INFORMATION TO USERS

This manuscript has been reproduced from the microfilm master. UMI films the text directly from the original or copy submitted. Thus, some thesis and dissertation copies are in typewriter face, while others may be from any type of computer printer.

The quality of this reproduction is dependent upon the quality of the copy submitted. Broken or indistinct print, colored or poor quality illustrations and photographs, print bleedthrough, substandard margins, and improper alignment can adversely affect reproduction.

In the unlikely event that the author did not send UMI a complete manuscript and there are missing pages, these will be noted. Also, if unauthorized copyright material had to be removed, a note will indicate the deletion.

Oversize materials (e.g., maps, drawings, charts) are reproduced by sectioning the original, beginning at the upper left-hand corner and continuing from left to right in equal sections with small overlaps. Each original is also photographed in one exposure and is included in reduced form at the back of the book.

Photographs included in the original manuscript have been reproduced xerographically in this copy. Higher quality 6" x 9" black and white photographic prints are available for any photographs or illustrations appearing in this copy for an additional charge. Contact UMI directly to order.

UMI

A Bell & Howell Information Company
300 North Zeeb Road, Ann Arbor MI 48106-1346 USA
313/761-4700 800/521-0600

DISSERTATION

ORGANIZATION AND MANAGEMENT OF NATURAL RESOURCES AND ENVIRONMENTAL RESEARCH

Submitted by

Bruce Peter Van Haveren

Department of Rangeland Ecosystem Science

In partial fulfillment of the requirements

for the Degree of Doctor of Philosophy

Colorado State University

Fort Collins, Colorado

Fall, 1998

UMI Number: 9922038

COLORADO STATE UNIVERSITY

November 11, 1998

WE HEREBY RECOMMEND THAT THE DISSERTATION PREPARED UNDER OUR SUPERVISION BY BRUCE PETER VAN HAVEREN ENTITLED *ORGANIZATION AND MANAGEMENT OF NATURAL RESOURCES AND ENVIRONMENTAL RESEARCH* BE ACCEPTED AS FULFILLING IN PART REQUIREMENTS FOR THE DEGREE OF DOCTOR OF PHILOSOPHY.

Committee on Graduate Work

A.A. Dyer

Robert M. Lucero

R.G. Woodmansee

Advisor

Department Head

ABSTRACT OF DISSERTATION

ORGANIZATION AND MANAGEMENT OF NATURAL RESOURCES AND ENVIRONMENTAL RESEARCH

The organization and management of research lacks a unifying theoretical foundation. A post-reformative theory of research management is based on six cornerstones: 1) the research enterprise consists of multiple dimensions and this multiformity is potentially synergistic; 2) knowledge is gained incrementally throughout the research process; 3) research is a form of societal investment possessing both risks and potential gains; 4) research organizations are inherently self-organizing and dynamic; 5) research is increasingly pluralistic and heterogeneous; and 6) research evaluations must focus on processes, outcomes, or overall effectiveness, in terms of both intrascientific and extrascientific contributions.

Based on observations of 14 environmental research groups at six environmental research laboratories, group research organizes naturally and informally in environmental research settings primarily because of the interdisciplinary nature of environmental research. Groups were not necessarily identifiable in organizational charts. Often they were spontaneously occurring dyads or clusters of individuals with similar interests or interdependent skills. A formal division and branch structure hinders group research

because of fiefdom attitudes of branch chiefs. Epistemological differences exist within research groups and may present obstacles or result in dysfunctional groups. Research groups must spend considerable time on problem definition, problem analysis, working towards a group goal, and developing a common system of inquiry. Perceived performance did not correlate well with measured performance. If perceived performance is a part of research evaluation, perceptions of performance must be specific as to performance criteria. For the research groups studied, member-perceived publication quality was not well correlated with measured publication quality. Director-perceived performance did not reflect measured performance. Goal congruence between group leaders and members was high in the case of the fourteen research groups. However, communication about expectations and performance broke down between laboratory directors and research groups.

The dynamic constellation, an organizational model stressing a flexible, organic, group-oriented structure and integrator and boundary-spanner roles, is recommended for natural resource and environmental research organizations. A multidimensional research portfolio is suggested as a management approach. Managing research portfolios in a pluralistic and heterogeneous environment involves a large number of essential tensions, but these tensions also become an effective management tool.

Bruce Peter Van Haveren
Rangeland Ecosystem Science
Colorado State University
Fort Collins, Colorado 80523
Fall, 1998

ACKNOWLEDGMENTS

Doctoral programs are never solo efforts. My wife, Pearl, as always was my best friend, supporter, confidant, and my prodder whenever I needed prodding. This indeed has been a mid-life experience. During the time I was involved with my doctoral studies, I turned 50 and became a grandpa. My children, Shawna, Chad, and CJ, all passed the age of 20 and started on their own education and career paths. I hope that my perseverance and lifelong learning ambitions have served as a positive example for them.

My graduate committee, Professors Dennis Child, Al Dyer, Jack Hautaluoma, Bob Lawrence, and Bob Woodmansee, provided both advice and support at critical times. Dr. Woodmansee, my advisor, showed extraordinary patience with his "nontraditional" student. Thanks also to Dr. Freeman Smith, a long-time friend and colleague, for advice and for serving as a last-minute substitute on my committee.

I thank the Environmental Protection Agency, Office of Research and Development, for funding and cooperation that allowed me to conduct the environmental research laboratory study.

Finally, I want to dedicate this dissertation to my maternal grandparents, Mr. and Mrs.

Peter Verlare, both deceased, for kindling my interest in natural resources at an early age. They taught me to value open spaces and renewable natural resources. My parents, also now deceased, valued education. They would have been proud.

PREFACE

During my 30-year career in natural resources management, I became intrigued with how natural resource science organizations work, how they are structured, how they are managed, how they relate to their clients, and how they convert inputs into outputs. More specifically, through my participation on interdisciplinary teams of scientists tasked with a variety of problem-solving and research responsibilities, I have come to believe that the adept use of such groups will lead to more useful scientific results, a better set of problem solutions, and a higher level of research effectiveness. Interdisciplinary teams have considerable potential in natural resource and environmental research because of the complexity and cross-disciplinary nature of the management issues and research problems.

President Thomas Jefferson established a precedent for the federal sponsorship of natural resources and environmental research when he sanctioned the Lewis and Clark Expedition of 1803-06. He took a personal interest in the Expedition, conceiving the idea, setting expedition objectives, assigning specific responsibilities to Captain Lewis, and even planning many details of the project himself. There was no debate over whether the expedition represented basic or applied research. President Jefferson had in mind both strategic and political goals for the project. But he also had a genuine interest in new scientific and natural history discoveries. He was taking the initiative and executing his duties under the U.S. Constitution, which directed Congress to "promote the Progress of Science and useful Arts."

As the 200th anniversary of the Lewis and Clark expedition nears, the reasons for conducting scientific investigations of natural resource and environmental phenomena are just as compelling as they were 200 years ago. Natural resource and environmental issues keep changing and must be addressed in ways that incorporate scientific inquiry. The world will always require fundamental knowledge about natural and human-modified ecological systems.

This dissertation explores the underlying tenets of scientific inquiry and the organization and management of research in natural resource and environmental research settings. The theoretical framework proposed and the research management techniques outlined should be of interest and use to research administrators, project leaders, and scientists alike.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

LIST OF KEYWORDS

dynamic constellation
environmental research
epistemology
interdisciplinary research
natural resources research
reformative
research effectiveness
research evaluation
research groups
research organizations
science policy
scientific inquiry

CHAPTER I

INTRODUCTION

Science is changing in response to a number of societal trends and events and the administration of scientific research must keep pace with these changes. In short, science organizations in general and research administrators in particular require new management frameworks, alternative organizational designs, and new methods of evaluating scientific activity. Moreover, a general theory of research administration is long overdue.

This dissertation deals with contemporary, and often contentious, issues surrounding the administration of natural resources and environmental research. I explored the nature of scientific inquiry, the formulation of science policy, the use of science in policy- and decision-making, the organization and management of research programs, the concept and measurement of research effectiveness, and the use of interdisciplinary groups in natural resources and environmental research. These are critical issues for the 1990's and beyond.

Research is defined as a set of specialized social and cognitive activities aimed at achieving certain research goals and performed by an individual, or specialized group of interacting individuals, functioning within a given institutional environment located in a specific sociocultural setting (Stolte-Heiskanen, 1987). For purposes of this dissertation I will define natural resources research as scientific activities that contribute to the management, conservation, and development of natural resources, including research and scientific assessments designed to understand the structure and function of the biosphere and the impact that human activities have on it. I will not distinguish between natural resources and environmental research. This is consistent with the recent changes in science program classification proposed by the Office of Science and Technology Policy (OSTP), which combines natural resources and environmental research into a single category (Mervis, 1993). It is also consistent with Romesburg's treatment of natural resources and environmental science as a single category (Romesburg, 1991).

Since a review of the literature revealed that research administration lacks a theoretical foundation, my primary goal is to build an overarching theory for the organization and management of natural resources and environmental research. Such a theory is required to understand the role of science in natural resources and environmental management, to increase research effectiveness, to improve the management of research programs and projects, and to improve cogency in scientific activities.

SUMMARY STATEMENT OF THE RESEARCH PROBLEM

The problem domain is the organization and management of natural resources and environmental research, with a specific focus on interdisciplinary group research phenomena. This is an area that has received very little attention to date and is ripe for study. Group research has seldom been studied in any context. The organizational conditions that foster effective group research are not known. There are very few published strategies for managing natural resources and environmental research and the effectiveness of existing strategies has not been evaluated.

The dissertation problem analysis appears in Appendix A and is summarized in Table A2.

RESEARCH OBJECTIVES

The following dissertation objectives were developed from the problem analysis and are dealt with evaluatively in succeeding chapters.

1. Develop a comprehensive post-reformative theory of natural resource and environmental research and its administration. The theory is aimed primarily at the group/project level and secondarily at the institution/laboratory level of analysis.

2. Identify the major dimensions and characteristics of natural resource and environmental research.

3. Examine the concepts of scientific quality and value and research performance and effectiveness as they apply to environmental and natural resources research.

4. Develop an epistemological basis for, and explore the methods of, scientific inquiry by research groups.

5. Explore natural resource and environmental research group processes.

6. Determine the organizational conditions that influence interdisciplinary group research.

7. Develop a framework for evaluating research performance and effectiveness of research groups in natural resource and environmental settings.

8. Assess the productivity, performance, effectiveness, and contextual variables of natural resource and environmental research groups.

9. Explore the relationships between organizational characteristics and research group performance and effectiveness at EPA's Environmental Research Laboratories.

10. Investigate the relationships between perceived performance and measured performance.

11. Develop an index of in-process research group performance that combines the scientific productivity and quality facets of performance.

12. Examine the organizational structure of research laboratories or centers in relation to research group performance and effectiveness.

13. Assess existing models of research planning.

14. Develop a framework for managing research at the laboratory/center/program level.

15. Explore management strategies to increase research effectiveness.

WORKING TENETS

A theoretical framework, as defined in this dissertation, is a comprehensive set of propositions and theories of research administration applied at the group/project and institution/laboratory levels in the arena of natural resources and environmental research. A series of working tenets was developed from my observations of research organizations and from the review of literature. They are primitive theories or propositions about key issues in research organization and administration. These tenets complete the research problem analysis summarized in Table A2.

1. Interdisciplinary groups of professionals working under the proper organizational and managerial conditions create synergistic and interactive effects that produce benefits beyond that of traditional unidisciplinary work groups.

2. In essence, the organizational dimensions of the parent organization, especially organizational culture, predict how natural resource management research is organized and conducted. Agency values and beliefs (culture) and client expectations determine the multiple dimensions of research type, which in turn determines organizational design factors and research portfolios.

3. The effective organization and management of research groups (organizational design and leadership imperatives), paying attention to user demands

(organizational policy and culture) and interdisciplinary team approaches to conducting research (group theory, epistemological imperatives), will increase the return on research investments within natural resource management agencies.

4. Based on preliminary evidence in the literature, research management prescriptions may be developed from investigations of the organizational factors contributing to effective interdisciplinary research.

5. The culture and overarching policies of the parent organization influence the research needs, type of research conducted, application of research results, and the structure of the research organization. Successful research organizations reflect the unique culture of their respective parent or sponsoring organizations.

6. Interdisciplinary research groups, structured formally and informally, will be found at natural resources and environmental research laboratories and centers.

7. Measured scientific performance is strongly related to perceived scientific performance.

CHAPTER II

BACKGROUND

The dissertation problem analysis derives from my interviews of research directors and site visits with research scientists in governmental and academic research organizations throughout the United States and Europe. During the period March-August, 1991, I visited numerous research institutes in England, Austria, The Netherlands, Sweden, and Finland for the purpose of interviewing institute directors, project leaders, and scientists. I continued those visits in mid-March of 1992 in Norway and Denmark. The objective of the visits was to learn about European science policies and research organizations firsthand, identify key issues and future trends in science management, explore the organizational concepts and science implications of group research, and to develop a set of propositions concerning research groups in the natural resources/environment field.

During 1994-95 I participated in a series of science policy evaluation and formulation colloquia at Columbia University. This was a unique opportunity to interact with senior scientists and science administrators and debate the evolution and future of U.S. science policy. As a result of that experience, I explored the concepts of "research effectiveness" and "research groups" in considerable detail. I focused on the key organizational or contextual variables associated with research effectiveness, as identified from the literature review and my interviews of U. S. and European research leaders. The concept of performance and effectiveness is a hot topic in this decade. The European science community has been concerned with research evaluation for several years.

In the United States, the Government Performance and Results Act of 1993 (GPRA) has focused attention on organizational performance at the federal level, including science organizations. As a part of this study, I assessed the performance and effectiveness, especially as measured against organizational goals, for a defined population of environmental research groups taken from the Office of Research and Development, Environmental Protection Agency. I also looked at epistemological considerations, including the research approaches and methods of inquiry used by research groups, in terms of group processes and outcomes.

FUNDING FOR NATURAL RESOURCES AND ENVIRONMENTAL RESEARCH

Federal funding for environmental R&D totaled $4.5B (Gramp et al., 1992) or $3.9B (Saundry and Fingerhut, 1995) in 1992 depending on how the data are viewed. Following declines in the 1980's, funding rose at an average rate of 9 percent between 1990 and 1992, because of global change initiative, statutory mandates, and environmental management needs. Federal environmental R&D funding is about 7 percent of all other federal R&D funding and 14 percent of all other federal non-defense R&D funding (Schaefer, 1991).

Figure 2.1 shows federal support of natural resources and environmental research for the period 1970-1995. In terms of constant (1987) dollars, funding peaked in 1979. Figure 2.1 shows the same data expressed as a percentage of total federal R&D funding. Funding for natural resources and environmental research remained close to the $2.0B level in 1996 and 1997, gaining only 0.1 percent over FY94.

Federal funding for natural resources and environmental R&D increased from approximately 400M in 1970 to 1.9B in 1996. However, in constant (1987) dollars the funding has fluctuated between 1.1B and 1.6B (Figure 2.2). Figure 2.2 indicates that natural resources and environmental research funding has averaged about 2.5 percent of total federal R&D funding, ranging from a high of 3.7 percent in the late 1970's to a low of 2.0 percent in the late 1980's. The decrease that occurred in the early 1980's was due largely to the increase in defense-related research. The increase experienced since 1988 might be attributed to the "peace dividend," in which funding for national defense is being reallocated to domestic discretionary funds. Environmental and natural resources research in 1994 and 1995 amounted to about 3 percent of the total Federal R&D budget.

Projections into the future do not look good for environmental and natural resources research. The research budgets for USDA and USDI are expected to take an aggregate cut of 35 percent in fiscal years 1997-2002. However, reports of funding levels for natural resources and environmental R&D are confusing due to uncertainties in labeling research as natural resources research or environmental research. Research programs coordinated by the White House Committee on Environment and Natural Resources

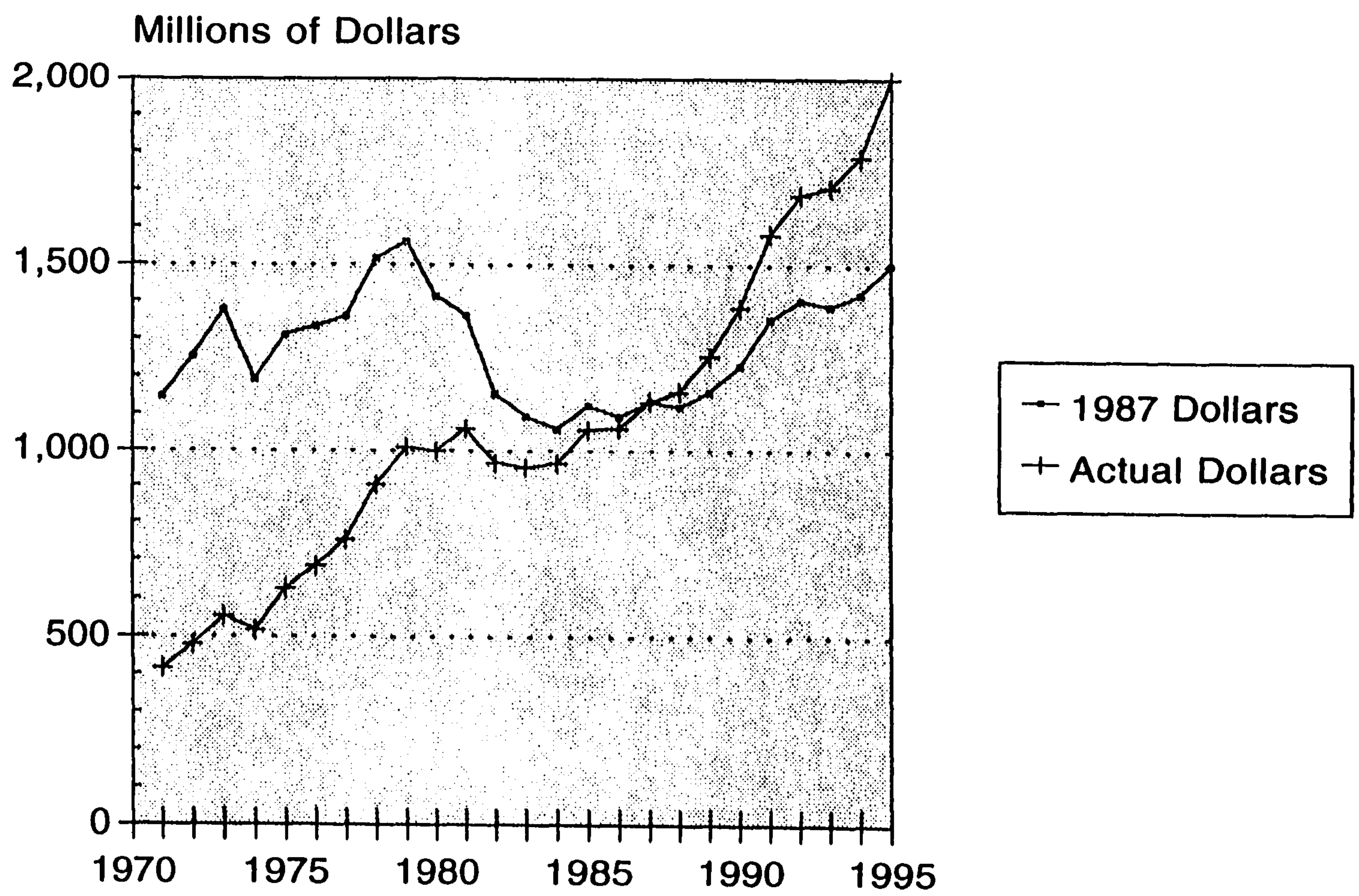


Figure 2.1. Federal R&D funding for natural resources and environmental research.

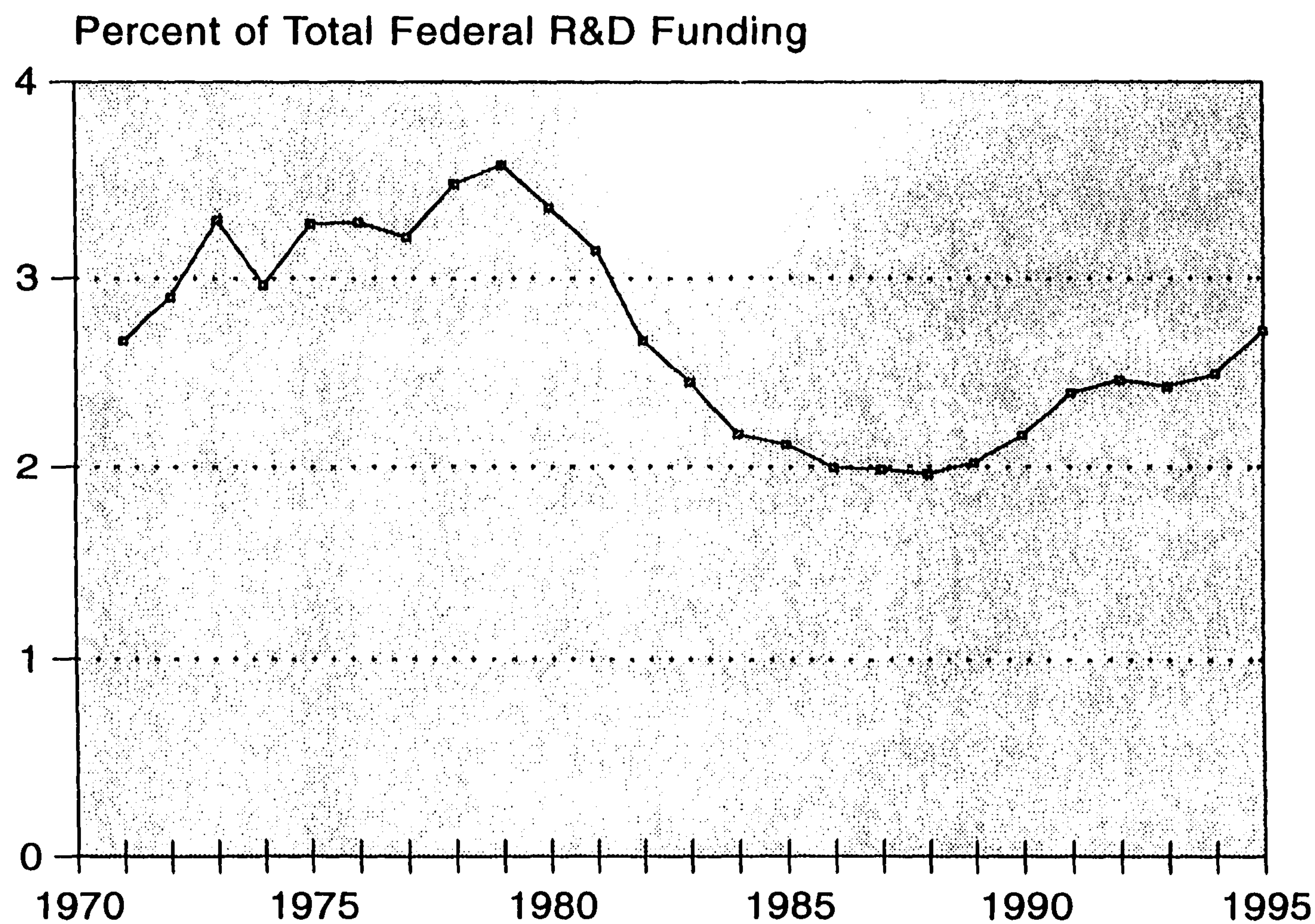


Figure 2.2. Natural resources and environmental research funding as percent of total Federal R&D.

(CENR) totaled $5.1 billion in 1994 and $5.3 billion in 1995 (National Science and Technology Council, 1995). CENR's figures may be inflated over those given by other sources because CENR has included a major portion of the Department of Energy's energy research and development activity and NASA's Earth Observing System research within the natural resources and environmental research portfolio.

In general, natural resources research funding in constant dollars has declined sharply during the past two federal administrations (Day and Ruttan, 1991). This trend parallels the funding for applied research in general, which has leveled off in the past five years (National Science Board, 1994).

EPA research funding in 1992 was 9.4 percent of the total nondefense federal funding for environmental R&D. Citing a need to approach environmental problems on an integrated multimedia basis, EPA increased funding for interdisciplinary environmental research to $75M in FY92, up 49 percent from FY90 levels.

Agencies with resource management or regulatory responsibilities account for 17 percent of the Federal funding for environmental R&D. EPA has a 46 percent share of the Federal management agencies' total funding for environmental R&D.

Although the current federal administration has taken the position that the U.S. is underinvesting in research and technology development (Fallows, 1996), funding levels in terms of real dollars will continue to decline. Thus, U.S. science is moving away from

Vannevar Bush's endless frontier and the concomitant myth of infinite benefit, with a corresponding infinite funding growth, towards an underinvestment theory.

THE CHANGING RESEARCH PROCESS

Changing roles and declining budgets in natural resources research suggest that existing research processes must be reviewed for possible "re-engineering." The ability to respond to environmental and resource management challenges is defined by what research is conducted, how it is organized, and how well it is presented and used in establishing and implementing policy. I agree with Davies (1994) that we have improved our thinking about research needs, but not about organization and policy questions. The organization of natural resources research has not changed markedly in the past 30 or 40 years. Equally disturbing is that we have not improved information transfer across the science-policy and science-management interfaces.

Two fundamental options face federal R&D administrators: improve the coordination and integration of the present decentralized system or design a new more centralized system. The definition of environmental research includes investigations to understand the structure and function of the biosphere and its parts and the impact that human activities have on it (Schaefer, 1991).

These indicators of change suggest a need for a new conceptual framework, new institutional arrangements, and new research planning and evaluation strategies for natural resources and environmental research. An underlying theory of research management is needed. With the decline in funding availability and increasingly complex nature of natural resources and environmental issues, research must become more focused, more effective, and more responsive to clients. Furthermore, with the trend toward interdisciplinary group research, new approaches are in demand for the organization and management of such research groups. Management strategies will include flexibility, an investment approach to research coupled with increased accountability and evaluation systems targeted at research effectiveness, and a more pluralistic and interdisciplinary research system possessing broader research portfolios. These imperatives provide the foundation for this dissertation.

SCIENCE POLICY TRENDS IN THE UNITED STATES

From the 1790's to the 1940's science and technology in the U.S. were driven primarily by individual inventors, universities, and corporations. Federal R&D policy during this period was essentially nonexistent, prompting Crow (1994) to refer to this time as the *laissez faire* period in U.S. science policy. A partnership between science and government was established at Los Alamos in the early 1940's. The Manhattan Project was the model for this partnership (Crease and Samios, 1991). After the war the Atomic Energy Commission set up a string of a national laboratories. However, a precedent was set by having the laboratories managed under an administrative contract by universities, nonprofit organizations, and for-profit corporations. The strong support for physics derives from the dominance of nuclear physicists in policy-making circles, which in turn stems from the success of the Manhattan Project. In the first few decades of this century scientists worked alone or in small groups of two or three.

As a result of the Manhattan Project, little science became big science. Administrative contracts were created in the belief that special management methods are required by the special environment in which research thrives (Rhodes, 1986). They also protected two kinds of independence--relative independence from bureaucratic restrictions and the relative independence from political pressures. The intellectual condition was freedom of inquiry. The institutional condition was a laboratory environment that facilitated such freedom of inquiry. Thus, the Manhattan Project established several precedents for American science. Creating and maintaining a healthy scientific culture is one aim of science management and it involves tending to both the intellectual and institutional conditions of research (Crease and Samios, 1991).

Science is currently in the midst of dramatic, paradigm-level changes. The science-society contract born of the Manhattan Project and solidified during the post-war optimism of the late 40's and early 50's is being questioned on a number of fronts. That contract could be described as fragile at best. Stakeholders are asking "Where is the delivery?" The scientific community is in the process of establishing a new contract with policy makers, not based on demands for more autonomy and ever-increasing budgets, but on the implementation of an explicit research agenda rooted in societal goals. Success of research will be measured by progress towards a better quality of life, rather than by the number of publications or citations or research grants. The ultimate enrichment of the human spirit comes from our ability to expand our realm of experience and knowledge. Ironically, our approach to supporting research and making research policy decisions is largely *ad hoc*, anecdotal, and subjective--in other words, nonscientific (Brown, 1992).

U.S. science policy was strongly influenced by the policy of containment over the past fifty years (Bloch, 1994) and many of the great research thrusts of the 1960's and 1970's were generated by external fears and forces (Staats and Carey, 1973). Science policy was said to be in a confused, ambivalent state in the early 1970's (Smith, 1973). Twenty years later the confusion still exists but current societal trends are forcing science policy out of ambivalence.

The 1990's represent a "reformative era" in U.S. science policy. Figure 2.3 portrays this era as an abrupt shift from the so-called normal science of the post-World War II period to the post-normal, pluralistic period that the U.S. and other countries are entering.
The science-society contract is being renegotiated. The terms of the new contract will include at least: tighter budgets, more useful and more timely research products, less autonomous and more directed science, and improved accountability through performance measurement.

Most basic researchers work within a system organized around traditional disciplines in which they are expected to publish and a system that encourages specialization and discourages radical approaches or interdisciplinary initiatives. The support of these traditional disciplines is a reflection of political history rather than a "scientific stream of consciousness." We need to ask what kinds of research are really needed to understand natural systems and what lines of inquiry offer the greatest probability of improving the quality of life.

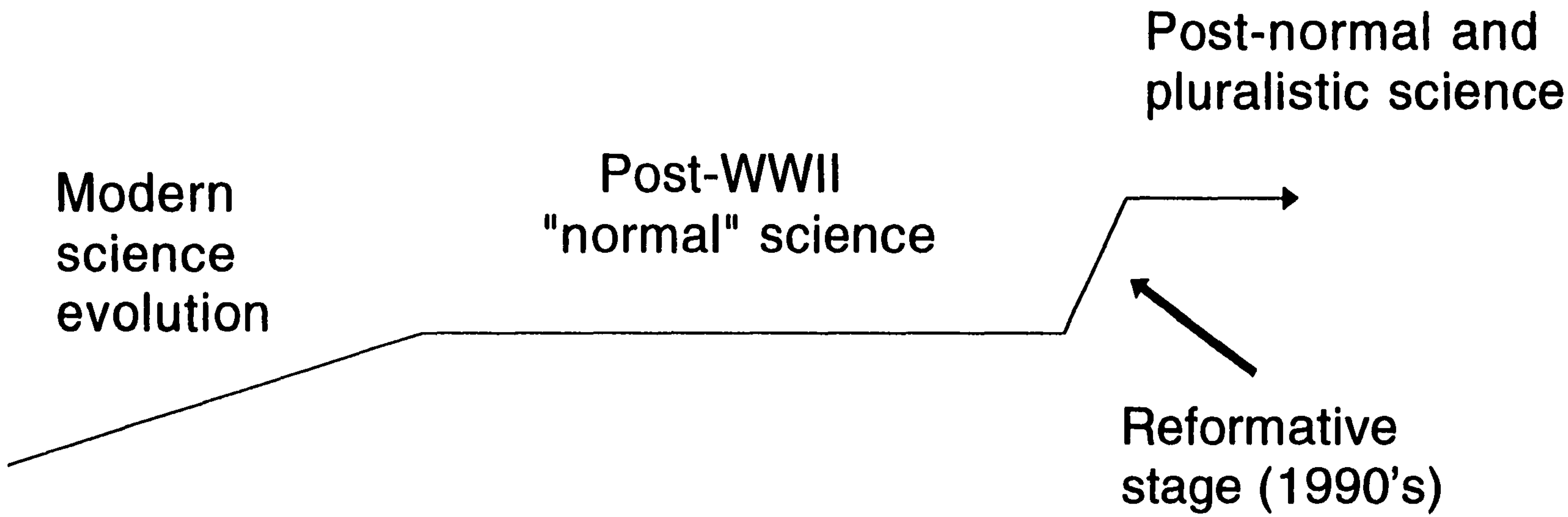


Figure 2.3. Science policy evolution in the United States.

The recent professional literature on science policy and research management predict a growing trend toward interdisciplinary research. The increasing importance of interdisciplinary research has created serious organizational challenges, since interdisciplinary research runs counter to traditional arrangements. It is not clear what organizational, managerial, and incentive factors have differentiated successful from unsuccessful research (Rosenberg, 1991).

We are entering a new era in science management. Trends in science that paralleled industrialization included increased differentiation, increased dependence of scientists upon one another, changing occupational structure, and much more complicated work system. Tighter budgets, closer tracking of performance, increasing uncertainty, greater synthesis and less reductionism, and a systems orientation to many research problems will characterize science in the coming decades. Teamwork in science is still seen as a new phenomenon. Rapid development toward interdisciplinary research and adoption of the principles of project research have increased the interest in exploring questions about these trends. Collaboration among a group of scientists with specialized skills is necessary in the systematic attack of practical problems (Ziman, 1994). Contemporary issues seldom define themselves neatly within the traditional academic boundaries. The economic and political forces that pull science towards greater involvement in practical problems inevitably shape the organization of research into multidisciplinary groups.

This interdisciplinary nature of research has been accompanied by shifts in the bodies of scientific knowledge. The increasing importance of interdisciplinary research has created serious organizational problems in some research circles, since interdisciplinary research runs counter to traditional organizational arrangements and reward systems. The organizational structure of universities, reflected in the departmental structure, poses serious limitations as the solutions to research problems become increasingly interdisciplinary.

Another significant trend in science has been the emergence of synthesis or holistic approaches to research. Science still is dominated by reductionism, defined as seeking to understand phenomena by detailed study of smaller and smaller components (Odum, 1977). However, modern science cannot afford to be solely reductionist; synthesis and systems approaches are needed to address increasingly complex societal issues.

PAST RESEARCH ON NATURAL RESOURCE AND ENVIRONMENTAL RESEARCH GROUPS

Schreyer (1974) concluded that studies of scientific research as a group phenomenon were rare. His study population was the research branch of the Forest Service, from which he sampled 936 scientists. He looked at individual researcher orientation or cognitive style and how effective they were in certain research contexts. He developed a theoretical conceptualization of the influence of individual orientation factors on the effectiveness of scientific research. Research effectiveness was split into scientific and applied contributions. Researcher orientation was defined as a scientist's preference for research situations that are either more basic or more applied. Cognitively simple styles refer to specialized, narrow, inductive research approaches and cognitively complex styles refer to broad areas of research activity, involvement with several facets of a research problem, and deductive research approaches. Schreyer maintained that most Forest Service researchers operate in a group context. He found that it is possible to identify scientists as being integrative or reducing in their overall perspective.

Barnowe (1973) also studied the research branch of the Forest Service. His unit of analysis was research installations (laboratories). He examined relationships between measures of scientific and applied dimensions of research installation effectiveness. He found that an organizational setting characterized by autonomy linked with moderately coordinated, participatory, and technically competent leadership supported research installation effectiveness. Client linkage was the highest correlate of the applied contribution to effectiveness, which supports the idea that a research organization (in this case a Forest Service laboratory) is part of a problem-solving system in which contacts between researchers and users are critical. He also concluded that task groups composed of scientists with heterogeneous professional backgrounds are appropriate for applied research and homogeneous groups are more appropriate for basic research.

Shaw (1967) did not evaluate research groups, but did analyze the publication records of 3,000 Agricultural Research Service (ARS) scientists from the early and mid 1960's. Ph.D. scientists averaged 1.68 publications per year, whereas M.S. scientists averaged 1.24 publications per year. Experience was also a predictor of productivity. According to Shaw, dividing credit among authors in multiple-authored papers discouraged teamwork. Also, scientists who produced the most publications were likely to produce more papers of high quality.

Dailey (1978) looked at R&D team problem-solving processes and team productivity in relation to four independent variables: 1) team cohesiveness, 2) task certainty, 3) task interdependence, and 4) team size. His study population was 45 project groups (281 scientists) from 15 public and private research organizations in the western U.S. Although the research organizations sampled were from the basic research and space technology fields, Dailey's findings are pertinent. Team cohesiveness and task certainty explained about 60 percent of the variance in collaborative problem-solving and team cohesiveness explained 30 percent of the variance in team productivity. Team size and task certainty were both significant predictors of team productivity. Large groups have trouble avoiding process losses in problem-solving tasks. In general, it appears from Dailey's work that organizational environment is important to both problem-solving and research productivity.

Katz (1982) concluded that reliable measures of project performance have yet to be developed. He was interested in the effects of project longevity on communication and performance of 50 project groups (3 to 15 members each) in the research facility of a large U.S. corporation. Project groups became increasingly isolated from key information sources within and outside of their organization. Both technical performance and intra-project communication decreased with time. Project groups became more homogeneous and more committed to particular solution strategies after five years. Based on my observations and experience, I believe these findings may also be applicable to natural resource and environmental research organizations.

Long and McGinnis (1981) believed that organizational context represents a strong factor influencing scientific productivity. Within the normative structure of science, contributions to the body of scientific knowledge are the fundamental criteria of any evaluation of productivity. Academic, government, and industrial science organizations differ in their goals and uniquely shape their research and researchers.

PRIMARY ISSUES IN RESEARCH POLICY AND MANAGEMENT

The first of a series of science policy colloquia, sponsored by Columbia University, was held in June, 1995. In a special breakout session on "Science, Technology and the Environment," the workgroup unanimously concluded that environmental research in the U.S. is seriously fragmented and rated the institutional structures for environmental research as "terrible." Certainly, one of the key issues in natural resources and environmental research is "If we had a clean slate, how would we organize the research and what kinds of institutions are needed?"

A fundamental question for natural resources and environmental management organizations is whether research functions should be integrated with or separate from the policy/management functions. Secretary of the Interior Bruce Babbitt decided to pull all biological research functions from Interior bureaus in 1993 and reassemble the research into a new agency--the National Biological Survey--in order to avoid duplication of effort and beef-up longterm science. Babbitt believed that the scattered nature of biological research in Interior resulted in ad hoc, myopic, mission-specific research that prevents looking at the bigger, longer-term biological picture. According to Stone (1993), parochial needs of the individual agencies have driven Interior's biological research in the past. This move bucked the direction of the National Academy of Sciences panel that studied National Park Service science and recommended that science be a full partner not a stranger to the Parks (National Academy of Sciences, 1992).

The debate over whether or not to separate research from management is not restricted to the U.S. Integration of management and research in the Finnish Forest Research Institute was considered problematic by Luukkonen (1991). Research was weak on objectives-setting and lacked experimental design, the Institute's publications were limited to internal reports instead of peer-reviewed journals, and the research was guided by policy interests of the parent organization--the Ministry of Agriculture and Forestry. Evaluation panels also questioned the scientific qualifications of Institute staff. Dr. Nils Roll-Hansen of the Norwegian Institute for Studies in Research and Higher Education expressed concern that a management agency could not sponsor a research group without that group losing scientific objectivity and independence (Roll-Hansen, 1992).

The research-management segregation vs. integration question will not be resolved anytime soon. There is no easy solution. The integration of the research function with the parent organization's mission and culture is a critical issue in natural resources and environmental management. However, the research organization and management concepts discussed later in this dissertation (Chapter V) may help to illuminate this issue.

Concerns of Research Sponsors and Managers

Research sponsors are interested in funding research that will result in scientific progress and fulfill societal needs. Natural resources and environmental research seeks to gain an understanding of ecological systems and the impact of human activities on environmental health. Research results are often aimed at improving management policies and decisions. Sponsors therefore are interested in accountability issues and the problem of transfer and utilization of research results.

According to Bengston (1989), research managers must be concerned with 1) the value of research relative to budgetary support, 2) the value of different lines of research, and 3) setting future directions for research programs or organizations. They also need to know how to evaluate research effectiveness and how to organize and manage research groups. Brooks (1973) raised two central questions regarding the organization and administration of science. These questions may be found at multiple levels from the individual laboratory to national programs. First, science administrators must determine how to organize, staff, and direct the search for knowledge so as to obtain the greatest rate of scientific progress for given investment of human and material resources. Secondly, administrators must figure out how to couple the existing body of knowledge and the search for new knowledge to existing needs for policy or action.

Research managers often call for increased research funding as a panacea for the problem of unmet research needs. According to Averch (1985), the public policy problem with regard to research funding may not be one of under-investment. For many science organizations, the normal mode of operation remains aggregate budget maximization. They are always demanding new resources; their ability to do anything new depends on receiving new inputs since they are unable to reallocate the resources they have. Science organizations do not have the intelligence systems, administrative systems, and incentives to address R&D as a portfolio problem. An objective of generating a reasonable portfolio of research activities would be an improvement over the conventional, fixed-ratio means of allocating resources. Nearly 40 years has elapsed since Weinberg's first publication on scientific criteria (Weinberg, 1963). Science policy makers and administrators still do not have a set of criteria for making program choices. A portfolio approach combined with stakeholder-established criteria is very much needed.

Another central problem of scientific organizations is how to reconcile the scientific need for autonomy and integrity in its own internal processes of exploration and self-criticism with the demands of society that the fruits of science be guided into channels which society deems beneficial. Brooks (1973) calls this a dynamic tension between science and society, based on an equilibrium configuration that will change with time and the political system.

It is ironic that the organizations charged with innovation have been responsible for generating little research and less innovation about their own effectiveness (Argyris, 1968). For example, collaboration in research settings is seldom discussed and in the past was de-emphasized on the grounds that it is incompatible with scientists' desire for autonomy and independence of activities (Cheng, 1979). The relationship of organizational factors to research effectiveness is seldom discussed. The over-emphasis on the importance of individual effort to scientific advancement was more applicable in years past when research was conducted mostly by single scientists. Modern research is no longer an individual enterprise, but is increasingly being carried out by professional teams consisting of technicians and scientists from different disciplines. Research effectiveness now depends on how well the collaborating scientists and technicians can work together as a team and coordinate efforts in support of the needs of each other and the research problems. Their skills and contributions must be made to converge toward the solution of research problems. Research managers must come to terms with the problem of coordination in research settings.

CENR summarized the major criticisms raised by independent reviews of the federal government's environmental research structure. These criticisms are reproduced in Table A1.

Key Issues Addressed in Dissertation

The following issues are based on the concerns of research sponsors and managers and form the basis for the dissertation problem.

A. The role of science and research in natural resource and environmental management

B. Research performance and effectiveness, including the concepts of scientific value and quality

C. Group process including systems of inquiry used by research groups

D. Multidimensional and interdisciplinary research

E. Organizational structure and other contextual variables in relation to research groups

F. Research planning and management, goal-setting, and cogency

CHAPTER III

RESEARCH APPROACH AND METHODS

The EPA study described herein was designed to investigate group process, organizational and management issues, and performance/effectiveness characteristics among selected research groups operating within a relatively homogeneous organizational setting (the EPA Office of Environmental Processes and Effects Research laboratories). This was not a formal research evaluation, but rather an investigation of group research processes and products and several organizational factors that have been shown previously to influence group research performance. Included was an exploration into the concept of research effectiveness as it applies to environmental and natural resources research. The design and conduct of the study is described in this chapter; the results are discussed in Chapter IV.

It is highly likely that successful interdisciplinary group research is being conducted by EPA and other organizations that support natural resources and environmental research. However, it is just as likely that the organizational conditions that foster effective interdisciplinary research are not well known to research administrators and project leaders. The literature on research management suggests that there is very little definitive information on group research effectiveness or performance. Plainly speaking, we do not know much about the organization and management of group research in natural resource and environmental research settings. This study attempted to discover those organizational factors that lead to successful interdisciplinary group research efforts within EPA's Office of Environmental Processes and Effects Research (OEPER).

Research group effectiveness in relation to organizational context was evaluated at six environmental research laboratories. A primary goal was to look at the contextual factors and research performance of small (three to eleven scientists) research groups. A secondary goal was to examine the concept of organizational effectiveness at the research group level. Intensive investigation was made of the Environmental Protection Agency's six environmental effects and processes research laboratories (hereafter referred to as "OEPER laboratories") using a combination of interviews, questionnaire responses, case histories, and bibliographic analysis methods. Interviews of research project personnel, laboratory management, and research clientele were combined with quantitative and perceptive measures of organizational characteristics and research performance.

This study of the OEPER laboratories was not designed as a formal, full-blown research program evaluation. Rather, I have limited my efforts to an analysis of group research processes and products and the organizational factors that influence group research in moderately-sized environmental research laboratories. This report is descriptive and evaluative in both tone and substance. Evaluative comments and conclusions are supported by my in-person observations, by the objective data collected from the OEPER laboratories, or in some cases by inference patterns supported in the literature. I have included only a few limited prescriptive remarks, in the form of recommendations, at the end of the report. A limited prescriptive discussion also appears in the section dealing with the current status and trends in federal environmental research.

Furthermore, the analysis focused on organizational and management variables and did not consider the scientific merit of the OEPER laboratories' research contributions. A penetrating assessment of the scientific merit of any research program is best accomplished using a peer evaluation system.

Utilizing case history and interview approaches, intensive investigation was made of the six OEPER laboratories. Interviews of research project personnel, laboratory management, and research clients were combined with quantitative and perceptive measures of organizational characteristics and research performance. Propositions about research administration and group research were explored and evaluated using these data.

This study was largely descriptive. Where evaluative comments and conclusions are made, they are supported by my observations, by the objective data collected from the OEPER laboratories, or by the relevant literature supporting certain patterns of inference. I have limited prescriptive discussions to a very few items pertinent to current status and trends in federal environmental research.

SELECTION OF RESEARCH GROUPS, LOGIC OF ANALYSIS, AND DESCRIPTION OF MEASURES

I selected a population of relatively homogeneous research laboratories that conduct interdisciplinary research. I developed a survey instrument to collect in-depth information from a representative sample of small research groups from within those laboratories. The level of analysis was the research group. The data were used to test hypotheses derived from the literature review. I chose to concentrate on research organizations that conduct research primarily in the natural resources and environmental science arena. To control institutional settings, the sample was drawn from the six Environmental Research Laboratories (ERL) reporting to the Office of Environmental Processes and Effects Research within EPA's Office of Research and Development. At the time of the study, all six laboratories reported to the same research director in Washington, DC. The following assumptions and givens applied:

1. The unit of analysis was the research group.

2. All six ERL field laboratories, each containing a certain number of groups, were sampled.

3. A research group was included in the sample if it was stable and active for at least three years, was primarily an intramural effort involving at least three EPA scientists, and conducted interdisciplinary research.

4. The dependent variable of interest is research group performance, which was operationalized using an in-process performance model constructed from the literature on research effectiveness. Performance models were based primarily on

specific institutional research goals. Output measures, which quantitatively and qualitatively reflect contributions to the general advancement of science, were based on an evaluation of research products.

5. Independent variables were drawn from structural, leadership, goal, epistemological, task, group process, and social-emotional dimensions extracted from the organizational effectiveness literature.

6. A survey form, containing questions based on the dependent and independent variables, was completed for each group through interviews with group leaders, scientists, and research administrators.

7. Data included both perceptual/subjective and objective measures of performance/productivity and effectiveness.

8. Important to use a variety of performance indicators and a multifaceted approach towards assessing performance and effectiveness, since there is no one-to-one relationship between scientometric data and performance.

SURVEY INSTRUMENT AND ON-SITE INTERVIEWS

A three-part survey instrument was designed to gather data on research group characteristics and effectiveness. The first part was a 12-item survey aimed at laboratory directors' perception of research group effectiveness. A 50-item survey formed the second part of the instrument and was designed for research group leaders. The third part of the instrument consisted of a 55-item survey given to research group members. Each item utilized a 7-point Likert-type response scale (Katz, 1982). The survey instruments are shown in Appendix C.

On-site interviews were conducted at each laboratory between July 5 and October 15, 1993. A typical laboratory visit involved discussions with management; interviews of Branch chiefs, program and project leaders, and laboratory scientists; collection of data using three different sets of surveys; and collection and review of laboratory publications and other documents. Based on discussions with laboratory management, potential research groups were identified for each laboratory. A research group was defined as three or more EPA scientists working together on a well-defined research problem. This definition is similar to the one used by (Stankiewicz, 1980) for a study of Swedish research group performance. Fourteen research groups meeting this definition were selected from the six laboratories.

CONSTRUCTS OF PERFORMANCE VARIABLES

Since reliable measures of group performance have yet to be developed, interscientific performance of the research groups was measured quantitatively by counting publications and citations and subjectively by having research laboratory directors and group leaders evaluate the performance of each group. This subjective approach was deemed successful by Katz (1982).

In addition to the perceptual data on research performance, bibliometric evaluation techniques were employed. Bibliometric indicators of research performance have a strong positive correlation with more conventional, non-quantitative approaches such as peer review and evaluation. Scientific papers are a legitimate indicator of research productivity and the citations to those papers are a legitimate indicator of the impact of the cited paper (Westbrook, 1960; Wade, 1975; Martin and Irvine, 1983; Mullins, 1987; Narin, 1987; Luukkonen, 1990; Cole, 1992; Nederhof and Van Raan, 1993). Although citation analysis has its limitations, it provides one perspective for examining scientific activity (Garfield, 1993).

Laboratory publication lists were searched by author and by keyword in order to construct publication lists for each research group for the period 1989-93. The resulting lists were scanned carefully, paying particular attention to each publication's author list and title to ensure that the publication "fit" the group. It was assumed that each laboratory publication list represented the total population of publications produced by scientists at the laboratory and likewise that the resulting group publication lists represented the total population of publications produced by members of the respective research groups. However, publication tracking at the OEPER laboratories is a decentralized process and differs somewhat from laboratory to laboratory. For example, some laboratories include workshop or seminar presentations, while others do not. The laboratories utilize different software systems for tracking publications, which increased the difficulty of extracting separate publications lists by research group. It is also possible that some publications are not reported to the person responsible for inserting them into a laboratory's publication database. I found two examples of journal papers that appeared in the *Science Citation Index* (SCI) but were not included in laboratory publications lists. This was an infrequent occurrence and did not affect the results. At least one journal paper per year for the analysis period, 1989-93, was randomly chosen from each of the research groups' publication lists. These "short lists" were used for an analysis of citations from the SCI. The SCI was searched for the analysis period (1989-93) and total citations counted. The annual citation rate (Table 3.1) was calculated for each group. A citation bonus (Table 3.1) was added for publications cited at least 10 times (one bonus point) and at least 25 times (three bonus points). The ≥10 and ≥25 citation thresholds were used by Hart (1993) in a study of range science literature. The citation rating (Table 3.1) was computed by summing the annual citation rate and the citation bonus.

Professional journals containing a high proportion of frequently-cited papers are considered high-impact journals (Garfield, 1992). Impact factors for environmental journals, as computed and ranked by Garfield (1992), are shown in Table 3.2. High-impact publications (Table 3.1), those papers appearing in high-impact journals, were counted for the period of analysis and divided by the group size to produce the high-impact publication productivity. A publication quality index (Table 3.1) was computed as the product of the citation rating and the high-impact publication productivity. Finally, the scientific performance index (Table 3.1) was computed as the product of the publication productivity per scientist and the publication quality index for each group.

Table 3.1. Description and construct of scientific performance variables.

VARIABLE	DESCRIPTION	CONSTRUCT
PUBLICATIONS PER SCIENTIST	Publications per scientist based on counting total publications attributable to the research group.	$\frac{\sum \text{publications}}{\text{group size}}$
HIGH-IMPACT PUBLICATIONS PER SCIENTIST	High-impact publications per scientist based on counting total high-impact publications attributable to the research group. High-impact publications included papers published in high-impact environmental journals (Table 3.2).	$\frac{\sum \text{high-impact publications}}{\text{group size}}$
ANNUAL CITATION RATE	Total citations to group publications divided by total years citable (=5). One paper per year from the period 1989-1993 was selected randomly for citation analysis. Citation counts were taken from the Science Citation Index.	$\frac{\sum \text{citation counts}}{\sum \text{years citable}}$
CITATION BONUS	1 point for each paper cited at least 10 times and 3 points for each paper cited at least 25 times.	
CITATION RATING	The sum of the Annual Citation Rate and the Citation Bonus.	(Citation Rate) + (Citation Bonus)
PUBLICATION QUALITY INDEX	The product of the high-impact publications productivity and the Citation Rating.	(Hi-Impact Pubs per Sci) × (Citation Rating)
SCIENTIFIC PERFORMANCE INDEX	The product of the publications productivity and the Publication Quality Index.	(Publications per Scientist) × (Publication Quality Index)

Table 3.2. Journal impact factors for environmental journals; 1989 data from Garfield (1992).

	Journal Impact Factor
Environmental Science and Technology	2.89
Environmental and Molecular Mutagenesis	2.07
Biogeochemistry	1.85
Water Resources Research	1.73
Deep Sea Research	1.70
Environmental Health Perspectives	1.65
Chemosphere	1.63
Environmental Toxicology and Chemistry	1.50
Climatic Change	1.49
Atmospheric Environment	1.47
Journal of Environmental Quality	1.20
Archives of Environmental Contamination and Toxicology	1.17
Industrial Health	1.13
Geomicrobiology Journal	1.12
Polar Biology	1.12
Remote Sensing and Environment	1.11
Environmental Pollution	1.10
Waste Management Research	1.07
Critical Review Environmental Contributions	1.06
Maritime Pollution Bulletin	1.06
Environmental Research	1.03
Environment	1.03
Journal of Environmental Engineers	1.02
Journal of Toxicology and Environmental Health	1.00

CONSTRUCTS OF CONTEXTUAL VARIABLES

Organizational climate consists of a set of measurable properties of the organization's work environment perceived directly or indirectly by the people who live and work in this environment (Litwin and Stringer, 1968). Survey items were designed to look at the sub-climate variables of commitment to group, job satisfaction, task-related cohesiveness, social-emotional cohesiveness, morale, adequacy of rewards, and administrative/technical support. The construct for the organizational climate measure is shown in Table B2. Organizational culture was not included as a contextual variable, as it is very difficult to operationalize and measure (Druckman et al., 1997).

Commitment to group goals is strongly related to performance (Locke et al., 1988; Katzenbach and Smith, 1993). A goal commitment construct assessed the extent to which research groups were committed to group goals. This construct involved three member survey items and two leader survey items and is shown in Table B2.

Dailey (1978) found relationships between team cohesiveness and its performance and between collaborative problem-solving and team performance. A group cohesiveness index was constructed from the average of six leader and member survey items (see Table B2 in Appendix B). Likewise, a collaborative problem-solving index was constructed from the average of five leader and member survey items (Table B2).

Degree of interdisciplinarity for each group was based on a construct composed of both leader and member perceptions of interdisciplinarity (Table B2). The diversity of disciplines within each group (diversity index) was determined simply by counting the number of different disciplines represented in a group and dividing by the group size (Table B2).

The extent of client involvement was evaluated using a construct consisting of six leader items and six member items, all of which focused on client involvement in the research. The construct is shown in Table B2.

Leadership style was determined from responses to five leader and three member survey items. The construct is shown in Table B2 and Figure B1.

