Not Getting the Drift

A Hard Look at the Early History of Plate-Tectonic Ideas

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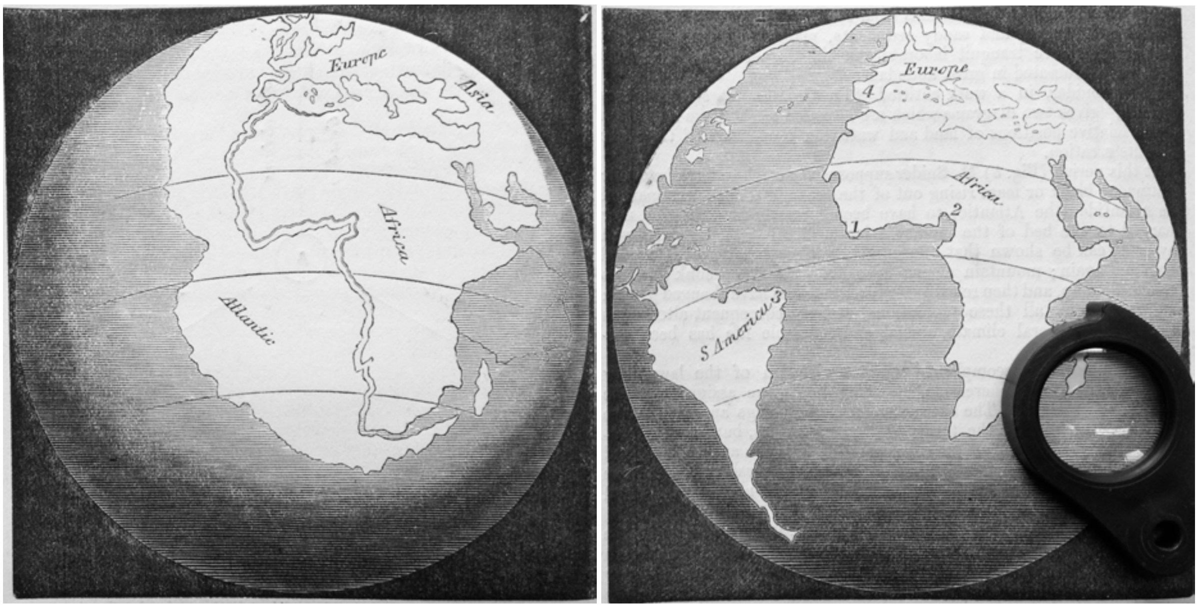

Pepper's continental-drift hypothesis in 1861.
No one got it. History forgot it.

A fully documented revisionist history that finally explains why
Alfred Wegener's hypothesis of continental drift was rejected for so long.

This book is being given away for open access,
without the approbation of a scientific publisher. As Wegener put it:
"What anyone can see does not need the support of other opinions."

Not getting the drift: a hard look at the early history of plate-tectonic ideas

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Abstract

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Textbooks teach the principles of science. Lyell's geology textbooks emphasized vertical crustal movement. He avoided far-fetched continental-drift hypotheses by Hopkins in 1844 and Pepper in 1861. Their notions of drift were supported by fossil and paleoclimate evidence, but their causes were global magnetism and electrochemical crystallization and dissolution. Dana's textbooks from 1863 to 1895 taught that the symmetry of North America proved it had always stood alone; thus Americans were conditioned to reject Wegener's concept of a Carboniferous supercontinent. Unaware of Wegener's hypothesis in 1912, Schuchert launched a textbook series that guided American geological opinion from 1915 to the 1960s. His paleogeographic models required Carboniferous land bridges to connect fixed continents. He and coauthors Longwell and Dunbar eventually realized that Wegener's continental-drift hypothesis would disprove land-bridge theory and solve problems of mountain ranges, paleoclimates, and fossil distributions, but they guarded against it in their textbooks. Already in 1927, Holmes proposed that convection with sea-floor spreading drove continental drift, but editor Schuchert would not publish that breakthrough. Geologists Du Toit, Van der Gracht, Holmes, Shand, Bailey, and Grabau showed the merits of continental drift, but their publications had little impact. Willis accepted the invitations of Schuchert in 1932 and Longwell in 1944 to write papers opposing Wegener's hypothesis. Simpson contributed with paleontologic opposition. In 1944 Holmes published a British textbook that showed how continental drift could change geology. It was Holmes, Carey, and Wilson, as much as the Americans Hess and Dietz, who should be credited with instigating the plate-tectonic revolution.

1. Introduction

A great breakthrough in the field of biology came in 1859, when Charles Darwin published his book *The Origin of Species*. He showed that plants and animals were not created in their present forms, but evolved through time from other species. This hypothesis generated much interest and debate, and organic evolution was soon more or less accepted among biologists. Biological institutions throughout the world recently celebrated the 150-year anniversary of that great scientific breakthrough.

One might say that a corresponding breakthrough in the field of geology came in 1912, when Alfred Wegener published his paper *The Origin of Continents (Die Entstehung der Kontinente)*. It was about inorganic evolution. The continental entities were not created in their present forms and positions, but evolved through breakup and horizontal displacement. Geologists took note of this hypothesis, and some debated it. But it was rejected until 50 years later, when the concept of continental displacement became an essential feature of geology's unifying theory, now known as plate tectonics.

Having just passed the 100-year anniversary of Wegener's first paper, it is time to confront the main reason why the anniversary was not celebrated: the long denial of continental drift is an embarrassment to earth scientists. We don't even know some of the important parts of this history. Maybe we have not wanted to know. The purpose of this book is to draw attention to these parts, not to embarrass us further, but to encourage healthy scientific debate. The history of the continental drift hypothesis reminds us that scientific consensus can be wrong. Even the best scientists can err. Unorthodox hypotheses should be encouraged and treated fairly. To be against a scientific consensus is not the same as being against science.

The first surprise in this revisionist history of the continental drift hypothesis is that we not only missed celebrating its 100-year anniversary in 2012, but we missed celebrating its 150-year anniversary in 2011. Let's start by looking at these very early ideas.

2. Hypotheses of neptunian continental drift

CHARLES LYELL'S HORIZONTAL "TRANSFER" OF CONTINENTS (1837)

The progress of geologic understanding over the past two centuries is well documented in beautifully written and illustrated textbooks. Sir Charles Lyell (1797-1875) was the most productive and influential of the early textbook authors. His main income came from his books. He began his career with a geology professorship at King's College in London (1831), but resigned within two years, apparently over disputes concerning his geological teaching and the teachings of the Bible.

Lyell completed his initial 4-volume geology textbook in 1834. After that he revised and improved it frequently. It reached maturity by the 5th edition, in 1837. Afterward, he split the subject into two major books, his *Principles of Geology*, which deals mostly with geological processes and how they operate, and his *Elements of Geology,* which records the events that have taken place in historical geology. He kept these two textbooks up to date for decades, the 11th edition of *Principles* appearing in 1875, with about 1300 pages, and the 6th edition of *Elements* in 1871, with about 800 pages and 800 engravings.

Lyell emphasized the dramatic changes of the globe. The complete title of his first volume was *Principles of Geology, Being an Attempt to Explain the Former Changes of the Earth's Surface, by Reference to Causes Now in Operation*. This principle, that the same causes that we know today also operated in the past, is often called uniformitarianism. Together with the frontispiece of his first volume he gave one of his favorite quotes, by a mentor, John Playfair (1748-1819):

"Amid all the revolutions of the globe, the economy of Nature has been uniform, and her laws are the only things that have resisted the general movement. The rivers and the rocks, the seas and the continents have been changed in all their parts; but the laws which direct those changes and the rules to which they are subject, have remained invariably the same." –PLAYFAIR, Illustrations of the Huttonian Theory, § 374.

Lyell documented vertical changes of the seas and the continents. At that time, oceanic crust was thought to be the same as continental crust, only at lower elevation. Marine sediments and fossils now form high mountains; they had clearly risen from below the sea. Entire continents seemed to have once been covered by marine sediments with marine fossils. It was natural to think that the continents had earlier sunk below the sea, and then returned to their previous levels. Lyell was eager to demonstrate such vertical changes. Horizontal forces must have existed as well. They could be demonstrated in the folded layers seen in mountain ranges. But horizontal movement was not a point that Lyell emphasized in his early works.

Lyell dealt with all aspects of the Earth, including the causes and history of climates. England, Europe, and North America are all in the northern hemisphere, and it was that part of the world where rocks and fossils had been best studied and the ancient climates best understood. The record of rocks and fossils proved that the climate of the northern hemisphere had been much warmer in earlier geologic times, as Lyell skillfully explained:

Lyell 1837, p 138.

Climates of the Northern hemisphere formerly hotter. –

That the climate of the Northern hemisphere has undergone an important change, and that its mean annual temperature must once have resembled that now experienced within the tropics, was the opinion of some of the first naturalists who investigated the contents of the ancient strata. Their conjecture became more probable when the shells and corals of the secondary rocks were more carefully examined; for these organic remains were found to be intimately connected by generic affinity with species now living in warmer latitudes. At a later period, many reptiles, such as turtles, tortoises, and large saurian animals, were discovered in European formations in great abundance; and they supplied new and powerful arguments, from analogy, in support of the doctrine, that the heat of the climate had been great when our secondary strata were deposited. Lastly, when the botanist turned his attention to the specific determination of fossil plants, the evidence acquired the fullest confirmation; for the flora of a country is peculiarly influenced by temperature: and the ancient vegetation of the earth might, more readily than the forms of animals, have afforded conflicting proofs, had the popular theory been without foundation. When the examination of animal and vegetable remains was extended to rocks in the most northern parts of Europe and North America, and even to the Arctic regions, indications of the same revolution in climate were discovered.

The northern continents had once been tropically hot, even in the Arctic regions. Lyell sought to explain such climatic variations by the positions of the continents. He used many pages of text to explain how the locations of oceans, continents, and mountains could influence climate both locally and globally. If continents are suitably arranged, warm ocean currents such as the Gulf Stream can bring mild climate to high northern latitudes. The opposite result is also possible. Snow-covered lands and ice-covered oceans will reflect solar rays, and further cool an already cold planet. He emphasized that the previous locations of lands, mountains, and oceans must have been very different from today, and so too was the climate.

Lyell never claimed that continents had moved great distances horizontally. But certain parts of his discussion, and his fold-out map (Fig. 1), could lead a reader to this interpretation. It is interesting to think about this potential misunderstanding as we read his descriptions:

Lyell 1837, p. 184.

If we now proceed to consider the circumstances required for a *general* change of temperature, it will appear, from the acts and principles already laid down, that whenever a greater extent of high land is collected in the polar regions, the cold will augment; and the same result will be produced when there is more sea between or near the tropics; while, on the contrary, so often as the above conditions are reverse, the heat will be greater. (See Map, Pl. 1) If this be admitted, it will follow, that unless the superficial inequalities of the earth be fixed and permanent, there must be never-ending fluctuation in the mean temperature of every zone; and that the climate of one era can no more be a type of every other, than is one of our four seasons of all the rest.

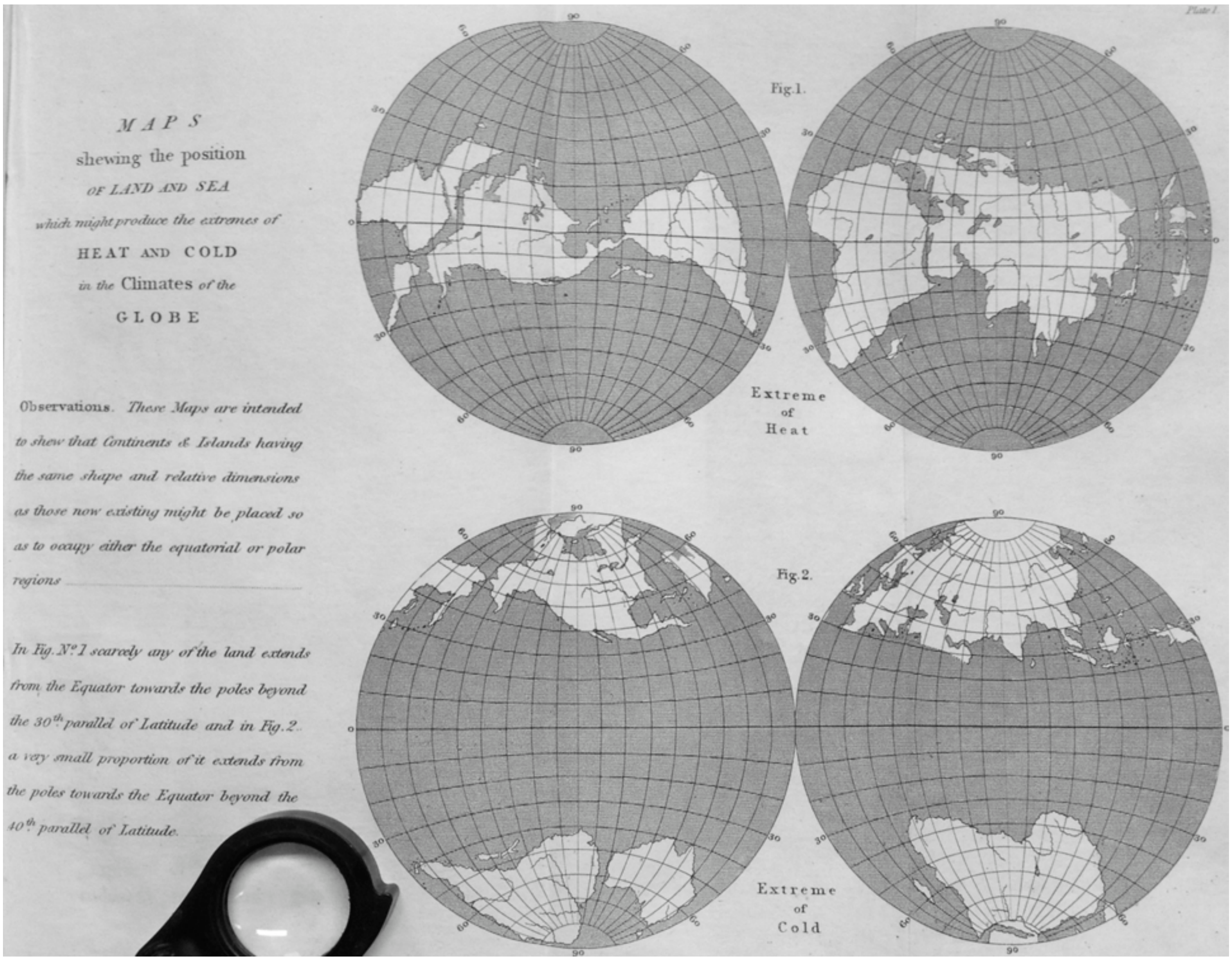


Figure 1. Lyell's folding map Plate 1 to illustrate that the position of land and sea is not fixed. The hand lens for scale has outside frame diameter 2.5 cm (1 inch). From Lyell (1837)

Lyell's Plate 1 consisted of two curious globe maps showing "transferred" continents. The purpose of these maps was to help explain how the positions of continents can affect climate:

Lyell 1837, p. 129-130.

But we have still to contemplate the additional refrigeration which might be effected by changes in the relative position of land and sea in the southern hemisphere. If the remaining continents were transferred from the equatorial and contiguous latitudes to the south polar regions, the intensity of cold produced might, perhaps, render the globe uninhabitable. We are too ignorant of the laws governing the direction of subterranean forces, to determine whether such a crisis be within the limits of possibility. At the same time, it may be observed, that no distribution of land can well be imagined more irregular, or, as it were, capricious, than that which now prevails; for at present, by drawing a line in a particular direction, the globe may be divided into two equal parts, in such a manner, that one hemisphere shall be entirely covered with water, with the exception of some promontories and islands, while the other shall contain less water than land; and, what is still more extraordinary, on comparing the extratropical lands in the northern and southern hemispheres, the lands in the northern are found to be to those in the southern in the proportion of thirteen to one!* To imagine all the lands, therefore, in high, and all the sea in low latitudes, as delineated in the annexed plate (PI. I.), would scarcely be a more anomalous state of the surface.

Position of land and sea which might give rise to the extreme of heat. — Let us now turn from the contemplation of the winter of the "great year," and consider the opposite train of circumstances which would bring on the spring and summer. To imagine all the lands to be collected together in equatorial latitudes, and a few promontories only to project beyond the thirtieth parallel, as represented in the annexed map (Fig. 1. Pl. I.), would be undoubtedly to suppose an extreme result of geological change. But if we consider a mere approximation to such a state of things, it would be sufficient to cause a general elevation of temperature. Nor can it be regarded as a visionary idea, that, amidst the revolutions of the earth's surface, the quantity of land should, at certain periods, have been simultaneously lessened in the vicinity of both the poles, and increased within the tropics.

A careful reading of this and other parts of Lyell's text shows that he was envisioning vertical movements of continents and oceans, not horizontal motions. Although he wrote: "If the continents were transferred to the south-polar regions" he really meant: "If the continents near the equator were to sink, and new continents were to rise near the south-polar region." His choice of words certainly gives the idea that continents might somehow be transferred horizontally. And his figures on Plate 1 show the continents in recognizable form, as if they had been horizontally displaced. He would not speculate on these things. He wrote: "We are too ignorant of the laws governing the direction of subterranean forces, to determine whether such a crisis be within the limits of possibility."

EVAN HOPKINS' PROOF OF NORTHWARD SHIFTING OF THE CONTINENTS (1844)

Lyell's textbooks helped educate and inspire most of the geologists of his day. One of them was Evan Hopkins (1810-1867), who has been completely overlooked by geologic historians (he is apparently unrelated to William Hopkins.) He held the title Civil Engineer and Fellow of the Geological Society. Hopkins had traveled widely, especially in the southern hemisphere, having worked as ore geologist and director of gold and silver mines in Central and South America. With geological experience also in Mexico, Australia, and Europe, his background would surely have satisfied Lyell, who had stressed the need for geologists to travel:

Lyell 1837, p. 83.

If it be true that delivery be the first, second, and third requisite in a popular orator, it is no less certain that to travel is of first, second, and third importance to those who desire to originate just and comprehensive views concerning the structure of our globe.

It is almost as if Lyell had invited Evan Hopkins to contribute "comprehensive views", as indeed he did. Hopkins published a remarkable book. He claimed that continents had moved not only vertically, but also horizontally across the surface of the

Earth, and that they were still moving in a steady-state process. Hopkins thought that Lyell would approve of this suggestion. He wrote:

Hopkins 1844, p. 78-79.

We find that the variable nature of climates from pole to pole arises principally from the obliquity of the rays and the height from the sea, and not from internal heat; therefore we can only account for the changes of climate indicated by the organic remains by changes in the relative position of the dry land and the sun's rays.

Professor Lyell suggested that the changes in the position of land and sea may have given rise to the vicissitudes in climate. The Professor does not bring proofs of more than a mere rise and fall of land from the level of the ocean, which could not furnish us with tropical heat in the arctic regions, therefore he assumes the possibility of geographical changes, such as the shifting of land from the southern hemisphere to the northern, to reconcile the effects with the facts.

But Lyell had never claimed great horizontal shifting of continents. Hopkins therefore removed this particular paragraph in the second edition of his book in 1851. It is one of very few deletions that Hopkins made. I feel quite certain that Lyell had read Hopkins' first edition, and then personally complained about this paragraph, asking Hopkins to remove it. I can think of no other reason why Hopkins would do so. Lyell, however, did not change the suggestive maps and statements about transferred continents from further editions of his own book.

Following this paragraph about Lyell, Hopkins offered substantial geological arguments for the horizontal displacement of continents on a global scale. As you read Hopkins, I think you will be impressed with his understanding, detail, and clarity:

Hopkins 1844, p. 79-82.

Let us consider what would be the nature of the deposition in a large tract of land like Australia, supposing it gradually floated from its present position to the north polar region. Here and near it, tree ferns, Cycadeæ, Araucariæ, Cassiarinæ grow upon the land: corals and sponges abound on the coast even of Van Diemen's Land; also Trigonia, Cerithium, Isocardia, a Cardium like *C. hillarium* of the greensand, and quadrupeds of the peculiar marsupial races, to which the Stonesfield animal is referred by Cuvier. These would be deposited, and their place would become gradually occupied by others as it approached the equator, where it would be inhabited by a different variety. These would again disappear on the arrival of the land in the north, and their place would be taken by others. The contents of the deposition, supposing the land undulated above and below the level of the sea during its movement from the south to the north, would represent the order of deposition and organic remains similar to those now found in the rocks of the northern hemisphere.

THE DIVISION OF THE SURFACE OF THE GLOBE INTO ZONES OF DEPOSITION

If, as we have endeavoured to establish, the sedimentary rocks have been deposited in different zones during the movement of the surface from the south pole towards the north, we may distinguish their respective zones of deposition in the following order: –

Zones	Deposits
No. 1. South frigid	The most ancient: – Cambrian and Silurian.
No. 2 South temperate . .	The Carboniferous, or the great coal formation.
No. 3 South tropic	Oolitic or Saurian group.
No. 4. North tropic	Cretaceous and tertiary of Europe.
No. 5. North temperate . .	Alluvial deposits of Europe.

1. Commencing with the most ancient deposits, we find Orthoceratite, Trilobite and other marine relics, but scarcely any land plants. That this ancient deposit should not contain land plants is not surprising. The newly-discovered countries within the south frigid zone, although placed in latitudes in which herds of wild herbivorous animals are met with in the northern hemisphere, nay, where man himself exists, are most wintry in their aspect, almost entirely covered with ice and snow even in summer, and completely destitute of animal life and vegetation.

The living representatives of the fossils have not yet been identified; but according to Dr Buckland, the nearest approach among living animals to the external form of Trilobites is that afforded by the genus Serolis in the class Crustacea. The genus Serolis inhabits the Straits of Magellan and the coast of Patagonia. The beach is often seen covered with them dead; they are found alive only by dredging in deep water. This region is but little known, but the above approximation is sufficient for our present purpose. All the deposits found within this region are comparatively recent, containing organic remains of those only which inhabit the bordering sea.

2. South temperate deposits, or Carboniferous groups.

The forms of life buried in this system of strata are exceedingly numerous and varied, and generally in an excellent state of preservation, allowing of a most strict comparison with existing types. They consist of many races of plants, abundance of Zoophyta, with multitudes of Mollusca, Crustacea and Fishes. The plants are in some respects very similar to existing races, as the large group of ferns generally, the Equiseta, Lycopodiaceæ, Araucaria, Cycadeæ, Coniferæ, andc. The remains of these plants are often abundant in coal-seams. The coal plants of North America are for the most part identical with those of Europe, and all belong to the same genera. Specimens from Greenland are referrible to ferns, analogous to those of our European coal mines. The fossil plants brought from Melville Island warrant similar conclusions. The coal formation of Bogota, which is situated within 4° north of the equator, at an elevation of about 8000 feet above the level of the sea, contains the same kind of plants – arborescent ferns, and Lycopodiaceæ of the same species as those now growing in the southern hemisphere. The living representatives of the above are found in New Holland, New Zealand, Brazils, Chili, and various islands within the south temperate zone. Not a single species of Cycadeæ is known to grow in the north temperate region; their principal localities are equinoctial America, and southward of that part, the Cape of Good Hope, Madagascar, India, the Molucca Islands and New Holland.

The enlarged size of the arborescent ferns depend not only on a warm temperature, but also on a shady and moist place. Within the tropics they are found at an elevation of about 4000 feet above the sea, from twenty to thirty feet high, flourishing only in shady parts of great humidity. It is in the south temperate zone of America that the ferns approach to a magnitude of those found in the European coal formation. Trees grow in this region to a very large size, and the shrubs and smaller plants become particularly luxuriant and productive.

Hopkins 1844, p. 83.

Tree ferns, which require abundance of moisture and an equalization of the seasons, are found in Van Diemen's Land in south latitude 42°, and in New Zealand in south latitude 45°. The orchideous parasites also advance to the 42° south latitude.

Hopkins continued his presentation of the European deposits for many more pages, showing that the climates of Europe had changed through geologic time, as the continental crust moved from a southern to a northern position. Modern paleomagnetic results can tell us the previous latitudes of the continents, and we know now that Europe has indeed moved northward through most of the Paleozoic. Hopkins was not completely correct. He interpreted the oldest deposits to have come from the South Frigid Zone, but the South Temperate Zone would have been more accurate.

As you might expect, Hopkins had obtained many of his facts from the authoritative textbooks of Lyell. Hopkins was trying to find a solution to a geological puzzle that Lyell had presented. Lyell had already clearly stated the problem of Carboniferous coal deposits on Melville Island:

Lyell 1837, p. 110.

Plants, it is affirmed, cannot remain in darkness, even for a week, without serious injury, unless in a torpid state; and if exposed to heat and moisture they cannot remain torpid, but will grow, and must therefore perish. If, then, in the latitude of Melville Island, 75° N., a high temperature, and consequent humidity, prevailed at that period when we know the arctic seas were filled with corals and large multilocular shells, how could plants of tropical forms have flourished? Is not the bright light of equatorial regions as indispensable a condition of their well-being as the sultry heat of the same countries? and how could they annually endure a night prolonged for three months?

Lyell did not take this problem lightly. He discussed it for several pages in this book. He wondered if perhaps the Earth had no axial tilt in the Carboniferous. If this were the case, there would be no summer and winter seasons, and no long period of winter darkness. But even so, the sun would just barely rise above the horizon at such high latitudes, not suitable for tropical plants and corals. Lyell had no answer to these questions in 1837, but indicated that more information was necessary. In fact, Hopkins' answer in 1844 was the right one.

A MECHANISM FOR GLOBAL SHIFT OF CRUST

Hopkins argued strongly that continents had moved across the surface of the globe, and the mechanism that he called upon to move them was very creative. His mechanism is revealed already in the title of his book: *On the Connexion of Geology with Terrestrial Magnetism*. He proposed that the Earth's magnetism was a second form of gravitation. For both of these mysterious forces, the strength is inversely proportional to the square of the distance. In his theory, gravity draws mass toward the center of the Earth, and magnetism draws the surface water and the crust of the Earth from the South Pole toward the North Pole (Fig. 2):

Hopkins 1844, p. 7.

If the earth be a magnet, as we have endeavoured to prove, it must produce the effects observed; if it not be a magnet, it possesses a property identical in its results to one; therefore all we require in our investigations is the knowledge of the law of these actions, as the mere *name* of the primary cause of the action cannot have a material influence on our researches. If we continue to call it *gravitation*, we must add to it a property which was not applied to it before, viz. polarity, – call it *magnetism*, and the term embraces all we require in astronomy as well as in geology.

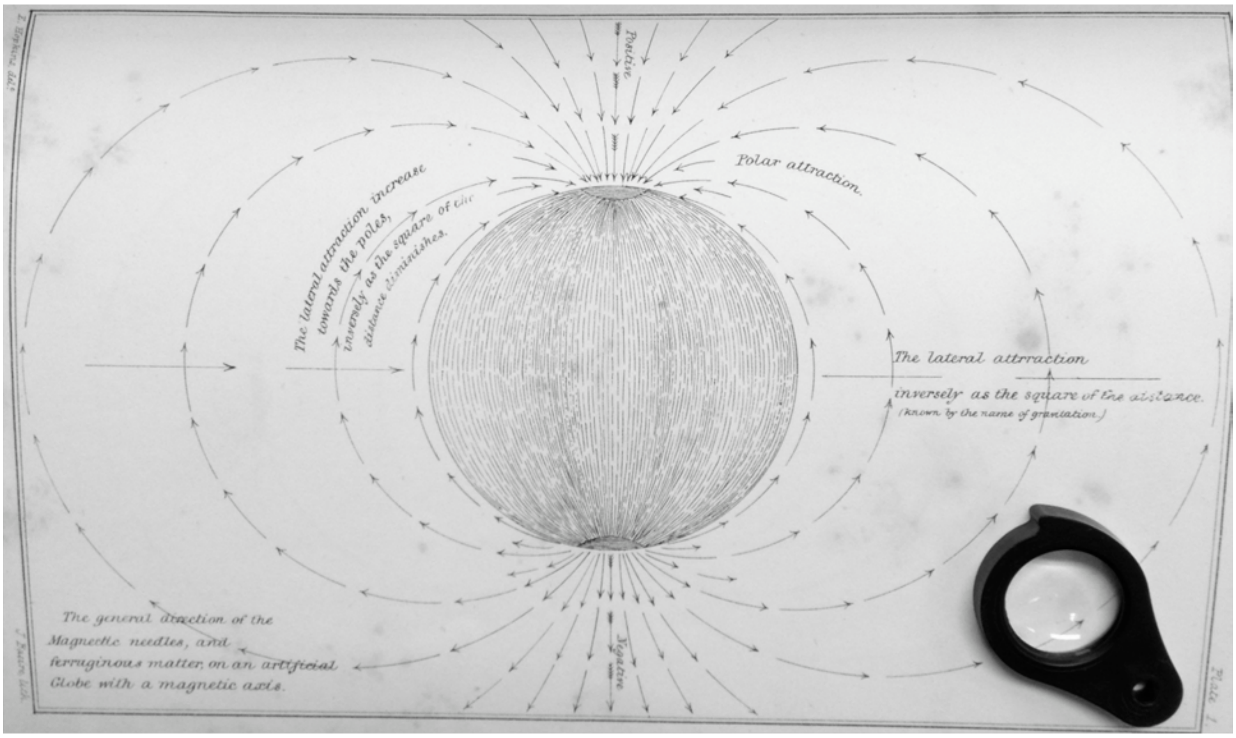


Figure 2. Hopkins' Plate 1. While normal gravitational forces draw toward the center, polar forces draw toward the North Pole. I think there is a drafting error on the right side of the globe: the term *lateral attraction* should be *inward attraction*. From Hopkins (1844).

Of course, the Earth's crust, including the continents, cannot simply move northward. Crustal material in the north must somehow be removed to make space, and crustal material in the south must be somehow added. Hopkins had to explain these processes, and he did it using the common assumption that crust was the same material on the continents and under the oceans.

Pioneer geologists James Hutton, John Playfair, and Charles Lyell had shown that both basalt and granite, that formed much of the crust, were formed from magma that had cooled and hardened. But Hopkins had not been convinced about plutonism, as this theory was called. Hopkins was a neptunist, believing that the basalt and granite of the crust had crystallized from saturated aqueous solutions. By 1844, not very many geologists were still neptunists, but Hopkins certainly was.

Hopkins was primarily an ore geologist, concerned mainly with gold. Most of the gold and other ores that he studied had indeed crystallized from warm aqueous solutions, not from magmas. Although neptunism could not be directly applied to igneous rocks, his arguments were very sound and show much correct understanding of rock-deformation processes. The schists and gneisses that host many ore deposits, and indeed, form large parts of the crust, also recrystallized in equilibrium with such warm water solutions. But Hopkins went too far. He felt he could discredit most aspects of plutonism:

Hopkins 1844, p. 36-37.

In the first place, we have no proof of the existence of an igneous element. Granting its existence, it could not produce a solid, but merely the melting of a substance already formed; therefore there is nothing gained by such an assumed agent. Besides, it is extremely difficult to produce crystals from fusion, and those which are imperfectly formed are produced when in the act of sublimation. Siliceous and calcareous crystals have often been formed by art in the moist way, but never by igneous fusion. The crystals forming the primary base could never be imitated by fusion, even though every other necessary circumstance should concur, especially those with or without an intermediate prism, terminating with pointed pyramids at both ends, as those of quartz and calcareous spar. Even those rocks called ancient lava, such as basalt, trap, andc., are of the same aqueous composition as any other rock, their pores being always filled with mineral salts. There is not a single case to support an igneous doctrine; whereas by means of the natural solutions and the polar current, we can, not only account for, but imitate the natural productions. There is scarcely a substance known but what is either found naturally in solution, or may be dissolved in an aqueous menstruum. The apparent insolubility of quartz has given rise to some of the difficulties which have embarrassed geologists; but as silica is found in that state in the primary base, we need not trouble ourselves with the question how quartz may be redissolved.

We shall consider the ocean as the primary menstruum from pole to pole, – a compound of all the elements in solution through which the magnetic currents circulate. From analogy and by experiment, crystallization would commence at the negative pole, and would continue to form until its growth would extend to the positive pole in meridional lines, thus producing the polar grain or lamination explained in the previous chapter. [See Fig. 3.] In the primary rocks we recognise in every crystal the action of the constant and undeviating laws of the polar force and chemical affinity, giving to the mass a regular grain, and to every crystal a definite form and composition. Hence the above may be considered an experimental and natural truth.

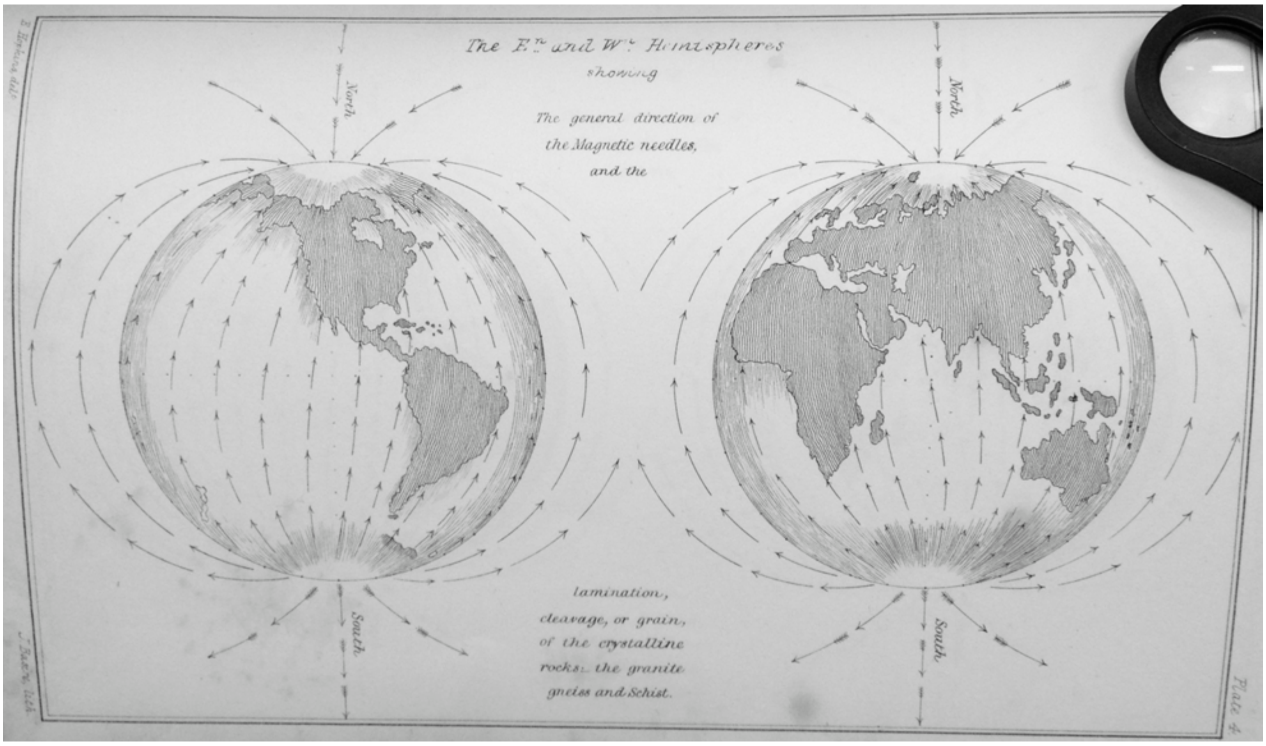


Figure 3. Hopkins' Plate 4. The oriented grain of the crystalline rocks seems to show that they have been modified by the polar magnetic attraction. From Hopkins (1844).

Although Hopkins argued that ancient basalt (trap) had not come from melt, he could readily accept that modern volcanoes do erupt melt, and that modern basalt is formed in that way. Ancient and modern basalt do not look identical, because ancient basalt is generally somewhat altered. To explain modern volcanism, Hopkins claimed (incorrectly) that subterranean heat is only local, produced by exothermic chemical reactions. In his theory, intense heating and even melting could occur where chemical reactions are especially active. This chemical explanation for the origin of volcanic heat was as good as any; radioactivity and the heat that it generates would not be discovered for another half a century.

For Hopkins, the Earth's crust forms through relatively cold chemical processes, as he described and illustrated here:

Hopkins 1844, p. 39-40.

The granite being the fundamental base, or the crystalline shell of the globe, its thickness is not known. It has a polar structure, and when the quantity of mica is considerable, granite divides into parallel plates, or in other words becomes laminated, and exhibits the meridional structure explained above.

Gneiss is the laminated part of the granitic base, the same identical mass; the distinction being produced by the ingredients tending to arrange themselves in parallel plates; quartz follows quartz, felspar follows felspar, and mica follows mica. (See Plate VI). [See Fig. 4.]

As this crystalline arrangement and lamination of the fundamental is produced by the continual circulating action of the magnetic currents through the semifluid mass, the transition of the crystalline aggregation to the laminated structure is necessarily insensible; the action being like a simultaneous growth of the granite northward. Hence a micaceous granite produces micaceous gneiss, chlorite granite, chloritic gneiss, andc.

Schist, or Crystalline Slate. — This variety forms the termination of the granitic base, the branches and leaves, as it were, of the great granitic trunks. The mica granite passes first into gneiss, and the latter into mica schist by an almost imperceptible gradation. This rock has been represented as stratified by a mistake in confounding the stratified with the laminated structure. (See Plate XVI.) It is the final decomposition of the felspar that distinguishes slate or schist from gneiss. (Plate VI.)

It will therefore be observed that the primary crystalline, from the granite to the schist, belongs to one formation, and is essentially composed of the same minerals, variously modified by the polar force, and passes by insensible gradation from the base to the final slaty structure in a more or less vertical and meridional direction; but subject to constant changes and disturbances from local causes.

These rocks are very extensively developed in South America, and may be traced from Chili to the Caribbean sea. A section was taken across the three Cordilleras, where the rocks were seen cut by ravines upwards of 2000 feet deep, thus exhibiting natural sections, and showing the nature of their transition vertically as well as horizontally; the minute, and very laborious investigation of which is the foundation of the present observations. The crystalline series in Europe falls into insignificance when compared with those of America, and it is in such extensive areas that the real character of the crystalline base can be clearly ascertained.

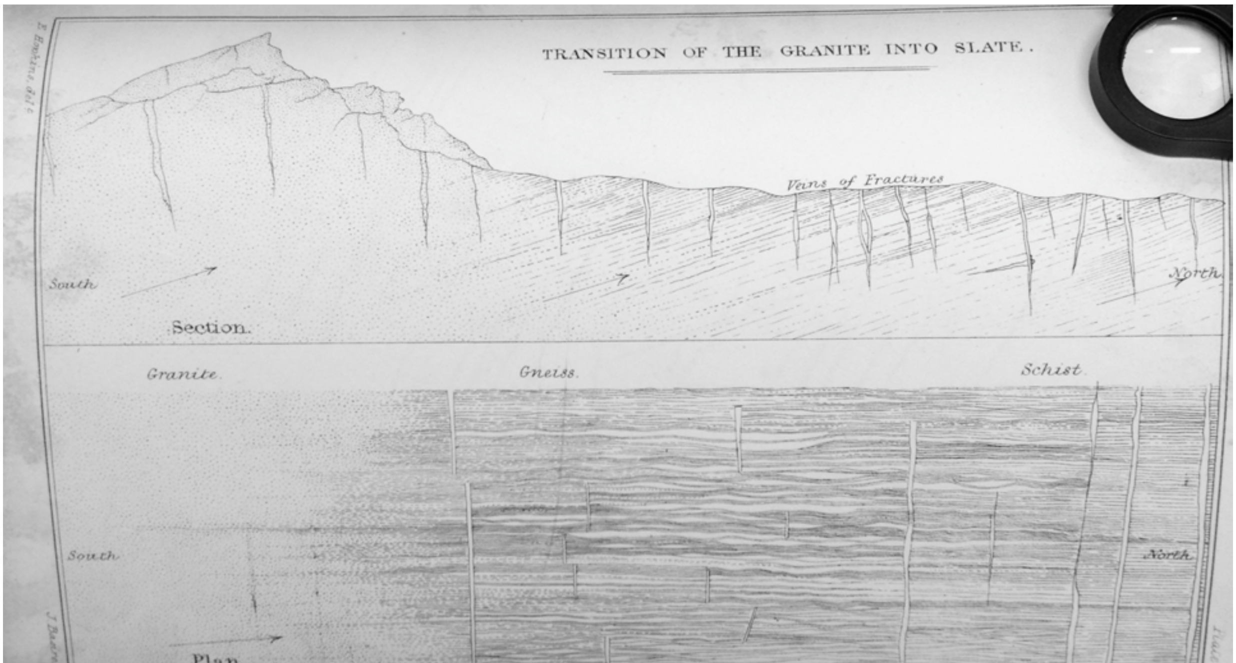


Figure 4. Hopkins' Plate 6. The upper drawing is a vertical section, and the lower drawing is the same, but cut horizontally. In Hopkins' drawings, the massive coarse-grained *Granite* is progressively transformed from south to north; first to coarse-grained *Gneiss* with oriented minerals, and then to finer grained and more micaceous *Schist*. In modern geology we know that such changes can take place, but not in a south-to-north transition. From Hopkins (1844).

"WE HAVE NO NEED TO INVENT STRANGE HYPOTHESES"

Hopkins developed his unifying theory in great detail over several chapters, and then summarized it in a late chapter of the book as follows:

Hopkins 1844, p. 110-112.
GENERAL OBSERVATIONS

If we now take a general view of the effects of terrestrial magnetism in combination with other secondary agencies really existing, we find a sufficient natural cause to explain all the characteristic changes which have been observed in the earth's condition, in the degree, combination and sequence which actually belong to them. Each of the phenomena, taken singly, is capable of demonstration in all its details of circumstances by the operations of terrestrial magnetism in connexion with some special branch of natural science.

First. We have ample proofs of the existence of the magnetic fluid enveloping our globe, and that it has two points of convergence, which we call poles; that this fluid has a motion from the south to the north pole, and has an influence on all matter, causing all bodies to fall towards the earth, which we call attraction of gravitation, and also tends to cause bodies to arrange themselves in a meridional direction, called polarity, as shown by the magnetic needle; and that the latter action tends to propel all matter northward; and finally, that it acts both mechanically and chemically on all matter.

Secondly. This northerly movement is observed in the ocean, which is found to carry all substances that happen to float in it from the south to the northern regions.

Thirdly. The grain of the primary crystalline presents a polar structure in all parts of the world, thus showing the universality of the action. The modification in the transitions of the different rocks, the elongation, fractures and dislocations, show the general northerly movement of the whole surface.

Fourthly. Volcanoes and earthquakes appear to be the effects of the chemical action and meridional force of the magnetic currents.

Fifthly. The formation of mineral veins, their general character, order, and numerous dislocations prove the action of a polar force; the constant operation of which is essential to account for the observed subterranean phenomena.

Sixthly. This northerly movement co-exists with the formation of our globe; it is the increased density of the currents at the poles which is the cause of its oblate figure; and it perpetually changes the surface of the earth, by bringing the consolidated masses, as they are formed in the southern hemisphere and other parts of the globe, towards the north pole, and thus exposing them to the temperature of different zones; consequently we find the relics of the southern in the northern hemisphere. Such series of beds are never found in the south, nor is the order of the deposits ever seen inverted. Therefore we have no need to invent strange hypotheses to account for the observed facts, but simply apply the natural operations of nature, *i.e.* the prime mover of terrestrial physics – magnetism, to guide us in our geological researches.

Hopkins argued: "we have no need to invent strange hypotheses to account for the observed facts." But his hypothesis of moving continents must have seemed strange to everyone except himself. Now we can see that it was a logical system that accounted for a great amount of unexplained climate and fossil facts, including the tropical rocks and fossils in Europe and Melville Island. The hypothesis of moving continents was the only way to account for them.

Hopkins got rather philosophical in the final chapter of his book:

Hopkins 1844, p. 123.

...the spot on which we have our existence will by the same harmonious law of nature, independent of the globe itself, ultimately decay, and be reduced to its primary elements at the north pole. [See Fig. 5, left side.] Great Britain, and other countries which are situated in the same parallel, will in a very few thousand years disappear from the surface of the globe, and other more southerly lands will take their place. Hence geology is not that crude, inconsistent and useless science which some have imagined it to be; on the contrary, it cannot be surpassed in its utility nor in the sublimity of its objects: not only is it next to astronomy, but it also forms part of one and the same system of physical operations; and besides, it instructs us "that we are placed in a part of a scheme – not a fixed but progressive one – every way incomprehensible; incomprehensible in a measure equally with respect to what has been, what now is, and what shall be hereafter."

Hopkins quoted here from a book by the bishop Joseph Butler in 1740. Butler was trying to find religious meaning in the processes of nature. Hopkins saw the progressive formation and destruction of crust as part of this larger scheme of nature.

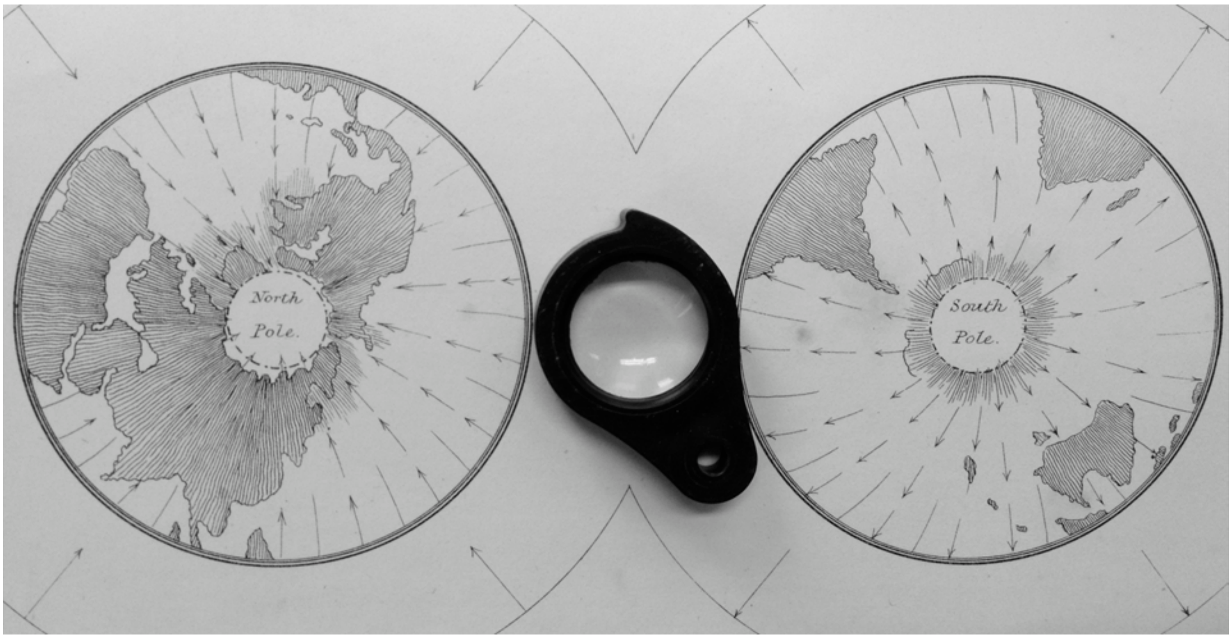


Figure 5. Hopkins' Plate 5. Crust forms in the southern hemisphere, and moves to the northern hemisphere where it subsequently dissolves. From Hopkins (1844).

In 1851 Hopkins published a revised and slightly enlarged second edition. The only deletion that I have noticed is the paragraph about Lyell. His theory was the same, but somewhat better explained, with additional illustrations and more clarifying statements. He added a few plates to show examples where aqueous solutions had created crust and where they had destroyed crust. In general, he argued that north-south oriented cleavage fractures tend to remove material from the rock, whereas east-west fractures are filled with secondary mineral deposits that add material. We can agree today with only some of these interpretations. Cleavage does often remove some volume of crust, and vein deposits do add volume. And he was correct that aqueous solutions in the rocks facilitate these changes. But modern geology does not confirm his claims that there are consistent global orientations of these fractures, or that crust is mainly created in the southern hemisphere and removed in the northern hemisphere.

Hopkins had a holistic theory for the development of the Earth, just as modern plate tectonic theory is holistic. His philosophical paragraphs are fascinating to read:

Hopkins 1851, p. 118.

The phenomena to which we shall now direct attention will remind us that everything in nature is in a perpetual state of change. There is nothing permanent but the laws and the harmony of the movements of the celestial system, and probably the dimensions of the heavenly bodies. If we regard the conditions of the beautiful and varied organic covering of our globe, the certainty of constant change is ever before us. The primary elements feed vegetable life, this again nourished the animal, and both perish to feed the future planet, and thus continue to supply the wants of successive generations.

Hopkins 1851, p. 119.

These variations, though hardly sensible from one generation to the next, become so great as to alter, not only the relative height of dry land, but also the latitude and longitude in the course of centuries. The surface of the higher lands is daily worn away by rains, and the mud, sand and organic remains are carried down to the valleys, andc., preparing sedimentary rocks for future generations.

Hopkins 1851, p. 121.

We have shown that the sea and all matter move from south to north, and that these great movements observed in nature may be imitated by means of an artificial sea, with an electro-magnetic axis. A crystalline film placed between two poles will receive new crystals from the liquid at the negative pole, whilst the same amount of the film will decompose and dissolve again into the liquid at the opposite pole; and thus the floating substance becomes perpetually propelled towards the positive pole, *i.e.* the south pole of an artificial magnet, which corresponds to the north pole of our globe.

In the preceding paragraph, and less well formulated on pages 68-69 of his 1844-book, Hopkins summarized his mechanism of moving continents across the globe. It is a global-scale process of electroplating, with the north-polar area acting as the anode and the south-polar area as the cathode; crustal material is added by crystallization in the south and removed by dissolution in the north. The seawater serves as the electrolyte that allows the dissolved ions from the north to move to the south for electro-deposition. The entire crust shifts as a unit.

It is remarkable that Hopkins could publish such an advanced theory in 1844. It had only been a few years since the process of electroplating had first been described. Electroplating was state-of-the-art science in Hopkins' time, but neptunism was passé.

To be overly kind to Hopkins' hypothesis, we might call it *electroplate tectonics*. A more reasonable and slightly derogatory name would be *neptunian crustal shift*. Lyell and other geologists seem not to have publically commented on Hopkins' hypothesis of neptunian crustal shift.

But was Hopkins' hypothesis ignored because of the neptunism, or because of the drastic idea of shifting continents? I feel that the neptunism alone would not have been reason enough to ignore his hypothesis. From his descriptions, discussions, maps, and illustrations, it is clear that Hopkins was a very talented geologist. It would have been easy enough for Lyell or other geologists to correct the neptunian mechanism of Hopkins' hypothesis, and use the rest. To appreciate that neptunian interpretations can include valuable geologic information, we can look at Hopkins' Plate 16. There he drew two geologic cross sections of the same hypothetical situation, one with a neptunian interpretation, and the other with a plutonian view (Fig. 6). They are indeed very similar. Plutonists such as Lyell could easily use geologic information that had been misinterpreted by a neptunist.

I think that Hopkins' hypothesis was ignored because the idea of global-scale translation of crust, by steady-state creation of new crust and destruction of old crust, was too extreme. This wild idea is what made our modern plate-tectonic hypothesis so revolutionary.

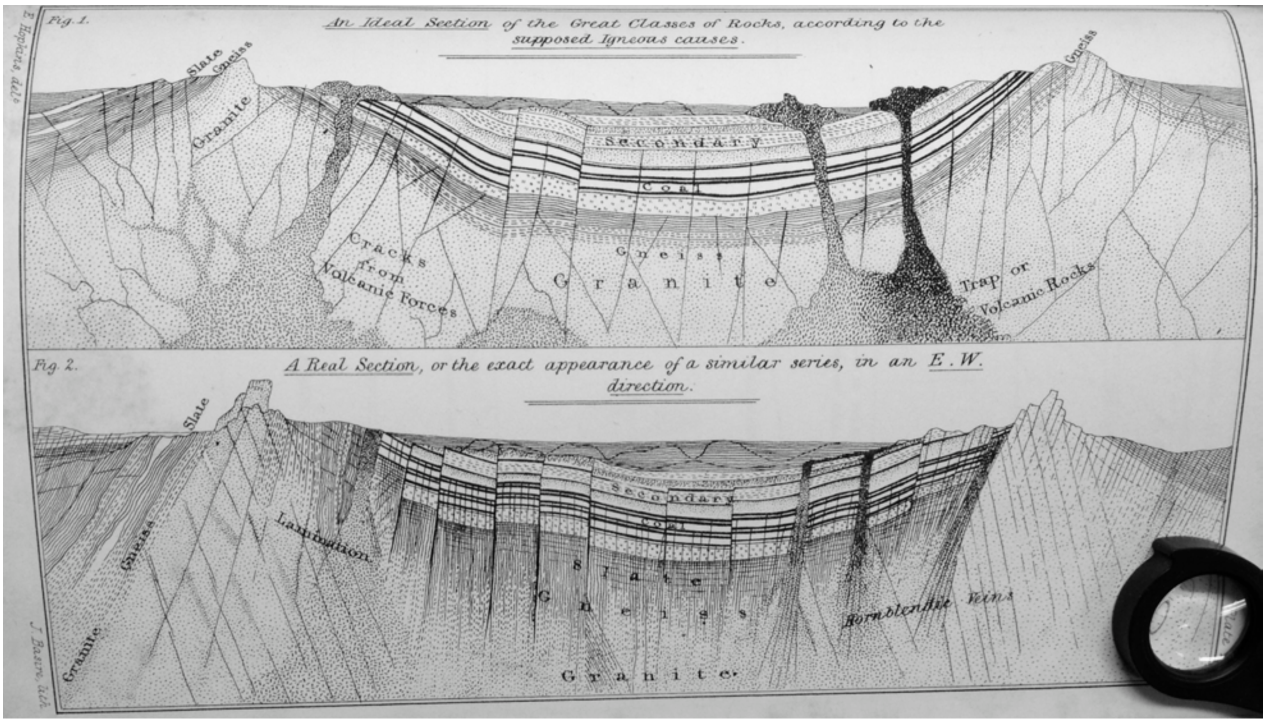


Figure 6. Hopkins' Plate 16. Two interpretations of the same rocks. The upper section is drawn according to plutonism, which Hopkins labeled "Ideal.". The lower section is drawn according to neptunism, which for Hopkins is "Real." For Lyell and for modern geologists, the upper section would be the real one. Although one of the above interpretations must be incorrect, they both contain valuable information. From Hopkins (1844).

Hopkins' book received favorable reviews in scientific journals of his day. Mining engineers valued it for the detailed descriptions of ore deposits and ore-forming processes. Records show that Evan Hopkins participated in geological meetings in London, and we can be sure that Charles Lyell was aware of Hopkins' explanation for the tropical plants on Melville Island. I have found no evidence that Lyell ever commented on Hopkins' suggestion that continents and fossils had moved northward. I suppose that he did not want to give Hopkins' foolish theory undeserved exposure.

THE SNIDER-FIT OF CONTINENTS BORDERING THE ATLANTIC OCEAN (1858)

Hopkins argued that continents moved together with the entire crust of the Earth. In 1858, another hypothesis of continental movement appeared. Antonio Snider-Pellegrini (1802-1885) published a set of two dramatic globe maps (Fig. 7) indicating that the Atlantic Ocean had formed when a continent had broken up and separated. On these maps, South America is labeled as *Atlantide* or the lost continent Atlantis. Australia was originally connected to western Africa, so all continents (except the relatively unknown Antarctica) were once united.

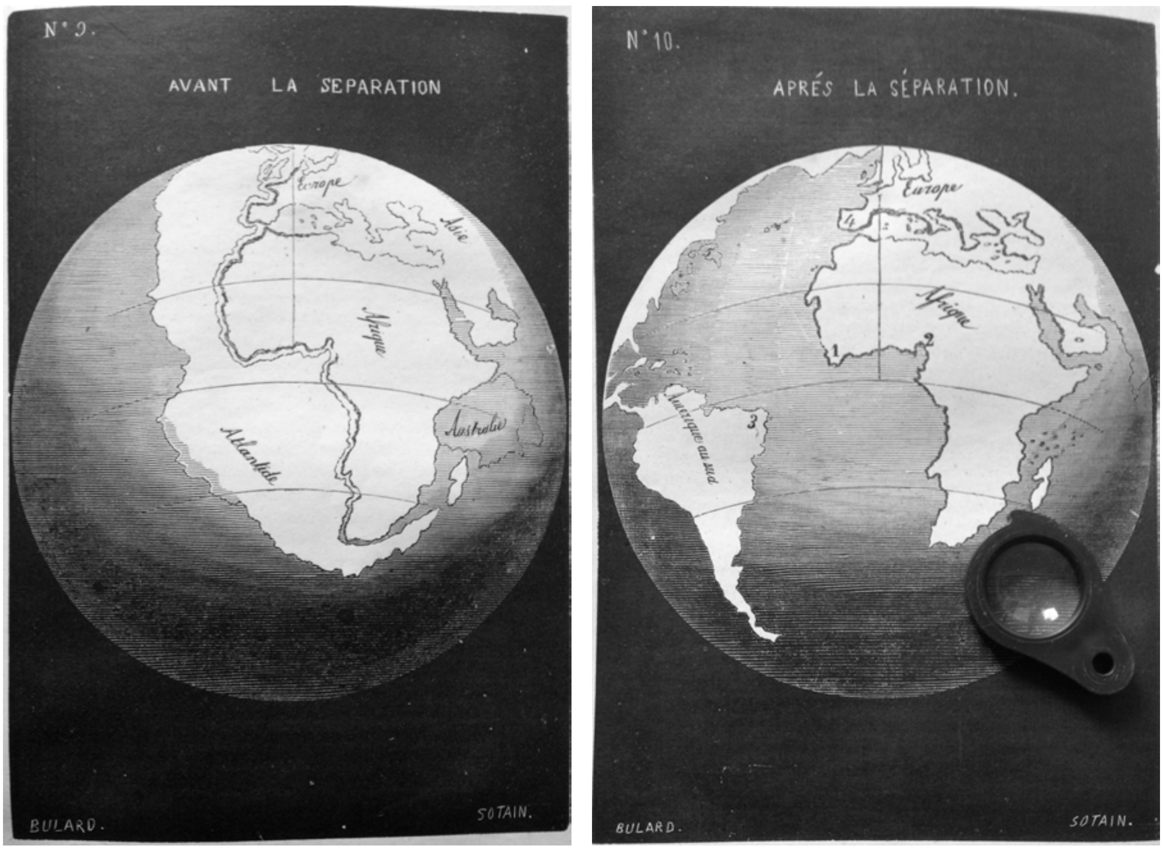


Figure 7. Snider's Plates 9 and 10, showing the continents before and after the formation of the Atlantic Ocean. From Snider (1858).

Snider-Pellegrini was an Italian-born business agent who operated between France and America. He wrote several unorthodox books under the pen name A. Snider. *La Création et ses Mystères Dévoilés* expressed many remarkable ideas spanning the fields of theology, geology, anthropology, and philosophy.

There were several important mysteries at that time yet to be unveiled. Ideas of transmutation and transformation, eventually known as evolution, had been around for decades, but the evidence was not convincing. Darwin had not yet presented the correct mechanism of species evolution.

Without Darwin's theory, and with no understanding of geological time or the sequence of rock strata, Snider worked within the paradigm of Old Testament mythology for the formation of the Earth. It is stated in the Bible that creation was accomplished in six days. That time span could easily be disproven by geology, so Snider followed other biblical apologists in simply changing those six days to six epochs; each of them lasting for as much as 36,000 years.

Thus the biblical flood was a cataclysmic event that took place in the fifth epoch of creation. How could there suddenly appear enough water on the planet to generate such a flood, and what became of all this water afterward? Snider proposed that not only the atmosphere and oceans, but also the solid Earth was involved. His arguments are somewhat unclear to me, but it seems that the water exploded outward from within the Earth, accompanied by great earthquakes. The Earth cooled during this flood, and its diameter was reduced from 12,000 leagues to the present 9,000 leagues. The single landmass of the Earth, shown in his Plate 9, cracked and separated into the continents that we now have. The eastern lands, but not the Americas, were swept over by this flood of water. Thus, North American Indians survived the flood and were not descendants of Noah. Maybe that idea helped Snider accept the mistreatment of the Indians that he had been witness to.

Snider used scientific evidence and logic as much as possible. He supported his theory of an original supercontinent by citing the recent discoveries of similar fossils in America, Europe, and Asia. He contended that these animals lived and died before America had separated from Europe and Africa. This hypothesis has something in common with modern plate-tectonic theory. But in fact, Snider's fossil distributions are not related to continental movements. Snider referred to the mastodon, mammoth, and other fossils of Pleistocene age. They are geologically young, and did not exist when the European and North American continental masses broke apart. These animals simply walked between Asia and North America across the Bering Land Bridge. A continental connection existed there during the Pleistocene ice age. At that time, so much ocean water was stored on the continents in the form of glacial ice, that sea level was about 100 meters lower, exposing a continuous continental shelf between Alaska and Siberia.

Snider did not discuss coal or plant fossils in the context of the opening of the Atlantic Ocean. Although Snider was correct in suggesting that there was a single large continent and that fossil evidence could support this hypothesis, the specific fossil evidence that he used was irrelevant toward this conclusion.

Snider was not a geologist, or even a scientist by profession. But Snider had great respect for the scientific method and the value of multiple working hypotheses. Although he believed in creation, he was writing scientific ideas, not religious ones. In his Preface, he did not hope that God would approve of his writings, but that history's great scientists would have approved. The following quotes and statements show this scientific attitude:

Snider 1858, p. 12-13 (translated here from French).

I sense that I have taken too many liberties in dealing with the philosophical parts of creation. At least I forced myself, as much as possible, to follow the time periods of the Bible.

If one of my ideas does not seem sufficiently proved, considering the large number of assumptions, it will be no less useful for subsequent research.

“As long as we are not given complete certainty,” said the English scholar Newton, “it is advisable to tolerate hypotheses, applying them only among the probable things.”

Euler, the famous German also said: “I do not think that taking great liberties when proposing hypotheses is harmful to the understanding of truth; because I am convinced that it is only after we test our theories by trying many hypotheses that we are allowed to arrive at the truth.”

The French philosopher Descartes, in dealing with magnetism, gave a striking example of the benefit of hypotheses.

Finally, the American Franklin, my fellow countryman, said: “I consider a theory always useful when it classifies the facts methodically. It will only be a hypothesis, but an indispensable hypothesis that will clear up a large number of known facts.”

Thus, whatever the boldness of some of my hypotheses, I am convinced that these great men would have approved them. I am sure that if Franklin lived, he would share my opinion on the formation of America and on the origin of its first inhabitants.

A. Snider.

Snider began his interpretations by accepting certain dogma. He then tried to apply scientific facts and reasoning to achieve new results. This is the typical procedure also for scientists, who generally begin with scientific doctrine, not religious dogma. Most researchers will accept the doctrine of their science, and not get involved in questioning or testing it. That would usually be a waste of valuable research time; time that could be better spent making further scientific progress.

Snider's map, showing the fit of continents around the Atlantic, was drawn by Charles Bulard, a scientific illustrator for French astronomical observatories. Bulard could be considered a planetary mapper. In scientific publications that same year, he accurately illustrated the Sun, the Moon, and the planets of the solar system.

It is a strange coincidence that a century later a man named Edward Bullard would publish a similar map (Fig. 8), that would convince many skeptics of the close fit of the continents and the likelihood of continental drift. His map was drawn by an EDSAC2 computer, not by the hand of a draftsman. For many people not yet familiar with computers, this suggested that the map was free of human bias. Bullard called it “The fit of all the continents around the Atlantic” but it soon became known as the “Bullard-fit”. Although Snider’s map in 1858 was drawn by Bulard, we will call it the “Snider-fit”.

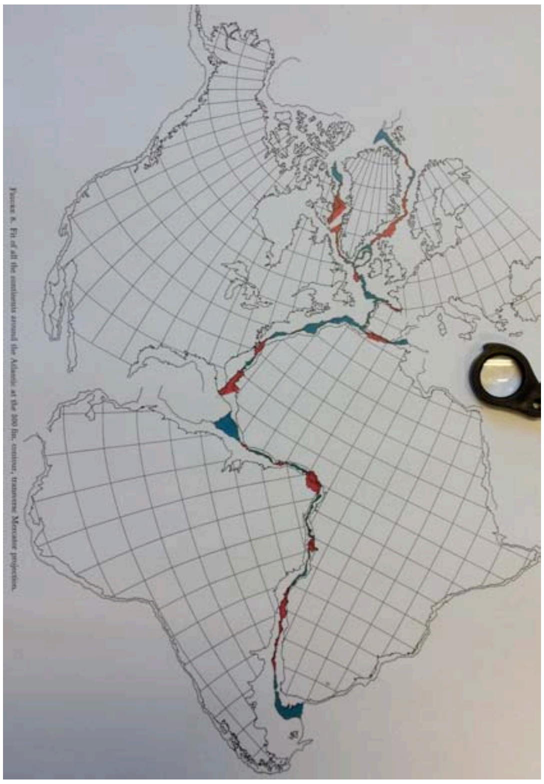


Figure 8. The Bullard-fit, an icon of the plate tectonic revolution. From Bullard et al. (1965)

JOHN HENRY PEPPER'S HYPOTHESIS OF NEPTUNIAN CONTINENTAL DRIFT (1861)

Snider had expressed hope that his bold hypotheses might prove useful for subsequent research. The Snider-fit did indeed prove useful. It appeared as a key figure in a science-education book of richly detailed Victorian style, with the equally detailed title: *The Playbook of Metals, Including Personal Narratives of Visits to Coal, Lead, Copper, and Tin Mines; With a Large Number of Interesting Experiments Relating to Alchemy and the Chemistry of the Fifty Metallic Elements.*

The author, John Henry Pepper (1821-1900), was usually referred to as Professor Pepper. He was a renowned science performer at the Royal Polytechnic Institution in London. He had a repertoire of hundreds of demonstrations involving all branches of science and technology. His performances were entertaining and always educational. He had no sympathy for tricksters, and he liked to expose deceptive magic and conjuring tricks by explaining the science behind them.

Pepper was also a science author. His first two major books were *The Boy's Playbook of Science* in 1860 and *The Playbook of Metals* in 1861. He called them playbooks, as he meant them to be enjoyed, but they were full of sophisticated science. They were very popular, found in essentially all secondary schools in Great Britain.

The *Playbook of Metals* includes lots of geology. The first 115 pages and 71 figures deal with coal, especially its formation and the techniques of mining it. Pepper's geological explanations are as correct as those of any geologist of his day. The book describes the geological environment of the coal deposits, the stratigraphy and fossils of the coal and associated rocks, and the methods of exploration and mining.

I am inspired not only by Pepper's keen scientific mind, but also his warm heart. He described the harsh conditions endured by mine workers, especially women and children. And he pointed out the reluctance of industrial leaders to making changes, not only social improvements, but technological ones that would have reduced waste and pollution and thereby increased profits.

Pepper had taught himself geology, mostly by studying the books of Lyell. He had also discussed geology at length with Evan Hopkins. In his Preface, he thanked Hopkins for personal instruction. Pepper was aware of three critical problems concerning coal geology: 1. Coal fossils indicate that England previously had a tropical climate, which had led Hopkins to claim that the Earth's crust had moved. 2. Coal plant fossils in North America were of the same species as England, which would be easy to explain if there had been a previous land connection. 3. Sedimentary deposits indicated that a continent had existed east of the present coast of North America during the time of coal formation. Hopkins had not written about these last two problems, but Lyell had done so, and Pepper had read and understood Lyell's writings.

Pepper had access to the little-known book by Snider, and realized that a modification of Snider's hypothesis could explain all three of these geologic problems. Pepper had Snider's eye-catching globe maps redrafted (Fig. 9), and placed them near the beginning of his chapter on coal, indeed, near the beginning of the book itself. Here is how he began the presentation:

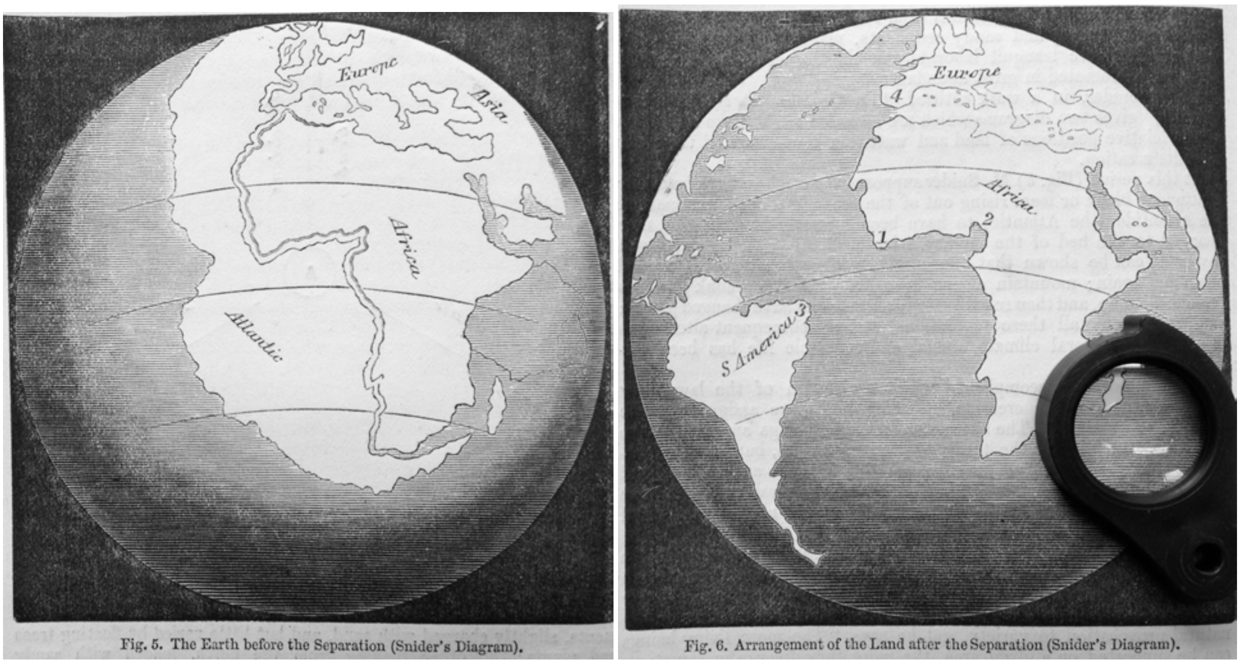


Figure 9. Pepper's Fig. 5. *The Earth before the Separation (Snider's Diagram)* and Fig. 6. *Arrangement of the Land after the Separation (Snider's Diagram)*. From Pepper (1861).

Pepper 1861, p. 9.

M. A. Snider, in a work entitled ”La Création et ses Mystères dévoilés,” gives two diagrams which are intended to show the alteration of the relative positions of land and water on the surface of the globe since its creation.

At this period (Fig. 5) M. Snider supposes the earth to have been one continuous block or mass rising out of the ocean, and the space marked the Atlantide (the Atlantic) to have been formerly dry land, but now changed to the bed of the mighty Atlantic Ocean. Sir Charles Lyell says, “It can be shown that the earth’s surface has been remodelled again and again; mountain chains have been raised or sunk; valleys formed, filled up, and then re-excavated; sea and land have changed places; yet, throughout all these revolutions, and the consequent alterations of local and general climate, animal and vegetable life has been sustained.

In this paragraph, Pepper followed the suggestion of Lyell, that the land mass had sunk directly down to form the Atlantic sea floor. Snider’s map does not really suit that interpretation, but Pepper wanted to also mention the hypothesis of Lyell.

We can skip over Pepper’s next two paragraphs. They are a digression, the first being a quote by Lyell on the apparent introduction of new species from time to time, and the second a long quote by Charles Darwin on the evolution of species. Pepper probably included these paragraphs to steer away from Snider’s creationism. He was clearly eager to mention Darwin’s exciting new hypothesis. He then went on to interpret the global changes in an alternative way, more in line with the ideas of Hopkins:

Pepper 1861, p.11.

The second diagram (Fig. 6) represents the same terrestrial globe after the division of its parts at the surface, with the formation of the North and South Atlantic Ocean. The great uniformity of the fossil plants of the coal measures of Europe and North America is a convincing proof of the former existence of a continent or chain of islands where the Atlantic now rolls its waves. Four-fifths of the fossil coal plants collected in Nova Scotia have been identified with European species; and there are also other geological proofs of the existence of an ancient land situated to the eastward of the present Atlantic coast of North America. 500 B.C. Herodotus mentions fossil fishes as occurring in the rocks of Egypt, and states this as a proof of that country having been formerly an arm of the sea, like the Red Sea. Without advocating the truth of the first diagram, enough has been said to indicate some of the points of the “drift theory,” by which it is attempted to explain the origin of coal.

The last sentence in Pepper’s paragraph above could easily confuse the modern reader. Although it refers to the “drift theory” this is not a theory of continental drift; it is a theory of drift-wood! The English coal deposits were known to be the product of tropical vegetation. According to the “drift theory,” huge trees had grown in the tropics and then floated as driftwood to the English shore, where they were buried and converted to coal.

Pepper had presented this driftwood theory in the previous pages, although he indicated that it was not the favored hypothesis for coal formation. He was trying to stimulate and educate schoolboys, and give them an understanding of science through the method of multiple working hypotheses. He explained why most geologists did not accept the “drift theory,” but advocated the “theory of submergence”; that tropical plants had grown in England and then been submerged and turned into coal. He wrote, in italics, "*It is assumed that there was a period in the history of our globe when a damp and steamy heat ... prevailed on the surface of the earth.*" He showed a figure depicting this type of tropical fern-tree forest (Fig. 10), and another figure showing some of the plant fossils that were the basis of these geological interpretations.

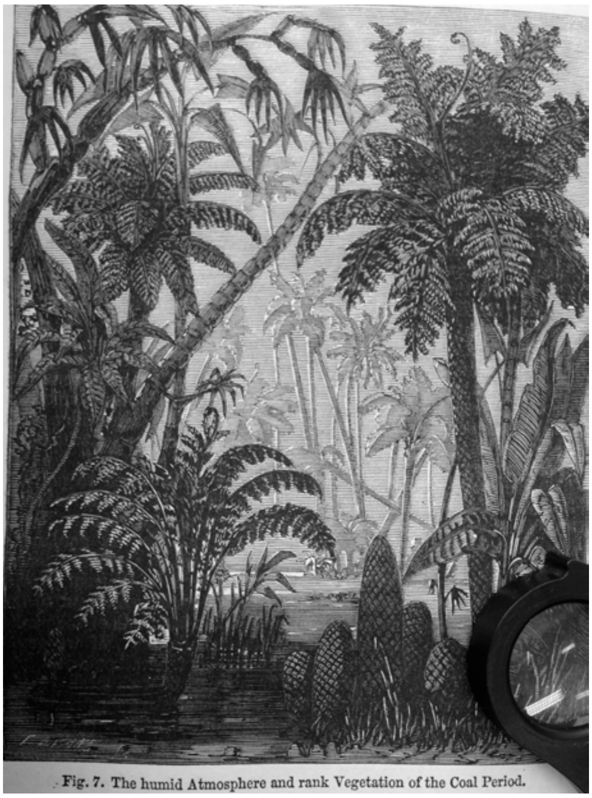


Figure 10. Illustration of the typical vegetation of the Coal Period. Pepper (1861).

We now continue Pepper's presentation, although to reduce its length, I have cut off most of his seven detailed arguments here after the first few words. Then comes the part of Pepper's text where he presented Hopkins' mechanism of moving crust across the surface of the globe:

Pepper 1861, p. 11-13.

The opponents of the "drift theory," it must be admitted, bring formidable arguments against it; and the following objections, arranged in a condensed form under seven heads, by the late Mr. G. F. Richardson, may not be uninteresting.

1. The purity of coal, and its freedom from extraneous substances. ...
2. The generally uniform thickness of each coal seam...
3. The exceeding minuteness of many of the coal seams, ...
4. On the other hand, the size of many of the coal seams considered with reference to the immense compression which they have unquestionably undergone, ...
5. The high state of preservation in which many of the plants occur,...
6. An additional objection to the drift theory is founded on chemical facts; ...

7. The multiplied instances of trees found erect, establish the fact of the coal plants having chiefly grown on the spot where they are now entombed.

Midway between the "drift" and "submergence" theories, it will be perhaps instructive to pause in order to mention a theory which has been urged with great power and ingenuity by Mr. Evan Hopkins, C.E. – viz., the actual movement of the crust or outer crystalline shell of the earth as it were in a spiral direction from the South to the North Pole; so that any given country like Great Britain shall, in process of time (to be numbered by thousands of years) have its position moved from a warm to a colder latitude by the mechanically-destructive and chemically-solvent power of water, aided by electro-chemical currents and crystallization, just as a plate of copper may be gradually dissolved at the positive pole of the battery, and again deposited at the negative pole to form an electrotype. Mr. Hopkins is entirely opposed to the Plutonic theory, and warns his hearers not to look at the strata of the earth through "red spectacles," but to admit a little more "soda water" (having a cooler and quieter agency in this globe than fire) into their speculations as to the formation of rocks. The arguments most forcibly used in favour of the idea of a movement of the earth's surface from south to north by the electro-magnetic currents are: 1. The changes of latitude which have occurred in various recorded instances.* 2. The result of the change of latitude – viz., change of climate. Hence it is urged that formerly England was differently placed, and enjoyed a tropical or warmer climate; and during that period the coal-plants grew and expanded into those gigantic proportions which seem to be the specialty of the flora of the coal-measures. Could elephants live in Siberia at the present time? yet the remains of these warmth-loving animals are found in abundance there. Wines were formerly made of the grapes grown in the open fields of England; and it is stated that when Cæsar invaded Britain 1915 years ago, the site of the city of London was in latitude 40° 30', and therefore in a climate corresponding to that of Portugal, which gives us our much-loved "Port;" whereas now we know that the latitude of Greenwich is 51° 28' 38''.

Pepper related Siberian mammoths to northward movement of continents, an incorrect suggestion that Snider had also made. And Pepper did not use the more relevant fossil and stratigraphic evidence that Hopkins had based his interpretations on. On the other hand, Pepper brought in Lyell's key evidence that fossil coal plants were identical in Europe and North America, which neither Hopkins nor Snider had noted. And Pepper realized that the identical species best suited a close connection of these continental masses. Pepper took the globe maps from the book by Snider, but he wisely ignored most of Snider's ideas and evidence. Pepper understood, as we do today, that Snider's maps were the most important part of his book

Pepper also understood that stratigraphy and fossils are the keys to geological interpretations. Recall that Hopkins had described a generalized stratigraphy of Europe:

No. 1. South frigid	The most ancient : – Cambrian and Silurian.
No. 2 South temperate . .	The Carboniferous, or the great coal formation.
No. 3 South tropic	Oolitic or Saurian group.
No. 4. North tropic	Cretaceous and tertiary of Europe.
No. 5. North temperate . .	Alluvial deposits of Europe.

Hopkins had not illustrated these stratigraphic units, but Pepper did. Therefore, I show some of Pepper's illustrations of these layers as they occur in England (Figs. 11 and 12).

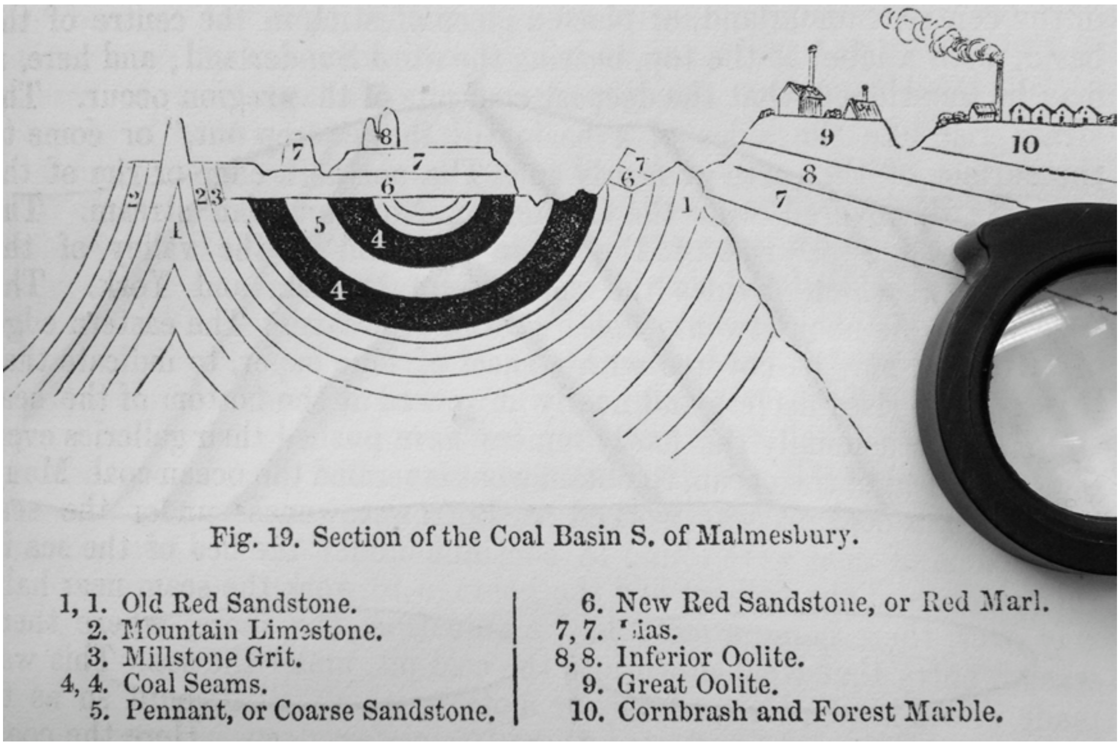


Figure 11. Pepper's figure showing the important stratigraphic units. These same units had been Hopkins' main arguments for northward displacement. From Pepper (1861).

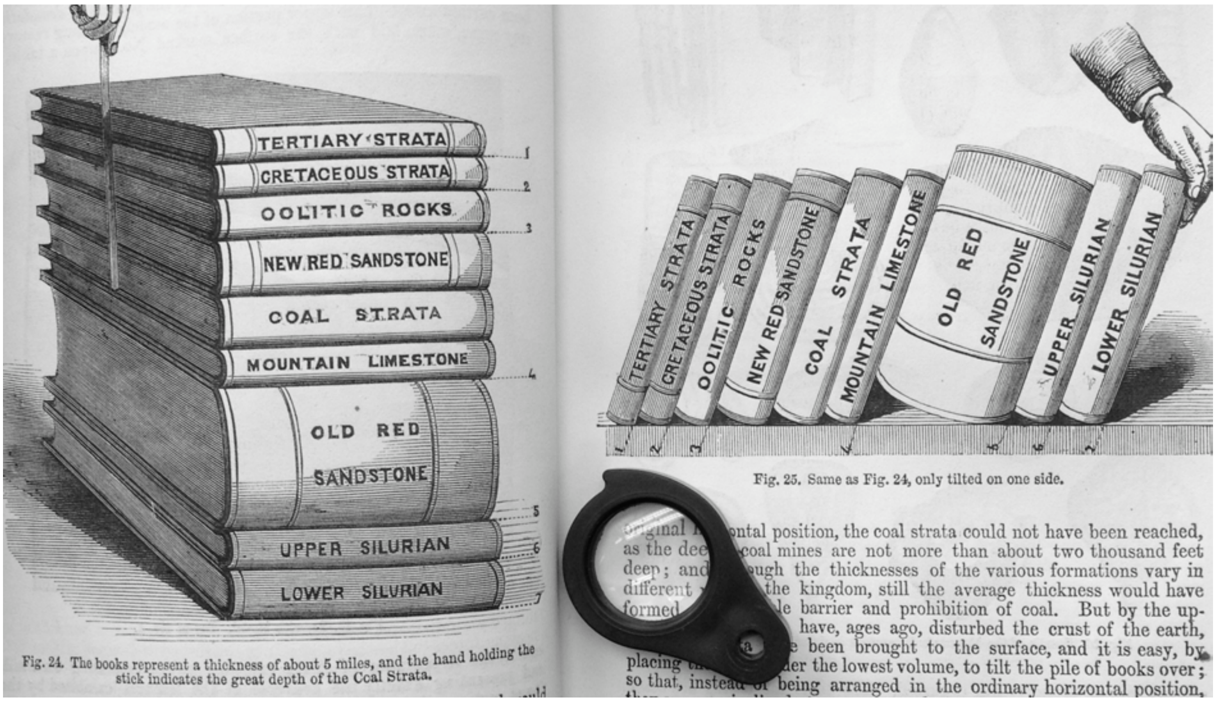


Figure 12. Pepper's clever figures to explain stratigraphy, thickness and depth, and why coal strata and other layers are exposed at the surface. From Pepper (1861).

The Mountain Limestone, just below the coal strata, also had fossils that indicated warm tropical seas. Pepper illustrated these fossils as well (Fig. 13):

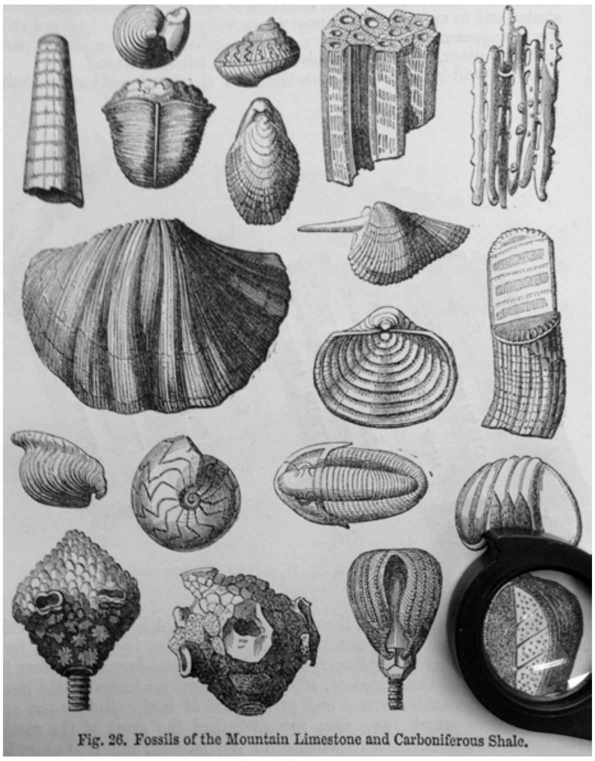


Figure 13. Fossils from a warm shallow sea at the time of the coal deposits. From Pepper (1861).

Having read Lyell and other geology books, Pepper knew that neptunism was not acceptable to modern geologists. He playfully teased Hopkins by writing that he "warns his hearers not to look at the strata of the earth through red spectacles."

Hopkins may have been a neptunist, but he had a clear understanding of the formation of mineral ores, especially gold, which is generally formed by precipitation and fluid-induced changes. These processes are more similar to ideas of neptunism than to plutonism. In a later chapter on gold, Pepper explained this as follows:

Pepper 1861, p. 181-182.

Having spoken at p. 177 of the eruption of melted rocks, it is right to bear in mind that many intelligent geologists ignore altogether the action of fire, and maintain that the gold was entirely deposited by electro-chemical decomposition. Mr. Evan Hopkins is one of the most determined opponents of the red-hot theories, and he remarks in his pamphlet on "The Geology of the Gold-bearing Rocks of the World," "When persons see the large masses of gold obtained from the surface of the quartz, or the edges of the primary slates, they are too apt to think that such productions are caused by melting or intense heat." Such ideas, Mr. Hopkins says, are incorrect, and from the circumstance of the metal being marked with the most minute striæ of the quartz, which contains the usual proportion of water, he maintains "that this is a state of things totally inconsistent with an intense melting action, but identical to that resulting from a battery and an aqueous solution."

In his performances and in his books, John Henry Pepper was popular for his abilitiy to understand science and make it fun. I appreciate also how he could deal with multiple working hypotheses. He often presented two competing scientific ideas and helped the reader choose the better of the two. He showed how to make better hypotheses by taking the useful parts of existing ones. He did not accept Snider's views on creation, but saw the importance of his globe maps. He did not accept Hopkins' neptunism, but saw that he was probably right concerning the formation of gold and other ores.

Pepper's hypothesis included the following concepts that are all considered correct today according to plate-tectonic theory:

- The existence of a supercontinent in Carboniferous time, when the coal measures were deposited. Even Australia was shown in this supercontinent.
- The opening of the Atlantic Ocean through the break-up and horizontal separation of parts of the supercontinent. The continents moved slowly, not catastrophically, and are still moving today.
- Valid fossil evidence – the identity of plant species in the coal deposits of America and Europe – to demonstrate the earlier connection of Europe and America.
- The gradual displacement of England, from tropical latitude during the deposition of the coal-bearing rocks, to its present northern position. The coal plants not only formed in a tropical climate, but at tropical latitudes.
- Motion of the entire crust, ocean floor as well as the visible continents. Continents move due to removal of old rock in front, and the addition of new rock behind.

Pepper's model correctly identified the amount of northward displacement of England since Carboniferous time. But the idea that Siberia had moved significantly northward since the time of the mammoths is not correct. That suggestion might capture schoolboys' imaginations, but was inconsistent with the speed and timing of the other crustal movements. We know now that mammoths lived in the Pleistocene, only 10,000 years ago. Considering the rough average of 5 centimeters movement per year for lithospheric plates, Siberia might have moved 50,000 centimeters or half a kilometer since Pleistocene time, not a few thousand kilometers. And England (with Europe) has only moved a hundred or so meters since the time of the Romans. But in Pepper's time the ages of the geological periods were not known, and such calculations were not possible.

The logic of Pepper's hypothesis was properly scientific and the continental mobilism he proposed could have served as a working hypothesis for the origin of coal. It was unfortunate that Pepper did not present more of Hopkins' stratigraphic and fossil evidence for continental displacement. And he did not mention that Hopkins had written a book where these ideas were more fully explained. Pepper did not weigh down his playbook with references.

The models of Hopkins and Pepper are different in important ways. Since Hopkins used the word shifting, and considered the continents and oceans to shift as a single crustal unit, I call his model *neptunian crustal shift*. Pepper's model is closely related, but since it involved separation of individual fragments, I call it *neptunian continental drift*.

Hopkins did not take the opportunity to modify his own theory after Pepper's book was published. Hopkins published a new edition in 1865, but did not mention the Snider-fit, or Pepper's modifications of his hypothesis.

Coal and metals were important to science and industry in Victorian England. Pepper's *Playbook of Metals* was a science-education bestseller. It was reprinted for more than 25 years, and distributed from London, New York, and Australia by the publisher Routledge. The early printings were dated: 1861, 1862, 1866, and 1869. After that, no dates were shown. Some of the later printings were claimed to be "A New Edition" although all the printings were exactly the same. The volume was given many different covers (Fig. 14). Copies were often bound in full leather, embossed with a school's emblem, and awarded at graduation ceremonies to the student who had most excelled in science during the year.

Figure 14. Pepper's *Playbook of Metals* (later called the *Boy's Book of Metals*) was bound in brightly decorated covers and spines during the decades that it was in print.

3. Former Continental Connections

"WHERE TH' ATLANTIC ROLLS, WIDE CONTINENTS HAVE BLOOM'D"

Pepper wrote that there existed "convincing proof of the former existence of a continent or chain of islands where the Atlantic now rolls its waves." Pepper had taken this phrase directly from Lyell's most recent edition of *Elements of Geology* (the title was changed to *Manual of Elementary Geology* in the 3rd, 4th, and 5th editions.) Here is how Lyell described the proof of a former continent where the Atlantic now rolls:

Lyell 1857, p. 387.

It was stated in the last chapter that a great uniformity prevails in the fossil plants of the coal-measures of Europe and North America; and I may add that four-fifths of those collected in Nova Scotia have been identified with European species. Hence the former existence at the remote period under consideration (the carboniferous) of a continent or chain of islands where the Atlantic now rolls its waves seems a fair inference. Nor are there wanting other and independent proofs of such an ancient land situated to the eastward of the present Atlantic coast of North America; for the geologist deduces the same conclusion from the mineral composition of the carboniferous and some older groups of rocks as they are developed on the eastern flanks of the Alleghanies, contrasted with their character in the low country to the westward of those mountains.

As continental highlands weather and erode, great amounts of sand are deposited in the low-lying areas or shallow seas nearby. The deposits are typically coarsest and most voluminous near the ancient land source, and they are thinnest further away. The sands contain abundant quartz and K-feldspar, proving that they are from continental crust. From such evidence, Lyell deduced that continental highlands had stood east of the present North American coast. He saw that the now-missing continent had shed sediments not only in Carboniferous time, but also in earlier periods. He mentioned these older sediments again, three pages later:

Lyell 1857, p. 390.

Similar observations have been made in regard to the Silurian and Devonian formations in New York; the sandstones and all the mechanically-formed rocks thinning out as they go westward, and the limestones thickening, as it were, at their expense. It is, therefore, clear that the ancient land was to the east, where the Atlantic now is; the deep sea, with its banks of coral and shells to the west, or where the hydrographical basin of the Mississippi is now situated.

Thus, Lyell had two independent lines of evidence – fossils and rocks – to support the hypothesis that Europe and North America had been connected across the Atlantic. He had already formulated these ideas, and presented them in nearly the same words, in his earlier book *Travels in North America in the Years 1841-1842* (Lyell 1845, p. 70).

Lyell's poetic expression "where the Atlantic now rolls its waves" was paraphrased from James Beattie's famous poem *The Minstrel*. The first stanza of the second part refers to the lost continent Atlantis, which sank into the Atlantic, according to the myths. Lyell gave the last three lines of this stanza in his book on *Travels in North America* (1845, p. 80). That particular stanza laments man's reluctance to accept change. Since a theme of my book is the reluctance of scientists to accept changes to their own interpretations, I print the entire stanza here.

Beattie 1774, book 2, stanza 1.

Of chance or change O let not man complain,
Else shall he never, never cease to wail:
For, from the imperial dome, to where the swain
Rears the lone cottage in the silent dale,
All feel th' assault of fortune's fickle gale;
Art, empire, earth itself, to change are doom'd ;
Earthquakes have rais'd to heaven the humble vale,
And gulphs the mountain's mighty mass entomb'd,
And where th' Atlantic rolls, wide continents have bloom'd.

The British geologist Edward Hull (1829-1917) wrote a book on the Carboniferous coal deposits, where he too incorporated Lyell's interpretation of a connection or chain of islands across the Atlantic (his term Central America refers to the central part of North America):

Hull 1861, p 157-158.

This great Carboniferous formation spread originally in one continuous sheet over the whole of Central America, probably from the flanks of the Rocky Mountains to the shores of the North Atlantic, and from the Gulf of Mexico to Newfoundland; and though we are unable strictly to define the original margin and limits of this great coal-generating tract, yet there is reason to believe, as has been pointed out by Sir C. Lyell, that land existed at that period where now rolls the Atlantic, and that the British Islands were connected with America by a chain of islands, or a tract of land, over which the plants of the Carboniferous period migrated and spread themselves in dense forests. Such an hypothesis seems the most satisfactory explanation for the remarkable fact, that the Carboniferous vegetation of America is identical, at least generically, with that of Europe; which could not have been the case under any of the received theories of the distribution of plants and animals, if these regions had been separated by wide barriers of ocean.

Moreover in tracing the Carboniferous strata, from Texas and Missouri on the south-west to the Alleghany Mountains and Nova Scotia on the east and north, we find a constant thickening of the sedimentary materials, such as sandstones and shales, which become both more abundant, and of coarser texture, as we approach the sea-board of the eastern states. This points to the position of the old land, from which these materials were derived, as having lain somewhere in the North Atlantic; and combined with the evidence derived from the vegetation, becomes almost demonstrative of the axiom, that what was land is now sea.

Hull mentions theories of the distribution of plants and animals. Since Darwin had shown that species were not created where they are found today, it had become interesting to consider the evolutionary origins of the different species, and how they had radiated to the various parts of the globe.

The Swiss-American geologist Jules Marcou (1824-1898) worked with the same ideas, and in 1860 he became the first scientist to publish a map showing a continent spanning the Atlantic Ocean. In his *Carte du Globe a l'époque jurassique* he showed the continents of America, Africa and Australia to be connected as the single land mass "Américo-Africo-Australie" (Fig. 15). He based his interpretations on Jurassic marine fossils. These fossils were much younger, and urelated to the land-fossils that Lyell and Hull had used as evidence for a similar continental connection.

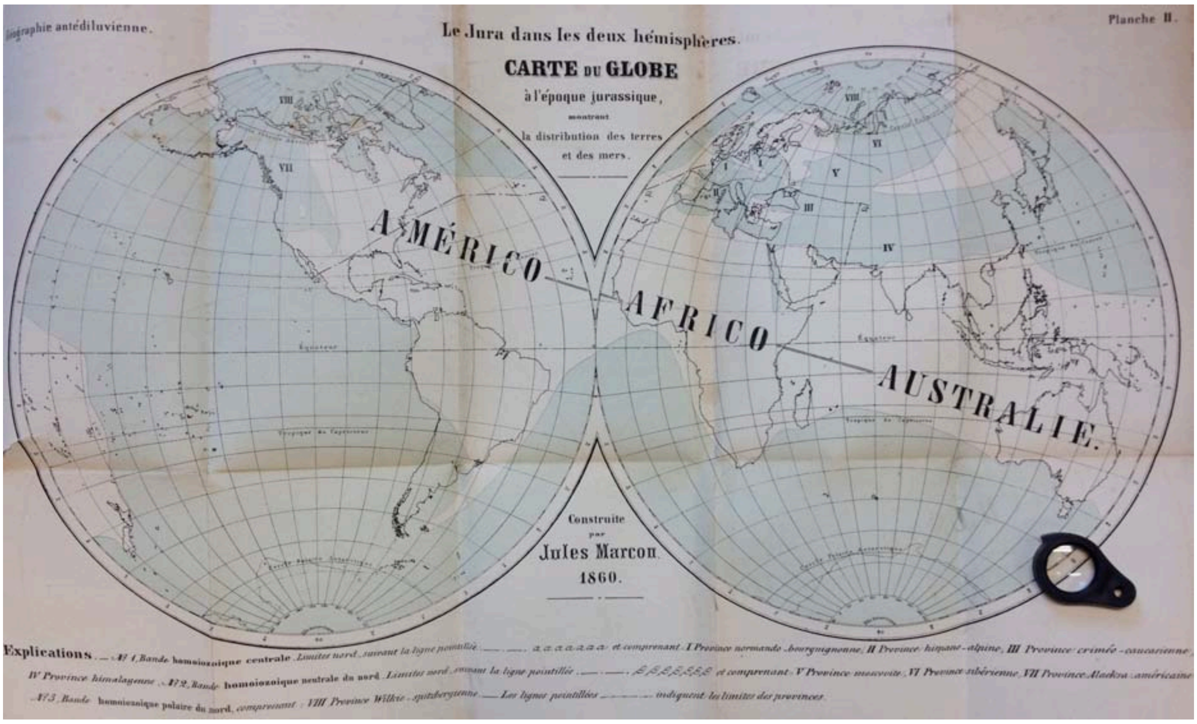


Figure 15. Marcou's Planche B. The Jurassic continent Américo-Africo-Australie. From Marcou (1860).

Note the great width of Marcou's connection. North America and Africa were united by a landmass, not a mere chain of islands. Marcou could deduce that the land formed a continuous barrier, because marine animals were not crossing from northern to southern seas. This connection would also satisfy Lyell's requirement of a large continental mass extending eastward from North America. The actual width of Marcou's connection was speculation, as there was no way of knowing how wide this former continent was. But Marcou had license to speculate; he was a respected geologist, both in Europe and America. A few years earlier he had published the first coast-to-coast geological map of the United States. His interpretations were sometimes controversial, but not to be scoffed at.

Soon many European scientists were working with the hypothesis of previous continents across the Atlantic and Indian Oceans. They based their interpretations on the distribution of various fossil species at different times. Land plants, land animals, and marine animals could all be used for these interpretations. Edward Hull published an article *The Geologic Age of the North Atlantic Ocean* (1885) with a map indicating that the “Continent of Atlantis” stretched from Canada to Greenland to Scandinavia in Carboniferous time. This continent shed sediments westward to North America and eastward to England, both in Silurian and Carboniferous times. The British paleontologist Alfred Jukes-Browne (1851-1914) used Hull’s colored Atlantis map as a frontispiece for his own textbook of historical geology in 1886.

