Early Work Leading to the Explanation of the Banded Geomagnetic Imprinting of the Ocean Floor

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The idea of writing a historical paper is new to me. All I can hope to do is to give my own subjective slant, as I saw things develop in the period from 1938, when I first became interested in geophysics, to 1965, when I left the field to work in remote sensing (in which I have been engaged ever since). When I talk about geophysics, from which I have been away for so long, I feel like Rip Van Winkle, who fell asleep for 20 years.

It was Lachlan Gilchrist, a professor of physics at the University of Toronto, who first interested me in magnetic prospecting in 1939. Gilchrist was much interested in magnetic prospecting and had hired Arthur Brant, a University of Toronto student who had won a scholarship to study geophysical prospecting in Berlin and had received his doctorate there.

Gilchrist was best known at Toronto for establishing the interdisciplinary courses of physics and chemistry, physics and geology, and engineering physics (these may have been the first interdisciplinary courses of this nature on the continent.) Tuzo Wilson was the first student to take the new Physics and Geology course in Toronto, and then no one else took it for 7 years until Jack Cartwright (now a geophysicist with Imperial Oil, Canada) and I began in 1938. I left in the middle of my third year and joined the Canadian Navy in 1940 with a group of physics and electrical engineering students who were loaned to the Royal Navy for radar duties.

When I returned in 1945 to finish my degree in physics and geology at Toronto, geophysics was well started. There was a staff of three: Arthur Brant, who later went to Newmont Mining and is well known in exploration geophysics; Norman Keevil, who started the radioactive age determination work at Toronto; and John Hodgson, who later became Dominion Seismologist with the Department of Mines and Technical Surveys (Ottawa).

At that time, the "flavor" at Toronto in geophysics was mostly mineral exploration geophysics, and I became bitten by that bug, along with several other students who were there then. This was largely due to the influence of Arthur Brant, who, a year or two earlier, had located the Steep Rock iron ore deposit in northern Ontario, and Norman Keevil, who had started his own geophysical company (which later expanded into the Teck Corporation, one of the largest mining companies in Canada).

In the summer of 1946, I worked for Norman Keevil as a tripod magnetometer operator in Northern Ontario. As I painfully took one miserable reading every 5 minutes or so on an antique Askania magnetometer, between swatting black flies, I thought there must be a faster way to gather the data. This bad experience led me to aeromagnetic work, which later became my passion. I remembered Arthur Brant speaking about development work under way on an airborne magnetometer at the Gulf Research and Development Corporation in Harmarville, Penn. At the end of my field work in 1946, I went to see E. A. Eckhard and Leo J. Peters at Gulf Research and Development. I was looking for a job connected with the airborne magnetometer. This device had been developed by R. D. Wykoff and Victor Vacquier of Gulf shortly before the war and was turned over to the U.S. Navy for use in submarine detection. Gulf was anxious to begin using it for geophysical exploration. They had no position for me, for I did not have a Ph.D., but they helped me get a job as party chief with Fairchild Aerial Surveys, a company that had just signed a contract with Gulf to conduct what was to be the first commercial aeromagnetic survey. This was conducted in 1947 and 1948 in the Llanos areas of Venezuela and Colombia and was recently reflown in Venezuela.

The airborne fluxgate magnetometer was the key instrument without which the theory of plate tectonics would not have been developed. It was the fluxgate magnetometer (later replaced by the proton magnetometer) that was used by Mason and Raff in their historic survey off the west coast of California, beginning as early as 1952 [see *Raff and Mason,* 1961].

In 1948, I returned to Toronto from South America and worked with the Dominion Gulf Company, a newly formed mining subsidiary of Gulf Oil. It was created by E. A. Ekhardt of Gulf Research and Development Corporation to exploit the airborne magnetometer as a mineral prospecting tool. Up until then, it had only been used for petroleum exploration.

After a year with Dominion Gulf, I recognized my need for more education, and in 1949 I registered for graduate work at the University of Toronto, where Tuzo Wilson had become head of the geophysics program. Brant and Keevil had gone off to industry. My ambition was to interpret aeromagnetic surveys in a quantitative way. The state of that art was summed up in the words of a Gulf Oil executive:

> We have a lot of bright young men who can design instruments and make magnetic surveys, but when it comes to interpreting the aeromagnetic maps, they look like a bunch of monkeys trying to read the New York Times.

