1

Extension and reception of paleomagnetic/ paleoclimatic support for mobilism: 1960–1966

1.1 Introduction

In the early 1960s, paleomagnetists continued to buttress their case for mobilism by extending surveys, by conducting further field stability tests, by bringing magnetic cleaning into general use, by utilizing new radiometric ages, and by further enlisting evidence of paleoclimates. This was the acme of the contribution of continental paleomagnetism to the mobilism debate, and by the mid-1960s the global paleomagnetic test of continental drift was essentially completed and Wegener's general notion of continental drift confirmed. By then, work on continents was being overtaken by an avalanche of new data from the oceans, especially topographic surveys and surveys of geomagnetic anomalies reflective of reversals of remanent magnetization of the oceanic crust. There was also a renewal of interest in drift mechanisms.

Participants from North America continued to criticize continental drift harshly, but a few began to welcome paleomagnetism's support for it. There was also a renewal of interest in drift mechanisms. I shall trace the reception at this time of the paleomagnetists' case by certain notable figures in Earth sciences.

The Newcastle and Canberra groups continued to argue for continental drift and polar wandering; the London, Imperial College, group discussed continental drift in a manner that could include polar wandering without requiring it, but notably Deutsch questioned, as he always had done, the need for polar wandering at all, and argued for continental drift as the dominant process. A new paleomagnetic group in Salisbury (Harare), Southern Rhodesia (Zimbabwe), contributed importantly. Paleomagnetists organized symposia, wrote and edited books. There was the 1962 anthology, *Continental Drift*, to commemorate the fiftieth anniversary of Wegener's theory, and in 1963 the huge NATO symposium on paleoclimatology with proceedings edited by Nairn. Irving (1964) wrote *Paleomagnetism and Its Application to Geological and Geophysical Problems*, the first general text in English.

1.2 Dott reexamines the Squantum Tillite

I begin by returning to the controversy concerning the Squantum Tillite, an old obstacle to Wegener's continental drift (I, §3.10–§3.12). The Squantum Tillite of the

Boston area, purportedly of Permian age, was troublesome during the classical stage of the controversy, being seen as a glacial deposit in a region otherwise characterized by Late Paleozoic deposits laid down under a hot climate (I, §3.11). It became troublesome to the paleomagnetic case for mobilism for a similar reason, because it indicated cold climate at a time when paleomagnetically determined latitudes were low (I, §3.12). In timely fashion, in 1961, R. H. Dott Jr. revisited this longstanding problem.

Born in 1929, Dott received his B.S. and M.S. degrees in 1950 and 1951 from the University of Michigan, majoring in geology.[1] He entered Columbia University in 1951 and received a Ph.D. in geology in 1956. He took a job in the oil industry in 1954 before finishing his Ph.D. thesis, and from 1954 until 1956 worked in the Pacific Northwest on turbidites. He spent 1956–7 as a first lieutenant in the US Air Force, stationed at the Air Force Cambridge Research Center, Bedford, Massachusetts, where, in his spare time, he studied the Squantum. Dott recalled:

> First let me paint a little background. The Squantum interested me because I had been working in Oregon and California (oil industry) for two years prior to going to Air Force active duty in Boston (1956–57). While on the West Coast, I had been studying a lot of ancient submarine "mudflow" and turbidite deposits, and had become enthralled with their sedimentology. Of course these kinds of deposits had only recently been recognized through the seminal work of Ph. H. Kuenen, etc. and the marine folks like Heezen. As a graduate student at Columbia (and classmate of Heezen), these new wonders were very hot stuff. Against that background, the experience of California and Oregon was exhilarating to say the least. When I found I was to be in the Boston area for two years and not doing geology, I decided to look at the already controversial Squantum to compare it with what I had been seeing in the west. This began simply as something to do to keep in touch with rocks, but – at the urging of an older colleague when I got to Wisconsin – it evolved into my paper.
>
> *(Dott, August 21, 2000 email to author)*

