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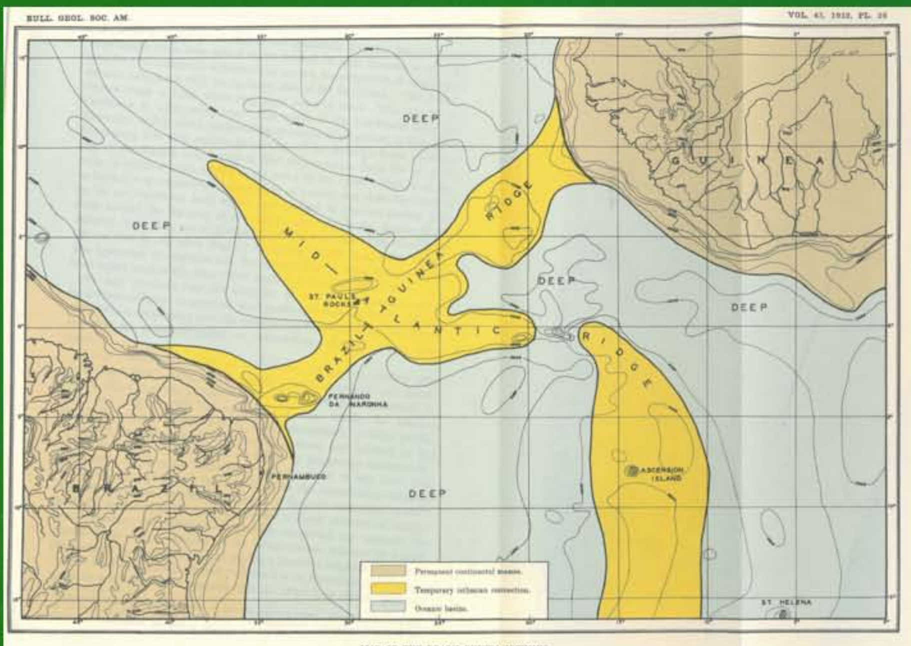

MAP OF THE BRAZIL-GUINEA ISTHMUS

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Front-cover image: The Brazil–Guinea isthmian link, according to Bailey Willis, 1932.

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EDITOR'S INTRODUCTION

DAVID R. OLDROYD

The paper by **Claudia Schweizer** and **Johannes Seidl** discusses the holistic views of Ami Boué concerning the Earth and geological studies of the planet. Readers may recall the words of Archibald Geikie in his *Founders of Geology* (1895):

> Werner's mineralogy embraced the whole of Nature, the whole of human history, the whole interests and pursuits and tendencies of mankind. . . . He would contrast the mountainous scenery of the granites and schists with the tamer landscapes of the sandstones and limestones. Tracing the limits of these contrasts over the surface of the area of Europe, he would dwell on their influence upon the grouping and characteristics of nations. He would show how the development of the arts and industries of life had been guided by the distribution of minerals, how campaigns, battles, and military strategy as a whole, had been dependent on the same cause. The artist, the politician, the historian, the physician, the warrior were all taught that a knowledge of mineralogy would help them to success in their several pursuits . . .
>
> By such continual excursions into domains that might have been thought remote enough from the study of minerals, and by the clear and confident method, playful vivacity and persuasive eloquence with which they were conducted, Werner roused his hearers to a high pitch of enthusiasm. No teacher of geological science either before or since has approached Werner in the extent of his personal influence, or in the breadth of his contemporary fame.

Ami Boué was, via Robert Jameson in Edinburgh, a product of the Wernerian tradition. And while the direct ideas of Werner himself are rather little known (at least to Anglophones), since they mostly lie hidden in his surviving lecture notes in Freiberg, with the help of Schweizer and Seidl, we can see—from an analysis of the cosmopolitan Boué's work—what a Wernerian approach to 'geognostic' studies might look like in practice. Boué's holism even extended to an attempt to provide a geological map of the whole Earth (in 1843). Schweizer and Seidl reproduce two versions of this rather little-known cartographic accomplishment and discuss how the map was compiled.

It is well known that for many years the parallelism of organisms on opposite sides of oceans was explained by the former existence of land bridges or 'isthmian links', spanning what are now large stretches of water. This theory was proposed by Charles Schuchert and Bailey Willis in an attempt to explain phenomena that were also explicable in terms of Wegener's 'drift' hypothesis. **Allan Krill** has discussed the isthmian links theory in a recent e-book that he has privately published, and claimed that Schuchert and Willis deliberately 'fudged' their data and their evidence. I was so taken by his arguments that I invited him to give them in greater detail in the paper that is offered here. If Krill's arguments are accepted, they would go a long way towards accounting for geologists' reluctance to adopt Wegener's hypothesis in the light of what seemed a satisfactory alternative advocated by two eminent geologists. So the isthmian links model was regularly taught in geology classes up to the 1950s and early '60s.

Debra Lindsay provides a paper on the history of palaeobotany in North America, a topic on which she has written previously in *Earth Sciences History*. She focuses on debates between Othniel Marsh and Lester Frank Ward, which concerned the stratigraphic position of the 'Potomac Formation' in Virginia, for which arguments could be found for assigning it to either the Jurassic or the Cretaceous. Marsh, using animal fossils, favoured a Jurassic determination. His junior, Ward, favoured the Cretaceous, using palaeobotanical evidence. At the time, the general opinion sided with the more influential Marsh, but Lindsay shows that in the longer run Ward's arguments prevailed. I had previously encountered Ward when teaching a course on the history of Darwinism, where he figured as an exponent of the 'liberal' version of Social Darwinism. It was gratifying to know, then, that he had sound scientific accomplishments preceding his later sociological work!

Turning northwards, we have a paper by **Howard Plotkin** and **Kimberly Tait** about studies of a circular structure up in the Canadian subarctic, which was thought by the prospector F. W. Chubb to have had a volcanic origin and might be a source of diamonds from a diatreme. Subsequently, it was interpreted by V. Ben Meen, of the Royal Ontario Museum in Toronto as an impact crater, even though no meteoritic materials were located. Later fieldwork proved Meen correct and led to the discovery of other meteorite craters on the Canadian Shield and the development of criteria by which they could be authenticated. The 'Chubb Crater' was found to have a depth and diameter that fitted well on the so-called Baldwin Curve, and supported the relationship between the meteoritic origin of lunar craters and terrestrial impact structures. The controversy over the crater's origin is reminiscent of debates about the Meteor Crater in Arizona and the Riess Crater in Germany, but was not so heated!

Often referred to by Anglophones, but rarely read, are two letters from Giovanni Arduino to Antonio Vallisnieri Jr (published in 1760) about certain strata in the pre-Alps of Veneto, in which a four-fold division of strata was suggested for the region (Primary/Primitive; Secondary; Tertiary; and 'Quaternary'[1]). These two letters are presented in *Earth Sciences History* in fluent translations by **Theodore Ell**, the first appearing in the present issue while the second will be published in 2012. Ell describes the circumstances in which the letters were composed and published and gives an idea of Arduino's character and work habits. The first letter was chiefly about his fieldwork in Valdagno, near Recoaro, and suggests that he was initially approaching his studies from the perspective of the study of discrete rocks and minerals, caves and mineral waters, and via chemical analysis (which was insufficiently developed to provide much stratigraphic insight). The fourfold stratigraphic subdivision, with the suggestion that it had a wider application than Veneto, appeared in the second letter. However, in 1758, Arduino had prepared a manuscript profile of the strata of Valdagno, which foreshadowed the fourfold division. This well-known diagram is reproduced here, together with a translation of its difficult-to-decipher text below. Ell's paper is usefully complemented by one of our book reviews, in which **Gian Battista Vai** discusses in some detail a recent edition of Arduino's correspondence, edited by Ezio Vaccari. This review adds further information about Arduino's theoretical views and terminology.

