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ABSTRACT

This work has been done as part of the effort to plan the National Institute of Education (NIE). The report, one of a series, describes four different methods used by three Federal agencies--the National Institutes of Health, the National Science Foundation, and the Office of Naval Research--in managing fundamental research. Fundamental research is defined as activity undertaken to add to a store of knowledge about basic processes. Separate sections describe the dual panel, single panel, mail, and "no" review methods used by the agencies. Each section overviews and summarizes the method and examines the supporting activities and organization of the research agency involved. (Author/DN)

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NATIONAL INSTITUTE OF EDUCATION: METHODS FOR MANAGING FUNDAMENTAL RESEARCH

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A WORKING NOTE
prepared for the
DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE

This Note was prepared to facilitate communication of preliminary research results. Views or conclusions expressed herein may be tentative and do not represent the official opinion of the sponsoring agency.

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PREFACE

This working note contains descriptions of the methods that the National Institutes of Health, the National Science Foundation, and the Office of Naval Research use in managing fundamental research. These descriptions were obtained by interviewing managers and other staff personnel in these agencies and interpreting their responses. None of the descriptions have yet been returned to the agencies for their comment or approval. This will be done in the near future.

This work was done as part of the effort to plan the National Institute of Education (NIE). If authorized by the Congress, the NIE would conduct research and development in the field of education. This report is one of a series on the Institute. The others are:

- National Institute of Education: Preliminary Plan for the Proposed Institute (R-657-HEW)
- National Institute of Education: Methods for Managing Practice-oriented Research and Development (WN-7677)
- National Institute of Education: Methods for Managing Programmatic Research and Development (WN-7678)
- National Institute of Education: Organizational and Managerial Alternatives (WN-7679)
- National Institute of Education: Evaluation of Methods for Managing Research and Development (WN-7680)

This report only describes the methods that the selected agencies use in managing fundamental R&D; it does not evaluate their relative merits. A comparative evaluation of these methods appears in WN-7680.

Many of the R&D managers interviewed during this study expressed the need for additional study of the methods used in managing non-military R&D in the federal government. The literature on this subject is slight in comparison with the literature concerning the management of industrial and military R&D. This series of reports seeks to provide a basis for research into improved management practices for non-military federal R&D. The principal purpose of these reports, however, is to enable the planners of the National Institute of Education to benefit from the experience of other federal R&D agencies in developing the NIE's R&D management procedures.

CONTENTS

I. INTRODUCTION

PURPOSE OF THIS REPORT

This report describes four different methods that three federal agencies use in managing *fundamental research*.

The format of description will be to treat one agency at a time by (1) presenting the steps that its managers and researchers actually take in managing research activity, and then (2) repeating this presentation for the other agencies. The intention is to present the data on which the interpretive and evaluative statements made in WN-7679 and WN-7680 are based. This approach was taken because agreement on what people do is easier to get than agreement on the effects of their actions, or what they should do. Agreement on the effects of their actions is lacking because insufficient research on the management of federal R&D has been done. Agreement on what R&D managers should do is difficult because it is ultimately a question of value.

The description for each agency will be called a paradigm, since not every detail and variant in what an agency does will be described. Each description is meant only as a model that depicts the essential steps in an agency's R&D management process.

A step is deemed essential if changing it would significantly alter an estimate of the basic philosophy underlying the R&D management process being described. By looking at the essential steps, it is easier to infer what the basic underlying philosophy is, and how to project it onto a new situation, such as education R&D. This is, in fact, a meaningful definition of a paradigm; that is, the projection of something which is difficult to describe onto reality, where its consequences are observable and hence describable.

The paradigms that will be treated in this report are listed in Table 1 together with the agencies using them.

Table 1

AGENCIES TREATED AND THE METHODS THEY USE
TO MANAGE FUNDAMENTAL RESEARCH

Agency	Paradigm
National Institutes of Health	Dual Panel Review
National Science Foundation	Single Panel Review Mail Review
Office of Naval Research	"No" Review

METHOD OF RESEARCH

The data used to construct the management paradigms were obtained by interviewing federal R&D managers. Roughly half of the people interviewed were program directors and the other half were supervisory and staff personnel. Exhaustive coverage of every manager in an agency was not attempted, but rather key personnel and those recommended by key personnel were approached for interview. Altogether 38 managers were interviewed, some on repeated occasions. In addition, some data from the academic literature and from agency documents were used. A list of the people interviewed will appear in the final version of this report.

The paradigms are a distillation of replies made by managers commenting on the nature and relative importance of their various activities. Necessarily, this approach to research is vulnerable to biases and sometimes produces information that is difficult to verify. Nevertheless, by asking all managers similar questions, and by filtering the responses as objectively as experience made possible, a fair representation of reality is thought to be presented. This approach is within the tradition of naturalistic observation as a method of research.

To gain clarity of exposition, some of the auxiliary mechanisms used by some agencies to overcome shortcomings in their management processes were omitted. Thus, matching the paradigm descriptions, the agencies interviewed, and the paradigm evaluations in WN-7680 to conclude that one agency does a better job of managing research than another agency is not justified. The operations and usefulness of these auxiliary mechanisms are discussed in Appendix B and the evaluation report, WN-7680.

TYPES OF R&D ACTIVITY

This report is limited to the management of fundamental research. Methods for managing two other kinds of R&D activity, practice-oriented R&D and programmatic R&D, appear in WN-7677 and WN-7678 respectively.

In simplest terms fundamental research can be described as activity undertaken to add to our store of knowledge about basic processes. Fundamental research is conducted according to the standards of disciplined inquiry in that experiments must be replicatable, and postulated truths must follow logically from stated assumptions or demonstrated fact. The mechanism for imposing this discipline is that a researcher's professional status is determined by the quality of his work as judged by his scientific peer group.

Fundamental research is conclusion-oriented as opposed to decision-oriented. Thus, its performers are searching for unique propositions about nature that have the universality and permanence of laws. Propositions are unique in that the scientific community eventually reaches agreement that a proposed proposition is either true or false within the context assumed for its proof. The situation is opposite in decision-oriented research, or in development, where the objective is to find the "best" solution to a practical problem at hand. Usually, there will be several solutions, one of which is better than the others based on local and temporal value judgments.

Another distinguishing characteristic of fundamental research is that the directions pursued are chosen primarily by the researchers themselves. The research community is the appropriate judge of these directions since progress is guided more by consideration of where solutions are possible, and where intuition hints that new results are likely to be found than the immedicate practical value of results. This does not mean however, that practical problems can have no influence on the directions taken in fundamental research as a whole. Entirely new lines of research can be started in response to a practical need, and levels of effort among areas of inquiry can be adjusted to reflect relevance to practice.

A deeper description of the fundamental research mechanism has been proposed by T. S. Kuhn. The model above fails to convey adequately the compartmentalized nature of fundamental research, the coherence of results within these compartments, and the dynamics of knowledge development. A summary of Kuhn's work is included as Appendix A of this report.

TYPES OF MANAGEMENT ACTIVITY

To facilitate presentation, the research management process will be broken into three types of activity:

- o Program Planning,
- o Program Development,
- o Program Evaluation.

These categories are deliberately chosen to group together qualitatively similar management activities.

Program Planning management activity is defined to include all the actions taken to foster, detect, and incubate research in new and ill-formed areas of inquiry. Also included are the procedures for deciding which new research areas will be added to the set of ongoing streams of activity. One example of such a new area is NIH's effort to understand how to increase a person's conscious control over his body's autonomic responses as a means of countering disease and malfunction. This is a fundamentally new concept in NIH, one which differs greatly from more traditional surgical and chemical techniques researched in NIH. The mechanisms of control are very imperfectly understood at the present time, and the concept is not clearly superior to existing prevention and cure modalities, but its potential explanatory power and practical utility seem very great. These characteristics are typical of new research areas in fundamental research.*

Program Development is defined to be the activity of managing the continuous process of refining and elaborating knowledge in an area. As a management process, Program Development is typically, though not always,

*Using terminology developed in Appendix A, a new research area is more concisely defined as a new knowledge paradigm.

an iterative and continuing sequence of stages involving:

- o assessment of research needs,
- o generation of project ideas,
- o selection of projects to support,
- o monitoring of project performance,
- o evaluation of project outcomes, and
- o utilization of results.

In various management paradigms, these stages are managed in different ways. Sometimes they are done by program directors, sometimes by panels of scientists, and sometimes not at all. In most R&D, and especially when fundamental research is being done, action proceeds concurrently in several of the stages. This activity is called Program Development because from program management's perspective, fundamental research is evolutionary and expositional in nature: the goal of reserach is adding fine structure and precision to a basic idea, and this happens through a sequence of project generation, project selection, and outcome events that develop over a long period of time.

Program Evaluation is the management activity of assessing what has been accomplished at some point in time by the researchers in a knowledge area, and the judgment of what ought to be done next. The ways in which agencies accomplish this management activity is the third topic to be discussed in this paper.

Discussion of management methods will focus on what is done at the *program director's* level, and only occasionally at higher levels. This is necessary because of the decision to describe procedures that managers use in practice. Interactions at the higher levels are more political, and thus subject to greater variation and personality dependencies. Not much insight into ways of allocating a budget between mathematics research and physics research is gained by looking at the *procedures* agencies use. The influences surrounding the making of these decisions is the subject of studies in the science policy and political science literature. Attention here will center on what occurs at the interface between the performer and his immediate manager. In some agencies this latter individual is called a program officer, or science administrator.

II. DUAL PANEL REVIEW

OVERVIEW

The Dual Panel Review concept of reviewing research proposals by first one and then a second panel of scientists has evolved from a genesis in the Biologics Control Division of the Public Health Service. In the early 1900s, this Division established a panel of non-government scientists to provide technical advice on regulatory programs, and called it the Advisory Board for the Biologics Control Division. In 1930, this Board was renamed the National Advisory Health Council (NAHC), and continues today as an advisory board to the Surgeon General of the Public Health Service.

NAHC began making grants for extramural research in 1918. For some years thereafter, NAHC retained the functions of reviewing applications for scientific merit, and deciding which grants would be paid. But, as the volume of grants increased, the workload had to be divided. The Council decided to create panels of extramural scientists that recommend funding priorities to the Council for its use in deciding which grants would be paid. Each of these panels was called a "study section" and was managed administratively by a federally employed "executive secretary." As the Council's extramural research program grew even larger, the Council also turned responsibility for determining scientific merit over to panels, retaining only the functions of reviewing the study sections' actions and deciding the final order of payment.

A 1937 reorganization of federal health agencies transferred the Division of Biologics Standards and its advisory board, the NAHC, to the National Institutes of Health (NIH), which had existed for seven years as an intramural laboratory. The reorganization also added the National Cancer Institute and its advisory board, the National Advisory Cancer Council to NIH.

NIH created the Research Grants Office in 1946 to run the panels set up by NAHC, and to administer the Office of Scientific Research and Development projects transferred to the Public Health Service at the end of World War II. Within a year the Research Grants Office was raised to division status and called the Division of Research Grants (DRG). As the

Cancer Institute began extramural research, its applications were channeled through the DRG's panels just as NAHC's. And like NAHC grants, grants from the Cancer Institute received final approval from the National Advisory Cancer Council. In the years following World War II, more institutes were added to NIH, and each one adopted this "Dual Review" method of program management.

