What Kind of Revolution Occurred in Geology?

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The one thing upon which we can all agree is that just over ten years ago a major revolution occurred in the science of geology. Geologists switched from accepting a static earth-picture, to endorsing a vision of an earth with its surface constantly in motion. (Cox [4]; Hallam [12]; Marvin [28]; Wilson [56]). It is true that early in this century the German geologist Alfred Wegener [52] argued that the continents as we today find them have "drifted" to their positions from other positions widely different. However, other than amongst a number of scientists drawn almost exclusively from the Southern Hemisphere, his ideas fell on deaf -- or more precisely, contemptuous -- ears. Then in the mid 1960's, almost literally overnight, the geological community swung around and embraced the hypothesis of continental drift, or what we shall see is perhaps more accurately called "plate tectonics".

Given the fact that the major topic of debate amongst philosophers of science in the past fifteen years has been over the exact nature of a scientific "revolution", one might think that so dramatic a revolution so close at hand, in a science which is really not _that_ technical (at least is not as incomprehensible to the outsider as modern particle physics), would have attracted immediate and detailed attention by the philosophical fraternity. This however would have been to reckon without the average philosopher's obsession with white swans, black ravens, and red herrings, and his distaste for anything vaguely resembling real science. (By saying "his" here, I am not being quite as sexist as I sound; female philosophers of science have shown a refreshingly perverse interest in what real scientists really do.) The revolution in geology has been greeted by philosophers of science with absolutely crashing silence. To the best of my knowledge, not one of the major -- or minor -- journals in the philosophy of science field has mentioned continental drift and plate tectonics, let alone discussed them. (In contrast, there are at least two histories of the geological revolution, not to mention collections of the seminal

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contributions. ([4]; [12]; [28]).

As might be expected, the real losers from this indifference are we philosophers, not the geologists. The geological revolution is exciting and dramatic, and it holds rich rewards for those of us concerned to understand the temporal development of science and the reasons making scientists change their minds. Rather than argue this case in the abstract, I shall show the validity of my argument and the truth of my conclusion by attempting what must necessarily be a preliminary and sketchy philosophical analysis of the geological revolution. Like almost every other philosopher of science, until very recently I had only the haziest notion of what had gone on in modern geology; hence, what I shall have to say will, I am sure, be riddled with errors. But if I can at least kindle the interest of others I shall feel satisfied, for I am sure that as they read into the literature they will grow to agree with me that modern geology is something which ought to figure largely in philosophical discussion.

On a more personal level I must add that I have found my excursion into the philosophy of geology particularly satisfying. For a number of years now I have been studying British philosophy of science of the nineteenth century, particularly its fine flowering of the fourth decade as manifested by the work of John F. W. Herschel and William Whewell. As might be expected from a famous astronomer and a noted tidologist, for these philosophers the paradigm science was Newtonian astronomy. However, as might also be expected from a time when geology was the trendy science and from a decade whose opening was marked by the publication of Charles Lyell's magisterial Principles of Geology [26], the philosophers of science showed a keen interest in the methodological and foundational problems arising out of geology. (See Ruse [39], [40], [44]). To me therefore, the opportunity to direct philosophy of science even a little back to its earlier areas of interest, becomes peculiarly satisfying.

1. The Geological Revolution.

The west coast of South America and the east coast of Africa surely do look awfully similar, and so it is not that surprising that almost as soon as the major features of our globe were mapped out there first occurred hints and suggestions that the continents have not always been fixed in their present positions, but have moved or "drifted" from positions quite different: S. America and Africa originally being part of some primeval continent and having cracked apart. This hypothesis certainly never found universal favour, although most geologists seem to have agreed that things today are not as they used to be in the past. Apart from anything else, from Lyell on there was all sorts of supposition of now-vanished land bridges between continents to explain present organic geographical distributions.

It was not until the beginning of this century that a really impressive case was made for continental drift. This came from the pen of the German scientist, Alfred Wegener. Arguing from a broad spectrum of evidence -- the fit of the continents, organic distributions, the fact that the earth's surface seems to have two average levels pointing to the permanence of the continents and the sea (see fig. 1) and so forth -- Wegener argued that today's continents evolved by drifting from some original super-continent, 'Panagaea'. (See fig. 2). Somehow he felt, although he never came up with much by way of causal explanation, the continents made up essentially of sial, float on the rather denser sea-bed of sima, and they can plow through the sima, as they move slowly around the earth.

Wegener did not win many converts. Perhaps the most famous of his advocates was the South African Alexander du Toit, who in 1937 published his version of Wegener's thesis [6], replacing the single proto-continent of Pangaea with two, Laurasia in the north and Gondwana in the south (this latter is usually, etymologically incorrectly, referred to as 'Gondwanaland'). But generally, reputable geologists brusquely dismissed continental drift as an untenable hypothesis -- something to laugh about rather than take seriously.

It was round about 1960 that, like a phoenix from the ashes, the hypothesis rose again. The key paper seems to have been by Harry H. Hess, published in 1962 but circulated before that, entitled "History of Ocean Basins," and containing in his own words "an essay in geo-poetry." [13]. And yet, perhaps talk of a "phoenix" is misleading, for it was not really Wegener's hypothesis that Hess revived. He did not endorse a picture of sial continents plowing through sima beds, even though he did suggest that the continents have shifted around the earth. Rather Hess suggested that the continents are embedded in sheets (or as they are now known 'plates') of sea-bed, which sheets slip around the surface of the globe carrying the continents with them. The movement of the plates is somehow a function of the globe's interior, its heat and its chemical composition, and what we have is new material welling up and forming new sea-bed at certain cracks of the earth's surface, the plates thus formed consequently spreading apart, and then at other cracks of the earth's surface, one plate slipping down beneath another until deep in the earth's interior it is destroyed. (One can therefore see why today's geologists find the term 'continental drift' a little misleading and prefer the term 'plate tectonics', although the earlier term seems stuck in the general imagination.)

Hess's hypothesis was given a dramatic boost in credibility by the ideas of two young Cambridge scientists, Fred Vine and Drummond Matthews [51]. As newly formed earth-surface cools, it becomes magnetized by the earth's field. There was growing evidence that every now and then (irregularly but of the order of half a million years) the earth's magnetic field reverses its direction. This means that, if the earth's surface is growing as Hess suggested, one ought to be able to trace it through geomagnetic reversals. Ideally from a rift (crack) of new growth, one lined up north-south, one would find parallel strips of rock, magnetized in a direction opposite to its neighbours. But, pointed out Vince and Matthews, not only does one find these 'magnetic anomalies', but, as one would expect were the theory of plate

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tectonics true, one finds that the reversals on one side of a rift are an exact mirror image of those on the other side.

After this, things rapidly fell into place. For instance, new ways (using computers) were found for fitting together continents, thus making the notion of original proto-continents more plausible. (Bullard et al [2]). Similarly, some of the more significant geological configurations could now be explained: for instance the Himalayas are the effect of India having drifted up from Gondwanaland and smashed into the under-belly of Asia. But perhaps the most dramatic work occurred in the study of earthquakes. These phenomena occur almost exclusively in certain restricted parts of the world. Through plate tectonics it proved possible to explain their nature, specifically their size, their restricted locations, and their proximity to volcanoes. One could now understand earthquakes as a function of, either the creation of new sea-bed, or more importantly, the disappearance and destruction of old sea-bed. (Sykes [49]; Isacks et al [19]).

So much for the barest outlines of what happened scientifically. Let us turn next to what the geologists think are the philosophical implications of their revolution -- for such implications they believe there are.

2. Was the Revolution Kuhnian?

Almost to a man -- or a woman -- those who have written about the geological revolution think it fits the pattern of a scientific revolution as described and analysed by Thomas Kuhn in his influential Structure of Scientific Revolutions [22].[1] Thus, consider the comments of two of the men who were deeply involved in the revolution. The Canadian geophysicist J. Tuzo Wilson thinks that as in astronomy at the time of Copernicus, we have just had a Kuhnian-type revolution in the earth sciences.