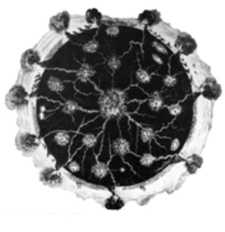
This issue is completed by a sympathetic obituary by **Kenneth Taylor** of Alexander M. Ospovat, who was for long the major authority on the ideas of Abraham Werner in the Anglophone world; and by seven book reviews.

[1] The term 'Quaternary' was not, *per se*, introduced by Arduino himself.

THE CHICANERY OF THE ISTHMIAN LINKS MODEL

ALLAN KRILL

Department of Geology and Mineral Resources Engineering
Norwegian University of Science and Technology (NTNU)
7491 Trondheim, Norway.
allan.krill@ntnu.no



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ABSTRACT

Two papers, 'Gondwana land bridges' by Charles Schuchert and 'Isthmian links' by Bailey Willis, were published together in 1932. They were apparently motivated by Schuchert's desire to defend his paleogeography of fixed continents against the threat of Alfred Wegener's continental mobilism. Schuchert and Willis both held to land-bridge theory but admitted that they could not accept each other's types of bridges. Schuchert insisted that some bridges had to be wide and of continental material, without explaining why he felt this was so. Willis insisted that wide continental bridges were isostatically and volumetrically impossible; so any ancient bridges that had sunk must have been narrow isthmuses of dense oceanic rocks. They wrote separate papers, but issued together, perhaps to lead readers to the impression that a compromise was possible; but it was not. They avoided alerting readers to fatal flaws in both their models, in part by limiting their discussion to the less familiar southern hemisphere (Gondwana) and never mentioning the continental connection between Europe and North America. Willis went further in his inventions than Schuchert, trying to explain the extremes of Permian climate. Fixed-continent paleogeography required glacial conditions at equatorial latitudes and tropical conditions at arctic latitudes. We now understand that these climate differences can only be explained by 'continental drift' (or plate tectonics), but in his valiant effort to support fixism, Willis postulated not only tectonic uplifts of oceanic isthmuses, but also uplifts in continental areas that were known to be stable.

1. INTRODUCTION

Two important and influential papers, 'Gondwana land bridges' and 'Isthmian links', were published together in the *Bulletin of the Geological Society of America* in 1932. The idea they promoted, that permanently fixed continents had earlier been connected by narrow land bridges, was instrumental in maintaining the American consensus against the theory of continental drift.

The authors, Charles Schuchert (1858–1942) and Bailey Willis (1857–1949), are familiar to nearly everyone interested in the history of geology. Schuchert was 74 years old in 1932, and still fully active as Professor Emeritus at Yale University. Willis, 75, was Professor Emeritus at Stanford. Both had earned the highest honors that could be awarded to North American geologists. They were members of the National Academy of Sciences and each had been chosen to serve a year as President of the Geological Society of America. In addition, Schuchert had been elected President of the Paleontological Society and Willis President of the Seismological Society.

These and many other honors were well deserved, and it is unfortunate that in this paper I am only going to point out some 'shenanigans' toward the ends of their careers. I do so because it is important for modern geologists to be reminded that even the best scientists are only human, that scientific papers can mislead, and that scientific consensus is not necessarily correct.

Although the two papers were highly influential it seems that they were not very critically read. Their main purpose, I shall endeavour to show, was to impede acceptance of the theory of continental drift, and for this, the bold titles and the prestige of the authors and the journal proved sufficient in many quarters. Only the most prestigious authors could have had these papers published. They were illustrated with six expensive folding map-plates, five of which were in color. But the plates contained no real data or research results, and essentially were only rough and inaccurate sketches on inappropriate base maps. Each of the maps could just as well have been printed with no loss of information as single-page black-and-white figures. But the color served the rhetoric of the papers.

2. WHY CONTINENTAL DRIFT WAS DISLIKED BY AMERICAN GEOLOGISTS

Schuchert initiated the joint project in April 1931, asking Willis to help him show how land bridges might have formed and then sunk to become deep ocean floor. The model of 'transient' land bridges connecting fixed continents seemed to be the best alternative to Alfred Wegener's (1880–1930) theory of continental drift.

Wegener's theory was unpopular in America. Schuchert (1928), Willis (1928) and several other American geologists had severely criticized continental drift in an American symposium volume (Van Waterschoot van der Gracht 1928), but an underlying reason for its unpopularity was not exposed there. A subtle, but significant problem with continental drift was that it disallowed the special geologic status of the North American continent. According to Wegener, North America was just another continental fragment that had broken away from Pangaea. But American geologists had long been taught that continents were individual entities, and that North America was the 'ideal'. James Dwight Dana (1813–1895) had emphasized this from 1863 to 1895 in the principal American textbook *Manual of Geology* (Dana 1895, p. 3 and p. 1028). Schuchert adopted the idea in the subsequent textbook *Historical Geology*, editions of which were used by large numbers of American students and geologists for some fifty years. He wrote:

> 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 (Schuchert 1915a, p. 576).

Willis had also followed Dana's principle of permanent continents surrounded by permanent oceans, and reformulated it as his own principle, for which he used italics: "*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*" (Willis 1910, p. 243). Wegener repeatedly scoffed at this principle and Willis's apparent rigidity (Wegener 1912, p. 187; 1915, p. 5; 1924, p. 26).

From study of the literature, I have noticed that Schuchert was the most active and influential opponent of Wegener's theory and I am convinced that he felt personally threatened by the model of drifting continents. He was one of the world's leading paleogeographers and was later referred to as the "foremost paleogeographer of our time" by the editor of a posthumous edition of one of his works (Schuchert 1955, p. iv). Schuchert had written a definitive monograph *The Paleogeography of North America* (Schuchert 1910). In his textbook and in many other publications, he used biogeographic principles and the idea of fixed continents to interpret the paleogeography of the world. This included global distributions of ancient marine animals and terrestrial plants and animals, as well as climates during all the periods (Schuchert 1915b). These were detailed interpretations that no other American geologist was capable of making or dared to try.

But paleogeography with fixed continents was a daunting puzzle. The geography of the past was clearly not the same as that of the present. At various times in geologic history, animals and plants had seemingly dispersed in remarkable ways, but had been restricted in other ways, both on the land areas, in fresh-water river systems, and in the oceans. These biogeographic patterns required complicated paleogeographic models.

Ancient climates were even more problematic. From the Permo-Carboniferous, for example, there were relics of large glacial ice sheets in seemingly inexplicable places on the southern continents and even near the equator at sea level. The ice sheets had apparently flowed in strange directions, such as toward the land, away from present coastlines; and there must have been land where there is now deep ocean. Some ice sheets seemingly moved away from the equator, as if it were coldest there. Some apparently moved toward the South Pole, from whence the ice should have been coming. At the same time as these ice sheets existed in the southern hemisphere, Greenland and other northern areas had apparently been ice-free. It seemed that conditions there were tropical, as indicated by coal fossils and fossil corals.

