SHELF LIST

0 0301 0070737 8

MBL/WHOI

0 0301 0070737 8

RESEARCH IN THE SERVICE
OF
NATIONAL PURPOSE

Library of Congress Catalog Card No. 67-62109

For sale by the Superintendent of Documents, U.S. Government Printing Office
Washington, D.C., 20402 - Price \$2

Research
In The Service
Of National Purpose

*Proceedings
of the
Office of Naval Research
Vicennial Convocation*

Edited by
F. Joachim Weyl
Office of Naval Research

Contents

FRONTISPIECE—The Vicennial Convocation Program	iv
FOREWORD	v
THE OFFICE OF NAVAL RESEARCH AT TWENTY: THREE HISTORICAL APPRAISALS	1
Pioneering in Federal Support of Basic Research • Dr. Alan T. Waterman, <i>National Academy of Sciences</i>	3
Catalyzing Advances in Military Technology • The Honorable John S. Foster, Jr., <i>Department of Defense</i>	10
Innovating in the Support of Naval Operations • Ad- miral Horacio Rivero, USN, <i>Department of the Navy</i>	14
PRESENTATION OF EDWIN BIDWELL WILSON AWARD	19
Address • The Honorable Emilio Q. Daddario, <i>United States Congress</i>	21
The Edwin B. Wilson Award • Text of Scroll	26
Response • Dr. Allan T. Waterman, <i>National Acad- emy of Sciences</i>	27
CONVOCATION ADDRESSES • SCIENCE AND PUBLIC POLICY	29
Introduction • The Honorable Garrison Norton, <i>Na- val Research Advisory Committee</i>	31

Basic Science and Agency Missions • Dr. Harvey Brooks, <i>Harvard University</i>	33
Promises and Constraints on Science • Dr. Frederick Seitz, <i>National Academy of Sciences</i>	48
The Open World of Science • Sir Solly Zuckerman, <i>United Kingdom Ministry of Defense</i>	66
Science and National Security • Dr. William O. Baker, <i>Bell Telephone Laboratories</i>	92
KEYNOTE ADDRESS • VICENNIAL BANQUET	123
Perspectives on Naval Research • The Honorable Paul H. Nitze, <i>Department of the Navy</i>	125

OFFICE OF NAVAL RESEARCH VICENNIAL CONVOCATION

Wednesday, 4 May 1966, 9 a. m. - 4 p. m.
The Departmental Auditorium
Constitution Avenue between 12th and 14th Streets, NW
Washington, D. C.

PROGRAM

- 9:00 a.m.   **OPENING SOLEMNITIES—Navy Band**
Chairman: Dr. F. Joachim Weyl
Deputy and Chief Scientist, Office of Naval Research
INVOCATION—Rear Admiral Henry J. Rotrige, CHC, USN
APPRAISAL OF THE OFFICE OF NAVAL RESEARCH
By The Scientific Community
Dr. Alan T. Waterman
By The Makers of Military Technology
The Honorable John S. Foster, Jr.
Director of Defense Research and Engineering
Office of the Secretary of Defense
By The Fleet
Admiral Horacio Rivero
Vice Chief of Naval Operations
PRESENTATION OF FIRST EDWIN BIDWELL WILSON AWARD
The Honorable Emilio Q. Daddario
Member of Congress, Connecticut
- 10:00 a.m.   **MORNING SESSION**
Chairman: The Honorable Garrison Norton
Chairman, Naval Research Advisory Committee
Basic Science and Agency Missions
Dr. Harvey Brooks, Dean, Division of Engineering and Applied Physics,
Harvard University
Opportunities and Constraints of Science in the U. S.
Dr. Frederick Seitz, President, National Academy of Sciences
- 12:00 noon   **ADJOURNMENT FOR LUNCH**
- 2:00 p.m.   **AFTERNOON SESSION**
Chairman: Dr. E. R. Piore
Vice President and Chief Scientist, IBM
The Open World of Science
Sir Solly Zuckerman, Chief Scientific Adviser
Ministry of Defence, Great Britain
Science and National Security
Dr. William O. Baker, Vice President for Research
Bell Telephone Laboratories
- 4:00 p.m.   **ADJOURNMENT**

Foreword

These Proceedings are the record of a convocation which was held on the 4th of May, 1966, in Washington D. C. as one of the key events to mark the twentieth year since the Office of Naval Research was formally established by Act of Congress. Attended by eleven hundred guests from all over the country and abroad at the invitation of the Honorable Robert W. Morse, Assistant Secretary of the Navy for Research and Development, and Rear Admiral John K. Leydon, USN, Chief of Naval Research, it had the purpose of projecting in philosophical perspective the present day interdependence of science and Government whose history of genesis and growth is spanned by the life time and constitutes the story of the Office of Naval Research. Basic issues of principle were to be illuminated, no less than current problems of practice, and guidelines developed for the appraisal of future trends, the aiming of future action. Its theme was therefore set as Science and Public Policy.

No narrower context could have done justice to ONR's activities in the past or allowed an unconstrained examination of their possible significance for the future. This was well recognized by the messages which reached the Office from numerous friends on the occasion. Thus, a well-known physicist states that: "ONR was the model for federal support and very many of us owe the beginnings of our research program to that agency." Reference is made to "the truly major role that ONR has played in bringing the relations between Government agencies and university scientist to the present enlightened state" by another distinguished scientist, and an oceanographer of note—to quote just one more—uses the following words: "The creation of ONR, the selection of its goals and ideals, and the skill and efficiency with which these were carried out in the intervening years have served as a beacon to show the Govern-

Research and National Purpose

ment and the scientists of the United States how to cooperate and keep world leadership in scientific research and development here in this country."

Within the framework of the convocation program, the same theme reaches an even sharper focus in the addresses of the opening session which preceded the principal convocation lectures and whose task was preeminently that of providing quintessential appraisals of the Office of Naval Research—as agent on the past and as resource for the future. The three viewpoints from which this was to be undertaken correspond to the three distinct worlds which the work of ONR brings to an intersection: The scientific community; the engineering and industrial sphere of the makers of military technology, the builders of military systems; and—finally—the consumer community out at sea—the Fleet. In the end each of these assessments presents the judgments of history, the challenges of the future, in terms of the revolutionary changes which the entire process has wrought during these last two decades in the ways in which the world of science participates in and draws sustenance from the pursuit of national purposes.

These discussions found their natural conclusion in the announcement of the establishment by the National Academy of Sciences of the Edwin Bidwell Wilson Award "for outstanding contributions in the service of the Federal Government to the effectiveness of its efforts to encourage and to benefit from the advancement of science," and its conferral on Alan T. Waterman, whom the past twenty years have consistently found at the center of the developments here at issue, as its first recipient. It was our good fortune that the presentation could be made by the Honorable Emilio Q. Daddario of Connecticut, so that the ceremony acquired a certain hieratic depth in the fleeting linkage of recognition with which it tied the Office of Naval Research to the Congress and the Academy.

Thus, it set the stage for the main convocation addresses to deal at depth and in full generality with four central issues in the domain of science and public affairs. As we come to look

at the image of ONR in the mirror of this record of historical events and guiding principles, let us not yield to the temptation of falling in with an occasionally uttered opinion that all this was just a unique and accidental happening, that it all came about when, in the immediate shadow of World War II, only a military agency—unsuitable as it might otherwise be—had the strength to get it started. There is no contradicting the fact that military contingencies led to laying the foundations for the contemporary dispensation in the area of science and public affairs. However, to think of this as an historical accident would be tantamount to writing off as irrelevant the accumulated experience of one of the most important experiments in which this country has engaged during these last twenty years and which will certainly be meaningfully with us for the remainder of the century.

In the endeavor of thus bringing together what might be considered outlines and fragments of an intellectual history of Science and the Government in the post-war years—as seen from the vantage point of the Office of Naval Research—we have been generously supported by a grant from the Alfred P. Sloan Foundation. Moreover, we are deeply indebted to a large number of people who have given freely of their time and their thought to this effort. There are, first of all, the participants in the program to whom we here convey once more our warmest thanks. There is next the Program Committee which—in addition to Mr. Robert Buchanan, Mr. Robert Mindak, and Mr. Edward McCrensky of the Office of Naval Research—included Dr. John Coleman of the National Academy of Sciences, Dr. William Raney of the Office of the Assistant Secretary of the Navy for Research and Development, Dr. Randal Robertson of the National Science Foundation, and Dr. David Robinson of the Office of Science and Technology,—to help us shape our final plans and to keep their home offices apprised of them. There are, finally, all those who—under Mr. McCrensky's experienced coordination and the dedicated vigilance of Miss Joan Kazmierski in his, and Mrs. Evelyn Williams in the Chief

Research and National Purpose

Scientist's Office—helped with the numerous housekeeping chores whose effective performance made of those plans a memorable reality.

The weather was radiant, the house full, and the occasion festive. We deeply appreciate the many of you who came in person or were present by message to show your continuing respect for the past and your confidence in the future of the Office of Naval Research.

F. JOACHIM WEYL

THE OFFICE OF NAVAL RESEARCH
AT TWENTY

Three Historical Appraisals

Pioneering in Federal Support of Basic Research

by
DR. ALAN T. WATERMAN
National Academy of Sciences

The country owes a debt of gratitude to the Office of Naval Research, a fact of which the country's scientists and engineers are well aware. Its conception arose within the war-time Office of the Coordinator of Research and Development headed by the late Admiral R. A. Furer and his deputy Captain Lybrand Smith. Its immediate purpose was to provide scientific liaison with the War Department and with that novel and highly effective civilian organization, the Office of Scientific Research and Development. The latter, under its brilliant Director, Dr. Vannevar Bush, set a pattern for the broad Federal support of R and D which in much of its essential character still continues.

It is of course well known that the Federal Government has for years successfully operated research laboratories and has a long and honorable record in support of particular fields, such as agriculture and geology, in the country at large. But the principle which the OSRD pioneered was a wide extension of the traditional military R and D contracting with industry to research establishments in general, and especially to academic institutions. This enabled the Federal Government to obtain the services of the most capable scientists and engineers in the nation, wherever they might be, to do research on weapons and devices of warfare. An essential and unique provision in the OSRD charter was its authority to select individuals and teams for particular research without the requirement of competitive bidding.

An outstanding asset of the Coordinator's Office in the Navy proved, especially later, to lie in its staff of brilliant young officers, many in the Naval Reserve. Their names are worth noting as pioneers in the career of ONR: Captain (then Commander) Robert D. Conrad, John Burwell, Gordon Dyke, Ralph Krause, Bruce Old, James Parker, James Wakelin and Thomas Wilson. A bit later these were to be joined by Thomas Killian, E. R. Piore and Roger Revelle.

The foundation was laid for the ONR in June, 1945, by the establishment of the Office of Research and Inventions under Executive Order of the Secretary of the Navy. In it were gathered together the major components of what was to be the ONR under the leadership of Vice Admiral Harold Bowen as Chief and Rear Admiral Luis de Flores as Deputy Chief.

The Act establishing the Office of Naval Research as a statutory agency was engineered with the cooperation of a discerning Congress, by a distinguished and brilliant group of personalities, notably James R. Forrestal, Secretary of the Navy, Struve Hensel, General Counsel, John T. Connor, his special assistant and previously General Counsel for OSRD, and Admiral Lewis Strauss, a member of his staff. An unprecedented provision in the new Act was the establishment of two deputies to the Chief of Naval Research—Deputy and Assistant Chief, the usual post held by a Naval Officer, and Deputy and Chief Scientist, held by a scientist. Also a noteworthy feature of the new Act was the designation of a Naval Research Advisory Committee appointed by the Secretary of the Navy to advise him and the Chief of Naval Research regarding research for the Navy and the policies and programs of the ONR.

For the insight and vision that identified and clarified the research mission of the ONR the man responsible was the head of the ORI Planning Division, Captain Robert Dexter Conrad, as all who worked with him came to know. He it was who was responsible for first stressing the importance to the Navy and to the nation of "pure and imaginative" research, as expressed in the ONR enabling Act. Indeed it was

under this banner that he was able to recruit for his Research Division the scientists and engineers that have ably carried on this tradition.

A unique and highly significant contribution of the ONR was to be the initiation and development of its contract research program, especially in basic research. At first this program aroused some consternation in academic and government circles. Federal support would mean Federal control, especially from a military agency. Besides, how could one establish, on the one hand, by this means a sufficient degree of justifiable relevance to naval problems, and, on the other, would not this policy result in backing only applied research, a consequence not relished by academic institutions? The clearest expression of the ONR answer to this dilemma was given by the Basic Research Group of the Research and Development Board in the Department of Defense, as stated by its Chairman, Dr. Warren Weaver, then also Chairman of NRAC, in the following key paragraphs:

It is most strikingly and emphatically true that basic research is not impractical research. The whole history of science constitutes a most impressive proof of this statement.

There are two aspects of basic research: One, that of the researcher himself who is motivated by curiosity and interest in science rather than applicability; the other that of an experienced science administrator in an agency with a practical mission. The latter can make reasonable judgments regarding the applicability of basic research to the practical problems of his agency. In this way selected mission-related basic research may be supported by such an agency to its advantage without controlling or disturbing the aim of the investigator or the course of the research.

Time does not permit more than mention of the other research support offices that emerged at about the same time or slightly after the passage of the ONR Act, viz.: the National Institutes of Health, the Atomic Energy Commission, followed

by the precursors of what are today respectively the Office of Aerospace Research, the Army Research Office, and finally the National Science Foundation.

During the five years after the war a considerable evolution took place. Academic institutions learned that Federal aid to research was possible without Federal control. Agencies with practical missions found that even basic research done by academic professors could prove useful to them. In the physical sciences the vehicle of support was still the research contract, the only instrument available to the ONR and the other military establishments. In its adaptation to basic research the contract document underwent considerable alteration. Competitive bidding on announced problems was not necessary; nevertheless, selection among submitted proposals provided competition of another kind. Also, as in OSRD, on selected critical problems the staff sought out especially qualified scientists and engineers to do the research. However, inevitably the results were more general and less predictable. The contract form was simplified and a flexible system of accounting was worked out. Much credit for the solution of these and other administrative problems went to W. W. (Kip) Edwards, for years a civilian administrator in ONR, and to the aforementioned John T. (Jack) Connor, then ONR's General Counsel. It is a pleasure still to find on the ONR staff Edward McCrensky, whose services on personnel matters have proved so valuable over the years. In like manner, difficulties were encountered and resolved in reconciling research patents with the existing system for industrial development and production contracts. In this area great credit must be given to Captain George N. Robillard for quietly developing a comprehensive patent policy for the entire Navy and thereby reconciling long-standing Bureau difficulties as well.

A senior partner in the new office was of course the Naval Research Laboratory, with a distinguished record going back before World War I and including many illustrious directors—two, Harold Bowen and Fritz Furth, who were later to be Chiefs of Naval Research. Perhaps not many now recall that

the first demonstration in this country of a practical radar took place at NRL, when echoes were detected from passing vessels on the Potomac, using a radio pulse system, following the pioneer experiments of Tuve and Bright in obtaining a return signal from an ionized layer in the atmosphere. It may also be recalled that with characteristic Navy foresight, in the early research on the atomic bomb NRL secured from the Congress an allotment of \$100,000 for the separation of isotopes.

The Special Devices Center was noteworthy for its recognition and continuation of the outstanding pioneer work of Admiral Luis de Flores in improving the man-machine relationship expressed in the phrase "human engineering." In addition to its importance in training, this program made notable progress in aircraft and submarine instrument display design. And the Underwater Sound Reference Laboratory did valuable work in testing and maintaining standards.

Special mention should be made of the ONR Liaison Office in London, a Navy legacy from OSRD, which still continues to play a significant role in scientific liaison between the U.S. and U.K.

From these random remarks one should not conclude that the sole contribution of the ONR lay in its conception, its organization or its planning. The substantive programs initiated and sponsored by the ONR have been many and important. A partial list would include: leadership in following and assisting in the development and use of electronic digital computers; research on human engineering, with special reference to aircraft and submarines; pioneering in deep-sea research, as with the Piccard bathyscaphe, and under the Arctic ice, as with the Nautilus; high altitude balloon exploration; high altitude research rocketry, notably with the Viking and the Vanguard; a large variety of extra-terrestrial research—radio telescoping, cosmic rays, solar disturbances and radiations, and ionization layers and streams, typified by the work at NRL of Herbert Friedman and his collaborators. Likewise there

were systems developments, such as the minitrack system for world-wide satellite tracking; pioneering in automatic navigational systems; ultra-long-range undersea detection; pioneering with AEC in nuclear accelerators for universities; development of molybdenum and titanium technology; establishment of low temperature research centers throughout the country; and continuing progress in studies of hydrodynamics, turbulence and boundary layer problems.

Of especial importance to the Navy, as it always seemed to me, was the Naval Research Section for its training of young naval officers in the application of modern scientific research and in developing among the civilian components of the Planning Division a realistic sense of Navy operational problems. Indeed this Section undertook some of the earliest studies along systems analysis lines.

I greatly regret that time does not permit me to pay tribute by name to the somewhat extraordinary list of individuals who have served with ONR, both civilian and military, for their cooperative and substantial share in ONR's fine record. Their competence is well attested by the positions they have gone on to fill, in ONR and elsewhere.

The ONR will go down in history as a pioneering effort of great effectiveness, both to the Navy and to the country. Of special importance was its timeliness.

For the Navy it identified and endorsed the importance of basic research as an important key to the solution of rapidly unfolding developmental problems associated with modern sea-power, taking advantage of the lessons directly learned in protracted and all-out naval warfare. It has provided in the Navy a focal point for leadership and coordination of fundamental R and D essential to unified progress. It has recruited and established an able group of civilian scientists and engineers working in collaboration with young naval officers. It organized a central office for taking care of patents and it has provided a home for three effective research laboratories for in-house research.

For the country the ONR was able to take prompt and effective steps to restore research activities for scientists and engineers returning to their institutions, by providing for their research equipment and by helping with their salaries and those of their graduate student assistants. But more than this, in mutual consultation with academic and industrial scientists and administrators the ONR evolved policies and procedures, as I have said, which pioneered the way for increasing participation of the Federal Government in a comprehensive program of scientific research throughout the country. By this means the Federal Government has been able to enter into a most effective partnership with academic and industrial scientists and engineers, and their institutions, for the prosecution of research in the national interest and in the interest of science.

Catalyzing Advances in Military Technology

by

THE HONORABLE JOHN S. FOSTER, JR.
Director of Defense Research and Engineering

Well known to all of us, and now succinctly reviewed by Alan Waterman, is the way in which ONR supported and guided the development of basic sciences in the United States during the years following World War II. A story of equal significance is ONR's effort in devising, experimenting with, and establishing new schemes for carrying on, through ever more numerous and effective channels, the essential dialogue between the Navy and the scientific and technological community. By themselves, such schemes are simply devices for encouraging an atmosphere of effective communication, but in doing this they have assumed such surprising importance that it is useful to recall what they are. By no means were all of these ideas invented or even first tried by ONR—only some of them were, but ONR certainly developed them into useful tools.

One important device has been the highly qualified, highly motivated, broad band study group which—even if sometimes convened in the winter—is always referred to as a Summer Study. These group efforts have become commonplace, perhaps even sometimes annoying. Nevertheless, they have played the major role of providing contact between the country's technical community and its military community, in a setting in which problems can be discussed rather than requirements assigned. This direct personal discussion between experienced military and government professionals, and members of the scientific

and engineering community, has proved to be the most important modern way of transferring ideas about the nature of military problems to the scientists, and ideas about military uses of science and engineering into military technology.

A second mechanism fashioned by ONR into an important instrument of catalysis is the Working Technological Advisory Committee. Such a Committee consists of professionals active in the field, who meet with Navy people to discuss problems and to give two kinds of informal advice: advice to the Navy on what it might do, and advice to themselves on the research and development work that they themselves should do in their laboratories. This way of generating technical guidance and shaping up new research efforts has provided in its numerous variants many new ways of insuring relevance and quality in government-sponsored research.

I believe, finally, that ONR was the first to realize the advantages to the Government of the modern peacetime form of the university-operated, government-sponsored laboratory. It has established a number of such laboratories, and each has become a major leader in its field of endeavor. Because of the intimate personal relationships with the academic community, ONR has been able to foster in this manner a high degree of coupling between research interests in government laboratories and research interests in government-sponsored laboratories as well as with industrial research efforts. A remarkable number of ONR-sponsored research endeavors have been carried out by close cooperation among a mixture of government and government-sponsored laboratories, with industrial participation and assistance.