Magnetic interpretation of survey data was based on the theory of the two-dimensional inclined dyke, which assumed that any anomaly was caused entirely by induced magnetism. The effect of remanent magnetism was not taken into account, as there was no way of knowing its direction or intensity except by laboratory tests on local samples. There is no known method of making in situ measurements on remanence, a shortcoming that still persists. This is what led me, for my thesis, into the study of rock magnetism, later to be known as "paleomagnetism." In 1950, I submitted my Ph.D. thesis title: "Correlation of the Susceptibility and Remanent Magnetism With the Petrology of Rocks From Some Precambrian Areas in Ontario." It was to be a study of an area centering on Bancroft, Canada, where the Geological Survey of Canada had first conducted an aeromagnetic survey. My plan was to extract samples of rocks, cube them, and then build and use a spinner magnetometer, such as that described by *McNish and Johnson* [1938], to measure the intensity and direction of their remanence. I could use

Cover. Leg 106 of the Ocean Drilling Program drilled the first "zero-age" crustal hole in the median valley of the Mid-Atlantic Ridge, south of the Kane Fracture Zone, and carried out the first drilling in an active, submarine hydrothermal field. The cover shows four scenes of the drilling operations taken with a high-resolution low-light video camera system. (upper left) The drillstring during a test "spud-in" (i.e., the drilling of a test hole) on lightly sedimented pillow lavas in 3340 m of water on a small axial volcano in the rift valley. The circular object with the vane is a compass. (upper right) Deployment of a guidebase designed to provide lateral support for the drillstring during initial spud-in. The guidebase is approximately 5.2 m square, 3.3 m high, and weighs approximately 15,900 kg. (lower left) The guidebase and drillstring during a typical reentry operation. (lower right) Drilling at the foot of an 11-m high, active "black smoker" chimney discovered in the rift valley 25 km south of the Kane Fracture Zone. Thick (greater than 13 m) deposits of Cu, Zn, and Fe sulfides were recovered. Note the shrimplike organism, common in the vent area. (Photographs and interpretation courtesy of Robert Detrick of the Leg 106 Scientific Party. Detrick is with the Graduate School of Oceanography of the University of Rhode Island, Narragansett.)

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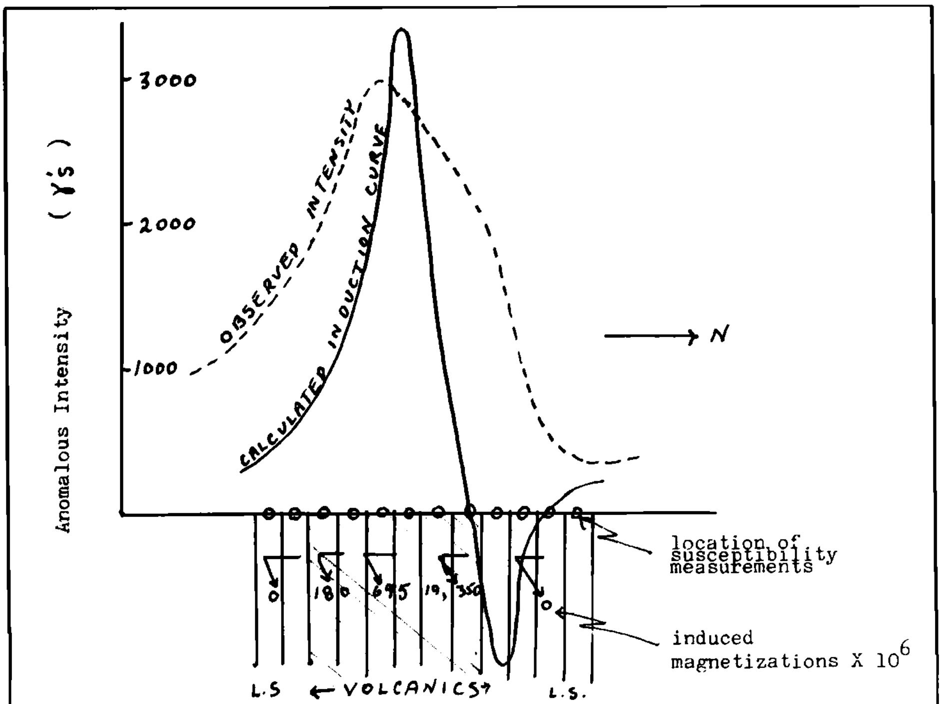

