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WILLIAM MAURICE EWING

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A Biographical Memoir by

EDWARD C. BULLARD

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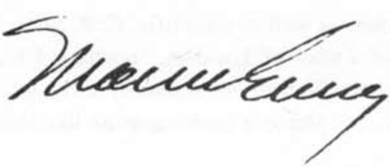

WILLIAM MAURICE EWING

May 12, 1906–May 4, 1974

BY EDWARD C. BULLARD*

CHILDHOOD, 1906–1922

WILLIAM MAURICE EWING was born on May 12, 1906 in Lockney, a town of about 1,200 inhabitants in the Texas panhandle. He rarely used the name William and was always known as Maurice. His paternal great-grandparents moved from Kentucky to Livingston County, Missouri, at some date before 1850. Their son John Andrew Ewing, Maurice's grandfather, fought for the Confederacy in the Civil War; while in the army he met two brothers whose family had also come from Kentucky to Missouri before 1850 and were living in De Kalb County. Shortly after the war he married their sister Martha Ann Robinson. Their son Floyd Ford Ewing, Maurice's father, was born in Clarkdale, Missouri, in 1879. In 1889 the family followed the pattern of the times and moved west to Lockney, Texas.

Floyd Ewing was a gentle, handsome man with a liking for literature and music, whom fate had cast in the unsuitable roles of cowhand, dryland farmer, and dealer in hardware and farm implements. Since he kept his farm through the

*This memoir is a corrected and slightly amplified version of one published by the Royal Society in their *Biographical Memoirs* (21:269–311, 1975). The main changes are that more detail is given in the first section and that numerous trivial errors in the bibliography have been corrected. The Royal Society has given permission for the republication.

years of the depression, he must have been a farmer of persistence and ability. He is spoken of with great affection by all who knew him; he was a marvelous storyteller and an accomplished violinist who played the old hoedown pieces with enthusiasm. His daughter Rowena has "such vivid memories of him playing, always standing so straight and tall."

Ewing's mother, Hope Hamilton Ewing, was born at Breckenridge, Stephens County, Texas, in 1882. She was the daugher of Isaac Hamilton of Illinois and Martha Ann Carnahan of Arkansas, and she and her family moved to Lockney in 1892. The Ewings and the Hamiltons were among the earliest settlers along the edge of the high plains of northern Texas. In 1901 she married Floyd Ewing; she was nineteen and he was twenty-two. In 1902 they set out on a homesteading venture in eastern New Mexico near Portales. They traveled in the traditional way with a wagon, two mules, a horse and a cow; they dug a well, set up a windmill, and constructed a "half dug-out" with a sod roof. A few months later they returned to Texas and drove a herd of fifty cattle to their ranch. Unfortunately, they had moved into an arid area in the worst year of a five-year drought. The story of the ensuing disasters has been told with great skill and sympathy by Maurice's brother Floyd, who was a professor of history at Midwestern University, Wichita Falls, Texas (F. F. Ewing, 1963). In 1904 they returned to Texas.

Maurice was the fourth of ten children. The three oldest had died very young in New Mexico so that he grew up as the eldest of seven. Mrs. Ewing was determined that her children should receive a good education and should have a wider choice of careers than was to be found in a small west Texas town. All but one, the eldest daughter Ethel, went to a university and had professional or academic careers. Ethel married very young and for many years was a successful teacher of the piano in Tulia, Texas. Bob became a naval captain and now works at the Marine Science Institute at Galveston, Texas. Rowena married J. A. Peoples, a geophysicist and an early colleague of Ewing's; Lucy married C. H. Clawson, a professor of psychology at Amarillo, Texas; John, the youngest, worked for many years with Ewing at Lamont and is now at the Woods Hole Oceanographic Institution where he was, for a while, Chairman of the Department of Geology and Geophysics. It is remarkable that so many of Maurice's brothers and sisters should have followed careers which intertwined, in different ways, with his own.

Maurice enjoyed telling stories of his father's farm at Lockney. No doubt the stories improved with the passage of time, as when he said that he spent much of each spring killing rattlesnakes with a hoe while chopping cotton. The family was not well off, but he remembered his childhood as a happy time and all his life kept the slow speech, the self-confidence, and the kindliness of the rural Texas of his youth.

At public school in Lockney he at first preferred grammar and languages to other subjects; later, in high school, he developed an interest in science and mathematics. He ascribed the change to the excellence of the teaching in the Lockney high school. In 1922, when he was sixteen, he was awarded the Hohenthal Scholarship to the Rice Institute in Houston, Texas.

A STUDENT AT THE RICE INSTITUTE, 1922–1929

The journey to Houston had to be done in the most economical way. On one occasion, probably in his sophomore year, he started off on a motorcycle which he bought for $12 from a man who had taken it to pieces and could not get it together again. He had a $10 bill in his pocket and a blanket roll strapped behind him. On the first day the chain of the motorcycle broke and he ran out of gasoline; he abandoned the machine and boarded a freight train where he shared a car with two hoboes. The brakeman found them and took Ewing's watch and money; he persuaded him to return them by explaining that he was on his way to college and needed them. Later he was attacked by the hoboes. He got away from the train by pretending to be a homicidal maniac, was hit with a blackjack, and after a long cross-country chase, hid in some brambles in a churchyard and escaped. He lost his blanket roll and most of his clothes, but still had his $10 and his watch. He felt he was too scantily clad to board a street car but persuaded the police to drive him to Rice.* The ingenuity, the persuasiveness, the physical toughness, and the courage are typical of the mature Ewing. Clearly the boy was the father to the man.

In his early days at Rice, Ewing earned money by working in an all-night drugstore; he used to say that his main duties were to take coffee and sandwiches to the call girls who lived in the hotels around the old Humble Building. Later he left the drug store and took part-time jobs assisting with classes and in the library. This brought in about $34 per month. It must have been a hard life, but at the beginning of his third year at Rice he was able to say, in a letter to his parents: "Well, because of the grades I made last year, I was invited to a banquet of the Houston Philosophical Society ... and I sure aim to go."

It was, I suppose, at Rice that he acquired his lifelong habit of working most of the night as well as all day. He also showed his interest in teaching and gave much time to coaching fellow students. His sister Lucy has described how during vacation he would stand over her while she played the piano, insist that she do it right, and explain the background of the piece.

*This story is taken from a letter M. Ewing wrote to his parents just after the event.

At the Rice Institute he at first majored in electrical engineering but later changed to physics and mathematics. Not surprisingly, he found physics, then in the great formative period of quantum mechanics, more exciting than contemporary engineering. He also found physicists more congenial than engineers (the Rice professors of engineering he described as "sarcastic Yankees"). In physics he was greatly influenced by H. A. Wilson, an Englishman and a well-known but unorthodox physicist, who claimed that he was the only one of his contemporaries in the Cavendish Laboratory who did not get a Nobel Prize (it would be interesting to look again at his ideas on nuclear systematics and see if they still look as implausible as they did at the time). Wilson ran a weekly colloquium at which the papers on the "new physics" were discussed as they came out, and where occasionally there would be a talk by a distinguished visitor ("Men," said Ewing, "whom I would otherwise have thought hardly mortal").

At Rice in the 1920's Ewing became a physicist. He learned not only the subject but also the attitude of mind. All his life he preferred simple arguments; his theory was set out in detail, well understood, and carefully explained; his instruments were ingenious and often made by himself without regard for current fashions. He told me that when he was in his late forties he heard a graduate student complaining to another that "Doc" expected him to use a galvanometer: "Never mind," replied the other, "all these old men will soon be dead."

During the vacations at Rice he worked in a grain elevator and later with an oil prospecting crew in the shallow lakes of Louisiana; this was his first introduction to underwater geophysics. It was an exciting time, when gravity and seismic measurements were revealing the salt-domes against whose sides the oil of the Gulf Coast fields is trapped. While still an undergraduate he wrote his first scientific paper (1926), entitled "Dewbows by Moonlight," which describes a rainbow seen on the dew-covered grass of the campus.

While at Rice he played the trombone in the marching band. There he was seen by a fellow student, Avarilla Hildenbrand; as she afterwards told it: "When he came striding down the street working his trombone slide in and out, my heart stood still. He was my man." They were married in 1928.

Ewing obtained his B.A. in 1926. It is curious that H. A. Wilson then advised him that he had no aptitude for experimental work and should stick to theoretical physics. Rarely has a professor given worse advice. Ewing started graduate work in the Physics Department at Rice in the fall of 1926 and obtained an M.A. in 1927 and a Ph.D. in 1931. His Ph.D. thesis, entitled "Calculation of Ray Paths from Seismic Travel–Time Curves," was reported in two papers with Don Leet (1930, 1932a). The topic is central to much of Ewing's later work. Refraction seismology was not, at that time, well understood; there was, for example, a curious controversy as to whether the refracted ray went straight up and down or was refracted along the interface at the critical angle. A sound and detailed knowledge of the ray theory of propagation in a layered medium was critical for the seismic investigations of the next twenty years, and it was a fortunate chance that led Ewing so early in this direction. Regrettably, the collaboration with Leet, who was Director of the Harvard seismological station, broke down with bitter feelings on both sides. Ewing regarded this quarrel as having an adverse effect on his career in the thirties. However, jobs and grants were scarce for everyone, and I was never convinced that Leet's disparagement had as much effect as Ewing believed.

