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Report of the Working Group on Basic Research in the Department of Defense

EXECUTIVE OFFICE OF THE PRESIDENT
OFFICE OF SCIENCE AND TECHNOLOGY POLICY

June 22, 1978

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THE HISTORY

BASIC RESEARCH IN THE DEPARTMENT
OF DEFENSE

A REPORT OF THE
SCIENCE ADVISOR'S PANEL ON BASIC
RESEARCH IN THE DEPARTMENT OF DEFENSE

TO
THE OFFICE OF SCIENCE AND TECHNOLOGY POLICY
AND
STEERING COMMITTEE ON BASIC RESEARCH IN MISSION AGENCIES

JUNE 22, 1978

OFFICE OF SCIENCE AND TECHNOLOGY POLICY
WASHINGTON, D. C. 20500

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SUMMARY, CONCLUSIONS, AND RECOMMENDATIONS

This is a report by a Panel established by the President's Science Advisor in agreement with the Secretary of Defense to review the policies and practices of the basic research program in the Department of Defense (DOD).

The DOD has supported basic research for decades, and it must continue to do so if it is to pursue its overall national defense objectives at the highest possible level of effectiveness, efficiency, and insight. There are three fundamental reasons. Many known technological problems stem from gaps in knowledge which only basic research can fill. Basic research is a source of new concepts which introduce major changes in technological and operational capability. And finally, it is a source of insight for DOD policy-makers and others in evaluating and reacting to the possibilities inherent in technical proposals and in technological developments anywhere in the world.

The support of research has in the past benefited the DOD greatly, and will do so in the future since DOD's capabilities are based squarely on the technological strength of the United States. The use of high technology to preserve and insure our military posture and thereby to stay ahead of our potential adversaries may very well be the key element in our security in the years ahead.

Though DOD funding of basic research has been level in dollars since the mid 1960's it has shrunk to about half of its earlier size in purchasing power in that short time. Though the basic research program is a principal source of the new knowledge, new options, new technical concepts and whole new capabilities so important for the future strength of any first order armed services, it is now substantially below the level needed to meet DOD needs and well below the full potential of the research community to provide valuable contributions. There is now a new policy to reverse this decline and to increase the basic research budget in constant dollars over the next few years. The Panel welcomes and applauds that reversal and believes that if properly administered it will make possible the quality and excellence essential to the research needed to maintain the strength of the United States armed forces.

The Panel's examination of the institutional nature of the DOD basic research program, including the characteristics of the various types of performers, showed a variety of interpretations of current DOD policy on basic research, particularly on the role of relevance as a criterion in funding decisions.

Certain themes were persistent in the Panel's discussions. One is that basic research goes well beyond the solution of known problems. Its results impact a wide range of DOD activities, from policy-making and long-range planning to daily operations. Such use of research stems from the new concepts it generates and from the perspective they afford in review of technological initiatives available to the United States, its allies, and to potential adversaries. Identifying and using these less conventional results of research require great emphasis on appreciation of the significance of new knowledge to the national defense mission and on maintaining the communication necessary to make the connection. This in turn demands that the leadership, both of the basic research program and of the DOD as a whole, possess great sensitivity and judgment about the multiple uses of research.

Another theme is that excellence by the standards of the scientific community is the most useful criterion in evaluating the basic research program. Only such excellence in performers can result in major advances important, and sometimes vital, to the DOD. The resources and time available to the Panel did not allow a specific review of the basic research work in DOD by service laboratory and scientific field, which could have led to detailed recommendations for increasing or decreasing emphasis.

The principal conclusions and recommendations of the Panel are:

1. Necessity That DOD Support Basic Research

It is vital that the Defense Department support an extensive, vigorous, imaginative and high-quality basic research effort. (See page 3.)

2. Clarification of Policy

The Panel urges the Secretary of Defense to affirm vigorously and publicly the critical importance of basic research to the DOD, and to clarify his policy for the support of basic research. (See page 5.)

3. Funding Level

The Panel endorses the DOD decision to continue substantial increases in the level of basic research funding for the next few years, but cautions that DOD officials must be vigilant to insure that the implementation is such as to achieve the several intended objectives. (See page 7.)

4. Relevance

The Panel recommends that the criterion of relevance be applied primarily to broadly-defined fields and subfields of science, rather than as a filter through which every individual project must pass. In selecting individual projects, judgment of the probable quality of the proposed research should be paramount. (See page 9.)

5. The Performers of Basic Research

The maximum quality and effectiveness of the DOD basic research is dependent on a diversified effort of universities, in-house laboratories, industry, and not-for-profit organizations utilized in a manner which properly recognizes the differences in their character and the distinct contribution each can make. (See page 12.)

6. Communications

Effective two-way communication between the basic research community and the multitude of users within DOD is complicated, difficult, but vital; it cannot be made to happen merely by formal system or directive. The Defense Department should increase the number of channels through which this communication can take place, and all levels of management in the Department should recognize its critical importance. (See page 17.)

7. DOD Management of Basic Research

The Panel drew conclusions concerning several management issues: the appointment of a full-time official to provide broad oversight for basic research, the occurrence of gaps in program coverage, the role of DARPA in basic research, and business practices for the procurement of research. (See page 19.)

INTRODUCTION

This report is concerned with basic research policies and practices of the Department of Defense and the part basic research plays in the overall effort of the DOD to carry out its missions. It summarizes the conclusions of a Panel established by the President's Science Advisor, with the agreement of the Secretary of Defense, to examine whether or not DOD policies and procedures concerned with basic research are of a nature such as to fulfill the technical components of the DOD's responsibilities, and whether or not there is a proper division of effort between near-term and long-term objectives.