To try to explain the distribution of fossils, European paleogeographers had found it necessary to postulate huge east–west continents; and Schuchert adopted such explanations, developing and promoting them in scientific publications and in his textbook (see, for example, Figure 1). In the northern hemisphere there were the continents Eria, Baltica, and Angara. In the south were Amazonia and Gondwana. The latter included parts of South America, Africa, India, and Australia. The Atlantic Ocean did not yet exist, but instead there were supposedly smaller oceans, 'Poseidon' and 'Tethys', in the north and 'Nereis' in the south. Parts of these huge continents—the land-bridge parts—were, however, supposedly somehow less permanent than the rest and sank at the end of the Mesozoic to form parts of the Atlantic and Indian Oceans. Many geologists doubted that continental crust was capable of sinking in such a manner but Schuchert and previous European paleogeographers, especially Eduard Suess (1831–1914), had argued strongly that it must have sunk.

According to continental drift theory, the continents had moved laterally. Drift theory would abolish Schuchert's ancient continents and oceans, and the detailed paleogeographic models that he so proficiently employed. By 1923 Schuchert had begun to realize that the ground rules of 'drift paleogeography' were altogether different from his own. One could say that mobilism was a different game than traditional fixism. But Schuchert was a fixist and he would continue to play the fixist game, even if he had to break some rules himself. Schuchert probably didn't see it as a kind of game, but I think Willis may have done so (Krill 2011). He liked a geologic challenge, and accepted Schuchert's invitation to meet the nearly impossible challenges of fixist paleogeography.

To summarize here, I see two main reasons for the American rejection of drift theory: the geological status and presumed fixed position of the North American continent, and the geological status and manifest fixed position of Charles Schuchert. I suggest that geohistorians have missed these two reasons because they have not taken into account sufficiently the content and influence of geology textbooks. For an otherwise excellent historical account, including discussion of the isthmian links papers, read *The Rejection of Continental Drift* (Oreskes 1999). But textbooks receive little attention in this book.

My two main reasons might seem to ignore the commonly held idea that continental drift was rejected for lack of a suitable mechanism. This misconception was firmly established by another leading textbook (Longwell, Flint and Sanders 1969, p. 553). But it was not the main reason: Arthur Holmes (1890–1965) had proposed a convection-current mechanism and explained it to Schuchert in 1927 (see Oreskes 1999, pp. 119, 193), and then published it (Holmes 1928, 1930, 1931). Schuchert thus knew that there was a possible mechanism for continental drift and never used the mechanism-argument against it.

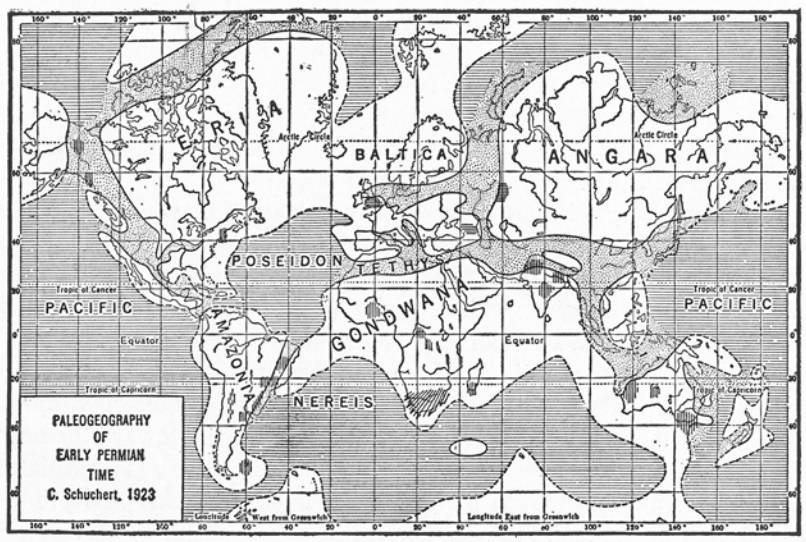

Fig. 168. — Map showing the distribution of Permian glaciation. The limits of Gondwana Land are problematical.

Figure 1. Schuchert's paleogeographic map of Permian lands and glaciations, redrawn for the 1924 edition of his textbook. It was used again for the 1933 edition, with the new caveat added: "The limits of Gondwana Land are problematical". From Schuchert and Dunbar (1933, p. 283).

3. TWO PAPERS INSTEAD OF ONE

Schuchert's 'Gondwana land bridges' and Willis's 'Isthmian links' were different papers though they were issued as a joint reprint with a single cover. I have two copies of this reprint, one signed by Schuchert and the other by Willis.

Schuchert and Willis would have liked to write a single paper as co-authors. But this, it seems, they could not do. As Schuchert put it, their views were "somewhat divergent" (Schuchert 1932, p. 877). Willis wrote that they were "not of one understanding" (Willis 1932, p. 919). And these were understatements: in fact they saw fatal flaws in each other's papers (and also in their own). They remained separate authors because it was enough for each to take responsibility for his own chicanery without also being responsible for the other's. Although a few critical readers of these papers must have seen the flaws, the eminent authors counted on supportive readers to overlook the problems and see the two papers, taken together, as a viable alternative to Wegener's continental drift.

Schuchert and Willis knew that Wegener's drift theory could solve the paleogeographic problems. But they had decided to disallow Wegener's theory, each dismissing it in a single sentence.

> 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 1932, p. 930).

> 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) (Schuchert 1932, p. 878).

Citing Hoffmann's paper was, I believe, a bluff. Few of Schuchert's North American readers would ever see that German-language article. Had they done so, they would have found that Hoffmann mentioned continental drift only in passing. He distinguished between three conflicting theories: permanence theory, bridge theory, and drift theory. Hoffmann preferred bridge theory, but he admitted that most of the evidence also fitted the drift theory and he concluded that geologists and geo-physicists—not paleontologists like himself—would have to decide whether drift theory was feasible.

It should be remarked that Schuchert was advocating bridge theory whereas Willis kept closer to permanence theory. It is important to understand the differences. Permanence theory held that continental crust is permanently buoyant whereas oceanic crust is permanently dense. Because of isostasy, continental crust could never sink to great ocean depths. Ocean-volume considerations also supported permanence theory. If continents had existed and then foundered, the volume of the ocean basins would have increased dramatically. Sea levels have fluctuated throughout geologic time, but there has never been a drop on the scale suggested by this model. Wegener's theory, it may be noted, elegantly avoided such problems.

Willis's contribution was to postulate oceanic, not continental bridges. He proposed that tectonic forces could elevate narrow oceanic bridges for a time; and afterwards they could sink isostatically to their previous deep levels. Also his bridges were only narrow isthmuses; so the volume of the ocean basins would not have increased significantly when they sank.

Schuchert and Willis could compromise somewhat, but not enough to agree. Schuchert wrote:

> The writer has long been endorsing the principle of permanency of continents and oceans, but not at all in the rigid form propounded by Dana, because he is convinced that even though continents and oceans do not interchange their relative levels, nevertheless great parts of the present continents have been broken down and sunk into great depths. He further holds that land bridges existed during Paleozoic and Mesozoic time in the Atlantic between Brazil and Africa, and between Africa and India, but in the Pacific he sees no possibility for land bridges outside of Australasia and possibly in Melanesia. Therefore geologists must find a way to sink into oceanic depths such former land bridges as western Gondwana and Lemuria (Schuchert 1932, p. 880).

