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Henry R. Frankel
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THE CONTINENTAL DRIFT CONTROVERSY

Volume I: Wegener and the Early Debate

Resolution of the sixty year debate over continental drift, culminating in the triumph of plate tectonics, changed the very fabric of Earth Science. Plate tectonics can be considered alongside the theories of evolution in the life sciences and of quantum mechanics in physics in terms of its fundamental importance to our scientific understanding of the world. This four-volume treatise on *The Continental Drift Controversy* is the first complete history of the origin, debate and gradual acceptance of this revolutionary explanation of the structure and motion of the Earth's outer surface. Based on extensive interviews, archival papers, and original works, Frankel weaves together the lives and work of the scientists involved, producing an accessible narrative for scientists and non-scientists alike.

Wegener's theory of continental drift captured the attention of Earth Scientists worldwide. In the early 1900s he noticed that the Earth's major landmasses could be fitted together like a jigsaw and went on to propose that the continents had once been joined together in a single landmass, which became known as Pangaea, and that they had later drifted apart. This first volume describes the reception of Wegener's theory as it splintered into sub-controversies over the geometrical fit of continental margins and disjuncts between biotic and geologic provinces. Without a convincing resolution of any of the sub-controversies or physical measurement of continental drift, scientific opinion remained divided between the "fixists" and "mobilists."

Other volumes in *The Continental Drift Controversy*:
Volume II – Paleomagnetism and Confirmation of Drift
Volume III – Introduction of Seafloor Spreading
Volume IV – Evolution into Plate Tectonics

HENRY R. FRANKEL was awarded a Ph.D from Ohio State University in 1974 and then took a position at the University of Missouri–Kansas City where he became Professor of Philosophy and Chair of the Philosophy Department (1999–2004). His interest in the continental drift controversy and the plate tectonics revolution began while teaching a course on conceptual issues in science during the late 1970s. The controversy provided him with an example of a recent and major scientific revolution to test philosophical accounts of scientific growth and change. Over the next thirty years, and with the support of the United States National Science Foundation, National Endowment for the Humanities, the American Philosophical Society, and his home institution, Professor Frankel's research went on to yield new and fascinating insights into the evolution of the most important theory in the Earth Sciences.

To Paula

THE CONTINENTAL DRIFT CONTROVERSY

Volume I: Wegener and the Early Debate

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Foreword

I have been asked by Prof. Frankel's publisher to provide a brief foreword to this first volume of his definitive history and philosophical study: *The Continental Drift Controversy*. I am well aware, speaking as a biographer of Alfred Wegener, of the immense difficulties that faced Prof. Frankel in undertaking to detail this complex and fascinating story.

The debate over continental drift has the same role and stature in the history of the earth sciences as the debate over Darwinian evolution in the history of the life sciences, and the debates over relativity and quantum theory in the history of physics. In the largest sense, the history of earth science, the history of biology, and the history of physics in the 20th century are all histories of the consolidation of opinion and the formation of broad consensus -- that these theories were the best way to organize and advance these sciences.

When we look at the ways the history of these three scientific realms has been written, we are immediately aware of a striking asymmetry. While the history of evolutionary biology, and the history of relativistic and quantum physics, are today conducted on an industrial scale, with (literally) thousands of active researchers, the history of earth sciences can boast (at best) a few score scholars at work at any one time. While the study of the lives of Darwin and of Einstein are burgeoning industries in and of themselves, with hundreds of contributors actively involved at any one time in sifting the most minute details of their thought and careers, most major figures in the earth sciences have never been considered biographically at all.

It is therefore the more remarkable that Henry Frankel has accomplished, in this and the three succeeding volumes of his *The Continental Drift Controversy,* an historical task that many would judge, on its face, to be impossible. Working as a single investigator for more than 35 years, he has produced *on his own*, a comprehensive multivolume history of a debate every bit as complex and intricate as those that characterize the emergence of modern evolutionary biology and modern relativity and quantum physics. The work before you is, however, not a preliminary study, not a tentative sketch, not an essay, but an abundantly documented and definitive history of the debate over continental drift from its beginnings to its final resolution.

There is more. Frankel's work here captures not only this fundamental transformation in the theoretical content of earth science in the 20th century, he also chronicles and captures an equally fundamental shift in the way the earth sciences, and indeed all sciences, are conducted. If the careers of Frank Taylor, and Alfred Wegener (the early exponents of the theory of continental motion in the 20th century) might have raised hopes that we could write the history of the earth sciences in the same way we write the history of biology and the history of physics, concentrating on Darwin, and concentrating on Einstein, these hopes gave way quickly. By the 1930s, and certainly in the postwar era, the debate over continental drift was no longer associated with the name of Wegener, or with his particular theoretical ideas. Working from this historical truth, Frankel demonstrates the amplitude and multifocal character of the emergent debates in middle decades of the 20th century. There were many important players, but coordinated research efforts were rare. Problems and confusions were abundant; satisfactory solutions were elusive.

