DOCUMENT RESUME

ED 052 983 SE 012 118

AUTHOR Handler, Philip
TITLE The Physical Sciences. Report of the National Science Board Submitted to the Congress.
INSTITUTION National Science Foundation, Washington, D.C. National Science Board.
PUB DATE 70
NOTE 77p.
AVAILABLE FROM Superintendent of Documents, U.S. Government Printing Office, Washington, D.C. ($0.50)

EDRS PRICE EDRS Price MF-$0.65 HC-$3.29
DESCRIPTORS Astronomy, Chemistry, *Federal Government, Nuclear Physics, *Physical Sciences, *Policy, Science Facilities, *Science History, *Scientific Enterprise, Scientific Research

ABSTRACT

Recent advances in the physical sciences, including astronomy, chemical synthesis, chemical dynamics, solid-state sciences, atomic and nuclear science, and elementary particles and high-energy physics are summarized in this report to Congress. The nature of physical science, including its increasing unity, the relationship between science and technology, the connections between pure and applied science, and the importance of new ideas and instruments in planning new experiments and setting scientific priorities are examined. The relevance of the new advances and the nature of science to the well-being of the United States physical science research efforts are discussed. Both government sponsored and industrial research are reviewed, and both sectors are included in the 16 specific recommendations made by the Board. (AL)

The cover design is a "cosmograph": the product of an invention by Edward Lias. Cosmographs are visual records of patterns produced by interfering sound waves. This modern art form has limitless variations, because each combination of frequencies produces a different diffraction pattern.

THE PHYSICAL SCIENCES

REPORT OF THE NATIONAL SCIENCE BOARD
SUBMITTED TO THE CONGRESS
1970

NATIONAL SCIENCE BOARD
NATIONAL SCIENCE FOUNDATION

Library of Congress Catalog Card No. 77-605085

For sale by the Superintendent of Documents, U.S. Government Printing Office
Washington, D.C. 20402 - Price 50 cents

LETTER OF TRANSMITTAL

January 2, 1970

My Dear Mr. President:

It is my privilege to transmit herewith the second Report of the National Science Board, prepared in accordance with the provisions of Section 4(g) of the National Science Foundation Act as amended by Public Law 90-407. This Report is addressed to the present state of the physical sciences, their recent accomplishments, their apparent opportunities and challenges, and the requirements if these opportunities and challenges are to be accepted.

The physical sciences are the pacemakers of our civilization. With the materials and understanding they provide we are enabled to secure the national defense and construct a world in which our fellowmen are healthier, more comfortable, and more richly endowed, in which mankind is freed to pursue truly human endeavors. Research in the physical sciences today will, tomorrow, underlie more penetrating understanding of the nature of life in health and disease as well as find application in the countless aspects of engineering which translate scientific understanding into societal benefit.

As this Report recounts, our Nation has ample reason to be proud of its accomplishments in all areas of the physical sciences for the last two decades. Yet there is every reason to believe that the best and most rewarding science lies ahead. As in the past, each next step is more difficult, more complex, and more expensive than the last while the potential for application is seldom evident in prospect.

We recognize that the frontiers of astronomy, physics, and chemistry must appear remote from the immediacy of the problems posed by the environment and decaying cities or the complexities of foreign affairs. Yet we urge that our Nation not surrender its leading position in the worldwide scientific endeavor, that we continue in the search for that fundamental understanding which must constitute the principal legacy we may leave to succeeding generations as, in their turn, they seek to utilize the fruits of science to alleviate the condition of man. Although the precise manner of societal utilization of future scientific discoveries is unpredictable, there can be no doubt that to conduct scientific research is to construct a bridge to a brighter future.

But the magnitude of that effort must rest upon a balanced judgment of the opportunities and needs of the research endeavor on the one hand and the urgency of diverse alternative demands upon available national resources. At the same time, we are not unmindful of the danger to the national future if, in our anxiety to utilize science and scientists to combat the societal problems of the moment, we so reduce the pace and scope of the scientific endeavor itself as to fail to build a platform for tomorrow's applied science.

There are many important calls upon the public purse, and the support of science is one such. Decisions with respect to how the resources of the Federal Government are to be allocated are not a function of this Board but rather of the President and the Congress. Advocates of specific utilization of those resources must necessarily make the best possible case for those programs which they advocate. Only with such a background can the final adjudication occur.

It is precisely because other national needs are so compelling that the Board has here attempted to make the best and strongest possible case for the support of the physical sciences for consideration by those who must make the ultimate decisions.

It is to assist in formulation of these judgments, and in the hope that the seemingly urgent will not be permitted to obscure that which, in the long run, is the truly important, that this Report was prepared and is conveyed to you for transmittal to the Congress.

Respectfully yours,

Philip Handler

Philip Handler
Chairman, National Science Board

The Honorable
The President of the United States

CONTENTS

ACKNOWLEDGMENTS

The National Science Board is grateful to five consultants who joined with the Board Committee charged with the preparation of this report in the discussion and writing sessions which resulted in this document. These consultants are: Dr. Leo Goldberg, Dr. George S. Hammond, Dr. Nelson, J. Leonard, Mr. Victor McElheny, and Dr. Leonard I. Schiff.

The Board is especially indebted to Dr. Raymond J. Seeger (Senior Staff Associate, Office of the Assistant Director for Research), who served as Executive Secretary to the Committee; Dr. William E. Wright (Division Director, Mathematical and Physical Sciences), Staff Liaison to the Committee, and Dr. M. Kent Wilson (Head, Chemistry Section) for the final editing of the report.

The Board also received assistance from many individuals in the Federal Government, educational institutions, private industry, and professional societies.

The following Government agencies provided representatives to participate in a meeting concerned with the preparation of the report and to review a draft of it: Atomic Energy Commission, Department of Agriculture, Department of Commerce, Department of Defense, Department of Health, Education and Welfare, Department of the Interior, National Aeronautics and Space Administration.

The individuals noted below provided the Board with a wealth of ideas and information through written material, meetings, or comments on the penultimate version of the report: Dr. W. O. Baker (former Member, National Science Board), Vice President—Research, Bell Telephone Laboratories, Inc.; Dr. Karl H. Beyer, Jr., Senior Vice President, Research Laboratories, Merck, Sharp and Dohme; Dr. Arthur M. Bueche, Vice President, Research and Development, General Electric Company; Dr. Theodore L. Cairns, Assistant Director, Central Research Department, E. I. duPont de Nemours and Company; Dr. Paul F. Chenea, Vice President, Research Laboratories, General Motors Corporation; Dr. A. M. Clogston, Director, Physical Research Laboratory, Bell Telephone Laboratories, Inc.; Dr. Richard Crane (Chairman, Physics and Society Committee, American Institute of Physics), Professor of Physics, University of Michigan; Dr. Milan D. Fiske, Manager,

Physical Sciences Branch, Research and Development Center, General Electric Company; Dr. Wayland C. Griffith (Vice Chairman, NSF Advisory Commitee for Planning) Vice President for Research and Technology, Missiles and Space Company, Lockheed Aircraft Corporation; Dr. W. E. Hanford, Vice President, Research and Development, Olin Mathiesen Chemical Corporation; Dr. Milton Harris, Chairman, Board of Directors, American Chemical Society; Dr. E. D. Kane, President, Chevron Research Company; Dr. Irving Kaplansky (Member, NSF Advisory Committee for Mathematical and Physical Sciences) Professor and Chairman, Department of Mathematics, University of Chicago; Dr. J. Ross MacDonald, Vice President, Corporate Research and Engineering, Texas Instruments, Inc.; Dr. Oscar T. Marzke, Vice President, Fundamental Research, U.S. Steel Corporation; Dr. Wendell C. Peacock, Research Scientist, Beckman Instruments, Inc.; Dr. Roland W. Schmitt, Research and Development Manager, Physical Sciences and Engineering Research and Development Center, General Electric Company; Dr. Verner Schomaker (Chairman, NSF Advisory Committee for Mathematical and Physical Sciences), Professor and Chairman, Department of Chemistry, University of Washington; Dr. Frederick T. Wall, Executive Director, American Chemical Society; Dr. Albert E. Whitford Professor of Astronomy, University of California at Santa Cruz, and Former Director, Lick Observatory, (President, American Astronomical Society).

The Advisory Committee for Mathematical and Physical Sciences of the National Science Foundation also discussed the report at two of its meetings.

Finally, the Board is indebted to Mrs. Lois S. Niemann, Administrative Assistant to the Vice President for Instruction and Research at Louisiana State University, and to the many persons at the National Science Foundation who offered advice and counsel and especially to Miss Helen Potter and the National Science Board Office for other editorial and secretarial assistance.

SUMMARY AND RECOMMENDATIONS

Preamble

The underlying premise of national science policy for two decades has been that continued strength in science and technology is essential to the welfare of the Nation and its influence and leadership in the world. We believe that policy to be still valid. No one can guarantee how rapidly scientific knowledge may become applicable to the problems which beset our world. Scientific knowledge alone is not sufficient to ensure that solutions will be found or implemented. The National Science Board firmly believes, however, that scientific knowledge and understanding are necessary, and that the steady advancement of science is essential if the potential applications of science are to be realized in the most timely, productive, and economical fashion.

Therefore, the National Science Board begins by stating what it believes to be the basic tenets of United States science policy:

a. The United States will strive to remain competitive at or near the forefront of each of the major areas of science and, to this end, will continue to identify and support scientific excellence.

b. The Nation is committed to the principle that every young person should have the opportunity to pursue advanced education to the extent of his ability and motivation irrespective of geographic origin or economic means.

c. The Federal Government has a responsibility to ensure that new scientific knowledge is utilized as rapidly and effectively as possible in support of national goals and for the welfare of the world's peoples.

The National Science Board supports and commends efforts by the scientific community to address major problems of our society. At the same time, the Board is concerned that scientific endeavors intended to enhance the long-term national future not be sacrificed to the urgencies of the day. Accordingly, the Board recommends that future planning for the total Federal support of science through all agencies strive to be commensurate with the three tenets above.

A clear recognition within the Federal Government that the pursuit of science as a national mission is imperative to the achievement of these ends. The future of the country requires the advancement of science, and the advancement of science explicitly requires the advancement of the physical sciences. Many of the following recommendations, however, do not apply solely to the physical sciences. The more general recommendations are given first.

RECOMMENDATIONS

1. Excellence in science is a national goal and should be explicitly so considered by the National Goals Research Staff. Further, the National Science Board expresses its desire to participate in the preparation of a Government-wide plan for the realization of this national goal.

2. In the continuing process of establishing scientific priorities within the political sector, including actions by the Congress and the Bureau of the Budget, there should be an even greater input by the scientific community through a variety of mechanisms.

3. Within the framework established by the political process, there should be assured support of the best research in the physical sciences and implementation of new ideas and programs of exceptional scientific promise. The potential for increase of fundamental understanding is not only the best criterion of scientific excellence but is also just that feature of science which is most likely to lead to new technology. This principle should continue to play a major part in setting scientific priorities.

4. The Federal Government should expand its programs of institutional and departmental support for graduate education and provide stable levels of support so that academic institutions can afford to take the initiative and make the commitments inherent in educational and research ventures and in supporting young researchers.

5. The United States scientific effort is currently threatened with possible mediocrity. Funding limitations currently

imposed by the Federal Government on scientific research should be lifted before the present vitality of the physical sciences, which is essential to the progress of all science, is lost. Support levels in the physical sciences should be made comparable to those recommended in the studies of the Committee on Science and Public Policy of the National Academy of Sciences in the fields of astronomy, chemistry, and physics.

6. The National Science Foundation is the only Federal agency whose primary mission is the advancement of science. Because of this mission, a substantial fraction of the necessary increase in research support should be channeled through the National Science Foundation to provide greater stability and balance to the total national effort and to give the National Science Foundation opportunity for greater initiative in the development of research programs in the physical sciences.

7. All agencies should continue to give special attention to research programs in the physical sciences which support individual investigators and small groups in such fields as chemistry, solid-state science, atomic and molecular physics, and the smaller research projects in astronomy, many of which are now underfunded to the point approaching stultification. These programs, which are highly competitive, of prime scientific and technological significance, and particularly adaptable to the training of graduate students in these fields, make an enormous contribution to the physical sciences and often establish the essential groundwork for larger and more complex efforts. It is ill-advised to fund such programs at a level at which a majority of high-quality proposals must be rejected or underfunded.

8. The acquisition and construction of new instrumentation is the pacing item for research in much of the physical sciences. Radio astronomy alone requires an investment of approximately $200 million in major new facilities in the next ten years. Commensurate efforts must also be made in chemistry, physics, and optical astronomy. Plans for major new facilities should include realistic long-range

plans for operating support, including provision for successive generations of auxiliary instrumentation, and for periodic updating of the equipment. These long-range plans should be consistent with the three- to ten-year period required for the design, construction, and bringing into operation of major facilities.

9. Expensive research facilities, including instrumentation, should be established as national or regional resources. Basic responsibility for the creation and operation of such facilities can be vested in single institutions or in groups of institutions. The pattern of users groups, such as those operating in high-energy physics, should be encouraged to spread throughout the physical sciences. Federal agencies should be prepared to bear part of the added cost of utilization of such facilities as a trade-off against duplicating facilities and expensive instrumentation at additional locations. The importance of first-class resident staffs at all large facilities must not be overlooked. Systems of peer judgments, however, should be employed at such facilities to insure their availability to the best scientists for the most significant experiments.

10. Federal agencies should continue to review older or less productive large facility installations for selective phasing out in order to relieve funds for building and operating new facilities which are closer to the forefront of developments in scientific techniques and capability. Large facilities, both old and new, should be continually supplied with the most modern sensing and data-processing equipment to assure their optimum use. Such modernization is requisite even at the expense of some existing facilities which are still useful.

11. The United States should continue to work for international participation in the planning and utilization of large research facilities, including exchange of scientists and complementarity of programs and equipment.

12. The Department of Defense, along with the other mission-oriented agencies, notably the Atomic Energy Commission, the National Aeronautics and Space Administration, and

the National Institutes of Health, should continue to support basic research in all areas of the physical sciences which show reasonable promise of having a bearing on their missions. The National Science Foundation should be provided with funds to assume support for those worthwhile ongoing research programs for which mission-oriented agencies may no longer be able to provide continuing support because of fiscal reasons or change in mission emphasis.

13. The present trend to decrease funding for the scientific aspects of the space program should be reversed. Provisions should also be made for more active participation by academic scientists in these programs, including adequate funding of the academic research groups. Funding must be assured also for supporting research and technology in physics, chemistry, and especially in optical and radio astronomy in order to ensure the greatest scientific payoff from the space capability. It is urgent to upgrade the scientific programs associated with future lunar landings, including the subsequent analyses of mission data and lunar samples.

14. The universities should intensify their efforts to adapt their graduate programs to the changing needs of industry, Government, and the educational system. Special consideration should be given both to the time requirements for the doctorate and to the establishment of "practitioners degrees" at the doctoral level in the physical sciences.

