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Trends in Scientific Research

Rapid evolution of the frontiers is a hazard for scientists young and old.

Philip H. Abelson

Scientific research is in the midst of rapid evolution. New opportunities for research effort develop and are quickly exploited, and often their potential is soon exhausted. The rapidity with which changes occur poses problems for the student who desires to make a career in scientific research, either fundamental or applied. It also challenges the universities, for they have a responsibility to educate their students for the realities that may develop in the next 40 years. Therefore it is important to ask where science is heading and to attempt to identify the forces that are shaping its future.

I will begin by arguing that it is possible to identify major trends in many areas of science. Although no one can foresee in detail when or where specific scientific discoveries will be made, trends likely to have continuing influence for an extended period can be predicted. It was clear in the early 1930's that the invention of the cyclotron and of the Van de Graaff generator made atomic nuclei accessible to experimental studies. The characteristics of the cyclotron were such as to encourage optimism that higher energies could be obtained by constructing larger devices. By 1933 or 1934 it was obvious that a rich field for continuing study had been opened. Soon the discovery of useful artificial radioisotopes of sodium, phosphorus, and sulfur made it apparent that tracer isotopes would be important in the development of many areas of science, particularly in chemistry and biology.

Later, at the end of World War II, it was obvious that there would be a large effort in nuclear physics, because machines of higher energy could be constructed. The nuclear reactors developed during World War II were destined for intensive improvement and application to atomic power.

Perhaps less obvious were the beginnings of development of molecular biology. The first steps toward the recent successes in cracking the genetic code were taken shortly after the end of the war. At a course in viruses started at Cold Spring Harbor in 1946, many competent investigators, some of them physicists, were trained in new techniques. From this group came much of the impetus that set the intellectual stage for present activities.

Exhaustion of Research Possibilities in Some Fields

An important phenomenon in science is the exhaustion of fields of inquiry. Given a set of techniques and concepts, fields do become mined out. As an example from the borderline of chemistry and physics, consider the

The author is editor of *Science* and director of the Geophysical Laboratory, Carnegie Institution of Washington, Washington, D.C. The article is based on the Third Annual Klopsteg Lecture, which he delivered at Northwestern University 6 November 1963 under the title "Where Is Science Heading?" Parts of the article were included in talks to the University of Maryland chapter of Sigma Xi (25 April 1963) and the National Association of Science Writers in New York (11 September 1963).

identification of isotopes of the elements. This was an active area in the 1920's and '30's, but nearly all the stable isotopes of the elements have now been discovered. There are few—if any—stable isotopes of the elements still to be found. That work is essentially completed for all time.

In geology, much of the important information that can be gained through standard geological exploration techniques has been acquired. In biology, work on the descriptive gross morphological aspects seems to be practically completed.

A further development in the mining-out process has occurred in recent years, namely, a trend to more rapid exhaustion of the fundamental research potential of fields. This can be seen in recent studies of the noble-gas compounds. Only about a year ago the first xenon fluoride compounds were described. Since then more than a hundred papers have been published on the subject. Considering the detail in which the compounds have been studied, it would seem that within another year little more will remain to be discovered about these substances with techniques now known.

Paradoxically, intense activity and the rapid exhaustion of new, accessible areas of scientific research have not been accompanied by marked alterations in the basic structure of most of science. For instance, there have been changes in only minor aspects of the structure of physics, chemistry, and geology during the past 10 years. The great activity in high-energy physics has not altered our concepts of large parts of physics, nor has it much affected our view of phenomena in the range of nucleon-binding energies. Solid-state physics has interacted constructively with the rest of physics, but the impact has not been great. By and large, the fundamentals of physics have not changed profoundly during the past decade.

Much the same is true of chemistry. The basic principles that guide the chemist have altered only slightly. Progress in gaining detailed comprehension of mechanisms and kinetics of organic chemical reactions has modified, but not revolutionized, the preexisting structure of knowledge. The noble-gas compounds were a surprising development, but their discovery has not overturned the periodic table of elements.

Much has been said of the great expansion in science both of numbers of men and of publications. We are told that 90 percent of the scientists of all time are living today. Analysis of rates of publication shows that most of the entire scientific literature of the world has been published in the last 10 years.

