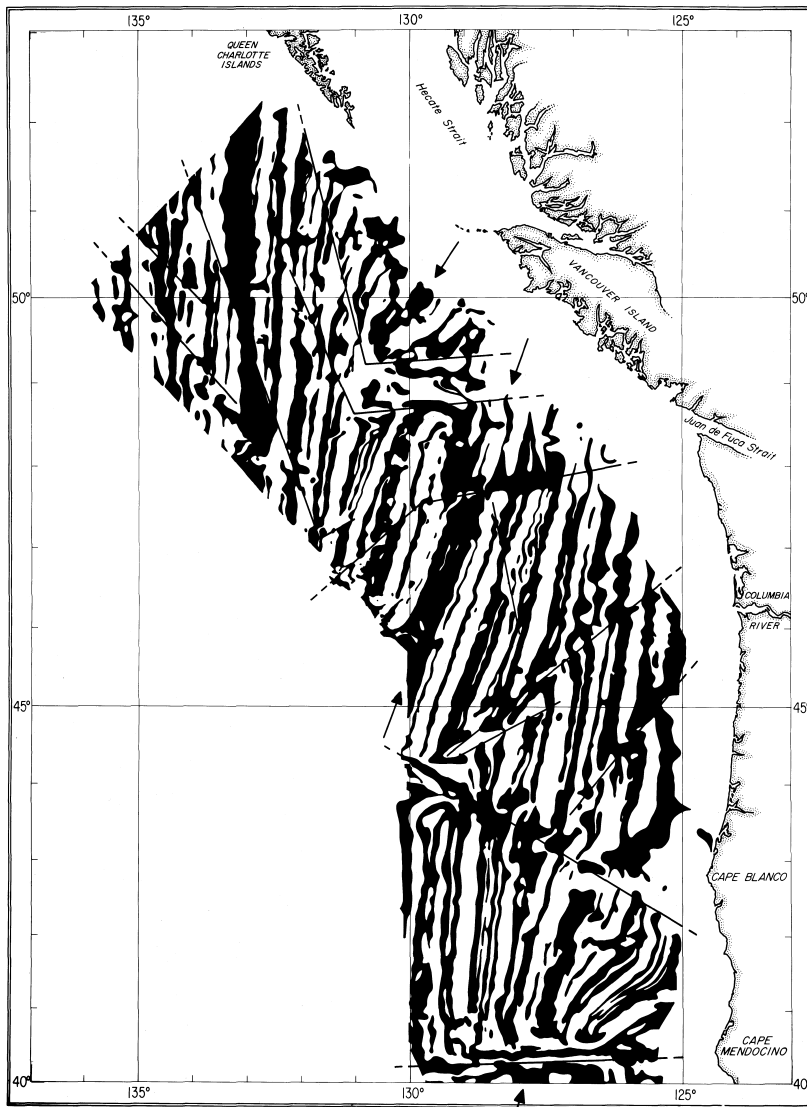


CHAPTER 4

THE ZEBRA PATTERN

Lawrence W. Morley



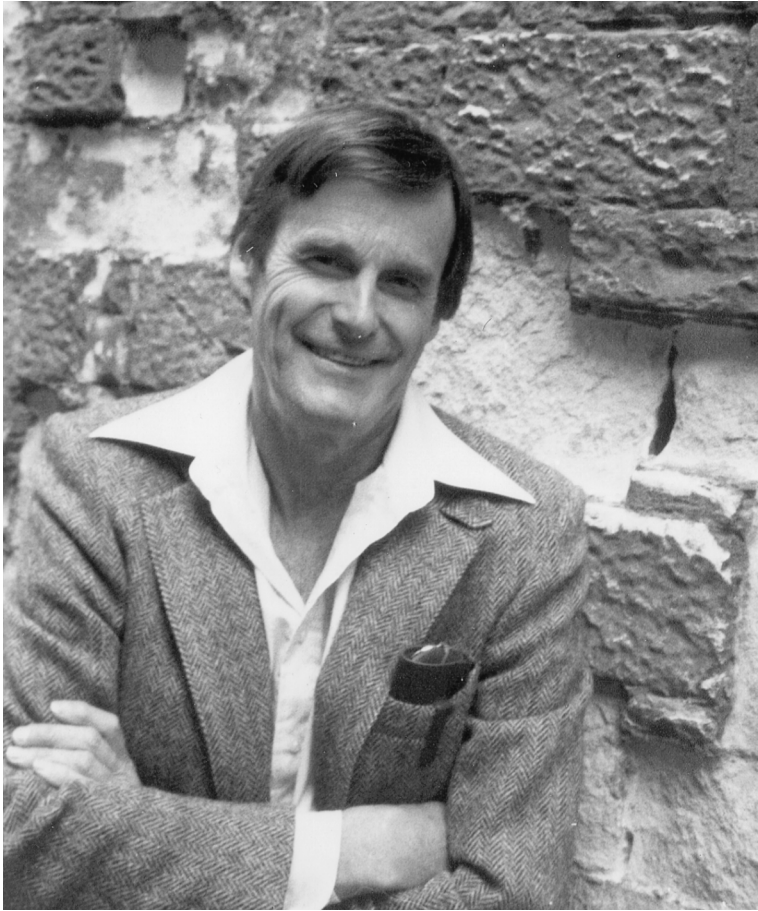
(From Raff and Mason, 1961, Figure 1, reproduced courtesy of the Geological Society of America.)

