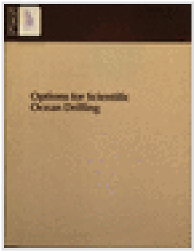

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Options for Scientific Ocean Drilling

The Final Report of the
Committee on Ocean Drilling
Geological Sciences Board
Commission on Physical Sciences,
Mathematics and Resources
National Research Council

NATIONAL ACADEMY PRESS
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This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

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OPTIONS FOR SCIENTIFIC OCEAN DRILLING

INTRODUCTION

The Committee on Ocean Drilling was formed at the behest of the Congress, which, in authorizing the National Science Foundation's appropriations for FY1981, stated that "the National Academy of Sciences shall study marine earth science research and report to the Congress." Although stated in these general terms, the request was made with respect to NSF's proposed Ocean Margin Drilling Program (OMDP) and was defined by NSF as asking the Academy to "examine the scientific worth of the OMDP in terms of the overall research goals in the geological sciences especially as related to the marine areas." Under this charge the Committee began its work.

Soon after the Committee's inception, however, a series of events drastically modified NSF's proposed program and refocused the Committee's charge to include an overall appraisal of the merits of ocean drilling in general and a comparison of several available operating options. It is on this modified charge that the Committee has concentrated its attention.

BACKGROUND

The National Science Foundation's proposed new program of scientific ocean drilling was conceived in the mid-1970's as a follow-on to its highly successful Deep Sea Drilling Project (DSDP). The need for a new program was evidenced by these developments: (1) *Glomar Challenger* was getting older and would soon need major refurbishment to continue drilling in the deep ocean basins; (2) Drilling by *Challenger*, plus other surveys, suggested that a number of scientifically significant problems were close to or beyond the limit of *Challenger's* capabilities; and (3) the *Glomar Explorer*, originally designed for other work, was available for conversion and had the potential to be a superior drilling vessel.

Early iterations of the program were based on the report of a planning conference held at Woods Hole, Massachusetts, in March 1977. The report, "The Future of Scientific Ocean Drilling" (FUSOD), was published in July 1977 by the JOIDES* Executive Committee. Both the FUSOD report and NSF's original plan visualized a continuation of Challenger-type drilling in the deep oceans but contemplated (a) drilling deeper in selected places and (b) operating in stormy high latitudes where Challenger is inefficient or cannot operate at all. In addition, the new program would include drilling in the transition zones between the continents and ocean basins, where important geologic problems have been identified and where drilling requires the design and installation of blowout prevention equipment. It was this latter objective that originally led to the name "Ocean Margin Drilling Program," even though drilling along the margins was but a fraction of the total program.

The unknown potential of the continental margins for oil and gas production meant that the new program could benefit the search for resources as well as the pursuit of pure science. To exploit this additional possibility, a cost-sharing partnership was forged between the federal government and a group of oil companies. Jointly funded for initial planning, the partnership was renewable at the option of all participants.

Because of the continuing interest of the petroleum industry in learning more about the composition of continental margin sediments, emphasis was accorded these regions and the program became skewed toward Ocean Margin Drilling. This plan had several variants, mostly based on the Houston

*JOIDES is the acronym for a consortium known as the Joint Oceanographic Institutions for Deep Earth Sampling.

Scientific Ocean Drilling (HUSOD) report entitled "Ocean Margin Drilling Program" and issued by the Interim Planning Committee, JOI Inc.,* March 1980.

In October 1981, the ten petroleum companies of the partnership voted, for reasons not related to the scientific value of the program, not to support the program beyond the initial planning. Withdrawal of the petroleum companies turned the program focus away from the ocean margins and reopened the larger question of what sort of drilling program is justifiable over the next five to ten years. Program design was, of course, dependent on the technology and on the ships available. Consideration of these questions forms the main thrust of this report.