I may sum up ONR's contribution in this important field of administration by noting that, by the competence of its personnel, and by its imaginativeness, ONR established a new kind of relationship between government offices and laboratories and the scholarly community, in which both have worked together for the advancement of science and technology and the improvement of the Navy's capabilities for national defense. This spirit of cooperative endeavor that has marked

the ONR way, has paid off in research and development accomplishments on many occasions. It was this spirit, these relationships and the associated researches that made possible the great emergency effort of the search for the Thresher and the rapid development of the equipment that succeeded on that occasion, and recently was so useful in the search for the lost weapon off the Spanish Coast.

I would like to point out a few major technical accomplishments created under the guidance and leadership of the Office of Naval Research that are particularly striking examples of the way in which this imaginative management has been translated into an improved Navy, and gives promise of further improvements in the years to come. Under ONR sponsorship at the Woods Hole Oceanographic Institution, and slightly later at the Lamont Geological Observatory, the Scripps Institution of Oceanography, and Hudson Laboratories, as well as in Navy in-house laboratories and by industrial contract, ONR was in large measure responsible for the creation of what may be called "the new sonar": the long-range use of underwater sound for ASW, for probing the oceans, and for general geophysics. ONR's encouragement and cooperation, and its spirit of working with the scientists and engineers to create a new area of science was most important in making this possible. This effort included not only acoustics, as such, but major advances in oceanography and geophysics, and great accomplishments in the development of modern signal processing technique. Of equal span and length of fetch in time has been ONR's support of marine engineering research which laid the foundations for much of our recent progress in true deep water technology. Of current importance in this effort is its concern with the pioneer U.S. work on "man in the sea," a further step towards the ultimate objective of achieving access to the whole depth of the ocean.

Another characteristic example, a classic of its kind, is the origin of the concept of Polaris in the course of the Nobska Summer Study at Woods Hole in 1956 where experts in each of the key areas that proved decisive for its feasibility happened

to be together and recognized this. The result has revolutionized the modern Navy, and changed and strengthened our entire national defense posture.

While these examples are particularly clear and definite we should also recall that many other major advances in the technology of defense, were first conceived and pursued under the sponsorship of ONR because the same sponsorship had played a role in the underlying basic discoveries and their recognition as relevant to the purpose on hand. Some interesting examples include the electrostatically suspended gyro, the MASER and LASER, modern electronic navigation system, explosive echo ranging, and the understanding of the sea surface that led to modern oceanwave forecasting. American science and engineering, as well as the U.S. Navy, owe a debt of gratitude to the Office of Naval Research and to the people who have made it effective for the past 20 years. I look forward with you to great future accomplishments.

Innovating in the Support of Naval Operations

by

ADMIRAL HORACIO RIVERO, USN

Vice Chief of Naval Operations

The Office of Naval Research is like my office part of the shore establishment of the Navy, and the reason for its existence, as it is for the rest of the shore establishment, is to serve and support the operating forces of the Navy—the Fleet. I can truly say that we would be very happy if all parts of the Shore Establishment had as good a record as ONR in support of the Fleet.

In the first place, ONR has distinguished itself by flagrant violation of Parkinson's Law. In 20 years its staff, at headquarters and in the field, has grown less than one-third from some 3,750 to not quite 4,900 people, while the program it administers has multiplied about seven times—from \$50 million to some \$325 million. This is an accomplishment which well deserves emulation.

The substantive and broadly recognized contributions that ONR has made to the strength and technological advancement of the Fleet are too numerous to relate this morning. Moreover, many of them would be difficult to identify as such because of their indirect nature. To illustrate this indirectness, let me choose a minor but current example as illustration: Our recent operations off Palomares, Spain, to recover that nuclear bomb would have been impossible had we not been able to draw on the research activities being supported by ONR and on equipment the existence of which was owed strictly to research purposes. What must be emphasized is that

these contributions have kept our Navy at the forefront of our national military team.

Of course, the recognition that scientific development is vital to the readiness of our Navy dates a long way back. Let me quote from a statement by the Secretary of the Navy. "It is of little service to a nation to have any Navy at all unless it is a fair expression of the highest scientific resources of its day. The destructive power of modern implements has become so great as to dominate in actual warfare." Now this was not Secretary Nitze whom I have quoted. This was Secretary of the Navy William C. Whitney in his Annual Report dated 1885. It is just as true today as it was then and I think we all recognize, in and out of uniform, that scientific research is the very basis of all the Navy's great achievements of the past and of the present.

This year marks the 20th Anniversary of the statutory Office of Naval Research as such, but even long before Naval Research was given institutional recognition by the creation of this Office, the Navy and its people were deeply involved in scientific research and occupied in some ways positions of widely accepted leadership. An early example in which we continue to take great pride are the experiments which were conducted by our own Naval scientist A. A. Michelson at the Naval Academy when he was a professor there and which produced some of the first precise measurements of the velocity of light. Moreover, ONR's own in-house laboratory which antedates the creation of ONR, the Naval Research Laboratory, has enjoyed for many years the highest reputation throughout the land in scientific research.

From the point of view of the Fleet, however, the examples which for us represent the contributions of Naval research are the ones all around us, and—although both Dr. Waterman and Dr. Foster already mentioned it, I should like to return to the Polaris nuclear deterrent. It is—you might say—a representative contribution, indeed. Looking at its two basic components: the nuclear powered submarine and the inter-

mediate range ballistic missile, let us start out by remembering that it was the Navy who spent the first government funds ever allotted to the study of atomic fission in 1939, even before the Manhattan Project, and that the Navy generated the first interest in applications of atomic power to propulsion. ONR's contributions to inertial navigation systems, in turn, permitted us to locate our weapons launch point under the seas with sufficient accuracy to help us guide the missiles to their targets. Equally important has been ONR's early work in solid state physics, computers, and other electronics. We should recall also that ONR began the basic study of nitropolymers as early as 1947, from which was to evolve much of our current solid fuel technology, equally significant to both Polaris and the Minuteman weapons system. The technological performance levels achieved by the Polaris system culminate, of course, in the operational fact that it can be considered immune to enemy attrition by any means likely to be available in the next five years, to use the words of Mr. McNamara in his most recent Posture Statement. Here is the measure of confidence that we have in the system and of its importance to the national security.

Similar results of basic research, conducted in the course of recent years, have done their share in making possible our nuclear powered surface ships, the first of which are now making history in the South China Sea as they are deployed in action. All our ships, finally, are taking advantage daily of ONR research as they make their navigational fixes more often and more accurately with the aid of navigational satellites; they depend for much of their air defense on weapons based on infrared research which ONR had pushed and pioneered for many years beginning in the late '40s; and our current approach to command and control, with its use of communication satellites and the extensive application of automatic data processing to signal and message, owes much to the work done by ONR in the past.

There is, as I suggested, an endless procession of such examples, not only in the area of hardware but in other areas as well. From teflon to tissue transplants, from sunspots to sea depths, and from supercavitating propellers to high-altitude plastic balloons,—the universe is ONR's field of study and the Fleet has reaped the profit.

I am told that the total cost, even when most generously estimated, of the research programs which ONR has sponsored in 20 years is less than \$2 billion. We might ask ourselves what it would have cost us to attain the current level of technical readiness in our forces without the benefit of the science and technology which has sprung from this \$2 billion expenditure. Project Hindsight, a study on this and related questions conducted by the Office of the Secretary of Defense, is in the process of generating hard data in support of the general conjecture that this would have increased the cost of our military establishment by a sizable factor,—assuming all along that the same time to achieve this build-up would be available as our current commitments to learning and exploratory work have secured for the Western World.

As to the future needs of the Fleet, we also have some ideas, and it is a measure of the intimacy of the relationship that exists between ONR and the Fleet that most of these ideas probably somehow originated with and—at the least—are continually tempered by research in which the Navy participates under ONR's aegis. No doubt, some of the more important future needs of the Fleet are below the surface of the seas. This is an area in which we have done relatively little in comparison with what, as we have learned to see it, can and needs to be done. No matter how emphatic our concern with ASW, it has not yet brought us the breakthrough that would allow us to penetrate the opaque environment which shields the submarines of all nations. We are confident that we will be able to confine any submarine threat for the present with what we have available now, but beyond this there are yet greater possibilities to consider for the future. The means of

preventing that a potential enemy have free play beneath the waters, on which travels the commerce of the world, are too important to us to permit major areas or relevant technology to lie fallow. We are sure that our deep submergency projects which I briefly mentioned before will produce meaningful results and—as we learn more about the vast unexplored volumes of the ocean—develop new concepts that will most likely result in great changes in our tactics and in the strategy of Naval warfare. These are things which it is difficult even to imagine at this time.

This is not to say that we should channel all or most of our effort into the undersea area. There are many other great areas in Naval warfare which require attention and in all of these we expect ONR contributions. It has been one of the great virtues of the ONR effort that great latitude has been available to those who attack the problems of research. Their uninhibited curiosity and their competent judgment have brought the Fleet rewards which, in many cases, we had no reason to expect. There has grown up in the Navy a tradition of fostering close relations with the world of science which finds its living expression today in the existence and strength of ONR. Our confidence rests on an organization which, as early as 1949, made Dr. Vannevar Bush say of the Navy in his book on "Modern Arms and Free Men" while discussing programs of sponsored research: "The Navy in particular has done a magnificent piece of work in this field. It understands scientific and university-service relationships. . . ."

The Fleet now knows that this understanding has greatly contributed to its present strength. We shall continue to depend on ONR that we always keep it as we set our course for the uncharted waters of the future.

PRESENTATION OF THE
EDWIN BIDWELL WILSON AWARD

Address

by

THE HONORABLE EMILIO Q. DADDARIO
United States Congress

I should like to thank the Office of Naval Research and the National Academy of Sciences for inviting me to participate in this program which marks a singular event in the history of science and the Government. The skill, vigor, and imagination of the men who share with me this stage today, and the institutions which they represent, are largely responsible for the remarkable solutions which we have found here in the United States for the complex problems that are encountered when science, the university community and the Government mix.

There is first of all the National Academy of Sciences with which I have had frequent contacts during the last several years. The steadily growing span of its advisory activities has had a significant share in establishing the atmosphere of mutual reliance between the leaders in the world of science and of public affairs. Through the establishment of the Edwin B. Wilson Award, the Academy has now taken a further step in strengthening the link between science and the Government.

Next there is the Office of Naval Research whose twenty productive years of activity have been duly noted this morning for their contributions to the design and the structure of the best possible relationships between science and government. Many who have made their careers in the Federal service are alumni of the Office of Naval Research. It is a pleasure to recognize ONR as a proving ground for the ideas and for the men who are now formally to be recognized through the E. B. Wilson Award.

At the center, however, of the activities that concern us here

is the scientist in the Federal service. He is making a reality out of the broad ideals that govern the interaction of national goals and technological capabilities. It is he who brings into being, maintains, and adapts the machinery through which the processes of government act in strong support of advancement in science. It is his imagination and judgment, his ability to understand in equal measure the hopes of science and the realities of government, which determine the form and substance, as well as the quality and thrust, of Federal programs in research and technology. The man who is honored in the name of the award and the man who is about to receive it exemplify the best in the growing group of public servants who have assumed such responsibilities.

Edwin B. Wilson, born in Hartford, Connecticut, showed himself by ingenuity, stamina, and breadth of understanding to be, indeed, the proverbial Connecticut Yankee. He succeeded in filling his eighty-five years with what amounted in intellectual terms to no less than three lives. He was the last surviving student of the great American scientist, Josiah Gibbs; he was a faculty member at Yale, M.I.T., and Harvard; and he participated in an advisory capacity in many bold new ventures in science and technology in which the United States engaged—in war and in peace, in the physical as well as the biomedical sciences. He represented American scientific scholarship at its best. Last but not least, a long and fruitful association tied him to the National Academy of Sciences, which now honors him again by this award. This included the Academy's vice-presidency from 1949 to 1953 and an unprecedented span of nearly fifty years as Editor of its Proceedings. This is the man, then, who on reaching his seventieth year and his retirement from Harvard University, was to function for fifteen more years as a civil servant in the Office of Naval Research, contributing to the long-term clarity of ONR's underlying philosophy and to the quality and productivity of its programs. Attached to the Boston Branch Office of ONR, he would roam over the campuses of New England, using such

transportation as might be required, no matter how inconvenient the mode or the hour, undeterred by the vicissitudes of weather, and but rarely prevented by minor indisposition, in order to breathe confidence and inspiration into one generation after another of scientists who were learning what it felt like to do research under the sponsoring aegis of the Federal Government. Let us hope that his service to science and the government gave to him as much as he gave to it. It is indeed fitting that this award, which marks an important new turn in the recognition of the role of the career scientist in government, is named after him.

And now we turn to the man who will be the first to receive this honor. An examination of his career shows that he specializes in "firsts." Indeed, two of the major Federal organizations that carry forward the work of the Federal government in support of science came into being under his leadership. I am speaking, of course, of the first Chief Scientist and Deputy Chief of Naval Research and the first Director of the National Science Foundation. Among his many honors is the Robert Dexter Conrad award, which, I am told, is sometimes called the Navy's Nobel prize and of which he is also the first recipient, the award having been instituted on the occasion of the tenth anniversary of the Office of Naval Research and since shared by some of his most distinguished colleagues. His achievements and his character are known to you in such detail that any further eulogy here would be superfluous. I know that all of you agree with me that the National Academy of Sciences could have made no better choice for the E. B. Wilson Award than Alan T. Waterman.

I should like to close by saying that in the two decades of ONR's short history, government and science have entered into a cooperative relationship that can only become increasingly important for the future of both. It is important that the working machinery in which this relationship finds its expression be tough and resilient, and thus that Federal career scientists, who are charged with building and maintaining this machinery, have the competence and the support that are

required. The scientific community must recognize its responsibility for helping to bring such men into government, and the government must recognize its responsibility to train and to treasure them. If new life will have been breathed into this recognition on the part of both communities, then the bestowal this morning of the first Edwin B. Wilson Award on Dr. Alan Waterman will have served its worthiest purpose.

The Honorable Emilio Q. Daddario of Connecticut (left) presents the first Edwin Bidwell Wilson Award of the National Academy of Sciences to Dr. Alan T. Waterman in recognition of his "outstanding contributions in the service of the Federal Government to the effectiveness of its efforts to encourage and to benefit from the advancement of science."

The President and Council
of the
 National Academy of Sciences
 take the pleasure of presenting
 The Edwin Bidwell Wilson Award

for outstanding contributions in the service of the Federal Government
 to the effectiveness of its efforts, in the pursuit of its concerns to
 encourage and to benefit from the advancement of science

to

Alan T. Waterman

in recognition of achievements set forth in the following

CITATION

As creative scientist and administrator of exceptional talent, he has exerted far-reaching influence on the development of science, as well as on its conduct in the framework of national purpose and public policy.

He pursued a career in the public service of pioneering new patterns of Federal scientific activity, serving successively as a key member of the wartime Office of Scientific Research and Development, as the first civilian leader and intellectual inspiration of the Office of Naval Research, and as Director of the National Science Foundation from its birth to its maturity.

His broad understanding of science, his foresight, and his seasoned judgment have enabled him to guide the organizations under his leadership in the creation of a resilient partnership between science and public affairs which is now a vital element in the intellectual heritage of this country.

May 4, 1966

Washington, D.C.

Frederick Seitz
 President

Shirley Tewe
 Home Secretary

Text of Scroll

Response

by

DR. ALAN T. WATERMAN

It is a real pleasure to accept this award, which I appreciate very much, indeed; especially so on account of its name. I knew E. B. Wilson and admired him most heartily. His help in the early days of ONR was of inestimable value. I should also say that it made the award particularly meaningful to receive it from a member of Congress, because—as a former employee in the Executive Branch of the Government—it isn't always that one finds the Legislative Branch bearing such gifts.

Everyone in a situation like this will inevitably be aware of the fact that the real significance of such an award lies in its existence and in its function as a symbol which people consider important. The very occasion which we are celebrating today brings this symbolic intent to a brilliant focus in its commemoration of the combined unified effort on the part of scientists, the Federal agencies functioning in the service of national purpose, and the people of the country. I hope that it is a symbol of this endeavor that the present award will become important. I am sure that future recipients of the award will feel as I do that this should be the case. Thank you very much.

CONVOCATION ADDRESSES

Science and Public Policy

Introduction

by

THE HONORABLE GARRISON NORTON
Naval Research Advisory Committee

There is much that presumably needs to be said on an occasion such as this convocation. Let me, however, resist all temptation and confine myself to a single observation: On its twentieth birthday, we should not assume that the Office of Naval Research has come of age and that from now its place in the world is assured.

From Dr. Waterman and Secretary Foster, from Admiral Rivero and Congressman Daddario, you have heard well deserved words of praise for the Organization whose 20th anniversary we celebrate today; but in the interest of future Naval research, I would sound a note of caution at this point. Today, ONR operates in a radically changed and still changing frame of reference. Twenty years ago this office had the distinction of being almost the sole source of government support of basic research in our universities. Under these circumstances ONR earned the respect, I might say the affection, of the scientific community. The idea that a government agency in peacetime should support on a continuing basis, large-scale basic or pure research was a daring one. As an ancient Naval Reserve Officer, I am proud that the United States Navy was the first to accept this challenge.

Today, however, this idea has been enthusiastically accepted throughout Government. Dollarwise, ONR's contribution in this field is now only a drop in a bucket whose size continues to increase. If we take ONR's future for granted, if we assume that the scientists who today staff this office and its distinguished laboratories and field activities, will automatically be

succeeded by men of the same caliber, we are making, I believe, a dangerous assumption. Only by preserving within ONR an atmosphere attractive to the new generation of creative scientists for whose services the competition becomes keener every day, can the Navy bring to bear upon its problems the best minds of the future.

I am confident that the next 20 years of ONR will demonstrate that the Navy has met this challenge too.

Basic Science and Agency Mission

by

DR. HARVEY BROOKS

Harvard University

Today we are celebrating the 20th anniversary of a decision historic in the annals of U. S. science, the decision of the Navy to assume the leadership in the stimulation and support of basic and applied science in the United States. I don't know how much the original participants in this decision foresaw the significance of what they were doing, or what great consequence it would have for U. S. leadership in world science and technology. I rather think they did appreciate their role. One senses in talking to these early participants in ONR a little of the atmosphere of early Christians, spreading a new gospel of the partnership of science and government.

As with all evangelical movements, however, the very success of this one has brought its own problems. We find ourselves today in a period of stock-taking, a time of pause and a time of soul searching. Perhaps, for the first time since the war, the assumptions on which our science policy for the past twenty years have been based are being seriously questioned and even challenged. There have been several such pauses in the past, but in none of them have the fundamentals of our policy been as profoundly questioned as today.

In some ways this questioning is natural and healthy. After all, the original circumstances and environment in which ONR began have been deeply altered by the very successes and imitations which it generated. Today we have at least eight separate federal agencies which are deeply involved in the support of academic research, and many others involved at least peripherally. When it began, ONR was almost single-

handedly responsible for the health and strength of U. S. basic science. It could defend its research investments in terms of the broad Navy dependence on all aspects of modern science with the full knowledge that if it failed to act, the total U. S. scientific strength on which its military strength depended would languish and falter. Today, while many of the same arguments apply, they no longer apply so uniquely, and hence with so much force. It is harder to defend oneself against those who argue "let NSF do it" or "let ARPA do it". The penalty for failing to support basic science no longer seems so apparent or pressing as it did to the early missionaries, or—what is more important—to their budgetary masters. Perhaps the time has arrived for a reassertion of the role of mission-oriented agencies in the support of basic research, and for a more sophisticated statement of this role in a political environment which is far more sophisticated about science and technology than was the world of the late 1940's.

It is, of course, easy to say that the mission-oriented agencies should support the basic research that is relevant to their missions, even the training of skilled people who might later serve their mission. This is a statement with which few would now disagree, but it really begs the question. The whole argument is about what basic research really *is* relevant to a mission, and what time horizon one should be talking about. To some extent we seem to be coming into an era where basic research is everybody's business, and therefore nobody's business, except possibly NSF's. Where does the line between "pure" basic research and mission-oriented basic research really lie? Does the proliferation of other research supporting agencies, and especially the growth of an agency with an explicit mission to support science in terms of its own internal system of values (namely NSF), imply that mission relevance should be interpreted in ever more narrow and specific terms? I do not believe so, whether the matter be judged from the standpoint of the health of science itself, or from the standpoint of the vitality and success of agency missions which depend upon science. I do not believe that science can be divided up

into neat little packages, each of which can be related uniquely and bodily, as it were, to the mission of one agency. Two scientific investigations which begin by asking the same questions and using the same methods may end up at an entirely different point merely because of the environment and the communication network within which they are conducted. One should look at mission relevance not in terms of mutually exclusive compartments, but rather in terms of distribution functions with different centers of gravity but substantial overlap in the tails. It is precisely this overlap which provides the internal communication within science which is responsible for the rapid application of science.