THE 1930'S SEISMOLOGY AT SEA

In 1929 Ewing became an Instructor in Physics at the University of Pittsburgh, but a year later moved to a similar position at Lehigh where he remained till 1940. He had a heavy teaching load in elementary physics but at once started to develop research in geophysics. The work of the next few years is not of great interest; it consists of a variety of projects, some of them suggested by local industry; for example, the paper on prospecting for anthracite (1936a) and one on locating a buried power shovel (1938d). The main theme, however, is the understanding of the methods of small-scale seismology with explosive sources (1932b, 1934a,b,c,d, 1935, 1936c).

The change came in November 1934, on the day on which he was visited in his seismic truck at Lehigh by Dick Field and William Bowie. They came to suggest that he might interest himself in applying the seismic method of prospecting to the study of the continental shelf. Bowie was Chief of the Division of Geodesy of the Coast and Geodetic Survey, very much a member of the Establishment and something of a southern gentleman. R. M. Field was a Harvard man and a professor of geology at Princeton. He was a major eccentric, but he was also the man whose vision and enthusiasm started the bandwagon of marine geology on its triumphant course (for brief accounts of his life see Hess 1962 and Bullard 1962). He had largely founded and was Chairman of the American Geophysical Union's "Committee on the Geophysical Study of the Ocean Basins." He had a pretty clear idea of what he wanted done and why, as can be seen from the first report of his committee (Field 1933). I can easily visualize the meeting with Ewing, since I was taken by Field to see Bowie on a similar errand in 1937. Field would have been persuasive, persistent, talkative, and irrepressible, while Bowie would have lent an air of solidity and charm; together they would have been irresistible, particularly when they offered funds and ships. I do not know what made Field approach Ewing; it is likely that Field had heard him talk at the American Geophysical Union (1931, 1934a). For Ewing it was what he wanted above all else, a problem worth tackling and the possibility of support and facilities.

It was decided that the first project would be to shoot as many refraction seismic lines as possible spaced out between Cape Henry on the east coast of Virginia and the edge of the continental shelf 120 km out to sea, where the depth of water was about 100 m. This line was to be extended inland by measurements on land between the coast and the outcrop of basement rocks 120 km inland. The start was not propitious; the Coast and Geodetic Survey allowed Ewing and his two assistants (A. Crary and H. M. Rutherford) to embark in their ship *Oceanographer* (the yacht *Corsair* given to the survey by H. P. Morgan). Immediately before sailing, the captain was injured in a motor car accident, and an assistant, who was to have helped Ewing, was killed. The ship was fully occupied with surveying, and Ewing's work had to be fitted in while she was anchored at night. Shots were fired with seismographs on the bottom; this gave experience in handling the gear at sea, but no geological information was obtained. In the time available only reflection shooting could be attempted, and not surprisingly, no identifiable reflections were received from the basement.

The work convinced Ewing that the job could be done. On 1 July 1935 he wrote home: "I got proof that the measurements can be made at sea ... the people sponsoring the work ... think they can get the ship of the Scripps Oceanographic Institute for our exclusive use. If so we can clean up an important job in a few months. This is by far the most important project with which I have yet been connected. It is so arranged that I see no possibility of anyone stealing the credit from me." The anxiety about the credit for the work is typical of one side of his character; he was having a hard struggle to get established and could hardly believe that something would not go wrong.

When *Oceanographer* returned to port, Ewing set about the observations on the land section of the line. This was a task that his previous experience had made familiar. Meanwhile Field exercised his persuasive powers on Henry Bigelow, the Director of the Woods Hole Oceanographic Institution. He obtained the use of the R. V. *Atlantis* for two weeks. She was a steel-hulled ketch, 43 m in length over all and with a displacement of about 380 tonnes. She had sails and a diesel engine; the sails were often used, not only for propulsion but also to reduce her tendency to roll. The crucial work was done in this vessel in October 1935. Her Master was Fred McMurray, a very skilled and experienced seaman. On the first day Field, Columbus Iselin, and Henry Stetson accompanied Ewing's party on a short trip to test the gear. Four days later Ewing, Crary, and Rutherford set off for a two-week cruise. At each station a seismograph measuring the vertical component of the motion was lowered to the sea floor from the anchored ship on an insulated electric cable. Signals from the instrument were transmitted up the cable to a recorder in the ship. Charges of explosive were lowered from the ship's boat at distances of up to 11 km from the ship. The instant of explosion was transmitted to the ship by radio; the time of transmission of the wave traveling through the water gave the distance. Four refraction lines were shot on the Cape Henry section and three on a line running south from Woods Hole.

The object of the investigation was to study the nature of the transition from the ocean to the continent. Is the "shelf break," where the sea floor suddenly turns down from the shallow water of the continental shelf to oceanic depths, a fault in the basement, or is it the edge of a rubbish tip of sediments built out from the land over sunken continent or, perhaps, over ocean floor? Where is the true edge of the continent? In what sense has it an edge? These questions are fundamental for geology, and it is remarkable that they had never seriously been approached before. There were, of course, speculations based on the results of drilling on the exposed part of the shelf, but no one had had the skill or the enterprise to attempt what Ewing did.

He discovered a pile of sediments 3800 m thick. The work is a classic example of a discovery of great practical importance made in searching for knowledge. All the oil obtained from the sea floor comes from sedimentary basins like that discovered by Ewing. He told me that about 1936 he had approached an executive of a large oil company and asked for support for the work. He was told that there was no shortage of oil and that the company was not in the least interested in looking for it at sea.

Ewing's reputation was made—he had done something new and of first rate importance. The work was, of course, preliminary. It was open to the criticism that too little shooting had been done; the time–distance curve at the outermost station had only two points on it through which two lines were drawn by using seismic velocities extrapolated from stations nearer shore. To most people these were details which time and further work would remedy. Ewing's own reply to enquiries about how he could be sure with so few data was: "That's how you tell the men from the boys." To Leet however it was not so; he published a slashing attack on the whole operation and its conclusions (1937).

Ewing had expected that Field and his geological friends would seize on the information and produce interpretations in terms of structure and history. It did not happen, though his first paper (1937) was followed by one by B. J. Miller (1937) which was supposed to discuss and explain the results; it is a rather dull piece of work which sets out possible views and leaves the main questions undecided. Ewing, whose own paper was strictly factual, was surprised and perhaps a little disappointed. He had not, I think, realized how complete was the gap in knowledge represented by the ocean floor. For generations the oceans had been a place where geologists could safely deposit many of their difficulties; almost nothing was known and almost anything could be assumed.

Ewing decided to ignore the criticisms and to use what ship time he could get for other projects but to continue the study of the shelf sediments on land (1939c, 1940b). Further work on the shelf at sea was done in 1940 and 1943 but was not published until 1946; the most striking result of this later work was the discovery of seven km of sediment beneath the delta of the Orinoco (1946c, 1948b).

Work at sea continued on a wide front. Even before the first seismic work was published he had started gravity measurements in the U.S.S. *Barracuda* in collaboration with Harry Hess of Princeton (another protégé of Field), who had made similar measurements in the U.S. submarines S21 and S48 in 1928 and 1932. They borrowed the pendulum apparatus devised by Vening Meinesz and used it to explore the gravity low that he had found over the Puerto Rico trench in 1926. They found that it ran around the island arc of the Lesser Antilles and was clearly analogous to the low found by Meinesz around Indonesia (1937a, 1938e). Ewing used a quartz oscillator designed and constructed by W. A. Marrison of the Bell Telephone Laboratories to time the pendulums; this was an important improvement on the use of a spring-controlled chronometer, which was liable to change its rate on diving.

Gravity measurement, at first in submarines, later in surface ships, was a life-long interest on which he published many papers, most of them in collaboration with Joe Worzel (1950h, 1952g, 1954b, 1956d, 1966i).

Work was started about 1939 on the design and construction of a deep-sea camera (1946a, 1967c). A quotation from

Ewing's paper (1946) well illustrates his attitude to such things. He describes the failure of previous rather half-hearted attempts at photography in deep water and concludes: "The principal problems in underwater photography are not optical. . . . The problems are to find an interesting subject, and to put the camera in focus with it, to provide proper illumination, to hold the camera reasonably steady while the exposure is made, and to get the camera back afterwards" (p. 308). In this work and in deep-sea seismology the problems of making watertight equipment for use in deep water were faced for the first time. This involved considerable difficulties and a good deal of development. The published photographs (1944, 1945, 1946a,c, 1967c) are outstandingly clear; they were obtained in depths of up to 730 m. Ripple marks were found in a depth of 150 m in the Gulf of Maine, later they were found to be common in oceanic depths. This was of considerable interest, as most geologists had supposed that ripple marks in sediments were a sign of shallow water. Actually little harm was done by this assumption, since most sedimentary rocks found on land have been formed in relatively shallow water or, at any rate, not in oceanic depths. Ewing's camera was the prototype of all subsequent deep-sea cameras, the results from which have given a detailed view of the ocean floor which could have been attained in no other way; they have been of great assistance in understanding the results of dredging and coring.

