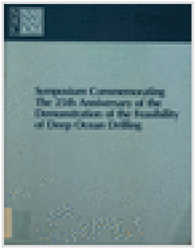

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Symposium Commemorating The 25th Anniversary of the Demonstration of the Feasibility of Deep Ocean Drilling

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Commission on Physical Sciences, Mathematics, and Resources
National Research Council

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OPENING REMARKS

Philip M. Smith, Executive Officer,
National Research Council

On behalf of Frank Press and Bob White, I want to welcome you all to this day-long symposium. It should be an interesting day for those who participated in, those who remember, and those who may be hearing today for the first time about the great events of some 25 years ago and about how we progressed from Mohole to the Deep Sea Drilling Project and onward into the current program.

In 1958, the Director of the National Science Foundation asked the President of the Academy to set up a formal committee to, in effect, codify and make legal the informal operating committee that had been in place for a year or two to think about how one might exploit the new technology then emerging to undertake deep sea drilling. Willard Bascom will tell us a great deal about that exciting period of time.

It's hard to remember now, because of the intervening years, how much groundbreaking went on in that period. The Science Foundation was then a young eight-year-old agency; it had limited experience with large consortiums, big operational projects, and national programs like the ocean programs and Antarctic program. So there was a certain degree of learning going on in that period on the part of public officials and the scientific community about how large-scale endeavors of this sort could be mounted and supported at the taxpayer's expense. Scientifically, of course, we were in a different era, just at the beginning of the International Geophysical Year (IGY) and all those things we were learning. It is hard to now imagine that there were geologists who did not believe in plate tectonics. But there were people, myself among them, who had been taught by glacial geologists like Goldplaithe and Flint that the last ice age ended about 200,000 years ago. We have, in fact, made remarkable sets of scientific discoveries and technological advances, and these have been exciting developments in international cooperation. In addition, we have learned a great deal about the management of large-scale scientific enterprises like the DSDP.

We are here today to review both the history (the early years, and the early developments) and the evolution of the Ocean Margin Drilling program that is now under way. In its early years, I was aware of and knew sketchily about many of the developments in the Mohole project. At that time, my focus was largely on the Arctic and the Antarctic regions, and my proximity to the program was somewhat tangential. In the late 1970s, I had a closer look at the program when I was in the White House Science Office and we were talking about joint ventures between government and industry. Although the original plans did not come into being, I learned a great deal about relationships between government and industry and, particularly, about how unstable those relationships could sometimes be. Who would have dreamed in 1978 that the cost of energy would be what it is today and that oil companies would be rapidly withdrawing from commitments of every kind?

Dr. Robert M. White, President, National Academy of Engineering

Philip Smith has already welcomed you on behalf of the two Academies, but because of my previous association with this program at the time of the formation of the Joint Oceanographic Institutions, I wanted to welcome you to this symposium. When I occupied Phil's position as Executive Officer of the Academy and Administrator of the National Research Council in 1980, I was asked by members of the ocean community whether I could assist in strengthening the Joint Oceanographic Institutions. In fact I did, and it was a very exciting time for me. Today, some of the key people involved, Bill Bascom first among them, will provide you with an extended perspective on the ups and downs of the program.

If one had to identify a single scientific and engineering program that has contributed more than any other in recent years to our fundamental information about the planet, there is no question in my mind but that the ocean drilling program would have to be ranked at the top. We can now anticipate another period of great excitement as the new drilling vessel undertakes its work.

I was also in a position to watch and be involved in those ups and downs. I think there is a lesson here in the history of the Deep Ocean Drilling Program for all of us; the program's success is testimony to the indispensability of having an important and seminal idea, sound engineering, a long-term perspective, and a firm grasp of what is needed from both science and engineering.

But my task is not to make a speech; it is rather to welcome you and to wish you well on this 25th Anniversary Symposium.

D. James Baker, Jr., Symposium Chairman and President, Joint Oceanographic Institutions, Inc.

It's a very great honor for me to welcome this distinguished group of speakers who have graciously taken time from their schedules to come to this Symposium. I'm speaking today on behalf of Dr. Walter Munk, who is Chairman of the Academy's Ocean Studies Board, and Dr. Arthur Maxwell, Chairman of the Board of Governors of the Joint Oceanographic Institutions, both of whom were original co-conspirators present at the famous breakfast at Walter Munk's where the original idea of deep ocean drilling was conceived. We'll hear more about that later.

Dr. Maxwell worked closely with Dan Hunt of the Joint Oceanography Institution, Mary Hope Katsouros of the Ocean Studies Board, and Arch McLarren of Texas A&M to put together this Symposium, and I want to extend my gratitude to all of them for their hard efforts and to all of you for attending today.

We in the scientific community are grateful to the NAS for their role, not only in supporting the initial program of ocean drilling but also for their continuing role in providing scientific advice, counsel, and review to the program as it has evolved over the past 25 years.

We are also grateful to the NSF where many of the key figures there have been involved in ocean drilling at one time or another: Bill Merrell, Sandra Toye, Al Shinn, Peter Wilkniss, Don Heinrichs, Al Sutherland, Grant Gross, have all been involved with the program. And we also owe a debt to the other nations of the scientific world who have been involved in the program: Germany, France, United Kingdom, Japan, Soviet Union, Canada, and now 12 countries represented by the European Science Foundation.

Finally, we want to thank our speakers who represent each of the major milestones in the program. We are very pleased that we were able to get such distinguished representatives of the different parts of the activities. I know you are eager to hear from the speakers, but let me just say a word about the organization of the Symposium.

We have organized around four major activities of ocean drilling, two of which were successful in their own right, and two of which were successful in that they laid the foundation for the other two. We will start with Project Mohole and then proceed to the DSDP, which was run through the Scripps Institution of Oceanography. Then we'll talk about the Ocean Margin Drilling Program and then summarize the current Ocean Drilling Program, which is operated through Texas A&M University. We will finish up with a discussion of benefits to industry.

I'll introduce each speaker in turn. It gives me particular pleasure to introduce Bill Bascom, who was the Director of the Mohole Project from 1959 to 1962. Bill's book, _The Hole In The Bottom of the Sea_, of which there is a copy here in the library, describes early ocean drilling in detail, but was used by a generation of oceanographers, including myself, as an introduction to the entire field of oceanography. That gives you an idea of the range of Bill's abilities and expertise. Bill will talk about Project Mohole.

PROJECT MOHOLE

Speaker: Willard Bascom

I am delighted to help celebrate 25 years of deep ocean drilling at this place where I spent eight years of my working life. I will show a motion picture of the drilling and talk about the project history, quoting from papers and reports written at the time so that my memory is not at risk, and try to illuminate those remote days when there was much uncertainty about the nature of the crust and the deep sediments beneath the ocean — and about whether it was possible to drill in deep water.

First, a word of thanks to the National Academy of Sciences, past and present. The Mohole Project was unlike any other Academy undertaking. It was direct involvement in the natural world — in a risky project — rather than merely discussion by a committee about what could be done under the aegis of the Academy. It was undertaken because of the specific personalities who were present here in 1958 to 1962: Detlev Bronk, president, and his staff: Douglas Cornell, the Chief Executive Officer; John Coleman, who first recruited me as a part-time staff member in 1949; Howard Lewis, who dealt with publications and public relations; Bill Thurston, Executive Secretary of the Earth Sciences Division; and myself, a mining engineer turned geophysicist.

Our concerted efforts had the full support of the Governing Board, partly because five members of the original AMSOC Committee were Academy members and because, as Dr. I. Rabi, a Nobel physicist on the Board, concisely put it, "Thank God, we're finally talking about something besides space."

Thanks also to Frank Press and Bob White and Phil Smith for sponsoring this 25th celebration of deep ocean drilling so we can consider the great advances in engineering and understanding that have been made. It is too bad that Roger Revelle and Walter Munk are travelling abroad and cannot be present here today. Thanks also to Mary Hope Katsouros for organizing this meeting, which originated when Peter Johnson suggested it to President Press.

Four of the original group of scientists who supported the beginnings of deep water drilling with word and deed have gone on the Great Adventure to that land where one supposes the correct answers to all the earth's mysteries are already on file. Let us particularly remember Bill Rubey and Harry Ladd of the USGS, who were responsible for drilling test holes on Eniwetok and Bikini; Maurice Ewing of the Lamont Geophysical Observatory, who longed to drill from his old wooden schooner Vema; and Harry Hess of Princeton University, co-sponsor of the project. Surely their spirits are here with us today.

The idea of drilling to obtain samples of the deep rocks or of obtaining deep cores of undersea sediments was thought of by many people, including Charles Darwin, who in 1888 persuaded the Royal Society of

London to drill a hole on Funafuti in the Ellis Islands. Also by T. A. Jaggar, director of the Volcano Laboratory on Hawaii, who in 1943 proposed drilling 1000 core holes in the deep ocean bottom each "1000 feet deep so geology would not remain a speculative science." And others.

But the suggestion that sparked the actual drilling stemmed from a meeting at the National Science Foundation early in 1957 of a proposal review panel that included Walter Munk and Harry Hess. They had considered some 65 research proposals, each made by a scientist of some stature in geology-geophysics, and decided that none of these was likely to produce any major advance in earth sciences. Walter Munk suggested that they should consider what project, regardless of cost, would do the most to open up new avenues of thought and research. He thought that sampling the earth's mantle would be most significant.

Not long after that there was a breakfast meeting of the American Miscellaneous Society, a specifically disorganized organization, at Walter's house in La Jolla, where the subject was further discussed. The circle was then expanded to include Gordon Lill, Josh Tracey, Harry Ladd — and soon after that, Roger Revelle, Bill Rubey, and Maurice Ewing.

In September 1957, urged by Roger Revelle and Tom Gaskell, the International Union of Geodesy and Geophysics, meeting in Toronto, adopted a resolution that said, "The composition of the Earth's mantle below the Moho is one of the most important unsolved problems of geophysics." It "urged the nations of the world — to drill to the Moho discontinuity."

In April 1958, I occupied an office on the floor above this lecture hall, opposite to that of Bill Thurston. One day in a flash of genius I called across, "Bill, it's the Mohole!" So the drilling project became; no one afterwards called it anything else. Thurston and Harry Hess, who was then Chairman of the Earth Sciences Division, called a meeting of geophysicists in the great hall of the Academy immediately precedent to a meeting of the American Geophysical Union. It was chaired by Harry Hess and the first speaker was Gordon Lill.

Let me read to you from earlier writings about what happened at that meeting:

> As soon as Lill proposed the drilling program, some unexpected opposition developed that took three forms. The first question was: 'What good will it do to get a single sample of the mantle? The material beneath the crust is probably not homogenous and one sample cannot be expected to be representative. Ten or even 100 holes may be needed before we will know what the mantle is made of.' To which Harry Hess answered, 'Perhaps it is true we won't find out much about the earth's interior from one hole, but if there is not a first hole there cannot be a second, or a tenth or a hundredth hole. We

must make a beginning.' The second objection dealt with money: 'This project will cost many millions of dollars, you can't even estimate how much. If it is paid for out of geophysical research money, it will strip all other projects of funds for years. If that amount of money were divided up amongst the existing institutions, we would all be able to do more and better geophysics.' Roger Revelle answered that one. He said, 'I imagine that an argument like that was used against Columbus when he asked Queen Isabella for funds for his adventurous voyage. One of the Queen's advisors undoubtedly stepped forward and said your Majesty, it won't be important even if this crazy Italian does reach India by sailing West. Why not put the same amount of money into new sails and better rigging for the other ships; then the whole fleet will sail half a knot faster.' That devastating analogy silenced that part of the opposition. A third objection was: 'It is impossible to drill a hole in the bottom of the ocean for the foreseeable future. Nobody has any idea how it can be done. Why doesn't AMSOC forget about oceanic drilling until it's done some research on deep drilling techniques on land.' The answer to this was given by A. J. Field, an engineer from Union Oil Company, who showed movies of the barge CUSS 1 [Figure 1], a name compounded from the initials of the oil companies that owned it, Continental, Union, Shell, and Superior. It was drilling an oil well at sea off the California coast in water about 200 feet deep with a full-sized oil drilling rig. Admittedly, this ship was a long way from being capable of drilling to the Moho but it demonstrated the new possibilities to everyone present. Almost until that moment the capabilities of floating drilling platforms had been kept closely guarded commercial secrets. Virtually no one present had seen nor heard of such equipment before, but now they could see a new sort of tool which looked as though it could be developed into a deep sea drilling rig. A wave of enthusiasm went through the audience and they saw the project in a new light. 'If American technology can go this far it can drill to the Moho.' In fact, CUSS 1 itself looked as though it might be used to drill shallow holes and sample the upper parts of the sedimentary crust on the seafloor. By this time, even those who had been on the fence were persuaded that the Deep Sea Drilling Project was a better idea than it first seemed. I proposed a resolution that said, 'The AGU supports the project outlined by Gordon Lill.' It carried unanimously.

Under the aegis of the Academy and with the support of geophysicists generally, it was not long before a grant of $15,000 was received from the National Science Foundation as a down payment on a feasibility study. Thus it was that the idea, the scientists, the Academy, and the money all came together.

The feasibility study began at once with me as its unpaid executive secretary. We looked into various possible test sites in both the Atlantic and Pacific; we examined nearly all the floating drilling vessels in the world. Behind the scenes the work was proceeding nicely, but by October the committee was becoming disturbed about the misinformation reaching the public. Rumors were flying that made deep sea drilling sound more like science fiction than science.

It was decided to scotch these wild rumors with a complete public statement for, by then, possibilities had narrowed and our thoughts on how the work should proceed were more orderly. At least the committee could say with reasonable certainty what the scientific objectives were, which drilling sites seemed the most promising, and what kind of equipment might be used. The result was an article in the Scientific American (April 1959) written by me and entitled "The Mohole," which summarized our thinking at that time.

In September 1959, an international symposium on oceanography was held at the United Nations in New York. Some of you may have been there. At that time we released what became known as the red book: Drilling Through The Earth's Crust (NAS 717). I want to read a paragraph from that book, mainly because it was often said afterwards that the objective of our project was "one deep hole" and nothing else.

> The AMSOC committee believes "That it is desirable to drill a series of holes into the strata beneath the ocean, culminating in one that pierces the Moho and samples the mantle. The information produced will be unique, since direct sampling and measurement is the only way to obtain much needed knowledge about the composition of the earth, its ancient history and the development of life. In addition to the immense scientific value, the oil industry will benefit from the experimentation with new ideas and equipment for deep drilling. And the value of information and experience to be gained will far outweigh the cost of obtaining it."

Elsewhere in that same publication, we noted that no one knew what the composition was of the second layer where seismic waves moved at 4.5 to 5.5 km a second, or the third layer where they ran 6.7 to 7.0 km a second, or the sub-Moho material where velocities were 8.3 km a second. Nobody knew whether these were basalt or consolidated sediments or limestone or what. Perhaps we still do not know; maybe someone will tell us later on today.

The committee also hoped that we would find fossils that would fill in breaks in the land record, and it wanted to confirm remote geophysical measurements by directly measuring rock conductivity, radioactivity, temperature and so forth. The red book also noted the chance of some great, unexpected discovery. It also said "it is possible that the sudden revelation of the geologic history of the uppermost layers by the cores taken in the test drilling will generate so much scientific excitement that there will be a general clamor for a program of ocean drilling in many places." Very proper.

The story was picked up by the press and published widely. Before long, letters from crackpots began to arrive. Some went to the Secretary of the Navy saying: "Don't let those crazies drill a hole in the bottom of the ocean. They'll drain all the water out of it and we won't need a navy." Others were from people who said, "I've seen the middle of the earth in my dreams and I can tell you what's there."

It is one thing for scientists to speculate on the nature of Venus or Mars and to say what they will do with rock samples of these planets. It is quite another to actually go there and bring the samples back. That requires careful engineering and considerable operational risks. So it was with the first deep ocean drilling.

The things that are known and taken for granted today were uncharted territory when we first started thinking about deep sea drilling. It was not known then if a ship could be held close above a hole in the bottom in water over two miles deep; if drill pipe, not laterally supported by the side of a hole, would whip wildly when rotated and bend excessively; if sea water would be a proper circulating fluid in the drill hole, how a diamond bit (needed for hard rock) would work for coring in soft sediments; if wireline coring techniques would return satisfactory cores; if a steady weight could be maintained on the bit as the ship heaved up and down; the extent to which longitudinal waves would move up and down the drill pipe, etc.

A number of the men who were with us at that time are in the room here today. We have Ed Horton, Pete Johnson, Francois Lampietti, Bob Snyder, and Jeff Savage. Dr. Jack McLelland, Chad O'Hanion, and John Marriner couldn't make it, I'm sorry to say. Two of our naval architect advisers, Captain Harold Saunders, USN ret., of David Taylor Model Basin, and Robert Taggart have also gone on to the Great Adventure.

I should like to speak now about three major engineering problems the AMSOC staff solved with a series of inventions that laid the foundations of all future deep ocean drilling.

First, Dynamic Positioning. In early 1958 there was a tug-boat strike in New York Harbor and ship operators soon found how difficult and risky it was to dock large ships without tugs. In my capacity as executive secretary of the Maritime Research Committee, I investigated and wrote a staff paper with my assistant, John Gregory, called "Maneuvering Devices for Ships" that could be used for docking if tugs were not available. (These included bow thrusters, vertical axis propellers, active rudders and omnidirectional steering screws.)

Before that, as a project engineer measuring waves from the first large thermonuclear test at Eniwetok, I had invented the taut-moored buoy that held position closely in deep water.

Now, as soon as our early calculations showed that if a drilling ship were anchored in deep water it would move about so much that the drill string would be likely to break, it occurred to me that if a ship were equipped with maneuvering propellers and if the pilot had a fixed reference point such as a piling or a buoy to guide him, he could maneuver indefinitely to hold the ship within a small area — I had in mind the space between piers at 51st Street in New York.

Such was the invention of "dynamic positioning". I first used that phrase, and the word "epicenter" to describe the ship's position, in a paper I wrote in the summer of 1959 and submitted to Science magazine. By the time Science rejected the piece as predominantly engineering I was too busy writing A Hole in the Bottom of the Sea to submit it elsewhere.

Reducing that invention to practice in a drilling operation required a team of people and the work was assigned as follows: Peter Johnson, assisted by Jeff Savage, was given primary responsibility for designing, installing and maintaining a circle of buoys in deep water around the drilling sites, Chad O'Hanian was responsible for sensing the buoys in rough seas at night with radar and sonar, Captain Southerland of "Murray" and "Tregurtha" oversaw the construction of four large Harbormaster outboard motors built to our specifications (Figure 2), and Robert Taggart was given a contract to build a console that would simultaneously control the four propellers. I specified that he use a "joystick" type control that had directional sense and a wheel to change direction (Figure 3).

All of these people did an excellent job but we had enough misgivings about the position sensing system that Bob Snyder devised a backup tiltmeter as an alternate system. It was a sort of analog of the slope of the pipe at the bottom. Luckily, it was not needed.

Second, the "guide shoe". Early calculations showed that excessive tension due to pipe bending would likely come at the surface where the pipe left the rotary table, a point where the tension from the weight of the pipe below was already great. This bending would be caused by the rolling of the ship and because it would not be precisely above the hole. We took the precautions of specifying a round fluted kelly and extra high strength tapered drill pipe, but the main invention was that of Ed Horton, who devised a trumpet-shaped "guide shoe" through which the pipe passed as it left the ship (Figure 4). This physically prevented the pipe from bending more than an acceptable amount and depth stresses in it within our specifications.

Third, we anticipated the eventual need for hole reentry from the very beginning and we approached that problem by working on a more immediate one, that of reducing bending stresses in the pipe where it entered the hole. It would prevent the pipe from kinking but in a different way from the guide shoe at the surface. Whatever was used would have to be held in place by a structural steel "landing base" sitting on the bottom, attached to a casing that extended well down into the hole. Eventually, this design, for which Ed Horton and Jack McLelland were mainly responsible, became an increasingly flexible tapered casing that projected above the landing base with a small funnel at the top. On March 12, 1961, at our La Jolla drilling site, we installed a landing base with 150 feet of casing below it and the tapered casing rising above it. This was drilled into the bottom in 3,111 feet of water, the J-slots were disengaged to free the drill pipe, and the hole deepened another 1035 feet. It was the first step in hole reentry.

Francois Lampietti, the resident mathematician of our group, endlessly calculated the stresses in various combinations of drill pipe and worried about vibrations, longitudinal waves in drill pipe, and such esoteric matters. He was, of course, helped by the others, especially including, Ed Horton, Arthur Lubinski, and Jan Leendertse in making what was probably the first major study of long strings of pipe in the ocean.

When drilling time approached, we had put so much emphasis on the problems of preventing the drill pipe from breaking that many people asked, what will you do if the pipe breaks? To prevent undue concern on that point we rented a second string of pipe, magnafluxed it, and stored it in San Diego. Then the answer to the question was: "If we drop the first pipe we'll go back and get the alternate string." It was never needed.

I should like to say something about the attitude of the oil industry at this time. I gave many lectures on engineering as well as scientific matters in Houston at the Petroleum Club and the Rice Hotel to very skeptical audiences. After one such lecture was over a couple of old guys out of the oil patch came up to me to say, "Son, you ever see a real drilling rig without an outboard motor?" "Yes sir." They shook their heads like they were talking to the village idiot and walked away.

After we drilled successfully, as planned, I lectured in both those places again. The same kind of people who previously thought we were crazy to try now said, "That was nothin'. Anybody in the oil business could have done it." Even our contractor, Global Marine, had difficulties believing dynamic positioning would work and three or four months after we had made the decision to go with it, Global was still trying to raise several million dollars from the Office of Naval Research to build an anchoring system. So we received very little support from those who afterwards copied our ideas and inventions freely.

Let me show you a motion picture and then I'll say a few words beyond that.

"The First Deep Ocean"
Produced by Willard Bascom
for Ocean Science and Engineering Inc.
in 1963
(winner of Industrial Film Institute first prize)
showed the actual drilling at sea.

The total cost of all the purchases, the work at sea, mobilization and demobilization, subcontracts, staff and committee work — everything — was $1,536,500.

The first drilling was deemed highly successful by everyone including the President's Science Advisory Committee, the National Science Board, and the Academy's Governing Board.

The following letter, addressed to Dr. Bronk and Dr. Waterman was received from President Kennedy:

> I have been following with deep interest the experimental drilling in connection with the first phase of Project Mohole. The success of the drilling in almost 12,000 feet of water near Guadaloupe Island and the penetration of the oceanic crust down to the volcanic formations constitute a remarkable achievement and an historic landmark in our scientific and engineering progress.
>
> The people of the United States can take pride not only in the accomplishment but in the fact that they have supported this basic scientific exploration.
>
> I extend to you my congratulations and ask that you pass them on to the special committee and the staff of the National Academy of Sciences, the National Science Foundation, the Global Marine Exploration Company, and especially to all these on board the CUSS I and attendant vessels who have combined their talents and energies to achieve this major success.
>
> s/s John F. Kennedy
> President of the
> United States

The engineering staff began at once to design a much improved ship to carry out the widespread drilling program that was originally planned. We specifically called it an "Intermediate Ship" to make sure no one would think it could be used to reach the Moho. It would have been a very satisfactory and inexpensive ship; indeed, several oil drilling companies used our design. Had it been built and used as we proposed, the second phase of the Mohole project would have come out as well as the first.

Unfortunately, we had made the Guadaloupe Island drilling look too easy. As Harry Hess said, "You should have failed a few times and made it look harder." The staff was unable to communicate the engineering and technical difficulties of drilling to the mantle to some of those on the AMSOC Committee. The result was the Committee split down the middle over whether to recommend an "Intermediate ship" or to attempt to build one that could drill to the ultimate objective on the first try without solving the really difficult engineering problems that have not been solved to this day.

At the same time it was quite clear to everyone that the NAS was not the proper base for the large engineering and operational drilling program that would ensue. Our staff heartily agreed that an outside contractor should be selected — indeed, we briefed some 15 bidders about what was needed.

The bids were very carefully reviewed by a committee selected by NSF and the top three bidders were deemed to be outstanding. All of them would have given our staff important positions in the project. But, after a long delay the fifth-rated contractor, Brown and Root of Houston, who had sufficiently good political connections that it had not even bothered to attend the bidder briefings, was mysteriously selected.

About that time, our staff resigned to set up Ocean Science and Engineering Inc. and our attention was transferred from scientific drilling to undersea diamond mining.

Later on, congressional hearings led by Senators Kuchel and Allot remarked on the sustained ineptitude of Brown and Root and noted that great pressure had been put on NSF by V.P. Lyndon Johnson and Rep. Albert Thomas of Houston. The news media had a field day with that and after some $55 million had been spent and nothing had been accomplished, the project was cancelled. Thus Mohole became Nohole.

It is sad that the first deep ocean drilling which began with such promise came to such a sad ending and, in the end, gave the Mohole Project a bad name. Once it left the Academy here it became very political and expensive. But once we had shown that deep ocean drilling was possible it was inevitable that more deep drilling would take place.

I look forward to hearing from the other speakers about what happened next.

D. James Baker, Jr.

It is terrific seeing the start of the ocean drilling activity which, as you say, cost $1,500,000. Today, the budget request for 1987 for the Ocean Drilling Program is $45,000,000. We won't get all of that but probably get most of it. Of that $45,000,000, $15,000,000 per year is being put in by foreign countries, to give you an idea of the size of the project today. By the time we finish this ten-year program, we will have spent close to $1,000,000,000 on deep ocean drilling, but it's worth it, believe me.

Philip Smith, Executive Officer
National Research Council

Upon review of a project like this, you suddenly discover that a lot of ideas that people think are new and just being introduced into the program, in fact, were thought of 25 years ago and I just wondered if this film and Bill's discussion shouldn't be made a part of the indoctrination for every new person who comes into Ocean Drilling. You'd be surprised how many people came up to me just during the coffee break and said, "I didn't know they thought about that 25 years ago."

We're going to move on now to the successor to Project Mohole, the project that did, in fact, what Bill was talking about — do a global reconnaissance of holes in sediments all over the world and came up with some major new scientific results. We will have two speakers, the first of whom will bridge the gap between Phase 2 of Mohole and the DSDP — Arch McLerran, who had served not only these two projects but also was a key figure in helping organize and getting started the new Ocean Drilling Program which commenced in 1983 at Texas A&M. Then Arch will turn it over to Mel Peterson.

DEEP SEA DRILLING PROJECT

Speaker: A.R. McLerran

It is indeed a privilege to be here today. In fact, it has been a privilege for me to have been a part of the Ocean Drilling Programs since 1962 when I was an independent consultant and did some design work on pipe handling equipment for Brown & Root, prime contractor on Phase II of Project Mohole. Apparently as a result of that work, I was invited to join the staff of the National Science Foundation (NSF) project office in Houston, Texas, which was under the direction of Don Woodward, who played a prominent role in Phase I of the Project Mohole. I joined the project there in April 1964. It has been a very interesting and rewarding part of my career.

I had been interested in offshore for a number of years and my prior experience had been with an equipment manufacturer who was designing and building equipment for the offshore drilling industry which was relatively in its infancy at that time.

Phase II of Project Mohole was the engineering, construction, and operation phase and Brown & Root, the prime contractor at that time, understood it to be their mandate to drill the Mohole. Everything they were doing was aimed at the ultimate goal of drilling a hole in 13,000 to 15,000 feet of water with a penetration up to 20,000 feet below the seafloor. They estimated a total operational period of approximately three years to drill the selected site because most of the drilling would be in very dense and hard basalt. Phase I of Mohole had proven that basalt appeared to be the predominant material on the second layer of the earth's crust. There is no question but that Phase I of Mohole had demonstrated for the first time the feasibility of drilling in very deep water. It was a very exciting undertaking and a number of new and innovative ideas were used for this demonstration. It must be remembered that the actual drilling lasted only a few days and penetrated less than 1,000 feet into the seafloor, whereas the drilling of the Mohole would take many months of continuous drilling while staying over the same site in the open ocean.

In looking at Project Mohole with the benefit of hindsight, I have a different outlook on the magnitude and difficulty of the task from that proposed by the AMSOC staff at the end of Phase I of Project Mohole. In testifying before a Congressional Committee concerning the increasing cost of Phase II of Mohole, Dr. Leland Haworth, Director of the NSF at that time, made one of the understatements of the century when referring to Phase I of Mohole; he said "Several major engineering problems were not uncovered by this undertaking". That was because Phase II turned out to be a major research and development project.