The conventional wisdom deals often with the debt of technology to science, but speaks less frequently of the debt of science to technology. Some of the most challenging and fundamental problems of solid state physics or molecular physics have arisen from studies which were originally suggested by technological needs. Scientific work involves a multiplicity of choices of direction, many of which depend on very small influences in the mind of the investigator. Even in a system of complete scientific freedom the cumulative effect of the small biases placed in the mind of the investigator by his sponsor can have a profound effect on the direction and impact of his research. The mere need to defend what he is doing to a particular sponsor may be the factor which will trigger an important application. It seems to me no accident that both versions of the maser and the laser were conceived in university laboratories devoted to the broad advancement of electronic communications, and sponsored by the military services.

But the invention of these devices, and the race to develop and improve them, also opened up a host of new fundamental questions regarding electric and magnetic interaction in crystals, some of which led far afield from the original device applications. In fact it is striking that each solid state device invention has opened up a new branch of pure solid state research, whose existence was scarcely suspected until the appearance of the device formed the entering wedge into the

field and also provided the motivation for its generous support and exploitation.

Our system of science support in the U. S. is what I like to term a mixed decision system, and I think one of the major sources of our scientific and technological strength. By a mixed system I mean one in which scientific choices are governed by a wide diversity of priorities, institutional environments, and motivations. Not all pure research is too pure, and not all applied research is too applied. In the apt terminology of Alvin Weinberg, our choices are seldom dominated exclusively by either scientific criteria or social criteria, and our variety of research institutions and sponsoring agencies emphasize these two types of criteria in varying mixes. We recognize the fact that the sponsor of research and its performer may quite legitimately have different sets of motivations and priorities, and that the very tension between the two may be highly creative.

One of the great paradoxes of this age of specialization is that it is harder and harder to delimit the boundaries between scientific fields or the relation between the scientific fields and federal missions. The "modern" technological agencies—Defense, Space, Atomic Energy—draw scientific sustenance from almost all areas of basic science. Within our present diversity of support systems, how is an agency to decide just what is relevant to its mission? How is it to decide when it can safely depend on the research sponsored by other agencies? When should it depend on in-house capability, and when should it look to the academic community for intellectual support? When is an activity which it sponsors to be considered above critical size? It would be wrong to expect that these questions could be answered *a priori*, that is, in the absence of past history. What an agency sponsors will depend not only on its own mix of skills and its appraisal of the skills of other agencies, but also on previous history. One cannot set down a system of rules.

Why should a mission-oriented agency sponsor or conduct basic research? It needs a fund of knowledge adequate to the fulfillment of its mission at a satisfactory rate of progress, and to

provide it with as broad a range of future technological choices as possible. This fund of knowledge must be effectively available to the technical and managerial arms of the agency, and may have to be adapted to its unique technological requirements. The needed research comprises a chain—or, more precisely a network—extending from work of obvious immediate relevance to the mission back through research in fields of less and less obvious relevance. This chain must be followed reasonably far back towards the most fundamental or abstract fields in order to evaluate how much further the state of the art could be pressed quickly if needed, to appraise the reliability of technical judgments and evaluation of systems based on current understanding, to predict what technical choices may be open to us in the future and what accomplishments might be possible for other nations—in short, to avoid technological surprise. These requirements involve a subtle blending of scientific knowledge and sophistication with knowledge of agency needs and technological thinking. One cannot depend upon the program officers of other agencies to be aware of all the needs of the Navy!

The fund of basic knowledge required by an agency may be divided into three general classes.

(a) Fields of science in which the mission orientation admits of no clear limits to agency interest, and requirements differ in both kind and volume from those of any other component of the nation's technological community. In the Navy underwater acoustics, physical oceanography, and deep sea technology are clear examples. In such cases the rate of progress which would result from free play of academic interest in the science for its own sake would be unlikely to satisfy the objectives of the agency.

(b) Fields of science which are of vital importance to the agency mission, but whose importance is shared almost equally with other agencies of the government. In the case of the Navy examples which may be cited include electronics, materials, meteorology, and human factors engineering. On the other

hand, there may be specific aspects of these broader fields which are of unique relevance to one agency, or have a special flavor which is characteristic of the agency's needs, e.g., high strength metals and composites for deep submergence in the Navy.

(c) Fields which at present show no obvious promise as sources of concepts or research results for near term agency exploitation but which, in the mainstream of imaginatively advancing science, can produce results of potentially significant repercussions. These areas—such as pure mathematics or elementary particle physics—may have significance either through discoveries which arise directly in the science or, more commonly, the chain of evolution of scientific ideas which always links the fields of greatest scientific interest to those of extensive value.

In considering the relevance of a field to agency missions an aspect often forgotten is the significance of new *tools* of basic research for future technology. Thus, for example, one might have questioned the relevance of the results of nuclear physics to the Navy's mission; yet it is clear that this field, by challenging the most advanced electrical technology, has been the instrument of many technological advances of great importance to the Navy and to Defense; high powered klystrons, high speed circuitry, developments in computer software and pattern recognition, high vacuum technology, and many other fields, of which I will give more detailed examples later.

The approaches to these different types of basic research may be different, but some participation in all three is needed if the agency is to realize the maximum and most timely advantage from ongoing science.

The first category I mentioned—the fields uniquely relevant to the mission of a particular agency—often include areas which do not receive much attention through the internal dynamics of science. In a field such as this, of which underwater acoustics is a good example in the Navy—or indeed, classical hydrodynamics—the agency's support should be limited only

by available manpower and promising scientific opportunities. Almost any advance on a broad front will be virtually certain to benefit the agency. In such unique fields, also, a fair fraction of the fundamental knowledge needed will have to be obtained through in-house or captive laboratory effort. The efficient execution of any applied mission will almost certainly require organized and directed effort in areas where understanding is so limited that basic research is necessary to advance the state of the art. Even here, however, the agency cannot afford to be too parochial, and must be prepared to support some basic research whose immediate relevance may not be apparent. A classic example is ONR's extramural support for fundamental research on long range acoustic propagation in the 1940's at the Lamont Geological Observatory at Columbia. At the time there was no specific end item in mind, yet when missiles began to look like potential weapons systems, the need arose for test ranges where missiles could impact at sea and be accurately located. The results of the ONR sponsored research immediately became the basis of the missile impact location system now used at all the test ranges. The discovery of the deep sound channel, now important for long range underwater detection, was also a direct outgrowth of ONR sponsored research in one of the oceanography laboratories.

An important function of an agency is to stimulate interest within the broad scientific community on problems important to its future. One way in which this has been done is by finding ways to translate applied problems of an agency into generic scientific problems which can attract the continuing interest and attention of first rate scientists. To achieve this in any lasting way it is not sufficient to call attention to the importance of a field; it is essential also to demonstrate its intellectual challenge and the opportunity for genuine scientific progress. ONR in its past has shown a particular talent for this kind of imaginative stimulation. Examples are its fostering of basic work in certain key university centers on coastal geography and also on the general theory of port logis-

tics. This work has paid off handsomely not only for the Navy but the DOD generally in the present situation in the Far East. In such areas it is necessary to recognize the significance of a field *long before* its practical importance. Without this kind of imagination applied research often becomes little more than research on how to solve the problems which will have disappeared or changed unrecognizably by the time solutions are found. The secret of successful research planning is to have the science ready when the time is right.

In fields of research whose importance to a mission is shared with other agencies, the problem is often not so much one of stimulation as of maintaining close contact with a broad area of science and technology and being in a position to influence its evolution and identify the technological opportunities emerging from it at an early stage. Here a combination of in-house and extramural effort is desirable, with organizational devices to maintain contact with as broad a base of extramural effort as possible. The Joint Services Electronics Program, supported jointly by the three military services, is a good example of the kind of device which is valuable. From the university-based JSE Laboratories have flowed a steady stream of contributions, direct and indirect, to the development of naval equipment and weapons systems.

It was from work supported in these laboratories that the ideas for the maser and the laser emerged, for example. Increasing sophistication of signal detection methods, in guidance and control technology, in antenna design, in radio navigation systems, in high power microwave tubes, and in a host of other areas have emerged from this long-supported activity.

The third area of support I mentioned earlier is that of fields whose relevance to an agency's mission is not at all obvious or readily demonstrable in a before-the-fact sense. This is, perhaps, the area in which the general philosophy of mission-oriented support of research is most under attack today, and where the attitude of "let George do it" is most evident both inside and outside the agency concerned. I am not here

suggesting that an agency can or should ignore what is happening in other parts of the government, but neither can it afford in the long run to lose contact with the field. Unlike the first two areas I have mentioned, an agency in this area has little concern with the total volume or rate of research. It is not here concerned with stimulating fields which are in danger of falling into scientific backwaters or with supporting a volume of activity consistent with the advancing requirements of its own technology. Rather its involvement might be described as a "listening post" commitment, a kind of scientific early warning device. This listening post activity can often be best achieved through extramural support. The relevance of the work is usually not sufficiently certain to justify the permanence of commitment which is entailed in intramural support. An agency must participate actively in the continuing assessment of the significant prospects and implications of basic research, and must assure its own capability to make prompt and vigorous response to innovations made possible by the combination of discoveries in widely different fields of science. The lead time for the evolution of such discoveries into the possibility of a system development is so long, that a high price can and should be paid for the earliest possible forewarning and appreciation of the significance of such discoveries. But since payoff will be infrequent, the agency must select a low level of participation in a broad range of scientific disciplines, laying its emphasis on gaining entry to the highest quality work and the most productive groups in each field of this type.

The listening post type of activity requires a much more creative role from scientific program officers than the mere administration of a broad scientific program. They must be in close scientific communication both with their contractors and with the applied needs of their agency. Above all they must have the time to think, to put their feet up on the table and to speculate frequently on where their science and technology are going. And they must have the time to travel into the field, not only to see what is going on in the laboratories which they

sponsor, but also to stimulate the thinking of their contractors and carry ideas from one to the other.

Perhaps nowhere has the value of the listening post type of commitment been better exemplified than in ONR's long standing participation in the support of nuclear research. The benefits to the Navy from nuclear research have come not so much from the research results themselves as from the derivative technology, which has resulted from the fact that accelerator design and particle detection instrumentation have continually pressed the state of the art in areas of technology which have proved important to the Navy and to the military services generally.

For example, the first high power klystrons were designed specifically to power the Mk III linear accelerator at Stanford, increasing by a factor of 1,000 the power available from such microwave generators when the development was first started in 1947. In the meantime the linac itself has become a commercial device of considerable importance both for medicine and for industrial radiography, including incidentally the radiography of very thick objects such as missile propellants *in situ*. The electron linac development also provided an important impetus to the more general development of microwave technology. A variety of microwave components, such as attenuators, phase shifters, and high power windows, and new microwave measurement and calibration techniques, were developed in connection with these machines. All of this microwave technology has been of great significance in connection with modern high power radar. Of course, this technology didn't emerge from Navy supported work alone, but the fact that the Navy was one of the earliest in the field undoubtedly contributed to the rapid importation of these techniques into defense technology.

Nuclear physics early generated exacting requirements in the area of fast pulse electronics, and the demand for extremely short resolving times in coincidence counting of particles has stimulated many circuit developments which have interacted

fruitfully with parallel research in computer technology. The most widely used commercial oscilloscopes were developed originally to fill the needs of nuclear physicists.

A decade ago the increased use of scintillation counting led physicists to press for the development of very sensitive photomultiplier tubes. They advised the government on developmental contracts with industry to develop tubes specifically tailored to the needs of nuclear physicists. Similar tubes now find application as the most sensitive detectors of faint light in many areas of military and civilian technology.

It is doubtful whether any of these new technologies would have been developed as rapidly or as economically by any other means, because at the time the developments were started, the need for them in other areas than nuclear physics could not have been foreseen sufficiently clearly to provide the necessary focus and incentive for the development effort. This is only one of many examples of how the tools of basic research anticipate the needs of other more every-day technologies, and thus serve as a stimulus to bring forth the art and science that become available for other uses when the time is right. In my opinion the military services ought to use this technique much more frequently than they do, to cultivate the technologies that are likely to be needed ten to fifteen years from now.

Today nuclear physics and elementary particle physics are challenging technology in new directions. One example is the marriage of microwave technology with cryogenics to produce higher efficiency microwave power components, an area in which an ONR sponsored contractor is currently the leader. Another example is the development of sophisticated data processing techniques for "on-line" experimentation and "pattern recognition" of bubble chamber and other particle detection patterns. Some of this work has been and still is ONR supported, giving the Navy a "window" on new technologies likely to be of profound significance for future military applications, but in ways not now clearly foreseeable. As the Navy's fractional commitment to the national nuclear and high energy

physics program has decreased over the years to less than 7%, it must be more and more selective in its support of this area, but it seems doubtful whether its participation could drop much lower than at present and still afford it a meaningful window on the field.

The current conventional wisdom has it that nuclear and elementary particle physics are "useless" subjects, worthy of support, if at all, only for their "cultural" value. This is why I deliberately chose the Navy's nuclear physics program as an illustration of the value of a listening post commitment to a few of the most vital frontiers of advancing science. This is not an argument for indiscriminate support of *any* basic science on the part of mission oriented agencies. What I have tried to emphasize is that the ecology of the scientific effort is far more complex than the naive connections one can make between pure science and applications before the fact. These connections are often not direct but proceed through many layers of neighboring sciences and instrumental and industrial technology. While I am all for projects like "Hindsight" which attempt to trace the origins of modern weapons systems, I would warn that such efforts are very likely to lose the trail just at the most interesting point when it disappears into the general scientific background and sophistication of the times.

As one reviews the history of American science and technology in the last twenty years, one cannot fail but be struck by the strategic role which ONR-sponsored work has played. In fact, when one considers its present tiny *fiscal* role in research support compared with what it was in the early days, one is surprised at its still major importance and influence. Wherever the most important advances are being made, one still seems to find ONR present with at least token support. A mere catalogue of areas in which ONR-sponsored scientists have pioneered shows how frequently ONR has been there with the right science at the right time even though few foresaw the usefulness and relevance when ONR first began to sponsor it. Let me merely list a few examples:

1. The discovery of the Van Allen belt, and the development of a research satellite which was available to take data in connection with the Starfish nuclear test when such data were needed quite unexpectedly and urgently.

2. The metallurgy of high temperature molybdenum alloys, which proved to be vital in connection with the Polaris program.

3. The development of the thermochemistry of titanium and its compounds, which proved to be a "bible" of valuable information when titanium became of practical importance.

4. The early launching of an arctic research program, data from which suddenly proved vital when it became necessary to install the DEW line.

5. Early support of work in Bayesian statistical analysis which proved to be of great value as more sophisticated methods of detection of signals in noise became increasingly important in radar and sonar.

6. The development of the mathematical theory of diffraction and scattering of electromagnetic waves from large obstacles, which became later very important in application to the problem of minimum radar return from missiles and decoys.

7. Support of the earliest work in the field of time-shared computer systems.

8. Support of the early fundamental work on the propagation and phase stability of very low frequency electromagnetic waves, which led directly to feasibility of VLF radio-navigation systems—incidentally, a fine example of cooperation between extramural, Navy, and other government laboratories.

9. Support of fundamental work on the theory of wind generated waves, which led eventually to operationally useful techniques for forecasting ocean waves.

10. The discovery of microplankton in the oceans, and the realization of the importance of small organisms in affecting acoustic properties.

11. The invention and development of a method for the rapid freezing of blood.

12. The support of fundamental work in oceanic geophysics, which led directly to the development of a useful geophysical navigation technique.

13. Support of the earliest work on numerical modeling of the atmosphere, which is now beginning to lead towards a practical method of numerical weather forecasting.

14. The discovery of the so-called deep sound channel as an outgrowth of fundamental investigations in an oceanographic laboratory.

15. The development of the concept of an integrated fleet air defense system.

16. The support of early fundamental work on shock tubes and shock dynamics, which was the direct forerunner of the use of shock tubes in the study of re-entry problems and the development of a practical nose cone material—a primary example of a basic research tool which, through remarkable prescience, was ready to be applied in testing and development programs when needed, even though nobody had conceived the ICBM when the work was first supported.

17. The development of the plastic cornea for eye repair, an example of assiduous and inspired follow-up on an initially fortuitous observation.

One could go on with this list indefinitely, but I think I have recited enough examples to make my point. On the other hand, we shouldn't fall into the trap of believing that the basic idea is everything. I suspect many of these ideas would have been lost in the noise if there hadn't been alert and intelligent program officers and Navy scientists who had the wisdom to appreciate their potential and see that it was further developed. The accomplishments of ONR sponsored research must be a source of pride not just to the scientists who did the pioneer work, but to the creative administrators and Naval officers who cultivated their scientific gardens so fruitfully. I think too many of us in the scientific community have been recently too inclined to forget the importance of this role, and on the other side, too many in the policy making positions of government,

while giving lip service to basic science, have been too inclined to forget that scientific results can't always be whistled up to order when needed. They have to have been brewed years before, and in military development there is nothing more costly than the basic scientific results that weren't available when the time was ripe.

Promises and Constraints on Science

by

DR. FREDERICK SEITZ

National Academy of Sciences

In discussing the topic of "Promises and Constraints on Science" today, I am placed somewhat in the position of a pathologist examining a reasonably healthy animal, if indeed science and science-based technology in our country can be referred to in biological terms. The position is by no means an entirely ridiculous one since it is basically the task normally assigned to the professional critic who earns his living by showing how deftly he can wield his scalpel to dissect both the healthy and the unwell subject. I must admit that I personally prefer to discuss plans and actions; however, it is not unpleasant to assume the role of critic occasionally.

One of my colleagues in the Academy, Professor Roger T. Williams, a biochemist at the University of Texas and an expert on nutrition, is at present writing a book, which I have had the privilege of reading in draft form, that deals with a topic he came to comprehend in somewhat the manner of a revelation in the course of his lifetime. In brief, he has noted that each healthy human being, while completely consistent with himself, both physically and psychologically, inevitably differs from others. Each reacts differently to given medicines and other influences, internal and external, without being untrue to the species as a whole. The fact that Jack Spratt could eat no fat and his wife could eat no lean does not imply that either was abnormal. This type of built-in diversity of our population as a whole implies that our species gains collective strength which it could not if all individuals had exactly the same aptitudes and reactions.

The Past Value of Our Distinctive Pattern   Since I shall inevitably be comparing the United States with other nations, and noting some differences, I must admit at the

start that these differences are not without their merit. In the last analysis, we did as a nation take some remarkable initiatives in the development of both science and technology. For example, we developed the first practical steamboat, were the first to experiment with practical use of anesthetics in modern times. We gave great attention to the mechanization of farming, developing both the cotton gin and the McCormick reaper. We constructed the first mountaintop observatory and developed the electric light bulb to a useful state; similarly, we had the first successful self-powered airplane. In response to the needs of mass education, we developed the university department, which I believe could become a far more powerful institution for higher education and related research than the German institute which was a great innovation of the last century. We took the initiative in developing the cyclotron and in pushing research in polymeric chemistry for purposes of mass production. During the war we were willing to risk a significant part of our resources in a successful attempt to develop a nuclear chain reaction.

We all recognize that the peoples of the world are now moving toward the establishment of a common world culture, in which science and science-based technology will form a very major part of the foundation. Occasionally I become frightened at the thought that our species may be overwhelmed by homogeneity. On further reflection, however, the sense of panic leaves me when I realize the enjoyment all of us have in being contrary when there seems to be advantage to it. I doubt if we will ever lose this trait.

Strong Points   Let me begin by emphasizing the strong points of our national posture in relation to science and science-based technology, which as I mentioned above is intrinsically healthy. First, we have a large and effective machine for generating science, involving many fine

institutions, and have access to the wealth and productivity of an enormously affluent society. It is very important in thinking of the future to recognize that there is enough freedom in our national system because of the diversity of the participating institutions and our great wealth to steer the scientific machine more or less as we please as new challenges and opportunities appear, provided we maintain a strong posture in education for science and engineering, emphasizing both quantity and quality. The freedom available is sufficiently great that by bringing proper judgment and eloquence to bear, the scientists and engineers can sell new ideas to society without sacrificing other gains.

