Perspectives of Earth and Space Scientists

AGU ADVANCING EARTH AND SPACE SCIENCE

FEATURE ARTICLE
10.1029/2022CN000160

In Pursuit of Understanding, a Career in Marine Geology

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Key Points:

- Curiosity about the marine environment and the history of the oceans was a potent force in my research career
- Working with other scientists having different areas of expertise broadened my understanding of processes in climate change and the marine environment
- Opportunities in new programs afforded me the chance to work cooperatively and gain new insights into the history of climate and the aquatic environment

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Citation:

Moore, T. C. Jr. (2022). In pursuit of understanding, a career in marine geology. *Perspectives of Earth and Space Scientists*, *3*, e2022CN000160. https://doi.org/10.1029/2022CN000160

Received 15 FEB 2022
Accepted 22 JUN 2022

Abstract My interest in the oceans first developed when I was a teenager, but I did not actually go to sea until I was in the U.S. Navy. With that experience, I developed a love of the oceans and an interest in oceanography. My graduate training was a time when oceanography and marine geology were blossoming with new ideas and new tools to explore the ocean world. The theory of plate tectonics was becoming widely accepted and scientific ocean drilling was just starting. My thesis study area was in the tropical Pacific. Soon after receiving my PhD, I sailed on Deep Sea Drilling Project Leg 8, which drilled the first transect across the Pacific equator. The nature of the sediments there posed many scientific questions that continued to intrigue me. Some of these questions remained unanswered for a long time. Early in my career I was fortunate to work with a group of specialists from outside my field of expertise to study the global climate during the last glacial maximum (the CLIMAP Project). Subsequently, I spent 8 years working in the oil industry. This experience taught me the skills of interpreting seismic reflection records that help unravel the history of sediment deposition. When I returned to academia, these skills proved particularly useful in studies of large lakes. Late in my career, I returned to studies of the tropical Pacific, where new tools and techniques helped answer some of my unanswered questions.

Plain Language Summary I became interested in the mysteries of the marine world as a teenager, but it was in the Navy that my love of the oceans and my interest in the field of oceanography fully developed. In the early 1960s, after serving in the Navy, my continuing curiosity about the marine world led me to graduate studies at the Scripps Institution of Oceanography. This was an exciting time for the marine geosciences. New tools and techniques for ocean exploration were being developed and the theory of plate tectonics had just burst onto the scene. In the ensuing years I worked with outstanding researchers and climate modelers studying the climate at the time of maximum ice extent during the last ice age. This team effort greatly broadened my understanding of ice sheets, climate models and factors controlling climate change. The initiation of scientific ocean drilling also gave me the opportunity to cooperate with international groups of researchers studying oceans of the past. After 8 years working for Exxon, I returned to academic research, studying the depositional history of large lakes, and returning to my long-term interest in the tropical Pacific. In this latter area of study, I was finally able to answer some of the questions that had puzzled me since my graduate student days.

1. Introduction

I once heard someone decry the old adage often given to young people: "follow your dream". They noted dreams may not be practical and can turn into nightmares. They substituted the advice: "follow your curiosity". For those with a strong sense of curiosity, discovery and understanding are relished rewards. The common factor for many scientists, I believe, is a very strong innate curiosity, particularly a curiosity about how the natural world works. The field of Oceanography was rapidly growing when I entered as a graduate student at Scripps Institution of Oceanography (SIO) almost 60 years ago. The field had built on the widespread interest in marine biology. With the strong support of the U.S. Naval Research Program, the scope of the field was growing, especially at a select few oceanographic institutions. It was an auspicious time to begin a career, particularly as a marine geologist. What lay ahead was the development and acceptance of the new plate tectonics paradigm, the continued development of new tools and techniques to study the oceans, including the initiation of scientific ocean drilling in the deep sea, and the birth of a new subfield—paleoceanography. The intellectual challenges and excitement associated with these developments and the discoveries they engendered have been extremely rewarding.

2. The Start

The earliest sign of an interest in the marine world came when I was a senior in high school and read William Beebe's book “Half Mile Down” relating his deep dives off Bermuda in a bathysphere. It was a dark and mysterious world, full of bizarre creatures. My curiosity was aroused. After high school (1956) I attended the University of North Carolina. Unfortunately, there was no college level course in Oceanography offered at that time. I had to select a major at the end of my sophomore year. Having taken courses in all the basic sciences, I knew that science (as opposed to the liberal arts) was my calling. But the question was, which science? By a process of elimination, I settled on Geology. It addressed my curiosity about the natural world and offered opportunities to explore that world both in the field and in the laboratory.

One of my professors, Roy Ingram, was awarded an undergraduate research grant. He asked me to undertake a survey of surface marine sediments in Back Sound, between Harkers Island and Shackleford Banks on the North Carolina coast. The main finding of the study was that there was a high concentration of heavy minerals lying in the shoaler waters just inland of Shackleford Banks. Here, wave action was just strong enough to winnow out the lighter sand grains leaving a placer deposit of ilmenite, rutile, and magnetite. This introduction to the marine world built on my early interests, but it still wasn't the open ocean that I wanted to explore.

