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Report of the Panel on RESEARCH AND EXPLORATORY DEVELOPMENT

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5-14 JULY 1967

Defense Science Board - National Academy of Sciences
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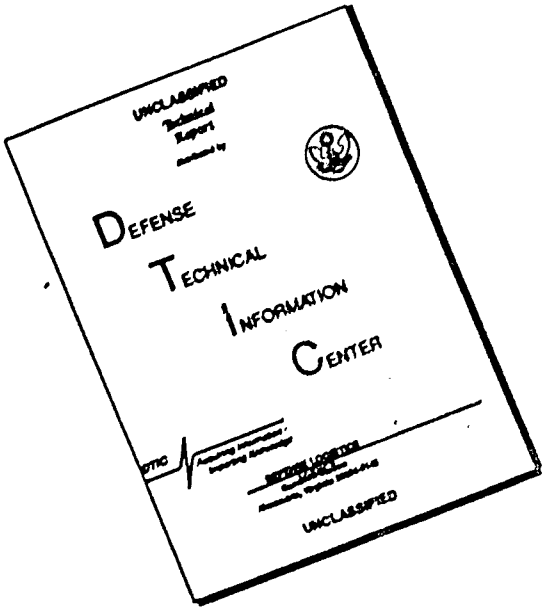

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Panel

on

RESEARCH AND EXPLORATORY DEVELOPMENT

5 - 14 July 1967

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I. INTRODUCTION

Our group was tasked with the examination of issues related to the optimum funding for the Research (6.1) and Exploratory Development (6.2) programs, viewed with particular reference to the decreased effort each year since FY 65. The complete task statement from Dr. Foster is attached as Appendix A. We addressed all of the issues raised in that statement; since the structure of this report is not parallel to the structure of the task statement, we have indicated the points where specific task questions are addressed by lower case letters in the margin.

The work of the Defense Science Board Task Force on Basic Research Policy led by Dr. A. D. Suttle, Jr. was of great help to our group. That Task Force addressed principally the quality, relevance, coupling, and level of effort of DoD research; we stood on their broad shoulders, especially on the question of level of effort.

We also gratefully acknowledge the assistance of Dr. D. M. MacArthur, Mr. R. W. Nichols, Dr. R. E. Uhrig, Col. R. S. Isenson, Dr. W. W. Hammerschmidt, and Dr. R. M. Thomson. The members of our committee were R. L. Sproull (Chairman), R. W. Cairns, R. E. Norberg, A. D. Suttle, Jr., and J. H. Wakelin, Jr.

II. THE SITUATION

Two principal developments have led to the decreased effort: DoD has increasingly had to compete with other Federal programs and national objectives; within DoD, Program Package 6 (Research, Development, Test and Evaluation) has been kept sensibly constant in the face of increasingly urgent demands for large 6.3 and 6.4 programs such as MOL.

The funding trends for 6.1, 6.2 and 6.3 for the period 1962-68 are presented in Figures 1 and 2. In each case the absolute dollar level is indicated along with an estimated "equivalent" technical effort, taking into account inflation in the cost of doing business at 5 percent per year.[1] Curve C of Figure 1 shows that during the period 1965-68 the absolute dollars for 6.1 have increased by 4.3 percent, while Curve D shows that the technical effort (man years per year) has decreased approximately 30 percent. In 6.2 there has been a decrease of 20 percent in absolute dollars while the technical effort is down by 38 percent.

It has been claimed that this reduction in 6.2 might be justified in view of the fact that 6.3 has increased by 108 percent since 1965. It should be recognized that the major new expenditures in 6.3 such as MOL, SAM-D, NIKE-X(AD) and the like do not utilize the same segments of the scientific and engineering community as other 6.2 efforts, and it is clear that MOL, in particular, will not contribute much to a broad technology base because, by definition, it will use "off the shelf technology." Figure 2 shows for the period 1962-68 the trend in total 6.2 and 6.3 obligations. It also shows the impact on this trend if the funding for MOL is not included and if the effect of a 5 percent inflationary correction during the 1965-68 period is included. It is seen that: (a) the total 6.2 and 6.3 dollars have increased by 22 percent, (b) the dollar total of 6.2 and 6.3 has been essentially level if MOL is not included, and (c) the total 6.2 and 6.3 effort under that condition has actually decreased by 17 percent.

Figure 1

RESEARCH & EXPLORATORY DEVELOPMENT EXPENDITURES

Ⓐ - Exploratory Development (6.2) New Obligational Authority; (NOA) (in current dollars)
Ⓑ - Exploratory Development, NOA adjusted for 5% per year inflation
Ⓒ - Research (6.1), NOA (in current dollars)
Ⓓ - Research NOA adjusted for 5% per year inflation

