Chapter 3

Reversals of Fortune

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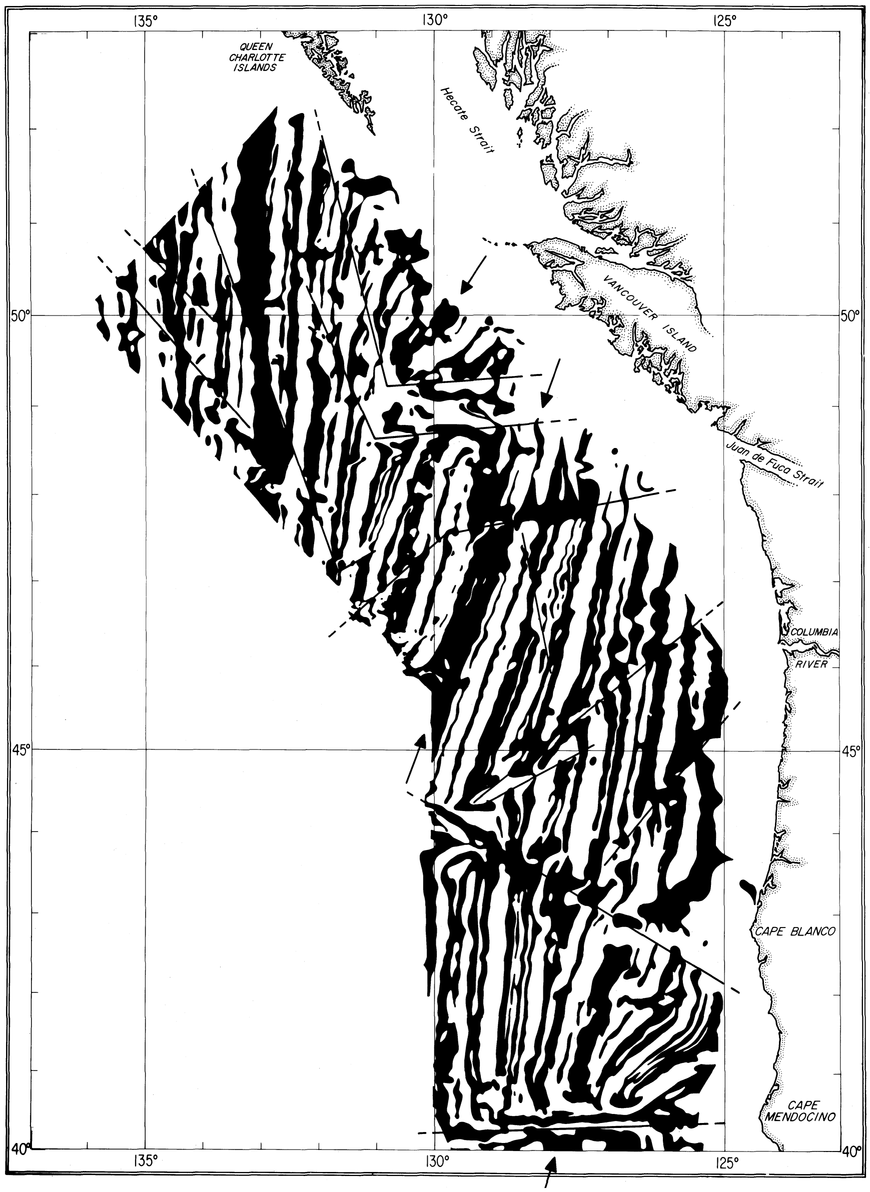
My contribution to the new understanding of global tectonics predated the recognition of "plates," and the formulation of the plate tectonic paradigm, in 1967. In 1963, Drummond Matthews and I added a rider to the concept of sea floor spreading, which had been proposed by Harry Hess of Princeton University.[1] According to Hess, "conveyor belts" of crust and upper mantle move symmetrically away from mid-ocean ridges and passively drift continents apart.[2] We proposed that the conveyor belts might also act as tape recorders that record reversals in the polarity of the earth's magnetic field in the 'fossil' (i.e., remanent) magnetism of the oceanic crust. This record can be played back by measuring the changes in the intensity of the earth's magnetic field at or above sea level from ships or aircraft.

The validity of our idea depended on the reality of three phenomena: sea floor spreading, reversals of the earth's magnetic field, and the importance of remanent magnetism in the oceanic crust. Not one of these was widely accepted at the time, even by experts in the respective fields. In addition, from the limited magnetic data available to us, largely from the North Atlantic and the northwest Indian Oceans, it appeared that the record was not very clearly written. For example, there was no obvious symmetry of the observed anomalies in the earth's magnetic field about the ridge crests in these areas as predicted by the simple model. This seemed to imply a rather diffuse zone of formation of oceanic crust at a ridge crest, presumably by a process of extrusion and intrusion of basaltic magma. It was not surprising, therefore, that in 1963 the idea was, in general, rather poorly received.

