The Recent Revolution in Geology and Kuhn's Theory of Scientific Change

Rachel Laudan[1]

Virginia Polytechnic Institute and State University

The 1960s witnessed a striking change in geology. Since at least the seventeenth century, one of the central problems of the subject had been the origin of the major irregularities of the surface of the globe--continents and oceans, mountain chains and ocean islands--irregularities that were not anticipated by most physical theories. Traditionally these features had usually been explained either as residual traces of events occurring during the very early history of the globe, or as the result of vertical movements of the earth's crust, caused, for example, by changes in the heat budget. The last two decades have seen an end to all this. The vast majority of geologists now believe that these irregularities largely result from the lateral movement of thin rigid plates covering the earth, a theory now known as "plate tectonics", but a theory which also has obvious parallels with the hypothesis of continental drift, in which it was postulated that continents can move laterally. The historical relations of these theories have been explored by a number of authors. ([4], [9], [10], [11], [13], [19], [20], [23], [24], [29], and [30]). Turning to the history and philosophy of science for an account of scientific change that could encompass this development, geologists almost without exception dubbed it a "Kuhnian revolution". J. Tuzo Wilson [27], the Canadian geophysicist, was perhaps the first to argue for this analysis, and his conclusions were reiterated by three of the first four histories of the subject to appear. In 1973, Ursula Marvin claimed that "the story of continental drift as a geologic concept, with its slow, tentative beginnings and violent controversy, followed by the spectacular band-wagon effect which has swept up the majority of earth scientists, bears out in dramatic fashion a thesis developed by Thomas S. Kuhn."([18], p. 189). For her, the most important feature of Kuhn's analysis was his rejection of the notion that "science progresses in a linear manner by the steady increment of shared knowledge." ([18], p. 189). Allan Cox was equally impressed by the non-linear development of geology, as well as the incommensurability of pre- and

PSA 1978, Volume 2, pp. 227-239

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192471 Published online by Cambridge University Press

post-plate tectonic research, and the heuristic value of plate tectonic theory in directing further investigation. The development of plate tectonics, he concluded, "fits the pattern of Kuhn's scientific revolutions surprisingly well."([4], p. 5). Arthur Hallam, in turn, announced that, with respect to the earth sciences, "it is quite clear that plate tectonics is the currently held paradigm."([13], p. 107). Although rather more critical of a Kuhnian analysis than the former authors, he nonetheless concluded that "the earth sciences do indeed appear to have undergone a revolution in the Kuhnian sense," ([13], p. 108) and he urged that "we should not be misled by the fact that, viewed in detail, the picture may appear somewhat blurred at the edges."([13], p. 108).

The major attack on this interpretation has come from David Kitts, who has argued that by assuming that a Kuhnian revolution has occurred in geology "we may miss something significant about the history of geology and, more importantly, something fundamental about the very nature of geologic knowledge."([17], p. 115). In Kitts' view, indeed, Kuhnian revolutions <u>cannot</u> occur in geology. In order to establish this point he makes the following claims. For the derivation of singular historical statements, which he takes to be the main aim of geology, geologists depend on a body of "fundamental and comprehensive scientific principles."([17], p. 115). So strong is this dependence, Kitts adduces, that "the laws of physics are not questioned within the context of geologic inference. They are simply presupposed."([17], p. 117). Kitts' reason for this strong claim is that without such reliance on comprehensive physical theories geologists would either be able to make any historical claims they wished, or alternatively, would be restricted to assuming that the past was exactly like the present ([17], p. 117). Kitts goes on to equate such general physical laws or theories with Kuhnian paradigms, and asserts that "geologists have had no role in the revolutions which have led to the overthrow of comprehensive theoretical paradigms."([17], p. 119). Not only is this the case, but, Kitts continues, "it is clear that for Kuhn, paradigms exercise their pervasive influence by virtue of their being general knowledge systems. He recognizes different degrees of comprehension, but he does not consider any hypothesis which is concerned wholly with particular events."([17], p. 119). Kitts concludes that since "the hypothesis of continental drift <u>is</u> concerned wholly with particular events" ([17], p. 119), the fact that it has been rapidly and widely accepted does not qualify it as a Kuhnian paradigm. Geologists are wrong in identifying it, or any other geological theory, as revolutionary, since, in Kitts' view, such theories are never general enough to satisfy what he takes to be the Kuhnian criteria for paradigms. Even where a geological theory appears to be of universal form, Kitts suggests that it acquires that generality by virtue of being incorporated into physical theory. Thus, although "there is in plate tectonics a crucial theoretical, and therefore general, dimension which is not reducible to a description of events",([17], p. 124), even plate tectonics cannot be regarded as a paradigm, for its "theoretical dimension is not provided by a geologic hypothesis formulated within the last decade but it comes from the familiar and inviolable 'super paradigm'." ([17], p. 124). Thus unlike the geologists and historians mentioned earlier, Kitts is unwilling to allow that geology in the 1960's and 1970's underwent a revolution in any sense related to the Kuhnian use of that term.

