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Basic Research Policy

OF THE

Department of Defense

Report of the

Defense Science Board Task Force

20 February 1968

OFFICE OF THE DIRECTOR OF DEFENSE RESEARCH AND ENGINEERING
Washington D.C., 20301

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BASIC RESEARCH POLICY OF THE DEPARTMENT OF DEFENSE

Report of the

Defense Science Board Task Force

20 February 1968

Office of the Director of Defense Research and Engineering
Washington, D. C. 20301

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OFFICE OF THE DIRECTOR OF DEFENSE RESEARCH AND ENGINEERING
WASHINGTON, D. C. 20301

20 February 1968

TO: THE SECRETARY OF DEFENSE

THROUGH: THE DIRECTOR OF DEFENSE RESEARCH AND ENGINEERING

The Defense Science Board has approved the enclosed report for your consideration. It is the work of our Task Force on Basic Research Policy, appointed early last year at the request of Dr. Foster. After meeting with senior research administrators throughout the Federal Government, the task force has arrived at the recommendations presented herein.

The task force emphasizes the needs of a sound, high-quality DoD research program and recommends measures to improve the present program. These measures would require a strong, sustained effort to put them into effect. We recommend that this effort be made.

On behalf of the Defense Science Board and this task force of the Board, I wish to express our appreciation to Dr. Foster and members of his staff for their interest and assistance in the conduct of this study.

F. Seitz

Frederick Seitz
Chairman
Defense Science Board

OFFICE OF THE DIRECTOR OF DEFENSE RESEARCH AND ENGINEERING
WASHINGTON, D. C. 20301

31 January 1968

MEMORANDUM FOR CHAIRMAN, DEFENSE SCIENCE BOARD

SUBJECT: Report of Task Force on Basic Research Policy

The Task Force of the Defense Science Board, appointed at the request of the Director of Defense Research and Engineering, has carefully studied the present posture of the Defense Department toward basic research and has looked particularly at questions of relevance, coupling, quality and funding. We are pleased to submit the attached report, which represents the consensus of our findings modified by suggestions that members of the Board have graciously tendered. We appreciate the suggestions we have received from the Board members and from the many officials of the Federal Government who met and talked with them.

We particularly commend the following recommendations:

(1) That the DoD maintain and continue its support of basic research in a substantial way.

(4) That it is desirable to press research in fields which have not received as much attention as others.

(6) That there be increased funding of approximately 10 percent for each of the next 5 years.

(9) That the military departments and the Defense agencies develop and maintain a current list of projects where fundamental research could be useful to the Department.

(11) That several specific steps be taken to encourage capable young scientists and engineers to attack projects of interest to the DoD.

We feel that all members of the Task Force can accept the 14 recommendations as a consensus of our findings.

We wish to thank particularly Dr. Robert E. Uhrig who served so ably as our executive officer. This report could not have been prepared had we not received splendid cooperation from the Assistant Secretaries and Deputy Chiefs for Research and Development in the Military Departments. We also appreciate the cooperation of the senior administrators of the other government agencies with active programs in basic research.

We ask that you review this report carefully and that, if it is agreeable to you and the Board, it be forwarded to Dr. Foster.

If the Task Force can supply any additional information, we will be pleased to develop it for you.

Cordially yours,

A. D. Suttle, Jr.
Chairman
Task Force on Basic Research Policy

CONTENTS

RECOMMENDATIONS

(1) *We recommend that the DoD establish as policy the vigorous support of a broad program of basic research that carries a proportionate share of the nation's needs for basic research and embraces especially new or rapidly changing topics.*

The relevance of research to the DoD's needs must be broadly interpreted, and the level of effort must be commensurate with the effective use of qualified manpower and support by other groups, both public and private. An annual increase in constant dollars of 8 to 10 percent each year for 5 years appears reasonable.

(2) *We recommend the support of the best, strongest and most timely basic research programs without regard to type of performing organization.*

Specifically, we recommend departing from the current practice of securing more funds for one element at the expense of others when funding is constant or increasing and of protecting certain programs for reasons other than their merit or military necessity when a reduction in support is required.

The Director of Defense Research and Engineering should take steps to ensure that scientific merit and military necessity are the governing criteria in allocating research funds. In selecting recipients of grants or contracts, however, appropriate consideration must be given to the DoD's continuing need for professionally trained personnel, both in-house and on the part of its contractors. Accordingly, the support of research important to Defense goals at universities of demonstrated technical competence is doubly justified because, in addition to achieving its technical objective, the DoD would contribute—at a suitable level and in a manner beneficial to all parties—to the development of the essential pool of trained people.

(3) *We recommend that the quality, competence and potential of investigators and their institutions be the governing criteria in awarding research grants and contracts.*

We feel that it is unsound to reduce the support of quality programs in order to initiate new work.

We further recommend that Project THEMIS be considered a separate, identifiable program for budgetary purposes, and that it not be allowed to grow at the expense of other university programs.