THIS MAP WAS CHOSEN AS THE CENTERPIECE OF MY ESSAY BECAUSE, in my opinion, it was the trigger that set off the escalation of investigations and ideas that culminated in the theory of plate tectonics. Professor R. G. Mason and his co-author, A. D. Raff, undertook several voyages, plowing back and forth across the northeastern Pacific in a ship towing a magnetometer that continually measured the intensity of the earth's magnetic field.¹ These data were compiled and presented in the figure above. I shall attempt to trace the train of events and discoveries over the years from 1946 until 1963 that led to the theory of plate tectonics. I shall also try to show that it was this map and its later interpretation that provided the increased interest and research activity that led to the legitimization of the theory of continental drift and plate tectonics. Of necessity, it will be limited to my personal experience in recalling the facts as they were revealed to me through my own work and research and through the available scientific literature. My direct involvement in paleomagnetic research cuts off in 1963, when my consuming interest switched to remote sensing. Anyone prominent in this field after 1963 should not feel slighted by my lack of reference to their work.

EARLY INTERDISCIPLINARY TRAINING

My interest in geophysics goes back to my undergraduate years at the University of Toronto, Canada. In 1938, I had enrolled in mathematics and physics and was headed toward a career in actuarial science. During the summer after my first year, I worked with a life insurance company calculating policy dividends on a mechanical calculator. The work was so boring that the thought of spending my career in that environment caused me to choose the option in my second year which was honors physics and geology. I soon found out that this course was intended as an interdisciplinary experiment, that there were only two of us enrolled, and that previously there had only been one graduate in the past six years, J. Tuzo Wilson. He later became my Ph.D. supervisor and still later he became one of the group that originated the theory and the name *plate tectonics*.

I also soon learned about the huge gulf that existed, at the time, between the geologist's mind and the physicist's mind. Physicists looked down upon geology as being a descriptive and qualitative subject that did not really qualify as a science, and geologists thought that physicists were "egg heads" who did not live in the real world. Needless to say, the two groups had difficulty in communicating professionally. The physics and geology course was set up so that half of my lectures were in the Geology



Lawrence Morley, around 1963. (Photo courtesy of Lawrence Morley.)

Department and the other half in the Physics Department. It would be a few years before I could figure out how to fit the two subjects together, so different were their cultures. Throughout my career, this interdisciplinary training put me in the habit of reading the new literature from both disciplines, which I realized, in retrospect, enabled me to envisage the hypothesis that would explain the zebra pattern.

In my studies, I had become quite interested in geomagnetism, especially in its application to mineral prospecting. In 1940, in the middle of my third year, I left the university to join the British Navy as a radar officer at sea. In later years, this experience in electronics helped me devise instruments for my Ph.D. thesis. It also aroused my interest in oceanography. After World War II, I graduated in 1946 and took a job

with a small geophysical prospecting company doing magnetometer surveys using the most sensitive magnetometer known at the time. It was an Askania magnetometer, invented in Germany and designed for prospecting for magnetic ore. It measured the changes in the intensity of the vertical component of the geomagnetic field, which deflected a magnetized needle balanced horizontally on a knife edge. This was high-tech at the time. It would take a whole day to collect a mile-long magnetic profile of data. This tedious work, combined with unbelievable clouds of mosquitoes in the Canadian Shield, led me to think that perhaps I had made a mistake in dropping actuarial science.

THE AIRBORNE MAGNETOMETER

I remembered from lectures that the Gulf Research and Development Corporation, a research subsidiary of the Gulf Oil Corporation, was working on a new invention – an airborne magnetometer that had been used experimentally during World War II to detect submerged submarines.² I dreamed of the possibility of being able to collect a line-mile of data with an airborne magnetometer in 20 seconds instead of taking a whole day on the ground. What a powerful prospecting tool this would be! It could be used to cover extremely large areas very quickly and cheaply. With these thoughts in mind in the fall of 1946, I emerged from the bush and headed for the Gulf Research and Development Corporation located in Harmarville, Pennsylvania, just outside Pittsburgh, to see if I could get a job connected with their airborne magnetometer. They were putting the final touches on the latest version that Victor Vacquier (now at the Scripps Institute of Oceanography) had adapted for use as an airborne instrument to be towed on an 80-foot cable behind an aircraft to avoid being magnetically contaminated by stray magnetic fields originating in the aircraft itself. They didn't hire me because "I didn't have a Ph.D." However, they introduced me to an executive of Fairchild Aerial Surveys, Los Angeles, named Max Phillips, with whom Gulf had just contracted to magnetically survey the major part of the Llanos areas east of the Andes in Venezuela and Colombia. To my surprise and delight I was instantly hired as the party chief for the two-year project (1947–1948). As it turned out, that instant determined my career for the next 20 years.

