Part II

The Early Work: From Paleomagnetism to Sea Floor Spreading

When World War II broke out, arguments about crustal mobility were put on hold as earth scientists applied their special knowledge and skills to surf forecasting, submarine navigation, anti-submarine warfare, and other pressing issues of the day. Afterward, a group of British geophysicists who had worked on magnetism and warfare (mine-sweeping and demagnetizing ships) turned their attention to rock magnetism. Initially, they hoped to answer questions about the origins of the earth's magnetic field. But they discovered something else entirely: rocks on land recorded evidence that the position of the land masses relative to the earth's poles had changed over the course of geological time. Some of them began to think again about continental drift. Yet these data did not immediately cause a stampede, for they were new and uncertain, and people doubted their reliability.

Meanwhile, American scientists had been measuring the magnetism of rocks on the sea floor, partly out of curiosity, partly because the U.S. Navy hoped these measurements might suggest new means to hide or detect submarines. The result surprised everyone: a distinctive pattern in which some rocks were magnetized in concert with the earth's current field and some in opposition to it. When plotted on paper in black and white, the pattern looked like zebra stripes. Scientists wondered what these magnetic stripes meant, and no one at first connected the pattern to continental drift. Then, another group of scientists proved that over the course of geological history the earth's magnetic field had reversed its polarity many times. Suddenly the meaning of the stripes became clear: the sea floor was splitting apart, or "spreading," and new volcanic rocks were magnetized in alignment with the earth's field each time they erupted on the sea floor. Once the idea was in place, it took only a few years to demonstrate that it was right.

CHAPTER 2

STRIPES ON THE SEA FLOOR

Ron Mason

IN 1955, THROUGH A FORTUNATE SEQUENCE OF EVENTS, THE SCRIPPS Institution of Oceanography was in a position to make an accurately positioned survey of the earth's magnetic field covering a significant area of the northeast Pacific. This was the first survey of its kind, and it was to have a quite unexpected outcome: the discovery of a linear pattern of magnetism in the rocks of the sea floor not previously seen anywhere else. These linear magnetic patterns later came to be called sea floor magnetic "stripes" (because that's what they looked like when plotted on a map) and they pointed to apparent movements of the ocean floor in excess of 600 miles (1,000 kilometers). The lineations themselves became the first step in what eventually became a new global theory of the earth: plate tectonics.

It all started in 1952, but my interest in geophysics goes back to my student days. On completing my undergraduate course in physics at Imperial College, London, in the immediate prewar years, I was looking for an alternative to spending my working life in a laboratory when I discovered geophysics. It appealed to me as a developing, outward-looking subject with various interesting opportunities, so I opted to take the master's course in geophysics at Imperial. But before I could settle down to a steady career I found myself drawn into the war effort, where I gained experience that was to prove invaluable in later life. When the war ended I returned to Imperial as a lecturer in geophysics. And that is when life started to become interesting.

In 1951 I took a year's sabbatical, which I spent at the California Institute of Technology (Caltech). While there, in the spring of 1952, I attended the annual meeting of the University of California Institute of Geophysics, held that year in La Jolla. The location of the meeting, right by the ocean, and the several presentations on marine seismology, a branch of geophysics new to me, set me thinking about other geophysical

Ron Mason with piston core sample of sediments from the sea floor, on the *Spencer F. Baird,* 1954. (Photograph courtesy of the Scripps Institution of Oceanography, used by permission of the University of California.)

techniques that had been or might be used for studying the oceanic crust. Apart from seismology, very little seemed to have been done. Some important gravity work had been undertaken using instruments installed in submarines, thus avoiding the large ups and downs of surface ships, which would swamp the small gravity variations expected of sea floor structures, but I was unaware of any attempt to exploit the earth's magnetic field, other than Project Magnet.

Project Magnet was a joint effort of several bodies, including the Office of Naval Research (ONR), the U.S. Geological Survey (USGS), and the Naval Ordnance Laboratory (NOL).[1] The main purpose of its initial phase was to map the magnetic anomalies associated with volcanoes and other structures in the Aleutians, and with two atolls in the Marshall Islands, Bikini and Kwajalein, using a magnetometer installed in an aircraft. It was the first serious attempt to study the magnetic anomalies arising from oceanic structures. Talking casually to Scripps' seismologist Russ Raitt during the morning coffee break, I asked whether anyone had thought of investigating the magnetic anomalies associated with sea floor structures by towing a magnetometer behind a ship, an operation that could enable ships to obtain valuable data while engaged in other operations. "What's that?" came a deep voice from behind me. Roger

Revelle, director of Scripps, had overheard the conversation. After the briefest of explanation, Roger, in his characteristically direct way, asked, “Well, do *you* want to do it?” to which I promptly replied, “Yes,” and I became Scripps’ magnetometer man.

