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The Expanding Earth - an Essay Review

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ABSTRACT

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The Wegener bombshell of gross continental separation promptly triggered the concept of 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), Barnett (1962), Brosske (1962), Neyman (1962), Creer (1965), Dearnley (1965), Jordan (1966), Steiner (1967), and Meservey (1969) ran against the orthodox tide, which, in geology, is lethal

Discovery that pan-global oceanic rifts had palaeomagnetic growth zones, and confirmation by JOIDES that all ocean floors are post-Palaeozoic, fit equally displacement or expansion models The plate model combines ocean floor growth with "axioms" that orogenesis implies crustal shortening, that trenches are underthrusts, and that earth radius is constant All three "axioms" are probably invalid

The plate theory has fatal falsities Africa and Antarctica are ringed by expanding rifts and each should have post-Palaeozoic subduction zones to swallow more than 3,000 km of crust These do not exist This dilemma could be side-stepped by fixing one continent to its mantle, but escape is impossible with two such continents The Permian equator now lies 37° north of the equator in North America, 40° north in Europe, and 17° north in Siberia, which is impossible on an earth of constant radius without at least 6,000 km of post-Palaeozoic subduction *within* the Arctic On the plate model the present Pacific must be smaller than the Permian Pacific by the combined area of the Arctic, Atlantic and Indian Oceans Yet the continents round the periphery of the Pacific have all moved further apart in the direction of the Pacific margin Meservey has shown the topological impossibility of progression from any Pangaea configuration to the present distribution of the continents except on an expanding earth

Phase-change from inherited metastable super-dense matter, change of *G* with time, and secular growth of mass at the expense of energy, have been offered as causes of expansion These could be adequate, but raise other anomalies Some new fundamental principles of physics may still remain to be discovered

GENERAL

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 such as 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), and more recently 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.

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, and analysed the causes of orogenesis with an insight which merits much wider currency and recognition than it has received.

Bogolepow, in three Russian papers in 1922, 1925, and 1928, which are referred to in his 1930 work *Die Dehnung der Lithospare* probably preceded Lindemann in suggesting earth expansion. 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

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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

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 on 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, 1961a,b) 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 subatomic 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 (1906) likewise had observed a secular emergence through geological time. Egyed calculated an average increase of 0.5 mm 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 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

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 m.y , which, translated into rise volume, would account for an 80-m 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

At the Hobart symposium on continental drift (March 1956), I 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 30-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 to the finding of Hilgenberg's and Egyed's papers Accordingly the hypothesis of an expanding earth was actively explored and found to solve many hitherto puzzling features of global tectonics.

Heezen (1959) stated to the Nice-Villefranche international colloquium 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 right along 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 ot 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

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.

Brosske (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, 1954), 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 Barnett (1969) recalled the resemblance of the southern continents to the petals of a flower, which indeed had struck many observers from as far back as Francis Bacon, who even at that date (1620) wondered whether the analogy implied that the earth had expanded. 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).

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.

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 conveyer-belt model.

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.

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 Fairbridge (1965), 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

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 4,400 km 2,750 m y. ago, and 6,000 km 650 m.y ago, compared with 6,378 km 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 m.y.

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 emphasises 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 6,250 and 5,990 km according to earth ellipticity assumed, these radii correspond to maximum increases of the equator of 804 and 2,437 km respectively Deuser compared this with the 2,600 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 escape Meservey's argument

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 all translation on a sphere can be defined by rotation about a pole, his analysis and constructions (e.g., his fig. 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.

Carey (1970), in his presidential address to the Australian and New Zealand Association for the Advancement of Science, 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 7,000 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 (Fig 1).

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 indeed is true, and it was this very misfit, which

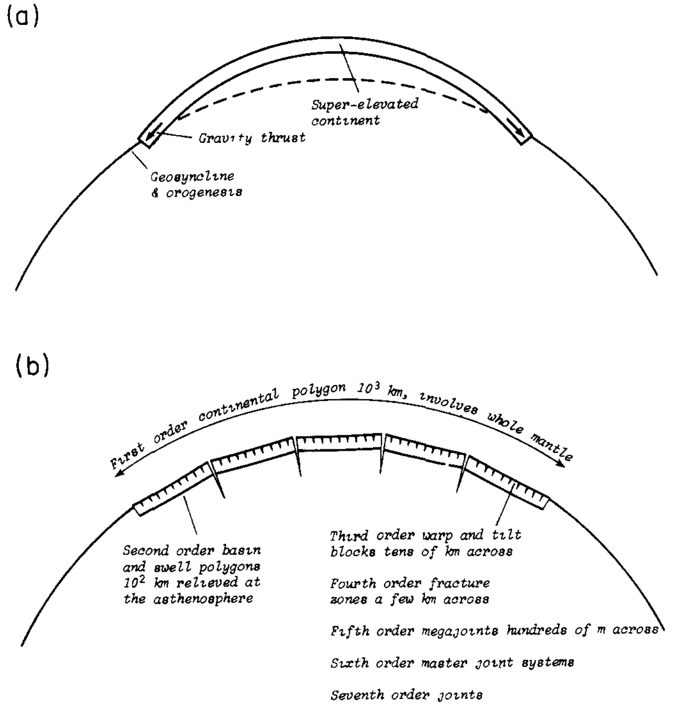

Fig 1 a Simplistic but gravitationally impossible model of expanding earth b Continuous gravitational adjustment of continental curvature through hierarchy of tensional polygons

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. 2), 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/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