Our educational system does have its difficulties arising from the inevitable shortages in manpower and money; on the other hand, it is by no means retrogressing. It has coped successfully with the problems of mass education in the past, and one can honestly expect it to do even better in the future if society continues to provide it the support it needs.

The scientists and engineers of our country do have access at present to about 3 per cent of our Gross National Product for research and development and related testing; moreover, that percentage is growing somewhat faster than the Gross National Product. We cannot claim that the system is being starved even though we must admit that certain areas of research, particularly those related to the upper levels of science and engineering education, could profitably absorb funds at a somewhat greater rate and provide society with benefits that more than compensate for the added costs.

It is true that our expenditures for research and development are by no means uniform in all areas. For example, much of the money is devoted to defense, the space program, and the field of atomic energy. Industrial chemistry and electronics are also fairly well treated, either independently or in connection with the other fields. There is evidence that the support of agricultural research seems to be faltering at a time when the world food supply appears to be approaching a critical state.

We can state, however, that we do have a substantial surplus of food for our own needs at this moment, even though it may be dwindling because of exports, and we do have a fine tradition of transferring basic agricultural research into practical results. It is evident that building research is not carried out in our country in anything like the systematic way common in some other parts of the world. On the other hand, it must be recognized that this field is complicated by a diversity of industrial suppliers and by the fact that labor unions tend to resist labor-saving devices. Our intrinsic ability in the field of construction is shown by the fact that we have been highly effective in evolving techniques for planning and carrying through highway construction.

We were late in entering the space race, probably because of political rather than scientific or technical shortsightedness, but we did have successful Ranger shots to the moon, Mariner voyages to both Venus and Mars, and are well on the road toward developing successful rendezvous techniques which are highly important for the Apollo mission. We cannot claim the first earth orbiting satellite as we would have if we had taken the field of scientific satellites with appropriate seriousness following the Korean War, but we did discover the Van Allen Radiation Belt because our scientific interest was basically broad and sound.

I believe it is safe to say that the greatest constraints on the development of science and science-based technology in our country originate in tradition. The straitening effects of tradition are by no means new in human society. Regardless of the seriousness of our constraints, however, I am certain I would not trade them with the constraints found in many other lands.

To consider a few examples, India, which at present has a population that is expanding by at least two-and-one-half per cent a year, has many difficulties arising from nationally revered traditions to overcome before it will be able to feel certain

that it is on the road toward developing modern industrialization under conditions that guarantee a continually rising standard of living.

The European peoples, who invented modern science and were the first to benefit from its fruits, find it very difficult to overcome roadblocks associated with the outmoded form of selfseeking nationalism which they developed over six or more centuries, even though it should now be evident to all reasonable people that far more can be gained through close cooperation than through the type of competition which characterized the pre-1914 European world.

While China has discarded the form of provincialism which it assumed following the Golden Era of the Han Dynasty nearly two thousand years ago, and seems prepared to adopt a more international outlook, one wonders if this renaissance of internationalism will not be accompanied by orthodox attitudes at least as unprogressive as those which characterized the wall-bound China of earlier centuries.

In recent years the leaders of the Soviet Union have appeared willing to abandon the oppressive posture which characterized the darkest days of the Stalin era and to join the world community more fully as a partner. One cannot quite forget, however, the suddenness with which the Hitler-Stalin Pact was consummated at a very critical hour in world affairs and wonder whether the rift between Moscow and Peking is really as wide as the current vituperation suggests. The Cuban missile crisis did little to put our minds at rest.

Historical Background of Science In order to understand the constraints which our own characteristically American outlook imposes on us more fully, we should compare our own attitudes toward science with the traditional attitudes found in Europe, remembering not only that modern science was invented in Europe but that it was fostered there through a period in which it clearly would have died out if it had been given to us alone to foster.

Historians are, of course, professionally obligated to point out that the roots of modern science lie very far in the past, extending back not only to the civilizations of the Greeks and Romans but earlier to Mesopotamia. Nevertheless, the Europeans, on becoming aware of the older developments at the time of the Crusades 800-odd years ago, were not only able to absorb them but to recast them into something distinctly new and useful. The question of why this should have happened is an eternal conundrum which will be of enduring interest to the philosopher and the historian. Some have said that the essential feature which distinguished European society from earlier ones can be associated with the fact that it did not cultivate slavery. There is no doubt that the absence of slavery was vitally important in conditioning the nature of European society, yet we may note that neither the Eskimos nor the North American Indians had slavery to a significant degree and yet they did not thereby automatically invent the scientific method. The understanding of the riddle must, of course, lie in the full complex of social institutions and attitudes of the Europeans including the issue of slavery. I am inclined to believe that the two factors which made science flourish in European culture were the enormous respect paid to practical innovation at all levels of society and the deep interest in culture and philosophy. Both attributes appear very early in the evolution of European society and endured essentially without interruption.

When I was young, it was common to take the view that the Europeans lived in relative darkness until some time between the early Crusades and, say, 1400, when the Renaissance began to blossom. The period before the Crusades was treated as if relatively unimportant for what happened later. Actually, I believe that the foundations for the attitudes which made the scientific revolution possible were in fact laid well before the year 1000. European culture was in the ballistic range for all practical purposes by the time of the Crusades. It could still be sensitive to cultural importations and could have been

terminated by a massive invasion, as might indeed well have occurred in the thirteenth century when the Mongols began scorching the earth. However, the two characteristics highly important for science, namely a deep interest in both practical and philosophical affairs, made evident on the one hand by the remarkable successes in temperate climate agriculture which permitted the establishment of urban communities and in the other by the establishment of the universities, were highly developed well before the Crusades. By the time the great books of Alexandrian science were turned over to European society in the twelfth and thirteenth centuries, the ground was not only fertile for absorbing them but for transmuting them into tools which would eventually be able to add fuel to the Industrial Revolution.

Our National Attitude   If we examine our national history relative to that of Europe, we have to admit that we have had relative blindness on one side. We completely shared the European's respect for practical innovation. We were not only excellent at developing new useful devices but in turning the techniques of the Industrial Revolution over to the mass production of such devices in ways which the Europeans are only now beginning to emulate. On the other hand, our society showed relatively little general sympathy for cultural or philosophical matters, at least until World War II and its aftermath. Such areas of human endeavor were regarded as somewhat peripheral to our national mission until quite recently. Had European science been arrested in the eighteenth century after the time of Newton for some reason, the torch would not have been kept burning brightly on this side of the Atlantic. It probably would have done no more than glimmer in occasional recesses. It is true that agricultural research has been supported in the United States at the Federal level for about a century; however, the fact that we were so predominantly agrarian until this century shows that the support of agricultural biology was given with quite obviously practical goals in view.

Perhaps it is not redundant to remind you once again of what we owe to the Europeans for their dedication to science. In this connection one should mention the revelations associated with the development of the modern picture of the solar system and of universal gravitation; the development of Newton's equations which form the foundation for science-based mechanical engineering; the discovery of the chemical elements and the fields of industrial chemistry which emerged from this discovery; the revelation of electromagnetic phenomena and the worldwide communications net they have made possible; the discovery of the electron and ionized atoms and the countless devices in the field of electronics which have emerged from this knowledge; the discovery of the nucleus and the development of an entirely new source of power; the explorations of the universe which have so broadened our concepts of the vastness of the cosmos in which we live; the discovery of the means whereby inherited characteristics are transmitted and the revelation of the world of the gene and the genetic code. It is true that American scientists played a role in this development; however, we must not forget that until the 1930's most of our outstanding scientists spent a critical period of higher education in Europe. I happen to belong to the first generation which, with a few exceptions, received all of its training on this side of the Atlantic.

Unique Navy Role It will reflect to the credit of the Navy for a long period to come that it established the Office of Naval Research at a very critical moment of transition in the phase of our national history related to science. For it was the Navy which first responded to the challenge described in Vannevar Bush's report, "Science, the Endless Frontier." Other agencies eventually followed suit and several have already eclipsed the Navy in the magnitude of the support they provide for basic science; however, all have been deeply influenced by the wisdom and foresight which the Navy showed in 1945 and 1946 when it decided that applied science could not thrive in the long run unless basic science was well nourished.

In effect, the Office of Naval Research took the initiative in breaking with our older peacetime tradition of borrowing basic knowledge from abroad and supporting only those aspects of science which can obviously be tied to specific applied missions. The Navy recognized explicitly that henceforth the United States had a prominent role to play among the leaders in world science. Western Europe could be expected to regain its original impetus, and in fact would be joined by other nations such as the Soviet Union and Japan as well. The time had come, however, for our country to recognize that it too had an obligation to contribute a proper share to the advance of world science.

New Pattern   It seems quite evident to me today that we
Not Secure   cannot assume that the battle is won—that
                         there is universal understanding of the impor-
                         tance of basic science—universal understanding
of the need to support basic science liberally for our own national welfare. It is clear that most mission-oriented agencies are still prepared to shift funds from the support of basic research to mission-oriented research whenever their budgets tend to become stationary, even though the fraction of funds devoted to basic research may be quite small. We can also note that Congress seems at times far from unanimous in the view that the National Science Foundation should be kept free of programs tied to applied missions.

I think it safe to say that our national position in basic science will remain insecure for at least a generation ahead. As long as that is the case, our national welfare will inevitably remain in jeopardy to some extent. Fortunately, as I noted above in one of the more optimistic passages, our system contains an enormous amount of flexibility. The shortcomings associated with the budget of one year can be corrected in the budget for the next. Our wealth in money, facilities and professional talent is great enough to guarantee such flexibility.

Let me turn now to what might be called secondary constraints within our system. In using the term "secondary" I do not want to imply that such constraints are unimportant. To many working scientists they will seem far more relevant or immediate than the issues I have already discussed.

First, we know that there is an increasing spirit of competition between "big science" and "independent science." For example, many solid state physicists and chemists feel that the money being given to the space program and to high energy physics is depriving them of rudimentary support for exceedingly important work connected with the education of students at the doctorate level and the orderly advance of fields of science that have been enormously fruitful in the past and will continue to be in the future. The factors involved are, of course, rather complex. One cannot hope to explore Mars in detail or to gain further understanding of the fundamental particles of which matter is composed without having exceedingly expensive devices. In contrast, one can always get along one more year with older equipment if one is working in chemistry or solid state physics, even though the unit cost of new equipment is relatively low. Moreover, one must recognize that a Congressman may well receive much more credit if a single large installation is established in his district than he will if the equivalent amount of money is distributed over many laboratories. I believe the answer to this problem lies in patience and eloquence. The independent investigator is vital to science—he must take the time to tell his story clearly and eloquently with the expectation that those who govern our country will eventually listen.

Another issue that introduces constraints into our system is the matter of geographical distribution, now of major concern in Congress.

Quite apart from whether or not one believes that geographical imbalance is good or bad—and much has been said on both sides of this issue—the fact remains that

representation in Congress is geographical, and unless funds are distributed with some relation to geography, everyone will suffer to some degree in the future, including the so-called rich. The significant numbers of Representatives in "have-not" areas will lose interest in the support of science by the Federal Government. This principle is so nearly self-evident that I would have felt that it would have been recognized and adopted by practically all Federal agencies long ago. Even at this late date I can only urge each Federal agency to reexamine the geographical distribution of its funds for basic science with the thought that this will probably mean more for all in the future. The argument that money must go where quality is has, of course, much wisdom associated with it, but it must be realized as well that human nature is such that good men will also go where money is—particularly under circumstances like those we now face in which funds are not rising as rapidly as the scientific community could absorb them. There are those who will say that this point of view is too political or too pragmatic. To them I can only say that if Federal funds can be effective in building up science and engineering in one part of the country, they can also be effective in building up science in other parts if reasonable institutions are available.

Above all, I think it is exceedingly important that we take steps at this time which will ease regional bitterness. It is true that if one could concentrate all individuals having the genes, both dominant and recessive, important for scientific aptitude in one or two portions of the country, there might conceivably be benefits to the nation as a whole. At the present time, however, we know far too little about human heredity to attempt such a process of concentration even if it could be put into effect. It seems about as likely that a potentially great scientist will be born in one part of our country as in another in the foreseeable future. The more nearly homogeneous we are in providing the opportunities for higher education in science on a reasonable basis throughout our country, the better will our population be involved in science.

Should a greater balance in geographical distribution ultimately be achieved mainly through Congressional activity, and without the explicit advice and support of the best scientists throughout our country, it is very possible that the resulting distribution of money would not be optimum, even within the framework of more uniform distribution. There is always the danger that significant amounts of money will go to institutions of little capacity and promise, rather than to nearby institutions of greater capacity and promise, if the best scientists and engineers speak only for their own narrow institutional needs and lose the respect of those who control Federal budgets.

Let me turn next to opportunities in science.

Opportunities in Science When I was a young lad, showing a boy's interest in science, parents and relatives gave me the familiar array of books and scientific kits on birthdays and Christmas in order to cater to this interest. The assortment of books inevitably included copies of Jules Verne's works, which I must confess I read very soberly and laboriously somewhat like a monkish scholar working his way through the books of the Old or New Testament. As everyone knows, Jules Verne, who was just approaching the peak of his reputation a century ago, had a marvelous sense for future technology viewed in the large. He saw innovations several generations ahead of his time, describing in the language of his day such things as the journeys of the Nautilus and a trip to the moon. As a boy I was inclined to regard him as one of the greatest scientists in human history.

Some thirty-five years later, I returned by chance to Jules Verne as part of reading matter gathered up at a bookstand for a transatlantic plane trip and had a great revelation. Even though his romance and vision seemed in a sense even more remarkable than it had when I was a boy—maturity had brought me closer to him as an individual—I realized that he would have had a bit of difficulty passing an examination in undergraduate physics. The feats of the Nautilus, which plowed along at forty or fifty miles an hour, required a far better

power plant than anything one could expect from the sodium electrolytic cells Verne provided for it. His moon voyagers travelled in a vehicle which was shot out of a cannon, mounted at sea level and located, of all places, in Florida. This decision on location was made after a long and colorful competition for site. They experienced some ten thousand g of acceleration on take-off, as they gripped the handles on their armchairs. The crates of live chickens, eggs, and vintage wines in the hold came through the ordeal unscathed. I might say that the gun out of which they were fired was loaded with gun cotton. Verne disposed of the nose-cone problem by stating that the space vehicle went through the atmosphere too fast to heat up. The voyagers experienced normal gravity within the cabin until they reached the equal gravity point between the earth and the moon, whereupon gravity reversed its direction in the course of a few dramatic minutes in which the passengers floated about.

It dawned on me that Verne's great strength rested on the fact that he had an amateur enthusiasm for science and science-based technology. He was not sufficiently professional in his own approach to science to be embarrassed about cutting major corners. He had the soul of a romantic novelist and was willing to let the details of attaining the future take care of themselves, while he painted with a broad brush. It would have been much more difficult for him to do this had he had the reputation of a professional scientist or engineer to uphold.

A situation opposite to that of Verne is demonstrated by the case of my good friend, Clifford Furnas, who wrote a book in the 1930's trying to look ahead for a century, keeping his feet firmly on the pathway of good science and technology. Although he was sure at the time that he had stretched predictions well beyond the breaking point, he found twenty-five years later that all of the developments that he had predicted had already taken place.

My own sympathies in attempting to discuss the future promises of science are all with Furnas. It is difficult enough

to attempt to discuss the hopes for the next decade, let alone the long-range future. Let me try, however to make a few bold strokes on the canvas.

Short of some vastly devastating disruption of *Science Secure* our society, there is no chance that science and science-based technology will be abandoned in the foreseeable future. Modern civilization would retrogress so swiftly and disastrously that the effects would be obvious to all. Just as mankind has been committed to tool making both to gain material advantage and to achieve esthetic goals, science and its application are a permanent part of our heritage.

It is also evident that we are now well along in the course of a transition to a state in which society will have all of the energy it needs for whatever ambitions it may have. At this moment we are moving well into the age of uranium, born during World War II. Fusion power will emerge when genuinely needed somewhere in the period ahead of us, in one form or another. At present it is merely being toyed with. It is true that raw materials will present a problem to us indefinitely. However, there is no reason to believe that we will not achieve what we want as long as nature provides such vast opportunities for improvisation.

In brief the planet now belongs to our species and can be used by us as our judgment decides. The spectrum of tools that we have at hand to explore and utilize it are already so vast, at least on the physical side, that few genuine mysteries concerning the physical constitution will be left in another century. In fact the degree of control of our planet which we are developing promises to be so far-reaching that it now is high time that we accept appropriate responsibility for using this control in the manner of a wise and loving gardener. Unless we do, the earth will respond to us by becoming a refuse-strewn desert.

Somewhat further ahead, it seems evident that we will also take possession of the solar system for whatever purposes may

suit us. It is far too early to know whether the other planets will offer us significant returns beyond scientific information. The view of Mars which we obtained last summer from the Mariner Probe indicates quite clearly that none of the other planets will ever be the verdant home to us our own is. Nevertheless given access to the vast amounts of power which lie at our doorstep we would be foolish to say that the planets will not also become part of our normal heritage.

On the biological side, we have an infinite realm to explore and use. In fact our understanding of biological phenomena is still in its infancy. I doubt if we will ever really exhaust this field of investigation. We must never forget however that nature has had three billion years to promote biological evolution on our planet. For generations to come, we cannot expect to have more than superficial understanding of the life processes. Physicists and physical chemists have been studying the types of color centers produced in simple alkali halide crystals by radiation for about fifty years and have as yet only a limited understanding of their properties. We should not be overconfident about our ability to master the biological world in any final sense. In this connection I am reminded of an incident which took place during one of John Von Neuman's lectures on computers about 1950. He had explained the general principles involved in digital computers and their uses and had entered into the question period. Someone asked if he had any comment to make about the human brain as a result of his work with digital computers. He replied that the brain showed the professional touch. The Lord has been experimenting with biological systems on our planet for several billion years and has developed a very deft hand. We should not expect to achieve professional status in this field in only a few generations.

Up to this time almost every aspect of science ranging from some of the most abstract parts of mathematics to atomic and nuclear physics has been put to practical use in technology by the applied scientist or the engineer sooner, rather than later. Galactic astronomy has perhaps been one of the exceptions. Yet

I do not believe that any one would deny that the perspectives of the universe we have gained through the studies of galactic astronomy have been useful in giving us a significant indication of our own limitations. One naturally wonders these days, as the costs of big science grow, whether all fields of science will continue to be useful in the everyday practical sense. For example, is high energy physics "worth" the investment it will require if continued if it does not lead to anything "practical?" It could well be that we are reaching a stage in which fields such as galactic astronomy and high energy physics will not yield any immediate practical results for several human generations. Nevertheless, I believe we should regard the continuation of research in what might eventually appear to be peripheral areas of human endeavor as an obligation to our own heritage. In our own day we owe so enormously much in countless ways to the far-sightedness of these earlier generations who pursued science for its promise that I do not see how any informed individual can believe that we would be justified in calling an absolute halt to any major frontier field at this time. This does not mean that it will be possible to pursue all areas of big science at the rate which imaginative and impatient scientists would like, for it seems clear that the community of scientists would have the intrinsic capability of using a very large part of our productive capacity if they were given access to it.

*The Hazards to
Science-Based
Civilization*   There are two great hazards to the advance of our science-based civilization. One is the very rapid rise in world population, which is in fact made possible by science-based knowledge. The other is the ever-growing potentiality for global war. There are regions of the earth where the growth of population is not only outstripping the growth of agriculture, but promises to outstrip everything else if it continues. The dangers of a global war do not need elaboration here.

It is evident that what is popularly called the population explosion must be taken with the utmost seriousness every-

where. In fact those societies which have succeeded in achieving a relatively close match between the rise of population and of resources owe it to themselves to help the societies which have not yet gained such control to do so. Yet in spite of the seriousness of the issue I do not believe that the growth of population remotely offers the threat to civilization that is inherent in unlimited global war. We are long past the situation characteristic of primitive societies in which manpower alone is a source of strength. In the main, the countries whose populations are outstripping their resources are at a disadvantage, since so much attention must be given to feeding the hungry mouths of individuals who will contribute little to society. Presumably the leaders in most countries will recognize within the coming generation that the losses associated with uncontrolled population growth greatly exceed the gains. Among other things, a hungry nation is almost certain to be politically unstable. Since the basic technical knowledge to control population growth is already at hand, I would expect to see great progress in the control of world population in the coming generation.