It was evident to the Panel that, at the present time, large changes are occurring in the defense structure of the United States. Major weapon systems are on the way out of the inventory for a variety of reasons: unacceptable unit cost, displacement by a new concept, a perceived increase in countermeasures, or a changing set of needs for the United States. Some of these systems have long been cornerstones of the strategic defense structure of the United States; still others have been among those first called upon to support national policy in confrontations and major crisis situations. Independent of both that past history and the reasons for changes now occurring in the existing inventory, there are current and future problems of replacing the systems phasing out with other systems of adequate military capability. These new systems should also have potential lifetimes in the inventory which justify the investment and should be manageable within the characteristics and quantity of personnel available. It is the view of this Panel that research has a large and, in these circumstances, timely function to fulfill in aiding those responsible for the selection of weapons systems and prosecution of the defense programs upon which our security will depend. Further, and even more dependent on research, these must be programs and systems which will see us safely enter the next century with a national posture, capability, and technological strength appropriate to that age.

The recent decision by the Secretary of Defense to strengthen basic research in the DOD and the President's charge to all departments to take similar actions in research, are essential to meeting these challenges. The resultant actions will have a more useful impact if the implementation procedures sustain an environment in which good research flourishes. The Panel holds the strong opinion that the best of advanced technology is central to the success of the Defense Department in carrying out its overall mission in the years ahead and that basic research is absolutely fundamental in advancing that technology.

The Panel did not attempt to pass judgment on the detailed content of the basic research program by laboratory and field, leading to detailed recommendations for budgetary allocations. (There are some 72 laboratories and at least 15 fields.)

The Panel heard presentations by a number of senior level managers from DOD, the National Science Board, industry, program managers, laboratory directors, and users of research results. The information presented was considered by the Panel in terms of questions important to decision-making in DOD in light of the collective experience of the Panel members. The Panel also had the benefit of the October 1976 Defense Science Board study "Fundamental Research in Universities." Its influence is found in many places in this report.

DISCUSSION

This section restates each of the Conclusions and Recommendations together with a brief summary of the arguments and discussion which led to them.

I. NECESSITY THAT DOD SUPPORT BASIC RESEARCH

IT IS VITAL THAT THE DEFENSE DEPARTMENT SUPPORT AN EXTENSIVE, VIGOROUS, IMAGINATIVE, AND HIGH-QUALITY BASIC RESEARCH EFFORT.

The strength, vitality, and security of the United States rests in large measure on technology and the wise application of that technology in a wide variety of fields. Prominent among these fields is the establishment and maintenance of military forces ready to carry out whatever missions the Nation may assign them, including combat operations.

The part played by basic research in the essential and continuous modernization of these military forces has not always been fully recognized. As recently as just prior to World War II, the Department of the Army and the Department of the Navy were doing almost no basic research. The Navy record, as an example, shows \$8.9 million for all research and development in 1940. The result was a defense force not well informed of technical possibilities nor fully aware of the engineering and scientific opportunities available to it. Early in World War II, these shortcomings were painfully recognized, and heroic efforts to overcome them were undertaken. These efforts introduced a variety of new technologies--for example, radar, the proximity fuse, nuclear weapons, homing torpedoes, jet aircraft, rockets, and missiles--which changed the conduct of that war, and continue to have impact today on the military strength and readiness of the United States.

After World War II, recognition of the contribution of research to military strength brought about a resolve to assure that the DOD would thereafter make the fullest use of advances in science. Since advances in many areas of science were recognized as vital to the long-term security of the nation, the Services undertook to play a significant role in supporting the advance of selected areas of science. These decisions led to reorganizations in all the Services, to the establishment of offices such as the Office of Naval Research, and to the initiation of basic research programs supported by the uniformed Services. The years immediately following turned out to be a period when a great variety of technical advances became available to the military, many of which were successfully developed and placed in the Services' equipment inventory. Another result of this working relationship was the creation of a community of scientists and engineers, both inside and outside the military, who understood military problems in depth. In the 1960's and 1970's a number of factors caused a weakening of this working relationship between the military and the scientific and engineering communities.

This historical experience makes clear that the Department of Defense needs, particularly at the most senior levels, knowledge and understanding of this rapidly changing and ever more complex technological potential. This knowledge and understanding in turn requires a basic research program funded and fully supported by DOD to insure that fields of direct importance are not neglected in research, education, or training. Also the DOD basic research program encourages direct and natural access by DOD technical personnel to research institutions and scientists and provides a means of bringing into DOD a set of highly-qualified technical personnel who further the process of communication. It provides a means to acquaint research personnel with pressing technical problems of defense and suggests new research directions and possible applications. These practices help create a pool of research scientists in relevant fields, acquainted with DOD needs and potentially available to help on problems where technical contributions are part of the solution. In the other direction, this communication provides members of the research community with access to potential users and with an opportunity for relaxed and understanding debate about radical new concepts of military application.

For all these reasons it is clear that not only is the national effort in basic research vital to the Defense Department, but also that the Department must itself take part in the support of basic research. In some areas activities of other agencies may remove much of the financial burden from the DOD (for example, much of the support of advance in medical knowledge may be funded by HEW), but even in those areas the DOD cannot abdicate all responsibility.

II. CLARIFICATION OF POLICY

THE PANEL URGES THE SECRETARY OF DEFENSE TO AFFIRM VIGOROUSLY AND PUBLICLY THE CRITICAL IMPORTANCE OF BASIC RESEARCH TO THE DOD, AND TO CLARIFY HIS POLICY FOR THE SUPPORT OF BASIC RESEARCH.

In their discussions with DOD officials, with university scientists and administrators, with representatives of industry, and others, members of the Panel became aware that there are many differing perceptions of DOD policy for the support of basic research. In part these differences may result from the effects of the budget stringencies of the recent past; in part from differing interpretations of the Mansfield Amendment and its sequel; and in part they may reflect a lack of knowledge of the new initiatives underway to increase funding of basic research. In many instances the differences in perception revolve around the topic of relevance which is discussed later in the report.