15. Additional and more effective ways should be found for industry, Government, and universities to cooperate in translating basic science into social utility and in opening up for basic research the new areas which are often suggested by technological problems.

16. An effort should be made to utilize more industrial and Government scientists on advisory panels which help select research projects for Federal support and on advisory committees which help develop national science policy.

REFERENCES TO THE TEXT

The following listing contains appropriate references to the text for the several recommendations:

Recommendation 2; pp. 35-38

Recommendation 3; p. 38; pp. 55-56

Recommendation 4; pp. 41-45; "Toward a Public Policy for Graduate Education in the Sciences," National Science Board, 1969

Recommendation 5; pp. 35-45

Recommendation 6; pp. 49-50; p. 54

Recommendation 7; pp. 39-40

Recommendation 8; pp. 39-40; pp. 55-56

Recommendation 9; pp. 51-54

Recommendation 10; p. 52

Recommendation 11; p. 56

Recommendation 12; pp. 49-51; p. 54

Recommendation 13; pp. 56-57

Recommendation 14; pp. 45-47

Recommendation 15; pp. 46-51; pp. 58-60

State of the Physical Sciences

In 1969, for the first time, man left the protection of the earth and landed on the moon. Whenever mankind looks back on the history of his achievements, this year will date that banner event. Why can this generation go to the moon when earlier ones could not? The answer is that man has accumulated a sufficient knowledge of the physical universe, an adequate control over the forces of nature, and a suitable technological and industrial enterprise to enable him to build and operate the instruments and vehicles needed. Solid-state computers solve the necessary problems in celestial mechanics and in orbit theory with great rapidity and reliability. The knowledge of how atoms interact with one another to form molecules enables chemists to produce exotic fuels whose reactions give rockets enormous thrusts that can still be delicately controlled. A knowledge of the properties of solids permits the production of materials which will withstand the forces and temperatures to which they are subjected in rockets and other space vehicles. Man's understanding allows him to design space vehicles which pass through the earth's atmosphere at escape speeds and to deal with such potential hazards to the space traveler as the solar wind and magnetic storms. Apollo 11 was, in this sense, a culmination of 400 years of progress in science as well as in technology.

Though man's flight to the moon assures 1969 a place in history, it will have several rivals for the position of the outstanding event of this century. The twentieth century will be known also as that age in which man discovered and learned to use nuclear energy. It will be known as the age in which the physics of the electron was employed to produce a communication system that allowed men to see and hear one another wherever they might be on the earth, or in space beyond the earth, and to produce high-speed computers which enabled men to utilize their brains in the same way that machines have made it possible for them to extend their brawn. It will be known as the age of chemical synthesis, when man, first in the laboratory, then in huge chemical plants, tailored molecules to fit his own purposes by rearranging atoms to produce materials with a variety of useful and pleasing properties undreamed of in earlier times. From these materials have come his clothing and fishing lines, his detergents and lubricants, his medicines, and even his food. It will be known as the time when man eradicated, or brought under control, many of the diseases which had plagued him throughout his history. We hope that the twentieth century may even yet be known as the one in which man finally eradicated hunger, in which he learned to control his population, to conserve his environment, and, most deeply, to live without war.

A. THE UNIVERSE

By 1969 the scientist has done more than demonstrate the application of his science. The vast laboratory of the universe is now more accessible to the scientist, and it presents him with a much extended array of physical and chemical circumstances. The gravitational force is mild on the earth but exceedingly strong near the dense and massive stars. The stars form galaxies and clusters of galaxies, which are the largest systems known to man. Starlight is generated by the interaction of nuclear particles, which are the smallest systems known to man. In the astronomical universe, temperatures range from a few degrees in intergalactic space to billions of degrees in the interior of stars. Densities range from a few atoms per cubic meter in space to more than 10^{15} times the density of ordinary terrestrial materials in neutron stars. Particle energies in the cosmic radiation extend at least to 10^{22} times the energy characteristic of molecules in air at ordinary temperatures. The study of matter and energy under these natural extremes and relating the results to those found in the much narrower range of the earth-bound laboratory are the joint domain of astronomy, physics, and chemistry.

Both in space and on earth, man's ability to observe nature with high precision and under extreme conditions presents the physical scientist with the most critical tests of his theories. The questions he faces are ever more difficult, and the means of answering are ever more complex and subtle. His attempts at understanding may be frustrating. If history is a guide, however, further development of our civilization will depend in part upon his success, and rich rewards will come to those peoples and nations whose scientists succeed.

Currently there is a scientific explosion in astronomy and astrophysics. The last decade has seen the discovery of "quasars" and "pulsars," X-ray stars and neutron stars, cosmic background radiations and cosmic-gas masers, infrared stars and infrared radiation from many cosmic sources, gamma-ray and neutrino astronomy, transuranic elements in cosmic rays and complex molecules in interstellar matter, and contemporary synthesis in matter ejected from stars. The detection of gravitational radiation from sources outside the earth has been reported. The rapid pace of discovery in astronomy and astrophysics during the last few years has given this field an excitement unsurpassed in any other area of the physical sciences. During the seventeenth century Newton's law of gravitation provided the major influence on the physical sciences. In the nineteenth century Mendeleev's periodic table filled that role. But today the investigation of these many recent major astronomical discoveries may provide a similar influence on the physical sciences. The current significant discoveries in the other physical sciences are, to a large extent, unanticipated consequences of known physcial principles and are fitted into a generally acceptable pattern of theory and experience. The new discoveries in astronomy have presented deeper mysteries and hints of physical processes more unusual than anything observed in the laboratory or predicted by current theory.

1. QUASARS

The discovery in 1963 of quasars created a revolution in the outlook of astronomers. The universe suddenly appeared more violent than had been conceived before. The vast energy production which the quasars appear to show cannot easily be accounted for by presently known energy-producing processes. The spectrum of their light is distorted in a puzzling fashion. Is this distortion due to the expansion of the universe? If so, quasars must be ten times farther away than the farthest galaxies previously observed. Or, does its origin lie in the intense gravitational field around the quasar? Or, is a quasar a much closer object that has been ejected with a speed almost that of light from the interior of a nearby galaxy? Even to approach these questions, one must use

373-722 O - 70 - 2

the viewpoint of Einstein's general theory of relativity. Twenty years ago that theory was regarded as the intellectual culmination of physical theory. Now it is the essential starting point for experiment and observation.

2. RELATIVITY

Our new ability to operate delicate instruments deep in space and our improved techniques for observing astronomical events with ground-based instruments offer us an opportunity to apply definitive experimental tests to the general theory of relativity. These experiments will involve such activities as the precise observation of the motion of extremely stable gyroscopes in earth orbit, refined detection and analysis of gravitational waves, and radar measurements of minute changes in the motion of the planets. Scientists in several United States laboratories are now engaged in the design and development of the sophisticated instrumentation needed for such measurements. Prototypes and test models of individual components are presently being constructed. The actual experiments usually will require very large installations, such as giant radio telescopes, or complicated space missions, as in the case of the gyroscope experiment. But if man is to achieve a fundamental understanding of gravity, such experiments must be done.

3. PULSARS

About two years ago a group of radio astronomers in Great Britain made a startling discovery, a new class of celestial objects that emit short pulses of radio energy at regular intervals of a few seconds or less, called pulsars. Subsequently, United States astronomers found the central star of the Crab Nebula to be a pulsar having all its visible light in the form of pulses coincident with those of its radio pulses and regularly spaced 1/30 second apart. These objects emit enormous amounts of energy, or they would not be observable at all. How could emission of such enormous amounts of energy be interrupted so completely and with such regularity? It now seems likely that the emission is not interrupted but that pulsars are rotating neutron stars which radiate directional radio, optical, and X-radiation beams. They are believed to be the most dense form of matter, compressed to the density of an atomic nucleus in the course of gravitational collapse following supernova explosions. Whereas the sun has a diameter of 864,000 miles, a neutron star with the same mass may have a diameter of only six miles. Since 1950, radio galaxies, quasars, and pulsars have presented man with challenging new phenomena for which no entirely satisfactory explanations are in sight.

4. OBSERVATION AND EXPERIMENTS IN SPACE

Unfortunately, in studying the universe, one cannot experiment in the usual sense. One can only observe; one cannot manipulate or alter the systems being observed because they are so vast and so distant. Observations of the universe, however, do often guide the design and performance of critical experiments on earth. They also frequently demonstrate processes which are useful to man's other purposes. For example, the observation that the apparent lifetime of stars exceed the capacity of then-known fuel supplies led to the inference of the existence of nuclear-energy sources—later identified with the nuclear processes which are a part of our atomic age. Also, the study of cosmic rays which impinge upon the earth from outer space has played a central role in high-energy particle physics over the past forty years and will continue to do so in the future. Many important physical entities—positrons, muons, pions, mesons, lambda particles, and charged hyperons—were discovered in cosmic-ray investigations. Until quite recently the ultimate validity of electromagnetic theory could be investigated only by the use of cosmic-ray particles. The most energetic particles in the universe, produced by processes not yet understood but certainly cosmic in scale, constitute one of the tools we use to unravel the smallest scale phenomena in the universe. There is an inverse relationship between the energy of the bombarding particles used to probe the structure of matter and the scale on which this structure can be resolved—the higher the energy the finer the resolution. Decisions for or against construction of nuclear accelerators larger than those which are now being built may possibly be based on the results of cosmic-ray studies made in space in the early 1970's.

B. THE MICRO-UNIVERSE

Systems that are either very large or very small are frequently the easiest to understand. Man derives inspiration and valuable hints about processes on earth from his study of the universe. His achievements in understanding the universe are closely tied with his ability to understand and manipulate matter with ever increasing precision and on an ever finer scale. Many of our human aspirations for the future depend upon our success in understanding earth-bound systems on a scale intermediate between the very smallest and the very largest. Our probing into the very large and the very small will continue to yield dividends, many unexpected, in the understanding of "people-sized" systems. This will come about both through increased theoretical understanding and through the development of experimental techniques which can later be adapted to different problems. Major contributions to man's ability to control his environment will come from his understanding of matter on the scale of atoms and at energies measured in electron volts. This is the domain of chemistry and of atomic, molecular, and solid-state physics. Furthermore, an increasing contribution will come from our ability to reassemble the results of studies at a molecular scale to bring them to bear on the understanding of systems at a higher level of complexity and organization, such as living organisms, populations, and natural and manmade environmental systems. This synthesis of results at the molecular level to provide an understanding of, and an ability to control, systems at higher levels of organization provides one of the great challenges to chemists, physicists, and engineers. For example, one cannot attack many of the fundamental problems of pollution without new and more delicate methods of chemical and physical analysis and without further elucidation of processes which produce pollution. Also, one cannot understand and predict the effect of trace impurities on human health and on the biosphere without deeper understanding of the biochemical processes in which they participate.

1. CHEMICAL SYNTHESIS

The vast array of manmade materials produced by the manipulation of molecules might lead one to believe that synthetic chemistry now constitutes an essentially complete system of knowledge. But such is not the case and much research remains to be done. For example, an important contribution to the problem of water pollution arises from the accumulation of synthetic detergents in our river systems. Some detergent molecules accumulate because they are not subject to degradation by micro-organisms in the water. Subtle changes in structure needed to make detergents biodegradable are now understood, but synthetic methods of producing the desired substances from economically attractive raw materials are not now known. In other problem areas progress is slow because structural modification of molecules is still something of a hit-or-miss affair, not because of obvious deficiencies in the synthetic methods, but because current theories relating chemical structure to material properties are not adequate to provide guidance.

Other synthetic materials hold promise for the future and will become productive as synthetic methods and theory relating structure to properties are further developed. For example, we ought to be able to produce synthetic materials with electrical conductivity equal to that of metals. Imagine the production of nylon-like fibers, finishing lacquers, and sheets of tough, pliable film having conductivity similar to that of copper or aluminum. Some might even display the remarkable characteristic of superconductivity. The range of new and useful electrical devices that could be fabricated from such new conducting materials defies imagination.

The prospect that chemical synthesis can and will produce more and more new substances having properties that will sustain, ease, and ornament men's lives is attractive, but a note of reservation is needed. We have not yet arrived at the point where it is always feasible to produce on demand a material having a desired property. Two further steps are needed to accomplish this objective. First, there must be an improved theory relating the properties of materials to their chemical structure. Second, there must be an economically attractive chemical path from available raw material to the desired end product. In general, satisfying either requirement cannot be guaranteed. Though much can be accomplished with the guidance of current theory or by trial-and-error methods, the potential payoff from deeper understanding is great. Such understanding offers more rapid and economical solutions less liable to unexpected side effects.

The preparation of new agents in medicine has been substantially expedited by recent developments in the methods of synthesis. New antibiotics such as modified penicillins are being produced by synthesis rather than by bacterial fermentation. The goal is to obtain compounds with lower toxicity and with greater selectivity against bacteria or with activity against a broader range of bacteria, or with activity against types of bacteria that have grown resistant to currently available antibiotics. Synthetic sex attractants make possible the elimination of particular species of insects, for example the cabbage looper, without the hazards to other forms of life inherent in broad spectrum agents. The present version of the "pill" depended heavily upon the development of new methods of synthesis and upon the availability of analytical instrumentation. Although better understanding of the social sciences is crucially involved, the chemical regulation of fertility by the use of synthetic compounds will greatly ease and facilitate the social engineering involved in limiting the human population.

2. CHEMICAL DYNAMICS

Chemical dynamics is the science of chemical change and complements structural chemistry. Structural chemistry deals with the static molecular organization of matter, and dynamics introduces time as an important molecular property. The dynamicist is concerned with the probability that one chemical structure will change into another and how rapidly these changes occur.

Chemical reactions occur with many and varied characteristics. Some reactions such as combustion processes release energy, while others can absorb and store energy supplied as heat, light, or electricity, as in storage batteries. Some reactions occur so rapidly that the average lifetime of molecules in a reacting system is a billionth of a second or less; other reactions occur so slowly that they cannot be observed easily during the life span of a man. The slowness of slow reactions arises from the time required for molecules to prepare for final action. These preparations may include the gathering together of partners for a reaction, the accumulation of some minimum energy content, or concentration of energy in just the right molecular vibrations to break existing bonds and form new chemical bonds between atoms. Furthermore, subtle changes in reaction conditions such as the presence of a catalyst often lead to enormous changes in reaction rates. Understanding and control of the rates of chemical reactions is a monumental task because it is necessary to work entirely with theoretical models and indirect evidence.

