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HOW I BECAME AN OCEANOGRAPHER AND OTHER SEA STORIES

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Let us now praise famous men
Men of little showing
For their work continueth
Broad and deep continueth
Great beyond their knowing

Rudyard Kipling, *Stalky and Co.*

BECOMING AN OCEANOGRAPHER

When I went to Pomona College as a 16-year-old freshman in 1925 I thought I wanted to be a journalist. While attending high school in Pasadena I had been on the editorial staff of the student paper and had had a good time. But in my sophomore year of college I took a course in elementary geology from an inspired teacher, Alfred O. Woodford. On the first or second session of the class he took us all up to Indian Hill, a flat-topped erosional remnant about 2 miles north of the college. It rises some 50 feet above the surrounding alluvial fan that debouches from the nearby San Gabriel Mountains. When the class had gathered around him he asked, "How did this hill get here?" This was a question none of us had thought to ask, let alone answer. Then he said, "Look around you, and tell me what you see and think."

Actually, Woody's question was by no means simple. Its precise answer is not clear even today, although a fairly reasonable explanation of the origin of Indian Hill and other similar features was given by Rollin Eckis

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(Woody's all-time prize student) in his classic paper, "Alluvial Fans of the Cucamonga District, Southern California" (Eckis 1928), which was written while Rollin was still a Pomona College undergraduate. After we had puzzled over Woody's question for half an hour he gave us several partial alternative answers, explaining that he was not sure himself as to which of these, if any, was correct.

I still remember the final examination in this course. It consisted of a series of quotations from a book entitled *The New Geology* by a religious fundamentalist, George Price. We students were requested to comment on these quotations in the light of what we had learned. I remember I answered that it all dependend on whether one accepted Lyell's doctrine of uniformitarianism—that the laws of physics have always applied everywhere, and consequently the present is the key to the past. From that time onward I gave up all thought of becoming a journalist.

Woody took a sabbatical leave during my junior year, leaving me to the tender mercies of two cold-blooded professors of physics and mathematics. As a hangover from my previous attachment to journalism I had become editor of Pomona College's so-called funny magazine, the *Sagehen*, and in the middle of the fall semester I was drafted to become co-editor of the student newspaper, together with a fraternity brother of mine, Murray Putnam. The paper was in the process of being turned from a weekly into a daily; after a few months the previous editor had had a nervous breakdown. Murray and I usually got to bed about 3:00 A.M. My physics class was at 7:30 in the morning. Needless to say, I did very badly, and no better in the calculus class during the next period.

I learned many years later that when Woody got back from his sabbatical he found the physics professor pushing hard to flunk me out of college. Through heroic efforts, he managed to avoid this. But it resulted in a permanent break between him and the physics professor.

Woody was the sole member of the Pomona Geology Department, and he could teach only a limited number of geology courses. He traditionally sent his students to Berkeley for graduate work in geology; usually they were given a teaching assistantship. This happened to me in 1930/31. There were lots of graduate students in Bacon Hall, the old brick geology building next to the Campanile. Nearly all of thcm had taken their undergraduate work at Berkeley. Many did not have teaching assistantships, even though they had obviously learned more geology at Berkeley than Woody could teach me at Pomona. I brooded for some time about the injustice of this situation. But I finally decided that I deserved that teaching assistantship, for Woody had taught me three things: a knowledge of the vocabulary of geology, so that I was able to read the literature easily; a recognition that in spite of that literature, little was actually known about the Earth and its history, but that it was possible through research to find out a good deal; and that geological research was wonderfully exciting and very good fun. My fellow students at Berkeley had learned so many geological facts that they had missed these three fundamentals.

In those days, Berkeley was—as it still is—a marvelous place. I have worked for the University of California during most of my adult life, but mostly in La Jolla, about as far from Berkeley as one can get and still be in the state. Thus, whenever I go to Berkeley a chill runs up and down my spine from the sheer joy of being there. The best parts were the old redwood faculty club and the chemistry buildings, both very close to Bacon Hall. The great thermodynamicist Gilbert Newton Lewis was head of chemistry; he spent part of nearly every afternoon in the faculty club playing cribbage or kriegspiel while we students watched in awe.

In the first half of this century a significant number of professors of chemistry in the United States, including Harold Urey, Joseph Mayer, Frank Long, and many others, had taken their PhD's at Berkeley under giants likc Lcwis, Latimer, Hildebrand, Eastman, Giauque, Branch, and others. I took the elementary course in chemistry. The lectures were given by Joel Hildebrand, and the other full professors acted as laboratory assistants. This was one of the great experiences of life. Hildebrand never mentioned the peculiar properties of arsenic or lead, or any other element. He was concerned entirely with the principles of chemistry, such as the laws of mass action and of ionic dissociation.

Several of us graduate students had desks in the large library room of Bacon Hall. Off this library was a private office occupied by the pioneering Berkeley geologist Andrew Cowper Lawson—usually called, for good reason, "the King." With his walrus moustache and fierce looks he seemed to us the very model of a curmudgeon. Several times a day he would stomp in and out of his office without saying a word to any of us. He was said to have a high-pitched voice, and there were many stories about him, mostly slightly indecent, among California geologists. He had recently married a woman 50 years his junior, the daughter of his old friend, Dr. Collins, the Canadian Government's chief geologist. For all I knew, he might have been a gentle soul underneath his crust.

The chairman of the Geology Department was George D. Louderback, usually called "Uncle George." Just after World War I he had played a major part in the faculty revolt at Berkeley that established the almost omnipotent role of the Academic Senate in the University of California. Uncle George was a native of San Francisco; he spoke a New Englandish dialect that was said to be nearly San Franciscan. He had a large office with many tables, all of which were covered by about a foot of papers, with a stratification going down to the early Tertiary, but he never had any trouble finding a paper when he wanted it. He faced life and science with a cool skepticism that we graduate students tried our best to emulate. I remember that his seminar in marine sedimentation was a superb example of what a seminar should be, given in a style that I still try to follow in my own classes.

In the spring of 1931 T. Wayland Vaughan, the Director of the Scripps Institution of Oceanography, came to Bacon Hall in search of a graduate student who was willing to spend a year in La Jolla looking at muds collected from the Pacific and Atlantic deep-sea floors by the nonmagnetic yacht *Carnegie*. The ship, which had belonged to the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, had been on a round-the-world combined oceanographic and geomagnetic expedition. Before the voyage was half-finished, she had been destroyed by an explosion in the harbor of Apia, Samoa. (Many oceanographic ships in those days came eventually to a similar bad end.) Fortunately, the samples and data had been shipped back to the United States before the accident, and the mud samples were resting safely in La Jolla.