> As before, the new beliefs do not invalidate past observations; the new beliefs depend upon reinterpretations of geology and geophysics, and they demonstrate the interdependence of the two disciplines. The acceptance of continental drift has transformed the earth sciences from a group of rather unimaginative studies based upon pedestrian interpretations of natural phenomena into a unified science that is exciting and dynamic and that holds out the promise of great practical advances for the future. ([56], preface).

Similarly Alan Cox of Stanford, a pioneer in work on paleomagnetism, endorses a Kuhnian view of the geological revolution, even preferring to use Kuhn's language.

> ... we have followed Kuhn in using the terms "scientific revolution" and "paradigm" to describe plate tectonics and sea-floor spreading, rather than the more traditional "hypothesis" or "theory." In this way a sterile argument was avoided about whether plate tectonics

> should be described as a hypothesis or a theory. Moreover the development of plate tectonics, although describable in terms of several theories of the history of science, fits the pattern of Kuhn's scientific revolutions surprisingly well. It appears reasonable, therefore, to regard developments in the earth sciences during the past decade as the emergence of a major new scientific paradigm. ([4], p. 5).

Of the two histories of the revolution that I have read, the one author states:

> It seems to me, however, that Kuhn has highlighted major features of science with a most illuminating conceptual model and has been perceptive in challenging the conventional view of cumulative progress. The Earth sciences do indeed appear to have undergone a revolution in the Kuhnian sense and we should not be misled by the fact that, viewed in detail, the picture may appear somewhat blurred at the edges. ([12], p. 108).

The other author states:

> The story of continental drift as a geologic concept, with its slow, tentative beginnings and violent controversy, followed by the spectacular bandwagon effect which has swept up the majority of earth scientists, bears out in dramatic fashion a thesis developed by Thomas S. Kuhn in his book <u>The Structure of Scientific Revolutions</u>, first published in 1962. ([28], p. 189).

Clearly, the accepted paradigm for analyses of the geological revolution is Kuhnian.

Now, as is well known, since the <u>Structure of Scientific Revolutions</u> first appeared, philosophers of science have made something of a pastime of criticizing it. Indeed, not a few PhD theses -- of which I confess mine was one -- have been brought to successful completion on the basis of subtle and not-so-subtle assaults on Kuhn. Hence, as a philosopher one's initial reaction to all of this endorsement of Kuhn by geologists and their historians might be to dismiss it as naive, and to turn at once to channels one finds more fruitful and trustworthy. I shall not do this. Apart from the fact that I have not yet found any general account of scientific change less flawed than Kuhn's -- and most are a good deal more flawed -- such a move smacks of the arrogance that we philosophers of science are ever-ready to find in the attitudes of real scientists towards us. Moreover, one suspects that if so many people knowledgeable about the geological revolution endorse Kuhn, there must be something in his writings which reflects the spirit of recent geology.

I shall therefore take very seriously the claim that the geological revolution was a Kuhnian revolution, and shall look carefully at it. It is true that I shall have some critical things to say -- indeed I shall go as far as to argue that in important respects the revolution

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was not Kuhnian -- but I must emphasize strongly that my primary aim will not be yet one more refutation of the Structure of Scientific Revolutions. Rather my hope is to use Kuhn's stimulating ideas (and if they are not that, why do we keep talking about them?) as a means towards showing what might be a more adequate analysis of the geological revolution. What follows should therefore be seen in more of a positive than a negative light.

3. Kuhn's Thesis about Scientific Change.

A major difficulty facing anyone who would comment on Kuhn's ideas is that there is a significant gap between what Kuhn thought he said in his Structure of Scientific Revolutions, and what everyone else read him as having said. But, assuming that it was the public Kuhn to whom our geologists were responding, the following claims seem fairly central to the Kuhnian analysis of a scientific revolution.[2]

First, there is what one might call the sociological aspect of a scientific revolution. Practitioners in a particular area of science are weaned from one theory, or 'paradigm', or Weltanschauung, to another. Usually the people involved in making the breakthrough to the new paradigm are fairly young -- sufficiently experienced to know what is wrong with the old way of doing things, but not sufficiently set or full of achievement to be emotionally committed to the past. Often the older scientists of the discipline are unable to make the switch; they feel strong hostility to the new paradigm and its supporters; and matters are only resolved as the old-timers die off (Plank's Principle). Associated with a revolution one gets all kinds of scientific infighting: people try to control the journals and what gets published in them; text-books get rewritten à la 1984, with the new paradigm right at the beginning and blunt suggestions that only a fool could fail to accept it (or rather no suggestion at all that anyone might fail to accept it); people with the right beliefs get the right jobs; and so forth.

Second, we get the psychological level to a revolution. People in a new paradigm see things differently from those in an old paradigm. Kuhn is fond of using classic psychological experiments in perception to illustrate his point: now you see a rabbit, now you see a duck; now you see a young woman, now you see an old woman. The flip from one paradigm to another is therefore something of a gestalt experience -- now you don't have it, now you do. A conversion experience, happening not gradually but in a flash, would be another way of illustrating things. And as happens with conversion experiences, the newly converted feel inspired and excited: they want to push into the new field and to infect others with their feelings.

Next, we have the epistemological dimension to a revolution. When one switches from one paradigm to another, one breaks new ground with respect to methodology and to data. On the one hand, the rules of the game change. What was important or significant in one paradigm is no longer so in the other, and vice versa. What counts as the proper way

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of doing things, of getting and evaluating evidence, changes. For a pre-Copernican the difference between the inferior and superior planets was interesting but not very significant; for a post-Copernican the difference was crucial and any theory which did not even try to explain it was unacceptable. On the other hand, the data of science gets transformed. Because we no longer see things in the same way, we no longer see the same things. Priestley saw de-phlogisticated air; Lavoisier saw oxygen, a new gas.

Fourthly and finally there is the ontological dimension to a Kuhnian revolution. At times of revolution, argues Kuhn, in some very real sense it is not simply a question of the world seeming to change for us, but rather the world really does change. Given the only sense of reality that we can understand, reality on either side of a revolution is different. There is no ultimate given against which science responds: our knowledge of the world and the world itself are inextricably bound together.[3]

These various strands run together in Kuhn's work, adding up to a view of science which can best be described as "relativistic". There is no ultimate progress in science; no absolute truth towards which science is asymptotically creeping. One can never go back to an old paradigm, but like styles in music or painting one cannot really say that the present is "truer" than the past. A great many people have concluded that Kuhn portrays science as an irrational affair. This is not quite accurate. Because the rules of good science change over a revolution, there is no ultimate touchstone of objectivity by which to assess the rationality of a revolution; but this does not mean that a revolution is irrational in the sense of slightly crazy. There are criteria making a revolution sensible: increased problem-solving ability of the new paradigm; simplicity; metaphysical attractiveness; and so forth. Hence, it is perhaps best to say that Kuhn looks upon the movement of science as "arrational".

4. Sociological and Psychological Factors in the Geological Revolution.

How then does the revolution in geology fit with this view of science? Somewhat unfairly my main focus in this paper will be on the third aspect of Kuhn's thesis, the epistemological dimension, because this is the kernel of the philosophy of Kuhn's analysis. I say that my focus is "somewhat unfair" because a several-year intensive study of the Darwinian revolution has convinced me that sociologically and psychologically Kuhn has much of value to say about scientific change [44]. And I suspect the same is the case here. Hence, although this is supposed to be a "philosophical" paper, I am certainly not going to ignore these dimensions entirely.

Briefly, take first the sociology of the geological revolution. There is little doubt that before the 1960's continental drift was generally greeted with contemptuous hostility. One early critic wrote: "Wegener's hypothesis in general is of the foot-loose type in that it takes considerable liberty with our globe and is less bound by restrictions or tied down by awkward, ugly facts than most of its rival theories." (Chamberlin [3], p. 87). And one of the historians of the revolution remembers how her colleagues "laughed heartily" at the hypothesis. (Marvin [28], foreword). Whether there was actual suppression of ideas before the revolution I cannot say, although apparently at least one person ran into trouble publishing pertinent ideas -- his hypothesis, one similar to that of Vine and Matthews for explaining magnetic anomalies, was rejected as "too speculative for publication and more suitable for discussion at a cocktail party." (Cox [4], p. 226).[4] Also in confirmation of Kuhn is the way that the text-books have been rewritten since the revolution,[5] and the fact that many of the key figures in the revolution were young: Vine and Matthews, for instance, were just beginning their careers in science (Vine was a graduate student), as also was Cox. Wegener incidentally was thirty when he conceived his hypothesis.