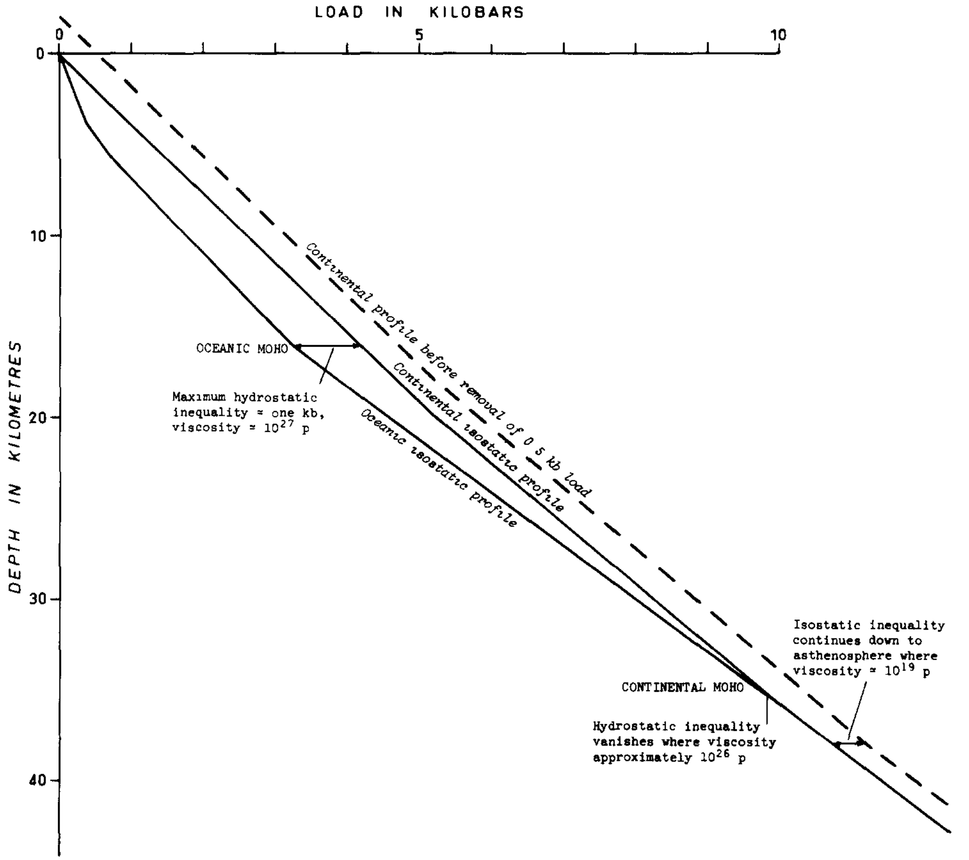

Fig 2 Contrast of isostatic and hydrostatic inequalities

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. 1), 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.

PALAEOMAGNETIC DATA

Cox and Doell (1961) 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 (~ 0.6 mm/year) proposed by Egyed, but made unlikely the radius increase of 1,100 km proposed by Carey (1958). This analysis involved explicitly the assumption that the Eurasian block had remained a stable unit unchanged since the Permian. However, Carey's model (1958, pp. 203—204 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 Carey in the reference they cited, the Permian radius would have been about 4500 km Ward (1963) repeated the same error. Van Hilten (1963) recognised this possibility (which he called "orange-peel effect") though he did not use the specific separations shown in Carey 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 hypothesis of Carey and Heezen" Van Hilten (1965) carried this further and from palaeomagnetic data alone deduced that differential movement had occurred between the blocks of the Eurasian continent

Van Andel and Hospers (1968), notwithstanding their disclaimer to the contrary, also depend 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 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. The Eurasian complex has dispersed substantially since the Permian.

Van Hilten (1964, p. 41) had emphasised the palaeomagnetic discordance between the Siberian and European blocks in the Permian. Ward (1966) criticised van Hilten's method in that "in equation (1) of van Hilten's (1963) paper we see that the geocentric angle [p] between a rock unit and the corresponding pole is considered to change proportionately to the ancient radius" In this Ward has misunderstood van Hilten's notation. To quote van Hilten "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 distance from the sampling site in degrees or radians *on the present-day earth*, at which the palaeopole must be plotted. This "angle between the rock-unit and the corresponding pole" must, of course, "change proportionately to the ancient radius". The criticism of van Hilten by van Andel and Hospers (1969, p. 115) is also caused by this misreading of van Hilten's notation.

Van Andel and Hospers (1969) seek that palaeoradius which yields the minimum scatter for the ancient pole position as the hypothetical radius is varied, and assume that the minimum scatter would identify the most probable ancient radius. Minima are derived for Europe-Siberia (but these do not lie on a coherent block and hence cannot give any indication) The Carboniferous, Permian, and Triassic data of North America do not yield mimima so the palaeo-radius cannot be determined for these data either. Van Andel and Hospers combine the palaeoradii reported by several authors to derive statistically probable ancient earth radii. If the invalid Eurasian data are eliminated from these sources, nothing remains.

Palaeomagnetic measurements are not sensitive enough to detect earth expansion, and show no future prospect of doing so. For example, consider the most favourable case of two stations giving palaeomagnetic inclinations of 85° and 20° (corresponding to latitudes 80°4′ and 10°19′) with a 5° cone or 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. In general the localities on a single continental block are less than 70° apart, and then not in the direction of the palaeomeridian, confidence cones are commonly greater than 5°, dating of sequences so far apart is rarely as close as ten m.y. Each of these variables further extends the palaeo-radius uncertainty beyond 30%.