The Llanos area was totally virgin territory as far as knowledge of the geology was concerned. It was mostly covered with thick jungle and there were few open areas or rock outcrops to map the geology. Thus, the dream about the power of this instrument for conducting reconnais-

sance surveys over very large areas quickly and cheaply was realized a lot faster than I could have imagined. This was the world's first commercial aeromagnetic survey, and it effectively demonstrated the power of the instrument. As a result of this survey, sedimentary basins were discovered that today have become important oil-producing regions. (Most of the potential oil and mineral areas of the world, including the continental shelves, have now been covered by aeromagnetic surveys.) Ten years later, the same magnetometer was adapted by R. G. Mason and A. D. Raff for use as a shipborne instrument that could be towed on a long cable to avoid being affected by the ship's magnetization. It was this instrument that was used to cover the very large area in the northeast Pacific from which the zebra pattern map was produced.

I returned from South America to Toronto in September 1948 to work with Dominion Gulf Company, a Canadian subsidiary of the Gulf Oil Corporation. They wished to exploit the use of their new "toy" in the exploration for minerals as well as for oil. In the first flight over a portion of the Precambrian Shield in northern Ontario, we recorded a magnetic anomaly so intense that it ran the recorder off-scale. For the next ten years, there was a feeding frenzy of prospectors looking for magnetic ore deposits in the Canadian Shield. The end result was that enough iron ore was discovered to meet the demand for the next 50 years.

I joined the Geological Survey of Canada (GSC) in 1952, as their first geophysicist, to take charge of an aeromagnetic mapping project. In the GSC the magnetometer proved useful for assisting geological mapping in areas covered by lakes and swamps. In 1960, we initiated a contract project to cover the whole Canadian Shield, at half-mile spacing at a cost of about \$30 million over 17 years. More than 5,000 aeromagnetic maps at a scale of one inch to the mile were published and made available to prospectors for 75 cents each. The benefits of this survey to the mining industry in Canada have never been calculated, but they must be more than several billion dollars and are still going strong.

POSTGRADUATE THESIS WORK

In 1949, I left Dominion Gulf to return to the University of Toronto for postgraduate training because of a remark made by one of Gulf's senior geologists: "geophysicists are great at going out and gathering and compiling data, but when it comes to interpreting the maps, they look like a bunch of monkeys trying to read the *New York Times*." This remark certainly described me, so in an effort to understand and interpret the

geological causes of magnetic anomalies that were depicted by contour lines on aeromagnetic maps, I began the study of rock magnetism under Professor J. Tuzo Wilson, who had recently been appointed head of the Geophysics Department. Wilson was mostly interested in macro-geology: the theory of continental growth, mountain-building, and geosynclines. In studying rock magnetism, I was very much on my own at the time: Wilson was not a “drifter,” a word used to describe “the lunatic fringe” who believed that continental drift might have been possible.

Understandably, I got no help from either the Physics or Geology Departments because none of the staff in either department had any idea what I was trying to do with such an esoteric subject. I also found the international literature very thin in this field. Previous investigators had begun to use the term *fossil magnetism* because they found that remanent magnetism appeared to be ‘frozen’ into the rocks in the direction of the earth’s magnetic field prevailing at the time the rocks became consolidated.³ They postulated that the process was analogous to the fossilization of sea life, so the term *fossil magnetism* began to be used. Later, when its potential began to be fully realized, it was given the more erudite Greek description *paleomagnetism*.

French investigators in the 1920s had measured the remanent magnetization of ancient earthenware pots found by archaeologists. The purpose was to determine their ages, or alternatively, if they knew the age of the pot, they could estimate the angle of declination between the geomagnetic and geographic north poles at the time the pot was fired. It had been shown experimentally that if an earthenware pot were fired in a kiln to a temperature above the Curie point for hematite (675°C) and subsequently cooled to room temperature, it always acquired a remanent magnetization in a direction parallel to the earth’s field.⁴ As pots are always placed vertically on their bottoms when being fired, it means that it is possible by measuring the direction of the remanent vector, to calculate the magnetic orientation of the earth’s field. The French scientists used this technique to try to track the secular movement of the earth’s magnetic poles during history, a parameter of great interest in the days before satellite navigation, as marine navigators depended on this knowledge.

Before starting my thesis research, I was mostly interested in measuring the content of the mineral magnetite in rocks of the Canadian Shield, because it has long been known that it is chiefly the varying magnetite content in the rocks that causes the magnetic anomalies. To measure the magnetite content of a rock in the field, I used an adapted wartime mine detector. It measured magnetic susceptibility, which had