THE CASE FOR FUTURE DRILLING

There is no question about the success of NSF's DSDP/IPOD** program or of its contribution to geological sciences. DSDP has amassed an unmatched record of exploration into the least known parts of the earth's crust. The accumulated data and the consequent increased understanding of the earth's structure and dynamics will mold geologic thinking for many years to come. The willingness of other nations to participate in the scientific work and to help finance the operation is emphatic evidence of the high international regard in which the project has been held.

The many accomplishments of DSDP have been well-documented, and we cite here only a few examples, such as verification of the sea-floor spreading model, demonstration of large-scale vertical movement and the discovery of past chemical, physical, and biological ocean environments different from those

*Joint Oceanographic Institutions, Inc.
**International Phase of Ocean Drilling

of the present. Hidden behind the more glamorous headlines is what may be DSDP's most enduring contribution--the building of a reconnaissance geological section of the sediments that constitute the upper part of the oceanic crust. But this reconnaissance section, valuable as it is, is built upon only about 500 drill sites in the deep oceans, or one data point for each 275,000 square miles. Further investigation of this section, its variations, and its relation to continental crust hold promise of major advances in understanding earth history, composition, structure, and resources.

Past success alone does not justify continuation. Starting with the FUSOD meeting in 1977 and culminating with a meeting in November 1981 of a group called the Committee on Scientific Ocean Drilling (COSOD), various interested groups have discussed the scientific merits and outlined the desirable content of a follow-on program. The several reports vary more in emphasis than in content, and all have two features in common. First, they are problem oriented. This is a logical and proper outgrowth of the DSDP program, which started as geologic reconnaissance and then identified specific important problems for attack. Second, although the proposed programs all require the drill as an essential testing tool, drilling is to be considered only as part of an integrated effort that uses all available tools--geophysical and geological surveys, follow-up analyses, syntheses, etc. This integrated attack upon chosen problems is a very important feature of the proposed program. The drill, albeit required for the tests, is also the most expensive of the various tools available, and it would be wasteful to use it without the guidance provided by the other techniques. The Committee notes favorably that the NSF plans adopt this rationale.

In our opinion, the several groups that have considered ocean drilling have made a very strong scientific case for a continued program. One of the best summary statements is in the introduction to the COSOD report, which we quote here with slight modification (indicated by italics):

"The drilling of sediments and rocks of the ocean basins makes contributions to many branches of science. The continuous and detailed record of microfossils preserved in ocean sediments may give the best data for describing evolutionary changes and for understanding their causes. Sediments bear the imprint of ocean temperatures and currents, information critical to the reconstruction of oceanic circulation of the past, and hence to the reconstruction of ancient climates *and ultimately to a better understanding of the nature of modern climate and of climatic change*. Drilling provides access to the rocks of the oceanic crust, and thus is helping to unravel its structures and motions, information required to understand the phenomena of seafloor spreading and continental drift, and, more broadly, the structure of the earth as a planet. Deep sea sediments record the contributions of the rivers and winds of the past, and thus the history of the continents, records otherwise lost by erosion of the land. In addition to greatly increasing our knowledge of earth history in general, the scientific information gained by drilling is basic *to the understanding needed to guide* the search for mineral and petroleum resources both on land and beneath the seas. As the ocean is the last frontier for these resources, the importance of a thorough understanding of its geologic history and framework cannot be overstated.

"Before the *Glomar Challenger* . . . set sail on her initial trials, JOIDES identified as primary objectives for the Deep Sea Drilling Program 'the determination of the age and processes of development of the ocean basins.'

Implicit in these objectives was the need to have long cores for 'biostratigraphy, physical stratigraphy, paleomagnetism . . . and for studies of the physical and chemical aspects of sediment dispersal, deposition, and the post-depositional changes in sediments.' The success of the program in achieving or progressing toward these goals is almost legendary. Indeed the results confirmed the concept of seafloor spreading, the relationship of crustal age to magnetic anomalies, the basaltic nature of the oceanic crustal rocks, and, through the systematic sampling afforded by the drill, initiated an entirely new field of study: paleoceanography.

