CHAPTER 11

PLATE TECTONICS: A SURPRISING WAY TO START A SCIENTIFIC CAREER

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BETWEEN 1963 AND 1968 A SMALL GROUP OF GEOPHYSICISTS, WORKING at the Universities of Cambridge, Toronto, and Princeton, the Scripps Institution of Oceanography, and at Lamont Geological Observatory of Columbia University, put together the group of ideas now known as the theory of plate tectonics. Some of these people were already major figures in the field, whereas others, like me, were only just out of graduate school. As the far-reaching success of these ideas became clear, we all rapidly became famous, to a degree that surprised even those of us who were already well known. Nothing any of us did before or after was as scientifically spectacular, although all of us spent our working lives as active scientists publishing papers.

The published papers that led to the theory were collected by Allan Cox of Stanford University, who grouped them by subject area and wrote excellent introductions.[1] The history of the events is well described by Bill Menard of Scripps, who was closely involved in many of them.[2] Perhaps because these two excellent books provide such a good historical record, the editors of this collection asked us to write about the events from our own point of view. Although this is in some ways easy to do, it is not a normal activity for scientists, nor one at which they have much practice. Furthermore, the events took place more than 30 years ago. But this sociological side of scientific discovery has (rightly) become recognized as of great importance by those, such as Thomas Kuhn, who write about the history of science, even though the formalism that they have generated seems to me at least strange and somewhat artificial.[3] But it is

Dan McKenzie, astride the San Andreas Fault in 1967. (Photo courtesy of Breck Betts, Scripps Institution of Oceanography.)

no good complaining when so few of those involved have written about their experiences, since such accounts must form part of the raw material used by historians of science.

I have tried to organize this essay around the events of the mid-1960s, with an account of how I came to be at the Department of Geodesy and Geophysics at Cambridge, or Madingley as it is often called, after Madingley Rise, which houses it. Little has yet been written about the history of this remarkable place, where many of the discoveries that led to plate tectonics were made, and so I thought a brief history might be of interest. I myself only became involved in plate tectonics after I finished my Ph.D. in 1966. The final version of the theory was put together very rapidly during 1967–1968. I thought I should say something about what major scientific advances look like to those involved at the time, rather

than to those who write about them afterward. The most interesting aspect of the period since 1968 is how the ideas that were so new and revolutionary a few years before were so quickly and thoroughly integrated into our understanding of how the earth has evolved, and how quickly those scientists who were involved in these discoveries became recognized internationally.

How I Came to Be Where I Was in 1967

I was born in February 1942, after my parents had decided that Hitler was not going to win the war. My father was a surgeon who qualified just before the war started. His father was also a surgeon, with a practice in London on Harley Street, a large house in Highgate in North London, and a chauffeur. My father, like me, went to Cambridge as an undergraduate. My mother's background was quite different. Her father was a laborer who shoveled coal into the furnace in a power station in Leeds, which was then a grim industrial town in northern England. She was intelligent, and benefited from one of the earliest socialist measures in the United Kingdom, which opened up free university education to about 200 people a year who won scholarships. She won a scholarship and applied to Cambridge, which invited her to an interview where she was made to read aloud. They rejected her because of her thick northern accent, and she never forgave them. She was never entirely happy that I became a Cambridge academic. Until I was 7 we lived in the country but we moved to London when my father, like his father, became a Harley Street doctor. My grandfather was grand enough to have a chauffeur who drove him to work every day from Highgate, but we could only afford to rent a flat above my father's consulting rooms in Harley Street.

My mother, whose maiden name was Fairbrother, later wrote a book about this period, when she was bringing up two children largely by herself in the country, and a second book about our life once we moved to London.[4] My brother and I figure prominently, but she used our middle names, Peter and John, to try to save us from problems at school. I am still embarrassed by her vivid picture of me as an introverted, hesitant child, in most ways less successful than my younger brother. I went to three excellent private schools in London, although I was not at all successful academically until I was about 14. Then I started to learn proper mathematics (I have never been able to add or spell), physics, and chemistry, and to win prizes. Although my father made some unconvincing attempts to deflect me into medicine, I was clear that I wanted to go to Cambridge and be a physical scientist. This I duly did in 1960, after an interview in which I talked to a distinguished classicist about Dostoevsky and British wild orchids, on the strength of which he gave me a place at Cambridge to read physics. Cambridge interviews are now much more serious and professional affairs, and I am sure that my mother would now be accepted.

The natural sciences course at Cambridge differs from those at most English universities because it requires students to take lectures and exams in three sciences (mathematics does not count as a science, although it is required for physics and chemistry). I chose to take geology as my third science, largely influenced by the 19th-century books on the subject by Charles Darwin, Charles Lyell, and Archibald Geikie in the school library. But the geology course at Cambridge was awful: I learned the hundreds of fossils necessary to identify stratigraphic zones, and to draw and name the parts of echinoids, ammonites, and so on. Although I liked the people, and especially the field trips, I thought the course was stupid, and gave up geology after a year to take a degree in physics. It was partly the people, especially Maurice Hill and Drummond Matthews (who was always known as "Drum"), and partly the idea of using physics to understand the processes operating within the earth, that attracted me back to the earth sciences. I joined the Department of Geodesy and Geophysics at Madingley Rise as Sir Edward Bullard's graduate student in 1963.

Madingley Rise

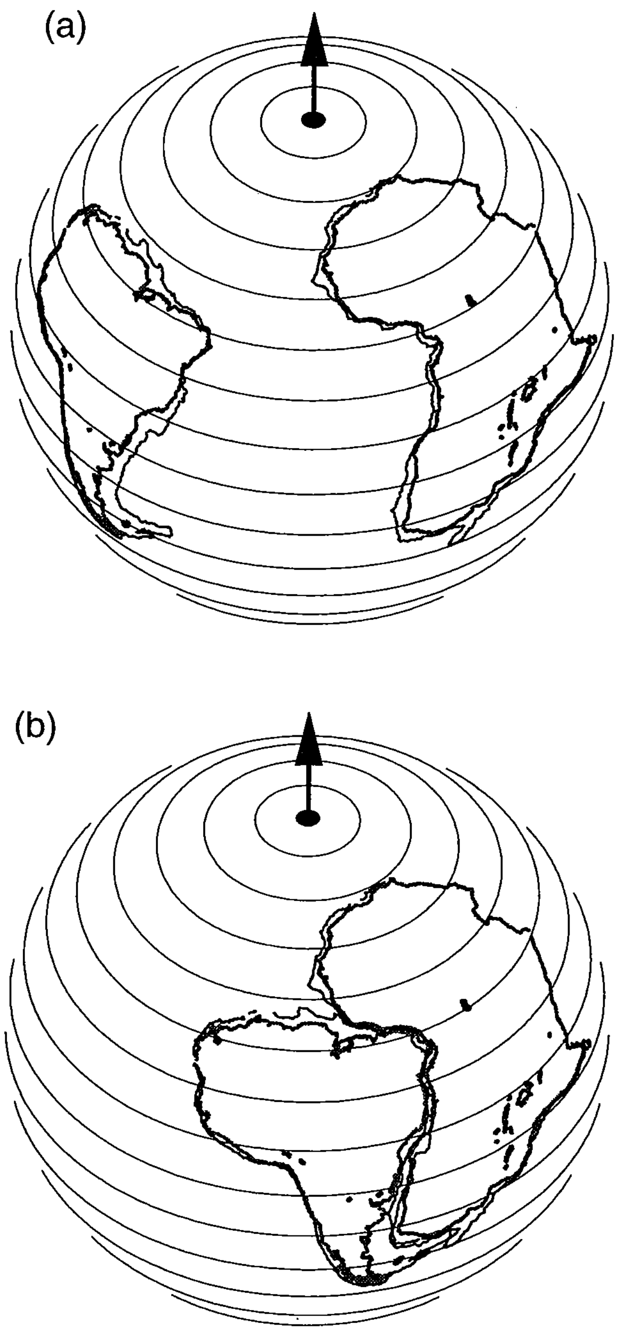
This is not the place to give a detailed account of the history of how there came to be a Department of Geodesy and Geophysics at Cambridge (now the Bullard Laboratories of the Department of Earth Sciences). But this place played such an important role in the development of our present ideas that I think I should give some account of its origins. Fragments of the story are described in the biographies of some of those involved: Sir Gerald Lenox-Conyngham, Maurice Hill, and Sir Edward Bullard, which have been written for the Royal Society.[5] The department was started by two people, Sir Gerald Lenox-Conyngham, a retired colonel from the Indian Army, and Professor Hugh Newall, who was professor of astronomy at Cambridge. The correspondence between them is now in the Bullard Archives in Churchill College, Cambridge. Their idea was to found an Imperial Geodetic Institute at Cambridge, like the Prussian Geodetic Institute in Potsdam, and on a similar scale. But there

was no money. The department therefore initially consisted of Lenox-Conyngham alone. He obtained a post for an assistant in 1931 and hired Edward Bullard, who was always known as "Teddy," and who had just obtained his Ph.D. (in quantum scattering of electrons) in the Cavendish Laboratory under Rutherford.

Teddy was a first-class experimental physicist. He started by using gravity to study earth processes, but was soon exploiting seismic methods on land and at sea, as well as the measurement of heat flow. His interest in the earth's magnetic field developed during the Second World War, when he was concerned with magnetic mines. He became frustrated when he returned to Cambridge after the war, where Lenox-Conyngham, now in his late 70s, was still head of the department (there was no retirement age), and he left for the University of Toronto. He did not return to Cambridge until 1956, after the group who worked there with Keith Runcorn had broken up when Keith left to become professor of physics at Newcastle. The paleomagnetic work on continental drift, which was carried out by Keith Runcorn, Ted Irving, Ken Creer, Jan Hospers, and others with such energy and success at Cambridge in the early 1950s, stopped with Keith's departure, and had surprisingly little influence on later developments–even at Cambridge, where it had been so prominent. Later discoveries have clearly shown that the conclusions based on palaeomagnetic measurements were correct, and their failure to convince the wider community of geologists and geophysicists remains to me the most interesting part of the history of plate tectonics.[6]

Teddy and Maurice Hill together developed a very active group concerned with marine geology and geophysics. Although they were in competition with similar larger groups at Lamont and Scripps, they retained excellent relations with the Americans. They attracted undergraduates whose backgrounds were in physics and who wanted to build instruments or do theory, and numerate geologists. I arrived as Teddy's graduate student in October 1963, after Fred Vine and Drum Matthews had published their explanation of how the magnetic stripes in the oceans are formed, and while Teddy Bullard, Jim Everett, and Alan Smith were using a well-known theorem discovered in the 18th century by the German mathematician Euler to reconstruct the position of the continents around the Atlantic before they were rifted apart.[7]

People often say to me how exciting it must have been to be at Cambridge at this critical time. But this is a retrospective view. At the time it looked quite different. Fred and Drum were searching for magnetic anomalies in the North Atlantic magnetic records from the many cruise records available at Cambridge, but found little convincing evidence



that their suggestion was correct. Why they were having such problems only became clear when Carol Williams, a graduate student at Cambridge, and I reanalyzed the same data in 1971, using what were by then standard methods.[8] The navigation was carried out with a sextant, by taking sights on the sun and stars (satellite navigation only came into general use in 1968). Because the North Atlantic is so often covered with clouds, the navigation errors in the data were sometimes as large as 30 miles (50 kilometers). So it was difficult to match anomalies between different ship tracks.

Another problem is that magnetic anomaly patterns from slowly spreading ridges like the North Atlantic are often variable and hard to recognize without a theoretical model. Teddy tried to find evidence that the boundaries between two geological provinces of different ages in Africa matched similar boundaries on the other side of the South Atlantic in South America. But the evidence was not very convincing, because rifting generally exploits such geological boundaries rather than cutting across them. I wrote a Ph.D. thesis on mantle convection, which taught me fluid mechanics and enough materials science to know that all materials creep at high temperatures and low stresses. Although it now seems strange, neither I nor the other graduate students changed thesis topics to work with Fred and Drum. The only person to join them was Tuzo Wilson from the University of Toronto, when he was on sabbatical at Madingley. He wrote his well-known paper on transform faults

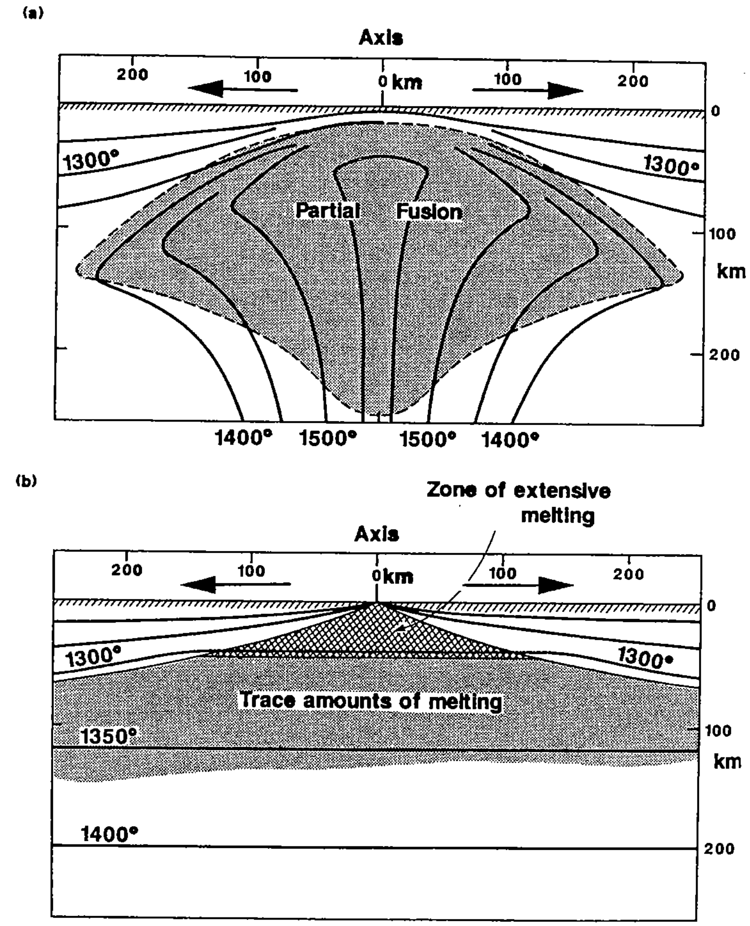
The fit between Africa and South America obtained by Teddy Bullard and his colleagues using Euler's Theorem (Bullard et al., 1965). The theorem states that any motion of a rigid plate on the surface of a rigid sphere corresponds to a rotation of the plate about some axis that passes through the center of the sphere. The problem on the Earth is that every point on its surface is on a moving plate, and no rigid sphere exists. So one plate must be chosen and taken to be fixed. Then the motion of any other plate with respect to this fixed plate corresponds to a rotation about an axis. In this figure Africa has been taken to be fixed, so South America moves. (a) shows the location of this axis, marked with an arrow, that Teddy and his colleagues found for the motion between Africa and South America. The circles are lines of latitude about this axis, just like the usual lines of latitude about the Earth's rotational axis. (b) shows the original position of the two continents before the South Atlantic opened, obtained by fitting the edges of the continents together. These edges are under the sea, and are not the present coast lines. As the continents move, every point on the South American plate moves in a direction that is parallel to the latitude lines. This behavior is easily seen by comparing the positions of the latitude lines in the two pictures before and after opening. Their position on South America does not change.

while he shared an office with Harry Hess from Princeton University, and a lesser known paper with Fred Vine on magnetic anomalies while Fred was still a graduate student at Cambridge.[9] As graduate students we were not especially stupid, and three of us (Bob Parker, John Sclater, and myself, in addition to Fred Vine) have since been elected to the Royal Society. At the time it simply was not obvious to us that what Fred and Drum were doing was so important.

October 1966–October 1968

Teddy persuaded the organizers of a NASA conference to invite me to talk about high temperature creep.[10] It was held in New York, close to Columbia, and was attended by many people whose names I knew from their publications, and to whom I was introduced by Teddy. Two of the papers made a deep impression on me: one by Fred Vine, who had moved to Princeton, showing that the Pacific magnetic anomalies beautifully confirmed his and Drum's suggestion, and the other by Lynn Sykes from Lamont, whom I met for the first time, showing that the sense of motion on transform faults agreed with Tuzo's proposal and could only be explained by sea floor spreading.[11] At last there was good data to confirm the earlier ideas. I returned to Cambridge, completely convinced I should work on sea floor spreading. I stayed for a month, during which I was examined for my Ph.D. I also carried out all the calculations for my first paper on plate tectonics, showing how a spreading ridge can account for the elevated heat flow that Teddy had found to be associated with what we now knew to be spreading ridges.[12] The idea for this paper came from work by Xavier Le Pichon and his colleagues at Lamont, although they had argued that the heat flow observations were not compatible with sea floor spreading. I carried out the necessary numerical calculations at the Seismological Laboratory at Caltech, where I went for six months as a postdoctoral fellow in January 1967, and was pleased with the paper.

Unlike Xavier, I used an analytical solution to the equations, which I obtained by first converting them to dimensionless form. I had learned how to do this from the fluid dynamicists, particularly Adrian Gill, who was then at Scripps, and my approach is still widely used to construct thermal models of spreading ridges.[14] However, what most pleased me was to be able to demonstrate that the heat flow (and, later, the topography and melt generation) of spreading ridges does not require a hot upwelling region of convection beneath the ridge, but can simply be explained by plate separation and the upwelling of hot mantle into the



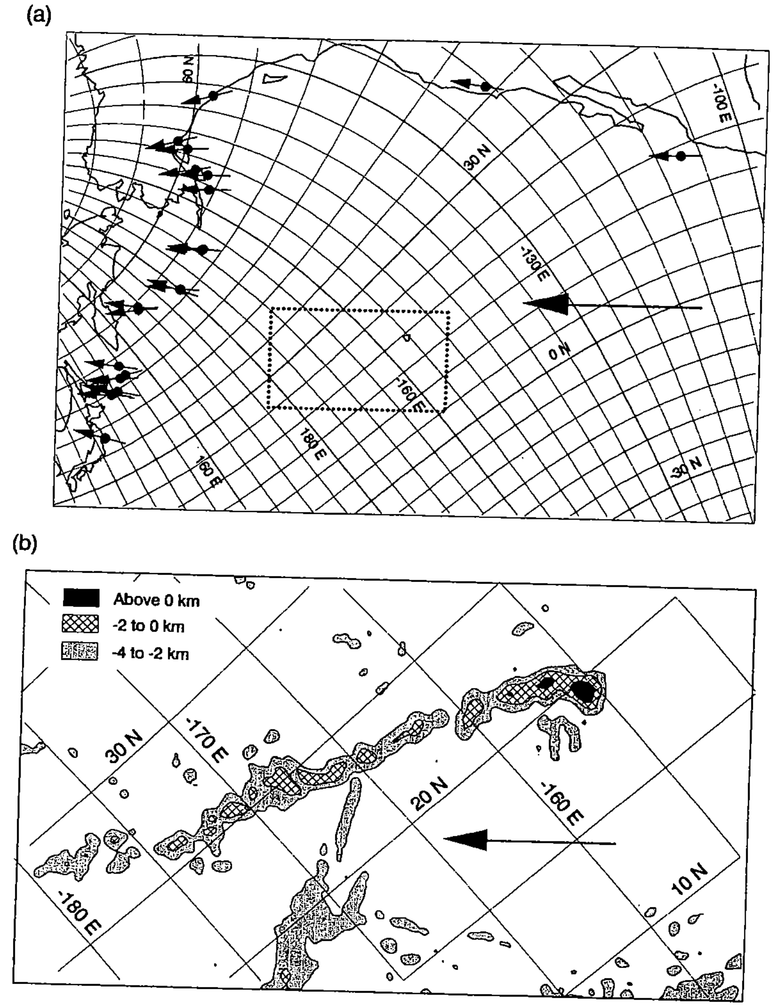
When I first became interested in plate tectonics it was generally accepted that ridges were everywhere underlain by plumes of solid mantle material that was hotter than the surrounding rock, and for this reason moved upwards. This proposal is illustrated in (a), but is very difficult to reconcile with how ridges behave. New ones form suddenly when continents split, their axes are offset sharply in many places by enormous faults, and they sometimes suddenly jump into older parts of plates. In the first paper I wrote on plate tectonics, I suggested instead that ridges are entirely passive, and are formed by passive upwelling of the same hot mantle that everywhere underlies the plates (D. P. McKenzie, 1967. Some remarks on heat flow and gravity anomalies, Journal of Geophysical Research, 72: 6261–6273). This model is illustrated in (b), and satisfies all the observations and avoids the problems faced by (a). (Modified from D. McKenzie and M. J. Bickle, 1988. The volume and composition of melt generated by extension of the lithosphere, *Journal of Petrology* 29: 625–679, and R. L. Oxburgh, 1980. Heat flow and magma genesis. In *Physics of the Magmatic Processes,* R. B. Hargraves, ed. Princeton: Princeton University Press, pp. 161–199.)

space in between the two plates. This idea resolved a long-standing puzzle, which I discussed with Tuzo when he was at Madingley in 1965. Africa is surrounded by spreading ridges, all of which have moved away from the continent as they spread. If they were convection cells, how could these cells know how to move at exactly the right velocity to remain beneath the spreading ridges on either side of Africa, thousands of kilometers apart? There is obviously no problem if ridges have no deep structure, but are simply formed wherever plates separate.