PERFORMANCE RELATIONSHIPS

The following relationships are derived from the review of literature. They are evaluated in Chapter IV using survey responses and publication data from the 14 research groups.

R1: A positive relationship exists between a group's publication productivity and its publication quality.

R2: Most of the variance in measured group performance may be explained by organizational climate and related contextual variables.

R3: A positive relationship exists between group performance as perceived by laboratory directors and measured group performance.

TRACKING THE OUTPUTS AND OUTCOMES OF RESEARCH GROUPS

All six laboratories maintained databases of publications produced by laboratory scientists.

These databases varied from WordPerfect files to commercial bibliographic databases to customized management information systems. This lack of consistency and standardization in tracking publication products across all six laboratories was an obstacle to an efficient processing and assessment of the performance variables. Evaluations of research performance would be facilitated by having an automated publication database system common to all six laboratories.

Publication lists were analyzed to determine authorship patterns, publication productivity by group, and publication type. Publications were classified according to the following categories: journal articles, articles in prestigious journals, internal reports, EPA reports, book chapters, and symposia/conference papers. Prestigious journals included *Science*, *Nature*, *Journal of Geophysical Research*, *Water Resources Research*, *Ecology*, and *BioScience*.

CONTENT ANALYSIS OF PUBLICATIONS

One or more publications from each of the 14 EPA research groups were read for technical content. The primary purpose was to gain knowledge about the nature of research conducted by each group.

STUDY TASKS AND TIMETABLE

Sample selection and design of the survey form was completed within 30 days of the project start date. The preliminary study design was presented to OEPER management for approval prior to data collection. A telephone pre-survey to stratify the sample and select interviewees was attempted. However, the pre-survey in nearly all cases did not successfully uncover the informal research groupings discovered during on-site visits to the laboratories. On-site data collection at the ERL's required approximately 4 months to complete. At each laboratory visit I interviewed the laboratory director and as many branch chiefs as possible. The interviews were completed by October 15, 1993 and publication data were assembled by late December, 1993. Initial results were presented to EPA officials in January and March, 1994.

CHAPTER IV

PERFORMANCE AND EFFECTIVENESS OF SMALL RESEARCH GROUPS: RESULTS AND DISCUSSION

This chapter reports on the organizational factors influencing the formation of both formal and informal research groups, the performance of 14 research groups selected from the six OEPER laboratories, and the nature of interdisciplinary research activity at the laboratories. The discussion is both descriptive and evaluative. Prescriptive material on research organization and management follows in Chapter V. High-performing groups are evaluated in detail. Two research groups are contrasted in terms of their performance and effectiveness.

ENVIRONMENTAL PROCESSES AND EFFECTS RESEARCH AT EPA

The role of the Office of Environmental Processes and Effects Research (OEPER) at EPA was to provide EPA programs and regional offices with information on pollutant transport, fate, and effects in aquatic, terrestrial, and ground-water environments and to conduct related research on biotechnology, ecological risk-assessment, and bioremediation (U. S. Environmental Protection Agency, 1990). Process-oriented research deals with the physical, chemical, and biological factors controlling the entry, movement, and fate of pollutants in the environment. Environmental effects research is concerned with the concurrent effects on nonhuman organisms and ecosystems. Research results provide the scientific information necessary to understand, predict, and manage environmental risks.

Six environmental research laboratories were operated under the direction of OEPER in Corvallis, Oregon; Ada, Oklahoma; Duluth, Minnesota; Gulf Breeze, Florida; Athens, Georgia; and Narragansett, Rhode Island. In addition the Duluth laboratory operated a remote facility at Grosse Ile, Michigan and the Narragansett laboratory included a remote branch at Newport, Oregon.

Research at the OEPER laboratories was conducted within all the environmental media--the atmosphere, soil, ground water, surface water, and coastal and marine water environments. Funding was allocated for a variety of research themes. Major research activities at the laboratories focused on such themes as global change, estuaries and near coastal systems, environmental sustainability, freshwater systems, wetlands, the Great Lakes, biotechnology, ground water, arctic systems, oil spills, contaminated land sites, contaminated sediments, and new and existing chemicals.

In addition to carrying out applied research on current problems, the laboratories also maintained the capability to perform basic and long-term research on core areas central to OEPER's mission (U. S. Environmental Protection Agency, 1990).

Research results produced by the OEPER laboratories took the form of journal articles, reports, computer programs, or handbooks; direct assistance took the form of consultation, short-term studies, training courses, conferences, seminars, or meetings used to describe current research. Clients included EPA program offices, regional staffs, other Federal agencies, and state environmental quality agencies.

HISTORY AND INSTITUTIONAL SETTING OF THE OEPER LABORATORIES

The Federal Water Pollution Control Act Amendments of 1961 (Public Law 87-88) authorized the Secretary of Health, Education and Welfare to establish seven research laboratories for the purpose of conducting research related to the prevention and control of water pollution. Laboratories were created in Narragansett, Rhode Island; Athens, Georgia; Gulf Breeze, Florida; Duluth, Minnesota; Ada, Oklahoma; Corvallis, Oregon; and Anchorage, Alaska. Field stations were later added in Grosse Ile, Michigan and Newport, Oregon. Initially the regional "water laboratories" were part of the U. S. Public Health Service. In 1967 the laboratories were transferred to the Federal Water Pollution Control Administration within the Department of the Interior. After EPA was created in 1970, the laboratories became part of a nationwide network of environmental research centers and their scope expanded beyond the original mandate of water pollution control research.

A reorganization in 1975 resulted in the existing six environmental research laboratories (ERL) reporting to the Director, Office of Processes and Effects Research, in EPA's Washington Office. At the time of this study, each ERL was responsible for a particular segment of the overall OEPER research program (U. S. Environmental Protection Agency, 1990). These laboratory responsibilities and characteristics are summarized in Table 4.1.

Table 4.1. Locations and responsibilities of the OEPER laboratories.

Laboratory	Location	Size	Research Responsibilities[1]
Environmental Research Laboratory-Corvallis[2]	Corvallis, Oregon	130	Terrestrial and watershed ecology; regional- and landscape-scale ecological functions; response of inland ecosystems to environmental changes such as climate and atmospheric pollutants
Robert S. Kerr Environmental Research Laboratory	Ada, Oklahoma	80	Groundwater research, including the transport and fate of contaminants in the subsurface; developing methods to protect and restore groundwater quality
Environmental Research Laboratory-Athens	Athens, Georgia	65	Modeling the fate and transport of pollutants in environmental media; assessing the human and environmental exposures and risks associated with pollutants in freshwater, marine and terrestrial ecosystems
Environmental Research Laboratory-Duluth[3]	Duluth, Minnesota, including field stations in Grosse Ile, Michigan and Monticello, Minnesota	90	Aquatic toxicology and fresh-water ecology; developing standard biological and chemical methods; developing models to predict the impact of chemical and physical pollutants; developing water quality criteria for contaminants in freshwater ecosystems for the protection of aquatic organisms, including the people who consume these organisms
Environmental Research Laboratory-Gulf Breeze	Gulf Breeze, Florida	55	Impact of hazardous materials on marine and estuarine environments, especially ecotoxicology, biotechnology, and pathobiology
Environmental Research Laboratory-Narragansett[5]	Narragansett, Rhode Island and a field station in Newport, Oregon	75	Assessing risks to marine and estuarine ecosystems, including marine and estuarine disposal, water and sediment criteria, and global change impacts on marine systems

[1]From National Academy of Sciences (1993) and U.S. Environmental Protection Agency (1990)
[2]Established in 1961 as the Pacific Northwest Water Laboratory
[3]Established in 1961 as the National Water Quality Laboratory
[4]Established in 1961 as the National Marine Water Quality Laboratory

ORGANIZATIONAL CHARACTERISTICS OF THE LABORATORIES

The organizational environment is described generally for the six laboratories. Since the study was not designed to compare and contrast laboratories, no attempt was made to specifically describe each laboratory's environment. Important differences are noted as appropriate.

Research Orientation, Processes, and Outputs

Research at the OEPER laboratories consisted of the following: long-term discovery research (>5 years), designed to increase knowledge of environmental processes and effects; short-term (0-5 years), regulatory-relevant, developmental research models and techniques; problem-solving and technical assistance; and training.

According to EPA, budget allocations and research priorities for major research program areas have been guided by research committees composed of representatives from ORD, program offices, and regions (U. S. Environmental Protection Agency, 1990). Research planning within OEPER was accomplished using a system of issue planning. Issues were developed by issue managers who were expected to involve more than one laboratory in the process of developing the research issue.

Some ERL scientists believed that this approach is too "top-down" and prevents them from planning longterm research agendas. There is some indication that program offices and regions have less involvement in research planning now than in the past.

Boundary-spanning, communicating regularly across the boundary of the organization and interacting with other organizations, is an important process and function at EPA laboratories. The concept of gatekeepers arose from the need to transfer information between an organization's internal and external markets (Averch, 1985). Boundary spanners and gatekeepers generally are knowledgeable about the goals, services, and resources present in other organizations (Gillespie and Mileti, 1979; Hall, 1987). They are also valuable because they are the means by which external ideas and information are transferred to project groups (Katz, 1982). According to Allen and Cohen (1969), research managers fail to make effective use of gatekeepers.

The OEPER laboratories needed to communicate with each other, with other EPA laboratories, and with other entities involved with environmental research in order to maximize the effectiveness of their research activities. Boundary-spanning was observed at all the laboratories to different degrees, and usually involved senior scientists with broad interests.

Integration, defined here as integrating research activities and results within projects and across projects, is a critical process and key function at the EPA laboratories. Integrators must have an orientation towards organization-wide goals as contrasted to parochial goals (Galbraith, 1973). Their focus is establishing work relationships and a unity of effort required by complex tasks that span several subunits of the organization (Hall, 1987). Examples of integrator roles in research occur at both the planning and synthesis stages. Interdisciplinary research requires much attention to integration (Norstrom, 1986).

Organizational Structure

Formal organizational charts tell only part of the story of organizational structure. All formal organizations regardless of size, whether they reside in government or in the private sector, have informal or invisible organizational structures that differ to some degree from the charted organizational form. Studies of research organizations must recognize this phenomenon and look beyond the organizational chart when evaluating structural characteristics.

Typically, laboratory organizational charts show a traditional branch structure (Figure 4.1), where the branches are differentiated functionally or on the basis of major research theme. The number of branches varies from three (Corvallis and Gulf Breeze) to six (Duluth). Four of the laboratories have three organizational strata (one management layer between laboratory director and nonsupervisory scientists) and the other two, Ada and Corvallis, have four organizational strata. Branches at the OEPER laboratories ranged in size from as few as three to as many as 15 scientists. The 14 research groups sampled ranged in size from three to 12 permanent full-time scientists (Table 4.1).

In addition to structural design and size, other contextual factors of importance in organizational studies include organizational environment, technology, and goals. According to (Hall, 1987), a functional organizational form is more effective when the external environment is stable, technology is routine, and horizontal interdependency is low.

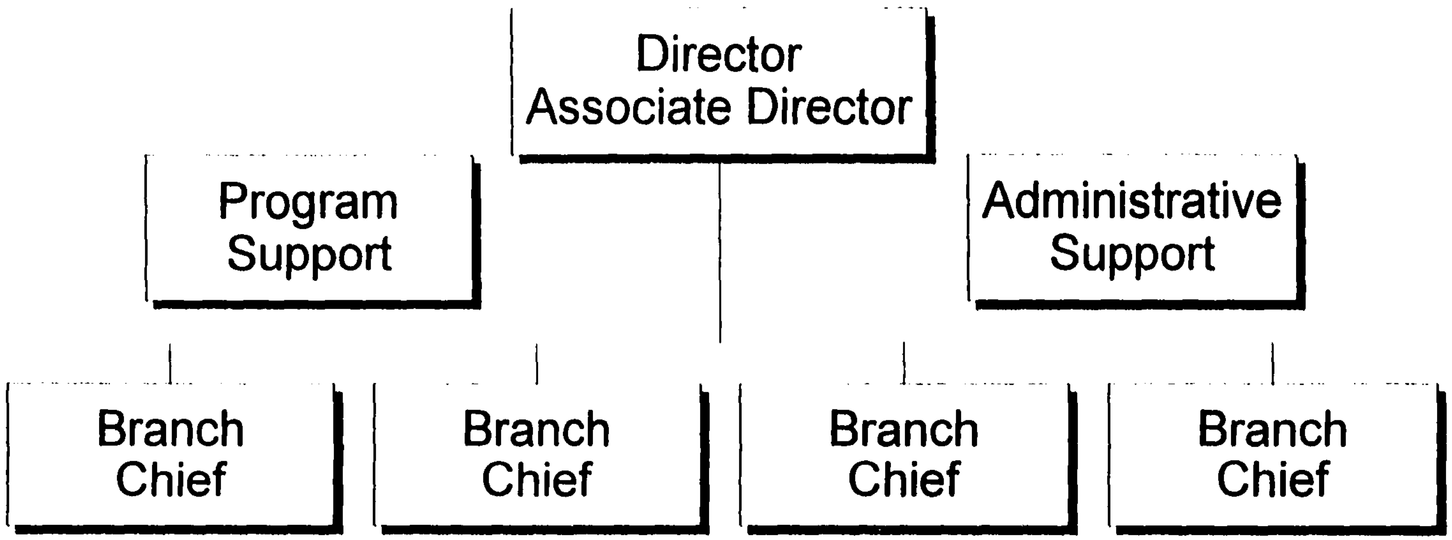


Figure 4.1. Typical organizational structure at OEPER laboratories.

An organizational form based on product or output grouping is appropriate when the external environment is less stable, technology is nonroutine, and horizontal interdependency is high. However, in an uncertain environment, where the organization has dual goals of client satisfaction plus innovation, a hybrid structure is likely to be more effective (Hall, 1987). Research organizations have typically used a functional structure to further goals of scientific quality. However, the functional structure does not promote interdisciplinary group approaches to research. Some form of functional organization form must be maintained for routine administrative purposes and for maintaining scientific quality within disciplines (peer review, apprenticeships). Laboratory goals include both scientific quality and client satisfaction goals. More importantly, the OEPER laboratories were characterized by high levels of environmental complexity and instability which together translate into an atmosphere of uncertainty.

Based on the ideas of Daft (1992), the following organizational adaptations are required to deal with the environmental uncertainty and should be considered desired outcomes of a laboratory's organizational design:

1. An organic (free-flowing, loose, flexible) organizational structure as opposed to the traditional tight, bureaucratic, highly structured form.

2. An organizational climate that promotes teamwork and participative decision-making.

3. Horizontal differentiation of tasks with effective horizontal communications and many integrating roles.

4. Extensive planning and external boundary spanning

5. A trusting environment that stimulates creative thinking

Organizational goals influence organizational structure. OEPER laboratories have overarching goals of providing EPA programs and regional offices with information on pollutant transport, fate, and effects in aquatic, terrestrial, and ground-water environments and conducting related research on biotechnology, ecological risk-assessment, and bioremediation (U. S. Environmental Protection Agency, 1990). The Corvallis laboratory used a two-pronged approach to evaluating research performance: policy relevance and science quality (Murphy, 1993, personal communication). The Associate Director of the Norwegian Institute for Nature Research expressed the same thoughts in slightly different words: "environmental research must be both scientifically acceptable and useful" (Gunnerod, 1992, personal communication). A laboratory's organizational design therefore must reflect the need to satisfy both sets of goals. There must be a dual focus on maintaining scientific quality or integrity (which includes the goal of advancing basic scientific knowledge) and producing well-integrated, interdisciplinary research products for use by policy or decision makers. In addition, the nature of the work (for example, discovery research, policy-relevant research, scientific assessments), as identified for each research issue within a research plan, is an important consideration in research organization design (Van Haveren and Hamilton, 1992; Van Haveren, 1998).

Ordinarily, the need for a flexible, multi-focused organizational structure evokes discussion of matrix-type organization structures. In fact, several laboratory directors commented that their laboratories were "well-matrixed." However, matrix organizations are not necessarily organic in their actual operation and are probably no more effective than a rigid functional structure in terms of adapting to shifting environments. A different hybrid structure is needed for environmental research laboratories. The volatility and uncertainty of natural resources and environmental research issues cries out for organizational flexibility.

At first glance, Duluth's organizational structure was an enigma. The organizational chart portrays six branches, each assigned to a major research theme (Figure 4.2). However, it soon became apparent that laboratory scientists identified more with research programs and projects than with their formal organizational units, thus revealing an operational organizational form that is informal and essentially invisible. According to laboratory management, and which my observations confirmed, there was an attempt made to integrate disciplines within each branch. However, the branches do not operate as independent fiefdoms and branch chiefs did not appear to be overly "turf protective." This climate was perhaps due to the fact that branch chiefs had been rotated recently and many were in an "acting" status.

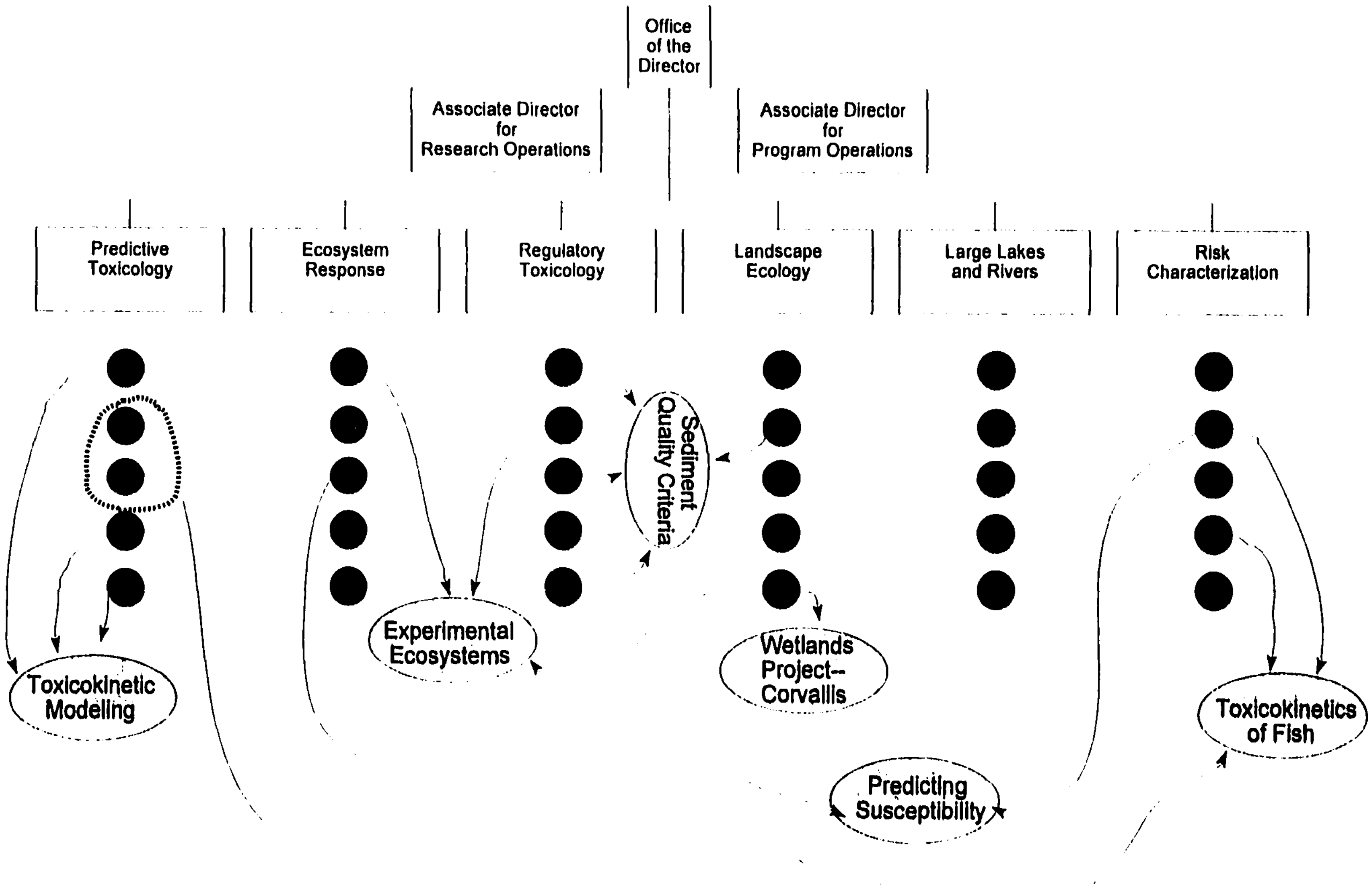


Figure 4.2. Constellational matrix at the Duluth laboratory.

Embedded within and "matrixed" across the branches were “clusters” of scientists that collaborate on a research problem. In most cases these research clusters transcend branch boundaries, although occasionally the clusters were contained entirely withing a branch. In some instances, smaller clusters or dyads have developed within the larger research program clusters. These smaller clusters might be described as moving circles of research emphasis--collections of research projects and tasks that rely on one another for information and expertise. In another example, 11 scientists and managers formed a team to prepare a research workplan for a TCE/Dioxin ecological risk assessment and exposure effects testing research initiative. Although cross-branch clustering was observed to a certain degree at all the laboratories, Duluth provides the best example of this "dynamic constellation" form of organization, which is discussed in Chapter V.

At the time of this study several of the laboratories were using or were in the process of developing team approaches to address priority research problems. For example, six research teams were created at Athens at the beginning of FY94 to plan, coordinate, and oversee the technical conduct of research that cuts across Branch missions and expertise.

The teams, each headed by a senior scientist Research Team Leader, were organized primarily to ensure laboratory responsiveness to the ORD Strategic Research Issue Plan objectives. Teams have the responsibility to identify research needs, research problems, and required resources. With the addition of the issue-based teams, Athens has gone to a matrix-like operation with Branches continuing to be cost centers and project managers. Line management, which includes the Branch Chiefs, retains the responsibility for the quality, usefulness, and timeliness of research results. Ada has similarly constituted seven teams in an attempt to achieve cross-branch collaboration. The teams were empowered by laboratory management providing separate funding for each team and expecting teams to determine which projects were funded.

RESEARCH GROUPS AT THE LABORATORIES

Informal research groups existed at all the laboratories. The best examples were found at Ada, Athens, Gulf Breeze, and Narragansett. Many of the OEPER research groups did not appear on formal organizational charts. Small, informal research groups emerge due to common research interests or because of a need to share expertise and apply a variety of skills to a research problem. Research groups tended to attract the skills needed to successfully conduct the research tasks. At Ada, a very effective groundwater bioremediation group developed around a series of field-oriented research projects. The Athens laboratory has championed two exceptional teams--computational chemistry and multispectral identification. The computational chemistry team develops models or algorithms to predict physical and chemical properties of unknown substances and is highly regarded by peers in the other OEPER laboratories. The multispectral identification team represents a unique cross-branch capability for identifying new compounds using a variety of spectral techniques. Neither of these research groups appear on the laboratory's formal organizational chart, but were contributing a great deal to Athens' mission.

Gulf Breeze recently created an interdisciplinary field research team to develop methods of determining the ecological health of estuaries and bayous. The team is looking for key factors--water, sediment, flora, fauna--which may be used as sensitive parameters or indicators of ecological health status. An oil spill bioremediation protocol team at Gulf Breeze is testing various vendor products to determine their effectiveness and toxicity when combined with oil. The outcome will be a scientifically-based, objective process for determining oil-spill clean-up protocols for open waters, shorelines, marshes, wetlands, and terrestrial environments.

Although not sampled, the New Bedford Harbor Project at Narragansett is viewed by project participants as an excellent example of team research and a model of good interagency cooperation. The project derived from a Superfund site issue that was likely to go to court and involved the real-time monitoring of dredging activities to see if dredge materials were releasing contaminants. The client was the EPA regional counsel; the Corps of Engineers was also involved as an action agency, not as a responsible party. The project was designed to benefit policy and decision making at the regional office level, but also to contribute to basic science. The project was the recipient of several awards and considered by laboratory management to be highly successful in terms of client satisfaction, technology transfer, and policy relevance. At the time of this study, Narragansett was using the New Bedford Harbor project to develop a watershed cumulative effects research program for coastal ecosystems.

OEPER management deserves credit for allowing several small but promising fundamental research efforts to continue even though research priorities have changed. There should always be support for selected cutting-edge research projects that may not have immediate or specifically-targeted applications.

Characteristics of the 14 research groups sampled are shown in Table 4.2. The groups considered themselves to be moderately to strongly interdisciplinary (Table D1), with group leaders agreeing with group members on the extent of interdisciplinarity (Table D2).

Inter- and Intra-laboratory Research Clusters

As evidenced by co-authorship patterns and my interview results, dyads and clusters form within laboratories and occasionally across laboratories (Figure 4.3). A cluster may include a scientist from outside EPA, such as a contractor or sub-contractor, a scientist from a collaborating university or a scientist from another agency. These patterns were evident at all the laboratories, but more prevalent at Duluth and Athens.

I was also interested in the extent to which the OEPER laboratories collaborated on overlapping research programs. For example, Corvallis and Duluth both support research programs in global climate change and wetlands. Duluth and Narragansett both conducted research on sediment quality criteria. Athens and Ada had similar research programs on bioremediation. Narragansett and Gulf Breeze each supported research on marine ecosystems. Publication patterns supported laboratory management's contention that some collaboration was occurring between laboratories. However, several scientists interviewed stated that inadequate travel budgets hindered inter-laboratory collaboration.

Table 4.2. Characteristics of the EPA research groups sampled.

GROUP CODE	GROUP NAME	SIZE	TYPE	CLIENT TYPE	NATURE OF RESEARCH	STAGE OF RESEARCH
SEDCR	Sediment Quality Criteria	6	Development	Public	Problem-directed	Concluding
GLOBC	Global Climate	3	Development	Scientific	Exploratory	Implementing
OILSP	Oil Spill Protocols	5	Applied	Scientific	Problem-directed	Multiple
FLDTM	Field Research	6	Applied	Policymakers	Exploratory	Implementing
MICRO	Microcosm	3	Basic	Program Office	Problem-directed	Multiple
BENTH	Benthic Effects	5	Applied	Program Office	Problem-directed	Multiple
WETLD	Wetlands	3	Applied	Program Office	Problem-directed	Multiple
GLOBM	Global Mitig. & Adaptation	4	Applied	Program Office	Policy-related	Multiple
WSHED	Watershed Response	3	Applied	Program Office	Problem-directed	Multiple
WLDLF	Wildlife Ecology	7	Applied	Program Office	Problem-directed	Concluding
OZONE	Ozone	12	Applied	Program Office	Policy-related	Implementing
GLOBP	Global Processes & Effects	6	Basic	Scientific	Problem-directed	Initiating
BIORM	Remediation	4	Applied	Policymakers	Problem-directed	Multiple
APPLN	Applications & Assistance	11	Applied	Program Office	Problem-directed	Multiple

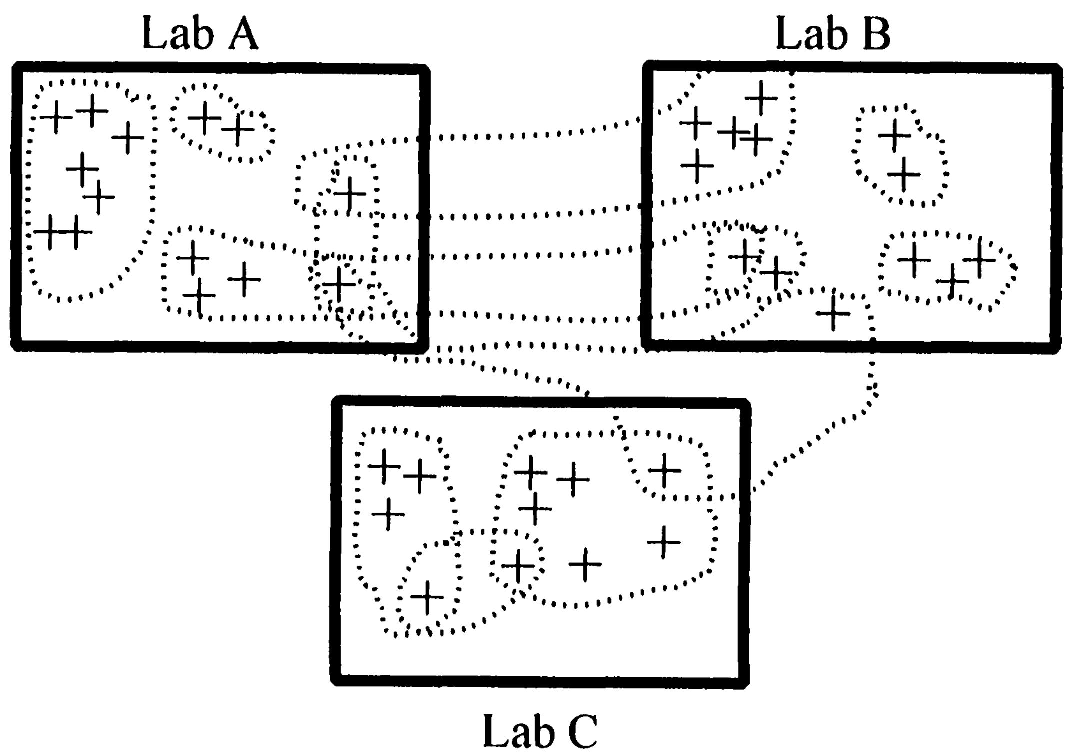


Figure 4.3. Dyads and clusters form within and across laboratories.

Inter-laboratory research clusters were touted by senior management, but I found little evidence of close collaboration resulting in a large number of co-authored publications. For example, the wetlands group at Corvallis included one member stationed at Duluth. However, this person did not co-publish very often with Corvallis members of the group. The sediment quality cluster was different. I found publications that were authored by scientists from three different laboratories and a scientist from an EPA headquarters program office.

Given the advances in capability to exchange information electronically, I would expect to see an increase in collaboration on research topics of mutual interest. I would also expect to find the formation of both formal and informal "collaboratories" to share data electronically, as promoted by Wulf (1993). At the time of this study, collaboratories were relatively rare.

Organizational Factors Influencing Group Formation

A rigid organizational structure with relatively imperious branch walls had the tendency to discourage informal groups from forming at the OEPER laboratories. Divisions and branches often become fiefdoms in bureaucratic organizations having a high degree of formality. An open, organic structure such as that found at Duluth provided the environment and culture for research clusters to form. The toxicokinetic research clusters at Duluth (Figure 4.2) were an excellent example of the clustering phenomenon. I observed similar research clusters within the Tema Institute at the University of Linkoping in Sweden. Pearson (1983) also observed a number of research structures, both formal and informal, operating at the same time in a single research establishment.

Contextual Factors Associated with Research Groups

The contextual characteristics of the research groups are summarized in Table D1. Refer to Table B2 for the group identities. Leadership styles are shown in Figure D1. Since group leaders and members were given separate but similar survey instruments, their perceptions of the contextual variables can be compared. Figures D2 through D8 and Table D2 show the results.

PERFORMANCE OF SAMPLED GROUPS

Table 4.3 shows the performance results by research group for the 14 groups studied. The number of publications ranged from 10 to 119 per group for the five-year period of analysis. Publications per scientist ranged from 2.2 to 29.8. The high-impact publications ranged from 1 to 18 per group (0.2 to 3.8 per scientist). The annual citation rate varied from 0.3 to 2.2 per group. The scientific performance index ranged from a low of 0.7 to a high of 282.3. Nine groups had a performance index of less than 10. The other five groups each scored higher than 50.

Publication Productivity

Table 4.4 shows the publication performance averaged for the 14 research groups. The average production of 1.9 pubs per scientist per year agrees with the finding of Shaw (1967) of 1.7 scientific papers per year for Ph.D-level and 1.2 papers per year for M.S.-level ARS scientists. However, Shaw's data are from the early to mid-1960's and one

Table 4.3. Scientific performance results by group.

GROUP CODE	NO. OF PUBS	PUBS/ SCIENTIST	HIGH IMPACT PUBS	HIGH IMPACT PUBS/ SCIENTIST	ANNUAL CITATION RATE	CITATION BONUS	CITATION RATING	PUB QUALITY INDEX	SCI PERFORM INDEX
SEDCR	42	7.0	18	3.0	1.6	0.8	2.4	7.3	51.2
GLOBC	11	3.7	2	0.7	1.5	0.5	2.0	1.3	4.9
OILSP	11	2.2	2	0.4	1.8	0.3	2.1	0.8	1.8
FLDTM	30	5.0	5	0.8	0.8	0.3	1.1	0.9	4.7
MICRO	10	3.3	1	0.3	0.3	0.0	0.3	0.1	0.3
BENTH	57	11.4	10	2.0	1.4	1.1	2.5	5.1	57.9
WETLD	19	6.3	1	0.3	0.3	0.0	0.3	0.1	0.7
GLOBM	119	29.8	15	3.8	1.7	0.8	2.5	9.5	282.3
WSHED	25	8.3	3	1.0	0.3	0.0	0.3	0.3	2.7
WLDLF	30	4.3	18	2.6	0.7	0.0	0.7	1.8	7.8
OZONE	71	5.9	6	0.5	0.8	0.2	1.0	0.5	3.0
GLOBP	108	18.0	13	2.2	1.6	1.2	2.8	6.0	107.3
BIORM	49	12.3	7	1.8	2.2	1.7	3.9	6.8	83.4
APPLN	43	3.9	2	0.2	1.5	0.7	2.2	0.4	1.5

Table 4.4. Summary of publication performance (all groups).

PERFORMANCE MEASURE	MEAN	MAX	MIN
Publications/Scientist/Year	1.9	6.6	0.5
High-impact Publications/ Scientist/Year	0.3	0.8	0.0
Citation Rate	1.2	2.2	0.3

might expect slightly greater productivity today, considering the advances in automation. Furthermore, such comparisons assume that all papers carry equal weight. This is not the case since each scientific paper represents a certain level of cognitive complexity (Leary, 1985). Also, issue papers discussing a promising new research direction cannot be compared equally to research papers summarizing several years of scientific investigation or major synthesis works that integrate separate but related pieces of a large research program. Multiple-author papers pose another difficulty. Should all authors receive full credit for the publication? Shaw (1967) concluded that splitting credit for a multiple-authored publication discourages team research.

Publication productivity over time is shown in Figure 4.4 for four of the most productive groups. Productivity varied from group to group and from year to year within a group.

Publication Quality

Table 4.5 summarizes the citation frequency results. The number of frequently-cited publications by a group, with self-citations removed, is an important component of publication quality. These are tedious data to collect and analyze but, like journal impact data, reveal important information about the scientific quality and impact of a research group's publication output.

Publication quality was regressed on publication productivity. The linear regression produced an R^2 of 0.72 and is shown in Figure 4.5. Others have similarly reported strong relationships between publication productivity and quality (Shaw, 1967; Narin, 1987).

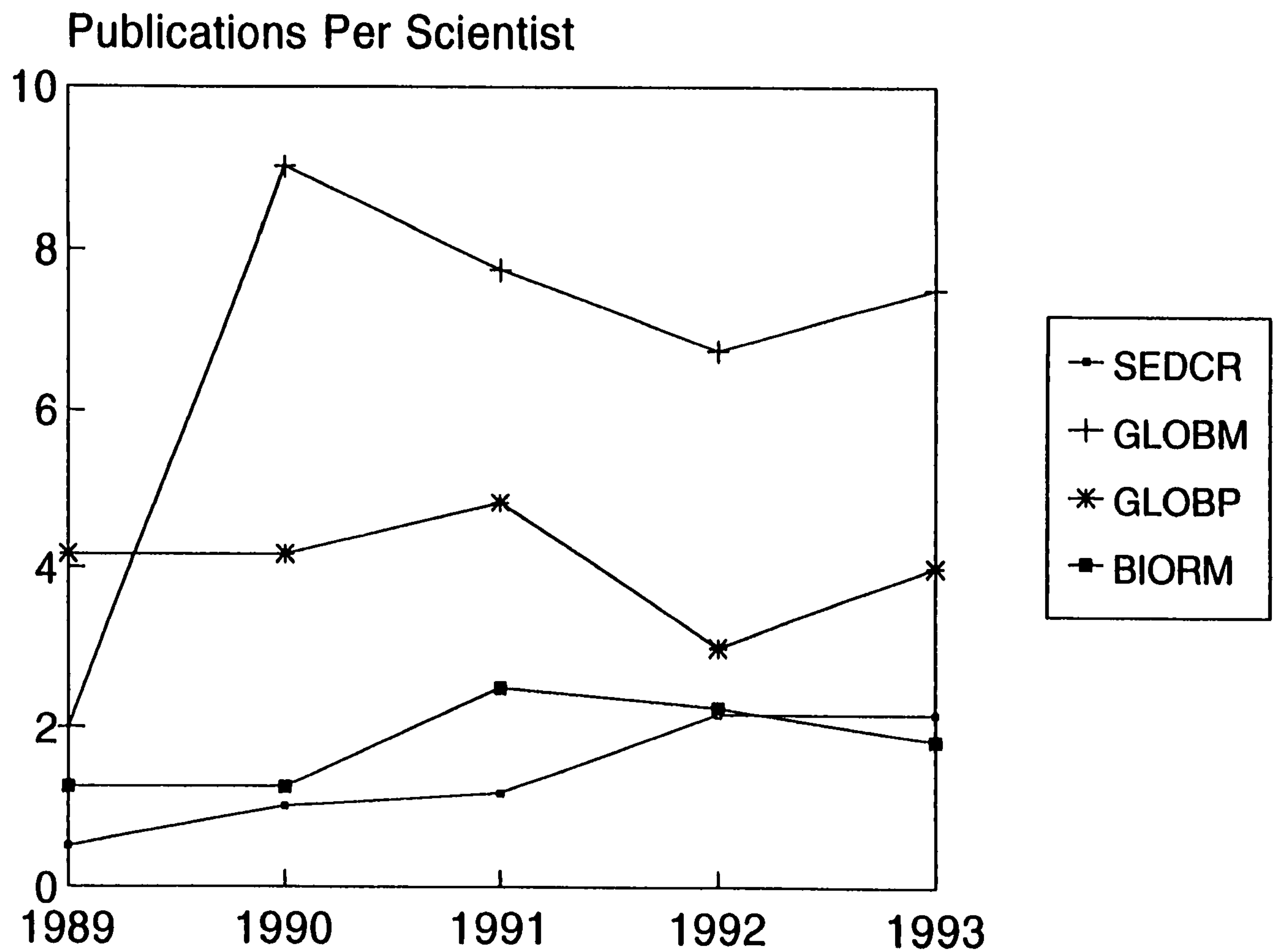


Figure 4.4. Annual publication productivity of selected research groups.

Table 4.5. Frequently-cited publications of research groups.

Group	Number of Publications with Indicated Number of Citations				
	≥25	≥20	≥15	≥10	≥5
SEDCR		1		1	
GLOBC					
OILSP			1		
FLDTM					
MICRO					
BENTH			1	1	2
WETLD					
GLOBM				2	
WSHED					
WLDLF					
OZONE				1	
GLOBP				2	1
BIORM	1			1	1
APPLN					

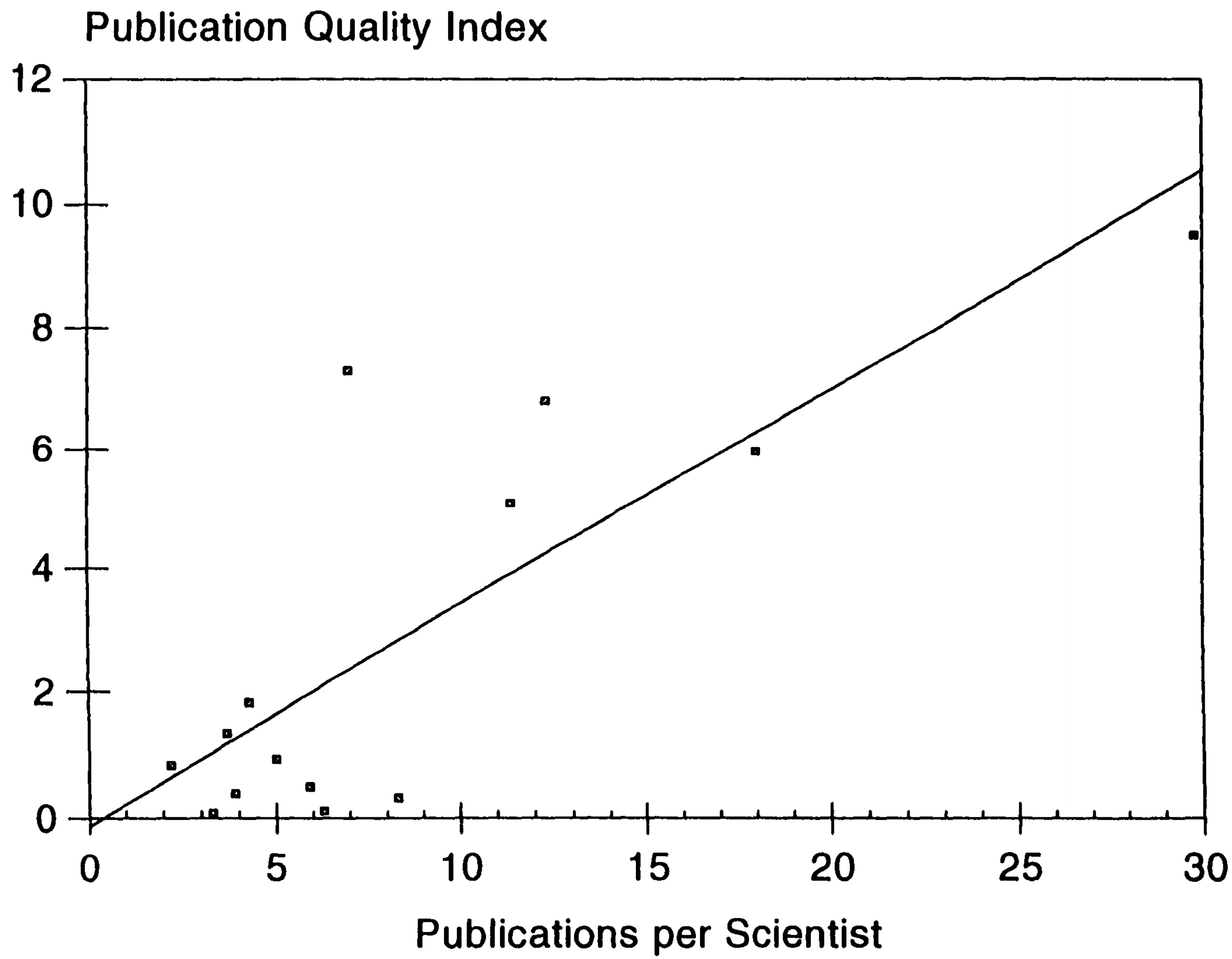


Figure 4.5. Linear regression of publication quality on productivity (R^2=0.72).

High-impact and Prestigious Papers

Several of the groups had papers published in high-impact journals. There are distinct advantages to publishing in such outlets. Since they are highly-cited journals, they tend to be very visible and credible within the technical field they represent. Only four prestigious papers were published, all by one research group--the Global Mitigation and Adaptation group. Two papers appeared in Science and two in Nature. It is possible that all four papers resulted in the group's prior involvement with acid precipitation research.

Synthesis Papers

These are papers that summarize and integrate the results of research over several years or several projects. They are usually prepared at the end of a research effort and may involve scientists from several laboratories engaged in the research program. In general, I observed a paucity of synthesis papers across all the groups. It is possible that research priorities change, preventing the preparation of synthesis papers. Does this signify that projects are not brought to a stage of completion? There were some indications that research efforts at the OEPER laboratories were not "closed out" with synthesis papers.

Setting the Pace: Publications by Senior Management

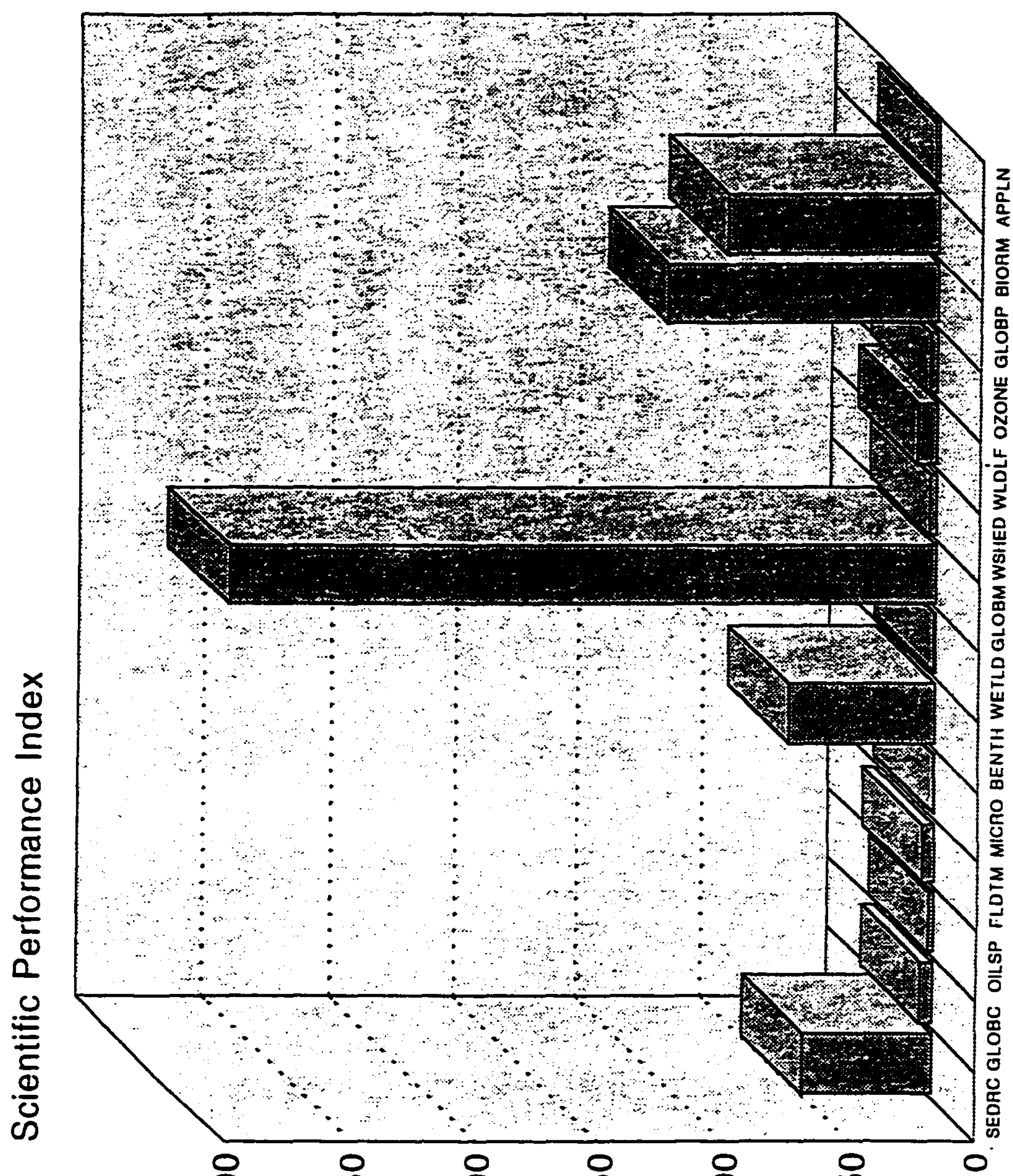
Research directors have an opportunity by virtue of their leadership position and visibility within the organization to set the pace for other scientists by occasionally authoring papers in high-impact and/or prestigious journals. Laboratory directors and associate directors at three of the OEPER laboratories published routinely. One significant research planning paper was published by the OEPER directorate in EPA headquarters.

Index of Scientific Performance

The index of scientific performance combines both publication productivity and publication quality. The results are shown in Table 4.3 and Figure 4.6. Team size, team cohesiveness, and task certainty were shown to be significant predictors of team productivity in one study of research teams (Dailey, 1978). In another study, research results were better when social relations between managers and researchers were based on mutual confidence (Pearson, 1983). Although Locke et al. (1988) found a strong relationship between performance and commitment to group goals, no such relationship was detected for the 14 research groups in this study.

An attempt was made to explain the variance in scientific performance using a linear regression model, in which the contextual factors from Table B2 were included as independent variables. The regression model explained about 50 percent of the variance in performance (R^2=0.48). Individually, group performance is plotted against organizational climate (Figure D9) and against goal congruence (Figure D10), but no definitive patterns are evident. For the 14 research groups studied, it is believed that individual initiative on the part of one or more scientists in a research group has more influence on group performance than does organizational climate. In addition, group performance may also be a function of the phase that a research group is in. Research groups in the formative stage, or groups just beginning new projects, are not as likely to be as productive as groups that have collected and analyzed their data. However, the publication quality component of performance is less dependent of research phase.

Figure 4.6. Scientific performance of 14 research groups.


Scientific Performance Index
300
250
200
150
100
50
0
SEDRC GLOBC OILSP FLDTM MICRO BENTH WETLD GLOBM WSHED WLDLF OZONE GLOBP BIORM APPLN

Perceived vs. Measured Performance

Group leaders tended to rate scientific performance slightly higher ($\bar{x}$=5.3; N=14) than did group members ($\bar{x}$=5.2; N=14); there was no significant difference between the means. Both leader-perceived and member-perceived scientific performance are plotted against measured scientific performance in Figure 4.7. There is no apparent agreement between perceived and measured performance of the 14 groups. Director-perceived performance was plotted against four measures of scientific performance (Figure 4.8). In all four cases, R^2 values were less than 0.25, suggesting that laboratory directors did not have accurate perceptions about the scientific performance of research groups.

Laboratory directors disagreed with both group leaders and group members on nearly all aspects of perceived performance (Table 4.6). However, there was very good (p=.99) agreement between laboratory directors and group leaders on scientific quality and good (p=.95) agreement on national and international reputation. There was also good agreement between laboratory directors and group members on international reputation and transferring results to clients. Group leaders and group members only agreed on national and international reputation (p=.99) and staying within budget (p=.95). Laboratory directors rated group scientific performance lower than did either leaders or members ($\bar{x}$=4.4; N=14). Again, there was no agreement between perceived performance and measured performance.

Extrascientific Performance

There were inconsistencies across laboratories in reporting conference and workshop

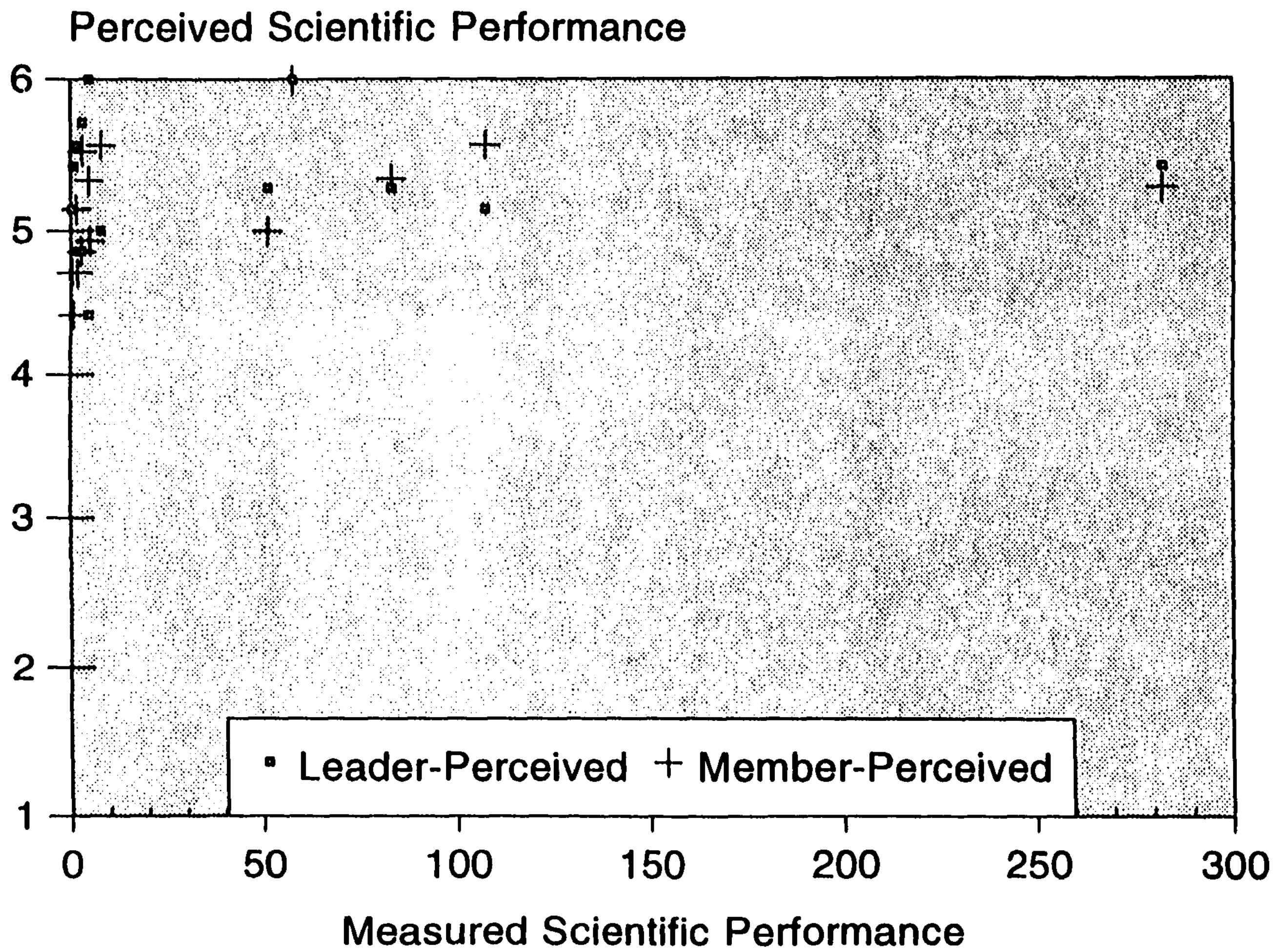


Figure 4.7. Perceived scientific performance vs. measured scientific performance.