After the initial success of the seismic work on the continental shelf Ewing decided that the most important thing to do was to extend the work to deep water. The prize was great: it should be possible to find how much sediment there is on the ocean floor (if the oceans have existed, much as now, through the whole of geological time there should be many kilometers of sediment). One might also hope to obtain an indication of the nature of the basement beneath the sediments, to estimate the thickness of the crust, and to find the depth to the Mohorovičić discontinuity, if it exists beneath the oceans.

The methods employed on the shelf were hardly practicable, the ship could not be anchored and no electric cable was available to bring the signals from the sea floor to the ship. Ewing (1938b, 1946c) first tried stringing the gear along a steel cable. From the ship the cable led down to the sea floor where it carried a watertight pressure vessel containing a four-channel oscillograph; further along the cable were four geophones and three bombs fired by a clock and a battery in the pressure vessel. I was fortunate to be present at the trials of parts of this equipment in *Atlantis* in 1937. It was a somewhat hazardous and a very difficult undertaking. The ship frequently dragged the whole string along the sea floor which prevented any record from being obtained; if cable was paid out to prevent the dragging the whole thing would pile up on the sea floor instead of lying in a straight line. There were many other difficulties, one of the most troublesome was the failure of explosives to go off at depth. When I was with him, Ewing decided that cast TNT might be better than the flake TNT, which was all we had on board. I said: "Maurice, we haven't got any and there's nothing we can do about it." He looked at me and smiled and said: "Don't you think perhaps . . . ." We melted the flake TNT in an electric coffee pot and poured it into molds made by folding paper. Ewing was a wonderful improviser; he had a pressure vessel for the recorder made from an oxygen cylinder with the top cut off; it did not last long, after it had been lowered for a test of watertightness, the wire came up carrying only the eyebolt which had been welded to the cylinder. Some believed that a large fish had eaten the cylinder. In his laboratory at Lehigh he had a pressure vessel to test equipment; it was made from an old fourteen-inch naval shell which Al Vine had found in an army junk yard. On one occasion he was testing blocks of TNT which were supposed to be strong enough to protect detonators from the pressure; he noticed that the pressure was rising rather rapidly and decided that the TNT had caught fire in the press. I do not remember what he did, but when he told me about it he did not seem greatly concerned. As a young man he sometimes appeared rash but, in fact, he knew what he was doing and was quick in making a sensible decision. Behind the large, rather shambling bear there was a very acute mind and a tremendous drive and determination to get the job done.

The attempt to shoot seismic lines in deep water with this equipment was unsuccessful (1946c). In view of the difficulties the scheme was abandoned and a new method tried in 1939 and 1940 (1938b, 1946c). In this the instruments and the explosive charges were sent to the bottom attached to balloons filled with thirty gallons of gasoline and with no wire to connect them with the ship, an idea suggested by the work of Auguste Picard. At the conclusion of the experiment ballast was dropped and the recorders, geophones, and firing clocks were returned to the surface by the buoyancy of the balloons. Ewing also used such balloons to recover a free-falling camera (1946c). I do not know if he was aware of the long and largely unsuccessful history of such devices going back to the seventeenth century (Hook and Moray 1667, Deacon 1971).

Preliminary work was done in shallow water around Bermuda in 1939; this gave the thickness of the coral cap. In 1940, a record of one shot at one geophone was obtained at each of two stations in depths of 2600 and 4800 m. The velocity of P-waves in the sediment was determined, but, according to the published papers, no indication of the basement beneath the sediment was found (1946c, 1948b); George Woollard, however, tells me that one recording did show a wave refracted from beneath the sediments.

The difficulties that prevented geologically significant results from being obtained in deep water could have been overcome, and, when the work was stopped in 1940 by the development of the war, it was clear that a method of seismic shooting in deep water was available. The surprising thing is that in 1937 none of us realized that these heroic expedients were unnecessary. All that was needed was to put the instruments and the explosives near the surface of the sea and to treat the water as another layer in the problem. There are, however, circumstances in which it is desirable to have seismographs and other equipment on the sea floor without wires to a ship; for these Ewing's method has been widely used in very sophisticated forms in recent years (without the hazardous gasoline-filled balloons). One of these instruments was developed at Lamont; it was deployed on the sea floor and recorded on land through a cable. It could be left on the bottom for a month or more (1961m).

During the whole of the period up to the war both funds and ship-time had been meagre and difficult to get. It is perhaps inevitable that institutions such as the Coast and Geodetic Survey and Woods Hole Oceanographic Institution should have regarded work on quite new lines as a thing to be fitted in among their regular business. Ship-time for seismic shooting had to be taken from other projects which had already been planned and which were clearly worthwhile. It was, in a way, a generous gesture to let Ewing share *Atlantis* for short periods with other projects. In fact he got forty-five days of shared time in the five years from 1935 to 1939. Clearly the availability of ship-time was the limiting factor in what could be accomplished.

The work at sea and the preparations for it involved an enormous expenditure of effort. Ewing and his students regularly worked far into the night and disrupted their home lives to a degree which was, perhaps, tolerable for the students but was damaging for Ewing. The work at Lehigh has been described by Woollard:

> It was a tight little group, and although we worked most nights on instruments or data analysis, and spent most weekends in the field, one night a week was devoted to relaxation. We'd start with spareribs and beer in a cheap little German restaurant, migrate up to the University rifle range for a couple of hours' shooting, and then end up at either Ewing's house or my apartment for more beer, music, and discussions . . . followed by scrambled eggs and coffee in the wee hours before calling it a night.

No doubt it was all great fun but it is not a recipe for a happy married life, and, when the strains and absences of wartime were added, Maurice and Avarilla parted and were divorced in 1941. Their son Bill, who was born in 1932, lived with his mother; he became a captain in the Air Force and was killed in an aircraft crash while in his thirties. Shortly before he died he was stationed near Lamont and he and his father got to know each other again.

Overworking was probably inevitable if worthwhile results were to be obtained with so little ship-time and money, but it was also a marked trait in Ewing's character. He was driven by an inner urge to compulsive overwork. He believed that every opportunity must be seized and exploited to the full. He seemed to feel that the world was against him, but was always sure that he and his band of students and friends would overcome the difficulties and show that with small resources they could achieve the apparently impossible. His confidence in his own ability and in the effectiveness of his students was one of his most endearing characteristics, but was not always appreciated by others.

THE WAR, 1940–1946

In 1940 Ewing became convinced that the United States would become involved in the war with Germany and that the

Navy would need his kind of knowledge and skills. He obtained leave from Lehigh, who made him an Associate Professor when he left (he had been made an Assistant Professor in 1936). He went to the Woods Hole Oceanographic Institution where he was a Research Associate from 1940 to 1944. Allyn Vine and Joe Worzel, who had worked with him at Lehigh, moved with him. Woollard followed later.

He and his group got to work with great speed. Even before government finance had been found for the work, they and Columbus Iselin, the Director at Woods Hole, had written a manual for the Navy entitled *Sound Transmission in Sea Water* (there is a copy in the Woods Hole library) and had redesigned and greatly improved the bathythermograph, which had been devised some years before by Athelstan Spilhaus. After a month or two they were, as Worzel put it, rescued from starvation and "practical socialism" by contracts from the Bureau of Ordnance and the Bureau of Ships of the U.S. Navy.

Ewing's style of work had an electrifying effect on what had been a rather slow-moving marine biological station. In his unpublished memoirs Iselin wrote: "He had a profound effect on the success of this laboratory. He arrived here first as a very young professor.... He brought with him several Lehigh students and the place has never been the same since. They literally worked night and day, and seven days a week."

The wartime investigations of Ewing and his group are described in a paper published after the end of the war (1946c), and a list of some of his reports is in National Defense Research Committee (1946). For a few months they were able to continue the refraction shooting on the continental shelf and in deep water. Soon, however, more pressing matters needed all their attention. Among the things studied was the "bubble pulse" from explosions. It had been known since 1898 that multiple shocks were produced by the detonation of a single charge underwater. The phenomenon had also been noticed by Ewing while doing seismic shooting in Louisiana during his vacations from Rice. The cause had been correctly stated by the discoverer (Blochmann 1898) to be the collapse and rebound of the gas bubble, which overshot its equilibrium size, collapsed to a small radius and expanded again to produce a shock wave of intensity comparable to that of the original explosion. The explanation had been lost sight of and to both the American and British navies the phenomenon was something of a mystery. It was of importance as the bubble pulses can substantially increase the damage from underwater explosions. Ewing obtained pressure–time curves and arranged for H. E. Edgerton of MIT to take underwater photographs of the bubble (1946c, 1948b). These showed that it performed nonlinear oscillations during which it collapsed to a very small volume. Ewing obtained an empirical relation between the time interval between pulses and the size and depth of the charge. The theory was worked out by Chaim Pekeris and provided a correction to the empirical formula. It is curious that the explosives group at Woods Hole led by E. Bright Wilson was doing closely similar work at the same time (Arons 1948). Dr. A. B. Arons tells me that security was so tight at Woods Hole that he had only a vague idea that Ewing's group was working on the same matters as his own. It is a salutary example of the dangers of an excessive regard for security between groups in an organization. The whole thing was done yet again at about the same time by H. F. Willis and G. I. Taylor in England.