HEAT FLUX

Beloussov (1967), while agreeing that earth expansion would satisfy his insistence that the continents have never shifted with respect to the mantle beneath them, and that orogenesis is a vertical process with secondary gravitational spreading, nevertheless could not accept expansion because of astronomical and geochemical conflict, and the implied improbably high previous density of the earth, specifically he regarded the overall equality of heat flux between continental and oceanic areas as inconsistent with expansion, because heat generation in the continents is dominated by the high radiogenic content of continental rocks. He considered that equality between oceanic and continental areas implied that the continents had been enriched in radiogenic material from their subjacent mantle, whereas the oceanic mantle was undifferentiated (compare the several contributions on this by Ringwood and his colleagues). In fact this could still be true on an expansion model if new mantle is assumed to be derived continuously from the core. But the flaw in Beloussov's argument is the assumption that the continent-ocean parity of heat flux represents an equilibrium state. This cannot be so. On the expansion model the oceanic prisms have been lifted some 25 km, and if this occurred instantaneously the sub-oceanic mantle temperatures would be 200—300° higher than the sub-continental mantle contours. The uplift is not instantaneous but is very much more rapid than the heat dissipation. Rocks have high thermal inertia, and the half life of the excess heat of new oceanic prisms would be of the order of 100 m.y. Hence oceanic heat flux has two components — the dissipating heat, as the uplifted prism returns at an exponentially diminishing rate to an equilibrium gradient between the ocean floor and the deep interior, and the currently generated radiogenic heat, which therefore must be substantially less than that generated in the continental prisms

THE TRENCHES

Plate tectonics and expansion schools agree in respect to ocean spreading. They differ mainly in the interpretation of the trenches. This issue then must be the crux of this review.

For thirty years after the 1928 AAPG symposium on the Wegener hypotheses, 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 recognised 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. 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. The trenches were assigned this role. Tanner (1973) has emphasised 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) reached similar conclusions (p. 334). "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'."

The trenches are a phenomenon of the Pacific margin, which includes the Caribbean and the Scotia arc (as the Americas are moved back against the African template) and the Sunda arc (when Australia is fitted back against Eastern Ghats). The trenches bow eastward (or less commonly equatorward), which implies deflection by the earth's rotation.

Which side moves? Seismically the arc side is vigorously active, while the Pacific side enjoys seismic peace Continuous seismic profiles show regular sediments undisturbed for thousands of kilometres on the Pacific side 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 that is active? Surely also the gross motion must be an *upward* tumour below the arc and disjunctive basin, bringing *up* the isotherms The gross motion could 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.

Folding and trench sediments

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 is inescapable But this profound disturbance is not there Some of the trenches are empty. Other show serenely slumbering sediments quite undisturbed (e.g., von Huene and Shor, 1969, Scholl et al., 1970) Other 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.

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, 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

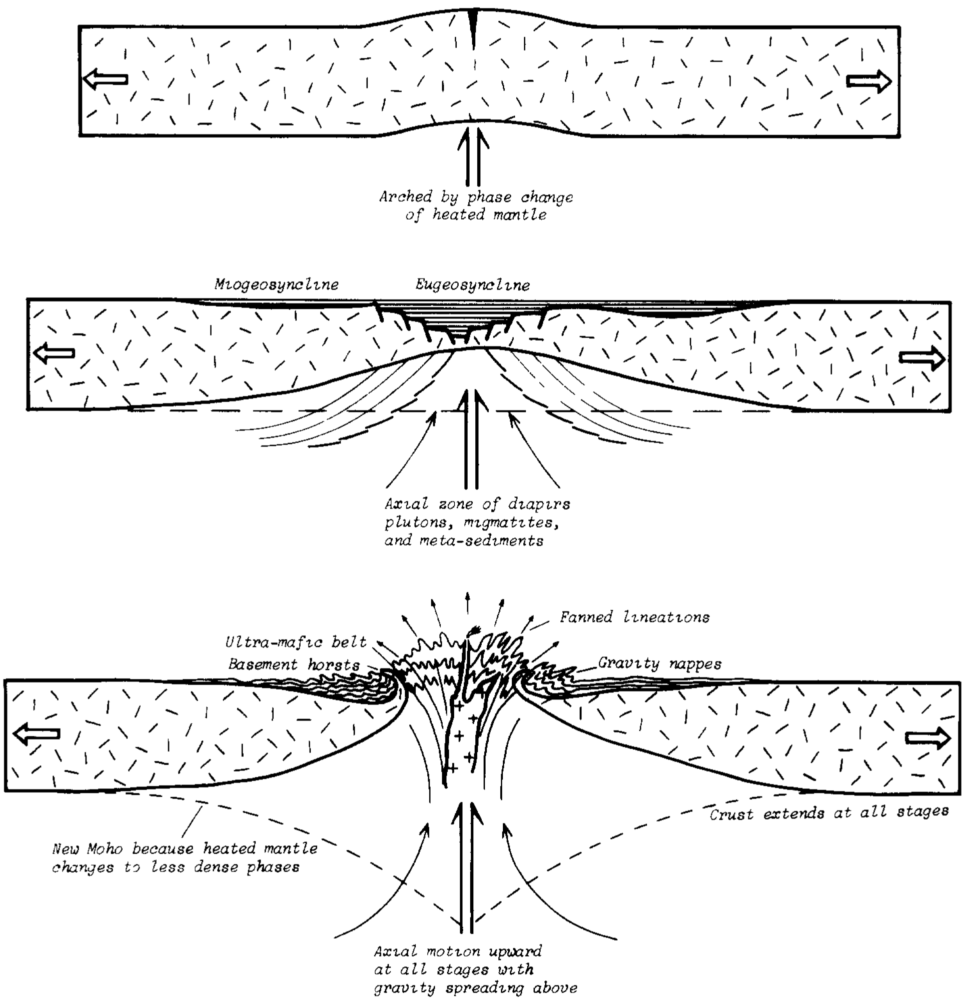

Fig 3 Simplified symmetrical model of geosyncline and orogenesis in a dilating orogen The pattern is commonly modified by east-west asymmetry and material asymmetry at continent—ocean interface

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. 3). Katz (1971) concluded that, contrary to the expectation of subduction compression, the Pacific segment of South America has 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.