However, against Kuhn is the fact that Hess was not that young when he had his ideas, nor for that matter was Wilson. (I confess that I do not know how old they were when they became drifters of some sort or another.) Also worth note is the fact that although some still reject continental drift, their numbers are not many, and many geologists who for years had opposed the hypothesis swung round and accepted it. For instance, James Gilluly, author of a classic text in geology [10], early editions of which rejected drift (although in fairness it must be noted that the rejection was sympathetic), has now written that present evidence is "cumulatively so compelling that the reality of plate tectonics seems about as well demonstrated as anything ever is in geology." (Gilluly [9], p. 648). Whether these contrary sociological facts point to anything of philosophical interest will be considered later. But overall it must be allowed that Kuhn seems sociologically informative. The geological revolution did mainly follow the path Kuhn predicted.

Psychologically speaking Kuhn also scores. We have seen already how liberating and exciting geologists find their new theory, and it seems clear that for many of them they came to the theory by something very much akin to a conversion experience. One participant, Tanya Atwater has written:

> Sea floor spreading was a wonderful concept because it could explain so much of what we knew, but plate tectonics really set us free and flying. It gave us some firm rules so that we could predict what we should find in unknown places. At Scripps, Bill Menard had been browsing on the origins of fracture zone offsets for a long time and he immediately began trying to make the rules tell him something about them. The distinct bend in the Mendocino set us off on the direction change. At first Bill and I were catching each other at odd moments, scribbling sketches on envelopes and scraps of paper, but we got more and more excited until we began hunting each other up in the morning to compare the previous night's thoughts. It was wonderful working with Bill because he knows the oceans so incredibly well. Whenever we found a new geometrical relationship,

> he could think for a moment and draw out of his mind some appropriate examples from the real world. The creation of brand new fracture zones by changes in direction of spreading was a prediction that fell straight out of the sketching games; there was no well-documented case. We went ahead and published it -- his enthusiastic optimism overriding my trepidation. I was utterly amazed when we got some new lines near the great magnetic bight and the pattern was there, just as predicted. That day I was converted from a person playing a game to a believer. (Atwater [1], p. 410).

And then:

> From the moment the plate concept was introduced, the geometry of the San Andreas system was an obviously interesting example. The night Dan McKenzie and Bob Parker told me the idea, a bunch of us were drinking beer at the Little Bavaria in La Jolla. Dan sketched it on a napkin. "Aha!" said I, "but what about the Mendocino trend?" "Easy!" and he showed me three plates. As simple as that! The simplicity and power of the geometry of those three plates captured my mind that night and has never let go since.
>
> It is a wondrous thing to have the random facts in one's head suddenly fall into the slots of an orderly framework. It is like an explosion inside. That is what happened to me that night and that is what I often felt happen to me and to others as I was working out (and talking out) the geometry of the western U.S. I took my ideas to John Crowell one Thanksgiving day. I crept in feeling very self-conscious and embarrassed that I was trying to tell him about land geology starting from ocean geology, using paper and scissors. He was very patient with my long bumbling, but near the end he got terribly excited and I could feel the explosion in his head. He suddenly stopped me and rushed into the other room to show me a map of when and where he had evidence of activity on the San Andreas system. The predicted pattern was all right there. We just stood and stared, stunned. ([1], pp. 535-6).

Since so much written about Kuhn in recent years by philosophers has been critical, in his defence I must say that he prepares one for autobiographical recollections like that. Such accounts come as something of a shock to those of us raised on the super-rational picture of science of the logical empiricists. I find it no surprise that it is Kuhn who is a success with working scientists.

5. Methodology.

We come now to Kuhn's epistemology. (I take it that a necessary condition for the success of Kuhn's ontology is the success of his epistemology, and shall therefore postpone until later any consideration of the former.) Does the recent revolution in geology fit Kuhn's thesis on this score? There are two questions here. First, in the geological revolution did one get a change in the rules and the methods

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of good geologizing? Second, did the data in some way change, at least inasmuch as we interpret it? Let us take these in turn.

Of course in a trivial way one can say that the rules did change. Before the revolution the rule was: Don't explain using continental drift. After the revolution the rule was: Do explain using continental drift (or plate tectonics). But this is all a bit thin, and certainly not something that would distinguish a Kuhnian thesis from any other. Can we identify some rules beyond these that geologists use and see if they changed? There are I think two possibilities: general rules of scientific method, which are not necessarily restricted to geology, and specific rules which may well be peculiar to the science of geology.

As far as general rules are concerned, one might I suppose start with the basic level with things like _modus ponens_, but lest this be thought too stringent (although I do not concede that it is), let us take some of the broadest criteria for what would be considered as good science. These could certainly be incorporated into methodological dicta. An obvious suggestion would be that the scientist ought to strive to produce science that is hypothetico-deductive, that is to say axiomatic with theoretical terms in the premises and so forth. Unfortunately, that happy and simple time when one could claim that in the better class sciences the hypothetico-deductive ideal holds, and one could carry most of one's audience with one, has now passed. Apparently, apart from a few philosophical fossil relics like myself and Mary Williams, no one today believes that the hypothetico-deductive model has any applicability -- in physics or anywhere else. We must therefore search on for examples of general rules. Fortunately however, we do not have to search far, for these days there is another claim about science which is fairly commonly held -- positively fashionable in fact -- which can certainly furnish a general rule. This is the claim that the best kind of science explains in many different areas from one hypothesis: in other words, that one's science ought to exhibit what William Whewell [54] called a 'consilience of inductions'.

Now a consilience is certainly something to be found in physics. Newtonian astronomy is the paradigm case of a consilient theory.[6] Starting from his basic axioms, Newton can in turn explain the motions of the planets (_e.g._, Kepler's laws), the motions of terrestrial bodies (_e.g._, Galileo's laws), the tides, the moon, and so forth. Similarly we find consilience in the better biological theories. Not surprisingly given his influences, we find that Darwin's theory of the _Origin_ is consilient, as he explains through natural selection in behaviour, paleontology, biogeography, taxonomy, embryology, anatomy, and other areas. (Ruse [37], [38]). And this consilience is still a distinctive mark of the modern synthetic theory of evolution. (Ruse [36], [43]).

What about geology: pre-plate-tectonic geology that is? Darwin was a geologist before he was a biologist, and as we might expect, some of the work for which he has been most praised -- his coral-reef theory

[5] -- was consilient. Starting from an overall thesis about the world being in a constant state of elevation and subsidence, Darwin felt that he could explain all sorts of phenomena: how coral reefs are formed, through the gradual sinking of the sea-bed beneath the coral; how it is that barrier reefs, similarly caused by subsidence, always occur with atolls; why fringing reefs never occur with other kinds of reef, because they alone are caused by elevation; and other like facts. (Ghiselin [8]; Ruse [44]). In other words, we undoubtedly find a consilience taken as a mark of good science in pre-plate-tectonic geology. Geologists ought to try to be consilient.

Turn now to the new theory or paradigm, if one may so call it without prejudging the issue. Consider the following:

> Certainly the most important factor is that the new global tectonics seem capable of drawing together the observations of seismology and observations of a host of other fields, such as geomagnetism, marine geology, geochemistry, gravity, and various branches of land geology, under a single unifying concept. Such a step is of utmost importance to the earth sciences and will sure mark the beginning of a new era. (Isacks, et al [19], p. 362).

This passage, it must be added, comes in a major paper analysing the application of plate tectonics to seismology (the study of earthquakes). Or go back to Gilluly, after lifetime opposition a convert to the new theory.