By the time graduation rolled around the primary employers of young geologists with a BS degree (the oil companies) were in one of their periodic recessions and were firing more than hiring. Fortunately, I already had a job. The U.S. Navy had funded my 4 years of college, and in return, I owed them at least 3 years of active service.

3. The Navy Years

Given the sad state of employment opportunities for young geologists in 1960, I was willing to give the Navy a fair chance as a possible career. I asked to be assigned to what I considered to be the “real” Navy: a destroyer out of Norfolk VA. I was assigned to the USS Eaton (DD 510), a Fletcher class destroyer built in 1940. The Eaton had been extensively refitted for antisubmarine warfare and was one of a squadron of destroyers assigned to this purpose. Its main advantage over newer vessels was that it was fast, with a top speed of 40 knots.

The Navy taught me a lot. Right away I learned that I am very prone to sea sickness but could overcome it in 3–5 days. Fortunately, our operating schedule was usually 2 weeks at sea and 2 weeks in port—just short enough time in port that I did not entirely lose my “sea legs”. I was assigned to the engineering department and learned the intricacies of steam propulsion and the techniques used to repair battle damage and put out shipboard fires. I learned to drive the ship and became a qualified “officer of the deck”. In the third year of my service, I took over the duties of the Navigator and learned about LORAN C and piloting in coastal waters. The tedium of exercises in tracking submarines was relieved on several occasions. Picking up the astronaut capsule that landed in the ocean and participating in the Bay of Pigs fiasco as well as the Cuban missile crisis blockade were certainly not routine. However, our main task was to practice chasing submarines. Practicing war is far better than actually going to war. Nevertheless, when you know that the new albacore-hull, nuclear submarines could outrun and out maneuver anything our destroyer did, it felt like we were practicing the last war, not preparing for the next one.

The Navy years were worthwhile. Those 3 years gave me time to mature and evaluate my own goals. The experience taught me some useful nautical skills; and instilled in me a real love of the ocean in all its beauty and fearsome power. During my third year in the Navy, I took a night course in Oceanography and started applying to oceanography graduate schools. I applied to all the major graduate programs but was particularly interested in SIO and Woods Hole Oceanographic Institution (WHOI). Graduate training at WHOI is carried out through MIT. At that time the MIT application form required not only a transcript of courses taken, but also a list of the texts used in those courses. The Navy had purchased all my texts for me, but they required that I turn them back in at the end of the semester. Remembering all the texts I had used three to 7 years previously was impossible; thus, the application to MIT was never filed. Fortunately, I was accepted at Scripps in 1963.

4. Graduate School

I quickly made two close friends in graduate school: Ross Heath, my office mate, and Wolf Berger. Like me, they both had had a break in their academic careers and were eager to explore this new field. At that time, SIO ship time was block funded and the grad students were all research assistants. We had an introductory student cruise around the California borderland and were exposed to techniques of coring, dredging, plankton net tows, and Nanson bottle casts, all in a couple of days. Once we finished work late at night, almost everyone went to bed, including our professors. Ross and I, however, stayed up and guided the ship around the Channel Islands, using the "precision depth recorder" (PDR) to explore the bathymetry of this interesting area. It was great fun to have control of a scientific cruise for the first time.

We became familiar with SIO and our fellow grad students while taking the core courses in physical, geological, chemical, and biological oceanography. We had the sense that the graduate students, as a group, had a better grip on the broad scope of science going on at SIO than any of the faculty. In fact, some of the intra-faculty feuds stood in the way of progress of graduate student research.

In our second year of grad school Ross and I were given an unbelievable opportunity. Per Scholander wanted to study the physiology of the pearl divers in Tahiti. He needed an SIO ship there to act as his research base, and he did not want to spend his time sailing it down there. We were asked, "would you guys like to take the R/V Stephen F. Baird down to Tahiti and maybe do some thesis research on the way?" Ross and I jumped at the chance. We put together a plan to make a north to south seismic reflection transect down to the equator, and then do a detailed survey of abyssal hills just north of the equator. We would take core samples in a variety of topographic locations within the survey area. At the time this sort of work pushed the limits of technical capabilities. Taking seismic reflection records in the deep sea was just getting started at SIO. There was no GPS navigation. We were out of the range for LORAN navigation. There was no swath mapping, only the PDR, and only a recently invented way of easily placing cores accurately with respect to topography.

