APPENDIX A

Alternatives To The Ocean Margin Drilling Program

Suggested by

OTA Panel Members

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Alternative Suggested by

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Extension of the program schedule appears to be already occurring, and I consider it a good thing. My personal and scientific preference would be for some additional delays in the conversion of Glomar Explorer and development of a riser capability, with the intervening years to be filled in by continued Glomar Challenger drilling, utilizing the exciting capabilities of the hydraulic piston corer. In addition, during these intervening years, extensive geophysical work should be funded on continental margins and in other prospective drilling areas.

Glomar Challenger **cannot continue indefinitely. The ship apparently** has a finite remaining **economical life.** A few more years of operating with the hydraulic piston corer, however, would be strongly supported by the scientific community but I certainly do not advocate eliminating the OMD Program.

In summary, I advocate a slightly modified program, as outlined briefly above: some delays in development of Glomar Explorer capability, with funding of additional Challenger HPC work and extensive geophysical surveying, both on continental margins and in other parts of the world. Ideally, this alternative program would simply delay the major part of the OMD Program, but would provide time for additional utilization of HPC for stratigraphic and climatological purposes and for much more extensive geophysical surveying. The stratigraphic and climatological objectives with HPC are important, but in my mind are no more important or of higher priority than the deep-drilling objectives of OMD. Instead, they represent an attempt at refinement and an opportunity to gain more data points in the shallow part of the section; whereas OMD offers the first-ever opportunity for deep drilling, both deepwater and deep-penetration, on continental slopes and rises.

There is a great deal of concern in the marine geological community that will preclude optimal utilization of HPC. The alternative program described briefly is a compromise, trading increased support of HPC geophysics for delay in timing of OMD.

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Alternatives Suggested by

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Alternative Scenarios

There are a number of alternative scenarios that could be suggested, some productive, many destructive. In any of these it should be recognized that no one is against the fact of drilling, for drilling provides the moment of truth - the hard data that confirms or denies the geophysical interpretations. There may well be, on the other hand, differences of opinion on methodology, on timing, on focus, and on how the costs should be borne.

a. We might start with the Luddite approach, eliminate the drilling because of its very high cost compared to other options. The emotions behind this approach are real and stong, but they presume that the funds exist for application to other purposes. In the no bottom line budgeting process this is not really true. If there is a real limit to the budget of NSF, it may be true. This alternative cannot be appraised realistically unless one knows whether there are trade offs and what they are.

b. The Hedberg approach suggests that industry play a more important -even a major- role. This is an appealing option, but there is no free lunch. I doubt whether the Congress is prepared at this time to lease the large tracts that industry would need to justify the major investment. I would also have some qualms, were I in industry, about how far I could go with cooperative ventures of this sort before there were the anti-trust problems.

co The present program is an NSF program with NSF as the prime agency for footing the bill. The rationale is that it is a science program and NSF is the primary science support agency. This could be argued. The present program is at least as much a technology program as a science program, and many of the industry people hint that they are looking for appreciable technological fallout from it. The limitation to water depths greater than 2,000 meters supports this suggestion.

Industry probably would not move into riser drilling at abyssal depths for a decade or so. What a splendid opportunity OMD presents for letting someone else pick up the tabs for mistakes. This should not be construed as an argument against drilling, but might well be taken as an agrument for DOE participating in the funding. DOE is throwing all sorts of money at other technologies. One also has the gnawing feeling that the relevance of OMD to specific USGS missions ought to create more enthusiasm for funding from this source than has been obvious to date.

d. Many of the scientific objectives in the continental margins could be reached by drilling vessels in existence or nearly so. If the whole drilling program spelled out by FUSOD were to be carried out, obviously it would be necessary to have a vessel with the capability of drilling in abyssal depths. If the focus is on the continental margins, and ocean crust and paleoenvironment can be shoved under the

rug, perhaps some reappraisal is in order. I submit , and Bally has submitted in some of his statements, that proper geophysical and geological investigations can locate drilling sites on the continental margins that are responsive to the scientific questions and that could be drilled using existing vessels. The scientific rationale for the Glomar Explorer weakens markedly as the emphasis on the continental margins grows stronger. If this approach were followed, to drill with leased vessels on the margins, then the possibility of continuing the Glomar Challenger or a suitable replacement to carry on abyssal drilling should be examined carefully.