However, in 1965, following the recognition of the Juan de Fuca Ridge southwest of Vancouver Island and the definition of the Jaramillo event within the geomagnetic reversal timescale for the past 3.5 million years,

Fred Vine, at Cambridge in 1967, examining magnetic profiles. (Photo courtesy of Fred Vine.)

it became clear that over this ridge the magnetic field anomalies are not only symmetrical about the ridge crest, but also reflect the reversal history.[3] This, in turn, implies an essentially constant rate of spreading on the Juan de Fuca Ridge, of just over an inch (2.4 centimeters) per year per ridge flank for the past 3.5 million years. In 1966, the acquisition of new magnetic anomaly data for the Pacific-Antarctic Ridge, in the South Pacific, and the Reykjanes Ridge, south of Iceland, confirmed the Juan de Fuca Ridge result.[4] This enabled Jim Heirtzler, of Lamont Geological Observatory, Columbia University, and me to make a convincing case for the validity of the sea floor spreading hypothesis at a meeting held at the Goddard Institute for Space Sciences, New York, in November of that year.[5] I also presented the case at the annual meeting of the Geological Society of America in San Francisco in November 1966. It is probably true to say that these presentations, and the publications by Jim Heirtzler and Walter Pitman also of Lamont, and myself, in December 1966, were instrumental in finally convincing most earth scientists of the validity of sea floor spreading, and hence of continental drift.[6]



Summary of anomalies in the earth's magnetic field measured at sea level off British Columbia, Washington, and Oregon. Areas of anomalously high field strength are shown in black. Straight lines indicate faults offsetting the anomaly pattern; arrows, the axes of three short ridge lengths in the area – from north to south, the Explorer, Juan de Fuca, and Gorda Ridges. Reproduced courtesy of the Geological Society of America. (Raff and Mason, 1961, note 11.)

Early Interest in Continental Drift and Geophysics

In April 1955, at the age of 15, I was a student at Latymer Upper School, West London, and studying for my 'O' (Ordinary) level examinations, which were just two months away. It was the Easter holiday and in a rather desultory attempt to study for geography, I opened a physical geography text, probably for the first time, for as I recall, on the first page of the first chapter, there was a diagram illustrating the approximate fit of the Atlantic coastlines of South America and Africa. In the text, it stated that although it had been suggested on the basis of this fit that these continents were once part of a supercontinent that subsequently split and drifted apart to form the South Atlantic Ocean, geologists had no idea whether there was any truth in this hypothesis. I was struck at once both by the boldness of the idea that seemingly stable continents might have drifted across the face of the earth in the past, and by the fact that we did not know whether this had occurred. It seemed to me that one could hardly conduct any meaningful study of the history of the earth until one had resolved this issue. Surely there must be some way of proving or disproving the concept of continental drift.

In my 'O' level examinations I did well in mathematics, physics, and geography, but only just managed to pass English, Latin, and French. I was not an "all-rounder" academically. However, my limited abilities were well-suited to the English 'sixth-form' system whereby, between the ages of 16 and 18, one studied just three or four subjects to 'A' (Advanced) level. My own preference would have been to take 'A' levels in mathematics, physics, and geography, but I was persuaded to take the then more conventional combination of pure and applied mathematics, physics, and chemistry, which could lead on to a wider spectrum of possible careers in the physical sciences or engineering. I retained my interest in geography, however, and continued to be an active member of the school's Geographical Society. As a sixth-former I had a great interest in the physical environment and read everything I could find, for example, about the International Geophysical Year (IGY), which lasted for 18 months (!) during 1957 and 1958. My performance in the 'A' level examinations earned me a state scholarship and a place at St. John's College, Cambridge, to study natural sciences.

Undergraduate Years at Cambridge, 1959–1962

Only subsequently have I realized just how unusual the natural sciences course at Cambridge was at the time, given its large range of options. By now I was determined to drop chemistry; I had also decided that I wanted to be a schoolteacher and would probably teach physics and/or mathematics. I had spent four terms, after 'A' levels and before entering Cambridge, teaching mathematics at Latymer, an experience I had not only survived but enjoyed. My director of studies respected my wishes, but pointed out that we needed to identify one or more options in addition to mathematics and physics in order to fulfill the degree requirements. He ran down the list and alighted on geology. He said, "How about geology?" There was a slight pause, and he looked up. "Do you like the open air? Would you enjoy the fieldwork?" "Why, yes," I said, "of course." "Well that's done then, geology it is."

At the time, it was not possible to do a degree course in geophysics, and there was very little geophysics in other, related courses such as geology. For the first two years of geology I had to content myself with trying to identify anything in what I read or was taught that was incompatible with the concept of continental drift. I found nothing. In the third and final year of the degree (so-called Part II), one studied just a single discipline, in my case geology with emphasis on mineralogy and petrology. There was also a short, introductory course on solid-earth geophysics given by Sir Edward Bullard, head of the Department of Geodesy and Geophysics at Cambridge, which was essentially a small graduate research school. Sir Edward (universally known as "Teddy") was an entertaining and inspiring lecturer. He certainly conveyed the excitement in the field at the time, not least it seemed to me in the area of marine geophysics, where the development of new techniques during the previous decade had yielded some surprising and fascinating results. Teddy's department included a marine geophysics section (the only one in the country) headed by Dr. Maurice Hill, and funded largely by the U.S. Office of Naval Research. Teddy himself had been involved with the development of a new technique to measure heat flow through the floor of the deep ocean basins; Hill had pioneered the use of the proton precession magnetometer to make underwater measurements of the earth's magnetic field.[7] Teddy's short "taster" course was followed by a longer, more conventional course on geophysical methods given by Hill. These two courses confirmed and strengthened my belief that this was the subject that interested me most, and that if I were to undertake postgraduate research it would be in this area, ideally in the subdiscipline of marine geology and geophysics. At about this time, however, I also contacted the university's Department of Education with a view to doing a postgraduate Certificate of Education the following year.