Within the orogenic arcs, dominating motion is *up*, with lateral gravity spreading 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. Metamorphic minerals in the core such as kyanite have come up more than 20 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 motion across the trench, the whole combined with overall dilation all the way from the trench across the diapiric orogen and volcanic zone and 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.

Blue schists

The concept that the blue schists, which occur at the orogenic margin nearest the trench, were first dragged down to depths of several tens of kilometres, and then rose by even greater amounts in opposition to the continued downward movement of alleged down-thrusting lithosphere slab, is highly improbable kinematically Viscosities must be of the order of at least 10^{18} and probably higher (this is a relatively low-temperature zone), and the density differential not greater than 0 3. 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 10 or 12 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 blue schist belts contain a variety of rocks, such as low-grade pumpellyite schists, eclogite (perhaps representing in part the original horsts of oceanic floor) and more deformed belts of glaucophane schists, and they may contain hydrothermal veins where the outgassing fluids from the geotumour have penetrated them.

In contradiction to this model, the blue schists have been interpreted as requiring very high pressures at relatively low temperatures for their formation. The high pressure interpretation 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, confining pressure, fluid pressure, and stress-difference Increasing 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 kbar $kcal^{-1}$ $mole^{-1}$ for calcite-aragonite). Stress-difference above yield stress induces a variety of rheological modes, including migration of lattice imperfections, slip on 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.

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. The only relevant experiments I am aware of are those of Newton et al. (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 static conditions.

The interpretation of the blue schists may be further complicated by tectonic 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. Deformation strain energy may have been differentially available in restricted zones. Likewise hydrothermal outgassing may be concentrated. 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.

Seismic data

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 upper layers 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. 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. As crustal extension migrates eastward, which thins the crust there, the gravity spreading off the rising orogenic tumour is biased towards the east Hence these overthrusts dip mainly west. This is true, not only along the west Pacific orogens, but also along the Cordilleran of N and S America. 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 denser than its pressure-temperature environment. However Jeffreys (1967) has concluded from an examination of S_H phases that these earthquakes result from shear failure relieving stress-difference, and not from impulsive change of bulk density.

An astonishing aspect of the plate-tectonic literature is the interpretation by Isacks et al. (1968) of the first-motion data from the Benioff zone, which has been repeated by so many others. 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 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 of the earthquakes have this pattern) But they 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 a 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 intrusion. 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. 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 maximum 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 sub-oceanic crust 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

Another matter which has probably been misinterpreted is the attenuation factor (Q) of shear waves It is commonly assumed that this attenuation is due to reduced viscosity with rising temperature Viscosity is only relevant if it causes significant stress relaxation during the elastic vibration cycle Taking an improbably long period 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 1—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. It seems much more likely that attenuation has no connection with viscosity but rather with stress-difference, and that a low Q means that the rocks traversed 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 strength) so that additional slip occurs, absorbing energy. 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

Higher velocity under Benioff zone

Early arrival residuals (of the order of 1 sec) 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). Waves travelling along this zone would be expected to travel faster than similar waves through a similar medium in a non-strained state. (An impulse travels faster along a highly stressed string and hence emits a higher frequency note than along a less stressed string.)

Contrast of trenches and median ridges

Although these two groups are genetically similar in that they are both part of the global extension system, the parallel ends there, for there are fundamental differences.

Trenches and orogenic arcs	*Oceanic rift zones*
asymmetrical	symmetrical
Pacific margin	medial, initially within continents, subsequently within rift oceans

east-facing arcs	stepped linears
crust youngs eastward away from trench	youngest at trough
deep focus earthquakes	only shallow earthquakes
andesites	basalts

The initial Precambrian Pacific rift (between the Cordilleran and the Cathaysian) may have closely resembled the Atlantic rift — symmetrical, basaltic. But with increasing width of the Pacific Ocean, new rifts became necessary between the primary Pacific and Tethyan rifts to accommodate the global extension of the crust. The second generation rifts were marginal to the Pacific and within the Pangaean craton. The latter were in a symmetrical environment with thick continental crust on each side. The former were in an asymmetrical environment with thick continental crust on one side and oceanic crust on the other, younging away from the new rift. The latter had sub-continental mantle for partial melting to produce tholeiitic magmas. The former had a differentiated mantle, which had already been processed in the initial formation of the Pacific, and partial melting produced andesitic magmas. In the Atlantic type rifts thermal conditions combined with movement rates are such that deep stress-differences relax as they develop, and only shallow earthquakes occur, symmetrically to the rift zone, and on associated transform faults. The boundaries of the tumour at all depths abut not-long-consolidated Quaternary crust, where stress-differences relax By contrast, in the Pacific trench-arc system thermal-relaxation and movement rates are such that the central arc zone likewise has no deep earthquakes, but the Pacific side of the tumour abuts cold sub-Pacific crust which has cooled since the Jurassic. Hence this movement boundary is marked by deep focus earthquakes.