Here the name Lemuria referred to the supposed continental land bridge between Africa and India. Note that Schuchert mentioned several different bridges, but not the one between Europe and North America (Eria–Baltica, in Figure 1). As I shall point out below, this was an intentional omission.

In his conclusions, Schuchert wrote:

> The writer's colleague, Professor Willis, feels that the previous pages still show the influence of the Suess or continental type of transoceanic land bridge, and that the writer has not expressed the idea of isthmian links as defined by him. He says that if the land bridges are of continental character, they should be of continental rocks and structure, which has been the writer's conception of them (Schuchert 1932, p. 887).

Schuchert never explained in this paper why he believed that the bridges were wide and that they were of continental rocks and structure. It must have seemed to the uncritical reader that there was no compelling reason for Schuchert's version, and that his desire for wide continental bridges could be ignored in favor of Willis's narrow oceanic isthmuses. That was the intention of the two papers. But it was a ruse. The hypothesized continental connections must have been wide and of continental material, and both Schuchert and Willis knew this. The geological evidence was clearest between Europe and North America. Schuchert had called that bridge Eria. However, both Schuchert and Willis limited their discussions in these papers to southern land bridges. Never once did either of them mention any land bridge between Europe and North America.

In previous publications and in previous editions of his textbook, Schuchert had explained the evidence that these bridges were wide and continental. In the next revised edition of his textbook (1933) he explained that narrow isthmian bridges were possible for Gondwana, but not for Eria. But rather than show an updated map with narrower bridges, Schuchert simply reused the map drawn for his 1924 edition with a caveat added for the southern hemisphere (see Figure 1). For Eria, arguing somewhat in a circle as far as the paleontological evidence was concerned, he wrote:

> 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 (Schuchert and Dunbar 1933, pp. 209–210).

Here we see why Schuchert and Willis would not mention any North Atlantic land bridge in their 'co-publication'. That bridge must have been continental crust because a continuous mountain range had been broken off, and because continental sandstones were eroding from it. Moreover, it must have been wide, so that Devonian fresh-water fish could migrate from one continent to the other. If it had been an isthmus, no rivers would have run the length of it, and no fish could have used it. The geology of these areas was familiar to American and European readers of these papers, but could be overlooked. Schuchert and Willis helped readers overlook the evidence by never mentioning this bridge. In fact, Schuchert and Willis both knew of similar evidence for the Gondwana connection. There, one also found a broken-off fold belt and continental sediments. Willis even alluded to them (Willis 1932, p. 933). But this evidence was less impressive, and unfamiliar to American and European readers.

4. SUITABLY INAPPROPRIATE WORLD MAPS

Schuchert and Willis each printed his own map of the world for Permian time (see Figures 2 and 3). Schuchert mentioned that he had drawn the land bridges as narrow as possible on his map, and sent it to Willis on the opposite coast of America. Willis reduced Schuchert's bridges even further and sent the map back (Schuchert 1932, p. 877). Schuchert probably had to accept these changes to his map to keep Willis onside in their venture.

Consider now the base map that they used. It showed no submarine features, not even the continental shelves or the mid-ocean ridges. Submarine features should have been emphasized for these papers, because both authors accepted that shelves were submerged continental crust, and that ancient land bridges should still be detectable at depth. Considering the nature of the case that Schuchert and Willis were trying to argue, it is remarkable that a map that lacked submarine features was accepted by the journal editors.

The map that Schuchert and Willis used was Goode's projection, not the standard Mercator projection that Schuchert had used in his textbook. All map projections must distort some features, but Goode's projection maintained the scale of land areas, while splitting and distorting the oceans. The northern Atlantic Ocean is so badly split that Iceland is shown twice, and so distorted that specific features there might seem irrelevant. Both Schuchert and Willis showed a land bridge at Iceland, but it was easy for readers to overlook. It was not as wide as the Eria that Schuchert described in his textbooks and was drawn in such a way as to be ignored, while at the same time attention was drawn towards the southern hemisphere.

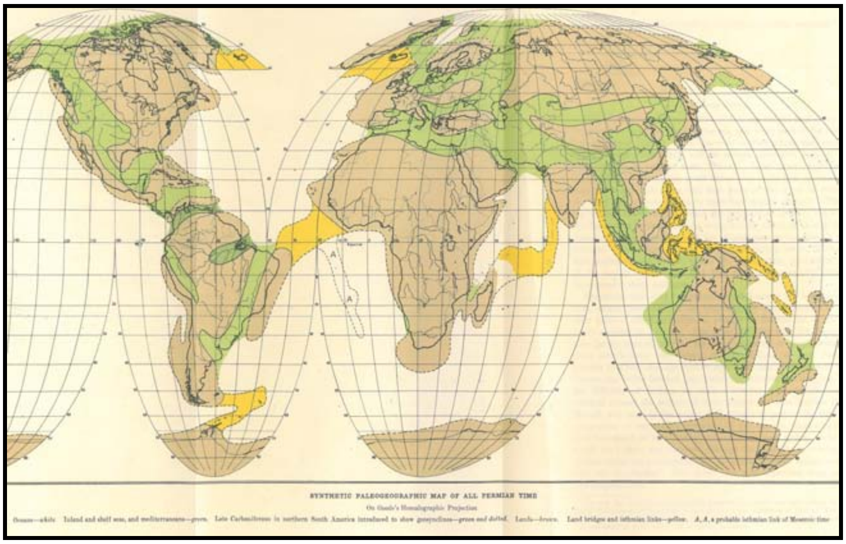

Figure 2. Schuchert (1932), Plate 24: 'Synthetic paleogeographic map of all Permian time on Goode's homalographic projection'. Colour keys: Green-Inland and shelf seas and mediterraneans; Green and dotted-Late Carboniferous in northern South America; Brown-lands; Yellow-Land bridges and isthmian links.

In fact, for the southern Atlantic Ocean, the particular distortions of Goode's projection just suited Schuchert and Willis's needs. The Atlantic appears narrowest between Africa and South America, right where the authors wanted to convince readers of the presence of the sunken Gondwana land bridge. Moreover, the map split the South Atlantic and hid the similarity of the coastlines on either side. With the split, the Mid-Atlantic Ridge, where they chose to show it, does not appear in the center: it seems much closer to Africa than to South America. Willis could even connect it to Africa. Imagine how foolish it would have looked to use the standard Mercator projection with the matching coastlines and the Mid-Atlantic Ridge perfectly centered, and then ignore the ridge in placing a Gondwana land bridge across it! Scientists know how to present their data in ways that help support their interpretations.