To have been taught geophysics by two of the leading exponents of the field in my final year as an undergraduate was good fortune enough, but there was more. In January 1962, Cambridge was host to the 10th Inter-University Geological Congress, an annual three-day meeting, organized primarily by undergraduates, the venue for which rotated around British universities that offered degree programs in geology. The theme for this particular meeting was "The Evolution of the North Atlantic," and the lead, guest speaker was Professor Harry Hess of Princeton University. As a student of geology, and mineralogy and petrology in particular, I was already familiar with and an admirer of Hess' work, which ranged over mineralogy, petrology, tectonics, geophysics, and marine geology. His talk, entitled "Impermanence of the Ocean Floor," was essentially equivalent to his paper published subsequently under the title "History of Ocean Basins." The first part was a summary of the geological and geophysical characteristics of the deep ocean floor and the second part an explanation of them in terms of mantle convection and what soon became known as "sea floor spreading," thanks to Bob Dietz, of the U.S. Navy Electronics Laboratory and the Scripps Institution of Oceanography, San Diego.[8] This radical idea was proposed in part to explain what was known of the ocean floor and mid-ocean ridges in general, but also to explain the distinctive characteristics of ridge crests in particular. From Hess' personal point of view it had the additional merit of providing what he regarded as the most plausible and satisfactory explanation for the subsidence of the flat-topped seamounts that he had discovered while captain of the USS *Cape Johnson* and on convoy escort duty across the Pacific during the latter part of the Second World War.[9]

To me this was an inspiring and exciting synthesis and explanation; above all, it was a testable hypothesis and a potential explanation of continental drift. The whole tenor of this meeting was, if only by implication, favorable toward the concept of continental drift. This was in great contrast, I suspect, to the climate of opinion in North America at the time, where such ideas would have been regarded as verging on the heretical. It is probably true to say that throughout the first half of the 20th century there was not the same degree of opposition to the concept of continental drift in Britain and the British Commonwealth countries as there was in North America.[10] In particular, in the late 1950s and early 1960s, there was renewed interest in the idea following the development of paleomagnetic techniques to determine paleolatitudes. In March 1962, for example, I attended a meeting of the Natural Sciences Club at Cambridge addressed by P. M. S. Blackett in which he provided an elegant summary of the results of such paleomagnetic studies; continental drift was assumed to be axiomatic. In contrast, in North America up to that time, the use of the paleomagnetic method to determine paleolatitudes had been largely neglected, if not shunned, perhaps because of the underlying assumptions.

In April 1962 I was awarded a three-year Shell Studentship to undertake a Ph.D. in the marine geophysics section of the Department of Geodesy and Geophysics at Cambridge. This was quite surprising in that the department consisted almost entirely of theoreticians (mathematical physicists) and applied physicists, who built new instruments. The one exception was Dr. Drummond H. Matthews, a geologist with a background similar to my own, who had entered the department as a graduate student in January 1958. It was decided that I should be Drum Matthews' first research student, and that I should work on the interpretation of magnetic data. At any one time, there was at least one graduate student working on each of the main geophysical techniques, such as gravity, heat flow, refraction seismics, magnetics, and so on. There was a 'vacancy' in the magnetic area and it was entirely appropriate that Drum Matthews should supervise me, not only because of our similar backgrounds, but also because he was involved in the acquisition of magnetic data at the time and had measured the magnetic properties of some basaltic rocks dredged from the ocean floor as part of his Ph.D. thesis. By this time, surface, deep-towed, buoy, and differential proton magnetometers had been built in the department, and the main requirement was to develop techniques and ideas for interpreting the magnetic field data, which were accumulating at an ever increasing rate, but were largely uninterpreted.

Presidents of the Geological Society at Cambridge – the Sedgwick Club – were traditionally drawn from the final year undergraduates, and held office for one term. During this term they were expected (or was it required?) to give a presidential address. In May 1962, as president for the summer term, I gave a talk entitled "HypotHESSes" at the 870th meeting of the club. This topic was a natural choice for me, in that Harry Hess' range of interests closely paralleled my own, and I had been inspired both by his papers and by his talk a few months earlier. The address was therefore something that I enjoyed preparing. In addition, it was useful review for my finals, which by then were very imminent. In the talk I summarized Hess' work and ideas on layered igneous intrusions, on the mineralogy and crystallography of pyroxenes, on the alteration of ultrabasic rocks and in marine geology, emphasizing the connections between them. I assumed that most or all of my audience had been present at the meeting in January, and the talk was intended therefore to provide the background to the development of Hess' current ideas. As a consequence I only made brief mention of the substance of his January talk.

Drum Matthews and Tony Laughton, from the National Institute of Oceanography, Wormley, were present, and it soon became clear during the discussion that followed that they had not been present at Hess' talk in January. This was their first encounter with the concept of sea floor spreading. Someone, quite possibly Drum or Tony, asked whether I thought that the north-south 'grain' of linear magnetic anomalies recently discovered in the northeast Pacific might be related to sea floor spreading.[11] (The lack of any reference to these anomalies, and to the central magnetic anomaly observed over ridge crests, was a notable omission from Hess' talk and subsequent paper.) I replied that I felt that they must in some way be an expression of mantle convection as envisaged by Hess, but I had no idea how this effect was produced.

Background to the Vine–Matthews Hypothesis

When I joined the Department of Geodesy and Geophysics at Madingley Rise, Cambridge, in October 1962, Drum Matthews was at sea in the northwest Indian Ocean. At the time he was coordinator of the U.K. contribution to the International Indian Ocean Expedition. Initially I was put under the wing of Maurice Hill. My main assignment was clear, however: to review published magnetic surveys and traverses at sea, the methods that had been used in interpreting them, and current lines of approach.