Fig. 1. **Comparisons of the observed intensity curve and the calculated induction curve over the Glanmire volcanic band, Bancroft, Canada (L. Morley, from his unpublished Ph.D. thesis, University of Toronto, 1952).**

these characteristics to calculate the contribution to the anomaly and then compare it with the observed anomaly. Up until that time, the main people to have studied remanence in rocks were the German researcher *Koenigsberger* [1934], the French researcher *Mercanton* [1910] and colleagues, the Thelliers [*Thellier and Thellier*, 1941], and *Bruckshaw and Robertson* [1949] of Imperial College, London. I studied Koenigsberger's work intensively. He had gone deeply into magnetic properties of rocks, had postulated the theory of thermoremanence, and had made many measurements on the natural remanence of various rock types and their coercive forces. To measure susceptibility, I converted a wartime mine detector into an in situ susceptibility meter and calibrated it by using a known amount of spherically worn magnetite particles mixed with nonmagnetic Ottawa sand.

Shortly after I started my thesis work, I saw John Graham's paper entitled "The Stability and Significance of Magnetism in Sedimentary Rocks" [*Graham*, 1949]. This paper was important to me for two reasons: First, it was the first paper that I had seen that recorded the use of a remanent magnetometer that had sufficient sensitivity to measure remanence in sedimentary rocks (mine could only handle igneous and metamorphic rocks). Second, it was the first paper that I had seen that mentioned the possibility of being able to confirm or disprove continental drift by measuring fossil magnetism on different continents. This inspired me to go into the field during the summer of 1950 and to try to answer some unsolved structural problems by measuring remanent magnetic directions in the Grenville subprovince of the Precambrian shield.

The results were a disaster. This was in the days before magnetic cleaning, and most of the rocks that I collected and laboriously cubed with a diamond saw were quite unstable. After measuring a cube, I would simply tap it on the table, and upon remeasuring, I would find that the direction of the magnetization had changed by as much as 90°! About the only partially useful conclusion that I could reach was that the average ratio of induction to remanence in those igneous and metamorphic rocks was about 3:1. Working on the theory of coercive force, I actually tried magnetic cleaning, without knowing that John Graham had been working on it, but my equipment was too crude to do anything effective.

Because the remanence of my samples was unstable, I could not combine it with the susceptibility measurements to calculate the total anomalous field. I could only use the susceptibility to calculate the anomalous induction field and compare it with the total field that had been observed by the airborne magnetometer. The difference between the two would be the anomalous remanent field. Figure 1 is a sample diagram from my thesis, which was completed in 1952.

The 1953 AGU Spring Meeting, the first AGU meeting that I had attended, was a very exciting one. Keith Runcorn, who was then at Cambridge University, explained that the trick in getting consistent measurements was to choose the right rocks; to do this, according to Runcorn, one had to have a "green thumb." Up until this time, he had worked largely with sedimentary rocks and had had good results. The really controversial part of his talk, however, concerned the way in which he dealt with reversely magnetized rocks. He had pretty well accepted as proven the periodic reversals of the earth's field. He argued that this did not affect his derivation of the positions of ancient magnetic poles, as long as the direction was altered by exactly 180°, for whenever he plotted the pole positions from a reversely magnetized sample, he would simply treat its magnetization as if it were exactly the opposite. His main critics at that meeting were Balsley and Buddington, who did not accept the theory of a periodically reversing earth's field, preferring to explain reversely magnetized rocks by an intrinsic self-reversing mechanism [*Balsley et al.*, 1952].

Like me, Jim Balsley had "backed into" paleomagnetism from the necessity of trying to understand aeromagnetic interpretation. He was the first person to carry out a full-scale aeromagnetic survey, a task that was done for the U.S. Geological Survey (USGS) in the naval petroleum reserves in Alaska. In 1947, he brought the USGS magnetometer aircraft to Canada and demonstrated it to the Canadian Geological Survey, after which Canada acquired two surplus magnetometers from the U.S. Navy and converted them for survey purposes. This was the beginning of the very successful systematic aeromagnetic survey program of Canada, which has been conducted by the Geological Survey of Canada for the last 36 years and has resulted in the complete aeromagnetic survey of Canada at a standard line spacing of 0.5 mile (0.8 km). This has enormously increased our knowledge of Canadian geology and has resulted directly and indirectly in several billions of dollars worth of mineral discoveries.