We made the seismic reflection transect down to the equator at ~150°W (Heath & Moore, 1965), and then selected the survey area along that transect near 8°20′N. We had to construct a buoy topped with a radar reflector and anchor it in 5,000 m of water. In the survey we positioned ourselves relative to this buoy. The bathymetry was printed on a PDR roll of paper with all the confusing hyperbolic echoes coming from sharp breaks in the topography. For coring, in addition to the traditional piston cure, we used the Benthos "free fall" coring device. You simply dropped them over the side and marked the PDR record. They fell through the water at ~15 knots, and once they struck bottom, they released a float that jerked out the sediment core liner and brought it to the surface. The coring device itself stayed on the bottom. Once the float reached the surface a blinking strobe light helped us locate it. Night operations only! With limited manpower this was an exhausting effort, but largely a success. Part way through the effort we learned that Dr. Scholander did not receive the funding to carry out his intended research; thus, when we finished, we headed back to San Diego.

I had determined that the stratigraphy of marine sediments would be my academic focus. My mentor in this effort was Bill Riedel and the biostratigraphy that I used was based on the radiolarians. They have tests made of clear, amorphous silica, with intricate geometries of great beauty. The Radiolaria had been studied since the mid 1800s, but the Challenger Expedition (1872–1876) had found specimens in the surface sediments of the tropical Pacific that were identical to ones found in Eocene marine deposits on land. The obvious conclusion was that radiolarian species were very long lived and useless for biostratigraphic dating. This belief persisted until the 1950s when Bill Riedel showed that the sediments the Challenger had sampled where highly mixed agglomerations of specimens of several different ages. He determined that radiolarian species, like most other microfossils, did have specific age ranges. The cause of the mixing of ages in near-surface sediments in the tropical Pacific remained a mystery that was not fully explained for many years.

All the cores taken in our survey area contained these highly mixed assemblages of radiolarians. The recovered cores were too short to reach beneath the zone of mixing. Working out a detailed stratigraphy was only partially successful (Moore, 1970). However, many of the cores contained manganese nodules at the sediment surface (Moore & Heath, 1966). Geochemical analysis of these nodules by colleagues at SIO showed they had an unusually high content of Ni, Cu, and Co (Somayajulu et al., 1971)—enough to be of interest to the mining community. The result was that Ross and I received fellowships from the International Nickel Company. The likely cause of this trace metal enrichment was also not known.

We soon learned that the budding interest in mining manganese nodules arose from a rumor that Howard Hughes was planning to build a ship (the GLOMAR Explorer) designed specifically for mining manganese nodules. It sounded like something that the eccentric billionaire might do; mining companies took it seriously. Much later we learned that the building of the ship was funded by the CIA. Its sole purpose was to raise a sunken Soviet submarine from the deep tropical Pacific, some 3,000 km west of our thesis area.

5. Opportunity Knocks and Knocks Again

During our years at SIO Ross and I often wandered down the hall to Tjeerd (Jerry) van Andel's office where fresh coffee was available. We enjoyed chatting with Jerry and the other students that showed up, and we ended up working with him on several projects involving sedimentation and stratigraphy. Just as we were finishing our degrees, Jerry accepted a position at Oregon State University, which was expanding its Oceanography School. Jerry was kind enough to offer Ross and me positions and take us along with him.

5.1. Scientific Ocean Drilling

We took part in several major oceanographic expeditions under Jerry's leadership (e.g., Heath et al., 1977; Moore et al., 1970; van Andel & Moore, 1974; van Andel et al., 1967, 1970, 1975), but new opportunities soon came our way. First, there was the initiation of the Deep Sea Drilling Project (DSDP). This was the start of a long-running effort that has recovered sediments and basement rocks from the world's oceans. DSDP and subsequent programs, have greatly expanded our knowledge of the deep ocean environment and ocean history. It has also created an international network of geoscientists that has enhanced communication and cooperation within our expanded community. I served as a biostratigrapher on DSDP Leg 8, which drilled the first transect of sites across the Pacific equatorial zone of high productivity. It gave me access to a suite of cores that sampled the mid Eocene through the Quaternary (Moore, 1971, 1972). It also gave support to the new sea floor spreading theory, which had exploded on the scene in the late 1960s, by showing the biogenic sediment thickness maximum moving northward with time.

I maintained close contact with the scientific ocean drilling programs throughout my career and participated in seven drilling cruises. In the early 1980s I was on a committee that evaluated the use of the GLOMAR Explorer in the new Ocean Drilling Program (ODP). The CIA no longer needed it. The GLOMAR Challenger had served DSDP well for more than a decade. However, we needed a newer, somewhat more capable drill ship for ODP. We evaluated the GLOMAR Explorer as it lay in port near San Francisco. With its gimbled drill floor and huge moon pool, the ship was much more than we needed.