When Alfred Wegener wrote, in the final edition of his work: *Origin of Continents and Oceans* that: "the Newton of drift theory has not yet appeared," he expressed a widespread historical conviction concerning the outcome of significant debates in the history of science. According to this paradigm, eventually, in every science, all major problems will be resolved by the emergence of a single figure, a "Newton." Wegener was convinced that such a figure must appear. Yet this figure never emerged: there is no Newton of continental drift, no Newton of plate tectonics. There is no single theorist to whose name we may attach the solution of all the major difficulties, the resolution of all the significant anomalies, the pointing of the one way forward. Frankel has clearly seen this and not tried to invent a fictional Newton for continental drift.

Frankel's active grasp of the new way that major theoretical shifts happen – without a guiding genius – is the most remarkable aspect of this book. In modern and contemporary science, governed by multi-author papers, multiyear research programs, intercontinental consortia, coordination of disparate subfields, and science by committee, final agreement is achieved through allegiance of a vast community of investigators to a series of techniques and findings, not to a name or an individual. To tell the story this way is to tell the story of how science is *now* done, and not to wish for a fairytale history in the present, that would mimic heroic science in the past.

Faced with a field of scientific endeavor lacking a single dominant theorist, and therefore without a single individual whose papers one might study, whose work one might trace, whose correspondence one might follow, whose ideas one might highlight, Frankel undertook the necessary labor: he actually pursued the daunting task of reading almost all the theoretical literature pertinent to this question across a span of 60 years. Having oriented himself to the literature, he contacted every principal player in the world who was still living, and interviewed as many as would speak with him. Some were initially reluctant, but as the years went by it became more and more evident to everyone in the earth science community that Frankel's history would be the definitive history of that debate, and not to speak with him was to volunteer to be left out. The project grew in size, scope, and complexity as the years went on, but Frankel has resolutely pursued a consistent and measured strategy.

To deal with the manifold conceptual complexities of this continental drift debate Frankel has developed a typological approach to problems, difficulties, and solutions. Here the training and instinct of the philosopher have amply supported the work of the historian. Geologists are accustomed to thinking in terms of problems and solutions, and because there is no "lower bound" to their curiosity about the earth, they are exquisitely talented at generating challenges to any explanatory hypothesis on any scale -- right down to the molecular, and right up to the cosmic. The result is debates that are long, intricate, fractious, and difficult to follow. Frankel's typological approach renders these matters comprehensible where they might otherwise be bewildering.

The philosophical and historical development of a major scientific controversy can, of course, be told schematically and compactly, but to do so sacrifices nuance and complexity. Since this nuance and complexity is precisely what makes the debates interesting and allows us to see how sciences actually work, Frankel determined to produce a *histoire raisonée*, told as much as possible in the words of the principal thinkers and controversialists and preserving their unique diction and approach and the manifold variations in their particular concerns, while ordering their disputes in a way that is readily comprehensible. It is difficult for me to express how complicated this task must have been and how brilliantly Frankel has achieved it.

Readers will, I think be grateful to Frankel for the calm and measured manner in which he has written this work. Most previous writers, faced with the theoretical complexity of this debate, have exploded into adjectives and adverbs accompanied by much arm waving and antic expostulation. This curse has beset almost every popular book ever written on this topic. Here instead the reader will find a well constructed and gripping narrative, which preserves the complex scientific detail, but invites one in to this fascinating world and helps the reader patiently to find a way through its labyrinth. Frankel is a wonderful guide and worthy of your trust.

Mott Greene
John Magee Professor of Science and Values, University of Puget Sound
Affiliate Professor of Earth and Space Sciences, University of Washington

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Abbreviations

AAPG	American Association of Petroleum Geologists
AGU	American Geophysical Union
ANZAAS	Australian and New Zealand Association for the Advancement of Science
APW	Apparent polar wander
BAAS	British Association for the Advancement of Science
CSIRO	Commonwealth Scientific and Industrial Research Organisation
ETH Zurich	Eidgenössische Technische Hochschule Zürich
FRS	Fellow of the Royal Society (London)
GSA	Geological Society of America
IAU	International Astronomical Union
IGY	International Geophysical Year
IPS	Institute of Polar Studies
IUGG	International Union of Geodesy and Geophysics
Lamont	Lamont Geological Observatory
NAS	National Academy of Sciences (USA)
NSF	National Science Foundation (USA)
RAS	Royal Astronomical Society (UK)
RS1	Research Strategy 1
RS2	Research Strategy 2
RS3	Research Strategy 3
S_2A_3	South African Association for the Advancement of Science
SEPM	Society of Economic Paleontalogists and Mineralogists
Scripps	Scripps Institution of Oceanography
USGS	United States Geological Survey

