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How the mobilism debate was structured

1.1 The three phases of the continental drift controversy

The *continental drift*, or more generally the *mobilism* controversy lasted sixty years. It was the longest and most important controversy of the last century in Earth science, and one of the more important in all of science in that period.

Within it were many sub-controversies and their important feature was that some were long-lived too, even surviving the controversy itself. For example, the apparent conflict noticed in the 1920s between the "Permian" glacial Squantum Tillite of the Boston region and the equatorial situation assigned to that part of North America by drift theory was not resolved until the beginning of the twenty-first century, when radiometric dating assigned it to the Late Precambrian glaciation (Snowball or Panglacial Earth) (Thompson and Bowring, 2000). Also outliving the controversy and of much more general significance was the debate about the mechanism of continental drift, which began in the early 1920s and, although much progress has been made, is still not entirely resolved ninety years later. Throughout much of the controversy a solution to the mechanism question was regarded by most workers as essential, but at the end it was jettisoned and left in the wake of plate tectonics. Not only was it left unresolved by plate tectonics, it had to be first set aside in order for progress to be made – set aside as a problem for the future. In the end, the avalanche of evidence for the geometrification of tectonics and the convincing kinematic picture it gave overwhelmed concerns about dynamics – the mechanism difficulty, as I shall call it.

The durability of the sub-controversies lends a certain repetitiveness to the history of the controversy. Repeatedly problems were thought to have been laid to rest and then revived and discussed all over again and so on, and this is why, in the narrative, they and the scientists involved appear and reappear time and time again. These are not inadvertent repetitions (although I am sure there are some of those too), they are a characteristic feature of the story – how it unfolds. In these three volumes, scientists and their work are introduced in rough chronological order beginning in Chapter 2.

In this first chapter I comment on what I believe is a remarkable similarity in the arguments used throughout the controversy. This common thread provides a degree

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of coherence to the entire debate, and, I think, is a means by which it can be understood. Some readers may find the formalities of this chapter a little tedious. Perhaps they should skip to Chapter 2, and if, after some time, they feel the need of help finding this common thread then they could return here.

It was Émile Argand, one of the first converts to continental drift, who first introduced the terms "mobilism" and "fixism," and I view the controversy in terms of these two competing traditions. Fixists maintained that, except perhaps very early in Earth's history, continents remained fixed in the same place relative to one another. Fixists did not however always agree among themselves. Some claimed that the axis of rotation moved relative to the Earth as a whole (polar wander) so that although continents remained fixed relative to each other, they have changed their positions relative to the geographical poles. Nor were fixists agreed as to how continents were formed – whether they increased in size, how mountain belts formed, and how intercontinental biotic and geologic disjunctions (disjuncts) arose. Disjuncts are occurrences of similar phenomena now separated by wide expanses of land or, more commonly, ocean where such occurrences are wholly absent. Some fixists, the landbridgers, explained biotic disjuncts by supposing that species of life migrated across former transoceanic landbridges. Others, who for other reasons were called permanentists and who were especially prevalent in North America, abandoned landbridges in favor of long isthmian connections and island hopping as a means by which land organisms migrated across oceans. In contrast, most mobilists declared that continents have changed their position relative to one another and relative to the geographic poles, that continents have undergone horizontal displacement, changing their latitude and longitude over time. A very few mobilists, who mustered under the flag of mobilism, favored Earth expansion, claiming that even though the latitude and longitude of continents have not changed, their distances from one another have increased as Earth has expanded.[1]

The debate between fixism and mobilism evolved through three phases. Fixism was almost universally assumed until Frank Taylor in 1910 and Alfred Wegener in 1912 introduced their mobilist theories of continental drift (Taylor, 1910; Wegener, 1912a, 1912b) and inaugurated the first or classical phase. Throughout this phase, fixism remained ascendant, although a number of old-time drifters and a few new converts carried the flag for mobilism. During this phase, which is the subject of Volume I, participants argued over who had the better solution to a nest of problems, among them: explaining the congruency of opposing continental margins especially across the Atlantic basin, explaining biotic and geologic disjuncts, and explaining the origin of Tertiary mountain belts and the vast far-flung Permo-Carboniferous glaciation. Although mobilists were very greatly outnumbered, neither tradition gained a decisive overall advantage during the classical phase. Mobilists offered better solutions than fixists to some problems, and fixists offered better solutions to others, but every solution offered was plagued with difficulties. The standard operating procedures of both mobilists and fixists was to propose new

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solutions to problems in terms of the basic tenets of their own tradition, to attack their opponents' solutions by raising difficulties against them, to defend their solutions against attacks by attempting to remove difficulties, and to argue that their own solutions were preferable to those of their opponents. These procedures were so prevalent that I shall call them research strategies (RS), and later in this chapter describe some of their more prominent features. Introducing the idea of a difficulty-free solution, I shall argue that none were produced during the first phase. As a result, neither mobilists nor fixists had to admit defeat; they were never obliged to acknowledge that their position was no longer tenable because their opponents had succeeded, whereas they had failed to produce a difficulty-free solution.