The German paleontologist Melchior Neumayr (1845-1890) included many of these ideas in his 2-volume history of the Earth (Neumayr 1887). His Brazilian-Ethiopian Continent connected South America and Africa (Fig. 16). His Indo-Madagasic Peninsula, a similar land bridge that spanned the Indian Ocean, was already known by the name Lemuria (Sclater 1864). Lemurs are found on Madagascar but not in Africa, whereas fossils showed that lemurs had lived in India. It was presumed that the lemurs used the ancient Lemurian land bridge to get across the Indian Ocean.

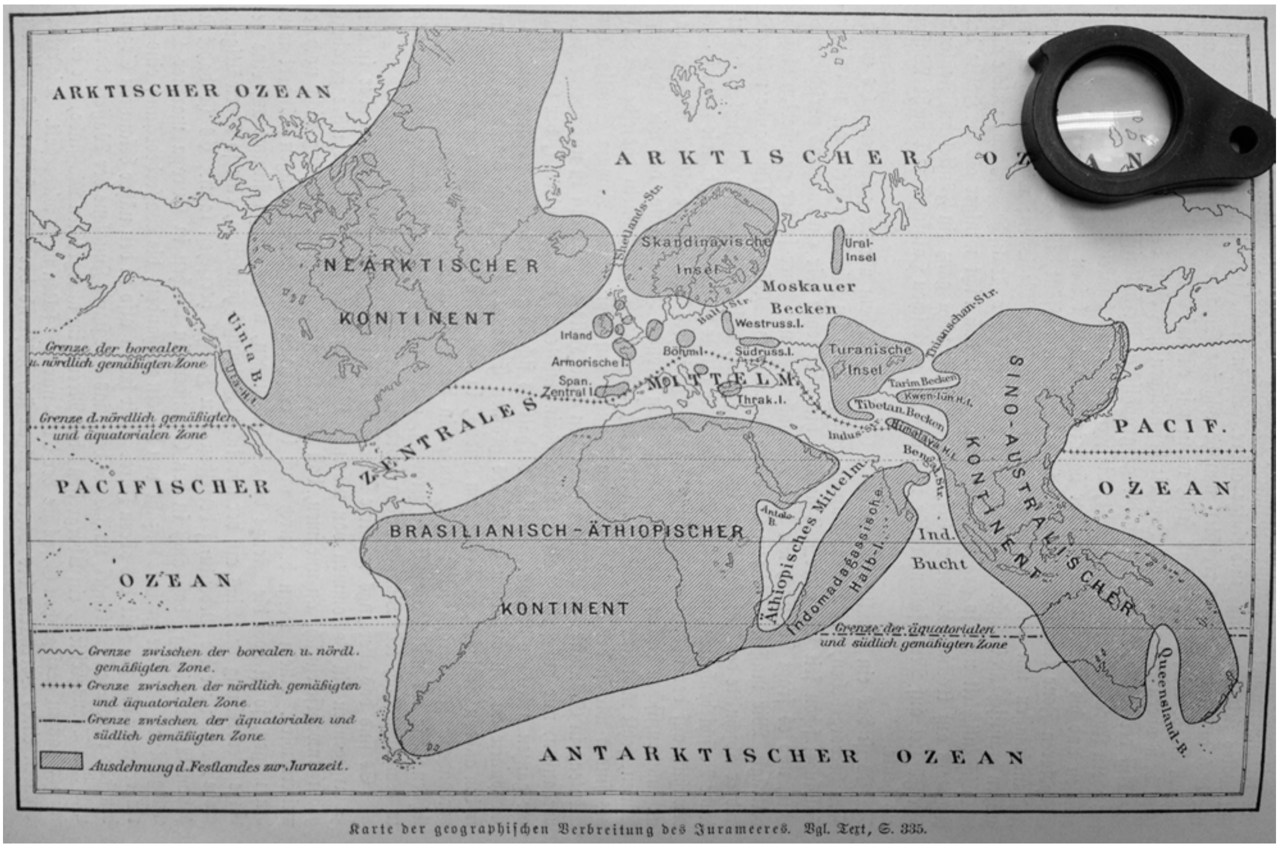


Figure 16. Neumayr’s illustration of the southern continent in his textbook *Erdgeschichte* (Earth History). His interpetation of Jurassic paleogeography shows two large continents, whereas Marcou’s earlier map had one. From Neumayr (1887).

The hypothesis that continental crust had sunk to form the depths of the Atlantic Ocean was in agreement with Lyell’s ideas of vertical crustal movement. No geologists were using a hypothesis of horizontal movement. Probably Lyell did not give that hypothesis much thought.

JAMES DWIGHT DANA'S CREATION-ORIGIN OF CONTINENTS AND OCEANS (1863)

Various land-bridge hypotheses were popular in Europe, but not in North America. James Dwight Dana (1813-1895) was strongly against such hypotheses. Dana was Lyell's counterpart, America's leading geologist and geology textbook author in the 19th century. He is best known today for his mineralogy textbooks, the most recent versions of which are still being used in universities throughout the world. But Dana wrote much more, including a geology textbook that could compete with Lyell's. Dana's first *Manual of Geology: Treating of the Principles of the Science with Special Reference to American Geological History* had nearly 800 pages and 1000 illustrations. It went through five editions, 1863, 1866, 1876, 1880, and 1895. Dana felt that North America had a special status among continents. He explained this in the Preface:

Dana 1863a, p. vii.

PREFACE.

Two reasons have led the author to give this Manual its American character: a desire to adapt it to the wants of American students, and a belief that, on account of a peculiar simplicity and unity, American Geological History affords the best basis for a text-book of the science. North America stands alone in the ocean, a simple isolated, specimen of a continent (even South America lying to the eastward of its meridians), and the laws of progress have been undisturbed by the conflicting movements of other lands. The author has, therefore, written out American Geology by itself, as a continuous history. Facts have, however, been added from other continents so far as was required to give completeness to the work and exhibit strongly the comprehensiveness of its principles.

Not only does North America stand alone in the ocean, but the locations of the Precambrian rocks and the Cambrian sediments deposited on them convinced Dana that North America had stood alone since earliest time. He presented a map of the Precambrian rocks, which seems to show that the shape of the ancient continent matches the shape of the modern one (Fig. 17). Beginning with its solitary placement, it had developed mountain belts along its margins. The higher mountains faced the wider Pacific Ocean and the lower mountains faced the narrower Atlantic Ocean. These features, and others, seemed to make North America an ideal continent.

Figure 17. Dana’s map of North America in Azoic (Precambrian) time. The white areas are Precambrian, and follow the general pattern of the Atlantic and Pacific coasts. From Dana (1863a).

Dana could explain in great detail how rocks, plants, animals, and landforms had developed through the ages. But he had no geological reason for the positions of continents on the globe. He had a reason, but it was not geological. It was theological. Dana had been brought up in a deeply religious family. As a mature geologist he would not abandon two important teachings of Genesis: that God had created Man, and that God had initially positioned the continents and oceans to make an appropriate setting for future civilization.

Dana abridged his *Manual of Geology* to make a more inexpensive version: *A Text-book of Geology, Designed for Schools and Academies* (1863, 1874, 1874, 1883.) Its explanations are more concise than the much larger *Manual of Geology* so I quote from them:

Dana 1863b, p. 76-77.

The large Azoic area on the map, p. 73. [See Fig. 17.], represents the main portion of the dry land of North America in the later part or at the close of the Azoic age; for it consists of the rocks made during the age, and is bordered, on its different sides, by the earliest rocks of the next age. It is, therefore, the beginning of the dry land of North America, the original nucleus of the continent, to which additions were made, in succession, with the progress of the ages, until its final completion as the age of Man was opening. The smaller Azoic areas mentioned appear to have been mere islets in the great continental sea.

Each of the other continents was probably represented at the same time by its spot, or spots, of dry land. All the rest of the sphere, excepting these limited areas, was an expanse of waters.

The evidence appears also to show that both waters and land were lifeless wastes, except it be that sea-weeds and Protozoans were in the oceans.

The facts to be presented under the Silurian age teach that the great, yet unmade, continents, although so small in the amount of dry land, were not covered by the *deep* ocean, but only by *shallow* oceanic waters. They lay just beneath the waves, already outlined, prepared to commence that series of formations—Silurian, Devonian, Carboniferous and others—which was required to finish the crust for its ulimate continental purposes.

We thus gather some hints with regard to the geography of America in the period of its first beginnings. It is stated, in Genesis, that on the third day the *waters were gathered together into one place, and the dry land was made to appear*, and also that, as a second work of the same day, *plants were called into existence* as the *first life* of the earth. The Azoic age in geology witnessed, with little doubt, the appearance of the first continents and probably of the first plants.

The outline of the northern Azoic area on the map, p. 73 – the embryo of the continent – is very nearly parallel to that of the present continent. The Azoic lands, both in North America and Europe, are largest in the more northern latitudes.

In the conclusions, Dana continued to explain his creationist interpretations:

Dana 1863b, p. 336-337 (also 1883, p. 394-395).

Geology may seem to be audacious in its attempts to unveil the mysteries of creation. Yet what it reveals are only some of the methods by which the Creator has performed his will; and many deeper mysteries it leaves untouched.

It brings to view a perfect and harmonious system of life, but affords no explanation of the origin of life, or of any of nature's forces.

It accounts for the forms of continents; but it tells nothing as to the source of that arrangement of the wide and narrow continents and wide and narrow oceans that was necessary to the grand result.

It teaches that strata were made in many successions as the continents lay balancing near the water's level, sometimes just above the surface, sometimes a little below; but it does not explain how it happened that the amount of water was of exactly the right quantity to fill the great basin, and admit of oscillations of the land beneath or above its surface by only *small* changes of level; for if the water had been a few hundred feet below the level it now has, the continents would have remained mostly without their marine strata, and the plan of progress would have proved a failure; or if as much above its present level, the land through the earlier ages would have been sunk to depths comparatively lifeless, with no less fatal results both to the series of rocks and the system of marine and terrestrial life; and in the end there would be broad and narrow strips of dry land and archipelagoes, in place of the expanded Orient and Occident.

Dana interpreted the relationship of water volume to ocean-basin volume to mean that God had determined it to be just so. But other geologists, and modern ones, consider it to be a simple coincidence. We skip over Dana’s next paragraph, which explained his view that Man's body, although very similar to the bodies of the great apes, had been created in the image of God. Then come two paragraphs that again relate to the rocks and continents, the “world-body.”:

Dana 1863b, p. 338-339 (also 1883, p. 395-396).

So with the earth, Man’s world-body. Its rocks were so arranged, in their formation, that they should best serve Man’s purposes. The strata were subjected to metamorphism, and so crystallized that he might be provided with the most perfect material for his art, – his statues, temples, and dwellings; at the same time they were filled with veins, in order to supply him with gold and silver and other treasures. The rocks were also made to enclose abundant beds of coal and iron for his utensils and machinery. Mountains were raised to temper hot climates, to diversify the earth’s productiveness, and, preeminently, to gather the clouds into river-channels, thence to moisten the fields for agriculture, afford facilities for travel, and supply the world with springs and fountains.

The continents were clustered mostly in one hemisphere to bring the nations into closer union; and the two having climates and resources the best for human progress, – the northern Orient and Occident, – were separated by a *narrow* ocean, that the great mountains might be on the remoter borders of each, and all the declivities, plains, and rivers be turned toward one common channel of intercourse. So, also, the species of life, both of plants and animals, were appointed to administer to Man’s necessities, moral as well as physical.

Dana had found divine reasons for the global positions of continents and the narrow Atlantic Ocean. Today we can argue that he was beginning with a religious doctrine, and forcing the evidence to fit that doctrine. But he did not see it that way. He thought that geological evidence proved there must be a God. Otherwise the Earth would not have been so well designed:

Dana 1863b, p. 339-340 (also 1883, p. 397).

It is hence obvious that the earth’s history, which it is the object of Geology to teach, is the true introduction to human history.

It is also certain that science, whatever it may accomplish in the discovery of causes or methods of progress, can take no steps toward setting aside a Creator. Far from such a result, it clearly proves that there has been not only an omnipotent hand to create, and to sustain physical forces in action, but an all-wise and beneficent Spirit to shape all events toward a spiritual end.

Man may well feel exalted to find that he was the final purpose when the word went forth in the beginning, LET LIGHT BE. And he may thence derive direct personal assurance that all this magnificent preparation is yet to have a higher fulfillment in a future of spiritual life. This assurance from nature may seem feeble. Yet it is at least sufficient to strengthen faith in that Book of books in which the promise of that life and “the way” are plainly set forth.

All four editions of this abridged textbook, from 1863 to 1883, contained these same paragraphs. But when Dana died, William North Rice (1845-1928) made a few minor revisions of the book for a 5th edition (1897). Rice was a Yale Ph.D., and although he was a Methodist minister, he was also an evolutionist. He removed Dana's arguments that a Creator had designed the Earth for man's purposes.

The first three editions of Dana's *Manual of Geology* (in 1863, 1876 and 1880) all contained the same creationist interpretations, but not so plainly. However, these books ended with a 6-page summary of Genesis, trying to make geological sense out of the seven days of creation. Dana removed this summary before the fourth and final edition was printed in 1895. But his views had not changed in his last years. In 1890 he published a separate little book entitled *The Genesis of the Heavens and the Earth, and All the Host of Them*.

Instead of concluding his final *Manual of Geology* with Genesis dogma, he concluded it with another doctrine:

Dana 1895 p. 1027-1028.

The idea – Continents always Continents – announced by the author first in 1846, has been affirmed by all that has come to light, and Geology now has, as regards North America, a record of the chief consecutive events in a continuous process of development.

Geologists and biologists could disprove the teachings of creationism and Genesis. But several geology professors who were already firm believers in creationism before Darwin's breakthrough, remained creationists throughout their lives. The most famous of these were Louis Agassiz at Harvard, and Arnold Henry Guyot at Princeton.

Most of Dana's colleagues and successors surely did not accept his creationism. But he was a brilliant scientist, and his opinions on all other geological matters were extremely influential. American geologists trusted his textbooks, and had no reason to doubt his doctrine that the North American continent had always stood alone in the ocean. It might also be mentioned that Dana had a book for the general public, with these same teachings. *The Geological Story Briefly Told,* was really not so brief, with its 263 pages and 245 figures. Europeans were not reading these books, and they were learning that North America had earlier been connected to the eastern continents.

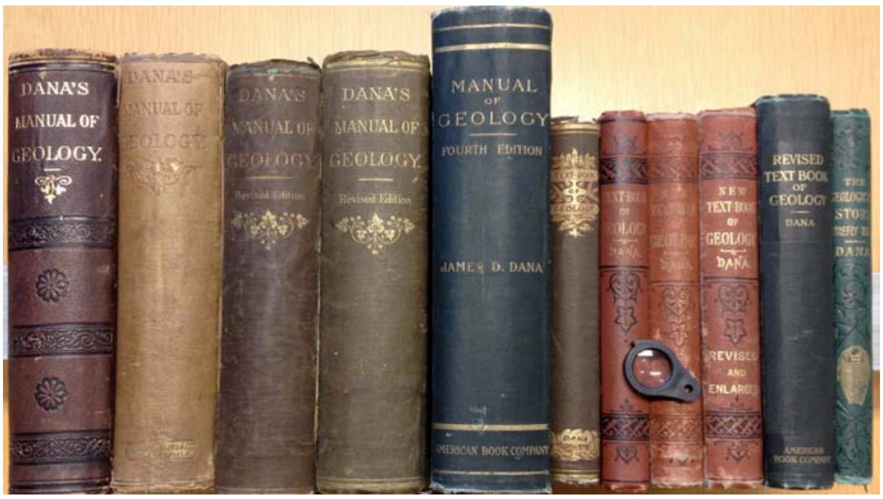


Figure 18. Five editions of Dana's *Manual of Geology* for colleges, five editions of his *Text-book of Geology* for schools, and *The Geological Story*, widely read by the general public.

HOT CLIMATE OF THE ARCTIC'S CARBONIFEROUS ROCKS

The occurrence of tropical coal plants in northern latitudes was a key piece of Hopkins' evidence that continental masses had drifted northward. Although his idea of horizontal displacement was far-fetched, he felt it was a simple way, and the only way, to correctly explain this data. Other geologists were aware of these problems, and were trying to deal with them in other ways.

In *Principles of Geology*, Lyell discussed the coal plants on Melville Island and the problem of understanding them. He took the problem seriously. But in later editions of his book he glossed it over a bit, since there seemed to be no way of solving it. The problem of tropical fossils in northern latitudes did not go away. New discoveries of coal had made it all the more striking. In his book on British coal-fields, Hull mentioned some new locations in the far north:

Hull 1861, p. 216.

Coal and lignite occur on Jameson Land, Banks' Land, and Melville Island. In Albert Land, in lat. 78°, Sir E. Belcher found bituminous schists with coal, and apparently connected with these strata, limestones with *Productus* and *Spirifer.* We have, therefore, ground for believing, from the monuments of the Carboniferous age, that our coal-vegetation extended into regions which are at present so inhospitable as almost to exclude the existence of vegetable life. How great and wide-spread the changes of climate, and how mysterious the cause!

The next important geology textbook in England was by Joseph Beete Jukes (1811-1869) and Archibald Geikie (1835-1924), who wrote:

Jukes and Geikie 1872, p. 510

...in latitudes where now sea and land are buried in ice and snow throughout the year, and there are several months of total darkness, there formerly flourished animals and plants very similar to those living in our own province in corresponding geological periods; and it would appear that similar animals and plants were then widely spread over the whole world.

They specifically rejected Lyell's hypothesis that an excess of land near the Equator would cause sufficient warming also near the poles. They pointed out that a climate specialist had refuted that hypothesis already in 1859. But Lyell was presumably not convinced, because he had not withdrawn or changed his suggestion in subsequent editions of his textbooks.

Jukes had died in 1869 (fell off a horse) and Geikie's later textbooks in 1879 and 1893 dropped this enigma. Scientific puzzles can be optimistically presented early in one's career, but if they remain unsolved, they become a distraction or even an embarrassment.

In Dana's first textbook, he also tried to find an explanation for tropical climate on Melville Island. He indicated that the warm climate in the Arctic was not limited to the Carboniferous, but had lasted through the Permian, Triassic and Jurassic:

Dana 1863a, p. 738.

…there is no reason to believe that there was any alpine or sub-frigid vegetation at Melville Island, or that the plants differed essentially from those of Pennsylvania. This warm climate of the poles was hardly less striking in the middle Mesozoic. For, while Reptiles are especially characteristic of the tropics, there were Ichthyosaurs and Teleosaurs in the Arctic.

It is clear that Dana had not heard of the hypothesis of Hopkins – that continents had moved northward, experiencing colder climates as they came into higher latitudes. Had Dana known of that hypothesis, he would have gladly mentioned it here, listing it as an additional "supposed cause":

Dana 1863a, p. 737.

Three causes have been presented to account for the cooling of the climate of the globe in past time: --

1. *The decreasing density and cloudiness of the atmosphere*, through a diminution of the proportion of carbonic acid and moisture.

2. *The increasing extent and height of the land.*

3. *The secular refrigeration of the globe.*

Two other supposed causes are sometimes brought forward:--

4. *A change in the earth's poles*. – Such an event would only change the location of the frigid zone or polar climate. When it has been proved that there was a polar climate anywhere in the Palæozoic ages, and what its location was, it will be soon enough to arrange this among possible causes. Astronomers deny its possibility.

5. *A passing of the earth through warm regions in space during its earlier eras*. -- This cause is so far within the region of the hypothetical as hardly to merit consideration until all others admitting of investigation have been proved insufficient.

Dana went on to discuss the causes 1, 2 and 3 in more detail, but he did not conisder causes 4 and 5 worth discussing. Hopkins' hypothesis would have been in that latter category, had Dana known of it.

This hot-climate problem was familiar to every geology textbook author. We can read how Joseph Le Conte (1823-1901) presented it. Le Conte was the authority in geology on the west coast of North America. He produced a college textbook (editions in 1877, 1882, 1891, 1896) that was also widely used, in competition with Dana's. Le Conte was fascinated by the clear evidence of tropical climate in north-polar regions. He explained the evidence, and used italics to emphasize this curious situation:

Le Conte 1896, p. 393-394.

Climate. – The climate of the Coal period was probably characterized by greater *warmth*, *humidity*, and *uniformity* than now prevails over the greater part of the earth's surface. Most of these characteristics, if not all, are indicated by the nature of the vegetation.

1. The *warmth* is shown by the existence of a tropical or subtropical vegetation. Of the present flora of Great Britain about one thirty-fifth are Ferns, and none of these Tree-ferns. Of the Coal flora of Great Britain about *one half* were Ferns, and many of these Tree-ferns. At

present in all Europe there are not more than sixty known species of Ferns: in European Coal-measures there are nearly 550 species, and these are certainly but a fraction of the actual number then existing. That this indicates a tropical climate is shown by the fact that out of 1,500 species of living Ferns known forty years ago, 1,200, or four fifths, were tropical species. The number of known living Ferns is now about 3,500, but the proportion of tropical species is still probably the same. Even in the tropics, however, the proportion of Ferns is far less than in Great Britain during the Coal period. Again, Tree-ferns, arborescent Lycopods, Cyads, and Araucarian Conifers, are now wholly confined to tropical or sub-tropical regions. The prevalence of these tropical families and their immense size, compared with their congeners of the present day, would seem to indicate at least sub-tropical if not tropical conditions. And these conditions prevailed not only in the United States and Europe, but northward into polar regions; for *in Melville Island, 75° north latitude, Grinnell Land, 81° 43', and Spitzbergen 77° 33' north latitude, have been found coal strata containing Tree-ferns, gigantic Lycopods, Calamites, etc.*

Le Conte wrote most of this paragraph for his 1882 textbook, but added new information with each updated edition. The evidence of tropical plants in Spitsbergen was added in 1891, and the evidence of Grinnell Land in 1896. In all these editions, Le Conte continued by discussing evidence of high humidity, great uniformity of species, and uniformity of climate conditions in Carboniferous time. He and others were convinced that the entire Earth had experienced warm climate in the Carboniferous and Permian. The atmosphere functioned as a "greenhouse" and the combination of carbonic acid vapor and aqueous vapor formed what he called a "double-blanket," thus explaining the warm conditions worldwide.

GLACIAL ICE SHEETS OVER SUBTROPICAL LOWLANDS (1856 / 1903)

Tropical plants in arctic regions presented an awkward problem, but that turned out to be only half of the problem. The more difficult half came to light in 1856, in one of the greatest surprises in the history of geology. Three young geologists, William Thomas Blanford (1832-1905), Henry Francis Blanford (1934-1993), and William Theobald (1829–1908), discovered glacial deposits in the lowlands of India. These deposits were from the Carboniferous or Permian, at a time when coal plants were living in the arctic and the whole Earth was presumed to be uniformly warm. Despite their bewilderment, these geologists could not avoid the implication that these Indian deposits were glacial:

Blanford et al. 1856, p. 47-48.

The lowest bed, which we find resting on the gneiss, is most generally the "boulder bed," which occasionally assumes the local form of a coarse conglomerate...this "boulder bed" is a peculiar one. It consists essentially of boulders of granite and gneiss, those of the former comparatively small and the latter of much larger size, frequently from 4 to 5 feet in diameter, imbedded in a matrix, which varies from a coarse sandstone to the very finest shale. In some places (as e.g. near Purgono) the matrix is a dark-green silt, without any admixture of sand, but full of boulders of all sizes. Occasionally it is very fine in grain and sometimes assumes a shaley structure. A good instance of this, and one in which the boulder bed is seen resting on the gneiss, is shown in the accompanying Sketch *(Fig. 4)*. [Fig. 19.] of a nullah section near the village of Kandusa on the Northern boundary of the field.

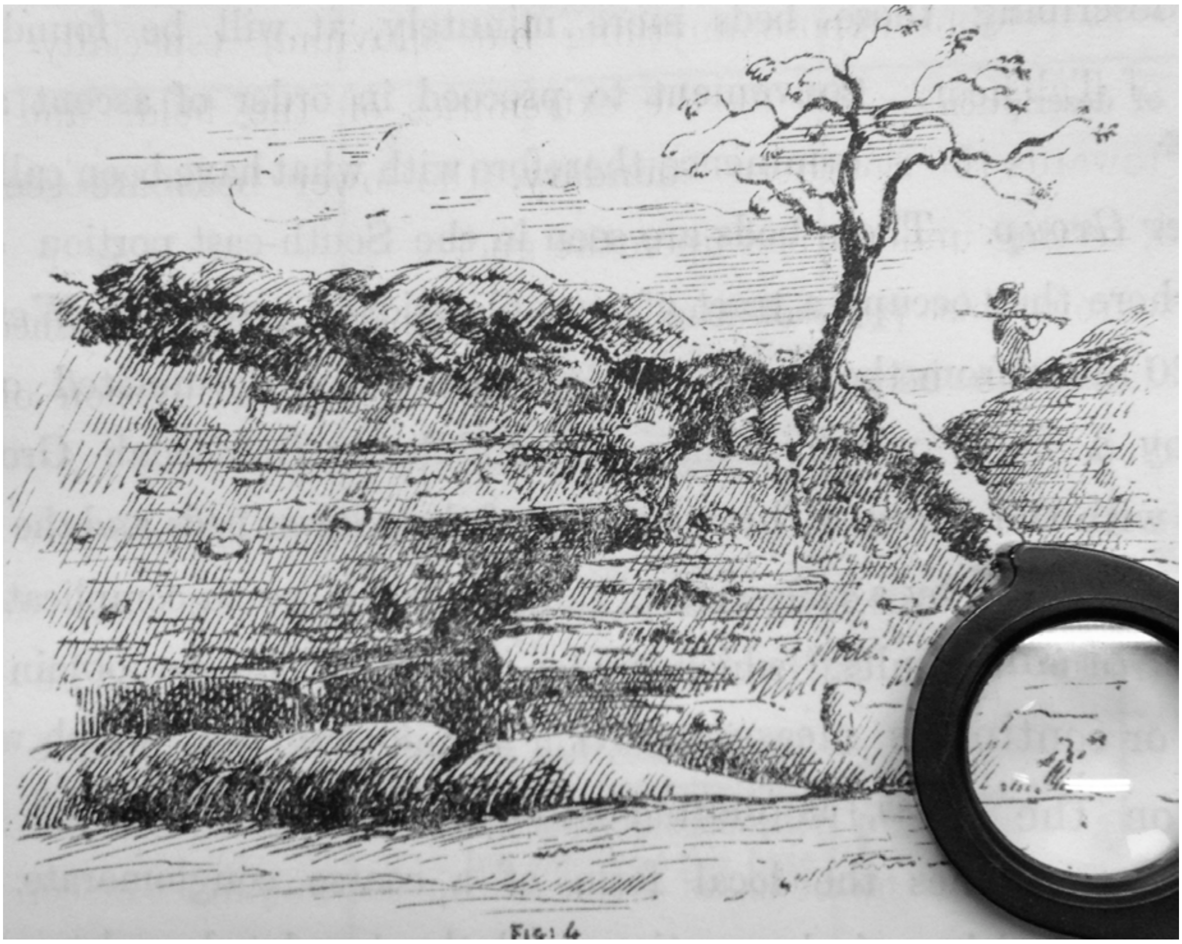


Figure 19. Boulders are scattered in a shale matrix. The first geological illustration of glacial deposits in the lowlands of India. From Blanford et at. (1856).

Blanford et al. 1856, p. 49.

The question naturally, indeed inevitably, suggests itself—How these enormous blocks of stone, manifestly requiring a great force to abrade and transport them, are found mixed with a sediment so fine, that in any, except a very sluggish current, it must have been swept away, and could not have been deposited? It seems difficult, in so hot a country, to conceive what yet appears to be the only probable explanation of this phenomenon, which indicates that the boulders must have been conveyed hither by *some* floating substance and deposited among a very fine sediment which was at the time in course of deposition by a very sluggish current.

These authors were hesitant to use use the word “ice” in so hot a country. In this first paragraph of their interpretation they preferred the euphemism “*some* floating substance.” The italics were theirs. They considered whether floating tree roots could have dropped these boulders into muddy sediment. But there were far too many boulders, and the deposits were immense, over 100 feet thick. The deposits were also widespread. They found them in several places in their mapping areas and in another area 200 miles to the north, where they were up to 1000 feet thick (Blanford et al. 1856,

p. 80). Since they did not find glacial polish or striations in the gneiss beneath the deposits, they dared not yet suggest that an ice sheet had ridden over this part of India. They also felt that the boulders were too rounded to have been carried by true glaciers. They interpreted the deposits as having been dropped from the melting of grounded icebergs. There were no fossils in the deposits, but there was a distinctive plant fossil *Glossopteris* in overlying layers, showing that the deposits must be Permian or Carboniferous in age.

Their first report inspired discussion back in England and further studies were made in India and elsewhere. Within a few years many more such boulder clays were found, in South America, Australia, and Africa. Both the boulders and the underlying gneisses showed striations that could only have been formed during movement of glacial ice. There could be no doubt that these deposits were of glacial origin. They were all associated with *Glossopteris* and of Carboniferous to Permian age.

Lyell did not present these discoveries in his textbooks. These data did not fit with the other evidence of warm Carboniferous-Permian climate. Dana seems to have been the first textbook author to mention them:

Dana 1880, p. 829.

In Britain the Permian bears evidence of floating ice in its stony accumulations (p. 431); in India the Permian contains bowlders of all sizes, up to five or six feet, and also glacial scratches; and similar facts have been observed in rocks supposed to be of the same age in the southern parts of Africa.

Recall that Dana had anticipated these problems in 1863: “When it has been proved that there was a polar climate anywhere in the Palæozoic ages, and what its location was, it will be soon enough to arrange this [a change in the Earth’s poles] among possible causes. Astronomers deny its possibility.” By the 1880 edition of his textbook this polar climate was proved to his satisfaction, so in that edition he explained why astronomers and geophysicists considered it impossible for the Earth’s poles to change.

Le Conte did not mention the Permo-Carboniferous glaciation in his textbooks. In 1903, a few years after Le Conte’s death, Herman Fairchild (1850-1943) published the 5th edition of Le Conte’s book. Among the very few additions that Fairchild made were two new details:

Le Conte (Fairchild) 1903, p. 430.

The Permo-Carboniferous of Australia, India, South Africa, and Brazil all contain enormous glacial deposits and other evidences of glaciation. Apparently Permian glaciation was on a vaster scale than that of the Pleistocene in the northern hemisphere.

The peculiar Permian flora of the southern hemisphere, called the “Glossopteris Flora,” is unlike that of the Carboniferous and allied to that of the Mesozoic, and indicates land connection of all the southern continents.

Fairchild added these two paragraphs where there was available page space for them. But that space was not really appropriate. This new information about Permian-

Carboniferous glaciation was printed at the end of a chapter. It was at odds with with the earlier conclusion that climates had been hot everywhere on Earth during the Permian-Carboniferous.

None of these authors could explain how tropical deposits are to be found in the Arctic while glacial deposits are found near the Equator. In Hopkins' hypothesis of crustal shift, the explanation would have been easy: the tropical deposits had moved northward to the arctic, and the south-polar glacial deposits had moved northward to the Equator. This is also the modern plate-tectonic explanation. But no geologist was promoting a horizontal displacement theory. Pepper was a chemist, not a professional geologist; he was not concerned with geological interpretations. He had also moved to Australia. Hopkins was completely out of the picture. In 1867 he was trying to demagnetize a metal ship, another of his innovative magnetization projects. He had caught a violent cold, and died at the age of 57. (We might say, as he had written in 1844, that he "perished to feed the future planet.")

Archibald Geikie's solution to the problem of Permian-Carboniferous climate was to cast doubt on the validity of the glaciation:

Geikie 1893. p.1341-1342 (in 1903 reprinting).

Most probably the luxuriance of the flora is rather to be ascribed to the warm moist climate which in Carboniferous times appears to have spread over the globe even into Arctic latitudes. On the other hand, evidence has been adduced to support the view that in spite of the genial temperature indicated by the vegetation there were glaciers even in tropical and sub-tropical regions. Coarse bowlder-conglomerates and striated stones have been cited from various parts of India, South Africa, and Eastern Australia, as evidence of ice-action. There appears, however, to be some element of doubt as to the interpretation of the facts adduced. It may be matter for consideration whether the bowlder-beds could not be accumulated by torrential waters, and whether the striated surfaces on the stones might not have been produced by internal movements in the rocks, like slickensides.

And Dana, in his last edition of 1895, again mentioned the problem:

Dana 1895, p. 698.

The above facts have led some geologists to the conclusion that over India, Australia, and South Africa, there were glacial conditions in the course of the Permian era – a time when Europe and America were under luxuriant vegetation.

The problem of these southern glacial conditions was so perplexing that Dana temporarily wavered from his doctrine of "continents always continents." In an attempt to explain the spreading of *Glossopteris* plants between India, South America and Australia, he allowed ocean floor near Antarctica to rise above sea level in the Permian, thus making land bridges to explain plant distribution and glacial advance. But he realized that even those geographical changes could not have caused enough general cooling to result in ice sheets in the southern continents and equatorial India. To make the problem seem manageable, Dana suggested that the glaciers had been limited to high summits (a hypothesis that had been thoroughly considered and disproved by those who had studied them):

Dana 1895, p. 737.

The idea that Antarctic land of so great extent became emerged in the Permian era, or about that time, suggests a reason for the existence of evidences of glacial phenomena in the Permian of South Africa, India, and Australia. For such a geographical change would certainly have caused a general refrigeration of southern climates; and if sufficient to produce icy winters and glaciers about high summits, all the observed facts would have their explanation.

Although Dana admitted to a previous land bridge on page 737, he withdrew the suggestion toward the end of the same book:

Dana 1895, p. 1028.

An exception to the general principle [continents always continents] has been made by putting a hypothetical continent in the Indian ocean. But the facts suggesting the hypothesis have been shown to be explained otherwise.

THE PROBLEM OF SINKABLE LAND BRIDGES (1910)

European geologists contended that land bridges must once have existed. But what great forces or changes could explain their disappearance? They could not have been removed by erosion. High mountains can get eroded down with time. They are attacked by harsh climate and weathering, with the loosened material being transported by rivers to the oceans. But once land is reduced to sea level, most weathering ceases. Under the sea there is not enough erosion or transport to further reduce their levels. Wide land bridges must have sunk to great ocean depths, and for that to have happened, large-scale internal processes must have been involved.

Geologists could not explain how land bridges had sunk, but nevertheless, they accepted their existence. Geologists were often forced to accept geological processes that had not yet been fully explained. No one at that time could understand what internal forces created mountains. Nor could they explain the cyclic pattern of glaciations and interglaciations. In geology, the first goal is usually to recognize what has happened, and the next goal is to explain how it happened. And in general, geology authors write mostly about things that they understand, not about problems that seem to have no explanation.

The question of whether or not there existed ancient land bridges belongs to the subject of paleogeography. In 1910, two of North America's most talented geologists wrote major papers that established them as leaders in this field. Charles Schuchert (1858-1942) wrote a 230-page monograph entitled *Paleogeography of North America*. It covered the history and methods of paleogeography, and these were followed by a great deal of documentation and 49 full-page paleogeographic maps of North America. The purpose of such maps was to show the positions of oceans, shallow seas, and land areas at different times. Here, as a typical example, we see his map of latest Carboniferous time (Fig. 20).

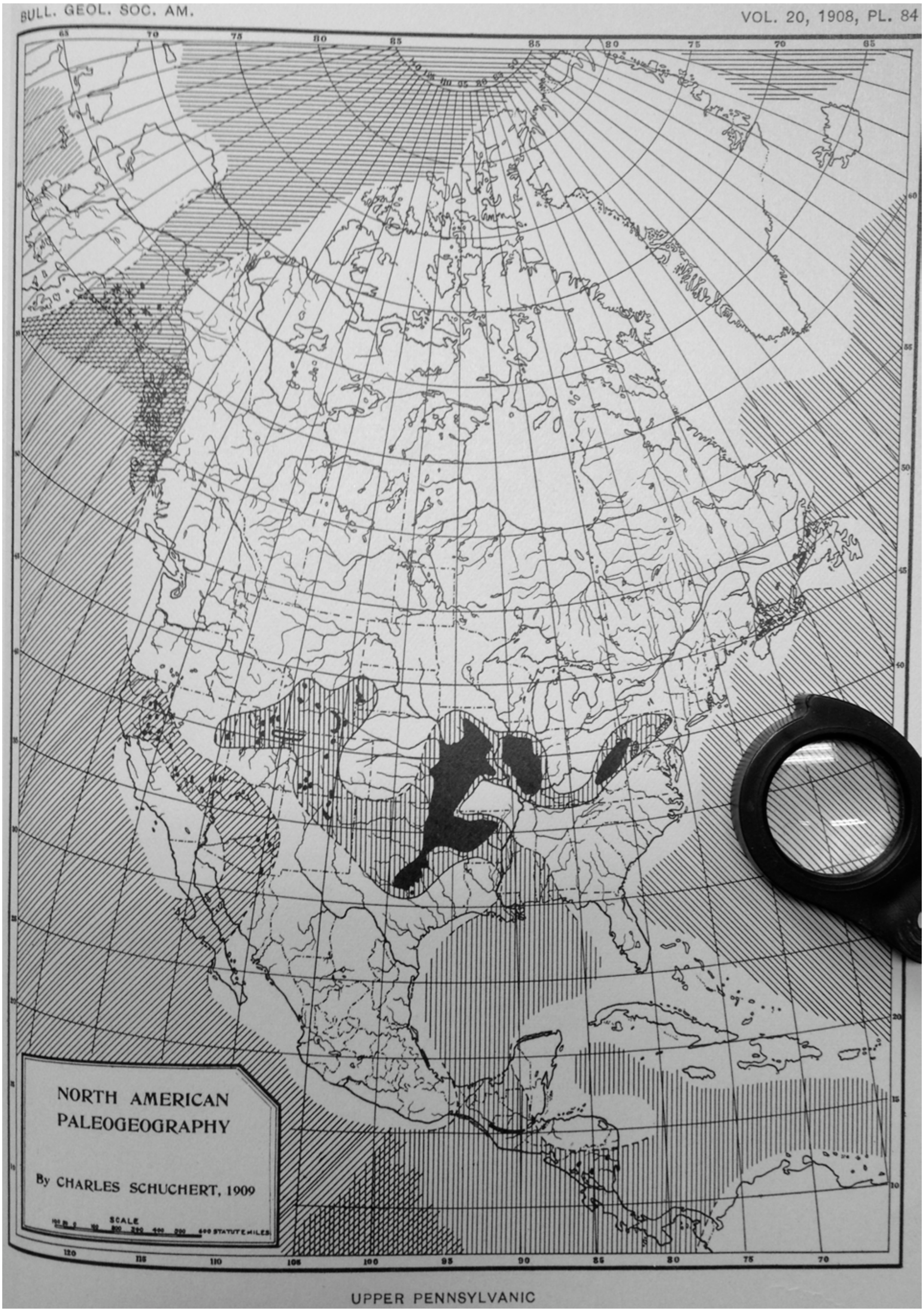


Figure 20. Schuchert's paleogeographic map of North America for Upper Pennsylvanian (Carboniferous) time. It shows a land bridge across the northern Atlantic. From Schuchert (1910).

Schuchert was professor of paleontology at Yale University. He was an authority on the marine fossils and sedimentary deposits of North America, Europe, and much of the world. He was fully convinced of the former existence of wide land bridges across the Atlantic Ocean. These land bridges were so wide, that the Atlantic Ocean as we know it, had not yet formed. European geologists had been making this claim for decades:

Schuchert 1910, p. 459.

Throughout the Paleozoic the northern Atlantic waters were separated from the southern Atlantic by the great continent Gondwana, uniting Africa and South America across the medial region of the present Atlantic. It is, therefore, not correct to speak of the northern Atlantic until the present form of this ocean has been attained, which seemingly had its inception late in the Mesozoic. Von Ihering has named the southern Atlantic waters south of Gondwana *Nereus* (he writes it Nereis) evidently after the father of the 50 Nereids....

As the northern Atlantic has an independent evolution from the southern Atlantic, it is here proposed to call the former the *Poseidon ocean*, after "the lord of the sea," known to the Romans as Neptune.

There was no wide and deep Atlantic Ocean. Instead, the Poseidon Ocean was probably a shallow sea during most of the Paleozoic, "and during the Mesozoic it was practically nonexistent." (Schuchert 1910, p. 510.) Schuchert explained these interpretations best in 1915, so we shall read about them below.

Bailey Willis (1857-1949) was North America's other pioneer paleogeographer. He was one of the leading geologists at the U. S. Geological Survey. He had compiled the wall-sized *Geologic Map of North America* and the 900-page *Index to the Stratigraphy of North America*. In 1910, he published a series of 15 paleogeographic maps of North America (Fig. 21) very much like Schuchert's. Willis (1910) also wrote a 20-page article in the journal *Science*, entitled *Principles of Paleogeography*. It did not provide detailed map data and interpretations as Schuchert had done. Willis's purpose was more to lay out the fundamental principles of the new science of paleogeography.

Willis did not accept land bridges. He had two excellent arguments as to why they could not have once existed and then sunk to form ocean floor: 1. The sea levels throughout the world would have dropped, because a sinking land bridge would increase the volume of the ocean basins. Additional water would not have been added to keep the ocean basins full. 2. A land bridge could only sink by its crustal material becoming denser, since the height of the Earth's surface is everywhere determined by local isostasy, the position that crustal material floats on the underlying mantle. Here, in his rather difficult writing style, Willis expressed these key arguments:

Willis 1910, p. 243.

PERMANENCE OF OCEAN BASINS

Oceanography is a science which has yet scarcely ventured over the threshold of the present upon the long vista of the past, but the guidance of paleogeography leads that way. From the study of ancient lands and epicontinental seas we are led directly to the recognition of ancient ocean basins; it is, however, particularly among European geologists, still a mooted question whether the hollows, which the waters occupy, have constantly existed as hollows or may have sunken in. The evidence that the hollows have constantly existed is strong. Upon it rests an assumption, which must be either affirmed or denied, there being no third condition, and which may be stated in the affirmative form as a principle:

The great ocean basins are permanent features of the earth's surface and they have existed, where they now are, with moderate changes of outline, since the waters first gathered.

This conclusion rests upon three principle facts:

The continents have never been submerged to oceanic depths and consequently can not have been replaced by deep hollows.

The oceanic basins have always been of such capacity that they contained by far the larger part of the waters, which have overflowed on the continents only as relatively shallow epicontinental seas; hence no considerable part of the existing basins can ever have been occupied by land.

There is a relation between the intensity of gravity and the relative altitude of a continental or oceanic plateau, which proves that the plateaus have assumed different altitudes according to the densities of the subjacent material. The transformation of a continent into an ocean basin, or *vice versa*, would require, therefore a change in density of an enormous volume of material, and there is neither evidence nor explanation of such a change.

Willis 1910, p. 244.

We have good reason to assume that the volume of oceanic waters has not changed materially from what it was at the inception of existing conditions, it being apparently true that contributions from within the earth have been relatively small during geographic eras, and none being known from without.

The ocean basins are now somewhat overfull; they are not large enough to hold all the waters, which therefore extend over the margins of the continents. During certain epochs of the past the waters have spread farther, the basins having been less capacious; again during certain other epochs the waters have withdrawn into deeper or wider basins. These variations have lain within narrow limits as compared with the total volume of the oceans, and they have occurred repeatedly, in alternation. Had a continent ever existed in place of one of the ocean basins, it must on sinking to ocean depths have produced a disturbance of these nicely adjusted relations, of which the geologic record shows no trace; which must, however, have been of such magnitude that it would have marked off an older era of small lands from a later one of great continents. No such event has taken place, and no continent of oceanic extent has sunk to oceanic depths.

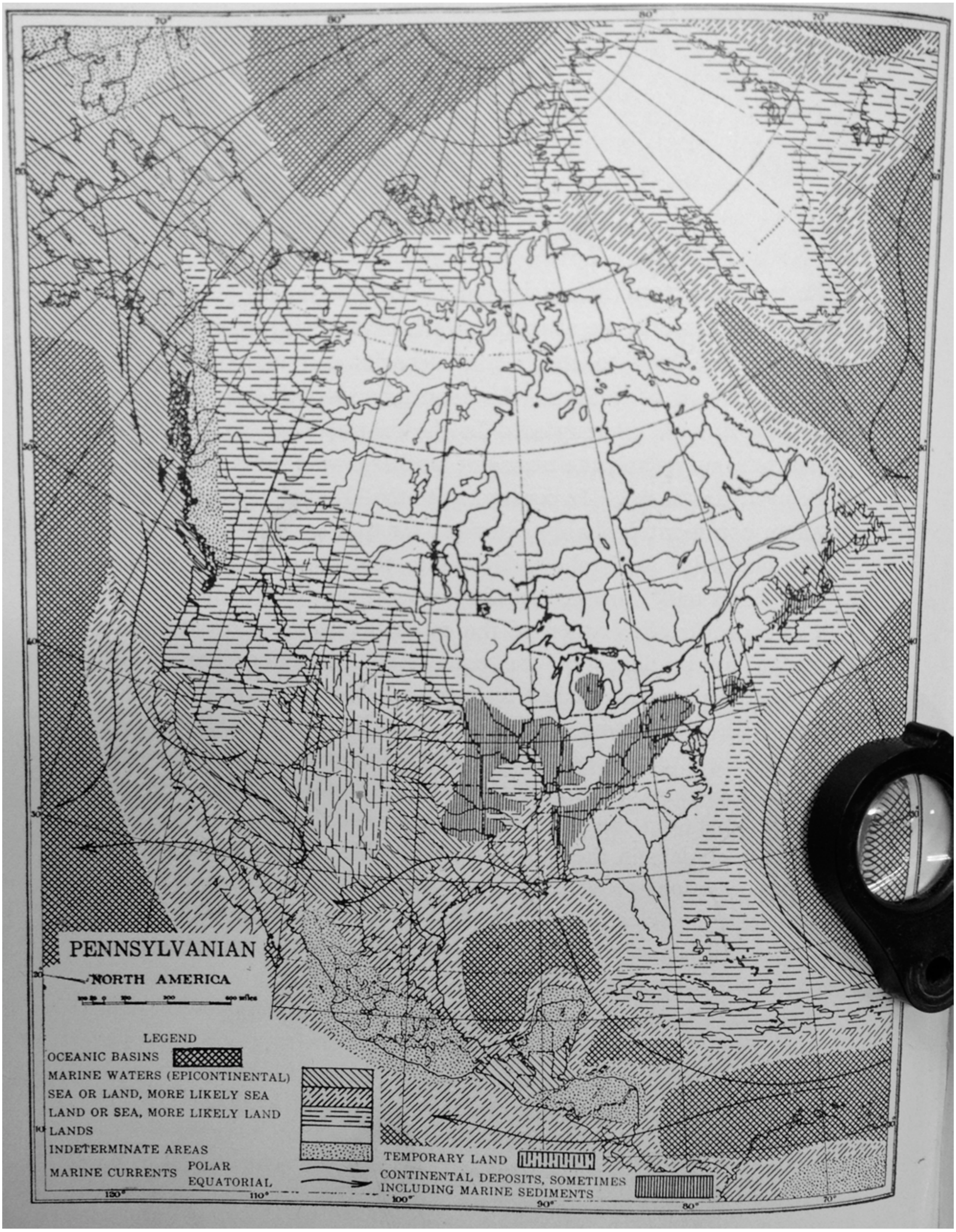


Figure 21. One of Willis's 15 paleogeographic maps of North America. This one, for Pennsylvanian time, indicates a "likely" land bridge across the northern Atlantic. From Willis and Salsbury (1910).

CHARLES SCHUCHERT'S *HISTORICAL GEOLOGY* (1915)

Whereas Willis was a structural geologist, concerned mostly with the internal movements of the Earth and its crust, Charles Schuchert was a paleontologist and stratigrapher. He understood fossils and what they could tell about early trans-Atlantic connections. Fossils allow paleogeographers to identify areas that were previously isolated or previously connected with each other. We can easily do the same with modern animals; the exotic animals of Australia show that there have been no land connections there in recent geologic times. Both marine and land organisms are useful for deciding if seas were connected and if lands were connected. Schuchert was familiar with Willis's arguments against the sinking of continents to form ocean floor, but from the fossil evidence there seemed to be no alternative to the land-bridge hypotheses.

Schuchert taught a course each year at Yale on the geological history of the Earth. In 1915, he published his own new textbook *Historical Geology*. It was the second part of a two-volume set entitled *A Textbook of Geology*. The first part was *Physical Geology*, by Louis Pirsson (1860-1919), also at Yale. These quickly became the leading North American textbooks in geology. They formed a 1051-page book, which could be bought either as one volume or two. Competing textbooks at that time were Le Conte's *Elements of Geology,* which was now getting out of date, and Chamberlin and Salisbury's *Geology*, in three volumes.

Schuchert's *Historical Geology* was outstanding. He had collected a vast amount of information, which he understood thoroughly and presented lucidly. Here we can read his presentation of the theories of ancient land connections and their break-up. He was tackling the puzzling paleogeographic data, not skirting them, as others seem to have done.

Schuchert 1915, p 463-465.

Permanency of Continents and Ocean Basins

Older and Newer Views. – In the earlier days of Geology, it was held that there was no stability in the continents and oceans as such, and that there had been complete interchange between them. Even Sir Charles Lyell taught that all parts of the ocean bottoms had been land. Now, however, most geologists hold with Dana that the oceanic basins and the continents have in the main, although not in detail, been permanent features of the lithosphere at least since the close of Proterozoic time. There is likewise much agreement among geologists in the belief that the oceanic basins are sinking areas, also spoken of as the *negative areas* of the lithosphere, because the sum of their crustal movements is downward; and in general it appears that the oceanic basins have gradually attained not only greater depth but somewhat enlarged area as well. On the other hand, the continents are the rising masses of the lithosphere in relation to sea-level, and for this reason are also called the *positive areas*, because the sum of their movements is upward (see page 228). Of these two great features of the lithosphere, the ocean basins are the more permanent, while the continents are either locally enlarged or great masses of them are warped under or fractured and depressed beneath the oceanic level. That the internal forces of the earth have wrought great changes in the outlines of the continents and therefore in the shapes of the oceans as well will be shown in subsequent pages. In general, however, it may be said that the present oceans and continents have been more or less permanent features, and that they have been where they now are, with moderate changes in their outlines, since their origin in

Proterozoic time or earlier. The greatest changes seemingly have taken place in the equatorial region and in Antarctica south of Australia and South America.

Proof of Permanency. – The proof that there has been no complete interchange between continents and oceans is seen in the following facts: (1) On the continents there are almost no deposits of deep-sea origin, and the few isolated cases are met with only on the margins or on continental islands (discussed more fully in the next chapter). (2) The marine deposits on the continents are nearly always those of very shallow seas, in fact, not at all unlike those now accumulating on the continental shelves. Further proof of this is given by the fact that the contained fossils indicated that the life was all of shallow-water types. If there had been complete interchange, on some continent there should be preserved a long and unbroken oceanic record in both the deep-sea sediments and animals, but nowhere has such a series of materials been discovered. (3) It is now established that the lithosphere is denser and therefore heavier beneath the ocean basins than under the lands, making it impossible for them to have interchanged their positions without destroying the equilibrium of the outer shell.

Deep troughs or geosynclines have, it is true, been repeatedly developed within the continents, near their margins, but even though some of these have in places subsided as much as 40,0000 feet, yet at no time have they held other than very shallow seas. This is proved by the character of their sediments and the entombed fossils. Stated in another way, there has been subsidence with compensating sedimentary accumulation. This is certainly true for Appalachian and Cordilleran geosynclines, out of which there eventually arose the Appalachian and Rocky Mountain systems. This matter will again be discussed in Chapter XXX.

Schuchert refers here to Chapter XXX. I interrupt his presentation to point out that on the first page of that chapter, he states that continents are permanent features, and that North America is especially so:

Schuchert 1915, p. 576.

North America: the Type Continent

Definition of Continent. – Dana long ago well said: “America is the type continent of the world.” North America *is* the type continent, because of its simplicity of geologic structure, not only throughout its vast extent but also throughout the geologic ages. The other continent of the northern hemisphere, on the contrary, is more complex in structure, since only in the course of time, through the welding together of several land masses by orogenic (mountain-making) forces, has Eurasia been formed. A typical continent, Dana states, is “a body of land so large as to have the typical basin-like form, – that is, independent mountain chains on either side of a low interior.”

Schuchert did not share, or mention, Dana’s belief in the divine creation of North America. But Schuchert did firmly believe in the primacy of that model continent. He also saw the need for a land-bridge connection. We now jump back to continue his earlier discussion:

Schuchert 1915, p. 465.

Examples of Continental Fragmenting. – As the ocean basins are the more permanent features of the earth’s surface and as they are the periodically sinking areas, it is to be expected that more or less large parts of the ancient continents should have been dragged beneath the waters. As an example may be cited Madagascar, a great island, 975 miles long, with an estimated area of 230,000 square miles, lying in the Indian Ocean off the east coast of Africa. No naturalist doubts its former connection with Africa, because of their animals, and yet the channel of Mozambique which now separates it from the mainland is from 240 to 600 miles wide, and represents a land area that has gone down to a depth ranging between 5000 and 10,000 feet. To the northeast of Madagascar lie many small islands, the Seychelles, and to the northwest occurs the Comoro group, all of which are also held to have been parts of Africa and Madagascar. Not only this, but many biologists and geologists hold that all of these lands are but parts of the comparatively recent land Lemuria.

Africa is unlike most of the continents in that it has no marginal mountain chains in the northern and central thirds, other than the Atlas Mountains along the northwestern border. The great peninsula south of the Sahara Desert consists of high plateaus ranging between 3000 and 6000 feet, and the ancient seas invaded only in the north and very sparingly along the east coast. The continent is a great elevated segment or block of the lithosphere with the geologic appearance of broken-down eastern and western margins. The faulted nature of its coasts is further seen in the fact that they are singularly free of large indentations and harbors. South America, on the contrary, has all along its western coast, in the high Andes, the elevated border required for a continent, but along its east coast we seek in vain for folded mountains, and it is therefore held by some of the ablest geologists that eastern Brazil has gone down into the Atlantic.

Let me interrupt again to point out a special significance of the paragraph above. Schuchert could identify that the west coast of Africa and the east coast of South America were faulted coastlines, that is, that the Atlantic Ocean's present shape is the result of faulting. This is not so different from what Snider wrote in 1858 with the help of Bulard's dramatic map: the Atlantic Ocean formed as great cracks or faults broke the land to produce the present coasts. But Snider had the land move horizontally, whereas Schuchert had it sink:

Schuchert 1915, p. 466.

Gondwana Land. – The broken-down and submerged parts of the continents referred to above are parts of a former great transverse equatorial land, and as there will be occasion to refer to this from time to time, it is desirable to introduce the subject here, though its proper place for presentation is toward the end of the book.

Besides the facts given above, there is much other evidence of a geologic, paleontologic and zoölogic character relating to the distribution of plants and animals since the Paleozoic, tending to show that Brazil was once widely connected with northwestern Africa across what is now the deep Atlantic Ocean. This lost continent is the *Gondwana Land* (from a district of the same name in India) of Neumayr (1883) and Suess (1885) and of the zoögraphers, a vast transverse land stretching from the northern half of South America across the Atlantic to Africa and thence across the Indian Ocean to peninsular India, including Lemuria. It was in existence throughout the Paleozoic, but the Atlantic bridge and Lemuria sunk into the oceans during the Mesozoic. Gondwana when complete was comparable to another transverse land of the north, *Eria* or Holarctica, which existed when North America was continuous with Greenland and Eurasia across Iceland to the British Isles (see Figs. 434 [Fig. 22] and 488.)

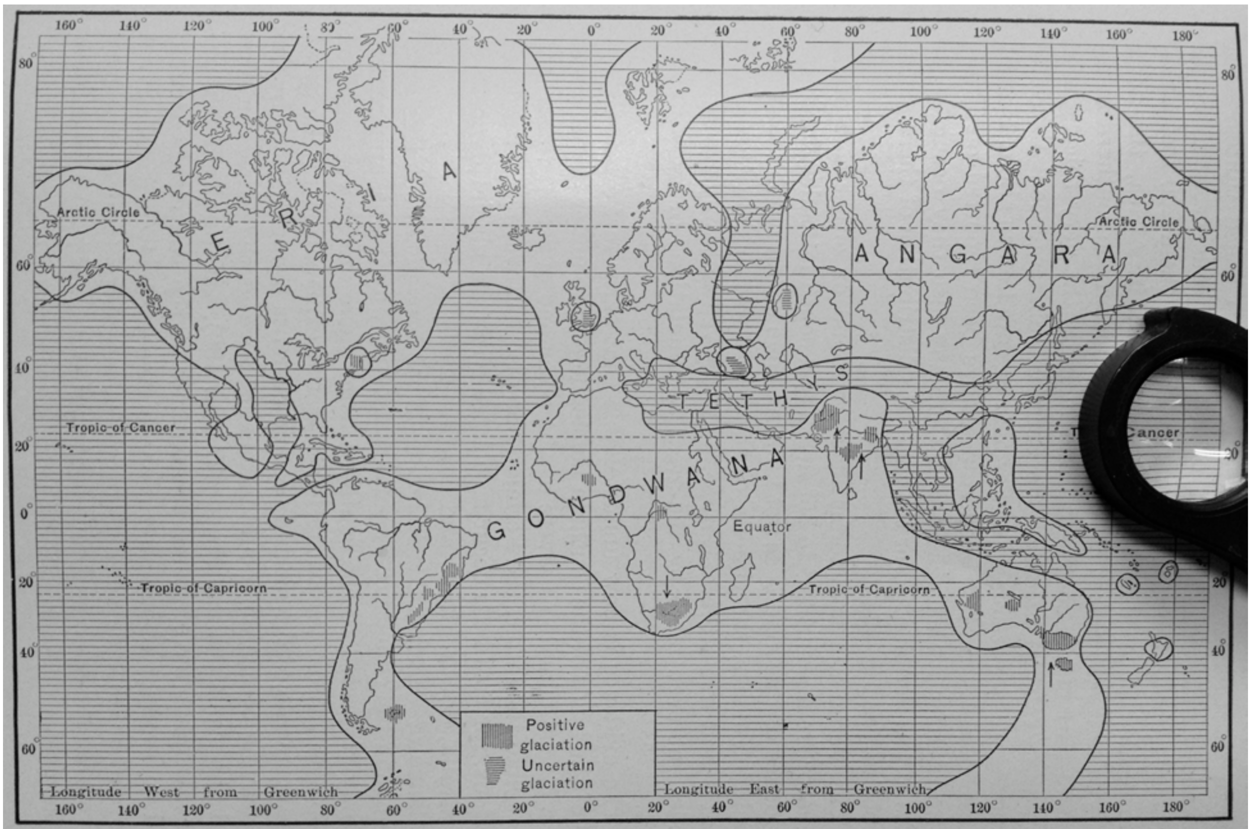


Fig. 434. — Paleogeography and glaciated areas of early Permian time. Note the transverse shape and the connected condition of the continents. Arrows indicate the direction of glacier flow. Carnegie Institution of Washington.

Figure 22. Schuchert's Fig. 434. In Permian time the continents were continuous, where today we have the Atlantic and Indian Oceans. In the present coastlines, Schuchert noted "the geologic appearance of broken-down eastern and western margins." From Schuchert (1915).

The Great Northern Transverse Continent Eria. – We have seen that the Caledonian Disturbance resulted in the making of mountains that extended throughout northwestern Europe. It was then that Laurentia (Canadian Shield-Greenland) was welded upon Baltica (Sweden-Finland), forming the most western part of the great northern transverse land mass that extended unbroken far into Asia. Therefore, at the very beginning of Devonian time there came into existence an almost circumpolar land, whose only submerged portion lay in the North Pacific, and which was formed by the union of Laurentia, Baltica, and Angara (see Fig. 434.) The great Canadian geologist, Sir William Dawson, of McGill University, labored long to make known the plant life of the Devonian, and since he termed it the Erian flora after the Erian rocks in which it is entombed, taking the name from lake Erie and the Erie division of the New York state geologists, Suess in 1909 gave the continent the name of Eria. It is the old ancestral continent of the modern Holarctic region of the zoölogists (see Fig. 434.)

We see here that already in 1915, Schuchert and many European geologists understood the welding together of Laurentia and Baltica in the Caledonian mountains. These are geological terms and concepts that most geologists today associate with modern plate-tectonic theory. But they were understood already in Schuchert's time. The ancient continents were joined together; dinosaurs and other reptiles were wandering freely across all the land areas:

Schuchert 1915, p. 827-828.

...almost all of the twenty-five orders of Mesozoic reptiles were already represented in the Triassic, though often by comparatively small and rare forms. Their footprints are known in eastern North America in great variety (Figs. 460, 461), but good skeletons are exceedingly rare. In Europe, on the other hand, their skeletons are more common, and, so far as known, the animals were of the same kinds as those of America. From this and other evidence it appears that the great northern continent Eria (Fig. 434) was still intact and was the land across which the plants and animals of Triassic time readily migrated to and fro. *Crocodile-like reptiles* of the sprawling type (*Mystriosuchus*, Fig. 458) and other active forms (*Aëtosaurus*, Pl. 31, Fig. 9) were common. Genuine *turtles* occur in the Triassic rocks, showing the group originated in the Permian. No lizards, snakes, or birds are as yet known in rocks of this era.

The *dinosaurs*, however, were the lords of the land, and they were present in great variety and in great size; in fact, some known by their footprints only, must have been larger than elephants (Fig. 461). In the Upper Triassic they had become adapted to all the land habitats. ...

Schuchert did not mention mammals in connection with these continental connections, because mammal fossils are extremely rare in the Mesozoic. Mammals began to rapidly evolve and multiply after the Mesozoic, after Gondwana and Eria had broken to form the Atlantic Ocean.

A characteristic of the vast Gondwana continent was that it had a glacial climate in Permian time, in contrast to Eria. Note the areas of Permian glaciation indicated on Schuchert's map. He explained that the evidence for this glaciation had been unmistakable for nearly fifty years (i.e. since about 1865.) This glacial evidence had been disregarded by some earlier geologists, but Schuchert understood that it was important:

Schuchert 1915, p. 758-761.

Great Glacial Period of Permian Time. – For nearly fifty years geologists have described unmistakable glacial deposits of Permian age in the continents of the southern hemisphere, but it is only recently that their results have been widely accepted. It is now known that glacial deposits – bowlder clays called tillites – are of wide distribution, for eight or nine such beds occur in South Australia above the Coal Measures, some of them 200 feet thick, interbedded in 2000 feet of marine strata; in Africa, the thick Dwyka series (1300 feet in Natal) extends from the southern to the central part of the continent; and in India the very thick glacial deposits (Talchir) preceded the Permian submergence. In North America, tillites seemingly of Permian age are known about Boston, Massachusetts, and striated stones have been reported on Prince Edward Island; in England and Germany also they occur at the base of the Permian. For the complete distribution of these glacial deposits, see Fig. 434.

The Permian glacial formations are found on either side of the equator from about 20° to 35° north and south latitudes, but little evidence of this kind is as yet known above 35° in north temperate lands. Further, the climate of that time was arid in the United States and in northern Europe, as is proved not only by the red beds, but even more by the great accumulations of gypsum and salt.

The evidence is now unmistakable that early in Permian times all of the lands of the southern hemisphere were under the influence of a glacial climate as severe as the polar one of recent times, and that, like the latter, the Permian one also had warmer interglacial periods, for coal beds occur associated with the glacial deposits in Australia, South America, and Brazil.

Cause of Permian Glaciation. – What brought about this great change in the climate of Permian time, and why it was, apparently, mainly restricted to the southern hemisphere are as yet unsolved problems. Most geologists look for the explanation in the great derangements of the air and oceanic currents brought about by the upheaving of mighty mountain chains, such as the European Alps and the Appalachians (Figs. 427 and 430). Naturally, such upheavals must have altered the outlines of the continents as well, and so brought about alterations of the oceanic currents. Even the carbon dioxide content of the atmosphere (and oceans) may have been lessened, and thus contributed to the production of the colder climate.

Gondwana and Tethys

Gondwana, the Great Southern Transverse Continent. – Something about this continent has been stated in Chapter XXI, and now further proof of its existence must be given. Such evidence relates mainly to the wide-spread deposits of Permian age which have a flora in their lower beds that has been called the *Glossopteris flora* (Fig. 432). This flora occurs throughout the southern hemisphere, and paleobotanists hold that it could only have been so widely distributed across a continuous land (see Fig. 434). Belief in the existence of Gondwana is wide-spread among European geologists, but many American workers do not yet believe in it, mainly because they hold strongly to the theory of the permanence of the oceanic basins and continents. Without this continent, on the other hand, paleontologists cannot explain the known distribution of Permian land life, and, further, its presence is equally necessary for the interpretation of the peculiar distribution of marine faunas beginning certainly with the Devonian and ending in the Cretaceous.

Tethys, the Greater Mediterranean. – To the north of Gondwana lay the great medial ocean which Suess has named Tethys, after the consort of Oceanus (see Fig. 434). The present Mediterranean is a remnant of this once grand middle ocean which widely extended unbroken from France and Spain into the eastern Indian and Pacific oceans, from time to time connecting with the Arctic Ocean by way of the Ural geosyncline. How often it was in open connection with the Atlantic is not yet clear, but that it had such communication is seen in the similarity of certain southern European and Gulf State faunas (Helderbergian, Kinderhook, and Comanchian).

During the recent Pleistocene ice ages, northern Europe and northern North America were heavily glaciated. But they had not been glaciated in Permian time. Yet, somehow, India and Africa had been glaciated. In his chapter on the Pleistocene ice ages, Schuchert discussed the problems posed by this Permian glacial evidence:

Schuchert 1915, p. 953.

Causes of Glacial Climate

As yet there is no accepted explanation of why the earth from time to time undergoes glacial climates, but it is becoming clearer that they are due rather to a combination of causes than to a single cause. Probably the greatest single factor is high altitude of the continents, with great chains of new mountains (the hypsometric causes) which disturb the general direction and constitution of the air currents (the atmospheric causes) and the ocean currents as well.

Hypothesis of Polar Wandering. – It has often been suggested that the axis of the earth has shifted and that the north pole in Pleistocene time was 15° or 20° south of its present position. To explain the equatorial glaciation of Permian time some writers have shifted the north pole to the region of Mexico, and the South Pole to the Indian Ocean; even if this were possible, however, the distribution of the ice-fields did not center about these imaginary poles. As the earth is essentially as rigid as steel, the dynamic conditions in such a mass would not permit of such changes without leaving a record of them in the structure of the earth-shell. There are no such records discernible. Moreover, it was long ago demonstrated mathematically by G. H. Darwin that migrations of the axis of the earth sufficiently extensive to be of geological importance have not occurred. “It would appear,” says Barrell, “that the assumption of polar wandering as a cause of climatic change and organic migration is as gratuitous as an assumption of a changing earth orbit in defiance of the laws of celestial mechanics.”

Schuchert then explained in similar detail some of the other possible causes of glacial climate: Effect of continental emergence, Effect of volcanic ash in atmosphere, Effect of oceanic spreading, Effect of carbon dioxide in atmosphere, and Effect of variation in solar energy. His book is remarkably thorough.

We can see that Schuchert had strong evidence for the continental connections, and for the interpretation that large parts of the Atlantic Ocean had only recently formed. This interpretation went against the principle of Bailey Willis, that the ocean basins were permanent features. Actually, everyone agreed that land bridges could not easily sink. Both Pirsson (p. 256) and Schuchert (p. 465) pointed out that continental lithosphere was less dense than oceanic lithosphere, and the interchange of these two was not possible. Nevertheless, Schuchert was certain that Atlantic land masses must have sunk, and as they sank the volume of Earth’s surface (vadose) water must have increased by the addition of new (juvenile) water to keep sea levels more or less the same.

It seemed also from fossils collected beneath the abyssal ocean floors, that none of the Earth’s deep oceans were older than the Mesozoic:

Schuchert 1915, p. 481.

Modern Origin of Abyssal Animals. – In the abysses there is not a trace of Paleozoic life, though from twenty-five to thirty-five genera, with a larger number of species, are known in the present shallow seas. Again, an analysis of the present abyssal animals reveals no ancestors older than the Triassic, and most of them date from the Jurassic and Cretaceous. Walther concludes from this evidence that the present deep ocean fauna migrated from the shallow waters during Mesozoic time, and that “the peopling of the deep sea can be traced at the earliest to the Triassic.”

While this evidence is correct, it does not necessarily follow that the oceans have all become abysmal since the close of the Paleozoic. While the juvenile water added to the oceans since that era has been considerable, yet it may not exceed 10 per cent; we see, accordingly, that these basins must have been long previously nearly as capacious as they now are to hold the vadose waters. Moreover, we must not forget that Gondwana was broken through by the Atlantic toward the close of the Mesozoic, and Eria during the Tertiary – areas that have taken up great quantities of water and thus brought about a general reduction (eustatic) of the sea-level. It is this geologically recent fragmenting of the continents that has resulted in the present high average of the lands above the strand-line.

Schuchert agreed with the German paleontologist Johannes Walther that there seem to be no fossils in deep ocean basins older than the Triassic. Today we know the reason for this; older oceanic crust and fossils have been destroyed by subduction. But Schuchert assumed that most of the existing ocean floors were as ancient as the Earth itself. He summed this up toward the end of his book:

Schuchert 1915, p. 981.

Origin of Oceans and Continents. – When the earth had attained to about its present diameter, in the Archeozoic, its atmosphere gave rise to the processes of solution, to weathering of rocks, and to the washing together of sediments formed on the earth's face, and the resulting finer materials together gravitated to lower places and eventually into the oceanic basins. The latter depressions probably are primarily due to the separation of the molten magmas during the formative eon into lighter or continental and heavier or oceanic portions. Therefore the oceanic basins have as a rule tended to sink and to follow more decidedly the shrinking centrosphere, thus becoming the negative areas, while the lands, somewhat lighter in specific gravity, become more so through the solution processes, resulting in the rising protuberances of the lithosphere, the positive areas. Even though the oceans have continued to attain greater depth, the increasing volume of sediment and water has demanded enlarging basins, and some of the required space has been taken from the continents. In this way the great northern transverse continent, Eria, and the similar medial one, Gondwana, were transformed into the longitudinal continents of the present.

Permanency of Oceanic and Continental Areas. – Since the beginning of Paleozoic time the oceanic basins and the continental masses have been more or less permanent. The permanency, however, is not rigid, but is rather flexible. Most rigid in the oceanic areas and more flexible in the continental masses. There is, moreover, a periodic flexibility in the oceanic areas, for many times have they spilled their waters widely over the land in shallow inundations. Over and over again the flooded lands have reasserted themselves above the general water-level because of renewed oceanic subsidence and the withdrawal of the waters. These are the main features in the present theory of the permanency of continents and oceans, a hypothesis that is less rigid than the original one of Dana (1846).

Schuchert was not blindly following Dana's doctrine of permanency, or any other doctrine. He was evaluating the data and the possible interpretations, and developing the best ones. He was a brilliant geologist, and this textbook is a masterpiece of data accumulation, clear understanding, and presentation. His sources of information were international. The names of his continents, *Gondwana* and *Eria* were from the 5-volume work *The Face of the Earth*, by the Austrian geologist Eduard Suess (1831-1914). European geologists had been using the land-bridge ideas, and now Schuchert was teaching them in North America.

Schuchert presumed that the presence of these previous continents had directed ocean currents and wind currents in various ways. When these current patterns were better understood, they would help to explain the Permian climates, with glaciation in the south and tropical conditions in the north. Schuchert felt that large continental areas must have existed and later sunk, despite the objections that Willis had discussed.

As it happened, Alfred Wegener had published an article in 1912 that could have helped Schuchert see another way to solve these difficult problems. But Wegener did not yet have the attention of North American geologists. It seems to have taken several years before his results were noticed in America, even by Charles Schuchert.

4. Alfred Wegener's Displacement Theory

OCEAN BASIN FORMATION WITHOUT SINKING CONTINENTS (1912)

Alfred Wegener had a Ph.D. in geophysics, and specialized in the dynamics of the atmosphere. He served as meteorologist, glaciologist, and naturalist on several Greenland expeditions (Fig. 23). He was not trained as a geologist, and had no education in the history of fossils, rocks, or mountains. As he looked at a map of the world in 1910, it occurred to him that the narrow Atlantic Ocean might have formed from rifting of a continent. The ocean has a relatively constant width, with matching coastlines on either side.

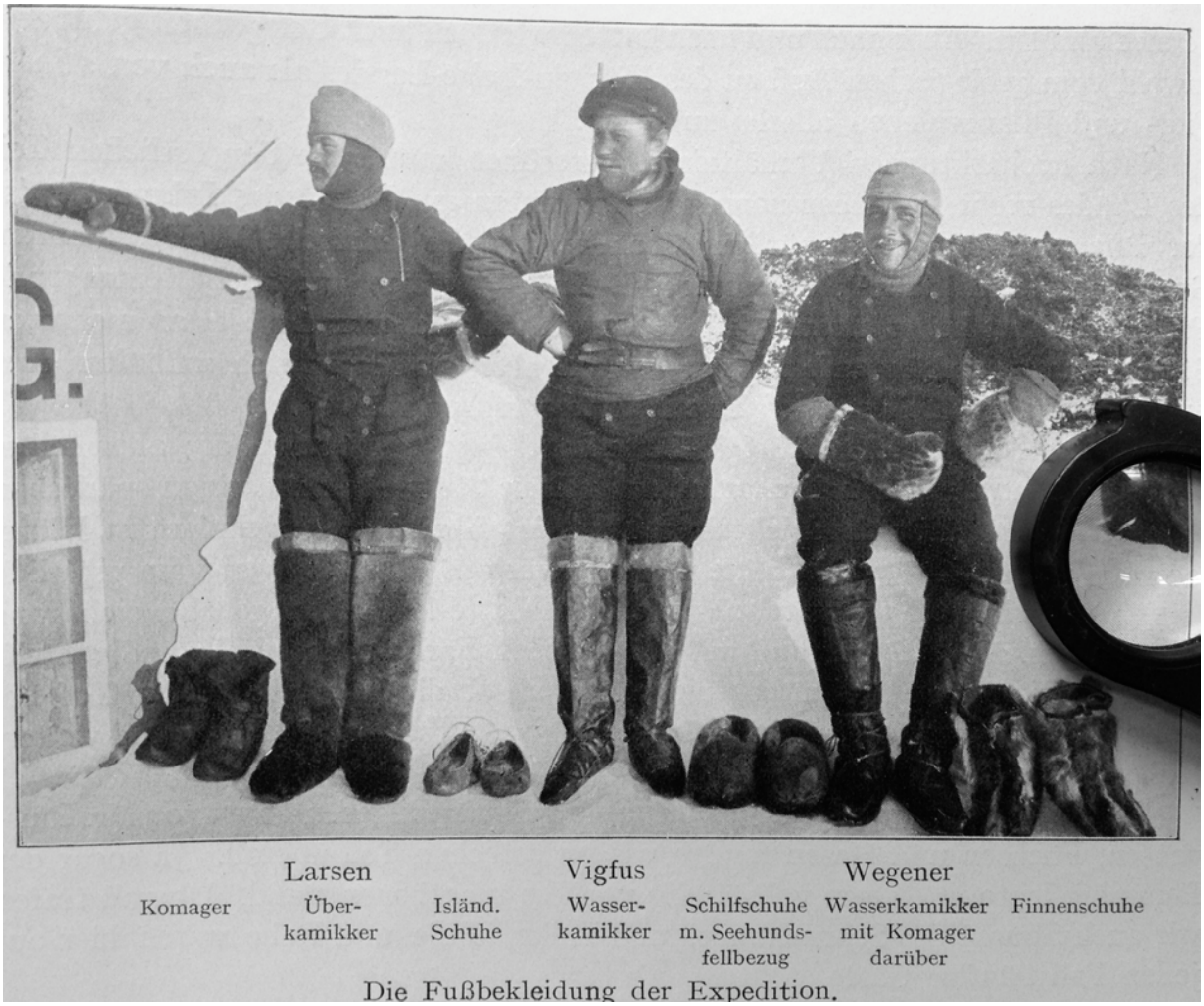


Figure 23. Alfred Wegener and two of his fellow explorers displaying their shoe collection in 1912. From Wegener and Koch (1919).

In the fall of 1911, Wegener happened to come across a paper that summarized the paleontologic evidence for a previous land bridge between Africa and Brazil. Here was independent evidence that the Atlantic Ocean had not previously existed. He then took the idea of Atlantic rifting seriously. He was an able and enthusiastic researcher, and within a few months he had learned a great deal of geology. He knew that geologists had no way of explaining the supposed sinking of continental crust to form the Atlantic, or why such a sinking would leave standing the Mid-Atantic Ridge. His idea of horizontal displacement of continents could eliminate not only the problem of sunken land bridges, but also the problem of Permian climates.

Wegener gave two talks on this hypothesis at German scientific meetings in January of 1912. Soon after, he published a substantial scientific paper. It appeared twice, first in *Petermanns Geographische Mitteilungen* (20 printed pages, broken into three different installments; April, May, and June) and then slightly shortened in *Geologische Rundschau* (15 printed pages, in July). The longer version included ten figures, whereas the shorter one had just three. Both of these journals were highly respected and widely read by European geographers and geologists. His papers began with this paragraph:

Wegener 1912a, p. 185 (translated here from German).

The origin of the continents.

In the following paper, a first rough attempt will be made to interpret the genesis of the large forms of our Earth's surface, i.e. the continental plates and the oceanic basins, by one single encompassing principle, namely by the principle of horizontal mobility of the continental plates. Everywhere that we have previously let the old land connections sink to the depths of the oceans, we want to consider now a rifting and carrying off of the continental plates. The picture that we get in this way of the nature of our Earth's crust is new and in some ways paradoxical. However, as will be shown, it does not lack physical justification. Already after a preliminary assessment of the main results of geology and geomorphology it reveals such a large number of surprising simplifications and interrelations that it appears justifiable, even necessary, to let this new, more efficient working hypothesis replace the old hypothesis of sunken continents, whose obvious shortcomings are clearly shown by the opposing teachings of the permanence of the oceans. In spite of this broad basis, I call this new principle a working hypothesis. I would like to see it treated as such until every possibility of doubt is excluded, when the continuing horizontal displacement has been demonstrated by exact astronomical position determinations. It is also not superfluous to point out that this is a first draft. An examination in detail will probably show that the hypothesis must be amended in some points.

Note how confident Wegener was about this idea of horizontal displacement. He expressed it as a principle, but he was willing to call it a working hypothesis. His confidence may have been justified, but this first paragraph surely alienated many skeptical geologists.

Wegener had learned much geology, but he had not learned the way geologists think and the way they present a new interpretation. It is natural for geologists to describe their observations first. Only after the features have been described, do geologists discuss how the features might have formed. But in Wegener's second paragraph, which gives an outline of his paper, it is clear that he was not taking this approach. He began his paper by considering how continents might be able to move, not by showing the evidence that they had indeed done so:

Wegener 1912a, p. 185 (translated here from German).

In the first chapter, on the basis of geophysical and general geological results, the question will be considered as to whether great horizontal displacements are at all imaginable in the apparently rigid Earth crust, and in what ways they take place. The next chapter gives a first modest attempt to pursue the past rifts and displacements of the continental crust during Earth history, and to uncover their connection to the creation of major mountain ranges and to polar misalignments. Finally, in the third chapter, are briefly stated those astronomically determined locations that are suitable to show probable continuing displacements, and an attempt is made to interpret the polar fluctuations.

In the first installment of this three-part paper, he simply stated that it was well known that continents had been connected, by the supposed land bridges. He thought that the continents could instead have moved horizontally. He pointed out that a few other scientists had expressed related ideas:

Wegener 1912a, p. 185 (translated here from German).

Before we begin with the presentation, just some short historical remarks are given. In England a hypothesis is currently being spread, particularly by O. Fisher's book "Physics of the Earth's Crust", in which the Pacific Ocean represents the last trace of a once larger basin from which the Moon mass took its origin by separation from the Earth. We cannot deal with this hypothesis here... It is only mentioned because W. P. Pickering (1907), during the follow-up of these thoughts, also expresses the opinion that America on this occasion was torn off from Europe-Africa and separated from it by the width of the Atlantic. Because the parallelism of coastal expressions is obvious, the thought of tearing the American continent off the Old World is thus here already expressed, and in my judgement is also correct.

Wegener was apparently not aware of Pepper's book or the Snider-fit. He did not try to illustrate a fit of his own (he did that in 1915), but only stated that the parallelism of the coasts is obvious.

Wegener entitled his paper *The Origin of Continents*, but it was also about oceans. From geophysical reasoning, he understood the origin of oceans better than any geologists of his time. He argued that the continental and oceanic crusts were fundamentally different. He showed a hypsometric curve, which plots the area of the Earth against the range of elevations (Fig. 24). It is not a single smooth curve; there are two preferred levels, one for continents, a few hundred meters above sea level, and the other for oceans, at about 4300 meters below.

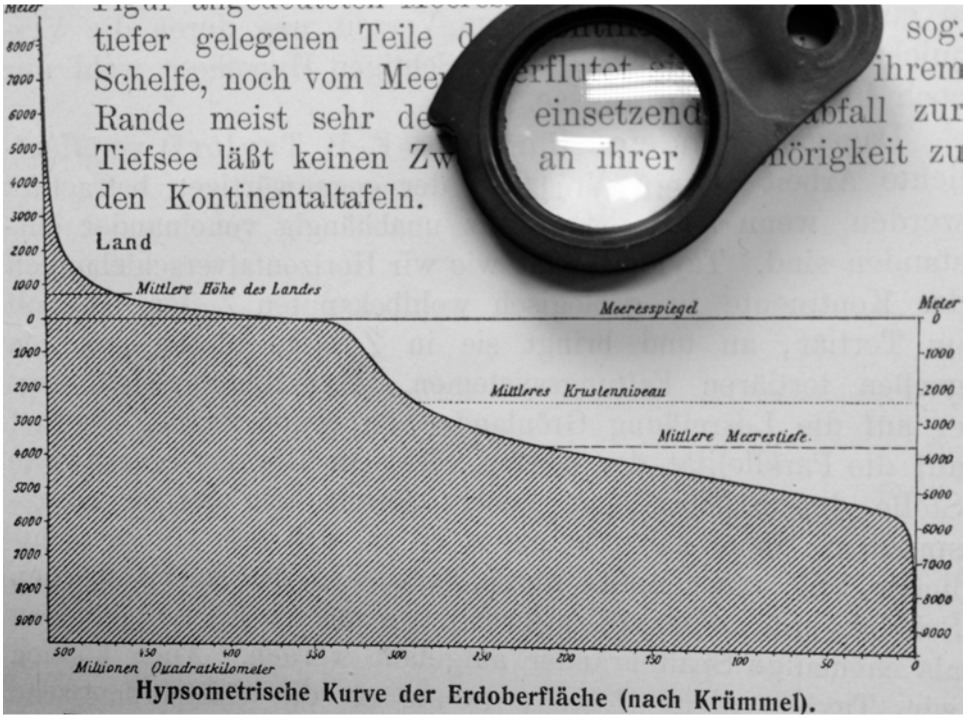


Figure 24. Hypsometric curve of the Earth's surface. The figure text states "nach Krümmel" but Wegener surely also saw this diagram in Pickering (1907). From Wegener (1912a).

Wegener explained that from knowledge of isostasy, these two different levels must indicate two different materials, light sial (rich in silica and aluminum) for the continents, and heavier sima (silica and magnesium) for the oceans (Fig. 25). These terms were in common use at that time, and are equivalent to our modern terms silicic and mafic rock.

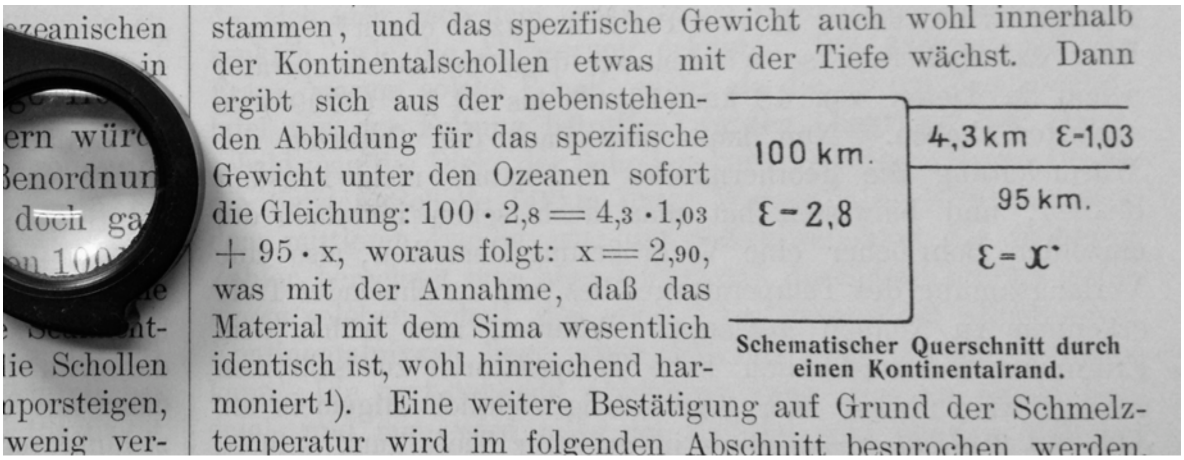

stammen, und das spezifische Gewicht auch wohl innerhalb der Kontinentalschollen etwas mit der Tiefe wächst. Dann ergibt sich aus der nebenstehenden Abbildung für das spezifische Gewicht unter den Ozeanen sofort die Gleichung: $100 \cdot 2{,}8 = 4{,}3 \cdot 1{,}03 + 95 \cdot x$, woraus folgt: $x = 2{,}90$, was mit der Annahme, daß das Material mit dem Sima wesentlich identisch ist, wohl hinreichend harmoniert[1]). Eine weitere Bestätigung auf Grund der Schmelztemperatur wird im folgenden Abschnitt besprochen werden.

Figure 25. Wegener's calculations involved continental sial (density = 2.8) floating on sima (density = X), which is overlain by ocean water (density = 1.03.) Oceanic crust is not indicated here, because it was thought to be the same as the mantle (sima). Wegener (1912a).

Since sial cannot change to sima, continental crust cannot simply sink to form oceanic crust. Bailey Willis had argued the same thing in his 1910 paper. But while Willis concluded that ocean basins must therefore be permanent, Wegener argued that permanence was not necessary. Wegener quoted Willis's principle in the original English. At the same time, he criticized Willis by pointing out that his final conclusion was particularly dogmatic (*besonders schroff*):

Wegener 1912a, p. 187.

"The great ocean basins are permanent features of the earth's surface and they have existed, where they now are, with moderate changes of outline, since the waters first gathered." Besonders schroff ist die Schlußbemerkung: "This conclusion appears to place the permanence of ocean basins outside the category of debatable questions."

Although Wegener was critical of Willis, they were in agreement that continental crust could not sink. But whereas Willis' played down the evidence of earlier continental connections, Wegener considered the connections to be a proven fact:

Wegener 1912a, p. 187 (translated here from German).

... we are compelled to assume, for earlier geological periods, land bridges between far separate continents through deep ocean. Hundreds and hundreds of paleontological discoveries form constantly growing evidence that fauna and flora of such continents lived in completely undisturbed exchange, directly across today's deep sea.

In the next installment, Wegener came to other types of geological evidence, including the second major problem that his theory removed:

Wegener 1912a, p. 256 (translated here from German).

The Permian ice age forms an unsolvable problem for all explanations that do not dare to take into account the horizontal movement of continents. Irrespective of other arguments, these relationships warrant "giving the movement of the earth's crust in the horizontal direction serious consideration as a working hypothesis" as Penck has already pointed out.

If we reconstruct the conditions of Permian time according to these ideas, then all the glaciated areas are moved concentrically around the southern tip of Africa, and we only need to put the South Pole in the much reduced glacial area, taking away all unexplained features. The North Pole would be situated at that time in the Pacific, the Bering Strait being wide open.

WEGENER'S PROPOSED ORIGIN FOR THE MID-ATLANTIC RIDGE

Wegener understood the formation of new ocean crust and the origin of the Mid-Atlantic Ridge already in his 1912-paper. His early understanding of the oceans has been generally overlooked.

There is a widespread misconception that the Mid-Atlantic Ridge and the ocean floors were terra incognita in Wegener's time. Actually, the Mid-Atlantic Ridge was described by the British *Challenger* expeditions in the 1870s. The ridge became well known and discussed when Ignatius Donnelly (1831-1901) interpreted it to have been the mythical lost continent, in his best-selling book *Atlantis*:

Donnelly 1882, p. 47.

Deep-sea soundings have been made by ships of different nations; the United States ship *Dolphin*, the German frigate *Gazelle*, and the British ships *Hydra*, *Porcupine*, and *Challenger* have mapped out the bottom of the Atlantic, and the result is the revelation of a great elevation, reaching from a point on the coast of the British Islands southwardly to the coast of South America, at Cape Orange, thence south-eastwardly to the coast of Africa, and thence southwardly to Tristan d'Acunha. I give one map showing the profile of this elevation in the frontispiece [Fig. 26], and another map, showing the outlines of the submerged land, on page 47. It rises about 9000 feet above the great Atlantic depths around it, and in the Azores, St. Paul's Rocks, Ascension, and Tristan d'Acunha it reaches the surface of the ocean.

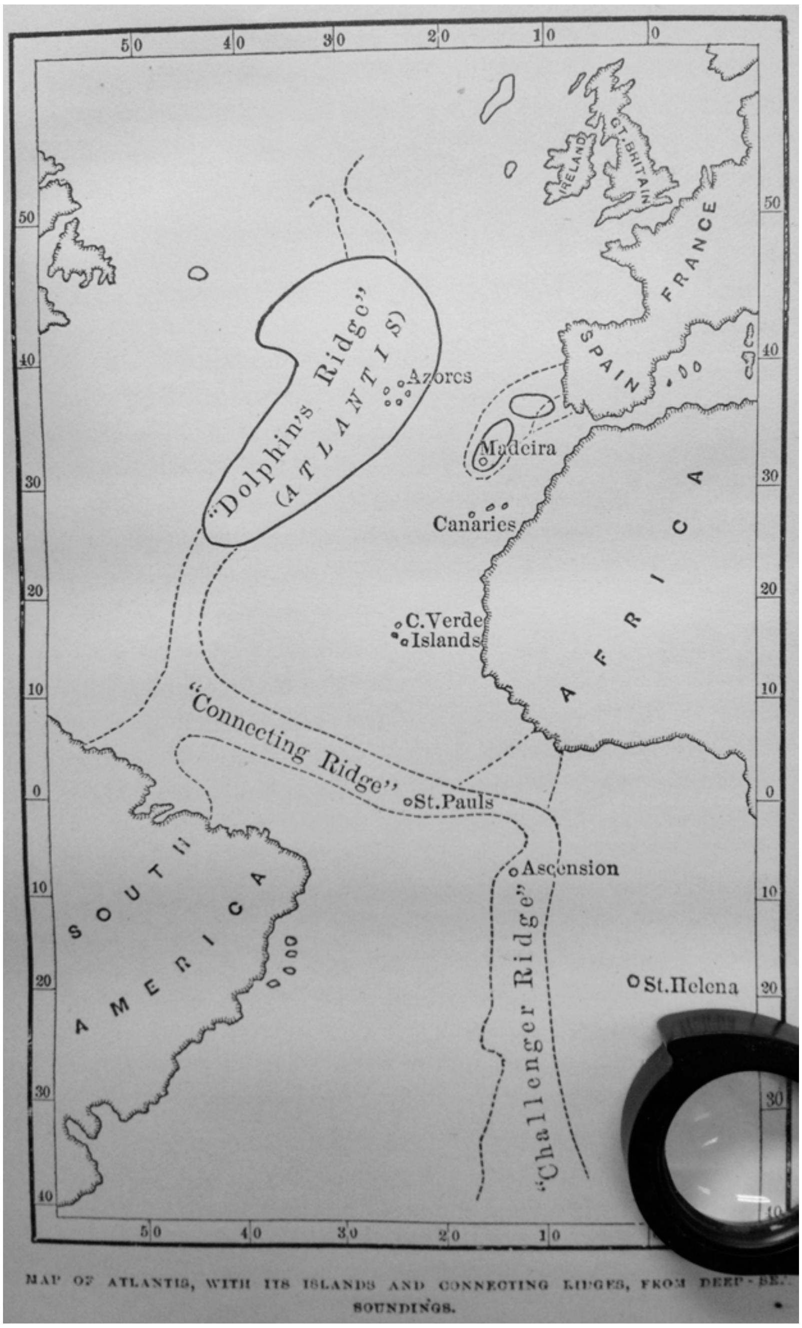


Figure 26. The ridge in the mid-Atlantic, the northern part of which was claimed to be the lost-continent Atlantis. From Donnelly (1882).

Donnelly further explained that the *Challenger* expedition had determined that the surface of the entire ridge consisted of volcanic rocks. His map shows three names for the different parts: Dolphin's Ridge, Connecting Ridge, and Challenger Ridge.

Donnelly's book was widely read, although his ideas were not highly regarded by scientists. But they were also well aware of the Mid-Atlantic Ridge. James Dwight Dana pointed it out in his geology textbooks, beginning in 1883:

Dana 1883, p. 9-10 (Dana's italics).

The form of the ocean's bed. — The accompanying map [Fig. 27] shows the general form of the ocean's bed beneath the larger oceans. From north to south, along the middle of the Atlantic, there is a wide zigzag plateau, *conforming in trend to the American coast*. It lies at a depth of 6,000 to 12,000 feet, while on either side the bottom slopes away to depths mostly between 15,000 and 20,000 feet. In the small area of 4,000 fathoms and over, situated to the northwest of the island of St. Thomas, the United States Coast Survey steamer "Blake" found, in 1883, a depth of 27,366 feet. This greatest depth and the largest Atlantic area of deep water exist in the *western* part of the ocean.

The Pacific also has a central relatively shallow plateau, having the direction of the longer axis between Fuegia and southeastern Asia; and its deepest portions are in the *western* half. One deep area is east of Japan; another, the deepest, south of the Ladrones.

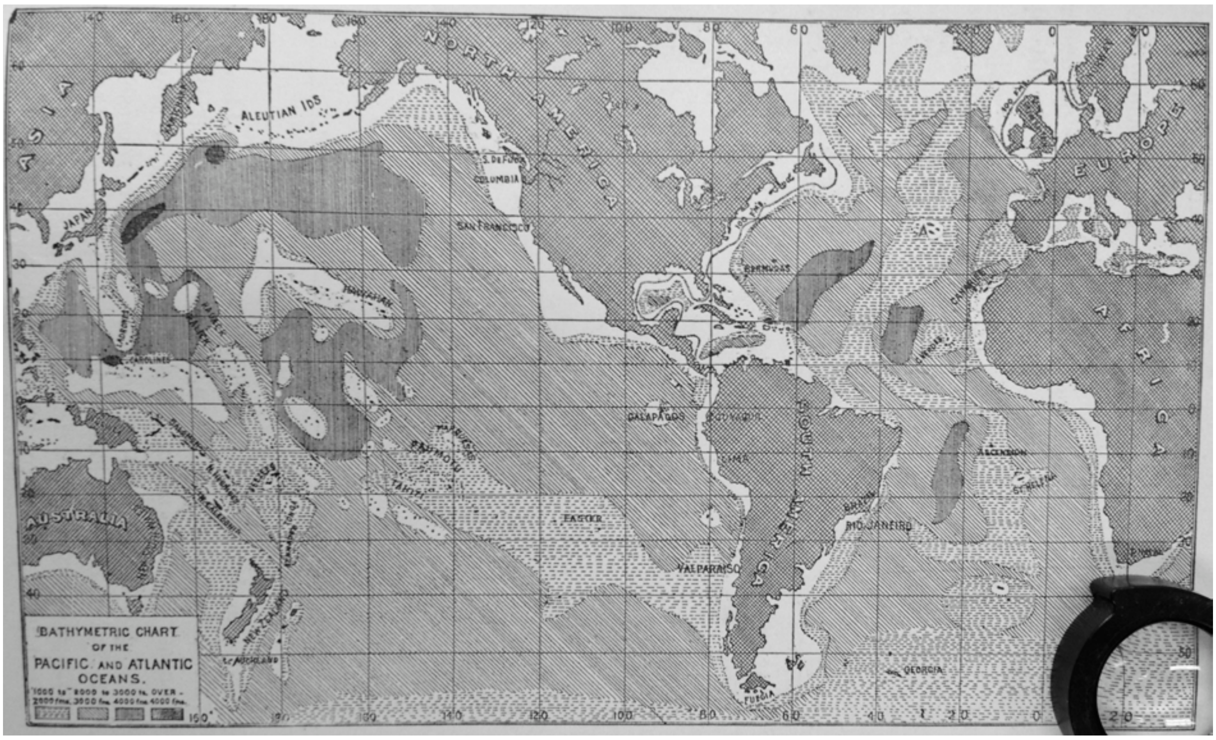


Figure 27. Map of the plateau along the middle of the Atlantic in the *New Text-Book of Geology.* From Dana (1883).

In his 1895 *Manual of Geology*, Dana compiled a new bathymetric map (Fig. 28) that showed depth-sounding data from other sources: the United States and British Hydrographic Department, and the United States Fish Commission. But the ridge was not highlighted as in his 1883-*Textbook* map, which was based on *Challenger* data.

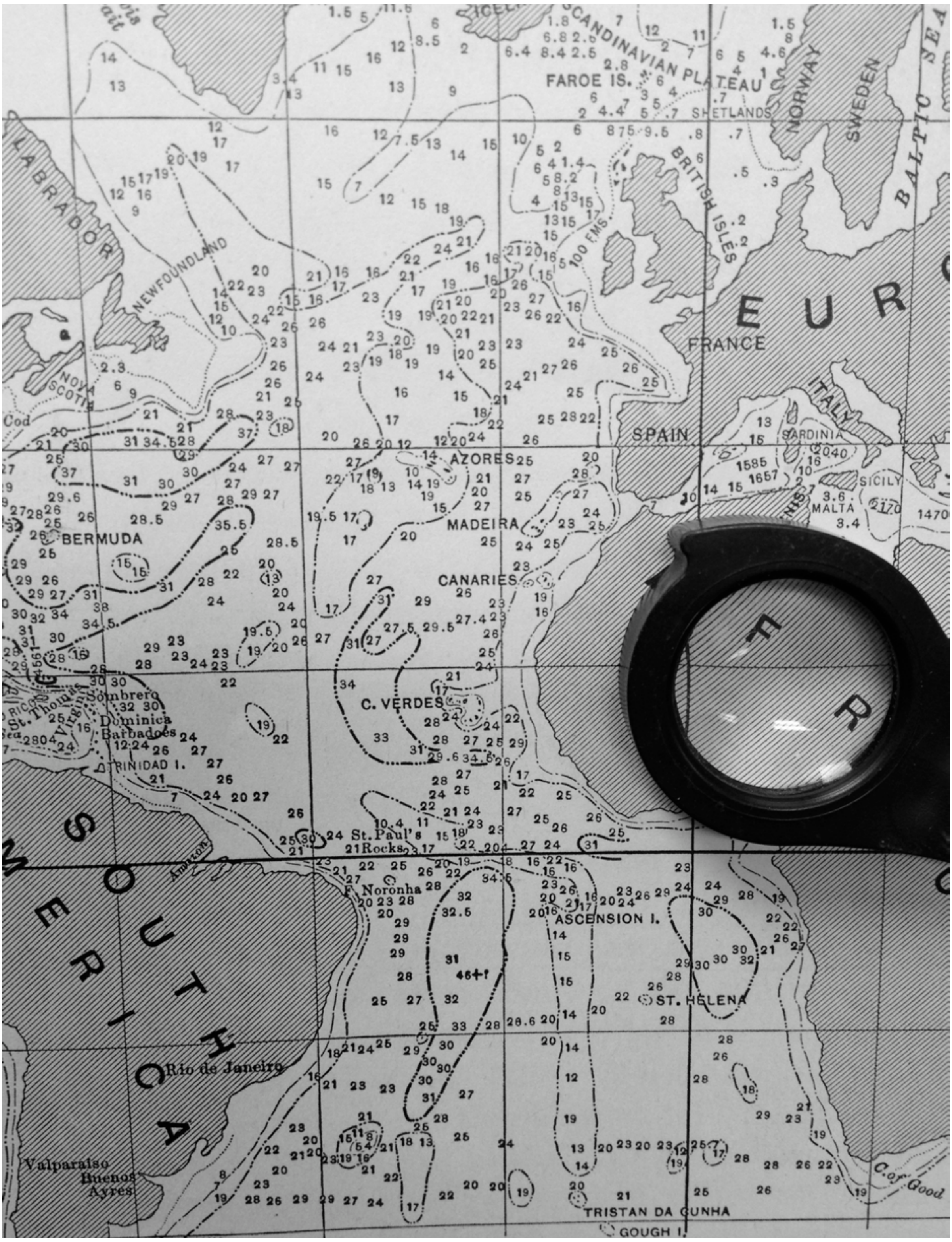


Figure 28. Detail of Dana's *Bathymetric Map of the Pacific and Atlantic Oceans.* Individual soundings are shown, but not enough contours were drawn to clearly highlight the mid-Atlantic ridge. From the 4th edition of Dana's *Manual of Geology* (1895).