There are several reasons for expecting an acceleration of progress in chemical dynamics. Only recently have concepts clarified to the point where they could serve as the stimulus for definitive experiments. Two things had to happen before much real progress could occur in the application of quantum mechanics to the understanding of reactions. First, the complexity of the paths or mechanisms of many reactions had to be realized and at least partially understood. Second, experimental methods for the study of elementary reactions had to be found. Many processes do not occur in a single step but consist of a series of definable chemical reactions. One of the great accomplishments of the past four decades has been the dissection of many such complex reactions. The understanding of the molecular basis of visual excitation is a current example. As mechanistic analysis of such reactions has progressed, some puzzling facts concerning chemical reactivity have started to fall into place. Thirty years ago the literature was full of curious examples of compounds having seemingly similar structures that showed enormous differences in reaction rates. Many such differences now appear reasonable and systematic because careful consideration of the steps in a reaction has shown that small structural changes have a profound influence on the reaction rate in a key step.

Chemical physicists now have the tools for investigating the simplest reactions. In experiments with molecular beams, they can aim reactive molecules having known energy content at other molecules and measure the results from single collisions. The few systems that have been studied in this manner are so simple chemically that the results are of little immediate use to chemists working with the complex chemical substances of biochemistry and chemical industry. However, new experiments with molecular beams are being designed. When they can be carried out, giant steps will be taken in bridging the gap between understanding the simplest reactions and understanding the more complex ones of practical chemistry.

373-722 O - 70 - 3

A final ingredient needed to set the stage for rapid advance in chemical dynamics is the development of better theory. By comparison with the progress of theory in structural chemistry, the theory of chemical dynamics has moved at a snail's pace during the past few decades. Fundamental, directly relevant, experimental data of static chemical structures are more readily available than are data relating to chemical dynamics. However, experiments with elementary reactions, including processes as simple as the collision of electrons with atoms and molecules, are providing the basis for a new generation of general chemical-rate theory. At the same time, there is a surge of interest in the special information to be gained from the chemical behavior of energy-rich species. Photo-chemistry, the study of reactions induced by absorption of light, is the principal focus of activity, but a number of other ingenious methods have been devised for production of highly excited atoms and molecules. The results show that chemical reactivity may depend strongly on the energy of a molecule. The demand for expansion of the scope of reaction-rate theory is causing an encouraging reevaluation of the entire field.

Control of chemical changes provides us with opportunities to control ourselves and our environment. Life depends upon near-perfect synchronization of thousands of continuous chemical reactions occurring in living organisms. Controlled chemical change is also incorporated in many manmade systems. An example is the combustion of gasoline in an automobile engine. The process is useful because it allows self-portable conversion of chemical energy to mechanical energy, but it is also crude and dirty because combustion of the fuel leads to noxious atmospheric pollutants. The modern automobile is a marvel of mechanical engineering but uses chemical processes that are as primitive as touching a match to dry kindling. This incongruity of the automobile is repeated over and over again in manmade devices. Mechanical and electrical designs are far advanced in comparison with the design of working chemical units in many of the machines that we invent. In order to upgrade the chemical components of engineered systems, we must depend upon increasing knowledge of chemical dynamics to make possible a kind of chemical systems analysis far more sophisticated than we now have.

3. SOLID-STATE SCIENCE

Solid-state science has been especially fruitful in discoveries and concepts which are of both fundamental scientific importance and readily applicable to technology. This study of the behavior of atoms, electrons, and energy in solids is currently one of the most productive activities in science and is an outstanding example of the beneficial mixing of the disciplines of chemistry and physics. Experiments and theories about various types of imperfections in solids have revolutionized thinking about the mechanical properties of materials. A wide variety of techniques enables the electronic structure of metals, semiconductors, and insulators to be determined in extraordinary detail.

Consider the use in science and technology of the newly found understanding of just one single phenomenon in solids; namely, superconductivity. One of the more obvious future applications of superconductivity is in power transmission. The power loss in a superconducting transmission line would be zero because the electrical resistance of a superconductor is zero. Superconductivity, however, has been demonstrated only at very low temperatures. Power transmission by superconductors will become commercially attractive when the savings on power loss exceed the cost of refrigeration of the line. The continuing development of superconducting alloys with higher working temperatures provides hope that the economic crossover may occur in the near future, thus allowing economic long-line transmission of power from distant hydroelectric or nuclear plants.

A contemporary application of superconductivity is its use in very high-field electromagnets. One use of such magnets is in plasma containment, a key problem in the development of a controlled thermonuclear reaction. The most likely way to contain such a high-temperature plasma is in a magnetic field of suitable design. A conventional electromagnet consumes power; a superconducting electromagnet does not. Unless strong magnetic fields can be generated with negligible power consumption, the thermonuclear reactor will consume most, if not all, of the power it produces. There is also hope of containing such plasmas in radio-frequency resonant cavities. For the radio-frequency fields to be high enough to contain thermonuclear plasmas economically, the walls of the resonant cavities must be superconducting.

Any or all of these developments may make it possible for the country to move in socially as well as economically desirable directions. The very remoteness of major power plants, the diminished fuel requirement, the absence of effluent, and the burial of transmission lines would each contribute to an improved quality of the environment.

A device which rests heavily on the fundamental theory of superconductivity is the Josephson junction. Among many uses, it can be employed as a voltmeter which will measure electrical potentials to a precision a hundred thousand times greater than that of any conventional voltage-measuring device. Most electrical measurements can be turned into a voltage measurement. Consequently, all such measurements may, in principle, enjoy a corresponding improvement in precision, which will have many uses in scientific instrumentation and other technology.

These are but examples of applications of superconductivity that are ahead. Similar examples could be given of applications of many other phenomena which research in solid-state science is bringing into the realm of our understanding.

4. ATOMIC AND NUCLEAR SCIENCE

Atomic physics has experienced a remarkable upsurge in activity in the past few years. In university, industrial, and Government laboratories, where atomic studies had become practically dormant, lively research has now been revived. The achievements include redetermination of fundamental constants with greatly increased accuracy and precision, the development of laser beams and atomic clocks, and the increased understanding and control of plasmas. All of this has expanded the interface between atomic physics and other fields—chemistry, engineering, solid-state science, optics, geophysics, meteorology, and astronomy.

Nuclear energy for military purposes has been of critical importance for twenty-five years. However, nuclear energy has just become economically competitive in our rapidly expanding civilian power industry. Last year more than fifty percent of all electrical generating capacity contracted for in the United States was nuclear powered. Reactor experts are confident that nuclear power plants can be designed to produce power at still lower costs; but to do so, more accurate basic physical data will be needed. Improved theory of nuclear reactions will also help provide for a more extensive theoretical exploration of alternative designs. We can anticipate significant economies in design procedure as well as more efficient designs in terms of power cost. The economic impact of even small reductions in power cost will be tremendous. For example, if the price of electrical energy can be sufficiently reduced, magnesium production will be competitive with that of aluminum, thereby giving aluminum its first competitor for an economical, light, strong metal.

One of the most dramatic consequences of the coming of age of large nuclear power complexes could be the impact on hunger and poverty throughout the world. For the first time mankind can be divorced from nature's caprices in providing natural energy sources, such as waterfalls and fossil fuels, often where they are least needed by civilization. A test nuclear power complex now under study will produce, along with 1,000 megawatts of electrical power, twice the output of ammonia and phosphorus of the largest fertilizer factory now in operation in the United States. Such a plant by itself would supply the fertilizer need for an agricultural operation sufficient to feed more than two million persons.

Radioactive tracer techniques have provided a research probe of great capability and have helped make possible an entirely new level of understanding of biological phenomena at the molecular and cellular levels. Clinical use of radioisotopes and radiation sources in the control and treatment of cancer is extensive. Much of the electronic instrumentation for medicine originated in nuclear physics. Techniques involving nuclear phenomena, such as the Mössbauer effect, neutron diffraction, and neutrography for soft-tissue studies, are now being assimilated by the medical profession.

In the last two years experiments in nuclear physics have become more elaborate and more precise. This progress in experimental nuclear physics has been matched by advances in nuclear theory. The wave of theoretical and experimental advances is quite startling to those who thought the field had passed its peak of interest. New particle detectors permit measurements, which heretofore required months, to be made in a few hours. New accelerators, including large tandem Van de Graaffs, sector-focusing cyclotrons, and high-intensity electron linear accelerators, have laid open for the first time the entire periodic table of the elements to precise investigation. Utilization of highly developed electronic instrumentation in conjunction with on-line computer control has allowed nuclear physicists to attack vital and central problems of nuclear structure which had previously been beyond their capabilities. Beams of electrons and heavier charged particles from the newer high-energy accelerators provide effective probes for studying hitherto inaccessible phenomena in the interior of the nucleus.

Accelerators have found widespread applications in other fields of science. Using beam-foil techniques, it is possible to produce highly excited atomic ions and to study the transition rates between pairs of excited states. The results are of crucial importance in atomic physics and astrophysics, especially in the interpretation of the spectra of quasars. Properly directed ion-beams are "channeled" through solids with exceptionally low energy losses and can be used to probe crystal structure and to locate the position of impurities in crystals. Ion implantation has given solid-state science a new tool of many uses. Accelerators have long been used in studies of the rates of those nuclear reactions which generate energy and synthesize new elements in stars. There has been a great upsurge in measurements on the light nuclei in recent years in connection with attempts to detect neutrinos from the sun. It is now clear that the interactions of intermediate and heavy nuclei must be studied with great detail and precision before the advanced stages of stellar evolution can be understood. This is particularly true in regard to the final implosion-explosion stages which result in supernovae and even more violent astronomical events.

The high-energy accelerator can also be made to produce a copious beam of negative pions for cancer therapy. Such particles are uniquely suited for this purpose. The range of pion beams is so well defined that the lethal heavy-particle radiation resulting from their capture by atomic nuclei may be localized in the tumor. It is believed that the advent of superconducting linear accelerators will permit the development of therapeutic pion sources whose cost and size will be sufficiently small to allow construction of such machines in all major hospitals and cancer treatment centers.

In the last few years, nuclear physics has become a qualitatively different field as vaguely perceived ideas concerning nuclear structure and behavior have come into sharper focus. The nucleus is a microcosm spanning many forces and laws of the universe. Nuclear science, both in physics and in chemistry, has provided a treasure house of new phenomena and is increasingly a versatile servant of science and society.

5. ELEMENTARY PARTICLES AND HIGH-ENERGY PHYSICS

The frontiers of modern physical science range from the domain of the very large to the domain of the very small. Just as astronomy and astrophysics probe the former so do elementary-particle and high-energy physics probe the latter. The exciting advances in one are matched by those in the other. The results insure progress in mankind's understanding of the universe on its grandest scale and on its most fundamental scale. Without the one, the understanding is mundane and parochial; without the other, it is shallow and empirical.

High-energy physics attempts to establish the fundamental laws of matter. It searches for the laws governing the four fundamental interactions—strong nuclear, electromagnetic, weak nuclear and gravitational. In this search, high-energy physics has found new features of natural laws, such as the violation of parity or "mirror symmetry" and the asymmetry between matter and antimatter or violation of "charge symmetry." Apart from seeking an understanding of the interactions between the basic units of matter and antimatter, high-energy physics seeks to find the reasons for the existence of the particles themselves. Why do atoms consist of nuclei and electrons? Why do nuclei consist of nucleons—neutrons and protons? Why do nucleons have structure? More importantly, do these subhierarchies adequately describe the physics of the very small? What about the other worlds beyond the microscope where modern accelerators have exposed neutrinos, the chargeless sisters of electrons; the mesons, messengers of the nuclear force in mimicry of the photon's role in electromagnetism; and the baryons, higher states, with rich and varied properties, of the neutron and proton? Even the nomenclature recalls our ancient traditions of knowledge—the twin neutrinos and electrons, interacting only through the weak nuclear force, are called leptons, the mesons and baryons, paired in the strong nuclear interaction, are called hadrons.

In this field, experiment and observation dictate the pace of discovery. Theory is hard put to accommodate and assimilate, but it has succeeded in codifying in a simple and elegant way the rich spectra of the hadrons. Triumphs of the theory have been the prediction and subsequent discovery of new particles once the underlying classification was understood.

An exciting sequence began when cosmic-ray observers discovered some very strange particles which experimenters at high-energy accelerators subsequently produced. The puzzle about these particles was that, although they were copiously produced and decayed into particles which were known to interact strongly, they lived amazingly long compared to the lifetime expected for such particles. The solution was simple. The "strange" particles, as they were dubbed, were always produced in pairs and could then interact strongly; but in decaying, which they do singly, they interact very weakly. But there was an additional puzzle. Although production of mesons or baryons in pairs seemed understood, the production of a baryon simultaneously with a meson seemed "strange." This led to a clear recognition of "strangeness" as a new quantum number. Whether we liked it or not or whether we understood it in terms of current reality or not, there was a new law of physics in the record books—"strangeness" is conserved in the strong nuclear interaction and is violated in the weak nuclear interaction for which the characteristic interaction time is relatively long.

The weak nuclear interaction moved to center stage. The definitive tests are not yet complete, but it is probably a universal interaction applying to all the hadrons and leptons. It is especially important in lepton physics, because leptons do not share in the strong nuclear interaction. One of the most exciting discoveries in physics in recent years concerned these leptons. Charged leptons occur in four forms—negative electrons and positive electrons (positrons), negative muons and positive muons. The discovery followed the dictates of symmetry—experimenters found neutrinos and antineutrinos which always paired with electrons and positrons and completely independent twins which always paired with the two muons. There exist electron neutrinos and antineutrinos, and there exist muon neutrinos and antineutrinos.

The symmetry of the weak interactions stopped there. Theory surmised and experiment showed that electrons spinning relative to their motion in the sense of a right-handed screw thread did not behave identically with their left-handed brothers. This seemed to violate the mirror symmetry of physical laws, since left-handedness transforms to right-handedness on mirroring. Why should the laws of physics be different for the image than for the object? The situation was saved by the experimental discovery that right-handed positrons behaved like left-handed electrons and vice versa. This resulted in a great measure of general satisfaction that physical laws were invariant to the combined operations of mirroring, parity and charge, and matter-antimatter exchange.

For all men, symmetry, even in the sophisticated form evidenced by the weak interactions, is a thing of beauty and conceptual usefulness. There is a strong theme of symmetry in all approaches to scientific understanding—from Newton's action and reaction to Dirac's matter and antimatter. In this respect high-energy physics brought us to a crossroads in our basic understanding of nature. The surprising discovery was made that the continued symmetry operation of replacing particle by antiparticle and of mirror reflection is not a perfect symmetry of nature. A certain type of meson broke these symmetry rules. No other such violations have been found. The clarification of this phenomenon is one of the great challenges facing particle physics. Charge-parity violation implies that certain natural processes are no longer invariant to time reversal. We believe intuitively that the physics would not change were the earth to stop and instantly reverse its motion around the sun. Is this not true in the world beyond the microscope? It is to high-energy physics with its preoccupation with the smallest units of matter that we must look for the answer.