It is the purpose of this paper to criticise both the standard account of recent events in geology, and also Kitts' attack on that account. I agree with Kitts that recent events in geology constitute a Kuhnian revolution only if that concept is understood in a very weak sense. I shall not spend much time arguing against the standard account, but devote the bulk of the paper to discussing Kitts' position. In brief, I have two chief quarrels with it. First, I believe that he is interpreting Kuhn too rigidly. Kuhn himself is willing to allow a Darwinian revolution in biology, or a Lyellian revolution in geology, neither of which involved an overthrow of fundamental physical and chemical principles. Provided a theory has a general form, over and above being a description of specific events, as plate tectonics undoubtedly does, then I see no reason why, provided it satisfies other conditions that Kuhn lays down, it cannot be a paradigm. Second, and more serious, I am uneasy about Kitts' analysis of the nature of geology. This is a bold claim to make, since Kitts has thought about the philosophy of geology longer and more deeply than any other contemporary scholar, but just because this is the case, his ideas need careful consideration.

Kitts, it should be noted, claims that he is giving a purely descriptive account of the practise of geologists, and refraining from any normative account of how they _should_ behave. ([17], p. 117). Yet I believe that it can be argued that even as a descriptive account, Kitts' analysis is too restrictive. Geologists frequently have been, and continue to be, concerned with more than simple historical description, and moreover they are prepared on occasion to challenge physical theory when it seems to them to conflict with the best available geology. As Stephen Brush has shown [3], the geologist Thomas Chamberlin was prepared to develop a whole cosmology, and a rather successful one, in order to rescue his geology from conflict with the previously available cosmologies. True, it is rare for geologists to engage in such criticism of basic physical theory, but then as Kitts points out, many physicists and chemists never do so either. ([17], p. 118). Nonetheless, the fact that this questioning is rare does not mean it never occurs. Such an occurrence is impossible in Kitts' view because he believes that the assumption of physical theory is necessary for the reconstruction of geological evidence. However, this is to overlook the fact that the whole of physical theory is not needed for each such reconstruction, and that physical theory itself is often not fully consistent. Thus I believe Kitts is overstating the case when he claims that geology can never experience a Kuhnian revolution because geologists always accept physical theory. In the case of the rejection and later acceptance of continental drift theory, I hope to show that the status of the theory with respect to

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192471 Published online by Cambridge University Press

basic physical principles was not the decisive factor.

In order to demonstrate this, some revision of the standard historical account of the career of drift theory is in order. This standard account suggests that drift was originally rejected because "no one had devised an adequate mechanism to move continents...through a static ocean floor." ([11], p. 163). Furthermore, so the story goes, continental drift (in the form of plate tectonics) was accepted once a suitable mechanism was found. Although the word "mechanism" is used loosely in this connection, its usual meaning can be understood as physical cause. If the standard account is correct then, and the main reason for the rejection of drift was that it was inconsistent with the physics of the earth, then Kitts' point that geological science is always subservient to physics gains support. However, although I believe that the lack of a mechanism played a role in the rejection of drift, there are two reasons why I think that the standard account places too much stress on it as the primary factor. The first of these reasons is that certain geological theories have in fact been accepted even when there was no acceptable physical mechanism. The second is that drift (or plate tectonics) was accepted without the discovery of a mechanism for moving the continents (or plates). I shall examine these in turn.