The second phase of the controversy, the subject of Volume II and the first two chapters of Volume III, is marked by the rise in the early 1950s of paleomagnetism, initially in the United Kingdom, as a result of information obtained at the Department of Geodesy and Geophysics at the University of Cambridge and the Physics Department of Imperial College, London. Paleomagnetists quickly developed and articulated their new procedures. They acted in accordance with these standard research strategies to garner support for mobilism, to anticipate difficulties that might be raised against their work, and to remove difficulties as they were raised by others. By about 1959, British, Australian, and South African paleomagnetists had developed an explanation of the accumulating paleomagnetic data which showed that the continents had changed their positions relative to each other more or less as Wegener had proposed; they gave fulsome support to mobilism. Their work signaled that all might not be well with the doctrine of fixism. It definitely rekindled interest in mobilism, but not many outside paleomagnetism were convinced that mobilism merited acceptance. I shall argue that the paleomagnetists' explanation of their data, despite its often uncomprehending and hostile reception by all but a few, actually warranted the acceptance of mobilism. I shall argue that these paleomagnetists had developed an essentially difficulty-free mobilistic solution, while fixists still had not provided any such justification for their continued support of the traditional view.

The third and final phase of the controversy, the subject of most of Volume III and Volume IV, began in the mid-1950s when there was a massive influx of new information about the seafloor obtained through the use of new geophysical techniques and instruments made possible by extensive funding for defense purposes.[2] This phase began in earnest about a decade after World War II and intensified during the early stages of the Cold War. This new information was gathered primarily at Columbia University's Lamont Geological Observatory, Palisades, New York; Scripps Institution of Oceanography, La Jolla, California; Woods Hole Oceanographic Institute, Woods Hole, Massachusetts; and the Department of Geodesy and Geophysics at the University of Cambridge. Various fixist and mobilist theories were developed to explain this new information. One mobilist theory proposed that new seafloor is created at the center of mid-ocean ridges, where it separates, moving away

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sideways creating new seafloor. This theory, seafloor spreading, spawned two key hypotheses. One sought to explain the remarkable striped patterns of marine magnetic anomalies as records of reversals of the geomagnetic field resulting from seafloor spreading. The other explained the movement of seafloor between ridge-offsets by what became known as ridge-ridge transform faults. Confirmation of the former in 1966 and the latter the following year led to the overnight acceptance of mobilism by most scientists working within the controversy. I shall argue that both hypotheses became difficulty-free as a result of these confirmations. With this resolution, seafloor spreading and continental drift morphed into plate tectonics, which commanded swift approval throughout the Earth science community. It became the reigning theory in Earth science.

The ideas or concepts of difficulty-free solutions and research strategies employed by participants to defend their views and attack those of their opponents are embedded in my account. I now need to elaborate them.

1.2 Solutions, theories, hypotheses, and ideas or concepts

These words refer to explanations or proposals that were designed to solve problems in various ways. Solutions were proposed to solve one problem; theories too, like solutions, solved problems. Theories were designed to solve just one or, more commonly, to solve several problems; they generally provided a common solution to several problems, and within them each solution had a common element. Wegener's theory of continental drift sought to solve many problems and contained many solutions. Wegener's theory related together many problems by providing a common framework: Earth's continental crust was once united in Pangea, which fractured and the fragments drifted apart to form the present continents. Sometimes scientists expanded the range of a theory by using it to solve problems that it was not originally designed to solve; hypotheses may refer to solutions that were not established when proposed or to theories before they were established. Theories were hypothetical when proposed; solutions may have been. Solutions and theories may have been dismissed, or they may have become established. Sometimes, however, they still are habitually referred to as hypotheses even though they now are firmly established. In the Earth sciences, the Vine–Matthews hypothesis is perhaps the most famous example. Other proposals are labeled as concepts or ideas. Such proposals often hypothesized the existence of a new process or entity, and the most famous example from Earth science is the concept of transform faults. J. Tuzo Wilson, recognizing that there was a new kind of fault, wisely coined a new term – transform fault. When others discussed Wilson's idea, they often referred to it as a concept. Wilson's idea (like the Vine–Matthews hypothesis) was a corollary of seafloor spreading. Wilson explained the different types of transform faults, offered about a dozen examples, and argued that the existence of transform faults provided strong support for mobilism.