My first task was to look for a suitable magnetometer. While trawling the United States, I discovered that the Lamont Geological Observatory (now the Lamont-Doherty Earth Observatory) had towed a magnetometer across the Atlantic four years earlier. This fact was not known at Scripps; the results were not to be published until a year later.[2] However, my visit to Lamont had one favorable outcome: Lamont offered to loan us their magnetometer for Scripps’ upcoming *Capricorn* expedition to the southwest Pacific (September 1952–February 1953). This presented a great opportunity for us to familiarize ourselves with the problems associated with operating a ship-towed magnetometer, and we gratefully accepted. After a scramble to get it to the west coast in time, we towed it successfully over more than 8,000 miles (12,500 kilometers) of ship’s tracks, during which we recorded magnetic anomalies associated with seamounts, atolls, scarps, and other features of the sea floor. Although the results had limited quantitative value, it was clear that there was a future in ship-towed magnetometry. We just *had* to acquire a magnetometer of our own.

THE SCRIPPS MAGNETOMETER

The heart of the Lamont magnetometer was a military ASQ-3A magnetic airborne detector (MAD), originally designed for installation in aircraft for the detection of enemy submarines. It was an instrument known as a fluxgate magnetometer, in which the measuring element was mounted in a gimbals mechanism and automatically maintained in the direction of the earth’s magnetic field. It therefore measured the strength of the field without being adversely affected by the motion of the aircraft. Modified by NOL for geophysical investigations (and used in Project Magnet), it was further modified by Lamont, where the fluxgate unit was installed in a streamlined “fish,” a container for towing it behind a ship.

From our experience on *Capricorn,* we felt that we could do no better than to base our magnetometer on the Lamont instrument. A particular advantage of doing this was that its highly developed electronic and mechanical components were immediately available as surplus from the military. However, although the Lamont instrument was quite adequate for making qualitative surveys of sea floor structures, its system was prone to unpredictable drift, which made it unsuitable for exacting geophysical tasks. We therefore implemented a development program aimed at producing an instrument with a more accurate and stable measuring system, for which my wartime experience stood me in good stead. We also experimented with the design of the fish and its towing arrangements, and with the length of the tow cable, so that the fish would ride as smoothly as possible, minimizing magnetic "noise" caused by erratic movements.

Through the generosity of Varian Associates in Palo Alto, we were able to study the short-term and long-term stabilities of our final instrument by comparing it with their newly developed proton-precession magnetometer, an instrument based on atomic principles. By contrast with the fluxgate instrument, which measures relative values of the magnetic field and needs to be calibrated, the proton magnetometer is an absolute instrument, whose output gives the true value of the magnetic field. The comparisons showed that our instrument was highly stable over periods of a few hours and had a steady long-term drift that could easily be corrected.

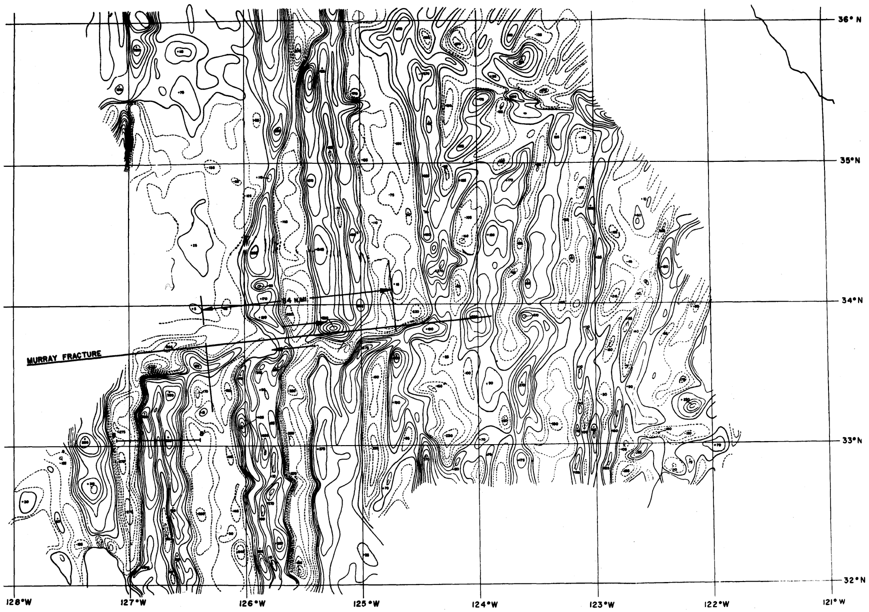
THE *PIONEER* SURVEY

Early in 1955 I learned from Scripps' marine geologist Bill Menard that the U.S. Coast and Geodetic Survey (USCGS) ship *Pioneer* was about to commence a detailed bathymetric survey off the west coast of the United States. The object was to produce a map of the sea floor topography by recording the depth along a grid of long parallel east-west lines about 5 miles (8 kilometers) apart, using a continuously operating echo sounder. Joining points of equal depth on adjacent lines would enable surveyors to construct a contour map of the sea floor. The area to be surveyed extended from the foot of the continental shelf outward for between 250 and 300 miles (400 and 500 kilometers), and from the Mexican border in the south to the southern end of Queen Charlotte Islands in the north. A radio navigation system with fixed beacons ashore would enable the position of the ship's tracks to be accurately determined. The probable error of a position would vary with time of day and with distance from the beacons, but was expected to be on the order of 300 feet (100 meters). The survey would occupy the best part of two years.

