KENNETH JINGHWA HSU

Challenger at Sea

A Ship That Revolutionized Earth Science

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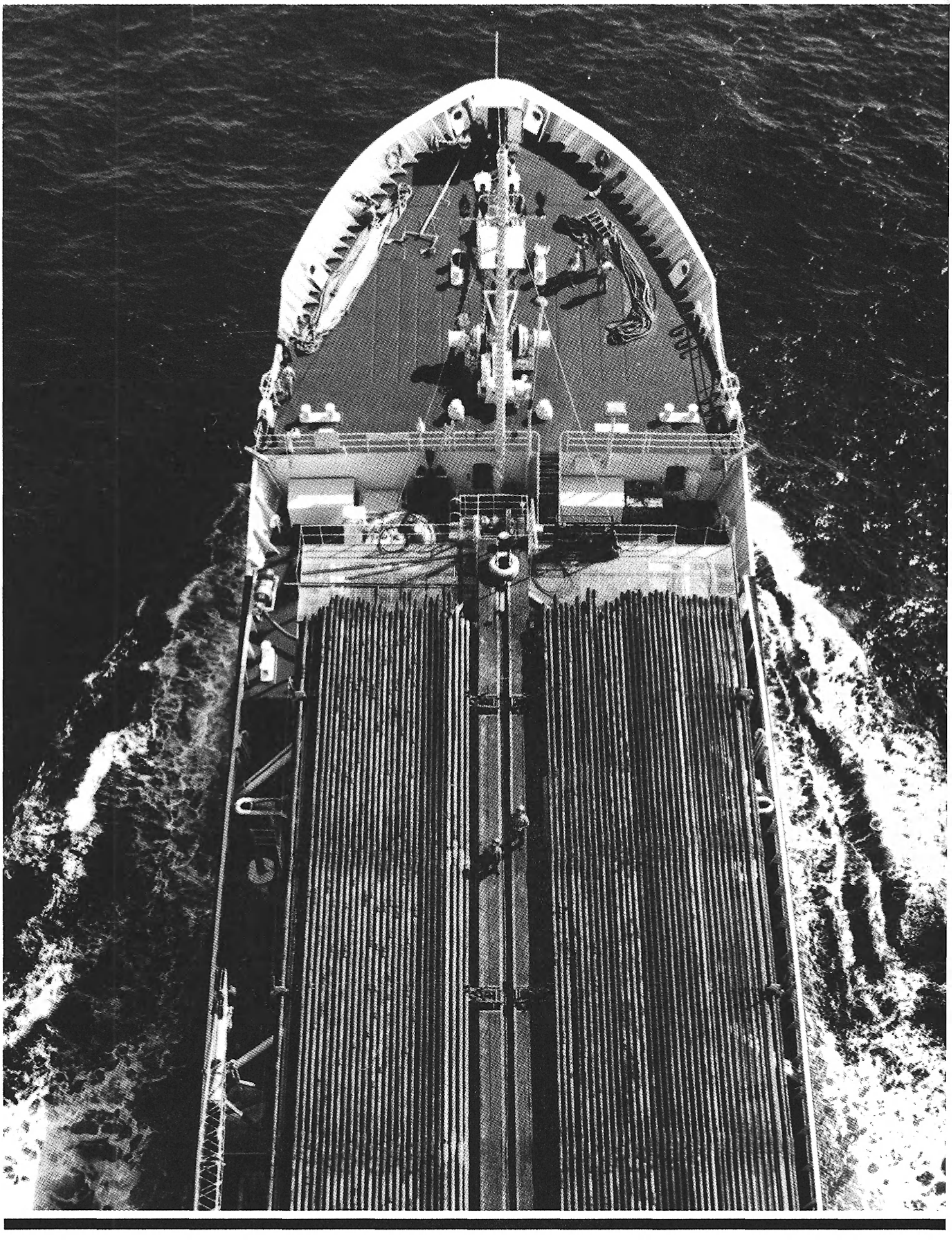

Challenger at Sea

A SHIP THAT REVOLUTIONIZED EARTH SCIENCE

Kenneth J. Hsü

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IN MEMORY OF Seymour Oscar Schlanger, 1927–1990

Scientist, Philosopher, and Friend

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Preface to the American Edition

I received a letter from Hoimar von Ditfurth in the summer of 1979 when I came back to Chengtu from a vacation in eastern Tibet; he asked me to write a book on ocean drilling for general readers. Von Ditfurth had been a medical doctor, and his contact with patients caused him to lament the communication gap between scientists and the public. He quit his medical practice and became a free-lance publicist. His book on evolution was a best seller, he was the editor of a popular science magazine, and he was well known for his science programs on German television. I had contributed articles to von Ditfurth's magazine, and helped with his television programs depicting the Mediterranean desiccation and the extinction of the dinosaurs.

I had wanted for more than a decade to write a book on geology for laypersons. Like von Ditfurth, I was alarmed by the false images of scientists presented to society. None of my four children chose a career in science. My daughter did study biology, but quit when she decided that modern bioscience was dominated by the financial interests of the pharmaceutical industry and gene manipulation. The image of scientists was not helped by popular books such as *The Double Helix:* in contrast to the portrait of a selfless Madame Curie, who devoted her life to seeking truth, the latter-day chemists pictured themselves as career-makers, pirating ideas while refereeing research proposals, stealing data by raiding and ransacking the offices of colleagues, all in the name of a race for the Nobel Prize. Physicists too had not helped their own image, when a few of the less scrupulous in their ranks told half-truths to promote atomic power. I wanted to write a book to show that we geologists, at least, are nice guys!

In 1970 I had written a manuscript called *The Mediterranean Was a Desert*, but I had not been able to get it published. A publisher friend, Bill Freeman, told me that the story was fascinating, but that the subject matter fell, as they say in German, "between chair and bench"—there was too much science for a trade book, but not enough for a scholarly treatise. I asked von Ditfurth if the *Mediterranean* manuscript was what he had in mind. No thanks, he answered; he wanted the whole story of the ocean drilling by

Glomar Challenger, and he sent me a contract with a generous advance.