Most of my time at the Seismological Laboratory at Caltech I spent learning earthquake seismology. At the time, my geophysical background knowledge was rather poor. I knew a little old-fashioned geology, and some bits and pieces of geophysics I had learned during my three years as a graduate student. But Cambridge had (and has) no courses for graduate students in earth sciences, and no one worked on earthquake seismology at Madingley. So I took Caltech courses, some in earth sciences and some in physics, especially from the physicist Richard Feynman, whose showmanship annoyed me, but whose mastery of analytical methods I found dazzling.

I also went to my first American Geophysical Union (AGU) meeting, in the spring of 1967 in Washington, D.C. I have always found such meetings hard to exploit properly. I try to do so by going through the abstracts of the talks carefully, making a list of where I need to be when. Jason Morgan, from Princeton University, included an abstract of a paper he had recently published, which reinterpreted some Lamont data from the Puerto Rico Trench. I knew several of the Lamont people were cross with what Jason had done, and that they intended to give him a hard time, principally, I suspected, because Jason had reinterpreted their data. I was not involved and not interested, so I left the session just before his talk. As is now well known, he did not talk about his published paper, but about plate tectonics. His talk generated little interest, and I did not become aware of what happened until November of that year.

But two aspects of the meeting did make a deep impression on me. By April 1967, most marine geophysicists had seen the Pacific magnetic anomalies that I first saw in New York, and were equally impressed. They had looked at their own records, found similarly beautiful profiles, and submitted abstracts to the AGU meeting.[15] The result was a sea floor spreading bandwagon – what Drum Matthews always disparagingly referred to as 'me-too' science, and what I think Kuhn means when he talks about scientific revolutions as changes in worldview.[16] There were no new ideas, nor any proper modeling, just an overwhelming sense that the whole ocean floor was covered with stripes. The other was a paper



This figure shows the motion of the Pacific plate, obtained from the motion on faults during earthquakes, when the North American plate is fixed. The solid dots show the locations of a number of large earthquakes, produced by the motion between these two plates. The arrows show the direction of motion between the two sides of the faults on which these earthquakes occur when the side of the fault that is part of the North American plate is taken to be fixed and the Pacific side is moving. The map is in a special projection, chosen so that the motion of the Pacific plate is everywhere parallel to the big arrow if the plate moves rigidly. (b) Contours of the depth of the ocean round Hawaii, using the same projection as (a). If the volcano that forms the Hawaiian Ridge is fixed to North America, the ridge should be parallel to the large arrow, which is approximately true.

by Father William Stauder, from Saint Louis University, concerned with the mechanisms of earthquakes along the Aleutian Trench, which I misunderstood. Earthquakes are generated by movement on faults, when one side of the fault slides past the other. The direction of this motion is known as the *slip vector.* I thought Stauder had plotted these slip vectors, whereas in fact he plotted what he believed to be the directions of greatest and least stress. This rather technical issue sounds trivial but is not: plate tectonics is concerned with slip vectors, not with stresses, and the relative motion between plates produces earthquakes whose slip vectors show the direction of their relative motion.

I returned to Caltech, and moved to a postdoctoral research position at Scripps at the end of June. I had sent off the paper on ridges, and was looking around for other good problems. I read all of Harry Hess' papers and had returned to Teddy's paper on the geometric fits because I remembered I had disliked the method he used to fit the continents together.[17] I was reading his section on Euler's theorem when it occurred to me that this was what was needed to describe plate motions. Once I had the idea of doing so, everything else followed, and within a day or two I had thought through the consequences. I saw that I could divide the surface of the earth into plates using the earthquake zones, and use a National Geographic globe with a transparent plastic cap to work out where the relative poles of rotation were from the transform faults and other features on the plate boundaries. My problem was that I had no data, such as spreading rates from magnetic anomalies, because the Scripps data had not been digitized or interpreted. Then I remembered Stauder's AGU talk, and realized I could use the slip vectors from earthquakes that he and his students and colleagues had determined to obtain the direction of relative motion between the two sides of the faults which moved in earthquakes (which I found he had not done), and a mapping program that Bob Parker, who had also moved to Scripps, had written with a projection that made the whole idea simple. I explained everything to Bill Menard at Scripps, who could see nothing wrong and soon became equally enthusiastic.

Shortly thereafter, John Mudie, who was also doing postdoctoral reseach at Scripps, returned from a conference at Woods Hole and gave an account of Jason Morgan's talk there. Although John's account was somewhat unclear, it was immediately obvious to me that Jason had had the same idea as I had, and I asked Bill what I should do. Without hesitation he said, "Publish." I thought I could write a nice compact paper on the idea and used this as a carrot to write up the last part of my Ph.D., a long, dull, worthy paper on the effect of rotation on convection in a rotating sphere.[18] As soon as I finished, I wrote the first published paper on plate tectonics with Bob Parker.[19]

It took only two or three days. Bob and I went down to the main post office in La Jolla in late October to send it off, but were nearly defeated. It was Saturday, so the office was closed. But we got quarters and fed the stamp machine until we had enough stamps to send it to the journal *Nature* in London by air mail. There was just enough space for the address. We never saw the paper again. *Nature* received it on November 14, but we never got referees' comments or page proofs. The paper was published on December 30, 1967.

Soon after Bill Menard told me to publish he received Jason's paper to review.[20] In his book Menard says that he received a pre-print of this paper in the spring of 1967, but he never mentioned this to me, either at the time or later.[21] I don't remember reading Jason's paper at the time, although I may have done so. Jason and I took different approaches to explaining the same ideas, and used different data, from seismology and from marine magnetic anomalies, to show that the ideas worked in practice.

When I wrote the *Nature* paper I was inexperienced, both in writing scientific papers and in presenting data. It was only the fourth extensive paper I had written. Unlike Jason, I made little attempt to explain how the ideas connected with earlier work, and tried to be as economical with words as possible. I did not publish the figure I drew showing the observations themselves. I have always regretted not doing so, and therefore show it here as the figure on page 179. My other regret concerns Hawaii. The projection Bob and I used suggested that the Hawaiian Ridge was formed by a volcano that was fixed to the North American plate, because it was parallel to the direction of movement between the two plates. This result was incomprehensible at the time, and furthermore confused the whole theory, which was constructed in terms of relative motions between plates, and explicitly rejected the whole concept of absolute motions. So I decided to say nothing.

Jason noticed the same thing, and proposed an explanation which has been widely accepted.[22] He argued that Hawaii was on top of a hot part of the mantle that is rising toward the surface, because its density is lower than that of the surrounding colder mantle. Such structures are called *convective plumes.* He proposed that there are a number of such plumes beneath volcanic islands such as Iceland, Reunion, and the Society islands, and that these plumes were all fixed to each other, and did not move around like the plates. The volcanic ridge that extends northwest from Hawaii is then produced as the Pacific plate moves in this direction over the plume. I have never liked this idea, because I do not understand how the plumes can be fixed to each other if the whole mantle is convecting. If the whole mantle consists of moving fluid, how can anything be fixed rigidly to anything else?

I have sometimes been asked whether the approach I used to describe plate tectonics was modeled on the theory of the dynamics of rigid bodies that led Euler to his theorem. It was not. It was, however, strongly influenced by the theory of dislocations, and especially by W. T. Read's book on the subject, which showed how useful the idea of a dislocation was, even though its behavior could not be calculated from the equations that control the electrons and nucleii of which a solid consists.[23]

I left Scripps at the end of October, and drove my old car with all my possessions across the country to Lamont, where I stayed for three months. I found everyone there working on plate tectonics. I discovered that Jason had talked about his ideas at the AGU meeting in the spring, and thought I should try to delay the *Nature* paper so that he would have priority in publication as well as in his discovery of the theory. I was not successful because *Nature* had already set the type. I am now glad that I failed, because our discoveries were independent. I was somewhat dismayed by the number of people working on plate tectonics at Lamont, because I have never liked working quickly and in competition with others. Xavier Le Pichon had finished a global reconstruction of plate motions before he returned to France; Lynn Sykes, Bryan Isacks, and Jack Oliver were writing their monumental review of the implications of plate tectonics for seismology; and Jim Heirtzler and Walter Pitman were systematically going through all the marine magnetic data to work out the history of the ocean basins.[24]

I found Lamont quite different from Madingley: the people were much more competitive and uninterested in theoretical ideas and calculations. People were also secretive about what they were doing. What was clear, however, was that the basic ideas of plate tectonics worked very well in the oceans, but not on the continents, where the deformation is widely distributed. So I decided to ask Lynn Sykes and Peter Molnar, who was then a graduate student at Lamont, to teach me how to construct fault-plane solutions for earthquakes, and see if I could modify plate tectonics to describe continental as well as oceanic tectonics. My colleagues, students, and I have since spent many years working on this problem, which is much harder than plate tectonics.

In the spring of 1968 I moved again, to Princeton, where I at last came to know Jason well. Although I knew that simultaneous discoveries had occurred commonly in many areas of science, I was astonished to discover that he and I had been working on the same problems in the same way for much of the previous year. As Menard points out, most of the discoveries that led to plate tectonics were made independently by two people.[25] But I was still amazed when the same thing happened to me. And it was not just that we had both thought of plate tectonics. Jason had also used the plate model to calculate the heat flow and topography of ridges, and Stauder's slip vectors of earthquakes to determine the poles of relative motion between the Pacific and North American plates.[26] We had both spent the previous year solving the same set of problems by the same methods. This discovery surprised me profoundly: I had not understood that scientific enterprises reach a particular point where the next step is obvious to those who use a particular approach, and that it is a matter of chance who will receive the credit.

The last major theoretical problem in plate tectonics concerned what happens where three plates meet. Jason and I discussed this problem at Princeton without getting anywhere. Tanya Atwater, who was still a graduate student at Scripps, came on a visit and joined in, also without much progress. Even though the problem is purely geometric, we found it surprisingly hard to solve. The solution came to me long after these conversations, in the autumn of 1968 when I had returned to Cambridge. Jason and I published a theoretical paper, which both posed the problem (which did not seem to have worried other people) and solved it, and Tanya worked out in detail the important geological consequences that the evolution of the triple junctions have for the tectonics of western North America.[27]

WHAT IS PLATE TECTONICS?

For me the central idea is the rigidity of plate interiors. It is this property that allows the surface motions of the earth to be described by so few parameters. Early versions of the theory – continental drift, paleomagnetic reconstructions, and sea floor spreading – implicitly or explicitly used this property, but did not recognize its importance.[28] I myself do not describe continental tectonics as plate tectonics, because continental deformation occurs in wide zones where the idea of rigidity is of limited use. Geologists such as John Dewey and Jack Bird recognized that the continental geological record contains structures and stratigraphy produced by plate boundaries, and have sketched plate geometries that could generate the features concerned. But they are unable to show that the motions involved were those of rigid plates, and in many cases I suspect, but cannot yet prove, that they were not. I believe that the type of distributed deformation that is now occurring in the eastern Mediterranean is a more accurate model for most continental deformation than is the tectonics of rigid plates.

A number of later developments are often loosely described as plate tectonics, such as studies of forces that maintain plate motions. Loose use of carefully defined terms is common among geologists, and leads to endless terminological confusion and controversy of the most sterile kind. Plate tectonics was clearly defined as a kinematic theory: one that is concerned with geometry. It is not a dynamic theory: one that is concerned with the driving forces.

In my view, plate tectonics was discovered by the group of paleomagnetists working at Madingley in the mid-1950s, in a specially built nonmagnetic hut that still exists, when they showed that the relative motion of continents could be described by continuous rigid rotations. What Jason and I did was to reduce the ideas to a set of physically reasonable propositions that are sufficient to describe all the earlier proposals that had been shown to work. Who invented the term *plate tectonics* itself is unclear. Several people tried to coin a term, partly (it must be said) with the aim of being able to say that they discovered the theory. None of these proposals stuck. One of the earliest uses of the term that I know of in print was by Jason and myself in our paper on the evolution of triple junctions in 1969. But I certainly would claim only to have written down a term by which the theory was by then widely known.

It seems to me unlikely that plate tectonics will require changes. It is a precisely formulated theory that provides an accurate description of the large-scale tectonics of the earth. Physical theories of this type are with time incorporated into larger and more complete descriptions of processes involved. This process will require studies of other tectonically active rocky planets. Sadly, our solar system does not at present seem likely to be of much use.

Reflections on the Process of Scientific Discovery

Few active scientists take much interest in general models of scientific discoveries, and I am no exception. However, like many physical scientists, I know a bit about the ideas of Francis Bacon, Karl Popper, and Thomas Kuhn, and it is perhaps of interest to those who do study the history and philosophy of science to see whether those who have been involved in scientific advances find such models useful in describing their activities.

Many historians of science have been concerned with theories in physics and chemistry. These are fundamentally different from those in areas like astronomy and earth science, because experiments are not only possible, but are an essential part of the development of any new theory. In many parts of the earth sciences, including tectonics, experiments are impossible: nothing human beings can do can affect the processes involved, and these cannot be scaled properly to conduct laboratory experiments. This difference affects every aspect of the subject: you watch what is happening, but cannot isolate the process that interests you. There is always "noise": processes that are going on which don't interest you. This inability to isolate a particular process also has advantages, because your observations contain information about processes other than the one that you intended to study. So you must always keep an eye open for the unexpected.

Because controlled experiments are impossible in so many areas, hypothesis testing in its strict form is not an activity familiar to most earth scientists. I spend my time trying to construct models that can describe what I and others have observed, using ideas from mathematics and other physical sciences. I judge I have succeeded when I find that my model can account for some well-known observation that was not understood, and which it had not occurred to me to connect with the observations that I was trying to model. For this test to be reliable, it is essential that I separate all observations that I come across into four groups: those that are simply wrong, those whose origin I understand, those that are so complicated that I am never going to know whether or not I understand them, and those that I am sure that I cannot understand using our existing theories. At any time there is usually general agreement that a number of existing observations fall in my last group. For instance, at the present time, I think most earth scientists would agree that our theories of convection in the mantle are in this state, because the seismological observations are best explained by whole mantle convection, whereas the geochemical observations suggest that the upper and lower mantle convect separately.

Another problem in this group is the formation of the solar system. Furthermore, I am sure that major new ideas will come from research in these areas. The last group is therefore the important one, and it is not large; at any time few problems in it are tractable. In the earth sciences, and I think in astronomy also, advances occur when someone realizes that a major problem in the last group has become tractable, generally because of an advance in technology that can be used to make a new observation. There are many examples in both fields of such events. In the case of plate tectonics, it was our ability to measure the magnetization of rocks, first in the laboratory and then at sea with a towed magnetometer, and to determine the mechanisms of earthquakes, that rapidly led to the final theory. The paleomagnetic observations did not have the impact that in retrospect they should have had, because almost all earth scientists thought they were incorrect, and so put them into the first, rather than the last, of my groups. Why they did so remains for me a puzzle, and also the most interesting historical question to be raised by the discovery of plate tectonics.[29]

I certainly would not describe Jason's and my activities in 1967 as hypothesis testing: as soon as I realized that earthquakes and their mechanisms were the direct expression of plate tectonics, I knew I was right! The great and immediate success of the theory was the result of everyone else reacting in the same way.[30] Everyone knew about earthquakes, and no one knew how they were produced (this is not quite true: most believed earthquakes resulted from slip on faults, but no one understood how the same fault could slip in the same direction time after time). So in 1967 most people put earthquakes in my last group. What made plate tectonics so immediately convincing was that it was principally designed to account for sea floor spreading, continental drift, and magnetic anomalies. With no further input, it also accounted for the distribution of earthquakes, which in the oceans lie in narrow bands on plate boundaries.

Plate Tectonics after 1968

The final version of the theory was rapidly understood and accepted by earth scientists everywhere. In the Soviet Union, however, the new ideas were resisted by a few elderly geologists who were heads of their institutes, principally I suspect because these people found it hard to think in the new way. In the west most observations from terrestrial and marine geology were quickly reinterpreted in terms of the new theory. But at first most of this work was not sufficiently carefully carried out for me to be able to put the observations into one of my four categories. The problem was that plate tectonics is a much more precise and rigid theory than most of those with which geologists were familiar. To test whether it works requires computers and a variety of programs: to determine poles of rotation, to reduce magnetic observations to projected profiles of magnetic anomalies, to calculate theoretical anomaly profiles from the reversal timescale, to plot fault-plane solutions of earthquakes, and to

digitize and plot maps of reconstructions by carrying out rigid body rotations. Of the main laboratories involved in marine geology and geophysics in early 1968, only Lamont was in a position to test the new theory properly. Several of the important programs had been written by Xavier Le Pichon, and ceased to be used at Lamont when he returned to France. During 1968, these problems slowly became clear, so John Sclater, who was at the time at Scripps, and I decided to do a thorough job on the Indian Ocean, the one ocean that was largely being neglected by those working at Lamont.

My proper education as an observational scientist now began. John and I went to sea to collect our own data. We negotiated with everyone else who had data from cruises. We discovered that Project Magnet, a U.S. military program that had flown magnetometers to make maps of the magnetic field, had flown many profiles across the Indian Ocean, navigated by someone making star sights from the cockpit with a sextant, as mariners do. We digitized all these data, and reduced them to the format used by Lamont, using a small computer on which we could get free time at night (no one would give us a decent grant to do things properly, because neither of us had faculty positions). But the programs ran very slowly. It took several hours to produce a reconstruction of the plates at an earlier geological period on the small computer that we used, which was the size and weight of a large American refrigerator. Each plot required us to pass a few thousand cards through the card reader, containing the locations of the coastlines. Even the main computer, which served the whole campus of the University of California at San Diego, to which Scripps belongs, would have taken an hour or so for each plot, and cost $300 an hour, which we could not afford. I now carry out such calculations in a few seconds on a notebook computer the size of a small book, which is more than 100 times faster than the main campus machine was in 1967. However, the original programs still work fine on the notebook, and I used them to make one of the figures for this essay. We discovered a program for disadvantaged undergraduates that would pay them the minimum wage if we could produce 25 cents an hour, and trained them to reduce and plot the observations. I wrote the necessary programs, some while I was at sea. I also wrote programs that I thought would be able to recognize magnetic anomalies automatically (they could not), and spent months deciding whether I could recognize individual anomalies on the profiles.