e. The HOUSOD report provides a few crumbs for all, but satisfies no one. Perhaps it would be more productive to bite the bullet and concentrate efforts in one area, such as the East Coast or the Gulf Coast. This concentration would keep the vessel near good logistic ports would minimize drilling time lost in steaming from one location to another, and would greatly increase the chances of solving the problems in that area. If this alternative were followed, it would again be desirable to remove the 2,000 meter restriction and to drill in the place with the greatest promise of providing answers to the scientific questions. Again, this would abandon abyssal drilling and the question of continuing Glomar Challenger type drilling should be reexamined.

f. Finally, it seems to me that the crux of the problem is whether this is a science program or a technology program. If it is the latter, then I do not think that it should be financed by the National

Science Foundation. If it is the former, then the focus should be on how to do best science in the best place with the best available technology. If it is a mix, as it is reputed to be, let us be sure we are doing the science with the best technology and that the costs are equitable borned by those institutions which have, or should have, a stake in the game.

Alternatives Suggested by

Dr. James D. Hays

Lamont-Doherty Geological Observatory

Alternatives to the Program

The most appealing alternative to the present program, one that could address exciting first order scientific problems, stimulate the broad interest of the scientific community and not cost the taxpayer much more than the present deep sea drilling program would be a program that had two major thrusts. The first would involve a continuation of the present Glomar Challenger drilling program, the second a Continental margin geophysical survey program.

Continuation of Glomar Challenger Drilling

During the last two years a major technological advance has occurred in the recovery of soft sediments from the ocean floor. A hydraulically driven piston coring device (the Hydraulic Piston Corer, HPC) has successfully recovered hundreds of meters of undisturbed sediment and has proven that it is possible to obtain continuous sequences of this length. This device opens the way to a whole series of exciting studies including (1) the evolution of global climate measured on time scales of a decade to millions of years. (2) the evolutionary development of marine plankton during the last 10-15 million years. (3) the sedimentary structure of deep-sea fans deposits which are the most probable reservoirs of any deep-water hydrocarbons. (4) the suitability of various types of deep-sea deposits as repositories for nuclear wastes.

There is no doubt in my mind that these studies plus margin and crustal drilling by a Challenger type vessel would produce far more good science than the OMD program at a fraction of the cost. I'm **also sure these studies would have wide International support.**

Continental Margin Geophysical Program

Continental margins can be studied in a variety of ways. Drilling is only one way and it happens to be the most expensive. So it should be used only after all other means of gathering information have been utilized. It is clear that the more one knows about a margin the more likely one is to make a wise choice in choosing a drill site.

Information about the evolution of Continental Margins can be gained by studying rocks of ancient margins that are now on land. This kind of work should be encouraged. The submerged modern margins can be studied with geophysical techniques and much can be learned from deep-penetration seismic reflection work. I propose that this be the heart of the academic ocean margin program during the next decade (much as proposed in the Bally report). In the meantime Industry will continue to drill wells on the shelves and data from these wells will become part of the public domain. Industry will also continue to develop increased skill for drilling in deeper and deeper water. If in the future after an academic geophysical program and additional Industry shelf drilling, it is judged that there is great scientific merit in a deep-water, deep-penetration scientific drilling program, it will be possible to design it in a thoughtful way. Since deep-water drilling technology will have advanced, it will be far less risky and perhaps cheaper than the proposed OMD program.

I recognize there are other aspects to the program such as resource assessment and technology development. However, these are always billed as bi-products of the scientific effort. I'm not able to judge their value but if they turn out to be the main driving force behind the program then the National Science Foundation should not be the lead agency.

Alternatives Suggested by

Dr. John Imbrie

Brown University

An Alternative Program

A. Setting priorities. What is needed to transform the present, diffuse plan into effective research strategy is an overriding principle that can be used to set scientific priorities. Such a principle emerges naturally from a consideration of the present status of the earth sciences in the context of the national energy crisis. This principle can be expressed as follows: Our first scientific objective should be to understand the structure and history of the continental margin of the United States. Moreover, this research should be conducted in such a way that attention is given first to water depths shallower than 2000 meters -- where the practical prospects for exploiting any reserve that may exist are relatively good -- and then proceed gradually into deeper water where exploitation prospects are now much poorer. As time and resources permit, other scientific objectives should be addressed later in the program.

B. Some guidelines for a restructured program.

1. Geophysical program. The geophysical part of the program should be funded at a higher level and given more prominence than it is in the Houston plan. At all depths, extensive, modern geophysical surveys, conducted by or in collaboration with academic scientists, should precede the planning for the drilling program. Surveys should include both

wide-aperture arrays to explore depths greater than can be reached by the drill, as well as narrow-aperture multi-channel arrays that will provide testable models for the drilling program. Funding of the geophysical program should be administered separately from the drilling program.

