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ALLOCATION OF RESOURCES FOR BASIC RESEARCH IN A MISSION ORIENTED GOVERNMENTAL AGENCY

Burton V. Dean, Samuel J. Mantel, Melvin Brown, Donald C. Friedel and David Huettner

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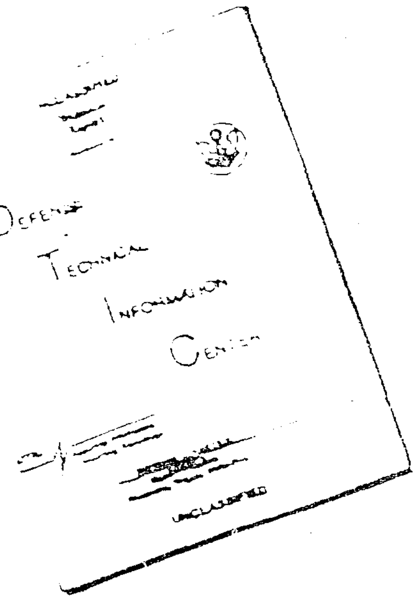

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A study is made of the process by which basic research is funded in a mission-oriented agency of the federal government, with particular attention to the Office of Naval Research. To provide a rationale for improved funding procedures, a model is developed to associate a value with a research portfolio. Two models are developed, the first in the case where funding is made on the basis of supporting science per se, the second corresponds to the case where funding is made on the basis of mission-relatedness. An optimum portfolio would maximize the combined value of mission oriented and science oriented research subject to budget and other constraints.

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I. INTRODUCTION

This paper constitutes a final report of a research study performed under ONR Contract Nonr - 1141(19), entitled "Quantitative Methods in Research and Development Management." The purpose of this research has been to improve our understanding of the nature of funding of basic research by a mission-oriented government agency, and to help provide a rationale for more effective funding in the future.

The study has concentrated attention on research funding by the Office of Naval Research with emphasis on the development of models for resource allocation which incorporate uncertainties in costs and payoffs, multiple approaches to the solution of research problems, long term investments in research, and diffusion in the applications of research results.

The research element in this study is a concern with the application of cost effectiveness methods to the allocation of resources to research activities. By a <u>research activity</u> we mean [1]

> A period of time encompassing a specific definable research effort. It starts as the result of a decision to pursue a select course of action or as part of a sequence of related actions. It ends when an identifiable output of the research effort has been produced. This output can be a new chemical reaction, a new material in laboratory quantities, or a circuit design. It can either result in utilized or unutilized research. It is unutilized when it fails to survive the transition to technology, or, having successfully transitioned to technology, is never used in any mission application. The output of a Research Activity can also be an investigation and discard of an alternative approach to a program objective. This is obviously unutilized research, although still significantly contributive to the research effort as giving direction to the program.

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[1] Witmer, B., Memorandum, "Research Activity/Event", <u>ONR Cost Effectiveness Study</u>, February 14, 1967.

President Lyndon B. Johnson has indicated the major benefits to be received from basic research when he said:[2]

"We know that we can continue the flow of benefits to mankind only if we have a large and constantly replenished pool of basic knowledge and understanding to draw upon."

However, while basic research, in general, is beneficial to our society, and in particular to our defense position, over extended use of cost-effectiveness methods without testing and in an unwarranted manner must be guarded against. Dr. Leland J. Haworth, Director, National Science Foundation has indicated that support and interest in the applications of basic research in the short run is dangerous, and in fact [3]

"...great caution must be practiced in this area lest attempts to mold basic science in the direction of immediate usefulness not only hurt basic science itself, but also, at least in the long run, thwart its very purpose."

A second concern with the over extended use of cost-effectiveness in basic research has been raised by Dr. Donald F. Honig, Special Assistant to the President, and Director, Office of Science and Technology, who has suggested that benefits of basic research cannot only be measured in terms

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[2] Letter sent to Congress, dated April 6, 1967 upon submission of the "16th Annual Report of the National Science Foundation," NSF Publication 67-1, US Government Printing Office, Washington, D.C.

[3] Ibid.

of practical applications[4]

> "...unfortunate instances of efforts to mix the two and to warp basic research projects in the direction of application--or even to judge basic research projects not by the standards of scientific excellence but by the likelihood of practical advance. This we are trying to change. We are trying to get clear recognition that even when basic research is supported by a mission-oriented agency, its role is to build up the basic reservoir on which applications will rest rather than to define an application supporting the mission in each and every project..."

At the present time although research is increasing, the rate of growth in research has been checked; however, the increases in research are comparatively larger than in development effort. This is indicated in the following table which shows funding during FY64 - FY69 period for Research, Exploratory Development, and total R&D, as provided by the Office of the Director of Defense Research and Engineering, Office of the Secretary of Defense, 24 July 1968.

DOD Summary Research and Exploratory Development Funding
(R&D in $ Millions)

	FY 1964	1965	1966	1967	1968	Request 1969
Research (6.1)	353	383	389	413	371	450
Exploratory Development (6.2)	1158	1128	1134	1042	948	980
Sub-total	1511	1511	1523	1455	1319	1430
Total DOD R&D	7608	7008	7480	7835	7959	8541

[4] "Honig on Research Policy", SCIENCE, 5 May 1967, p. 629.

Considerable concern is raised over the amount and allocation of basic research funds, for as Alvin Weinberg has said

> "We have decided that sending a man to the moon is worth $5 billion and that achieving better health for our society is worth $1 billion. I think that happens to be a wrong allocation between these two objectives ..."[5]

Currently, the incremental budgeting practice of basic research resource allocation generally has been to use subjective estimates on the relative values of newly proposed basic research projects and tasks, and to minimize incremental expenditures by maintaining a relatively constant level of effort over a period of time.[6]

This practice is clearly preferable to the so-called "zero-base" budgeting method which considers the allocations for each year as an absolutely new problem. In addition to insuring continuity in the general research program of ONR, the solution of the funding problem at the margin results in a fairly high degree of stability in the allocation of research funds. This stability, in turn, allows reasonably accurate forecasts to be made on the manner in which future research budgets will be allocated to individual line items and research work units.[7]

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[5] "Basic Science in Mission Oriented Endeavor", Meetings, SCIENCE, Vol. 156, May 5, 1967, p. 672.

[6] See "Costs of Naval Research Projects", RDT&E Planning Report, Spring Submission, June 1966.

[7] See B. V. Dean, S.J. Mantel, Jr., Lewis A. Roepcke, Mary Green, and J. Svestka, "Research Project Cost Distributions and Budget Forecasting," Technical Memorandum No. 107, Department of Operations Research, Case Western Reserve University, Cleveland, Ohio, June, 1968.

In spite of these advantages, however, there is no assurance that this method of resource allocation is rational or approximately optimal, although it is practiced by many public and private organizations.

This report presents an approach to providing a quantitative basis for basic research planning, programming and budgeting. Our concepts evolve out of the need to justify some fraction of mission oriented basic research on a cost-effectiveness basis within the general framework of Department of Defense decision making.[8] However, the establishment of the value of the ratio is, in itself, an agency decision parameter.

It is most important to note, at this point, that the existence of a model for the allocation of resources among various research projects does not imply that it would be desirable to implement the output of the model without considerable modification. The models presented below, like almost all symbolic representations of complex systems, are partial models. While it is hoped that the models are good representations of certain basic relationships, no model could possibly present a complete picture of all the forces impinging on ONR funding decisions. The modelling of some of the basic elements of the funding decision will allow the agency to devote more time and effort to a consideration of those elements which cannot be quantified.

The Office of Naval Research has enjoyed the highest esteem of the scientific community for more than two decades. American scientists have not only looked to the ONR as a source of research funds but have also

[8] See Hitch, Charles J. Decision-Making for Defense, University of California Press, 1965.

responded to the agency's direct stimulation of research through dissemination of ideas and attitudes of the foremost researchers in a number of scientific fields.

Until quite recently, the ONR has been the principal distributor of federal research funds in most of the scientific areas under active study in this country. The emergence of such agencies as the National Institutes of Health, the National Science Foundation, the Army Research Office, among many others, and the proliferation of privately supported foundations such as Ford and Rockefeller have led to a desire on the part of most research funding groups to specialize on areas of research of particular interest to the individual agency. Further, the growth in the number of groups supporting research has intensified the competition for the funds with which to finance projects. This competition has been severe, particularly affecting those agencies which look to the federal government for financial support.