The end result of all of this work was a monumental paper that filled one entire issue of the *Geophysical Journal of the Royal Astronomical Society*, and outlined the whole of the plate tectonic history of the Indian Ocean, and the new methods that were required to carry out such an analysis.[31] The main result was that the evolution could be accurately described in terms of plate tectonics. I was very disappointed. As far as I could see, we had done a great deal of work and had discovered nothing new. The entire enterprise therefore belonged in my second category. For this reason, I left mainstream marine geology and geophysics to work on the forces that maintain plate motions, and on continental deformation. Later events, especially from ocean drilling, have confirmed my initial reaction. Five years later in 1976, when I was elected to the Royal Society, I was both surprised and cross to discover that I was elected because of my work on the Indian Ocean, and not because of plate tectonics. The committee involved consisted of field geologists, and they were impressed by someone who could successfully work out the geological history of an entire ocean basin.

By 1969 the present theory was essentially complete. The only important new idea that has been required is that of propagating ridges, which Dick Hey of the University of Hawaii suggested to explain the shapes of magnetic anomalies in the eastern Pacific.[32] The development of the theory stopped very suddenly: in the 1960s continental drift became sea floor spreading, then plate tectonics, as the theory became more precise and as its scope increased. Then, equally quickly, the changes stopped: the theory was complete and rapidly became accepted. Many of the participants, including me, found it hard to adjust to this sudden change. The ideas, methods, and programs that they had developed with considerable intellectual effort a few years earlier became part of the standard undergraduate and graduate courses. One of the first courses on plate tectonics, which I taught at Cambridge in 1970, differed little from what is now taught everywhere. What took me by surprise was how quickly the ideas became detached from their originators as they became accepted. Except for those who were involved, and for historians of science, no one now knows or cares who was responsible for a particular part of the theory. It is even hard for modern undergraduates to understand that the whole theory is so new and caused so much exitement. They, quite reasonably, ask, "So, what did people believe before plate tectonics was discovered?" This is a question that I find unexpectedly difficult to answer, because I cannot remove the understanding that people now have to reconstruct our state of ignorance in the early 1960s. One young faculty member in China knew Dan McKenzie had been one of the people involved in the discovery of plate tectonics, but was astonished to meet me. He thought it happened so long ago that all of those involved were dead.

The effect of these discoveries on the careers of those involved was dramatic, especially on those of the younger people. Several of us had only just finished our Ph.D.s when we were invited to give the major talks at national and international conferences, elected to the Royal Society and the National Academies of the United States and France, and given major prizes by geological and geophysical societies. I found this all very flattering, although I have always regarded my success as a piece of luck. If my parents had not decided so early that Hitler was not going to win the war, I would have been too young to have been involved. This worldly success removed any concern I had about whether I would be able to obtain an academic job when I ceased to be a post-doc. What concerned me much more was whether I was going to be someone who had only one good idea, or whether I would be able to make progress with some of the harder problems in the earth sciences that were still not understood, especially the tectonics of continents. My uneasiness about this question did not evaporate until 1978, when I discovered how sedimentary basins formed, by stretching wide regions of the continents. This idea is the antithesis of plate tectonics, because the deformation is distributed over a wide region, rather than occurring on a single plate boundary. This difference is probably why both I and everyone else were so slow in understanding what was going on. It was also one of our first successes in our efforts to understand continental deformation, which is a harder problem than plate tectonics, and is a less dramatic story that still continues. Perhaps one day I will write an account of this effort, which is so different from the discovery of plate tectonics. But not here!

ACKNOWLEDGMENTS

In the four years after I obtained my Ph.D. in November 1966, I worked at five different universities (Cambridge, Caltech, Scripps, Lamont, and Princeton) for periods that never exceeded six months. Throughout this time I held a research fellowship at King's College Cambridge, and will always be grateful for its support. King's paid, housed, and fed me when I was in Cambridge, and provided a stable point in my peripatetic life. At the beginning of the period, I was generously supported by grants from the U.S. Air Force at Caltech and Scripps, and lived in comfort. But toward the end there was less and less money. At one stage, John Sclater signed a purchase order for 20 days of my services, in the same way as he purchased laboratory supplies. The money enabled me to live at Scripps for four months (extremely frugally and in fear of a knock on the door from the Immigration Service!). I will always be grateful to the people, principally Don Anderson at Caltech, Walter Munk at Scripps, and Harry Hess at Princeton, who gave me the opportunity to be involved in this work at the beginning of my scientific career. The enlightened generosity of these people and of the organizations from which their support came, especially the U.S. Air Force, the Office of Naval Research, and the National Science Foundation, made a deep impression on me, and left me with a lasting admiration for the United States and its way of doing science. This is Earth Sciences Contribution number 6240.

CHAPTER 12

WHEN PLATES WERE PAVING STONES

Robert L. Parker

FOR ONE TIME ONLY, AT ITS INCEPTION, PLATE TECTONICS WAS called the *paving-stone theory of tectonics,* the name Dan McKenzie and I gave to the organization of the earth's surface into a small number of internally rigid bodies in relative motion. When Dan and I wrote the first paper on plate tectonics, "The North Pacific: An Example of Tectonics on a Sphere," we were two unknown new Ph.D.s, fresh out of graduate school.[1] My thesis had been on the mathematical modeling of electrical currents in geophysical systems, and Dan's was on the shape of the earth, so we were both beginners in the science of geology. But we were fortunate to be working as students in a department at the center of a whirlpool of intellectual activity that was bringing about the first true understanding of marine geology and its importance for global tectonics. This essay is a brief review of the scientific and personal events leading up to that first paper.

THE VIEW FROM CAMBRIDGE UNIVERSITY, FALL 1967

In the fall of 1967 I was a postdoctoral fellow at the Institute of Geophysics and Planetary Physics (IGPP), which is part of the Scripps Institution of Oceanography in the University of California at San Diego (UCSD). I had graduated the year before from the Department of Geodesy and Geophysics at Madingley Rise in the University of Cambridge in England. Although I was working in the United States, which was to become my home, my perspective at the time was that of a Cambridge graduate. During my three years as a research student I had not worked on marine geology or indeed anything remotely geological. But it was impossible not to be aware of the great events going on in the department, which seemed velocities of today back into the geological past, other factors controlled the evolution of the boundary shapes. Initially we hoped some general principle might be discovered governing these factors, but that goal has proved elusive; there appears to be no alternative to a painstaking empirical analysis of the geological record. Nonetheless, plate tectonics succeeded in providing the framework for making sense of the large-scale processes governing the development of the earth's crust.

Subsequently, I did not make the further development of paving-stone theory a major part of my scientific career. I did some minor work on kinematics of plates; I helped my Scripps colleagues delineate the fine details of the marine magnetic anomalies using their near-bottom magnetometer system.[13] Perhaps my only other important contribution was as co-author on the first paper explicitly stating the square-root age rule for sea floor depths.[14] I was caught up in another revolution in the earth sciences going on at about the same time: the creation of geophysical inverse theory. In addition to the pioneers of plate tectonics, the two founders of modern inverse theory, George Backus and Freeman Gilbert, were also sabbatical visitors in Cambridge during the 1960s. I was extremely fortunate to get to know them personally, and to become their colleague in due course. Inverse theory is the set of mathematical methods that allows one to draw sound conclusions from a physical model in the face of severely incomplete and inaccurate measurements – a common situation in earth sciences.[15] Here was a subject in which I could indulge my personal fascination with abstract mathematics to a much greater extent than in plate tectonics. It was clear in 1967 that an enormous amount of work lay ahead to confirm the model of plate tectonics, work that would involve the synthesis of great quantities of geological and geophysical information. I knew my talents lay in another direction.

The term *paving-stone theory* appears six times in the first *Nature* paper, once in the abstract (which the *Nature* editors wrote). Everywhere else (21 times) we refer to the inactive interior regions as "plates" in a completely modern and familiar way. I am not sure who first used the term *plate,* but in any case our name for the new tectonic system, *paving-stone theory,* did not receive popular favor. But even if the metaphor of the paving stone failed to catch on, the concept it described has proved to be much more durable.

CHAPTER 13

My Conversion to Plate Tectonics

Xavier Le Pichon

I HAPPENED TO BE WORKING AT THE LAMONT GEOLOGICAL LABORATORY (now Lamont-Doherty Earth Observatory) of Columbia University in New York while the plate tectonic model was elaborated, first from September 1959 to September 1960, before my military service, and then from February 1963 to February 1968. Here, I present my views on the plate tectonic conception from the perspective of someone who was at the key acting laboratories. This testimony does not pretend to be an exhaustive and impartial history of the elaboration of the plate tectonic concept. I make extensive use of earlier papers that I have published on the subject.[1] Finally, I briefly place these views within the context of the evolution of plate tectonics from an ocean-based model in the 1970s to a space-based one today.

Lamont: Fixists Versus Mobilists

The revolution of ideas that led to plate tectonics, from 1955 to 1968, was greatly influenced by the continuous interaction among scientists of three laboratories, Lamont and Princeton University in the United States, and Cambridge University in England. Each of these laboratories was dominated by a strong personality: Maurice Ewing at Lamont, Harry Hess at Princeton, and Edward ("Teddy") Bullard at Cambridge. Although of quite different origins and intellectual capacities, they had in common a deep interest in the geology of the oceans. It was Richard Field, a professor at Princeton, who generated this interest in Ewing, Hess, and Bullard during the 1930s.

Maurice Ewing inherited from Field a burning zeal for the exploration of the oceans. With him, marine geology entered a new era. From

Xavier Le Pichon, on the *Vema,* in 1966 or 1967. (Photo courtesy of Xavier Le Pichon.)

the scattered approach based on discontinuous point measurements, Ewing moved to a global approach based on continuous measurements. He was the first to deliberately install himself within the oceanic world, inventing ad hoc the tools he needed to obtain the maximum amount of new data on every kind of subject. Although he was a theoretician by training, he was not comfortable with speculation. He made a religion of data.

When I arrived at Lamont in 1959, with a Fulbright Fellowship to study oceanography, "Doc," as Ewing was known by his students, sent me around the world on his three-masted schooner, the research vessel *VEMA*. "Oceanography has to be learned at sea," he told me. His deep interest in the exploration of virgin territories probably came from his northern Texas origins. He loved to be where nobody else was. When I told him in 1968 that I had decided to go back to France, he asked me how I could return to such an old country. "If I had to start a new life today, I would go to Australia." Actually, when he did move, he went back home to Texas. But, if Texas was always close to his heart, the ocean remained to the end his real Wild West. I believe that right up to the time of his death in 1974, he still did not accept that plate tectonics had succeeded in revealing the secrets of "his" ocean. In 1970, he confided to me that each time his ship came back, he was waiting for the new evidence that would show that the whole plate tectonic model was wrong: the ocean could not be that simple.

The big thing at Lamont in 1959 was the discovery of the rift valley that runs along the crests of mid-ocean ridges. Earthquakes and volcanic eruptions characterize the whole length of the rift. *VEMA* cruise 16, in which I was going to participate, was supposed to test the continuity of the rift valley from the Atlantic Ocean to the Indian Ocean. Ewing and Bruce Heezen (one of his collaborators who later acquired worldwide fame through the physiographic diagrams of the oceans he made with his associate Marie Tharp) had predicted in 1956 the continuity of the rift valley through the oceans along the mid-ocean seismic belt.[2] This seismic belt had been described in 1954 by a Frenchman from Strasbourg, Jean-Pierre Rothé.[3] We were going to zigzag for nine months above this famous seismic line to test the prediction. As it was estimated to be 37,500 miles (60,000 kilometers) long, the almost unknown rift valley suddenly became the most important structure on Earth. It became clear then that no model of the evolution of the earth that ignored the rift could be considered valid.

This fundamental discovery made by the Ewing team had followed another, of similar magnitude, made in 1955 by Ewing and Frank Press at Lamont.[4] Seismological observations had actually established what had been inferred from gravity measurements by geophysicists: the uplift of the crust-mantle interface (called the Moho after Yugoslav seismologist, Mohorovičić) to a depth of about 3 to 6 miles (5 to 10 kilometers) under the ocean floor. Under the continents, the Moho lies at a depth of about 20 miles (30 kilometers). The oceanic crust is on average four times less thick than continental crust, and the great difference between the continents and oceans pleaded for separate origins and distinct evolutions. Up to that time, most seismologists argued on the basis of the difference of propagation of surface seismic waves across the Pacific and the Atlantic that the Pacific Ocean was the only true ocean, whereas the Atlantic Ocean had an intermediate-type crust. Ewing and Press' discovery of a shallow Moho under the Atlantic eliminated the concept of an intermediate-type crust there. It also laid to rest the possibility that the mid-Atlantic ridge consisted of remnants of continental crust. From then on, the whole debate about the dynamics of the earth would be concerned, first, with the significance of this radical difference of structure between oceans and continents and, second, with the significance, within the oceans, of the rift valley.

At Lamont, two schools of thought prevailed. To Heezen, then a young geologist who had just finished his thesis under Ewing's direction on the morphology of the northern Atlantic Ocean, everything could be simply explained if one accepted the idea of rapid Earth expansion. Warren Carey of the University of Tasmania, at a symposium there in 1956, argued that oceans were geologically recent structures formed by expansion from the rift.[5] This was sea floor spreading without subduction. Heezen was consequently considered a mobilist. Mobilists were either expansionists, followers of Carey, or drifters, followers of Alfred Wegener. But Maurice Ewing thought that the idea of such a fast expansion (a 75 percent increase in the earth's radius in 100 million years) was physically absurd. He remained a fixist; he preferred to explain the tectonic activity of the rift by deep convection currents that did not reach the surface but were the cause of extension and volcanism, without wholesale movement of the crust.

For Ewing, such speculations were premature. What did they bring to science? New facts were within reach of our dredges, corers, cameras, and magnetometers. With his younger brother, John, he was inventing marine seismic reflection, a technique that would continuously record the thickness of the sedimentary layers. This technique was soon to reveal the very thin ocean sediment cover, and its total absence near the rift. But Ewing could not stop Heezen from developing his ideas, and ultimately, conflict between the two men could not be avoided. Ewing could not accept that one of his scientists would act in a completely independent way, without any control of the director of the laboratory. In 1967, it would lead to an open and painful split.

During this whole time, the Lamont team was far from monolithic, contrary to what has often been stated since. There were two schools. One, which was more geologically inclined and included the students of Heezen, was mobilist and expansionist. The other, which was more geophysically inclined, and to which I belonged, was fixist and believed in long-standing ocean-continent distribution. Although Lamont faction leaders could hardly work with each other, the younger scientists had many vivid exchanges, especially when they were at sea. The debate was open and always stayed open.

PRINCETON AND THE SEA FLOOR SPREADING HYPOTHESIS

Walter Sullivan, the science chronicler of the *New York Times,* wrote that on March 26, 1957, Heezen presented at Princeton University a seminar about the newly discovered rift.[6] Harry Hess, chairman of the university's Geology Department, stood up to comment. He said in essence: "You have shaken the foundations of geology."[7] It is clear that this curious and open-minded man, who was always ready to reconsider his own hypotheses (which he apparently did not want to take too seriously), absorbed a great deal in this seminar. Hess, through a rather complex chain of reasoning, had become convinced that the oceanic crust is not chemically differentiated from the mantle, but consists of serpentinite, formed from partially hydrated peridotite. In this way, the mantle would crop out on the ocean floor. Hess had also become convinced, following the Dutch geophysicist Vening Meinesz, with whom he had worked on gravity measurements in the 1930s, that ocean trenches were convergence zones where the floor of the oceans buckles under the adjacent continents. Combining these hypotheses with the rift expansion concept, Hess revived the idea of convection currents as the driving force of continental drift, as proposed by British geologist Arthur Holmes in the 1920s.[8] Hess' conveyor belt was moving right up to the sea floor: the upper mantle rose along the rift where it became hydrated and moved undeformed from the rift to the trenches, only to plunge back into the deep earth.

This hypothesis takes into account the radical difference in structure between oceanic and continental crusts. It attributes the small thickness of oceanic sediments to the youth of the oceanic basins. The volcanic and extensional tectonic activity at the rift is explained by the divergence of the two conveyor belts – one moving east, one moving west. Hess' model was basically correct, yet it was originally based on one false hypothesis: we now know that the oceanic crust consists principally of basalt and not serpentine. The mantle is no more exposed on the ocean floor than it is anywhere else on Earth.

Hess' model was widely circulated as a contract report in 1960 and 1961, in many places, including at Lamont, although it was not published until 1962. In between, Robert Dietz proposed its now famous trade name of *sea floor spreading*.[9] Hess, with his usual open-mindedness, presented his ideas as a working hypothesis that should not be taken too seriously, as "an essay in geopoetry."[10] His caution may also have been at least partly due to the aggressiveness of the fixist school in the United States. Most of the senior geophysicists would then shoot at sight the few mobilists trying to present their ideas at the American Geophysical Union (AGU) meetings. Six years and one detour through Cambridge would be necessary to establish sea floor spreading as the prevailing model at Lamont as well as in the rest of the United States.

Cambridge and the "Vine and Matthews" Test

Paleomagnetism provided the decisive test. This test was proposed independently in 1963, in Canada by Lawrence Morley, and at Cambridge by Fred Vine. Both had good knowledge of the magnetization of rocks. Morley had studied paleomagnetism, and Vine had worked with paleomagneticists. At Cambridge, Edward ("Teddy") Bullard was well known for his interest in the earth's magnetic field and for paleomagnetic investigations. The point was crucial because, in contrast, there was no such tradition and little interest in this domain at either Princeton or Lamont. The only person interested in paleomagnetics in Lamont was Neil Opdyke, a young post-doc who had been hired by Ewing in 1964 to work on the magnetostratigraphy of the core samples. Opdyke has often stated how isolated he felt during his first years at Lamont.[11]

Yet by this time, paleomagneticists no longer doubted the existence of reversals of polarity of the earth's magnetic field.[12] For Morley and Vine, if there was sea floor spreading, the lavas that flow on the floor of the rift valley must be magnetized in the contemporaneous magnetic field, which is alternately "positive" and "negative." Lavas erupting on the sea floor today would acquire a magnetic polarity consistent with the earth's present magnetic field; at other times, in the past, when the field was reversed, the polarity of the lava flow would go the other way. The floor of the oceans must then consist of magnetized stripes parallel to the rift and having alternate polarities. Morley went further than Vine, as he rightly concluded that the resulting magnetic anomalies should be symmetric with respect to the rift. One should therefore be able to use them to measure the rate of sea floor spreading.

There was at the time no existing survey of the magnetic patterns surrounding a properly identified ocean ridge crest. In the North Atlantic Ocean, where the Lamont teams had mostly worked, and in the northern Indian Ocean, where Cambridge's Fred Vine and his instructor, Drummond Matthews, worked, the magnetic patterns were highly irregular. Nice linear patterns had been mapped by two Scripps Institution of Oceanography geophysicists, Arthur Raff and Ronald Mason, during a magnetic survey that was started in 1956 off the U.S. west coast, but no rift was known there.[13] Vine's paper (published with Matthews as a coauthor in *Nature* in 1963), as well as Morley's paper (rejected by *Nature* and the *Journal of Geophysical Research* in 1963, probably in good part because it contained no new data) were completely ignored.[14] I remember that Charles Drake at Lamont called my attention to Vine and Matthews' paper but, to my knowledge, the paper was not seriously debated among us. As stated by Vine, when the sea floor spreading magnetic anomaly concept was proposed, few actual observations supported it and, in a way, this concept created more problems than it solved. The absence of supporting observations at the time is demonstrated by the fact that neither Vine, nor Matthews, nor Morley, nor anybody else considered any follow-up to these two papers during the following two years.