I wrote the first draft of this book in the spring of 1980, while we were out drilling in the South Atlantic, just as Caesar wrote *The Gallic Wars* in the heat of battles. The advantage of this approach is its immediacy. To make a work on science more readable for laypersons, I decided to interrupt the flow of the narrative with biographical sketches, stories of amusing incidents, excerpts from my diary or from the daily operations reports, and even philosophical digressions. I was able to consult the shipboard reports of all previous cruises, which are considerably more spontaneous and truthful than the operations reports published later in the *Initial Reports of the Deep Sea Drilling Project* series. It would not have been possible to recapture the atmosphere of Jackson's Leg 55 or Schlanger's Leg 61, if I had not been able to read their daily operations resumes, written in moments of intense emotion. Only veterans could appreciate that we entered a different world when we sailed away on the *Challenger*. For this reason, I treasured the passages added from my diary, even though an editor might find them interruptive or irrelevant. The immediacy was also a disadvantage: words of anger can be polemical, and they do not make good writing.

My original intent to write about the people on the *Glomar Challenger* produced a 200-page manuscript. Hoimar von Ditfurth did not accept that; he wanted to have more science and more pages. He also pointed out to me that I had a contractual obligation to deliver a 400-page work.

The first revision changed the book from a source book on the history of geology to a readable textbook of marine geosciences. In addition to discussing our experiences while chasing magnetic lineations, for example, I had to add a long chapter 4 summarizing the history of the earth science revolution of the 1960s; I had to explain geology, in addition to recounting what we did and how. Von Ditfurth was happy with the new version, more so than I was.

I wrote the text in English; it was translated into German and published in 1982. A Chinese translation of the original English manuscript appeared in 1984. Then I looked in vain for an American publisher; I began to appreciate Bill Freeman's statement that the book, especially after the revision requested by von Ditfurth, probably did fall between the cracks.

It was, however, good reading for those who had sufficient knowledge of the subject matter, and they told me their impressions. Al Traverse found it a most valuable reference when teaching beginning geology to non-majors; he copied numerous illustrations from the book as handouts for his class. My daughter, Elisabeth, liked the way I tried to make geology interesting. Not knowing anything about the subject, however, she had to persevere to follow my suspenseful style. (At her suggestion, I now include a short epigraph at the beginning of each chapter so that readers know what is coming their way.) A chemist from Basel and an engineering professor from Zürich told me they had enjoyed reading the book and learning what their colleagues in geology are doing. Other friends told me that I should give up any hope that the book will be leisurely reading for everyone; I might as well accept the fact that my readers will only be those who want to learn something.

The reactions of Chinese readers were similar. Many of them had missed the earth science revolution, and they were happy to find a summary in Chinese in a readable volume. Others appreciated the historical perspectives and the philosophical insights. Thousands of copies were sold in a few months, but almost all to students or to professional geologists.

The manuscript of the English text lay in a drawer for years, and might never have seen daylight again but for two events. Bill Menard wrote *The Ocean of Truth*, a personal history of the earth science revolution. This was to be the first of two volumes, telling the stories until 1968, but Menard died before he could start the second volume on the history of deep-sea drilling. Although my manuscript had been written before Menard's, it happens to be a natural sequel, picking up the threads where Menard left off, with a minimum of repetition. About the same time, the paperback edition of *When the Mediterranean Was a Desert* was published, and Ed Tenner wrote me that the Princeton University Press would like to publish more science for general readers.

I asked Tenner if he would reconsider my manuscript on the *Glomar Challenger*, which had not been accepted because of a split decision. Writing in the throes of acrimonious competition for shiptime, I had been too harsh on some of my JOIDES friends—who happened to be referees of the manuscript. Their marginal notes had made me aware of my one-sidedness. Now I promised a revision that would delete all inappropriate "bursts of passion." (I can afford to make such a promise, because those who might be interested can find those passages in the German and Chinese editions.) Al Fisher was the final referee. He told me, and apparently also Tenner, that

"*Ein Schiff*... [the German title of this work] has to be published." Fisher could read more between the lines, because he was a veteran of the *Challenger*.

I was very pleased to have this chance to revise the text ten years later. First of all, the original manuscript was completed in 1981, two years before the termination of the *Glomar Challenger* drilling. The least I could do was present a complete history, 1968–1983. Second, I was no longer involved in the science politics of ocean drilling, having turned to field geology in China. What appeared important to me at one time, especially when I was aboard the *Challenger*, now seemed so trivial; I could take a distant view. Not only the "bursts of passion" (of which there were not too many in the original text anyway), but also the petty critiques are now deleted. Besides, I could now use the comments of previous readers to make the book better.

Instead of accepting the opinion that this book falls "between chair and bench," I would take the more positive view that there is something for everybody. The many veterans of the *Challenger* may treasure the memory of life on a drilling expedition. Friends and colleagues in geology might be interested to learn in more detail what we did out there. Beginning students of geology might find a readable reference, a narration with a historical perspective that helps them to understand the hows and whys. For historians of science, it is a source book imbued with the atmosphere of a bygone age.

With these objectives and those readers in mind, I carried out the revision. The book is now mainly for geologists, students of geology, and historians of geology. It is not easy reading for general readers, although it is written in a language understandable to anyone with a college education. It is not arranged in an orderly fashion, and it is interspersed with side tracks. It would not make a conventional textbook, but it could be an interesting reference after one has acquired systematic knowledge from classroom instruction. It is not a scholarly treatise on the history of geology, and the references are not footnoted. It is the story of a participant, and the partisanship is undisguised. The science historian will take it with a grain of salt, like one does when reading Caesar's *Gallic Wars*.

The German title of the book is translated as *The Ship That Revolutioned a Science*. I took the view that Vine and Matthews, like Karl Marx, wrote only the manifesto; the Leninistic action of revolution was carried out by the *Glomar Challenger*. On the eve of the creation of the Deep Sea Drilling Program, there was a schism between continental geology and marine geophysics. Tension was in the air, and the danger of "civil strife" was imminent. The drill vessel came onto the scene at the most timely moment. Geologists like myself would not be convinced until the revolutionary geophysical theories were verified by classical methods of geology, stratigraphical and sedimentological. Without the *Challenger* at sea, the theory of seafloor-spreading, like the theory of continental drift, would have divided, not united, the earth science community.

Organized around this central theme of revolution, the revised manuscript now includes four parts. The first four chapters, constituting Part One, describe the events leading up to the eve of the revolutionary action by the *Challenger*. The next eight chapters, making up Part Two, narrate the verification of the predictions by geophysical theories that triggered the revolution, as the *Challenger* sailed from the Atlantic to the Pacific and from there to the Indian Ocean during the first and second phases of the Deep Sea Drilling Project, 1968–1973. The next four chapters, Part Three, tell the stories of discoveries on new ground after the breakthrough, mainly those made during the third phase of the Deep Sea Drilling Project, 1973–1975, when the *Challenger* returned via the Southern Oceans to the Atlantic. The last four chapters, Part Four, are concerned with the mopping-up actions of the International Phase of Ocean Drilling, 1975–1983.