In the case of the ONR, these forces have resulted in a sharply increased emphasis on "mission-oriented" research. Scientific investigations which are more or less directly related to Navy missions and Navy problems can be "justified" by the Navy before an economy-minded Congress. The need for "justification", has, in turn, led to the need for ONR to gain an improved understanding of scientific activity and its relation to future Naval technology and activity. In addition, the emphasis on "justification" requires a better understanding of the processes by which basic research can be effectively administered and controlled.[9]

The concept of "mission-orientation" must be taken in the context of a specific research agency. The ONR is primarily concerned with "basic" research, that is, research conducted with a prime objective of contributing to the body of knowledge in a scientific area. To the extent that such a contribution to knowledge can be related to the Navy's mission, the research is called "mission-oriented." The ONR, however, also supports "science-oriented" research. Here, the relationship between research and mission is less clear or is stretched over so long a time horizon that the major impact of the research is felt to be on the science rather than on the mission.

The ONR views both science and mission related research as proper uses for its funds, but it is clear that the distinction between mission and science orientation cannot be cleanly drawn. This report considers the management of both kinds of research in the context of a decision problem, the purpose of which is to select research projects for funding. This context provides a natural setting to illuminate many of the salient features of both research administration and the relationships between basic research and Naval technology.

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[9]These comments as well as those appearing below were expressed by several ONR personnel in interviews and discussions with the authors.

It is to the formulation and solution of the research funding decision problem that this report is primarily directed. An underlying assumption of our approach is that the basic organizational structure of the ONR does not change in such a way as to produce a major alteration in the general methods of research funding within ONR. In particular, the list of Naval research project areas, as indicated in the following table, is the portfolio for future planning, programming, and funding.[10]

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[10] "Costs of Naval Research Projects", RDT&E Planning Report, Department of the Navy, Spring Submission, June 1966.

Table Naval Research Project Areas

I. Chemical Sciences

A. Physical Chemistry
B. Chemical Chemistry (Non-metallic)
C. Organic Chemistry
D. Inorganic Chemistry
E. Analytical Chemistry
F. Fuels and Propellants
G. Solid State Chemistry

II. Physical Sciences

A. Instrumentation
B. Solid State Physics
C. Atomic and Molecular Physics
D. Nuclear Astrophysics
E. Elementary Particles
F. Plasma and Ionic Physics
G. Theoretical Physics
H. ASW Acoustics

III. Mathematical Sciences

A. Theoretical Mathematics
B. Applied Analysis, Theoretical Mechanics, Mathematical Physics
C. Numerical Analysis
D. Mathematical Statistics and Probability
E. Theories and Techniques of Logistical Analysis and Decision Making
F. Theories and Techniques of Information Processing
G. Information Processing Systems and Devices
H. Mathematical Topics Relevant to Specific Military Problems
I. Basic Methodology in Systems Research

IV. Earth Sciences

A. Earth Physics
B. Atmospheric and High Altitude Physics
C. Interagency Committee on Oceanography (ICO Staff)
D. Geography
E. Astronomy and Astrophysics
F. ASW Oceanography
G. ASW Arctic Research

Table - Naval Research Project Areas (continued)

V. Biological and Medical Sciences

A. Immunology and Hematology
B. Regulatory Mechanisms
C. Regulatory Physiology
D. Microbiology
E. Biochemistry
F. Hydrobiology
G. Biophysics
H. Biological Orientation
I. Ecology
J. Cellular Biology
K. Biological Countermeasures
L. Chemical Countermeasures

VI. Psychological Sciences

A. Sensory Mechanisms
B. Neural and Perceptual Processes
C. Motor Mechanisms
D. Psychological Traits
E. Selection Methods and Performance Criteria
F. Learning and Training
G. Individual Effectiveness
H. Group Effectiveness
I. Engineering Psychology

VII. Material Sciences

A. Physical and Mechanical Metallurgy
B. Ceramics and Related Inorganic Skills
C. Organic, Polymeric and Fibrous Materials
D. High Temperature and Special Materials
E. Surface Phenomena, Corrosion and Prevention

VIII. Electronic Sciences

A. Physical Properties of Solids and Cases
B. Radio Astronomy and Astrophysical Studies

Table - Naval Research Project Areas (continued)

IX. Engineering Mechanics

A. Hydrodynamics
B. Aerodynamics
C. Structural Mechanics

X. Energy Conversion

A. Single-Step Energy Transfer
B. Multistep Energy Transfer
C. Energy Utilization

XI. General Sciences

A. Navy Laboratories
B. Contractor Laboratories
C. Interdisciplinary Research

II. IDENTIFICATION OF THE PROBLEM

A. ONR as a system

Basic research is funded by ONR through a process that involves simultaneous flows of scientific, technological, and (military) intelligence information, and funds. Involved in these simultaneous interchanges are a number of organizations and individuals. A schematic representation of these flows is presented in Figure 1.

Figure 1 represents the major paths by which ONR can interact with the scientific community, obtain and transmit information on the current state of activity in various sciences, and help to support scientific activity through research contracts.

Agencies responsible for analyzing and predicting the probable future Naval environment and needs (e. g., the Center for Naval Analyses) provide ONR with realistic goals to be met by a future Navy. Contact with a realistic Naval environment is further assisted by frequent interaction with line officers of the Navy, a number of whom are at any time assigned to ONR itself.

Naval laboratories, other government laboratories, and private industry help ONR to determine what Naval capabilities it may reasonably expect to be developed in future time periods.

Several of the communications channels shown in Figure 1 are two-way channels, with the ONR communicating Navy needs to external research communities who react with research proposals. Figure 1 is not meant to be

BASIC SYSTEM DESCRIPTION

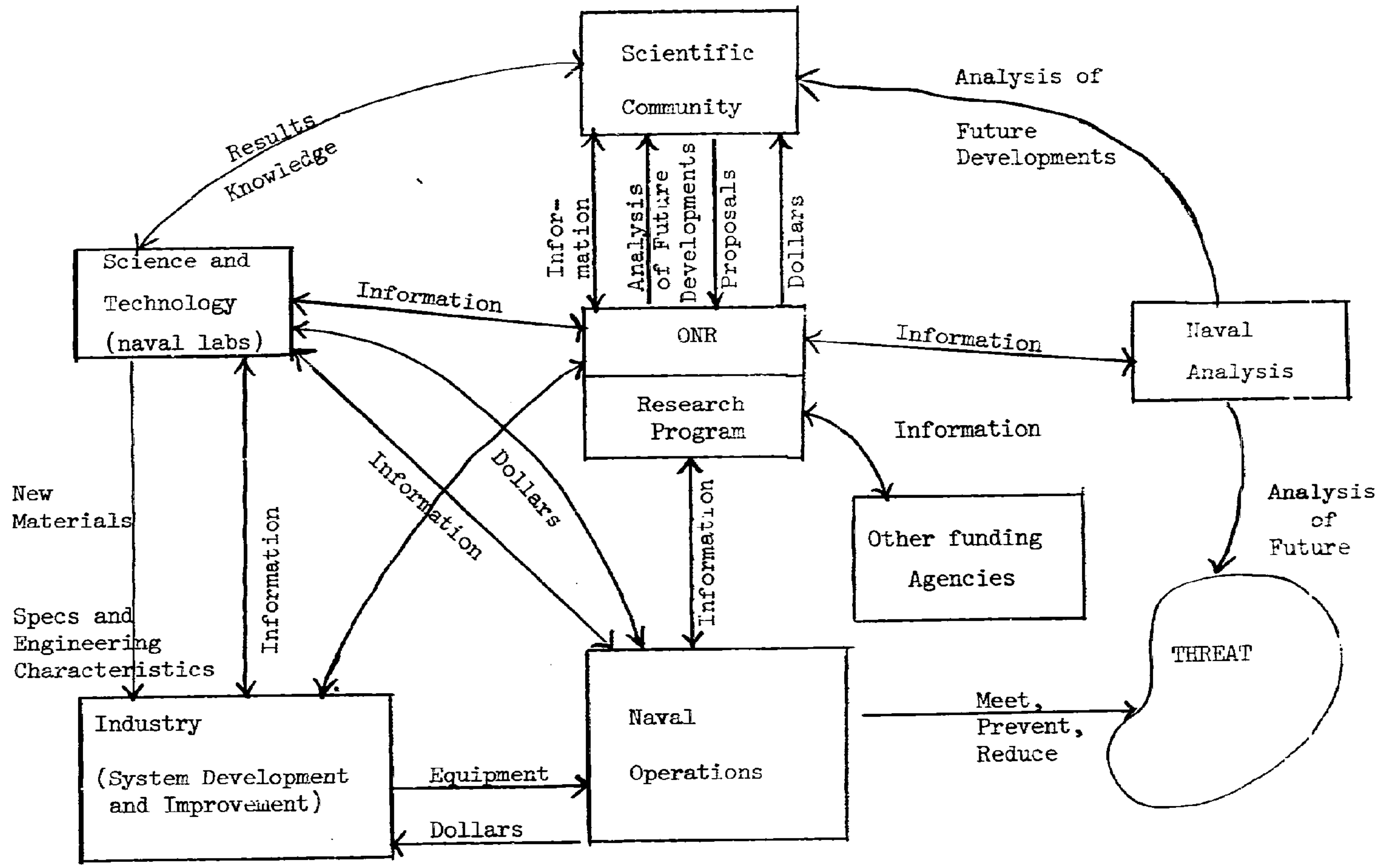

FIGURE 1

exhaustive, but the system it represents takes on the characteristics of a continuing dialogue in which the scientific community plays a creative role in reacting to Naval needs by suggesting alternative research paths through which the needs may be satisfied.