been shown to be proportional to the magnetite content of a rock. I calibrated it by mixing known amounts of magnetite grains with non-magnetic sand. I was going to devote my thesis entirely to this work. However, shortly after I started my research in 1949, I came across John Graham's paper, which added "another string to my bow," namely, the measuring of magnetic remanence in rock samples.⁵ He referred to a paper by R.H. Johnson that described a 'spinner' magnetometer adapted for measuring the direction of remanent magnetization in small rock cubes that had been cut from spatially oriented rock specimens broken away from solid bedrock.⁶ Using this technique, Graham demonstrated that sedimentary rocks have the ability to hang onto their primeval remanent magnetizations. He did this by taking a number of closely spaced rock samples from a sedimentary layer that had been folded into an anticline. Even though the rock was folded, it managed to hang onto its original magnetization and was said to be magnetically stable. By studying the magnetism in rocks, it would be possible to unravel geological folds and faults in complicated structures. But what inspired me was this: Graham also wrote that it might be possible to prove whether or not the continents had drifted! Geologists and geophysicists in the 1920s thought they had buried Wegener's idea of continental drift, which he first published in 1912.⁷ How can continents drift like ships through solid rock across oceans? they asked. Yet the circumstantial evidence was mounting. Graham was right in the end: *paleomagnetism* did give quantitative proof to continental drift, but not in the way he expected.

Off I went into the Grenville subprovince of the Canadian Shield in the summer of 1951 with my spinner magnetometer and the magnetic susceptibility meter. I labored for three months, spending most of my time sawing about 300 rock cubes with an inadequate diamond-studded circular blade. When I returned to the laboratory in Toronto, I remeasured all my samples, and to my dismay and disgust, most of the measurements had changed radically. It was so frustrating. I found that by merely banging them on the table, I could change the measured direction of magnetization by as much as 90°! I fooled around for a bit, magnetizing and demagnetizing my samples with a permanent magnet, but basically I considered the paleomagnetism part of my thesis a failure. There was, however, an interesting facet. The fine-grained volcanic rocks held their original fossil magnetism, whereas the coarse-grained batholithic rocks did not. Furthermore, the ratio of the magnetic remanent component to the component induced by the earth's field was higher in the volcanic rocks. This fact later became important in the interpretation of the zebra map.

The thing that interested me was that all the mathematical models, developed for interpreting magnetic anomalies, took into account only the induced component.⁸ They ignored the remanent component, because there is no way to know the direction of the remanent component without extracting and measuring a rock sample. This is not very practical when you are interpreting an aeromagnetic map hundreds of miles square. The induced component is always in the same direction as the earth's field. I used to dream "wouldn't it be wonderful if we could pull the main switch on the earth's magnetic field?" We could then do our paleomagnetism without having to cut rock samples out of the outcrops. After receiving my Ph.D., I left this research in order to join the Geological Survey of Canada in June 1952 to manage their aeromagnetic program.

POLAR-WANDERING CURVES

In 1953, I was able to attend my first meeting of the American Geophysical Union. The keynote speaker was Dr. Keith Runcorn, a recent Ph.D. graduate from Cambridge, England. To my surprise, he was speaking on paleomagnetism. To my knowledge, he had not published the results of his research at the time, so I knew nothing about him or his work. He was a very dramatic and articulate speaker. He presented the paleomagnetic results from the Torridonian Old Red Sandstones in the United Kingdom. They were so spectacular that, from that time on, earth scientists sat up and took notice of paleomagnetism (the first time I had ever heard the term used). He related how the magnetizations of these sediments were unusually stable, although he did say one had to have a "green thumb" in selecting samples that were stable. Also, an idea new to me was that *he calculated the position of the geomagnetic North Pole* from the direction of the magnetization at the time these sediments were laid down or had become consolidated. This implied that if the same thing were done on other rocks of the same age from all over the world, the pole should be in the same position. He went one step further and stated that if many rocks of many ages from all over the world were measured, we would be able to plot the position of the pole for the whole of geological history and *thereby create a polar-wandering curve*. Because of continental drift, however, a polar-wandering curve for each continent would have to be constructed. Plotting the polar-wandering curves for each continent from that moment on for the next 12 years became the "holy grail" for most paleomagnetists.

A REVERSING EARTH'S FIELD?

Runcorn admitted that there was there was a slight problem with the results he had obtained. A number of his samples had a reverse polarity of exactly 180° . Without skipping a beat, he said that this was not a big problem. He simply counted them as though their polarities were not reversed – “The Earth’s field was probably reversed at the time.” Wait a minute: was he saying that the earth’s magnetic field was actually flipping inside out throughout geological history, that the North Pole periodically became the South Pole and the South Pole became the North Pole almost instantaneously? The American researchers working in paleomagnetism were there: John Graham of the Carnegie Institute and the Jim Balsley–Arthur Buddington team from Princeton University. During the question period, they spoke up, strongly disagreeing that polarity reversals were happening. Their explanation was that certain rocks had a peculiar mixture of ferromagnetic minerals that somehow caused a so-called self-reversal during the time of their emplacement. The debate went on for about 15 years before it was settled. In the end, Runcorn was right. It is now known that the earth’s field has been reversing throughout geological history.