2. OMD drilling program. **Planning for drilling operations should follow extensive geophysical surveying. Drilling should commence in** waters shallower than 2000 meters, and use existing drilling vessels with **riser capability. Coring should aim at 100 percent coverage.** A decision to **use or not to use the Glomar Explorer for depths greater than** 2000 meters should be deferred until several years into the program, when both the scientific and engineering problems will be better defined. Hopefully, the normal progress of industrial drilling would by that time make the leap to abyssal drilling a less risky enterprise.

3. Phasing. The first phase of the OMD program would not be concluded until substantial progress has been made along three East Coast transects. A second phase, involving riser drilling to address scientific problems away from the U.S. continental margins, would then begin.

4. **Challenger program.** The Challenger-based coring effort **should be continued, at least during** the early years of the OMD program. I **addition to hydraulic piston coring,** this effort might well include crustal drilling and the investigation of non-U.S. continental margins. Research this kind is now planned for Challenger Legs 76-82. As a continuation of the IPOD program, a renewal of financial contributions from foreign countries can be anticipated.

Alternatives Suggested by

by

Dr. John G. Sclater

Massachusetts Institute of Technology

Background

The Challenger Project has been a great success and has had a new lease of life with the hydraulic piston core program and the deep and still open hole drilled about 500 m into ocean crust in the area of the Galapagos spreading center.

I view the OMD drilling program proposed at the Houston meeting as basically a continuation of this Challenger program onto the passive and active margins of the oceans and an attempt to extend crustal drilling to greater depths. This extension of the program to the margins and into thicker accumulations of sediments will require a major advance in technology and have a much greater cost. In view of the technology advancement and the cost it is necessary to re-evaluate carefully the scientific basis of the program.

I think the margins are an important area to study at this time. First, most continents are covered by over two kilometers of sediment and these sediments were deposited by processes analogous to those taking place at the margins today. As we believe we can tackle these margins in a quantitative rather than a qualitative fashion they are an exciting new area of scientific endeavour. Secondly, as there is a possibility of large accumulations of oil and gas any well posed study investigating how these margins were created would improve our chances of finding if and where such accumulations could be found. With the present shortage of oil and natural gas such research is obviously in the national interest.

Clearly eight major oil companies agree with this position. Given that they continue to support 50% of the project I think the science as proposed by the Houston group with certain qualifications worth the cost. As a result of these qualifications I would like to suggest substantial administrative improvements to the project.

1). The Program should be extended over a longer period and start later.

For budgetary reasons this appears to be happening already. However, there are other equally good reasons for slowing it down:

(1) it will enable completion of 2 years of hydraulic piston core drilling on the Challenger and a reentry and completion to maximum depth of the still open ocean crustal hole near the Galapagos spreading center,

(2) it will enable more and better studies to be carried out on the conversion costs of the Explorer, and

(3) it will enable a geophysics program to be developed and partially completed before any of the decisions are made as to where to drill the deepest and most expensive holes.

2). **The program should be restructured and also renamed.**

It is not just an ocean margin drilling program. It is an attempt to apply geophysical and drilling techniques to solve major problems on the ocean margins and in the deep sea. I suggest that to reflect the importance of the geophysics to the program that the $118 million for science be split into two parts.

(1) **$70 million should be separated completely from the present** budget and be given to another program to do the broad based scientific geotraverse work necessary for picking good drilling sites. This project should be given a separate name. Continental Margin Geotraverse (CMG) is an obvious suggestion.

(2) $48 million should be left within the present project to cover site specific geophysical work and other science.

3). The Continental Margin Geotraverse Project

This project allowing for 10% inflation over ten years would cost around $5 million/year at 1980 dollars. It would have a slightly increased budget early in the project when most of the geophysical data was being gathered and a slightly reduced budget at the end when the project was nearing termination.

At present one of the oceanographic institutions (Lament) has proposed to the National Science Foundation and ONR to build and equip a 200 channel, 10 km long, multichannel array for the academic community. This array which is a step beyond the state-of-the-art of industry will enable academic scientists to tackle many problems not soluable with present equipment. The budget estimate is on the order of $9 million dollars. It will cost a further $.5 million to run and $.25 million in processing for each month at sea (costs estimated from Continental Margins Report, page 16, line 10, operating costs $18 million divided by 24 months). Five million dollars a year plus what is already being put into acquiring this data by other branches of NSF and ONR will enable the academic community to run a state-of-the-art multi-channel system for six to eight months each year and do other complementary geophysical surveys (seismic refraction and gravity) in the same area.