In the 1897-volume of Dana's *Revised Text-book of Geology* there was yet a third bathymetric map (Fig. 29). This one shows the narrow Mid-Atlantic Ridge quite clearly. It also shows the broader East Pacific Rise and the Chile Rise, although none of these features had yet been named. German textbooks in geography and geology also showed such maps of the ocean floor, and Wegener was fully aware of these ridges.

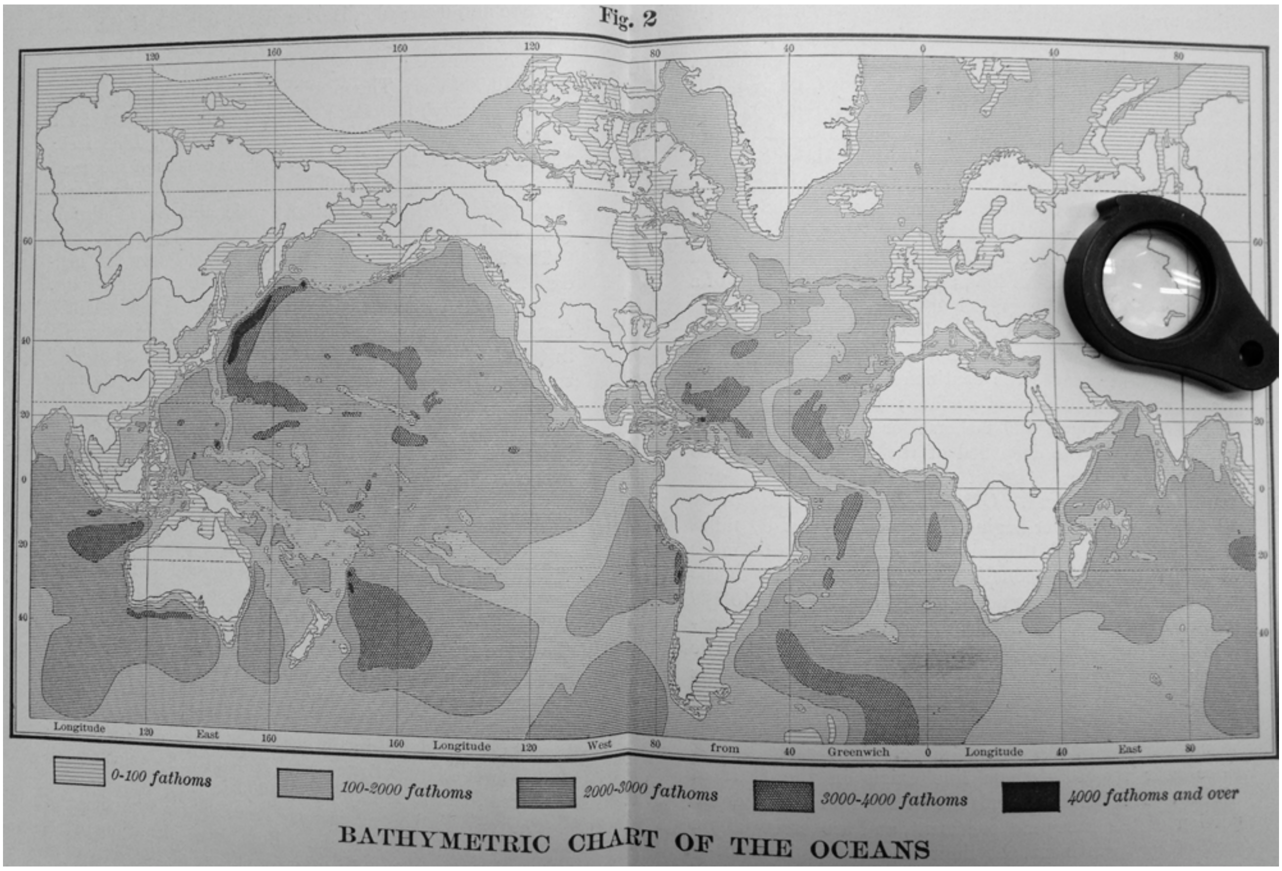


Figure 29. Dana's Bathymetric Chart of the Oceans, showing plateaus known today as the Mid-Atlantic Ridge, the East Pacific Rise and the Chile Rise. From Dana (1897).

Although Dana and others knew of the Mid-Atlantic Ridge, they could not explain its origin. And not all geologists had taken notice of it. In their first geology textbook Pirsson and Schuchert wrote that "the ocean bottom is, in general, monotomonoously level" (Pirson, 1915, p. 83).

Alfred Wegener interpreted the formation of the Mid-Atlantic Ridge correctly in 1912. Few geologists noted his interpretation then or have been aware of it since. It was best formulated in his shorter paper:

Wegener 1912b, p. 281. (translated here from German).

With the splitting of the plate, the underlying highly tempered sima must be exposed, leading to submarine lava eruptions. Especially, this seems for example to be the case with the Mid-Atlantic Ridge. Submarine eruptions take place almost noiselessly. The heavy sima, according to the law of communicating conduits, will only ascend to the extent that isostasy prevails, unless special pressure forces push it higher. Thus the opening of a fissure needs by no means to lead to catastrophic phenomena. Indeed, these movements at the back edge of the moving plate will have in principle few volcanoes, compared to the front edge, where the pressure prevails.

Wegener also gave a remarkably correct interpretation of the relationship between age and depth of ocean floor. It was known that the western part of the Pacific Ocean and the eastern part of the Indian Ocean were relatively deep, and that these deeper parts were of Jurassic age or older. In Wegener's hypothesis, the Atlantic Ocean was younger than this. Because the Atlantic was also somewhat shallower, he suggested that oceanic depth is directly related to the age and cooling of the rocks beneath the ocean floor. Wegener in 1912 was apparently the first to suggest that the Mid-Atlantic Ridge stands high because it is the location of the youngest and most thermally expanded rocks. These interpretations of ocean depths are completely correct, but went unnoticed, and were independently discovered many decades later.

Wegener 1912a, p. 305-306 (translated here from German).

It further occurs to me now, that there is a way to explain the differences of the ocean depths. Since we must assume that also the large areas of the deep ocean floor are in isostatic compensation, the difference indicates in our view that old deep ocean floors are specifically heavier than the young ones. Now the thought is not to be dismissed that freshly exposed sima surfaces, like the Atlantic or western part of the Indian Ocean, retain for a long time not only a lower rigidity, but also a higher temperature (perhaps an average of around 100° higher throughout the uppermost 100 km) than the old, already strongly cooled down ocean floors. And such a temperature difference would probably be sufficient to explain the relatively slight differences in depth between the big oceanic basins, even if it is not sufficient, as earlier mentioned, to explain the weight difference between continental and oceanic material. These differences in level also seem to suggest that the mid-Atlantic swell is to be considered as that zone in which the bottom of the still progressing widening of the Atlantic Ocean bursts continually and makes room for fresh, relatively fluid and hot sima from depth.

2) The cubic coefficient of expansion of granite is 0.0000269. A 100° rise in temperature results in an expansion of the volume by 0.00269. This would also be the decrease of the specific weight if it had first been 1. If it was 2.9, then one gets for sima of 0° a specific weight of 2.900, for sima of 100° a specific weight of 2.892. With isostasy, an already noticeable difference in level must result from this difference in weight.

These paragraphs are typical of Wegener's work. He documented and quantified his ideas whenever possible.

Also in these 1912-papers, Wegener sensed the correct mechanism for continental displacement. He stated in passing that the cause was probably some sort of convection in the mantle:

Wegener 1912b, p. 281 (translated here from German).

Probably one would do well, for the time being, to regard the displacements of the continents as the results of random currents in the Earth's interior; it seems to me that the time is not yet ripe for an analysis of the causes.

But after his 1912-papers, Wegener abandoned the idea that internal currents caused the displacement. Thus his interpretations in 1912 were best in several ways: the thermal cause of elevation of the Mid-Atlantic Ridge, the volcanic spreading of the Atlantic Ocean out from the ridge, and the deep-current mechanism of continental displacement. Unfortunately for his theory, he turned away from these suggestions, and geologists hardly noticed that he had made them.

OSMOND FISHER'S SUB-OCEANIC CONVECTION CURRENTS (1889)

Wegener came close to understanding the geometry and mechanism of plate tectonics already in 1912. But in fact the British geologist Osmond Fisher (1817-1914) came even closer in the book *Physics of the Earth's Crust*, which Wegener referred to. It is a very mathematical book, with formulas and calculations on nearly every page. In the concluding Summary (pages 342-381) Fisher recapitulated Earth processes without the math. Although Fisher incorrectly envisioned the mantle to be liquid, it flows as if it were. Here are some remarkably correct statements on the formation of oceanic crust, the Mid-Atlantic ridge, and the origin of the Atlantic by horizontal displacement of continents (at an early stage in Earth's formation):

Fisher 1889, p. 342.

We have commenced our discussion of the wide subject of the Physics of the Earth's Crust with underground temperature, because the distribution of heat in the interior of the earth is one of the cardinal conditions upon which all physical questions connected with it depend. We have pointed out that, having regard to such depths as artificial excavations reach, the law of increase is on the whole an equable one, amounting on an average to about one degree Fahrenheit for every 51 feet of descent, if it be not even slightly more rapid.

Fisher 1889, p. 349.

The conclusion is inevitable that, if the crust of the earth is about 25 miles thick more or less, the liquid stratum which underlies it cannot be inert. The crust cannot be in the course of formation out of a motionless liquid; but rather the bottom of the crust must be prevented from freezing, as fast as it would otherwise do, by heat being brought up by some means from below, so as to melt it off somewhat less rapidly than the freezing would otherwise progress. The only way in which this can be effected is by convection currents with the substratum. The case is analogous to that of a sheet of ice forming upon a lake from the bottom of which warm springs arise. Every skater knows how thin and treacherous the ice is apt to be on such a lake.

Fisher 1889, p. 355.

The shifting of the crust toward a mountain-range, which is testified by the corrugation of the rocks of which it is fomed, requires a more or less liquid substratum to admit of it. The sinking of areas such as deltas, and other regions of deposition, demands a like arrangement: and in short, it appears that the crust, in the form in which it exists, must be in a condition of approximate hydrostatic equilibrium, such that a considerable addition of load will cause any region to sink, or any considerable amount denuded off an area will cause it to rise.

Fisher 1889, p. 358.

We next attempt to deal with the condition of the earth's crust just described. It will be observed that it is analogous to the case of a broken-up area of ice, refrozen and floating upon water. The thickened parts which stand higher above the general surface also project deeper into the liquid below. There will also be what we call an effective level belonging to the liquid, which is the level to which, if it was inert, it would rise in a hole carried through the crust. We have supposed the crust to have the specific gravity of granite, and the liquid substratum to have that of basalt. Hence their ratio is about 0.905, whilst the specific gravity of ice is 0.9176. These numbers are so nearly the same that the two cases are exceedingly analogous, and the downward protuberance of the crust, as compared with the elevations above the surface, will agree closely with the immersed part of an iceberg as compared with the part exposed.

Fisher 1889, p. 360-361.

Now supposing that a tract of the crust crushed together by lateral compression; and that about two-fifths of the thickness goes up, and three-fifths goes down. If it were to remain in this position, we should have the ratio of the part above the effective level of the liquid to the part below it as 2 to 3. This would be impossible if it floated; just as it would be impossible that an iceberg should stand 200 feet above the water while only 300 feet were immersed. But the tract of crust does not exactly float; for it is held up to some extent by its attachment to the neighbouring crust. Nevertheless it cannot be held up long in what would be so constrained a position. It must then sag downwards; and the most thickened part would sink the most. Hence depressions would arise on both sides of the ridge, and the ocean, which covers the general surface, would be deeper than elsewhere along two channels parallel to, and at some little distance from, the ridge. But should the ridge be steeper on one side than on the other, as seems inevitable, the ocean would be deeper on the steeper side. This relative position of the depths of the ocean to mountain-chains is in accordance with nature.

Fisher 1889, p. 360-361.

We apply to the downward protuberance of the crust into the substratum, under any elevated tract, the popular expression of "roots of the mountains". The existence of these roots of the mountains are not a mere matter of speculation. They have been felt by aid both of the plumbline and of the pendulum. The great mass of the Himalaya mountains was, during the Indian Trigonometrical Survey, found to attract the plumbline...

Fisher 1889, p. 365.

There are currents ascending from the depths below under the oceanic areas. The general result is in strict accourdance with the conclusion arrived at in Chapter VI upon entirely different grounds. We have gained besides the additional information as to the situation of the rising and descending currents, for the latter must necessarily occur beneath the continents, where the substratum is more dense. The immediate cause of such currents prevailing is the heat of the interior, which is everywhere present, ...

Since upward convection ceases at the sea-board, where the currents will become horizontal, there must be some depth of the ocean which corresponds to a maximum play of rising currents. This may probably be indicated by areas that, owing to the great upward pressure, may be slowly rising, so as to form the remarkable plateaux which occupy extensive tracts of the sea bottom. It is on these plateaux that the volcanic islands of mid-ocean are based; and it is obvious that upward currents of the intensely hot magma, pressing against the underside of the crust, is exactly that which would tend to rupture it, and open fissures, and originate volcanic vents.

Fisher 1889, p. 376-377.

Now the calculations in the seventeenth chapter lead to the conclusion, that the ascending currents are situated beneath the oceanic areas and the descending currents are situated beneath the land. Consequently the liquid magma must flow from the oceanic toward the continental areas, and must acquire a more or less horizontal motion as it approaches the main coast lines. It is clear that this will tend to press the supernatant crust from the oceanic towards the continental areas, and to produce compression along their common boundary. As soon as compression begins to take effect, roots to the elevated portions will be produced, which dipping into the substratum will offer an increasing obstacle to the flow of the currents, and intensify their operation. Thick deposits also, by depressing the crust into the liquid would have a similar result, and thus encourage local compression.

Although the movement of the currents is no doubt slow, yet it must be remembered that the density of the material is greater than that on which it operates. The action would resemble that which we may notice when cakes of ice float down a river, and impinge upon an icy barrier, where, their edges tipping down into the influence of the stream, the blocks get pushed over one another, and piled together, much like strata dislocated by thrust planes.

Where the currents ascend beneath the ocean they would give rise to a tensile stress, the correlative of the compression of the land. Fissures would thus be produced, which would open volcanic vents, and, when filled with solidified lava, become dykes of igneous rock in the suboceanic crust. Such fissures however, owing to the mode of their formation, would not be accompanied by compression. At the same time the varying intensity of the rising currents would cause an instability of the ocean bottom, such as is evidenced by marine deposits being found at various altitudes upon the volcanic islands of mid-ocean.

Fisher 1889, p. 378.

The geographical distribution of volcanos presents fewer difficulties upon the suppostion of a thin crust and liquid substratum, than upon any other. The linear arrangement of many of them points to their being situated along great systems of fissures; and such systems of fissures are indicative of a thin crust. Fissures, which run for long distances in nearly straight course, point, as already mentioned when discussing faults, to a movement perpendicular to the fissured surface; or else they point to a rending pressure within the fissure itself; ...

We recognize two principal types of volcanic regions, coastline and oceanic. We believe the former to be connected with the agencies which have raised the continents which they skirt. Trains of such vents are attached laterally to the great compressed and elevated coast ranges, and usually stand near the edge of a steep shore. The oceanic volcanos on the other hand appear unconnected with compressive action, for the oceanic islands consist almost all of them of volcanic rocks; whereas if they were connected with mountain ranges, the peaks of schistose or other hard inclined strata could not well be absent: moreover fragments of stratified rocks have not been found among the ejecta of these volcanos.

They occupy a medial position with respect to the coast lines, being in the Atlantic rudely parallel to the opposite shores, and in the Pacific always active in the central patch of the Hawaiian Islands.

Fisher 1889, p. 380-381.

We have also seen reason to infer that the crust beneath the oceans has not been compressed into elevated ranges, as it has been in many parts of the land. Still further, we have seen that the oceans lie in veritable depressions or basins, below the spheroidal surface of earth, as if they have been hollowed out.

If the ocean basins have been hollowed out, how was the material removed, and whither has it gone? Professor Darwin's theory of the genesis of the Moon suggests an answer to this question. If, as his investigation renders probable, the moon broke away from the earth fifty million or more years ago, it is worth while to inquire whether the formation of the ocean basins may not have been the consequence of that catastrophe.

The mass of the moon is such that a cavity about forty miles deep, extending over the oceanic areas, would have supplied the requisite material. The density, singularly enough, would have been just right. The matter removed would no doubt in time conglomerate into the lunar sphere, and so we may bid adieu to our satellite. Returning to the earth, we inquire what would happen here. If that was still wholly liquid, or even plastic, the cavity would no doubt soon be obliterated. But if there was already a crust formed, thinner of course than it is now, but yet thick enough to hold together, the part of this crust which covered the place of the cavity would probably go away with the rest of the matter removed. The hole would be filled up by the influx of the molten substratum from beneath and around. The remaining crust would separate into larger and smaller fragments, and partly float toward the cavity. Thus when the newly exposed surface of the molten substratum again solidified, a fresh crust, of greater density than before, would be formed out of the heavy substratum over the middle of the area where the hollow had been made, and also in the channels between the fragments which had floated towards it; the Atlantic being the chief of these channels.

Thus would have been formed at an early period of the earth's history a separation into oceanic and continental areas. The rise of the substratum into the cavity from which the moon's substance was detached, would determine the upward convection currents to those areas, and by the action of these, combined with the other agencies of which geology takes cognisance, the present state of things on the earth's surface would have been gradually evolved.

It might seem preposterous that the Moon could have formed from part of the Earth, resulting in the ocean basins. But that is not so unlike the modern understanding. As Fisher explained, the density of the Moon compares well with that of the crust and mantle of the Earth. The best modern hypothesis is that a Mars-sized planetissimal collided with the Earth, and parts of it, as well as parts of Earth's crust and mantle formed the Moon (see a summary in Condie 2005).

A cavity created by the removal of primordial material would be quickly filled by lateral flow. Fisher, and others in his time (most notably G. H. Darwin, an astronomer and son of Charles Darwin) thought that the Pacific Ocean is a trace of this cavity. The general shape of the Pacific may indeed be old, but not as old as this.

Of course we know now that the Atlantic was formed by horizontal displacement, much later than this early catastrophic event. Fisher did not suggest that the Atlantic is still widening. He thought that once the oceans and continents had found their positions, they were fixed. Only their crusts were deformed and renewed by the convection processes. Fisher correctly understood many phenomena, expecially the existence of convection currents beneath the oceans. But his book was mostly geophysics, and contained no geological illustrations. It was not a work that would catch the attention of most geologists. It would have appealed to the geophysicist Wegener. Although he mentioned it, I am not convinced that he had actually seen it. He did not take advantage of Fisher's insight, and he did not cite it again in his later publications.

WEGENER'S FIRST GEOLOGY BOOK (1915)

In 1915, Wegener enlarged his scientific paper from 20 to 80 pages, and from 10 to 20 figures. He extended the title as well, to now include oceans: *Die Entstehung der Kontinente und Ozeane*. It was published separately as a book in a respected scientific series (Vieweg), not as an article in a scientific journal.

Wegener again printed the statements by Bailey Willis, including the second particularly dogmatic one. Wegener was actually quoting Willis's slightly out of context. "This conclusion" that Willis had referred to, was the theory of isostasy, not the permanence of the oceans. Wegener did not yet know it, but by quoting Willis in a negative way he was identifying, or perhaps creating, a future adversary.

In the 1915 edition, Wegener showed a sketch of continents before the Atlantic Ocean had opened (Fig. 30). Since we have already seen the Snider-fit and the Bullard-fit, it is appropriate to now call this one the Wegener-fit.

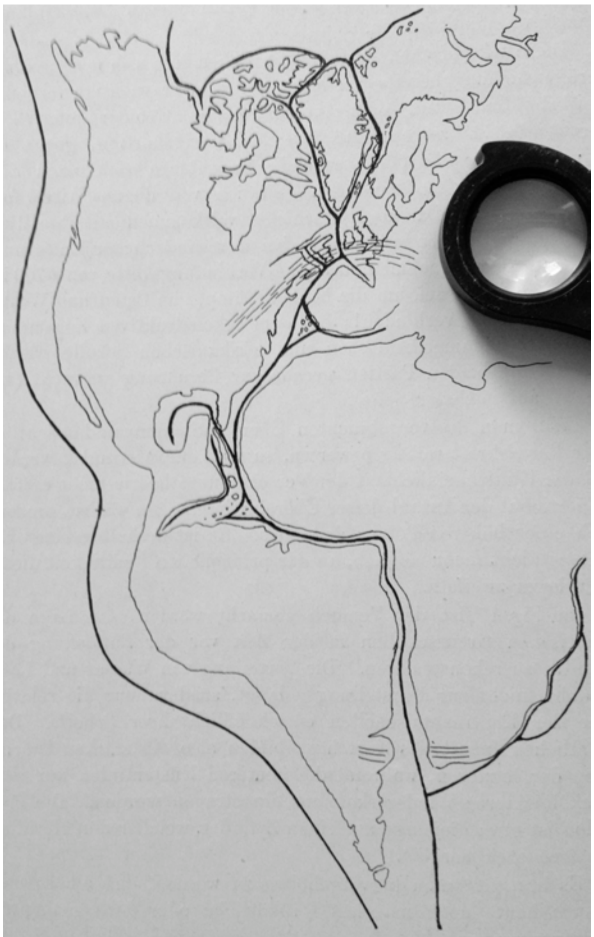


Fig. 17. Rekonstruktion des voratlantischen Kontinentalblocks.

Figure 30. The Wegener-fit. *Reconstruction of the pre-Atlantic continental blocks.* From Wegener (1915).

Wegener did not try to plot much geologic evidence on this fit-map. But note two geologic elements that he did try to represent by dashed lines: the continuation of the fold-belt from France, Britain, and Ireland across to Newfoundland and the Appalachians, and the continuation of the Cape Mountains fold-belt from South Africa across to Argentina. Wegener had discussed the northern fold-belt-connection in his 1912 paper. But the southern connection was a new discovery:

Wegener 1915, p. 67 (translated here from German.)

These tectonic relations have already improved in another very important way since my first publication: Keidel could show at the International Geological Congress in Toronto (1914) that the Cape Mountains, which are very unusual compared to the rest of Africa, continue to South America in the form of the Sierras south of Buenos Aires, to which they correspond completely in construction and geologic history. Here, with fixed continents, one would have to suppose that these stubs were connected in the east and west by a chain 6700-km in length, which has sunk! With reconstructive joining of the South American and African masses, on the other hand, both portions are brought just into contact (cf. Fig. 17 on the next page.)

The mountains that Wegener referred to south of Buenos Aires are generally known as the Sierra de la Ventana. Keidel (1916) wrote a monograph showing how well they correspond in detail to the Cape fold-belt. He did not refer to Wegener or suggest that any continental displacement had taken place.

Wegener illustrated only those two connections in 1915, but he discussed many more. Additional ones would eventually be recognized. His theory had great predictive value for both South American and African geology.

It seems that neither Charles Schuchert nor most other North American geologists ever read Wegener's 1912-articles or saw his 1915-book. In 1928, Schuchert used this same Wegener fit-map (see Fig. 50), which he had taken from a German textbook (Behm 1923), not knowing that it actually came directly from Wegener's first book. Wegener's early work was not easily accessible to North American or British geologists. During the war years 1914-1919, many German publications were unavailable, and in England they were mostly forbidden. When Wegener's work became known in the 1920s, it did not seem necessary to go back and read his earlier versions.

Unfortunately, Wegener abandoned his initial interpretation of the age and origin of the Mid-Atlantic Ridge before publishing his book in 1915. It seems that researchers on the Valdiva- and German South-Polar Expeditions had collected rocks from the Mid-Atlantic Ridge that they interpreted as being continental in character. Upon learning this, Wegener concluded that the ridge included parts of the rifted continents. If that were the case, its higher level would be explained by the occurrence of those bouyant rocks. The ridge could not represent the youngest part of the ocean, as he had suggested in 1912. He no longer suggested that there was ongoing fissuring and volcanism along the ridge. Here is how Wegener misinterpreted the Mid-Atlantic Ridge in 1915 and in all his later works:

Wegener 1915, p. 69-70 (translated here from German.)

It should be considered immediately at this point the question of how the origin of the Mid-Atlantic Ridge can be interpreted from the point of view of the displacement theory. From isostatic considerations, the swell must consist of light material, which because of its low density likely comes from the continents, either from above (sediment), or from below (melted sal). In any case, one may suppose that it itself is the former graben sole from the time when the Atlantic represented only a relatively narrow rupture zone which was filled with sunken border areas, coast sediments and perhaps also melted salic masses. All the islands that today crown the long swell probably already appeared at this time as broken pieces of the rift edges. When in the further course of the displacement the Atlantic sima surface stretched out like rubber, this more brittle material did not participate in the expansion, but always remained collected, midway between both continents.

Wegener had it right in 1912, but not in 1915, where he dropped the volcanic interpretation of the Mid-Atlantic Ridge. The Dutch professor G. A. F. Molengraaff (1860-1942) read Wegener's book and saw how his interpretation could be improved. He published the following suggestion, in English, citing Wegener's pages 68 and 69:

Molengraaff 1916, p. 625-626.

Perhaps we may see in this remarkable Mid-Atlantic Ridge the final result of volcanic activity along an enormous fracture of the same extent, where from numerous fissures and vents volcanic material was discharged, thus a volcanic mountain-chain and cones being formed, which nowadays subside through yielding under the influence of gravity and nearly all have sunk back to a level approaching the average level of the deep submarine ridge. Here and there a few islands, where volcanic activity lasted longer or has existed to this day, still rise above the sea, and others (of which naturally only a few have been discovered accidentally by soundings) still rise to different heights above the average level of the ridge but no longer attain the surface of the sea. Among these latter we mention three submarine mountains which near the western part of the Azores rise from the bottom of the ocean, which has there a depth of about 3,000 meters, to elevations of 146, 128, and 88 meters, respectively, below sea-level. The cause for the extrusion of such enormous masses of volcanic material might perhaps be sought in the disruption of the American continent from the European-African one with which it formerly cohered. This disruption was assumed by Pickering and Taylor and a plea for it is again brought forth by Wegener on page 68 of his paper quoted before. On this supposition the Mid-Atlantic Ridge would in my opinion indicate the place where the first fissure occurred and the sima was first laid bare. From this it would follow logically that the ridge itself must consist entirely of sima and not of sial, as Wegener assumes on page 69.

Most contemporary geologists missed the interpretation by Molengraaff. So in 1928 he published it again, this time in a widely read American symposium volume on continental drift. That second time it was surely noticed by those who were seriously interested in the continental-drift debate.

Molengraaff was another talented geologist for whom travel was of first, second, and third importance. This interpretation of the Mid-Atlantic Ridge was written in the context of studies of the coral reefs of the South Pacific. And earlier in his career, he had mapped the Transvaal of South Africa, and provided excellent descriptions of the Permo-Carboniferous tillites and glacial striations there.

WEGENER'S SECOND AND THIRD EDITIONS (1920, 1922)

The second edition of Wegener's book came in 1920 with 130 pages and 33 figures, and the third edition in 1922 had 140 pages and 44 figures. It was essentially the same book, but improving with each edition. In the second and third editions Wegener removed Willis's conclusion. But he still used Willis's first statement, and continued to label Willis as an opponent to the displacement hypothesis.

In his second edition in 1920, Wegener illustrated the whole supercontinent (Fig. 31), not just the part that borders the present Atlantic Ocean.

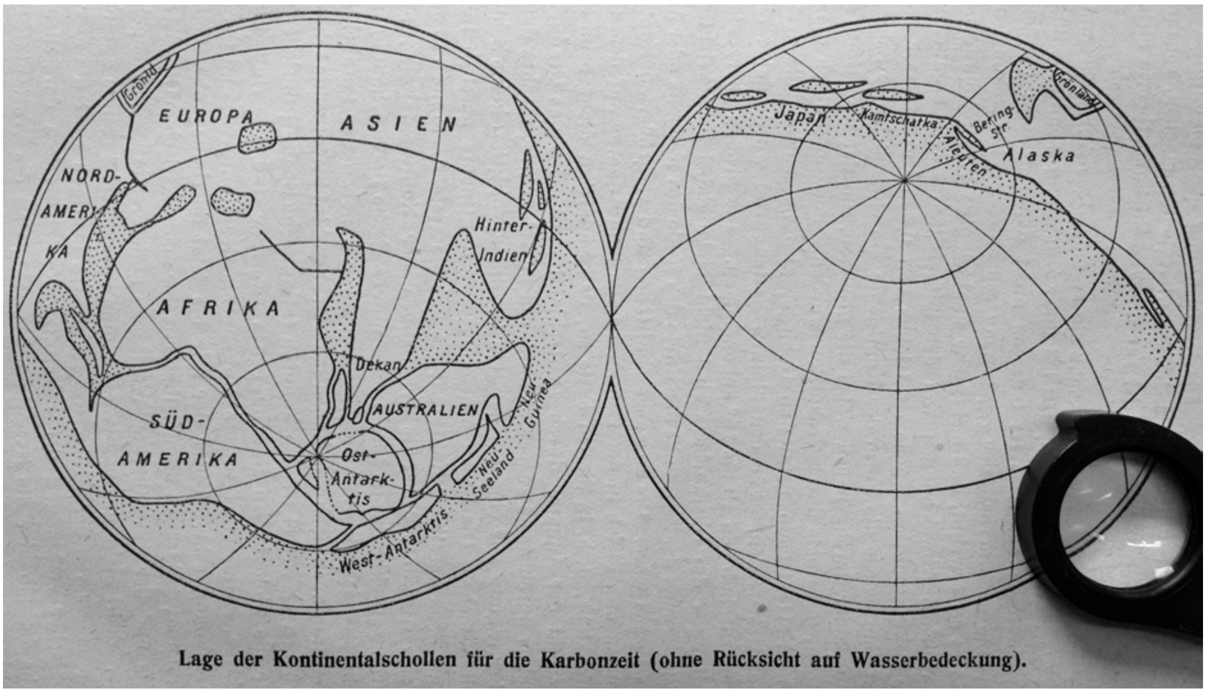


Figure 31. Wegener's first published illustration of the supercontinent. *Position of the continents in Carboniferous time (without consideration of water-covered areas*). From Wegener (1920).

His second book was again in German, and not readily available to English-speaking geologists. A book review in English was written for *Geological Magazine* by Philip Lake (1865-1949). He was well known for his textbook on the geology of Great Britain (Lake and Rastall, 1910.) From this textbook one sees that Lake was preoccupied with local descriptive detail, but uninterested in climate or environmental conditions. Lake's review of Wegener's book was entirely derogatory. He chose his words carefully for maximum negative impact. Here are the first 2 of his 38 paragraphs:

Lake 1922, p. 338.

Wegener's Displacement Theory

In examining ideas so novel as those of Wegener it is not easy to avoid bias. A moving continent is as strange to us as a moving earth was to our ancestors, and we may be as prejudiced as they were. On the other hand, if continents have moved many former difficulties disappear, and we may be tempted to forget the difficulties of the theory itself and the imperfection of the evidence. Those who study the distribution of animals and plants must be especially subject to this temptation, and it will be instructive to note how far they agree in their demands upon the moving continents.

Wegener himself does not assist his reader to form an impartial judgment. Whatever his own attitude may have been originally, in his book he is not seeking truth; he is advocating a cause, and is blind to every fact and argument that tells against it. Nevertheless, he is a skilful advocate and presents an interesting case. Perhaps he may claim that if his readers cannot approach the subject without prejudice, he can hardly be expected to perform the functions of a judge. He does not make the attempt.

Lake's review left the reader with a thoroughly negative impression of Wegener and the displacement theory. English-speaking geologists had easy access to Lake's critical review, while Wegener's book was essentially unavailable to them. So before most geologists had a chance to read Wegener's own work, they were prepared to disregard it.

In the same year as this review appeared, Wegener came out with a third edition, again in German. It was somewhat improved, but not much enlarged. The publication was part of the same book series as his first and second editions, and was again subject to strict page limitations. Wegener had to be very selective. He chose to eliminate a few of his earlier illustrations, such as the Atlantic fit-map, which he had improved for the 1920-edition. Instead, he showed the positions of continents on the globe at three different time periods. He also cut short the chapter on ancient geological climate zones, even though the identification of distinct paleoclimate belts was among his most convincing evidence. He explained in the preface that he chose not to expand that chapter because he would soon publish a much more detailed work on paleoclimates, together with the well-known climatologist Wladimir Köppen.

Wegener's theory was favorably received in Germany, and many foreign geologists and paleogeographers were also interested in it. Wegener's third edition was translated into English, French, and Spanish in 1924, into Russian in 1925 and Swedish in 1926. Since none of Wegener's other works were translated to English, this was the book that would represent Wegener's evidence to English readers for the next 42 years. It was unfortunate that there had been page restrictions in the German version, so that most of Wegener's paleoclimate map-evidence was not included.

Sir John W. Evans (1857-1930), president of the Geological Society of London, helped to promote the English translation of Wegener's book, which now had the title *Origin of Continents and Oceans*. Evans also established the word *Pangäa* as the name of Wegener's supercontinent. Wegener had not intended to name the supercontinent at all. He referred to it simply as the primordial continent. He called the ocean *Panthalassa*, or all-sea, the term already in use for what geologists imagined to have been the Earth's primordial ocean. In a discussion of the mechanism for displacing continents, Wegener happened to mention (p. 131) *die Pangäa der Karbonzeit* (the all-land of Carboniferous time.) The word Pangäa was capitalized in German simply because it was a noun. Evans wrote the Introduction to the English edition of Wegener's book, and there he used the word Pangæa as a proper name. He also saw to it that Pangæa was included as a proper name in the Index of the book. So now the name *Pangæa* was available for this ancient supercontinent. Wegener, however, preferred not to use it in his later publications. I think that he was too modest to use a formal name for the supercontinent that he had discovered.

Evans understood how important it was that Wegener's hypothesis and evidence be given a fair hearing. He ended his Introduction to Wegener's book with these comments:

Evans, in Wegener 1924, p. xii.

Whatever may be the outcome of these observations and whatever modifications may prove to be required in the author's views on the evolution of the present configuration of land and sea, he has done a most valuable service in directing attention to a new and important element in the transformations that the world has suffered, an element which no one will henceforth be able safely to ignore.

I have elsewhere criticized some of the details of the author's conclusions. It would be out of place to repeat these criticisms here. My only care has been to ensure that in this translation he should be allowed to state his own case in his own way. With this object the translation has been submitted to Professor Wegener and has been carefully revised by myself and it may be regarded as an accurate and authoritative exposition of his views.

Evans felt that no one could "safely" ignore Wegener's ideas of continental displacement. But most geologists did ignore them, and the field of geology was exposed to unrecognized dangers.

CHAPTER 1 OF WEGENER'S ENGLISH EDITION (1924)

Wegener's first interpretation of the Mid-Atlantic Ridge was missed because geologists did not read Wegener's original work. But his third edition was translated to English, and was widely read. The entire first chapter is reprinted here because it introduces Wegener, and demonstrates his style and many aspects of his theory. It is the chapter quoted most often by those who said that they had read his book, but rejected his hypothesis.

Wegener 1924, p. 1-10.

THE ORIGIN OF CONTINENTS AND OCEANS

I. THE ESSENTIALS OF THE DISPLACEMENT THEORY

CHAPTER 1

THE DISPLACEMENT THEORY

He who examines the opposite coasts of the South Atlantic Ocean must be somewhat struck by the similarity of the shapes of the coast-lines of Brazil and Africa. Not only does the great right-angled bend formed by the Brazilian coast at Cape San Roque find its exact counterpart in the re-entrant angle of the African coast-line near the Cameroons, but also, south of these two corresponding points, every projection on the Brazilian side corresponds to a similarly shaped bay in the African, and conversely each indentation in the Brazilian coast has a complementary protuberance on the African. Experiment with a compass on a globe shows that their dimensions agree accurately.

This phenomenon was the starting-point of a new conception of the nature of the earth's crust and of the movements occurring therein; this new idea is called the theory of displacement of continents, or, more shortly, the displacement theory, since its most prominent component is the assumption of great horizontal drifting movements which the continental blocks underwent in the course of geological time and which presumably continue even to-day.

According to this idea, to take a particular case, millions of years ago the South American continental plateau lay directly adjoining the African plateau, even forming with it one large connected mass. This first split in Cretaceous time into two parts, which then, like floating icebergs drifted farther and farther apart. Similarly, North America was close to Europe; and, at least from Newfoundland and Ireland northward, they formed with Greenland one connected block, which broke up by a forked rift near Greenland at the end of Tertiary time and farther north even in the Quaternary era; whereupon the constituent blocks moved apart from one another The shelves, the portions of the continental masses overflowed by shallow seas, will always be considered in this book as parts of the blocks, the boundaries of which for great distances are not given by the coast-lines, but by the steep descent to the deep sea floor.

Similarly, it will be assumed that Antarctica, Australia and India lay adjoining South Africa, and with the latter and South America formed, until the beginning of the Jurassic period, a single large—even if partly submerged at times by shallow water—continental area, which in the course of Jurassic, Cretaceous and Tertiary time split and crumbled into smaller blocks which drifted away in all directions. The three maps of the earth reproduced in Figs.1 [Fig. 32] and 2 show these developments during the Upper Carboniferous, Eocene and Lower Quaternary periods. The case of India is somewhat different: it was originally connected by a long continental tract, mostly, it is true, covered by shallow seas, to the Asiatic continent. After, the separation of India from Australia on one side (in the Lower Jurassic) and from Madagascar on the other (during the transition from the Chalk to Tertiary) this long connecting portion was more and more folded together through the continuous gradual approach of India to Asia and constitutes to-day the mightiest mountain fold of the earth, the Himalayas and the numerous folded ranges of the high lands of Asia.

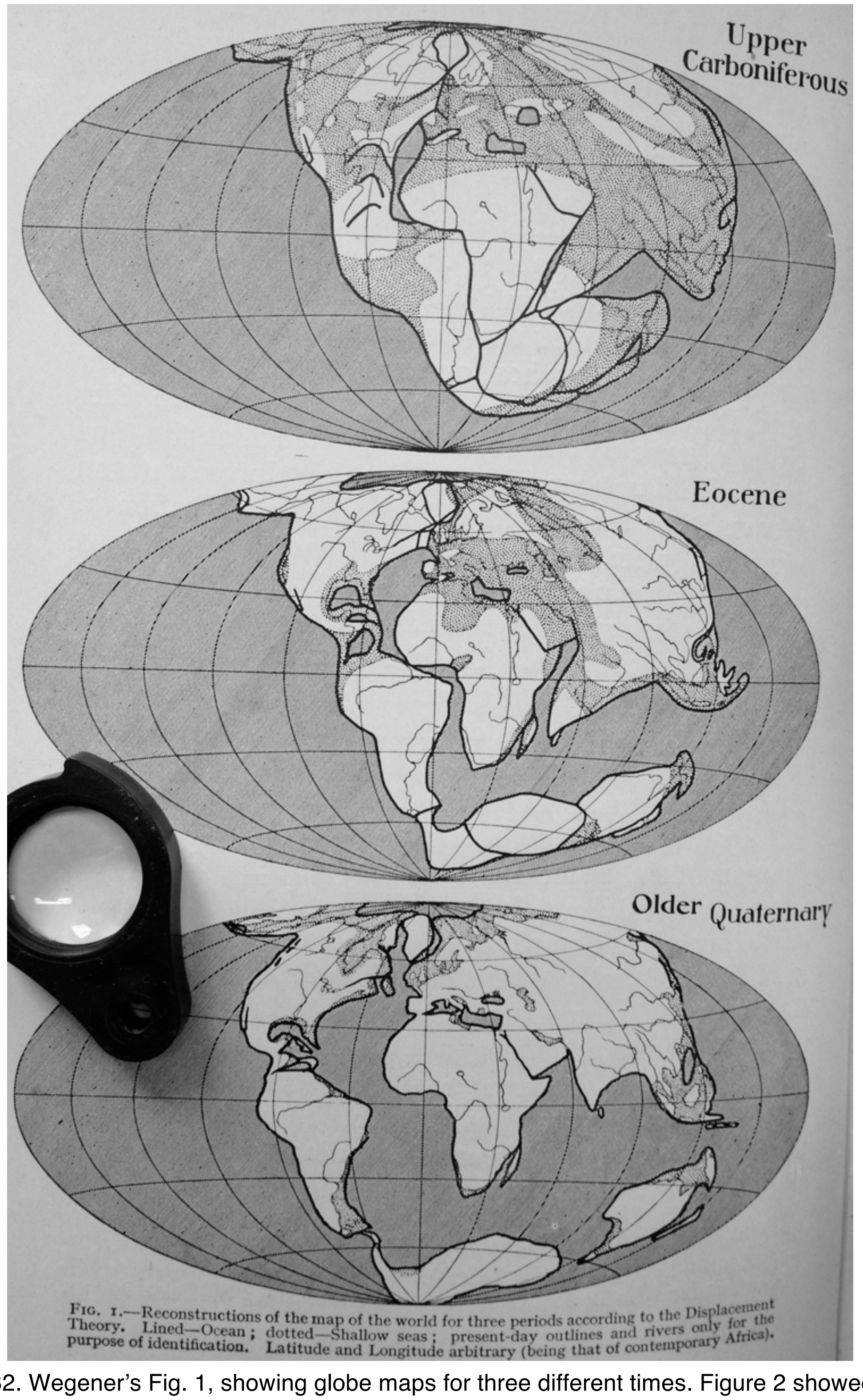


Figure 32. Wegener's Fig. 1, showing globe maps for three different times. Figure 2 showed these same reconstructions from another map perspective. From Wegener (1924.)

The preceding four paragraphs and his Figures 1 and 2 agree reasonably well with modern plate-tectonic ideas. The next five paragraphs, reprinted below, contain many misunderstandings:

In other regions also the displacement of the blocks occurs in causal connection with the origin of the mountain systems. By the westward drift of the two Americas their anterior margin was folded together to form the mighty range of the Andes (which stretches from Alaska to Antarctica) as a result of the opposition of the ancient well-cooled and therefore resistant floor of the Pacific. A similar case is that of the Australian block, which includes New Guinea since it is only separated therefrom by a shelf. The recent high ranges of New Guinea occur on the side which is anterior with reference to the movement; as our map shows, this direction of movement was different before the break from Antarctica, for the present east coast was then the front side. Next the mountains of New Zealand lying immediately in front of this coast were folded, later becoming detached as festoons of islands by the altered direction of movement and then lagging behind. The present cordilleras of East Australia originated in a still older period; they were formed (at the same time as the more ancient folds, “Pre-cordilleras,” in South and North America, which are the foundations of the Andes) on the front margin of the continental masses, which were drifting as a whole before the separation.

Besides this westward wandering we also see to a large extent a striving towards the equator of the continental blocks. With this is connected the formation of the great Tertiary belt of folding stretching from the Himalayas to the Alps and Atlas mountains which were then in the equatorial zone.

The previously mentioned separation from the Australian block of the former coastal ranges of New Zealand, forming later a festoon of islands, leads us up to the phenomenon that smaller portions of the blocks are left behind by the westward wandering of the larger blocks. In this manner the marginal ranges of the East Asiatic continental coast separated as festoons of islands. The Lesser and Greater Antilles lag behind the movement of the Central American block, and similarly the so-called arc of the South Antilles between Patagonia and West Antarctica. Indeed all blocks tapering in a meridional direction show a curve of their points towards the east on account of this lag. The latter is well shown in the south point of Greenland, the submarine shelf of Florida, Tierra del Fuego and Graham Land, and the manner in which Ceylon has broken away from India.

It will easily be seen that this complete and extensive conception of the displacement theory must emanate from a definite acceptance of the relation of the oceans to the continental blocks. In fact, it is assumed that these two phenomena are fundamentally distinct, that the continental blocks with a thickness of about 100 km. swim in a magma out of which they only project about 5 km. and which is uncovered in the floor of the oceans.

Thus the outermost lithosphere no longer completely covers the entire earth (whether it ever did can be left undecided), but has become smaller and smaller by continued folding and compression during the course of geological time, thereby increasing in thickness and splitting ultimately into more and more separated smaller continental blocks. The latter to-day cover but a quarter of the earth. The floors of the oceans form the free surface of the next layer of the body of the earth which is also assumed to exist under the continental blocks. The existence of this involves the geophysical side of the displacement theory.

The detailed establishment of this new hypothesis will form the major part of the book. Some historical remarks, however, should be given beforehand.

The first notion of the displacement of the continents came to me in 1910 when, on studying the map of the world, I was impressed by the congruency of both sides of the Atlantic coasts, but I disregarded it at the time because I did not consider it possible. In the autumn of 1911 I became acquainted (through a collection of references, which came into my hands by accident) with the palæontological evidence of the former land connection between Brazil and Africa, of which I had not previously known. This induced me to undertake a hasty analysis of the results of research in this direction in the spheres of geology and palæontology, whereby such important confirmations were yielded, that I was convinced of the fundamental correctness of my idea. I first brought forward the idea on January 6 th, 1912, in a lecture to the Geological Association of Frankfurt-on-Main entitled "Die Herausbildung der Grossformen der Erdrinde (Kontinente und Ozeane) auf geophysikalischer Grudlage." This lecture was followed on January 10th by a second on "Horzontalverschiebungen der Kontinente" to the Society for the Advancement of Science of Marburg. In the same year (1912), also, both of the first publications on the theory took place.[1]

Afterwards the participation in the traverse of Greenland under J. P Koch of 1912/13 and later war-service hindered me from further elaboration of the theory. In 1915, however, I was able to use a long sick-leave [Wegener's brother Kurt produced a fifth edition his book (Wegener 1936). In the preface he explains that Alfred's sick leave was the result of his being shot in the neck during war duty.] to give a somewhat detailed description of the theory in the Vieweg series under the title of this book.[1] As a second edition of this was necessary after the close of the war, the publishers generously consented to transfer the book from the Vieweg to the Wissenschaft series, whereby the possibility was given for a considerably enlarged work.[2] The present edition is again virtually rewritten, as the process of the grouping of the data which affect the question according to the view-point of the new theory has meanwhile made further progress and an extensive recent literature about the subject has appeared.

During the above-mentioned work of examining the literature I several times chanced on views concordant with my own by older authors. Thus a rotation of the entire crust of the earth—but whose parts, however, did not alter their relative positions—had already been assumed by many authors, as Löffelholz von Colberg,[3] Kreichgauer,[4] Sir John Evans and others. H. Wettstein has written a remarkable book,[5] in which, however, among many absurdities, a leaning towards great relative horizontal displacements of the continents is shown. The continents, (the submarine shelves of which however he did not consider) undergo, according to him, not only displacement but also deformation; they wander collectively westwards drawn by the tidal forces of the sun on the viscous body of the earth (as also E. H. L. Schwarz assumed in the *Geogr. Journ*., 1912, pp. 284-299). But the oceans were considered by him as sunken continents, and he expressed fantastic ideas about the so-called geographical homologies and other problems of the face of the earth, which we will pass over. Like the present writer, Pickering in a work on the similarities of the South Atlantic coasts [1] has expressed the supposition that America was torn off from Europe-Africa and was dragged across the breadth of the Atlantic Ocean. But he did not consider that all the facts of the geological history of both these continents necessitated the assumption of an earlier connection up to the Cretaceous period, and thus he places the connection in the very remote past, and thought that the breaking away was connected with the theory of G. H. Darwin that the moon was thrown off the earth. Traces of this he believed are still to be seen in the Pacific basin.[2]

F. B. Taylor approaches the sphere of the displacement theory in another way. In a work which first appeared in 1910 [3] he assumes not unimportant horizontal displacements of the individual continents in Tertiary time and brings them into connection with the great Tertiary systems of folding. As an example he comes to practically the same view as that of the displacement theory to explain the separation of Greenland from North America. It is true that in the case of the Atlantic Ocean he assumes that only a portion of its width is due to the dragging away of the American block, whilst the remainder has been submerged and forms the elevation in the floor of the Middle Atlantic. Like Kreichgauer, Taylor sees in the drift of the land from the poles the guiding principle in the disposition of the great mountain ranges, whilst the displacement of continents plays but a minor role and indeed is only briefly treated.

As already mentioned, I became acquainted with all these works only when the displacement theory in its main outline had already been worked out, and with many others considerably later still. The possibility is not ruled out that in the course of time still further works will be discovered which will be in accordance with the displacement theory or which will anticipate this or that point. On this subject a historical examination has not yet been instituted and is not intended in the present book.

In the final paragraph of this first chapter, Wegener's comments suggest that he was not aware of the book by Pepper (1861) when he began his project. That book may have been one of "many others" which he came across later. But if he did see it, he did not pick up the suggestion that continents move by creation and destruction of crust. Pepper had not presented that idea clearly, and only someone who had read Hopkins' book as well, would have gotten it. Hopkins' book was quite rare.

Note that in his Figure 1, Wegener chose Africa as the frame of reference, keeping it in the center of his displacement maps. He moved the other continents around, but not Africa. He did this because Africa was more or less in the center of the supercontinent. If the other continents were depicted as moving away from it, there would be less distortion by map projection as they moved to distant parts of the globe. This was simply a presentation technique, but it required that the North and South Poles also move on the maps. Wegener thought the poles of rotation had moved, but he wanted to keep this hypothesis open. Since he did not want to fix the poles in his maps, he fixed Africa instead.

Unfortunately, his drawings seemed to show that the poles had indeed moved, even though polar wander was not a requirement of his theory. The horizontal lines across his globes encourage this misunderstanding. They seem to show the Equator lying across ancient Africa. These horizontal lines were not needed, and were not shown in a few of his globes (such as Fig. 33). Climate indicators convinced Wegener that the continents had moved with respect to the poles, but the evidence could not rule out the possibility of some polar wandering as well.

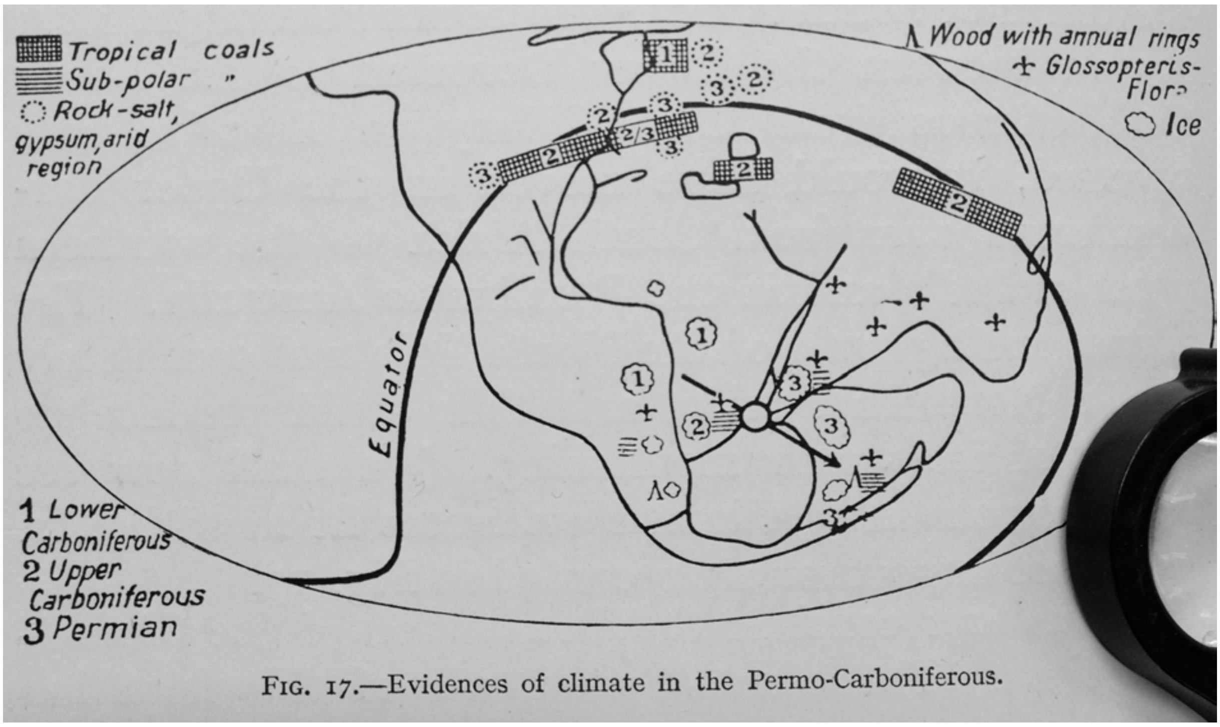


Figure 33. Map showing all types of climate evidence (not only glaciations) for the Permo-Carboniferous. The projection distorts the equator more than the continents. From Wegener (1924).

LIST OF CONTENTS OF WEGENER'S ENGLISH EDITION (1924)

Wegener had not simply made a hypothesis or a speculation, but had developed it into a complete and well supported theory. We get an impression of the detail and depth of his evidence by reading the extensive table of contents of his book:

THE ORIGIN OF CONTINENTS AND OCEANS – CONTENTS blocks. The disposal of the mass of oceanic water in the reconstruction of the submerged bridging-continents. Isostasy. Its demonstration by gravity measurements. Summary.

II. DEMONSTRATION

CHAPTER III – GEOPHYSICAL ARGUMENTS

The two frequency maxima of the altitudes of the earth's crust. Continental blocks and floors of the oceans as two different layers of the body of the earth. Principles of earth magnetism. Velocity of earthquake waves. Dredged samples. Sima and sial. Thickness of the continental blocks according to Hayford and Helmert. Specific gravity of sial and sima. Smoothness of the ocean floors. Absence of folded mountains on the floors of the oceans.

CHAPTER IV – GEOLOGICAL ARGUMENTS

Width of the Atlantic Rift. The Zwarte Berge in Cape Colony and the Sierras of Buenos Aires. The eruptive rocks of Brazil and South Africa. Sediments of Brazil and South Africa. African origin of the Permo-Carboniferous erratics of South Brazil. The direction of the folding of the gneiss massifs of South America and Africa. Regional movements of South America after the break. The Atlas Mountains without an American continuation. Character of the Atlantic Islands. The Carboniferous (Armorican) foldings in Europe and North America. Silurian-Devonian (Caledonian) folding. Algonkian foldings. Terminal moraines of the Pleistocene ice-caps. The convincing character of the independent controls. Basaltic zones of Greenland and Northern Europe. The Old Red in North America, Northern Europe, Greenland. Intrusive rocks in Greenland and North America. Lateral displacement of the masses of Grinnell Land and Greenland. Explanations with regard to the reconstruction of the pre-Atlantic connections of the continents. Abrolhos Bank. The Niger delta. The Newfoundland block, Iceland, The submarine bank in mid-Atlantic. Madagascar. Australia. New Zealand. The collision of the Australia-New Guinea block with the Sunda Archipelago. Tasmania and East Antarctica. West Antarctica and the Drake Straits.

CHAPTER V – PALÆONTOLOGICAL AND BIOLOGICAL ARGUMENTS

The views of twenty specialists on the land-bridges. Late age of the Atlantic. Evidence of earthworms. Reptiles and mammals of both sides of the North Atlantic. Carboniferous fauna-Examples of other affinities. Faunal and floral affinities of both sides of the South Atlantic. Juan Fernandez. Hawaian Island. Lemuria. The three components of the Australian fauna.

CHAPTER VI – PALÆOCLIMATIC ARGUMENTS

Organic evidences of climate. Inorganic evidences of climate. Spitsbergen as an example of an alteration from a tropical to a polar climate. Alteration of the climate of Central Africa from polar to tropical. The hypothesis of the wandering of the poles. Former attempts to verify the hypothesis of the wandering of the poles. Inexplicability of the Permo-Carboniferous glacial phenomena. Solved by the displacement theory. Ice, *Glossopteris* flora, coals, rock-salt as evidence of climate in the Permo-Carboniferous. Provisional position of the poles for the periods after the Carboniferous and up to the present day. The Quaternary glaciation of North America and Northern Europe.

CHAPTER VII – GEODETIC ARGUMENTS

The absolute duration of the geological periods. The amounts of the annual displacements to be expected. The increase of the distance between Greenland and Europe. The question of the variation of the difference of longitude between Europe and North America. The secular alteration of latitude of the European and North American observatories.

III. ELUCIDATION AND CONCLUSIONS

CHAPTER VIII – THE VISCOSITY OF THE EARTH

Immersion equilibrium (Isostasy) of the earth's crust. Shifting of the poles of rotation. Transgressions and Regressions (of the sea) caused by wandering of the poles. The coefficient of viscosity of the earth according to earthquake observations, the tides of the solid earth, and the oscillations of the poles. Possibility of a magmatic layer (asthenosphere). Influence of the great size of the earth. Paradoxical properties of viscous bodies. Soft solid, and hard fluid bodies. Temperature distribution in the earth's interior.

CHAPTER IX – THE FLOOR OF THE OCEAN

Extension of the floor of the sea by the drifting apart of the continental blocks. Depths and covering of the floor of the oceans. Possible explanation by the temperature relations. Currents in the sima. Deep-sea troughs.

CHAPTER X – THE SIALSPHERE

Outlines of the continental blocks. Section through the layers of the earth. Insignificant importance of the sedimentary strata. The original complete covering of the earth with sial. Gradual decrease of folding in the earth's history. The irreversible character of the evolutionary processes of the sial-layer. Inclusions of sima in the sial. The nature of vulcanicity.

CHAPTER XI – FOLDS AND RIFTS

Development of the explanation of folded ranges by compression. Evidence of the gravity measurements. Folding and erosion, subject to the preservation of isostasy. Unsymmetrical character of the folding process. Greater thickness of the sediments in folded regions. Folding on the anterior margin of the drifting blocks, and equatorial folding. General conditions for normal folds, echeloned folds, lateral displacements, and rifts. The East African rift valley. Foundering of the Ægean Sea area, Predominant meridional rifts.

CHAPTER XII – THE CONTINENTAL MARGIN

The disturbance of gravity on the margin of continents. Pressure-relations. Formation of fiords. Submarine continuations of river-valleys on the Atlantic coasts. Festoons of islands. Echeloned festoons. Their geological structure. The bulging form of the coasts behind the East Asiatic festoons. Richthofen's explanation of the festoons. Sliding coastal ranges. The Peninsula of California and the earthquake fault of San Francisco. The continental margin of Further India. Pacific and Atlantic types of coast.

CHAPTER XIII – THE DISPLACING FORCES

The drift from the poles of the continental blocks. Their westward drift. Meridional rifts. Nature of the force causing the drift from the poles, and its mathematical expression. Westward directed forces. Forces derived from the deviation of the figure of the earth from the ellipsoid of rotation. Similar forces in the geological past. Cause and effect.

PUBLISHED REACTIONS TO WEGENER'S BOOK

Reactions to Wegener's work were mixed. In general, they can be grouped geographically. Geologists of the southern hemisphere were somewhat positive to Wegener's hypothesis, because the geographic and geologic fits of Gondwanaland were so clear. Many European geologists were also positive, because experience in the Alps had already convinced them that there was great horizontal movement between continents. It had become apparent that the Alps formed as the result of hundreds of kilometers of horizontal compression between Africa and Europe.

North American and British geologists, working on either side of the northern Atlantic were far more skeptical or outright negative to Wegener's displacement. The fit of continents there was not obvious, and Wegener had made a glaring mistake, saying that the north Atlantic had not opened until after the beginning of the Quaternary ice age. Such a late opening would require continents to be moving remarkably fast.

A number of articles in the journal *Nature* appeared in 1923. Here we can read the beginnings of a few of these articles. They give a sample of the reactions to Wegener's book (the German edition, as the English edition had not yet appeared.)

An article by W. B. Wright included reproduction of Figure 1 from Wegener's book, showing the three dramatic globe maps.

W. B. Wright 1923, p. 30.

The Wegener Hypothesis

DISCUSSION OF THE BRITISH ASSOCIATION, HULL

On Monday, September 11, the meeting room of the Geological Section of the British Association was the theatre of a lively but inconclusive discussion on the Wegener hypothesis of the origin of the continents. This hypothesis, which is a development of the well-established theory of isostasy, regards the continental masses as cakes of light siliceous material floating on a heavier basaltic, fluid or viscid, substratum, which in its turn reaches the surface in a solidified form on the floors of the oceans. The continents, which are thus movable, are supposed in Carboniferous times to have formed a single mass, and to have split up by rift-valley formation and started floating apart in late Cretaceous or early Tertiary times. The mountain ranges fringing the Pacific are supposed to have been produced along those margins of the continents which are or have been, in virtue of their motion, impinging on the hard oceanic crust, the belts of thick sedimentation along the continental shelves localising the folding.

The union of the continental masses in former geological times explains many peculiarities in the distribution of life both past and present. It also affords an easy explanation of the hitherto unsolved problem of the Permo-Carboniferous glaciation, by supposing the pole to have been located in South Africa and the other glaciated parts of Gondwanaland to have been grouped around. When a reconstruction of this sort is made it is found that the main Carboniferous coalfield of the world lay, at the time of their formation, within the tropics.

The discussion brought forth a great diversity of opinion regarding the validity of the hypothesis, almost the only point on which there seemed to be any general agreement being an unwillingness to admit that the birth of the North Atlantic could have occurred at so late a date as the Quaternary.

Another article in *Nature* represented typical attitudes of geologists from the southern hemisphere:

[Anonymous] 1923, p. 131.

The Distribution of Life in the Southern Hemisphere, and its Bearing on Wegener's Hypothesis

One of the most important results of the acceptance of Wegener's theory of the palæogeography of the world would be the simplification of the facts of the Permo-Carboniferous glaciation of Australia, India, South Africa, and South America by bringing the glaciated areas together into one single glaciated region. It is undoubted that if this were done much of the difficulty of accounting for the simultaneous glaciation of regions so diverse in latitude would disappear. Considerable interest, therefore, attaches to the recent discussion on Wegener's hypothesis, which was held before the Royal Society of South Africa, for its bearing on this important aspect of the subject.

The general attitude of the geologists who took part in the discussion was one of suspended judgment. It is admitted that the folded ranges of the Sierras of Buenos Aires appear to be of similar age and structure to those of the southern folded belt of the Cape Province, and would be brought into fairly accurate alignment if the South American coast were fitted into the African coast after the manner of Wegener's map of Carboniferous land distribution, but it was held that this might be accounted for in several other ways more in accord with the known facts of geology.

Philip Lake had reviewed the 1920-edition of Wegener's book. Then, for the 1922-edition, he wrote another scorching review article. Here Lake appears to have coined the term *continental drift*, using it in his title. This was before the English translation of Wegener's book was available. His article in *Nature* began as follows:

Lake 1923, p. 226.

Wegener's Hypothesis of Continental Drift

Wegener's hypothesis is based on the idea that the continental masses are patches of lighter rock floating and moving in a layer of denser rocks, and this denser rock forms the floor of the oceans.

Lake's term continental drift appealed to the skeptics. Like a drifting boat, the continents seemed to have no motor. The forces were not explained or understood. The term continental drift caught on and was eventually used by nearly everyone. But Wegener did not use it. He consistently used the neutral term *Verschiebungstheorie* (displacement theory) and never *Trifttheorie* (drift theory).

Lake's first sentence subtly misrepresents Wegener's hypothesis. It was not really based on the idea of isostasy or how continental masses floated or moved. It was based on the fit of continental margins, and a solution to problems such as sunken continental bridges, inexplainable climate indicators, and matching fossils and rocks. As in his previous review article, Lake could barely find a positive point worth mentioning.

The last two paragraphs in Lake's review concerned the Cape Mountains fold-belt, that we have already noted from Wegener's 1915 fit-map and book:

Lake 1923, p. 228.

In South Africa a folded mountain range runs from east to west. In Buenos Ayres a folded range belonging to the same period has been described. According to Wegener one was the direct continuation of the other. But before they reach the western coast the South African folds, and the range that they have formed, turn to the north and run roughly parallel to the western coast. Wegener's explanation of this deviation is far from convincing.

It will thus be clear that the geological features of the two sides of the Atlantic do not unite in the way that Wegener imagines, and if the continental masses ever were continuous they were not fitted as Wegener has fitted them.

In the view of Philip Lake, none of Wegener's evidence was valid. John W. Evans wrote an article in *Nature* in order to correct the negative impression given by Lake. He used a title similar to Lake's, a common practice in follow-up discussions. Now the phrase continental drift, instead of continental displacement, was well on its way to becoming established. Evans began his article as follows:

Evans 1923, p. 393.

The Wegener Hypothesis of Continental Drift

The chief value of the discussion on the Wegener hypothesis is that it has given rise to a reconsideration of the problems presented by the configuration and relations of the major features of the earth's surface.

The elaborate structure of theory built up by Dr. Wegener, and so effectively criticised by Mr. Lake (see NATURE, February 17, p. 226), will have few, if any, thorough-going defenders in this country, but some of its leading features cannot be lightly dismissed.

The contrast between the attitudes expressed by Lake and Evans is striking. Although Evans was not in full agreement with Wegener, he was convinced that the evidence for displacement was worthy of consideration.

MECHANISMS INCAPABLE OF MOVING CONTINENTS (OR SCIENTISTS)

As Wegener mentioned in the first chapter of his book, and explained more fully in the last chapter, he thought he knew what made continents move. There were two different forces that might explain displacement, and both were related to the rotation of the Earth.

As the Earth rotates, centrifugal force throws material outward and toward the Equator. This produces an equatorial bulge. According to measurements, known already at that time, the Earth is not a sphere, but an oblate spheroid. Its radius is 21 kilometers longer at the Equator than at the poles. Wegener reasoned that continental material, standing a bit higher than the ocean floors, would be thrown from the poles toward the Equator. This force was known in Europe as the Eötvös effect. Wegener usually referred to it as *Polflucht*, the pole-fleeing force.

His other force was *Westwanderung*, or westward drag, a tidal effect. The gravitational pull from the Moon produces a tidal bulge, not only on ocean water but also on the solid Earth. Earth's eastward rotation continually carries this tidal bulge forward, toward the east. The Moon continually pulls it slightly back, toward the west. Modern calculations show that this force on the tidal bulge is slowing the rotation of the Earth and lengthening the day by about a second every 50,000 years. Wegener suggested that the Atlantic Ocean formed from the westward pull of the American land masses.

Sir Harold Jeffreys (1891-1989), a mathematical geophysicist, convinced most geologists that Wegener's forces were hopelessly weak. They were incapable of moving continents (Jeffreys 1924). But Wegener was not willing to abandon these interpretations.

Few geophysicists bothered to look for more appropriate forces to move continents. After all, Philip Lake and others were telling geophysicists that there was no purpose in looking; Wegener's geological evidence was not valid. But these geologists were rejecting Wegener's evidence because they knew of no geophysical forces to move continents.

The continents are moved by convection currents in the mantle. Wegener had insinuated this in his 1912-paper, but did not mention such currents at all in the first three editions of his book.

WEGENER'S USE FOR LAND-BRIDGE ARGUMENTS IN HIS THEORY

Wegener wanted to determine just when the various continents had separated. Now we know the timing from study of sea-floor paleomagnetic anomalies. Without magnetic data, Wegener came upon another method; the opinions of prominent paleogeographers as to when their postulated land bridges must have sunk. Wegener explained this method first in his 1920-edition, and then again in his 1922-edition, which was translated to English as follows:

Wegener 1924, p. 73-76.

The question whether a connection has prevailed between two continents will frequently be answered in different ways by specialists in different directions, because each of them tends to generalize the results of his own particular field of research. It was therefore a happy thought of Arldt, when trying to obtain a synopsis on broader lines, to take a vote on each land-bridge and each period from the various specialists. It goes without saying that this procedure gives rise to many uncertainties. But any other way seems scarcely possible, because of the vast amount of the material facts. The results also appear to justify his method. For the purpose he made use of the papers or maps of Arldt, Burckhardt, Diener, Frech, Fritz, Handlirsch, Haug, Ihering, Karpinsky, Koken, Koszmat, Katzer, Lapparent, Matthew, Neumayr, Ortmann, Osborn, Schuchert, Uhlig and Willis. The table of p. 74 shows an extract from Arldt's statistics, and the first four land-bridges are illustrated by curves in Fig. 15 [Fig. 34]. In this, three curves are drawn for each land-bridge, namely, for the number of votes in favour, of those in opposition, and of the difference, the latter thus giving the strength of the majority; this is emphasized by shading on the area concerned. These four early land-bridges are those extending over present-day oceanic areas, and are of special interest to us. The result, as is seen, is very clear in its broadest outlines in spite of many differences of opinion. The connection between Australia and India (united with Madagascar and South Africa) disappeared soon after the beginning of the Jurassic period; the connection between South America and Africa disappeared in the Lower to Middle Cretaceous; and the connection between India and Madagascar disappeared at the transition from Cretaceous to Tertiary. At all three places a land connection had prevailed from the Cambrian period until these points of time. The connection between North America and Europe was vastly more irregular. But nevertheless here also, in spite of the frequent differences, there exists a far-reaching agreement of views. The connection was repeatedly disturbed in the more ancient periods, namely, in the Cambrian and in the Permian, as well as the Jurassic and Cretaceous, but obviously only by transgressions, which later permitted the restoration of the continuity. The final breaking off of relations, which corresponds to the present-day separation by a broad ocean, can only first have happened in the Quaternary.

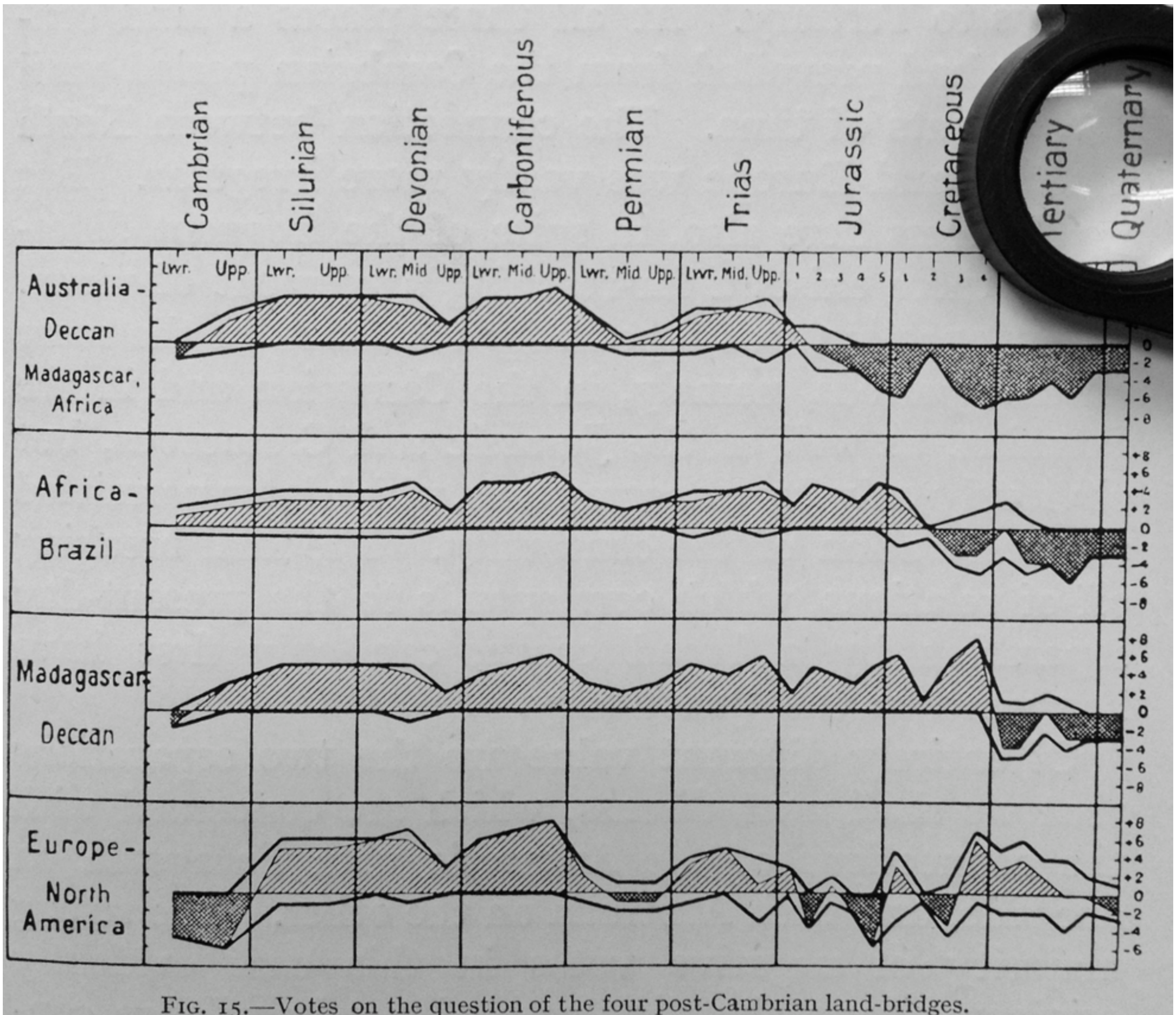


FIG. 15.—Votes on the question of the four post-Cambrian land-bridges. The number of favourable votes is shown by the upper thick curve, the number of unfavourable votes by the lower thick curve. The difference is simply shaded if favourable; cross-hatched if unfavourable.

Figure 34. Wegener's compilation of Arldt's survey of land-bridge interpretations. From Wegener (1924.)

This method gave reasonably good results, except for the bridge between Europe and North America. There was no consensus among paleontologists as to when that land bridge was destroyed. Many authors claimed that a land bridge there lasted all the way to Quaternary time, as shown by the lower row of curves in Wegener's diagram. Wegener decided that the North Atlantic must have opened in the Quaternary, after the glacial deposits had been formed.

This led him to make several incorrect interpretations. If the North Atlantic had opened so recently, the rate of displacement there should be hundreds of meters per year, something that might be proven using modern survey measurements. He lost some credibility looking for this type of proof. And since the Atlantic opened after the Pleistocene glacial deposits had formed, those should have been continuous and ought to be matched on both sides of the Atlantic. This idea was also criticized. Thus, his serious mistakes in displacement rate and geologic correlations were near Greenland, his favorite part of the world.

Among the 20 paleogeographers mentioned above by Wegener, we find two familiar names: Schuchert and Willis. Especially Charles Schuchert's opinions on these matters carried great weight. He had been chosen as President of the Geological Society of America in 1922. He turned 65 in 1923 and retired from regular teaching at Yale University to become professor emeritus. He published a second edition of his *Historical Geology* in 1924. There he cited papers that had appeared as recently as 1924, but did not mention Wegener or the hypothesis of continental displacement. As we shall see, without the help of Wegener's theory, Schuchert was still having problems explaining Permian climates.

5. Coping with Ancient Climates

Climates of the geologic past were a puzzle that could not be solved on a map of fixed continents. The positions of deserts, rain forests, and glacial ice caps on the modern globe made no sense. The most obvious problems were the locations of tropical coals and glacial deposits in Permo-Carboniferous time. But there were similar climate problems at most other geological times. Paleontologists were well aware of these problems. The paleobotanist Frank Hall Knowlton (1860-1926) summed up the situation this way:

Knowlton 1919.

It is perhaps not too much to say that it has now been demonstrated beyond reasonable question that climatic zoning such as we have had since the beginning of the Pleistocene did not obtain in the geologic ages prior to the Pleistocene. I think this statement of conditions is very generally accepted by geologists and paleontologists – in fact, I am at a loss to know how the data available can be otherwise interpreted.

NEW INSIGHT ON THE PERMO-CARBONIFEROUS ICE OF SOUTH AFRICA (1924)

In the first edition of his textbook, Schuchert had given much attention to the problems of Permian climate. Since then he had been following the paleoclimate problems closely. In the second edition of his textbook, Schuchert added new information and a detailed map from a research paper by Alex Du Toit (1878-1948), South Africa's leading geologist:

Schuchert 1924, p. 428.

South Africa has the best known Permian tillites, and here Du Toit in 1921 has brought together the evidence. All of Africa and Madagascar south of 22° and 23° respectively was covered by ice sheets that at their maximum were between 4000 and 5000 feet thick. Of snow-accumulating centers there were two major and two minor ones, which, coalescing, moved toward the southwest and south out into the oceans. The high land was in the north and especially northeast, rising here to about 4000 feet above sea-level. The Transvaal ice sheet was the most extensive, moving at least 700 miles to the southwest. The tillites of the Dwyka series are in the northeast less than 100 feet thick, but in the south attain to 1500 feet, and in southern Karoo to 2000 feet (see Fig. 144, p. 428) [Fig. 35].

Eight or nine horizons of glacial rock débris derived from floating icebergs occur in *South Australia* above the Coal Measures, some of them 200 feet thick, interbedded in 2000 feet of marine strata...

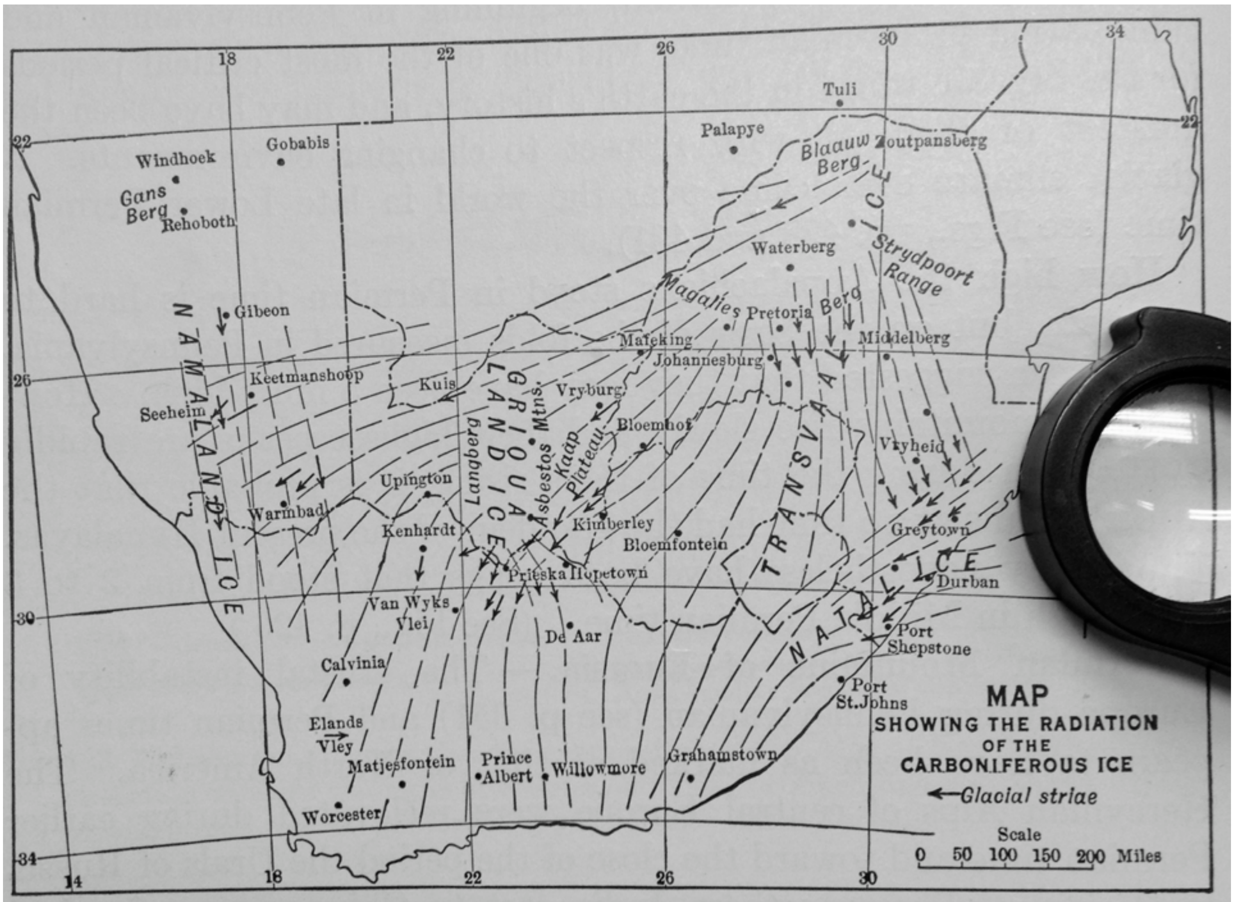


Fig. 144. — Map of the Permian glaciation of South Africa, as generalized by Du Toit. Also see map on page 431.

Figure 35. Du Toit's map of Carboniferous glacial movement directions. From Schuchert (1924).

Notice that Schuchert's description and figure caption describe the South African glaciation as Permian, but the inset title of Du Toit's map labels it as Carboniferous. Students probably did not notice this discrepancy. It was not a misprint. Du Toit was confident that the glaciation was Carboniferous, but Schuchert had called it Permian in his first book and would continue to do so. To simplify the situation for his students, Schuchert did not call attention to this debate, but presented the glacial climates in the Permian chapter of his textbook, and the tropical coal climates in the Carboniferous chapter. The glaciation is now generally referred to as the Late Paleozoic Ice Age, and thought to have lasted from early Carboniferous to mid-Permian time (Crowell 1978.)

The characteristics of ice sheets were well understood by geologists, and also by Wegener, who was an expert on the continental glaciation of Greenland. Ice flows outward in all directions, from its center of greatest thickness. The glacial striations shown on Du Toit's map are quite remarkable. They indicate that the ice there was flowing toward the south, away from the Equator. The striations also show that ice came from the northeastern part of the map where there is no land today. According to Schuchert's Gondwanaland theory, the land that had existed there had sunk to ocean depths long after the glaciers had melted.

Schuchert was obviously interested in the ice flow directions on Gondwanaland. In his first textbook he had shown them as arrows pointing north in Australia and India, and south in Africa (see Fig. 22). In 1915, neither he nor anyone else could explain those ice flow directions. What centers of ice were they flowing from?

Now for his second textook, Schuchert redrew his map and removed those arrows (Fig. 36). One might suppose that he removed the arrows because they could still not be explained. But in fact, he knew that they now had been explained, by the theory of continental drift.

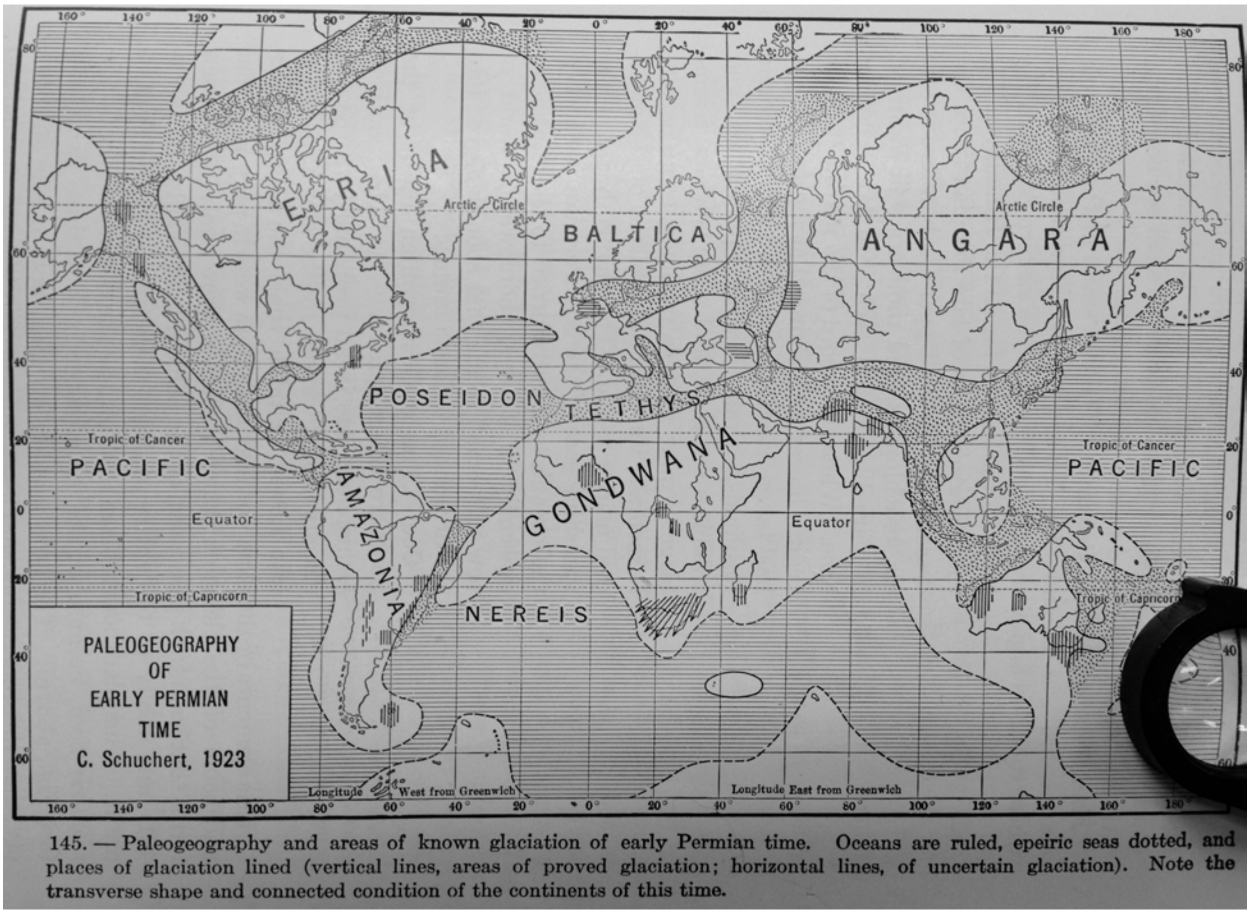


Figure 36. Map signed and dated 1923 in the inset. Arrows from Schuchert's 1915 map showing ice flow directions in Australia and India have been removed. From Schuchert (1924).

Schuchert knew that Du Toit (1921) had made a breakthrough in the understanding of the Permo-Carboniferous glaciations. Inspired by the ideas of Wegener, Du Toit had drawn a map showing the "Hypothetical arrangement of Land and Water" during the Upper Carboniferous (Fig. 37). When he plotted the distribution of continental ice sheets on this map, the isolated glacial deposits fit together in a single ice cap of reasonable size. The directions of ice movement made a meaningful pattern, with ice flowing more or less away from the center, in India, Australia and also South America.

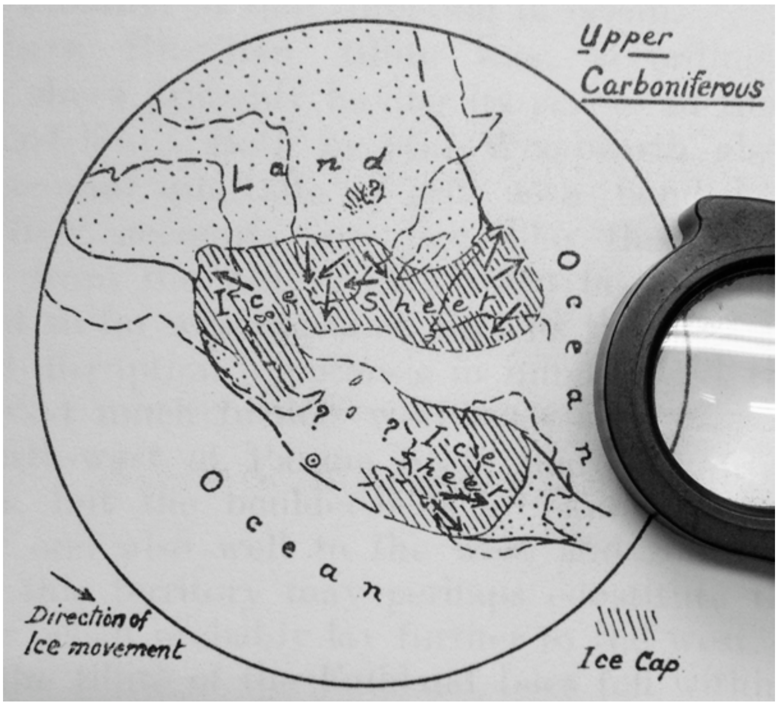


Figure 37. Du Toit's map showing a Carboniferous ice cap and directions of ice movement. He noted in the text that the anomalous southward-pointing arrow in India was based on meagre information. From Du Toit (1921).

Du Toit's discussion of the glaciation was also interesting. He listed the latitudes of the various tillites in Western Australia, New South Wales, Tasmania, the Falkland Islands, Argentina, Brazil, and India. He noted that these locations are not on the Equator, but close to it; within 18° north latitude and 20° south. In his rather awkward writing style, he continued as follows:

Du Toit 1921, p. 219 and 219-229.

It is consistently manifest that the glaciation during this epoch can with justice be described *as nowhere equatorial, but almost wholly extra tropical.*

This statement by no means disposes of the extreme difficulties introduced by latitude, for they remain well nigh insuperable under any of the older views, which one and all presuppose that the spacing upon the globe of the several units remained practically the same from the Carboniferous down to the present day.

On the contrary, the solution follows in the simplest manner with the perception of the idea that the several sections represent portions of Gondwanaland actually disrupted and forcibly torn apart from one another subsequently to the Triassic. This revolutionary idea of continental and oceanic evolution, ably championed by Wegener, was recently applied to this problem by the writer, and not only has a most remarkable mass of evidence in its favour hard to account for otherwise, but has proved extraordinarily stimulating in its application and indeed can be looked upon as a master-key to the Past.

Under the hypothesis adopted, all the areas known to have been glaciated, even Peninsular India, can be brought together inside the forty-fifth parallel south, which we may note is close to the existing northerly limit of drift ice, so that the Carboniferous glaciation of this continent on these assumptions would present no greater difficulties than that of the Northern hemisphere in the Pleistocene. So many points of similarity with the latter obtrude themselves, it might be remarked, as to convince me that the conditions during the much earlier epoch must have been very closely comparable indeed.

Schuchert cited Du Toit's paper and used parts of it. It seems that he started to put more detail on his world map by drawing Du Toit's new arrows in South Africa instead of his previous ones (see Fig. 36). But he did not add the new arrows to South America. And he removed the northward-pointing arrows in India and Australia. He had no reason to doubt those data. He must have removed the arrows so that students using this textbook would not be distracted by those ice-flow directions and the new ideas of continental drift.

ARTHUR P. COLEMAN ON ANCIENT ICE AGES (1926)

Arthur P. Coleman (1852-1939) was six years older than Schuchert, and was one of North America's leading glacial geologists. He too had been elected for a term as president of the Geological Society of America. His specialty was the Pleistocene, but he was also fascinated by the Earth's earlier glaciations. As emeritus professor at the University of Toronto, he published a book *Ice Ages Recent and Ancient* where he described all known glaciations. He had personally visited the important Permo-Carboniferous localities in India, South Africa, Australia, and South America. He had been the one to discover the glacial ice movement to the northwest in Brazil, as shown on Du Toit's map.

Coleman's written descriptions of the glacial deposits are excellent. Here, I include part of his description of Indian glaciation, because it documents that these tillites were spread over immense areas by continental ice sheets lying near sea level. No mountains existed in India at that time. Ice had moved northward, carrying boulders 750 miles across the lowlying continent:

Coleman 1926, p. 109-111.

The Talchir glacial deposits reach lat. 17° 20' toward the south and extend northward, according to the geological map, to lat. 24°, a distance of nearly 500 miles from south to north. Between this and the Salt Range tillites there are several hundred miles of later formations, including the Deccan traps. If we assume that tillite deposits are buried under these later rocks the length of the glaciated area from south to north is about 1,100 miles. The map shows lower Gondwana rocks as extending 600 miles from east to west.

It is understood, of course, that the boulder conglomerate is not continuous for these dimensions; but it is so widely distributed that it is reasonable to suppose the whole region to have been ice covered.

As the Salt Range tillites occur along the foothills of the Himalayas, which are the loftiest mountains in the world and bear immense glaciers, one might at first imagine a vast piedmont glacier at the end of the Carboniferous, spreading hundreds of miles south of the great range.

This idea is quickly negatived, however, since we know that the Himalayas are one of the youngest mountain ranges in the world and were elevated many millions of years after the tillite was laid down.

There are marine beds with the tillite both in the Salt Range and at Umaria in Central India, so that the ice probably reached sea level and was of the continental type, and its direction of motion, as shown by striæ and the transport of boulders, was from the south or southwest, some of the boulders having been carried 750 miles to the north. It did not move from the pole toward the equator.

In most respects, the Indian tills are very like those of the Pleistocene, as several of the Indian geologists have suggested. The Archæan boulders so often enclosed in them are just like the granites and gneisses of the Pleistocene boulder clay in Europe and North America; and except for its consolidation to rock, the matix is the equivalent of the modern clay. Striated stones are just as frequent and as characteristic as in recent glacial deposits; and in a few places, such as the Ramghur coal field mentioned earlier, there are interglacial beds of shale and sandstone not unlike the stratified clays and sands of the Toronto Formation in America. The thickness of the Palæozoic drift varies greatly but may reach 900 or 1,000 feet, considerably more than any Pleistocene section reported in North America.

The glacial deposits rest on an ancient peneplain of Archæan rocks with gentle mounds and hollows, just as the Pleistocene tills rest on the old Laurentian peneplains of Canada and Scandinavia.

In every respect except one this late Palæozoic glaciation runs parallel to that of the Pleistocene. The known evidence of ice action in India points toward movement only in one direction, northwards, while the Pleistocene sheets spread out in all directions from a center.

Now an ice sheet on low ground, as it seems to have been in India, must necessarily extend in all directions, since it is not the slope of the surface it rests on that sets it in motion, but the thickness of ice toward the central parts, as was shown in the account of the Pleistocene glaciation.

The Indian ice sheet should push southward as well as northward. Did it really stretch as far to the south of lat 17° as to the north? It extended 1,100 miles to the Salt Range in the north. If it extended the same distance to the south it would reach the equator!

In the absence of positive evidence such suggestions are, of course, speculative; but the analogy of the great Pleistocene sheets makes it highly probable that the Talchir ice reached much farther to the east and west and south than is known at present.

A similar relationship will be described later in connection with the South African glaciation.

Note that Coleman was quite certain that the present deposits reflect only parts of the original glaciation. One only sees the deposits that have not been covered by younger sediments or removed by later erosion. The deposits are from the segment of the Indian ice sheet that had moved northward. But the ice sheet must have been much larger, and other parts must have moved in other directions. In his detailed descriptions from the Permo-Carboniferous glacial deposits of other continents, Coleman drew similar conclusions; just as the glacial ice in India extended south toward the Equator, the ice in Africa extended north toward the Equator.

Coleman presented a map of Permian or Carboniferous Glaciation (Fig. 38). On this map he plotted the data in such a way that glaciation is mostly limited by the same line of latitude. But his written text explained that the Permo-Carboniferous glacial ice actually extended far beyond that limit, toward the Equator.

Coleman also described tillites of Triassic age in Africa. They occur within a few degrees of the Equator. These are a bit younger than the Permo-Carboniferous tillites, so he mentioned them only in a short chapter of the book concerning Mesozoic glaciation. He saw the obvious connection: “One cannot help wondering if these glacial deposits do not really belong to a somewhat earlier time, so as to harmonise with the powerful glaciation on four continents at the end of the Palæozoic.” He did not show the Triassic deposits on any map. If he had made a map of “Triassic or Permian or Carboniferlus” glaciation the African tillites might have extended as a wide belt all the way to the Equator.

By dividing the chapters of his book along a rigid time scale, Paleozoic first and then Meozoic later, Coleman avoided showing the bigger picture. Schuchert did the same thing in his textbooks, discussing the glaciation always as Permian, which seemed to make the glacial climates of a distinctly different age than the warm climates of the Carboniferous coals. Schuchert did not put the equatorial African deposits on his Permian map since they were Triassic in age. And he found no other occasion to mention those glacial deposits near the present Equator.

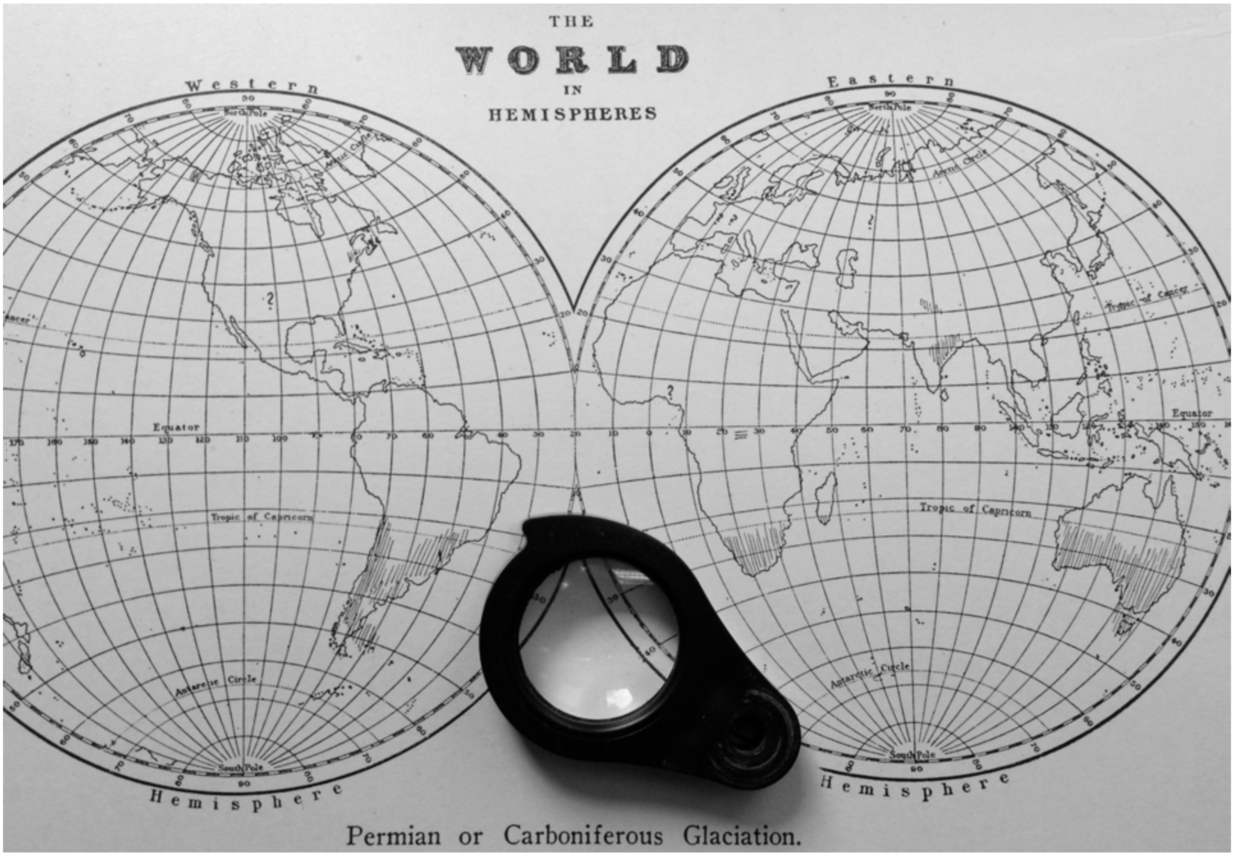


Figure 38. Colemans’s map of Permian or Carboniferous (but not Triassic) Glaciation. The glacial indications in South America, Africa and Australia are incorrectly shown to terminate at about the same latitude. His written locality descriptions are more correct. From Coleman (1926).

Coleman discussed Wegener’s hypothesis, but I wonder if he had actually read or understood the details. He only cited the German title, not any English translation. He mentioned Wegener’s hypothesis under the heading: *The supposed drift of continents*.