My thesis supervisor, Tuzo Wilson, was sitting beside me and he urged me to get up and “say something” about my thesis results. I remarked at the meeting that whereas Runcorn was working on sedimentary rocks, I had been working on igneous rocks in the Precambrian Shield and that my rocks were quite unstable – that I could change their magnetic orientation by banging them on the table. This attracted some attention, because it placed some doubt on the reliability of paleomagnetic data. I was invited the next summer, in 1954, to attend a National Science Foundation conference at Idyllwyld, a mountain retreat outside Los Angeles. There were 25 attendees, about half of whom were young paleomagnetic investigators, the other half were senior scientists, representing a broader area of expertise, including famous physicists with expertise in ferromagnetism, geologists, and geophysicists. We were honored by the presence of the Nobel laureate, Linus Pauling. The conference lasted three days. Its main objective was to try to cast some light on whether or not the earth’s field had been reversing during geological history. All the paleomagnetists presented their results and conclusions. Long discussions and disagreements ensued, but nothing was resolved.

In 1955, the year after the AGU meeting in Washington, I had a chance to visit John Graham in his laboratory at the Carnegie Institution, Department of Terrestrial Magnetism. He set me straight on the

instability of my igneous rocks by explaining that all rocks had two kinds of magnetization, a soft component and a hard component. The hard component was the one we were after because it had consistent results from within the same geological formation. It is considered to be the true paleomagnetic magnetization. The soft component, which may have been caused by any number of things – from lightning strikes to glacial scraping – had to be eliminated before measuring. He had devised a method of doing this by subjecting the sample to an alternating demagnetizing field at the center of a Helmholtz coil over the period of a few minutes while gradually diminishing the field to zero.⁹ After such “magnetic washing,” the samples showed a consistency in their results. I don’t know whether or not he was the first to do this, but from then on all investigators magnetically “washed” their samples before measuring. This technique meant that almost all rocks could be used paleomagnetically and that one did not have to have a “green thumb.”

At the Geological Survey in Ottawa, I decided to start some research in paleomagnetism and devised another spinner magnetometer like the one I had made at the University of Toronto three years earlier. Philip Dubois, whom I met at the geomagnetic retreat in Idyllwyld, California, and who had completed his doctorate at Cambridge University under Keith Runcorn, joined me at Geological Survey of Canada in 1956 for a year or two to do paleomagnetic research. In 1957, he had published the first comparison of the polar-wandering curves for Europe and North America.¹⁰ It showed a separation of the two curves equal to what one would expect from the presumed drifting apart of the two continents. This was the first study I had seen that tried to fulfill John Graham’s 1949 dream of using rock magnetism to measure continental drift. Because of the uncertainty about the position of some of the ancient poles, however, a lot of skepticism about the results remained. In any event, loyal followers of the polar wandering school continued to gather such data from all over the world.¹¹ The more data gathered, the greater the confusion became about polar-wandering curves. The theory seemed to work for the younger rocks, but fell apart with the older rocks, whether sedimentary or igneous. The British directionalists spread their gospel to the United States, where they also seemed to ignore the reversal problem at first. This philosophy dominated the paleomagnetic literature until about the mid-1960s, when plate tectonics was born.

During the 1950s, however, a few paleomagnetists concentrated on the problem of reverse magnetization. They were intrigued by the sug-

gestion that the earth's field may have been reversing periodically throughout geological history. In 1955, Tr. Einarsen and T. Sigurgeirsson examined the polarity of a large number of samples taken from a thick section of lava flows in Iceland, which is part of the mid-Atlantic ridge.¹² They showed that there were as many with reversed polarity as there were showing normal polarity. I had observed several reverse polarities in volcanic rocks in my own thesis work in the Canadian Shield. In 1958, Andre Larochelle joined the Geophysics Division of the GSC. I urged him to return to McGill University for his Ph.D. and to study the reverse polarity of the rocks in Yamaska Mountain, a volcanic plug southeast of Montreal. A negative anomaly had shown up in an aeromagnetic survey we had done over the area. I was pretty sure it was caused by inverse remanence, which Larochelle confirmed by his paleomagnetic sampling. In addition to this, we had observed many other negative anomalies occurring in the vast areas we had surveyed in the Canadian Shield. The criterion I used to identify an aeromagnetic anomaly that was caused by inverse remanence in the underlying rock was as follows: if the negative anomaly occurred to the north of a larger positive anomaly, the effect was not ascribed to inverse magnetism but to the northerly dip of the earth's magnetic field. If, however, there was a negative anomaly without the associated positive anomaly to the south, it was interpreted as negative polarization. Yamaska met this criterion, as did numerous other anomalies we had surveyed. Together with all the other evidence in the literature of reverse polarity, even in sedimentary rocks, this put me firmly on the side of those who advocated a reversing of the earth's magnetic field. The year was 1957.

