propose of extension in the foundations; both processes would occur, and the partition between them would not be easy, nor all that relevant.

Figure 168 shows approximately the depth in km below sea level to the base of the Upper Cretaceous. The fault zone distribution clearly frames the general shape of this subsidence. Along the shore of the gulf the

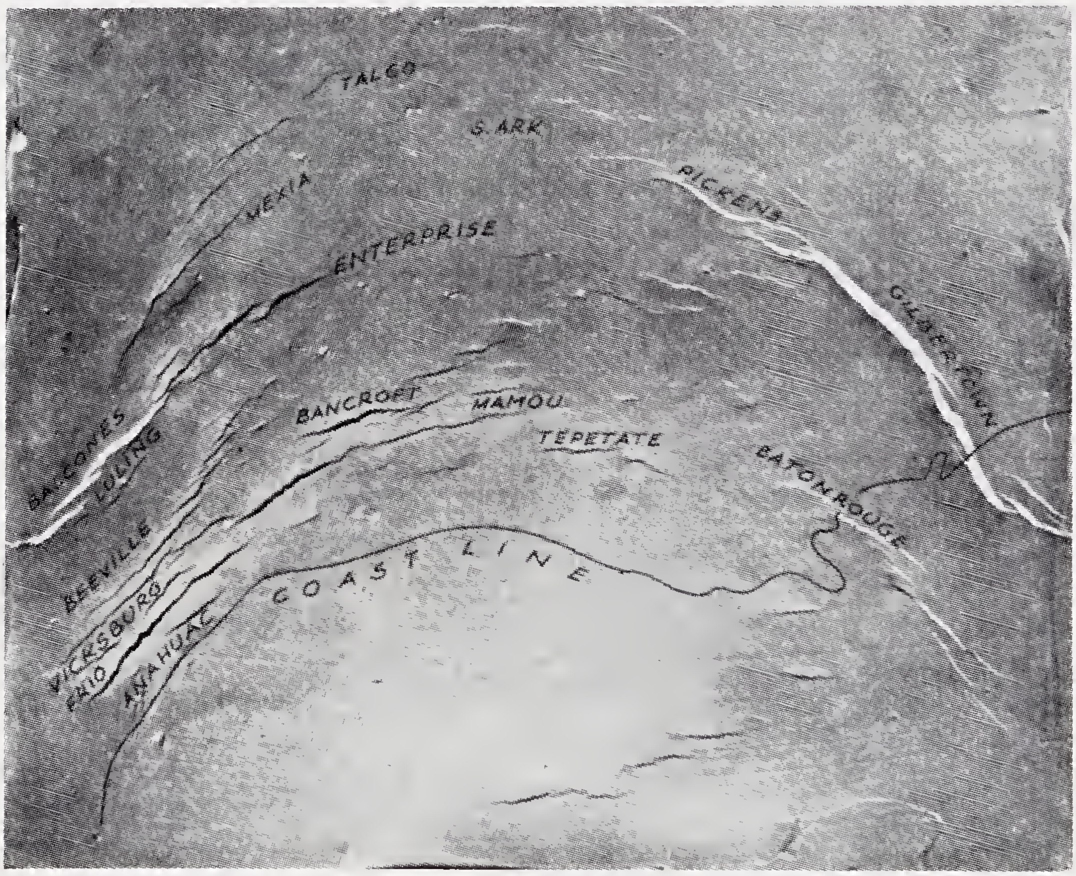


Fig. 169 Clay model made by Ernst Cloos to show the tensional origin of the Gulf of Mexico. The coastline and known fault zones have been superimposed on the clay model. (from *Am. Assoc. Petrol. Geol., Bull.* 52: 437).

Cretaceous has been depressed to eight km below sea level. But this does not represent a warping down of the pre-Cretaceous basement, since geophysical investigations indicate that the Mohorovicic discontinuity rises antipathetically with the fall of the sedimentary floor. The section across this rim is like figure 7F, with the addition of a thick clastic wedge.

Studies of the Palaeozoic sediments of south central United States indicate a source land (which has been called Llanoria), which lay to the south where the Gulf of Mexico now lies. Many palaeogeographers have postulated that Llanoria now lies foundered beneath the Gulf. Latterly, since the geophysical work has negated this, the suggestion has been advanced that Llanoria was literally eroded down to the Mohorovicic discontinuity and no longer exists as such. But erosion only proceeds to sea level, and what could lift the base of Llanoria above sea level for long periods to enable it to be eroded away? Llanoria has simply been drawn out of the gulf during the central American dilatation, and now forms Guatemala and Campeche.

The Gulf of Mexico diothesis commenced in the late Jurassic when the Tethyan torsion dividing Brazil from the coast of Africa splayed between the Americas. One tensional splay ran up the North Atlantic separating Florida and the Bahamas from the bulge of Africa. This tensional zone had already become active in the Rhaetic when an echelon series of grabens developed from the Gaspé peninsula through the Yankee states to the Carolinas. The sinistral torsion was mainly confined to a 300 km wide belt represented now by the Antilles, great and small (Fig. 165). But the meridional elongation dragged cratonic blocks from the North American polygon, so that the Bahamas, the Guatemala-Yucatan block, the Honduras block, and the Costa Rica - Panama belt lagged behind passively, while the North American polygon (with the Mexican and Florida salients) moved northwards; South America also moved northward (Colombia and Venezuela moved from south of the equator to north of it) but at a rate more slowly than the central American block, and still more slowly than the North American; the result was meridional dispersion.

All these rift troughs had restricted connections to the ancestral Pacific Ocean, and as the region was tropical, evaporites developed in all of them, and then extended through to Nigeria. Such are the salt beds of the Sigsbee knolls in the centre of the Gulf, Louann-Werner of southeastern United States, the Minas Viejas and the Salina of Mexico, the Punta Alegre of Cuba (see Kirkland and Gerhard, 1971), and the diapirs of the Exuma Sound intracratonic graben of the Bahamas (Ball *et al.*, 1968). These deposits were tectonically analogous to the salt deposits of the Dead Sea Rift, the Afar depression, and the western Mediterranean, all of which formed in restricted grabens during the early stages of continental separation. Extensive mafic vulcanisms developed in both the Cuban and the Jamaica-Venezuela troughs, and continued into the Palaeogene within this V. Most of Cuba is made up of metavolcanics and sediments the diapiric gut of this

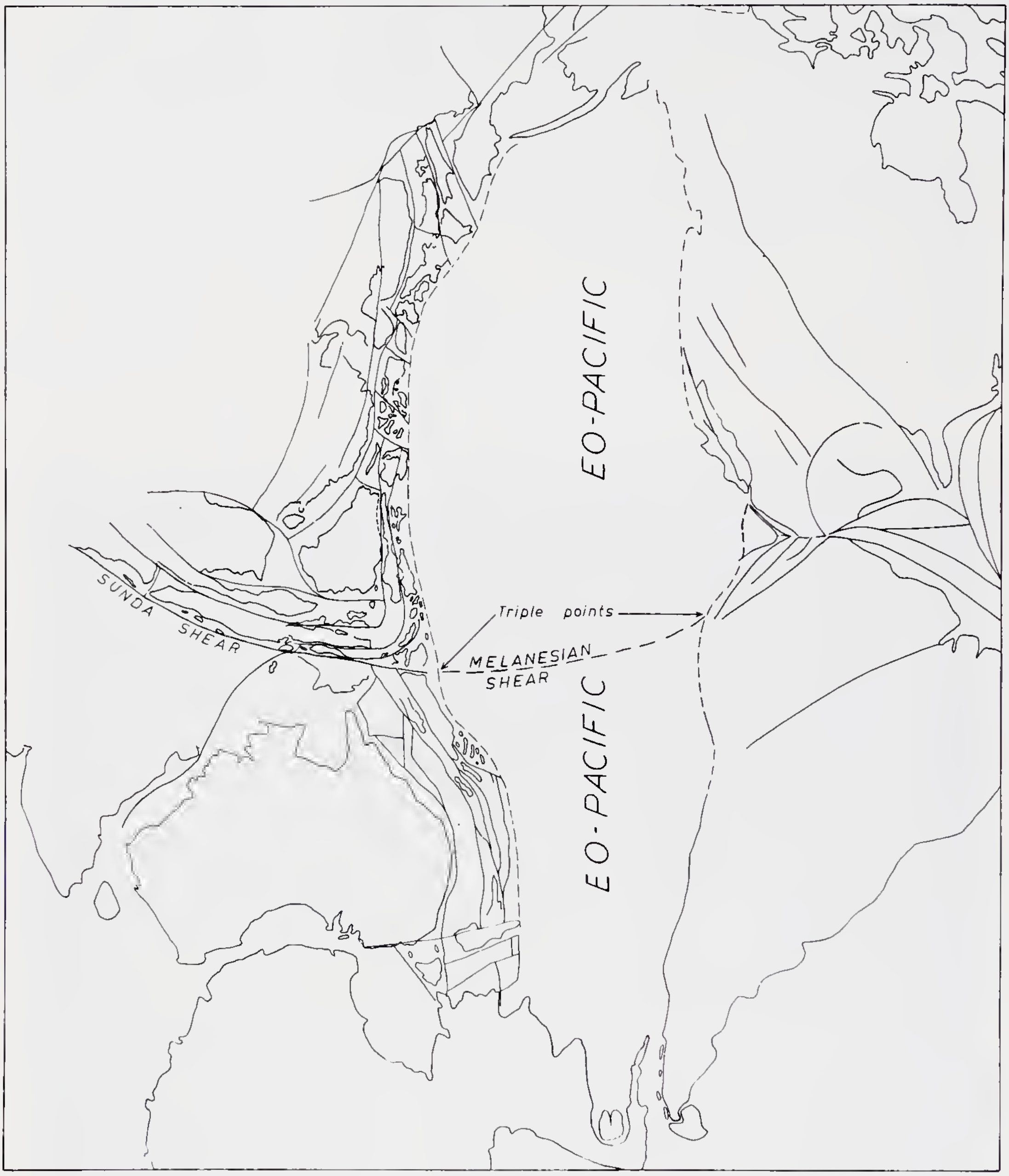


Fig. 170 Pacific Ocean of the early Mesozoic. This is not a rigorous projection but a composite. (Indeed no rigorous projection exists to incorporate large expansion). The central American region is based on figures 97, and 165; the Cordilleran on figure 148; southeast Asia on figures 178, and 179; the northwest Pacific on figure 180; and the southwest Pacific on figure 196. Elongation between Florida and Venezuela is due to artifact of projection and the inevitable "gaping gore" (p. 39).

everted trough, but bordering horsts of the Bahama pattern still outcrop along the northern margin of Cuba (Iturade-Vinent, 1975).

Movement continued during the early Cretaceous, and in the late Cretaceous a tension rift valley was formed on the west flank of the diothesis due to the drag of the Guatemala block being drawn past southwards. This trough, which has been called the Parras Trough received over 6 km thickness of Upper Cretaceous sediments. The Difunta Formation, which accumulated during the most active stage, itself reaches a thickness of 4 km (Eardley, 1951, Fig. 161).

SOUTH-EAST ASIA

Three first-order elements combined to shape southeast Asia: the dextral Ninetyeast shear system, the Tethyan shear system, and general dispersion of the parts. Ultimately these three elements stem from a single cause - expansion of the rotating earth. Reversal of these three elements restores southeast Asia to its early Mesozoic configuration (Fig. 179).

THE NINETYEAST RIDGE

The Ninetyeast ridge runs straight for 5000 km close to the 90°E meridian. It has been assigned every genesis possible for a submarine ridge: a dextral megashear (Carey, 1964, 1969, 1970, and herein), a horst (Francis and Raitt, 1967), a subduction crust-swallowing underthrust (Le Pichon and Heirtzler, 1968), a fracture zone (McKenzie and Sclater, 1971), a spreading ridge (Veevers, Jones, and Talent, 1971), a mantle plume (Morgan, 1972, Von der Borch, 1975), a "dead" transform fault (Crawford, 1973), an early spreading ridge changing into a transform fault then to a dextral megashear (Pal, 1972), and an anticlinal margin of an interplate boundary associated with a fracture zone "of exceptionally great offset" (Bowin, 1973). For my part, I confirm my initial conclusion *(loc. cit.)* that tectonically, the Ninetyeast ridge marks a dextral megashear along which India and Asia are offset nearly 4000 km with respect to Australia (Fig. 171). As such I can agree with Francis and Raitt that it is a horst, with McKenzie and Sclater that it is a fracture zone, with Veevers *et al.* that it is a spreading ridge, with Pal that it is a dextral megashear, and with Pal and Bowin that it is an interplate boundary, with arching. The dextral offset intersects and steps the Indian Ocean spreading ridge system. On a smaller scale the Ninetyeast ridge itself shows sigmoidal, en echelon, blocks and gashes, as would be expected as tension gashes on a dextral megashear. On the ridge crest these trend northeasterly (Bowin, 1973). Because the ridge is isostatically compensated with reduced P velocities in the upper mantle, Bowin postulates gabbro and serpentinised peridotite as the compensating material. Whereas this may well be true at least in part, simple elevation of the isotherms, through out-gassing, causing phase changes, may satisfy all the geophysical data, as in so many other ocean ridges (e.g. Shatsky, Eauripik-New Guinea, Ontong Java).

The Ninetyeast ridge began as tne floor of a rift valley along the initial rupture between India and Australia. In this trough vesicular basalts, volcano-clastics, lignites, and riverine sediments accumulated. The trough between the Naturaliste-Leewin horst and the Darling scarp in the extreme southwest of Australia is structurally analogous, although not necessarily coeval in sedimentation. Cook (1975) found that the low reflectivity of the ulminite of the Palaeocene coal in DDSP 214 implied sustained low thermal gradient inconsistent with the volcanic plume hypothesis. The subsequent separation of India and Australia involved large normal and dextral separation.

Whenever such a megashear intersects a contemporary orogenic belt, the fracture offset changes into an oroclinal couple. Vulcanism and plutonism characteristic of orogens imply higher temperatures deep into the mantle, so

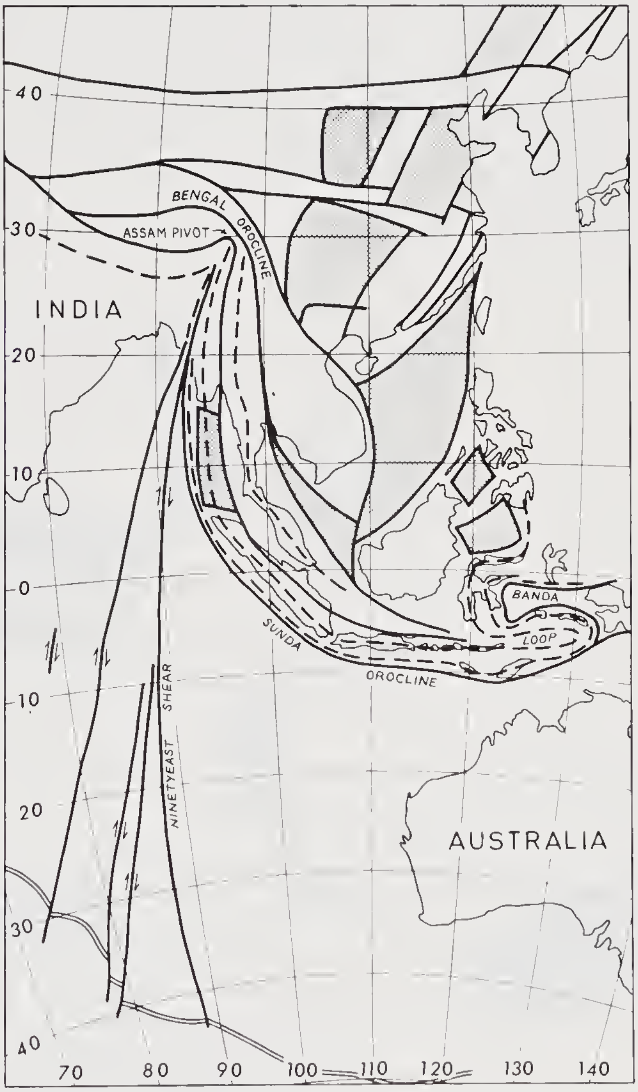


Fig. 171

Separation of Australia and India by:

(a) Ninetyeast shear

(b) Bengal and Sunda oroclines

(c) compensating sphenochasms.

that orogens bend rather than fracture unless the rate of strain is very fast. Hence the Tethyan belt, trending roughly east-west through the Himalaya and north of it, wheels in Assam to trend southwards through Burma and Thailand, and south-eastward through Sumatra and the Malay peninsula, before regaining the east-west trend through Java, the Moluccas, and New Guinea. The amount of offset between the two east-west trends is some 33° or 3900 km. Move India south this amount along the Ninetyeast ridge with respect to Australia (or Australia north with respect to India) the amount of this oroclinal offset, then the angle of Australia's Northwest Cape is brought directly opposite the Andhra re-entrant of the Bay of Bengal (Fig. 172). If Australia is now moved west the 3000 kilometers necessary to straighten the stretched dragfold (oroclinotath) around the Banda Sea (Fig. 173), Australia is brought right back to its Palaeozoic position with respect to India indicated by the geological disjuncts. In this position the Jurassic palaeomagnetic pole derived from the Tasmanian dolerites approaches the Jurassic palaeomagnetic pole derived from the Indian Rajmahal Traps and the Sylhet Traps (Fig. 174), the Permian fauna of Umaria falls adjacent to the equivalent Lyons fauna which it so closely resembles (Dickins and Thomas, 1958), and the Cretaceous Trichinopoly and Minilya faunas also come together (see Teichert, 1939).