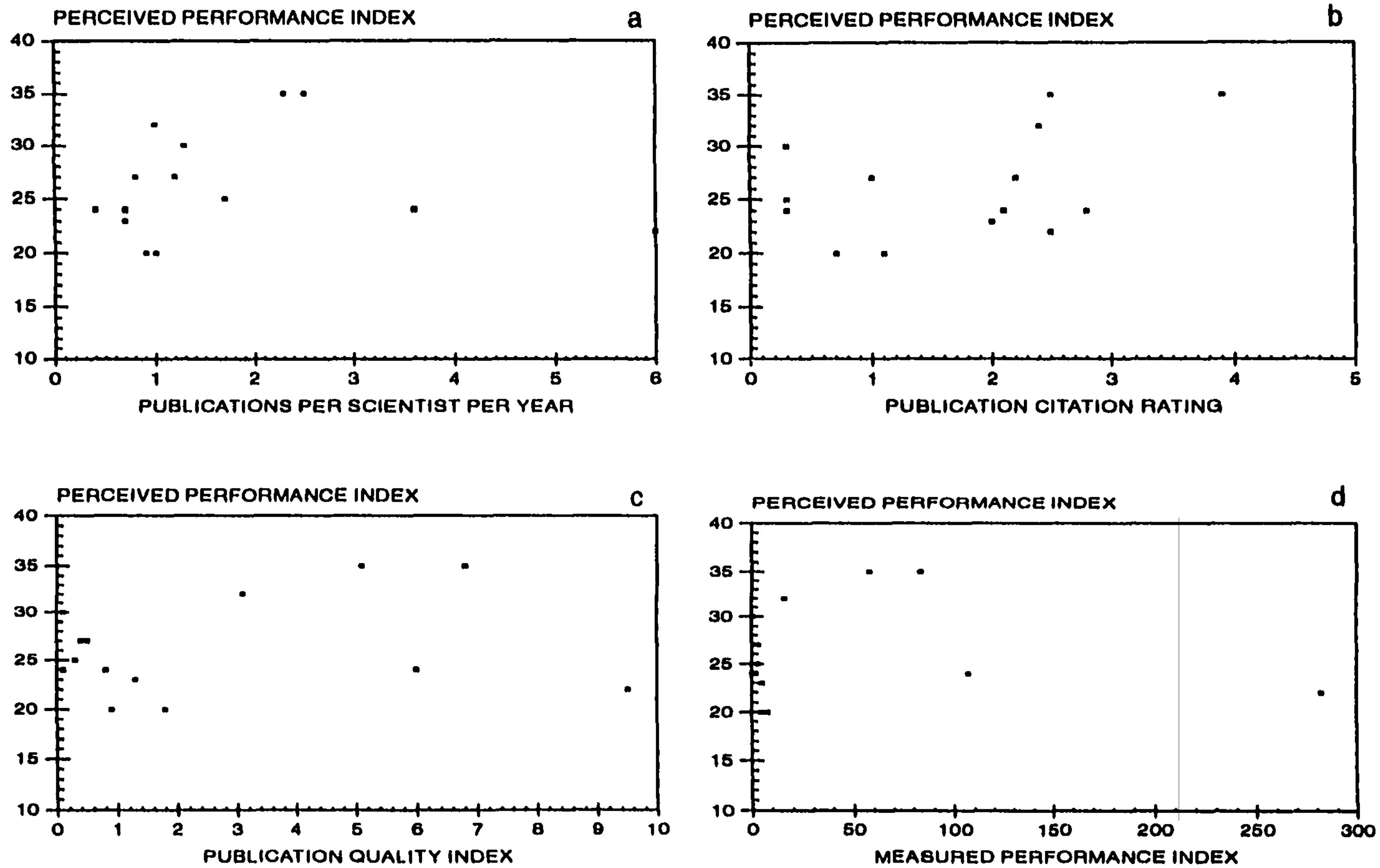


Figure 4.8. Director-perceived group performance vs. (a) publication productivity, (b) publication citation rating, (c) publication quality index, and (d) measured performance.

Table 4.6. Agreement on perceived performance.

SURVEY ITEM	DIRECTOR vs. LEADER	DIRECTOR vs. MEMBER	LEADER vs. MEMBER
Responsive to Clients	-0.048	0.394	0.034
Effective Contribution to Lab Mission & Goals	-0.013	-0.046	-0.132
Productivity	0.372	-0.290	-0.403
Innovativeness	0.064	0.102	-0.076
National Reputation	0.497*	0.174	0.613**
International Reputation	0.564*	0.592*	0.626**
Scientific Quality	0.612**	-0.039	0.261
Policy Relevance	-0.102	0.127	0.438
Scientific Impact	0.367	0.146	-0.110
Meeting Deadlines	0.204	0.000	0.288
Staying Within Budget	0.447	-0.107	0.481*
Transferring Results to Clients	0.376	0.471*	0.406

*significant at p=.95
**significant at p=.99

papers. Incidences of research group members contributing to training courses and providing technical assistance to EPA program offices and regions also were not well documented. For this reason, extrascientific performance could not be measured accurately in this study.

Characteristics of High-Performing Groups

High-performing groups have one or two individuals who are very productive. They carry the group. These high-performers tend to be the same people who are involved with boundary spanning activities, creating new clients and new projects, or who are good at integrating across projects. In addition, high-performing groups published in high-impact journals and published papers that were cited frequently. Not only were the high-performers productive, but they published high-quality papers. No relationship was found between the high-performing groups and organizational contextual variables. Pearson (1983) likewise found no systematic connection between organizational conditions and the quality of research. He concluded that 1) the organization to which the laboratory is attached determines the quality of the laboratory's work; 2) formal control systems and frequent assessments lead to higher quality of results; and 3) participation and structure, in terms of leadership style, are important for group success.

EFFECTIVENESS OF SMALL RESEARCH GROUPS

Research effectiveness is a function of how well research goals are met. Goals for EPA research are established by research sponsors (Congress), national-level research administrators, laboratory directors, division and branch chiefs, group and project leaders, and finally by individual scientists. Goal-setting is hierarchical and therefore depends upon the effective communication of goals down through the organization. The assessment of research effectiveness depends in large part on how quantitative the goals and the outputs or outcomes are expressed. If, for example, a laboratory director sets specific quantitative performance goals for a research group on an annual basis and communicates those goals clearly, research effectiveness is relatively easy to determine. On the other hand, if the goals are vaguely stated or poorly communicated, there are likely to be differences in perceptions about the group's annual performance and effectiveness.

Figure 4.9 shows a plot of director-perceived group effectiveness vs. leader-perceived and member-perceived group effectiveness for the 14 groups studied. If all points plotted along the 1:1 line, one would conclude that both goals and performance were well-understood and well-communicated. However, the fact that many groups plot above the 1:1 line (director ratings of 4.0 and below) indicates that laboratory directors have the perception that goals were not met while group leaders and members perceive that they were effective in meeting goals. The average director-perceived effectiveness for all groups was 4.7, compared to 5.4 for both leader-perceived and member-perceived effectiveness. I consider the difference (0.7) to be a borderline problem. A difference of 0.5 or less would indicate good communication and understanding of goals and performance. A difference of 1.0 or more would indicate miscommunication and misunderstanding of goals and/or performance.

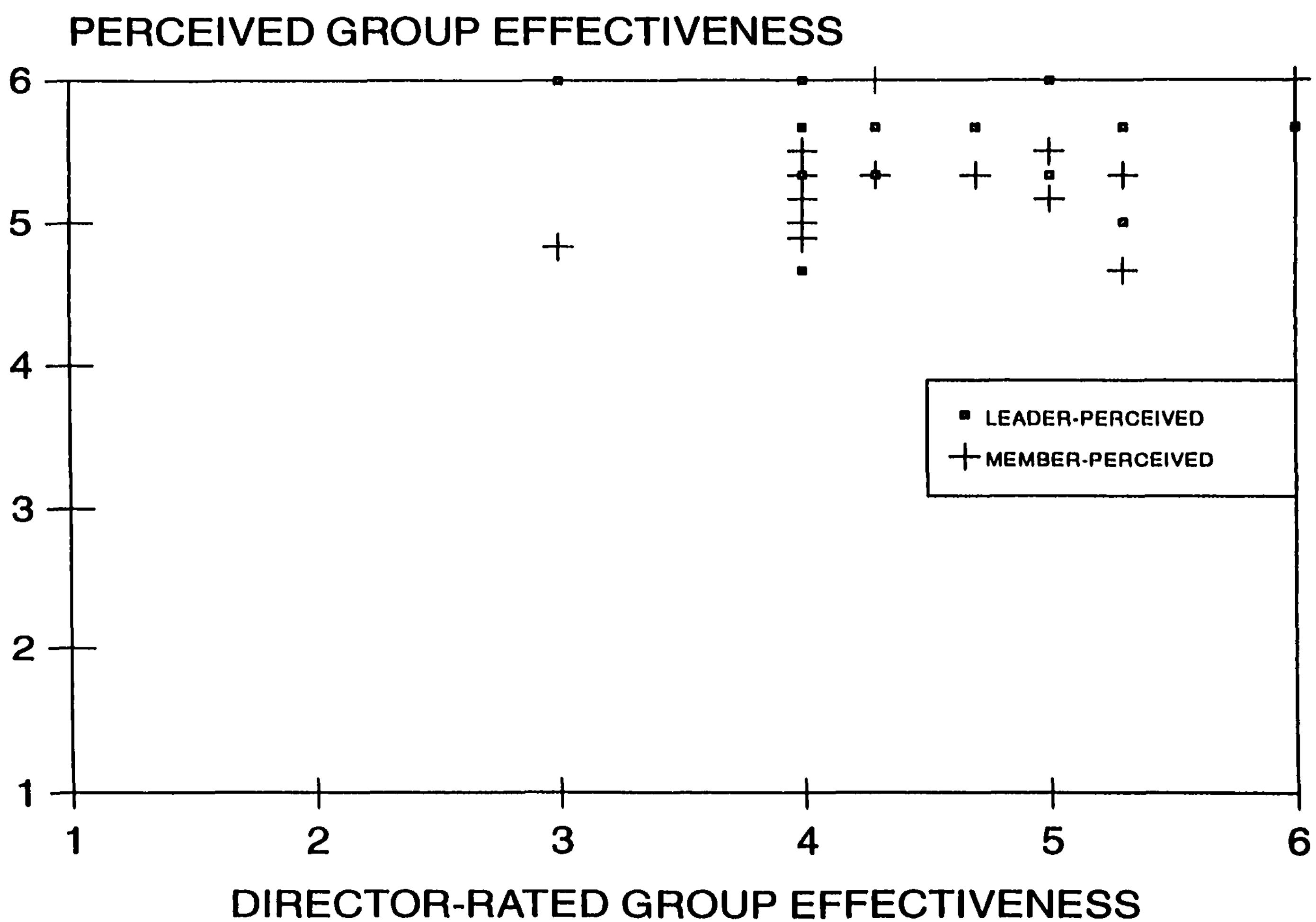


Figure 4.9. Director-perceived vs. leader- and member-perceived group effectiveness.

COMPARISON OF TWO RESEARCH GROUPS

Two research groups were selected for comparison. The wetlands research group at Corvallis was initiated in 1986 as an applied research program having the primary purpose of providing technical support to the EPA Office of Water and the EPA regional offices (Leibowitz et al., 1992). Information on wetlands was needed to improve EPA's ability to fulfill its regulatory responsibilities relating to wetlands. The contaminated sediment research effort began in the late 1980's. A research work plan was prepared early in 1990 (Ankley et al., 1990). This research effort involves developing and validating methods for assessing the ecological hazard of contaminated sediments in Great Lakes harbors and identifying and validating specific techniques for deriving chemical-specific and non-chemical specific sediment quality criteria.

Table 4.7 shows a comparison of the two research groups. The information was developed from the questionnaire results, scientific performance data, and interviews of research group leaders and members. As indicated in Table 4.7, the two research groups differed fundamentally in terms of the nature of the research work. Whereas the sediment group was problem-directed and concerned primarily with developing methods and criteria, the wetlands group was policy-oriented and focused on applied research that would assist regulatory activities. Group differences also showed up in terms of the primary use of research results. The sediment group indicated that impact prediction and mitigation prescriptions were the principal uses of their research results, whereas the wetlands group felt their research results would contribute to policy options as well as mitigation prescriptions. The principal beneficiaries of research were considered to be the

Table 4.7. A comparison of two research groups.

	SEDIMENT CRITERIA	WETLANDS
Nature of the Work	Developmental/ Problem-Directed	Applied research/ Policy-related
Use of Results	Predictions of impacts/ Prescriptions for mitigation	Contributes to policy options/ Prescriptions for mitigation
Principal Beneficiaries	Public	EPA program offices/ State & Federal regulatory agencies
Program Duration	<5 years	>5 years
Publications per Scientist	7.0	6.3
Publication Quality Index	7.3	0.1
Scientific Performance Index	51.2	0.7
Degree of Client Involvement	3.8	5.8
Director-Perceived Scientific Performance	5.3	5.0
Director-Perceived Effectiveness	6.0	5.3
Group-Perceived Effectiveness	5.2	6.0

public and EPA program offices/state regulatory agencies for the sediment group and wetlands group, respectively. The research products data shown in Figure 4.10 also reveal differences between the two groups. More than half of the wetlands group's publications were either internal reports or symposia papers, whereas the sediment group published articles in high-impact and prestigious journals. The wetlands group chose publication outlets appropriate for their principal beneficiaries (EPA program offices/state regulatory agencies), but the sediment group chose to publish results in scientific journals not readily available nor widely read by their principal beneficiaries (the "public").

Scientific productivity was similar for the two groups. However, the sediment group had a substantially higher publication quality index, 51.2, compared to 0.7 for the wetlands group. The sediment group tended to publish in high-impact journals. However, this difference was not markedly reflected in the director perceptions of scientific performance. Effectiveness, as perceived by the respective laboratory directors, was higher for the sediment group. Interestingly, the wetlands group perceived themselves to be more effective (6.0) than did the sediment group (5.2). Also, the wetlands group felt they had much greater client involvement (5.8) in their research than did the sediment group (3.8). My interview results confirmed that the wetlands group spent a great deal of effort interacting with clients at workshops and other informal meetings. A preference for presenting papers and interacting with research users at workshops and conferences is consistent with longterm research on complex environmental and natural resources management problems. These venues offer opportunities for personal interactions between research scientists and their management/policy counterparts for the purpose of

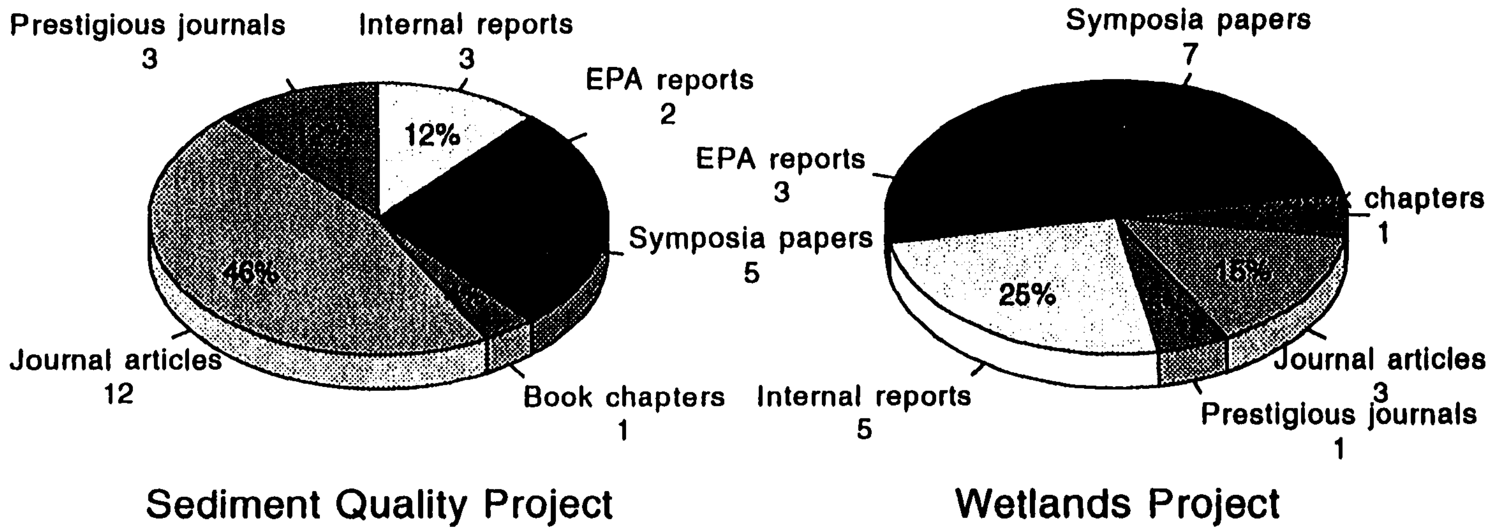


Figure 4.10. Breakdown of research products by category.

conveying scientific understanding. Therein lies a dilemma for research evaluations. The sediment group had a scientific performance index of 51.2, compared to 0.7 for the wetlands group. The sediment group was also perceived, by its laboratory director, to be very effective. And yet the wetlands group was more involved with clients than was the sediment group, based on their self-evaluations. Which group is the more effective? The sediment group focused its efforts on producing products primarily for the scientific community (although this group considered its principal beneficiaries of research to be the "public"), hence the group had an intrascientific emphasis. The wetlands group, on the other hand, focused its efforts on the environmental management and policy community, thus the group had an extrascientific emphasis.

Clearly, one cannot use the same research performance evaluation model for both groups. What about research effectiveness? Effectiveness is a measure of how well goals are met, provided that goals are clear, well-understood, and committed to. Table 4.7 shows that there was a disagreement between laboratory director-perceived effectiveness and group-perceived effectiveness. Why a difference in the case of both groups? Perhaps goals were not sufficiently clear nor mutually understood. Communicating goals from the laboratory director or division chiefs to a research group can be problematic. In any large organization, information flows through filters and may run into barriers. Likewise, the communication of research results relative to goals may also have to contend with information barriers and filters.

IMPLICATIONS FOR RESEARCH EVALUATION

Assessments of research productivity on an annual basis are problematic. Research projects generally span two or more years and often there is a considerable lag between project initiation and the first publication. A project may generate only one major publication every two or three years. Research assessments should be conducted over longer time intervals--perhaps five years.

Multiple-authored papers also present problems in research evaluation. Should all authors receive equal credit or should credit be divided and apportioned according to some formula? Shaw (1967) suggested that dividing the credit (each author receives less than one credit and the sum of credits totals one) discourages teamwork. There is no consistent approach to crediting multiple-authored papers. In this study each author of a multiple-authored paper received full credit for a publication regardless of the order of authorship. In multiple-authored publication efforts, it is incumbent upon the authors to divide the workload equally. Furthermore, a multiple-authored paper should reflect the added diversity and talent of the authors and result in greater value.

Current approaches to research evaluation do not recognize the differences in cognitive difficulty from one research problem to another. Leary (1985) defines two basic classes of research problems: object research and method or strategy problems. With regard to object research, a unit of scientific productivity is a valid answer to a scientific question. Leary limited his discussion of productivity object research. He suggested that not all scientific questions are of the same difficulty to answer. "Why" questions require an explanation and are more difficult to answer than "What if" questions (predictive activity), which are more difficult to answer than "What" or "What is" questions (descriptive activity). Leary suggested using a grid (question difficulty X answer generality) as a first step in assessing scientist productivity and basing rewards on this framework. The same approach could be used for evaluating group research. Such a system would encourage scientists and research groups to work on the most difficult research problems.

Ideally, a research effort should be evaluated at each stage. Knowledge accumulates during each stage of the research effort and should be incrementally captured with an internal report or publication. Thus, a research plan would represent one stage to be evaluated, followed or preceded by a review of literature. Preliminary results represent another stage and an increment of knowledge. Conferences, workshops, and symposia are appropriate outlets for publishing preliminary results or research-in-progress. Mid-project evaluations could be done at this stage as well. Final results are usually documented at the end of a project, but this does have to represent the final increment of knowledge for the research effort. Synthesis papers, drawing upon and across the results of several projects, represent another potential increment of knowledge. Finally, some lines of inquiry may continue for several years. One such example at the EPA laboratories was the acid precipitation research of the 1970's and 1980's. For such lines of inquiry, it is both useful and scientifically responsible to occasionally publish a status-of-knowledge paper. Thus, a research effort may, and probably should, be evaluated at several different stages.

Citation analysis provides a rough index of scientific value. In order to evaluate the quality of a publication or the cogency of a research effort, content analysis must be employed. Essentially, the content of research plans (or problem analyses) and publications is analyzed by an objective person or peer group knowledgeable in the subject matter. This is tedious and time-consuming work, but provides a far better measure of scientific value than citation analysis. Used together, citation and content analyses provide an excellent assessment of research quality and value.

CHAPTER V

ORGANIZATION AND MANAGEMENT OF GROUP RESEARCH

The results presented in Chapter IV raise many new questions about assessing the performance and effectiveness of research as were addressed in the EPA study. In essence, my observations of group research have convinced me that the science community in general lacks a basic theoretical framework for the organization and management of research. More specifically, the planning and management of natural resources and environmental research does not have a theoretical underpinning. This is especially true of group approaches to research.

Science policy in the U.S. and elsewhere in the world is changing fundamentally. We are in the midst of a reformative period in science, or the "period of reorganization" as coined by Sarewitz (1996). The ways in which we approach the organization and management of scientific research, including some of the very basic tenets of science policy, require rethinking. In Chapter IV I looked at the perceived and measured performance and effectiveness of small research groups in a federal laboratory setting. There was a great deal of variance in group performance that could not be explained by traditional organizational contextual variables. I am convinced there is much we do not know about research performance and effectiveness. With the increasing pressures to reinvent science policy and scientific research, it is time to take a closer look at the emerging theory that applies to research organizations as well as the nature of work processes in such settings.

REDEFINING THE DIMENSIONS OF SCIENTIFIC ACTIVITY

Chapter II included an exhaustive discussion of research types. I concluded, as many others have, that the traditional three-way classification of research into a basic, applied, and development continuum is outmoded and served primarily a political purpose. This linear model of science is being replaced by more complex models. The terms "basic" and "applied" have become value-laden (Byerly and Pielke, 1995), rendering the classification rather parsimonious and inadequate (Martin and Irvine, 1989). This classification served the needs of government accountants but "applied" and "basic" became code words in political debates.

Basic and applied research are no longer dichotomies. I prefer the classification scheme of Stokes (1994b) presented in Chapter II, in which fundamental and applied research are orthogonal to one another and the interaction produces what Stokes refers to as use-inspired fundamental or strategic research. The result is not a linear continuum but a two-dimensional representation of research purpose. However, purpose is not the only applicable dimension of scientific research.

Some research must be proactive to management's needs, for example where science enlightens the way for policy development. Such research, called "upstream research" by Arnold et al. (1994), requires considerable foresight and independence from immediate political concerns (also known as upstream research). Thus, there is a certain lead time associated with policy-relevant research, producing an additional dimension of research activity. Martin and Irvine (1989) discuss research orientation, time horizon, and degree of unpredictability of outcomes as important dimensions of research activity. Hanley (1994) suggests that the most productive research is likely to be found in long-term (10 years) research programs of coordinated, interdisciplinary, interrelated studies. Therefore, degree of interdisciplinarity may be another research dimension.

Falk (1988) introduced the dimension of relative autonomy of problem choice. At one end of the continuum, autonomous research would involve minimal external influence on problem choice. At the other end, exonomous research is carried out as part of externally prescribed research problems focused within narrow, specific problem domains. Research orientation or "character of the research" was also considered a research dimension (Martin and Irvine, 1989). According to Armstrong (1994), research investment contains a degree of risk which can be managed. This suggests that degree of risk or certainty of outcome is an important research dimension, not to be confused with the dimension of timespan. Long timeframes for research are acceptable if the potential payoff is very high.

Typologies and dimensions of research, like organizational goals, depend in part on perspective and value, so the various stakeholders, managers, and scientists may view the research portfolio differently. However, from the earlier discussion of dimensions of scientific activity as applied to natural resources and environmental research, we know that the following seven dimensions are critical to research sponsors, managers, and scientists:

1. Time horizon. Martin and Irvine (1989) define short-term as one to three years, medium-term as three to five years, and long-term research as five to thirty years. What is the time horizon for a research organization? Time horizon is a function of the time required to work through the research approach, including data collection, and to meet the project objectives. In its review of the Department of the Interior's proposal to create a National Biological Survey, the National Academy of Sciences (1993, p. 50) made a point to distinguish between short-term and long-term studies, suggesting that this is an important dimension of research investment planning. Smith and McGeary (1997), in advocating a portfolio approach to managing research, suggested a balance between shorter-term research related to agency missions and longer-term investments that would be important in future years. For purposes of this dissertation, I will define short-term as one to three years, mid-term as four to seven years, and long-range as greater than seven years.

Time horizons are becoming shorter because many research organizations are under pressure to produce "science on demand." Witness the enormous demands placed on the new Biological Resources Division of the U.S. Geological Survey to provide timely and relevant research to a wide assortment of clients within and outside government.

2. Fundamental understanding vs. applied or policy-relevant knowledge. This dimension reflects the needs of the client and research sponsors. Research is a purposeful work activity. It is intended to advance the frontiers of scientific knowledge, to solve societal problems, to enlighten policy formulation, to develop new tools and methods, or to provide a scientific foundation for management activities. This is a complex, multifaceted dimension, not a continuum. It incorporates the research model of Stokes (1994b), which redefined the relationship between basic and applied research and provided a theoretical underpinning for the concept of strategic research. This dimension also incorporates the idea of generating new knowledge vs. utilizing existing knowledge (Katz, 1982). This dimension answers the question: What is the purpose of this research? As a societal investment, pure curiosity-driven research is fundamentally different from strategic and anticipatory research or the analysis and synthesis of existing scientific information. Different kinds of investments require different policies, funding strategies, and performance evaluations.

3. Breadth of investigation. This dimension refers to narrow penetrating research vs. broad pioneering exploration. Examples might be detailed research on stomatal structure in aspen trees on the one hand and the synecological role of aspen in subalpine ecosystems on the other hand. Descriptive, exploratory studies and narrow, independent studies can both be valuable for generating and developing important ideas. Both approaches are needed in natural resources and environmental research.

4. Reduction vs. synthesis. Reductionism is the idea that complex phenomena can be understood by reducing them to their basic units and looking at the mechanisms through which the units interact. This is traditional, Baconian science. Much of biological research has relied on reductionism. In fact, reductionism is so ingrained into our culture, that it is strongly identified with the scientific method (Capra, 1982). However, a need also exists for integrative or holistic approaches which attempt to understand entire systems from an interdisciplinary, collaborative perspective (Miller, 1984). Both reduction and synthesis are required in natural resources and environmental research.

5. Anticipatory and strategic vs. goal-directed and tactical. Good examples of anticipatory or "upstream" research are difficult to find amongst the myriad of mission-oriented natural resources and environmental research programs. Anticipatory research lays the ground work for future investigations, identifies and analyzes emerging issues, and seeks to prevent issues from becoming intractable problems. Strategic research is designed to build the base of knowledge and skills in an area of evident interest to a broad class of users external to the research community (Averch, 1991). Most research is directed at achieving mission goals. It is here-and-now research done in response to immediate concerns of research sponsors. Tactical research, in the natural resources and environmental sense, seeks to address the immediate concerns and information needs of managers. This dimension is not to be confused with the time horizon dimension, since anticipatory research may be relatively short-term and mission-oriented research could extend several years in duration. Anticipatory research is not necessarily the same as fundamental understanding; strategic research bridges the old concepts of applied and basic research. Likewise, goal-directed or mission-oriented research can be conducted for purposes of either fundamental understanding or policy relevance. Tactical research, on the other hand, is likely to be short-term and policy-relevant or of direct use to managers.

6. Intramural vs. extramural. Intramural research is conducted in-house; extramural research is accomplished through contracts with universities, private research groups, or other research agencies. Some degree of balance between intramural and extramural research is desirable. Extramural research adds new dimensions and perspectives to in-house research programs.

7. *Small and focused vs. large and comprehensive.* Big science and little science--there is a place and purpose for both. Small projects conducted by individual researchers were responsible for most of the scientific breakthroughs in the world. The Manhattan Project, our first experiment with big science, touched off five decades of nuclear research with enormous scientific, military, and social implications. The International Biological Program, funded by NSF during the late 1960's through the late 1970's, was not only an attempt to understand the structure and function of major ecosystems but also a grand experiment in interdisciplinary research. It was big, comprehensive science composed of many individual small investigations plus an attempt at integrating those pieces into a systems level view of how ecosystems function. Ecosystem research probably requires relatively large-scale approaches in terms of resources and numbers of investigators. At the same time, such research depends on the integration of many narrowly-focused, process-oriented studies.

A fresh approach to defining dimensions of scientific activity would consider the likely types of scientific information needs of natural resource and environmental policy makers, managers, and operational scientists. It is clear, then, that scientific activity is multidimensional and much more complex than the traditional linear model of science inferred. This multidimensionality is one of the cornerstones of a theory construct for research management and is a key factor in designing research organizations.

ORGANIZATIONAL DESIGN CONSIDERATIONS

In the process of interviewing research administrators, I observed that there are two dominant philosophies about the management of research. Weinberg (1974, 1984) captured these succinctly in his contrast of strategists and institutionalists. Institutionalists, he claims, stress the recruitment of good scientists and nurturing of the institution and assume that good results will be produced by good people adequately supported. This is similar to the Human Relations and Open Systems Models in the competing values approach. Strategists, on the other hand, focus on a plan and the substantive scientific problems (Internal Process and Rational Goal Models). Based on the logic developed in Chapter II, then, the strategist and institutionalist approaches are orthogonal to one another. Rarely does one find a research administrator that is purely a strategist or purely an institutionalist. At the national scale, the contrasting modes of administering science are very similar, parallel in fact, to the institution or laboratory scale. The particular mode to be adopted depends on the nature of the problem to be solved. According to Weinberg (1974), strategists tend to be associated with applied research and institutionalists with basic research.

Keidel's Organizational Dimensions

Keidel (1995) employs a different approach to organizational analysis. His triad model appears in Figure 5.1. This triadic approach to organizational analysis works very well for

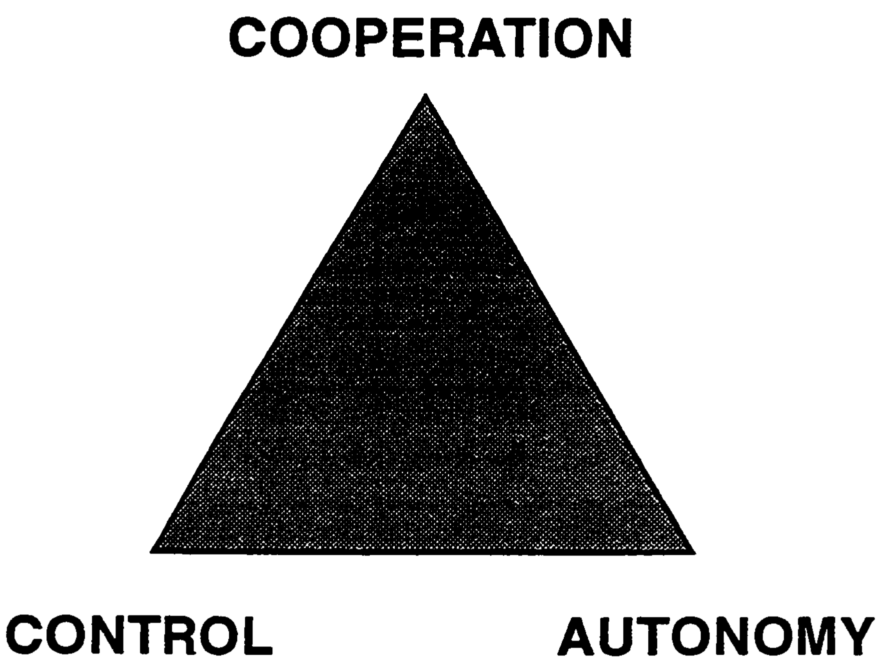


Figure 5.1. Keidel's organizational design dimensions, after Keidel (1995).

envisioning and modeling design tradeoffs and is similar in concept to the competing values approach of Quinn and Rohrbaugh (1983). Autonomy versus cooperation is equivalent to accountability versus synergy. Control versus cooperation is similar to consistency versus innovation. Autonomy versus control suggests bottom-up versus top-down. Clearly, the two-variable tradeoffs resemble managerial thinking patterns and choices that prevail today. The challenge to managers, according to Keidel (1995), is to think in terms of triangular tradeoffs--that is, balancing all three organizational variables.

In terms of research organizations, the control variable would include processes such as peer review, research planning and goal setting, research evaluation, and cogency in conducting scientific investigations. Autonomy would encompass the freedom to choose research problems and approaches, including the carving out of unique research niches, and clearly supports innovation. The cooperation dimension in research includes research dyads and larger groups, integration of work processes and results, interdisciplinary approaches to research, boundary-spanning, research partnerships, and other pluralistic activities. Pluralization, then, is associated with Keidel's cooperation dimension. The integrator and instigator roles first identified during the EPA study and discussed in Chapter IV, as well as the increasing trend towards interdisciplinary research, are signs of natural resource and environmental research becoming more pluralistic. Pluralization in science encompasses at least the following:

-multiple dimensions of research character

-balance of intramural and extramural research

-interdisciplinary and cross-disciplinary projects

-diverse research portfolios

-diverse workforces and perspectives

-integration and cross-fertilization of ideas and activities

-organizational flexibility and new types of institutions

-broad spectrum of research approaches

-inclusive and participatory leadership and decisionmaking

-multiple lines of inquiry about similar research issues

-socially-distributed knowledge

-new patterns of formal and informal international cooperation

-research networks and collaboratories

-research teams

The pluralistic model of research (Brooks, 1973; Smith, 1973) is more tenable if the various research entities are effectively networked, if it can be presumed that they are sharing relevant and mutually beneficial information, and if it can be demonstrated that science and its customers benefit from the pluralism. In theory, the pluralistic model of research increases the pool of intellectual variants and allows for constructive criticism of, and selection among, those variants. One way to test for the benefits of pluralization through citation analysis to see how research groups working on similar research problems are communicating via the scientific literature. Following Keidel's scheme and adding the pluralist dimension to Weinberg's two modes of research administration, a triadic model of research administration is developed (Figure 5.2).

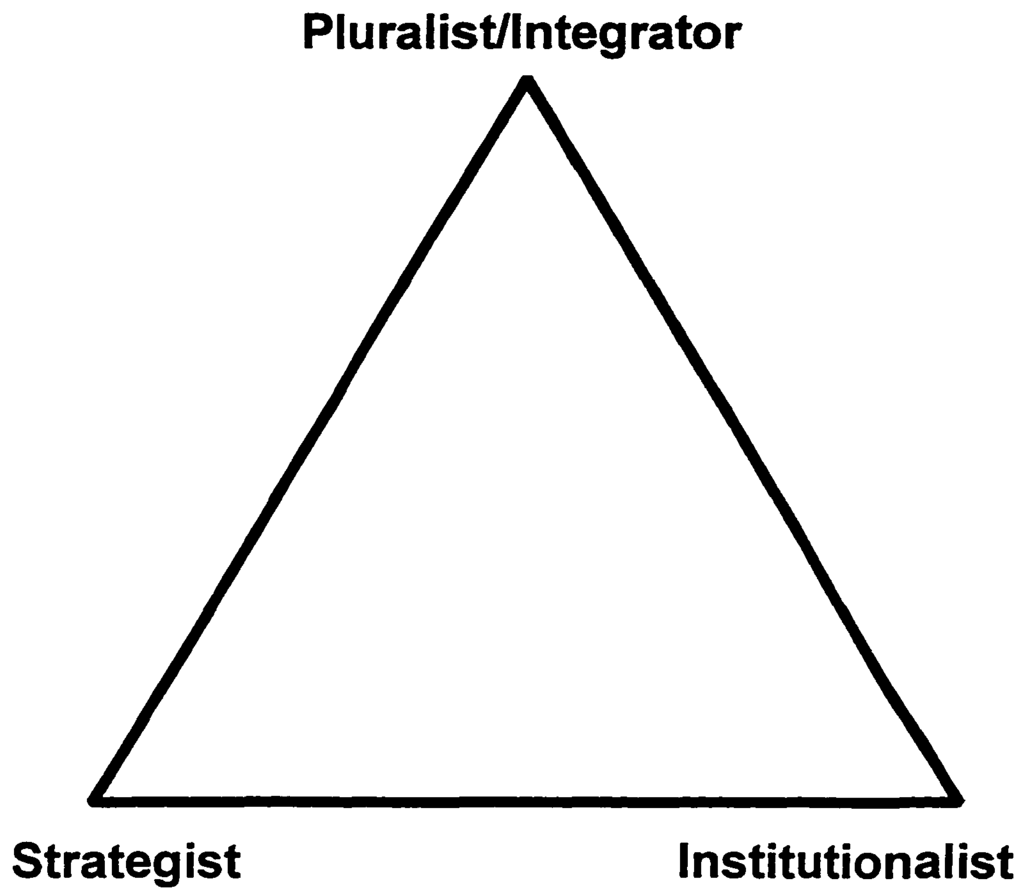


Figure 5.2. Triadic model of research administration.

Brooks (1973) raises two important questions regarding the organization and administration of science:

1. How does one organize, staff, and direct the search for knowledge so as to obtain the greatest rate of scientific progress for given investment of human and material resources?

2. How does one couple the existing body of knowledge and the search for new knowledge to existing needs for policy or action?

These questions may be found at multiple levels from the individual laboratory to national research programs. The focus today has shifted to the issue of moving knowledge into decision and action. The best scientific strategy (according to Brooks) lies in a balance between isolation from practical problems and a narrow channeling on the basis of practical needs. This is another argument for Stokes' (1994a,b) orthogonal approach to basic understanding and use-oriented research dimensions.

One of the key issues in scientific organization is how to reconcile the scientific need for autonomy and integrity in its own internal processes of exploration and self-criticism with the demands of society (and the parent organization or stakeholders) that the fruits of science be guided into channels which society deems beneficial. Science is not exogenous to society. Society, science, and technology are interactive. Uncertainties, assumptions, and compromises must be fully exposed to public scrutiny (Newby, 1992). Brooks (1973) calls this a dynamic tension between science and society.

Clear goals, clear direction, and expensive facilities suggest a centralized, strategic approach. Indefinite goals, unclear direction, and inexpensive facilities suggest a decentralized, "institutionalist" approach to research management. Research aimed at solving a specific problem is risky and inefficient. A decentralized, institutional approach was used for the Manhattan Project, even though the goal was relatively clear. They instead focused on the goal of making the atomic bomb and then supported the various institutions selected to do the research. Under the competing values scenario, the Manhattan Project might be considered a combination of the Open Systems and Rational Goal Models. A strong scientific institution must be both coherent and independent, yet responsive and responsible. So an essential tension (competition among values) exists between the four models. One could argue that the ideal research environment would involve some mix of all four values.

Eliminating the Fiefdoms

My observations of the EPA laboratories and other research organizations have convinced me that rigid branch structures have no place in research settings. I agree with Kanter (1983) that organization segmentation, for example departments and multiple organizational layers, results in roadblocks to innovation--a real anathema to research organizations. Organizations that use a "segmentalist" approach see problems very narrowly and are concerned with compartmentalizing both problems and solutions. The traditional bureaucratic model of organizations is characterized by functional division of labor, hierarchical controls, and procedural regulations, which produce functional fiefdoms and organizational rigidity (Durant, 1992). Byrne (1993), referring to such vertically-structured organizations, stated that people feel loyalty and commitment to the functional fiefdoms, not the overall organization and its goals. Kanter (1983, p. 293) proposes that segmentalist organizations are less responsive to their external environments. Information flows more freely across integrative structures and the resulting culture encourages identification with larger units and issues rather than smaller units and specialties. External changes and "danger signals" are seen earlier by a larger group instead of just a few individuals.

Wheatley (1994) refers to the machine model of organizations in which managers manage by separating things into parts. If organizations are machines, then control makes sense. However, if organizations are process structures, it is managerial suicide to impose control through permanent structures. This is particularly true in the case of adaptive organizations like today's research laboratories and other science centers. What is needed instead is a form of bounded control, where control exists only through clearly specified goals and available resources (Churchman, 1973).

Kanter (1983) talks about segmentalist organizations not being flexible and responsive, Byrne (1993) castigates functional fiefdoms, Durant (1992) refers to the traditional bureaucratic model of organizations producing organizational rigidity, and Wheatley (1994) is frustrated with organizational reductionism. Thus, there is a common thread running through the current organizational science literature. Fixed, rigid, and static organizational structures, control-oriented turf-protecting branch chiefs, unit boundaries that are virtually impenetrable, poor horizontal communications, functional division of labor, and segmentalist cultures should all be eliminated from innovation-minded organizations. Do segmentalist chiefs and their fiefdoms exist in science organizations today? They most certainly do exist in universities and most government laboratories.

Project Teams, *ad hoc* Teams, and Dyads

A logical premise stemming from the literature and from my observations of many research settings is that small research groups form the basic work unit for natural resources and environmental research organizations. This is another cornerstone for an underlying theory of research management. The groups may or may not be formally recognized organizational units. Some of the more successful research groups I observed were *ad hoc*. Often the groups are dyads and triads created informally in response to common interests or mutual dependencies. It should also be noted that research groups are rarely, if ever, permanent and that scientists serve with more than one group at a time. Groups are created, achieve their goals, and then dissolve or reconfigure into new groups.

Katzenbach and Smith (1993) define a team as a "small number of people with complementary skills who are committed to a common purpose, performance goals, and approach for which they hold themselves mutually accountable." In addition, research teams achieve some measure of synergy, are characterized by open, honest, and frequent communications, and their members act interdependently with genuine interest in each others' expertise. Successful research organizations encourage such teams to form and create a climate and structure where everyone comes into contact with everyone else by way of new teams and new configurations (Kanter, 1983). Innovative and integrative organizations foster constant communication; information flows quickly through the organization.

The open systems perspective holds that teams are self-organizing, forming in response to specific research needs and problem domains. I observed this happening at the Duluth laboratory. Successful teams attract team members who are interested in the research topic, have the needed skills, and are able to contribute positively to the group effort. According to (Katzenbach and Smith (1993), commitment is the key to high-performing teams.

Figure 5.3 is an organizational model for teams as developed by Katzenbach and Smith (1993). The vertices of the triangle are what teams deliver. The sides are the elements needed to make that happen. This model is similar to Keidel's triad. Collective work products represents Keidel's collaboration dimension; performance results are related to Keidel's control dimension; and personal growth is similar to Keidel's autonomy. Figure 5.4 idealistically shows the transition of a working group to a high-performing team in terms of team effectiveness and performance impact. Katzenbach and Smith (1993) admit that there is no recipe for building high-performing teams from working groups.

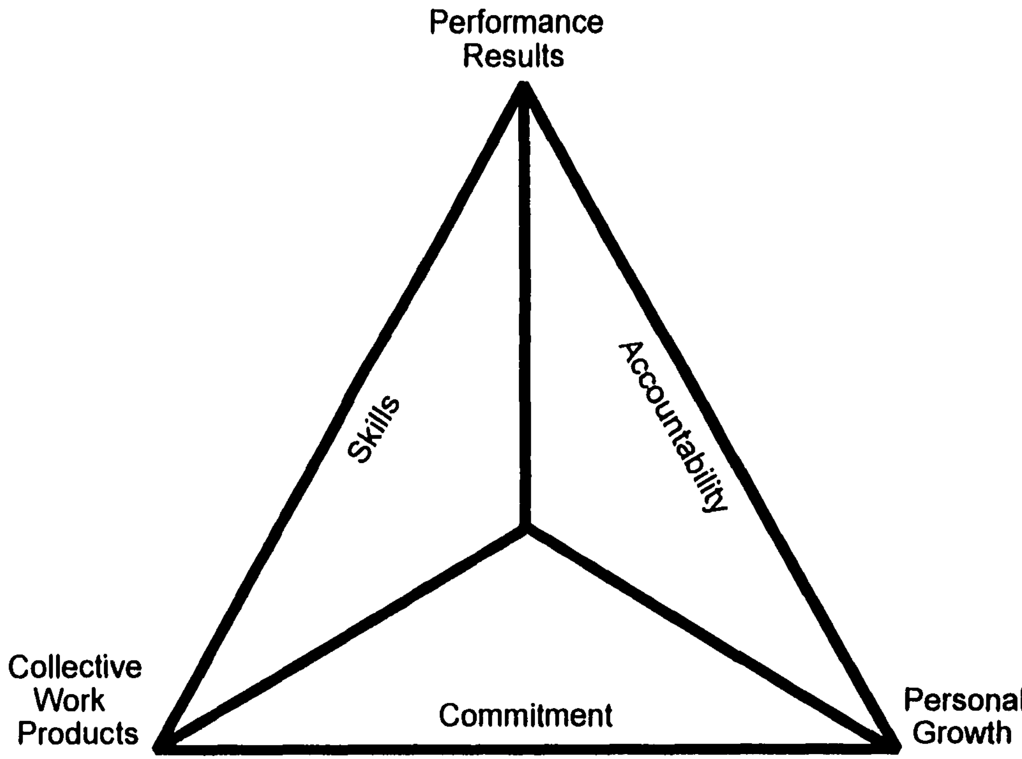


Figure 5.3. The basics of high-performing teams, after Katzenbach and Smith (1993).

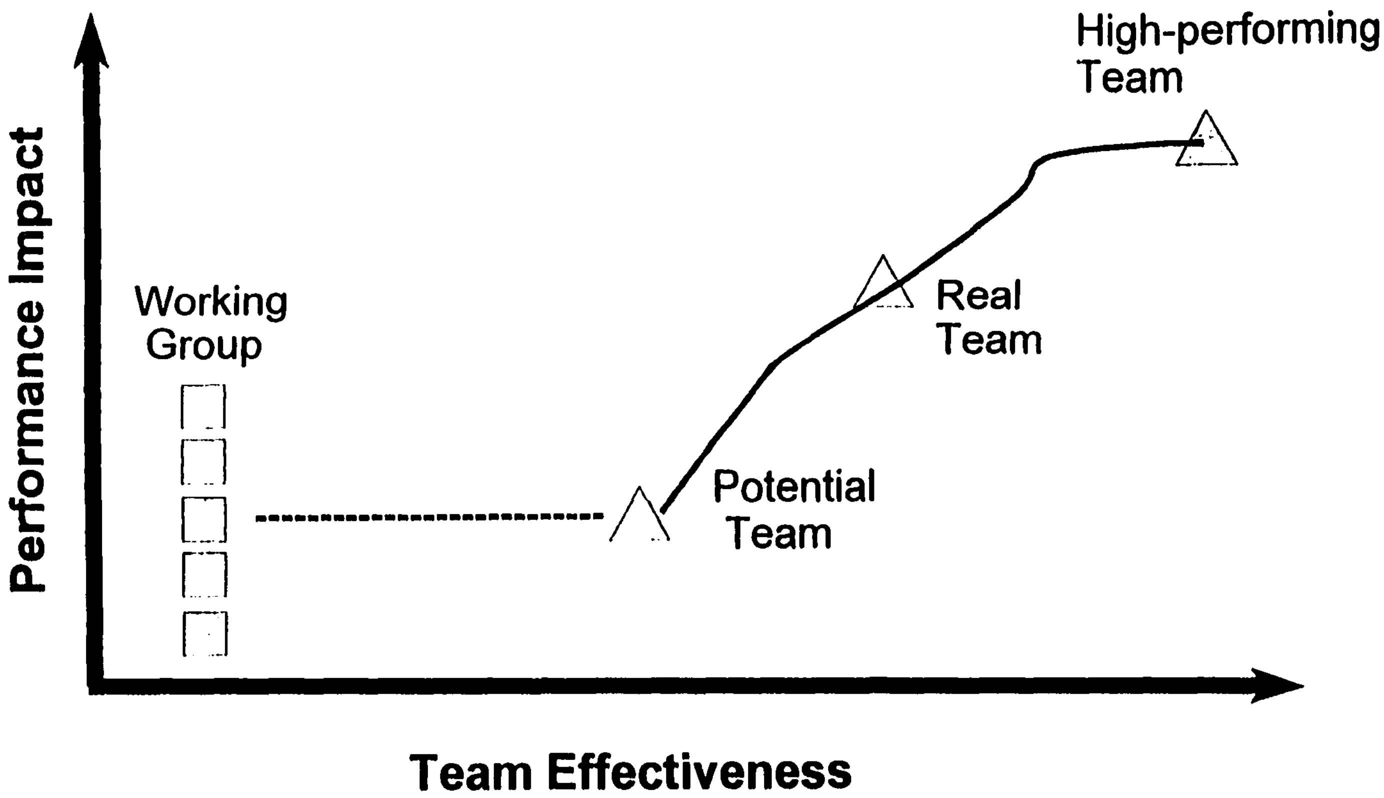


Figure 5.4. Performance and effectiveness of working groups vs. teams, after Katzenbach and Smith (1993).

Integrators and Instigators

High-performing research groups in the EPA study all had one characteristic in common. They were carried by the energy and motivation of one or more individuals who were either integrative or boundary-spanning in their cognitive orientation. I have labeled these roles "integrators" and "instigators," respectively. It became apparent that laboratories required both types of individuals in order to successfully carry out their research missions. Integrators and instigators see a bigger picture than other scientists. Although they may have been excellent bench scientists in the reductionist sense, they have developed a broader perspective with age and experience.

Schreyer (1974) studied the individual orientations of Forest Service research scientists, defining individual orientation as predispositions to particular kinds of problem-solving behavior influenced by the individuals cognitive style in response to the perceived environment. He defined a cognitively simple orientation as being predisposed to using specialized, narrow, and inductive research approaches and a cognitively complex orientation as being inclined to investigate broad areas of research and/or several facets of a research and favoring deductive research approaches. He hypothesized that certain individual orientations are more conducive to effective research in certain contexts. He found it possible to identify scientists as being integrative or reducing in their overall perspective and concluded that integrators are necessary for research organizations to be successful, both in terms of output and application. Integrators working together on the same project create much more effective research situations (Schreyer, 1974, p. 227).

I first observed the integrator role operating in a research setting at three of the EPA laboratories I visited. The roles were not the same, however. At two of the laboratories, integrators were acting as facilitators to unify and synthesize information developed by separate projects. One of the integrators, as evident from the literature, had collaborated with at least a dozen other scientists at his laboratory. At another laboratory, integration was in the form of boundary spanning. The integrator, a senior scientist, was working within a collaborative, multiagency, multinational research planning group to design comprehensive environmental research for the Great Lakes. These integrator roles were not recognized in the titles or position descriptions of the scientists (or in one case a branch chief) doing the integration. They were individuals who naturally gravitated to that role and had the cognitive ability to undertake the complexities of research integration.

Ironically, I identified the integrator and instigator roles before reading Schreyer's excellent work on researcher orientation (Schreyer, 1974). He described an integrator as a person who interacts with a wide spectrum of people as an effective group worker and information processor, particularly effective in complex problem-solving situations (Schreyer, 1974, p. 207). She/he is also likely to be a highly stimulated scientist. At the EPA laboratories, integrators bridged both projects and scientific disciplines. Integrators in research settings are expected to take fragments of research results and synthesize them into a more general finding.

Hall (1987) refers to integration as establishing and organizing a set of relations among member units of a system that serve to coordinate and unify them into a single entity or activity. Integrators see problems as wholes. Rather than walling off the problem, integrators create mechanisms for the exchange of new information and ideas across suborganizational boundaries (Kanter, 1983). The task of the integrator is not to do the work but to integrate the work process. Integrators function as internal boundary spanners, building relationships across work functions (Hautaluoma and Woodmansee, 1994). The integrator has a wide range of contacts and exposures and is at the crossroads of several information streams. According to Galbraith (1973, pp. 93-95) the effective integrator also has an orientation toward organizational goals rather than parochial goals, is unbiased, knowledgeable, and enjoys a measure of trust and credibility across the organization.

In her literature review on interdisciplinary research, Parker (1993) identified the following skills that would be associated with the integrator role: collaborative participation; obtaining some degree of knowledge of the pertinent concepts, jargon, and methods from other disciplines; organizing the input of individual researchers into interrelated and relevant patterns. She also listed integrative techniques such as meetings to achieve certain interactions, joint data gathering and analysis, and shared responsibility for outputs.

Kanter (1983, p. 178) concluded that the highest proportion of entrepreneurial accomplishments was found in companies that were the least segmented, that instead had integrative structures and cultures emphasizing pride, commitment, collaboration, and teamwork. Segmentalist organizations were also found to be less externally responsive (p. 293). Information flows more freely across integrative structures and the culture encourages identification with larger units and issues rather than smaller units and specialties.

Integrators become important when the environment is highly uncertain. More information processing is required to achieve coordination. Organizations actually perform better when levels of integration match the level of uncertainty in the environment (Daft, 1992, p. 82). Schreyer (1974, p. 35) similarly concluded that more complex cognitive systems can function more effectively in changing situations and situations wherein new perspectives and solutions are required.

Instigators are very adept at spanning organizational boundaries, breaking out of paradigms, and finding promising new research directions. They may work inside their organization developing new research themes or initiating new projects, or they may spend most of their time working with external groups and organizations on collaborative projects. They kindle ideas and inspire other scientists. They are the entrepreneurs of the research world. At the EPA laboratories, instigators tended to work well with other agencies and groups external to their home laboratory. They are likely to have an integrator orientation as opposed to a reducing or reductionist orientation. Instigators assume a politically-oriented role in organizations. They are people who form a "polis" around the need for change. More than anything else, they keep stirring the pot and adding new ingredients. At the EPA laboratories I observed two senior scientists and one branch chief taking on the instigator role. These individuals were constantly involved in new initiatives both within their laboratories and with other agencies. They incite action in new research directions. They were often successful in bringing non-EPA funds into their laboratories.

Integrator and instigator roles are essential to any adaptive organization. I am convinced that both roles are necessary for any research organization to be successful. They are an essential part of the cooperation role in Keidel's organizational analysis model. The following proposition results: Effective natural resources and environmental research organizations will have one or more individuals (usually senior scientists) functioning in integrator and instigator roles.

Research Collaboration

Science assessment efforts like the Interior Columbia Basin Ecosystem Management Project and the Southern Appalachian Ecosystem Assessment are early examples of a project organization with external linkages ("extended project structure"). They are symbolic of science moving from hierarchies to networks (Meidinger, 1997). Intraorganizational relationships are designed around information exchange rather than supervision and control. Other forms of collaboration include intra- and inter-organizational research teams, national and international research networks, and collaboratories. Collaboratories, or virtual laboratories facilitated by the Internet, are based on data-sharing arrangements between collaborating researchers. The National Center for Ecological Analysis and Synthesis in Santa Barbara provides the physical space and computing facilities for research teams to meet for short periods of time.