The department was a very friendly and happy place, not least because of the lead provided by the senior staff. Coffee and tea breaks were something of an institution. Technicians, academic staff, students, and visitors (typically from North America, because of Teddy Bullard's reputation and contacts there) almost literally rubbed shoulders seated at long tables. Conversations ranged from serious science to whether Teddy's new car befitted his status. It clearly didn't but he would be the last person to be bothered by this. There was an air that doing science was fun, and that there had never been a better time to do geophysics. Certainly, as far as marine geophysics was concerned, the ship time and resources that had been made available since World War II had led to major developments in instrumentation and techniques that were yielding new data at an accelerating rate. The general philosophy in the department was that one might well waste one's first year as a research student investigating a difficult problem that ultimately proved to be intractable, but that it was only by taking such a risk that you increased your chances of doing significant and original research.

I had had little opportunity to discuss the project with Drum, but I had gotten the impression that he had in mind constructing analogue models with iron filings and putty, to simulate the volcanic topography at ridge crests, and then measuring the disturbances in an applied field caused by the induced magnetization in the model topography. Even if the simulated anomalies bore no relation to the observed anomalies, one might still be able to make a correction for the induced magnetization of the volcanic topography. Everyone was mystified by the fact that despite the relatively strong magnetization of the volcanic rocks of the ocean floor there was no systematic correlation between topography and the magnetic anomalies developed over it. In many ways this was the crux of the problem.

My completed literature review, dated January 1963, included the following statements: "Seamounts and volcanic islands give rise to large and obvious anomalies but these can rarely be explained by models assuming uniform magnetization throughout and directed parallel to the present earth's field. This discrepancy suggests that there may be a large thermoremanent component of magnetization, probably often reversed relative to the present earth's field."[12] Ron Girdler and George Peter, both working at Lamont at the time, considered it essential "to assume reversed magnetization in order to interpret a linear anomaly in the Gulf of Aden and support this by convincing calculations."[13] However, they favored a mineralogical self-reversal mechanism to explain the reversed magnetization rather than a reversal of the earth's magnetic field. I continued: "This does not strike one as being a necessary corollary. If current theories regarding impermanence of the ocean floor are correct, paleomagnetic evidence would suggest that the thermoremanent component of oceanic basalts should, in most cases, be approximately normal or reversed.[14] All too little is known about the magnetic properties of oceanic basalts. Work on dredged samples suggests that they are not essentially different from exposed basalts but would indicate that they invariably have a very strong remanent component, such that the remanence is very much greater than the induced intensity.[15] Values of susceptibility (that determine the induced magnetization) assumed in models simulating magnetic anomalies, often, necessarily, have to be high, higher than is reasonable in the light of existing measurements (of susceptibility) on basalt samples."[16]

I concluded that it seemed "highly desirable that any interpretation technique should be able to take account of remanent magnetic intensity even if unknown. Certain computer programs would appear to be capable of doing this. Possibly (analogue) model studies could also, but there is no evidence for this. Such studies would also appear to necessitate very elaborate, cumbersome and, presumably costly apparatus." "The use of computers in more recent years to simulate anomalies, and conversely, magnetic bodies from anomalies, may herald a breakthrough in interpretation methods. Computer techniques are probably more potent than model studies and easier to handle, judging by the dearth of model studies in the literature."

On reading these conclusions Drum's face visibly fell. Quite apart from the fact that he was, I suspect, looking forward to playing around with physical models, he had not had the time to keep abreast of the rapid developments in scientific computing at that time. However, plenty of help was at hand. Several other research students in the department were using computer methods, albeit in other contexts, and Teddy himself was not only at the forefront of developments in computing in relation to geophysics, but had also written a program to compute a magnetic anomaly profile across a two-dimensional model. However, this program was written in machine code and could only assume induced magnetization in the body producing the anomaly. I had taken a course in computer programming during the previous term and, although I had learned machine code, it was clear that all future programming would be in higher-level autocode. I therefore set about writing my own two-dimensional program for the interpretation of profiles, using mathematical expressions published in several places in the literature, and allowing for any direction of resultant magnetization, that is, remanent plus induced magnetization.[17]

Drum Matthews returned from the Indian Ocean with a large quantity of magnetic data, including a detailed survey of the crest of the Carlsberg Ridge in the northwest Indian Ocean at 5°N. This survey, measuring 51 by 39 nautical miles and known as Area 4A, was the largest and most detailed survey of a known mid-ocean ridge crest at that time.[18] Clearly if I was to interpret this quantitatively, I would need a program capable of calculating the anomaly over three-dimensional features so that I could carry out the correction for induced magnetization as envisaged by Drum.

At about this time, I visited Imperial College, London, where a mathematician, Dr. K. Kunaratnam, had just completed his Ph.D. under Professor J. M. Bruckshaw and Dr. R. G. Mason.[19] He had developed a variety of techniques for interpreting both gravity and magnetic anomalies in either profile or survey form, assuming two- or three-dimensional models respectively. His program for a three-dimensional source region used a particularly elegant method both to approximate the body and to calculate the anomaly in the earth's magnetic field developed over it.

This was an important consideration at the time in view of the slow speed and small storage capacity of computers. The program could also deduce the direction and intensity of magnetization of a specific topographic feature, given details of the topography and of the anomaly observed over it. Such a program was ideal for interpreting the anomalies measured over isolated seamounts. Kunaratnam had developed it in order to interpret the anomalies associated with seamounts within the area surveyed off the west coast of North America in the mid- to late 1950s by Mason and Arthur Raff.[20] In a similar vein, part of my Ph.D. project was to interpret earlier magnetic surveys acquired by the department, and these were typically of isolated seamounts. Dr. Kunaratnam gave me a draft copy of his thesis and said that I was welcome to use any of the mathematical formulations he had developed. I therefore wrote the equivalent of his three-dimensional program for use on the Cambridge (Mathematical Laboratory) computer, EDSAC 2. Subsequently, in order to interpret larger surveys of irregular topography, such as the whole of Area 4A, I would need to utilize a more conventional, if less efficient, formulation in which the bathymetry (ocean floor topography) is approximated by a grid of vertical prisms.