Hallam addresses the former point when he points out that "gravity, geomagnetism, and electricity were all fully accepted long before they were adequately explained." ([13], p. 110). Even within the realm of geology "the existence of former ice ages, notably in the Pleistocene, is universally accepted but there is no general agreement about the underlying cause." ([13], p. 110). Hallam has put his finger on an important point here, although he does not go far enough as none of his examples are quite parallel to the case of continental drift in certain important respects. The problem with drift was not simply that there was no <u>known</u> mechanism or cause, but that any <u>conceivable</u> mechanism would conflict with physical theory. It is one thing to accept an hypothesis in the absence of a suitable cause if there is no competing theory that appears to rule out the very possibility of a cause, quite another if there is such a rival theory. In the case of drift, a rival did exist. Evidence drawn from a wide range of fields, including astronomy, cosmology and experimental physics, but especially from seismology, suggested that the mantle of the earth, through which the continents were supposed to move on Wegener's theory, was solid. For example, not only were earthquake foci found to a depth of 700 kilometers, but the earth transmitted shear waves to a depth of several thousand kilometers, a phenomenon which could not occur in a liquid ([16]). Even one of the few supporters of drift in the northern hemisphere, Reginald Daly of Harvard University, was convinced that the evidence was overwhelmingly in favor of the solidity of the earth to a considerable depth. ([5], ch. 3).

Even given this conflict with physical theories about the interior of the earth, however, it is still possible that drift might have been accepted, had the evidence for it been stronger. After all, as Wegener himself was quick to point out, the theory of isostasy (or vertical adjustments in the earth's crust) was widely accepted, even though such adjustments implied that flow had to occur in the mantle, that on rival physical theory was supposed to be solid ([26], p. 43-46). The reason for this was that there was very good evidence that isostasy occurred. Both the relative rarity of gravity anomalies, and the undisputed rise of the land around the Baltic following the last Ice Age attested to this fact. Geologists were convinced that isostasy occurred, even if it conflicted with geophysical analysis of the structure of the earth's crust.

The situation was quite different with Wegener's theory. At the time when Wegener proposed his theory of continental drift, there was no geodetic means of testing directly whether or not the continents had moved relative to each other or to the poles. (Indeed, until well after the widespread acceptance of plate tectonics, there were no geodetic measurements that were sufficiently accurate to show the very slow rate of plate movement.) In the absence of such direct evidence, Wegener put forward three lines of indirect evidence in support of his theory of continental drift ([25], [26]). First, he claimed that the similarity of the coastlines of South America and Africa could best be explained by his hypothesis that at one time the two continents had been joined, and that they had subsequently split apart. He made the further point that, in his belief, many of the rock formations on the one continent matched those on the other exactly, a claim that, if true, would obviously lend support to drift. Second, he advanced some paleoclimatic evidence. Working from the assumption that different types of climate have always formed approximately parallel bands between the equator and the pole, and that at least some of these climates at present are associated with characteristic rock formations (polar climates and glacial tills, for example), then the geologist might expect to find such deposits at similar latitudes in all periods of earth history. The fact that he does not do so was best explained, Wegener claimed, by the theory that the continents have moved. Third, most paleontologists were agreed that fossil fauna and flora found on continents now separated by hundreds of miles of ocean are very similar. Here, too, Wegener argued that the best explanation is that the continents have drifted apart. Now at first sight this is an impressive list of evidence for continental drift, drawn from a wide number of fields, particularly when expounded in full detail. ([11], p. 160-167 and [8]).

However, it is perhaps not sufficiently appreciated that Wegener's purported evidence was by no means beyond doubt. Take the question of the "fit" of the continents. Was the fit of South America and Africa merely an isolated coincidence or a major problem for geological science? Most geologists simply were not sure [21]. Even if it were significant, how could the continents have moved without crumpling? The additional evidence of matching formations was also questioned since geologists had been too badly burned in the previous century trying to trace Werner's "universal" formations to have much faith in matching formations, particularly non-fossiliferous ones, over long distances. If the evidence from the fit of the continents was dubious, so equally was the evidence from paleoclimates, since many geologists were not at all convinced that Wegener's alleged "tillites" were in fact tillites at all. Even the peculiar distribution of certain species lost much of its force as evidence when paleontologists pointed out that there were other cases of odd species distribution that could not conceivably be explained by continental drift ([19], p. 118) and that in any case, given the relative difficulties of moving continents on the one hand, and providing and removing land bridges on the other, they would, quite reasonably, prefer the latter.