In his 1978 State of the Union Message, President Carter said:

"The health of American science and technology and the creation of new knowledge is important to our economic well-being, to our national security, to our ability to help solve pressing national problems in such areas as energy, environment, health, natural resources. I am recommending a program of real growth of scientific research and other steps that will strengthen the nation's research centers and encourage a new surge of technological innovation by American industry. The budget increase of 11% for basic research will lead to improved opportunities for young scientists and engineers and upgraded scientific equipment in the nation's research centers. I am determined to maintain our nation's leadership role in science and technology."

In consonance with this statement, the Department of Defense is increasing its research activity.

In view of the present confusion which exists both within and without the DOD as to DOD policy, and in view of the urgent need that the new initiatives implementing the Presidential policy fully achieve their several objectives, the Panel believes a policy statement by the Secretary of Defense is important and timely. The Secretary should enunciate his own interest in basic research, his desire for a new surge of technological innovation in DOD, and his policy on the interpretation and application of such topics as relevance. The current Secretary has scientific credentials that would bring to the policy unprecedented weight in both the technical and operational communities of the Department and to the scientific and engineering communities throughout the country. The Panel believes that

such a statement by the Secretary, together with the President's statement, would be a substantial contribution to public discussion and understanding of the role, the need for, and the value of basic research programs in mission agencies generally.

III. FUNDING LEVEL

THE PANEL ENDORSES THE DOD DECISION TO CONTINUE SUBSTANTIAL INCREASES IN THE LEVEL OF BASIC RESEARCH FUNDING FOR THE NEXT FEW YEARS, BUT CAUTIONS THAT DOD OFFICIALS MUST BE VIGILANT TO INSURE THAT THE IMPLEMENTATION IS SUCH AS TO ACHIEVE THE SEVERAL INTENDED OBJECTIVES.

The Panel is concerned with the cumulative effect of the general decline of the total national basic research effort in the last ten or fifteen years and believes the United States in general should be similarly concerned. The DOD portion of that national decline occurred through the practice of near-level dollar funding of basic research from year to year, resulting in a decline in the program about equal to the rate of inflation. Some justify this decline on the basis of increased direct costs of the Vietnamese War and the emphasis on short-range programs. Whatever the merits of this argument at that time, it does not apply now.

At present, the funding of basic research in DOD in constant dollars is about half that of 1966. The Panel believes the current level of funding has reduced the utilization of national basic research below a minimally acceptable level. The planned increases in funding will make possible

- o supporting research with longer-term payoff;
- o supporting research of greater potential value, but higher risk;
- o bringing in new performers, including the young investigators who are needed for the long term;
- o revitalizing relations with universities; and
- o supporting additional areas of basic research.

Successful administration of the program requires the will to take and endure risk of failure. This resolve has been weak in recent years in the face of declining funding.

The Panel fully agrees with the DOD decision to increase funding for basic research. The increases are important if future United States defense posture is truly to be based upon superior technology. The research program is one high-leverage means to increase the output of the whole RDT&E program.

The Panel believes that the planned gradual year by year increases will occur at a rate which should result in healthy and orderly growth in the research community supporting the defense establishment. Growth for ten years at a rate similar to the rate of decline of the past ten years is indicated. The Panel does not undertake to forecast the needed level of

research in DOD at the end of the coming ten years. However, if as is likely, the technological challenge facing the United States continues to grow in the next decade as it has in the last, the increase planned will surely not be excessive. Further, that increase will play a key role in providing the essential building blocks for ongoing support of our national defense in the more distant years.

While the Panel believes increased funding is vital, the timing did not permit review of DOD's detailed plans. The funding increase has several objectives, which will by no means come automatically. The diligent and thoughtful attention of DOD officials will be required.

IV. RELEVANCE

THE PANEL RECOMMENDS THAT THE CRITERION OF RELEVANCE BE APPLIED PRIMARILY TO BROADLY-DEFINED FIELDS AND SUBFIELDS OF SCIENCE, RATHER THAN AS A FILTER THROUGH WHICH EVERY INDIVIDUAL PROJECT MUST PASS. IN SELECTING INDIVIDUAL PROJECTS, JUDGMENT OF THE PROBABLE QUALITY OF THE PROPOSED RESEARCH SHOULD BE PARAMOUNT.

There is no topic more controversial, more subject to variety in interpretation, or harder to define, than "relevance." It clearly means quite different things to different people in the DOD, in the research community, and in many other offices and activities impacting the DOD basic research program. As a problem it seems partly a matter of what it is and what it means, and partly a question of how to administer and use it.

As a mission agency the DOD is concerned with planning and maintaining a program in basic research. It is also concerned with and interested in the product of that research program. It then does something useful with that output--sometimes quickly, sometimes later, and sometimes later still in combination with newer research results.

In taking these steps it is clear that the Department of Defense should not be carrying out an uncontrolled basic research program in the full range of all proposals which might be made. There must be some priority and some focus of the DOD research program for the anticipated DOD needs. It is clear that some projects will be of the quality, imagination, and content as to show strong promise of solving identified problems of major importance to the Defense Department. Clearly such projects should be among those supported.

The question of relevance is one part of the larger issue: how can one deploy the funds supporting basic research so as to maximize the useful results over the long run? The problem has much in common with prospecting for ore. When a new body of ore is discovered one must explore its extent. But one must also continue to search for new bodies of ore. Science likewise moves in two stages. At intervals truly major discoveries are made which drastically reorient thinking and open vast new possibilities. It is important to explore the consequences. Fortunately when a major new discovery is made, scientists flock to exploit the new opportunities. Predictably, there is scientific gold to be mined.