He began on a positive note, by writing: “One of the most eagerly discussed geological theories having a bearing on glaciation is that of the movement or drift of land masses from one place to another on the surface of the earth.” (Coleman 1926, p. 257). But despite those words, Coleman was not eager to discuss Wegener’s theory. He did not mention any of Wegener’s data or that his theory could solve any other climate-related problems. Coleman did not discuss the tropical coals of the northern hemisphere. He limited his discussion to glacial deposits.

Coleman stated that he rejected the hypotheses of Wegener and of Du Toit because the continent they proposed was too large to receive moist winds from the ocean. It would be too dry to produce the necessary snow and glacial ice, just as Alaska and Siberia had been too dry to be glaciated in the Pleistocene.

He showed a map of world glaciation in Pleistocene time, where ice-free Alaska and Siberia are clearly visible (Fig. 39). Canada and Scandinavia had been heavily glaciated in the Pleistocene. If they had been at the same latitude in the Permo-Carboniferous, why had they not been glaciated then? And why was southern Africa heavily glaciated in the Permo-Carboniferous but not at all in the Pleistocene? Coleman had no answers to these questions, but did not dwell on them, or show how Wegener’s hypothesis could answer them.

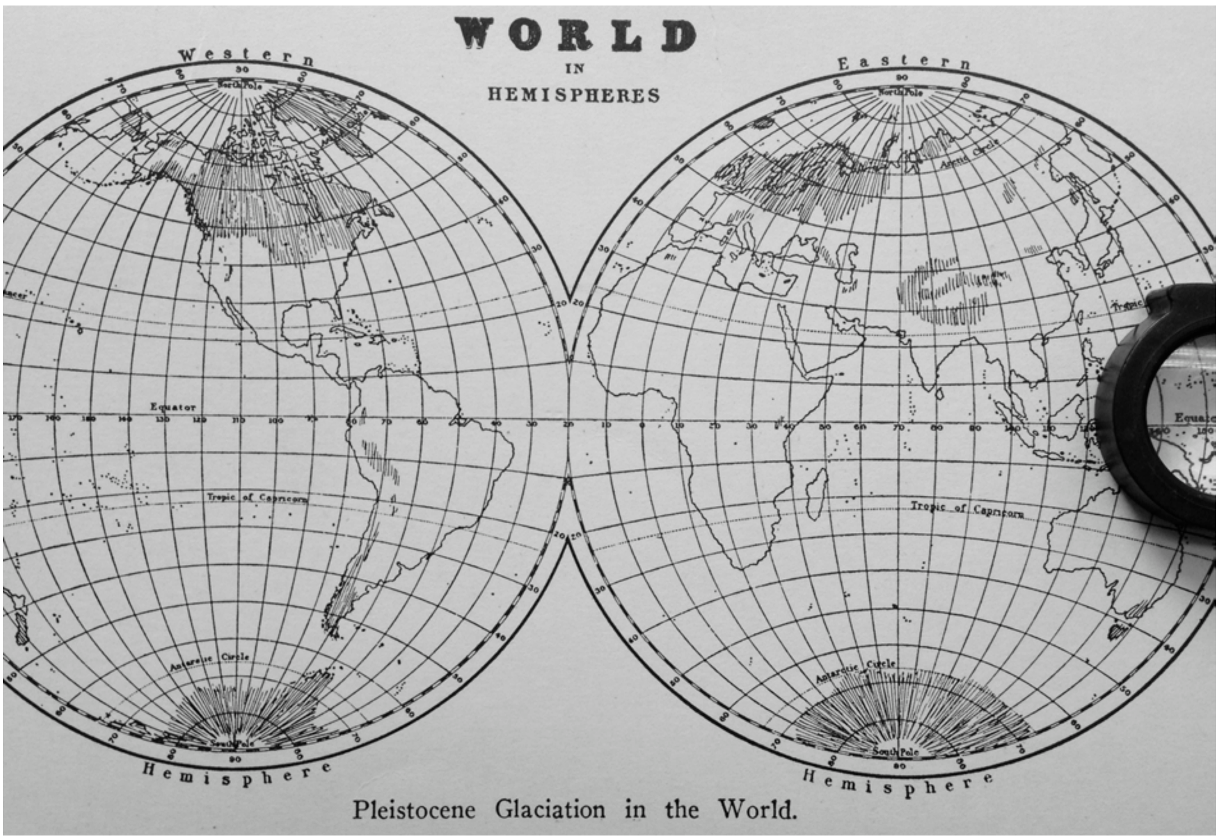


Figure 39. Colemans’s map of Pleistocene glaciation. Note the glaciation of the northern hemisphere. These northern areas experienced warm climate without glaciers during the Permo-Carboniferous glaciation. From Coleman (1926).

Coleman did not consider any of the possible advantages of the theories of Wegener and Du Toit, but rejected them because their postulated continents were too large and would therefore be too dry. He stated this objection to continental displacement a second time, quite unnecessarily. And that second time, he divulged his “main” reason, "after all," for rejecting Wegener and Du Toit:

Coleman 1926, p. 260.

The supply of moisture for the vast Permo-carboniferous ice sheet, as shown on Du Toit’s map, would have to come from more than double the distance mentioned above. This seems to me a real difficulty if one admits that the glaciated regions were grouped together in the way suggested; but, after all, the main doubt one feels in the matter is in regard to the physical possibility of continental masses wandering apart for thousands of miles in opposite directions. In the case of India there would have to be a voyage of 3,500 miles to the north, while South America would travel 2,000 miles west, Australia 1,000 or more miles east, and Antarctica 1,500 miles south.

Coleman could find no way of explaining the Permo-Carboniferous glaciation, either the cause, the ice-flow directions, the low-latitude locations, or the evidence for multiple glaciations and warm periods. The cause of multiple glaciations and interglaciations of the Pleistocene were also an unsolved mystery. His final conclusions on the last page of the book therefore include this sad statement:

Coleman 1926, p. 282.

It may be expected that the present writer, after pointing out defects in all the previous attempts to solve the tangled problems of glacial periods, should propose something which he considers more satisfactory. This I do not feel competent to undertake. During many years of study of glaciation I have hoped to find a solution of the difficulties in several theories at different times, but have always encountered some point where they failed.

The solutions Coleman was lacking could all be found in a German book by Köppen and Wegener published in 1924, which we will consider below. Even the (Milankovitch-) explanation for glacial and interglacial events in the Pleistocene is explained there. But Coleman seems to have missed that book. He wrote another paper on this same topic in 1932, and still made no mention of the book by Köppen and Wegener.

Coleman was North American’s leading expert on ancient glaciations. His argument that Wegener’s supercontinent would be too large and too dry to produce the Permo-Carboniferous ice sheet was often given as reason to reject this part of the displacement theory.

KöPPEN and WEGENER’S *DIE KLIMATE DER GEOLOGISCHEN VORZEIT* (1924)

Wegener’s strongest evidence for the existence of the supercontinent was how well it could explain all ancient climates, not just the glacial ones. In 1924 he published a book, *Die Klimate der Geologischen Vorzeit* (The Climates of the Geologic Past) together with his father-in-law, the esteemed climatologist Wladimir Köppen (1846-1941).

This book included two very different accomplishments. 1) It showed that fossils and rocks did indeed define ancient climate belts, when plotted on the supercontinent that Wegener had proposed. The belts resemble the Earth's modern climate belts. The current system of defining climate belts, by the way, is known as the Köppen Climate Classification. 2) It summarized the important work of Milutin Milankovitch, which explained the cyclicity of glacial and interglacial events in the Pleistocene ice ages. Milankovitch had written a monograph in 1920, explaining how variations in the spin and orbit of the Earth could explain the complexity of the ice ages. The book by Köppen and Wegener gave this work helpful exposure, but it would be another 50 years before the cycles would be generally accepted and become known as Milancovitch Cycles.

Die Klimate der Geologischen Vorzeit was one of Wegener's most important works on his displacement theory, yet it has still not been translated to English. It had ten maps showing the positions of continents, climate indicators, and climate belts at different geological times. The maps were translated to English and published by Van der Gracht in 1928 (see p. 138). Köppen and Wegener's first map, of modern climate belt and desert locations, is shown here (Fig. 40).

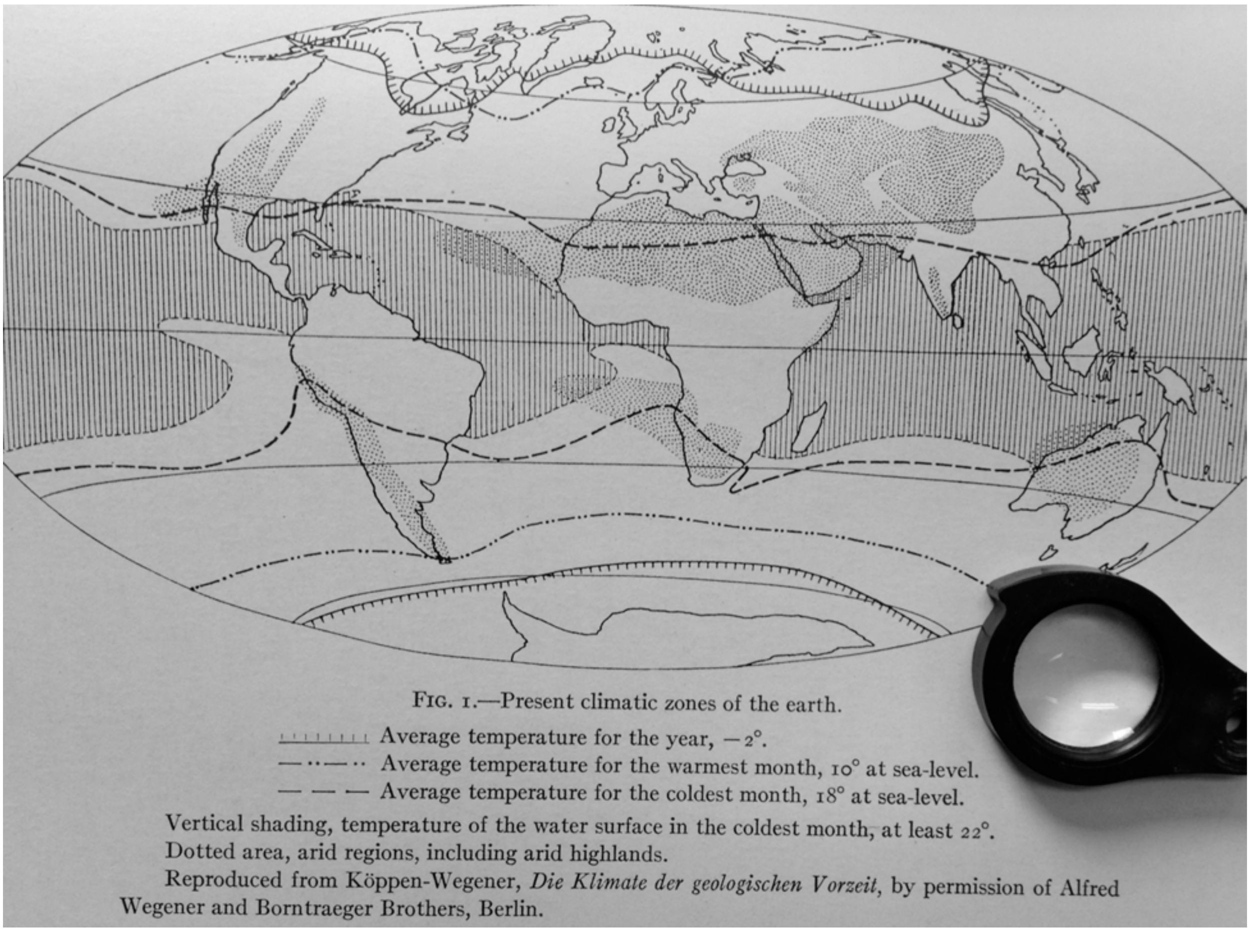


Figure 40. Köppen and Wegener's map of present climate zones, as translated and published by Van der Gracht (1928).

The tropics are characterized by warm seas, abundant rainfall, and jungles. Note that desert sands and arid regions (dotted areas) are not found along the Equator, but in belts to the north and south of the tropics. This modern climate zoning must be understood before one can interpret the ancient climate zones. The simplest explanation is that air near the Equator gets heated, causing it to expand and rise. As it rises it cools, causing its moisture to condense and fall as rain. This rainfall results in tropical jungles. The rising air leaves behind a low-pressure area down below, and cooler air sweeps in to the equatorial low-pressure area from the north and south. This cooler air warms as it moves toward the Equator, absorbing moisture from the land, and thus producing the desert and arid areas to the north and south of the equatorial jungles.

When Köppen and Wegener plotted the ancient climate evidence on the maps of Wegener's repositioned continents, they found distinctive belts. Tropical coals helped identify the position of the Equator, whereas salt and gypsum deposits indicated the arid belts to the north and south. Glacial deposits showed the position of the south-polar region. The positions of the Equator and South Pole that were determined by this method were about 90° apart, as would be expected.

The presentations of Köppen and Wegener (1924) are much too extensive to quote here. But Wegener neatly summarized the book as part of an article for an Earth Science encyclopedia that was published in 1926. That summary is brief enough to include here:

Wegener 1926, p. 181-183. (translated here from German)

We currently have no physical law that allows us to theoretically determine the position of the poles millions of years ago. We have only one way to approach this. We must derive the position empirically using all the climate evidence that is available in the form of geological and paleontological discoveries. This must be done separately for each geological age, because the pole position has changed constantly but at varying speed. Together with W. Köppen, I have attempted this in our book: "The Climates of the Geologic Past" (Berlin 1924), where references are given for all the details.

Most important for the determination of the climate zones are glacial striations, coal and salt, because their connections to climates are physically strict. Glacial ice, or more precisely a continental ice sheet, as opposed to a mountain glacier, absolutely indicates polar climate. Coal from fossil wetlands can only be formed in areas of precipitation. These are mainly in the equatorial rain zone, like the current tropical wetlands of Sumatra, Ceylon and Africa. Coal can also form in sub-polar precipitation zones, such as the Quaternary and post-Quaternary peat bogs of North Germany and Tierra del Fuego. Outside of these zones, coal can form on the eastern margins of continents in the areas of monsoon rains, like today in Florida and Eastern Asia. On the other hand, the formation of salt and gypsum is restricted to arid areas, in which evaporation dominates over precipitation. Such arid areas are arranged into two zones, lying between the three zones of precipitation.

Less certain in each individual case, but in the whole quite compelling, are the testimonies from the organic world. Tropical flora can be distinguished from snow-forest areas, where trees have annual rings. These are further distinguished from the treeless polar flora. Of the marine animals, those producing great amounts of lime, in particular the builders of carbonate reefs, indicate warm water. The current reef corals, for example, are found only in water where the temperature never sinks below 20 degrees. - There are numerous other climatic phenomena that also could be used, but here we will be content with these indications.

As an example of determining such a pole position, we consider the Permian and Carboniferous periods. The most important climate data for these times are indicated in Figs. 5 and 6 [Fig. 41]. These are as follows:

1. Ice. Evidence of Permo-Carboniferous glaciation has been found in eastern Brazil, northwestern Argentina, on the Falkland Islands, in Belgian Congo (by Stutzer and Grosse). The glacial evidence is particularly beautiful in South Africa. There, according to Molengraaff's description, one can see striations on the glacially polished rock surface pointing from north to south. Furthermore, traces are found in the Indian Peninsula, in western and central Australia and even in eastern Australia. Unfortunately, age determinations are still not precise enough to indicate with certainty the timing of these glaciations; whether they were simultaneous or sequential. It is certain that the latest glaciation in Australia was Permian, later than that registered in South Africa, which was Carboniferous. The traces of glaciation in Brazil are probably Early Carboniferous.

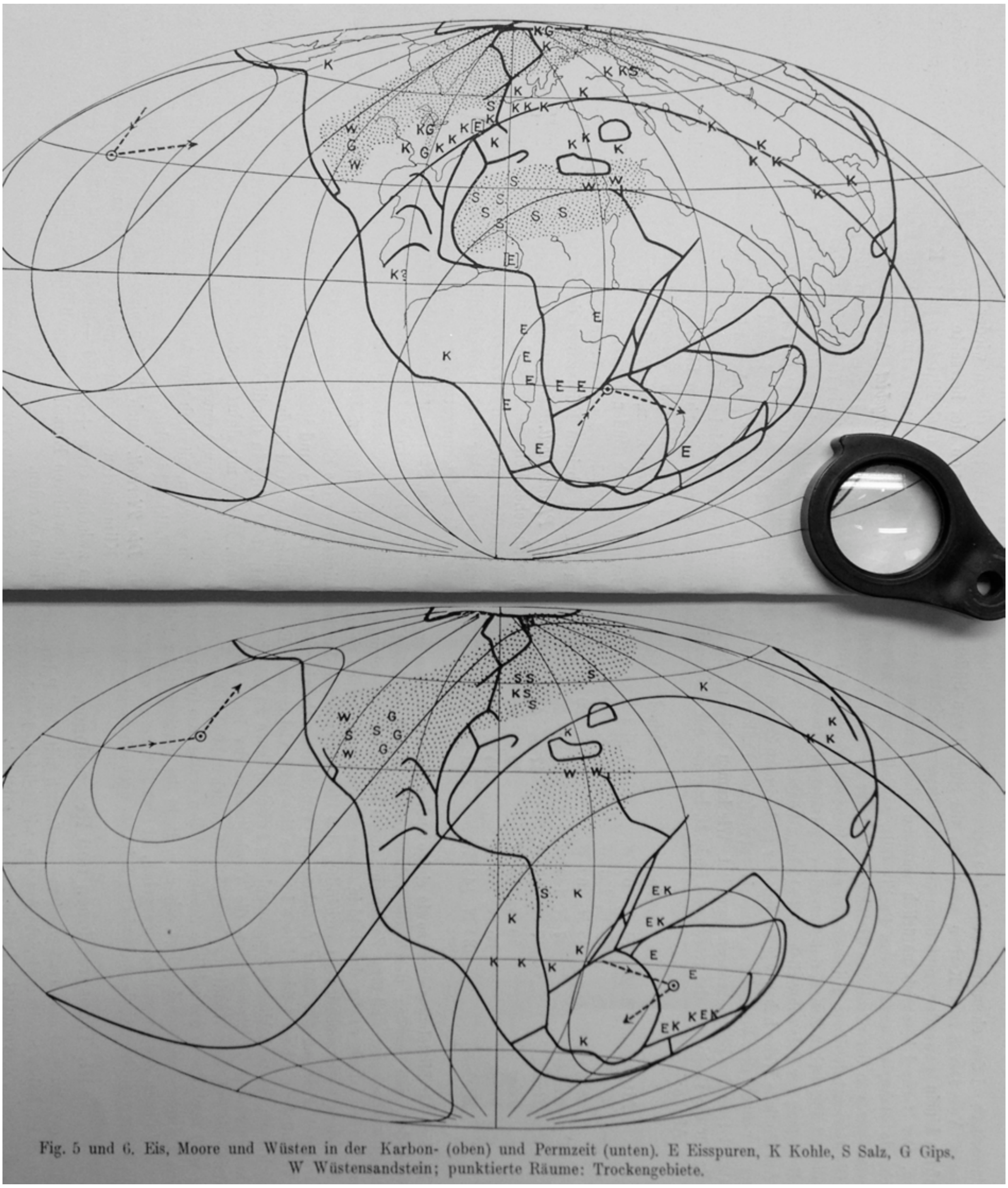


Figure 41. Figs. 5 and 6 of Wegener's encyclopedia article summarizing the book *Die Klimate der geologischen Vorzeit*. E= Ice deposits, K=Coal, S=Salt, G=Gypsum, W=Desert sandstones. From Wegener (1926).

2. Coal. Thin coal beds overlie the Permo-Carboniferous moraines in South Africa, the Deccan, and Australia. They evidently come from subpolar precipitation areas, just like our Quaternary peat bogs. However, the wide belt of thick commercial coal deposits stretches through North America, Europe and China. According to H. Potonié, the preserved plant fossils indicate tropical plants. This is shown, among other things, by their very rapid growth, the absence of annual tree rings, their cauliflory, and the occurrence of tree ferns and climbing ferns.

As the map of Fig. 5 shows, this belt lies exactly on the great circle 90 degrees away from the middle of the glaciated area. Also on Spitsbergen is found Carboniferous coal. According to Andersson it makes up more than two thirds of the total coal resources there. This coal is Early Carboniferous (Kulm). The plant fossils there are subtropical, just as they are on northeast Greenland at latitude 81 degrees north, and on Melville Island. Here we are evidently dealing with a subdivision of the Carboniferous arid zone, like today on Florida. The coals of the main coal belt are somewhat younger. The Chinese coals are Late Carboniferous, partly also Middle Carboniferous. The coals of Europe and North America (Pennsylvania, the Appalachians, Illinois, and Missouri) are Late Carboniferous. In Europe, namely in France, Thuringian Forest, Saxony, Bohemia, even the Permian still contains coal, of course, only in the lowest layers just above the Carboniferous. By the Middle Permian signs of dry climate appear again, just as dry climate ruled here in Devonian time. In Europe we see a movement of the coal-producing zone towards Central Europe in the Carboniferous, and movement back again in the Permian. No coal is found in Late Permian.

3. Salt, gypsum, arid region. From the Early Carboniferous no signs of arid regions are known. On the contrary, Early Carboniferous salt and gypsum are found in the eastern Urals and in Newfoundland just over the coal beds. Spitsbergen had an arid climate as early as the Early Carboniferous. We see that the arid zones followed on the heels of the coal formation. The greatest salt and gypsum deposits, however, do not appear until the Permian, in fact only in Late Permian, after coal production had stopped, in Eastern Russia, North Germany, the Southern Alps and the United States. It is impossible to misinterpret that the arid zone moved from the Late Carboniferous to the Late Permian in a north-south direction from Spitsbergen to the Southern Alps. It has to be the arid zone for the reason that the coal zone was moving to the south, but also due to simultaneous movement of the South Pole, which migrated in the Permian toward Australia, as well as due to the complete picture of the other climate zones of that time.

4. The flora. The so-called Glossopteris flora is probably a flora beyond the tree line of that time, at least in part. The tree line does not necessarily have to be identical with the 10-degree isotherm of the warmest month, as it is at present. However, it likely has a similar significance, namely that the free air temperature becomes too low for the growth of plants, while the sun's rays sufficiently warm the ground. This polar Glossopteris flora is found on the southern continents generally in layers, lying partly under, partly over the glacial layers. Consequently, they are similar to the interglacial layers of the European ice age. However, this flora goes beyond the glacial boundaries, because it is found additionally in the former German East Africa and in Kashmir and Afghanistan and is also frequently mixed, like in south Brazil and South Africa, with the tall trunked Lepidodendron-flora. However, it stands in clear contrast to the tropical flora of the European Carboniferous. Wood with annual tree rings, probably corresponding to the "snow forest climate zone" (according to Köppen's classification) so far has been found for this time only at two places, namely (by Arber) in New South Wales, Australia, and (by Halle) on the Falkland Islands.

5. The fauna. Also the faunal evidence fits comfortably to our climate maps. Particularly the marine carbonate reefs are limited to the area between approximately latitudes 30° north and 30° south on our maps. – For all further details, refer to our book mentioned above.

The best-fit line through the Carboniferous coal deposits defined the ancient Equator. That the Carboniferous fern-trees do not show annual rings is a good argument for the interpretation that they were located near the Equator and not subjected to a season of winter darkness without growth. The arid deposits lie to the north and south of the coals, and glacial deposits are located about 90° away from the

Equator, defining the position of the South Pole. Dashed arrows show the North and South Poles to have moved, but the movement of the poles is not required by Wegener’s evidence or theory.

In this summary, Wegener showed only these two maps. But in the book *Die Klimate der geologischen Vorzeit*, they showed separate maps for the nine different ages: Devonian, Carboniferous, Permian, Triassic, Jurassic, Cretaceous, Eocene, Miocene, and Pliocene. On those maps, it was clear that meaningful paleoclimate zones could be drawn on Wegener’s maps of mobile continents. Such zones do not exist on maps of fixed continents.

After the publication of the book by Köppen and Wegener, paleogeographers who rejected continental drift would never compile various types of climate evidence on a single map of the world. Such a map would display the glaring problems of fixed continents, and the success of Wegener’s hypothesis.

BROOKS’ *CLIMATE THROUGH THE AGES* (1926)

Another book on paleoclimate soon appeared: *Climate Through the Ages*, by C. E. P. Brooks, a British meteorologist. He showed separate maps for Permo-Carboniferous desert deposits and for glacial deposits. Neither map showed coal deposits, as he had little faith in climate interpretations based on fossils. He simply stated on page 270: "It is not possible to discriminate between equatorial and temperate coals"

His world map (Fig. 42) indicates the location of Carboniferous mountain ranges, because they would affect wind currents and climates. It shows the glacial deposits in India, and correctly shows that there were no Permo-Carboniferous mountains there. Brooks was aware of Köppen and Wegener's displacement interpretations, but was not interested in contrasting the two models in any detail. He would only consider the doctrine of fixed continents, but wrote (p. 294): "It must be admitted, however, that the glaciation of India is the least satisfactory part of the geographical theory of Upper Carboniferous climates."

For the second edition of his book in 1949, he found the way to eliminate this least satisfactory part. He removed the glacial deposits of tropical India from the new map. In the text he noted that Bailey Willis had suggested in 1932 that these glacial deposits might be from alpine glaciers, not continental ice sheets. In publications, authors must select or remove data as seems appropriate.

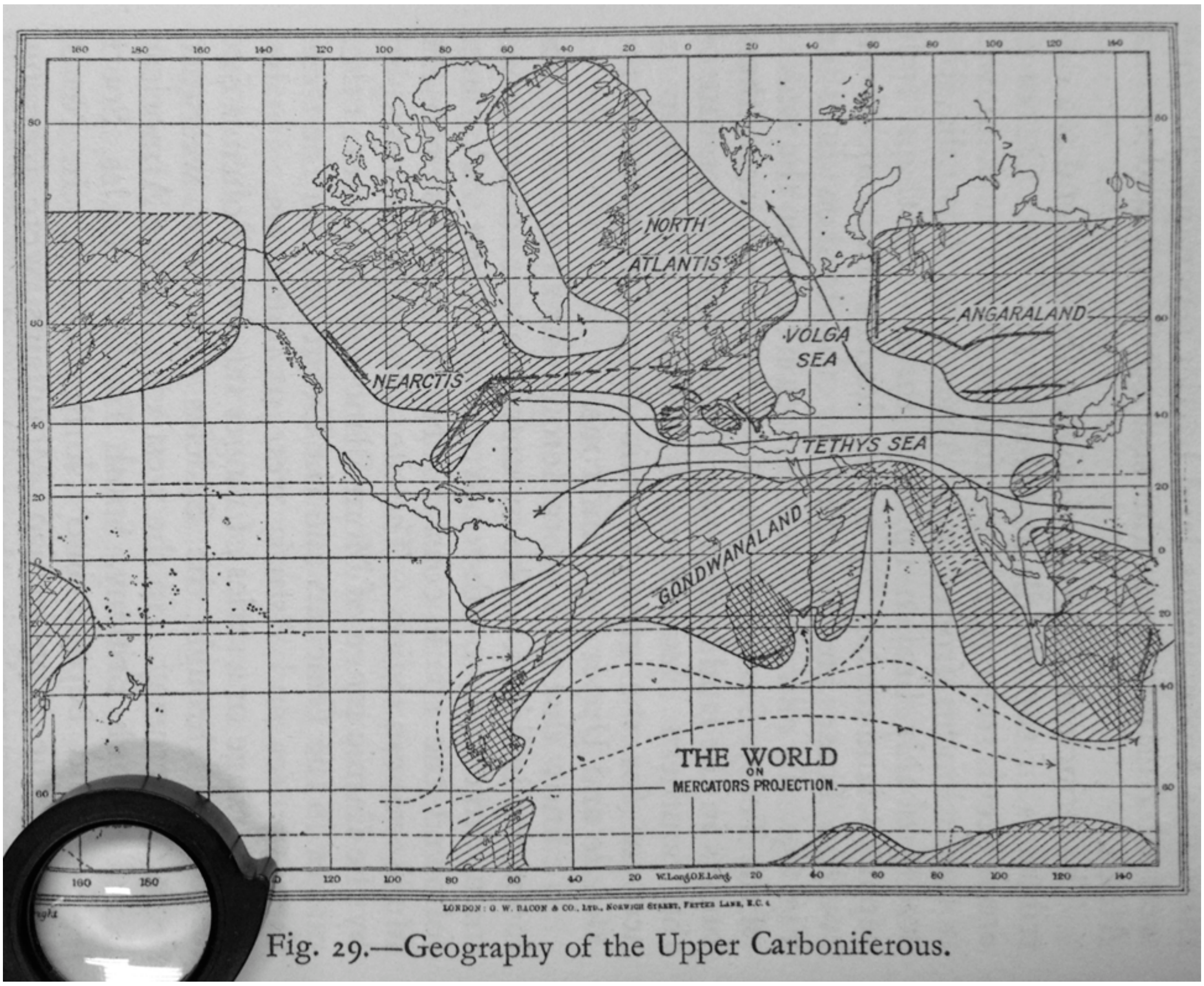


Figure 42. Brooks' paleogeographic map showing ancient continents, ocean currents, glaciated areas (right-slanting line pattern), and mountain ranges (isolated thin dark lines.) Desert deposits and coal deposits were not shown. From Brooks (1926).

6. Three Books that Boosted the Displacement Theory

ÉMILE ARGAND'S *LA TECTONIQUE DE L'ASIE* (1924)

Émile Argand (1879-1940) was one of the great Alpine geologists of his time. He understood how the Alps were formed. The deformation was primarily by horizontal movements, rather than by vertical uplift. When Wegener proposed global-scale horizontal movements, Argand was thrilled, and as early as 1916 he lectured to his Swiss colleagues about Wegener's theory (Oreskes 1999.) Argand coined the terms *mobilism* and *mobilist* for the idea of continental drift and those who supported it, and *fixism* and *fixist* for the doctrine that continents could not move great horizontal distances.

Argand had a facility for working with the three-dimensional shapes involved in Alpine geology. He could draw geologic maps and profiles in great detail from memory (Carozzi 1977) and he could envision how the shapes must have developed through time. Having conquered the Alps, Argand took on the tectonics of Europe, Asia and the world. He compiled large tectonic maps of all Asia, showing the patterns of rocks and styles of folding. His first map was finished in 1912 and his second in 1922. Although neither of these maps was properly published, this project brought him much recognition.

Argand knew the Alps firsthand from fieldwork. As a technical rock climber, he had checked even some of the most inaccessible places. But he did essentially no fieldwork or travel beyond the Alps and Europe. Of first, second, and third importance to him was not travel, but the study of published accounts and maps. In compiling his tectonic maps, he had read or consulted thousands of maps and geologic papers, in various languages. He could speak French, German, English, Italian, Spanish, Greek, Russian, and Sanskrit, and could read Latin and some Chinese (Carozzi 1977). But he was not so fond of writing, and produced relatively few scientific publications during his career.

In August 1922 he presented his celebrated map at the International Geological Congress in Brussels. But he had not prepared a manuscript to publish in the congress proceedings. The editors desperately wanted his written contribution, so they delayed publication until his manuscript was ready. Finally in 1924 he submitted it, and the proceedings were immediately published, including his 200-page paper *La Tectonique de l'Asie*.

Argand's paper had few sketch maps and geologic profiles, but they were intriguing. One map showed the close fit of continents in Gondwana, and the huge area of shallow marine sediments that would be shortened and compressed to form the Himalayas (Fig. 43, area c). And the profiles showed how mountain ranges formed by continental collisions (Fig. 44). But his paper must have been a disappointment. For some reason, his large colored map was not included and was totally unavailable to readers. This publication consisted largely of discussion of the rock patterns and the various folds of an unavailable map.

The 200-page paper had little that was concrete and nothing quantitative. There was not a single table or list of any kind. The only numbers were a few longitude or latitude coordinates, a few map distances, a few publication dates, and identification numbers in the map legends. There were no bibliographic references to the maps or literature he had used, except the names of a few authors, most notably Bailey Willis, who had done pioneering geology work in China that was published in English. Argand did not discuss rock types, rock layers, or their characteristics and thicknesses, which are the usual substance of tectonic and geologic interpretations.

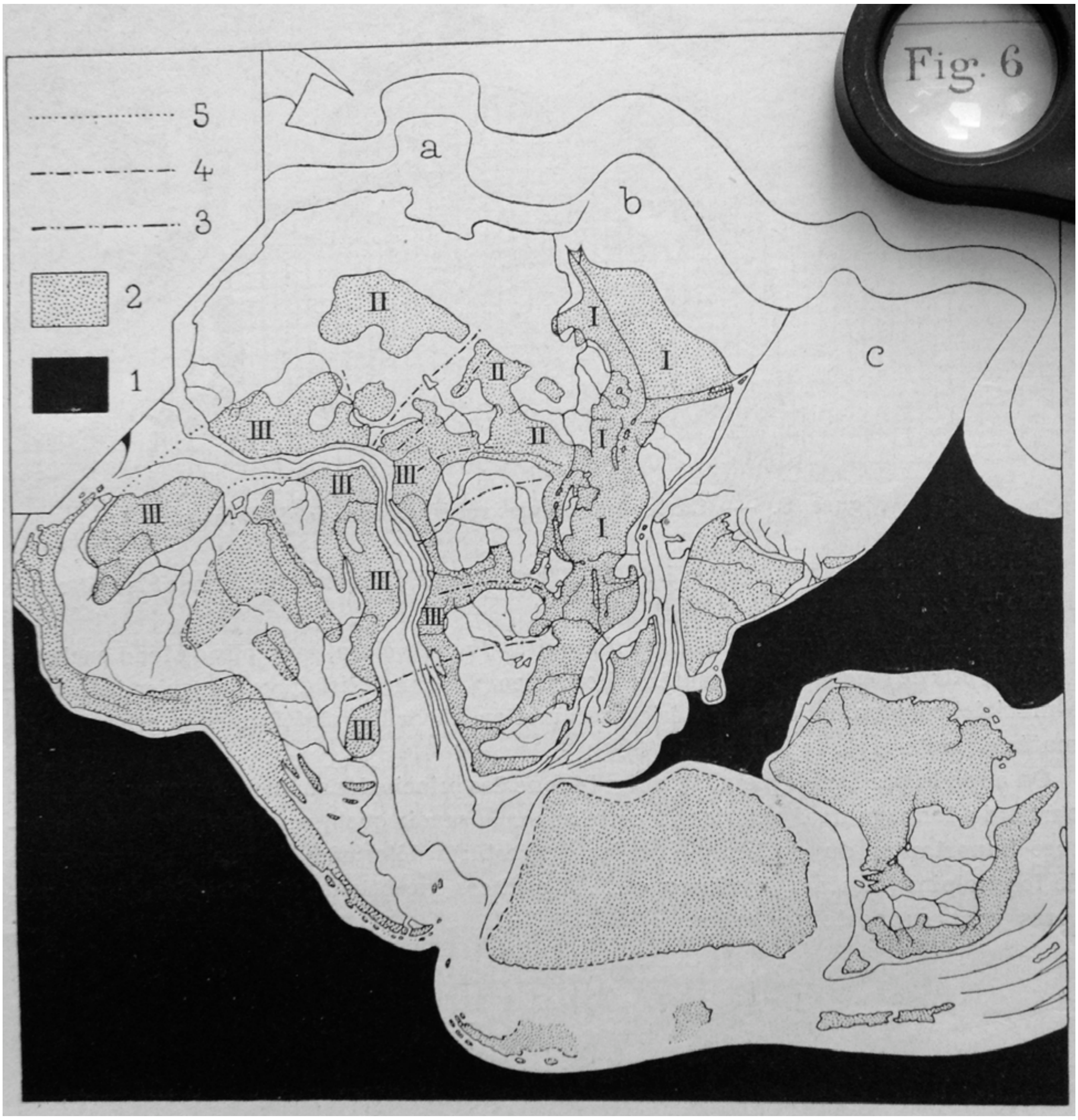


Figure 43. Argand's Fig. 6 showing some positions and details in the Gondwana continent. In the explanation of the legend, number 1 is sima, 2 is anticlinal uplifts, 3, 4 are fold axes. Areas a, b, and c, are the parts of Gondwana that formed Tethys, and then mountains in his cross section Figs. 15, 14, and 13. From Argand (1924).

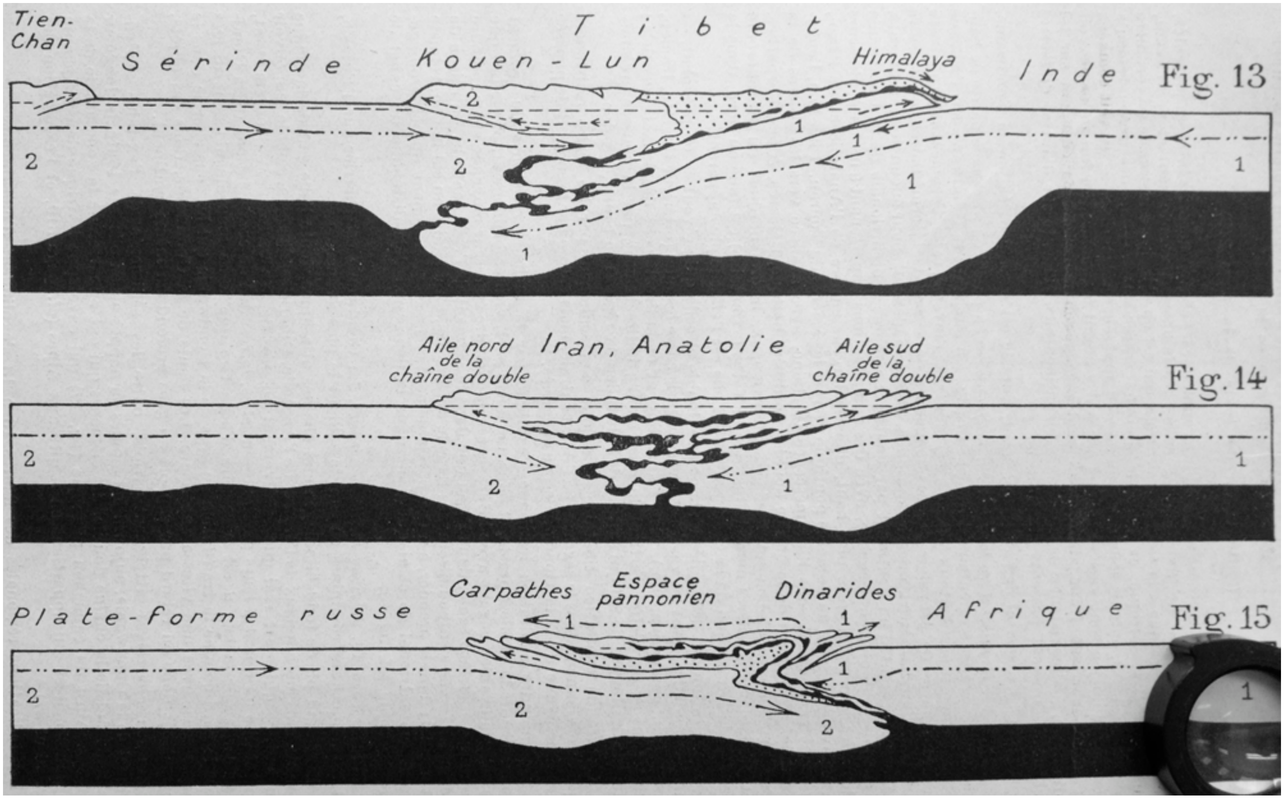


Figure 44. Argand's cross section Figs. 13, 14, 15. Number 1 is Gondwana, 2 is Eurasia. Black is sima beneath the continental blocks. From Argand (1924).

Without such documentation, the paper is what geologists would call "arm waving." Had such a manuscript come from an unknown author, it would certainly have been rejected. Half a century later, Carozzi (1977) translated Argand's paper and improved it by adding the large color map.

Wegener appreciated the support that Argand gave to the displacement hypothesis. Wegener used Argand's Figs. 6, 13, and 15 in the 1929-edition of his book. But in fact, Argand's endorsement partly misled Wegener. Argand repeated the same incorrect interpretations that Wegener had made in 1915, 1920, and 1922: that the Mid-Atlantic Ridge contained old continental material, left behind after the opening of the Atlantic. He envisioned that the folds of the Andes and the folds of New Zealand and eastern Australia had been pushed up by the resistance of oceanic sima as continental crust plowed through it. Geophysicists and geologists, including Bailey Willis, scoffed at this idea, and many have wondered why Wegener firmly held on to it. I think it was partly because he was encouraged to do so by the support from Argand.

REGINALD A. DALY'S *OUR MOBILE EARTH* (1926)

The only North American geologist to write a book that was favorable to Wegener's continental displacement was Reginald A. Daly (1871-1957), Professor of Geology at Harvard. He saw that the horizontal motion of the continents could help explain the origin of mountain ranges, much as in our modern plate tectonic interpretations. Here is Daly's introduction to this topic in his book *Our Mobile Earth*:

Daly 1926, p. 259-263.

Having listed the minor kinds of mountain structures, we are now to consider the fourth and most important kind. Here the main problem is the origin of the force responsible for the principal mountain chains. The preceding chapter gave the first part of the answer to this question, an answer agreed upon by practically all geologists. It was there stated that folding, thrusting, and up-ending of strata are the necessary results of pressure, exerted at the ends of the originally flat beds; that is, horizontal pressure. We recall Figures 142 and 143, giving sections which illustrate the experimental imitation of mountain folds and thrusts by lateral pressure; and we remember that mountain ranges are chiefly made of geosynclinal prisms, that is, very extensive and thick lenses of sedimentary, stratified rocks.

We now attack the second part of the question of origin, namely: Why has there been horizontal compression of the rocks at all? There has been much discussion of this, one of the chief questions of natural history. I shall not attempt to weigh the relative merits of the various older theories of mountain-building. These theories, all more or less unsatisfactory, are noted in readily accessible text-books of geology. Less widely published is a new, startling explanation, first announced by an American geologist, Frank B. Taylor, and independently worked out by Alfred Wegener, a German meteorologist. Many geologists have found their idea bizarre, shocking; yet an increasing number of specialists in the problem are already convinced that it must be seriously entertained as the true basis for a sound theory of mountain-building. The subject has been under discussion only a few years and is laden with difficulties, so that a full and objective treatment is still impossible. Nevertheless, every educated person can not fail to be interested in this revolutionary conception.

Taylor and Wegener believe that the mountain chains of the globe were formed by the horizontal crushing of geosynclinal prisms which lay in front of *slowly moving, migrating continents*. For example, according to this hypothesis, the formation of our Rocky Mountain system was due to the slipping of all North America in a westerly direction, away from Europe, and that for a distance of hundreds of miles (Figure 163). This migration of the vast continental block took place under the direction of a force so powerful that the strata of the Rocky Mountain geosyncline were crumpled and thrust into mountain turmoil.

Some geologists, especially European geologists, saw at once how the new hypothesis explains automatically not only mountains but also a dozen other mysteries in their science. But there is a difficulty. Neither Taylor nor Wegener has shown *why* the continents should move. They have not discovered the force which did the gigantic work of overcoming the resistances to continental migration. Nor have they evaluated those resistances. For these reasons geologists are going slow in placing such mobility of continents among the accepted principles of science.

This conservatism is justifiable until some one has discovered the force available for the movement of continents. To offer to the general reader a new suggestion on this fundamental question is a decidedly bold step. However, after prolonged study of the subject, I have come to the conclusion that the suggestion now to be presented is in principle unescapable, if the continents are not securely anchored and have bodily migrated. The supplementary hypothesis really implies a restatement of the idea of continental migration.

The general hypothesis is, then, to be presented in the following form: The continents appear to have slid down-hill, to have been pulled down, over the earth's body, by mere gravity; mountain structures appear to be the product of enormous, slow *landslides*. Each chain has been folded at the foot of a crust-block, with the thickness of the earth's crust, taken to be about 40 miles. In an analogous way, a ladder, inclined against a wall at a low angle, exerts at the foot of the ladder horizontal pressure, which is a fraction of the weight of the ladder.

A more detailed statement of the case may be made from two points of view. First, were the continents actually tilted toward the geosynclines before the respective mountain chains were formed? Secondly, why should the crust of the earth have been thus deleveled on this world scale?

Daly reproduced Wegener's three globe maps as his Figure 163, and made very favorable comments. In a later section, Daly referred again to Wegener's three maps, and followed them up with maps of his own, of the Atlantic (see Fig. 45) and the Pacific coastlines:

Daly 1926, p. 280-284.

Wegener has attempted to reconstruct the original continent. The uppermost drawing of Figure 163, page 261, illustrates his conception of what the lands were like in the so-called Carboniferous period of the earth's history. The Americas are shown cheek by jowl with the Old World. The middle map of the figure represents a stage when the sliding of the Americas had opened up the Atlantic Ocean, then narrow. The third map, below, represents a still later stage, when the migration of the continents had gone farther, the Atlantic basin being correspondingly widened.

5. The new hypothesis details automatically account for the general absence of mountain chains bordering the Atlantic, Arctic, and Indian Oceans. For in the more recent geological time these have been regions of tension, not of horizontal compression. On the other hand, the Pacific should be, and is, well framed in mountain chains. In brief, the Atlantic and Pacific types of coast-line, fundamental features of the earth, are adequately explained by the hypothesis of the migration of continents. Figure 169 [Fig. 45] is a map of the Atlantic basin with its great gulf-like extension, the Arctic basin. The Circum-Pacific mountains of America are shown in solid black and are marked with the letter *P*. The east-west, older Hercynian-Appalachian system of the northern hemisphere is in part shown at *I* by axial lines. The slightly younger east-west system of the southern hemisphere outcrops in Argentina and South Africa at *II* where again the axial trends or "directories" are marked by short lines. The youngest east-west system, the Belt of Mediterranean Rugged Mountains, is similarly indicated at *III* in both the New and Old Worlds. The truncation of the mountain axes by the Atlantic coast is clear in every case. The contoured Mid-Atlantic Swell is a special feature of the Atlantic basin; its possible significance has already been mentioned.

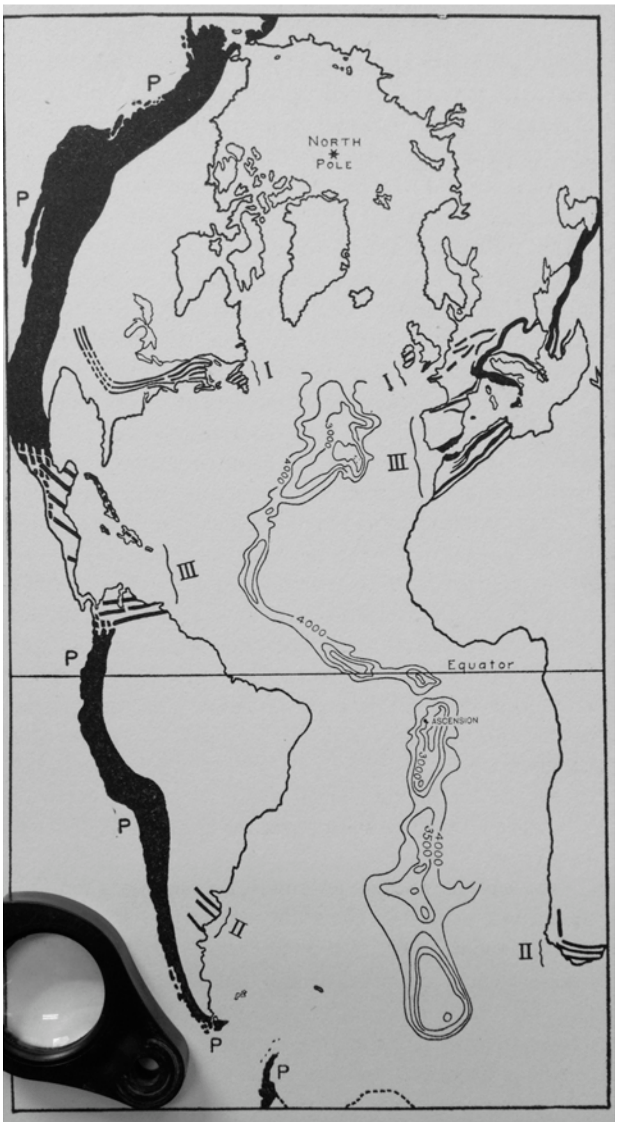


FIG. 169. ATLANTIC TYPE OF COAST-LINE.

Figure 45. Daly's map of the Atlantic Ocean, showing the match of orogenic belts (I, II, III) on each side, and the Mid-Atlantic Swell, from which he postulated that the continents had slid away. From Daly (1926).

Daly was supporting Wegener's ideas of displacement, but suggesting an alternative mechanism – the gravitational sliding away of the continents from the Mid-Atlantic Ridge. Like Wegener and Argand, he thought that the Mid-Atlantic Ridge contained continental crustal material:

Daly 1926, p. 279-280.

The zone where the New and Old Worlds were torn apart is plausibly regarded as the so-called Mid-Atlantic Swell, which is a long and broad "ridge" on the ocean's floor; its height averages about one mile, or 1,600 meters. The map of Figure 169 bears contour lines of the Atlantic bottom, at 500-meter intervals, from the 4,000-meter contour upward. These lines locate the Mid-Atlantic Swell. On its back the volcanic island of Ascension has been built. The ridge or swell is in the middle of the Atlantic basin, about half-way between Brazil and Africa. The Ascension Island and other volcanoes on the swell have erupted fragments of typical continental rocks to the surface. The swell may, therefore, conceivably represent a long strip of the original continent, a strip left behind when that continent was torn into fragments, which slid away, respectively to westward and to eastward.

In a chapter called The Origin of Mountain Ranges, Daly wrote that the same processes could explain the great amount of crustal shortening involved in the formation of fold mountain belts. Glassy basalt, in solid form but without crystalline structure, would be quite ductile at high temperature. The lower parts of the crust could be removed as they plunge down into this ductile material, as the sediments and the upper part of the crust were folded and horizontally compressed (Fig. 46). The idea resembles subduction and formation of a melange of scraped-off sediments. We now know that the substrate is not basalt or basaltic glass, as Daly envisioned, but ultramafic rocks of the mantle. Although his working hypotheses were not completely correct, they were useful. They could have been improved, but no other North American geologists were willing to work with hypotheses that involved mobilism.

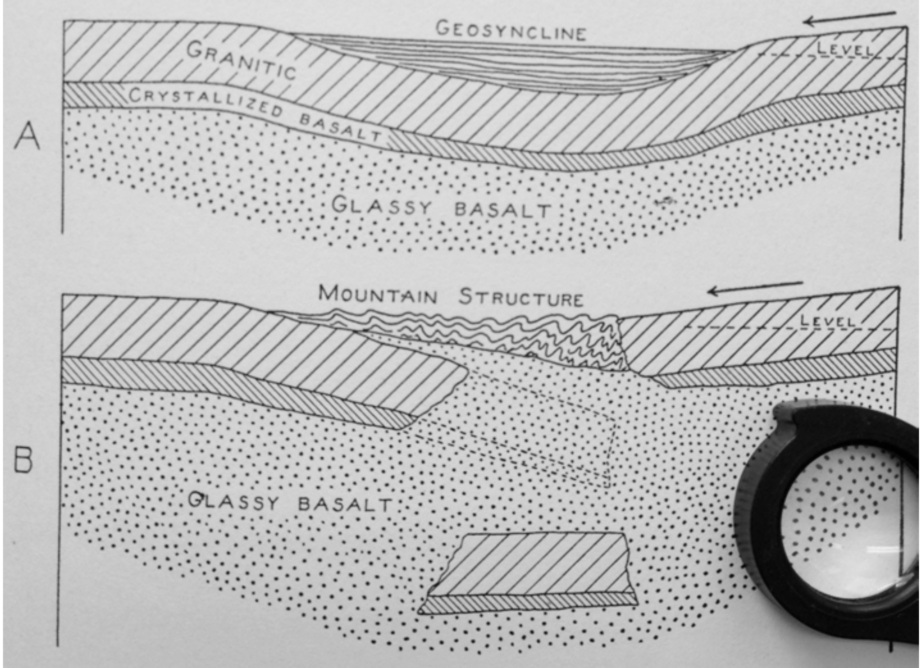


FIG. 166. DIAGRAMS ILLUSTRATING THE CRUMPLING OF GEOSYNCLINAL PRISMS OF LAYERED ROCKS BY THE SLIDING OF A LARGE BLOCK OF THE EARTH'S CRUST.

Figure 46. Daly's figure showing formation of a fold-mountain belt and a process that resembles subduction. From Daly (1926).

ALEX DU TOIT'S *GEOLOGICAL COMPARISON* (1927)

In the third edition of his book in 1922, Wegener had noted six connections between South America and Africa. We have already discussed one of these: the Cape fold-belt of South Africa that matched the Sierras of Brazil. Wegener understood that the strongest evidence that continents were closely juxtaposed is the direct continuation of linear geologic features. He explained this type of evidence in one of his best analogies, the torn newspaper:

Wegener 1924, p. 55-56. (p. 37-38 in the original German edition 1922).

The correspondences of the Atlantic coasts already mentioned, namely, the folding of the Cape mountains and of the Sierras of Buenos Aires as well as the correspondence between the eruptive rocks, sediments, and strike-lines in the great gneissic plateaus of Brazil and Africa, the Armorican, Caledonian, and Algonkian systems of folding, and the Pleistocene terminal moraines [I delete a few line here.] It is just as if we put together the pieces of a torn newspaper by their ragged edges, and then ascertained if the lines of print ran evenly across. If they do, obviously there is no course but to conclude that the pieces were once actually attached in this way.

This citation shows that Wegener felt he had found at least six continuations across the Atlantic. There were actually only five here, because his matching of Pleistocene glacial deposits on both sides of the northern Atlantic is not a valid continuation. But these five continuations were only a start; Alex Du Toit published a book with very many more.

Recall that Du Toit was the first to understand the strange directions of ice transport for the glaciation of Gondwanaland. He had also inspired Reginald A. Daly to write the book about mobilism. Du Toit had led a geological field trip in Africa for Daly and others, and it was after that trip that Daly wrote his book.

Daly had been impressed with Du Toit's knowledge and ability, and helped him get a grant from the Carnegie Institution of Washington to compare the geology on both sides of the southern Atlantic.

Having gotten the grant, Du Toit postponed his African projects, so that he could travel to South America in 1923, where he spent five months on field trips and consultations with leading geologists there. Du Toit became fully convinced that Africa and South America had been joined. In 1927 the Carnegie Institution published his *Geological Comparison of South America with South Africa* as a monograph. This book had been delayed by a few years, as he wanted to first publish *The Geology of South Africa* (Du Toit 1926), an important reference book that African geologists had been waiting for.

Most of the *Geological Comparison* consists of descriptions and documentation of South American geology. Chapter 7 is perhaps the most interesting, as it summarizes points of direct comparison between South America and South Africa. It is too much for us to follow all the geological and geographical details, but it is interesting to see what types of comparisons he made. I reprint his summary below, but have shortened it considerably, indicating his correlations only by their numbers or letters and a few key words followed by three dots (...). Although my citations are abbreviated, they give an impression of the extent of his knowledge and geologic evidence (and also the awkwardness of his writing and inconsistency of his labeling.) Most of his correlations seem very convincing. A few of his arguments are negative evidence, based on a lack of data. I have marked these as [0], because I consider them insignificant. A few of his arguments are in fact multiple correlations, which I have marked as [2], as I think they have double significance.

Du Toit 1927, p. 109-117.

CHAPTER VII

BEARING ON THE DISPLACEMENT HYPOTHESIS

While the general geological resemblance between those portions of the two continents that face the South Atlantic basin has long been perceived, the outcome of these present studies is nothing less than extraordinary, considering the enormous stretches of ocean parting these two land-masses.

Such points of resemblance have now become so numerous as collectively almost to exceed the bounds of coincidence, while they are, moreover, confined not to one limited region nor to one epoch, but implicate vast territories in the respective land-masses and embrace times ranging from pre-Devonian almost to the Tertiary. Furthermore, these so-called "coincidences" are of a stratigraphical, lithological, palæontological, tectonic, volcanic and climatic nature.

Of prime importance, moreover, is that evidence obtainable from the study of the phasal variations displayed by particular formations when traced within their respective continents.

In illustration, let us consider the case of two equivalent formations, the one in South America beginning on or near the Atlantic coast at *A* and extending westward to *A'* and the other in Africa starting similarly near the coast at *B* and stretching eastward to *B'*. Then it can be affirmed that more than one such instance can be designated, where the change of facies in the distance *AA'* or *BB'* is *greater* than that found in *AB*, although the full width of the Atlantic intervenes between *A* and *B*. In other words, these particular formations along the two opposed shores tend to resemble one another more closely than either one or both of their actual and visible extensions within the respective continents. With the multiplication of such examples, drawn from more than one geological epoch, such a singular relationship can no longer be regarded as wholly fortuitous and a definite explanation therefore has accordingly to be sought. An analysis, moreover, shows that this unexpected tendency is equally marked, whether the formations involved be marine, deltaic, continental, glacial, eolian, or volcanic.

If, on the other hand, the two land-masses are pictured as having been moved closer together, as in figure 7 [Fig. 47], a great number of observations and deductions are now found to be brought into apparent harmony, and these possible "coincidences" are disposed of in the simplest fashion.

This is precisely what the displacement hypothesis effects, thereby providing a simple explanation of many otherwise puzzling observations. The fact that many eminent scientists have cast doubt upon its geophysical possibility should not be permitted to cloud the issue any more than that the existence of former "land bridges" should be denied because of cogent objections based upon the doctrine of isostasy.

It is not proposed to discuss here the physical basis of that hypothesis, nor is it desired to deal with this problem as a whole, such as has been done by Wegener (1924). The intention is merely to set forth some of the data regarding Africa and South America and to state the conclusions to be drawn therefrom, that are distinctly awkward of explanation under the current and orthodox view of "land bridges," but which, on the contrary, appreciably favor the "hypothesis of continental disruption." Incidentally, some few observations will be made having a bearing upon other parts of Gondwanaland, that in turn suggest lines of future research in those particular countries.

Of prime importance is the extraordinarily close correspondence in the outlines of the opposed shores of the two continents, as has been pointed out and discussed by others long before Wegener, and which is particularly marked when comparison is made not with maps, but on the face of a terrestrial globe. Next is the presence of the central Atlantic rise beneath the ocean, with its surprisingly symmetrical position nearly midway between the Old World and the New.

Interpreted *mathematically*, the great regularity of these three features, extending through the entire length of the South Atlantic, would betoken an *enormously high probability* that such features had owed their origin to one and the same set of tectonic forces at a relatively late geological period. Upon this rise are, furthermore, aligned certain of the volcanic islands of the southern Atlantic. This otherwise profitable subject must, however, be relinquished in favor of the more momentous geological aspect.

Commencing in the south, the following relationships can be considered as more or less established:

(I) The section south of Bahía Blanca and that below the Zuurberg in the Uitenhage district show several points of agreement in that (a) The upper Triassic (1) is predominantly of volcanic origin, (2) rests discordantly upon Permian or older beds that are affected by Permo-Triassic movements, (3) is influenced by mid-Cretaceous disturbances... (4) is overlain by marine Cretaceous...clays..., (b)...Uitenhage invertebrate fauna...[2], (c)...flat marine ...

(II) The ranges north of Bahía Blanca undoubtedly correspond with the Cape Fold ranges, in each case showing: (a) Intense folding, with oveturning toward the north or northeast, strata up to the Perian being involved. (b) The quartzites of the Sierra de la Ventana correspond lithologically… (c) The fossiliferous Devonian is like that of… (d) The glacials in Argentina duplicate the characters of the Dwyka tillite..., (e) dark shales overlie the glacials..., (f) dolerite intrusions are absent in each case [0], (g) Consolidated ferruginous gravels … duplicate in most extraordinary fashion... [2], (h) ...

(III) To the northeast of the Sierra de la Ventana the "Gondwanides" are fading out, thus paralleling the conditions…

(IV) Comparing the region stretching from Uruguay to Minas in Brazil with that between Clanwilliam and the Kaokoveld, we find: (a) The almost horizontal Furnas sandstone of Paraná is similar to the equivalent Table Mountain sandstone of Clanwilliam and Van Rhynsdorp. (b) Each is thinner and softer..., (c) Each is succeeded by the marine Devonian shales..., (d)...sandstone with *Spirifers* duplicates..., (e) The base of the glacials...exactly as is the case, (f)...uneven floor... [0], (g)...glacials absent... [0], (h) A southeasterly source is presumed for the Brazilian glacials..., (i)...shales are identical…, each containing the reptile *Mesosaurus*, not known in other parts of the world... [2], (j)...a deltaic phase..., (k)...Silicified wood is common in both countries..., (l)...positive earth movements... [0], (m)...underlying strata..., (1) Each series transgresses..., (2) strata with the "*Thinnfeldia* flora" … and the fresh water crustacean *Cyzicus* is common to both these areas; [2], (3)...Red beds and dinosaurian remains belonging to allied forms; [2], (4)...sandstone of arid climate,,,, (5)...volcanics interbedded with sandstones..., (6)...volcanics form coast..., (7)...basalts with but little olivine..., (8)...effusions terminate the succession in both countries. (m) Widespread injections of dolerite… (n) Marine Jurassic and Cretaceous beds are absent... [0], (o) Kimberlite and melilite-basalt pierce the strata..., (p)...Cretaceous continental deposit, (q) The detrital diamonds of southwest Africa that have been derived from Tertiary marine beds near Lüderitz are quite unlike those won from the kimberlite pipes of South Africa, but show crystallographic and physical points of resemblance with the gems obtained in eastern Brazil from gravels or from pipes, such as the Bôa vista Mine… (r)...quartzites, limestones and slates..., (s)...alkaline rocks, both plutonic and effusive, on either side of the Atlantic, for example, ...

(V) Proceeding farther north we find: (a) Opposed to the disturbed area of Cretaceous and Tertiary of Angloa and Loanda are those of Bahía and Sergipe..., (b)...folded Permian...limestones... [2]

(VI) Farther north is found a belt of Cretaceo-Eocene along the coast from Natal westward that rises up to form the plateau along the boundary of Ceará with Piauhý and is seen again in the northern part of Maranhao. These strata can be compared with the beds of approximately similar date in the coastal portions of Gold Coast, Dahomey, and Cameroons, extending up the valleys of the Niger and Benue... Reference might be made to the well-known volcanic line of the Cameroons that extendws southwestward into the ocean, in view of the presence off the corner of Brazil of the phonolite island of Fernando Noronha.

(VII) The Gondwana outlier of Maranhao and Piauhý constitutes a fairly close parallel with the development known as the Lubilache in the western part of the Congo Basin in (a) the absence of the glacial group, [0] (b) the absence of coals, [0] (c) the equivalence apparently of the upper red sandstones of Brazil with the cream and red friable sandstones in the Congo.

(VIII) The Silurian and Devonian strike south-southwestward through the Sahara and southwestward through West Africa, with isolated patches of the latter system in Gold Coast Colony, and are apparently not much disturbed generally, a condition that can be paralleled with the corresponding systems forming the syncline of the lower Amazon Valley resting on the Archæan granite...diamonds... [2]

While the above can be taken as merely summarizing the evidence on the subject, it will be clear to all acquainted with the literature that many other points of similarity or else of analogy could readily be discovered in the geology of these two land-masses. Sufficient, I venture to think, has already been set down to bring out the astonishing geological agreement between more or less comparable sections of the respective coastal zones, from which it will be conceded that the evidence so far would distinctly appear to favor the displacement hypothesis rather than orthodox ideas. It is nevertheless highly desirable that more impartially minded persons should criticize the data here given and judge whether those amazing resemblances have been correctly interpreted or whether some more rational explanation for them could not be formulated.

Accepting provisionally this hypothesis, it will next be instructive to attempt some graphic representation thereof.

Regarding the various possible dispositions of the continental masses, it can first of all be remarked that actual contiguity of the opposed shore-lines can most definitely be ruled out, and, secondly, that even apposition of the borders of the continental shelves, as favored by Wegener, may perhaps hardly be warranted, for utilizing the line of reasoning based on phasal variation, the differences actually noticed between the various facies of the equivalent formations where they come closest together are of such a degree as to demand a fairly wide gap. *One of the order, perhaps of from 400 to 800 km., would indeed seem to be needed, if all the observed phenomena are to be satisfactorily accounted for.*

Such is schematically represented in Figure 7, which is tentatively suggested as perhaps best meeting the needs of the case, though the intervening space is actually a little less than the phasal variations would rightly demand.

Further critical comparative studies should enable a better orientation to be made, the diagram advanced being admittedly only a first approximation. Granted, too, that such crustal slipping could have taken place, it must not be overlooked that some regional distortion might have resulted during the drifting apart of the floating continental blocks, an action inferred from the inflection of the Andine foldings in the neighborhood of Cape Horn.

It will be noticed that in this figure the Falkland Island have, following the discussion in Chapter VI, been moved up into a position between Cape Town and La Plata. Only now can the remarkable correspondence of the various fold-lines, and even of the more moderate archings, of different ages be properly appreciated. The rupture of the Afro-American mass is also more than hinted at in the strict parallelism of the zigzagging of the two coast-lines near Rio de Janeiro and Angola respectively. It is surprising, too, though not deliberately arranged, that the space between the two shores should correspond so closely in its two boundaries with the plan of the "central ridge" of the South Atlantic. These two sections of coast-line, it should furthermore be noted, are composed of crystalline rocks, whereas both to the north and south considerable stretches can be found where late Tertiary and Quaternary marine sediments fringe the shores and consequently tend to obscure the original outlines of the supposedly fractured masses.

Further elaboration is hardly necessary, and the diagram is accordingly left to the criticism of the reader.

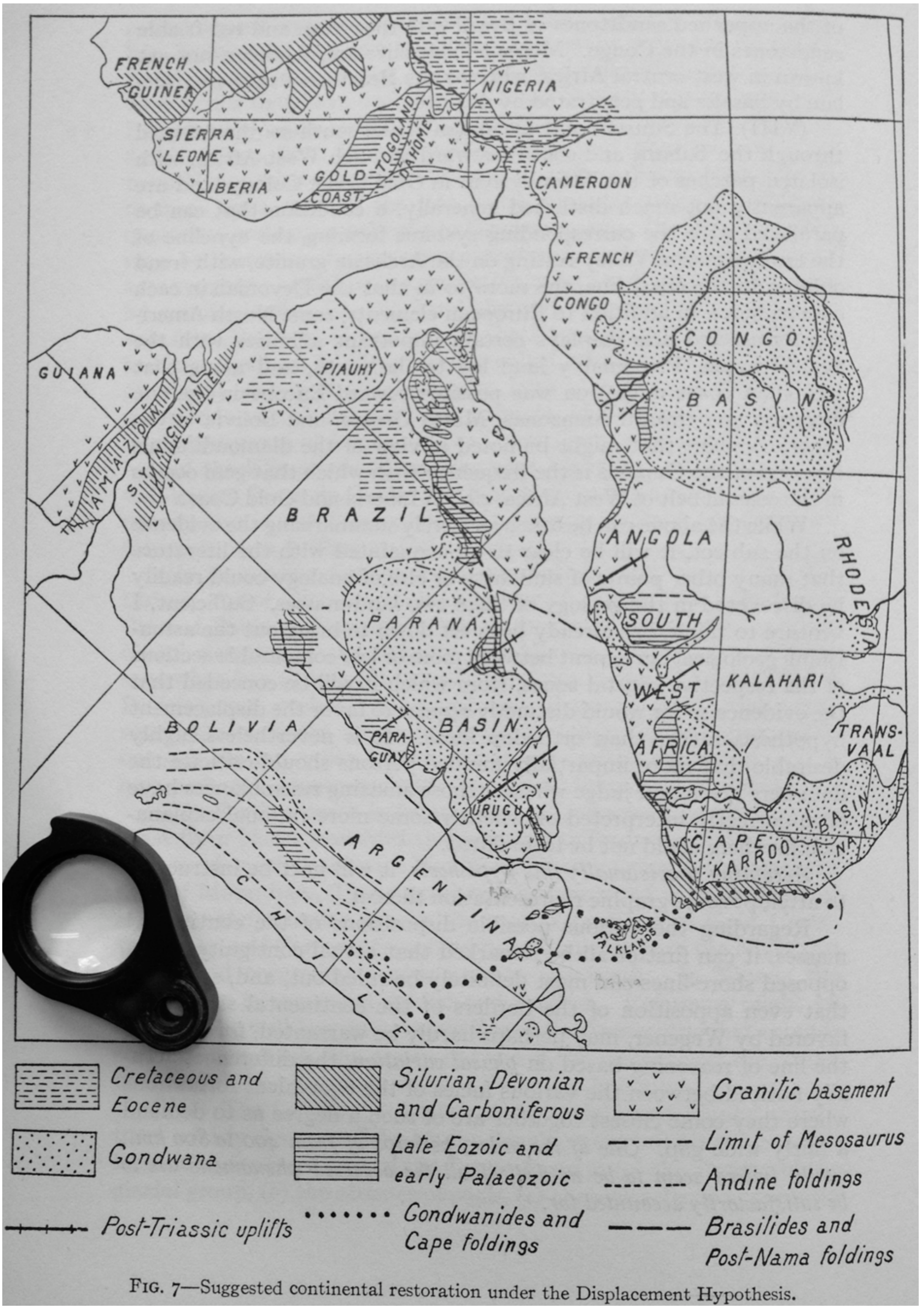


FIG. 7—Suggested continental restoration under the Displacement Hypothesis.

Figure 47. The Du Toit-fit. Leaving room for Mid-Atlantic Ridge rocks which were thought to come from here. From Du Toit (1927).

Du Toit left a 500-kilometer gap on his map, wide enough to account for the Mid-Atlantic Ridge, which he thought included continental material. He also moved the Falkland Islands far to the north, which is now known to be incorrect.

Du Toit's evidence convinced many African and South American geologists that the two continents had indeed been joined. Wegener's hypothesis had made predictions that succeeded admirably. The displacement theory was now being used as a working hypothesis by many geologists in the southern hemisphere, and by some in Europe.

SCHUCHERT'S CRITICAL BOOK REVIEW (1928)

It is not easy for geologists to read and understand foreign geology, even if it is written in a language that they can read. Rock units are named after type localities, so it takes great motivation to tackle a book like Du Toit's, with its foreign names and unfamiliar local geography. The North American geologist who was perhaps most motivated and most capable of reading *Geological Comparison* was Charles Schuchert. Soon after its publication he carefully studied it and published a book review in the *American Journal of Science*. He scorned Du Toit and his work. Schuchert's first two paragraphs read as follows:

Schuchert 1928b, p. 266.

The Continental Displacement Hypothesis as Viewed by Du Toit.

Ever since the return home, some years ago, of R. A. Daly and F. E. Wright from their geological field studies in South Africa, we have heard that Du Toit, the most enthusiastic supporter of the Wegener displacement hypothesis, would visit South America and give us his observations on the similarities between his own land and that continent. Under a grant from the Carnegie Institution of Washington, Du Toit has spent five months travelling widely in Brazil, Uruguay, and Argentina, always with the best guidance that the local geologists could give him, and now we have his results.

The greatest value of the book lies in Du Toit's clearly presented and detailed descriptions of the geology he saw, plus the best information taken out of the literature of South America; and likewise in his views regarding the intercontinental correlations with South Africa, with which he is of course most familiar. These correlations are, however, those of a field worker, a geologist depending almost wholly on the physical evidence, and one who shows that he has but little insight into, and less faith in, paleontology. Indeed, he actually goes so far as to say that fossils are incompetent to solve the correlations demanded. This is the greatest weakness of the book, met with in many places, and finally toward the close of the discussion we read: *"Geological evidence almost entirely* must decide the probability of this hypothesis, for those arguments based upon zoö-distribution are incompetent to do so" (p. 118). Curiously, whenever the fossils agree with his conclusions he uses them, although to him all Permian fossils are clearly of Upper Carboniferous age.

These first two paragraphs were probably enough; North American geologists must have felt that it was not necessary to read more of this book review. And they would certainly not bother obtaining Du Toit's book and trying to read his biased interpretations of South American and African geology.

But Schuchert's review was unfair, even this first citation. He quoted Du Toit out of context, for the purpose of misleading his readers. Note above that Schuchert ended the quote by Du Toit with no period at the end of the sentence. There should have been a comma there, followed by the rest of the sentence. Schuchert stopped it in the middle, because the second half of the sentence would have shown that Du Toit's argument was reasonable after all. The complete sentence should read (my underlining): *"Geological evidence almost entirely* must decide the probability of this hypothesis, for those arguments based upon zoö-distribution are incompetent to do so, being as a rule equally, though more clumsily, explicable under the orthodox views involving lengthy land connections afterward submerged by the oceans." Du Toit's point was that terrestrial animals and plants might indeed cross the Atlantic on a land bridge. No amount of fossil evidence could convince a fixist who was willing to believe in sunken land bridges. But land bridges could never explain other types of geological evidence, such as the matching of rocks and fold mountain ranges. Du Toit was trying to deal fairly with the significance of African and South American fossils; Schuchert was not.

Du Toit had not tried to count his positive comparisons, although he labeled them somewhat haphazardly, using both letters and numbers. But Charles Schuchert counted them carefully, and gave the number of comparisons to be 51. This was an impressive number, far more than Wegener's first 6. Wegener's hypothesis had predicted that such comparisons could be found, and here they were. This type of successful prediction is the proof of a good hypothesis. Nevertheless, Schuchert would not grant that the comparisons were impressive, and tried instead to find errors. He had been using the land-bridge theory for decades, and he would continue to do so:

Schuchert 1928b, p. 274

The writer has long been studying the faunal and floral assemblages of western Gondwana, and for about thirty years he has been impressed with the general correctness of the reconstruction of this continent as first presented by Neumayr and given wide acceptance by Suess. According to this interpretation, the two continents have not moved geographically in greater amounts than are demanded to compensate for the folding and faulting found in the orogenic or mountain areas; and into early Cretaceous times they were united by a wide land bridge...

Du Toit's evidence was extensive, and he had presented it reasonably. Schuchert faulted him more for his enthusiasm than for his geology:

Schuchert 1928b, p. 271-272.

The geological resemblances between Africa and South America, Du Toit tells us, are "nothing less than extraordinary," implicating "vast territories," and embracing "times ranging from pre-Devonian almost to the Tertiary . . . . and are of a stratigraphical, lithological, paläontological, tectonic, volcanic, and climatic nature" (p. 109). In Chapter VII he synopsizes the previous discussion of 108 pages and brings out once more these similarities and analogies, along with some dissimilarities, grouping them in eight paragraphs, according to latitude and otherwise; they cover six pages. In detail they amount to fifty-one counts, a number which he believes must convince anyone in favor of his view that Africa and South America were at least closely adjacent, if not actually united, to one another up into early Mesozoic time. The presentation is overwhelming, not because of the correctness of all or even of most of his interpretations, but because of his unbounded enthusiasm and his ability at special pleading. To meet these fifty-one counts with detailed counter arguments would take even more space than the length of his book and in the end quite a number of them would remain as actually existing similarities and analogies. Such, however, have long been known and explained by paleogeographers. Granting this, does it follow from the various favorable counts that South America once lay adjacent to Africa? Or, as Du Toit states it, "from which it will be conceded that the evidence so far would distinctly appear to favor the displacement hypothesis rather than orthodox ideas?" However, his enthusiasm has not completely blinded him, since he adds: "It is nevertheless highly desirable that more impartially minded persons should criticize the data here given and judge whether those amazing resemblances have been correctly interpreted or whether some more rational explanation for them could not be formulated (p. 115.)"

Du Toit had indeed become an enthusiastic proponent of mobilism. Note in this last sentence Du Toit used the word "amazing" to describe the resemblances between South America and Africa. Schuchert noted this, above, and then mockingly used the word "amazing" to ridicule Du Toit for apparent mistakes. Schuchert's eighth paragraph begins: "In the matter of Du Toit's correlations, we find on page 56 the following amazing statement..." Schuchert's ninth paragraph begins similarly: "On pages 77-78 we have an equally amazing statement where he is treating the Glossopteris flora..." Most authors would not be allowed to mock another author in print. But Schuchert was not just an author; he was an editor of the journal where this book review was published.

Schuchert's scalding review struck two blows against mobilism. It discouraged American geologists from reading or believing Du Toit's book. And perhaps more importantly, it discredited Reginald Daly's judgment regarding mobilism. Daly had helped Du Toit obtain a research grant from the Carnegie Institution of Washington. Having seen this review, geologists must have felt that American money had been wasted in sending Du Toit to South America. Daly had already published *Our Mobile Earth*. But after this review, Daly abandoned those suggestions of Earth mobility. He wrote more books on the Earth (1933, 1938, 1940), but in them, the Earth seemed anything but mobile. In contrast to the word *Mobile* in the title, he used the words *Architecture* (1938), *Strength* and *Structure* (1940). And he barely referred to his first book or his earlier ideas of continental mobility. However, he continued to present mantle convection and continental drift in a positive light to his graduate students at Harvard (Oreskes, 1999, p. 228-230)

7. *Theory of Continental Drift* Labeled and Libeled

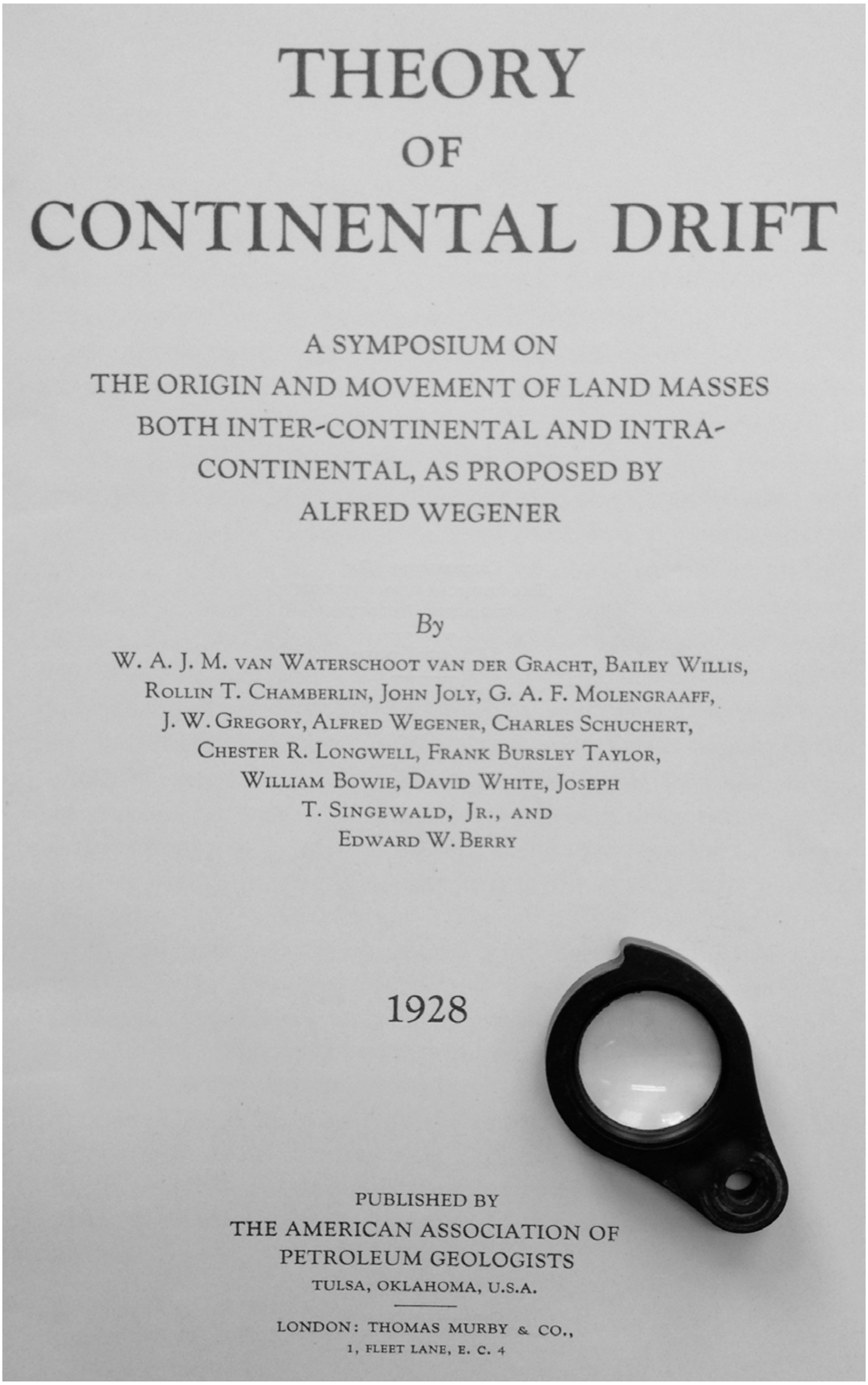
The decisive North American judgment of the Wegener hypothesis came as a publication in 1928 with the title: *Theory of Continental Drift. A symposium on the origin and movement of land masses both inter-continental and intra-continental, as proposed by Alfred Wegener.*

The convener of the symposium was Willem A. J. M. van Waterschoot Van der Gracht (1873-1943), a Dutch geologist who understood the strength of mobilism. Van der Gracht had been in America for nine years and was Vice President of the Marathon Oil Company. His purpose in convening the symposium was to encourage his North American colleagues to try working with Wegener's hypothesis. Although Van der Gracht was a mobilist, he strove to represent both sides of this scientific controversy. In organizing this meeting, Van der Gracht wrote a letter encouraging Charles Schuchert to attend, since Schuchert was known to be strongly against the displacement theory (this letter was mentioned in Oreskes 1999, p. 193.)

The symposium itself was very brief. It was held in New York on the evening of November 15, 1926. It was not an international symposium at all, but was a special activity that was added on to a general geological meeting. The debate was very one-sided. It seems that Van der Gracht was basically alone in supporting Wegener, although I have found no record of who attended the meeting or what they said. Wegener was not at the meeting. He never visited North America. But one historical account, repeated by others, embellished the story by writting that Wegener sat at the meeting quietly smoking his pipe.

The papers of the symposium were to be published by the American Association of Petroleum Geologists (AAPG). In an attempt to give drift theory more positive exposure in the published volume, Van der Gracht solicited written contributions from mobilists who had not attended the meeting. European geologists John W. Gregory (1864-1932), John Joly (1857-1933), and G.A.F. Molengraaff submitted papers, but were probably not present.

The volume was published in March 1928, 15 months after the symposium. The title of the book firmly established the phrase continental drift, rather than continental displacement. The North American authors were so critical of this theory that readers now considered it dead. The title page could have served as its tombstone (Fig. 48):



THEORY
OF
CONTINENTAL DRIFT

A SYMPOSIUM ON
THE ORIGIN AND MOVEMENT OF LAND MASSES
BOTH INTER-CONTINENTAL AND INTRA-
CONTINENTAL, AS PROPOSED BY
ALFRED WEGENER

By
W. A. J. M. van Waterschoot van der Gracht, Bailey Willis,
Rollin T. Chamberlin, John Joly, G. A. F. Molengraaff,
J. W. Gregory, Alfred Wegener, Charles Schuchert,
Chester R. Longwell, Frank Bursley Taylor,
William Bowie, David White, Joseph
T. Singewald, Jr., and
Edward W. Berry

1928

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LONDON: THOMAS MURBY & CO.,
1, FLEET LANE, E. C. 4

Figure 48. The title page of the AAPG book that firmly established the term “continental drift.” In the opinions of most North American geologists, it also laid this theory to rest. From Van der Gracht (1928).

Van der Gracht put in a great effort in support of Wegener and his mobilism. Here is his opening statement:

Van der Gracht 1928, p. 3-4.

Before starting on what I have to say as an introduction to what I hope is going to be an animated and fruitful discussion of the general problem of continental drift, I want to make a few preliminary statements.

The problem of continental drift has raised considerable and spirited discussion in geological circles. Many authorities, entitled to all respect, advocate it; others are undecided but favorably inclined; still others do not favor it, and some are violently opposed. The whole controversy reminds me vividly of the discussions during my student days on the problem of sheet-overthrusting in the Alps. As now in the discussion of continental drift, so there was then much opposition, in which no less an authority than Albert Heim took a leading part before his conversion to the new idea. Its mere possibility was then as firmly denied, as is now the possibility of continental drift. The facts have since proved beyond any doubt that these sheets exist, not only in the Alps, but universally. Still their detailed mechanism, their “possibility,” remains almost as much a riddle as it was then. The possibility has only been demonstrated by fact, not explained.

I personally approach the problem of this evening with an entirely open mind. I am strongly impressed by the concurrence of major facts, which point favorably to the existence of some kind of continental drift on a large scale, but I also admit many difficulties of detail, and am fully aware of the grave difficulties which will have to be overcome in *explaining* how such drift could be “engineered,” and what are the forces which cause it. Mindful of the Alps, Emile Argand even refuses to explain, although he warmly advocates, drift. This possibly goes too far; we should at least try to see whether or not it is utterly impossible,

It will certainly not be possible to settle this problem, or even to discuss it thoroughly in one single evening. But I want to lead this discussion with the purpose, if possible, of bringing the two camps a little closer together. Do not let us lose ourselves in minor details; we shall not settle them for generations. Let us try to agree a little better on main principles. Is drift to any considerable degree possible? Are there evidences of it? Does it give a plausible answer to the many problems which, so far, have never been adequately explained? If we could agree a little better, and would no longer refuse even to consider the hypothesis of inter-continental drift, but would take it seriously as a possibility, we could co-operate much better in trying to solve the problem of the evolution of the face of the earth.

ABSTRACTS OF ALL 15 PAPERS IN THE AAPG SYMPOSIUM VOLUME (1928)

For North American geologists, this book was the definitive document concerning mobilism. To give a balanced overview of it, I reproduce the abstracts of all the contributions. The first paper, by Van der Gracht, was by far the longest, probably because the strength of mobilism would otherwise be underrepresented in the volume. Also his abstract was long, including even a Table of Contents:

W. A. J. M. van Waterschoot Van der Gracht, Marland Oil Co. (USA), p. 1-75.
The problem of continental drift

A brief outline is given of the present knowledge of the constitution of the interior of the earth and of the physical states of matter, which we have to consider. The insufficiency of the contraction theory to explain the surficial history of the earth, and particularly the problem of the major mountain chains, is exposed. The continental drift theories of F. B. Taylor, Alfred Wegener, and R. A. Daly are outlined in their latest aspect. Wegener's views have been most widely published and worked out in the greatest detail. A discussion follows of the main arguments which have been proposed in support of the drift theories, as well as of some of the principal objections. The most serious of the latter is the lack of sufficient explanation for the mechanism of a drift of the magnitude of the acid continental crust ("sial") over a *solid*, basic substratum ("sima").

A discussion of the theory presented by John Joly (1923-1925), reaches the conclusion of a periodicity of fluidity and solidification of the basic (sima) substratum, caused by the generation of heat through radioactive changes in the atoms. This heat accumulates faster than it can dissipate into space, and through a period roughly estimated at 30 million ears, will cause the sima sphere to become fluid under the outer sial crust. This should greatly increase the forces which tend to cause a locally differentiated westward drift of the outer crust and their effect, actual drift. In fact, if Joly's thermal theory is right, such drift seems the only means by which accumulated heat can sufficiently be relieved and dissipated into space. It is calculated that, given sufficient drift, a period of 5 million years would suffice to re-solidify the basic substratum.

These alternate periods of fusion and re-solidification are causally connected with the main world-wide diastrophisms, Joly's "revolutions."

If Joly's reasoning is correct, a general westward drift becomes a necessity, and a locally differentiated drift most probable. This would support the drift theories and eliminate the worst objection to them. There are, however, objections to some of Joly's views.

This is worked out and added to in greater detail; the author adds further conclusions of his won; the causes of *relatively differentiated* drift are discussed, not only as between the major continents, but also intra-continental, more local drift, its relations to isostasy, and the general continental deformation it must cause. Shifting of the earth's poles need only be relative, and does not necessarily imply major changes in the location of the earth's axis of rotation in space, thereby eliminating another objection against Köppen-Wegener's plausible theory of geological climates. There is no necessity for dislocating the earth's axis.

The position of the author is as follows: he considers the theory of inter-continental drift worthy of very serious consideration and gradually has come to regard it ever more favorably. It offers a plausible explanation for several problems, never satisfactorily explained before. The results of further thought and geological research seem increasingly to support this theory, rather than to oppose it. Serious objections become ever more weakened by further research. In this spirit the theory is offered for serious discussion here in America, where, so far, it has found but scant support. The author realizes that no such theory is ever a finished product or perfect; he approaches it with an open mind and will welcome anyone's argument in order to come nearer the truth. He offers his own thoughts and additions in the same spirit.

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CONCLUSION.

Bailey Willis, Stanford University (USA) p. 76-82.

Continental Drift

After considering the theory of continental drift with avowed impartiality, the author concludes by means of geophysical, geological and paleontological reasoning that it should be rejected, because the original suggestion of the idea sprang from a similarity of form (coarse lines of Africa and South America) which in itself constitutes no demonstration, because such a drift would have destroyed the similarity by faulting, and because other contradictions destroy the necessary consequences of the hypothesis.

Rollin T. Chamberlin, University of Chicago (USA), p. 83-87.

Some of the Objections to Wegener's Theory

A brief synopsis is given of the objections to the Wegener drift theory. It is not a general theory of earth behavior; it is a description of only one breaking up of a land mass, which does not satisfactorily fit the facts as now known and does not fit in the generally accepted record of geological time. The framework of the present continents was developed in pre-Cambrian time. Geological evidence does not show that a great continental mass split apart in comparatively late time. Geophysical evidence does not support the causes assigned to the drift displacement. The author indicates that the planetesimal hypothesis sufficiently explains the known crustal shortening of the earth. The planetesimal hypothesis is an integral part of comprehensive geological philosophy, but Wegener's hypothesis is not.

John Joly, Trinity College (Ireland), p. 88-89.

Continental Movement

The thought is expressed that continental movement is not improbable during periods of fluid movement. A force sufficient to wrinkle the western side of North and South America would be competent to shift these continents as a whole.

G. A. F. Molengraaff, Delft Institute of Technology (Holland), p. 90-92.

Wegener's Continental Drift

The author stresses the inadequacy of the hypothesis in that it postulates primarily a westward drift. He points to the Mid-Atlantic Ridge as the old line of separation from which America drifted west and Africa drifted *east*. A similar line of separation or great rift valley may now be forming in East Africa. The author is rather favorably inclined toward drift theory, but he would not confine the movement to a westerly direction.

J. W. Gregory, Glasgow University (Scotland), p. 93-96.

Wegener's Hypothesis

The Wegener hypothesis would explain the distribution of land and water by lateral drift of the continents. The author believes the present distribution is more largely the result of vertical movements of the crust. He does not positively object to the hypothesis but points our that vertical movement or differential rising and subsiding of crustal blocks due to a shrinking earth would have the same effect as lateral drift.

Alfred Wegener, University of Graz (Austria), p. 97-103.

Two Notes Concerning My Theory of Continental Drift

The author of the drift theory discusses geological climate in the United States and points out that one of the objections to his conception of land distribution, namely, the assertion of the presence of glacial deposits within his tropical belts, is not justifiable inasmuch as it is not positively known that so-called glacial deposits were really glacial. An advantage the drift theory has over all other geological theories is its susceptibility to verification by astronomical observations. Exact research is now being conducted that will throw light on the theory.

Charles Schuchert, Yale University (USA), p. 104-144.

The Hypothesis of Continental Displacement

The following paper shows that Wegener's attempt to fit the Americas against Euro-Africa leaves discrepancies of as much as 1,500 miles; that there is no fitting at all in the Central American region; that when Newfoundland is united with Ireland and the easternmost cape of Brazil fitted into the African Bight of Biafra, Central America is parted from South America by 1,200 miles, and Alaska from Siberia by 600 miles, leaving in the latter instance a deep ocean that is fatal to all intermigration of marine and land life between these continents.

It next explains that the tectonic structure and the faunal assemblages on either side of the Atlantic fit badly, that we have here only similarities and not identities, and that the faunas do not have more than 5 per cent of species in common instead of the 50-75 per cent called for on the basis of Pangäa. The detailed historical geology and stratigraphy of Newfoundland and Ireland, contrasted in adjacent columns, show that there are here no exact identities and few similarities.

Finally, the writer discusses the small residuum of the Wegener hypothesis that has been becoming more and more apparent during the past fifteen years, to all students of geosynclines and mountain structures, namely, that the continents appear to have moved horizontally and differently throughout geological time, but how much and in what directions are problems for the future. He also points out briefly how more harmony may be hoped for between geologists, paleontologists, and geophysicists working along these lines.

Chester R. Longwell, Yale University (USA), p. 145-157.

Some Physical Tests of the Displacement Hypothesis

Evidence so far advanced by advocates of the hypothesis is by no means convincing. Geophysicists recognize only tiny forces acting horizontally on the continents. The assumption that the sima is devoid of strength toward secular forces takes no account of mountain structure. Apparent coincidence of widely separated coast lines is probably accidental, as may be seen by comparison of Australia and the Arabian Sea. Petrographers, as well as stratigraphers and paleontologists, find that Wegener's geological "controls" are not well established.

Frank Bursley Taylor, (USA), p. 158-177.

Sliding Continents and Tidal and Rotational Forces

In the first part of the paper, the author discusses the manner of formation of arcuate mountain ranges. Many leading geologists are advocating a wholly speculative hypothesis--the sinking of great sub-oceanic segments, with heavy landward underthrust, which cause folding and uplifting along the continental margin. With this mechanism they strive to account for the making of the circum-Pacific mountain ranges. They take no heed of Suess' explanation, or of the facts which he advanced in support of his idea. The author presents his views with a careful discussion of the similitude of continental ice-sheets and continental crustal-sheets. In the latter part of this paper, he discusses the interpretation of the principle features produced in Tertiary diastrophism and applies fundamental principles of earth sciences, and astronomy and cosmogony as well, to Asia and all the other continents as units of crustal movement.

William Bowie, U.S. Coast and Geodetic Survey (USA), p. 178-186.

Comments on the Wegener Hypothesis

Wegener's hypothesis is discussed by means of geophysical objections. Although it does not actually oppose the principle of isostasy, it does, nevertheless, possess features not in harmony with the idea that the earth's crust in all parts is weak enough to be maintained in isostatic equilibrium by the constant action of gravity. The author raises questions about the hypothesis which suggest that it is not fully in accord with earth facts. If the earth material under the oceans is devoid of strength, why do such violent earthquakes occur in those regions, and why does the ocean floor maintain such pronounced relief? Why does Wegener collect so many of his floating sial masses in the northern hemisphere and why have some of the continents evidently moved away from the equator instead of toward it? Doubt is expressed about the shifting of the poles to the great extent postulated by Wegener.

David White, National Research Council (USA), p. 187-188.

Discussion of Floating Continents

The Köppen-Wegener maps showing continental aggregates and climatic data throughout geological time, though clever, are open to serious criticism. If the continents could drift apart in geologically late time why did they not break up in earlier eras during greater diastrophic revolutions? Geologists should not forget the principle of isostasy.

Joseph T. Singewald, Jr., Johns Hopkins University (USA), p. 189-193.

Discussion of Wegener Theory

The two fundamental premises on which Wegener's displacement theory is based, namely, (1) the flotation of continents of sial on an underlying viscous layer of sima, and (2) the displacement of the continental sial by tangential forces, are both conceded by geologists. The disagreement is largely in the application of these premises to the solution of earth problems. Wegener makes a strong case on circumstantial evidence. But his reasoning is not wholly convincing. Despite objections raised to the theory, it possesses a great degree of probability, and is supported by considerable evidence. The difficulties between Wegener and his opponents are largely because of disagreement as to the facts observed.

Edward W. Berry, Johns Hopkins University (USA), p. 194-196.

Comments on the Wegener Hypothesis

The author objects to the Wegener hypothesis because (1) the method of presentation is not scientific, (2) the facts of geophysics do not support it, (3) it fails in explanation of geological climates, and (4) paleontologically it raises more distributional problems than it solves.

W. A. J. M. van Waterschoot Van der Gracht, Marland Oil Co. (USA), p. 197-226.

Remarks Regarding the Papers Offered by the Other Contributors to the Symposium

The criticism which is voiced in this symposium is largely directed against Wegener's conception of continental drift. The arguments can be divided principally into geophysical arguments about the possibilities and explanation of drift, and geological and paleontological arguments against the facts cited in support of Wegener's drift and the consequences which a drift, such as the exponents of his theory sponsor, would be expected to have.

An outstanding feature of this symposium is that the majority of those contributors who attack Wegener's theory express themselves as not fundamentally opposed to the conception of such a thing as intra- and inter-continental drift, even on a considerable scale. This is a very important step forward. As I said in the concluding remarks of my own paper, it will take generations before this complex problem will be plausibly solved, if ever. But to approach the truth, co-operation is needed. It is a most gratifying feature of this symposium, in which world-wide talent takes part, that such co-operation seems brought very much nearer and that there is little tendency summarily to set aside the idea of major continental drift as something visionary, unscientific, and impossible. There still are wide differences of opinion, but our minds have become more open: there is less eagerness to differ than to attempt to work out something on which we can agree; there is more constructive co-operation in approaching truth.

These abstracts give an objective summary of the papers presented in the symposium. Judging from the writing style and content, I think that Van der Gracht wrote the abstracts for both Wegener and Molengraaff. Probably they had not submitted abstracts with their papers, and as editor, Van der Gracht wrote for them. Van der

Gracht made every effort to be complete and balanced. Now, in my less-balanced but necessary exposé, we can look at important parts of the papers themselves.

EXCERPTS AND COMMENTS ON THE VARIOUS PAPERS

W. A. J. M. Van Waterschoot Van der Gracht was strongly in support of using continental drift as a working hypothesis. He had read the book by Köppen and Wegener, with its valuable map-data on ancient climates and that showed paleoclimate zones. That book was not available in English, so Van der Gracht reproduced the ten essential world maps in his own 75-page paper. He even had English figure-texts and legends prepared for them (see their Fig. 1, p. 107.) Those ten figures were not in any way related to Van der Gracht's text; he simply distributed them evenly throughout it. His addition of these extraneous figures is the first clue that there were no strict limitations on the number of pages or illustrations in this symposium volume.

Van der Gracht had remarkable insight into the formation of older mountain ranges. He recognized the existence of an earlier ocean, an "old Paleozoic Atlantic," that had opened and closed to form the Caledonides. Geologists now call this the Iapetus Ocean. It existed from Cambrian to Silurian time, about where the North Atlantic now lies. Sediments were deposited along both its continental margins, and then when it closed by subduction and continental collision, horizontal compression of these sediments resulted in the formation of the Caledonian and Appalachian fold-belts. These are now found on both sides of the modern Atlantic Ocean. Van der Gracht understood all this. He also correctly indicated that still earlier activity had occurred along this same line. Here he was thinking of a Precambrian orogen, known as the Grenvillian in North America, and the Sveconorwegian in Europe. Today we call such a cycle of opening and closing of an ocean basin a "Wilson-cycle."

Van der Gracht 1928, p. 204.

I mentioned before that there are indications that, in very early Paleozoic time, there may have existed a considerable rift, which occupied, more or less, the position of the present Atlantic. This geosynclinal area might quite possibly have been of much greater importance than a mere epi-continental geosyncline. It may have been some kind of old Paleozoic Atlantic, which, as I pointed out before, was largely closed by the Caledonian diastrophism, the traces of which we now find on either side of the present Atlantic rift, including the African Saharide chains. This old Paleozoic line of weakness (which may have been preceded by still earlier lines) may have had much to do with the process which again tore open the present Atlantic in the Mesozoic. It is quite possible that this rift was a very old one, and in consequence it may be quite admissible, as set forth by Dr. Chamberlin, to explain the pre-Cretaceous mountain chains on each side of the Atlantic by this old line of weakness. This would, of course, bring these mountains in close relation to the present Atlantic coast lines.

Van der Gracht had been a colleague of G. A. F. Molengraaff in the Netherlands. As previously discussed (p. 80), Molengraaff had published an article in 1916 in which he correctly interpreted the geologic significance of the Mid-Atlantic Ridge. When

Molengraaff was invited to submit a paper for this AAPG symposium volume, he used it as an opportunity to repeat these ideas about the Mid-Atlantic Ridge, to make North American geologists aware of them:

Molengraaff 1928, p. 91.