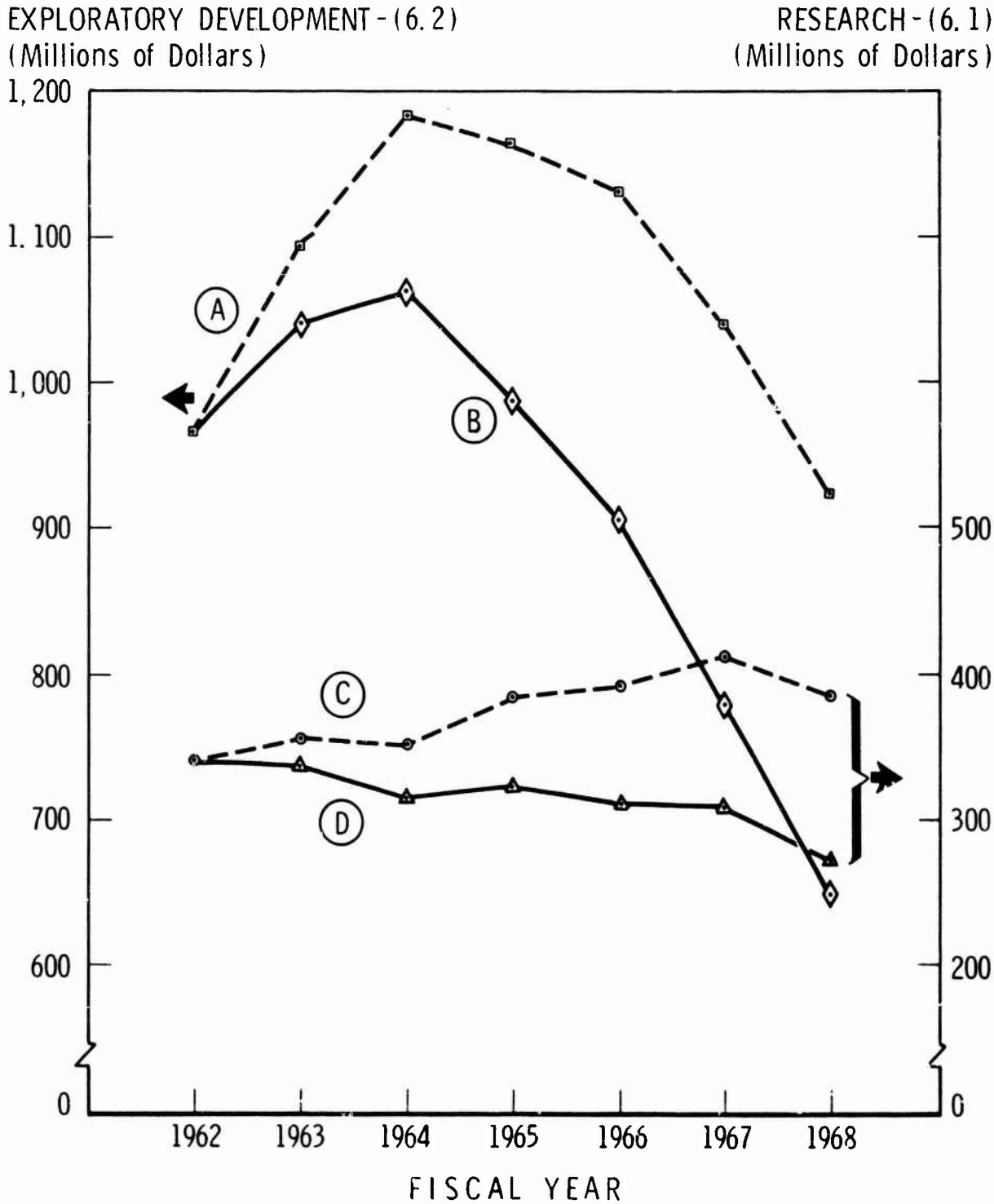


Figure 2

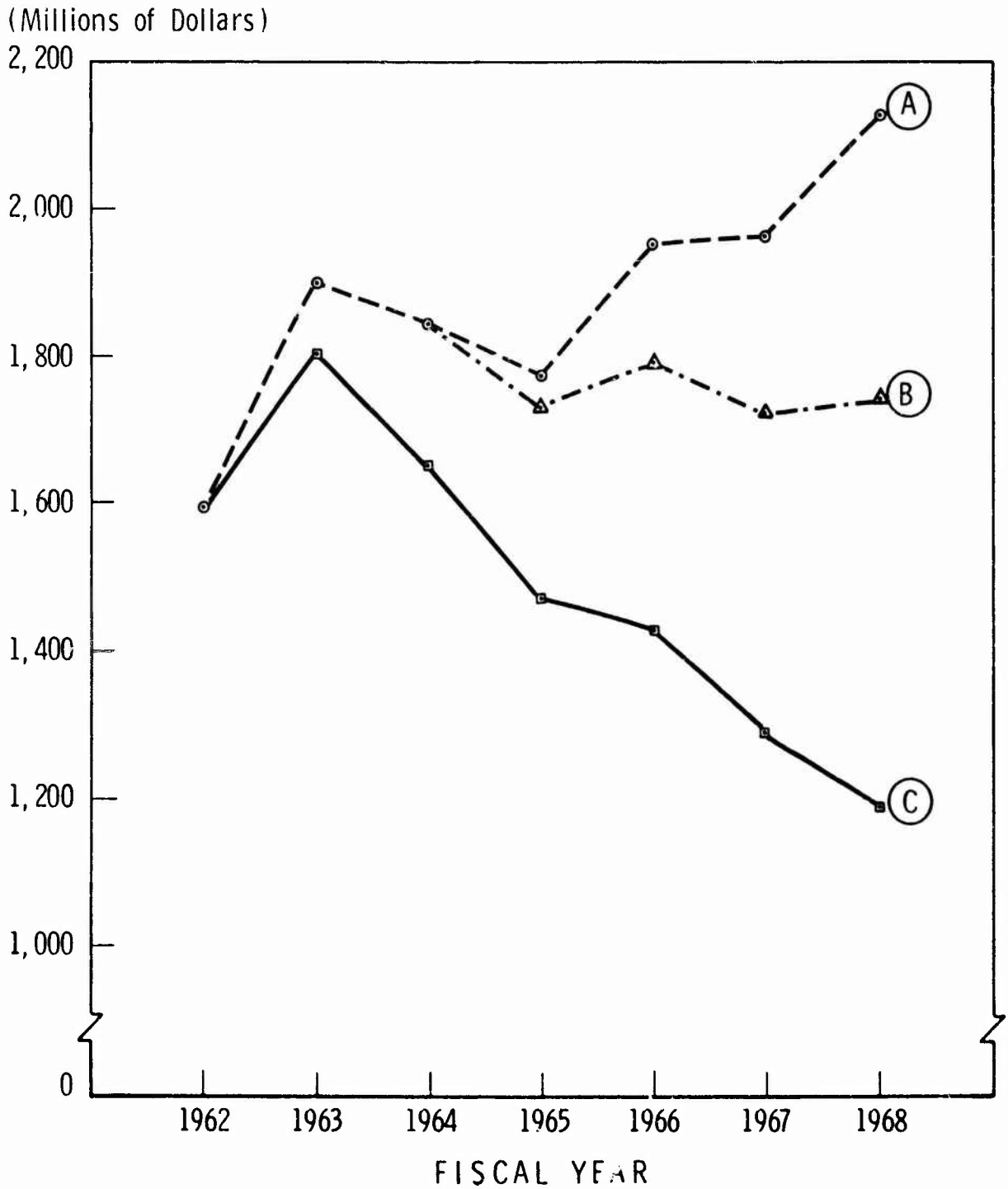
TOTAL EXPLORATORY & ADVANCED DEVELOPMENT EXPENDITURES

(NEW OBLIGATIONAL AUTHORITY)

Ⓐ - Total exploratory and advonced development (current dollars) (6.2 & 6.3)

Ⓑ - 6.2 & 6.3 less MOL (current dollars)

Ⓒ - 6.2 & 6.3 less MOL (dallar adjusted far 5% per year inflation)



III. IMPACT OF BUDGET TRENDS

How can one measure the impact of missing the boat? Suppose the U.S. had not supported work in molecular beams or in electronic processes in solids. It is hard to believe that the development of the laser or transistor at the hands of our competitors would have had the benign effect these developments actually exhibited on our defense capability. Of course one can always doubt that science will have the same "open-endedness" in the future that has characterized the path. We on this panel have no such doubts. Although such faith is intrinsically unprovable, Appendix B discusses some current examples that buoy one's faith. (b)

The effect of cuts in 6.2 funds can be assessed a little more definitely, although it cannot be separated entirely from excellence of management. (Better management could always be counted on to offset cuts, but are we not managing as well as we know how?) An example that happened to be accessible to us, the TF39 engine, is described in Appendix C.

The effect of decreasing 6.1 and 6.2 funds on the performers produces decreasing capabilities and other changes, which differ according to the nature of the performer:

1. Industry. The effects are obvious and presumably more severe the smaller the company, since a small company may have a larger fraction of its effort in a single contract and may have a more insecure financial base. We know of no companies that have collapsed, but several years may be required to produce the full impact of 6.2 reductions on contractor capability. (d)

2. In-house Laboratories. The effects here may be slight, partly because of the staffing and budgeting process and partly because of the House Appropriations Committee language (which asks for the bulk of the 6.1 cuts to come from universities). Cuts in these laboratories clearly would run counter to the current serious and promising attempts to improve their quality, yet the budget squeeze seems to us to be too severe to allow any group to be protected. We have little wisdom here but do suggest a highly selective approach, if necessary closing some laboratories, rather than making debilitating across-the-board cuts.

3. Universities. The impact of the 6.1 budget in FY 68 will be particularly severe in the universities presently funded by DoD. Of the approximately $100 million (exclusive of "Defense Agencies") of DoD contracts with universities proper, $9 million was extracted before the budget submission to provide increased THEMIS funding. As much as $12.8 million more will presumably be removed by Congressional action, and even further cuts may occur later. (e)

The rather casual language, frequently heard, suggesting that other Federal programs (OE?) will be able to make up for the reduction in DoD is quite without foundation. The cuts come in fact at a time when the man-years of science and engineering and the numbers of graduate students supported by other agencies are declining. NDEA fellowships are decreasing in number in FY 68; NASA traineeships are decreasing; NIH may have modest increases, but these will be of little help to "DoD fields"; AEC and NSF funds available for individual investigators are essentially static.

The cuts threaten to be so deep in some instances as to force current graduate students to interrupt their research training. The adverse consequences of this for DoD-university relations would last a long, long time. Recall that the universities are required to share costs in graduate training far beyond audited cost sharing. DoD contract support at a relatively steady level has been factored into university planning of academic staff and facilities. A major, abrupt change in these long-standing relations will be treated as a breach of "treaty" by university administrators. The implications for a cordial response to future DoD requests and requirements are ominous indeed. In the future, less university money will be likely to go into fields of DoD interest.

The DoD fraction of support of universities is now only about 2/3 what it was in 1962. The implications for the supply of DoD consultants and trained students in "DoD fields" are clear. An important conclusion from HINDSIGHT, detailed briefly in Appendix E, was the concentration of the training of major DoD contributors, a surprising fraction of whom had come from the universities with major DoD support.

It has become fashionable to excuse reduction of sponsorship of university research by stating that Federal support of such research damages teaching. This excuse, or argument, is

wholly specious. The proper function of a university is the education of students, both undergraduate and graduate students. Research is the essence of training graduate students, and Federal support at increasing levels is absolutely essential if the (e) quality of such training is to be maintained. A university with an exciting research program is also a more effective institution for undergraduate instruction in the sciences and engineering. The liberal arts colleges have decided advantages in the humanities, but they have produced fewer and fewer science majors in recent years who have gone on to successful graduate study. The problem has been, of course, that the students are not receiving the stimulating teaching of upperclass undergraduate science that is universal in universities with strong research programs.

* * * * *

There is a direct and particularly damaging impact on the DoD of 6.1 and 6.2 cuts through the impact on the program managers in ONR, AFOSR, ARO, and ARPA (referred to generically as "OXR"). The DoD is relying heavily on this special group for selectivity in its research funding, for establishing and maintaining healthy relations with universities and other contractors, for drawing contractors' effort together and making the "whole greater than the sum of the parts," for "coupling" science into component development and systems genesis, and for utilizing contractor R&D in Defense programs. When budgets are cut so deeply that "new starts" are impossible and the job of the program manager is largely to do cutting-by-formula, the whole successful OXR system is threatened. Cutting out whole programs would probably be preferable to percentage cutting when the latter is as deep as the combined DoD and House Appropriations Committee actions indicate for FY 68. (Elsewhere in this report we suggest a way of strengthening the position of the OXR program manager.)