The dangers inherent in total war are another matter. It seems evident that the emotional impulses which make it possible for leaders to initiate great wars lie in substantial part outside the rational aspects of our makeup. They seem to be associated with portions of our hereditary code which were inscribed when our pattern of life was far more animal-like than it is at present. It is evident that we do have substantial ability to bring our conscious wills sufficiently to the fore to overcome much of this instinctive behavior, otherwise we would never have managed to evolve relatively stable social units containing several hundred million people. We must recognize, however, that centuries and much trial and error and anguish have been required to achieve such a state. Moreover, the stability of large groupings is still far from absolute. We have neither the centuries at our disposal nor the opportunity for very extensive trial and error in dealing with the

dangers of global war. The pessimist who wishes to use the past to judge the future has a right to claim that the future of a society founded on science-based technology is hopelessly booby-trapped. The one great hope we can have is that as the common peril becomes more and more obvious to the peoples of the world, fear based on wisdom will generate the critical degree of restraint. Our species stands near the crest of a great watershed. The generation now being born will probably know in its span of life whether we have the restraint to continue the advance of mankind indefinitely into the future.

The Open World of Science

The Impact of Security on the Balance and Quality of Scientific Effort

by

SIR SOLLY ZUCKERMAN

United Kingdom Ministry of Defense

Those, and I imagine there may be many, who are unfamiliar with the nature of the U.S. Office of Naval Research will no doubt find it paradoxical that the organizers of these celebrations should have proposed for a convocation address the theme of The Open World of Science. Some bewilderment could well be excused. Far from an open world of science, a closed environment regulated by the demands of military security is what one would ordinarily equate with an organization whose basic purpose is to underpin with the best technical knowledge the most powerful navy in the world of today.

Happily, the Office of Naval Research, whose twentieth anniversary we are here to celebrate, is in no conceivable sense ordinary. The succession of brilliant men who have directed its affairs have continuously exemplified the basic unity of the open world of science and the kind of science from which the technology of modern defense stems, and have been as much concerned to encourage the growth of basic science as they have to promote the technological pre-eminence of the navy they served. They have also proved that neither goal could have been achieved if the closest relations had not been encouraged between scientists within the American Government

machine and the leaders of science in the outside world—whether in the Universities or in Industry. In these respects the Office of Naval Research has established a pattern of operation which is the envy of the wide world. In the recognition of this achievement, I have been asked by the President of the Royal Society of London to bring you the congratulations and good wishes of the Society on this happy occasion.

The Scientist at His Bench   The particular topic about which I have been invited to speak relates to the significance of communication and secrecy in the world of science. Since it happens to be only a single aspect of the wider issue of the conditions of scientific progress, I intend to discharge my task in the context of a general and brief review of the latter problem recognizing that by so doing I am abiding by a tradition of self-enquiry which seems to have become a peculiar habit of scientists. I know of no other field of culture, whether it be politics or art, religion or music, whose practitioners are given to as much communal analysis as we are. And I assume we engage in it because we hope it will lead to a better understanding of the processes whereby scientific knowledge grows, of the constraints which inhibit its free development, and of the pitfalls to be avoided as we strive after some kind of scientific perfection.

Let us, therefore, begin with the individual research worker. Usually we shall see him growing up in a university department where his first field of inquiry would have been greatly influenced by the work, however narrow or however wide its intellectual scope, that was going on around him. To some extent, he, therefore, begins as a victim of fashion; and to an extent which will vary with his own powers, and particularly with his capacity for truly original thinking, he will continue as such, given that he carries on as a research worker at all. A far-reaching advance in the laboratory in which he may be working, whether it happens to be in his own or in a related field of interest, may turn the direction of his inquiries. So, too, will some new major development in another laboratory.

He can be expected to demand conditions which will allow him to proceed as fast as his inspiration impels, and in the direction it commands. He cannot predict when success will crown his efforts. A lot of his work will turn out to be plain hard slogging—but no less fascinating for that—whether he is researching in some esoteric branch of natural knowledge, or directing his energies to advancing some obviously utilitarian field of applied science. What he basically wants and needs is the assurance that he will be allowed to give full rein to his curiosity without being harried, until the moment comes when he himself thinks his ideas have either flowered or run into the sands—and it is time to change direction—or give up research. He wants a library and journals in which to publish. He wants an environment in which there are no bars to the acquisition of the knowledge gained by others. He needs the opportunity to discuss, at scientific meetings and seminars, mutual interests with colleagues. When appropriate, he would like guidance and encouragement, and when he discovers something new, the acclaim of colleagues—there have been very few scientists who have wished to remain anonymous, or to suppress the discoveries which they have made. Every scientist is a member of a world-wide community of scientists, all of whom work in the same field. The “community” may sometimes consist of no more than a half-dozen men; sometimes it may number hundreds or even thousands. But whatever its size it constitutes the particular environment in which the scientist finds his interest, and where basically he seeks to be recognized and judged.

Whether a scientific discovery proves to be far-reaching or trivial, it is inevitably an act of creation. What matters to the scientist, as Koestler¹ so eloquently puts it, is “the emergence of order out of disorder, of signal out of noise, of harmony out of dissonance, of a meaningful whole out of meaningless bits, of cosmos out of chaos.” Few can enjoy this kind of revelation—which indeed applies to any field of creative activity—or derive

1. Koestler, Arthur (1965). *Encounter*, Vol. 25, p. 32.

from it any sense of fulfilment, without trying to communicate it to others. This is so even though communication is not inevitably associated with acceptance. Something which is new is opposed to something which is old; and often acceptance of the new is less dependent either on its intellectual force or potential practical value than on the strength of the conventions and vested interest which sustains the old.

As it exists in the world today, science is a cultural development of medieval Western Europe. Why it should have been that the particular system of scientific method that has proved so successful, and which has now swept the world in a way no single religious or political system has, should have emerged in the 17th century rather in some earlier age, and in Western Europe rather than in, say, China or Greece, or some corner of Islam, is a matter for speculation. Unlike what happened in Europe, it may have been, as many have suggested, that the emergence of the urban civilizations of these other parts of the world was associated with a diversification of labor which led to an unfortunate separation of the craftsmen out of whose labors man's earliest views of science were born, from a specialized class of privileged philosophers and priests who were not concerned with the actual doing of things.

But be this as it may, our kind of scientific environment, an environment of experiment and controlled observation, emerged in a sparsely populated Europe, in which illiteracy was the rule rather than the exception, and in which the Galileos and Newtons, the Leonardos and the Wrens, as well as the informed Princes and merchants to whose society they belonged, were not only interested in the advance of pure knowledge, but generally conscious of the fact that material riches and power followed in the train of science. As Christopher Wren put it in a draft constitution which he drew up in 1660 as the basis for Charles II's prospective Charter for the Royal Society, the purpose of the members of such a Society should be to "prosecute effectually the Advancement of Nat-

ural Experimental Philosophy, especially those Parts of it which concern the Encrease of Commerce, by the Addition of Useful Inventions tending to the Ease, Profit, or Health of our Subjects.”²

The fathers of modern science saw it as a body of exact knowledge which could lead to useful application, or as we would call it today, technology. But their successors did not always view it in the same light. In the United Kingdom, for example, pure science has in general flourished over the years, whereas scientific technology has only too often languished. In other countries, notably the United States and Germany, the importance and value of an up-to-date technology has always been widely recognized. Almost always when efforts have been made to spur the United Kingdom to an interest in science and in education in the sciences, as happened during a large part of the 19th century, and as has been going on continuously during the two decades since the Second World War, attention has been directed, for purposes of example, to the achievements of other countries, such as the United States, and the emphasis of the debate has been essentially utilitarian. There was as much talk of the need for more science in 19th century England, with Commissions counselling the establishment of Ministries of Science and of Government Boards of Science, as there can have been anywhere in the world. Perhaps the most powerful propagandist for science of modern times was Lyon Playfair, who warned his fellow-Britishers as early as 1851 that “as surely as darkness follows the setting of the sun, so surely will England recede as a manufacturing nation, unless her industrial population becomes much more conversant with science than they now are.”³ And a little later he added: “In this country we have eminent ‘practical’ men and eminent ‘scientific’ men, but they are not united and generally walk in paths wholly distinct.

2. Bernal, J. D. (1939). *The Social Function of Science*. London. Kegan Paul.

3. Cardswell, D. S L. (1957). *The Organisation of Science in England*. London. Heinemann.

. . . From this absence of connexion there is often a want of mutual esteem and a misapprehension of their relative importance to each other."

I often wonder whether the different ways in which the U.K., the U.S.A and Germany have treated the connexion between science and technology do not to some extent reflect differences in the ways these three countries increased their wealth during the 18th and 19th centuries. In Britain—and this, I believe, was not the case in the United States or Germany—revenues from non-industrial investment overseas were an important addition to the profits which manufacturing industry provided. Those days are long since past, as only too evident from the nature of Britain's present balance of payments problem.

But by whatever routes different countries
Scientists Today may have made their way into the present age, and to the enjoyment of what has become a common scientific heritage, scientists since the Second World War are now all in much the same position—the prime actors in a new dynamic phase of the world's social and political evolution. All who belonged to countries which were involved in the war found themselves part of the most determined and directed drive to gain and harness scientific knowledge that the world had ever known, and supported on a scale which made the cumulative total of the resources that in previous decades had gone to science seem trivial. The immediate results, as we all know, were the emergence of radar, jet engines, penicillin, the atom bomb, D.D.T., and a host of other things.

The secondary results of this harnessing of science were as numerous, and in some ways more important to the future of the world. The demand in which scientists and engineers found themselves twenty-five years ago has not only continued, but has spread to all countries. There can never have been a period in history when any other profession has been so uni-

versally cultivated. All countries, old and new, have had to expand and improve their facilities for higher education and, in particular, for scientific education. The size of the full-time university population of the United Kingdom has tripled in the past twenty years, and postgraduate education has multiplied even more—yet the call for more scientific manpower seems as demanding now as it was when it all began. In all industrialized countries, too, financial support for science and engineering, instead of falling back to pre-war levels, is now far higher than it has ever been. The 20 billion or so dollars available for research and development in the United States compares with some 3 billion in 1946 and the £750m. in the United Kingdom with £70m.—a vast difference even after allowing for the effects of inflation, and for increase in our numbers.

Increasing numbers of scientists and engineers, and increasing support for research and development, constitute what one might describe as an intrinsic aspect of the new wave of technological exploitation which is now engulfing the world. Its external aspect is made up of those divergent forces which lead to the ever-mounting demand for more science and technology—more science to assure better health or better measures of birth-control; more agricultural science to improve agricultural output in impoverished parts of the world; more research and development for weapons-systems; more resources for space science and satellite communications; a better scientific outlook to improve transportation and other public services—and in the private sphere the host of innovations which are always welcomed by the ordinary consumer. In a period of violent and rapid transformation, everyone looks to “science” for a more secure and a happier future.

Is an atmosphere of this kind, an atmosphere which the scientist himself has helped create, one which provides the best conditions in which science can flourish?

*Some Major
Organizational Changes
in the Past
Twenty-Five Years*

When I contrast the state of science today with what it was like twenty-five years ago, several big changes in the way we conduct our affairs stand out. First, we are much more organized. Those of us who started our scientific careers before the Second World War can appreciate this fact only too readily. Second, much more research and development is now carried out by teams of research workers, sometimes big teams, rather than by individuals, than was ever the case before. Third, though we borrow each other's techniques we have also tended to become more specialized, with an efflorescence of new journals. And, fourth, technological developments have not only made more science possible, but, on average, very much more expensive per research workers employed—particularly in the field of defense science. For example, there can be no area of science in which computers have not made it possible to undertake researches which could never have been dreamt of before. But, compared to the kind of equipment the scientists enjoyed in the thirties, computers cost big money. So do radio-telescopes and particle accelerators and space vehicles.

An immediate consequence of all this is that however considerable, in terms of actual money, the resources that are now made available to scientists—pure and applied—their allocation calls increasingly for agreement about priorities. How is this to be done? Demands on the interests of scientists are fast outrunning their numbers. The interests of scientists are growing all the time. How can one conceive of a conscious balance of effort over the whole field of science when the latter is always changing, or again when so large an amount of the money that is made available for research and development comes from Defense budgets, and when a great deal of the work which is carried out by scientists is shrouded in secrecy?

Priorities in Scientific Work There has been much public debate about priorities in science these past few years, particularly these past three or four years, and a considerable intellectual effort has been made in different countries to agree on the principles which should dictate the proportion of public expenditure which should go first to the whole of research and development, second, to basic as opposed to "mission-orientated" or applied research—which some have aptly differentiated as curiosity-directed as opposed to need-directed research—and, third, to separate fields of science. We are still far from agreement on any of these matters. I myself am somewhat skeptical about our chances of ever finding a set of universal principles which will tell us how much support pure research should receive, whether we accept as a working assumption that the cultivation of basic science should be regarded as an "overhead" cost to the economic exploitation of scientific knowledge in general, whether it is something which should be supported as one of man's cultural activities, or whether the justification is a mixture of both these propositions.⁴

This particular issue is, however, not one which I need explore on this occasion. But the question of how priorities are to be decided within the budget which is set for us is important to my theme of the conditions for scientific progress.

Priorities in Basic Research The problem is clearly not the same in the open world of basic research as it is in the more secret, even if wider, world in which applied science and development flourish.

Some might suppose that because certain areas of basic science look as though they will pay off better and sooner than others, any good administrator could decide how much support to give different fields of basic science. That is not my view. I

4. Toulmin, Stephen (1966). *The Complexity of Scientific Choice II: Culture, Overheads or Tertiary Industry? Minerva*, Vol. 4, p. 155. *Basic Research and National Goals: A Report to the Committee on Science and Astronautics, U.S. House of Representatives, by the National Academy of Sciences* (1965) Washington. U.S. Government Printing Office.

believe that scientists themselves can best decide how to allocate such resources for basic science as are provided from a total Research and Development Budget. They may not do the job very well, but it is inconceivable that anyone else could do the job better. whatever the criteria by which the relative claims of different fields of work are judged, whether of possible utility or intellectual merit.

Nothing, as I have already implied, is static in science—neither fields of interest nor methods, techniques or what you will. Molecular biology began, as it were, yesterday; radio-astronomy, which took off from radar, the day before that; as the involvement of European countries in overseas colonial territories declines, their interest in systematic helminthology and parasitology and tropical diseases declines; nuclear physics increased the number of atomic elements and radio-chemistry revived organic chemistry; lasers emerge and find a use in highly different fields ranging from tele-links in weapons systems to ophthalmic surgery. As the techniques of one branch of science become applied to another, new border-line subjects emerge, and these then become established as disciplines of their own. The changing pattern of science is thus not only like that of a turning kaleidoscope; it is that of a turning and expanding kaleidoscope, the beads and bits in which are added to hourly, and in an unpredictable way. As the pattern changes so does the intellectual stimulus of its different parts. No one scientist understands the whole pattern, for in one sense all scientists are specialists. But in another we are all becoming border-line scientists. It is, therefore, a fair proposition that scientists as a class would be more likely than would non-scientists to get the sense of the whole changing pattern of science. It is essentially for this reason that I hold that the main responsibility for deciding how to allocate the resources set aside for basic science should be firmly laid on representatives of the scientific community itself.

For all practical purposes, of course, the resources available to basic science are totally deployed at any given moment, and

proposals for change are automatically resisted. One reads about fixed research budgets being adjusted, and of dying points in research giving way to growing points, but one seldom hears about deliberate efforts to bring about the demise of a project once it has been started. The tendency is for a piece of research to go on till it dies a natural death because of intellectual, leading eventually to financial, inanition. Nonetheless, the new and inexpensive good research worker usually gets what he wants after the quality of his ideas and the excellence of his performance have been endorsed by the more experienced scientists who are usually called upon to act as judges by money-giving institutions. This happens even for scientists belonging to countries where very little money is spent on science—for if a man cannot get at home what he feels he needs in order to pursue his researches, he usually finds a way of obtaining it somewhere else—usually in the U.S.A.

The situation is very different when big money is wanted for some major new departure in basic science, say, for the provision of a new radio-telescope or an accelerator. In these circumstances, the men who pronounce in the national interest on the quality and promise of scientific ideas have to agree among themselves that the resources which are being sought would be better spent on the new scheme than on some other expensive work to which they are already devoted—and it is very difficult either to get agreement to such a decision or to implement it if it is agreed. There is always resistance to this kind of change. Basic scientists are usually specialists so far as field of interest is concerned, and redeployment, which would result from any such decision, does not always follow the rules of the market place.

Alternatively, the judges will have to agree that if new money can be raised, which is the usual way research budgets are adjusted, it would be better spent on the new scheme than on any of, say, a half-dozen other schemes in different areas of science which are also on the table awaiting decision. And someone else would have to agree that the new money could, in fact, be provided.

In the end the choice is largely determined by a combination of chance, advocacy, and other intangibles. I would hardly imagine that any scientist would be prepared to argue that the present pattern of interests in basic science in different countries represents some rationally conceived and implemented plan. Working scientists might be more inclined to attribute the broad outlines of the pattern to the interaction of the inertia of past decision, and the play of present fashion.

Priorities in Applied Research and Development When we come to some kinds of basic research which are mission-oriented and, more particularly, to applied research and then development, we are in an area of choice where the alternatives are, as a rule, both more expensive, and where judgment implies some prophetic view of the usefulness of the tangible things which may materialize from the work in the kind of world which will exist, say, ten years hence.

Most, if not all of us, have experienced the unknowns in this kind of exercise, the doubts which balance conviction, and, when it comes to development work, the underestimation of difficulties. We know that the concept of utility is a relative one because the future usually turns out differently from what one imagined; and we also know that when it comes to utility, some line of approach to the same general end, and of which we might be unaware, may turn out to be far better than the one we have pursued. "When it works", so the saying goes, "it's obsolete."

In deciding the allocation of the large sums of public money which may be involved in all these more advanced stages of the scientific process, the voices of other judges—of politicians, administrators, and industrialists—must be heard. But so, too, must that of the scientist, even if his views on the subject of utility may not be specially relevant. For although he may be as impotent as his non-scientific colleagues in predicting the social consequences of scientific discovery, the scientist knows better than they what the technical facts are,

and about the possibility of their successful exploitation. What is more, if scientists were not included in the councils where priorities of vast public expenditures for applied science and development are determined, they would merely become the slaves of their fellow-citizens. Such a system would hardly last in this modern world of ours for more than a few years—given that it lasted that long!

When scientists are called in to play a part in the determination of priorities, more is called for than just the qualities that make them good scientists in the laboratory or the field. But since these qualifications are basic to the rest, let us first remind ourselves of what they are—in the ideal.

The Maintenance of Standards First and foremost a scientist must be a man who adheres strictly to the rules of scientific method; a realization that experiments and observations have to be properly controlled must be part of his second nature. Obviously he should also have an intimate and wide knowledge of his field of specialization. He should be endowed with a creative imagination, judgment and technical skill, as well as with persistence and a proclivity for hard work. His capacity for honest self-criticism should be highly developed and combined with an ability to take criticism from others. He should also be open-minded to the fact that progress, technical or otherwise, in one field may affect progress or direction in another. I could add other qualities, but these are enough to go on with in painting the outlines of a scientific paragon.

Scientists vary enormously both in the extent to which they embody these virtues and in their general abilities. Partly as a result, the scientific process has itself generated a unique set of devices for assuring the high quality of scientific work. The first is the fact that scientists, whenever they possibly can, publish the results of their researches. If a particular piece of scientific work is no good, it will, either immediately or in time, be exposed as such; and a scientist who becomes known for bad work is soon finished. The second is

the existence of national academies of science with limited memberships restricted to the best men. To the lay-man, and to scientists as well, these academies are the embodiment of the prestige of science and, as a result, become the most important guardians of quality. The third is the existence of international institutions which accord rewards for scientific work of the highest quality, such, for example, as the Nobel prizes.

So far I have spoken only about the qualities which one would expect to find in the ideal scientist, and about the mechanisms whereby the world of science assures its own standards of excellence. To them must be added more general virtues when we seek for people who would be competent to advise on science policy, by which I essentially mean advice about the most reasonable ways in which the scientific resources of a country can be cultivated and deployed. First of all, we would seek not just narrow specialists, but men who have some idea of what goes on in fields of science other than their own, and who have the competence and interest to learn. Second, we would seek people with the minimum of prejudice—certainly about subjects if not about persons. And finally—and this perhaps is the most difficult—we would seek men whose sophistication extended outside the domains of science, men who had an awareness of the vast changes which are taking place in the world as a whole, and of the forces which are bringing them about.