I applied and was accepted for the job of studying them. My fiancée, Ellen Clark, had been born in La Jolla, and she and I had spent a good deal of time there in her mother's beach house (where, by the way, we live today). Hence, we thought it would be nice to spend the first year of our marriage in these familiar surroundings. In preparation for the work at Scripps, I spent both the summer- and inter-session at Berkeley, twelve weeks in all, taking the four elementary physics courses that ordinarily took two years during the regular college term. These helped to make up for my dismal physics performance at Pomona.

Ellen and I went down to La Jolla in September 1931 to live in the upper story of one of the small wooden cottages on the Scripps campus. John Wells, today a famous specialist on fossil corals, lived downstairs, and a well-behaved but noisy family of skunks lived under the house. This was during the middle of the Depression, and I remember our rent was about $10 a month. We and the couple next door (Horace Byers, later to become a well-known meteorologist, and his wife Frances), shared a maid. We each paid her $6 a month.

After a few weeks Dick Fleming, another research assistant (now Professor Emeritus R. H. Fleming of the University of Washington), came to my lab in what is now the Old Scripps Building and said, "You're the new boy here. Tomorrow morning we have to go to sea. I'll pick you up in front of your house at about 2:30 A.M." Sure enough, he appeared in pitch darkness at the appointed time, accompanied by Maynard Harding, also a research assistant. We drove down to the San Diego Yacht Club at Point Loma, where the Scripps Institution's so-called oceanographic ship—a 64.5-foot ex-purse seiner named *Scripps*—was waiting for us, with her one-man crew, a former railroad engineer named Murdy Ross. (Murdy was used to keeping a steam locomotive in tip-top shape, and that meant covered with grease. He tended to apply the same principles to *Scripps*.)

We sailed out through San Diego Harbor, past the bell and the whistling buoys to an area over 1000 meters deep, some 15 miles west of Point Loma, which we later called the San Diego Trough. Here we stopped the engine and hove to. I learned that this same trip was to be taken every week as part of a year-round study of the ocean conditions off San Diego. We lowered a set of Nansen bottles equipped with reversing thermometers into the water. When the proper depth had been reached a small brass weight, called a messenger, was attached to the wire, dropped through the water to "trip" the bottles, the whole "cast" was retrieved, and the process was repeated for a series of different depths. Along about 11:30 Dick Fleming said "You're the new boy, so you get to cook lunch." I dutifully went down to the galley and cooked what seemed to me a wonderful meal of steak and boiled potatoes, with a salad of lettuce and tomatoes. The three others came down to the galley, bolted their meal in absolute silence, as sailors do, and announced "We're liable to get seasick down here. We'll go back up topside and continue the sampling, and you can stay down here and do the dishes."

About 4:30 the station was completed; we headed back to San Diego, arriving well after dark. I thought it was one of the finest days I had ever spent. I believe I decided then and there, more or less subconsciously, without actually saying so, that I would spend the rest of my life as an oceanographer. Being at the same time a sailor and a scientist just seemed too good to be true. Of course this meant that I would have to spend my life at an oceanographic institution, of which in those days there were only three—the Woods Hole Oceanographic Institution, the Oceanographic Laboratories of the University of Washington, and the Scripps Institution of Oceanography. For a California boy, Scripps was the obvious choice, although Dr. Vaughan and the faculty of course didn't realize it, and I never actually told them. I just stayed.

I believe part of the reason for my decision was the result of a field trip I had taken a couple of years before with Rollin Eckis in the Santa Rosa Mountains at the eastern edge of San Diego County. Like most good geologists, Rollin climbed cliffs and walked along precipices as casually as a mountain goat. I tried my best to follow him but it was often an ordeal, for I was afraid of heights. One of the great things about being an oceanographer was that the only heights to climb were the rope ladders attached to the masts of ships, and on these you could hang on with your hands as well as your feet.

One day Dr. Vaughan (whom we called T. Willey) gave me some calcareous mud from the Bahamas to look at. It consisted mainly of small spherulites, called "oolites," and many small needles, which I was able to identify as aragonite and to describe in my first scientific paper. Thus began my lifelong love affair with calcium carbonate and carbon dioxide. Erik Moberg, Scripps' sole chemical faculty member at that time, had been working with David Greenberg, a biochemist at Berkeley, on the role of carbon dioxide in the buffer mechanism of seawater. Together with a chemical technician, Esther Allen, they had found a discrepancy between the theoretical relationships of carbonate, bicarbonate, free CO_2, and hydrogen ion concentrations and their experimental results. Moberg asked me to look into the reasons why, and after a considerable time I was able to show that the various dissociation products of boric acid played a minor but important part in the buffering of seawater. The four of us published a long paper, "The Buffer Mechanism of Sea Water" (Moberg et al 1934), but we were pretty much preempted by a paper published by four European marine chemists, named Buch, Harvey, Wattenberg and Gripenberg, at about the same time that the paper by Moberg, Greenberg, Revelle, and Allen appeared. Both papers had identical conclusions. What a wonderful law firm those eight names would have made!

During this period, Dick Fleming and I undertook experiments to determine the solubility of calcium carbonate in seawater. We found, as is now well known, that ocean surface waters nearly everywhere are vastly oversaturated with calcium carbonate. (I was later able to show, as did Wattenberg, that the deep ocean waters are undersaturated because of the effect of hydrostatic pressure on the solubility.)