> So far as I know, no one has yet suggested a model for the generation of plate motion that is acceptable to anyone else. Nevertheless, the arguments from magnetic strips, from the distribution of blue schists and ophiolite belts, from sedimentary volumes, from the mutual relations of volcanic and plutonic belts, ocean ridges and deeps, and from the JOIDES drill cores, are cumulatively so compelling that the reality of plate tectonics seems about as well demonstrated as anything ever is in geology. (Gilluly [10], p. 648).

So much for new rules or methodology at this level.

Perhaps a better case for the Kuhnian thesis can be made if we restrict ourselves more closely to geology itself. What kind of distinctively geological methodology do we find pre-plate tectonic geologists using? Do we find any reflection of this in post-plate tectonic geologists' work? Rightly or wrongly most geologists think that Lyell was the major figure in their history's past, and that it was he, with his commitment to 'uniformitarianism', who set them on the right path. Kuhn incidentally seems to agree, for Lyell's Principles of Geology [26] is one of his examples of a paradigm-creating work. ([22], p. 10). So what peculiarly geological, methodological dicta do we find in the Principles?

Historians of Lyell differ strongly, not to say bitterly, over the true interpretation of Lyell's achievements and his real importance for geology. (Rudwick [33]; Wilson [55]). But all come together in agreeing that the label 'uniformitarian', first bestowed on Lyell by his friend and critic William Whewell, insensitively masks the several things that Lyell aimed to do, as also does the counter-label 'catastrophism' for Lyell's opponents. Following recent commentators (Rudwick [34]; Mayr [30]; Ruse [39]), we can I believe distinguish three things that Lyell was trying to do, or three criteria that guided his geological conduct. First, Lyell was an <u>actualist</u>: he wanted to explain past geological phenomena in terms of causes of a kind acting today. He wanted no strange new causes to explain the past. Second, restricting now the use of our term, Lyell was a <u>uniformitarian</u>: he wanted to explain past geological phenomena in terms of causes of a <u>degree</u> or <u>intensity</u> acting today. He wanted no super-forces in the past, although he did allow that sometimes causes really do build up in the effects that they have. Some day Niagara Falls will have eaten its way back to Lake Erie, and what a flood we shall have then! Third, Lyell was committed to a <u>steady-state</u> view of the earth: allowing substantial cyclical fluctuations, Lyell thought the earth in a constant holding pattern, neither building up nor running down.

Lyell's "grand new theory of climate" illustrates admirably his guidelines. Faced with fairly strong evidence (fossil palms) that in the past Europe was warmer than it is now, rather than concede that the Earth is on a directional cooling-down, Lyell argued that climate is cyclical, within fairly definite limits. The alterations in climate he explained causally ultimately in terms of such things as erosion and deposition, subsidence and elevation, which alter the relative shapes of land and sea, and thus set up different ocean currents and so forth. Eventually, this all leads to different climates.

Obviously Lyell invoked his theory of climate to keep within his steady-state guide-lines. But there was more than this. To achieve his ends Lyell did not invent catastrophic causes of kind and intensity unknown -- "without help from a comet" rushing up close to the earth (Lyell [27], I, 262) -- but rather he used forces which we today experience,like the Gulf Stream. In other words, Lyell stayed faithful to his dicta of actualism and uniformitarianism.

Now, if we take these three guides of Lyell and turn to modern geology, what do we find? Perhaps of the three, it is Lyell's steady-statism which we should least expect to find, or rather be least disappointed if we do not find, for Lyell subscribed to steady-statism for what, scientifically speaking, can only be described as very fishy reasons.[7] He thought that inorganic direction leads to organic progression, which in turn leads to evolution and hence to the belittling of man's unique status (perhaps he was right!). Also, Lyell's particular brand of deism liked a world which neither runs up nor runs down, but which ticks along like well-oiled clockwork. However, despite reduced expectations, it would seem that modern geology does give us a little bit of a bonus here. Ignoring the question of the beginning (to be picked up in a moment), the picture sketched by plate tectonics is of the huge plates on the earth slowly rising, moving across, and sinking, somehow fuelled by heat, gravity, the laws of chemistry, and so forth. (See fig. 3). Everything just keeps turning over and over, endlessly. In other words, plate tectonics does rather strike one as being steady-state. This is not to deny that things change: Africa and South America split apart and India drifted up from Gondwanaland to smash into Asia. However, Lyellian steady-statism never denied this kind of thing. Indeed, it demands a change in the lands and the seas, although this is to be achieved by elevation and subsidence rather than lateral motion. Nor is the finding of steady-statism in plate tectonics to say that today's geologists are deists, and that this influenced their geologizing. But there does seem still to be a commitment to a steady-state world. One might of course argue that things on the present theory could run down, and this is true. But as today's geologists face their problems -- the San Andreas fault, the geology of the Gulf of Aden, the Himalayas -- they seem to be guided by the rule that everything must be understood as part of an ongoing, constant process.

What about actualism and uniformitarianism (in the restricted sense)? Do modern geologists want to explain in terms of causes of kind and intensity holding today? Clearly they do. Take first the negative side of the rejection of Wegenerian continental drift. To say that it was rejected because it was catastrophic, as one commentator (Marvin [28]) has said, is not really accurate. It was rejected because it failed the tests of actualism and uniformitarianism. Wegener wanted the continents to plow across the earth's surface, rather like a rubber ducky sails across the top of one's bath water. But the simple fact of the matter is that continents are not rubber duckies and sea-beds are not bath water. As we understand today's forces of nature -- kind and intensity -- a solid lump of rock stuck in another solid lump of rock, simply cannot move. That is that. Hence, because it violated the geological game-rules Wegener's hypothesis was rejected.

Take next the positive side of the argument and look at Hess's classic paper. Now it must be admitted that Hess quite openly concedes what he calls "the great catastrophe". "In order not to travel any further into the realm of fantasy than is absolutely necessary I shall hold as closely as possible to a uniformitarian approach; even so, at least one great catastrophe will be required early in the Earth's history." (Hess [13], p. 23). In particular, Hess argues that some time, not long after the formation of the solid Earth, there was a single-cell convective overturn which separated out the Earth's core from its mantle and caused the bilateral asymmetry of the Earth's surface (i.e., division into land and water). (See fig. 4). After this overturn, because of the separation of core from mantle, such a drastic process was no longer possible, and so the steady-state, plate-tectonic process took over.

However, I am not at all sure that this "catastrophe" would be that

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alien and objectionable to a Lyellian. On the one hand, Lyell himself admitted that strange things may have happened when the world was set up: "He [i.e., God] may put an end, as he no doubt gave a beginning, to the present system, at some determinate period of time; but we may rest assured that this great catastrophe will not be brought about by the laws now existing and it is not indicated by any thing which we perceive." (Rudwick [35], p. 148). This is from a lecture given by Lyell in 1832.) On the other hand, I do not think that Hess was even supposing a catastrophe of the order that Lyell himself was prepared to deny! Hess did not want different laws of nature, or causes of a kind and intensity unknown -- merely that they come together to effect one unique major phenomenon. Something akin to (albeit much greater than) Niagara Falls backing all the way up and Lake Erie overflowing.

Laying aside this catastrophe, as the author himself admits, the commitment to actualism and uniformitarianism (narrow sense) in Hess's paper is quite remarkably striking. A key fact -- perhaps the key fact -- on which Hess based his case was that the sedimentation on the sea-bed is fairly thin, indicating that at current rates of deposition the sea-bed surface is young (260 million years, for an Earth now known to be 4 1/2 billion years old). Now to get round this problem Hess could have assumed causes of a kind and intensity unknown -- an expanding Earth would do the trick, because then the sediments would be spread more thinly. However, "I hesitate to accept this easy way out. ...[It] is philosophically rather unsatisfying, in much the same way as were the older hypotheses of continental drift, in that there is no apparent mechanism within the Earth to cause a sudden increase in the radius of the Earth." (Hess [13], p. 32). In other words, because he was firmly committed to actualism and uniformitarianism, Hess felt that from current evidence of sediment deposition he had to conclude that the sea-beds are young (although the evidence is that the continents are old), and therefore he had to suppose a hypothesis to explain this -- sea-bed spreading, which is the foundation of plate tectonics.