"This technology has taken geological sciences through more than a decade of unprecedented advancement and has been instrumental in bringing us to our present level of understanding of the origin and history of the ocean environment. That understanding stems primarily from reconnaissance drilling based on reconnaissance geophysical studies. We now need to advance our level of technical expertise in both drilling, . . . geophysical surveying, and in down-hole instrumentation. It is clear from the discussion and position papers presented at the Conference on Scientific Ocean Drilling that we are entering into a new era of ocean exploration utilizing the concepts of natural laboratories on the seafloor and carefully chosen arrays of drill sites to study general processes and global problems. In the past decade we have learned that the keys to geological processes and much of the history of the earth for the past 200 million years are recorded in the sediments and rocks of the ocean basins. We have only begun to read and to interpret the story that they hold."

More detailed justifications follow different formats in the several reports. We will summarize them under the four headings used by FUSOD.

1. Paleoenvironment

This is the subject area to which *Challenger* has devoted most effort. Therefore, many of the questions have been posed by the data already provided. The questions relate particularly to the history of ocean sedimentation, to chemical and physical environments, to biological evolution, to the thermal and circulation history of the oceans, to the responses of oceans to orbital and other geophysical variations, and to the response of the deep sea environment to sea level fluctuations. The hydraulic piston corer, recently developed by DSDP engineers, allows the taking of overlapping five-meter undistorted cores of soft sediments, aggregating several hundred meters. These remarkable samples studied with new highly sensitive magnetometers can lead to a refined global magnetic stratigraphy. With such a framework, all manner of paleoenvironmental studies--biological, chemical, and physical--can be integrated and correlated from ocean to ocean.

Much of the paleoenvironment program could be carried out from *Challenger*, but some prime target areas include sediments too thick or latitudes too high for effective or safe *Challenger* operation. For a truly worldwide investigation of paleoenvironments, *Explorer* (without riser) would be the most effective vehicle.

2. Composition and evolution of the oceanic crust

These problems relate to the generation, structure, and composition of igneous rocks below the ocean sediments. Of particular interest are hydrothermal processes associated with the spreading centers. These processes dominate the chemical control of the oceans and also generate metallic ores.

As our mineral resources dwindle, the investigation of ore-forming processes becomes even more important. We need to know how the hydrothermal systems vary spatially away from the ridge crusts, how magma is generated below the spreading center, and how and at what rate the new crust is formed.

Answers to such questions can lead to a better understanding of the composition of the crust and upper mantle and to progress in understanding the dynamics of the plate tectonics model. Some of this important work can be done by Challenger, but the deep penetration into fractured basalts that would be necessary could be most effectively handled by Explorer. In a few cases, a riser would be necessary so that heavy mud could be used to inhibit caving.

3. Studies of active margins

Where two plates collide, one typically overrides the other, which is drawn down or subducted back into the mantle. The zone of collision is usually delineated by a deep-ocean trench. Where both the subducted and overriding plates are oceanic crust, plate borders are commonly marked by volcanic arcs (e.g., the trenches and arcs of the western Pacific). Where a continent rides the leading edge of the plate, the result is usually a long chain of coastal mountains, generally well-exposed and consisting partly of crumpled sediments and partly of volcanics (e.g., the South American Andes). Drilling along active plate boundaries will provide information which, added to geophysical and geological investigations, will allow better understanding of the dynamics of subduction. These integrated data will provide a more complete view of continental geology. An excellent start has been made by Challenger in investigating these dynamic regions of the earth's crust, but many important new targets require deep penetration into the seafloor and probably cannot be reached by Challenger. Such targets would require the greater capabilities of Explorer.