Global expansion rifts thus form a series graded not only in stage of development but also in symmetry and effective depth, and no two levels of this stage-symmetry-rate-depth hierarchy are necessarily similar

(1) Initial Pacific rift — e.g., East Pacific Rise
(2) Initial Tethyan rift
(3) Second generation Pacific rifts — e.g., arc-trench systems
(4) Trans-Pangaean rifts — Indo-Atlantic-Arctic system
(5) Lineaments bounding basins — e.g., Congo-Kalahari-Victoria
(6) Young tumour linears — e.g., Shatsky, mid-Caroline, Ontong Java
(7) Differential tilt blocks — tens of km across
(8) Master joints
(9) Joints

The first four involve the whole mantle, and divide the globe up into primary mantle polygon prisms some thousands of km across and 3,000 km deep The fifth category form polygon prisms a few hundred km across and a few hundred km deep, where they are relieved in the low velocity zone. Such polygons are essential, not only to accommodate the primary expansion, but to adjust the curvature of the primary block surfaces to the increasing radius. The Tethyan rift belt differs from the others in that it has to accommodate global zonal motion leading to sinistrally coupled *S* oroclines and sinistrally rotated blocks.

The gross morphology of a tumour and its associated gravity and seismic velocity distribution is similar, irrespective of whether or not its troughs fill with sediments. Where this occurs the troughs deepen isostatically in the first instance, but they subsequently regurgitate, and the sediments are folded and overthrust during the upward diapirism and spreading under gravity

EVOLUTION OF THE OCEANS

Most take it for granted that there have always been large oceans like those of today — 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 have been formed since the Palaeozoic, so it is assumed that equivalent other ocean areas have been "consumed" (Maxwell's demon is needed at the helm to ensure so clean a sweep that no remnant of old ocean remained anywhere!) Oceans such as the Arctic, Atlantic, and Indian, in any case, date only from the Mesozoic and have doubled their area since the Eocene. 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.

According to this view, the primitive universal crust in the earliest stage of expansion was stretched in a pattern of disjunctive polygons some hundreds of km across. The outgassing of the deep interior concentrated along these polygonal boundaries, bringing heat so that at all depths rocks tended towards less dense paramorphic phases, causing the polygonal outlines to arch (undations of van Bemmelen). The continuation of the process led to rupture and rifting of the arches, giving at the surface a pattern of broad stillstand basins separated by rift troughs, bordered by raised rims. Lake Victoria basin and its bordering troughs are a rather small present-day example. The polygonal rifts became the primitive geosynclines which, with the continuation of the process, eventually regurgitated their contents with much volcanic and plutonic igneous activity and outflow of nappes (of metasediments and plutonics, as well as of less altered sediments) onto 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, the weight of which further depressed the basins both by isostasy and paramorphic increase of density under greater load So we get the typical form of the ancient terrains — narrow troughs of intensely metamorphosed rocks, often branching round "cratons", along with broad basins of ancient but little-altered strata. At this time there were no great

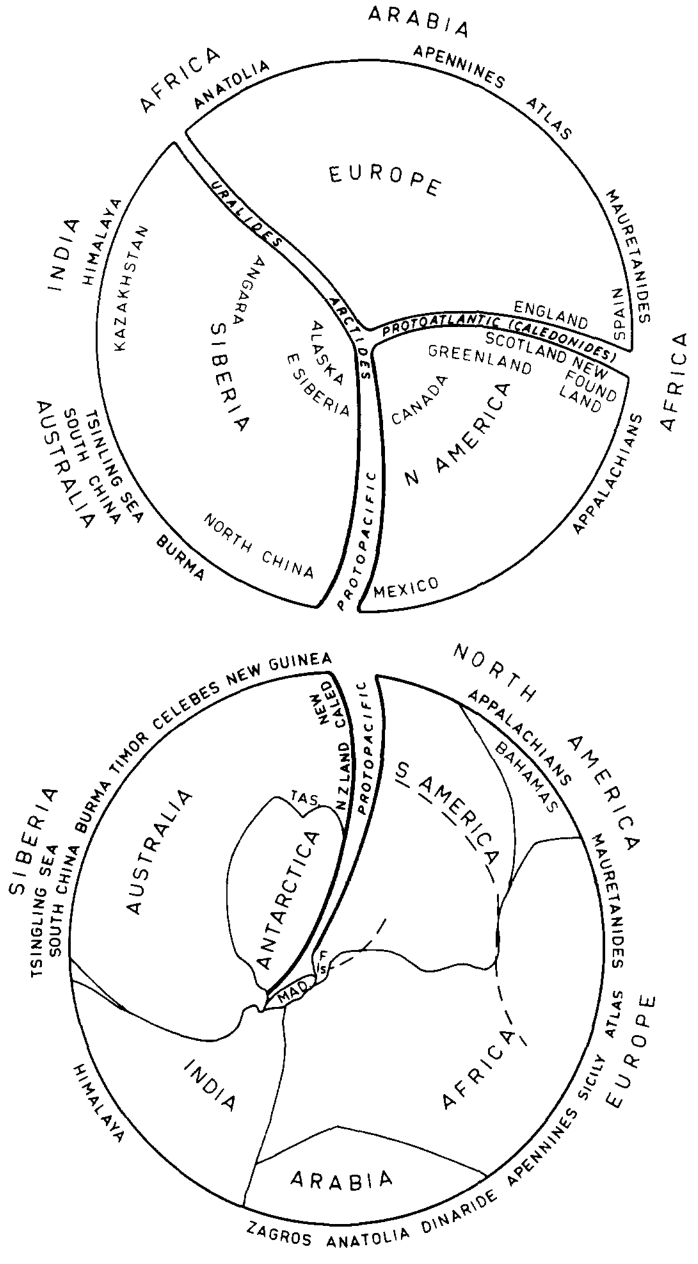

Fig 4 Orthographic sketch of the primitive hemispheres divided by the Tethys

oceans anywhere on the earth, but broad intracontinental seas (of the Witwatersrand type), always shallow during their development, and long narrow deep troughs.