But having drawn my blue-print, I have to ask whether it is a realistic one? Is the environment of today one which provides the best conditions either for the emergence of the scientific ideal or for scientific progress? My own answer would be, I fear, very equivocal. While the material opportunities for scientists have never been better, there are many dangers in the present situation. The scientific world is being subdivided not only by its multifarious interests, but also by the facts of international and national secrecy, commercial as well as military. Those who are responsible to their peers for advising on national science policies are in serious difficulty

because of these secret worlds of science. So, too, are individual scientists. Equally, the vast resources which have been made available for scientific work have created what some have called a scientific rat-race, and have also made it possible for some kinds of pseudo-science to emerge and to confuse the scene. Let me deal separately with these issues.

The open world of science is mainly concerned with *Secrecy* the secrets of nature which are laid bare by the researcher. In the closed world of science are the secrets of the researcher which have to be guarded because of national or commercial consideration. No active defense scientist, nor any academic scientist who may at some time or other have been engaged in government work, would question the proposition that certain kinds of information must be protected in the interests of national security. This kind of secrecy is necessary, often vital; but in a very real sense it is also something which is bad for science.

That is so because communication is a necessary part of the scientific process. Work which is done in secret almost always suffers in quality because it is not exposed to the full blaze of scientific criticism. Secrecy also means that the main body of science is always in danger of being kept in ignorance, at least for a time, of some germinal idea or some new and far-reaching technique. In addition, secrecy in Government laboratories may hinder the economic exploitation of new discoveries, and lead to the pursuit in parallel, and in partial if not complete ignorance, of fields of study which necessitate highly expensive facilities. The question which we need to ask, therefore, is whether the considerations which argue for secrecy always outweigh its deleterious effects, and how the latter can be mitigated. We first need to ask what is it that we gain by secrecy, even in the defense field?

In the days before scientific journals began to appear—in the latter half of the 17th century—the small number of scientists who were alive at any one time kept each other informed, by way of direct correspondence, about the work

they were doing. Journals provided an easier and wider method of communication, and also eventually a means whereby the individual scientist could satisfy his desire to proclaim his discoveries to the world, in an effort to "get in first". The fear that one's incipient discoveries might be pre-empted from some unknown quarter is both an urge to secrecy when a piece of work is in progress, and a spur to publication when it is completed—or, indeed, before it is completed. But in spite of the understandable impulse which may lead a scientist to suppress information about some brain-child of his until he feels the time is ripe to disclose it, I do not think that secrecy plays any useful part in the open world of science. Indeed, I should be inclined to say that the temptation to succumb to the impulse would normally be in inverse proportion to the quality of the work, and to the quality of the man concerned.

We are no longer in the days of, say, Lavoisier when, because of the small numbers of scientists in the world, it might have been expected that discoveries would usually be the product of only one mind, and that they would emerge at one particular moment in time. Scientists who were once numbered in their tens are now counted in thousands and tens of thousands. We all base ourselves on a common pool of knowledge. We all know the general form of the "hot problems" in physics, or chemistry, or genetics. When we exclude the small number of absolutely novel discoveries or hypotheses which constitute the foundation stones of the body of scientific knowledge, and which are added to only rarely, we need not feel surprised if in these days the same ideas are formulated more than once, in different parts of the world, and often about the same time. It is almost inconceivable that a Mendel could today report the basic law of genetics, and that no-one would pick it up till thirty years later. What is more likely is that we would find that more than one Mendel had been thinking along the same lines, and that two, three, or four of them were about to publish their results at about the same time—and much the same

results. In the pursuit and exploitation of new concepts we almost always seem to find several laboratories moving along in parallel. We can recall only too well how much parallel development of the same projects occurred in England and America, on the one hand, and Germany on the other, during the course of the Second World War.

Secrecy, of course, plays a powerful role in the commercial world. It is indeed difficult to conceive of an industrial undertaking which diverts its own resources to research, revealing to its competitors knowledge which it may have gained at great expense. But the question is to what extent should commercial scientific secrecy be taken? I would not go so far as some who declare that it would pay to publish everything, in the hopes that doing so would be more likely to confuse than benefit a rival firm, which would otherwise be focussing its intelligence efforts on trying to find out what really mattered. But I have a feeling that a contrary policy of publishing little or nothing probably goes too far. In any event, there are considerations more important than secrecy which affect the assessment of the commercial value of the results of industrial research and development—but this is an issue which is of only peripheral interest to the theme I have been set, and I cannot pursue it here.

The point which remains central to my topic is the extent to which it is wise to carry secrecy in the basic researches which we pursue in our Government laboratories, whether their motivation is “pure” or “mission-orientated”. A further question which arises is who should decide—the scientists or an independent body of security officials? What, to put a third question, does “need to know” mean in the field of basic science—I am, of course, excluding from consideration the need for security when we talk of development for clearly defined weapons systems. Here there can be no question—security is an essential rule.

To try and illustrate the problem, let us turn to one of the best technical secrets of all time, the work which led during

the course of the Second World War to the development of the nuclear bomb. At its start it was hardly a secret at all. It was certainly not a military secret. The scientists in whose minds the idea was born themselves decided, as a corporate voluntary act, to curtail open publication of any information which might point in the direction of a bomb. When the idea of a nuclear weapon became an officially defined project, Government administrators both in the U.K. and the U.S. found themselves in a quandary because of the number of refugee scientists who were involved. But in spite of the administrators' opposition—to quote from the Official British History⁵ of the subject—"the greatest of all wartime secrets was entrusted to scientists excluded for security reasons from other war work." Later, officials in our two countries argued about the manner and extent to which security should be maintained, and at the end of the war the British authorities expressed strong opposition, on grounds of security, to the publication by Dr. Smyth of his famous report on the Manhattan project, a volume which the U.S. authorities felt needed to appear in print. Here the British authorities concerned were almost certainly too cautious. Niels Bohr, whose intervention in the politics of nuclear affairs had a better reception at the hands of President Roosevelt than at those of Mr. Churchill, as well as Sir James Chadwick, one of the most distinguished of the British contributors to the project, believed that the mechanics of the bomb could not be held secret for long, and for this reason they argued for the international control of the atom well before the end of the Second World War. What happened? Even though every effort was made to prevent information passing to other governments about the design and construction of bombs, and the construction and operation of plants producing fissile material, other Governments—and not just the U.S.S.R.—did find out. And many who have not yet revealed a knowledge of the subject probably know its secrets,

5. Gowing, Margaret (1964). *Britain and Atomic Energy 1939-1945*. London. Macmillan.

which no doubt they would already have put to use had they judged it in their political interest to do so, and had they had the resources, scientific and financial, which the enterprise would have demanded.

What, then, did we gain by the imposition of secrecy? The main prize was obviously time. And essentially this is probably all that security ever gains in any scientific field. In the end, in most cases sooner rather than later, we can expect other people, our opponents or competitors, to discover what we know. The purpose of security in the technical field is to prolong the time it takes them to learn, and so to add to their costs. In the case of the bomb the prize was at first priceless. It has been argued by some that the additional military security which we can attribute to the nuclear secrecy of more recent years has been all but counterbalanced by certain political problems which it has also generated.

In the light of this story I turn back to my three questions: How far should we keep the basic researches we do in Government laboratories secret; who should decide; and what does the "need-to-know" principle mean in this area of science?

The answer to all these questions is, to my mind, pointed by the fact that the most potent knowledge which ever emerges from the pursuit of basic science can never be recognized as such, and consequently that it can never be guarded at its birth. In spite of the fabulous influence it was to have on military and political affairs, could anyone today conceive in retrospect of any reason why news of Einstein's work should have been suppressed when it first emerged? Why was the enormous importance of D.D.T., on the one hand, and antibiotic action on the other, not recognized at the start? If it had been, would some pharmaceutical house have been justified in suppressing the information? What conceivable good could have resulted from any efforts to suppress the basic work which lead up to radar—at a time when its practical significance was, in fact, not recognised?

Another consideration which affects my own answer to

these questions of security in basic science, whether pure or "mission-orientated", is that I do not believe that the source of the great ideas which have transformed the scientific scene has ever been the secret laboratories of governments. It is in the open world, not the closed world of military science, where the big ideas have germinated, sprouted and flowered. This is not because the inherent creative quality of the men who have worked in Government laboratories was or is on average below what one finds in, say, a university department. Far from it. My hunch is that the young man who starts working under conditions of secrecy in a field which is defined for him by his superiors is less likely to enjoy the riches of imaginative discovery than the man working in a free and open environment where his work is exposed to the full blaze of scientific criticism. Secrecy and great thoughts do not thrive together.

The third point which determines my judgment in this matter is the fact that the vast growth in scientific activity of the past two to three decades has paradoxically made it difficult for anyone to keep up with what is published, even in the open world of science. We are involved in what has been called a "crisis in communication". So much is published that, secrecy or no secrecy, the average scientist is likely to be ignorant of published observations which could help galvanise his own work. Whatever else, he does not want to do work which has been done already. When one adds to this the fact that the research worker often fails to realise the significance of one or some of his own observations—we all have had experience of this—it becomes all the more important that there should be no unnecessary barriers to information which someone else can provide. A trivial thought, captured from anywhere, from some printed sentence, from the storehouse of memory, can suddenly illuminate what has been obscure, and by so doing bring about a revolution in understanding.

It is all very well hoping that some modern computer system by which the mounting flood of new scientific informa-

tion is indexed, processed, and assembled is going to get us over the difficulty of communication. Whatever can be done by modern bibliographical methods to draw a scientist's attention to a piece of information which might be critical to his work—and a lot can be done—we have to recognise, as Fox⁶ so rightly says, that “machines cannot distinguish good papers from bad ones . . . nor can they answer those often crucial questions . . . the ones the enquirer does not know how to ask.”

In order to clear channels of communication in the open world of science, we have had to return in recent years to the direct exchange of information between small groups of people working in the same field, people who correspond with each other, and meet repeatedly at national and international symposia. I share the hope that these and other devices will improve the situation. But the chances would be all the better if men who work in basic areas of science, whether or not they relate directly or indirectly to technical problems of national defense, were not unnecessarily impeded in exchanging their scientific knowledge with others.

It seems ridiculous that there can be men working on the same problems of basic science, who for reasons of presumed security can be unaware of each other's existence, and certainly unaware of each other's results. But this, I am sure, is the case for scientists within our respective countries, leave alone on the two sides of the Atlantic. I know the need for security when it comes to project work—but this is not what I am talking about. I am talking about basic science, whether it is “curiosity” or “need-orientated”. And to go back to my earlier point—how on earth can those who are responsible for advising about the proper allocation of a country's scientific resources to basic science contribute of their best when they may be unaware of what is happening behind their own country's security curtains, where a significant proportion of the work is done. It is bad enough that they are sometimes unaware of what is going on in the open!

6. Fox, Theodore (1965). *Crisis in Communication*. London. Athlone Press.

There are enough barriers already in the open world of science leading to the separation of individual research workers not to erect others which may not be necessary. All in all, and on the basis of my own experience, I should, therefore, conclude that more is lost by throwing security cloaks over the kind of basic science which is done in Government laboratories than is ever to be gained. What is more, I do not believe that anyone has sufficient knowledge and skill to take upon himself the responsibility for imposing any "need to know" principles in these areas of science.

Secrecy prevents free critical discussion and, as I have said, so conduces to a decline in quality. If basic science must be pursued in Government laboratories, even in Government laboratories which are also concerned with applied and development work, leading, say, to weapon systems, I would, therefore, argue that wherever possible arrangements should be made for it to become part of the open world of science. This is the policy which we are trying to implement in the United Kingdom, in the full recognition that occasionally there may be difficulties in differentiating certain kinds of applied science from what is basic. Were this policy to be effective, the men concerned would not only be better able to judge the quality and necessity of their own studies; they would also stand a better chance of gaining prestige and stature in the world of science as a whole, instead of living apart as isolated members. It might be that exceptions should on occasion have to be made to the conclusion I am putting forward; but I must confess that possible ones relating to basic science which have flitted through my mind do not encourage me to weaken my proposition. The thought keeps returning that the same characteristics of unpredictability and the same methods of inquiry apply to basic research whether it is "pure" in the sense that it does not relate as yet to some known field of exploitation, or if it is being pursued in a field where the possibilities of practical exploitation are already recognized. The danger that a piece of work, if published, might give a clue to the applied interests

of the establishment in which it is being carried out could, I feel, be mitigated by administrative procedures. The essential point about the publication of the results of truly basic research is that the research worker himself should always be conscious of the right to publish—unless good reason is shown why he should not.

There is an additional point. In the world of military science secrecy does not, as we are only too well aware, inevitably prevent treachery, any more than treachery has been completely eliminated from other parts of the military machine. If it had been, what need would there be for parts of the secret services we maintain?

The basic science which I am talking about is the body of knowledge which has been rigorously established through the use of genuine methods of scientific inquiry. I am talking particularly about what can still be called the "natural sciences". As scientists, we know what they are, and we know how they have been built up with the years. We also know how new sciences are born. If it were ever to become the case that the occult sciences became scientific, or that water-divining became a science, they would do so only because scientific methods of inquiry had made them so. Primitive man found his minerals by following outcrops of rock back into the earth, having first learnt that some other material, a metal—gold, copper or tin—for which he had found a use, could be extracted from the rock by smelting. He learnt to recognize the rocks he wanted by particular characteristics which he could discern by eye. Today the geologist uses not only the eyes with which he was born, but also new eyes that have been furnished through the advances of science—magnetometers, geiger counters, boring equipment, and so on. He uses scientific methods of observation and analysis to build a corpus of knowledge which not only "explains" the past and present, but also foretells part of the future. That is the only way a science can be created, the only way areas of human interest ever become transformed into bodies of knowledge consisting of proposi-

tions which have the dual characteristics of effective stability and predictive value.

Science, Pseudo-Science and Value Judgments   Secrecy cannot but distort this kind of orderly growth. So, too, does the confusion about science which is only too often conjured up in the lay-mind during these days of rapid scientific development.

With so much science in the air, areas of interest or discussion become treated as scientific whether or not they are subject to the real discipline of scientific method. We live in an age of more and more science, and also alas, more pseudo-science. This, as I suggested earlier on, is one of the unfortunate facts of our time. It, too, certainly does not help provide the best conditions for scientific progress. The public is led to believe that anything is science if it has numbers in it, or demands slide rules, or is carried out by people with a Ph.D.

There is a border-zone of interest where the issues with which we deal start incorporating value-judgments, such as the concept of effectiveness, leading on to cost-effectiveness. This is a kind of gray zone between genuine science and pseudo-science. Within it I would also include some kinds of operational analysis and systems analysis with which the defense scientist has become fairly familiar. In my own view, some kinds of systems-analysis begin to verge on the scientific. Others do not and never will, even though their object is to make precise and to translate into numerical language the issues on which major decisions need to be taken. They cannot become truly scientific because when one course of action is chosen and others rejected as a result of a piece of systems analysis, the situation becomes totally transformed. The implementation of the choice makes it impossible to return to the subject to test the situation afresh—which it is always possible to do in a true science.

This is a major difference. Science, as I said, is more than just numbers, and more than slide rules. It is more than just objective or dispassionate analysis. When scientific enquiry

steps out of its own ground and treads into the area of value-judgments, it starts to become something more than science, and something which then begins to partake of the controversial character of economics, or even of politics.

Tradition has it that economists can never agree between themselves about matters of economic policy, whereas scientists, as scientists, always in the end agree about their own subjects. Is there not a saying that if there were twelve economists in a room discussing some field of economics, you would hear thirteen opinions expressed? If there were twelve scientists in a room discussing a particular area of science, you would in the end—at least theoretically—listen to only one opinion. The reasons why economists immediately differ when discussing economic policy are, according to a recent article by Fritz Machlup,⁷ fourfold. First, they are straightaway confronted by differences in semantics. The established rules of science are there to help the scientist over that obstacle. Second, economists are apt to differ from each other in logical approach. Here again the scientist ought to be protected by the rules of the game. Third, economists are very prone to differ in their factual assumptions. Once more the advantage is to the scientist. And finally economists differ because of differing value-judgments associated with the aims of different courses of action.

I hope that these differences between the social and the exact sciences are merely a reflection of the fact that the latter are further advanced in their formal evolution than is, say, the subject of economics. I certainly see no logical reasons why, in time, economists should not be provided with the basic scientific framework and apparatus of working which will be able to impose the necessary discipline on subjective and wishful thoughts, as is now possible within the body of science.

But at the moment it is only when we come to value judgments that in theory the scientist is exposed to the same blaze of difficulties as the economist, with the added disadvantage

7. Machlup, Fritz (1965). Why Economists Disagree *Proc. Amer. Philos. Soc.*, 109, 1.

that because the scientist is always expected to agree with the body of scientific fact, it is also expected that there will be a single scientific point of view on matters which are external to the facts themselves. This disadvantage is a real one, simply because the occurrence of such disagreements tends sometimes to discredit what is thought of as *the* scientific point of view on matters of policy, scientific and otherwise. On the other hand, because scientists normally operate in conditions which do not permit of arbitrary disagreement, they are probably better able than others to appreciate the factors which lead to disagreement, when disagreement occurs. And they are better able, at all times, to tell the difference between science and pseudo-science—that at least they should be.

Nonetheless value-judgments are the final determinants of priorities in the allocation of resources in the world of science. The big issues are in the end political—military security, health, welfare, and so on. The scientist, as I have insisted, has a part to play in the choice, in the same way as he appreciates better than anyone else that there can be no secrets in basic science—and few indeed in applied science. As Wiesner⁸ has put it, “an advancing technology and an uncertain world call for an extraordinary effort to encompass technical considerations with which the majority of the people are largely unfamiliar.” This, if anything, is clearly a central job for the scientist. The less secrecy, the greater the exchange of information, the more open the world of science, the better will the scientist be able to discharge his responsibilities not only in the laboratory but also in the arena where policy is made—however much his technical considerations have to be qualified by value judgments.

8. Wiesner, Jerome (1965). *Where Science and Politics Meet*. New York. McGraw-Hill.

Science and National Security

by

DR. WILLIAM O. BAKER

Bell Telephone Laboratories, Incorporated

Today we celebrate the role of science, a great symbol of the freedom of man's mind, in defending the freedom of man's person, his society and maybe, ultimately, of his soul. Science denotes a vast realm of knowledge and even of practice, but we shall be thinking of it now as the medium of discovery of new knowledge, such as provided by basic and applied research. We shall recognize gains in the national security related to the support of basic research in all kinds of institutions by the Department of Defense. This support in the last decade changed from \$80 millions to \$300 millions annually (Fig. 1). And we shall see examples of the use and products of applied research, for which the Department of Defense increased its commitment from \$439 millions in 1956 to about \$1800 millions in 1966 (Figs. 2 and 3). As a background to this, we must recall that two decades ago in 1946 total research and development expenditure of all kinds for the components of what is now the Department of Defense was \$418 millions, with the Department of the Navy responsible for \$183 millions, or appreciably more than either of the other two major service agencies. For current calibration, we should now recall that the corresponding Department of Defense total for research, development, and related plant for 1966 is listed at \$6,881 millions (Fig. 4).

Now these familiar figures, both earlier and later, representing a large segment of our national professional resources in the second half of this century, are nevertheless a minor part of our national product and a small cost for freedom and se-

FIGURE 1.—Trends in Federal obligations for basic research, by agency.

FIGURE 2.—Trends in Federal obligations for applied research, by agency.

Billions of dollars

SOURCE: NATIONAL SCIENCE FOUNDATION

FIGURE 3.—Trends in Federal obligations for development, by agency.

FIGURE 4.—Federal obligations for research and development, by agency and character of work.

curity in our times. The interesting question is not the justification of these efforts, whose products are already evident to our nation and to the world. Rather, it is a need to understand more deeply the role of science in achieving the defense we have and in preparing for the compelling and sobering challenges to our security in the years to come.

Evident Role of Science in Military Facilities

Indeed, we know well enough how the science of the past vicennium has supported our major defensive activities. Microwave generation, transmission and reception have enabled the use of radar, guided missiles, ballistic missiles and fire control systems. Nuclear fission and energy have enabled modern strategic

weaponry and submarine and surface ship propulsion. New rocket fuels and analytic mechanics have enabled the use of earth satellites, space vehicles and related missile systems. Chemotherapy, synthetic chemical pesticides and growth regulators have permitted modification of human environment and survival under a diversity of geographic and climatic conditions. Solid state devices, including transistors, diodes, oxide magnets, isolators, etc., have performed in these foregoing functions and have particularly enabled the development of high speed digital machines, leading to a new methodology of defense and war management. Advances in metals and organic materials have permeated our present weapons systems and are especially evident in the design and construction of such vehicles as the A-12 class of aircraft, having new dimensions of upper atmosphere maneuver and range.