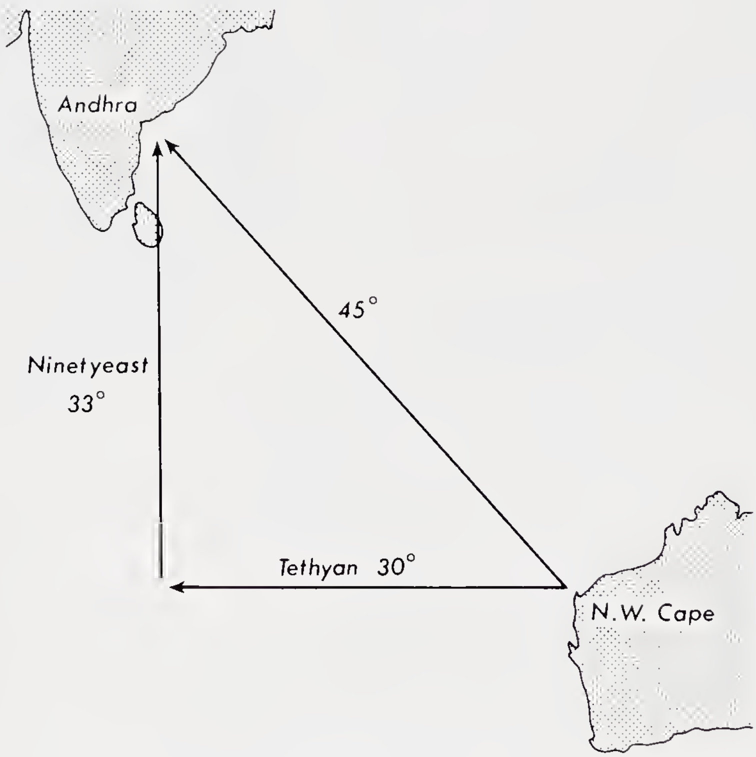


Fig. 172 Tethyan and Ninetyeast components of relative displacements of India and Australia.

Beyond the oroclinal zone, the megashear resumes as a series of fractures which extend up through eastern Asia, cross-cutting the sinistral shears of the Tethyan belt (Figs. 175 and 177). A line from Assam to the Kolyma in far east Siberia, separates two grossly contrasting tectonic regions on even the earliest structural syntheses of Asia. Compare, for example, the 1945 compilation by Holmes, reproduced in Fig. 176. This line, which is the continuation of the Ninetyeast shear, divides the Asian intracontinental region from the periPacific dextral zone. Chen Guoda and his co-authors (1975) have confirmed the fundamental dichotomy of China along this line. Their report is summarised in the Commonwealth Geological Liaison Office Newsletter (75NLIO, p.9) as follows: "The crust of China is composed of two major blocks, or the W. and the E. halves, with the N-S diwa [geo-depression] region in between. The two halves manifest a sharp contrast in geotectonic history and characteristics, structural trends and systems, landforms, geophysical properties, crust thickness, and in the depth of the Moho surface. They also differ greatly in the mode of tectonic movement, the direction of the horizontal and vertical movements, metallogenesis, and in seismicity. Between the two, the N-S diwa region

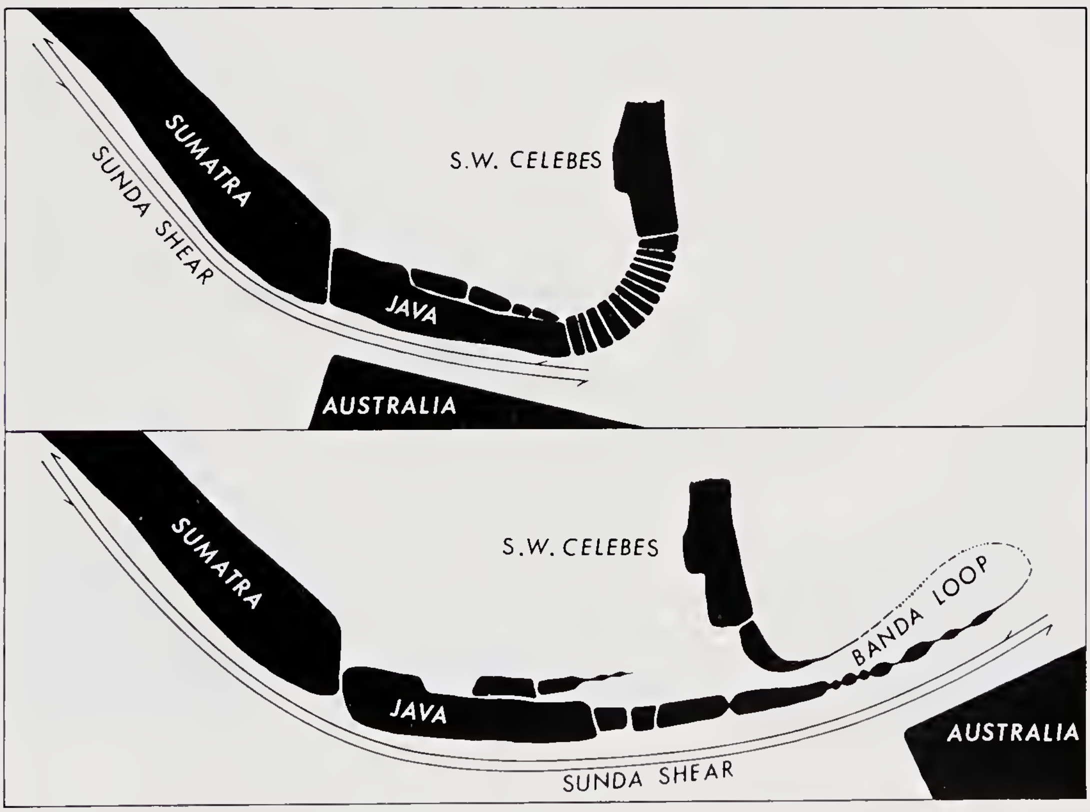


Fig. 173 The Banda Loop is greatly lengthened and attenuated by the relative movement of India and Australia.

is a narrow belt of fracture zones 2000 km long, in which lies the abyssal fault of Yinchuan-Kunming. It characterizes the transition between the two halves in all geotectonic aspects."

Palaeomagnetic latitudes at sites west of AA (Fig. 175) show significantly greater northerly shifts than those at sites east of AA, confirming the general dextral shift between them. The disjunctive openings between the lesser Sunda Islands and central China (Banda, Celebes, Sula and South China Seas, and the Szechwan basin) adds up to approximately 34° of extension (Fig. 186), in agreement with the oroclinal offset between the Himalayas and the Sunda-Archipelago (Fig. 171).

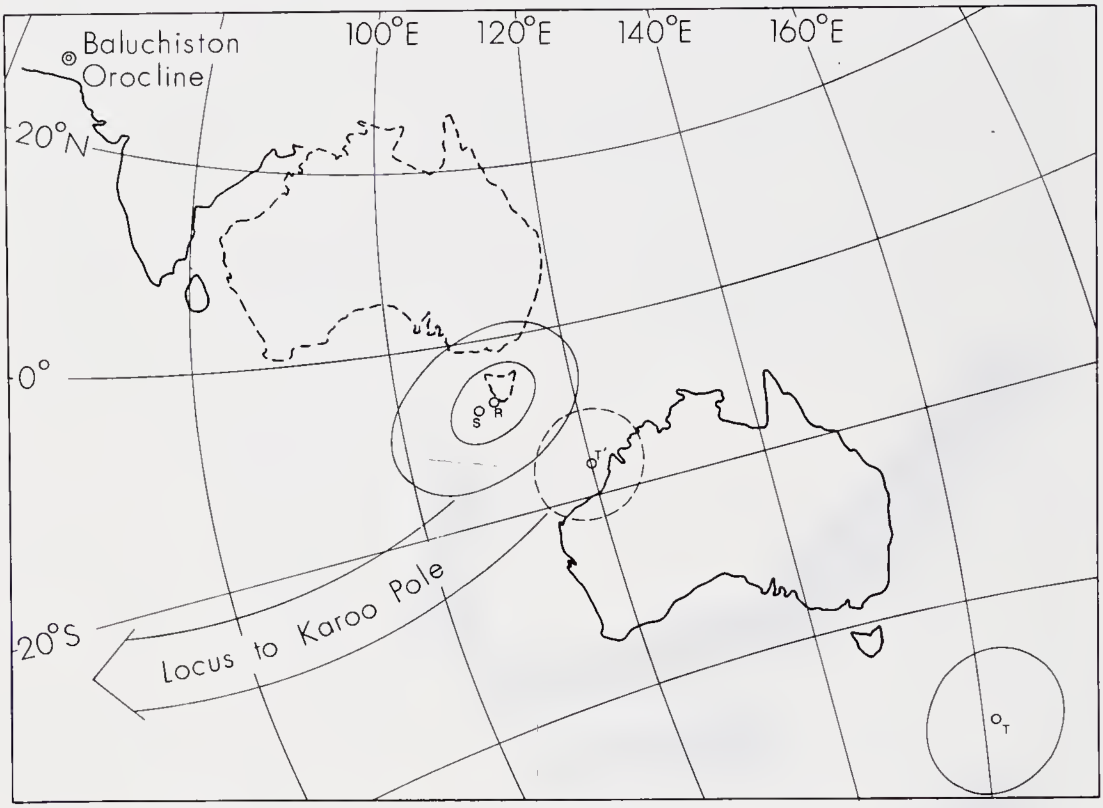


Fig. 174

Jurassic palaeomagnetic tie of India and Australia. T is Jurassic pole from the Tasmanian dolerites; R from the Indian Rajmahal Traps; S from the Sylhet Traps (all data from Irving, 1964, p. 308-309). Restoring Australia to the Bay of Bengal (broken lines), moves the Tasmanian pole close to the Indian poles. A closer fitting of the coasts would further improve the overlap. The broad arrow shows the locus of these Jurassic poles as the Indo-Australian block is turned back about the Baluchustan Orocline to close the western Indian Ocean. This brings the Indo-Australian poles towards the Jurassic poles determined from the Karoo dolerites of South Africa. Small additional rotations are required to close the Persian Gulf and the Red Sea.

ASIA-AUSTRALIA RECONSTRUCTION

The structural pattern between Asia and Australia looks too complex to unravel. But the reversal of three processes, the Tethyan torsion, the Ninetyeast and periPacific dextral megashear, and a general dispersion, solves this problem quite simply (Figs. 178 to 184). In the Indian Ocean sector these three displacements are accommodated by shears and differential opening of sphenochasms. In the Cainozoic orogenic zone the displacements cause oroclinal bending, attenuation, and drag loops. The east Asia sector has a group of compensating sphenochasms bounded by disjunctive rifts and shears, on land and at sea.

The restoration of such a region is the antithesis of a jig-saw puzzle. Pieces cannot be placed wherever they may appear to fit. Each block must retain its topological relation to all its neighbours, and to all faults. It may have separated from its neighbour by insertion of new crust in

Fig. 175 Photograph of six foot diameter relief globe showing the topographic expression of the bent trends of the Bengal orocline (broken lines) and the transverse trends of the Ninetyeast-peri-Pacific shear zone (continuous lines). The displacement is mainly by bending in regions of Cainozoic orogeny but by fractures in regions previously consolidated.

between, or by transcurrent movement along a megashear, but all blocks must appear in the same spatial order (albeit dispersed). Orotaths and oro-clinotaths must be grossly shortened in the reconstruction, and the two ends of the orotath must be directly contiguous without intervening blocks or structures. Compare, for example, in figures 178 and 179 how the orotaths (which are indicated by numbered stars in Fig. 179) control the reconstruction. Northwards beyond figures 178 and 179 are the Bashi orotath linking Luzon and Formosa, the Ryukyus linking Formosa and Kyushu, the Kuriles linking Hokkaido to Kamchatka, and the Aleutians linking central Kamchatka to the Alaska Range. Further out is the Bonin-Marianas orotath linking Honshu to Halmahera. All these orotaths are greatly shortened in the Pangaean restorations (Figs. 179, 180,

Progressive evolution from figures 179 to 178 is suggested in figures 180 to 184. In these, synchronism is not attempted, because more data and a much closer analysis would be needed. Hence, in these diagrams, the various displacements have been distributed for the most part between the four stages even though some of the movements were confined in fact to only one or two of these stages. Moreover, these diagrams are not intended to be palaeographic or even palinspastic restorations. All islands and coasts

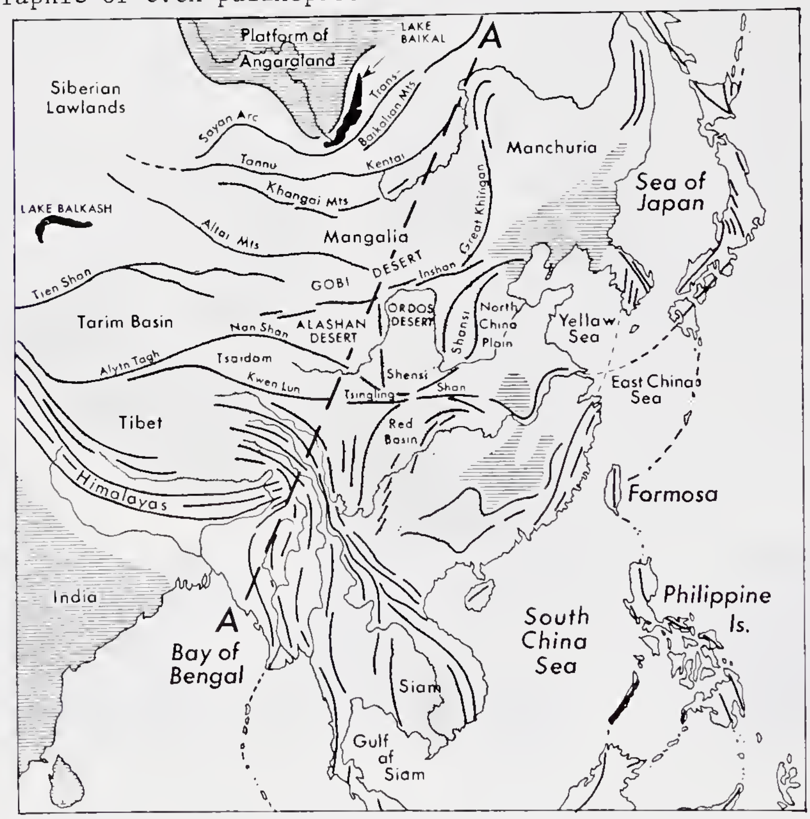


Fig. 176 Synthesis of tectonics of Asia from Holmes, 1945, figure 222. The line AA (added by the present author) divides Asia into two contrasting regions. This is the Ninetyeast shear zone.

have been drawn in their modern outlines for identification, in order to show the movement pattern during the evolution of the western Pacific margin.

Audley-Charles (1965, 1966a, b) attempted a reconstruction of the paleogeography of this region through the Permian and Mesozoic, arising out of his extensive field work in Timor, where he separated the Permian rocks into autochthonous Cribas and Atahoc Formations derived from Australian denudation, and allochthonous Aileu and Maubisse formations tectonically transported southward. In the wider synthesis Audley-Charles' main thesis is that the spatial relationship between Timor and Australia has been relatively unaltered throughout the Mesozoic and that the spatial relationship between Asia and Australia have not altered since the beginning of the Upper Triassic at least. However, a careful study of Audley-Charles' papers indicates clearly that this conclusion is rather a preferred prejudgment than a necessary consequence of the data he cites. As the author himself states (1966b, p. 2-3): "For brevity no attempt will be made to prove this spatial stability, rather it will be taken as a premise, and through discussion of the palaeogeography of Australasia, based on that hypothesis, the evidence for its validity will be evinced. Not only does the Mesozoic palaeogeographic history of Australasia emerge plausibly, but the chief

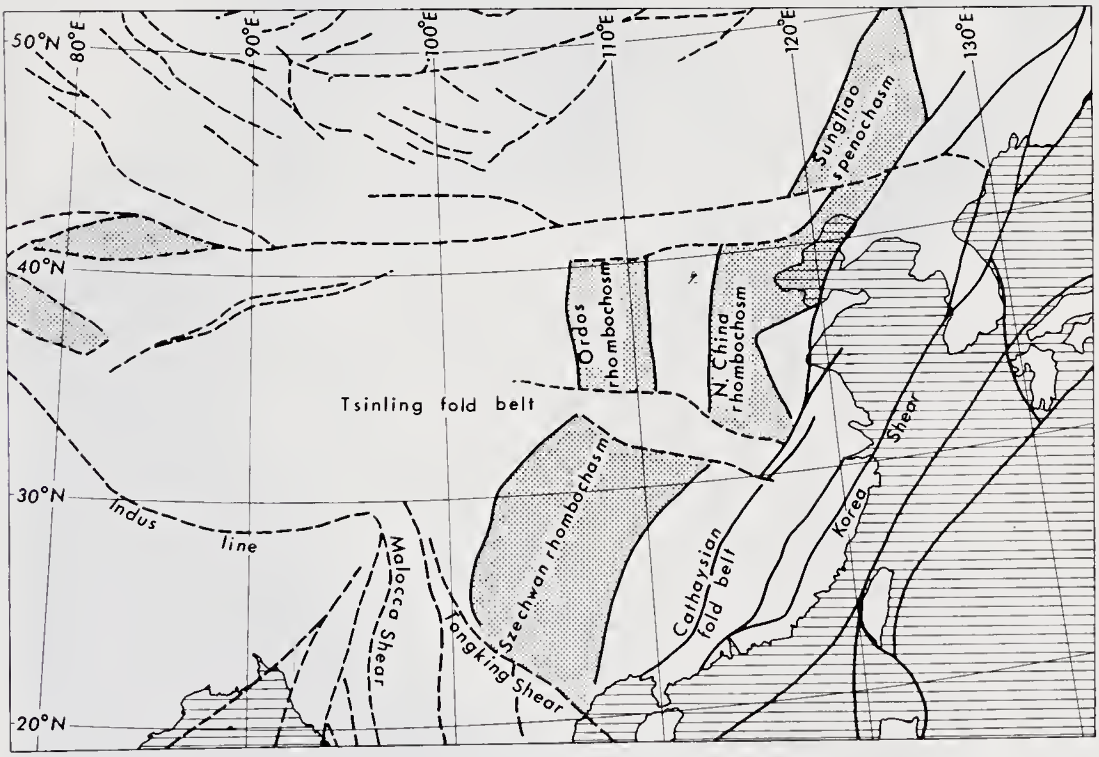


Fig. 177 Intermeshing in east Asia of sinistral Tethyan torsion (broken lines) with dextral peri-Pacific shear system (continuous lines). Stippled areas are disjunctive grabens.