Until recently, the institute program management staff and the Council have had less influence on the allocation of money to subject areas than DRG and its study sections. In the years just after the establishment of DRG, executive secretaries did the research programming, ran planning workshops and conferences, and traveled to universities stimulating research applications in promising areas. In recent years the institutes have increased their influence on the allocation of health research money by doing more research programming. They are also using new instruments like R&D centers and research contracts, where DRG's management role is minimal.

As the volume of grants awarded by the institutes grew, NAHC grants became a decreasing portion of all grants processed by DRG. Today, all the grants reviewed by DRG are paid by one of the divisions or institutes of NIH. Some of the NAHC authority has been transferred to NIH and some remains in the Public Health Service.

The essential features of Dual Review are the following:

- *Support is given to individuals*, not institutions, to work on a problem they have proposed.
- *Support is awarded for a limited period* of time, but can be renewed through presentation and approval of a new application.
- *Each proposal is subjected to two reviews*, first by a discipline-oriented peer panel to check on scientific merit, and then by a more problem-oriented council to check on program relevance.
- The *power to review* grants for scientific quality *and* the *power to program* research projects *reside in different managerial units.*

- The *majority of managerial effort is devoted to evaluating proposals for determination of scientific merit,* rather than Program Planning, progress monitoring or Program Evaluation.

SUMMARY

General Characteristics

Primary output:	Additions to fundamental knowledge about basic processes or practical problems.
Mechanism of support:	Finite duration project grants are made to individual research scientists. Generally, these scientists are employees of a university, research institute, or firm.
Managerial emphasis:	Evaluation of proposals receives far greater emphasis than any other management activity.
Staffing plan:	Program directors serve full-time in their positions. Most have had at least some experience as bench scientists in the past, but almost never return to the bench.

Program Planning

Sources of new program ideas:	Programs are collections of similar projects derived from the list of active projects, rather than interlocking units of planned work. New programs start first in the scientific community, but once started are nurtured by managerial action.

Mechanism for planning:	The same set of active projects is divided along discipline lines by DRG, and into medical problems by the institutes. Around these focii, both DRG and the institutes run workshops to stimulate interest in topics which they feel are important. No quantitative planning methods are employed.
Coordination:	Areas of interest are coordinated with other agencies through informal contact at the program director's level. Program priorities are influenced by the institute's director's office.

Program Development

Sources of project ideas:	For the most part, project proposals are unsolicited; but occasionally, a scientist's interests in a program area are guided by contact with a program director.
Means of proposal review:	Every scientist's proposal is assigned a numeric score first by a panel of his peers (study section) and then re-reviewed by an institute advisory council. The first review is discipline-oriented and run by DRG; the second is more practice-oriented and run by the institute.
Allocation of budget:	The institute pays grants in the order determined by the study section's scores and approved by the Council.
Monitoring of performance:	Substantive progress on projects is not closely monitored.

Evaluation of outcomes:	No formal procedure. Reliance is placed on including scientific accomplishment as an item of consideration in the proposal evaluation process.

Program Evaluation

Mechanism of evaluation:	Most institutes have no formal procedures. One institute uses a structured conference to evaluate programs.

ACTIVITIES

While the most managerial effort is placed on proposal evaluation activities in Dual Review, Program Planning receives some attention. The planning effort is directed at increasing the rate at which new research areas are developed by the scientific community.

Program Planning

In the Dual Review system of management, there are two sides in the planning process. One is the Division of Research Grants (DRG), which takes a discipline-oriented view of the research effort; and the other consists of the ten categorial institutes which take a problem-oriented view. DRG has divided NIH's grant research effort into 48 different areas of inquiry and appointed an executive secretary and scientific panel for each one. Each area corresponds closely to the interest of a scientific discipline or sub-discipline. On the institutes' side this same research effort is divided differently; first by institute (heart, cancer, and others) and then (usually) by a health problem area. For both the institutes and DRG, these units of activity will be called programs.

Neither side uses a direct method for planning new programs, but rather relies on indirect methods which encourage existing programs to grow in preferred directions. Thus, the two sides exert complementary, but indirect influences on the same collection of research activities. A common method of influencing program direction is to conduct workshops.

A typical workshop format is for a program director (or executive secretary in the case of DRG) to invite a half-dozen or so scientists expert in different aspects of a new research area plus some other scientists who might be enticed into the area to a two or three day meeting. The panel meets to assess the state of knowledge about the new area and to speculate on what kinds of projects would be most promising. The intent of these workshops is to draw scientific attention to the new area and to reach agreement on priorities for immediate action. Word of these priorities travels quickly through scientific circles. An example of such a workshop is the session on sickle cell anemia held recently by the hemotology study section of DRG. Other indirect methods for Program Planning used by some institutes will be described in subsequent publications.

In the Dual Review method, new programs are instituted in the organization through a process of mutation. When the number of projects in a new research area grows sufficiently large in its parent study section, DRG divides the study section's workload in two, forming two study sections. One of these study sections contains the new research area. Conversely, if the workload in a study section withers, DRG merges it with another study section. In this way, the workload is continually equalized across study sections, and the whole system "tracks" the research effort. By administrative statute, the DRG Director must review each study section's charter at least once every ten years, and decide whether to continue, divide, or merge it. A similar process occurs with respect to health problem areas in the institutes except that there is no time limit on the interval between reviews.

The National Advisory Councils are used by the institutes as a minor aid in research program planning, but not in the same way by each institute. During Council meetings, where study section actions are re-reviewed, the institute director and program staff listen to comments made by Council members and translate them into programming emphases. Some directors solicit the opinion of Council members more deliberately by requesting issue papers from them or appointing them to task forces. Executive secretaries also observe these meetings and bring comments back to the study sections.

Program Development

The dual aspects of Dual Review are clearly exhibited in the Program Development cycle.

In the institutes, the principal parties are the program directors and the National Advisory Council. Their counterparts in DRG are the executive secretaries and the study sections. Each pair of counterparts perform similar functions in the Program Development process.

In Dual Review, the great majority of research proposals are unsolicited, but a portion are responses to suggestions made during a personal visit by a program director. The size of this portion varies greatly with the institute; some emphasize the practice and some do not. On DRG's side, the executive secretaries travel some, but less than program directors, so the institutes are more influential in stimulating research proposals with this mechanism.

The program directors stimulate proposals in areas of work judged by the institute staff to be particularly important to the institute's mission. In this way, the institute has some control over the directions that continuing programs take.

Another means of influencing the direction of a developing program is to give applicants whose proposals are considered most relevant to the institute's mission advice in the preparation of their proposal. By knowing what catches the study section's attention, and the clearest ways to present ideas, the program director can considerably enchance the chances that an applicant's proposal will be accepted.

Three times a year a great flood of proposals descends on the DRG. Referral officers sort through these proposals, and assign each one to one of the 48 study sections and one of the ten institutes. The executive secretary of the study section receives the proposal and assigns it to one or more study section members for in-depth review. With the executive secretary sitting as co-chairman along with a member of the panel, the study section meets to evaluate proposals. Each is discussed in turn, and at the end of discussion, each panelist votes a numerical score by secret ballot. The numerical average of these scores is the proposal's score.

The graded proposals are then resorted and delivered to the institutes. Each institute receiving proposals from many study sections (and each study section grades proposals for several institutes). The institutes then re-scale the study sections' rankings to equalize "tough" and "easy" panels. The program directors go over the list of proposals, looking for the few which seem to have received improper review, were disapproved by more than one study section member, cost over $100K, or have special program interest. At the Council meeting, each program director discusses the package of proposals in his area with the Council, bringing to its attention the particular proposals he has singled out. The Council goes over these proposals in some detail. The Council then votes to return the proposal to study section for another review, votes to pay the grant, or re-assigns a priority score. Sometimes the Council changes a proposed project's budget, and then re-assigns a priority score. In addition to the proposals flagged by the program director, individual Council members may bring up any other proposals for discussion. Before the meeting the institute mails to each Council member summaries of all proposals to be reviewed at the next meeting, so that prior preparation is possible. Over 95% of the proposals considered at a Council meeting are never discussed, however, since the proposal workload is so great at each session. (There are 48 study sections, but only 10 Councils). All proposals not discussed are passed "en bloc."

The institutes have different ways of setting the order in which approved grants are paid. One is to pay the proposals in order of priority score until the extramural budget is exhausted. Another is to order the proposals within each program of the institute and pay further down the priority scale in important programs and less in others. In any case, there are more approved grants than money available to pay them by a factor of about two. The backlog of approved but unfunded grants is a principal statistic used by the extramural program in arguing for a larger extramural research budget. Applicants whose grants are paid receive an allotment of money to be spent over a period of five years or less, under some item restrictions on salaries and equipment. Minor rebudgeting is approved by the program director, but otherwise research progress is not carefully monitored. Project outcomes are not evaluated in any formal way.

From this description, it is evident that the largest fraction of management effort is applied to evaluation of proposals and not project planning, stimulation, monitoring, or evaluation. This emphasis is consistent with the policy of relying on the individual investigator for the bulk of project ideas. A flow diagram of the Program Development process appears in Figure 1.

Program Evaluation

Program Evaluation is not a significant part of the Dual Review paradigm as practiced by most of the institutes, although two of them have evolved some tactics for doing so. One of those methods is described in Appendix B.

ORGANIZATION

Structural Relationships

An organization chart for the NIH showing the organization of two selected institutes and the Division of Research Grants is shown in Figure 2. One of the institutes is divided by health problem area, the recent method; and the other by funding instrument, the traditional method.

Staffing Relationships

Program Directors. With few exceptions all the program directors have M.D.s or Ph.D.s and usually some experience as a bench scientists. There is a small level of migration from intramural research to program management, but none in the reverse direction. Program directors are selected by the institute's Associate Director for Extramural Research and have responsibility for a single program area. Virtually none serve another role besides their assigned responsibility.