Perhaps the best-known work by Ewing during the war was his discovery and exploitation of the low-velocity sound channel in the ocean, which occurs at depths of 700–1300 m and is known as the SOFAR channel (an acronym for Sound Fixing and Ranging). The channel may be looked on as a pipe along which sound is repeatedly reflected, or as a wave-guide in which sound waves are trapped. Thus, if an explosion is made near the depth of minimum sound velocity, the sound will spread in two dimensions instead of in three and can reach great distances before its intensity falls below that of the ambient noise. In one of Ewing's experiments a charge of a few pounds dropped off the west coast of Africa was heard off the Bahamas. The phenomenon has obvious applications, and a network of SOFAR stations has been in operation for many years. A related matter is the seismic "T phase" which Ewing showed to be propagated across the ocean in the SOFAR layer (1950f,g, 1952d, 1953c, 1957e).

The propagation of sound in the sea is a more complicated phenomenon than might be expected. The gradients of temperature, pressure and salinity bend the rays and produce shadow zones and focusing effects. Such matters are of importance in submarine detection and in the operation of submarines. Some work had previously been done by the Coast and Geodetic Survey but it had not been published and its importance was not appreciated by the Navy. The work at Woods Hole greatly clarified the subject; much of the information relevant to geophysics was subsequently published as a book (1948b, c). This book also contains an important paper by Pekeris on the theory of sound transmission in the sea.

All through the war Ewing kept fairly closely to the subjects in which he was an expert; in these he made very substantial contributions. He seems not to have had any wish to enter into questions of more general policy concerning the conduct of the war, he certainly had no desire to set himself up as an *éminence grise* to any military or political figure. In this he differed from many of his contemporaries on both sides of the Atlantic.

In 1944 he married Margaret Kidder whom he had met at Woods Hole; they had four children: Jerome, Hope, Peter and Margaret.

THE LAMONT GEOLOGICAL OBSERVATORY AND *VEMA*

In 1944 Ewing was invited to join the Geology Department of Columbia University as an Associate Professor (he became a full Professor in 1947). He accepted and moved there in June 1946 bringing many of his group with him. At first they worked in great congestion in a few rooms in the Schermerhorn Building on the main campus in New York, but in 1948 the widow of Thomas Lamont, a well-known banker, offered the University his estate at Palisades, a few miles from Columbia across the Hudson River. A fund of $250,000 was included with the gift. The University offered the place and the money to Ewing. Just at this time he and his group had a similar offer of a country mansion and financial support from MIT. They were tempted but decided to stay at Columbia; in this Ewing was influenced by the presence of W. H. Bucher in the Geology Department and by the friendliness of General Eisenhower, then President of Columbia.

To get the Lamont estate was an opportunity that might never occur again. Ewing hastened to make the decision irreversible by moving in equipment (the move has been entertainingly described by William Wertenbaker [1974a,b]). It was a lovely place with a fine house and 155 acres of grounds; in a few years the house was full and several new laboratories were built. It no longer looks like a gentleman's residence, but it is still a wonderful place, with open space and trees, set in a village away from the stresses of New York City.

The new institution was, rather oddly, named the Lamont Geological Observatory. At first it was part of the Geology Department of Columbia University. In the early sixties it became an independent institution within the University. Ewing had the title of Director from 1949. A description of Lamont in its early days has been given by George W. Gray

(1956). During the first few years Ewing and Paul Kerr, who was Chairman of the Geology Department, succeeded in raising substantial funds from mining companies, the Rockefeller Foundation, and others. By 1969 these amounted to about $1,000,000. In 1968 Ewing went to see Mr. A. C. Newlin of the Doherty Foundation in search of a grant of $700,000; much to his surprise he was offered $7,000,000. Columbia accepted and in 1969 the name of the institution was changed to the Lamont–Doherty Geological Observatory.

The space and the funds which were now available were the basis of the remarkable developments of the period after 1949. A large part of the running costs came from the Office of Naval Research (ONR). As in so many fields ONR showed an enterprise, responsiveness, and good sense in the allocation and management of research funds which have not always been characteristic of apparently more relevant bodies. I suppose that it is, in theory, undesirable for the Navy to handle the bulk of the funds for civil research, but things have never been the same since their activities were restricted, and they did a wonderful job for Ewing.

In 1953 Ewing and Worzel returned from the Royal Society's discussion on "The Floor of the Atlantic Ocean" to find that the Navy had cancelled an arrangement to supply Lamont with a ship. They hastily hired the iron-hulled schooner *Vema* (62 m overall, 1010 tonnes displacement, propelled by sails and a diesel engine). For a small extra payment they also got an option to purchase. It is not easy to persuade a university to pay $150,000 for a rather antiquated sailing ship, and Ewing and Worzel only succeeded a few hours before the option ran out.

The decision to purchase *Vema* was crucial; it provided Ewing with a ship of his own which he could use how and where he needed it. For many years *Vema* was the center of

Lamont's seagoing activities. She was larger than *Atlantis* and could operate worldwide. She spent a very large proportion of her time at sea and did all kinds of marine geophysical work in the Atlantic, Indian, and Pacific Oceans. Having her made an enormous difference to the amount of work that could be done; from the end of the war until recently ships were harder to get than money, and if you had a ship you could usually get the operating expenses from ONR, the National Science Foundation, or some other agency. The possession of the ship was particularly important for the development of instruments and equipment where it is difficult to estimate the time that will be needed. A couple of failures to make a new instrument work will rapidly disenchant an institution that lends you ships, but if you have your own ship and some money, you need not be so embarrassed by the difficulties and delays of doing new things.

In 1962 *Vema* was joined by the *Robert D. Conrad* built by the ONR. She is 63.4 m overall and has a displacement of 1360 tonnes.

From the foundation of Lamont, Ewing was in a new position; he was now the Director of what rapidly became a large and diverse organization working in many fields, in some of which he would not have claimed to be expert. The wonder was the extent to which he was expert, did know what was going on, and was an all-pervading influence. It is not possible here to describe the work of Lamont as a whole and attention must be concentrated on the parts that were central to Ewing's interersts.

SEISMOLOGY AT SEA

Early in 1949 Ewing fulfilled a long-felt wish; he got two ships at once (*Atlantis* and *Caryn* from Woods Hole). Worzel went out in one and Hersey in the other in the hopes of finding the depth of the Mohorovičić discontinuity (the

Moho) under the deep sea by shooting a long seismic refraction line. It had long been suspected from the results of gravity measurement that, if the Moho existed at sea, it would be much shallower than it was on the continents, but there were several other possibilities. A reversed line 56 km in length was shot and the Moho found 5 km beneath the ocean floor (1949d, 1950d). Ewing and the others were cautious since the seismic velocity beneath the Moho was a little lower than it usually was under the continents; it was, however, pretty clear that they had a result of first-rate importance. It was now almost certain that the familiar basement rocks of the continents were absent beneath the oceans, or at any rate beneath the place between New York and Bermuda where the line had been shot. In fact it has proved a universal rule that the ocean floor is quite different from the continents, the Moho is very shallow, and the sub-Moho seismic velocity is usually near the continental value (1955a). The exploitation of this method of studying the crust beneath the floor of the deep sea became one of the main tasks of Lamont during the 1950's and 1960's (e.g., 1952a, 1953b, 1954a, 1956m, 1959c). There was, however, another means to the same end.

The wartime work on sound in the ocean, and especially the cooperation with Chaim Pekeris on the theory of the propagation of waves in a layered medium, turned Ewing's thoughts to the propagation of surface waves across the oceans. This is more complicated than the interpretation of the records of P and S waves from explosions; since the wave length exceeds the vertical dimensions of the layers, "ray optics" is inapplicable, and the solution of the wave equation is essential. The waves are dispersive, and thus their phase and group velocities are different; they are of two main types, Rayleigh waves and Love waves, which have different dispersion relations. In return for this complexity it is possible to obtain average properties of large areas of the earth. Refraction shooting gives a picture of the layers under the line of shots, the dispersion of the surface waves from an earthquake will show whether the results from the refraction shots are typical of a whole ocean. The resolving power is poor but the area covered is large.