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. Perhaps the first was an equatorial rift — the Proto-Tethys, but early were meridional rifts: a Proto-Pacific (Cordilleran-Cathaysian and Andean-Tasmantides) and a Proto-Atlantic (Caledonides) all of which existed before the end of the Proterozoic (Fig. 4). At that time the Australian and East Asian continents were separated from the Americas only by such a narrow seaway trough in which sediments accumulated. These geosynclinal seas overspilled into the cratonic basins at times of high ocean-water to ocean-capacity ratio 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. 5). Likewise the

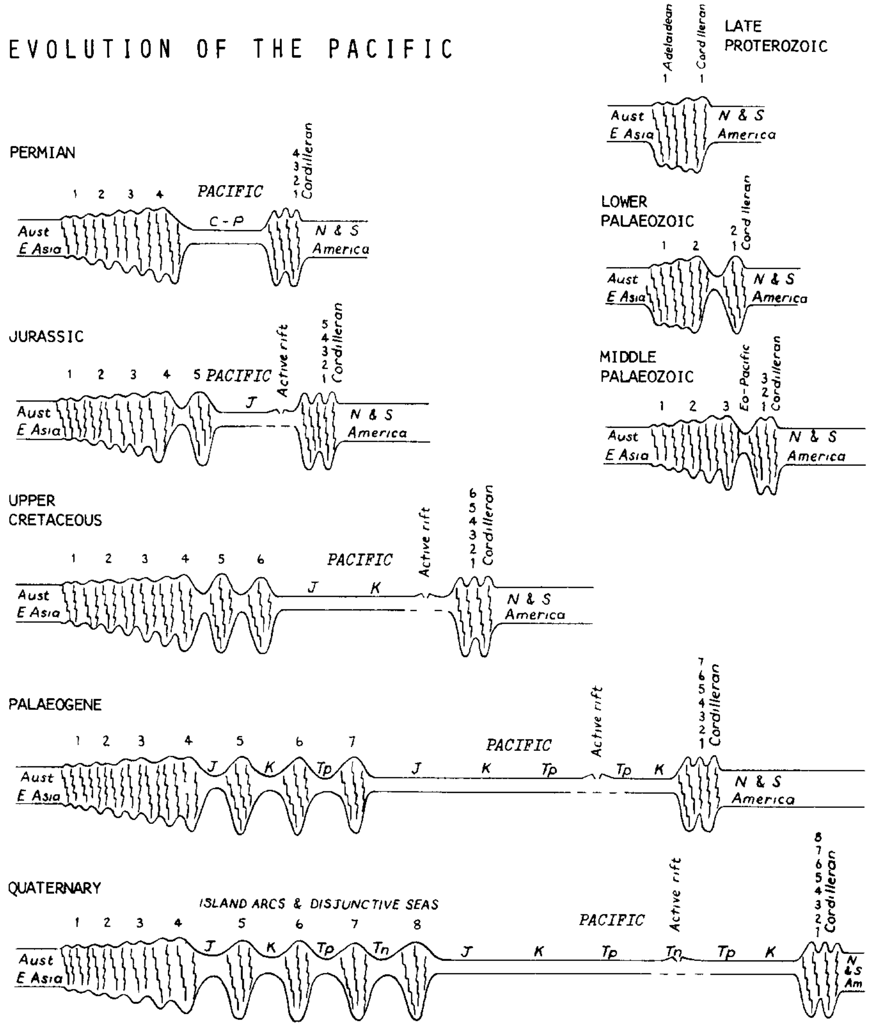

Fig 5 Evolution of the Pacific Ocean

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.

Thus the Pacific Ocean grew asymmetrically. The Australasian marginal orogens migrated ever eastwards — at first side by side, and later separated by disjunctive seas. 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. Because stretching concentrated along the eastern side of the orogenic tumours, rift trenches, thinned crust, and negative gravity anomalies flank the eastern side of the orogens, and as a consequence the superficial gravity spreading of the regurgitating tumour is biased towards the east. This is true, not only on the east-facing arcs of East Asia and the Tasman, but also along the Cordilleran of N. and S. America. 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 onto the coast of California. The Proto-Tethys was like the Early Palaeozoic Pacific, a narrow through-going seaway which, from time to time, flooded over into intra-continental shallow seas. During the Tertiary, the differential expansion of the southern and northern hemisphere caused a progressive equatorial (Tethyan) shear of the southern hemisphere eastwards with respect to the northern, which *S*-dragged the Tethyan orogens and rotated anticlockwise detached cratonic blocks such as Spain, India, Guatemala, and Newfoundland, and offset the andesite line from the Philippines to Fiji.

Prior to the Gondwana cycle the northern hemisphere may have been the more rapidly expanding hemisphere, so that the sense of the Tethyan shear may then have been dextral. The northern hemisphere certainly shows more evidence of early disruptive dispersion than the southern. The Caledonide rift (Proto-Atlantic) connected the northern Cordilleran near Alaska to the Proto-Tethyan seaway near the Anti-Atlas of Morocco. This trough regurgitated its heart in the Calidonide orogeny, and subsequent rejuvenations of the rifting followed a similar but not identical line, as proposed by Wilson (1966). The Uralide rift also shows early disruptive dispersion. But apart from the very active ancestor of the South Pacific, disruption of Gondwanaland before the Carboniferous was less intense.