Concern for symmetries in our descriptions of the laws of nature is not new. Over the centuries it has been believed that the appearance of symmetries was one of the most fundamental aspects of nature. Upon several occasions in the past, however, experimental results have led scientists to abandon their intuitive ideas and to discard certain symmetry principles. Eventually, however, a way would be found to reformulate a part of basic theory so that those symmetries were restored. In each case the new insight into nature thus gained opened for exploration new areas of science and technology. For example, the cruder work of Copernicus, Galileo, and Kepler preceded Newton's formulation of the basic laws of dynamics and expression of the gravitational law in a mathematically symmetrical form. Today this theory can be used to explain the motion of everything affected by gravity, from baseballs to satellites. The laws of electromagnetism required over a century for their gradual refinement, and the search by Maxwell for a mathematically symmetrical treatment of electric and magnetic forces was an essential element in their perfection. These laws now permit the understanding of all electromagnetic radiation, including light, and the development of radio, television, radar, and computers. Concern for the symmetrical treatment of space and time played a crucial role in Einstein's development of the theory of relativity and produced in the process the essential key to the release of atomic energy.

The concept of symmetry, therefore, has been too fruitful to be abandoned lightly. It is, of course, possible that nature is really not symmetrical. However, experience indicates that it is worth man's effort to try to find fundamental errors or omissions in his description of nature whose correction or inclusion might retain symmetry. The history of science, indeed, leads the scientist to suspect that a key to new levels of understanding nature, and thereby to improved technology, lies hidden in the debris of apparently broken symmetries.

II

Nature of the Physical Sciences Enterprise

A. UNITY OF SCIENCE

In recent years the development of the physical sciences has been characterized by a rapidly increasing degree of unity in concepts, models, and experimental techniques. Modern chemistry, for example, uses concepts and theories originally evolved in physics. Conversely, many physical concepts themselves could not have been fully developed without information and generalizations transferred to physics from chemistry. Astronomy has also shared in this unification of the physical sciences. Not only have physical effects seen in the laboratory been shown to have counterparts in the stars and in interstellar space, but also the universe itself provides a laboratory in which the behavior of matter can be studied under extreme physical conditions not attainable on earth. There is, indeed, a large common area among chemistry, physics, and astronomy, where research interests strongly overlap and where the difference is more in style and perspective than in subject matter.

In general, the physicist is most interested in finding "simple" systems with which to test theories or models he is trying to develop or to verify. While the chemist is also interested in studying systems which illustrate principles and theories, he tends to be more concerned than the physicist with the large variety in the forms of organization of matter and with different instances of general principles. Traditionally, physicists have concentrated their interest in molecules to those containing only a few atoms in order to understand with high precision the quantitative relationship between molecular properties and the basic postulates of quantum theory. Even this distinction in style and approach between the fields of chemistry and physics has largely disappeared since many chemists are deeply involved in molecular theory. Moreover, those physicists who enter the field of biophysics soon discover the special fascination that comes from the study of large molecules in a complex environment. The underlying conceptual unity of physics and chemistry is now extending rapidly into the study of biological systems, and it is becoming increasingly possible to understand the functioning of biological structures in terms of the models and the principles of physics and chemistry.

The natural sciences are approaching a single conception of the organization and structure of matter at varying levels of complexity. The physicist is concerned primarily with atoms and subatomic particles. The chemist deals with atoms as they form millions of different molecules. The biologist in turn deals with tens of millions of species, each one a unique organization of matter.

The trend toward conceptual unification of physics, chemistry, and biology has a counterpart in experimentation and instrumentation. Physical techniques are increasingly used to measure and characterize chemical and biological systems ranging from such simple physical properties as density, viscosity, thermal and electrical conductivity to the more complex areas of optical spectroscopy, electron microscopy, X-ray structure analysis, and magnetic resonance. This extension of physics instrumentation into biology and chemistry is not a one-way street. In fact the application of physics instruments in these fields has often led to their improvement and refinement and has greatly stimulated their engineering development for routine use. For example, the use of X-rays for analysis of crystal structure, originated by physicists and refined by chemists, has made possible the analysis of the structure of molecules of biological interest containing thousands of atoms and resulted in improved X-ray instrumentation. Furthermore, chemical analysis and methods of purification and characterization of materials are necessary preludes to precise and reproducible studies of their physical properties or of the physical processes going on in them.

The unity of the physical and, to an increasing extent, the biological sciences involves also the expanding use of mathematics as a common language among all the fields. This trend has been reinforced by the advent of the high-speed computer, which has made it possible to work with realistic mathematical models of physical systems and to predict their properties and behavior from a few simple, general assumptions applicable to all matter. The computer is also an indispensable tool for analysis of very complex systems in which many closely related changes occur both at the same time and in sequence, as in chemical synthesis.

Astronomy has been able to demonstrate a type of homogeneity of the universe; the laws of behavior and organization of matter and energy seem to be everywhere essentially the same. This idea in turn has become a working hypothesis of enormous power for further exploration. Low-energy nuclear physics has provided a key for understanding the origin of the elements and the evolution of stars. Space probes in combination with our ability to detect radiation in various parts of the electromagnetic spectrum, with high sensitivity, using laboratory techniques developed in physics and electrical engineering, have opened up new windows on the universe and provided clues to physical processes going on in the depths of space.

An important aspect of this unity in the physical sciences is their mutual dependence and reinforcement. We cannot expect to advance too selectively either in the sciences themselves or in the derived technology. Too much selectivity results in missed opportunities and missed clues to important discoveries or measuring techniques. When opportunities, technological or scientific, are opened up by a new discovery, their exploitation can often be planned or programmed, but the discoveries themselves are seldom the result of such a planned development.

Too much selectivity also may leave us without the necessary foundation on which to build new and needed fields of science and technology. For example, our hopes for an early achievement of controlled thermonuclear power were largely dashed by our lack of prior knowledge of plasmas. This principle of broad advance applies to all sciences, but especially to the physical sciences where the structure of technique and understanding is so tightly meshed.

B. TWO-WAY INTERACTION BETWEEN SCIENCE AND TECHNOLOGY

Technology and science reinforce each other in a complex, two-way interaction. For example, the modern computer would not have been possible without many important recent developments in solid-state physics, but our understanding of the structure, properties, and processes of solids has been immeasurably increased by the ability of the digital computer to carry out complex calculations of electronic structure. The computer contains applications of high-speed circuit techniques developed first for the purposes of nuclear physics. In turn the computer is a powerful tool for the automatic selection, processing, and presentation of nuclear data, thereby making possible the study of extremely rare nuclear events. Now the sophisticated data-processing methods developed for nuclear physics are finding application in other areas of computer use which involve the recognition of coherent patterns in a very complex and often apparently random assortment of information.

Solid-state science and metallurgy have made possible the superconducting magnet, which has subsequently found application as an essential research tool in solid-state physics and plasma physics, and has brought closer the realization of controlled thermonuclear fusion in the laboratory. Chemistry in general is an especially fruitful area for the rapid transfer of new information from science to technology. Laboratory studies of chemical reactivity are necessary for the design and operation of chemical plants and for the development of the field of petroleum technology. Moreover, it is now becoming apparent that many of the unanticipated environmental problems created by technology may be stated as chemical problems. Environmental pollution in particular may be understood in terms familiar to the chemist and chemical engineer, and a large amount of relevant chemical information already exists for use in seeking solutions to this complex social and economic problem.

C. CONNECTIONS BETWEEN ESOTERIC CONCEPTS AND PRACTICAL APPLICATIONS

The development of the transistor and nuclear power has particularly dramatized the connections between apparently abstract physical theories and practical applications. Theories are frequently generated by consideration of problems that seem impossibly remote from the concerns of social man. However, the conviction of scientists that a viable theory must be widely relevant provides powerful guidance for application. Such theories often provide the only language and concepts in terms of which new inventions can be made. It is only when these theories become part of the common intellectual coinage of a large number of scientists and engineers that continuing invention in such fields becomes possible. The evolution of solid-state electronics is a testimonial to this fact. Its practical development required the adjustment by engineers and production people to an entirely new scientific environment in less than a single professional generation. What applies to technology often applies equally to other disciplines. For example, the ability to understand and measure radioactivity has revolutionized archaeology by making it possible to date more precisely human artifacts and other remains. A highly sophisticated experiment in elementary-particle physics is now being used in an attempt to locate additional tombs and chambers in an Egyptian pyramid. Similarly, concepts of chemical dynamics developed from the study of small molecules now provide understanding of the mechanisms of enzymatic action in controlling the chemistry of life.

D. IMPORTANCE OF NEW IDEAS AND NEW INSTRUMENTS

New concepts and principles, new physical processes and models, and new measuring techniques which extend precision and sensitivity are extremely important to the development of the physical sciences. Many such advances in technique open up whole new areas of research involving unanticipated phenomena. These advances occur with surprising frequency even in areas of science which are supposedly well understood. Often such developments have the character of being obvious and logical in retrospect. A good example is the Mössbauer effect, recoilless gamma-ray emission by atomic nuclei in crystals, which was implicit in a theoretical paper by Lamb in 1939 but not developed experimentally or even appreciated until 1957. Since its discovery, however, the Mössbauer effect has rapidly evolved as a new tool for investigations in solid-state science, biology, and even in medical practice.

The extension of the extremes of environment—very high or very low temperatures, very high or very low pressures, very high magnetic fields or field-free regions—is also an important tool for advancing science. Often these extensions of the experimental conditions permit study of entirely new classes of phenomena. The exploitation of such capabilities is essential to the continued progress and vigor of the physical sciences even when the precise usefulness of a new technique cannot be predicted or fully understood at the outset.

It is also important to the advance of science that new laboratory techniques be engineered into instruments which can be used by scientists less specialized than the inventors. The development of a practical and relatively inexpensive helium liquefier in the 1940's by Collins in collaboration with Arthur D. Little, Inc., had an enormous influence on the progress of solid-state science by making low temperatures readily available to physicists and chemists who did not have the time or resources to develop their own low-temperature equipment. Similarly the commercial development of the electrostatic accelerator, the mass spectrometer, the nuclear-resonance spectrometer, the electron microscope, high-pressure equipment, and hundreds of other instruments, initially handmade with great travail by laboratory scientists, has permitted researchers to concentrate on the scientific questions rather than on merely reproducing research technologies already pioneered by others. The rapid commercialization of laboratory techniques and instruments has generated a new style of research in which the United States has been in the lead. It has been made possible by the quality and scale of United States research activity, the magnitude of Federal development programs, and the entrepreneurship of our industry.

E. PRODUCTIVE IDEAS AND THEMES

New theoretical concepts and ideas, developed in the physical sciences originally for a rather restricted purpose or for the explanation of a specialized phenomenon, often are productive in an unexpectedly broad range of situations. An illustration of this situation is the theory of superconductivity developed by Bardeen, Cooper, and Schrieffer in 1958. This idea has altered our whole perspective on solid-state physics and has had an important influence on the development of ideas about nuclear structure. It has given theoreticians a tool for integrating the collective and individual particle descriptions of both the behavior of electrons in crystals and the behavior of neutrons and protons in nuclear matter—descriptions which seemed mutually contradictory and yet which were each required for the explanation of different properties.

The concept of particle tunneling, that is, the possibility of a particle penetrating a classically impenetrable barrier, has been a similarly fruitful idea. This idea was initially advanced in the 1930's to explain the disintegration of atomic nuclei. In the last few years it has led to the invention of a new electronic device, the tunnel diode, which has become an important component of computers as a very high-speed switch. The invention of the tunnel diode and its immediate practical application caused a great increase in research on electron tunneling generally. This quickly led to a new technique for fundamental studies of the electronic structure of metals and superconductors. These studies in turn resulted in the prediction and discovery of new types of phenomena involving quantum effects on a scale large enough to permit the engineering of new devices. Because of the tight interweaving of the physical sciences, specific ideas or techniques, developed at first for a particular purpose, turn out to have an extremely productive generalizability.

F. ECONOMY OF THOUGHT

Much has been written and said about the information explosion in science. Certainly, knowledge has increased tremendously in recent years as research data have poured from the laboratories. It is, however, characteristic of the advance of science that, as understanding increases, descriptions of nature can be simplified so that the advance of science is accompanied by information compression as well as explosion. The aim of scientific effort is not information per se but rather understanding and insight, and it is this insight which enables us to describe a wide range of observations and experiments by a simple physical model from which much can be deduced. The law of gravitation, as formulated by Newton, replaced the more complex descriptions of the Ptolemaic epicycles and of Kepler's laws. The laws of quantum theory brought much of physics and virtually all of chemistry within a single framework of basic assumptions. In both cases complexity in physical description was replaced by descriptive simplicity and computational complexity. The latter is being brought under control by developments in mathematics and more recently in computers.

Often the new physical description seemed incomprehensible and esoteric, but scientists, by their persistent drive toward generalization, make it become a part of their common intuition, almost a part of the subconscious processes by which they think about the world. When we can use the same concepts to describe processes in the interiors of stars and in the laboratories on earth, a great economy of thought is involved. Such encapsulation of knowledge is often essential for the rapid development of technology. It also permits more informed decision-making with respect to alternative paths which are involved in technological development.

G. MEASUREMENT, DESCRIPTION, AND CONTROL

In the physical sciences the scientific process may be thought of in terms of three operations: measurement, description, and control. Measurement consists of the extension of the sensitivity and accuracy of the human senses by physical instruments. Measurement can be divided into two subcategories, observation and experimentation. In the observational sciences all that man can do is to observe and use instruments. Astronomy is the classic example of an observational science; man can observe the universe but cannot alter it. In an experimental science man not only observes and uses instruments to extend his senses, but, also has an opportunity to alter or prepare the situation which he is observing. Experimentation also helps to develop instrumentation which can then be used effectively in the purely observational sciences.

The next important aspect of science is description. Observation and experimentation by themselves are virtually meaningless without a conceptual framework or context within which to fit what is observed. Theoretical models of even tiny pieces of the universe provide the context without which observation would present a meaningless and chaotic pattern. Man cannot "observe" entities like electrons or atoms without at least a tentative model of what he thinks they are, how they behave, and how they interact with instruments. The process of description thus includes the development of abstract models, and it is the correspondence of these abstract models to reality which in fact comprises the description. Theory also helps to suggest which new observations are likely to be most important. Sometimes the observations cannot be fitted into the context. It is at such critical times when the pattern of observation and experiment becomes sufficiently disjointed from the context that new exciting theories are born.

The use of experiments, in which one controls the situation being observed, is a large step towards the first stage of the engineering process: the control of nature for a purpose other than observation and understanding. The fact that experiments involve control of nature shows why the progress of science, especially in the physical sciences, is so intimately related to the progress of technology. The instrument which one uses for analyzing the chemical composition of a substance being studied in the laboratory can become the instrument for controlling the composition of the constituents in a chemical process for the production of useful materials. Increased ability to control environment in an experimental situation becomes increased ability to control environment for other useful purposes. Most industrial instruments, controls, and processes have evolved from the research laboratory, and such evolution will surely continue.