Once more, it was Harry Hess who was to open a new pass. I shall not describe in any detail this detour through Cambridge, on which I have no direct information and which is described elsewhere in this book.[15] But one should note that Hess had already played a major role in the elaboration of Vine's ideas when he presented his own ideas at a most remarkable British institutional meeting, the annual interuniversity Geological Congress organized by the undergraduate students, in January 1962. In January 1965, Hess came back to Cambridge for a sabbatical with Tuzo Wilson, a Canadian geophysicist gifted with a stunning vitality and extraordinary intuition. Wilson had been converted in 1963 to sea floor spreading, to which he had immediately proposed a corollary, the "hot spots" hypothesis. The idea came to him while flying over the Hawaiian islands: the Hawaiian islands would have been formed by a deep mantle hot spot acting as a torch on the overlying drifting ocean floor. As the ocean floor drifted over the hot spot, a chain of volcanic islands would be created.

The association of Hess, Wilson, and Vine was a prodigious one, and when the Tuzo Wilson "hurricane" had dissipated, the essential notions of plates, plate boundaries, and transform faults (when two adjunct plates slip as they move against each other at a ridge crest) were established.[16] Starting from Hess' ideas and an intuition of Vine on the

Atlantic equatorial faults, Wilson established the rules of plane plate tectonics.[17] Then, on theoretical bases and following again a suggestion by Hess, Wilson identified the Juan de Fuca Rise, in the Pacific Ocean west of Canada, and had Vine identify the magnetic anomalies and the sea floor spreading rate. The symmetry of the anomalies was rather good, despite the modest rate of sea floor spreading, but the modeled relationship of the anomalies to the chronology of the earth's magnetic field reversals was rather poor. This was not surprising, for it was later recognized that the chronology available at the time was incorrect. Yet, the time had come to test the predictions.

It is somewhat surprising that Bullard seems to have made no contribution to this episode, for in the preceding year he had presented a paper at the Continental Drift Symposium in London in which he applied for the first time the rules of motion for rigid spherical caps on a sphere to the reconstruction of continents before the opening of the Atlantic Ocean.[18] Before the Second World War, the French scientist Boris Choubert had made the first fit of the continents precisely along their continental margins. Later, Carey, whose work influenced Bruce Heezen, had tried to demonstrate that such a fit required using a globe with a smaller radius.[19] This symposium had clearly revealed the difference between the British scientists, who were now almost all mobilists (essentially because of the recent paleomagnetic results), and the American scientists, who were still mainly on the fixist side.[20]

Return to Lamont: Testing Time

What were we doing at Lamont during this time? We were exploring the world ocean, from rift to trench, from the Atlantic to the Pacific through the Indian Ocean. Ewing kept two ships at sea permanently. He believed that the earth could not be understood unless it was studied globally, using every scientific discipline. He was constantly looking for new technologies which, more often than not, were introduced for the first time as a standard tool in the ocean by Lamont teams: underwater photography, seismic refraction, continuous magnetic and gravity recording, continuous seismic reflection, heat flow apparatus on piston corers, nephelometer (an instrument for measuring particles suspended in seawater), satellite navigation, and more. Manik Talwani, a Lamont gravity specialist, had organized an entirely computerized data reduction and storing system, and Lamont was the only laboratory with a complete set of data on the world ocean that could be rapidly and easily retrieved. Furthermore, the Seismology Department, under the leadership of Jack Oliver, used the global seismographic network installed in 1962 by the United States to initiate a systematic study of global seismology.[21] No other laboratory had similar potential to test Hess' hypothesis, and the autocratic direction of Maurice Ewing imposed a multidisciplinary approach to the study of the oceans that was probably unequaled elsewhere.

When I came back from military service in 1963, John Ewing, a marine seismologist and the younger brother of Maurice, asked me to interpret seismic refraction data obtained prior to 1959. These data had not yet been published because no one knew how to interpret them. It was known that the mid-Atlantic ridge was in isostatic equilibrium: its elevation had to be compensated by a thickening of light material beneath it. One could expect that the crust, which is lighter, would thicken over the heavier mantle. But this is not so. The base of the crust, the Moho, rises as much as the sea floor. Was Archimedes' principle being violated? This was the beginning of a study of the structure of the mid-Atlantic ridge that I did for my Ph.D. thesis in collaboration with several other scientists over the next three years. Once we recognized that the seismic refraction data were correct, we concluded with Manik Talwani in 1964 that the isostatic compensation resulted from a modification of the mantle immediately below the crust, which had to be much lighter than usual, which meant that it was probably significantly hotter.[22]

We began a study of the magnetic anomalies being analyzed by Jim Heirtzler, who was in charge of the Magnetic Department at Lamont.[23] At the ridge axis, and the zone immediately surrounding it, we found large linear magnetic anomalies: zones of intense magnetization caused by volcanic intrusion of rocks that were either highly magnetic or highly susceptible to induced magnetization by the prevailing Earth field. On the other hand, the anomalies on the flanks of the ridge were quite different. They were larger but less intense. This proved that the flank anomalies could not be displaced axial anomalies moved laterally by the sea floor spreading. Our computations showed that the deepening of the sea floor was not sufficient to explain this difference. Only much later was it demonstrated that these differences in wavelength and amplitude resulted from large variations in the earth's magnetic field and from changes in the magnetization of the rocks.

The study of the distribution of the sediments on the ridge, which I made with the two Ewing brothers, revealed large latitudinal variations and a remarkable contrast between the ridge, which had no significant cover, and the basins, which were filled with a thick and undisturbed sediment cover.[24] This was a phenomenon quite different from the regular thickening proportional to the age of the sea floor, as predicted by Hess. We thought that these anomalies were not compatible with the idea of steady sea floor spreading.[25]

But the major stumbling block for us was the presence of undeformed sedimentary filling in some oceanic trenches where Hess had proposed that oceanic crust was being underthrust. The sinking of the conveyor belt below the continent should have accumulated deformed water-saturated oozes and muds within the trenches. Yet the only tectonic evidence was the presence on the oceanic side of the trenches of faults that were obviously due to distension and not to compression.[26]

In late 1964 or early 1965 I noticed, at about the same time as, but independently from Tuzo Wilson and Fred Vine, the remarkable similarity of the magnetic anomalies above what is now known as the Juan de Fuca Ridge, off western North America, with the anomalies above the Reykjanes Ridge, south of Iceland. I pointed it out to Manik Talwani and Jim Heirtzler. It was obvious that a portion of active mid-ocean ridge crest was present to the north of Mendocino off western North America. But how could one explain the remarkable similarity in the magnetic anomaly pattern over any portion of mid-ocean ridge crest, in both the Atlantic and the Pacific? After considerable debate, we considered that too many observations remained unexplained by the sea floor spreading model, and our conclusions were published in *Science* with a fixist interpretation.[27] This appeared in 1966. However, one of us in the Lamont marine geophysics group, Walter Pitman, clearly expressed his dissent with our conclusions at the time. He told me then: "You are going too far; I would be afraid to publish such conclusions." Walter was the first of us to have entered this gray domain where we knew our previous fixist ideas were not right, but were not yet sure that sea floor spreading could work.

The most illuminating example of our dilemmas at the time is the interpretation of the oceanic pattern of distribution of heat flow. With Lamont's heat flow specialist, Marcus Langseth, in 1965, we were trying to analyze and interpret the numerous heat flow measurements he had made in the Atlantic. In particular, I made the first numerical computations of the heat flow pattern that should be produced by Hess' sea floor spreading model. While the overall pattern of heat flow distribution was consistent with Hess' model, quantitatively, the disagreement was obvious. The computed heat flow was three times larger than the measured one. In contrast, deeper convection currents, those that could not reach the sea floor (as proposed by Ewing), would produce a heat flow pattern in good agreement with the measurements. I was convinced then that we had obtained the quantitative demonstration that the Hess model did not work. This is the conclusion we published in 1966.[28] Sea floor spreading should leave a clear heat flow signature – but it was not present. Our computations were correct, our measurements were correct, but our conclusion was wrong.[29]

It is interesting to note that Dan McKenzie, a young scientist from Cambridge who was then working at Scripps and who would soon play a significant role in the elaboration of the plate tectonic model, made the same computations one year later, arguing that the heat flow was compatible with sea floor spreading. To obtain the correct results, he chose a temperature inside the mantle three times smaller – 550°C instead of the 1500°C that we had chosen. This latter temperature was then and is still considered to be much closer to the actual mantle temperature. But McKenzie was already convinced of the validity of the sea floor spreading model, and he preferred to adjust the parameters rather than arrive at an obvious discrepancy.[30] At the time, whether the fixist or the mobilist model was adopted, a certain number of observations did not agree with the predictions. The choice made was heavily influenced by the environment, the working philosophy, and the discipline in which one worked.

The Magic Profile: Conversion to Sea Floor Spreading

As far as I was concerned in late 1965, the difficulties that resulted from applying the sea floor spreading model – to the interpretation of the magnetic anomalies and the distribution of sediments, the apparent impossibility of reconciling subduction with the quiet sediment fill in the trenches, and the three-times-too-small heat flow through the mid-ocean ridges – led me to adopt a convection model without sea floor spreading, inspired by the ideas that had just been published by Felix Vening Meinesz.[31] Convection would be confined to the ductile part of the mantle, below about 30 miles (50 kilometers). It would induce fusion of basalt that would in part come to the sea floor and in part create the shallow compensating mass of the ridge. This was the conclusion of my thesis, written in late 1965 and early 1966 and defended at the University of Strasbourg on April 21, 1966.

On February 13, 1966, I left Lamont for Recife, Brazil, to participate as chief scientist in a South Atlantic cruise that would lead me to Buenos Aires and then to Cape Town. I then joined the faculty at Strasbourg, where I defended my thesis. It was April 26 when I returned to Lamont, where many of my colleagues were now "converted" to sea floor spreading. Walter Pitman showed me the "magic" magnetic anomaly profile obtained over the South Pacific ridge crest, the *Eltanin*-19 profile that had been presented by Jim Heirtzler at the American Geophysical Union (AGU) meeting in Washington, D.C., on April 27.[32] My wife still remembers that on my way back from the laboratory, I asked her to get me a drink and told her: "The conclusions of my thesis are wrong: Hess is right."

This extremely painful "conversion" experience has been crucial in shaping my own vision of what science is about. During a period of 24 hours, I had the impression that my whole world was crumbling. I tried desperately to reject this new evidence, but it had an extraordinary predictive power! Why then was the heat flow three times smaller than expected for sea floor spreading? Why were the magnetic anomalies so different over the flanks of the ridge? Why was the sediment fill in the trenches undisturbed? I did not know, but I was progressively forced by the convincing power of the magnetic anomaly profiles to assume that in all these unexplained observations, there must have been hidden parameters that had not yet been taken into account. Since that time, I know that good data and correct models do not guarantee that your conclusions are definitive: the possibility of hidden parameters is always present.

The presentation of the magic profile at the AGU stunned everybody. The 600-mile (1,000 kilometers)-long profile revealed a perfect symmetry with respect to the axis of the mid-ocean ridge crest. Furthermore, it could be interpreted simply and perfectly with the sea floor spreading model, using the Earth magnetic field reversals chronology obtained by the young Lamont paleomagnetic group (led by Neil Opdyke) by measuring the magnetic polarity of oceanic sediment cores. In particular, the magnetic anomaly profile as well as the sediment cores revealed the presence of a new magnetic event that Richard Doell and Brent Dalrymple, at the U.S. Geological Survey (USGS), had just independently identified. They called it the *Jaramillo event,* a short duration of normal magnetic field. With this new event, the correlations from one ridge crest to the other became evident. Suddenly, the balance of phenomena explained or left unexplained by the sea floor spreading hypothesis appeared positive, and acceptable without serious reservation to any scientist familiar with the whole picture. The massive move toward mobilism was then inevitable.

I still cannot understand how I missed seeing this magic profile before my departure from Lamont. I have absolutely no recollection of seeing it, so much so that in earlier retrospective papers I wrote that I had left Lamont in January.[33] But a check through letters sent by my wife showed that I only left on February 13 as just indicated. I guess that I was so

buried in the writing of my thesis and the preparation of the cruise that I ignored everything else. Alternatively, I may have subconsciously ignored evidence that clashed so much with the conclusion of my thesis – which I had to defend two months later and which I could not possibly change without delaying the defense. The profile was in any case widely available by mid-February, as Jim Heirtzler showed it to Fred Vine when Vine visited Lamont shortly after he joined Harry Hess at Princeton. Vine included it in his magisterial synthesis published in the December 1966 issue of *Science.*[34] In that paper, he compared the magnetic sections over two portions of mid-ocean ridge in the Pacific with the Reykjanes profile in the Atlantic.

Now we had the key and the data were at our disposal. Immediately, under the leadership of Jim Heirtzler, we started working, one scientist to each ocean. I got the Indian Ocean. Lynn Sykes, in Lamont's Seismology Department, had tested Wilson's transform fault model using earthquake fault plane mechanisms and showed that it worked.[35] Jack Oliver, with his student Bryan Isacks, had demonstrated that the oceanic lithosphere did indeed dive into the mantle along the trenches.[36] In spite of the skepticism of its director, Lamont had moved massively into the mobilist party. Ewing knew that he could no longer contain this rising tide of sea floor spreading: from then on, he would wait silently for the evidence that would demonstrate the falseness of this model.

It was during a conference organized by NASA in New York on November 11 and 12, 1966 that the victory of mobilism was clearly established. Teddy Bullard, who presided, could not find a single scientist to defend fixism.[37] Vine, the Heirtzler group, and Sykes presented their latest work. But it was during the April 1967 AGU meeting that, to use Bob Dietz's phrase, "the total and instantaneous conversion of the American community to continental drift" occurred. It is there, too, that a young Princeton scientist, Jason Morgan, took the critical step toward the present mobilist theory by establishing the bases of plate tectonics on a spherical earth, and not on a flat-plane earth as had been done previously by Tuzo Wilson. Morgan, soon to be joined by Dan McKenzie and Robert Parker, showed the predictive power of kinematic computations. The mobilist model had become quantitative.[38]

All Together for the Final Chorus

I have retained a precise memory of that morning of April 19, 1967, at the spring AGU meeting during which Fred Vine and H. W. Menard, from the Scripps Institution of Oceanography, presided over a "sea floor spreading" special symposium. The large amphitheater was full and expectations were very high. "Sea floor spreading" was the subject of most discussions: 70 abstracts on the topic had been submitted to this AGU meeting. At the end of the session, the program announced that Jason Morgan would present a paper which, according to its title, concerned the formation of oceanic trenches by viscous convection. Manik Talwani and I were preparing to listen very attentively because we had had a vigorous argument with Morgan on this subject. Morgan assumed for his model the absence of any long-term rigidity even at the surface, and we considered this assumption incompatible with the gravity data. But to our great surprise, Morgan announced that he would present a different paper, entitled "Rises, Trenches, Great Faults and Crustal Blocks." Thus the talk he made did not correspond to the abstract he had sent. He was going to discuss the geometric problems concerned with the relative motions of plates (he called them "blocks"), which he assumed to be rigid away from the Atlantic rift. What Tuzo Wilson had done qualitatively on a plane, Morgan was now doing quantitatively on a sphere, establishing the principles of plate kinematics. Morgan has a special gift for disorienting his listeners. This gift was especially well displayed on that occasion, and very few people, if any, actually paid attention to what he said. As for Manik Talwani and me, our dispute with Morgan appeared to be closed, since he now assumed rigid blocks at the surface. We could not understand this "about-face."

Morgan had written an 11-page extended outline of his presentation, including the nine figures illustrating his talk. This short paper was sent to about ten people immediately after the meeting. I was among them. Morgan does not remember all the addressees of his paper.[39] He writes: "I am quite sure I gave copies to Bill Menard (at Scripps) and Tuzo Wilson (at Toronto) and, I am fairly sure, to Lynn Sykes (at Lamont), Carl Bowin (and/or Joe Philipps, at Woods Hole) and Fred Vine (at Princeton). I might have sent one to Jerry Van Andel (Scripps) as I used magnetic profiles from the Circe cruise." Morgan lost his own copy and none of those who received it appears to have made it available to the scientific community. I thought I had lost my copy too. Yet, during an office move, I found it and arranged for it to be published in *Tectonophysics* in 1991 with Morgan's permission. The exact substance of his presentation at the AGU was finally in print, nearly 25 years later.[40]

According to Morgan, "this short description of the main ideas in plate tectonics was written the week before the AGU.[41] The last two pages were written and reproduced the night before the meeting." The extended outline has the same title as the paper later published in the *Journal of Geophysical Research (JGR)*.[42] Eight of the nine figures would later be included in the *JGR* paper, as well as most of the text. In particular, the first two paragraphs make up most of the substance of the abstract of the March 1968 paper. The published version was accepted in revised form on November 30, 1967, seven months after the extended outline had been circulated. Although it is more elaborate, it adds nothing to the spherical plate tectonic model, as defined in the earlier April 1967 version.[43]

On the basis of this document, it seems extraordinary that, in the hall packed with the best geophysicists and geologists in the United States, nobody got excited by or even interested in the implications of Morgan's ideas. They were too new, too different from anything that had been done. Even among those who received the extended outline and had time to digest the new concepts, I apparently was the only one to have considered it sufficiently important to drop everything else and start working along these new lines. As I have written elsewhere, the source of my June 1968 paper was Morgan's 1967 extended outline.[44] I decided immediately to test this kinematic approach, in spite of the skepticism of my colleagues, who considered it more important to continue to decipher the magnetic anomalies. I had to elaborate a rather complex methodology and a system of computer programs, which kept me busy until July. I could then verify that each of the different rift openings behaved according to spherical geometry: plates (as they were later to be called) were indeed rigid, and Morgan was right. Part of my work got incorporated in the 1968 paper by Heirtzler and colleagues on magnetic anomalies and crustal motion.[45] I first extended Morgan's kinematic analysis of the Africa/America accreting boundary to the Antarctica/Pacific, the Eurasia/America, and the Africa/India (actually the Africa/Arabia) accreting boundaries to test his concept. On Heirtzler's suggestion, I used an oblique Mercator projection to test the geometry of opening of these accreting plate boundaries. I also devised numerical search methods to define the magnitude and direction of the plate motion as "Eulerian vectors" – that is, as motions around a hypothetical pole of rotation. By the end of August 1967, this first part of my work was completed. At the time, neither Morgan nor I knew that Dan McKenzie and Robert Parker were working at Scripps on their "paving-stone theory," and Morgan had no knowledge either that I was exploiting his model.

Morgan had spent the months of July and August at Woods Hole, where he finished the version of his paper that was submitted to *JGR* on August 30. This version is close to the revised published paper although its discussion of the Africa-America-Pacific-Antarctica-Africa circuit was not correct. It contained an error in the determination of the Pacific-Antarctica rotation vector that I later pointed out to him. Morgan presented his paper at a seminar in Woods Hole in August, and returned there during September 7–8 to attend a two-day conference. According to Morgan, those attending the conference included Ken Deffeyes from Princeton, John Mudie from Scripps, myself, Walter Pitman, and possibly Heirtzler and quite a few others. In a personal letter that I have kept, I mentioned that people invited from Lamont were Maurice and John Ewing, Joe Worzel, Manik Talwani, Marcus Langseth, and me. Morgan presented his paper. I also presented my kinematic analysis, including the oblique Mercator plots. It was the first time that Morgan heard about my work. From then on, we freely exchanged data and documents. This helped Morgan to rework his Pacific-Antarctica rotation vector and the corresponding Africa-America-Pacific-Antarctica-Africa circuit. This was also a great help for me because at the time I was attempting to obtain a world kinematic model.