At the time, turbidity currents were a hot topic among oceanographers. Recognized by none other than Daly (1936), who called them density currents; they are underwater currents made denser by a substantial sediment load, which propels them rapidly forward under gravity. As they cross the continental shelf they erode underwater canyons and travel hundreds of miles out into the ocean floor. Ph. H. Kuenen (1937) produced them in the laboratory. After World War II, Kuenen and C. I. Migliorini (1950) revived interest in them. The next year, D. B. Ericson, M. Ewing, and B. C. Heezen (1951) of Columbia University mapped several huge submarine canyons that crossed the edge of the continental shelf off the eastern United States and Canada, and argued that they had been eroded by turbidity currents. After reviewing reports of the breakage of trans-Atlantic cables coincident with the 1929 Grand Banks (Newfoundland) earthquake, Heezen and Ewing (1952) proposed that unconsolidated sediments on the continental shelf had been dislodged by an earthquake and slid down the continental slope, forming a gigantic and rapidly moving current, turbid with suspended sediment that eroded canyons and then moved out across the ocean floor, breaking cables as it went (§6.5). Establishing that cables located closer to shore broke before those further out, they estimated the current's velocity.[2]

Because Dott had been at Columbia, he knew about the work of Heezen and colleagues. As a sedimentologist, he was interested in turbidites, the deposits laid down as turbidity currents slow down and their sediment falls out of suspension. He had already studied turbidites and submarine mass flow deposits in California and Oregon. Slumps, turbidites, and glacial tills were all poorly sorted and could mimic one another.[3] Indeed, J. C. Crowell, who had begun to express doubts about the glacial origin of some "tillites," had questioned the characterization of the Squantum as glacial.

> The writer felt, for example, on visiting the Squantum "tillite" near Boston on a Geological Society of America excursion in 1952 that these rocks perhaps also formed by slumping and accordingly required re-investigation before their glacial origin could be accepted.
>
> *(Crowell, 1957: 1005)*

Dott decided that the Squantum strata resembled turbidites more closely than tillites, and had more likely been deposited from local gravity-driven turbidity currents than from glacial activity. Dott did more, arguing that several key "glacial" indicators, i.e., scratched and polished underlying rock, and erratics, embedded in laminated mudstones, and derived from distant sources, were missing from Squantum strata.

> The most convincing evidence, a widespread gouged and polished pavement beneath till-like deposits in clearly nonmarine strata, is lacking at Boston and in many other places . . . The Squantum possess neither significant numbers of erratics in laminated mudstones nor any rock fragments that could not have been derived locally. Unlike the Gowganda [a Precambrian glacial deposit in western Ontario] and some of the better-established tillites of the southern hemisphere, all the debris in the Squantum could have originated merely by redeposition of local gravels with addition of torn-up and bent fragments of contemporaneous soft muds.
>
> *(Dott, 1961: 1301–1302; my bracketed addition)*

Dott (1961: 1296) also questioned the dating of the Squantum, arguing that its assignment to the Permian, based primarily on two "poorly preserved casts of presumed fossil trees which lacked any bark impressions or internal structure," was likely mistaken. He pushed its age back to the Mississippian or Devonian, basing his estimate primarily on recent radiometric dating of associated rocks. This changed, but did not remove, the difficulty the Squantum beds presented to the mobilism argument because the paleomagnetically determined latitudes of eastern North America for the Devonian through Permian were all low.

Reminiscent of Köppen and Wegener (I, §3.15), Dott argued that warm climate findings from North America for the Late Paleozoic were inconsistent with the glacial interpretation, as were the paleomagnetic data which indicated that North America had been "rather close to the equator" throughout the Paleozoic.

> The Squantum was thought to be [glacial] but it is unusual in its isolated geographic position ... [it is] particularly troublesome to advocates of continental drift, because plotting of [its] predrift [position] brings [it] rather close to the equator. [The] Past [position] of North America ... [is] ... more in accord with other Paleozoic evidence, such as paleomagnetic data, extensive coals, and somewhat younger evaporite deposits, than [is its] present [position].
>
> *(Dott, 1961: 1290; my bracketed additions)*

But he did not take a position on mobilism, instead arguing, based on new knowledge of turbidites, that many ancient supposedly glacial deposits should be reexamined before they could be used to draw conclusions about continental drift or polar wandering.

> Promiscuous postulation of ancient glacial periods still exercises imaginations of scientists; however, most supposed examples of ancient glacial deposits must be critically re-examined. Many others may have been formed by one of the several possible alternative mechanisms. To be valid, postulated glaciations must be compatible with paleogeographic and paleotectonic evidence. Theories of geotectonics, paleoclimatology, and paleobiogeography based wholly or in part upon supposed ancient glaciations, including continental drift and polar wandering, cannot be evaluated honestly until evidence for all such periods has been critically reanalyzed. If the present paper does nothing else, it underscores the great difficulty in interpreting glacial-like deposits.
>
> *(Dott, 1961: 1303)*

Did this mean that Dott had come to doubt extensive Permo-Carboniferous glaciations in the Southern Hemisphere? No, although he thought that some of them were questionable, the evidence that most were glacial was excellent. In fact, they served as the touchstone for what counted as good evidence for ancient glaciation.