H. COMMUNICATION SYSTEM OF SCIENCE

The communication processes of science are in some ways quite different from those in many other human activities. Science usually progresses through integration of the results of the apparently isolated activities of hundreds of individual scientists, each concerned with a narrow problem of his own choosing. Yet there is an elaborate and highly developed system of control which turns a mass of interrelated activities into a coherent process. Science is a social process of great sophistication and complexity, and much of its decision-making is highly decentralized. The social process of science is efficient for the progress and advance of scientific knowledge, much more efficient than a highly centralized process could ever be for this purpose. The understanding by the practitioner of this social system of science is an important part of the process of training for research. Because the understanding of this social system of science is taught by implicit indoctrination rather than by explicit instruction, it is often not well understood outside the scientific community.

It is an oversimplification and even incorrect to say that science cannot be planned. The major problem in such planning is the proper differentiation between those decisions which must be made centrally and collectively and those decisions which must be made on a highly decentralized basis. In general the centrally made decisions are those which allocate resources to major programs. In the pursuit of any research project there are many decisions which must be made on a highly decentralized basis. However, competition within the social system of science gives those decisions a value which insures the effective exploitation of the centrally made decisions.

When the country decides to build a major new accelerator, the country is planning to conduct a series of experiments requiring the characteristics of that particular accelerator. This does not mean, however, that the precise experimental program to be conducted with the accelerator has to be planned in advance. The experimental program, indeed, can only be developed as the science develops. Each new experiment depends to an extent on all experiments which have gone before, not only those done with a particular accelerator but those done with all other accelerators all over the world. The planning of any given experiment can only take place in the context of all the knowledge and understanding existing at a particular time. If it takes place in a narrower context, the research becomes inefficient. Detailed experiments planned too far in advance will also be inefficient because relevant new information will appear before the experiment is actually done. Similarly, when the country decides not to build a major new radio telescope, the country is planning not to conduct any observations requiring the characteristics of that particular telescope. The exact program which is thereby foregone cannot be specified in detail; one never really knows what is being given up.

Such mixed systems, which have both a highly centralized or collective component as well as a highly decentralized or individual component, appear in other parts of society as well. In industry the central, corporate management of Company X may decide that the timely introduction of a new product requires the construction of a new plant at Site Y. Many studies are made before and during the decision-making, planning, and construction processes. However, the labor force is not hired before the plant is built. In the final analysis each worker, individually, decides whether he will work for Company X or some other company. That is the essence of decentralized decision-making. But Company X, based on its analyses, makes a highly centralized decision, confident that it can recruit an able labor force.

Similarly we can plan centrally and collectively for the progress of science. We could plan centrally and collectively for the stagnation of science. However, the process of planning in science at the level of detail equivalent to the employment decision of the individual industrial worker, that is, the exact nature of the next experiment, has to be carried out in the last analysis by each research group working in the context of existing knowledge. The able scientist senses the intellectual market for his idea or experiment much as the able businessman senses the economic market for his product. The most relevant question to ask about scientific planning at this level of detail is whether the decisions of particular research groups were made with full cognizance of the existing state of the art and whether their record of planning has been productive in the past. It is for this reason that the planning process in science must continue to contain this highly important, individual, competitive component. The man who is planning his experiment or his calculation is ideally the best person in the world to plan that particular experiment or calculation. The entire social system of science and its system of sanctions and rewards press him to be aware of all the work in the whole world which is relevant to his particular experiment or calculation.

The ideal is never fully realized in practice, but the reality is sufficiently close to the ideal to make the social system of science highly efficient in achieving its goals of insight and understanding. To date, the United States need not fear the judgment of history regarding the success of our mixed system in terms of both research and graduate education in the physical sciences. Our system has proven to be fully competitive on a world-wide basis.

I. SETTING OF PRIORITIES

The determination of priorities in science is a dynamic, complex, and subtle matter requiring a balance among many different considerations ranging from the quality of the people in a field to the estimated value of potential applications. It is sometimes asserted that the scientific community has no system for determining priorities within science, and that the Federal Government has no policy for allocating scientific resources. Neither of these statements is true.

The fact that much of science does not use a highly visible, centralized, priority-setting mechanism does not mean that other mechanisms do not exist. Actually, science uses a multiplicity of such mechanisms. One priority-setting mechanism operates when a scientist determines the problem on which he works and how he attacks it within the resources available. This determination is made taking into account other similar and related work throughout the world. Another mechanism operates as proposals of competing groups of scientists are evaluated and funded on the basis of systematic refereeing and advice of peer groups. Still another mechanism operates as aggregate budgets for various fields of science are influenced by the number and quality of research proposals received in that field. Like any market mechanism this system is not perfect and requires regulation and inputs from outside the system itself. Such inputs come from the mission-oriented agencies which balance their needs for new knowledge against their operating needs and from a whole host of outside judgments implicit in the budgetary and appropriation process. Trouble occurs either when these external judgments are completely substituted for the priority setting of the scientific community or when the priority setting of the scientific community becomes too autonomous.

The decentralized scientific priority-setting mechanisms are aimed at making growth of scientific understanding and insight as rapid as possible, but scientists do not live in a vacuum and are sensitive to the concerns and priorities of the society around them, as well as to the problems of mission-oriented agencies which have research funds. Academic scientists are especially sensitive to the interests and concerns of students who come into the scientific enterprise with new ideals and values not completely determined by the perspectives acquired by the senior scientists in the course of their working lives. The continuing entry of able and energetic students into the scientific process tends to stimulate a continual reevaluation of priorities among academic scientists and within the scientific community as a whole. The process of selection of faculty members for universities is itself another decentralized priority-setting mechanism; the interests of faculty determine the choice of research problems and the type of proposals which are submitted. Faculty members are not paid primarily from Federal funds. Therefore, the faculty selection process is in large measure an independent input to the priority-setting system.

The somewhat idealized system described applies primarily to research activities which involve relatively small grants with individual investigators working with a small group of students and colleagues. These natural priority-setting mechanisms in fields dominated by such activity work quite well, and little is to be gained and much may be lost by trying to establish and enforce a highly centralized priority determination. An area of concern might be the possible neglect of certain underdeveloped subdisciplines because they may have too few scientists to attract the attention they deserve, and existing proposals may be underrated even by peer groups. Such subdisciplines may include fields which are of importance for applications but do not appear to be as scientifically challenging as other areas, often because the general problems are not subject to easy dissection into manageable research problems. Current examples of possibly neglected subdisciplines may be electrochemistry and analytical chemistry.

Special measures to stimulate proposals in such fields may be desirable. One well-tried method, for example, is the use of a sheltered competition among research proposals in a well-defined area. To some extent basic research supported by a mission-oriented agency always constitutes such a sheltered competition, bounded by the mission relevance of the subject matter. However, there are also the dangers that a sheltered competition will attract proposals of low scientific quality and will prolong some projects beyond the point of usefulness. Once a sheltered competition has developed a sufficient number of proposals of high quality to compete on their own terms in a broader field, the purpose of the program has been realized. It should then be phased out gradually, but with an accompanying increase in aggregate funds to take into account the newly established research programs.

The problem of priorities is rather different when major facilities or the creation of new research institutions is involved. Here some form of central determination is essential because such expensive facilities cannot be duplicated extensively. Later we indicate some of the factors that ought to be considered in allocating funds for major new facilities in the physical sciences. Once the commitment has been made to construct and operate major facilities, national planning must assure the funds necessary to utilize the facilities effectively, including adequate funding for the programs of user groups. For each facility there exists a range of productive operating levels. Below the low end of this range it becomes difficult or impossible to keep first-class scientists involved and interested, and the operation becomes ineffective. There is another higher level of operation and utilization above which the use of the facility may result in diminishing returns when compared to alternative investments. When agencies plan for the allocation of operating funds, including the support of outside user groups, they must plan so that the level of utilization lies between the extremes mentioned above and so that the program is in reasonable balance with related work. Since the United States accounts for about thirty percent of the world output of papers in the physical sciences, it is reasonable to expect that it should account for this proportion of the truly important contributions in those fields of science requiring major facilities and instrumentation.

At any time there will be certain fields of science that are particularly ripe for exploitation and which deserve special priority in terms of facilities investments. We believe that examples of such fields within the physical sciences are radio astronomy and the even newer observational astronomy windows (gamma rays, X-rays, infrared, energetic particles, and the solar wind) which the earth's atmosphere partially or totally obscured prior to the development of our competence with balloons, rockets, and satellites. On the other hand, we feel it important to note that the term "priority" not be interpreted so as to result in complete stagnation in all other fields of the physical sciences or in exclusive concentration on programs requiring major one-of-a-kind instrumentation. Also, in making facilities investments we must realistically appraise the prospects of success. Investments merely for the purpose of "catching up" with other nations are likely to be wasteful unless they place us truly at the forefront. We run the risk of this happening if we delay too long in implementing plans which have reached a certain stage of maturity. In such cases it may be more economical in the long run to make an even larger initial investment in order to "leap-frog" capabilities existing elsewhere than to make a more modest investment to duplicate or parallel capabilities already existing.

Dynamic, complex, and subtle systems for setting priorities are common in everyday life. A fire in the home or a sick child may instantly change a man's priorities. Such effects also exist in our political sector. An agency's annual budget summarizes and states the agency's priorities for that particular year under the known constraints. Many problems and alternatives have been considered in the course of preparing that budget, and it contains, either explicitly or implicitly, a complete statement of established priority.

III

Health of the United States Effort in the Physical Sciences

Science has always flourished in those nations which were the economic and industrial leaders of the world at the time. Contrary to a common belief, the excellence of the United States in the physical sciences was already beginning to be evident early in the twentieth century. A continually rising investment in research in the physical sciences and a steady growth in the number of scientists, even during the depression of the 1930's, coincided with a rapid evolution of scientific achievement and technological capability in the United States. This state of affairs is no coincidence, for the science and the economy of a nation are mutually interdependent. Advanced industry provides the capability for research, and research creates the knowledge out of which the advances in technology are conceived or developed.

The progress of science is also one measure of the advancement of a civilization. Man's understanding of the universe and his ability to describe, predict, and control his environment are measures of his culture, and the degree to which a nation contributes to this common human enterprise is a measure of its place in world civilization. A nation which turns inward on itself and becomes exclusively preoccupied with its own immediate problems will not only lose its claim to respect in the world but may fail to solve those problems as well. The determination of the size of the national support of science is an important decision for the Nation. The multiple relationships between science and the rest of society make that decision particularly difficult because adverse effects will appear only slowly and will become increasingly difficult to reverse as the state of United States science subtly deteriorates. The public and its representatives and agents in government must be aware of the importance of progress in fundamental science to the solution of the problems currently facing the country and to the anticipation and solution of the problems which will surely face it in the future as populations grow and as the world society increases in complexity. The interdependence among the sciences means that we cannot progress very far either in societal problem-solving or in scientific understanding if we attempt to work too selectively on those parts of science which are perceived at any given time as self-evidently relevant to the current problems and concerns of the society. The advancing fronts of knowledge, understanding, and application are too closely interconnected for such an approach. A narrow approach would be nowhere more damaging than in the physical sciences, which provide the conceptual framework and the tools of measurement for much of the rest of both science and technology.

The world scientific enterprise is a mixture of cooperation and rivalry—among individuals, institutions, and nations. Both the competition and the cooperation are necessary. A strong national scientific enterprise is necessary to appreciate, utilize, and exploit the discoveries of others. Because of the breadth of its scientific effort, the United States has been in a position to take advantage of many ideas initially conceived elsewhere in the world. Now United States leadership in science and technology is being challenged not only by the Soviet Union but also by Western Europe and Japan. In Western Europe and Japan investments in basic physical science and related education are growing at rates comparable to those in the United States during the late 1950's and early 1960's, and that growth is occurring in the newest, most promising or exciting fields. These nations are in a good position to take advantage of the latest capabilities in instrumentation and experimental techniques because they have small commitments to older equipment and facilities. To remain at the forefront the United States must maintain a distributed, but balanced, effort—incorporating the very new but at the same time retaining much that is familiar. This need is not unique to science. A football team composed entirely of seniors may have an all-winning season, but graduation leaves the Old School destitute of experienced players the following season. A team comprised entirely of sophomores may sometimes be necessary and may seem to have certain advantages of youth in the early quarters. However, they may fail under the pressure of the final few minutes and lose to a more experienced team. The ideal team then is a mixture of veterans and rookies. It is the coach's job to field the right mixture.

The fruits of international science do not appear solely in the economy but appear also in our general culture. The influences may be either subtle or dramatic. The thrill of human accomplishment when a man first stepped on the moon was not nationalistic but was shared by most of the peoples of the world. In a much less obvious way the recognition of common motives in science has a unifying effect similar to that found in world literature and world art. The scientists of this country will continue to make their contribution to our position of international leadership if our present momentum in science is maintained.

About five years have elapsed since publication of the reports of the National Academy of Sciences on ground-based astronomy,[1] chemistry,[2] and physics.[3] Each of these reports attempted to project ahead five years rather carefully and ten years in a more speculative fashion. Reviewing the five-year projections, one cannot fail to be struck by the enormous vitality and productivity of the physical sciences during this period. In almost every case, the scientific accomplishments since 1964-65 have considerably outrun the expectations at that time. There have been general gains both in fundamental insights from specific discoveries and in the development and exploitation of new observational and theoretical techniques.

The present vitality of the physical sciences, despite budgets which fall well short of the funding recommendations in those National Academy of Sciences reports, is being sustained largely by the results of past scientific investments, both in manpower and in equipment, and by the continuing hope of the scientific community that the lag in public support is temporary. Scientists still are generating new instrumentation ideas and new research plans because most of them believe that some of these plans will come to fruition in the near future. Should confidence in this belief fade, the adverse effect on productivity in the physical sciences would be serious.

The physical sciences effort in the United States is a joint venture of universities, Government, and industry. Each of these partners provides personnel, funds, and facilities. Yet each partner has a different reason for doing science and, therefore, performs a different and necessary function in the whole system. We believe that this partnership enables the United States to use its resources extremely productively. For example, during 1967 the Soviets launched 67 scientific spacecraft compared to 31 launched by the

United States. Yet almost all the advances in space and planetary science came from the United States program. This is due in large measure to the more general involvement of the scientific community in these experiments, to the cross-fertilization of ideas resulting from the greater breadth of the United States scientific effort, and to the excellence of the output of United States industry.

Today our Government, our universities, and our industries jointly hold the greatest research capability in the physical sciences that the world has ever known. They do so, moreover, at a time when the physical sciences face the most exciting prospects in history for discovery, for understanding, and for applications to many diverse needs of our society. It is sadly inconsistent that inadequate funding frustrates their ability to respond to new ideas and new opportunities and threatens the United States scientific effort with mediocrity.

Before turning to details we conclude this general assessment by reemphasizing that the outstanding progress in the physical sciences during recent years, both in fundamental discoveries and technological applications, has been achieved with nearly level research budgets and with major facilities which are rapidly becoming obsolete. It is clear there will be a day of reckoning for United States science and for the national well-being. That day may be very near—the highest energy accelerator is in the Soviet Union, not the United States; clashing-beam apparatus exists in Western Europe, not the United States; a nuclear accelerator specifically devoted to studies of astrophysical reactions exists in France, not the United States; new radio telescopes are being built elsewhere, not the United States; pulsars were discovered in Great Britain, not the United States; major United States manufacturers of modern chemical research instrumentation now find that approximately fifty percent of their market lies abroad.