(4) *We recommend that basic research in areas that have not been emphasized in the past, such as social and behavioral sciences, biological science and chemistry, be reevaluated and their relative importance in providing the fundamental knowledge needed to solve DoD problems be reassessed.*

If appropriate, new or increased support should be provided.

(5) *We recommend that, in the support of research, a balance be retained between programmatic research and individual projects.*

We observe that the greatest return and the most efficient research come from relatively small grants to individuals for 2- to 5-year studies that are of vital interest to the investigators and are also relevant to the Defense mission.

(6) *We recommend that research (6.1) funds for each of the next 3 to 5 years be increased by 8 to 12 percent in current dollars.*

It is gratifying that the Director of Defense Research and Engineering has made such a recommendation for Fiscal Year 1969. Moreover, we feel that support from emergency funds during the current year can be justified—and, indeed, is indicated. This recommendation is made with full knowledge of current and predicted budget restrictions.

(7) *We recommend that the Director of Defense Research and Engineering establish and conduct a technical audit program for in-house laboratory programs similar to the one relating to industrial and academic research.*

Problems of the in-house laboratories have been repeatedly investigated by many groups in the ODDR&E; the seriousness of these problems is evidenced by the fact that these institutions are continually reevaluated and restudied. The difficulty experienced in implementing changes shows a need for stronger management. We would be remiss if we did not also recommend that the reorganization of in-house laboratory management be considered.

(8) *We recommend that the Director of Defense Research and Engineering establish as DoD policy that program managers and supporting personnel in the military departments be assigned the responsibility for coupling the work of research scientists with Defense needs.*

Each program manager must be given facilities and time to acquaint himself with both the state of the art in his area of cognizance and the requirements of his department. It would be unwise as well as unrealistic to place the responsibility for coupling on scientists and engineers who are conducting research; but they should be encouraged to learn about the DoD's needs in their respective research fields.

(9) *We recommend that the DoD issue detailed, up-to-date descriptions of apparent military needs for new scientific knowledge as a guide to scientists interested in submitting research proposals.*

An example of such a description is *Military Themes of High Scientific Merit for Oriented Basic Research*, issued by the Army Research Office in April 1966.

(10) *We recommend that the Director of Defense Research and Engineering designate his Deputy for Research and Technology to conduct an aggressive program directed toward improving the DoD's relations with the scientific community and professional societies.*

It is evident that these relations need improving. We suggest that learned societies be invited to conduct symposia on topics of mutual interest at Defense installations. We recommend that personnel of the DoD and its contractors be encouraged to participate (within the bounds of security) in the activities of scientific organizations, especially by publishing papers and attending scientific meetings. We are impressed by the good rapport between the scientific community and the National Institutes of Health and also the Atomic Energy Commission. It is important to prevent the pressure of administrative duties from "decoupling" the scientists and engineers in the ODDR&E from their professional affiliations.

(11) *We recommend that a particular effort be made to attract extremely competent, imaginative young people to basic research relating to Defense problems.*

Specifically, we recommend that 15 to 25 percent of THEMIS funds be reprogramed appropriately to support these individuals' projects; the funds should be assigned on a project basis to investigators and should not be incorporated in institutional grants.

As a means of attracting young scientists and engineers, the following activities should be considered:

- Special seminars on specific topics, lasting from 1 day to 2 weeks;
- Special fellowship assignments in R&D laboratory and administrative units (analagous to Presidential or congressional fellowships);
- Summer employment at in-house Defense laboratories and at laboratories of DoD contractors;
- Employment on a "no-loss" basis for a significant period—6 months to 2 years—by a military department, an in-house laboratory or another DoD agency involved in research.

(12) *We recommend that the Director of Defense Research and Engineering inaugurate a program specifically to use graduates of the Defense Science Seminar in an organized, productive way in the interest of the Department of Defense.*

We further recommend that a special effort be made to place at least one of them, as a junior member, on each DSB task force.

(13) *We recommend that the research programs of the three military departments and the Defense agencies be more fully coordinated at the program level.*

While duplication of research in itself is not necessarily undesirable, there should be a logical reason for undertaking deliberately duplicative work.

(14) *We recommend that the Director of Defense Research and Engineering carefully review the management and planning of DoD programs in basic research and critically consider—in addition to strengthening and more strongly supporting the OXRs—establishing a central basic research group that reports directly to someone in his office.*

This central group could be established as a major branch of the Advanced Research Projects Agency, or it could be developed as an independent agency, such as the Defense Communications Agency or the Defense Intelligence Agency. Not only could it serve as a responsive unit to pursue work in interesting new fields, but it could also assume some responsibility for conducting basic studies of common interest to the military departments. It is suggested that a senior scientist highly regarded in his profession be chosen as the director of the program.

INTRODUCTION

The Defense Science Board Task Force on Basic Research Policy was established to study the most effective way the Department of Defense (DoD) can use its basic research resources. In his memorandum of 15 February 1967 (Appendix A), the Director of Defense Research and Engineering asked that the Task Force examine:

(1) the *relevance* of DoD basic research to long-term Defense goals;

(2) the *coupling* between DoD research organizations and the scientific community, emphasizing the coupling among DoD groups;

(3) methods to measure and improve the *quality* of the work; and

(4) the general rationale for the amount, balance and distribution of *funding*.