To my mind the Mid-Atlantic Ridge is nothing but the cicatrix of the former rent or fracture, along which the disruption of the American continent from the European-African continent took place. America drifted from the rent on which the volcanic Mid-Atlantic Ridge has been built up in a westerly direction, but Africa drifted toward its present position in an easterly direction. It has since then been the site of volcanic activity, and this activity is not yet completely exhausted.

If so, this mid-Atlantic fracture is strictly comparable to the great rift-valley (Ost-Afrikanische graben) in East Africa, along which the disruption of the more westerly greater portion of Africa from the smaller easterly portion appears to be now in active preparation. In this great rift-valley and its offshoots now the great East African lakes are found, and, partly along it, volcanic activity is well developed.

If this supposition is correct one must expect to find the Mid-Atlantic Ridge to be composed entirely of effusive volcanic material of relatively high specific gravity.

The latest measurements of gravity, rather recently made in the Atlantic Ocean above the Mid-Atlantic Ridge on board a submarine by Vening Meinesz, have proved that the Mid-Atlantic Ridge shows a *positive* anomaly of gravity (*excess* of gravity), which remarkable fact gives support to my suggestion on the nature and origin of this ridge.

In a paper published in the year 1916 I have summarized, on pages 625 and 626, this suggestion about the nature of the Mid-Atlantic Ridge as follows:

> Perhaps we may see in this remarkable Mid-Atlantic Ridge the final result of volcanic activity along an enormous fracture of the same extent, where from numerous fissures and vent volcanic material was discharged, thus a volcanic mountain-chain and cones being formed, which nowadays subside through yielding under the influence of gravity and nearly all have sunk back to a level approaching the average level of the deep submarine ridge. Here and there a few islands, where volcanic activity lasted longer or has existed to this day, still rise above the sea, and others (of which naturally only a few have been discovered accidentally by soundings) still rise to different heights above the average level of the ridge but no longer attain the surface of the sea. Among these latter we mention three submarine mountains which near the western part of the Azores rise from the bottom of the ocean, which has there a depth of about 3,000 meters, to elevations of 146, 128, and 88 meters, respectively, below sea-level. The cause for the extrusion of such enormous masses of volcanic material might perhaps be sought in the disruption of the American continent from the European-African one with which it formerly cohered. This disruption was assumed by Pickering and Taylor and a plea for it is again brought forth by Wegener on page 68 of his paper quoted before. (Edition 1915 of Wegener's well-known book.) On this supposition the Mid-Atlantic Ridge would in my opinion indicate the place where the first fissure occurred and the sima was first laid bare. From this it would follow logically that the ridge itself must consist entirely of sima and not of sial, as Wegener assumes on page 69.

Molengraaff’s suggestion in 1916 seemed to have had no effect, and neither did this restatement of it. If Molengraaff’s paper convinced any fixists of this new mechanism for mobilism, I have not found publications mentioning it.

Recall that Wegener began his third book, the one that had been translated to English, by telling that he first got the idea of continental displacement from the apparent fit of the continents across the Atlantic. Many North American geologists had read and understood that part of his book. Charles Schuchert and Chester R. Longwell (1887-1975), two professors at Yale, strongly opposed Wegener’s infatuation with this apparent fit. Longwell tried to cast Wegener as a simpleton, impressed with superficial patterns:

Longwell 1928, p. 152.

I am quite aware that this problem has passed the stage of the jig-saw puzzle. Nevertheless it is a fact that the hypothesis was first suggested by the apparent correspondence in coast lines on opposite sides of the Atlantic, and I am convinced that this sort of argument still has very great weight with advocates of the idea. In fact, it is not improbable that gazing at the map of South America and Africa has the effect of hypnotizing the student. The coast lines appear to be such exact counterparts, even in detail – Wegener must be right!”

Following this statement, Longwell used three printed pages, including two large and vacuous map figures (Fig. 49), to show that Australia could be forced-fit in the Arabian Sea between Africa and India.

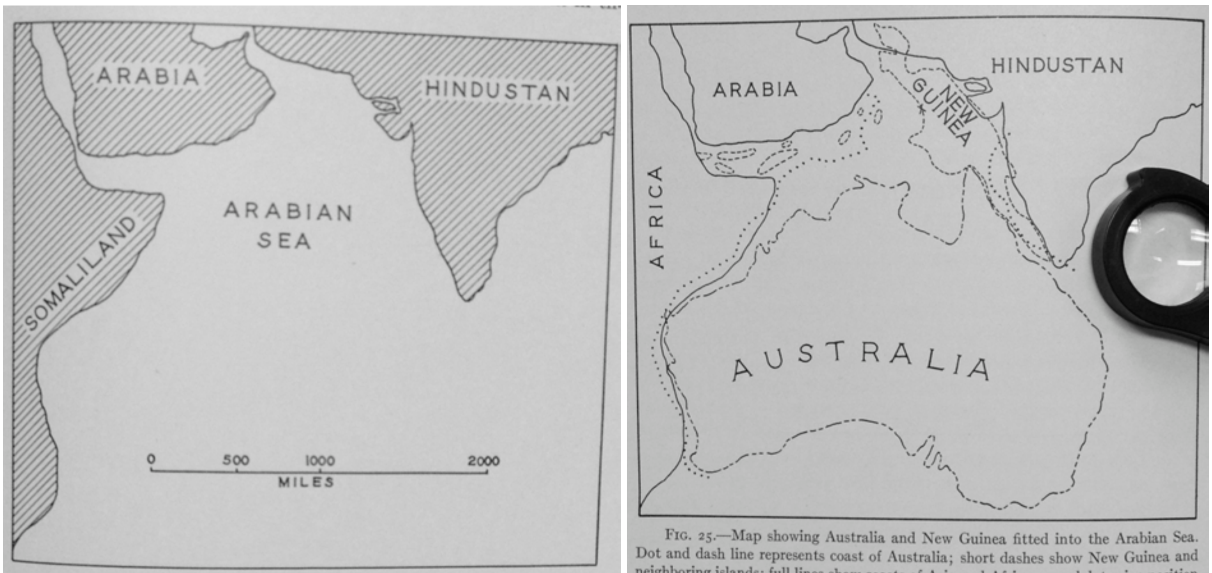


FIG. 24.—Map of the Arabian Sea and bordering lands. Notice the large dimensions. Traced from Andree’s *Handatlas*.

FIG. 25.—Map showing Australia and New Guinea fitted into the Arabian Sea. Dot and dash line represents coast of Australia; short dashes show New Guinea and neighboring islands; full lines show coasts of Asia and Africa; round dots give position of 100-fathom line off Australia and the Australian platform. New Guinea turned 30° clockwise about the point x. All outlines traced from Andree’s *Handatlas*.

Figure 49. The Longwell forced-fit. From Longwell 1928.

There is no geological matching of the African-Australian-Indian landmasses once they are fit together:

Longwell 1928, p. 155.

Of course Wegener would never agree to the assumptions here made. They do not harmonize with his conception of Pangaea, as may be seen by reference to his maps. Certainly there appears to be no evidence, other than the suggestive similarity in coastal configuration, that Australia ever occupied the position of the Arabian Sea. However, this case is worth some study, in connection with the better known case of South America and Africa, in order to convince ourselves that apparent coincidence of widely separated coast lines is probably accidental wherever found and should not influence anyone unduly in considering the displacement hypothesis.

Schuchert had another way of poking fun at Wegener’s fit:

Schuchert 1928a, p. 113.

The striking similarity of the coast line between Africa and Brazil has long vexed geologists and geographers, and a friend of the writer recently remarked that it must have been “made by Satan” for that very purpose.

Here Schuchert seems to be suggesting, or rather he is letting his anonymous friend suggest, that Wegener was inadvertently doing the work of the devil. I was very curious as to whom Schuchert’s friend might be. It am now convinced that it was Chester Longwell; Longwell wrote the same remark in another symposium volume thirty years later (see p. 242.)

Bailey Willis also scoffed at the significance of matching coastlines:

Willis 1928, p. 77.

It is obvious, if one inspects a map of the South Atlantic Ocean, that the eastern coast of South America closely resembles the western coast of Africa. The resemblance is such as might have resulted if the two continents had once formed a single continental mass and had separated from each other along a rift without change of outline.

Wegener observed this resemblance and founded thereon the theory of continental drift. The fact that similarity of form is not a firm basis of reasoning for any one particular explanation of the likeness, he passed over. Gilbert once studied the details of the moon’s face and experimentally produced similar craters by dropping balls into mud. He got exact resemblances of form, but did not regard the theory of impact craters as demonstrated for the very reason that form is not conclusive proof of its own genesis. But let that pass.

This comment on the appearance of the Moon may have been an oblique reference to a paper that Wegener had published in 1921 called *Die Entstehung der Mondkrater*. There he had dropped balls of powder onto a tray of deep smooth powder, in order to create impact craters. He was testing alternative hypotheses: whether the Moon’s craters were formed by volcanic explosions or by meteorite impacts. Wegener was familiar with Gilbert’s work on this, but it is surprising if Willis was also familiar with Wegener’s.

Willis made demands on the drift theory that were impossible to satisfy. Here was a catch-22: If the continents had indeed drifted apart, their coastlines would have been deformed and would no longer resemble each other. Therefore, the fact that the coastlines do resemble each other, proves that they have not drifted apart:

Willis 1928, p. 80.

We may consider this from another angle. The original suggestion of continental drift sprang from the similarity of form between the coasts of Africa and South America. We have assumed that similarity to be as perfect as the author of the theory would postulate that it is. Its very perfection renders it impossible that there should have been normal faulting along either coast. Neither Africa nor South America can have lost any recognizable sections by submergence due to faulting and yet have preserved the identity of outline upon which the theory rests. But if they have moved apart they must have been subjected to stresses competent to produce losses by faulting on a large scale.

If there has been movement, faulting must have destroyed the similarity of outline. If the similarity of outline be a fact, then there has been no significant normal faulting and there can have been no movement of one continent away from the other.

Willis was not a fool, as one could make him seem by ending this citation here. In fact he had an important point, but he was presenting it very indirectly. It took him a few more paragraphs to properly explain it. Wegener had claimed that the folded mountain ranges on the Pacific side of the Americas were the result of compressional deformation, as the continent plowed through the ocean. Willis was arguing that if compressional deformation formed at the leading (western) edge of these continents, then extensional deformation should form at the trailing (eastern) edge. Willis was quite right in this judgement. He was putting a new twist on the objection to continents plowing through resisting oceanic crust.

Schuchert had carefully considered every piece of geological and paleontological evidence that Wegener and Du Toit had presented (but he had not yet published the review of Du Toit’s book). He saw the similarities between rifted continents, but was not satisfied. He described them as “slight”:

Schuchert 1928a, p. 118.

But under Wegener’s hypothesis these slight similarities should be striking identities, and many of the marine faunas, for instance, should have, not 5 per cent of identical species, as is actually the case, but between 50 and 75 percent, which is not true at all.

Schuchert tested the relationships claimed by Wegener and Du Toit by checking maps and the literature. He presented the evidence in table form. He seems to admit that many of the similarities between Africa and South America were not simply slight, but also “striking”:

Schuchert 1928a, p. 120.

Now let us see what these relations are, basing a synopsis of them upon Du Toit, Krenkel, and Keidel (Table I). From this table we see that about all that is strikingly harmonious in the two continents is the orogeny at the close of the Proterozoic, the Lower Devonian (Bokkeveld) faunas, the Lower Permian tillites, and the Glossopteris floras; in Permian time, Mesosaurus and

Noteosaurus; in late Triassic time, Eurthrosuchus and Scaphonyx-like reptiles; and finally, in latest Triassic time, the plateau lavas. We gladly admit that these are striking similarities or identities, but after all they furnish slender evidence on which to base so important a conclusion as that Africa and Argentina were united to one another until Cretaceous time. Against this view are many more and greater dissimilarities, none of which is more striking than the almost total absence of the horde of African Permian reptiles and amphibia and the African Triassic dinosaurs in all of South America.

Schuchert's dismissal of striking similarities, and emphasis of dissimilarities, including absence of certain fossils, was not typical for him. All paleontologists know that the absence of fossils is a type of negative evidence that should not be used to make strong conclusions. It is commonly said, especially among paleontologists, that: "absence of evidence is not evidence of absence."

If fossils are not found, it does not mean that the species were not present. It may have been that conditions were unfavorable for fossilization, or the fossil-bearing rocks were removed by erosion, or the fossils were not yet discovered. In the paragraph above, Schuchert admitted that there were "striking similarities or identities" but he based his conclusion on evidence that was negative: he claimed that an "almost total absence" of certain reptiles and amphibians in South America was more striking. His argument was not only based on absence of evidence; it was based on "almost" absence.

If lines of print could be read from one piece of torn newspaper to another, as Wegener had put it, Schuchert argued that the pieces were never joined because other lines cannot be read. This was not Schuchert's usual way of drawing geological conclusions. He had a reputation of taking complex data and making sense of it. He generally used positive evidence to make sound scientific interpretations. Yet in the case of continental drift, he was completely and consistently negative. North American geologists apparently did not see this, but continued to fully rely on his highly respected scientific judgment.

European geologists were more willing to accept Wegener's evidence and his hypothesis. In Van der Gracht's opinion, this was because European geologists had already been convinced of the global scale of horizontal displacement involved in the making of the Alps. Alpine geologists, such as Argand, could see that African crust had collided with European crust. Schuchert and Longwell had other explanations for why Wegener's hypothesis was more acceptable to Europeans. Schuchert felt that American geologists were somehow more advanced or had higher standards:

Schuchert 1928a, p. 140.

The battle over the theory of the permanency of the earth's greater features introduced by James D. Dana has been fought and won by Americans long ago. In Europe, however, this battle is not yet fought to a conclusion, since there are leading geologists who still follow Lyell and believe in the impermanence of the continents and oceans, and others who do not hesitate to push the earth's pole anywhere in order to explain single floral or faunal peculiarities.

Longwell felt that the AAPG was being generous in supporting this discussion at all. His article begins with these two paragraphs:

Longwell 1928, p. 145-146.

The mere fact that a group of geologists has undertaken a serious discussion of possible continental drifting indicates a change in viewpoint within comparatively few years. Physical geology of the old rigid school had no place for any such suggestions as those forming the basis of the displacement hypothesis. The Taylor-Wegener doctrine, on its part, shows little respect for time-honored ideas backed by weighty authority. Perhaps the very completeness of this iconoclasm, this rebellion against the established order, has served to gain for the new hypothesis a place in the sun. Its daring and spectacular character appeals to the imagination both of the layman and the scientist.

But an idea that concerns so closely the most fundamental principles of our science must have a sounder basis than imaginative appeal. Physical geologists are attracted by the displacement hypothesis, in its general form, chiefly because it promises a solution of certain troublesome enigmas. How can we satisfy the demand of historical geologists for former land connections and extensive borderlands where none exist today? Why are the Atlantic and Pacific coasts so strikingly different? What is the meaning of rifts, thrusts, folds, and other conspicuous features of crustal deformation? Why and how are mountains formed? If Wegener or anyone else can throw new light on these baffling problems, he is entitled to a hearing. However, certain demands are made of this new and romantic speculation before it is admitted into the respectable circle of geological theories. It must meet the test of established scientific principles, and it must not create more problems than it pretends to solve. Very naturally, we insist on testing this hypothesis with exceptional severity; for its acceptance would necessitate the discarding of theories held so long that they have become almost an integral part of our science.

Schuchert always had appropriate information and quotations to support his opinions. Usually these were constructive, but not in the case of continental drift. In his paper in this volume, he came with several harsh opinions made by earlier geologists. By quoting others, he lent the criticisms credibility, without being fully responsible for their inaccuracies. Here he quoted paleontologist Edward Berry:

Schuchert 1928a, p. 140.

In regard to the geological climates as set forth by Köppen and Wegener, Berry says:

Neither has the slightest idea of the bearing of fossil faunas or floras on the problems which they set out to explain, and therefore wherever their conclusions lead they explain something which never existed.

Berry had published that opinion in 1927 in the *American Journal of Science*, where Schuchert was an editor. Berry had also criticized Wegener in 1922, and this too was quoted by Schuchert:

Schuchert 1928a, p. 140.

Berry says in 1922:

I can see no record of such a former union [of South America and Africa] in anything that we know of the stratigraphy, structure, faunas or floras . . . I much prefer the older hypothesis of land-bridges and subsidence.

This quotation from 1922 should have been considered obsolete in 1928 when Schuchert used it. Even though Berry could see no record of a former union in 1922, more recent books by Du Toit (1927) and by Köppen and Wegener (1924) had provided much additional documentation of the former union.

As noted earlier, Philip Lake had been extremely critical of Wegener's book. Schuchert quoted him to make the harshest comments.

Schuchert 1928a, p. 139.

And Lake (1922), who has gone into it at length, states: "Whatever Wegener's own attitude may have been originally, in his book he is not seeking truth; he is advocating a cause, and is blind to every fact and argument that tells against it. Much of his evidence is superficial. Nevertheless, he is a skillful advocate and presents an interesting case."

Schuchert used an exaggerated measurement by Lake to make a point against Wegener:

Schuchert 1928a, p 120.

Lake also says (1922) that when one moves the Americas rigidly and without distortion against Euro-Africa, then the Sierras of Argentina fail to meet the Cape mountains by 1,200 miles.

Schuchert knew that Lake's estimate of 1,200 miles was exaggerated. In this AAPG paper, Schuchert had tested the fit on his own 10-inch globe, and had found that the mismatch would not be more than 350 miles. The mismatch is much less if South and North America are not moved as a completely rigid unit. The close fit of Bullard (1965) shows that there is no mismatch of the Sierras of Argentina and the Cape Mountains.

Schuchert repeated another misleading statement by Lake, implying that Wegener had molded, not just rotated, his jigsaw pieces to make them fit:

Schuchert 1928a, p. 125.

Regarding Wegener's reconstruction of the tectonic lines between northeastern North America and northwestern Europe, Lake says that they fit very well, but that this results from taking great liberties with the earth's crust;

> "he has pressed Newfoundland and Labrador strongly towards the northwest and has turned the former through an angle of about 30 degrees. The westerly motion of Newfoundland may be admitted as consistent with the hypothesis; but if, in addition to moving the masses of sial, we are also allowed to mould them as we will, the coincidences that we deduce become evidence of imaginative powers, not of former realities."

Like Lake's earlier reviews, Schuchert's article was one continuous attack on Wegener's mobilism. Schuchert felt he had a secret weapon, which he now used. It was the revelation of a previously unknown geosyncline extending northeastward parallel to the coast in eastern Brazil. He was quite certain that this geosyncline did not extend into Africa, as it should according to the Wegener-fit. Schuchert introduced it in this way:

Schuchert 1928a, p. 125.

We will now take up a structural element in the eastern part of South America that is unknown to Wegener, because it has never been presented in generalized form. In its revelation of how little the geology of Brazil is related to that of Africa, it deals, however, a crushing blow to the displacement hypothesis.

Schuchert then used a few pages to describe this structural element, which he christened the Franciscan Geosyncline. He drew it on a simple sketch-map (Fig. 50) that he had taken from a German schoolbook (Behm, 1923). Unknown to Schuchert, this was actually Wegener's fit-map from 1915. The absence of the Franciscan Geosyncline in Africa was a type of negative evidence that Schuchert should not have relied on. It would later be shown (see p. 227) that this geosyncline did actually extend into Africa. The other geosynclines that Schuchert drew fit reasonably well on Wegener's map. But Schuchert was not looking for evidence to support the displacement theory.

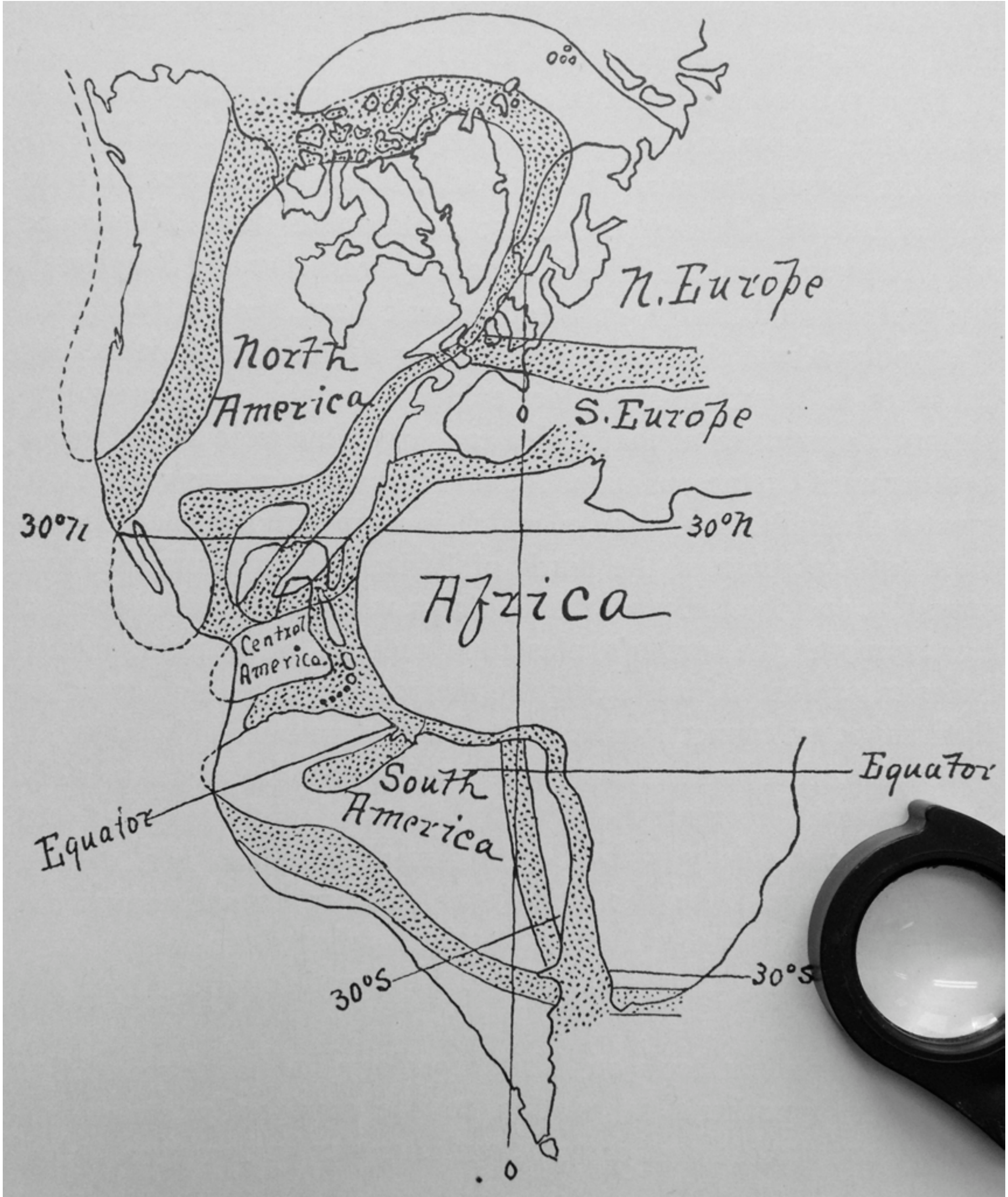


FIG. 18.—Western Pangaea on the same base as Fig. 17. Dotted areas indicate geosynclines after Schuchert; the intercontinental space from South America north to the Amazonian gulf should not have been dotted.

Figure 50. The Schuchert-fight, intended to discredit the Wegener-fit. A South American geosyncline seemed to not continue into Africa, as it should according to Wegener's reconstruction. Schuchert called this "a crushing blow" for the displacement theory. From Schuchert (1928a).

Reginald A. Daly did not contribute to the symposium volume, and probably had not attended the meeting. In the symposium volume, Schuchert intentionally misquoted Daly's book *Our Mobile Earth* to falsely imply that Daly was critical of Wegener. I have given the full Daly-citation earlier (p. 117), and here we can see how Schuchert misused it. Daly's paragraph began with two sentences that were favorable toward Wegener, which Schuchert did not include: "Some geologists, especially European geologists, saw at once how the new hypothesis explains automatically not only mountains but also a dozen other mysteries in their science. But there is a difficulty. Neither Taylor nor Wegener has shown *why* the continents should move." Schuchert left out this praise for Wegener's hypothesis, and wrote only the next part of the paragraph dealing with its difficulty:

Schuchert 1928a, p. 108.

Daly, in his new book, says:

Neither Taylor nor Wegener has shown *why* the continents should move. They have not discovered the force which did the gigantic work of overcoming the resistances to continental migration. Nor have they evaluated these resistances. For these reasons geologists are going slow in placing such mobility of continents among the accepted principles of science.

By beginning with this difficulty, and by ending the citation here, Schuchert made it appear that Daly was simply finding fault with Wegener's hypothesis. Schuchert hid the fact that in the very next sentences, Daly proudly announced that he had now discovered the force that Wegener had been looking for. Daly had written: "The continents appear to have slid down-hill, to have been pulled down, over the earth's body, by mere gravity." Daly's words, in their fuller context, are printed earlier (p. 117).

Most of the other North American contributors to the symposium volume were nearly as negative as Schuchert. William Bowie ended his nine-page paper with this curious statement:

Bowie 1928, p. 186.

What I have said is destructive criticism, but I believe that all of the questions I have asked are fair, and surely if they can be answered in a convincing manner by the advocates of the Wegener hypothesis, that hypothesis will gain many advocates who are now opponents.

Rollin Chamberlin accused Wegener of dogmatism:

Chamberlin 1928, p. 83.

Wegener's theory, which is easily grasped by the layman because of its simple conceptions, has spread in a surprising fashion among certain groups of the geological profession. Other groups of the profession ask: "Can we call geology a science when there exists such difference of opinion on fundamental matters as to make it possible for such a theory as this to run wild?" The following are a few of the reasons why it seems to the writer that Wegener's theory is utterly untenable. The limits of space allow these objections to be listed only in skeleton outline without elaboration or qualification. But Wegener's own dogmatism, even where space did permit greater accuracy of statement, makes categorical comments somewhat less objectionable than would otherwise be the case.

That claim by Chamberlin, that space limitations prevented him from elaborating on his opinions, was not really valid. He wrote only five pages, one of the shorter papers in the symposium. Schuchert had written 41 pages, and Longwell had squandered nearly three published pages presenting his two large forced-fit maps. Authors in this volume were clearly allowed to use as much page-space as they pleased.

In his five pages, Chamberlin wrote critical opinions in the form of 18 numbered paragraphs. He offered no documentation or explanation for most of them. Here are a few of these 18 paragraphs:

Chamberlin 1928, p. 85-86.

9. The American continents fit against the Old World very badly indeed, except for the northeast corner of South America fitting with the Gulf of Guinea, and even there they do not fit any too well. This simple similarity of outline between the Gulf of Guinea and the northeast corner of South America, which may or may not have any fundamental significance, seems to be the starting point of Wegener's theory.

10. If Wegener is right the rocks should correspond province by province on the two sides of the Atlantic. H. S. Washington (Journal of Washington Academy of Sciences V. 13, pp. 339-47) has made a careful petrographic comparison of the corresponding shores and found that the rocks do not check. The pie was not cut as Wegener says it was.

16. A great deal of Wegener's argumentation seems very superficial, and the facts involved seem to the writer in many cases to point to very different conclusions from those utilized by Wegener.

17. Wegener's hypothesis is of the foot-loose type, in that it takes considerable liberty with our globe, and is less bound by restrictions or tied down by awkward, ugly facts than most of its rival theories. Its appeal seems to lie in the fact that it plays a game in which there are few restrictive rules and no sharply drawn code of conduct. So a lot of things go easily. But taking the situation as it now is, we must either modify radically most of the present rules of the geological game or else pass the hypothesis by. The best characterization of the hypothesis which I have heard was a remark made at the 1922 meeting of the Geological Society of America at Ann Arbor. It was this: "If we are to believe Wegener's hypothesis we must forget everything which has been learned in the last 70 years and start all over again."

Chamberlin clearly saw geology as a game, with "restrictive rules" and a "sharply drawn code of conduct". He would not allow Wegener to change "the present rules of the geological game" and have it "start all over again."

Rollin Chamberlin had been introduced to this geological game by his father, Thomas Chrowder Chamberlin (1843-1928). Rollin had become a professor at the geology department that his father had founded at the University of Chicago. T. C. Chamberlin had written a famous paper, *The Method of Multiple Working Hypotheses* (1889). Now his son was refusing to employ an able hypothesis that should have been put to work.

Recall that Schuchert had quoted some negative opinions that Edward Berry had expressed in previous years. Berry was a professor of paleontology, but in this symposium volume he attacked Wegener's hypothesis more on the basis of geophysical arguments than paleontological ones. Berry's abstract and paper both state that Wegener's hypothesis fails in explanation of geological climates. That conclusion is simply stated, and not supported by any facts:

Berry 1928, p. 195.

The hypothesis fails entirely, in my opinion, in explanation of geological climates. Wegener obviously does not know what geological climates were like, nor does he seem to me to be conversant with the established facts of historical geology, since many of his age determinations are erroneous.

Neither do I consider that the distribution of fossil or recent organisms can be explained by the Wegener hypothesis. Continental drifting, exactly as in the case of the supposed land-bridges over oceanic areas that it was designed to replace, raises more distributional problems than it solves. This is too vast and complicated a subject to attempt to elaborate here in even the briefest sort of a way.

From that statement, we see that Berry rejected both sinkable land bridges and continental displacement. Neither of these theories had a valid mechanism, and seemed to be matters of faith. He would not deal with any real evidence that related to these alternative theories, but only mentioned vague “distributional problems.” He concluded with this statement:

Berry 1928, p. 196.

I regret that brevity has necessitated categorical statements on my part, but I have a feeling that it is as futile to discuss the interior of the earth until we have more facts, as it is to attempt a “scientific” proof of a future life, or the divine inspiration of the Pentateuch.

The Pentateuch is the part of the Bible that includes the Book of Genesis. It is full of statements that cannot be scientifically debated. Berry was a paleontologist, not a geophysicist. He was not qualified to debate what might be going on in the interior of the Earth. He had his opinions, and made his categorical statements. And he pretended that the need for brevity kept him from providing any evidence for those statements. But there was no need for brevity; authors had more than a year to write their papers, and Berry’s three-page contribution was among the shortest in the symposium.

Alfred Wegener’s article was translated by Van der Gracht, and I think he probably also wrote the title and abstract. It was not Wegener’s style to proudly write “My Theory” or to use the phrase “Continental Drift.”

In his article, Wegener tried to help the North American skeptics by showing them ways to test and potentially disprove his displacement hypothesis. He pointed out precisely the geological evidence in America that might contradict his hypothesis. This was typical for Wegener; he was using mobilism as a working hypothesis, to challenge ideas and make predictions. He was not looking for easy solutions or ignoring data that did not fit his ideas.

Wegener 1928, p. 97-100.

An objection could be raised against my conception of the distribution of climate in the Carboniferous and the Permian, on the ground that several geologists claim that there are indications of considerable regional glaciation in North America, in areas which, under my reconstruction, should have had a geographic latitude of 10 to 20° in the Permian. I name some examples. First the so-called Squantum tillite, near Boston. It can be traced over a considerable region as a conglomerate with a thickness as great as 2,000 feet. Scratched boulders are reported in it, and furthermore there exist deposits of banded clays, which closely resemble seasonal "varves," such as were formed during the time that the Pleistocene land ice melted. Other massive conglomerates have also been interpreted as glacial in large areas of the upper Carboniferous and Permian of Oklahoma and Kansas. Isolated large boulders, imbedded in marine deposits of this region, have been explained as having been transported by floating ice. Finally, it has been suggested that the great boulders, in the enormous middle-Carboniferous conglomerates of Colorado, which attain a thickness of 6,000 feet, must have been transported by ice. These presumable traces of glaciation have been described in several publications. I particularly mention those of Sayles, Weidmann, Raff and Ulrich, Woodworth, and Coleman. Other geologists have seen these localities and expressed opinions regarding these deposits. I am not aware that doubts have so far been expressed, or published, concerning their glacial nature.

If all or any of these conglomerates are truly glacial, they would be in flagrant contradiction to my conception....

[Here I skip over 7 paragraphs.]

It would be impossible to explain the previously named conglomerates as glacial except in utter conflict with all the other well-harmonizing proofs for a tropical and sub-tropical climate during these same periods. I use this opportunity to draw the attention of American geologists to these conflicting conclusions, hoping that further work will clear up this apparent incongruity...

Wegener's hypothesis predicted that future work would show that the Squantum tillite and other possible glacial deposits in North America had been interpreted incorrectly. Just as he predicted, it was later found that none of these conglomerates indicated Permo-Carboniferous glaciation. The Squantum tillite is apparently Late Precambrian.

Although no geologists at this symposium were impartial, most of the authors faulted Wegener for his partiality. Singewald was less critical than others, but nevertheless:

Singewald 1928, p. 189.

The psychology of Wegener's presentation is that he observed certain phenomena that suggested a theory that gave a plausible explanation for some problems with which geologists had been wrestling. He then set out to probe that theory in the role of an advocate rather than to test it. He has assembled a great array of data collected over a wide range of literature in support of the theory.

Bailey Willis concluded his article with these three paragraphs

Willis 1928, p. 82.

Having endeavored to give the theory of continental drift that impartial consideration which should be accorded to every hypothesis that may possibly advance research, I am forced by the author's own statement of the argument to conclude: that the original suggestion of the idea sprang from a similarity of form which in itself constitutes no demonstration; that if the drift had occurred in the manner described and with the results deduced by him, then the actual similarity could not have survived but must have been destroyed by faulting; that the hypothesis is in other respects in contradiction with its necessary consequences; and that for these reasons it should be rejected.

When we consider the manner in which the theory is presented we find: that the author offers no direct proof of its verity; that the indirect proofs assembled from geology, paleontology, and geophysics prove nothing in regard to drift unless the original postulate of drifting continents be true; that the fields of related sciences have been searched for arguments that would lend color to the adopted theory, whereas facts and principles opposed to it have been ignored. Thus the book leaves the impression that it has been written by an advocate rather than by an impartial investigator.

It matters little what we think of it. The future will deal fairly with it according to the principle that truth alone survives.

Lake, Schuchert, Longwell, Singewald, and Willis all criticized Wegener's scientific method by calling him an advocate for his theory. They meant that Wegener was acting as a defense lawyer, willing to defend his client (mobilism), even if he might know that the client is actually guilty.

Wegener had written that he began with a hypothesis and then looked for evidence to support it. The fixists presumed that he would hide evidence that argued against it. But in fact, that is not how Wegener operated. If they had they read his work more objectively, they would have seen that he wanted to have his hypothesis tested critically. In Wegener's paper for this AAPG volume, he told fixists how to disprove his theory: they simply needed to find conclusive evidence of ice sheets in North America in Permo-Carboniferous time. According to fixism, Canada should have been covered by ice sheets in the Permo-Carboniferous just as it had been in the Pleistocene. No defense lawyer offers evidence that can potentially convict his client. The fixists' assumptions about Wegener acting as an advocate tell something about their own methods. They were being advocates for their client (fixism), which indeed was later proven guilty.

A careful reading of Bailey Willis's article shows that he was not specifically arguing against displacement, but against the forces that Wegener was calling upon. Finally, Willis ended his article with this curious comment: "It matters little what we think of it. The future will deal fairly with it according to the principle that truth alone survives." What did Willis mean by this? To write "it matters little" makes me think of this argument between fixists and mobilists as some sort of a game or sport. And "the future will deal fairly with it" implies that the present players were not being so fair.

One plays hard in sports, and it is the referee's duty to insist that play is fair. But there had been no referee when Wegener misquoted Willis in 1912 and 1915. And there was no referee now. For Willis, the referee in this game would be "the future". It seems that Willis could imagine Wegener eventually winning, getting some of the recognition he deserved.

Willis may have begun to realize that the fixists would eventually lose this game, but he was not a quitter. Alfred Wegener was not a quitter either, but the game ended abruptly for him. He published his fourth edition of *Die Entstehung der Kontinente und Ozeane* in 1929. In 1930, a few days after his 50th birthday, he died, probably of a heart attack, during a winter expedition in Greenland. His body was found in the snow the next summer, but it was left to become buried and transported in the glacial ice sheet.

8. Wegener's Hypothesis Frozen Out of North American Science

ARTHUR HOLMES' SOLUTION TO THE MECHANISM PROBLEM (1927, 1931, 1933)

In 1927 the British geologist Arthur Holmes (1890-1965) recognized that convection currents in the mantle must be the mechanism by which continents move. In a letter to Charles Schuchert he explained this mechanism. He wanted to publish his hypothesis for mantle convection in the prestigious *American Journal of Science*, where Schuchert was an editor:

Holmes letter to Schuchert 1927 (Oreskes 1999, p 193)

I shall be glad to write a paper for Am. J. Sci. on these topics if you think it not too speculative. I have been hoping to find a clear road through all our tectonic difficulties, but every hypothesis raises fresh difficulties of its own. However, on the whole I find a combination of Wegener's ideas, with magmatic convection currents inside the earth on a gigantic scale to provide the energy, seems best to fit our needs.

Holmes was one of the most highly respected British geologists. He had recently been selected, together with Schuchert and three other experts, to write an authoritative book on the age of the Earth (Knopf et al. 1931). The book was to summarize the most recent geologic, paleontologic, physical, and astronomical evidence.

Holmes' field of expertise was quantitative geology and radioactivity. In 1913, at the age of 23, Holmes had established himself as a leader in this field. He published a book, *The Age of the Earth*, that for the first time, put numerical ages on many of the geological periods. His book was a breakthrough, and fun to read. It began thus: "It is perhaps a little indelicate to ask of our Mother Earth her age, but Science knows no shame and from time to time has boldly attempted to wrest from her a secret which is proverbially well guarded."

Holmes followed this early success with other books and scientific papers of the highest quality. Now in 1927, he was writing to Schuchert about another advance in quantitative geology – calculations of radioactivity in the mantle showing that convection cooling must occur there, and that this convection would move the continents.

Schuchert showed Holmes' letter to his colleague Chester Longwell. He then sent a negative reply to Holmes: "I will not ask you to write the paper..." (Oreskes 1999, p. 193).

Longwell also wrote a letter to Holmes, arguing that gravity could sink the postulated continents without the need for horizontal displacement. But Holmes was certain that the hypothesis of sinking continental land bridges was wrong. His reply to Longwell included this statement about continental drift:

Holmes' letter to Longwell 1927 (Oreskes 1999, p 193)

It is impossible (within the conditions and limits of present day knowledge) to get rid of lands that formerly occupied the sites of present oceans except by moving them sideways. I can see no alternative at all to continental drift, and I have come to that conclusion from a position of strong prejudice against such processes.

After being turned down by Schuchert in America, Holmes presented his paper at a scientific meeting in Scotland on Jan 12, 1928. A brief summary without illustrations appeared in the British *Geological Magazine* in 1928. It took three years before the paper itself was was published, in the *Transactions of the Geological Society of Glasgow*. Most North American geologists never saw it. The paper was ready in 1928, but he added some references to papers that appeared during the next few years. A German translation was published (Holmes,1930), a year before the English original (Holmes, 1931).

The paper showed convection currents much like those assumed today to operate in plate tectonics (Fig. 51). It included this description:

Holmes 1931, p. 574.

Let us suppose, to illustrate the effects of slight radioactivity, that the output within the substratum is only 1/700 of that of plateau basalt. On geochemical grounds it is difficult to see how this can be anything but an underestimate. The volume of the substratum (60 km, to 2,900 km.) is 88.75 x 10^{10} cu. km., and the total heat generated per year on the assumed figure is 142.5 x 10^{17} calories. This is equivalent to the cooling of 62 cu. km. of basaltic magma from 1,000°C to a crystalline rock at 300°C. Obviously, to get rid of this quantity of heat some process much more drastic than ordinary volcanic activity would be called for. The volume of lava in the 1929 eruption of Vesuvius was only 12 x 10^{-3} cu. km. The annual loss of heat from all volcanic sources has been estimated at 6 x 10^{17} calories (82, p. 83).

For the accumulated heat of 200 million years to escape through the sites of the oceans it would be necessary for one third of the whole of the ocean floors (taken at 60 km thick) to be engulfed and heated up to 1,000°C, and replaced by magma which cooled down to form new ocean floors at 300°C. A process competent to bring about this result on the scale indicated would be some form of continental drift involving the sinking of old ocean floors in front of the advancing continents and the formation of new ocean floors behind them.

We may therefore conclude that

(a) if the crust of the earth makes good the loss of heat by conduction to the surface, and

(b) if the substratum has only 1/700 of the heat-generating capacity of plateau basalt; then

(c) the substratum cannot yet have cooled sufficiently to have crystallized, but must still be in the stage of convective circulation, and

(d) to avoid permanent heating-up, some process such as continental drift is necessary to make possible the discharge of heat.

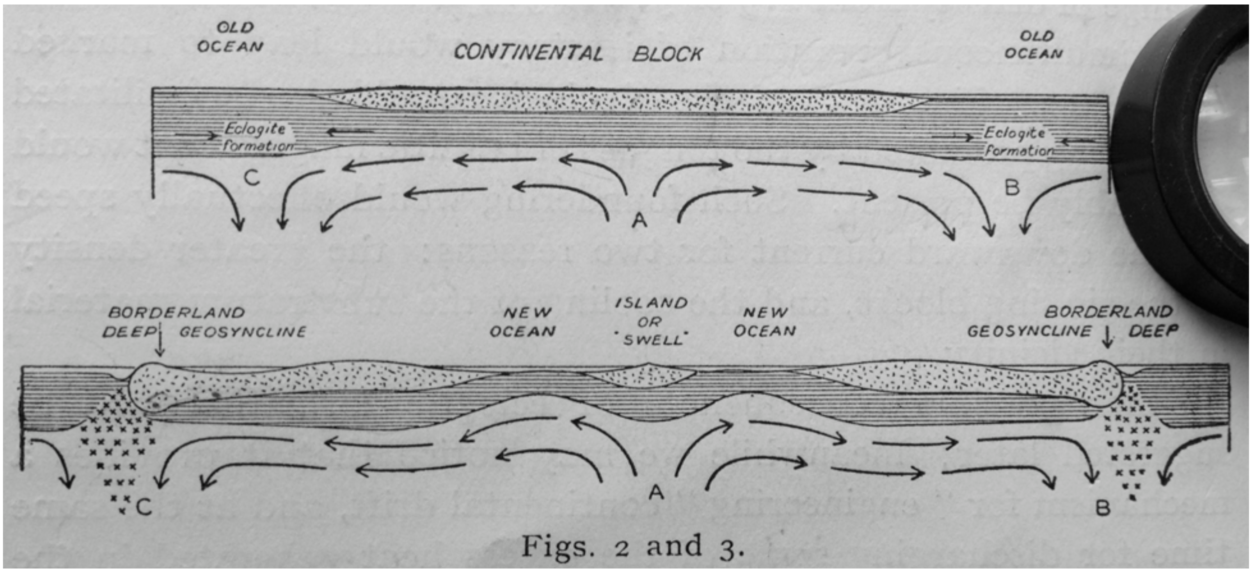


Figure 51. Holmes' illustration of convection currents in the mantle. From Holmes (1931).

The mantle keeps from overheating through convection, and continental drift is simply a result of this convection. It is interesting to note that already in his first book (1913, p. 18) Holmes had realized that radioactivity produces an excess of heat. "A newly recognized source of heat must now be taken into account, and indeed, so relatively abundant is the supply, that our present difficulty is to understand why the earth is not hotter than we actually find it." Now, fourteen years later, he had found the way the Earth keeps from overheating, and the way continents were displaced. In geology, one often finds what one is looking for.

In the above paragraphs, Holmes estimated that about one third of the Earth's oceanic crust would be renewed by spreading and subduction in about 200 million years. Today we know that this is an underestimate, just as Holmes said it was. Not one third, but all of the Earth's oceanic crust is renewed in this amount of time. Holmes indicated that the velocity of these mantle currents is on the order of 5 centimeters per year, which fit well with Wegener's models. This agreement might be expected, because Holmes was using Wegener's results as a means of guiding his calculations:

Holmes 1931, p. 584-585.

In the case of Gondwanaland the evidence of late Carboniferous glaciation is here accepted as a definite geological proof that such drifting has since taken place ...

The deduction cannot be escaped that South Africa lay near the South Pole ...

Holmes showed that convection currents could explain not only the drifting apart of continents, but their collision as well. In his figure (Fig. 52), note the arrows showing global-scale compression between India and Asia to form the Himalayas, and between Africa and Europe to form the Alps. He related the compression and continental drift to a small number of convection cells involving the full mantle, which he illustrated as extending down to the core.

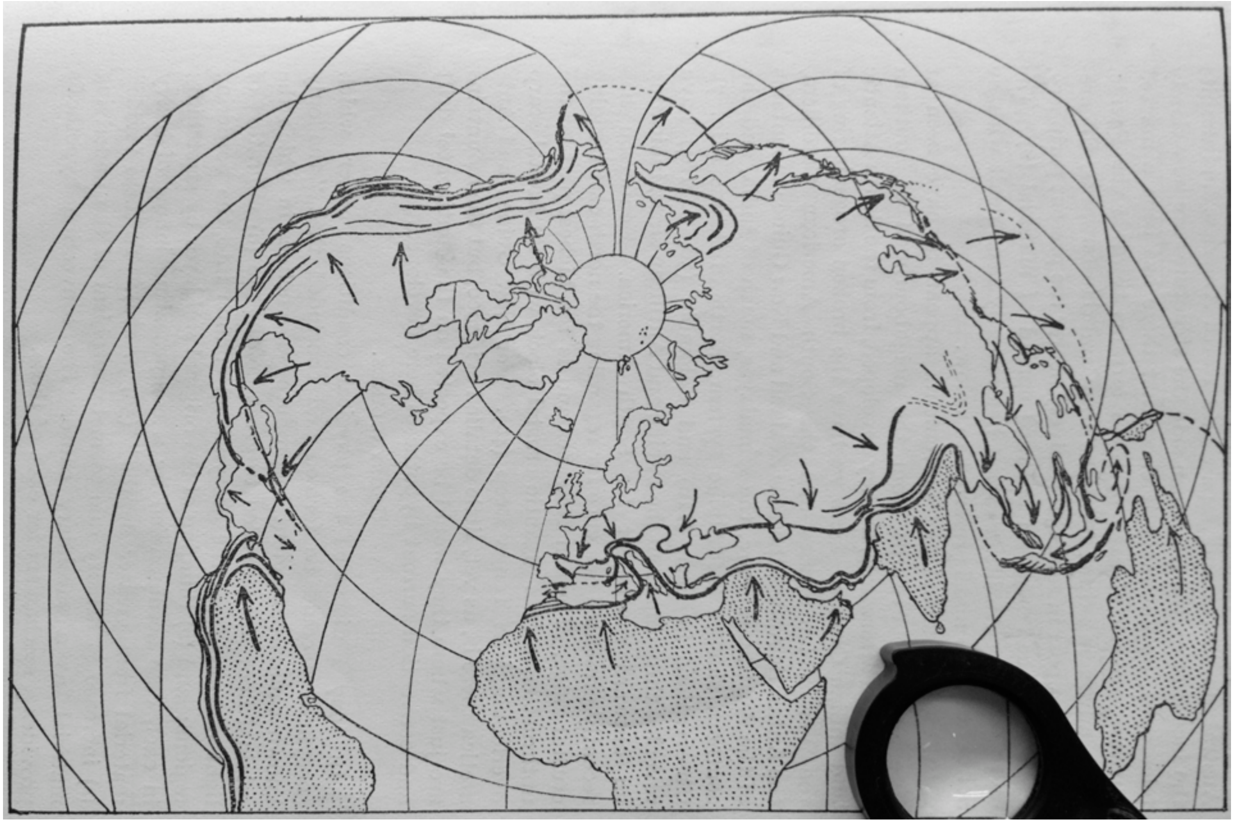


Figure 52. The directions of movement of continental crust. From Holmes (1931).

Holmes published a related paper in the *Journal of the Washington Academy of Sciences* in 1933, following a lecture that he gave in the USA. Here he used the same two illustrations of convection currents as in his 1931-paper. In his explanation, note words commonly used for the same processes in modern plate tectonics: diverge, converge, spreads, dragged down.

Holmes 1933, p. 188:

Vertical currents ascend in certain places and on reaching the top the material promotes sub-crustal fusion and spreads out laterally, flowing horizontally to the neighboring descending currents. The horizontal flow is likely to exercise a powerful viscous drag on the lower levels of the crust, throwing the latter into tension where the currents diverge and into compression where they converge (Figs. 12 and 13.) If the distance between ascending and descending currents be of continental dimensions then the drag due to horizontal flow may produce continental displacements, with distension behind and mountain-building in front. Material from the substratum ascends into the distended area, where it cools and differentiates, while heavy compressed crustal material (eclogite from the basaltic layers) is dragged down with the descending currents.

He hardly mentioned continental drift in that paper; it dealt with the thermal history of the Earth, and mountain building. In any case, few geologists were in the habit of reading that journal, and Holmes' convection model remained largely unknown. But some geologists were now aware that convection currents could easily explain continental drift. Schuchert and Longwell were two of these, because of the letters they had exchanged with Holmes. In their contributions to the 1928 symposium volume *Theory of Continental Drift*, they avoided the standard argument that there was no reasonable mechanism for moving continents.

WEGENER'S MENTION OF THE CONVECTION-CURRENT MECHANISM (1929)

Wegener promoted *Pohlflucht* and *Westwanderungen* as his favored mechanisms for moving continents. He was not against a mechanism of internal currents; he had suggested this idea already in his first paper in 1912. But he had never been convinced of it.

In Wegener's last book, published in 1929, he mentioned mantle convection currents twice (on p. 60-61 and p. 184-185). If he had been aware of Holmes' results, he might have given convection currents more attention, perhaps with an illustration. As it was, Wegener ended his chapter of Displacement Forces by noting that mantle convection might be important for continental displacement, opening of oceans, and formation of compressional mountain ranges:

Wegener 1929, p 184-185 (translation by Biram 1966, p. 178-179).

Recently, several authors, such as Schwinner (1919) and especially Kirsch (1928), have made use of the concept of convection currents in the sima. In conjunction with Joly's idea that the sima under the continental blocks is heated by the large radium content, and that in oceanic regions it cools, Kirsch assumes a circulation of sima beneath the crust: It rises below the continents up to their lower boundary, then flows along under them to the ocean regions, where it flows downwards, returning to the continents after reaching greater depths. Because of the resulting friction, he says, the sima tends to disrupt the continental cover and to force the fragments apart. We mentioned earlier that the relatively great fluidity of the sima, as assumed here has been regarded as unlikely by the majority of authors to date. In considering the earth's surface, however, there is no mistaking that the split-up of Gondwanaland and also that of the single continental block composed of what is now North America, Europe and Asia, can be conceived as the effect of such sima circulation. This idea also apparently offers a reasonable explanation of the opening up of the Atlantic Ocean. It cannot therefore be utterly rejected on the grounds that the superficial phenomena of the earth would gainsay it. If the theoretical basis of the ideas should prove adequate to support them, they could in any case be considered as contributory factors in the formation of the surface of the earth; it is still not possible at present to survey the theoretical background.

Our discussion will have shown the reader that the problem of the forces which have produced and are producing continental drift (except the pole-flight force, already thoroughly investigated) is still in its infancy.

We may, however, assume one thing as certain: *The forces which displace continents are the same as those which produce great fold-mountain ranges.* Continental drift, faults and compressions, earthquakes, volcanicity, transgression cycles and polar wandering are undoubtedly connected causally on a grand scale. Their common intensification in certain periods of the earth's history shows this to be true. However, what is cause and what effect, only the future will unveil.

The expression "continental drift" appears in each of the last two paragraphs quoted above. Wegener used the word *Verschiebungen*, which could be better translated as movements or displacements. He did not use the term *Triften*, or drift. But the translator, and nearly everyone after 1928, thought that Wegener used the term continental drift.

Few English-speaking geologists realized that Wegener had considered and supported a convection-current mechanism. Most geologists had not read Wegener's work. The third edition, the one that was translated into English, was often referred to as if it were his last volume. Even today, geology textbooks suggest that Wegener's only working hypothesis was that continental crust plows through the sima of the oceans.

COMMENTS ON ALFRED WEGENER'S ACCOMPLISHMENTS AND SHORTCOMINGS

Wegener's hypothesis was rejected by most geologists during his lifetime. But he knew that his theory had solved major geologic problems that fixism could not explain: the problems of sinkable land bridges; the extremes of Permo-Carboniferous climate; and the horizontal compressions that produced fold mountain belts. He stated the solutions to these problems concisely in his various books. Here are some of these key statements:

1. On the disappearance of land bridges.

Wegener 1929, p 21 (my translation, which I prefer to Biram's).

On the basis of displacement theory, we can satisfy all the legitimate demands of both the Land-bridge Theory and the Permanence Theory. We can now say: Land bridges, not in the form of connecting-continents that later sank, but by contact of today's continental blocks. Permanence, not of individual oceans or continents, but of the total areas of deep oceans and continents.

2. On the Carboniferous-Permian climate:

Wegener 1922 (p. 108, translation by Skerl 1924).

The whole of these evidences of the climate of the Permo-Carboniferous period give such a convincing picture of the climatic zones prevailing then that I do not see how this conception of the position and direction of movement of the poles can be dismissed. In this way these evidences become a strong proof of the accuracy of the displacement theory.

3. On convection currents moving continents and producing mountain ranges (italics his):

Wegener 1929, p. 184-185 (translation by Biram 1966, p. 178-179).

there is no mistaking that the split-up of Gondwanaland and also that of the single continental block composed of what is now North America, Europe and Asia, can be conceived as the effect of such sima circulation. This idea also apparently offers a reasonable explanation of the opening up of the Atlantic Ocean. ...

The forces which displace continents are the same as those which produce great fold-mountain ranges. Continental drift, faults and compressions, earthquakes, volcanicity, transgression cycles and polar wandering are undoubtedly connected causally on a grand scale.

Schuchert, Willis, and other North American fixists could not accept mobilism. They continued to uphold Dana's doctrine that North America had been created where it stood, alone in the ocean. Wegener was frustrated that North American geologists were denying the evidence for displacement:

Alfred Wegener 1929, p. 138 (my translation, which I prefer to Biram's).

We refrain here from embellishing our statements with quotations from the literature. What anyone can see does not need the support of other opinions, and he who does not want to see, cannot be helped in any case.

A goal of my research has been to understand why Wegener's work was rejected. To be flippant, one might say that his first mistake was being born in the wrong country, and his final mistake was dying prematurely on a Greenland expedition. A bit more seriously, one can say that Wegener, too, was rather fixed in his ideas; he clung to the mechanisms of *Polflucht* and *Westwanderung* long after they had been shown to be too weak to displace continents.

Wegener's works (Fig. 53) were not readily avalable to American readers; only his 1922-book was actually translated in his lifetime. His publications on displacement theory were far too similar. His first two papers, in 1912, had the title *Entstehung der Kontinente,* and his first book-title was nearly the same: *Entstehung der Kontinente und Ozeane*. Although he expanded and improved the book three more times, it kept the same title, size, and style, and his critics assumed that the books were essentially the same. It seemed to many geologists that his later books were not worth obtaining or translating.

The term *Entstehung* (origin) of continents might have appealed to geographers, since the breakup of a continental mass can be considered to represent the origin of new geographic continents. But the term was not really appropriate for geologists, because the continental material itself was not originating this way. A title such as *Pangæa and Its Breakup* would have emphasized Wegener's evidence better than *Origin of Continents*. John Evans had given Wegener the convenient term Pangæa for the supercontinent, but Wegener seems to have been too modest to ever use it.

Wegener's book with Wladimir Köppen was full of important maps and data. Yet when he published his third and fourth editions, he did not include most of this evidence.

Wegener seems to have preferred to work alone on his geologic publications. Except for the book with Köppen, who was his father-in-law, Wegener did not ask for help with his geology. Köppen was a highly respected climatologist, but he was unknown in geological circles. There were several geologists who probably would have worked with Wegener as co-author: Molengraaff, Evans, Du Toit, Holmes, and Van der Gracht. Van der Gracht drew a paper out of Wegener for the symposium volume in 1928, and even translated it to English. But Wegener did not actively seek geologic cooperation or use it. This tendency can be seen in his acknowledgements. In his 1915 book he thanked Professor Cloos. In 1920 he again mentioned help from Cloos, and discussions with Köppen, and four other colleagues. The prefaces to his other books suggest that he had no further help. (I may be influenced here by an absence of evidence).

He published his first paper in *Geologische Rundschau* in 1912. After that, he seems to never have submitted work to a geologic journal. He was a professor of meteorology, not geology, and did not attend international geological meetings.

But probably nothing Wegener could have done would have helped. Du Toit, Van der Gracht, and Holmes were all geologists, and they wrote in English. They were no more successful in convincing North American geologists than Wegener had been.

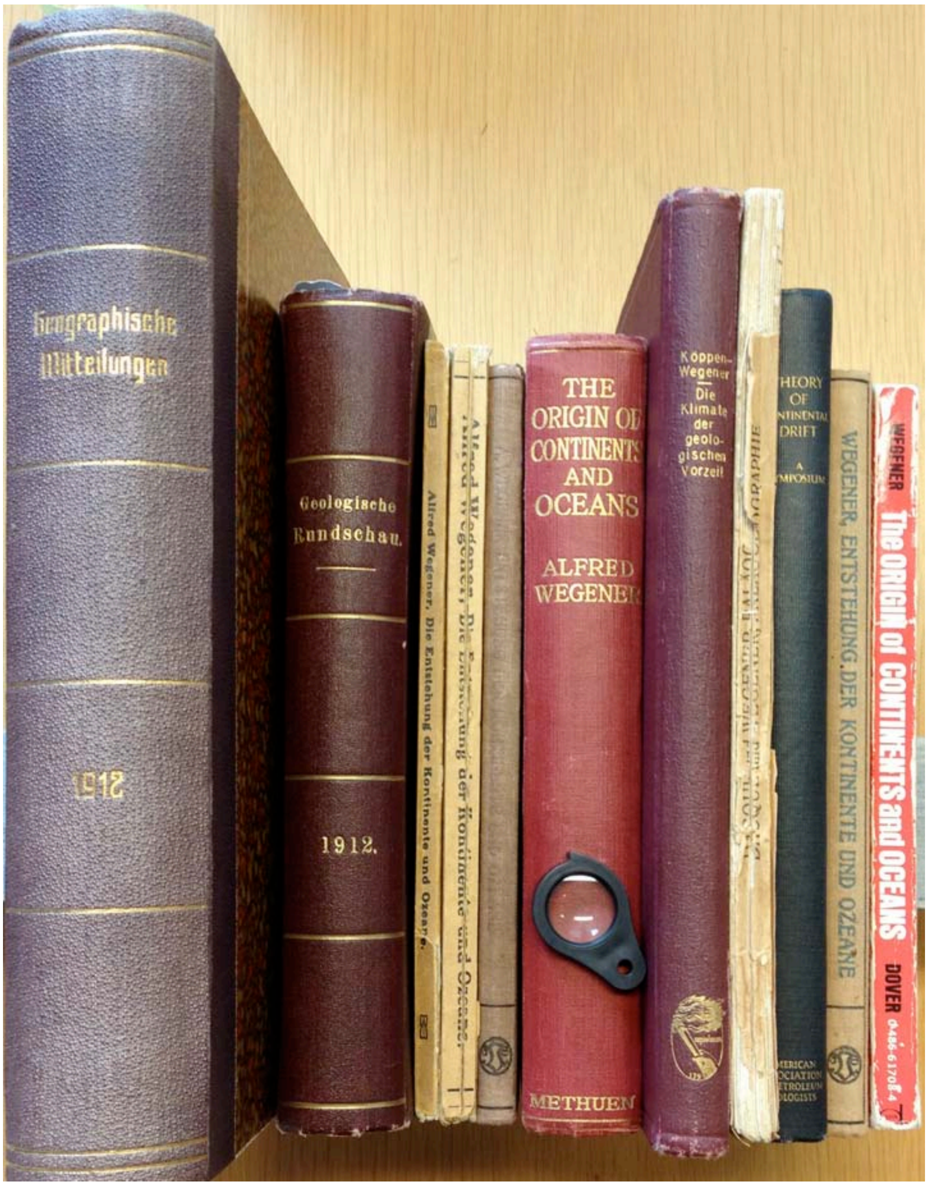


Figure 53. Wegener's geological publications. From left: An article in two respected journals. Three editions of his book, and an English translation (printed on thick paper.) Paleoclimate book with Köppen, and paleogeographic summary. AAPG symposium volume. Fourth edition of his book, and its English translation.

MOUNTAIN-BUILDING FORCES – "ONE OF THE GREAT MYSTERIES OF SCIENCE" (1929)

Horizontal forces and horizontal displacements are obviously required to generate mountain ranges. The rocks are clearly pushed horizontally much more than vertically. The cause of these horizontal forces had always been a problem for geologists. Mountains are such impressive features of the Earth's surface that geology textbooks need to discuss their origins.

Early ideas on mountain building were part of the contraction theory: the interior of the Earth had presumably been cooling and contracting since its formation, and mountains developed like wrinkles on the tough skin of a drying apple. But if this were the case, one would expect the wrinkles to be uniformly distributed over the entire globe, and to have developed continually through geological time. The limited number of fold belts, as well as their locations and ages, argued against this theory. Finally, the discovery of radioactivity convinced geologists that the Earth was not cooling at all. The Earth should be heating up, as Holmes had noted in 1913. The contraction theory was abandoned, and there was no satisfactory hypothesis to replace it.

To understand how mountain ranges were being explained, we can read the description by Chester Longwell in the 1929 edition of *Physical Geology*. North American geology students learned about the geology of mountain ranges from that book. Longwell had a few co-authors, but he wrote most of the book, including the chapter that dealt with mountains.

The first important point Longwell made was that mountain belts are the result of horizontal compression. To illustrate this, he showed a laboratory experiment performed by Bailey Willis (Fig. 54).

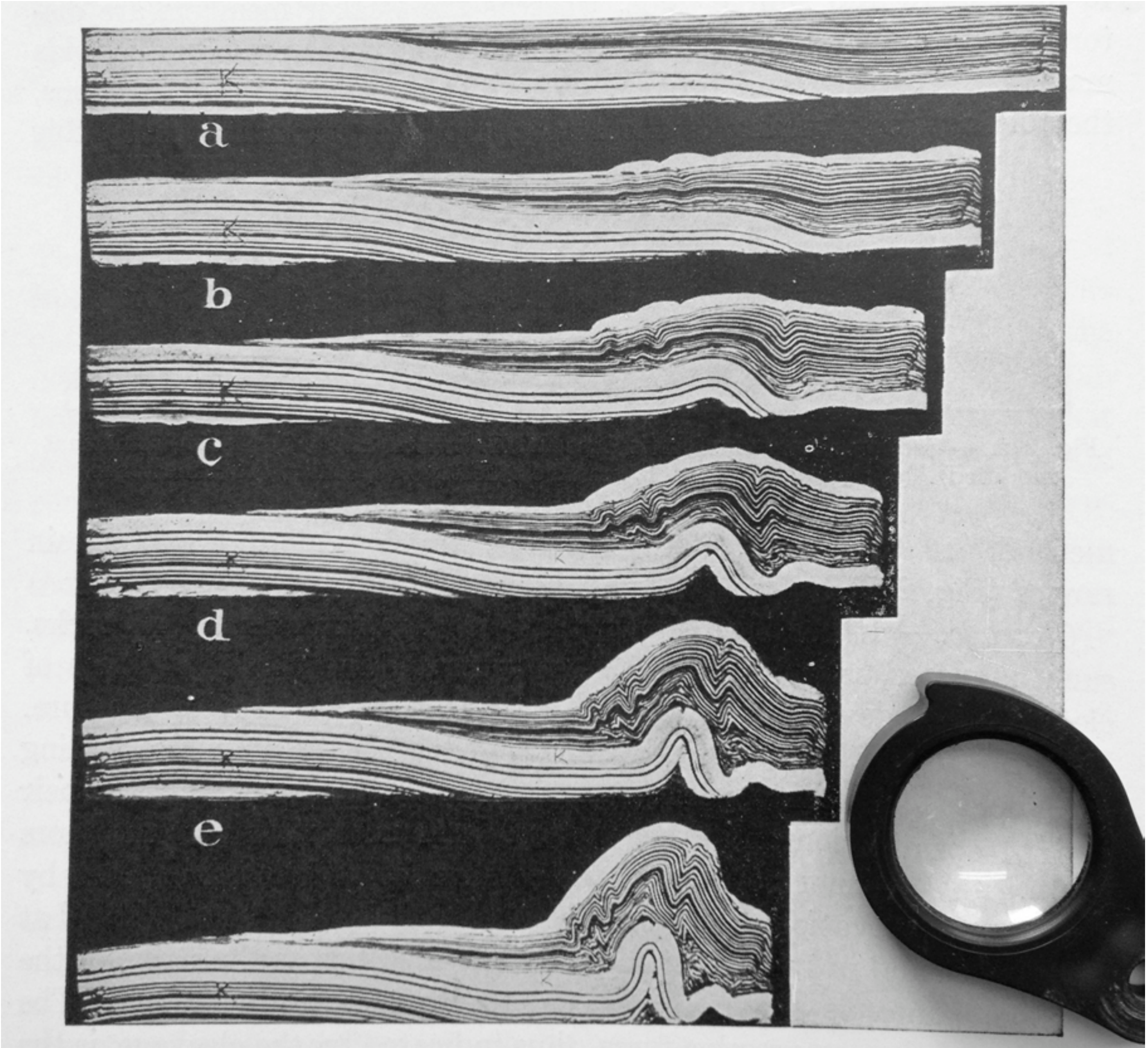


Fig. 273. — Layers of wax and plaster folded by lateral pressure, imitating structures found in mountain ranges. The thrust is from the right, and in successive layers from *a* to *e* the amount of shortening can be seen. (Willis, U. S. Geol. Surv.)

Figure 54. Horizontal compression leads to fold structures that are typical of those found in fold-belts. From Longwell et al. (1929).

Longwell described some of the evidence that horizontal compression was the main force in the building of the Alps and other fold mountain ranges:

Longwell 1929, p. 398-400 (also 1932 p.398-399 / also 1939 p. 423).

Thrust Faults and Recumbent Folds of the Alps. – Alpine structure is characterized by great folds that have been pushed over to a horizontal attitude, and by flat thrusts that were related to these overturned folds. These features are developed on an unprecedented scale, with the result that the Alps consist of a series of great rock sheets, driven one over another and overlapping like the shingles on a roof. The Germans call the individual sheets *decken*; the French refer to them as *nappes*.

Because of their location, the Alps have received more intensive study than any other mountains. Accordingly, in spite of astonishing complexity, their structure and history are well known. Like the Appalachians, they resulted from deformation of thick marine deposits; but a large part of the Alpine sediments bears evidence of deposition in deep water, far from any shore. Land lay to the north, in the present position of central Europe, where mountains of nearly the same date as the Appalachians were being eroded. Orogenic movement began in Mesozoic time, with pressure from the direction of Africa. The soft sediments on the sea floor were bowed up slowly, until islands, and chains of islands appeared above sea level. During early Tertiary time the compression accelerated powerfully, and an enormous rock sheet was driven northward over the geosyncline. Beneath this sheet the plastic sediments suffered extreme distortion. With recurrent thrusting during the Tertiary other sheets were driven forward, and all were severely folded (Fig. 276) [Fig. 55]. Erosion cut valleys and "windows" through the sheets, exposing the entire series; and in parts of the Alpine area nearly the whole of one or more sheets has been swept away, leaving remnants of old rocks to form isolated peaks standing on younger rocks that were overridden and covered during the thrusting movement. Isolated peaks that have this anomalous relation are called "mountains without roots." The Matterhorn and the Mythen are famous examples. Some of these masses are 50 or even 100 miles north of their original positions. Heim, the great Swiss master of Alpine structure, tells us that the Alpine zone as a whole was made narrower by considerably over 100 miles due to the thrusting and folding. Locally, as in the Simplon Tunnel section, the original width was reduced as much as 90 per cent.

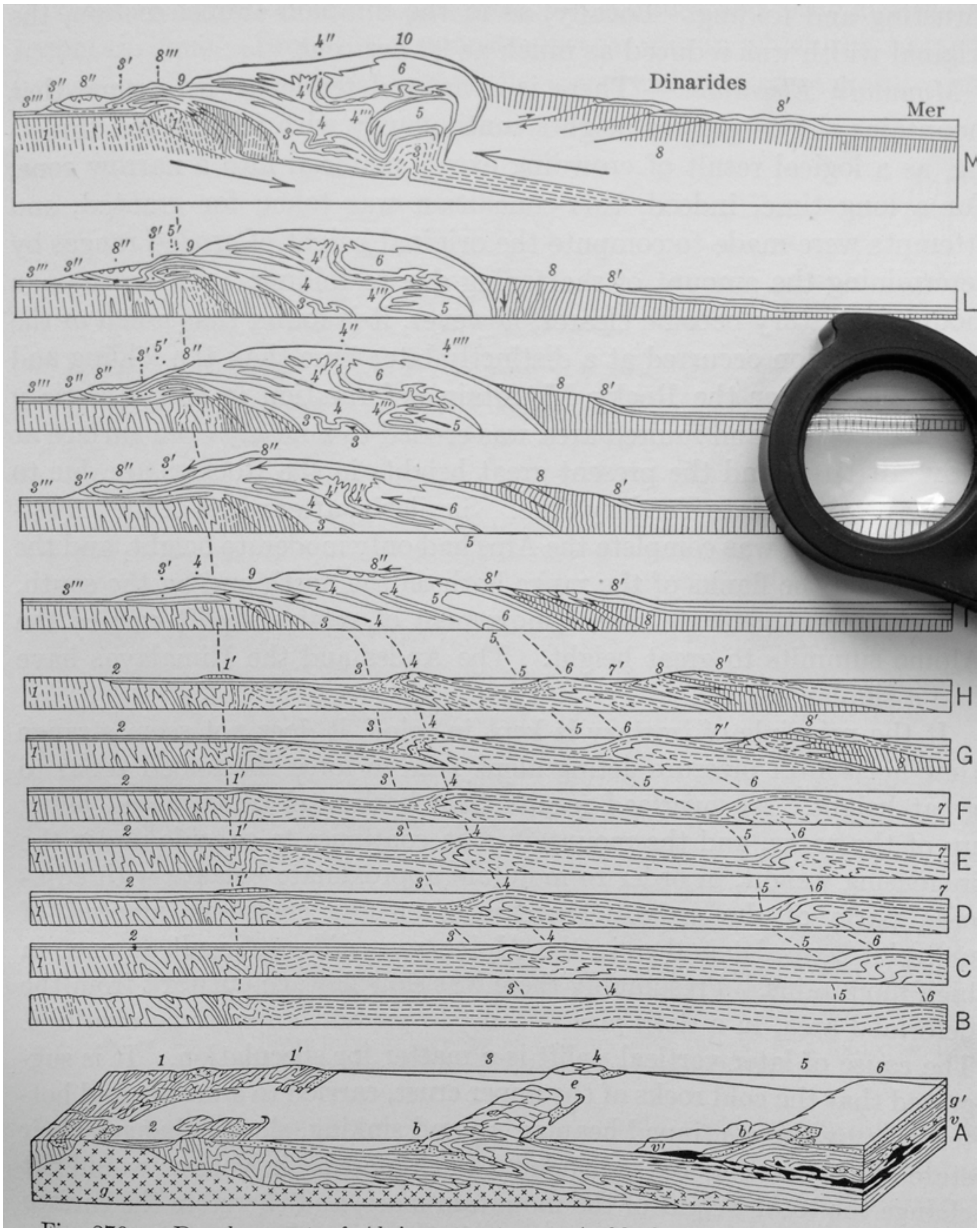


Fig. 276. — Development of Alpine structure. *A*, block diagram representing part of the Alps in an early stage. *B* to *M*, cross sections to show successive stages from beginning of folding (*B*) to the final intricate structure of thrusts and folds (*M*). Figures show horizontal shifting of corresponding masses. Northwest on the left. (Emile Argand.)

Figure 55. Longwell's Fig. 276, showing horizontal compression that formed the Alps. Redrawn after Émile Argand. From Longwell et al. (1929).

There was no doubt that mountains display horizontal shortening of the continental crust. Longwell wrote that the displacement was “considerably over 100 miles”. But he was being very conservative here. The first edition of this textbook (Pirsson 1915, p. 368) had put Heim’s estimate for Alpine shortening at 300-650 miles.

A few pages later, Longwell discussed the yet unknown cause of this horizontal compression. He called it one of the great mysteries of science:

Longwell, Knopf and Flint 1929, p. 404-405 / 1932, p. 406-407 / 1934, p. 296.

Within the last few years some geologists have suggested that whole continents shift horizontally through long distances. It is claimed, for example, that Africa moved northward against the old Mediterranean geosyncline and crushed it to form the Alps and neighboring mountains; that the great folded chains of Asia were caused by southward shifting of that continent; and that the American cordilleras are the result of slow, long-continued westward drifting of North and South America. It is urged that no other explanation will suffice in view of the stupendous shortening recorded by mountain folds and thrusts. But even if we should admit the moving of continental masses, the fundamental problem of orogeny would remain unsolved so long as the ultimate forces and conditions to cause such movement are wholly unknown.

It must be admitted, therefore, that the cause of compressive deformation in the Earth's crust is one of the great mysteries of science and can be discussed only in a speculative way. The lack of definite knowledge on the subject is emphasized by the great diversity and contradictory character of attempted explanations. It is a fascinating problem, but lengthy discussion of its various aspects has no place in this volume. The facts and relationships of mountain structure present a large field of study in themselves, aside from the problem of ultimate forces.

Longwell knew that Holmes' mechanism of convection currents might solve this great mystery. But Longwell did not mention this idea, although he seems to have considered doing so in 1932. The Index for the 1932 edition listed "Continental-drift hypothesis, p. 406" which referred exactly to these two paragraphs. But the term continental drift is not found there; that term must have been deleted from page 406 after the Index was compiled. Longwell saw continental drift as a way of explaining the formation of fold mountain ranges, but he chose not to mention this possibility anywhere in these textbooks, except by mistake in that 1932 Index.

BAILEY WILLIS'S TEXTBOOK *GEOLOGICAL STRUCTURES* (1923, 1929, 1934)

In the last sentences of the citation above, Longwell wrote of compressional deformation: "lengthy discussion of its various aspects has no place in this volume. The facts and relationships of mountain structure present a large field of study in themselves." That large field of study is Structural Geology, and Bailey Willis was a leading structural geologist. In 1922 he had published a general textbook called *Geologic Structures*, with a revised edition in 1929. In 1934 he enlarged it to over 500-pages (Willis and Willis, 1934). The book described fold mountain belts in detail, but never once did it discuss the cause of the horizontal compressive forces. Unlike Longwell, Willis totally avoided this awkward problem, this great mystery of science.

Willis' presentation of compressive deformation in the Alps did point out something of philosophical importance: geologists must acknowledge that deformation has occurred, even if they have to wait for the understanding of that deformation. He was discussing the structure of mountain ranges, but the point is equally relevant to continental displacement. It is interesting to read Willis' description of the Alps:

Willis 1934, p. 188-192.

An overthrust sheet is a thick layer of folded and faulted rocks. It is bounded by thrust planes of great horizontal extent, both above and below. It may be identified by the age, stratigraphic sequence, and fossils of the strata composing it. Thus identified, the Alpine decken have been numbered and named, for instance, in Fig. 90. [see instead Fig. 56, which is Willis' Fig. 91]

The central portion of the sheet is called the *carapace*, the origin (which is at the right of these sections) is the *root*, and the northern end is called the *head*.

It is maintained that successive overthrust sheets have been forced out of the earth at their roots and that they have been pushed over one another in extremely intricate relations. It was once thought that the uppermost nappe had been pushed up to the height of 40,000 feet or more in the central Alpine zone. An alternative view is that the great thrusts developed at some similar depth beneath the surface and that the present elevation of the Alps is an effect of later uplift. Whichever view is taken, all that mass of the decken which would extend over the Alpine summits must have been eroded in separating the heads from the roots.

A concept so extraordinary, faced by mechanical and erosional difficulties of insuperable magnitude, could not without strong supporting evidence have gained the support of well-informed, earnest geologists, as it has. That evidence is stratigraphic and faunal. During various epochs in the history of the trough, where the strata of the Alps were deposited, the stratigraphic sequence on the northern side differed in lithology and fossils from the stratigraphic sequence and fossils deposited contemporaneously in the southern part of the trough. For instance, a thick deposit of mud in the north would be represented in the south by an alternation of limestone and shale or by massive limestone. In this connection the word *facies* is used. Any epoch may be represented in the Alps by a northern and a southern facies.

The argument runs to the effect that the *heads* of the decken which are now found overlying thrust faults in the *north* are of the same facies as the *roots* the same decken exhibit in the *south*. Therefore the strata of the heads must have been deposited originally in the south and could have reached their present positions only by having been thrust over the mass of the Alps. Successive decken must have been thrust forward one below the other in sequence.

This argument has been widely but not universally accepted. Those who remain incredulous have been answered by Albert Heim, who is himself the best informed of all Alpine geologists, as follows:

> On reading their [the objectors] discussions one meets in nearly all cases the more or less clearly recognized origin of their opposition; they are unable to imagine the mechanics of the overthrusting. But neither have we succeeded in that as yet. The facts of the decken structure are so evident that there is no alternative but to accept them and to seek and wait for the understanding.

The strength of this position is the very intimate knowledge of stratigraphy, faunas, and structures of the Alps, gained during the last three-quarters of a century by the indefatigable mountaineers who have devoted themselves to that geologic investigation.

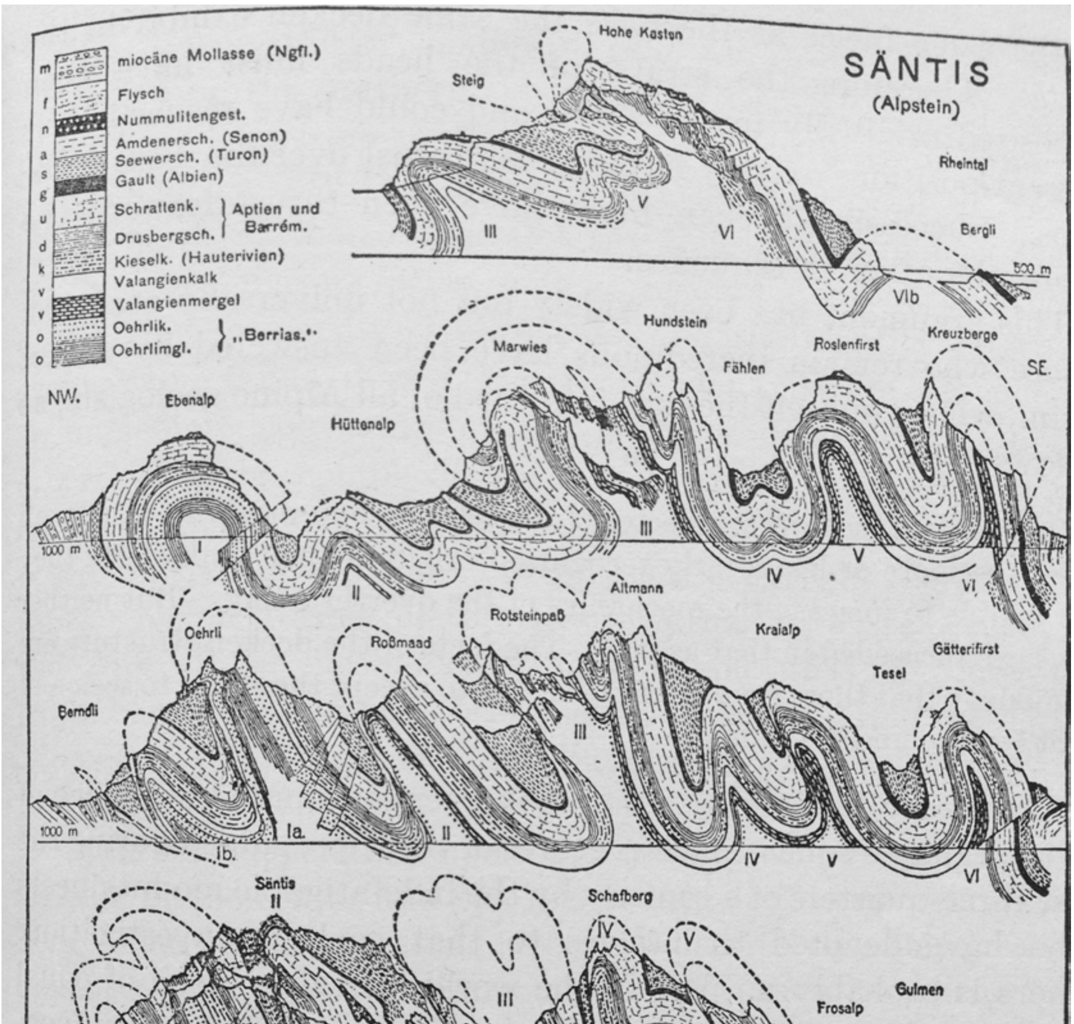


FIG. 91.—Sections through Mt. Säntis. (*Heim, "Die Schweiz."*)

Figure 56. Willis' reprinting of a figure by Albert Heim. These 6-kilometer long sections show typical horizontal compression in the Alps. From Willis (1934).

No fixists had a mechanism to explain the cause of horizontal compression. Just how much horizontal compression was being recognized in fold mountain ranges? Willis avoided putting any numbers on it. But most geologists, even fixists, were forced to accept that continental material had moved hundreds of miles to form the Alps and the Himalayas. That is why Wegener and Argand showed ancient India as being incredibly long in this maps of Pangæa (see Fig. 32) and Gondwana (see Fig. 43). India was presumably shortened by horizontal compression to form the Himalayas.

In his 1928-paper against the displacement hypothesis, Schuchert had explained this vast shortening quite frankly:

Schuchert 1928a, p. 141-142.

Long before this, it has become evident to the reader that the writer is iconoclastic toward the Wegener hypothesis as a whole. On the other hand, he is wholly open-minded toward the idea that the continents may have moved slowly, very slowly indeed, laterally, and differently at different times. Every student of tectonics, in his reading during the past fifteen years in regard to the generalizations attained through a study of mountain structures and their meaning, must have said to himself again and again, that there has been actual differential continental displacement. These generalizations, when based upon the individual and smaller mountain ranges, are not impressive, but when one begins to consider the Cordilleras of the United States, with their present width of more than 1,000 miles, the question looms large as to how much western California has moved to the east. No one has yet figured this out. Furthermore, when one turns to the Alps and is told by the best of authorities that their present width of some 150 miles was originally 500 and perhaps 625, which means that their southern limit has moved from 350 to 475 miles to the north, he begins to remember the statement of Galileo in regard to the earth: “And yet it does move.” Even more impressive are the statements of Termier regarding the mountains of central Asia, which have a combined present width of 1,845 miles from north to south, but which originally had an estimated width of 3,600 miles. In other words, the foreshortening may have been of the order of 1,800 miles. Accordingly, we are obliged to conclude that the continents do actually move extensively, but so slowly that it has taken Asia many hundred million years to accomplish the previously mentioned movement. But do these movements mean that the whole, or even parts, of the granitic sial have moved horizontally as extensively through the basaltic sima as postulated by Wegener? The writer is not the one to answer this question, but he feels that we must be open-minded toward at least some unknown amount of continental displacement. Nevertheless, like Termier, he is “struck less by the mobility, than by the permanence” of the earth’s greater features.

Schuchert was willing to accept that India had moved 2900 kilometers (1,800 miles) during the formation of the Himalayas. But to him this was not comparable to continental drift. He would not accept that South America and Africa had moved 4500 kilometers away from each other.

SCHUCHERT AND WILLIS JUST ABOUT WALK ON WATER (1932)

In 1932, Charles Schuchert was 74 years old, fully active as professor emeritus at Yale University. Bailey Willis, 75, was professor emeritus at Stanford. They had not yet been given the Penrose medal, but otherwise they had earned the highest honors that could be given to North American geologists. They were members of the National Academy of Sciences and had each been chosen to serve a year as president of the Geological Society of America. Schuchert had been elected president of the Paleontological Society and Willis president of the Seismological Society.

Since before their important publications on North American geology and paleogeography, they had supported the Dana-doctrine that North America was a model continent, standing more or less alone throughout geologic time. Now they wanted to defend this doctrine against the claim that North America was only a continental fragment that had broken away relatively recently.

Although they were both against mobilism, they were not in agreement concerning land bridges. Schuchert's solution, since his first textbook in 1915, required a wide transverse continent that had connected Africa and South America. It was removed by faulting along the present-day continental coasts. The fossils showed that species of the northern Atlantic (the Poseidon) had never been in contact with those of the southern Atlantic (the Nereis). There had probably been no deep Atlantic Ocean as we know it, until the end of the Mesozoic. Schuchert showed these separate seas quite clearly on his paleogeographic map of 1923 (see Fig. 22). At the end of Mesozoic time, the continental crust fractured and sank to form the deep Atlantic. Willis would not accept the theory of sunken continental crust, for the reasons he had stated in 1910.

To counter some of Wegener's arguments in the 1928 symposium volume, Willis had suggested that there had been narrow land bridges, such as the Isthmus of Panama. If they were very narrow and consisted of oceanic material, they might have sunk without significantly increasing the volume of the world's ocean basins and lowering global sea level.

Schuchert wrote to Willis, suggesting that they cooperate in publishing a paper arguing for the existence and disappearance of ancient land bridges (Oreskes 1999.) They both felt it was important to promote an alternative to mobilism. In the end, they wrote two separate papers, *Gondwana Land Bridges* (Schuchert) and *Isthmian Links* (Willis) that were published together in the *Geological Society of America Bulletin* in 1932. These two papers were put together as a single reprint.

Schuchert and Willis could not agree enough to write as co-authors. They explicitly took no responsibility for the other's paper (Schuchert p. 887, Willis p. 919.) They must have had difficulty enough formulating their own arguments and espousing their own awkward conclusions.

Only deified authors could have gotten those two papers published in the conditions they were in. The papers were full of irrelevant data and discussion. Schuchert's paper was mostly about Cenozoic fossils, the time period younger than the Gondwana land bridge and with very little relevance to it. Willis's paper contained data that was clearly outdated and inaccurate.

The articles were embellished with six expensive folding maps, five of which were in color. But the maps contained no real data or research results, only rough suggestions on simple base maps. Each of these maps could have been published with no loss of information as single-page black-and-white figure in the text.

Schuchert and Willis each produced their own color map of the world in Permian time (Fig. 57 and Fig. 59), although one map should have sufficed. For the base map, they used Goode's homalographic projection, not the standard Mercator projection that Schuchert had used in his textbooks. All map projections distort some features, but Goode's projection splits and distorts the Atlantic and Indian Oceans, which are just the features that should have been of main interest here. The north Atlantic is so badly split that Iceland is shown twice, and so distorted that it is barely recognizable.

Why would they choose Goode's base map? It must have been because the Atlantic Ocean was distorted just according to their needs. The ocean appears most narrow between Africa and South America, right where they needed to convince readers of the sunken Gondwana land bridge. And the projection destroys the similarity of the coastlines on either side of the Atlantic, and the Mid-Atlantic Ridge. Note, in the South Atlantic, how the ridge appears much closer to Africa than to South America, and in the North Atlantic, near Iceland, the ridge cannot be shown at all. Imagine how foolish it would have looked to use the standard Mercator projection with the Mid-Atlantic Ridge perfectly centered, and then ignore the ridge to place a non-existent Gondwana land bridge across it.

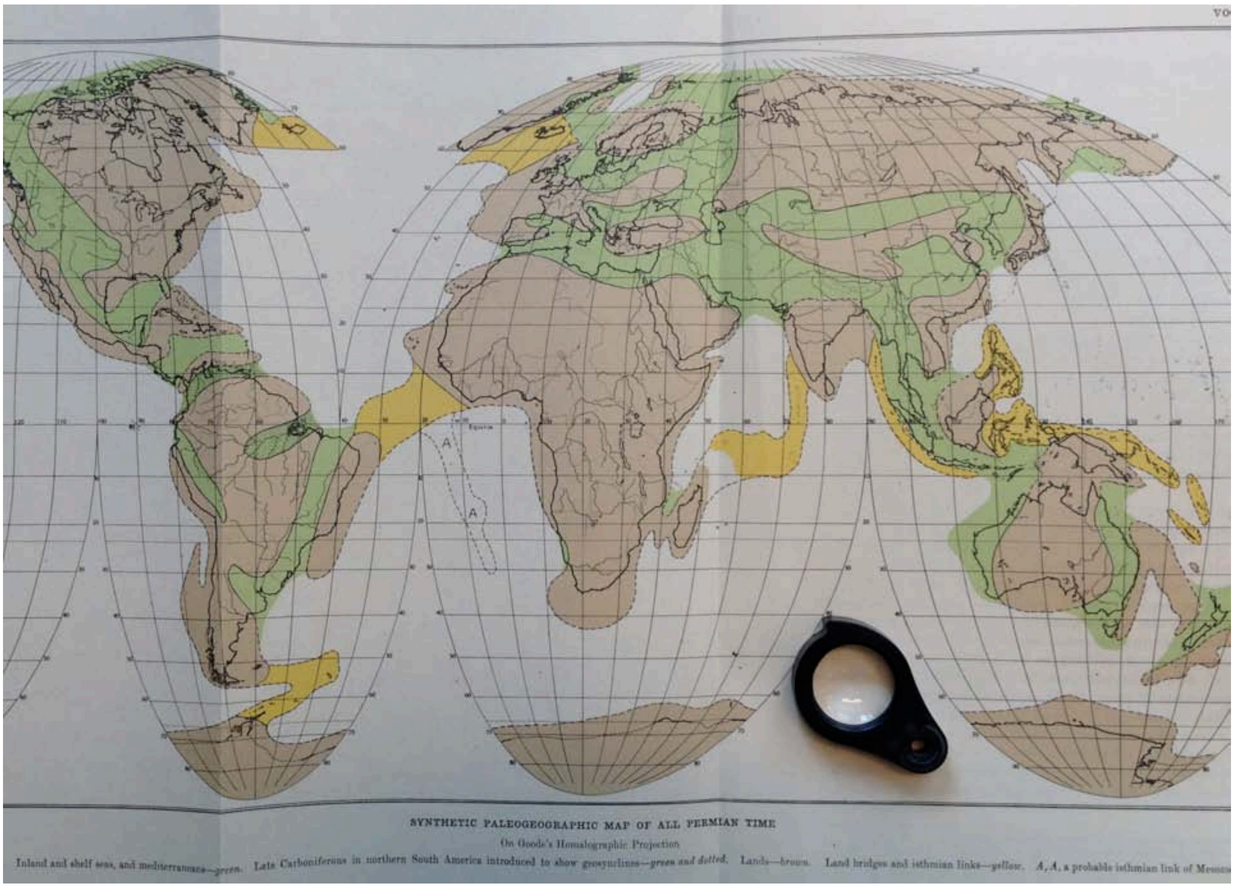


Figure 57. Schuchert's folding color plate showing his Synthetic Paleogeographic Map of All Permian Time. The South America–Africa land bridge looks reasonable on this map projection. From Schuchert (1932).

Schuchert called his map (Fig. 57) a "Synthetic Paleogeographic Map of All Permian Time." It was a synthesis of all Permian time, including early and late Permian. But it was not a synthesis of the paleogeography. It showed no paleoclimate indicators, which Köppen and Wegener had demonstrated to be essential for paleogeographic understanding. It contained very little information to justify the use of colors. There were only five different types of generalized data, which I number here as follows: 1. Oceans are white. No bathymetry is shown, so there is no evidence for the remains of sunken land bridges. 2. Shallow seas, and some deeper “mediterranean” ones, are green. Of these, Schuchert’s Franciscan and Amazon geosynclines occur in South America and do not extend into Africa. We remember this as his "crushing blow to the displacement hypothesis". 3. Lands are brown, with no indications of Permian ice sheets or ice-flow directions, no tropical coal deposits, no desert sand-gypsum-salt deposits. There are no indications of the high Permian fold-belts that existed in Europe, Russia, and North America. The rivers shown are modern, and had nothing to do with the Permian paleogeography. 4. Postulated land bridges or isthmuses are yellow. There are very few of them, and the one across Iceland could not be properly shown.

Neither Willis nor Schuchert seriously evaluated evidence for where the land bridges might have been. By default, this job went to Willis. He explained (1932, p. 919) that twelve years earlier he had drawn some isthmian links on maps that he obtained at that time, and he now printed these in the present paper. The maps were obviously outdated. They had few bathymetric contours and did not show the depth-sounding data that the contours were based upon. His detailed map of the Atlantic between Africa and South America had barely enough bathymetric detail to show the Mid-Atlantic Ridge. Daly’s modest map of the Atlantic Ocean from his 1926-book (see Fig. 45) had more detail. Willis may have been satisfied with his outdated maps, but the journal editors should not have accepted them in 1932.

Willis showed a brightly colored area that he called the Brazil-Guinea ridge and the Brazil-Guinea isthmus (Fig. 58). The bright color and bold lines he drew made it look like a legitimate feature. He used the same color for the Mid-Atlantic Ridge, or at least for part of it. He left parts of the Mid-Atlantic Ridge uncolored, an interpretation not supported by the bathymetric contours. He did not show sounding data on the map, but mentioned it in this way:

Willis 1932, p. 930.

The existence of the Brazil-Guinea ridge is indicated by soundings, of which a number are spaced in a haphazard fashion, but others range in three lines, at intervals of 30 to 60 miles within each line, and thus fairly well define the deeps and shallows. The depths on the ridge vary in general from 10,000 to 12,000 feet below sealevel. Those in the adjacent deeps attain 18,000 feet over wide areas. The general relief of the ridge above the basins thus ranges from 6,000 to 8,000 feet. It is noteworthy, however, that in certain instances volcanic peaks rise to or above the ocean surface, as in the islands of Fernando Noronoha, 200 miles off the Brazilian coast.

This type of description may seem geological and quantitative to congenial readers, but it serves no useful purpose. This map and his description of it are typical of the style of this article, which I would characterize as 35 pages of scientific pretence. Of his five folding plates, only one was published in black-and-white. It was the map showing the Isthmus of Panama, which was convincing without the use of color.

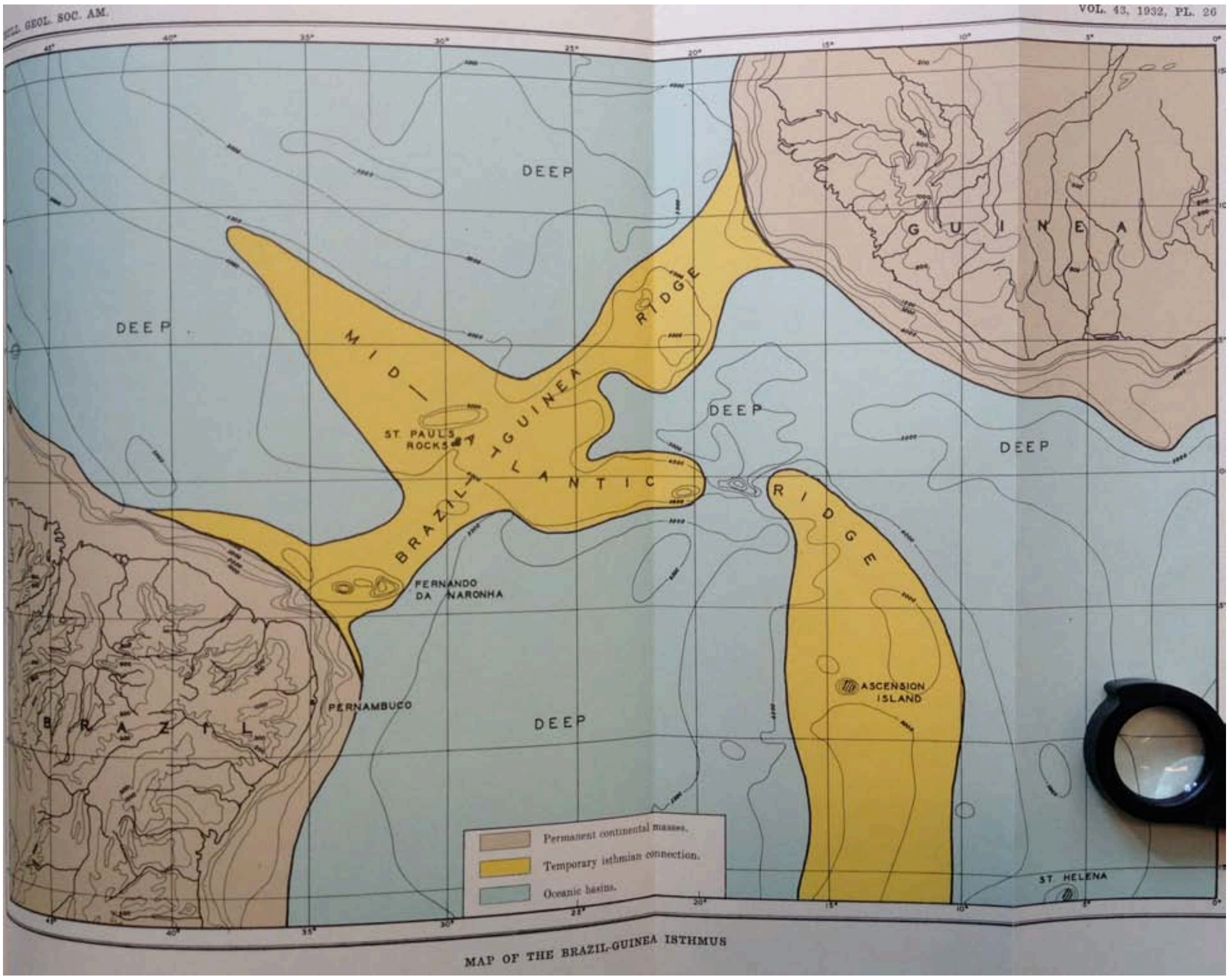


Figure 58. Willis' color folding plate showing outdated bathymetric data, and the suggested isthmian land bridge between South America and Africa. From Willis (1932).

In their maps, one can quickly see how Schuchert and Willis manipulated their evidence and presentation to support their conclusions and to avoid counter-arguments. They manipulated their written text as well, with distortions and omissions, but analysis of it here would be tedious (for some additional details, see Krill 2011c). I will mention only a few points.

They would not consider the mobilism hypothesis; they were only dealing with fixism theory. In Schuchert's view, the theory of continental drift was no longer a hypothesis after the 1928 AAPG volume. It could be eliminated in a single sentence:

Schuchert 1932, p. 878.

Those who lean toward the Wegener theory of continental drift as a possible explanation for the present and fossil distribution discussed in this study are referred to Hoffmann (1925), who rejects the theory as raising more difficulties than solutions in explaining the present distribution of life; and, for the paleontologic and some of the geologic difficulties, to Schuchert (1928).

Citing Hoffmann's paper here was a bluff. Schuchert's North American readers would not consult that German-language article. Had they done so, they would have found that in Hoffmann's view there were not two but three hypotheses: *Permanenztheorie*, *Brückentheorie* and *Verschiebungs-* or *Trift-Theorie.* (Here we see the German term for drift-theory, a term that Wegener never used.) Hoffmann preferred the bridge-theory, but noted that most evidence for this theory fit also the drift-theory. Finally he concluded that geologists and geophysicists, not paleontologists like himself, would have to decide if Wegener's theory is realistic.

Willis also quickly eliminated the theory of continental drift, although it is not clear why. The theory was "not here under discussion", just as the permanence of ocean basins was outside the category of debatable questions in 1910:

Willis 1932, p. 930.

BRAZIL-GUINEA RIDGE

The existence of a land connection between South America and Africa in late Carboniferous and Permian time is demonstrated by evidence of faunal and floral migrations that is generally accepted and has given rise to the theoretical Gondwana continent and also to the theory of Continental Drift. The latter theory is not here under discussion except in so far as the setting up of an alternative may affect it. But no paleogeographic study of Permian conditions can disregard the fact that the south Atlantic was spanned during that period. Our fundamental thesis being that continents and ocean basins are permanent features of the earth's surface, we may consider the fact of former intercontinental connections as a critical test.

Willis understood that Permian climate was the best test of the positions of continents. On his world map, he was more daring than Schuchert, willing to plot some Permian glacial data. But he did not present the data fairly. He showed the huge Indian ice sheet as only two narrow stripes, and the Australian glaciation as only coastal features. He showed the African ice sheet as flowing only southward, as if from a point source. All experts agreed that these continental glaciers must have extended further than the existing deposits. But to uninformed readers, his discussion seems well founded:

Willis 1932, p. 942.