Science is doing more research, and is publishing more, but it is accomplishing less in some of the classical disciplines than at any time during the last 100 years. A major factor in this situation is that motivations for scientific research have changed. Much of the current activity is devoted to applied research, in which field it has been notably successful. This effort exploits the treasury of fundamental knowledge that has been accumulated over centuries. The permutations and combinations in which this knowledge can be assembled and used are innumerable. Applied research will remain vital for a long time.

As a scientist I am most concerned with research devoted to understanding nature. In what immediately follows I shall discuss opportunities in that area. I am quite optimistic about the potential of such efforts in the next decade. The development of new instrumentation, the transfer of technique from one field to another, the so-called multidisciplinary studies, will enable science to continue to grow in depth as well as scope.

Opportunities Created by New Apparatus

The availability of federal funds for research has had special impact on the general availability of scientific instruments. I have no knowledge of detailed studies of the fate of money received by investigators, but from examining hundreds of grant proposals I would guess that about half the funds are expended in equipment and supplies. Many vigorous young organizations have come into existence to exploit the new opportunity and are creating new kinds of equipment for this expanding market.

Much of the equipment that is creating new research opportunities is produced commercially; some is specially constructed. A small sampling of various disciplines shows that all are benefiting from instrumentation and new apparatus. Astronomy is a good example. New apparatus has led to the creation of radio and radar astronomy. The development of a sensitive infrared device capable of detecting a quantum of infrared radiation has made possible the discovery of hot spots on Venus, and it will undoubtedly find other applications.

Space research utilizes recently invented equipment, including launch vehicles, satellites, and instrument packages. In nuclear physics, discoveries are dependent on the accelerators or the detectors, such as bubble chambers or spark chambers. Chemistry has become increasingly dependent on instrumentation: in some laboratories whole programs are built around the use of gas-liquid chromatography; studies of natural products are aided by new equipment for measuring optical rotatory dispersion; most organic chemical research laboratories employ nuclear magnetic resonance, which permits unique assignments of structure to complicated compounds. Perhaps nowhere has the impact of instrumentation been greater than in biochemistry and molecular biology. Many of the important advances of the past few years have stemmed from the use of column chromatography, the analytical ultracentrifuge, radioactive tracers, or the amino acid analyzer. Even the behavioral sciences have felt the impact of new equipment. Some psychology departments today use more electronic equipment than most physics departments used a decade or two ago.

Electronic computers are having and will have many applications in practically all branches of science. They are especially useful in high-energy physics, space research, geophysics, crystallography, medical studies, and the behavioral sciences.

Up to the present, research workers have employed standard models of computers for their work and have designed their research to fit the capabilities of the machines. Ultimately the needs of various branches of science may lead to computers designed for maximum effectiveness in specific uses. This could be an extremely important development. Some feel that further developments in mathematics could change methods of presenting problems to computers, rendering the machines more effective in handling many scientific questions.

The impact of instrumentation and new apparatus is likely to increase. A visit to any instrument show makes it evident that the stream of new devices is flowing strongly, with no signs of abatement. For a time American manufacturers were dominant in development and production. Recently vigorous foreign competition has appeared, which will tend to spur our industry to even greater efforts. There must be limits to what instrumentation can do in expanding research potentials, but the bounds are not yet visible.

A second method of creating new frontiers in research is the application of techniques from one field in another. A notable example can be cited from solid-state physics. Twenty-five years ago most physicists knew no chemistry and were proud of the fact. Today, several of the most important fields in physics are dependent on chemistry. Transistors require very pure chemicals, and the new lasers involve the preparation of special materials with controlled impurities. Without chemistry there would be no solid-state physics. Perhaps of more lasting significance has been the development of truly multidisciplinary studies (*1*). Some of the most vital research areas today are biochemistry, biophysics, geochemistry, geophysics, cosmochemistry, and astrophysics.

Having noted some major influences at work in science as a whole, let us look more closely at some of the various sciences.

Physics

Historically, research in physics has been strikingly successful by three different measurements of value: Results have had great philosophic significance; the new knowledge and technique of physics have illuminated other areas of science; and important applications have been found. Discoveries in physics have been particularly important in shaping the rest of experimental science, largely because the behavior and interconversion of various forms of energy, especially at the electron volt level, are crucial to so many phenomena. During the 1920's and '30's theoretical chemistry drew much from developments in physics. In turn, other areas of science were nurtured by new developments in chemistry. Nuclear physics, too, during part of its development, interacted fruitfully with other sciences.