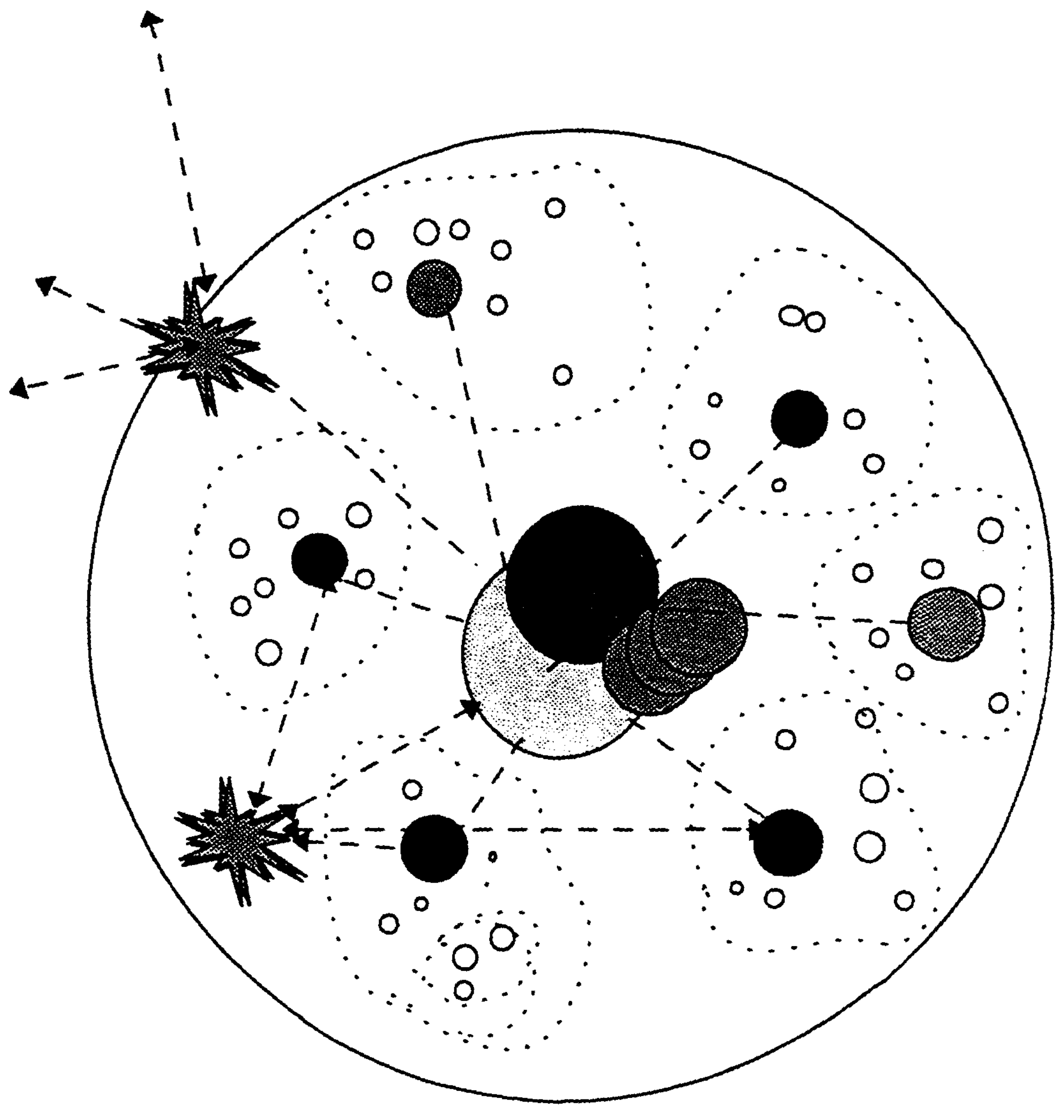
The Dynamic Constellation: A Proposed Structure for Research Organizations

Byrne (1993) reviewed various organizational structures that were designed to achieve horizontal integration and eliminate vertical hierarchies. His "starburst" and "pizza pie" structures gave me the idea for an organizational structure I refer to as the *dynamic constellation*. Fourcade and Wilpert (1981) and Dumaine (1994) both used the term constellation in reference to work groups and teams. Durant (1992, p. 268) suggested organizing around dense clusters of interaction. Meidinger (1997) referred to this type of structure as "loosely coupled organic networks."

The dynamic constellation is a product of integrating the ideas of many others. More importantly, all the elements of the dynamic constellation, as shown in Figure 5.5, were observed in one or more of the EPA laboratories.

The dynamic constellation is my concept of an ideal research organization. It is an open, adaptive system which is perpetually in motion. The constellation contains clusters of scientists and technicians loosely organized around research themes, projects, and scientific interests. A cluster may be as small as a dyad of scientists that often collaborate on publications or as formal as a chartered project. As I observed at two of the EPA laboratories, there may be clusters within clusters. The clusters are constantly changing, as opposed to traditional bureaucratic organizations which have rigid, static organizational segments. As Kanter (1983) stated, the innovative organization must continually create teams that represent new and different configurations, offering the potential for many people to find a connection with nearly everyone else. Clusters may

Figure 5.5. Dynamic constellation model of a research organization.



take the form of a "shamrock organization," consisting of a small full-time professional core, a flexible part-time and/or temporary workforce, and outside contractors (Handy, 1989).

The keystone of the dynamic constellation is that it is highly adaptable. Chubin et al. (1979) remind us that modern research organizations must adapt or be modified in response to the complexity of the problems they seek to investigate. The dynamic constellation is designed to handle that complexity. Institutions with an adaptive response anticipate and detect problems and opportunities and turn them into benefits (Holling, 1978). The dynamic constellation is designed to produce multiple benefits from interdisciplinary team research.

What is the manager's role in the dynamic constellation? Effective organizations are self-organizing systems. Information access is a key to self-organizing systems. Barriers to horizontal and vertical communications are a problem in most organizations. If organizational streams are well-stocked with information, information will find its way to where it needs to be. The manager's job is to keep the streams clear and to diffuse information as networks instead of linearly. The new world of information is associative, networked, and heuristic (Wheatley, 1994). Management does not mean protecting and controlling turf. The new managers (or leaders) must be equilibrium busters. Management and leadership in research organizations should consist of designing settings which stimulate and integrate productive individual actions. Organizational forms must facilitate those processes. The dynamic constellation provides organizational overlaps, team mechanisms, and other communication channels which Kanter (1983, p. 295) says keeps more ideas circulating.

Organizational purpose is shared through goal congruence and shared vision. It is the manager's job to set clear goals hierarchically, to obtain goal agreement, and to make sure everyone in the organization understands those goals. Research performance, or effectiveness, is then measured against those goals at each level of the goal hierarchy. I cannot improve on Wheatley's perspective of the future:

> "We have begun to speak of more fluid, organic structures--even boundaryless organizations-- construing them as learning organizations with a self-renewing capacity. Organizations of the future that will replace bureaucracies will have structures that come and go as they support the process that needs to occur and where form arises to support the necessary relationships. Structures should emphasize the interactions we need. Contrary to fixed, static organizations of the past, we really don't want organizational equilibrium. We want open systems that engage with their environment (customers, linkages) and continue to grow and evolve--capable of self- renewal. Self-organizing systems are characterized by resiliency rather than stability. They possess the capacity for spontaneously emerging structures, depending on what is required. In adaptive organizations, the task determines the organization form. Organizations structured around core competencies, along with adaptive organizations, both avoid rigid or permanent structures and instead develop a capacity to respond with great flexibility to external and internal change. Expertise, tasks, teams, and projects emerge in response to a need. Such an organization can only exist if it has access to new information, process the new data with high levels of self-awareness and a strong capacity for reflection. An organization that focuses on core competencies identifies itself as a portfolio of skills rather than a portfolio of individual organizational units. It can respond quickly to new opportunities because it is not locked into rigid boundaries of pre-established end products or disciplinary units. It is sensitive to its environment and at the same time resilient from it" (Wheatley, 1994).

Organizations do not change easily. Science organizations are in some respects the most rigid organizations in society. Values of the past created the institutions of the present, and changing social values will engender the institutions of the future (Cortner et al., 1995). Change, or reculturing, must come from the inside and includes redefining boundaries, processes, behaviors, incentives, and rewards (Chubin, 1996).

ESSENTIAL TENSIONS OF RESEARCH MANAGEMENT

The challenge of balancing conflicting perspectives and functional requirements confronts every organization (Quinn and Rohrbaugh, 1983; Keidel, 1995, p.7). Management is often defined as making choices among competing alternatives. The wise manager deals with these choices at two levels--strategic and tactical. Strategic choices are long-term in nature, whereas tactical choices are day-to-day business decisions. There is considerable evidence to indicate that competing alternatives are found throughout science and are faced by both scientists and research managers (Weinberg, 1963).

Kuhn (1977) coined the term "essential tension" to describe pairs of competing demands in science. One example of an essential tension is the need for both convergent and divergent thinking in scientific research. This concept is similar to the "creative tensions" observed by Pelz (1967) in his study of researcher performance as a function of organizational climate. Scientists in the study performed best under conditions of creative tension such as stability (independence/autonomy) plus challenge (vigorous colleague interaction) or collegial support plus intellectual conflict. Quinn and Rohrbaugh (1983) talk of competing values, flexibility vs. stability, external emphasis vs. internal emphasis, and means vs. ends. Another area of tension lies in the area of choice of agents or research performers (Guston, 1996). This involves fundamental issues of goal alignment between research sponsors and research performers and what instruments of control over the performers are available.

In Table 5.1 several essential tensions inherent to research activity are presented and

Table 5.1. Essential tensions of research management, using dimensions from Keidel's organizational analysis triad.

Autonomy	vs. Cooperation
intradisciplinary	vs. interdisciplinary orientation (Kuhn, 1977; Fox, 1994)
scientific advancement	vs. societal relevance (Weinberg, 1963; Brooks, 1973)
autonomy	vs. heteronomy (Beckman, 1989; Pelz, 1967)
research activity	vs. expert advice (Nordic Comm. for Environ. Res., 1991)
personal harmony	vs. intellectual conflict (Pelz, 1967)
divergent thinking	vs. convergent thinking (Kuhn, 1977)
linear thinking	vs. systems thinking (Capra, 1982)
intramural	vs. extramural (CENR, 1995; Guston, 1996)
individual research	vs. team research (Ziman, 1994, p.158; Fox, 1994)
Control	**vs. Cooperation**
reductionism	vs. synthesis (Capra, 1982; Shapere, 1986)
segmentalized/hierarchical	vs. organic/parallel/integrated (Kanter, 1983)
reacting	vs. anticipating (Schaefer, 1991)
rational	vs. intuitive (Capra, 1982)
self-assertive/competitive	vs. partnership/cooperative (Capra, 1982)
administration	vs. ministration (Cowan, 1972)
top-down	vs. bottom-up (Martin and Irvine, 1989)
Control	**vs. Autonomy**
narrow penetrating investigation	vs. pioneering exploration (Pelz, 1967)
mission/policy/relevance	vs. independence/continuity (Schaefer, 1991; Davies, 1994)
managerial questions	vs. theoretical questions (Cortner et al., 1995)
supply-push	vs. demand-pull of results (Martin and Irvine, 1989)
intensive management	vs. trust (Pelz, 1967)
program-supportive	vs. niche-building (Van Haveren, 1994)
closed organization	vs. open organization (Kanter, 1983; Beckman, 1989)
proven performers	vs. promising new initiatives (Beckman, 1989; Kanter, 1983)
investments in planning	vs. investments in personnel (Weinberg, 1974)
targeted or strategic research	vs. curiosity-driven innovation (Stokes, 1994a,b; Fox, 1994)
agency-targeted	vs. investigator-initiated (Fox, 1994)
theory advancement	vs. experimentation (Herrick and Jamieson, 1995)

placed into categories similar to Keidel's (1995) organizational analysis dimensions and management tradeoffs. Keidel's dimensions were shown in Figure 5.1. The cooperation vs. control tension is identical to Capra's *yin* and *yang* comparison (Capra, 1982). In Chinese philosophy, *yang* corresponds to self-assertive, aggressive, controlling, competitive, and reducing behaviors while *yin* corresponds to integrative, responsive, collaborating, and synthesizing behaviors. Since Keidel adds autonomy as a third dimension, which is an organizational variable certainly present in research organizations, two other tension categories result. The collection of essential tensions found in the literature fall appropriately into one of the three tension categories.

The result is a concise framework for managing science. In this framework, tensions are seen as a natural and essential part of managing science. The manager's goal is not to strike an equal balance between the competing interests, but to balance the demands according to constant changes in the external environment. Balancing the tensions is similar to adjusting the balance on a stereo system. Balance is adjusted relative to where the listener is sitting. Instead of frustrating obstacles, these essential tensions are tools and opportunities available to the research manager. The management implications of Table 5.1, in terms of a research investment portfolio, are discussed later in this chapter.

The triadic model of research administration shown in Figure 5.2 represents a three-way or three-dimensional management model for research administrators. Essential tensions are inherent to this model. Recognizing that all three administrative approaches are critical to managing research in the post-reformative period, the effective administrator will provide both balance and appropriate bias to the three approaches. The addition of the pluralist dimension and the perspective of managing research using essential tensions is the third cornerstone in the post-reformative theory of research management.

RESEARCH EVALUATION

The fourth cornerstone of the post-reformative theory underlying research management asserts that research evaluation considers both intrascientific and extrascientific outputs, impacts, and social and scientific value and is inextricably linked to the hierarchical goal structure of the organization. The research evaluation system shown schematically in Figure 5.6 can be used either in-process or *ex post*.

Bell (1992) points out that science has excellent protection devices in the form of peer review and scientific method and that these work if not abused. To achieve total quality management, research managers need a research evaluation system that is tied to research portfolio analysis. In-process evaluation of research projects provides information about congruence of intended and realized goals (Russell, 1983). Such in-process evaluations are necessary for guiding the adjustment of objectives and management strategies at the portfolio level of analysis.

Research evaluations conducted in the absence of explicit goals have weak outcomes. An exception is found in evaluations that include peer review. Enlightened, unbiased peer review may be used to determine the value of contributions to scientific knowledge.

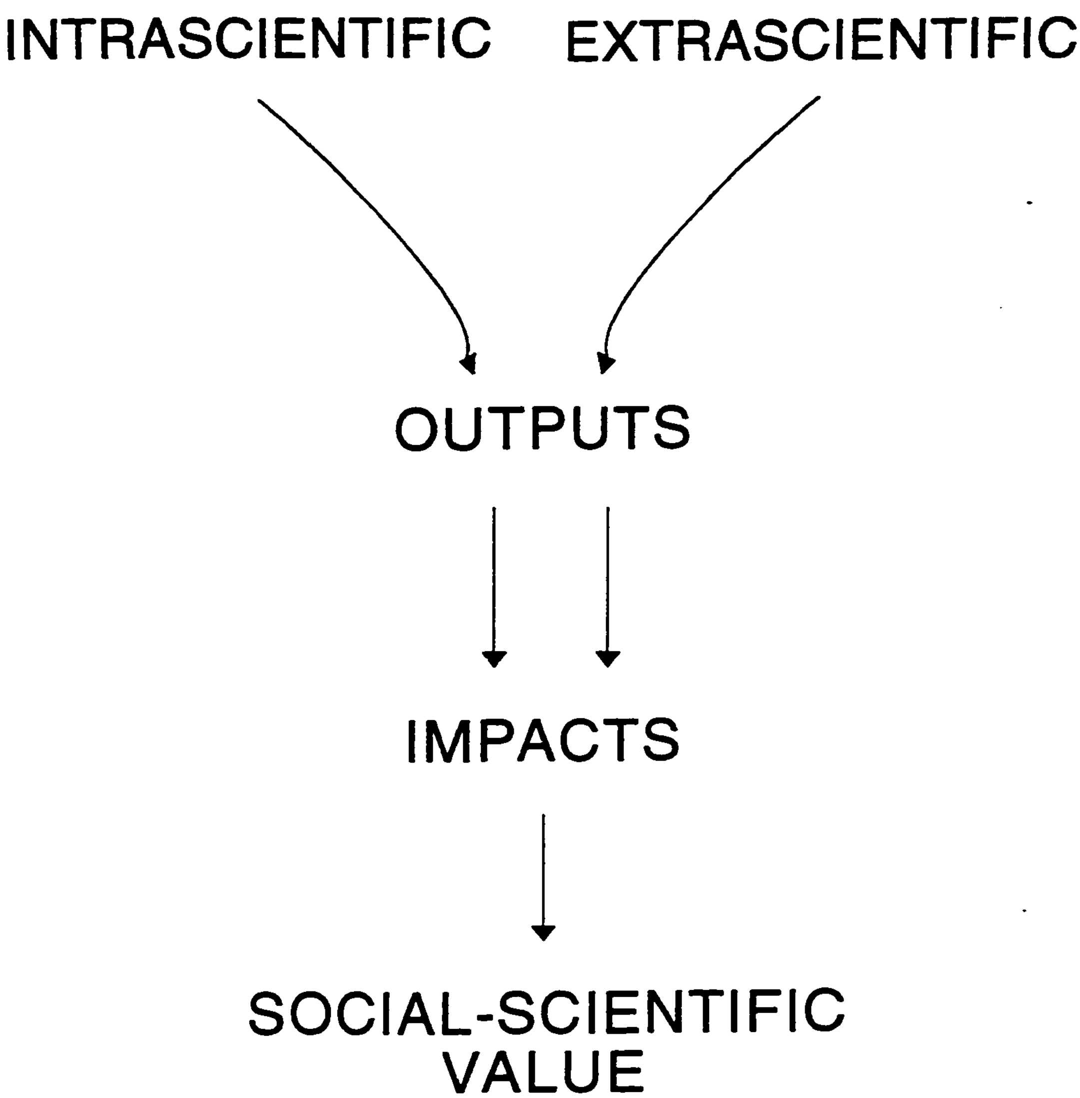


Figure 5.6. Model for the evaluation of scientific research.

The evaluation of the EPA laboratories discussed in Chapters III and IV focused on the intrascientific outputs and impacts of 14 research groups. A comprehensive evaluation would include all six cells of the evaluation matrix, depending on the laboratory and project goals.

In applying the portfolio approach to managing research, the research type issue (character of research) is viewed as a multidimensional array of various scientific activities. Any evaluation model must recognize and treat the differences in the character of research.

The results of research evaluations can be used in strategic planning to identify organizational strengths and weaknesses and collaboration opportunities. Portfolio analysis and management adjustments to the portfolio should also be based on evaluation results.

Research performance should reflect the accretion of knowledge that occurs throughout the research process. This potential knowledge accretion is depicted in Figure 5.7. Generating a product at each stage in the research process and subjecting each product to peer review enhances scientific cogency and ultimately research performance (Leary, 1991). Figure 5.7 represents another proposition, the fifth cornerstone, in the development of a theory underlying research management. A research problem is first identified and framed by discussions between researchers and users and then explored through a literature review. Problem analysis serves to further formulate the research

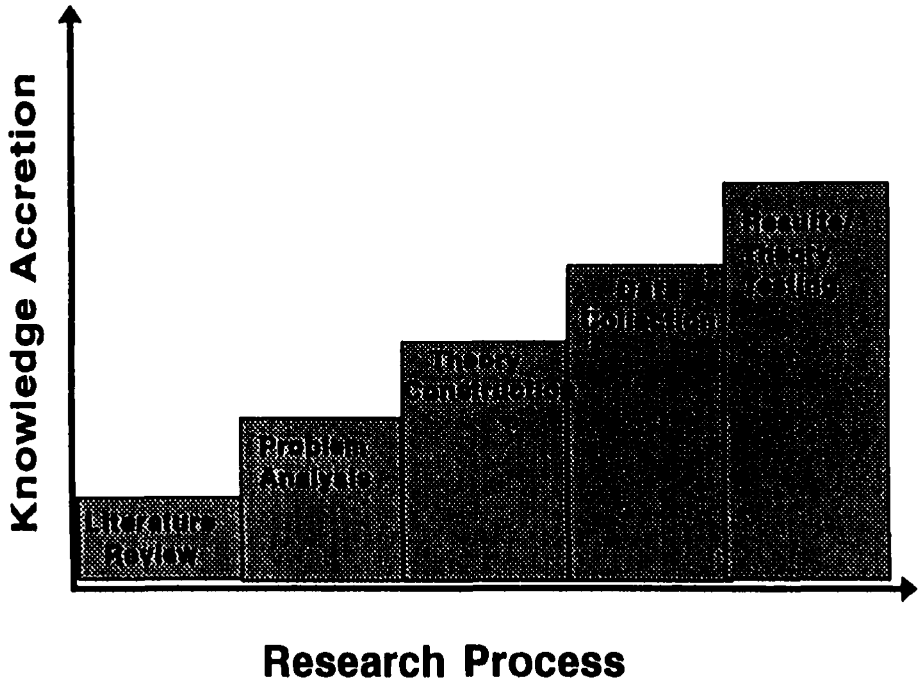


Figure 5.7. Accretion of knowledge during the research process.

problem and examine alternative ways of solving it. Theory construction is, or should be, hierarchical. Scientific concepts give birth to propositions, which in turn lead to theories (Leary, 1991). According to Hilpinen (1989), the accumulation theory of cognitive progress says that truth is regarded as an aggregate of true propositions and progress occurs when one more truth is included in the belief system or when false propositions are replaced by true propositions. Knowledge continues to build inductively through the data collection phase of research and deductively through the theory testing phase. Finally, additional knowledge is added through interpretation of results.

Knowledge accretion is a critical proposition within the theory construct for research management and has important implications for research evaluation.

MANAGING RESEARCH PORTFOLIOS

The public policy problem with regard to research funding is (1) to choose a portfolio of research programs and projects that track under- or over-investment and (2) to be able to change that portfolio as conditions change and more information is gained. But science and technology bureaucracies, including universities, do not have the intelligence systems, administrative mechanisms, and incentives to address R&D as a portfolio problem. For them the normal mode of operation remains aggregate budget maximization.

Traditionally, research organizations have always demanded new resources. Their ability to undertake anything new depends on receiving new inputs, since they are unable to reallocate the resources they have. An objective of generating a reasonable portfolio of research activities would be an improvement over the conventional, fixed-ratio means of allocating resources (Averch, 1985). Research managers must be concerned with 1) the value of research relative to budgetary support, 2) the value of different lines of research, and 3) setting future directions for research programs or organizations.

An Investment Approach to Managing Research

Research is often referred to as an investment (Huddy, 1979; Callaham, 1981; Averch, 1985; Martin and Irvine, 1989; Lee, 1993; National Academy of Sciences, 1993, p.143; Armstrong, 1994; National Academy of Sciences, 1995; National Science and Technology Council, 1995; Skolnikoff, 1995; McGeary and Smith, 1996). Averch (1985), Khorramshahgol and Gousty (1986), Chubin (1994), Cozzens (1994), Greenwood (1994), the National Academy of Sciences (1995), McGeary and Smith (1996) and Van Haveren (1996) have applied the concept of investment portfolios in discussing scientific research goals and activities. Kai Lee, in his insightful book *Compass and Gyroscope*, defines investments as ways of putting time, energy, and other humanly controllable resources to work. He claims that successful investments earn a positive return--a stream of benefits that over time more than repays the resources put in. Different investments yield returns in different ways and investors make choices according to the rate of return of alternative investments and their riskiness (Lee, 1993). Should and can we apply an investment concept to the management of natural resources and environmental research? In public forest research we make investment decisions whenever we initiate, expand, or close research projects (Jakes and Risbrudt, 1988).

The U.S. as a whole is thought to underinvest in research because it spends only 1.9 percent of its GDP on research, as compared to Germany (2.5%) and Japan (3%). According to Skolnikoff (1995), the current Administration has set a target of approximately 3 percent of GDP for the total national R&D investment (public and private). The FY93 total national investment in R&D was calculated to be 2.6 percent, of which 1.1 percent was government funding. The very large contributions of R&D to macroeconomic growth and productivity suggest that R&D is a very good social investment relative to other social investments (Averch, 1985, p. 38; Griliches, 1987). According to Griliches (1987), basic research has stronger effects on productivity growth and federally-financed R&D does not have as large an effect on productivity growth as that of privately financed R&D. The current administration views economics as an essential part of national security and considers R&D a form of investment linked to economic health (Fallows, 1996).

The strategy used to date for setting the overall level of resources for R&D assumes that an underinvestment exists relative to some optimum level of investment (Averch, 1985). An underinvestment theory of research funding suggests that more resources will always be required for research. The theory holds because market incentives for research, especially basic research, are inherently weak. Another view is that the policy problem is more one of avoiding a maldistribution of resources as opposed to not having enough resources.

The resource strategy used by the scientific community implies that federal research funds are a necessary and legitimate low-cost investment in the future, relative to public investments designed to influence the present (Averch, 1985, p.9). However, investments with known returns and immediate results will usually be more attractive than the prospect of high, but uncertain, returns sometime in the future. This is a fundamental public policy problem facing all governments in the future and should suggest to the research administrator that research outputs and outcomes be expressed in terms of societal value.

Callaham (1981), in discussing criteria for choosing among forestry research programs, considered scientific research to be a form of capital investment necessary for economic health. The link between investment in research and growth of productivity was repeatedly demonstrated in the agricultural, forestry, and forest products sectors and in various industrial sectors (Bengston, 1989). Although forestry R&D investment in the U.S. amounts to less than 0.2 percent of the total value of timber-based products and services (Callaham, 1981), forest management research was shown to return between 9 and 111 percent (Jakes and Risbrudt, 1988). Research on wildlife and fisheries constitutes only about 0.2 percent of sportsmen's expenditures for hunting and freshwater fishing and wildlife research investment is about 6 percent of all national expenditures for wildlife management. Timber management research equals about 6 percent of the national expenditures on the management of timber resources on all forest lands. In fire research the level of research investment is about 2 percent (Callaham, 1981).

The public policy and research management challenge with regard to research funding is to 1) choose a portfolio of research programs and projects that track under- or over-investment and 2) be able to change that portfolio as conditions change and more information is gained. However, according to Averch (1985), science bureaucracies do not have the intelligence systems, administrative systems, or the incentives to address R&D as a portfolio problem. For most government science agencies the normal mode of operation remains total budget maximization. According to my observations and experience, agencies are always demanding new resources. Their ability to take on new projects depends on receiving new funding since they are unable to reallocate the resources they have. Averch (1985) suggests that an objective of generating a reasonable portfolio of research activities would be an improvement over the conventional, fixed-ratio means of allocating resources.

McGeary and Smith (1996) propose borrowing the investment portfolio concept from financial investment theory and applying it to decision-making on the allocation of funds for science and technology. They suggest that allocating research funding is analogous to financial investing. Within an atmosphere of unpredictability, diversification of the research portfolio is critical for maximizing overall results and minimizing risks. Where uncertainty is high, which is the case with post-normal science and fundamental research questions, a broad portfolio is favored. Healthy portfolios will have a mix of research types.

Frame (1988) defines the portfolio as a collection of projects, either interrelated or independent of each other, that must be co-managed under a single management umbrella. Projects may be undertaken by individual scientists or by research groups. The resource strategy used by the scientific community implies that federal research funds are a necessary and legitimate low-cost investment in the future, relative to public investments designed to influence the present. However, investments with known returns and results will usually be more attractive than the prospect of high, but uncertain, returns sometime in the future (Averch, 1985).

From the viewpoint of the research sponsor, the ideal area for research investment is a high degree of past success plus a great potential for future success at a low cost. Research managers must be concerned specifically with 1) the value of research relative to budgetary support, 2) the present and future value of different lines of research, and 3) setting future directions for research programs or organizations. As in financial investing, diversification might be used as a strategy to manage risk within the research portfolio.

The dimensions of a research portfolio are similar to asset categories in financial investment strategies. Research managers must ask asset allocation questions such as: How should research resources be apportioned among different areas of inquiry? Between high-risk exploratory and mission-related research? Between the development and diffusion of new knowledge? What are the appropriate investment portfolio dimensions? What constitutes acceptable levels of return-on-investment and riskiness for natural resources and environmental research?

An investment approach to research incorporates the concept of information value (David, 1994). Thus, research evaluations should include an assessment of the value of information produced by research projects. In looking at how we benefit from research, we should be pragmatic and not ideological. The debate should be over the relative importance of research outcomes, rather than a debate about methods (e.g. basic vs. applied research vs. strategic research). GPRA intends that we focus on results not on inputs.

Thus, there are precedents for applying an investment portfolio approach to planning and managing natural resources and environmental research. However, in my many visits to research organizations across Europe and the U.S. I observed very few research administrators applying the investment concept. The creation of NSTC and the passing of GPRA has set in motion powerful forces that will profoundly influence the priority setting, management, evaluation, and funding of federal R&D programs in the future (Gunderson and Rodriguez, 1994). Managers must have a theoretical basis and tools necessary to determine whether investments in research programs should be given lesser or greater priority for future funding.

We currently do not have the quantitative, scientific base for advising whether the current level of investments in science is too large or too small, or discerning whether the returns to such investments have declined or increased over time and for what types of investments. The natural resources and environmental management communities have never debated, in any systematic and comprehensive manner, the appropriate levels of research investment. Clearly, institutional changes must be made before the science community can adapt the investment approach.

Portfolio theory as applied to scientific research ensures diversity among research performers and research types and represents a good strategy for allocating resources, for meeting stakeholder conditions, and for dropping obsolete programs. It also allows for cross-investigation comparisons, identification and consideration of emerging issues, and investment in longer-term activities.

Treating research activities as an investment becomes the sixth cornerstone in developing an underlying theory of research management. The investment approach will require additional work, both in terms of theoretical development and quantitative tools.

Research portfolio analysis begins with a consideration of the existing strengths of the organization--essentially the right skills in sufficient numbers of scientists and technicians. This is the internal dimension of the portfolio. Research projects or themes are also judged to be either proven past performers or promising new opportunities. This is the external dimension, because research potential must be viewed from a client or sponsor perspective from outside the organization. Orthogonal portrayal of these two dimensions results in the four quadrants depicted in Figure 5.8. These quadrants are labeled, in counter-clockwise fashion, as the α (alpha), β (beta), ω (omega), and δ (delta) quadrants.

The α quadrant is home to projects/themes which are backed by substantial organizational strengths and which represent promising new research directions, in other words the future superstars of the portfolio.

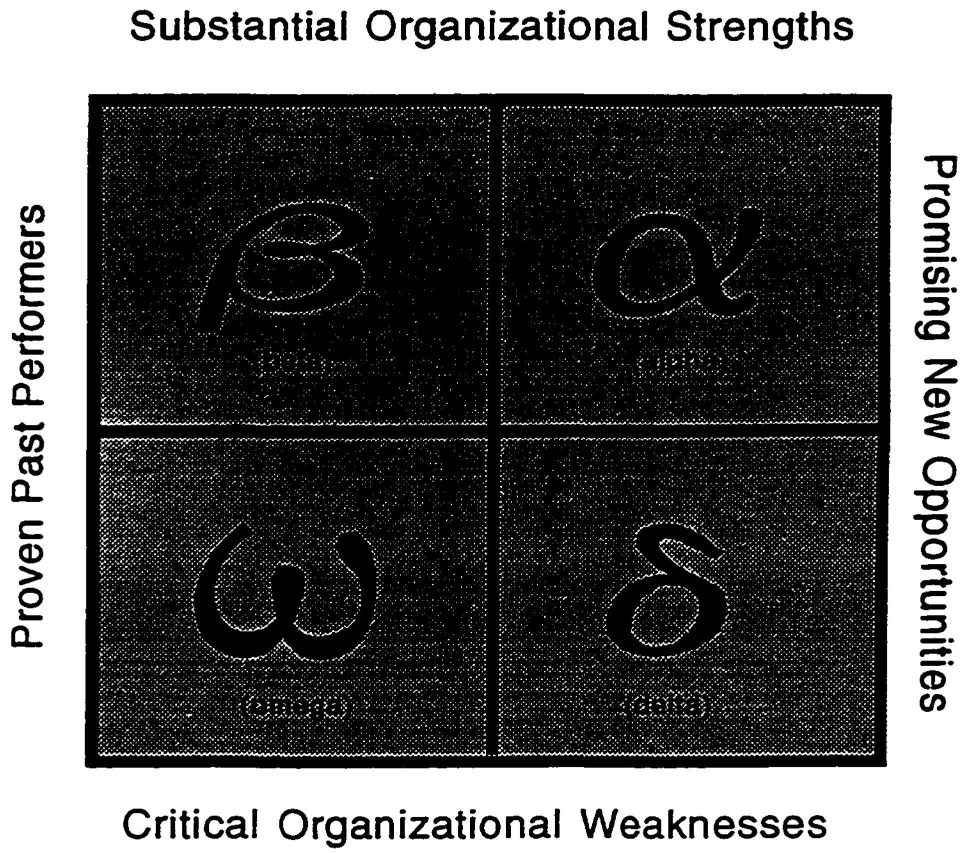


Figure 5.8. Schematic for research portfolio analysis.

Project or themes in the β quadrant are the cash cows. They enjoy solid reputations as proven performers. However, success does not last forever and β-quadrant projects will eventually reach maturity and begin to decline in favor, if not in resources. The ω quadrant houses projects at the end of their productive life. These are research efforts which need to be terminated. They are maintained in the portfolio pending the completion of final project reports, publications, and other information transfer activities.

Substantial management involvement is required in the δ quadrant. These are projects that require strategic changes because of critical organizational weaknesses and because they represent promising new research opportunities. Another way to view a research portfolio is to think of the four quadrants representing a life cycle of research initiatives. As depicted in Figure 5.9, research initiatives tend to begin in either the δ or α quadrants, progress over time into the β quadrant, and eventually end up in the ω quadrant. Resources have to be allocated to all four quadrants. These decisions are tied to the strength/weakness/opportunity/threat analysis and portfolio matrix concepts in strategic planning. And this also translates into an essential tension-- keeping and renewing projects that are proven performers and yet sponsoring promising new initiatives. Since the reward systems in innovating organizations are more investment-oriented or future-oriented than past-oriented, ω-quadrant projects should be terminated once they have stopped producing useful research products.

A real life example of strategic planning and portfolio management was found in the "General Operating Principles" used by the National Biological Service at its inception as

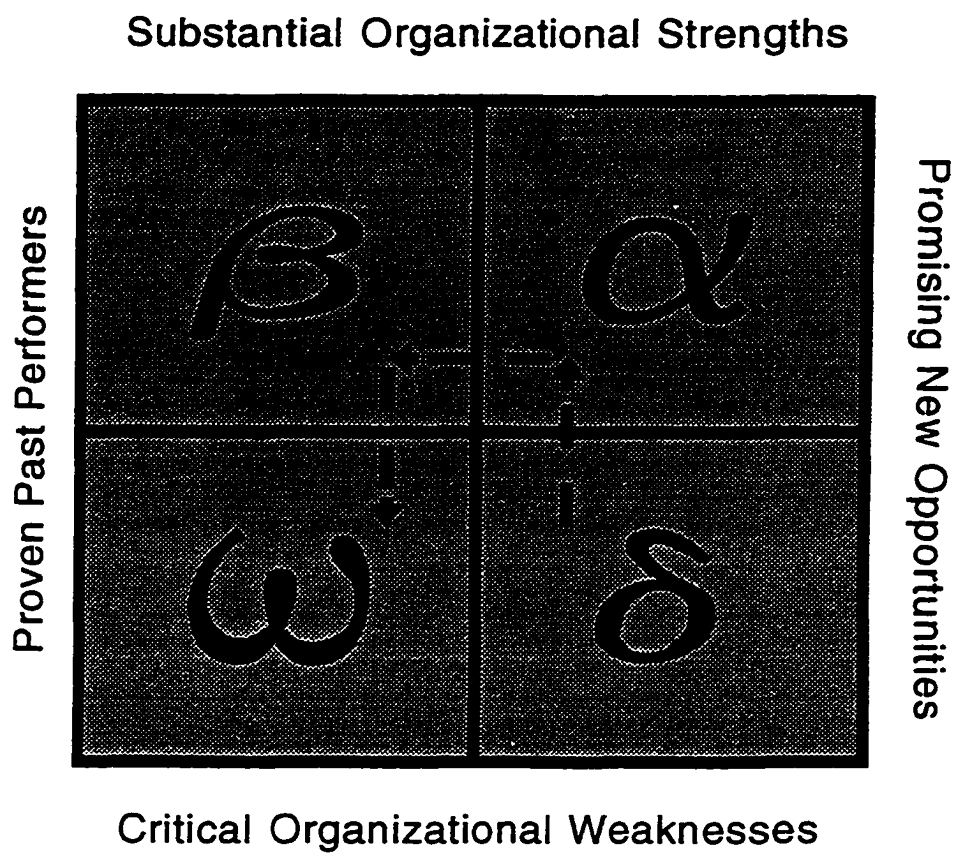


Figure 5.9. Research portfolio analysis: evolution of research initiatives.

a new Interior agency in 1995. Those principles, which could have served as the basis and direction for research portfolios at the national, regional and center levels of research activity, included the following (Pulliam, 1995):

- Scientific excellence
- Relevance to management/policy needs
- Integrated ecological systems perspective
- Balance of tactical and strategic activities
- Partnerships

This was an excellent example of strategic planning at the agency level. The NBS did not survive Congressional resistance to its creation and late in 1996 it was merged into the U.S. Geological Survey.

Research Niches and the α Quadrant

Keidel (1995) defines differentiation as the quality of being special. In the research business, differentiation is achieved by staying at the cutting edge of a specific research area and by building and maintaining new research niches. Differentiation also means establishing world-renowned scientists and publishing state-of-the-art papers in high-profile journals. Although the purpose of niche-building is to promote scientific leadership and attract new resources, niche-building does not rule out collaboration with other research performers. Both strategies, differentiation and collaboration, may be used simultaneously.

The decision to establish a research niche involves two considerations. The new research direction must be attractive to stakeholders and also must have a high potential to be successful. A scheme for niche analysis is shown in Figure 5.10. For example, the Forest Service has maintained a niche in forest ecosystem research by participating in the National Science Foundation's International Biological Program (IBP) and the National Science Foundation's Long-Term Ecological Research (LTER) program. The H.J. Andrews, Hubbard Brook, and Coweeta experimental sites are all examples of this research niche. These long-term studies are attractive to stakeholders and have proven to be successful research endeavors over the past thirty or more years. Another example of a successful research niche is the EPA focusing on Great Lakes harbor sediment problems. This research program ranks high for both niche criteria in Figure 5.10. Both the EPA and USGS-Biological Resources Division intend to pursue research on endocrine disruptors. This is both an example of a new research niche (Figure 5.10) and offers the potential for collaboration on an important new research topic.

In the early 1990's the Forest Service made the decision to create a new boreal forest research initiative. An international boreal forest research association was created in cooperation with other countries and several boreal forest research conferences have been held. However, this does not appear to have the potential to be a successful research niche for the Forest Service. There is little stakeholder support and the Forest Service recently scaled back its scientific resources in Fairbanks. Consequently, a niche analysis shows this initiative to have a low ranking (Figure 5.10).

New initiatives that have the potential to occupy important research niches are often

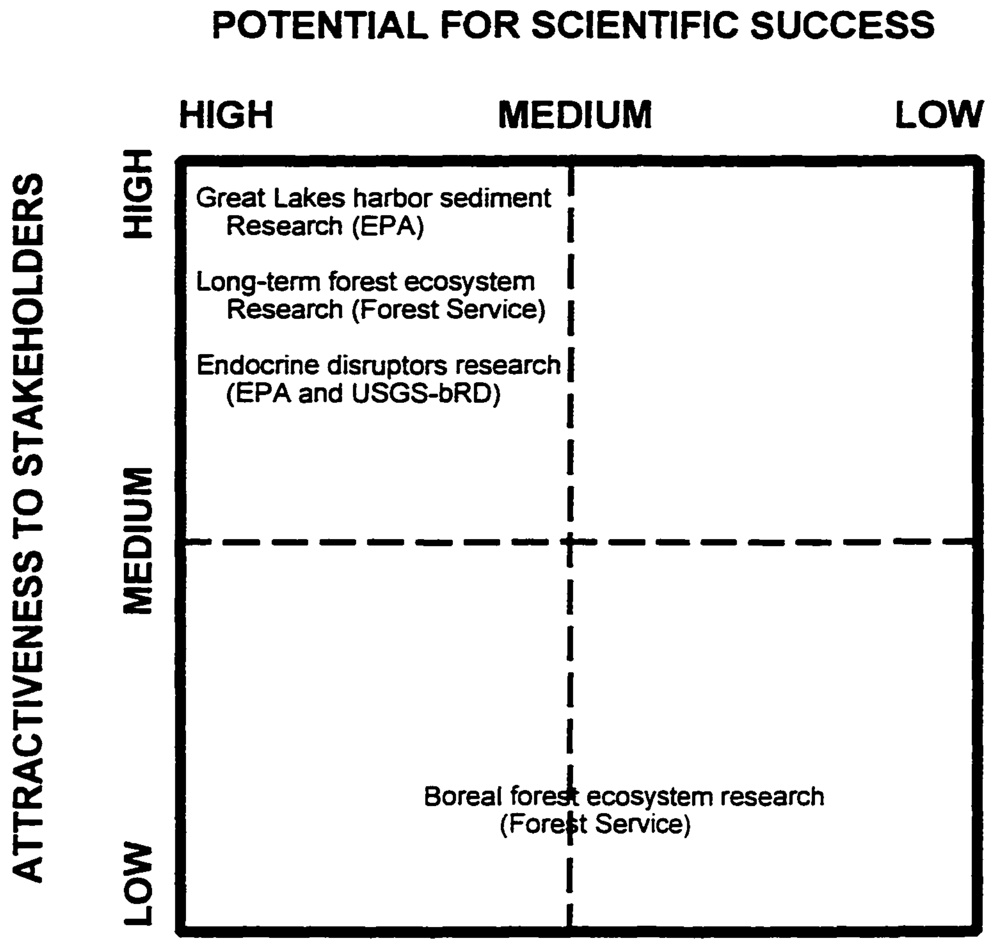


Figure 5.10. Research niche analysis.

spawned from strategic planning which defines opportunities and important new directions in which to go (Rip, 1994; Pacific Northwest Research Station, 1997). Opportunities for new niches in natural resources and environmental research include 1) improving methods for documenting ecological change; 2) improving the understanding of natural processes and their interactions with human activities at a variety of scales; 3) increasing the capacity to predict consequences of change; and 4) and increasing the range of potential solutions by improving partnerships and communications among natural and social scientists, managers, policymakers, and other stakeholders (Scavia et al., 1996).

A Strategy for Managing δ-Quadrant Projects

A management strategy is required to migrate δ-quadrant projects to the α quadrant. A scheme for developing a management strategy is shown in Figure 5.11. The strategy is based on Keidel's organizational design triad (control/cooperation/autonomy) and emphasizes an appropriate mix of resource reallocation, research collaboration, and differentiation or niche-building. Sample strategies are shown in Figures 5.12a and 5.12b. Management techniques to implement the strategy are shown in Figure 5.13.

In Figure 5.12a the strategy chosen emphasizes collaboration. In employing collaboration as a strategy, management may choose from a variety of techniques shown in Figure 5.13, including boundary spanning, network research, on-site research teams, and collaboratories.

In the case of Figure 5.12b, the management strategy emphasizes both differentiation and resource reallocation. To achieve differentiation, management may increase the visibility

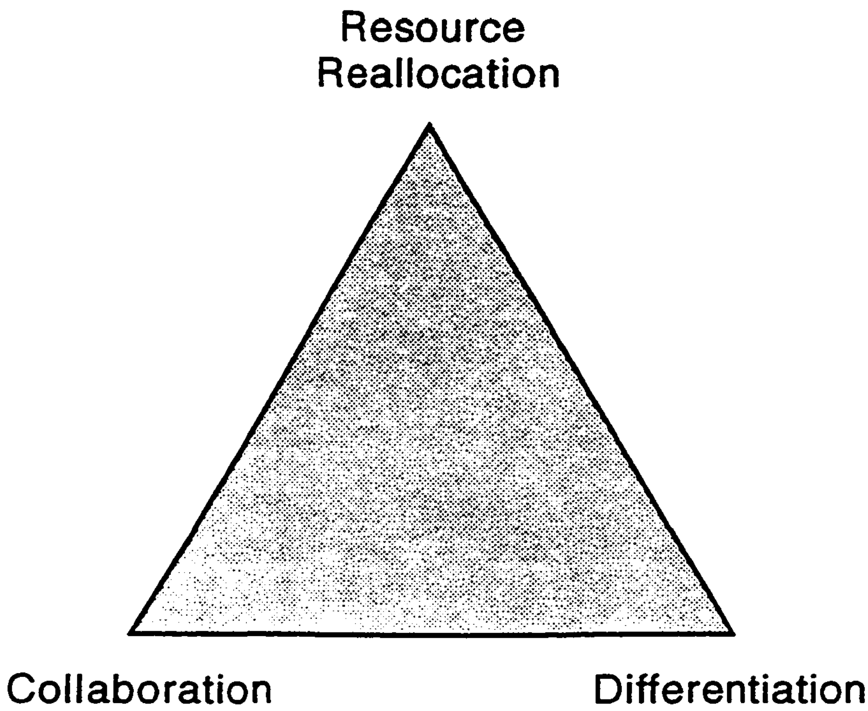


Figure 5.11. Triadic strategy for managing δ-quadrant research (based on Keidel, 1995).

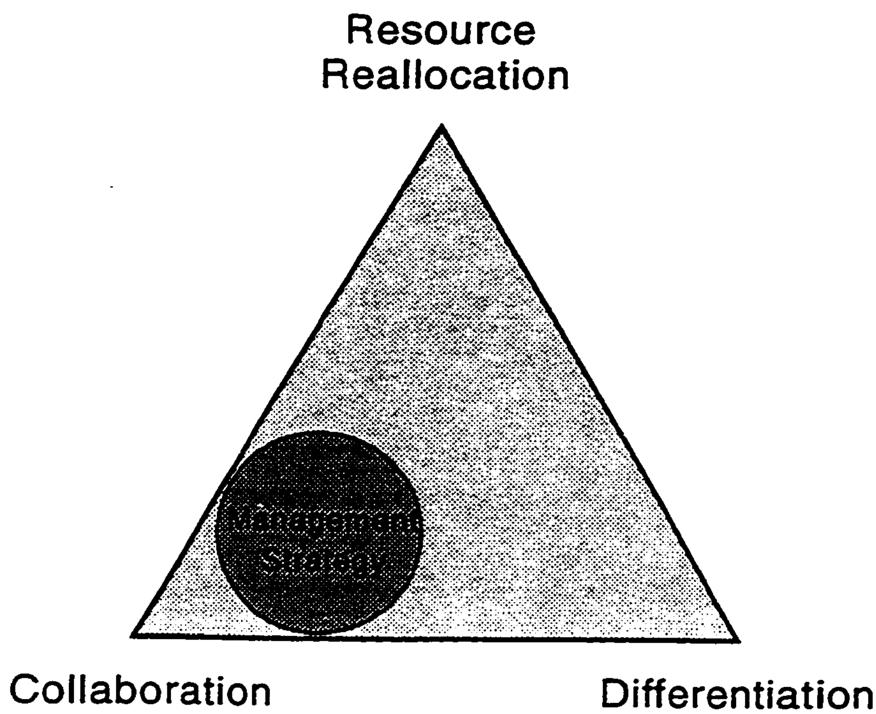


Figure 5.12a. Triadic strategy for managing δ-quadrant research (bias toward collaboration).

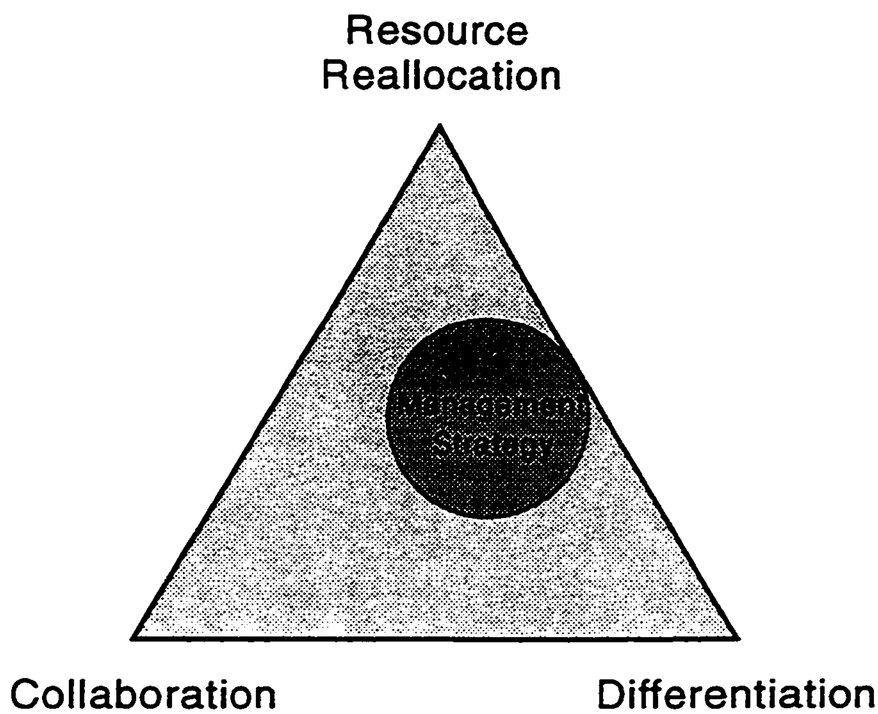


Figure 5.12b. Triadic strategy for managing δ-quadrant research (bias toward differentiation and resource reallocation).

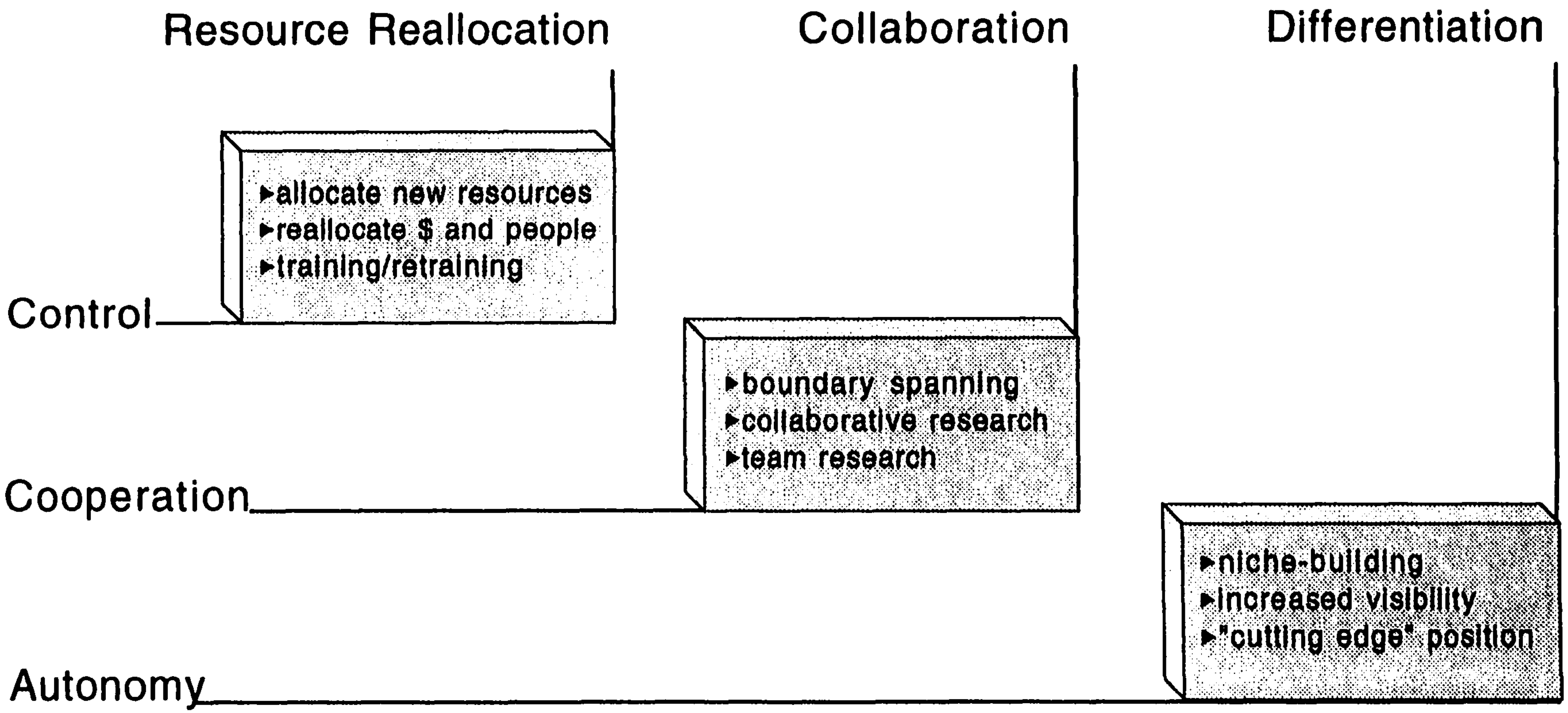


Figure 5.13. Research portfolio management strategy.

of key programs and projects, work towards establishing a "cutting-edge" positions for research programs having considerable potential, or create a new research niche. Differentiation techniques may be used in concert with resource reallocation methods, including the acquisition of new resources (scientists and/or dollars), reallocating people or funding to target programs, or training/retraining existing personnel.

Drucker (1995) suggest that governmental organizations need to rethink, which means to identify the activities that are productive, that should be strengthened, promoted, and expanded. The same holds true for portfolio re-shaping. Kanter (1983) writes of "change masters," those who know which pieces of the past to preserve while moving toward a different future--keeping and renewing proven performers and yet sponsoring promising new initiatives. An effective research manager has to become a change master.

In modifying research portfolios, managers need to be aware of the consequences of providing increased resources to projects or activities. More support may lead to greater returns, but may also lead to diminishing returns or no net change in research productivity or effectiveness. Projects in the α or δ quadrants are likely to respond with greater returns. However, projects in the β quadrant may respond with diminishing returns and activities in the ω quadrant would likely yield little or no returns in response to increased resources.

Other Management Adjustments to the Research Portfolio

Portfolio diversity combined with a flexible organization provides opportunities for cross-fertilization of research ideas and outcomes. The whole of the portfolio can become greater that the sum of its individual parts through integration and synergy. This is the responsibility of an integrator. The integrator works across programs and projects within the portfolio to integrate the results of several projects and extract new information synergistically.

Accountability and Portfolio Shaping

Portfolios vary with level of analysis. An agency research director has a set of goals that differ from that of an experiment station or laboratory director. A project manager has an entirely different set of goals that he or she is accountable for. The dimensions of the portfolios should be roughly the same throughout an organization since they reflect stakeholder expectations. However, the shape and content of the portfolio would vary with level of analysis and the demands of the stakeholders at each level.

The LOS Center (Norwegian Research Center in Organization and Management) in Bergen, Norway, engages in research on organization and management to help Norway achieve a better organized public sector and improve the interplay between the public and private sectors. LOS supports an active and dynamic research portfolio centered around studies on reform and change. To guarantee results and accountability, staff scientists work under five-year contracts. They must produce or their contracts are not renewed.