IV. APPROACHES TO THE DETERMINATION OF 6.1 AND 6.2 FUNDING LEVELS

It is obvious that the level-of-effort in Research and Exploratory Development has not been and probably cannot be based on a quantitative theory. Yet several quantitative comparisons and approaches are possible, each with its own strengths and weaknesses. (c)

1. Comparison with Industry

There is no industry with as large stakes and as high risks as the Defense "industry," but nevertheless there is industrial experience that provides at least a semi-quantitative guide to 6.1 and 6.2 funding strategy. In order to compare DoD and industry figures, the only tractable approach is to use NSF figures.[2,3] NSF calls "Basic Research" about 75 percent of 6.1; it calls "Applied Research" the rest of 6.1, essentially all of 6.2, and a small fraction of 6.3.

The first point to be made is that DoD defines its character by its decisions on basic and applied research. (The Existentialists have taken credit for the idea that a man defines his identity by the choices he makes, but this is really an idea originated in Greek and early Christian philosophy.) Figure 3 compares the decisions on basic research by DoD and by the 20 companies in each of several fields that have the largest R&D expenditures. (The figures are nearly the same if one takes only the 4 such companies; the figures are different but comparisons among types of companies are the same if one takes median values for all companies, but these figures obviously include many small companies with no R&D at all.)

DoD has clearly chosen to behave (or has been forced by competitors to behave) like a company in a technically competitive industry, with a strategy to develop new equipment and processes and to make old ones obsolete in a short time cycle. Preserving this position requires about 0.5 percent or more of the total DoD budget to be basic research, which means 0.7 percent or more to be 6.1, which means about $500 million or more to be 6.1 in FY 68.

Figure 4 is a similar chart for Applied Research (essentially 6.2). Again, preserving the historical position requires more then $1,500 million in 6.2 in FY 68.

Figure 3

DoD BASIC RESEARCH PERFORMANCE

(COMPARED WITH 1964 DATA FOR SELECTED GROUPS OF MANUFACTURING COMPANIES WITH THE TWENTY LARGEST R & D PROGRAMS IN EACH INDUSTRY) Source, NSF 66-25 and 66-28
(Ref. 2) (Ref. 3)

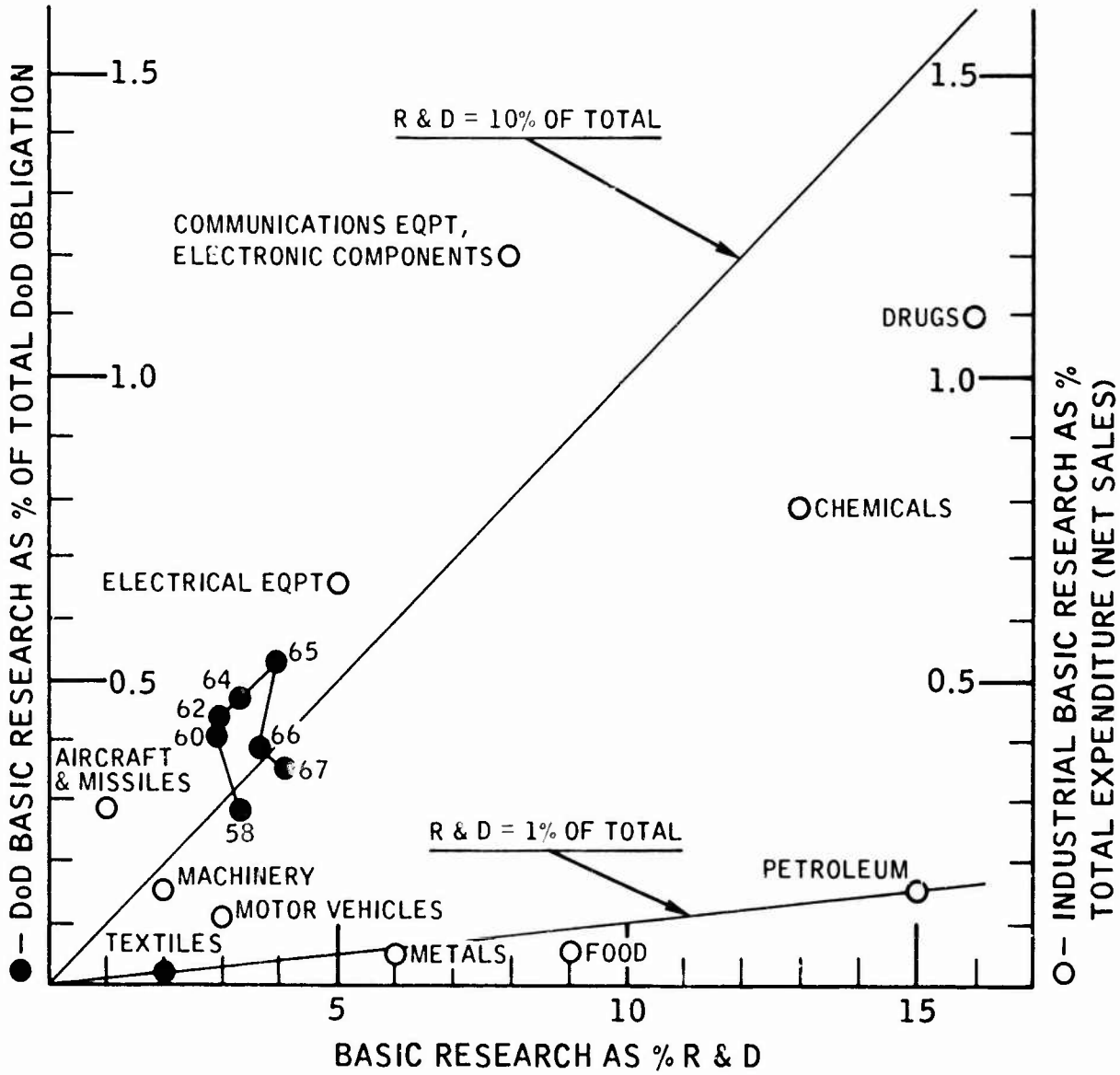
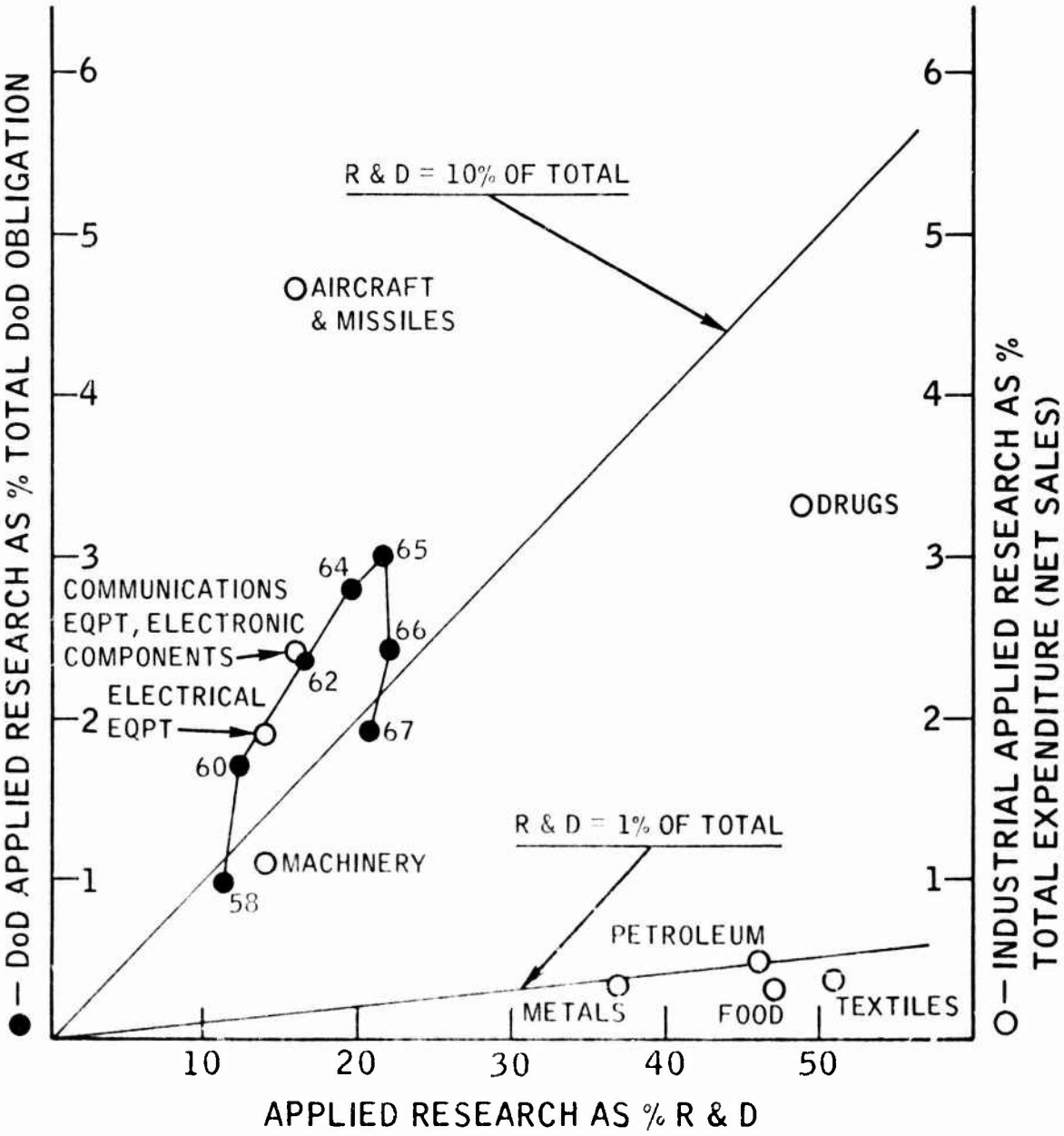