Advisory Council Members. At least six advisory council members are required by law to be authorities in health or science fields important to the institute's concern. Two are *ex officio* representatives, one required

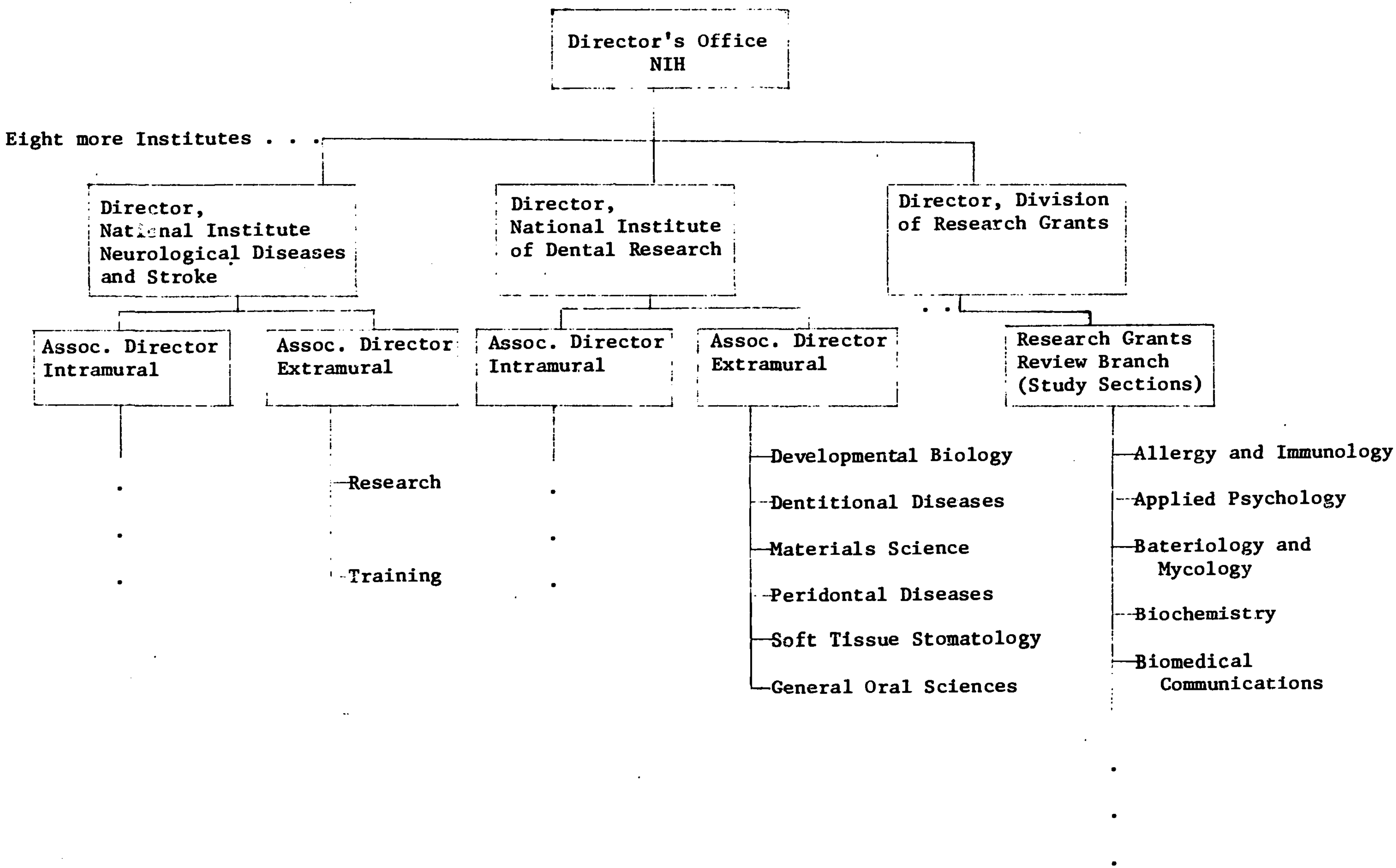
Figure 1: Program Development for Dual Panel Review

Activity	External	Institute		Division of Research Grants		
	Scientists	Program Directors (nominally 7/inst.)	Advisory Council Members (14)	Referral Officer (8)	Executive Secretary one per panel	Study Section Members (10 to 16 per panel)
Assessment of research needs, synthesis of new approaches, stimulation of new proposals.	o Some send a preliminary version of their proposal to their program director, especially if asked to do so.	o Attend conferences, talk to scientist contacts. o Travel in the field recruiting new researchers. o Suggest improvements in submitted proposals.			o Attend conferences; talk to scientist contacts, study section members. o Suggest improvements in submitted proposals.	o Discuss with "invisible college" colleagues.
Evaluation of proposals. [Study section meets 3 times per year for 2 or 3 days and ranks 30 to 40 proposals. The Council meets 6 weeks later, 3 times per year.]	o Send proposal to NIH.	o Request that researchers send notice of proposal submission to Prog. Dir. Watch its progress through DRG.		o Sort incoming proposals to executive secretaries and Institutes.	o Assign incoming proposals to one-to-three panelists for preparation of written review before the panel meeting. o Arrange site visit for proposals which require one (about 10%).	o Prepare written summaries of assigned proposals. Read as many of the rest as time is available.
		o Attend study section meeting, but very rarely asked to comment.			o Co-chair study section meeting with member of study section.	o Meeting as a group, discuss each proposal in turn for about one half hour. Consider site visit report if any. Make changes in proposed budget level, if needed. o After discussing, approve or disapprove modified proposal by majority vote of the panel. Then score proposal on a 1 to 5 scale by considering: scientific merit, feasibility, competence of principal investigators
			Proposals from several study sections.	Proposals to several institutes.		
		o For proposals felt to have received unfair review, prepare justification for changing score assigned by study section. Discuss with Institute management before Council meeting. o Normalize scores across study sections to equalize tough & easy panels.	o Read study section summaries before Council meeting to extent time is available.		o Write summaries (the "pink sheets") of study section's action using member prepared summaries. Circulate to prog. dir. for comment.	o Score of proposal is average of scores assigned by each member.

Figure 1: Program Development for Dual Panel Review

Activity	External	Institute		Division of Research Grants		
	Scientists	Program Directors (nominally 7/inst.)	Advisory Council Members (14)	Referral Officer (8)	Executive Secretary one per panel	Study Section Members (10-16 per panel)
		o Present briefs at Council meeting. Bring up any proposal which: - costs over $100K - was disapproved by more than one study section member - is especially relevant to Institute's program. (Less than 1% are in this category.)	o Discuss the proposals one at a time for one half hour. Then ratify study section score or modify proposal and rescore, or send back to the study section. o Members bring up any other proposals they wish to discuss. o Over 95% of proposals ratified en block. o Council meeting chaired by Institute Director.		o Comment on proposals being discussed by Council when asked to do so by a Council member or Institute Director. Comments often solicited.	
Allocation of Institute budget to projects.	o If proposal turned down, revise and resubmit application. Utilize comments of prog. dir.	o Sort incoming proposals to program areas. o Set pay limit in each program area. o Pay grants in priority score order.	o Comment on important trends in a program area during Council meeting. Comment on staff presentations during Council meeting.			
Monitoring and supporting ongoing projects.		o Tend to administrative needs as requested by researcher. Authorize minor changes in grant plan made necessary by research results.				
Evaluation		o Require that researchers send reprints of published work to the prog. dir. These are selectively read for information only.	o Keep abreast of progress in field of competence through usual means.			o Keep abreast of progress in field of competence through usual means.

Figure 2: Organization Chart of NIH



from the Department of Defense, and the other from the Veterans Administration -- and as selected, they are generally scientists. Of the remaining six positions, one or two are usually lay representatives and the rest scientists. Advisory Council members are nominated by the institute director but must be approved up the line to the Secretary of HEW. The term of service is four years. Council members are paid $100 per day for their services.

Referral Officers. The Division of Research Grants' Director selects referral officers from the ranks of executive secretaries to serve on a part-time basis.

Executive Secretaries. In qualifications, background, and term of service, executive secretaries are indistinguishable from program directors. They are selected by the Director of the Research Grants Division in consultation with panel members and others. There is some migration from executive secretary positions to program director positions, but virtually none the other way.

Study Section Members. The executive secretary and the study section chairman recommend replacements for panel members from the community of scientists. These nominations are then approved up the line to the Secretary of HEW, but seldom denied. The term of office is four years. The study section chairman of chosen by the executive secretary. Panelists are paid a $100 consulting fee for their services.

III. SINGLE PANEL REVIEW

OVERVIEW

The National Science Foundation's Biological and Social Science Divisions use a reduced form of Dual Review which will be called Single Panel Review. It is essentially half of the Dual Panel Review system, since the basic paradigm includes only one panel and one program director in a partnership relationship. The Single Review program director performs all of the tasks performed by both the executive secretary and the program director in Dual Panel Review, except that he need not compose summaries which communicate the deliberations of one panel to the other.

The essential features of Single Panel Review are the following:

- *Support is given to individuals*, not to institutions, to work on a problem they have proposed.
- *Support is awarded for a limited period* of time, but can be renewed through presentation and approval of a new application.
- *Some proposals are submitted to a panel of the applicant's discipline peers* for their comment and opinion.
- The *majority of managerial effort is devoted to selecting a set of projects thought to optimally balance the various lines of fundamental research being pursued.*

SUMMARY

General Characteristics

Primary output:	Additions to fundamental knowledge about basic processes or practical problems.
Mechanisms of support:	Finite duration project grants are made to individual research scientists. Generally, these scientists are employees of a university, research institute, or firm.

Managerial emphasis:	The majority of research effort is devoted to selecting the set of projects that is thought to optimally balance the lines of fundamental research being pursued, including the prospects for normal science progress and revolutionary discovery.
Staffing plan:	Program directors serve full time in their position. Most have had at least some experience as bench scientists, but a sizable fraction return to research in a university or other setting after serving one or two years as a program director.

<u>Program Planning</u>

Sources of new program ideas:	Research programs are clusters of projects grouped by disciplines; but within a program, selected threads of inquiry may be emphasized. Care is taken to detect and nuture new threads of inquiry which are potentially of revolutionary impact.
Mechanism for planning:	Every program director is responsible for spotting and stimulating new and important threads of inquiry, but in addition this responsibility is assigned full-time to a senior program director working at the institute director level.
Coordination:	Areas of interest are coordinated with other agencies through informal contact at the program director's level. Program priorities are influenced by the director's office.

Program Development

Sources of project ideas:	Scientists are funded to work on problems they have selected. Occasionally, a scientist's interests are guided by contact with a program director. Program directors advise applicants on the preparation of proposals.
Means of proposal review:	The program director selects a group of borderline proposals that are not clearly fundable or rejectable. The program director and the panel debate which subset of these borderline projects is best to support along with the clearly fundable ones.
Allocation of budget:	In addition to working with his panel, the program director discusses priorities and opportunities with scientists during the year. At the biannual budget allocation time, the program director chooses the set of projects that his discussions and review panel session have convinced him offer the best combination of possibilities for revolutionary advance, normal science progress, and talent development in the lines of research being pursued.
Monitoring of performance:	Substantive progress on projects is not closely monitored, though complete lack of progress (as indicated by progress reports) is grounds for termination.

Evaluation of outcome: No formal procedure. Reliance is placed on including scientific reputation as an item of consideration in the proposal evaluation process.

Program Evaluation

Mechanism of evaluation: No formal procedures.

ACTIVITIES

There is considerable variation in the role played by the program director in Single Panel Review. In some sections, the panel and the program directors work as partners in the decision process with the program director acting as a decision maker and the panel serving in an advisory capacity. In other sections, the panel exercises much more decisionmaking authority, especially in the selection of projects and replacements for panelists whose term of service has expired. To get the greatest difference between Dual Panel Review and Single Panel Review, the paradigm presented below describes the former situation where the program director and the panel are in partnership.

As in Dual Review, some Program Planning effort is exerted in Single Review. However, somewhat different methods are used. A primary difference is less use of conferences and workshops.

Program Planning

As in the Dual Panel Review paradigm, programs are a cluster of projects grouped by scientific discipline, and not collections of logically interrelated projects deduced from a set of overarching goals. Thus, "fields of inquiry" might be a better name than "programs" for these units of activity. Especially important new ideas that seem likely to develop into a distinctive field of inquiry are nurtured by the program staff through the usual means; traveling, workshops, and persuasion. Some responsibility for directing and encouraging these new ideas lies with the program directors, each of whom has responsibility for one field of inquiry. But in addition, a senior program director reporting at the top management level spends all his time searching out and stimulating nascent fields of inquiry which offer significant potential. His principal means of stimulation are the same as the program director's.

Program Development

At intervals throughout the year, the program director visits scientists in his field of inquiry, discussing new trends, important events, and his own and others' opinions. These discussions help the program officer form his scientific priorities, and keep him informed. These discussions are useful for stimulating research proposals.