In eight one-day sessions, the Task Force has investigated the present status of basic research policy in the Department of Defense, and has conferred with the following officials:

- Honorable Robert A. Frosch, Assistant Secretary of the Navy (Research and Development)
- Honorable Alexander H. Flax, Assistant Secretary of the Air Force (Research and Development)
- Dr. Charles L. Poor, Deputy Assistant Secretary of the Army (Research and Development)
- Lt. General A. W. Betts, Chief of Research and Development, Department of the Army
- Dr. J. T. Thomas, Deputy for Research and Laboratories, Army Materiel Command
- Rear Admiral Thomas B. Owen, Chief of Naval Research
- Mr. Edward M. Glass, Assistant Director (Laboratory Management), Office of the Director of Defense Research and Engineering (ODDR&E)
- Dr. James Shannon, Director, National Institutes of Health
- Dr. Leland Haworth, Director, National Science Foundation
- Dr. Randal Robertson, Associate Director for Research, National Science Foundation
- Dr. Robert C. Seamans, Jr., Deputy Administrator, National Aeronautics and Space Administration (NASA)
- Dr. Gerald Tape, Commissioner, Atomic Energy Commission (AEC)
- Dr. Spoffard English, Assistant General Manager for Research and Development, Atomic Energy Commission

In addition, the Chairman and the Executive Officer of the Task Force have visited with the President's Science Advisor, Dr. Donald F. Hornig. The Chairman also has talked with Dr. C. H. Townes, Dr. Edward Teller, Dr. Henry Eyring and Dr. Paul Weiss, incorporating many of their valuable suggestions into this report.

In the course of our meetings and these conferences, we developed a rather deep appreciation of the special problems the various organizations have in dealing with their research and the relationship of their programs with the DoD's research program. There is unanimous agreement that the Department of Defense must remain active in conducting basic research, for the specialized research needs of the DoD cannot be filled by other organizations. The Defense agencies and the military departments must pursue basic research and, in the process, maintain close liaison with outstanding scholars who are doing basic research in fields that are important to the Department of Defense.

Both Dr. Hornig and Dr. Haworth feel strongly that the DoD must remain active in basic research—that a strong research program is not only justifiable but essential to the well-being of the Defense establishment and is consistent with the orderly development of research in the United States. This is consistent with the recommendation of an earlier Defense Science Board Subcommittee on DoD Research Policy[1]. Dr. Haworth feels that the basic research supported by the DoD should increase at the rate of 12 to 15 percent per year for the next decade, at least in the universities; even with such an increase, rising costs will absorb a substantial fraction of this growth. He pointed out that there is no assurance that the National Science Foundation will grow fast enough that the DoD could significantly reduce its commitment to basic research without harming the total national effort.

On 14 July 1967, after this Task Force had been established, the Panel on Research and Exploratory Development, of the Defense Science Board—National Academy of Sciences Berkshire Summer Study, issued its report. It addresses such questions as the impact of limited DoD research and development funds, the determination of optimum basic research (6.1) and exploratory development (6.2) funding levels and interactions with the Congress.

Generally, in this report we avoided repeating the Summer Study Panel's conclusions, but some cogent remarks on research continuity and funding are included. We agree with, and enthusiastically support, the findings expressed in the Panel's report.

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[1]*Report of the Defense Science Board Subcommittee on Department of Defense Research Policy*, Part I, "Policy on Support of Basic Research" (Washington, D. C.: Office of the Director of Defense Research and Engineering, 31 December 1963).

1. RELEVANCE

Since the mission of the DoD is defense and not science, the primary justification for the Department's involvement in research is the undeniable importance of such activities to the nation's security. Furthermore, while the DoD can readily justify research in those areas that are relevant to its mission, a broad effort is warranted by that mission's diversity.

The matter of relevance is very elusive. In 1935, who would have predicted that nuclear physics would soon be relevant to the needs of the Department of Defense and that, 10 years later, the applications of nuclear physics would materially affect many aspects of strategy? The real problem is not to establish the relevance of research, since it is possible to show it clearly in numerous (if not all) cases, but rather to show that a given field or a given project is *not* relevant.

There is little disagreement or misunderstanding about research and its relevance to a particular system or mission. Problems arise only when "secular" considerations are introduced, presenting this key question: What work that we do now is relevant to the rate-limiting steps in the future to yet uninvented systems or unconceived missions? Stated this way, the problem becomes more difficult. There are, however, two approaches that can be offered as axioms:

(1) *If it's worth doing at all, it's worth doing better.* Thus, research projects associated with materials for higher speed flight, information-storage elements for higher density computer memory, and techniques for more effective use of human ability are assuredly "relevant" to DoD needs.

(2) *If there's a lot of room between the state of the art and fundamental limitations, there is ample room for improvement.* Thus, since even the most spectacular microminiaturization of computer storage elements now results in a volume per bit stored that is millions of times greater than the fundamental limit, we can be sure that research will bring forth much smaller storage elements.