ASYMMETRY OF EXPANSION

The asymmetry of the earth was summarised by Carey (1962). Neither north and south, nor east and west are tectonically equivalent. Ocean and land are generally antipodal. A north-polar ocean surrounded by a northern land girdle and three southward projections contrast with a south-polar continent girdled by a southern ocean and three northward projections With the opening flower-bud analogy the south-pointing continents are the petals and the oceans are the gaps. The southern hemisphere has much more new ocean, continents have dispersed more, and grossly the southern hemisphere has expanded more than the north, but even within a hemisphere the expansion is non-uniform, with crustal dispersion from a few megatumours in the deep mantle This is as would be expected, because outgassing raises the temperature along its path, which in turn causes phase-change swelling there, rifting the crust, and at the same time reduces the viscosity, so that pressure relief and out-gassing is facilitated Such feedback mechanisms inevitably produce asymmetry. Asymmetry of thermal flux, with consequent density changes inevitably yields a "pear-shaped" figure and more precisely a bumpy geoid, even when in isostatic equilibrium

Greater expansion in the southern hemisphere implies that the parallels of latitude must sweep southwards across the continents, even though they have not moved with respect to their underlying mantle (Fig. 6). 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 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

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,

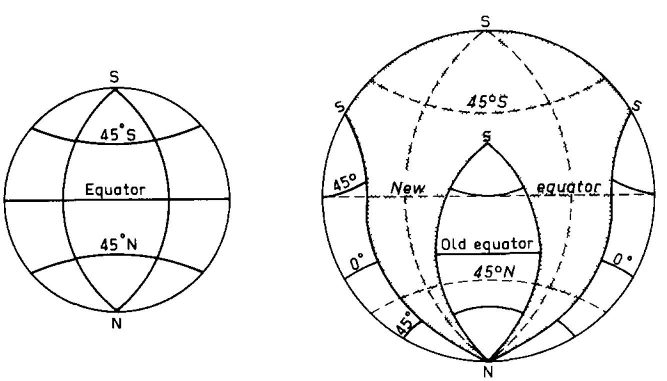

Fig 6 Migration of parallels of latitude across continents during asymmetrical expansion

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 in indeed what has occurred. All northern continents are displaced west with respect to their southern neighbours. To restore Pangaea before the opening of the Atlantic, North America has to be moved substantially more than South America to join Africa, because of the Tethyan shear between them Blocks in the Tethyan zone (Spain, Corsica-Sardinia, Italy, Arabia, India, Honduras-Guatemala, Newfoundland) all show sinistral rotation. Sinistrally coupled oroclines with *S*-pattern (Italy, Baluchistan-Himalaya, Sunda) imply major sinistral shift along the Tethys The border of the Pacific as expressed by the andesite line is offset sinistrally from the Marianas trend to Samoa, where it resumes its trend. The southern hemisphere as a whole has turned some 30° east with respect to the northern hemisphere because of the differential expansion.

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 freely, 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 2½ 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 eastwards with respect to Asia, and those of the Tasman Sea have grown eastward with respect to Australia

The *first answer* is I do not know. Empirically I am satisfied that the earth is expanding.

The *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. In the controversy between Kelvin, master physicist of his generation, who limited the age of the earth to the order of 100 m.y., and Geikie, who knew empirically that much longer time was needed, most geologists wilted before the physicists' heat. If Geikie were 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, and had he done so, he would not have been believed.

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

(1) The expansion has continued throughout the whole history of the earth.

(2) The rate has accelerated with time, perhaps exponentially. This does not necessarily exclude a constant cause, as the acceleration could result from feedback.

(3) 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.

(4) The surface expression is asymmetric with respect to axis, hemisphere, and octant This may also be due to feedback.

(5) The source is below the crust and almost certainly below the mantle.

(6) The surface temperature of the earth has allowed water at the surface at least for the last three aeons.

(7) 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.

(8) 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 constant volume, decreases its rate of rotation. As the earth 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.

(9) Orogenesis, continental dispersion, and polar wandering, may all be related directly or indirectly to the prime cause.

(10) A source of sufficient energy is necessary.

(11) The same principle should be applicable at least to the terrestrial planets, if not more generally.

The *fourth answer* is to test proposed solutions against these limits. 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, Halm, Keindl, the Walker brothers, Egyed, and Heezen, 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 superdense 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. 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 seem inevitable even without an inherited super-dense ultra-phase But not enough. Serious constraints are implied on the origin of the solar system. The surface gravity remains improbably high at a stage late enough to expect some evidence of it (7 above) Initial rotation rates, even to a stage late enough to be detected, become improbably high (8 above). Hence 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

Dirac, Dicke, Ivanenko and Sagitov, and Hoyle and Narlikar, proposed the secular decrease in the gravitational constant, irrespective of its geophysical implications (though aware of them). Wilson (temporarily), Egyed (latterly), Jordan, Dearnley, and Steiner, used the physical suggestion to solve the geological problem. The gravity potential energy of the earth accounts for 61% of its total energy (elastic compression 22%, heat 8%, radioactivity 2%, 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. The estimated average rate given by Hoyle and Narlikar (10^{-10}/year) would correspond to a circumference increase of about 2 mm/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 m.y. were much faster than this average. Also, the expansion is increased by other feedbacks. 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 also results 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, 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 (3) of my boundary conditions. The problem of boundary condition (7) — 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.

$$\frac{R}{R} = -\frac{G}{G} \text{ and } \frac{\omega}{\omega} = 2\frac{G}{G}$$

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 $4 \cdot 10^{-10}$/year, which is a little wider than the maximum rate proposed by Hoyle and Narlikar (1971). 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 G) spring from a common cause

Secular increase in mass

Hilgenberg proposed that the change in volume was due primarily to growth in mass, from the absorption of energy from the aether. All-pervading aether is no longer accepted, 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 Then long ago 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 is energy — presumably radiation, but conceivably some other mode Is it a fundamental law of nature 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? MacDougall et al (1963) pointed out that the fact 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 (7) of improbably high surface g in the not-too-distant past, and (6) of solar heat flux which seems to be inherent in dependence on variable G alone, and also the boundary condition (2) 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. 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 $3 \cdot 10^{27}$ grams (half mass of earth) is equivalent to $21 \cdot 10^{40}$ joules ($E = mc^2$), which is equivalent to $7 \cdot 10^{25}$ 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/cm^2 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.