I would like to express my gratitude to the many people who assisted with the production of this book. Eva Pour typed the manuscript, Albert Uhr did the drafting, and Urs Gerber made the glossy reprints ready for reproduction. Ueli Brigel helped in many ways. I would also like to acknowledge my indebtedness to the many persons who gave me permission to reproduce illustrations. I appreciate the advice of many readers of the previous editions; their suggestions helped greatly in this revision.

Zürich, November 1990

Preface to the Chinese Edition

There have been two milestones in the history of the earth sciences: geology was founded in the late eighteenth century, thanks to James Hutton's uniformitarianism and William Smith's stratigraphy, and then underwent a second revolution during the 1960s and 1970s. Prior to the foundation of the science, the deluge described by the Bible was considered the first cause of all causes in geology. This was the basic philosophy of Abraham Gottlob Werner and his Neptunist students. The Hutton-Smith revolution was Baconian: observation was to be the basis of all conclusions. After the authoritarian influences of people like Roderick Murchison, geology became increasingly more doctrinaire. Charles Lyell's substantive uniformitarianism made the straitjacket even tighter: the past could not have been much different from the present. Carried to extremes, the doctrine said that past continents must have been situated where they are now. The theory of continental drift was thus considered fundamentally unacceptable by leaders of North American geology.

The revolution of the 1960s was spearheaded by marine geoscientists. Investigations of terrestrial magnetism, seismology, and geothermics had discovered a wealth of data that could not be explained by the doctrines formulated on the basis of observations made on land. The first cause of earth processes was now seen to be thermal convection in earth's mantle. The corollaries were seafloor-spreading, generation of seafloor magnetic lineations, transform faults, mid-plate volcanism, mantle hot-spots, and deformations on passive and active continental margins.

There was tension in the air, and the threat of civil (or uncivil) strife between the old guards of land geology and the young Turks of marine geophysics. The controversy was reminiscent of the battle between the Neptunists and Vulcanists of the late eighteenth century, or that between the "drifters" and "fixists" in the middle decades of this century. The *Glomar Challenger* was timely; using classic methods of stratigraphy, new postulates based upon fancy new theories were verified. The controversy was resolved, and the revolution of the earth sciences could be accomplished.

The Laplacian absolutism has become deeply rooted in many of us. It was thought that scientific truth would be within our grasp, if we just had enough observations, experiments, and data processing. One of the reasons I wrote this book was to delineate the subjectivity in science, and to point out the blind alleys. Analyzing the mistakes of my own career, I found the ultimate cause of many a poor judgment of mine in the Confucian philosophy of my upbringing.

I was once called a "traitor to the traditional cause," because I was a "fixist" for twenty years, and hoped to find in vertical movements the driving forces of all tectonic processes, before I was "converted" to the thinking of the seafloor-spreaders. Thinking it over, I realize that habits and emotions play a critical role in our scientific judgment. My Confucian respect for elders was reinforced by my emotional attachment to my teachers in America who helped me during dark hours of need; I defended their faith religiously. In chapter 5 of this book, I describe the agony of the realization that I had been wrong, that my beloved teacher had been wrong. I understood only all too well Werner's pupil, Citizen Jean François d'Aubuisson de Voisin, when he said:

> The facts which I saw spoke too plainly to be mistaken; the truth revealed itself too clearly before my eyes, so that I must either have absolutely refused the testimony of my senses in not seeing the truth, or that of my conscience in not straightaway making it known. There can be no question that basalts of volcanic origin occur in Auvergne.

I too was staggered by what I saw, and there could be no question that the seafloor has spread.

Loyalty is a Confucian virtue, and vanity is a Confucian vice, but both are fatal to good judgment in science. Again in chapter 5 of this book, I tell of my hurt vanity when the selection committee chose Vine's theory of seafloor-spreading, not my observations on Franciscan mélanges, as a frontier lecture of the 1966 San Francisco Geological Society meeting. If I had swallowed my pride and attended Vine's talk, I might have realized then and there that ocean-floor subduction and the genesis of mélange were corollaries of his theory; I might not have stayed lost in a dead end for another three years.

The human element figures not only in our failures, but also in our successful endeavors. Courageous persistence in the face of adversity was exemplified by Sy Schlanger's explorations of mid-plate volcanism and by Dale Jackson's verification of the hot-spot theory. They were not the only virtuous ones; there were many acts of chivalry, generosity, modesty, and compassion by my friends who set sail on the *Challenger.*

This preface was written at the request of Professor He Qixiang. I have, therefore, emphasized aspects of our Confucian failings. I hope that the publication of this work in Chinese helps remedy an injustice—my Chinese colleagues missed the earth science revolution of the 1960s. In the midst of the so-called Great Proletarian Cultural Revolution, they had to read the *Selected Writings of Chairman Mao,* while their colleagues in the West were reading Vine and Matthews's manifesto on seafloor-spreading. Chinese scientists lost their chance to join this historic effort because of the evils committed by the "Gang of Four." Now that reform has been effected, my Chinese compatriots can march with their international colleagues in the pursuit of truth. I have high hopes for their eventual accomplishments, and I hope the lessons of history told in this volume will be helpful in a small way.

Peking, August 1983

Preface to the German Edition

This is a book about a ship, about a revolution, and most of all about the people who made the revolution with the ship. The ship is the drilling vessel *Glomar Challenger.* The revolution is the revolution in the geological sciences. The people are my teachers, my friends, my colleagues, my students, and, of course, myself. The book is, therefore, largely autobiographical.