By this time I had become "captain" of the *Scripps*, having taken out a proper small-craft operator's license, and we ventured out beyond Catalina Island 100 miles or so on two- to three-week-long cruises. Besides Murdy Ross, we had acquired another crew member, a cook and general handyman named Frank. In spite of our primitive marine facilities we were also beginning to learn something about the deep North Pacific. Dick Fleming spent several months on the US Naval hydrographic ship *Hannibal* in 1933, and I went on a cruise off northern California in the Coast and Geodedic Survey ship *Pioneer*. In the summer of 1934 I went on another cruise, to the Gulf of Alaska and across the Pacific from the Aleutian Islands to the Hawaiian Islands, on the USS *Bushnell*, the flagship of what was then called the Submarine Force. This was a major naval exercise under the command of a rear admiral, with six submarines (the entire submarine fleet of the United States at that time) and two submarine tenders. My part of it was to take a series of oceanographic stations between the Aleutian Islands and Hawaii. The admiral and I shared a common weakness—we both tended to get seasick when *Bushnell* was pitching into a head sea. The place of minimum motion was right at the fantail, and we spent a good many hours together there. I got along well with the other officers on *Bushnell*, and her captain, later Rear Admiral A. T. Bidwell, urged me to join the Naval Rescrvc, which I did in 1936. From this, many consequences followed. The most important was that I spent seven years, from July 1941 to early 1948, on active duty as a reserve officer, mostly in Washington, DC, but also in the Pacific. Though I never heard a shot fired in anger, I became to a very considerable extent the Navy's oceanographer, involved (in the role of project officer) with many research and development programs in applied oceanography. [See my article "The Age of Innocence and War in Oceanography" (Revelle 1969).]

HARALD SVERDRUP FINDS A SHIP

After considerable, somewhat rude prodding by T. Willey and Jno A. Fleming, the director of the Carnegie Institution's Department of Terrestrial Magnetism, I finally finished my study of the Carnegie muds and was given a PhD degree in the spring of 1936 (Revelle 1944). Having given up on the idea that I should go away, the faculty offered me a position as an instructor at the munificent salary of $150 a month, a considerable improvement over my previous salary of $100 a month. During that summer Dr. Vaughan retired; he was succeeded by the great Norwegian physical oceanographer and geophysicist Harald Sverdrup. Unlike the common notion of Norwegians as blond vikings, Sverdrup was a small, dark, quiet man, tough as nails, but gentle at the same time. He had spent seven years frozen into the Arctic ice on Roald Amundsen's ship, *Maud*, together with four Norwegian shipmates. Amundsen's idea was to repeat Fridjof Nansen's earlier attempt on his ship, *Fram*, to drift across the polar sea with the sea ice, which was believed to move at relatively high velocity. In both cases the ice proved uncooperative, and the ships remained fairly close to where they had started. In *Maud*'s case this was in the Chukchi Sea, west of the Bering Strait. Sverdrup spent most of one year with the Chukchis, a kind of eskimo. He learned what he thought was the Chukchi language. But it turned out that the Chukchis have two languages, one for women and one for men; because he had spent most of his time in the village he had learned the women's language, causing great merriment among the Chukchi men when they came home.

Sverdrup was at that time (and probably still should be considered) the greatest oceanographer of our century. His coming to the Scripps Institution worked a transformation. Before his coming, none of the faculty at Scripps had had much training or even experience as real oceanographers.

Sverdrup was one of the leading products, together with Carl Gustaf Rossby and Jack Bjerknes, of the Scandinavian school—mostly Norwegians, but with some Swedes, Danes, and Finns mixed in—that had literally invented physical oceanography and dynamical meteorology in the modern sense of these terms.

As a young "postdoc" I would have done much better, scientifically, to stay at Scripps and work with Harald. But I was already committed to spend the year of 1936–37 in Norway at the Geophysical Institute of Bergen, headed by another famous Norwegian oceanographer, Björn Helland Hansen.

In September 1936 my wife Ellen and I and our two daughters, one a babe in arms, the other nearly four years old, sailed across the Atlantic to London on a moderately slow boat. Of course there were no trans-Atlantic planes in those days. From London we went by train to Edinburgh, where there was a general assembly of the International Union of Geodesy and Geophysics. I presented a series of papers, detailing some of our work on the distribution of nutrients and other properties in the North Pacific, and met many famous scientists, including Darcy Thompson, Harold Jeffreys, Jack Bjerknes, George Wüst, Joseph Proudman, Björn Helland Hansen (whom I had already met in La Jolla several months previously), Martin Knudsen, Walfried Ekman, Colonel R. B. Seymour Sewell, J. D. H. Wiseman, and Columbus Iselin, who was beginning his remarkable career as director of the Woods Hole Oceanographic Institution. Edinburgh at that time was a dirty gray, depressing city, filled with the acrid smell of burning coal, quite unlike its present charming face. But we were entertained splendidly by the Lord Provost, and Darcy Thompson convinced us that Edinburgh really was the "Athens of the North." I never shall forget the sight of Edinburgh Castle illuminated with searchlights, rising out of the darkness above our heads.

We sailed to Norway from Newcastle; in Newcastle we saw many pigeon-breasted, obviously malnourished young men with very poor teeth, wearing black shirts, followers of the British fascist Oswald Mosley. Those were bad days in Europe, with Adolf Hitler screaming his bloody nonsense over the radio, the on-going tragedy of the Spanish civil war, and some fascists in every country.

Bergen was nevertheless a wonderful place for us. We learned to ski and became very good friends with several of the scientists at the Geophysical Institute and their families. I didn't learn much about oceanography, but I did learn a good deal about people.

During the year we were in Bergen our little boat *Scripps* blew up while loading fuel. Frank, the cook, was killed, and Murdy Ross critically maimed. Harald Sverdrup mourned Frank's death and Murdy's injury.

But at the same time he must have thought of the accident as a blessing, for he was able to persuade Robert P. Scripps, the son of E. W. Scripps and nephew of Ellen Browning Scripps, both of whom had contributed greatly to the Institution in its early days, to buy a new ship for us. This was a Gloucester-type, two-masted, topsail schooner named *Serena*, which had belonged to the movie actor Lewis Stone. Sverdrup renamed her *E. W. Scripps* after Bob's father, cut off the topmasts but kept the other sails and rigging, and installed a more powerful diesel engine. (After seven years frozen in the Arctic ice, Sverdrup had little patience for the romantic side of sailing. During her career as our research vessel, *E. W. Scripps*'s masts became progressively shorter, her sails smaller, and her engines more powerful.) With this ship we were able to go far out to sea and to stay at sea for a month or more without refueling or re-supplying. After the first step of obtaining Harald Sverdrup as Director, the acquisition of *E. W. Scripps* was the second great leap forward in the evolution of the Scripps Institution toward a genuine world oceanographic institution.