Scripps immediately sought permission from the U.S. Navy Hydrographic Office, the sponsors of the survey, to tow its magnetometer behind the *Pioneer*. Unfortunately, this was not immediately forthcoming, because the Hydrographic Office was concerned that towing the fish and handling it overboard might slow down their operation! So we missed the first few monthly cruises. Eventually, through the persuasive efforts of Roger Revelle, these fears were allayed, and my assistant Art

Raff and I joined the ship in August 1955. This was to be the first of 12 monthly cruises, the last of which took place in October 1956. By this time we had covered an area extending from 32°N to 52°N, a distance of more than 1,250 miles (2,000 kilometers).

The August 1955 cruise was in the nature of a trial run. We had no idea what to expect. Our hope was that we would be able to produce a meaningful contour map of the magnetic field by following the procedure used in making the bathymetric map, that is, by joining points of equal field value on adjacent tracks. There were a number of uncertainties: would the magnetometer prove sufficiently stable during the rigors of several weeks at sea, and would the spacing of the ship's tracks prove sufficiently close to enable us to contour the results? The first was answered by our calibrations of the magnetometer at Palo Alto, both before and after the cruise. These showed that the magnetometer was more than adequately stable. They also enabled us to adjust our readings so that they represented the true value of the earth's magnetic field. As it turned out, the spacing of the ship's east-west tracks was not a problem in plotting the results, because the dominant trend of the contours was north-south.



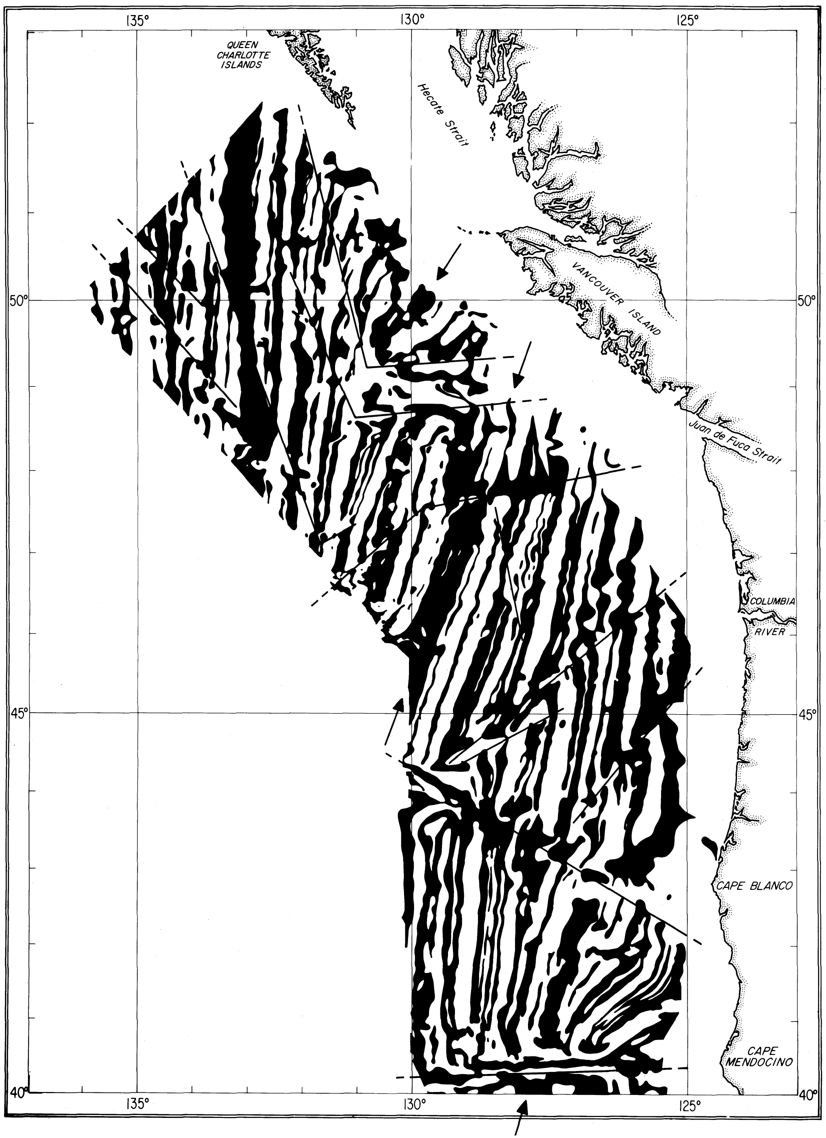
Initial results of the *Pioneer* survey of rock magnetism off the west coast of North America, after three months' operation. The distinctly linear pattern in the southwest corner of the map area, where we started, persuaded us to continue with the survey. (From Mason, 1958, Figure 2.)