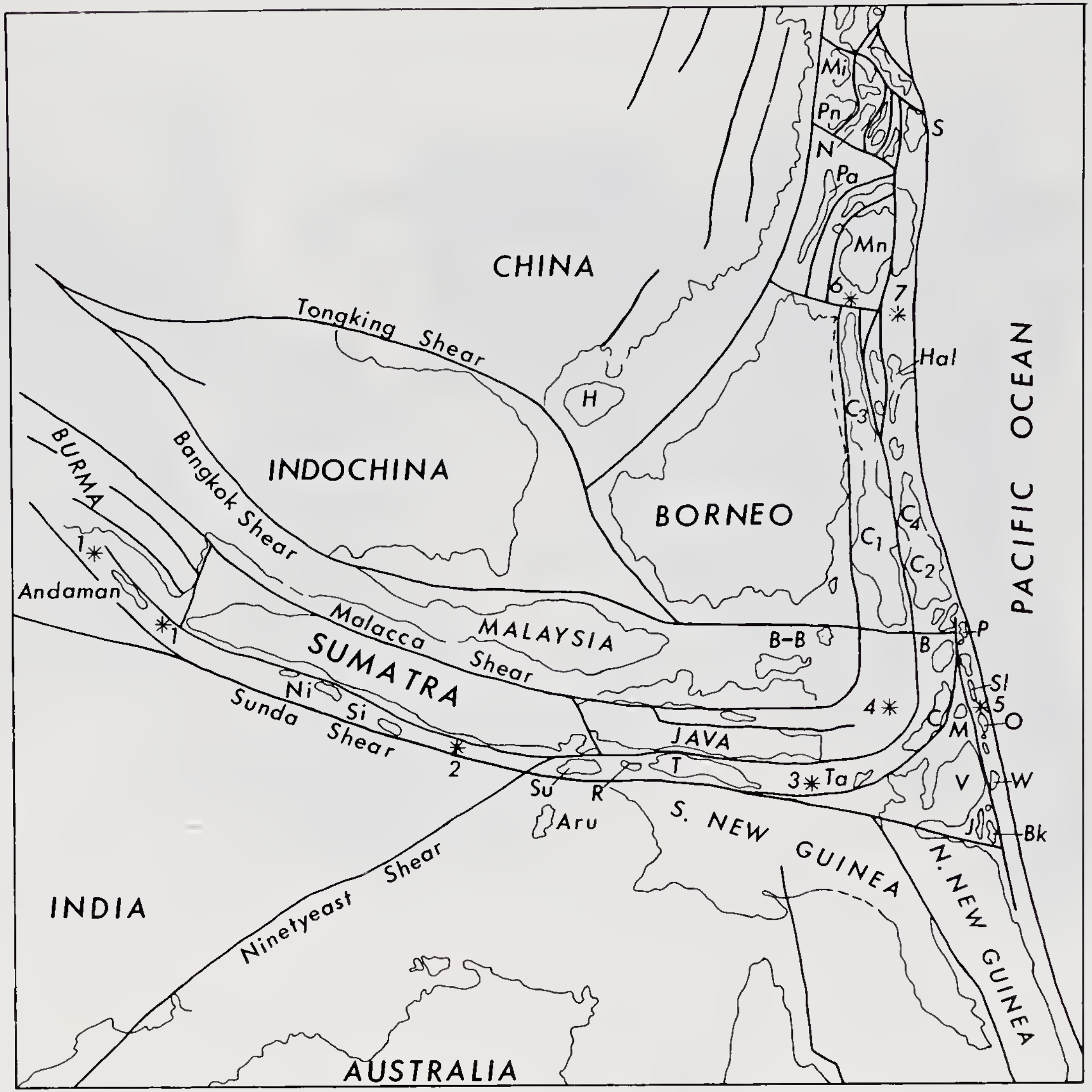

Philippines
Rhombochasm
N. New Guinea
S. New Guinea
Australia
Sorong Shear
Celebes
Rhombochasm
Sulu
Rhombochasm
Borneo
Rhombochasm
Borneo
Tongking Shear
Indochina
Siam
Sphenochasm
Megashear
Bongkok
Shear
Malacca
Sumatra
Sunda Shear

Fig. 178 (opposite)

South-east Asia : tectonics.

Fig. 179 (right)

Tectonic reconstruction of south-east Asia before disruption by Tethyan and peri-Pacific shears. Numbered asterisks indicate shortened orotaths:
1, Andaman-Nicobar; 2, Nicobar-Sumba; 3, Timor-Ceram; 4, Banda loop; 5, Sula spur; 6, Sangi-Kawaio; 7, Talair.
Key to smaller islands:
A, Aru; B, Buru; B-B, Bangka-Billiton; Bk, Biak; C, Ceram; J, Japen; L, Luzon; M, Misool; Mi, Mindoro; Mn, Mindanao; N, Negros; Ni, Nias; O, Obi; P, Peleng; Pa, Palawan; Pn, Panay; R, Roti; S, Samar; Su, Sumba; Sl, Sula; Si, Siberut; T, Timor; Ta, Tanimbar; V, Vogelkop; W, Waigeo.
This reconstruction is not a palaeogeographic map. All islands and coasts have been drawn in modern form for identification, in order to show the movement pattern during the evolution of the western Pacific margin.




182
India
Indochina
M
Sumatra
China
Borneo
H
K
Eo-Pacific
N N Guinea
S N Guinea
Australia


181
India
Indochina
Sumatra
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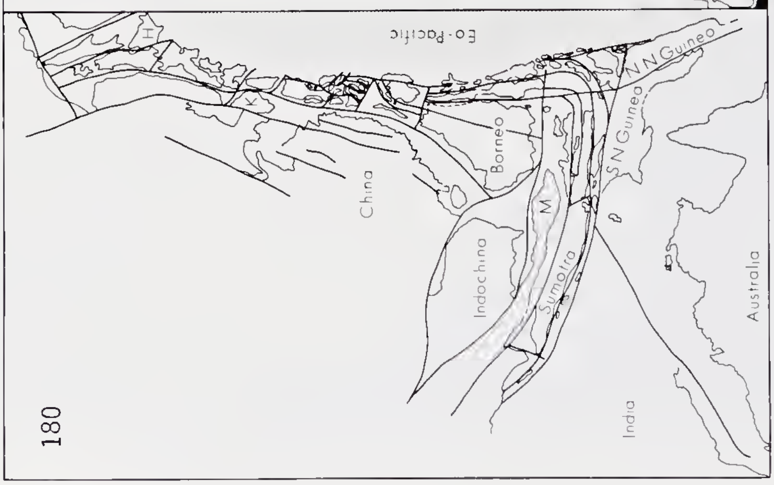
N N Guinea
N Guinea
Australia


180
India
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Eo-Pacific
N N Guinea
S N Guinea
Australia

Figs. 180 - 184 Kinematics of the disruption of South-east Asia.

palaeogeographic changes can be related genetically to the tectonic history of the whole region." This reasoning leaves Audley-Charles' palaeogeographic synthesis as a possible solution with respect to these data, but not necessarily the only one, nor the correct one. When his data are plotted on figure 179 instead of figure 178 (which is Audley-Charles' base) nothing is lost and many things are simplified. Indeed, Audley-Charles himself, in collaboration with Carter and Milsom (1972) subsequently abandoned altogether any vestige of spatial stability, and placed Sumba against the Knox coast of Antarctica 3500 km away from its Timor neighbour, and West Celebes also against Antarctica, even further from its Celebese twin! Only Timor, which has a special status for Audley-Charles, does not budge throughout the transformations.

TECTONIC ZONES OF SOUTH-EAST ASIA

It is instructive to study the successive zones as we traverse between the Indian Ocean and the northwest Pacific Ocean (Figs. 180 to 184):

(1) The first zone consists of three units: (a) the Indian and (b) the Australian continental blocks, and (c) the intervening floor of the Indian Ocean, which has been inserted between them to accommodate the Tethyan offset, the periPacific dextral offset, and the general dispersion.

(2) Between this relatively simple large-block zone and the first orogenic zone is the Sunda shear which forms the north boundary of the Indian Ocean polychasm, and the southern boundary of the Tethyan torsion zone. It suffers late inflection around the Banda loop and continues along the northern boundary of the Australian block against the New Guinea cordillera (compare Fig. 178 and 179). The Sunda shear enters New Guinea as the Tarer-Aiduna-North Panial fault zone of Visser and Hermes (1962, plate X). The Dauki shear between the Shillong plateau and the Rajmahal Hills of Assam (Evans, 1964), and the Sorong fault from Sula along the northern edge of the Vogelkop (Visser and Hermes, 1962), belong to this general Sunda shear zone.

(3) The Arakan-Sunda belt. This is a zone of young orogens where the three movements are expressed in oroclines and orotaths (Fig. 185). The stretched orogens characteristically form island boudins. This is predictable and inevitable, because the mode of elongation depends on the stress-difference and the temperature. When the shear stress is high and the isotherms low, elongation can only occur by fractures, yielding imbricate *schuppen*. Where the isotherms are higher, extension without fracture occurs, at a rate

depending on the temperature. Feed-back processes ensure that intermittent hot-spots develop along the belt, so that all orotaths present an island-chain boudin pattern. An outer lineament of this zone runs through the front ranges of Burma, the Andaman and Nicobar groups, Nias, Siberut, Sipura, Sumba, Savu, Roti, Timor, Tanimbar, Kei, Ceram, Buru, to southeast Celebes. A second lineament includes the Naga Hills, Arakan Yoma, Sumatra, Java, Bali, Lombok, Sumbawa, Flores, thence via the attenuating chain round the Banda Sea to southwest Celebes. This whole zone has doubled in length. The degree of stretching increases progressively towards the southeast as far as Sumba.

(4) The Malacca shear spearates the Arakan-Sunda zone from the Malaya-Indo-China zone. This is an important sinistral shear of the Tethyan torsion and hence may have minor significance along the Irrawaddy within Burma, where the dextral offset along the northerly trend would cancel the Tethyan sinistral movement. It has suffered the general regional inflection. Burton

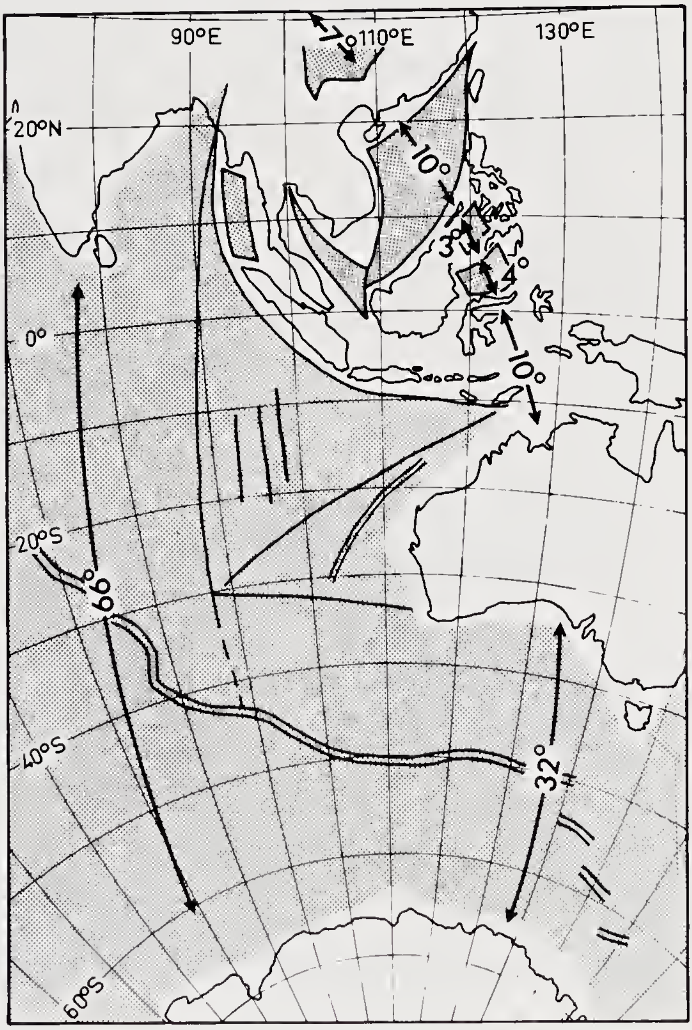

Fig. 185

Distributed extension between Antarctica through Australia to China equals the single separation between Antarctica and India. Adjustment occurred on the Ninetyeast shear and the Assam and Sunda oroclines. The respective separations between India, Antarctica, and Australia are all clean breaks (apart from the Heard-Kerguelen continental fragments). In contrast, the Mesozoic-Tertiary orogenic zone, which was more deformable at the relevant times, suffered distributed extension, with many small basins, bendings and stretchings. (Compare figure).

(1965) has described the Bok Bak fault extending more than 1,000 km through Malaya with a sinistral offset of some 50 km. This belongs to the Malacca shear zone. Proctor and Jones (1966) have suggested that rather than one such fault, "there is more likely to be a series of sub-parallel wrench faults extending over a very broad zone." In concurring in this Burton added that such sinistral dislocations prove to be even better developed in northwest Malaya than originally supposed and that other similar strike-slip faults probably occur in Kelantan and Thailand. Burton correctly attributes this faulting to the relative eastward drift of Australia as Gondwanaland disintegrated. The Khlong-Marui and Ranong faults of Garson and Mitchell (1970) and Ridd (1971) offset the Malay peninsula for some 200 km sinistrally near the narrow neck in Thailand. The great Z-shaped pattern of the orogenic belt formed by the Himalaya, Burma and the Sunda arcs expresses the coupled Assam and Sunda oroclines, the former clockwise or dextral and the latter anticlockwise or sinistral. Faulting associated with the former (the Ninetyeast megashear) is dextral, while the faulting associated with the latter is sinistral. The Khlong-Marui and Ranong faults fit this pattern. More such faults may be expected as mapping proceeds, but the position is complicated by the sinistral Tethyan shear system.
(5) The Malaya-IndoChina zone forms the axial trunk of the T where the Tethyan orogenic belt meets the Pacific marginal orogen. It has partaken in the regional oroclinal bending about the Assam pivot. The Siam sphenochasm accommodates part of the oroclinal bending. There is no significant elongation. It is the watershed between the Tethyan sinistral dragging of

Fig. 186 Yunnan-Malaya Lower Palaeozoic geosyncline axis (broken line). Crosses indicate deep facies (1, Paoshan; 2, Fang; 3, Langkawi Is.; 4, Mahang). Open circles indicate shelf facies (5, Lashio-Mandalay; 6, Taunggyi; 7, Na Suan; 8, Ban Na).

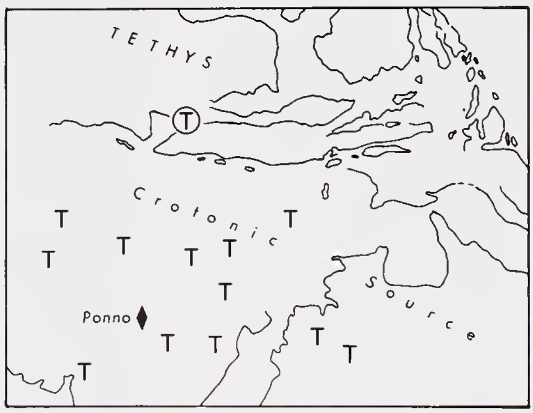

Fig. 187 Permo-Carboniferous tilloids (T). Phuket on Thai peninsula is ringed. Diamond indicates Panna diamond pipe. Golconda is off the bottom of this figure.

the Sunda archipelago and the periPacific dextral dragging of the archipelagoes of the western Pacific. This belt is the axial zone of the Yunnan-Malaya Middle Palaeozoic geosyncline described by Burton (1967, Fig. 1), which extends for 3000 km from Paoshan in Yunnan, through the Shan States of Burma, and along the spine of the Malay peninsula, and which yields a characteristic graptolite-*Tentaculites* fauna. Along this belt black shale facies occurred both in the Lower Silurian and the Lower to Middle Devonian, with a hiatus in between. At present, this geosyncline trends meridionally, but when the Bengal orocline is straightened, the Yunnan-Malaya geosyncline is the direct extension of the southern Tethys through Kashmir and Nepal, where similar Llandovery fauna and facies occur (see Fig. 187). It also extends eastwards into the Tasman geosyncline via West New Guinea, where *Monograptus turriculatus* occurs, and thence to Victoria (where Gill has described similar graptolites and *Tentaculites*) and Tasmania. This distribution from Kashmir to Tasmania becomes continuous direct, logical, and 30% shorter, on figure 179 than on the present distribution.

(6) The Tongking shear is a major dextral offset which increases its dis-

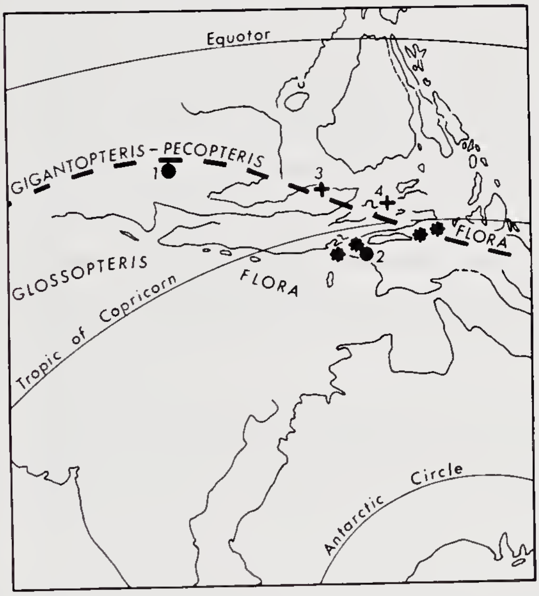

Fig. 188 Permian floras of South-east Asia. Broken line indicates boundary between *Glossopteris* and Gigantopteris floras. Filled circles indicate *Glossopteris* with admixture of northern elements: 1, Phetchabun; 2, Rijsterborgh River. Crosses indicate Pecopteris floras of Malaya 3 and Djambi 4. Asterisks indicate Permian coral faunas of Sumba-Tanimbar belt.

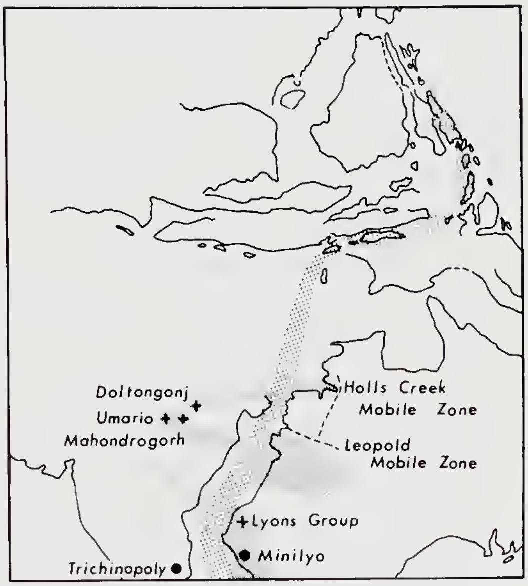

Fig. 189 West Australian geosyncline merges into the Timor-East Celebes geosyncline (light stipple), and branching rifts (denser stipple), and cratonic blocks run into the Indian and Australian shields. Crosses indicate the Artinskian faunas with *Calceolispongia*. Filled circles, the similar Maestrichtian faunas of Trichinopoly and the Minilya River.

placement progressively south-eastwards against the chain of rhombochasms which follows. Note how it chops off the southern end of Borneo, SW Celebes and SE Celebes.