Once I verified the rigidity of plates, as Morgan and McKenzie had done for the Atlantic and Pacific, I moved to the next stage, which was to combine the motions of plates to obtain the first predictive global quantitative model. I found that a unique solution could only be obtained by using six plates instead of Morgan's 12. I used Morgan's America/Pacific Eulerian vector to ensure the closure of the model. This six-plate model accounted for most of the world seismicity, as Bryan Isacks and his colleagues would later show.[46] Even now, it is difficult for me to forget my extraordinary excitement the day I realized that my six-plate model worked, and that it could indeed account as a first approximation for the broad geodynamic pattern. I remember coming home early in the morning for breakfast after a night at the computer and telling my wife: "I have made the discovery of the century." Well, I was young and my enthusiasm carried me too far. But this statement is a good indication of how we felt during those days of frantic discoveries.

Finally, I made the first kinematic reconstruction of the evolution of the surface of the earth based on magnetic anomalies. To do this, I had to fit the magnetic anomalies that had identical ages on both sides of the rift in the same way as Bullard and his co-authors had done to fit the continental margins on both sides of the Atlantic.[47] This was the beginning of a paleogeographic method, which has proven to be especially powerful. The fit of the anomalies was done on the computer and involved combining rotations that were no longer small but could reach several tens of degrees. Small rotations can be treated as vectors, whereas this is

not true of large ones, which must be treated as matrices. Not knowing that, it took me some time to discover the origin of large discrepancies in my early computations. The rules of spherical geometry were poorly known at the time among geophysicists. Neither Morgan nor McKenzie, according to what they both told me in the fall of 1967, believed that such an approach was possible. They apparently did not have enough confidence in plate rigidity. McKenzie told me then: "John Sclater wanted me to do it – but I did not want to."

On December 13, 1967, I wrote to Morgan: "Thank you for the McKenzie–Parker paper. I am a little bit surprised that they do not seem to know about your paper." Thus it must have been in early December that Morgan sent me a pre-print of the McKenzie and Parker paper that had been received at *Nature,* November 14, 1967, and would be published in December.[48] This is how I discovered that McKenzie had been working on the same subject. The relationship between my paper and Morgan's is quite obvious, but both papers were written completely independently of McKenzie.

McKenzie had arrived at Scripps in June 1967.[49] Allan Cox wrote that "in June, 1967" Dan got the idea of using rigid-body rotations to describe plate motions while rereading the paper by Bullard and co-workers on fitting the continents together. Robert Parker had just completed a general computer program called SUPERMAP for plotting worldwide geophysical data using any conceivable projection.[50] Parker introduced the idea of using a Mercator projection in plate tectonics."[51] As noted above, I independently started using oblique Mercator projection in late May/early June 1967, and presented the first oblique Mercator maps with the Eulerian pole of rotation as pole of projection at the early September Woods Hole meeting.

In a letter written to me on October 11, 1983, McKenzie explained the relationship between his paper and Morgan's paper: "I was at the 1967 AGU meeting and attended the session in which Morgan spoke, up until the time he did so. But I had read the abstract . . . and thought I would gain nothing from sitting through the talk and arguments and left to go elsewhere. The paper generated little general interest, and I did not hear about it until after Bob (Parker) and I had sent off our paper to *Nature.* When I did, I tried to delay publication, but the editor refused, saying that the issue had been made up. . . . I did not know until I read your paper [a pre-print of Le Pichon, 1984] that Jason [Morgan] had sent you a preprint so early. The first I knew of what he had done was a brief account from John Mudie when he returned from Woods Hole in August. By this time, Bob and I had already produced the Mercator maps of the slip vectors, and John's report acted as an incentive to get something written. I had talked a great deal to Bill Menard about plate tectonics and had convinced him that it worked for the Pacific. *JGR* [*Journal of Geophysical Research*] sent him Jason's paper to referee and, I suspect because of our conversations, he was very critical of it when he showed it to me. I asked him what I should do and he said to go ahead and publish, which we did as everyone knows. When I came to Lamont and Princeton in the autumn of 1967 and discovered what had happened I felt very embarrassed and it was then that I tried to hold the *Nature* paper."

Thus, McKenzie heard about Morgan's work from a brief report of the early September Woods Hole meeting and then, presumably immediately after, from Menard, who received Morgan's paper to review, also in early September. It is then that McKenzie decided to immediately write his short *Nature* paper, probably feeling that his approach (using the horizontal projections of the slip lines of earthquake fault-plane solutions to determine graphically the position of the pole of rotation with oblique Mercator plots), which he had by then been working on for several months, was sufficiently different to justify doing so.[52]

In his book *The Ocean of Truth,* Menard confirmed that he received the early extended outline.[53] He wrote, "Jason Morgan sent me a preprint of his manuscript in its early draft, probably in the late spring of 1967." Menard must have had this extended outline available to him when he wrote his 1966 book, as he quoted the first sentences of this early preprint. Menard added: "The manuscript certainly circulated among my students, and we discussed it. The original draft, however, was difficult to fathom and it did not have the impact of the final publication." Yet, as discussed earlier, the plate tectonic concepts were clearly presented in this early draft, now published, which was not significantly different from the later 1968 version, in spite of what Menard wrote. Actually, it is clear that the concepts were too new and appeared irrelevant to both Menard and his students. Menard, who had co-chaired the AGU session in which Jason Morgan presented his paper, wrote in his 1986 book: "I not only did not remember hearing Jason's famous talk, I didn't remember presiding over the session." Finally, Menard stated: "I believe I also reviewed the paper for an editor."[54] This can only refer to the August 30 version submitted to the *Journal of Geological Research,* which he presumably found upon his return from the *Nova* expedition sometime after September 12, according to the information he gave in his book. At the time, as mentioned by McKenzie in his letter to me, he was "very critical of it."

It is astonishing that McKenzie twice so nearly missed the opportunity to learn about Morgan's model. The first occasion was when he left the room just before Morgan's talk on April 17. The second occasion was when Menard, who had received the extended outline of the April 17 communication in late April, failed to mention it to McKenzie, although they "talked a great deal" together "about plate tectonics" and although Morgan's "manuscript had circulated among Menard's 'students'" and had been "discussed" by them (quote from the book of Menard). But the approach followed by McKenzie was sufficiently different from the one followed by Morgan that it lent credibility to his story.[55]

To me, the most surprising part of it is that McKenzie confined himself to discussing the plate kinematics of the Pacific-America plate boundary based on earthquake fault-plane solutions, and did not consider the kinematics of the Atlantic ridge. In the equatorial Atlantic, good data on transform and earthquake fault-plane solutions were available and the opening of the Atlantic Ocean is the subject of the fit of Bullard's paper, which gave the initial intuition to McKenzie.[56] But he wrote to me in 1988 "that the Atlantic data did not cover a sufficient range of azimuths to determine an accurate pole. So I thought (and still think) that the North Pacific is the best example to use, and it has the great advantage that the same pole produces both spreading and consumption." I did not agree at the time. I wrote in the letter of December 13, 1967, to Morgan: "The main objection I have to the work of McKenzie and Parker is that it may be a dangerous assumption that the continental system of eastern Eurasia, Aleutians, and North America is perfectly rigid. If there is slow deformation of this system, then you might expect the results they get. It seems to me that the spreading floor evidence suggests that the North Pacific was larger in Cretaceous (more than 65 million years ago) than it is now or that several thousand kilometers of *(east-west)* shortening have occurred in Asia since this time. I prefer the first solution. Maybe the actual pole is somewhere between McKenzie's position and yours."

I added in the same letter: "I have a second version of my paper typed now. It has been reviewed within Lamont. My problem is that it has grown out of proportion. I will send you a copy when it is ready, probably before Christmas. I am returning to France before the Spring, so I am rather anxious to have it cleared before I leave." By this time, I had just decided to go back to France and I was to leave in early February 1968. This would have the consequence that I would be cut off from the Lamont data bank and would not be able to work on the development of plate tectonics for the next two years. I had just been offered an associate professorship by Frank Press at Massachusetts Institute of Technology (MIT), which I had seriously considered. However, I thought that if I stayed in the States, I had to be where the action was, and that was clearly at Lamont. Maurice Ewing knew about the MIT offer and wanted to keep me; he tried to push me into heading Bruce Heezen's department, with whom Ewing was now in open conflict. I acted as an intermediary between Ewing and Heezen, who was torn apart by this personal conflict. This experience was so painful that I think it contributed to my decision to go back to France, which I announced to Ewing on December 11.

It is in this context that I had shown my paper to Maurice Ewing and asked him whether he wanted to be an author, as was usually the case in Lamont for papers based on data collected there. He declined and told me: "This is your work; publish it alone." He may have done this because he wanted me so much to stay at Lamont. Alternatively, he may have refused to go against his fixist ideas. In any case, this is how I became the sole author of the most important paper of my career, which was very unusual at Lamont at the time, especially for a young scientist. I waited to submit it until Morgan's paper was accepted, in order to respect his priority. Morgan's paper was delayed three months by Menard's review and could have been published in December 1967 instead of March 1968 if Menard had immediately accepted it, as Wilson and Oliver later did mine. It could also have been published in abbreviated form in June or July, had Morgan decided then to publish a cleaned-up version of his extended outline. I had more luck than he had. My reviewers, Wilson and Oliver, recommended immediate acceptance of my paper. The title would be "Sea Floor Spreading and Continental Drift."[57] The succession of papers that established the plate tectonic model then followed: McKenzie and Parker in *Nature,* in December 1967; Morgan in the *JGR,* in March 1968; and mine, also in the *JGR* in June 1968. Thus, Princeton with Jason Morgan, Cambridge with Dan McKenzie (although the work was done at Scripps and McKenzie insists that he greatly benefited from the environment there), and Lamont with my own contribution were finally united in the definition of the plate tectonic model.

Soon after, Isacks and co-authors demonstrated in their September 1968 paper, "Seismology and the New Global Tectonics," that geophysical data were compatible with my global plate kinematic model.[58] Their paper had a major impact on the geological community. Neither my paper, nor any of the critical ones that followed, would have been possible without the availability of the Lamont sea floor spreading data.[59] I wish here to acknowledge the firm leadership of Jim Heirtzler, who bulldozed us into producing this impressive collection of published work in a short amount of time.

From Ocean- to Space-Based Plate Tectonics

When I reflect with hindsight on what now has become history, it seems clear to me that the elaboration of plate tectonics was mostly the work of a few scientists in continuous interaction with the privilege of rapid access to crucial data and ideas. I wish, however, to highlight the prodigious intuition of Harry Hess and Tuzo Wilson and the immense energy of Maurice Ewing. I also regret the oblivion into which Warren Carey and Bruce Heezen have fallen. I believe, furthermore, that not enough attention has been paid to the privileged relationships existing between a few key laboratories. In a sense, the history of the elaboration of plate tectonics can be read as a concerto for three instruments in which Princeton, Cambridge, and Lamont successively held the soloist role until they joined together in the final chorus.[60] Each of these laboratories was characterized by a grand "multidisciplinarity" and by a strong unity under a charismatic leader. They had, moreover, taken large initiatives toward both continental and oceanic research. Finally, their teams included scientists who did not hesitate to venture outside the strict frames of their specialties, to risk the formulation of very general hypotheses that had to submit to the test of field observation, and to prepare for eventual modifications.

I have been often asked why the Lamont people had such a late conversion to sea floor spreading and why, once they were converted, they were such efficient actors in the development of plate tectonics. In his 1983 letter that I quoted earlier, Dan McKenzie wrote: "Lamont to me has always seemed a very valuable place to test new ideas. But the quantity of data available does not encourage their development. I think that it is no accident that sea floor spreading and plate tectonics were developed elsewhere." On the basis of my experience, I think there is some truth to this. I had devoted my whole research time looking in detail at enormous amounts of data on all aspects of the mid-Atlantic ridge. Any hypothesis that was mentioned on its origin immediately evoked tens of observations that would not fit it. This indeed blocked any progress. In a sense, trees were hiding the forest. On the other hand, once the model became obvious, it was easy to unroll my data bank and to interpret it in terms of this new model. This rather simplistic explanation cannot be the only one. I have insisted earlier in this essay on the quasi-absence of serious awareness of paleomagnetic results in Lamont and on the prevalent fixist culture of the geophysicists there. I know that, as far as I am concerned, these were two very serious obstacles. When I was a Ph.D. student at Lamont, my supervisors never seriously exposed me to paleomagnetics and continental drift.

But why then were Scripps people, who also had such a large data bank, slower to jump on the bandwagon, especially in view of the fact that there was a serious paleomagnetic culture on the west coast? I mentioned earlier as important factors the intensive exchanges between the different departments and the culture of "multidisciplinarity" imposed by Ewing. In addition, one should not underestimate the decisive advantage of the computerized data bank put together by Manik Talwani. Finally, Lamont people had inherited from Ewing a tradition of very hard work.

It is not my role to comment on the relative importance of my own contribution in this concerto. All I can say is that my papers, in particular the "Sea Floor Spreading and Continental Drift," were intensively used by the scientific community in the early years of plate tectonics. I was the most cited solid earth scientist for the period, 1965–1978 and one-quarter of the 2,500 citations were assigned to this paper.[61] This highlights the impact of the demonstration that a global plate kinematic model could indeed be used as a framework for plate tectonic studies.

In 1973, at my laboratory in Brest, Jean Francheteau, Jean Bonnin, and I published a book entitled *Plate Tectonics,* which was the first attempt to present in a coherent fashion the plate tectonic model, from plate kinematics to processes at plate boundaries, in book form.[62] This kind of work could be done without having access to a large data bank! Our book was very well received and widely used – probably because its logic responded to the needs of the scientific community at the time.

The book also illustrates the remarkable change plate tectonics brought to the field of geodynamics. Since the beginning of the 20th century, geophysics and geology had mostly traveled separate ways. What little interaction there was tended to be tense, if not antagonistic. It is remarkable that the role of earthquakes was essentially ignored in tectonics, and that mountain-building was considered as a spasmodic process not directly related to seismic activity. Plate tectonics reconciled geophysics and geology and showed that earthquakes were the direct expression of the continuous tectonic activity at the surface of the earth. From then on, seismologists and tectonic geologists would have to work together to study geodynamic processes. But the great difficulty of the study of these processes was that plate kinematics was ocean-based. All

the observations that allowed quantitative plate kinematic determinations, magnetic anomalies, transform faults, and fault-plane solutions at plate boundaries were obtained in the oceans. Continental tectonics, which could be observed and studied in detail, was difficult to relate directly to this quantitative model. Plate tectonics was used more as a help to build a scenario of the genesis of mountain belts than as a quantitative model of earth deformation.

The mutation from an ocean- to a space-based plate tectonics occurred in 1986, after five years of space geodetic measurements between the Westford (United States) and Onsala (Norway) very long baseline interferometry (VLBI) stations. These provided the first space-based direct estimate of the present rate of opening (0.8 inch or 2 centimeters/year) at the rift across the Atlantic Ocean. It was soon followed by the first estimate of the rate of sea floor shortening between Hawaii and Tokyo (3 inches or 8 centimeters/year). A turning point in my scientific journey was reached when I learned about this latter measurement. Of course, I was thrilled to see my 1968 estimate of the amount of subduction in the Japan trench confirmed. But more important, I realized then that we were entering a new era of plate kinematics.

This was rapidly confirmed by increasingly numerous measurements from VLBI techniques, but also satellite laser ranging (SLR) techniques and global positioning systems (GPS). Soon, it was established that the latest ocean-based plate kinematic model (obtained with velocities averaged over 3.5 million years) was in excellent agreement with the space-based model, where all measurements were obtained over a few years on continental sites.[63] Motions of plates could be obtained nearly instantaneously. Moreover, these motions applied to a significant portion of the recent geological past. From then on, the action in plate tectonics moved back to the continents, where one could directly measure not only the instantaneous motions of plates, but the deformations over the plate boundaries, including some complex intracontinental deformation zones. Until then, these had been considered gray areas for plate tectonics, since we had no quantitative knowledge of the kinematics of their deformations. In the same way that seismology had been reconciled to tectonics by the ocean-based plate tectonic model, geodesy became a new and essential partner in the space-based continental plate tectonic model. Today, the integrating power of plate tectonics is becoming more and more evident as tectonics moves from purely kinematic descriptions to dynamic modeling.

In 1990, Jason Morgan, Dan McKenzie and I were awarded the Japan prize for our contributions to the plate tectonic theory. While discussing this together in a hotel in Tokyo, I remember that Dan said: "Never more in our life will we be able to contribute to such a decisive and exciting discovery." This, I thought, was true. We had been involved in a mutation of the whole of earth sciences that occurred within a very short time, and we knew it when we lived it. This explained the extraordinary feeling that carried us through these fascinating years and the sense that never more will we live through something even remotely similar to it.

PART V
FROM THE OCEANS TO THE CONTINENTS

Continental drift was first proposed on the basis of geological evidence accumulated from fieldwork by geologists on the continents. In contrast, plate tectonics was developed largely on the basis of evidence from the sea floor, or earthquakes under it, collected mostly by geophysicists. When geologists realized what was happening, the most alert among them saw an opportunity for a radical reinterpretation of geological history based on the new model of crustal mobility. Moreover, important geological features that had never been fully understood – like California's great San Andreas Fault – suddenly could be explained, clearly and elegantly, by the new model. With every old understanding up for grabs and new understandings emerging daily, one of the 20th century's greatest scientific revolutions happened.

from the Immigration Service!). I will always be grateful to the people, principally Don Anderson at Caltech, Walter Munk at Scripps, and Harry Hess at Princeton, who gave me the opportunity to be involved in this work at the beginning of my scientific career. The enlightened generosity of these people and of the organizations from which their support came, especially the U.S. Air Force, the Office of Naval Research, and the National Science Foundation, made a deep impression on me, and left me with a lasting admiration for the United States and its way of doing science. This is Earth Sciences Contribution number 6240.

CHAPTER 12

WHEN PLATES WERE PAVING STONES

Robert L. Parker

FOR ONE TIME ONLY, AT ITS INCEPTION, PLATE TECTONICS WAS called the *paving-stone theory of tectonics,* the name Dan McKenzie and I gave to the organization of the earth's surface into a small number of internally rigid bodies in relative motion. When Dan and I wrote the first paper on plate tectonics, "The North Pacific: An Example of Tectonics on a Sphere," we were two unknown new Ph.D.s, fresh out of graduate school.[1] My thesis had been on the mathematical modeling of electrical currents in geophysical systems, and Dan's was on the shape of the earth, so we were both beginners in the science of geology. But we were fortunate to be working as students in a department at the center of a whirlpool of intellectual activity that was bringing about the first true understanding of marine geology and its importance for global tectonics. This essay is a brief review of the scientific and personal events leading up to that first paper.

THE VIEW FROM CAMBRIDGE UNIVERSITY, FALL 1967

In the fall of 1967 I was a postdoctoral fellow at the Institute of Geophysics and Planetary Physics (IGPP), which is part of the Scripps Institution of Oceanography in the University of California at San Diego (UCSD). I had graduated the year before from the Department of Geodesy and Geophysics at Madingley Rise in the University of Cambridge in England. Although I was working in the United States, which was to become my home, my perspective at the time was that of a Cambridge graduate. During my three years as a research student I had not worked on marine geology or indeed anything remotely geological. But it was impossible not to be aware of the great events going on in the department, which seemed

Bob Parker with bicycle, at the Scripps Institution of Oceanography. (Photo courtesy of Bob Parker.)

to be a focus of tremendous creative energy. Let me first survey what I see as the primary influences that went into that paper.