5.2. The CLIMAP and SPECMAP Projects

A second opportunity arose. The National Science Foundation announced a funding program called the International Decade of Ocean Exploration (IDOE). It encouraged large, multi-institutional research projects that brought a broader understanding of ocean processes. We heard of a cooperative effort being organized at Brown University and the Lamont Doherty Geological Observatory (LDGO). At Brown, John Imbrie and Nilva Kipp had developed a technique of defining assemblages of microfossils using Q-mode factor analysis and relating these assemblages to the environment (mainly sea surface temperature) using what they called a transfer function (Imbrie & Kipp, 1971). LDGO had the world's largest collection of deep-sea piston cores. Jim Hays was their core curator, as well as another radiolarian expert. The Brown and LDGO researchers were leaders in an effort to map the temperatures in the Pleistocene ocean. This was a formidable task. Earlier, Jerry van Andel and John Imbrie had written a paper about using factor analysis to define related groups of minerals in sedimentary deposits (Imbrie & van Andel, 1964), a paper which I found very interesting and potentially useful in defining radiolarian assemblages. Ross and I asked John and Jim if we could join with them in a project to map the world ocean at the time of the last glacial maximum. They graciously accepted us into the cooperative effort.

The proposed project certainly met the criteria set forth in IDOE and was funded. Our first hurdle was to identify the sediments that were deposited during the last glacial maximum in as many cores as possible. The most definitive way of doing that was by oxygen isotope analysis. For this we enlisted Nick Shackleton of Cambridge

University. He was the acknowledged leader in rapidly turning out oxygen isotope data on what were then very small samples. He had an adjunct appointment at LDGO.

Andy McIntyre and Bill Ruddiman had already revealed how the Gulf Stream and North Atlantic current had shifted before, during, and after the last glacial maximum (e.g., McIntyre & Ruddiman, 1972). This provided a terrific start to the project. However, there was much left to do in the Pacific, Indian and southern oceans. Soon there were at least 14 professors, post docs, and graduate students working on developing transfer functions for foraminifera, diatoms, nannofossils, and radiolarians. They were also searching for cores in which the level of the last glacial maximum could be identified. This effort reenforced what I already knew: good graduate students make good colleagues in any research effort, as well as independent researchers themselves later in life.

My efforts in constructing the glacial ocean were focused on the Pacific. The sediments of the Pacific are highly varied in composition, requiring a group of micropaleontologists studying foraminifera, calcareous nannofossils, diatoms and radiolarians to cover the area. The final map we drew showed a more extensive equatorial upwelling zone reaching far to the west. Unlike in the North Atlantic, however, the North Pacific Drift did not shift greatly in latitude during the ice age, but rather showed a less diffuse, sharper gradient between the subarctic and central water masses (Moore et al., 1981).

It was a busy time in the beginning of the Project. However, it was not so busy that we could not contemplate what we were producing and how it could be used. At the time models of global atmospheric circulation (GCMs) were being developed by NOAA and other organizations. We hit upon the idea that such models could use our data to depict global climate at the time of the ice age maximum. This was a tremendously exciting prospect, but we had much to learn about what GCMs required as input. We needed a global data set of summer and winter sea surface temperatures, which did not exist for even the modern global ocean at that time. We also needed data on coastlines, land topographic elevations, as well as ice sheet extent and elevation. Furthermore, we needed a way to check the accuracy of the model runs. This we proposed to do by creating a global view of pollen data at the last glacial maximum, from which we could deduce temperature and precipitation. It was going to be a wonderful experiment, if only we could pull it off. Modelers from NOAA's Geophysical Fluid Dynamics Lab (GFDL) in Princeton and modelers at the Rand Corporation expressed interest in being part of this experiment. Thus, the CLIMAP Project was born.

Even with the people already involved with the project at Brown, LDGO and Oregon State, we had neither the manpower nor the expertise to carry out the massive collection of data required as input to the GCMs. We had already experienced the collegiality and productivity of a multi-institutional project, so we did not hesitate to expand our group further. We needed other experts that could provide the needed data. The coastlines and land elevation were relatively easy. We already knew that sea level had dropped 120 m at the time of the glacial maximum, and land elevation had not changed much since that time. All we needed to do was—add 120 m to existing topography and draw the coastline at the 120 m bathymetric contour. For the ice sheets though, we needed specialists. We contacted George Denton and his colleagues from the University of Maine. Their job would not be simple, but the scientists from Maine were just as excited by the experiment as we were. The gathering of pollen data was a task comparable to that of the marine micropaleontologists trying to reconstruct sea surface temperature. It required the cooperative efforts of Tom Webb (Brown) with colleagues Linda and Cal Heusser (LDGO), Herb Wright (Wisconsin), Thomas van der Hammen (Leiden), and several others.

In early 1975 Ross Heath and I accepted positions at the Graduate School of Oceanography at the University of Rhode Island. We moved there with Paul Dauphin, our lab manager and two graduate students: Margaret Leinen and Nick Pisias. They were already established researchers in their own right. This move not only provided Ross and me with hard money salaries, it also put us closer to Brown and LDGO and enhanced our ease of communication and cooperation.