When we returned to La Jolla I found that another marine geologist, Francis Parker Shepard, then of the University of Illinois, was spending more and more time at Scripps. Though he was interested in every aspect of submarine geology, Fran was primarily a geomorphologist. He had little interest in or aptitude for theory, but he was a keen and tireless observer. By poring over hydrographic charts, crude as they were in those days, he had discovered or rediscovered the great abundance, large size, and extreme depths of the submarine canyons that cut into the continental shelves off every continent. He hoped to make detailed echo-sounding surveys of the canyons off California, collecting samples of their rocky walls and bottom sediments and measuring water currents within them, in order to understand their origin and mode of formation. During 1938 and 1939 he made a prolonged visit to La Jolla, bringing with him two graduate students from the University of Illinois, Kenneth Emery and Robert Dietz, who developed in later years into famous marine scientists, considerably exceeding the accomplishments of their teacher. More immediately important from Harald Sverdrup's viewpoint was a grant of $12,000 that Fran had obtained from the Geological Society of America—the largest grant the Society had made up to that time—which was mainly to be used to pay most of the costs of operating *E. W. Scripps* for a year. (Today one of the large Scripps vessels costs more than $10,000 *per day* at sea.) With this windfall of money, Shepard, Dietz, Emery, and I, together with several others, embarked on an extensive study of what Fran called the "Continental Borderland," the series of undersea troughs, basins, canyons, ridges, and banks, laced with islands, off southern California.

We found a great variety of bottom types, from the varved sediments of the virtually anoxic Santa Cruz Basin to rocky phosphorite on the flanks of the outer ridges. Because of our crude sampling instruments, we could learn relatively little about the underlying geology of the borderland. In contrast, today's ability to drill for considerable depths into the seafloor might make it possible to learn whether the borderland is a mosaic of shards from distant continents similar to some other parts of the California Coast Ranges.

My fondest memory of this period concerns my first close encounter with Walter Munk, my friend and colleague for nearly 50 years. Scripps had begun a summer fellowship program for undergraduates, and Walter, then an undergraduate at Caltech, was one of our first fellows. Fran Shepard was interested in measuring the bottom currents in the submarine canyon that lies northwest of Scripps. This was accomplished by anchoring a rowboat over the canyon and making current measurements from it with an instrument that had to be raised and lowered, from the bottom to the surface, about 70 meters for each reading. We divided into three teams, each making measurements for four hours in the rowboat and then off for eight hours. Walter and I formed one team. Because he was so cheerful, willing, and enthusiastic, this was a very pleasant experience. He retains those same qualities today.

In 1939 and again in 1940, *E. W. Scripps* made two cruises to the Gulf of California, the longest voyages we had ever undertaken in our own ship. The Gulf was then a wild, remote place, sparsely inhabited by a few Mexican and Indian fishermen. Harald Sverdrup led the first of these expeditions, the second was a geological cruise, led by Charles Anderson of the US Geological Survey, Fran Shepard, and me, together with J. Wyatt Durham of Berkeley, Ken Emery, Bob Dietz, and others. Wc found that the waters of the Gulf are very rich biologically. (Hernán Cortés in the sixteenth century had called it the "Vermillion Sea" because of the red color of the waters during springtime. We recognized that this color results from extraordinarily heavy spring blooms of phytoplankton.) There was a heavy fallout of organic remains on the seafloor during the spring and summer, including numerous tests of diatoms. In the Gulf's many deep basins, where the bottom waters were virtually stagnant and very low in oxygen, this resulted in the rapid accumulation of thickly varved, highly siliceous green and black sediments, stinking of hydrogen sulfide and rich in organic matter. We concluded that we had discovered the conditions of deposition of the famous diatomaceous shales of the Miocene Monterey Formation in California. But we did not suspect what we now believe—that the Gulf was being formed by the splitting of the Earth's crust along an embryonic spreading center.

ATOLLS AND ATOM BOMBS

In the United States, nearly all oceanographers during World War II were deeply involved in research and development related to problems of the war—development of underwater sound equipment for detecting and tracking submarines; development of devices for submarines to help them avoid anchored mines and to protect them against acoustic detection, studies of the effects of variations in ocean temperature, salinity, and bottom conditions on the behavior of underwater sound; methods for predicting surface waves in the deep sea and surf in shallow water; methods for determining shallow-water depths from aerial photographs of shallow-water surf; usc of smokc to protect surface ships from kamikazes; search-and-rescue methods for downed aviators, using what was known about surface currents and the drift of life rafts; and studies of the noises made by marine mammals, fishes, and marine invertebrates that interfered with underwater acoustic devices. These various activities had brought bright graduate students, as well as more senior physicists, chemists, biologists, mathematicians, and engineers into the marine sciences, where many of them stayed after the war.

In 1946 the newly created Department of Defense decided to carry out tests of the effect of atom bombs on naval ships. This "Operation Crossroads" was to be conducted at Bikini Atoll, a remote location in the Central Pacific. One aspect of the tests was a study of the possible effects of nuclear weapons on the atoll itself and its animal and plant inhabitants. I was assigned to the staff of the joint task force commander, Admiral W. H. P. Blandy, to organize a survey of the atoll, which could serve as a baseline for evaluation of the potential effects. This task expanded to include all aspects of the oceanographic problems associated with the tests, particularly the waves that would be generated by the explosions, and the diffusion and fate of the radioactivity that would be released. Most of the oceanographers in the United States spent a good many months in 1946 at Bikini on these various tasks. John Isaacs, Jeffrey Holter, Alexander Forbes, and Allyn Vine were given responsibility for different kinds of wave measurements, ranging from automatic photography from huge towers erected on Bikini Island to rugged underwater pressure devices that were placed on the bottom of the lagoon, directly underneath the location of the bomb blast. Marston Sargent was responsible for the biologists and geologists who spent several months in the spring and summer of 1946 on the old hydrographic survey ships *Bowditch* and *Sylvania* studying the biology and geology of the atoll and the surrounding seafloor. John Lyman, then a lieutenant commander and later chief oceanographer of the old Naval Hydrographic Office, was in charge of the six small ships that were assigned to study the diffusion of radioactivity outside the atoll. Walter Munk, Gifford Ewing, W. S. von Arx, and William Ford studied the currents and diffusion within the lagoon. Kenneth Emery worked on the bottom topography and geology. He discovered a flat-topped submerged mountain, 2000 meters deep, close by the atoll, which he called Sylvania Seamount (Emery et al 1954). (The existence of this seamount, far beneath the sea surface, with its flat, wave-cut summit virtually free of sediment, next to an atoll, is a mystery that becomes harder to understand the more one thinks about it.)