A common difficulty is to confuse the relevance of research with its immediate applicability. Let us look at two examples that show how literally we must construe research relevance to serve the national interest. First, consider the tree of science whose fruit was the laser. Its roots are:

- the investigation of the electron,
- the study of vacuum technique,
- the development of quantum theory, and
- investigations of other fundamental, "pure," but nontheless prodigious subjects.

Molecular-beam experiments such as that of Stern-Gerlach represented a vital step. Later, the development and support of a molecular-beam and microwave spectroscopy laboratory at Columbia University were additional key elements. The maser itself, invented in that laboratory, was at one time a contender as an excellent low-noise amplifier, but it was later surpassed by the development of cooled parametric amplifiers.

At this stage, a critical study of relevance would have labeled the maser and all its intellectual ancestors as irrelevant. But, as Professor Nicolaas Blombergen points out, the maser also introduced the concept of pumping; soon the laser emerged from a different application of pumping, and doubtless additional devices will appear later. Again, all this research appears to be relevant to Defense needs.

The second example of research that we must consider relevant is high-purity crystals. The development of pure materials seemed rather remote from pressing reality in the austere period before World War II. Then the spectacular improvement of noise figure in microwave radar receivers, achieved by purifying and controlling the composition of silicon and germanium used in diode mixers, focused attention on the importance of purity. It is now accepted that the availability of purer germanium led to the discovery of the transistor in 1948.

During the next dozen years, improved methods of manufacturing transistors and other solid-state devices and the development of epitaxial growth and thin-film devices made it seem that further work in high purity was irrelevant. Within the last few months, however, the sensational reports concerning the LSA solid-state microwave oscillator (progeny of the Gunn effect) have justified renewed attention to purity, since variations in "doping" of even a few parts in 10 million substantially reduce efficiency.

To have taken a narrow, short-sighted view of relevance at crucial times in either of these examples would have seriously hampered—maybe even prevented—their development. In neither case was it clear at an early stage how the results of the research would be eventually used by the DoD. Anyone calling the Stern-Gerlach experiment relevant at the time would have been ridiculed. On the other hand, many scientists and engineers understood the potentiality of electronic processes in gases or solids with respect to control devices. It should have been clear that developments would be highly useful.

The whole problem of the relevance of basic research has been faced by industry in the course of selecting and supporting its long-range research programs. In some competitive industries, the conduct of high-quality basic research judged by a research director to be relevant to his company's interests is regarded as essential to the survival, health and growth of that company. Whether one studies industrial or military research, the application of relevance as a guide for research support entails a high degree not only of practical, scientific sophistication, but also of imagination and intuition, because the results of good scientific studies are frequently unpredictable. One must avoid the pitfall of following too narrow paths and forcing scientists to seek only obvious, short-range objectives.

The primary danger we see is the tendency to devote exclusive or excessive attention to projects that will result in almost certain, but small improvements, at the same time ignoring or neglecting work in other, sometimes remotely related fields whose results could be either improvements by several orders of magnitude or new technology that would render all the "directly applicable" work obsolete.

Specific statements may be made about broad scientific areas that clearly relate to the technology of weapon systems. Without running too much risk of scientific myopia, a detailed description of military needs for new scientific knowledge can be produced. An example of this is the brochure, *Military Themes of High Scientific Merit for Oriented Basic Research*, issued by the Army Research Office in April 1966. A forerunner was a report of the Panel on General Sciences[2] 10 years earlier which addressed the needs of all three services. Such DoD-wide publications should be issued on a regular basis, say, annually.

Several of our distinguished visitors emphasized that the primary responsibility for determining relevance should rest with the program manager in the Department of Defense. Dr. Hornig was most disturbed by some indications of recent attempts in the DoD to place the burden of proving relevance on the investigators. Dr. Tape stressed that, in the AEC's evaluation of a program's relevance, both short- and long-term aspects are carefully considered.

We strongly recommend that the relevance of a basic research program to the DoD's mission must be evaluated by the program monitor. The individual research scientist, whether at a university, an industrial or an in-house laboratory, judges the relevance of a particular activity to his goal—the efficient accomplishment of a certain research task. He should be expected to keep in mind the matter of overall relevance to the DoD's requirements, but the ultimate responsibility for making a relevance judgment must lie with the DoD monitor who selects, evaluates and compares this research program with other complementary or coupling programs.

In the THEMIS and Joint Services Electronics Programs, much of the responsibility for maintaining relevance has been delegated to the on-campus project manager, but relevance can be assured in a general way by the review committee or DoD monitor who is responsible for the program.

The DoD has a continuing responsibility to see that relative priorities within various research fields are retained. Research in certain fields is clearly the responsibility of other agencies. For instance,

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[2]In the Office of the Assistant Secretary of Defense (Research and Development), the predecessor organization of the ODDR&E.

research in many medical fields is the responsibility of the National Institutes of Health, and the Atomic Energy Commission is primarily responsible for research in many areas of nuclear science and technology, e.g., high-energy physics. In those cases, the DoD research manager's primary responsibility requires him to be well acquainted with the entire program so that he may initiate work in peculiar areas of special interest and importance to the DoD. (This is discussed extensively later in this report.) In other areas where only the DoD has great interest, Defense must assume the primary—possibly the complete—responsibility for the necessary research effort.