The *Glomar Challenger* has been cruising since the summer of 1968, and hundreds, if not thousands, have participated on her cruises. The initial reports of the drilling cruises will include about one hundred volumes when they are all published, and each has more or less one thousand pages. In my book there will be omissions, obviously, and some aspects will be unduly emphasized. Having declared this book to be autobiographical, I hope I will be excused if I have concentrated on subjects I know more about, or on the people I know best, including myself. I can be excused, perhaps, because I was lucky enough to have been involved in many of the activities, and acquainted with most of the people, responsible for this revolution in geology. I hope my omissions are not essential to my story, and I hope my omissions will not be considered a slight of the many achievements by numerous persons not mentioned in this slim volume (slim compared with the hundred monographs that claim only to be the *initial* reports of the Deep Sea Drilling Project). If I have made serious omissions, I hope to be told, and to make corrections if there is a revised edition. Meanwhile, it should be remembered that I have written a personal memoir, not an official history of the research cruises of the *Glomar Challenger.*

One of the reasons that prompted me to write this book was that I wanted to present geologists as we think we are. Geologists in Hollywood movies or in detective stories are often odd people. Since geology is not usually taught in secondary schools, most people know little about the subject, and even less about the persons in the profession. Distinguished geologists, like distinguished persons in other professions, are devoted to their science, but most of them are not fanatics, and they are distinguished most of all by their modesty, even if they rarely suffer fools.

Another reason to write such a book for laypersons is to illustrate with one particular example how science is made. Science is a human endeavor, and scientific ventures can bring about frustration, disappointment, or even anguish or pain, but they can also lead to contentment, fulfillment, joy, and happiness. There is jealousy, selfishness, arrogance, littleness, but also generosity, modesty, humility, and fairness. Mistakes in science are made by all, and I have tried in this work to analyze the emotional makeups that led to mistakes, and certainly included a self-analysis of my own errors.

The *Glomar Challenger* has sailed to all parts of the world's oceans, she has been active longer than a decade, and all matters of geology have been the subjects of research. It was not an easy task to decide if the organization of the book should be geographic, chronological, or thematic. The compromise—using all three formulas—has its imperfections. I started with *Glomar Challenger*'s inaugural journey from a port in the Gulf of Mexico, and continued with a narration of her cruises to the Atlantic to test the seafloor-spreading theory, to the Pacific and the Indian oceans to check out predictions of the theory of plate-tectonics, to Antarctica to explore the past history of climatic changes, and back to the Atlantic to investigate the newly discovered dramatic happenings in ancient oceans. In fact, except for a short incursion to the Pacific early in the project, this was more or less the ship-track of the *Glomar Challenger* during the first three phases of the Deep Sea Drilling Project. Naturally, the narrative was not able to follow a strictly chronological order; the ship has been back and forth between the Atlantic and the Pacific repeatedly, and the same themes have been investigated over and over again. The last four chapters of this book summarize the last phases of the Deep Sea Drilling Project after the program was internationalized in 1975. New explorations were being made, and new problems were coming up, providing ammunition for a future revolution.

The manuscript was completed a year ago. Meanwhile, the *Glomar Challenger* has been continuing her activities. I should not let this book go to press without mentioning at least a few of last year's successes. Off the West African margin, a sample of a 130-million-year-old salt was brought up, a feat that had been attempted several times before. East of Barbados, the drill string penetrated ocean sediments that had been thrust under the edge of continents—another major achievement. Finally, the 600-meter barrier, which had hindered all previous drilling into the ocean crust, was broken; the Leg 83 team managed to deepen a previously drilled hole in the Pacific and drill more than one thousand meters into the ocean crust! It has now become obvious that a drill ship is not just an *ad hoc* tool for testing some new ideas, to be abandoned after the fulfillment of a mission. Like Galileo's telescope, which revolutionized astronomy, and Lawrence's cyclotron, which revolutionized nuclear physics, a drill ship is an indispensable tool for the advancement of the earth sciences. I have been invited by the U.S. National Science Foundation to a meeting next week to discuss the future of ocean drilling. There are ambitions to use a big vessel and continue ocean exploration until the end of this century.

I would like to thank all who helped make the completion of this book possible, especially Albert Uhr and Urs Gerber for their preparation of illustrations, Carolina Hartendorf and Barbara Das Gupta for their secretarial assistance, and Dr. Ueli Briegel and my wife, Christine, for their help with minor revisions of the German text. I would also like to gratefully acknowledge the kindness of the persons and organizations that allowed me to reproduce their figures.

The book is written for my late wife, Ruth, and for the *Schlüsselkind* she had hoped to care for. It is written for my children, Elisabeth, Martin, Andreas, and Peter, so that they would be able to understand their father better. Most of all, it is written for my wife, Christine, for all that she has done.

Zürich, May 1982

Acknowledgments

In this book, numerous illustrations have been reproduced from, or redrafted on the basis of, illustrations in earlier publications. I would like to thank the following persons and organizations for permission to reproduce figures from their works.

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PART

One The Eve of a Revolution

1963–1968

CHAPTER

1 Moho and Mohole

Harry Hess wanted to drill down to the Moho, the base of the earth's crust. The venture failed, but technological innovations developed for the Mohole Project made the Deep Sea Drilling Project possible.

Impressions of a Scientist

As they say in America, if you dig a deep enough hole, you will reach China. Nobody ever seriously prepared to do so, but Harry Hess once did persuade the Congress of the United States to invest millions of dollars in a hole to the mysterious realm of Moho, ten kilometers below the ocean.

I first came across Harry Hess in the Prudential Insurance Building in Houston, Texas. That was February 1954. I was fresh out of graduate school, and had just pulled down my first job, with the Exploration and Production Research Laboratory of the Shell Oil Company. Hess was already a well-known geologist, then chairing the Geology Department of Princeton University. He came to Houston on a fund-raising tour. For his talk to the Princeton Alumni Association, he chose to speak on *guyots*—the newly discovered sunken flat-top mountains of the Pacific.

Harry Hess had been the young commander of the troop-transport M/S *Cape Johnson* of the U.S. Pacific Fleet during the Second World War. While ferrying troops for landings in the Marianas and the Philippines and on Iwo Jima across the Mid-Pacific, the echo-sounder of his vessel registered a seafloor topography that was far from monotonous. Undersea mountains appeared one after another on the echogram; they rose thousands of meters above the deep ocean floor (Figure 1.1). They all had steep sides and a flat top, looking somewhat like mesas on the Colorado Plateau of the western United States. Crisscrossing over the mountains revealed a circular outline. This morphology indicated to geologist-commander Hess that those underwater features were sunken volcanic islands with their tops chopped off. He called them guyots, in honor of