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Readers will understand from the above that I shall not be so foolhardy as to attempt to strictly define the words in the title of this section. To attempt to do so would be to limit their usefulness and divorce them from the very varied literature that has grown up over the past half century around the fixism versus mobilism debate. Instead I shall try my best to make myself clear from the context.

1.3 Problems and difficulties[3]

I begin with a taxonomy of *problems* and *difficulties*. Scientists addressed problems by proposing solutions and theories. As already noted, solutions were designed to solve one or perhaps two closely related problems; theories were designed to solve numerous problems, and the number of problems they solved increased as workers found new ways to apply them. Other scientists questioned these proposals by raising difficulties. I then introduce the notion of a *difficulty-free solution*, and delineate a set of *research strategies* that participants in the controversy employed to defend their own solutions and to attack those of their opponents.

Difficulties were objections that were raised against these proposed solutions and theories, obstacles that were in their way all along or placed there later by opponents. Stated in this way, there can be no difficulties without problems and their proffered solutions. *Problems* arose when scientists became puzzled by phenomena they could not explain. Sometimes more data were gathered to establish the legitimacy of a problem and to clarify it. A solution was then offered. Difficulties were usually raised by scientists with opposing views. They were also raised by supporters in the same camp and even by scientists themselves against their own solutions. Difficulties were removed either by amending the flawed solution or theory, or by showing that the raised difficulty was itself unfounded, a phantom difficulty. The mobilism controversy was replete with proffered solutions to problems, and with real and phantom difficulties.

1.4 First and second stage problems

There are first and second stage problems. First stage problems arose through the discovery of a puzzling phenomenon. A scientist offered an explanation by postulating a hypothetical process. For instance, scientists noticed and were intrigued by the similarity in shape of the opposing coastlines of South America and Africa and proceeded to explain it by suggesting that they were once united into a single landmass, which split into two parts that drifted apart. Scientists then wondered, how did the single landmass split apart? This new problem was a consequence of solving the first stage problem. Secondary problems have as their subject matter entities or processes that are invoked to solve primary problems. Secondary problems cannot arise until a solution has been offered to a first stage problem.

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1.5 Four examples of first stage problems

(i) *Permo-Carboniferous glaciation*. By 1910 many Earth scientists believed that large areas of the Southern Hemisphere continents and peninsular India had been glaciated at times during the later Carboniferous (~320–300 million years ago) and Permian Periods (~300–251 million years ago) because they had found thick deposits likely of glacial origin of this age in Australia, in southern Africa and South America, and in India. Evidence of Late Paleozoic glaciations had not, at the time, been described from Antarctica. The extent and location of these deposits was startling, and explanations were sought for them. Workers were particularly puzzled by the presence of glacial deposits in India within the present tropics. To make matters worse there was good reason to believe that the climate in the Northern Hemisphere continents, some of them approximately antipodal to glacial occurrences in the Southern Hemisphere, had been mild or even tropical throughout much of the Permo-Carboniferous because of the occurrence there of thick limestones, coral reefs, vast coal deposits of tropical aspect, and evaporites. Wegener solved this problem by invoking continental drift. He argued that the continents had been united during the Permo-Carboniferous with the northern continents in low northern latitudes and the South Pole just south and east of Africa. This arrangement placed the South Pole at the center of the glacial deposits and kept them within mid to high southern latitudes. Thus Wegener could claim that the glaciation had not extended to regions formerly located on or near the equator; such argument later allowed him and Wladimir Köppen, a leading climatologist, to argue that Earth's climate during the Permo-Carboniferous was divisible into latitudinal zones very much as it is today.

(ii) *Past and present biotic disjuncts*. Before Wegener began to speculate about continental drift, paleontologists and biogeographers had found that many similar ancient terrestrial life forms had inhabited regions now widely separated from one another. To explain these disjuncts, they proposed the existence of landbridges that served as migratory routes across oceans. The bridges later sank, becoming part of the seafloor. Wegener realized that continental drift offered an alternative solution, and argued that disjuncts arose because regions formerly close together had since moved apart.