The study of surface waves had been a favorite topic with theoretical seismologists since the beginning of the century. It was known that the dispersion curve for waves crossing the Pacific was different from that across the continents. This implied an important difference of structure in the upper 10–50 km and was consistent with the shallow oceanic Moho suggested by gravity observations. The observations of surface waves traveling across the Atlantic had been interpreted by Beno Gutenberg and Charles Francis Richter (1936) as indicating a structure intermediate between those of the continents and of the Pacific. Ewing saw that here was a tool which needed development and which could provide a short cut to the study of some of the major features of ocean basins.

His attack on the problem was both experimental and theoretical. Seismographs were installed in a vault at Lamont; the instruments had to be capable of recording much slower oscillations than those used for recording the "first arrivals" of P waves. Such instruments had been neglected, since most designers were more interested in improving the sensitivity at periods of 0.1–3 s rather than in the more difficult and apparently less rewarding task set by periods above 10 s. This was an example of the parsimonious and lopsided development of seismology which became so apparent at the meeting of experts on bomb-test detection in Geneva in 1958. These difficulties caused the injection of many millions of dollars per year into the subject, via the Advanced Research Projects Agency of the Department of Defense project "Vela Uniform," and rapidly transformed it. The work done at Lamont on instrument design during the 1950's (1958a) was an important base for the improved facilities and for the World

Wide Seismic Network. Later, instruments were developed that were capable of recording the natural oscillations of the Earth at periods between 10 s and 1 h. These periods of oscillation are now a major source of information about the interior of the Earth.

A series of papers by Ewing and Frank Press, starting in 1949, took up the theory of surface waves using more realistic models of the ocean floor than had been used previously (e.g., 1950c, 1955b). It was shown conclusively that the Atlantic was similar to the Pacific and not halfway to being a continent (the previous, erroneous view had arisen from an underestimate of the effect of the water on Rayleigh waves and from the absence of observations of short period Love waves). Ewing's first deep-water refraction station was typical of the oceans; this was a result of great importance, not only in itself, but in encouraging the more realistic use of surface-wave dispersion in the study of the Earth's crust and upper mantle. The method was applied in a series of papers by Ewing, Oliver and Press to elucidate crustal structure in many parts of the world (1955k, 1956h, 1959b); this work confirmed the universal difference in crustal thickness between the oceans and the continents which is one of the basic facts of geology. The work was summarized in 1956a and the theory in a book (1957a).

The work with earthquake records gave Ewing great satisfaction; he sometimes said that he wished that he could give up being the director of an institute and spend his time reading seismic records. The seismic work at sea was also near the center of his interests. Here he was stimulated by keen competition from Russell Raitt and George Shor at Scripps and from Maurice Hill at Cambridge; Ewing got the first measurement of the depth of the Moho, but occasionally they had the pleasure of discovering something before he did; for example, he missed the layer with a P wave velocity of about 4½ km/s which lies beneath the sediments almost everywhere in the oceans. The oversight was probably due to having the shots too far apart in a laudable desire to get a station finished and get on to the next and also to the wish to have enough explosive left for some more stations.

Neither the refraction lines nor the study of surface waves could give any detail about the stratification or structure of the sediments of the ocean floor. For this it was necessary to observe reflections from small discontinuities in the sedimentary column. What was needed was, in a sense, an improvement in the echo sounder with more power and a lower frequency to give penetration into the sediments beneath the ocean floor.

Ewing had tried to observe such reflections as early as 1935 but had not obtained any useful results. Work on the improvement of reflection shooting started at Lamont in 1949. At first the ship was hove to, and a single hydrophone was lowered to record the shots (1949a). Later observations were taken under way. Progress was slow (for an account of work up to 1960 see Hersey 1963), and it was over ten years before really good, continuous seismic profiling was achieved. The success was largely the work of John Ewing, Maurice's brother.

In the early 1960's a single hydrophone was towed behind the ship and 0.2 kg charges were thrown into the sea every two minutes. The charges were attached to balloons to prevent them sinking to a depth where the bubble pulse would occur. The operator had to tuck the balloon under his arm, light the fuse, and throw the charge and its balloon into the sea. To do this hour after hour on a rolling ship in the middle of the night is a tedious operation and not without its dangers. In 1961 a man was killed in *Vema* and about a year later the method fell into well-deserved disuse. By then several other types of sound sources such as the "sparker" and the "air-gun" were available. The sparker produces sound by an underwater spark, and the air-gun is a container filled with air at a pressure of 150 atm which is suddenly released through a valve.

Much new information was obtained during the 1960's about the sediment of the deep ocean (1962g, 1963f, 1964g, 1965f,l, 1966a,l, 1967l, 1968c). A quite unexpected discovery was the widespread occurrence of a conspicuous reflector named Horizon A. From the results of drilling, Horizon A is now known to be an Eocene deposit of hard amorphous silica, similar to the chert or flint found on land (1970l). One of Ewing's most spectacular seismic discoveries was that the Sigsbee Knolls in the Gulf of Mexico are salt-domes (1966e, 1968d). Drilling has given indications that there are hydrocarbons trapped in the surrounding sediments, but it would be imprudent to drill for oil in such depths till techniques of control have been developed. Not striking oil has become an important objective in the planning of deep-sea drilling.

The techniques of reflection shooting at sea are of great importance to the oil industry. During the 1950's and 1960's they developed methods for use on the continental shelf where a string of several hundred hydrophones is towed behind a ship. Because of the cost, the use of these methods by academic institutions did not become common until 1972. The technique is elaborate; it involves digital recording on as many as forty-eight channels and processing by computers at sea and on land. The results are spectacular. The use of such equipment in the Gulf of Mexico was Ewing's main scientific interest in the last few months of his life. The equipment could produce 30 million bits of information per kilometer, so that even he must, at last, have felt that he had as many data as he could use.

TOPOGRAPHY AND SEDIMENTS OF THE OCEAN FLOOR

Seismology was Ewing's first love, but he and his students pursued many other lines of investigation with an equal enthusiasm. The most basic tool of marine geology is the echo sounder. This was developed about 1914 by R. A. Fessenden and the Submarine Signal Company who used audio-frequency sources; in the 1920's ultrasonic instruments were produced which use frequencies of 10–30 kc/s. These instruments worked admirably, particularly after the introduction of a variable density recorder depending on the electrolysis of paper impregnated with potassium iodide. They had, however, unreliable timing arrangements depending on a centrifugal governor or on the frequency of the ship's electrical supply. The effect of this was catastrophic; a ship would sail across the Pacific with an echo sounder recording say 400 m too shallow, the soundings would then appear on charts as a ridge along the ship's track. At one time the charts of the North Pacific were crossed by several of these bogus ridges following ships' tracks in a roughly east–west direction. Ewing and his colleagues undertook the design of a Precision Depth Recorder, the PDR (1954e). This was based on the facsimile recorders used for transmitting photographs for newspapers. It had electronically controlled timing and paper that gave permanent records. Such instruments are now universally used in survey and oceanographic ships; they give a timing accuracy equivalent to about 1 m in depth.

The main stimulus to the development of the PDR was the desire to study the abyssal plains which stretch beyond the foot of the continental rise at depths of around 5000 m. The PDR showed how extremely flat they are; gradients of less than one in a thousand are common. Samples collected from the plains showed coarse sands, shallow water fossils, and bits of wood, which strongly suggested that the material was derived from the continental shelf. R. A. Daly (1936) had suggested that during the Pleistocene ice ages, sediment was stirred up by waves breaking on the exposed continental shelf and that the muddy water ran down the slope eroding the canyons. P. Kuenen, in Holland, had made laboratory experiments which suggested that a cloud of sediment dispersed in water can indeed run rapidly and turbulently down the slope and spread sediment over the ocean floor. He called these clouds of sediment and water "turbidity currents." Ewing set out to investigate the abyssal plain off the eastern seaboard of the U.S. He suspected that the 1929 earthquake on the Grand Banks had set off a turbidity current and showed (1952i) that the failures of submarine cables suggested that some cable-breaking agency was set off by the earthquake and propagated down the slope at speeds of up to 90 km/h. When he showed that there was coarse and apparently recent sediment at the foot of the slope and pointed out that long lengths of some of the cables had been carried away and buried, most people were convinced of the reality of turbidity currents as the carriers of the sediments of the abyssal plains. Later similar phenomena were found off Sicily and in other places.

In the course of the work on the abyssal plains the canyons that cross the continental edge were traced far beyond the slope over the plains. They had levees on each side and were clearly formed by some process involving flow from the canyons. The discovery of the deep extensions to the canyons made it improbable that they had been cut by subaerial erosion at a time when the land stood higher or the sea lower. The process by which canyons are formed is still not clear, especially when they are cut in hard rock, as are some of those off southern California.