The discovery of the neutron and artificial radioactivity in 1933, together with the development of particle accelerators, opened new fields of inquiry. Very quickly the potentials of radioactive tracers became apparent. The role of nuclear reactions in cosmogeny was established, and soon the practicality of various applications of nuclear energy was outlined. These developments have influenced the evolution of almost all science and much of technology.

The residue from the last 10 years of very-high-energy nuclear research is not so impressive. Although studies in this area have been supported on a level at least 100 times that of the 1930's, there have been no results leading to an important practical application, nor is any currently apparent. Interaction with other areas of science has been limited. Knowledge of spallation products arising from very-high-energy bombardment of nuclei has been important in the studies of products found in meteorites and thus in cosmology. High-energy research has also interacted usefully with investigations of cosmic rays. In more recent years there has been less new interaction, and nuclear physics has become more and more removed from the main currents of science.

Nuclear research has been one of the principal areas of research in physics and has commanded a large share of the nation's potentially creative talent. Thus it is not surprising that physics as a whole is no longer supplying leadership in science, and all branches of research suffer accordingly.

Those who are active in high-energy physics are convinced of the importance of their subject. They hope for discoveries that will illuminate the nature of fundamental particles and forces. Their enthusiasm guarantees continuing activity in their field.

A second major field of research in physics is solid-state studies. This area is especially vital, it seems destined to continue to be productive, and it has the advantages that result from its interaction with other areas of science. The intense light beams generated by lasers find applications in biology, and frequency doubling by lasers is creating new opportunities in optics. With maser technology we shall be able to measure time to *1 part in* 10^{14}, an unprecedented accuracy. Our present standard for time is based on the interval required for the earth to make one complete revolution around the sun. With the best measurements available it is possible to measure a year to only about 1 part in 10^9. Ultimately our time standards will probably be based on maser technology rather than on the present astronomical methodology. It will also be possible to improve the standard of length substantially.

The intensity of activity in some areas of physics indicates both the vigor of the fields and the problems that may lie ahead. About 500 companies are now conducting laser research. With so much talent at work there is a threat, although not immediate, of a mining-out of the field.

But developments in solid-state physics are not confined to masers and lasers. The past year has seen striking advances in superconductivity, from which have come the means for producing very high, very uniform magnetic fields. This development will find applications in other fields of research. Work on semiconductors is proceeding well, and new miniaturized electronic devices are now possible.

An aspect of academic physics that has been disquieting is the tendency for the research frontier to exert excessive influence on curricula. Some universities having a strong orientation toward research in nuclear physics tend to emphasize that subject in their graduate teaching. As a result, training in classical physics has suffered. Departments of engineering and branches of geophysics such as meteorology and oceanography complain because their students have little opportunity to obtain the appropriate basic physics training. Industrial and government research organizations report that it is difficult today to find young men who have adequate training, for instance, in mechanics. One of my friends who has an important position as a director of research said that he has trouble finding a young man who has had any more training than a course in statistical mechanics. He tells me that the young physicists he sees have trouble proving that a ball will drop.

Chemistry

Because it is central to research in many fields, pure and applied, chemistry will always be important. In current research, noteworthy developments in the fundamentals of chemistry seem to be infrequent; it is much easier to note areas in which chemistry is being applied in important ways.

Studies of reaction mechanisms and kinetics are claiming the attention of many first-rate chemists, who are enjoying considerable success in elucidating details of processes. Moreover, there is a vast number of chemicals, and the study of their reactions will surely go on for a long time. Great progress is being made in studies of catalysis. Polymers, both organic and inorganic, have proved to be such a rich field that study of them will continue to hold interest.

For the most part, however, the activities of chemists are likely to be most fruitful in applications to research in borderline fields or in industrial problems. Perhaps the most important academic frontier of chemistry is in biological studies, for example, photosynthesis, molecular biology, and biochemistry.

Another important area is materials research. Chemical technique forms the basis of new developments in superconductivity and in advances, for example, in nuclear reactors. Industrial applications of chemistry are extremely important, and new ones are always appearing. One of the great recent developments is the petrochemical industry. Means have been found to achieve all manner of chemical magic in the rearrangement of carbon atoms by catalytic means. Another important area is the pharmaceutical field. Even though acceptance of new products has slowed recently, the size of research staffs is a guarantee of further advances.