Chubin (1994) suggested that we need both accountability and portfolio shaping. Creative portfolio shaping in response to stakeholder needs should not be unduly hampered by demands for greater accountability. There should be room in the research portfolio for high-risk, high-gain investments.

A portfolio approach to research management does not require radical changes in the structure or procedures of R&D decisions, but does require a different management perspective and a long-term strategy.

RESEARCH PLANNING

The biggest challenge of long-range planning is anticipating future resource and environmental issues, problems, and regulatory needs. Anticipatory research is designed to lay the groundwork for future studies, to identify emerging problems, and to prevent them from becoming bigger and intractable. Anticipating future problems allows more time for prevention and mitigation and would, in theory, reduce the total cost of addressing problems. Such research should be protected from short-term budget pressures. Federal R&D programs must be organized and operated in such a way that balance is achieved between the independence-continuity force and the mission-policy-relevance force. To achieve this balance may require the creation of an organizational buffer that directs and modifies the flow of information from the scientist to the policymaker and vice-versa. An initial blueprint for coordinating Federal environmental and natural resources research at the national level was provided by CENR (National Science and Technology Council, 1995).

Any research planning strategy must begin with a strong linkage, built on mutual understanding and respect, between the separate estates of research and management. The understanding begins with a client orientation on the part of research and an investment orientation on the part of management (Figure 5.14). The other key links in the research-management linkage are collaborative research planning, research performance evaluations that involve the clients of research, and interim products of research representing the stages of knowledge accretion.

The Great Lakes Research Strategy, depicted in Figure 5.15, is an excellent model of regional research planning. Although not shown in Figure 5.15, the planning model includes Canadian research organizations. The model includes the potential for unique research niches as well as many opportunities for collaborative research on habitats, toxics, or species protection.

FINAL THOUGHTS ON RESEARCH EFFECTIVENESS

In spite of the fact that organizational effectiveness is so complex and difficult to deal with, effective strategies must be developed to make research more responsive and useful to sponsors and stakeholders. Research effectiveness in the future is expected to concentrate on four primary organizational values: human resources, external focus, internal process, and improved goal setting and measuring.

Effectiveness in terms of human resources will be defined by increasing workforce diversity, successful replacement of senior scientist capability, and adjustment to or accommodation of changing worker values and lifestyles. Research organizations will

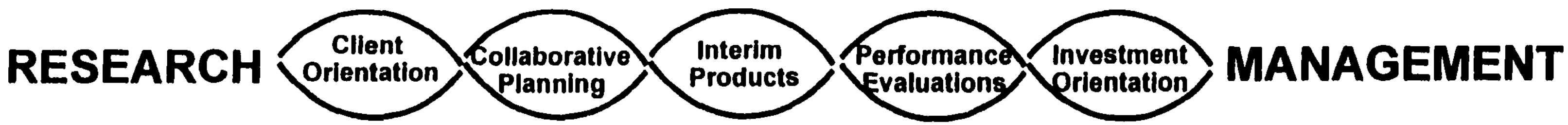
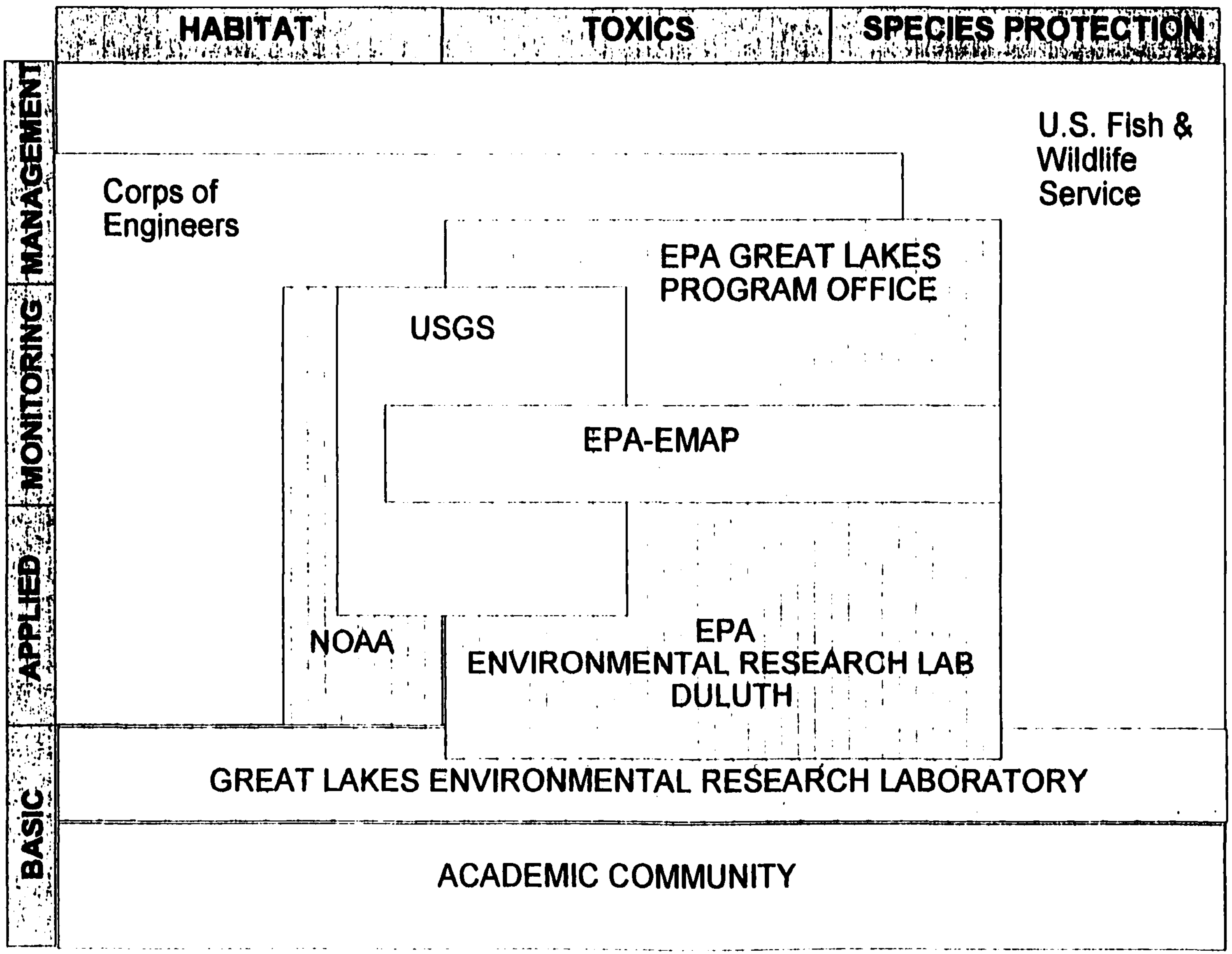


Figure 5.14. Research-management linkage, from Van Haveren (1996).

Figure 5.15. Environmental research strategy for the Great Lakes (U.S. agencies only), based on U.S. Environmental Protection Agency (1992).


HABITAT
TOXICS
SPECIES PROTECTION
MANAGEMENT
MONITORING
APPLIED
BASIC
U.S. Fish & Wildlife Service
Corps of Engineers
EPA GREAT LAKES PROGRAM OFFICE
USGS
EPA-EMAP
NOAA
EPA ENVIRONMENTAL RESEARCH LAB DULUTH
GREAT LAKES ENVIRONMENTAL RESEARCH LABORATORY
ACADEMIC COMMUNITY

effectiveness and goals are inextricably linked. Management has the responsibility to construct a hierarchy of goals and communicate those goals to ensure that all elements of the organization are contributing to its mission. Equally important, information systems must be devised to objectively measure the organization's progress toward meeting a complex array of goals.

Research organizations must move closer to their customers, improve accountability, and interact more with their external environment. It is impossible to ignore research effectiveness. I return to the basic definition of effectiveness: The degree to which organizational goals are met. Goals are a key component of an organizational strategy and give the strategy an enormous leverage (Keidel, 1995). Since goals vary within and outside the organization, the goals of the organization must reflect the values of all the stakeholders. The result will be an array of goals that resemble the competing values model of effectiveness. Goals must be structured in a hierarchical fashion, increasing in detail from the research center director to the bench scientists and technicians. Goal clarity and agreement across the organization and goal congruence within research groups are essential prerequisites to achieving research effectiveness.

Questions of efficiency and effectiveness arise in discussions of the relative merits of centralized vs. pluralistic models of research organization. The concepts are not the same. The centralized model, where all similar research is housed under one institute, may be more efficient in terms of deriving outputs for the least amount of input but is it more effective than the pluralistic model where similar research is spread across several institutions, thereby assuring different approaches and a sense of competition among research groups? The National Academy of Sciences (1995) concluded that the pluralistic approach is a great source of strength in science and that it enhances quality and the national capacity to respond to new opportunities and changing national needs. The trend towards greater pluralization is seen as a source of strength in U.S. science and technology policy (Smith, 1973).

Gibbons et al. (1994) define pluralization of the research system as moving towards 1) socially-distributed knowledge, 2) new types of institutions, and 3) new patterns of formal and informal international cooperation. Pluralization in research means cooperation in the broadest sense: team approaches to research, multicultural organizations, boundary spanning and collaboration with outside organizations, and integration of results across projects within a research portfolio.

Van Raan (1997) cites an example of increasing international collaboration as a strategy for countries and research groups to become more visible internationally. As part of this strategy, groups and individuals seek to publish their results in high-impact journals and to co-publish papers with colleagues in other countries. This is yet another sign of the trend towards more pluralistic science.

The challenge to research administrators is to accomplish organizational changes without sacrificing intellectual freedom and autonomy, not in relation to problem choice, but in terms of research approach and management methods. In terms of Keidel's scheme, this means moving the organization in the direction of greater control (goal-setting and performance measurement) and cooperation (collaboration with external partners and interdisciplinary team approaches). For research directors, this translates into a focus on strategic goals for the organization as a whole. The outcome of strategic planning and portfolio management should be less variation and an overall improvement in research effectiveness (Figure 5.16).

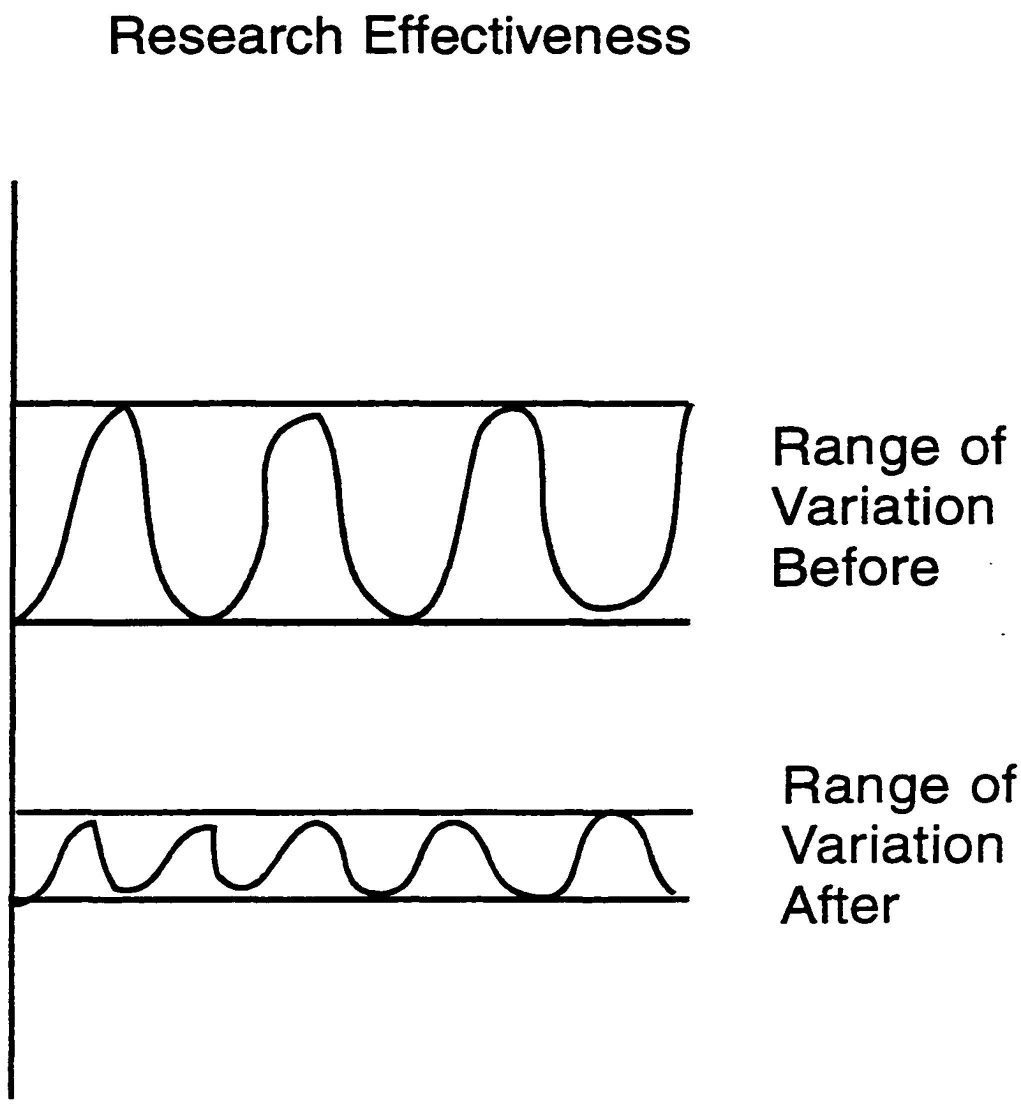


Figure 5.16. Research effectiveness before and after improved management.

CHAPTER VI

CONCLUSIONS

The organization and management of natural resources and environmental research has not received much attention in the literature. Unfortunately, no one to date has undertaken the task of building a theoretical foundation or a status-of-knowledge summary for research administration, particularly with regard to natural resources and environmental research. One of the purposes of this dissertation was to present a set of fundamental principles for organizing and managing natural resources and environmental research.

ESSENTIAL TENSIONS IN RESEARCH ADMINISTRATION

The literature on science policy and research administration suggests that a number of tensions are inherent in managing scientific research. The tensions manifest themselves in terms of pairs of competing demands in science, such as the need for both convergent and divergent thinking in scientific research, and were considered to be “essential tensions” by Kuhn (1977). A large number of essential tensions exist in natural resources and environmental research. These tensions suggest a multidimensionality of research (Cortner et al., 1996; Van Haveren, 1996) and actually provide increased opportunities for the research administrator to broaden the organization’s research portfolio. The administrator's challenge is to establish a dynamic balance between competing scientific endeavors. These judgements, made among essential tensions, are the very essence of research administration.

A MODEL FOR EVALUATING RESEARCH PERFORMANCE

The issue of relevancy of research, as well as metrics of accomplishments, is becoming more important for everyone in the science community. Because of the Government Performance and Results Act of 1993, all federal research organizations in the U.S. must get aggressive about performance tracking. The results of this study indicate the need for a two-pronged approach to research evaluation. Intrascientific outputs are those products from research that benefit the science community primarily. An example would be research results that advance the frontier of knowledge in a given scientific area. Extra-scientific outputs are those research activities that benefit on-the-ground resource management operations, provide information to environmental decision makers or policy analysts, or develop tools that improve resource or environmental management.

The evaluation model appears valid for natural resources and environmental research organizations, but quantitative performance data gathering, such as bibliometric indicators and peer assessment, must be improved to strengthen the intrascientific side of the model. Research directors need to concentrate on being objective sensors of both intrascientific and extrascientific information, collecting quantitative data on research performance, including outputs and outcomes, and actively seeking feedback from clients.

Based on the experience with this study, research assessment is best conducted at the individual scientist and group or project level and then aggregated up to the laboratory, research program, and higher levels.

Research assessment criteria vary according to research goals. As part of a goal-setting process, managers and scientists determine how research is to be evaluated. Evaluation criteria would be based on the research organization's core activities and processes. For example, long-term discovery research, regulatory-relevant developmental research, problem-solving, technical assistance, and training involve different evaluation criteria.

Group scientific performance in this study was assessed using a quantitative index of scientific performance composed of publication productivity (publication counts over a five-year period) and publication quality (both citation performance and journal impact). The exact construct of the index may be varied for evaluation purposes, depending on management goals for publication quality.

GROUP RESEARCH PERFORMANCE AND CONTEXTUAL VARIABLES

An analysis of variance in the scientific performance of the 14 groups showed that only 48% of the variance in group performance was explained by a combination of organizational climate within the group, extent of communication among group members, group cohesiveness, degree of collaboration among group members, degree of interdisciplinarity of group, client-centeredness of group members, and commitment to group goals. The majority of the variance in group performance was due to the individual performance of one or two scientists within the group. The five highest-performing groups each had one scientist who was clearly setting the pace for the rest of the group in terms of both publication productivity and quality.

PERCEIVED VS. MEASURED SCIENTIFIC PERFORMANCE

Perceived performance did not correlate well with measured performance in the research groups studied, suggesting that objective measures of performance be developed and used by research organizations whenever possible. If perceived performance is a part of research evaluation, perceptions of performance must be specific as to performance criteria. For the research groups studied, member-perceived publication quality was not well-correlated with measured publication quality.

Director-perceived scientific performance of the research groups did not correlate well with measured performance. Research goals should be hierarchically structured, expectations clearly communicated, and goals congruent among research administrators, project managers, and scientists. Goal congruence between group leaders and members was high in the case of the research groups studied. However, communication about expectations and performance broke down between laboratory directors and research groups. Both publication quantity and quality expectations should be programmed into performance goals for individual scientists and research groups. Data on achievements, both publication productivity and quality, need to be communicated to research administrators and compared with goals and expectations.

SELF-ORGANIZING GROUPS IN RESEARCH

Based on observations of the research laboratories and groups in this study, group research happens naturally and spontaneously in environmental research settings primarily because of the interdisciplinary nature of environmental research. Groups were not always identifiable in organizational charts. Often they were naturally occurring dyads or clusters of individuals with similar interests or interdependent skills. High-performing groups were found to cluster around one or two high-performing senior scientists.

A formal division and branch structure often hinders group research because of fiefdom-like attitudes of branch chiefs. At one laboratory an informal organizational structure that resulted from management vacancies led to numerous research clusters of common research interests and promising new directions for investigation.

Different disciplines can and do occupy and contribute to the same problem space. Analysis of authorship patterns shows that about 40 percent of the publications generated from the research groups studied were multi-authored (3 or more authors). The multiple perspectives brought by team members are likely to result in a more complete problem analysis, more working hypotheses, and the potential for stronger inference of research results.

Epistemological differences exist within research groups and may present obstacles or result in dysfunctional groups. Research groups often spend considerable time on problem definition and problem analysis and working towards a group goal. In addition, a common system of inquiry should be developed and adopted by the group to prevent epistemological dysfunction.

Interdisciplinary teams have considerable potential in natural resource and environmental research because of the complexity and cross-disciplinary nature of the management issues and research problems.

INTEGRATORS AND INSTIGATORS

High-performing research groups in this study all had one characteristic in common. They were carried by the energy and motivation of one or two individuals who were either integrative or boundary-spanning in their cognitive orientation. I refer to these roles as "integrators" and "instigators," respectively.

Integrators act as facilitators to unify and synthesize information developed by separate research groups or projects. In that sense, integration is a form of internal boundary spanning. Integrator roles are rarely recognized in the titles or position descriptions of the scientists doing the integration. They tend to be individuals who naturally gravitate to that role and have the cognitive ability to undertake the complexities of research integration. They tend to have a wide range of contacts and are at the crossroads of several information streams both within and outside the organization.

Rather than walling off the problem, integrators create mechanisms for the exchange of new information and ideas across suborganizational boundaries. Often, new findings or new lines of inquiry are discovered through integration.

Instigators are very adept at spanning organizational boundaries, breaking out of paradigms, and finding promising new research directions. They may work inside their organization developing new research themes or initiating new projects, or they may spend most of their time working with external groups and organizations on collaborative projects. They kindle ideas and inspire other scientists. They are the entrepreneurs of the research world. At the environmental laboratories housing the research groups in this study, instigators tended to work well with other agencies and groups external to their home laboratory. They are likely to have an integrator orientation as opposed to a reducing or reductionist orientation. Instigators assume a politically-oriented role in organizations. They are people who form a "polis" around the need for change. More than anything else, they keep stirring the pot and adding new ingredients. They incite action in new research directions. They are often successful in bringing outside funds into their laboratories.

Integrator and instigator roles are essential to any research organization. Today's research organizations require both types of individuals in order to successfully carry out their missions. Integrators and instigators see a bigger picture than other scientists. They see problems as wholes. They have developed a broader perspective and acquired good people skills with age and experience.

PRINCIPLES OF RESEARCH ADMINISTRATION

1. *Natural resources and environmental research is inherently multidimensional.* This multdimensionality extends to the research portfolios of individual scientists, projects, laboratories, centers, programs, and agencies.

2. *Research sponsors and administrators should view research using an investment orientation and manage programs and projects using a portfolio management strategy.* Research portfolio management, as part of a research planning strategy, provides a tool for improving the effectiveness of natural resources and environmental research. In the post-reformative period, successful research administrators will be those who construct and manage multidimensional research portfolios in collaboration with their sponsors, manage for both long-term and short-term risks, maximize the returns from the research investment.

3. *Research goal-setting is a hierarchical process that depends on effective communication of expectations.* Goals and expectations must be clearly communicated to group leaders and individual scientists.

4. *Research organizations should have flexible structures that include both integrator and instigator roles.* Organizational structure should be organic in nature, wherein clusters of scientists with mutual interests and skill interdependencies are encouraged to self-organize. Productive groups will organize around key people (high-performers and integrators) if the boundaries between organizational divisions are permeable.

5. *Research evaluation should follow a bi-modal model that considers both intra- and extra-scientific outputs and outcomes.* Communication of performance expectations, goals, and accomplishments is critical to the evaluation process.

6. *Leadership styles must fit the strategic direction of the research organization.* In the past, research leaders were considered to be either *strategists* (a focus on detailed research planning) or *institutionalists* (a focus on building and nurturing the research institution). In today's research organizations, administrators must also become *integrators*, increasing the diversity of research opportunities, building collaboration with other research organizations, and integrating research activities and results to achieve a measure of synergy and greater research effectiveness.

Finally, three organizational design imperatives are suggested for research organizations:

1) The character of the research and the kinds of research activities supported depend on the research organization's environment, including the mission and culture of the parent organization, and the needs of the research sponsors and stakeholders.

2) Research capacity and quality considerations, portfolio dimensions, performance objectives, and a strategy for evaluating research must be included in the design of a research organization.

3) Critical research management processes include goal setting, goal hierarchy and communication, goal congruence, vertical communications for reporting performance, portfolio planning and maintenance, and internal/external boundary spanning (integrator and instigator roles).

FUTURE RESEARCH NEEDS

Further work needs to be done to develop both qualitative and quantitative techniques to assess research effectiveness. Better measures of research outputs and outcomes are needed. This is particularly true of extrascientific outputs and outcomes. The evaluation model should be tested against actual performance data from a range of research organizations and at different levels of analysis. The following hypotheses were derived from this study and should be evaluated in a variety of natural resources and environmental research situations.

Organizational Responses to Changing External Environment

H:1 The organizational structure of natural resources and environmental research organizations will change in response to major changes in the external environment.

H:2 Collaboration between research performers will increase for selected natural resource and environmental research topics.

H:3 Natural resource and environmental research organizations will attract new resources by using differentiation strategies, including creating new research niches.

Research Performance and Effectiveness

H:4 Manager perceptions of a group's research performance are closely related to measured performance if research goals are understood and mutually agreed upon.

H:5 Implementing a portfolio approach to managing research will result in increased research effectiveness.

LITERATURE CITED

Ankley, Gerald; Duane Benoit; Philip Cook and Rodney Johnson. 1990. Development of Methods for Assessing Contaminated Sediments and Establishing Sediment Quality Criteria. Duluth, MN: U. S. Environmental Protection Agency. 71p.

Argyris, Chris. 1968. On the Effectiveness of Research and Development Organizations. American Scientist 56(4):344-355.

Armstrong, John A. 1994. The Concept of Strategic Research. Pp. 191-200 in AAAS Science and Technology Policy Yearbook, 1994, Teich, Albert H.; Stephen D. Nelson and Celia McEnaney (eds). Washington, DC: American Association for the Advancement of Science. 447p.

Arnold, R. Keith; Dickerman, M. B.; Buckman, Robert E. and Steen, Harold K. ed. 1994. View From the Top: Forest Service Research. Durham, NC: Forest History Society. 365p.

Averch, Harvey A. 1985. A Strategic Analysis of Science and Technology Policy. Baltimore: The Johns Hopkins University Press. 216p.

Averch, Harvey A. 1991. The Political Economy of R&D Taxonomies. Research Policy 20(3):179-194.

Babbitt, Bruce. 1995. Science: Opening the Next Chapter of Conservation History. Science 267:1954-1955.

Bauer, Henry H. 1990. Barriers Against Interdisciplinarity: Implications for Studies of Science, Technology, and Society (STS). Science, Technology, & Human Values 15(1):105-119.

Beckman, Svante. 1989. Temas Research and University Ideals. Pp. 31-41 in Initiation, Growth, and Consolidation: The Scientific Dynamics and Societal Relevance of a Non-Traditional Research Organization, Lind, Ingemar; Tord Maunsbach and Lena Olsson (eds.). 1989. Stockholm: Swedish National Board of Universities and Colleges. 125p.

Bell, Robert. 1992. Impure Science: Fraud, Compromise, and Political Influence in Scientific Research. New York, NY: John Wiley & Sons. 301p.

Bella, D. A. and K. J. Williamson. 1976. Conflicts in Interdisciplinary Research. Journal of Environmental Systems 6(2):105-124.

Bengston, David N. 1989. Exogenous Factors Affecting Research Institutions in Developing Countries. International Journal of Technology Management 4(3):317-331.

Bengston, David N. 1989. Impact Evaluation and Planning of Research Programs. Pp. 41-59 in Forest Resource Economics and Policy Research: Strategic Directions for the Future, Ellefson, Paul V. (ed). 1989. Boulder, CO: Westview Press.

Bloch, Erich. 1994. New Relationships Between Industry, Academia, and Government in Science and Technology. Pp. 73-80 in AAAS Science and Technology Policy Yearbook, 1994, Teich, Albert H.; Stephen D. Nelson and Celia McEnaney (eds). Washington, DC: American Association for the Advancement of Science. 447p.

Botkin, Daniel B. 1995. Our Natural History: The Lessons of Lewis and Clark. New York: G.P. Putnam's Sons. 300p.

Bradley, Dorotha M. and Ingram, Helen M. 1986. Science vs. the Grass Roots: Representation in the Bureau of Land Management. Natural Resources Journal 26(3):493-518.

Brewer, Garry D. 1981. Where the Twain Meet: Reconciling Science and Politics in Analysis. Policy Sciences 13(3):269-279.

Brooks, David J. and Gordon E. Grant. 1992. New Approaches to Forest Management: Background, Science Issues, and Research Agenda (Part One). Journal of Forestry 90(1):25-28.

Brooks, David J. and Gordon E. Grant. 1992. New Approaches to Forest Management: Background, Science Issues, and Research Agenda (Part Two). Journal of Forestry 90(2):21-24.

Brown, George E. Jr. 1992. The Objectivity Crisis. American Journal of Physics 60(9):779-781.

Brooks, Harvey. 1973. Knowledge and Action: The Dilemma of Science Policy in the 70's. Daedalus 102(2):125-143.

Brown, John Seely. 1995. Research Restructuring and Assessment at Xerox. Pp. 36-43 in Research Restructuring and Assessment: Can We Apply the Corporate Experience to Government Agencies? National Research Council. Commission on Physical Sciences, Mathematics and Applications Washington, DC: National Academy Press. 72p.

Bushe, G. R. and A. L. Johnson. 1989. Contextual and internal variables affecting task group outcomes in organizations. Group and Organization Studies 14(4):462-482.

Byerly, Radford Jr. and Pielke, Roger A. Jr. 1995. The Changing Ecology of United States Science. Science 269:1531-1532.

Byrne, John A. 1993. The Horizontal Corporation: It's About Managing Across, Not Up and Down. Business Week, December 20, 1993, pp.76-81.

Caldwell, Lynton K. 1970. The Ecosystem as a Criterion for Public Land Policy. Natural Resources Journal 10(2):203-221.

Callaham, Robert Z. 1981. Criteria for Deciding About Forestry Research Programs. USDA Forest Service General Technical Report WO-29. Washington, DC: USDA Forest Service. 52p.

Capra, Fritjof. 1982. The Turning Point: Science, Society and the Rising Culture. New York: Simon and Schuster. 464p.

Carnegie Commission on Science, Technology and Government. 1992a. A Science and Technology Agenda for the Nation: Recommendations for the President and Congress. December, 1992. New York: Carnegie Commission on Science, Technology, and Government. 37p.

Carnegie Commission on Science, Technology and Government. 1992b. International Environmental Research and Assessment: Proposals for Better Organization and Decision Making. July 1992. New York: Carnegie Corporation. 82p.

Carnegie Commission on Science, Technology and Government. Task Force on Establishing and Achieving Long-Term S&T Goals. 1992c. Enabling the Future: Linking Science and Technology to Societal Goals. September, 1992. New York: Carnegie Commission on Science, Technology, and Government. 72p.

Carnegie Commission on Science, Technology and Government. Task Force on the Organization of Federal Environmental R&D Programs. 1992d. Environmental Research and Development: Strengthening the Federal Infrastructure. December, 1992. New York: Carnegie Commission on Science, Technology, and Government. 143p.

Chamberlin, T. C. 1897. The method of multiple working hypotheses. Journal of Geology 5:837-848.

Chamberlin, T. C. 1965. The method of multiple working hypotheses. Science 148:754-759.

Chayut, Michael. 1994. The Hybridization of Scientific Roles and Ideas in the Context of Centres and Peripheries. Minerva 32(3):297-308.

Cheng, Joseph L. C. 1979. A Study of Coordination in Three Research Settings. R&D

Management 9(Special Issue):213-219.

Chubin, Daryl. 1994. Panel Discussion: Performance Assessment for R&D Programs. A panel discussion summary given at AAAS Colloquium on Science and Technology Policy, April 6-8, 1994, Washington, DC.

Chubin, Daryl. 1994. Summary Discussion. Paper presented in Session on "Research Assessment: Best Friend or Junkyard Dog?" 160th Annual Meeting of the American Association for the Advancement of Science, February 18-23, 1994. San Francisco, CA:

Chubin, Daryl E. 1996. Reculturing Science: Politics, Policy, and Promises to Keep. Science and Public Policy 23(1):2-12.

Chubin, Daryl E., Frederick A. Rossini, Alan L. Porter and Ian I. Mitroff. 1979. Experimental Technology Assessment: Explorations in Processes of Interdisciplinary Team Research. Technological Forecasting and Social Change 15:87-94.

Chubin, Daryl E.; Alan L. Porter and Frederick A. Rossini. 1986. Interdisciplinarity: How Do We Know Thee? Pp. 427-440 in Interdisciplinary Analysis and Research, Chubin, Daryl E.; Alan L. Porter; Frederick A. Rossini and T. Connolly (eds.). Mt. Airy, MD: Lomond. 482p.

Churchman, C. West. 1971. The Design of Inquiring Systems: Basic Concepts of Systems and Organization. New York: Basic Books. 288p.

Churchman, C. West. 1973. Basic Concepts of Operational Control. Pp. 160-176 in Public Science Policy and Administration, Rosenthal, Albert H. (ed). Albuquerque: University of New Mexico Press. 322p.

Churchman, C. West. 1979. The Systems Approach and Its Enemies. New York: Basic Books. 221p.

Cole, Stephen. 1992. Making Science: Between Nature and Society. Cambridge, MA: Harvard University Press. 290p.

Cortner, Hanna J.; Shannon, Margaret A.; Wallace, Mary G.; Burke, Sabrina and Moote, Margaret A. 1996. Institutional Barriers and Incentives for Ecosystem Management: A Problem Analysis. USDA Forest Service General Technical Report PNW-GTR-354. Portland, OR: Pacific Northwest Forest and Range Experiment Station. 35p.

Cowan, Thomas A. 1972. Paradoxes of Science Administration. Science 177:964-966.

Cozzens, Susan E. 1994. Strategic Evaluation and the Keystone Model of Basic Research. Pp. 281-291 in AAAS Science and Technology Policy Yearbook, 1994, Teich, Albert H.; Stephen D. Nelson and Celia McEnaney (eds). Washington, DC: American

Association for the Advancement of Science. 447p.

Crease, Robert P. and Nicholas P. Samios. 1991. Managing the Unmanageable. The Atlantic 267(1):80-88.

Crow, Michael M. 1994. Science and Technology Policy in the United States: Trading in the 1950 Model. Science and Public Policy 21(4):202-212.

Daft, Richard L. 1992. Organization Theory and Design. St. Paul, MN: West Publishing Co. 4th Ed. 558p.

Dailey, Robert C. 1978. The Role of Team and Task Characteristics in R&D Team Collaborative Problem Solving and Productivity. Management Science 24(15):1579-1588.

David, Paul. 1994. Panel Discussion: Performance Assessment for R&D Programs. Comments made at AAAS Colloquium on Science and Technology Policy, April 6-8, 1994, Washington, DC.

Davies, J. Clarence. 1994. Policy and the Organization of Environmental Research. Pp. 359-362 in AAAS Science and Technology Policy Yearbook, 1993, Teich, Albert H.; Stephen D. Nelson and Celia McEnaney (eds). Washington, DC: American Association for the Advancement of Science. 414p.

Day, Kelly and Ruttan, Vernon. 1991. The Deficit in Natural Resources Research. Bioscience 41(1):37-40,46.

Drucker, Peter F. 1995. Really Reinventing Government. Atlantic Monthly 275(2):49-61.

Druckman, Daniel; Singer, Jerome E. and Van Cott, Harold (eds). 1997. Enhancing Organizational Performance. National Research Council, Committee on Techniques for the Enhancement of Human Performance. Washington, DC: National Academy Press. 284p.

Dumaine, Brian. 1994. The Trouble With Teams. Fortune 125:86-92.

Duncan, Linda J.; Willard E. Fraize; Dabney G. Hart; John L. Menke; David L. Morrison; Brian H. Price; Brant E. Smith; Timothy K. Underwood and Kerry R. Zimmerman. 1994. Assessment of the Scientific and Technical Laboratories and Facilities of the U. S. Environmental Protection Agency. MTR94W0000082V1 (May, 1994) (Revised). McLean, VA: MITRE Corporation. 229p. + Appendices.

Durant, Robert F. 1992. The Administrative Presidency Revisited: Public Lands, the BLM, and the Reagan Revolution. Albany, New York: State University of New York

Press. 401p.

Durant, Robert F. 1992. Beyond Markets, Hierarchies, or Clans: Lessons from Natural Resource Management in the Reagan Era. Administration & Society 24(3):346-374.

Falk, Charles E. 1988. Evaluation of Current Classifications of Research: A Proposal for a New Policy-Oriented Taxonomy. Pp. 152-166 In The Classification of Research, Hensley, Oliver D. (Ed). 1988. Lubbock: Texas Tech Univ. Press. 179p.

Fallows, James. 1996. A Talk With Bill Clinton. Atlantic Monthly 278(4):20-26.

Foley, Gary. 1993. Testimony of Dr. Gary Foley Before the Subcommittee on Technology, Environment and Aviation, Committee on Science, Space and Technology, U. S. House of Representatives. March 2, 1993, Washington, DC. 8p.

Fourcade, J. M. and B. Wilpert. 1981. Group Dynamics and Management Problems of An International Interdisciplinary Research Team. Pp. 157-172 in Competence and Power in Managerial Decision-Making: A Study of Senior Levels of Organization in Eight Countries, Heller, Frank A. and Wilpert, Bernhard (eds). New York: John Wiley and Sons. 242p.

Fowle, John R. III and Ken Sexton. 1992. EPA Priorities for Biologic Markers Research in Environmental Health. Environmental Health Perspectives 98:235-241.

Fox, Marye Anne. 1994. The Contribution of Curiosity-Driven Research to Technology. Pp. 59-64 in AAAS Science and Technology Policy Yearbook, 1993, Teich, Albert H.; Stephen D. Nelson and Celia McEnaney (eds). Washington, DC: American Association for the Advancement of Science. 414p.

Frame, J. Davidson. 1988. Managing Projects in Organizations. San Francisco: Jossey-Bass. 240p.

Funtowicz, Silvio O. and Ravetz, Jerome R. 1990. A New Problem-Solving Strategy for Global Environmental Issues. National Forum (Phi Kappa Phi Journal) 71(4):38-41.

Funtowicz, Silvio O. and Jerome R. Ravetz. 1990. Uncertainty and Quality in Science for Policy. Dordrecht, The Netherlands: Kluwer Academic Publishers. 229p.

Galbraith, Jay. 1973. Designing Complex Organizations. Reading, MA: Addison-Wesley. 150p.

Garfield, Eugene. 1972. Citation Analysis as a Tool in Journal Evaluation. Science 178:471-479.

Garfield, Eugene. 1992. The Effectiveness of American Society of Agronomy Journals:

A Citationist's Perspective. Pp. 1-13 in Research Ethics, Manuscript Review, and Journal Quality, Mayland, H. F. and R. E. Sojka (eds). Madison, WI: Soil Science Society of America. 94p.

Garfield, E. 1993. Scientists Should Understand the Limitations as Well as the Virtues of Citation Analysis. The Scientist 7(14):12-12.

Gibbons, Michael; Limoges, Camille; Nowotny, Helga; Schwartzman, Simon; Scott, Peter and Trow, Martin. 1994. The New Production of Knowledge. London: Sage Publications. 179p.

Gramp, Kathleen; Albert H. Teich and Stephen D. Nelson. 1992. Federal Funding of Environmental R&D. AAAS Publication No. 92-48S. Washington, DC: American Association for the Advancement of Science. 72p.

Gramp, Kathleen M. and Michele M. Huguelet. 1994. R&D in Selected Agencies. Pp. 125-135 in AAAS Report XIX--Research and Development FY 1995, Intersociety Working Group (compiler). Washington, DC: American Association for the Advancement of Science. AAAS Publication Number 94-23S. 287p.

Gramp, Kathleen M. and Nelson, Stephen D. 1994. Historical Perspectives on Federal Support for Research and Development in FY 1995. Pp. 223-235 in AAAS Science and Technology Policy Yearbook, 1994, Teich, Albert H.; Stephen D. Nelson and Celia McEnaney (eds). Washington, DC: American Association for the Advancement of Science. 447p.

Greenwood, M. R. C. 1994. Partnerships for Change. Presentation made at AAAS Colloquium on Science and Technology Policy , April 6-8, 1994, Washington, DC.

Griffiths, Phillip A. 1994. Science, Technology, and the Federal Government. Pp. 147-158 in AAAS Science and Technology Policy Yearbook, 1994, Teich, Albert H.; Stephen D. Nelson and Celia McEnaney (eds). Washington, DC: American Association for the Advancement of Science. 447p.

Griliches, Zvi. 1987. R&D and Productivity: Measurement Issues and Econometric Results. Science 237:31-35.

Gunderson, Norman E. and Rodriguez, Elizabeth. 1994. The Government Performance and Results Act of 1993 (GPRA): How It Will Affect Federal Scientific Programs. Pp. 265-280 in AAAS Science and Technology Policy Yearbook, 1994, Teich, Albert H.; Stephen D. Nelson and Celia McEnaney (eds). Washington, DC: American Association for the Advancement of Science. 447p.

Gunnerod, Tor. 1992. Personal communication, March 27, 1992, Trondheim, Norway. Dr. Gunnerod is Associate Director, Norwegian Institute for Nature Research.

Guston, David H. 1996. Principal-agent theory and the structure of science policy. Science and Public Policy 23(4):229-240.

Hall, Richard H. 1987. Organizations: Structures, Processes, and Outcomes. Englewood Cliffs, NJ: Prentice-Hall. 358p.

Hammond, Kenneth R., Jeryl Mumpower, Robin L. Dennis, Samuel Fitch and Wilson Crumpacker. 1983. Fundamental Obstacles to the Use of Scientific Information in Public Policy Making. Technological Forecasting and Social Change 24:287-297.

Handy, Charles. 1989. The Age of Unreason. Boston: Harvard Business School Press. 278p.

Hanley, Thomas A. 1994. Interaction of Wildlife Research and Forest Management: The Need for Maturation of Science and Policy. The Forestry Chronicle 70(5):527-532.

Hart, Richard H. 1993. Viewpoint: "Invisible Colleges" and Citation Clusters in Stocking Rate Research. Journal of Range Management 46(5):378-382.

Hautaluoma, Jacob E. and Woodmansee, Robert G. 1994. New roles in ecological research and policy making. Ecology International Bulletin 21:1-10.

Hempel, Lamont C. 1991. EPA in the Year 2000: Perspectives and Priorities. Environmental Law 21(4, Part I):1493-1508.

Herman, Jacques. 1979. Transdisciplinarity, Methodological Paradigms and Research Dynamics in Organizational Context. R&D Management 9(Special Issue):241-243.

Herrick, Charles and Jamieson, Dale. 1995. The Social Construction of Acid Rain. Global Environmental Change 5(2):105-112.

Hilpinen, Risto. 1989. On the Characterization of Cognitive Progress. Pp. 69-80 in Imre Lakatos and Theories of Scientific Change, Ganroglu, K.; Goudaroulis, Y. and Nicolacopoulos, P. (eds). Dordrecht, The Netherlands: Kluwer Academic Publishers.

Hirt, Paul. 1997. Foresters and Old Growth in the Northwest: Looking Back on 100 Years of National Forest Management. Paper presented at Annual Meeting of the American Association for the Advancement of Science, February 14, 1997, Seattle, WA. 29p

Holling, C. S. (ed). 1978. Adaptive Environmental Assessment and Management. Chichester, UK: John Wiley & Sons. 377p.

Howson, Colin and Urbach, Peter. 1993. Scientific Reasoning: The Bayesian Approach. Chicago: Open Court. Second Edition. 470p.

Huddy, Michael D. 1979. An Evaluation of the McIntire-Stennis Cooperative Forestry Research Program. Ph.D. Dissertation. Michigan State University. East Lansing. 127p.

Irwin, Frances H. 1992. An Integrated Framework for Preventing Pollution and Protecting the Environment. Environmental Law 22(1):1-76.

Jakes, Pamela J. and Risbrudt, Christopher D. 1988. Evaluating the Impacts of Forestry Research. Journal of Forestry 86(3):36-39.

Jasanoff, Sheila. 1992. Science, Politics, and the Renegotiation of Expertise at EPA. OSIRIS (2nd Series) 7:195-217.

Kanter, Rosabeth M. 1983. The Change Masters: Innovations for Productivity in the American Corporation. New York, NY: Simon and Schuster. 432p.

Katz, Ralph. 1982. The Effects of Group Longevity on Project Communication and Performance. Administrative Science Quarterly 27:81-104.

Katzenbach, Jon R. and Smith, Douglas K. 1993. The Wisdom of Teams: Creating the High-Performance Organization. Boston: Harvard Business School Press. 291p.

Keidel, Robert W. 1995. Seeing Organizational Patterns: A New Theory and Language of Organizational Design. San Francisco: Berrett-Koehler. 206p.

Kessler, Winifred B., Hal Salwasser, Charles W. Cartwright, Jr. and James A. Caplan. 1992. New Perspectives for Sustainable Natural Resources Management. Ecological Applications 2(3):221-225.

Keyfitz, Nathan. 1993. Genuine Interdisciplinary Study is Possible as Well as Necessary. Options (Newsletter of the International Institute for Applied Systems Analysis) (September, 1993):13-14.

Khorramshahgol, R. and Y. Gousty. 1986. Delphic Goal Programming (DGP): a Multi-objective Cost/benefit Approach to R&D Portfolio Analysis. IEEE Transactions on Engineering Management EM-33(3):172-175.

Kitchener, Richard F. 1995. The Conduct of Inquiry: An Introduction to Logic and Scientific Method. Fort Collins, CO: Colorado State University. Book manuscript in preparation. Revised Spring, 1995.

Kowalok, Michael E. 1993. Common Threads. Environment 35(6):12-20, 35-38.

Kuhn, Thomas S. 1970. The Structure of Scientific Revolutions. International Encyclopedia of Unified Science, Neurath, Otto (Editor-in-Chief). 2. 2nd edn. Chicago: University of Chicago Press. 210p.

Kuhn, Thomas S. 1977. The Essential Tension: Selected Studies in Scientific Tradition and Change. Chicago: University of Chicago Press. 366p.

Kuflik, Arthur. 1992. Personal communication, September 22, 1992, Burlington, Vermont. Dr. Kuflik is Professor of Philosophy, University of Vermont.

Kutz, Frederick W., Rick A. Linthurst, Courtney Riordan, Michael Slimak and Robert Frederick. 1992. Ecological Research at EPA: New Directions. Environmental Science & Technology 26(5):860-866.

Lackey, Robert T. 1995. Ecosystem Health, Biological Diversity, and Sustainable Development: Research That Makes a Difference. Renewable Resources Journal 13(2):8-13.

Leary, Rolfe A. 1985. A Framework for Assessing and Rewarding a Scientist's Research Productivity. Scientometrics 7(½):29-38.

Leary, Rolfe A. 1991. Cogency in Forest Research: II. Pp. 44-57 in Quantity and Quality in Forest Research. Leary, Rolfe A. (comp.). USDA Forest Service General Technical Report NC-148. St. Paul, MN: North Central Forest Experiment Station. 57p.

Leary, Rolfe A. 1993. Letter correspondence, January 28, 1993. North Central Forest Experiment Station, St. Paul, MN. 5p.

Lee, Harold N. 1973. Percepts, Concepts and Theoretic Knowledge: A Study in Epistemology. Memphis, TN: Memphis State University Press. 257p.

Lee, Kai N. 1993. Compass and Gyroscope. Washington, DC: Island Press. 243p.

Lee, Robert G. 1991. Scholarship Versus Technical Legitimation: Avoiding Politicization of Forest Science. Pp. 1-10 in Quantity and Quality in Forest Research. Leary, Rolfe A. (comp.). USDA Forest Service General Technical Report NC-148. St. Paul, MN: North Central Forest Experiment Station. 57p.

Leibowitz, Scott G.; Eric M. Preston; Lynn Y. Arnaut; Naomi E. Detenbeck; Cynthia A. Hagley; Mary E. Kentula; Richard K. Olson; William D. Sanville and Richard R. Sumner. 1992. Wetlands Research Plan FY92-96: An Integrated Risk-Based Approach. Baker, Joan P. (editor). EPA/600/R-92/060. Corvallis, OR: U. S. Environmental Protection Agency. 123p.

Litwin, George H. and Stringer, Robert A. Jr. 1968. Motivation and Organizational Climate. Boston: Harvard University. 214p.

Locke, Edwin A., Gary P. Latham and Miriam Erez. 1988. The Determinants of Goal

Commitment. Academy of Management Review 13(1):23-39.

Luukkonen, Terttu. 1990. Bibliometrics and Evaluation of Research Performance. Annals of Medicine 22:145-150.

MacDonnell, Lawrence J. and Sarah F. Bates. 1993. Rethinking Resources: Reflections on a New Generation of Natural Resources Policy and Law. Pp. 3-20 in Natural Resources Policy and Law: Trends and Directions, MacDonnell, Lawrence J. and Sarah F. Bates (eds.). Washington, DC: Island Press. 241p.

Martin, Ben R. and John Irvine. 1983. Assessing Basic Research: Some Partial Indicators of Scientific Progress in Radio Astronomy. Research Policy 12:61-90.

Martin, Ben R. and John Irvine. 1989. Research Foresight: Priority-Setting in Science. London: Pinter Publishers. 366p.

Mathisen, Werner C. 1990. The Problem-Solving Community: A Valuable Alternative to Disciplinary Communities? Knowledge: Creation, Diffusion, Utilization 11(4):410-427.

McGeary, Michael and Smith, Philip M. 1996. The R&D Portfolio: A Concept for Allocating Science and Technology Funds. Science 274:1484-1485.

McKelvey, Bill. 1982. Organizational Systematics: Taxonomy, Evolution, and Classification. Berkeley: University of California Press. 511p.

Medawar, Peter B. 1969. Induction and Intuition in Scientific Thought. Memoirs of the American Philosophical Society, Volume 75. Philadelphia: American Philosophical Society. 62p.

Meidinger, Errol E. 1997. Organizational and legal challenges for ecosystem management. Pp. 361-379 in Creating a Forestry for the 21st Century: The Science of Ecosystem Management, Kohm, Kathryn and Franklin, Jerry F. (eds). 1997. Washington, DC: Island Press. 475p.

Mervis, Jeffrey. 1993. Clinton Moves to Manage Science. Science 261:1668-1669.

Miller, Alan. 1984. Professional Collaboration in Environmental Management: The Effectiveness of Expert Groups. Journal of Environmental Management 16:365-388.

Miller, Alan. 1993. The Role of Analytical Science in Natural Resource Decision Making. Environmental Management 17(5):563-574.

Mitchell, Rodger, Ramona A. Mayer and Jerry Downhower. 1976. An Evaluation of Three Biome Programs. Science 192:859-865.

Molina, Alfonso H. 1994. Understanding the Emergence of a Large-Scale European Initiative in Technology. Science and Public Policy 21(1):31-41.

Moulton, Gary E. (ed). 1986. The Journals of the Lewis & Clark Expedition, August 30, 1803-August 24, 1804. Chapter 2. Second Printing, 1988. Lincoln: University of Nebraska Press. 612p.

Mullins, Nicholas C. 1987. Evaluating Research Programs: Measurement and Data Sources. Science and Public Policy 14(2):91-98.

Murphy, Tom. 1993. Personal communication, July 20, 1993, Corvallis, Oregon. Dr. Murphy is Director, Environmental Research Laboratory, Corvallis, Oregon.

Naiman, Robert J.; Magnuson, John J.; McKnight, Diane M. and Stanford, Jack A. 1995. The Freshwater Imperative: A Research Agenda. Washington, DC: Island Press. 165p.

Naiman, Robert J., Magnuson, John J., McKnight, Diane M., Stanford, Jack A. and Karr, James R. 1995. Freshwater Ecosystems and Their Management: A National Initiative. Science 270:584-585.

Narin, Francis. 1987. Bibliometric Techniques in the Evaluation of Research Programs. Science and Public Policy 14(2):99-106.

National Academy of Sciences. Committee on Criteria for Federal Support of Research and Development. 1995. Allocating Federal Funds for Science and Technology. Washington, DC: National Academy Press. 97p.

National Academy of Sciences. National Research Council. 1992. Science and the National Parks. Washington, DC: National Academy Press. 122p.

National Academy of Sciences. National Research Council. Committee on Environmental Research. 1993. Research to Protect, Restore, and Manage the Environment. Washington, DC: National Academy Press. 242p.

National Academy of Sciences. National Research Council. Committee on Forestry Research. 1990. Forestry Research: A Mandate for Change. Washington, DC: National Academy Press. 84p.

National Academy of Sciences. National Research Council. Committee on the Formation of the National Biological Survey. 1993. A Biological Survey for the Nation. Washington, DC: National Academy Press. 205p.

National Science and Technology Council. Committee on Environment and Natural Resources. 1995. Preparing for the Future Through Science and Technology: An Agenda for Environmental and Natural Resources Research. Washington, DC: U.S. Government

Printing Office. 68p + Appendices.

National Science and Technology Council. Committee on Environment and Natural Resources. 1995. Strategic Planning Document--Environment and Natural Resources. March, 1995. Washington, DC: Office of Science and Technology Policy. 107p.

National Science and Technology Council. Committee on Environment and Natural Resources. 1997. Program Guide to Federally Funded Environment and Natural Resources R&D. February, 1997. Washington, DC: National Science and Technology Council. 80p.

National Science Board. 1993. Science and Engineering Indicators 1993. NSB 93-1. Washington, DC: U. S. Government Printing Office. 514p.

Nederhof, A. J. and A. F. J. Van Raan. 1993. A Bibliometric Analysis of Six Economics Research Groups: A Comparison with Peer Review. Research Policy 22:353-368.

Newby, Howard. 1992. One Society, One Wissenschaft: A 21st Century Vision. Science and Public Policy 19(1):7-14.

Nichols, Rodney W. 1971. Mission-Oriented R&D. Science 172:29-37.

Nordic Program Committee for Environmental Research. 1991. A Nordic Environmental Research Programme for 1993-1997. Copenhagen, Denmark: Nordic Council of Ministers. 153p.

Novak, Alfred. 1964. Scientific Inquiry. BioScience 14(1):25-28.

Novak, Joseph D. and Gowin, D. Bob. 1984. Learning How to Learn. Cambridge, England: Cambridge University Press. 199p.

Odum, Eugene P. 1977. The Emergence of Ecology as a New Integrative Discipline. Science 195:1289-1293.

Pacific Northwest Research Station. 1997. Research Priorities for Entering the 21st Century. Portland, OR: Pacific Northwest Research Station, USDA Forest Service. 17p.

Parker, J. Kathy. 1993. Interdisciplinary Research and Problem-Solving: A Review of Literature. Final completion report under contract to USDA Forest Service, Northeastern Forest Experiment Station. Broomall, PA: The Oriskany Institute. 122p.

Pearson, A. W. 1983. Team-Building and Group Process Analysis in Interdisciplinary Research Teams. Pp.136-140 in Managing Interdisciplinary Research, Epton, S. R.; R. L. Payne and A. W. Pearson (eds). Chichester, UK: John Wiley & Sons. 245p.

Pelz, Donald C. 1967. Creative Tensions in the Research and Development Climate. Science 157:160-165.

Pirsig, Robert M. 1974. Zen and the Art of Motorcycle Maintenance. New York: Bantam Books. 373p.

Platt, John R. 1964. Strong Inference. Science 146:347-353.

Popper, Karl R. 1961. The Logic of Scientific Discovery. New York: Basic Books. 480p.

Popper, Karl R. 1966. The Open Society and its Enemies, Volume I, The Spell of Plato. Fifth Edition. 1966. Princeton, NJ: Princeton University Press. 361p.

Popper, Karl R. 1966. The Open Society and its Enemies, Volume II, The High Tide of Prophecy: Hegel, Marx, and the Aftermath. Fifth Edition. 1966. Princeton, NJ: Princeton University Press. 420p.

Price, Don K. 1965. The Scientific Estate. Cambridge, MA: Harvard University Press. 323p.

Pulliam, Ron. 1995. May 11, 1995 Memorandum Re: Plans for Incorporating DOI Bureau Needs Into NBS Priorities National Biological Service, Washington, DC.

Quinn, Robert E. and Rohrbaugh, John. 1983. A Spatial Model of Effectiveness Criteria: Toward a Competing Values Approach to Organizational Analysis. Management Science 29(3):363-377.