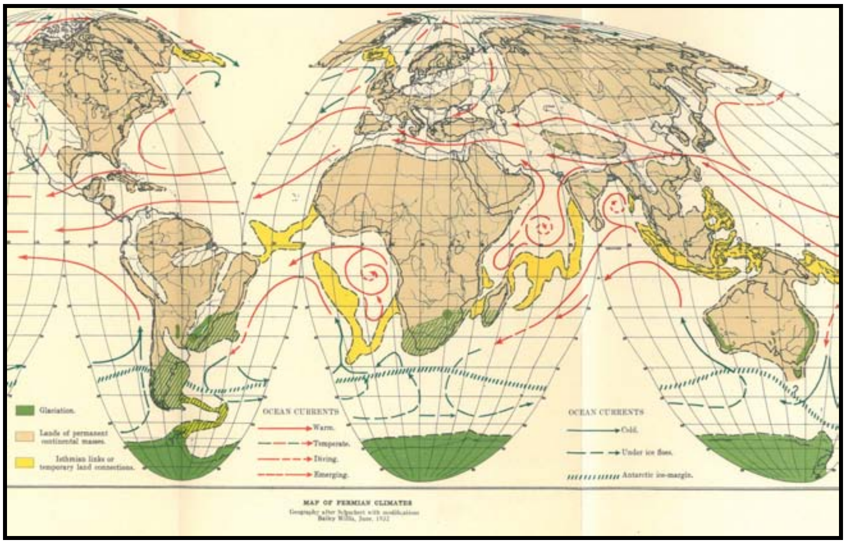

Figure 3. Willis (1932), Plate 29: 'Map of Permian climates, geography after Schuchert with modifications'. Color keys: Green-Glaciation; Brown-Lands of permanent continental masses; Yellow-Isthmian links or temporary land connections. Ocean currents shown as arrows: Red-warm, Blue-cold, Blue-red dashed-Temp.

Schuchert called his map a ‘Synthetic paleogeographic map of all Permian time’. It was a synthesis for the whole of the Permian, including early and late Permian. But it was not a synthesis of the paleogeography. It gave no indications of the Permian fold-mountain belts. It showed no Permian climate indicators, such as tropical coal deposits, or arid sand deposits, gypsum deposits and salt deposits—paleogeographic evidence that Köppen and Wegener (1924) had fully explained on drift maps. It did not show Permian ice sheets with ice-flow directions. It had too little information to warrant the use of colors and the apparently sharp bathymetric differences. Oceans were shown in white. Shallow Permian seas were green. Permian land was brown, including some that is now continental shelves and some that is deep ocean floor. Land bridges or isthmuses were in yellow—except for the parts that for no apparent reason were shown in brown.

Willis drew the bridges narrower on his map. He also reduced the amount of brown beyond the present continental margins. But even he allowed some continental crust to have submerged off the coast of southern Brazil, because Permian glaciers came from there.

5. SUITABLY INDISTINCT BATHYMETRIC MAPS

Willis and Schuchert agreed that bathymetric data should be used to determine where the land bridges might have been. The job of placing the bridges went to Willis. He explained in his paper that in 1920 he had drawn some possible isthmian land bridges on “the available bathymetric charts” (Willis 1932, p. 919) and apparently it was these maps that he published. His only reference to the base maps was a footnote stating: ‘Carte General Bathymetric des Oceans, Prince of Monaco, 1910–1920’ (Willis 1932, p. 930). (The reference should have been given as *Carte générale bathymétrique des océans du Prince de Monaco*). These bathymetric charts were well respected in their time.[1]

On Willis’s version of the maps, the actual depths were quite indistinct. All ocean depths were colored the same, except continental margins, which were shown brown (see Figure 4). In contrast, the German geographer Gerhard Schott (1912) had published a map of the Atlantic using the same bathymetric data. A part of Schott’s map is shown as Figure 5. Shades of blue clearly show the various depths. The Mid-Atlantic Ridge is continuous and strikingly clear for the length of the Atlantic Ocean.

Some readers may not be aware that the Mid-Atlantic Ridge had been known since the *Challenger* expedition of 1872–1876. In his geology textbook, Dana had shown it on a bathymetric map and described it as follows (his italics): “From north to south, along the middle of the Atlantic, there is a wide zigzag plateau, *conforming in trend to the American coast*” (Dana 1883, p. 9). For his 1895 textbook, Dana compiled his own bathymetric map of the Atlantic and Pacific Oceans, using data he had obtained from the United States and British Hydrographic Department, and the United States Fish Commission (Dana 1895, p. 19). This map showed individual soundings, and was nearly as detailed as the maps of Willis and Schott.

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1 The maps were the product of a global project carried out under the auspices of His Serene Highness Prince Albert I of Monaco (1848–1922), a keen oceanographer. It is not entirely clear from the dates given what edition of the *Carte générale* was used by Willis. The first edition was issued in 1905, but it had many unsatisfactory features and work was soon started on a new edition. But publication of a revised edition was interrupted by the War and was only completed in 1930 as *Carte générale bathymétrique des océans dressée par ordre du Prince Albert I^er^ de Monaco . . . Deuxième édition 1912–1930* . . . For details of the huge but not wholly successful project, see GEBCO (2003). From the dates given by Willis, it would appear that he used some information from the 1st edition and also from the earlier part of the work for the 2nd edition.

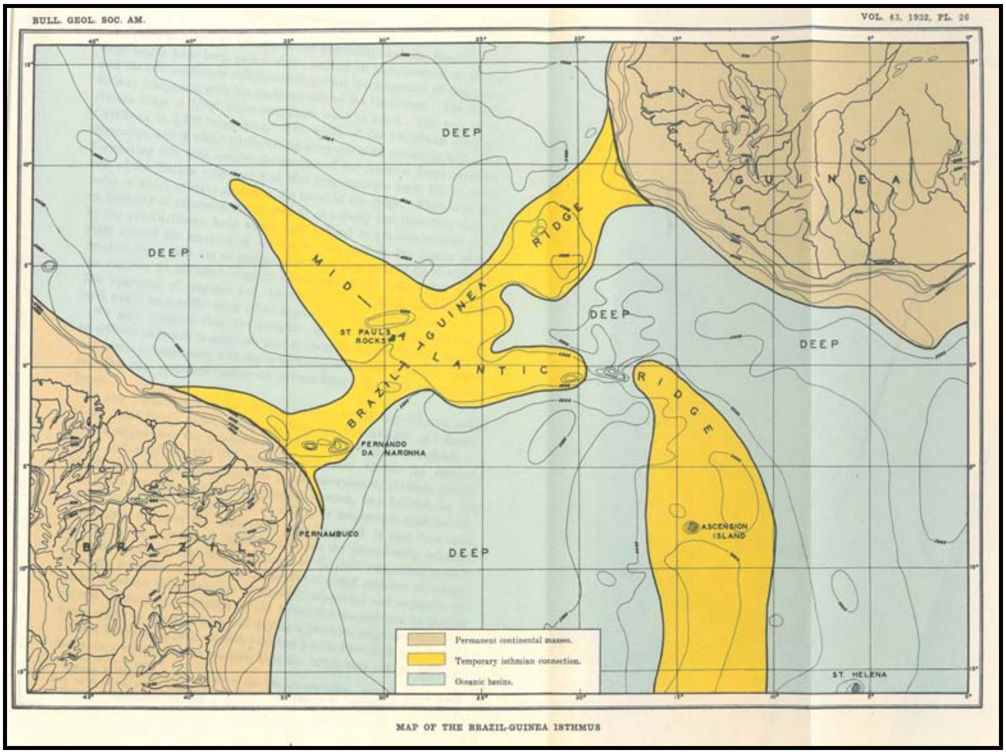

Figure 4. Willis (1932), Plate 26: 'Map of the Brazil–Guinea Isthmus'. Color keys: Brown–Permanent continental masses, Yellow–Temporary isthmian connection, Blue–Ocean basins.