(iii) *The scattered paleopole problem*. In the early 1950s the rapidly emerging field of paleomagnetism attracted attention. Paleomagnetists studied the natural remanent magnetism of rocks to obtain a record of the orientation of Earth's magnetic field at the time of formation. A pattern quickly emerged. Poles corresponding to the directions of remanent magnetism of samples from Upper Tertiary and Quaternary rocks (the past 25 million years) were, within error, coincident with the present geographic poles: the time-averaged geomagnetic field could be represented by a dipole at the Earth's center directed along the axis of rotation. Poles or paleopoles are the points at which the axis of this

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dipole intersects Earth's surface. Paleopoles calculated from the directions of Early Tertiary (Paleogene) and older rocks differed greatly from the present geographic pole, and it was soon found that poles from the same continent fell sequentially on curved paths, later called apparent polar wander (APW) paths; samples of the same age from the same landmass (strictly each continental craton) gave similar paleopoles, but samples of differing age did not. Paleogene and older samples from different landmasses gave different poles and APW paths. Paleomagnetists realized that these diverging APW paths from different landmasses could not be reconciled without invoking continental drift, and they very quickly recognized that Wegener's theory conceived forty years earlier could explain the main features of their observations.

(iv) *The reversal problem*. Another paleomagnetic problem that gained considerable attention throughout the 1950s arose from the discovery, dating back to the turn of the century, that many rocks have a remanent magnetism of polarity opposite (antiparallel) to that of the present Earth's magnetic field. This is called reversed remanent magnetism; rocks that have it are said to have *reversed polarity*. Two competing solutions were offered: reversed polarity could have been caused by some mechanism whereby rocks become magnetized spontaneously in the opposite sense from the ambient field (*self-reversal*), or by reversals in polarity of the Earth's magnetic field. By the late 1950s a very good case could be made that field reversal was the correct solution, but this was not generally accepted until the mid-1960s when it promptly became a cornerstone of the theory of plate tectonics.

1.6 Four examples of second stage problems

(i) *The mechanism problem of continental drift*. Wegener, having postulated extensive horizontal displacements of continents, sought to identify the forces that caused them. To solve this secondary problem, he postulated two mechanisms: flight from the poles, which sought to explain equator-ward drift of continents, and tidal force to explain their westward drift.

(ii) *The mechanism problem of plate tectonics*. What are the processes responsible for plate motions? Some workers proposed mantle convection that drags along lithospheric plates; others tied convection directly to lithosphere plates, invoking gravitational forces that directly pull lithospheric plates down subduction zones or forces that push them away from spreading ridges.

(iii) *The mechanism problem of landbridges*. Paleontologists and biogeographers who proposed landbridges to account for the primary problem of biotic disjuncts thought about what might have caused landbridges to sink after they were no longer needed as migration routes.

(iv) *What caused reversals of magnetization?* As noted above, reversals of magnetization could be caused by spontaneous self-reversal or by reversal of the

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geomagnetic field. Each spawned secondary problems. Workers who opted for spontaneous self-reversal had their various mechanisms. Workers who preferred field reversals offered theories of reversal of the geomagnetic field, although it is only very recently that realistic field reversal theories have become possible.

1.7 Difficulties

Scientists regularly identified and addressed problems during the controversy, and it was no small accomplishment to construct solutions and theories to explain them. The hardest task of all, however, was to construct solutions that were free from difficulties. Indeed, I claim that in the mobilism controversy the identification and removal of difficulties played a very central role. To think, as some have maintained, that the raising of difficulties was silly polemics, and their removal was a mopping-up operation left to the ungifted but hard-working scientists is, I believe, to misunderstand completely what actually happened during the mobilism debate; participants engaged in the controversy expended considerable skill and imagination raising difficulties against solutions proffered by their opponents and removing those raised against their own. Time and time again throughout the mobilist controversy, the identification and removal of difficulties were the keys to progress, not just routine filling in the gaps.

Difficulties raised fell into two main categories, *data* and *theoretical difficulties*. *Data difficulties* arose when the data used to evaluate a solution or theories were found to be suspect. *Theoretical difficulties* arose when a proposed solution or theory was plagued with inconsistencies or ambiguities. There were three general sorts of data difficulties, which I call *unreliability*, *anomaly*, and *missing-data difficulties*, and two sorts of theoretical difficulties – *external* and *internal*. I deal with each in turn.

1.8 Unreliability difficulties

Unreliability difficulties comprised a variety of real or imagined irregularities in the collection of data, its analysis, or interpretation. They include the questionable use of previously tested procedures, or of untested new ones, and the use of outdated procedures, procedures that had been superseded by new ones in the hope of raising standards. They may have involved alleged theoretical or sampling biases of the investigator. Unreliability difficulties were also directed against scientists who, while supporting their own theory, misused data of others; for example, scientists sometimes overstated the reliability of data (their own or that of others) that were particularly favorable to their solution or theory: not infrequently they ignored data not supportive of it. Here are four examples.