Twice during the year the program director sorts a batch of perhaps 100 proposals that have been submitted by individual investigators into three piles: the obviously inferior ones; the obviously superior ones; and twenty to thirty marginal proposals, some of which can be supported and some of which cannot. The program officer sends the twenty or thirty marginal proposals to his panel, and insists that they read them. He then calls a meeting of the panel to discuss the twenty or thirty proposals. Among the types of proposals that might be considered at such a meeting are an off-beat idea that the project officer has not seen before, or ideas that appear to be outdated, or the work of a new Ph.D. The program director and the panel debate the merits and demerits of each proposal, not necessarily in order. The panel does not assign a score to the proposals, but tries to inform the program director of their opinions and information. The panel may also glance at the "clearly fundable" proposals passed by the program director.

After the meeting, the program director selects the package of proposals that fits his budget and he believes to be an "optimal" balance of innovative but risky ideas, normal science progress, special opportunities, and chances for development of new research talent in the lines of research he is pursuing. The budget of some proposals may be altered to eliminate weak portions, or increase strong ones. The program director negotiates with the applicants over these changes. The program director's final choice is then reviewed and approved by his organizational superiors.

Grants are awarded for a fixed amount expendable over a set period of time, usually less than five-years. During this time, annual progress reports are submitted; but unless major difficulties arise, the investigator is free to choose his course. Project outcomes are not evaluated on a formal basis.

A description of this Program Development process appears in Figure 3.

ORGANIZATION

Structural Relationships

Each program director has responsibility for one field of inquiry and runs his own panel. Several of these fields of inquiry are grouped together into an organizational unit called a section. Typically, a section has the name of a discipline, and each of the programs in it is a field of inquiry in the discipline. Several sections together constitute a division, the second highest level organizational unit in NSF. This structure is diagramed in Figure 4.

Staffing Relationships

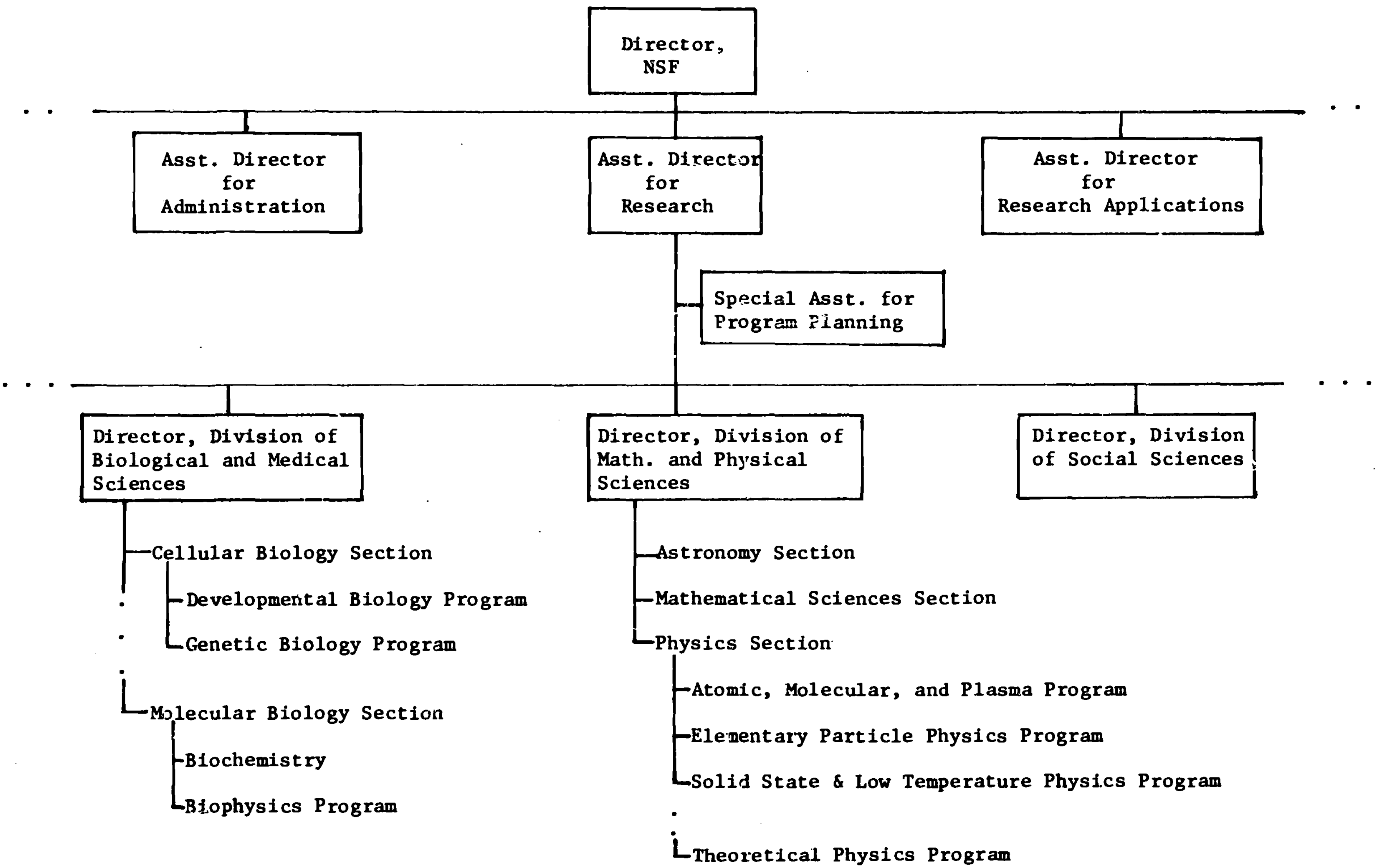
Program Directors. With rare exception, all program directors have a Ph.D. or equivalent and research experience. All serve full-time, but a sizable fraction have temporary appointments. These program directors serve for a year or two and then return to their research environment. The rest of the program directors serve for longer terms. Program directors are selected by their immediate supervisor.

Panelists. Panelists are selected by the program director in consultation with his supervisor and approved by higher echelons within NSF. The panelist's term of office is nominally two years, but there is no statutory requirement. The fields of inquiry are delineated so that all panelists can be specialists in a common research paradigm, but have different experimental or methodological approaches. Panelists are not paid for their services.

Figure 3: Program Development for Single Panel Review

Activity	Researchers	Program Director	Panel (6 to 15 members)
Assessment of research needs, synthesis of new approaches, stimulation of new proposals.	o Discuss problems, progress, new ideas, and new people with program director. o Submit preliminary proposals for comment, if desired.	o Site visit throughout research community, discussing problems, progress and new ideas. o Suggest improvements in submitted proposals.	o Converse with program officer before, during, and after review session about new directions and program needs. o Converse with colleagues about current research program and prospects.
Evaluation of proposals (biannual).	o Send proposal to NSF	o Review of stack of proposals received. Select 20 to 30 borderline cases after accepting some and rejecting others. Send these to panelists for pre-review.	o Read the 20 to 30 proposals before meeting.
		o Chair the review session. Keep meeting on the subject and make sure needed information is obtained. o Need is for an assessment of each proposal's innovativeness, timeliness, riskiness, and potential of investigator.	o In session compare all proposals against . state of art . feasibility . competence of principal investigator. o Debate strong and weak points of each proposal with program director.
Budget allocation		o Select list of projects that optimally balances important factors and meets budget constraint. May be desirable to adjust some project budgets more than others. Negotiate with investigators. o Defend package of proposals before immediate supervisor.	o Converse with program director before, during, and after review session.
Monitoring and supporting ongoing projects	o Submit informal progress report annually.	o Tend to administrative needs as requested by researcher. o Read progress reports. Check for major problems.	
Evaluation of project outcomes		o Selected reading of publications resulting from projects. o Discuss results with performers when occasion arises.	

Figure 4: Organization Chart for Single Panel Review



IV. MAIL REVIEW

OVERVIEW

Replacing the peer panel in the Single Review paradigm with an *ad hoc* selection of reviewers who provide written comments via mail creates a third research management model, which will be called Mail Review. Mail Review is used in many places in the federal government, but particularly in the Engineering and the Mathematical and Physical Sciences divisions of NSF. Their experience with it extends back to World War II when many of the NSF managers now using it worked in the Office of Scientific Research and Development (OSRD). OSRD used Mail Review somewhat differently from the way NSF does now. In OSRD good proposals were accepted, marginal ones were mailed to scientific peers for comment, and poor ones were dropped. In NSF every proposal receives mailed comment.

In its basic form Mail Review differs only slightly from Single Panel Review, especially with regard to the program officer's responsibilities. An extended version of the basic model, however, has evolved in NSF.* The extended model differs from the basic model in two ways: (1) there are two rounds of Mail Reviews, and (2) after the project awards have been made, a panel is convened to review the list of awards. The first innovation was added as a means of letting each reviewer have the benefit of seeing the comments made by all the other reviewers of a proposal before he writes his final opinion. The second innovation is a device for Program Evaluation.

The essential features of this extended form of Mail Review are:

- o *Support is given to individuals* not to institutions to work on a problem they have proposed.
- o *Support is awarded for a limited period of time*, but can be renewed through presentation and approval of a new application.
- o *Proposals are evaluated by obtaining mailed comments* from a number of scientists expert in the area of the proposal.
- o *The majority of managerial effort is devoted to selecting a set of projects thought to optimally balance* the various lines of research being pursued.

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*This version was invented and used by Wayne Gruner, now Senior Staff Associate, Research Directorate, National Science Foundation.

- *Programs are evaluated* by periodically convening a panel that reviews the list of active projects.

General Characteristics

Primary output:	Additions to fundamental knowledge about basic processes or practical problems.
Mechanism of support:	Finite duration project grants are made to individual research scientists. Generally, these scientists are employees of a university or non-profit firm. A small amount of formula support is awarded to the grantee's institution.
Managerial emphasis:	The most managerial effort is devoted to selecting the set of projects thought by program management to optimally balance the lines of research being pursued including prospects for normal science progress and revolutionary discovery.
Staffing plan:	Program directors serve full-time in their position. Most have had at least some experience as bench scientists, but only a portion return to research in a university or other setting, after serving as program directors.

Program Planning

Sources of new program ideas:	Research programs are clusters of projects grouped by disciplines; but within a program, selected threads of inquiry may be emphasized. Attempts are made to identify and develop potentially new threads of inquiry.

Program Development

Sources of project ideas:	Scientists are funded to work on problems they have defined. Occasionally, a scientist's interest in a program area is guided by contact with a program director. Program directors advise applicants on the preparation of proposals.
Means of proposal review:	The program director sends each potentially fundable proposal to a few or more specialists in its field of inquiry. The returned comments are complied and sent back to each reviewer for additional comment.
Allocation of budget:	The program director discusses priorities and opportunities with scientists throughout the year. He also interacts with the audit panel convened to review his program. At the biannual budget allocation time, the program director chooses the set of projects that his discussions and previous review panel sessions have convinced him offer the best combination of possibilities for revolutionary advance, normal science progress, and talent development in the lines of research being pursued. A great deal of adjustment is often made in individual proposals.
Monitoring of performance:	Substantive progress on projects is not closely monitored although complete lack of progress (as indicated by progress reports) is grounds for termination.

Evaluation of outcomes:	No formal procedure. Reliance is placed on including scientific reputation as an item of consideration in the proposal evaluation process.