Rachelson, Stan. 1977. A Question of Balance: A Wholistic View of Scientific Inquiry. Science Education 61(1):109-117.

Rhodes, Richard. 1986. The Making of the Atomic Bomb. New York, NY: Simon and Schuster. 886p.

Rip, Arie. 1994. The republic of science in the 1990's. Higher Education 28:3-23.

Roll-Hansen, Nils. 1992. Personal communication, March 23, 1992, Oslo, Norway. Dr. Roll-Hansen is Science Historian, Norwegian Institute for Studies in Research and Higher Education.

Romesburg, H. Charles. 1991. On Improving the Natural Resources and Environmental Sciences. Journal of Wildlife Management 55(4):744-756.

Rosenberg, Nathan. 1991. Critical Issues in Science Policy Research. Science and Public Policy 18(6):335-346.

Ruivo, Beatriz. 1994. "Phases" or "Paradigms" of Science Policy? Science and Public Policy 21(3):157-164.

Russell, Martha G. 1983. Peer Review in Interdisciplinary Research: Flexibility and Responsiveness. Pp. 184-202 in Managing Interdisciplinary Research, Epton, S. R.; R. L. Payne and A. W. Pearson (eds). Chichester, UK: John Wiley & Sons. 245p.

Sarewitz, Daniel. 1996. Frontiers of Illusion: Science, Technology, and the Politics of Progress. Philadelphia: Temple University Press. 235p.

Saundry, Peter D. and Fingerut, Jonathan T. 1995. Federal Funding for Environmental Research and Development, Fiscal Year 1994. Washington, DC: Committee for the National Institute for the Environment. 72p.

Scavia, Donald, Ruggiero, Michael and Hawes, Ellen. 1996. Building a scientific basis for ensuring the vitality and productivity of U.S. ecosystems. Bulletin of the Ecological Society of America 77(2):125-127.

Schaefer, Mark. 1991. The Federal Research Puzzle: Making the Pieces Fit. Environment 33(9):16-20, 38-42.

Schmandt, Jurgen. 1984. Regulation and Science. Science, Technology, & Human Values 9(1):23-38.

Schmandt, Jurgen and James E. Katz. 1986. The Scientific State: A Theory with Hypotheses. Science, Technology, & Human Values 11(1):40-52.

Schooler, Dean Jr. 1971. Science, Scientists, and Public Policy. New York: The Free Press. 338p.

Schreyer, Richard M. 1974. Individual Orientation and Research Factors Influencing Scientific Effectiveness in the U.S. Forest Service. Ph.D. Dissertation (Natural Resources). University of Michigan. Ann Arbor, MI. 314p.

Sesco, Jerry. 1990. Experiences in Developing a Strategic Plan for the 90's for USDA Forest Service Research. Pp. 55-58 In Research Management for the Future. International Union of Forest Research Organizations Working Party S6.06/6.08 (comp.). USDA Forest Service General Technical Report NE-157. Broomall, PA: Northeastern Forest Experiment Station. 143p.

Shapere, Dudley. 1986. External and Internal Factors in the Development of Science. Science and Technology Studies 4(1):1-9.

Shaw, Byron T. 1967. The Use of Quality and Quantity of Publication as Criteria for Evaluating Scientists. Miscellaneous Publication No. 1041, USDA Agricultural Research

Service. Washington, DC: U. S. Government Printing Office. 78p.

Simon, Herbert A., Langley, Patrick W. and Bradshaw, Gary L. 1981. Scientific Discovery as Problem Solving. Synthese 47(1):1-27.

Skoie, Hans. 1996. Basic research--a new funding climate? Science and Public Policy 23(2):66-75.

Skolnikoff, Eugene B. 1995. Evolving U.S. Science and Technology Policy in a Changing International Environment. Science and Public Policy 22(2):74-84.

Slaughter, Sheila. 1993. Beyond Basic Science: Research University Presidents' Narratives of Science Policy. Science, Technology, & Human Values 18(3):278-302.

Smith, Bruce L. R. 1973. A New Science Policy in the United States. Minerva 11(2):162-174.

Smith, Philip M. and McGeary, Michael. 1997. Don't Look Back: Science Funding for the Future. Issues in Science and Technology 13(3):33-40.

Soule, Michael E. and Kohm, Kathryn A. eds. 1989. Research Priorities for Conservation Biology. Washington, DC: Island Press. 97p.

Staats, Elmer B. and Carey, William D. 1973. Fiscal and Management Dilemmas in Science Administration. Pp. 177-199 in Public Science Policy and Administration, Rosenthal, Albert H. (ed). Albuquerque: University of New Mexico Press. 322p.

Stankiewicz, Rikard. 1980. Leadership and the Performance of Research Groups. Lund, Sweden: Research Policy Institute. 141p.

Stokes, Donald E. 1994. The Impaired Dialogue Between Science and Government and What May Be Done About It. Pp. 121-145 in AAAS Science and Technology Policy Yearbook, 1994, Teich, Albert H.; Stephen D. Nelson and Celia McEnaney (eds). Washington, DC: American Association for the Advancement of Science. 447p.

Stokes, Donald E. 1994. Science the Endless Frontier as a Treatise. Presented at seminar on Science the Endless Frontier--Learning from the Past, Designing for the Future, December 9, 1994, Columbia University, New York City.

Stolte-Heiskanen, Veronica. 1987. Comparative Perspectives on Research Dynamics and Performance: A View from the Periphery. R&D Management 17(4):253-262.

Stone, Richard. 1993. Babbitt Shakes Up Science at Interior. Science 261:976-978.

Stone, Richard. 1993. Long-Term NSF Network Urged to Broaden Scope. Science

262:334-335.

Stone, Richard. 1993. New Rules Squeeze EPA Scientists. Science 262:647.

Teich, Albert H.; Stephen D. Nelson; Kathleen M. Gramp; Bonnie Bisol Cassidy; Michele Huguelet-Tihami and Kei Koizumi. 1994. Congressional Action on Research and Development in the FY 1995 Budget. AAAS Publication Number 94-35S. Washington, DC: American Association for the Advancement of Science. 76p.

Toren, Nina. 1979. The Structure and Management of Government Research Institutes: Some Problems and Suggestions. R&D Management 10(1):5-10.
U.S. Congress. Office of Technology Assessment. 1991. Federally Funded Research: Decisions for a Decade. OTA-SET-490. Washington, DC: U. S. Government Printing Office. 314p.

U.S. Congress. Office of Technology Assessment. 1990. Agricultural Research and Technology Transfer Policies for the 1990's. OTA-F-448. Washington, DC: U.S. Government Printing Office. 50p.

U.S. Congress. Office of Technology Assessment. 1991. Federally Funded Research: Decisions for a Decade. OTA-SET-490. Washington, DC: U. S. Government Printing Office. 314p.

U.S. Environmental Protection Agency. 1990. Environmental Processes and Effects Research: Information Guide. EPA/600/9-90/019. March, 1990. Washington, DC: U. S. Government Printing Office. 35p.

U.S. Environmental Protection Agency. 1992. Great Lakes Research Strategy for Restoration and Maintenance of Great Lakes Integrity. Condensed Great Lakes Research Plan, Draft, February 11, 1992. Duluth, MN: U.S. EPA Environmental Research Laboratory. 13p.

U.S. Environmental Protection Agency. Expert Panel on the Role of Science at EPA. 1992. Safeguarding the Future: Credible Science, Credible Decisions. EPA/600/9-91/050. Washington, DC: U.S. Government Printing Office. 52p.

U.S. Environmental Protection Agency. 1994. ORD Customers Service Work Group--Status and Recommendations. Presented at EPA/ORD Leadership Meeting, March 21, 1994, Covington, KY. 9p.

Van Haveren, Bruce P. 1996. Managing Research as an Investment. Transactions of the 61st North American Wildlife and Natural Resources Conference 61:499-504.

Van Haveren, Bruce P. 1998. Natural Resources Research: Strategic Planning and Organizational Design Imperatives. Science and Public Policy, in progress.

Van Haveren, Bruce P. and Hamilton, Larry E. 1992. Strategic Planning for Natural Resources Research. Preprint of paper presented at Fourth North American Conference on Society and Natural Resources, May 17, 1992, Madison, Wisconsin. 7p.

Wade, Nicholas. 1975. Citation Analysis: A New Tool for Science Administrators. Science 188:429-432.

Wallace, Mary G., Cortner, Hanna J. and Burke, Sabrina. 1995. Review of Policy Evaluation in Natural Resources. Society and Natural Resources 8(1):35-47.

Weimer, Walter B. 1979. Notes on the Methodology of Scientific Research. Hillsdale, NJ: Lawrence Erlbaum Associates. 257p.

Weinberg, Alvin M. 1963. Criteria for Scientific Choice. Minerva 1(2):159-171.

Weinberg, Alvin M. 1967. The Philosophy and Practice of National Science Policy. Pp. 26-38 in Decision Making in National Science Policy, De Reuck, Anthony; Goldsmith, Maurice and Knight, Julie (eds). Boston: Little, Brown and Company. 310p.

Weinberg, Alvin M. 1974. Institutions and Strategies in the Planning of Research. Minerva 12(1):8-17.

Weinberg, Alvin M. 1984. Values in Science: Unity as a Criterion of Scientific Choice. Minerva 22(1):1-12.

Westbrook, J. H. 1960. Identifying Significant Research. Science 132:1229-1234.

Wheatley, Margaret J. 1994. Leadership and the New Science: Learning About Organization from an Orderly Universe. San Francisco: Berrett-Koehler. 166p.

Whewell, William. 1847. The Philosophy of the Inductive Sciences. London: John W. Parker.

White, Gilbert F. 1993. Editorial: Identifying Research Challenges. Environment 35(6):1.

Whitley, Richard and Penelope A. Frost. 1971. The Measurement of Performance in Research. Human Relations 24(2):161-178.

Wilkinson, Charles. 1992. Crossing the Next Meridian. Washington, DC: Island Press. 376p.

Wilson, Edward O. 1998. Consilience: The Unity of Knowledge. New York: Knopf. 332p.

Wilson, E. O. 1998. Integrated science and the coming century of the environment. Science 279:2048-2049.

Wood, Christopher A. 1994. Ecosystem Management: Achieving the New Land Ethic. Renewable Resources Journal 12(1):6-12.

Wulf, William A. 1993. The Collaboratory Opportunity. Science 261:854-855.

Yaffee, Steven L. 1995. Lessons About Leadership from the History of the Spotted Owl Controversy. Natural Resources Journal 35(2):381-412.

Yosie, Terry F. 1993. The EPA Science Advisory Board: A Case Study in Institutional History and Public Policy. Environmental Science & Technology 27(8):1476-1481.

Ziman, John. 1994. Prometheus Bound: Science in a Dynamic Steady State. Cambridge, UK: Cambridge University Press. 289p.

APPENDIX A

DETAILED RESEARCH PROBLEM ANALYSIS

ASSUMPTIONS AND PREMISES

The following assumptions (A1 through A6) and premises (P1 through P6) are based on the review of the literature and my initial observations of research organizations in the U.S. and Europe. They are also summarized in Table A2.

A1. Research has a legitimate role to play in natural resource and environmental decision and policy making

A2. Research performance includes outputs, outcomes, and effectiveness

A3. Group members bring different paradigms, goals, expectations, and systems of inquiry to the table

A4. Natural resources and environmental science tends to be problem-oriented, involves complex research problems, and requires the scientific contributions of several disciplines and multiple dimensions of research activity

A5. A research organization is an open system; research and other scientific activities consist of inputs, work processes and outputs; organized science is both a purposeful and sociological activity

A6 a. Managing research may be compared to managing financial investments

b. Cogency is essential to scientific constructs and knowledge accumulation and thus is a necessary part of research planning

P1. If information is one of the key ingredients to management, then good information creates the potential for good management. Since one of the basic tenets of science is to produce accurate, reliable information, then science has the potential to contribute to good management

P2. Research effectiveness is defined as how well research organizations are able to meet a hierarchical set of research goals.

P3. An increased emphasis on group research will require shifts and modifications in scientific approaches, including epistemological and group sociological considerations

P4 a. Natural resource and environmental research is multidimensional

b. Interdisciplinary research groups exist in the natural resources and environmental research community

c. The adept use of interdisciplinary groups will lead to more useful scientific results, a better set of problem solutions, and a higher level of research effectiveness

P5 a. Organizational factors, predominantly structure and management style, influence the formation of research groups

b. The contextual variables presumed to influence research group performance, include group size, organizational climate and environment, goal agreement and commitment, client involvement, leadership style, group communication and cohesiveness, and the degree to which problem-solving is collaborative

P6 a. Goal-setting is a hierarchical process that relies on effective vertical communications, a group process that leads to goal convergence, and a strategy that results in organization-wide goal commitment

b. Knowledge accumulates incrementally in any system of scientific inquiry

c. Increased cogency in research activities leads to increased strength of inference in research results

Table A1. Major criticisms raised by independent evaluations of the federal government's environmental research structure [after (National Science and Technology Council, 1995)].

Major Criticisms of the Federal Environmental Research Structure

- No clear leadership
- Inadequate links between research and policy and inadequate mechanisms for assessing the state of knowledge
- No comprehensive national environmental research plan that looks beyond near-term regulatory or management needs
- Imbalance between intramural and extramural R&D
- Lack of funding for ecological and social sciences and for finding engineering solutions to environmental problems
- Insufficient attention to long-term monitoring, data collection and management, and interpretation

Table A2. Summary of dissertation problem.

ISSUE	ASSUMPTIONS	PREMISES	PROBLEM STATEMENTS	DISSERTATION OBJECTIVES
Role of science and research in natural resource and environmental management	Research has a legitimate role to play in natural resource and environmental decision and policy making	If information is one of the key ingredients to management, then good information creates the potential for good management. Since one of the basic tenets of science is to produce accurate, reliable information, then science has the potential to contribute to good management	The role of research in natural resources and environmental management is not well understood	Develop a comprehensive post-reformative theory of natural resources and environmental research and its administration
Research performance and effectiveness, Including the concepts of scientific value and quality	Research performance includes outputs, outcomes, and effectiveness	Research effectiveness is defined as how well research organizations are able to meet a hierarchical set of research goals.	A conceptual framework for research effectiveness is lacking Existing research evaluation systems and metrics are inadequate	Examine the concepts of scientific quality and value and research performance and effectiveness as they apply to natural resources and environmental research Develop a framework for evaluating the performance and effectiveness of research groups Investigate the relationships between perceived and measured performance

Group process including systems of inquiry	Group members bring different paradigms, goals, expectations, and systems of inquiry to the table	An increased emphasis on group research will require shifts and modifications in scientific approaches, including epistemological and group sociological considerations	Group research processes and outputs are seldom studied	Develop an epistemological basis for, and explore the methods of, scientific inquiry by research groups Explore natural resource and environmental research group processes
Multidimensional and interdisciplinary research	Natural resources and environmental science tends to be problem-oriented, involves complex research problems, and requires the scientific contributions of several disciplines and multiple dimensions of research activity	Natural resource and environmental research is multidimensional Interdisciplinary research groups exist in the natural resources and environmental research community The adept use of interdisciplinary groups will lead to more useful scientific results, a better set of problem solutions, and a higher level of research effectiveness	Multidimensional and interdisciplinary approaches in natural resources and environmental research have not been studied	Determine the organizational conditions that influence interdisciplinary group research Identify the major dimensions and characteristics of natural resource and environmental research

Organizational structure and other contextual variables in relation to research groups	A research organization is an open system; research and other scientific activities consist of inputs, work processes and outputs; organized science is both a purposeful and sociological activity	Organizational factors, predominantly structure and management style, influence the formation of research groups The contextual variables presumed to influence research group performance, include group size, organizational climate and environment, goal agreement and commitment, client involvement, leadership style, group communication and cohesiveness, and the degree to which problem-solving is collaborative	The specific organizational and managerial factors that contribute to effective interdisciplinary group research are not well known.	Assess the performance, effectiveness, and contextual variables of natural resource and environmental research groups Examine the organizational structure of research labs or centers in relation to research group performance and effectiveness Explore the relationships between organizational characteristics and research group performance and effectiveness
Research planning and management, goal-setting, and cogency	Managing research may be compared to managing financial investments Cogency is essential to scientific constructs and knowledge accumulation and thus is a necessary part of research planning	Goal-setting is a hierarchical process that relies on effective vertical communications, a group process that leads to goal convergence, and a strategy that results in organization-wide goal commitment Knowledge accumulates incrementally in any system of scientific inquiry Increased cogency in research activities leads to increased strength of inference in research results	There is a lack of effective strategies for managing research There is no conceptual framework for managing natural resource and environmental research in the post-reformative period	Assess existing models of research planning Develop a framework for managing research at the laboratory/center/program level Explore management strategies to increase research effectiveness Develop an index of in-process research group performance that combines the scientific productivity and quality facets of performance

LEVELS OF ANALYSIS OF RESEARCH ORGANIZATIONS

Research activity may be studied at many different levels. These levels are hierarchical in terms of goals and functions, as shown in Figure A1.

At the national policy/strategy level, natural resources and environmental research has had few structures and coordinating mechanisms. National policies and strategies have been fragmented and left to Federal departments and agencies. The one exception has been the global climate change research program, which was coordinated across many Federal agencies by the President's Council on Science and Technology.

CENR developed a comprehensive, national plan for natural resource and environmental research that focuses on a long-term research strategy and the near-term development of knowledge to support policy making (National Science and Technology Council, 1995). Evaluations are rare at this level. However, the Carnegie Commission conducted a review of environmental research and advanced several recommendations for improving its overall structure and coordination (Carnegie Commission on Science, Technology and Government, 1992d), as did the National Research Council (National Academy of Sciences, 1993).

Reviews of program-specific research are quite common. In the recent past, reviews or evaluations have been conducted for conservation biology (Soule and Kohm, 1989); freshwater ecology (Gore et al., 1990; Naiman et al., 1995a,b); agriculture (National Research Council, 1989; U.S. Congress, 1990); forest management (Brooks and Grant,

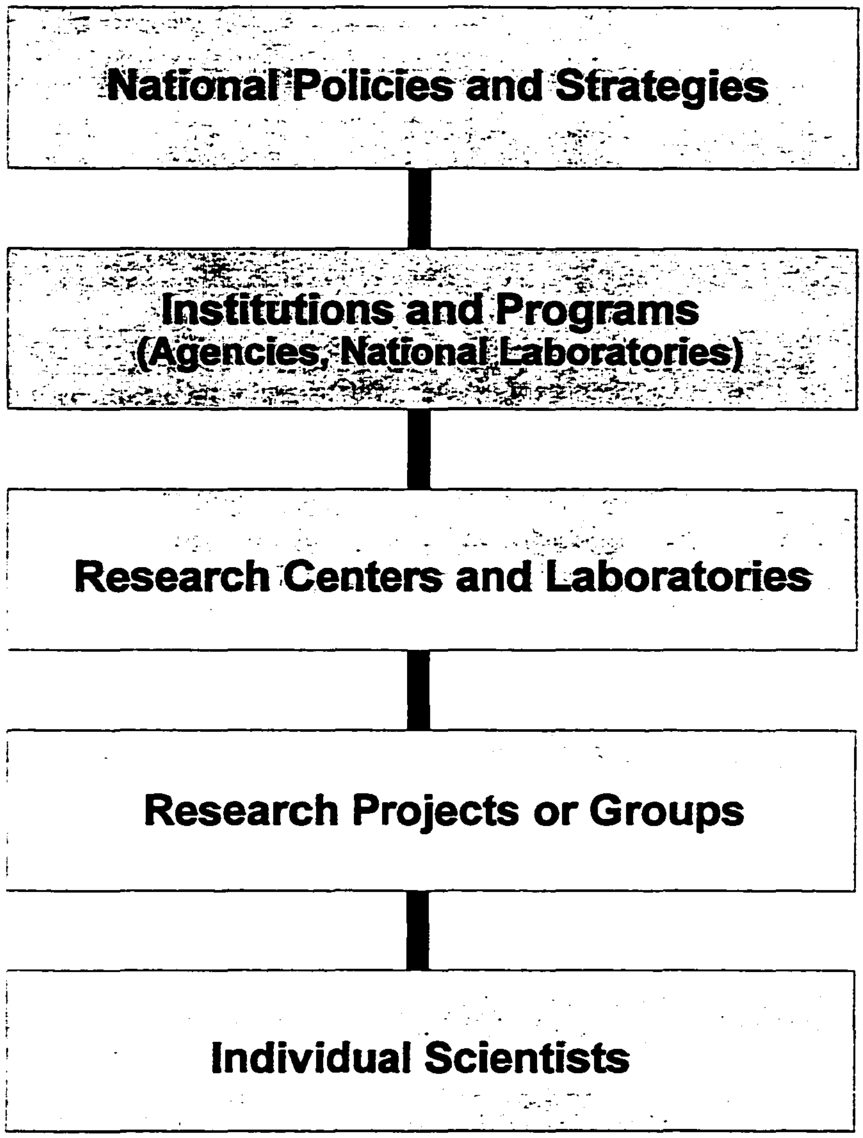


Figure A1. Levels of analysis in investigations of research organizations.

1992a; Brooks and Grant, 1992b; National Academy of Sciences, 1990; Kessler et al., 1992); the sustainable biosphere initiative and long-term ecological research (Stone, 1993; Magnuson, 1990).

At the institutional level, the research programs of Federal departments, agencies, and major national laboratories have been the subject of evaluations and studies, usually with the purpose of determining research policy and direction.

Research centers and regional laboratories are often evaluated for research priorities, scientific quality, research management, and infrastructure needs. Most of these evaluations tend to be qualitative and targeted at center/laboratory directors or their superiors.

Research evaluations at the project and group level are common and tend to be more objective and quantitative in nature. This dissertation is concerned, at a meta-analysis level, with the evaluation and management of research projects and groups in the environmental and natural resource arenas.

RESEARCH TYPES AND ORGANIZATIONAL CONTEXTS

The character or type of research (basic, applied, development) conducted determines the performance measure used in research evaluations. If science is assumed to be primarily problem-solving activity, then performance means success in solving different types of problems in different environments. Measuring research performance must reflect the problem of understanding how knowledge is created and applied (Whitley and Frost, 1971).

The Office of Technology Assessment (U.S. Congress, 1991) defined basic research as pursuing fundamental concepts and knowledge (theories, methods, and findings) and applied research as focusing on the problems in utilizing fundamental concepts and forms of knowledge. Fundamental vs. directed was an alternative classification to basic vs. applied (U.S. Congress, 1991). Basic research was considered exploratory and experimental and intended to lead to new ways of approaching fundamental problems (Schaefer, 1991).

According to Romesburg (1991), there are three divisions of purpose in science: applied science, basic science, and applied-basic science. Further, he claimed there are two divisions of knowing: knowledge at the level of constructs and knowledge at the level of isolates. Constructs are well-defined, directly measurable concepts constructed to serve science; isolates are special abstract constructs isolated from direct observation. For example, the concept of niche is an isolate. The purpose of applied science is to provide knowledge to manage natural resources. The purpose of basic science is to understand nature for understanding's sake. A secondary purpose is to make discoveries which may be useful to applied science, to applied- basic science, or to management (the pool of knowledge concept). He defined applied-basic science as basic science conducted in an applied (or constrained) area. This is similar to the concept of strategic science, which was defined by Fox (1994) as fundamental investigations in certain target areas of the sponsor's mission. Strategic research may be basic or applied research that has a good chance of furthering the goals of the nation or the organization (Martin and Irvine (1989). According to Griffiths (1994), strategic research is work that has a good chance of reaching agreed-upon goals.

Fox (1994) argued that a balance must be struck between investigator-initiated and agency-targeted research, between individual research activity and group research activity, between disciplinary and cross-disciplinary investigations, between investments in personnel and investments in infrastructure, and between curiosity-driven innovation and strategic research. Here we have a suggestion that the basic vs. applied classification has limits and perhaps we should be considering many more dimensions to research activity. Cortner et al. (1996) believed that any comprehensive research problem is composed of different types of research, each with different goals and uses. The authors divided research along two continua--by time scale from short-term to long-term and by orientation from theory to practice. Their goal was to have a healthy mix of all types of research. Van Haveren and Hamilton (1992) suggested a two-dimensional research framework, including dimensions of fundamental/systems vs. applied/adaptive research and strong vs. weak influence on policy formulation. Funtowicz and Ravetz (1990) define policy-related research as research that provides scientific knowledge and advice on policy problems. Brown (1995) used a three-dimensional framework that includes the dimensions of research style (reframing vs. incremental), context (open loop vs. problem-grounded), and kinds of knowledge (problem-specific vs. fundamental). Van Haveren (1996) proposed a three-dimensional space for framing and orienting natural resources research. His dimensions were practice vs. theory, short-term vs. long-term, and low risk vs. high risk. Other possible dimensions include breadth of investigation (narrow and penetrating vs. broad and pioneering), reduction vs. synthesis, anticipatory/strategic vs. goal-directed/tactical, and small/focused vs. large/comprehensive.

Skoie (1996) stated that a distinction between basic and applied research is no longer accepted by all. A new distinction emphasizes the primary intentions behind the investments made by those who finance the activity. That is, a research portfolio may serve a number of different goals. Furthermore, Skoie (1996) believed that different types of research should be run differently and funding kept separately. Weinberg (1963, 1967) made similar arguments many years earlier.

Simon et al. (1981) gave a more epistemological perspective. According to them, the scientific method embodies such activities as designing experiments, gathering data, inventing and developing instruments, formulating and modifying theories, deducing consequences from theories, making predictions from theories, testing theories, inducing regularities and invariants from data, discovering theoretical constructs, and others. Scientific inquiry may be theory-driven or data-driven (Baconian induction). This assumes that scientific inquiry is a cyclical process (data gathering, description, explanation, theory testing, and back to data gathering). Both data-driven and theory-driven, however, give only partial views of the scientific enterprise. Both explanatory theories and descriptive theories may be developed. Descriptive theories are derivable from explanatory theories.

According to Hanley (1994), a distinction between basic and applied research is nonsense. Ziman (1994) concluded that the long-standing distinction between pure or basic science and applied science has lost its meaning. Rosenberg (1991) suggested that the linear model of innovation (sequential model of R&D) was a simplistic and economically naive model and is now dead. Nichols (1971) also described the linear model as simplistic. Martin and Irvine (1989) likewise believed that the traditional 3-way distinction between basic research, applied research, and technology development was inadequate. They referred to research type as "research orientation" and suggested that the degree of unpredictability of outcomes is one dimension of research. Unpredictability is lowest in targeted research and highest in curiosity-driven research. Time-horizon was mentioned as another dimension. They defined short-term as one to two years, medium-term as three to five, and long-term as five to thirty years.

Stokes (1994b) gives us a fresh perspective on this issue of research classification by converting the traditional linear model into a two-dimensional model as shown in Figure A2. This concept was also mentioned by Slaughter (1993), who maintained that basic research is juxtaposed to applied science, creating a dichotomy. However the barriers between the two, he claimed, are highly permeable. As shown in Figure A3, the two-dimensional model of science yields four quadrants of a simple science portfolio. Stokes (1994a,b) refers to the use-inspired basic research quadrant as "Pasteur's Quadrant" because Pasteur was interested in both basic understanding and application of his research.

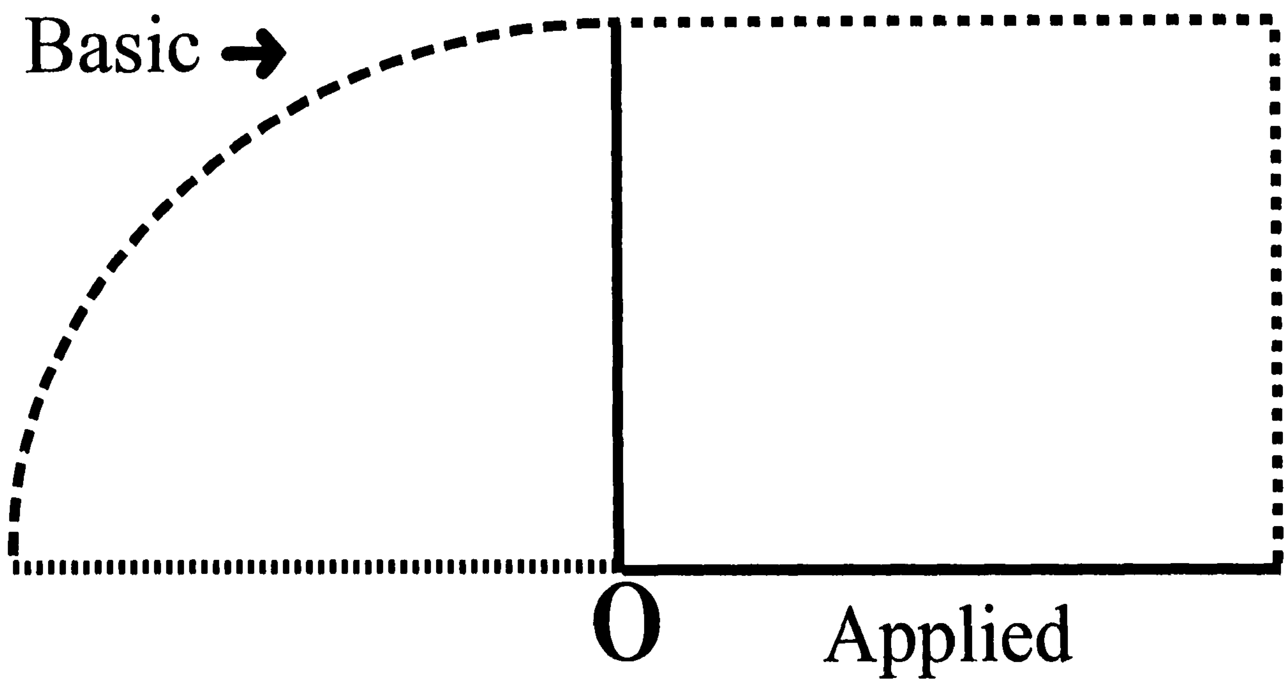


Figure A2. A two-dimensional model of science after Stokes (1994b).

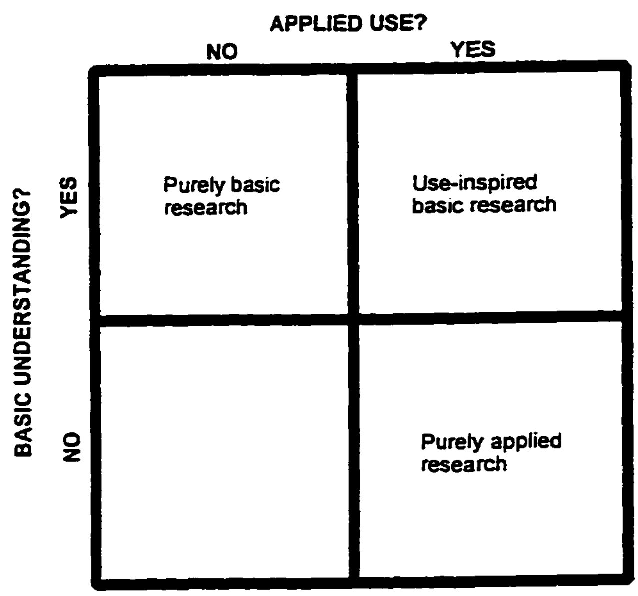


Figure A3. A simple science portfolio based on the two-dimensional model of science of Stokes (1994b).

The purely applied research quadrant could be called "Edison's Quadrant" since Edison was committed to practical applications. The purely basic research quadrant, according to Stokes, would be labeled "Bohr's Quadrant" because Bohr's was interested purely in furthering a basic understanding of particle physics. Stokes likened Pasteur's Quadrant to the present concept of strategic research.

Why limit science to these two dimensions? That research consists of multiple dimensions is one of the cornerstones of a new theory of research management. The traditional linear model of science does not recognize this multidimensionality property of research (Miller, 1993). The linear model of science is said to be simplistic and misleading (National Academy of Sciences, 1995). Categories of basic and applied are contrived categories, contrived for administrative and sometimes political expediency.

Science works best when there is a balance and interaction between different types of research. Research is an interactive system encompassing a spectrum of operational levels from basic science at one end of the spectrum to adaptation and application at the other. However, the linear model of science does not capture the multidimensionality of science and the interaction between the operational levels.

There must be a constant flow of information between the different types of research with the ultimate goal of incorporating research findings into general use and practice or contributing to the stream of knowledge about the natural world.

A BASIC MODEL OF RESEARCH ORGANIZATIONS

Figure A4 provides a very basic conceptual model that will apply to all research organizations. Research systems involve inputs of resources (people and dollars). Through a variety of processes, these inputs are converted into outputs in the form of information and information products. Ideally, these outputs will result in positive outcomes of benefit to both society and the scientific community.

Davies (1994) suggests that we have improved our thinking about research needs, but not about research organization or policy questions. Indeed, most of our management energies have spent on the input side of the model in Figure A4.

Research organizations are considered to be open systems (Kanter, 1983; Toren, 1979). The open systems approach to organizations assumes that organizations have to adapt to changing environments in order to survive and that organizational effectiveness is a function of the consistency of fit between the organization's structural attributes and the environment in which it operates. Wheatley (1994) states that, contrary to fixed, static organizations of the past, we really don't want organizational equilibrium. We want open systems that engage with their environment (customers, linkages) and continue to grow and evolve--systems capable of self-renewal.

Research and other complex organizations with dynamic environments and adaptable structures have been called "adhocracies" (Hall, 1987).

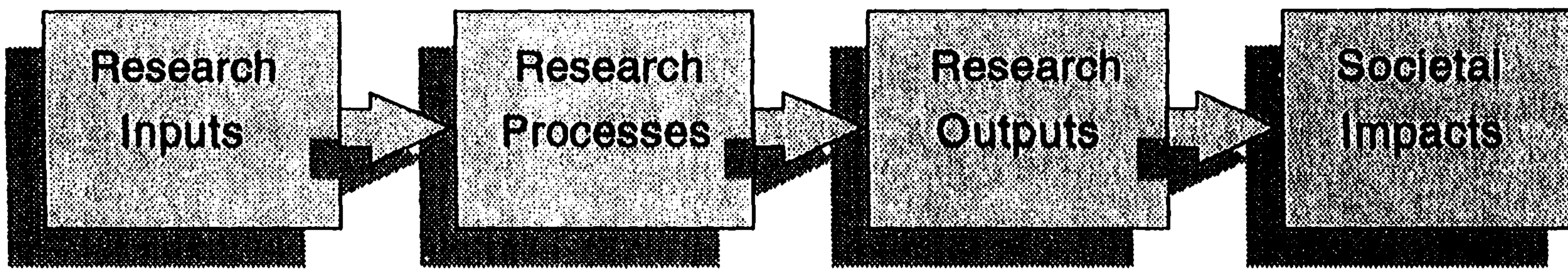


Figure A4. Conceptual model of the research system.

According to McKelvey (1982), organizations may be differentiated on the basis of their dominant competency (technical and managerial knowledge). Thus research organizations, because they require very specific technical and managerial competencies, are substantially different from other non-scientific organizations. It is the population of research organizations that is of particular interest in this dissertation. Furthermore, I have chosen to focus attention on research groups within research laboratories, research centers, and experiment stations which conduct research on natural resources and environmental themes.

Organizational complexity is a function of size, structure, and technology (Hall, 1987). In choosing a population of research organizations to study, these factors should be relatively homogeneous within the population.

The organizational model in Figure A4 may be expressed in the form,

INPUTS + PROCESSES = OUTPUTS

OUTPUTS + TECHNOLOGY TRANSFER = OUTCOMES

Inputs and processes are viewed as explanatory variables in explaining outputs. Outputs have both intrascientific (or intrinsic) and extrascientific (or extrinsic) components. Intrascientific outputs are of interest primarily to the scientific community, in other words other scientists. Averch (1985) refers to this as the internal market for scientific information.

Organizational theorists have developed various models of organizational behavior.

However, because it becomes very difficult to conceptually represent and visualize in more than two or three dimensions, few models adequately represent the multidimensional complexity of organizations. A rudimentary model of organizations is presented in Figure A5. The basic components of an organization's mission (values, principles, culture, goals) may be aligned along either dimension. The external dimension refers to the relationship the organization has with its environment. Organizational environment is defined as all elements that exist outside the boundary of the organization and have the potential to affect the organization (Daft, 1992). The external environment of a research organization includes research sponsors, parent organizations, collaborating or competing research groups, research clients, and the broader scientific community.

The internal dimension refers to structural and contextual factors such as formalization of procedures, degree of specialization, hierarchy of authority, complexity of tasks, centralization of power and authority, professionalism, size, the nature of work process, and technology. Interestingly, the larger the science organization, the more it responds to its internal dynamics (Schmandt and Katz, 1986) and the smaller and more integrated the science component is in a larger organization, the more responsive it is to the decisionmaker (Schmandt and Katz, 1986). Other factors such as goals, strategy, and culture transcend both the internal and external dimensions.

Beckman (1989) assigns "closed" and "open" to the endpoints of his structure (or internal) dimension and employs "autonomy" and "heteronomy" to describe the relationship of the organization to its environment (external dimension).

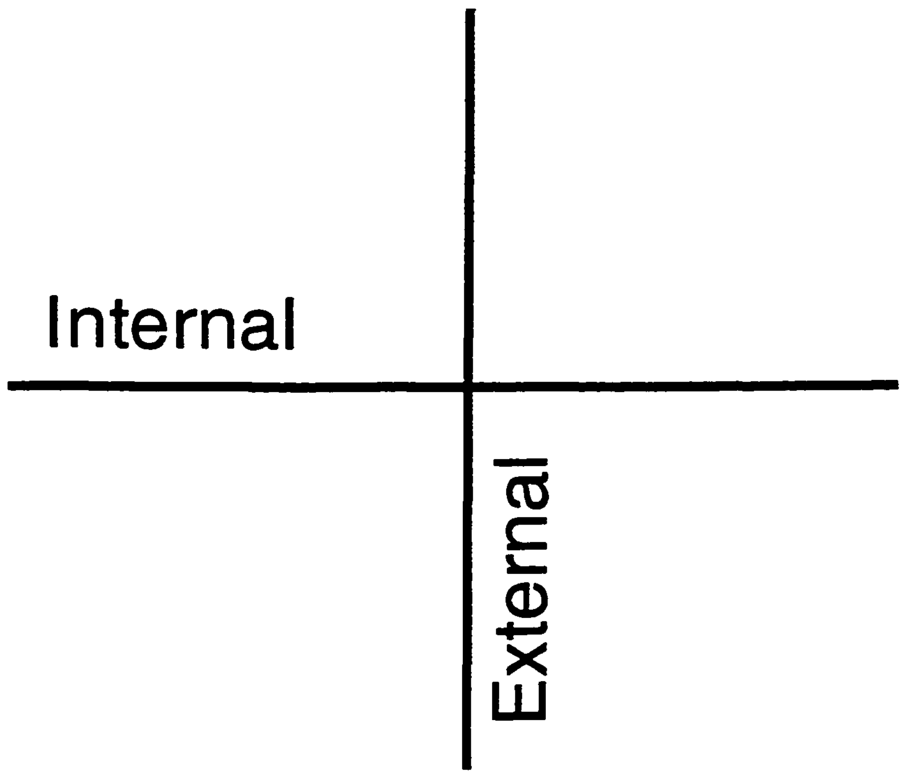


Figure A5. Internal and external dimensions of organizations.

He refers to these two master dimensions as "choice in principles". An autonomous organization would enjoy a measure of independence and self-direction with respect to the environment. A heteronomous organization, according to Beckman's model, is closely integrated with and guided by outside entities and processes. The closed organization has a hierarchical structure, formalized procedures, and an authoritarian, control-oriented management style. An open organization, on the other hand, is loosely ordered, weakly formalized, and has a liberal and participatory management style.

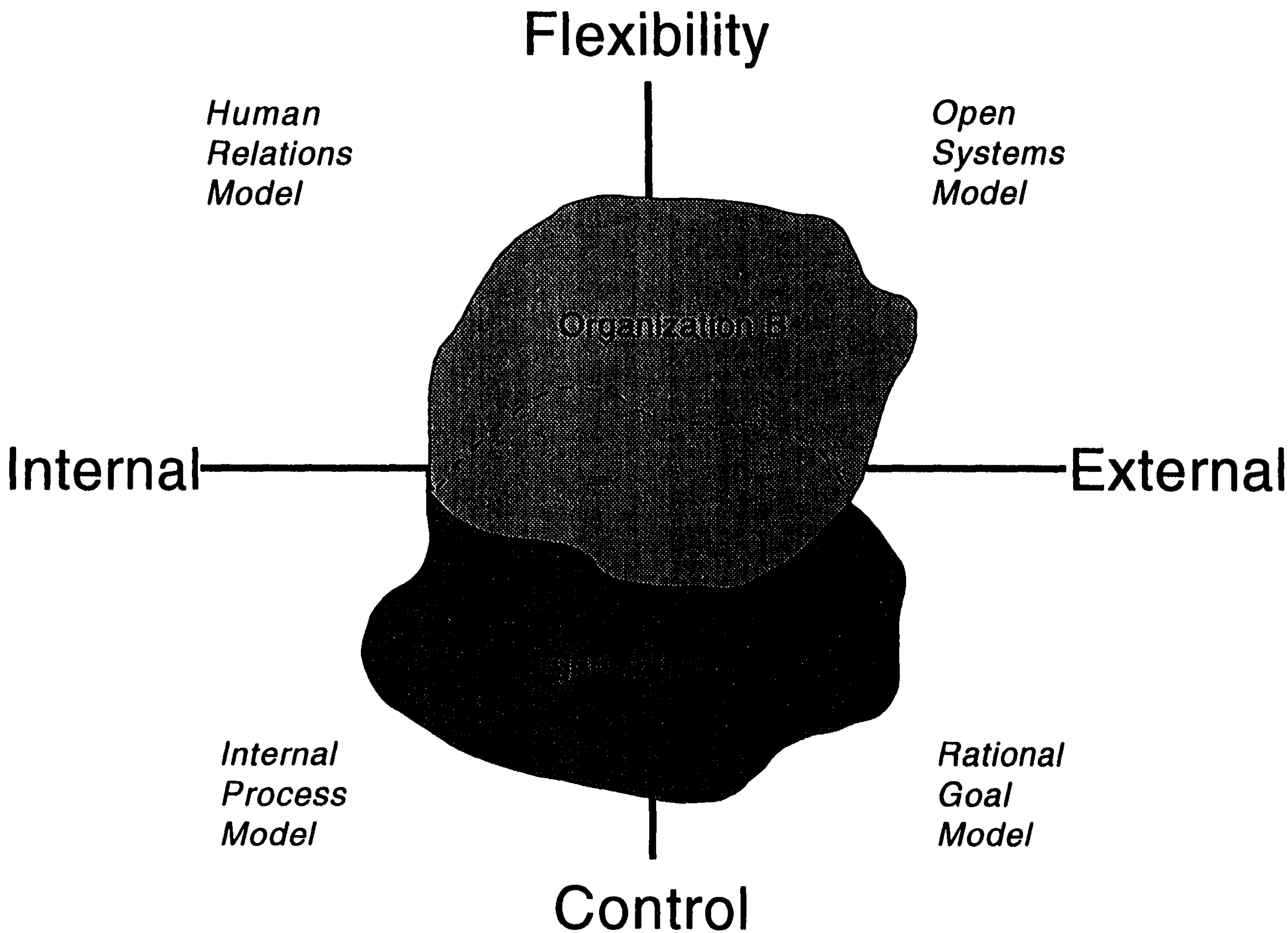
Combining the two dimensions yields four basic organizational paradigms to which organizational ideals and values cluster. Beckman refers to the resulting quadrants as the Bazaar, the Factory, the Temple, and the Professional Oasis. The Oasis and the Temple stress organizational autonomy and therefore would espouse the values of academic freedom, self-direction, and legal/political/ intellectual independence from the environment. In research organizations, basic research would be placed above applied research and personal professional needs above organizational needs. The Bazaar and Factory models as applied to science would favor sociotechnological usefulness and economic potential over academic or professional freedom. Universities resemble the Oasis whereas federal research laboratories look more like the Temple. Natural resource and environmental research organizations are being pulled away from the Temple towards a more open relationship with their environment. This is a desirable change if it means increasing collaboration without losing scientists' autonomy in terms of method selection or the pursuit of promising new research directions.

The 3-dimensional organizational effectiveness model of Quinn and Rohrbaugh (1983), utilizing the dimensions of structure, focus, and degree of closeness to organizational outcomes, is depicted in Figure A6. Organizational structure, the vertical dimension has endpoints of flexibility and control whereas the focus dimension (horizontal) refers to external vs. internal focus of the organization. The third dimension, not shown, reflects a means-ends continuum and refers to the degree to which the organization focuses on final outcomes. The resulting quadrants reflect the dominant organizational values. This model is referred to as the competing values approach to organizational effectiveness (Hall, 1987; Daft, 1992). Organizations may be plotted on this model according to their dominant values.

Druckman et al. (1997) used a triad to represent his organizational dimensions. The vertices represent what an organization is, needs, and does. The sides of the triangle correspond to organizational strategy, functions and processes, and performance/effectiveness.

Any work activity involves a series of work functions. Hautaluoma and Woodmansee (1994) presented four work functions involved in the system of producing and using scientific information: 1) producing basic scientific knowledge, 2) collecting, interpreting, synthesizing, and disseminating scientific information, 3) policymaking about natural resources and environmental issues, and 4) using policies and scientific information to manage natural resources and the environment. For each of these major work functions, one could also list the sub-functions or work processes involved.

Figure A6. Three-dimensional organizational effectiveness model after Quinn and Rohrbaugh (1983), wherein two organizations are plotted on the basis of competing values (Hall, 1987).


Flexibility
Human Relations Model
Open Systems Model
Organization B
Internal
External
Internal Process Model
Rational Goal Model
Control

RESEARCH MANAGEMENT IN THE PRE-REFORMATIVE PERIOD

The characteristics and idiosyncracies of research management in the pre-reformative period are summarized below. The orientation is natural resources and environmental research but the findings may be broadly applied to the physical and biological sciences. This is essentially a rear-view perspective because we are now in the reformative period.

A. Knowledge accretion and problem-solving proceeded primarily along disciplinary paths.

B. Research approaches were mostly reductionistic.

C. Researcher autonomy was the norm. There was little client interaction and few research partnerships.

D. The traditional scheme of research classification, which makes a distinction between basic research, applied research, and development, was largely an accounting and political convenience. A more useful classification enabling meaningful dialogue about the character of research was needed.

E. The single-dimension, linear model of science predominated the pre-reformative period. The two-dimensional model of science was advanced during the early 1990's and is evolving into a multi-dimensional model.

F. Inference has improved with advances in statistical theory and methods and with increasing reliance on the hypothetico-deductive method. Procedures for drawing inferences within an inter-disciplinary group environment were lacking.

G. Management theory, as applied to the administration of research programs and centers, focused on the strategist vs. institutionalist philosophies.

H. Research evaluation focused internally on inputs and intrascientific outputs.

I. Studies of research policy and organization paid scant attention to level of analysis.

J. Research organizations were adhocracies characterized by dynamic environments but struggling to have flexible structures that enable free-flowing communications, ideas, and innovations. They focused on the individual scientist as the primary producing unit.

APPENDIX B

DESCRIPTION OF CONSTRUCTED INDICES

Table B1. Description and construct of indices of perceived performance.

INDEX	DESCRIPTION	CONSTRUCT
DIRSCIPERF	Laboratory Director's perception of group's scientific performance	AVG{DIR-1,DIR-3,DIR-5,DIR-6,DIR-7,DIR-9}
LDRSCIPERF	Group leader's perception of group's scientific performance	AVG{LDR-17,LDR-26,LDR-28,LDR-30,LDR-31,LDR-32, LDR-34}
MEMSCIPERF	Group members' perception of group's scientific performance	AVG{MEM-16,MEM-38,MEM-40,MEM-42,MEM-43, MEM-44, MEM-46}
DIRCLIENT	Laboratory Director's perception of group's responsiveness to clients	AVG{DIR-4,DIR-12}
LDRCLIENT	Group leader's perception of group's responsiveness to clients	AVG{LDR-29,LDR-39}
MEMCLIENT	Group members' perception of group's responsiveness to clients	AVG{MEM-41,MEM-49}
INTERDISC	Degree of interdisciplinarity as perceived by leader and members	AVG{LDR-8,LDR-16,LDR-18,MEM-9,MEM-15,MEM-17}
EFFECTIV	Laboratory Director's perception of group effectiveness	AVG{DIR-1,DIR-2,DIR-3,DIR-4,DIR-5,DIR-6,DIR-7, DIR-8,DIR-9,DIR-10,DIR-11,DIR-12}
GOALEFF	Perception of goal-related effectiveness	AVG(DIR-2,LDR-27,MEM-39)

Table B2. Description and construct of indices of contextual variables.

INDEX	DESCRIPTION	CONSTRUCT
CLIMATE	Organizational climate	AVG{LDR-22,MEM-21,LDR-23,MEM-22,LDR-21,MEM-8, MEM-20,MEM-35,MEM-23,MEM-26,MEM-37}
COMMUNIC	Group communication	AVG{LDR-19,MEM-18,MEM-25,LDR-50,MEM-52}
COHESIV	Group cohesiveness	AVG{LDR-20,LDR-22,LDR-23,MEM-19,MEM-21, MEM-22}
COLLAB	Collaborative problem-solving	AVG{LDR-18,LDR-19,MEM-17,MEM-18,MEM-25}
DIVERS	Diversity index	(no. of disciplines) ÷ (no. of unit members)
INTERDISC	Degree of interdisciplinarity	AVG{LDR-8,LDR-9,LDR-10,LDR-11,LDR-12,LDR-15,LDR-16,LDR-18,MEM-9,MEM-10,MEM-11,MEM-12,MEM-13,MEM-14,MEM-15,MEM-17}
CLIENT	Degree of client involvement	AVG{LDR-6,LDR-40,LDR-41,LDR-42,LDR-43,LDR-44,MEM-4,MEM-28,MEM-29,MEM-30,MEM-31,MEM-32}
COMMIT	Degree of commitment to group goals	AVG{LDR-21,LDR-45,MEM-8,MEM-20,MEM-27}
LEADER	Leadership style	{LDR-37,LDR-38,LDR-47,LDR-48,LDR-49,MEM-53, MEM-54,MEM-55}

Table B3. Description and construct of indices related to goals.

INDEX	DESCRIPTION	CONSTRUCT
GOALCONG	Goal congruence	AVG{LDR-46,MEM-34}
GOALCLAR	Goal clarity	MEM-6
GOALAGREE	Goal agreement	AVG{MEM-7,MEM-33}

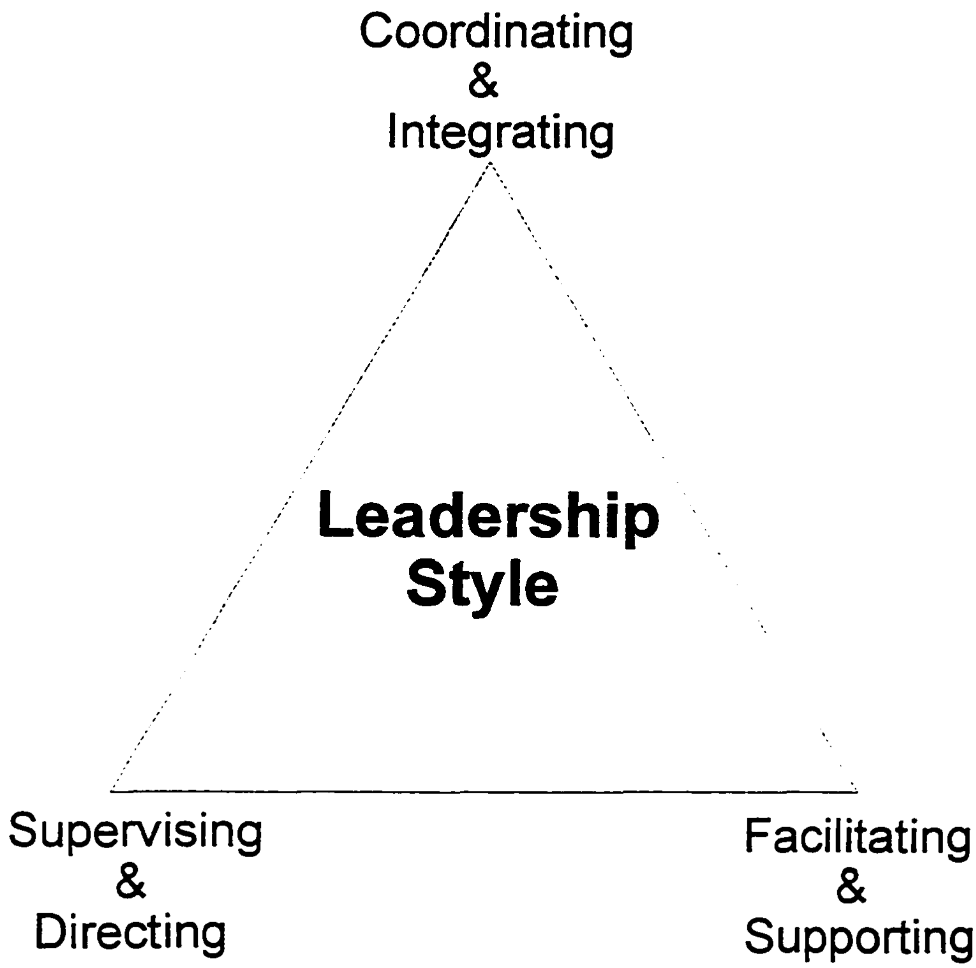


Figure B1. Leadership style dimensions, after Keidel (1995).

APPENDIX C

PERFORMANCE AND EFFECTIVENESS SURVEY INSTRUMENT

EPA ENVIRONMENTAL PROCESSES AND EFFECTS RESEARCH LABS
RESEARCH UNIT EFFECTIVENESS STUDY

Laboratory Director Survey

Lab______________________________

Research Unit____________________________________

For each of the statements in the first column below please circle the number in the second column which most accurately describes your perception about the work of the research unit identified above.