THE WONDER OF THE MID-OCEAN RIDGES

The most important ingredient in the formulation of the sea floor spreading and the later plate tectonic theories was the knowledge of the existence of the mid-ocean ridge system. The presence of mid-ocean undersea mountains had long been known, but recognition of their full extent into a connected worldwide system had to await the invention and use of the echo sounder, or fathometer. Even when its existence as a unified system became known, few geologists had the opportunity to consider its implications because of its inaccessibility. It remained to oceanographers and geophysicists, with an interest in ocean basin geology, to explore and explain. They discovered that the floor of the ocean

was covered largely by relatively recent volcanic rocks, as opposed to the continents, which have a mixture of very young and very old rocks. The seismologists supplied the information that both the mid-ocean ridges and the continental margins were seismologically very active. This information implied that the mid-ocean ridge system was a significant part of the earth's basic structure. It suggested that the earth is analogous to a cracked egg, intermittently leaking hot liquid out along the extent of the ridge system. It must have been these thoughts that led Hess and Dietz to the theory of sea floor spreading – a concept that is fundamental to plate tectonics.¹³

MASON AND RAFF

I do not know what led Ron Mason and Arthur Raff to undertake their magnetic survey of the northeastern Pacific in the 1950s. Because seismologists had plotted a large number of earthquake epicenters in the vicinity of the mid-ocean ridges as well as near the edges of the continental shelves, a lot of curiosity had been aroused about the largely unknown geology in the ocean basins. There had also been a lot of speculation about the origin of the continents: did they grow outward along their margins, as Tuzo Wilson was espousing, or did they split apart from one large supercontinent, by the process of continental drift?¹⁴ Most of the scientific establishment had long since poured cold water on Wegener's theory. Now they began to wonder if perhaps he had been at least partly correct.

Another factor that must have led Mason and Raff to do this survey was that there was available to them an airborne magnetometer which they could adapt to tow behind a ship.¹⁵ The airborne/shipborne magnetometer was perhaps the only instrument capable of revealing some basic reconnaissance information about the geology of the ocean basins over large areas. I was familiar with aeromagnetic data over the continents and, like Mason and Raff, we at the GSC were gathering magnetic data from a shipborne magnetometer over the continental shelves off Newfoundland and Nova Scotia. However, I never thought of operating over the ocean basins, as there was no economic incentive for the discovery of oil or minerals in the deep ocean basins.

Mason and Raff completed their work and published their results in the form of the zebra maps. They admitted they had no explanation for the cause of these linear anomalies. They tried correlating them unsuccessfully with gravity data and with sea-bottom topography. The data

remained in the literature for about four years with no plausible explanation as to their cause. Then, along came Hess and Dietz.

OCEAN FLOOR SPREADING

The concept of ocean floor spreading was first envisaged in detail in 1960 by Professor Harry H. Hess, a renowned professor of geology at Princeton University.¹⁶ Unfortunately, his paper was in the form of a report to the Office of Naval Research and, as it was supported by a contract, it did not appear in the open literature. The following year, in 1961, Robert S. Dietz published essentially the same hypothesis in the journal *Nature*.¹⁷ In his paper, he clearly described the “spreading sea floor theory.” In essence, he stated that the earth’s mantle (the main solid part of the earth outside the liquid core) is comprised of very viscous rock of pitchlike consistency, moving in a number of convection cells that are in a state of constant, slow movement, fueled by heat generated by the decay of radioactive minerals. This movement forces the rock up from the deep part of the mantle and spews it out all along the mid-ocean ridges in the form of lava. At the ocean ridges, the material bifurcates, half moving out from the ridge one way and the other half moving the other way. As more lava arises, the solidified lava that was there before moves out, making room for the new material. This process continues so that the whole ocean floor behaves like a wide conveyor belt until it reaches the edge of the continental shelf. At this point, the heavier ocean floor sinks under the continental shelf, and continues down to the depths, eventually turning back toward the ridge from which it originated, thus completing the convection cell. Dietz suggested that there are a number of convection cells operating independently, all related in position to the mid-ocean ridges and continental shelves.

An ingenious and outlandish hypothesis, it nevertheless provided a mechanism for continental drift. Instead of the continents having to push through solid rock in their migrations, they ride around like so much scum on the top of a boiling porridge pot. The ocean basin rocks pushing up against continents also explained the process of mountain-building, which geologists had been debating for 100 years. And it explained why all the rocks in the ocean basin are comparatively young lavas. The really old rocks are all in the continents. Like continental drift itself, it was a nice dream, but how were we going to prove it? I was not aware of this concept until after I saw Raff and Mason’s zebra pattern map. Unbeknown to me, Arthur Holmes had actually proposed such a theory in the 1920s.

THE EUREKA MOMENT

Raff and Mason's zebra map first appeared in August 1961. I literally freaked out when I saw it! I had been studying aeromagnetic maps from all over the world – both on the continents and on the continental shelves – and had never seen such a regular linear pattern of positive and negative anomalies stretching for 600 miles (1,000 kilometers) or more. All the continental maps with which I was familiar had anomalies in a sort of random bird's-eye maple pattern, nothing like these long, linear features, and Mason and Raff were unable to explain them. They might just as well have been maps of features on Mars because the geology was so unknown. Mason and Raff at first thought they might be caused by long ridges and troughs in the ocean bottom, but no correlation was found. They thought that there might be a series of long, dike-like bodies. This was checked by gravity surveys, but none was found. The data remained in the literature for a year and a half with no plausible explanation.

I had these maps on my mind for nearly two years before I spotted the Dietz paper on ocean floor spreading. Eureka! The idea came to me so suddenly that I sat down and submitted the following (unexpurgated) paper to *Nature* in February 1963.