4. Passive margins

Passive continental margins result from rifting, formation of spreading centers, and the generation and lateral movement of new crust. The rocks of passive margins record a history of continental break-up and chemical transition between continent and ocean crust. Some of geology's most intriguing problems lie here, but they are also some of the most difficult to tackle. Atop the continental transition are thick wedges of sediment--some half of all the marine sediments deposited during the past 200 million years. Because of these enormous wedges of sediments, the passive margins have the potential of containing gas and oil, a potential that precludes deep penetration without a full riser and blowout prevention system. Marine drilling with a riser has been initiated by the petroleum industry, but only on the continental shelves, and only where the potential for hydrocarbons is greatest. Moreover, the data from that drilling are not always readily available to non-industry scientists. Drilling to study the continent-ocean basin transition will be out on the continental slope, in deeper water, and with advanced technology. It must await a vessel with *Explorer's* capacity.

In summary, we believe that the various advocate groups have made a strong and convincing case for continuing scientific ocean drilling through the 1980's. The problems that can be addressed are among the most exciting facing the geological sciences, and their solution would make major contributions to our understanding of this planet. *We urge that support of such a program be clearly recognized as a long-term commitment, without which there can be no effective planning or efficient execution. We urge also that support for ocean drilling (or indeed any other similar large projects) not detract from NSF's regular program of basic research support.*

PROGRAM OPTIONS

As noted above, the withdrawal of industrial support from the Ocean Margin Drilling Program sensu stricto occasioned a reevaluation of the whole future of scientific ocean drilling. NSF summarized the possibilities in terms of four basic options:

1. Terminate ocean drilling in 1983 when Challenger finishes its present contract.
2. Continue a riserless drilling program using a refurbished Challenger for an additional five years.
3. Convert Explorer to a non-riser drilling ship and use her as a newer and superior deep-sea drilling platform.
4. Convert Explorer with full riser and blowout prevention hardware to conduct both riser and riserless drilling programs.

A fifth option, unstated by NSF, was the continued use of Challenger without refurbishing. This option appears likely to lead to slowly rising costs as repairs must be made, and to a drastic decline in productivity. It appears useful only for a very short-term program and would be less cost-effective than any of the positive options. We have dismissed it as ineffective.

The basic choice is between #1 and the others--to drill or not to drill. We conclude that the scientific justification for a continuing program is very strong and therefore recommend that option #1 no longer be considered. A choice among the remaining options is less clear. Starting with #2, each succeeding option provides increasing capabilities and allows an attack on more problems. But each is more costly than its predecessors. The choice of options therefore depends partly on how much can be afforded and partly on the cost effectiveness of each option in achieving important scientific goals.

Option #4, Explorer with full riser, would allow attack upon the greatest range of scientific problems. It is also the most expensive (development and construction of the riser blowout-prevention system alone is estimated at $50-100 million) and may not be fundable at this time. Therefore, if we defer this option because of probable economic difficulties, the choice would lie between a refurbished Challenger and an Explorer without a riser but with the possibility that this capacity would be added later.

As noted in the scientific justification, either Option 2 or 3 can yield a strong scientific program, and the decision is really one of cost effectiveness. The case for preferring the Glomar Explorer seems to us very strong for the following reasons:

1) Without minimizing either the past contributions of Challenger or its potential for continuing contributions, the added dimensions that Explorer (even without a riser) would provide seem well worth the small incremental costs of her conversion and operation (Table 1). The ability to operate in higher latitudes and at higher sea states will allow expansion of present sediment studies to yield real global patterns. Especially important will be drilling off Antarctica, an area of inclement weather which is a critical region in oceanic and atmospheric heat transfer. Also the potential for deeper penetration of both sediments and the igneous basement will allow work on problems beyond the ability of Challenger.

2) Explorer, even more than Challenger, should be considered for what it really is--a large versatile facility rather than a large single tool or project (Figure 1). It is comparable to an astronomical observatory, a large

accelerator, or an NCAR.* It is a platform from which many questions can be addressed, many problems tackled. Moreover, Explorer is a nearly new, sturdily built vehicle with a potentially long productive life. It is also large enough to accommodate sophisticated analytical instruments that facilitate shipboard research and training. It is a long-term investment in scientific opportunity, and the opportunity to acquire such a versatile observatory at this price tag is almost certainly time-limited. Explorer will not be available for the indefinite future, and to duplicate her capabilities with a new ship would cost much more, perhaps twice as much as her conversion.