The contract was awarded to Brown & Root of Houston, Texas in 1962 to drill the Mohole, with the work to be done in five stages. Stage A was Engineering and Design and the first year was largely spent trying to select the platform to do the work. At that time, the deepest hole that had ever been drilled on land was about 27,000 feet. They were faced with the problem of trying to design a system to drill 35,000 feet from a bobbing platform out in the ocean that could maintain a station over the drill site and survive a 100-year hurricane. At that time, most of the offshore drilling was being done from fixed platforms or bottom-sitting barges; floating drilling was really just beginning to come into its own and there wasn't too much known about it. At that time, scientists believed that the Moho or mantle could only be reached by drilling in the deep ocean basins.

Brown & Root had come to the conclusion that a ship-type hull would not give the stability that was required and a more stable platform must be developed. Their studies indicated that due to extreme loads at the upper part of the drill string, even with the guide shoe that had been developed in Phase I, when drilling in a hostile environment with sea water as the principal circulating medium, fatigue damage would be a very serious problem and drill string life would be unacceptable from a ship-type platform. This led to the development of the concept of the column-stabilized self-propelled dynamically positioned platform finally proposed. The semi-submersible platform concept was just coming into being and had been used, I believe, probably by one or two contractors at that time, but only in an anchored mode in a few hundred feet of water.

So platform development became a major undertaking because they were advancing the state-of-the-art by a considerable amount at that time. After extensive model testing at the David Taylor Model Basin, Brown & Root contracted with Gibbs and Cox of New York, who were one of the leading naval architects in the United States, to develop the detailed design of this platform. The design was completed, the construction contract was awarded, and the platform was under construction in San Diego, California at the time of termination of the project.

A few years later in 1974, I visited Blohm and Voss Shipyards in Hamburg, Germany and there to my amazement was the Mohole Platform, or so it appeared to be. It turned out it was the semi-submersible, with the name Chris Chenery, being built for The Offshore Company and, dimensionally, it was almost exactly the same as the Mohole Platform. In fact, they had obtained copies of the Mohole design drawings from the National Technical Information Service and were using the basic design for their platform. As far as I know, it has operated successfully in the severe environment of the North Sea, proving the validity of the original design. Semi-submersibles of many configurations are widely used for offshore drilling today.

The development of a number of new elements were necessary to have a complete drilling system. A means of holding the platform at the drill site without anchors was a critical requirement for the success of the program and such a system did not exist. Honeywell and General Motors Defense Research Laboratories were awarded contracts for the development of a Dynamic Positioning System (DPS). Each company was successful and subsequently manufactured DPSs for sale to the offshore drilling industry and other marine applications.

Honeywell developed both a long and a short baseline system using the phase shift of the incoming signal from a seafloor beacon to determine platform displacement, and General Motors developed a pulse position measurement system which measured the difference in the time of arrival of the signal at the hydrophones. The DP systems were under construction at the time of termination of the Project Mohole. They were completed after termination because we know that the Deep Sea Drilling Project was coming down the pike and would need a similar system. The completed system was offered in the Request for Proposal for the DSDP drilling ship as Government Furnished Equipment (GFE) that would be available and it was subsequently used in part. One big difference in the system used for DSDP was that, at the time that the system was originally built, analog computers were the standard and the system was built with an analog computer to control the ship. It took a large room just to hold the computer involved, in addition to all the position reference equipment. Fortunately, when DSDP came along, the digital computer was beginning to come into use and was substituted for the analog computer when the DPS was installed on the Glomar Challenger.

We went to sea, originally, with the two reference systems aboard, the pulse measurement and the phase measurement system, and the digital computer that was furnished by Global Marine, the ship owner. Fortunately, after a period of time, which I will mention later, both systems performed satisfactorily except that the phase comparison system at that time had three mechanical phase comparators and it was very difficult to keep these units synchronized. We finally eliminated the phase comparison system, added another frequency to the pulse measurement system and that system operated for almost 17 years with minimal down-time due to equipment failures. So the first attempt at DPS was a highly reliable system and DPS's are widely used in marine operations including drill ships, semi-submersible platforms, and other floating platforms.

As stated, it appeared that drill pipe was the critical element because in drilling a hole in which you had three years of time and millions of dollars invested, a drill pipe failure would be catastrophic. Therefore, you must design a zero failure rate in the drill string. That was approached by trying to get better materials, better manufacturing processes, and better inspection methods. A great deal of research was done on improving the physical properties of drill pipe by improved chemistry and quality control in the manufacturing process. The specifications of the pipe developed are similar to S-135 Grade used by industry today. Some of the research resulted in the development of an internal drill pipe inspection system that allowed inspection of 90-foot lengths of pipe while stored in the horizontal pipe racks. This pipe inspection equipment was later transferred to DSDP and was successfully used for 17 years.

It was recognized that for hole-cleaning purposes and for hole stability on the long duration site occupancy, a rise system was mandatory. A large amount of basic research in riser design was required. This basic research in riser design was undertaken and a design for a free-standing riser was developed with a subsurface buoy to maintain the necessary tension in the riser when disconnected from the platform. Syntactic foam was proposed as flotation buoyancy material. This is the same material being commonly used by the offshore drilling industry today. At that time, the production of syntactic foam was a rather imprecise operation and there were no uniform quality standards. Project Mohole was instrumental in establishing the first ASTM standards for syntactic foam. Also, the mathematics and analytical processes for risers were developed at that time and are the basis for many of the deep water riser designs used by industry today. Dr. Arthur Lubinski, who did work on Phase I of Mohole, was a consultant to Brown & Root for Phase II of the project and had a great deal to do with the design of the drill string and the drilling riser.

The Deep Water Reentry System was developed and a prototype scanning sonar reentry system was built and tested under Phase II of Project Mohole and was the basis of the system in the later years of DSDP (Figure 5).

There were many other ancillary systems that were used. The point I am making is that some people thought that Phase II of Mohole may have been a waste of money. I do not feel that is the case because there was a lot of valuable research that was done that is currently being used by industry. I think that NSF did not take proper credit for sponsoring this research. In fact, after termination of Mohole and at the beginning of the DSDP, NSF seemed to be embarrassed by the mention of the word Mohole and they went to great lengths to avoid connecting the DSDP with Mohole in any way. They issued some rather lengthy statements tending to disassociate the two programs. Therefore, they did not take proper credit for the good things that came from Project Mohole. I personally feel that at the time the project was terminated, Brown and Root had developed the technical capabilities necessary to drill the Mohole. A site had been selected off Maui and much of the hardware was under construction when the project was terminated.

I will remember August 26, 1966, very well. I was on vacation with my family and was called back to the Houston office on the day before my birthday to be on hand to deliver the termination telegram to Brown and Root. So it was not a very happy birthday that year, with the prospects of soon being unemployed.

A number of items were completed after the notice of termination, such as the DPS, the sonar reentry system and some other systems that we felt would be applicable to the DSDP.

One of the contributing factors to the termination of Project Mohole was a dispute within the earth scientists providing the planning of the ocean drilling, over the priority for deep crustal drilling and shallower sediment drilling. The original sponsors of the drilling wanted to get on with the drilling of the Mohole and to obtain samples of the deep crustal rocks, whereas those primarily interested in sedimentary drilling wanted sedimentary drilling to precede the drilling of the Mohole. To calm the dispute, which was threatening all scientific drilling, NSF agreed to fund a separate Sediment Drilling Program (SDP), provided proper proposals were received.

There were a number of abortive attempts to get institutions together to propose drilling programs to NSF but, from what I have read from the files, everybody was willing to join such a program as long as they could be top dog, or top gun, as they may say these days. Finally, Scripps Institution of Oceanography, University of California, San Diego; Lamont-Doherty Geological Observatory, Columbia University; Rosenstiel School of Marine and Atmospheric Sciences, University of Miami; and Woods Hole Oceanographic Institution, formed the Joint Oceanographic Institutions for Deep Earth Sampling (JOIDES). They presented proposals to NSF, which resulted in the award of a contract to Scripps Institution of Oceanography in 1966, to do what NSF referred to as "An ocean sediment coring program"; but at Scripps, and generally among the scientific community and the public, it became known as the DSDP. Undoubtedly, it was the beginning of one of the most successful scientific programs that has been on the scene in this century.

But back to Mohole; now that I have retired, I must be qualified as an expert and can express an opinion on causes of Mohole failure. I think some of the major reasons that Mohole was not successful are (1) the major initial underestimation of the complexity of the task, which resulted in increased cost; (2) a breakdown in the scientific leadership at that time with a disagreement between the hardrock and sedimentary scientist; and (3) the lack of support from the oil industry. Regarding the latter, I am not sure where the fault lies, whether it was the contractor's (Brown and Root) failure to solicit industry's input and support or whether the industry was trying to protect their "trade secrets". I recall that in the mid-1960s, before Mohole termination, Shell Oil Company, which was one of the leaders in deep water drilling technology, had a school or symposium that lasted only a few days, in which they described their latest technology and state-of-the-art in deep water drilling. I am not sure what the enrollment fee was, but I do recall that it was more than $100,000.

And now to DSDP. In 1966, the DSDP was in the process of starting their mobilization and I was asked by NSF if I was interested in joining that project as the NSF on-site representative. I agreed to do so, as I felt it was an opportunity to continue in a very fascinating program that would advance the state-of-the-art in offshore drilling. Dr. John Wilson, Deputy Director of NSF at that time, asked me "Where do you think your office should be located?" That was a hard decision, believe me, it was. I chose La Jolla. There was some uncertainty about the chances of success of the program at that time. Could we consistently drill and recover cores in the deep ocean? The original contract was limited to 18 months of drilling.

We made a survey of some of the drilling industry who were using wireline coring on a regular basis — largely sulphur mining companies operating offshore and, to a lesser extent, oil drilling companies -- and found that the results they were getting from wireline coring in sediments were not very satisfactory. Predictions of low recovery were made by some critics of the program. We had to prove we could get consistent reasonable core recovery if the program was to continue beyond 18 months.

Beginning in 1967, Scripps started putting their staff together and issued a rather comprehensive Request For Proposal (RFP) for a drill ship. The decision had been made to use a contractor-owned and contractor-operated ship instead of using a government-owned ship, which I think was a wise decision. The RFP required the bidders to submit bids for furnishing and operating a ship on a fixed day rate basis.

The RFP reflected the great amount of work that had gone into the planning of the program. The areas of operations and ports of call for the first 18 months of drilling were designated. The day rate for the ship was to include everything except the fuel and the drilling and coring consumables. The contractor had to furnish his own ships supplies and transport his people and supplies to and from the various ports at his expense. Scripps would pay for their own supplies and cost of transporting their supplies and personnel. That was the basis on which the final contract was awarded. Also, a detailed evaluation procedure to be used in selecting the contractor had been worked out in advance of receipt of the proposals. That was done to avoid embarrassment and controversy that did surround the Mohole program.

In June 1967, five bids were received for furnishing the ship for DSDP. Global Marine, Ocean Science and Engineering, and SEDCO submitted bids considered to be responsive to the RFP; Western Offshore and Seamole submitted bids which were considered to be non-responsive. By the way, Seamole was owned by a dentist from Corpus Christi, Texas and I think his experience had been limited to oral drilling rather than deep sea drilling.

There was an extensive evaluation of the bids in September 1967. There were three panels of experts involved: drilling experts who were largely from industry; marine experts who represented some of the leading naval architects and shipyards in the country; and a science panel of prominent earth scientists which were to review the proposal to determine if the bids would meet the scientific requirements of the program.

As a recollection of the time, I recall that Dr. William Rand, who had owned a company that operated a small core drilling ship off the California coast, had been hired as Project Manager for Scripps and was manager during the period when the RFP was prepared and the bids were evaluated. Dr. Rand had very strong feeling that the drill ship should be equipped with a folding mast or derrick similar to those used for land drilling and on some small coring ships. Others of us felt very strongly that a fixed derrick should be used. We were concerned about the dynamic loads in raising and lowering a large mast of the size required by the program, at sea during bad weather. Dr. Rand felt so strongly about this that he stated that if the ship selected did not have a folding mast, he would resign. The responsive bidders all offered a bid with a folding mast as requested but also offered an alternate bid with a fixed derrick. In all cases, the cost with a fixed derrick was substantially lower. The drilling panel felt very strongly that a fixed derrick should be used; in fact, the minutes of those panel meetings reflect the opinion of the experienced offshore operators that a fixed derrick should be used and the only way they would allow a folding mast on the ship would be to weld it in the upright position after it had been raised the first time. Dr. Rand resigned shortly thereafter and was succeeded by Ken Brunot, who was Project Manager until 1971.

It was quite an interesting competition and probably all three of the ships could have done the job, but on the basis of their higher numerical rating by the evaluation panel, Global Marine was selected.

Global Marine proposed to furnish a new ship that was under construction at that time. The keel had been laid in the summer of 1967 and plans had already been prepared so the ship could be easily converted to a deep sea drilling ship in case they were awarded the contract. Plans for the DPS, including the addition of tunnel thrusters and the scientific laboratories were quickly completed and added to the construction contract.

The ship was launched in the Spring of 1968 and named Glomar Challenger, after the famous British ocean research ship HMS Challenger. There are a number of people at Global Marine that deserve recognition for their contributions to the success of the project. Russ Thornburg, who had been involved in Phase I of Mohole; Curtis Crook, John Graham, Bob Bauer, and many others in that organization did a lot of good work and helped get DSDP off to a good start.

There were several items of equipment from the Mohole Project that were supplied as GFE to be installed on the ship, the major items being the DPS and the large logging winches that had been designed to handle 40,000 feet of logging line. Several other items of equipment were furnished by Global Marine, using designs developed for the Mohole.

There was an unusual provision in the contract that had been awarded to Scripps in 1966. I believe it was probably the result of some of the Mohole problems. The contract specified that this drilling and coring should be conducted using existing technology; in other words, that was interpreted to forbid research and development in the program. Well, maybe we fudged a little on a few things, but it was a minor part of the effort to get the project started.

The Glomar Challenger sailed from Orange, Texas on July 20, 1968, for sea trials and acceptance test. Everybody was very confident that everything was going to go out and work correctly the first time. We had a requirement in our contract that the ship must maintain station using the DPS for five days using the automatic mode, before the ship would be accepted. Everybody knew it was going to work, so it was just a matter of putting everybody aboard, going out for five days of trials and then starting work under the contract. I was aboard along with Dr. William Neirenburg, Director of Scripps; Dr. Melvin Peterson, Project Chief Scientist of Scripps; and a lot of other super cargo, including Global Marine officials. We were accompanied by the RV Eureka, which was donated by Shell Oil Co., and she was to take the supercargo off at the end of five days when the sea trials were completed. Well, 23 days later, on August 11, we finally accepted the ship. In the meantime, the Eureka and several other ships had accumulated many miles by just shuttling people back and forth between shore and the Glomar Challenger.

The major problem turned out to be an error in the DPS computer program. In those days, programming was sort of "black art" and nobody was supposed to understand the other fellows' program; it was very proprietary, and they guarded it very carefully. After the other ships' systems had been tested with no major problems, we started the dynamic positioning test and things purred along just like a kitten for about two days, when suddenly at about 2 o'clock in the morning the ship went full speed astern. It shook the whole ship and people began to roll out of their bunks, wondering what was happening. They turned the positioning system off and tried to locate the problem, but were unable to find anything wrong. After several hours the system was started again and seemed to be working fine. About 24 hours later, full speed astern again! After days of shuttling experts from shore, they finally isolated the problem as an error in the computer program. I was told that under a certain eet of conditions, the computer would get a signal to divide by zero, which even a computer could not handle very well. But after we finally got that problem solved, we started on a voyage which I think was successful beyond our greatest expectations.

DSDP Site 1 was spudded in the Gulf of Mexico on August 12, 1968, in 9,275 feet of water and completed on August 16, after penetrating 2,528 feet below the seafloor. However, our success created a problem; the Scripps contract contained a provision that penetration below the seafloor should not exceed 2,000 feet. In the excitement of our success, those of us on the ship did not have time to read the contract and created a bit of a flurry among bureaucrats back on shore. We had our wrist slapped a bit upon our return but, shortly thereafter, that limitation was removed from the contract.

The second site was drilled on top of one of the knolls, or mounds, in the deepest part of the Gulf, known as the Sigsbee Knolls. Dr. Maurice Ewing, who was co-chief scientist on the first cruise, was one of the discoverers of those knolls and was the proponent of drilling on one of these mounds, which he believed to be salt domes. Most people in the oil industry did not agree with his theory, they felt there was no oil in the deep ocean basins. In fact, some petroleum geologists were reported to have said that they would "drink all the oil they could find in the deep basins of the Gulf of Mexico". We drilled into the top of one of these knolls in 11,720 feet of water and after drilling 472 feet, we recovered about eight feet of core saturated with oil and other materials that indicated we were, in fact, on top of a salt dome. In retrospect, I believe that discovery caused a lot of the major oil companies to start research programs for deep water drilling and production, or at least gave their programs more credibility and support, because oil was now known to exist in the deep oceans.

One other amusing incident occurred. On the Glomar Challenger we had installed the first commercial satellite navigation system, Serial No. 1, and we were having some problems with the unit. Everybody on board was claiming to be an expert, but nobody could fix it. So after finishing the third site, we decided to stop just outside the port in Miami and have a factory technician meet the ship to see if the problems could be corrected. We arrived at the rendezvous point at 2 o'clock in the morning and were met by some people from the University of Miami in a small boat. Even though the Glomar Challenger was a "dry ship", they managed to slip a couple of bottles of champagne aboard and we toasted our early success. On their boat they also brought a serviceman/technician to work on the satellite navigation system. He came aboard with his little suitcase and his tools and started to work; but nobody had told him he was to make the repairs at sea. He expected the ship to wait while he made the repairs and when the ship sailed, he protested loudly. It was 12 days before he could get another boat back to shore. He felt he had been kidnapped. He did agree the food was outstanding!

The program proceeded with a great deal of success. One of the early discoveries on the first leg was the fact that some of the reflectors that were seen in the seismic profiles were, in fact, very hard rock called chert. These layers destroyed the bits that we were using at that time. We established shortly thereafter what proved to be a very strong and beneficial liaison with the oil industry on technical problems. These meetings were informal but the companies would have to give us assistance from their technical experts as needed. We convened a panel of drilling experts soon after the first leg to discuss the chert problem and to determine what could be done to allow us to reach our scientific objectives when we encountered these hard layers.

They made two recommendations: the first was to change from the drag type bits that we were using, to roller bearing cone-type bits; and the second was to develop a hole reentry capability. We proceeded to follow both of these recommendations after getting the contract changed so we could do some R&D and both of the efforts were successful.

The roller cone bits gave us great penetration in all types of formations. In June, 1971, we went to sea to test a new sonar reentry system. We lowered a funnel shaped reentry cone to the seafloor on the drill pipe, released from it, and drilled ahead until the bit was worn out. The drill pipe was then retrieved, a new bit installed, and then lowered until it was suspended a few feet above the reentry cone. A scanning sonar tool was lowered through the drill pipe on an electric logging cable until the rotating head extended a few inches below the bit. Once the cone had been located, the ship was maneuvered until the pipe was over the cone and reentry was made.

Those were two of the early developments and there were a number of other technical developments that came about in the course of time. I won't dwell on them anymore as I am sure that Mel Peterson will fill you in on some of those developments and other history of the DSDP in his presentation which follows.

D. James Baker, Jr.

Arch is one of that small group of dedicated people who have spent a large fraction of their lives making sure that a program of ocean drilling really does succeed. He was involved in the Mohole, he was involved very much in the DSDP at Scripps, and he moved over to A&M when the Ocean Drilling Program started in order to help get that organized and he's now retired; but who knows how long that will last.

We have as our next speaker another person who has dedicated his career to the success of ocean drilling. When it became clear that Scripps was going to be a contractor for the program in the period 1966-1968, Mel Petersen moved over from his job as a professor of geology and geophysics at Scripps to take on the position of Project Director for DSDP, a position he held for 15 years, and presided over and directed the program through many of its initial successes. So Mel's going to talk to us today about the DSDP.

DEEP SEA DRILLING PROJECT
Speaker: M.N.A. Peterson

I still remember the time on the acceptance trials when our senior technician, Dave Wirth, decided to try the air gun. We were two-thirds of the way through these contractual trials for the Glomar Challenger and had been hanging on by our teeth, hoping to get that required several days of dynamic positioning stably kept. There was this big explosion off the stern. Curtis Crook from Global Marine, which had designed and built the Glomar Challenger, and I were in the science lounge at the time. I realized immediately what it was, but Curtis had never heard the gun go off before. Dave stuck his head into the science lounge and said, in his finest Swiss accent, "We just tested the air gun". Curtis jumped out of his seat and shouted, "If you guys have knocked us off station, you've bought yourself a ship!" It turns out that Dave really had checked with the bridge and requested permission to test the air gun. But they did not know what an air gun was and that it went off under water. Actually, the low frequency of the air gun offered almost no risk to the dynamic positioning. Global Marine's concern, of course, was that they were receiving no income from the new ship until accepted and on a day rate!

Interestingly enough, and following up on the general concept, "there is very little new in the world", even for some of the Mohole thinking; no basic patents were ever issued on the semi-submersible drilling rig. The reason for that was that back in the early part of the century people were designing a concept for a series of aerodromes across the North Atlantic that were going to have hotels and casinos, etc. Airplanes would hop across the Atlantic from one to the next for rest and fuel, and thereby establish an air transport system. Semi-submersible flotation was patented in these designs. Lindbergh then flew across the Atlantic in one flight and the handwriting was absolutely clear on the wall; that kind of thinking perished and the patents for the semi-submersible platforms passed into public domain and were not brought back again until era of the Mohole and of offshore oil drilling.

I'm going to try to mix reminiscences with a series of summary statements of results of the Deep Sea Drilling Project (DSDP). You can't start talking about the DSDP without talking also about JOIDES; that's basically the start of it, in its intellectual framework and scientific thinking. JOIDES came together following the statement that Leland Hayworth had put into the Congressional Record, saying that he had isolated some money that could carry on a program of ocean sediment coring if the institutions could get together to provide the intellectual background and support. Arch has already mentioned the number of false starts to a drilling program, but JOIDES did come together and, in fact, remains together to this day. It has shown the world how to advise a major program and how to involve an international constituency. The original members of JOIDES were Scripps, Lamont, Miami, and Woods Hole; this configuration operated the brief Blake Plateau drilling program

TABLE 1 Organizational history of the Joint Oceanographic Institutions for Deep Earth Sampling (JOIDES)

Initial JOIDES Group	1964
4 Charter members:	
Scripps Institution of Oceanography	
Lamont-Doherty Geological Observatory	
University of Miami	
Woods Hole Oceanographic Institution	
Membership Expansion	1968 - 1975
International Phase	1975 - Present
JOI Inc.	1976
U.S. Institutions Incorporate	
JOIDES: A committee structure supported through JOI, Inc.	1978

In the mid 1960s, the University of California was elected to be the prime contractor for this program. Global Marine Inc., of Los Angeles in turn was selected by the University of California to be the drilling contractor, and it is to the everlasting credit of this young and energetic company that the Glomar Challenger took shape and, as we all know, took her place in history as one of the great ships of exploration (Table 2).

TABLE 2 Contractural drilling history of the Deep Sea Drilling Project, 1968-1983.

Acceptance of Glomar Challenger	August 12, 1968
Phase I (18 months)	August 12, 1968 to February 11, 1970
Phase II (30 months)	February 12, 1970 to August 11, 1972
Phase III (36 months)	August 12, 1972 to August 11, 1975
International Phase of Ocean Drilling with Extensions (76 months)	August 12, 1975 to November 19, 1983
Demobilization of Glomar Challenger	November 20, 1983

I came on board as the first chief scientist of the program and then later took over as total program director.

It has already been mentioned in these proceedings that the vessel after which this Glomar Challenger was named was the original HMS Challenger (Figure 6). It was the HMS Challenger that began that century of oceanographic work that created the background into which the drilling program was to fit; 1972 was sort of the centennial of the HMS Challenger work. A tremendous proportion of this work, that we basically fit into, was accomplished following World War II.

It was no accident that the drilling program and the space exploration program came at the same time. They really were part of the same sense of a global view of the earth, and they both derived from the same two new technologies. One was the new materials, alloys of suitable strength; in our case, it was the improved S135 steel, suitable with even a nontapered drill pipe to reach the floor of the typical deep ocean. The other was high-speed data processing that made the dynamic positioning a possibility in its fully automated mode. The Glomar Challenger was the world's first fully automatic deep water dynamically positioned vessel.

The early days of planning required close cooperation to integrate the ship, the drilling system and capability, and the scientific outfitting. The ship was launched with a very nice celebration by Global Marine down in Texas (Figure 7a, b). The basic positioning system has already been discussed by Arch McLerran. The Challenger also had a reentry capability that was, of course, very close to what had been conceptualized, but never constructed, for the Mohole drilling. Shown in Figure 8 is the reentry cone which was positioned on the sea floor.

From the intellectual viewpoint, however, the ship was, first and foremost, a laboratory (Figure 9). One of the great contributions to deep sea drilling has been to work out the biostratigraphic correlations; scientists like Hans Bolli enthusiastically participated. We take a lot for granted today but back then, when the drilling started, there was not that much known in terms of the details of biostratigraphic correlation in the oceans; also, correlations had not been locked very well to the magnetic or other signatures in the ocean. It became a very important thing to lock the several major micro-fossil groups together. Even the nannofossils, little calcareous algae that drop their hard parts, had only recently been opened up as a fairly new field of biostratigraphic correlation. Today, oil companies just wouldn't think of being able to operate without the capability offered by these nannofossils, where you can get a perfectly good stratigraphic age from a volume of sediment that you can pick from under your fingernail.

We were fortunate in being able to install satellite navigation (Figure 10). We received the very first unit off the production line of ITT; Magnavox came along a little later. We fitted the ship with what we thought was a pretty good array of new equipment, including the capability to receive from satellites (Figure 11) and to have a weather observing capability, cloud cover photographs; we even had a meteorologist on board continuously.

Figure 12a-e illustrates basically the working end of the drilling system that drills the holes and takes the cores. This system passes through the center well to the floor of the ocean (Figure 13). Figure 14 was taken from "Alvin" when this research submarine went down to study benthic recolonization of the disturbed area and around one of our holes. Shown is the hole left by the drilling.

From Leg Two, Figure 15 shows the very first basement core; the site before this had, in fact, touched basement and gotten a few kernels of basement rock in the core catcher. This is the first basement core showing the contact with the sediment, of great significance to the vigorous discussions of what "acoustic basement" really was, within the seafloor spreading context.

By the end of the program, we had drilled 624 separate sites the world over, in all of the major oceans and marginal seas with the important exception of the ice-bound arctic (Figure 16). I might mention that the experience of DSDP opens some serious possibilities for drilling and coring the Arctic Ocean basin.

The stylized track of the Challenger, throughout all of that drilling, shows that we went to fairly high latitudes and criss-crossed the ocean; the Indian Ocean and the southeast Pacific Ocean still are a little thin in our coverage (Figure 17). The ports of call associated with the 96 two-month legs would make an impressive geography lesson.

There are two major repositories for the DSDP cores which are stored at cold temperature, just above freezing, one at Scripps and one at Lamont (Figure 18). The collection, of course, is being added to by the new Ocean Drilling Program; the cores for the new program are at Lamont for the Atlantic and Texas A&M for the Pacific and Indian Oceans.

We can see the variations in the program as one looks at the summary statements. Figure 19 is simply calendar-year accumulated miles of steaming. Early on in the program there was a fair amount of steaming, particularly as we circumnavigated the world. At the time that the international phase began in 1975, there was an abrupt drop in the amount of steaming and an increase in the amount of time on station. This was basically the result of that style of planning that came in as the result of the interests of the non-U.S. members and also the maturation of the program, having, in a sense, completed a first look at the world ocean. One saw, in that earlier half, the development of the kinematic model of the growth of the ocean basins, the seafloor spreading idea, plate tectonics, and a good start on paleooceanographic studies. At about the halfway point, at the beginning of the international phase of ocean drilling, one began to look at the possibility of drilling more deeply into the basalts, to spend a fair amount of time drilling these tough objectives on the mid-ocean spreading ridges, to seek much more continuous and high quality core, and to begin looking at the continental margins a little bit more seriously, thereby spending more time on single sites than we had hitherto.

A summary of the overall performance record reflects exactly what I just said; there was more cruising in the early half, more actual drilling in the latter half (Figure 20). The summary shows also weather down time, breakdown time, and standby time in port.