(i) Wegener was accused of having created an unreliability difficulty when marshaling support for his explanation of the congruency of continental margins facing

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the Atlantic. Alex du Toit, although a vigorous drifter, claimed that Wegener had mistaken the actual shape of the continents, he had ignored the fact that they extend beyond their coastlines to the shelf edge; continental shapes could only be determined from bathymetric data. Actually, Wegener had used bathymetric data, and it was du Toit's criticism that was incorrect.

(ii) Paleomagnetists working during the late 1940s and 1950s who sought to design trustworthy techniques for collecting, measuring and analyzing the remanent magnetization of rock samples encountered an unusually large number of complex unreliability difficulties. Theirs was an uphill battle, and they went to great lengths to imagine and anticipate unreliability difficulties that might later be raised against them.

There were three reasons why paleomagnetism was plagued in this way, and I shall spend a moment explaining them. The first was that within a few short years in the early and mid-1950s paleomagnetists had launched physically based, global surveys and challenged on a broad front the monolithic fixism of the classical phase. Consequently there was general incredulity that results obtained so swiftly and so readily, with such apparent ease, could be reliable and yet conflict so dramatically with the widely believed doctrine of fixism, based, as it was then thought to be, on evidence from much of Earth science.

Second, it was a young discipline without preexisting standards. Early workers designed and built their own instruments, developed field criteria to recognize and weed out potentially magnetically unstable samples, and established their own procedures to obtain, to analyze, and to verify their data. For instance, they had to design procedures allowing for the magnetic heterogeneity of a sample to ensure that their measurements were representative of the sample as a whole, and they often thought it necessary to check the general reliability of a new magnetometer by comparing results with those obtained on previous instruments. Early workers also realized the need for sound statistical techniques to determine the accuracy of data and struggled to apply them.

The third general reason why paleomagnetists had to be sensitive to reliability issues pertains to the very nature of remanent magnetism and the difficulty of extracting information about the ancient field millions of years ago. Paleomagnetists had first to determine if the remanent magnetism was sufficiently strong and then if it was stable. They had to estimate also when it was acquired, no small task. In mid-century, because radiometric dating of rocks was still in its infancy, and igneous rocks at the time were often poorly dated, paleomagnetists turned to sedimentary rocks because in general they were better dated and promised to provide a more detailed record of field behavior. They first had to determine which of the many sorts of sedimentary rocks gave coherent results and were likely therefore to yield a record of the ancient field. They soon found that sedimentary rocks possess weak remanence, are commonly magnetically unstable, some even

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changing their magnetism after collection. However, British workers soon discovered that fine-grained red sandstones or siltstones regularly gave consistent results of great age, and they exploited their discovery focusing on that lithology. A further complication was that both igneous and sedimentary rocks can become partially or entirely re-magnetized, acquiring secondary magnetizations (overprints) of uncertain ages, and early paleomagnetists had to develop field and laboratory tests to identify them. Initially, data showing signs of overprinting were rejected. Eventually procedures were developed to correct for overprints and finally for removing them altogether.

Although these early paleomagnetists usually took great care to screen data, others often questioned the reliability of their opponents' methods, arguing from a perfectionist standpoint that insufficient care had been taken in doing this or that. For others, unfamiliar with these new techniques, it required a special effort to tell which studies, if any, were reliable. Consequently, Earth scientists unfamiliar with these new studies were generally wont to take little notice or even to dismiss them. Not only were they new and unfamiliar, and not only did the new results challenge long and generally accepted fixist norms, but Earth scientists generally were aware that within the quarrelsome paleomagnetic community there were differing opinions as to the reliability of results and what they signified.

(iii) To support his postulated westward drift of Greenland relative to Europe, Wegener invoked geodetic data that were later shown to be unreliable; he overestimated their reliability. This is an example of a theorist using, somewhat recklessly, someone else's data to support his solution. It was not uncommon for data to be assumed more reliable than they really were. Cautionary remarks by the original observers may go unheeded. Data may be unreliable for reasons unknown at the time. The theorist's understanding of the data gathering process may be limited.

(iv) Wegener and du Toit were criticized for emphasizing paleontological studies that supported mobilism, and muting or ignoring those that favored fixism. The American paleontologist, George Gaylord Simpson, claimed (*circa* 1940) that mobilists ignored studies which indicated that the number of biotic disjuncts, their stock-in-trade, had been overestimated.

1.9 Anomaly difficulties

Anomaly difficulties arose if there was a tension between a solution or theory and data on which it was based; that is, if the data indicated something highly unlikely given the solution or theory, or if an incompatibility arose between the data and a prediction of the solution or theory. During the debate, the critic's main purpose in raising anomaly difficulties was to show that the solution,

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