Figure 4

DoD APPLIED RESEARCH PERFORMANCE

(COMPARED WITH 1964 DATA FOR SELECTED GROUPS OF MANUFACTURING COMPANIES WITH THE TWENTY LARGEST R & D PROGRAMS IN EACH INDUSTRY) Source, NSF 66-25 and 66-28
(Ref. 2) (Ref. 3)



Obviously Figures 3 and 4 must be used with caution. Overall activity of DoD is measured here by the total budget, to compare with net sales of a company; this seems fair since the GI serves as "salesman" and "delivery force" for the "manufacturing" (procurement) part of the operation. "Communications," "Electrical," and "Aircraft and Missiles" include large DoD programs. The trends shown from FY 65 to FY 67 are obviously artifacts of the war; the point could be made, however, that even if the FY 66 and FY 67 points fell on top of the FY 65, DoD would be defining itself as static in the degree of technological involvement while the world becomes more technological each year. Table I gives some of the numbers on which these two figures are based, so that the reader can verify the relations between DoD categories and NSF labels.

Another approach, this time addressed to the derivative rather than to the value of the function, is illustrated in Figures 5 and 6.

Table 1. Data on DoD (millions of dollars)

	1958	1960	1962	1964	1965	1966	1967
DoD Nomenclature							
6.1			341	352	383	390	414
6.2			968	1182	1165	1131	960
6.1 + 6.2			1309	1534	1548	1521	1374
NSF Nomenclature (Source: NSF 66-25)							
Basic Research	111	168	204	260	263	273	298
Applied Research	378	693	1107	1511	1488	1643	1524
Basic + Applied	489	861	1311	1771	1751	1916	1822
R&D	3389	5761	6711	7272	6796	7421	7313
Total Obligation (Billions)	38.1	42.0	49.4	50.9	50.5	63.5	72.8

Figure 5

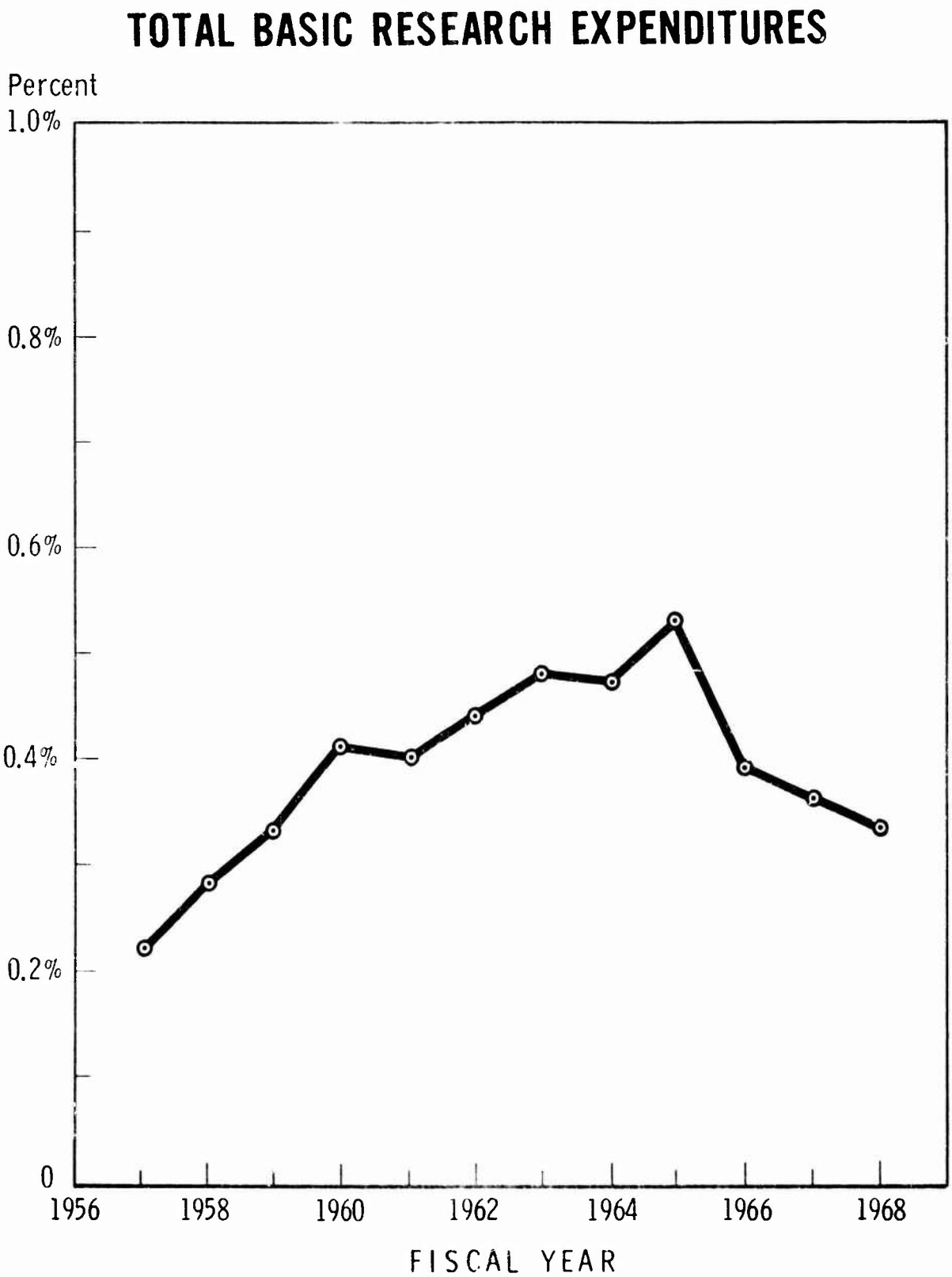
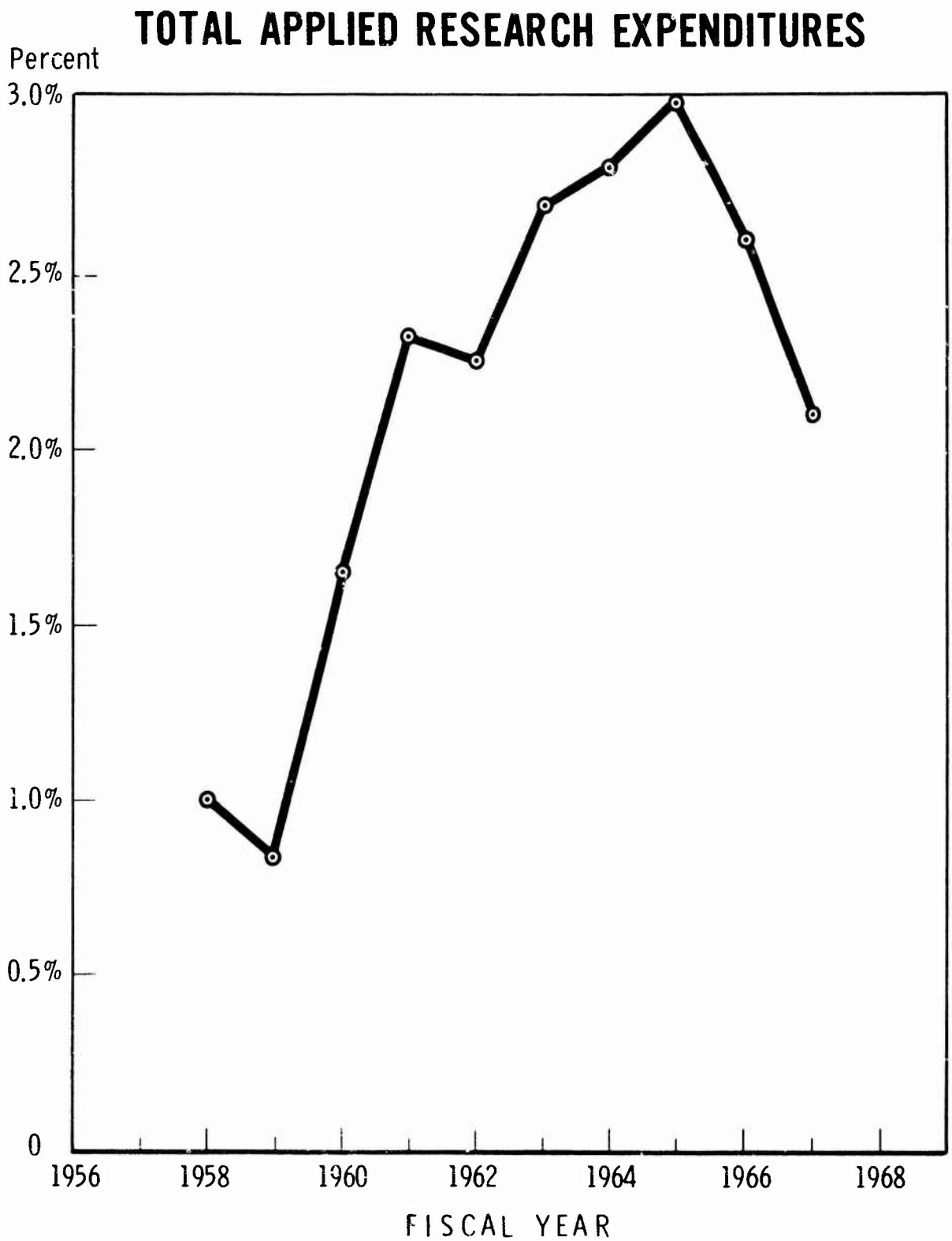