An integrated statement of the holes drilled in each calendar year shows a diminishing trend following the midpoint of the program, just about the time when JOIDES got going in the international mode and had time to get the planning into it (Figure 21). The picks in the latter half represent where we were working in an area near Central America where steaming distances were very small in and around Balboa and the hydraulic piston coring emphasis in the last two years when the whole point was to take relatively short cores in a large number of places.

An integrated statement of the total amount of core recovered, with respect to subbottom depth, is an inventory of the cores that were accumulated, just with respect to depth (Figure 22). The big side peak, in shallow depths, is simply a statement of one of the more serious facts of life; you have to drill through the shallow layers before you can get to the deeper layers.

A comparable kind of curve, but integrated with regard to age, of total cores taken, also had a programmatic inventory purpose (Figure 23). We wanted to be sure that we had not given short shrift to some particular stratigraphic interval or objective simply by accident. We kept track of this all the way through the program. There is some detail in the curve that is a reflection of variations of sedimentation rates and thickness of available sediment for a given age. Probably the reason the Atlantic shows this detail in a more exacerbated form is that the Atlantic has no sediment traps around it , whereas the Pacific is a more open ocean and does have sediment traps around it in the form of deep sea trenches (Figure 24a, b).

As to the integrated cumulative recovery, with respect to age, again comparing the Atlantic and Pacific, one sees that the Pacific Ocean, in terms of our coring, which reflects a certain statistical reality, is considerably younger (Figure 25a, b). The curve for the Atlantic shows a greater skewing towards older sediments. The Pacific Ocean is, in a sense, a younger ocean in the aspect that it showed to the drilling and coring operations. The Atlantic Ocean is oldest, consistent with the seafloor spreading model, at its easily accessible edges near the continents; the opposite is true for the Pacific Ocean, where the eastern half of it is dominated by the very young and rapidly spreading East Pacific Rise, and the western side of it is dominated by the young back arc basins behind the volcanic arcs and trenches. The oldest areas of the Pacific are in the remote west central portion and beneath difficult drilling conditions. Therefore, the aspect of coring that we recovered from the Atlantic is considerably older than from the Pacific Ocean. Water depths of the holes drilled are distributed on the major hypometric features of the oceans (Figure 26). The mid-ocean ridge area and the mature, older oceanic crust are clear. Of course, the longest drill pipe we used was around 7 km and that becomes the upper limit of this distribution, but remember that the ocean basins, including the deep sea trenches, get up to about 11 km depth.

We did have a technical development program (Figure 27); Arch McLerran already has mentioned that we drilled into chert. That told us we didn't have the bits that were really going to do the job; the diamonds just tore out. We entered into a bit development program in close consort with industry. Our operations manager, Swede Larsen, developed a series of the bits as we grew up with this particular problem (Figure 28). A center bit could fit into the hole where the cores would come through to permit drilling without coring. For those who may not fully know the style of the coring, it was basically to drill the doughnut and save the hole, which became the core sample. Core bit development along with reentry gave us excess capability relative to the program as it had been earlier conceived. We had worked on two fronts at the same time and had won on both. Being able to reenter the borehole, given the capability to change a bit, and having also designed bits that were now surviving all the way through the typical sediment cover in the ocean, encouraged us to think that we could drill deeper; that was one of the reasons, of course, why the early thinking within the international phase of drilling aimed toward the deeper penetrations that could be achieved, either in the basaltic basement or in the thicker sediments of the passive margins.

The maximum penetration that we achieved was off South Africa, reaching 1.7 km, all in sediment. The maximum penetration that we have achieved into igneous rock was site 504B near the Galapagos, where we went in a little over 1 km.

Figure 29 shows where reentry cones were set during the DSDP. These locations remain of interest because of the possibility of getting back into these holes again either by drill ship, such as the JOIDES Resolution, or by wire line reentry sites if you know that the hole has not plugged. The X across some of the reentry sites indicates that they are blocked with junk — steel cable, drill pipe, etc., — that was broken off and dropped.

The original reentry trials were back in 1970 and then it took us until 1973 to get into an operational mode. In the end, we had accomplished 124 separate reentries, with as many as 27 repeat reentries into a single hole (Figure 30). These numbers include reentries to change bits or to do an instrumental study and so forth. Of course, that leads directly into the instrumental program that also includes our well logging program.

There was a logging program early on for the first few legs. Then there was a fallow period when it seemed that the logging was not the answer to what scientists were really looking for at that time; they were interested in the whole development of the kinematic history of the ocean basin. The desired program was to specifically core only seismic reflectors and then into those oldest sediments, to verify and continue to test this whole new idea of seafloor spreading, ocean basin development, and continental drift. During the second half of the program, along with continuous coring, logging assumed a much larger importance.

An instrument emplacement program matured in the latter part of the program, based again on the reentry capability. Seismometers were emplaced in the hole, and some of these operated very successfully.

One other important development was hydraulic piston coring (Figure 31). This is simply a means to push rapidly against the core barrel at the bottom of the hole and retrieve a core that is very undisturbed. The point was to get undisturbed cores. One of the problems in coring, particularly in coring something with the consistency of thick yogurt or sour cream, which is not a poor description of some of the soft sediments, is that the heaving bit would tend to disturb the sediments very badly. Using the pump pressure to actuate a core barrel following the buildup of pressure against shear pins, we were able to get very good cores. The upper core in Figure 32a was taken in the Gulf of California without hydraulic piston coring; in the same general location, the lower core was taken with hydraulic piston coring, and demonstrates the tremendous improvement in quality; a blowup of it shows the extremely fine laminations preserved in the core (Figure 32b) This lamination is not characteristic of the deep ocean sediments; it can happen only in areas that are without oxygen in the bottom waters. The reason is that there are no burrowing organisms living in the absence of oxygen to disturb the sediments, and therefore this fine record can be preserved.

A more typical situation from the deep and open ocean also shows the advantages of hydraulic coring. The core on the right in Figure 33 is with hydraulic piston coring. The white spots are burrows. Burrowing organisms continually turn the sediment over to some numbers of centimeters. Two cores, taken 100 meters apart, demonstrate the continuity between layers that are cored (Figure 34). You can see why this kind of development was so significant to scientists who were interested in paleoceanography and willing to expend long years of their lifetime studying these samples that are far superior because they have not been seriously disturbed.

It wasn't all skittles and beer, however; we did have our problems, including drill pipe losses (Figure 35). There were five major losses of drill pipe; only two of those really involved a break of the drill pipe itself. Three of them were failures of equipment above the drill pipe, above what one would call the rotary table. Not a bad record in long-term.

We also developed the pressure core barrel which was designed purposely to recover the methane hydrates; those are solide that exist under high pressure and low temperature at the bottom conditions, composed of methane and other components of natural gas and water. And, yes, the core barrel worked and did recover some hydrates (Figure 36). On the other hand, while drilling off Central America in the Pacific, while not even using the pressure core barrel, we drilled into a spectacular example of those hydrates and recovered samples looking just like a dirty snow bank. People on the ship bad fun putting pieces of it into a plastic bag, which would promptly begin to inflate itself. The fun also had a serious purpose, to quickly estimate the volume of gas evolved from this very labile substance.

The data is almost all in the data base now, that can be computer manipulated; that data base will be totally completed with all the scientific data before the DSDP comes to a close.

Figure 37 is some of the summary statistics of the program, showing the number of holes drilled by calendar year, split in terms of the number of occasions in each year in which we drilled to greater than 500 meters and greater than 1,000 meters of penetration. Notice the proportion of deep hole; the 500 meter proportion is much higher in the early part of the program and what that says is that when you start going significantly beyond 1,000 meter penetration you begin spending a lot more time on any site that attempts to do that, at the expense of the intermediate depth holes. Similarly, for meters penetrated, note how much core penetration was accomplished in the earlier half of the program compared to the later half; this is the price of continuous coring and deeper holes and more difficult objectives, particularly drilling the basaltic crustal layer (Figure 38). A general improvement was experienced in the recovery of both shallower and deeper materials through the years, deepite more difficult objectives (Figure 39a, b).

The program really was international all the way through from early on; even for Leg Two, there were two non-U.S. scientists, Cathy Nigrini and Maria Cita. Leg One was all U.S. Thereafter, you can see from the percentage of U.S. vs. non-U.S. scientists, that the program was very international (Figure 40). Nothing ever again approached Leg 13 for proportion of non-U.S. scientists; Leg 13 was in the Mediterranean and we put a very large number of Europeans on board. In fact, so much so that the National Science Foundation admonished us to sustain less than half the number of scientists on board, thereafter, as being the proper maximum for non-U.S. participation. And you can see this effect, thereafter, in the U.S./non-U.S. proportions. Nonetheless, it can scarcely be contested that the early enlightened participation by the world community laid the foundation for the succeeding, very successful, international participation and contribution, which carries on in the Ocean Drilling Program.

In regard to participation by individual countries, of course, the United States goes right off scale; a participation in Figure 41 is defined as being a scientist on one of the cruises or requesting samples.

The five nations that were formally involved other than the United States, were the Soviet Union, Japan, France, United Kingdom, and Federal Republic of Germany (Figure 42). As shown in Figure 43 participation also included a number of people from the non-U.S. countries that have more recently become part of JOIDES and part of the new program (ODP).

The effect of becoming international was, in a sense, to become less international. The effect, basically, was to reduce the United States' participation and also the participation of the other nations that did not take part. For the five non-U.S. countries that joined, their slice of the action went up very substantially, but all the others had to go down. Figure 44 is a famous illustration, because this is the effect on those non-U.S. countries that did not join. Their participation had to go down and it is this effect that tended to drive the European Science Foundation and the smaller European countries, as well as Canada, to become members of the new drilling program, which has happened. Two other countries that initially showed great interest in DSDP, Australia and New Zealand, have not yet found a way to join.

We currently have about 90 volumes of the Initial Reports of the DSDP published and will, of course, have 96. The distribution of Initial Reports on a world-wide basis is shown (Figure 45); however, this particular illustration was made following the separation of the Soviet Union from the International Phase of Ocean Drilling. The Soviet Union was a member for five years from 1974 to 1979 and received its 100 books just like everyone else. Again, notice the very wide distribution of the Initial Reports, a distribution that is to be carried forward, effectively, in the new program.

One other major output from the program is the sample and data distribution to the community, world-wide (Figure 46). One can see that in the early part of the program, there was only sample distribution. Then we began to get the data base together and offered data distribution and processing. You can see that being able to get data, tailored to specific needs, was a very important and popular contribution.

It is possible to show a general utilization of the cores, by sample requests in terms of categories of requestors: U.S. academic, industrial, and governmental vs. non-U.S. academic, governmental, and industrial (Figure 47). The U.S. shows just about half of the action in all regards on a total program basis.

The program addressed the major scientific activities and exploration that, in a sense, have become part of the popular literature. For example, the San Andreas rift in California can be related to that whole set of processes making up the earth's geological activity, the growth of the ocean basins, the history of crustal subduction or the downward thrusting of the floor of the ocean along the deep sea trenches and associated volcanism and earthquakes surrounding the Pacific Ocean. The evidence of intercontinental collision, as in the case of India with Asia, leads to the whole concept of continental accretion, continental fragmentation, and so on. The "hot spot" concept applies to Hawaii, with respect to the migration with the floor of the Pacific northward beneath the equator but, more specifically, northwestward across the hot spot that keeps bubbling lava up and has created the entire chain of islands, the Emperor Sea Mount and the Hawaiian chain. Indeed, we drilled during Leg 55 on Seiko Sea Mount, well up the Emperor Sea Mount chain toward the Aleutians; we drilled clear through the coral reefs, the lagoonal deposits, and the tropical soil, and cored down into the volcanic edifice itself. The magnetic signature from the basaltic rock indicates that the latitude of formation, at the time of the cooling of this lava, was within one degree of the latitude of the big island of Hawaii today. This stands as an eloquent demonstration of the continuous migration of the floor of the Pacific and its northwesterly trend of movement, and of the fact that a major change in the direction of migration of an enormous sector of the Pacific crust is represented by the elbow between the Hawaiian and Emperor Sea Mount trends. The implications of all of this, in terms of continental growth and development, and creation of terrains that are collected into the continents, carry the concepts of seafloor spreading, and plate tectonics past this present generation of seafloor spreading, which is only about 200 million years old. And yet the earth itself is some four to five billion years old. So there is time, even at the present rates of seafloor spreading, if it continues into the past, to have 20 such generations of seafloor spreading, so to speak. That record is all lost out in the oceanic realms and it is only to be reconstructed within the continental realms. So we carry those concepts with us into the total global picture.

I want to say something about Bob Bauer. We have a very good friend in him. He was the chairman of the board of Global Marine and he and his cohorts A. J. Field, Curtis Crook, Russ Thornburg, and John Graham, who were the designers of that particular hull that was _Challenger_'s, all were enthusiastically involved. Bob Bauer was the senior man. The program attracted many dignitaries, including the Queen of England, who visited Scripps.

Drilling 624 separate locations (Figure 48) over much of the world provided a basis by which one could evaluate how a ship like the _Challenger_ could perform in operations throughout the entire ocean. It is a trial worth doing, the reason being that it's rare to get 15 years of consistent work on a single ship.

I'm proud to say that in the financial history of the DSDP it was never over budget. We lived through that very difficult period, from our point of view, in the sense of planning ahead budgetarily. It was presumably a good period from the oil drilling industrial viewpoint; the end of the DSDP and the Glomar Challenger's activities coincided just about precisely with the onset of the difficulties of the drilling industry, still with us today.

Each cruise had its fun; emblems for most of the cruises are stenciled all over the ship. We had two captains on board the Glomar Challenger; one was Lloyd Dill, the other was Joe Clark. Joe Clark was master all the way from acceptance trials clear through to the demobilization. I was with him on acceptance trials and at Mobile, Alabama, at the end. Lloyd Dill came aboard a couple of legs into the program, and was there until just about the end of the program when he retired.

I did ask the two captains what they thought were the most harrowing incidents. As far as Lloyd Dill is concerned, it was during the last of our Antarctic legs. They got caught in a storm. It was a vigorous storm from down in the Drake Passage. They turned tail and ran, mainly because they couldn't bring Challenger's bow into the wind. So they ran hundreds of miles into the lee behind Patagonia.

As far as Joe Clark is concerned, his most harrowing experience, as he remembers it, is the time that we were to go under the bridge across the Shimonoseki straits, near Japan. Everybody had worked out the calculations just right. The drilling tower was going to clear the bridge's highest point, at its middle, just at slack tide, by something like 6 feet. Well, things delayed a little bit in terms of getting underway; that was because the pilot that we had on board to leave port had told us that we also had to have another pilot to go through the straits. We had to wait half an hour to get him aboard and by then the tide was already running in the direction that the ship was going to go. As the pilot was taking the ship toward the bridge, he chickened out. He saw that derrick getting closer and closer to the bridge and ordered full astern. The ship spun sideways and went under the bridge anyway, with the current. The bridge painters, up there on scaffolding, were scrambling like ants. Meanwhile, this pilot was saying something to himself, over and over in Japanese. After he got off, someone asked the other pilot what he was saying. It translated, "Why me? Why me?"!

The cooking on the ship was legendary and contributed to the morale of the whole program. In Figure 49, John Duke and I are casting off the last line on the very last leg. Figure 50 shows the Challenger all stripped down, drill pipe off, scientific equipment off, crew off, everything off, down in Mobile.

The present circumstances of the Challenger are that she was sold for junk and towed to Brownsville, Texas. In fact, we came very close to getting the Challenger into a maritime museum situation; it would have been in the Intrepid Air, Sea, Space Museum. We worked closely with Elliott Sivowitch of the Smithsonian for some months. It all started with trying to get some of the Mohole antiques from the ship into the Smithsonian. Elliott is here at this meeting. We got very close. The museum had the money approved, they had the permission from the harbor authority, and it was 24 hours late. Global had signed the contract to dispose of the ship. John Duke and I went down there and we did lift off the dynamic positioning system, including the components that had been built for Mohole. These are now in the Smithsonian, along with the bridge hardware such as the engine telegraph and wheel. I understand they are going to put up a display sometime. The ship, as far as I know, is still in Brownsville, Texas, tied up against a mud flat.

Now for thoughts of the new era we're entering into right now. This is part of a continuing program, and a whole era in the development of understanding. Each program is a chapter as we move forward. Discovery is what it's all about.

D. James Baker, Jr.

I was reminded, when listening to Mel's talk, that the number of miles that were travelled by the Challenger are very close to the number of miles that were travelled by El Tannin, which was another ship that went on a long voyage of exploration and discovery in the Southern Ocean, run by the United States. Actually, the El Tannin in 10 years covered about 410,000 miles, a similar order of magnitude of distance, and it's a ship that is operated in a similar way, that is it was out for 60-day legs at a time. There may be a lesson there; that that kind of operation is a very efficient one.

Well, this afternoon we have a similar structure. This morning we talked about Project Mohole and how that led to the DSDP. This afternoon we're going to talk about Ocean Margin Drilling and how that led to the Ocean Drilling Program. To start the discussion as we started this morning we have Bill Hay, who's been involved with the drilling program for a number of years and was President during those years of ocean margin drilling.

OCEAN MARGIN DRILLING
Speaker: William Hay

The origins of the Ocean Margin Drilling Program go back to some conferences that were held by JOIDES in the 1970's and dealt with what could be done if a more sophisticated drilling system with the capability of fluid recirculation and well control were available. The International Phase of Drilling (IPOD) was the international program of JOIDES. It was formulated in the early 1970's for a 3-year period of drilling with the Challenger, to be followed by drilling with some form of well control, so the idea has been around a long time. But it wasn't until the late 1970's that the scientific community began to face the problem squarely as to how this might occur. The idea arose in governmental funding circles that the greater expense of such drilling operations could be handled by developing a larger funding consortium, not just with foreign partners, but with the petroleum industry in this country as well. The Ocean Margin Drilling Program began in early 1979 at a meeting at Rice University between representatives from the U.S. science community and the petroleum industry. The meeting was called and opened by Frank Press, who was at that time Science Advisor to President Carter, then presided over by Phil Smith. This was the first time that a group of scientists with some novel ideas and hitherto unachievable goals had met with a group of industry geologists and engineers who had some idea of the technology that would be involved in achieving those objectives. It is worthwhile to go over a brief recounting of some of the ideas that were suggested and some of the reactions that followed.

I was asked to chair the scientific discussions at that meeting and it was certainly an interesting experience. I have never worked formally in industry but my history of involvement with drilling goes back well before there was ocean drilling — back to the time when we used to be eager to get cuttings from wells and to look at them for micropaleontological purposes. Such samples used to be considered great treasures no matter how you got hold of them, so I had some idea of what could be done using drilling fluids and return circulation, and what conventional drilling was like on land.

During the course of that meeting in Houston, I became aware of the fact that we had raised an entire generation of marine geologists weaned on the Glomar Challenger who never knew that anything but cores could be recovered from a hole. It was a revelation to me, but a much greater revelation to the industry people who were there.

The first concept that was explored was the length of time required to drill a hole. The idea was that we would have a 10 km drill string and, of course, every hole that was proposed involved using the full 10 km capacity. The first hole that was proposed was to drill the ocean margin on the Blake Plateau, where the water depth is about 2 or 2 1/2 km, 5 km of sediment would be penetrated, followed by drilling 2 1/2 km into the basement, probably granite. You could have cut the silence after that proposal with a knife but, finally, someone from industry said, "Why do you want to drill 2 1/2 km of granite at the bottom of the hole?" "Well, it might be interesting," was the answer. "Well, has anybody drilled a 2 1/2 km of granite on land?" At that time, only the Soviets, with their Kola Peninsula deep hole, about which we knew very little, and perhaps the mining industry, using rather different techniques, had achieved such penetrations in crystalline rocks. Clearly, some of the ideas needed a little more investigation before they could be formally proposed. It was also very interesting that although scientists who had been involved with the Glomar Challenger knew about drilling from a ship and were aware that it takes about a month to drill and complete a 1 km deep hole with the Challenger, seemed surprised that it was going to take at least 10 months to drill a hole 10 km deep. The scientific community was honestly startled by the fact that the holes being proposed would take six months to a year each to drill. This aspect of the project hadn't been factored into the thinking of the academic community, and it was a matter of real concern to the industry participants.

Another question arose over the need for continuous coring. Continuous coring had become a rule with the DSDP about one-third of the way into the life of the project. It had become an accepted axiom for the academic scientists, but as soon as the industry people understood that the intent was not only to continuously core the 5 km of sediment at the Blake Plateau site, but the 2 1/2 km of granite that lay underneath it, they inquired, "Had anybody heard of cuttings or logs?" Well, practically none of the academic scientists had, and only a couple of us older micropaleontologists had had any experience working with them. So the possibility that one might not have to core everything came up for discussion. Today we're still not considering using cuttings because, of course, there is no return circulation possible in the Ocean Drilling Program, but the other possibility that was brought up at the Houston Meeting was the use of logs to fill in gaps between cores. Logging had been tried in the DSDP, and generally it didn't work; it was at best partially successful. Today, logging is a very valuable part of the Ocean Drilling Program and it is filling in data on parts of the holes where core recovery is poor. The academic community is learning a lot more about how to use logs and the education which started with the Houston meeting has become very important.

Another interesting discussion was initiated when one of the DSDP old timers said, "Well, we can't think of spending a year on a hole because you have to finish in two months whatever you start out to do." Again, the industry people were silent until someone finally asked, "Why?" "Well, because the legs are two months long, and you have to finish each leg and go on to a new topic after two months." In the course of several subsequent meetings the industry scientists made the suggestion that it would be a good idea to define the objective and then figure out how long it would take to achieve it, and to design the project around the achievement of the objective rather than around the two month legs. This turned out to be a longer educational process than one might have expected because the next thing the academic scientists discovered was that it would not only be necessary to carefully define the objective and determine how much time it might take to achieve it, but that we would have to design the hole around the objective, especially if it is deeply buried. It would be necessary to determine exactly how far down the hole is to be drilled, and the hole must then be designed from the bottom up. Only by designing the hole in this way can one be sure that it will be possible to get to the objective with a hole of the right size in order to take the right samples and make the appropriate measurements at the bottom. This was a new concept to the academic community, but I think it has now begun to play more of a role in thinking how we achieve objectives in ocean drilling. The planning process that evolved from discussions on the Ocean Margin Drilling Program was: first, scientists would gather all the information on a particular area; second, determine the scientific objectives which could be achieved in the area; third, select sites and propose what the holes would look like; fourth, analyze the drilling of the holes to find out how long it would take; and fifth, develop a program for drilling, sampling, logging, making measurements, and implanting instruments.

We spent about three years at the planning exercise and I would like to describe what was accomplished:

First of all, we recognized the need for looking particularly at the ocean margins. There is some knowledge of the passive margin shelves, both from onshore and shallow offshore drilling, but the continental slope and rise are virtually unknown. The active margins, although quite small in terms of their area, are also relatively poorly known. In terms of the volume of sediment, the ocean margins are big unknowns. They contain one-quarter of all the sediment in the world and we really don't know what its composition is. To give an idea of the varieties of opinions, it is useful to recall that at the time we started discussion of the Ocean Margin Drilling Program, the majority of the academics thought that the sediment sequence of the east coast margin of North America would all be carbonate below the mid-Cretaceous. Most of the industry people already knew that it is mostly deltaic sediments below the middle Cretaceous.

In terms of geologic history, the post-Paleozoic rocks comprise two-thirds of all the sediment on the earth; the Mesozoic and Cenozoic sediments of the passive continental margins are a very important component part, about 40 percent of the whole yet we still don't know very much about them. In all of our projections of the global geochemical cycling we make the assumption that igneous rocks are weathered to produce sedimentary rocks that have the same composition as the sediments preserved on the continents. We hope that this assumption is generally correct, although we know that it is in part wrong, because some sediments, such as evaporites, are preferentially preserved in the deep parts of passive margins.

Much attention has been given to obtaining an offshore record that could confirm the hypothesis that stratigraphic correlations can be based on global sea level changes, as proposed by Peter Vail and his colleagues at Exxon and used by a number of people in the industry. The hypothesis suggests that the sediments forming transgressive and regressive sequences during rising and falling sea levels are going to be radically different as they are traced from the continental shelf to the deep sea. What the scientific community would like to know is the overall history of passive margins: what materials they contain and what the history of those materials has been.

Other areas of interest were the active margins, and to explore in particular what happens as material is subducted.

A third area that was identified was the ocean crust: What does it consist of? We know about the inferred seismic velocity of the crustal materials, but what the materials actually are is a larger question. At the time these meetings took place we were just beginning to find out that the ocean crust consists of different materials at different places and at different ages.

And then finally, the history of the ocean environment was considered important to understanding the development of the modern Earth. For example, in the South Atlantic, warm surface waters presently overlie much colder intermediate and deep waters. However, in the late Cretaceous that ocean basin was smaller and filled with water much warmer than today's deep waters; it had an intermediate, relatively cooler water layer and a relatively warmer deep water layer. A totally different kind of ocean thermal structure existed then, and a totally different kind of ocean circulation must have existed.

In order to investigate these four topics a number of geographic areas were selected for study. It was realized that for this program many months would be spent on a single site, drilling to considerable depths. It would not be possible for the ship to wander all over the globe. The ship ought to stay close to sources of supply, so the areas selected as candidates for exploration for the passive margins included the East Coast of North America, the Gulf of Mexico, and the region off Morocco. For the active margins the candidate areas were regions off Oregon and Washington, off Central America, off the Antilles near Barbados, and off Peru. For the ocean crust, three areas on the mid-Atlantic ridge and two areas in the Pacific were selected for study. For the paleoenvironment, it was realized that relevant information would come back from regions selected for other studies, but the Antarctic was specifically targeted in order to extend our latitudinal coverage of our knowledge of the polar oceans.

After these target areas were selected, ten energy companies (Atlantic-Richfield Co., Cities Service Co., Conoco Inc., Exxon Corp., Mobil Oil Co., Pennzoil Co., Phillips Petroleum Co., Standard Oil Co. of California, Sunmark Exploration Co., and Union Oil Co. of California) and the National Science Foundation pooled funds and a contract was issued to Joint Oceanographic Institutions, Inc. (JOI) to produce a series of atlases compiling all the existing knowledge of these areas. JOI put out RFP's for these studies and many members of the scientific community responded and worked on these compilations. All but one of the atlases have now been published. They contain an enormous amount of information but, unfortunately, they do not contain any speculation or interpretive discussion about the areas. They are strictly factual reports, but they have generated a number of interesting ideas which we will want to investigate sometime in the future.

It is interesting to note that when the ocean margin drilling program faded away in 1982, most of these involved breathed a great sigh of relief because by then it had become obvious that by 1986 the petroleum industry would have accelerated its exploration into deep water and would already be drilling deep holes in 9,000 and 10,000 feet of water; we expected that the information from this deep industrial drilling would be available to us by 1988 or so and hence, the need for the Ocean Margin Drilling Program had evaporated.

It is also interesting to look over the list of the ten supporting companies and note that not all of them are still in existence. Many petroleum companies have merged and the conditions in the industry today are radically different from anything that Ocean Margin Drilling planning group thought about at its last meetings only four years ago. In 1982, we were quite confident that a lot of this fundamental geological information would be gained by industry by now; today, we can only suggest that perhaps some of this information will be gained by industry or some other body by the year 2000.

For the passive margin areas, the goal was to extend the stratigraphy that is known from the COST and industry holes on the shelf or the upper part of the slope across the deeper thickly sedimented part of the slope and rise to the DSDP holes in the outer continental rise where the stratigraphic section is about 1 km or so thick. To accomplish this, two deep holes were proposed on the Atlantic margin of North America, one relatively shallow site to be drilled early in the program and one of much greater penetration to be drilled later in the program when there would presumably be a significantly greater capability for such deep drilling.

On the conjugate margin off Morocco there is much less sediment and holes were suggested there that would presumably penetrate to the very base of the stratigraphic column.