When the data set for the GCM model input was complete, the Rand and GFDL models gave similar results (CLIMAP Project Members, 1976, 1981; Gates, 1976). However, the large grid scale used by these early GCMs averaged values over hundreds of square miles, whereas pollen data represented a much smaller area. It was difficult to reliably compare pollen-based climate estimates to model output. Nevertheless, the comparisons did show a high degree of similarity between the model output and the pollen data.

Nick Shackleton did not just determine the glacial maximum in a host of cores; he developed a detailed, continuous record of oxygen isotope fluctuations stretching back well into Pleistocene time. Looking at these distinctive climatic pulses provided the impetus to learn spectral analysis techniques (guided by John Tukey of Brown). This new aspect of our cooperative research led to the seminal "pacemaker of climate" paper by Jim Hays, John Imbrie, and Nick Shackleton (Hays et al., 1976). This paper tied the Milankovitch orbital variations of the earth to fluctuations in global ice volumes. It also led to Nick Pisias's paper (Pisias & Moore, 1981) that showed the spectral character of these ice volume fluctuations had evolved over time. These discoveries were the foundation for a new cooperative effort called SPECMAP that focused on the link between the variations in solar energy received at the earth's surface, orbital variations, and climate change. The exact mechanisms that translated orbital variations into changing climate is still an area of important research, and the exact cause of the evolution in the spectral character of the proxy climate record is still unknown.

Throughout the CLIMAP project we never saw any bickering or petty jealousies. Even though we had never worked together before, the project excited us all. We celebrated each new revelation, no matter who made the discovery. We worked together with a common goal. Moreover, the breadth of expertise involved in achieving that goal meant that we were all learning and broadening our appreciation of the complexity of climatic processes. We had frequent open meetings at LDGO where anyone could attend and informally present data or ideas. This was where all aspects of developing the picture of the ice age world were presented. These meetings attracted many scientists who were not directly funded by the CLIMAP Project, and they often had important contributions to make.

5.3. The Move to Industry

In 1980 my personal life overtook my professional life when my marriage ended. It was necessary to seek other employment if I were to meet my financial obligations. Unlike in 1960, the oil companies had been hiring for a while. A few years earlier scientists at the Exxon Production Research Company (EPR Co.) had published AAPG Memoir 26 in which they laid out a well-defined architecture of sediments on continental margins. They claimed that the same architectural pattern was seen in continental margins around the world. This was an astounding claim; but there were two things wrong with it. First, the authors did not show sufficient data to establish the claim that the architecture changed globally in a similar way through time. Second, although the shifts in depocenters form onshore to offshore and back apparently took place very quickly, the mechanism causing these shifts was not explicitly proposed. It was unheard of that any oil company would publish such a profound new view of sedimentation on continental margins. They are usually quite secretive. The shortcomings of the publication did not detract from its importance. Because of the confidentiality of the industry data, the academic world would have to acquire such data itself if we were to verify the ideas presented. For my own enlightenment, I could apply for a position at EPR Co. As an employee I could judge the validity of their claims myself.

Once at EPR Co., the group to which I was assigned had three main areas of effort: (a) supply workers and consultants to Exxon affiliates for special projects; (b) teach courses in seismic stratigraphy, basin analysis, etc., and (c) carry out proposed research. As it happened, items 1 and 2 were where our main efforts lay. However, I also attended schools that brought me up to speed with the techniques of oil exploration and the interpretation of seismic data. It was much like attending another graduate program and I did enjoy it.

While there, I worked on large and small projects that involved seismic reflection data from the Gulf of Mexico, the Colombian foreland basin, the Magdalena River valley and delta, the Guajira Peninsula, the foreland basin and Alto Plano of Bolivia, the Bahamas, The Eel River basin of California, the Bering Sea and the Arctic shelf north of the Bering Straits, offshore China, the East Indies, the northeast continental margin of the U.S., and the Nile delta. Through these studies, plus the schools and field trips, I became convinced that the patterns of sea level rise and fall first proposed by Peter Vail and colleagues were real, with variations related to sediment type and source (Greenlee & Moore. 1988; Moore et al., 1987). The mechanism for apparent changes in sea level through time was most likely changes in the volume of ice on land. This was a fairly novel idea in the 1980s. It meant there must have been a substantial volume of ice on land throughout the Phanerozoic.

I made two significant contributions to corporate knowledge based on my experience in academia. First, the Gulf Coast Division of Exxon looked for Pleistocene sands trapped around salt domes that often hosted hydrocarbons. They were using as a guide the old four phase history of glaciation and deglaciation taught in the 1950s.

Estimated sea level changes were based on benthic foraminifera. I convinced them that the marine oxygen isotope record, which had recently been extended back to the Pliocene by Nick Shackleton, was a much more detailed and reliable sea level model (Feeley et al., 1990). My second contribution, working with the Long Range Research Group at EPR Co., was to develop an automated statistical approach to seismic facies analysis. This was in lieu of the prevailing "eyeball" estimation of seismic facies.