The major steps, however, are unpredictable because they involve discovery of phenomena or concepts which were not previously known. Examples are the discoveries of superconductivity, nuclear energy, and the maser (which then rapidly led to the laser). A recent study by the National Science Board of 65 discoveries in astronomy, chemistry, mathematics, and earth sciences rated by scientists as being among the most important made between 1950 and 1975 found that only 40 percent were mentioned in the

request for funds which supported the work, and another 17 percent even fell entirely outside of the proposed area of research. Even after a new discovery is made, realization of its full potential depends on imaginative exploitation.

A primary task of the research manager is to assure a flow of major new discoveries and their imaginative exploitation. This cannot be done if all that is funded is research on a problem of known significance along a path which will assuredly lead to success. Such a policy would only assure tiny steps along a familiar path. Whether seeking to exploit known discoveries, or seeking to generate major new ones, the research manager will maximize results by identifying the scientists, renowned or not, with the greatest ability to perform the research.

The almost universal characteristic of major discoveries is that they open possibilities in fields far from that in which the scientist was working at the time of his or her discovery. Therefore, in selecting areas to support, the research manager must define the area in very broad terms. The responsibility for taking the broad view is especially important when it is the government which is doing funding because the forces at work on private investors often limit greatly the scope which can be supported with the funds available.

It should be noted that "fields of potential use" is for the military by no means a fixed or absolute listing. Changes from time to time as research and technology evolve will find research which at one time might be viewed as totally separated from military potential becoming at a later time the very heart of a new family of weapons systems or capabilities. Similarly fields in which major effort might today be occurring can at some time in the future shift to a lower priority.

The Mansfield Amendment stated:

"None of the funds authorized to be appropriated by this Act may be used to carry out any research project or study unless such project or study has a direct and apparent relationship to a specific military function or operation."

After brief experience the "direct and apparent" criterion was deleted and replaced in the legislation by a much broader and more workable criterion:

". . . in the opinion of the Secretary of Defense, a potential relationship to a military function or operation."

This second criterion is still in effect.

The Panel heard a range of opinions from DOD personnel concerning the degree to which a narrow interpretation of the relevance requirement currently impacts DOD basic research. These opinions varied from stating that few good quality proposals are without relevance to a military objective, to others stating the emphasis on relevance has led to a steady evolution towards:

- o Proposing only projects with short-term objectives where relevance in the application sense is easily identified.
- o Supporting only such short-term projects.
- o A condition in which more venturesome proposals which may be of dramatic and lasting long-term impact are avoided because specific application within a few years cannot be assured.
- o Supporting projects which are applied research in character and are directed to the solution of identified problems.

Such evolution has the effect of diluting and reducing a basic research objective and purpose: to generate new knowledge, new understanding and new concepts from which truly innovative and imaginative solutions of military importance sometimes flow.

From the point of view of some performers of high quality research, the perceived pressure for an assured relevance has led to a decline in their interest in seeking support from DOD. This too has had an impact on the excellence of the overall program. It is unfortunate that the use of relevance considerations, though intended to increase the usefulness of research, has perhaps had the opposite effect.

The sensible consideration of potential relevance requires a long view, wide scope, a statesmanlike and mature understanding of what is now occurring in science and technology along with a grasp of trends and possibilities of the future. It requires the exercise of thoughtful judgment in ruling that some field of endeavor has limited or broad application to a mission agency future task.

Thus, in deciding which proposed projects to support, the research manager in government should not use narrowly-identified relevance as a filter through which every project should pass, but rather should apply a broad concept to wide areas of science. Within these areas, project selection should be controlled primarily by the perceived quality of the investigator, by the originality of the proposal, and by its balance with work already in progress.

V. THE PERFORMERS OF BASIC RESEARCH

THE MAXIMUM QUALITY AND EFFECTIVENESS OF THE DOD BASIC RESEARCH IS DEPENDENT ON A DIVERSIFIED EFFORT OF UNIVERSITIES, IN-HOUSE LABORATORIES, INDUSTRY, AND NOT-FOR-PROFIT ORGANIZATIONS UTILIZED IN A MANNER WHICH PROPERLY RECOGNIZES THE DIFFERENCES IN THEIR CHARACTER AND THE DISTINCT CONTRIBUTION EACH CAN MAKE.

The basic research program of the DOD is carried out by four groups of performers--universities, in-house laboratories, industry, and not-for-profit laboratories--each possessing special characteristics, each capable of making a distinctive contribution.

The proper balance of DOD support for basic research among these groups of performers cannot be intelligently determined by any overall formula or arbitrary ratio. Continuing judgments must be made, based on merit: quality of proposals, scientific opportunity, availability of unique facilities or instrumentation, and--most important--on demonstrated excellence. Additionally, stability of the program, stability of the performing organization, and appreciation that high-quality basic research is often a long-term endeavor should enter those judgments. Other factors arise from the nature and character of the performing organizations and play a part in determining the balance of the program. Though well known, those factors are worth mentioning here.

A. Universities

Universities are the primary performers of basic research and the source of virtually all new members of the scientific community. They are also institutions in which basic research is a major function, since basic research is almost inextricably interwoven with advanced education. The strength and weaknesses of universities as performers of research to meet the needs of mission agencies are closely related to the fact that education is the universities' primary mission. It is important that managers of basic research recognize this distinction between them and other performers. The primary business of universities is learning in all its aspects; it includes the generation of new knowledge (which is also the objective of basic research) and then preserving and transmitting that new knowledge. In pursuing this broad purpose the universities produce scientific findings that are relevant, some of them highly so, to the specific missions of public agencies. The volume of these findings is influenced by the amount of research supported by mission agencies.

The university population is a mixture of independent-minded faculty, inquisitive new students, and highly-motivated advanced students who should be considered working scientists and engineers. Universities select faculty members and advanced graduate students for their originality, creativity, and independence in their scholarly fields.