3) Although none of the cost estimates available to date can be considered really firm, a number of careful iterations and revisions tend to confirm that the annual operating costs of Explorer would exceed those of Challenger by no more than 10 percent. Even this difference may be offset by new foreign partners attracted by Explorer's greater capabilities and by the potential for conversion of her engines from marine diesel fuel to heavy oil.

Conversion of Explorer (without a riser but designed to accommodate one) would apparently cost between $50 and $100 million (1982 dollars); this is expensive but not a large amount for a facility with such potential. Moreover, a large part of these "up front" costs could be defrayed by simply maintaining the present operating budget for one to two years after Challenger finishes her stint in 1983.

When we take all these factors into consideration, the Explorer option seems clearly the most productive and cost effective. Both COSOD and the NSF have recommended this route and we strongly agree with their choice.

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*National Center for Atmospheric Research

FURTHER CONSIDERATIONS

Although deeper penetration and operation in higher latitudes and/or sea states with a corresponding explosion in fundamental knowledge of crustal processes are the prime factors, several other arguments add strength to the rationale for continued scientific ocean drilling and to the preference for moving up to Explorer. These are:

1) The universal approval of the proposed program by so many independent groups, ranging from panels of prominent earth scientists (e.g., the Giletti Committee) to one comprising leaders from many disciplines (e.g., NSF's "Blue Ribbon Panel").

2) The ever-increasing number of interested personnel. Many young investigators are involved--witness the composition of the COSOD meeting in Austin in November 1981. This was a meeting of "proponents" of drilling, more than half of whom have yet to sail on Challenger or be directly involved in DSDP analyses, but all of whom realize the scientific and training potential of the problems Explorer could attack.

3) The opportunity to train students. The Challenger program has translated directly into educational opportunities both at sea and in the sample analyses that have eventuated into numerous important doctoral dissertations. When fully outfitted, Explorer would be in truth a floating laboratory, with instruments equivalent to those in our best marine geology laboratories. And with her larger accommodations, more students could participate directly, at sea as well as ashore.

4) International aspects. The contributions to international cooperation have been substantial and perhaps not sufficiently publicized.

DSDP/IPOD has been a unique program in international cooperation and a uniquely successful experiment in international relations. *Explorer*, with its greater capacity, offers the opportunity for increasing the number of cooperating countries. The value of continuing this effort should not be overlooked or underestimated.

CONCLUSIONS AND RECOMMENDATIONS

The conclusions of the NAS/NRC Committee on Ocean Drilling are summarized as follows:

1. The contributions of deep-sea drilling to the earth sciences have been of immeasurable importance; the project has "paid" for itself many times over. The new knowledge has been important not only in basic science but also in focusing the plate tectonics model on the location and formation of resources. However, even after 13 years of deep-sea drilling, the ocean basins remain a large piece of the earth's crust about which comparatively little is known. Yet they are an area which, if the plate tectonics model holds true, provides an extraordinary chance to help understand the whole earth. The oceanic crust is the site of fundamental earth processes that control or affect mountain building, earthquakes, ocean chemistry, ocean circulation and climate, and the placement of ore deposits. Another decade of ocean drilling holds promise of new contributions as important as the discoveries of the past ten years.
2. The current plans outline an attack on important geological questions involving global composition and structure and the processes that produce them. The plans are a logical follow-on to the current Deep Sea Drilling Project.

3. The new program should include approval for a long-range commitment. DSDP/IPOD operations have been severely hampered because of uncertain life expectancy. Extensions have been for short periods of time, with the threat or prospect of termination persisting almost up to each renewal deadline. This has not allowed time for orderly site surveys or even for good cruise planning. Longer-range planning will be essential for the new program.