Statement	Strongly Disagree					Strongly Agree
1. This research unit has been productive in the sense of contributing scientific knowledge, methods, or technical assistance within its field of work	1	2	3	4	5	6
2. This research unit has contributed effectively to the mission and goals of this Laboratory	1	2	3	4	5	6
3. This research unit has been innovative in generating new ideas, approaches, methods, inventions, or applications	1	2	3	4	5	6
4. This research unit has been responsive to client organizations	1	2	3	4	5	6
5. This research unit has achieved a national reputation in its field of work	1	2	3	4	5	6
6. This research unit has achieved an international reputation in its field of work	1	2	3	4	5	6
7. The work produced by this research unit is of high scientific quality	1	2	3	4	5	6
8. The work produced by this research unit has a high degree of environmental policy relevance	1	2	3	4	5	6
9. The work of this research unit has had a positive impact on the scientific community	1	2	3	4	5	6
10. This research unit has been successful in meeting its schedules	1	2	3	4	5	6
11. This research unit has been successful in staying within its operating budgets	1	2	3	4	5	6
12. This research unit has been successful in transferring research results to appropriate users	1	2	3	4	5	6

EPA ENVIRONMENTAL PROCESSES AND EFFECTS RESEARCH LABS
RESEARCH UNIT EFFECTIVENESS STUDY

Research Unit Leader Survey

Lab____________________________

Research Unit______________________________________

1. The work of this research unit can be classified primarily as (choose one):

____basic or fundamental research ____technical consultancy

____applied research ____developmental (methods/models)

____scientific assessment/synthesis of literature

2. The work of this research unit can be classified primarily as:

____long-term (>5 years duration) ____short-term (<5 years duration)

3. The research results produced by this research unit will be used primarily for (choose one):

____making predictions of environmental effects/impacts

____providing explanations of environmental phenomena/processes

____issuing prescriptions for environmental impact mitigation

____providing environmental policy options

4. The principal beneficiaries of this research unit's work are (choose one):

____other scientists or research projects within EPA/ORD

____scientific community outside of EPA

____EPA Program Offices

____the public

____state and federal environmental policymakers/regulatory bodies (e. g. EPA regional offices)

5. The nature of the research conducted by this unit is primarily (choose one):

____exploratory ____problem-oriented ____policy-related

6. The frequency of contact between the research unit's clients and unit members is best described as (choose one):

____daily ____monthly ____between 1 and 4 times annually

____weekly ____quarterly ____annually

7. Which of the following best describes the stage of research that this research unit is in (choose one):

____Initiation phase (problem identification/definition/formulation, project planning, exp. design)

____Implementation phase (discovery/observation, data collection/analysis, hypothesis testing)

____Concluding phase (inference, synthesis, drawing conclusions, writing results)

____Dissemination phase (publishing results, information transfer)

____The unit is involved in multiple projects at various stages

For each of the statements in the first column below please circle the number in the second column which most accurately describes your perception about the work of this research unit (considering the research unit as a whole including EPA scientists and on-site cooperators).

Statement	Strongly Disagree ____ Strongly Agree
8. Different bodies of knowledge are represented within the unit	1 2 3 4 5 6
9. Unit members use different problem-solving approaches in attempting to solve research problems	1 2 3 4 5 6
10. Unit members perform different roles during the process of solving research problems	1 2 3 4 5 6
11. Unit members work on a common research problem	1 2 3 4 5 6
12. There is group responsibility (excluding contractors) for the final products produced within this unit	1 2 3 4 5 6
13. The unit shares common facilities (office and lab space)	1 2 3 4 5 6
14. The nature of the research problem determines the selection of unit personnel	1 2 3 4 5 6
15. Members are influenced by how others in the unit perform their tasks	1 2 3 4 5 6
16. Research products (e. g. joint publications) that combine the individual contributions of more than one unit member represent an integrated effort rather than a collection of disciplinary contributions	1 2 3 4 5 6
17. The publications of this research unit are in high demand and often cited in the scientific literature	1 2 3 4 5 6
18. A high degree of interaction between unit members is necessary for the types of research problems undertaken by this research unit	1 2 3 4 5 6
19. Task-related communication between unit members (excluding contractors) is common and frequent	1 2 3 4 5 6
20. There is agreement among unit members concerning the research approach (the general system of inquiry or epistemology) taken by the unit	1 2 3 4 5 6
21. Unit members demonstrate a high level of commitment toward the unit's research activities	1 2 3 4 5 6
22. The members of this unit get along well intellectually	1 2 3 4 5 6
23. The members of this unit get along well socially (excluding contractors)	1 2 3 4 5 6

24. The research conducted by this unit is highly visible and the outcome stakes are high	1 2 3 4 5 6
25. The level of difficulty of this unit's research is relatively high and the outcomes are relatively uncertain	1 2 3 4 5 6
26. This research unit has been productive in the sense of contributing scientific knowledge, methods, or technical assistance within its field of work	1 2 3 4 5 6
27. This research unit has contributed effectively to the mission and goals of this Laboratory	1 2 3 4 5 6
28. This research unit has been innovative in generating new ideas, approaches, methods, inventions, or applications	1 2 3 4 5 6
29. This research unit has been responsive to client organizations	1 2 3 4 5 6
30. This research unit has achieved a national reputation in its field of work	1 2 3 4 5 6
31. This research unit has achieved an international reputation in its field of work	1 2 3 4 5 6
32. The work produced by this research unit is of high scientific quality	1 2 3 4 5 6
33. The work produced by this research unit has a high degree of environmental policy relevance	1 2 3 4 5 6
34. The work of this research unit has had a positive impact on the scientific community	1 2 3 4 5 6
35. This research unit has been successful in meeting its schedules	1 2 3 4 5 6
36. This research unit has been successful in staying within its operating budgets	1 2 3 4 5 6
37. As a unit leader I tend to be task-oriented	1 2 3 4 5 6
38. As a unit leader I tend to be people-centered	1 2 3 4 5 6
39. This research unit has been successful in transferring research results to appropriate users	1 2 3 4 5 6
40. This research unit involves clients in the initiation phase of its research (planning, problem identification/ formulation)	1 2 3 4 5 6
41. This research unit interacts with clients primarily at the technology transfer or information transfer phase of its research	1 2 3 4 5 6
42. This research unit interacts with clients primarily by way of disseminating research results through project completion reports and scientific publications	1 2 3 4 5 6
43. This research unit involves clients in the actual conduct of research	1 2 3 4 5 6
44. This research unit involves clients in the evaluation of research projects or programs	1 2 3 4 5 6
45. Unit members' dedication to the unit's goals is equal to or greater than members' dedication to their individual work goals	1 2 3 4 5 6
46. Unit leader goals and unit member goals are congruent	1 2 3 4 5 6

47. My leadership style is best described as (choose one):

____democratic/participative

____supportive/caring/facilitative

____evaluative/technically-oriented

____direct/authoritarian/pragmatic

48. As a supervisor/manager I would best describe myself as (choose one):

____results-oriented ____people-oriented

____principle-oriented ____planning oriented

49. My principal role in unit meetings is (choose one):

____supervising/directing ____motivating/activating

____facilitating/supporting ____integrating/coordinating

50. The frequency of unit meetings is approximately (choose one):

____daily ____weekly ____twice weekly

____monthly ____less than monthly

EPA ENVIRONMENTAL PROCESSES AND EFFECTS RESEARCH LABS
RESEARCH UNIT EFFECTIVENESS STUDY

Unit Member Survey

Lab____________________________

Research Unit_________________________________

Respondent______ ____EPA ____Non-EPA

1. The nature of the research conducted by this unit is primarily (choose one):

____exploratory ____problem-directed ____policy-related

2. The research results produced by this research unit will be used primarily for (choose one):

____making predictions of environmental effects/impacts

____providing explanations of environmental phenomena/processes

____issuing prescriptions for environmental impact mitigation

____providing environmental policy options

3. The principal beneficiaries of this research unit's work are (choose one):

____other scientists or research projects within EPA/ORD

____scientific community outside of EPA

____EPA Program Offices

____the public

____state and federal environmental policymakers/regulatory bodies (e. g. EPA regional offices)

4. The frequency of contact between the research unit's clients and unit members is best described as (choose one):

____daily ____monthly ____between 1 and 4 times annually

____weekly ____quarterly ____annually

5. The research unit's goals are reviewed (choose one):

____annually or less frequently ____at least twice annually

For each of the statements in the first column below please circle the number in the second column which most accurately describes your perception about the work of this research unit (considering the research unit as a whole including EPA scientists and on-site cooperators).

Statement	Strongly Disagree ____________ Strongly Agree
6. The research goals of this unit are clear and unambiguous	1 2 3 4 5 6

7. Most members of this research unit agree with and are working towards the unit's goals	1	2	3	4	5	6
8. Most members of this research unit are highly motivated and committed to the project	1	2	3	4	5	6
9. Different bodies of knowledge are represented within the unit	1	2	3	4	5	6
10. Unit members use different problem-solving approaches in attempting to solve research problems	1	2	3	4	5	6
11. Unit members perform different roles during the process of solving research problems	1	2	3	4	5	6
12. Unit members work on a common research problem	1	2	3	4	5	6
13. There is group responsibility (excluding contractors) for the final products produced within this unit	1	2	3	4	5	6
14. Members are influenced by how others in the unit perform their tasks	1	2	3	4	5	6
15. Research products (e. g. joint publications) that combine the individual contributions of more than one unit member represent an integrated effort rather than a collection of disciplinary contributions	1	2	3	4	5	6
16. The publications of this research unit are in high demand and often cited in the scientific literature	1	2	3	4	5	6
17. A high degree of interaction between unit members is necessary for the types of research problems undertaken by this research unit	1	2	3	4	5	6
18. Task-related communication between unit members (excluding contractors) is common and frequent	1	2	3	4	5	6
19. There is agreement among unit members concerning the research approach (the general system of inquiry or epistemology) taken by the unit	1	2	3	4	5	6
20. Unit members demonstrate a high level of commitment toward the unit's research activities	1	2	3	4	5	6
21. The members of this unit get along well intellectually	1	2	3	4	5	6
22. The members of this unit (excluding contractors) get along well socially	1	2	3	4	5	6
23. My professional needs are satisfied through my working experiences in this research unit	1	2	3	4	5	6
24. I have adequate input to the goal-setting process in this research unit	1	2	3	4	5	6
25. Communication between members of this research unit (excluding contractors) is open and frequent	1	2	3	4	5	6
26. Unit members are adequately rewarded for their work efforts	1	2	3	4	5	6
27. Unit members' dedication to the unit's goals is equal to or greater than dedication to individual work goals	1	2	3	4	5	6
28. This research unit involves clients in the initiation phase of its research (planning, problem identification/ formulation)	1	2	3	4	5	6
29. This research unit interacts with clients primarily at the technology transfer or information transfer phase of its research	1	2	3	4	5	6
30. This research unit interacts with clients primarily by way of disseminating research results through project completion reports and scientific publications	1	2	3	4	5	6

31. This research unit involves clients in the actual conduct of research	1 2 3 4 5 6
32. This research unit involves clients in the evaluation of research projects or programs	1 2 3 4 5 6
33. Unit members' individual work goals are congruent and are aligned with the unit's research goals	1 2 3 4 5 6
34. Unit leader goals and unit member goals are congruent	1 2 3 4 5 6
35. The morale of this research unit is high	1 2 3 4 5 6
36. There is agreement among unit members concerning the research approach (the general system of inquiry or epistemology) taken by the unit	1 2 3 4 5 6
37. There is adequate administrative and technical support for this research unit	1 2 3 4 5 6
38. This research unit has been productive in the sense of contributing scientific knowledge, methods, or technical assistance within its field of work	1 2 3 4 5 6
39. This research unit has contributed effectively to the mission and goals of this Laboratory	1 2 3 4 5 6
40. This research unit has been innovative in generating new ideas, approaches, methods, inventions, or applications	1 2 3 4 5 6
41. This research unit has been responsive to client organizations	1 2 3 4 5 6
42. This research unit has achieved a national reputation in its field of work	1 2 3 4 5 6
43. This research unit has achieved an international reputation in its field of work	1 2 3 4 5 6
44. The work produced by this research unit is of high scientific quality	1 2 3 4 5 6
45. The work produced by this research unit has a high degree of environmental policy relevance	1 2 3 4 5 6
46. The work of this research unit has had a positive impact on the scientific community	1 2 3 4 5 6
47. This research unit has been successful in meeting its schedules	1 2 3 4 5 6
48. This research unit has been successful in staying within its operating budgets	1 2 3 4 5 6
49. This research unit has been successful in transferring research results to appropriate users	1 2 3 4 5 6
50. The research conducted by this unit is highly visible and the outcome stakes are high	1 2 3 4 5 6
51. The level of difficulty of this unit's research is relatively high and the outcomes are relatively uncertain	1 2 3 4 5 6

52. The frequency of unit meetings is approximately (choose one):

____daily ____weekly ____twice weekly

____monthly ____less than monthly

53. The leadership style of the unit leader is best described as (choose one):

____democratic/participative

____supportive/caring/facilitative

____evaluative/technically-oriented

____direct/authoritarian/pragmatic

54. I would describe the unit leader as primarily (choose one):

____results-oriented ____people-oriented

____principle-oriented ____planning oriented

55. The unit leader's principal role in unit meetings appears to be (choose one):

____supervising/directing ____motivating/activating

____facilitating/supporting ____integrating/coordinating

APPENDIX D

CONTEXTUAL FACTORS

Table D1. Indices of contextual factors for the 14 research groups.

GROUP	CLIMATE	COMMUNIC	COHESIV	COLLAB	INTERDISC	CLIENT	COMMIT
SEDCR	4.5	4.8	4.5	4.6	4.8	3.8	5.0
GLOBC	5.5	4.9	5.5	5.6	5.5	4.3	5.5
OILSP	4.3	3.8	4.3	4.4	4.7	3.7	4.8
FLDTM	4.6	5.2	4.3	5.8	5.0	3.8	5.2
MICRO	5.4	5.0	5.3	5.7	5.3	3.8	5.7
BENTH	5.0	4.8	5.2	5.5	5.6	3.8	5.5
WETLD	4.6	3.4	3.5	4.0	4.7	5.8	4.8
GLOBM	4.0	4.6	4.2	4.8	4.4	3.5	4.6
WSHED	4.9	4.1	4.9	5.0	4.9	3.7	5.1
WLDLF	3.9	4.0	4.0	5.0	5.2	3.9	5.2
OZONE	4.5	3.7	4.7	5.0	4.9	3.5	5.0
GLOBP	4.9	4.6	4.6	5.3	5.2	3.4	5.3
BIORM	3.9	3.6	4.0	4.5	4.8	4.0	4.3
APPLN	4.7	4.3	4.8	4.9	5.1	4.2	4.6

Table D2. Group leader-member agreement on contextual indices.

INDEX	LEADER AVERAGE	MEMBER AVERAGE	r
Organizational Climate	4.8	4.5	0.361
Group Cohesiveness	4.7	4.4	0.255
Group Communication	4.3	4.4	0.753**
Collaborative Problem-Solving	5.2	4.9	0.430
Commitment to Group Goals	5.1	5.0	0.092
Extent of Client Involvement	3.8	4.1	0.413
Degree of Interdisciplinarity	5.0	5.1	0.509*

** significant at p=0.99
* significant at p=0.95

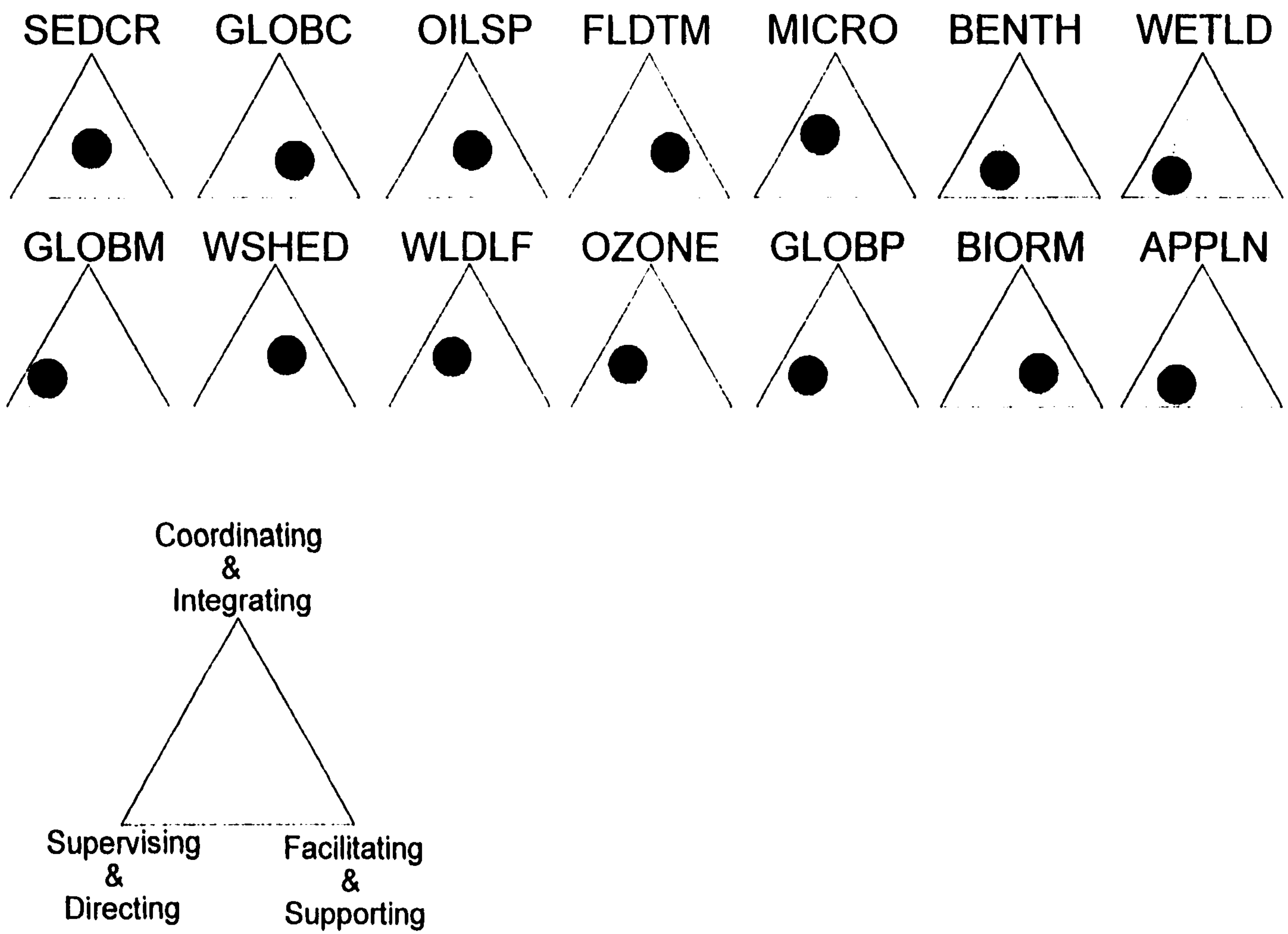


Figure D1. Leadership styles of research group leaders.

Figure D2. Group leader vs. member perceptions of organizational climate.

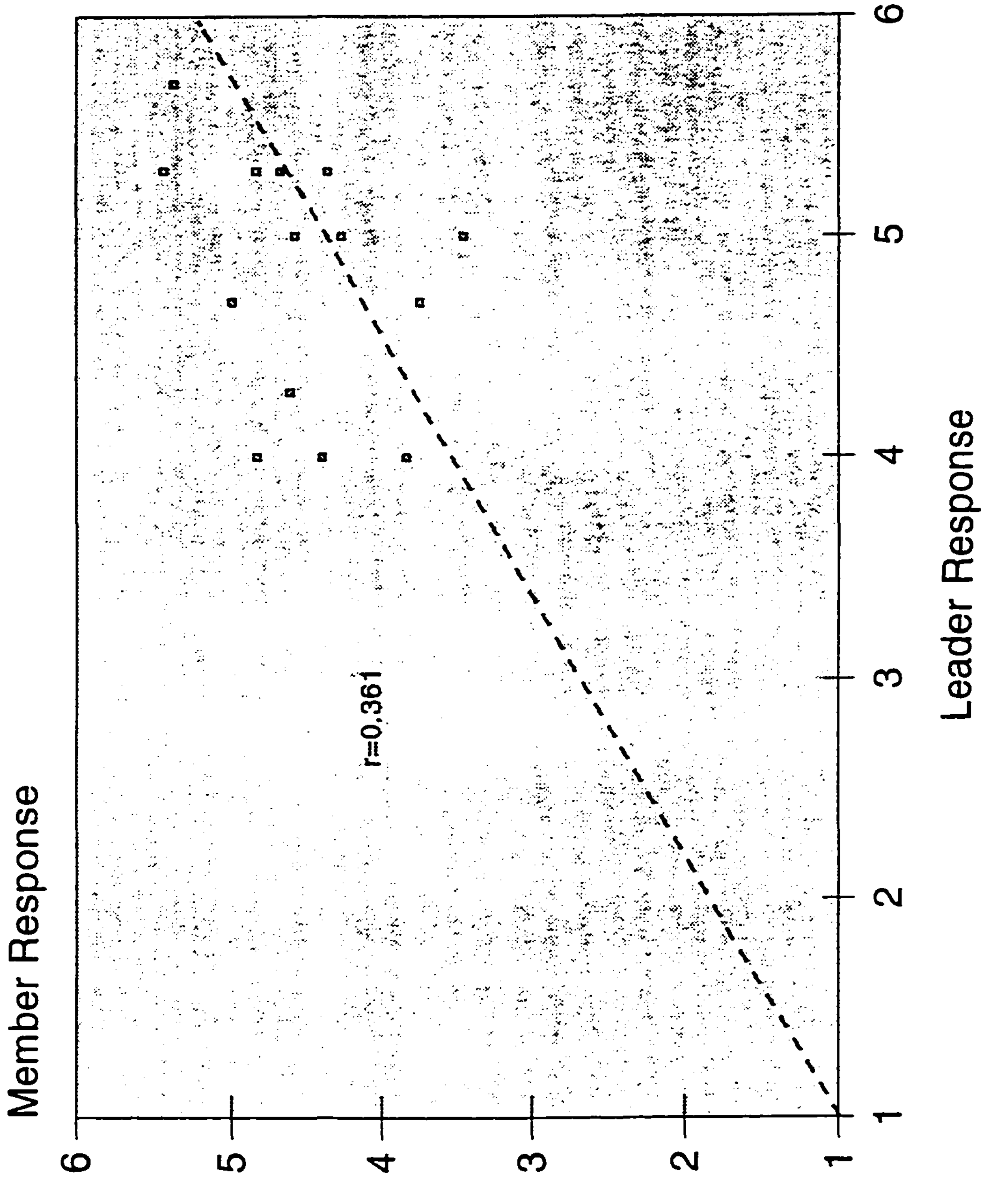
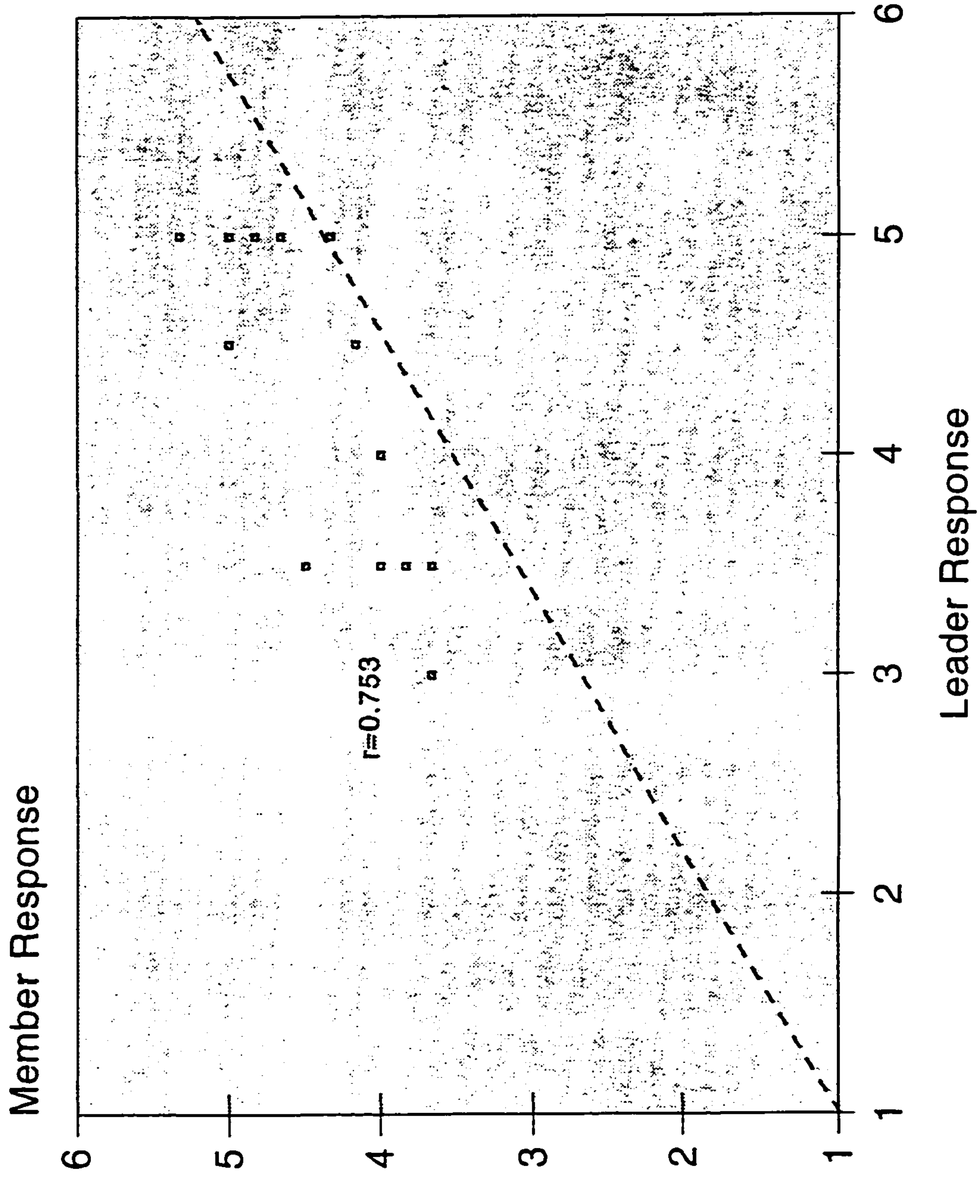

Member Response
Leader Response
r=0.361
1
2
3
4
5
6

Figure D3. Group leader vs. member perceptions of group communication.



Member Response
Leader Response
r=0.753
1
2
3
4
5
6

Figure D4. Group leader vs. member perceptions of team cohesiveness.

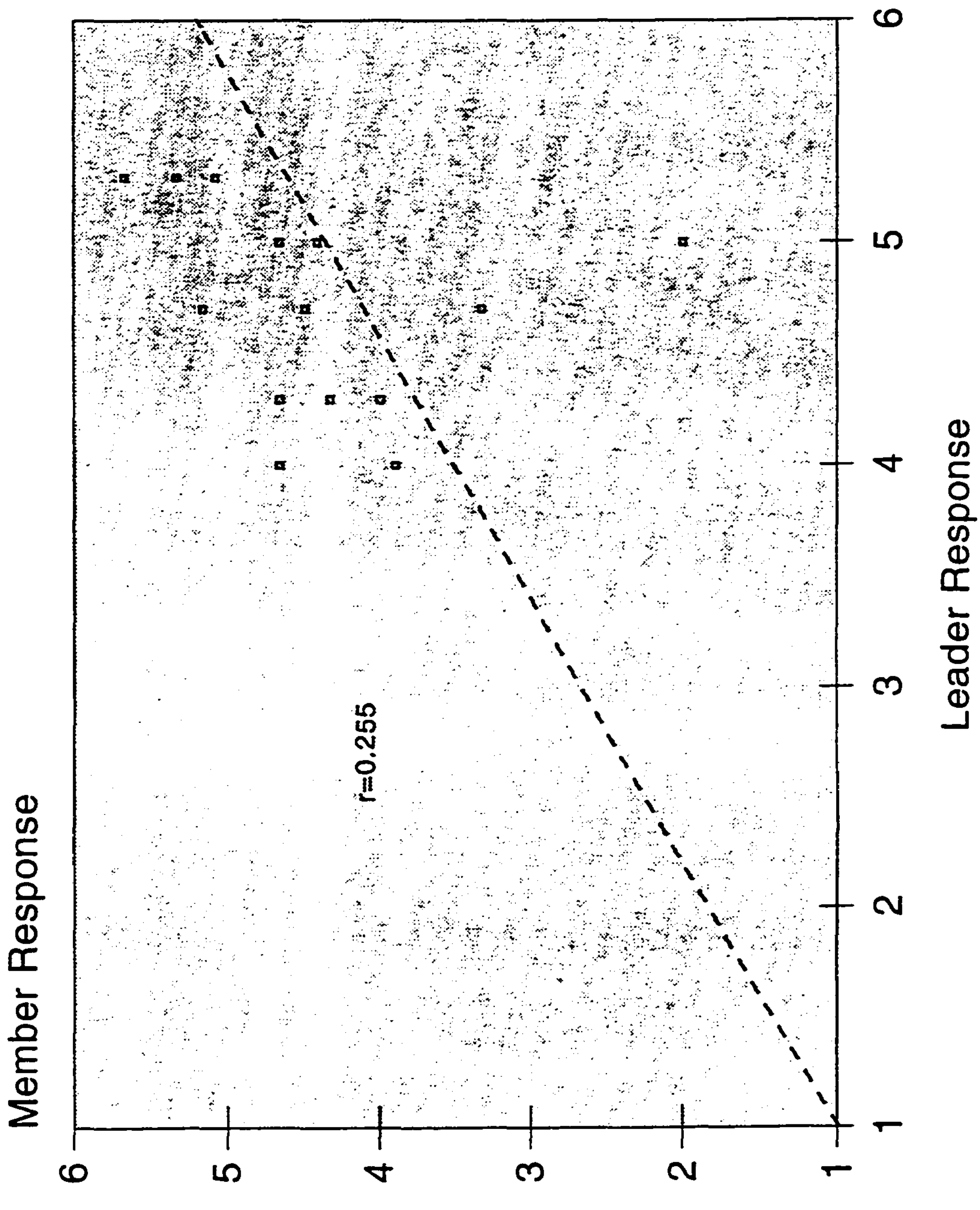

Member Response
Leader Response
r=0.255
1
2
3
4
5
6
1
2
3
4
5
6

Figure D5. Group leader vs. member perceptions of collaborative problem-solving.

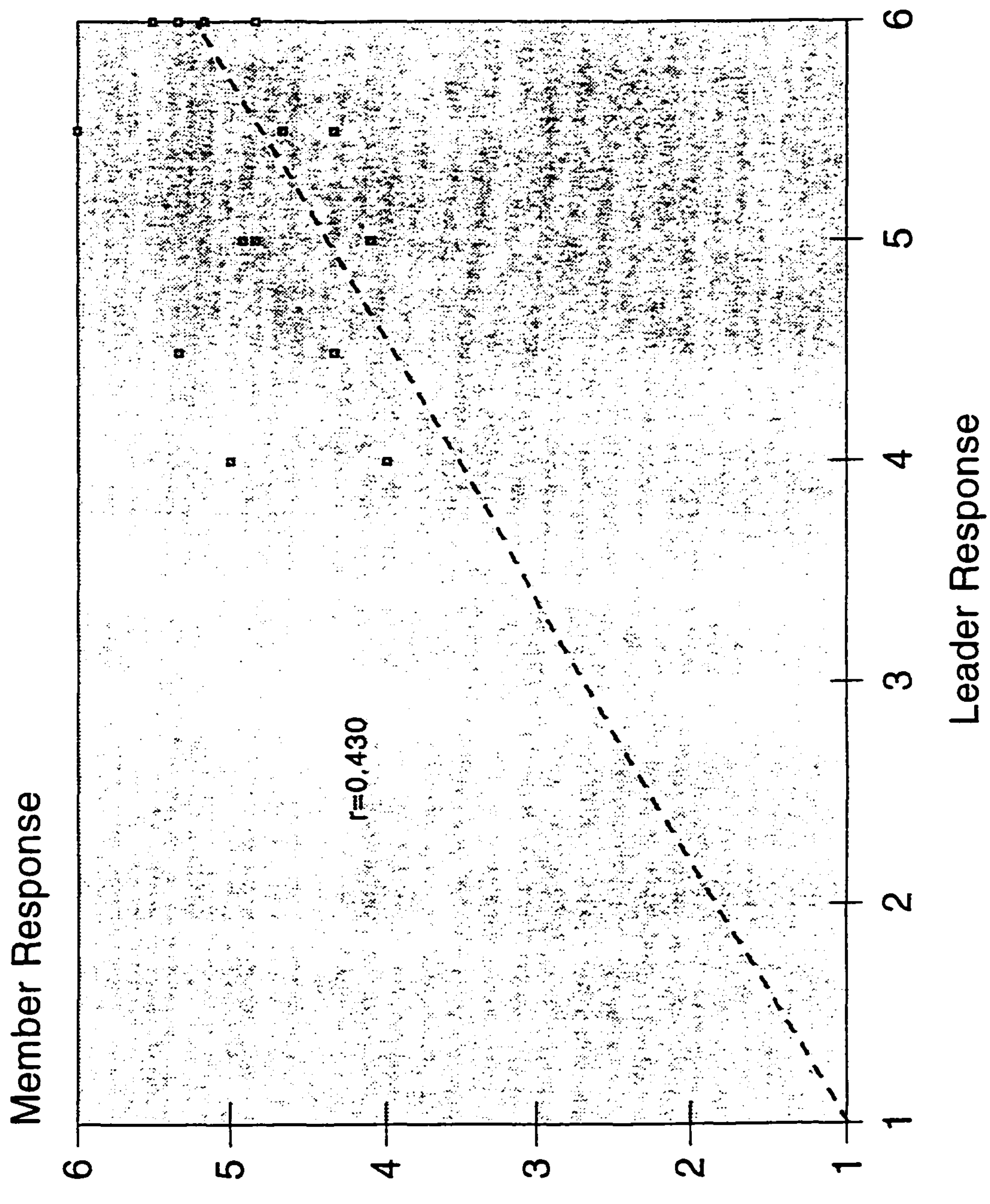

Member Response
r=0,430
Leader Response
1
2
3
4
5
6

Figure D6. Group leader vs. member perceptions of interdisciplinarity.

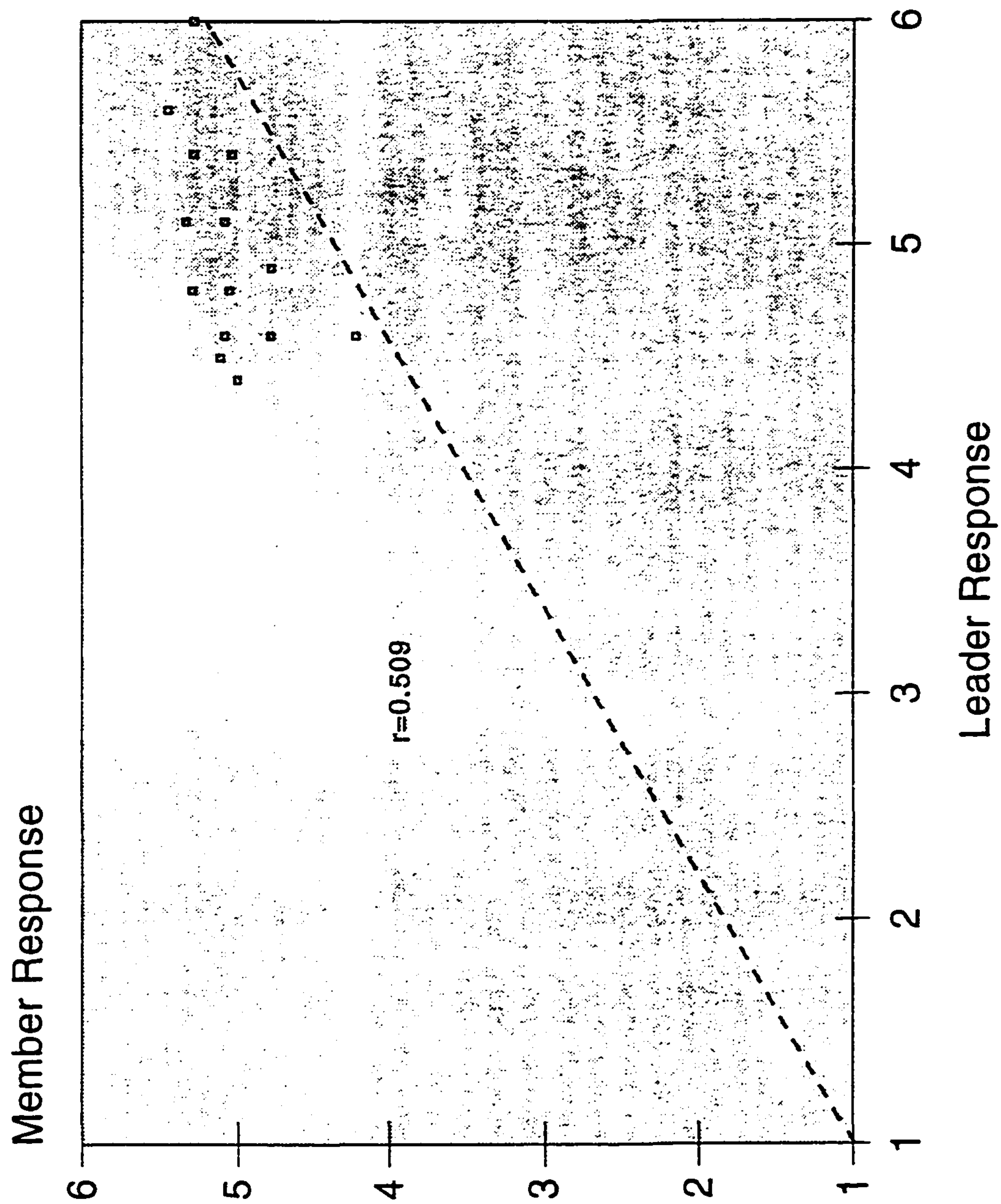

Member Response
Leader Response
r=0.509
1
2
3
4
5
6

Figure D7. Group leader vs. member perceptions of client involvement.

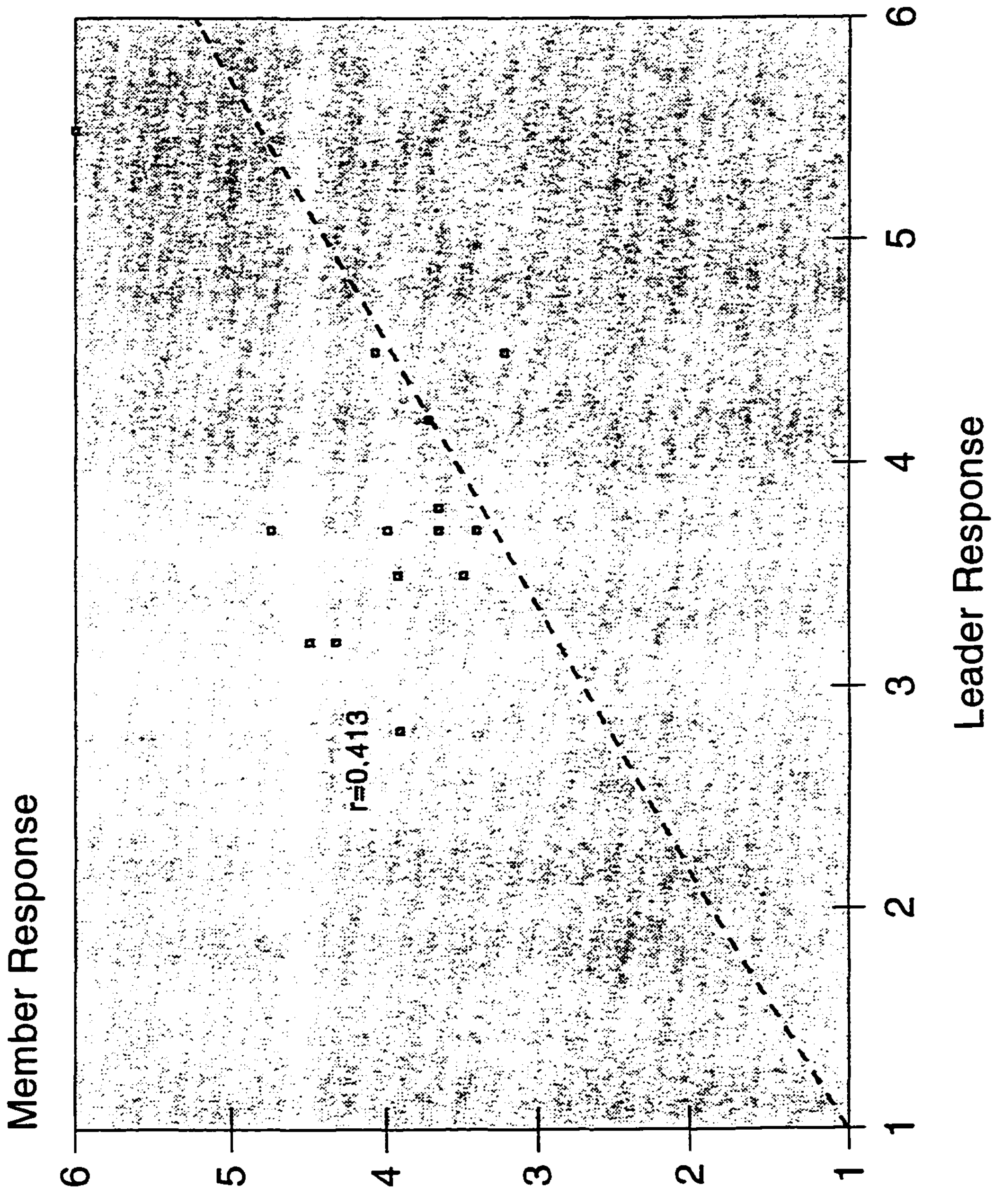
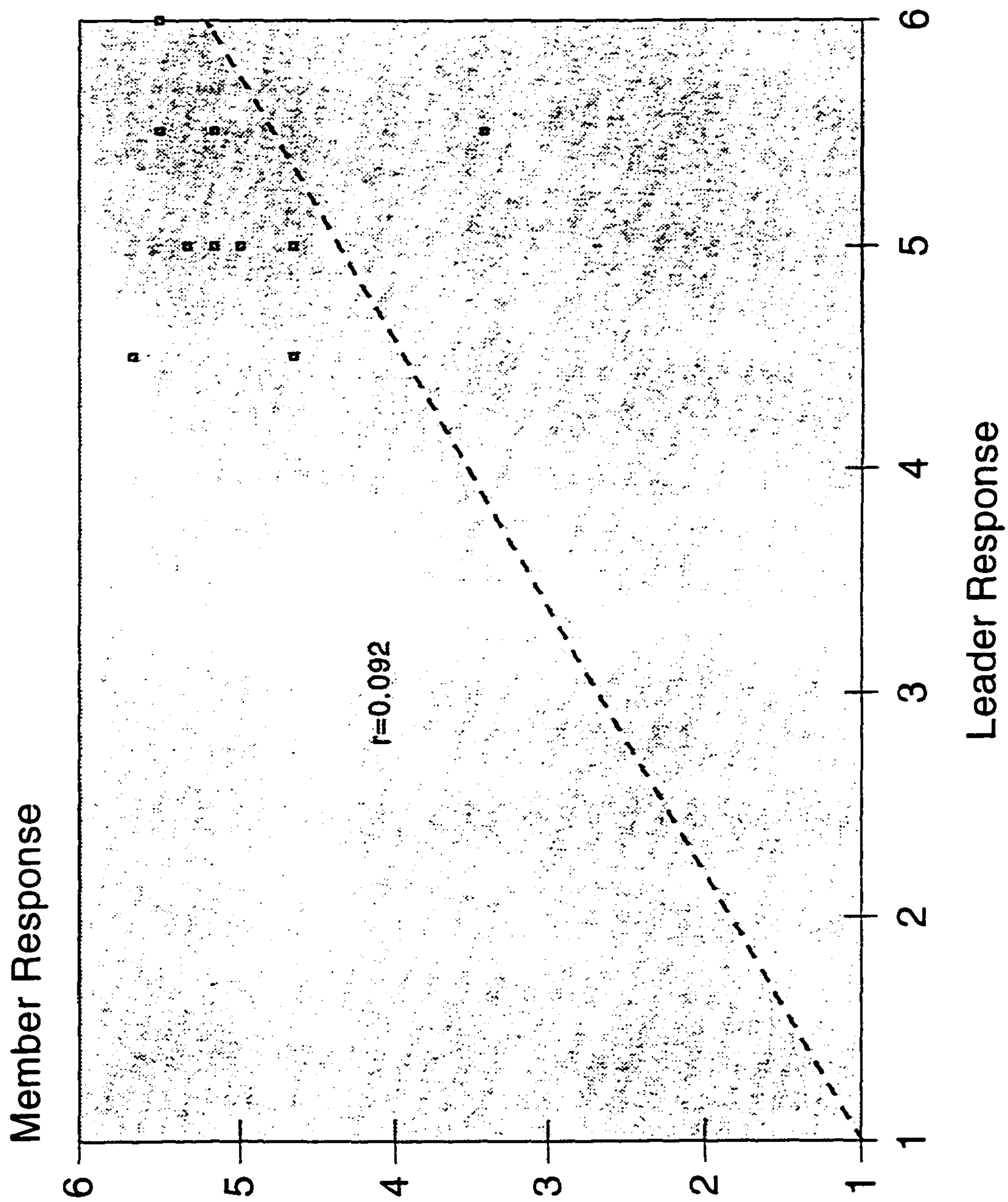

Member Response
r=0.413
Leader Response
1
2
3
4
5
6
1
2
3
4
5
6

Figure D8. Group leader vs. member perceptions of commitment to group goals.


Member Response
Leader Response
r=0.092
1
2
3
4
5
6

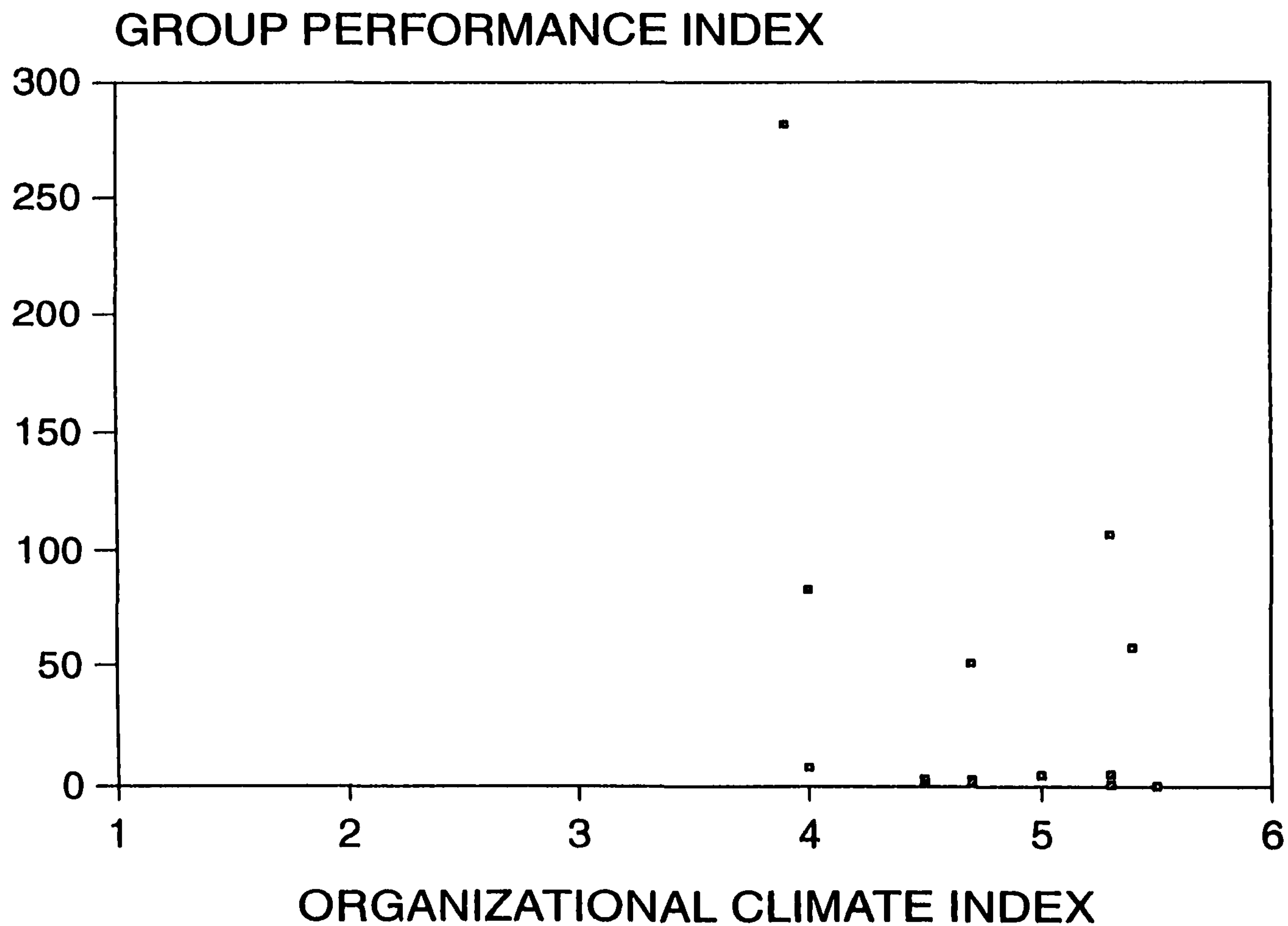


Figure D9. Scientific performance as a function of perceived organizational climate.

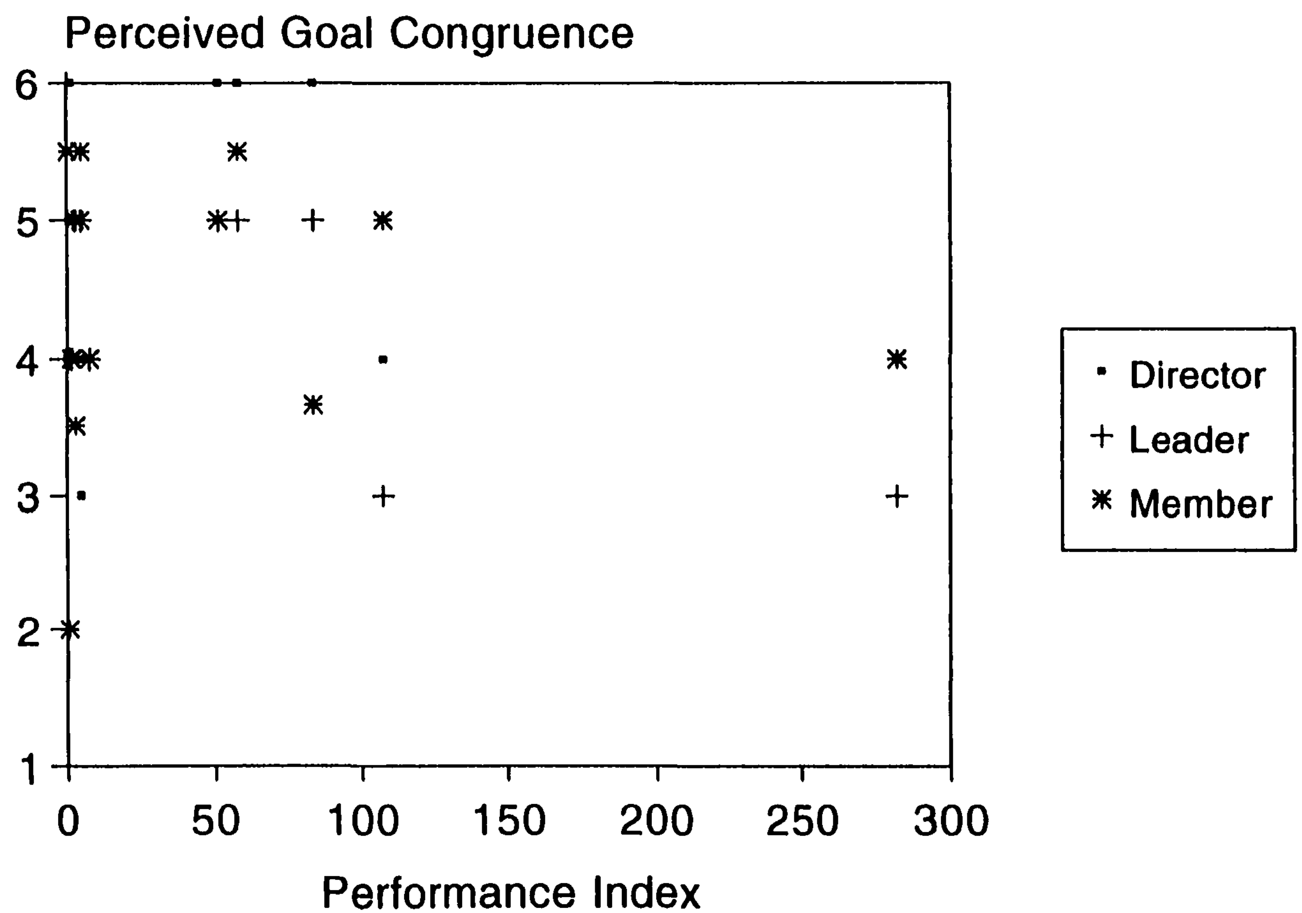


Figure D10. Measured scientific performance vs. goal congruence as perceived by laboratory directors, group leaders, and group members.

APPENDIX E

EPISTEMOLOGICAL CONSIDERATIONS IN GROUP RESEARCH

In Chapter II I raised issues concerning the group process difficulties found in research groups. A key issue with interdisciplinary research group members is understanding each others' approach to scientific inquiry, including problem definition, problem-solving, data collection, and inference processes. Because scientists with different disciplinary backgrounds often see problems in vastly different ways, research groups may have difficulty in reaching consensus on a research goal and the methods used to reach a goal.

Epistemology is both the theory of knowledge and a branch of philosophy concerned with the definition, nature, and criteria of knowledge. This chapter looks at the nature of scientific knowledge, the theory of scientific method, various systems of inquiry for obtaining knowledge, and the implications for managing a diverse portfolio of research activities. In particular, I discuss the epistemological aspects of interdisciplinary research groups and how such groups might reach knowledge goals. Finally, I define the concepts of value and quality in science and discuss their implications for research evaluation. The epistemological dimension of research management includes the concepts of scientific value, knowledge gain, validity, cogency, and truth, all of which relate to quality in science.

SCIENTIFIC KNOWLEDGE AND METHODS

The goal of scientific method is scientific knowledge (Kitchener, 1995) and science is seen as the process of knowledge growth. The central aims of science are concerned with a search for understanding, a desire to make nature not just predictable but also understandable (Toulmin, 1961). The theory of scientific method, or scientific methodology, can and should be discussed both descriptively and prescriptively. Descriptive is factual (what scientists do); prescriptive is normative (what scientists ought to do). A description of scientific inquiry, in relation to group research, follows. The prescriptive material is presented in Chapter V.