CLIMATIC TEST OF ISTHMIAN LINKS

Climatology offers a crucial test of the validity of any hypothetical distribution of lands and seas during past ages. The rotation of the globe and the general circulation of the atmosphere constitute a group of conditions from which local variations develop according to the distribution of land and sea. The general laws governing the circulation of atmosphere and oceanic currents are known and may be applied to determine the probable conditions of temperature and humidity for any geographic arrangement. We may apply this test to Gondwanaland and the isthmian links, respectively.

The test is indeed a critical one, since it involves explanation of the fact that the four continental masses projecting into the southern oceans were glaciated during the Permian, even within the tropics. During the same general lapse of Permo-Carboniferous time the Arctic enjoyed a climate consistent with plant growth adequate to accumulate peat, which has become coal. The conditions were undoubtedly both extreme and exceptional.

On Arldt's maps of Gondwanaland the southern border of the continent is drawn approximately along latitude 35 degrees south, and the land covers the tropics throughout 180 degrees of longitude. [... Three sentences deleted here.]

A different solution to the problem of Permian glaciation was attempted by Wegener (Alfred Wegener: The origin of continents and oceans, English Transl., 1924, p. 100.), who grouped the continents closely around southern Africa and shifted the pole to latitude 45 degrees south, longitude 50 degrees east, southeast of the Cape of Good Hope. Coleman (A. P. Coleman: Glaciation and continental drift. Geogr. Jour., vol. lxxix, March 1932, p. 252-255.) has pointed our that under this rearrangement of land and sea there would be no source of moisture for snowfall, and Siberian lack of glaciation would prevail.

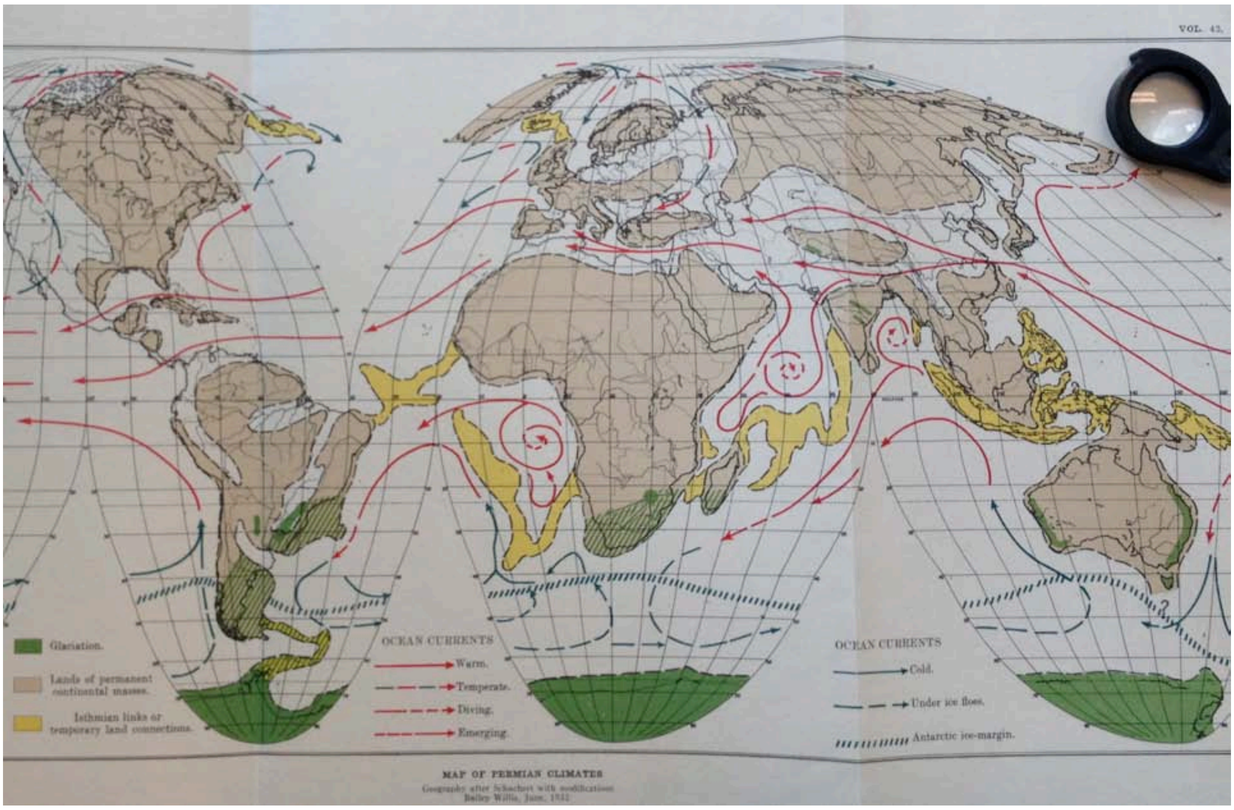


Figure 59. Willis' color folding plate showing his Map of Permian Climates. Note the distortion of the Mid-Atlantic Ridge. Continental glaciation of India is denied; it should have been green. From Willis (1932).

Willis cites A. P. Coleman (1932) in the paragraph above. In that paper, Coleman repeated the same evidence and interpretations as in his 1926-book. He again stressed the fact that the tillites of India could not be attributed to local mountain glaciation. These deposits were part of a huge continental ice sheet that existed at low elevation. Boulders transported by the ice were not local, but had been carried for 750 miles from their source. Willis cites that paper and surely knew this. For seventy years fixists had struggled with the evidence of widespread continental glaciation in India. Willis did not struggle with this evidence, but simply denied it. He made it sound like the deposits were from mountain valley glaciers:

Willis 1932, p. 950.

The breadth of the area between the two belts is 350 miles. It has been assumed that it also was covered by the deposits and that the ice-sheet was of continental proportions, but there seems to be good reason for not exaggerating the probable facts. If the sedimentary terrane was formerly continuous between the two basins, its volume, the area of erosion, and the glaciation, were at least five times as great as the now remaining portions. This constitutes a group of grave improbabilities. On the other hand, the position and relations of the two troughs are entirely consistent with the suggestion of a tectonic origin – synclines or fault valleys – and the preservation of the inlaid sediments in the deeper sections.

This misrepresentation apparently drew no protest from Schuchert or Coleman. They would not correct Willis' errors. Neither would the editors, who gave Willis free reign in this article. Narrow isthmian links could only solve a few of the geologic and paleontologic problems, and there was a wealth of data that could not be satisfied by that model. The two articles *Gondwana Land Bridges* and *Isthmian Links* were written for geologists who wanted to believe that fixism was a viable alternative to mobilism.

As I analyze Willis' 1928- and 1932-papers and compare them with his 1929- and 1934-textbook *Geological Structures*, I have no doubt that he understood mobilism to be basically correct, but had decided to deny it. In his books he avoided discussing the obvious problem of great horizontal movements that caused mountain ranges. This is a glaring omission. One could say that he was avoiding mobilism and postulating isthmian links, essentially proving the false to be true.

Another prominent fixist at this time was Sir Harold Jeffreys, a mathematical geophysicist who also disputed Wegener's theory. Even though Willis and Jeffreys were arguing the same side, Willis did not like Jeffreys' style. When Willis went to England and met Jeffreys for the first time, he wrote the following report back home to his wife, who was coauthor of his textbook:

Willis 1929, in Oreskes 1999, p. 87.

Jeffries (sic) is not at all the man I had expected to meet… not an honest fighter, but a shifty opponent. He did not once look me in the eye. He made no answer to the openings…I gave him to discuss controversial topics. "He's a great mathematician." Well, I guess the devil is also, but he, if he proved the false to be true, would do it charmingly and like a gentleman.

Jeffreys was not a geologist and maybe he did not understand the geological evidence for mobilism. But Willis certainly did, and he was proving fixism to be true, charmingly and like a gentleman.

SCHUCHERT and DUNBAR'S *HISTORICAL GEOLOGY* (1933)

Schuchert produced a new edition of his textbook *Historical Geology* in 1933 together with his Yale colleague Carl O. Dunbar (1891-1979). Here was an opportunity to start afresh and tackle difficult problems, as Schuchert had done in the first edition of this book in 1915. Students should have a revision of the paleogeographic maps and a completely rewritten text according to a new point of view:

Schuchert and Dunbar 1933, p. iii.

PREFACE TO THE THIRD EDITION

The first edition of this textbook appeared in 1915, and a second in 1924. The reception given to both, and the extent to which they have been adopted by American colleges and universities, have been gratifying. There has been an oft-expressed desire, however, for a text somewhat less advanced and less technical in its point of view. As the time for the third edition approached, therefore, the original author invited his junior colleague to join him in a revision of the book that should be expressly designed for the beginning student. The junior author has undertaken this task after fifteen years of experience in teaching Historical Geology, during which time the senior author has devoted himself mainly to original research, to the evaluation of new discoveries as they appeared in the literature, and to the revision of his paleogeographic maps made necessary by the acquisition of new knowledge. This united effort has resulted in a considerable rearrangement of the subject matter, a complete rewriting of the text according to the new point of view adopted, and the replacement of the majority of the illustration by new ones.

The new Chapter 1 sets the stage for the subject of Historical Geology. As you read this short chapter, imagine how satisfying it must have been for new students to study geology using this well written textbook. I interrupt between paragraphs, to make a few comments.

Schuchert and Dunbar 1933, p. 1.

TEXTBOOK OF HISTORICAL GEOLOGY

CHAPTER 1 HISTORICAL GEOLOGY

“To him who commands time nothing is impossible” – HESIOD

Physical Geology deals with the architecture of the outer shell of the Earth, and with the geological processes that have operated to mold it. Historical Geology, on the other hand, concerns itself with the results achieved by these forces during the past geologic ages, presenting in chronologic order the procession of important changes through which the Earth has passed. It is a fascinating study, including not only the physical history of the Earth from the time of its solar origin, but the orderly appearance and evolution of all its life as well. It is, in other words, as much a biological as a geological science, bringing together, into a connected whole, facts from sources as diverse as Structural and Stratigraphic Geology, Paleontology, Biology, Oceanography, and Astronomy. Perhaps more than any other branch of the geological sciences, Historical Geology leads into Philosophy, in the search for the meaning behind the story it presents.

[In the next paragraph, note that Schuchert and Dunbar point out that the geological record is imperfect, and strong conclusions cannot be made from lack of evidence in historical geology.]

Historical Geology carries the history of the Earth back of the human record, through probably two thousand million years whose only annals are written in the rocks and in the fossils which they contain. This geologic history is, however, but an imperfect chronicle, abounding in omissions or alterations of record, due to erosion or deformation or metamorphism. Probably not even all the grander features, such as great mountain making or recurrent spreading of the seas, are yet known, and relatively little regarding the detail of the formations and faunas, despite a century or more of study by geologists and paleontologists in all lands.

The keynote of all geologic history, whether of physical events or of life, is *change*. On the physical side, the Earth has, we believe, cooled from a molten to a solid state. As it solidified, the water vapor of its heavy primal atmosphere condensed into rain and fell upon the hot crust, rose as steam over and over again, and finally came to lie in the depressions and to fill the ocean basins. Geologic time, properly speaking, began when the Earth had a cold exterior, and when the rain and wind commenced their ages-long task of wearing down the high places and transporting the débris into the low ones.

The oceans have always been where we see them now, but they appear to have grown repeatedly larger and deeper, while the continents have lost parts of their marginal areas and have attained from time to time higher average levels. When the oceanic basins subside, it follows that the continents must stand higher above sea level, though they have not risen at all. The added gravity and the resulting greater pressure then cause them to flatten down through continental creep. Through crustal movements and continental creep the oceans are permitted to spread repeatedly over the continents as shallow seas, and they are made to spill more widely through the added land wash that the rivers transport into them. The floods are as often withdrawn by the recurrent deepening of the oceans, but *there never has been a general interchange in position between the continental masses and the basins of the oceans.*

[The above paragraph summarizes the doctrine of fixism. It begins "The oceans have always been where we see them now" and it ends with a similar statement, in italics. This was Schuchert and Dunbar's version of Dana’s doctrine of permanence, "Continents always Continents." It belonged here in their Chapter 1, and was worthy of italics.]

Grand ranges of mountains have been raised many times, only to be degraded little by little and spread out as sheets of sediment over the bottoms of the adjacent seas. “Rocks fall to dust and mountains melt away.” The original continents were probably larger than the present lands, which have suffered from the repeated founderings of their margins into the ocean depths. The oceanic basins, in other words, have grown larger at the expense of the original lands.

Just as the surface of the Earth is in a continuous state of slow change, so also is its atmospheric envelope. When the lands are moving and rising into mountains, igneous activity is greatest; deep-seated magmas are injected into the Earth’s crust, lavas reach the surface, and new water vapor and gases are added to the atmosphere.

The history of life has been a drama of endless struggle against this ever-changing environment. Life probably began in the seas soon after they were fit for habitation. For long eons it remained in the primitive form of minute, soft-tissued, free-living organism, unsuited to preservation. Its early history is, therefore, obscure. Eventually it deployed into an astonishing variety of marine invertebrates and aquatic plants before the lands were peopled by any forms of life. From some of these spineless denizens of the sea sprang the ancestral fishes, which later made their way into the streams. Certain of these were eventually trapped in the rivers of arid regions, where recurrent droughts, with the inevitable stagnation of the water, led to the habit of gulping air, which finally transformed the swim-bladder into a lung, freeing them from the aquatic environment and preparing them for the conquest of the lands. Thus, the first sprawling ancestors of the higher land animals made their appearance by the middle of the Paleozoic era. The long upward struggle toward higher types of life is recorded by abundant fossils in the younger rocks, and forms one of the major themes of the story of Historical Geology.

After this elegant introduction, teachers and students were surely inspired to use this book. The Preface mentioned a revision of paleogeographic maps and a complete rewriting of the text. But revision and rewriting did not apply to the parts concerning fixism. Those parts were essentially the same as the second edition. And they did not support the new isthmian-links hypothesis of Willis. Schuchert and Dunbar were still forced to claim that huge amounts of continental crust with their mountain ranges were connected across the Atlantic and then sank to ocean depths. But instead of Eria being “The Great Northern Transverse Continent” it was now reduced a bit, to "A Great Northern Land Bridge."

Schuchert and Dunbar 1933, p. 209-210.

ERIA, A GREAT NORTHERN LAND BRIDGE

Throughout Devonian time North America was apparently connected to Europe by a land bridge which later subsided beneath the north Atlantic. This hypothetical land has been called *Eria*. Although the evidence for such a land bridge is circumstantial, it is none the less convincing.

The Acadian folds cross Nova Scotia and Newfoundland and strike along a great circle directly toward Ireland. The present ragged coast lines of Acadia and Newfoundland show that these mountain folds have been broken off and must originally have extended farther east. Likewise, the Caledonian ranges formed in western Europe at the close of the Silurian follow the axis of Scandinavia but curve westward across Scotland and Ireland to strike directly toward the Acadian area. These folds have also been broken off at the west. During Devonian time, moreover, the “Old Red” sediments, which reach such a vast thickness, were coming chiefly from the northwest into Ireland and Scotland from highlands that have since become submerged in the Atlantic. In short, there is clear structural evidence of land extending northeast from the Acadia area and southwest from Britain, and the folds on opposite sides of the present ocean are almost precisely in line. Conclusive evidence that these two lands met is to be found in the land plants and fresh-water animals preserved in the Devonian rocks of the two regions, which are so much alike on both sides of the Atlantic that it seems clear they were free to migrate across an easy land bridge. How wide the bridge may have been is now impossible to determine, but it seems probable that the shallow bank between Britain and Greenland, from which the island of Iceland rises, may be a vestige of this old land.

Large amounts of continental crust and mountain areas clearly had broken off, and must have sunk. This was a wide land bridge, not an isthmus. And fresh-water animals migrated "across an easy land bridge." Easy, in this context, means wide. Fresh-water animals could not have migrated along an isthmus, because no rivers could run the length of it. Of course, Wegener’s full-width land connection would have been the easiest.

On the next page there is a footnote: in the Lower Devonian of South Africa “there is much to indicate the presence of glaciers moving from west to east.” In other words, continental glaciation in South Africa was not limited to Permian time. Schuchert and Dunbar could accept South African continental glaciers in Devonian time and in Permian time. But they never accepted the widely held interpretation that the main glaciation took place in the Carboniferous, between these two periods. And they did not mention the Triassic African glaciation that Coleman (1926) had discussed (see p. 104.)

In the chapter on the Upper Carboniferous, they avoided the problems of simultaneous tropical forests and glaciations. Instead of normal climatic zonation throughout geologic time, they tried to make the extreme climates either local features or short-lived events. They ignored the problems of four months of darkness on Spitzbergen, and the fact that tropical-climate fossils there are not limited to Carboniferous time. Here is the relevant part of the section on Pennsylvanian (Upper Carboniferous) climate:

Schuchert and Dunbar 1933, p. 257-258.

There is also much evidence that the climate was warm, even to high latitudes, during much of the period. The mere presence of abundant vegetation is no evidence, for it is well known that the most extensive modern accumulation of peat is in subarctic regions where slow growth is more than counterbalanced by slow decay; but the *character* of the Carboniferous vegetation indicates a lack of freezing winters, at least in the lowlands where the plants are preserved. The trees, whether tree ferns, seed ferns, cordiates, or the great scale trees, bore succulent foliage of almost unprecedented luxuriance. The leaves were not merely large but their texture indicates rapid growth under warm, humid conditions. For example, the very large size of the individual cells, the arrangement of the stomata (breathing pores), the smoothness and thickness of the bark, the presence of aërial roots, and the absence of growth rings in the woody trunks are all features of significance. One of the foremost paleobotanists of our times has concluded that "the climate of the principal coal-forming intervals of the Pennsylvanian was mild, probably nearly tropical or subtropical, generally humid, and equable."[3]

The animal life of the time also seems to support this view. Insects, for example, attained an extraordinary size and, so far as known, averaged larger than in any other period of Earth history. Since it is well known that the modern orders of insects have their large representatives in the tropics, with smaller and smaller species in regions of more rigorous climate, the significance of the Pennsylvanian insects is obvious. To this may be added the fact that at certain times during the period corals were able to thrive in great abundance and to form reefs as far north as the arctic islands of Spitzbergen (lat. 78° N.) The presence of these ancient reefs in the sea cliffs of a land now treeless and ice-covered speaks eloquently of the climatic contrast between the present and the Pennsylvanian age in this region. The exceptional abundance of the large fusulines in the limestones of the northern hemisphere, and even as far north as Spitzbergen, seems to have a significance like that of the insects.

Nevertheless, we must not assume that there were no exceptions to the picture we have drawn of widespread warmth. Great changes have occurred in the temperature of most of the lands in the few tens of thousands of years since the Pleistocene ice age. During a period like the Pennsylvanian, of millions of years' duration, there may have been important fluctuations in the world climate.

The extensive continental glaciation of India, South Africa, South America, and Australia, which some geologists attribute to Late Pennsylvanian time, we regard as of Permian age. It is discussed in the next chapter.

Without a doubt the polar regions were cooler than those of low latitudes and it may well be that the mild climate of Spitzbergen was due to the local influence of a warm ocean current which then streamed into the Arctic. Nevertheless, the evidence for mild climate is so widespread that we can not avoid the belief that in general the Pennsylvanian was an exceptional period in the climatic history of the Earth.

This Carboniferous chapter of the book focused on the tropical conditions. In the Permian chapter, the word *tropic* was never written, and the only coal mentioned was in north China and eastern Australia. It was covenient, but not valid, to separate Pennsylvanian and Permian climates in this way.

A map showed early Permian glaciation, but it did not show early Permian deserts. Some problematic facts are mentioned, such as northward-flowing continental glaciers in equatorial India, but the facts were not put together in any meaningful way. Here is how climate is described in the Permian chapter:

Schuchert and Dunbar 1933, p. 280-284.

CLIMATE

It was but a natural sequel to these and other great changes in the physical geography that climatic extremes were introduced. The extensive withdrawal of all the epeiric seas during the Permian removed one of the chief agents in stabilizing the temperature and providing moisture in the winds that crossed the interiors of the continents. The enlarged lands must have interfered greatly with the spread of warm ocean currents toward the poles, and this was particularly so in the southern hemisphere, where a land bridge (Gondwana) (p. 292) crossed the Atlantic and another isthmus probably connected Australia and Antarctica. At the same time each lofty mountain range which stood athwart a prevailing wind belt must have increased precipitation on the windward side and reduced it on the lee. The extensive highlands were chilled by their altitude. Under these conditions local evidence of the most diverse extremes of climate is not the paradoxical but the natural thing.

Deserts. – During the Permian and the following Triassic, deserts were probably more widespread than at any other time save the present. The dune sands (Fig. 166), the red beds, and the widespread deposits of salt and gypsum in the central and western United States indicate a vast interior more arid than the present Great Basin. The salt beds that stretch from Kansas to New Mexico have been estimated to include 30,000 billion tons of salt and would require the evaporation of more than 22,000 cubic miles of sea water with a salinity like that of the modern oceans. This precipitation, it must be remembered, occurred while only a part of the Permian formations were being deposited. It produced the largest salt deposit known.

Severe aridity also spread over central and western Europe at this time. Red beds are of wide extent in England and Germany, reaching northeastward into the Urals of Russia. Upon these in Germany lie the great salt deposits of the Stassfurt region, where potash salts occur with the sodium chloride, indicate complete desiccation.

In South Africa the Permian deposits are largely non-marine red beds, though here they are not associated with salts or dune sand, and, on the contrary, include a wonderful assemblage of fossil reptiles.

While aridity seems to have prevailed over much of the United States and central Europe, there was abundant rainfall in some regions, notably north China and eastern Australia, where important coal fields lie in the Permian rocks.

The Permian Ice Age – At times during the Permian period great areas in the southern continents were covered with ice sheets. South Africa has the most spectacular evidence of glaciation, for here the ancient Dwyka tillite at the base of the Permian sequence includes large faceted boulders and rests upon the heavily scored and polished floor over which the ice moved. The ice cap covered practically all of southern Africa up to latitude 22°S, and also spread to Madagascar (which was then part of the continent.) There were three or four centers of movement, but the greatest seems to have been in the Transvaal, which then was a plateau from which the ice moved southwestward for a distance of at least 700 miles. The tillite reaches a thickness of less than 100 feet in the northeast but increases to 2000 feet in southern Karroo.

Australia was likewise the scene of extensive and repeated glaciations, the ice apparently moving northward across Tasmania, Victoria, and New South Wales. A series of sheets of tillite are interbedded in some 2000 feet of Permian sediments which have at least one horizon of commercial coal. *South America* bears evidence of glaciation in Argentina and southeastern Brazil, even within 10° of the equator. In the northern hemisphere, peninsular *India* within 20° of the equator was the chief scene of glaciation, with the ice flowing north; in the Salt Range on the southern flank of the Himalayas the thick Talchir tillite underlies the marine Permian formations.

In the northern landmasses, on the other hand, evidences of glaciation are very restricted. The only certain evidence in North America is a small deposit near Boston, the Squantum tillite, which may be the result of a valley glacier from the rising Appalachians. Striated boulders in conglomerates have been found in the Permian beds of Prince Edward Island and in different places in Alaska, but the proof of their glacial origin is not conclusive. The same may be said of doubtful occurrences in England, Germany, south Russia, and central Africa.

It is almost certain that the Permian ice age, like the recent one of Pleistocene time, was a relatively short stage in the duration of the long geologic period. [Only Shuchert seemed to feel that this short duration was almost certain.] The three widely spaced repetitions of glacial beds in the thick Australian sequence may indicate recurring glacial stages restricted to this continent. In any event, the main glacial deposits are in each region confined to a limited horizon of the older Permian rocks. The presence of large reptiles in the higher Permian red beds of South Africa would suggest a mild-temperate climate without freezing winters at the time when they lived.

The exact *time of the ice age* is difficult to prove in any of the glaciated regions. Possibly it was not the same in all the continents, though it would seem more probable that such extensive refrigeration must have affected the temperature of the whole world at once. It now appears that the best-dated glaciation occurred before the middle of the period, but long after the beginning of Permian time.

The *causes* of glaciation constitute an enigma that will be discussed in Chapter XIX. The most remarkable feature of the Permian glaciation is the *distribution* of the ice sheets. They were chiefly in the southern land masses and in regions which now lie within 20° to 35° of the equator. This circumstance more than any other has lent attractiveness to the belief in "continental drift." If the southern continents were united to Antarctica until after Permian time, the glaciation may not have spread into low latitudes. A later "drift" of these continents toward the north would account, far more easily than any other means yet postulated, for the present distribution of the glacial deposits. But this premise itself is still in the realm of speculation!

The paragraph above is the only mention in the entire book of the theory of continental drift. It was labeled a *belief* instead of a hypothesis or a theory. No additional evidence in support of displacement was given.

Wegener (1922) and Köppen and Wegener (1924) had shown that a proper paleogeographic map of the Permian should include all climate data, not only the glacial deposits. Schuchert had now spent the last 10 years updating his paleogeographic maps, and one might expect him to show the various climate indicators, such as desert deposits and tropical coals. Schuchert and Dunbar were not willing to do this. They reprinted Schuchert's map from 1923, and hinted in the figure caption that it was not satisfactory: "The limits of Gondwana Land are problematical." (Fig. 60). This outdated map must have been an embarrassment to Schuchert, but he felt that an updated map would have been worse.

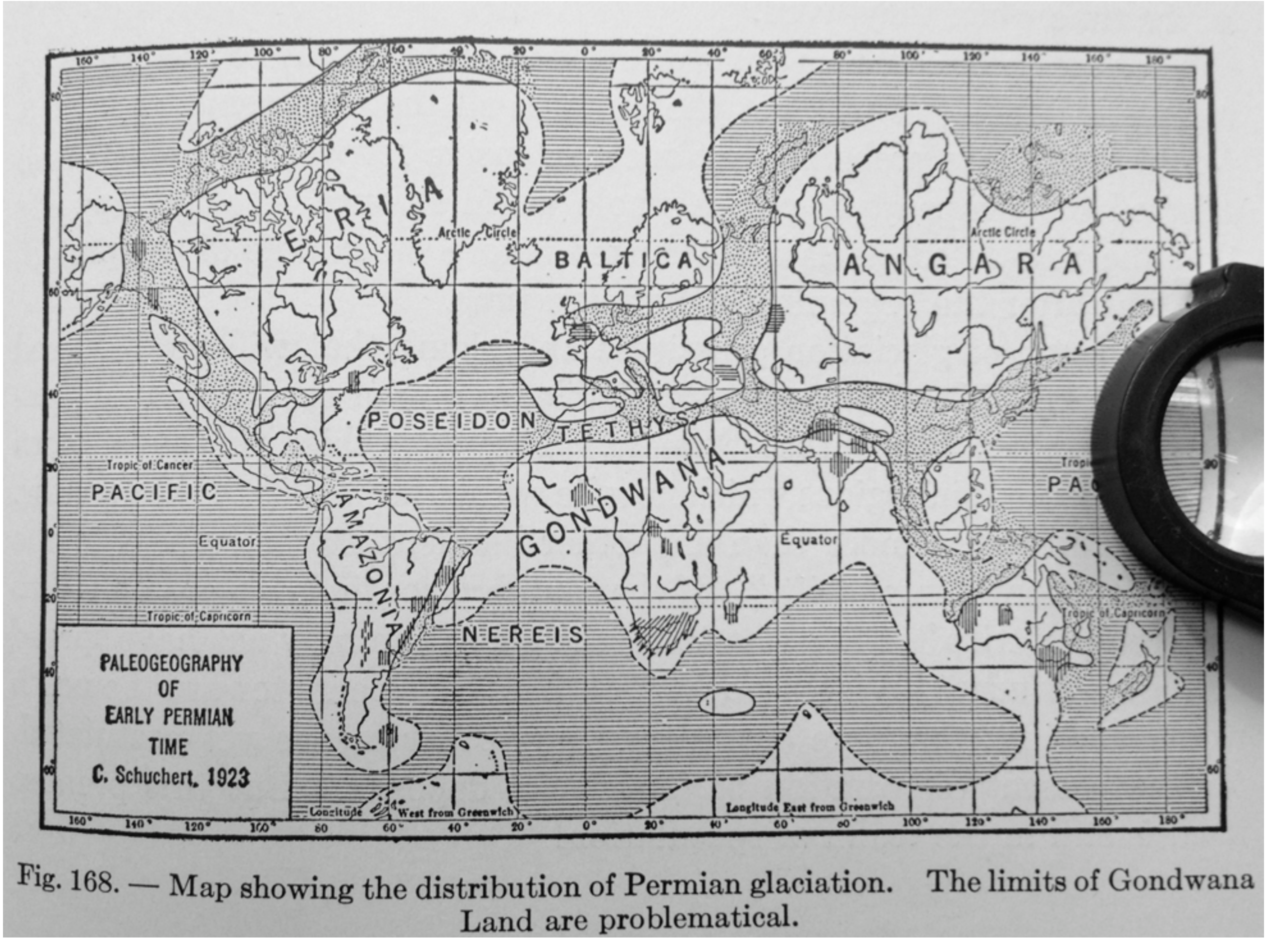


Figure 60. Schuchert’s paleogeographic map of 1923, pressed into service again in 1933. From Schuchert and Dunbar (1933).

Recall that Schuchert had removed the arrows showing glacial-flow directions in India and Australia. Now he would have surely liked to remove the arrows in South Africa so as not to have any arrows at all. But he could not remove those arrows without updating the rest of the map. So Schuchert and Dunbar removed the discussion of glacial flow directions, including Du Toit's detailed map of the South African glaciation. Now students would no longer think about what those arrows in South Africa might represent, or ponder the strange directions of ice flow. The glacial deposits were shown as small discrete patches, and students might easily think of them as the results of high-mountain glaciers.

Schuchert was an expert at connecting the dots, when the evidence led to a result that he wanted to promote. He knew that if he described the glacial features properly, and connected the dots, the displacement theory was the most obvious conclusion. It was best to leave out information and not discuss the most awkward details.

Later in the chapter on the Permian period came the old model of land bridges to explain the curious distribution of certain plant species in the Carboniferous. Schuchert and Dunbar now described the land bridges as narrow isthmuses or even chains of islands:

Schuchert and Dunbar 1933, p. 292-293.

Gondwana Land Bridges – The living animals and plants of the island of Madagascar show so many resemblances to those of Africa that a former land connection is generally recognized. Similar faunal and floral evidence in some of the Paleozoic and Mesozoic formations of India, Africa, South America, and Antarctica strongly suggests transverse land bridges spanning the present Atlantic Ocean from Brazil to Africa and, by way of Madagascar, across the Indian Ocean to India.

The width of the land bridge across the Atlantic is entirely conjectural and its position problematical. Indeed, many geologists deny that it ever existed because they are unable to account for the complete disappearance of so large a land mass into oceanic depths. Nevertheless, there were striking resemblances in the land life of India, Africa, and South America, during several periods previous to the Cretaceous, that seem to demand a connecting land bridge. None of these instances is more convincing than that of the Glossopteris flora, which is so characteristic of the Permian of the southern hemisphere and so much alike in South America, Australia, Africa, India and Antarctica. Since these plants are entirely unknown in North America, they could have reached both Africa and South America only by means of a southern land bridge. The Permian and Triassic reptiles of Brazil also find their nearest relatives in South Africa. Space does not, however, permit other illustrations of similar tenor that have led to a widespread belief in these inter-continental connections.

On the assumption that South America, Africa, and peninsular India were originally broadly connected to form a great transverse continent, this hypothetical land mass was called *Gondwana Land*. The geologists who accept this hypothesis assume that the existing continents were separated later by the breakdown of the intervening areas to form the southern Atlantic and Indian oceans. It now appears probable that these continents were never widely connected but were joined during the Paleozoic and most of the Mesozoic by narrow land bridges like the present one of Central America and the Isthmus of Panama which unites North and South America. These may have been of the nature of island arcs rising as mountain ranges from the ocean floor, at times forming isthmian links between the continents and at other times being reduced to island chains. Land connections appear to have been in existence at times from the early Paleozoic to the middle Mesozoic, when they began to break down, though island remnants and submarine banks appear to have been present up to Miocene time (see Chap. XVIII).

There is no mention here of the alternative theory of continental displacement. One is tempted to quote Philip Lake, as Schuchert earlier had done: "...in his book he is not seeking truth; he is advocating a cause, and is blind to every fact and argument that tells against it." Schuchert and Dunbar had decided that in this 1933-edition they would tell the same story about the history of the Earth as Schuchert had told in 1924 and 1915. Like all geological interpretations, that old story had its weaknesses. But to change the story would be to ruin it. They felt that the task of writing a new story could be left for other authors.

The fourth edition of *Historical Geology* appeared in 1941. Again the book was greatly revised, except for the paragraphs about the Permian paleogeography. The most significant change in that chapter was the removal of the 1923-map. How to replace this map must have been a problem, but Schuchert and Dunbar solved it in a very creative way. They found a base map that showed no Atlantic Ocean (Fig. 61). They could now remove the land bridges across the Atlantic, without the removal being obvious to those who knew the earlier editions. And on this new base map, the glacial deposits could be redrawn, and the arrows removed from South Africa. The Equator was broken and curved, so it was not easily noticed how close the glacial deposits were to the Equator in Africa and South America. They did not show the glacial deposits as ragged remnants of regional glaciation, but drew them as smoothly rounded patches, as if there were no connections between them. They look like they could have been deposited from high mountains, which, of course, did not exist at those places at that time.

Since there was no Atlantic Ocean, one could no longer see the matching coastlines on either side of it. In fact, it is the Pacific Ocean that looks very narrow, almost like a rift. If Satan were really interested in promoting continental drift, as Schuchert had suggested in 1928, he might have used this map to tempt a weak soul to fit the Americas together with Asia and Australia.

Scientists often present their data in ways that lead to their preferred conclusions and discourage alternative interpretations. Such practices usually go unnoticed, but not so easily when the conclusions were incorrect, and when they are still of interest 70 years later.

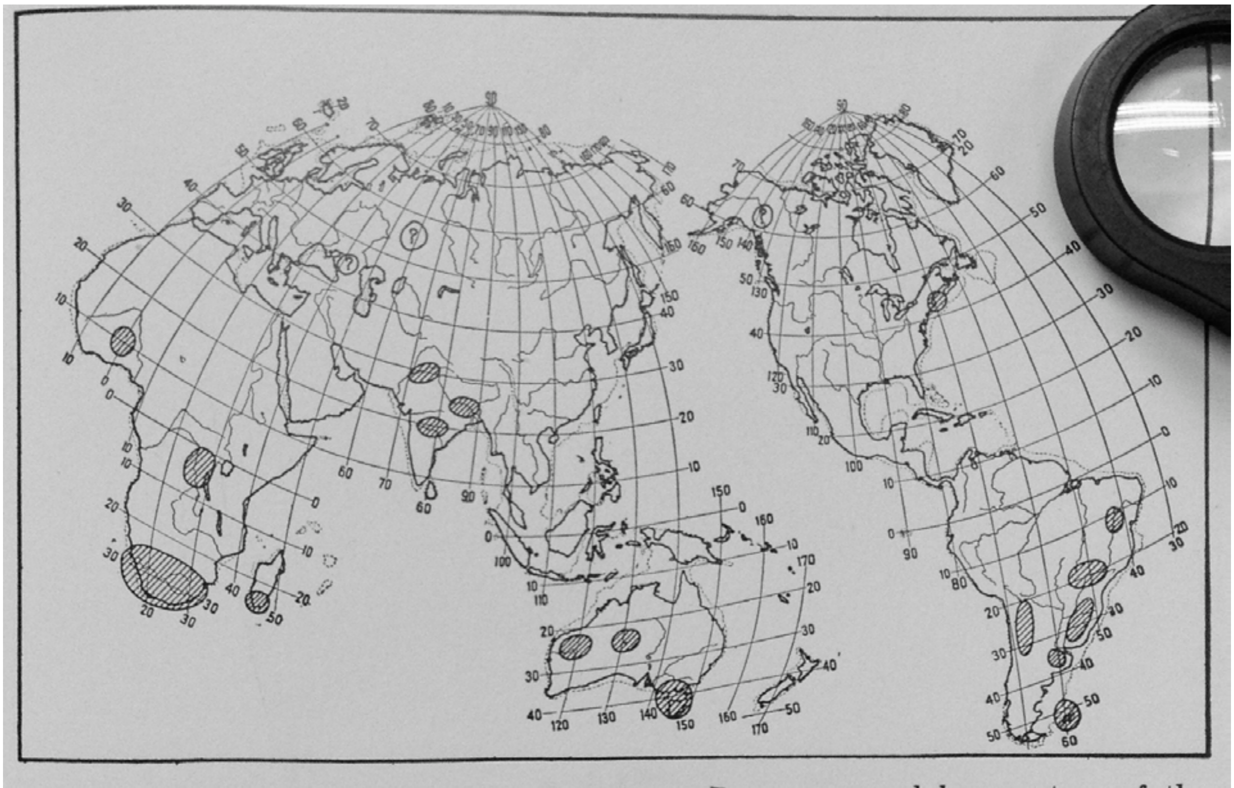


Fig. 187. Distribution of Permian glaciation. Base map used by courtesy of the American Museum of Natural History.

Figure 61. Schuchert and Dunbar found a new base map to illustrate Permian glaciation. The land bridges across the Atlantic would not be missed, because the Atlantic Ocean itself was missing. From Schuchert and Dunbar (1941).

THE MID-ATLANTIC RIDGE AS A BELT OF EARTHQUAKES AND VOLCANOES (1934)

Schuchert and Dunbar had avoided showing the Atlantic Ocean on their map of Permian glaciation. But on most other maps, the match of the coastlines on either side of the Atlantic was still apparent. And the shape of the Mid-Atlantic Ridge also fit these coastlines. The 1934-edition of Longwell's *Physical Geology* included a remarkable map showing the belts of volcanoes and earthquakes throughout the world (Fig. 62). There one sees that the Mid-Atlantic Ridge is characterized by earthquakes and some volcanoes that rise above sea level. This map supports the hypotheses of Molengraaff (1916,1928) and Holmes (1931) that the Atlantic Ocean was opening from the center.

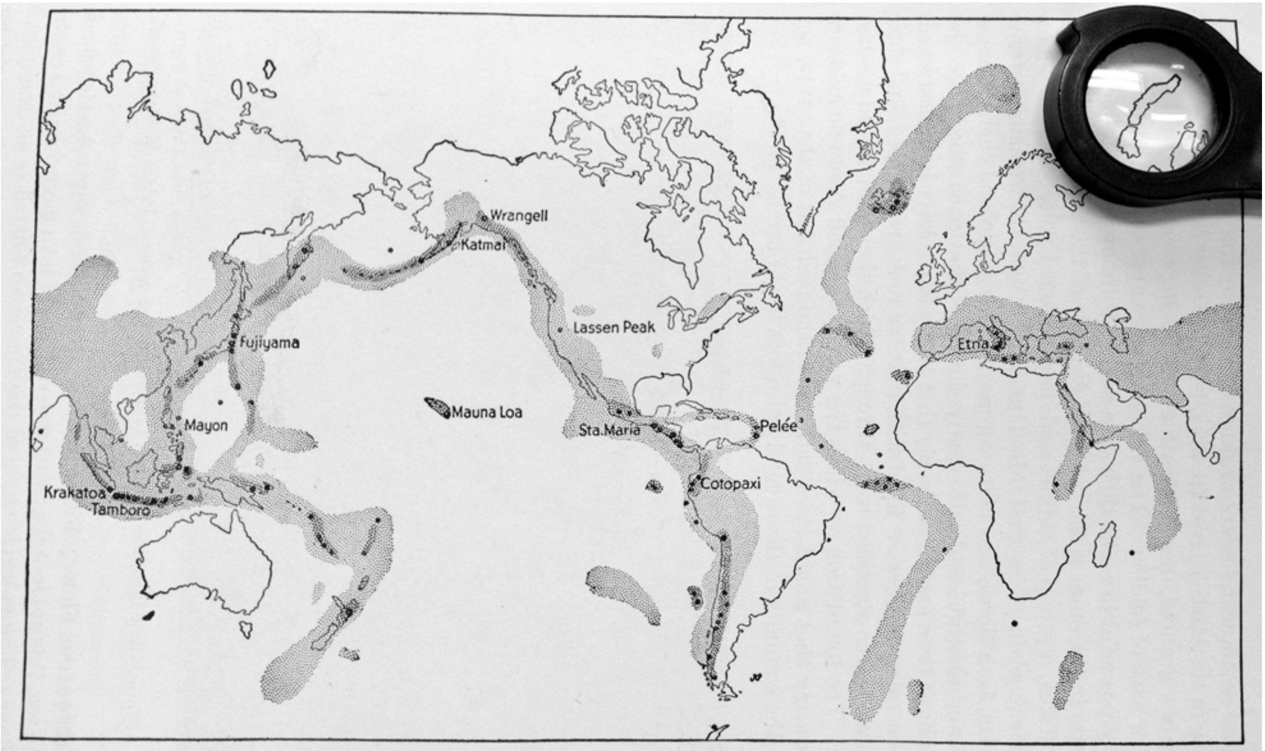


Fig. 182. — Distribution of volcanoes and earthquake belts throughout the world. Active and recently extinct volcanoes shown by small circles. Shaded areas are subject to frequent earthquakes; the darker shading indicates zones of especially violent earthquake activity.

Figure 62. The Mid-Atlantic Ridge is clearly seen as a zone of earthquake activity, but the ridge was not explicitly mentioned in the text. From Longwell et al. (1934).

The chapter on volcanoes that included this map was written by the coauthor Adolph Knopf (1882-1966). He did not comment on the Mid-Atlantic Ridge, and maybe he had not given it much thought. This map was included in both the *Textbook*- and *Outlines*-editions of Longwell's *Physical Geology* from 1934 to 1948.

DU TOIT'S SECOND BOOK ON MOBILISM: *OUR WANDERING CONTINENTS* (1937)

Ten years after his book on African-South American comparisons, Alex du Toit produced a large volume covering the entire subject of continental drift, including its history, the opinions of the geologists involved, the data, and the interpretations. It superseded Wegener's book from 1929 as the most comprehensive work on continental drift.

Du Toit's version of Pangæa was slightly different than Wegener's: the ancient Tethys Ocean divided Pangæa across the middle. Marine sedimentary rocks of Tethys had been stacked up to form the fold mountain belts of the Himalayas and Alps. The southern continental mass was called Gondwanaland and the northern was termed Laurasia.

Du Toit showed an improved map of the Permian glaciations. Northward-flowing ice was now known from the central African part of the "Great Ice Cap". Du Toit's map correctly showed flow directions outward in all directions. It was after this publication that Schuchert and Dunbar removed all such flow-direction arrows from their new map in 1941.

Alex Du Toit understood that the theory of continental displacement would be the unifying theory of geology:

Du Toit 1937, p. 4-5.

An outstanding consequence of the Hypothesis is the orderly and interrelated nature of all associated phenomena. The drifting away of a part of a continent automatically severs its connection with the remainder and with its life, removes it generally to a different latitude and climate, causes it ultimately to come to rest through the crumpling up of its leading margin, induces tilting of the rearward part with possibly far-reaching effects in the way of warping and rifting, institutes a new system of drainages and gives rise to eruption and intrusion of igneous matter which tends towards an intermediate composition at the leading edge and an alkalic one at the rear. Areas now widely separated stand revealed as fragments of greater masses and their observed similarities find their explanation therein and have not to be gratuitously interpreted as due to an extra-ordinary set of coincidences—geographical, stratigraphical, tectonic, biological, and climatic.

Current ideas, on the contrary, regard the continental masses as having existed over great periods as more or less *independent units* save when temporarily connected, and consequently any close relation between their respective histories would not strictly be expectable, though such is flatly contradicted by the evidence. The explanations currently given for the grander features of the globe bristle with difficulties, primarily because of this tendency to regard each mass as a distinct entity. Instead of such units behaving more or less arbitrarily they become under our hypothesis parts of a living whole, each influenced by and reacting upon its neighbours in a definite and orderly fashion throughout geological time. Indeed it is modestly suggested that the *Displacement Hypothesis represents the "Holistic" outlook in Geology.* Furthermore, unlike current views of Earth structure, this illuminating *hypothesis can be tested on the basis of prediction.* Several remarkable deductions in the case of South America and South Africa have been thus verified by field work.

The foregoing advantages being so overwhelmingly in favour of the "New Geology", the critic may, and with justification, ask why the Hypothesis has apparently found so few whole-hearted supporters. The answer is, first, that it cuts at the basis of customary geological interpretations and is hence not particularly welcome, and, secondly, that no forces have so far been, nor according to its opponents can be, invoked competent to move the continents about as supposed.

The first objection is largely a psychological one, and has to be overcome by the marshalling of the relevant data, their skilful analysis and presentation and the closer study of the evidence, wherever such is in apparent conflict with preconceived ideas. Incidentally this would involve the rewriting of our numerous text-books, not only of Geology, but Palæogeography, Palæoclimatology and Geophysics.

The second is admittedly a serious one, but, as Rastall has stressed, is no more weighty than the lack of an adequate recognized cause for past ice-ages, a puzzle that has exercised the ablest minds for about a century...

Du Toit's geological evidence for continental drift was extensive. And he accepted that convection currents in the mantle must be the underlying mechanism. But he did not understand convection processes as well as Holmes had done. In Du Toit's version, the oceans were not involved in the convection, and the convection cells were flat, with almost no vertical component (Fig. 63). To function properly, convection must be driven by vertical density differences, and could not be mainly horizontal as his figure shows.

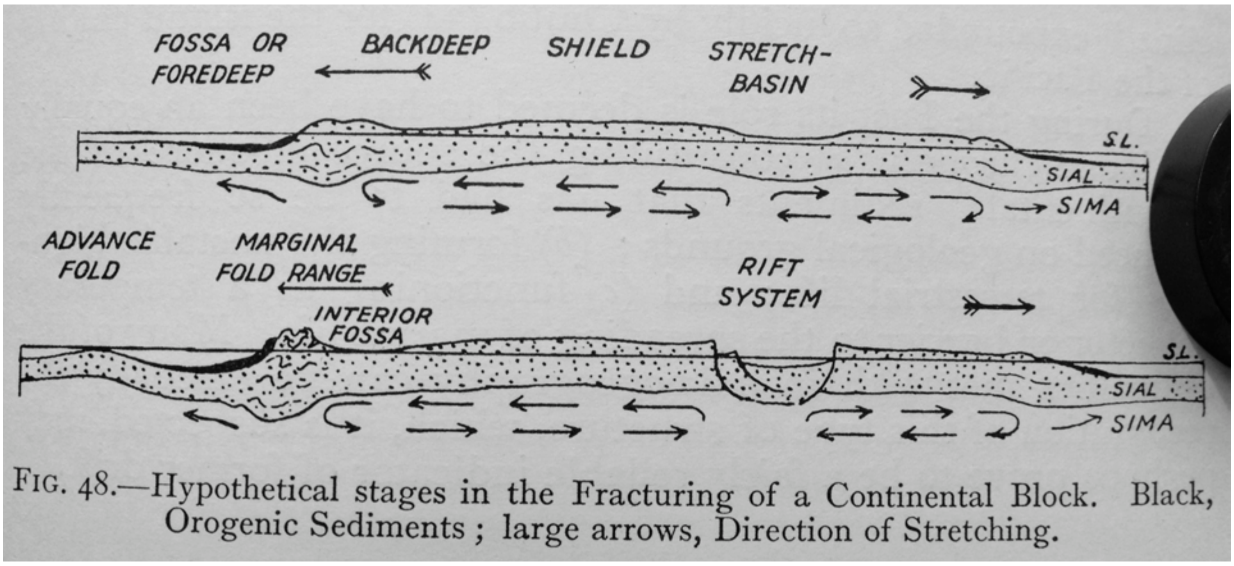


FIG. 48.—Hypothetical stages in the Fracturing of a Continental Block. Black, Orogenic Sediments ; large arrows, Direction of Stretching.

Figure 63. Du Toit's illustration of a convection model. From Du Toit (1937).

The North American scientist who understood convection currents best was David Griggs (1911-1974), a geophysicist at Harvard. He published a paper in the *American Journal of Science* in 1939 that employed theory and experiment to model convection cells. He showed that convection could explain the two great problems of mountain belts: the thick geosynclinal sediments and the horizontal compression. He also explained how convection could generate the mountain ranges around the Pacific Ocean and the deep-focus earthquakes that were known to dip 45° toward the continents. Griggs wrote that he was looking for further geological applications for this theory of convection currents. But he avoided noting that convection cells had been related to the theory of continental drift. The title of his paper was *A Theory of Mountain*

Building, and he steered clear of the suggestions of Wegener, Holmes, and Du Toit. An article in the *American Journal of Science* was not the place to support mobilism.

DISCUSSIONS OF DU TOIT'S BOOK IN THE *AMERICAN JOURNAL OF SCIENCE* (1943)

Recall that Du Toit's first book on mobilism in 1927 was discredited in a book review by editor Schuchert in the *American Journal of Science*. Du Toit probably expected that journal to give the same treatment to *Our Wandering Continents*. He must have been pleasantly surprised when this second book got a very favorable review, signed W. A. Rice (1938) I think this was William Abbott Rice, the grandson of the evolutionist William North Rice, who had removed creationism from Dana's posthumous textbook.

But Du Toit's gratification was short-lived. America's leading vertebrate paleontologist, George Gaylord Simpson (1902-1984) published an article in the *American Journal of Science* that thoroughly criticized Du Toit and this second book on mobilism. It was not a book review, but he mentioned Du Toit by name on 13 of the 29 pages. Simpson had become one of the *Journal's* associate editors after Schuchert stepped down. One could say that America's most influential vertebrate paleontologist was now following in the footsteps of the most influential invertebrate paleontologist.

According to Simpson, paleontologists were practically unanimous in their opposition to mobilism. In retrospect, it is remarkable how confidently Simpson could argue the wrong side of this scientific debate. Here is Simpson's introduction to his paper:

Simpson 1943, p. 1-2.

In the course of repulsing an attack on the Wegener theory, W. A. J. M. van Waterschoot van der Gracht (1928, p. 224) annihilated the paleontological arguments by saying, "There are few subjects where there exists a greater diversity of opinions regarding practically everything than in paleontology." The remark was made in the course of a symposium on continental drift that exemplified greater diversity of opinions than paleontology can offer. Doctor van der Gracht's dictum becomes amusing when it is noticed that on his particular subject the verdict of paleontologists is practically unanimous, almost all agree in opposing his views, which were essentially those of Wegener. For instance in canvassing opinions at some length, Du Toit (1937, chapter II) was able to cite no paleontologists as active protagonists of continental drift and only one as sympathetic with it—this one, Seward, is a specialist on the anatomy of primitive plants and as regards the drift theory he disagrees with a clear consensus of the paleobotanists more immediately concerned with phytogeography (e. g. Berry, 1928; Chancy, 1940).

The fact that almost all paleontologists say that paleontological data oppose the various theories of continental drift should, perhaps, obviate further discussion of this point and would do so were it not that the adherents of these theories all agree that paleontological data do support them. It must be almost unique in scientific history for a group of students admittedly without special competence to a given field thus to reject the all but unanimous verdict of those who do have such competence.

The paleontologist A.C. Seward had caught Simpson's attention and scorn because he had written a favorable review of Du Toit's new book in *Geological Magazine*.

Note how carefully Simpson chose his words. He did not write that paleontological data oppose the various theories of continental drift. His wrote that paleontologists say that paleontological data oppose them. Paleontologists might say this, just as geologists might say it, without having their own data to document this opinion. But there were some very obvious paleontological data that supported continental drift: the occurrence of Carboniferous tropical coral- and coal-fossils currently lying uncomfortably near the North Pole. The fossil tree-ferns have no annual tree rings, and neither they nor the invertebrate animals could have tolerated the months of dark arctic winter. Still, Simpson's statement could be valid if paleontologists were not talking much about these data.

Simpson's specialty was the evolution and distribution of vertebrate animals in Cenozoic time, well after the Mesozoic breakup of Pangæa. The data from his main research actually had very little significance in this debate. The compelling evidence involved fossils before the Cenozoic, and the important fossils were plants and invertebrate animals, not his four-legged vertebrates.

Du Toit simply could not win. His 1927-book was criticized by Schuchert for ignoring fossils. Now his 1937-book was criticized by Simpson for using them. Here is another catch-22 for a mobilist to deal with:

Simpson 1943, p.19.

If Du Toit means to imply that any identity of species on different continents constitutes a closer faunal resemblance than is consistent with present continental positions, then he is flatly wrong. If, as the logical alternative, he means to say that identical faunas have been found on separate continents, then he is even farther from the truth.

No two local faunas, even if near each other and separated by no barrier, are likely to be composed of exactly the same species. On the other hand faunas on opposite sides of the earth and connected only indirectly and imperfectly may have species in common. Nothing as to the nature and length of a continental connection can be inferred from the usual vague statements, on one hand, that the faunas are different or on the other that they have some similarities. Absolute qualitative resemblances and differences hardly exist. The pertinent data are the degrees of similarity.

Simpson discredited continental drift arguments by using statistics on vertebrate fossil occurrences. But because these land mammals are so rarely preserved, they are hardly appropriate for such statistical analysis. Invertebrate animals and plants would have been much better for statistics, since vast collections of the same species can be made, and one can more reasonably deduce the range of distribution. The Australian geologist S. Warren Carey (1911-2002) explained the ruse in a table that Simpson had presented:

Carey 1988, p. 102.

Simpson, for example, scorned the evidence for faunal ties between Africa and South America, citing the Triassic reptiles as having only a remote degree of kinship consistent with their present separation across a wide ocean:

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

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

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

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

The naïveté of such an argument is stark. The faunal lists for the recent mammals are (for all practical purposes) complete. The probability is remote of finding a new species, still less a new genus or a new family. Among the Triassic reptiles, by contrast, a whole genus may be known only from a single bone. At the time Simpson wrote, not a single Triassic reptile was known from the whole Australasian octant of the globe from Malaysia to New Zealand. Since then a nearly complete skeleton has been found in Hobart, and others elsewhere, along with several amphibians. It would be absurd to suggest that the Triassic faunal lists are even 1 percent complete. If we took a random 1 percent from the Ohio and Nebraska mammals, the comparative percentages would drop drastically. Moreover, column C covers some 60 million years, so to make a valid comparison for column A, it would be necessary to take a random 1 percent from all the Ohio mammals known since the Eocene Epoch and compare them with a similar random list from Nebraska.

In his table above, Carey had removed five of Simpson's eight columns, for clarity. (I think he took the simplified table from Holmes (1944) who mildly criticized Simpson as well, see p. 212.) But this simplification does not discredit Carey's argument: Simpson was intentionally being very naive with his statistics.

Simpson concluded this paper with a statement concerning only mammals, but it left readers with the impression that fossils in general supported the hypothesis of fixed continents with no land bridges:

Simpson 1943, p. 29.

Conclusion

The known past and present distribution of land mammals cannot be explained by the hypothesis of drifting continents. It can be accommodated to that hypothesis only by supplementary hypotheses effectively indistinguishable from those involving stable continents and not really involving or requiring drift. This distribution could be explained in terms of transoceanic continents but it is more consistent with fully stable continents. There appear to be no facts in this field that are more completely or more simply explicable by transoceanic than by stable continents and the supposed evidence of this sort is demonstrably false or misinterpreted. The distribution of mammals definitely supports the hypothesis that continents were essentially stable throughout the whole time involved in mammalian history.

Simpson was considered by most scientists to be the leading paleontologist of his time. For the next twenty years, his support for fixism, confidently and frequently stated, seemed to discredit any paleontologic arguments in favor of mobilism. He had indeed filled the vacancy left by Schuchert as one of the leading fixists. It is curious that Simpson involved himself with the continental-drift debate at all, since his research was mainly on the evolution of mammals in the Cenozoic.

Simpson accepted neither Du Toit's drifting continents, nor the sinkable land bridges. But Simpson never had a harsh word for Schuchert, the North American leader of the land-bridge theory. Simpson had his loyalties, as do other scientists. He had studied at Yale from 1922 to 1926, probably forming his first opinions about continental drift together with Schuchert, Dunbar, and Longwell. He became a fixist, and was thereafter never willing to consider the paleontologic evidence for mobilism.

When Simpson finally acknowledged the validity of mobilism in 1970, it was on the basis of paleomagnetism, unrelated to his own field of research. He wrote: "direct fossil evidence is still curiously scanty or equivocal." (Frankel 1987, p. 232). Du Toit had written essentially the same thing in his first book, subjecting himself to Schuchert's criticism. The fossil evidence that had helped convince Wegener and Du Toit of mobilism was neither scanty nor equivocal, but maybe it was indirect. Simpson had limited his comment to direct evidence. Simpson, as a respected scientist, chose his words carefully.

Du Toit (1944) defended *Our Wandering Continents* in a rejoinder to Simpson's criticisms. He politely thanked Simpson for the new evidence that he provided, but disagreed with Simpson's conclusions.

Du Toit's rejoinder brought on another critical paper in the *American Journal of Science*, this time by Longwell. His purpose in writing was to support Simpson's critique and encourage others to criticize mobilism in like manner. He brought in many types of argument, not only paleontological ones. He, too, seemed to be trying to correct the positive book review by Rice. He insisted that he was being fair to the hypothesis of continental drift:

Longwell 1944, p. 220-221.

Possibly some readers will have decided, on the basis of comments up to this point, that I belong to the camp unalterably opposed to "drift," and that I am singling out for criticism items of Du Toit's paper that are aside from any main issue. I should like to clear myself of suspicion on both these counts. The Wegener hypothesis has been so stimulating and has such fundamental implications in geology as to merit respectful and sympathetic interest from every geologist. Some striking arguments in his favor have been advanced, and it would be foolhardy indeed to reject any concept that offers a possible key to solution of profound problems in the Earth's history. On the other hand, critical examination and rigorous testing of the concept in all its aspects do not imply unfriendly opposition toward an unwelcome intruder into the realm of "orthodoxy." Intemperate statements have been made on both sides of the question; but these merely reflect weaknesses of human nature. Du Toit seems to sense a strongly entrenched opposition in what he calls "current geology, sublimely unconscious of its impotence." What is this "current geology," which apparently is synonymous with "orthodox geology"? I associate with numerous men in the science, and find them, on the whole, a rather open-minded group, not given to arrogant judgments on the many abstruse problems of the Earth, and emphatically not uniform in their thinking on these problems. They find much in their geologic work to occupy their attention aside from the conflicting hypotheses on the history of continents. However, thanks to recent published discussions most of them know the ideas on continental drift, and are willing to give these ideas a fair hearing. To be sure, most of them are not active protagonists of the hypothesis.

What should be done to improve the situation in "current geology"--other than to cultivate the open-minded attitude toward hypothetical matters in general and continental drift in particular? Du Toit states that he regards the hypothesis of drift "as essentially established by the Paleozoic and Mesozoic evidence." Does he mean established as fact, and would he have it taught among the established facts of geology? I fear that without more unequivocal evidence than we now can muster there would be many skeptics among the students. Perhaps he would be satisfied if numerous geologists entered into active and sympathetic discussion of the problem, treating it as a live issue in the science. I agree that this would be helpful, provided the discussion were governed by rigid scientific standards. The concept of drifting continents will be strengthened only by establishing a body of incontrovertible evidence in its favor; not by reiteration of diffuse and qualitative arguments, some of them based on data that are subject to question.

Nine pages later, Longwell ended his paper with this paragraph:

Longwell 1944, p. 230-231.

The frankly critical tone of this brief discussion may brand it as an unfriendly attack in the eyes of proponents of the "drift" concept. Zealous believers commonly follow the motto, "He that is not for us is against us." However, I have devoted time to the discussion only because of a genuine interest in the hypothesis of continental drift. I have an interest also in some other hypotheses designed to explain major features of the earth in terms of its history. No one of these hypotheses has yet won me as a protagonist, and at present it is inconceivable to me that any scientist could adopt one such hypothesis to the exclusion of all others. Perhaps a definite choice of creed brings some peace of soul that is denied to the scientific skeptic. However, those who declare unswerving loyalty to any hypothetical concept appear to settle into an "orthodoxy" of their own, which interferes with breadth of scientific vision. In the genetic study of major earth-features I can not believe we have arrived at a stage that permits discarding the method of multiple hypotheses. In this study the several concepts at our disposal are essential tools, which should be sharpened continuously with the abrasive of objective criticism.

Longwell's comment about being interested "also in some other hypotheses designed to explain major features of the earth" was a bluff. There existed no other hypotheses that seriously tried to explain the origin of geosynclines, the Mid-Atlantic Ridge, and fold mountain belts. He still considered fold mountain belts to be a mystery of science, as one can read in his geology textbooks both before and after he wrote this paper.

BAILEY WILLIS CALLS CONTINENTAL DRIFT A GERMAN FAIRY TALE (1944)

Longwell wrote as if he were already an editor of the *American Journal of Science*, which he became a few years later. A main purpose of his paper was to invite others to join in on the criticism of the continental-drift hypothesis:

Longwell 1944, p. 221.

Let us hope, therefore, that specialists in some other fields will emulate the example set by Simpson, by publishing critiques of evidence that comes within the range of their own competence. The result should be elimination of errors and other weaknesses from existing literature on the drift hypothesis, leaving a residue of data that can be accepted as dependable and significant.

Longwell was not asking others to try employing the displacement hypothesis, but to write papers finding fault with it. Following this invitation, Bailey Willis wrote a five-page discussion. His *Isthmian Links* article in 1932 had been at the invitation of Schuchert. Now, with Schuchert gone but not forgotten, this paper was at the invitation of Longwell. Willis used the title *Continental Drift, ein Märchen* to poke a little fun at Wegener. *Ein Märchen* means a fairy tale. Teasing an opponent about his nationality is not usually allowed in a scientific publication, but this was exceptional: America was at war with Germany, and Willis had just been awarded the Penrose Medal; he could write as he pleased.

Not only the title, but also other parts of this paper should have been flagged by the reviewers and editors. The main point of the paper was to argue that continents could not plow through sima, as Wegener had claimed in the third edition of his book. In his fourth edition, Wegener had also recommended the convection-current mechanism, but Willis and the referees probably had not noticed that. Wegener's fourth edition had not been translated into English. But Willis surely knew that Du Toit, the leading continental drift author, promoted convection currents as the mechanism.

Willis' statements in the last paragraphs of this paper were simply outlandish. The views he expressed there were completely opposed to his earlier principles and to any geological common sense. It was as if he were flaunting his irreproachability, tossing out wild ideas to see if anyone would dare challenge them. First, he suggested that high mountains are caused by volume expansion of underlying rocks, not by the processes of folding and thrust-repetition that he had documented in his research and textbooks:

Willis 1944, p. 512.

Is the relation to sea level a permanent condition? There is abundant evidence in marine fossils raised to high altitudes and also in sediments piled to thicknesses of thousands of feet on subsided lands that it is not. We may recur to the fact of the eighteen thousand foot uplift of Tibet during the latest geologic epochs as proof of local increase of the earth's radius; an increase which on the evidence of gravity measurements is not due to added mass and must be attributed to augmented volume, that is to expansion; as of rising dough.

Then in his final paragraph, he ignored the arguments about isostasy and the volume constraints of ocean waters and basins. These were principles that he had held since 1910. Now he suggested that wide land bridges had once existed, even across the Pacific Ocean, and that they later sank to present depths of 5,000 meters:

Willis 1944, p. 512-513.

Prof. Douglas Campbell of Stanford has recently called attention to the remarkable likenesses that are found in the floras of Hawaii, the islands of Oceania, Australia, and South America. He proves beyond question that there have been land connections between these now widely separated districts. At one time I would have sought to trace the connections as mountain ranges, as I did when cooperating with Charles Schuchert on the similar problem of identity of terrestrial organisms in Africa and South America in Permian time. The concept of isthmian links appeared sound, biologically, dynamically, and climatologically, as tested by the geographical requirements of the case. It does not, however, fit the Pacific conditions, except perhaps to link the Antarctic continent with Australia and South America. The connections in Oceania are too broad and too complex to be explained by mountain ridges. The alternative assumption of uplift, followed by subsidence of the ocean bed, of the emergence of lands which have now subsided beneath the waters is more reasonable. That it contradicts certain preconceptions of mine regarding the permanent levels of continents and ocean beds does not affect the evidence. In the Philippines the Manila basin is a subsiding area. It is surrounded by volcanoes from which lavas are erupted and gases are constantly escaping. I attribute the subsidence to these conditions in a cooling mass a hundred miles in diameter. Closely adjacent lies the similar area of the Sibuyan sea, where raised coral reefs demonstrate active uplift. I consider the uplift to be due to increase of volume of a subjacent body in which the temperature is rising. Similar conditions on a larger or smaller scale are widespread throughout the southwestern Pacific. To them I would attribute the former expanse of land which once bore the now dispersed floras and also its disappearance beneath the waters. The average depth of the Pacific, 4,000 to 5,000 meters, is not excessive in comparison with known uplifts and subsidences in continental areas.

Even sinkable land bridges across the narrow Atlantic were now fully discredited, by considerations of isostasy, water-volume, and Simpson's raft-dispersal theory. It was not reasonable to begin suggesting great land bridges across the Pacific. Did anyone notice what wild idea Willis was promoting here? Did anyone in 1944 really care what arguments fixists were using against mobilism? I think that Willis dashed off this little paper with its humorous title just for fun. He was daring the editors, peer reviewers, and readers to call foul. It seems that none of them did.

9. Continental Drift in English-Language Geology Textbooks

Students learn most of the principles of geology in their first few geology courses. The textbooks that they are told to buy have a great influence on their opinions and attitudes. Whereas the North American textbooks by Schuchert and Longwell would not treat continental drift as a working hypothesis, other books gave it more respect. Now we will look more closely at a few of these textbooks.

LAKE and RASTALL'S *TEXTBOOK OF GEOLOGY* (1927, 1941)

Philip Lake (1865-1949) was the British author who wrote negative reviews of Wegener's early work and seems to have coined the term "continental drift." He was best known for his *Textbook of Geology*, with coauthor Robert H. Rastall (1871-1950). They divided the book by topics: Rastall wrote the first 15 chapters, covering physical geology, and Lake wrote the next 16 chapters on the stratigraphy of Great Britain. Stratigraphy can naturally involve topics of historical geology, but not in Lake's presentation. He paid little attention to fossils, geological environment, or climate.

Their textbook was first published in 1910, and then revised in 1912, 1920, 1927, and 1941. A conflict must have developed between Lake and Rastall as to Wegener's hypothesis. Rastall was editor of *Geological Magazine*, and was well known to support Wegener (Rastall 1929). Lake and Rastall mentioned continental drift in their 1927-edition, cautiously introducing the idea:

Lake and Rastall 1927, p. 184.

Continental Drift – Of late years a further complication has been introduced into the subject by the idea, first put forward by Taylor in America, but largely developed by Wegener, that the continents need not necessarily remain in the same geographical position throughout their existence, but may be drifting about, much as ice-floes drift about in the sea. The motive power for these supposed movements is not clear, and the whole thing is very speculative at present, but it does afford a possible explanation of certain anomalies in the distribution of animals and plants both in past and present times.

It was not much, but at least it was a neutral suggestion. Rastall was proud that continental drift was now included in the book, and pointed it out in the preface to this edition. And the next year, Holmes (1928) revealed "the motive power for these supposed movements" in Rastall's *Geological Magazine*.

When Philip Lake retired some years later, he authorized Rastall to make any further revisions to their textbook that might be appropriate. For the next edition, in 1941, Rastall expanded continental drift from that little paragraph to a full page of text. He presented it in a very positive light, and recommended that students read the books by Wegener and Du Toit. He concluded this page by encouraging skeptics to keep an open mind:

Lake and Rastall 1941, p. 196.

Perhaps the fairest thing to say now is that at any rate the theory of continental drift can no longer be dismissed as the fantastic speculation of a few cranks: it has to be regarded as a serious contribution to the study of many problems hitherto unsolved. There are several cases in geology of phenomena of first-class importance, admitted by every one to have occurred, which have as yet received no theoretical explanation of general acceptance: as, for instance, mountain-building and ice ages.

SAMUEL SHAND'S *GEOLOGY WITHOUT JARGON* (1933)

The Scottish geologist Samuel J. Shand (1882-1957) became professor of geology at the South African University of Stellenbosch in 1911. He was instrumental in building a modern department of geology there over the next 26 years. His specialty was petrology, and he wrote two textbooks that were internationally used. The first was *The Study of Rocks* in 1926, aimed at the introductory level. Revised editions of this book remained in print until 1959. He followed this with an advanced text, *Eruptive Rocks*, which was kept on the market until 1974.

In 1933 he published a little geology book entitled *Earth-Lore, Geology without Jargon*. Not only was jargon discouraged in his book, but also geological fundamentalism. He explained in no uncertain terms that geological data, which he termed "The Book of the Rocks," were more reliable than the non-scientific biblical teachings of "The Creation Saga". His last chapter was an especially long one, entitled "Drifting Continents." Shand, like many other geologists in the southern hemisphere, had no doubt that Wegener was on the right track in his geological interpretations.

Shand introduced this final chapter by reprinting the Snider-fit of 1858. Shand seems to have been the first geologist to refer to Snider or his map. (Pepper was not a geologist.)

Shand then clearly presented the ideas and maps of Frank B. Taylor (1860-1938) and Alfred Wegener in support of drift. His figures showed Taylor's depiction of the Mid-Atlantic Ridge, which he called the Dolphin Ridge (see also Fig. 26). Shand redrafted Wegener's fit-map of 1920, and his reconstruction of continents at the South Pole to explain the distribution of Carboniferous glaciation. Shand wrote that evidence for continental displacement was overwhelming. But he also pointed out that the mechanisms that Wegener invoked for displacing continents could not be correct. Here are the last few paragraphs of this extensive final chapter:

Shand 1933, p. 131-134.

That is briefly the history of the continents according to the Wegener hypothesis. If all these points could be established we should have to credit Professor Wegener with the greatest piece of geological synthesis that has ever been accomplished. His theory not only solves the problem of mountain-building by supplying a simple and entirely adequate cause for it, but is able to meet the most extreme claims that geologists can make in the matter of compression and overthrusting in mountain ranges. If Alpine rocks seem to have been driven seventy miles from the site where they were laid down on the sea-floor, that is not too great an effect to have been brought about by the impact of Europe on Africa. Is the latest estimate of the Appalachian folding twice as great as any previous one? Let it be so; it might be greater still without straining the hypothesis. In short, the Wegener hypothesis offers us a fuller and more satisfactory solution of the problem of mountains than any other that has yet been proposed.

But that is not the only merit of this far-reaching theory. It clears up to a large extent the mystery of past climates, and it answers some most difficult questions about the distribution of living and extinct animals and plants. How well it does this we shall try to show by means of a few illustrations.

In South Africa, in Carboniferous times, a very remarkable sedimentary rock was laid down. It consists of large and small boulders of many different kinds of rock, embedded in a fine-grained sand or silt. The boulders and the sand are not assorted according to size, or arranged in layers, as they would be if the deposit had been formed by rivers or laid down on the sea-shore; but they are jumbled together quite as if the whole lot had been tumbled out of a rubbish-shoot. A certain number of the boulders have flattened faces, with deep scratches on them; and where the deposit has been removed by erosion in recent times, one can see that the rocky floor on which it lay has had all its irregularities rubbed away and is scored with deep, parallel grooves, as if by the action of a mighty planing machine. Now nobody who has ever seen the deposits of rock-waste thrown down by glaciers when they melt away, and the grooved floors over which they have travelled, can feel any doubt about the origin of this boulder-deposit in South Africa; it is identical in every respect with a glacial moraine or deposit of glacial debris. However difficult it is to convince oneself that a warm country like South Africa was ever covered by ice, the facts admit of no other interpretation. We must believe that in the Carboniferous period South Africa was buried beneath an ice-sheet as thick as the one that covers Greenland at the present day.

But that is not all. Precisely similar deposits dating from about the same geological period have been found in South America, the Falkland Islands, India, the Malay Straits and Australia; but not in any country further north than these. How is it possible for glacial conditions to have ruled in these warm countries, some of which lie nearly on the Equator? The difficulty of understanding it becomes all the greater when one thinks that over large parts of Europe and North America, in the same period, there were forests and steamy swamps in which an abundant vegetation lived and gave rise, as it decomposed, to thick seams of coal. But all these difficulties disappear if we can believe, with Wegener, that away back in Carboniferous times all these southern continents formed part of one great land mass lying around the South Pole (Fig. 32.)

The zoologist who studies the distribution of living animals comes across some very extraordinary facts. There are certain freshwater fishes, frogs and mussels, as well as an important family of earthworms, that are widely spread in Europe, Asia and North America but quite unknown in the southern continents. On the other had there are lizards, snakes and even butterflies in the southern lands that have never been found north of the Himalaya or in North America. A very remarkable case is that of the tiny fresh-water crustacean called *Peripatus*, which has been found in mountain pools in Tasmania, Victoria, India, the Cape of Good Hope and South America, but nowhere else on earth. Among the mammals, the lemurs are restricted to Africa, Madagascar, India and Malaysia. The nearest living relatives of the Australian kangaroos are the South American opossums. Most of the animals mentioned above are little, delicate creatures that cannot live in the sea or make a long journey across it; so in order to explain their distribution one must either believe that creatures identical in every respect originated independently at half-a-dozen different centres, or else that the lands in which they are not found were at one time united.

When one sees how many different kinds of evidence favour the Wegener hypothesis, it is difficult to resist being carried away by it. But a hypothesis is not necessarily true just because it accounts for a number of phenomena, or because we have not succeeded in imagining any satisfactory alternative to it. A serous objection to the Wegener hypothesis is that the force derived from the Earth's rotation, which is supposed to be the cause of continental drifting, has been shown to be utterly inadequate to do what the hypothesis demands of it. Again, Wegener's hypothesis differs in a very important respect from that of Suess and Taylor; for Wegener gives one explanation for the mountain arcs of Asia and another one for the island festoons. Taylor gave the same explanation for both, and in this respect his version is the better one. In this matter of the interpretation to be put upon the island festoons it seems almost certain that the verdict will go against Wegener; and this will destroy the only evidence that there is for a westward drift of Eurasia. But there will still be a strong case for westward movement of the two Americas; and since, according to Wegener, America drifted west at the same time that Eurasia was creeping south against Africa, it must follow that the forces that caused the movement were not the same in the two cases. But if that is so, then Wegener's idea that continental drifting is a consequence of the Earth's rotation melts into thin air, for the effect of the rotation would be the same upon all the continents.

Now if the drifting of the continents does not depend upon the Earth's rotation, then there is no known reason why it should take place in one direction rather than another. Once freed from the limitation that movement can only take place toward the Equator or toward the west, the hypothesis of continental drift—no longer the Wegener hypothesis but the joint hypothesis of Suess, Taylor and Wegener—will gain new strength and flexibility. East-coast mountains like the Appalachians, and Poleward mountains like those of South Africa, which are not accounted for by the Wegener hypothesis, will then be capable of explanation by the same mechanism as west-coast and Equatorial ranges.

But then we must give up the pretence that we know what it is that sets the continents drifting. We only know that the cause does not reside in the crust, and that it has nothing to do with the Earth's rotation. The secret must lie, together with the secret of volcanism and the secret of terrestrial magnetism, in the magma beneath the crust.

Shand understood where Wegener was right, and he pointed out that Wegener's mechanism of drift must be wrong. But Shand did not yet know that convection currents in the mantle could move the continents. He was not aware of Holmes' hard-to-find papers.

Due to his solid academic reputation and his strong publication record, Shand was offered a professorship at Columbia University in New York City, beginning in 1937. That same year his second edition of *Earth-Lore* was published, with slight additions.

Since Shand's book was openly critical of modern “Bible apologists” and offered strong support to the theory of continental drift, it would not have been welcomed at Columbia, or at any American universities. Shand never expanded this book into a college textbook, as one might expect him to have done. He continued his research and teaching at Columbia, but published nothing more in support of continental drift. When he turned 65 in 1950, he retired from Columbia and returned to the geological institute in Scotland where his career had begun.

E. B. BAILEY'S *INTRODUCTION TO GEOLOGY / GENERAL GEOLOGY* (1939)

E. B. Bailey (1881-1965) was director of the Geological Survey of Great Britain as well as a geology professor. He was highly respected for his research on the Caledonian mountains of Scotland. He wrote a geology textbook that was popular in Great Britain. The index for that book lists the term "Continental Drift" as occurring on eight different pages. Bailey was supportive of continental drift each time that he mentioned it. When put together, these eight citations present a rather convincing case for that theory. I especially like the paragraph on his page 391, which shows that he understood what is now called the Wilson-cycle – opening of an ocean, deposition of marine rocks, and closing again to form a compressional mountain range:

Bailey 1939, p. 7.

Mountain Structure. – Let us mention one further discovery. Sometimes we find rocks that have been driven many miles horizontally, in great slices, over their fellows. The picture one gets is much the same as that presented by an arctic sea, where ice floes have crashed into, and ridden over, one another.

One of the main questions at present discussed by geologists is: Do continents drift? To this no definite answer can be given, but it will be possible in the sequel to indicate the sort of evidence that suggests the possibility that, for instance, South Africa and South America once lay side by side (Fig. 307) [Fig. 64]. Perhaps the matter may be settled by measurement, for a succession of longitude observations taken at a particular site in Greenland makes it appear possible that Greenland is nowadays drifting at an appreciable rate towards America.

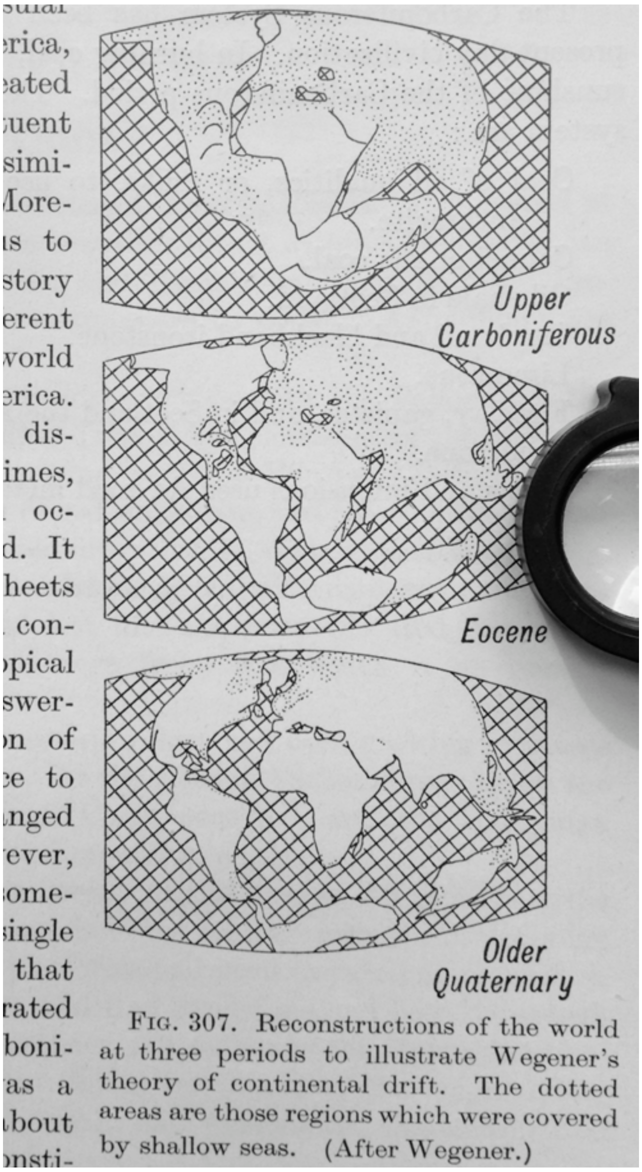


Figure 64. Bailey's Fig. 307 showing Wegener's reconstruction as a reasonable working hypothesis. From Bailey (1937).

Bailey 1939, p. 19.

We have in these few instances noted that Britain formerly enjoyed much warmer climates than at the present day. This supports the view that the continents are drifting and that Britain long ago was much farther south than it is to-day.

Bailey 1939, p. 124.

The folded mountains with their immense thrusts are clear evidence that different portions of the earth's crust sometimes move toward one another for considerable distances. Reference has already been made to the hypothesis of drifting continents. It is thought by many that folded mountain chains are a symptom of this drift; that the Alps, for instance, were raised during a collision of the European and African blocks.

Those who adopt the drifting of continents as an explanation of folded mountains have still to find a satisfactory motive force. Convection currents in the layer below the continents have been postulated; but the subject is too deep for ordinary geologists.

Perhaps it is well to point out that though the drifting of continents is a very hypothetical idea, the floating of continents seems to be established. It is pretty clear that the several continents are made of relatively light material which does not extend in bulk far under the intervening oceans. As for the interior of the earth, it is certainly denser than the common rocks of the continents. One pictures the continents floating something like icebergs with much of their light material submerged. The idea is summed up in the word isostasy, which claims that the major features of geographical relief are in equilibrium with the underground distribution of density. Gravity determinations seem to demonstrate that the Himalayas, for instance, are buoyed up by a great downward bulge of light continental material, projecting into a heavy substratum.

Bailey 1939, p. 384.

In Scotland, Precambrian rocks form the Outer Hebrides and the coastal belt of the North-West Highlands. Their age is fixed by the fact that they are overlain unconformably by fossiliferous Lower Cambrian. They seem to belong to the eastern rim of the Canadian Shield, quite possibly separated from the main exposure by a drifting apart of Europe and America. We shall return to this subject when we consider the Cambrian.

Bailey 1939, p. 391.

Another feature found in some of the great geosynclines is an association of radiolarian chert and basic pillow lavas, intruded into by serpentines and gabbro. It has been suggested that the radiolarian chert is an abyssal deposit, like the radiolarian ooze of to-day, and that its presence marks the extreme deep-water development of the particular geosyncline. It has further been claimed that geosynclines have been developed by a stretching of the earth's crust, during what may be called a continental drift-apart. The stretching is supposed to thin the crustal layer, and to allow basic and ultrabasic magma to reach the bottom of the sea, there to associate with radiolarian ooze. If such drift-separation continues, a new ocean bed may be developed, covered with products of submarine eruptions. If, as has thrice happened in the post-Cambrian history of Europe, a drift of separation gives place to a drift of approach, then a folded mountain chain comes into being.

Bailey 1939, p. 395.

Let us return to the resemblances which exist between the Cambrian, and probably Lower Ordovician, of the Scottish North-West Highlands and the contemporaneous rocks of parts of Newfoundland and the Appalachian Mountains. We find:

(1) The faunas are the same.

(2) The rock types, quartzite and limestone, are the same though they do not agree in their detailed arrangement.

(3) The structural position is the same. Both lie at the north-west edge of the Caledonian Mountain Chain, folded and thrust in Early or Mid-Palaeozoic times.

These resemblances certainly support the view that America and Europe have drifted apart; but they certainly do not prove any such proposition.

Bailey 1939, p. 441.

Let us now turn our attention to a hypothetical supercontinent that has been called Gondwanaland, after the Gondwana district of central India. Gondwanaland includes peninsular India, most of Africa, South America, Australia and Antarctica. It is treated as a unit because its constituent parts show many extraordinary similarities of geological history. Moreover, from Upper Carboniferous to Triassic times inclusive, the life-history of Gondwanaland was very different from that of the rest of the world in Asia, Europe and North America. Both these features are well displayed in Upper Carboniferous times, when extensive land-glaciation occurred throughout Gondwanaland. It is astounding to learn of ice-sheets in India, South Africa and Brazil contemporaneous with forests of tropical type in Britain. It almost unanswerably suggests that the position of the earth's crust, with reference to the poles and equator, has changed during geological time. As, however, the glaciated localities extend somewhat beyond the limits of a single hemisphere, it would appear that the earth's crust has not migrated as a whole, but that in late Carboniferous times Gondwanaland was a fairly compact unit clustered about the South Pole, and that its constituent parts have since drifted asunder into their present positions (Fig. 307).

Gondwanaland had a very remarkable flora after its glaciation, that is, for the most part, in Permian times. The flora is as widespread as Gondwanaland itself, and yet is quite distinct from the contemporaneous floras of the northern hemisphere. Its commonest genus is *Glossopteris* (Fig. 266).

All geologists are agreed as to the reality of the resemblances which unite Gondwanaland, but many ridicule the idea that its constituent parts have drifted apart. They postulate former land-bridges, as they are called, across the intervening oceans, and suppose that these bridges have foundered.

Bailey 1939, p. 464.

A strong argument for drifting continents is supplied by the rich Cretaceous flora of Greenland, containing some plants, such as the bread fruit, which now only occur in low latitudes.

All of these statements by Bailey agree well with our current geological understanding in the framework of plate tectonics.

ARTHUR HOLMES' CLOSING CHAPTER IN *PRINCIPLES OF PHYSICAL GEOLOGY* (1944)

In 1944 Arthur Holmes published a textbook for new students: *Principles of Physical Geology*. It soon became the standard geology textbook at British universities. In a section entitled "The Cause of Mountain Building" Holmes used several pages to present the hypothesis of sub-crustal convection currents. He showed two stimulating figures. One of these was a convection diagram from 1931. The other was the experimental convection model that David Griggs had published in 1939. Holmes showed how convection might explain the deposition of rocks of a geosyncline, and the subsequent horizontal compression of these rocks to produce the folding and thrusting of a mountain range.

Holmes' final chapter "Continental Drift" seems like an extended and updated version of Shand's final chapter "Drifting Continents." Holmes too discussed the history of drift ideas, including the Snider-fit and the hypothesis of Frank B. Taylor. Holmes' chapter strongly influenced the attitude of British geology students to the ideas of continental drift, and I see no alternative but to reproduce it all. Holmes pointed out where the problems lay, but left no doubt that he saw continental drift as being the basis of a grand new understanding of geology.

Holmes 1944, p. 487-509.

CHAPTER XXI

CONTINENTAL DRIFT

CONTINENTAL AND OCEANIC RELATIONSHIPS

The continents are essentially thin slabs of sial, distributed to form a northern pair, known together as Laurasia, and a more scattered southern group, collectively referred to as Gondwanaland. The outer peripheries of the members of each group are defined by the orogenic belts of the last great tectonic revolution (Figs. 209 and 210) [Fig. 65], and the coast-lines generally backed by mountains, are of Pacific type (p.401). [Both Figures 209 and 210, with arrows showing the direction of crustal motion, were published by Holmes already in his 1931 paper, but this was not pointed out to textbook readers.] The inner margins of the members of each group, against the Arctic, Atlantic, and Indian oceans, are fractured and in many places downfaulted towards the sea, and the coast-lines are of Atlantic type (p.400). Across the floors of these intervening oceans the sial layer, where present at all, is patchy in distribution and very much thinner than in the bordering continents. Much of the Pacific floor, however, lacks a layer of sial altogether. The structural units of the face of the earth thus fall naturally into the following pattern:

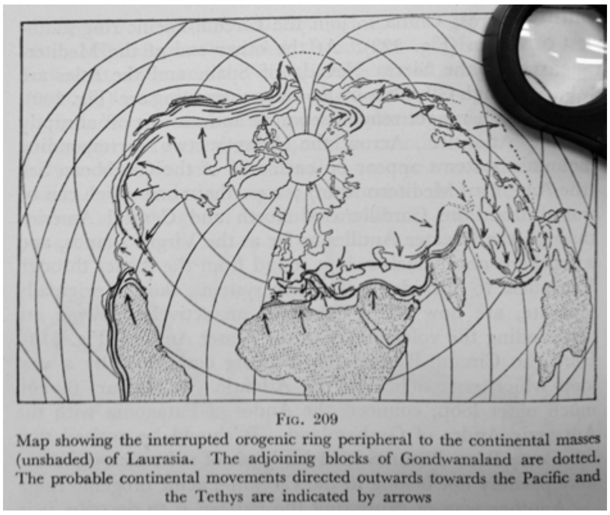
1a The continents of the *Laurasia* group

1b The intervening North Atlantic and Arctic oceans

2a The continents of the Gondwanaland group

2b The intervening South Atlantic and Indian oceans

3 The oceanic basin of the Pacific, everywhere outside 1 and 2 (see Fig. 208)



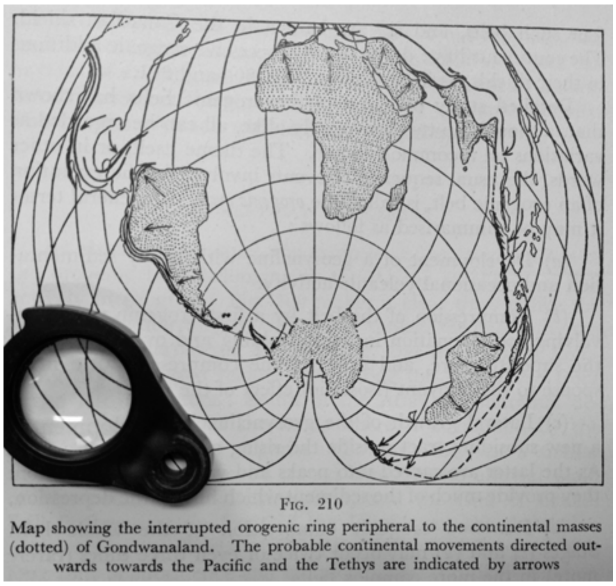


Figure 65. Holmes' Figures 209 and 210, which were taken directly from his 1931 paper.

To what extent these primary units and their arrangement have been stable or otherwise during geological time is one of the fundamental problems of geology. For nearly a century this question has been vigorously debated as one line of evidence after another has been discovered and followed up. Before then the widespread occurrence of marine sediments over the lands suggested that the continents could sink to oceanic depths and the ocean floors rise to become dry land. It was gradually recognized, however, that these deposits merely prove flooding of the lands by shallow seas; they do not demonstrate interchange of continent and ocean. For this reason, amongst others, Dana expressed the view in 1846 that continents and oceans have never changed places, and that the general framework of the earth has remained essentially stable. Nevertheless, Edward Forbes, tackling the subject from the biological side in the same year, found it impossible to explain how animals and plants had migrated from one continent to another unless some parts of the oceans had formerly been land. Thus began the long controversy regarding the permanence of the continents and ocean basins.

Support for permanency is found in the fact that deep-sea deposits like those now forming on the ocean floors are confined to one or two marginal islands (p. 320) and are consistently absent from the strata now exposed on the continents proper. Moreover, from the standpoint of isostasy it is very difficult to picture a process which could bring about widespread changes of level amounting to two miles or more. The explanation given for the subsidence of geosynclines and the uplift of orogenic belts can hardly be applied to areas of continental extent. But since the continents themselves are vast complexes of orogenic belts of different ages, it is obvious that they must have been profoundly modified during geological time, and that a good deal of variation in extent and position must therefore be conceded.

The extreme advocates of permanency have also had to yield ground in face of the evidence that certain regions that were undoubtedly land long ago are now parts of the Atlantic and Indian oceans. In Britain the sediments of the Torridonian and some of those of the Old Red Sandstone and Carboniferous were derived from a land that lay to the north and west of Scotland. On the other side of the Atlantic the Appalachian geosyncline was largely filled with sediment from the south east. The gold-bearing conglomerates of the Gold Coast were carried there by a great river that drained a land lying to the south. In each of these cases the site of the ancient land is now open ocean.

What, then, has happened to these vanished lands? Theoretically there are three possibilities:

(a) They may have subsided bodily to great depths, while retaining their original positions on the earth's surface. This is the apparently obvious answer, but it raises a serious isostatic difficulty. If a tabular iceberg split into two, the separated bergs might slowly drift apart, but neither could sink. This analogy has a value in introducing the idea of continental drift as an alternative to continental sinking.

(b) Bodily horizontal displacement may have occurred. If this happened there would be no subsidence. Labrador might be the land that formerly lay adjacent to Scotland. The gold-bearing tracts of the Guianas and Brazil might be the source of the gold deposits of the Gold Coast.

(c) More probably, however, the crustal layers, including the sial, may have been stretched out horizontally between the displaced continents, the sial thereby becoming thin and patchy. In this case the resulting isostatic readjustment would involve sinking. The known structure of the Atlantic and Indian ocean floors is consistent with this explanation. [Here we see an important misunderstanding of the time: the mid-ocean ridges were thought to contain continental rocks.]

For many years it was naturally assumed without question that if interchanges between continent and ocean had to be postulated the movements involved could not be other than vertical. The suggestion that there might have been lateral displacements of the continental masses on a gigantic scale is generally ascribed to F. B. Taylor in America (1908) and to Alfred Wegener in Germany (1910). [The suggestion may have been made by Wegener in 1910 but published in 1912.]

For several years these pioneers developed their unorthodox hypotheses quite independently. Actually, however, the same idea had occurred to Antonio Snider more than fifty years before. In a book with the optimistic title *La Création et ses Mystères dévoilés* (Paris, 1858) he published the two maps here reproduced as Fig. 256 [Fig. 66]. Snider's reconstruction of Carboniferous geography was intended to explain the fact that most of the fossil plants preserved in the Coal Measures of Europe are identical with those of the North American Coal Measures. [This correlation was not by Snider, but only by Pepper (1861). Holmes apparently did not have access to Snider's book.] Although the two diagrams reappeared in J. H. Pepper's highly entertaining *Playbook of Metals* (London, 1861), the idea they embodied was evidently regarded as too fantastic and outrageous to be worthy of attention. Not unnaturally it soon became completely forgotten.

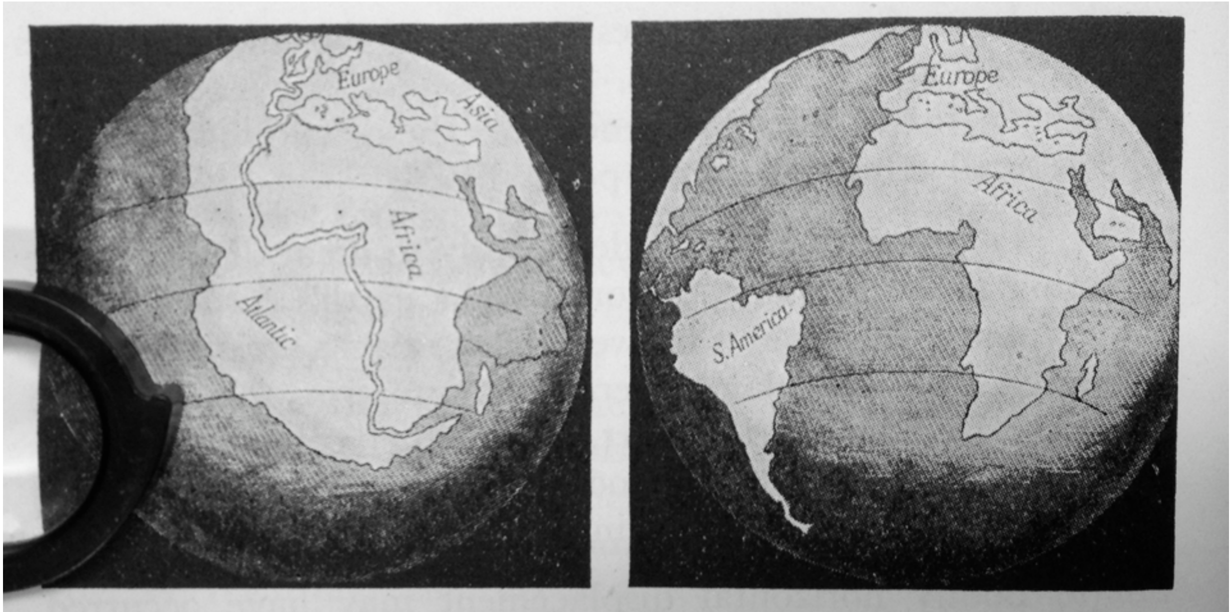


FIG. 256
Maps published by A. Snider in 1858 to illustrate his conception of continental drift. The left-hand map represents the supposed coalescence of the continents in late Carboniferous times

Figure 66. Holmes' Fig. 256. Globe maps attirbuted to Snider (1858), but are the redrafted versions Pepper (1861).

TAYLOR'S HYPOTHESIS OF CONTINENTAL DRIFT

It was not until Wegener published his famous book on the subject in 1915 that the possibility of continental drift began to receive serious attention. But Taylor must be given credit for making an independent and slightly earlier start in this precarious field. His immediate object was to account for the distribution of mountain ranges. He pictured the original Laurasia as being a continuous sheet of sial and supposed it to have spread outwards toward the Equator, more or less radially from the Polar regions, much as a continental ice sheet would do. Wherever the resistance was least the crust flowed out in lobes, raising up mountainous loops and arcs in front (cf. Fig. 209). Such movements, of course, would be impossible without complementary stretching and splitting in the rear. And, indeed, there is ample evidence of downfaulting and disruption in the coastal lands and islands of the Arctic and North Atlantic, and especially in the highly fractured region between Greenland and Canada, the map of which looks like a jig-saw puzzle with the separate bits dragged apart. In the Southern Hemisphere the originally continuous Gondwanaland similarly spread out, breaking up into immense rafts which also migrated towards the Equator and raised up mountains in front (Fig. 210). The basis of the South Atlantic and Indian oceans are interpreted as the stretched and broken regions left behind or between these drifting continents.

For two reasons Taylor's hypothesis received scant attention. As we have already seen (p. 384), a certain amount of lateral continental movement is implied by the structures of orogenic belts, but it seemed to be unnecessarily extravagant to invoke thousands of miles of horizontal displacement when from twenty to forty--rarely more--would suffice. [Although Holmes' familiar British Caledonides were thought to involve such small dispacement, other orogenic belts showed much more.] Secondly, Taylor's attempt to explain the alleged movements was quite unacceptable. He postulated that the moon first became the earth's satellite during the Cretaceous, and that at the time of its close approach and capture it was very much nearer to the earth than it is to-day. The resulting tidal forces were supposed to be sufficiently powerful not only to alter the rate of the earth's rotation, but also to drag the continents away from the poles.

Apart from the improbability that the earth was without a moon before the Cretaceous, there are two fatal objections to this hypothesis:

(a) If the late Cretaceous and Tertiary mountain building is to be correlated with the supposed capture of the moon, then we are obviously left with no explanation for all the earlier orogenic cycles.

(b) If the tidal force applied to the earth by the newly captured moon had been sufficient to displace continents and raise mountains on the scale required, then, as Jeffreys has shown, the friction involved would have acted like a gigantic brake and the earth's rotation would have been brought to a standstill within a year.

Taylor's "explanation" is completely untenable, but from the criticisms one very important conclusion may be drawn. The fact that the earth continues to rotate shows that neither tidal friction nor any other force applied from outside the earth can be responsible for mountain building or for continental drift, if it occurs. We have already found a cause for mountain building inside the earth, and if a cause for continental drift be also required, it too must be looked for within the earth.

WEGENER'S HYPOTHESIS OF CONTINENTAL DRIFT

Wegener's highly complex conception of the evolution of the continents is graphically illustrated by his own strange, but now familiar, maps (Fig. 257) [Fig. 67]. His picture of the world in Carboniferous times is strikingly similar to Snider's, except that India and Antarctica are tucked in between Australia and Africa, with the horn of South America forming an outer wrapping. For this compressed combination of Laurasia and Gondwanaland he proposed the name *Pangœa.* [It was Wegener's English editor John W. Evans who actually proposed this as a name.] In one important respect, however, neither Wegener's Carboniferous map nor Snider's gives adequate expression to its author's ideas. Snider urged that in consequence Europe and North America must have been near the Equator. This implies that South Africa must have been near the South Pole. [Again, Holmes is incorrectly attributing remarks by Pepper to Snider.] Conversely, Wegener inferred that the Carboniferous South Pole occupied a position just off the present South African coast (Fig. 260). His scheme thus involves not only continental drift, but also extensive wandering of the poles.