Introduction

It was in the mid-1970s when I originally became interested in the controversy surrounding the continental drift theory of Alfred Wegener, thinking of it as a possible subject for testing philosophical accounts of scientific growth and change. There had just been a scientific revolution in the Earth sciences that no philosopher of science had even begun to examine, and in the late 1970s and 1980s I wrote several papers on the drift controversy testing some of these accounts.[1] I also became interested in the controversy for its own sake, and wrote papers concerned with key aspects: the very different reception of Wegener's ideas among specialists; debates over: the origin of the vast Permo-Carboniferous glaciations that intermittently clothed much of the southern continents from 300 to 250 million years ago; the broken (or disjunctive) distribution of past and present-day life forms; the early paleomagnetic work of the 1950s that re-energized the controversy; the development in the early 1960s of the notion of seafloor spreading and of its corollary the Vine–Matthews hypothesis.[2] I then thought of working them up into a book but realized that I still had only a minimal understanding of what later transpired, no feeling for the way the controversy was resolved. Like many others at the time, I underestimated paleomagnetism's support of continental drift, and I did not understand some subtle and some not so subtle features of plate tectonics.

Now, twenty-five years later, after studying the continental drift controversy to its conclusion, Cambridge University Press has brought out my account in four volumes. It is an attempt to tell the story from end to end in some detail, from its initiation in the early twentieth century to its conclusion in the late 1960s as a general theory describing the mobile nature of the Earth's surface features – plate tectonics. The story is of new discoveries and ideas, and it is also a social history in which the operative workers and institutions are identified as they appear and their stories told.

The continental drift or mobilism versus fixism controversy, as it is sometimes called, involved almost all branches of Earth science and no single person is competent in them all. So is it sensible that a philosopher, yes with a degree in biology but with no direct experience of research in Earth science, should attempt such a task? I do have a long-standing interest in scientific reasoning and in theory choice, the initial reasons why I was attracted to the controversy. Within the mobilism controversy there were many sub-controversies and I was struck by the similarities in the manner in which participants, whatever their interests, attacked their opponents' solutions and defended their own. I was also struck by how their arguments centered about the identification of difficulties faced by their opponents' arguments, and by proactively imagining the difficulties that might be raised against their own. My analysis is described in Volume I, Chapter 1.

I could not have written this account without the assistance of many of the participants. My approach was to read through their various papers, and then send them questions about how they came to undertake their investigations and why they made, or did not make, certain claims. But, they often did much more: they gave me tutorials, enabling me to understand better their various fields of enquiry. The narrative begins with an account of geological theorizing in the early twentieth century and is followed by an account of the drama of Alfred Wegener's life (Volume I, §2.2 to §2.5) and his theory of continental drift, which I summarize here.

Over a period of seventeen years in approximately a dozen publications between 1912 and 1929 Wegener described his revolutionary theory of continental drift. He imagined continents floating on a denser substrate through which they plowed, impelled by Earth's tidal and rotational forces. He placed his mechanism at the center of his theory. Working backwards in time he closed the Atlantic, Antarctic, and Indian oceans and assembled the continents like pieces of a jigsaw into a single landmass, which he called Pangea (Volume I, §3.2). According to Wegener, Pangea lasted from the Late Carboniferous to the Cretaceous, from about 300 million to 100 million years ago. He reconstructed the breakup of Pangea into the present continents and mapped their drift to present positions. At this stage his frame of reference, his grid of latitudes and longitudes, was arbitrary, and he adopted the convention of keeping Africa fixed and moved other continents relative to the grid (Volume I, Figures 3.1 and 3.2). This was a grid of convenience; it was not an authentic geographical grid appropriate to the times in question. Later, with his father-in-law Vladimir Köppen, Wegener used the distribution of climate-sensitive deposits to determine geographic latitudes for his maps (Volume I, §3.15; Figures 3.6, 3.7, and 3.8). The manner in which Wegener fitted continents together and justified that fit by appealing to evidence of ancient climates is of great interest historically. He fitted them geometrically by their shapes and matched them by their geological features, as workers do today. He noted that the matches in his reconstructed Pangea were generally excellent. He then placed them in appropriate latitude zones, relative, that is, to the geographical pole at that time. Jigsaw and latitude complemented one another. This agreement (or consilience) between data from diverse sources that were independent of one another was perhaps Wegener's strongest argument (Volume I, §3.2); in different contexts, consiliences such as this were a recurrent theme throughout the mobilism debate.