To conclude, I believe that _if_ the evidence for continental drift had been stronger, then the absence of a mechanism would have counted against it much less than was in fact the case. Indeed, the amazing feature of the early reception of continental drift is that it was taken seriously at all. It is a measure of the desperation of geologists following the breakdown of Suess's synthesis of geological data, based on the contraction hypothesis, that they were willing to consider it ([5], [7], [15]). After all, it was only one of a number of rival theories that were put forward in the first part of this century to explain the origin and development of the surface features of the earth. In one of the few historical accounts to recognise the comparative nature of the evaluation, Greene has concluded that "in 1912 it [Wegener's theory] was a legitimate but very tentative deduction from a great body of geological and geophysical evidence assembled in the last quarter of the nineteenth century, one of many different hypotheses created from the same materials. It had no particular claim to predominance." ([12], p. 477-8). This also leads to the conclusion that there was nothing corresponding to Kuhnian "normal" science in the fifty years before the acceptance of plate tectonics. There was no dominant paradigm in which all the geological community was working. There were conflicting theories, none of which had a hold on the majority of scientists. Nor was geology in a pre-paradigm stage, for there had been paradigms previous to this in the history of the subject. Of course, this half century could be the "period of crisis" prior to a revolution. Indeed, in view of the lapse of time Kuhn allows for the Copernican revolution, it is quite possible he would regard geology in the first half of the twentieth century in this light. But if so, and if we have to take such a long view, Kuhn's analysis is of little interest to the historian or philosopher trying to understand the cut and thrust of the scientific enterprise, for such periods, at least in geology, are more the exception than the rule.

It was during the 1950's that the situation changed dramatically. During the course of the decade two new lines of evidence--from paleomagnetism and from oceanography--became available, and although neither had to do with the question of mechanism, they raised again the question of whether or not the continents had moved relative to each other in the past. Despite the claim of certain historians that the "direct" evidence for continental drift, "that is, the data gathered from rocks exposed on our continents" ([11], p. 161), was just as good in Wegener's

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192471 Published online by Cambridge University Press

day as in the 1950's, in point of fact the first set of new evidence that drift had occurred was gathered from the sedimentary rocks of several continents. Methods had been developed for ascertaining the direction of the earth's magnetic field at various periods in the past by measuring the so-called remanent magnetism of rocks. To everyone's surprise the directions of magnetism at various stages of earth history turned out to be very different from the present orientation of the earth's magnetic field. Various hypotheses to explain this result were proposed and tested, prominent among them the possibility that the earth's magnetic poles had wandered, and even the possibility that the earth's field had not always been di-polar. Eventually, however, by the end of the 1950's, a small but influential group of scientists had become convinced that the most plausible explanation for these results was that the continents had moved relative to each other ([2], [22]). They were convinced that drift was now a fact to be explained, and not just another hypothesis. This was not because they had discovered a cause of the movement; that remained as mysterious as before.