It is to be understood that the system illustrated in Figure 1 is in no sense closed. The various agencies shown, for example, undoubtedly interact with other agencies among which one might include NIH, NSF, various Army, Air Force, and DOD funding offices, and even certain organizations outside this country.

It is further to be understood that, although the various interactions are shown as between agencies, formal interactions tend to be confined to those flows which involve money. Flows of information tend to take place at a much more informal level, and are usually in the form of personal contact among individuals in each of the various organizations involved.

B. Internal Organization of ONR

A proper understanding of the decision making processes within ONR requires some knowledge of the way in which ONR is organized internally. This is suggested schematically in Figure 2.

There are essentially four levels of organization within ONR; the Admiral's level, the division level, the branch level, and the program manager level. The Admiral, who holds the title of Chief of Naval Research, is responsible for all the activities of ONR, and reports administratively to the Assistant Secretary of the Navy for R&D.

Organizations of Scientific Divisions and Branches of ONR

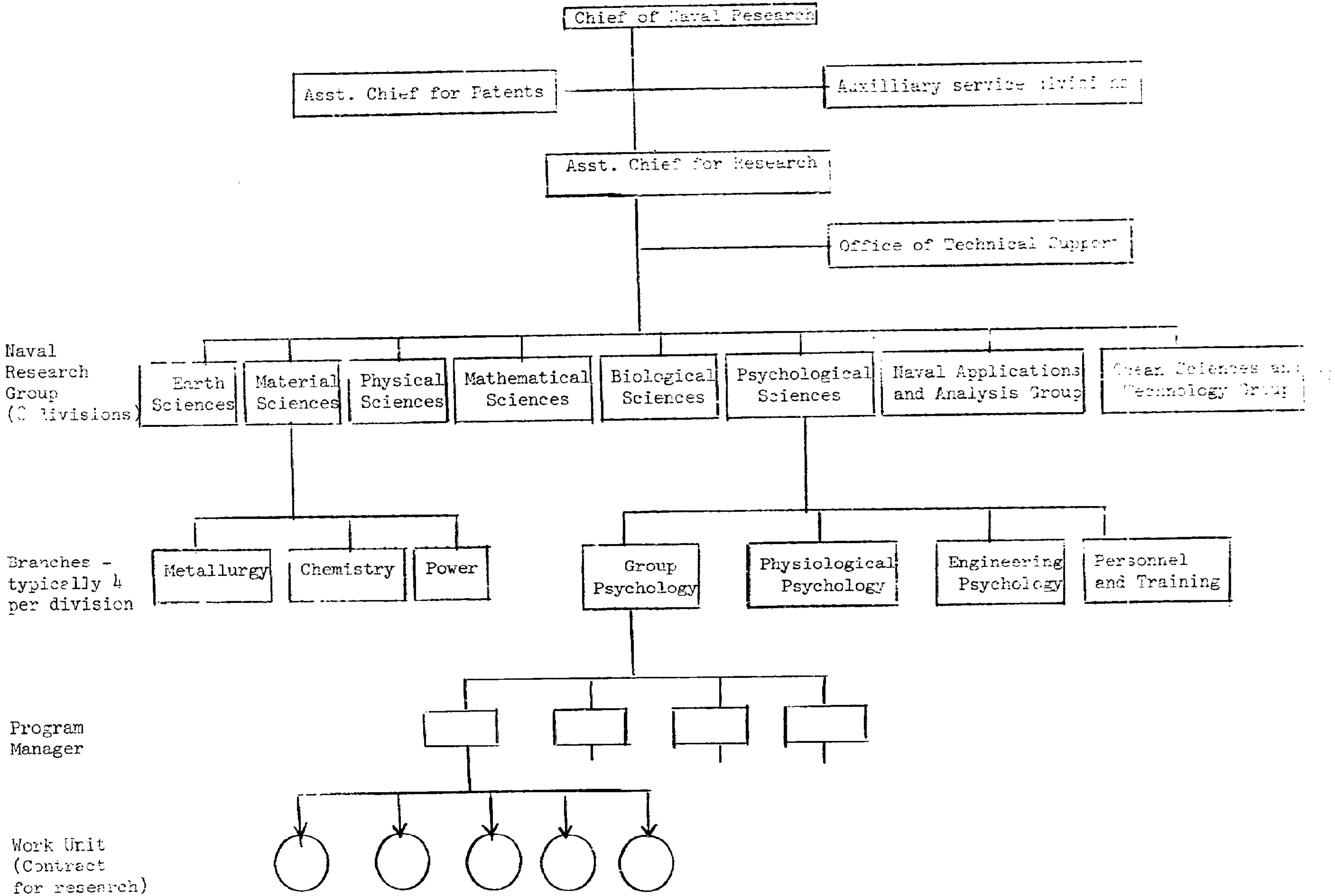

FIGURE 2

as of March 1967

ONR is subdivided into eight research divisions, the responsibility of each roughly corresponding to a broad scientific area. The divisions are:

1. Earth Sciences Division
2. Material Sciences Division
3. Physical Sciences Division
4. Mathematical Sciences Division
5. Biological Sciences Division
6. Psychological Sciences Division
7. Naval Applications and Analysis Group
8. Ocean Science and Technology Group.

Each division is, in turn, divided into a number of branches with each branch having the responsibility for funding research in a particular area of science or technology within the divisional field. For example, Information Systems and Structural Mechanics are among the several branches within the Mathematical Science Division. Within each branch there are a number of program managers (Scientific Officers) who directly administer and monitor research contracts (work units.)

Control is exerted downward through successive administrative levels of ONR largely by means of decisions made in response to budget requests submitted upward from the lower administrative levels. Further control is exerted as a result of the fact that all funding decisions must, in principle, be made at the division level. All funding decisions are reviewed at that level, and most of the work units under consideration for original funding (as opposed to ongoing work units being considered for continued funding) are carefully examined at regular meetings of the ONR division heads.

While control flows in a downward direction through the ONR hierarchy, scientific information flows in all directions. Characteristically, ONR personnel at all levels are in personal contact with members of the scientific, technological, and military communities.Information and ideas generated through these contacts are eventually communicated to all other levels within the ONR where the information is used in the formulation of agency funding policies. Program managers are particularly encouraged to study and develop new areas of interest which can be helpful in meeting Naval needs.

It is clear that funding decisions are made at each of the administrative levels of ONR, but in the discussion that follows, we will concentrate our attention on those decisions which are directly concerned with the selection and budgeting of individual work units. This will involve the development of resource allocation models to incorporate uncertainty in costs and payoffs, multiple approaches, long-time investments, and diffusion in result applications, with the purpose of establishing a cost-effectiveness measure for basic research. It was noted that funding decisions are, in principle, made at the division level, but the recommendations made within the various branches by program managers are followed in most cases. We can therefore think of the funding decisions as if they were actually made by the program managers, and will find it convenient to do so.

III. PROPOSAL SELECTION

Review of previous procedures

It is relevant to examine those procedures by which ONR has contracted for research in the past. Typically, research proposals would be submitted to ONR at random times throughout the year. All proposals are nominally unsolicited, although many may have in effect been solicited by means of indirect, personal suggestions to a scientist that ONR might be interested in funding certain types of research. It is usual, in fact, for almost all proposals to at least have been discussed informally between the proposer and the cognizant scientific officer at ONR.