Program Evaluation

Mechanism of evaluation:	A permanent panel meets for two or three days to review the project selection decisions made by each program officer. The format is a roundtable discussion where panelists ask questions after a short presentation by the program director. Each panel reviews all the projects in a given section, which may comprise roughly a half-dozen programs.
Timing of evaluation:	A review session is held biannually, shortly after each period during which projects grants are awarded.
Implementation of results:	Program directors listen to the comments of panelists and incorporate their suggestions in future allocations.

ACTIVITIES

The Mail Review paradigm does not differ greatly from Single Review except in the way that the panel is used, so much will be a repetition of material presented in the Single Review sections. The principal difference is that in Mail Review the program director does not work with a panel in making project selection decisions. Because of this fact, there is less deviation from the Mail Review paradigm in NSF among various research programs* than is the case in Single Review.

*Except that the evaluation panel is not always used.

Program Planning

As in the Single Panel Review paradigm, programs are a cluster of projects grouped by scientific discipline, and not collections of logically interrelated projects deduced from a set of overarching goals. Thus, "fields of inquiry" might be a better name than "programs" for these units of activity. Especially important new ideas that seem likely to develop into a distinctive field of inquiry are nurtured by the program staff through the usual means: traveling, workshops, and persuasion. Some responsibility for detecting and encouraging these new ideas lies with the program directors; but in addition, a senior program director reporting at the top management level spends all his time searching out and stimulating nascent fields of inquiry which offer significant potential. His principal means of stimulation are the same as the program director's.

Program Development

At intervals throughout the year, the program director visits scientists in his field of inquiry, discussing new trends, important events, and his own and others' options. These discussions help the program officer form his scientific priorities, and keep him informed; but also serve to stimulate research proposals.

Proposals are received in the program director's office throughout the year from individual investigators. The program director sorts through the stack of received proposals and selects those that seem to qualify for funding. Each selected proposal is sent to four or five specialists for a written review of its merits. The specialists are chosen to match the subject area of the proposal and are given guidelines on the important criteria to consider in writing their review. The program director selects these reviewers from a list kept for the purpose. If the proposal is complicated or receives mixed reviews initially, additional first round reviewers are included. The first round of replies are compiled and mailed to each first round reviewer, who then revises his original review and sends it back to the program director. Upon receiving all the second round reviews, the program director has an expert, external assessment of a proposal's worth.

Twice a year, the program director examines the backlog of reviewed proposals and selects a subset of them which fits his budget and which he believes to be an "optimal" balance of innovative but risky ideas, normal science progress, special opportunities, and chances for development of new research talent in the lines of research he is pursuing. The budget of some proposals may be altered to eliminate weak portions, or increase strong ones. The program director negotiates with the applicants over these changes. The program director's final choice is then reviewed and approved by his organizational superiors.

Grants are awarded for a fixed amount expendable usually over a set period of time, usually less than a five-year period. During this time, annual progress reports are submitted, but unless major difficulties arise, the investigator is free to choose his course. An investigator who is not making reasonable progress is given one year's notice prior to a final decision to revoke his grant, but this occurs infrequently. Project outcomes are not evaluated on a formal basis.

A flow chart of this process appears in Figure 5.

Program Evaluation

Programs are evaluated by a panel of scientists, specifically appointed for the purpose. The panel convenes shortly after each period when project awards are made to review the list of projects selected and rejected, and cross-examine the program directors on their decisions. The panel evaluates all the programs managed by a section, which in NSF's case means half a dozen or fewer programs.

The meeting format is not highly structured, but is rather informal. The chairman of the panel, elected by the panel members themselves, asks each program director in turn briefly to describe his decisions. Following this presentation, the meeting is opened for roundtable discussion. The attitude of the program staff is to "throw open the files, and let them see anything they want." Back and forth discussion of each program proceeds for a while until all opinions are on the table. The panel can vote a resolution if desired, but usually the spoken comments of the members are deemed adequate evaluations.

These evaluations are reflected in future decisions only if the program directors wish to follow them, but clear signals are difficult to ignore. Scientists' opinions about the abilities of a program director easily find their way back to the program director's supervisors. In addition, the section supervisor attends the panel meeting, and talks with the panelists on an individual basis whenever the need arises.

Figure 5: Program Development for Mail Review

Activity	Researchers	Program Director	Mail Reviewers (3 or more per proposal)	Evaluation Panel (6 to 15 members)
Assessment of research needs, synthesis of new approaches, stimulation of new proposals.		o Site visit throughout research community, discussing problems, progress, and new ideas. o Suggest improvements in submitted proposals.		o Converse with program director during, before, and after evaluation session. o Discuss with invisible college colleagues.
Evaluation of proposals. (biannual)	o Generate project ideas and submit proposal.	o Review stack of proposals. Select all those potentially fundable. o Send each proposal to three or more reviewers (depending on complexity and uncertainty about quality) for written comment.	o Compare proposal received against . state of art in speciality, . feasibility, and . competence of principal investigators. o Send written comments to program director.	
		o Assemble first round reviews in package and recycle to reviewers.	o Repeat above for second round considering comments of other reviewers.	
		o Select list of projects which optimally balances important factors and meets budget constraint. May be desirable to adjust some project budgets more than others. Negotiate with investigators. o Defend package of proposals before immediate supervisor.		
Monitoring and supporting on-going projects.	o Submit informal project report annually.	o Tend to administrative needs as requested by researcher. o Read progress reports. Check for major changes. Give one year prior notice, and then terminate grant, if necessary.		
Evaluation of project outcomes.		o Selective reading of publications resulting from projects. o Discuss results with performers when occasion arises.		

ORGANIZATION

Structural Relationships

The same organization chart as for Single Panel Review applies.

Staffing Relationships

Program Directors. With rare exception, all have Ph.D.s or equivalent. The rate is one departure per approximately two positions for half of the staff, and one departure per approximately eight years for the rest. Each program director is selected by his immediate supervisor.

Mail Reviewers. Mail reviewers are selected by the program director with the objective of obtaining a match between topics in the proposed research and specialties covered by the reviewers. The list from which reviewers are drawn is frequently updated, and so long that only rarely does any reviewer see more than three proposals per year. A great deal of work must be exerted to build and maintain the list of mail reviewers.

Panel. With Foundation management approval required, the program directors and their supervisors select replacement panelists from the community of scholars. The term of office is two years, but there is no statutory requirement. The panelists span a greater range of research interests than the programs evaluated and may include applications-oriented researchers. Panelists are not paid for their services.

V. "NO" REVIEW

OVERVIEW

During the decade just after World War II, the lead agency in support of basic science in the United States was the Office of Naval Research (ONR) in the Department of the Navy. ONR had inherited this role from the Office of Scientific Research and Development (OSRD). OSRD had sponsored several large projects, such as the Manhattan project, and had functioned as a presidential level center for mobilizing scientific manpower in support of the war effort. To many, OSRD's results proved the importance of scientific research to national security, and the need for federal support of science. While agreeing with the need for continuing federal support, many scientists felt uneasy about having this support come through the military, and campaigned actively for support through an independent agency.* This effort, which was lead by Vannevar Bush, culminated in the creation of the National Science Foundation in 1950. In the meantime, however, science needed monetary support or the large effort built up during the war would have had to be disbanded. A coalition of forces managed to achieve this in 1946 by creating ONR and providing it with the needed funds. ONR used them to build strong basic research programs. By the middle 1950s, NSF had grown large enough to be the lead science agency, and ONR began to shift its emphasis to research on specific Navy needs.

Throughout its history, and especially during the basic science era, ONR has employed a distinctive form of R&D management. Its program officers have a great deal more autonomy and responsibility for building their program than most program officers, and they stress attracting the best researchers to their program, not choosing the best projects by evaluating proposals. ONR's philosophy amounts to putting its money on people rather than projects.

The program officer's mode of operation has been to consider the needs of the Navy and then identify the areas of science most relevant to those needs. Within each area, the program director tries to find the most capable researchers and add them to his program. This approach to R&D management is a modified form of the philosophy followed in OSRD.

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*Schaffer, Dorothy, The National Science Foundation, Praeger, New York, 1969, Chapter I.

It was natural that ONR adopted these policies, since much of its budget and staff came from OSRD when ONR was created. ONR went further, however, in relying on the program officer for decision making and in not using formal, external advisory mechanisms. NSF, which obtained most of its initial staff from ONR, has gone in the other direction, since it incorporates more outside advice in the management process than either ONR or OSRD.

While program directors rely little on formal review procedures, they expend a great deal of effort maintaining an informal network of contacts in the scientific community. These sources are used as a means of detecting new ideas, finding new researchers, and deciding which researchers to add to their program. Because review is not done formally but informally through this network of contacts, ONR's system is called No Review with the qualifier "No" in quotations.

Over the years, the ONR directorship together with its divisions have evolved variants of the basic management philosophy, but the purest form, used when ONR had responsibility for fostering basic science, will be described here. It relies least on formal mechanisms for linking advice from the outside into the management process. Unlike the earlier times, when all of ONR's budget came from the Navy's line item R&D appropriation, much of the budget today is obtained from operating arms of the Navy and from the Office of the Secretary. Each sponsor has different types of needs and, consequently, ONR has had to modify its original method of management to fit these new demands.

In its pure form, the essential features of the ONR paradigm are:

- *Support is given to individuals*, not to institutions, to work on a problem they have proposed.
- An understanding is reached with an investigator that *he will be supported for an indefinite period* of time, contingent on continued productivity.
- *Directing and attracting the most capable researchers* to the agency program receives the most managerial effort.

SUMMARY

General Characteristics

Primary output:	Additions to knowledge about basic processes.
Mechanism of support:	A finite duration contract is awarded to individual research scientists for level of effort support of salary and equipment expenditures, though a promise of continuing support is understood. Generally, these scientists are employees of a university, research institute, or sometimes a firm.
Managerial emphasis:	Finding and keeping a collection of the most capable researchers in fields of inquiry relevant to agency needs requires the bulk of management effort.
Staffing plan:	Program directors serve full-time in their position. Most have had at least some experience as bench scientists but only a few return to research after serving as program directors. A good portion of the program directors are young Ph.D.s.

Program Planning

Sources of new program ideas:	Research programs are collections of the work being funded, grouped by scientific disciplines. Program directors are constantly on the look-out for new ideas which arise in the scientific community, which are interesting to the program director. and which seem important to science and the agency mission.

Mechanism for planning: The most creative and capable people in new areas are sought and encouraged. Emphasis is placed on attracting young Ph.D.s.

Coordination: Areas of interest are coordinated with other agencies through informal contact at the program director's level.

Program Development

Sources of project ideas: Project ideas per se are not important to the allocation process, but to qualify for support at renewal time scientists do submit a statement of work which indicates topics to be pursued.

Means of proposal review: Other than a site visit by branch office personnel to check on research facilities, and a reading of proposals by the program director, there is no formal proposal review procedure.

Allocation of budget: The program director discusses priorities and opportunities with his contracted scientists and others throughout the year. Discussions are also held with managers in operating environments. As opportunities come along, the program director allots his budget of funds to "optimize" the levels of effort in his program.

Monitoring of performance: The program director makes at least one, and often two site visits per year to each scientist supported. Discussions cover the trend and significance of results obtained by the scientist and by other scientists, and problems encountered. In addition an annual "short form" progress report is submitted.

Evaluation of outcomes: Performers are not evaluated on a formal basis, but each program director keeps up to date on who makes the discoveries in his area of responsibility.

Program Evaluation

Mechanism of evaluation: Not done formally.