In many ways the results of Drum's magnetic survey of Area 4A were so spectacular that they did not need quantitative interpretation. While making the survey he was concerned, on the basis of the bathymetry, that they were not over the ridge crest. With hindsight this was understandable because approximately one-third of the survey area is occupied by a transverse fracture zone (including what we would now call a transform fault), and away from this, the central valley is not well, or continuously, developed. At one point the central valley appears to be blocked, presumably by volcanism. In contrast to the bathymetry, away from the fracture zone the magnetic anomalies generally form areas of positive and negative anomalies separated by steep anomaly gradients that parallel the trend of the ridge and largely disregard the bathymetry beneath them. Within the areas of positive anomaly, there are some positive correlations with bathymetry, and within areas of negative anomaly some negative correlations. This is what one would expect for reversely and normally magnetized features respectively at this latitude. Thus qualitative inspection alone indicated that, away from the fracture zone, the area is underlain by blocks or avenues of normally and reversely magnetized crust that parallels the trend of the ridge crest. Moreover, the center or crest of the ridge is more reliably identified by a large amplitude negative magnetic anomaly, implying that it is underlain by normally magnetized crust, than by the median valley, which is less continuous.

Once I had seen the Area 4A magnetic survey and made this preliminary assessment of the results, it was a very small step to the formulation of what became known as the Vine–Matthews hypothesis, particularly in view of my prejudices regarding sea floor spreading and continental drift. (The latter, incidentally, amounted to no more than testing a hypothesis, which I had been taught was the essence of the scientific method.) What was now clear, however, was that Drum's decision to devote ship time to such a detailed and time-consuming survey was an inspired one.

I am unable to pinpoint exactly when the idea first came to me. It is clear from the quotations given above that I was quite close to it when writing my literature review in January 1963. It seems probable that it occurred in February or March 1963. Unbeknown to me, at precisely this time, Lawrence Morley, of the Canadian Geological Survey, penned a letter to *Nature* proposing exactly the same idea. He was unable, however, to draw on a survey of a known ridge crest and had to make the case with reference to the linear anomalies mapped in the northeast Pacific, which were not obviously related to a mid-ocean ridge. Morley's paper was rejected by *Nature,* and subsequently by the *Journal of Geophysical Research,* for being too radical and speculative. It was four years before I became aware of this remarkable coincidence and 18 before Morley's 'letter' was reproduced in print in full.[21]

Meanwhile, Drum Matthews and I had decided that however convincing the case might be to those well-versed in the interpretation of magnetic anomalies (which turned out not to be true) there were very few people in this category. In the hope of convincing a wider audience, therefore, we decided that I should undertake some computer-based interpretation before writing up the idea for publication. Thus it was May before I sat down and wrote the first draft of the Vine and Matthews paper; Drum was on his honeymoon at the time. It differed from the published paper in that it did not include the first two paragraphs and the penultimate paragraph (excluding the acknowledgments). It did, however, include more details of the acquisition and reduction of the Area 4A survey. It was reviewed internally by Maurice Hill, Teddy Bullard, and ultimately Drum. I cannot be certain, but I suspect that Hill was very unhappy with it, that Teddy was quite excited about it (recognizing the tremendous implications if it turned out to be correct), and that Drum, caught in the middle, did not know what to think, except perhaps that having a research student was something of a mixed blessing. All agreed that the full details of the acquisition and reduction of the Area 4A survey were inappropriate to a letter to *Nature* and so this section was removed. The problem then was that it became rather long on interpretation and speculation and short on original data.

In order to solve this problem, as I think he in particular saw it, Hill gave us permission to include two long, unpublished magnetic profiles across the crests and flanks of the North Atlantic and northwest Indian Ocean ridges acquired by the group in 1960 and 1962 respectively. The title was changed from "Magnetic Anomalies over the Oceans" to "Magnetic Anomalies over Oceanic Ridges," and the introductory paragraphs were added to set the scene and incorporate the ridge profiles. Knowing now the difficulty that Larry Morley had in getting his article published, this could have been a very significant addition, and I suspect that Maurice Hill had a very shrewd idea as to what would be acceptable to *Nature*. I think that the paper was submitted to *Nature* in late June or early July, probably by Drum or Maurice, for I have no record of it. It appeared in *Nature* for September 7, 1963. By this time the three of us were at sea on the RRS *Discovery* on a four-month expedition in the northwest Indian Ocean, returning to the United Kingdom in December.

A Poor Reception

Initial reaction to the paper was, to say the least, muted. In particular, those most familiar with the interpretation of magnetic anomalies were less than impressed. At the Royal Society Discussion Meeting on Continental Drift held in London on March 19–20, 1964, Vic Vacquier, the only speaker to mention the hypothesis, said that this "attractive mechanism is probably not adequate to account for all the facts of observation. A theory consistent with the facts is still needed to account for the existence of the north-south magnetic lineations in the north-eastern Pacific. Where the East Pacific Rise can actually be seen, no lineated magnetic pattern was found."[22] Manik Talwani, of Lamont, in writing a review of marine geophysics, referred to our idea as "improbable, startling."[23] In papers published in 1965 George Peter and Harry Stewart, of the U.S. Coast and Geodetic Survey, Washington, and Jim Heirtzler and Xavier Le Pichon, at Lamont, failed to invoke or mention the possibility of reversely magnetized crust in modeling oceanic magnetic anomalies.[24] On the other hand, in letters to *Nature,* Harry Hess referred to the "fruitful Vine and Matthews hypothesis" (a pun of which he was quite proud), and George Backus, of the Institute of Geophysics and Planetary Physics, San Diego, who had probably been converted by an enthusiastic exposition from Teddy Bullard, wrote constructively, suggesting a possible test for the hypothesis.[25] Backus pointed out that, if the idea is correct, the magnetic anomalies measured along east-west profiles across the South Atlantic Ocean should increase in width to the south, reflecting the fact that the separation of South America and Africa becomes greater as one moves south.