Effects of Basic

Science on Strategy

of Systems

Development

But there is a role of science in national security which underlies all of these critical although specific discoveries and applications. This has been rapidly growing in importance during the past two decades, but it is still at an early and formative state. This new role is the part of the scientist, the master of the particular, in systems engineering and systems development, wherein the overall operation is so commonly considered to be the prime province of the generalist. Here is where the Department of Defense, guided by policies, and foresighted men such as several in the Office of Naval Research, recognized one of the historic benefits for our security and strength. For what was actually sensed, wittingly or not, in the bold Navy programs of supporting basic academic research by a mission-oriented agency, was the fallacy of identifying specific mission requirements in scientific and technical components at an early stage of research.

Rather, resources were provided for the support of individual faculty research, some institutional programs, and the sophisticated education of students. Neither faculty nor stu-

dents were ever required, in this explosion of mid-century scientific and technical knowledge, to seek the specific projects which *appeared* relevant to the mission of the naval forces. Rather, they were encouraged to seek the understanding of principles and the grasp of basic experimental techniques. And indeed, it has turned out they could recognize later, especially as members of both the independent and defense industrial community, and also as prime advisors to the military services and to the Federal government generally, the particularly promising options from which, in fact, our great defense systems have been derived.

Now there may have been an element of luck in all of this, perhaps the kind of fortune which reasonable and humane causes deserve. But, in any case, it is a remarkable episode in the progress of learning and of science. We shall try to illustrate what it has meant and some of the opportunities which this practice implies for the future—this practice of being able to optimize the elements of a certain system by having them understood by those who are deeply learned in a particular phase of science.

Incidentally, this theme of the wise use of the master-of-the-particular for interpreting the elements of a system eventually to be assembled by the generalist, explains a few other things about our institutions and our scientific resources in these past two decades. It accounts, for example, for the occasional malaise or even mild agony expressed by engineers and engineering groups, about how they always seem to come in late (just in time to be blamed for some detailed defect), when large new weapons systems, space systems or indeed nuclear power generating or even information handling systems are proposed for national functions. The basic reason is that with the accumulation of all knowledge doubling every fifteen years or so, the options which must be considered in a new systems evolution have rather quickly come to rest so deeply on the foundations of basic physical, mathematical and life sciences that it is quite impractical for the traditional engineer to be required to set

up the best systems plan at the very beginning of a program.

Missile Systems and Science Systems Let us review a few bases of major facilities for the national security in which these aspects of scientific resources are displayed. Obviously the design and development and imple-

mentation of intercontinental ballistic missiles, both land and sea launched, represent one such vital program. Our national technical strategy in this endeavor, beginning with the original report of the von Neumann Committee and the Nobska Study, and extending virtually to the present refinements of the Poseidon and Minuteman missiles, is a fascinating reflection of hybridization among science, technology and engineering, to produce successive and dramatically new generations of defense capabilities. Many essential elements of the systems, including particularly command and control, involving launching, guidance and management of the warhead; the materials of the structure and especially of the nose cone and engine nozzles; and the propulsion, fuel and combustion features were evolved at the fringes of scientific understanding. Other features, such as the plasmas created on re-entry, various hostile environments in which the rockets and their re-entry bodies might have to operate, and many related things were beyond even fairly rudimentary scientific insights. Here, an extraordinary pluralistic strategy, in which many elements of the national scientific and engineering community were brought together early, was begun. Several National Academy of Sciences-National Research Council committees were newly tasked, or those such as the Materials Advisory Board were strongly re-oriented, toward probing the various essential elements of missile systems. This was all without the strict assignment of functional or administrative responsibilities, which resulted internally of course in the fascinating, well-known, confusions of the Army program, the Navy program and the Air Force program, along with an additional cluster of soon-to-be-activated NASA rocket programs. (We should also not miss incidental AEC items such as a

ROVER or ORION, thrust in for extra interest.)

Now the issue is not whether this was a good or efficient administrative structure, but rather to see the mode that was actually exercised for transformation of the best scientific specialities into developmental and systems engineering needs. Some elements of this pluralism are so spectacular that they stand out without deeper analysis. For example, in the field of inertial guidance, the academic engineering studies, especially of Professor Stark Draper at MIT, were so much closer coupled to the necessary computing and other applied sciences in the field than anything which an established industry or a new contractor set up for such a purpose could possibly provide in the time allotted, that it was directly agreed that this academic laboratory should become the principal inertial guidance developmental and systems source. The really subtle factor, however, was that the MIT Laboratory for Instrumentation was literally encircled by scientific talent in physics, metallurgy, solid state, astrophysics, etc. In the ONR pattern, this latter capability was not mission-directed but self-directed. Hence, individual elements of the guidance mission could receive exquisitely particular attention, either directly or indirectly through the intellectual environment. The necessary confidence that these delicate and incredibly precise guidance mechanisms could be fitted into all the other elements of a complex system was already dramatic tribute to the basic notion. This was that enough elements of the total missile system would be understood in fundamental physical terms so that inevitable adjustments in guidance, propulsion, materials and warhead would not be blocked by blind dependence on a single rigid empirical engineering plan. Indeed, such resiliency can be introduced only through some minimum level of basic understanding among the principal elements of a system, and also corresponding availability of a common language for thought and action in the integration and evolution of the system. At least in retrospect, the value of the strategy of invoking non-mission oriented basic research in the early conception of complex new systems is unsurpassed.

I can speak with some feeling on this in relation to missile systems of other kinds, since the earliest effort on radio guided missiles, which became the NIKE systems for air defense, occurred in our research area at Murray Hill before there was any government sponsorship. This was under conditions of diverse scientific insights, in which such efforts as supersonic aerodynamics, shockwave theory, the work of Courant, Hermann Weyl, von Neumann and others in analytical mechanics, the studies of Kistiakowsky and Wilson on explosion-produced shocks, the work of Bethe and Teller on thermal equilibria, the basic studies of a host of others on telecommunications and signal theory and feedback and servomechanisms were all thrown together. Now once more in this case we see, as the program was shaped into a systems development, the essential pattern—of assigning a specific systems program to one kind of institution (in general, the industrial defense contractor) while retaining continuing opportunities for constant reviewing and reconfirming of elemental scientific principles on which the whole system concept must ultimately be based.

The Scientist and Systems Development Now this is institutionally quite different than having the generating scientists operate the developments, as happened by necessity during the flourishing days of the OSRD and NDRC, a quarter of a century ago. Furthermore, modern systems integration and production are just too difficult, expensive and far-flung to have the particularist attempt to mastermind engineering for design and manufacture. This is where the big change has come, and where industrial applied science and technology have progressively matured, so that all are increasingly openminded about the fluidity any particular project should retain. This fluidity means to admit a ceaseless flow of review and re-examination, and sometimes of revision and (often annoying) redoing of parts of the system, as a result of exterior scientific insight and even hindsight. And we cannot say too often how vital is this elasticity of industrial and Defense Department coupling with the national and world scien-

tific community. Here once more the principle emerges that it is *real* science and not mission-oriented or labeled science which must forever support and invigorate the technology of our defense and security. This the ONR has said long and often.

Of course there still are occasional hints, sometimes identified acoustically as between a mumble and a grumble, from various contractors and military agencies that scientific revisions in systems development are highly disruptive and too frequent. Even insertion of advance technology, such as has occasionally been forwarded in continuing improvements of nuclear propelled submarines and other vessels (by overseers—or Rick-overseers?) is “complained” to bear heavily on contract schedules and production.

New Institutions for Systems Engineering and Science Usage Broadly, however, an historic advance has been made in this matter by the creation of not-for-profit systems engineering and technical direction institutions and laboratories. These SE/TD resources were specially demanded by the rapid scientific options generated during the crucial initiation of our major bomb and missile programs a decade ago. Some were first associated with special industrial groups, such as the Space Technology Division of the Ramo-Wooldridge enterprise. Others did systems evaluation, analysis and synthesis in distinguished activities like the Navy’s Operation Evaluation Group and the Air Force’s Rand Corporation. Then it became increasingly clear that this issue, of orderly insertion of new findings and critical judgments into the ongoing systems developments, would require a new and special institutional structure, as well as unusual personnel and leadership. Accordingly, various kinds of not-for-profit quasi-public but Defense Department sponsored and supported enterprises have been founded in the past decade. One important function has been always to assure the matching of military requirements and Defense Department objectives to the technical development and pro-

duction of the contracting industry. But constant impedance matching of the progressing system to the equally rapidly advancing scientific environment and proliferation of new options has become another major function of these not-for-profit enterprises.

Thus, with respect to the national missile programs presently being considered, both the Navy-sponsored Applied Physics Laboratory of Johns Hopkins and the Air Force's Aerospace Corporation have been established so that the most current science could be continuously assessed regarding its application for the major projects to which these institutions have been assigned. In a study of some of these corporations, carried out by an Ad Hoc Group of the Board of Visitors of the Air Force Systems Command and recently issued by the Department of the Air Force, we said "Above all, the Group asserts that the nation is still in an evolutionary and experimental phase of mobilizing its technical resources for countering the constantly increasing threats posed by powerful advanced and ingenious systems. . . . This independent and continuing production of new options and expert reviewing of the progress of science and engineering relevant to the military missions form an essential basis for the technical planning which must be undertaken. . . . In summary, we believe that the not-for-profit institutions offer one practical and efficient method of boldly mobilizing the essential resources required to accomplish vital parts of the Air Force's systems planning and engineering functions."

But whatever may be the mechanisms of the future, these new institutions already represent excellent opportunities. We must expect and prepare for continuing revolutionary influences of new science on military systems conception, development and acquisition. Indeed, with the expanding use of incentive contracts for military systems development and production, the necessity for recognized mechanisms such as the nonprofit institutions has become even more compelling. For through them, essential revisions of technology can occur dur-

ing a systems contract period. Otherwise, it is now economically as well as administratively increasingly difficult for industrial contractors to respond to the changes from new scientific findings, as was done with greater or lesser degrees of responsibility during the earlier period of cost plus fixed fee or similar arrangements.

Scientific

Discovery and

Satellite Recovery

To illustrate the central thesis, let us look now at a few detailed instances with which one happens to be particularly familiar.

This thesis shows how a capability of rapid generation and assimilation of extra-mis-

sionary, new science has become indispensable for national military strength, through systems planning and management. Consider another feature of our missile and space capabilities, namely the composition of the various weapons systems which enables them to endure the gradients of environment from launching, transit of the whole earth's atmosphere into outer space, and worst of all, controlled re-entry at 18,000 miles per hour or more through the chemically-active composite which surrounds this planet. The kind of rocket delivery we were able to produce in the 1955 to 1960 time would not have functioned with the nose cones and other structural heat shield and protective features known at the time of the initiation of the ICBM program. Now under the urgent compulsions of the mid-fifties, even before the launching of the first SPUTNIK, orderly engineering plans had progressed to yield the best refractory metallic nose cone and heat shield that could be conceived. However, for six or seven years before this there had been carried forward (in the best pattern of ONR research, although it did not happen to be directly associated with those programs) a basic scientific study of the mechanism of high temperature conversion of polymeric bodies into highly conjugated dehydrogenated structures, and eventually into diverse forms of refractory carbon itself. Surprising energy absorptions and the critical retention of relatively rigid geometrical form had been among the unanticipated and, of course, nonmission-

directed findings from this work. Many of the older concepts of the formation of chars and even of the oxidation qualities of hydrocarbon networks had to be revised. Accordingly, in May of 1955 following several weeks of intensive study organized by the Department of Defense's and National Research Council's Material Advisory Board, three chemists reported to the Board the expectation that organic composite nose cones composed of certain types of fibers combined with various reinforcing agents, including both inorganic and organic fibers, would provide mechanically feasible structures. These ablating shields should be capable of heat absorption exceeding the best copper systems under design at that time by more than an order of magnitude. The rest of the story is relatively well known, for at this point intensive programs of mission-oriented research and development in both industrial contract laboratories (including primarily the aerospace industry centers of such corporations as AVCO, General Electric, Lockheed and others) and also in appropriate governmental centers were established. Thus, from the first successful recovered ballistic nose cone displayed by President Eisenhower a few years later, to the most sophisticated heat shields for the recovery of manned earth satellites today, such organic ablative barriers have been the basis for our re-entry vehicle construction.

While any amount of speculation could say that empirically this sort of energy absorber would have been applied eventually regardless of the basic information on its structural and electronic characteristics, the fact is that certain critical decisions could be made quickly and effectively at an extremely early stage in a tightly programmed effort, because of the general scientific grasp which basic research had provided. Naturally, the original concept was far short of the refined applied science and engineering, involving many aspects of aerodynamics, mechanics and thermodynamics, chemistry and so forth, which eventually were used in the evolution of ablative bodies. The important thing is that the systems development and systems procurement for the missile system were able and willing to

absorb new basic scientific results rapidly. Through the flexible and versatile contract systems and inhouse technical support, the Department of Defense and its service departments were able to mobilize responsible new developmental and engineering talents, without disrupting or even seriously delaying the rest of the complex weapons system and its awesome warhead.

Another cardinal area in which this recognition of the role of scientific specialty, without the commitment of the scientists subjectively to a particular systems mission, has been expertly cultivated is in the whole

The Nucleus,
the Crystal,
and the Weapon

field of nuclear weapons and energy sources. Indeed the research and development programs of the Atomic Energy Commission themselves reflect a large element of this wisdom. But it is also interesting to look for the particular cases in fields where contributions of the specialist might have been little expected or have appeared to be of minor systems significance. One of these is provided in the influence of solid state science and technology in detectors and counters for nuclear particles. It was found in connection with the research on semiconductors and transistors that appropriately treated single crystals of germanium, silicon and certain other structures were particularly sensitive and efficient counters of elementary particles, through the hole-electron pair generation which occurred by interaction of the impacting particle with the crystal lattice (Fig. 5). In recent years, many applications of this fundamental quality have been made to interesting scientific problems in nuclear physics. For instance, following the work of Brown, Burton and their co-workers at the Murray Hill Laboratories on development of these crystals, Walter Gibson, an able former student of Professor Glenn Seaborg, used them as particle counters to derive the total distribution of atom masses yielded by the neutron-induced fission of uranium. From coincidence counter experiments covering specific regions of total kinetic energy, new and unsuspected features of energy release were indicated by these studies. Thus the laborious classical radio-

FIGURE 5.—Semiconductor particle counter.

chemical means of determining fission mass distributions of the kinds and numbers and weights of the atoms which result from splitting of the uranium nucleus, have been replaced. The counter-derived curve of the mass yield distribution is obtained in a few hours of work, in contrast with many months of effort that is needed by earlier means (Fig. 6). Further, the fission mass yield is obtained for specific regions of total kinetic energy and has yielded some new insights into the fission process itself. But this is not our major point here, anymore than would be the recent dramatic use of these lithium drifted germanium detectors by Wu, Rainwater and Devons and also by Anderson, Fincks and their associates for the elucidation of the shape and structure of the muonic atom. (This is derived as the "atom" emits, fleetingly, X-rays when the muons jump between orbits around the nucleus during their life of a few millionths of a second.) Also, we shall only mention their use, coupled with modern computer techniques, for the recent discovery of an excited state of the alpha particle. Rather, we shall look once more at their astonishing impact on major systems development, of sun-powered, electrically regulated satellites of primary importance for the national security. These systems are crucially influenced by the behavior of crystals under particle bombardment in outer space.

Particles in   The first active satellite system for communi-
"Empty" Space   cations was the forerunner of present and
                                         oncoming global systems of the highest value
                                         for international security support services.

This TELSTAR was also a close relative of many satellite systems important for naval navigation, position fixing, weather studies and so forth (Fig. 7). This satellite was destined to operate in parts of the earth's magnetosphere containing Van Allen belts of trapped radiation, phenomena already studied with strong ONR sponsorship. The first space exposures of the vital semiconductor solar cells, transistors and diodes in these satellites gravely shortened their functional life (and in the case of some experimental vehicles launched by other agencies actually pre-

FIGURE 7.—Canister suspension cords, aluminized polyester-glass fibre sheet and lacing of heat insulation in TELSTAR satellite.

vented almost immediately their normal operation). But basic scientific correlations of radiation damage effects were already established for the crystal junction particle counter and other junction devices, by Brown, Gabbe and their co-workers. They also identified from counting equipment already installed in the first TELSTAR satellite the position and range and quality of the dangerous radiation (Fig. 8). Immediate corrective measures, benefiting also from basic studies in military laboratories concerned with radiation damage effects, were taken for this and subsequent satellites. Thus, for example, the initial 15-watt output of the Bell Solar Cell arrays which provided satellite power was reduced only by $3\frac{1}{2}$ watts during the first year of relatively intense bombardment and, of course, very much less proportionately in the succeeding years. In addition, however, the whole scientific study is continuing (now pri-

FIGURE 8.—Solid state particles-counting device for measuring radiation in outer space, with the TELSTAR satellite.

marily under NASA sponsorship but based on the same novel techniques and led by the same specialists as before) so that a scientific morphology of the earth's environs is being derived by the most modern physical methods. These include particularly computer programs which reduce and plot directly the basic information with nearly real time speed. Indeed Brown, Gabbe and their colleagues have been estimated to handle as much space radiation information in the ongoing results from the solid state counters which they have constructed and

operated as most other space particle probes combined. Altogether, the emerging knowledge of our planet's environment, including effects of the STARFISH high altitude nuclear explosion and some other similar events, is now approaching a level to permit many valuable systems engineering and systems analysis estimates. These apply to the performance of various kinds of space vehicles, communications systems, events susceptible to distant magnetic fields, the solar wind and kinetics of cosmic rays. Thus the distribution of electrons with energies of about 1 Mev is usually separated into an inner and outer Van Allen belt with maxima at radial distances of about 1.25 and 4.5 earth radii on the magnetic equator (Fig. 9). On the other hand, the proton distribution for particles energetically between one and a hundred Mev is represented in a single maximum increasing monotonically in radial distance with decreasing proton energy (Fig. 10). Theoretical understanding is slowly but steadily advancing as to the cause of the slot between the electron belts, and of the whole mechanism of initiation and loss of these Van Allen regions.

No one could possibly have planned or predicted the directions of study or the facilities which have proved so effective in this research, on the basis of the most exhaustive systems engineering plans of a few years ago. Rather, it was essential for the scientific options to arise separately, although in easy association with the systems programs for communications satellites, weather satellites, military satellites and so forth. Nevertheless, the two domains had to retain adequate independence as well as intimate correlation. This "neatest trick of the vicennium" is again concordant with the visions for the progress of science in the service of security possessed by the founders and sponsors of the Office of Naval Research some two decades ago.

Discovery is Continuing, as Needs Grow As to other specific examples, the exciting feature is the diversity of them which are now available. Instead of any sign of saturation or plateau in discovery of effects relevant or significant in the ONR sense to the furtherance of our military security, there is on the other hand a constantly

FIGURE 9.—Radiation content, in electrons/cm²/sec, of series of magnetic contour lines in space around the earth.

FIGURE 10.—Distribution of protons at energies from 40 to 80 Mev.

rising inventory of new knowledge. For the sake of particular emphasis and in a way not distant from our comments about contributions to nuclear physics and engineering, the recent studies by Matthias, Geballe and co-workers on the superconductivity of beta uranium may be noted. For some reasons, uranium is one of the most heavily studied of all elements, having been subjected to the most refined investigations of modern physics, chemistry, metallurgy and solid state science. Matthias and Geballe and associates, studying superconductivity, confirmed that pure uranium in the beta phase, a form stable between 661°C and 769°C , can be carried over to room temperature by the addition of 1.75 atom percent of chromium or platinum. The resulting solids were determined, in detail, to be conventional superconductors with a T_c of the platinum sta-

bilized phase of 0.88°K and that of the chromium stabilized phase, 0.77°K . They then re-examined the studies of the superconductivity of alpha uranium, which was claimed to have a transition temperature ranging from $.7^{\circ}\text{K}$ to somewhat above 1°K . Moreover, recently Dempsey, Gordon and Romer have failed to find anomaly in specific heat measurements in the appropriate temperature region and it now looks that the alleged superconductivity of the alpha phase uranium is caused by filaments of retained, stabilized alpha prime, beta and gamma phases or perhaps even of filaments of other compounds. The sensitivity of such a structure to mechanical working was demonstrated by Matthias and co-workers and they suggested there may also be a very fine-meshed network of beta stabilized filaments in alpha uranium phases. Now this indication has broad significance for many of the solid state properties of this interesting element, and yet a deeply specialized investigation of particular properties of uranium was necessary in order to reveal something about the phase composition which has been sought by other means for decades.