The number of proposals available for the consideration of any branch of ONR is always considerably greater than the number which its research budget can fully support. Moreover, for all practical purposes, a large fraction of that budget is already committed to continuing some of the ongoing projects from previous years. This is a result of the well-recognized, although unofficial policy of avoiding abrupt cutoffs in funding to current recipients of research funds.

In effect, it is those funds which remain after allocations to support prior commitments which are used to fund new projects from proposals which are currently available. The selection of such projects is based on an appraisal of a number of subjective attributes. Among these are the importance of the scientific effort to the advancement of the field as a whole, the capability of the principal investigator, the research institution involved, the contribution that the research would make toward graduate education, and the probable utility of the research results to the Navy. These and many other subjectively appraised factors enter into the eventual funding decision. It frequently happens that a research proposal is deemed worthy of support, but that ONR either cannot afford to support it or that some other agency should appropriately bear some or all of the cost. This often results in an arrangement whereby ONR and some other agency jointly support a research task.

Trend toward mission orientation

The recent trend toward greater emphasis on mission-related research has largely left the old procedures for selecting projects unaffected. The principal change appears to be that a much greater subjective weight is now accorded to the relevance of the research to future Naval needs, although this concept is usually not made explicit or precisely defined.

What is desired

A rational funding procedure would seek to reduce the required subjective appraisals to a minimum, and would attempt to combine these appraisals in a consistent and meaningful fashion.

The result of such a procedure would be an expression for the value, in some sense, of a research portfolio in terms of a number of subjective estimates and a number of parameters. One could then hope to determine that portfolio which maximizes the value function, subject to a budget constraint and any other restrictions that can be explicitly stated.

IV. INITIAL APPROACHES

There exists a number of general objectives of the Chief of Naval Research and ONR, which have major relevance for this study.[11]

"Objectives" of Chief of Naval Research and ONR[12]

O_1 - Perform such duties as the Secretary of the Navy prescribes relating to:

(1) Encouragement of Naval research
(2) Promotion of Naval research
(3) Initiation of Naval research
(4) Planning of Naval research
(5) Coordination of Naval research

O_2 - Conduct of Naval research in augmentation of and in conjunction with R and D conducted by the Bureaus and other agencies and offices of the Department of the Navy.

O_4 - Receive estimates of appropriations for research by the several Bureaus and offices for assistance in coordinating Naval research in carrying out other ONR duties.

O_6 - Keep the Chief of Naval Operations advised of findings, trends, and potentialities in research and disseminate information to interested Bureaus and offices within the Navy Department and to other Governmental or private agencies, as may be appropriate, on naval and other research matters.

O_8 - Study and collaborate with the Chief of Naval Operations and the Bureaus in the formulation of the principal development programs of the Navy.

[11, 12] Edited from ONRINST 5430.1, 5 Feb. 1954.

Such general objectives provide the overall framework for the specification of research task objectives. However, there are no quantitative methods available which provide a value function to measure the extent to which a specific research task achieves agency objectives.

An attempt to establish a value function led to the tentative proposal described by Dean and Mantel[13], at an early stage of the present study. It was proposed that value be derived entirely on the basis of relevance to the Navy. This relevance, in turn, was to be measured by an after-the-fact subjective value put on "ONR events" which were defined to be significant past research results which were felt to be critical to the development of Naval systems.

By examining historical data and evaluating past research results, it was hoped to be able to identify those scientific areas which have proven most fruitful for Naval technology. By assuming a kind of stability about the process, one might further hope that those areas which were most productive in the past will continue to be so in the future.

While this attack seemed sensible, it involved the generation of a great deal of subjective information and considerable research on the impact of past research results. In fact, it was estimated that the effort required would probably exceed that required to carry out Project Hindsight[14], which this attack resembled in certain respects.

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[13]See Dean, Burton V. and Samuel J. Mantel, "On a Basic Research Cost Effectiveness - Resource Allocation Model," Tech. Memo 84, Operations Research Group, Case Institute of Technology, May 1967.

[14]See Sherwin, C. W., et al, "First Interim Report on Project HINDSIGHT (Summary)". Office of the Director of Defense Research and Engineering, DOD, 20-301, Washington, D.C., June 1966, (revised Oct. 13, 1968).

It became necessary, therefore, to search for other techniques which could be used to assign values to a research portfolio. A second attempt was therefore made to study the current funding process so as to be able to describe more fully the manner in which funding decisions were actually being made. The authors hoped to develop a normative decision model which might provide a starting point for further studies leading to prescriptive models based on more explicit thoery.

It was found that normative models would either need to be oversimplified to the point of being of little value, or else so complex as to make the definition and estimation of parameters a practical impossibility on the basis of any real data.

A further apparent difficulty was the fact that the proposal selection problem is not so much concerned with how to distinguish good research from bad, but rather how to allocate limited research funds among proposals for research which is already judged to be inherently good. Feasible normative models seemed totally incapable of providing a fine enough "grid" to discriminate effectively among such proposals.

A new approach was therefore needed. By distinguishing between two types of research which are funded by ONR, two models can be constructed. The first model is that developed for funding science-oriented research, which is research whose primary purpose is to acquire knowledge for its own sake and not with any particular naval system in mind. The second is based upon mission-oriented research, which is seen as being helpful or necessary for developing a desired naval system.

V. MODEL FOR SCIENCE ORIENTED RESEARCH

Introduction

In most scientific areas funded by ONR, the major portion of available funds is allocated on the basis of forseeable results which may contribute to future Naval capability. In no field, however, are funds allocated entirely on this basis. Moreover, in such fields as pure mathematics, it is highly unlikely that foreseeable results play a very significant role in most funding decisions.

A model is therefore required to establish a rationale for funding research which is decoupled from specific, foreseeble Naval applications.

A model will be developed below for this purpose. This model will relate a measure of effectiveness to a funding policy, via the scientific achievements and technological innovations that are expected to result therefrom.

The inputs to the model appear to be obtainable in practice, so that the model can in principle be used as an aid in formulating real funding policy.

Underlying this model is the observation that funding for science oriented research is frequently based upon the demonstrated excellence of individual scientists, or the expected excellence of their top students. The rationale for this procedure is that eminent scientists, who have made significant contributions in the past, are likely to produce significant scientific contributions in the future, typically, contributions of an

Representation of Research Output

A necessary ingredient of the output model is the capability to describe the potential scientific results that may be expected from a funded research project.

It would be desirable to have an objective scale of scientific excellence which would measure potential scientific achievements, weighted according to the associated probabilities of their accomplishment. Objective scales of this type, which are both reliable and meaningful, are not currently available. At this time, and for the foreseeable future, therefore, it is necessary to employ subjective estimates. Moreover, with each task may be associated a variety of potential major achievements, each with some probability.

A promising approach appears to be the following. With each research task, enumerate major accomplishments that have any significant prior likelihood of resulting. Classify each of these potential accomplishments according to a subjectively determined coarse scale (e.g., 3 levels) of likelihood. Again, classify each potential accomplishment according to a coarse scale of "criticality", indicating the approximate degree to which the accomplishment may be regarded as "important".*

Symbolically, let i be a running index associated with the potential major accomplishments, A_{ni}, that might plausibly result from research task n. We then have, associated with A_{ni}, the quantity $P\{A_{ni}\}$ where $P\{A_{ni}\}$ is the (coarse) subjective probability of achieving A_{ni}.

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*The usefulness and feasibility of such classifications is well demonstrated in Cost-Effectiveness in R and D Laboratory Resource Allocation" (in print).

With each research task n, however, there will be some associated funding level, C_n. It is evident that $P\{A_{ni}\}$ will in fact depend on C_n. We therefore write this probability as $P\{A_{ni}|C_n\}$.[15]

Implicit in this last statement is the fact that the rate of research output varies with ONR funding level, and that the probability of any particular potential accomplishment, within the time period under consideration, will therefore also be a function of the ONR funding level.

Evaluation of Potential Research Accomplishments

The potential scientific accomplishments A_{ni} are, by definition, of such a character as to defy any prior estimate of their ultimate Naval value, expressed in terms of specific, foreseeable Naval applications; e.g., a theorem in pure mathematics.

That is, one cannot relate the scientific achievements A_{ni} to particular "systems" whose Naval utilities might be estimated directly. Instead, it is necessary to associate utilities with the A_{ni} themselves.

We therefore define values v_{ni} to be the expected Naval utility of the various A_{ni}. The v_{ni} are by their very nature subjective estimates, on an arbitrary scale (e.g., 0-100). These utilities are subjectively determined and relative, and are assumed to be additive. The method by which these quantities are estimated will be discussed below.