Several investigators and authors writing on the subject of continental drift and convection currents in the earth's mantle have referred to the puzzling linear magnetic anomalies in the Eastern Pacific Ocean Basin reported by scientists of the Scripps Institution of Oceanography.

If one accepts, in principle, the concept of mantle convection currents rising under the ocean ridges, travelling horizontally under the ocean floor and sinking at ocean troughs, one cannot escape the argument that the upwelling rock under the ocean ridge, as it rises above the Curie Point geotherm, must become magnetized in the direction of the earth's field prevailing at the time. If this portion of the rock moves upward and then horizontally to make room for new upwelling material and if, in the meantime, the earth's field has reversed, and the same process continues, it stands to reason that a linear magnetic anomaly pattern of the type observed would result. This explanation has the advantage, over many others put forward, that it does not require a petrologically, structurally, thermally or strain-banded oceanic crust. It requires a convection cell whose axis of rotation is at least as long as the linear magnetic anomalies and whose horizontal distance-of-travel stretches from ocean rise to ocean trough. In addition to this it requires a large number of reversals of the earth's magnetic field from at least the Cretaceous period to the present (since no older rocks than the Cretaceous have been found in the ocean basins).

R.L. Wilson reported that Mrs. J. Cox, in a recent search of the palaeo-magnetic literature, was able to find 136 normally polarized cases and 141 reversely polarized from the Carboniferous to the present. Since there is no evidence to suggest that the earth's field should have been 'normally' polarized for any more periods or for longer periods than it has been reversely polarized, it is entirely possible that there may have been as many as 180 reversals since the Lower Cretaceous. This would be one reversal about every half million years on the average (a figure which T. Einarson(5) gives from his investigation of Icelandic lavas). He also suggests that the time taken for a reversal of the field is geologically very short – a few centuries to 10,000 years.

From an examination of the Scripps magnetic maps, the width of a complete positive and negative cycle, averaged over the widest part of the available surveyed section, is about 35 kilometers. To travel this distance in 1,000,000 years (time of two reversals), the convection current must have a rate of about 3.5 centimeters per year. This figure is only good to an order of magnitude, because no accurate data are available on the length of the periods of reversals. A better way to arrive at the rate of convection travel and the reversal period would be to measure the ages of rocks at widely spaced locations in the Pacific and to count the number of reversals occurring between these points.

Mason and Raff (1) report that some of the many guyots which were detected on the echo sounder produced magnetic anomalies, while others apparently had little or no effect. It seems unlikely that these guyots would be divided into two classes – those containing magnetite and those containing little or none. A more likely explanation would be that the ones which give little or no effect are negatively polarized to an intensity which nearly equalizes their magnetization induced by the present earth's field. If the 'non-magnetic guyots' always occur in the negative anomaly bands, and the magnetic ones in the positive bands, this would be evidence that they cooled below the Curie Point at approximately the same time as the rock surrounding them, because they were magnetized in the same direction. Indeed, since at that time they would have been in the shallow water of the ocean ridge, they would have protruded above the surface and have their tops flattened by erosion. As they proceeded along with the mantle convection current, they would pass into deeper water. This is an alternative explanation of origin to that suggested by Darwin for the flat-topped guyots in the deep Pacific.

There are a few difficulties. The seismic results postulating 3 layers above the Moho must be incorporated into the theory. Mason and Raff (1) offer three models to marry the seismic and magnetic results:

- (1) A 2 km-thick slab of intensely magnetized lava of $K = .015$ units underlain by a relatively non-magnetic crustal layer.¹⁸
- (2) A topographical plateau 2 km high composed of material $K = .015$ underlain by a main crustal layer of the same magnetic susceptibility.
- (3) A 6 km-thick slab extending from the bottom of the unconsolidated sediment to the Moho composed of 2 seismic layers, but all of the same magnetic susceptibility $K = .005$.

From measurements of several thousands of basaltic lavas from the Canadian Shield(6), none have been shown to possess a magnetic susceptibility of as great as .015 c.g.s. This would mitigate against accepting models (1) and (2). On the other hand, many lavas have a susceptibility as high as .005. This is not to imply, however, that we are postulating nothing but lavas down to the Moho. The other seismic layer in between must be explained. If this layer were unaltered ultramafic rock, it would not be sufficiently magnetic to cause the observed anomalies, nor would it have a significant seismic velocity contrast with the mantle material. Hess (7) has suggested that the main crustal layer beneath the oceanic basalt could be serpentinized ultramafic rock. This would satisfy both the magnetic and seismic requirements, since the serpentinization process both increases the magnetic susceptibility of the ultramafic rock and lowers the seismic velocity.

Thus Mason and Raff's (1) model number 3 is favored, with the modification that adjacent prisms would be magnetized oppositely. The prism producing the positive anomaly would be normally polarized with a total magnetization (remanent plus induced) equal to $> .005$ c.g.s. The prism producing the adjacent negative anomaly would be inversely polarized, so that the remanent magnetization would approximately cancel the induced."