Fortunately, the positions of the ship's tracks were made available to us in real time, so we were able to plot the results on a chart and build up a picture of the field as we went. The initial results were discouraging; they were apparently so erratic as to make it virtually impossible to contour them. At this stage we might easily have abandoned the whole operation. But as the data accumulated, the nature of the field began to emerge: it was dominated by bands of approximately north-south trending contours that extended the full 100 miles (160 kilometers) north-south extent of the August cruise.[3] This was a period of great excitement, because nothing like it had ever been observed before, on land or at sea. There was no longer any question of abandoning the survey.

After a further 11 monthly cruises, the survey was completed in October 1956, and the rather tedious task of plotting the results was completed in the first half of 1957. The outcome was a map dominated by contours trending mainly between north-south and northeast-southwest. However, before the results could be properly appraised it was necessary to separate out the geologically related magnetic anomalies from the earth's background field. We did this by overlaying our map on the map of the earth's magnetic field published by the Hydrographic Office, and subtracting the one from the other graphically. This rather tedious procedure greatly simplified the original map.

The final map showed that the bands of dominantly north-south contours bounded strips of positive or negative anomaly.[4] These lineated anomalies, up to one percent of the earth's background field in amplitude and a few tens of miles in width, covered most of the 1,250 miles (2,000 kilometers) north-south extent of the survey, with interruptions in places and some changes of direction. In particular, it was interrupted as it crossed two of the great east-west faults of the northeast Pacific, the Mendocino and the Murray, and a previously unrecognized fault, subsequently named the Pioneer Fault. At the Mendocino and Pioneer Faults there appeared to be no relation between the patterns on opposite sides, but at the Murray Fault they could be matched in such a way as to suggest that since its inception the fault had been offset by about 100 miles (160 kilometers) in a right-lateral sense (that is, crossing the fault, the pattern on the far side would be offset to the right). The absence of a match across the two other faults raised an intriguing question: could it be that displacements across them were so great as to exceed the width of the map? This was a matter that could only be settled by extending the survey in their neighborhoods, but this would have to wait for another day.

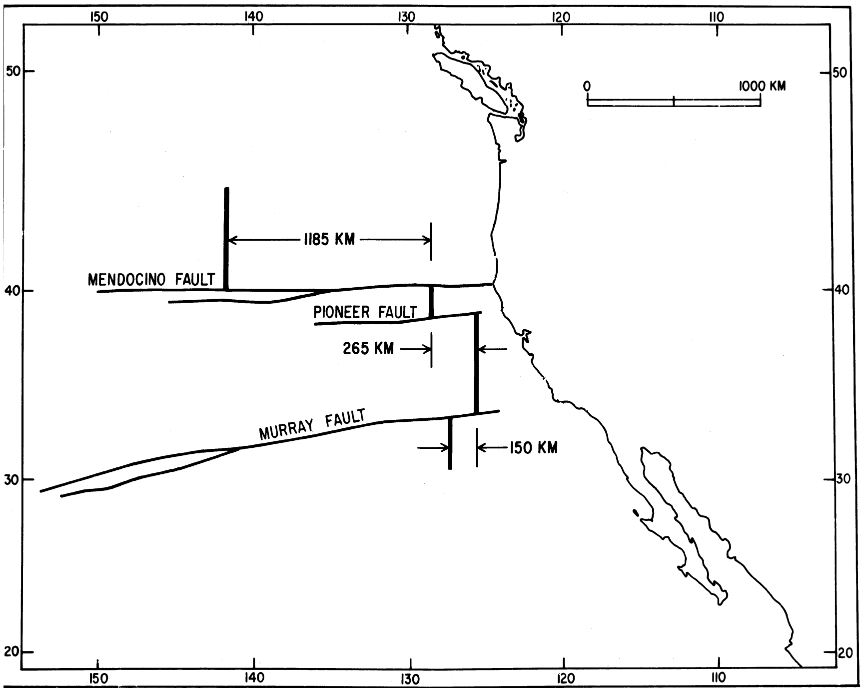
Late in 1956 I was diverted to head an International Geophysical Year (IGY) project in the equatorial Pacific. This was one of Scripps' contributions to the international program. It involved the setting up and oper-



Summary map of the magnetic pattern in the area north of the Mendocino Fault, off the northwest coast of North America. The areas of positive magnetic anomaly are shown in black. (From Raff and Mason, 1961, Figure 1, reproduced courtesy of the Geological Society of America).

ation of temporary magnetic observatories on several islands spanning the magnetic equator. Its purpose was to study certain natural phenomena arising in the ionosphere, and we were certainly not prepared for the spectacular effects of the British nuclear tests on nearby Christmas Island, reflected in our magnetic records.[5] To take care of the logistics of this operation Scripps chartered a schooner, with its owner, University of Hawaii mathematician Martin Vitousek, while I took care of the science and provided a link between Scripps and the field operation. This kept me away from Scripps for much of the time, and Art Raff took care of the remainder of the *Pioneer*'s 1956 field season and worked up the 1956 data. In the meantime, Vic Vacquier came to Scripps to develop its ship-towed magnetometry program.