(7) The Szechwan-Borneo-Arafura zone is a chain of rhombochasms which extend from China to Australia, and indeed to Antarctica (see Fig. 185). It includes the filled Szechwan rhombochasm which is 10 km deep, along with the Hoang Ho rhombochasms, the South China Sea, Sulu Sea, Celebes Sea, Molucca Passage, and Banda Sea, and a number of smaller grabens. Between the rhombochasms the blocks are mainly segments of pre-Tertiary platforms. The northwest-to-southeast elongation of this belt is some 3500 km, or over 300%. India has separated from Antarctica by 66°. Australia has separated from Antarctica by only 30°. The remaining 35° to balance the displacement is distributed in these rhombochasms. Whereas a single separation developed between India and Antarctica, the separation east of the Ninetyeast shear zone was distributed between the Australian, Moluccan, Celebes, Borneo, and southeast China blocks. This rhombochasm zone also permits the accommodation between the Assam and Sunda oroclines and the east-west belts which follow.

(8) The central China fold belt runs latitudinally, and includes the Kunlun, the Tsinling, Yu Hwai, Tapieh Shan, and Yangstze fold groups. This belt is part of the Palaeozoic Tethyan zone. It was not orogenic during the periPacific shearing, so responded by fractures instead of bending as in the zones 2-6 above.

(9) The Ordos-Shantung belt of rhombochasms elongates easterly in contrast with the Szechwan-Arafura rhombochasms which elongate southeasterly. The belt extends westwards to the Tarim basin.

(10) Cutting T-like athwart these Tethyan trends are the periPacific zones of dextral shears, disjunct, rhombochasms, with southsoutheast extension (Bering, Okhotsk, Sea of Japan, Philippine Sea, Caroline Sea, Bismark Sea, Coral Sea and Tasman Sea) which separate the *static* Pacific crust from the WNW-migrating continental masses of Australia and Asia. Framing these rhombochasms are the active arcs of Kamchatka, the Japanese islands, Formosa, the Philippines, to Halmahera, and the South Pacific groups from New Guinea to New Zealand. The movements are mainly Tertiary. The rhombochasms increase in width southwards. This zone has suffered more than any other zone intensive fragmentation, attenuation, boudinage, and dispersion. The gross movements are periPacific dextral shear, transverse extension normal to the Pacific margin increasing southwards, strong sinistral offset across the Tethyan torsion, and general dispersion. The longest fault zones are

dextral. The transverse faults such as Japan's *fossa magna* and the Nagasaki fault and the Philippines shears may be dextral or sinistral according to their relations to their associated rhombochasms. They all involve extension transverse to their strikes.

(11) Finally there are the currently active tensional trenches - all arcuate and bowed eastwards (or southwards), like a belt of glacial bergschrunds, or gaping rifts at the head of landslides, which mark the boundary between the static (Pacific) side and the separating zones to the west (Fig. 191).

INDIA AND MALAY PENINSULA

Burton (1970) reconstructed the Yunnan-Malaya geosyncline through the Malay Peninsula from its beginnings in the Cambrian to its cratonisation in the Triassic. He concluded that "it is evident that, with varying degrees of certainty, the following tectonic elements can be recognised in the Malay Peninsula from east to west: (1) eugeanticlinal ridge; (2) eugeosynclinal furrow; (3) miogeanticlinal ridge; (4) miogeosynclinal furrow; (5) shelf. This pattern evidently requires for completion a continent or craton to the west, and *not* to the east or northeast as postulated by the Dutch school. It appears that Gondwanaland fulfilled this role." With all this I agree. But Burton sought his cratonic land source in India, by assuming that the Bay of Bengal was a sphenochasm, and that the Arakan-Sumatra Sunda belt did not exist during the relevant time (Palaeozoic and Lower Mesozoic rocks do occur). Truly India was the cratonic source, but from the Tethys across the Indus line from the south as shown in figures 180 and 187. All of Burton's data are satisfied in this model. Burton also featured the locally inexplicable occurrence of diamonds in the Phuket tilloids of peninsular Thailand which require a western source, for which he nominated Golconda near Hyderabad. However Crawford (1973) pointed out that Golconda was not a diamond field but only a diamond market. The Panna diamond pipe lies on the northern edge of the Deccan craton before it plunges below the Indo-Gangetic plain. This, or perhaps a diatreme still further north (now covered), would be a logical source for the Phuket diamonds (see Fig. 187).

Ridd (1971) followed Burton's model and gave further evidence for a cratonic source of sediments to the west of the Malay peninsula in the Lower Palaeozoic (which would be *south* on the orientation shown in Figs. 186 and 187). He also compared a Permian tilloid in the Phuket Group in peninsular Thailand with the Talchir Boulder Beds (and by implication with those of the Eastern Himalaya, Sikkim, Simla, Kashmir and Hazara) which tectonically and palaeogeographically are entirely analogous with the Phuket Group (Fig. 188).

Ridd also recalled the *Glossopteris* flora found by Kon'no (1963) near Phetchabun, which had mixed elements of both the *Glossopteris* and *Gigantopteris* floras (see Fig. 189). A similar situation occurs on the Rijsterborgh River of West New Guinea (Visser and Hermes, 1962, p. 67), and at Hazro in Anatolia (Wagner, 1959 and 1962, p. 748). These localities occur on the Tethyan shore of Gondwanaland, where the southern temperate zone merged into the tropics. Plumstead (1965 and 1973) has pointed out that the *Glossopteris* flora and Gondwanaland continents are not co-extensive. The *Glossopteris* flora does not extend north of the Amazon, nor north of the Niger, normally regarded as integral parts of Gondwanaland, but does occur in parts of southern Asia normally regarded as Laurasia. I emphasised (Carey 1964) that the boundary of the *Glossopteris* flora was not the Tethys, as has commonly been supposed; the boundary was the humid tropic zone. On a smaller earth this runs up the Amazon and Niger Valleys (leaving Colombia, Venezuela and northwest Africa outside the *Glossopteris* flora) and crosses the Tethys briefly in south central Asia.

Paradoxically, McElhinny, Haile, and Crawford (1974) concluded that "palaeomagnetic results from Malaya show that the Malay Peninsula lay 15° N during the late Palaeozoic, a result incompatible with the hypothesis that it once formed a part of Gondwanaland adjacent to India or Australia". However, this contradiction lies not in the data given by McElhinny and his co-authors, but in their interpretation of them. Indeed, far from being contradictory, the palaeomagnetic results in question agree precisely with the reconstruction of this region which I had deduced solely on tectonic grounds, without use or even knowledge of the palaeomagnetic measurements to come (see pp. 211-213).

AUSTRALIA AND INDIA

As far back as 1938 I had made a reconstruction of the relation of Australia and India (Carey, 1938, fig. 40) from palaeogeographic disjuncts and unravelling the complex tectonic trails, which was identical with the present reconstruction of figure 179. Even in 1938 I had already concluded that the Moluccan loops were impressed bends, and had recognised the great Tethyan shear through this area: "The whole New Guinea region bears the stamp of a colossal shear system. In Papua, in the Mandate, and in the Dutch Territory, we find shear fault after shear fault, all tearing the north side *westwards*, fold after fold showing shearing deformation *westwards*. The stresses which are responsible for the great westerly displacement are of continental dimensions. They are probably related to the main architectural pattern of the globe" (Carey, 1938, p. 75).

Teichert (1939, p. 86) referring to a Cretaceous fauna of the Minilya district of the northwest basin of Western Australia commented that "this Maestrichtian fauna is of unique interest in Australia, owing to its very close relationships with that of the Ariyalur beds of the Trichinopoli District on the east coast of India." On the reconstruction of figures 179 and 190 the Minilya and Trichinopoli beds are directly apposed. In the breakup of Gondwanaland, the opening of the western Indian Ocean occurred early, with India, Antarctica, and Australia moving away from Africa as a nearly coherent block swinging about the Baluchistan orocline (Fig. 197). Although rifts occurred between India and Australia and Australia and Antarctica as early as the Upper Palaeozoic, and let the sea in from time to time, no great separation of these blocks occurred until the late Cretaceous. Hence, Trichinopoli and the Minilya River were facing shores of a Red-Sea-like gulf during the Maestrichtian. Teichert (*loc. cit.* p. 85) also refers to the *Buchia-Belemnopsis* fauna from Broome in N.W. Australia; "This fauna is very closely related to and partly identical with faunas of Oxfordian and Lower Kimeridgian age in the Timor - East Celebes geosyncline, especially on the island of Misool . . . It is also linked with the fauna of the Himalayan Spiti." The relation of these localities on figure 190 is clear. The West Australian trough was a branch of the Tethyan seaway which carried similar forms through to New Caledonia and New Zealand.

This same trough had late Palaeozoic forebears which let the sea into Mahandragarh, Daltonganj, and Umaria along smaller branch rifts in the Indian peninsula, where the fauna is identical (even to the evolutionary stage of that unusual crinoid *Calceolispongia* and to the ornamentation details of the gastropods) to the corresponding fauna in the Lyons Group immediately apposed to it across the narrow seaway. Thomas (1954) stated that all the Umaria forms "show very close resemblance to species in the Lyons Group, particularly with a fauna in beds 1500 feet from the top." Ahmad (1960) used this relationship to reconstruct in detail the Gondwana palaeogeography of the area, and more recently he (Ahmad, 1970) has reviewed in detail the facts relevant to these marine incursions. Crawford (1970) adopted the same relation between Australia and India and Antarctica as I had done (Fig. 190), and supported this with Precambrian geochronology, the Gondwana rifts transverse to the opposing coasts, carbonatites, and vulcanism, but he has also (1974) found close links between India and Antarctica, a dilemma which is discussed below.

Fig. 190

Relations of Australia, India, and Antarctica according to Crawford (1970): BK, Bundelkhand; CA, Carnarvon basin; CK, Cuddupah basin; E, Greenough block; FY, Fitzroy trough; G-G, Godavari trough; H, Halls Creek mobile zone; LL, Leucite-lamproites; LN, Leeuwin-Naturaliste block; M-M, Mahanadi trough; P, Pondicherry eruption; SB, Sinhbhum; SF, Albany-Frazer block. Open circles mark carbonatites.

THE WA-TIM-EC RIFT

Umbgrove (1938) established continuity of the Timor-East Celebes geosyncline, active throughout the Mesozoic, with a history distinct from that of the Sunda-Banda arc to West Celebes, and the Sunda craton behind. This S-shaped geosyncline included the Sula spur through Obi to the Vogelkop, the eastern arms of Celebes, Buru, Ceram, the Kei and Tanimbar groups, Timor, Roti, Savu, and Sumba. Australia and New Guinea were the source of its sediments. Teichert (1939) extended the geosyncline to include the coastal trough of Western Australia, and referred to it as the Western-Australian-Timor-East-Celebes geosyncline. In order to shorten this long name, in the following discussion I will contract this to the WA-TIM-EC rift or trough or geosyncline or orogen, as the context may require.

The WA-TIM-EC rift was the primary rupture between Australia and India in the dispersion of Gondwanaland. It is one of the radial rifts from the Gondwana dispersion centre from which radiated the rifts between Australia and India (which became the Wharton rhombochasm of Fig. 193), Australia and Antarctica (which became the Wilkes rhombochasm), Antarctica and South America (which became the South Pacific Ocean), South America and Africa (which became the South Atlantic Ocean), and Africa and India (which became the Lemurian chain of rhombochasms between Natal, Madagascar, and Seychelles plateau, the Laccadive-Maldive-Chagos plateau and the Western Ghats). This Gondwanaland dispersion centre is the southern analogue of the Alaskan centre from which the Arctic-Atlantic, Ob, and North Pacific sphenochasms radiate (Figs. 69 and 70). These twin dispersion centres have ancient genealogy. A transient marine connection via the WA-TIM-EC rift and the Australian-Antarctic (Bass Strait) rift may have let the *Calceolispongia*-bearing fauna with characteristic *Bryozoa*, not only into north Western Australia and the branch troughs to Umaria in central India, but perhaps also into Tasmania from the west (Banks, Hale and Yaxley, 1955). Teichert (1958, and elsewhere) has argued strongly for a marine trough, of long standing, forming the western boundary of the Western Australia craton. I agree that a rift zone persisted there between Australia and India, with recurrent marine incursions throughout the Phanerozoic. It was morphologically equivalent to the Uralian trough between Europe and Siberia but it was less developed - and more intermittent. Teichert *(loc. cit.)* states that the Tethys affinities of the faunas along the trough are very obvious, although there are noticeable indigenous elements.

Throughout most of the Mesozoic the WA-TIM-EC rift was relatively narrow, and the Ninetyeast ridge lay contiguous with Australia's northwest shelf. JOIDES deep-sea drillings (214, 216) on the Ninetyeast ridge show shallow water and estuarine sediments (including lignite) and vesicular and amygdaloidal basalts, which appear to be subaerial. These are quite consistent with their location in the rift valley stage at the southern end of the WA-TIM-EC zone. During much of the Mesozoic, sedimentation in the trough kept pace with deepening, so most of sediments are neritic or paralic, and sometimes euxinic and saline. Notwithstanding the prevailing shallow water, a thickness of more than 15 km of sediment was deposited (Veevers, 1969). During the Lower Jurassic, diapiric orogenesis caused an epoch of emergence, which recurred in the Palaeocene and again in the Middle Miocene, when diapiric nappes spread on to neighbouring neritic facies. But late in the Cretaceous and in the Palaeogene, the Wharton sphenochasm (Fig. 191) opened rapidly with very great attenuation of the Andaman-Nicobar, Nias, Siberut, Sipara, Sumba orotath, which forms the north-east (stretching) side of the sphenochasm. This movement was relieved on the Sunda megashears on each side of this orotath. The smaller triangle between the southwest arm of the Wharton sphenochasm, the Darling fault, and the Naturaliste nematath was due to the lagging of Australia in the general westward zonal motion of the crust. This extension is mostly latitudinal with meridional growth of the oceanic crust, which terminates in the Bunbury rift valley and the Darling scarp. Even the Kimberly block was separated from the Australian craton by the King Leopold and Halls Creek active zones. In the mobile zone, the Wharton rhombochasm extension is expressed in the "drag" of the Banda loop, the extension of the Sumba-Savu-Roti-Timor-Tanimbar-Ceram-Buru zone (these are island boudins of the stretched WA-TIM-EC orogen), and the Sunda shear between Timor and Misool.

The timing of the several movements involved in this separation of India and Australia, and the dispersion and contortion of the East Indies, will gradually crystallize as more constraints emerge. The evolutionary steps depicted on figures 180 to 184 are kinematic and do not pretend to be synchronous across each diagram. Some displacements, shown distributed across the four diagrams, may have been completed early, others late. Constraints will come from different sources:

(a) Palaeontological disjuncts and contrasts and sedimentational and tectonic histories as exposed in outcrops (e.g. Crawford, 1970, and Audley-Charles, 1972).

(b) Deep sea drilling giving the sedimentation history and crustal ages as, for example, reported by Heirtzler and his eleven co-authors (1973).

(c) Correlated linear magnetic anomalies, as for example the analysis by McKenzie and Sclater (1971).

(d) Palaeomagnetic latitudes and azimuths, as for example in the study by Irving.

The rotation of India (with Australia and Antarctica) away from Arabia and Africa on the Baluchistan orocline seems to have been largely Permo-Triassic. The main separation of Australia and India, by opening of the

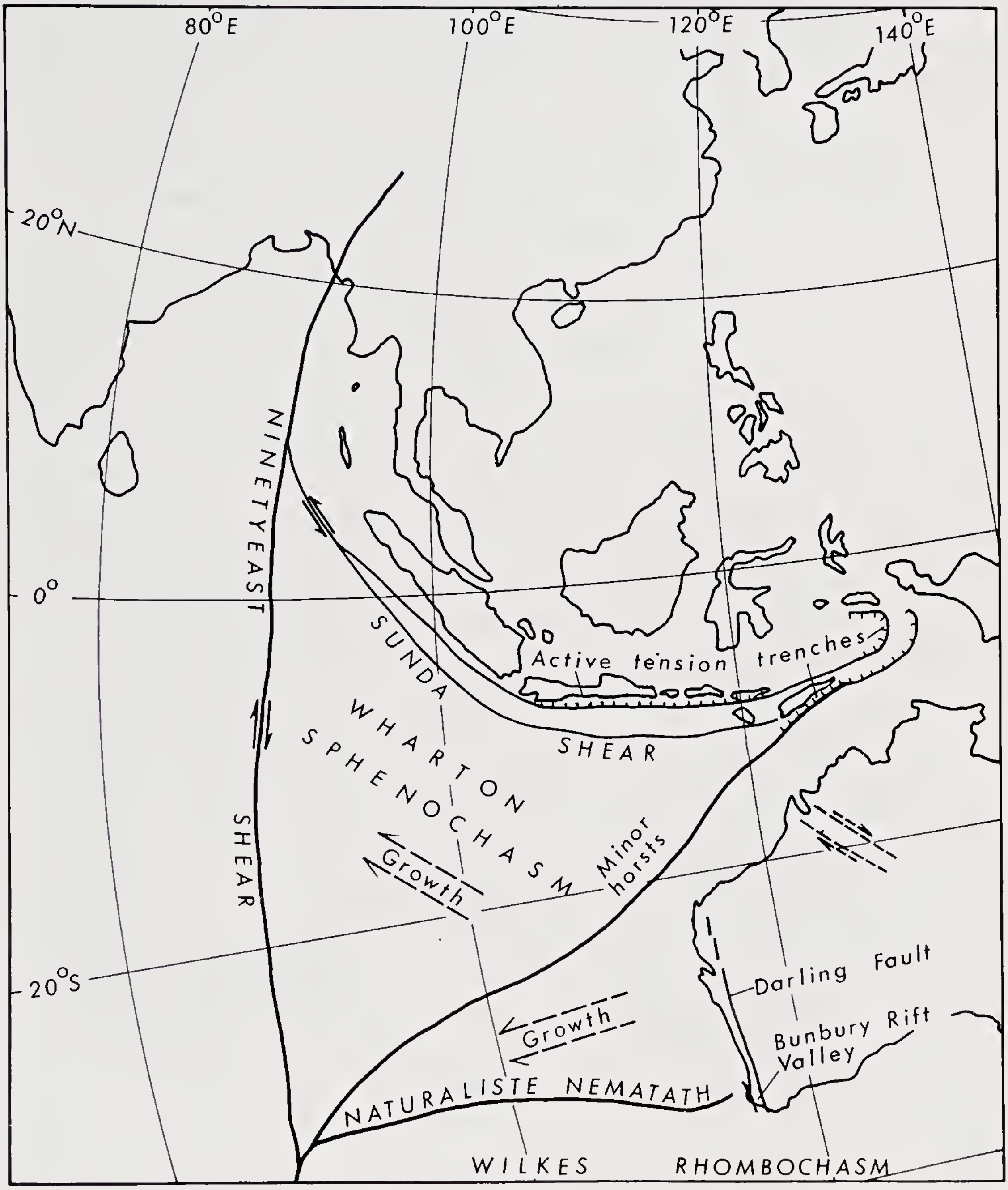

Fig. 191 Growth of eastern Indian Ocean between Ninetyeast Ridge and the Australian block.