We find a similar commitment to this methodology in the work of other scientists who have endorsed the new outlook. Those working on geomagnetic reversals, for example, were very concerned to show how causes of a kind and intensity that we know today can effect various magnetic phenomena in cooling rocks, so that they could then argue that the magnetic anomalies which we find on the sea beds can only be explained by spreading, as new bed is created and old bed absorbed. (See Cox [4], especially section IV, "Geomagnetic reversals: the story on land"). But there is no need to labour the point. The key items of Lyellian geological methodology are obviously as crucial to today's geologists as they were to the earth-scientists of the past.

Finally, to conclude our discussion of methodology, note that even with respect to the most specific of details there is continuity. At least from the time of William Smith, the "Father of Geology", it has been axiomatic amongst geologists that a good guide to the past is the tracing of various strata. (See Laudan [25] for a good discussion of

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Smith's contribution to geology). For instance, if one finds two identical sets of strata separated in some way, one can be fairly certain that they were together and some cause separated them. But this is precisely the methodological assumption behind one of the most dramatic pieces of evidence for the new geology, namely the exact way that the North-west geology of S. America matches the geology of the Gold Coast of Africa. (See fig. 5). Similarly, for Lyell and Darwin, organic distributions (today's or fossil) were crucial tools in the inferring of the geology of the past. Likewise modern geologists turn *to organic distributions for help (as indeed, did some of those who* argued against Wegenerian continental drift). (Marvin [28]). Finally, the opportunity is too good to miss mention of the frontispiece of Lyell's Principles, which shows how he used erosion and so forth to infer elevation and subsidence. (Fig. 6). The same inferential machinery directed towards the same ends is in use today. (Fig. 7).

The conclusion we must surely draw is that, with respect to methodology, there is strong continuity across the recent geological revolution. At all levels, the guide-lines for good geology are shared by pre- and post-tectonic geologists. Let us turn next to the question of the data.

6. The Facts.

Epistemologically speaking, Kuhn argues that the facts of science are "theory laden". When we pass through a revolution in some way the facts change because we interpret them and thus "see" them in a different way. (Ontologically, Kuhn wants to go further than this and argue that the facts really are different.) A favourite example of Kuhn's, one that we might use as a paradigm here, is drawn from the chemical revolution: although Priestley and Lavoisier both discovered oxygen, only Lavoisier saw it as oxygen, as a new gas. Until his death, for Priestley oxygen was dephlogisticated air. (Kuhn [22], p. 117).

With respect to the question of facts, three things stand out in the recent geological revolution. First, it is just not true that everything changed as drastically as from dephlogisticated air to oxygen. People knew of earthquakes and volcanoes before the geological revolution; they know of earthquakes and volcanoes after the revolution -- they knew also that they tend to come in certain places, like the Pacific coast of the Americas and not Guelph, Ontario. People knew of major geological phenomena like the San Andreas fault and the Himalayas before the revolution; they know of them after the revolution -- also they knew of certain fascinating details, like the fact that high up the Alps there are recent fossil formations. (Lyell's opponents took this as definitive evidence of catastrophes -- and indeed, why should they have not done so?) People knew before the revolution that there are some very odd facts of organic geographical distribution, which surely demand that land-sea boundaries were not always as they are now; people know the same since the revolution.

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Of course, one would not deny that the interpretation put on these and like facts (in the sense of explanation) has changed across the revolution. But who would deny this? That was the whole point of the revolution! The question at issue is whether a pre-plate tectonic and a post-plate tectonic geologist experienced radically different things when they experienced (say) an earthquake. Were their perceptions like dephlogisticated air and oxygen, with the kind of complete breakdown in communication that that implies? It is hard to say that they were. One does not get the kind of total failure of communication and comprehension that is necessary to give plausibility to a Kuhnian viewpoint. An earthquake is an earthquake is an earthquake. Moreover, one cannot really argue, as Kuhn would have us argue, that pre- and post-revolutionary geologists differed in the status they accorded facts -- like the inferior-superior planet distinction before and after Copernicus. Lyell and Darwin knew that earthquakes and volcanoes are important and central to a study of the Earth, as do plate tectonic geologists. And the same holds true of the San Andreas Fault, the Himalayas, and so forth.

The second point in this section dealing with facts is that a very distinctive feature of the geological revolution is the incredible amount of new and crucial information that there was. If one thinks of the Darwinian revolution, the amount of new material uncovered between 1855 -- when virtually no one was an evolutionist -- and 1865 -- when virtually everyone was an evolutionist -- was really not that great. Bates came up with his facts about (and explanation of) mimicry in butterflies, Archeopteryx was discovered, Prestwich found evidence that man coexisted with extinct animals, but there was not much more. (Ruse [44]). However, the most distinctive fact about the geological revolution is the large body of new information gathered by such means as coordinated worldwide surveys -- new information which was absolutely crucial in turning continental drift from an unsupported pipe-dream into a well-confirmed hypothesis.

It was discovered that the sea-bed is covered by only a thin layer of sediment, thus implying its youth. It was discovered that the sea-bed is not absolutely flat like a pancake, but that running through the oceans are these huge mountain ranges, "ridges", with all sorts of peculiar properties (like a rift or valley running along the summit of some). It was discovered that the sea-bed does not present a uniformly magnetized face, but that as in the S. Atlantic one gets reversed strips, arranged in a mirror image about a rift.[8] It was discovered that earthquakes have definite patterns -- light earthquakes along rifts, light earthquakes in other places (where the plates are supposed to go down) and major deep earthquakes behind these latter light earthquakes (where the plates are supposed to be breaking up). See fig. 9. All of these facts and more made plate tectonics. And yet they were simply not available to earlier geologists.

Now I am not sure that the point being made here brings much comfort to Kuhn (although I shall suggest later that it may not bring much comfort to others either). His point seems to be that the facts

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change. My point is that the facts appeared, <u>for the first time</u>. One way to defend Kuhn is to say that in the geological revolution we had, not a change of paradigms, but a change from pre-paradigm geology to paradigm geology. But there is a penalty to pay. One now has to allow that until the 1960's geologists were fumbling around like sociologists. And this seems not true. From at least Lyell on, consensus was that continents and seas stay in place on the globe -- although what is continent and what is sea at any particular time is quite another matter.[9]

The third and final point to be made about the facts is that, notwithstanding what has just been said, there are some things which go further in a Kuhnian direction than anything hitherto mentioned. Take for example the phenomenon that most lay-people think of when modern geology is discussed: the fit of S. America with Africa. The pertinent questions to be asked are: What is S. America? What is Africa? The most obvious answers are "areas of land bounded by their oceans"; but in the opinions of geologists matters are not quite that simple. Continents are bounded by areas of shallow sea before the bottom gets really deep -- "shelves". Why not include the shelves in the continents -- it is a bit arbitrary to be guided absolutely by the level of the sea. But even if one does decide to include the shelves in the continents, there still remains some ambiguity. How deep is "really deep" and when does a shelf cease to be a shelf?

It cannot be denied that in their matching efforts geologists have chosen different boundaries. (Marvin [28]). Indeed, to an outsider there is some suspicion that some first find the best fit and then justify their choice afterwards. But, be this as it may, here at least a case might be made for saying that geologists are not working with raw data, but with "facts" that have been sifted and influenced by the theory that the geologists hold dear.

Although whether this point is strong enough for Kuhn is another matter. Certainly, there seems no support for Kuhn's strongest thesis, his ontological thesis. The facts of continents, shelves, and seas do not really change: it is all a question of where one wants to draw boundaries. Nor even, reverting to epistemology, do we seem to have anything as strong as the oxygen-dephlogisticated air case. I do not think that this fiddling with the coast-lines indicates total inability to see the viewpoint of others (even though one may not accept it). Moreover, the impression one gets is that -- although to the lay-person fitting together the continents is the key point in the new geology -- to many of the new geologists themselves it is somewhat peripheral. What really counts is the newness of the ocean beds, and magnetic reversals, and so forth. That one should somehow be able to fit together the continents is a consequence. In short, it hardly seems that here we have something to sway us all back to Kuhnianism.