The matter of assigning priorities between fields is one of the most difficult tasks faced by any mission-oriented sponsor, but it is crucial to the accomplishment of the mission. A reasonable, rational and (it is hoped) experienced approach to this problem, even though it may be less than perfect, is essential. In a sense, the requirement for relevance tends to hamper and often defeat basic research.

For example, suppose a basic investigator studying the theory of information systems conceives a unique new idea and requests support from one of the OXRs[3]. If the OXR manager is interested, he must justify this project through six to ten administrative levels before it is finally approved. As the proposal advances through this review process, the justification tends to become more and more applied. Usually it has to compete with some strongly mission-oriented programs that appear responsive to the immediate needs of those who make the final reviews. Thus, what started out as a basic research project may be deformed into a highly organized effort that eliminates much of the potential envisioned by the original investigator. More often it is ranked below projects of a more applied nature and, consequently, left unfunded.

This sequence of events points up the need to separate the DoD's basic research program from development work. It is clear that many of the needs of individual services for basic research are substantially the same; the difference comes in the application of results. Very often it is tradition, rather than some proprietary need, that determines which military service supports a particular area of basic research.

One of the dangers this country faces is the possibility that a potential enemy will make a scientific breakthrough and be able to carry the work to an operational stage before we know about it. Our development of the atomic bomb during World War II is an example of a technological discovery that was translated into an operational weapon before being disclosed to the enemy. The Japanese got their only knowledge of the weapons used against them from information supplied by U.S. news releases. They were thus left with the conventional and relatively useless defense of trying to shoot down the planes or evacuating their cities.

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[3]In this report, the departmental offices of scientific research, e.g., Office of Naval Research, are referred to collectively as the "OXRs."

Certain basic research fields that are of interest to all the military services should be emphasized through a centrally operated DoD research program. The program might be initiated within the ODDR&E, or it could be an expansion of the present work carried on by the Advanced Research Projects Agency (ARPA). It might, for example, include materials science, information processing, and social and behavioral sciences —all now part of ARPA's program—as well as fundamental chemistry, earth sciences, basic mathematics, and atomic and molecular physics.

We urge that serious consideration be given to establishing a centrally operated basic research office under the auspices of the ODDR&E. The Defense Atomic Support Agency, the Defense Intelligence Agency and the Defense Communications Agency are organizations that should be studied as possible models.

The new office would be responsible for maintaining a well-balanced program of relevant basic research, properly weighted to Defense needs. It would address *only research*, leaving development to the several military departments. Major emphasis should be placed on areas that are important to the Defense mission or for which other agencies—National Science Foundation, National Institutes of Health, NASA, etc.—are not now responsible. Some work, however, should be done in all relevant fields to enable the DoD to evaluate the state of the art.

To facilitate management and ensure complete coverage, a substantial fraction (perhaps half) of this work should be programmatic in nature, e.g., Project THEMIS, ARPA's interdisciplinary laboratories, and the Joint Services Electronics Program; but individual research projects should be strengthened also.

A central DoD research laboratory for basic research, analogous to the Bell Telephone Laboratories, should be considered. Such a facility could serve to develop young scientists and engineers in the research specialties of relevant fields and eventually could provide high-quality staff for other in-house laboratories. This laboratory should be closely associated with nearby universities and should offer research opportunities to their students. An existing facility, such as the Naval Research Laboratory or the Air Force Cambridge Research Laboratories, might be designated as the DoD research laboratory under the auspices of the central DoD research agency.

The relationship between Project THEMIS and the portion of the current DoD research program that is carried out by universities should be subject to continuing scrutiny. While we support the concept of Project THEMIS to develop new academic centers of research interested in solving Defense problems, we do not believe that it should be conducted at the expense of the existing university research program, which has served the DoD effectively. For many years, the Department of Defense has supported a number of excellent academic research groups that have been extraordinarily productive and have made important contributions to our nation's security. Those centers are, in mose cases, models of fine graduate education and superior research; they benefit students and faculty and contribute to scientific progress and the satisfaction of Defense needs. These groups must be continued to provide the range of scientific talent necessary to attain Defense objectives. Should this national resource be allowed to erode, our country's security a decade from now could be in serious jeopardy.

We recommend that Project THEMIS be considered a separate, identifiable program for budgetary purposes. Probably several years will pass before the programs initiated under THEMIS will reach the quality or status of research performed at recognized centers of excellence. Every effort must be made to see that THEMIS funds are used as effectively as possible in attaining research excellence. Low-quality programs must be phased out regardless of geographic or political considerations. The universities must not come to regard THEMIS as a permanent source of funds or as an institutional development program.