Going further out to sea Ewing naturally became interested in the mid-Atlantic ridge. Here he, Heezen, and Tharp found that the deep depression, which was known to occur on many echo sounder profiles near the crest of the ridge, was a continuous valley (1956k, 1960i). It gradually became apparent that it was a worldwide feature of the mid-ocean ridges (except along the East Pacific Rise), that it always runs near the shallowest part of the ridge, that it is displaced where the ridge crest is displaced on what are now called "transform faults," and that it has steep sides which are pretty clearly fault scarps. This discovery was entirely unexpected and has proved central to the development of tectonic theory. It was a result of Ewing's policy of keeping any ships he could get going back and forth across the ocean measuring anything that could be measured, collecting anything that could be collected, and not worrying too much about anything except getting to know the ocean floor. It is remarkable that he was able to find a major topographic feature which all the hydrographic departments and research ships of the world had missed. They had all been across the central valley many times but had not seen that it differed from all the other valleys on the ridge in being continuous.

It had long been known that earthquakes occur on the mid-Atlantic ridge and the more recent studies of Gutenberg and Richter (1941) and of Rothé (1956) had shown that they were concentrated in a narrow belt near the crest. The uncertainty of location was perhaps 100 km, and Ewing suggested that they all actually occur in the central valley and that their distribution could be used to trace the course of the ridge and its central valley in the long sections where there were no adequate lines of soundings. These ideas and some additional lines of soundings (e.g., 1960i) enabled him to demonstrate the worldwide extent of the ridge and the valley (though on the East Pacific Rise there are earthquakes but no valley).

The topographic studies were accompanied by the collection of sediments in coring tubes. The art of coring had been revolutionized by B. Kullenberg's piston corer which used the hydrostatic pressure to prevent the core jamming in the barrel as it goes into the bottom. This machine had been used with great effect by Hans Pettersson during his *Albatross* expedition of 1947–1948; it increased the length of core that could be taken from about 3 to 30 m.

Ewing became an obsessive collector of cores. To examine a core in detail is a lengthy operation; for a core of deep-sea ooze it involves carbon-14 age determinations on the upper parts and separating, identifying, and counting foraminifera over the whole length. For a core of red clay it requires paleomagnetic studies, chemical analyses, and γ-ray counts to determine uranium and its daughter products. Ewing collected cores at a rate greatly in excess of the rate at which they could be examined in detail. He split them lengthwise, looked at them all in a rough way, had a few studied in detail, and put them all into storage. Understandably the people who were paying for the operation became restive. Why did he collect so many? Ewing replied that when he found two that were alike he would consider slowing down the rate of coring. The real reason, I think, lay deeper. He once said to me: "I go on collecting because now I can get the money; in a few years it will not be there any more, then I shall have the material to keep my people busy for years" (I do not remember the exact words). In fact the Lamont collection of cores is an invaluable and almost inexhaustible mine of information about the floor of the deep sea.

A related investigation concerned the particles suspended in the ocean water which might be expected to throw light on the processes of sedimentation (1963e, 1965d,h, 1967n, 1969c,i,r, 1970g).

The picture of the western Atlantic which emerged during the 1950's from the work at Lamont was paralleled by work by others in the eastern Atlantic and in the eastern Pacific. Some sort of order and system gradually emerged, and it became clear that the geology of the oceans must be studied in its own terms and not as an appendage to continental geology. The way was now clear to extend the discoveries to the whole ocean; that is, to two-thirds of the Earth's surface. In this Ewing and Lamont played a leading part and made many important discoveries, particularly in the western part of the South Atlantic. The work in the North Atlantic was summarized in a masterly book by Heezen, Tharp, and Ewing (1959k).

In 1952 Ewing (1953e) made a technical advance which was of an importance comparable to that of the introduction of refraction seismic shooting at sea. He took the airborne, fluxgate magnetometer, which had been developed during the war by Victor Vacquier for submarine detection, and towed it behind a ship. This was the start of a great enterprise which is still in progress and whose results are the main basis of the recent development of ocean-floor tectonics. The instrument is troublesome to use; it drifts, it is cumbersome, it has moving parts: it has now been replaced by the proton magnetometer introduced in sea work by Maurice Hill. It was, however, Ewing who first got the bandwagon rolling and whose example led to the surveys of Mason and Raff off the coast of California which revealed the zebra-like pattern of magnetic lineations (for the pre-1960 history of magnetic measurements at sea see Bullard and Mason, 1963).

SEA-FLOOR SPREADING AND PLATE TECTONICS

By 1960 the general nature of the sea floor had, in large measure through the work of Ewing and his colleagues, become clear. The shelf, slope, rise, abyssal plains, abyssal hills, ridge, and central valley were all understood in a descriptive sense, as Ewing, Heezen, and Tharp had shown (1959k) and as was shown on a larger scale in the collective work edited by Hill (1963, but mostly written in 1960). These works take the features one at a time, describe them, and give what may be called their local history. Behind this, however, there were the most important questions. What was the history of the oceans? How had they been formed? Had they always been there? These questions are not seriously approached even in

Hill (1963). However, in the study of the sea floor and in other directions, particularly in paleomagnetism, a considerable head of steam was accumulating which, in the early 1960's, ripped apart what had become the established views of most geologists, at any rate in the northern hemisphere. The critical questions were: what is occurring along the central valley and why are the ocean floors so young (no sediments older than 150 Ma had been found but many samples of all younger ages)? The outcome is well-known and the route to it has been described by many authors. During the 1960's it was established beyond doubt that the oceans are young because they have been formed recently and that ocean floor is being formed today in the central valley of the mid-ocean ridge along the line of earthquakes that Ewing discovered there. The data from which all this was established came, in large part, from the work at Lamont, but the initial steps in the great synthesis did not (though Heezen was teetering on the edge of the ideas).

The course of Ewing's thoughts on these matters is not easy to trace in his papers. He was not given to sweeping generalizations about large-scale processes; he believed in the accumulation of information about the sea floor and that the major discoveries are made at sea. After it became clear that there were no buried continents beneath the oceans he believed that the oceans had always been where they are today. Gray (1956) quotes him as saying:

> We have every reason to believe that in that 2000 feet of unconsolidated sediment [on the ocean floor] the whole history of the Earth is better preserved than it is in the continental rocks. ... As we punch deeper into the ocean sediments we may reach levels holding traces of the first animals that concentrated calcium carbonate, then evidence of atmospheric oxygen from the earliest green plants, and ultimately the primeval sediment of the earliest erosion, marking the advent of water in the sea. [This does not sound like Ewing's conversation and is, presumably, a summary.]

Just at this time the permanence of the relation between continents and oceans was being questioned by workers in paleomagnetism. There are occasional references to arguments against continental drift in Ewing's papers in the 1950's (e.g., in 1952e he said that if America had moved away from Europe there would not be time for isostasy to be reestablished).

In fact, Ewing took remarkably little part in the controversy that raged between 1955 and 1965. He probably thought that work at sea would make all such things clear, as in fact it did. At the first international oceanographic conference, held in the United Nations Building in New York in 1959, Ewing gave the first of the invited talks. He talked on "Shape and Structure of Ocean Basins." I waited, fascinated, for him to commit himself on these matters, but he said very little about them and in the published account (1961g) there is no reference whatever to the wider questions. In *The Sea* there is a review article by Heezen and Ewing (1963l), in which it is said that there is tension beneath the central valley which may be accommodated either by compression of the continents or by expansion of the earth (the latter view was held by Heezen but not by Ewing).

About 1964, following the publication of papers by Hess and by Vine and Matthews, a number of the younger workers at Lamont began to examine their magnetic data from the new point of view and became convinced of the reality of seafloor spreading. It is remarkable that Ewing not only allowed but encouraged James R. Heirtzler, Neil Opdyke, Lynn Sykes, and others to pursue this investigation and to publish views that were basic to the subject on which he had spent his whole life but were contrary to his own beliefs. His openmindedness led to what was, perhaps, Lamont's greatest success.

Ewing had always insisted that data and cores should be properly stored and catalogued and that all data from a given area should be available to anyone working on that area. In most other institutions data were regarded as the private property of the man who collected it or of the chief scientist of the cruise; whoever had it worked it up, published it, and kept it in ways and places of his own choosing. Lamont's policy of communal data storage gave them a two-year lead. They had it all available and in a very short time published a series of papers on magnetic lineations, the focal mechanisms of earthquakes, and the paleomagnetism of deep-sea cores which established the reality of plate tectonics.

A number of papers (1966c, d, m, n) written early in 1966 show Ewing deeply concerned about sea-floor spreading and impressed by the evidence but finding it unacceptable, at any rate for the Atlantic. He pointed out (1966d) that there were places in the northwest Pacific where Cretaceous sediments appeared at the surface and where the thickness was such that it could reasonably be supposed that sediments going back at least to the Triassic were present. There were also other difficulties, some specific, such as the discovery (1966c) of Miocene sediments in the central valley (they were probably from a transform fault and not from the central valley, without a detailed survey it is easy to confuse the two), some matters of general principle, such as the lack of variation of heat flow across the ridge (there is, in fact, a variation of the expected kind; Ewing [1966m] used a considerable body of Lamont data but, because of the high probability of damaging the equipment, had taken none in or close to the central valley; he ignored results from workers elsewhere).