7. Revolution or Evolution?

I hope now it will be agreed that although I have had critical things to say about Kuhn, much that is positive has been gained from the discussion. Indeed, somewhat immodestly already I feel that we have made advances towards understanding the geological revolution. Let us sum up and see what we have.

We saw that a great many geologists found the geological revolution literally that: revolutionary. They found the new theory of plate tectonics exciting and liberating, and they switched into it rapidly and with gusto. Coupled with this, it seems to have been a fairly general revolution. Not everyone went along with it (Marvin [28] discusses critics), but one does not sense the significant and persistent opposition that there was to Darwin's mechanism of natural selection (although there are analogies to the switch in the 1860's of the biological community to evolutionism). These two facts, psychological and sociological, are made much clearer and more understandable by what we have learnt at the level of epistemology. On the one hand there was no call for new methodology, breaking with the ways of the past. Plate tectonicists could be -- had to be -- as actualistic and uniformitarian as Lyell. Hence, geologists old and new could continue to approach problems in the ways that they had always done. Methodologically there was no revolution. On the other hand, what was crucial about the revolution was not that the facts changed, but that so many new facts came tumbling in. As with methodology, we do not get the strain of being required to reject what has gone before; but at the same time there was so much new information requiring new ideas and theories, that when plate tectonics was proposed geologists leapt forward rather as if they had been converted at a revivalist meeting.

In other words, what I see as the key to the geological revolution was the vast amount of new information gathered, certainly since the war, but perhaps even more concentratedly between (about) 1955 and (about) 1965. Unlike Kuhn we can perfectly well say that the revolution was rational, because there was no essential change in geological methodology -- all the standards of good geology remained in place. Unlike Kuhn we can also explain why so many, including the older people, went over to a moving Earth -- they were able to do so simply because the rules did not change, nor did all the facts of the past have to be thrown away or recycled. But like Kuhn, although not for the same reasons, we can say why geologists really went through conversion: there were so many new facts -- not retreads, but brand new radials.

So, if we are not going to be Kuhnians, what then are we going to be? In recent years a number of philosophers of science have endorsed "evolutionary" metatheories of scientific change, as opposed to Kuhn's "revolutionary" metatheory. (For example Popper [32]; Toulmin [50]). I do not mean that these philosophers of science deny that what we commonly call "revolutions" occur in science. Rather, the point is that these significant changes are not abrupt as Kuhn argues, but more gradual and in significant respects analogous to evolution as we encounter and understand it elsewhere. There are however two points which make me initially dubious about the application of these evolutionary

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metatheories to our revolution, one general and the other specific.

Generally, my worry stems from the fact that although one can of course mean very much as one likes by 'evolution' -- certainly everyone else has done so in the past hundred years -- if one wants to say something usefully informative, rather than simply reiterating the tautology that scientific change involves change, presumably one wants to draw a significant analogy between scientific evolution and evolution as it is elsewhere most commonly experienced and thoroughly understood, namely in biology. But straight away one runs into a monstrous disanalogy, which makes talk of theory "evolution" a lot less interesting than one might have expected: the raw data of biological evolution, mutations, are random in the sense that they do not occur when needed nor are they directed towards the adaptive ends of their possessors. However, new innovations in science very much are produced to suit the need -- Vine and Matthews for example introduced their hypothesis specifically to explain the funny things they found about the magnetism of the sea-floor. A brilliant creative scientist is not akin to a gene going wrong. For this reason I wonder how valuable talk of theory evolution, including geological theory evolution, really is.[10]

My specific worry about an evolutionary interpretation of the geological revolution is that, to me, 'evolution' implies some sort of gradual change. Certainly there have in the past been "saltationary" evolutionary theories involving jumps -- indeed, even today it is thought that something along this line holds in parts of the plant world -- but if things get too saltationary then evolution goes out of the window. However, I am not sure that we really get anything of the gradualness required for evolution in the geological revolution. On the one hand, methodologically we do not seem to have much change at all. On the other hand, the change that we do have -- much new basic information and new theories to explain it -- seems to have come fairly abruptly. This does not seem very evolutionary. Nor does it help much to mention Wegener, suggesting that his speculations point to a gradual change to a moving Earth-surface. Most did not accept Wegener when he hypothesized, and as we have seen when there was a switch it was not really to Wegener's position or one like it.

I suspect that at this point I am going to be in hot water with the historians -- or perhaps hot lava. All who have written historically on the geological revolution have been at pains to show that the plate tectonicists had their predecessors -- and that moreover these predecessors were not simply Wegener. Hess, for instance, had a predecessor (and influencer) in the brilliant geologist Arthur Holmes, who was putting forward the idea of ocean-floor spreading as far back as 1931 [14]. And the whole question of geomagnetic reversals and their record in the rocks, so crucial to modern geology, goes back at least to the work of the Japanese paleomagnetist of the 1920's Motonari Matuyama [29]. But, whilst I cannot deny these predecessors I am rather inclined to downplay them somewhat -- at least in the context of the point I am making. The matter at issue is whether we see a gradual change in the

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opinions of the members of the geological community spread over a number of years; not whether we can dig up the occasional precursor. I suggest we do not see such a change. The common tale is that right into the 1960's people laughed at continental drift. Then they stopped laughing, and switched.[11] Moreover, I would point out -- at the risk of alienating my historical colleagues yet further -- that on this matter the accounts of historians are highly suspect. Almost by definition, historians (even including Kuhn when he wears that hat!) are bound to be evolutionists, imposing an evolutionary interpretation on history. (See Kuhn [21]. Note the sub-title of Marvin's book!) No one wants a description of forty five identical arguments opposing Wegener. The historian's job is to dig out the brilliant anticipation of Hess, even though it was the work of one man and went unheeded. If this is not the historian's task, then why all the interest in Mendel? In short, I stand by my claim that the geological revolution was not so very evolutionary.

By this time it might be felt that I am arguing that there is something very mysterious about the geological revolution. This is not really so. What I am arguing is that certain current metatheories of scientific change are not really that applicable, although this is not to deny that, as I hope this paper shows, the metatheories can stimulate one towards a more adequate analysis. My claim is that the revolution in geology was less abrupt than a Kuhnian would have it; more so than an evolutionist would have it. I believe that there was a continuity of methodology across the revolution -- a very strong and crucial continuity -- and for this reason one can certainly speak of the revolution as being rational.[12] The set of canons of good geological science developed before plate tectonics drove geologists to accept the theory, despite in many cases a long lifetime's opposition to continental drift. But the change was rather abrupt, and the key to this lies in the fact that the crucial cause behind the revolution was the wealth of new information -- knowledge of the thinness of the sedimentation of the oceans; evidence of the varied state of the seabeds, particularly of the rifts; the finding of geomagnetic reversals and the uncanny way they get mirrored across rifts; the exact plotting of earthquakes and of their intensities; the fit of geological strata between Africa and S. America; and much more. It was not, as Kuhn would have it, that old facts vanished and new facts appeared in their stead; rather new facts appeared where none had existed hitherto, and their cummulative weight was to make geologists plate tectonicists.

8. Conclusion.

Apart from historical/scientific inaccuracies, I am very much aware of this paper's philosophical inadequacies. I would like to hope that I may have highlighted a few of the salient features of the geological revolution and perhaps have pointed in the direction of finding an acceptable philosophical analysis. But I have certainly done no more. I have provided no analysis of the exact way in which geologists think their theorizing fits and explains the facts. Is the hypothetico-

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deductive ideal really inadequate here, and if so, what substitute should one offer? Without some sort of answer to these questions I cannot see that analysis of the geological revolution can go much further, for one must see how it is that geologists think that their new theory adequately explains all of the new information. Also missing in this paper is any real discussion of what some philosophers have called the 'causal-phenomenal' dichotomy in science. Where do today's geologists stand on the ultimate causes of what makes the Earth's surface move around, how much room exists for different opinions, and how do causal speculations relate to lower-level parts of modern geological theory? Is modern geology irreducibly historical in a way that physics is not, or does the revolution in geology take us one step closer to the overall unification of the sciences, as some have argued has been the case with the revolution in genetics of the 1950's? (Schaffner [45], [46], [47]; Ruse [36], [41]; but see Hull [15], [16], [17], [18]).