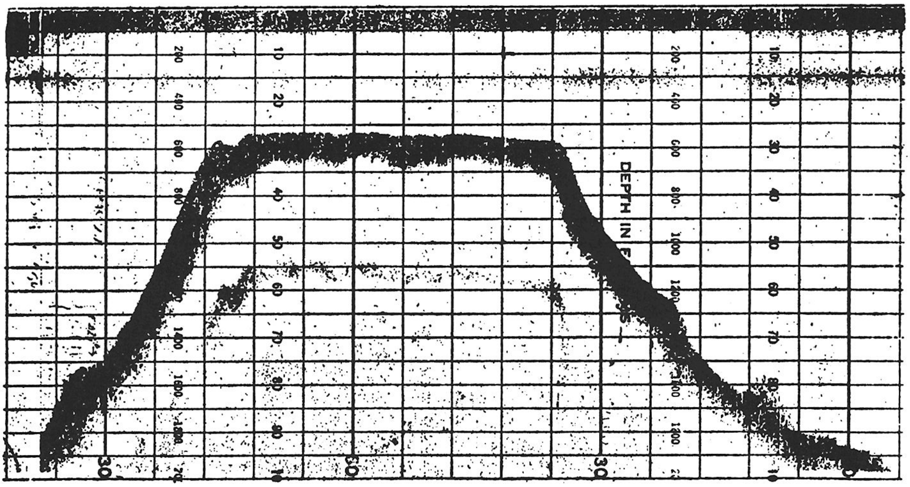
Arnold Guyot, Princeton's first geology professor, who also lent his name to the building that houses the Princeton geology department. When the war ended, Hess could report the discovery of 160 of the drowned islands in the Pacific basin.

Oceanographic vessels returned to Hess's old hunting ground and were able to find more guyots. They also used a tool for dredging, a scissor-like hook at the end of a long steel cable, to grab and twist off pieces of rock from the steep sides of the sunken islands. They turned out to be mostly volcanic rocks, as Hess had expected.

Submarine volcanoes are not uncommon on the present-day seafloor; they are called *seamounts*. What made the guyots unusual were their flat tops. Hess assumed these were caused by erosion—the guyots were once volcanic islands, but their conical tops had been planed off by the pounding of ocean waves. The islands seemed to have stood firm while waves were doing their job of cutting down the parts above sea level. Then, for some reason, subsidence started, and the flat-topped islands sank three or four thousand meters to become guyots.

I was always quick to jump to conclusions. I had learned in school that volcanoes in the oceans should sink under their own weight, but the rate of subsidence is finite. When seamounts (underwater volcanoes) are active, molten liquids, or lavas,

1.1. Guyot. Using echo-sounding, Harry Hess discovered numerous flat-topped submarine mountains, which he named guyots. The one shown in this figure rises 2000 m above the surrounding ocean floor, and the top is about 1000 m below sea level. The diameter of this guyot is more than 20 km. Deep-sea drilling has verified the postulate that guyots are sunken volcanoes.



pouring out of the interior of the earth pile up so fast that subsidence cannot keep up with the build-up. The seamounts eventually rise above sea level, forming volcanic islands. However, volcanoes become inactive sooner or later. Then ever-present gravity does its work and pulls the volcanic islands back down to the abyss.

That a volcanic island should sink under its own weight in an ocean had been, in fact, a common assumption in geology. Charles Darwin invoked the idea to explain the origin of coral atolls. Therefore, I was a little surprised that Hess should be disturbed by the presence of guyots in the deep sea. Too shy to bring up the point during the discussions after the talk, I nevertheless wrote Hess a letter.

I received a prompt reply from Professor Hess, typed single-spaced on three sheets of stationery, apparently by himself. He apologized that he could not find time to give me a more detailed explanation; the usual activities of a newly begun semester had taken a heavy toll on his working hours. He was also apologetic about his less than professional skill in handling a typewriter. Just the same, he patiently explained to the slightly arrogant young man why the simple-minded scheme of subsidence under load cannot explain all the problems in connection with the origin of guyots.

The facet of the problem that bothered Hess most was the flat top. Waves did their work slowly. Why did the islands seem to have paused for such a long time, waiting until the waves had removed mountains and cut the volcanic islands down to sea level? When and why did the flat-topped mountains start their irrevocable journeys down to the abyss? After discussing the pros and cons of the various explanations at some length, Hess reiterated his puzzlement and questioned the possibility of a simple answer.

Years later both of us learned more about the oceans. Guyots did indeed sink under gravity. Hess's difficulty could be traced to his assumption that the flat top was due to erosion. When we look at the islands of the Pacific today, some with volcanoes rising hundreds of meters above sea level, it seems a formidable task indeed to cut those volcanic mountains down. However, flat-topped islands—such as Saipan, Tinian, and others that were used as air bases for U.S. B-24 bombers during the war against the Japanese—are not uncommon. They are flat not because waves pounded the mountains down, but because the topography has been evened out by the deposition of flat-lying sediments. Elsewhere, flat-topped islands may owe their lack of relief to horizontally accumulated lava flows or ash beds. If we accept Darwin's theory that atolls build on a sinking volcanic foundation, the sediments trapped in the lagoons of an atoll should gradually cover the tip of the volcano to make a flat top. Those atolls on which coral grows too slowly eventually sink to become guyots. From this point of view, guyots are simply coral atolls that died young. In the tropical Pacific, where coral growth could always keep pace with the sinking of the foundations, sunken volcanoes are now crowned by living reef corals, as Charles Darwin once theorized.

I shall come back to the story of guyots in a later chapter. What impressed me back in 1954 was not what Hess wrote, but that he did write. I was not convinced by his arguments then, but I did treasure the autograph from the famous man.