5.4. The Return to Academia and the Study of Large Lakes

After 8 years at EPR Co., I felt that I had repaid them for the training I had received, and I would return to an academic position. Once there I could both apply what I had learned in industry and more freely choose the areas of research I wished to pursue. I found employment at the University of Michigan, where an opportunity arose to study the history of large lakes using my newly acquired skills in seismic stratigraphy.

5.4.1. The Great Lakes

Again, the lake research involved joining teams of researchers. First, working with Dave Rea at Michigan, we recognized an opportunity to combine seismic stratigraphy and piston coring with the use of oxygen isotopes to detect melt water surges entering the Great Lakes. These surges occurred as the Laurentide ice sheet retreated at the end of the last glacial. Our team included Mike Lewis (Geological Survey of Canada), who is a leader in the study of Great Lakes history from land sections. Larry Mayer (University of New Brunswick) provided the skills and equipment for acquiring high-resolution seismic reflection data. Allison Smith (Kent State University) studied the ostracod fossils from the piston cores and Dave Dettman (Michigan) made the δO^{18} measurement on these microfossils. Jim Walker (Michigan) developed the model to calculate the outflow volume from the Great lakes system through time (Dettman et al., 1995; Dobson et al., 1995; Lewis et al., 1995; Moore et al., 1994, Rea, Moore, Lewis, et al., 1994, Rea, Moore, Anderson, et al., 1994). The model we developed for outflow did not seem to indicate sufficient flow to "cap" the North Atlantic with fresh water and stall North Atlantic Deep Water formation (Moore et al., 2000). However, outflow of pack ice from the Arctic might have served that purpose (Moore, 2005).

5.4.2. Lake Baikal

When the Soviet Union collapsed in the early 1990s, the support for what had been a large and important community of Soviet scientists was in disarray. Support for research projects largely disappeared, as did support for many individual scientists. The National Science Foundation was aware of this problem and encouraged U.S. scientists to develop cooperative projects. Debbie Hutchinson and Kim Klitgord of the USGS led an effort to study Lake Baikal in eastern Siberia (Hutchinson et al., 1992, 1993; Scholz et al., 1993). They included Chris Scholz, an expert in acquiring and interpreting seismic data from African rift lakes and myself. Together, we received support from NSF to work with Russian scientists in acquiring and interpreting Lake Baikal seismic reflection data. One result of our studies was the discovery of prograding clinoforms on Academician Ridge, located on the western side of Lake Baikal's central basin. Very similar clinoforms were found in the southeastern part of the northern basin. The surprising part was that the clinoforms in both areas indicated a sediment source to the east, but The Academician Ridge was separated from any possible eastern source by the wide, 1,600 m deep, central basin. Obviously, the central basin must have opened after the sediments prograded onto what is now the Academician Ridge (Kaz'min et al., 1995; Moore et al., 1997). Another surprise came to light when we tried to tie the seismic interpretations from the central basin to those of the southern basin. This effort was complicated by reflections that seemed to cross each other at very low angles. It turned out that the crossing reflections were generated by gas hydrates. Gas hydrates are commonly seen in some nearshore marine sediments, but no one had suspected they existed in Lake Baikal (Moore et al., 1997; Scholz et al., 1998). My experience in limnology taught me that each lake is like an ocean on a different planet. They each have their own chemistry, their own ecology, and their own preserved sediment record.

5.5. The Return to the Tropical Pacific Ocean

As my lake research projects wound down, I turned my attention back to the tropical Pacific area, which had been the focus of my early career. There were still questions to be answered. This revisit turned out to be timely. Under the leadership of Mitch Lyle, Nick Pisias, Larry Mayer, Alan Mix, and several others, ODP Leg 138 employed a technique of using the hydraulic piston corer to recover an undisturbed, complete sediment section. One hole was continuously cored. Then a second (and sometimes a third) nearby hole was cored in which the cored intervals overlapped the gaps between cores in the first hole. Using the rapidly acquired physical property records of recovered cores, adjustments to coring depths could be made in real time.

This mode of operation was made possible only by using the hydraulic piston core combined with the rapid, pass-through measurement of sediment physical properties in recovered cores. With this approach, we could demonstrably recover the complete stratigraphic section at any given site. And it made possible several important advances. Careful analysis of the improved seismic reflection data at the sites showed that the fluctuations in the physical property records directly controlled the seismic reflection signature and that this signature could be traced over large areas (Bloomer et al., 1995), Thus, it was not surprising when researchers such as Nick Shackleton, Heiko Pälike, and Thomas Westerhold studied sites in the tropical Pacific drilled with the "complete recovery" technique, they were able to make detailed correlations not just between holes drilled at the same site, but also between different sites within the region (Pälike et al., 2005; Shackleton et al., 1995; Westerhold et al., 2012). Taking the lead from the tuning of the Pleistocene time scale in the 1980s, the fluctuations in the physical properties were also found to be directly related to the Milankovitch orbital cycles. They could then be used as a template to tune the geologic record well back into the early Cenozoic (e.g., Westerhold et al., 2013).