At almost the same time as the paleomagnetic results were coming in, the science of oceanography was also turning up surprising results. In Wegener's time, geologists had only explored the land surface of the planet, a mere third of the total surface. By the 1960's, by contrast, the results of two decades of exploration of the ocean floor were available to geologists. Various geophysical techniques for measuring heat flow and gravity anomalies, for example, as well as methods of collecting actual samples from the deep sea bed, had been developed and applied. Contrary to most scientists' intuitions, the ocean floors were strikingly dissimilar to the continents. They were marked by a world-wide system of "mid-ocean ridges" (actually enormous mountain chains) with peculiar physical characteristics, particularly a median rift valley marked by high heat flow. There seemed to be tensional features and the suggestion was made in 1960 and 1961 that the sea floors were "spreading". ([6], [14]). In 1963 Vine and Matthews predicted that the unusual patterns of magnetic anomalies that had been observed round the mid-ocean ridges were in fact the record of global magnetic reversals that had occurred while lava was welling up, solidifying and moving apart from the tensional cracks ([4], p. 232-237). Since global magnetic reversals had by this juncture been dated on the continents by radioactive methods, here was a potential test of the theory that the sea floor was moving apart and a measure of the rate at which this was occurring ([4], section 4). After some false starts, in 1965 parallel strips of magnetic anomalies were found and dated on both sides of one mid-ocean ridge system ([4], p. 265-264). By a couple of years later, most scientists were convinced that the sea floor was spreading. This did not automatically add support to continental drift theory. As in the case of the paleomagnetic results, a number of possibilities were considered, including the theory that these ridges were cracks resulting from the overall expansion of the earth. In order for sea floor spreading to be linked to continental drift, a theoretical innovation was required. Since the continents are different mineralogically from the sea floors, they had always been considered

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192471 Published online by Cambridge University Press

separate entities. Now the suggestion was put forward that the mineralogical differences were unimportant compared to the structural unity. Continents and oceans were welded together in rigid "plates" perhaps one hundred kilometers thick. It was postulated that the important entity that moved laterally was neither the continent nor the sea floor, but the plate. These plates were created along one edge at the mid-ocean ridges by the cooling of molten lava, and moved slowly apart, accounting for sea floor spreading, and destroyed at the other edge, either by sinking into earthquake zones or by being piled up into mountains. The theory of "plate tectonics" was ingenious, and explained the major tectonic features of the earth very economically. However, it was still sadly lacking in independent evidence until two predictions, based on the theory, were made, and shortly thereafter confirmed in a way that geologists found very impressive.

The first, proposed by J. Tuzo Wilson, was that if the earth really were covered by mobile rigid plates of material, there should be three kinds of junctions between the plates ([27]). Not only should there be the mid-ocean ridges and "subduction zones" already known, but there should also be a previously undescribed type of fault, which Wilson named a "transform fault" with a characteristic direction of movement, in the opposite direction to that expected on any other theory. By the late 1960's this prediction had been confirmed. The second prediction resulted from the realisation that if the globe really were covered with mobile, rigid plates, then their movement would be rather closely constrained, and describable mathematically. When theoretical plate motions were compared to actual ones, the agreement in many cases was good to three significant figures. With the publication of these results, the acceptance of plate tectonics was essentially complete by the earlier 1970's.

Thus far I have mentioned nothing about the problem of mechanism, and the conflict between lateral movement of continents or plates and physical theory. It has sometimes been suggested that embedding the continents within the plates overcame this problem, since no longer were the continents required to plow through the ocean floor, but rather were carried along in it, like logs in an ice floe ([11], p. 165). But such a change in the theory by no means solved the problem of mechanism. There still remained the questions of how the deep-rooted continents, even embedded in the sea floors, could move through the solid mantle, and what force would be sufficient to propel them. The former question was rendered less urgent by a reinterpretation of seismic data. Analysis of seismic evidence had long indicated that there was a puzzling narrow low-velocity zone some one hundred kilometers below the surface of the earth, but no one had known quite what to make of it. During the 1950's, when drift was being revived, it was suggested that this was a plastic zone, deep enough in the earth that plates, including the roots of the continents, could slip on it. In this way the worst conflict with rival theory was avoided [1]. However, the latter question of the nature of the force powerful enough to move the plates remained unresolved. As J. Tuzo Wilson concluded in 1976, "One very large question remains unanswered:

What is the nature of the forces that move plates about?" ([30], p. 217). Here there is still as much tension with physical theory as ever there was, a conclusion that one of the participants, D.P. McKenzie, has argued for in a recent paper ([20], p. 97). Put another way, a *kinematics* of plate tectonics is essentially complete. Those historians and geologists who say that plate movement was accepted because a mechanism was found are thinking in terms of this kinematics. But the *causes* of plate movement are still a mystery. There is no lack of hypotheses, but no geophysicist would disagree with the claim that they are all tentative and fraught with difficulties. Those historians and geologists who say that plate tectonics was accepted in the absence of a mechanism are thinking of a *dynamics*.