During the 1920s, the theory of continental drift was widely discussed. For a brief period, many saw virtue in Wegener's ideas. As things settled down, it became clear that its reception by individuals correlated strongly with their specialization and region of study – for example, those who worked on the Permo-Carboniferous glaciations of the continents of the Southern Hemisphere favored drift, whereas those who worked within what they saw as geologically self-contained regions, especially in North America and the Soviet Union, rejected drift. I describe these important relationships in Volume I, Chapters 6 through 9.

By the 1930s, Wegener's progressive ideas had lost ground to the doctrines of fixed continents and oceans. Especially hurtful was the demonstration that Wegener's mechanism would not work. As a result, over the next forty years few adopted continental drift theory or used it as a basis for research or teaching; it was widely ignored or reduced to a footnote in many geology courses and texts. Especially was this true not only in North America but elsewhere too. Volume I covers this "classical stage" in the drift debate.

It was in the early 1950s, when acceptance of continental drift was at a low point, and discussion of it going nowhere, when out of the blue, paleomagnetists breathed new life into an essentially moribund controversy. Paleomagnetists had taught themselves how to determine the history of the geomagnetic field as recorded by the magnetism of rocks, and how to construct motions of the migration of Earth's rotational axis pole relative to continental blocks. These motions were expressed as paths of apparent polar wander (APW) relative to each fixed continent (Volume II, Chapter 3): They can also be expressed as motions of continents relative to a fixed pole. They learned how to combine their work with the findings of paleoclimatologists, establishing generally excellent agreement between their inferred latitudes and the evidence of past climates in the same continent (Volume II, §3.12; Volume II, §5.10–§5.14; Volume III, §1.7; Volume III, §1.18). They found huge disagreements between polar wander paths from different continents that made no sense unless continents had moved relative to one another in much the same way as Wegener's theory required. It was a rough blow to fixism.

This work led to a revival of interest in continental drift in Britain, the Soviet Union, South Africa, and Australia and prompted R. A. Fisher to remark at the time, "I think a lot of geologists must be timidly peering out of their holes on hearing the strange news that geophysicists are talking about continental drift." This work led, by the end of the decade, to the first physical confirmation of continental drift (Volume III, §2.17).

Critics may say that I have given too much space, certainly, proportionately, more than others have, to the 1950s paleomagnetic revival of the drift debate. Anticipating work that I shall describe in a moment, there are four reasons for doing so. (1) Apart from a short early review, the history of this phase of the controversy has never been properly described. (2) The narrative would be truly incomplete were I not to record how those who did not favor continental drift (they were the majority) took little or no notice of its new paleomagnetic support; variously, they did not understand it, failed to read the key supportive papers, or rejected it on the basis of hearsay. (3) Reversals of the geomagnetic field were discovered by paleomagnetists working on land, and this was essential to what transpired later. Had paleomagnetists not discovered reversals by direct observation, first in stratigraphic sequence on land and then in deep ocean cores, showing them to be a general property of Earth's magnetic field, how would they have been recognized at sea and the kinematics of plate tectonics thereby quantified? It is hard to imagine plate tectonics without reversals of the geomagnetic field. Certainly progress would have been very slow. (4) In the 1920s Wegener and Köppen established the geographical frame of reference for continental drift. Likewise in the 1950s, paleomagnetists laid the groundwork for establishing quantitatively the geographic framework for plate tectonics by determining the motions of continents relative to the spin axis. Their work is described in Volume II and the first two chapters of Volume III.