Yet another example is the extraordinary insights which are arising about electrical plasmas in solids. Ruthemann and Lang first observed plasma oscillations in metals, about 20 years ago, apparently resulting from high frequency density waves in the "gaseous" conduction electrons after excitement by an energetic electron beam. But it is now found that in the presence of a simple dc magnetic field, low frequency electromagnetic waves propagate in solid state plasmas. In ordinary metals with high electron densities, plasma frequencies are high, such as about 10^{16} radians per second. The quantum of plasma oscillation or plasmon has an energy H_{ω_P} of about 10 electron volts. This, of course, is so much greater than kT at room temperature that we do not see much of plasmons in metals until outside energy is put in, such as by a controlled electron beam. In semiconductors, electron densities lie in a range of 10^{14} to 10^{18} per cc as contrasted to about 10^{22} per cc in metals. Hence their plasma frequencies are in the microwave

or far infrared region as has been found by Kittel, Buchsbaum, Hebel, Boyle, Wolff and others. Obviously a great variety of plasmas can occur in solids, in contrast to the rather conventional gas plasma of electrons and ions in which both components are mobile. For instance, in an intrinsic semiconductor, where at ordinary temperatures electrons and holes are present in equal numbers, we can have there a two-component plasma (Fig. 11). In the metal potassium, however, there is a sea of conduction electrons with fixed K^+ ions, so there is a single mobile component (Fig. 12). Generally in a solid the behavior of conduction electrons is set, of course, by the periodic crystal field, and each solid may be somewhat different in this respect, so the range of electron dynamics for solid plasma is almost as great as the range of kinds of conducting solids themselves. The effective masses of these plasma particles also may vary from

WHEN $\omega \ll \omega_c$

FIGURE 11.—Mechanism determining plasma frequencies when $\omega \ll \omega_c$.

FIGURE 12. Response of single-component plasma.

.01 of the free electron mass upward. Thus, a wide range of new effects leading to the possibilities of amplification, signal processing, storage and indeed a deeper understanding of the very essence of metal behavior is promised by these new effects. They range all the way from the longitudinal waves driven by coulomb forces from electron beam bombardment, to propagation of transverse or partially transverse electromagnetic waves. This much more recent concept, whose first discussion was by Constantinov and Perell in 1960 and Aigrain, was confirmed by Buchsbaum and Galt in earlier cyclotron resonance measurements. When these are two component waves from electrons and holes they are called *Alfvén waves*, whereas when there is a single component plasma and the mobile carriers have a net charge, the magnetic line acquires both mass and charge density, and is denoted a *helicon wave*.

*New Options
Are Found
Steadily* Whatever the consequences of these remarkable findings may be, they will surely be significant for a basic understanding of metallic conduction and electromagnetic systems of the future. We see also a comparable diversity of new phenomena appearing, not in the old medium of metals, but in the old radiation of light. Since the discovery of coherent light by Schawlow and Townes, there have been many assessments of the impact of the new science on technology. Mostly, these examples simply accent that there is no static body of discovery to be evaluated, but rather that the forward motion of research, carried out independently of a specific laser system or application, always suffices to be doubling and redoubling the options for eventual systems application. This was shown strikingly in the past year by crucial results on scientific strategy of decisions made earlier. I am referring to the decision that gas lasers should be sought, and especially understood, for their power generating potential, even though the then current (1960 onward) systems estimates put such things as pulsed emission from solid state lasers very far ahead (Fig. 13). Accordingly, it is encouraging to find Dr. C. K. N. Patel's discovery of the carbon dioxide and other molecular gas lasers (Fig. 14). In combination with such additives as nitrogen and helium, the CO₂ lasers have emitted hundreds of watts of power continuously around 10.6 microns in wave length.

Specific developments of this system, such as by the Perkin Elmer Corporation, Raytheon and other independent industrial centers, are leading to sources of infrared radiation beyond any vision of a few years ago. Their significance for signaling, navigation, ranging, weaponry, reconnaissance and other uses remains to be developed. The potential, however, compares fully with all that we have achieved in electromagnetic radiation sources before, including the discoveries and exploitation of microwave radiation itself.

So we can salute the philosophy of research like that propagated by the Office of Naval Research, for the strong and en-

FIGURE 13.—Bell Telephone Laboratories gas lasers with their inventors, led by Dr. Ali Javan.

FIGURE 14.—Molecular-gas (CO_2) laser.

during part it has played in the security of our nation and of our lives. As is fitting, the Navy itself is also being modulated by this philosophy, and sees in such astonishing options as the new realms of quantum electronics, a future responsive to its exacting needs. Many of these lie, of course, in the decisive realm of command and control facilities, on which we've had little time to touch. Nevertheless, the same themes that we have tried to develop before apply ever more strongly there. Indeed, crucial and basic scientific judgments of the role of solid state components, of new signal processing techniques, and the impact of digital computing machines led in operational terms to the establishment of the DCA (Defense Communications Agency) through the offices of Special Assistant to the President for Science and Technology, at the end of President Eisenhower's administration and at the beginning of President Kennedy's. The next step was action on the findings and the recommendations of President Kennedy's Orrick Committee for the establishment of the National Communications System, available to all security forces as well as to other government agencies. Development of new circuit switching schemes for global networks, in contrast to the traditionally dedicated message switching networks of the separate military services, marks one more domain where combinations of basic scientific principle dictated what is proving to be an increasingly effective course of action.

With regard to the Navy's own concerns for electronics and command and control systems, the appreciation of what basic discovery can provide is indirectly reflected in the recent testimony of Assistant Secretary Morse, that one third of the 50 research programs carrying priority designation are in the electronics field. Thus, in a total research and development test and engineering budget for the next fiscal year of \$1.7 billion, the Navy has followed its own scientific precepts. It consistently allotted to systems such as the Integrated Light Attack Avionics system, the Omega navigation system, using atomic clock frequency control, and various submarine satellite navigation sys-

tems, a large share of the total budget resources; only recently are some of these efforts being seriously delayed.

So altogether we pay honor to our hosts, and especially indeed to the small and rare company it is, of gifted men and women, of teachers, researchers, scientists and engineers who have felt the meaning or even had the funding, of the Office of Naval Research. We refer also to the coordinate or comparable programs of the government and of the dwindling company of independent institutions and corporations. For it is from these few leaders, that have come the bases for critical systems judgments and concepts, critiques and decisions of the past two decades. And this was when our nation has for the first time in its history, and for one of the few times in world history, taken a lead nation role in seeking the security and stability of a Free World.

It is intriguing to note that in the distinguished Study of Basic Research in the Navy, conducted by the Naval Research Advisory Committee (many of whose eminent members or former members are present this afternoon, such as Dr. Seitz and Dr. Piore, who have already been heard from), the report hardly mentioned the word "system" in its conclusions and recommendations, published in June of 1959. Naturally the ideas of system analysis were familiar to the executors of the study, at Arthur D. Little Inc., and the report speaks of responsibilities of the Navy, such as oceanography and meteorology and marine phases of biology and biological sciences and the claustrophobic phases of psychology and the behavioral sciences, as special areas for research. We now know something of how to conduct systems developments in these fields and, indeed, in the less than ten years since the report was completed, we have industrial as well as federal resources which provide increasingly coherent technologies for these broad missions. But we have also learned in these decades very much more about what basic and applied science do in the evolution of complex systems. The NRAC report pointed out that of the 2% of college graduates who receive a doctorate in science, only

about one in five or .4 of 1% has the skill or motivation to stay in basic research, and an even smaller number actually continues with significant contributions. Now we are finding how to couple that minute fraction with the massive operation of developing the great systems for national defense, wherein hundreds and thousands of able technologists and engineers indeed create marvelous combinations of new materials and effects.

For all this we are grateful, but we also see an even more exacting future ahead. It is one in which the Navy and the Defense Department must deal with cosmic environments of outer space, of inner depths, of vast earth surface changes, and meteorological variations. The oceans must be treated as a total environment, not conveniently classified according to the Naval Bureau or the weapons system which has to function thereon. The capabilities of computing machines will let us begin these titanic tasks through modeling and simulation studies, through more perceptive data analysis and through the merciful compaction of display by graphics generated directly from the digital machines. Our global and even interplanetary sensing and signaling systems must be more elegantly adapted to the human channels through which they must ultimately pass. We are gaining in these sectors, but if Newton said that the vast sea of knowledge yet lay before him, we can conclude that that sea of knowledge is full of Naval science and we had better keep going, charting the voyage with ever deeper probes into the essence of things.

KEYNOTE ADDRESS
The Vicennial Banquet

Perspectives on Naval Research

by

THE HONORABLE PAUL H. NITZE

Secretary of the Navy

Research conducted by the Navy has had a profound effect on our way of life. We are living better electrically, electronically, medically and physically because of many commercial applications resulting from research sponsored by Navy. Our programs have taken us into space and to the other extreme—into the depths of the seas. We are catching up with science fiction exploits, generally attributed to the 25th century, and may even have surpassed them when we began communicating with porpoises in our advancing exploration of the undersea environment.

The brilliant array of speakers who have preceded me in your convocation has undoubtedly cast much light on the history of our research efforts. However, on this Twentieth Anniversary of the highly successful Office of Naval Research, we should examine what the organization has meant to the Navy and to the scientific community. A review of the historical origins of ONR will give insights into those aspects of the pattern which might persist for something like the next two decades, even in an era of dynamic technological change.

The activities of the Navy in shaping the relationship of the government with science have a long past and have played a significant role. Thus, a little over a hundred years ago, the National Academy of Sciences was created to serve as a link between science and the special needs of the government during the Civil War. Navy support was important in its formation and vital for the success of this endeavor. The Academy's first report, moreover, was on a Navy subject. It dealt with the in-

triguing and practical problems of calibrating compasses aboard ships equipped with iron smokestacks. Since then, the technical problems have become more complicated, and so have the relations between government and science.

World War I led to the formation of the National Research Council to enable the Academy more effectively to make the contributions which a considerably matured structure of science could offer to a new set of wartime needs. World War II, in turn, generated additional organizations, such as the National Defense Research Council and the Office of Scientific Research and Development as part of the Executive Branch of the Government itself. In the course of those years, the basic sciences and the universities were mobilized in strikingly effective ways to support the wartime objectives. Radar and the development of atomic energy are but two of the better known products. The methods by which these scientific resources were actually put to work were, for the most part, *ad hoc*, informal, not to say improvised, and were motivated by a common desire and urgency to get the job done. The closest of working relations, in addition to shared personnel between the National Research Council and the Office of Scientific Research and Development, assured a most effective employment of the resources of both.

By the end of the War, there were quite a number of responsible people who understood the essential national need of a continuing connection between the government and basic research. The problem was to translate this improvised emergency dependence into a peacetime relationship of equitable, continuing character which would be consistent with the institutional objectives and limitations of both interests.

The blueprint was provided by Vannevar Bush's report "Science, the Endless Frontier," of July 1945. This report contained the principal features of the pattern which was seen to emerge and whose validity has been fully endorsed by the passage of time. The fundamental premise was that only the systematic pursuit of scientific research on a broad front, with the

support of the Federal Government, would ensure the *choices* our nation would require to pursue an effective course of development along the lines which history had prescribed. The key word in this sentence is "choice." With his keen insight, Vannevar Bush drew this perspective correctly; but in the intervening twenty years many have lost sight of the fact that scientific research does not make technological development and does not produce the tools for solving national problems: all it does is offer choices for doing this. The solid state physics research for which the Office of Naval Research supplied impetus during the late 40's and early 50's offered us options, of which some, like the technology of producing amplifiers for hearing aids or the technology of building lightweight, very compact computers, were adopted with vigor, while others have been allowed to lie fallow. With equal clarity the report outlined the conditions under which universities would accept indispensable support by the Federal Government and traced the manner in which such support should be administered by governmental agencies.

The Bush Report most persuasively induced the Congress to make unprecedented appropriations for the support of research. The Office of Naval Research resulted from the first successful Congressional action implementing the Bush Report. Another five years had to elapse before it was possible to take the broader step of establishing the National Science Foundation for the general support of the basic sciences. Other steps followed, and we can unequivocally state that universities and agencies of the Federal Government have been successfully able to follow the guidelines which Vannevar Bush's report set out in 1945. Under them, we have developed administrative tools and institutional arrangements of great variety. We have developed understanding and support for research, not only within government but amongst the public, and the universities have developed a particularly effective new style of graduate education in the sciences which was made possible by the kinds of support that became available. As a consequence,

our universities have achieved undisputed world leadership in this field. In setting such patterns, ONR reduced the perspicacious ideals of the Bush Report to concrete practice. It worked out the detailed methods, trained the research administrators, and, in the process, supplied staff for agencies—such as the National Science Foundation, the Atomic Energy Commission and NASA—that were getting into the same business.

Concomitantly, there were changes also within the Department of Defense. Many of the Navy's laboratories were started during World War II. Among these were the Naval Electronics Laboratory in San Diego, the Naval Ordnance Laboratory in Corona, the Naval Ordnance Test Station at China Lake, and the Underwater Sound Laboratory in New London. They now had to be integrated permanently into the Navy's R&D establishment. Laboratories became more numerous also in the other services, and non-profit organizations were created to satisfy special needs. Along with the earlier parallel Research Offices in the Departments of the Army and the Air Force, there now appeared within the Office of the Secretary of Defense the Advanced Research Projects Agency, supporting research both inside and outside the Department of Defense. If the outlays for space and atomic energy are added to defense, a figure of ninety percent of the total R&D is reached. Such heavy Government investment clearly must be made for good cause: Our government does not spend nearly fifteen percent of its budget in the research and development business because science has a powerful lobby; in fact, scientists are still rather untutored at the game; nor because the United States is hungry for a better understanding of nature, or what lies on the other side of the moon; nor—finally—as a contingency investment against the risk that some other nation will surpass us. Each of these factors certainly plays a part, but basically the investment is made because our society has understood the dependence of its future on science and technology.

There are the evident short-term gains which are defined in terms of particular equipment and capabilities to meet present

needs, such as rockets and electronics to launch satellite communication systems, or ships and missiles which give force to our voice in the rivalry of nations. The long-term gains, on the other hand, have to do with the state of a technology, richly branching toward the future, with the vitality of industry and commerce, and with the assurance of the health and welfare of our entire population in the face of mounting problems, as well as opportunities. Science and technology have become essential pillars of our society and society is therefore willing to provide strong support.

Historically, the urgent need for new choices in defense technology has set the pace for Federal support of science; and scientists have applied themselves most successfully in translating basic work into solutions of practical military value to the government. More and more, however, attention is turning to other problems of society:—problems of management of the economy, problems related to the dissipation or conversely the exploitation of natural resources, problems of the social structure itself. The concentration of government support of research through defense agencies and defense-related fields can be expected to shift, but we cannot yet state clearly how far and in what manner.

I have described a constantly shifting situation in the relationship between government and the scientific community. It is a relationship that becomes increasingly complex, with more diverse channels of communication, more avenues of support, and more interested voices speaking with varying degrees of authority. Twenty years after its founding, the Office of Naval Research exists in an environment vastly different from the one in which it was created—an environment, however, which ONR has helped to create. Twenty years ago, ONR was the dominant voice of government in the support of basic research. Today it is not, and properly so, for the Navy should not be expected to shoulder the rapidly growing responsibility for all of science and technology. Research capability is a national resource, to be maintained, augmented, and put to use by all

agencies whose mission makes them dependent thereon.

There are those who would argue that, given the inevitable limitations upon human and economic resources, our research capability would be more efficiently utilized were it centralized in a single government agency. This would raise the question of whether the Navy continues to need an Office of Naval Research; of whether it is in the national interest that the Navy have such an Office. To this question, there can be, it seems to me, no answer except an emphatic yes. The reasons are many and basic, applying to other technology dependent branches of the government with as much force as they do to the Navy. In the end, these reasons all come to a focus in the continuing usefulness of the Navy. As an organization, it must maintain an R&D program for the sake of improved methods and equipment which perform in accordance with the requirements of the day, permit timely introduction and do not cost more than they should. As we have been told many times, only basic research can create the prerequisites for such improvements. In passing, I would note that the exercise of actually attempting to trace such parentage is often more academic than fruitful, for the trace quickly becomes dim and no rational sequence seems to prevail. This is inevitably the nature of creative efforts, and we should accept it, for in the end we seek ideas, basic answers and basic data for which—once we have them—applications are seen. Yet data by themselves are sterile; it is the ephemeral idea that makes them useful. We must, therefore, remain in a position to influence and stimulate thinking in the scientific community along lines of ultimate Navy relevance. We must have our own contacts with that community, as must—needless to say—other branches of the government.

Our own Navy laboratories provide one family of such contacts. Their strength in recognizing new ideas of value to the Navy lies in that their scientific endeavor is automatically weighted toward areas of Navy interest by their environment. Obviously, there will be other new ideas suggested more by opportunity than by need, and we must certainly be able to

take them wherever we can find them. Hence, we must be in contact with the body of scientists generally, both in this country and abroad. As Sir Solly Zuckerman pointed out so ably this afternoon, the ideas of science transcend political boundaries. Yet our need to reach the profession does not explain by itself why we must have an ONR. For a fuller understanding, we must return again to the ideas set out in Vannevar Bush's report.

It is fundamental to the approach of basic research that scientific truth must be pursued on its own terms, governed by internal standards of relevance, and free from pressures of external purpose. The lifeblood of good science requires respect for and faith in this internal integrity of science. Basic research cannot directly confront social, or political, or military purposes. This is not a weakness, but a quintessential characteristic which gives research its strength. If, therefore, the government is to strike a bargain with research, if it is to support it in the fashion that ensures best performance, there is need for a special buffer organ which sees to it that the interests of both parties are advanced. This buffer is the government organization which functions as a "middle-man" between the scientific, generally discipline oriented communities and the operating government agency with its mission purpose. ONR functions in this manner for the Navy. As such, its success in the deepest sense has been the result of the *creative* role which it has played between the researcher in the private community and the long-term needs of the Navy. ONR has deliberately not compromised the motives of the researchers in academic establishments or non-academic laboratories; at the same time, however, it has sought to shape an overall program of research which would assure contributions to the solution of long-term Navy problems. Moreover, as new technologies have opened up, ONR has actively promoted the Navy's commitment to them so that the line organizations with development responsibility might begin to learn something about them for later practical exploitation. Still, it will happen occasionally that some elements of the

Navy, primarily exposed to the pressures of immediate requirements, entertain doubts about what ONR is doing. For related reasons, the academic researcher occasionally forgets that the Office of Naval Research is a part of the Navy. Both attitudes are consequences of the special circumstances with which any research-supporting organization must learn to cope if it is to be an actively contributing intermediary between research and practical need.

The relationship of government institutions like ONR with the universities, and, in general, with the scientific community, was aptly described by John W. Gardner, now Secretary of the Department of Health, Education, and Welfare, in a 1964 report to the Agency for International Development on the subject "A.I.D. and the Universities." He said: "Such collaboration with non-governmental groups brings to bear on a national problem the full range of talent and institutional resources of our pluralistic society. But if collaboration is to be successful, three cautions are in order: (1) The Federal agency involved must have a nucleus of first-class people capable of dealing with outside individuals and institutions on terms of professional equality. The notion that a Federal agency can let its direct-hire staff deteriorate and get all of its talent on contract is a dangerous delusion. (2) The relationship between government and the university must be defined in such a way as to preserve to each party independence of action in those functions that it must perform unimpeded. (3) The relationship must be such that each party not only can perform at its best, but can gain added strength from its participation. Only under such circumstances will the government be able to justify its participation and the universities be able to put their best talent and resources at the disposal of the government."

Most recently, the Material Establishment of the Navy has started to undergo a far-reaching reorganization to adapt it more effectively to current and foreseeable needs in coping with the engineering and management complexities that have followed in the wake of modern science. At the same time, the

operational commitments of the Navy are dictated to an ever larger degree by complex and shifting patterns of interdependence, a fact which widens the spectrum of requirements and calls for tools of increasing power in their identification. The accelerated advance and diversification of science, finally, brings with it the potential of a greatly increased range of options for meeting such future requirements. If there is one thing, above all others, for which the Navy today needs the mentality that ONR has fostered and which it represents on the Navy team, then it is for the purpose of helping to make increasingly cogent choices in this increasingly demanding environment.