Earth Science

The earth sciences are in the midst of evolution. Although the potentialities for great discoveries by older techniques seem largely exhausted, there remain many questions, and the introduction of new instrumentation, new techniques, and multidisciplinary efforts makes the earth an object of great interest for study. A major development in earth science during the past 15 years has been the expanding activity in geochemistry and geophysics. This is evident in the changing nature of publications in these fields, and in the way the program of the annual meeting of the Geological Society of America is now dominated by papers involving the application of laboratory science to studies of problems of the earth. The trend toward multidisciplinary efforts is powerful and is certain to develop further.

Let us consider some of the opportunities for research in the earth sciences. One of the dominant questions is the history of the earth. By paleontologic methods of dating, it could be thoroughly studied for only the period of the last 550 million years. The development of five independent methods of dating by radioactivity has made it possible to date events throughout the earth's total history, including rocks as old as 3200 million years. Advancement in our knowledge of events early in the earth's history creates opportunities in the study of the origin and early evolution of life, a subject not fully accessible before now. Efforts in this area in large part take the form of studies of organic geochemistry. Since life began on earth, some 65×10^{20} grams of organic matter have been deposited in rocks. This organic matter has been made particularly available for study by the invention of new laboratory tools, such as gas-liquid chromatography, the amino acid analyzer, and electronic means of measuring optical rotatory dispersion.

Another great opportunity is certain to unfold through the application of exploratory drilling for research purposes. A major untapped potentiality is study of the rocks under the ocean bottom. These rocks, representing 70 percent of the area of the earth, have scarcely been looked at. The recently successful Mohole drilling indicates that it is technically feasible for man to drill into the bottom of the ocean, even in the deepest of water, and to bring back cores for study. Such cores could reveal a great deal of the past history of life, could help settle the question of continental drift, and might well lead to great economic opportunities.

Even the continental crust has been only partly explored. The sedimentary basins where there is hope of finding petroleum have been investigated extensively, but rocks underlying the sediments have not been well explored.

The chemistry of the earth and especially the chemical processes that have occurred constitute an important field with many discoveries to be made. The earth itself is a huge laboratory, in which chemical changes are constantly occurring. These changes lead to unusual concentrations of large quantities of relatively rare elements to form economically valuable ore deposits. Our understanding of the processes involved is still primitive. Hidden ore deposits pose a challenge to the geophysicist to devise better means for locating them. Man must forever be interested in the study of the earth; economic necessity alone guarantees that. The earth is one of our great resources, and its chemical content must increasingly condition life upon it. Accordingly, man will make every effort to understand the processes that alter composition, not only on the crust but even in the deep interior of the earth. In the depths lie great mystery, for the depth to which man can send exploratory equipment is limited. Yet he will continue to probe the problem by geophysical methods, including seismologic investigations and laboratory studies to determine the behavior of materials at very high pressures.

Three other branches of geophysics present problems of considerable scientific interest and great practical challenges. These are oceanography, meteorology, and hydrology. Much of the ocean is not well explored, yet it is important to us in many ways—one being its potential role as a source of food. The challenge of meteorology is that we may hope to understand enough about the weather to be able to predict it and even ultimately to control it. A growing water-resource problem is forcing us to become more competent and interested in hydrology. We cannot live without water, and civilization does not function well on limited supplies of it.

Space Science

Exploration of the upper atmosphere and space commands great attention. The region relatively close to the earth, that is, in the first few hundred miles above the earth, is particularly important to us. We want to know, for instance, the atomic chemical species, the nature of the charges, and the electric and magnetic fields, and particularly how these are altered by solar activity.

Work on these problems is moving rapidly. It has attracted first-class talent, and with the new satellite equipment and rocket sounding equipment is making very great progress. Recently I have learned of some experiments involving flying a mass spectrometer to high altitudes. This would be a relatively certain way of discovering the composition of inner space. With excellent men and fine support there is little doubt that in the next 5 or 10 years we shall see the solution of many of the important problems of this region. One limitation is that the solar cycle lasts for 11 years, and so there may be delay in solving some of the problems simply because the phenomena that the sun can exhibit do not all occur within a few years. Furthermore, the interaction of the sun is yet so mysterious and so variable that several solar cycles may be required before the research possibilities begin to be exhausted.