While interest in continental drift was being revived by work on land, the tectonics of oceans floors was little known. In the later 1950s and 1960s there was a flood of data and ideas about the ocean floors.[3] Especially critical were studies of what turned out to be largest mountain ranges on Earth, albeit underwater – the mid-ocean ridges. Several suggestions were proposed to explain their origin (Volume III, Chapters 3 through 6). Seafloor spreading (1960) proposed that these ridges marked the places where hot material welled up, cooled and became rock, just as lavas on land do. To balance this new crust being created at the ridges, oceanic crust was thought to be descending in subduction zones beneath the numerous deep ocean trenches and being resorbed into Earth's mantle (Volume III, §3.20). This idea was confirmed in 1967 (Volume IV, §7.4). Soon the ridges were found to have quite remarkable tell-tale magnetic anomalies. So what was more natural (although revolutionary and not immediately accepted) than to propose, as it was in 1963 and confirmed in 1966, that these anomalies corresponded to reversals in the geomagnetic field recorded as these hot upwelling lavas cooled and became magnetized just as paleomagnetists on land had shown they did (Volume IV, Chapters 2 through 6). Furthermore, when these reversals recorded by seafloor spreading (Volume IV, §6.6, §6.8) were calibrated by studies, first on land, of radiometrically dated reversals (Volume II, §8.15; Volume IV, §6.4), and then with astonishing consistency of reversals in deep-sea sediment cores (Volume IV, §6.5), they were used to map the motions of ocean floor (Volume IV, §6.6, §6.8, §7.6). Once rates of seafloor spreading were determined and estimates were made to determine, for example, when the Atlantic opened, they were found to agree with estimates based on land-based paleomagnetic findings (Volume IV, §6.6, §6.8, §7.6). These consiliences enhanced the strengthened support for seafloor spreading and showed the success of the paleomagnetic case for mobilism developed during the 1950s.

Enigmatically the mid-ocean ridges were offset by huge fracture zones sometimes thousands of kilometers in length and the motion across them appeared to be in the wrong sense. In 1965 these fracture zones were recognized as a completely new sort of structure, called transform faults, and, in 1967, their existence was confirmed (Volume IV, Chapters 4 through 6). These allowed major structures to be kinematically linked and many fundamental crustal boundaries to be recognized.

The great fracture zones of the northeast Pacific Ocean were originally thought to be great circles, circles that have their centers at the center of the Earth (Volume IV, Chapter 7). On closer inspection, they were found to be not small, but great circles, circles which, like lines of latitude, had their centers along Earth's axis of rotation. This was also found to be so for the fracture zones associated with active mid-ocean ridges. For any particular ridge, the small circles that define the fracture zones along it were found to be concentric about a point on the Earth's surface, called the Euler pole or pivot point (Volume IV, Chapter 7). Tellingly, fault plane solutions to the earthquakes occurring along these ridge-associated fracture zones (they are transform faults) gave slip vectors that were essentially horizontal and indicated that current relative motions across fracture zones were in a strike-slip sense and occurred about the same pivot point. Most tellingly, these present-day relative motions were found to be consistent with motions over the past tens of millions of years determined from the analysis of marine magnetic anomalies.

These discoveries led directly to the theory of plate tectonics (Volume IV, Chapter 7). It is a kinematic theory which says that the Earth's lithospheric shell is divided into a number of large plates that are moving relative to one another along three sorts of boundaries: extensional at the active mid-ocean ridges, compressional at the great subduction zones, and strike-slip along the great transform boundaries. Plates are composed mainly of oceanic lithosphere, although most of them contain a large segment of continental lithosphere, the great landmasses.

As just mentioned, land-based paleomagnetic techniques determine the position of land-masses relative to the geographical pole, most importantly their latitude (Volume II, Chapter 3). The techniques of plate tectonics determine the motions of plates relative to one another; the method is generally blind to past latitude and provides no record of position relative to Earth's axis of rotation, except in those situations when marine magnetic lineations or profiles can be exploited to provide paleolatitudes. Hence the two methods are complementary, land-based paleomagnetic results providing the geographic frame of reference for plate tectonics reconstructions.

Plate tectonics offers no explanation for the forces that drive plates, a point that was made abundantly clear by the discoverers of plate tectonics. The great irony of the mobilism controversy is that for over forty years the lack of an acceptable mechanism was generally regarded as a strong reason to reject continental drift. Ironically, it is remarkable that plate tectonics was accepted almost immediately even though it is a kinematic not a dynamic theory. Once accepted, the lack of mechanism was no longer a difficulty but an advantage, freeing the discussion of the relentless and unnecessary burden it had carried for so long. Many very different and

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independent measurements and analyses had showed that large-scale horizontal tectonics were a reality; they were no longer in doubt and objections to their existence because there was no acceptable mechanism became groundless. Indeed, in retrospect, the perceived lack of mechanism never was a good reason to reject drifting continents, even if there were good reason to reject certain proposed mechanisms.

Notes

1 Frankel (1978, 1979a, 1979b).
2 Frankel (1976, 1978, 1980b,1981, 1982, 1984a, 1984b).
3 This flood of new information about the ocean floors was made possible by massive governmental support of marine geology prompted by defense concerns. See Schlee (1973), Bullard (1975b), Menard (1986), Strommel (1994), Rainger (2000), Hamblin (2005), and Doel *et al*. (2006).