At the 1953 AGU Spring Meeting, Jim Balsley was strenuously pushing the theory of self-reversing rocks. Such rocks, he said, contained two main ferromagnetic components of differing Curie points. During the process of cooling, the mineral with the lower Curie point would get caught in the demagnetizing field of the other one. If, after fully cooling, it had a stronger magnetic moment than the first, the result would be a reversely magnetized sample.

I had not planned to speak during the discussion period because of the negative results of my thesis, but I was sitting beside Tuzo Wilson, who had been my supervising professor at the University of Toronto. He jabbed me and strongly suggested I "say something." I told the story of my unstable samples, which must have impressed some people, **since it apparently established me as a field worker in rock magnetism and resulted in my being invited by John Graham to visit his laboratory. He showed me how to increase the sensitivity of my magnetometer and introduced to me his "magnetic washing machine," as he called it. I don't know whether this device was the first of its kind or not, but it was the first that I had encountered.**

As another result of my impromptu speech, I was invited to the Palaeomagnetic Conference in June 1954, at Idyllwild, in the mountains outside Los Angeles, Calif. This conference was organized by Louis B. Schlichter of the University of California Geophysical Laboratory and the National Science Foundation. The purpose of the meeting was to try to resolve whether reversely magnetized rocks indicated the reversal of the earth's field or a self-reversing process. The participants (pictured in Figure 2) were a mixture of palaeomagnetists, famous physicists from the field of ferromagnetism, and physical chemists. No proceedings or minutes were published, which it did not really matter, as aside from yet another review of the controversy, nothing new came out of the meeting.

I returned to the Geological Survey of Canada (GSC), where my main concern was orga-

Fig. 2. Participants at the National Science Foundation Conference on Anomalous Magnetization of Rocks, Idyllwild, Calif., August 7–9, 1954. (Row 1, kneeling, left to right) Charles Kittel, John W. Graham, Sir Charles Wright, Ernest H. Vestine, S. Keith Runcorn, Louis B. Shlichter, Francis Bitter, Ronald G. Mason, and an unidentified participant. (Row 2, standing, left to right) David T. Griggs, Takesi Nakata, Walter M. Elsasser, Linus Pauling, J. A. Clegg, John Verhoogen, Emile Thellier, Carl Eckart, Gustaf O. S. Arrhenius, James R. Balsley, an unidentified participant, C. Duncan Campbell (?), John C. Belshe, Lawrence W. Morley, Arthur F. Buddington, and Philip M. Dubois.

Fig. 3. Composite aeromagnetic map of Canada, compiled by the Geological Survey of Canada (1960–1985). Dark tones indicate positive anomalies, while light tones show negative anomalies.

nizing the aeromagnetic survey of Canada. By this time (1955–1956), we were beginning to see rather large-area compilations of aeromagnetic data of the Canadian shield (Figure 3). I was naturally interested in any anomalies with negative polarization. This is very difficult to recognize in an aeromagnetic survey, as every positive anomaly at Canadian latitudes always has a negative magnetic anomaly directly to the north that is associated with it. I would have loved to have been able to pull the main switch to the earth's magnetic field so that we would be able to see an aeromagnetic field of just the natural remanence without the dominant induction component.

In 1956, Philip Dubois, who had worked under Runcorn at Cambridge University, joined me at GSC. He had published the first comparison of the polar wandering curves for Europe and North America, which showed a separation of the two curves equal to what one expected from the presumed continental drift [*Dubois*, 1957]. This was the first study that I had seen that tried to fulfil John Graham's 1949 dream. Dubois left in 1957 to teach at the Phillips Academy, Andover, Mass.

In 1958, Andre Larochelle joined the GSC Geophysics Division. He became interested in paleomagnetism, and I urged him to obtain his Ph.D. He did so, at McGill University in Montreal, where his thesis was on the negative anomaly of the Mount Yamaska volcanic plug near Montreal. He demonstrated that the rocks were indeed negatively polarized and that they contained only one magnetic component. When he came back to GSC, he proceeded to build an astatic magnetometer of the Blackett type and a magnetic cleaning apparatus that was above the standard of those in other labs. He also automated the whole process of collecting and measuring samples, which greatly speeded up the work at GSC.