A time scale with a comparable accuracy of dating throughout the Cenozoic means that rates of processes taking place in the ocean can be accurately compared throughout this time interval. It also means that biostratigrahic events of the Cenozoic can be precisely dated. Results from ODP Leg 199 identified the ages of almost 200 first and last occurrences of radiolarian species through the Cenozoic (Nigrini et al., 2006).

Most sites drilled in the tropical Pacific have a hiatus across the Eocene - Oligocene boundary, as well as a high degree of mixing or reworking of sediments in the uppermost Eocene. These radiolarian datums, together with the excellent recovery of just three complete Eocene-Oligocene boundary sections on ODP Leg 199 and Integrated Ocean Drilling program (IDOP) Expedition 320, allowed the development of a detailed record of pulses of sediment mixing in the late Eocene (Moore, 2013; Moore & Kamikuri, 2012). The intensity of these pulses increased as the boundary was approached; therefore, whatever caused these sediment disturbances probably also caused the pervasive erosion of the Eocene—Oligocene boundary in the region. It was suggested that internal waves on a deep pycnocline may have caused the sediment mixing and erosion (Moore, 2013).

Although the nature of sediment mixing near the Eocene-Oligocene boundary had a likely explanation, I still had troubling questions about the pervasive reworked radiolarian tests in near-surface sediments of this region. Why were these mixed assemblages replete with species from nearly all ages of the underlying sediments, whereas there was no such evidence of sediment mixing seen in the calcareous microfossils? This puzzlement had been around since before I was a graduate student. As more sites were drilled in the tropical Pacific other questions arose and remained unanswered. Why were there never any radiolarians preserved in sediments close to the basement contact, and what accounted for the widespread presence of chert in the region, even in sections that were less than 100 m thick? The answers to these questions began to unfold on the site survey cruise for IODP Expedition 320/321.

Two advances in deep sea exploration technology shed light on these questions: high resolution, migrated seismic reflection data collected and interpreted in real time, and multi beam (swath) mapping of the seafloor. The swath records revealed a detailed, 10 km -wide map of seafloor topography as we steamed along. These two advances allowed the scientists on board the cruise to discern pits at the top of seamounts that were buried by a sediment cover (Figure 1). These pits resembled the craters on volcanoes. However, their location was in water too deep for explosive volcanism. Such pits were also found along minor faults revealed by the seismic data. Our observations led us to propose that hydrothermal waters moving in the upper crustal aquafer, beneath the sediment aquitard, found a path to the surface through minor faults and through the tensional cracks that formed in sediments draped over seamount peaks (Moore et al., 2007). The movement of these heated waters explained the precipitation of calcite veins in the fractured upper crust and the overgrowth of calcite on calcareous microfossils near the sediment-crust boundary. It also explained the dissolution of biogenic silica near this boundary. As these waters seeped higher in the overlying sediments, they cooled and precipitated the dissolved silica as chert (Moore, 2008a, 2008b). As the hydrothermally driven waters discharged through pathways in the fractured sediment, they quickly cooled. They became very corrosive to carbonate; thus, enhancing the pathway to the seafloor