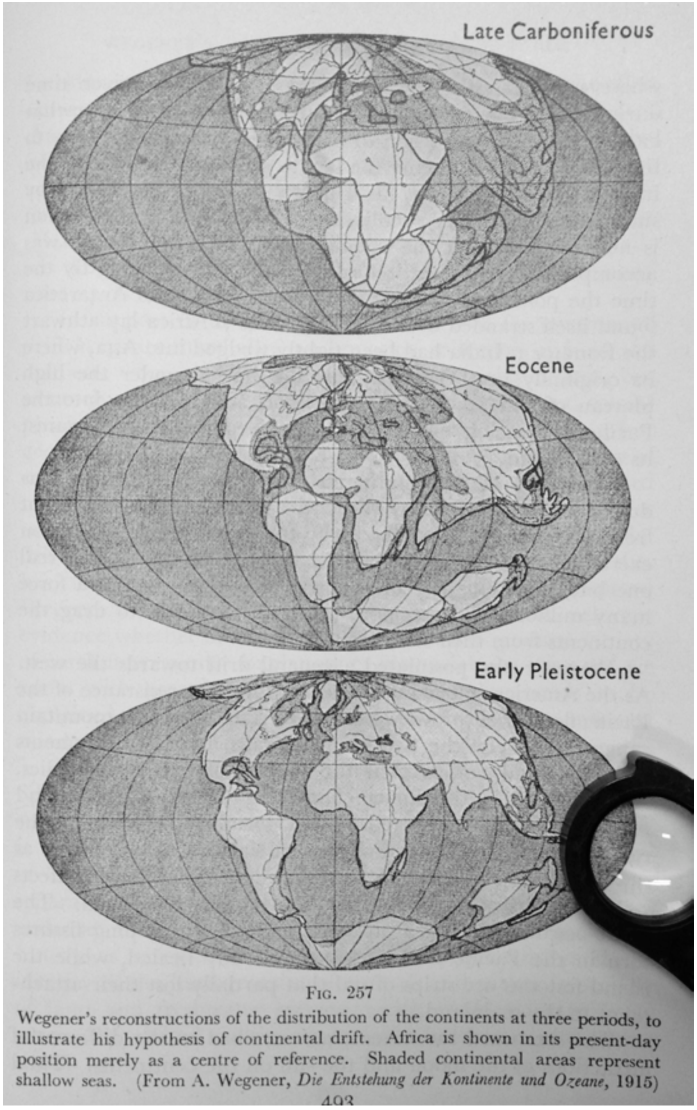


Figure 67. Holmes' Fig 257. Holmes thought that these maps were in Wegener's first book (1915), but they did not appear until the third edition (1922).

The present distribution of the continents is regarded as a result of fragmentation by rifting, followed by a drifting apart of the individual masses. The southern continents began to unfold during the Mesozoic era by being dragged away from wherever the South Pole happened to be at any given time during the progress of the outward movements. Somewhat later North America began to break loose and to drift away to the west, Greenland being the last to go. The Atlantic is the immense gap left astern, filled up to the appropriate level by sima from below. A peculiarity of Wegener's interpretation is his insistence that the opening of the North Atlantic was accomplished almost entirely during the Pleistocene. [As mentioned earlier, this was indeed one of Wegener's most serious mistakes.] By the time the poles had reached their present positions Antarctica found itself stranded over the South Pole; Africa lay athwart the Equator; India had been tightly wedged into Asia, where its originally northern part now lies buried under the high plateau of Tibet; and Australia had advanced far into the Pacific, by-passing the Banda arc and coming to rest against its eastern end.

The drift of the continents away from the poles was dramatically described by Wegener as the *Polflucht*—the Flight from the Poles. He ascribed it to the gravitational attraction exerted by the earth's equatorial bulge. The force is a real one but, unfortunately for Wegener, it would require a force many millions of times more powerful than this to drag the continents from their moorings.

Wegener also postulated a general drift towards the west. As the Americas moved westward against the resistance of the Pacific floor, their prows were crumpled up into great mountain ranges. Between the two immense rafts a trail of fragments lagged behind and formed the islands of the West Indies. The stretched-out isthmus connecting South America and Antarctica similarly lagged behind, forming the horns of the two continents and shedding bits of sial that now remain as the island loop of the Southern Antilles. The alleged effects of the westerly drift of Asia are less happily conceived. The great oceanic deeps are supposed to represent gaping fissures torn in the Pacific floor and not yet fully healed, while the island festoons are strips of sial that partially lost their attachment to the mainland.

The westward movements are ascribed to the differential attractions of the moon and the sun on the continents. Tidal friction acts like a brake on the rotating earth, and as the effect on protuberances is greater than that on lower levels of the crust the continents tend to lag behind. If they did lag behind, they would appear to drift to the west. But here again the force invoked is hopelessly inadequate to overcome the enormous resistance that opposes actual movement. The tidal force barely affects the earth's rotation, and is actually ten thousand million times too small to move continents and raise up mountains. [Here Holmes is pointing out another of Wegener's most serious mistakes.]

In support of his presentation for the case for continental drift Wegener marshalled an imposing collection of facts and opinions. Some of his evidence was undeniably cogent, but so much of his advocacy was based on speculation and special pleading that it raised a storm of adverse criticism. Most geologists, moreover, were reluctant to admit the possibility of continental drift, because no recognized natural process seemed to have the remotest chance of bringing it about. [Note Holmes' careful wording here. He could not write that "no natural process was known", because he had published the process in 1931. He did not want to mention this bit of historical information.] Polar wandering, the "flight from the poles," and the westerly tidal drift have all been discarded as operative factors. Nevertheless, the really important point is not so much to disprove Wegener's particular views as to decide from the relevant evidence whether or not continental drift is a genuine variety of earth movement. Explanations may safely be left until we know with greater confidence what it is that needs to be explained. Let us, then, turn to the evidence with an unbiased mind.

The chief criteria for continental drift are based on the following considerations:

(a) If two continents, now far apart, were originally united, it should be possible to detect the fact by the recognition of certain features that were shared in common by the separated lands, *e.g.* orogenic belts of which the broken ends can be naturally joined up; other details of geological history as revealed in the sedimentary sequences; and the identity of the fossil remains of animals and plants (especially of land and freshwater species) which could migrate freely across the united continents but not across an intervening ocean.

(b) If the continents formerly occupied widely different positions on the earth's surface, then the distribution of climate zones, as inferred from geological evidence, should have correspondingly changed.

THE OPPOSING LANDS OF THE ATLANTIC

The parallelism of the opposing shores of the Atlantic has been a subject of discussion ever since Francis Bacon first drew attention to it in 1620. To Wegener it suggested that the Atlantic is an enormously widened rift with the sides still matching "as closely as the lines of a torn drawing would correspond if the pieces were placed in juxtaposition." [I think this is a very loose translation of Wegener's statement of a torn newspaper.] The fit, even of the broken lines, is far from being as perfect as this, however. No significance can be attached to an argument based on a geographical pattern that is little more than a temporary accident. In Tertiary times the outlines of the coasts were very different from those of today. Nor is parallelism to be expected as a normal result of continental drift, for it is mechanically impossible that the sial blocks could have moved apart without a certain amount of rotation and a good deal of marginal distortion due to stretching and faulting. We know that there are patches of sial on the Atlantic floor, the two most notable being the long S-shaped swell that traverses the ocean floor from end to end, and the broad rise that links Greenland to Britain by way of Iceland and the Faroes. [Holmes and others at this time incorrectly thought that the mid-Atlantic ridge stood high because it consisted partly of light continental material.] If all this intervening sial were closed up again, until it became a sheet of normal continental thickness, it would make a land many hundreds of miles wide. Consequently, if we imagine the Atlantic to be closed up, it is obvious that not only the present shores but also the edges of the continental shelves would be still separated by a distance of this order. Matching of the geological correspondences will clearly be much less precise than would be expected if the coast-lines dovetailed as perfectly as on Wegener's too closely fitting maps.

Nevertheless, the actual similarities are very remarkable. As illustrated by Fig. 258 [Fig. 68], the transverse orogenic belts all appear to match surprisingly well. The westward convergence of the Caledonian and Hercynian fronts towards Ireland is continued in North America, where the fronts finally cross. The "fit" is not altogether satisfactory, however, because the times of most intense folding in the Appalachians are not the same as in Europe. For it is quite certain that the two Caledonian fronts should be linked together as indicated. There is another stretch of Caledonian front along the eastern side of Greenland, precisely where it should be to fill the gap between the North-west Highlands and Spitzbergen. But it should be remembered that the Iceland ridge stands badly in the way of a former close-up, and it must not be overlooked that the Greenland front may have been directly connected with the loose end of Newfoundland. The British Caledonian belt might then join up with a Caledonian belt known to traverse the Sahara. This particular problem cannot be settled until the geology of Greenland and the Sahara are better known.

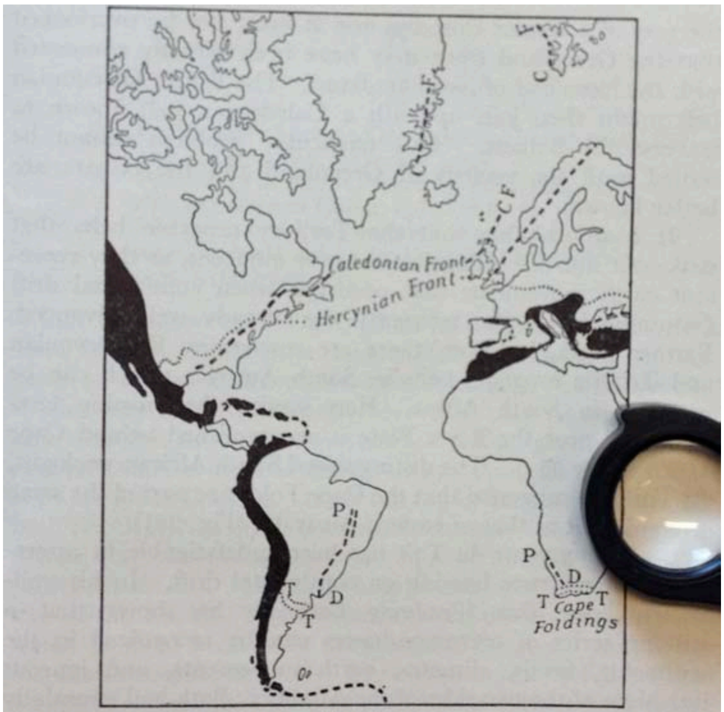


FIG. 258
Map to illustrate the tectonic correspondences on the opposing shores of the Atlantic

Figure 68. Holmes' Fig. 258.

It is improbable that the Tertiary orogenic belts that strike out into the Atlantic were ever adjacent, as they represent earth movements that occurred when continental drift (assuming it for the moment) was already well advanced. Farther south, however, there are converging Pre-Devonian and Triassic orogenic belts in South America which can be matched in South Africa. Here again, the crossing foreshadowed near the River Plate is accomplished behind Cape Town (Plate 95A). The distinguished South African geologist, Du Toit, has suggested that the Cape Folds are part of the same orogenic belt as that of eastern Australia (Fig. 261.)

For many years Du Toit has been indefatigable in assembling the evidence bearing on continental drift. In his well-known book *Our Wandering Continents* he shows that a striking series of correspondences can be recognized in the sediments, fossils, climates, earth movements, and igneous intrusions of the two sides of the Atlantic. Both had essentially the same geological history during Paleozoic and early Mesozoic times, and the combined evidence points very persuasively to the high probability that they were then very much closer together than now. Du Toit considers it possible that the original distance between the present opposing shores may have been as little as 250 miles. But this is a minimum estimate and not perhaps the most probable. [The original distance was in fact less than 250 miles, not more, as later shown by the map of Bullard (1965). Holmes thought that rocks of the Mid-Atlantic Ridge must have come from this space between.]

The chief adverse argument is a paläontological one. Columns A and B of the adjoining table indicate the degree of resemblance found between land animals that are free to migrate from one part of a continuous continent to another. C indicates the actual resemblance so far found between the known fossil remains of South African and South American Triassic reptiles.

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

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

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

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

A possible reason for the obvious discrepancy is that the proportion of individual land animals preserved as fossils is so minute that the chances of finding fossils of the same genus in widely separated localities are slight, and of identical species very remote. Negative evidence may be destroyed at any moment by fresh discoveries, whereas genuine positive evidence can never be explained away. [Here we see that Holmes was politely discrediting Simpson's statistical analysis, which he considered to be "the chief adverse argument." He simplified Simpson's table, by removing many columns. This simplified table was copied by Carey in 1988, who added his own brutal criticism (see p. 190).] And positive evidence is by no means lacking. Near the top of the Carboniferous in both South Africa and South America there is a thin band of deltaic clay containing the bones of a small freshwater reptile called *Mesosaurus.* The little animal has been found nowhere else in the world. The region between the ancient deltas in which he lived must have been drained by rivers, and it was therefore occupied by land. The choice evidently lies between accepting continental drift or postulating a giant land bridge across what is now 4,000 miles of ocean. Here the late Carboniferous glaciation of Gondwanaland helps us in making a decision.

THE CLIMATIC ZONES OF THE LATE CARBONIFEROUS

Beds of *tillite* (consolidated till or boulder clay), now known to be of late Carboniferous age, were first recognized in tropical India in 1857, [Actually published in 1856.] in South Australia in 1859, in South Africa in 1870, and in Brazil in 1888. As these amazing discoveries were followed up it became unmistakably clear that Gondwanaland had been glaciated on a gigantic scale at a time when Laurasia enjoyed mild or tropical climates.

The widespread Dwyka tillite of South Africa has been partly obliterated by erosion, and is partly hidden by later formations. But innumerable exposures still occur at intervals from the Transvaal towards the Cape and from South-West Africa to Natal. In many places it can be seen resting on a glaciated floor, characteristically scored with striations. *Roches moutonnées*, excavated rock basins, drumlins, and varved clays have been discovered. The tillite itself contains grooved and ice-faceted boulders and erratic blocks (Plate 95B), some of which have been transported for hundreds of miles from the north. In some localities two or three tillites are known, with intervening interglacial deposits, showing that, as in the Pleistocene ice ages, there was more than one major advance and retreat of the ice. The successive glaciations were not all from the same centre, but migrated from west to east. The associated deposits show that the glaciated region was one of moderate relief, and for the most part low-lying. At the margins the ice terminated in shallow water, marine, brackish, or fresh, which followed up the ice as it retreated. No high mountain range or plateau lay to the north, from which great valley glaciers might have descended. The glaciation was the work of a continental ice sheet that spread outwards under pressure of its own great thickness.

The ice came from centres lying far to the north, and in the latest of the glaciations from beyond Natal, outside the present continent. Since it must have radiated not only toward the south, but outwards in all directions, it follows that the Dwyka tillites should be only part of a once continuous ring of such deposits, surrounding the region of ice dispersal. Confirming this deduction, tillites of the same age have been found in the north of Angola, in the eastern Congo, in Uganda, and in Madagascar, and in the first three of these territories it has been established that the ice moved from the south. As indicated in Fig. 259 [Fig. 69], the ice sheet advanced beyond the Equator.

In India, far to the north of the Equator, similar evidence has been found in Orissa and the Central Provinces, and still farther north in the Punjab. The Himalayas did not then exist, nor did any other mountain range from which the ice might have spread over the plains. Here again we see only a segment of a great ice sheet, a part in which the ice radiated northwards, away from the present Equator. Four of the Australian States, together with Tasmania, were also glaciated from what is now the south. In addition to the equivalents of the Dwyka tillites there is evidence in Australia of an earlier glaciation about the middle of the Carboniferous, and of a later one in the Permian, both of which were also experienced in South America. The tillites of Dwyka age, however, are those best represented in South America, where the ice advanced from what is now the Atlantic over parts of Brazil, Uruguay, and Argentina, and the whole of the Falkland Islands. Of all the fragments of Gondwanaland the only one that has failed to furnish evidence of the late Carboniferous glaciation is Antarctica. As most of Antarctica is at present shrouded by ice, this is not a matter for surprise.

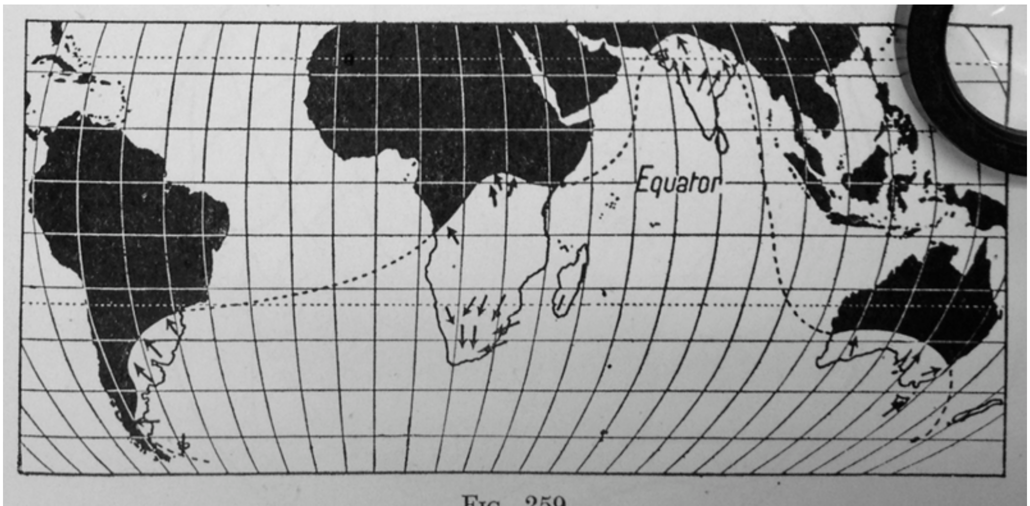
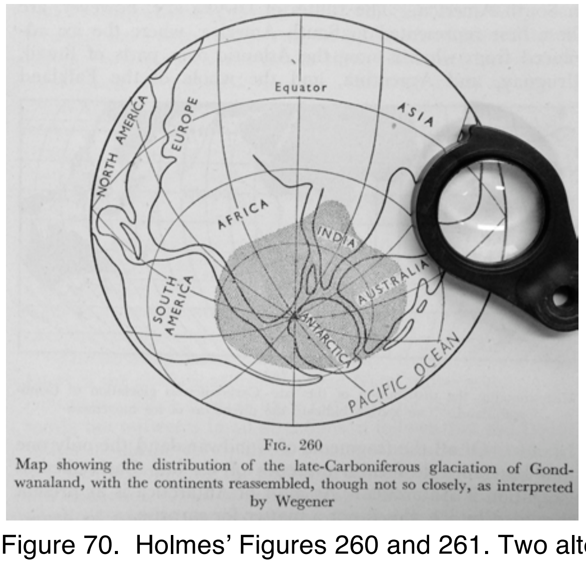


FIG. 259
Map showing the distribution of the late-Carboniferous glaciation of Gondwanaland. The arrows indicate the directions of ice movement

Figure 69. Holmes' Figure 259, supporting his point that "the ice sheet advanced beyond the Equator"

A glance at Fig. 259 [Fig. 69] shows that the glaciated lands now occupy a considerable area of the tropics on both sides of the Equator. With the continents in their present positions such a distribution of ice-sheets is hopelessly inexplicable. The suggestion that Gondwanaland rose from sea level to a plateau so enormously high that it was above the snow line is negatived by ample evidence that it was nowhere very high. But whether it was or not, the tropics could not have been glaciated down to sea level without the development of still greater ice sheets over the northern lands. The only evidence of Carboniferous glaciation in the north is found in Alaska, which has probably never been far from the North Pole, and near Boston, in the Appalachian orogenic belt, which at that time may well have been a high mountain range. On the other hand the great Carboniferous coal forests were flourishing from North America to China while Gondwanaland lay under ice. Moreover, deposits of laterite and bauxite that could only have formed in a tropical climate are found in the Upper Carboniferous of the United States (Kentucky and Ohio), Scotland (Ayrshire), Germany, Russia (south of the Moscow basin), and China (Shantung). The inference that the equatorial zone of the time is roughly indicated by this lateritic belt is irresistible.

No amount of polar wandering, even if it could be admitted, would give a distribution of climatic girdles around the globe corresponding to the picture outlined above. Wherever the South Pole is imagined to have been in order to account for any one of the glaciated regions, it would still have been too distant from the others to account for more than one of them. The problem, indeed, remains an insoluble enigma, unless the straightforward inference is accepted that all the continents except Antarctica lay well to the south of their present positions, and that the southern continents were grouped together around the South Pole. In attempting a circumpolar reassembly the position to be allotted to Antarctica is necessarily uncertain. Wegener places it between Australia and South America (Fig. 260); whereas Du Toit, guided by meagre stratigraphical and tectonic clues, thinks it may have been between Australia and Africa (Fig. 261) [Fig. 70]. With either arrangement the ice sheets all fall within an area comparable with that glaciated in the Northern Hemisphere during the Pleistocene. Moreover, as indicated in Fig. 260, the lateritic belt then comes into line with the Equator of the time, and other known details of the Carboniferous climatic girdles also fall consistently into their appropriate places. The site of the Hawaiian Islands would have been approximately over the North Pole at this time. Consequently, no evidence of a North Polar ice cap is to be expected. The nearest of the present land areas where sights of glaciation might reasonably be looked for are California and Alaska. The Carboniferous rocks of California are marine sediments, where again no evidence could be expected. But in Alaska a late Carboniferous tillite occurs, just where it ought to be.



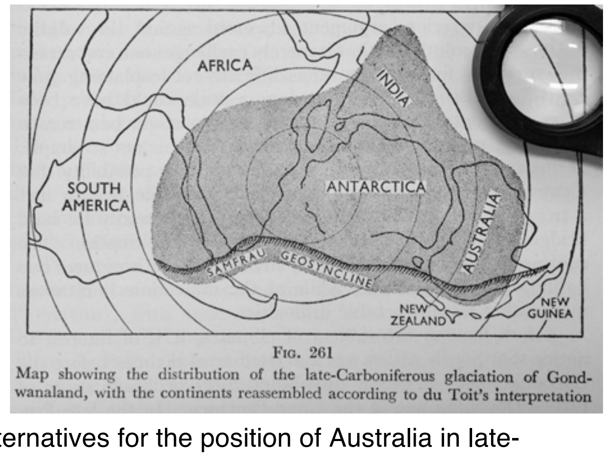


Figure 70. Holmes' Figures 260 and 261. Two alternatives for the position of Australia in late-Carboniferous time.

[From the above paragraphs, we see that Holmes' enthusiasm for the climate evidence is far greater than for the other types of drift evidence. He clearly understood that continental drift is the only way of explaining paleoclimates. Wegener also understood this, but climate arguments never received the attention they deserved in the discussion of continental drift. Part of the problem was that Wegener had put most of the paleoclimate evidence in the book with Köppen, which was not translated to English and was easily overlooked.]

The only serious argument advanced against the validity of the above solution is that it merely exchanges one embarrassing problem for another—the difficulty of explaining how continental drift on so stupendous a scale could have been brought about. By itself this consideration might be a reason for sitting on the fence, but the real antithesis is not so simple. If one rejects continental drift and accepts the possibility that Central Africa could have been glaciated while Britain had a tropical climate, one must also admit the necessity for land bridges, which have since subsided to oceanic depths. The continental drift solution has the advantage that it reduces two baffling problems to one, while at the same time it removes many other less intractable difficulties.

Before leaving the subject of climates, it is of interest to notice that South Africa was glaciated several times before the Carboniferous. A widely distributed tillite occurs in strata of Lower Devonian age in the Cape Province. In the late Pre-Cambrian, glaciation occurred on a scale comparable with that of the late Carboniferous—the regions affected including the Transvaal, Rhodesia, the Congo, Angola, and South-West Africa. [This Late Precambrian glaciation actually occurred on such a huge scale as to make it incomperable.] Still earlier in the Pre-Cambrian yet another tillite is preserved in the Transvaal, and farther back still South-West Africa provides evidence of what may be one of the earliest glaciations known. It thus appears that for at least 1,000 million years the position of Africa relative to the South Pole did not significantly alter. Africa gives us no evidence of having drifted from a situation far to the south until comparatively late in its geological history.

THE SEARCH FOR A MECHANISM

It has been shown that in looking for a possible means of "engineering" continental drift we must confine ourselves to processes operating within the earth. To be appropriate, the process must be capable (a) of disrupting the ancestral Gondwanaland into gigantic fragments, and of carrying the latter radially outwards as indicated in Fig. 210: Africa and India toward the Tethys; Australasia, Antarctica, and South America out into the Pacific; (b) of disrupting Laurasia, though much less drastically, and again with radially outward movements towards the Tethys and the Pacific, as indicated in Fig. 209. We have already seen that the peripheral orogenic belts probably mark the regions where opposing systems of sub-crustal currents came together and turned downwards. The movements required to account for the mountain structures are in the same directions as those required for continental drift, and it thus appears that the sub-crustal convection currents discussed on pages 408 to 413 may provide the sort of mechanism for which we are looking (Fig. 262) [Fig. 71].

[Holmes still did not point out that many of his ideas here and some of the figures were from his article in 1931. In this textbook he was presenting his current ideas, but not the history of those ideas.]

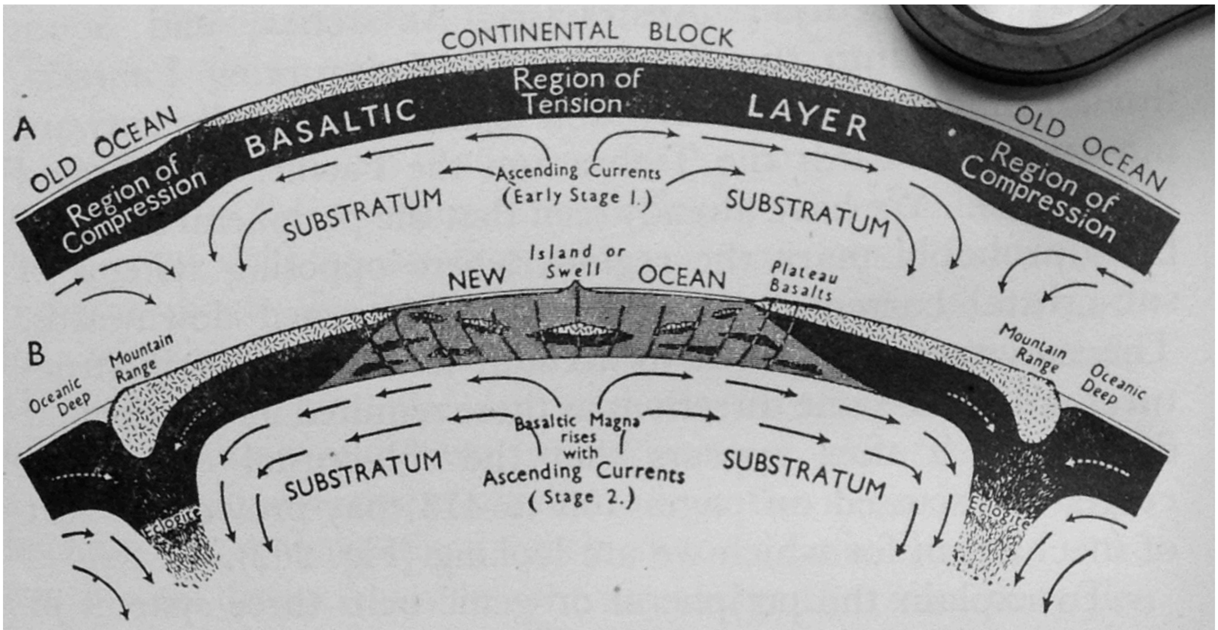


FIG. 262

Diagrams to illustrate a purely hypothetical mechanism for "engineering" continental drift. In A sub-crustal currents are in the early part of the convection cycle (Stage 1 of Fig. 215). In B the currents have become sufficiently vigorous (Stage 2 of Fig. 215) to drag the two halves of the original continent apart, with consequent mountain building in front where the currents are descending, and ocean floor development on the site of the gap, where the currents are ascending

Figure 71. Holmes' Fig. 262.

To explain the peripheral orogenic belts three systems of convection currents are called for (or three co-ordinated groups of systems), with their ascending centres situated beneath Gondwanaland, Laurasia, and the Pacific respectively. Incidentally, it should be noticed that the coalescence of the usual chaotic or small convective systems into three gigantic ones involves a coincidence that can rarely have happened in the earth's history, and one that is just as likely to have come about during the Mesozoic era as at any other time. The often-asked question: How is it that Pangæa did not begin to break up and unfold until Mesozoic time? thus ceases to have any significance. If continental drift could have been caused by the gravitational forces invoked by Wegener, then it should have occurred once and for all very early in the earth's history, since those forces have always been in operation. If convection currents are necessary, continental drift may have accompanied all the greater paroxysms of mountain building in former ages but, if so, it would usually have been on no more than a limited scale. That there was a quite exceptional integration of effort in Mesozoic and Tertiary times is forcibly suggested by eruptions of plateau basalts and building of mountains on a scale for which it would be hard to find a parallel in any earlier age.

There are, therefore, good reasons for supposing that at this critical period of the earth's history the convective circulations became unusually powerful and well organized. Currents flowing horizontally beneath the crust would inevitably carry the continents along with them, provided that the enormous frontal resistance could be overcome. The obstruction that stands in the way of continental advance is the basaltic layer, and obviously for advance to be possible the basaltic rocks must be continuously moved out of the way. In other words, they must founder into the depths, since there can be nowhere else for them to go (Fig. 262).

Now this is precisely what would be most likely to happen when two opposing currents come together and turn downwards beneath a cover of basaltic composition. The latter then suffers intense compression, and like the sial in similar circumstances it is eventually drawn in to form roots (*c.f.* Figs. 215 and 216). On the ocean floor the expression of such a down-turning of the basaltic layer would be an oceanic deep. The great deeps bordering the island festoons of Asia and the Australian arc (Tonga and Kermadec) probably represent the case where the sialic edge of a continent has turned down to form the inner flanks of a root, while the oceanic floor contributes the outer flank.

It is not difficult to see that a purely basaltic root must have a very different history from one composed of sial. The density of sial is not significantly increased by compression. Consequently, when a sialic root is no longer being forcibly held down, it begins to rise in response to isostasy, heaving up a mountain range as it does so. But when rocks like basalt or gabbro (density 2.9 or 3.0) are subjected to intense dynamic metamorphism they are transformed into schists and granulites and finally into a highly compressed type of rock called *eclogite*, the density of which is about 3.4. Since this change is known to have happened to certain masses of basaltic rocks that have been involved in the stresses of mountain building, it may safely be inferred that basaltic roots would undergo a similar metamorphism into eclogite. Such roots could not, of course, exert any buoyancy, and for this reason it is impossible that tectonic mountains could ever arise from the ocean floor. On the contrary, a heavy root formed of eclogite would continue to develop downwards until it merged into and became part of the descending current, so gradually sinking out of the way, and providing room for the crust on either side to be drawn inwards by the horizontal currents beneath them (Fig. 262).

[Geologists in 1944 still thought that the mantle was mafic, not ultramafic.]

The eclogite that founders into the depths will gradually be heated up as it shares in the convective circulation. By the time it reaches the bottom of the substratum it will have begun to fuse, so forming pockets of magma which, being of low density, must sooner or later rise up to the top. Thus an adequate source is provided for the unprecedented flows of plateau basalt that broke through the continents during Jurassic and Tertiary times. Most of the basaltic magma, however, would naturally rise with the ascending currents of the main convectional systems until it reached the torn and outstretched crust of the disruptive basins left behind the advancing continents or in the heart of the Pacific. There it would escape through innumerable fissures, spreading out as sheet-like intrusions within the crust, and as submarine lava flows over its surface. Thus, in a general way, it is possible to understand how the gaps rent in the crust come to be healed again; and healed, moreover, with exactly the right sort of material to restore the basaltic layer. To sum up; during large-scale convective circulation the basaltic layer becomes a kind of endless travelling belt on the top of which a continent can be carried along, until it comes to rest (relative to the belt) when its advancing front reaches the place where the belt turns downwards and disappears into the earth.

To go beyond the above indication that a mechanism for continental drift is by no means inconceivable would at present be unwise. Many serious difficulties still remain unsolved. In particular, it must not be overlooked that a successful process must also provide for a general drift of the crust over the interior: a drift with a northerly component on the African side sufficient to carry Africa over the Equator, and Britain from the late Carboniferous tropics to its present position. The northward push of Africa and India, of which the Alpine system and the high plateau of Tibet are spectacular witnesses, could not have been sufficient by itself to shove Europe and Asia so far to the north. To achieve this, the aid of exceptionally powerful sub-

Laurasian currents directed towards the Pacific is required. The total northward components might then overbalance the southward components, and a general drift of the crust would be superimposed on the normal radial directions of drift.

It must be clearly realised, however, that purely speculative ideas of this kind, specially invented to match the requirements, can have no scientific value until they acquire support from independent evidence. The detailed complexity of convection systems, and the endless variety of their interactions and kaleidoscopic transformations, are so incalculable that many generations of work, geological, experimental, and mathematical, may well be necessary before the hypothesis can be adequately tested. Meanwhile it would be futile to indulge in the early expectation of an all-embracing theory which would satisfactorily correlate all the varied phenomena for which the earth's internal behaviour is responsible. The words of John Woodward, written in 1695 about ore deposits, are equally applicable to-day in relation to continental drift and convection currents: "Here," he declared, "is such a vast variety of phenomena and these many of them so delusive, that 'tis very hard to escape imposition and mistake."

SUGGESTIONS FOR FURTHER READING

A. Wegener, *The Origin of Continents and Oceans*. Methuen, London, 1924.

A. L. Du Toit, *Our Wandering Continents*. Oliver and Boyd, Edinburgh, 1937.

T. H. Holland, *The Evolution of Continents: A Possible Reconciliation of Conflicting Evidence. Proceedings of the Royal Society of Edinburgh*, Vol. LXI., Part II., No. 13, 1941.

Only three references are given here, making this the second shortest reference list of his 21 chapters. Holmes did not include references to his own important contribution: his 1931-paper *Radioactivity and Earth Movements*. He had included this reference in an earlier chapter, but readers would not see that it had important ideas regarding continental drift. Similarly, radioactive heating in the mantle, which Holmes understood to be the underlying cause of continental drift, is not mentioned in this final chapter. That topic was covered elsewhere in the book, but I think its absence here was unfortunate.

Holmes summarized geology in much the same way that we understand it in our plate-tectonic theory. British students who first learned geology using Holmes' textbook were thus favorably disposed to continental drift. It is clear that it was the best, indeed the only theory for explaining many geological phenomena. Holmes would not risk his reputation by strongly supporting this hypothesis. He added a disclaimer in the last paragraph that these ideas are purely speculative, have not yet been adequately tested, and thus far have no scientific value.

AMADEUS GRABAU, THE CONTINENTALLY DISPLACED PROFESSOR

Shand did not revise or expand his geology book after he had gotten the professorship at Columbia University. Its criticizm of the biblical story of Genesis, and its support for the theory of continental drift would have made it controversial there.

There are limits to academic freedom, even for professors. Two decades earlier at Columbia University there had been a geology professor who had too little regard for such limits. Amadeus W. Grabau (1870-1946) was a leader in the same fields of research and teaching as Charles Schuchert. But Grabau's career took a sharp turn midway through.

Grabau seems to have offended many people in many ways (Mazur 2004). Although he had been raised in a devoutly Lutheran home, he married a Jewish woman who then became an atheist. Her name was Mary Antin. She had arrived in America as a penniless immigrant, and gained national attention as a bestselling socialist author in 1912, with secret writing help from Grabau.

Grabau's connection to the radical Antin probably bothered his colleagues. But worse were Grabau's sympathies for Germany in the First World War. One of his colleages wrote a letter to a newspaper that became nationally known. The letter's purpose was to scold an unpatriotic professor, who was unnamed, but locally recognized to be Grabau. His colleagues probably felt that he was disloyal to them, to his religious upbringing, and to his country.

Grabau's academic production was phenomenal. He wrote an immense two-volume reference work, *North American Index Fossils*, with a student as coauthur (Grabau and Shimer 1909, 1010). A few years later, he published the first textbook devoted entirely to stratigraphy, a monumental 1185 pages.

Although Grabau was a geologist, a paleontologist, and a stratigrapher, he saw paleogeography as the greatest goal of these disciplines. He expressed this clearly in the Preface and in the final statement of his stratigraphy textbook:

Grabau 1913, p. ix-x.

Palæogeography, as a science, is of a very recent development, most of the works of importance having appeared in the last five years. In America Schuchert and Bailey Willis are the acknowledged leaders, while in Europe many able minds have attacked the problems of Palæogeography from all angles.

Grabau 1913, p. 1147.

When the science of Stratigraphy has developed … we may hope that Palæogeography, the youthful daughter science of Stratigraphy, will have attained unto that stature which will make it the crowning attraction to the student of earth history.

Today, in the context of plate tectonics, paleogeography has attained much of the stature that Grabau hoped for it. The ancient geographical positions of continents and oceans are of prime importance to understanding their sedimentary deposits, fossils, and resources, as well as the development of mountain ranges.

Grabau saw that the modern globe was incapable of explaining ancient climates, especially the Permo-Carboniferous glaciation. He advocated polar wandering as a working hypothesis. Maybe the axis of the Earth had been different during Permo-Carboniferous time. Perhaps the South Pole had been located near India and the North Pole in the Pacific Ocean. This interpretation contrasts with that of Schuchert, who favored the alternative idea of extreme global climate fluctuations in the Permian.

Grabau was dissatisfied with the limitations inherent in orthodox paleogeography and paleogeographic maps. He pointed out that many landmasses and sedimentary deposits could not be plotted on a modern base map, because of the great amounts of horizontal compression during mountain building. He was not (yet) advocating global-scale migrations of continents and sedimentary basins:

Grabau 1913, p. 1146-1147.

CONSTRUCTION OF PALÆOGEOGRAPHIC MAPS. In the construction of palæogeographic maps it is first of all necessary to bear in mind that modern geographic maps can at least serve only as a distorted base for such depiction. Thus the Appalachian region of North America, and the region of the Alps in southern Europe are areas where the earth's crust has been greatly foreshortened, and where, hence, localities far apart at an earlier time were brought close together. It is, of course, impossible to allow for such foreshortening, if the localities where certain formations crop out to-day are to be brought into the seas in which they were deposited. Thus, as will be seen on the maps for the Lower Cambric (Fig. 264a) [Fig. 72], the New England land barrier between the Atlantic and the Pacific extension in the Appalachian or Cumberland trough is much too narrow, while the width of that trough is also too small. The same is true for the land-barrier in North Britain, between the Atlantic and Arctic oceans. Since, however, the rocks carrying the faunas of these two seas are found so much nearer together to-day than was the case at the time of the deposition, such faulty construction seems to be unavoidable.

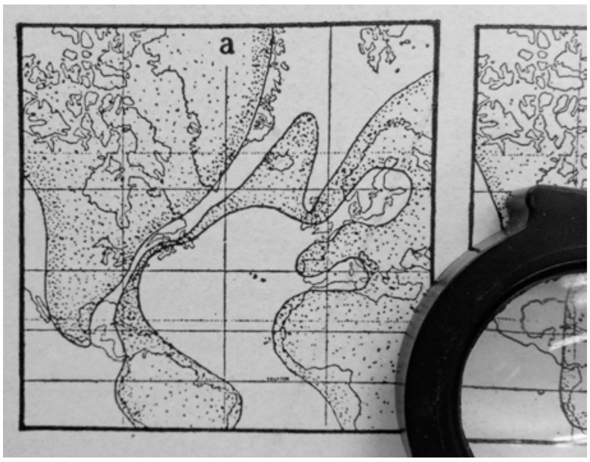


Figure 72. Grabau's Fig. 264a, showing a remarkable land bridge extending from Scandinavia to South America. This bridge divides the marine fauna into a Pacific province (touching northern Great Britain) and an Atlantic province (including southern Great Britain). These paleogeographic features could not be properly shown; they had been much wider, but were compressed during later mountain-building episodes. From Grabau (1913.)

Like Lyell, Schuchert, and many others who worked in the Appalachians, he was fully aware that a sizable landmass existed east of the North American continent, where only deep ocean exists today:

Grabau 1913, p. 1147.

Important factors that must not be overlooked in the construction of palæogeographic maps are the nature of the sediment and its source. Where coarse clastic sediments abound in the formation, a land of sufficient size must have existed to furnish this sediment. This is especially the case when the sediment consists of well-assorted material, such as quartz-sand or pebbles, when it must be remembered that such assorted material may represent only a part, perhaps less than one-fourth, of the original rock which was its source. In general, it may be said that much closer discrimination between marine and non-marine sediments than has generally been the case is necessary.

Grabau had excelled in his research, publications, and teaching at Columbia from 1901 to 1918. He was well liked by his students, but not by some of his colleagues. During the Great War there were few students available for his courses. The university terminated his position. He lost his job, and separated from his wife and daughter.

He was unable to get an appropriate position at any university. He must have had the wrong enemies and not the right friends. Finally he was offered a professorship at Peking University, with excellent research facilities and field support, and a salary that was eight times the one he had been receiving at Columbia. He moved to China in 1920, and continued his research and teaching there for the next 25 years. From the success of his students, he earned the reputation as the "Father of Chinese geology." He returned only once to America, for the International Geological Congress in Washington D. C. in 1932.

Soon after Grabau arrived in China, his next geology textbook appeared back in North America. It was a two-volume set, an 864-page *General Geology* (1920) and a 976-page *Historical Geology* (1921). It included 31 paleogeographic maps of North America, as well as many paleogeographic maps of Europe, Asia, and the world. These were his own versions of the Earth's paleogeography, offering alternatives to the interpretations previously published by Schuchert and Willis.

Grabau explained his paleogeographic interpretations even more fully than Schuchert had done, giving more of the details of fossils and stratigraphy. He expanded the discussion of Cambrian faunal provinces (Fig. 73), which I find quite fascinating. These provinces became key evidence for later recognition of the Iapetus Ocean in plate tectonics.

Grabau 1921, p. 227-231.

Separate Faunal Provinces – In the study of Cambrian faunas it becomes apparent that there are distinct faunal provinces, and that the organisms of one have little or nothing in common with those of the others, which would indicate the existence of effective barriers between those provinces, such barriers being either land, currents, etc., or climate. The known provinces are the Pacific, the Atlantic, and the Indian. The Pacific province was the largest, and in North America, waters from this province filled the geosynclines and transgressed over the low land between. On the other side of the ocean, it overlapped Chinese territory, for fossils, often of the same species as those characteristic of the North American Cambrian of this province, have been found in China.

The Atlantic province was separated from the Appalachian geosyncline, which belonged to the Pacific province extension, by the land mass of Appalachia. This appears to have extended through the center of Newfoundland, for in the western part of that land deposits of the Pacific province are found, while in the extreme eastern part those of the Atlantic province occur. Other deposits of the Atlantic province are still preserved in Cape Breton, New Brunswick, and eastern Massachusetts, while on the opposite side of the ocean, the Anglo-Baltic geosyncline and a second embayment, the Mediterranean, which extended to Bohemia, contain deposits with the Atlantic fauna. Of much interest is the fact that while the deposits of Wales and the adjoining English districts belong to the Atlantic type, those of northwest Scotland belong to the Pacific type, indicating that a sufficiently continuous barrier extended for part of the time, at least, from central Newfoundland to the Scottish Highlands and thence to Scandinavia, and that the Appalachian geosyncline was continued to the north of this barrier (Figs. 1032, 1038). The Indian province was distinct from both the others. [Grabau's Fig. 1032 was a redrawn version Fig. 72.]

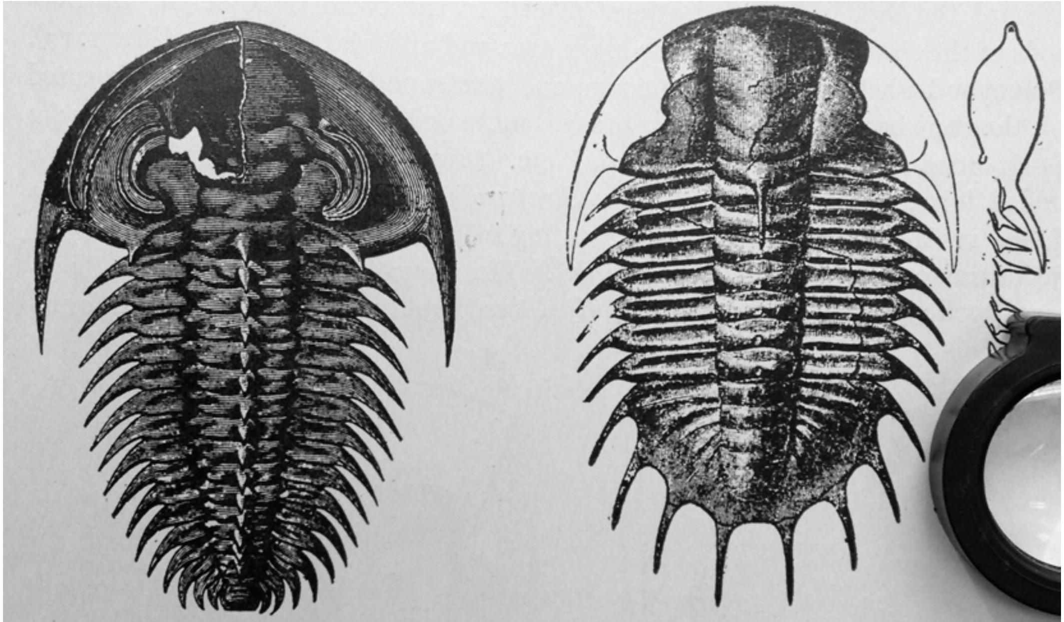


FIG. 1024. — *Holmia kjerulfi.* Lower Cambrian, Atlantic province, Europe. (Kayser.)

FIG. 1025. — *Olenoides curticei.* Middle Cambrian, Pacific province. (I. F.)

Figure 73. Grabau's figures of two characteristic trilobite species, one belonging to the Atlantic province and the other belonging to the Pacific province. From Grabau (1921).

On his paleogeographic maps he showed more information than others had done. He tried to indicate not only the coastlines, but also the locations of highlands, and the flow-directions of ancient rivers (Fig. 74), based on the preserved record of sedimentary deposits and fossils.

FIG. 1399. — Palæogeographic map of North America, showing the distribution of land and sea (black) in Carbonic time. (Original.)

Figure 74. Grabau’s figure showing the highland *Appalachia* as an eastern extension of North America, and the land bridge *Atlantica* across Greenland to Europe. Note his attempt to indicate Carboniferous rivers and highlands, not only coastlines. From Grabau (1921).

Grabau's texts *General Geology* and *Historical Geology* were written for the same college market that the books by Pirsson and Schuchert were serving. This was stiff competition, and Grabau's books were printed only once. On the other hand, his earlier book *Principles of Stratigraphy* had no competitors, and was reprinted without revisions in 1924, 1932, and 1960. In 1957, Yale professors Carl Dunbar and John Rodgers wrote a new *Principles of Stratigraphy,* which soon took over that college market.

Grabau first mentioned Wegener's hypothesis in a paper on the migration of geosynclines, published in 1924. He respected Wegener’s ideas, and reprinted many paragraphs from the English translation of Wegener’s book. But he noted that geophysicists were skeptical about such extreme horizontal displacements. After several pages of quotations, he simply commented that the geophysical means by which continents could move was not known:

Grabau 1924, p. 273.

These are of course assumptions and their validity depends on the establishment of the theory on a geophysical basis. That there was enormous compression of the Asiatic continent with the result that India now lies relatively much farther to the north than before, can of course not be disputed, but this theory brings us no nearer the solution of the problem of the reason for the location of the geosynclines where we find them, and of the folding of their strata at such varying periods, and in diverse directions.

Grabau eventually became convinced that mobilism had the right answers to the problems that he had mentioned in 1913. The development in his conversion to mobilism is clear: In 1913 and 1921, Grabau taught fixed continents with land bridges, but plainly pointed out difficulties with these theories. In 1924 he respected Wegener's working hypothesis, but was not convinced that it was feasable. By 1936 he was using the reconstruction of Pangæa in his paleogeographic interpretations.

Grabau devised a new model for the history of the Earth, which he called the pulsation theory. He contended that worldwide sea level had risen and fallen on a time scale of millions of years. The changes in sea level can be recognized by the sequences of sedimentary rocks. He first introduced this idea of pulsation at the International Geological Congress in 1932. But he avoided any mention of Pangæa in his publication there. Soon after, he more fully developed the hypothesis, and explained it in a four-volume set of books comprising 3,222 pages (1936a, 1936b, 1937, 1938) entitled *Palæozoic Formations in the Light of the Pulsation Theory*. Now he had fully embraced mobilism, and built his pulsation theory around it. These books included several large folding plates showing the entire continent Pangæa.

His Pacific-type and Atlantic-type geosynclines fit together much more reasonably on a map of Pangæa, without any intervening Atlantic Ocean. He drew this part of Pangæa in a map that we can now call the Grabau-fit (Fig. 75):

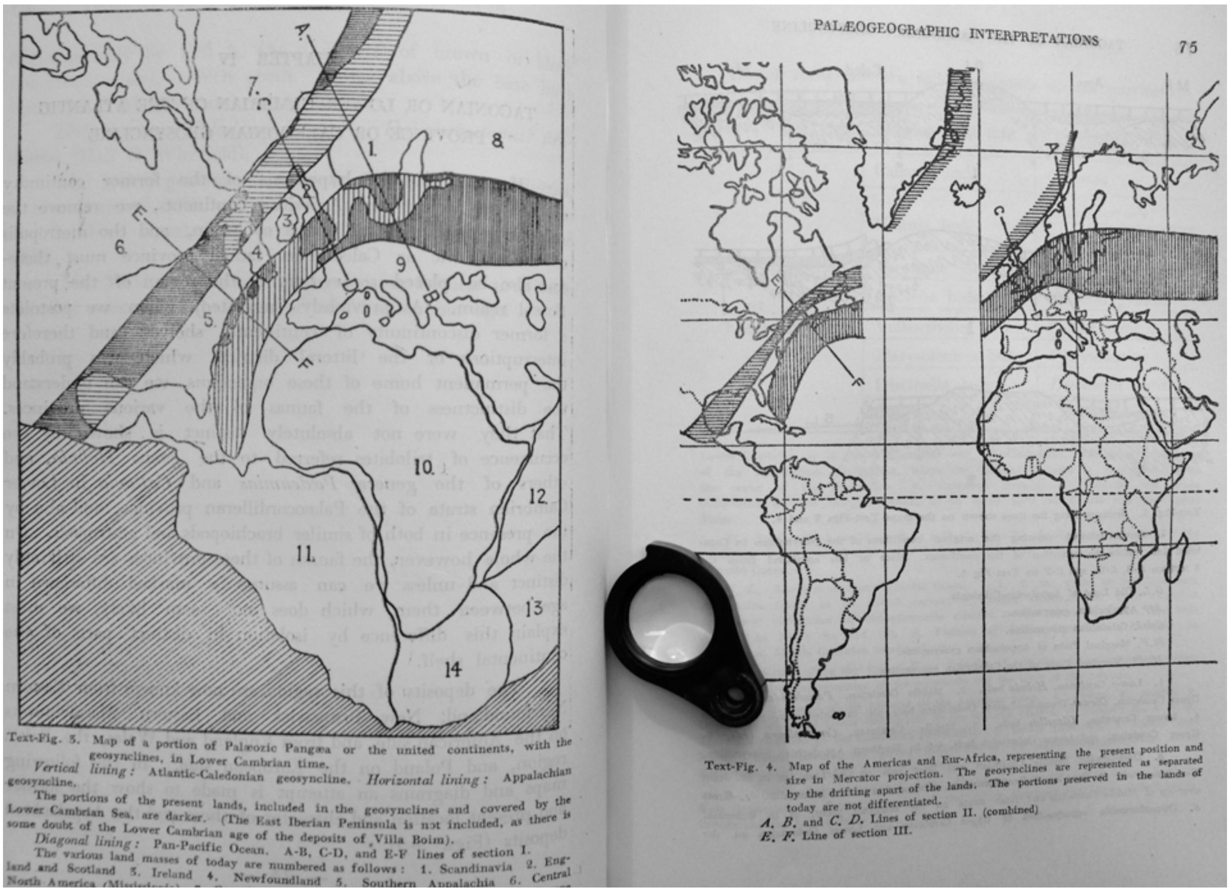


Figure 75. The Grabau-fit, making sense of Paleozoic North Atlantic geology. His figure text reads: Map of a portion of Palæozoic Pangæa or the united continents. From Grabau (1936a).

These maps showed the same parts of the geosynclines that he had dealt with in 1913. Now they made more sense to him. And he could extend those interpretations even further north. The Norwegian geologist Olaf Holtedahl had shed new light on the geosynclines in the Arctic regions. Grabau published a map to show the solution that mobilism could provide there (Fig. 76).

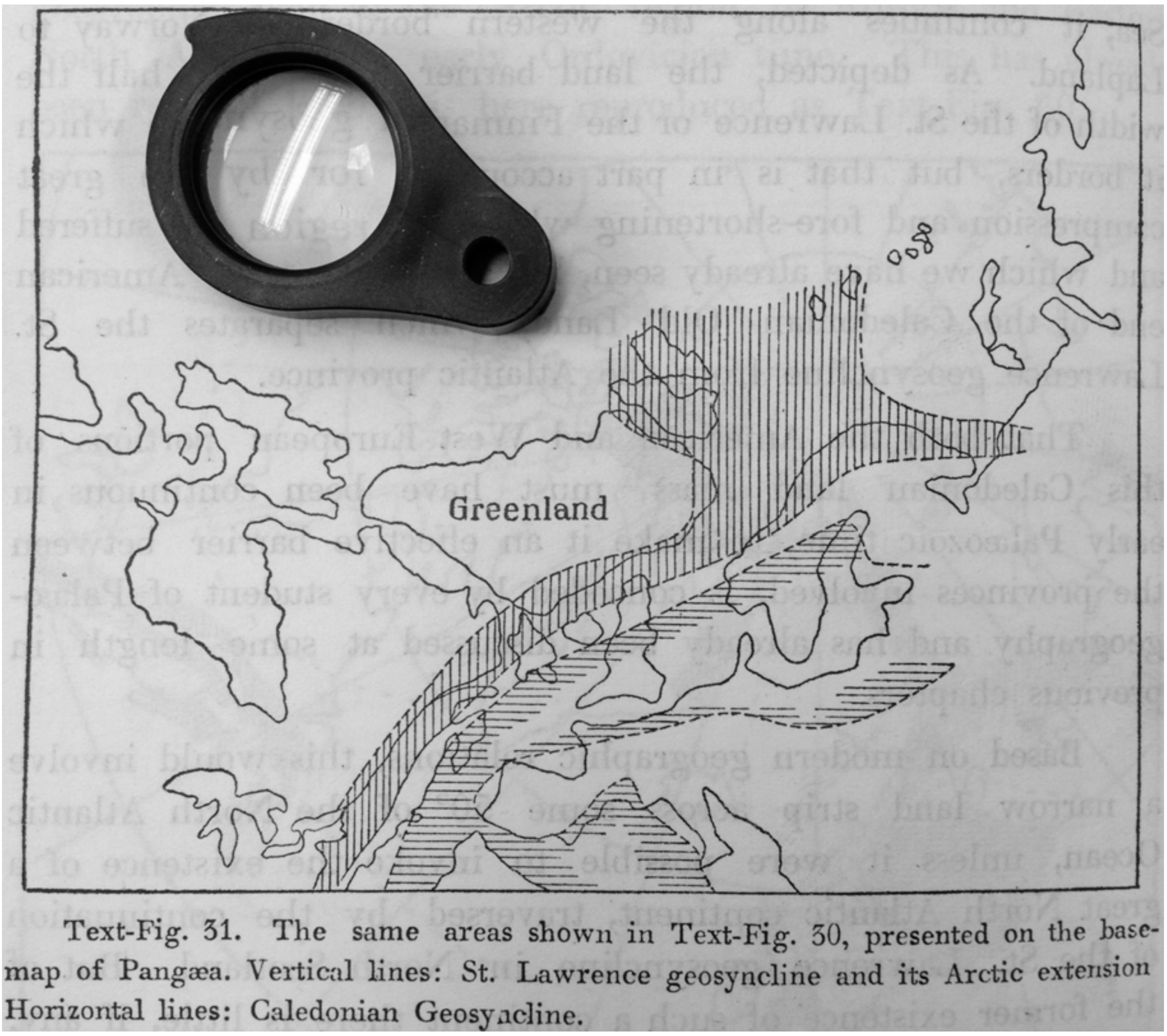


Figure 76. The map of Pangæa also solved problems of geosynclines in the Arctic. From Grabau (1936b).

Grabau followed up the four-volume set with another book of visionary ideas: *The Rhythm of the Ages; Earth History in the Light of the Pulsation and Polar Control Theories* (Grabau 1940). It further developed the ideas of periodic sea-level fluctuations. He proposed that the periods of the Paleozoic (Cambrian, Ordovician, Silurian... be replaced with thirteen distinct “pulsation systems” (Taconian, Cambrian, Cambrovician...), each including a trangression and regression. Pangæa wandered slightly, more or less locked to the South Pole. He suggested that the gravitational pull of some unknown extraterrestrial object held Pangæa close to the pole.

For Grabau, the supercontinent Pangæa and the oceanic pulsations steered the geological evolution throughout Paleozoic time. He showed the relative movements of Pangæa in a series of 25 large folding plates, all in color. Grabau was generously funded in China.

Grabau’s Plate 3 (Fig. 77) shows Pangæa in Cambrian time, with three distinct faunal provinces: the Pacific province (Appalachian geosyncline), the Atlantic province (Caledonian geosyncline) and the Indian province (the geosyncline stretching from Peking to Antarctica.) The south-polar glaciation was centered on northern Africa at that time. His other large plates show how Pangæa wandered slightly, so that the pole moved to South Africa, Antarctica, Australia, India, and finally to its present position in Antarctica.

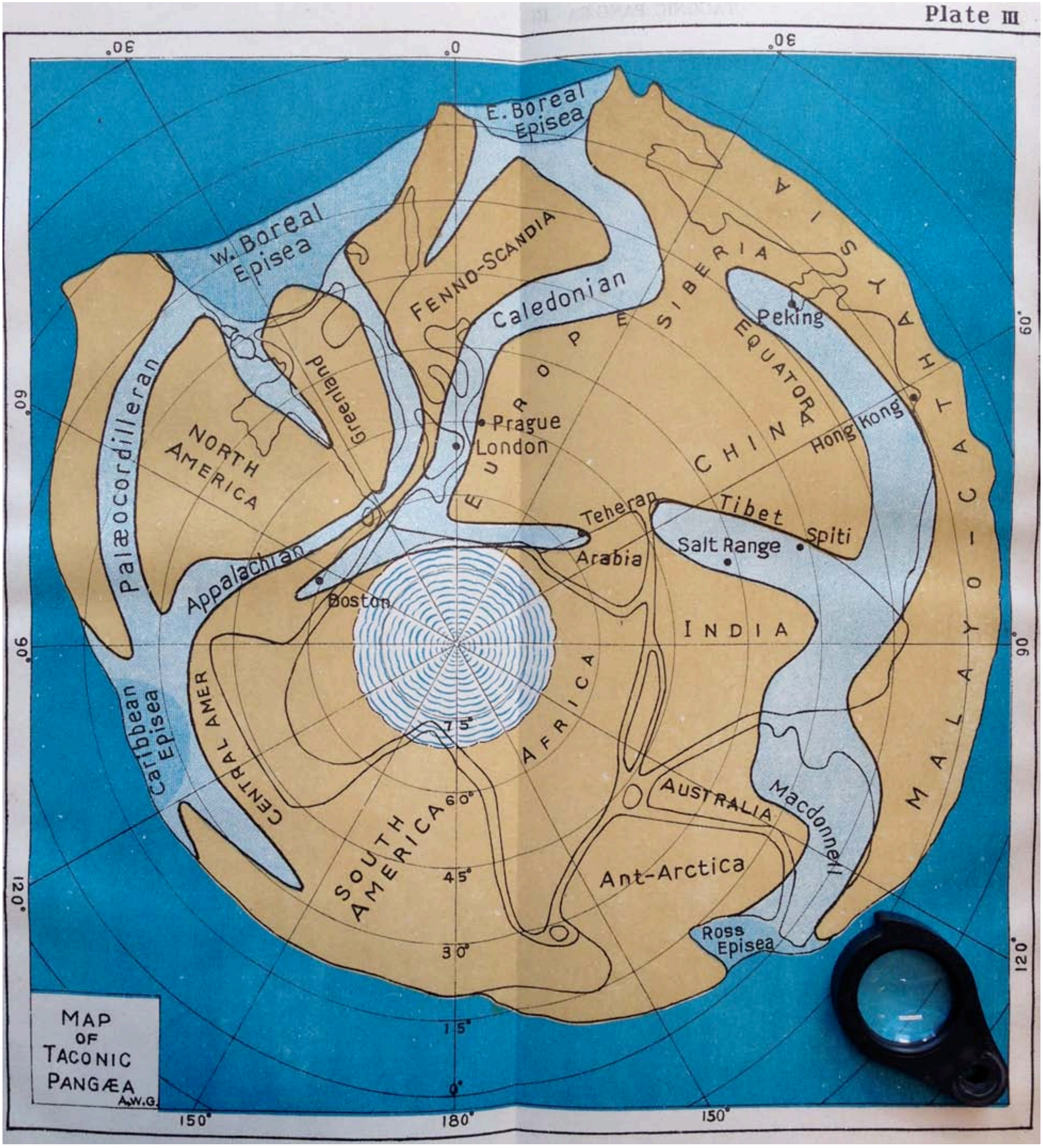


Figure 77. Grabau's Plate 3, reconstruction of Pangæa in Cambrian time. From Grabau (1940).

Grabau criticized Schuchert for his attack on the Wegener-fit of Africa and South America. He wrote that the Franciscan geosyncline of South America, which Schuchert (1928) had called a "crushing blow to the displacement hypothesis," had now become a "boomerang." He showed on his Plate 8 (Fig. 78), that the Franciscan and Amazonian geosynclines do indeed cross Pangæa into northern Africa in Devonian time. He wrote:

Grabau 1940, p. 332.

It would thus appear that this missive hurled by the opponents of continental drift, in the expectation that it would smash at a blow the whole structure built by Taylor and Wegener and by Du Toit and his followers, has missed its mark and may now be picked up by the protagonists of the new view and incorporated as one of the corner-stones of the rebuilt scientific edifice devoted to earth evolution.

Wegener's was a fit-map, Schuchert's was a fight-map, and Grabau's was a retrofit-map. Most geologic maps argue in subtle way for the interpretations of the authors who draw them. No geologic map is just a map.

Note that in Plate 8, the south polar ice cap was positioned over southeastern Africa. As the pole moved south in Carboniferous time, its position would fit the glacial striations that had been mapped by Du Toit and illustrated by Schuchert. Note also that Greenland-Scandinavia-Britain formed a single continental landmass that Grabau called *Atlantica*. Geologists have often called this Devonian landmass the "Old Red Continent." He correctly showed *Appalachia* to not only include the eastern part of North America, but also the western part of Africa.

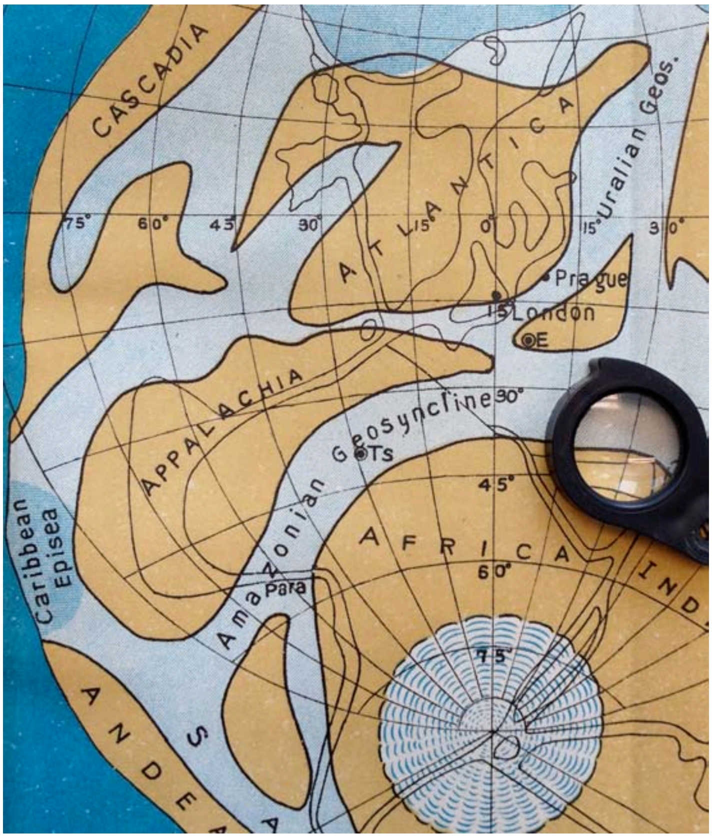


Figure 78. Grabau's Plate 8, showing the Amazonian Geosyncline crossing from South America to Africa where it forks to form the Franciscan Geosyncline. The landmass Atlantica unites North America, Greenland and Scandinavia. Appalachia is a part of Africa, and a polar ice cap covers southeastern Africa in Devonian time. From Grabau (1940).

Grabau was willing to abandon earlier committments, both in his personal life and in his science. He moved from America to China, and he moved from fixism to mobilism.

He was able to retract his own published opinions. Early in his career, Grabau had established a reputation as the leading critic of a controversial interpretation by Edward Oscar Ulrich (1857-1944) a paleontologist / stratigrapher with close ties to Schuchert. Grabau succeeded in discrediting Ulrich’s theory, but in his later years, he suddenly switched to Ulrich’s side. In a footnote in his 1940 book, he wrote:

Grabau 1940, p. 112.

The establishment of the Ozarkian System as an independent system is the monumental achievement of Dr. Ulrich, and that he has triumphantly maintained it in spite of all opposition is a tribute to Ulrich’s scientific acumen. As one of his former critics I take pleasure in acknowledging my conversion to the “Ozarkian doctrine.”

After Grabau emigrated to China he was a *persona non grata* in North America. His books and research articles continued to appear, but western geologists were mostly unaware of them. After the general acceptance of plate tectonics it became clear to some geologists that Grabau had been a visionary. Albert Carozzi, who had translated and republished Argand’s *Tectonics of Asia*, also saw to it that Grabau’s 1940-volume was reprinted in 1978. That publication was of keen interest for another reason: Grabau's ideas of world-wide sea-level pulsations had much in common with the exciting new theory of sequence stratigraphy.

Grabau, as well as Wegener, envisioned that Pangæa had been intact throughout Paleozoic time. We now know that most of the northern continental fragments were assembled into Pangæa gradually during the Paleozoic. Grabau’s Caledonian and Appalachian geosynclines in Cambrian time (see Fig. 77) were opposite margins of the wide Iapetus Ocean. Grabau’s Pangæa-paleogeography had its flaws, but it was far better than the alternative of fixism.

Grabau wrote many books (Fig. 79), but never produced a revised edition of an earlier title.

Figure 79. Amadeus Grabau was highly productive; the books mentioned here alone contain 8,800 pages. From left: 1909, 1910, 1913, 1920, 1921, 1924, 1936a, 1936b, 1937, 1938, 1940, 1961. (See references for titles).

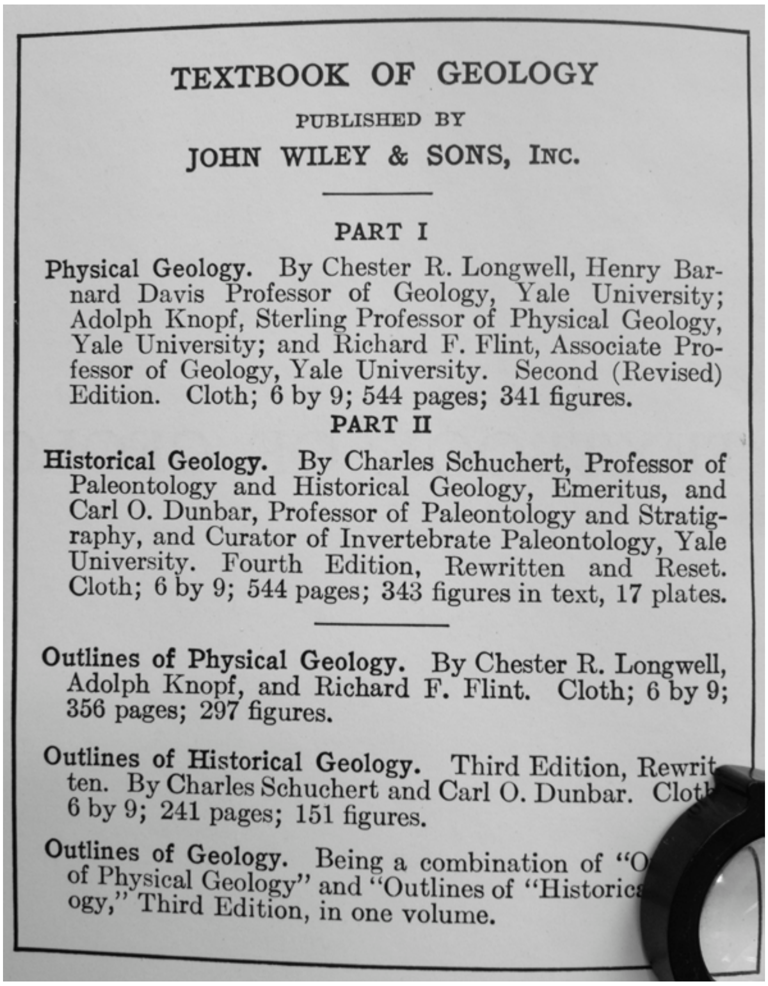
WILEY'S *TEXTBOOK OF GEOLOGY*, GROUNDING YOUNG MINDS

Revised editions of geology textbooks can show new developments in the science. They can also help us to understand the minds of the textbook authors.

Successful university textbooks are usually revised every few years. Some sentences and paragraphs are new, while others are rewritten. Figures are added, replaced, or removed. In the preface to the new edition, the authors proudly mention some of the improvements. But they cannot also mention that an important purpose of a new edition is to make the previous book obsolete. Publishers insist on a new edition every few years, so that students will buy new books instead of reusing old ones. This is a textbook example of planned obsolescence. But the insistence on a new edition also involves risk for the publisher. If the changes are too great, the book seems so unfamiliar to the course instructor that he might consider changing to another publisher's book. If the changes are too minimal and come too frequently, the instructor might get annoyed about having to adjust his lecture notes, and give up on this bothersome textbook series.

There are currently many geology textbooks that compete for the North American college market. Previously, that market was completely dominated by the *Textbook of Geology* (Fig. 80), written and revised by respected Yale professors and published by John Wiley and Sons. The first edition was the two-volume set, *Part I Physical Geology* by Louis V. Pirsson and *Part II Historical Geology* by Charles Schuchert. They were available separately or as a single book. For a few decades there were two versions of each volume, a full *Textbook* version, and an abridged *Outlines* version. With two versions available, instructors had a choice of size and price for their students. But it's not size that matters, it's what you do with it; both versions warned students that continental drift was just speculation that should be avoided.

Physical Geology appeared as revised editions (either as *Textbook* or *Outlines* versions) in 1915, 1920, 1929, 1930, 1932, 1934, 1939, 1941, 1948, 1955, 1962, and 1969. Editions of *Historical Geology* were published in 1924, 1931, 1933, 1937, 1941, 1949, 1960, and 1969. In most other years there were reprintings without revisions.



TEXTBOOK OF GEOLOGY

PUBLISHED BY

JOHN WILEY & SONS, INC.

PART I

Physical Geology. By Chester R. Longwell, Henry Barnard Davis Professor of Geology, Yale University; Adolph Knopf, Sterling Professor of Physical Geology, Yale University; and Richard F. Flint, Associate Professor of Geology, Yale University. Second (Revised) Edition. Cloth; 6 by 9; 544 pages; 341 figures.

PART II

Historical Geology. By Charles Schuchert, Professor of Paleontology and Historical Geology, Emeritus, and Carl O. Dunbar, Professor of Paleontology and Stratigraphy, and Curator of Invertebrate Paleontology, Yale University. Fourth Edition, Rewritten and Reset. Cloth; 6 by 9; 544 pages; 343 figures in text, 17 plates.

Outlines of Physical Geology. By Chester R. Longwell, Adolph Knopf, and Richard F. Flint. Cloth; 6 by 9; 356 pages; 297 figures.

Outlines of Historical Geology. Third Edition, Rewrit ten. By Charles Schuchert and Carl O. Dunbar. Clot 6 by 9; 241 pages; 151 figures.

Outlines of Geology. Being a combination of "O of Physical Geology" and "Outlines of "Historic ogy," Third Edition, in one volume.

Figure 80. Two choices of geology textbooks available to North American college students. Two sizes, same great quality and same fixed doctrine. Colophon from Longwell et al. (1939).

The authorship of these textbooks is rather complicated. Besides Schuchert (born 1858) and Pirsson (born 1860) nine other Yale authors eventually became involved. These coauthors were all at least a generation younger: Adolph Knopf (1882), Chester R. Longwell (1887), Alan M. Bateman (1889), Carl O. Dunbar (1890), William M. Agar (1894), Richard F. Flint (1901), Karl M. Waage (1915), John E. Sanders (1926) and Brian J. Skinner (1928).

The two original authors had very different writing styles. Pirsson wrote long compound sentences with semicolons to separate the clauses. His sentences are choppy, with abundant commas. Schuchert's sentences are simple, clear and direct. I find his scientific writing a pleasure to read. From attention to such characteristic features, I am certain that Schuchert wrote about half of Pirsson's 1915 *Physical Geology*. Pirsson's writing appears unedited in the chapters on igneous rocks, metamorphic rocks, fractures, faults, earthquakes, groundwater, glacial flow, and ore deposits. But Schuchert obviously penned Pirsson's chapters on organic deposits, sedimentary rocks, the origin of mountain ranges, and other subjects. He had to present many of these same topics again in his own volume.

Curiously, Schuchert's contribution is not mentioned in Pirsson's acknowledgements. Schuchert's motivation for writing large parts of Pirsson's text was to produce the best possible result, and he preferred no acknowledgement of it.

Pirsson had arthritis, and it became severe after publication of *Physical Geology* in 1915. He died of it in December 1919. A few months after his death, a second edition of *Physical Geology* appeared. The only important difference was that a chapter on ore deposits, originally written in Pirsson's characteristic style, was now completely rewritten and signed by Alan M. Bateman. Curiously, this major change was not mentioned in the new preface. I think that Schuchert was in charge of this second edition, and even wrote the new preface, signing it in Pirsson's name. Schuchert also seems to have made some improvements to his own sections, but no changes were made to sections that I attribute to Pirsson.

Schuchert selected six members of the Department of Geology at Yale University to revise the book for a third edition in 1929. This is explained in the preface of that book. Chester Longwell, the new editor, also specifies which authors – Longwell, Knopf, Bateman, Flint, Agar, and Dunbar – wrote which chapters. Pirsson's characteristic writing was now completely gone. Out of loyalty, Pirsson was acknowledged as having determined the successful balance of topics, and his widow, Eliza Brush Pirsson, still held the copyright for the 1929-book.

For *Historical Geology* a new edition did not appear in 1920. The second edition came in 1924, authored by Schuchert, who was now 66 years old. Carl Dunbar, who was 32 years younger than Schuchert, became coauthor of the third edition in 1933.

The various authors of these two books took their share of the writing and responsibility. But both of these books were Schuchert's cherished offspring, and his younger coauthors treated them that way. They tried to politely overlook the books' fixism, which was an unfortunate birth defect from 1915 that they all had to live with. The defect was barely noticeable for the first decade, but became increasingly awkward over the next forty years.

LONGWELL, ET AL.'S *PHYSICAL GEOLOGY* (1939)

Recall that in 1944 Longwell wrote in a journal article that continental drift "merited respectful and sympathetic interest." But he must have felt that only mature geologists should be exposed to this iconoclasm (as he had called it in 1928.). His textbooks for young students mentioned continental drift, but mostly to warn them not to take it seriously.

Longwell alluded to continental drift only one place in each edition of his book. He never presented the best evidence in support of it. His most thorough treatment was in his 1939 textbook. There he went so far as to explain the convection-current mechanism. It came in a section entitled The Ultimate Cause of Crustal Movements. His full presentation is reprinted here:

Longwell et al. 1939, p. 429-431.

Within the last few years some geologists have suggested that whole continents shift horizontally through long distances. It is claimed, for example, that Africa moved northward against the old Mediterranean geosyncline and crushed it to form the Alps and neighboring mountains; that the great chains of Asia were caused by southward shifting of that continent; and that the American cordilleras are the result of slow, long-continued westward drifting of North and South America. It is urged that no other explanation will suffice in view of the stupendous shortening recorded by mountain folds and thrusts. Students who favor the hypothesis of "continental drift" point out considerable geologic evidence which, in their view, strongly supports the concept. For example, if the maps of North and South America, as they appear on a globe, are moved eastward against Europe and Africa, not only do the continental margins match remarkably, but some old mountain belts in America – among them the Appalachians – appear to be continuous with mountain belts of the same geologic dates in lands east of the Atlantic. But what would furnish the motive power for breaking up and transporting continental masses?

One hypothesis that has received considerable attention attributes crustal deformation to slow-moving convection currents within a thick shell of the Earth (Fig. 295) [Fig. 81]. At first thought, it would appear that such currents are impossible, in view of abundant evidence proving rigidity in the Earth. It is urged, however, that a thick zone below the crust may be nearly devoid of strength, because of high temperature (p. 398). Some mathematical physicists agree that slow convection may operate in such a zone, provided an adequate source of heat exists. Advocates of the hypothesis assume that minute quantities of radioactive elements are distributed to depths of several hundred miles in the Earth, and that disintegration of these elements provides sufficient heat to set up convection. A current rising below a large continental mass will be divided and turned laterally at the base of the strong crust; when the currents arrive at a continental margin, heat is lost through the ocean floor, and the cooled subcrustal matter sinks, thus completing the circulation. Frictional drag at the base of the crust may be strong enough to divide the continental mass and separate the parts. The forward edge of each continental fragment encounters resistance to movement and becomes deformed by folding and thrust faulting. This action causes thickening of the granitic shell; additional thickening is caused by frictional drag of the current as it turns obliquely downward, with the result that a large "root" is developed, made of the light granitic material (Fig. 295) [Fig. 81]. By the principle of isostasy, the buoyant effect of such a "root" would support a high mountain range (Fig. 273, p. 401).

This hypothesis appears to offer help in the solution of major geologic problems; but the concept is highly speculative, and is open to serious objections. However it must be admitted that the cause of diastrophism is one of the great mysteries of science and can be discussed only in a speculative way. The lack of definite knowledge on the subject is emphasized by the great diversity and contradictory character of attempted explanations. It is a fascinating problem, but lengthy discussion of its various aspects has no place in this volume. The facts and relationships of mountain structure present a large field of study in themselves, aside from the problem of ultimate forces.

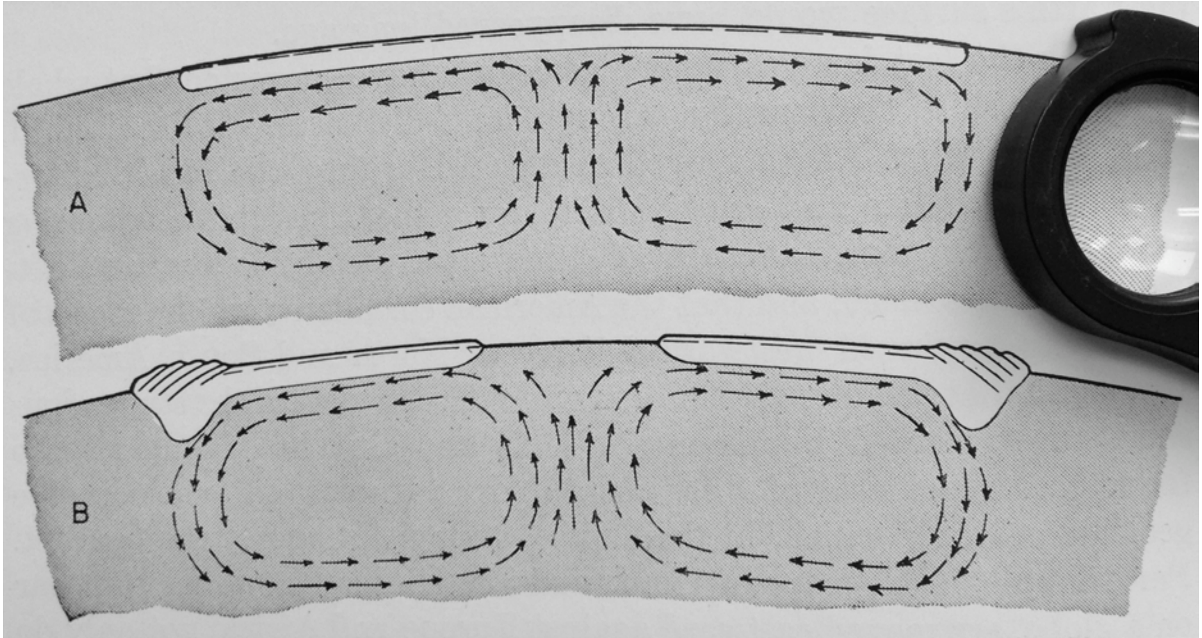


Fig. 295. Vertical section of the Earth to a depth of several hundred miles, to illustrate the hypothesis that slow convection currents cause crustal deformation. In *A*, the white part of the section represents a small continental mass (1000 miles across to the scale of the diagram), with slow convection operating beneath it. *B* represents the same mass pulled apart, and the forward edges of the fragments deformed and thickened to form mountain sytems, cut by large thrust faults and buoyed up by " roots."

Figure 81. Longwell's Fig. 295, showing a hypothesis of convection currents in the mantle. From Longwell et al. (1939).

At the end of the chapter there followed a list of eight annotated Reading References, including these three:

Longwell et al. 1939, p. 432.

6. The Origin of Continents and Oceans; by Alfred Wegener. Translated from the 3rd German edition by J. G. A. Skerl. 212 pages. E. P. Dutton and Co., 1924. This book outlines the theory of continental drift and cites supporting evidence of several kinds. It is an entertaining volume.

7. The Surface History of the Earth; by John Joly. 206 pages. 2nd edition. Oxford University Press, 1930. The author, a special student of radioactivity, suggests that heat from disintegration of radioactive elements has been a major factor in crustal deformation.

8. The Thermal History of the Earth; by Arthur Holmes. Journal Washington Academy of Sciences, Vol 23, pp. 169-195, 1933. A clear and concise presentation of the hypothesis that convection currents in the Earth are a major cause of crustal deformation.

Wegener's book, reference 6, was called "an entertaining volume" clearly implying that continental drift had little scientific value. Wegener's book was out of date and out of print. Du Toit's recent book *Our Wandering Continents* would have been a much more useful reference. Students would not realize that references 7 and 8 include interpretations that might support continental drift.

Longwell's Figure 295 showed how easily convection currents might move continents. But Longwell was not a supporter of such currents, and wrote that they were "open to serious objections." I wonder just what these objections were. In the next edition, he removed this dramatic figure of convection currents. He also removed references 7 and 8 above, but still listed reference 6 as "an entertaining volume." In all of Longwell's textbooks, this bibliographic citation seems to be the only place that he ever printed Wegener's name. He never mentioned continental drift more than one place in any of his books, and it was never associated with enough information to suggest that it had scientific merit.

CARL DUNBAR'S "DEVICE TO HIDE CRITICAL AREAS" (1949)

Schuchert's *Historical Geology* was also being updated regularly. Like Longwell's book, it gave only a single mention, or rather a warning, of continental drift in each edition. After Schuchert's death in 1942, Carl Dunbar came out with a new version in 1949, largely rewritten. He was now the sole author and considered this 1949-book to be the first edition. But the chapter on the Permian was still nearly identical to the one that Schuchert had written. Dunbar did add a comment that the wide Gondwanaland was "now discredited," but the land bridges were still part of the text. Of course, land bridges could not be shown on the map, since the Atlantic Ocean had been split in two, as in the 1941-edition (see Fig. 61).

A prominent feature of *Historical Geology* had always been Schuchert's paleogeographic maps of North America for the various geologic periods. Each chapter of the book covered a geological period and included a set of three maps, representing the paleogeography of the early, middle and late stages of that period. The maps showed the positions of deep oceans (lined), shallow seas (dark grey) and dry land (light grey), based on the rocks and fossils that are preserved from those times.

In the 1949-edition, Dunbar again included Schuchert's maps. But in that edition Dunbar had engaged an artist to paint clouds over parts of the maps (Fig. 82). The coasts were clouded in 28 different maps: all three maps of the Cambrian, Ordovician, Silurian, Devonian, Mississippian, Pennsylvanian, Permian (four maps), Triassic, and Jurassic periods.

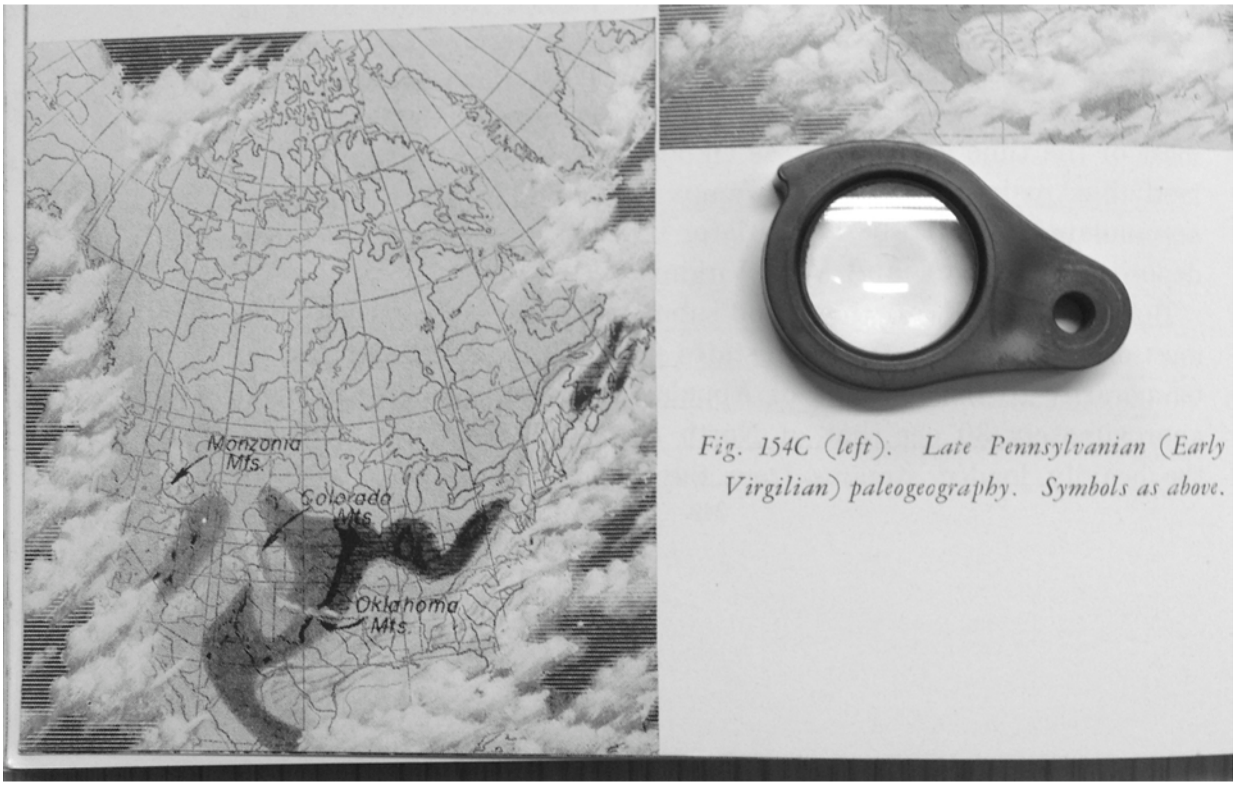


Figure 82. Clouding the issue of the Atlantic Ocean. Paleogeographic map of the Late Pennsylvanian (Carboniferous). From Dunbar (1949).

Dunbar explained the purpose of these clouds in his Preface:

Dunbar 1949, p. viii.

These panels are newly prepared from Professor Schuchert's latest unpublished maps. The clouds have no meteorologic significance; they are merely a device to hide critical areas for which evidence is lacking or inconclusive. Just criticism has sometimes been made of paleogeographic maps which show no distinction between well-documented portions and those that are based entirely on inference. This is an attempt to avoid that shortcoming. These maps are, of necessity, highly generalized, and in most instances they indicate the maximum extent of the seas within a given epoch rather than the exact outline at any particular moment of time.

It is a common practice for geologists to cover a corner of a geologic map that is unneeded or unwanted. Often a legend or inset map is placed in a corner. But it was Dunbar's unique solution to use painted clouds to cover parts of the map that he knew Schuchert was not happy with.

Just what critical areas bothered Schuchert and Dunbar? The center of the continent was always visible, although evidence there too is sometimes lacking or inconclusive. Two critical areas were consistently covered. One was Schuchert's land bridge between Newfoundland and the southern tip of Greenland. The other area was the edge of the Atlantic Ocean. A continental highland must have existed east of the present coast, as Appalachian geologists knew, because it was shedding sediment westward into the shallow marine basin that would later become the Appalachians. Schuchert and others had called this landmass Appalachia, and drew it as small as possible. But in continental displacement theory it was big; the African part of Pangæa. Dunbar wanted to avoid this topic, and these eastern areas of the map were surely the important ones. The other clouds created balance and helped draw attention away from the Atlantic coast.

The clouds were finally gone from the Atlantic coast on all three maps of Late Cretaceous time (Fig. 83). By then, Pangæa had broken up and the Atlantic Ocean had opened. For the three maps of Cenozoic time, no clouds were shown anywhere on the maps (Fig. 84).

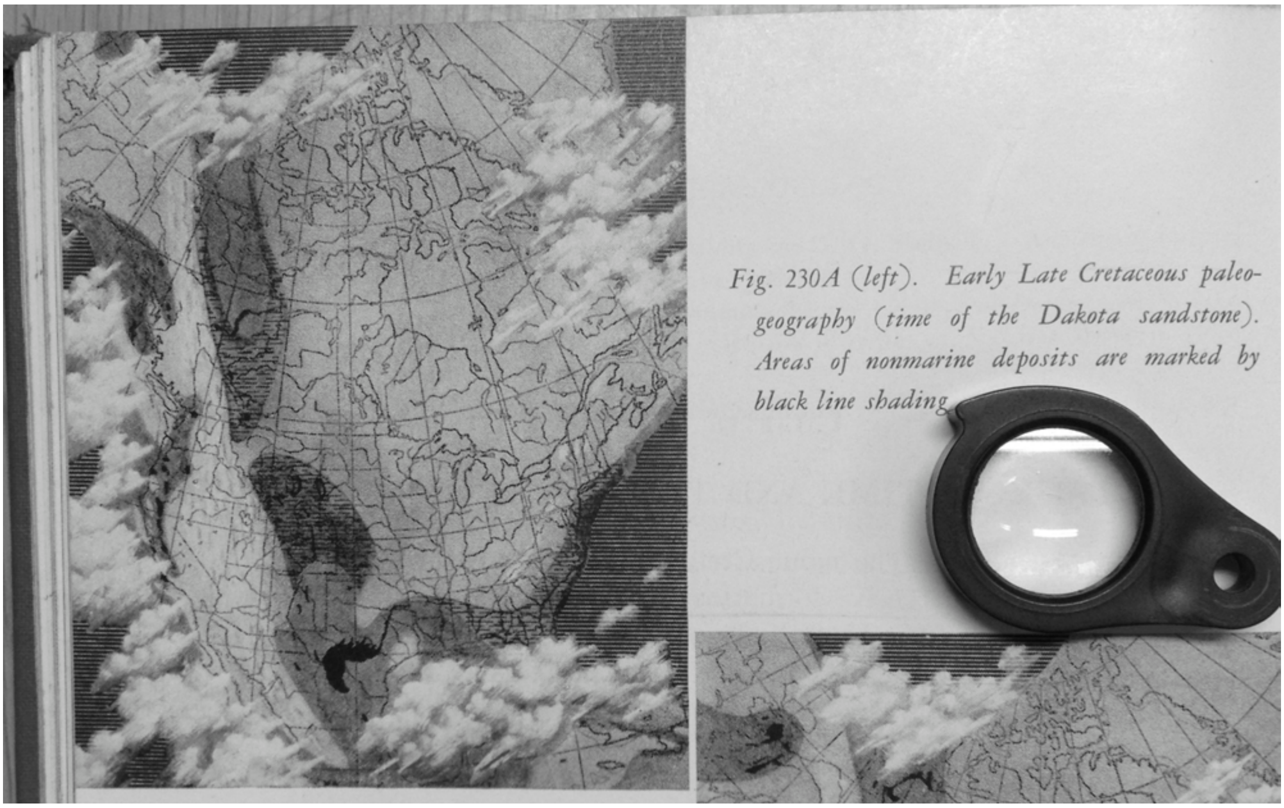


Figure 83. The coast is clear. Paleogeographic map of the Early Late Cretaceous. From Dunbar (1949).

Figure 84. All clear. Paleogeographic map of the Oligocene. From Dunbar (1949).

When Dunbar wrote that the clouds were “a device to hide critical areas” we can understand this as meaning a device to hide the ocean, which did not yet exist east of North America. Dunbar and Schuchert continually tried to hide evidence for mobilism in their writing and their illustrations.

Dunbar must have felt very bad about tampering with Schuchert’s maps. Perhaps to make amends, he had 84 of Schuchert’s maps published posthumously as an atlas (Fig. 85). Schuchert had never published the maps himself; he would not correct the Atlantic coast, or cover it with clouds.

We can sympathize with Schuchert. He loved his paleogeographic maps, and he would have liked to publish them. Dunbar put it this way, in his introduction to Schuchert’s atlas:

Dunbar, in Schuchert 1955, p. vii

Being a bachelor, Schuchert made of paleogeography his mistress and constant companion. As the endless stream of stratigraphic papers crossed his desk, some hours of each day and most of his evenings were spent abstracting and plotting data. A special drafting table was built beside his desk with space for more than 100 maps so that new information could be added readily as it came to light, and, in order to facilitate change, the data were plotted in pencil. Most of the maps are now dog-eared and worn, and not a few have been patched or renewed, for they are the result of a growth over a long period of years and represent literally the work of a lifetime.

Schuchert never stopped studying the new developments relevant to paleogeography. He must have been aware of the “boomerang,” as Grabau had called it, that he had flung in 1928, and that came back at him as more information became available. Schuchert surely saw vast amounts of data that would strengthen the geologic and paleontologic fit of continents across the Atlantic.

Schuchert must have hated how his maps had been spoiled by Wegener's hypothesis, since about 1933. That was when he felt forced to reprint a map from 1923 instead of an updated version. Too bad that he could not have made North America part of Pangæa and become proud of his map compilations once again. But no, he would stick with his paleogeography – until death do them part. He had his reputation, and his paleogeographic position was fixed.

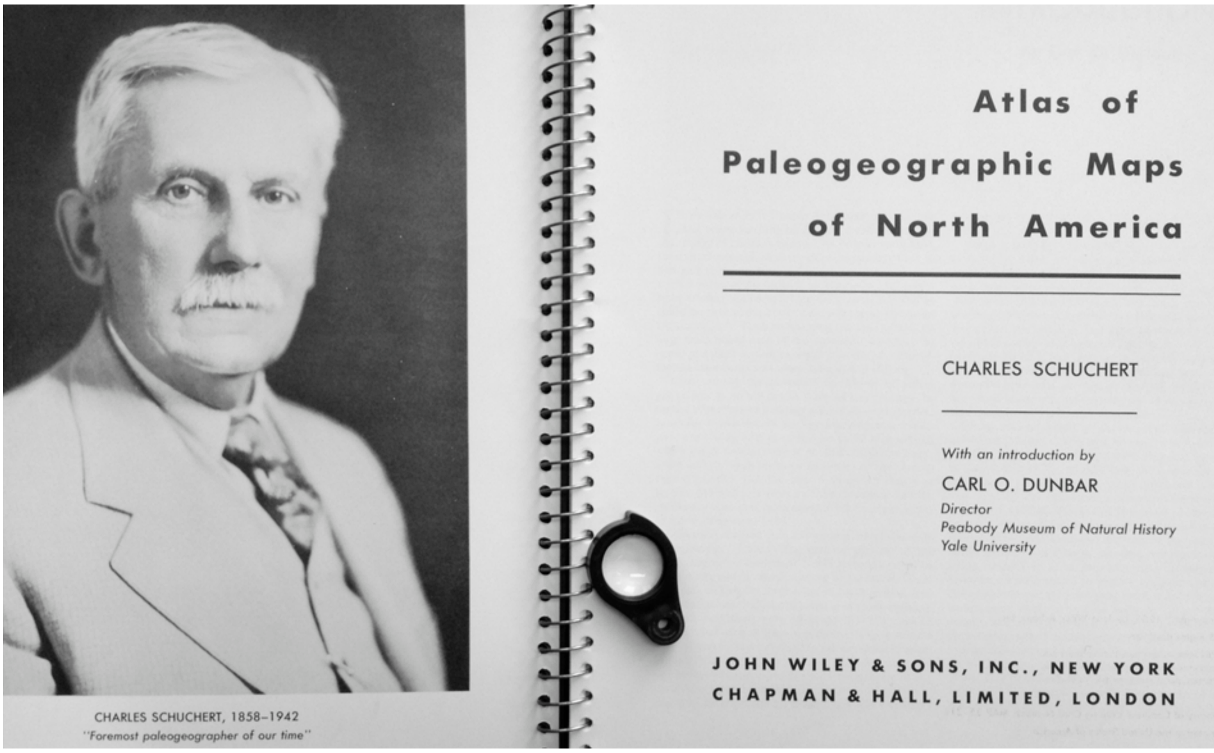


Figure 85. Frontispiece portrait and title page. From Schuchert (1955).

Dunbar produced the next edition of *Historical Geology* in 1960. Schuchert's paleogeographic maps were again included, but now all the clouds were gone. Dunbar had learned that it was better not to admit to any critical areas at all.

Dunbar called this the second edition of the book, but it was the sixth edition of the chapter on the Permian, and other paragraphs that were directly related to the presentation of fixism. They were left essentially intact. As before, continental drift was only mentioned once. It was still called a belief, not a hypothesis, and followed by the line that: "this premise itself is still in the realm of speculation!" The support of fixism in *Historical Geology* had been written by Schuchert, and these parts would always be his.

REVISED EDITIONS OF LONGWELL'S *PHYSICAL GEOLOGY* (1955, 1962, 1969)

After Schuchert's death in 1942, Longwell lived to update *Physical Geology* for several more editions. In the 1955 edition, he rewrote the paragraphs that include the discussion of continental drift:

Longwell and Flint 1955, p. 354.

Theory of Convection Currents. In a vessel of boiling water the hot liquid rises and cooler liquid sinks, forming currents that circulate up and down. The Earth outside the central core is not liquid, but its strength may be very low and temperatures near the core probably aremuch higher than at the base of the crust. It is argued that convection may go on by solid flow, at the rate of an inch or less in a year, with millions of years required for a round trip between core and crust. The theory pictures great convection cells, each thousands of miles across. Two adjacent cells that turn away from each other at the base of the continental crust may pull the crust apart and carry the fragments long distances. Where two cells meet and turn downward, they press parts of the crust together and also exert a downward pull. The compression may buckle parts of the crust upward, but directly over the descending currents the crust should be bent down to form a trough; thus the stage is set for a geosyncline, to be filled with sediments from bordering lands, perhaps islands, that keep rising under continued pressure. The entire geosyncline finally is crushed to make mountains.

Some have used the convection mechanism in a daring hypothesis of "continental drift," in which the Atlantic Ocean basin is represented as a great rift, opened and continually widened by drifting apart of the continental masses east and west of it. This concept had its origin in the remarkable matching of the opposing shorelines as seen on a map; the ocean basin would be almost filled if the Americas were pushed eastward, Africa and Europe westward, to meet along the Mid-Atlantic Ridge. The Appalachian belt, which breaks off abruptly along the east coast of Newfoundland, would then be almost continuous with mountains of nearly the same date in western Europe. Other geologic features are urged as evidence for the separation. As the Americas were carried westward, resistance to the movement crumpled the western edges of the continental plates to form the Andes and the chains of the North American Cordillera.

With continued study the hypothesis of continental drift raises fully as many questions as it appears to answer. The hypothesis of convection currents also faces serious physical difficulties, even if it is considered without any relation to the concept of continental drift.

It is commendable that Longwell mentioned the Mid-Atlantic Ridge in the context of continental drift. But he did not point out the belt of earthquakes and volcanoes along the ridge. And he never elaborated on the "many questions" raised by continental drift or the "serious physical difficulties" faced by convection-current theory.