Wharton sphenochasm and slip along the Sunda shears, seems to have suffered only restricted movement until the end of the Jurassic, but developed rapidly during the Cretaceous. The separation of Australia and Antarctica, which like the WA-T1M-EC, began as a rift zone as early as the Permian, did not open much until the Palaeogene, although substantial Permo-Triassic dextral slip along the Australia-Antarctica rift valley may explain in part the palaeomagnetic anomaly and "puzzling inconsistency" between the Tasmanian and Antarctic dolerites, which have been emphasized by McElhinny (1967), and McElhinny and Embleton (1974). Derivatives of this dextral shift are still preserved in the New Zealand Alpine Fault and in the dextral shift between Tasmania and Australia (see Fig. 193). The tectonic diapirisms along the WA-T1M-EC, which yield much more efficient land bridges between Asia and Australia, did not regurgitate until the Miocene.

WALLACE'S LINE

The WA-TIM-EC was an active trough throughout the Mesozoic, and led to one of the most distinctive faunal boundaries in the world - Wallace's Line (Fig. 192). Over a century ago the famous naturalist, A.R. Wallace (1864), wrote: "It is well known that the natural productions of Australia differ from those of Asia more than those from any of the four ancient quarters of the world differ from each other. Australia, in fact, stands alone: It possesses no apes or monkeys, no cats or tigers, wolves, bears or Hyenas;

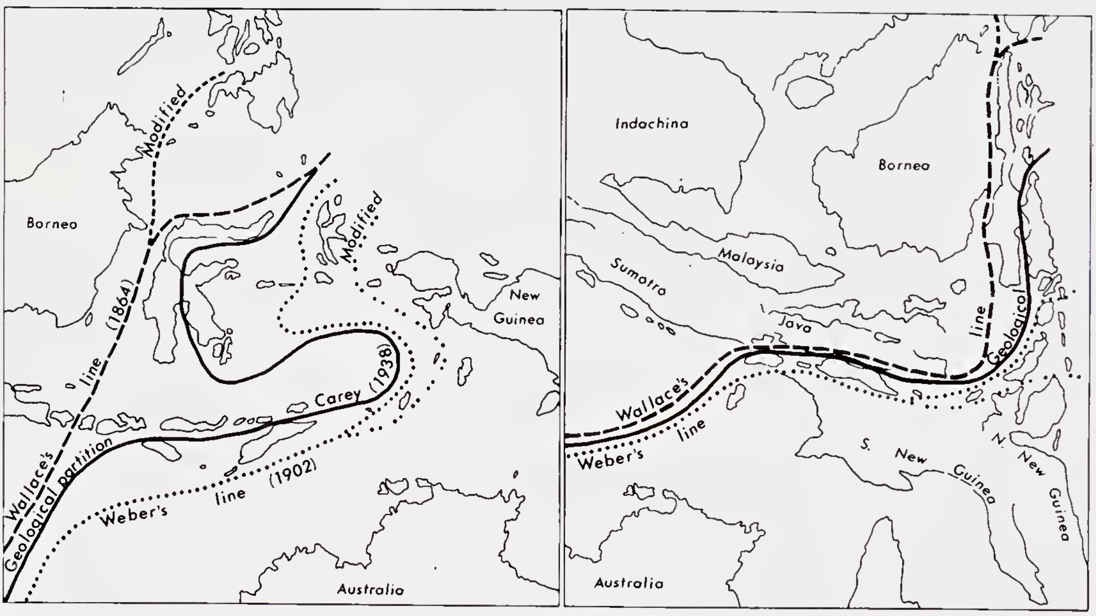

Fig. 192 Geological background of Wallace's Line.

no deer or antelopes, sheep or oxen; no elephant, horse, squirrel or rabbit; none, in short, of those familiar types of quadruped which are met with in every other part of the world. Instead of these it has marsupials only, kangaroos and opossums, wombats and the duck-billed platypus. In birds it is almost as peculiar. It has no woodpeckers and no pheasants, families which occur in every other part of the world; but instead it has the mound-building brush-turkey, the honey-suckers, the cockatoos, and the brush-tongued lories, which are found nowhere else in the globe. All these striking peculiarities are found also in those islands which form the Austro-Malayan division of the Archipelago."

Since this brilliant generalisation the relationships of the Indo-Malayan and the Austro-Malayan faunal provinces has been one of the most fascinating topics among biologists. The line of demarcation has been shifted to and fro according to the faunal criterion of the investigator. But the fundamental fact of the duality of the provinces has stood. Setchel (1929) summarised many of the more important viewpoints that have been expressed. The original line of Wallace passes through the Straits of Lombok, then northwards between Borneo and Celebes. "The inference that we must draw from these facts is undoubtedly that the whole of the islands eastwards beyond Java and Borneo, with the exception perhaps of Celebes, do essentially form a part of a former Australian or Pacific continent, although some of them may never have actually been joined to it." (Wallace, *loc. cit.*).

Max Weber showed the incompleteness of Wallace's reasoning when applied to other animals than those capable of flight, and suggested a line between Celebes and the Moluccas as being more fundamental. Pilseneer reviewed the evidence and suggested either the recognition of a region of transition, or the shifting of the line further east. Merrill and a goodly backing of biologists subsequently, also suggested a transition zone practically delineated by Wallace's line to the west and Weber's line to the east, a zone in which the Australian and Asiatic faunas intermingle. Although there is, no doubt, a zone of transition, the important truth remains that there is nevertheless a fundamental faunal duality in the Indies. The WA-TIM-EC trough (see, for example, Raven, 1935, and Brongosperma, 1936) was the real moat between these faunal provinces. The South America-Antarctic-Australia frontier faced the Asiatic faunal community across this moat until the regurgitation of the geosynclinal gut which commenced in the Palaeogene, culminated in the Neogene (with allochthonous sheets spilling from the north on to present-day Timor) and continues to this day, leaving two mutually alien faunas to battle for *lebensraum*. The marsupials had persisted on the Aus-

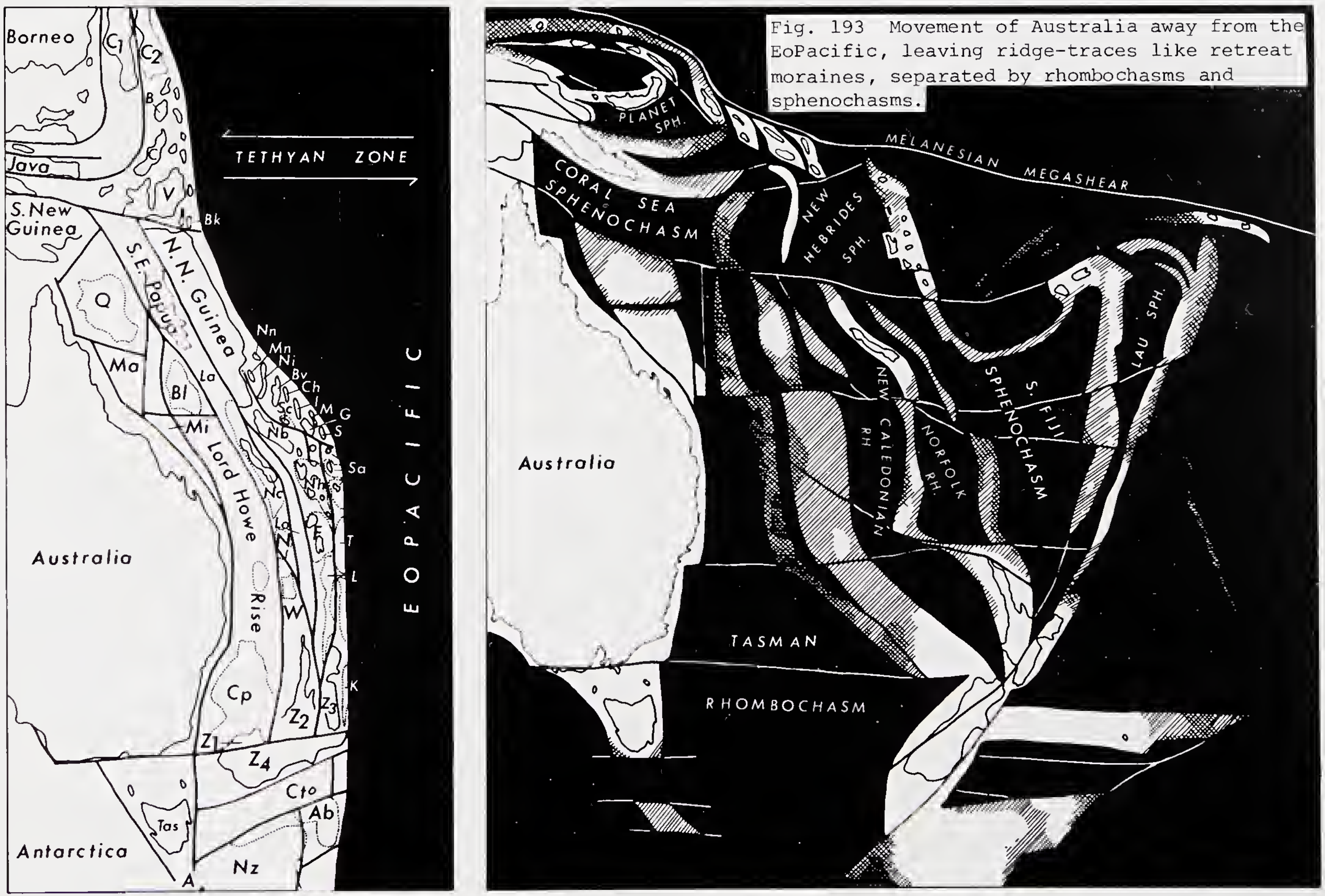

Fig. 193 Movement of Australia away from the EoPacific, leaving ridge-traces like retreat moraines, separated by rhombochasms and sphenochasms.

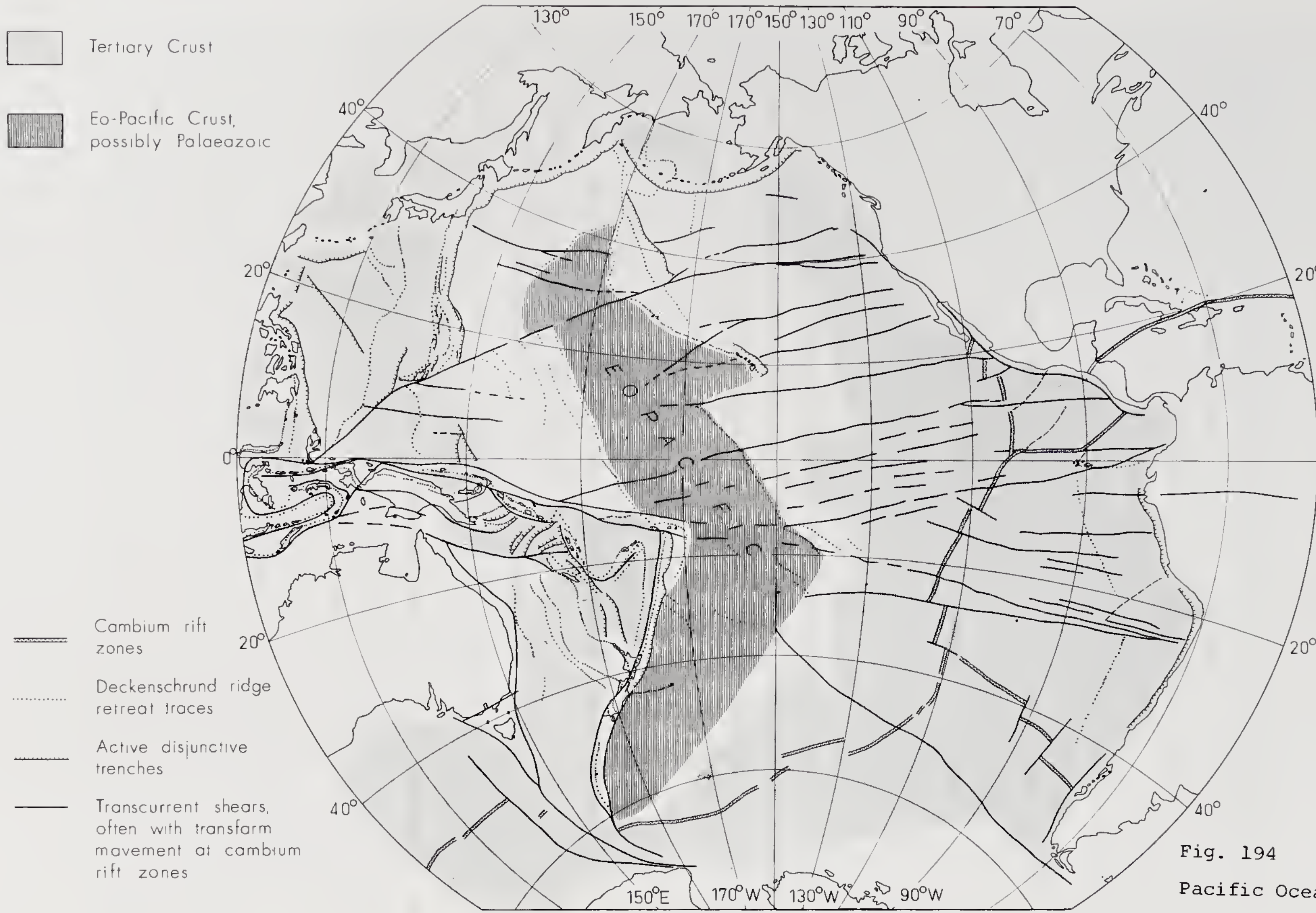

Fig. 194

Pacific Ocean.

tralian side, filling every ecological niche -- the large grazers (the macropods) to the smaller tree and ground herbivores, the insectivores, the large animals of prey - *Thylacoleone* (the marsupial lion), *Thylacinus* (the Tasmanian tiger), and the dasurids (the Tasmanian devil and the smaller marsupial "cats"). The equivalent marsupial faunas had long been extinguished on the Asian side when the rise of the carnivores led to the concurrent multiplication of fleet-footed herbivores among the ungulates and ruminants, who could co-exist with the carnivores. With the Miocene emergence of bridges from the pit of the geosyncline the battle-lines between these rival regiments were drawn. The Australian marsupial ground troops were no match for the Asian carnivores, for they colonised all the newly emergent lands and even established bridgeheads on formerly Australian territory. Hence Weber, a mammalogist, drew his boundary on the Australian margin of the WA-TIM-EC. But the Australian air force was superior, so Australian birds colonised all the new lands, and drove out their Asian rivals from West Celebes where they now face Borneo across the Macassar Strait, have captured the Lesser Sunda islands, and have advanced along the Sunda chain, through Flores, Sumbawa and Lomboc, and stand poised for the invasion of Bali. Hence Wallace, an ornithologist, drew the Asia-Australia faunal-province boundary entirely on the west side WA-TIM-EC with considerable migration into originally Asian territory.

Thus there is no universal Wallace's Line to separate the Asian and Australian faunal provinces, but rather a broad zone of transition, within which a sharp line could be drawn for each competing pair which would occupy the same ecological niche. If the Asian combatant of the pair is dominant, the line will fall entirely on the Australian side of the WA-TIM-EC and vice versa. By contrast there are some angiosperms, beetles, etc., which show an utter disregard for this boundary. These presumably depend on different modes of dispersal, e.g. prevailing winds or flotsam in prevailing currents.

Audley-Charles and Hooijer (1973) have described the Pleistocene commuting of ancestral pigmy elephants (stegodonts) between Timor and Flores, at present separated by the Savu Sea 3 km deep. They therefore suggested that corridor migration had occurred via Alor, Wetar, and Atauro islands. However at some stage in the Neogene, Timor must have been adjacent to Flores across the Sunda shear (Figs. 180-184).

INDIA-GONDWANA PARADOX

It is fair to conclude that the relation of India to Australia as shown in figures 179 and 192 rests on sound tectonic, palaeomagnetic, palaeogeographic, geochronological and palaeontological grounds. Markl (1974) reached the same conclusion from study of the magnetic lineations on the floor of the Wharton basin, and Veevers *et al.* (1971) from the Mesozoic coastal basins of Western Australia. Larson (1975) identified the late Jurassic magnetic lineations between northwest Australia and Lomboc, which "strongly suggest that some continental landmass lay north of Western Australia in the Jurassic period." But here is the paradox. India is also closely related to Antarctica (Du Toit, 1937; King, 1950a, 1953, 1958a, 1958b, 1963, 1964, 1965; Smith and Hallam, 1970; Dietz and Holden, 1970, 1971; Plumstead and McKenna, 1973; Smith *et al.* 1973; Crawford, 1973; Veevers Powell and Johnson, 1975), and also to Madagascar, and to Africa. It is topologically impossible to assemble a Pangaea on the present-sized earth, so that India fits appropriately against all these neighbours. Most assemblers have granted India its ties to Africa, and usually to Antarctica, leaving a wide gap between India and Australia. To make matters worse, Meyerhoff and Meyerhoff (1972, pp. 294-300) insist that India was never far from Iran nor from China or Russia! "India has been part of Asia since Proterozoic or earlier time. This is a geologic fact, which nothing can change" (*loc. cit.*, p. 297). Meyerhoff and Teichert (1971, p.4042) point out that "detailed mapping from the central Indian Shield to southeastern Afganistan and Iran [*Pakistan Geological Survey 1949; Krishnan 1960; Gee 1965; Ganss 1968; Stöcklin 1968; Sastry and Shah 1969*] shows that several distinctive rock formations and faunal zones extend from central Iran and Afganistan on to the Indian Shield (Madhya Pradesh). These stratigraphic units and paleontological markers, which are continuous, include the late Proterozoic-Cambrian Hormuz Salt Series, two of the *Productus*-bearing beds, and the Aptian-Albian *Orbitolina* zone and associated warm-water shoal carbonates." Crawford (1974) insists that the Tibet and the Tarim basin and parts of northern China must be regarded as parts of Gondwanaland, and he produced a reconstruction with India wrapping round Antarctica, and with Tibet, the Tarim, and North China contiguous with it across the Himalaya and in contact with southwest and west Australia.