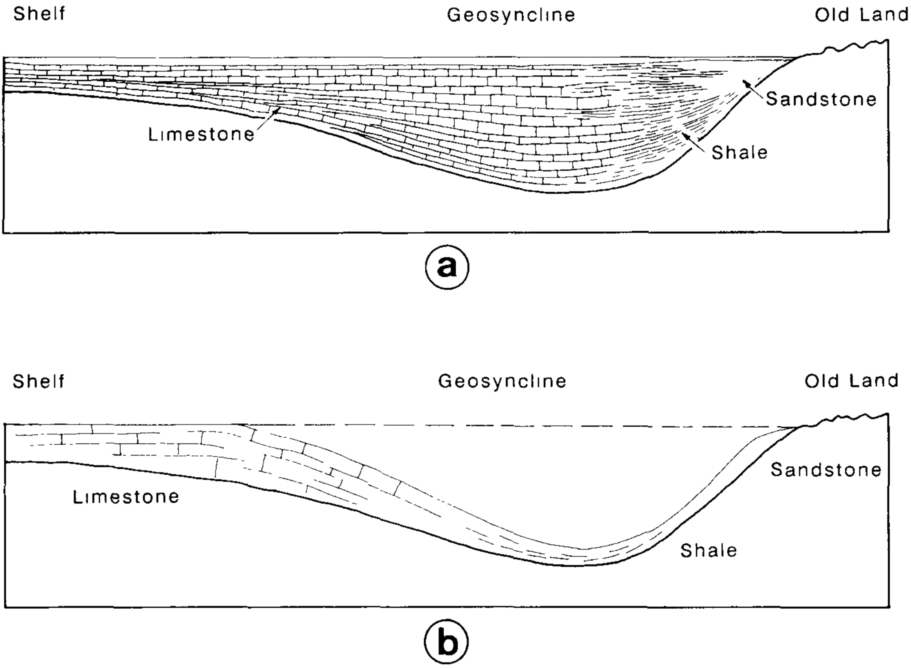
My second encounter with Harry Hess took place in Washington, D.C., a few years later. I was appearing before a distinguished national audience during the annual meeting of the American Geophysical Union to present my first professional lecture. It was scheduled as the first talk of the first session—a rather ticklish spot. Furthermore, I had sent in the theme in a moment of lightheartedness, and I came to realize only belatedly that I had chosen a poor subject for a start. Normally a young man beginning his professional career would choose to report on a fact-finding research project, in which he could make a good impression on his peers by demonstrating his intelligence and diligence. I had instead selected a theoretical subject—the origin of geosynclines.

The concept geosyncline was invented by James Hall, a nineteenth-century geologist, to designate ancient sites of sediment accumulation that seemed destined to become mountain chains. Hall was the State Paleontologist of New York, and he studied the Paleozoic rocks in the Appalachian Mountains and in the plateau country to the west of the mountains. The mountain rocks, which are deformed, are quite similar to those on the plateau, which are flat. However, Hall found that a rock formation of any given age is several times thicker in the mountains than on the plateau (Figure 1.2); strata seemed to have been crumpled where they are much thicker. Before the sedimentary formations were crumpled during the process of building mountains, they had been laid flat. The bottom of a sedimentary pile should thus be most depressed where the pile is thickest. Such a warped surface had been called a *syncline* by geologists, and James Hall added geo- to the expression to emphasize its imposing dimensions. European scientists noted, however, that crumpled strata in the mountains were not always thicker than flat strata in plateau country, but the former seemed to have been deposited in deeper water. They too used the expression geosyncline to designate a site that was eventually to become a mountain chain.

The term geosyncline was to become a catchword, and appeared in everyone's theories even though no one seemed to know what a geosyncline was. There were no modern analogues. Instead, people speculated, somewhat idly, on why geosynclines sank, permitting the accumulation of thick piles of sediments. In the short discourse I planned to present orally, I would try to demythify the concept. I had some simple arguments to show that geosynclines are simply depressions on the surface of the earth, such as ocean basins, continental margins, and so on. After sediments were accumulated, the seafloor would subside under the added load, just as guyots sink under the weight of added volcanic piles.

The idea was not new, although I did give it a new twist with some recent data. The idea was also not all that bad; what I said

1.2. Geosyncline. James Hall in 1859 postulated that sediments of mountains form unusually thick sequences (a); his European colleagues found that such sediments were deposited in deep-sea troughs (b). Both types of depositional sites were called geosynclines, and a geosynclinal theory of mountain-building was introduced. This dogma led to a blind alley and hindered the progress of geology for a century, and is now replaced by the theory of plate-tectonics.



then is still valid today, although I did miss a point or two (see chapter 6). However, it was presumptuous for a young man to tell his seniors the obvious; I grew increasingly nervous as the time of the meeting approached.

At the appointed hour, I went to the General Services Administration Building in Washington, D.C., where the meeting was being held. I was early, half an hour early. Gradually people filed in, and many venerated scientists of the older generation took their seats in the front rows. Hess was to open the meeting and chair the first session. Eight o'clock struck, the hour I was to give my talk, but he was nowhere in sight. As the time passed, 8:05 . . . 8:10 . . . 8:15 . . . 8:20, I sat there and became more and more edgy, gradually losing my nerve. Finally, at about half past eight, Hess ran in and climbed up to the rostrum. Wiping sweat off his brow, he murmured an apology—he had looked for the wrong GSA (Geological Society of America) Building, and nobody seemed to know where it was. (It was in New York City!) Catching his breath, he tried—without remarkable success—to pronounce my name, as he introduced the first speaker. By then I was completely overcome with stage fright. When I stepped before the microphone, I realized that I could hardly find my voice. I stuttered, and had to read my written script in broken sentences. I did finally stagger through, but was met by dead silence at the end of a miserable presentation. It was a "bomb," as they say in show business, and the lack of response was a manifestation of the hostility toward the brash young man. Hess, however, was genuinely sorry; he thought that people remained silent because they did not want to start discussions, as the session had been delayed by his late arrival. He apologized profusely to me after the meeting, when I was blaming myself for my less-than-brilliant "début."

As the years went by, I learned some self-confidence, while Hess remained modest. He did much for the earth sciences. His idea of seafloor-spreading started a scientific revolution, and his quest for an imaginative research project opened a new era of ocean exploration.

The last time I saw Hess was in the spring of 1969. We had just completed a drilling cruise in the South Atlantic (chapter 5), and I had come to Princeton to discuss the cruise results with my shipboard colleagues. This cruise has achieved fame as the expedition that verified the predictions of the theory first elaborated by Harry Hess. However, Hess was so modest that he actually seemed embarrassed to be proven right. Toward noontime, Hess and I walked from Guyot Hall to the Princeton Faculty Club for lunch. He did not mention the subject of seafloor-spreading, nor did we even discuss our findings. Instead, we chatted about the difficulties of privately endowed universities in the days of high inflation. He had just stepped down as chairman of the department, but he still had many national and international "obligations." He was overworked and seemed tired.

Hess died of a heart attack shortly after my visit. He will always be remembered by us as the one who made the first move that led to one of the most successful undertakings in the earth sciences—the Deep Sea Drilling Project (DSDP) of the Joint Oceanographic Institutions for Deep Earth Sampling (JOIDES).

Moho

One of Hess's many "obligations" had been to serve on a committee to help select research projects worthy of financial support by the U.S. National Science Foundation. Back in the fifties, when physicists were asking for and getting more and more funds for bigger and better accelerators, geologists seemed to content themselves with pocket money for making geological maps. It was said that Hess and his committee were going over one research application after another on a spring day in 1957. They grew increasingly restless and irritated after finding nothing exciting in the proposals, and began to ask themselves if anything worthwhile in geology was still left to be done.