These are precisely the characteristics needed for basic research and make this population in the universities a logical place to expect the original and startling new ideas from research that result in scientific and engineering breakthroughs. It is that single-minded focus on a particular field which characterizes the work of most university faculty members and generates the rapid advance in scholarship when a new breakthrough occurs. Because of these characteristics the academic community cannot successfully be directed to perform highly-specified research. It makes its greatest contribution in performing a major role in research that is truly basic. The Panel does not intend to overlook the growing interest in capability to work on applied research programs. Moreover, there is a trend toward establishing interdisciplinary centers (e.g., energy, environment, materials) which should be encouraged. These activities also deserve DOD support.

It is important to recognize that universities differ from one another with great variety in size, sponsorship, quality of work in a given academic field, in facilities, areas of interest and the capability of both individual and team effort to extend the frontiers of knowledge and understanding. Somewhere in this huge wealth of research and scholarly activity, mission agencies will find many science and engineering skills which serve both their long-term and, indeed, their immediate interests. They can find and encourage such research in progress or in proposal, but they cannot order it done. Despite their traditional intellectual independence the universities require funding by public agencies if they are to carry on basic research of a quality to generate the stream of new ideas they are capable of producing and which the nation needs. With few exceptions research at the frontier of current 1978 knowledge is expensive and cannot flourish today without the combined support of private donors, foundations, state governments and Federal agencies. From the viewpoint of mission agency research, such support of universities should not be considered an aid program for the university but as a means of investing for future technological dividends. It is the means of realizing the potential of universities to do work carrying with it the possibilities of breakthroughs which give rise to significant changes in the defense capabilities of the country. This investment has been low in recent years and is now moving toward a more productive level.

It is in the universities that is found the development of the community of scientists and new investigators whose originality, independent interest, and expertise in the problems of national defense are broader than expertise in a given scientific field. Development of such a community depends, in the last analysis, upon a personal commitment by the individuals in that community. The provision of information, opportunities to work in-house and encouragement for specific members of the community to become consultants and advisors to the DOD, as was done after World War II, appear to be effective measures for a mission agency to take in encouraging such commitments. The Panel endorses the decision by DOD to make a concerted effort to renew and enhance working relationships with the nation's universities.

B. In-house Laboratories

The in-house laboratories of the services tend to have assigned missions, with the major portion of their efforts in direct support of the mission area. Basic research is only a small portion of the total program of in-house laboratories, but it is a vital portion. Like other parts of the research program, in-house basic research has been at a low level and should be strengthened selectively as the total DOD program grows. Increases in basic research in the in-house laboratories should depend on demonstrated excellence in both program and research staff.

It is well understood that intramural basic research supports a number of vital DOD laboratory functions which cannot all be acceptably performed by nongovernment organizations. In addition to being research and development resources in their own right, DOD laboratories do R&D planning, evaluate proposals, monitor contract performance, appraise new system concepts, and aid the DOD to function as a buyer with sophistication and knowledge in fields of technology. As part of the military departments, they can follow scientific advances outside the DOD, and they can be uniquely effective in recognizing emerging research results which need transition to exploratory and early development phases of new military systems. Moreover, the laboratories are the sole repository of government technical experience that can be applied to new program directions and new program starts.

In some important areas which attract little attention in the non-government research community, such as explosive or propellant chemistry, the DOD laboratories are primary sources of creative new approaches. They perform a similar role in fields where specialized facilities are the key to progress and it is impractical to maintain similar facilities outside of the laboratory community.

There is yet another part of the laboratory business which is vital to the efficient and sensible use of research results. This is developmental engineering and technical support to field operations. This sort of work has a major influence on focusing the laboratory basic research within the laboratory mission and contributes significantly to the laboratory's ability to maintain its technical strength, its vigor, and its usefulness for the future.

The Panel notes that there are currently management practices which impair the ability of the in-house laboratories to carry out these functions with strength and vigor.

The quality and success of any laboratory rest on a policy that management demands excellence and on the delegation of enough management flexibility to allow excellence to be demanded. Impeding progress toward excellence is an administrative environment in the laboratories which lacks flexibility in personnel management. The inability, on a practical and timely basis, to remove less effective staff members or to reassign others

in line with changing missions results in losses of efficiency. Whether Civil Service and internal DOD regulations are indeed so restrictive as to prevent effective personnel management is much argued. Independently of the details of that argument, however, laboratory directors unanimously assert that they are unable to reach their personnel goals without excessive expenditures of time and energy. An additional and sometimes major problem is the detailed "management" of laboratory research programs by successive layers of higher authority. This seems to occur because of inappropriate use of the authority of the line of command from senior levels of the Department to the laboratory level, a situation which is not often found in research programs of other performers.

In some cases, the undercutting of the authority and responsibility of the laboratory technical director has been reduced by the use of the block funding concept, but the problems are far from solved. It is important that the laboratory senior managers, once given the responsibility for research excellence, really be provided with the necessary funding control, authority, and independence to meet those responsibilities, including some funds under their discretionary control with ex post facto accountability.

C. Industry

Industry has tangible assets in the form of talent and facilities which give it a significant capability for the performance of basic research work. In addition, some of its intangible components have value. The organization of industrial laboratories often makes them particularly capable of research requiring interdisciplinary effort, for example. Their other intangible assets include managerial independence and experience, and their industrial environment. The groups which do basic research successfully in industry are for the most part isolated from the demands of scheduled projects, but they are still in an environment formed in large part by an institution with primarily technological, often DOD-related commitments. At the same time they participate in the activities of the scientific community. This combination is significantly different from and complementary to the universities and in-house laboratories. Industrial basic research groups in such an environment have shown that they can do high-quality basic research.