4. Of the various operational options, we prefer Explorer, starting in a riserless mode with a view to adding the riser system after a few years of operation. Not only would this stretch out the capital costs of conversion, but it would probably provide the most cost-effective scientific program.

5. Either the Explorer or the Challenger will be an important national tool for testing scientific hypotheses and as such will be equivalent in significance to an accelerator, an astronomical observatory, or an NCAR. Such facilities should be supported separately and should not compete directly with NSF's ongoing support programs for individual team research in the standard disciplinary fields.

6. In summary, the Committee on Ocean Drilling recommends strongly the proposed continuation of NSF's ocean drilling program. It is a high quality, long-term basic research program with an international flavor that can be achieved no other way; and its output will be important not only to geology but to related fields. The Committee also recommends acquisition of the Glomar Explorer. This floating scientific facility will have significantly greater capabilities and a much longer productive life expectancy than the venerable Glomar Challenger. The incremental costs of Explorer seem modest when weighed against the scientific benefits it promises to yield.

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TABLE I
Summary of Cost Comparisons
(In millions of 1982 dollars)

A. Refurbish *Challenger*, operate through 1992

	FY'83	FY'84	Annual 1985-1992	Cost/yr.
Operations	20.0	21.7	22.5	
Science	7.0	7.0	11.6	
Capital	0.0	5.1	1.9	
Program Total	27.0	33.8	36.0	$ 36.8
Less Foreign	-8.3	-6.0	-8.0	
NSF Approp.	18.7	27.8	28.0	28.8

B. Phase *Challenger* out. Convert* & operate *Explorer* through 2005

	FY'83	FY'84	FY'85	1986-2005	Cost/yr.
Challenger Operations	19.4	2.2	0	0	
Explorer Operations	0	0	0	22.6	
Science	5.9	6.4	6.5	12.1	
Capital Costs	6.0	33.5	36.1	0	
Program Total	31.3	42.1	42.6	34.7	38.9
Less Foreign	-8.3	-5.8	6.3	17.0	
NSF Appropriation	23.0	36.3	36.3	17.7	21.3

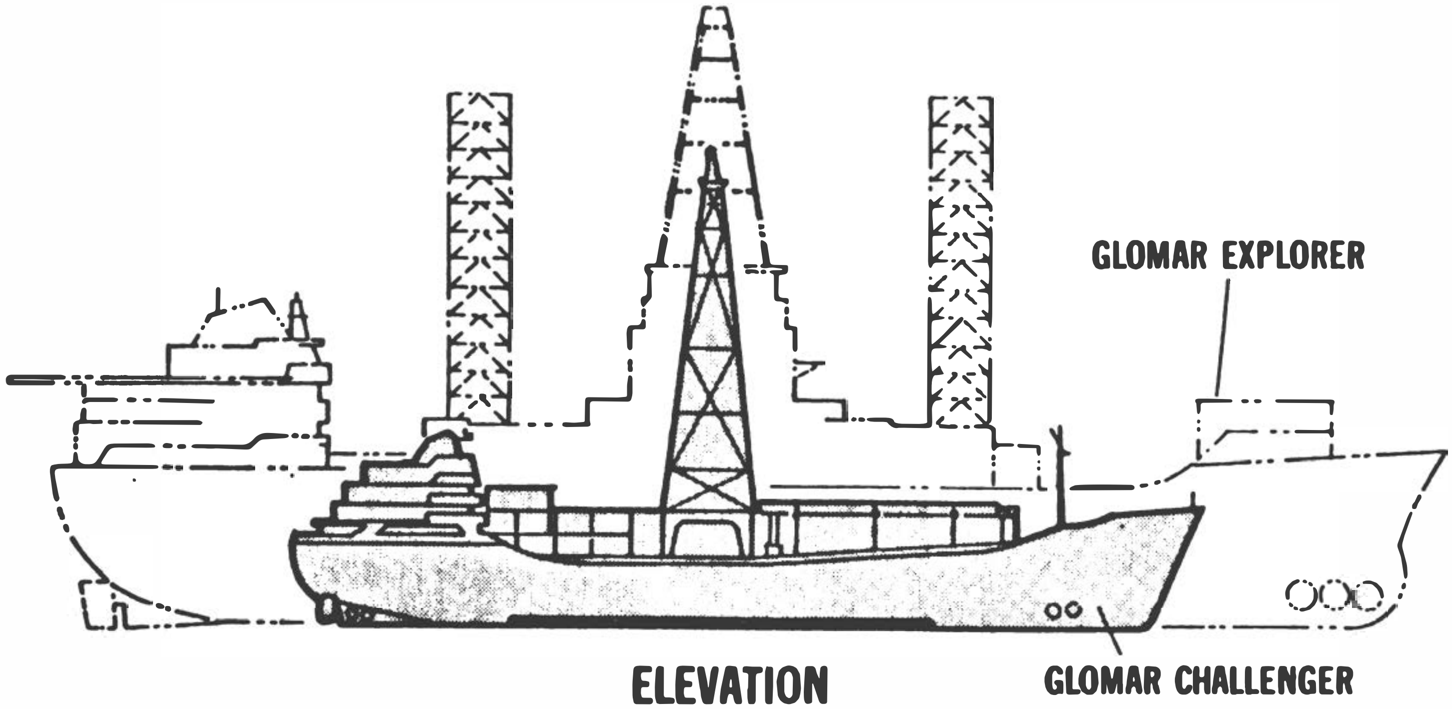
*All conversion costs funded by FY 1984-1985 appropriations