A. THE UNIVERSITIES

1. UNDERFUNDING OR OVEREXTENSION?

Universities have always been beset with problems. In spite of this fact, during this century United States universities have built an impressive record of achievement and of excellence in teaching, in research, and in public service. Furthermore, they have always been sensitive to the need to extend higher education to an increasing fraction of our population. They now find themselves caught in a dilemma; changes in style, many of them costly, are obviously needed at a time when current financial problems seem enormous. The higher education system in the United States is seriously overextended in terms of the availability of funds to meet its responsibilities. The universities and colleges expanded in response to the urging and inducements of society and of government at all levels. However, even the short-term adjustments to immediate social and educational needs require these institutions to make long-term commitments. This is true in almost every aspect of their operation. It is especially true for programs of research and training in the physical sciences. For the past decade, the universities expanded the base of graduate-science training and research in response to national needs and implicit national policy. The product is an immensely valuable national resource of faculty and students, buildings and capital equipment. By urging the need for more scientists with advanced training and by making many of its own commitments contingent on matching long-range institutional commitments, the Federal Government has assumed a share of the responsibility for academic science which goes far beyond particular research projects. It must meet this responsibility by sustaining the enterprise which it helped to create. Otherwise we may end up with a large assembly of excellent institutions and talented research groups which do not have sufficient support to remain vital and productive.

The tremendous expansion in graduate education and research has been heaviest in fields such as chemistry, solid-state science, and atomic and molecular physics. Many developing institutions have turned to these fields because, in addition to offering many exciting scientific opportunities, they are cheaper on a per scientist or per student basis. These are good areas in which to start a development effort, but existing funds have been spread so thinly that it has become increasingly difficult for even a burgeoning institution to find support.

For example, the number of Ph.D.-granting chemistry departments in the United States grew from 110 in 1957 to 172 in 1967; that number has now passed 200. The increase in chemistry staff at Ph.D.-granting institutions is projected to grow at a rate of ten percent a year for the next ten years. This growth will be heavily concentrated in the newer or smaller departments which, therefore, will have to be equipped almost from the ground up at an average cost per department in excess of one million dollars. The limited funds for chemistry instrumentation in the budget of the National Science Foundation have precluded a truly balanced program. At the same time there is ample evidence that such instrumentation is essential to most of the important new discoveries in chemistry.

In Europe, the Soviet Union, and Japan one can identify in many fields of science a laboratory or research group which is better funded, better equipped, and better staffed than any single laboratory in the United States. This is true despite the fact that total United States support in the field exceeds in most cases that of other nations. In other words, the lag in financial support in the United States is creating a situation in which our scientific effort is too widely dispersed for the resources available. We emphasize that this is a recent phenomenon, born partly by the impressive effort on the part of other nations to catch up with the United States and partly by the lag in the last three or four years in the support of basic science by our Government. Even in those few years, however, the gap between what could be done productively and what can be done practically within existing budgets has become so large that we must examine the basic policies and tenets of our present support system.

In this situation we are faced with three choices of policy. The first would be to continue as at present with level or declining funding but attempt to maintain the present broad base of graduate departments and national laboratories. This course of action would continue to spread resources thinner and thinner. The second choice would be to accept present funding levels indefinitely and begin a planned phasing out of a number of laboratories and graduate science departments. This course of action would free funds to build up a concentration of equipment and people in fewer places, judged to be most likely to push the cutting edge of the United States scientific effort. The third choice would be to implement what appears to be a continuing national commitment to excellence in science as well as to a broad and broadening base of opportunity for participation in graduate training and science and to provide the resources necessary. We have considered these alternatives carefully, but the following discussion shows that the third alternative is the only one which is realistically open to the Nation.

Our population with ages between 25 and 45 years is projected to reach 62.4 million people in 1980, barely ten years from now. That is the group which will be doing most of the Nation's work and rearing most of the Nation's children.

Figure 1 permits a comparison of the slopes of three separate graphs:

I. Gross National Product in Billions of (1958) Dollars.[4]

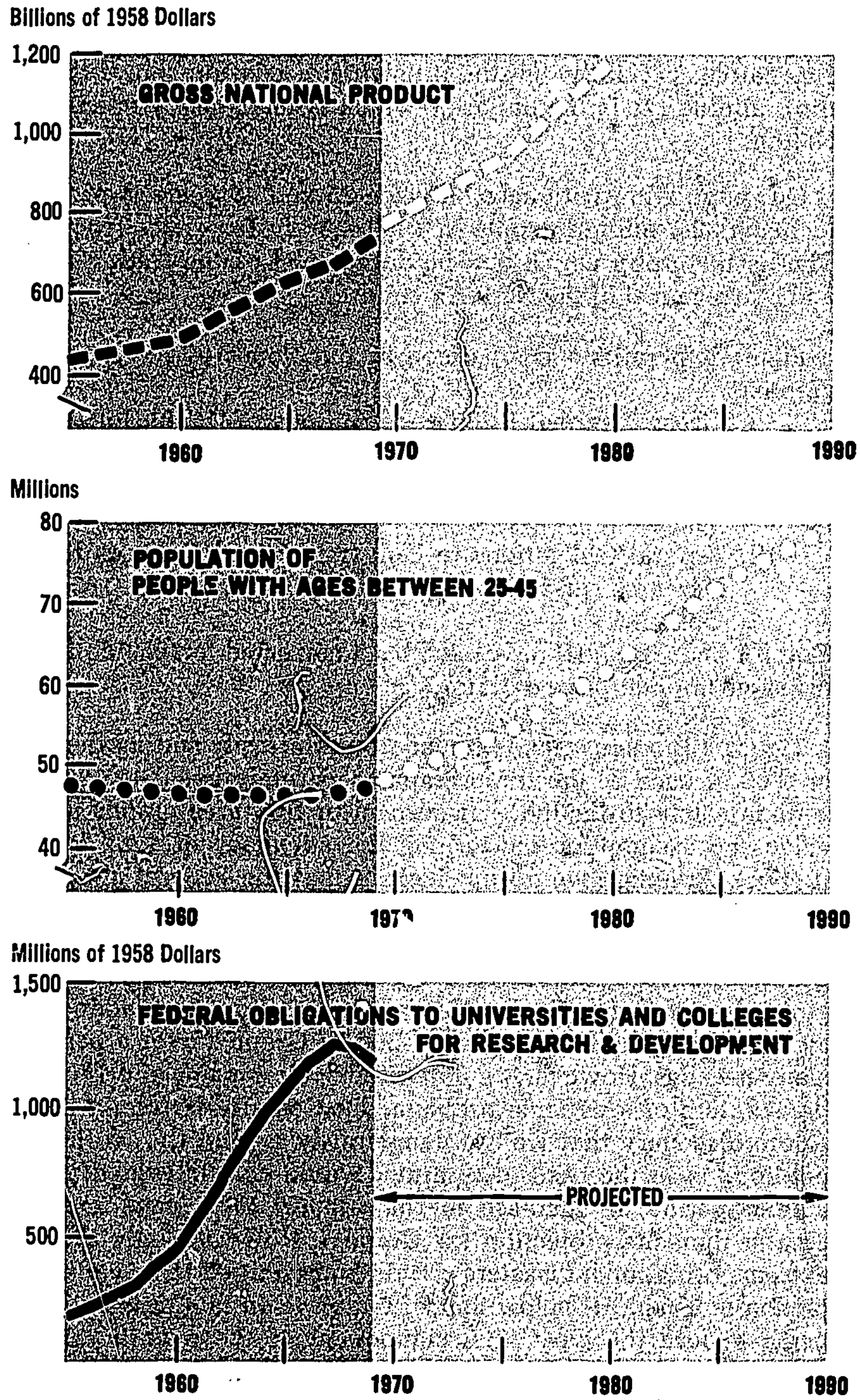
II. Population in Millions of People with Ages between 25 and 45 Years.[5]

III. Federal Obligations to Universities and Colleges for Research and Development in Millions of (1958) Dollars.[6]

Graph I and Graph III have each been corrected to 1958 dollars by the same implicit price deflator.[7]

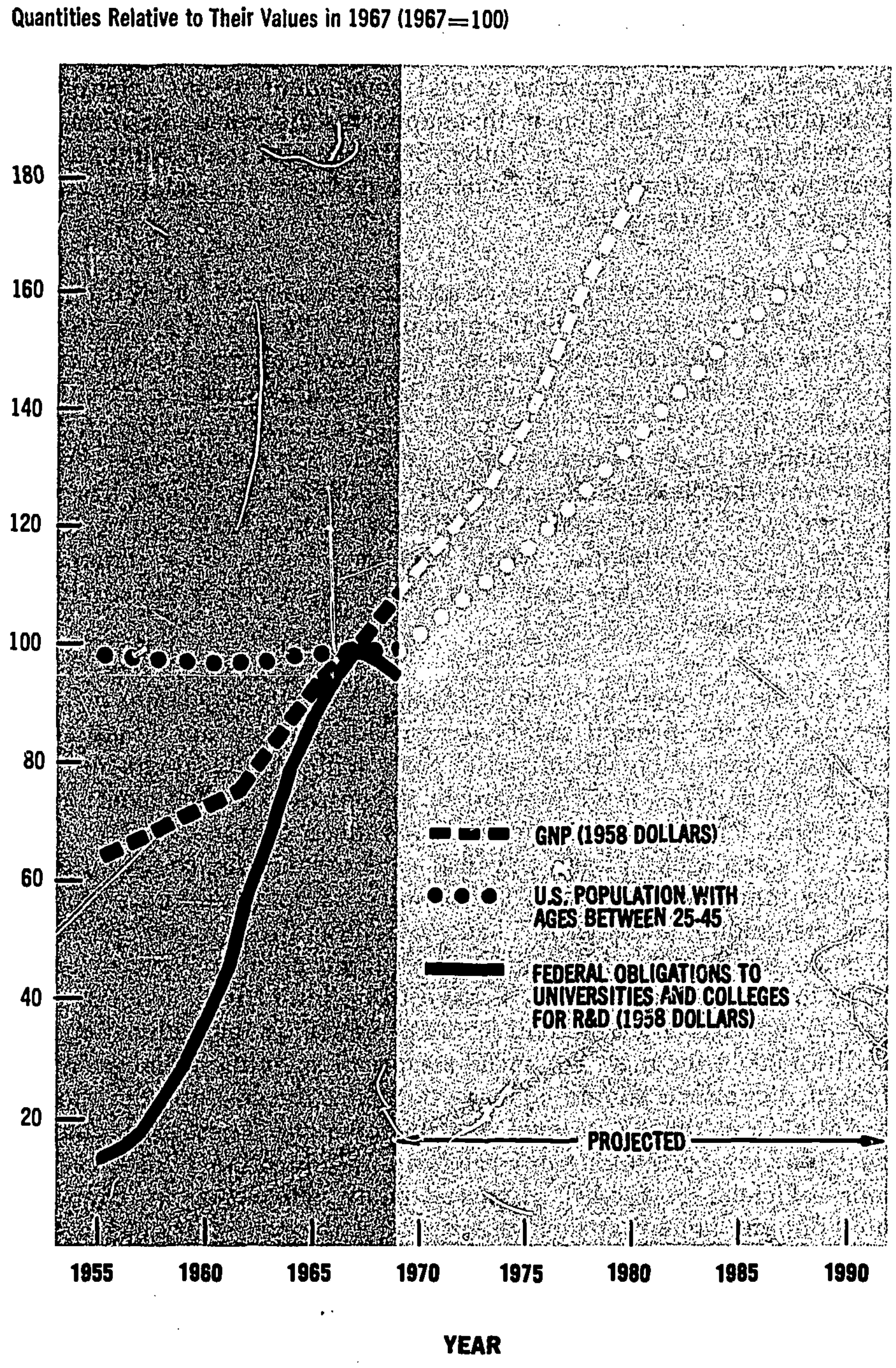
Figure 2 is a superposition made by arbitrarily setting the value in 1967 of each of the three graphs separately equal to 100. This is done solely to facilitate discussion of the three graphs and involves no assumption concerning the adequacy of 1967 level of support. Although we do not believe that the Federal support of research and development in academic institutions in 1967 was an optimal figure, we do believe that in general the projection of Graph III should fall between the projection of Graph I and the projection of Graph II. The Nation cannot afford to sustain a projection of Graph III in excess of that of Graph I over a period of many decades. However, an opportunity for a major effort towards the solution of a national problem, such as air or water pollution or the discovery that we are dangerously behind the world competition, as in the case of Sputnik, would certainly justify for a limited period an increase in the support of science at a rate greater than the projection for Graph I. However, only the most extreme of national disasters should be permitted to drive the projection of Graph III below that of Graph II. In that case we would begin to lose the most vital component of the overall scientific enterprise—newly trained young people. We will not be able to

Figure 1



Billions of 1958 Dollars
1,200
1,000
800
600
400
GROSS NATIONAL PRODUCT
1960
1970
1980
1990
Millions
80
70
60
50
40
POPULATION OF
PEOPLE WITH AGES BETWEEN 25-45
1960
1980
1990
Millions of 1958 Dollars
1,500
1,000
500
FEDERAL OBLIGATIONS TO UNIVERSITIES AND COLLEGES
FOR RESEARCH & DEVELOPMENT
PROJECTED
1960
1970
1980
1990

Figure 2 **A COMPARISON OF GROWTH**



supply the rapidly growing group of adults with the jobs, goods, and services which it both deserves and expects, if the Federal Government does not restore reasonable growth to its support of the sciences. That support is a vital component of our scientific enterprise, and the flagging of that support is the cause of our deep concern for the future. The United States cannot long maintain a position of leadership if it is dependent upon the science and scientists of other nations.

Failing that third alternative of adequate resources, the National Science Board is strongly of the opinion that the second alternative is far to be preferred over the first. The second alternative will not really "save" much money and has risks similar to those of withdrawing the people within the castle walls to endure a siege. The surrounding fields go unharvested and the later sortie may fail. However, the National Science Board believes those risks are to be preferred to immediate capitulation to eventual mediocrity. Further, such capitulation would involve reneging on a national commitment which has now been reiterated by three successive administrations.

However, strongly favoring the third alternative does not mean we believe the present establishment for science and graduate education should be subsidized to grow without any modification of present patterns or without any birth control exercised over new laboratories or new institutions of advanced training. It is clear that in some areas of the physical sciences the instrumentation needed for frontier research is becoming so complex and expensive that additional ways will have to be found for greater sharing of such facilities among numerous research groups. We also believe that additional ways will have to be found for encouraging greater specialization and division of labor among educational institutions in the most advanced sectors of scientific training. We feel further efforts should be made to reduce the cost and duration of training to the Ph.D. level or its equivalent. These and other possible ways of making our science and education efforts even more efficient are further discussed below. However, we cannot hold out any hope that such changes offer the possibility of maintaining the productivity of our scientific enterprise on a level budget, and we urge in the strongest terms that any attempt to do so would be a costly deception of the Nation.