The deep holes to be drilled on the east coast of the U.S. would most certainly face the problem of relatively high gas pressures and fluid flows in the wells. It is hard to drill off the east coast and not hit something that will create a problem for drilling, especially to the kinds of depths that were being proposed. But in the Gulf of Mexico, the situation is even more dramatic. The Gulf of Mexico is a basin which, as it opened during a short episode of seafloor spreading, split apart an earlier salt deposit. Subsequent loading of sediment onto the Texas-Louisiana margin has caused the salt to be squeezed south as a laterally advancing salt front which has produced a steep slope called the Sigsbee Scarp, bordering the north side of the deep basin floor. Beneath the migrating salt front is an unknown amount of sediment of unknown age; some of the sediment is probably relatively young and quite a large volume of sediment may be involved. We know little, if anything, about the geology of the deeper part of such margins and if one is to consider drilling through the salt mass, one must be prepared to experience an effect similar to pricking a balloon with a pin. Dramatic pressures may exist beneath the salt. In planning for the Ocean Margin Drilling Program it became apparent that no one felt that during the proposed 10 years of the program, we would have gained enough experience to be able to drill through salt with confidence that if we encountered overpressures below it, we would be able to control such a well. Nevertheless, there were exercises exploring what might happen if one did drill into a highly pressured zone at depth and started to get a release of gas into the recirculation system. If the gas comes up the return system to the ship, it fills the riser with gas rather than water; suddenly all the densities begin to change and the riser may collapse, resulting in total loss of control and a free flowing well on the bottom. How quickly can trouble be sensed back on the ship and how quickly must preventative measures be taken? For some of the situations, it turns out that the speed of reaction is quite critical and there would only be minutes or seconds to make the correct decisions. This is a significant problem that is going to face any drilling in deep water.

So, although there is great interest in finding out more about the salt in the Gulf of Mexico and what is beneath it, the technical difficulties were so great that it had to be removed from the proposed ten-year drilling plan.

Operations in the active margins involve drilling into subduction systems in which there may be high fluid pressures caused by the subduction process itself. What would be the nature of the fluids that would be encountered? Will there be only sea water or will there be sea water and gas or sea water and hydrocarbons of some sort? Again, the drill may enter a high-pressure situation; and it is very important to plan for handling it in a controlled manner.

Three holes, two shallow and one deep penetration were proposed off the Oregon coast. The deep hole would have about a 5 km total penetration to ocean crust through a margin that is being actively compressed and which must have some relatively high fluid pressures within it.

Off Central America, the idea was to drill through an accretionary zone and down into material being subducted; again, there must be striking pressure differences at such depths.

One of the regional atlases that had some of the most spectacular results is that of the west coast of South America. Off Peru, fracture zones strike at right angles to the shore. As the fracture zones start to be bent down into the trench that borders the continent, small spreading centers develop in the center of each fracture zone, bringing up young hot material so that the heat flow in the fracture zone is increased just as it enters the subduction zone. The copper deposits of the Andes lie in the projection of these subducting fracture zone spreading centers. The effect is as though there were tiny hot spots moving underneath the continent, heating the rock and causing remobilization of material to form ore deposits inland.

The Peruvian margin, in contrast to that of Central America, is bounded inshore by cratonic material which is being actively abraded by the ocean crust. As the ocean crust bends over the seaward rise into the trench system off South America, it is broken into a series of horsts and grabens which look rather like gear teeth; these are slowly grinding this continental margin away, carrying the material down into the subduction zone.

Much is known about the on-shore area here and drilling in the relatively unknown offshore, although it would require well control, would contribute greatly to our understanding of the relation of subduction and the formation of ore deposits.

Another area which has since been incorporated into the present Ocean Drilling Program is off the Antilles near Barbados. It is already known from Glomar Challenger drilling that there is an active thrust fault in this area with relatively high-fluid pressures below the fault and relatively lower pressure above it. This is a very interesting dynamic situation for investigation.

The atlases that represent the candidate ocean crust areas show a very interesting thing: that the seafloor spreading system has a certain amount of trouble operating. The mid-ocean ridge system keeps generating new ocean crust but the new crust doesn't fit into the space that is available, with the result that the orientation of short ridge segments keep changing. Ocean crust is created and seafloor spreading goes on for a while, then the system jams and has to reorient itself to spread in a slightly different direction to break out of the deadlock. The result is that a very large proportion of the Atlantic ocean floor, much larger than had been anticipated, is what is called "fracture zone crust." This type of crust is very difficult to drill and because it is highly fractured and deformed, the results from drilling it are not always easy to interpret.

Other areas of interest in the ocean crust concerned the very young rapid spreading crust of the Pacific and the hydrothermal systems associated with spreading centers. We have only achieved 1 km of penetration into the ocean crust until now, so there was great excitement over the possibility that the OMD would be able to achieve a 7 1/2 km penetration of the crust beneath 2 1/2 km of water.

The last of the atlases to be produced, which is at the printer now, is on the Antarctic; the number of candidate holes suggested for Antarctic seas was not very different from the holes that have been proposed for the Ocean Drilling Program.

At the end of this compilation exercise, which provided a basis for preliminary selection of drill sites, the Scientific Advisory Committee was to select specific sites and then have drilling plans for those sites worked out in detail to determine how long it was going to take to drill them, recover the cores, and complete the objectives. We never got to that phase of the project, but the Scientific Advisory Committee did select a set of preliminary sites and worked out a 9-year plan. A number of the holes took many months to over a year to complete. The academic scientific community looked at this and said, in effect, "we didn't really think it was going to take that long to drill those holes and we think we'd rather have a lot more short holes for a while than to attempt these." Although the plans to drill deep holes using well control has been put on the shelf for a while, the problems will still be there; they are not going to go away. They are simply going to become larger in terms of their importance as unknowns to us.

The Ocean Margin Drilling Program left a legacy in the form of the concept of a U.S. Scientific Advisory Committee. When the U.S. institutions involved in the DSDP became involved with foreign partners we discovered that the foreigners were much better organized at proposing sites than we were. They had much better systems for getting site surveys completed, and for carrying out all the necessary studies to present a good case for getting a particular hole drilled. In the U.S. scientific community, we were accustomed to putting the information together in a very ad hoc manner and as best we could. Along with the new drilling program has come a U.S. Science Advisory Committee which helps organize the U.S. effort, and which has funding to help U.S. scientists to make credible proposals for drill holes for the Ocean Drilling Program.

As Dillard Hammett will discuss, we looked at the Glomar Explorer as a possible vehicle for the Ocean Margin Drilling Program and that was an interesting venture in itself. I am personally pleased that the funding for the Atlas Program and additional funding in NSF for ocean drilling emerged out of this whole exercise. A major contribution from the Ocean Margin Drilling Program exercise was a fair amount of education of the academic scientific community as to what is possible, what is not possible, and how difficult it may be to reach some objectives in the deep sea ultimately.

D. James Baker, Jr.

I think that's right that the objectives of that program are still to be met and I hope to see that at some time in the future.

Our next speaker is Dillard Hammett, who played a key role in the new program, the Ocean Drilling Program. I think it was Bill Neirenberg who suggested when it looked like we would not be able to use the Explorer because of cost, that we might be able to lease a new vessel and such ships might be available. We started to look into that and talk to a number of people and eventually settled on SEDCO. Dillard was the one who convinced us that "anything could be done" and I think he charmed everybody into really believing it and it actually turned out that way. He's going to tell us about some of the engineering having to do with ocean margin drilling.

OCEAN DRILLING PROJECT

Speaker: Dillard Hammett

The objectives of the Ocean Margin Drilling Program were presented by the previous speakers. To accomplish these objectives would require a drilling vessel with lots of space, capability and flexibility because the objectives were not defined and the work was in the unknown areas of the ocean margin. Everyone wanted flexibility because they knew very little about the geological formations they intended to drill. It is a long distance between wells, far offshore where the scientist wanted to drill. When you start drilling a well — one here in Washington D.C., and the other one in Dallas, Texas, and you drill one half way in between, you are really drilling them "nowhere" and you have no geological information. Recently in Africa, we drilled two good producing wells one-half a mile apart and then we drilled a dry hole in between them. Basically, that was the type of drilling expected in the Ocean Margin Program. You knew very little of what you were going to find below the ocean floor; therefore, you had to have a high degree of flexibility in the planning. This flexibility ended up to be very costly for the coring program as compared to oil exploration drilling practice. In coring, you have to recognize that all you're doing is penetrating the ocean floor, getting some cores, and bringing them to the surface.

It is a large accomplishment to get a good core in many of the ocean margin areas where drilling problems will exist. If you are subjected to a high pressure, you are subjected to formations that really you don't know how to control because they are sloughing. It takes a number of casing programs to hold that back and you do have to have a program with a lot of flexibility. That takes bigger equipment, that takes riser string circulation and, most of all, a lot more money. And that's really what in the end got the Ocean Margin Program -- it took a lot of money because it took a lot of time.

There was coring drilling done a long time ago by Shell Oil Co., both in the Gulf of Mexico and on the West Coast. Dynamically, stations operate out to about 4,000 feet of water. The Cal drill, I believe, has done coring operations all the way from Alaska to the east coast of the United States and a lot of information has been generated from that for oil companies and science.

And then we go into the DSDP. My understanding is that from the scientific information that has been generated and the developments that have come out of it, the industry has learned a great deal.

There were other events going on at the same time; drilling was being done at 800 feet of water depth with an anchored vessel. By 1970, we had already drilled in 5,000 feet of water; in 1978, with the capability we felt we had, we would, in 1980, be able to get to 8,000 feet of water. In the Red Sea, seabed sampling had been done in 8,000 feet of water, where a technology of establishing on the floor was economical to recover the mud and process it for silver.

Deeper operations are those of ocean mining — 16,000 feet of water — and, of course, DSDP — 18,000 feet of water. Ocean mining with minerals on the ocean floor, the sleds and risers that were used with it, and the processing involved while moving across the ocean floor with instrumentation, showed that these techniques for deep depths could be accomplished.

Within the petroleum industry, which has been mentioned in the participation of the Ocean Margin Drilling, we were drilling at that time out in deep depths; but most of the experience was back in shallow enough water, where we could seat on the ocean floor or could drill a well and test it. We did not have to worry with high gradients (8,000 feet of water depth), the gas back at the surface, or controlling it in the deep depths.

In the past, drilling was done in 600 feet of water; yet it had already moved to 5,000 feet of water in the 1970s. With dynamic stationing, drilling with subsea blowout preventers (BOPs) were aiming at moving out to the continental rise area in 13,000 feet of water within the petroleum industry.

There were many vessels available in the industry, about seven of them that had the capability of working out in that water depth. An experience of that came from the Mooring Systems, a riser and Subria stack on the ocean floor. When we moved into deeper depths, we had to accomplish four things: (1) "dynamic positioning" or "dynamic stationing", used not only in the coring drilling in the vessels that I've attempted to describe, but also in the Glomar Challenger; (2) sonar/TV re-entry; (3) electric BOP controls; and (4) a special buoyancy for the riser (Figure 51).

With dynamic stationing in the 1970s in order to be able to prove the capability to drill a live oil well, you actually had to stay on location 180 days and, as mentioned by Mel previously, when the weather came up, you didn't have the opportunity to abandon that location and move around behind an island — you had to stay there and complete a well through the 100-year storm. So there's a difference there between drilling and coring and drilling a producing, or pressured, zone. So in dynamic stationing, the reliability had to be for long time periods. Reentry had to be done because we were no longer working with guidelines and, rather than just reentering with a core bit, you're reentering with a BOP stack that weighs about 200 tons, you're latching on to a well head so that you can hold 10,000 p.s.i. pressure. So there's a difference in degree of reentry. You can have a hydraulic line to the ocean floor and it takes many minutes to activate it on the ocean floor; by that time, the blow out is in the riser and your riser has collapsed and you've lost control of the well. So in the degree of ocean margin drilling, and the industry as it was at that time, you had to come up with electro-hydraulic so that when you push the button it's like the electric switch — the light came on immediately and we could control that stack in deep water in a few seconds.

The biggest and most outstanding thing as it came up in the ocean margin was the riser. The riser is still today our most difficult thing to accomplish in the deep water.

In the dynamic stationing — there are many types of orientation of thrusters and the types used as mentioned before in the industry now — we're using primarily fingers on the ocean floor, and rather than, as seen in Willard's movie, having to do with the reflectors at the surface, everything comes from a pinger off the ocean floor. Very reliable systems today hold on for long time periods with hardly any maintenance.

In the sense of reentry, when the BOP stack is mentioned, it is not just a drill string and a bit, you're handling a riser which is about 36 inches to 54 inches in diameter, that weighs and costs a lot, and also takes a very large storage. It takes a big ship to store each of these sections you're looking at, 15,000 feet of riser.

You are also handling a BOP stack which not only has to reenter that cone, but also hold high pressure once it is reentered and connected to the well head. Syntactic foam, mentioned before, is used to support the riser and the coupling at the end of the riser. The coupling has syntactic foam on it at the end, and is a high stress area. It requires a means of connecting a riser very easily, but at the same time carrying the full stress up through the riser. It is a cumbersome operation and so if you're in 15,000 feet of water, with bad weather coming on, you have to have a means of disconnecting, still controlling the well, and standing there on the location while the storm passes; so you do have to have methods of suspending the operation with constant tensioners to hold up on the top of the riser.

There are a lot of computer simulation programs run in the Ocean Margin as there have been in the past on the stresses in the riser as they are subjected to various currents and wave conditions. Coring, as mentioned, has worked and in coring it is very simple with just the reentry of the bit and the coring operation that goes along with it but in the drilling operation you're running bits, you're circulating back to the surface.

For example, consider a land or a shallow water location. The fracture gradient and the hydrostatic overburden pressure that you have is quite a bit different when you're in shallow water. As you get in deeper water, you have a water overburden, rather than a sediment overburden, and the difference between your frac pressure and your hydrostatics gets closer; so if you're at a point where your method of controlling the well in deeper water gets very close and very sensitive to the gas coming back, to bleed that gas off is a very sensitive operation at 3,000 feet of water, and we can tell you that, because we had a blowout at 3,000 feet of water, which collapsed the riser. It's sensitive at 3,000 feet, and it's even more sensitive at 15,000 feet of water depth.

The reentry system, as mentioned, has worked very well through the years — reentry in high current with high pressures and large BOPs. The reentry cone that is used in the Ocean Drilling Program is about 11'6" in diameter. To give you an idea of comparison, in 30,000 feet of water with a cone at the ocean floor, it's about equivalent to trying to place a 3" diameter cup from a 60-story building (Figure 52). This has been done a number of times and we do have the capability of doing so in deep water.

The Explorer was the ship that was looked at for conversion. Lots of money was spent on evaluating the conversion to meet the criteria and plans of the Ocean Margin Program. It was thoroughly investigated, the conversion costs and types of equipment that could go on board were explored, and it was concluded that the costs for conversion were too high and that existing ships and technology could do the Ocean Margin Program. In 1985 the total capability would be floor drilling at 13,000 feet of water depth using a riser and, that at that time, actually the feeling was that the technology for drilling in 13,000 feet of water would be available by 1982.

There were vessels of opportunity, existing vessels, that were available. At the end of the Study Phase of the Ocean Margin Program it became obvious that the scientific world could go on out and complete the sedimentary drilling that was going to be the next phase of the scientific program; and rather than just letting the Earth Science program die, the objective should be to take what we learned in the Ocean Margin Program and suggest to the scientific world that there were vessels of opportunity that could be used in the sedimentary program for extending on into what became the Ocean Drilling Program.

We learned a lot from the studies done on the Ocean Margin Drilling Program. Where the scientist can justify work programs, I am sure we drillers can provide the ship, the equipment, and the technology.

I would like to say that the vessels, the scientists, and the engineers that we have out there today have been able to accomplish everything that I heard of ten years ago — the Ocean Margin Program objectives — and have carried these over to the ODP.

I believe that we are going to learn as much, if not more, from the earth science program from ODP as has been learned in the space program. Also, more of what we learn will have application to our life than what we're learning from the space program.

I would like to say also that there are a lot people here with the title "Doctor" in front of their names. I would like you to know that we drilling people tag along with the scientists and I feel that what we have added to the program is our "can do/will-do" attitude.

D. James Baker, Jr.

Dillard didn't go into the details of how the ocean margin drilling program ended and why it ended, and I'm not sure anyone really understands. But I know there are a lot of people who have different pieces of the story and I hope that sometime we can have a historian of science and engineering try to put together that whole issue. But it is a fact that the Explorer is still sitting in San Francisco Bay waiting for a taker and maybe there will be a taker at some point. There's no getting around the fact that the way you learn about the earth is to bring back samples and the only way you get samples of the deeper part of the earth's crust is to put a drill down there.

If the astronomers can convince the country to put a space telescope up there's no reason why we shouldn't convince the country to do deep riser drilling, and you can be sure that we will be continuing to try to do that.

Our next speakers are the modern heroes of ocean drilling who, like Mel and Arch, have dedicated themselves to the success of ocean drilling and are the leaders of that very successful new program. The drilling part is based at Texas A&M and the logging part at Lamont-Doherty. The first speaker is Phil Rabinowitz who comes from Lamont and is now at A&M.

OCEAN DRILLING PROJECT
Speaker: Philip Rabinowitz

In similar fashion to Arch McLarren, who had a choice of where he wanted to move to, I also had a choice and it was narrowed between La Jolla and College Station and for convoluted reasons that only a Texas "Aggie" can understand I chose College Station.

The ocean drilling program is an international program of scientific ocean drilling and commenced its field operations with a shakedown cruise and sea trials cruise in the Gulf of Mexico in January of 1985. The drilling vessel SEDCO/BP 471, or better known to the scientific community as the JOIDES Resolution (Figure 53), is presently drilling in the Pacific Ocean off the Galapagos and attempting to deepen a drill hole that was drilled in a previous program of deep sea drilling. This is the deepest hole that has ever been drilled into the hard rock basaltic ocean crust.

The ocean drilling program, as you are aware, is a successor program to the highly successful DSDP. It is an international program with partners to the U.S. including Canada, France, Japan, United Kingdom, West Germany, and a consortium of 12 countries within Europe called the European Science Foundation.

The ocean drilling program also has a new science operator — Texas A&M University, and a new drill vessel with expanded facilities. These facilities include a much larger drill string, 30,000 foot drill string, enabling us to drill and retrieve cores in almost any part of the world's ocean basins.

We have highlighted two capabilities. We have already drilled in the far north in the North Atlantic in Baffin Bay and in early 1987 we plan to go to the Weddell Sea off Antarctica. We also have a very ambitious program bordering the Antarctic Margin in the southernmost Indian Ocean.

We have riser capabilities which when utilized should enable us to retrieve cores in previously inaccessible geological environments.

We have a 12,000 sq. ft. laboratory stack, a 7-story laboratory stack, with state-of-the-art equipment for analyzing a reasonably complete suite of the physical and chemical properties of the rocks. We also have a scientific and technical complement of 50 persons, that is a fairly large scientific and technical complement, very excellent sea keeping and very excellent heat compensation which is especially important for obtaining high-quality cores.

Figure 54 is a picture of the JOIDES "Resolution" before conversion. In addition to many other changes, we removed the BOPs. We took the derrick off, and strengthened the derrick to sustain a much higher load; we added a 400-ton, probably the world's largest, heat compensator and built a 7-story laboratory stack. I should note that the conversion of the vessel from an oil drilling ship to a floating scientific research center would not have been successful or reached fruition without the dedicated and sometime heroic efforts of two of our previous speakers, Arch McLarren and Dillard Hammett.

Figure 55 shows the laboratory stack of areas where we made the conversion. There is a 7-story laboratory stack, with down-hole measurements on top, a core-entry sedimentology laboratory, and numerous other laboratories: paleontological, petrological, physical properties, paleomagnetics, chemistry, gas, etc. We also have areas for the computers and science lounge. The bottom three levels below the main deck are mainly service labs, refrigerated cost, etc. The vessel is terrific. It is a huge vessel, the laboratory stack is seven stories high and comprises 12,000 sq. ft., but is dwarfed by the rest of the vessel.

Many of the instruments that we have aboard the vessel are prototype instruments and the first time used at sea. Some are just the first time ever used anywhere.

We have a cryogenic magnetometer, state-of-the-art microscopes with TV viewing capabilities for paleontology and for petrology, a scanning electron microscope capable of magnifying up to 20,000X, a state-of-the-art chemistry and gas laboratory, X-ray fluorescence, X-ray diffraction, and state-of-the-art laboratories. We also have geophysical laboratories for towing seismic gear and magnetometers when the ship is in transit between sites and we have down-hole measurements and logging laboratories.

During coring operations, the 9 1/2 m cores are sectioned into 1 1/2 m sections. A lot of bookkeeping is done. We computerized all the bookkeeping and they are put on screens all over the vessel. The cores are sectioned in halves, with an archive half and a working half.

We do not have very good direct view of the rig floor. On the screens we monitor all our bookkeeping, age of cores, etc. We also have capabilities of viewing the rig floor. We have a TV camera on the rig floor and we can watch it on the screen anytime we desire.

We have about one-half dozen microscopes in the sediment laboratory for looking at smear sections of selected materials. They are state-of-the-art microscopes, top of the line microscopes. We have capabilities for group viewing. Rather than having one scientist look through a microscope, we can watch on a TV screen and look at the slides on the TV screen. We have an automated thin section lab, Logitech thin section lab; we can produce a lot of thin sections rather rapidly.

The physical properties equipment that we have includes instruments to measure the velocity of sound in sediments and strength of the sediments. We also have half a dozen paleontological laboratories.

Many types of cores and drill presses are available to cut the rocks. We photograph all of the cores that come aboard the vessel both in black and white and color. These photographs are made accessible to all of the scientists aboard the vessel.

The X-ray diffraction and X-ray fluorescence units are state-of-the-art. These are the only ones I know of that are aboard research vessels at the present time.

In the chemistry laboratory we have gas chromatographs for monitoring hydrocarbons. It is very important for safety purposes to measure hydrocarbons since we are drilling without blow-out preventers and risers.

Presses, capable of exerting about 50,000 lbs. of pressure on the sediment samples enable us to look at the chemistry of the interstitial waters. Computer facilities include two VAX 750 that serve as a central data librarian aboard the vessel and about 50 pro-3 50 microcomputers aboard the ship enabling scientists to do their science in real time.

We have lots of other computers that are dedicated to specific machines as well. We are in isolated areas for extended periods of time, so we have an extensive electronics shop to enable us to monitor and fix all of our equipment.

We have a second look laboratory, which is a small laboratory adjacent to our refrigerated core storage area. It is used to take a second look, at times, of the cores. We store all the cores in refrigerated core storage.

The previous program of scientific ocean drilling certainly demonstrated that the earth is very dynamic, the continents are in motion, and demonstrated the viability of plate tectonics as a model for the evolution of the earth. What we were interested in this program and the ocean drilling program over the next 10 or 15 years was to look at the processes that are driving plate tectonics. In particular, some of the areas of investigation are very much similar to that described by Bill Hay for the Ocean Margin Drilling Program, looking at active margins and at the accretion, subduction of sediments, formation of backarc basins; also in looking at passive margin evolution, the continent-ocean cross boundary, subsidence history of continents, intraoceanic crustal problems, the nature of oceanic crust, hydrothermal processes, and a relatively new field of paleoceanography that only came into existence with the Deep Sea Drilling Program. With the advent of hydraulic piston coring we can look at paleo-currents, temperature regimes, and climatic variations.

In seismic lines off the eastern United States, we see what is called a "reef complex". To the best of my knowledge, the reef complex is an artifact of our seismic techniques. We do not know if, indeed, this is a reef, a reef capping carbonate bank or if, indeed, this is an igneous plug. Certainly, answers to questions like this are important to our understanding of the evolution of passive continental margins.

From our seismic measurements, it appears that at passive margins oceanic crust smoothly abuts continental crust. However, seismic only shows us reflectors and not what the composition of the rocks is. From these data, we do not know where the ocean/continent boundary is and we cannot say with any degree of certainty whether it is near to or as far out as a couple of hundred km from the shelf edge. We just don't know.

Seismic techniques suggest very strongly that the continental crust has been subsiding. For example, off eastern U.S., the continental crust has subsided on the order of 10-15 km. That is a lot of subsidence of crust material and what we're trying to look at is "why?"

If we learn the solution to these problems we will know something more about the why's of continental separation, but the solutions are also important when we think in terms of our fossil fuel and mineral resource evaluation. So the solutions are not only important for academic purposes and for learning the processes of why continents begin to separate, but certainly will allow us to know something more about mineral and fossil fuel resource evaluation.

At convergent margins, those with subduction going on, we know from our earthquake studies that with a nice smooth plane of material descending, old oceanic material is being subducted and consumed. There are sediments on top of the oceanic plate, and what we do not know is to what degree the sediments are scraped up and accreted onto the continental rise and to what degree the sediments are trapped in graben type structures and subducted below the island arcs.

Again, solutions to problems like these are very important if we want to understand continental growth. It's also very important if we want to evaluate our fossil fuel and mineral resource potential.

There is also a question as to the petrology of ocean crust. There are numerous models based on what we believe the ocean crust to be made of based on seismic techniques, what we believe to be abducted oceanic crust that we see in ophiolite zones around the world, and what we believe to be oceanic crust from the windows we have of the ocean crust through fracture zones. However, we do not have a single sample of the deep part of the oceanic crust. In fact, the upper part of the hard rock crust we know has an average seismic velocity of about 5 kps. We know it has a thickness of between 1 and 2 km — it's called Layer 2; there are numerous other terminologies: Layer 2, a, b, and c. Layer 3 is a layer that has an average thickness of about 5 km, has an average compressional wave velocity of about 6.6 - 6.7 km per second, and that overlies mantle rock with an average velocity of about 8.1 km per second. We know very little of the composition of the ocean crust below this level.

Right now we are drilling a hole today, a hole off the Galapagos; we are deepening a previously drilled DSDP hole about 1,100 m into the oceanic crust. We have deepened the hole over the last week or two by about another 150 m, 160 m and we are still working on it. But we have not even entered Layer 3, much less drilled a Mohole.

If ever we are to understand the processes that are driving plate tectonics, we are going to have to do a lot better than this and we believe we will.

We are presently on Leg 111, having completed ten cruises and the shakedown cruise. We have addressed some of the passive margin types of problems on Leg 103 off the Galatia Bank, and Leg 104 on the Norwegian Continental Margin on the Varnon Plateau. We have addressed some of the active type continental margin problems on Leg 110 off Barbados. We will address some more bordering the Trench on Leg 112.

We have addressed ocean paleoenvironmental problems on numerous legs including Leg 104, and Leg 105. In another DSDP hole that was drilled into old ocean crust (108 million-year old crust) we logged and did a number of innovative geophysical experiments. We lowered seismometers into the hole as the research vessel circled the hole to test for anisotropy.

We also tested a number of ocean crustal objectives on Cruises 106 and 109 where for the first time we entered the region which was devoid of sediment cover and spudded into hard rock and drilled core. Barry Harding will discuss that in a little while.

The first two years of drilling is going to end in the Waddell Sea with Leg 113 off Antarctica. On Leg 114 we will be drilling in the southern South Atlantic; and at that point, we are going to commence an ambitious Indian Ocean program where we will be drilling in the southwest Indian ridge. If we get the proper permission, I don't know whether we will or not, we'll drill in the Red Sea to look at the very young evolving ocean crust. Again we will spend about 18 months in the Indian Ocean before going into the Pacific.

James D. Baker, Jr.

What Phil didn't discuss was the fact that when the program was moved to Texas A&M, they had an enormous responsibility to spin up a program from a fairly small group and to take a ship which was a commercial drilling ship and turn it into a scientific drilling ship and they did that, Phil and his team; not only did they turn the ship from its commercial operations into a scientific ship, but they did it on schedule and within the budget and we were operating within a few days of the original schedule for the program that was laid out when the contract was first put together. I am not sure that the foreign partners who were involved in the program thought it could be done. We knew it could be done. They did it and it was a fantastic achievement. I think one of the things that points to that is the fact that the laboratories on board the ship are in many cases much better than any such laboratories on land and we've had requests from scientists to go out on the drill ship; they are not interested in drilling or using drilling samples, they want to take their own samples out, spend a couple of months out on the drill ship just so that they can have access to the lab facilities. I think that is a real tribute to the quality of the operation that we have.

One of the persons who has been responsible for that is Barry Harding, the Chief Engineer for the program, and he is going to tell us a little bit about engineering with ODP.

OCEAN DRILLING PROJECT
Speaker: Barry Harding

I want to share with you today some of the technological improvements and advances that we've made at ODP since the project relocated to Texas A&M.

We're expanding on the framework that was started at DSDP. DSDP made a lot of advancements and Mel told you about quite a few of them: reentry capabilities, hydraulic piston cores etc.; so I'm going to take off from there and talk about some of the engineering advancements that we've had the opportunity to make.