For me, 1964 was a fallow year as far as the hypothesis was concerned, and I concentrated on producing more substantial, or at least conventionally acceptable, material for my thesis, as well as helping to run a Scout troop. The most significant thing I did was to get married, in March, and probably the next most significant thing was to write to Harry Hess at Princeton, toward the end of the year, to ask him if there was any possibility of finding me a job there once I had completed my Ph.D., hopefully by September 1965. I had heard that Hess was to spend a sabbatical at Madingley Rise during the early part of 1965, and I did not want to confront him face to face with this question, but to give him time to think about it. I did not receive a written reply, which was unsurprising, but it meant that I was on tenterhooks when his arrival was imminent. Much to my delight, on his arrival the first thing he said to me was that he thought that our idea was great, and the second thing he said was that he thought he would be able to find a position for me at Princeton.

The Juan de Fuca Ridge

Tuzo Wilson, from the University of Toronto, was also on sabbatical at Madingley Rise during the early part of 1965, and it was during this time that he formulated the concept of transform faults.[26] In applying this idea to the worldwide system of ridges and trenches, he eventually arrived at the Gulf of California and recognized that the San Andreas must be a major transform fault system. Farther to the north, the Queen Charlotte Islands strike-slip fault system appeared to be another transform fault terminating in ('transforming' into) the Aleutian trench at its northern end. However, off the states of Washington and Oregon there is a gap and offset in the seismicity associated with these two fault systems. The logic of Wilson's hypothesis predicted that there should be a short length of ridge between the two faults in this area which he named the Juan de Fuca Ridge after the Strait of Juan de Fuca, which forms the boundary between the United States and Canada along this coastline. Tuzo was explaining this to Harry Hess and me when Harry suddenly interrupted him and said, "If you want to put a ridge there, that is one of the few oceanic areas for which there is a detailed magnetic survey, and if Fred is right, there should be a clear expression of the ridge in that survey." I dashed upstairs to the library to look at the volume of the *Bulletin of the Geological Society of America* containing the relevant article by Raff and Mason.[27]

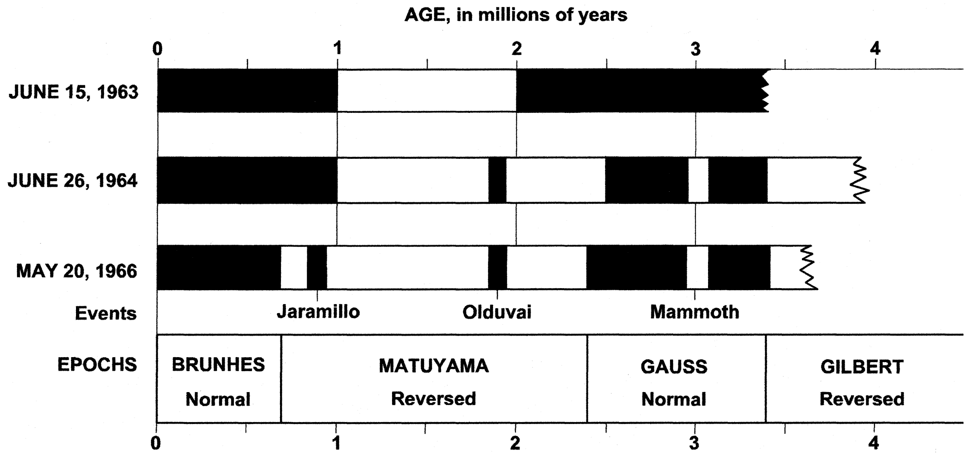
I cannot remember whether I took a quick look at the summary map before rushing back to Tuzo's office and setting it before Tuzo and Harry. All three of us stared at it in amazement. Not only were there linear magnetic anomalies paralleling the trend of Tuzo's putative ridge, but there was also a symmetry to the pattern of anomalies about the ridge crest. Despite the fact that this diagram had been in the literature for four years, no one it seems had noticed this symmetry. The irony of the discovery of the Juan de Fuca Ridge, or rather its non-discovery at an earlier date, is that because the survey was undertaken for military purposes during the Cold War – detailed maps of the bathymetry and gravity field were required for the nuclear submarine deterrent – only the full details of the magnetic data were declassified. Although the topographic expression of the Juan de Fuca Ridge is obscured by sediment from the Columbia River fan, which spills over it and infills much of the topography very rapidly, there is still enough on a detailed survey to reveal the location of the ridge. Had the detailed bathymetry been released at the same time as the magnetics, this would almost certainly be a very different story. In fact, Bill Menard, of the Scripps Institution of Oceanography, San Diego, was aware of the Juan de Fuca Ridge and the shorter Gorda Ridge to the south of it, having discovered the latter in 1952, and having seen the classified data. He was quite convinced however, that these ridges were not equivalent to mid-ocean ridges.[28]