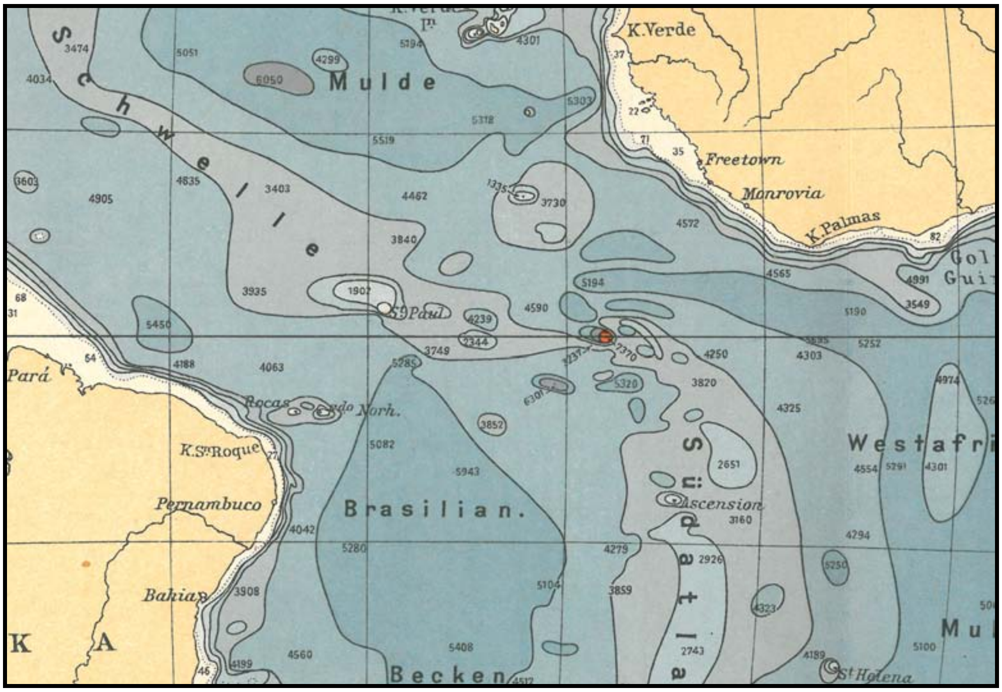

Figure 5. Detail for the corresponding area from Schott (1912), Plate 5. Depths in meters. Both this map and the one by Willis were drawn from the same bathymetric data. Note the differences in interpretation and presentation.

From 1922, the U. S. Hydrographic Office had been collecting echo-sounding bathymetric data from the world's oceans. And a modern bathymetric map of the southern Atlantic, based on echo soundings, was published in Germany in 1927 (see Figure 6) as an early portion of the records of the German oceanographic survey conducted by the vessel *Meteor*. Willis was probably not aware of these later data sources; or at least he failed to utilize them. It is a 'nice question' whether knowledge of the *Meteor*'s data would have helped or hindered Willis's argument, but one can at least say that he did not use the most detailed bathymetric data then available, or clearly display on his map the details that he did have.

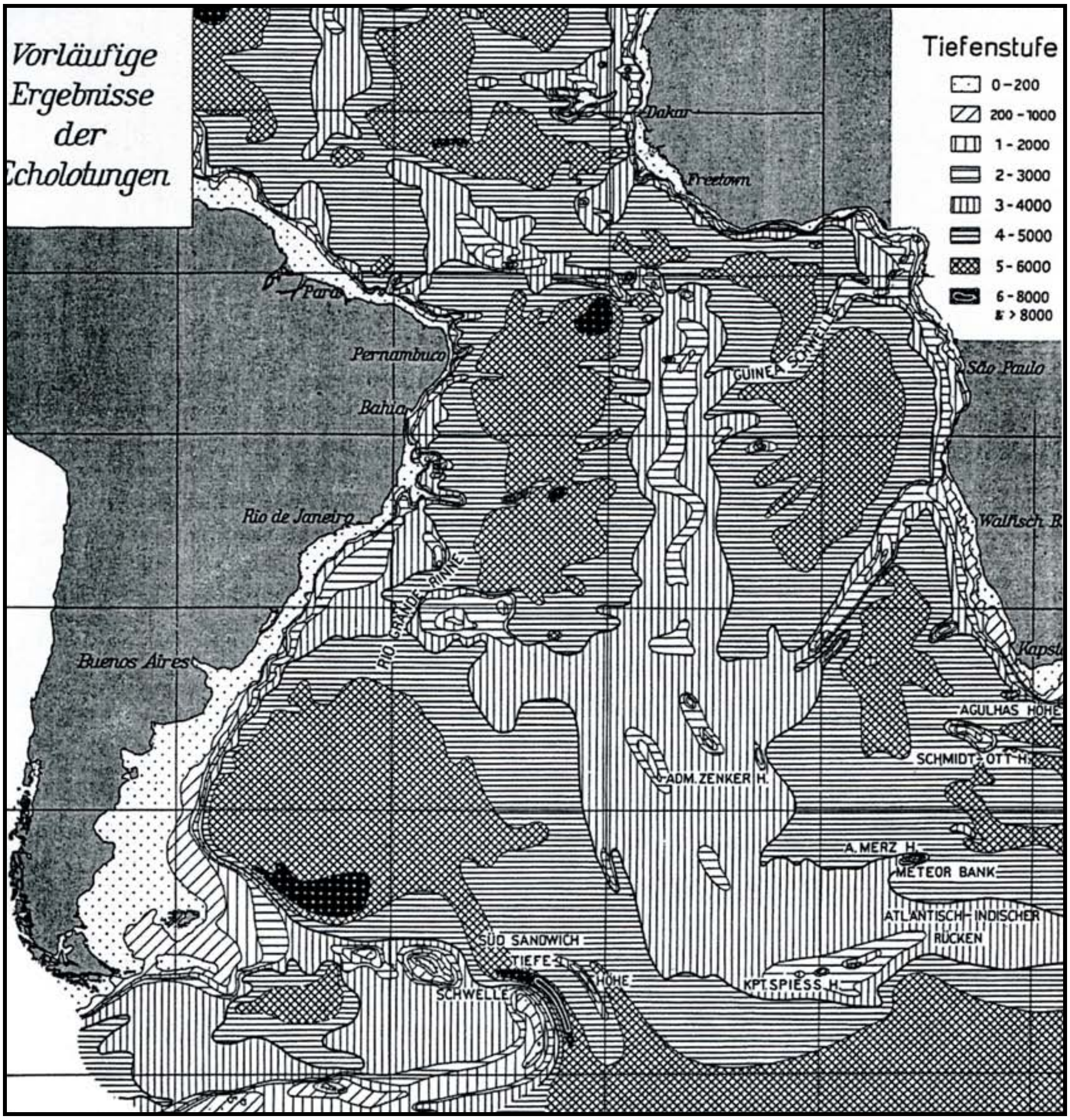

Figure 6. Bathymetric map based on echo soundings made during the Meteor*'s oceanographic survey of the Atlantic (Spiess* et al.*, 1927, Map 4). Depths in meters.*

Returning to Willis's map, we see that he wrote the word 'DEEP' in five places on his map of the Atlantic between Africa and South America, but didn't emphasize the various depths. Instead, he drew bold lines and colored an area that he called the 'Brazil–Guinea Ridge'. The bright color made it look like a legitimate feature, although his map was nothing more than a sketch. From Schott's map it is easily seen that about half of Willis's Brazil–Guinea Ridge could have been labeled 'DEEP' (over 4,000 meters).