Engineering and technology has had a much increased emphasis in the scientific community as a result of COSOD I in 1981. The scientists more clearly defined what it was they were looking for and that was at the tail end of DSDP. With those goals in mind, the engineering and operational technologies required were more clearly focused.

As you have already been told today, Arch McLerran is responsible for recruiting five key members from the DSDP staff and getting them to relocate to College Station. We had five key DSDP members and we filled in around those five with seven industry professionals and we have a total engineering and operation staff of about 16 people right now. We are hoping to add a couple of people in the next six months.

The member countries are interested in getting involved. They want to loan us engineers on a temporary basis for 12 to 18 months, to effect the exchange of technology. They pay the salary of the individual and we provide a living allowance for them. We have already done this once with France and one month ago we got a new German engineer and, hopefully, we will be getting a Canadian and British engineer, maybe next year. So all of the member countries want to help us out and also want to bring some of the technology back to their country.

As you have already been told, the conversion of the SEDCO version, BP471, was the high priority item in 1984. That was the "must". It was to get the ship into the shipyard, get it converted, add laboratories, add the new drilling equipment, strengthen the derrick, order all of the consumable items that were needed, and order multiple sets of coring equipment that we must have on board the vessel. It was an enormously busy year from about June all the way through the end of the year up to sea trials.

The large engineering project, the first large engineering project that we were asked to tackle, was drilling on the mid-Atlantic ridge where there was no sediment cover. Besides the ODP staff, we let two sub-contracts to two outside firms to help us accomplish this. One was for the hardware and the system that we must use to initiate or start a hole on the mid-Atlantic ridge. The second one was for the drilling systems and the actual penetration, the bits, the muds, and the special drilling fluids that were needed.

The first contract for the hardware and the guidebase system was let to SEDCO. SEDCO's in-house engineering team designed the hardrock guidebase, worked with our staff as far as what would be required and did an excellent job.

The second drilling contract was let to a firm out of Oklahoma City named Southern International. Southern International has done quite extensive hardrock drilling; as a matter of fact, they are now engaged on a project in Sweden. They are doing the crater drilling up in Sweden (in the Siljian Ring) to try and prove or disprove the theories concerning the origin of abiogenic methane right now.

Right in the middle of the Atlantic Ocean, almost due east of Bermuda, is Leg 106. That is where the hardrock guidebase system was deployed on site 648B on the rim of an underwater volcano.

We have since revisited the hole one time during Leg 109. We did not get as much hole drilled on those two legs as hoped, but there were several reasons why not. One of the reasons, we had only a limited number of drilling days and drilling supplies. As you know, our legs are divided into 60-day cruises and we didn't get all 60 days to drill on site 648B. Part of the problem was the scientists get "antsy", the more bits they see wear out. They always go with a contingency plan just in case of a catastrophic loss and the scientists did want to get some science. They didn't want to allocate the entire Leg for technology, engineering advancements, and drilling advancements and so we pulled off first time and that's when the black smokers were discovered in the mid-Atlantic.

Figure 56 is a schematic of the guidebase system as designed for the mid-Atlantic ridge. It was designed to be deployed on a maximum 20^{o} slope on the rubbleized basalt formation. The first time out, we decided that if we could set the base on a level surface it would be easier, and so we set it on top of a volcano. We set it on the crater rim of a volcano and looking back we probably couldn't have chosen a worse place to set it in terms of parameters to drill through. We have not yet even reached the seafloor. We have not penetrated the entire crater rim of the volcano. We are just approaching that after two drilling legs.

The base was 17 feet square and 7 feet tall and the legs extended the total height to 11 feet. After setting the base, we filled it with cement to firmly anchor it. We had cement bags that would unfold and fall down under the base to take contour of the seafloor system of the rocks and of the basalts. Once cemented in place, we began initiating a hole. We started drilling an 18 1/2" hole.

Looking back now, that was a big mistake. We were trying to set a 16" string casing. We were displacing too much rock, and were creating more problems in the rock by the drilling. We have since learned that we need to start with smaller hole sizes, set smaller strings of casing to drill this rock. Once we got into drilling with 9 7/8 bits, the rock drilled quicker. The drilling disturbed it less, and fewer pieces of the rock fell into the hole, hence better penetration rates.

Once the guidebase was set we could reenter a hole using either sonar or an underwater TV camera. The camera rides on an external frame down the drill pipe and actually, when we were doing our initial reentry, we used the TV system more than the sonar because using the sonar it was hard to distinguish the base from the surrounding basalt pillows. We could not tell the base just on sonar and so the real-time TV worked extremely well.

In talking with the office this morning, we did a reentry in hole 504B and we scanned 40 seconds. We made reentry in 11,000 feet of water. We looked around 40 seconds and stabbed the pipe. So that's pretty quick.

For the "threading the needle" analogy mentioned by Dillard Hammett (Figure 52), that's a pretty quick reentry. We've been able to do the reentries fairly routinely. The sonar is also working extremely well. On a normal reentry cone situation, with flat bottoms, the sonar's been working extremely well also.

To facilitate reentry, we have an external TV frame that runs down the drill pipe; the camera sits back from the bit a short distance. We have two 250-watt halogen bulbs for illumination and have gotten some extremely good underwater videos. Everybody in the lab stack can watch the reentries real time, but we've also recorded them on video tape so we can bring them back and we can show others. We can show the scientific community what is out there. We have good pictures and footage of the black smokers on the mid-Atlantic ridge.

The guide shoe on our rig, which was one of the developments for Mohole, is a clamshell-type device. We unclamp right at the moon pool doors, and are able to run our reentry cones and our hardrock base through the moon pool of the ship.

To initiate the spudding of the hole on Leg 106, we used a Norton Christensen drilling motor to begin with and then we changed to a coring motor that had a hollow rotor so we could pull a core barrel through it. One of the projects that we want to develop further is the use of coring motors in our coring techniques, so we don't have to turn the whole drill string from the top and we can develop more downhole torque for drilling.

During Leg 109, we were actually wearing the bits out on the side. What we did is on the leading edge, we built up with some hard facing on the leading edge of each leg, and then we put some tungsten carbide buttons in on the shirttail so we could build a rougher bit for hardrock drilling. We were continuing to re-core and re-recore material that was sloughing into the hole. On Leg 109, we took a cementing specialist out so that we could cement very rapidly down the drillpipe and squeeze the cement in the formation to more or less create a cement wall cake and then drill that back out so we could stabilize the sides of the well bore.

On Leg 109, we effected another first of a kind, reverse reentry. While drilling, we did not have much of the bottom hole assembly in the hole and we had a failure of a mechanical jar. The jar was just above the secondary reentry cone. The 16" casing was tied to the reentry cone. While rotating the drill string, the jar failed. Sticking up out of the hole was the mandril of the mechanical jar. While retrieving the drill string out of the hole, the crew on board was just about to give up. That was the second fishing job on this expedition, by the way.

The first fishing job occurred when a hydraulic jar failed down in the well bore, and it wasn't too tough to fish that out because it was confined. We went in with an overshot and latched on to that thing and pulled it out. When we found out that we had the second fishing job facing us, that was kind of a monumental task. We ran the TV camera down to the seafloor to see what we had. And what they actually did was take a small funnel, a skirt, if you will. They welded it on a piece of casing, cut some holes inside the casing for slots because the mandril had a couple of "dogs" or "ears" sticking out on it and so with use of the TV system, they were able to go down, and I believe it was on their third attempt they got over the top of the jar and were able to pull it out. In his morning telex, the operations superintendent, signed it "A born-again fisherman". Hopefully, we'll never have to try it again.

We left hole 648B after Leg 109 with two strings of casing set, one shoe at 3350 m, the 16"; and the other (10 3/4") at 3369 m. The seabed is at 3341 m. We have drilled some 12 1/4" open hole below that, and we have gotten a total depth of about 50 m in that hole right now. The hole is open and we are not sure exactly when we will be required to go back to advance hole 648B more.

Let me just share with you some of the other technology improvements of the Ocean Drilling Program. You've already heard about the 30,000 foot drill string. We have drill string made of the same S140 steel. So we have an S140 composite drill string of 5" and 5 1/2" drill pipe and we're capable of running 30,000 feet combined drill string length and/or penetration.

We've got a larger, more stable drilling vessel that can maintain station in much rougher water. To date, we have drilled in 15 to 16 foot seas and 35-40 knot winds and maintained station with no problems. The drill ship has been remarkable in terms of its station-keeping capabilities.

The reentry capabilities I've touched on. We have new reentry capabilities, both in a TV system and in another type of sonar. We have the sonar system that DSDP used, which was made by EDO Western, and we have another type of sonar system made by a company called Mesotech.

We've made some coring improvements to some of the coring systems that we brought over to ODP from DSDP. We've made advancements in the hydraulic bit release which is where we can drop the bit off in the bottom of the hole for logging. We've made some changes in bit designs and we're continuing to do that. We have a couple diamond bits out on the ship and we hope to deploy the diamond bits and PDC bits, hopefully, in hole 504B.

We've made improvements to the XCB system. The XCB system is a continuation after piston-coring reaches refusal, whereby you cannot shoot the piston into the formation or where you cannot pull the piston out because of overpull. We then change the cutting shoe on the bottom of a core barrel. Instead of shoving out with pump pressure, we put a cutting shoe on. The cutting shoe extends below the four cone rotary drill bit by about 8" and then we turn the entire drill string and it's the small cutting shoe that actually cuts the core. We call that the extended core barrel system and it operates on a spring so when we're in soft formation, it pokes its head out of the drill bit and when we're in hard formation, it retracts back inside. We have also made improvements in the flow of water in the XCB new cutting shoe designs and we're working on a lockable flapper for the XCB system. The flapper prevents back-flow up the drill string. We can lock it open so if we are using wire-line tools, the flapper will shut. It is a real small improvement, but it is big thing to the loggers.

Another thing that we have done (I wouldn't say we have perfected it), is we were stuck once on hole 648B on Leg 109 and we dropped the severing charge out of the drill string, out of the end of the bit. We couldn't get loose by normal overpull methods and so we actually set a charge off in the open hole below the bit and we got ourselves loose.

We had a strain on it and it worked. It was either that or sever the drill collar and junk the hole, so we decided we had nothing to lose by dropping the charge out into the open hole and we actually went. We knew we were stuck at that point right above the bit and so we took a strain on the drill pipe, and dropped the charge out of the end of the pipe and freed the drill string.

One other thing I failed to mention about reentry capabilities; we have a minicone situation which we call our free fall reentry cone. On holes where we don't attempt or don't expect to make multiple bit runs, if we get down and let's say the bit fails at 50 feet, one or two cores from where the scientists want to be, we have a mini-reentry cone that's 8 feet in diameter and we make it in two halves.

If the bit wears out or we don't quite get to the scientific objective, we bolt the two halves of the minicone around the drill pipe and let it free-fall to the ocean floor. It embeds itself in the soft sediment. One time it embedded itself so far it was flush with the ocean floor. We also put two glass spheres on it for sonar reflection so we can locate it. That gives us a secondary means of reentry whereby we can get back in a hole and get those last two or three cores for the scientists. We have run it twice and it has been successful and it seems to be something that we will keep in our repertoire of tools.

With regard to some of the future engineering challenges for ODP, we want to continue to work with down-hole motors; we want to continue to develop use of coring motors and use them for unsupported studends; we want to continue to develop and use drilling jars. We want to continue to look for a better, stronger, tougher drilling jar for our application that has a bore that we can pull a core barrel through.

We have a system called a Navidrill, which is a small mud motor; it has a high speed mud motor capability with 3 3/4" diameter and we use it right after extended core barrel, or XCB. The Navidrill is the next step, whereby we could drill harder formation with a downhole mud motor at high RPMs. We have some tests that we will be undergoing in Germany in December at Norton Christiansen and we hope to take this on one of the legs in March or April next year.

One of the things the Navidrill kind of ties to is that we're hoping to adapt some of the mining technology and learn something from the mining industry and marry ODP's type of coring systems together with some of the things from the mining industry. We are looking at some of their technology to see if there is something that they don't do better that we can use.

We are working on a new drill pipe inspection tool Arch told you about. The system that was used by DSDP on the Challenger wouldn't work because the physical restraints on the SEDCO/BP471 or JOIDES "Resolution" and ultrasonics has come a long way in the last 10 years, so we're looking for a new drill pipe inspection tool.

So far, we have inspected pipe in Barbados after nine legs and we have inspected over 500 joints and we only downgraded 42 joints. So that's a pretty good rejection rate and that was based on very localized pitting.

Once the drill string is back in College Station we are going to re-inspect it, with the same inspectors. We will cut about four joints open to take a physical look to see if the pitting is as bad as the equipment told us it was.

Another thing that we're working on is a shipboard capability of drill string analysis. We've got a drill string analysis program for heave motion and we're working it both with different size packages, reentry cones, hardrock bases on the bottom. As far as deployment and different water depths and sea states, we are hoping to make this a shipboard operational program that our operations people can punch in; the software will be loaded on the computer on the rig. We can then actually analyze the potential stresses in the drill string and we will know whether we ought to start a trip or whether we should terminate a trip and start back the other way.

We are working more with the scientific community on downhole packers for downhole testing and fluid sampling; we have a workshop in about ten days on working on a new, improved pressure core barrel. The scientists want a different type of core barrel and so we've got a small group of scientists getting together with our engineers in College Station to work on it. One tool may not do everything needed, so we're going to be looking and hearing what they want to do with the samples, what they want to collect, both hydrates and fluids.

We had a very successful meeting in College Station of a committee called the TEDCOM Committee. The TEDCOM Committee is an industry-manned committee; it's a technology, engineering, and development committee that supports ODP. These guys are supposed to keep us abreast of some things that are going on, maybe things that we don't read about or don't know about. We had a very successful meeting last week, with a total of 25 participants from industry, from the drilling side. It was very beneficial to us and we're hoping to maintain regular contact with members. We've tried to take technology that's already proven and adapt it to our world. That way we don't start from scratch every time. We try to adapt the technology that is available.

D. James Baker, Jr.

One of the things that Barry mentioned is that we are trying to make sure that we have adequate support for the development of new technology in the drilling program. That's an important objective, one that we're working hard towards.

As one of the organizers of this symposium, one of the things that Dan Hunt and Arch McLarren wanted to make sure we represented was the involvement of industry in ocean drilling. There's been a very mutually-beneficial involvement and I think it came home very strongly during the ocean margin drilling program.

We are very fortunate today to have two people who have been involved in the program for a long time: Ed Horton, whose name was mentioned earlier by Bill Bascom as the inventor of the guide horn, and Bill Silcox, who is a member of the OMB science advisory committee. Ed Horton is going to talk first on benefits to industry.

BENEFITS TO INDUSTRY
Speaker: Edward Horton

I'd like to start in on this about benefits to industry, and I'd just like to add in that I think it's a two-way flow and Bill Silcox is going to be discussing this same subject. Before we got here, we made a deal; that is, since Bill has the slides, he's going to talk about the hardware, and I'm going to talk about some of the engineering approaches that we've had over the last 25 years.

I'd like to have you think of this as a time sandwich. A time sandwich is different from a time capsule, in that a time capsule is something you put in the corner of a building and then, after the building falls down, people can open this up and find out what people were thinking about when they should have been thinking how to keep the building up.

This time sandwich that I'm talking about is this, think of it as two slices of bread and we've got 25 years in between. What I'd like to do first, is talk about this first slice of bread which would have been 1960, give or take a couple of years. Then we'll go into the upper slice of bread. Bill Silcox is going to fill that in with all of the hardware that came out of this.

So, since Bill Bascom showed you a little bit about where we were in this early phase of the program, I'd like again to go back to 1959. At that time, I was working with Standard Oil of California — there were five Standard Oil Companies and one Humble Oil Company. If we turn it around where we are today, now we have one Standard Oil Company and we seem to have a lot of humble Oil Companies. That's just a sign of the times.

Anyway, I was working for Standard at that time. As a matter of fact, I was working for Bill Silcox. Through a network, which I guess is a new term now, I'd found out about Bill Bascom and made arrangements to meet him down at an airport. Our first subject of conversation was that he'd like to drill a hole down to the Mohorovicic discontinuity.

My reaction to that was, "What is that?" After a few more discussions, it turned out that this is a hole that was 35,000 feet by sea, and 50,000 feet by land. And shortly thereafter, I left one Bill and went to work for this other Bill. Just like the movie said, we got ourselves a room and Bill said, among other things, "Work on the drill pipe."

I'd like you to think about what we had to work with in the way of analytical tools. At that time, one of the ways of solving these kinds of problems was with wave spectra, but wave spectra at that time was not an engineering tool; it was not accepted by a lot of people, but people were aware of it. There were a number of oceanographers who knew a lot about wave spectra, but as far as applying it in an engineering way and coming up with a solution to a drill pipe problem, it really wasn't in vogue at that time.

We really didn't have good acceptable methods of analyzing ship motion. Manley St. Denis, if you remember, had written a number of papers, but as far as applying it to an industrial problem, it really wasn't done at that time.

Going on a little bit further, we knew how pipe bent and we knew how to solve beam problems and we had lots of equations that would be able to do this. Most of those were closed form, which meant that if you were not very good at handling double integral signs or triple integral signs, it was a very difficult problem to solve. With this in mind we got a great deal of help from the industry, Arthur Labinsky of Standard Oil of Indiana in particular. I remember working with Henry Woods also, who was at Hughes, and a number of other people. In fact, I think that Bill Fisher, who was with Chevron, also helped us out.

It was this kind of an approach and these tools at the time that we used. The problem wasn't that you weren't able to solve these problems, but that you were never really sure of your answers. I'd like to cite one example of the way we were able to solve the problem at that time.

One of the things that really concerned us was the resonance of the drill pipe. You knew that you had energy in the wave band that could excite the drill pipe. The drill pipe's natural period was around 3 to 3 1/2 seconds when it was 12,000 feet long. We would go through these equations to solve them. We could say that if we shifted things a little bit here and a little bit there, one could get this mental picture of running the pipe down and when you got down to maybe 11,000 feet, this thing would suddenly start cycling up and down like a yoyo. You just couldn't imagine anything that could be more devastating to the program.

We got together with some people out at the Naval Civil Engineering Laboratory and built a torsional pendulum that had the same natural period as the drill pipe. We got the Navy to run us out on this YFNB hull (which was the same hull as the CUSS I at the time) and we had the CUSS head into the seas to try to get this torsional pendulum excited, because that would tell us whether we had a problem with the drill pipe or not.

Well, we didn't get it excited. There was nothing in this sort of quick experiment which would give us any indication that we would have a problem. It's that kind of thing that gave us these concerns at that time. Right now, 25 years later, we drill in deep water just as if there was no problem at all. At that time, it was the uncertainty that made the problem interesting to me and put the question marks in it.

The same would be true with some of those tests and the bending of the tapers. We knew how to calculate how tapers would bend and what the behavior of these systems might be if we could model them right, but we really couldn't calculate them well. We could come up with an answer but the answer might take six weeks to develop. In any case, this was the problem in the early 1960s.

Let me turn this thing completely around and get ourselves up to where we are now.

During the last 25 years, we really have found oil in deep water. There is oil in 1,200 feet of water. We're producing oil in 1,000 feet of water today, even water a little bit deeper than that. We have discoveries now in 1,800 feet of water. The way that we are looking at some of those deep water platforms is not too different from the way we were looking at some of the problems that came out of the early days. We are looking at compliant structures. These are structures which actually move with the waves and the methods of solving these problems are almost identical to the ways that we started off 25 years ago.

There's one major difference now, though. This major difference is that we have very, very good computational methods and so, instead of spending six weeks to determine whether something is right, you can actually interact with your computers. What this means, is that we can conceive of a type of structure, we can analyze it and then we can modify it and determine whether this structure is really good. Then we can compare those types of structures with some alternate concepts. The ability to do this, in my view, is one of the major things that's happened in the last 25 years.

There's one additional step which I'd like to point out and that is that when you would come up with a solution 25 years ago, you could not necessarily find many people who would agree with it. Right now, in these methods of designing compliant structures (things that wiggle, things that bend) there are a number of people who will come up with essentially the same answers. So as we evaluate ship motions and changing the shape of vessels, and talk about the way a platform might behave in the deep water we can reach general agreement. There's one thing that does come out of this — at this time we have too much information. Nobody can solve a single problem. We have wave spectra and people will ask you to solve it 10 or 15 different ways. So we end up with a family of solutions from which we can pick. Sometimes that gets a little bit cumbersome, but it's certainly better than the way we were 25 years ago.

BENEFITS TO INDUSTRY

Speaker: William Silcox

The purpose of my presentation will be to show how oil-industry-developed technology benefited deep sea drilling and conversely how deep sea drilling benefited the oil industry. To do this, my presentation will be a historical view starting with the very first floating drilling vessels which went to sea back in 1953. In doing this there will be some duplication with what Dillard Hammett presented in his talk, but this duplication will be minimal. As we go through my slides I will concentrate primarily on the subsea and shipboard systems used in exploratory drilling; however, towards the end I will also show briefly the evolution of subsea completion systems essential for the production of oil from subsea wells.

As a way of introducing myself and my involvement in the Deep Sea Drilling Program, I first became involved in the program just after the request for proposals for the drilling vessel were sent out to drilling contractors. Dr. William Rand was in charge of putting this RFP together. I believe that he dropped out of the program before the proposals were received. I was a member of the Technical Advisory Committee, made up of industry representatives from most of the major oil companies, which assisted the NSF in evaluating these proposals and selecting the contractor, Global Marine. This was in 1967, I believe. At that time I was working for Standard Oil Company of California, now known as Chevron Corporation.

The Technical Advisory Committee met as a group as problems arose. During the start up phase of the program meetings were called several times a year. In addition to these meetings, individual members were contacted to assist in problem solving in their areas of expertise. As construction of the Glomar Challenger progressed and the specialized subsea and shipboard equipment was assembled, there was constant interplay between members of the NSF, Scripps, oil industry engineers, equipment suppliers, and the drilling contractor, Global Marine. I believe that the teamwork which evolved during those early days laid a solid foundation of cooperation which lasted throughout the Deep Sea Drilling Program and in no small way was responsible for the program's success.

During the 1950's and 1960's many of the ideas and methods and much of the equipment, now accepted for offshore drilling without question, were being conceived, developed, and put into service. The center well drilling vessel and the semi-submersible were conceived and made functional to provide more stable floating drilling platforms. The dynamic positioning system, as placed in service on the old CUSS I, was the forerunner of the systems now used routinely on both drill ships and semi-submersibles.

Those were the fun days to be in the offshore business. Ideas were taken from concepts to working tools and equipment sometimes in a matter of just days. The first sonar television reentry system was conceived, designed, built, and used successfully in just one week. A $17,000 drill ship (a lot of money in 1964) standing idle at sea really put the pressure on to get going again.

During the pre-Challenger days when Brown and Root had the contract to develop a drilling platform for Deep Sea Drilling, a very large semi-submersible configuration was selected. This concept and its size was a giant step out in offshore floating drilling technology. The cost reflected by this step out eventually resulted in the NSF falling back to a less ambitious approach, that of working with the oil industry and offshore contractors in advancing the then rapidly evolving deep water drilling technology to meet the needs of the Deep Sea Drilling Program. Improving on existing technology to get started and then continuing to work with the offshore industry to develop new methods and equipment, as the needs arose, proved to be the correct approach. In the end this resulted in one of the most successful scientific programs.

When it became apparent that the Glomar Challenger was becoming weary from its 12 years of deep ocean drilling, a new industry task group was formed to work with the NSF to select a second generation drill ship. The Glomar Explorer, a government owned dynamically positioned vessel fitted with a heavy lift system, seemed the logical choice for the next phase of deep ocean drilling. The NSF, working with its engineering contractor, Santa Fe International Corp., and the industry task group took a close look at the Explorer and it was decided that the cost of converting it to a drill ship and upgrading its positioning system for long term station keeping in open seas would be prohibitive. Again, the approach followed in the Challenger program, that of working with the offshore industry in utilizing and building on existing technology appeared to be the quickest and most cost-effective way to proceed.

In putting together the advanced ocean drilling system, the first approach was to solve all problems at once. The goal was to drill through blow out preventers and drilling risers in 12,000 feet of water. The oil industry had successfully drilled through blow out preventers in 6,800 feet of water, but the problems associated with well control, i.e., blocking of a kick through 12,000 feet of kill line and riser tensioning opened up in whole new areas for technology development which the offshore industry was not prepared to tackle.

The question once again arose within the scientific community — what was going on prior to the advanced ocean drilling program, deep water drilling technology science. It was decided, and properly so, that the first phase of the advanced ocean drilling program would be to continue with the Challenger type of drilling, no blow out preventers, no riser, but with reentry capability. Well control and riser problems could then be solved at a more leisurely pace. With this guidance, a request for proposals was sent out. SEDCO was selected as the new contractor and an existing drill ship was modified for advanced ocean drilling in less than one year.

My involvement in scientific drilling ended in 1985 when I retired from Chevron.

The 18 years I was involved in the scientific drilling programs were quite rewarding and I know I will miss working with the NSF and Texas A&M personnel.

Rather than describe each slide individually, I will briefly recap the overall slide presentation. The slides will now cover the evolution of floating drilling vessels and their support systems.

One of the very first floating drilling vessels, in 1952, was the Myrna Lynn, a wooden Navy mine sweeper. This vessel, bought as surplus, was converted into an exploratory drill ship by mounting an A-frame derrick over the side amidships and installing a double drum, mud pumps, a pine rack, and a gimbal-mounted spider to support the pipe while making connections. The pipe was jetted into the ocean floor and cores were taken by dropping a weighted core barrel through the pipe into the formation at the tip of the pipe. Cores taken were from shallow depth below the ocean floor.

Next came a converted Navy patrol boat. Again with a more conventional derrick with a rotary table mounted over the side. This vessel went to sea in 1953. In calm waters, holes several thousand feet deep could be drilled.

To provide a more stable drilling platform, in 1955 the next generation of drillship had a hole out in its deck down through the bottom of the ship, a Navy LSM. The derrick and rotary table were mounted over this moon pool. The subsea equipment and drill string were lowered through the moon pool and drilling was accomplished through blowout preventers and a marine riser. This configuration is still used on all drill ships in service today.

In 1962, a submersible drill rig, a buoyant structure which was floated to location and then set on bottom while drilling, was converted by adding more buoyancy so that it could drill in the floating mode. The conversion was oalled a semi-submersible, as it could then drill either floating or sitting on bottom. In the floating mode, because of its relatively small water plane, it was very stable in heave and because of its geometry it was stable in pitch and roll.

By 1962 the offshore oil industry had developed the two drilling vessel configurations which have become the worldwide standard for floating drilling.

Mooring systems started out with four point spreads. Additional needs quickly led to eight point or more anchoring systems. In 1961, the NSF contracted with Global Marine to determine if a drill ship could be held on location by thrusters alone. The system worked and was the forerunner of the dynamic positioning systems in use today. In this area of expertise the NSF led the oil industry.

As pointed out previously, a system incorporating both sonar and television was developed in 1964 for reentry into a subsea exploratory hole when all guidelines to the drilling bases were out and the anchors had slipped allowing the drill ship to move off location. One of the engineers who helped develop this system was loaned to Scripps and helped in the development of the reentry system used successfully on the Glomar Challenger.

Again I would like to thank the NSF for inviting me to this meeting. I thoroughly enjoyed my years of association with the Scientific Drilling Programs.