> A preserved, extensive, grooved and polished pavement overlain by poorly sorted, till-like material – particularly if nonmarine – is the most compelling glacial evidence. Very large erratic boulders are suggestive of ice movement, as are abundant rafted erratic fragments in fine muds ... Independent biologic or isotopic cold-temperature indicators are sorely needed to strengthen glacial interpretations. General stratigraphic relationships and tectonic setting serve as important factors in judging probability of alternative interpretations, particularly in geosynclinal sequences. From these criteria, the writer judges that only the Permo-Carboniferous glaciation in certain parts of the southern hemisphere is firmly established. The Gowganda and some other Precambrian deposits are very likely glacial, but most postulated examples must be re-evaluated.
>
> *(Dott, 1961: 1289)*

Dott (1961: 1303) had no problem with the evidence for Permo-Carboniferous glaciations in South Africa, India, cratonic Australia, and southeastern Brazil; they were geographically extensive, chiefly found in non-marine sequences on stable cratons as for the great Pleistocene continental glaciation. He did question the

Permo-Carboniferous glaciations in southeastern Australia,[4] Argentina, Chile, and Bolivia, because they occurred in what were then active orogenic belts, and therefore were suspect, but he (1961: 1303) thought that even they would likely be confirmed after reevaluation.

Dott had carefully discussed criteria for identification of glacial deposits, ranked them with regard to their relative importance, and considered other paleoclimatological findings about North America during the Late Paleozoic, including the paleomagnetic positioning of North America near to the equator at that time. Dott argued that Squantum strata probably had been deposited from turbidity currents that could have been in low latitudes and hence their presence was not inconsistent with mobilism. But he was not yet a mobilist; the paleomagnetic evidence and the widespread Permo-Carboniferous glaciation were not sufficient for him. As Dott (January 2001 email to author) recalled, "First my paper was not about drift but about sedimentology, and the research was done from 1957 to 1960 – before much of the paleomagnetic data had reached me. Second, being a North American geologist, I was cautious about drift . . ."

He first learned about continental drift as an undergraduate at the University of Michigan majoring in geology.

> I had first been exposed to continental drift either in 1950 or 1951 at Michigan by Digby McLaren (Cambridge graduate, then Ph.D. candidate at the University of Michigan [later Director of the Geological Survey of Canada]). Digby McLaren is a guy who likes to provoke, and he reckoned we isolationists needed to hear this wild theory. I do not remember *ever* hearing of drift or Wegener until that day! He gave a fascinating noontime "brownbag" talk, which aroused my interest in the idea.
>
> *(Dott, January 2001 email to author; my bracketed addition)*

When Dott arrived at Columbia University in the fall of 1951, he quickly learned of the general opposition to continental drift, but he maintained his interest in it.

> Then I arrived at Columbia in the fall, 1951. Now I heard a bit about the opposition to Drift from various sources, and bought Du Toit's fascinating book [*Our Wandering Continents*] in a used bookstore in downtown Manhattan. I think I read it in one night.
>
> *(Dott, January 2001 email to author; my bracketed addition)*

At Columbia, the influential Walter Bucher was strongly opposed to continental drift (see Volumes I and II), and Dott remarked on the reaction of graduate students.

> Did I have courses from Bucher? Yes, indeed, at least 2 or 3, and he was on my dissertation committee; he also gave me a French reading exam. He was a charming man whose great enthusiasm was very contagious. But his opposition to Drift was well known and taken by students with amused deference.
>
> *(Dott, January 2001 email to author)*

Dott recalled a debate that the graduate students arranged between Bucher and Lester King (I, §6.10 for other recollections of this debate).

Lester King came to town either in the fall of 1952 or spring of 1953. We graduate students arranged a kind of debate between King and Bucher on Drift. We students felt afterward that King clearly had "won" in terms of the beauty of his presentation. What a magnificent orator he was with a beautifully modulated voice and a commanding presence. I thought he should have been on the stage. I was also mesmerized by him again in 1963 during an Antarctic symposium I attended in Capetown. So I was by [1953] 1963 very sympathetic to the idea and open to pro-arguments.