Thus, in view of recent developments in plate tectonics as well as the example of Chamberlin cited earlier, I believe that it can be shown that descriptively Kitts' account of geology, however accurate when applied to the day-to-day activities of the majority of working geologists, is inadequate as a general rule. Although Kitts' specifically disavows any intention of going further and making a normative claim, any attempt to extend his analysis and claim, as Duhem did earlier, that sciences other than physics must *always* take physical theory for granted, would be an unjustifiable prescription. But even in the more modest descriptive form, I find Kitts' reasons for his attack on a Kuhnian interpretation of Kuhn untenable.

This leaves the issue of whether the more flexible interpretation of Kuhn espoused by geologists and historians is adequate. As I have already remarked, it seems to be too coarse-grained to do justice to the historical details. Furthermore, certain definitive features of Kuhn's account are lacking; there is, for example, no incommensurability between the pre- and post-tectonic geological theories; neither was plate tectonics proposed and advocated by a younger generation of geologists. Its proponents came from all stages of the career spectrum, including those who had earlier decisively rejected drift. If all Kuhn had meant by a revolution was a period of rapid theory change, then it would be appropriate to invoke his work. However, Kuhn surely had a great deal more in mind when he described scientific revolutions, almost none of which is exemplified in the construction and acceptance of plate tectonic theory.

As Frankel has argued in an interesting paper, Lakatos' methodology of scientific research programmes is perhaps more helpful in trying to understand the details of the changes ([9], [10]). Among the many new discoveries in geology in the 1960's, some, such as transform faults and the magnetic anomaly patterns around the mid-ocean ridges, were the result of testing the predictions made by the new theory. That is to say, the theory predicted novel facts in the full early Lakatos sense of being both unexpected on the basis of previous knowledge and temporarily novel. These facts were clearly important in the acceptance of plate tectonics, and we do not even need to consider Lakatos' later (and weaker) senses of novelty in order to make this analysis. But it

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192471 Published online by Cambridge University Press

seems to me there is no reason to jump from this point to a full-blooded acceptance of Lakatos' analysis. In order for Lakatos' methodology of scientific research programmes to apply to this case, the predictions have to result from a series of auxiliary hypotheses added to the unchanging hard core of a research programme. But in this case it is by no means clear that there was a hard core. At most the hard core amounted to the statement that lateral movement was possible. But every other aspect, including the entities that were postulated (continents, sea floors, and plates) and the kinds of movement changed drastically. If hard cores are no more specific than this, one wonders what force they have.

In conclusion, there seems to me no reason for terming the theory change in geology in the 1970's a Kuhnian revolution. To do so is to take any precision there might be out of this concept. Furthermore, there is no need to adopt Kitts' analysis of the nature of geology in order to reject the idea that the subject has undergone a Kuhnian revolution. It now remains to be seen whether any of the other accounts of theory change developed in the last few years offer a better understanding of the introduction of plate tectonic theory in modern geology.

Notes

[1]I am indebted to David Hull for first raising with me the question of why drift was accepted in the continued absence of a mechanism. Richard Burian, Henry Frankel, Lorenz Krüger, Larry Laudan, Walter Pilant and Victor Schmidt all made helpful comments on an earlier draft of this paper.

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192471 Published online by Cambridge University Press

References

[1] Anderson, D.L. "The Plastic Layer of the Earth's Mantle." Scientific American 207 #1(1962): 52-59. (As reprinted in [29]. Pages 28-35.)

[2] Blackett, P.M.S., Bullard, Edward, and Runcorn, S.K. (eds.). A Symposium on Continental Drift. London: The Royal Society, 1965.

[3] Brush, S. "A Geologist Among Astronomers: The Rise and Fall of the Chamberlin-Moulton Controversy." Journal for the History of Astronomy 9(1978): 1-41, 77-104.

[4] Cox, A. (ed.). Plate Tectonics and Geomagnetic Reversals. San Francisco: Freeman, 1973.

[5] Daly, R.A. Our Mobile Earth. New York and London: Scribner, 1926.