One of the principal scientific consequences of Crossroads was the proof of the hypotheses of Charles Darwin and James Dwight Dana about the origin and history of coral atolls. One hundred and ten years earlier these two scientists had proposed that atolls have a sunken volcanic core that had been eroded by rain, wind, and rivers but not by ocean waves because it was first protected by a fringing coral reef and later, as it began to sink, by a barrier reef. Finally, when the volcano became completely submerged it was covered with an upward-growing platform of shallow-water coral and coralline algae perhaps thousands of meters thick. Such structures could form only where the ocean waters were warm enough for shallow-water reef-forming corals and coralline algae to live and flourish while the volcanic core sank slowly and continuously beneath the sea. Darwin and Dana supposed that high volcanic islands such as the Marquesas and Tahiti must be geologically very young and had not had time to sink beneath the waves. Darwin postulated thc cxistcncc of submerged, wave-eroded—and hence flat-topped—seamounts outside the tropic seas where reef corals could not grow. We now know these as Harry Hess's guyots. The theorems about coral atolls were vigorously disputed over the succeeding century.

In 1947 I organized a resurvey of Bikini, working from my desk in the Navy's Bureau of Ships. One aspect of this resurvey was a program of drilling into the atoll, hopefully down to what we supposed to be its volcanic core. The work was done under the supervision of Harry S. Ladd and Joshua Tracey of the US Geological Survey, together with Gordon Lill, who was attached to my other office in the Geophysics Branch of the Office of Naval Research. It turned out that coring is extraordinarily difficult in the limestone material of the reef; nonetheless, Harry Ladd and his colleagues were able to find successively older strata of shallow-water coral-reef limestone down to the Eocene, some 40 million years ago. Although the volcanic core was not reached on this expedition, it was clear that the atoll was very old, and that Darwin and Dana were essentially right about its origin and history. Seismic reflection studies in 1946 had already shown a marked discontinuity at somewhat greater depths than the drill penetrated, which we assumed to represent the surface of the volcanic core. Later studies by Russell Raitt on our Mid-Pacific Expedition in 1950 showed this surface to be quite irregular, apparently eroded only by wind and running water above sea level, just as Darwin and Dana would have expected (Emery et al 1954).

THE NEW AGE OF EXPLORATION

At the end of World War II not much more was known about the floor of the deep sea than had been described in Sverdrup, Johnson, & Fleming's *The Oceans*, written in the 1930s but published in 1942. The general distribution of deep-sea sediments, basically according to the *Challenger* classification of organic oozes, red clays, and "muds," was known. But it was generally believed that the deep-sea floor was mainly a flat and featureless plain, somewhat shallower in the middle of the Atlantic—the Mid-Atlantic Ridge—and somewhat deeper around the borders of the Pacific—the Mindanao and other deep-sea trenches. One new discovery had been made—the existence of "guyots," or flat-topped seamounts at depths of several thousand meters. Harry Hess, on active duty as a reserve naval officer in command of a naval transport, had found these by watching and listening to his echo sounder as his ship crisscrossed the Pacific between Hawaii and the Philippines. Because of the prevailing notion that the oceans were a permanent feature of the Earth's surface, Harry thought these seamounts must have been islands eroded by waves several billion years ago. Their apparent sinking was probably due to the filling up of the ocean by thousands of meters of deep-sea sediments over billions of years.

In contrast to the stagnation of knowledge about the Earth beneath the sea, the development of instruments and techniques that could be used for deep-sea research had proceeded rapidly during the war. A great deal had been learned about the behavior of sound in seawater, including Maurice Ewing's discovery that a comparatively small signal, emitted at the right depth (for example, the explosion of a 1-lb block of TNT at a depth of 500 meters), could be heard over distances of thousands of kilometers. One of the most important developments was an accurate recording echo sounder that could give a continuous profile of the ocean depth along the ship's track. Sensitive, rapidly responding devices for magnetic field measurements had been developed for submarine detection. Another useful invention was a relatively simple one, the rubber O-ring, which made it possible to encase instruments in pressure-resistant cylinders that became more and more tightly sealed as they were lowered to greater and greater depths.

The wartime research and development effort had brought many physicists into research on underwater sound and other marine phenomena, using the many new developments in electronics. (In the 1930s, electronic equipment was quite unreliable; we oceanographers had a saying that there should be less than one vacuum tube per instrument.) Before the war, the Federal Government had provided only sporadic and meager support for marine science; now the Navy's Bureau of Ships and Office of Naval Research, and later the National Science Foundation, were prepared to finance oceanographic research on generous and liberal terms. And finally, many surplus naval ships were available that were capable of extended high-seas voyages at relatively modest operating costs, of the order of $500 to $800 per day at sea. The combination of circumstances soon made it possible for oceanographers and marine geologists in the United States to join in a worldwide scientific enterprise. This was nothing less than a new age of exploration, comparable in scope and intensity to the great ages of exploration of the fifteenth to the eighteenth centuries. The difference was that the new explorers concentrated on the world that lay beneath the surface of the sea.

Ships and scientists of several nations took part in this great undertaking, including Soviet oceanographers with their large, multipurpose ships—the names of Gleb Udintsev and Vladimir Kort stand out in my memory; Cambridge University geophysicists led by Teddy Bullard and Maurice Hill; and geophysicists of the British National Institution of Oceanography at Wormley, with Tony Laughton. In the United States a highly productive effort was made by Maurice Ewing, with his colleagues Bruce Heezen, Frank Press, and Joe Worzel, at the Lamont Geological Observatory. However, in this account I concentrate on what was most familiar to me, the role of the Scripps Institution near the beginning of the new age of exploration.

Three expeditions were organized by Europeans in the late 1940s: the Swedish *Albatross* expedition, led by Hans Pettersson on board an ancient freighter that had been renamed *Albatross* after Alexander Agassiz' nineteenth century exploring ship; the Danish *Galathea* expedition aboard a similar converted freighter, led by the biologist Anton Bruun; and the British *Challenger* expedition, led by the British geophysicists Tom Gaskell, Maurice Hill, and John Swallow.

The work of *Albatross* and *Galathea* was centered around the use of a single giant winch containing 12,000 meters of tapered steel wire rope, on which sampling and measuring instruments could be lowered to and retrieved from the Pacific trenches, the greatest depths of the sea. *Albatross* used this contraption to collect long cores of deep-sea sediments, employing a new type of "piston corer" invented by the Swedish oceanographer Bjorn Kullenberg. Under the right circumstances, this instrument was capable of taking cores 10 to 15 meters long, more than five times as long as any previously recovered. *Galathea* had used the great winch for a different purpose—to dredge the trench bottoms for animals and bacteria that might be living there. *Challenger*, a much smaller ship, about the same size as its nineteenth century prototype, did not have a great winch, but she used a recording echo sounder and other underwater acoustic devices to good advantage.