About this time, Mason, and later Mason and Raff, began publishing the extraordinary results of their survey of the East Pacific ocean basin [*Raff and Mason*, 1961]. By 1961, they had extended their survey so far that it was becoming obvious that magnetic banding in the ocean basins was the rule rather than the exception. For the next few weeks, I could think of nothing else. My regular duties were totally neglected as I searched the literature for clues. It was obvious that there was some explanation that was fundamental to the whole origin and geological structure of ocean basins. I became obsessed. Why was there a regular banding pattern? Why was it so different from the convoluted patterns over the continents, with which I was so familiar? At GSC we had also done ship magnetometer surveys dating back to 1958, but they had all been over the continental shelf areas of the Atlantic Coast and Hudson Bay. None were done over the deep ocean basins

From my knowledge of rock magnetism and aeromagnetic interpretation, I knew that this positive and negative banding had to be due to remanence. These data sat in the literature for at least 3 or 4 years with no explanation, a fact frankly admitted by Raff and Mason. They had tried to explain it on the basis of a north-south system of topographical ridges and valleys, which they ruled out when they compared the two types of data. They had also postulated that it might be caused by a system of north-south trending dykes, an explanation that they also later rejected.

As I have said, I was sure that the cause was remanence and that the positive and negative banding was associated with the possible periodic reversals of the earth's field. In this, I was reinforced by R. L. Wilson's statement, in her 1960 study, that "in recent geologic history, there were nearly as many cases reported of reversely magnetized samples in the literature as there were normally-magnetized samples" [*Wilson*, 1962].

Still, there was no complete explanation. In searching the geological literature on the ocean basins, it struck me that this banding might somehow be related to the East Pacific Ridge, simply because the banding was parallel to the ridge. Then I ran across Robert Dietz's paper on ocean floor spreading [*Dietz*, 1961]. *Eureka!* I knew immediately that this was the explanation. If the rocks at the midocean ridges were rising from depth, they would become thermoremanently magnetized in the direction of the earth's field prevailing at the time. They would then spread laterally in both directions toward the continents, according to Dietz's theory. A million or so years later, the earth's field would reverse, and in this way, a positive and negative banding pattern would gradually be built up.

From this moment (December 1962), I never had any doubts about the concept. It locked three theories together in a mutually supporting way: the theories of continental drift, ocean floor spreading, and the periodic reversing of the earth's field. For the life of me, I could not figure why Dietz hadn't thought of it, because he said in his paper that he had discussed his work with various people at the Scripps Institution of Oceanography (La Jolla, Calif.), including Victor Vaquier. I knew that Victor Vaquier had great familiarity with aeromagnetics and rock magnetism, and I had thought that he knew something that I didn't in this case, or he would have published this explanation himself.

Over the next 8 months I tried, desperately and unsuccessfully, to get my idea into print. I first submitted my paper to *Nature* in February 1963, and it was rejected on the grounds that the journal did not have enough room! In April, I submitted it to the *Journal of Geophysical Research,* where the editors kept the manuscript all summer. Late in August, I received a rejection notice from the editor accompanied by an enclosed note from the referee, with the signature cut off. The note apologized for the long delay in replying, stating that he had been engaged in field work in Hawaii all summer and had not received the paper until he returned to his laboratory. He said "it was an interesting idea by Morley but was something which was more appropriately discussed at a cocktail party than published in a serious scientific journal."

Just as I was planning to submit it to a Canadian journal, the September 7, 1963, issue of *Nature* came out with an article by *Vine and Mathews* [1963] entitled "Magnetic Anomalies over Oceanic Ridges," giving the same explanation that I had. Their note also referred to the Dietz paper.

For me, the main "ball game" was over. The work that remained after was merely a mop-up job to me, and I began to direct my efforts more and more to remote sensing, which has been my consuming passion since.

Acknowledgment

An earlier version of this work was presented as an invited paper at the 1985 AGU Spring Meeting in Baltimore, Md., as part of the session "Secular Variation and the Geomagnetic Field."

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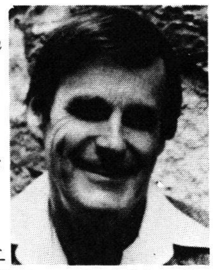

Lawrence Morley *graduated with a degree in physics and geology from the University of Toronto in 1946, after his undergraduate education had been interrupted by 4 years of war service as a radar officer in the British and Canadian navies. As described in this paper, he afterward worked for the Gulf Research and Development Corporation and later returned to the University of Toronto to receive his Ph.D. His later pioneering work with the Geological Survey of Canada and the research that led to his famous paper on the relationship of magnetic imprinting of the seafloor to seafloor spreading, the period reversals of the earth's magnetic field, and continental drift are also detailed above. In 1969, Morley was the founding director of the Canada Centre for Remote Sensing.*