ACTIVITIES

In the "No" Review paradigm no specific mechanisms are used for program planning although management emphasizes the necessity of maintaining surveillance for new program possibilities. The policies used to implement this responsibility are described in the following section, after a brief discussion of the nature of the research activity managed.

Program Planning

As in Dual Review, Single Review, and Mail Review paradigms, research programs are not logically interrelated sets of projects deduced from overarching goals. In this paradigm the emphasis is on people, not projects. The method of management is to select the fields of inquiry which are most likely to produce results of importance to the sponsoring agency, and then attract the very best people in these fields to the agency's programs. As a result, programs are more accurately reviewed as groups of people with similar interests rather than collections of projects with logically interrelated tasks.

The groups of researchers are labeled by scientific discipline or subdiscipline since the individuals in each group have selected problems and are using methods of solution which identify them as members of a particular scientific discipline. The grouping of programs by disciplines facilitates the management process since the program director's principle means of management is tapping into the invisible college to which his researchers belong.

The program director is held responsible for detecting the significant discoveries which portend major changes in his field of responsibility. He does this by knowing almost everyone in his field reasonably well, and talking with them often. When a significant new discovery comes along, the program director learns of it through these long-established personal contacts. If he decides that the discovery is important to the agency's mission, his job is to attract the very best people in this line of inquiry to his program.

With this method great reliance is placed on the program director for determining what new directions the research program will take. It is his responsibility to assess agency needs and find the ideas relevant to those needs. His interests, and his judgments in large measure determine what new directions will be pursued. ONR's Research Management Guide says that:

> A program can be designed to strengthen those scientific subfields indentified to be of special concern to future service needs. A program can also be effective as a means of creating a focus, or even establishing a new subfield when it is lacking in the discipline.*

Compared to Dual Review, Single Review, and Mail Review methods of management, this paradigm relies less on proposal pressure for setting the allocation of money to new areas. Instead it depends on the program director's choice. Whether this results in the system lagging or leading scientific programs depends on the program director's abilities.

To the extent necessary, program directors coordinate their programs by keeping in personal touch with their counterparts in other agencies. Over time an understanding is reached about the topics each party will emphasize. Project by project coordination is not necessary since the scientific community gives no credit for duplicated effort.

If the program director's supervising managers see an area or new idea which seems important, but is being improperly supported they will add a new program director who is interested in this area to the staff before they will interfere with current programs. In the words of one division director:

> In general, we won't have a program unless a staff member of our group is professionally and deeply concerned with that area. We

*U.S. Office of Naval Research, "Research Management Guide," Department of the Navy, Washington, D.C., 1969, p. II-9

> would not decide by fiat for a program in biophysics unless we had a physicist on the staff interested in the area, visiting campuses, and drumming up support from investigators.*

Program Development

Program Development is managed with the same general philosophy as Pro-Gram Planning. In fact, by the nature of this paradigm it is difficult to distinguish between Program Planning activities and Program Development activities.

The responsibility of each program officer is not to "take proposals and score them, but to develop programs."** He does this by maintaining "a really cogent grasp of his scientific or technical field,"*** and knowing who are the best people in his field. His chief responsibility is to attract these researchers to his program. The way this is done has been described as follows by a division director:

> Informally, we use the whole scientific community. There is a lot of travel. We visit contractors and non-contractors. We visit most of the professional society meetings, and have extensive conversations regarding research. The use of the scientific community is not for the review of individual proposals but with respect to individual investigators. At the time I was at the branch level, there were probably not five people in my field not known personally to me.****

Each researcher supported by the program director is site-visited once and usually twice per year. The program director finds out about new results the researcher has obtained, new results obtained by others and the importance of all these results to scientific progress. By continually checking with his researchers, and others too, the program director keeps aware of who is making the important contributions in his field of responsibility.

Particular attention is paid to monitoring the crop of new doctoral graduates. By talking to his network of contacts the program director can

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*Rettig, Richard, "Federal Support of Scientific Research, a Comparative Study," unpublished Ph.D. thesis, Massachusetts Institute of Technology, August, 1967.

**Rettig, op. cit., p. 90

***U.S. Office of Naval Research, "Research Management Guide," Navy Department, Washington, D.C., p. II-2.

****Rettig, op. cit., p. 97

find who the best students are and what they have done. Then he arranges appointments and finds out what each student's interests are. At a later time, the program director informally suggests to some of these graduates that they should submit request for support.

The program director stands a good chance of attracting these graduates, or anybody else he wants, becuase he offers the likelihood of long term support. The program director can promise continuing support to investigators, because he has been delegated great authority over the expenditure of his budget as a concommitent of responsibility for getting the best people into his program. Indefinite support is not guaranteed, however, since that would reduce incentives for performance and limit the possibilities for changing program direction to capture new opportunities. He makes support contingent on continued productivity and mutuality of interests.

Contracts with researchers last for a finite period, renewable in competition with other prospective contractors eligible at the same time. Each prospective contractor submits a proposal, but formal requirements are minimal. The proposal should state the investigator's qualifications, his facilities for research, a brief statement of research objectives and scientific methods to be employed, and a project budget.

As the first step in reviewing proposals, the closest ONR regional office sends an officer out to check on the proposing investigator's research facilities, and work environment. The program director reads this report, and the investigator's proposal; and then decides whether he will support the investigator and at what level. He often discusses this decision with his branch chief and with other program officers. Awards are not made at predetermined times, but occasionally as opportunities arise or needs become great.

In making his allocation the program director is cautioned against following the advice of his contacts against his judgment of what is best for agency needs or science in larger perspective. The Research Management Guide says that the program director:

> should be wise in supporting fields of special naval relevance that may not be currently popular. To effect this he should maintain personal contact with the recognized leadership in the discipline, ... but must also recognize that science has

> its fads and fancies and followers thereof. The program director must avoid becoming a 'follower' by steering clear of intimacy with any one camp. Indeed, it is almost always better for the ONR program director to rely, in the last analysis, on his own educated and open-minded judgment with the best interests of the Navy at heart, than to rely solely on biased peer-grouped (sic) judgments.*

A tabular description of the Program Development process appears in Figure 6.

Program Evaluation

No formal mechanism for Program Evaluation is employed; but on an informal basis, the branch director is responsible for maintaining the quality, balance, and productivity** of all the programs under him. He does this through continual interaction with his program directors. The branch chief allocates his budget allotment to the program directors.

ORGANIZATION

Structural Relationships

An organization chart for ONR is shown in Figure 7.

Staffing Relationships

Program Directors. With rare exception all have Ph.D.s or equivalent. All serve on a full-time basis, although half are young Ph.D.s, and stay only for a short time. More often than with other management models, research experience is a qualification for the program director's position. Program directors are selected by their immediate supervisor.

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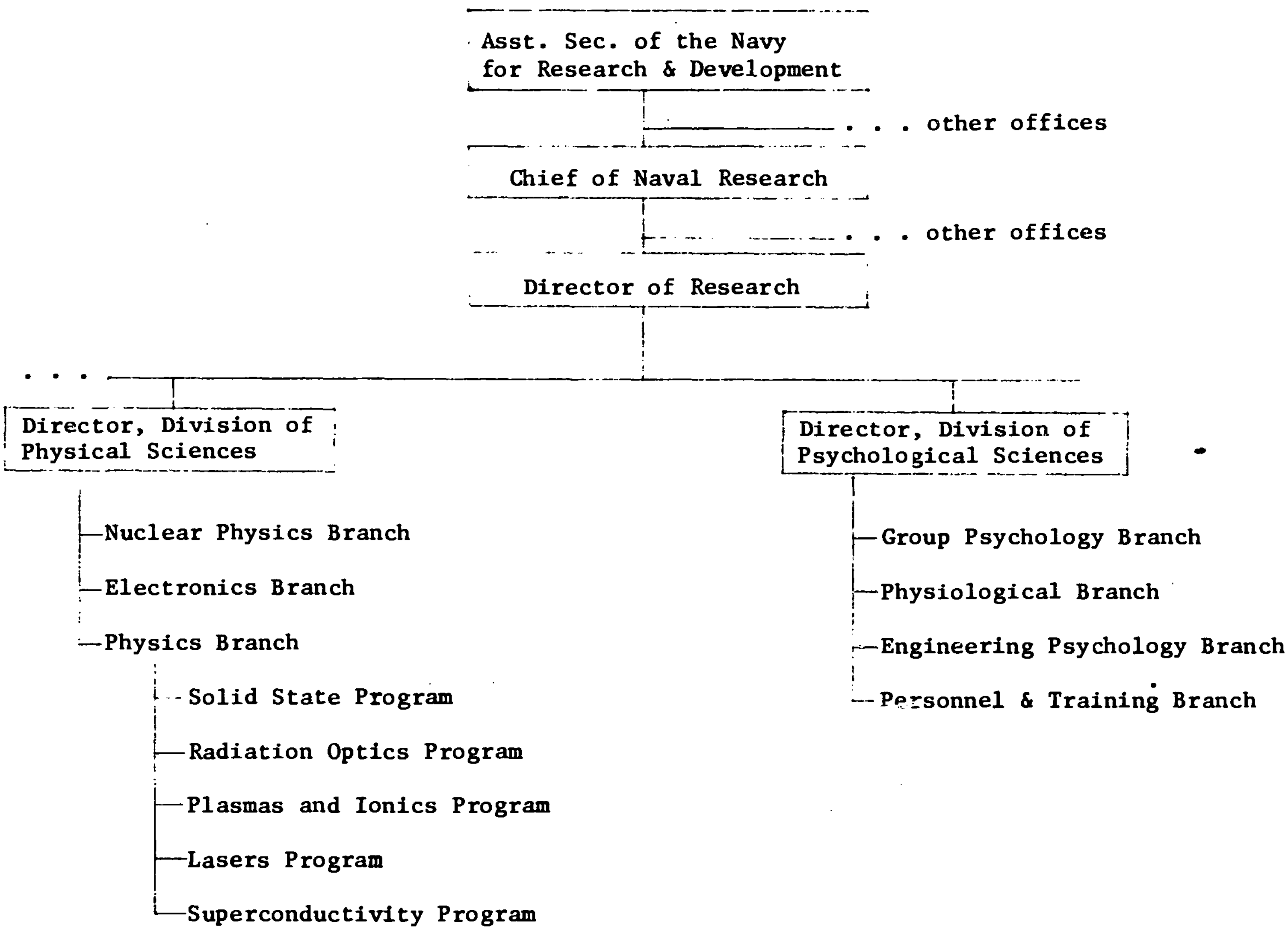
*Office of Naval Research, "Research Management Guide," Department of the Navy, Washington, D.C., 1969.

**Rettig, op. cit., p. 90.

Figure 6: Program Development for No Review

Activity	Program Director
Assessment of research needs, synthesis of new approaches, stimulation of new proposals	o Site visit throughout research community, find people doing good work in promising directions o Offer support to some of these researchers. Place emphasis on supporting young Ph.D.s
Evaluation of proposals	o Assess if continued support is warranted based on past record and proposed work
Budget justification and allocation	o Select a group of researchers and juggle awards to fit budget allowed by supervisor
Monitoring and supporting ongoing projects	o Site visit projects, assess progress, convey results to other researchers o Suggest changes in research projects
Assessment of project outcomes	o Selective reading of publications resulting from projects o Discuss results with researchers

Figure 7: Organization Chart for Office of Naval Research, 1969.