At the close of World War II Harald Sverdrup felt that he should return to Norway to help in the postwar reconstruction of the country. This feeling was compounded by his chagrin at not sharing the years of German occupation with his fellow countrymen. In 1948 he resigned as director of the Scripps Institution. Carl Eckart, the great theoretical physicist who had turned his attention from quantum theory and irreversible thermodynamics to the problems of underwater sound propagation in the sea, became the Scripps director, and I returned from my prolonged tour in the Navy as Associate Director.

The directorship was not really Carl's cup of tea; after a couple of years he resigned, and I became Acting Director. One of my first projects in this new position was to organize a deep-sea exploring expedition, the first ever undertaken by the Scripps Institution of Oceanography. This expedition was made possible because Sverdrup had acquired an ocean-going tug, which we called *Horizon*, from the Navy as one of the ships to be used in the California Cooperative Fisheries investigation that he had organized in 1946.

The expedition we had in mind would require two ships to carry out seismic refraction studies of the oceanic crust and the upper portions of the underlying mantle. One ship was to lie still in the water, deploying a series of cable-connected hydrophones. The other, starting about 50 miles away, would set off large charges of TNT as sound signals at regular intervals; it would come close by the listening ship and then proceed for an equal distance in the opposite direction. This work was to be done by Russell Raitt of the Scripps Institution faculty and his associates.

Captain (later Rear Admiral) Rawson Bennett was the Director of the US Naval Electronics Laboratory (NEL) in Point Loma, where we kept our ships. The NEL oceanographers were enthusiastic about participating in the proposed expedition, and it was not hard to convince Rawson that the laboratory's research vessel (which had no name but only some letters and a number—*EPCER 857*) should be the second ship for our expedition.

Our proposed track led from San Diego southwest to the equator, north to the Hawaiian Islands, then southwest to Bikini Atoll, and homeward along a track at about 40°N latitude. The operation was to be called the "Mid-Pacific Expedition," thus starting the Scripps Institution custom of giving each one of its expeditions a distinctive name.

After many months of preparation, the two ships started off bravely toward the equator in June of 1950. Among the scientists on *EPCER 857* were H. W. Menard, Robert S. Dietz, Ed Hamilton, and Robert Dill. On *Horizon* were Russell Raitt, Arthur Maxwell, Kenneth Emery, Jeff Frautschy, and I. Jim Faughn, one of the unsung heroes of the new age of exploration, was captain of *Horizon.*

After about four days at sea the clutch on one of *EPCER 857*'s engines broke down. It was strongly hinted by her officers that the ship should return to San Diego. Fortunately, *Horizon* carried enough fuel for both ships to go all the way to Hawaii and beyond, while *EPCER 857* had only enough fuel for a few days' sailing. The two ships pulled alongside each other so that the Navy ship could be refueled, but Jim was careful to provide them with only three days' worth of fuel. Thus they had no choice but to proceed with us toward the equator. For the next refueling stop three days later, he provided them with enough for five days' sailing, still not enough to reach Point Loma. Thus we limped along for some three weeks till we arrived at Pearl Harbor in Hawaii. Being familiar with the usual leisurely pace of Navy shipyards, the officers and crew of *EPCER 857*, anticipating a stay of at least two months, sent for their families. I went to see the commandant of the yard, told him that we were on a high-priority expedition for the Office of Naval Research, and pleaded with him to expedite the *EPCER 857* repairs. He promised to give the ship top priority in the yard. *Horizon* took aboard the entire scientific party of both ships and proceeded westward to an area where Harry Hess had discovered several guyots. We found that the flat-topped seamounts were the eroded summits of a giant undersea mountain range, extending westward from Necker Island in the Hawaiian chain for at least 1500 kilometers. (It was later shown that this mountain range, which we called the Mid-Pacific Mountains, extends all the way to Wake Island in the central North Pacific.)

After an echo-sounding survey of one of the first guyots, we lowered a rock dredge in order to obtain a sample of the material on the mountain summit. The dredge came up partly filled with manganese hydroxide crusts, evidently very similar in composition to the familiar manganese nodules that had been dredged by HMS *Challenger* and other early expeditions from the deep-sea floor. But among these black, irregular fragments was a white object, a fossil shallow-water coral, which Ed Hamilton, our paleontologist, was able to identify tentatively as Cretaceous in age, i.e. approximately 80 million years old. Harry Hess's idea that the guyots had lain undisturbed for billions of years while sediments accumulated in the deep sea around them was clearly wrong. The Mid-Pacific Mountains could not be much older than their flat-topped summits.

The next surprise came from Russell Raitt's seismic observations. The top layer of the seafloor, about 100 meters thick, had the expected sound velocity for marine sediments; beneath it was a material with much higher velocity that was most likely volcanic rock, and beneath that layer, one or two kilometers thick, was denser material with still higher velocity. (Russ called it the second layer.) Underneath the second layer at a depth of 4 to 5 kilometers, the sound velocity jumped abruptly to more than 8 kilometers per second. This high velocity corresponded to that found, from seismic measurements in continental areas, for the mantle material below the "Moho," the Mohorovičić discontinuity that lies at a depth of 35 to 50 kilometers or more under the continents.

The surprising result of the seismic studies was the thinness of the sedimentary layer. It was generally believed that the rate of sedimentation in the red clay areas of the deep sea that surround the Mid-Pacific Mountains was at least one millimeter per thousand years, or one meter per million years. If the sediments were only 100 meters thick, the entire sedimentary column could have been laid down in only 100 million years. The guyots and the bottom stratum of the sediments in the deep sea surrounding the mountains were of about the same age. The supposed column of sediments extending far back into Precambrian time simply did not exist.