WRAP-UP AND SUMMARY

D. James Baker, Jr.

I'd just like to make a couple of points. One is that one of the purposes of a symposium like this is not only to celebrate an anniversary of a successful demonstration of a new technique, but also to show the intimate link between science and technology. When you look at the advances of science, you have to recognize that the technology is essential, and I think we see that in ocean drilling programs. The second point is that a program like the Ocean Drilling Program cannot succeed unless you have a sympathetic and helpful federal agency to help put it together; that's the role that the NSF has played in this program. The Ocean Drilling Program and its previous incarnation, the Deep Sea Drilling Project, has moved around in the Foundation. It has been everywhere — from a direct office reporting to the director, all the way to reporting to one of the divisions, and yet, at all points the program has had superb program managers. The NSF has provided guidance, management, and oversight over the program and I think that we in the science community with this help have learned how to operate a program. We have ended up with a program that everyone feels a part of. The science community feels that they are an essential part of running the program. This makes for very exciting scientific advisory meetings because everyone feels that they have a piece of the action. I think that is one of the things that makes it very successful. Another piece of this program which has worked — and it is because of the NSF — is the interaction between the Foundation and foreign countries. The foreign country involvement in this program comes from bilateral agreements which are developed by the Science Foundation. It is their tactful dealing with these countries that has allowed the program to survive even such times as when the Soviets first came into the program and deposited their first installment into Guy Stever's personal bank account, who then had to make an arrangement to get that out of there. But in any case, the program is a series of elements all of which work together as a committed community. There are sympathetic and supportive agencies in both the U.S. and outside and there has been new technology that has allowed us to put it together. You have seen all of that today. In that sense, ocean drilling is a model for future programs — future large programs that operate facilities. However, we do face a crisis in the U.S. in terms of funding for science. That is, it costs us more to do new things. And yet the funding that we have had for science over the past decade or so has been basically level funding when you count in inflation. And you can't really move ahead with level funding. You have to have new funding. The country has to do that, not only for national security reasons, but also for the basic health of science and technology. It is important for us to keep pace and to stay involved with the frontiers of science and technology. The recent report from the White House Science Council by David Packard and Allen Bromley points out that we need a major new influx of funding in science and technology in the country — basic science and technology. And one of the things that you can do — there in the audience — is to work on your own congressmen and senators to convince them that it is a good thing to fund basic science and technology in the country. We have seen a successful program that has been funded. Most of the people in this room have been involved in that. Congratulations to all of you and thanks for coming to this symposium.

APPENDIX A
PROGRAM

8:30 a.m. Registration

9:30 a.m. OPENING REMARKS:
Philip M. Smith, Executive Officer, National Research Council

Robert M. White, President, National Academy of Engineering

D. James Baker, Jr., Symposium Chairman and President, Joint Oceanographic Institutions, Inc.

10:30 a.m. PROJECT MOHOLE
Willard Bascom

11:30 a.m. DEEP SEA DRILLING PROJECT
A.R. McLerran & M.N.A. Peterson

12:30 p.m. Luncheon & Viewing of Exhibit

1:30 p.m. OCEAN MARGIN DRILLING
William Hay & Dillard Hammett

2:30 p.m. OCEAN DRILLING PROJECT
Barry Harding & Philip Rabinowitz

3:30 p.m. BENEFITS TO INDUSTRY
Edward Horton & William Silcox

4:30 p.m. WRAP-UP & SUMMARY BY CHAIRMAN
D. James Baker, Jr.

5:00 p.m. Reception

APPENDIX B
LIST OF SPEAKERS

Philip M. Smith, Executive Officer, National Research Council

Robert M. White, President, National Academy of Engineering

D. James Baker, Jr., Symposium Chairman and President, Joint Oceanographic Institutions, Inc.

Willard Bascom, Director of Drilling Project, National Academy of Sciences

A.R. McLerran, retired

M.N.A. Peterson, Chief Scientist, National Oceanic and Atmospheric Administration, Project Manager, Deep Sea Drilling Project

William Hay, Curator, University of Colorado Museum

Dillard Hammett, Hunt Oil Company

Barry Harding, Manager, Engineering and Drilling Operations, Texas A&M University

Philip Rabinowitz, Texas A&M University

Edward Horton, President, Deep Oil Technology, Inc.

William Silcox, Manager of Shore Technology and Planning Staff, Chevron Corporation

APPENDIX C
INVITEES

John B. Anderson
Peter F. Barker
Willard Bascom
Laura Bautz
Alan Berman
Rosina Bierbaum
W.W. Bolton, Jr.
Larry Booda
Brian D. Bornhold
John R. Botzum
LCDR Lawson W. Brigham, USN
William Bruning
Anthony Calio
Edward Cannon
Paul R. Ciesielski
James Cimato
James Curlin
David DeMaster
Richard Denison
Henry J.B. Dick
Charles Drake
James Drewry
Charles N. Ehler
Rober Eisenbud
David H. Elliot
William A. Erb
Wayne Esaias
Larry Flick
Steven Franko
Melvin Friedman
Robert Friedman
Edward A. Frieman
Dieter Fuetterer
John H. Gibbons
David D. Grassick
Gordon Hamilton
Dillard Hammett
John Hannaham
Bruce Hanshaw
Barry Harding
William W. Hay
G. Ross Heath
Edward Horton
Gretchen Hund
Dan Hunt
Jean Jarry
Lionel S. Johns
Peter Johnson
Junzo Kasahara
Raphael G. Kasper
James Kennett
John Kermond
Tom Kitsos
John A. Knauss
Joseph Kravitz
Francois Lampietti
Lucien LeClaire
Howard Levenson
Gordon G. Lill
Andrew Luhtanen
Keith Manchester
Curt Marshall
Claus Marx
Gilbert Maton
Jack I. McLelland
A.R. McLellan
Arthur E. Maxwell
RADM J. Bradford Mooney, USN
Marvin Moss
Robert Niblock
Chad Ohanian
Terry Offield
Grace Ostenso
Ned Ostenso
Vice ADM T.B. Owen, USN (Ret)
Roland D. Paine
Robert Palmer
John S. Perry
Morris Phillips
M.N.A. Peterson
C. Barry Raleigh
Philip D. Rabinowitz
Kip Robinson
Robert W. Rowland
Fred E. Saalfeld
Kurt Sandred
Jeff Savage
RADM J.R. Seesholtz, USN
William Silcox
John Steele
William Stelle
William L. Sullivan
Robert Snyder
Joshua I. Tracy
Stanley Wilson
Patrick Windham
Robert S. Winokur
William Woodward
William Young

APPENDIX D
FIGURES

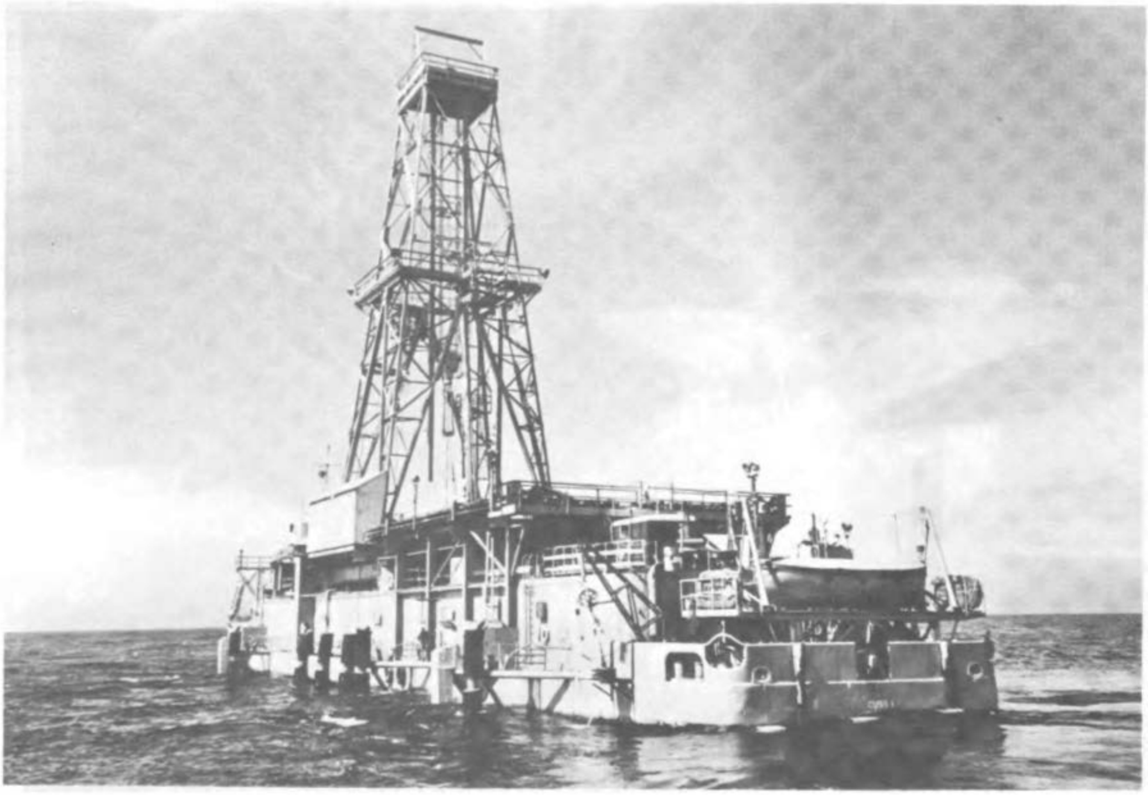


FIGURE 1 The drilling barge CUSS 1, at sea. The barge was equipped with a full-size rig for offshore drilling and outboard motors as part of a dynamic ship positioning system. Credit Fritz Goro, LIFE magazine, April 1961.

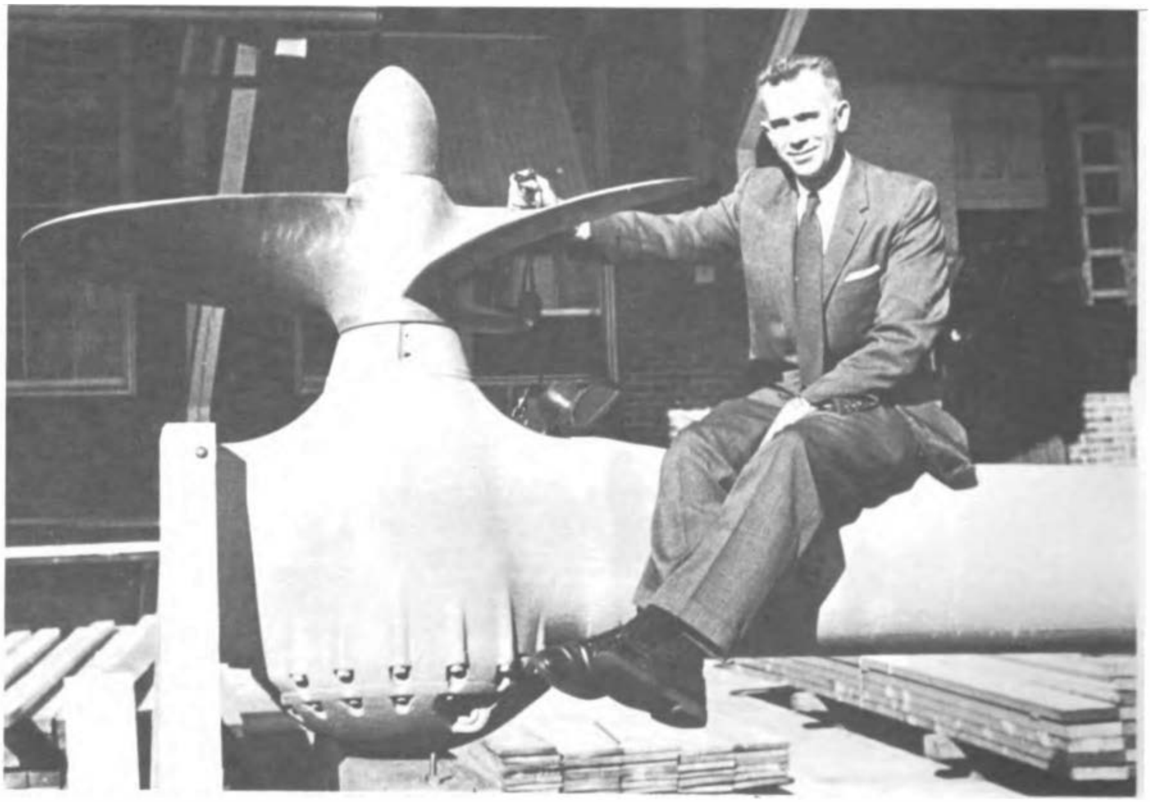


FIGURE 2 Willard Bascom sits on a Harbormaster outboard motor that was mounted on the CUSS 1 as part of its dynamic positioning system. Credit Fritz Goro, LIFE magazine, April 1961.

FIGURE 3 The steering console on CUSS 1. The joystick control was used as part of the dynamic positioning system. Credit Fritz Goro, LIFE magazine, April 1961.

FIGURE 4 Sketch of CUSS 1, illustrating parts of the dynamic positioning system, guide shoe, and drilling system. Diagram by Adolph E. Brotman in The Mohole: preliminary success in drilling through the earth's crust: LIFE magazine, April 7, 1961, pp. 37-40.

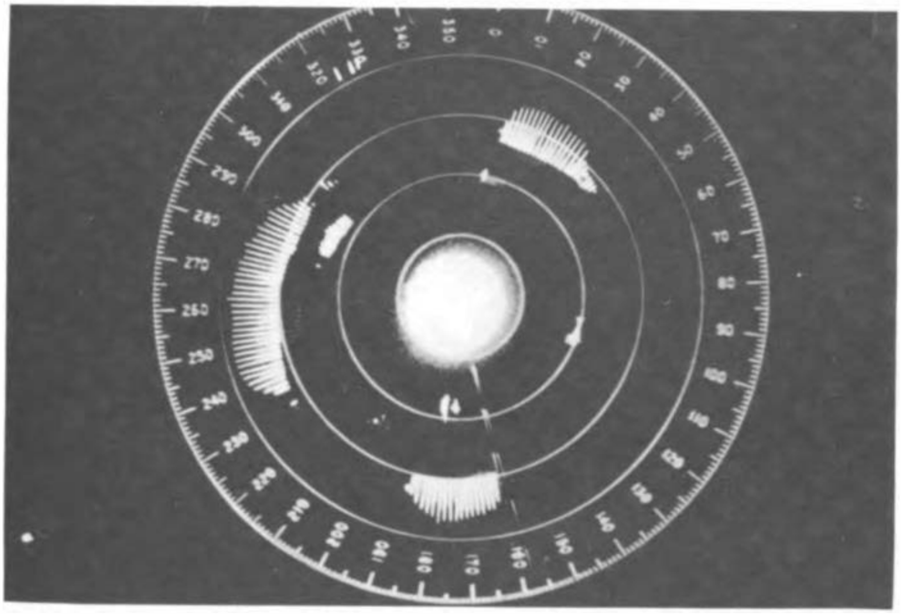


FIGURE 5 Sonar screen for the dynamic positioning system on *CUSS 1*.

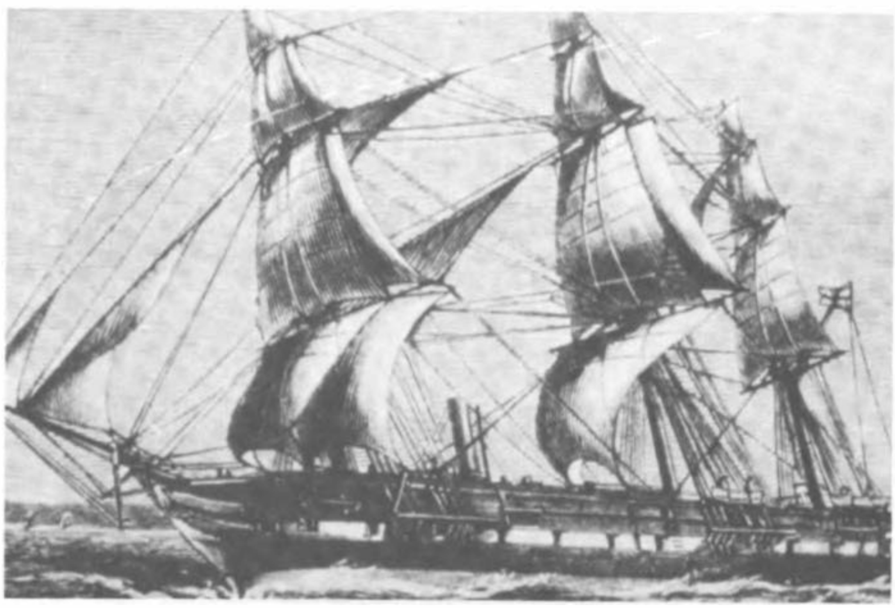


FIGURE 6 *H.M.S. Challenger*.

a

b

FIGURE 7 (a) Launch of the _Glomar Challenger_ hull. Note tunnels near the bow for the automatic dynamic positioning system. (b) _Glomar Challenger_ at sea.

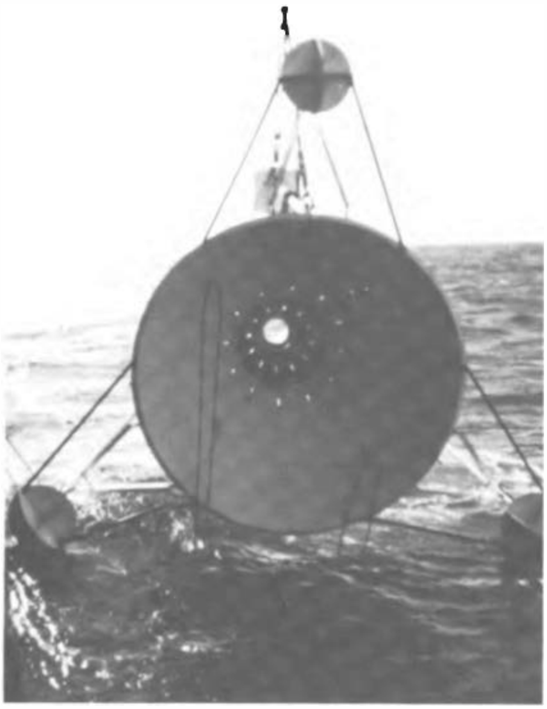


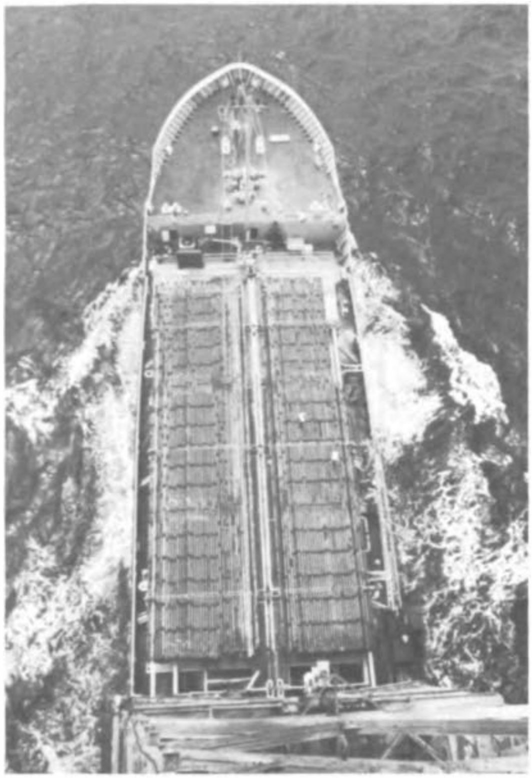
FIGURE 8 Reentry cone. The cone was lowered over the side of *Challenger*, keel hauled, and then set on the sea bottom.

FIGURE 9 A laboratory facility on board *Glomar Challenger*.

FIGURE 10 Part of the satellite positioning system on the Glomar Challenger.

FIGURE 11 Receiving apparatus for the satellite communications system.

a



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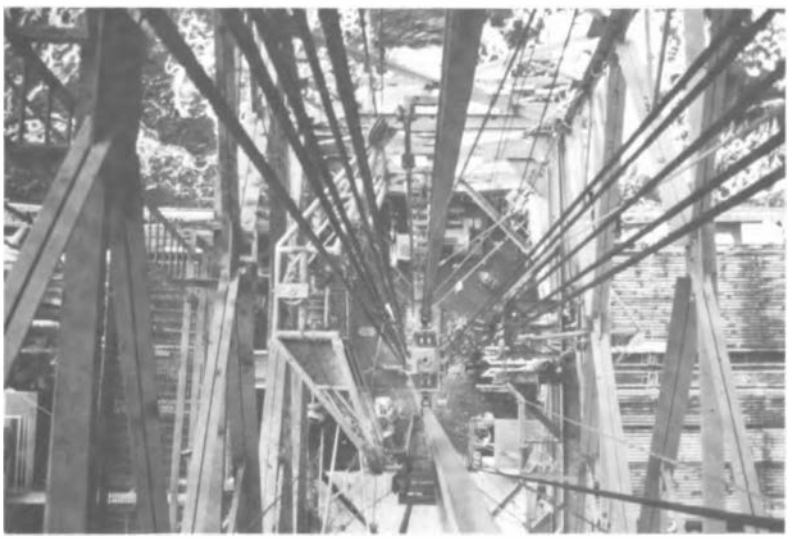

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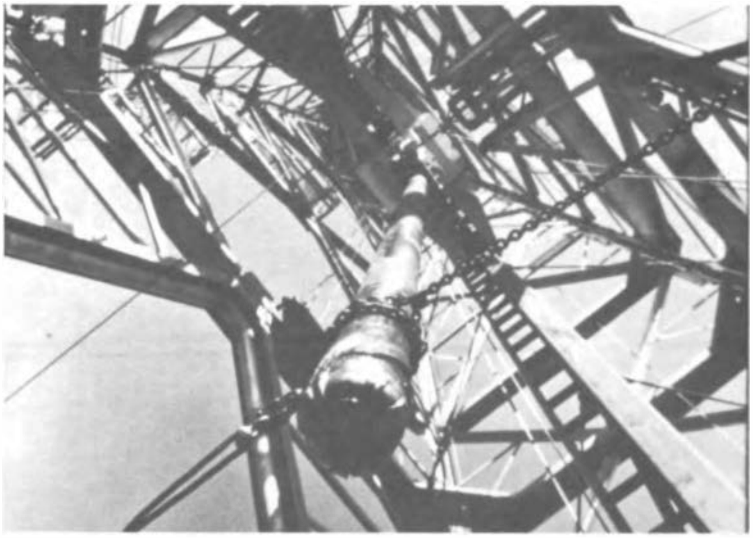

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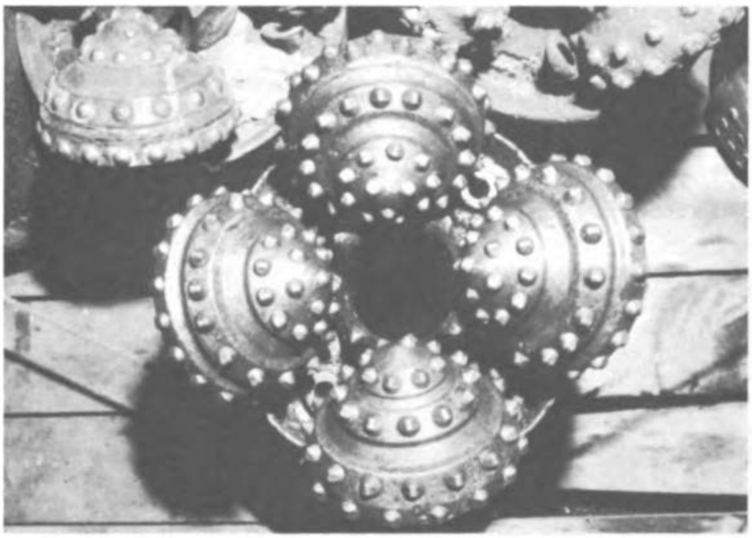


e

FIGURE 12 (a) *Challenger's* drill pipe storage viewed from the top of the drilling derrick. (b) The drill pipe handling system. (c) Drilling operations seen from the top of the derrick. Note the vertical guide bars for stability of the drill string. (d) Drill string being lowered to the moon pool. Note vertical guide bars. (e) Drill bit used for coring.

FIGURE 13 The drill string passing through the moon pool.

FIGURE 14 Hole in the seafloor created by drilling. Photo taken by the ALVIN.

FIGURE 15 The first basement core recovered by the DSDP. The core is being held by Mel Peterson (L) and Terry Edgar (R).

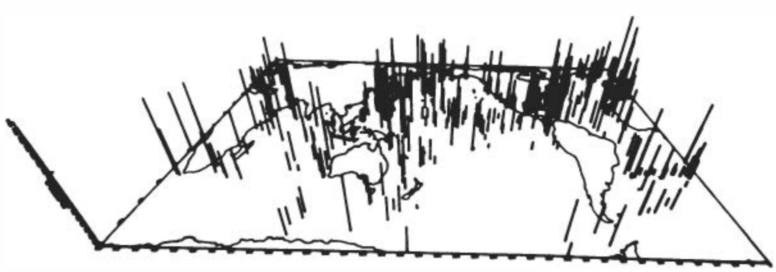


FIGURE 16 Sites drilled by the DSDP. All of the major ocean basins and marginal seas are represented, excepting the Arctic.

FIGURE 17 A stylized track of the Glomar Challenger during the DSDP (September 1969 to November 1983).

FIGURE 18 Core storage facility. The cores are placed in plastic containers and the temperature is maintained at just above freezing.

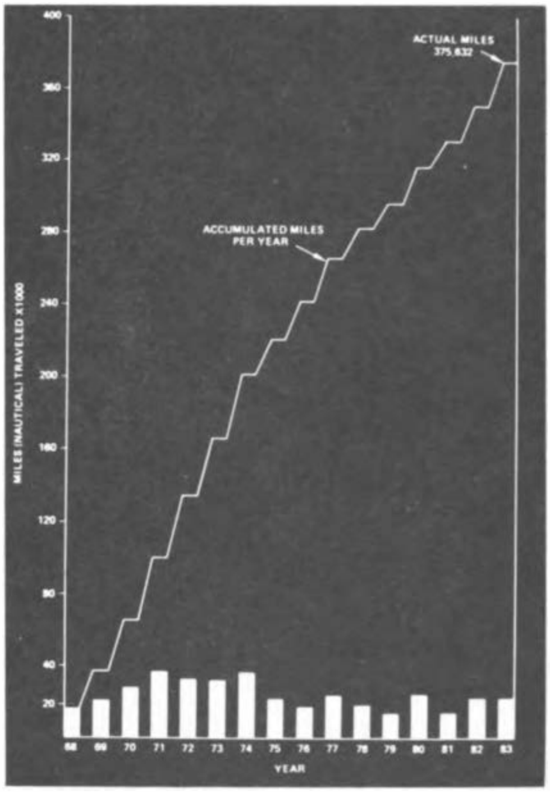


FIGURE 19 Nautical miles traveled versus year for the DSDP.

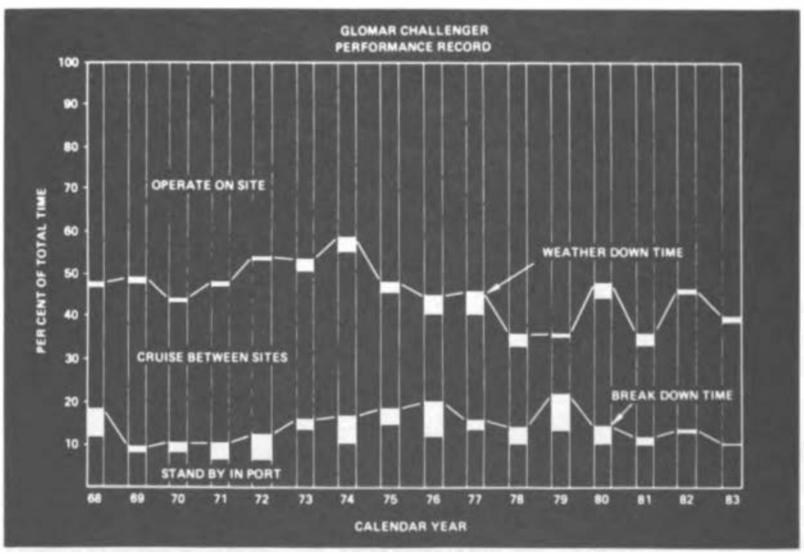


FIGURE 20 Performance record for the Glomar Challenger, showing the time spent on site and in port. In general, the time on site increases towards the latter part of the program.

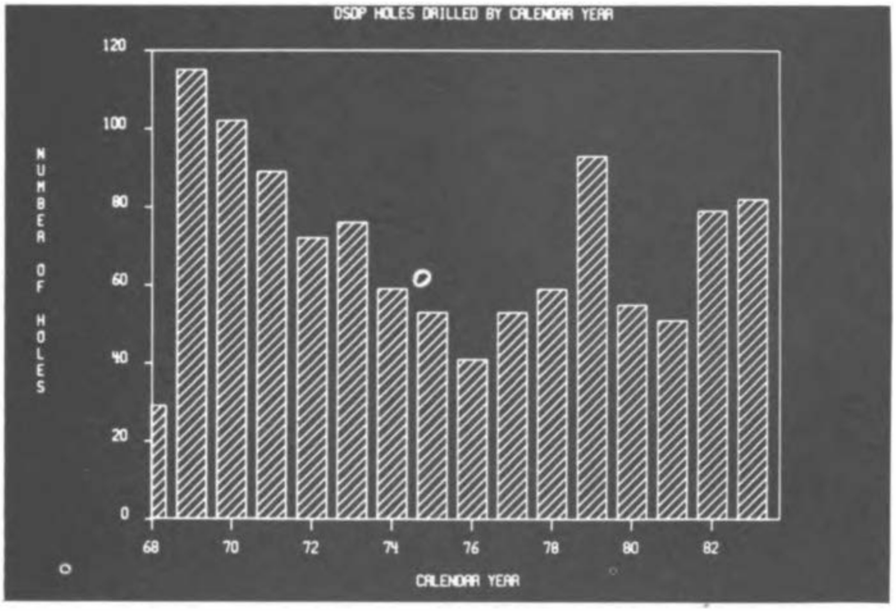


FIGURE 21 Number of holes drilled versus year. This diagram illustrates the emphasis on shorter, more numerous holes as the program matured.