The dilemma is to reunite India with all her kin in a single integration. A yawning gore must break at least one of the essential bonds. For example, King (1973, p. 855) after a critical review of the disjuncts found it necessary to accept the India-Australia relation advocated by Carey (1958a), Ahmad (1961), Krishnan (1969), and Crawford (1970), but then met the inevitable dilemma. In closing the unacceptable gap between India and northwest Australia, he opened a new gap between India and Africa. King wrote: "If Ahmad's placing of India is correct, the western side of the peninsula can no longer be apposed to Somaliland and Arabia in the reconstruction; and the readjustments required by the evidence from Madagascar and from India inevitably leave a great gap in the outline of Gondwanaland. A new element is needed to fill this gap. That element, we suggest, is the block of Iran-Afghanistan." Dietz and Holden (1971) met the problem by leaving a hole, either externally (their *Sinus australis*), or internally (their *Mare gondwanensis*); each of which caused improbable disjuncts. But these gaps, like King's gap which he proposed to fill with Persia, and each of the other gaps of the many other authors, are only artifacts - the inevitable creature of assembling on too large an earth. But *all* these close connections emerge automatically when Pangaea is assembled on a terrella of the appropriate radius. At the 1956 symposium in Hobart on continental crift, I presented the best Pangaea assembly I could make on an earth of present size, which is reproduced here as figure 14. Although this has frequently been cited since as the Carey reconstruction, it was submitted to the symposium not as a viable solution, but to establish the inevitable unwanted gaping gore which always appeared when reconstruction was attempted on a present-sized earth.

Reduce the radius, and the open gore closes, India can wrap (as it should) around north west Australia; direct contact is established between the east Asian and east Australian faunas (e.g. the early Cambrian archaeocyathids, the Cambrian trilobites of the *Redlichia* fauna, the Ordovician cephalopods and Calymenid trilobites, the Llandovery corals, the Devonian brachiopods, the Carboniferous foraminifera and the Permocarboniferous blastoids); the *Glossopteris* and Cathaysian floras mingled in New Guinea, Sumatra, Thailand, China and Turkey. The late Triassic *Monotis* pelecypods firmly bind the Tasman and Cathaysian provinces through the common abundance of the *M. typica*, *M. ochotica*, and *M. zabaikalica* groups, and by the absence of the *M. subcucularis* group. The Triassic amphibians and reptiles spread rapidly throughout Pangaea on both sides of the narrow Tethys. On this last integration I quote Cosgriff 1974: "One further implication of the study of Early Triassic vertebrate faunas should be noted at this point. This is that any world-wide reconstruction of Early Triassic geography should include land connection between all the major continental areas and also such insular areas as Greenland and Spitzbergen. In other words, it does not seem possible that the southern or Gondwanaland continents could have formed a block that was completely isolated from the northern or Laurasian areas. The temnospondyl-dominated faunas of this time interval from all parts of the world are taxonomically close to each other, and there are no elements in faunal composition that serve to distinguish the northern from the southern faunas. Brachyopids are found in North America and Spitzbergen as well as in South Africa, India, and Australia. Capitosaurids occur in North America and Europe and also in South Africa and Australia. Rhytidosteids are found in Spitzbergen, South Africa, and Australia. Trematosaurids were particularly wide in distribution; their range included North America, Greenland, Spitzbergen, India, South Africa, Madagascar, and Australia. The world possessed a single cosmopolitan temnospondyl fauna during the Early Triassic and this, in turn, implies that overland migration routes linked most of the principal continental areas."

India, notwithstanding its patent Gondwanaland vestments, was never far from Asia. Even the great latitude migration does not mean physical migration of India towards Asia. It simply means that more new ocean floor was inserted between the South Pole and India than between the North Pole and India. With the greater expansion of the southern hemisphere than the northern, the parallels of latitude migrated southward across India, firmly anchored to its own underlying mantle (Fig. 105). Even the equator swept across India. The migration of the latitudes across Australia was only half that of India because in the case of India, the whole of the new crust was inserted between Antarctica and India, whereas just as much new crust was inserted between Australia and Asia as between Australia and Antarctica (Fig. 185).

THE RIFT OCEANS

My main task at the 1956 symposium in Hobart on continental drift, was to establish that the Arctic, Atlantic and Indian Oceans were post-Palaeozoic rifts (Figs. 195-197). This proposition is now very widely accepted as fact. An enormous amount of detailed work has since been done, principally on the structure of the ocean floors, but also on the matching of disjuncts on the widely separated blocks. Whereas there are differences, such as the former position of Madagascar, or the amount of movement on the Nares channel between Greenland and Ellesmereland, nevertheless these syntheses are grossly similar and broadly similar to those presented by me to the Hobart symposium.

I do not propose to sift and weigh all these data, for this would add considerably to the length of this book, without contributing anything significant to the essential thesis of gross expansion of the earth. The most recent and most comprehensive synthesis of these data is the monograph by Owen (1976), who gives detailed maps and references. This admirable compilation will serve until it is superseded, perhaps by the same author, as more data appear. Meanwhile some overall relationships are worth noting.

The "pivots" of the Alaskan and Baluchistan oroclines have integrating functions in the general dispersion. For example, in figure 195, if North America is rotated on the Alaskan orocline, the Arctic disjunctive zone is largely closed. This is oversimplified but it expresses a gross truth. Likewise, in figure 196, if North America is rotated about the Alaskan oro-

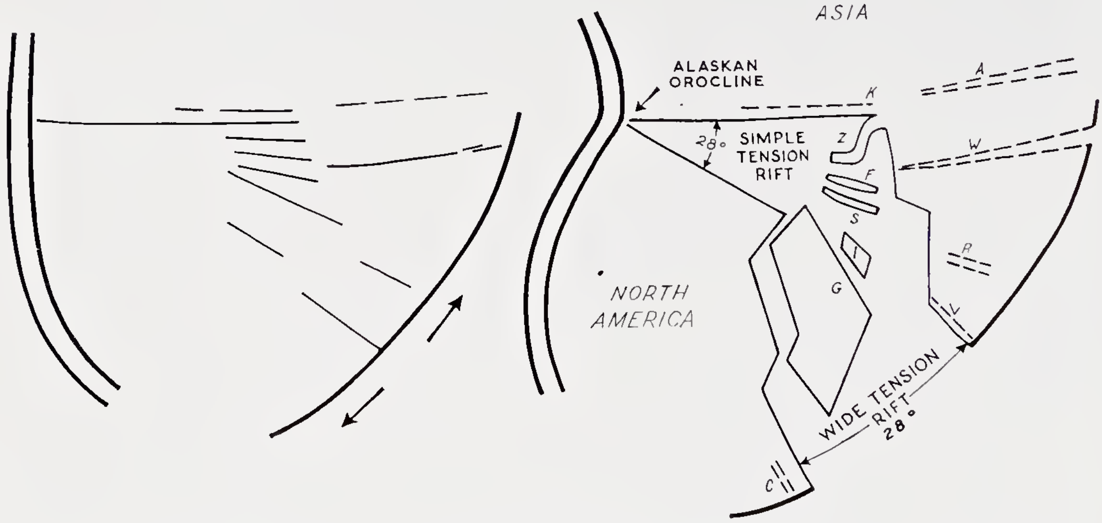

Fig. 195 Fracture pattern of North Atlantic, simplified by omission of transcurrent movement (from Carey, 1958a).

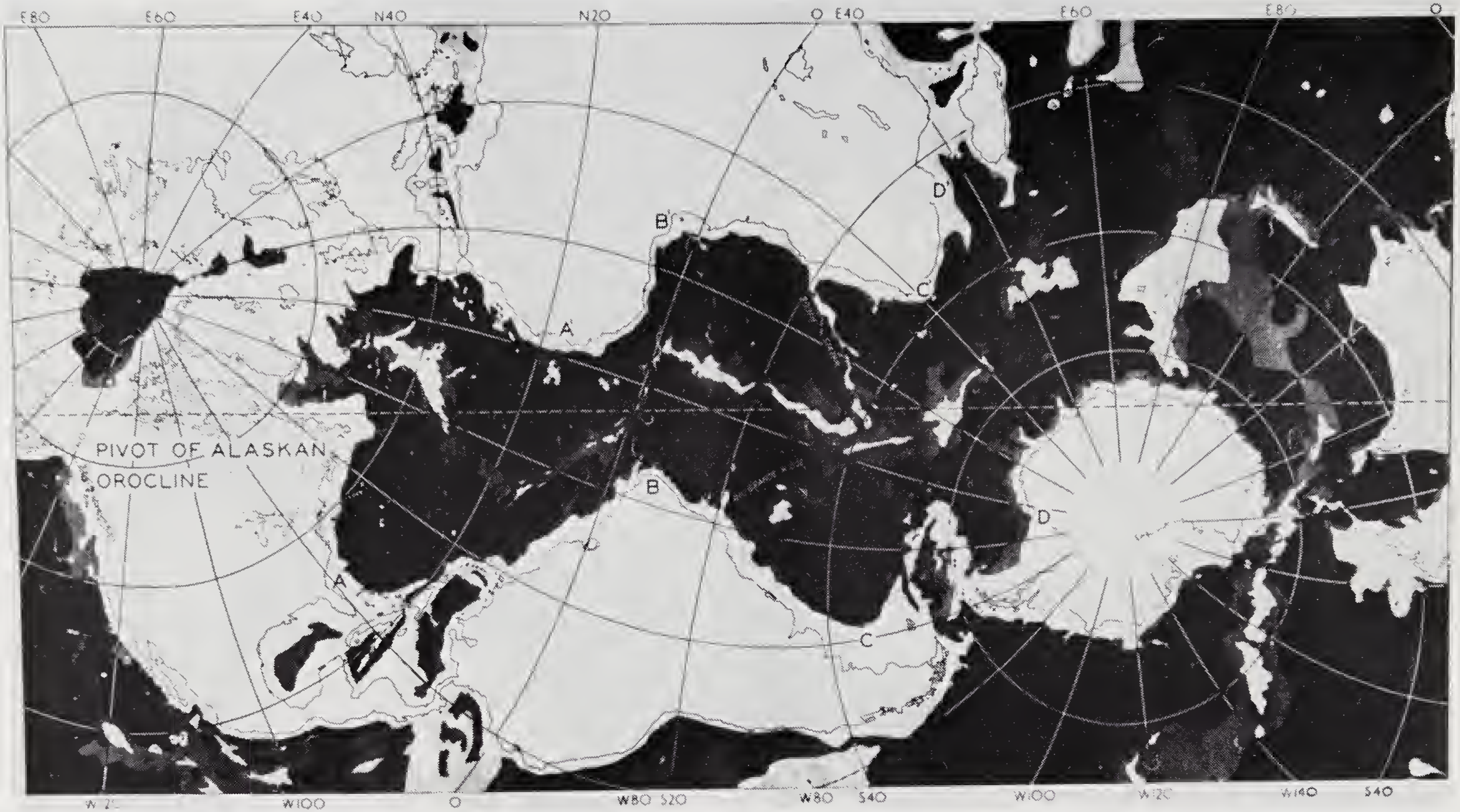

Fig. 196

Relation of Alaskan orocline to opening of Atlantic Ocean (from Carey, 1958a).

cline, Newfoundland converges on Ireland, and Iberia, the Bahamas-Newfoundland concavity converges on the convexity of northwest Africa (AA'), the Brazilian salient converges on the Gulf of Guinea (BB'), the Argentine-Falkland convexity converges on the Cape of Good Hope (CC'), Antarctica moves back towards its Pangaean starting point (DD'), and even Australia and the continental fragments (Madagascar, Heard plateau, etc.) all move back towards their Pangaean sources.

Similarly, in figure 196, the same kind of operation about the Baluchistan oroclinal pivot moves India back towards east Africa, Australia back towards Antarctica and all the continental fragments back towards their source.

Provided you allow for the greater dispersion of southern blocks (and the related westerly lagging of northern blocks), a marked north-south symmetry emerges.

Imagine large dispersion about the Alaskan pivot, so that the orocline becomes an oroclinotath, and you find its image in the Scotia oroclinotath. From the Alaskan pivot there are three main radial sphenochasms. The North Pacific (Fig. 170), the North Atlantic, and the Arctic-Ob which only goes as far as the equator. Similarly from the Scotia arc there is the South Pacific, the South Atlantic, and the Indian Ocean which only goes as far as the equator, but lines up with the Ob sphenochasm from which it is separated by the Tethyan zone. Thus the Arctic-Ob is the homologue of the Indian Ocean. All northern dispersions are more subdued than the southern ones, and generally offset westwards.

"Conventional" reconstructions of Gondwanaland place Antarctica in the Great Australian Bight, with Antarctica separating Australia from Africa. There are variations of this according as Madagascar is placed against Mozambique (Argand, 1924) or Tanzania (Du Toit, 1937; Schopf, 1970; Smith and Hallam, 1970; Veevers *et al.*, 1971; Frakes and Crowell, 1971; Owen, 1976), or as the re-entrant angle of the Bay of Bengal wraps around Enderbyland of Antarctica (Du Toit 1937, Schopf 1970, Smith and Hallam 1970, Frakes and Crowell 1971, Ford 1972, Owen 1976) or around northwestern Australia (Carey 1938, Ahmad 1952, Creer 1967a, Veevers *et al.*, 1971). Good geological arguments have been advanced for each of these connections, and at some time or other over the decades, I have been tempted to believe each of the presentations. The differences are largely due to the attempts to assemble a Mesozoic Gondwanaland on an earth of present radius, and also to the failure to recognise that the large latitude migration of India was due more to parallels sweeping across a static India than to migration of India towards Asia (Fig. 105). I think the northerly position of both Madagascar and India will prevail.

A quite different reconstruction of Gondwanaland, with the transantarctic mountains coast of East Antarctica against the Pacific coast of Australia, was first presented by me in an address to the Royal Society of Tasmania (Carey, 1945) and was adopted by Ahmad in his thesis investigation

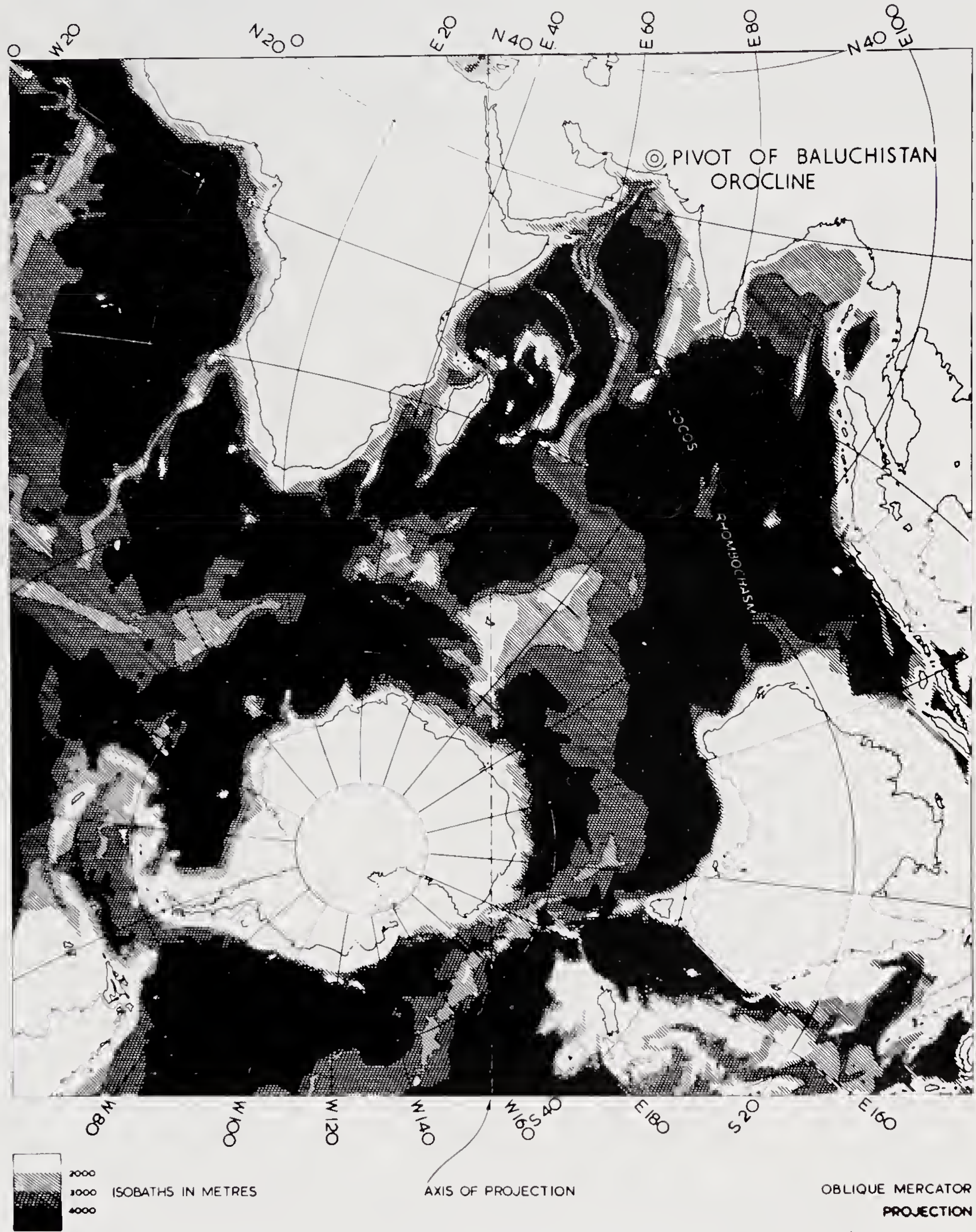

Fig. 197 The Indian Ocean sphenochasm (from Carey, 1958a).

under my supervision (Ahmad, 1952) and in later papers (e.g. Ahmad, 1968). Subsequently Creer (1967a) pointed out that this reconstruction satisfied the palaeomagnetic data more satisfactorily than the conventional Antarctica-core reconstruction. Although the magnetic growth lines (if they are correctly interpreted) have favoured the conventional position of Antarctica (unfortunately they have been interpreted with this reconstruction as presumption) nevertheless the alternate reconstruction eases evolution problems of the South Pacific, and of the east Asian Tethyan belt, and can be reconciled with kinematic homogeneity. Accordingly I am not yet prepared to reject this alternative as wholly obsolete.