Industry tends to do research in support of company interests, either short- or long-term, and such work is properly funded from company money. When basic research is done with outside funds, it is usually in close support of a well-defined research or development problem; and there are many areas where industry has special capability for such supportive research. When an industry is doing development or engineering work for the DOD, it makes good sense for that industry to perform basic research in areas DOD believes will help strengthen the performance of development work. DOD must concern itself with the health of basic research in the defense sector of industry, and in selected cases it can fund appropriate basic work directly through a research contract. The same mode can be used when only industry is equipped to accomplish some particular research task.

In recent years, industry has tended to focus on less basic, shorter term objectives in its exploratory work, and this has led to a decline in the amount of basic research it has funded. This has occurred simultaneously with a similar decline in DOD funding of such work. The reasons for these parallel declines, while not entirely clear in detail, do not appear to be the same. In the case of industry, important reasons include diminished profitability, the increase in interest rates, and changing perceptions as to the immediate value of research. Since the DOD has now initiated a reversal in the decline of its program, it now has an opportunity to take more advantage of the capability of industry.

The other support for basic research in industry is through Independent Research and Development funds which are currently used in very limited fashion for such research. Many industrial firms are open to policy guidance on appropriate basic research and the balance between basic research and other work supported by IR&D funding. This guidance can and should appear in the course of government reviews in a manner that generates an increase in the use of available IR&D funds for basic research without infringing on the independence of the industrial management involved.

D. Not-for-Profit Performers

The last and smallest group of performers of basic research is not-for-profit organizations, which resemble in some manner in-house laboratories as far as basic research is concerned and in other ways resemble industry. Here again, a particular field of expertise, a unique set of facilities, and basic research in support of other ongoing research and development efforts are appropriate for programming into this type of organization.

E. Conclusion

Since each type of performing organization has its own special strengths, the DOD must make sure it continues to work with all of them. Indeed the existence of close and constructive relationships with all performers, in a context of competition, is a strong characteristic of military affairs in this country that is not found elsewhere. Just as the DOD is concerned with the national capacity for production of defense equipment, so must it be concerned with the whole national capacity for the conduct of basic research.

VI. COMMUNICATIONS

EFFECTIVE TWO-WAY COMMUNICATION BETWEEN THE BASIC RESEARCH COMMUNITY AND THE MULTITUDE OF USERS WITHIN DOD IS COMPLICATED, DIFFICULT, BUT VITAL; IT CANNOT BE MADE TO HAPPEN MERELY BY FORMAL SYSTEM OR DIRECTIVE. THE DEFENSE DEPARTMENT SHOULD INCREASE THE NUMBER OF CHANNELS THROUGH WHICH THIS COMMUNICATION CAN TAKE PLACE, AND ALL LEVELS OF MANAGEMENT IN THE DEPARTMENT SHOULD RECOGNIZE AND EMPHASIZE ITS CRITICAL IMPORTANCE.

A well-recognized problem in industrial organizations is that of communicating the results from the research laboratory to those parts of the company engaged in development and engineering and in the other direction educating the research laboratory about the problems the user believes to need additional research. This problem has long been recognized as serious, even where the research laboratory is as close as across the street from the user engineering and development departments. To overcome these problems, even partially, good communication between the research and using communities is essential. But good communication alone is not sufficient; creativity and invention are essential in recognizing applications of basic research. The DOD encounters the same problem. It is compounded by the size, variety, and complexity of the basic research efforts being accomplished through DOD support, and by the very substantial variety of research being accomplished by other agencies of the government and the nongovernment scientific community.

The question: How to couple those two parts into a team which wants to go in the same direction? How to communicate the results of the basic research program so that it is heard and understood by the users? How to exploit the impact of that research on the development, acquisition and operational programs of the services concerned? Unless these goals can in the main be reached, the DOD has little case for conducting any program of basic research.

Traditionally this connection has been maintained by a too small community of people with credentials in both the research and user worlds who are capable of communication between these groups. One group, more familiar with the research end, is, of course, found in research program management offices (ARO, ONR, AFOSR, DARPA), the in-house laboratories and in parts of the materiel commands of the Services. Another group in the user part of the organization is to be found in development and acquisition offices in DOD, the Service headquarters staffs, Service materiel commands and the operating forces.

To date, no way has been found to systematize this communication process satisfactorily. Organizational arrangements, formal procedures and regular documentation all enhance the process, but the task of transferring technical information and ideas between these communities requires imagination, initiative, flexibility, special programs, and most important-- personal contact.

Replacing personal contact and discussion with well-codified lists of requirements tends to suppress both thought and healthy communication. Thus, programs are omitted on the basis of there being no requirement, and programs of questionable quality are justified as being responsive to a stated requirement. The Panel concludes that maximizing the number of contacts which bridge the gaps between research and user worlds is the critical means of dealing with this problem.

As a mission agency, the DOD must insure that all available new knowledge, all new information, and all new possibilities arising from its own research program are recognized, considered, and rapidly put to constructive use where possible and appropriate. The DOD-funded basic research program cannot, nor indeed should it, encompass all the research work that will turn out to be of significance to defense. The very substantial total of non-DOD funded work yields results which should be similarly recognized and considered. Effective contact with this work requires substantial interaction and communication to couple those major sources of research results to the defense community. Much of the communication with the non-DOD area occurs through working members of the research group maintaining contact with DOD research offices, and much occurs through communication channels of industry, an important and effective participant in this process.

There is a facet of communication and coupling that must be kept in mind. The harvesting of research results makes available to the user new options and capabilities of major significance whose impact can have the effect of changing long-recognized military equipment requirements or even changing national roles and missions. Frequently a requirement comes into focus as a result of bringing into being the capability to fulfill that requirement. This circumstance demands two-way communication with the very best of understanding, comprehension, and open-minded judgment to insure our defense program is realizing the maximum benefit.