(Dott, January 2001 email to author; my bracketed addition)

Dott got his next exposure to mobilism from Runcorn. Runcorn, on one of his North American jaunts, gave his "standard" talk on paleomagnetism, telling his audience what he and his cohorts had done at Cambridge and were now doing at Newcastle.

I was in Los Angeles back in the oil industry in the spring of 1958 (5 or 6 months before I began at the University of Wisconsin in September 1958), and Runcorn came to town. He gave a noontime talk to a large audience of petroleum geologists. I was fascinated by his talk! Most of the audience probably was either put off or asleep, but it really jolted me. As soon as I moved to Madison and started teaching, I began to watch the paleomagnetic data for pole positions as new papers appeared. In 1961 I began teaching Historical Geology, and immediately decided to compare "geologic" evidence of climate and latitude with the emerging paleomagnetic data. I made a series of paleogeographic maps for North America, which showed the comparisons, and they looked mighty good for Drift. I copyrighted my maps and began giving them to my class as a supplement to the textbook.

(Dott, January 2001 email to author)

Dott submitted his Squantum paper in April 1960, arguing that it was turbidite, not tillite. Perhaps if he had begun teaching historical geology a year earlier, he would have argued that his interpretation increased support for mobilism. Had he done so most readers would doubtless have dismissed it, as indicative of his bias toward mobilism. Dott's work was motivated by the topical question of whether ancient glacial tillites and subaqueous gravity flow deposits could be distinguished, not by paleomagnetism or continental drift, and it was understandable that he did not then and there take a position on mobilism.

Even if Dott's paper was an example of the "new" paleoclimatology that Nairn would soon call for (§1.3), Edward Bullard, the British geophysicist, who during the 1960s was to play a major role in advancing the fortunes of mobilism (§2.13–§2.15), was not convinced that Dott was correct. Bullard referred to his work in an address given to the Geological Society of London. Although Bullard had at the time begun to view mobilism with some favor, he did not think that paleoclimatology could provide a definite answer to the drift question.

. . . how good is our knowledge of past climates? Can we recognize an ice age with certainty? Even supposing these [Permo-Carboniferous] ice ages to be genuine are we sure, for example, that the Squantum tillite does not represent a contemporaneous ice age in New England? Dott (1961), who has studied the rocks recently, says that it does not, but can we be sure – perhaps he has been influenced by the improbability of ice occurring 90° of latitude from that in South

Africa, and therefore presumably somewhere near the Equator . . . Such arguments and doubts are endless and it is not profitable to work through them again . . . Clearly it is necessary to break away from this well-trodden circle of ideas.

(Bullard, 1964: 2–3; my bracketed addition)

Bullard's point was not that Dott's work was careless, but that some paleoclimatological studies, no matter how competent the practitioner and how good the exposure, were necessarily based on incomplete and, to a degree, ambiguous data. Needless to say, paleoclimatologists probably put little stock in Bullard's view of the limitations of their science, because he was, after all, a geophysicist with little knowledge of paleoecology or sedimentology.

Bullard's negative assessment, coming from someone who had only recently become disposed toward mobilism, illustrates, I believe, how many outside of paleoclimatology had come to feel that it alone was incapable of providing definitive solutions to problems of past climate. Perhaps Nairn might have agreed with Bullard, but he would still have thought the practice of paleoclimatology capable of improvement. I doubt if Dott would have found Bullard's skepticism acceptable, because he accepted as firmly established much of the Gondwana Permo-Carboniferous glaciation (the cornerstone of the classical argument for drift). Dott (1961: 1301) also wrote of his hypothesis about the origin of the Squantum as "seem[ing] more plausible than glaciation." Evidently he believed that there were useful statements that careful paleoclimatologists or paleoecologists could make about their data when viewed alongside the independent paleomagnetic data; Nairn would have approved.