TOTAL BASIC RESEARCH EXPENDITURES
Percent
1.0%
0.8%
0.6%
0.4%
0.2%
0
1956
1958
1960
1962
1964
1966
1968
FISCAL YEAR

Figure 6


TOTAL APPLIED RESEARCH EXPENDITURES
Percent
3.0%
2.5%
2.0%
1.5%
1.0%
0.5%
0
1956
1958
1960
1962
1964
1966
1968
FISCAL YEAR

A case can probably be made from Figures 5 and 6 that 6.1 sagged seriously after FY 65 and that Program Package 6 as a whole (and 6.2 in particular) has been severely limited since 1961.

We do not contend that we have established hard facts or conclusions from this section. We have tried only to suggest ways in which a quantitative analysis of 6.1 and 6.2 funding can be achieved by reference to industrial experience. In addition, an organized distillation of the experience of successful managers of technological industries should be obtained.

The strength of the approach suggested in this section is the success of American industry, and tying DoD management to the management of industry is "standing on the shoulders of Titans." The weaknesses are that the most appropriate statistics are not available, that no industry is just like Defense, that DoD is so large that its policies strongly affect industry (leading to a certain circularity), and that the risks in national survival are more serious than the risks in corporate survival.

2. Comparison with the "Competition"

Another approach is obviously to consider Soviet and CPR (Chinese Peoples Republic) research and development funding policy, but the Panel did not have data necessary for such an approach. We do have a strong curiosity and anxiety however, particularly about CPR policy. Does the CPR defense establishment define itself as an "industry" like communications, with the rapid cycle time of "new products" and correspondingly large basic research budget? Without facts but with anxiety, all we can do is to recommend strongly that ODDR&E attempt a sophisticated appraisal of the growth (first derivative) and rate of change of growth (second derivative) of the quality and quantity of ChiCom R&D. For this purpose unclassified sources, such as British travelers, may turn out to be most useful.

Soviet competition is clearly a more current threat. Obviously many variables, most of them imperfectly known, are involved, but the burgeoning open Soviet technical literature gives evidence of continuing expansion of the Soviet research effort.

Strength of this approach: Competition with potential enemies is clearly the measure of DoD success. Weaknesses: Data are very poor; our whole economic system is different from our competitors; relative effectiveness of utilizing research must be estimated.

3. Health of Fields Underlying Defense

The continued health of such disciplines as physics, chemistry, and electrical engineering is important to the development of weapons systems. Physics (Pake report)[4] and chemistry (Westheimer report)[5] have been recently studied by NAS committees. Both reports recommended that Federal support of these fields increase at the rate of 15 percent to 20 percent per year for the next few years. The growths recommended by these committees are required if U.S. physics and chemistry are to maintain their relative positions within world science and if they are to continue to provide the research base that industry and the defense establishment tacitly rely upon. No single Federal agency, not even NSF, feels the responsibility for the health of such fields. Defense, with more than half of the total Federal budget, should provide its share of the core support of research in the fields that underly defense, namely, the physical sciences, engineering, mathematics, and (to a lesser degree) the biological sciences. If it does not, it is wholly unrealistic to expect the remaining Federal departments and agencies to be able to carry these fields on their backs.

Strengths of this approach: Clear importance of fields based on past experience and tacit expectations; growth rates required to maintain relative position are established with high precision. Weaknesses: DoD share is a less precise number, and conceivably DoD could successively be parasitic.

4. Production of Technically Trained People

Defense is reported to employ, directly and indirectly, 26 per cent of the engineers and scientists in the U.S. Both the quantity and quality of new engineers and scientists with advanced degrees are tightly tied to the Federal support of graduate research in universities. Federal research funding (and thus the DoD share) in universities therefore should grow at least at the rate of the growth of the total DoD budget; the stipulation "at least" is appropriate because the technical content and sophistication of the DoD is continually increasing, and its percentage of people with advanced degrees is rising accordingly.

Strength of this approach: The 26 percent figure is well known and firm and far exceeds the fraction of university research that DoD supports. Weaknesses: Again, DoD could probably get by with being somewhat parasitic.

* * * * *

In principle, one might be reluctant to call on scientists and engineers to determine a quantity in which they are as emotionally and selfishly involved as the funding level for 6.1 and 6.2. Yet we see no substitute for the quintessence of the judgment of sensitive research managers who have been intimately involved in the creative process. The path from a basic discovery or need to a new component or weapons system is a tortuous one, involving many collateral inputs from other science and technology. See, for example, the recent excellent Materials Advisory Board report on innovation, the Tanenbaum report;[6] or see another, shorter, but highly revealing paper on this subject written by R. J. Maurer ten years ago for the ONR.[7]

Not only is the path topologically complicated, but it frequently covers many years. Thus a quantitative theory of DoD research support would have to predict at least the mean time to time of use and would have to establish present value based on an assumed discount rate. Perhaps all this will be done some day, but we are confident that meanwhile the less quantitative judgment of experienced practitioners is an acceptable substitute.

These remarks apply especially to the allocation of support to the various fields of science and engineering. We propose below some moves that should increase the skill and confidence of the distribution of support among fields, moves that would strengthen ODDR&E and the "OXR" program managers and their relations with sources of judgment and information.

We should always be seeking quantitative systems for allocating funds among fields. In this connection, TORQUE seems like an interesting experiment, but only for 6.2 funds; it may be too mechanical to be reliable, but it should at least serve as a reminder to program managers of the considerations applicable to distribution of funds among fields. (c)

V. WORKING WITH CONGRESS

There is no doubt that Congress now looks with a jaundiced eye (a)
at programs, arguments, opportunities, and people that it viewed more (e)
rosily some years ago. There are so many causes for this that it is hardly worthwhile to analyze how we got to our present position. We try in this section only to provide some guidance to improve the appreciation by Congress of the contributions and opportunities in 6.1 and 6.2 and the central necessity for nourishing these seminal areas.

We believe that we are all too blase about research and its vast potential. A score of years of aging of the influential spokesmen, the emergence of systems analysis, the emphasis on costs and predicting the unpredictable, the occasional overselling of science by impressive promoters, the irrelevance of much sophisticated technology to the particularly difficult and annoying type of warfare in Southeast Asia, and doubtless other causes have taken the spirit out of the spokesmen for science and creative engineering. There is just as much reason for enthusiasm as ever before, and this should be manifest in our relations with Congress.

Formal linkages with Congress, even though already a burden on DoD professionals, need additional attention:

(1) Testimony by the DDR&E should increasingly emphasize the long-term importance of the research and technology base. The format should include attention to long-term potential of promising fields and should demonstrate the difference between "product-improvement" and "order-of-magnitude technological change." The DDR&E understands how much more difficult it is to convey an appreciation of the need for 6.1 and 6.2 than it is to "sell" some impressive hardware program. Unfortunately, there seems to be no alternative to devoting increased space in testimony and reclamas and increased time in hearings and corridors to the difficult problem of "selling" Research and Exploratory Development.

(2) Testimony by the Services should attempt to demonstrate the value of past R&D for the short-term-future hardware procurement and to involve project managers from the OXRs in giving vigorous reviews of ongoing research efforts. The OXR officials should include the "I was there, and I saw the exciting . . ." examples in their testimony wherever possible, which means more interaction of OXR officials with 6.1 and 6.2 performers.

(3) Congressmen and Senators who are sympathetic to the DoD position should be provided with "speech fodder" and supporting material so that when they have opportunities to speak, at home or in Congress, they do not lack substantive arguments about our progress, problems, and prospects. There is a plethora of available material, but it must be sorted and edited by literate technical people before transmitting. Every effort should be made to make it easy to incorporate such material in speeches, without requiring major editing.