The purpose of this letter is to point up the possibility of calibrating the frequency and duration of reversals of the earth's field in geological history from a study of the ocean basins, and the idea presented is considered to support the theory of convection in the earth's mantle.

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THE PALEOMAGNETIC ESTABLISHMENT REJECTED THE IDEA

I never had any doubts about the concept. It locked three disparate and unproven theories together in a mutually supportive way: the theories of continental drift, sea floor spreading, and the periodic reversing of the geomagnetic field. It was like finding the key piece to an enormous jigsaw puzzle that made everything fit together. And it was based on actual quantitative data spread over thousands of square miles. To me, this was such a straightforward idea that I wanted to get it into print in a widely recognized journal as soon as possible, before someone else thought of it. I therefore put it in the form of a short paper that I submitted to *Nature* in February 1963. I received a rejection notice two months later stating that they did not have room to print it. I then immediately submitted it to the *Journal of Geophysical Research* in April but received no answer until late August. During this hiatus, I had assumed that my letter would shortly be published, but as I was impatient to get some feedback, I presented the idea at the June 1963 meeting of the Royal Society of Canada held in Quebec City. During the presentation, about 40 geologists and geophysicists were present. While I had not expected a standing ovation, I was somewhat surprised that not even one question was asked. In retrospect, I realize that there was probably no one there who had read anything about sea floor spreading or paleomagnetism and that I had not taken enough time to explain. I now know that this is a common fault among inexperienced authors.

In August 1963, I attended the San Francisco meeting of the International Union of Geodesy and Geophysics. Again, I still thought I was safe to talk about it before publication, so when I saw Runcorn and Hess engaged in conversation, I went up to them and briefly explained my idea. Runcorn was either bored or distracted, because he was obviously not listening, but Hess, who had been pushing his ocean floor spreading theory, was very interested and expressed the desire to meet with me again. Unfortunately, he died before we had the opportunity to do so.

Toward the end of August 1963, I received a rejection notice from the editor of the *Journal of Geophysical Research*, accompanied by a note from the reviewer with his name cut off from the letter. It stated: "Found your note with Morley's paper on my return from the field. His idea is an interesting one – I suppose – but it seems most appropriate over martinis, say, [rather] than in the *Journal of Geophysical Research*."

I received this bad news at the end of August and was thinking of publishing elsewhere, but in the September 7, 1963 issue of *Nature*, the now famous article by Vine and Matthews entitled "Magnetic Anomalies over Oceanic Ridges" appeared.¹⁹ It contained essentially the same idea that I had unsuccessfully attempted to publish twice. In the parlance of the time, I was "scooped." Obviously I could not publish elsewhere because I could have been accused of plagiarism.

I felt frustrated with the system. I knew that when a scientific paper is submitted to a journal, the editors choose reviewers who are experts on the topic being discussed. But the very expertise that makes them appropriate reviewers also generates a conflict of interest: they have a vested interest in the outcome of the debate. We could call this the "not invented here syndrome": scientists may be biased against good ideas emerging from someone else's lab. In retrospect, that is exactly what happened.

The hypothesis was not generally accepted until about 1965, the date commonly regarded as the birth of plate tectonics. At the time I took less interest in geophysics because I had received approval to set up a satellite remote sensing branch for the Canadian government, and soon left the Geological Survey of Canada to manage the Canada Centre for Remote Sensing.

In 1982, Professor Fred Vine and I had lunch together in London. He told me then that his paper also was not generally well received at first. However, by chance, both Professors Hess and Tuzo Wilson were taking a sabbatical at the same time at Cambridge University in 1965. The three of them got together. Wilson had envisaged a new kind of 'transform fault' that explained the structure of the mid-ocean ridges. I understood that it was at that time that the fully integrated theory of plate tectonics was put forward.

CONCLUSION

It seems to me that junior scientists are often cowed by the self-assurance of recognized authorities. In retrospect, what stands out for me is how Mason and Raff frankly admitted to having no plausible explanation for

the “zebra stripes,” which they had spent so much time and effort to acquire. If they had suggested that one of their interpretations was indeed correct, I would probably have accepted it, thinking that they had the situation in hand. Instead, their forthright approach created a space for a young scientist like myself to attempt an explanation of their data.

But if junior scientists were generally afraid to challenge accepted wisdom, what of the senior scientists? For two years, the “zebra stripe” data remained in the open literature, with no plausible explanation. Were senior scientists in turn cowed by what the recognized authority of their day, Sir Harold Jeffreys, had said 40 years earlier: that continental drift was physically impossible?

Plate tectonics ultimately required integration of evidence and insights from various fields. Many people missed the boat because they lacked the breadth to see the larger picture. Most geophysicists at the time, being grounded in physics, were not in the habit of reading geological literature, and probably would have missed the papers on sea floor spreading that countered Jeffreys’ dictum about the physical impossibility of continental drift. Likewise, few geologists understood anything about the geomagnetic field, ferromagnetism, or rock magnetism.

Hence a word to humble earth science students: don’t be cowed by the experts, and don’t be too narrow in your reading habits. Science has only scratched the surface of the natural world. Opportunities for new important discoveries are limitless.