1.3 Comparisons of paleomagnetic and paleoclimatic evidence: the 1959 Newcastle symposium and its 1961 publication *Descriptive Palaeoclimatology*

Creer, Irving, and Nairn (1959) obtained a Lower Permian pole from their paleomagnetic survey of the Great Whin Sill in northeastern England and found latitudes derived from it compatible with the Permian climate of Europe. Irving (1964), working mainly with his Ph.D. student James Briden, extended the comparison of paleomagnetic and paleoclimatological data in a number of ways. He also worked with David Brown on the distribution of Late Paleozoic amphibians, and argued that the consilience between the paleontologic/paleoclimatic and paleomagnetic data strengthened support for mobilism (Irving and Brown, 1964). Opdyke (1961, 1962) and Runcorn (1961, 1964a, 1965) wrote further about paleowind directions, and broadened their involvement in paleomagnetic and paleoclimatic comparisons. Blackett (1961), more or less following the plan of attack first utilized by Irving, appealed to paleoclimatology to support specifically the geocentric axial dipole (GAD) hypothesis and generally the reliability of the paleomagnetic data. Nairn, besides making paleoclimatological comparisons in his own work (Schove, Nairn, and Opdyke, 1958), began to work with paleoclimatologists and paleontologists.

Encouraged by Runcorn, Nairn edited two volumes in paleoclimatology, *Descriptive Palaeoclimatology* (Nairn, 1961) and *Problems in Palaeoclimatology* (Nairn, 1964a). Both grew out of Newcastle symposia attended by paleomagnetists and paleoclimatologists.

I begin with *Descriptive Palaeoclimatology*, then turn to Blackett's 1961 foray into paleoclimatology, and close with an examination of *Problems in Palaeoclimatology*. I shall consider the attitude of the symposiasts toward paleomagnetism and mobilism to ascertain the reception of the paleomagnetic case for mobilism in the early 1960s. Opdyke, Runcorn, Irving, and Creer participated in one or other of these Newcastle symposia, and I can follow their efforts.

In 1959 Nairn assembled a symposium in Newcastle from which the book he edited, *Descriptive Palaeoclimatology*, arose; sixteen authors wrote fifteen essays, divided into fourteen chapters. In his introduction, he provided a brief history of paleoclimatology: "The early history of palaeoclimatology can be divided into two phases, the turning point being the appearance of Wegener's theory." During the first phase only Northern Hemisphere geology was known, and old glaciations were unrecognized; it was generally believed that Earth's climate had been mild until the Late Tertiary when extensive glaciation began.[5] This phase ended with the discovery of large-scale Permo-Carboniferous glaciation in India and across the Southern Hemisphere, ushering in the second and present phase with its ever-present backdrop, the mobilism controversy. Nairn argued that paleoclimatologists needed now to advance to a third stage where their interpretation of paleoclimatological data would not be dictated by their attitude toward fixism or mobilism. By this he did not mean that paleoclimatologists should not make claims about mobilism or fixism, but that such claims should be conclusions, not initial premises. Nairn wanted to see climatology based on sound meteorology. Before deciding in favor of mobilism or fixism, he wanted paleoclimatologists to develop better criteria for determining the reliability of their assessments of past climates. In this way Nairn believed that paleoclimatology could then mature into the third phase, and it was the aim of the symposium and the book to further that development. He aptly entitled it *Descriptive Palaeoclimatology*.

> The third phase of paleoclimatology must therefore be free from prior assumptions about land–sea distribution. A meteorologically acceptable framework is needed to which references about past climates can be referred. This information in turn must be acquired by methods whose reliability and whose limitations have been established by the critical examination of criteria involved. When world cross-sections of the climatic sequences of different areas can be assembled it must prove possible to piece together climatic zones and in so doing palaeoclimatologists will have made a significant contribution to the history of the earth's crust ... It is with the descriptive part of this scheme, the meteorological framework, the climatic criteria and the regional climatic histories, that this volume is concerned.
>
> *(Nairn, 1961: 2)*

The essays fell into three groups: the meteorological framework, paleoclimatic indicators, and regional climatic histories. Authors' attitudes toward mobilism differed. The first group comprised Nairn's introduction and H. H. Lamb's presentation of the present energy budget of Earth's surface. There were essays on paleoclimatic indicators: by Opdyke on desert sandstones and paleowinds, Robert Green on evaporites, F. B. Van Houten on red beds, R. F. Flint on evidence for cold climates, N. Thorley on the use of O^{16}-O^{18} ratios to determine paleotemperatures, Nairn on paleomagnetically determined paleolatitudes, and by A. S. Romer, G. Y. Craig, and R. Krausel, respectively, on the use of vertebrates, invertebrates, and plants as past climate indicators. Regional paleoclimatological histories were presented by M. Schwarzbach for Europe and North America, T. Kobayashi and T. Shikama for the Far East, Lester King for Gondwana during the Paleozoic and Mesozoic, and E. D. Gill for Gondwana during the Cenozoic. Flint, Romer, Gill, Krausel, and Kobayashi and Shikama were silent on mobilism and its paleomagnetic support. That Romer failed to comment on drift is at first sight surprising because he was one of the few vertebrate paleontologists from North America who showed sympathy toward mobilism (I, §3.8). However, he wandered little from his basic concerns – the assessment of the information about past climates obtainable from vertebrate fossils and vertebrate evolution. He concluded that they provided some useful information, but not nearly as much as sedimentology and paleobotany, so his silence is perhaps not surprising.[6]