During the following two years Vic's priority was to extend the survey, which he did by running east-west lines, more than 1,250 miles (2,000 kilometers) long on both sides of all three faults, using Scripps' research ships.[6] These were sufficiently long that in all cases he was able to obtain matches between the patterns on opposite sides. On the Mendocino and Pioneer Faults the patterns were displaced in a left-lateral sense by 710 and 160 miles (1,140 and 260 kilometers) respectively, and on the Murray Fault the displacements ranged between 95 and 425 miles (150 and 680 kilometers) in a right-lateral sense.[7] At the time, these results were taken to imply transcurrent (strike-slip) displacements of the ocean floor of the same order as the largest of those observed on the conti-



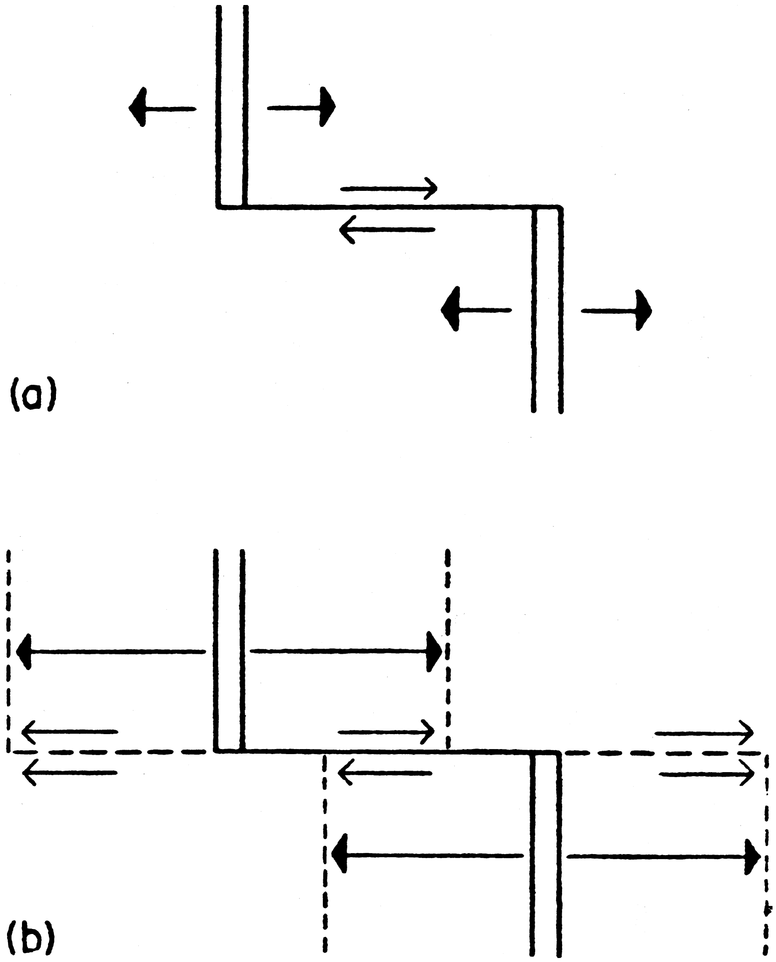
Summary of Victor Vacquier's lateral displacements across the Mendocino, Pioneer, and Murray Faults. (After Menard, 1960, Figure 6.)

nents. This was a quite unexpected discovery; that such large movements of blocks of the oceanic crust could take place with so little distortion of the magnetic pattern at their margins was taken as evidence for the rigidity of the upper part of the oceanic crust. However, Tuzo Wilson of the University of Toronto was shortly to publish his ideas about a new class of oceanic faults connecting offsets of ocean ridges, to which he gave the name *transform faults*.[8] This would throw doubt on the reality of our proposed displacements.

Wilson's idea invoked the concept of sea floor spreading proposed by Harry Hess and Bob Dietz: that new crust is formed by magmatic intrusion along the crests of mid-ocean ridges, and then drifts steadily away from those crests.[9] Ocean ridges typically suffer numerous lateral offsets, and most people assumed the two segments were initially aligned, and were displaced to their present positions by movement along transcurrent (strike-slip) faults. Wilson proposed that, on the contrary, the offsets were primary features, having always been in their present positions. As a consequence, not only would relative displacements between opposite sides of the fault be confined to that section between the two ridge segments, but the two sides would move in opposite directions away from their respective ridges; the patterns on opposite sides would be the reverse of one another, and no simple match would be possible. Confirmation of Wilson's transform faults came from the work of Lamont's Lynn Sykes. Studying the first motions of earthquakes occurring on offsets of the mid-Atlantic ridge, he showed that movements on their two sides were in the directions predicted by Wilson.[10] Outside of this ridge-to-ridge section, no relative movement between the two sides takes place, but the fault trace continues, because the spreading process has brought crusts of different ages and characters into contact with one another. Hence, corresponding features on the two sides are displaced by an amount equal to the offset of the ridge. If the faults studied by Vacquier were transform faults, related to segments of the East Pacific Rise now buried under the North American continent, then no relative displacement between opposite sides would have occurred. Instead of the hundreds or thousands of miles of offset, there might not be any.