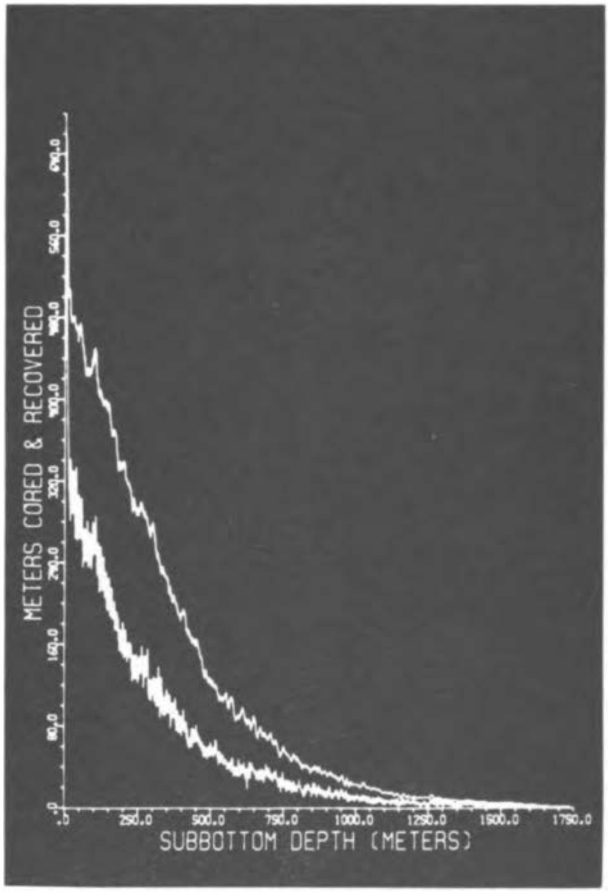


FIGURE 22 Core recovery as a function of subbottom depth. Shallow core recovery is greater partly because the shallow sections were cored at all sites, whereas the deeper materials were not penetrated at many locations.

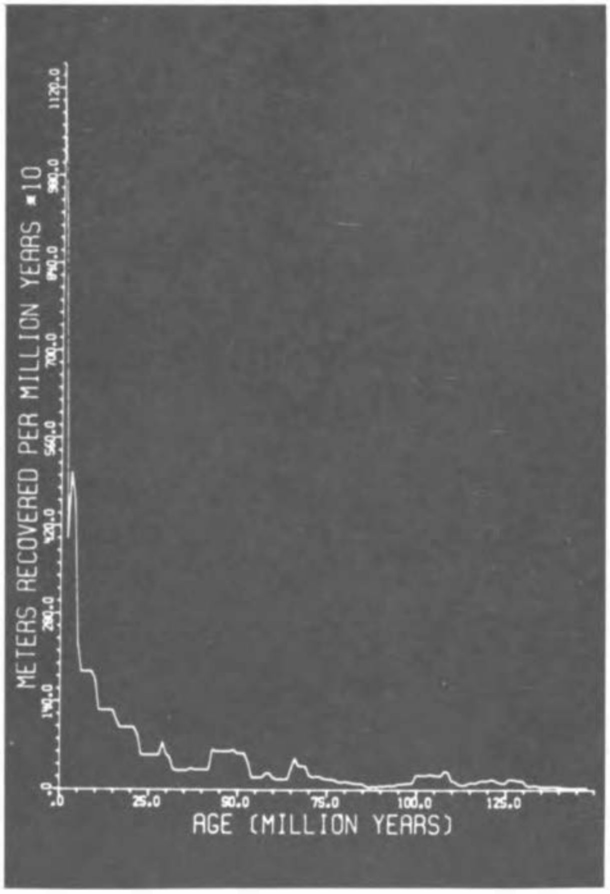
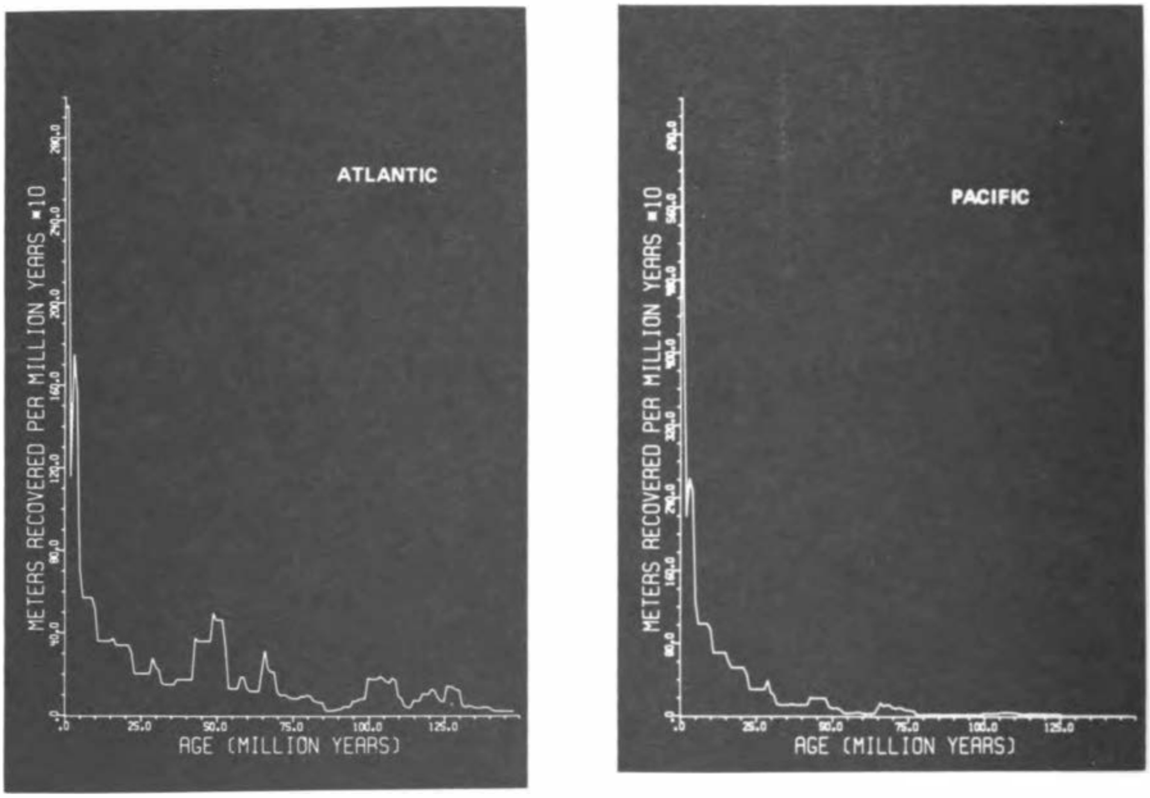
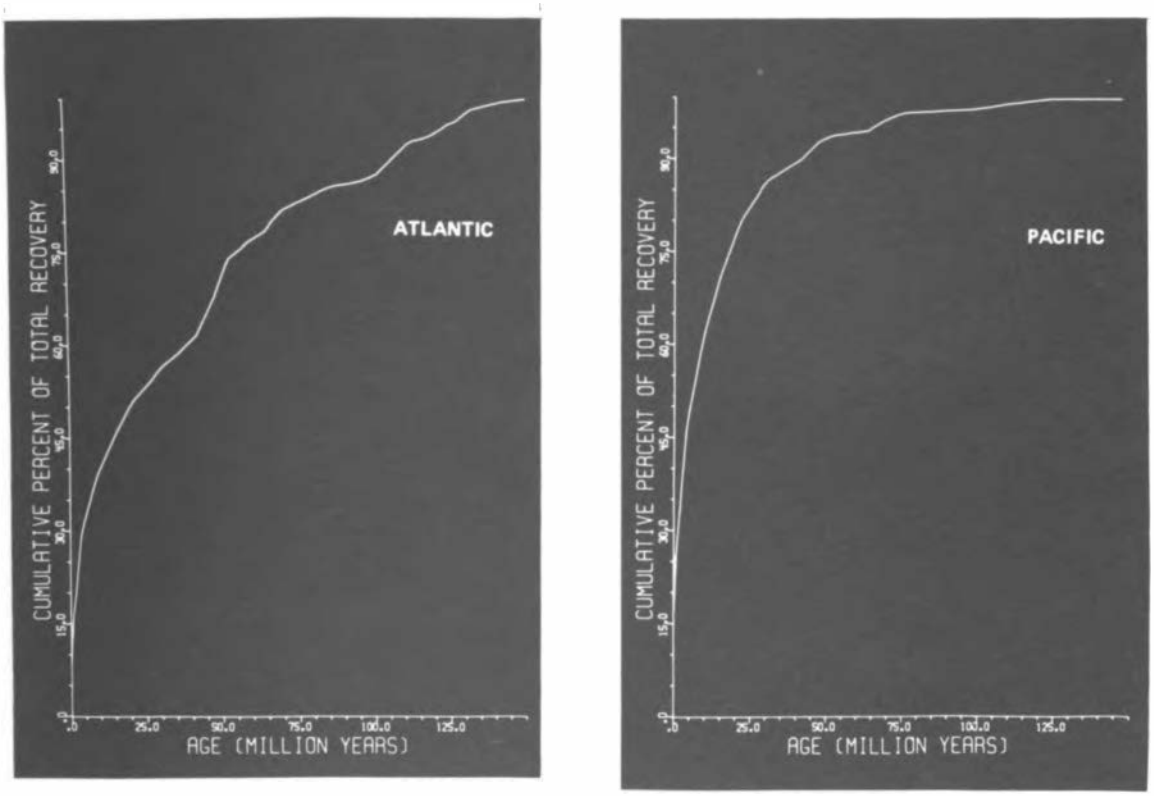


FIGURE 23 Core recovery as a function of age. Again, the dominance of younger sediments is a reflection of their occurrence at the seafloor surface.



a b

FIGURE 24 (a) Core recovery as a function of age for the Atlantic Ocean basin. Variations from a smooth curve are due in part to episodic changes in sedimentation rate and thickness of sediments for the various geologic ages. (b) Core recovery as a function of age for the Pacific Ocean basin. Fluctuations in this curve are attenuated by the sediment traps that surround the basin.



a b

FIGURE 25 (a) Cumulative recovery versus age for the Atlantic Ocean Basin. Note that the recovered sediments are generally older in this basin than in the Pacific Basin. (b) Cumulative recovery versus age for the Pacific Ocean Basin. This basin is younger than the Atlantic basin, resulting in younger cored sediments.

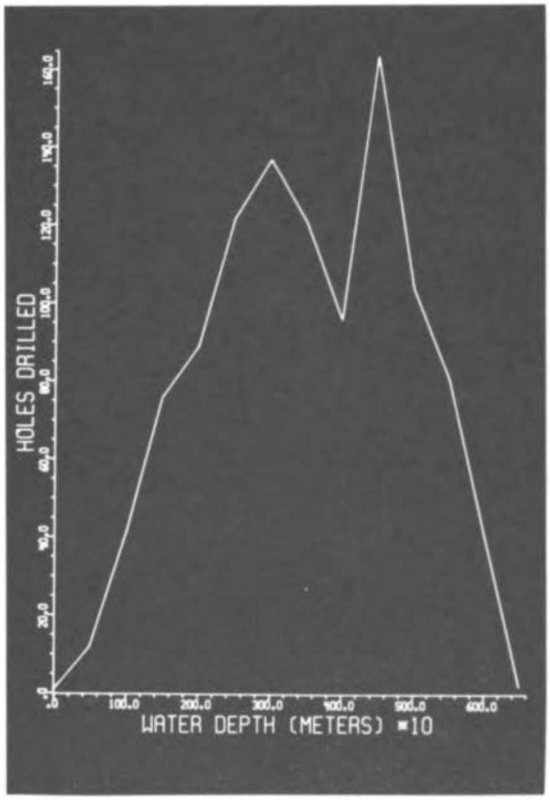


FIGURE 26 Sites drilled as a function of water depth. The shape of the curve is affected by the absence of sediments on the mid-ocean ridges and the 7 km limit on drill string length during the DSDP.

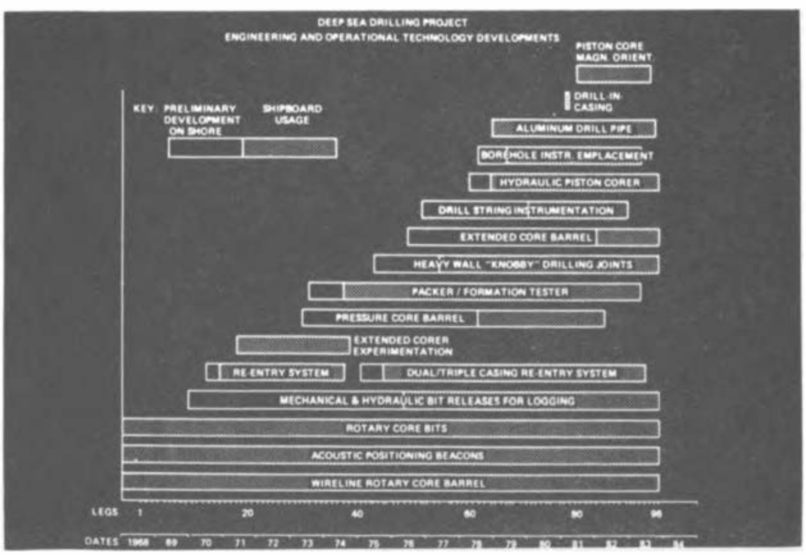


FIGURE 27 Engineering and technology developments of the DSDP.

FIGURE 28 Various drilling and coring bits developed for the DSDP.

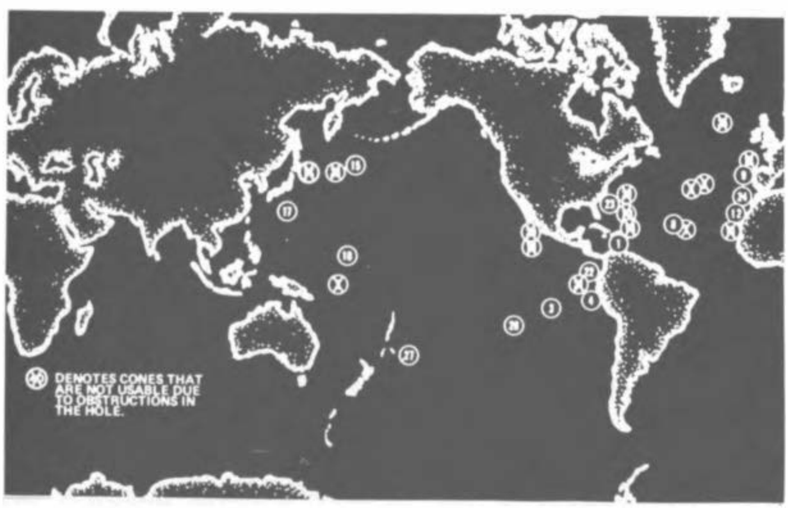


FIGURE 29 Locations of reentry cones set during the DSDP. These holes may be reentered and deepened during a later program. "X" indicates holes that are blocked by various obstructions.

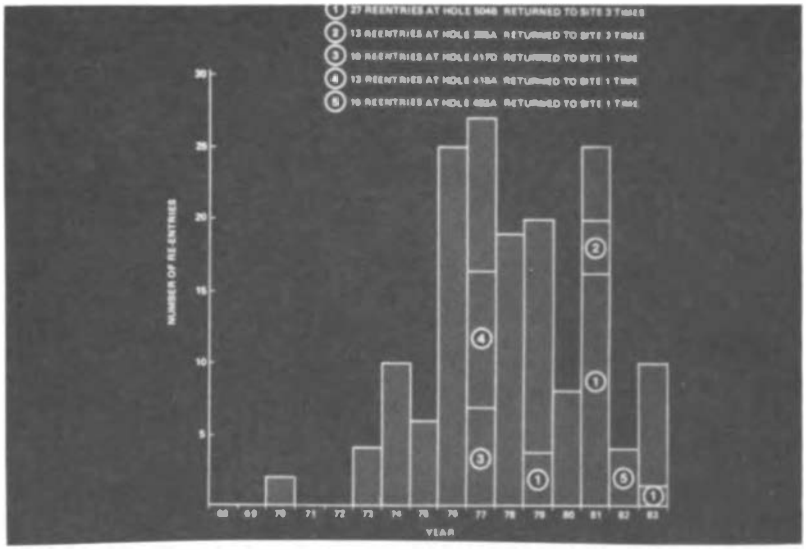


FIGURE 30 Number of hole reentries during the DSDP. By the end of the program, reentering and deepening holes could be done routinely.

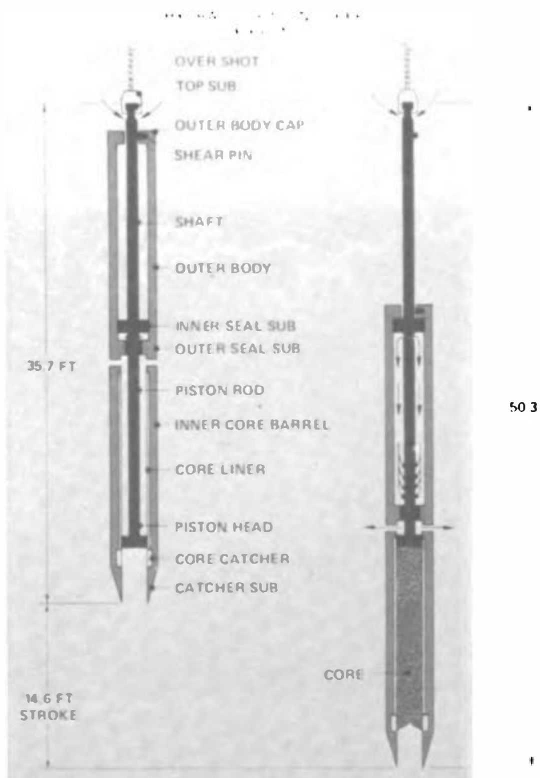
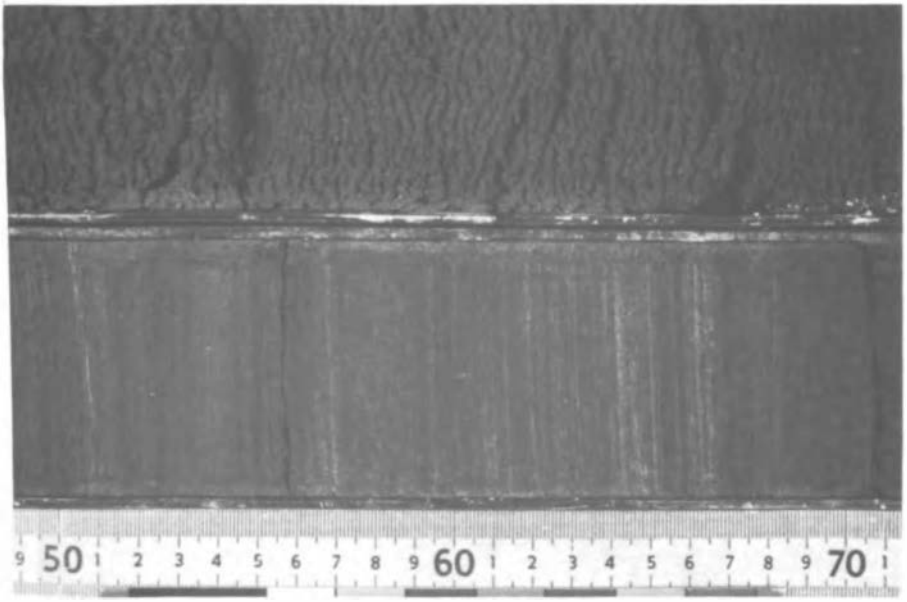


FIGURE 31 The Hydraulic Piston Corer (HPC) system. This coring system far outperformed earlier systems, preserving delicate sedimentary structures.



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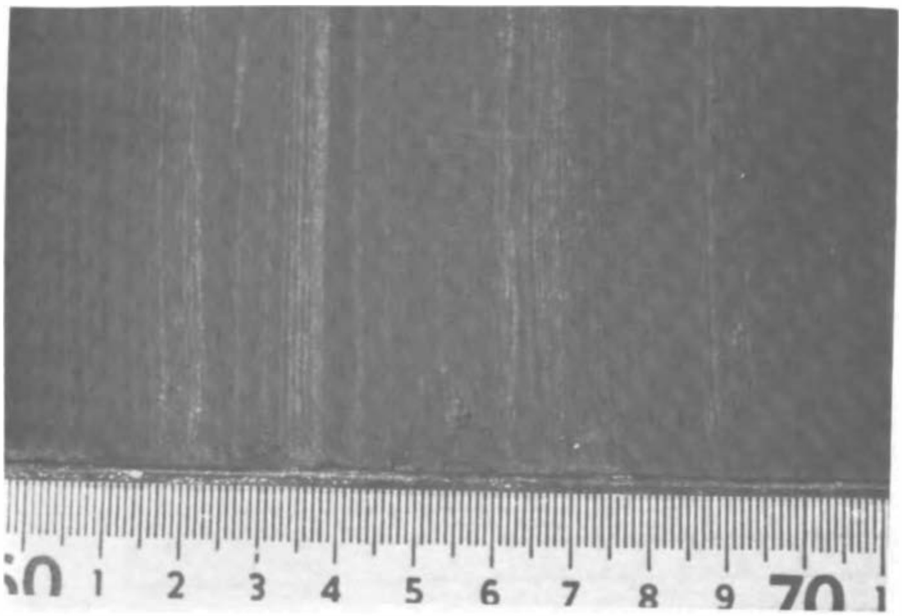


b

FIGURE 32 (a) A comparison of rotary and hydraulic (HPC) coring. The upper core was taken by a rotary corer, which disrupted and obscured the sedimentary structures preserved in the HPC core. (b) Closeup of HPC core shown in Figure 32(a). Note preservation of fine laminations.

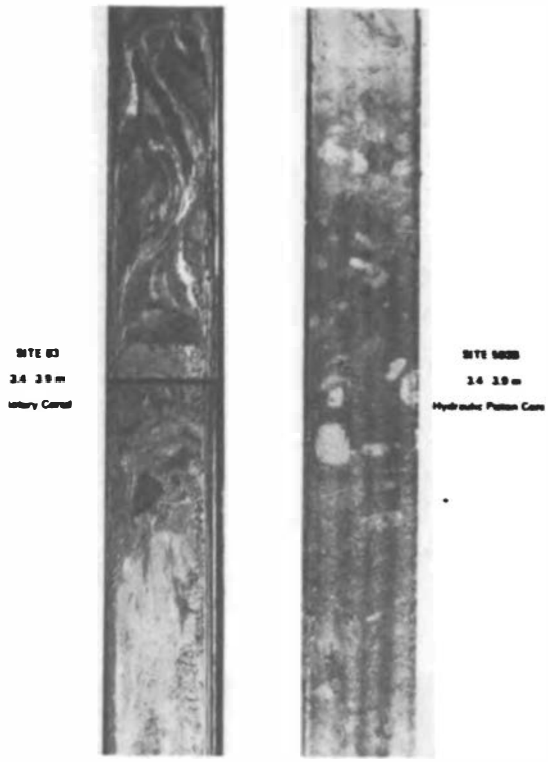


FIGURE 33 A comparison of rotary and hydraulic (HPC) coring. Note that burrows (white) are preserved in HPC and are distorted in the rotary core.

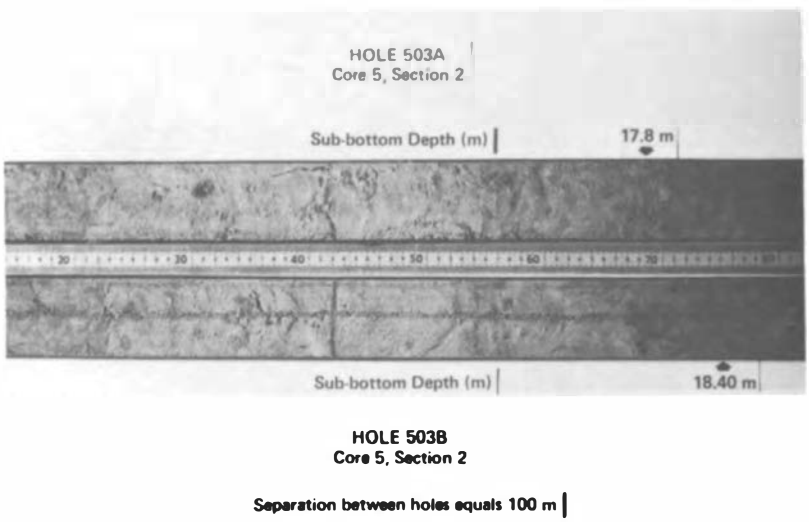


FIGURE 34 Lateral continuity in deep sea cores. The thin (5mm) black layer near middle of core is preserved at similar subbottom depths in cores taken 100 meters apart.

MAJOR DRILL STRING LOSSES 10-19-83

LEG	SITE	LOSS	CAUSE
4	24	480 JT'S + BHA	• ONE ELEVATOR BALE DISENGAGED, SHEARED PIPE AT TOP OF TOOL JOINT
10	87	90 JT'S + BHA	BACK OFF AFTER UNSUCCESSFULL ATTEMPT TO SEVER PIPE
36	326	389 JT'S + BHA	FATIGUE FAILURE AT KEEL DURING STORM
48	400	528 JT'S + BHA	• CORROSION FATIGUE FAILURE OF PINEND ON DRILLING PUP JT
84	567	579 JT'S + BHA	DEFECT IN NEW JT OF DRILL PIPE
93	603	629 JT'S + BHA	• BOX CONNECTION FAILED ON KELLY COCK SUB
	TOTAL	2695 JT'S (83,500 FT)	

• FAILURE OF ABOVE ROTARY TABLE EQUIPMENT

FIGURE 35 Major drill string losses for the DSDP.

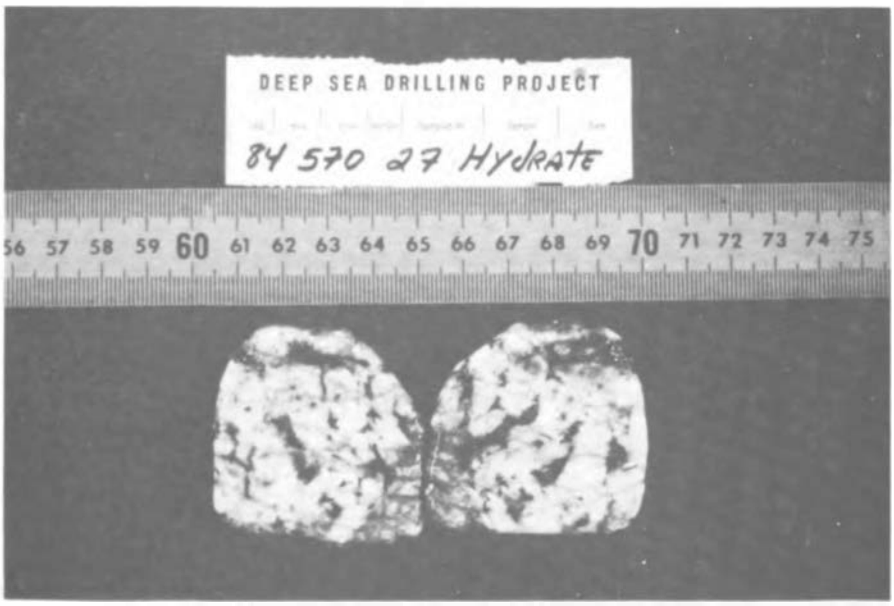


FIGURE 36 Gas hydrates recovered from Site 570 off of Central America.