I could go on asking questions almost indefinitely. But enough is enough. Hopefully I have stimulated some members of my audience to take up some of these problems. Unless of course they feel more stimulated to show what little I have done must be done all over again, properly![13]

Notes

[1]The one exception is David Kitts [20]. What I shall have to say, I believe, complements Kitts's views, rather than contradicts them.

[2]Since the geologists were reacting to the Kuhn of the _Structure of Scientific Revolutions_ [22], I shall not feel obliged to take account of Kuhn's later work.

[3]One of Kuhn's more sensitive interpreters, Fred Suppe [48], has suggested that really such an extreme ontological thesis should not be ascribed to Kuhn. I think that it fairly can, but as will be seen, for the purpose of this discussion not much hangs on the point.

[4]The author was L.W. Morely, a Canadian. The explanation which naturally comes to mind to anyone living North of the Border was that the rejection was a direct function of Morley's nationality. However, although this reason has a certain satisfyingly masochistic flavour, and perhaps even has some truth -- Vine and Matthews had their paper accepted by _Nature_ in 1963 -- it must in fairness be noted that Tuzo Wilson, a leader of the new geologists, is Canadian.

[5]Compare the second (1959) and fourth (1974) editions of J. Gilluly _et al_'s classic textbook, _Principles of Geology_ [10].

[6]I take it that no one would deny that Newton's theory was a good theory, even though there is today some question about its truth. Certainly we should be dubious of following Whewell and, without

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qualification, allowing that a consilience is a mark of necessary truth. See Laudan [23].

[7]This is not to say that Lyell was thereby any less a scientist, or any less a good scientist. Although most like to pretend that it is not so, many scientists are driven by nonscientific reasons, and at the time of Lyell it is not easy to think of anyone writing on geology in Britain who did not have some theological axe to grind.

[8]This apparently was very influential in changing people's minds. See fig. 8.

[9]Laudan ([24], pp. 134-5) has some interesting comments on the extent to which, if at all, Lyell's work could be said to have created a geological paradigm. I suspect however Laudan would agree that there is something a little odd about claiming that until 1965 geology was entirely pre-paradigmatic in a Kuhnian sense.

[10]Popper tries to get around this difficulty by making biological evolution quasi-directed. See Ruse [42] for criticisms of this ploy.

[11]A revealing, but I am sure typical autobiographical fragment is to be found in Stephen Gould's recent book Ever Since Darwin [11]. He tells how, when a student at Columbia, his professor primed an audience to sneer at a drifter. Like the parable of the prodigal son, the most important part comes right at the end: that self-same professor converted and spent the last two years of his life actively promoting the cause he had always opposed.

[12]I suspect that what I am saying may bring joy to the heart of Larry Laudan [24]. He emphasizes the distinction between a theory and a 'research tradition': "a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain." (p. 81). In Laudan's terminology, my point is that the geological revolution was rational because, although the theory changed, the research tradition remained the same.

[13]Since completing this paper I have been sent a major contribution to the philosophical study of the geological revolution by Henry Frankel [7].

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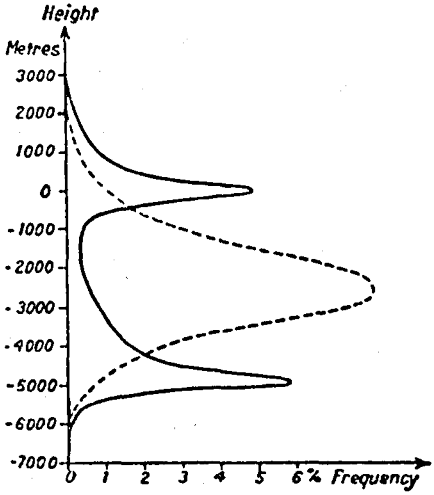
Figure 1



The distribution of the earth's elevations. The solid line represents Alfred Wegener's double-peaked curve of surface elevations. In Wegener's view the two peaks represent two fundamental levels, the surfaces of the sial and the sima. The dashed line is the Gaussian distribution which Wegener would expect if the earth had only one surface level that has been deformed to create continents and ocean basins. (The diagram was first used in *Die Entstehung der Kontinente und Ozeane*, second edition, 1920. This English version is from page 30 of *The Origin of Continents and Oceans*, translated by J. G. A. Skerl, 1924)

From [28], p. 70.
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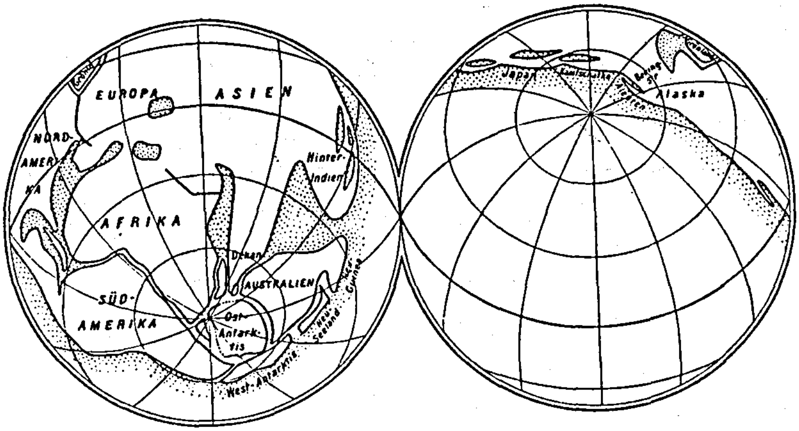
Figure 2



Wegener's "Pangaea" (which first appeared in Die Entstehung der Kontinente und Ozeana). From [28], p. 73. Reprinted by permission.

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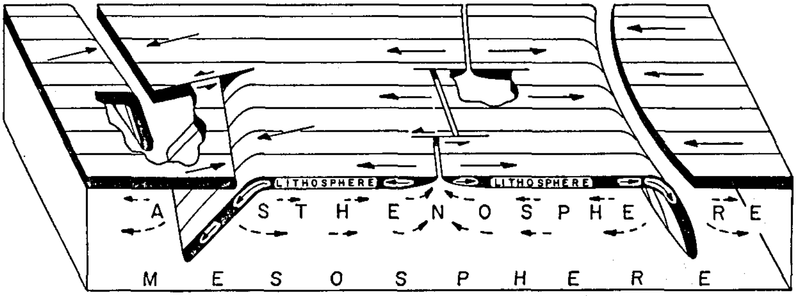
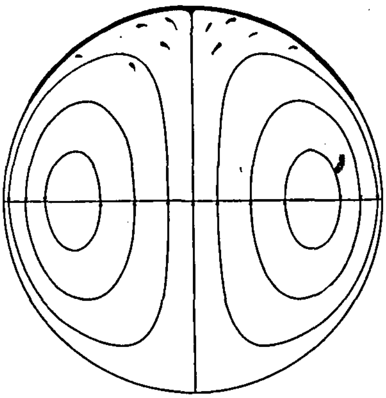
Figure 3



Block diagram illustrating schematically the configurations and roles of the lithosphere, asthenosphere, and mesosphere in a version of the new global tectonics in which the lithosphere, a layer of strength, plays a key role. Arrows on lithosphere indicate relative movements of adjoining blocks. Arrows in asthenosphere represent possible compensating flow in response to downward movement of segments of lithosphere. One arc-to-arc transform fault appears at left between oppositely facing zones of convergence (island arcs), two ridge-to-ridge transform faults along ocean ridge at center, simple arc structure at right

From [19], p. 5857. Reprinted by permission.

Figure 4



Single-cell (toroidal) convective overturn of Earth's interior. After Vening Meinesz. Continental material extruded over rising limb but would divide and move to descending limb if convection continued beyond a half cycle

From [13], p. 601. Reprinted by permission.