Such a program if set up on a national basis (as is the present Challenger program) would be able to tackle the margin geotraverses mentioned by the Continental Margins Report as well as providing the basic geophysics for future drilling. Further it is unlikely that the academic community could handle a larger project than the one I have outlined due to manpower and processing limitations. Thus this project would fulfill much of the goals of the Continental Margins Report (Bally Report).

4)* <u>The Drilling Program</u>

The drilling program should take place after:

(a) the basic geotraverses necessary for adequate site selection have been completed,

(b) the cost wells now available on the slope and some industrial wells that will be released next year have been worked up and

(c) a reasonable and believable estimate of the cost of the Explorer has been worked out.

A rough scenario in my own mind is that, if the project starts in 1981, the multi-channel seismic ship for the geophysical community will take 3 years to complete and 2 years thereafter, in conjunction with other geophysical programs, will have produced the necessary background data for site specific geophysics and drilling. Thus drilling on the shelf or rise would start around 1986 or 1987. I believe this represents a delay of two years to the present program.

5) Possible political problems with present structure.

If the project goes ahead it could well founder in the near future because of lack of industry support. With the present structure the whole project would fold.

This does not have to be the case. If my suggestion of splitting the program into two parts (it could be two separate projects or one project with two clearly defined parts) were followed then, if the oil companies pull out and half the money disappears, the project doesn't have to fold. First, the continental margins geotraverse project could continue. It will cost significantly less per year than NSF is now contributing to the budget. Second, what money is left in the NSF budget could be put towards drilling holes in shallow depths with presently available conventional drilling technology. Though this would be a blow to some of the major goals, the program would not be completely wiped out. Personally, I view the geophysical traverses on the margins to be as important scientifically as the actual drill holes themselves. Thus I do not think the loss of the deepest holes should be considered a mortal blow to the project.

Alternatives Suggested by

Dr. Tj. H. van Andel

Stanford University

My program alternatives are as follows:

1. Implement continental margin transect studies and associated programs of the Bally report for the required amount of time.

2. **Strengthen in a major way geophysical capabilities of the oceanographic institutions with truly modern geophysical ships, instrumentation and processing techniques including multibeam echosounding and nearbottom survey instrumentation.**

3. Continue a Glomar Challenger (or similar ship) program of drilling, with heavy emphasis on the HPC. This one, likely to be the ultimate blossoming and reward of the DSDP I would regard as one of the highest priorities in the marine sciences today.

4. Close down DSDP in 2-3 years time with completion of 3).

5* Reassess the need for margin drilling and the state of available technology toward the end of the 1980s when the program under 1 has been completed and digested.

This strikes me as a sensible and properly ordered program taking advantage of the state of the technology, of our present ability to state in operational terms what they key problems are, and logically continuing to take the main trends to where they may lead. All this without extraordinary strain on budgets and other resources. I would like to add that all reference to the resource importance, whether energy or minerals, of the OMD seems to me quite strained. All potential resources are just that, not realities , something perhaps for 20-30 years from now. I do not believe and, apparently, neither do the oil companies, that a real case can be made that the OMD program will significantly advance our access to these resources.

I believe that this approach maintains the momentum created by DSDP at the point where it is greatest (where the questions have been most clearly stated) , that it tackles the continental margin program where the largest return can be found (see Bally report for justifications) and that the total cost is commensurate with priorities of the total national earth sciences program. It is futher a program of manageable size and one that should be comfortably cost-effective. I DO NOT SEE IT AT ALL AS WHOLESALE NEGATION OF The OMD; on the contrary, I believe that it is the essential transitional step and that a responsible OMD is nhot possible without it. I am familiar with the sayers of doom who claim that , once terminated, no marine drilling program will ever by resurrected. I do not believe that that is true; after all, such a program was once erected and that in the face of the Mohole disaster, not actually a very invigorating climate. I believe that insisting on the <u>drilling phase</u> now is equivalent to claiming that continuity is more important than necessity or quality.

The NAE/Marine Board report has questioned the current timetables, and the budget flap we are finding ourselves in is likely to lead to further extension I do not think that extending the time table by a couple of years will help a lot , because these extensions will only yield the budgetary relief required by higher than expected costs and larger than anticipated national reductions in the investment in R&D. Consequently, extending the calendar will not do what is necessary, namely to do some other things first, and not begin this costly venture until we are surer of what it is we need to do and have a better (and cheaper) handle on the technology.