TESTS

At least two crucial tests of the reality of Earth expansion already stand. The convergence of the continents on the expanding Arctic, and the convergence of the continents on an expanding Pacific, are only possible on a grossly expanding globe.

In addition, new objective tests are imminent to determine the expansion of the earth.

(1) Two corner-cube reflectors have been placed on the moon. Three optical observatories at Canberra, Honululu, and Tokyo have telescopes capable of receiving reflected laser light from a lunar corner-cube, which will give the distance from each telescope to the corner-cube. Repetition after a few hours will solve the spherical triangle joining the three observatories with an accuracy of a few centimetres. According to the "plate tectonics" hypotheses these three observatories are approaching each other at a rate of several centimetres per year. According to the expanding earth model they are separating at a few centimetres per year. Remeasurement after a few years would establish the truth.

(2) Very long based interferometry (VLB1) using radio sources at enormous distances, which hence have negligible proper motions, is emerging as a tool capable of measuring intercontinental movements. Currently Clark and his co-authors (1974) report attainment of precise geodetic results at the twenty centimetres level. By the end of the seventies they hope to measure base-lines between Alaska, California, Hawaii and Japan to within five centimetres, by simultaneous observation of extra-galactic sources. Existing large dish antennas at Goldstone (U.S.A.), Madrid (Spain) and Parkes (Australia) may be combined with nine-metre mobile antennas in many places are contemplated. The technique has only become possible since atomic clocks enable the transfer of time across the globe with accuracy to the nanosecond level. Critical discrimation between plate hypothesis and earth expansion would be expected after a couple of years of observation.

(3) Particle physicists with access to the high-energy Brookhaven accelerator have contemplated sending a pulsed beam of neutrinos right through the earth from Chicago to Cocos Island in the Indian Ocean, to measure the length of this chord with an accuracy of a few centimetres. This again would, after a few years of repetition, prove whether the earth is expanding or not. Neutrinos are the most elusive of all sub-atomic particles. They have no rest-mass and no charge, and so do not respond to magnetic

fields. They can go right through an atom and even a nucleus with rarely any interaction, and so the bulk of them will pass right through the earth. To contemplate focussing them into a pulsed beam is to ask for the near-impossible, but a statistical approach to this seems practicable.

(4) Dr. Sukuma, a Japanese geophysicist, reported to the 15th IUGG in Moscow in August 1971 that he had attained an accuracy of three microgals in the measurement of absolute gravity. Now consider an earth expanding by 40% in 3×10^8 years:

$$\frac{1}{r} \cdot \frac{dr}{dt} = \frac{0.4}{3} \cdot 10^{-8}/\text{year} \quad \text{and} \quad g = \frac{MG}{r^2} = \frac{4}{3}\pi r \rho G \simeq 10^9 \mu\text{gals}$$

At constant mass:

$$\frac{dg}{dt} = \frac{dg}{dr} \cdot \frac{dr}{dt} = \frac{-2MG}{r^3} \cdot r \frac{0.4}{3} \cdot 10^{-8} = -2g.0\cdot 13.10^{-8} \simeq -2.6 \ \mu\text{gals/year}$$

At constant density:

$$\frac{dg}{dt} = \frac{dg}{dr} \cdot \frac{dr}{dt} = \frac{4}{3} \pi \rho G r. \frac{0.4}{3} .10^{-8} = g.\frac{0.4}{3}.10^{-8} \simeq + 1.3 \mu\text{gals/year}$$

Although these terrellae are simple it seems that accuracy of measurement of absolute gravity now in sight would detect earth expansion after a few years, and would discriminate between expansion at constant mass or at increasing volume *and mass*. However if mass were increasing (as I suspect it may be), and if at the same time the volume were increasing because of phase changes caused by increasing temperature gradient (as seems to be so), combination could occur where the surface gravity increased *or* decreased *or remained constant*.

Continued measurements of the orbital period of Mercury by Shapiro and his co-authors will reduce the upper limit of the current rate of change of G.

(5) Other geophysical tests such as length of palaeomagnetic arcs on a single block, and the counting of fossilised daily, monthly, annual rhythms on invertebrate skeletons involve unsound assumptions, and in any case seem unlikely to attain the necessary precision to yield a definite answer.

CAUSE OF EXPANSION

PHILOSOPHY

An eminent geophysicist recently said that geology is at a stage of development comparable with that of physics before Newton. Discoveries yet to be made in geology will be as fundamental and as sweeping as those of the last three centuries of physics. With this I agree, but hasten to add that fundamental discoveries yet to come in physics will also dwarf modern physics. Whatever our field of science, we have scarcely begun to scratch the surface. Progress stops whenever we smugly believe that the fundamentals are known and that future work will only crystallise details.

Consideration of possible causes of expansion leads back to the womb of primary postulates of science and cosmogony. We must contemplate three concepts of mass: inertial mass, passive gravitational mass, and active gravitational mass, which are not necessarily identical and interchangeable. When we speak of expansion, we must discriminate between the expansion of space, and the increase of distance between reference points in space.

Universal time, sidereal time, and ephemeris time are merely alternative empirical astronomical units of time, but there is a more fundamental difference between "clock time", based on any of these time-keepers, and "atomic time", based on the spectral frequencies of atomic vibrations. These two measures of time are equated now by definition, but they are independent. Atomic time may have pulsed more slowly than clock time in the past, and may run progressively more rapidly in the future. An Archaean age of say 3.6 billion years depends on atomic time for the numerical part, and on astronomical time for its unit; the relationship of the two may well have changed secularly over the time span involved. Dirac (1974) reaches the same conclusions from his large-numbers hypothesis, which implies "two space-time metrics; one, which is unmeasurable directly, enters into the Einstein equations (which remain valid), and the other is what is actually measured in laboratory experiments involving atomic apparatus" (see Davies, 1974).

Does time have a beginning? Is zero time, T_O, real, or a limitation of our minds which cannot encompass infinities? If T_O is real, is there plus-minus symmetry across T_O?

Even creation is dichotomous. McRea (1964) has said that in evolutionary cosmology, creation is something that has happened, once and for all, and that it is not itself subject to scientific discussion, but that it determines the conditions for the discussion. In steady-state cosmology creation operates on matter continually, and is hence at the foundation of current scientific analysis, but at the same time continuous creation removes the need for initial conditions for the discussion. These two creation concepts are quite different.

In considering expansion we reach the roots of philosophy, and of our faith in universal order. Thus Eddington noted remarkable numerical coincidences between the relations of fundamental constants, which group to give pure numbers of the order of unity, 10^{39}, or 10^{78}, with nothing much between. The observable universe contains about 10^{78} particles; the age of the universe is about 10^{39} atomic time units (i.e. the time for light to traverse an atom);. the ratio of the electrical to the gravitational attraction of an electron proton pair is about 10^{39}; and so on. The coincidence of such very large numbers led Dirac (1937, 1938, 1974) to philosophize that such should be significant. By equating the products within these widely separated groups, it was apparent that the gravitation "constant" G should vary inversely with the age of the universe. Hence the universe and everything gravity-bound within it, including the earth, should expand with time.

Jordan (1966) has stated another philosophical constraint, that of mirror interdependence, namely that if G is a function of another parameter, then that parameter varies with G, and so on for all other interdependencies.

WHAT CAUSES THE EARTH TO EXPAND?

My first answer is I do not know. Empirically I am satisfied that the earth is expanding.

* * *

My second answer is that I may not necessarily be expected to know. The answer could only be expected to be known if all relevant fundamental physics is already known. For example, in the controversy between Kelvin, master physicist of his generation, who limited the age of the earth to some 100 million years, and Geikie, the geologist, who knew empirically that much longer time was needed, most geologists wilted before the physicists' heat. If Geikie, whom we now know to have been right, had been required to guess at the real cause, armed with the full knowledge of physics of his time, he would almost certainly have failed to predict radioactivity and the transmutation of elements. Had he done so, he would not have been believed. Again, most now admit that Wegener's (1924) reconstruction of the gross dispersion of the continents from his Pangaea to their present configuration was essentially correct. But the best physical explanation he could offer was demonstrably deficient, with the result that his Pangaean baby was thrown out with his spurious bathwater. It would be nice to know with certainty the cause of expansion, but the lack of this knowledge must not in itself invalidate expansion.

* * *

As third answer I state the boundary conditions suggested by my empiricism. These may exclude some suggested solutions, and hopefully point the way to the right solution:

(i) The expansion has continued throughout the whole history of the earth.

(ii) The rate has accelerated with time, perhaps exponentially. This does not necessarily exclude a constant cause, as the acceleration could result from feedback, as in the ascent of a salt dome.

(iii) The surface expression has pulsed with time with a modulation of the order of a couple of hundred million years, though this pulse rate may not have been constant. This does not exclude a constant cause, as feedback mechanisms within the earth might modulate a steady cause.

(iv) The surface expression is asymmetric with respect to axis, hemisphere, and octant. This may also be due to feedback.

(v) The surface temperature of the earth has allowed water at the surface at least for the last three aeons, and some glaciation for at least two aeons.

(vi) Variation of gravity acceleration g at the surface has not been recognised, although no critical test has yet been proposed. Wide variation of g seems improbable, but a value as low as that now pertaining on the surface of Mars, or even less, or similar variation in the other direction, is not excluded by any fact yet stated. A lower limit for three aeons is set by the fact that the earth has retained its atmosphere since that time.

(vii) Limits are set by evidence of past rate of rotation of the earth. However a uniform terrella, which differentiates with time at constant mass and constant volume, increases its rate of rotation, whereas a terrella which expands at constant mass and increasing volume decreases its rate of rotation. As the earth has both differentiated and expanded through geological time, the sense and amount of change depends on relative magnitudes. If mass, tidal retardation, and atmospheric acceleration are added as variables, the limits are complex.

(viii) The sun contributes only one fiftieth of the angular momentum of the solar system, which puts constraints on hypotheses of varying G and mass.

(ix) Orogenesis, continental dispersion, and polar wandering, may all be related directly or indirectly to the prime cause.

(x) A source of sufficient energy is necessary.

(xi) The same principle should be applicable at least to the terrestrial planets, if not more generally.

(xii) A continuing source of water and carbon dioxide, but also of chlorine, nitrogen, and sulphur, from the deep interior is needed to provide the accretion of seawater throughout geological time. But this same source must also provide new mantle material to fill the widening gaps between the continents right down to the core. Current models of the deep interior cannot meet this requirement if the specification is correctly stated. However an inner core with composition similar to carbon chondrites (though necessarily in some metastable superdense state) could do so. For such a source below the mantle the causative phenomena fall beyond the range of laboratory experiments. Estimates of relevant temperatures in the deep interior are widely divergent. Chemical, atomic and nuclear phenomena rest on theoretical models, so that discovery is inhibited by what we think we know. As only one category of seismic waves is transmitted, critical assumptions have to be made to separate density and elastic parameters. Geology textbooks commonly state as fact that the earth has a liquid core some 3000 km. below the surface. A few are more careful and say the core is fluid, because the data are satisfied by any fluid (e.g., a highly compressed gas). No one raises any question that the core is a fluid. But as Danes (1962) has pointed out, this belief involves a critical assumption.

Two kinds of earthquake waves travel through the earth, shear waves and compressional waves. Both are recorded at the surface up to distances corresponding to a penetration of the earth down to 3000 km., which left no doubt that the earth behaved as an elastic solid to that depth. This discovery was surprising because hitherto all had assumed that the earth's interior at relatively shallow depth was liquid, which supplied the lavas to volcanoes and accounted for isostasy and the oblate shape of the earth. At penetrations below 3000 km. only one wave was recorded with a velocity intermediate between those of the two waves just above. Here at last was the expected liquid core, because a fluid does not transmit shear waves (unless the frequency is so high that the material does not get time to flow) and the compression-wave velocity should drop in a liquid because the effective rigidity of the material (which is zero for a fluid) is one of the components of the velocity equation. So the fluid core became accepted fact.

However, Danes has reminded us that this belief is not fact, but only plausible assumption. A core of very high rigidity could satisfy the seismic data at least as well, if not better, than a fluid core. The single wave received through the core would then be the shear wave travelling at increased speed because of the increased rigidity. The compressional wave would arrive at the antipodes several minutes ahead of the first wave now seen as the first arrival (PKP), but its amplitude would normally be so reduced by the strong reflection back into the mantle and by the strong dispersion within the core (both caused by very high rigidity of the core), that its amplitude would be below the normal threshold of microseisms. At each interface, the partition of the energy of a seismic wave between the several possible types of reflected and refracted waves is determined by the angle of incidence and by the elastic parameters of the two media. Danes has calculated the amplitudes of the several possible seismic phases at all distances, and concludes that a very rigid core predicts the observed pattern of amplitudes rather better than does a fluid core. Of course, the fluid core is also involved in the interpretation of many other geophysical phenomena, e.g., geomagnetism and the wobbles and nutations. But when these interpretations grew up, the fluidity of the core was taken for granted. It would be interesting to reinterpret these phenomena, starting with the assumption that the rigidity of the core was an order or more greater than that of the mantle. For example, suppose the rigidity to be three orders greater than that of the lower mantle (e.g., 10^{15} dynes/cm^2) and the viscosity to be 10^{19} poises, it would still behave like a fluid to loads lasting a week or more, although appearing ten thousand times as rigid as steel to seismic waves. This could satisfy the nutations. The magnetic field has been explained in terms of a self-excited dynamo driven by thermal convection currents in the fluid core, producing the field according to principles of magneto-hydrodynamics. So far, this is no more than a plausible guess and it is conceivable that a new start could be made on interpretation of the earth's magnetic field in terms of such phenomena as Hall currents and the high conductivities of the deep interior.

* * *

My fourth answer is to test proposed solutions against the constraints. Solutions so far proposed include (a) phase change at constant mass, (b) secular decrease in the gravitation constant G, (c) secular increase in mass, (d) secular change in $^{e}/m$, the electrical and inertial magnitudes of the electron.

PHASE CHANGE AT CONSTANT MASS

Lindemann (1927), Halm (1935), Keindl (1940), Shneiderov (1943, 1961), the Walker brothers (1954), Egyed (1956), and later papers), Carey (1958), Heezen (1959), and Mouritsen (1972), all thought phase change the most probable cause, but with various initial densities from 17 (Egyed) to many thousands, such as deduced for the white dwarfs. Ramsey's hypothesis, that in all the terrestrial planets the dense core is a pressure paramorph of Fe-Mg silicates, has much to commend it, but all who have thought about it recognise that the pressure within the earth (and still less so in Mercury) is insufficient to condense silicates to such a super-dense state. But this objection is not necessarily valid for the reverse transition from a metastable inheritance of ylem-like matter from the birth of the solar system. Kuhn and Rittmann (1941) postulated an inner core of primitive undifferentiated solar matter (largely hydrogen) on quite different grounds. Metastability with statistical reversion is not inconceivable (e.g. radioactive instability). The reversion law could conceivably allow exponential increase in rate (opposite to radioactivity). The best geophysical analyses imply a steepening of temperature gradients within the earth with time, which implies progressive change to less dense phases with time at *all* phase transitions.

So *some* expansion from phase transitions seems inevitable even without an inherited super-dense ultra-phase. But not enough. The super-dense initial-matter model excludes the Laplacian model of evolution of the solar system from a primary gas cloud, which in its modern American or Russian versions is the currently fashionable theory. However the gas-cloud kind of theory has not disposed of its own lethal difficulties; for Jeans' early caveat (that a cloud of at least 1000 solar masses is needed for the spontaneous collapse process) has been generally confirmed; nor has any of the models been able to combine the planetesimals and small proto-planets into the few existing planets (Metz, 1975, p. 818).

Again, with the super-dense model, the surface gravity remains improbably high at a stage late enough to expect some evidence of it (vi above). Initial rotation rates, even to a stage late enough to be detected, become improbably high (viii above). Still another difficulty is that each planet would need its own variety of metastable superdense primitive material in order to fit the spectrum of chemistries of the planets, which looks like a volatility sequence.

To sum up, although I do not abandon it out of hand I would suspect that, with phase change alone, I had not identified the prime cause of the empirical expansion.

SECULAR DECREASE IN G

In Newton's mechanics, and most that followed his, the gravitational "constant" G, was indeed a constant. Milne (1935) proposed that G was proportional to the age of the universe, that is that G increased with time at a rate $\frac{\dot{G}}{G} = 2 \times 10^{9}$/yr. Dirac (1937 and 1938) likewise proposed on philosophical grounds that G should decrease in proportion to the age of the universe. Dirac (1937, 1938, 1974), Dicke (1957, 1964, 1966), Brans and Dicke (1961), Kapp (1960), Ivanenko and Sagitov (1961), Prokhovnik (1970), Van Flandern (1974), Hoyle and Narlikar (1971), and Wesson (1973) have proposed secular decrease in G on cosmological grounds, irrespective of geophysical implications (though aware of them). Wilson, temporarily, (1960), Egyed, latterly, (1969), Jordan (1966), Dearnley (1965), Steiner (1967), Kropotkin and Trapeznikov (1963), and Wesson (1973) used the physical suggestion of a decrease in G to solve the empirical problem of earth expansion. Gilbert (1956) has pointed out that the assumption that $G \propto {}^{1}/t$, along with Dirac's empirical constants, gives directly an age for the universe of 4×10^{9} years, which is certainly of the right order of magnitude, -- an impressive success.

The estimated average rate given by Hoyle and Narlikar (10^{-10}/year) would correspond to a circumference increase of about 2 mm per year, or 400 km since the Triassic. Observed rates are greater than this but empiricism indicates that the expansion rate has been increasing with time, so that rates during the last 200 million years were much faster than this average. Also, the expansion is increased by other feedbacks. An experimental upper limit for the present rate of change of G has been determined at 4×10^{-10} per year by Shapiro and four co-authors (1971), who used caesium-beam atomic standards to measure the orbital period of Mercury over six years. Any change of G greater than the above limit would have exceeded the experimental errors of these observations.