[15]This statement indicates that research output is a function of cost. However, a valid query is: Is output a function of cost and how to test whether it is? If the outcomes are not functions of cost, then the questions being asked of the manager should require $P\{A_{ni}\}$ instead of $P\{A_{ni}|C_n\}$: it may be valid that $P\{A_{ni}\}$ is not a function of facility costs (see Bell Laboratories statement in Dean, Burton V., Evaluating, Selecting and Controlling R&D Projects, Research Study No. 89, American Management Association, 1967.

Adjustment for Criticality

The values v_{ni} associated with the A_{ni} are to be estimated on the assumption that the A_{ni} have been defined as important scientific accomplishments - "breakthroughs", loosely speaking.

In general, however, a particular science-oriented research task, although directed toward the attainment of such major accomplishments, will fall somewhat short of full attainment. That is, a successful research task will contribute to one or more A_{ni}, but will not in general supply all the results which may be crucial to all the A_{ni}.

To allow for this, we introduce factors W_{ni} to represent the criticality of the research task n to the accomplishment A_{ni}.

The W_{ni} are defined to be constants between 0 and 1, and are to be estimated subjectively.*

Criticality of Contribution

Suppose that a certain piece of research R_i has been successfully completed with results, R_i^*. Given that there are some capabilities A_j in mind, one would like to know $P(A_j|R_i^*)$, that is, the probability of achieving capability A_j given results, R_i^*. Now the way in which a successful research contributes to achieving a capability is very important. A result R_i^* may be absolutely necessary and sufficient in itself to give capability A_j. It may be one of several research projects which are all necessary to realize A_j. It may be one of a number of alternative projects, any one of which is necessary to the achievement of A_j. Finally, R_i^* may just be one of many research results which are not really necessary but may

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* In the following sections the reader will observe that the notation used is slightly changed from the material presented above. This is to differentiate between our generalized discussions of the variables to be considered and the specific uses of these variables in a model for the ONR.

be helpful to another research project. The degree or importance of the contribution of R_i^* to A_j is called the criticality of contribution and will be denoted by w_{ij}, which is defined to be $0 \leq w_{ij} \leq 1$.

Perceived Values of Contributions

Considering that we are discussing science-oriented research, it must be emphasized that the possible connection between research R_i and some capability A_j is not, by definition, of paramount importance when making the funding decision. (Obviously, few if any research projects are evaluated solely as science or mission-oriented, and the amount of attention paid to the contribution of a research to a capability is a matter of degree.)

Given this understanding, we can state that with each contribution of a research result R_i^* to a capability A_j there is a value, v_{ij}, which is associated with the contribution of R_i^* to A_j in an attempt to completely achieve A_j. The values, v_{ij}, are really time dependent, but we will assume them to be fixed for our one-period model. The time dependence of these values is easily understood from the fact that an A_j, accomplished in time for it to be useful in the development of some component of a system, is much more valuable than if the A_j were accomplished after other means had been developed to replace its use.

The achievement of a capability within a specified time is usually dependent on cost, since, in general, the more support channeled into a project, the faster the project will be completed. If fewer funds are available, then the project will probably proceed at a slower pace.

Value Model

The mathematical formulation of a value model based on the foregoing analysis is discussed in Appendix A, below.

In order to use this model effectively, the R_i^* should be classified by order of importance of the contribution. In general, the classes might represent the level of impact that the R_i^* have on the main body of the relevant sciences and technologies. Three classes are tentatively suggested:

Class 1 - which might include about 1 per cent of the R_i^*. Very important research results, for example, revisions or extensions of basic theory, the development of new formulas describing scientific phenomenona, and so forth.

Class 2 - which might include another 8 to 10 per cent of the R_i^*. Important research results, for example, the development of theorems, algorithms, and major applications.

Class 3 - which would include the main body of R_i^*. For example, applications of theoretical results, Master's and Doctoral degree thesis and less important extensions of theory.

The categorization of research results is useful since it will aid the analysis of expected contributions flowing from specific researches. It is assumed that knowledgeable people can correctly classify proposals into one of the three classes noted. Finer distinctions might be helpful, but are not necessary.

Concerning the various inputs to the model, $P(R_i^*)$ can probably be obtained from the researcher himself. A recent survey of research performance on the campus of Case Western Reserve University indicates that the researcher is a good source of information on the results likely to come out of his work and that he tends to be reasonably knowledgeable about the applications which might flow from the results he expects to achieve. This survey is summarized in Appendix B of this report.

It is, of course, important that the $P(R_i^*)$'s be updated from time to time as work progresses on the research. Further, the A_j's specified by the researcher should be supplemented by the ONR. It is clear, however, that the A_j's will be an incomplete set since it is highly unlikely that all applications of a given R_i^* will be foreseen. In general, the $P(A_j|R_i^*)$ can be best estimated by ONR as can the weightings of w_{ij} and v_{ij}.

As can be seen in Appendix B, researchers appear to be able to consider objectively the chances that their work will produce interesting and useful results. In suggesting military applications to which their results might contribute, the researchers tended to name only the more obvious ones, very possibly because they were not familiar with any but the more obvious military needs. Researchers were also able to specify the probable consequences of an alteration of their budgets. While these estimates could usually not be made in precise quantitative terms, the model does not require inputs of this kind.

The survey results reported in Appendix B indicate that it would be valuable to refine the questionnaire that was used, but this conclusion does not alter the fact that answers to our questions contained a great deal of valuable data. The summary of answers which is reported does not reflect the quantitative and qualitative richness which can be contained in the type of dialogue from which the summary was generated.

The model presented here for science oriented research is conceptually the same as that for the mission oriented research. Each contains a $P(R_i^*)$ and a $P(A_j | R_i^*)$, although the numerical values of these probabilities will not necessarily be the same in each case, since the A_j are known in the mission-oriented model and partially unknown in the science-oriented model. In the mission-oriented model the researcher is working for a <u>particular</u> result while in the science oriented model the researcher is just working and results come from his efforts. The criticality of a result could be more easily known in the mission-oriented than in the science oriented model. The perceived values of contribution of a result, R_i^*, to a particular capability A_j will be essentially the same in each model, but more precise information will be available in the mission-oriented model.

Extensions

The model proposed above is intended as a tentative approach to the practical problem of improving upon current methods of funding basic science-oriented research. As such, the model necessarily employs a number of simplifying assumptions.

As the model is further refined, some of these simplifications may be removed.

Among the more obvious refinements would be improved scales for the v_{ni}, w_{ni} and $P(A_{ni}|C_n)$. Other obvious refinements would include improved techniques for obtaining reliable subjective data.

Beyond these, however, are two extensions of particular interest. The first of these extensions is to develop a technique whereby the total research budget can be allocated between science-oriented and mission-oriented research. The central problem here is to establish an equivalence among values represented on the two value scales for these kinds of research tasks. At present, these scales are not commensurable.

The second of the extensions would allow for the fact that it would be helpful if funding decisions could be made continually, and with an imprecisely forecast total budget. This implies a dynamic version of the model. (At present, most decisions on funding new research are made in batches.)

Conclusions

A tentative mathematical model has been proposed for the funding of research which is highly decoupled from specific, foreseeable Naval applications.

The model represents the way in which any feasible research budget gives rise to research, from which major scientific achievements result with some prior probabilities. The values assigned to the achievements are attributes only of the science sub-areas involved, and are not to be related explicitly to foreseeable applications.

The model requires subjective estimates of parameters which are in principle obtainable. Effort is currently being directed to the development of techniques for obtaining reliable data for the model.

Optimal funding is given by the solution to a mathematical programming problem, which is solvable numerically.

A number of refinements to the model remain for future work. In addition to those refinements which are concerned with improving the scales of parameter measurement and with data collection techniques, extensions might be made to unite this model with one for mission-oriented research, and to include dynamic aspects of the funding process.

VI. MODEL FOR MISSION ORIENTED RESEARCH

Introduction

The major portion of basic research funds made available through ONR have been,and will continue to be, allocated on the basis of foreseeable results likely to contribute to future Naval capability. A model is therefore required to provide a rationale for the funding of basic research with foreseeable Naval applications and is developed below.

It is necessary to distinguish here between mission-oriented basic research and exploratory development. It is the former, and not the latter, which is the subject of the present study. Exploratory development may perhaps be defined as research effort directed toward the development of particular techniques and of particular articles of technology ("hardware"). This would of course include mission-oriented exploratory development, if the resulting technology is regarded as having Naval application. By way of contrast, mission-oriented basic research may be defined as research, the anticipated results of which are expected to contribute to some particular future Naval capability, although the specific mode of the contribution (hardware, software, etc.) is not clearly delineated.