Tuzo was planning to write up his discovery of the Juan de Fuca Ridge as an actively spreading ridge as an article for the journal *Science.* He proposed that he and I should write a second paper for *Science* on the interpretation of the magnetic anomalies over the ridge. He also persuaded me (and presumably Harry as well) that we should present these two papers at the annual meeting of the Geological Society of America to be held in Kansas City in November. My work schedule for the coming few months was now clear, if a little ambitious: to write the paper for *Science,* to complete and be examined on my thesis, and to move to Princeton. Progress on the *Science* article was slow, mainly but not entirely because of the competing demands of the thesis, and Tuzo became a little impatient because he was keen to submit the two papers together. The paper was finally finished in June, the thesis in August, and on September 16 my wife and I set sail from Southampton for New York on the *United States.*

Although the symmetry of the magnetic anomalies about the crest of the Juan de Fuca Ridge provided stunning support for the idea that they might result from a combination of sea floor spreading and reversals of the earth's magnetic field, there was a problem with the more detailed interpretation. If one assumed the reversal timescale for the past few million years as defined then by Allan Cox, Richard Doell, and Brent Dalrymple, of the U.S. Geological Survey, Menlo Park, the pattern of anomalies implied major changes in spreading rate with time.[29] Although inelegant, and counterintuitive if spreading was related to large-scale convection in the mantle, within this pre-plate tectonic paradigm it was not at all clear whether one would expect spreading to be at a uniform rate or somewhat erratic. Consideration of this and of the ambiguity in the thickness of the source region for the anomalies meant that writing the *Science* paper on the magnetic anomalies was not as straightforward as one would have wished. The two papers appeared in *Science* on October 22, 1965, just a few weeks before the meeting of the Geological Society of America in Kansas City.[30]

The Jaramillo Event

Tuzo was right, of course. The meeting in Kansas City provided me with an excellent opportunity to publicize my ideas and to meet North American geologists. One such meeting was particularly memorable and significant; indeed, it provided me with the last piece of the jigsaw puzzle and enabled me to make a convincing and essentially unarguable case for the validity of the Vine–Matthews hypothesis. It was with Brent Dalrymple, from Menlo Park, who told me that they were increasingly confident that they had discovered an additional detail of the reversal timescale at around 0.9 million years before present. It was a period of normal polarity, of perhaps 100,000 years' duration, and they had named it the *Jaramillo event.* I realized at once, having pored over the problem for so long and so recently, that with this revised timescale it would be possible to interpret the Juan de Fuca anomaly sequence with an essentially constant rate of spreading. To me, at that instant, it was all over, bar the shouting. From the situation less than a year earlier, when I thought that I might spend my whole career trying to convince people of the validity of our idea, I could now make a compelling case that the sea floor not only



Successive refinements of the geomagnetic reversal timescale for the past 3.5 million years, obtained by Cox, Doell, and Dalrymple, of the U.S. Geological Survey, Menlo Park, during the 1963–1966 period. The dates are publication dates (Cox, A., R. R. Doell, and G. B. Dalrymple, 1963. Geomagnetic polarity epochs and Pleistocene geochronology. *Nature* 198: 1049–1051; Cox, Doell, and Dalrymple, 1964, note 29; Doell and Dalrymple, 1966, note 4.)

spreads symmetrically about mid-ocean ridges but at an essentially constant rate, and in doing so, it faithfully records the timescale of reversals of the earth's magnetic field. At once, the possibility of documenting the evolution of the present-day ocean basins and the geomagnetic reversal timescale for the past 150 million to 200 million years opened up.

Independent Confirmation

In February 1966 I visited the Lamont Geological Observatory (as it was then called), at the invitation of Neil Opdyke. Neil had moved there in 1963 to set up a paleomagnetic laboratory, and was housed in the same suite of offices as Jim Heirtzler and his Magnetics Department, whose work included the study of marine magnetic anomalies. When I arrived, Neil was working on a diagram on a light table. It transpired that it was the diagram that would become 'figure 1' of his paper on the paleomagnetism of Antarctic deep-sea cores that was published in *Science* later that year.[31] "Look at this Fred," he said. "Not only have we found the complete Cox, Doell, and Dalrymple timescale in these cores, but we have also discovered an additional, normal event at around 0.9 million years. We have called it the Emperor event." "Well, I am sorry to have to disappoint you," I said, "but the Menlo Park group have already resolved such an event and named it the Jaramillo." Neil's jaw dropped. On the wall of the same room were pinned up the now famous *Eltanin* magnetic and bathymetric profiles across the Pacific-Antarctic Ridge that his Lamont colleague Walter Pitman had recently reduced. By this time Walt had joined us. "Furthermore," I said, "one can see it on these magnetic profiles just as you can on the Juan de Fuca Ridge." Walt's jaw dropped.

With the addition of the work on deep-sea cores we now had three independent records of the geomagnetic reversal timescale for the past 3.5 million years, and I could, with some confidence, set about reviewing the status of the Vine–Matthews hypothesis in the light of new data, new ideas, and the revised reversal timescale. Either during this meeting at Lamont, or soon after, Jim Heirtzler let me have copies of the *Eltanin*-19 profile and details of the aeromagnetic survey of the Reykjanes Ridge south of Iceland, which was the second extensive survey to demonstrate the symmetry of the magnetic anomalies about the ridge crest. Presumably this had already been prepared for publication, in that it appeared in print a few months later in the journal *Deep Sea Research*.[32]

In the months that followed my memorable meeting with Neil, Walter, and Jim, I prepared my review article and made a further visit to Lamont to give a talk. In May or June, Walter and Jim visited Princeton and were surprised, I think, to discover that my review article was rather wide-ranging and essentially complete. Inevitably, it drew heavily upon and reproduced a number of magnetic surveys and profiles acquired by others. I was anxious that I should not only have their permission to do this but also that the work should be published. This was true but for the notable exception of the *Eltanin*-19 profile. Should I withdraw it from the paper or wait until it was published? Jim very generously suggested that we should try to arrange to publish simultaneously in *Science*, and I was happy to agree to this.