The heroic age of geology, spanning the last decades of the eighteenth and the first half of the nineteenth century, had long since become a nostalgic memory of a far distant past, an age in which James Hutton founded geology as an observational science, William Smith discovered the value of fossils in ordering strata in temporal sequences, Charles Lyell demythified the book of Genesis and preached uniformitarianism, and Charles Darwin came up with his theory of evolution. It seemed that few discoveries of comparable significance had turned up during the twentieth century. Progress was, however, being made in geophysics, such as the discovery of the Moho.

On 8 October 1909, Yugoslavia was struck by an earthquake whose epicenter was some 25 kilometers south of the village of Papuspsko, near Zagreb. A local geophysicist, Andres Mohorovicic, made a routine study of the time it took the first earthquake waves to reach various registering stations—the so-called *first-arrival time.*

The shock of an earthquake sends out several different kinds of waves. Some are transmitted by alternately compressing and extending an elastic medium, similar to the way sound waves travel in air. These are called *compressional waves*, or *P-waves.* They are the fastest and should be the first registered by a seismograph at any station. In a homogeneous medium, the speed of propagation of the compressional wave, V_p, should be constant. Consequently the first-arrival time t should be directly proportional to the distance between the epicenter and the registering station, S, or

$$t = S/V_p .$$

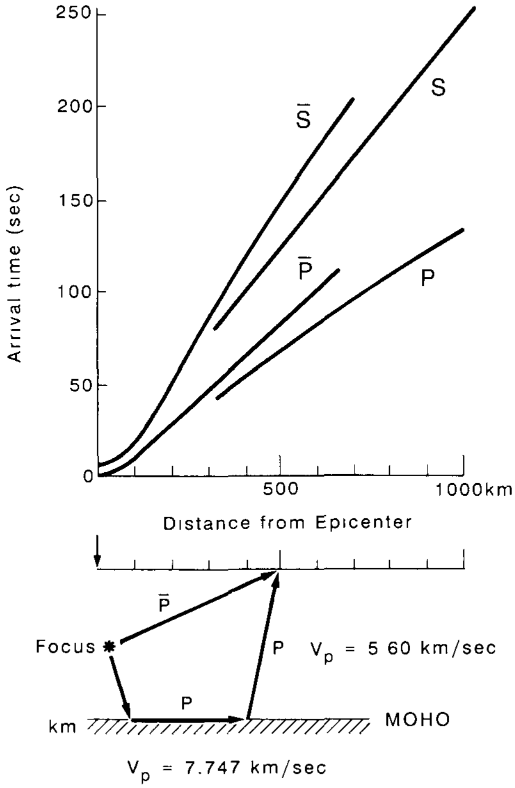
This is, of course, a simple linear relation in Newtonian kinematics that we all learned in middle school. Traveling to a station 200 kilometers from the epicenter should take the wave twice as long as traveling to a station 100 kilometers from the epicenter. Plotting the arrival time against the distance traveled for the stations within 300 kilometers of Zagreb, Mohorovicic obtained a straight line through the origin, illustrating that the simple predicted relation was indeed confirmed by the seismic records (Figure 1.3). As our middle-school physics teacher tells us, the slope of the straight line in this figure, or the travel time divided by distance traveled, is the inverse of the speed of the wave propagation. Mohorovicic's calculations showed that the compressional waves were traveling at a speed of 5 to 6 kilometers per second when they reached those stations.

Records of the earthquake were available from other seismic stations, with the most distant one at Tiflis in the Caucasus, some 2,400 kilometers from Zagreb. To his surprise, Mohorovicic discovered that the first-arrival times of stations more distant than 300 kilometers did not fall on the straight line going through the origin. Instead, the arrival times at those stations, when plotted against distance traveled, gave another straight line, which did not go through the origin; the line also had a more gentle slope (Figure 1.3). The slope indicated that the fastest earthquake waves traveled to more distant stations at 7 or 8 kilometers per second. How was he to explain this faster speed? Was there another kind of wave that traveled faster, or did the same compressional waves take a faster route?

The example of a person driving an automobile can easily provide an answer. If someone wants to travel from Zürich to Kilchberg, some 10 kilometers away, he takes the shorter lakeshore highway, despite the 50 km/hour speed limit. If, however, he wants to drive to Weesen, some 60 kilometers away, in the shortest possible time, he takes a detour to get on the freeway, where he can drive at 120 km/hour. Mohorovicic used the same logic to explain his data on travel times: the fastest-traveling waves to all stations are compressional waves, but the ones that made a detour to the freeway became the first to arrive at stations more distant than 300 kilometers. In scientific terms, the "detour" is called "wave refraction," and the "freeway" is a deeper and denser medium that permits a faster propagation of waves than the shallower underground.

We have known since the time of Isaac Newton that the interior of the earth is denser than the near-surface ground. In the middle of the last century George Airy, the Royal Astronomer of Great Britain, speculated that the earth had a thin, lighter, solid shell enveloping a denser, liquid interior, from which hot lavas from volcanoes were derived. He called the outer, solid shell the "earth's crust." Lighter crust floating on heavier liquid should maintain a flotation equilibrium. Such a model of gravitational balance has been called Airy's model of *isostatic* (equal pressure) equilibrium (see chapter 4). Both the sinking

1.3. Moho. Mohorovicic noted that seismic waves traveling to more distant stations move at greater speed, as shown by this reproduction of his record of the 8 October 1909 earthquake in Yugoslavia. He concluded that those faster waves moved through a denser rock (the earth's mantle), which is separated from the upper rock-layer (the earth's crust) by a boundary, which is now called the Moho.