This country's research productivity in the physical sciences will certainly be condemned to mediocrity if a number of factors continue to converge as they have in recent years: (1) the inability to gamble on young investigators without emasculating existing productive programs, (2) the inability to make modern research instrumentation available to Ph.D.-granting institutions, and (3) rising research costs, which leave almost every single effort underfunded. A concentration of the available money spent on a smaller total number of projects or institutions might well be more productive in terms of significant scientific results, and the scientists graduated might well have a better research training. Such a concentration of support, however, is not recommended because it runs counter to the needs for first-class training opportunities to be widely dispersed geographically and equality of access by students to graduate training based on ability alone without reference to place of residence or economic status. Having reviewed these matters we can only conclude that the problem is truly one of underfunding, not overextension. We do recommend, however, that the state governments through their individual coordinating mechanisms exercise restraint on the number of colleges and universities that are authorized to offer the Ph.D. degree.[8]

2. RESEARCH TRAINING

There is a need for a basic reexamination of the assumptions underlying doctoral training in the physical sciences. The present doctorate was designed primarily as training for an academic career. At least since 1935, however, more than fifty percent of those with doctoral degrees in the physical sciences have taken their first job with a nonacademic institution.[9] The present training is based on the assumption that there should be no difference in the training of those heading for a university teaching and research career and of those aiming primarily at college teaching or industrial research. This assumption may well be justified, but it is being increasingly questioned. Ten years beyond the Ph.D. only about thirty percent of physical scientists in the National Register list their primary activity as research and development. On the average, only about twenty percent of the physical scientists who receive the Ph.D. continue to contribute to the basic scientific literature over the long term. Such facts indicate that this problem needs serious investigation with the aid of detailed empirical studies of the career experiences of persons who hold a doctoral degree in the physical sciences.

The problem may not be so much one of the content of the educational experience as it is of the attitudes and values communicated by the organization and orientation of the graduate school. Experience in basic research on significant scientific problems is excellent preparation for more applied work and for technically oriented management and administration. The laboratory techniques of basic research today often become the techniques of applied research and engineering a few years hence. The critical attitudes and intellectual standards characteristic of the best research have an important carry-over into more applied or problem-solving activities, and basic research is probably an ideal vehicle for learning such approaches. On the other hand, the transfer of these attitudes and approaches to other areas of technical activity may not be automatic and might be more emphasized in instruction. Basic research training also carries the hazard of overemphasis on a narrow problem area or on a set of specialized techniques.

There is increasing belief that a somewhat different type of training, equivalent in intellectual stature to the present Ph.D. but aimed more suitably for nonresearch careers, should be available. Such training would still involve basic research experience, but possibly with greater breadth and variety and less depth and specialization than the present degree. In the light of evolving industrial needs and changing social priorities, a more nearly fixed time period, less sharp specialization, and less emphasis on an original, discrete contribution to knowledge should all be considered as possibilities in any review of the doctoral program. Consideration should be given to providing the student with a wider diversity of opportunities as he pursues his education. One possibility would be the establishment of practitioners degrees at the doctoral level. Deep specialization and an original research contribution might well be reserved for postdoctoral experience for students showing talent for making such contributions to science.

Further, wherever interdisciplinary education exists at the graduate level in universities, it provides a route for rapid transfer of new science into engineering or applied effort. Such programs are often especially useful in improving or expediting communications between industry or Government and the academic world. Good multidisciplinary programs, however, are still less common than is desirable. Some reluctance to initiate work spanning the traditional branches of the physical and life sciences, engineering, and the social sciences is clearly based on conservatism, but there are also very legitimate concerns. There is the danger that wide focus may generate superficiality because there are fewer external standards against which to judge quality and success. The costs of initiation may be much higher than those for beginning new work in an established field. The potential rewards in terms of increased exchange of ideas and concepts are so great, however, that special efforts should be made to stimulate and support good multidisciplinary programs. This would provide many opportunities for industry, with its already more effective interdisciplinary structure, to nurture a similar mood in the graduate teaching of physical science.

3. SOCIAL IMPLICATIONS

There is an increasing interest in the social and political implications of advances in technology and concern with the ways in which science is utilized by society. We welcome this increased interest and concern as an opportunity for physical scientists in particular to establish greater intellectual connection with the rest of society, with the rest of the university, and especially with the younger generation.

Many students feel increasingly threatened and alienated by technology and often confuse science with technology. To an unhealthy degree today's undergraduate regards study beyond the elementary level of the physical sciences as suitable only for the future specialist. Substantial effort and thought on the part of physical scientists both inside and outside universities will be required to reverse this situation, but this effort appears to be important in order to improve the ability of our social institutions to deal with the increasingly complex effects of technology. The current interest in the implications of technology may help provide more effective communication within the academic community. In addition, the problem of assessing the potential effects of technology and evaluating the increasing range of alternative technologies available to society can provide an important new motivation for both basic and applied research in the physical sciences.

4. CLASSIFIED RESEARCH

Classified research in the physical sciences was originally undertaken in universities as an emergency measure because that was where the necessary competence existed. Recently there has been a trend towards phasing out such activities, and the Department of Defense is to be applauded for reexamining its classification policies for university research and in many cases for declassifying the work. In the long run, classified research is incompatible with the principle of an open scientific community and with the concept of science as public knowledge, open to criticism and verification by the entire scientific community. Just as there is a perennial problem with industrial security, so may governmental classification impede efficient communication. Such classification, which may provide a measure of security in the short run, may also retard progress and thus reduce security as well as the social benefits of science in the long run. Thus secrecy in physical science research can seldom be justified as in the long-term national interest and is especially incompatible with the character of the academic community and the requirements for the training of students. Mission-oriented agencies should be encouraged to reexamine continually their classification and publication policies with a view to increasing their openness to free dissemination and criticism. This applies with particular force to academic science but is in the national interest even with respect to scientific activities of other types of institutions. The National Science Board recognizes that complete openness is not possible in every single instance, but it believes that the burden of argument should always lie with those who advocate restriction of the flow of scientific information and that the process of security classification of research should itself be subject to the scrutiny of disinterested parties.

B. THE GOVERNMENT

In 1939, President Franklin D. Roosevelt was explicitly informed of the German discovery of nuclear fission and of its implication that a large-scale release of atomic energy might be practicable. Physicists of that time found it necessary to call upon such a legendary figure as Einstein to urge action by the President because the significance of this discovery was not immediately apparent to other levels of the Federal Government. Furthermore, the Government had no avenue for exploiting it. It was fortunate that our Nazi adversaries had none either. Nor did they have the means of getting the attention of their leader in order to create the vast enterprise required for making nuclear weapons. We may be thankful that the situation was not reversed. Today, involvement of the Federal Government in the physical sciences precludes such a major disaster because there is little chance that even a minor advance in fundamental knowledge will escape attention. Failure to carry an advance into application is usually the result of a considered political or administrative decision that it is undesirable because of cost or other reason. The participation of many agencies of Government in the basic physical science research enterprise of the whole Nation has the vital effect of keeping these agencies aware of the forefront of technology and of assuring them of the opportunity for acquisition and assimilation of new basic knowledge as fast as it is developed.

1. BASIC RESEARCH

The tremendously important historical role played by various components of the Department of Defense in pioneering support of basic research in the physical sciences and in the application of new techniques to basic science must not be overlooked. In the years since World War II the Department of Defense has interpreted mission relevance in a liberal and enlightened way. This permitted and encouraged the development and application of new techniques developed under defense auspices within a wide scientific context. There are many areas in which defense support has played a key role. It was the Office of Naval Research that supported the development of the commercial helium liquefier that made low-temperature techniques widely accessible. It largely pioneered the field of radio astronomy and, more recently, the use of cyrogenic techniques for particle accelerators. Through the Advanced Research Projects Agency, the Department of Defense has supported development of the most sophisticated computer software. It has also been partly responsible for the dramatic revival of atomic and molecular physics. In general, the Department of Defense has shown an ability to move quickly to exploit new scientific opportunities and to move with sufficient resources to make a large impact in a short time, as it did with lasers and nonlinear optics. The Department of Defense also pioneered the project support system which permits individual scientists throughout the country to compete on a national basis for the available funds on the merits of their proposals. There is no doubt that national competition has made a tremendous contribution to the high average quality of work supported by Federal funds. In the current public disenchantment with many defense-supported activities, it would be tragic if the unusual and innovative role of the Department of Defense in the basic physical sciences was lost.

Each Federal agency which supports basic research has made similar contributions to the total scientific enterprise. The Atomic Energy Commission created national laboratories which, in addition to pursuing their own research missions, are models of effective service to the university community. The National Institutes of Health has an unusual and successful mixture of intramural and extramural programs. These health-related programs have rightfully included a great deal of modern chemistry. The National Aeronautics and Space Administration has provided a space capability for future exploitation. However, even these characteristics cannot insure success of the overall national effort. Increasingly the Department of Defense and other mission-oriented agencies have felt unable to provide the more stable, long-run support which is so essential. The National Science Foundation has the responsibility to insure such stable support. However, at present it is inadequately funded for the job.

2. MISSION-ORIENTED RESEARCH

In the physical sciences, mission-oriented support has been an important source of intellectual stimulation and should be continued. Mission-oriented work often presents new scientific opportunities which would not have been recognized without the stimulation of the mission even though later evolution of the work might take it well beyond the scope of the original mission-oriented problem. Radar and radio astronomy, for example, would probably never have been undertaken for their own sake if large radar antennas had not been built originally for applied purposes. Development of pure materials for technological application has enormously stimulated fundamental investigation in solid-state science by providing reproducible materials for study. Technological support for the development of superconducting magnets came largely from the Atomic Energy Commission and was justified primarily by the needs of research programs directed toward controlled thermonuclear reactions, and secondarily by the need for cloud chamber magnets. The Atomic Energy Commission development, support, and demand for such magnets resulted in a commercial availability which has brought enormous benefits to general, solid-state science. The specialized interests or perspective of mission-oriented agencies provides stimuli for new instrumentation and new experimental techniques which purely scientific motivation might not generate. Computers provide a generalized example of this. The scale of support for computers for defense and space purposes has resulted in benefits to all branches of science which would not have been available if computers had been developed only for their scientific value. Mission-oriented support in universities helps assure that new techniques flow into the general scientific enterprise.

Mission-oriented support of basic research in the physical sciences has also helped set high scientific standards for the applied efforts. The success of United States science in comparison to that of other countries owes a great deal to the close involvement of academic science with mission-oriented support, and the success of United States technology owes much to its close association with basic science. While this is especially true in the physical sciences, newer, social-problem-oriented agencies may also benefit from a similar pattern.

3. RESEARCH FACILITIES

Large installations at many Federal laboratories are somewhat overequipped in comparison with the staff available to use the facilities, largely as a result of manpower ceilings on those laboratories. Conversely, there is now a serious deficiency of funding in academic institutions in relation to the number of scientists qualified to do good research with the equipment currently available.

Consequently, a disproportionate fraction of our total resources is going into sustaining programs and facilities in existing laboratories and not enough into new starts. This situation is quite serious in the research programs concerned with the physics of nuclear structure and of elementary particles where the present plant is seriously underutilized and threatens to become more so as major new facilities come on line. Accelerators have grown in size, complexity, and cost from the relatively primitive cyclotrons built in the 1940's and the 1950's to such major national facilities as the Stanford Linear Accelerator. Agencies supporting programs in nuclear-structure physics and in elementary-particle physics do not have the funds to exploit all the existing facilities to the full extent of their capability and at the same time to provide for the new ones under construction or in the conceptual-design stages.

If we are to have a balanced program, however, there is no choice but to move ahead with the design, construction, and operation of accelerators with new capabilities, as well as modern ancillary facilities, such as bubble chambers, optical spark chambers, filmless spark chambers, on-line computers, and other tools used for the detection and observation of particle interactions. Such construction must proceed even if it means curtailed programs at existing machines, though in the long run such practice may turn out to be a false economy.

The new 200 Bev accelerator near Chicago is well underway. Another major advanced accelerator is under construction at Los Alamos, New Mexico. At such accelerators about eighty to eighty-five percent of the annual cost would be necessary to keep the operation going without doing any research. Thus, the remaining fifteen to twenty percent of the support is the margin to cover experimentation and innovation. Furthermore, the costs escalate at about eight percent per year. Consequently, on a level budget the margin for science will rapidly shrink to the vanishing point unless some installations are phased out altogether in order to release funds to support the remaining laboratories and the new laboratories that will soon come on line. We have already discussed the probable futility of a policy of undue shrinkage.

In both radio and optical astronomy the situation is somewhat different. Here there is a deficiency of equipment of frontier observing capability but still well within the current state of the art of design and construction. In the last three years Federal support for astronomy instrumentation has been dictated almost exclusively by economic considerations and has not reflected scientific needs and opportunities. There is also a much larger demand for observing time than can be met by existing facilities at full utilization. Here a shortage of funds for investment in modern sensing and data-processing equipment has prevented astronomers and astrophysicists from getting the most out of the existing facilities.

There is always the problem of striking a balance between large frontier research facilities and smaller research projects. This problem is acute in many areas—radio astronomy, optical astronomy, nuclear-structure physics, space science, and an increasing number of subareas of chemistry and solid-state science which require special instrumentation, such as low-energy particle accelerators, high magnetic fields, high-pressure equipment, or very low temperatures. There is need for greater specialization in instrumentation and greater division of labor among institutions and research groups. Regional facilities may become increasingly important, but such facilities require resident staff to keep the program vital, to continue to develop new techniques, and to help visiting scientists with the latest instrumentation. The present pattern of users groups in high-energy physics is likely to spread increasingly to other areas of the physical sciences in spite of great organizational difficulties. The trend towards on-line instrumentation and control of experimental variables by computers, which is now so pronounced in nuclear-structure physics, is also appearing in other parts of physics and chemistry. Such a trend must be made compatible with the pattern of decentralized research which has proved so stimulating to United States productivity. In a period of increasing centralization of front-rank facilities and research techniques, we must preserve a healthy measure of institutional and individual competition while avoiding a self-defeating scramble for resources. Doing so will not be easy and will not be cheap, but it has been demonstrated that this can be done; we must proceed to do it on a larger scale.

The balance between necessary regionalization and centralization on the one hand and the many necessary autonomous research groups on the other is very difficult to establish in practice.

Smaller facilities, readily accessible to local faculty and students, are very important in the design of experiments and in optimizing them before making use of major facilities. The high cost of experimentation with frontier research facilities makes careful preliminary design and testing mandatory. Thus, the decision between national facilities and local research support is not a case of one or the other. An extreme in either direction makes for a less productive scientific enterprise. Local facilities, moreover, usually have a much quicker response time in following up new opportunities and new discoveries made with major facilities. There is a constructive interaction between local and national experimental capability; between centralized and decentralized facilities.