THEMIS must be clearly identified as a program directed toward meeting the research needs of the Department of Defense. It must be emphasized that the purpose of Project THEMIS is to provide an opportunity for small groups of talented investigators at developing institutions to work on basic unclassified Defense research. The DoD should not make a long-term commitment of funds under THEMIS or regard this new program as different from any other.

Finally, on the subject of relevance, it should be pointed out that basic scientific research definitely *is* relevant to the mission of the nation's universities. Federal support of graduate research does not detract from the proper functions of universities and is, in fact, the major source of support for graduate scientific and engineering education in the United States today. As the largest employer of technical people in the nation, the DoD necessarily has a strong interest in the vitality and quality of scientific research and graduate education in universities.

2. COUPLING

For purposes of this report, coupling is defined as the establishment of effective two-way communication channels on a professional level between people doing basic research and people working on developmental and operational problems associated with the Defense mission. Coupling can be achieved in many ways, and it makes little difference what mechanism is used as long as it leads to good communications.

There are two promising approaches to coupling fundamental science and technology to the application of findings. We shall call them low-entropy and high-entropy coupling—"entropy" being used here in the sense of a measure of efficiency in transmitting information. Coupling in science and invention, as part of the creative process, is subject to fluctuations resulting from the successes and failures that characterize all such activity.

Low-entropy coupling is orderly and carefully planned. It is the desired process—even in some instances when it unintentionally becomes high-entropy coupling. The large, successful industrial laboratories are excellent examples. Incentives of salary, responsibility and promotion are designed to stimulate the scientist's attention to applied problems and the applications engineer's attention to science and fundamental technology. Colloquia, conferences, and task forces are commonly used techniques. A very important element is the orderly, strategic transfer of "pure" scientists or research engineers to work elsewhere in the company on problem-oriented projects. Also, as they grow older, many scientists move from pure science to more applied activities.

The DoD has serious problems with this kind of coupling. The basic work may be done by a contractor at a university, whereas the problems arise in the field or in the fleet. The OXR program manager and the in-house Defense laboratory director are responsible for coupling, but there are substantial limitations in both routes. The OXR manager has many other things to do; the worker in an in-house or university laboratory is frequently much more interested in his own science or technology and is rarely transferred.

High-entropy coupling is casual, even accidental. Unplanned but stimulating contacts of people from the different "worlds" of basic work, problems or needs are characteristic of this route. Mechanisms are the close association of scientists and engineers in other than professional activities in which conversation is encouraged (car pools, lunchrooms, etc.); browsing in libraries; and, especially, the transfer of people from one "world" to another, even between different parts of an organization.

In this kind of coupling, a major difficulty of the DoD is the isolation of contractors, OXRs and laboratories. Other problems relate to the laboratories' lack of amenities, restrictions on travel, and failure to establish a tradition that the returning traveler spreads the word on his experiences and contacts.

Coupling is not just one or the other of these two; it often comprises both. For example, in the enlightening Tanenbaum report[4], Tanenbaum's own case history, though clearly in the low-entropy category, reveals much high-entropy coupling. In fact, a case could be made from this example (high-field superconductors) that (1) contacts between individuals are the key events; (2) these contacts are either productive or neutral, and only very rarely counterproductive; and, therefore, (3) the essence of coupling is to maximize the frequency of contacts.

In the pursuit of excellence—Project THEMIS, for example—every effort should be made to bring out the research potential of young, highly talented but relatively unknown scientists and engineers. The development of new talent within a new university setting is an integral and essential objective. Thus, consideration should be given to reprogramming a significant fraction (perhaps 15 to 25 percent) of THEMIS funds to individual scientists and engineers who show a high potential. As a group, young scientists and engineers face great difficulties in getting support for their own research programs. Project THEMIS can offer a convenient palliative for this situation and, at the same time, cultivate a potentially life-long friendly association between the DoD and the capable scientists. Many key advisers to the DoD today began their careers under Defense-supported projects 10, 15 or 20 years ago.

The involvement of young scientists and engineers in the research and development activities of the Department of Defense is considered absolutely essential to its mission. The Defense Science Seminars held in the summers of 1964 through 1966 by Dr. William McMillan, of the University of California at Los Angeles, were highly successful. But other mechanisms that will bring into DoD activities a substantial number of bright young scientists and engineers, particularly those from universities, should be initiated. There are various ways of doing this, of which some are already being exploited on a small scale as follows:

- Special seminars on timely topics, lasting from 1 day to 2 weeks;

- Fellowship assignments (analogous to Presidential or congressional fellowships) in R&D laboratory and administrative units;

- Summer employment by in-house and defense-oriented laboratories; and

[4]*Report of the Ad Hoc Committee on Principles of Research - Engineering Interaction* (Washington, D.C.: Materials Advisory Board, National Academy of Sciences, "Tanenbaum Report," MAB-222-M, 1966).

. Employment on a "no-loss" basis for a significant period of time (6 months to 2 years) by the DoD—one of the military departments or an in-house laboratory. Streamlined appointment procedures for temporary employees (less than 2 years) are urgently needed.

It is highly recommended that the Department of Defense, at an early date, initiate new programs or expand present ones in all these areas.