By the middle of the 1960s everyone in the department at Cambridge had been converted to a firm belief in continental drift and the large-scale horizontal mobility of the crust. The head of the department, who was also my research supervisor, Sir Edward ("Teddy") Bullard, had long been what he termed "agnostic" on the subject, but he now became an enthusiastic proponent and supporter of research into these ideas. To him they were a confirmation of his conviction that the most urgent priority of the time in the earth sciences was to correct our almost total ignorance of marine geology. In contrast, we had frequently heard the story that the director of the Lamont Geological Observatory (a department of Columbia University), Maurice Ewing, had decreed to his people that no effort should be spared in an effort to prove continental drift wrong once and for all. Therefore, it comes as something of a surprise for me to learn from recent public reminiscences of the Lamont team that at the time almost everyone there was actually engaged in discovering plate tectonics, with the blessing of boss Ewing himself.

Four years before our paving-stone paper, Fred Vine and Drum Matthews had shown that the magnetic stripes which had been measured in only a few places around the ocean ridges were beautifully explained in terms of an idea proposed by Princeton's Harry Hess that new crust was being created at the ridges, combined with the discovery that the geomagnetic field has been constantly reversing polarity, that is, exchanging the positions of the north and south magnetic poles.[2] The confirmation of the global reversal of the magnetic field by radiometric dating was also new at the time, although reversely magnetized rocks had been known for over 60 years. Fred Vine was a fellow research student and Drum Matthews was a lecturer (I think) on the staff at the Department of Geodesy and Geophysics when they had the extraordinary insight to put these two phenomena together.[3]

While I was a student there, Tuzo Wilson, visiting Cambridge on a sabbatical leave from Toronto during 1964, made his seminal discovery: the nature of the great offsets in the magnetic record on the sea floor. In the eastern Pacific Ocean off the states of Washington and Oregon, great linear gashes called *fracture zones* had been found in the sea floor, several thousands of miles in length. Magnetic patterns on one side of a fracture zone also appeared on the other side almost identical in form, but displaced by vast distances. The traditional geological explanation for such an offset was that the patterns had originally been formed together, but had subsequently been separated by sliding one piece of the crust relative to the other along the line marked by the fracture zone. There were many difficulties with this explanation. For example, nowhere else on the earth had such huge horizontal offsets ever been seen. Moreover, after a shearing motion of this kind, one would expect to see evidence of folding or compression at the ends of the fault zone, yet there was none. To solve this puzzle Wilson invented the idea of *transform faults*.[4] I clearly recall coming out of the first seminar he gave to us on the subject and thinking I had witnessed something profoundly important. In 1965 Wilson published his paper in the journal *Nature* explaining transform faults and their relationship to the fracture zones. The remarkable resolution lay in the totally unexpected finding that the two sets of magnetic patterns had never been aligned, but had actually been created separated by the large offset as we see them today. Transform faults were another key ingredient in the paving-stone theory.

Two years after this, in 1967, Lamont's Lynn Sykes published an article in the *Journal of Geophysical Research* concerning earthquake mechanisms on ocean ridge systems: the mechanism gives the direction of motion of the crust in the immediate vicinity of the earthquake.[5] Sykes discovered that the motion on the offsetting ridges was compatible only with Wilson's model. That model was further confirmed by the absence of earthquakes on the fracture zone traces, which were predicted to be inactive by the transform fault model, but which would be lines of slip in the interpretation of classical geology. Sykes' work was based largely on a newly established seismic network, the worldwide standard seismograph network (WWSSN) of the U.S. Coast and Geodetic Survey – a set of calibrated and nominally identical seismometers globally distributed for the purpose of detecting nuclear bomb tests. In retrospect it was a piece of remarkable good luck that the seismic records from this network were not immediately classified, just as, in later years, enormous amounts of marine magnetic and gravity data collected by the U.S. Navy were kept secret.

In 1965 at Cambridge University, Teddy Bullard, with Jim Everett and Alan Smith (both research students at the time), had already fit the continental shelves around the Atlantic, using internally rigid bodies, moving the continents about their Euler poles of rotation, in a purely geometrical manner.[6] As Teddy was fond of illustrating with everyday objects like books, Leonhard Euler (the 18th-century Swiss mathematician) had proved that it is always possible to move a rigid body from one position to any other by means of a single rotation about an appropriately chosen axis; when one imagines moving a continental plate from one place to another on the earth, the positions where the axis intersect the earth's surface are called *Euler poles*. The continental reassembly project was not an attempt to run time backward and produce a continuous history of the relative positions (something that can be done today). It was simply to fit the edges of the continental shelves together and therefore reconstruct the ancient protocontinent of Gondwana. Although Warren Carey in Tasmania and others had performed similar reconstructions with model globes fitted out with plastic caps, skepticism remained deep about the accuracy of the fits.[7]

Bullard carried out a quantitative fit using bathymetric charts to locate the continental shelves, which he regarded as the proper edge of the continental crust. The astonishing match of the shelves from opposing sides of the Atlantic Ocean convinced most of the unbelievers of the fidelity of the reconstruction. Logically, the validity of the reconstruction required only that the edges of the continental regions should remain rigid throughout geological time and so maintain their present-day shapes. In principle, the interiors would be free to deform, although it is unlikely anyone actually thought of such a physically implausible state of affairs. So interpretation of the very precise fits that Bullard and his co-workers demonstrated regarding the land masses in the geological past was a tacit acceptance of the existence of internally rigid plates moving around on the surface of a globe.

Given all these developments, it is hard to believe in hindsight that the final piece of the puzzle wasn't obvious to everyone in the field. Of course, the full geometrical consequences were eventually realized by two people quite independently, Dan McKenzie (also a student at Cambridge, and with whom I shared an office) and Jason Morgan at Princeton.[8] In the minds of most people, I believe, the clear concentration of activity on the boundaries did not rule out internal deformations of the large, seismically inactive regions. They thought that the boundary regions were places where crust was created at ocean ridges or slid past other crust at the transform faults (the nature of the trenches was controversial), but these were not the only sites of major crustal deformation. I suppose the mental picture was of a generally plastic region, where compression or shear could occur over large areas. But as I remarked earlier, unless the internal deformations preserved the boundary shapes in a most unnatural way, this mental picture is incompatible with the very precise observed match of the continental shelves surrounding the Atlantic. Even a 5 percent deformation of the plate boundary shape would have been very noticeable.

Autobiographical Matters

I have already mentioned that I had graduated with my Ph.D. in geophysics from the Department of Geodesy and Geophysics in 1966 and in early 1967 I took up a postdoctoral fellowship at IGPP. One of my research projects was the calculation of electric currents flowing in the oceans due to electromagnetic induction caused by the daily variation of the earth's magnetic field. Surprisingly large amplitude signals had been observed in the time series recorded at magnetic observatories situated on the coasts, and I believed this effect was due to electric currents circulating in the water. As part of my Ph.D. thesis I had solved the associated differential equations in an extremely simple geometry – a thin strip of conductor – and Teddy Bullard had solved problem for a conducting disk.[9] Now we worked together on the calculation in a more realistic model ocean, with the known variations of depth and conductivity and the proper shapes for the coasts to confine the electric current.[10] UCSD's computer center provided a very powerful computer at the time, a Control Data Corporation (CDC) 3600, but the memory of 36,000 words was too small to hold simultaneously the data required to define all the world's oceans at the resolution I needed. So I had to break up the oceans into three major basins: the Pacific, Indian, and Atlantic Oceans. To minimize scale distortions, I decided to represent each region on its own in a map centered in the middle of the ocean basin. For display purposes, and for creating a finite-difference grid (the lattice of points at which numerical values were to be computed), I wrote a program for drawing maps to run on the CDC 3600.

The program was originally named SUPERMAP, after Harold MacMillan, prime minister of Great Britain (1957–1964), known in the press as SuperMac. SUPERMAP was written in Fortran-63 and recorded on punched cards; the database comprised a primitive coastline of about 5,000 points, digitized by hand by an undergraduate student as part of a summer job from a large Mercator projection map provided to me by Bill Menard. Even today, most scientists write computer programs for themselves in the quickest way, with little thought for maintenance or general use. I have always believed it is more efficient in the long run to build programs that can be used repeatedly and that are easily used and upgraded. These days reusable code is heralded as some kind of new discovery, but it was obvious even then what advantages a more forward-looking approach would bring. So, even though I needed only one kind of map projection for my electromagnetic induction problem, I made SUPERMAP a general-purpose program, running under an easily used command language. When I was writing it I had no idea, of course, that it would be soon pressed into service by earth scientists everywhere to perform plate tectonic reconstructions. In those early days its only rival was a program written by Xavier Le Pichon while he was at Lamont; written in the traditional quick-and-dirty mode, it was reputed to be the source of much frustration. In fact, the program SUPERMAP was much easier to use in 1967 than the present-day standard in the earth sciences, the Generic Mapping Tool program (GMT).[11] Thus SUPERMAP was ready for application when plate tectonics came along; all it needed was three or four lines to implement the oblique Mercator map projection, to generate one of the figures in our *Nature* paper. Strangely, I have been unable to locate a single listing or card deck of SUPERMAP. I understand that parts of the code are still active and running in a few computers around the world, but not at IGPP.

Without false modesty, I must make it clear that Dan McKenzie was the creative force behind the 1967 *Nature* paper. Dan spent the year or so after getting his Ph.D. at Cambridge visiting various places in the United States. He was at Scripps for the summer and fall of 1967; he went on to Lamont and Princeton early in 1968. Dan had been thinking about

the new, high-quality WWSSN seismic data and what it could say about tectonics. At the time most seismologists were working on getting the directions of the principal axes of stress from the seismic signals, but these proved to be puzzlingly inconsistent and seemed to vary quite unsystematically, even in a small region. The use of earthquake first motions pioneered by Sykes as a diagnostic for tectonics was a breakthrough. An earthquake breaks the ground along a plane (the fault plane), usually a zone of preexisting weakness. The initial ground motion can be traced back to the site of the earthquake from signals picked up by on seismometers around the world, and the orientation of the fault plane (called the fault-plane solution) can then be inferred. As I mentioned earlier, data had just become available from a well-calibrated global seismic network of long-period instruments, the WWSSN. Suddenly fault-plane solutions were reliable and could be found for many more earthquakes than before, as small as magnitude 6 in size.

Sykes' work at Lamont (where Dan had been and was going back to in 1968) convinced him that the first motions contained important information for tectonics – he started to look at the earthquake mechanisms in a systematic way, not just at the ocean ridges. Dan quickly realized that it might be possible to treat the interior aseismic (i.e., seismically inactive) regions of the earth as rigid bodies. This meant that the regions under consideration became so large that pictures based on a flat-earth model were no longer adequate. Dan wasn't sure how the geometry of the plane velocity vectors that he was used to would translate into a spherical setting. This is where I came in: during his visit to Scripps he told me about the problem, and I worked out for the spherical system how to represent the instantaneous velocities through angular velocity vectors and how those vectors were combined at the points where three plates meet. Furthermore, as I have already described, I had on hand my computer mapping program SUPERMAP, which we immediately put to work displaying the amazingly compelling results. It was my idea to use an oblique Mercator map projection, which made such a dramatic graphical demonstration in the 1967 *Nature* paper.

Computers and the Birth of Plate Tectonics

Nowadays most people will find it hard to appreciate how limited the available computers were at the time. At IGPP we had access to the University CDC 3600 in the computer center; the computer had a large magnetic core memory (36,000 48-bit words), 12 tape drives, a fast card reader and line printer, and, most important, a CalComp plotter for graphical output. I believe we enjoyed one of the best computer facilities at any institute doing research in earth sciences. The CalComp plotter was a simple robust device that moved a ball-point pen across the paper in the *y* direction, while the orthogonal *x* motion was provided by the rotation of an eight-inch diameter aluminum drum; graphs were drawn on long rolls of paper, ten inches wide. I have letters from 1968 in which Dan complained to me that the Lamont computer had too little memory to run SUPERMAP; later at Princeton he could successfully run the program but the computer had no plotter attached to it, so he could not draw the results. Therefore Dan sent the specifications for numerous tectonic cases he wanted to study to me at IGPP through the mail; I ran them on the CDC 3600 and sent the maps back.

It may be interesting to look at how computers had been used in other parts of the early development of plate tectonics. There were of course no general-purpose mapping programs; that is why I wrote SUPERMAP. The base maps in the publications were usually traced laboriously from atlases by staff illustrators or graduate students. Or they were simply haphazardly sketched – this is obviously how Tuzo Wilson's maps were made for the most part. Wilson made no use of the new technology at all; in fact his first demonstration of transform faults was with models made of paper built by Sue Vine, Fred Vine's wife.

The maps published by Bullard, Everett, and Smith were made by first computing the intersection points of selected parallels and meridians on the plate after rotation, and printing a list of these numbers; they were then plotted by hand onto large sheets of paper and joined with curved lines to form an image of the distorted latitude-longitude grid.[12] Then someone transferred the present-day coastline and continental shelf edge from a conventional map in a more traditional projection onto the curvilinear grid. No one thought for a moment that a computer plotter could do a fine enough job to reach the standards of scientific illustration in *Philosophical Transactions,* the journal where the continental fitting work was published. The Royal Society was (and still is) very fussy about diagrams, and would insist on having their own illustrators draw all the lettering and numerals on the diagrams. In contrast, the normal CalComp plotter product was drawn with a ball-point pen, and the one-hundredth-of-an-inch resolution of the stepper motor that drove the drum left easily visible staircases in lines drawn diagonally. But the continental fit of Bullard's paper depended on some heavy computing to minimize the misfit between the segments of the boundaries. In fact, it was the very "arithmetical" nature of the fit that Teddy thought might

convince doubters, who saw, probably correctly, too much exercise of artistic license in the sketches and model continents that had been offered earlier as proof.

Somewhat surprisingly perhaps, the earliest analysis of the magnetic stripes due to sea floor spreading depended solidly on the computer models of crustal magnetization, generated by a program in autocode written by Fred Vine; like the continental-shelf fitting programs, it was run on the Cambridge EDSAC II computer in the Mathematics Laboratory. Computer code based on the very same equations is still in use today in the analysis of marine magnetic signals, which have been recorded in every ocean.

On the other hand, I don't know how much, if at all, of the seismic analysis of earthquake mechanisms was computer-aided. I suspect none of it: the seismic traces were recorded on photographic film chips and times of arrival of the seismic waves were estimated by eye. The direction of the earthquake's first motion at the source was plotted on a stereographic or equal-area grid mapping the focal hemisphere, an imaginary hemisphere centered on the earthquake; a master net was drawn accurately once and then copied endlessly. However, I was not closely involved with the process so I cannot be sure when these tedious procedures and the finding of the fault planes were automated.

Concluding Remarks

Dan McKenzie and I were both undergraduates in physics at Cambridge University. We were the graduate students of another physicist, Teddy Bullard, who was himself a student of Ernest Rutherford. Among physicists at least, the prevailing philosophy at the time was that the touchstone of a good theory was that it should make testable predictions. Sciences that merely made observations and organized them were, in Rutherford's unkind words, "stamp collecting." To us the ability to make quantitative predictions capable of verification, or of falsification, was what made plate tectonics and the paving-stone model so appealing: one could use the information about the direction and magnitudes of the relative motions obtained on the boundaries between plates A and B and between plates B and C to predict what would be happening along the A–C boundary, both qualitatively in terms of tectonic processes and quantitatively in terms of rates and directions of motion. We realized right from the start that the predictive power was restricted to present-day motions, and that once one attempted to extend instantaneous velocities of today back into the geological past, other factors controlled the evolution of the boundary shapes. Initially we hoped some general principle might be discovered governing these factors, but that goal has proved elusive; there appears to be no alternative to a painstaking empirical analysis of the geological record. Nonetheless, plate tectonics succeeded in providing the framework for making sense of the large-scale processes governing the development of the earth's crust.

Subsequently, I did not make the further development of paving-stone theory a major part of my scientific career. I did some minor work on kinematics of plates; I helped my Scripps colleagues delineate the fine details of the marine magnetic anomalies using their near-bottom magnetometer system.[13] Perhaps my only other important contribution was as co-author on the first paper explicitly stating the square-root age rule for sea floor depths.[14] I was caught up in another revolution in the earth sciences going on at about the same time: the creation of geophysical inverse theory. In addition to the pioneers of plate tectonics, the two founders of modern inverse theory, George Backus and Freeman Gilbert, were also sabbatical visitors in Cambridge during the 1960s. I was extremely fortunate to get to know them personally, and to become their colleague in due course. Inverse theory is the set of mathematical methods that allows one to draw sound conclusions from a physical model in the face of severely incomplete and inaccurate measurements – a common situation in earth sciences.[15] Here was a subject in which I could indulge my personal fascination with abstract mathematics to a much greater extent than in plate tectonics. It was clear in 1967 that an enormous amount of work lay ahead to confirm the model of plate tectonics, work that would involve the synthesis of great quantities of geological and geophysical information. I knew my talents lay in another direction.

The term *paving-stone theory* appears six times in the first *Nature* paper, once in the abstract (which the *Nature* editors wrote). Everywhere else (21 times) we refer to the inactive interior regions as "plates" in a completely modern and familiar way. I am not sure who first used the term *plate,* but in any case our name for the new tectonic system, *paving-stone theory,* did not receive popular favor. But even if the metaphor of the paving stone failed to catch on, the concept it described has proved to be much more durable.

CHAPTER 13

MY CONVERSION TO PLATE TECTONICS

Xavier Le Pichon

I HAPPENED TO BE WORKING AT THE LAMONT GEOLOGICAL LABORAtory (now Lamont-Doherty Earth Observatory) of Columbia University in New York while the plate tectonic model was elaborated, first from September 1959 to September 1960, before my military service, and then from February 1963 to February 1968. Here, I present my views on the plate tectonic conception from the perspective of someone who was at the key acting laboratories. This testimony does not pretend to be an exhaustive and impartial history of the elaboration of the plate tectonic concept. I make extensive use of earlier papers that I have published on the subject.[1] Finally, I briefly place these views within the context of the evolution of plate tectonics from an ocean-based model in the 1970s to a space-based one today.

LAMONT: FIXISTS VERSUS MOBILISTS

The revolution of ideas that led to plate tectonics, from 1955 to 1968, was greatly influenced by the continuous interaction among scientists of three laboratories, Lamont and Princeton University in the United States, and Cambridge University in England. Each of these laboratories was dominated by a strong personality: Maurice Ewing at Lamont, Harry Hess at Princeton, and Edward ("Teddy") Bullard at Cambridge. Although of quite different origins and intellectual capacities, they had in common a deep interest in the geology of the oceans. It was Richard Field, a professor at Princeton, who generated this interest in Ewing, Hess, and Bullard during the 1930s.

Maurice Ewing inherited from Field a burning zeal for the exploration of the oceans. With him, marine geology entered a new era. From

CHAPTER 8

HEAT FLOW UNDER THE OCEANS

John G. Sclater

THE INTERNAL ENGINE OF THE EARTH IS DRIVEN BY RADIOACTIVELY generated heat and heat left over from the formation of the planet. The ultimate source of energy for the elevated temperature in mines, volcanoes, hot springs, earthquakes, the uplift of mountains, and global plate motions is this heat from the interior. The thick continental crust is known to have many more heat-producing radioactive elements than the much thinner oceanic crust. As a result, it was expected that the heat flowing out through the ocean floor would be significantly less than that flowing through the continents.

The outward flow of heat from the earth is determined as the product of the temperature gradient and the thermal conductivity. In the oceans, the temperature gradient is measured by forcing temperature-sensing elements into the soft sediments of the ocean floor. The thermal conductivity is determined directly by measurement on the sediments in situ or on a sample brought to the surface in a coring tube.