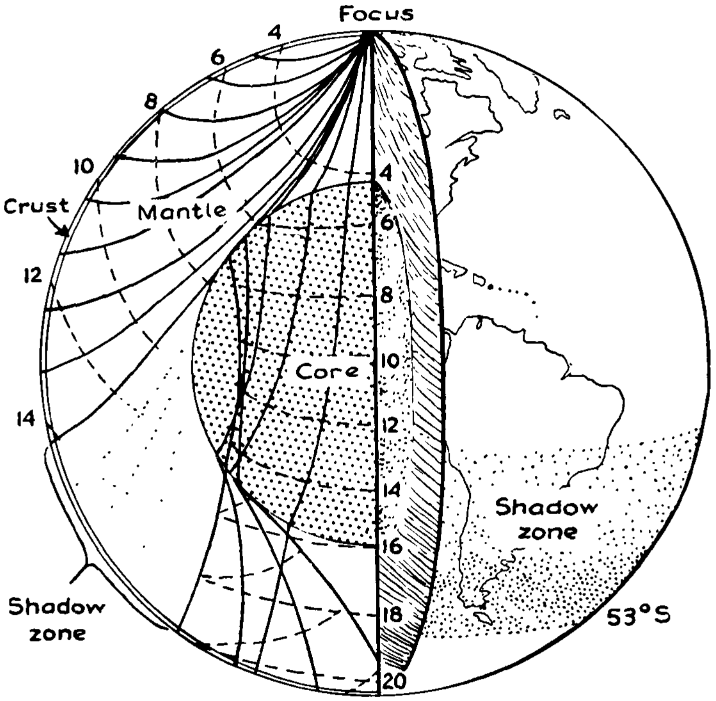


of guyots in the oceans and the subsidence of geosynclines under sedimentary load have been interpreted in terms of Airy's model. The now famous theory of continental drift advocated by Alfred Wegener during the early part of this century also invoked isostatic equilibrium to justify the "drift." Wegener's theory was rejected by geologists because they listened to their geophysicist colleagues, who maintained that the crust could not have been floating on a heavier liquid substratum, because the heavier rock material beneath the crust is a solid, not a liquid. What is the basis of this geophysical conclusion?

We have mentioned that an earthquake shock sends out several kinds of waves. The P-waves (compressional waves) are the fastest. Trailing behind are the slower-moving S-waves (shear waves). P-waves can go through solid and fluid alike; we hear sounds because acoustic waves are compressional waves and can be transmitted by air. S-waves, however, cannot go through a liquid. Through the analysis of seismic records, geophysicists have found that S-waves are not registered at some very distant stations, proving the presence of liquid in the earth's interior (Figure 1.4). However, the liquid interior is much deeper than Airy assumed; S-waves travel in a solid medium down to a depth of 2,900 kilometers below the surface. Farther down is the liquid core of the earth (Figure 1.4). This set of data permitted the recognition of an earth's core enveloped in a 2,900 km thick earth's mantle. Now Mohorovicic's data told him that the mantle has a lighter outer skin, which is the earth's crust; the crust under the continent is about 50 kilometers thick. P-waves going directly through the crust to nearby stations traveled at a speed of 5 or 6 kilometers per second, while P-waves making a detour through the mantle to more distant stations could manage a speed of 8 kilometers per second. Between the crust and the mantle a difference occurs in the physical properties of the earth's rock material—a finite difference, a discontinuous change. Because it was Mohorovicic who first recognized this discontinuous change in seismic velocity when a wave traverses the boundary between crust and mantle, geophysicists now use the term *Mohorovicic's Discontinuity* to designate the surface separating the crust from the mantle. With time, the term was shortened to Moho.

The rocks we see on or near the surface of the earth constitute the crust. One can make some deductions regarding what rock types are underground by studying the speeds at which various rocks transmit elastic waves and comparing them with the actual speeds of wave propagation through the crust and the mantle. The most common rock type hauled up by dredging in the oceans is *basalt*, which is solidified from lavas erupted by volcanoes. Basalt is a *mafic* rock—that is, its chemical composition is rich in magnesium and iron (ferrum). The minerals making up basalt are very-fine-grained, mostly small crystals (less than a millimeter long), because hot lavas erupted onto the seafloor are quickly quenched. If a molten liquid of the same composition as lava is placed in a crack or fissure one or two kilometers below the seafloor, the same minerals would crystallize out, but those crystals could grow slowly to larger sizes, several millimeters long. Such a slowly cooled rock is in all other aspects identical to basalt, but geologists choose the name *gabbro* to distinguish the coarse-grained variety. Since the speed of wave propagation through the ocean crust is about the same as that measured through basalts or gabbros, earth

1.4. Internal structure of the earth. Compressional waves can be transmitted through *a solid medium; they can go through the liquid core, but they cannot reach the so-called* shadow zone, as shown by this figure. Shear waves cannot be transmitted through the core, and thus cannot travel beyond the shadow zone. The structure of the earth has been determined from earthquake studies. Numbers on diagram denote minutes required for the travel of P-waves.



scientists agree that the crust under the ocean is made up largely of those rocks. The common rocks exposed under sedimentary strata on continents are granites and gneisses. However, data on speeds of wave propagation suggest that the lower part of the earth's crust under continents may also be a mafic rock.

Earth scientists could not agree during the 1960s on what kind or kinds of rock are in the mantle below the Moho. The rocks of the mantle, which transmit elastic waves at decidedly higher speeds, are certainly something other than basalts or gabbros. Two possibilities have been suggested: eclogite, which has the same composition as basalt (but these same chemical compounds have been crystallized into minerals considerably denser than those in basalt or in gabbro), and peridotite, whose chemical composition is different from that of basalt. (Peridotite has less silica, less aluminum, and less calcium oxide, but more magnesium oxide, than basalt: it is also denser than basalt.) Seismic waves can travel through either eclogite or peridotite at a speed similar to their speed through the mantle.

Earth scientists were interested in the mantle, because a knowledge of its composition would influence our interpretation of geological processes. Throughout the middle decades of this century, the overwhelming majority of North American geologists belonged to what is now called the "fixistic school"—they believed that the continents and oceans were permanent and had been fixed in their positions since the beginning of the earth. Of course, the evidence could not easily be ignored that ancient continents had sunk to become deep seas or that ancient seafloors had risen to become mountains. However, the "fixists" postulated that all such earth movements were predominantly up and down, and that there had been little horizontal displacement such as that advocated by "mobilists" such as Alfred Wegener with his theory of continental drift. For the "fixists," the assumption of a mantle made up of eclogite would solve all their problems. Basalt (or gabbro) and eclogite have the same composition, but basalt may become eclogite if it is subjected to a great pressure, or eclogite may be changed into basalt if it is heated up. When basalt is turned into eclogite, the volume decrease causes the ground to sink; continents would turn into oceans, and the subsidence could permit the accumulation of the so-called geosynclinal sediments. When eclogite is changed into basalt, the volume increase causes the ground to rise, and deep seas would be converted into plateaus or mountains, as shown by the rocks of the Alps.

The postulate of an eclogite mantle was favored mainly by theorists during the 1950s. Geologists working in the field