In the development which follows, it is important to retain the above distinction clearly in mind.

Subsequent sections will develop a mathematical model which assigns values to research effort. The value assignment proceeds by identifying future Naval "systems", relating these to "components", which depend on the development of relevant technologies or capabilities that in turn depend on current research effort. This gives rise to a "network" view of the research portfolio, in which interdependence of research effort is inherently accounted for. By relating the level of research activity to funding level, it is possible, at least in principle, to obtain optimum portfolios subject to budget constraints and certain types of other (e.g., political) constraints.

The optimization problem is examined, and a heuristic solution is proposed which can be expected to provide efficiently computed, "good" approximations to the optimum portfolio.

Value Model

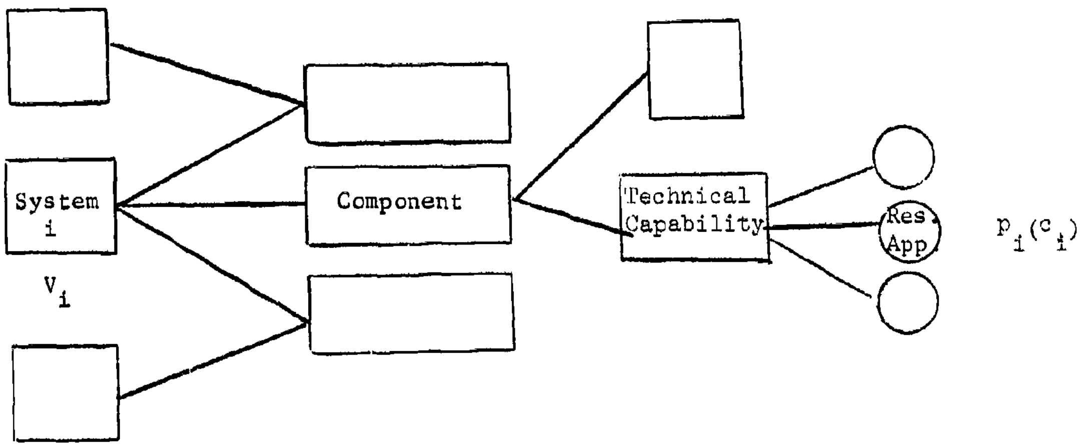
The mission oriented model assumes that one can identify, *a priori*, the Naval systems which are desired. An example of such a Naval system might be optical reconnaissance. Associated with each system is a relative military value V_i which is the speculated value of the ith system to the Navy. An important assumption is that the V_i are additive; that is, the value of two or more systems is equal to the sum of the values for each.

Each system is composed of one or more components, which are *all* required to be successful in order that the system be achieved. A component for the example system above is an optical sensor. Each component may contribute to one or more systems, but the failure of one component to be successful prohibits the realization of all the systems to which it contributes.

With each component to a system is associated one or more technical capabilities. Pattern recognition is an example of a technical capability for the previously mentioned system. A technical capability may apply to several components, and <u>at least one</u> capability must be successful in order to achieve a given component. Note that this is the first stage at which not all the elements are needed to be successful in order to achieve the next stage.

One finally sees that in order to have a technical capability <u>at least one</u> of a number of research approaches must be successful for each capability. Statistics might be a research approach for the pattern recognition above. Each research approach has a probability p_i of being successful, and a cost c_i of doing the research associated with it, where p_i depends upon c_i.

Schematically, the above discussion can be represented as follows:



where the success of one system, component, technical capability, and research approach is assumed to be independent of other systems, components, technical capabilities, and research approaches respectively.

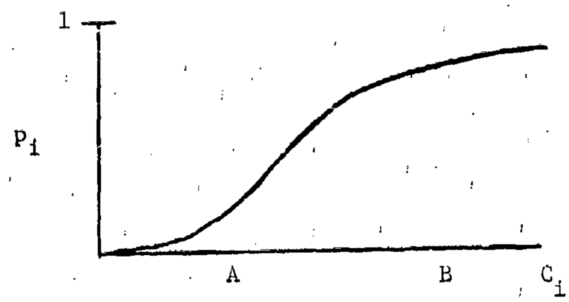
Because of the independence assumption, the value model may be derived in the following way. In order to get the technical capability, at least one of its n research efforts must be successul. That probability is $1 - \prod_{i=1}^{n} (1-p_i)$. To get the component, at least one technical capability leading to it must be successful. Its probability is $1 - \prod_{j\varepsilon J} (1 - \prod_{i=1}^{n} (1-p_i))$, where J is the set of technical capabilities leading to the component. The probability that the ith system is successful then becomes $\prod_{i\varepsilon I} (1 - \prod_{j\varepsilon J} (1 - \prod_{i=1}^{n} (1-p_i))$, where I is the set of components for the ith system.

The total expected value is then

$$V = \sum_{i\varepsilon I} V_i \prod_{i\varepsilon I} (1 - \prod_{j\varepsilon J} (1 - \prod_{i=1}^{n} (1-p_i)))$$

which is easily seen to be highly non-linear.

As mentioned earlier, each research approach has an associated cost and probability of success. The one period cost of research looks somewhat like the following:



where A represents a point before which it is not profitable to fund and B represents a point after which it is no longer profitable to fund.

Optimization Problem

In the immediately preceding sections a model was developed which assigns a value, V, to a research portfolio. The portfolio is specified by a number of parameters and by a set of cost allocations, c_i, to each proposed research task. Given a prescribed budget for mission-oriented research, \$C, it is desired to maximize V with respect to the c_i, subject to the budget constraint.

The quantity V, considered as a function of the c_i, is highly coupled and nonlinear. As a mathematical programming problem, the optimization does not therefore appear to lend itself readily to any of the standard algorithms for solution. In practice, however, a number of approximate, heuristic techniques are available which can generate "good" solutions in an efficient manner.

Parameter Specification

The expression for value, V, contains a number of parameters which need to be estimated in order to implement the model. The section which describes the model for science-oriented research refers to a recent survey conducted on the campus of Case Western Reserve University. That survey was conducted to ascertain whether the parameters of the science-oriented model could be estimated with feasible amounts of effort and with adequate precision. Preliminary results indicated that such estimates can be made.

The mission-oriented model contains a number of similar parameters. Where the models differ, there is inherently greater concreteness, and therefore certainty, in the mission-oriented model. It therefore seems likely that no insuperable difficulties should be encountered in estimating the parameter values for this model.

Extensions

The model described above is tentative, and embodies a number of simplifying assumptions. Some of these simplifications may be removed as the model is further refined.

Such refinements might include an improved scale for criticality and improved techniques for estimating probabilities and other subjectively determined quantities.

The present model is inherently static, being a one-period model. A dynamic version of the model would allow for the fact that funding decisions may be made continually, and subject to an imprecisely forecast and changing total budget.

It would be desirable to establish a commensurability between the value scales of the mission-oriented and science-oriented models. This would help to make possible a model which would be useful for the allocation of a total budget between the two kinds of research tasks.

Conclusions

A tentative mathematical model has been proposed for the funding of research which is foreseeably related to future Naval capability.

The model follows a network approach, starting from foreseeable Naval "systems", by which is meant functional capabilities which a future Navy should be capable of performing, and to which values are assigned on some relative scale. The details of assigning such values are not discussed here, but are adequately dealt with in the available literature.

The attainment of each system requires the prior development of generic "components", all of which are necessary to the system.

Each component requires the successful completion of at least one of a set of technical approaches.

A technical approach succeeds if at least one of a set of research tasks is successful. A slight modification is made here to allow for the fact that not all research tasks bear the same degree of "criticality" to the success of the technical approach.

The model requires subjective estimates of parameters which are in principle obtainable.

Optimal funding is given by the solution to a mathematical programming problem, which is solvable numerically.

A number of refinements to the model remain for future work. In addition to those refinements which are concerned with improving the scales of parameter measurement and with data collection techniques, extensions might be made to unite this model with one for science-oriented research, and to include dynamic aspects of the funding process.

VII IMPLEMENTATION OF THE MODELS

The basic problem of implementing the models developed in this report requires the consideration of several different sub-problems. First, there is the sub-problem of meeting the data requirements. Second, the mathematical models must be solved. Third, the solution of the models must be integrated into the ONR budgeting process.