I believe that he became convinced of the essential correctness of the "drifters and spreaders" views by the end of 1966. In November of that year a meeting was held at the Goddard Institute for Space Studies in New York. Just before the meeting started Ewing came up to me, looking, I thought, a little worried, and said: "You don't believe all this rubbish do you?" I admitted that I did, and I fancy that the following two days of systematic exposition, largely by his own students, convinced him (he did not contribute to the published proceedings of the meeting). He still found the ideas too simple and too uniformitarian. In this he was clearly right; quite complicated things have happened. Rates and directions of spreading have changed in the past, though the long intervals of no spreading that he later suggested in the Atlantic and Indian Oceans seem not to have occurred. I think his initial difficulties were due to knowing too much. If you have in your mind an enormous data bank, there is sure to be some fact that appears to contradict any general theory. You then become very wary of all general theories.

CAUSES OF ICE AGES

Starting in 1955 Ewing and W. L. Donn published a series of papers setting out a new theory of the causes of ice ages (1956g, 1958d, 1959d, 1961a, 1963g, 1964a, 1965a, 1966h, 1968i, 1971d). The problem is of long standing and has two aspects: first, why has there been a series of ice ages and interglacials during the past two million years and at various earlier periods and, second, why are such groups of ice ages separated by intervals of perhaps 100 Ma with no ice ages? Ewing believed that the ice cover in the Arctic Ocean is unstable and subject to occasional melting (for the mechanism of the instability see 1956g). When the ice melts, absorption of the Sun's heat and evaporation are increased, precipitation on the Arctic land masses is greatly increased, the snow cover lasts through the summer, absorption of radiation is reduced, and an ice sheet builds up. This part of the theory is given an added interest by the recent thinning of the ice in the Arctic Ocean and the possibility that within one or two generations we may be faced by the beginnings of a crisis that, both politically and technically, we are in no state to face. It would seem prudent to put a substantial effort into the study of these matters by drilling in the Arctic seas.

The second half of Ewing and Donn's theory is that the occurrence of ice ages depends on the pole being situated in an ocean and that polar wandering and continental drift will cause this to occur intermittently at intervals of the order of 100 Ma. Here there is a difficulty in that the pole is at present 700 km from the nearest land and cannot have entered the Arctic Ocean as recently as 2 Ma ago. Such a shift would imply that the pole moved relative to the land at a speed of 35 cm/a which is too high to be credible. Again, what we need is a detailed climatic history of the late Tertiary in the Arctic which could be obtained by drilling and might show that the recent sequence of ice ages goes back further than is usually supposed.

LUNAR SEISMOLOGY

A major interest of Ewing's later years was lunar seismology. This was a joint project between a number of institutions, but the instrument development was done mainly at Lamont–Doherty. Ewing took a close interest in the instrumental work and also in the interpretation of the puzzling and unexpected records, which show oscillations continuing for tens of seconds instead of the sharp arrivals usual on terrestrial records (1969m, 1970a,i,m, 1971o,s, 1972g,j,k, l,m,o, 1973g,i,j,k, 1974d,e,h, 1975b). The propagation of these waves is, perhaps, somewhat similar to that of the SOFAR and T phase signals which Maurice had discovered long before.

OTHER INVESTIGATIONS

It is not possible here to describe the full range of subjects that, at one time or another, caught Ewing's interest. The titles of the papers will indicate them. In seismology there is a series of papers on the interaction of seismic and atmospheric waves (1951b,e, 1952b, 1953a, 1967m, 1971x), another series on microseisms (1948a, 1952l,m, 1953f, 1956l, 1957c), three papers on the propagation of elastic waves in ice (1934b,c, 1951f). There are also five papers (1958g, 1960l, 1962c,d, 1963b) on the effects of nuclear explosions, five on petrology (1969j,k, 1970b,c, 1971p) and others on heat flow (1965c, 1966n) and paleontology (1959i).

THE MOVE TO GALVESTON

The relation between an American research institute, such as the Lamont–Doherty Observatory, and the university of which it forms a part is a delicate symbiosis. The university gains prestige, a small amount of undergraduate teaching, and the supervision of a large number of graduate students. Financially it will usually come near to breaking even, the overheads on the outside contracts balancing the direct payments to the institute from the general income of the university. Once it is a going concern the institute needs the university, not primarily for financial reasons but to attract graduate students; students need Ph.D.'s and only a university can give them. It is easy to see how this relation can go wrong; the administration of the university feels that it has responsibility, but in practice little control over an organization which has its own finances and which will, if it comes to a fight, have wide support in the scientific community. On the other side, any encroachment from the central administration will be taken by the institute as interference by people who are contributing little and are activated by motives of self-aggrandisement.

Such a confrontation gradually developed at Lamont–Doherty and came to a head in 1972. After the student riots, Columbia found itself in a difficult financial situation; Ewing believed that the new President, William McGill, was not only trying to enforce a stricter control over his activities, but was also attempting to take a part of the Doherty money for general university purposes. The details are complicated and it is not necessary to go into them here. Such a dispute was difficult for Ewing who all his life had half felt that things would, somehow, sometime, go wrong. He retired from Columbia with a month's notice and left Lamont, as did Joe Worzel, James Dorman, and Gary Latham. He would have reached the retiring age in 1973 and would then have had to retire as Director, though he could, presumably, have stayed on as a professor.

In June 1972 Ewing moved back to his home state of Texas and became Cecil and Ida Green Professor at the Marine Biomedical Institute of the University of Texas (now the Marine Science Institute) at Galveston. He hoped to develop marine geophysics at the Institute and to keep a close collaboration with Columbia in scientific matters; to encourage this he became a Research Associate at Lamont–Doherty and went there for short visits every few months, staying in an apartment that had been made from his old office. In the words of his successor, Manik Talwani: "He probably did more scientific work here on those visits than he did during the last year before leaving for Texas."

In Galveston, Cecil Green, himself a distinguished geophysicist as well as an outstanding industrialist, and his wife not only provided a professorship but also part of the cost of a ship, the *Ida Green.* Their generosity was a great support to Ewing at a critical time; it enabled him to get his work going again with hardly a break. Green told me that, just after the move, he asked Ewing whether he would be happy in a small institution with a director who was a medical man and a biologist; Ewing replied: "Of course, look at all these smiling faces, that's what matters." The parting from Lamont had been a bitter and deeply disturbing experience for him, but once it was done I think he was genuinely glad to be clear of the troubles of Columbia and to be at sea again in a small ship with a group of friends and students working on a well-defined objective.

The objective was the study of the Gulf of Mexico by the methods of reflection seismology. For this purpose the *Ida Green* was fitted with the latest 24-channel seismic equipment with digital recording. He lived to see the first results (1975a), but on 28 April 1974 he suffered a cerebral hemorrhage and died, on 4 May, without regaining consciousness. He was within eight days of his sixty-eighth birthday.

PERSONALITY AND ACHIEVEMENT

Ewing was, in a sense, a devoted family man. His love for his family shows very clearly in a recording made immediately after an accident in *Vema* in January 1954. The ship was 300 km north of Bermuda in a gale with mountainous seas. Ewing, his brother John, and the first and second mates were securing some drums of lubricating oil which had broken loose. A freak wave caught them unawares and all four, with the oil drums, were washed overboard. The Captain of *Vema* directed the rescue operations from the masthead, and Captain McMurray, the old friend who had been Captain of *Atlantis* in the thirties, maneuvered the ship. Thanks to his skill and long experience all but the second mate were rescued, Ewing by a very narrow margin. He was left with a slight limp and minor effects of internal injuries for the rest of his life. Next day he had recovered sufficiently to record a message which was sent to his children and was afterwards published (1954r, publication was due to an error in the Lamont office). It is a message from a man who has come through a harrowing experience, is not sure if he is going to live or if he will be paralyzed, and wants to send a message to his family while he still can. Its theme is that he had only survived because of the feeling that he must get back to the family and children he loved and that it was only their love that had saved him. About the genuineness of his feelings there can be no doubt—everyone who knew him well has testified to it; yet, in practice, he was unable to spare sufficient time to keep his first two marriages afloat. His daughter Margaret has described how, to see something of him, she used to walk back to his office with him after dinner and then go home through the dark grounds when her bedtime came. In 1965 he and his wife parted and were divorced; shortly afterwards he married Harriett Green Bassett who had been his secretary at Lamont. She continued in her job after marriage; this must have had certain disadvantages, but, with a lessening of Ewing's habit of working through a large part of the night, it did at least enable her to see more of him than had her two predecessors.

Ewing was not a committee man, but he would devote substantial time to organizations and causes that he regarded as important. First among these was the Navy to whose well-being he was deeply attached. He was on the Board of Governors of Rice University (1969–1972); Vice-President (1953–1956) and President (1956–1959) of the American Geophysical Union; Vice-President (1952–1955) and President (1955–1957) of the Seismological Society of America; and Vice-President of the Philosophical Society of Texas (1973–1974). He was, for a time, on the National Academy of Sciences committee responsible for the ill-fated Mohole project and took a large part in its enormously productive successor the Deep Sea Drilling Project, of which Lamont–Doherty was one of the five founding institutions. Ewing and Worzel were co-Chief Scientists on Leg 1.