The early measurements of heat flow in the oceans made by Roger Revelle, the director of Scripps Institution of Oceanography, and his student, Art Maxwell, gave values that were very similar to those on the continents.[1] Sir Edward ("Teddy") Bullard, who had designed the instrument they used, proposed that the extra heat was created by slow-moving convection currents in the upper mantle beneath the oceans.[2] Teddy and many others argued that these currents were the same as those responsible for moving the continents. Dick Von Herzen had participated as an undergraduate in some of the original Scripps expeditions on which heat flow measurements had been taken. He returned to Scripps for graduate studies under the direction of Russell Raitt to make the first systematic measurements of the flow of heat through the ocean floor.

After finishing his thesis Dick accepted a job as a scientific attaché at

John Sclater on the occasion of receiving the Rosentiel Award of the University of Miami for outstanding work by a young scientist in marine geology or geophysics, 1977. *From left to right:* Walter Munk, Freddie Sclater, John Sclater, Judith Munk, and Chris Harrington. (Photo courtesy of John Sclater.)

UNESCO in Paris. I first met Dick there, in the summer of 1964, on my way to join the research vessel *Argo* on the (Scripps) DODO expedition to the Central Indian Ocean. At the time I was a graduate student in the Department of Geodesy and Geophysics, Cambridge University, based at Madingley Rise. My thesis involved the use of a new type of heat flow instrument built by Clive Lister, who had preceded me in the marine group at Cambridge. Bob Fisher of Scripps, who was on sabbatical at Madingley Rise, had arranged that I take the Cambridge heat flow equipment to Mauritius for use on *Argo*. Dick and Victor Vacquier, the chief scientists for this leg of the expedition, had agreed to my participation. They planned to determine the relation of the axis of the Central Indian Ridge, as defined by the central magnetic high, to the heat flow field. This was to be my first expedition at sea on my own without the support of the Cambridge seagoing group run by my supervisor Maurice Hill.

The professionalism of the crew, technicians, and scientists on board *Argo* impressed me. Everyone from the captain on down worked to maximize the use of the ship for scientific purposes. Their combined efforts ensured the reliability and efficiency of the shipboard operations. Dick and Vic had a limited but clear set of scientific objectives. These were to maximize the number of crossings of the ridge and to take as many heat flow measurements near the crest as possible. Further, they had thought through carefully just what compromises were needed to attain these objectives. I saw, firsthand, the low-key but highly efficient American way of operating at sea. This contrasted favorably for me with the more structured but much less focused operation I had observed on a three-month expedition to the Indian Ocean on the *Discovery* the previous year. On my way back from sea I had the opportunity to work at Scripps for two months (November and December 1964). Returning to foggy, frozen southern England in December after the balmy Indian Ocean and a November in southern California was a true culture shock!

I next met Dick in November 1965, when he came to Cambridge to be the external examiner for my Ph.D. thesis defense. Teddy Bullard was my internal examiner. When preparing this essay I found a copy of my thesis that included Dick's handwritten comments.[3] It was his copy from my defense. The Cambridge University Librarian had very exacting standards for the format of theses, and in the days before word processors, these standards made rewriting both difficult and time-consuming. I now agree with many of Dick's comments, but I have to admit that I did not include most of them in the final version of my thesis. To do so would have caused too many problems with the university librarian and I wanted to return to Scripps and California as quickly as possible; there I had a job to follow Dick as the person directly responsible for the heat flow program at Scripps. One of the first things I found on arriving at Scripps was a copy of Dick's thesis.[4]

After his stint as scientific attaché with UNESCO in Paris, Dick returned to the United States to a position at the Woods Hole Oceanographic Institution. There he resumed his career as an active seagoing marine scientist. In this long and distinguished career he had many accomplishments. He was the first to document the high heat flow anomaly at the crest of the mid-ocean ridges.[5] He was co-chief scientist with Art Maxwell on Leg III of the Deep Sea Drilling program that showed the linear increase in age of the ocean crust away from the crest of the mid-Atlantic ridge.[6] His student, Dave Williams, together with Dick, Roger Anderson (a student of mine), and Vic Vacquier, and me, discovered the first hydrothermal vent emanating from a mid-ocean ridge.[7] Finally, he and others found higher than expected heat flow over the oceanic swells, which they attributed to thermal anomalies in the upper mantle.[8]

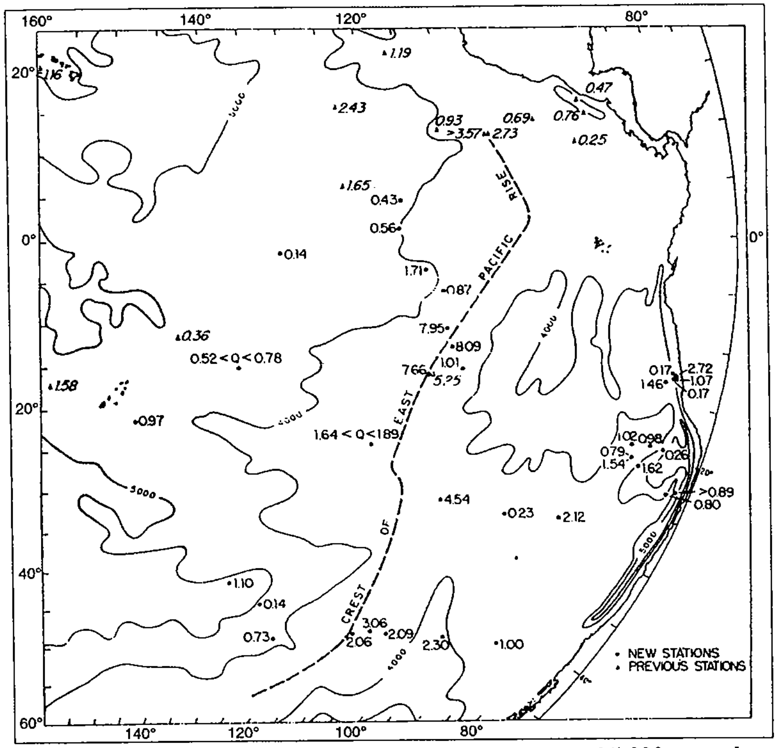
In the first part of this essay, I concentrate on Dick's contributions to the early heat flow measurements at sea and the influence they had upon Harry Hess in his development of the theory of sea floor spreading.[9] In the second part, I attempt to answer two questions of interest to those who study the history of scientific discoveries. Why did the heat flow community not come up with the idea of sea floor spreading before Harry Hess, and why, once he had published his ideas, did this community take so long to apply the concept to interpreting the heat flow data? Later in this volume Dan McKenzie, who overlapped with me at Madingley Rise, presents an approach to interpreting the occurrence of advances in the earth sciences. His approach stimulated me to reexamine these questions from his observation-oriented point of view. At the end of his essay, Dan raises his own question regarding the history of plate tectonics. "The paleomagnetic observations did not have the impact in retrospect that they should have had. . . . Why they did so remains for me a puzzle and also the most interesting historical question to be raised by the discovery of plate tectonics."[10] Using my explanation of the difficulty that the heat flow community had with sea floor spreading as a basis, I attempt to answer this question. I finish by presenting a summary of my own views on how advances occur in the earth sciences.

Von Herzen and the Early Heat Flow Measurements

Roger Revelle enlisted Teddy Bullard in the early 1950s to set up a program at Scripps to measure the heat flow through the floor of the oceans. In 1956, Teddy, Roger, and their student Art Maxwell summarized the results of measurements made at Scripps and those made by Teddy through the National Physical Laboratory in England.[11] Most of the measurements gave values very similar to those on the continents. However, on Scripps' 1952–1953 *Capricorn* expedition, Revelle and Maxwell had observed two much higher values at the crest of the East Pacific Rise. It was these measurements that led Dick to concentrate his systematic investigation of oceanic heat flow values on the relation between high heat flow and the rise.

In his first scientific paper, published in *Nature*, Dick summarized the measurements he took, and those reported by Bullard and others.[12] He plotted all the values on a topographic chart of the East Pacific Rise. Three features stand out: the large percentage of high values near the crest of the East Pacific Rise, the close–to-average values elsewhere in the oceans, and the scattered low values that occur even very close to the crest of the rise. Three years later, Scripps graduate students Bob Nason and Willie Lee used the Scripps heat flow equipment to show similar very high values at the ridge crest on a crossing of the mid-Atlantic ridge.[13]

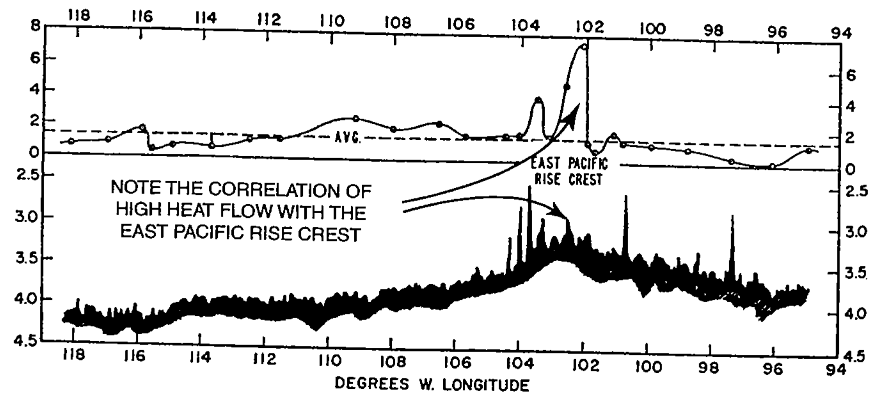
In 1962, on Scripps' *Risepac* expedition, Dick and Seiya Uyeda, from



Map of heat flow stations with generalized 4,000 meter and 5,000 meter bottom depth contours. Note the high values (> 2 cal/cm^2 sec) near the crest of the East Pacific Rise, the close-to-average values (1.0–2.0 cal/cm^2 sec) elsewhere in the oceans, and the scattered low values (< 1.0 cal/cm^2 sec) that occur everywhere, even very close to the crest of the rise. (Von Herzen, R., 1959. Heat-flow values from the South-Eastern Pacific. *Nature* 183: 882–883. Reproduced with permission of *Nature*, http://www.nature.com)

the Earthquake Research Institute of Tokyo University, ran a series of heat flow stations at 30 mile (50 kilometer) spacing across the crest of the East Pacific Rise at 14°S.[14] Values up to five times average occurred near the crest of the broad swell of the East Pacific Rise. However, a number of average or below average values also occurred within 30 miles (50 kilometers) of the highest values. In addition, they found low values, some less than one-third of average, at greater distances from the crest of the rise. They devoted considerable space in the resulting manuscript to an unsuccessful attempt to explain these values.

In a study published in the same year in the journal *Science*, Dick compared the heat flow values he had taken in the Gulf of California with

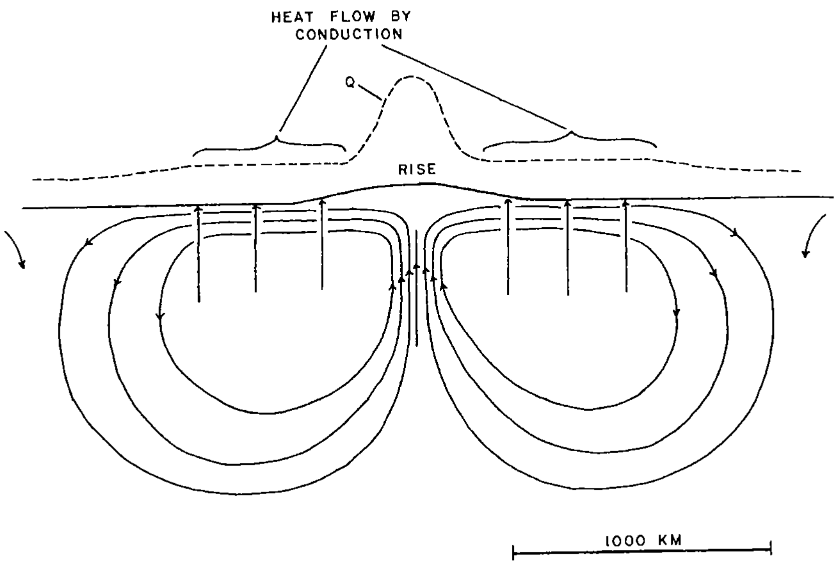


Heat flow and topography across the East Pacific Rise. Note the correlation between the high heat flow values and the crest of the East Pacific Rise. (Von Herzen, R. P., and Uyeda, S., 1963. Heat flow through the eastern Pacific Ocean floor. *Journal of Geophysical Research* 68: 4219–4250. Copyright 1963, American Geophysical Union. Reproduced by permission of the American Geophysical Union.)

those from the Gulf of Aden.[15] In both areas, he observed values significantly higher than the worldwide average. In the abstract, he stated, "[The gulfs] . . . closely coincide with the intersection of oceanic rises with continents and have likely been formed under tensional forces, which suggests an association with mantle convection currents." The figure on the next page, taken directly from his thesis, presents his idea of how these convection currents affect the surface heat flow field.[16] This concept with more geological embellishment appears in Scripps professor Bill Menard's 1964 book, *Marine Geology of the Pacific.*[17] Clearly the idea of an upwelling convection current beneath the rise was generally accepted at Scripps at the time. Both Dick and Bill knew from the absence of a major gravity anomaly that the East Pacific Rise must have a low density root. They inferred that higher temperatures caused this root and postulated that the upwelling limb of a major convection current within the mantle lay beneath the crest of the rise. The upwelling of hot mantle material created both the high heat flow anomaly and the elevation.

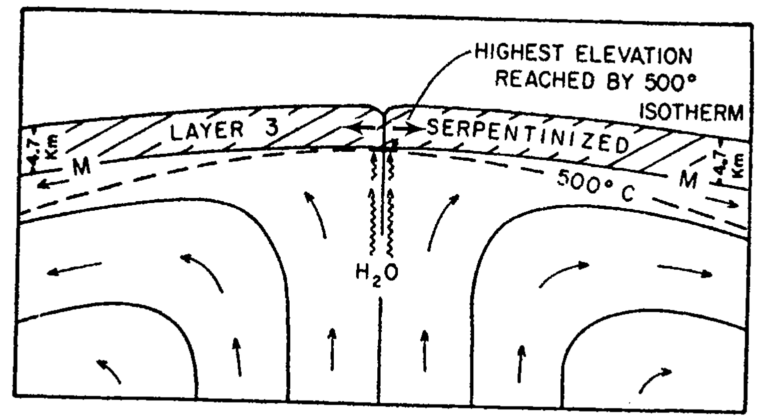
The Hypothesis of Sea Floor Spreading

In his now classic paper, Harry Hess used the following line of reasoning to justify sea floor spreading:



Possible mantle convection pattern beneath the eastern Pacific Ocean. Note the smooth increase of the predicted heat flow over the upwelling limb of the convection cell. The closed curves represent the motion of the material in the upper mantle. The arrows show the direction of flow. The straight lines with arrows represent the flow of heat by conduction from the convection current to the stationary crust above. (Von Herzen, 1960. Pacific Ocean Heat Flow Measurements, Their Interpretation and Geophysical Implications, Ph.D. thesis, UCLA, p. 119.) Used with the permission of Richard Von Herzen.

> The Mid-Ocean Ridges are the largest topographic features on the surface of the Earth. Menard (1958) has shown that their crests closely correspond to median lines in the oceans and suggests that they may be ephemeral features. Bullard, Maxwell and Revelle (1956) and Von Herzen (1959) show that they have unusually high heat flow along their crests. Heezen (1960) has demonstrated that a median graben exists along the crests of the Atlantic, Arctic, and Indian Ocean ridges and that shallow-depth earthquake foci are concentrated under the graben. This leads him to postulate extension of the crust at right angles to the trend of the ridges. . . . Paleomagnetic data presented by Runcorn (1959), Irving (1959), and others strongly suggest that the continents have moved by large amounts in geologically comparatively recent times. One may quibble over the details, but the general picture on paleomagnetism is sufficiently compelling that it is much more reasonable to accept it than to disregard it. . . . Menard's theorem that mid-ocean ridge crests correspond to median lines now takes on new meaning. The mid-ocean



The concept of sea floor spreading as proposed by Harry Hess. Abstracted from a diagram to portray the highest elevation that the 500 degrees C isotherm can reach over the ascending limb of a mantle convection cell, and expulsion of water from mantle which produces hydrothermal alteration, forming the mineral serpentine, above the 500 degrees C isotherm (Hess, 1962, note 9). The shaded area is the oceanic crust that is created by the intrusion of hot molten material. It moves away from the center of spreading with the intrusion of more material from below. Reproduced with permission of the *Geological Society of America.*

> ridges could represent the traces of the rising limbs of convection cells, while the circum-Pacific belt of deformation and volcanism represents descending limbs. The mid-Atlantic Ridge is median because the continental areas on each side have moved away from it at the same rate, about 1 cm/yr. This is not exactly the same as continental drift. The continents do not plow through oceanic crust impelled by unknown forces; rather they ride passively on mantle material as it comes to the surface at the crest of the ridge and then moves laterally away from it.[18]

Hess recognized that the high heat flow values could only be explained by massive amounts of intrusion at the crest of the mid-ocean ridges. It is tempting to believe that it was the coupling of these measurements with the insight of Bruce Heezen that led Harry directly to the concept of sea floor spreading.[19] The actual intrusion of mantle material into the crust at the crest of a mid-ocean ridge and the movement of this material away from the crest differentiates sea floor spreading from the mantle convection current hypothesis that preceded it.

The scientific community did not immediately accept the concept of sea floor spreading. For example Dick, Vic Vacquier, and others still found it necessary in 1963 and 1964 to test the relation between high heat flow and the crest of the mid-ocean ridges. In 1963, on the Scripps *Lusiad* expedition, Vic and Dick crossed the mid-Atlantic ridge a number of times in the South Atlantic.[20] They found a striking relation between high heat flow and the crest of the ridge as defined by the central magnetic high. In the following year, they made a series of crossings of the Central Indian Ocean. As expected, they observed the same relation between high heat flow and the crest of the mid-ocean ridge that they had found in the South Atlantic.[21] It was while traveling to join this expedition that I stopped off in Paris to meet Dick. I had just submitted my first scientific paper on heat flow measurements in the Somali Basin and the Gulf of Aden to a Royal Society symposium devoted to geological and geophysical studies in the Indian Ocean.[22] They presented their results in the same symposium.

A question that has puzzled me, in retrospect, is this: why did the three of us not combine at this time to analyze either (a) the two heat flow data sets quantitatively assuming sea floor spreading, or (b) attempt to reinterpret the magnetic data using the hypothesis of Fred Vine and Drummond Matthews?[23] This question becomes even harder for me to understand when I add three other facts. First, I had shared a cabin with Fred Vine for three months on the *Discovery II* the previous year. His hypothesis was not taken seriously by our supervisor and chief scientist on the cruise, Maurice Hill, but I had fought hard in support of Fred so he would get some ship time to test it by surveying a seamount near the crest of the Carlsberg Ridge. Second, I had available to me during the final stages of writing my thesis the manuscript of a paper on the Gulf of Aden by Tony Laughton, from the National Institute of Oceanography in England, to be published in the same symposium as my own paper and that by Dick and Vic.[24] In his paper, Tony, who had been on the *Discovery II* expedition with me, pointed out the necessity for the creation of new ocean floor in the center of the Gulf to fit the two edges back together again. Third, Harry Hess and Tuzo Wilson were both at Cambridge on sabbatical in 1964 and 1965, and I spent many weekends, during one of the coldest winters on record, in Madingley Rise with them in one of the few centrally heated buildings in Cambridge.

It was not until the publication of the interpretation of the magnetic stripes over Iceland's Reykjanes Ridge that the theory of sea floor spreading received overwhelming support, even from among the heat flow community.[25] Why such a significant delay? I believe that the answer to this question lies in the way the heat flow community approached observational data.