THE MAGNETIC LINEATIONS

The cause of the magnetic lineations – or stripes – led to much speculation. I was able to show that they could be explained by shallow slablike structures, immediately underlying the positive stripes and more highly

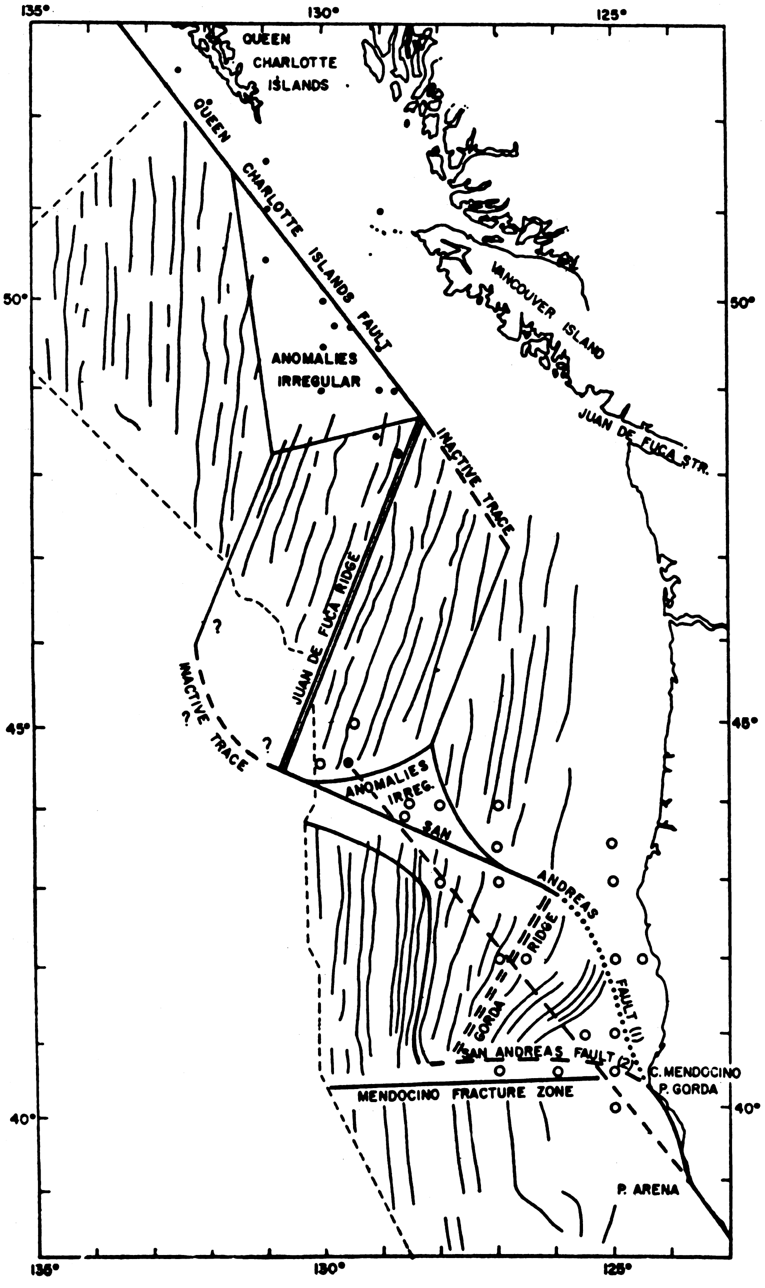


Evolution of a transform fault. (a) Two expanding ridges, connected by a transform fault. (b) The same after a lapse of time. The offset of the ridge has not changed, and movement across the fault is confined to the ridge-ridge section. (After Wilson, 1965, Figure 1.)

magnetized than the surrounding crust, but there was no plausible geological model to support such structures. The situation remained unexplained for five years, until Fred Vine and Drummond Matthews, studying two comparatively isolated volcano-like submarine structures in the northwest Indian Ocean, observed that whereas one appeared to be magnetized in the present direction of the earth's magnetic field (where a compass points toward the north magnetic pole), the other was magnetized in the opposite direction.[11] This, and other observations in their field area, suggested to them that perhaps half the oceanic crust was reversely magnetized. This led them to propose a new model to account for the magnetic patterns observed over mid-ocean ridges, bringing together ideas current at the time about sea floor spreading and periodic reversals of the earth's magnetic field.[12] New crust is formed as a result of magmatic activity along the crests of mid-ocean ridges, and as it cools it acquires permanent magnetization in the prevailing direction of the earth's field. If the earth's field then reverses its polarity, the next batch of crust would be magnetized in the opposite direction. Vine and Matthews suggested that these two processes – sea floor spreading and field reversals – would lead to successive strips of alternately normally and reversely magnetized crust drifting away from the axis of the ridge.