Appendix A

STRUCTURE OF FUNDAMENTAL RESEARCH

John Wirt

INTRODUCTION

This appendix is a paraphrased* summary of T. S. Kuhn's well-known work, the *Structure of Scientific Revolutions*.** Among all the models of the scientific research process examined, Kuhn's was found to be the most penetrating and operationally useful. It describes differences between the various stages of the fundamental research process and specifies mechanisms through which scientific progress is made. Knowing these factors, management can be more precise in setting conditions that promote progress, and better understand what results should be expected from fundamental research. Thus, Kuhn's model provides conceptual tools that aid in thinking about what procedures to use in managing fundamental research and when to use them.

Kuhn eschews the simple model that fundamental research produces an inexorable, steady accretion of proven propositions. His evidence is that historical facts do not fit with an "accretive" model of scientific research. In attempting to chronicle science as an incremental process, science historians suspect that these questions, which are relevant only if science develops by the accumulation of individual discoveries and inventions, are the wrong sort to ask. Simultaneously, these same historians confront growing difficulties in distinguishing the "scientific" component of past observations and beliefs from what earlier historians of science had readily labeled "error" and superstition." The more carefully they study, say, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, the more certain they feel that once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy

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*Any inadequacies or inaccuracies of interpretation are the author's responsibility.

**Kuhn, T. S., The *Structure of Scientific Revolutions*, International Encyclopedia of Science, Vol. II, No. 2, Univ. of Chicago Press, Chicago, 1970.

than those current today. If out-of-date beliefs are to be called myths, then myths are still possible in scientific knowledge because the same methods used conducting science in the past are still employed today. If out-of-date beliefs are to be called products of science, then science has included bodies of belief quite incompatible with what science holds today. Given these alternatives the historian of science is compelled, says Kuhn, to choose the latter alternative, and conclude that out-of-date theories are not in principle unscientific because they have been discarded. This choice, however, makes it difficult to see scientific research as a process of accretion alone, but also revolution where past accomplishments are discarded in favor of new accomplishments.

Problem Solving in Fundamental Research

The conventional model is that scientific knowledge is embodied in theories, laws, and rules, and that students learn a scientific field by learning these theories, laws, and rules. The conventional model is also that students learn to apply these fundamentals to practical cases by solving concrete problems. Kuhn asserts that this localization of the cognitive content of science is misplaced. Students do not learn laws, or theoris, or rules in the abstract, and then how to apply them. Rather, Kuhn says, a student learns a scientific field by learning the solutions to problems. Whether or not a student learns laws and rules in the abstract is debatable, for even scientists, claims Kuhn, are little better than laymen at characterizing the bases of their field. The solutions that students learn are the specific examples encountered from the start of scientific education, including solved problems in textbooks, problems at the end of chapters, laboratory experiments, and journal publications. Kuhn calls them exemplars. Given a new problem the student finds its solution by perceiving similarities between problems he can solve and the problem he wants to solve.

After solving enough problems some students are able to achieve a higher skill, which is the ability to compose new problems that are like ones already solved, but requiring innovation to solve them. Kuhn asserts that students never learn rules for solving problems, or selecting new problems, or formulating new problems; yet with study students gain an ability to pose new problems in their field that, when solved, are a contribution to knowledge. Kuhn calls these problems puzzles for their analogous characteristics with more familiar puzzle forms.

When a student attains this ability to pose and solve significant puzzles, he qualifies as a member of the scientific community with a speciality in the field of inquiry he has mastered. The process of acquiring knowledge of and the ability to extend scientific laws and concepts by learning the solutions to exemplary problems Kuhn and others call "learning by doing."

Knowledge Paradigm

Thus, Kuhn argues that the set of exemplars is one of the principal elements of knowledge in a field of inquiry. Exemplars define the domain of a field of inquiry, determine the rules that govern the formulation of new problems, specify acceptable forms of solutions, and express the scientific laws that have been discovered. Exemplars function for researchers in a field of inquiry in much the same way that court decisions determine rules and law for judges practicing the common law tradition. The exemplars are the points of agreement on fundamental entities and interactions which a scientific group must have to guide its way deeply into the complexities of nature. It is in this sense that exemplars are a tacit expression of the state of knowledge in a scientific field of inquiry.

In addition to exemplars, Kuhn has identified three other elements of tacit knowledge in a field of inquiry. These are that the members of a scientific community in a field of inquiry:

- Have a shared commitment to certain beliefs -- such as the molecules of a gas behave like tiny elastic billiard balls or that certain kinds of equipment should be used for experimentation;
- Agree on the meaning of some symbolic representations -- such as f=ma (which is operationally useless to someone who has not solved Newtonian problems); and

- <u>Share a set of values</u> -- such as the appropriateness of imposing social concerns during problem formulation, the degree of simplicity demanded in theories, and tolerable margins for error in predictions.

Altogether, Kuhn calls these elements a knowledge paradigm. These knowledge paradigms bear the same relation to the scientific laws and rules in a field of inquiry as the management paradigms of this report bear to particular philosophies of management. Kuhn asserts that it is not hard for researchers in a field of inquiry to reach a consensus on what the essential elements in their paradigm are, but usually impossible to get a consensus on what the fundamental concepts, laws, and rules are. Kuhn goes even farther by suggesting that rules and laws do not exist in science, and gives some evidence. In any case rules and laws are not needed for scientific progress to occur. Kuhn concludes that knowledge paradigms are the fundamental units in the scientific research process.

The size of a paradigm can be gauged by noting the number of researchers sharing it, which means the number who know the paradigm and use it in their work. In most cases, less than 100 researchers actively share a paradigm at any one point in time. Usually, these researchers will come from a variety of scientific disciplines and subdisciplines. The scientific community can be partitioned at the most aggregate level by categories such as natural scientists and social scientists; and at a finer level by disciplines such as physicists, chemists, astronomers, and others. At the next finer partition, the groups can be labeled by subdisciplines; organic chemists, solid-state physicists and so on. In most cases there are substantially more than 100 researchers in these categories. Thus, the community that shares a paradigm is at an even finer level of partition than subdisciplines groups. And, since the community sharing a paradigm is usually interdisciplinary in nature, the partition of scientific activity into paradigms produces groupings of scientists which intersect several of the discipline categories of scientists.

One example of a research community having a shared paradigm is the <u>phage</u> group of molecular biologists and others, which has recently been studied in the history of science literature. Another example is the group of economists who share the general equilibrium, perfect competition paradigm. Another example of a paradigm is Newton's Theory of Motion. This paradigm has been replaced by another paradigm, Einstein's Theory of Relativity.

Using the concept of a knowledge paradigm, Kuhn then presents a model for the dynamics of the knowledge development process. His model has four phases:

- Normal Science,
- Crisis,
- Revolution, and
- Preparadigm Research.

Each of these activities will be discussed in section below.

NORMAL SCIENCE

The first phase of the knowledge development process is normal science where the research activity is directed toward actualizing the promise offered by a new paradigm. Most scientists spend the great bulk of their lives in normal science pursuits. The attempt is to force nature into the preformed and relatively inflexible conceptual boxes that the paradigm provides. This is done by gathering facts that the paradigm displays as revealing, by increasing the match between fact and prediction by amending theory, and by further articulating the paradigm to extend its scope and precision. At any time a paradigm contains a core of "recurrent and quasi-standard illustrations" of theory and its application, but also a "penumbral" area of speculations and achievements whose status is still in doubt. An objective in normal science is to resolve these ambiguities and at the same time open up new ones for investigation. The researchers resolve these ambituities by solving the implicit puzzles posed by the ambiguities and framed by the shared paradigm. Concurrently, however, new ambiguities arise from apparently arbitrary factors, compounded of personal and historical accident which is always a formative ingredient in normal science progress. But, normal science is inherently a narrowly construed activity. The paradigm forces researchers to investigate a part of nature in a detail and depth otherwise unimaginable. There is no attempt to probe for unexpected novelty either conceptual or phenomenological for such a discovery could possibly upset the paradigm on which normal science depends for guidance. The game is to prove the expected. Any search for fundamental novelties is resisted. Unlike the other phases of fundamental research, normal science is cumulative in nature.

While the normal science does not aim at novelty, paradoxically, its result is almost invariably to expose anomalies between paradigm predictions and fact. Paradigms which do not reach this stage after an extended period of time are gradually dropped by the scientific community and assimilated into engineering. The paradox is that in achieving greater scope and precision with normal science activity, it becomes simultaneously easier to detect where theoretical predictions and fact are misaligned. The frequency with which simultaneous discovery occurs is testimony to the completeness of the normal science tradition and the way it prepared for change.

CRISIS

Appearance of anomalies in significant number signals the onset of crisis, another stage in fundamental research activity. Some anomaly always exists in normal science, for that is what makes puzzle solving a challenge. But, the presence of too many anomalies can raise a crisis. Crisis becomes acute when the complexity of a paradigm proliferates sufficiently to loosen the rules of normal puzzle solving, or increases faster than the paradigm's explanatory power. Crisis can also be heightened by external social pressures, as was the case near the end of the Ptolemaic system of astronomy, when difficulties with the calendar became increasingly irritating.

Response to Crisis

The responses to crisis are many and varied. The minimal response is to avoid the anomalies and work on other problems. Greater response is likely if the anomalies call a fundamental generalization of the paradigm into question, or if derivations of the paradigm are used in practice. A crisis state is marked by a turn to philosophical analysis, and debate on paradigm fundamentals. Articles expressing discontent often appear. Many resist the anomalies in the belief that old methods will prove adequate to resolving the differences and sometimes they prove correct. Others mount increasingly divergent theoretical and experimental attacks on the unsolved problems, exhibiting a willingness to try novelties not acceptable during normal science activity. Sometimes effort may concentrate on the attacking anomalies directly by doing research which exposes them more clearly. This activity generally results in novel discoveries unlikely to have occurred during normal science and proposal of many radically different theories.

Result of Crisis

The final outcome of a crisis state varies, too. One outcome has already been mentioned, that the crisis is resolved by reworking the old paradigm. Other times the anomalies can be so severe that the paradigm is set aside. In this case the researchers who shared the paradigm will migrate to other fields. Another result is that a new paradigm appears and eventually replaces the old one.

New paradigms emerge from the sequence of novel discoveries and proposals of theory that are the scientific community's response to crisis. In the past more than a decade of crisis activity has often passed before the new paradigm emerged. Study of the atomic nucleus is probably in a crisis state at the present time, since the number of competing theories is getting larger, and elucidating experiments show greater complexity than expected.

The emergence of a new paradigm is a complicated process and occurs over an extended period of time, contrary to the commonly accepted perception that scientific discoveries are traceable to a single event. Usually a "complex of observation and conceptualization, fact and assimilation to theory are inseparably linked in discovery." The awareness of anomaly, a constellation of unrelateable facts produced by the work of many people, and an array of proposals for theory are often formative ingredients in the detection of a scientific novelty. Sometimes facts and theories previously overlooked may be rediscovered. Kuhn describes the process by which a paradigm emerges by using the metaphor of a psychological experiment where subjects were given short and controlled exposure to a sequence of playing cards. All the cards in the sequence were normally figured but some were miscolored. For example, one was a red six of spades. The subjects were not told of the anomalies. At short exposure durations all subjects identified the cards without hesitation at their normal face value. As the exposure duration was increased, some subjects began to hesitate and display awareness of anomaly. Upon further increasing the exposure duration these subjects identified all the cards correctly. A few subjects, however, were never able to make the requisite adjustment of their categories, even at 40 times the average exposure needed to identify normal cards. Kuhn attributes the same set of characteristics to the emergence of a new paradigm: "previous awareness of anomaly, the gradual and simultaneous emergence of both observational and conceptual recognition, and the consequent change of paradigm categories and procedures often accompanied by resistance. Fact and theory emerge simultaneously.