The first successful measurements of heat flow from the interior of the Earth through the seafloor were made by Art Maxwell on the Mid-Pacific Expedition. Thousands of similar measurements have been made since 1950, but at the time we thought of these measurements as very difficult. The measuring instrument consisted of a spear containing two thermistors spaced two meters apart, connected to an electronic recording device in a sealed, pressure-proof cylinder. (The seal was formed by rubber O-rings, which we have already mentioned as one of the great wartime discoveries.) This device had been developed in the Scripps instrument shops during the previous two years by Teddy Bullard (later Sir Edward Bullard) and Art Maxwell. It was based on the fact that the temperature of the ocean water overlying the deep-sea floor is virtually constant over periods of many years, and hence the temperature gradient in the subsea clays or oozes must directly reflect the flux of heat from below, unlike the situation on land, where seasonal temperature changes near the Earth's surface mask the temperature gradient.

Art Maxwell was able to obtain six apparently satisfactory measurements with his instrument. The basic problem was to pay out enough wire rope so that the spear would sit firmly in place in the bottom mud without wobbling, and yet not to pay out so much that the wire would kink on the bottom with the accompanying danger of losing the instrument. To accomplish this we attached a weighted tripping mechanism containing a glass ball—an ordinary glass fishnet float—to the wire just below the spear, following a design made by John Isaacs. This ball broke and imploded when the instrument hit the bottom; the resulting sound signal could easily be heard on shipboard, and we could stop paying out wire.

We had a pretty good idea of the thermal conductivity of the red clay. Multiplying this value by the measured temperature gradient, we calculated that the heat flow through the seafloor was about the same as that which had been measured previously in mines and deep wells on land. This was a puzzling result, because the basaltic material that was believed to lie under the ocean was known to contain a far smaller amount of radioactive, heat-producing elements than the granitic rocks of the continental crust. Harold Jeffreys, in *The Earth*, had speculated that the heat flow through the seafloor would be not much more than 30% of the heat coming from the continental rocks.

Bullard, Maxwell, and I explained our result by proposing that the heat in the Earth's interior deep beneath the sea was carried to the ocean floor by slow convective motions of the mantle rocks (Bullard et al 1956). This conclusion seemed particularly reasonable after Art Maxwell, on a later Scripps expedition, had made a series of measurements across the East Pacific Rise to the Middle America Trench, in which he found high heat flow near the rise, progressing regularly to low flow in the neighborhood of the trench. We were not courageous enough, or perhaps not smart enough, to conclude what is now widely believed—that the rocks of the Pacific ocean floor are carried along by convective motion and the force of gravity from the ridges (where they originate as lava) to the trenches (where they are subducted as relatively cool lithospheric crust). This of course is the reason why the sediments of the deep-sea floor are so thin and thc seamounts are so young—the older ocean crust has long since disappeared in the process of seafloor spreading, first described in the mid-1960s by Harry Hess and Robert Dietz. It should be pointed out, however, that Maxwell might easily have obtained just the opposite result, because we know now that hydrothermal circulation within a few hundred kilometers of the mid-ocean ridges results in both very high and low heat flows at closely spaced intervals.

After about 10 days, during which *Horizon* worked alone, we were rejoined by *EPCER 857*, and the two ships proceeded together to Bikini Atoll. Some of us camped on the beach to study reef erosion processes in the intertidal zone, while Russ Raitt and his assistants on the ships made the seismic survey I have already referred to of the volcanic rock surface underlying the reef limestone. One thing I remember about this episode was how quickly the works of man, in the form of the massive debris of

Operation Crossroads—the Quonset huts and furniture, the bulldozers and the jeeps, the refrigerators and other food containers—had rotted away under the action of the tropical Sun and the salty air or had been covered over by the lush vegetation of the atoll.

On the return voyage the two ships separated. Bill Menard and Jeff Frautschy sailed along the 40th parallel while most of the rest of us went to Hawaii and flew home. Menard and Frautschy made another great discovery. They found a huge, undersea cliff extending east–west for several thousand kilometers and more than 1000 meters high. They followed it into the California coastal waters near Cape Mendocino, where previous charts had shown a marked break in depth from south to north. They called the entire structure the Mendocino Escarpment. Later, Menard and his colleagues found half a dozen features approximately parallel to the Mendocino Escarpment, separated by several hundred kilometers from each other, which they called "fracture zones." We now know that they are the surface expressions of several of the transform faults, first explained in the late 1960s by Tuzo Wilson, that cut across the mid-ocean spreading centers.[1]

Some two years after the return of the Mid-Pacific Expedition the Scripps Institution undertook another long voyage of geologic discovery, the "Capricorn Expedition" of 1952/53. This time we used our own two ships, *Horizon* and *Spencer F. Baird*, both converted sea-going tugs with powerful engines and large fuel capacity. *Baird* was equipped with a newly constructed giant winch, similar to the one used successively by *Albatross* and *Galathea*. As with all our expeditions in those days, "Capricorn" was a multipurpose affair, participated in by geologists, geophysicists, meteorologists, biologists, and physical oceanographers. Among the scientific members of the expedition were Gustaf Arrhenius, Willard Bascom, Richard Blumberg, Milton Bramlette, Robert Dill, Rhodes Fairbridge, Robert L. Fisher, Ted Folsom, Don Hilleary, John Isaacs, Philip Jackson, Martin Johnson, Alan Jones, Ronald Mason, Arthur Maxwell, H. W. Menard, Walter Munk, J. R. Nicholson, Willard North, Russell Raitt, William Riedel, Henri Rotschi, Stanley Ruttenberg, Maxwell Silverman, Harris Stewart, Edward Taylor, Richard von Herzen, and I. Robert Livingstone was our expedition physician as well as one of the scuba divers.

The two ships first went to Bikini and Eniwetok, where they took part in the awesome first tests of fusion weapons. In November they sailed together to the Fiji Islands and thence to spend Christmas in TongaTabu, the capital island of the kingdom of Tonga. Here we were royally enter-

[1] The principal results of the "Mid-Pacific Expedition" were described in my article "The Earth Beneath the Sea—Geophysical Exploration Under the Ocean," published in what turned out to be an obscure book edited by Louis Ridenour (Revelle 1954).

tained by Prince Tungi, then the hereditary Prime Minister for his mother, Queen Salote, and now King Taufa'ahau Toupou IV of the Tonga Islands. From TongaTabu we sailed through the Tonga archipelago to American Samoa, eastward through the Cook Islands to Tahiti and the archipelago of the Tuamotus, and then to the Marquesas. And finally a long voyage home eastward across the East Pacific Rise, which was then called the Albatross Plateau, and northward across the empty equatorial Pacific. (We never saw another ship in the open sea on either the "Mid-Pacific" or the "Capricorn Expeditions.")