Informal linkages are equally important. We believe that informal breakfasts or lunches with Congressmen, Senators, and their staffs should be employed far more than they have been as communication vehicles. The key example of such meetings would be the DDR&E, Deputy DDR&E(R&T), the particular Congressman or Senator, and frequently a scientist such as Charles Townes or Hans Bethe of unquestioned distinction and experience in DoD. Obviously, wide variation of format is possible, including when possible the Secretary or Deputy Secretary of Defense. Meanwhile, "in another part of the forest," meetings at lower levels, with Congressional staff, and with younger DoD-related scientists and engineers should take place. Clearly candor and continuity are essential. Admittedly the price in time and in limiting schedules is high, but the stakes are high and (as we have said) 6.1 and 6.2 do not "sell" themselves.

Another informal approach is to the electorate and the home town newspapers. The word should be passed to the private research community that DoD welcomes letters, phone calls, speeches, and private meetings by and from key spokesmen to the Congress. Complaining to the DoD must be matched by greater efforts in informing the Congress. The academic or industrial research worker should not stop with publication in the Physical Review or the Proceedings of the IEEE, but should tell his local press (with acknowledgement to his source of support), his sponsor, and his representatives in Washington.

There appear to be at least six central issues about which Congress needs continuing, clarifying education:

(a) The symbiotic relationship between education and research;
(b) The need for sustained and growing efforts on basic research problems;
(c) The dangers of applying geographical distribution formulae to R&D management;
(d) The inevitable impacts of budget reductions on in-house laboratories throughout the country;

(e) The (apparently) impossible problem of fully characterizing the "value" of 6.1/6.2 in "standard" cost-effectiveness procedures;

(f) The continued promise and "open-endedness" of science.

VI. FINDINGS AND RECOMMENDATIONS

We recognize the occupational diseases and risks of "outsiders" making recommendations. We are certain that at least some of the list below are vulnerable to the reactions that they are impossible of execution, that they are competitive with higher priority tasks of counter-production, or that "we are already doing it." Well, so be it!

1. The ODDR&E staff managing, interpreting, coordinating, and planning 6.1 and 6.2 programs should be expanded and strengthened in order to give these programs the support and critical attention they deserve.

2. As an especially important case of ODDR&E staffing and discipline orientation, a distinguished behavioral scientist should be added. The "community" of principal investigators in this field should be challenged (through a small ad hoc committee) to select such a man, with the alternative unspecified but obvious.

3. The trend from discipline orientation toward system orientation in the structuring of ODDR&E, in the analysis of 6.1 and 6.2, and in the budgeting process has gone too far and should be reversed. In particular, ODDR&E should be more effectively organized to generate strategies, to monitor the management, and to stimulate and supervise the exploitation of Research and Exploratory Development.

4. We recommend that ODDR&E prepare a quantitative analysis of basic research and exploratory development expenditures in DoD and in the most comparable American industries for a period of the preceding ten years. It is essential that a credible comparison of level of effort in these categories be readily available to DDR&E to facilitate planning and to provide a more meaningful method for estimating the level of funding for basic research and exploratory development. The current data are scattered among at least two publications by NSF and one by DoD with no clear basis for direct comparison.

5. ODDR&E should attempt to develop an improved quantitative rationale for the funding of 6.1 and 6.2. One approach might be based on comparisons with industry as outlined at the beginning of Section IV. Although this task has been done before (e.g., by NRAC some ten years ago) a new look should be taken by a small group of research managers.

6. ODDR&E should develop a continuing dialogue with the services and with the science and engineering community in order to rank in order the promising disciplines and fields for DoD support and eventual exploitation. Important as the output would be, the communication itself would also be productive.

7. At the end of Section III we emphasized the crucial role of the OXR program manager. ODDR&E should undertake to strengthen the positions of these people, in at least the following ways:

(1) Draw them into ODDR&E decisions on promising fields in much the same way as the in-house laboratory directors are being brought in.

(2) Give them the resources and encouragement to conduct regular topical conferences to assess the health and direction of Defense fields.

(3) Every effort should be made to funnel funds through these offices to reduce the depressing impact of budget cuts.

8. When new funds can be provided, new core contracts should be established to strengthen fields of DoD interest. The principal purpose should be to secure additional benefits from university programs at institutions where several principal investigators have or should be funded by DoD. In each institution, a "local manager" should be designated who would work with the DoD program managers, with DoD laboratories, and with other local managers in his field (this is the concept of the ARPA-IDLs, the THEMIS directors, and the Joint Services Electronics contracts, among others). No direction would be imposed by DoD, but in this way the "output" in research, students, and consultants should be more readily harvested than it can be from fragmented contracts. ONR, AFOSR, ARO, and ARPA each should have several of these "core contracts," and each agency should have at least one in each of the principal disciplines (physics, chemistry, etc.). The basic sciences should be funded from 6.1 and engineering from 6.2. Part of the aim of this program would be to extend the reach and to strengthen the position of the OXR program manager, just as the ARPA-IDL directors help and strengthen the ARPA Materials Office in "coupling," utilizing, and defending the programs. These cores should not be a substitute for individual contracts with individual principal investigators.

9. At least one core program in each of the OXRs should be in the behavioral science area, either in a single discipline or in an interdisciplinary association.

10. THEMIS and the other university programs should be treated together in budgeting and neither should be especially sacred. The original THEMIS explanation ("It is an addition to our existing university research programs, not a substitute for them."), while obviously sincere and sensible, now has a hollow ring. It is recognized that this is partly a problem of House Appropriations Committee language, but only partly so.

11. Relevance must always be an important criterion in the strategy of 6.1 contracting but it must not be confused with immediate applicability. Long-range relevance and quality should be the criteria in the next round of THEMIS and other 6.1 funding.

12. ODDR&E and the services should collaborate on a contingency plan of 6.1 and 6.2 programming to "refill the pipeline" of research and exploratory development. The opportunity may arise, and may even arise suddenly, to regain lost ground if the Vietnam war should be concluded, but advance planning will be essential in view of the pressure of other programs.

13. With reference to the Federal Contract Research Centers, DDR&E should establish a Planning Committee comprising the chief executive officers of the principal centers to work out plans for improving the programs, management, quality, and effectiveness of these centers.

14. The work of the Directors of Laboratories in the three services and of ODDR&E in consolidating and making more efficient the in-house laboratories should be strongly supported. This task will necessarily be complicated if the laboratories' budgets are cut, but the alternative of setting these labs out for special budget protection is not attractive. If cuts are made, they should be highly selective, emphasizing leanness and quality rather than immediate output.

15. The recommendations for interaction with Congress were spelled out in Section V. In brief, these were to put renewed spirit into the presentation of science, to develop the sense of long-term payoffs, to feature scientific and engineering discoveries in service presentations, to provide speech material, to develop further informal contacts at all levels using distinguished scientists and engineers, and to encourage the supported community to make its successes known and its needs felt.

16. The 6.1 and 6.2 parts of the RDT&E budget require and deserve much more space and time in presentations to Congress, in discussions with the services, and in discussions with and visits to

contractors. These interactions are intrinsically more difficult than the interactions in 6.3 and 6.4.

17. As a special case of relations with Congress, DoD should forcefully explain to the Congress its own view of university research and the effect of this research on graduate and undergraduate teaching. DoD has enormously strengthened American education by its contracting ("in the ONR tradition") and it should not be bashful. The ignorance outside the Pentagon of this process, of the trends toward more advanced education, and of the way high quality graduate study in the physical sciences and engineering is actually carried out is abysmal.

REFERENCES

1. The findings of several thorough studies of increasing costs of research and development vary from a low of 4.7% to figures in excess of 6%. Estimates based on individual experiences range upwards of 15%. We have accepted 5% as a conservative compromise. See also references 4 and 5.

2. Federal Funds for Research, Development, and Other Scientific Activities, Vol. XV, FY-65, 66, and 67. NSF 66-25, July 1966

3. Basic Research, Applied Research, and Development in Industry, 1964. NSF 66-28, June 1966.

4. Physics: Survey and Outlook. National Academy of Sciences, 1966 ("Pake Report").

5. Chemistry: Opportunities and Needs. National Academy of Sciences, 1965 ("Westheimer Report").

6. Report of the Ad Hoc Committee on Principles of Research-Engineering Interaction. Materials Advisory Board, 1966 ("Tanenbaum Report").

7. Solid State Physics Research, Performance and Promise. Office of Naval Research, October 1957, pp. 15-20.

APPENDIX A

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

June 30, 1967

MEMORANDUM FOR CHAIRMAN, DEFENSE SCIENCE BOARD

ATTENTION: Dr. R. L. Sproull
Chairman
Berkshire Study Group on Research and Exploratory Development

SUBJECT: Task Statement

Since FY 65 the effort supported in Research and Exploratory Development has been decreasing. In Research there has been a dollar increase of about 7% between FY 65 and FY 68, while the costs of doing business has increased by perhaps 16% or more. For Exploratory Development there has been a decrease of about 16% in current dollars since FY 64. I am most concerned by the fact that we lack satisfactory means for measuring the impact of these changes.