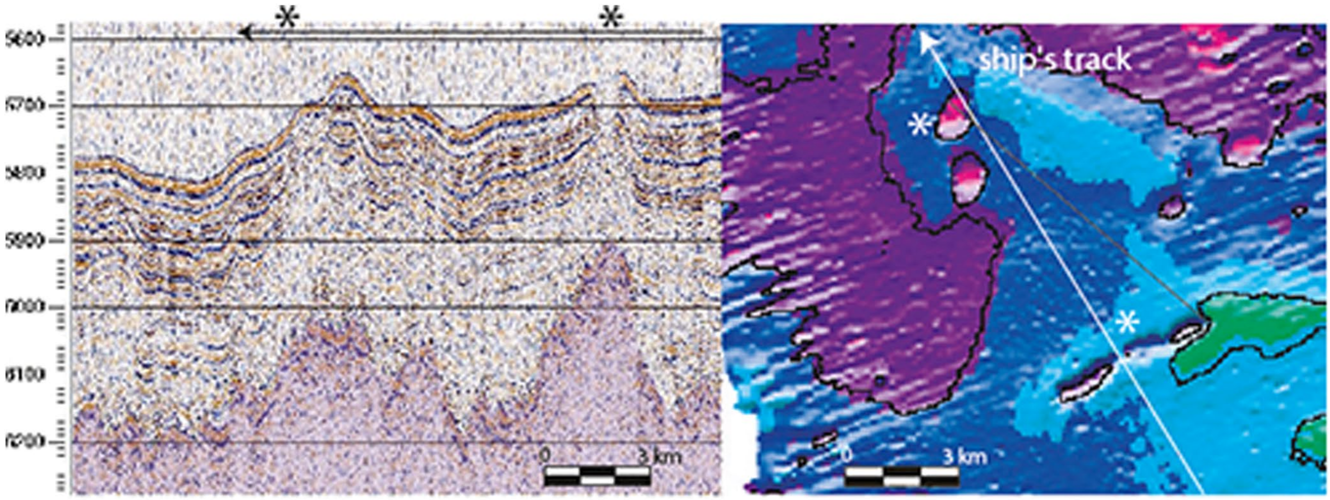

Figure 1. Advances in high resolution seismic imaging of sedimentary sections combined with swath mapping of the seafloor led to the discovery of widespread hydrothermal vents (pits) in the tropical Pacific sediments. The swath map (right) shows a very elongate pit crossed by the seismic line (left) and several subcircular pits seen in the swath map but not imaged by the seismic line. Note the discontinuous nature of seismic reflections over buried seamounts in the region where pits occur nearby. Acoustic basement is shaded in the figure. Seismic depths in milliseconds; contours in km. The hydrothermal circulation in old Pacific crust that created these pits is likely associated with chert formation within comparatively thin sedimentary sections and the widespread distribution of older reworked radiolarians in near surface sediments (from Moore et al., 2007, Figure 3. See also Moore, 2008a, 2008b, Moore et al., 2012.).

and creating a dissolution pit at the sediment surface. This cooling, however, reduced the corrosivity of the waters with respect to biogenic silica, The siliceous microfossils, freed from the calcareous sediment along the pathway of the venting, were carried to the surface. This resulted in admixed older siliceous microfossils being distributed far and wide in the surface sediments, though there were no older calcareous fossils preserved in the venting process (Moore et al., 2012).

Looking back at the low-resolution map of the abyssal hills in my thesis area, I suspect that the topography we mapped was strongly influenced by hydrothermal venting in the area. If so, then perhaps the venting fluids influenced both the abundance of manganese nodules on the surrounding slopes as well as the trace metal composition of these nodules. This proposed story of hydrothermal circulation in old crust seems to explain all those nagging questions about chert formation and sediment mixing, but these ideas have yet to be fully tested by direct measurement. As in many such investigations, new questions arise. Is the hydrothermal venting episodic or continuous? What is the chemical composition of the venting fluids? What sort of microbial community is associated with these vents?

6. Conclusions

Natural processes are rarely simple. My career has taught me that a curiosity-driven search for a deeper understanding of natural processes is most successful when (a) you are cooperating with others whose expertise complements your own, and when (b) new technologies open new opportunities and give a clearer vision of the natural world.

Examples of the first point highlight my whole career. The CLIMAP and SPECMAP projects brought together climate modelers, geochemists, glaciologists and paleontologists with several different specialties. The ocean drilling programs gathers international teams of scientists to focus on specific drilling programs. The team approach used in EPR Co. combined seismic stratigraphy and seismic facies analysis with well log analysis, basin structural development in exploring for new oil and gas reservoirs. Documenting the history of Lake Baikal and the Great Lakes required the cooperation of Canadian, Russian, and American geologists, geophysicists, paleontologists, geochemists, and modelers. Working with other experts is always a rewarding experience.

Examples of the impact of improved technologies that have helped my career (as well as those of many other scientists) include (a) Nick Shackleton's development of the micromass spectrometer that made the rapid development of ∂O^{18} records of climate change, (b) the development of the hydraulic piston corer that enabled scientist to collect undisturbed sections of unconsolidated deep-sea sediments, (c) the improvements in seismic reflection imaging of marine and lake sediments that reveal clear stratigraphic histories, and (d) the use of the swath mapping technology, which displays a detailed bathymetry of the deep sea.

7. Key Points: Lessons Learned

- Keep abreast of opportunities to further your research goals and expand your understanding. There are several international programs, such as those conducting ocean and continental drilling, that are open to proposals and participation. They also have archived a wealth of metadata, data, and core samples that are freely available for use by research scientists.
- Be aware of advances in technology and how you might use such advances. New, innovative techniques and measurement devices are continually appearing.
- Welcome the opportunity to work with other specialists, especially those in fields other than your own. The experience will broaden your understanding.
- Remember those unanswered questions that always arise as you delve into a research project. Advances in technology, thinking, and the results of other studies may help provide answers.
- No matter where the necessities of life take you, there is always something important to be learned.

Data Availability Statement

No data were presented in this paper.

Acknowledgments

The author wishes to thank The Department of Earth and Environmental Sciences and the University of Michigan for their making facilities available to me following my retirement in 2004 that has allowed me to continue my research efforts. I also thank all my colleagues (researchers, professors, modelers, graduate students, technicians, and shipboard personnel) from whom I have learned so much. I wish to express my appreciation of the National Science Foundation for their continued support of scientific ocean drilling and the scientists who go to sea. Each 2-month expedition on the drill ship results in at least 20,160 person-hours of work and study by shipboard scientists. It is money and time well spent.

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