In order to cast the several problems of implementation in meaningful terms, it is important for us to note clearly that while the day-by-day ONR process of funding research appears to be fairly casual to the outside observer, this appearance is quite deceptive. In reality, the funding process as practiced by ONR project managers is highly systematic. Interviews with ONR personnel from the project manager to the division head level reveal a surprisingly consistent understanding of the goals and objectives of the organization. Further, the professional staff of ONR is aware of and uses the sources of information shown in Figure 1 (page 13). Program managers and branch heads are encouraged to play a creative role in the initiation of programs which may contribute to future naval needs. Finally, in spite of the fact that almost every program manager spoke of a need for better communications within the ONR, they exhibited a good knowledge of the work in which their colleagues were interested. To sum up, the Office of Naval Research is characterized by a strong sense of purpose and is staffed by a highly competent group of well-informed scientists.

The value scales of the mission-oriented and science-oriented models are not commensurable, and thus the models do not contribute to the problem of dividing the total budget into mission and science-oriented shares. This must be done, as it is at present, by exercising well-informed but essentially arbitrary judgment.

Given a total ONR research budget allocated between mission and science-oriented research, when a proposal is received at ONR, it is classified in the mind of the relevant program manager in many ways. Among others, the manager classifies it as mission-oriented or science-oriented, though the distinction is largely one of degree. Before new research is considered, certain "sacred cows" receive funding, either because of their absolute necessity to a Naval system in the mission-oriented case, or because of the outstanding reputation of the principal investigator in the science-oriented case.

The subjective probability of success is not an element of major importance in the decision to fund "sacred cows." In most cases it is quite high because of the brilliant credentials of the investigator, or because the program is of sufficient importance to the Navy that much work has been done on developing a feasible attack on the research problem.

At the other extreme, a number of potential projects are set aside because of low probability of success or because they do not appear to meet Naval needs But most of the proposals fall between these two extremes. These are the projects where decisions are more difficult, and it is to these that this paper is devoted.

To implement the two models, certain data is required. For the mission-oriented model, one needs to know the cost of each project, the probability of success associated with such a cost, the technical capability being sought, the component to which the capability will contribute, the Naval system which is to be reached, and the value of that system. For the science-oriented model, the probability of a project's success is required, as well as the probability of a loosely specified technical achievement given a research success, known or foreseeable Naval systems that might utilize the technical achievement, a rough estimate of the criticality of the research to the technical capability or end systems, and an estimate of the value associated with the end systems.

Because these data requirements appear formidable, it was decided to find out just how available such data might be. Through a recent survey of preselected professors and researchers on the campus of Case Western Reserve University, it was discovered that the data was relatively easy to obtain. The individuals who were interviewed could give precise, although possibly not accurate, estimates for the probability that their research would be successful, the general nature of the expected results, the probability that their results would apply to pre-named technical capabilities and Naval systems, and the relationship between research success and the cost of the project. The authors feel that the staff of the ONR could validate and extend such a data base.

There are several heuristic methods available to solve the models presented in this paper. They cannot guarantee strictly optimum solutions, but they will give good approximations to the optimal solution. Among others, Schoeman[16] has shown how the mission-oriented model can be solved, under certain

[16] Schoeman, Milton E. F., "Resource Allocation to Interrelated R&D Activities," Technical Memorandum No. 109, Department of Operations Research, Case Western Reserve University, Cleveland, Ohio, June 1968.

restrictive assumptions. Attention should also be given to a hueristic method developed by Reiter.[17] His technique starts with a random but feasible solution (which he assumes can always be generated) which is altered stepwise by adding or taking away a single project that gives the best local improvement in the objective. The steps are continued until a local optimum is attained. A new starting point is generated and the process is repeated. This is done successively until one decides (on a cost-per-unit-improvement basis) that the best solution thus far is good enough. Other methods are available.

A consideration of the implementation of these models must include a discussion of their integration into the ONR budgeting process. It should be quite clear at the outset that the authors do not recommend that the outputs of the models ought to be taken as a command and acted upon willy-nilly. Both the mission-oriented and science-oriented models are partial, not complete, solutions to the funding problem. As such, the models can serve as useful tools which aid the analyst in organizing some of his information, and in understanding its impact on his actions. The "solutions" generated by the models are <u>ceteris paribus</u> solutions, and, as such, are valid only so far as the <u>ceteris paribus</u> assumption is valid.

In spite of this severe limitation, the models presented in this paper can prove quite valuable for the ONR. The preparation of the input data forces a careful analysis of the most important considerations in the decisions to fund or not to fund any research project: "If the research is carried out successfully, what purpose will it serve?" "What are the chances that it can

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[17] Reiter, S., "Choosing an Investment Program Among Interdependent Projects," <u>Review of Economic Studies</u>, Vol. XXX(1), February, 1963, pp. 32-36.

be done successfully as proposed?" "How important is this research when compared to the other projects that are being considered?" These questions must be answered; not answered casually, but carefully with the decision maker marshalling his information and intuition into a numerical estimate. This process of quantification not only induces care in the consideration of the fundamental aspects of the decision problem,it provides the decision maker with a base from which to deviate for cause.

Another major advantage to the use of models such as those described here, is their use in simulation exercises. It would be relatively easy to investigate the impacts of varying allocations to the different divisions and branches of the ONR. Such questions as, "What might be the impact of a decision to increase the ONR investment in projects which have extraordinarily high potential value, but which are also characterized by very high risks (i.e., low probability of success.)?" These questions can be considered quickly and easily via these models, and the answers can be evaluated in terms of the ONR's overall long term goals.

Finally, the actual use of the models will help the agency build the information and insight needed to improve and extend the models. Thus, use of the system makes it more valuable.

While the problems of implementation are not simple, there appears to be no overriding reason why the models cannot be integrated into the normal ONR decision process. Great care must be taken to avoid using the models as a mechanistic tool which can substitute for informed judgement, but rather as a useful aid in the funding process.

VIII CONCLUSIONS

This paper represents a tentative approach to finding the optimal allocation of funds to basic research by the Office of Naval Research. This attempt has led to a pair of simultaneous models, one for funding mission-oriented research and the other for funding science-oriented research. Although the two models are mathematically the same, they are nevertheless philosophically quite different. While each model can in principle be implemented, the mathematical optimization of each is subject to much further study.

Since the present approach is basically one period and static, there is need for extending it to an n-period dynamic context. Another possible extension involves the relaxation of the additivity assumption on the system values, an assumption which appears in all current models in the literature.

Appendix A

Value Model for Mission-oriented Research

The objective is to maximize the value of the research to the Navy. The total value associated with result i to the Navy may be represented as $V_i(C) = \sum_i P(R_i) \sum_j P(A_j|R_i)w_{ij}v_{ij}$, as a result of the additivity assumed for the utilities. However, since the capabilities to which result i can contribute are not all known, we actually have $V_i(C) \geq \sum_i P(R_i) \sum_j P(A_j|R_i) w_{ij}v_{ij}$, where the right hand side serves as a lower bound on the expected value of result i to the Navy. The mathematical model then becomes

$$\max \; V_i(C)$$

$$\text{s.t. } C \leq B \text{ where } B = \text{total budget available.}$$

However, by maximizing $\sum_i P(R_i) \sum_j P(A_j|R_i)w_{ij}v_{ij}$, one is really maximizing $V_i(C)$, so the model finally becomes

$$\max \sum_i P(R_i) \sum_j P(A_j|R_i)w_{ij}v_{ij}$$

$$\text{s.t. } \sum_i C_i \leq B$$

Clearly, the $P(R_i)$ are nonlinear functions of the funding levels C_i and will in general exhibit a saturation type of dependence for large C_i.

In the event that the budget exceeds the sum of saturation levels for all available research tasks - most unlikely in practice - the optimization problem becomes trivial, as it indeed should.

Appendix B

Data Acquisition Survey

A model has been postulated for allocating basic research funds by ONR which is comprised of two parts, corresponding to two criteria by which basic research is commonly funded. These are: (1) mission-oriented research; (2) science-oriented research.

This section summarizes a preliminary attempt to ascertain whether needed data can in practice be obtained for implementing the model for mission-oriented research. The method used was a series of brief interviews with selected faculty at Case Western Reserve University, actively engaged in current research, in a variety of research fields. Eight questions were asked of each professor. The questions, followed by the responses obtained, are reproduced at the end of this report. The faculty are identified only by code at the top of each answer summary, in order to avoid any possible compromise of priviledged information.