As expected, Opdyke and Nairn argued in favor of mobilism. Opdyke summarized the results of his own and others' paleowind studies, and drew comparisons with paleomagnetism. Nairn devoted most of his section of his joint paper to a summary of paleomagnetism (Nairn and Thorley, 1961). He emphasized the consilience between paleomagnetic and paleoclimatic results, singling out Irving's early comparisons and citing Opdyke and Runcorn's work on paleowinds. He also (Nairn and Thorley, 1961: 180) advocated that maps of individual landmasses based on paleomagnetic data (his "drift diagrams" (II, §5.4)), even if incomplete, "may be a useful basis for further research and form a preliminary framework to which other climatic data may be added"; he wanted paleoclimatologists to use paleomagnetically determined paleolatitudes as a tool for paleoclimatic studies.

Again as expected, Lester King argued in favor of mobilism, writing a persuasive essay on the success of mobilism to account for the Permo-Carboniferous glaciation and the distribution of *Glossopteris* flora. He welcomed the paleomagnetic support for mobilism.[7] Noting its independence from classical arguments, he argued that the mobilist interpretation of the paleomagnetic data offered further support for paleogeographies of Gondwana based on paleoclimatic and paleontological data. He hoped this new evidence would persuade Earth scientists who had been reticent to accept mobilism because of its lack of an adequate mechanism.

Some critics have been reluctant to accept Drift because they could find no mechanical explanation for it, recently developed palaeomagnetic techniques independently establish relative movement between the southern continents, and reinforce the concept of Gondwanaland during the Palaeozoic era . . .

(King, 1961: 311)

King believed the best way to establish that something is possible is to demonstrate its occurrence. He thought that paleomagnetism established the existence of Gondwana during the Paleozoic, and hence the need to posit continental drift.

Martin Schwarzbach, then at the University of Cologne, had written an important monograph on paleoclimatology, *Das Klima der Vorzeit*, first published in 1950, expanded in 1961 and translated into English two years later (Schwarzbach, 1963). Schwarzbach discussed mobilism and paleomagnetism in his contribution to *Descriptive Palaeoclimatology* as well as in his book. He described the consilience of the paleoclimatic and paleomagnetic data.

The way in which the climatic conditions of Europe and North America fit into the whole picture of the earth at the time can be particularly well illustrated by the example of the Devonian period: both these northern continents belong to a warm climatic zone, in contrast to South America and Central and South Africa. The position of the geomagnetic pole, which is determined palaeomagnetically, falls in the south Atlantic region; if one assumes that the geographical pole is also in the same position at that time the peculiar climate conditions can be explained, for the equator would then pass through North America and Europe.

(Schwarzbach, 1961b: 261–262)

Turning to paleowinds, he referred to papers by several German workers. He noted that Poole (1957) favored broadening the belts of trade winds extending them further to the north, while Shotton (1956), Opdyke and Runcorn (1959), and Schove, Nairn, and Opdyke (1958) preferred to move continents. He (1961b: 262) concluded, "many more observations of wind directions are required." He (1961b: 262) supported Wegener's view about the proposed Late Paleozoic glaciations in North America (I, §3.10, §3.12, §3.15) and that they and the Squantum Tillite (§1.2) "do not fit well into the climatic picture of early times. They could at most have originated from mountain glaciers, but certainly not from continental sheets." He criticized Stehli's fixist argument (RS2).

Stehli made use of the various species in the Permian fauna to infer a cool climate in what are now polar regions, but in doing so he has grossly underrated the element of chance involved in finding fossils.

(Schwarzbach, 1961b: 262)

Schwarzbach's views remained unchanged in *Das Klima der Vorzeit*.

Modern investigations of paleomagnetism have furnished unexpected arguments in favor of shift of the poles . . . Another important result is that pole positions, as estimated in different continents, are somewhat different. This can only be interpreted as showing that the position