To get more insight into Longwell's thinking in the 1950s, it is helpful to look at a scientific article that he published in Australia. Many geologists of the southern hemisphere had favored mobilism since the days of Wegener. In 1956, Australian geologist S. Warren Carey convened a symposium in Tasmania on continental drift. Since Longwell was an influential North American fixist, he was flown to the meeting as the Principal Guest. For this he was asked to write the introductory paper for the symposium volume, and then write an Epiloge summarizing the other eleven papers with his evaluation of the meeting.

Maybe Carey and the others wanted to see if they could convert Longwell to mobilism. Among the profusion of drift evidence, Carey presented a highly accurate map showing that South America and Africa fit almost perfectly along the 2000-meter isobath (Fig. 86). His stated purpose of this map was to refute a claim by Jeffreys that the angle of the corner is “really a misfit by almost 15 degrees.”

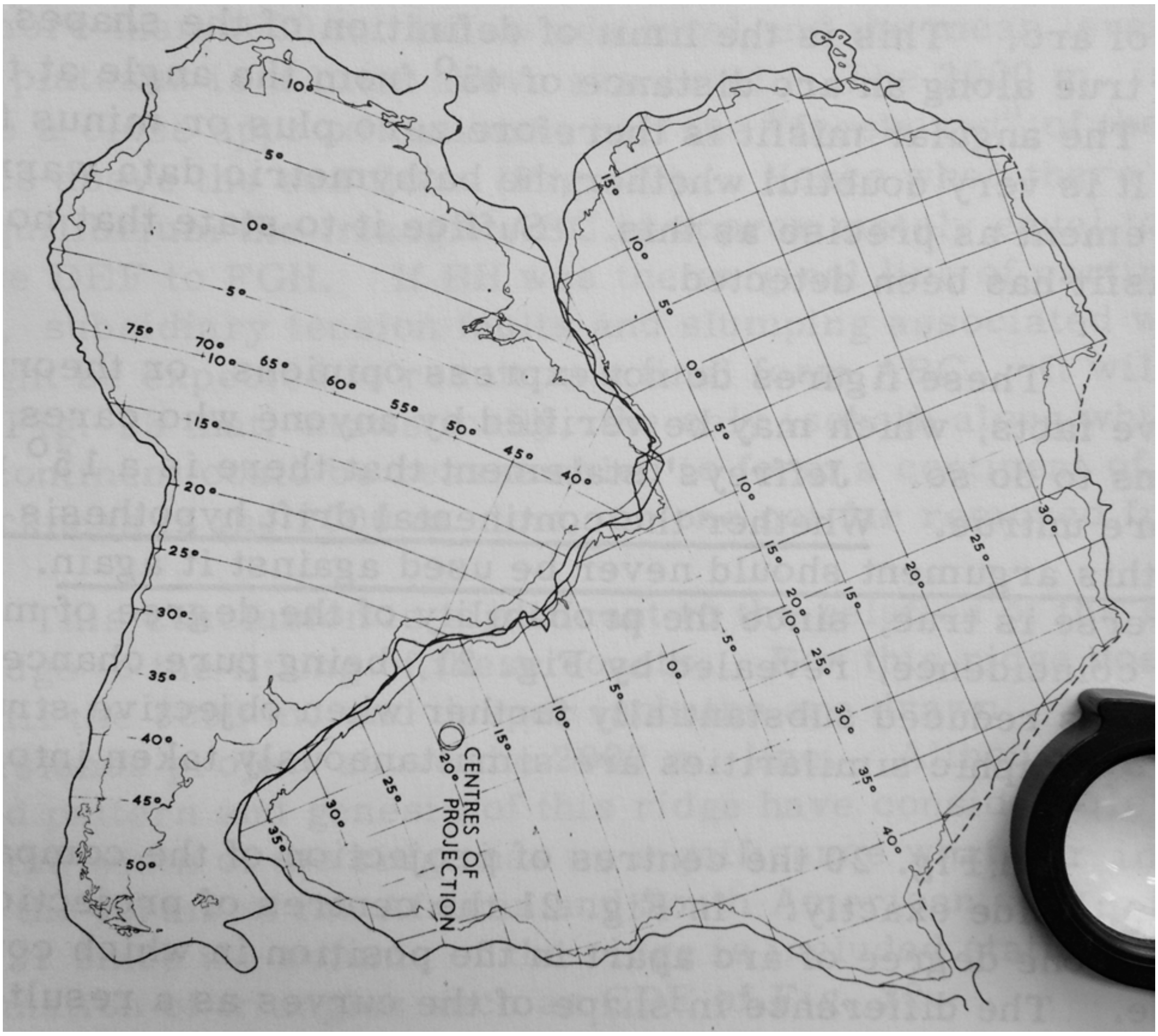


Figure 86. The Carey-refute, contesting a claim by Jeffreys that South America and Africa do not really fit. This map shows they fit quite well, and best at the 2000-meter isobath. From Carey 1958.

But no evidence would convince Longwell about continental drift. Here is the conclusion to his 12-page introductory paper. He refers to Satan, just as Schuchert had done in 1928:

Longwell 1958a, p. 10.

These critical comments may suggest that I am an incurable sceptic toward continental drift. I admit to scepticism which, it seems to me, is the soul of scientific inquiry; but I have no unfriendly feeling for the concept of drifting continents – on the contrary I find it attractive and in several respects credible. The Atlantic basin looks like a gigantic rift; if the fit between South America and Africa is not genetic, surely it is a device of Satan for our frustration; the east and west coasts of Africa are strangely bare – we may say raw – in comparison with continental margins generally; pairing of some structural trends on opposite sides of the ocean is at least suggestive of former continuity. In spite of this appeal, I remain incredulous on two main grounds: (1) Evidence known to me is suggestive and circumstantial only – no clinching argument has appeared. (2) Quite aside from the problem of a propelling force, the supposed horizontal creep of sialic blocks through sima seems to be highly improbable. Rigidity in the crust presumably decays downward and at some depth disappears. In that deep zone the principles of rheology must apply, as indeed they must in more limited degree at higher levels. But modes of tectonic failure in the outer, visible zone suggest strongly that some long-term rigidity is there a reality. No basic principle known to me seems compatible with the assumed horizontal movement of a sialic mass, as a unit, through sima. But developments may change this point of view. Not long ago I could not see a logical mechanism for polar wandering, yet this concept is now given a respectful hearing.

The explorations in paläomagnetism, now only well started, may open new vistas in Earth history. Workers in this new field are finding serious difficulties, and some students express doubts that results can be dependable. Others hail the magnetic records as the long-sought Rosetta stone, a key to major geological puzzles. Gold (1955) is confident that eventually the magnetic data will tell us whether polar wandering, or continental drift, or both together made the tangled skein of climatic records that seem insoluble on the basis of present world geography. If the sum of evidence finally convinces us that the continents have drifted, no doubt we can then come to agreement of geophysical principles that made this possible.

Longwell wrote that skepticism is the soul of scientific inquiry. That was his personal opinion. I feel that hypotheses are more important than skepticism. In any case, Longwell was more than skeptical; he was disingenuous. In his second main argument above (2), he ignored the convection-current mechanism for moving continents. He pretended that the only way continents could move was by sialic blocks creeping through sima, not creeping together with sima. Holmes had shown continents to move by mantle convection, with sialic blocks carried on sima, not creeping through it.

Now we continue to follow Longwell's textbook revisions. The 1962 edition of *Physical Geology* repeated the same three paragraphs from the 1955 edition (shown above.) But he added one extra sentence to the end, so that the last short paragraph now read as the following three sentences:

Longwell and Flint 1962, p. 438.

With continued study the hypothesis of continental drift raises fully as many questions as it appears to answer. The hypothesis of convection currents also faces serious physical difficulties, even if it is considered without any relation to the concept of continental drift. These difficulties do not at present condemn either concept, but both concepts are in the category of *hypotheses on trial.*

The idea of a *hypothesis on trial* (Longwell's italics) accurately describes his treatment of continental drift. In general, hypotheses are put to work, to see what useful predictions and contributions they can make. For this reason they are called “working" hypotheses. But continental drift and convection currents could not be working hypotheses for Longwell. For him they were suspect or accused hypotheses, and should be kept from working. He seemed to hope that they would be condemned.

Longwell still had no explanation for the horizontal compressive forces that form mountain ranges. In this book he offered only the ideas of contraction theory and convection-current theory. But the contraction theory was irrelevant, because it had been discredited fifty years earlier by the discovery of radioactive heating and the non-uniform locations of mountain ranges. He wrote that the convection-current theory had serious physical difficulties, but he never specified them. He also described the expanding-Earth theory, but this was a diversion, because expansion could never explain compressional mountains.

Having mentioned these three topics, to make it seem that there were several hypotheses for the origin of mountains, he summarized the situation as follows:

Longwell and Flint 1962, p. 439.
The puzzle of orogeny.
The wide diversity of ideas on the origin of mountains shows clearly that we have no trustworthy answers to the basic questions. A survey of several intelligent guesses reminds us of the varied opinions of the blind men about the overall appearance of an elephant. The problem of mountain making is too big and too much of the essential information is still hidden for any confident solution now. The challenge of the problem will stimulate more intensive efforts, and the architectural features of mountain belts will always be worthy of study for their own sake.

But Longwell had seen the elephant. Arthur Holmes was showing it to British students in his textbook. Longwell was keeping North American students from seeing it.

An important part of the elephant could be seen in the map of the world’s earthquake belts, that Longwell’s textbooks showed in 1934, 1939, 1941, and 1948 (see Figure 62). That map was in the chapter written by Adolph Knopf. Longwell took over Knopf’s chapters for the 1955 edition and moved this map from Knopf’s volcano-chapter to the earthquake-chapter. Longwell also improved the map, by having it redrafted (Fig. 87), with some labels, including the words MID-ATLANTIC RIDGE between Africa and South America. Longwell mentioned the Mid-Atlantic Ridge also in the discussion of earthquakes.

After Longwell had been to the Australian symposium on continental drift in 1956, he must have realized that this map showed too much of the elephant. At that symposium S. Warren Carey (1958) and Lester King (1958) had argued strongly that the Atlantic Ocean was currently opening along the Mid-Atlantic Ridge. Carey showed a map by J. P. Rothé (Fig. 88), where the positions of recent earthquakes were accurately plotted. The precise positions clearly delineated the mid-ocean ridges. These lines of the elephant were now unmistakable. Longwell read all the papers, and wrote a 3-page Epilogue to conclude the symposium volume. His only comment on the Mid-Atlantic Ridge was: "The Mid-Atlantic Ridge is indeed a strange accident if it has no genetic relation to the margins of the Atlantic basin. Arguments based solely on such features can only be speculative... (Longewell, 1958b, p. 356)"

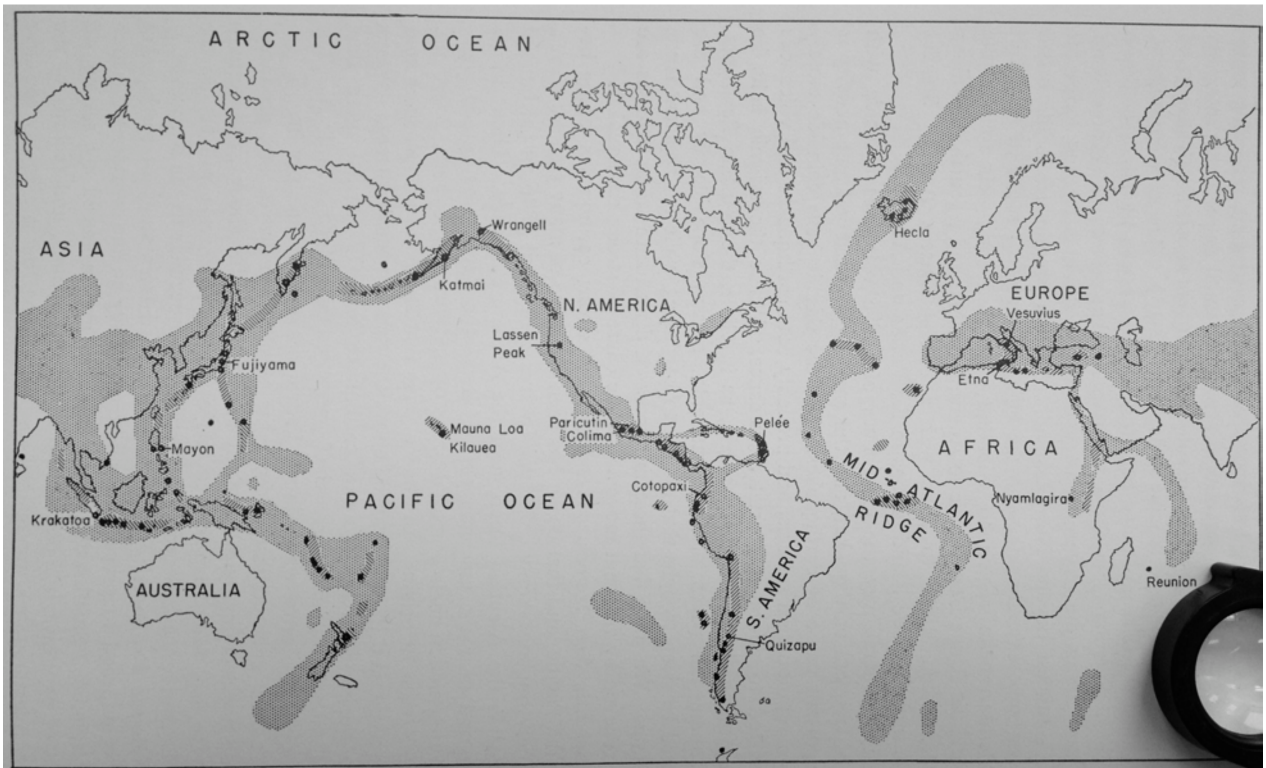


Fig. 19-13. Distribution of earthquake belts (shaded) and large volcanic centers (black). Some prominent volcanoes are named. Diagonal lining indicates areas of exceptionally strong seismic activity. (Modified from A. Knopf.)

Figure 87. The Mid-Atlantic Ridge is clearly shown, perhaps too clearly shown. From Longwell et al. 1955.

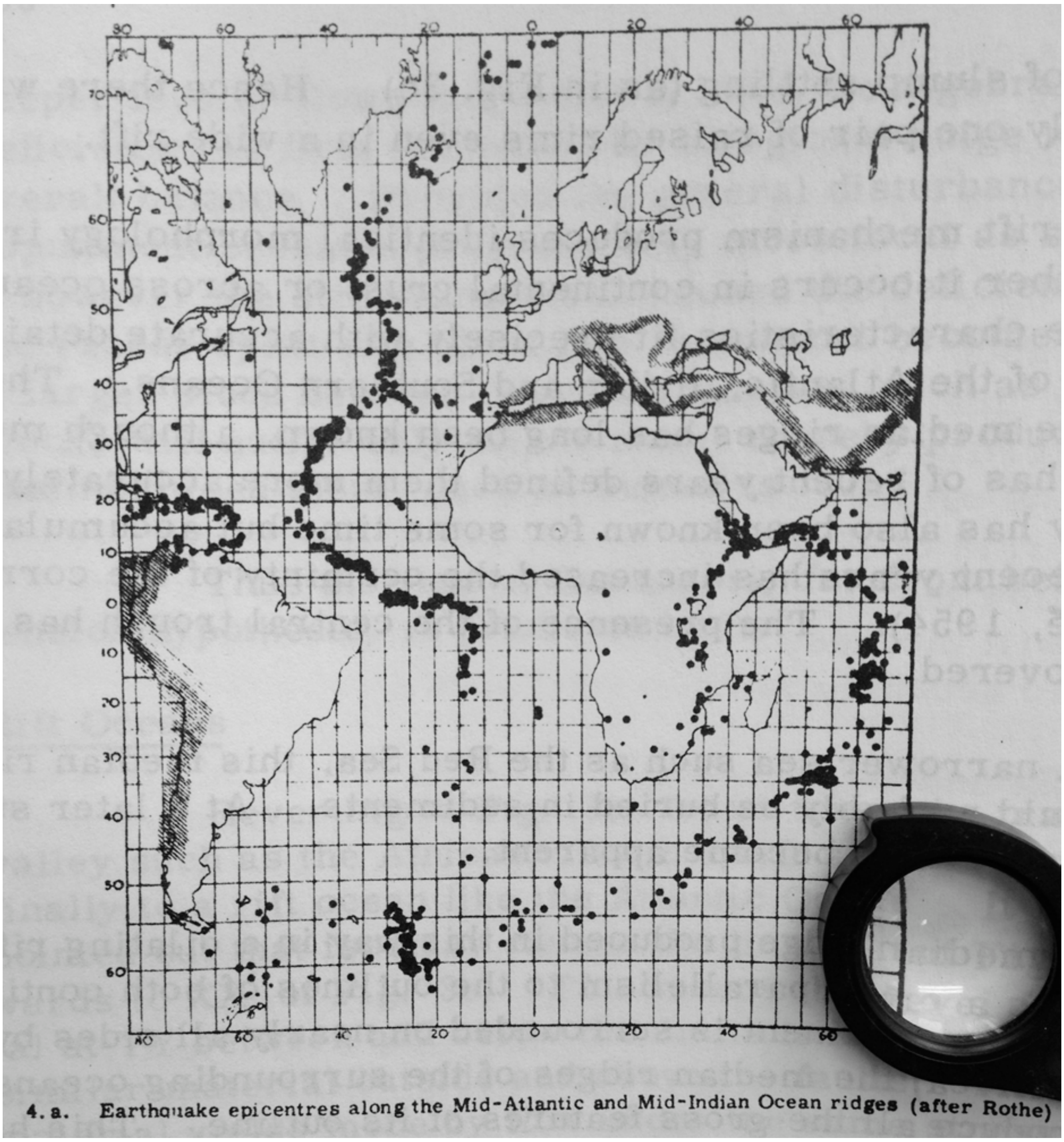


Figure 88. Modern earthquake data from Rothé, presented at the symposium on continental drift and published by Carey (1958.) The accurately located earthquakes clearly show the mid-ocean ridges in the Atlantic and Indian Oceans. From Carey (1958).

For the 1962 edition of his book, Longwell removed his nicely redrafted earthquake map. He replaced it with a crude map of earthquakes from before 1931 (Fig. 89). These older earthquake epicenters, measured by earlier technology, were very poorly located. The earthquake belts and Mid-Atlantic Ridge were much harder to actually see. He had hidden the elephant without painting clouds on the map.

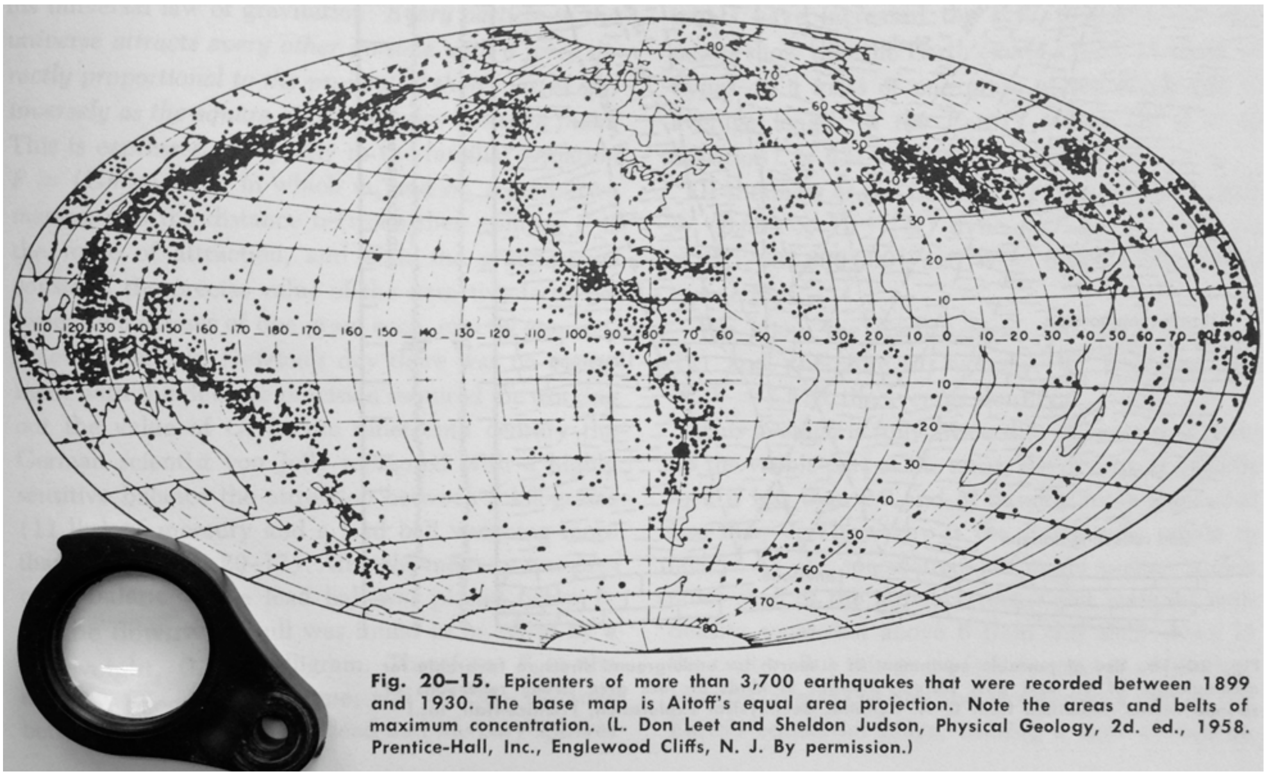


Figure 89. Earthquake map from the 1962 edition of Longwell's book, which replaced the map of his 1955 edition. These earthquakes were all recorded before 1931, and their locations are only approximate. The Mid-Atlantic Ridge is barely discernable. From Longwell et al. 1962.

In 1969, after the plate-tectonic revolution was well underway, another edition of Longwell's *Physical Geology* appeared. Longwell was now 82 years old, but he was still doing a little geological fieldwork, as well as writing scientific papers and revising his textbook.

In 1969, no reputable geologists were denying mobilism. The Preface of this book sounded promising; it began as follows:

Longwell, Flint and Sanders 1969, preface.

This volume is not just a revision of the Longwell-Flint *Introduction to Physical Geology* (1962); most of it is quite new. Certain rewritten chapters follow the pattern of those in the earlier book, but all the rest have been written *de novo.*

But the book was a disappointment for the instructors and students that had to use it. It was a retelling of Longwell's geological story, not integrated with ideas of mobilism. Nothing of the new plate-tectonic geology was mentioned until Chapter 22, the next-to-last chapter. Information there did not tie in with the other parts of the book. Most of that chapter was surely written by the new coauthor, John E. Sanders (1926-1999). Sanders had a Yale Ph.D. and had taught there from 1954 to 1964. Longwell, on the other hand, had retired from Yale and moved to California in 1955.

One can tell that Longwell wrote the two paragraphs about the history of continental drift in the new Chapter 22. They are in his writing style, and they contain statements characteristic of him:

Longwell, Flint, and Sanders 1969, p. 553.

Continental drift. The concept that continents can shift laterally followed close upon the proof that under the weight of continental glaciers the surface of the lithosphere had moved up and down by flowing. After all, ran the argument, if the lithosphere can flow up and down, why can it not also flow laterally? Early arguments in favor of drifting continents derived mainly from the similarity of the shape of the coasts on the opposite sides of the Atlantic Ocean (Fig. 22-7) [Fig. 90]. Further evidence came from parallels in the geology among rocks of Late Paleozoic age in India, Australia, South Africa, and South America, and also among rocks of both Early and Late Paleozoic age in the northern Appalachians and in northwestern Europe. Advocates of drift supposed that sialic continents formed a single mass until late in the Paleozoic but thereafter began to move apart like "rafts" floating in a "sea" of denser mafic rock. Moreover, these sialic rafts were thought to have skimmed off the deep-sea sediments, heaping them up into mountain chains on their forward sides. By this means drifting continents were thought to supply the motive power for folding. Forces of the Earth's rotation were cited as the driving mechanism of the drifting continents. According to enthusiasts some drifting is still in progress.

Formidable objections were raised against the specific contentions of nearly every early scheme of continental drift. But as we shall see, the hypothesis of continental drift has assumed new stature as a result of paleomagnetic studies. During the first rounds, although partisans favoring drift may have been right, they based most of their case on the wrong reasons and were unable to visualize a mechanism consistent with other evidence.

Fig. 22-7. One reconstruction of the hypothetical landmass believed by some to have existed before the continents drifted apart. The southern part of this landmass is called Gondwana-land. (After J. T. Wilson, "Continental drift." Copyright © April 1963 by Scientific American, Inc. All rights reserved.)

Figure 90. The Fig. 22-7 referred to by Longwell. Note that India is not attached to Asia as Wegener had interpreted. This was not one of the early maps, but a modern plate-tectonic version. From Longwell, Flint and Sanders (1969).

Ever since 1928, Longwell had been convinced that "early arguments in favor of drifting continents derived mainly from the similarity of the shape of the coasts." Longwell was referring to Wegener here, but students new to geology could not learn about Wegener's work from this textbook. No details about that early debate were mentioned. His Figure 22-7 (Fig. 90), showing the "early arguments" in favor of drifting continents, was a map of Pangæa dated 1963. Students were not told that Pangæa had been discussed and rejected prior to the 1960s, or that Longwell had been denying it for the past 41 years.

Finally, the last sentence of Longwell's second paragraph totally misrepresents the historical situation. The reasons, discussed in great detail by Wegener and Du Toit, were not wrong. The mechanism, was ably visualized by Holmes and Du Toit, and was perfectly consistent with other evidence. But these misleading statements by Longwell have been repeated, and now, 45 years later, misunderstandings still linger in the minds of most geologists.

In 1976, the year after Longwell's death, John E. Sanders produced his own textbook *Physical Geology*, with two new coauthors and a different publisher. This time Sanders could incorporate the role of continental drift and plate tectonics throughout the text. Richard F. Flint and Brian J. Skinner continued the Yale textbook tradition at Wiley, with new versions of *Physical Geology* in 1974 and 1977.

Through Longwell's entire career he stayed on the fence, as he once called it, between mobilism and fixism. I have not read Longwell's correspondence, except the parts that were printed in Oreskes (1999). But those excerpts suggest that it was Charles Schuchert (age 67) who placed Longwell (age 38) on the fence, in January 1926. At that time, Longwell was very enthusiastic about mobilism. He had just read a book on crustal sliding by John Joly and a paper on mantle radioactivity by Arthur Holmes. From these works Longwell saw how mobilism could solve the problems of horizontal movements within fold mountain belts. He wrote a review of Joly's book and Holmes' paper, to be published in the *American Journal of Science*, and sent a note to Schuchert, saying: "Joly and Holmes together have a beautiful theory, and I believe it will be epoch-making." (Oreskes 1999, p.120)

I am not aware of Schuchert's response, but the book review that appeared in Schuchert's journal was only a few paragraphs long, and it was anything but enthusiastic. One paragraph begins: "Professor Joly has made an important contribution in the field of speculative geology." The next paragraph begins: "The merits and weaknesses of Joly's argument are discussed in recent articles by Dr. Arthur Holmes. Holmes shows the improbability that the continents have ever floated freely in basaltic magma." No reader would think there was anything beautiful or epoch-making here. In their 1928 articles against continental drift, neither Longwell nor Schuchert mentioned Holmes, although they knew by that time that Holmes had discovered the convection-current mechanism for continental drift.

THE LAST EDITION OF *HISTORICAL GEOLOGY* (1969)

A new edition of *Historical Geology* was also published in 1969. For this revision, Dunbar had invited Yale colleague Karl Waage (1915-1999) to be his new coauthor.

They accepted and promoted mobilism, at least in Chapter 4, entitled *The Restless Crust,* and in Chapter 13 where Permian glaciation and climate were discussed. They mentioned that Wegener and Du Toit had been correct, but the references put the dates of their published works at 1966 and 1954. Wegener's theory was said to be from 1912, but it is not clear that it was published then; the reference [29] is 1966. And instead of referring to the obvious book by Du Toit with the revealing title *Our Wandering Continents* and its date 1937, they cited a 1954-edition of his *Geology of South Africa*.

Despite this clouding of the early history, Dunbar had now capitulated:

Dunbar and Waage 1969, p. 82-83.

In 1912 Alfred Wegener [29] advanced the new theory of continental drift. He proposed that the southern continents and India are fragments of a great southern landmass centered about the South Pole in Paleozoic time – a land-mass which he called Pangea (Fig. 13-25). Thus he accounted for the glaciation and the associated Glossopteris flora. Then, he reasoned, this great landmass broke up into the present continental units which slowly drifted apart to their present positions. As supporting evidence, he pointed to the remarkable parallelism of the opposite margins of the Atlantic Ocean.

Wegener's concept immediately challenged the interest of both geologists and biologists, for if the southern continents had once been united, the fossil record should reflect the fact. But to physical geologists there was one fundamental obstacle. The force required to push a continent thousands of miles would be colossal. No such force could then be imagined and it was argued that if a continent had been so moved it would act like a great bulldozer pushing mountains of rock ahead of it and leaving a wide belt of tension in its wake. Thus continental drift became the subject of one of the great controversies in geology with an enormous and contradictory literature, much of which now seems irrelevant. If convection currents in the mantle are as real as now believed, they supply the force and the continents merely float and drift like great tabular icebergs in the sea, carried along without friction on the flowing mantle. The fossil record can tell us much about the time of the breakup of Pangea and it proves conclusively that the continents have been separated at least since Cretaceous time.

Dunbar and Waage wrote: "no such force could then be imagined." This statement is incorrect.

Dunbar must have been uncomfortable with the "contradictory literature" of the continental-drift debate ever since he began working on Schuchert's book. Now Dunbar and Waage were hoping that readers would not go back and check those earlier texts. They wrote that much of that literature "now seems irrelevant." They must have felt, as many teachers do, that new students should not be alerted to the fact that scientists have typically human biases and make typically human mistakes.

In the chapter on the Permian, two important paragraphs each begin by giving credit to the early mobilists:

Dunbar and Waage 1969, p. 304.

Although Wegener's theory seemed to many geologists at the time to be preposterous, it has recently gained wide acceptance and the paleomagnetic evidence appears now to offer a basis for reconciling the contrasts between the Permian climate of North America and Europe and that of the glaciation in the southern continents.

The well known reconstruction by DuToit [1] (Fig. 13-25) was recently confirmed, in part, on the basis of comparison of the bedrock masses, the structural trends, and the glacial deposits (Fig. 13-26). Admittedly more evidence from many sources is needed to establish the ideas suggested above or to disprove them, but we feel confident that it will be available in the next few decades.

Dunbar and Waage implied here that they were being rather daring, already accepting mobilism in 1969, a “few decades” before other geologists might do so.

Dunbar and Waage did not produce another edition of their book to update the other chapters with regard to the new paleogeographic positions of continents. Even so, this textbook continued to dominate the college market for several more years. It was still being printed as late as 1976.

"ART, EMPIRE, EARTH ITSELF, TO CHANGE ARE DOOM’D”

Although I have criticized this long-lived series of geology textbooks (Fig. 91), they were excellent in most ways, deserving the high praise they received. Their only major flaw was the doctrine of fixism. But that flaw was indeed major. It was also a conscious decision by the authors to let that flaw go uncorrected for over 40 years.

Geologic understanding is often a matter of connecting the dots to draw the complete picture. This applies to microscopic geological features as well as global ones. If you already have a picture in mind, you usually try do draw that picture until you are forced by many additional dots to see something else. The authors of *Physical Geology* and *Historical Geology* displayed the picture of fixism and showed dots that seemed compatible with it. Over the years they came to realize that the dots actually would show mobilism, but they did not want to see it. As they revised their textbooks, they hid certain dots to make it difficult to connect them in that way.

Charles Schuchert guided the development of *Physical Geology* and *Historical Geology*. These books were his project, from before Longwell and Dunbar were involved. Schuchert would not give continental drift a chance while he lived, and Longwell and Dunbar would not do so after he was gone. For Dunbar, Schuchert's paragraphs on the Permian ice age were gospel. They were barely touched from the edition in 1933 to the one in 1960. One does not make changes to a sacred book, much as one would like to.

There were other geology textbooks in North America, but *Physical Geology* and *Historical Geology* were the obvious choices from 1915 to 1969. Through these books, geology students were grounded in fixism. They were warned about continental drift, and taught to shun it. Just as James Dwight Dana had been indoctrinated in creationism in his childhood years, North American geology students were indoctrinated in fixism in their first college courses. Early-formed beliefs are hard to shake later in life, and few North American geologists would ever consider continental drift to be a working hypothesis.

Figure 91. Schuchert's family of geology textbooks. Revised editions of *Physical Geology* (left) and *Historical Geology* (right).

10. From Continental Drift to Plate Tectonics

S. WARREN CAREY WITH A WAKE-UP CALL TO NORTH AMERICAN GEOLOGISTS (1959/1960)

After Chester Longwell retired and moved to California in 1955, the professors at Yale University were less negative toward continental drift. Yale then made a curious contribution to the transition from continental drift to plate tectonics. In his autobiography, John Rodgers related how this came about:

Rodgers 2001, p. 194-195.

Sam Carey warrants a digression. He was a very innovative geologist, a maverick indeed and consciously so, with some very good ideas and others that most of the rest of us considered crazy. When I took my first sabbatical year (1959-1960) to go to Europe, I got an NSF Senior Post-Doctoral Fellowship to pay my way, and Yale therefore had my salary to appoint a Visiting Professor. After careful consideration and consultation, and largely on the basis of a very innovative early article of his, we invited Professor Carey. Moreover, he made trips all over the United States and Canada, lecturing everywhere in his inimitable now-you-see-it-now-you-don't style (he had put himself through college by working as a magician) and North American geology has never been the same since. For he ended, once and for all, the condescending dismissal of the idea of Continental Drift that had characterized North American geologists. He also helped to lay the foundation for the theory of plate tectonics, "the new global tectonics" as it was called at the time, which revolutionized tectonic thinking during the 1960s. About 1955 Carey apparently anticipated the new global tectonics in a manuscript that was rejected for publication as too speculative; I haven't seen it, but I'm told that it included both the creation of new crust at the mid-ocean ridges, forming the oceans, and its destruction or at least swallowing at belts of compression, where new mountain ranges are built. But curiously, in later years he reneged on the latter and recognized only the creation of new crust, and he became a strong advocate of the theory of an expanding Earth, going so far as to claim that its radius has about doubled in the last five hundred million years, implying of course that its volume has nearly octupled and therefore that its density then was nearly eight times what it is at present. It is this aspect of the theory that makes most of us unwilling to accept it, preferring to balance the creation of new crust, on which we agree with Carey, by the destruction of old crust, on which we disagree. In any case, the best thing that I ever did for North American geology was to go away for a year, so that Carey could spend the year here.

Rodgers noted that he never saw Carey's manuscript that went unpublished in about 1955. It was unfortunate that Carey had not submitted it to the *American Journal of Science*, where Rodgers had been a main editor since 1950. Rodgers often helped geologists get their controversial ideas published, even when he did not fully endorse them.

Although I do not have more information about the content of Carey's lectures, we can look at his 1958 symposium volume *Continental Drift*, which he must have promoted. There we find a well illustrated article by Edward Irving (1958) on the relation of paleomagnetism to continental drift. He showed that the determination of paleolatitudes from various continents compared favorably with the positions that Köppen and Wegener (1924) had determined using paleoclimate indicators. The paleopole positions fit better with various drift reconstructions than they did with the current continental positions. The magnetic evidence did not prove continental drift, but supported it.

More importantly, Carey's own article in that volume had four eye-opening illustrations: his fit-map of Africa and South America (see Fig. 85), a map by Ewing of the world-wide "mid-ocean crack system", the map by Rothé (1954) showing that ocean-floor seismicity corresponds precisely to these cracks (see Fig. 86), and new diagrams showing that the mid-ocean ridges have central valleys, similar to rifts. If Carey's interpretations of an expanding Earth were some sort of illusion, at least these were real facts he was dealing with. And if he was pulling rabbits out of a hat, at least people could see that the rabbits and the hat were real. Some listeners must have been inspired to look into the hat themselves.

ROBERT DIETZ AND HARRY HESS (1961, 1962) WITH IDEAS WHOSE TIME HAD COME

Scientific journals require evidence and results that are new. It is second nature for scientists to present their work as original. Although authors are expected to mention previous work, they know how to do this without detracting from the novelty of their own contributions.

Two papers by American geologists are often cited as being the inception of the plate tectonic revolution. One was by Robert S. Dietz in 1961: "Continent and ocean basin evolution by spreading of the sea floor." The other was by Harry H. Hess in 1962: "The origin of ocean basins." Both papers dealt mainly with the oceans, but the implications for continental drift were obvious. It is relevant here to consider how these authors dealt with the earlier drift ideas. This was how Dietz referred to continental drift:

Dietz 1961, p. 856:

Former scepticism about continental drift is rapidly vanishing, especially due to the palæomagnetic findings and new tectonic analyses. A principal objection to Wegener's continental drift hypothesis was that it was physically impossible for a continent to 'sail like a ship' through the sima; and nowhere is there any sea floor deformation ascribable to an on-coming continent. Sea floor spreading obviates this difficulty: continents never move through the sima–they either move along with it or stand still while the sima shears beneath them. The buoyancy of the continents, rather than their being stronger than the sima, accounts for this. Drag associated with the shearing could account for alpine folding and related compressional tectonic structures on the continents.

Dietz did not explain in detail why skepticism to continental drift was vanishing, but only hinted at "the palæomagnetic findings and new tectonic analyses." These findings and analyses were to be found in Carey's symposium volume. Earlier on this page, Dietz had mentioned Carey's paper, without calling to attention the important evidence that could be found there. He only noted Carey's implausible expanding-Earth conclusion:

Dietz 1961, p. 856.
Carey's[11] tectonic analysis has resulted in the need for a twenty-fold increase in volume of the Earth. Spreading of the sea floor offers the less radical answer that the Earth's volume has remained constant.

Dietz refuted "Wegener's continental drift hypothesis". But Wegener's hypothesis was a straw man, which geologists had been beating for 30 years. His hypothesis had been replaced by Holmes's (1931, 1944) and Du Toit's (1937). In their drift hypotheses, the continents moved together with the sima, just as Dietz was proposing.

The breakthrough paper by Hess was widely circulated among geologists already in 1960, but was first published some months after Dietz (1961). Hess also proposed sea-floor spreading, but did not use this term for it. It was caused by convection:

Hess 1962, p. 607:
CONVECTION CURRENTS IN THE MANTLE AND MID-OCEAN RIDGES
Long ago Holmes suggested convection currents in the mantle to account for deformation of the Earth's crust (Vening Meinesz, 1952; Griggs, 1939; 1954; Verhoogen, 1954; and many others). Nevertheless, mantle convection is considered a radical hypothesis not widely accepted by geologists and geophysicists. If it were accepted, a rather reasonable story could be constructed to describe the evolution of ocean basins and the waters within them. Whole realms of previously unrelated facts fall into a regular pattern, which suggests that close approach to satisfactory theory is being attained.

Hess only mentioned Holmes. He did not cite a publication, and his mention of "deformation of the Earth's crust" was very vague. He did not tell that Holmes had used the convection idea to account also for the mid-ocean ridges, creation of new oceanic crust, destruction of old oceanic crust, and displacement of continents. Instead, Hess stated that he was inventing these things, that they were new and daring. Near the beginning of his paper, he wrote:

Hess 1962, p 599
I shall consider this paper an essay in geopoetry. In order not to travel any further into the realm of fantasy than is absolutely necessary I shall hold as closely as possible to a uniformitarian approach;

In the final paragraph, referring to his paper as a chapter of the volume in which it appeared, Hess wrote:

Hess 1962, p. 618
In this chapter the writer has attempted to invent an evolution for ocean basins. It is hardly likely that all of the numerous assumptions made are correct. Nevertheless it appears to be a useful framework for testing various and sundry groups of hypotheses relating to the oceans. It is hoped that the framework with necessary patching and repair may eventually form the basis for a new and sounder structure.

Hess was re-inventing an evolution for ocean basins that had been invented by Holmes. The paper by Hess did indeed form the basis for a new and sounder structure.

Both Dietz and Hess had presented old ideas, and now they were ideas whose time had come.

I do not think it was Hess's intention to take credit for Holmes' ideas. He always gave much credit to Holmes in later years. But he understood that used goods do not sell well in the marketplace of modern science. His paper would not have been appreciated if he had written "Many of these suggestions were published in a paper by Arthur Holmes in 1931, and again in his textbook in 1944." Science authors must write in ways that make their papers seem new.

J. TUZO WILSON'S SUMMARY OF THE NEW CONTINENTAL DRIFT (1963)

Scientific American is a monthly science magazine where respected scientists write about current scientific developments for the educated public. In the issue of April 1963, J. Tuzo Wilson (1908-1993) published a paper that might now be considered the first modern presentation of the new global tectonics. The title was "Continental Drift", and Wilson gave abundant credit to Wegener, Du Toit, Holmes, and many others. (He did not mention Carey.)

Wilson reconstructed "the supercontinent" by closing the Atlantic and Indian Oceans. He did this by guiding the continents back into place along the great fracture zones that extend from the mid-ocean ridges. Here are two paragraphs that refer to his map (see Fig. 89, the reproduction of this map in Longwell's textbook):

Wilson 1963, p.98.

There are therefore enough connections to draw all the continents together, reversing the trends of motion indicated by the mid-ocean ridges and using the continental ends of pairs of lateral ridges as the means of matching the coast lines together. The ages of the islands and of the coastal formations suggest that about 150 million years ago, in mid-Mesozoic time, all the continents were joined in one land mass and that there was only one great ocean *(see illustration on page 27)*. The supercontinent that emerges from this reconstruction is not the same as those proposed by Wegener, Du Toit and other geologists, although all have features in common. The widespread desert conditions of the mid-Mesozoic may have been a consequence of the unusual circumstance that produced a single continent and a single ocean at that time. Since its approximate location with respect to latitude is known, along with the location of its major mountain systems, the climate in various regions might be reconstructed and compared with geological evidence.

It is not suggested that this continent was primeval. That it was in fact assembled from still older fragments is suggested by two junction lines: the ancient mountain chain of the Urals and the chain formed by the union of the Appalachian, Caledonian and Scandinavian mountains may have been thrown up in the collisions of older continental blocks. Before that there had presumably been a long history of periodic assembly and disassembly of continents and fracturing and spreading of ocean floors, as convection cells in the mantle proceeded to turn over in different configurations. At present it is impossible even to speculate about the details.

Wilson explained and illustrated the important concepts of plate tectonics. He showed a map of the current plate boundaries, with arrows indicating their directions of movement. He showed how convection currents in the mantle cause spreading at the ridges, sinking with associated deep earthquakes at the trenches, and passive rifting and displacement of the continents. He did not yet have an understanding of arc magmatism, or the significance of transform faulting. But he illustrated what later would be called hot spots, and how a chain of islands is formed in the plate that moves above a hot spot.

Wilson's "Continental Drift" in April 1963 showed American readers where the new science of geology was heading. After that publication, I think that the plate tectonic revolution could be compared to a 1000-piece jigsaw puzzle being assembled by the residents of a nursing home. There were enough pieces out now to see its full size and most of the picture. Those who liked this sort of thing could play. There would be some elbowing in the rush to put in the pieces, but it was pretty certain that most of the pieces were there and that they would fit. The maverick Sam Carey had told many Americans about this puzzle and inspired them to get started putting it together.

MAGNETIC ANOMALIES AND THE RECORD OF SEA FLOOR SPREADING (1963)

While the puzzle was being assembled, a most surprising piece was found: sea-floor magnetism. Magnetic anomalies had made a permanent record of the movement of plates through time. Suddenly scientists could read how fast the plates were spreading both now and in the past. They could determine when the oceans had begun to open, and where the continents had been at various times since the breakup of Pangæa. Lawrence Morley picked up this piece of the puzzle, but Frederick Vine and Drummond Mathews managed to place it first, and it became known as the Vine-Mathews hypothesis (1963). This is an example of the right people being at the right place at the right time. This piece has historical importance. But the puzzle assemblage was well underway before this piece was placed.

EDWARD BULLARD'S OBJECIVE FIT OF CONTINENTS (1965)

Edward Bullard presented a map in 1965 showing the remarkable fit of South America and Africa. But Carey had published a map that was nearly as remarkable seven years earlier. It was necessary for Bullard to refer to the map of Carey, while at the same time making his own map appear as new as possible. Here is how he did it, in his introduction:

Bullard 1965, p. 41

The real 'edge of the continent' is the continental slope where the sea floor runs down steeply from 50 or 100 fm. to over 2000 fm. in a few miles. Wegener pointed out that the fit should be made at the continental edge, but did not pursue the matter in detail. Carey (1958) was the first to show that the fit of Africa and South America is much closer at the continental edges than it is at the coastline. In spite of this, Jeffreys has expressed a total disbelief in the reality of the fit; he says (1964): 'I simply deny there is an agreement.' The reason for this scepticism is not clear; perhaps it is connected with doubts about the accuracy of Carey's fits carried out on a globe provided with moveable transparent caps.

The matter is clearly important and the purpose of this paper is to put the facts beyond doubt by using the best data available and finding the 'best fits' by objective arithmetic methods. The results do not depend on the small scale and generalized topography shown on globes and are unaffected by errors of tracing or uncertainties of judgement as to what is the best fit. There is, of course, some arbitrariness and personal judgement in choosing what is to be fitted. We have fitted continental blocks that seemed by inspection to fit well and which could reasonably be supposed to have once been in contact and to have moved apart."

A look at Carey's map shows that the fit was excellent and Jeffreys was denying the obvious. But readers of Bullard's paper did not know this, because they did not have Carey's map on hand. By quoting Jeffreys in this way, Bullard made it seem like Carey's fit was easily disregarded. Jeffreys had been fighting continental drift for the past 40 years. As far as Jeffreys was concerned, even Bullard's fit could not "put the facts beyond doubt." Jeffreys would continue to deny plate tectonics for the next 24 years of his long life.

By writing that the purpose here was to find the best fits by "objective arithmetic methods," Bullard made it seem that Carey's methods were not objective. It was comfortable for Bullard and other geologists to overlook the accuracy and detail that Carey's map actually showed. Carey had used a 30-inch globe, and carefully traced the continental edges on spherical plastic overlays. Carey had been showing his map to geologists for years, but it was very rarely referred to. Bullard's map was certainly better in that it also included the fit of continents along the northern Atlantic. But the main reason that his map was widely accepted, was because the time was right, and he presented it in just the right way.

GEOLOGICALLY AND POLITICALLY CORRECT ARTHUR HOLMES (1965)

Arthur Holmes was cautious in his support of continental drift. After 1933, he published no more scientific papers promoting it. Recall that he downplayed the evidence for drift in the final chapter of his 1944 textbook (see p. 218) by ending it with the comment that "purely speculative ideas of this kind, specially invented to match the requirements, can have no scientific value until they acquire support from independent evidence."

Still, there must have been some question as to his leanings; might he be a closet mobilist? He publically denied this in 1953, in the journal *Nature*: "despite appearances to the contrary, I have never succeeded in freeing myself from a nagging prejudice against continental drift; in my geological bones, so to speak. I feel the hypothesis to be a fantastic one. But this is not science, and in reaction I have been deliberately careful not to ignore the very formidable body of evidence that has seemed to make continental drift an inescapable inference." (see Le Grand 1988, p. 136.)

His British and American colleagues could then feel certain that he would not espouse mobilism. In 1956, Holmes was awarded two of geology's highest honors: the Wollaston Medal in Britain and the Penrose Medal in America. The subject of continental drift was not mentioned in connection with either of the award presentations (Lewis 2002).

Holmes produced a second and much enlarged edition of this textbook in 1965, the year that he died at the age of 75. In that book he mentioned the theory of continental drift in several chapters. The last chapter was expanded and had the title *Continental Drift and Palaeomagnetism*. There, Holmes commented on the unfair treatment that the continental drift hypothesis had received:

Holmes 1965, p. 1203-1204.

Although the controversial storm has now abated and Wegener's name has become an honored one, the idea of continental drift is still assailed from a few unexpected quarters by 'the slings and arrows of outrageous' critics. The Russian geologist Beloussov, for example, in the 1962 translation of his *Basic Problems of Geotectonics*, refers in scathing terms to 'the total vacuousness and sterility of the hypothesis' and professes 'profound amazement' that it should ever have been seriously discussed. Two points in this amorphous denunciation merit brief comment. Equating the great concept of continental drift with Wegener's 'hypothesis' is an obvious source of unnecessary confusion. There never was a single hypothesis in this vast field. Of the large number of hypotheses involved, some were inadequate and some were wrong. But others have turned out to be far more fruitful than even Wegener himself could have expected. In particular it should never be overlooked that in many respects his assemblage of the Carboniferous continents (Fig. 862) is in better accord with all our modern evidence than several of the modifications proposed during the last fifty years.

Secondly, 'sterility' is about the least appropriate and most unjust epithet that could be applied to the continental drift concept. The latter, like the search for oil and radioactive minerals, and the exploration of the ocean floor, has won the distinction during the last few decades of being amongst the most stimulating incentives to geological and geophysical research. To have taken so high a place in the 'top ten', so to speak, is a praiseworthy record for any 'hypothesis'. One therefore wonders what could be the source of Beloussov's so grossly unfair judgment. The writer would hazard the guess that Beloussov, as one of the leading exponents of the geological importance of vertical earth movements, has fallen in the all-too-common fallacy of *either ... or ...* In this case *either* the predominance of vertical and oscillatory movements *or* the predominance of horizontal movements required for continental drift. It need only be added that neither type of movement excludes the other. Both are spectacular expressions of the working of the earth's internal machinery.

What is really important is not to disparage Wegener's great achievement because some of his conjectures can be shown to be wrong, but from the wealth of relevant evidence now available to assess the extent to which continental drift and other lateral displacements of the crust are genuine geological phenomena. Parts of the evidence have already been outlined in earlier chapters. But much more remains to be discussed, notably in connection with palaeomagnetism, to which we now turn.

Holmes chastised this well-known Soviet scientist for "outrageous," "unjust," and "unfair" treatment of the idea continental drift. He refrained from pointing out that some leading British and American scientists had been treating this hypothesis in just the same way.

WEGENER'S 4th EDITION FINALLY TRANSLATED TO ENGLISH (1966)

Wegener's 4th edition of *Die Entstehung der Kontinente und Ozeane* in 1929 had major improvements over the 3rd edition, which had been translated to several languages. A French translation of this 4th edition became available in 1937, and an Italian translation in 1942. An English translation finally appeared in 1966, after the theory of continental drift had become scientifically acceptable.

FULL ACCEPTANCE OF MOBILISM, BUT NOT OF WEGENER OR DRIFT (1969)

J. Tuzo Wilson (1968) proposed that the scientific breakthrough of mobilism be called the *Wegenerian revolution*. But most geologists were not willing to give Wegener such an honor. In 1969 geologists began using the term *plate tectonics*, and the revolution naturally took the same name.

In any case, by 1969, mobilism was generally acknowledged. That year was also notable for three significant publications. One of these was a new AAPG symposium volume, of over a thousand pages, that reversed the judgment of the 1928 volume (see p. 130). It was entitled *North Atlantic – Geology and Continental Drift, a Symposium*. It contained papers from an international meeting in Newfoundland in 1967, where North American and European geologists were now fully convinced of mobilism. Also in 1969 appeared new editions of the two leading North American textbooks *Physical Geology* and *Historical Geology*, which could no longer deny mobilism (see p. 248).

Textbooks continue to remark that Wegener and his idea of continental drift were wrong: in plate tectonics, it is lithospheric plates, not continents that are mobile. Textbooks point out that the term continental drift should only be used in a historical context, for this misinterpretation.

I hope that future textbooks will now make a distinction between these two historical terms. They can point out that Wegener preferred the term continental displacement, which put the emphasis on the geologic evidence that continents had been united and were now displaced. Skeptics preferred the term continental drift, which emphasized the lack of a suitable mechanism for the continental movement.

11. Closing statements

OTHER HISTORIES – RECOMMENDED READING

No history book is complete or completely impartial in its presentation of a complex issue. Certainly a revisionist history such as this one will not be accepted readily. Serious students of the history of continental drift will want to consult other presentations. Here are excellent books on important aspects of this history:

1937. Alex du Toit. *Our Wandering Continents.*

1973. Anthony Hallam. *A Revolution in the Earth Sciences*.

1973. Ursula B. Marvin. *Continental Drift, The Evolution of a Concept.*

1985. Robert Muir Wood. *The Dark Side of the Earth. The Battle for the Earth Sciences.*

1988. Homer E. Le Grand. *Drifting Continents and Shifting Theories.*

1990. John A. Stewart. *Drifting Continents and Colliding Paradigms*.

1999. Naomi Oreskes. *The Rejection of Continental Drift; Theory and Method in American Earth Science.*

2007. Ted Nield. *Supercontinent: Ten Billion Years in the Life of Our Planet*

2012. Henry Frankel. *The continental drift controversy: Wegener and the early debate.*

2012. Henry Frankel. *The continental drift controversy: paleomagnetism and confirmation of drift.*

2012. Henry Frankel. *The continental drift controversy: introduction of seafloor spreading.*

2012. Henry Frankel. *The continental drift controversy: evolution into plate tectonics.*

A LIST OF FIXISTS AND MOBILISTS WITH PUBLICATION DATES

Many scientists can be classified as either fixist or mobilist by their publications. I have compiled a list of such scientists and the dates of their relevant publications. There are probably a few mistakes here. I have not seen all the original publications, but have collected the names and dates from many sources, especially from Du Toit (1937.) I have not checked my results against the new information provided by Frankel (2012), but recommend others to do so.

Where a publication is listed as fixist, it does not mean that the author had actively rejected the mobilist alternative. At the time of publication, most of these fixists did not have enough information about mobilism to give it serious consideration. Many of the publications are considered fixist because the author used the theory of fixed continents and sunken land bridges to explain the distribution of fossils.

Three of the mobilists here (Darwin, Fisher, Pickering) could instead be classified as fixists. They argued that the continents were displaced early in the Earth's history, during formation of the Moon, and that they had been fixed since that time.

Fixists

Andrée, Karl: 1906, 1914, 1917

Andrews, E. C.: 1926, 1938

Arldt, Theodor: 1907, 1918, 1919

Barrell, Joseph: 1917

Berry, Edward W.: 1922, 1927, 1928

Betim, Alberto: 1929

Born, A.: 1933

Bowie, William: 1928, 1935

Brooks, C. E. P.: 1926, 1941

Bucher, Walter H.: 1933, 1941, 1952, 1956, 1957

Burckhardt: 1917

Chamberlin, Rollin T.: 1928.

Coleman, Arthur P.: 1925, 1926, 1932

Cloos, Hans 1937, 1939, 1947

Croneis, Carey: 1936, 1966

Diener, Carl: 1915

Douglas, G. V.: 1934.

Dunbar, Carl O. 1933, 1941, 1949, 1960

Frech, F.: 1897

Gutenberg, B.: 1927, 1932, 1936, 1949, 1951, 1954

Grabau, Amadeus W.: 1913, 1921, 1924, ~~1936, 1937, 1938, 1940~~

Gregory, J. W.: 1925, 1928, 1929

Handlirsch, A.: 1913

Haug, E.: 1908, 1911

Hobbs, W. H.: 1923

Jeffreys, Harold: 1924, 1929, 1935, 1952, 1959, 1962, 1969, 1970, 1976

Joleaud, Léonce: 1919, 1924

Just, Theodore: 1952

Karpinsky, A.: 1894

Katzer, F.: 1897

Keith, Arthur: 1923

Kober, L.: 1921, 1933

Koken, E.: 1893

Kossmat, Franz: 1908 , 1921, 1936

Krenkel, E.: 1925
Krumbein, William: 1936, 1966
Kuenen, P. H.: 1935, 1950
Lake, Philip: 1922, 1923, 1933
Lapparent, A. de: 1906
Leme, A. B. P.: 1929
Longwell, Chester R.: 1928, 1941, 1944, 1948, 1955, 1958, 1962
Matthew, W. D.: 1906, 1915
Mayr, Ernst: 1952
Mushketov, D. J.: 1936
Neumayr, M.: 1886, 1895
Ortmann, A. E.: 1902
Osborn: (1917)
Penck, A.: 1894, 1921
Reid, Harry Fielding: 1922
Scharff, Robert Francis: 1911
Schuchert, Charles: 1910, 1915, 1923, 1928, 1932, 1936, 1941
Schweydar, Wilhelm: 1921
Semper, Max: 1896, 1917
Simpson, George Gaylord: 1943a. 1943b, 1949, 1952, 1953
Sörgel, W.: 1916, 1917
Stille, Hans: 1924
Suess, Eduard: 1885, 1909
Termier, Pierre: 1924
Umbgrove: 1942, 1947
von Huene: 1925
von Ihering, H.: 1907, 1927, 1931
Washington, H. S.: 1923
White, David: 1907, 1910, 1928
Willis, Bailey: 1909, 1910, 1929, 1932, 1936, 1944
Wright, W. B.: 1923

Mobilists
Ampferer, Otto: 1906, 1911, 1924, 1925, 1926, 1928
Argand, Émile: 1916, 1922, 1924
Bailey, E. B.: 1929, 1935, 1939
Baker, Howard B.: 1911, 1912, 1914, 1932
Behm, Hans Wolfgang: 1923
Bogolepow, W.: 1930

Borchert, H.: 1932
Bull, A. J.: 1927
Caster, Ken: 1952
Carey, Samuel Warren 1955, 1956, 1958
Chelikowsky, Joseph: 1944
Choubert , Boris: 1935
Dacqué, Edgar: 1915, 1926
Daly, Reginald A.: 1923, 1925, 1926, 1933
Darwin, G. H.
Dixey, Frank: 1938
Edgeworth, David T. W.: 1928
Evans, John W.: 1923, 1925, 1926
Field, Richard: 1937
Fisher, Osmond. 1882, 1889
Gheyselinck, Robert: 1939
Gicouate, Moisés: 1945
Grabau, Amadeus W.: ~~1913, 1921, 1924~~, 1936, 1937, 1938, 1940
Harrison, Launcelot: 1924, 1926, 1928
Haug, E.: 1908, 1911
Hills, George F. S.: 1947
Holland, T. H.: 1933, 1937, 1941, 1943
Holmes, Arthur: 1928, 1929, 1930, 1931, 1933, 1944, 1953, 1965
Jaworski, Erich: 1922
Joly, John: 1925, 1926, 1928, 1930
King, Lester C.: 1953, 1945
Kirsch, G.
Köppen, Wladimir: 1924
Kreichgauer, P. D. 1902, 1926
Krige, L. J.: 1926, 1930
Lambert, Walter: 1921
Leuba, John: 1925
Longfellow, D. W
Maack, R.: 1934
Mather, W. W. 1843
Molengraaff, G. A. F.: 1916, 1928
Nissen, Henrik: 1934
Owen, Richard: 1857
Pickering, William H. 1907
Rastall, R. H.: 1929, 1941

Read, Herbert H.: 1949
Roubault, Marcel: 1949
Russo, Philibert: 1930
Schultz, Bruno: 1921
Schwinner, R
Seward, A. C.: 1933, 1943
Sewell, R. B.: 1938
Shand, Samuel J.: 1933, 1937
Simpson, George Clarke 1923, 1929, 1930
Smit Sibinga, G. L.: 1927
Snider-Pelligrini, Antonio 1858
Staub, Rudolf: 1924, 1928a, 1928b
Steers, James A.: 1932
Taylor, Frank B.: 1910, 1923, 1925, 1926, 1928
Toit, Alex. L. du: 1921, 1927, 1929, 1937, 1939, 1944
Van der Gracht, W. A. J. M. van Waterschoot: 1928, 1931, 1933
Wade, Arthur: 1927, 1934, 1935, 1941
Warring, C. B. 1887
Watts, W. W.: 1935.
Wegener, Alfred: 1912a, 1912b, 1915, 1920, 1922, 1924, 1926, 1928, 1929
Weiss, Fredrick: 1922
Wettstein, H. 1880
Wing Easton, N. 1921
Wooldridge, S. W.: 1937
Wright, William: ca 1922-23
Wright, C. S.: 1922
Zeuner, Frederick E.: 1946

Mobilism must have seemed far-fetched to most geologists when they first heard of it. But it made accurate scientific predictions and was eventually supported by much positive evidence. It explained fossil distributions, mountain ranges, and paleoclimates, and it finally eliminated the unpopular theory of sunken continents. As fixists became more aware of the evidence in support of mobilism, one would expect that many of them would have published another paper, now in support of it. In fact, they did not.

I can summarize the results like this: Of over 60 fixists, I find none (except Amadeus Grabau) who later published as mobilists. Of over 60 mobilists, I find none (except Grabau) who had previously published as fixists.

One might say "Fixists always Fxists." But this consistency relates to scientists' publications, not necessarily to their opinions, which might have changed. We can only say that if the fixists converted to mobilism, they seem to have not published papers to document that conversion.

Maybe this shows something about scientists, that they do not readily disavow their previous opinions, especially if those opinions were set in print. It might have to do with the nature of a publication record, and the nature of a scientist. Maybe scientists can be characterized as steadfast and dependable, not adaptable and persuadable.

In any case, scientists are people – they sometimes make mistakes, and they do not like to be corrected. An ambitious scientist might hope to "rewrite the book" on his science, but not if the book that needs rewriting is his own. Under the right circumstances, a scientist will correct his earlier mistakes. If he is personally involved in making new discoveries, he will be happy to publish a new paper that announces the developments and corrects his earlier conclusions. If his competitor makes the new discoveries, he will usually accept them and adopt the new results in his next published paper on this topic. But if there are no new discoveries, and it becomes apparent that he simply made an incorrect judgement, he will probably not publish further on that particular topic.

There are many avenues of research open to scientists, and it is easy to drop an awkward topic and go another direction. It is probably better for one's scientific career to pursue success in other areas than to try to correct previous mistakes. I think that as fixists began to realize that continents had moved, they quietly dropped out of sight on this topic. Textbook authors did not have that option; they had to keep publishing new revised editions.

A CONCERTED EFFORT TO BLOCK THE DRIFT HYPOTHESIS

One often finds what one is looking for, in daily life and in science. Alfred Wegener and Alex du Toit were looking for geologic evidence in support of mobilism, and they found them. Arthur Holmes was looking for ways that the Earth eliminates excess radioactive heat and moves continents, and he found them. Fixists were looking for weaknesses in Wegener's theory, and they found them. I was looking for cover-ups in the scandal of the continental-drift rejection, and I found them.

It was Charles Schuchert – professor emeritus at Yale University, Past President of the Geological Society of America, editor of the *American Journal of Science*, and author of the leading American textbook of historical geology – who headed the campaign against continental drift. Wegener's theory would have been taken as a working hypothesis, were it not for Schuchert's conscious decision to hide and discredit evidence in support of it. His tactical moves began in 1924 with the second edition of his textbook *Historical Geology*. He understood then that the directions of Permian glacial ice-flow supported mobilism, and he removed arrows from his map that showed those directions. He carefully worded his explanations to sidestep other evidence. His efforts continued in his publications, where he misquoted and ridiculed Du Toit, Daly, and others. He decided not to update and correct key maps and paragraphs of his textbooks. He succeeded in keeping Holmes' breakthrough article on convection-currents out of the *American Journal of Science*. His professional correspondence, which I have not looked at, except the parts revealed by Oreskes (1999), suggests that he carried out a lobbying effort for fixism and against mobilism. He must have felt that his reputation was at risk, and his friends helped him to maintain that reputation.

Schuchert encouraged his younger Yale colleagues Chester Longwell and Carl Dunbar to join him as textbook authors and to write as fixists. Out of loyalty to him and respect for his work, they remained fixists even after his death in 1942. Their textbooks taught the basic principles of geology, including fixism, to initially open-minded students.

After mobilism had been generally accepted, an additional honor was bestowed upon Schuchert. In 1973, the Paleontological Society of America established the *Charles Schuchert Award,* which is given each year "to a person under 40 whose work reflects excellence and promise in the science of paleontology." There is no doubt that Schuchert made outstanding scientific contributions and deserved the awards he was given. But maybe it is now time for an *Alfred Wegener Award*, given each year to a person in any science whose work, after years of rejection, is acknowledged as having been praiseworthy.

THE PERCEIVED STATUS OF THE NORTH AMERICAN CONTINENT

The hypothesis of continental drift was not only denied, but ridiculed by geologists in North America. I think this was because of strong feelings of continental chauvinism. A geologic declaration of independence for the North American continent had been taught through the textbooks of James Dwight Dana from 1863 to 1895.

Dana believed that God created the North American continent during the separation of land and water as described in Genesis. Charles Schuchert and Bailey Willis did not buy into that belief, but they seemed to accept that North America had been ideally situated since the waters first gathered. North American geologists were brought up with this doctrine of North America being the type continent, so there was deep-seated resistance to Wegener's suggestion that it was not really so special.

Stephen Jay Gould (2000) pointed out that Sigmund Freud had much insight into the psychology of scientific revolutions. Freud wrote that these revolutions result from a scientific development that shows people to have lesser status than previously perceived. The Copernican revolution (heliocentrism vs. geocentrism) was a struggle because it would no longer allow people to envision their planet as the center of the universe. The Darwinian revolution (evolutionism vs. creationism) meant that people could no longer consider themselves separate from animals. By the same reasoning, although not considered by Freud or Gould, the Wegenerian revolution (mobilism vs. fixism) meant that North American geologists could no longer view their continent as separate from others since the beginning of geologic time. Geologists from other continents had no such perceptions, and could more easily accept a shared geological history.

NORTH AMERICA GETS ITS OWN LITHOSPHERIC PLATE

I contend that a combination of wishful thinking and leading textbooks helped maintain an incorrect consensus. We should be looking for similar examples in current science. I think I have an example that is relevant here.

The early maps of lithospheric plate boundaries (Wilson 1963, Le Pichon 1968, Morgan 1968) showed that each of the continents had its own plate; all except North and South America, which shared the single American Plate. This meant that North American had gone from being the most special continent, to one of the least special. North American geologists must have been disappointed, whether they were aware of it or not.

The first American geology textbook that fully adopted plate tectonics was *Earth*, by Press and Siever (1974). On the inside front cover was a two-page map showing the plates of the world (Fig. 92). This map solved the problem for North Americans: there was indeed a North American Plate, separated from a South American Plate by a dashed line that marked in the legend as an “Uncertain plate boundary.”

The map was from an article in *Scientific American* by John Dewey (1972). There was no discussion of this plate boundary, or evidence for it, either in the original article or in the textbook. *Scientific American*, by the way, published many excellent articles on plate tectonics, and offprints of these were used from the mid-1960s to the mid-1970s to supplement textbooks, which were not up-to-date on this topic.

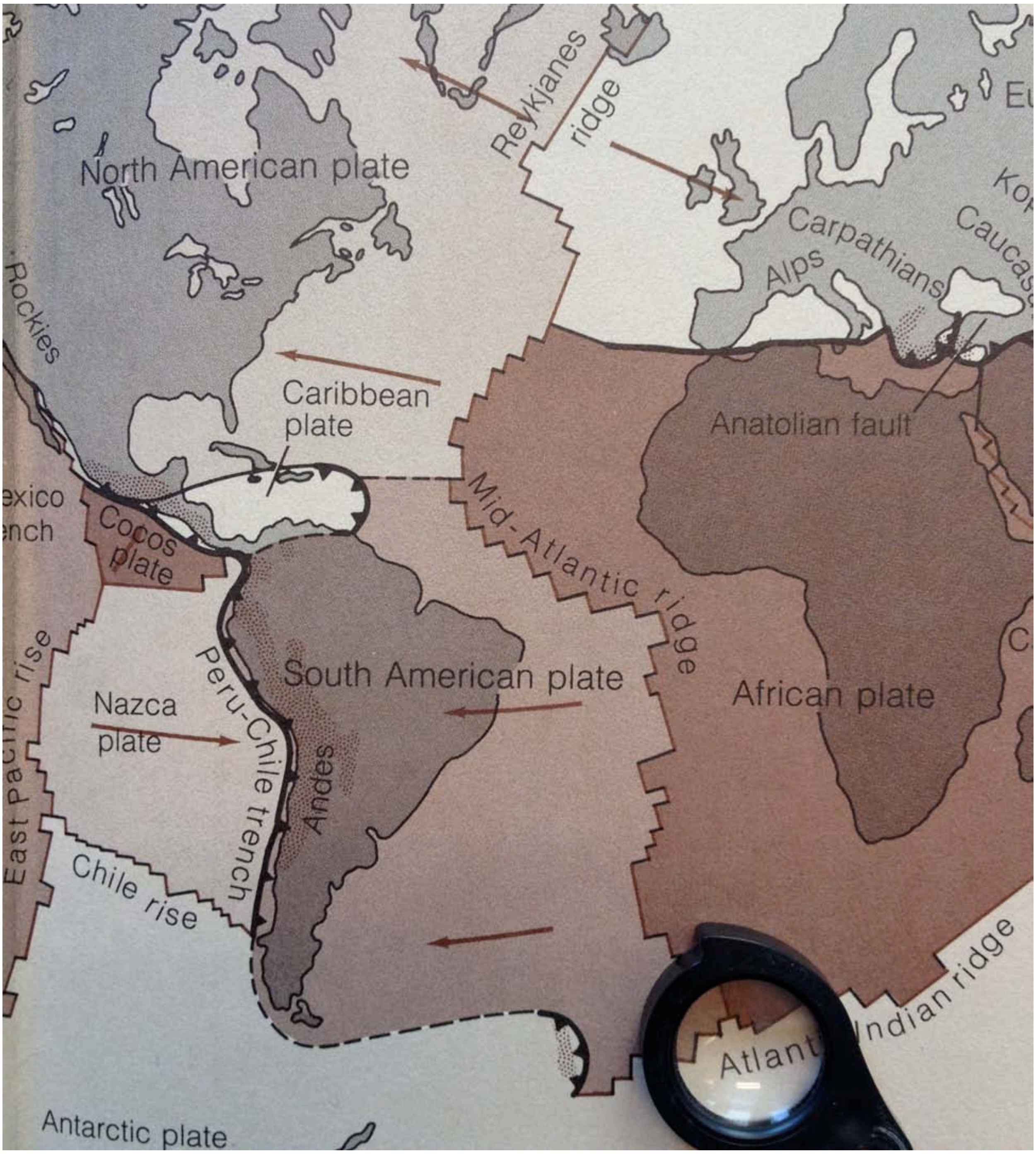


Figure 92. Part of the map showing “The Plates of Earth’s Lithosphere” from the inside front cover of *Earth*, by Press and Siever (1974).

This invention of a North American Plate was well received. Most current geology textbooks also show a plate boundary near here. It is usually drawn to look like a fracture zone, although students learn that fracture zones are not plate boundaries. Some textbooks mark it incorrectly as a transform plate boundary. Others leave it unspecified, or draw no boundary on the map. None call attention to it.

Locations of earthquake epicenters are the usual means of recognizing plate boundaries and defining plates. There are no earthquakes that suggest a boundary here (Krill, 2011b). The best evidence for two separate plates seems to be GPS data showing that the North American and South American Plates are moving with respect to two slightly different Euler poles (Dixon and Mao, 1997). I am no expert, but I suspect that if an imaginary plate boundary were drawn through any large plate, a similar amount of GPS data on either side of the line would give two slightly different Euler poles.

According to Müller and Smith (1993) considerable motion has occurred along this plate boundary. From 84-67 Ma, there was left-lateral transtension, with 100 km of strike slip movement and about 50 km extension. Then from 67-10 Ma there was right-lateral transpression, with 90 km of strike slip movement and 30-70 km compression. To me this seems like a little dance: facing partners hold hands and take small-step-back-and-big-step-right ("Stretch those arms!"), then small-step-forward-and-big-step-left ("Right back to start position!"). I think that the existence of the North American Plate is an example of a scientific conclusion that is so convincing that evidence for it simply must be found.

12. References

[Anonymous] 1923, The distribution of life in the southern hemisphere, and its bearing on Wegener's hypothesis: *Nature*, p. 131

Argand, É., 1924. *La tectonique de l'Asie*,. Comptes-Rendus de la 13e Congrès Géologique International, Liége, p. 171-372. (see also Carozzi.)
Bailey, E.B. 1939, General geology, *in* Bailey, E.B., and Weir, J., Introduction to geology: London, Macmillan, p. 1-244, p. 381-498.

Beattie, J., 1774. *The minstrel, or the progress of genius*, book 2, stanza 1.

Behm, H. W., 1923, *Entwicklungsgeschichte des Weltalls, des Lebens und des Menschen*: Stuttgart, Franckh'sche Verlagshandlung, 232 p.

Berry, E.W., 1928, Comments on the Wegener hypothesis, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium:* Tulsa, American Association of Petroleum Geologists, p. 194-196.

Blanford, W.T., Blanford, H.F., and Theobald, W. Jr., 1856, On the geological structure and relations of the Talcheer Coal Field, in the District of Cuttack: *Memoirs of the Geological Survey of India*, v.1, p. 33-89.

Bowie, W., 1928, Comments on the Wegener hypothesis, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p.178-186.

Brooks, C.E.P., 1926, *Climate through the ages, a study of the climatic factors and their variations*: New York, R.V. Coleman, 439 p.

Brooks, C.E.P., 1949, *Climate through the ages* (revised edition.): London, Ernest Benn, 395 p.

Bullard, E., Everett, J.E., and Smith, A.G., 1965, The fit of the continents around the Atlantic, *in* Blackett, P.M.S., Bullard, E., and Runcorn, S.K., *A symposium on continental drift*: London, Philosophical Transactions of the Royal Society of London, series A, v. 258, no 1088, p. 41-51.

Butler, J., 1740, *The analogy of religion, natural and revealed, to the constitution and course of nature*: London, John and Paul Knapton, 467 p.

Carey, S.W., 1958, A tectonic approach to continental drift, *in* Carey, S. W. editor, *Continental drift a symposium*: Hobart, University of Tasmania, p. 177-355.

Carey, S. W., editor, 1958, *Continental drift, a symposium*: Hobart, University of Tasmania, 363 p.

Carey, S.W., 1988, *Theories of the Earth and Universe, a history of dogma in the Earth Sciences*: Stanford, California, Stanford University Press, 413 p.

Carozzi, A.V., editor and translator, 1977, *Tectonics of Asia*: (English translation of Argand 1924). New York, Hafner Press, 218 p.

Chamberlin, R.T., 1928, Some of the objections to Wegener's theory, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p. 83-87.

Chamberlin, T.C., 1889, The method of multiple working hypotheses: *Journal of Geology* 5, p. 837-848.

Coleman, A.P., 1925, Permo-Carboniferous glaciation in the Wegener hypothesis: *Nature*, v. 115, p. 602.

Coleman, A.P., 1926, *Ice ages recent and ancient*: New York: Macmillan, 296 p.

Coleman, A.P., 1932, Glaciation and continental drift: *Geographical Journal*, v. 79, p. 252-255.

Condie, K.C., 2005, *Earth as an evolving planetary system*: Amsterdam, Elsevier, 447 p.

Crowell, J.C., 1978, Gondwanan glaciation, cyclothems, continental positioning, and climate change: *American Journal of Science*, v. 278, p. 1345-1372.

Daly, R.A., 1926, *Our mobile Earth*: New York, Charles Scribner, 342 p.

Daly, R.A., 1933, *Igneous rocks and the depths of the Earth*: New York, McGraw Hill, 598 p.

Daly, R.A., 1938, *Architecture of the Earth*: New York, D. Appleton-Century, 211 p.

Daly, R.A., 1940, *Strength and structure of the Earth*: New York, Prentice-Hall, 434 p.

Dana, J.D., 1863a, *Manual of geology: treating the principles of the science with special reference to American geological history*: Philadelphia, Theodore Bliss, 798 p., 1 pl.

Dana, J.D., 1863b, *A text-book of geology, designed for schools and academies*: New York, Ivison, Blakeman and Taylor, 354 p.

Dana, J.D., 1866, *Manual of geology: treating the principles of the science with special reference to American geological history*: Philadelphia, Theodore Bliss, 800 p., 1 pl.

Dana, J.D., 1874, *A text-book of Geology, designed for schools and academies*, 2nd edition: New York, Ivison, Blakeman and Taylor, 358 p.

Dana, J.D. 1875, *The geological story, briefly told, an introduction to geology for the general reader and for beginners in the science*: New York, Ivison, Blakeman, Taylor, and Company, 263 p.

Dana, J.D., 1877, *New text-book of geology, designed for schools and academies*, 3rd edition: New York, Ivison, Blakeman and Taylor, 366 p.

Dana, J.D., 1875, *Manual of geology*, 2nd edition: New York, Ivison, Blakeman and Taylor, 828 p., 1 pl.

Dana, J.D., 1880, *Manual of geology*, 3rd edition: New York, Ivison, Blakeman and Taylor, 911 p., 13 pl.

Dana, J.D.,1883, *New text-book of geology, designed for schools and academies*, 4th editiion: New York, Ivison, Blakeman and Taylor, 412 p.

Dana, J.D., 1890, *The genesis of the heavens and the earth, and all the host of them*: Hartford, The Student Publishing Co., 70 p.

Dana, J.D., 1895, *Manual of geology*, 4th edition: New York, American Book Company, 1088 p.

Dana, J.D., 1897, *Revised text-book of geology*, 5th edition, Rice, W.N. editor: New York: American Book Company, 482 p.

Darwin, C., 1859, *On the origin of species by means of natural selection, or the preservation of favoured races in the struggle for life*. London, J. Murray, 502 p.

Dewey, J., 1972, Plate tectonics: *Scientific American*, v. 226, n. 5, p. 56-68

Dietz, R.S., 1961, Continent and ocean basin evolution by spreading of the sea floor: *Nature* 190, p. 854-857.

Dixon, T.H., and Mao, A., 1997, A GPS estimate of relative motion between north and South America: *Geophysical Research Letters*, v. 24, p. 535-538.

Donnelly, I., 1882, *Atlantis, the antediluvian world*: New York, Harper and Brothers, 490 p.

Dunbar C.O., 1949, *Historical geology*: New York, John Wiley and Sons., 567 p.

Dunbar, C.O., 1960, *Historical Geology* 2nd edition: New York, John Wiley and Sons, 500 p.

Dunbar, C.O. and Rodgers, J., 1957, *Principles of Stratigraphy*: New York, John Wiley and Sons, 356 p.

Dunbar, C.O. and Waage, K.M., 1969, *Historical Geology* 3rd edition: New York, John Wiley and Sons, 556 p.

Du Toit, A., 1921, The Carboniferous glaciation of South Africa: *Transactions of the Geological Society of South Africa*, v. 24, p. 188-217.

Du Toit, A., 1926, *The geology of South Africa*: Edinburgh: Oliver and Boyd Ltd., 463 p, 39 pl.

Du Toit, A., 1927, *Geological comparison of South America with Africa*: Carnegie Institution of Washington, Publication no. 381, 157 p, 17 pl.

Du Toit, A., 1937, *Our wandering continents, an hypothesis of continental drifting*: Edinburgh, Oliver and Boyd Ltd., 366 p.

Du Toit, A., 1944, Tertiary mammals and continental drift, a rejoinder to Geroge G. Simpson: *American Journal of Science* v. 242, p. 145-163.

Evans, J.W., 1923, The Wegener hypothesis of continental drift: *Science* v. 111, p. 393-394.

Fisher, O., 1889, *Physics of the Earth's Crust* 2nd edition: London, Macmillan and Co., 391 p.

Flint, R.F. and Skinner, B.J., 1974, *Physical Geology*: New York, John Wiley and Sons, 497 p.

Flint, R.F. and Skinner, B.J., 1977, *Physical Geology*, 2nd edition: John Wiley and Sons, 594 p.

Frankel, H., 1987, The continental drift debate, *in* Engelhardt, H.T., Jr., and Caplan, A.L. (eds.), *Scientific controversies, case studies in the resolution and closure of disputes in science and technology*: Cambridge, Cambridge University Press, 639 p.

Frankel, H., 2012, *The continental drift controversy: Wegener and the early debate*, v. 1: Cambridge, Cambridge University Press, 632 p.

Frankel, H., 2012, *The continental drift controversy: paleomagnetism and confirmation of drift*, v. 2: Cambridge, Cambridge University Press, 544 p.

Frankel, H., 2012, *The continental drift controversy: introduction of seafloor spreading*. v. 3: Cambridge, Cambridge University Press, 492 p.

Frankel, H., 2012, *The continental drift controversy: evolution into plate tectonics*, v. 4: Cambridge, Cambridge University Press, 796 p.

Gould, S.J., 1999, Darwin's more stately mansion: *Science*, v. 284, p. 2087.

Grabau, A.W., 1913, *Principles of Stratigraphy*: New York, A. G. Seiler, 1185 p.

Grabau, A.W., 1920, *A textbook of geology, Part I general geology*: Boston, D.C. Heath, 864 p.

Grabau, A.W., 1921, *A textbook of geology, Part II historical geology*: Boston, D.C. Heath, 976 p.

Grabau, A.W., 1924, Migration of geosynclines: *Bulletin of the Geological Society of China*, v. 3, no. 3-4, p. 207-349.

Grabau, A.W., 1936, *Palæozoic formations in the light of the pulsation theory, v. 1, Cambrovician pulsation, part. 1*: Peking, University Press, 680 p., 5 pl.

Grabau, A.W., 1936, *Palæozoic formations in the light of the pulsation theory, v. 2, Cambrovician pulsation, part. 1*: Peking, University Press, 751 p.

Grabau, A.W., 1937, *Palæozoic formations in the light of the pulsation theory, v. 3, Cambrovician pulsation, part. 2*: Peking, University Press, 850 p., 10 pl.

Grabau, A.W., 1938, *Palæozoic formations in the light of the pulsation theory, v. 4, Orodovician pulsation*: Peking, Henri Vetch, 942 p.

Grabau, A.W., 1940, *The rhythm of the ages, Earth history in the light of the pulsation and polar control theories*: Peking, Henri Vetch, 561 p, 25 pl.

Grabau, A.W., 1961, *The world we live in, a new interpretation of earth history*: Taipei, , Geological Society of China, 229 p.

Grabau, A.W. and Shimer, H.W., 1909, *North American index fossils, invertebrates, v. 1*: New York, A.G. Seiler, 853 p.

Grabau, A.W. and Shimer, H.W., 1910, *North American index fossils, invertebrates, v. 2*: New York, A.G. Seiler, 909 p.

Gregory, J.W., 1928, Wegener's hypothesis, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p. 93-96.

Guyot, A., 1984, *Creation, or the biblical cosmogony in the light of modern science*: New York, Charles Schribner's Sons, 136 p.

Hallam, A., 1973, *A revolution in the Earth Sciences from continental drift to plate tectonics*: Oxford, Clarendon Press, 127 p.

Hess, H.H., 1960, History of ocean basins, *in* Engel, A.E.J. et al., editors, *Petrologic studies, a volume in honor of A.F. Buddington*: Boulder, Colorado, Geological Society of America, p. 599-620.

Hoffmann, H., 1925, Moderne Probleme der Tiergeographie: *Die Naturwissenschaften*, v. 13, 5, p. 77-83.

Holmes, A., 1913, *The age of the Earth*: London, Harper and Brothers, 196 p.

Holmes, A., 1928, Radioactivity and continental drift: *Geological Magazine* v. 65, p. 236-238.

Holmes, A., 1930, Radioaktivität und Geologie: Basel, *Verhandlungen der Naturfoschenden Gesellschaft in Basel* Band XLI, p. 136-185.

Holmes, A., 1931, Radioactivity and Earth movements: *Transactions of the Geological Society of Glasgow*, v. 18. p. 559-606.

Holmes, A., 1933, The thermal history of the Earth: *Journal of the Washington Academy of Sciences*, v. 23, p. 169-195.

Holmes, A., 1944, *Principles of physical geology*: London, Thomas Nelson and Sons, 532p.

Holmes, A., 1953, The South Atlantic: land bridges or continental drift?, book review: *Nature*, v. 171, p. 669-671.

Holmes, A., 1965, *Principles of physical geology 2nd edition*: New York, Ronald Press Co., 1288 p.

Hopkins, E., 1844, *On the connexion of geology with terrestrial magnetism, showing the general polarity of matter, the meridional structure of the crystalline rocks, their transitions, movements and dislocations, including the sedimentary rocks, the laws regulating the distribution of metalliferous deposits, and other magnetic phenomena*: London, Richard and John Edward Taylor, 129 p., 24 pl.

Hopkins, E., 1851, *On the connexion of geology with terrestrial magnetism, 2nd edition*: London, Richard Taylor, 199 p, 30 pl.

Hull, E., 1861, *The coal-fields of Great Britain, their history, stucture and duration, with notices of the coal-fields of other parts of the world*: London, Edward Stanford, 277 p.

Irving, E., 1958, Rock magnetism, a new approach to the problems of polar wandering and continental drift, *in* Carey, S. W., editor, *Continental drift a symposium*: Hobart, University of Tasmania. p. 24-61.

Jeffreys, H., 1924, *The Earth, its origin, history and physical constitution*: Cambridge, Cambridge University Press, 278 p.

Joly, J., 1928, Continental movement, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p. 88-89.

Jukes, J. B. and Geikie, A., 1872, *The students manual of geology*: Edinburgh, A. and C. Black, 778 p.

Jukes-Browne, A.J., 1886, *The student's handbook of historical geology*: London, George Bell and Sons, 597 p.

Kay, M., editor, 1969, *North Atlantic geology and continental drift, a symposium*: Tulsa: American Association of Petroleum Geologists, 1082 p.

Keidel, J., 1916, La geología de las Sierras de la provincia de Buenos Aires y sus relaciones con las montañas de Sud África y los Andes: Buenos Aires, *Anales del Ministerio de Agricultura de la Nacion*, 78 p., 13 pl.

King, L., 1958, A new reconstruction of Laurasia, *in* Carey, S. W., editor, *Continental drift a symposium*: Hobart, University of Tasmania. p. 13-23.

Knopf, A., Brown, E.W., Holmes, A., Kovarik, A.F., Lane, A.C., and Schuchert, C., 1931, *Physics of the Earth, IV the age of the Earth*: Bulletin of the National Research Council, no. 80, 487 p.

Knowlton, 1919, Evolution of geologic climates: *Bulletin of the Geological Society of America*, p. 499-566.

Köppen, W. and Wegener, A., 1924, *Die Klimate der geologsichen Vorzeit*: Berlin, Verlag von Gebrüder Borntraeger, 256 p, 1 pl.

Krill, A., 2011a, *Fixists vs. mobilists in the geological contest of the century, 1844-1969*: Trondheim, published by author, 299 p.

Krill, A., 2011b, Is there really a North American Plate?: *American Geophysical Union, Fall Meeting 2011*, abstract #T23D-2449

Krill, A., 2011c, The chicanery of the isthmian links model: *Earth Sciences History*, v. 30, p. 200-215.

Lake, P., 1922, Wegener's displacement theory: *Geological Magazine*, p. 338-346.

Lake, P., 1923, Wegener's hypothesis of continental drift: *Science* 111, 226-228.

Lake, P., and Rastall, R.H., 1910, *A text-book of geology*: London: Edward Arnold, 481 p.

Lake, P., and Rastall, R.H., 1927, *A text-book of geology*: London: Edward Arnold, 520 p.

Lake, P., and Rastall, R.H., 1941, *Lake and Rastall's textbook of geology*: London: Edward Arnold, 491 p.

Le Conte, J., 1877, *Elements of geology, a text-book for colleges and for the general reader*: New York, D. Appleton, 588 p.

Le Conte, J., 1882, *Elements of geology, a text-book for colleges and for the general reader, revised edition*: New York, D. Appleton, 633 p.

Le Conte, J., 1891, *Elements of geology, a text-book for colleges and for the general reader, 3rd edition*: New York, D. Appleton, 640 p.

Le Conte, J., 1896, *Elements of geology, a text-book for colleges and for the general reader, 4th edition*: New York, D. Appleton, 670 p.

Le Conte, J. and Fairchild, H.L.R., 1903, *Elements of geology, a text-book for colleges and for the general reader, 5th edition*: New York, D. Appleton, 667 p.

Le Grand, H.B., 1988, *Drifting continents and shifting theories*: Cambridge, Cambridge University Press, 313 p.

Le Pichon, X. 1968. Sea floor spreading and continental drift. *Journal of Geophysical Research* 73, 3661-3697.

Lewis, C.L.E. 2002 Arthur Holmes, an ingenious geoscientist: *GSA Today*, March 2002 p. 16-17.

Longwell, C.R., 1928, Some physical tests of the displacement hypothesis, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p. 145-157.

Longwell, C.R., 1944, Some thoughts on the evidence for continental drift: *American Journal of Science*, v. 242, p. 218-231.

Longwell, C.R. 1958a. My estimate of the continental drift concept, *in* Carey, S. W. editor, *Continental drift a symposium*: Hobart, University of Tasmania, p. 1-12.

Longwell, C.R. 1958b. Epilogue, *in* Carey, S. W. editor, *Continental drift a symposium*: Hobart, University of Tasmania, p. 356-358.

Longwell, C.R., editor, 1929, *A textbook of geology, part 1, physical geology*: John Wiley and Sons, 488 p.

Longwell, C.R., editor, 1932, *A textbook of geology, part 1, physical geology*: John Wiley and Sons, 514 p.

Longwell, C.R, and Flint, R.F., 1955, *Physical geology*. New York: John Wiley and Sons, 432 p.

Longwell, C. R, and Flint, R.F., 1962, *Physical geology*. New York: John Wiley and Sons, 504 p.

Longwell, C.R, Knopf, A., and Flint, R.F., 1934, *Outlines of physical geology*: New York, John Wiley and Sons, 356 p.

Longwell, C.R, Knopf, A., and Flint, R.F., 1939, *A textbook of peology, part I, physical geology*: New York, John Wiley and Sons, 543 p.

Longwell, C. R, and Flint, R.F., and Sanders, J.E., 1969, *Physical geology*: New York, John Wiley and Sons, 685 p.

Lyell, C.,1837, *Principles of geology, being an inquiry how far the former changes of the Earth's surface are referable to causes now in operation*, 5th edition, v. 1: London, John Murray, 462 p.

Lyell, C., 1845, *Travels in North America in the years 1841-2 with geological observations on the United States, Canada, and Nova Scotia*: New York, Wiley and Putnam, 231 p.

Lyell, C., 1857, *A manual of elementary geology: or, the ancient changes of the Earth and its inhabitants as illustrated by geological monuments*: New York, D. Appleton and Co., 685 p.

Marcou, J., 1860, *Lettres sur les roches du Jura et leur distribution géographique dans les deux hémisphères*: Paris, Friedrich Klincksieck, 364 p., 2 pl.

Marvin, U., 1973, *Continental drift, the evolution of a concept*: Washington D.C., Smithsonian Institution Press, 239 p.

Mazur, A., 2004, *A romance in natural history, the lives and works of Amadeus Grabau and Mary Antin*: Syracuse: Garret. 484 p.

Molengraaff, G.A.F., 1916, The coral reef problem and isostasy: *Proceedings of the Royal Academy of Amsterdam*, v. 19, p. 610-627.

Molengraaff, G.A.F., 1928, Wegener's continental drift, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p. 90-92.

Morgan, W.J., 1968, Rises, trenches, great faults, and crustal blocks: *Journal of Geophysical Research* v. 73, p 1959-1982.

Müller, R.D, and Smith, W.H.F., 1993, Deformation of the oceanic crust between the North American and South American Plates: *Journal of Geophysical Research, Solid Earth*, v. 98, p. 8275-8291.

Neumayr, M., 1887. *Erdgeschichte, v. 2*: Leipzig, Verlag des Bibliographischen Instituts, 880 p.

Nield, T., 2007, *Supercontinent, ten billion years in the life of our planet:* Cambridge, Massachusetts, Harvard University Press, 288 p.

Oreskes, N., 1999, *The rejection of continental drift, theory and method in American Earth sciences*: Oxford, Oxford University Press, 420 p.

Pepper, J.H., 1860, *The boy's playbook of science, including the various manipulations and arrangements of chemical and philosophical apparatus required for the successful performance of scientific experiments*: London, Routledge, Warne and Routledge, 440 p.

Pepper, J.H.,1861, *The playbook of metals, including personal narratives of visits to coal, lead, copper, and tin mines; with a large number of interesting experiments relating to alchemy and the chemistry of the fifty metallic elements*: London, Routledge, Warne and Routledge, 504 p.

Pickering, W. H., 1907, The place of origin of the moon, the volcanic problem: *Journal of Geology*, v. 15, p. 23-38.

Pirsson, L.,1915, *A text-book of geology, part 1 physical geology*: New York, John Wiley and Sons, 444 p.

Pirsson, L.,1920, *A text-book of geology, part 1 physical geology*, 2nd edition: New York, John Wiley and Sons, 471 p., 1 pl.

Press, F., and Siever, R., 1974, *Earth*: San Francisco, W.H. Freeman, 945 p.

Rastall, R.H., 1929, On continental drift and cognate subjects: *Geological Magazine*, v. 66, p. 447-456.

Rice, W.A., 1938, Our wandering continents by Alex L. Du Toit: *American Journal of Science*, v. 36, p. 391-393.

Rodgers, J., 2001, *The company I kept, autobiography of a geologist*: New Haven, Connecticut Academy of Arts and Sciences, 223 p.

Rothé, J.P., 1954, La zone seismique mediane indo-atlantique: *Proceedings of the Royal Society, A*, v. 222, n. 1150, p. 397-397.

Sanders, J.E., Carola, R., Anderson, A.H.,1976, *Physical geology*: New York, Harper and Row, 584 p.

Schuchert, C., 1910, Paleogeography of North America: *Bulletin of the Geological Society of America*, v. 20, p. 427-606, 56 pl.

Schuchert, C., 1915, *A text-book of geology, part 2, historical geology*: New York, John Wiley and Sons, p. 405-1026.

Schuchert, C., 1924, *A text-book of Geology, part 2, historical geology*: New York, John Wiley and Sons, 724 p.

Schuchert, C., 1928a, The hypothesis of continental displacement, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p. 104-144.

Schuchert, C., 1928b, The continental displacement hypothesis as viewed by Du Toit: *American Journal of Science*, v. 16, p. 266-274.

Schuchert, C., 1932, Gondwana land bridges: *Geological Society of America Bulletin*, v. 43, p. 875-915.

Schuchert, C. and Dunbar, C.O., 1933, *A textbook of geology, part 2, historical geology*, 3rd edition: New York, John Wiley and Sons, 551 p.

Schuchert, C. and Dunbar, C.O., 1941, *A textbook of geology, part 2, historical geology*, 4th edition: New York, John Wiley and Sons, 544 p.

Sclater, P.L., 1864, The mammals of Madagascar: *Quarterly Journal of Science*, v. 1, p. 213-219.

Shand, S.J., 1933, *Earth-lore, geology without jargon*: London, Thomas Murby and Co., 134 p.

Shand, S.J., 1937, *Earth-lore, geology without jargon, 2nd edition*: London, Thomas Murby and Co., 144 p.

Simpson, G.G., 1943, Mammals and the nature of continents: *American Journal of Science*, v. 241, p. 1-31.

Singewald, J.T., Jr., 1928, Discussion of Wegener theory, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p. 189-193.

Snider-Pellegrini, A. (A. Snider), 1858, *La création et ses mystères dévoilés*: Paris, Librairie A. Franck, 487 p.

Stewart, J.A., 1990, Drift*ing continents and colliding paradigms, perspectives on the geoscience revolution*: Bloomington, Indiana University Press, 285 p.

Van der Gracht, W.A.J.M. van Waterschoot, 1928, Introduction: the problem of continental drift, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p. 1-75.

Van der Gracht, W.A.J.M. van Waterschoot, 1928, Remarks regarding the papers offered by the other contributors to the symposium, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p. 197-226.

Vine, F.J., and Mathews, D., 1963, Magnetic anomalies over oceanic ridges: *Nature*, v. 199, p. 947-949.

Wegener, A.,1912a, Die Entstehung der Kontinente: *Petermanns Geographische Mitteilungen*, v. 58-1, p. 185-195, p. 253-256, p. 305-309.

Wegener, A., 1912b, Die Entstehung der Kontinente: *Geologische Rundschau*, v. 3, p. 276-292.

Wegener, A., 1915, *Die Entstehung der Kontinente und Ozeane*: Braunschweig, Friedr. Vieweg and Sohn, 94 p.

Wegener, A. 1920, *Die Entstehung der Kontinente und Ozeane, 2nd edition*: Braunschweig, Friedr. Vieweg and Sohn, 135 p.

Wegener, A., 1921, *Die Entstehung der Mondkrater*: Braunschweig, Friedr. Vieweg and Sohn, 48 p.

Wegener, A., 1922, *Die Entstehung der Kontinente und Ozeane, 3rd edition*: Braunschweig: Friedr. Vieweg and Sohn, 144 p.

Wegener, A., 1924, *The origin of continents and oceans*. Translation of 3rd ed. by Skerl, J.G.A.: London, Methuen, 212 p.

Wegener, A. 1926. Paläogeographische Darstellung der Theorie der Kontinentalverschiebungen, *in* Kende, O., editor *Enzyklopadie der Erdkunde 4*: Leipzig, Franz Deuticke, p. 171-189.

Wegener, A., 1928, Two notes concerning my theory of continental drift, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p. 97-103.

Wegener, A., 1929, *Die Entstehung der Kontinente und Ozeane 4th edition*: Braunschweig, Friedr. Vieweg and Sohn, 231 p.

Wegener, A., 1936. *Die Entstehung der Kontinente und Ozeane 5th edition*: Braunschweig, Friedr. Vieweg and Sohn, 242 p.

Wegener, A., 1966, *The origin of continents and oceans, translation of 4th edition* by Biram, J.: New York, Dover, 246 p.

Wegener, A. and Koch, J.P., 1919, Du*rch die weiße Wüste. die dänische Forschungsreise quer durch Nordgrönland 1912-13*: Berlin, Julius Springer, 248 p..

White, D., 1928, Discussion of floating continents, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p. 187-188.

Willis, B., 1910, Principles of paleogeography: *Science*, v. 31, p. 241-260.

Willis, B. and Salisbury, R.D., 1910, *Outlines of geological history with especial reference to North America*: Chicago, The University of Chicago Press, 306 p.

Willis, B., 1923, *Geologic structures*: New York, McGraw-Hill, 295 p.

Willis, B., 1928, Continental drift, *in* Van der Gracht, W.A.J.M. van Waterschoot, editor, *The theory of continental drift: a symposium*: Tulsa, American Association of Petroleum Geologists, p. 76-82.

Willis, B., 1929, *Geologic structures, 2nd edition*: New York, McGraw-Hill, 518 p.

Willis, B., 1932, Isthmian links: *Geological Society of America Bulletin* v. 43, p. 917-952.

Willis, B., 1944, Continental drift, ein Märchen: *American Journal of Science* v. 242, p. 509.

Willis, B. and Willis, R., 1934, *Geologic structures, 3rd edition*: New York, McGraw-Hill, 544 p.

Wilson, J.T., 1968, Static or mobile Earth, the current scientific revolution: *Proceedings of the American Philosophical Society*, v. 112, n. 5, p. 309-320.

Wood, R. M., 1985, *The dark side of the Earth, the battle for the Earth sciences*: London, Allen and Unwin, 246 p.

Wright, W.B., 1923, The Wegener hypothesis, discussion at the British Association: *Nature*, Jan. 6., p. 30-31.

13. About the Author and Acknowledgements

Allan Krill has had the luck of the draw, including one of the best geologic educations a person could hope for. His B.S. degree (Earth Science in 1976) was from the University of California, Santa Cruz, an unorthodox new university with a young faculty that was fully involved in the plate-tectonic revolution. His Ph.D. degree (Geology and Geophysics in 1980) was from Yale University, under the supervision of the benevolent grand old man John Rodgers. He married Alison Sippel, a Yale English major and visual artist, and they promptly emigrated to Norway, where they enjoy the stimulations of nonnative culture, language, and politics.

Krill worked for six years as a research geologist with the Geologic Survey of Norway, producing many geologic maps and papers. The papers included some controversial alternative interpretations of various aspects of Norwegian regional geology, most of which are currently considered correct. He served several years as an editor of the Norwegian Journal of Geology. For the past 26 years he has been a professor of geology at the Norwegian University of Science and Technology (NTNU). His was hired with Norwegian tenure, signed by the King of Norway, which gives him a remarkable degree of academic freedom. He makes a habit of expressing unorthodox and sometimes unpopular views.

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