There is no permanent, final solution to this long-standing problem other than to continue to build and maintain an effective body of knowledgeable personnel in both user and research communities who are capable of and interested in communication between those communities. The substantial changes now occurring in major defense weapons systems and equipment signal an increased need for awareness of the possibilities available from research on the part of people with responsibility for military operations as well as those with responsibilities for acquisition. That awareness should include a basis for evaluating claims of operational importance, since it is from these possibilities that the defense inventory of the next century will be selected. A strong, active, and continuous coupling between the scientific community and the user community will play a part in making those selections the wisest and best possible, in addition to helping insure that the process occurs at a rate that maintains a worldwide technology advantage for the United States.

VII. DOD MANAGEMENT OF BASIC RESEARCH

THE PANEL DREW CONCLUSIONS CONCERNING SEVERAL MANAGEMENT ISSUES: THE APPOINTMENT OF AN OFFICIAL TO PROVIDE FULL-TIME BROAD OVERSIGHT FOR BASIC RESEARCH, THE OCCURENCE OF GAPS IN PROGRAM COVERAGE, THE ROLE OF DARPA IN BASIC RESEARCH, AND BUSINESS PRACTICES FOR THE PROCUREMENT OF RESEARCH.

A. Focal Point for Basic Research

THE BASIC RESEARCH PROGRAM OF THE DOD NEEDS A SENIOR OFFICIAL IN THE OFFICE OF THE SECRETARY OF DEFENSE TO PERFORM FULL-TIME THE ROLE OF BROAD OVERSIGHT OF BASIC RESEARCH AND TO SERVE AS A FOCAL POINT BOTH INSIDE AND OUTSIDE THE DOD FOR ADVOCACY OF THE BASIC RESEARCH PROGRAM. THIS OFFICIAL NEEDS THE SUPPORT OF A SUITABLE MECHANISM FOR PERIODIC OVERSIGHT REVIEW OF THE PROGRAM.

The responsibility and the authority for expenditures on basic research in the DOD rest with the three military departments and DARPA, all under the policy guidance and review of the Office of the Secretary of Defense. Service participation has been important in insuring that each of the Services could communicate effectively with the scientific community and that scientific areas of particular importance to the Service would be adequately supported. However, the same features that promote Service interest and participation may on occasion promote the tendency to focus too sharply on areas of easily demonstrable Service problems, to reduce contact with the programs of the other Services and other government agencies or to avoid areas which may threaten a mission as currently constituted. One of the major functions of OSD is to work toward a program that crosses departmental boundaries and is broad enough in scope to cover areas of future importance to the whole DOD in addition to areas about which the departments are currently enthusiastic.

The basic research program of the DOD is now and for a long time has been performed by a large number of individuals dispersed in small concentrations throughout a number of organizations, and it is important that this condition continue. The research deals with a large number of disciplines and topics, some of which have much and some have little immediate importance to the Department. It is frequently under pressure to be targeted on urgent problems facing the Services rather than on longer-term, more speculative, or less manifestly relevant topics. The sort of basic research program needed for the long term needs of the DOD is difficult to achieve, as is assurance that such a program has in fact been achieved.

The current planned funding increases are important steps in restoring the basic research program to the desired character. Implementation of the increases will take place through a large number of individual actions throughout the three Services. It is important to assure that the several intended objectives of the funding increase are achieved. It is also important to assure that broad policies, such as those with respect to relevance and scientific quality, are being followed.

The Deputy Under Secretary of Defense for Research and Engineering (Research and Advanced Technology) is the official responsible for maintaining such oversight and also for acting as advocate and focal point. This is a logical assignment, but she has many additional responsibilities and is able to devote only a portion of her time and attention to basic research. The Panel believes that oversight of the broad characteristics of the basic research program is an important function which merits the full-time attention of a senior DOD official. This official would logically report to the Deputy Under Secretary of Defense for Research and Engineering. The individual chosen to carry this full-time responsibility should be someone well recognized in the research community, whose reputation was established by work in research. The name, record, and stature should be such that he or she would automatically come to mind in contacts between the research community and DOD.

B. Urgent Deficiencies

ON OCCASION, IT BECOMES APPARENT THAT THERE ARE MAJOR DEFICIENCIES IN BASIC KNOWLEDGE IN CERTAIN FIELDS IMPORTANT TO THE DOD. THESE ARE SITUATIONS IN WHICH ADEQUATE RESEARCH LEADING TO BASIC UNDERSTANDING HAS NOT YET BEEN DONE, AND IN WHICH SUCH UNDERSTANDING IS NOT ONLY RELEVANT, BUT URGENT. THE DOD SHOULD BE ALERT TO THE EXISTENCE OF SUCH SITUATIONS AND REACT APPROPRIATELY. CURRENT EXAMPLES ARE SOFTWARE AND HUMAN FACTORS.

As noted above, one of the functions requiring central focus is the recognition of broad areas where for one reason or another we lack an adequate basic understanding and the research program is insufficient. Frequently, in the course of system development, pacing technical problems turn out to be far more intractable than expected, or the solution of one set of development problems reveals an underlying difficulty not previously recognized. In other cases, someone realizes that difficulties we have learned to live with could be ameliorated if we had a better understanding of the fundamental factors. The difficulties of logistics management and failure modes of many solid state devices are examples of the latter class.

The DOD research management must be alert to the emergence of such situations and be ready to respond in a variety of appropriate ways. In some cases, the need for basic work will be urgent, as when a possible vulnerability of a major weapon system has been suggested, or when an enemy system appears to function by principles we do not understand. In other cases a prudent increase of investment in some scientific area is the appropriate response. But the most difficult case is that in which there is no recognized discipline in the area of DOD interest, or the number of practicing scientists is well below what appears to be needed. The "prudent increase in investment" class can be handled smoothly within the normal management procedures. The others will require special arrangements, which may entail the stimulation of increased academic interest in fields of concern to DOD.