Therefore the principal goal of your study committee should be to examine various issues related to the "optimum" funding level for the 6.1 and 6.2 programs. Included, should be:

a. How to present our case to Congress in the fall in terms of specific arguments and specific impact indicators.

b. How to measure the impact of increases and reductions in funding.

c. How to determine an "optimum" funding level, and how to allocate resources among various fields (for example, using techniques such as TORQUE).

d. How to assess the impact of funding patterns on the industrial R&D base and especially on small businesses.

e. How to interpret the background leading to the recent Mahon report recommendation that research support to the Universities be curtailed; and how to estimate and influence the Congressional mood in the future.

f. How to establish clearly the principle that, while new "centers of excellence" are required, we must not discriminate against established centers.

I recognize that these are complex issues which merit lengthy study and that the Panel has limited time. Nevertheless, I would like to receive a short written report on each topic.

(signed)

John S. Foster, Jr.

APPENDIX B

by R. E. Norberg

The Promise of Research

It is clear that scientific research and exploratory development have had a significant impact on the defense posture of the nation. Nevertheless, recent fund allocations in DoD seem to reflect a view that perhaps this impact will diminish in future years. We wish to state vigorously our opinion that there is in fact no evidence of a tapering off of the promise of research advances.

For example, recent advances in the generation of very short pulses (in the 10-12 nano-second region) may imply radical improvements in future radar systems. As another case, consider the early DoD support of molecular beam research, which led to the development of masers. The DoD applications at this stage of the work seemed limited, but the maser work led ultimately to advances in the whole subject of "pumping" atomic systems to excited states. The field now once again holds promise of the ultimate development of very high power lasers, a goal with exceedingly important defense implications.

There are a variety of areas of current research which may well produce dramatic new DoD developments. A few such areas include: superconducting tunnelling, hypersonics in solids and liquids, high efficiency fission, geometrical distribution of impurities in solids, pattern recognition devices, coherent radiation phenomena, the physics and chemistry of surfaces, bionics and magneto hydrodynamics. There is no reason to believe that the next decade will not be marked by radical technical advances.

APPENDIX C

by R. S. Isenson

An Example of the Consequence of Inadequate Funding of Exploratory Development

The TF39 turbofan engine for the C5A aircraft is the most recent engine to be selected for inventory development. The design criteria for the TF39 were frozen in 1965 to allow for hardware development. The Air Force's exploratory development program, "Gas Turbine Technology," is a component program. Thus the adequacy of each component of the TF39 becomes a measure of the earlier relevant supporting efforts.

The TF39, as currently being developed, has a stage-and-a-half fan with solid blades and evidences slightly improved TF36 (1956) and XV5A (1960) aerodynamics. A single-stage hollow blade fan could very likely have been specified had enough work been done during the period 1961-1965 to demonstrate the feasibility of the latter approach. The criteria that would have allowed the design of a single stage hollow blade structure were first applied in an exploratory manner in 1959 in the Light Weight Gas Generator program. This program was abandoned after only ten hours of test time in 1961 when the funds required to complete the program were judged to be of greater value when applied elsewhere. (Note: Program review decision, 1961.) Attempts to obtain additional funding in subsequent years were unsuccessful, and the program lay dormant. Desired funding was at a level of about one million dollars per year.

In the absence of the single-stage hollow blade technology, the weight of the C5A initially will be 30,000 to 40,000 pounds greater than otherwise necessary. At a production cost approximately $100 per pound for an aircraft, the five year, five million dollar investment in exploratory development would have been returned with the production of only a few aircraft. Further, because recent technical investigations of the hollow blade have demonstrated feasibility, it is likely that a second engine development program will be undertaken to permit eventual retrofit of the C5A. Thus, the total cost of the insufficient exploratory development program is seen in the increased production costs of the initial buy of the C5As and in the expense of an additional engine development program.

The argument that the described situation is as easily ascribable to a bad management decision as to inadequate funding is true but not

interesting. The decisions were made by responsible managers who, by and large, are currently making other decisions as to what will and what will not be supported in Exploratory Development. To the extent that their decisions are less than optimum, the decision making system is less than optimum. Of course DoD will always strive for better management. But it is wholly unrealistic to assume that better management will offset budget cuts.

APPENDIX D

by R. W. Cairns

Funding Levels and Planning of 6.1 and 6.2 Programs

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. To treat the latter phases effectively, one must understand the relationships and goals of mission-oriented RXD work. In the first place, it is recognized that research will provide scientific progress which may turn out to be applicable to the development of superior military capabilities. Conversely, it is most difficult to realize significant improvements in our present capabilities without relying in part on new scientific results that have not previously been applied.

Our present military capabilities depend entirely on basic scientific discoveries of the past. Because the period of development and successful engineering needed for the exploitation of these discoveries is measured in years, it is easy to undervalue this dependence. The non-scientist takes for granted the basic science behind a bomb or space vehicle because it has been revealed for a decade or longer. Similarly, he undervalues the practical importance of lasers and theoretical chemistry because such breakthroughs have yet to be developed into useful equipment or materials. Chances are that these basic discoveries will in turn become "old hat" when their practical benefits are finally revealed after a long time span.

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 where the participants look to technology of their own achievement as an essential contribution to success. Failure leads to disruption or decay of the competing organizations, yet the cost of the effort must be carefully weighed in order to avoid unnecessary dissipation of funds.

The importance of R&D to industry varies widely, but 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 both on a proportional and on an absolute basis (see Figures 3 and 4), as well as the largest proportions of company vs. government funding.

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 answer to this question is that we should spend as much as we can for Research programs relevant to the Department of Defense. Before overreacting to this bold statement, one might consider two viewpoints:

1. 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. The likelihood of such a disaster hitting us is best offset by our maintenance of world leadership in science judged relevant to military needs and applications.

2. Research cannot be turned on and off to suit the convenience of budget planners, without endangering its quality. The research organizations of industry are carefully fostered and stimulated through thick and thin. 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, references 4 and 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. A comparison of recent data on DoD research spending indicates an impending crisis and a serious underfunding in basic research relevant to DoD objectives. The funding level has been

only 5 percent of the total R&D program, and virtually static since FY 1965 ($409 million vs. $383 million), not allowing even for the effects of cost inflation during this three-year period.

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. In contrast, pursuance of the present course of action may be disastrous to our technological weapons superiority 15 or 20 years hence. Otherwise we must assume that the closely managed economies of Russia and China are not directed toward an all-out drive for scientific breakthroughs applicable to enhancing their own weapons effectiveness. We have repeatedly underestimated the capabilities of the communist countries in technology. Are we to repeat this serious mistake in the field of basic science? If so, the scientific gap that ensues will be very much more serious, and the cure much slower, than the technological gap we faced with the launching of Sputnik.

Applied research is the next step after basic research in exploitation of the new-found fundamentals on the scientific front. To a certain extent, the amount devoted to this type of activity is determined by the availability of freshly discovered scientific facts. It also depends on the application of innovative skills to the practical utilization of the new scientific revelations. Analysis of 6.2 funding in recent years indicates a pinching-off of funding for applied research, (or Exploratory Development in DoD parlance, see Table 1 in Section IV for a comparison of NSF and DoD terminology). The level is $923 million for FY 1968 vs. $1.165 billion for FY 1965, a diminution of over 30 percent if one takes account of inflation at 7 percent per year. The false economy revealed in these figures will inevitably affect our technological superiority in future weapons development. Industry as a whole spends 20 percent of its R&D dollar on applied research (see NSF 66-28). How then is it possible for DoD to get by with an amount under 15 percent of its total R&D expenditure for its allocation to this critical step in the research sequence?

An even more astonishing situation is revealed if one analyzes the distribution of 6.2 funds according to performer. Formerly, there was approximately an equal sharing of these expenditures between government in-house laboratories (including Federal contract research centers) and industrial contractors. As a result of reductions in total 6.2 funds and the lack of any policy of retrenchment in the government

labs, industry's portion has shrunk to a bare 10 percent.* Consequently their primary source of innovative research activity leading to promising new weapons development has been seriously curtailed. The only alleviation has been the availability of IR&D funding for a selected group of large contractors. It appears that an increase in 6.2 funds for industrial contractors of at least $500 million per year would be the minimum change necessary to restore a healthy encouragement of innovative research toward superior weapons technology.

Increases in funds for Research and Exploratory Development totaling about $1 billion/yr. over a period of several years would provide a balance of types of R&D effort close to that of highly competitive industries that are (like DoD) technology-dependent. The effect of such enhancement of research inputs to development would undoubtedly be to provide for much-needed new weapons systems and superior weapons performance in the next decade or two. Failure to realize the gloomy consequences of the economy moves of recent years on 6.1 and 6.2 funding will likely lead to a loss of U.S. technological superiority vital to world leadership.