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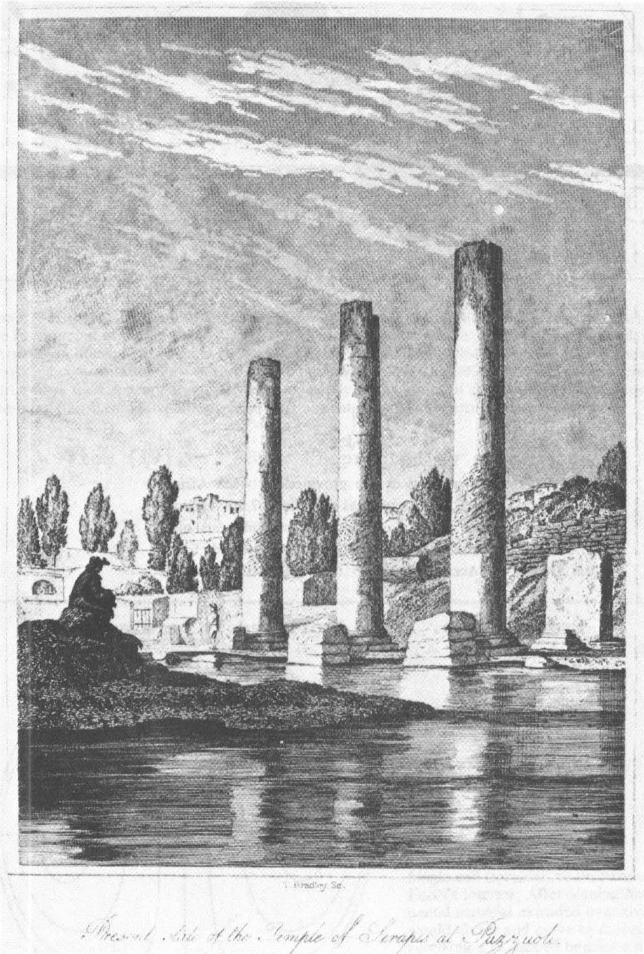
Figure 5

The matching of age provinces between Africa and South America. Dark gray areas are at least 2,000 million years old; stippled areas, more than 600 million years old. The heavy line marks the dated contact that extends from the vicinity of Accra, Ghana, to that of São Luis, Brazil.

From [28], p. 159. Reprinted by permission.

Figure 6



Frontispiece to Lyell's Principles [26].

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Figure 7

Measuring the height of the pre-earthquake upper limit of barnacle growth, or barnacle line, above the present water level at Glacier Island in Prince William Sound. The sharply defined upper limit of barnacle growth is typical of much of the Prince William Sound area

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Figure 8

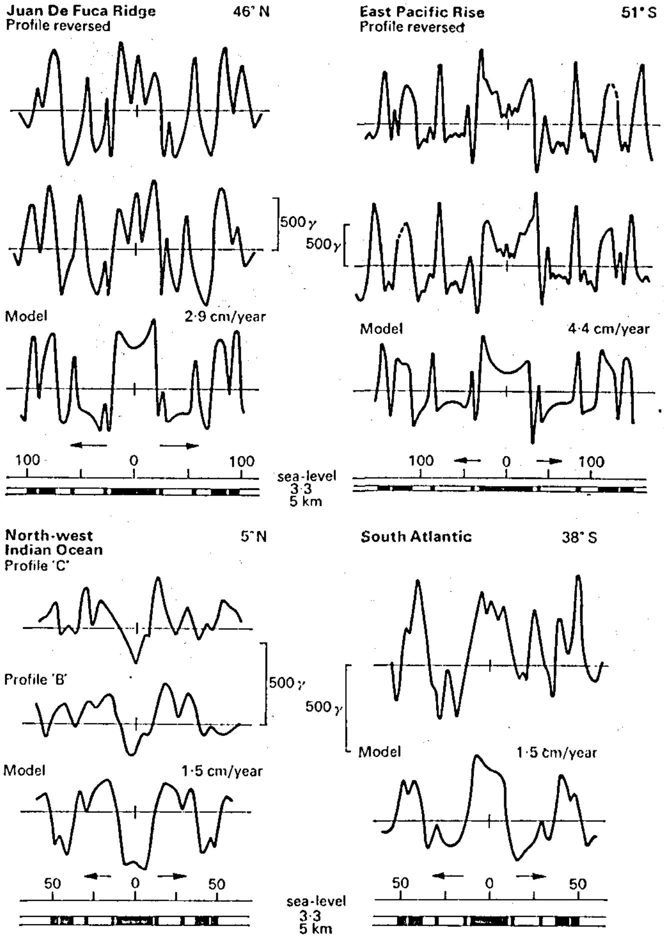

Diagram to illustrate the close agreement between observed magnetic profiles and theoretical models based on the Vine–Matthews hypothesis. After Vine 1966, Figs. 6–9.

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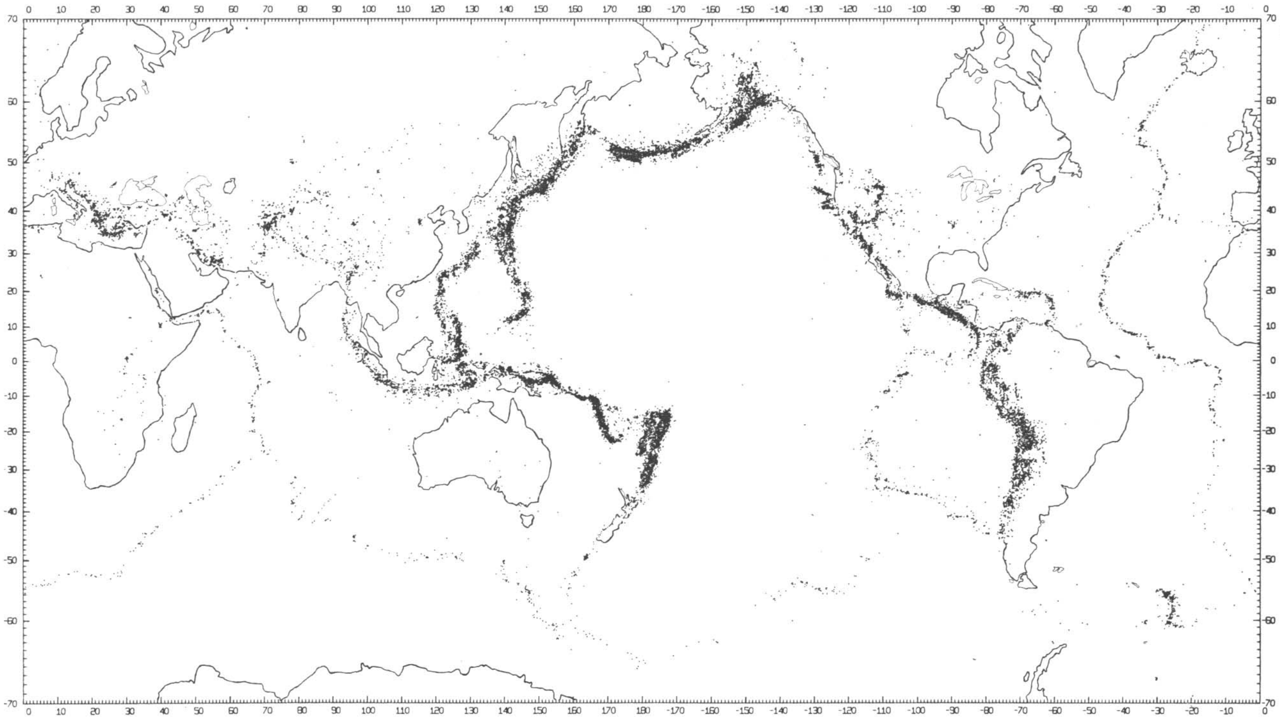
Figure 9



Worldwide distribution of *all* earthquake epicenters for the period *1961* through *1967* as reported by *U.S.* Coast and Geodetic Survey (after Barazangi and Dorman, 1968). Note continuous narrow major seismic belts that outline aseismic blocks; very narrow, sometimes steplike pattern of belts of only moderate activity along zones of spreading; broader very active belts along zones of convergence; diffuse pattern of moderate activity in certain continental zones

From [19], p. 5881. Reprinted by permission.

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