The paper on the *Eltanin* profiles by Jim Heirtzler and Walter Pitman was published two weeks ahead of mine, on December 2, 1966; this struck me as entirely reasonable.[33] There were many rumors circulating at the time regarding the lobbying and discussions that were going on in relation to these two papers. The only hard evidence I ever had of this, apart from the delay in publication, was a comment from Harry Hess, who was a good friend of Phil Abelson, the editor of *Science* at the time. Abelson had asked Harry whether he thought that my paper was worth publishing; apparently he felt that *Science* was carrying too many earth science articles at the time.

At the meetings held in New York and San Francisco in November

1966, at which I presented the content of this paper, Lynn Sykes, of Lamont, gave presentations of his recent work on focal mechanism solutions for earthquakes associated with mid-ocean ridge crests.[34] This confirmed the validity of Tuzo Wilson's transform fault hypothesis, and provided further, and entirely independent, evidence for the reality of sea floor spreading. Together with the symmetry of the magnetic anomalies about ridge crests and their correlation with the geomagnetic reversal timescale, this result finally convinced most earth scientists of the validity of the hypothesis of sea floor spreading, and hence of continental drift.

Postscript

In early 1963, when Drum Matthews and I first discussed the possibility of combining sea floor spreading with reversals of the earth's magnetic field to explain oceanic magnetic anomalies, we could not have dreamed that the idea would be spectacularly confirmed within less than three years. The magnetic data that we were working with showed no symmetry or regularity, except for a large, central anomaly over mid-ocean ridge crests, and the earth's magnetic field, if it had reversed at all, was thought to reverse at a fixed interval (possibly between a half to one million years). Taken together with the variable width of the stripes of the northeast Pacific, reversals with a fixed periodicity implied that the spreading rate must be very irregular. Ultimately, of course, it transpired that it is the time between reversals that is very irregular and that the rate of spreading has been remarkably constant for millions of years. The first problem, of the complexity of the magnetic profiles, was more serious, although as geologists we did not find it surprising. It implied that new crust is formed by a process of intrusion and extrusion of basaltic material, and by faulting, over a zone perhaps a few tens of kilometers in width. This seemed very likely by analogy with the central zone of active volcanism and faulting on Iceland, which lies astride the mid-Atlantic ridge. What we did not realize at the time was that spreading rates vary greatly, by a factor of ten, around the mid-ocean ridge system, and that the complexity of the crustal structure decreases with increased spreading rate, presumably as the zone of formation gets progressively narrower. The spreading rates in the North Atlantic and the northwest Indian Oceans are relatively slow, whereas those on the East Pacific Rise are three to five times higher, and that for the Juan de Fuca Ridge is twice that in the North Atlantic. Thus the Pacific ridges behave more like a tape recorder than we could ever have imagined. Indeed, it has been suggested that fast-spreading crust not only preserves a record of the reversals of the earth's magnetic field but, in addition, information on changes in the intensity of the field with time.

Within ten years of the confirmation of the Vine–Matthews–Morley (VMM) hypothesis, the same sequence of magnetic anomalies, reflecting the history of reversals of the earth's magnetic field during the past 160 million years, had been recognized in all the major ocean basins. By rewinding the tape recorder it was possible to determine the relative positions of the continents, and the sequence of continental drift, throughout this period of time. My one regret, as a geologist, was that this detailed record, written within the 60 percent of the earth's surface covered by oceanic crust, is only available for 4 percent of geologic time. Earlier phases of continental drift would have to be deduced from the more complex and fragmentary geological record within the 40 percent of the earth's surface covered by continental crust.

A surprising aspect of the widespread acceptance of the VMM hypothesis in 1966 was the fact that there was no direct evidence that the magnetic stripes are underlain by bands of normally and reversely magnetized crust. It was only inferred, there being no oriented samples from the volcanic rocks of the ocean floor. It was many years before there was evidence for this: initially from measurements of the magnetic field made very close to the sea floor, and ultimately from the recovery of oriented drill cores. In a similar vein, it was 20 years before it became possible to confirm the rates of spreading deduced in 1966 by an independent technique. By the mid-1980s, it was possible to determine the change in the distance between two points within the interiors of different plates, by making repeat measurements over several years using the satellite laser ranging technique.

Still outstanding is the nature and vertical extent of the magnetic crust that gives rise to the anomalies. The basaltic layer, which is typically less than one kilometer (0.6 mile) thick, is strongly magnetized when first formed, but its magnetization decays with time. It would appear that a lower crustal layer also preserves the magnetic record, and that this magnetization is more stable with time. Its contribution to the magnetic anomalies probably becomes more significant as the crust gets older, but the precise nature, geometry, and thickness of this layer are still not fully understood.

The contribution that I was able to make to this subject, during the 1963–1966 period, was a classic example of being "in the right place at the right time." I was lucky. I think I could claim that, to some extent, I maneuvered myself into an area that struck me as being fertile ground for a possible breakthrough. There is little doubt, however, that the intellectual environment in the Department of Geodesy and Geophysics at Cambridge at the time was an ideal spawning ground for such an idea. In the 1950s, much of the early paleomagnetic work that provided support for the theory of continental drift was carried out there. Teddy Bullard had worked on the origin and nature of the earth's magnetic field. Bullard and Maurice Hill were working in marine geology and geophysics, and Drum Matthews was one of very few people who had measured the magnetic properties of basaltic rocks dredged from the ocean floor. I also had the advantage of having heard talks by Patrick Blackett and Harry Hess on continental drift and sea floor spreading. Basically, there was very little left for me to do.