It appears to be essential to any realistic model of system oriented funding to include information as to:

(1) Probability of technical success
(2) Identification of potential Naval systems that might result from successful research
(3) Probability of eventual application of research, assumed technically successful, to Naval systems
(4) Sources and extent of funding
(5) Sensitivity of probable research output to changes in funding level

The questions were intended to elicit the above information. The data is necessarily sparse, and should be construed merely as suggestive and preliminary.

Survey Questions

1. What types of research are you currently doing?

2. To what extent, if any, are your research projects supported through a contract with a military organization?

3. Leaving the question of success to your personal interpretation, could you estimate the chances of your research being successful?

4. What military systems do you envision your research having some application to?

5. Could you estimate the chances that your research, assumed successful, will contribute to these various systems?

6. To the extent that privileged information not be compromised, could you estimate approximate research costs associated with your projects?

7. If your research budget were altered by a factor of 2 in either direction, what impact would this likely have on the probability that your research be successfully completed?

8. How many projects are being conducted in research areas similar to yours?

Survey Results

Respondent K

1. a) Studies of the rheological properties of colloidal and polymeric fluids.
   b) Diffusion in polymer solutions.
   c) Diffraction of light by colloidal spheres.
   d) Non-equilibrium statistical mechanics of fluids.

2. Not supported by a military organization

3. a) good
   b) fair
   c) good
   d) long-shot

4. a) Flow of napalm
   d) Aerodynamic and hydrodynamic turbulences as application to motion of missiles and vehicles through air and water.

5. a) High probability of contribution
   d) Would not contribute

6. a) $25,[illegible]0/year
   b) $10,000/year
   c) $10,000/year
   d) $5,000/year

7. a) Would have large impact if altered in either direction.
   d) Would have large impact if altered in either direction.
   b) Would have large impact if cut in half.
   c) Would have large impact if cut in half.

8. a) six
   b) six
   c) two
   d) zero

Survey Results

Respondent AB

1. Physical acoustics.

2. Not supported by a military organization.

3. Very good.

4. Jet airplane engine noise reduction and fundamental mechanisms.
   Combustion and stabilities in rocket engines.
   Speech communication.

5. Does now contribute - by supplying fundamental data and insights.

6. $40,000/year.

7. None at all.

8. Few (2) are doing the same specific research.
   There is, however, a great number of persons doing research which is closely related to this.

Survey Results

Respondant B

1. Non-parametric methods in statistics.
2. Not supported by a military organization.
3. Continuing and hope to continue to get results.

4. Signal detection
5. The research now contributes to signal detection and hopefully will continue to contribute.
6. $11,000/year.
7. If doubled, could increase personnel and get results faster. If cut in half, could decrease personnel and get results slower.
8. Approximately 40 in the country.

Survey Results

Respondant V

1. a) Numerical solutions of partial differential equations.
   b) Approximation theory.
   c) Eigenvalues of differential operators.
   d) Gerschgorin theory for matrices.

2. 50% of research is supported on a sole contract by the AEC.

3. 50 - 80% for each project.
   He does not start work on a project unless he's almost certain to finish it successfully.

4. In controlling orbital flight and in atomic reactor theory.

5. This is already being contemplated for use in atomic reactors.
   50% for controlling orbital flights.

6. \$20-\$30,000/year total for all projects.

7. Would have very little impact except that if reduced by 1/2, the project would go slower since he might have to teach more courses.

8. Around the country.

   a) 4 b) 3 c) 3 d) 2

Survey Results

Respondent B

1. a) Pressure dependence of flow and fracture in beryllium.
   b) Pressure dependence of plastic deformation in magnesium oxide.
   c) Pressure dependence of melting of organic polymers.
   d) Flow and fracture behavior of tungsten at pressure.
   e) Pressure dependence of dislocation mobility in covalent crystals.
   f) Mechanical behavior of crystalline polymers at pressure.
   g) Precipitation kinetics at pressure.
   h) Plastic flow in tungsten.

2. a) Supported by an Air Force grant from Wright field.
   b) Supported by NSF.
   c) Not supported by a military or government organization.
   d) Supported by NASA.
   e) Not supported by a military or government organization.
   f) Not supported by a military or government organization.
   g) Supported by NSF.
   h) Supported by NASA.

3. The research will probably be about 75% successful in obtaining the results expected of it.
   It, however, will be 100% successful to the extent that it will give some sort of advance in understanding the phenomena, which is the minimum it sets out to do.

4. All have to do with materials.
   a) Airframe and space vehicle structure.
   d) High temperature structural components for rocket aircraft.
   f) Ballistic absorption.
   Others don't have immediate application, but provide an understanding of the phenomena.

5. a) This is dependent on the effectiveness of the agency sponsoring the research and not on the success of the research.
   d) This is dependent on the effectiveness of the agency sponsoring the research and not on the success of the research.
   f) This depends on whether it contributes through some industry.

6. Approximately $15,000 per year per project for operating costs.

7. If the funds are cut in half, he could have only half the number of students working, and therefore some projects would have to stop, so the probability of success goes to zero. In some cases, however, the work would continue with just a faculty member. In any case, the time scale would be altered.

If the funds were doubled, this might open up new ideas and areas. This would increase the success of the overall laboratory which is to increase understanding of pressure phenomena.

8. In this general theme, there are no more than six in universities and industry across the nation.

Survey Results

Respondant F

1. (a) Optimization methods in engineering design.
   (b) Advanced structural dynamics.

2. 50% NASA, 50%ARPA.

3. 100%, if success relates to technical goals, education, advancement of career.
   80%, if technical goals only.

4. All research motivated by aerospace applications.
   But research is concerned with generally applicable techniques of design and analysis.
   Thus cannot be more specific than "lightweight aerospace structural applications."

5. Not meaningful.

6. Hard to attach specific costs to specific studies.

7. x 1/2 : research would take twice as long.
   x 2 : research would take somewhat more than half the time.

8. Score or more.

Survey Results

Respondant L

1. Control of complex industrial systems - theories, concepts, and techniques of modeling, decision making, adaptability, in contexts such as industrial plant.

2. 0% .

3. All research is in connection with student theses. No prior research objectives, so that all advances are successes. Chances of success excellent.

4. Applicable to a wide variety of military systems, but hard to identify specific systems.

5. Question is redundant, in view of 4.

6. $130 K/yr. direct + fellowships, etc.
   Totals about $200 K./yr.
   Sponsored by industrial group of 13 companies.
   Supports 30 grad students + 4-6 faculty
   + overhead (experimental facilities).

7. x 1/2 : program would collapse, because of the nature of the funding.

   x 2 : could do more than twice as much, because of large overhead expenses.

8. 3 or 4 other schools with similar programs. Also, most large companies with research organizations are doing some similar work - e.g., sponsors.

Survey Results

Respondant O

1. a) Hybrid and transient lubrication.
   b) Separation and containment of non-homogeneous fluids.
   c) Stability and dynamics of fluid interfaces.
   d) Swirling effects on 2-phase (boiling) flows.

2. a) NASA
   b) AFOSR
   c) AFOSR
   d) NASA

3. All should give information worth publishing.

4. a) Space vehicles, power systems.
   b) Gas core nuclear reactors. Rockets.
   c) Rocket fule storage.
   d) Space power systems.

   Research is specifically directed toward these applications.

   Other applications may be to engines, control devices, driving slugs of fluid. Valves.

5. By definition, 100%.

6. $18 - 20 K./yr per project.

7. x 1/2 : support fewer grad. students.

   x 2 : improve results by 50% or so.

8. a) 1 hybrid, 0 transient
   b) 0
   c) 1
   d) 2

Survey Results

Respondant EB

1. a) macromolecules
   b) Relate structure to properties.
   c) Medical applications.

2. 60 - 65% government (10% military).

3. Success measured by research funds attracted.
   Probability of technological success = 1.

4. NASA - cryogenic applications
   rocket fuel bladders
   nose cones

   DOD, Army, etc. - lightweight weapons, shields

5. 0.5.

6. NASA: $60K = 10% of total research budget.

7. X2 - no effect.

   X 1/2 - half productivity, but would not accept if much less.

8. (no answer)

Although the data is sparse and thus not amenable to statistical treatment, the results of this survey seem to indicate that the data necessary for implementation of the model for mission-oriented research can in practice be obtained.

The diversity of responses to certain of the questions - e.g., probability of technical success - suggests that the questionnaire used in the survey requires further refining to avoid ambiguities and to increase the quality of the data.

This is left for later studies, and in no way contradicts the above conclusion that real data can be obtained for the models.

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