It is essential that frontier instrumentation be made available for the most significant experiments. Resident staff at installations with unique facilities must compete with qualified outsiders for instrument time. A system of peer judgments for this purpose has been well institutionalized in the case of higher-energy physics. It may become increasingly necessary in other fields as well.

An especially acute problem may arise in the case of facilities funded by a mission-oriented agency for a rather specific purpose but which have more general scientific value. Examples are the Arecibo antenna built for ionospheric scattering work; the Goldstone space-tracking antenna of the Jet Propulsion Laboratory, National Aeronautics and Space Administration; and the Haystack radar of the Lincoln Laboratory. The subject of radar planetary physics has developed largely as a by-product of military radar development. Other examples are the reactor at the National Bureau of Standards, the satellites of the National Aeronautics and Space Administration, and the rocket astronomy capability at the White Sands Proving Ground. Declining priority for a particular applied mission should not justify closing down a unique facility if its potential productivity in a scientific context beyond the mission of the supporting agency justifies maintaining it. This consideration, of course, extends to modifications and capital improvements of such instrumentation. Again Arecibo and Haystack provide good examples. A similar issue arises in connection with nuclear chemistry and solid-state work on accelerators which may have become outmoded for nuclear physics. There are problems both of changing priorities as scientific opportunities unfold and of handling the associated funding problems among agencies.

We heartily endorse the recently announced Government policy of making the facilities of Federal laboratories available as far as possible to the academic community even when the work does not meet the strictest test of mission relevance. The implementation of this policy will require careful coordination at the Federal level. Unilateral action by mission-oriented agencies without due regard to the effect on the development of science can be very wasteful. The alternative to such waste is more coordinated planning for basic science on an interagency basis together with flexibility for reprogramming of the budgetary responsibility among agencies that must go with such coordinated effort.

The National Science Board has studied résumés of needs for new major facilities for the physical sciences. No group can state a detailed set of absolute priorities over such a broad spectrum as presented by the physical sciences. We can, however, state certain principles which we think should govern detailed selection in any field of science.

1. New proposals for one-of-a-kind facilities which are within the state of the art and offer significant advances in the range of parameters which can be studied should generally be given preference over duplication of existing facilities for additional research groups.

2. The scientific importance and novelty of a proposal should be given greater weight in the choice of major facilities than any alleged applications because experience seems to demonstrate that in the long run this also leads to the greatest impact on technology.

3. Although the capability to attack identified scientific problems should be a major consideration in choosing facilities, it should also be recognized that any extension of measurement capabilities into new domains of physical conditions is likely to yield unanticipated discoveries. Thus, extension of capability by itself should be given considerable weight even when the problems to be attacked cannot yet be clearly formulated.

4. As a rule, at comparable cost levels, general-purpose or multipurpose facilities should be given priority over special-purpose facilities with a limited domain of scientific useful-

ness. Total probable impact on a group of sciences or applications should be given more weight than impact over a more limited span of problems, no matter how challenging.

5. Complementarity to facilities available in other countries should be given important weight. Maximum advantage should be taken of exchange of scientists and auxiliary equipment, such as sensors and computers. Advice of foreign scientists will frequently be useful before reaching final commitments; this will facilitate cooperation later.

6. The reputation and career commitment of the scientists backing a proposal for novel equipment or major experiments are important factors in choice.

7. Where there exists a number of proposals of a similar type, effort should be made to force a community consensus on the best approach within the resources available. As an illustration, there are a large number of proposals for heavy-ion accelerators and at least four proposals for high-intensity pulsed reactors under consideration at the present time. It is important that at least one of each type of these facilities be built in the near future, and it would be disastrous if the competing proposals were used as an excuse for inaction. However, an initial decision cannot await complete agreement on the best approach on the part of all those most concerned.

8. Operating costs should be factored realistically into budget projections when a project is initiated.

9. Mechanisms for the screening of experiments and projects proposed for major reseach facilities should be established to insure that each such facility is used in the most productive way possible. As far as possible, projects proposed by foreign scientists should compete on an equal basis with proposals of United States scientists.

4. UNITED STATES CAPABILITY IN SPACE

During 1969 the Apollo and Mariner successes dramatically demonstrated the magnificent capability of the United States space program. If we are to take advantage of this capability, it is imperative that a detailed future program be planned and that funds be made available for the development of the research programs and appropriate instrumentation. The trend to decrease funding for space science should be reversed. Space experiments generally must be planned at least five years in advance in order to allow for the development and production of suitable flight hardware and for complementary ground-based research. Provisions must be made for more active participation in these research programs by the academic scientific community. Adequate support for collateral ground-based, balloon, and rocket investigations is a current problem. The biggest problem, however, exists in the support of optical and radio astronomy, and the need for additional observational facilities in the Southern Hemisphere is especially crucial. Future decisions concerning high-energy particle accelerators may be guided by the results of cosmic-ray studies. The Soviet Union, through its proton satellite series, has already shown that significant elementary-particle physics experiments can be done in space. The United States now enjoys generally recognized leadership in space science. It should be realized, however, that this situation can change easily if we lose our best people from the space program.

C. INDUSTRY

1. THE NATURE OF INDUSTRIAL RESEARCH

The industrial research system differs from the research system in universities. The problems faced in an applied laboratory generally require an approach such as the following: (1) to define the problem so that the answers will bear as directly as possible on the application; (2) to do whatever experiments are needed to answer the question in the stated terms; and (3) to translate the answers into a process or product design. By way of contrast, the fundamental objective of most nonmission-oriented research should be the production of answers to problems in a form that allows the answers to be generalized as much as possible. In principle, the methods of such research are: (1) to conceive of the simplest experiment that will yield a useful answer; (2) to do the experiment with enough care and cunning so as to produce a substantially reliable answer; and (3) to extract all logically permissible conclusions and inferences from that answer. However, mission- and nonmission-oriented research interact, and productive interaction requires that each develop a distinctive style.

Research oriented toward generalization is most often found in the university laboratories; it is done by specialists and is often referred to as "pure" research, a completely inappropriate term implying an impossible value judgment. The generalized conclusions from research in university laboratories are continuously useful in applied research laboratories. Conversely, the existence of applied laboratories helps keep the specialists from having a sterile role in our society. Friction can arise because practitioners of each kind of research may conclude that the men in the other camp do not know the objectives of "real" research.

There is, however, considerable overlap in styles and purposes. Although generality is not the prime objective of most research in an industrial laboratory, important generalizable results may come from the work of any alert investigator. There are many examples of great science motivated by industrial pressures. One is Langmuir's work on tungsten, which is the base of the electric lamp industry. Electric lamps were being built but were expensive and had short lives. Langmuir, looking at that product, was motivated into a research program that concerned the economics of light bulb production, and his work also earned him a Nobel Prize. Another example is the transistor. The group that developed it saw needs in the electrical industry. The transistor revolutionized that industry, and the scientists who led the work in the industrial environment also received the Nobel Prize. If one studies such historical examples, one is impressed by how heavily the industrial work depended on related work going on in universities and Government laboratories.

Efficient communication among industry, Government, and universities requires a free flow of information. There is a tendency in industry to "overclassify" its work because of the perennial problem of industrial security. It appears more and more that the most dynamic industries, in a technical sense, are freer and freer with the flow of their research information. A careful distinction must be made between research information and engineering or technological information. Industrial firms, however, should consider whether they are overclassifying their research results and, thereby, inhibiting the flow of information and, in a sense, their own technical growth.

The most important interaction between industry and universities occurs because scientists who enter industrial laboratories are educated in universities, the principal location of general research. Casual critics often question the logic of training in one research system for a career in another. The orientation, however, of good university research toward generalizable conclusions provides students with a basis for flexible reaction to the necessary changes of objectives that must be characteristic of the program in a dynamic industrial laboratory. The belief that general solutions to physical problems exist and can be discovered by systematic study is inculcated, not as doctrine, but as experience, and what is learned is taken along to the new laboratory. This flow of recent graduates is probably the most effective tie between the two research systems.

Another profitable mechanism is the flow of senior staff among industry, Government, and universities: industrial people on leave for a limited period of time doing research at university laboratories, conducting seminars and courses; and university people in residence at industrial laboratories. This occurs on a limited basis. The major inhibitions against greater interchange of people are the concern in the industry to protect proprietary information and the attitudes in some universities. Both attitudes require modification, and increased exposure will help do that as well. Not all wisdom resides on the campus, and there are areas of science that are paced in industrial research laboratories. After industrial experience the faculty can profoundly influence the style and attitude of their students. Interchange between Government and both industry and universities would provide similar benefits.

An important sidelight of this problem arises because large industrial research laboratories usually are found only in large firms. The smaller firms would also benefit from such exchanges of people, but find them difficult to arrange because of their limited manpower. This whole area requires a thoughtful study to determine a mechanism that will accomplish the desired result.

The state of research in industry seems healthy, but it does face problems. Even companies which have been leaders in research now find themselves caught by the narrow margin that separates profit from loss even in the face of continuing expansion of the total volume of business. Those companies which reinvest four to five percent of their income in research do so with an acute awareness that the time lag cannot be long between most of their research and some reflection of it in their profit margin.

2. THE ROLE OF INDUSTRY

The role of industry in the research effort in the physical sciences is vital. Industry provides the mechanism by which the results of research are translated into goods and services for the use of society and also builds the instruments and facilities which are used to advance the sciences.

Industry's interest in research is motivated by profit. In large measure, profit is generated by the reduction of the fabrication cost and by the introduction of new products. In our society, moreover, cost reduction and new products are heavily dependent on technology. Contemporary technology, in turn, relies more and more on science and on scientific advances through research.

A firm must be aware of the structure of relevant scientific fields, the research activities in those fields, and the impact that the results will have on technology and on the profit of the firm. This knowledge provides a view of the technical options available to the firm and to its competitors. Providing this flow of information into the firm is one of the functions of research within a company.

Some sentiment has been expressed that all research which ultimately benefits industry should be done in industrial laboratories. Such a system, if it could be instituted, would be prodigiously expensive in our competitive economy. Most new scientific knowledge would become proprietary because no company could afford to release results until it felt it had exhausted the technological implications or until it was certain that competitors had the same information. The costs in duplication of effort and delay in dissemination of knowledge would be horrendous. For this reason many kinds of scientific research must be done in the public domain or not at all.

Since university research has great benefit to related industries, the latter have a vested interest in the health of university programs. This interest is often expressed in the form of direct financial support of university research and education, but such direct support provides only a small portion of the total need. Some companies are responding to the current crisis in financing university research by increasing their gifts. Unfortunately, there is no real prospect that direct industrial subsidy can provide any large part of the required funds. Fundamentally, the reason for this also lies in the nature of our competitive economy. To achieve a large increase in the direct subsidy of university research, some system would be needed to spread responsibility rather evenly among all members of a competitive industry. At the present time the most equitable such system appears to be to continue allotting the money to the Federal Government in the form of taxes and to have the Government continue to reinvest a suitable portion of that income in the basic research needed to sustain the economy.

REFERENCES

1. "Ground-Based Astronomy: A Ten-Year Program," National Academy of Sciences-National Research Council, 1964.
2. "Chemistry: Opportunities and Needs," National Academy of Sciences-National Research Council, 1965.
3. "Physics: Survey and Outlook," National Academy of Sciences-National Research Council, 1966.
4. National Planning Association, Economic Projection Series, Report No. 68-N-1.
5. Bureau of Census, Population Estimates, Series P-25, No. 381, December 18, 1967.
6. Annual Survey of Federal Funds for Research, Development, and Other Scientific Activities, Vol. XVIII, Table C-97, NSF 69-39, January 1970.
7. Economic Report of the President, January 1969.
8. "Toward A Public Policy for Graduate Education in the Sciences," National Science Board, 1969, NSB 69–1.
9. "Career Patterns Report No. 2, Careers of Ph.D.'s," National Academy of Sciences-National Research Council, 1968.

U. S. GOVERNMENT PRINTING OFFICE : 1970 O - 373-722

NATIONAL SCIENCE BOARD

PHILIP HANDLER (Chairman, National Science Board), President, National Academy of Sciences
E. R. PIORE (Vice Chairman, National Science Board), Vice President and Chief Scientist, International Business Machines Corporation

R. H. BING, Rudolph E. Langer Professor of Mathematics, The University of Wisconsin
HARVEY BROOKS, Gordon McKay Professor of Applied Physics and Dean of Engineering and Applied Physics, Harvard University
MARY I. BUNTING, President, Radcliffe College
H. E. CARTER, Vice Chancellor for Academic Affairs, University of Illinois, Urbana-Champaign Campus
WILLIAM A. FOWLER, Professor of Physics, California Institute of Technology
JULIAN R. GOLDSMITH, Charles E. Merriam Distinguished Service Professor and Chairman, Department of the Physical Sciences University of Chicago
NORMAN HACKERMAN, President, The University of Texas at Austin
WILLIAM W. HAGERTY, President, Drexel Institute of Technology
CLIFFORD M. HARDIN, Secretary of Agriculture
ROGER W. HEYNS, Chancellor, University of California at Berkeley
CHARLES F. JONES, President, Humble Oil & Refining Company
THOMAS F. JONES, JR., President, University of South Carolina
***WILLIAM D. McELROY,** Director, National Science Foundation
JAMES G. MARCH, Professor of Psychology and Sociology, School of Social Sciences, University of California at Irvine
ROBERT S. MORISON, Professor of Biology and Director, Division of Biological Sciences, Cornell University
GROVER E. MURRAY, President, Texas Tech University
HARVEY PICKER, Chairman of the Board, Picker Corporation
MINA S. REES, President, The Graduate Division, The City University of New York
JOSEPH M. REYNOLDS, Boyd Professor of Physics and Vice President for Instruction and Research, Louisiana State University, Baton Rouge
FREDERICK E. SMITH, Professor of Resource Ecology, Graduate School of Design, Harvard University
ATHELSTAN F. SPILHAUS, President, Aqua International, Inc.
RICHARD H. SULLIVAN, Managing Director, American Book Publishers Council, Incorporated
F. P. THIEME, President, University of Colorado
VERNICE ANDERSON, Secretary, National Science Board

*Succeeded Dr. Leland J. Haworth as Director on July 14, 1969.

CONSULTANTS

LEO GOLDBERG, Higgins Professor of Astronomy and Director, Harvard College Observatory, Harvard University
GEORGE S. HAMMOND, Professor of Organic Chemistry and Chairman, Division of Chemistry and Chemical Engineering, California Institute of Technology
NELSON J. LEONARD, Professor of Organic Chemistry, University of Illinois, Urbana-Champaign Campus
VICTOR McELHENY, Science Editor, *Boston Globe*
LEONARD I. SCHIFF, Professor of Physics, Stanford University