Since the sequence Research-Exploratory Development - Advanced Development-Engineering Development has a built-in time span of about 10 years, it is hard to relate input to outcome at a given point in time. It is possible to relate research effort to ultimate weapons performance on a project-by-project basis over the whole period of time, looking backwards. Invariably, it appears that the basic and applied scientific inputs on which a new weapons system depends, turn out to be inadequate for reliable and effective performance. Consequently, the state-of-the-art must be enhanced during the development phases, in order to meet targeted performance and cost levels for the new system. By and large, industry tends to provide a broader base of science for its own commercial developments in order to avoid the embarrassment and cost of development delays resulting from a shortage of basic information. Likewise in DoD R&D, we must provide for flexibility of design and a margin of error in calculating ultimate performance, with particular regard to degradation effects. Recent trends toward economizing on 6.1 and 6.2 funding are unfortunately in the opposite direction. It is easy to predict that this short-sighted policy will result in development delays, disappointing equipment performance, a lack of innovative inputs for new devices and serious degradation effects in military use. However, these bad effects are all some years down the line, and hence obscured from present vision. HINDSIGHT 1975 is the best way to measure the impact, but it will then be too late to act.

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*Navy Program Analysis, March 1967.

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, developed to protect against a massive scale European-type war, 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. Instead of having a hard look at the present imbalance and making serious efforts to correct the planning deficiencies, we are prone to wring our hands and set everyone to work on a crash patchup of existing equipment. In 1940, we did not neglect basic research on atomic fission and aerodynamics, even under much greater military threat and with nothing but obsolete weapons in the military establishment. Today, we whould not sacrifice our scientific effort for the obvious needs in the field of operations, to the detriment of our military prowess 5 or 10 years hence. Our foresignt in 1940 led to an invincible military force in 1945. Continued dedication to a balanced R&D program is a must under any conditions. If we cut back on our long-range programs now, we are merely prolonging the adverse effects of the Vietnam conflict into the next decade.

APPENDIX E

by R. S. Isenson

The Contribution of Universities to DoD Technology

The graduate research program of the established universities produces scientific results relevant to the needs of DoD. However, the principal product of the program is people.

In DoD's HINDSIGHT study, 1,725 scientists and engineers were identified who long after leaving school had contributed significantly to defense utilized technology. Of these individuals, 10.5 percent had Ph.D. degrees and 22.5 percent had M.S. degrees. We believe it important to note that 50 percent of these Ph.D. degrees were awarded by 23 specific universities although these universities have awarded only some 25 percent of all Ph.D's. in the U.S. Further, although these 23 universities awarded only 11 percent of the M.S. degrees in this country, 46 percent of the identified contributors received their degrees there. The significant factor is that these 23 universities are and have been at the top of the list of universities doing DoD research. In brief, we observe a remarkable correlation between DoD support of university research and university production of scientists and engineers who continue to be most important to DoD. We have not attempted to explain the observations but caution against any action that could, for the future, impair what clearly has been a mutually profitable relationship.

APPENDIX F

by J. H. Wakelin, Jr.

Impact of Budget Reductions in Studies and Analyses in the Not-For-Profits and the Federal Contract Research Centers

The work of the not-for-profits and the Federal Contract Research Centers includes many fields of research, management and analysis as follows: (1) systems management and planning; (2) management studies, analyses and techniques; (3) research on design and development of weapons systems; and (4) studies and analyses in logistics and in personnel utilization, and on strategic and tactical systems for offense and defense. Yet, Congress has made no distinction between these organizations and has in its budget reviews considered that they are essentially similar. The budget cuts in RDT&E in the line items for studies and analyses total $11,500,000 or more than 25 percent of the $44,900,000 requested. In operations and maintenance the budget cuts total $12,290,000 or about 44 percent of the $27,535,000 requested for management studies. While some reprogramming may be possible the effect of these cuts means a drastic revision of the programs in organizations doing studies and analyses and in many cases a staff reduction will result. This will certainly reduce their effectiveness as contributors to Defense.

The Budget report emphasizes fiscal management issues in the administration of the not-for-profits and the Federal Contract Research Centers and makes a general statement that there are too many studies and analyses made which do not appear useful to the conduct of Defense business. On the first point, it does not appear logical to reduce the scientific and technical capability of these organizations purely on the grounds of fiscal practices. Such transgressions can be treated by the improvement of management practices quite apart from the technical program. On the second point the level of studies has most certainly increased and there may be areas in which studies are made that do not have a direct impact on the Defense program. If this is true then it holds for the in-house facilities as well as the contract operations. It is suggested that a much more hard-headed analysis be made of the study and analysis programs to the end that they are responsive to real needs of the Services and OSD. While the character of some of the programs is such as to be appropriate for transfer to the Services this will be difficult in the face of personnel ceilings and the lack of appropriate billets for the personnel to do the work under Civil Service. In the same Budget report, Congress has made substantial cuts in the civilian personnel requested by the Services. In

the case of the Army the Committee recommended a reduction of 11,000 in the 23,373 civilian personnel requested.

If cuts are to be sustained they should not be made across the board but by studied selection on the basis of value and quality of the programs in studies and analyses. The following points should be considered in the reduction of effort:

(1) Redefine or clearly state the need for quality and the importance to Defense of the programs in these organizations.

(2) The sponsoring agencies should make at least a yearly review of these programs and their value to the user. This can be done through visiting committees of knowledgeable people who will look at the programs in an objective way. Visiting committees have already been established for the Applied Physics Laboratory (Johns Hopkins) and for the Center for Naval Analyses. These committees are to concern themselves with the quality of performance and the programs in these organizations.

(3) The Boards of Directors should play a more effective role in the program content of their organizations and there should be a closer relationship between Board members and the highest echelons of the Military Services and the OSD who are the users of such studies and analyses.

(4) In order to obtain programs that make maximum use of the skilled personnel in these organizations, plans should be developed for the transfer to the Services and to OSD of those functions which can just as well be done by personnel in Civil Service.

(5) In the programs that remain with these organizations, greater latitude should be given in the areas of research and creative work along lines and in the fields of interest to the sponsoring agencies. The Services have not always given those organizations the type of project or program which is a significant task to the user. Many organizations as an accommodation to their sponsor have taken quick reaction capability jobs to an extent that has made serious inroads in their longer-range or more important tasks.

(6) A concerted effort must be made to interpret and explain to Congress the nature of the work performed in these organizations and why it is important to the Defense Department. This can be done on a case-by-case basis to indicate the character and the scope of their contribution to Defense research and planning.

BUDGET REDUCTIONS BY THE MAHON COMMITTEE
(FY 1968)

1. Studies and Analyses

Operations and Maintenance	Requested	Reduction
Management Studies (Services)	$25,100,000	$10,920,000
Management Studies (Defense Agencies)	2,435,000	1,370,000
Total	$27,535,000	$12,290,000

RDT&E

Studies and Analyses		
Army	$15,000,000	$2,000,000
Navy	9,000,000	3,000,000
Air Force	10,000,000	700,000
Defense Agencies	10,900,000	1,900,000
Related work in ARPA programs for psychological and social science research, information processing, technical studies and AGILE	($60,000,000)*	3,900,000
Total	$44,900,000	$11,500,000

*Not considered to relate totally to studies and analyses.

2. Federal Contract Research Centers

RDT&E	Requested	Reduction
Army	$ 15,900,000	$ 1,000,000
Navy	59,300,000	2,300,000
Air Force	152,640,000	5,200,000
Defense Agencies	36,709,000	1,500,000
Total	$264,549,000	$10,000,000

APPENDIX G

by R. S. Isenson

Relevance and Immediacy

We fully agree that the DoD or any other Federal agency, should order the research it supports in terms of both relevance and scientific opportunity. However, the interpretation that has been assigned to "relevance," as we can infer from Congressional and other testimony, appears to include a sense of immediacy. Particular care is commended to the DDR&E to counteract such an interpretation.

We believe that the layman is far more capable of understanding scientific objectives than it would appear from attitudes implicitly expressed by many individuals from the scientific community. We suspect, for example, that analysis of the following sort can usefully be shared with him.

1. In the sensor field, particularly radar, further simple advances in sensitivity are precluded by natural limits yet better operational characteristics are required. It is clear to us that improved characteristics will be achieved; we suspect that the advances will come through a combination of filter theory and target signature phenomena. Yet, we cannot specify today exactly what characteristics of the target signature are most useful for identification or classification. Only many, many weeks or years of careful scientific investigation may solve this problem.

2. We know that the theoretical, inherent strength of materials is far greater than currently available materials suggest. Only imperfectly do we understand why or how best to improve the situation. The military potential of higher strength is obvious. As in target signature analysis, only extensive and intensive scientific investigation holds promise towards achieving the very desirable objective.

We believe that the goals and value of science can be usefully explained to the Congress and to the layman in terms such as these. We believe that armed with this understanding, the willingness to fund these activities will be markedly enhanced. Finally we commend such an approach to the DDR&E.