Experimentation in coupling under the special conditions of DoD operation is clearly in order. Some experiments are already under way, e.g., the materials programs of ARPA; but there are many other possibilities. For example, one might try—

. colocating some OXRs at in-house DoD laboratories;

. increasing travel by professional staff members of in-house laboratories, as more funds become available;

. transferring scientists to applied programs, and—on the other side of the coin—

. granting sabbaticals to people in engineering departments and industrial laboratories;

. forming task groups in technological areas of interest, but not the same kind of task group that is assigned to work on a particular system or device;

. establishing incentives, awards to individuals and to whole departments when they achieve successful coupling;

. creating incentives to the worker in basic research to familiarize himself with more concrete problems; and

. developing the McMillan summer school, etc.

The Army attempts coupling in its in-house laboratories by integrating research with developmental activities, not isolating it.

In the Navy, most of the in-house basic research is done at the Naval Research Laboratory, while development activities are concentrated elsewhere. The Director of Navy Laboratories has taken steps to foster coupling between the in-house laboratories and operating components by such means as—

. including in laboratories' mission statements the responsibility for introducing new systems to the fleet and providing subsequent engineering support;

. arranging for rotational exchange of personnel between laboratories and operations analysis activities; and

. assigning laboratory representatives to sea.

The main OXR offices of the military departments—Army Research Office-Durham (ARO-D), Office of Naval Research (ONR), and Air Force Office of Scientific Research (AFOSR)—now play important roles in coupling. The report, *AFOSR Coupling Activities, 1966*, describes various ways in which this agency attempted coupling during 1966; for example:

. Several principal investigators of AFOSR-sponsored projects were closely associated with DoD technical development groups working in the investigators' respective areas of expertise;

. The AFOSR sponsored or cosponsored 53 symposia in fields relevant to the Air Force's mission; and

. There were various direct links between AFOSR programs and related activities of the in-house laboratories.

Conferences can provide a useful background to military development needs through scientific discussions in areas relevant to the DoD's interests, but often they are too big and impersonal to be of much real value. Smaller, more selective meetings similar to the Gordon Research Conferences might be more productive. Another possibility is a very broadly oriented study during the summer, when many scientists are available for a week or two. Military personnel and project managers could be assembled to discuss with scientists from universities, government laboratories and industry especially important scientific areas of great relevance to the DoD. This procedure has been highly effective in relations between academic and industrial scientists in fields of vital interest to industry, so it should also help in the consideration of DoD problems.

Closer communication between DoD staff and nongovernmental basic and applied scientists could be profitably effected by providing, in each major grant and contract, an invitation to visit and exchange ideas with colleagues at an appropriate laboratory or station. New lines of communication thus established should improve each party's understanding of the other's problems.

Deriving benefits even from good basic research is difficult. To be useful, results of fundamental research must favorably influence the course of applied research and development. Most industrial research laboratories avidly seize upon new scientific findings and put them to practical use quickly and efficiently whether the user originated them or not.

It is extremely important that information from research laboratories be put into well-organized working form, such as a handbook, and made available to the people who need it. Books presenting the state of the art in specialized fields of research, written by a single author—certainly by no more than several—have been effective in filling this need.

3. QUALITY

There is universal agreement that DoD-supported research, like all other phases of RDT&E (research, development, test and evaluation), must be of the best quality; and it appears that the most useful criterion for judging its worth is the opinion of the investigator's peers. Publication of results in scientific journals is highly desirable, although professional magazines vary in quality and standing from field to field and from journal to journal.

After extensively discussing methods of attracting able researchers to the DoD, the Task Force concluded that unbiased scientific judgments, supported by fair, firm and fast administrative action—i.e., termination of poor work and constant, increasing support of quality research—represent the only practical way to do this. In the face of accusations of favoritism, political pressures, requirements for a prescribed geographic distribution, etc., the DoD must exercise the greatest care to avoid not only the evils that cause deterioration in quality but even the appearance of tolerating them.

We generally feel that the system of grants to individuals for projects leads to higher quality research and somewhat greater productivity than programmatic research. This view, however, was found to depend to a considerable degree upon the specific situation at the OXR office and the research laboratory concerned. Given high-caliber personnel, either system of research can, and usually will, produce results of high quality. The emphasis must be on securing and retaining that class of personnel.

4. FUNDING

The impact of limited DoD funding for basic research has already been severe, particularly on the universities. The reduction in available Defense support for individual research projects at universities came at a time when the research budgets of other interested Federal agencies were, at best, holding their levels. Dr. Tape reported to the Task Force that the AEC has in hand university proposals totaling some $50 million that have been rated as "good" but cannot be funded. Similar patterns are found in the National Science Foundation, the National Institutes of Health, NASA and other public and private organizations.

In 1967, the DSB's Berkshire Summer Study Panel on Research and Exploratory Development examined levels of funding for basic research and concluded that, simply to preserve the present DoD position in relation to that of major U.S. technical industry, some 0.7 percent of the DoD's total FY 1968 budget (about $500 million) should be assigned to the program category of research (6.1).