Ewing had a passionate interest in the oceans; along with this went a desire to teach people about them. He was a great teacher, not in the formal sense of being skilled in classroom instruction, but in the way he could teach by example how to discover things. He spent much time at sea making things work, untangling greasy cables, looking at records, and deciding what to do next. He never asked anyone to do what he would not have done himself, and in fact he could and would do almost anything. He once told me that the pendulum apparatus he was taking to some island, perhaps Bermuda, was not quite finished, but that the ship had a lathe and he would finish it on the way. The long line of his distinguished students is testimony not only to his effectiveness as a teacher but also to his personal qualities which attracted them and kept so many of them at Lamont in a period when many superficially more attractive jobs were available. I knew him intermittently for thirty-seven years; to me he was uniformly friendly, welcoming, and amusing. He delighted in elaborating stories of the early days until no one knew what had really happened. I can imagine that, if you wanted something that he wanted for his own purposes, he could be a hard and difficult man, but I never saw it.

Ewing and his group discovered more new things about the Earth than any other group has ever done before. He himself was primarily interested in finding what was there. Lamont was set up for this purpose, "Observatory" was, perhaps, the right name; the emphasis was on data gathering and on its immediate interpretation and not on global theory. His success did not come merely from intelligence but from deeper gifts of character which enabled him to set up an effective organization of the kind he needed. As I was writing this a student from Cambridge came back from a month in *Vema*. I asked him how he found the ship, expecting complaints about her smallness and inadequacy. Instead he replied: "Superb, there's nothing like her anywhere. It's all so well run, you can get twice as much done as you can in any other ship." *Vema* was the center of Ewing's life and with her he discovered the nature of two-thirds of the Earth's surface. The last time I met him I asked him where he kept his ships. He replied: "I keep my ships at sea."

IN WRITING this notice I have had unstinted help and advice from Ewing's family, friends, and colleagues; though it is only fair to say I have not, in all instances, taken the advice. It is impossible to mention all by name, but I am specially grateful to his widow, Harriett; to his ex-wife Margaret; to his sisters Mrs. Rowena Peoples and Mrs. Lucy Clawson; to his brother John; and to his early students, A. P. Crary, Allyn Vine, George Woollard, and Joe Worzel. I am conscious that I have done an injustice to Ewing's colleagues at Lamont in that I have ascribed to him discoveries that were the result of the joint efforts of many people. I hope that the names in the bibliography will indicate the extent to which Lamont was a scientific commune. To have made it so was one of Ewing's achievements.

I wrote the original version of this notice in 1975 while Hitchcock Professor at the University of California at Berkeley and while Doherty Professor at the Woods Hole Oceanographic Institution. I revised it for the National Academy of Sciences while working at the Scripps Institution of Oceanography and at the University of Alaska.

The letters and unpublished documents on which this memoir is based have been deposited in the archives of Columbia University. Ewing's own papers are in the scientific archives of the University of Texas at Austin.

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HONORS AND DISTINCTIONS

AWARDS AND MEMBERSHIPS

Guggenheim Fellow, 1938
National Academy of Sciences, Member, 1948
Geological Society of America, Arthur L. Day Medallist, 1949
Philosophical Society of Texas, Member, 1953
Guggenheim Fellow, 1953, 1955
National Academy of Sciences, Agassiz Medal, 1955
U.S. Navy Distinguished Public Service Award, 1955
American Academy of Arts and Sciences, Member, 1951
Royal Netherlands Academy of Sciences and Letters, Foreign Member (Section for Sciences), 1956
American Geophysical Union, William Bowie Medal, 1957
Argentine Republic, Order of Naval Merit, Rank of Commander, 1957
Society of Exploration Geophysicists, Honorary Member, 1957
American Philosophical Society, Member, 1959
American Institute of Geonomy and Natural Resources, Inc., John Fleming Medal, 1960
Columbia University, Vetlesen Prize, 1960
American Geographical Society, Cullum Geographical Medal, 1961
Dickinson College, Joseph Priestley Award, 1961
Rice University, Medal of Honor, 1962
National Academy of Sciences, John J. Carty Medal, 1963
Geological Society of London, Foreign Member, 1964
Royal Astronomical Society (London), Gold Medal, 1964
Swedish Society for Anthropology and Geography, Vega Medal, 1965
Academia de Ciencias Exactas, Físicas y Naturales (Buenos Aires), Corresponding Member, 1966
Third David Rivett Memorial Lecturer (C.S.I.R.O., Australia), 1967
Indian Geophysical Union, Honorary Fellow, 1967
American Association of Petroleum Geologists, Sidney Powers Memorial Medal, 1968
American Association of Petroleum Geologists, Honorary Member, 1968
Saint Louis University Sesquicentennial Medal, 1969
Geological Society of London, Wollaston Medal, 1969
Sociedad Colombiana de Geología, Honorary Member, 1969

Royal Society of New Zealand, Honorary Member, 1970
Royal Society (London), Foreign Member, 1972
Rice University, Alumni Gold Medal, 1972
Robert Earll McConnell Award—American Institute of Mining, Metallurgical and Petroleum Engineers, 1973
Royal Astronomical Society (London) Associate, 1973
Houston Philosophical Society, Member, 1973
National Medal of Science, 1973
Canadian Society of Petroleum Geologists, Honorary Member, 1973
First Sproule Lecturer, University of Alberta, 1973
Distinguished Achievement Award for the Offshore Technology Conference, May 1974
American Geophysical Union, Walter H. Bucher Medal for 1974

HONORARY DEGREES

Sc.D., Washington and Lee University, 1949
Sc.D., University of Denver, 1953
Sc.D., Lehigh University, 1957
Sc.D., University of Utrecht, 1957
Sc.D., University of Rhode Island, 1960
Sc.D., University of Durham, 1963
Sc.D., University of Delaware, 1968
Sc.D., Long Island University, 1969
Sc.D., Universidad Nacional de Colombia, 1969
Sc.D., Centre College of Kentucky, 1971
LL.D., Dalhousie University, 1960

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This list is based on one kept by Ewing, on the Lamont–Doherty list of publications, and on my own collection of his papers. It includes only material published in books, journals, and conference proceedings. Things not generally available, such as reports to government agencies and grant-giving bodies, are excluded, as are *NASA Preliminary Science Reports* and reports with a security classification, even if they are now declassified (a list of war-time reports will be found in the now declassified National Defense Research Committee [1946]). Regretfully I have had to exclude published abstracts from the list; they are often interesting and frequently are not followed by papers. As they sometimes precede the corresponding papers by as much as four years they are of importance to those interested in priority of discovery.

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1930

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1931

L. D. Leet and M. Ewing. Velocity of explosion generated waves in a nepheline syenite. Trans. Am. Geophys. Union, 12th meeting: 61–65.

1932

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d. M. Ewing and A. P. Crary. Study of emergence angles and propagation paths of seismic waves. Physics, 5:317–20. Also in: Bull. Am. Assoc. Petrol. Geol., 5(1935):154–60.

1935

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1936

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b. M. Ewing and H. H. Pentz. Magnetic survey in the Lehigh Valley. Trans. Am. Geophys. Union, 17th meeting:186–91.
c. Seismic study of Lehigh Valley limestones. Proc. Pa. Acad. Sci., 10:72–76.
d. Frequency of water waves. Fld. Eng. Bull. U.S. Cst. Geod. Surv., 10:65.

1937

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b. M. Ewing, A. P. Crary, and H. M. Rutherford. Geophysical investigations in the emerged and submerged Atlantic coastal plain. Part I: Methods and results. Bull. Geol. Soc. Am., 48:753–802.
c. Science in the deep. Lehigh Alumni Bull., 24(5):8–9, 19.

1938

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e. F. Press and M. Ewing. Propagation of explosive sound in a liquid layer overlying a semi-infinite solid. Geophysics, 15:426–46.
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h. J. L. Worzel and M. Ewing. Gravity measurements at sea, 1947. Trans. Am. Geophys. Union, 31:917–23.
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f. F. Press and M. Ewing. The ocean as an acoustic system. In: *Symposium on Microseisms,* Publ. Nat. Res. Counc. Wash., D.C., no. 306:109–11.

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c. M. Ewing and J. L. Worzel. Gravity anomalies and structure of the West Indies, Part II. Geol. Soc. Am. Bull., 65:195–200.

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g. M. Ewing and F. Press. An investigation of mantle Rayleigh waves. Bull. Seismol. Soc. Am., 44:127–47.

h. B. C. Heezen, D. B. Ericson, and M. Ewing. Further evidence for a turbidity current following the 1929 Grand Banks earthquake. Deep-Sea Res., 1:193–202.

i. C. B. Officer and M. Ewing. Geophysical investigations in the emerged and submerged Atlantic coastal plain. Part VII: Continental shelf, continental slope, and continental rise south of Novia Scotia. Geol. Soc. Am. Bull., 65:653–70.

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