The gravity potential energy of the earth accounts for 61 percent of its total energy (elastic compression 22 percent, heat 8 percent, radioactivity 2 percent, etc.). The gravitational potential energy is the volume integral of mgh, which involves the gravitational constant G, and the radius. Hence, if G decreases, the earth radius must increase inversely to conserve energy, (unless the earth is not a closed system in respect to energy, and then whatever causes the change in G also conserves energy somewhere else in the universe).

Throughout the earth there are phase transitions where minerals condense to denser forms. At a given temperature these transitions depend on pressure. If G diminishes, so does the pressure at all depths, and minerals change phase to their less dense forms, resulting in further expansion. Gravity differentiation within the earth may also result in expansion, because if a uniform sphere progresses to a radially differentiated one, the total potential energy is greatly reduced. At constant temperature this would result in expansion to conserve energy, or if all this lost energy were dissipated as heat, as is commonly assumed, the rising temperature would further shift the phase transition boundaries, with denser phases becoming less dense, and hence there would be further expansion.

The secular decrease in G, if it is real, is probably competent to account for the expansion. Steiner (1967) proposed a 300 m.y. cyclic variation of G according to the position of the solar system in its "cosmic year" galactic orbit, and combined this with the Dirac-Jordan effect to give a pulsed overall decrease in G which fits (iii) of my boundary conditions. The problem of boundary condition (vi) (a high value of surface gravity relatively recently) remains. Change in G (if this were the only variable) would change the orbital diameter and orbital periods for all satellites:

$$\frac{\dot{R}}{R} = -\frac{\dot{G}}{G} \text{ and } \frac{\dot{\omega}}{\omega} = 2\frac{\dot{G}}{G}$$

where R and ω are orbital radius and period respectively. From these, and the precision with which R and ω are currently known, Shapiro *et al* (1971) estimated that the rate of change of G could not exceed 4×10^{-10} per year, which is a little wider than the maximum rate proposed by Hoyle and Narlikar. In any case this limitation does not apply if mass increases with time; the increasing mass changes orbital radius and period in the opposite sense to decreasing G. It could be that both changes ($\dot{m}$ and $\dot{G}$) spring from a common cause.

Dicke (1962) states that the luminosity of the sun is probably proportional to G^7 or G^8, while the radius of the earth's orbit would vary by G^{-1} (implying radiation flux by G^{-2}). He suggests that the surface temperature of the earth would vary by 4th root of the solar radiation flux at the earth's surface - that is as $G^{2.5}$. On this basis he estimates that the surface temperature would have been about human blood temperature one aeon ago, 58°C two aeons ago, and 80°C three aeons ago. However at the present time 77% of the incident radiant energy is reflected back into space. Increase in absorbed heat would alter the balance of water in ocean to water in atmosphere and extend the tropical cloud belt to higher latitudes, especially on the sunlit side, thus increasing the albedo of the earth. Dicke's estimates of palaeotemperatures must be regarded as upper limits rather than probable reality. Even so the history of life on the earth is not inconsistent with his estimates though, as he points out, Precambrian glaciation would constitute a problem. Even here the variation of obliquity could constitute the major cause of greater departure from the mean, and there are still other variables and uncertainties (see Wesson, 1973, p.33).

Dearborn and Schram (1974) have argued that escape velocity diminishes with decrease in G, and that the fact that clusters of galaxies and globular clusters currently exist, sets an upper limit on $\dot{G}/G$ of 6 x 10^{11} per year. But how valid is their basic premise? Certainly globular clusters and clusters of galaxies exist, but they *are* in a state of escaping from each other, and such mutual recession is the theme of the universe. Retrogressing towards very high G would drive all matter towards becoming neutron stars and black holes, and ultimately all matter as one black hole which may have been the initial condition of the universe *(ylem)*. From such a beginning, progressive decrease in G could lead to the state of the universe we currently observe.

Palaeogravity Measurement

The pragmatic question of course arises, why not measure *g* in former geological times? In correspondence with the late Arthur Holmes, we listed fifty geological phenomena affected by the value of *g*, but never by *g* alone, and measurable. The rate of isostatic adjustment, the speed of glaciers, salt-domes, slope of cross-bedding, foot-prints, thickness of lava flows, all had their moment of promise but failed to survive critical analysis. Glacial grooving is useless because much (eventually all) of the weight of the glacier is supported by the uplift pressure of the basal meltwater (see Carey and Ahmad, 1961, p. 874). An ass can carry another ass on his back, which loosens criteria based on muscle and bone of Palaeozoic animals. In any case the early colonisers were semi-aquatic and supported much of their weight on water or mud. Besides it could be that the fact that colonisation of the land by plants and animals, where they experienced weight for the first time, was delayed until the last tenth of geological time, proclaims in part that gravity was too heavy a deterent earlier. In any case the wings of a Carboniferous dragon-fly were supported by air which was denser in proportion to the ancient gravity. The volcanic-ash-trajectory test proposed by Wesson (1968) suffers the same feedback. Consolidation of pelitic sediments in basins, which have lain dormant since deposition, remains the best hope of estimating palaeogravity. (Stewart, 1972). It would be difficult to be sure that the test sediments had never been loaded by anything else, since removed (e.g. higher sediments, or an icesheet), and that they had never been loaded by temporary drainage of part of the pile above them. But even with these factors controlled, the problem remains unsolved. The consolidating force is simply the difference between the weight of the sediments and their buoyancy in their pervasive interstitial groundwater, which also increases with increasing gravity, so the consolidating differential from higher g cancels.

SECULAR INCREASE IN MASS

Hilgenberg (1933), and Neyman and Kirillov (1961) proposed that the change in volume was due primarily to growth in mass. Hilgenberg's source of mass was the absorption of energy from the aether. All-pervading aether is no longer accepted since the advent of relativity, but let us look more closely at growth of mass with time. I was told as a student that the universe was moving towards a heat death - when all matter had become radiation according to the transformation, $E = mc^2$. We know many modes whereby matter becomes energy - radioactivity, nuclear fusion, nuclear fission, novae, super-novae, and yet others. But it always seemed necessary that this process should be reversible and that modes should exist whereby energy begets matter, that $E \rightleftharpoons mc^2$ should be reversible, and that in the long run, as in most reversible reactions, a steady state should obtain. This seemed to be implicit in the laws of nature, because all theories of the universe involved either initial creation of matter or continuous creation of matter -- as hydrogen atoms in the most empty space, or as discrete stars, or as whole galaxies -- inevitable creation, within a philosophy in which matter could neither be created nor destroyed! Again surely $E \rightleftharpoons mc^2$ had to be a two-way phenomenon.

Long ago Newton (1704) wrote: "Why may not Nature change bodies into light, and light into bodies?" Jeans, in his *Astronomy and Cosmogony* (1928), said: "The type of conjecture which presents itself somewhat insistently, is that the centres of nebulae are of the nature of singular points at which matter is poured into our universe from some other and entirely extraneous spatial dimension, so that to a denizen of our universe they appear as points at which matter is being continuously created". What is this "extraneous spatial dimension"? It would be consistent with the laws of nature that the source be energy - presumably radiation, but conceivably some other mode. Is it a fundamental law of nature, as McCrea (1964) would have it, that all matter grows at a rate which is a function of its own concentration? Perhaps with the expansion of the universe as a corollary? Dirac's large number hypothesis (1974) requires that the number of particles in the universe increases in proportion to t^2, which conforms also to a steady-state in an expanding universe, which remains empirically constant for any observer within it. Dirac debates the relative merits of "multiplicative creation" which would satisfy Jeans concept of matter pouring in to the universe where it is already most concentrated and corresponds to Einstein's cylindrical closed universe, and "additive creation" which would satisfy Stother's model (see next page) where the matter appears in the most tenuous space. The latter corresponds to flat Minowski space originating with the "big bang".

Towe (1975) has claimed that "if atomic distances vary as t^{-1} as required by Dirac's large-numbers theory, then the *d*-spacings in crystal lattices in geologically older minerals should be observedly different from those in the same minerals forming today. The geological evidence, however is strongly against such a prediction: the lattice dimensions of a 3×10^9 year-old quartz crystal are the same as the lattice dimensions of quartz grown in the laboratory." Of course they are, and would be under Dirac's theory. Quartz would have *grown* initially with the *d*-spacings appropriate to the time of origin. But as the ambient fields and "constants" change, so would the size of the unit cell whatever its juvenility or senility. Gittus (1975) and presumably Towe assume that mass increment in a quartz crystal would take the form of new atoms of silicon and oxygen, and perhaps these 'new' atoms could occupy interstitial positions at vacancies and dislocations. However, surely if energy becomes matter at a rate dependent on the existing concentration of matter, by a reversal of

Einstein's equations, the new matter would appear, not as silicon or oxygen, but as quite elementary particles, eventually as protons and hydrogen, which would diffuse out of its uterine host, and in the case of the earth, contribute to the pervasive outgassing which has been characteristic of Earth evolution.

McDougall *et al* (1963) pointed out that Egyed's observation that Hubble's constant for the rate of expansion of the universe yields also the postulated rate of Earth expansion, and the rate of expansion of the moon's orbit (Klepp, 1964), may imply a universal fundamental process. Such a model would produce the same phenomenological behaviour of the earth that Hilgenberg sought from his aether sinks. It would avoid the problems (vi) of improbably high surface g in the not-too-distant past, and (v) of solar heat flux which seems to be inherent in dependence on variable G alone, and also the boundary condition (ii) of expansion increasing exponentially. But it may go much further and solve the unsolved enigma, that the sun bears 99.9% of the mass of the solar system but only 2% of the rotational momentum. Comparisons of Hubble's constant with rate of Earth expansion involves assumptions about the equality of clock time (as observed in the rotation of the earth and the revolution of the planets) and atomic time (as observed in nuclear processes, and in relevance of Milne's cosmological principle). With advancing time, all spectral lines should shift towards the blue, and all atoms should increase progressively in size. What then happens to our unit of length, whether it be a standard metre bar, or the size of a hydrogen atom?

Stothers (1966) argued that: (a) matter is created *where* it is lacking (between galaxy clusters) *because* it is lacking (due to universal expansion); (b) quasars are the manifestations of such creation; (c) matter is created sharing the universal genes of expansion. Wesson (1973), who has discussed several aspects of growth of mass within the earth, also notes that continuous creation might also explain the unaccountably high neutron flux implied by the dynamics of the solar corona and the solar wind. On the other hand growth of mass might be difficult to reconcile with isotopic data, which implies a common age for Earth, Moon and meteorites, and that all have existed as closed systems since formation (Patterson, 1956). Likewise, we do not observe any specific difference in Archaean minerals to suggest that new atoms have appeared within them, although, if we did, we would interpret it as diffusive metasomatism.

Another difficulty in proposing continuous creation within the earth, as distinct from in stars and galaxies, is that most models yield hydrogen as the newborn matter, implying an earth whose interior is mostly hydrogen, which indeed was proposed on other grounds by Kuhn and Rittmann (1941). To sustain the mass growth model we must either accept the kind of earth sought by Kuhn and Rittmann, or postulate as yet unidentified modes of energy-to-matter transformation.

The absorption of energy to permit mass growth would be very large. For example, to take an extreme limiting case, assume the mass of the earth had doubled in 200 m.y., then 3 x 10^{27} grams (half mass of earth) is equivalent to 21 x 10^{40} joules ($E = mc^2$), which is equivalent to 7 x 10^{25} watts (taken over 200 m.y.). (For comparison this is only an order less than the total energy output of the sun in all modes). If this energy were in the form of say neutrinos, the implied neutrino flux over the earth's surface would be some 30 megawatts per sq. cm, which is absurdly high. Hence a combination of decreasing G and mass growth would seem to be necessary if this hypothesis is to be pursued. Wesson (1973), after a critical review, concluded that the most probable cause of expansion was secular decrease in G or secular creation of matter, or more probably both. Perhaps they could be intrinsically related. Kapp (1960), like Stothers (1966), supported continuous creation of matter, not where matter is most concentrated, but in the vacuum voids farthest from galaxies. According to Kapp the creation of matter is balanced by annihilation, at a rate proportional to local mass concentration. Holmes (1965), p. 985-6) has pointed out that the equally sharp rings of pleochroic haloes from old weak sources and young strong sources denies Kapp's model, as does the concurrency of radiometric ages determined from different isotopes.

SECULAR CHANGE OF e/m

Among the constants of physics is the numerical ratio of the charge of an electron to its mass, usually expressed in the form e^2/Gm_pm_e, where e is the electrostatic attraction between a proton of mass m_p and an electron of mass m_e at unit distance, G being the gravitational constant. This ratio is a pure number, which Dirac's large number hypothesis requires to increase directly with time. In the long run this determines the size of a hydrogen atom - of all atoms, the spectral wavelengths of the Balmer series. Does e/m vary with time? Dirac's large number hypothesis would require this ratio to increase with time. This appears to conflict with the observed sharpness of ancient pleochroic haloes. Also Broulik and

Trefil (1971) found (at least for their model) little change in e since the beginning of the earth. Is the Doppler red-shift merely a vista of older light - not because light "gets tired" during hundreds of millions of years of journey, but because it started off that way, and that from the most distant nebulae we see Cambrian light? Does each atom, all matter, the whole universe, expand with the progress of time? Is this the meaning of the Hubble constant? I doubt it. There is too much cross-correlation in support of a genuine Doppler effect, which is confirmed for the most distant objects by concordant shifts in optical and radio bands, and for nearer measures such as rotation of our own galaxy, and for very near bodies such as the advancing and receding limbs of Saturn and Sun. However, accepting a valid cosmic Doppler red-shift does not necessarily exclude contributions from other concurrent processes. There are many unexplained anomalies in the overall red-shift model. John B. Eichler (in personal correspondence) has suggested modes whereby quanta lose energy to diffuse matter in space at a rate which only becomes detectable over cosmic distances, and elegantly explains some of these anomalies.

Baum and Nielsen (1975), stimulated by van Flandern's conclusion that the precise times of lunar occultations of reference stars implied secular decline in G, sought independent astronomic evidence that van Flandern's observed secular changes might originate in non-constancy of some other physical constant involved in our "measuring sticks". However, their investigations confirmed the constancy of (hc) the product of Planck's constant and the velocity of light, and by inference the individual constancy of h, c, e, and m. This conclusion would re-instate $^e/_m$ and the Bohr radius as constants, leaving G as the only candidate to accept the blame for the secular variations.

UNIVERSAL NULL

As a high school student I used to wonder about the claim that both momentum and energy had to be conserved. Truly, one was a vector and the other a scalar; but one had the form MLT^{-1} and the other ML^2T^{-2}, so that conditions might arise where one conservation law had to yield. It seemed clear to me then that in such circumstances the momentum law was the more fundamental; for whereas energy may transform into many modes (mechanical, thermal, electrical, elastic, magnetic, electromagnetic, and even into matter), momentum has no transformations, and momentum must be conserved as momentum still. Creation, and the ultimate origin and destiny of the universe, were also perennial problems. Postulation of a creator did no more than start the chicken-and-egg infinite series of creators to create creators. The only viable answer seems to be that the sum total of the universe is zero. That the sum of all matter and energy of the universe is zero, that the sum of all momentum in the universe is zero, and that the sum of all charge in the universe is zero -- a universal null!

A single particle in a universal void has neither velocity nor momentum. Once there are two particles, each has velocity and momentum relevant to the other. The momentum of the one cancels that of the other. The sum of all momentum of the universe is, I suggest, zero.

Energy does not exist without matter. Even a photon, which lacks rest mass, possesses energy only by virtue of motion with respect to matter. Conceive a primitive condition when matter is dispersed tenuously through all space. All the energy exists either as matter or as potential energy, the two complementary forms, which appear together. Any condensations or concentrations of matter which may occur substitute kinetic, heat, and other forms of energy for some of the initial potential energy. (The potential energy of the earth is the residue of the initial potential energy left after all matter on the earth fell from infinite distance to its present position. The lost potential energy has been converted to other forms of energy.

In this simple system, commencing from a universal null, infinitely small perturbations yield small amounts of matter, infinitely dispersed, balanced by infinitely small amounts of potential energy. The matter-energy sum remains zero, however large of however small the total dispersed mass or the total compensating potential energy, or however much transformation may occur in the form of energy. By analogy, we may compare the gravity-wave perturbation of a placid pond, or in the business world, the creation of colossal operating credits offset by compensating debits which together feed enormous industrial activity - with no initial capital beyond a book entry.

Indeed if one conceives a universal void - null time, null matter, null space, ultimately miniscule random perturbations should set off the compensating plus and minus, up and down, positive and negative, matter and potential, so that in due course a universe as we observe it would be the inevitable result. Even starting from such a universal void, if something could happen, sooner or later it will happen.

As the total mass of the universe increases (balanced by its total energy) new matter would appear increasingly at the centres of maximum mass-concentration, because there its potential energy is minimum and energy and mass can still equate. In a mature universe, most matter would enter at the centres of nebulae - as indeed Jeans deduced.

The entropy of the null initial condition should be zero, so that any subsequent condition of the universe would involve increase of entropy. Entropy however would exclude cyclic processes. I have never been convinced that I should believe the second law of thermodynamics - at least not as a universal. Maxwell's Demon proved to me that the law could not be universal. It has been argued that because Maxwell's Demon must use energy in opening and closing doors strong enough to reflect atoms, he simply acts as a heat pump and is ruled by the second law. This criticism is not valid. A grid bias can control vaster energy than it consumes. As for the "strong door", a weak field could cause large deflections of slow particles with negligible effect on energetic particles - the function demanded of the demon. However his major labour would soon shift to the other side of the door - to stop reflux of energetic particles, while allowing the return of the enfeebled.

The problem of zero time and of mirror symmetry across it evaporates in the null model, because zero time must then be pursued back towards the initial infinity, before the appearance of a random perturbation.

"We See Only What We Know"

Finally, I don't believe that discovery of the most fundamental principles of nature is exhausted. Indeed, the Hubble constant itself may vary with time! Hoyle "saw no objection to supposing that present laws are incomplete, for they are almost surely incomplete." Nor in this connection does Occam's razor intimidate me, for Mother Earth was a complex old lady before Occam had his first shave!

* * *

Which returns me to *my second answer* to the question of the cause of expansion, so I will sit on *my first answer* -- and my empiricism.

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