Vine and Matthews' hypothesis offered an elegant explanation of how the magnetic lineations of the northeast Pacific could have come about, although in this case there was no obvious connection with an ocean ridge. I was familiar with ideas about sea floor spreading and reversals of the earth's magnetic field, and I could have kicked myself for not thinking of the idea, particularly because, had I looked more carefully at our map, I would have realized that some of the seamounts might be reversely magnetized, and this might have headed my thoughts in the right direction. I had absolutely no doubt as to the correctness of their hypothesis. But to my surprise the idea was not universally accepted. One reason for doubt was that no detailed magnetic survey spanning an oceanic ridge was thought to be available. Perhaps more important, in 1963 neither sea floor spreading nor geomagnetic field reversals were universally accepted. In that regard, the Vine–Matthews hypothesis was built upon two other hypotheses, about which many people still had doubts.

However, it turned out that support for the Vine–Matthews hypothesis had been staring us in the face. In searching for examples of transform faults in the northern part of the magnetic map, Wilson had identified the Juan de Fuca and Gorda Ridges as short sections of young, active oceanic ridges, connected by a conjectured submarine extension of the San Andreas Fault, which he interpreted as a transform fault.[13] Both ridges were marked by seismic activity and symmetry of the magnetic pattern on opposite sides, as would be predicted by the Vine–Matthews hypothesis, extending in the case of the Juan de Fuca Ridge to 110 miles (175 kilometers) on each side. Shortly afterward, Jim Heirtzler and colleagues at Lamont published the results of an aeromagnetic survey spanning the Reykjanes Ridge, the most northerly part of the mid-Atlantic ridge, south of Iceland.[14] This showed magnetic lineations parallel to the ridge and symmetrical about it, extending to about 100 miles (160 kilometers) on



Sketch map showing the relation of the magnetic pattern and earthquake epicenters (dots and open circles) to the Juan de Fuca and Gorda Ridges. Two possible paths of a submarine extension of the San Andreas Fault are also shown. (After Wilson, 1965, Figure 3.)

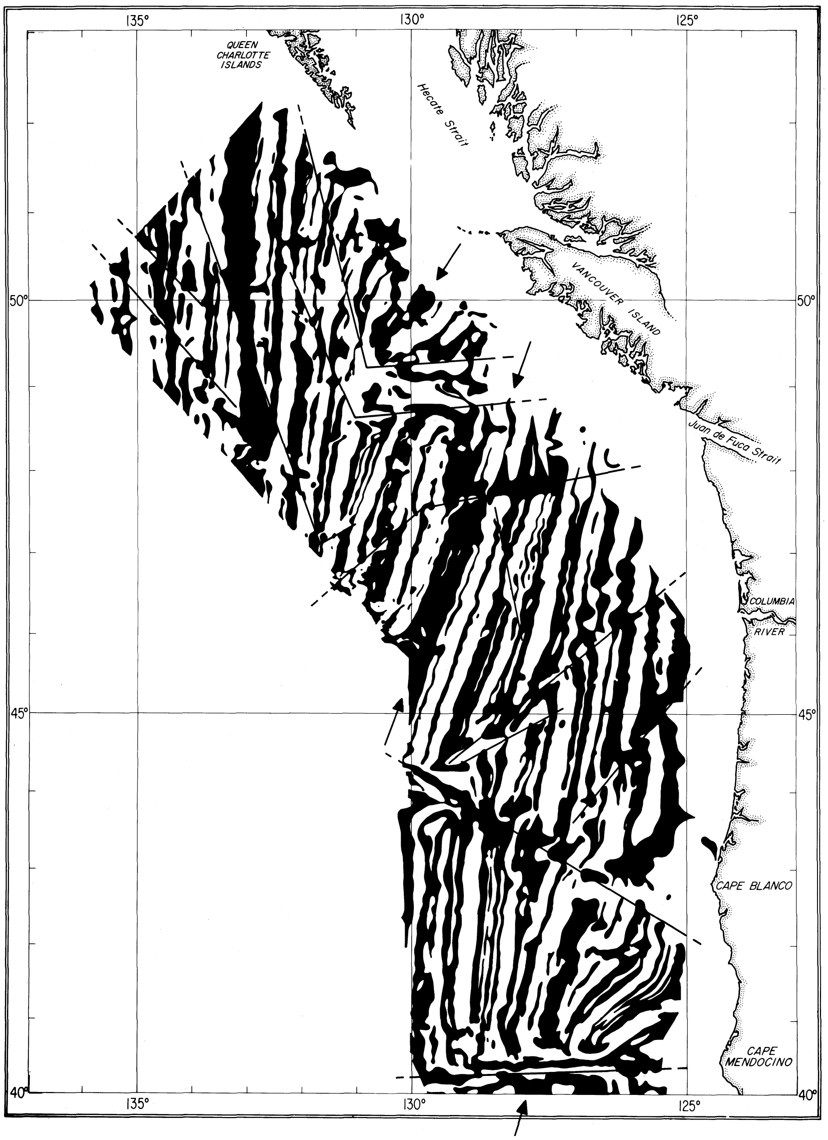
each side. To my mind, these discoveries should have clinched the Vine–Matthews hypothesis. However, doubts were still being expressed about the universal association of magnetic lineations with spreading axes. It was not until 1966 that the Lamont group, who had been among the most influential doubters, finally came around to accepting it.