C. Phase *Challenger* out. Convert* and operate *Explorer* through 2005.

	FY'83	FY'84	FY'85	FY85-95	95-2005	Cost/yr
Challenger Operations	19.4	2.2	0	0	0	
Explorer Operations	0	0	0	22.6	22.6	
Science	5.9	6.4	6.5	12.1	12.1	
Capital Costs	6.0	20.2*	22.8*	5.7	0	
Program Total	31.3	28.8	29.3	40.4	34.7	40.5
Less Foreign	-8.3	-5.8	-6.3	17.0	-17.0	
NSF Appropriation	23.0	23.0	23.0	23.4	17.7	22.9

*Conversion costs funded partly by FY 1984-85 appropriations, partly by 10 year lease-purchase of some equipment items

Source of data,
National Science Foundation.



	HGE	CHALLENGER
DISPLACEMENT	21,000 LT LIGHTSHIP	4,303 LT LIGHTSHIP
LENGTH	618 FT	400 FT
BEAM	116 FT	65 FT
DEPTH	51 FT	27 FT
HULL VOLUME	3,000,000 CU/FT	600,000 CU/FT

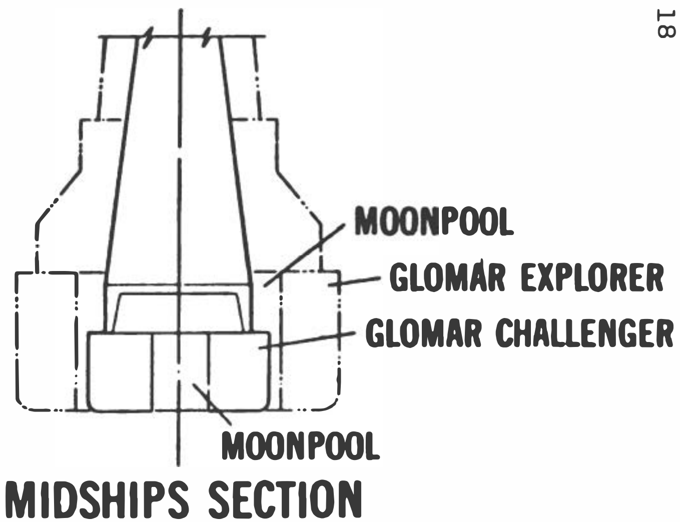


FIGURE 1 Comparative Sizes of Glomar Explorer and Glomar Challenger

Source: EOS and NSF.