Willis also had borderlines and color to indicate parts of the Mid-Atlantic Ridge. He left a break in the Mid-Atlantic Ridge to allow ocean water to circulate east of this postulated land barrier (see Figure 3), and perhaps also to disguise the fact that the ridge continuously matched the coasts on either side. He mentioned the soundings data that he used, but did not show such data on the map. The depth contours were marked in meters, but his descriptions were in feet, making them difficult to compare. Scientific descriptions are often like this: written to document that the author had evaluated the evidence, but not to help the reader evaluate it.

> 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 Noronha, 200 miles off the Brazilian coast (Willis 1932, p. 930).

This type of description might have seemed geological and quantitative to sympathetic or uncritical readers, but it served no scientific purpose. Such verbiage was characteristic for Willis's paper.

Willis gave similar descriptions and folding colored maps for the Africa–India Isthmus (see Figure 7) and the East Indian Isthmuses (see Figure 8). To draw these, he needed to cross deep ocean areas. He did this for the Brazil–Guinea Isthmus and the Africa–India Isthmus. But curiously, he did not try to connect the East Indian Isthmuses. The isthmuses depicted on all three of these maps were just rough sketches, as were the isthmuses that he and Schuchert sketched on their world maps. They were not drawn to carefully follow contours, or to look exactly alike. It was the suggestion of isthmuses—not the details—that were important. The large colored maps were, I suggest, deployed to make the sketches seem significant.

Willis's map (see Figure 3) showed ocean currents and how they might have been blocked and redirected by his isthmian links. He also described these currents in words, which would mostly have sidetracked readers with sophisticated but insignificant descriptions. When read critically, his currents and explanations were not at all convincing.

It may be noted that Willis did discuss the Caribbean basin as a dynamic area that could serve as an analog or paradigm that supported the plausibility of his various isthmuses. The Caribbean area is bordered on the west by the Isthmus of Panama and on the east by the 'Antillean isthmus'; and Willis argued that the region had undergone a variety of vertical movements over time. He depicted Caribbean bathymetry on a separate folding plate map (though not in color), with quite detailed contours. But significant argument that the Caribbean served as a secure basis for analogies was lacking and Willis quickly moved on to his unreliable bathymetric data and mapping for the oceans, trying to argue thereby that the mid-Atlantic might have undergone vertical movements similar to those that had taken place in the Caribbean area. His Caribbean map had detailed bathymetric information, but this was not given for the mid-Atlantic, even though, as shown above, such data were available for that region by 1932.

6. THE CLIMATIC TEST

Schuchert and Willis both understood that information about Permian climates provided a way to test the paleogeography, including the continents and postulated bridges. Schuchert had discussed ancient climates in research papers (*e.g.* Schuchert 1915b) and in his textbook. He knew the difficulty of trying to explain the locations of Permo-Carboniferous climate indicators on a map that assumed fixed continents. After Alex Du Toit (1921) and Köppen and Wegener (1924) had used climate evidence to support their interpretations of drifting continents, Schuchert became less willing to discuss certain details of Permian climate. And in the 1924 edition of his textbook, he removed arrows showing ice-flow directions from the glaciation map (see Figure 1). In the 1933 edition he deleted a map and discussion of the South African glaciation. Schuchert did not discuss climate in his 1932 article, but Willis was more daring. He included in his paper a many-page section entitled 'Climatic test of isthmian links'. Here he postulated Permian uplifts of certain continental areas in much the same way as he had done with parts of the ocean floors. He wrote that he was testing the paleogeography, but one might say rather that he was testing the tolerance level of Schuchert, the editors, and the readers.

> For our test of the hypothesis of isthmian links it will suffice to show that under that geographic development the oceanic and atmospheric conditions would change distinctly in favor of a glaciation in the southern areas where Permian ice-sheets accumulated, while tempered airs and waters would prevail in the Arctic.
>
> We have first to recognize that during a long period of Carboniferous time extensive epicontinental seas indicate quiescence of internal terrestrial forces and general prevalence of one of the grand climatic cycles of geologic history when genial temperatures spread to the polar regions. There was probably no ice or snow in either hemisphere anywhere during a summer season, and certainly no ice-cap. A very great change in the aspects of land and sea and in zones of climate was inaugurated and developed by the reawakening of terrestrial forces and the elevation of continents and mountain chains during the late Carboniferous and Permian. There is abundant sedimentary evidence that South America, Africa, India, and Australia were warped by these movements in such a manner as to produce high plateaus and basins. We postulate that the Brazil–Africa, Africa–India, and Trans-East Indian isthmii were elevated during this orogenic period (Willis 1932, p. 945).

Willis suggested that the redirection of ocean currents by isthmian links could help explain the glaciations in the southern hemisphere and the tropical climates in the Arctic region. He never mentioned the Iceland isthmus, but on his map showed it blocking the Gulf Stream, the current that brings warm water to the North Atlantic. I happen to live in Norway and appreciate that the Gulf Stream makes climate livable here. According to the red and blue arrows on Willis's climate map, the Gulf Stream was blocked, but a warm current flowed across the Arctic Ocean, and temperate waters flowed southward from the polar region. Like the Iceland isthmus, these warm currents were never mentioned.

Willis labeled his large plate 'Map of Permian climates, geography after Schuchert with modifications'. He plotted Permian glaciation in green. Much of the glacial data probably came from Schuchert's textbook. But a comparison with Schuchert's map (see Figure 1) shows that Willis's modifications did not present the glaciation data either accurately or fairly: the amount of glaciation was reduced and was simply eliminated in places where it was most difficult to explain!

As an example, consider how Willis reinterpreted the Indian ice sheet. He described the deposits as occurring in two narrow belts, one 300 miles long and the other 700 miles long. He was aware that all experts had agreed that these belts were part of a once continuous continental glacial deposit that had formed near sea level on a stable continent. For several decades geologists had puzzled over the evidence, but there was no denying that the glaciation was continental, had moved toward the equator, and had carried glacial erratics as far as 750 miles (see Coleman 1926, p. 110). Willis was willing to deny this interpretation:

> 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.

The elevation of India during the late Paleozoic and Permian was an incident in the general orogenic activity of those periods (Willis 1932, p. 950).

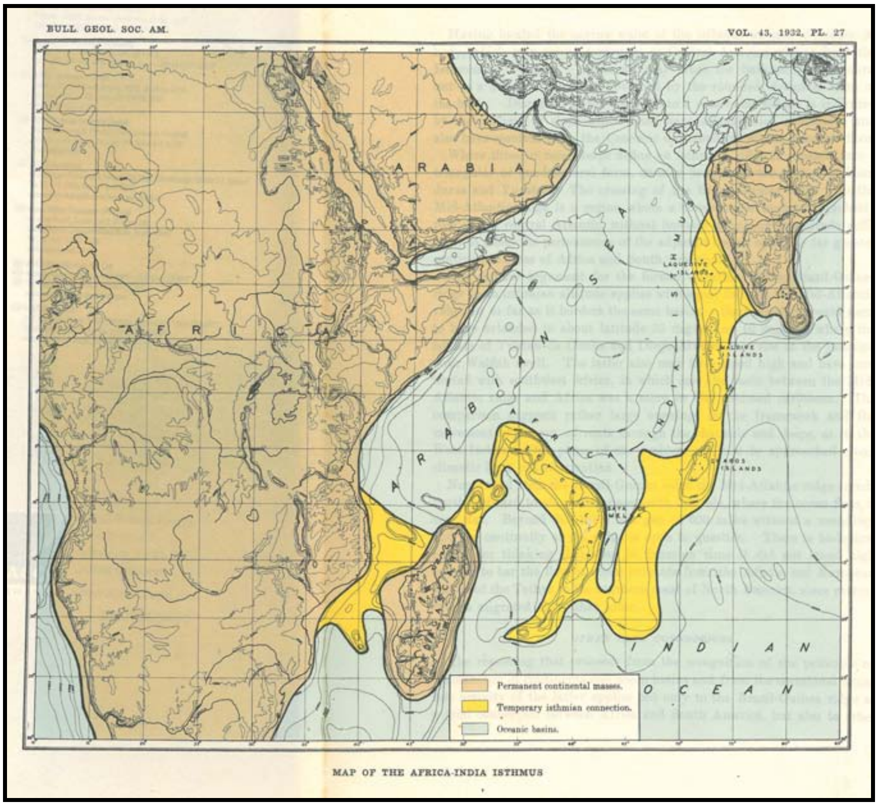

Figure 7. 'Map of the Africa–India Isthmus', from Willis (1932), Plate 27.