[6] Dietz, R.S. "Continent and Ocean Basin Evolution by Spreading of the Sea Floor." Nature 190(1961): 854-857.

[7] Du Toit, A.L. Our Wandering Continents. Edinburgh: Oliver and Boyd, 1937.

[8] Frankel, H. "Alfred Wegener and the Specialists." Centaurus 20 (1976): 305-324.

[9] ----------. "The Career of Continental Drift Theory: An Application of Imre Lakatos' Analysis of Scientific Growth to the Rise of Drift Theory." Studies in History and Philosophy of Science 10(1979): 21-66.

[10] ----------. "The Reception and Acceptance of Continental Drift Theory as a Rational Episode in the History of Science." In The Reception of Unconventional Science. (AAAS Selected Symposia Series.) Edited by S.H. Mauskopf. Boulder: Westview Press, 1979. Pages 51-89.

[11] Gould, S.J. "The Validation of Continental Drift." In Ever Since Darwin: Reflections in Natural History. New York: Norton, 1977. Pages 160-167.

[12] Greene, M.T. Major Developments in Geotectonic Theory Between 1800 and 1912. Unpublished Ph.D. Dissertation, University of Washington, 1978. Xerox University Microfilms Publication No. 7820726.

[13] Hallam, A. A Revolution in the Earth Sciences: From Continental Drift to Plate Tectonics. Oxford: Clarendon Press, 1973.

[14] Hess, H.H. "History of Ocean Basins." In Petrologic Studies: A

Volume to Honor A.J. Buddington. New York: Geological Society of America, 1962. Pages 599-620. (As reprinted in [4]. Pages 23-38.)

[15] Holmes, A. Principles of Physical Geology. London and Edinburgh: Nelson, 1945.

[16] Jeffreys, H. The Earth: Its Origin, History, and Physical Constitution. Cambridge: Cambridge University Press, 1st edition, 1924; 2nd edition, 1929; 3rd edition, 1952; 4th edition, 1959; 5th edition, 1970.

[17] Kitts, D.B. "Continental Drift and Scientific Revolution." Bulletin of the American Association of Petroleum Geologists 58 (1974): 2490-2496. (As reprinted in The Structure of Geology. Dallas: Southern Methodist University Press, 1977. Pages 115-127.)

[18] Kuhn, T. The Structure of Scientific Revolutions. Chicago: University of Chicago Press, 1962.

[19] Marvin, U.B. Continental Drift: The Evolution of a Concept. Washington, D.C.: Smithsonian Institution Press, 1973.

[20] McKenzie, D.P. "Plate Tectonics and Its Relationship to the Evolution of Ideas in the Geological Sciences." Daedalus 106 #3 (1977): 97-124.

[21] Pinkham, G. "Some Doubts About Scientific Data." Philosophy of Science 42(1975): 260-269.

[22] Runcorn, S.K. (ed.). Continental Drift. New York and London: Academic Press, 1962.

[23] Takeuchi, H., Uyeda, S., and Kanamori, H. Debate about the Earth: Approach to Geophysics through Analysis of Continental Drift. San Francisco: Freeman, 1967.

[24] Uyeda, S. The New View of the Earth: Moving Continents and Moving Oceans. San Francisco: Freeman, 1978.

[25] Wegener, A. Die Entstehung der Continente und Ozeane. Braunschweig: Friedrich Vieweg und Sohns, 1915. (Translated as [26].)

[26] ----------. The Origin of Continents and Oceans. (trans.) John Biram from the fourth (1924) German edition. New York: Dover, 1966. (Originally published as [25].)

[27] Wilson, J.T. "A New Class of Faults and Their Bearing on Continental Drift." Nature 207(1965): 343-347. (As reprinted in [4]. Pages 48-56.)

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192471 Published online by Cambridge University Press

[28] -----------. "Static or Mobile Earth--the Current Scientific Revolution." American Philosophical Society Proceedings. 112 (1968): 309-320.

[29] Wilson, J.T. (ed.). Continents Adrift. San Francisco: Freeman, 1970.

[30] -----------. (ed.). Continents Adrift and Continents Aground. San Francisco: Freeman, 1976.

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192471 Published online by Cambridge University Press