REVOLUTION

The scientific community has no calculus for determining whether or not to adopt an emerging paradigm (or if it is the case, one of several paradigms). Early versions of paradigms are rarely successful at solving all the anomalies which have arisen, and usually fail to solve all of the problems already solved in the old paradigm. New theories usually fit data poorly, and no objective measure for goodness of fit exists to provide a basis for comparison. Furthermore, and it is Kuhn's most difficult point to perceive, adopting a new paradigm entails a gesalt switch in the researcher's view of the world. Thus, adherents to different paradigms are dealing with incommensurate entities and talk past each other on the subject of which paradigm should be used.

The gesalt switch involves more than just a change in the way a set of data is interpreted. The model that different views of the world are only different interpretations of the same data can be traced to Descartes. Kuhn asserts that this paradigm is in a state of crisis itself due to results in philosophy, psychology, linguistics, and even art history but no alternative paradigm for explaining perception has emerged. The difficulty of rejecting Descartes' paradigm is seen in Kuhn's paradox, "that though the world does not change with a change of paradigm, the scientist afterward works in a different world." The history of science shows, says Kuhn, that scientists measure different variables and engage in a different set of laboratory manipulations after a new paradigm is adopted. "What occurs in (adopting a new paradigm) is not fully reducible to a reinterpretation of individual and stable data ... Interpretive enterprise ... can only articulate a paradigm, not correct it," where correct means to transform it it into truth equivalent to nature. Paradigms are necessarily artifacts, albeit esoteric ones, that are "not corrigible by normal science at all. Instead normal science ultimately leads only to the recognition of anomalies and to crisis. And crisis is terminated, not by deliberation and interpretation, but by a relatively sudden and unstructured event like the gesalt switch. Scientists then often speak of the 'scales falling from the eyes' or of the 'lightening flash' that 'inundates' a previously obscure puzzle, enabling its components to be seen in a new way for the first time ... No ordinary sense of the term interpretation fits these flashes of intuition that are part of the paradigm-emerging process. Though such intuitions depend on experience, both anomalous and congruent, gained with the old paradigm, they are not logically or piecemeal linked to particular items of that experience as an interpretation would be." Instead large portions of that experience are gathered, and transformed into a "rather different bundle of experience" and "thereafter ... linked piecemeal to the new paradigm but not to the old."

Kuhn calls the process of rejecting an old paradigm and adopting a new one a scientific revolution. As mentioned before revolutions occur gradually because the new paradigm is not clearly effective in resolving the crisis, and requires a fundamental, qualitative shift in perception that scientists may not easily adopt. Appeals to the esthetic may be required. Furthermore, assimilation often requires that some former theory and fact be abandoned, that which is sometimes difficult to do. Thus, scientists do not switch rapidly to a new paradigm as it emerges.

For all these reasons, a scientific revolution is not an additive or incremental occurrence. Eventually greater scope and precision in predicting nature will be achieved after a period of normal science activity, but a fundamental change of perception will have occurred and a different set of problems will be solved. Whether or not an absolute gain in knowledge is achieved is not a matter of analytic estimation.

PRE-PARADIGM SCIENCE

The usual pattern in mature science is successive transition from one paradigm to another via revolution over decade or decades-long cycles. The fields normally considered to be science, such as most of the natural sciences, display this pattern.

Some fields of fundamental research have not reached this stage, particularly in the social sciences. Kuhn labels these fields as being in the pre-paradigm stage of development, a stage in which all fields of fundamental research begins have begun.

Pre-paradigm research is recognizable by the existence of debate on fundamentals, and whether or not the field is a science. These debates serve to define schools of thought rather than solve problems. When the paradigm state is reached, such debate ceases. Another clue to the pre-paradigm condition is the absence of the puzzle solving activity that is so crucial to normal science. Lacking in agreement on fundamentals, methodologies, and instrumentation, pre-paradigm scientists must always start their major expositions from a set of specified first principles. Activity is characterized by a continual competition between a number of distinct views of nature all roughly compatible with the dictates of the scientific method, but not with each other. Fact gathering is more random than in the paradigm stage and more limited to the wealth of data at hand. Technology and practice (like medicine) are more likely to be a source of data, since esoteric facts which could not have been so easily discovered by casual means may lie exposed. The transition to paradigm science has been made when specialized journals form, and laymen can no longer keep track of progress by reading original papers. Books will not be closely related to professional status either.

IMPLICATIONS FOR FUNDAMENTAL RESEARCH MANAGEMENT

Most of the management implications of Kuhn's model are explored in WN-7680, where R&D management methods are evaluated. Some points, however, can be made here.

First, the role of exemplars in paradigm development makes the utilization of scientists or at least scientific opinion in project selection processes imperative. The logic of Kuhn's argument is that there is no other way to maintain the integrity of normal science progress unless those making the project selection decision know the stock of solved problems. The role of exemplars also suggests strongly that in managing fundamental research, reliance should be placed on unsolicited proposals from scientists as the source of project ideas.

A second aspect of Kuhn's models of importance to management is that sudden, dramatic discoveries of practical importance will not often appear, nor will the research process respond to shifting practical priorities. The emphasis is more on the esoteric, and penetrating exploration of problems which previous research implies can be solved than timely response to pressing practical needs. Promising approaches are pursued to great depths of detail though they are of rather narrow concern. Progress comes in large spurts coinciding with the occurrence of scientific revolutions. But, these happen on a scale of decades, not years, so management needs to be patient.

Another key implication of Kuhn's model is that disciplines are not the appropriate categories for distinguishing among scientists when placing them on panels or using their advice in other ways. A more useful categorization would be based on paradigms, since a group of scientists knowing the same paradigm yet having different discipline backgrounds will be able to communicate better with each other and make sounder judgments in their field than a group of scientists all from the same discipline but adhering to different paradigms.

Appendix B

ASSESSMENT WORKSHOPS

INTRODUCTION

The National Institute of Dental Research has devised "Assessment Workshops," as a means of evaluating and revising research programs. Assessment workshops are formal in procedure, but largely indirect in their effect on the research community.

DOMAIN OF A WORKSHOP

The assessment workshop technique is intended to deal with research paradigms* of small size. Examples on which NIDR has assembled panels are "Genetics of the Cleft Palate," and "Dental Amalgam" (the material used to fill teeth). Typically, there are fifty to one hundred researchers worldwide associated with the paradigm, which is less than the four hundred or more typically associated with basic science paradigms. Another gauge of the size of these research paradigms is that NIDR expects that each program director will conduct one or two assessment workshops each year on paradigms in his area of responsibility, returning to each paradigm about once each five years. The NIH organization chart in the Structural Relationships section of the Dual Review Paradigm description lists the areas of responsibilities that program directors have in NIDR.

PANELISTS

About fifteen panelists are chosen by the program director. Participants are very carefully selected to achieve representation of:

- all of the *significant research approaches* to the paradigm,
- *practitioners* who have reputation for quality research on the practical derivatives of the paradigm,
- *foreign researchers* (if they have contributed),

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*For a definition of research paradigms, see Appendix A.

- o *industrial researchers* and developers,
- o *spokesmen for lines of research* the institute wants to encourage, and
- o at least one or two *"elder statesmen"* of the paradigm.

All of the nominees must be very near the top quality in their specialty. The mix must include some who are known to hold countervailing views. Importantly, all must be conversant with a large part of the research paradigm under review. The ability of panelists to function harmoniously and supportively in a group session must be predetermined through observation in other panel sessions.

FORMAT

Some of the nominees are requested to submit a *state-of-the-art paper* which achieves two objectives. First, it summarizes the current state of the art in one approach to the research paradigm. Second, it states which topics within this approach should be supported in the future, based on their potential for practical and scientific results. These papers are circulated to the participants several weeks before the meeting. The authors are free to publish their work, but NIDR does not. Many of the participants prepare counter-papers, as some very strong positions against the current lines of research are taken by some authors.

The formal session is planned by a steering committee consisting of three consultants chosen by the program director. These consultants also serve as co-chairmen of the formal session. The consultants work with the program director and his staff in selecting the panels, in drawing up the agenda for the formal session, and in nominating those who will submit state-of-the-art papers. A sample agenda from the Assessment Workshop on Dental Amalgam is as follows:

Introduction

Structure of Amalgam

Phase Description and Influence of Composition on Microstructure

Influence of Alloy Particle Size and Shape

Influence of Treatment of Alloy Ingot

Properties

Mechanical

Dimensional

Corrosive

Clinical Performance

Methods of Evaluation

Results of Clinical Studies

Comparison with other Materials

Recommendations for Future Research.

The sub-entries in this agenda are what have been called *research approaches*. The major divisions of the agenda will be called *research areas*.

The agenda is to have three phases: First, set down the record of research; second, summarize, assess, and relate to current basic research activities the results of clinical research on the usefulness of practical applications of the paradigm. Third, discuss the best directions for research to take in the future.

Attendance at the session is limited to panelists and institute staff. No observers are allowed.

SCHEDULE

Assessment Workshops begin with an introduction by the program director. He emphasizes the two purposes of assessment workshops, acknowledges the high caliber of the participants, and gives a briefing on meeting dynamics. He informs the participants that they will be asked to evaluate meeting dynamics at the end of the formal session. The introduction lasts for about one-half hour.

Four-fifths of the workshop is devoted to presentation of the state-of-the-art papers prepared by the selected participants. One paper is presented on each of the research approaches on the agenda. Each formal paper is followed by a discussion period lasting one-half hour to an hour, where counter-papers are presented, and the panelists debate informally over which topics should be pursued within the approach. During discussion there is a strict prohibition against debating the practical usefulness of the research approach. Debate only on scientific merit is allowed. The purpose of debate is to get agreement on the current state-of-the-art and the prospects for advance in the state-of-the-art. The issue of practical usefulness is discussed later in the workshop.

The last few state-of-the-art papers deal with the clinical aspects of the research paradigm. The subject of these papers is to assess the performance of the system being researched in actual practice. An item of particular importance is whether or not performance has improved over the years. In the case of dental amalgam the question was whether or not amalgam restorations last longer, and are easier to install than before research started. The last state-of-the-art paper in the Amalgam Workshop was a comparison of dental amalgam with other restoration materials.

The last fifth of the meeting starts by having one of the three consultants on the steering committee present a paper that summarizes the directions for future research that seem to be the sense of the meeting. A lengthy discussion period follows where the importance of research to practice is discussed.

OUTPUT AND DESIRED EFFECTS

The tangible output of the Assessment Workshop is a synopsis of the meeting prepared by the three-man steering committee and the program director. It is published in a top-quality dental journal. The principal requirement of this review is that it be short and written so that more than the insiders can understand it.

An intangible output of the Assessment Workshop, which is perhaps the most effective, is the reorientation and general overview provided to the key researchers in the field.

One by-product of the workshop is that NIDR obtains materials useful in preparing budget justifications. Another by-product, although it was not mentioned by NIDR, is that NIDR's efforts to recruit new disciplines into an area are legitimized and directed.