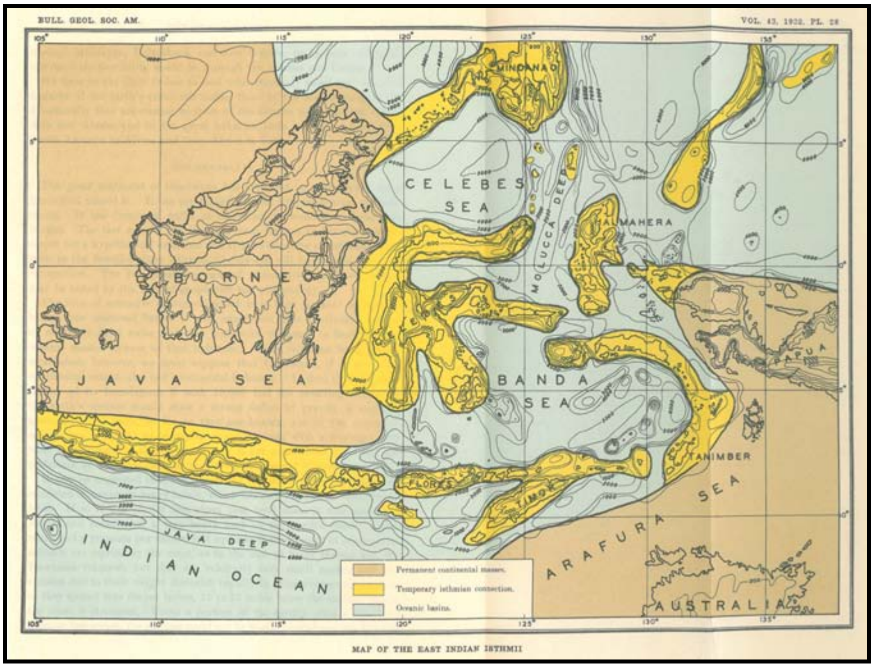

Figure 8. Plate 28: 'Map of the East Indian Isthmii'. From Willis (1932), Plate 28.

Here Willis seems to have been inventing fault-mountain ranges and local glaciers in an area that was well known to be flat and stable. These suggestions, and others, apparently drew no protest from Schuchert. He was not a co-author and was not obliged to correct Willis. Perhaps Willis was not so much testing the hypothesis of isthmian links, as testing how desperate Schuchert was to have this isthmian-links paper published.

7. CLOSING COMMENTS

The articles 'Gondwana land bridges' and 'Isthmian links' were apparently written for geologists who wanted to believe that fixism was a viable alternative to mobilism. They were published in a journal that would not challenge two such respected authors. Afterward, the isthmian links model was frequently cited as evidence against the theory of continental drift. From about 1928 and for the next forty years, there was a strong scientific consensus in America that continents were fixed and that drift theory was absurd. These two papers helped maintain that consensus.

Schuchert never really accepted the model of isthmian links. In the next revised edition of his textbook he mentioned the idea for the Gondwana land bridges (Schuchert and Dunbar 1933, p. 293) but not for Eria. Rather than update his 1923 map of Permian land bridges and glaciation, he reprinted it (see Figure 1). Then in the 1941 edition, when the 1923 map was much too old to be used again, he chose a peculiar base map that showed no Atlantic Ocean (see Figure 9). In this way, he avoided having to redraw or delete his wide land bridges. But in the text, Eria was still described as a wide continental land bridge that had sunk to the level of deep ocean floor (Schuchert and Dunbar 1941, p. 211; Dunbar 1949, pp. 216–217). For Schuchert and his junior co-author Carl O. Dunbar (1891–1979) Eria was a sacred cow. They had each believed in it when they were young and impressionable and they could not renounce it in their textbooks. Thus younger generations also came to believe in it and disregard continental drift.

After Schuchert died in 1942, his younger colleague Chester R. Longwell (1887–1975) invited geologists of all specialties to publish criticisms of continental drift (Longwell 1944, p. 221). Willis must have enjoyed providing creative arguments against it. At the age of 87, he accepted Longwell's invitation and wrote a short paper with the facetious title 'Continental drift, *ein Märchen'* (a German fairy tale). This time he proposed that thermal uplifts had raised land connections across the Pacific Ocean, even to Hawaii (Willis 1944, p. 513). He was, it seems, still testing the tolerance level of anti-Wegener editors and readers.

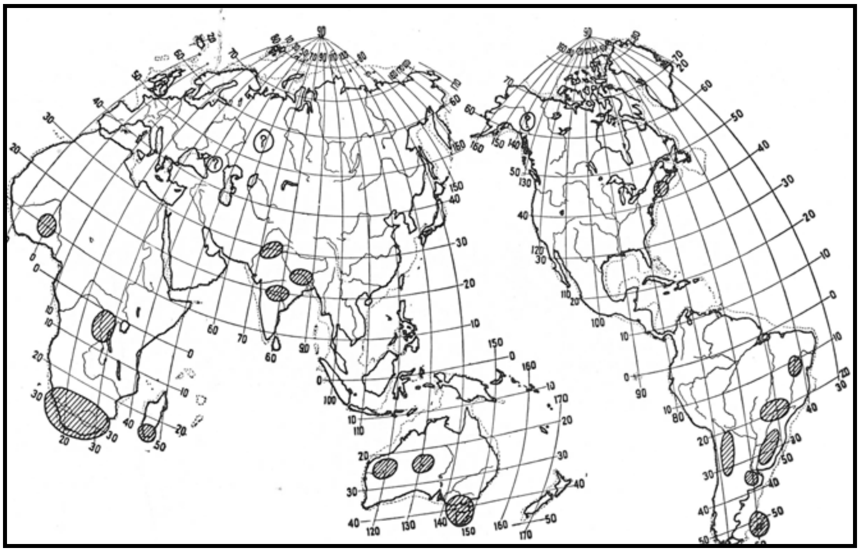

Figure 9. Schuchert's updated map showing the distribution of areas of Permian glaciation (shaded). This new base map was not suitable for showing the problematical land bridges across the Atlantic. From Schuchert and Dunbar (1941, p. 291).

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I thank David Oldroyd for pointing out the influence of the isthmian links model, and suggesting that I discuss it in more detail than in my book. This paper has benefited considerably from his editorial suggestions, and from useful comments by Homer Le Grand and a second referee. Dr Cornelia Lüdecke kindly provided the copy of the *Meteor* bathymetric map.

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