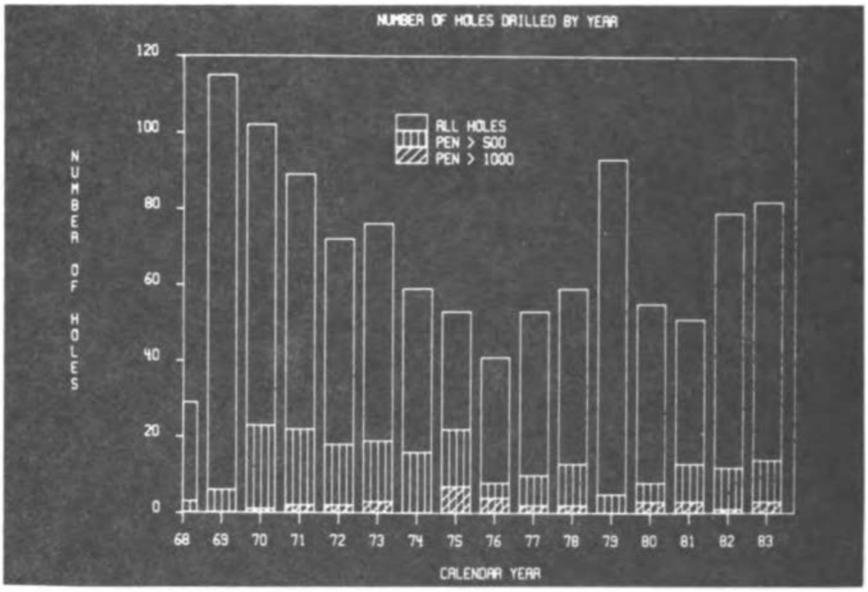


FIGURE 37 Number of holes versus year. Note that the early part of the program was dominated by shallower wells. Pen > 500 = hole penetration greater than 500 meters, Pen > 100 = hole penetration greater than 1,000 meters.

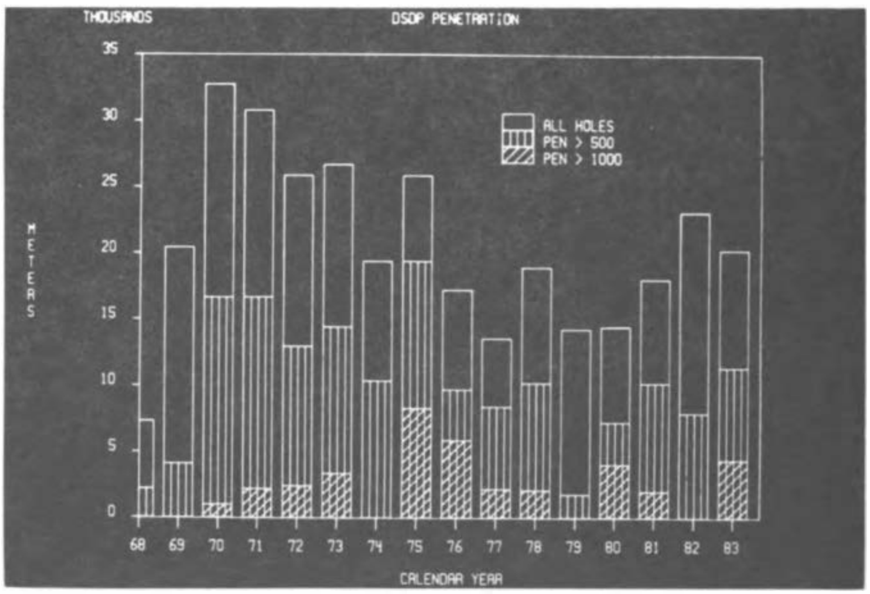
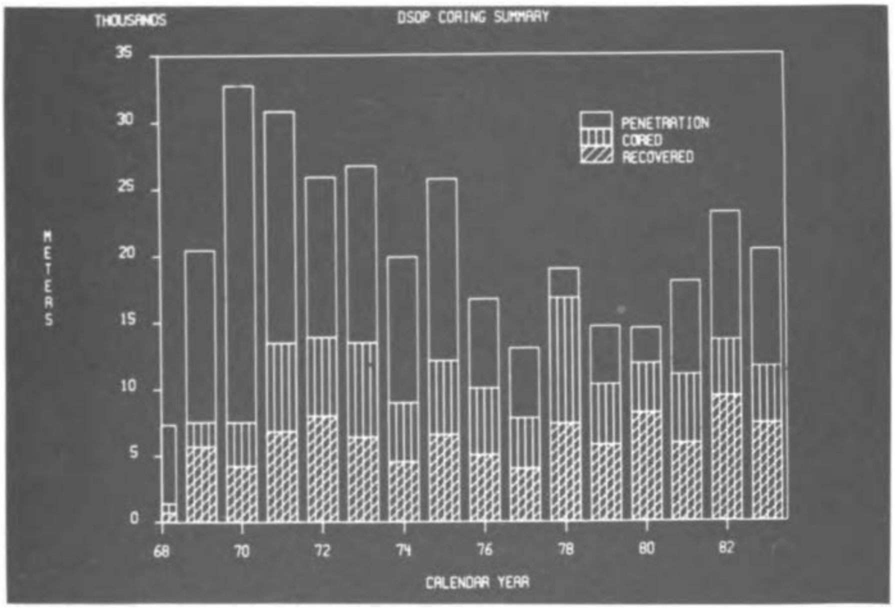
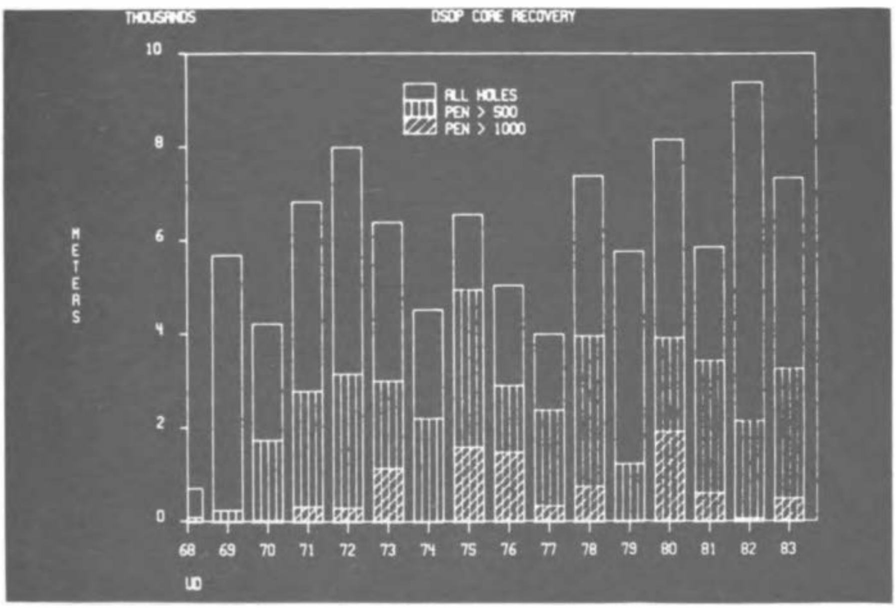


FIGURE 38 Depth of penetration versus year. This graph illustrates that the later part of the program netted less penetration; this was due to the more difficult objectives identified later in the DSDP. Pen > 500 = hole penetration greater than 500 meters, Pen > 100 = hole penetration greater than 1,000 meters.



a



b

FIGURE 39 (a) Core recovered versus cored interval. Coring was increasingly successful as the program progressed. (b) Core recovered versus total depth of hole. Coring of deeper holes was more successful later in the program. Pen > 500 = hole penetration greater than 500 meters, Pen > 100 = hole penetration greater than 1,000 meters.

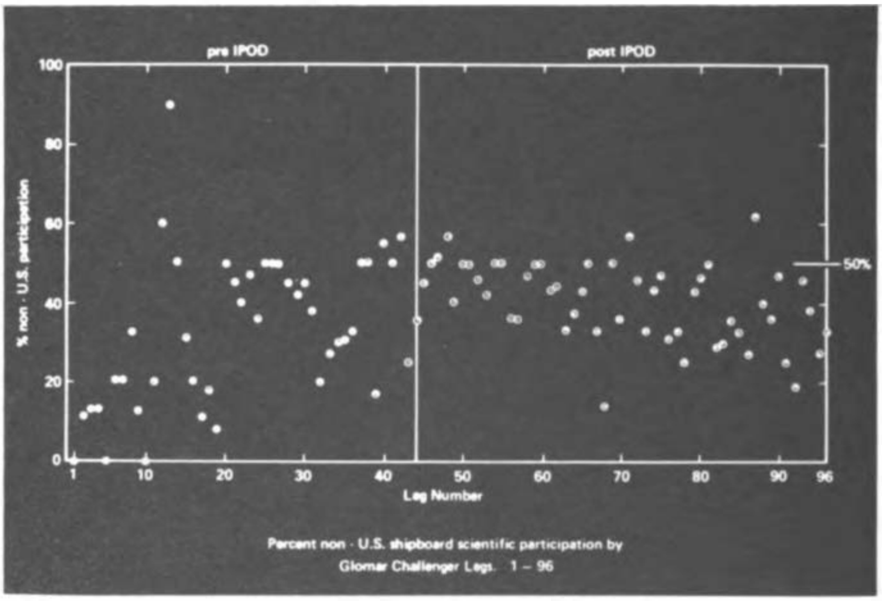


FIGURE 40 Percent non-US shipboard scientific participation for Legs 1-96, DSDP. Note that the international participation was a maximum on Leg 13, and that it was controlled at less than 50% after the beginning of the International Phase of Drilling (IPOD).

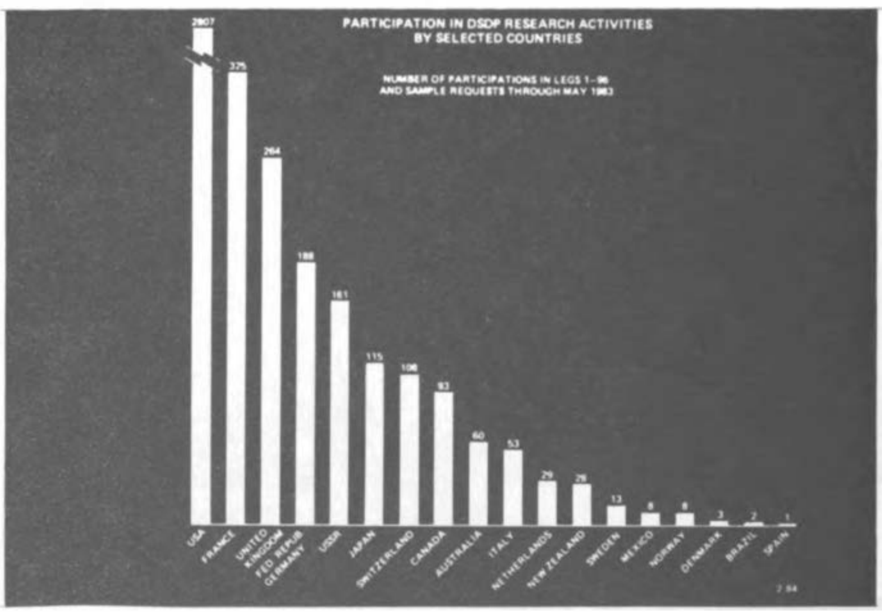


FIGURE 41 Participation in DSDP research activities by selected countries, through May 1983. For this figure, an activity is defined as a scientist on board or a request for samples.

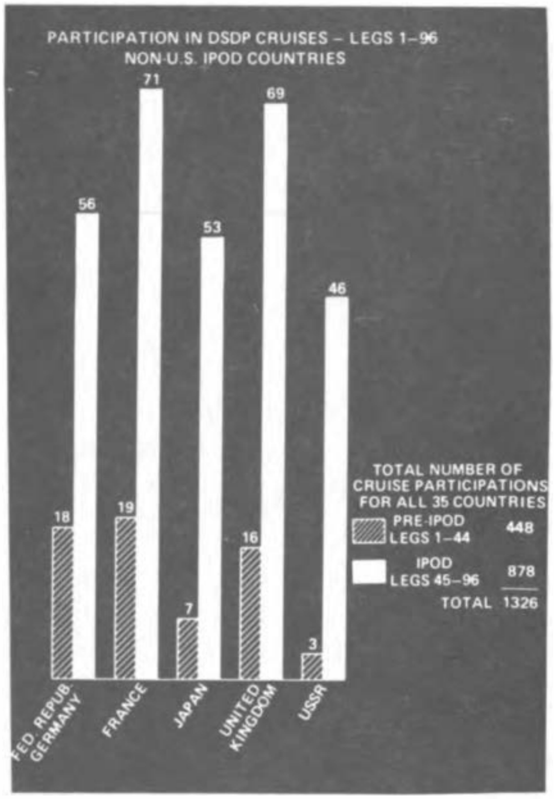


FIGURE 42 Participation in DSDP cruises, Legs 1-96, for non-U.S. IPOD countries.

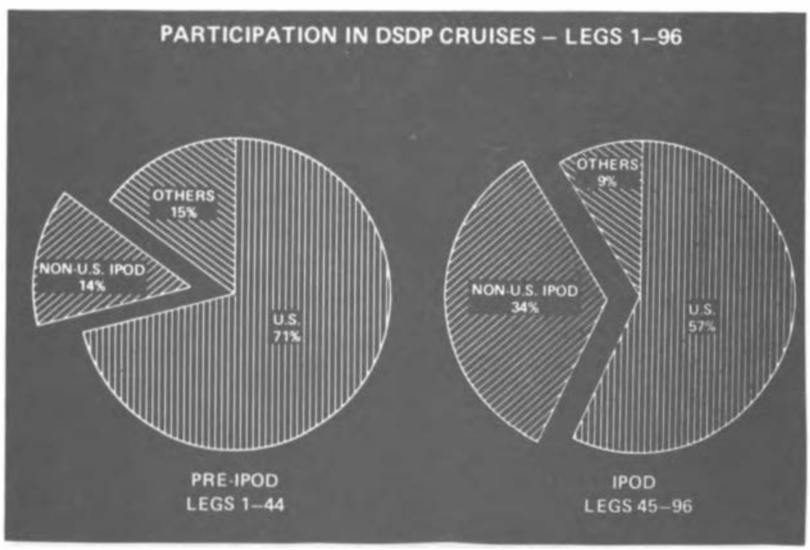


FIGURE 43 Pie diagram of participation in DSDP cruises. Since the beginning of IPOD, the activity of IPOD countries outside the U.S. has increased appreciably.

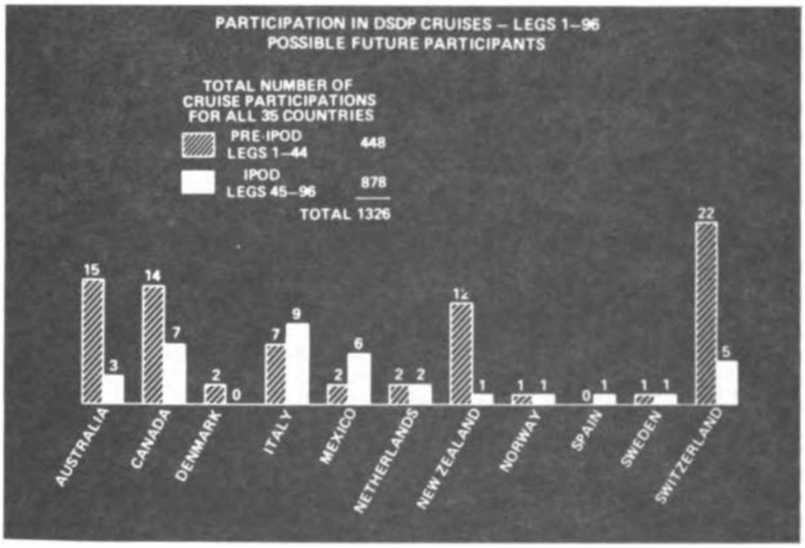


FIGURE 44 Participation in DSDP cruises--Legs 1-96. The effect of the IPOD was to decrease participation of non-member countries; this acted as an incentive for them to join the subsequent Ocean Drilling Program.

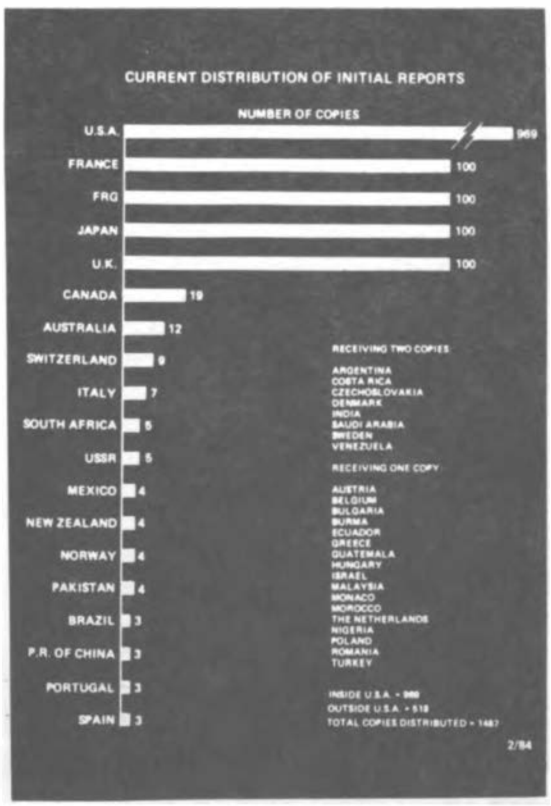


FIGURE 45 Current distribution of the Initial Reports of the DSDP. This figure postdates the separation of the Soviet Union from the program.

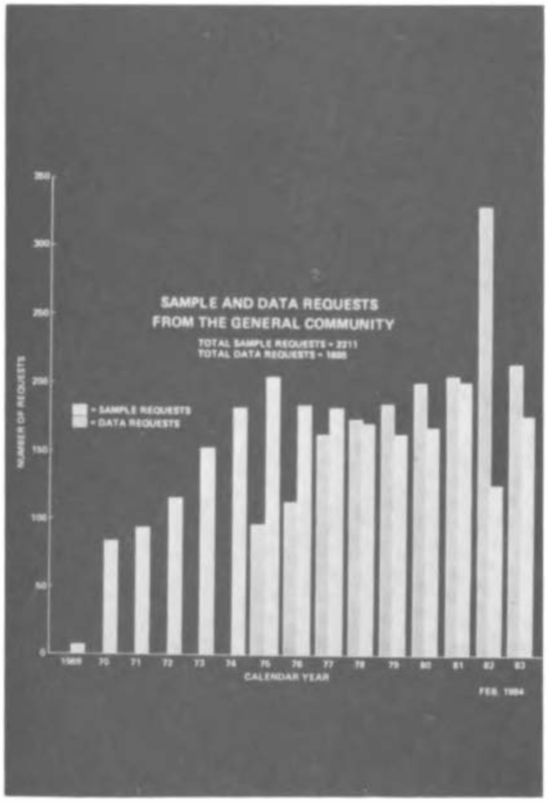


FIGURE 46 Sample and data requests from the general community by year. Total sample requests = 2,211; total data requests = 1,685.

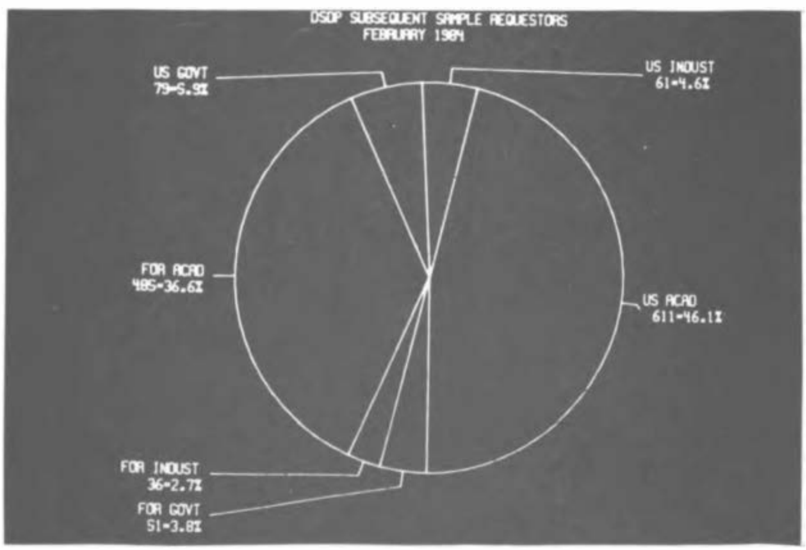


FIGURE 47 Sample and data requests by requestors, for February, 1984. Given are the number of requests and percentage of the total. FOR = foreign; US = United States.

FIGURE 48 Sites drilled during the DSDP. Note that the locations include all of the major ocean basins and marginal seas, with lesser representation of the Antarctic and no locations in the Arctic.

FIGURE 49 Casting off the ceremonial last line of the Glomar Challenger. To the right is Mel Peterson, to the left is John Duke.

FIGURE 50 A final view of the Glomar Challenger, stripped of her equipment in Mobile.

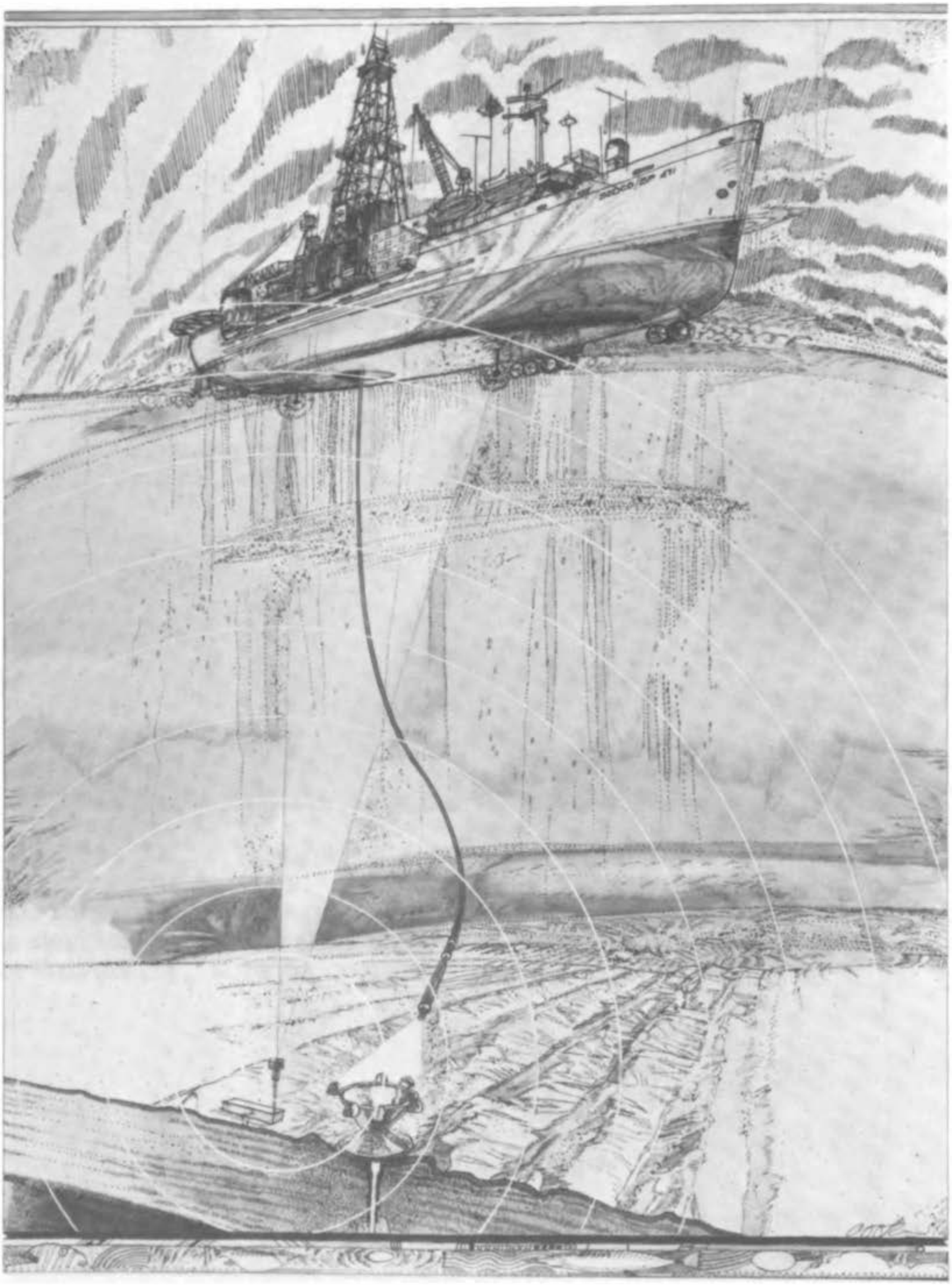


FIGURE 51 Artist's sketch of the JOIDES Resolution (SEDCO/BP 471), with dynamic positioning equipment along keel, sonar positioning system and reentry cone on seafloor, and TV reentry system on drill string. From SEDCO, Inc., 1984 Annual Report.

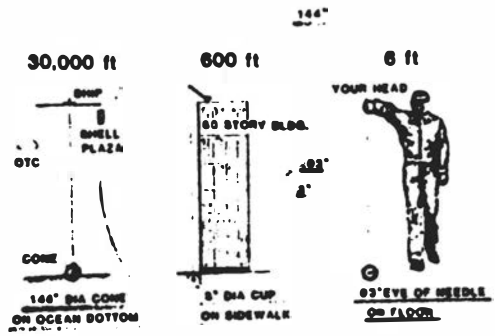


FIGURE 52 The difficulty of reentry. Shown are analogous situations to reentering a site in deep water. From D. H. Hammett, 1986, Drilling technology application: 15,000-ft. water depth: International Association of Drilling Contractors/Society of Petroleum Engineers 1986 Drilling Conference, IADC/SPE 14726, p. 63.

FIGURE 53 JOIDES Resolution (SEDCO/BP 471), drill ship for the Ocean Drilling Program (ODP). Note the large laboratory stack (with portholes) just forward of the derrick. The ship is 470 feet long, 70 feet wide, and the derrick rises 200 feet above the waterline. It is operated by Texas A&M University, science operator for the ODP.

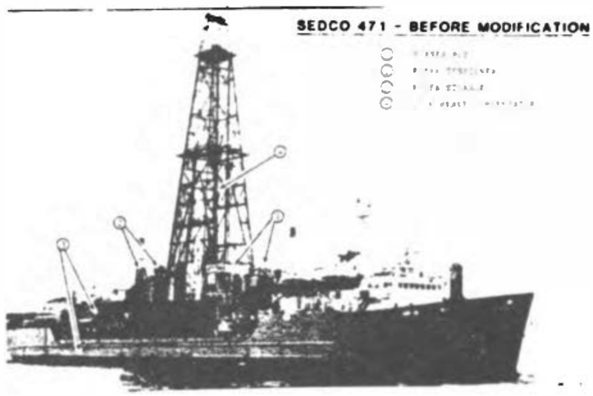


FIGURE 54 SEDCO/BP 471 before modification. Note absence of laboratory stack. From D. H. Hammett, 1986, Drilling technology application: 15,000-ft. water depth: International Association of Drilling Contractors/Society of Petroleum Engineers 1986 Drilling Conference, IADC/SPE 14726, p. 63.

FIGURE 55 Diagram of laboratory stack. Contact Ocean Drilling Program, Texas A&M University, 1000 Discovery Drive, College Station, Texas 77843.

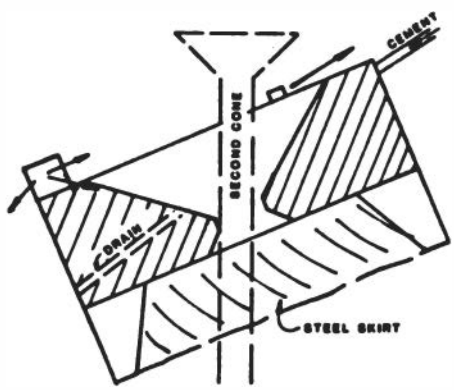


FIGURE 56 Gravity box base reentry cone/guidebase system designed for drilling on rough, broken, and tilted seafloor. From Hammett, D. H., 1986, Drilling technology application: 15,000-ft. water depth: International Association of Drilling Contractors/Society of Petroleum Engineers 1986 Drilling Conference, IADC/SPE 14726, p. 63.