Secular change of e/m

Among the constants of physics is the numerical ratio of the charge on an electron to its mass. In the long run this determines the size of a hydrogen atom — of all atoms, the spectral wavelengths of the Balmer series. Does the "constant" vary with time? 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? This, of course, accords with Milne's concept that "clock time", based on the rotation of the earth and periodicities of all astronomic bodies, and "atomic time" based on the spectral frequencies of atomic vibrations — two measures of time which are equated now by definition — are independent, so that atomic time pulsed more slowly than clock time in the past, and will run progressively more rapidly in the future. 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 But equally I don't believe that discovery of the most fundamental principles of nature is exhausted. 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.

ENERGY FOR EXPANSION

Excluding kinetic energy of the earth's galactic motion with the solar system and the earth's orbital and rotational energy, the principal energy sources of the earth are

	Joules	%
gravity potential	$2.5 \cdot 10^{31}$	62.5
elastic compression	$9.0 \cdot 10^{30}$	23
heat	$3.2 \cdot 10^{30}$	8
gravity differentiation	$1.5 \cdot 10^{30}$	3.75
radioactivity	$\simeq 1.3 \cdot 10^{30}$	1.13

The next largest is probably the isostatic departure from hydrostatic equilibrium which amounts to only $2 \cdot 10^{26}$ joules

Beck (1960) showed that redistribution of density within the earth could allow expansion at constant mass and constant gravitational potential energy If improbable density distributions are postulated, expansions of the order of 1,000 km are possible. Expansions without external addition of energy are possible even if the core is kept constant and only the mantle and crust are changed. In a more extensive study Beck (1961) considered the energy sources of an earth originating from cold accretion and evolving at constant mass, he concluded that some expansion was possible and plausible.

Allowing for radioactive heating and secular decrease in G to the extent postulated by Dicke (1957), Beck found sufficient energy to account for an expansion of radius of 100 km, but, without some additional source of energy, radius expansion of 1,000 km or more was improbable. Beck's analysis does not allow for the stored metastable energy postulated by Egyed in his inherited super-dense core, for the larger decrease in G postulated by Hoyle and Narlikar, for the released elastic energy implied by decrease in G, nor for the case of secular increase in mass postulated by Hilgenberg and discussed by the reviewer (infra). Beck's analysis implies that for the near-doubling of the earth's radius postulated by Hilgenberg, Halm, Keindl, Egyed, Carey, Heezen, Barnett, Creer, Jordan, and Dearnley, some energy contribution from these sources which were omitted from his analysis, or some other unspecified source of energy, would be necessary. Beck allowed 100 km for change in G as proposed by Dicke. However the maximum average rate of decrease of G (10^{-10} per annum) proposed by Hoyle and Narlikar would release gravity potential and elastic potential energy at the rate of $34 \cdot 10^{20}$ joules/year. This would imply 500 km radius increase since the Triassic (400 km from gravity potential energy and 100 km from elastic energy). In addition all phase transitions within the earth depend on temperature and pressure. Reduction in G means that the pressure at all depths diminishes, so that reversion to the less dense phase occurs at all transitions, further increasing the expansion beyond 500 km since the Triassic If the variation of G is pulsed by the galactic rotation as proposed by Steiner, even greater post-Triassic expansion is possible. Hence, if we accept the variation of G proposed by Hoyle and Narlikar, the energy is adequate both for the more recent expansion, and for doubling of the radius since the Archaean, even if no inherited metastable energy (Egyed), and constant mass, are both assumed.

Cook and Eardley (1961), considering only thermal expansion and phase changes as the cause of expansion, concluded like Beck "that the earth could not have expanded to the extent proposed by some advocates of continental separation". They calculated that for a doubling of the surface area since the Palaeozoic, an energy input of $5 \cdot 10^{22}$ ergs/sec would be necessary. The maximum change of G of 10^{-10}/year estimated by Hoyle and Narlikar would yield 10^{28} ergs/year from release of gravitational potential and elastic energy, which to the precision of both calculations is of comparable order. Any misfit could be bridged by other variables involved.

TESTS

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, Honolulu, 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) 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.

(3) 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

$$\frac{dg}{dt}=\frac{dg}{dr}\ \frac{dr}{dt}=\frac{-2MG}{r^3}\ r\ \frac{0.4}{3}\ 10^{-8}=-2g\ \ 0.13\cdot 10^{-8}\simeq -2\ 6\ \mu\text{gals}\,\text{year}^{-1}$$

At constant density·

$$\frac{dg}{dt}=\frac{dg}{dr}\ \frac{dr}{dt}=\frac{4}{3}\pi\rho G r\cdot\frac{0.4}{3}\cdot 10^{-8}=g\ \ \frac{0.4}{3}\cdot 10^{-8}\simeq +1\ 3\ \mu\text{gals}\,\text{year}^{-1}$$

Although these terrellae are simple it seems to 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*

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