The Panel has not made a systematic search for scientific areas in which there are urgent deficiencies, but two examples stand out in the most cursory examination as areas of major importance to the DOD where presently supported research effort is incommensurate with the importance. The two examples are fundamental software theory and human factors.

While great progress has been made in computer system hardware over the last 25 years, the associated software problems have come to require an annual DOD expenditure in the neighborhood of \$3 billion. Despite such levels of expenditure for software, reliable software for large military systems still involves substantial schedule and cost overruns. The need for more fundamental understanding in the form of theorems and general principles is recognized within the community, but no adequate scientific base exists, nor is there more than a handful of scientists capable of doing good basic research on the subject. This is a case in which the DOD must develop the national scientific capability and build the size of the community to the point that real progress can be expected.

Personnel costs now amount to more than half the DOD budget. The point has frequently been made that such a significant fraction of our total investment and the importance of personal performance in all military activities deserve far more basic research than is traditionally supported. The Panel can add nothing to the argument other than to repeat that improvements in the number and quality of workers in all the fields related to human performance must in the long run pay handsome dividends.

C. DARPA

IN SPITE OF ITS NAME, DARPA IS NOT PRIMARILY COMMITTED TO RESEARCH, BUT RATHER TO ADVANCED DEVELOPMENT PROJECTS, SOME OF WHICH REQUIRE A CERTAIN AMOUNT OF BASIC RESEARCH. IT FREQUENTLY UNDERTAKES PROJECTS OF SHORT DURATION, A MODE OF OPERATION NOT COMPATIBLE WITH THE STABILITY NEEDED IN BASIC RESEARCH. IT IS RECOMMENDED THAT WHEN DARPA SUPPORTS BASIC RESEARCH, IT EMPLOY ARRANGEMENTS WHICH PROVIDE THE STABILITY NEEDED FOR PRODUCTIVE PROGRAMS.

DARPA was originally established as ARPA, an office in the Secretary of Defense structure responsible for, among other things, research not identified with a specific military requirement, research relating to a major function with which two or more Services were concerned, and research determined to be best handled by an agency outside the military departments. It has since evolved into an organization dealing more with advanced development projects and less with basic research. DARPA still strives to be alert to the latest technology and the most recent output of basic research programs. It may even identify areas of science where research is needed.

It is sometimes difficult for the Services to initiate a new imaginative development program quickly. DARPA carries out this function as a small management group that draws on in-house laboratories, universities, and industry. The Department of Defense needs an organization able to function as DARPA does with short reaction times to explore new concepts and to exploit new applications of the latest in technology. The current use of DARPA as an organization attempting to match detected problems and gaps in DOD programs with new technology and new ideas is useful and timely. Further, the intent to avoid mere small improvements and match such problems with technological solutions in a manner that makes great strides forward in military capability is indeed appropriate.

The current mode of operation is fast moving and involves quick reaction. DARPA may move into an area, make a contribution, then move on to a new topic, all on a very short time scale, at least compared to the time scale over which basic research may lead to practical results.

Clearly the identification of deficiencies in knowledge or of special research opportunities is a significant contribution. In the 1960's it led DARPA to initiate the Materials Research Laboratories. The in-quickly, out-quickly mode which may be especially useful for much of DARPA's activities will rarely match the time scale on which basic research can be initiated, produce results, and be sensibly phased down, however. The Panel believes that special precautions are needed when DARPA wishes to stimulate basic research in some area. DARPA should consult with potential researchers about the time scale which may be needed to produce results, and examine the time scale on which DARPA needs the results and over which it is prepared to support the research. If DARPA is unwilling to commit resources for as long a time as the potential researchers feel is necessary were they to undertake the work, this fact should be well known by all parties since DARPA may need to seek arrangements with one of the Services for assuming longer-term responsibility. In fact, one useful role for DARPA may well be identification of basic research needs or opportunities, calling them to the attention of one or more Services, and should the Services desire a long-term commitment helping with initial funding.

D. Business Practices

BECAUSE OF THE UNCERTAINTIES INTRINSIC TO BASIC RESEARCH, AND THE EXTRAORDINARY NEED FOR FLEXIBILITY IN THE MANAGEMENT OF IT, THE DEPARTMENT SHOULD MAKE SURE THAT ITS MANAGEMENT AND BUSINESS PRACTICES IN THIS AREA ARE COMPATIBLE WITH THOSE OF THE BASIC RESEARCH COMMUNITY.

The uncertainties intrinsic to basic research generate a need for flexibility in the management and business practices used in this area. The intent of this flexibility is to enhance the performance and output of the basic research community. The major performers--universities, in-house laboratories, industry, and not-for-profits--differ institutionally in several respects. For maximum effectiveness, it is important that the instrumentalities of DOD support be flexible enough to accommodate the differences and take full advantage of them.

In arranging for basic research by non-government performers, the Services use grant and contract instruments, depending on which is more advantageous. The prime requisites are simplicity, flexibility and the ability to include special arrangements for equipment or government assistance to the conduct of the work where such arrangements will further the progress of the project. The majority of government contracts are for straightforward major procurements of hardware or materiel. The business regulations for such procurements are sometimes difficult to use in dealing with research in universities. Likewise, the majority of government grants are for assistance programs, many of whose features are unnecessary or inappropriate when the grant is for performance of a research project.

There is an additional problem in that many inappropriately view grants to universities as a measure of aid to support the university when grants are, in fact, an efficient way to fund some research which the government wishes to see completed. The Department is investigating the possibility of changing management and business procedures to enhance simplicity, flexibility, and productive use of the basic research community. These steps should reduce the diversion of time, effort and cost away from that central purpose; excessive administrative demands waste available funds. Simplifying contractual procedures and reducing paperwork are not inconsistent with sound management or auditing procedures and encourage the best research efforts along with earlier availability, understanding, and use of research results.