Space is a great frontier, but it too has its limitations in terms of numbers of potential puzzles. In the interplanetary regions there is a very high vacuum; it is affected from time to time by bursts of solar plasma, but mainly it is high vacuum, which means that not much that is tangible is there. As for the moon and the planets, we can estimate what their composition will be, from an elemental standpoint. We already know that many of the chemicals on the moon and the planets are similar to those on earth. Obviously, there must be some differences. For instance, we know that the lunar surface has been acted on by radiation from the sun, including ultraviolet light, x-rays, and particles, all of which can have chemical effects. The surface of the moon may well have a surprising composition. Once a sample has been examined, however, the mystery will disappear. My estimate is that the scientific puzzles to be studied in space are of less consequence than those of the major disciplines here on earth. It will not be possible to mine out the scientific values of space for a long time, simply because of the cost and the inherent time delays. In the Ranger program, many of the experiments were planned in 1958, and there has not been, as yet, a successful Ranger experiment. In general, there is a long interval between initial planning and execution. Even when the vehicles become more reliable there will be inevitable delays. Once a set of facts that might be useful in planning a new experiment is learned, a period of time must elapse before more elaborate experiments based on the newly acquired data can be carried out.

Biology

On a large over-all view, some of the greatest challenges in science today are in biology. With the development of large-scale support and a vast new array of instruments, with the application of chemistry and physics to biology, and with the fine talent that is now devoted to biology, it is certain that biological sciences have some of their most flourishing years ahead of them.

During the past 20 years the frontiers of biology have shifted. Work on the descriptive features of gross morphology has largely been completed, and the rate of discovery and description of new species has slackened. The older biology has given way to activities involving new laboratory techniques—as can readily be seen by reading the program of the annual meeting of the American Institute of Biological Sciences. No matter what the subdivision of botany or zoology, examination of the abstracts shows that much of the material reported is based on work with radioactive tracers, the electron microscope, or other laboratory tools.

The area in which most vital advances are being made today is molecular biology. The details of the genetic code are being worked out, though this problem may be more difficult than was anticipated. Very active research is being carried out on the mechanisms by which information in the chromosome is converted into an ordered sequence of amino acids in proteins. Success in this area has stimulated additional interest in the processes by which the fertilized egg differentiates and grows into an adult animal. Interest in this important field has always been great, but earlier techniques have produced about as much information as they could furnish. The application of all kinds of biochemical approaches is the key to revitalization of this area.

Parts of biochemistry have become mature. The past 30 years have witnessed intense effort in the identification and purification of enzymes and in the various detailed series of biochemical steps in which the enzymes are employed. Much of this work is now finished. Current pioneering efforts in enzymology are largely devoted to the study of isozymes, to the determination of protein structure, and to discovering active sites on enzymes.

There will be continuing activity in the determinations of amino acid sequences in proteins. When sufficient information is available, some great generalizations must certainly emerge, and it should be possible to erect a phylogeny based on amino acid sequence. Another possibility for obtaining phylogenetic information arises from comparative studies of deoxyribonucleic acid (DNA), which have been made possible by the development of special DNA-embedded agar columns. Studies providing a biochemical basis for phylogeny will require a long time, but they should be richly rewarding.

The origin and early evolution of life are among the great puzzles of science. A number of scientists have followed the path of Oparin, Urey, and Miller and have elaborated on the number of simple organic molecules that can be prepared by abiologic means. The work has not, however, taken into complete account the realities of the natural environment or interactions among the compounds themselves. Moreover, there is a long road between amino acids and a reproducing entity. Nevertheless, sometime, somehow, man will devise systems that will fill much of the gap. What seems most needed in this particular field now is a new approach. This could come any day and might well open an exciting area for experimentation.

Perhaps the greatest research frontier of our times is investigation of the human mind and the way it functions. There is increasing evidence that very fine scientists from many fields are beginning to concentrate on this problem. The area seems difficult, but its importance guarantees first-class efforts. Related to the study of the mental process is work in the behavioral sciences. It would be bold to suggest that this field can be conquered by laboratory science, but major contributions will be made and tools such as electronic devices and computers will be helpful.