The rationale of the Summer Study Panel is, in essence, as follows:

> The direct methods of management and decision-making characteristic of Advanced Development and Engineering phases are not generally applicable to the Research and Exploratory Development (RXD) phases. . . .
>
> It would be good to have a management method whereby one could weigh the outcome of selective research support in terms of need, cost and payoff. In the absence of such a method, the extent and depth of basic research support must be judged by experienced research managers who can bridge the time span intuitively and imaginatively. Otherwise the judgments may be too shortsighted and miss the biggest payoffs in the long run.
>
> In arriving at a rationale for research planning, one may obtain helpful guidance from industrial research. There are many analogies between military R&D and industrial R&D. Both function in a competitive situation. . . . in highly competitive lines such as chemicals and communication equipment there is the greatest attention to R&D planning and performance. In these fields we see the greatest support of basic research. . . .
>
> In spite of the long-range nature of the payoffs, highly competitive industry will devote up to 10 percent or more of its R&D total expenditure to basic research. (According to NSF 66-28, the chemical industry spent 13 percent of its research and development dollar in 1964 for basic research, less than 25 percent of which was derived from Federal

sources.) If industry can voluntarily sacrifice present profits to stave off the long-range threat of business obsolescence, how much can we spend against the threat of failure in our defense systems? . . .

The only threat to our military security that we cannot fend off by careful military, political and economic planning is the chance of an unanticipated scientific breakthrough by the Chinese or Russians in an area of potential military tactical or strategic significance. . . .

Research cannot be turned on and off to suit the convenience of budget planners, without endangering its quality. . . . Some of the best results have come only after a decade or more of incubation of top scientists in the optimum surroundings of excellent research centers. Good growth must be slow, and based primarily on long-range considerations. . . .

In terms of available skills and facilities, it has been determined (see Pake and Westheimer reports[5]. . .) that an overall growth in basic research performance in academic institutions of 15 to 20 percent per year is currently feasible. This figure is conservative and takes account of the parallel educational needs of the same academic institutions. . . .

With proper attention to relevance and utilization of basic research advances, it appears both feasible and desirable to increase the 6.1 funding level up to at least 10 percent of the total R&D program over a period of 5 to 10 years. . . .

Every kind of effort is being made to improve our fighting effectiveness today in Vietnam. To a significant extent, we have found that our weapons, . . . were not adequate for the peculiar conditions in Southeast Asia. Quick-fixes are the order of the day, when what is needed is the flexibility in design of weapons systems which only a rich supply of research information can provide. . . .[6]

To continue along the Panel's line of thought: In 1940, we did not neglect basic research on nuclear physics and aerodynamics, even under

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[5]*Physics: Survey and Outlook* (Washington, D.C.: National Academy of Sciences, "Pake Report," 1966). *Chemistry: Opportunities and Needs* (Washington, D.C.: National Academy of Sciences, "Westheimer Report," 1965).

[6]*Report of the Panel on Research and Exploratory Development* (Williamstown, Massachusetts: Defense Science Board—National Academy of Sciences Berkshire Summer Study, 5-14 July 1967).

a much greater military threat and with much less sophisticated weapons in our arsenal. Today it appears even more unwise to sacrifice our scientific effort because, in the previous 10 to 15 years, we failed to develop the necessary hardware. Such policies will, if continued, only compound the present serious problem.

Further reduction of basic research would leave the nation vulnerable to a greater variety of attacks. With a relevant, well-coupled research program, we predict that we can develop systems to meet the multiplicity of threats we will face from 1975 to 2000. The alternative of seeking individual "fixes" to meet each threat seems impractically expensive. While the necessary science is costly, it is much cheaper than developing specific hardware to answer each potential defense need.

APPENDIX A

DIRECTOR OF DEFENSE RESEARCH AND ENGINEERING
WASHINGTON, D. C. 20301

15 February 1967

MEMORANDUM FOR THE CHAIRMAN, DEFENSE SCIENCE BOARD

SUBJECT: Defense Science Board Task - Basic Research

I request that the Board establish a task force to study the most effective use of basic research resources. This project should examine in reasonable depth, to the extent practicable, (1) the relevance of DoD basic research to long term Defense goals; (2) coupling between DoD research organizations and the scientific community, emphasizing the coupling among DoD groups; (3) methods to measure and improve the quality of the work; and (4) the general rationale for the amount, balance, and distribution of funding.

I suggest that the study be initiated in an exploratory way to get a better feel for what can be done and what can't be done before a full articulated plan is developed. The first report of the task force would be a preliminary one on the results of this exploration.

It is particularly important that the task force work closely with appropriate offices in ODDR&E and ARPA and coordinate its studies with ongoing programs.

I suggest that the task force make its preliminary report at the May meeting of the Board and its final report by 1 October 1967. I am pleased that Dr. A. D. Suttle has agreed to serve as chairman of this task force. Dr. Donald M. MacArthur will act as the cognizant Deputy.

John Foster Jr.
John S. Foster, Jr.