Magnetic Stripes and the Age of the Sea Floor

One consequence of the Vine–Matthews hypothesis is that it offered the possibility of assigning ages to individual stripes, and hence to the underlying sea floor. The method depends on matching the pattern of successive magnetic stripes to the timescale of reversals of the earth's magnetic field. Because field reversals occur at irregular intervals, it follows that, assuming a constant rate of spreading, the widths of successive stripes will follow a similar pattern; under favorable circumstances the two patterns can be matched, and ages of particular stripes determined. In 1968 Fred Vine did this for the northern part of the magnetic map north of the Mendocino Fault.[15] The timescale he used, covering the past 3.5 million years, is based on measurements of the direction of magnetization of rock samples and determination of their ages using radiometric methods. This timescale could be extended by taking advantage of the fact that the patterns of magnetic stripes about mid-ocean ridges are remarkably similar wherever they occur. From a very long profile of the magnetic field spanning the Pacific-Antarctic Ridge, and assuming a constant rate of sea floor spreading, Pitman and Heirtzler extrapolated the timescale back to 11 million years.[16] As expected, the youngest parts of the Pacific sea floor are the actively spreading Juan de Fuca and Gorda Ridges; the oldest is in the northwest corner of the area, with an age in excess of 10 million years. The process has since been applied to most parts of the ocean floor where magnetic stripes have been identified.

Serendipity and Scientific Discovery

I started the project described here with no thought of its possible relevance to continental drift. The discovery of sea floor magnetic stripes was serendipitous: we were not looking for them, nor could we have been, because no one knew they existed! Now, so long after the event, it is difficult to assess what influence their discovery had on the development of plate tectonics, but it is clear that they helped to set events in motion.



Summary of the magnetic anomalies southwest of Vancouver Island. Areas of positive anomaly, assumed to represent normal magnetization of the underlying crust, are colored to match the radiometrically derived reversal timescale for the past 4 million years, extended to 11 million years on the basis of a long magnetic profile spanning the Pacific-Antarctic Ridge. The straight lines mark geological faults. (From Raff and Mason, 1961, Figure 1, reproduced courtesy of the Geological Society of America.)

The recognition of sea floor magnetic stripes led immediately to the hypothesis of large displacements of the ocean floor which, although later thrown in doubt by Wilson's transform fault hypothesis, stimulated interest in the mobility of the oceanic crust, and it raised interesting questions. The Vine–Matthews hypothesis provided a speculative answer to one of those questions, and the work of Tuzo Wilson, followed shortly by Lamont's aeromagnetic survey of the Reykjanes Ridge, proved it to be correct. For most people these results placed beyond doubt the existence of ocean floor spreading, a factor fundamental to the concept of plate tectonics.

As to my own views on the subject, at no time had I any doubt about the reality of continental drift or the validity of plate tectonics. This is principally because I came to the subject at a time when favorable data were rapidly accumulating and, in contrast to the attitude toward continental drift on the American side of the Atlantic, opposition in the United Kingdom was muted. I had frequent contacts with Teddy Bullard, Keith Runcorn, and other British workers in the field and I cannot recall any outspoken opposition. Bullard had officially sat on the fence for many years, finally coming out in print in support of continental drift in 1964.[17] But, in fact, he had been fully convinced four years earlier by the paleomagnetic data brought together by P. M. S. Blackett and colleagues.[18] In 1960, they had presented their data in such a way that, as he wrote, "it clearly indicated the reality of continental drift."

ACKNOWLEDGMENTS

The sea floor magnetism project owed its existence to the enthusiasm and support of Roger Revelle, then director of the Scripps Institution of Oceanography. Various members of Scripps, in particular Jim Snodgrass and Jeff Frautschy, made valuable contributions to the design and construction of the magnetometer's hardware. Art Raff was a tower of strength throughout.

We owe our thanks to Captain Pearce and the officers and men of the *Pioneer* for putting up with us, to the numerous students and others who maintained shipboard watches, sometimes under difficult conditions, and to Martin Packard of Varian Associates for making calibration facilities available between cruises. The work was supported by the U.S. Office of Naval Research and the U.S. Bureau of Ships.