Research in biology will be influenced to some degree by the increasingly important practical problems involving it. Practical aspects have long been exemplified by medicine and agriculture. Among the new developments that could affect all mankind profoundly one of the most important is the population problem. It has been pointed out repeatedly that the world cannot indefinitely sustain a growth in population at the current rate. Ultimately, maintenance of that rate must lead to an enormous disaster. Some kinds of controls seem inevitable. They, in turn, mean further research on reproduction. The use of sperm banks raises the possibilities of eugenic control and creates great moral issues. Some new experiments indicate that man may come to be in a position deliberately to change the genetic content of developing embryos. Achievement of such a possibility would give man more power to control his future, but it would also demand far more wisdom than he has so far seemed able to apply. Another factor that will influence biological research is the long-term social value of curing diseases like cancer and mental illness. The emotional and financial costs of cancer on a worldwide basis each year can be measured in hundreds of billions of dollars. If cures for this disease could be achieved, they would be worth an enormous sum to humanity during coming generations. Realization of the great long-term economic value of amelioration of disease is certain to provide the basis of support for much biological research of the foreseeable future.

Training New Scientists

In this résumé of important trends in research it is apparent that almost all active fields involve multidisciplinary effort. Opportunities in some older disciplines seem limited. With the fast-shifting nature of research frontiers, it is apparent that the young student is faced with a difficult problem in preparing for research. If he specializes too early and too completely he may find that much of his knowledge is obsolescent even before he finishes graduate school. The situation calls for flexibility, and for a mastery of the fundamentals of two or more disciplines.

In the end, almost any research must take into account energy and its interconversions, chemicals and their reactions. Thus, to be adequately prepared, a person planning a life in scientific work must have the fundamentals of physics and chemistry together with the necessary mathematics. He must be able to express himself, which means a solid grounding in English. Given such a foundation, he can then master the details of a special subject matter such as earth science, biology, or behavioral science and be in a position to evolve wtih the changing opportunities. This training also prepares him broadly for industrial applied research.

The universities have a special responsibility. They must ask themselves whether they are preparing students for the 1980's or for the 1940's. Many schools are training their students for the 1940's. The curricula call for far too much specialized training. The student is overloaded with required courses in his specialty. He is given neither opportunity nor guidance to train himself broadly. Indeed, some departments consider a student disloyal and rather undesirable if he indicates a wish to take too many courses elsewhere. Moreover, the prejudice is usually amply conveyed.

As long as universities are organized in departments along disciplinary lines such narrow viewpoints are certain to come to the surface. To meet the new challenges will require either a complete recasting of the administrative structure or at least the formation of interdepartmental arrangements designed to help the student, not to preserve the vested interests of the faculty.

Today we are living in an era of accelerating change. If the universities are to fill their traditional role of furnishing adequate education and guidance to the young, they must fully recognize and act on the challenges they face.

Note

1. The purpose of the Klopsteg Lectures is to help further the development of multidisciplinary science both in research and in education. The lectures have been endowed as an annual series at Northwestern University by Paul E. Klopsteg, a past president of the AAAS, formerly an associated director of the National Science Foundation, and for 16 years a member, and for 7 years chairman, of the Governing Board of the American Institute of Physics. The two previous lectures in the series were by Francis Otto Schmitt (M.I.T.), on "Biophysics: Wet and Dry," and S. S. Stevens (Harvard), on "The Pursuit of a Sensory Law."

NEWS AND COMMENT

Mohole: The Project That Went Awry (II)

In mid-1961, as Project Mohole entered its second phase, the ingredients for misfortune began to accumulate.

The experienced Bascom group, which had successfully conducted the West Coast test drillings, was on the way out; the AMSOC Committee, originator of the project, no longer wanted to be involved in day-to-day operations and had prescribed a more remote role for itself; and NSF was shopping for an engineering organization to design, build, and operate the vessel that would carry out Project Mohole.

But what was Project Mohole? Was it a quest for no more than a few lengths of rock core from the depths of the earth? Or was it a comprehensive drilling program that included the mantle among several of its goals? Closely tied to these questions was the issue of technique. Was *CUSS I* to be followed by the construction of a so-called "intermediate" ship, a vessel that could go deeper than the *CUSS* but not all the way to the mantle? Or was the ultimate ship to be built at once? Who was to decide? Was it the part-time AMSOC Committee, which got together no more than a few times a year; or was it NSF, which had to foot the bills and account for its activities to an often-querulous Congress? And, finally, if NSF did take the decision upon itself, would it not be venturing into proscribed territory? The Foundation was established to "initiate and support basic scientific research"; it was not intended to be an operational organization. Traditionally, a standing scientific or educational institution was the operating link between the Foundation and the research programs it supported. But with AMSOC backing away to a lesser role, the Foundation was drawing close to becoming the institutional base for Project Mohole.

A nasty and still unresolved fight was to break out on these issues, but in