An Observational Approach to Scientific Discovery

Until I started writing this essay I did not realize how much my personal philosophy of how science advances had dominated the way I think about earth science. To understand the answer to the questions I have posed, I need to start from the point at which I developed my own approach to the subject. This happened when I was an undergraduate at Edinburgh University. I planned to work as a geophysicist after I graduated, but such a degree was not available at Edinburgh at the time. As a compromise I proposed to supplement a conventional four-year geology degree with two years of physics. I did much better in my first year in physics than geology. Following the advice of my undergraduate advisor I changed my degree to experimental physics during my second year. I graduated imbued by the hypothesis-testing method of doing science, which I had picked up in the experimental physics courses that I had taken.[26] I started and finished my graduate career at Cambridge with the same basic approach.

At Madingley and Scripps in the 1960s, I became acquainted with most of the major figures in the development of sea floor spreading and plate tectonics. My major effect regarding plate tectonics as a student was negative. In 1964, I successfully convinced Tuzo Wilson that the magnetic anomalies observed on either side of the ridge axis on the recently published *Vema* cruise across the mid-Atlantic ridge in the South Atlantic were not symmetric. Thus I talked him out of developing a magnetic timescale from this profile. Later this profile was to become the basis of the Cenozoic magnetic timescale.[27] When I arrived at Scripps, I worked closely with Bill Menard, Vic Vacquier, Bob Fisher, and Art Raff, and by the end of 1966 my opposition to sea floor spreading had evaporated. In 1967, I shared a cottage with Dan McKenzie in La Jolla when he and Bob Parker, also a student at Madingley, wrote their path-breaking paper on tectonics on a sphere.[28] Dan and I applied the theory to produce the first quantitative tectonic history of the Indian Ocean.[29] Scripps students Jean Francheteau, Roger Anderson, Miller Lee Bell, and I showed that the creation of new plate at a spreading center could account for the heat flow and subsidence of the ocean floor.[30]

As a result of my intimate involvement in the development of such a major advance, I have lost my belief that advances in the earth sciences occur primarily as a result of hypothesis testing. Neither Harry Hess nor Tuzo Wilson was testing a hypothesis.[31] Rather, they were creating new concepts out of the synthesis of poorly-constrained observational information.

They thought their concepts had validity because they explained the patterns they recognized in so many different sets of data. Nor do I believe that advances necessarily result from a paradigm shift during a scientific crisis, as has been advocated by Thomas Kuhn.[32] The advances occurred in the earth sciences before the field even realized that there was such a crisis. It is interesting to note that no advance occurred back in the 1920s when scientists did think there was a crisis. They resolved the crisis by maintaining the status quo and rejected the necessity for a paradigm shift – which is another blow to Kuhn's hypothesis.[33]

The Observational Approach

Earth science is an observational discipline. However, many processes affect any observation. Earth scientists cannot separate any single process entirely from all the processes that have occurred. Further, except for a limited number of cases, laboratory experiments do not scale to the real world. Thus, unlike physics or chemistry, earth science is not an experimental discipline. Earth scientists, in most cases, observe and describe phenomena rather than conducting experiments to test hypotheses. Synthesizing data and/or recognizing patterns in "noisy" data are in many cases more important than any experiments that they could perform. Major progress occurs by constructing simple physical models that describe the patterns that earth scientists have selected out of the background noise. They have to exercise care with their observations because, occasionally, the background "noise" carries information that is critical to the process or processes under study.

In his observation-oriented analysis of scientific discovery, Dan McKenzie, in a later essay in this volume, separates scientific observations into four categories: (1) observations that are wrong, (2) observations that are correct and can be described by existing theories, (3) observations that are correct but are too complex to be described by any simple model, and (4) observations that are correct but there is no theory that describes them. A scientific advance occurs when a model that accounts for the data in Category 2 also accounts for the observations grouped in Category 4.

It is easiest to see how this approach works by applying it to the evolution of plate tectonics. Tuzo Wilson developed a concept that incorporated sea floor spreading, linear magnetic anomalies, and trenches to explain transform faults, fracture zones, and the fit of the continents.[34] Lynn Sykes, Dan McKenzie and Bob Parker, Jason Morgan, and Bryan

Isacks and colleagues showed that the same concept also explained the type and distribution of earthquakes.[35] In addition, Xavier Le Pichon showed that it provided a self-consistent description of the tectonics of the entire surface of the earth.[36] It was considered a major advance because one simple concept could explain so many different sets of observations, which previously had no theory to explain them.

The Heat Flow Community and Sea Floor Spreading

The early marine heat flow community was made up of seagoing scientists who had the ability to build and run sensitive equipment under adverse marine conditions. All were able scientists who combined physical endurance with a strong physics or geophysics background. They included some of the best marine scientists of their generation, such as Sir Edward Bullard, Art Maxwell, Dick Von Herzen, Marcus Langseth, Victor Vacquier, and Clive Lister.

I believe that, like me, they let their experimental physics background dominate the way they looked at the earth. We knew that the heat flow at the ridge crests was high.[37] In addition, Teddy Bullard and others at Scripps knew that the East Pacific Rise was elevated because it was hot, and that there was a correlation between heat flow and the depth of the ocean floor.[38] However, as experimental physicists, we were stymied by the fact that we could not explain the very low values, especially those found by Dick Von Herzen and Seiya Uyeda near the crest of the East Pacific Rise.[39]

When I was a graduate student at Madingley, Teddy Bullard jokingly complained that he had not accomplished very much in geophysics because his name had not been given to any hypothesis or law. To rectify this omission, the students and junior staff at Madingley, with support from Maurice Hill, created "Bullard's Law." This law asserted, "Never take one marine heat flow measurement within 50 kilometers of another measurement because it is likely that it will differ from the first by at least one order of magnitude." Although humorous, this incident shows just how little respect was paid to the early heat flow measurements by most geophysicists. It illustrates the problem that the community had with the interpretation of their empirical data. Without an explanation of the low values that created huge scatter in the data, no one was willing to attempt to interpret the overall pattern quantitatively. We concentrated instead on trying to understand these low values. As I mentioned previously, Dick Von Herzen and Seiya Uyeda devoted a large part of their text to trying to account for the low values found over the East Pacific Rise.[40] This paper influenced my thesis more than any other. I devoted about half of my thesis attempting to find an explanation for the low values.[41]

Why the Community Missed Sea Floor Spreading

We were physicists who wished to test a hypothesis. Led by Teddy Bullard, we wished to discover whether or not the heat flow measurements presented evidence for the upwelling limb of a convection cell beneath the mid-ocean ridge axes. Intuitively, we expected to observe a relatively smooth increase from near normal on the flanks to a factor of four higher than normal over the crest of the ridges. The apparently random occurrence of low values completely confused us, especially those near the crest of ridges, close to values 20 times higher. The scatter in the data was so high that the mean values over the crest were indistinguishable statistically from those on the flanks. The data neither strongly agreed nor disagreed with the hypothesis.

The low values and the scatter in the data became a major concern to both myself and others working in the field. As a consequence we overlooked the pattern in the measurements. We did not see either that the envelope of the high values showed a clear correlation with distance from the crest of all the mid-ocean ridges or that the drop-off rate for these high values varied with the width of the ridge. Thus, we did not realize that differing rates of intrusion at the individual ridge crests could explain the different drop-off rates. We believed our data, but without an explanation of the low values, we were unwilling to interpret the measurements quantitatively. We placed them within Category 3: too complex to be explained by any simple theory. Most geophysicists were less charitable. They could not believe a measurement that could differ by more than an order of magnitude over a distance of only 30 miles (50 kilometers). They placed the measurements in Category 1: observations that were obviously wrong. It took a geologist like Harry Hess to see the pattern in the data.[42] He recognized that what was critical for understanding the earth as a whole was not the isolated lows, but the large number of very high values at the crest of the mid-ocean ridges. It was more important to recognize the consistent envelope of the high values from one ridge to another rather than to be overly concerned with the scatter created by the low values.

The Successes

Once Harry Hess and Tuzo Wilson had articulated the key concepts of plate tectonics, the hypothesis-testing approach of the heat flow community moved the field forward very quickly.[43] Even before the concept was applied to earthquakes, Marcus Langseth, Xavier Le Pichon, and Maurice Ewing of Lamont Geological Observatory had introduced the idea of a 60 mile (100 kilometer) thick plate created at a ridge axis to try to explain the heat flow and subsidence data across the mid-Atlantic ridge.[44] From the poor fit of the observed to the predicted subsidence, and the observed to predicted decrease in heat flow with distance from the ridge crest, they argued that the concept did not work. The following year Dan McKenzie recast the problem non-dimensionally and showed that by varying the boundary conditions the same model could be made to match the heat flow data.[45] University of Wisconson professor Ned Ostenso and his graduate student, Peter Vogt, pointed out that Langseth, Le Pichon, and Ewing had omitted the loading effect of the water.[46] Rather than being a poor fit, the model actually gave a reasonable fit to the subsidence of the ridge. (Due to a surprising omission by *Nature*, Peter Vogt and Ned Ostenso have not received the credit they deserve for recognizing the importance of this correction. In the published paper, the entire paragraph that discussed the isostatic correction for the loading effect of the water was omitted. This omission went unnoticed because the paragraph *was* included in the reprints that the journal sent back to the authors!)

The thick plate concept adopted by the heat flow community was the forerunner to the plate models that Dan McKenzie and Bob Parker, Jason Morgan, Bryan Isacks and colleagues, and Xavier Le Pichon developed to establish the quantitative aspects of plate tectonics.[47] It also gave Norman Sleep, Jean Francheteau, my students Roger Anderson and Miller Lee Bell, and me a hypothesis to test.[48] Very quickly we established that the plate model that explained the ridges, trenches, and earthquakes could also account for the subsidence of the ocean floor as the age of the ocean crust increased.

The explanation for the very low heat flow values finally came from the work of Clive Lister.[49] He hypothesized that the low values were due to hydrothermal circulation in the ocean crust. Using Clive's concept as a basis for selecting an area where heat loss by hydrothermal circulation probably did not occur, my students and I showed that these carefully selected heat flow data fit the same plate model that accounted for the subsidence of the ocean floor.[50] In testing Clive's concept, Dave Williams and others found the first hydrothermal vent at a ridge crest.[51] This led to the realization of the importance of hydrothermal circulation on the ocean floor and the discovery of hydrothermal venting at the crest of all of the mid-ocean ridges.

WHY DID PALEOMAGNETISTS MISS SEA FLOOR SPREADING?

Later in this volume, Dan McKenzie raises two questions regarding the history of the development of plate tectonics: (1) Why did the paleomagnetic community not come up with the idea of sea floor spreading ahead of Hess? (2) Why did the rest of the earth sciences community take so long to accept their conclusions regarding continental drift?[52] I offer here some personal comments based on some obvious parallels between paleomagnetism and heat flow and my experiences answering the same questions for the heat flow community.

Both fields were relatively new at the time of the development of sea floor spreading and plate tectonics. Both fields were based on difficult observations. Like heat flow, paleomagnetism attracted a number of unusually able scientists, for example, P. M. S. Blackett, Keith Runcorn, Ted Irving, Allan Cox, Dick Doell, Victor Vacquier, Neil Opdyke, Christopher Harrison, and Ron Girdler. As a consequence of the difficulties with the technique, in the early stages scientists with a classical experimental physics background dominated the field.

Keith Runcorn and Ted Irving believed that their measurements confirmed the idea that the continents had moved.[53] In addition, they knew of Euler's theorem and called the points about which the continents rotated "pivot points." However, the early paleomagnetists could not explain why they found some rocks magnetized in the opposite direction to that of the present earth's field. This threw some doubt on the reliability of the entire operation. This doubt was exacerbated by the fact that one of the major figures in geophysics at the time in Europe, Sir Harold Jeffreys, did not believe in continental drift and doubted the reliability of paleomagnetic measurements. Indeed, he dismissed them with the following statement: "In studying the magnetism of rocks the specimen has to be broken off with a geological hammer and then carried to the laboratory. It is supposed that in the process its magnetism does not change to any important extent, and though I have often asked how this comes to be the case I have never received any answer."[54] I took the opinions of Jeffreys very seriously, since he was generally credited with having made prewar geophysics into a respectable discipline.

Harold Jeffreys and others who opposed drift used the fact that the reversals were apparently inconsistent and unexplained to disregard the entire category of paleomagnetic measurement. They believed that the paleomagnetic measurements were wrong and placed them in Category 1. Like heat flow scientists, the early paleomagnetists were physicists by training. Without an unambiguous explanation of reversals they could not answer Jeffreys and the opponents of continental drift. Thus they concentrated either on making more measurements of polar wandering to overwhelm the opposition with the weight of the observations, or on trying to understand the reversals. They placed their measurements in Category 3: correct observations that were too complex to be explained by any simple model. Apart from Runcorn they did not attempt to incorporate them into an overall theory of how the earth worked. They were limited further by their lack of understanding of the observations from the oceans that might support drift. Ron Girdler from Newcastle University and George Peter from Lamont showed that positive and negatively magnetized stripes of material on the ocean floor could explain the striped magnetic anomalies in the Red Sea.[55] They came the closest of all to arriving at the hypothesis of sea floor spreading before Harry Hess.[56] However, they did not expand upon this idea of reversals or suggest what process could have created the stripes.

It took a geologist like Harry Hess with training in synthesizing data and pattern recognition to place the results in broad perspective.[57] In a wonderful introduction to the paleomagnetic measurements – "One may quibble over the details, but the general picture on paleomagnetism is sufficiently compelling that it is much more reasonable to accept it than to disregard it" – Harry dismissed the opposition. This permitted him to use the paleomagnetic data to argue that the continents moved significantly over geologic time. As a geologist, he recognized that the important observation for understanding the earth as a whole was that the continents *had* moved. He used these measurements and the dates of extensional processes on the continents bordering the Atlantic Ocean to propose a spreading rate of one centimeter (less than half an inch) per year. That he was within a factor of two of the actual rate shows the power of his reasoning. Hess recognized a pattern that few others were willing to accept. He showed that a model that could explain why the mid-ocean ridges occurred in the center of the ocean basins (Category 2: observations that are correct but can be described by existing theories) could also explain the heat flow measurements and the paleomagnetic results (Category 4: observations that are correct but there is no theory that describes them).

Meanwhile Allan Cox and colleagues in the United States, and Ian McDougall and colleagues in Australia, had been systematically documenting magnetic reversals in rocks.[58] They determined that lava flows of the same age had the same direction of magnetization, but that this direction changed from time to time. They were able to date the time at which the field changed. Fred Vine and Drummond Matthews showed that combining reversals of the field with an intrusion process that added material equally on either side of a spreading center would create positive and negative magnetic stripes of material on the ocean floor.[59] (Unknown to me at the time, Lawrence Morley had developed the same concept but was unable to get his paper published.)[60] These stripes explained the linear magnetic anomalies observed on shipboard profiles and provided strongly positive evidence in favor of the theory of sea floor spreading. In this case, it was the understanding of reversals – the "noise" in the paleomagnetic data – that provided the most powerful confirmation of sea floor spreading: the striped marine magnetic anomalies.

The Process of Scientific Discovery

The key originators of sea floor spreading and plate tectonics, Harry Hess and Tuzo Wilson, each had a field-oriented and geographically diverse geological background.[61] They demonstrated great ability at both data synthesis and pattern recognition. This gave them the insight to look at the world from a totally new perspective. However, the physics-trained younger generation had skills at developing simple quantitative models from physical concepts. These skills permitted the comparison of the observations with predictions and led to the general acceptance of the theory.

As a result of my involvement in the development of plate tectonics I now believe that advances in the earth sciences occur in three stages. The first involves the origination of the concept; the second, the construction of a model where the predictions can be compared with a set of observations, the third, the application of the model to another set of data. What is common to each stage is the recognition of the importance of the observations. The first stage involves synthesis and pattern recognition; the second and third stages emphasize reliable measurements and the quantitative comparison of these measurements with predicted values.

The first stage can occur – as it did with sea floor spreading – as the result of the synthesis and recognition of patterns in large quantities of

data. It can also arrive serendipitously. For example, Marcus Langseth, Xavier Le Pichon, and Maurice Ewing created the first thermal model of the oceanic lithosphere to demonstrate that plate tectonics could not explain the heat flow and subsidence across a mid-ocean ridge.[62] Dan McKenzie generalized the approach, but fit the heat flow observations with a plate that was too thin.[63] However, his generalization showed the power of such an approach and led to a much clearer understanding of the concept.

The second stage involves the testing and refining of the concept. The earth science community accepted the concepts of sea floor spreading and plate tectonics so readily because of the ability of a group of scientists to construct models based on these concepts. The comparison of the predictions of these models with reliable observations permitted a quantitative evaluation of the concepts.

The third stage involves the application of the concept to describe a set of observations that are believed to be correct but are not as yet understood. In plate tectonics this occurred when a concept constructed to explain the features of the ocean floor and the reconstructed position of continents was found suitable to explain the worldwide distribution of earthquakes. For the thermal models, it occurred when it was realized that the concept that accounted for the heat flow data could also explain the subsidence of a mid-ocean ridge.

For the development of a concept, selecting the appropriate and key observations is most important. At this stage geological training involving pattern recognition is at its most valuable. However, in the second and third stages, the ability to construct a physical model and then test it is required. For these stages, the hypothesis-testing methods of experimental physics become more important. The approaches are complementary; for sea floor spreading and plate tectonics both occurred, and this accounts for the speed at which the hypotheses became accepted theories.

Conclusion

The problem with the hypothesis-testing approach as applied to the earth sciences is that field-measurement noise often contains crucial information about the process under study. In the case of the interpretation of the early heat flow and paleomagnetic measurements, it was the inability to get past this noise that prevented the observational scientists from moving on to a deeper interpretation of their results.

However, this takes nothing away from the early heat flow community. They made the original measurements under often appalling conditions in rough weather and from very small oceangoing tugs or converted sailing ships. The foremost of this intrepid group of marine scientists was Dick Von Herzen. He made arguably some of the most important measurements ever taken at sea. Harry Hess appears to have developed the concept of sea floor spreading almost immediately after he had read and digested the importance of Dick's discovery of very high heat flow measurements near the crest of the East Pacific Rise.

What appears surprising is that the heat flow community took uncommonly long to try to model the high values at the ridge axis. I believe that they did not press to explain their results quantitatively because of their concern about the scatter created by the low values. Although obviously an oversimplification in light of the efforts of Keith Runcorn and Ted Irving, I argue that the early paleomagnetic community may have behaved similarly because of their lack of a convincing explanation for reversely magnetized rocks. In both cases, the earth science community at large did not believe the early measurements and hence the implications of these measurement went unheeded.

In the 1950s and early 1960s geological-geophysical expeditions at Scripps, and to a lesser extent, the Lamont Geological Observatory concentrated on making theory-relevant observations in a real-world setting. (For example, Bill Menard encouraged Von Herzen to pursue heat flow measurements because he understood their significance for ideas about mantle convection.) The history of the development of sea floor spreading and plate tectonics demonstrates the importance of these thoughtfully taken observations. The concepts were developed as a result of correlating many different types of observations into a coherent pattern. The advances occurred so quickly because of the complementary nature of this basically geological approach, with its emphasis on data collecting, with the hypothesis-testing approach of scientists trained in experimental physics.

As a consequence of my analysis, I do not believe that it was by chance that the Department of Geodesy and Geophysics at Cambridge had such a major effect upon the field. Teddy Bullard, the chair of the department, and Maurice Hill, the leader of the marine group, actively encouraged and hosted the interaction between global-thinking geologists such as Harry Hess and Tuzo Wilson, observational marine geologists such as Bill Menard and Bob Fisher, and their much younger, dominantly physics-educated graduate students. I believe that it was the symbiosis created by this interaction that led so many of the students and younger

staff ultimately to contribute so significantly to this major advance in the earth sciences.

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