For the first six weeks of "Capricorn" the monster winch behaved very badly. It had been built specifically to explore the great Tonga Trench east of the islands. On our first attempt to use it in the trench we had paid out about 10,000 meters of wire when an appalling kink developed on the huge reel. We spent the next 24 hours making a long splice in the wire while 10,000 meters of it, with a Kullenberg coring device on the end, altogether weighing many tons, dangled over the side secured only by a wire clamp called a "comealong." After this frightening experience, in which *Baird*'s crew behaved with cool skill, we successfully lowered several sampling instruments down to the trench floor. But these came up scratched and battered with very little in them.

In fact, this was one of our most important discoveries about the trench—it was nearly empty of sediments. We found also that it was V-shaped in cross section. On subsequent expeditions, Bob Fisher of the Scripps staff showed that scarcity of sediments, exposures of hard rocks, and V-shaped cross sections were characteristic of most of the Pacific trenches (Fisher & Revelle 1955).

We found from careful echo-sounding that the trench was close to 10,800 meters deep, within 100 to 200 meters of the same depth as the two trenches that were then thought to be the deepest in the world, the Mindanao and Marianas trenches of the North Pacific (Fisher & Revelle 1954).[2] Apparently a depth of about 10,000 meters is about the limit for a hole in the bottom of the sea. More important was our discovery that on the seaward flank of the trench there was a flat-topped guyot, tilted downward toward the abyss. If we had had the sense to realize it, this was clear evidence of the proccss of subduction, which, as we now believe, slowly consumes the deep-sea floor.

The other principal geological and geophysical results of the "Capricorn Expedition" extended over a very wide area the discoveries of the "Mid-

[2] Fisher's most recent data from repeated soundings indicate that the Marianas Trench is 10,915 $\pm$ 10 meters deep, about 115 meters deeper than the Tonga Trench, while the maximum depth of the Mindanao, or Philippinc, Trench is 10,057 $\pm$ 5 meters, about 750 meters shallower than the Tonga Trench.

Pacific Expedition"—particularly the thinness of the sediments overlying the bottom volcanic rocks and the high heat flow through the seafloor. As Gustaf Arrhenius, one of our shipmates, had expected from his experience on the Swedish *Albatross* Expedition, Russell Raitt's seismic survey showed that the sediments in the equatorial zones of high biological productivity were several hundreds of meters thick. This did not mean that they had been deposited over a longer time interval than 100–150 million years but simply that the rate of deposition of the siliceous and calcareous remains of pelagic organisms was much greater in the equatorial regions. In contrast, near the crest of the East Pacific Rise there was very little sediment. Also in this region, Russell Raitt's seismic refraction studies did not show the shallow "Moho" and the simple pattern of rock layers of increasing sound velocity that we had come to expect from elsewhere in the deep Pacific. This different seismic structure may be explainable, as has been suggested in recent years, by the presence of great chambers of molten or nearly molten rock underlying the crest of the rise, which serve as the source of the volcanic rock that flows out of the mid-ocean ridges and continually renews the seafloor.

One outcome of "Capricorn" was the breaking of the Scripps taboo against women on oceanographic ships. Helen Raitt, Russell Raitt's wife, had met us at the Grand Pacific Hotel in Fiji, and she found an island freighter that would take her to our next stop in TongaTabu. But after we and she arrived there, it turned out that she might have to stay for several weeks in Tonga before finding a boat back to Fiji, where she could get an airplane. The obvious solution was to ask her to join us on board *Spencer F. Baird* with Russ and the rest of us. She proved to be a first-rate shipmate, standing regular watches and keeping up all our spirits. After our return she wrote a popular account of the expedition, *Exploring the Deep Pacific* (Raitt 1964), which was translated into Russian and Japanese and widely read among oceanographers and their friends.

Victor Vacquier, who later joined our Scripps faculty, had developed during World War II a sensitive instrument for measuring variations in the Earth's magnetic field. This was adapted for oceanographic use by fitting it into a torpedo-like casing, which could be towed far enough behind a research vessel so that the ship's own magnetic field would be negligible. We towed this contraption continually on "Capricorn," obtaining, among other records, interesting but difficult to interpret maps of the magnetic fields of seamounts. Away from the seamounts the magnetic record also showed many fluctuations, presumably reflecting the remanent magnetism of the materials beneath the seafloor.

On "Capricorn," the magnetometer was the particular charge of Ronald Mason, a young geophysicist from Imperial College in the London University. He found it hard to explain the "wiggles" in the record taken along the ship's track. He supposed that a two-dimensional map of the remanent magnetism would be easier to understand.

After the return of "Capricorn," Ron found that the Coast and Geodetic Survey ship, *Pioneer*, was about to undertake a topographic survey of the seafloor off the California and Oregon coasts out to several hundred miles. He proposed to me that he should go along on this survey with his towed magnetometer in order to make his dreamed-of magnetic map. He estimated that the total cost would be about $100,000. I tried my best to find this money in Washington, DC, but the magnetic experts of the US Geological Survey and other Washington scientific bureaus thought that Ron's proposed map would be useless and advised against any funds for the project. In the end, I had to use my Director's Contingency Fund to pay for the magnetic survey and the working up of the data.

Ronald produced a map showing the magnetic striations on the seafloor, which we now know represent a time series of reversals of the Earth's magnetic field. He found that the striations were displaced along Bill Menard's fracture zones. This result was later confirmed by Victor Vacquier and Arthur Raff, who found an apparent lateral displacement of fourteen hundred kilometers along the Mendocino Fracture Zone. These apparent displacements represent Tuzo Wilson's transform faults, and the entire ocean floor beyond the continental slopes exhibits magnetic striations like those first discovered by Ronald Mason.

The "Mid-Pacific" and "Capricorn Expeditions" were the first stages of the program of oceanwide exploration of the Earth beneath the sea undertaken by Scripps scientists over the following 25 years. During this period our ships sailed nearly two million miles through the Indian, Pacific, and Atlantic oceans, from the Arctic to the Antarctic, in more than 25 major geological/geophysical expeditions to study the seafloor and what lies beneath it. H. W. Menard, R. L. Fisher, Victor Vacquier, Arthur Raff, Russell Raitt, and George Shor were the leaders of this effort, but many others participated. As for me, "Capricorn" was my last long voyage. After 1953 the problems of directing Scripps, and at the same time trying to create a new univeristy, took all my time.

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