Scientific inquiry is a way of investigating things, events, and problems via the mind, the senses, and any mechanical or electronic extensions of the senses (Novak, 1964). It includes the formulation of conceptual models and theoretical frameworks. According to Simon et al. (1981), scientific inquiry may be theory-driven (logico-conceptual) or data-driven (i.e., Baconian induction or empirical). This assumes that scientific inquiry is a cyclical process.

According to Weimer (1979), four problem areas are fundamental to the understanding of the nature of science:

1. *Theory of rational inference.* What is the nature and justification of inductive inference?

2. *Theory and criteria of scientific growth.* How do we characterize and explain the growth of scientific knowledge?

3. *Theory of pragmatic action.* How do we rationally choose which theory to go with in scientific practice?

4. ***The problem of intellectual honesty.*** How do we match the actual practice of scientists with the theories they subscribe to?

The Origins of Scientific Inquiry

Toulmin's evolutionary model of science views scientific thought and practice as a developing body of ideas and techniques which are continually evolving in a changing intellectual and social environment (Toulmin, 1967). Tykociner (1966) likewise believed that scientific truth was dynamic and relative, but firmly based on a body of systematized knowledge. He defined scientific truth as a theory capable of interrelating all areas of knowledge into a consistent, integrated system.

Locke believed that the origin of all knowledge is the impressions made on the mind, either through senses or by the mind's own operations reflecting on those impressions. Having ideas and perception is the same thing. Ideas begin coming when a person first has sensation--ideas from sensation. Ideas of reflection are ideas derived from the mind reflecting on its own operation about the ideas obtained from sensation. Perception is the first step towards knowledge and the inlet of all knowledge in our minds. Locke believed that there are three modes of thinking: sensation, remembrance, and contemplation.

Whewell (1847) asserted that knowledge was composed of both thoughts (ideas) and things (observable objects and events). Scientific knowledge includes data, facts, concepts, propositions, and theories. Under Piaget's theory of knowledge, a scientific fact has three characteristics (Kitchener, 1986): it is an answer to a question and therefore is linked to conceptual analysis, it is a verification of experience, and it is part of a sequence of interpretations of experience. Facts require reevaluation because new theories and new interpretations constantly arise. Tykociner's (1966) system of systematized knowledge included data (a series of recorded events), facts (repeatable series of data), and classes (groups of facts showing common characteristics).

A scientific concept, according to Leary (1991), is a fundamental and factual unit of thought. Propositions are statements that have meaning and consist of concepts. A theory is a set of propositions or hypotheses used to state the workings of a thing or system. Theories are preconceived ideas (Davies, 1965). They are either unifications or generalizations and do not have any logical status. Two criteria must be satisfied by any satisfactory theory: the theory must unify and show the relation between previously unconnected quantities and it must be simple enough for critical experimental checks to be formulated. Theories are often confused with hypotheses, which are idea statements and simple knowledge claims. A hypothesis is a statement that refers to a pattern of an entire class of facts; a theory is a system of related hypotheses including some at the law level. Synthesis is recognizing patterns, forming concepts, structuring and manipulating information and ideas, and converting and transmitting ideas and information to other agents. Synthesizing ideas from information also produces knowledge.

Romesburg (1991) discussed two levels of knowledge in the natural resource and environmental sciences--knowledge at the level of constructs and knowledge at the level of isolates. Constructs are well-defined and directly-measurable concepts constructed to serve science. Isolates are special abstract constructs isolated from direct observation. An example of an isolate is the concept of niche in wildlife science. The transition from observed data to abstract theory requires creative imagination (Hempel, 1966). Scientific hypotheses and theories are not derived from observed facts, but invented in order to explain them. While hypotheses and theories may be freely invented and proposed in science, they can be accepted into the body of knowledge only if they pass critical scrutiny. Theories are usually introduced when previous study of a class of phenomena has revealed a system of consistencies.

Scientific methods are used to reach the goal of scientific knowledge and to solve problems. Kuhnian normal science consists of the strenuous and devoted effort to force nature into the conceptual boxes provided by one's professional education. Ordinary science is defined as experimentation within a current paradigm, often a ruling theory. Extraordinary science occurs during a paradigm shift. Funtowicz and Ravetz (1990) refer to this as post-normal science.

There is no such thing as *the* scientific method (Toulmin, 1961; Medawar, 1969, 1984; Mitroff and Kilmann, 1978; Romesburg, 1981; Simon et al., 1981; Rossini and Porter, 1981; Popper, 1983; Shapere, 1986; McRoberts, 1989; Kitchener, 1995). This is where many people, even practicing scientists, get confused. In fact, there are several valid methods for acquiring scientific knowledge and making inferences. Inductive methods rely on observation--e.g. surveys and direct observation of phenomena--to acquire knowledge and build inference. Inductive inferences have a high degree of uncertainty

(McRoberts et al., 1991). Deductive methods utilize experimentation and hypothesis testing. Knowledge is produced when tested hypotheses are either refuted or confirmed.

Scientific methods may also include making observations, performing experiments, deducing math proofs, constructing a model, applying a model to concrete problems, reasoning about the plausibility of a certain theory, attempting to clarify an important concept, a classification, or an entire conceptual framework. The "received view" holds that scientific method begins with observation. Datum is the end result of an observation. We now know that observation is not completely free of theory. It involves theoretical components and cannot exist without them. A theory is needed to direct and select scientific observations--not a formal theory, but a general idea or hunch about what factors are important and how they relate. Kitchener (1995) called these "primitive theories."

Why does science need an objective, systematic process of inquiry? Bacon's Doctrine of Idols held that both innate and extraneous idols tended to produce errors in reason (Spedding et al., 1968). The Idols of the Tribe derive from the human mind's erroneous tendencies due to human nature. The Idols of the Cave stem from each individual's unique character and personality, education, and beliefs. The Idols of the Marketplace are the errors caused by daily life and association with commerce. The Idols of the Theater are false philosophies due to artificial dogmas. Bacon advocated the use of deductive logic as the foundation of scientific method, but actually supported an inductive-deductive method.

Prior to Bacon, science was integrative and ecological. Holistic science, according to Shapere (1986), had its roots in Milesian philosophy (Miletus of the 6th century B.C.). Milesian philosophers deserve the reputation of founders of the knowledge-seeking enterprise. They embraced a holistic approach to science. A piecemeal or reductionistic approach to inquiry about nature assumed centrality in the scientific enterprise during the 16th thru 18th centuries. This led to specific bounded subject matters and relatively narrow domains of investigation. Bodies of theory developed within these domains. Much of our scientific progress is due to reductionism, but synthesis will be needed to solve many of society's problems. Synthesis is much more difficult to achieve than reduction, which is why reductionistic studies dominate the cutting edge of science (Wilson, 1998b).

Beginning with Bacon, the goal of science was developing knowledge to dominate and control nature. The Scientific Revolution replaced the organic view of nature with the metaphor of the world as a machine. Descartes believed in the certainty of scientific knowledge-- rejecting knowledge that was merely probable. Descartes was wrong. There is no absolute truth in science. All our concepts and theories are limited and approximate (Capra, 1982). Each theory is valid for a certain range of phenomena.

Descartes believed that knowledge was built from evident intuition and deduction. He advocated the use of deductive logic as the foundation for the analytical method, which led to the fragmentation of disciplines and widespread use of reductionism in science. The Cartesian approach has limited the directions of scientific research. Descartes believed that intuition and deduction are the only acceptable methods of arriving at knowledge. Intuition was defined as the undoubting conception of an unclouded and attentive mind. He also believed that all the sciences are conjoined with each other and interdependent. This is similar to Tykociner's (1966) zetetic system of knowledge.

Prior to Newton there were two opposing views of scientific method, represented by Bacon (empirical, inductive method) and Descartes (rational, deductive method). Induction refers to developing observational statements about specific events or things and then inferring generalizations (Churchman, 1971). Empiricism is the view that knowledge comes from and is reducible to experience. Locke and Hume both advocated empiricism, as did Piaget (Kitchener, 1986). Newton in his Principia said both methods, induction and deduction, were needed and unified the two views, developing the methodology upon which modern natural science is based.

All attempts to give a precise characterization of scientific method, whether experimental or logical, whether in terms of deductive or inductive logic, were essentially abandoned by the 1950's (Shapere, 1986). According to Toulmin (1961), it is fruitless to look for a single all-purpose scientific method because science has many aims and its development has passed through many contrasted stages. Science will always call for a broad range of different inquiries.

New philosophical analyses have emphasized that theories are radically underdetermined by observation. What are called scientific observations are far from being interpretation-free and are heavily theory-laden. Shapere (1986) suggested that "theory-ladenness" even predetermines the outcome of experiments. Studies of the history and sociology of science have revealed the presence of well-entrenched interpretation frameworks which guided the construction of evidence, observation, fact, explanation, theory and even determined the methodological rules, the criteria of scientific adequacy, the meanings of scientific terms, and the goals of science itself. The adoption of the piecemeal disciplinary approach to inquiry--the laying out of boundaries of specific areas of investigation--produced a standard against which theories could be assessed.

Through internalization along lines of specific disciplines, science has found it possible to achieve autonomy from external influences in building its future beliefs, methods, problems, rules of reasoning, explanatory patterns, standards, and goals (Shapere, 1986). It has happened because of a reliance on a body of background beliefs (paradigms) selected for their success in accounting for their domains of responsibility and in part for their coherence with theories of other relevant domains. Because the degree of autonomy is a function of the available background beliefs or paradigms, the information will to some extent be insufficient to guide the construction of new beliefs and research programs without a Kuhnian shift in direction. In other words, science tends to get stuck in paradigmatic ruts occasionally. Interdisciplinary group approaches to research may be one answer to avoiding or getting out of those ruts.

If facts underdetermine theories, then some other nonscientific considerations must enter in to fill the gap between scientific constraints and scientific belief. Since science is a social endeavor, then social considerations may be part of the gap. Shapere (1986) suggested that maybe everything about science is socially conditioned. Making decisions about what to study, what was relevant to the study, the appropriate methods for that study, and the character of an explanatory conclusion to the study all require learning how to learn about nature. If scientific inquiry is socially-conditioned, what are the implications for research groups and interdisciplinary group approaches to inquiry?

Students in the natural sciences have not been encouraged to develop integrative concepts. Most contemporary biologists tend to believe that a reductionist method is the only valid approach, and they have organized biological research accordingly. Research institutions direct their funding almost exclusively toward the solution of problems formulated within the Cartesian framework.

The Growth of Knowledge

Ignorance is endemic to scientific knowledge. Science has built-in ignorance due to limiting commitments and assumptions and tends to limit itself to a restricted agenda of defined, tractable uncertainties. Science favors a restricted agenda of defined uncertainties--those that are tractable--leaving out those that are not solvable by the existing framework of knowledge acquisition. Knowledge frontiers are pushed outward in directions favored by research sponsors and by the interests of individual scientists and disciplinary communities of scientists. The least attractive and least tractable research problems are left behind the frontiers, creating interstices of ignorance and uncertainty. How does scientific knowledge grow? There are several theories and models of scientific progress. Logical positivism, for example, is a theory of scientific progress where there is a gradual but continuous accumulation of factual observations and verified hypotheses (Cole, 1979). Tykociner (1966) used the term "zetesis" to describe the process of increasing knowledge. Kourany (1987) presented four models of scientific development:

1. *Cumulative.* A field of science progresses when it gains new facts, concepts, laws, theories through correct applications of scientific method. Hilpinen's (1989) accumulation theory of cognitive progress holds that truth is regarded as an aggregate of true propositions and progress occurs when one more truth is included in the belief system or when false propositions are replaced by true propositions. Wynne (1992) believed that scientific knowledge proceeds by exogenizing significant, tractable uncertainties. A similar view holds that new knowledge produced by research, including new data, theories, and discovery, provides new information about the range and likelihood of possible outcomes and therefore increases certainty about specific outcomes or reveals information about outcomes that were never previously anticipated (Ince, 1989).

2. *Evolutionary.* Current theories are replaced by new theories. Scientific progress is not made by the accumulation of routine results. It is made by discoveries--research results that make a significant change in what we thought we already knew (Ziman, 1987). This is somewhat similar to the evolutionary model of scientific growth advanced by Toulmin (1967). Kitchener (1996) introduced a related concept of knowledge growth he termed "epistemic stages," each stage representing an equilibration of scientific knowledge development. Transitions from one epistemic stage to another occur because the earlier

stages are less adequate in terms of problem-solving or explanatory power, capacity to attain goals and satisfy needs, and ability to answer questions. Each stage has new cognitive characteristics and is a reorganization of the knowledge previously acquired (Kitchener, 1986).

3. *Revolutionary.* Knowledge progresses by way of radical replacements of theories, facts, methods, goals. These replacements could also be called "Kuhnian shifts" from Kuhn's (1970) ideas on shifting scientific paradigms.

4. *Gradualist.* There is limited and gradual replacement of theories, facts, methods, and goals. According to Toulmin (1967), the factors that determine which intellectual variants are selected out and incorporated into the stream of scientific thought are determined predominantly by the professional values and aspirations of the community of scientists in question. But we cannot ignore the historical development of ideas and the evolution of the processes of hypothesis formulation, testing, verification, and refutation. So Toulmin (1967) would say that science develops as the outcome of a double process: a pool of competing intellectual variants is in circulation and a selection process occurs by which certain variants are accepted and incorporated into the branch of science in question.

Hilpinen's (1989) convergence theory assumes there is distance between a belief system and the truth. Cognitive progress takes place when the distance is decreased. Figure E1 shows this theory. $K_{existing}$ is the existing or unsatisfactory knowledge level and K_{goal} is the target or goal level. An inquiry process is employed to reach the goal level of knowledge.

$$K_{\text{existing}} \longrightarrow K_{\text{goal}}$$

Figure E1. In scientific research, an inquiry process begins with the current state of knowledge, K_{existing}, and progresses to a goal state, K_{goal}.

The measure of change from $K_{existing}$ to K_{goal} is dependent on the acceptance of new information and the avoidance or minimization of errors. It is easy to see how this epistemic model fits the case of disciplinary research, but a different model is needed for group research.

Knowledge growth is not necessarily a linear function. Tykociner (1966) saw the growth of knowledge as an asymptotic function converging toward completeness. However, the natural sciences have traditionally adopted a linear-additive model of science (Newby, 1992). The linear model of science represents a paradox, because the more we know about nature the more extensive our ignorance appears to be. Most models of knowledge growth are based on disciplinary science. What is needed is a new epistemological framework of how interdisciplinary research groups acquire knowledge. Wilson (1998b) talks about the borderlands, or places between the disciplines, where complex interdisciplinary problems reside. Our knowledge growth model, then, should assume that growth occurs both at the frontiers of science or leading edges of the knowledge webwork and within the interstices of that webwork. Both kinds of research activity are appropriate for interdisciplinary research groups.

INQUIRING SYSTEMS AND RESEARCH GROUPS

Is there such a phenomenon as group epistemology? Can an interdisciplinary research group acquire knowledge as a group? How does the knowledge differ from knowledge produced by single-discipline groups? If a problem solution or a research result is obtained through a group effort, is inference stronger than if an individual arrived at the same result? These are the fundamental questions that need to be addressed in considering how research groups acquire knowledge.

Science aims at establishing a body of knowledge, constructed on the basis of evidence. The goal of scientific inquiry is scientific truth (Poincaré, 1913). Scientific theories can never provide a complete and definitive description of reality--they are only approximations to truth. Scientific inquiry as it is now practiced is based on an epistemology of logical positivism, which is a philosophy that assumes that reality is objective and that it can be understood and reduced to a relatively small set of natural laws. Natural science investigates natural phenomena, which are perceived facts or events in space and time. Different kinds of knowledge, based on different underlying epistemological foundations, are needed for natural resources and environmental science (Clark, 1993).

Modes of Inquiry

The different modes of inquiry according to Churchman (1971); Rossini (1977); Lyles and Mitroff (1980); and Linstone et al. (1981) are:

1. *Lockean:* Empirical; agreement on observations of data; truth is experiential and does not rest on any theoretical considerations; careful control and scrutiny on the part of the inquirers, including accurate logs of procedures and observations; Churchman (1971) calls this community knowledge; involves members of a group reaching consensus on the formulation of a problem based on empirical evidence on the existence of the problem.

2. *Leibnizian:* Formal, but intuitive model; theoretical explanation; rational/logical approach; truth is analytic and does not rest on raw data of an external world; analytic knowledge obtained from deduction; seeks unity of science.

3. *Kantian:* Theoretical model and empirical data complement each other and are inseparable; truth is synthesis; multiple models and plurality of representations provide synergism. A consideration of multiple perspectives gives birth to a creative blend. The way the world appears to our observations depends very much on our basic theory about the structure of the world (Churchman, 1979). According to Churchman, we must conduct a theory of reality which will then guide us in the observations we make.

4. *Hegelian*: Dialectic confrontation between diametrically opposing models, definitions, or plans leading to resolution; the process of defending the status quo (existing paradigms) opposes the process of attacking the status quo; truth is conflictual as typified in a courtroom trial; holds that proven hypotheses and established paradigms should be challenged.

5. *Merleau-Ponty:* Reality is currently shared assumptions about a specific situation; acceptance of a new reality is negotiated out of our experience; truth is agreement which permits action.

6. *Singerian*: Pragmatic meta-inquiring system which includes application of the other systems as needed; the designer's psychology and sociology inseparable from the physical

system representation; ethics are swept into design; seeks to intensify the dispute between defenders and attackers of the status quo.

7. *Mill*: Eliminative induction; five methods of experimental inquiry to eliminate unlikely causes of events (Kitchener, 1995).

Teleological approaches are also recognized as a valid scientific method in natural resources and environmental research. Teleological approach means stipulating a target and assessing alternative policies as to how the target might be achieved (Dillon, 1976). Examples would include model development, technique or method development, and equipment development. Buhyoff and Leslie (1993) suggest that developing and improving methods for the delivery and use of research products is a part of scientific methodology. This is not just a question of information or technology transfer, but rather a question of development of methods which may permit the completion of scientific work of external validation and feedback, particularly when the inquiry involves the development of models.

There exists within science a variety of trial-and-error processes. The scientific method embodies such activities as designing and performing experiments, making observations, gathering data, inventing and developing instruments, formulating and modifying theories, reasoning about the plausibility of a certain theory, attempting to clarify an important concept, deducing consequences from theories, making predictions from theories, testing theories, discovering theoretical constructs, constructing a model, applying a model to concrete problems, classification, and others. Surveys, taxonomies, equipment design, systematic measurements, and tables all have their place in science as long as they are parts of a chain of precise logic about how nature works. In addition, we type research as to 1) basic science, 2) organization of science for application, and 3) science applied to immediate problems (McCain and Segal, 1988).

Scientific inquiry may be theory-driven or data-driven (Simon et al., 1981). Both data-driven and theory-driven give only partial views of the scientific enterprise. Blind laboratory exploration, with or without a theory framework, is a method of inquiry which may be described as basic research. Without a theory framework, basic research is serendipitous. Popper's concept of the natural selection (and elimination) of scientific theories is another process. Popper was the founder and leading advocate of natural selection epistemology. Popper believes that chance discoveries come too close to inductivist belief. Toulmin's evolutionary model of scientific development falls somewhere in the middle (Campbell, 1987). Under Toulmin's model, interdisciplinary group approaches to research should provide the competing intellectual variants (concepts, beliefs, interpretations) needed for science to evolve through selective retention and elimination of ideas. In an intellectually diverse group, the "gene" pool of ideas and perspectives is very rich.

Hume, the empiricist, believed that it is only by experience that the validity of synthetic propositions can be determined--not through *a priori* evaluation--but through comparison against empirical facts. Ayer (1952) takes this further and says that we test the validity of an empirical hypotheses by seeing whether it actually fulfills the function which it is designed to fulfill. According to Campbell (1987), knowledge claims must be testable and there must be mechanisms available for testing or selecting that are more than social, which is what differentiates science from other human speculations. The selective and eliminative system used to weed out among a variety of conjectures involves deliberate contact with the environment through experimentation and quantified prediction. The goal is to arrive at outcomes totally independent of the preferences of the investigator. This feature gives science its greater objectivity and its claim to a cumulative increase in accuracy in describing the world. This is *a posteriori* evaluation of hypotheses.

According to McRoberts (1989), the "method of science" consists of problem identification, discovery, and justification. McRoberts (1989) and Leary (1991) differentiate between discovery research, investigations that begin with observational data and end with conjecture, and justification research or hypothesis-generated research, which begins with a hypothesis and ends with an evaluation of the hypothesis. Discovery and justification are distinct processes, but form the two equally crucial parts of scientific inquiry (Beveridge, 1957). Most natural resource and environmental research is oriented toward discovery research (Leary, 1991). In discovery research knowledge begins with perception, proceeds to an analysis of parts, then to prescinding (isolating one of the parts), generalizing, and abstracting. The endpoint of discovery is the formulation of one or more hypotheses. Lee (1973) refers to these as steps or facets of inductive procedure. Discovery strategies include: trial and error, systematic search, serendipity, inspiration, illumination of the well-prepared mind, analogy, derivation from theory, induction, and retroduction.

Induction is the process of acquiring theoretic knowledge about the particulars of experience. Induction is defined as the ever-increasing accumulation of hard facts, and is better considered as a discovery phase activity than justification phase. Theories of glaciation, for example, are products of induction. Theories cannot be logically proven by empirical observations, but they can be refuted by them. Deductive consequences can sometimes be observationally verified. This corresponds somewhat with Popper who believed that unrefuted but corroborated hypotheses enjoy an epistemic advantage, independent of anyone's attitude toward them.

The Bayesian approach to induction suggests that the theories of science can be and ought to be appraised in terms of their probabilities (as opposed to deterministic theories). Bayesian inductive logic is an attempt to construct a logic of confirmation based on probability theory that incorporates the best of the hypothetico-deductive (H-D) method and modern statistical inference. The Bayesian theory is the only theory which is adequate to the task of placing inductive inference on a sound foundation (Howson and Urbach, 1993).

Justification research has two basic traits: identification of knowledge with proof and identification of knowledge with authority (Weimer, 1979). A knowledge claim cannot be accepted as genuine knowledge unless it can be proven and it cannot be proven except by submission to the appropriate epistemological authority. According to Weimer (1979), if knowledge is proven and certified under justification research, it remains the truth forever. Thus, there can be no scientific revolutions in justification research--only normal science growth. Discovery research, on the other hand, may result in scientific revolutions. Justification research has three parts: research strategy, empirical test, and inference. Justification begins with a set of hypotheses. A critical attribute of hypotheses is their testability. Justification strategies are classified on the basis of 1) logical intent and 2) number of hypotheses. Logical intent includes proof, corroboration, contradiction, and disproof. The H-D method is synonymous with justification research.

Several years ago I recall a Forest Service Experiment Station Director making the statement in a meeting that the H-D method was the superior scientific method. This was a bold statement to make without qualification. The H-D method, combining Popperian elimination of false hypotheses with a set of multiple competing hypotheses, may well offer strong inference in the case of experimentation. However, other methods are better suited and applied when constructing scientific theories and formulating hypotheses. Inductive processes are often used to “set up” longer-term H-D experiments. Inductivism by itself falls short of being an adequate scientific methodology (Medawar, 1969). Popper (1961) of course rejected the method of induction, which is inference that is based on deriving universal statements (hypotheses, theories, laws) from the results of observation or experiment. He argued that falsification by the deductive method is the correct scientific method.

Inference-making is drawing a conclusion presumably based on some evidence. The strength of inference depends on a) whether the conclusion follows from the evidence and on b) whether other conclusions are equally compatible with the evidence. Logic is concerned with studying the strength of the relationship between the premise(s) and the conclusion of an argument.

Premises--> Conclusion

(Strength or Degree of Justification)

Deductively valid arguments have the maximum amount of strength. Fallacies have zero amount of strength. Inductive logic involves some support.

Eberhardt and Thomas (1991) claim that much of what we know as the scientific method is based on the idea of experimental investigation of a hypothesis. They contrast observational studies, where the investigator has no control over the process being investigated, with an experimental approach (essentially the H-D method). The two approaches, although they may use the same mathematical procedures, differ considerable in the relative strengths of inferences as to cause and effect. Strong inferences are made possible by controlled experimentation, especially if replication is feasible. In discussing a classification of methods, they present an initial dichotomy: 1) conducting a controlled experiment vs. 2) observing some uncontrolled process by sampling. Experimentation on intact, functioning ecosystems is not really practicable in the classical sense of experimental design. Instead, parts of systems or model ecosystems are used. Most of the inferences about ecosystems based on this kind of approach are better described as conjectures.

Traditionally, the natural resources and ecological sciences were characterized by research approaches dominated by disciplinary functionalism and reductionism; an isolation of research from management and policy arenas; and funding incentive structures that fostered competition and independence over collaboration and interdisciplinary problem-solving. Reductionism is the idea that complex phenomena can be understood by reducing them to their basic building blocks and by looking at the mechanisms through which the building blocks interacted. The natural resource scientist's traditional role has been to conduct the research that provides the building blocks of knowledge and perform the synthesis of technical information which are used to construct foundations for natural resource management (Thomas, 1992). Most natural resources scientists see themselves as building the knowledge blocks, fewer others perform the synthesis, and fewer yet are directly involved in constructing the foundations for natural resources management. In short, reductionist approaches to science have dominated synthesis approaches. Reductionism has been so deeply ingrained in science, that it has been identified with the scientific method (Capra, 1982). Analytic science has the attributes of reductionism and assumed objectivity (Miller, 1993). According to Dillon (1976), reductionism fostered the proliferation of specialized deafness and tunnel vision through the building of independent disciplines. Strategies during the past 50 years for natural resources research were dominated by a mechanistic, reductionist view (Kessler et al., 1992).

Today we need management objectives that relate to desired future conditions in terms of ecological and aesthetic values and that sustain land uses and resource yields compatible with those ecological conditions. There are new synthesis disciplines emerging that are required to seek solutions to complex problems of ecosystem sustainability and human welfare. Holistic science can help to broaden the conceptualization of the research problem. Holistic science puts more emphasis on intuition, professional judgment, and imagination. It is not bound by empirical facts. Holistic and ecological views are also scientifically sound. A shift from reductionism to synthesis in research is partly responsible for the growth in interdisciplinary research (Kash, 1988). Thus, there is a role for both analytic and holistic science in natural resource decision-making.

Funtowicz and Ravetz (1990) suggested that post-normal science is a new scientific method to deal with complex problem-solving situations. The concept of uncertainty is at the core of post-normal science. The essential principle of post-normal science is that uncertainty and ignorance can no longer be expected to be conquered, but must be managed. For example, one role of science may be to draw causal connections between upstream policy decision options and downstream consequences of those policy options (Wynne, 1992).

A System of Scientific Inquiry for Research Groups

Research groups are usually faced with a researchable but complex problem requiring a unique solution. They have the advantage of defining a problem from several perspectives and considering many different possible factors bearing on the problem. Are research groups better suited for discovery research or justification research? Reduction or synthesis? For integrative problem-solving activities or broad, pioneering exploration? For inductive or deductive processes? It appears to be advantageous to have multiple, diverse contributions to hypothesis formulation, exploration, and *a priori* evaluation. However, a research unit is an open socio-technical system in an epistemological and societal environment (Herman, 1979). Group members bring to the table a variety of personalities, discipline paradigms, preconceived notions, and well-established scientific approaches to problems. There are different patterns of thinking. Even when different disciplines share a pattern of thinking, they may be working at different time or spatial scales. Thus, every inquiring system needs a theory of space (geometry) and a theory of time (Churchman, 1979).

Within the total volume of intellectual variants under question for a given research problem, what factors determine the lines of inquiry to be pursued? This is relatively easy to answer for single-discipline research. The lines of inquiry and methods of inquiry would be defined by the discipline's existing paradigms. We are concerned here with the initial formulation and plausibility of hypotheses--an inductive stage of hypothesis development--not their verification or elimination. With heterogeneous research groups, different methods and lines of inquiry are available. However, disciplinary paradigms may conflict. A discipline can be viewed as a culture. Does the group go with the dominant culture's method and line of inquiry? Or does the group develop its own method of inquiry? The more appropriate question is: What method and criteria does a group use in deciding on which hypotheses to keep and which hypotheses to abandon?

Interdisciplinary teams are relatively unstable organizational forms (Hautaluoma and Woodmansee, 1994). They require a great deal of up-front time and maintenance to function effectively. In order for a research group to function effectively, group members are faced with devising a method of inquiry they can all agree on. Coherence and integration within interdisciplinary research groups comes from a process of discourse and evolves to a consensus and convergence in terms of concepts and methods. The first step in interdisciplinary research is more careful discovery and specification of the assumptions in the various disciplines represented in the research group. Complex research situations require immersion in the methods and data of the disciplines involved (Keyfitz, 1993).

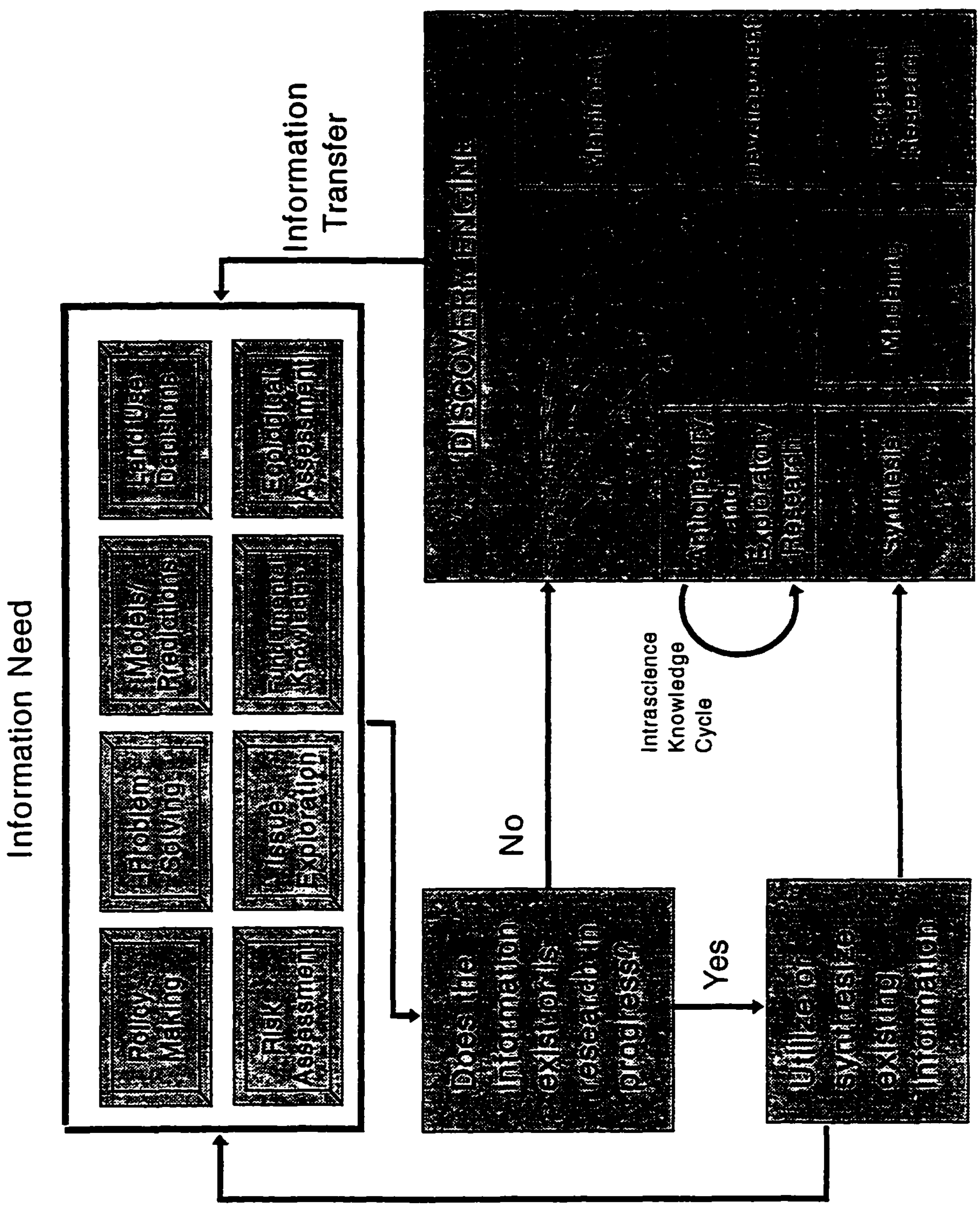
In order for the research group to function effectively (reach a knowledge goal), group members are faced with devising a method of inquiry they can all agree on. Coherence and integration within interdisciplinary research groups comes from a process of discourse and evolves to a consensus and convergence in terms of concepts and methods. The first step in interdisciplinary research is more careful discovery and specification of the assumptions in the various disciplines represented in the research group. Complex research situations require immersion in the methods and data of the disciplines involved (Keyfitz, 1993).

Knowledge to support renewable resources and environmental management must derive from two kinds of science: science of parts emerging from traditional reductionist, experimental science, and science of the integration of parts which derives from a whole-system perspective. A system perspective requires an interdisciplinary research environment (Kessler et al., 1992). A functional, reductionist approach to natural resources research is poorly suited to a management model that strives to sustain the diversity, complexity, and resiliency of ecosystems. The natural resource and environmental sciences require a “discovery engine” capable of delivering a variety of science products derived from the two basic kinds of science. Figure E2 shows a proposed discovery engine. The discovery engine is actually an iterative process of identifying information needs, determining whether or not the information exists or can be synthesized from available information, and generating the right kind of information to meet the needs of the research client.

A method of scientific inquiry designed for research teams is shown in Figure E3. This method produces strong inference through the use of multiple competing hypotheses and the application of the hypothetico-deductive approach. But it also relies heavily on inductive processes to generate, evaluate, and revise working hypotheses. The model of scientific inquiry shown in Figure E3 works for either individual or group research and replaces the simpler model in Figure E1. According to Rachelson (1977), scientific inquiry consists of both hypothesis generation and hypothesis testing. A complete operational model of scientific inquiry must include both.

Hypothetico-deductivism is a model of scientific inquiry devoted to the reasons for accepting or refuting a hypothesis once it is formulated. The H-D method has not been concerned as much with the generation of hypotheses as it has with the testing of hypotheses (Popper, 1961; Medawar, 1969; Rachelson, 1977).

Figure E2. The discovery engine.


Information Need
Policy Making
Problem Solving
Models/ Predictions
Land Use Decisions
Risk Assessment
Issue Exploration
Fundamental Knowledge
Ecological Assessment
Information Transfer
Does the information exist or is research in progress?
No
Yes
Utilize or synthesize existing information
Intrascience Knowledge Cycle
DISCOVERY ENGINE
Anticipatory and Exploratory Research
Synthesis
Modeling
Targeted Research

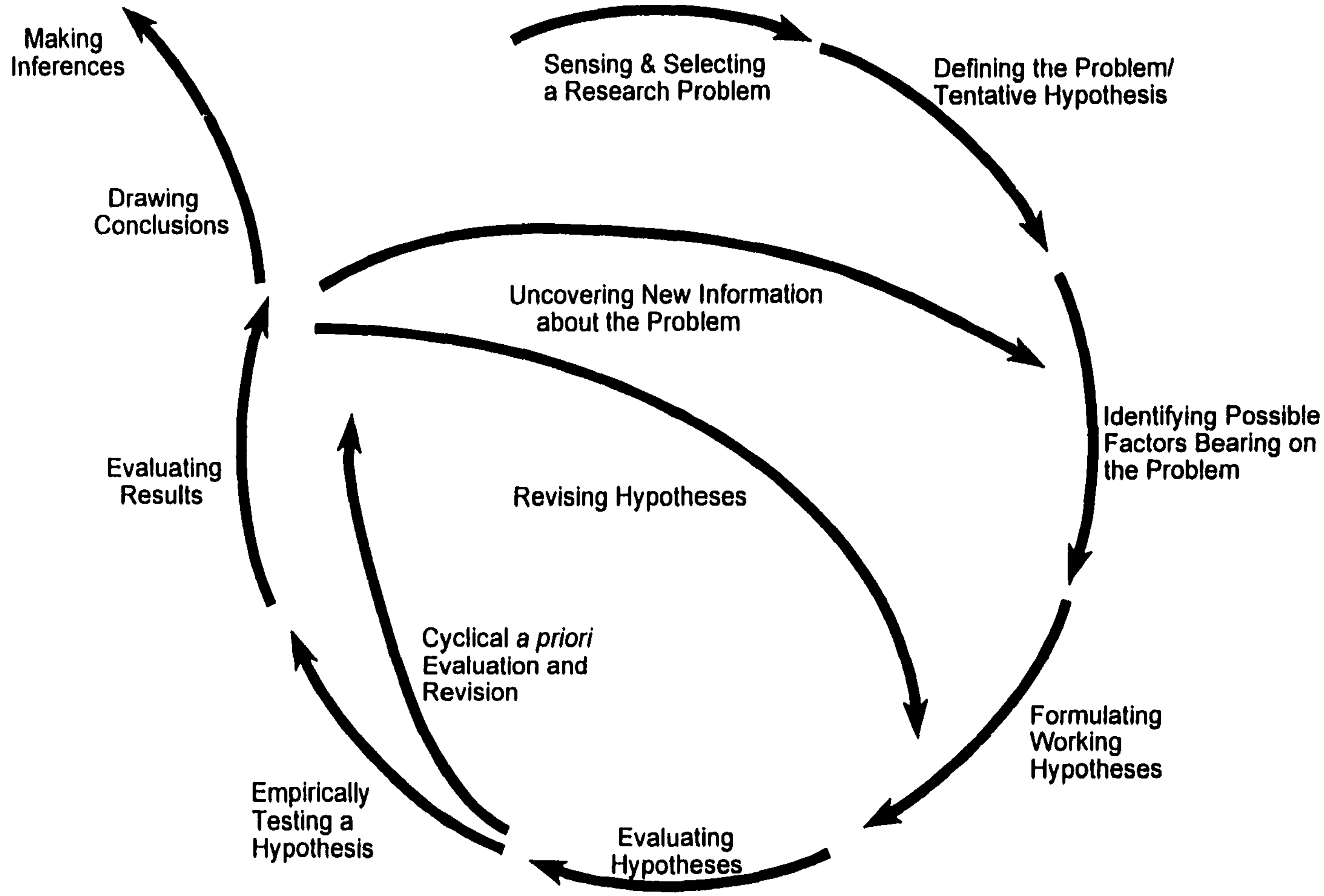


Figure E3. A hypothetico-deductive method of scientific inquiry.

This is a weakness of the H-D method. According to Weimer (1979), Popper did not consider how conjectures or hypotheses to be refuted are created in the first place. Popperian logic needs to accept some amount of inductivism. Induction does not follow logical rules but comes from insight, imagination, inspiration, and intuition. This is an aspect which has been neglected in science. Inspiration and intuition were viewed by the inductivism and hypothetico-deductivism schools of thought as being too closely related to religious and mystical ways of knowing. It is obvious from Figure E3 that the H-D method of scientific inquiry relies heavily on inductive thinking. A group approach benefits the discovery phase of inquiry through greater illumination of the problem, many more possible hypotheses, and the benefits of shared intuition and inspiration. Most of the steps involve inductive processes and therefore lend themselves to group research approaches.

A plausible hypothesis requires verification through the test of experience. This is the heart of the H-D-deductive method. Within the H-D method, theory refers to a broad, general conjecture about a process (Romesburg, 1981). The full-blown research hypothesis is a specific but primitive theory that is intended for experimental test. Hypothesis testing consists of cognitive activities which expose the hypothesis to experience, usually by experimentation. The *a posteriori* testing of theories is a deductive process. According to Popperian logic, all testable hypotheses must be falsifiable. Popper (1961) offered four ways to deductively test theories: 1) logical comparison of conclusions among themselves; 2) investigation of the logic of the theory; 3) comparison with other theories; and 4) empirical applications of the conclusions derived from the theory.

Figure E3 assumes that scientific inquiry is a cyclical process: data gathering, description, explanation, theory testing, and back to data gathering (Simon et al., 1981). The H-D-deductive process involves continuous feedback from inference to hypothesis (Medawar, 1969). In the physical sciences, according to Lee (1973), observed data combined with a problem or question suggest a working hypothesis. Further analysis of the relevant data yields a modified hypothesis. There may be several cycles of data analysis (or *a priori* hypothesis evaluation) and hypothesis revision, resulting in a full-blown testable hypothesis as shown in Figure E3. Scientific inquiry is a self-correcting, revisionary system.

Scientific inquiry often begins with observations guided by a rudimentary theory or simply by intuition. The receptacle theory of knowledge" says that knowledge streams into us through our senses. The "activist theory of knowledge" says that we must actively engage ourselves in searching, sensing, comparing, unifying, and generalizing (Popper, 1966). In either case, scientists engage in observations of events and objects in nature. The scientific observation continuum of Lee (1973) holds that observations become increasingly more explicit between perception and actual counting or measuring:

Inferences made ------------> Direct perception-------------> Counting/measuring
from perceptual data | of fact

Inferences made from perceptual data	------------> Direct perception of fact	-------------> Counting/measuring

An example can be taken from a common earthquake effect--a crack produced in the surface of the earth. If one observes a crack in the ground following an earthquake and the crack did not exist prior to the earthquake, the observation that it is an earthquake-produced surface crack is based on an inference made from perceptual data. On the other hand, if one observes the crack developing during an earthquake, that is a direct perception of fact. A more explicit observation would be the actual measurement of the width and length of the crack.

If followed using the accepted rules of logic, the method of scientific inquiry shown in Figure E3 will result in strong inference. Strong inference consists of the following steps: 1) devising alternative hypotheses; 2) devising a crucial experiment with alternative possible outcomes, each of which will exclude one or more of the hypotheses; 3) conducting the experiments so as to get clean results; and 4) recycling the procedure, creating subhypotheses or sequential hypotheses to refine the possibilities that remain (Platt, 1964). Scientific inference is the movement of thought from the known to the relatively unknown (Lee, 1973) or the bridge between observation and theory (Howson and Urbach, 1993). Strong inference is similar to shaping a tree by pruning-- undesirable paths for future growth are eliminated by tests that falsify competing hypotheses. This is an explicit logic of inquiry.

Mature theories have developed from a progression of ruling theories, working hypotheses, and multiple working hypotheses. These stages represent a succession from the defense of favored ideas to the utilization of strong inference (Platt, 1964). At the earliest stage of methodological development, plausible explanations are adopted as theories and when rigorously defended become ruling theories. The use of multiple, competing working hypotheses coupled with the logic of strong inference represent the most advanced stage of research.

Cognitive Processes, Frameworks and Styles in Research

According to Tykociner (1966), cognitive processes include bare observation, experimentation (or controlled observation), analysis, synthesis, imagination, supposition, ideation, comparison, and analogy. Simulation and modeling could be added to this list. Herman (1979) referred to four methodological poles of scientific investigation:

1. Epistemological (perception of problems)
2. Theoretical (definitions of concepts and propositions)
3. Morphological (articulation of hypotheses and theories)
4. Technical (refinement of data, tests of conjectures)

He also identified four methodological paradigms, including hypothesis testing, statistical analysis, logico-deductive analysis, and modeling, as a typology of epistemological approaches used in science. One could add induction to a list of methodological paradigms used in natural resources and environmental research. According to Romesburg (1981), induction is employed widely in wildlife science and is useful for finding possible relationships between classes of facts.

Although the four methodological poles of scientific investigation apply broadly across all of the sciences, methodological paradigms can usually be attributed to specific disciplines.

Holzner and Fisher (1979) call these paradigms "frames of reference." Frames of reference are the underlying structures of assumptions, dispositions toward certain decision rules in inquiry, and expectations which form the contexts in which inquiry proceeds. They are highly codified or standardized in the scientific disciplines, anchored in strong emotional investments of the individual scientist as well as the relevant disciplinary community, and are not easily modified or abandoned.

Petrie (1976) used the term "cognitive maps" to refer to the different frames of reference. Cognitive maps are the paradigmatic and perceptual apparatus used by a discipline and include basic concepts, modes of inquiry, problem definition, observational categories, representational techniques, standards of proof, types of explanation, general ideals of what constitutes a discipline, values, and orientation to the future. Pearson (1983) recognized that members of interdisciplinary research groups usually have different cognitive frameworks in addition to different personalities and that both influence the success interdisciplinary research.

Cognitive style, on the other hand, refers to the characteristic way in which people approach problems and reflects individual consistencies in style of thinking and reasoning (Miller, 1983). Cognitive styles are persistent and automatic and are embedded in individuals' personalities. A person will use a consistent strategy in approaching and solving problems. According to Miller (1983, 1985), the axes of cognitive style are objective-subjective and analytic-holistic. The cognitive styles are analytical scientist, systems theorist, humanist, and mystic. There are epistemological and political attitudes associated with each of the four cognitive styles. Thus, differences in both cognitive frameworks and styles help to explain why there is so much conflict between specialists and why there is resistance to the adoption of new problem-solving strategies within research groups.

Mitroff and Kilmann (1978) classified scientists as convergers and divergers. Convergers are realistic and reductionistic while divergers are idealistic and holistic. The authors looked at several classification possibilities, including Jung's, and developed four methodology types--analytic scientist (which corresponds to Jung's sensing-thinking quadrant), conceptual theorist (Jung's intuitive-thinking orientation), conceptual humanist (Jung's intuitive-feeling quadrant), and particular humanist (Jung's sensing-feeling quadrant).

Schreyer (1974), in his study of Forest Service research scientists, defined "cognitively simple" as specialized, narrow, inductive approaches to research; "cognitively complex" approaches included broad areas of research, several facets of a research problem, and deductive approaches. Herrick and Jamieson (1995), on the other hand, divided scientists into theorists and experimentalists. Pelz (1967) found that high-performing, creative scientists appeared to alternate between narrow, penetrating investigations (specialization or converging) and broader, expanding explorations (pioneering or diverging).

Understanding complex natural resources phenomena or systems requires diverse disciplinary approaches, frameworks of analysis, and ways of knowing (Bengston, 1994).

To encourage diversity and disagreement, a research administrator could form groups that include individuals who like each other but who use different cognitive strategies. Interdisciplinary group research must be approached with the attitude that paradigms may be significantly different and that such differences are desirable (Bella and Williamson, 1976). Group heterogeneity provides different paradigms, inquiring systems, and social values (Rossini and Porter, 1981).

However, intellectual distances among disciplines can create social-psychological barriers for participants in interdisciplinary research (Havick and Kelly, 1977). Group members must learn each others' cognitive maps. A failure to learn or at least appreciate each others cognitive styles may explain why many interdisciplinary research attempts do not succeed. I observed this phenomenon while working with interdisciplinary river assessment teams in the 1980's. As a result, groups may apply inappropriate methods to an inadequately formulated problem. A more adequate approach might be the integration of several cognitive styles. However, integrative thinking is difficult to achieve. According to Parker (1993), a lack of common conceptual frameworks was listed as one of the problems contributing to the failure of interdisciplinary research. The question of how to arrange cooperation among inquirers of different frames of reference within institutions is a critical part of interdisciplinarity (Holzner and Fisher, 1979). At the very least, group members must learn the observational categories and meanings of key terms used by the other group members (Petrie, 1976). Frameworks for linking disciplines or paradigms include common group learning, models, negotiation among experts, and integration by leader (Rossini and Porter, 1981).

In Pursuit of Group Knowledge: Multiple Perspectives, Knowledge Goals, Perceptual Alignment, and Problem Formulation

Method is at the heart of any science (Mitroff and Kilmann, 1978). The fact that different disciplines approach scientific inquiry in different ways may represent an obstacle for an interdisciplinary research group (Kuflik, 1992). Differences in basic epistemology may be difficult or impossible to resolve, especially when it comes to choosing among competing fundamental paradigms or research approaches, objectives and methods (Roberts et al., 1984).

The problem of research integration has its roots in epistemological differences that arise from and because of the different disciplinary perspectives represented on a research team (Rossini, 1977). Disciplines differ in the way generalizations are made (Gold and Gold, 1983). Ackoff (1962) suggested that research teams may contain dichotomies in the form of rationalists (theoricists) and empiricists (realists). Discovery and proof are distinct processes and different attitudes of mind are required for each (Beveridge, 1957). A research group may have little trouble working through discovery research, but then run into obstacles in justification research because experimental design and the validation of hypotheses are intraparadigmatic--they operate only within the framework of a single perspective (Linstone et al., 1981).

Normal science requires convergent thinking and ill-defined problems require divergent thinking (Taylor, 1986). The need for both convergent and divergent thinking results in an "essential tension" implicit in scientific research (Kuhn, 1977). The research group, more so than the individual scientist, must display both characteristics. The effectiveness of small group problem solving is a function of how a group manages or takes advantage of its similarities and its differences (Mitroff and Mason, 1981).

My experience with the river study teams suggested that these obstacles and the challenge in aligning scientific approaches were surmountable if members were open-minded, willing to listen, and there was sufficient time allotted for learning each others' methods. Williams (1976) suggested that team members must be selected who are sympathetic to other disciplinary views and who recognize the potential contributions of the other team members.

Multiple perspectives are critical in developing insights on complex sociotechnological problems, but different perspectives also mean different planning horizons. Linstone et al. (1981) use the analogy of different illuminating distances of mixed beams of light. For multiple perspectives, groups must be interparadigmatic rather than merely interdisciplinary, group members must have been nurtured on different inquiring systems. Group process design is equally important--conflict through interaction and conflict resolution leads to the generation of new insights. The group effort cannot be fast-paced; members need time immerse themselves in the subject matter. Complex situations require immersion in the methods and data of the disciplines involved (Keyfitz, 1993). Challenge in a scientific group comes from intellectual conflict and the questioning of ideas, while maintaining a climate of collaboration. Pelz (1967) referred to intellectual jostling as "dither" and says research teams need a certain amount of dither to stimulate each other.

The best type of dither or challenge appears to be differences in technical strategy or approach. Effective groups have reported both personal harmony and intellectual conflict.

Methodological pluralism is based on the premise that no one method is able to completely explain all of the known facts in a problem domain (Hetherington et al., 1994). We have learned that problems are approachable from many different directions (Ziman, 1994). As a framework of analysis, methodological pluralism promotes participation and decentralization in scientific inquiry and advocates the uncritical use of multiple methods, disciplines, and perspectives. Broader and less defined questions, such as those found in the natural resources and environmental arenas, can only be pursued through multiple, overlapping analysis and extensive discussion between diverse experts and the people directly affected. Progress may be striking if scientific knowledge can be fitted together into a coherent, multidisciplinary conceptual scheme.

It is necessary for team members to communicate with one another in each of four elements: research problem, conceptual model, scientific model, and solution stage, with the conceptual stage being the most important (Swanson, 1979). In the mutual learning process of a diverse group, the bridge between disciplines takes place through construction of shared models. According to Norgaard (1989), methodological diversity weaves a patchwork quilt of thinking. This is consistent with the view of Tykociner (1966), who believed that all the sciences were conjoined with each other and interdependent (probably also a view of Descartes).

Tykociner (1966) in discussing research teams asked "can there be no doubt that by uniting the endeavors of all participating members in a rational way, such teams can more effectively produce new knowledge, develop inventions, and achieve a higher level of understanding?"

How do interdisciplinary research groups agree upon a knowledge goal when members come to the group with a variety of perspectives and personal aspirations? First, it must be assumed that group members have made the commitment to participate in the research effort as a team. Second, group members must have a common understanding of the client's or research sponsor's expectations. Third, the group must adopt a superordinate goal (Hautaluoma and Woodmansee, 1994) and a common system of inquiry during the research planning stage.

Group members usually come into a group with individual knowledge goals based on their unique perception of what the research project is supposed to accomplish. As the group begins to communicate and to function as an effective team, perceptual alignment occurs as demonstrated in Figure E4. Perceptual alignment is the stage at which participating players understand each other (Molina, 1994). The initial goal structure of a research group typically resembles that shown in Figure E5. At G_1 the research group is in the initial stages of defining the research problem and each member has his/her own research goals. individual goals coalesce into a single group knowledge goal. This does not infer that individual goals have disappeared. They may well be, and often are, hidden in the background. How well individual members can articulate the group knowledge goal is a

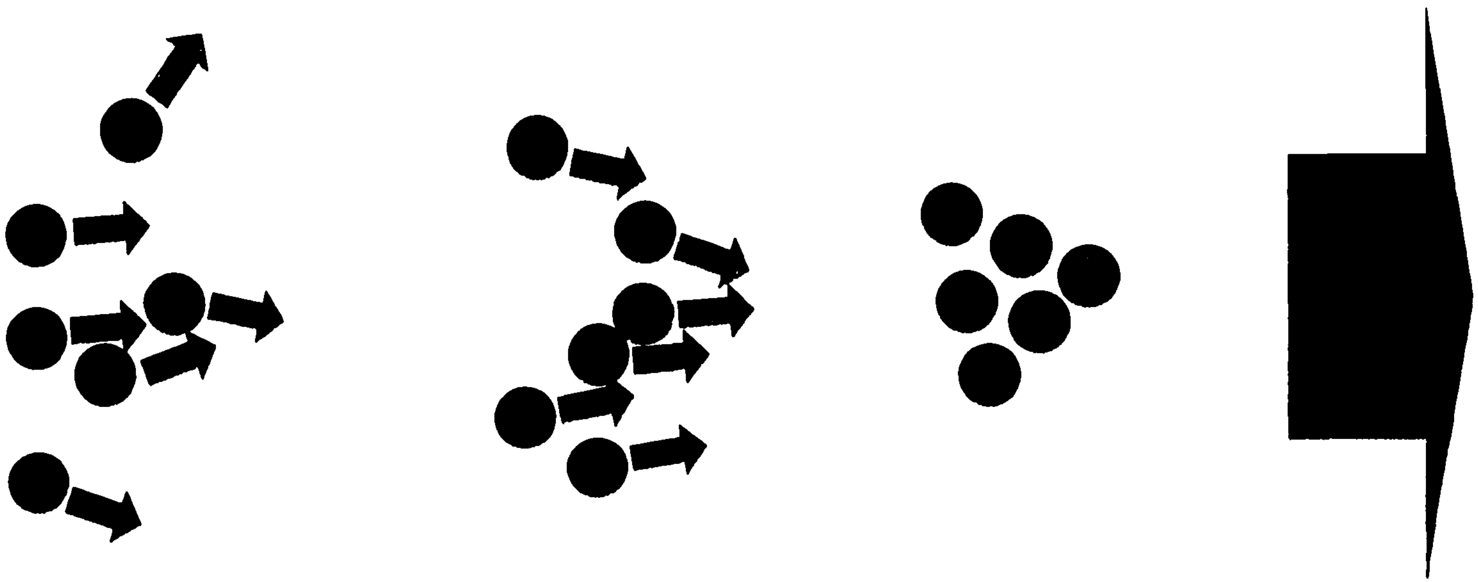


Figure E4. Perceptual alignment within a working group.

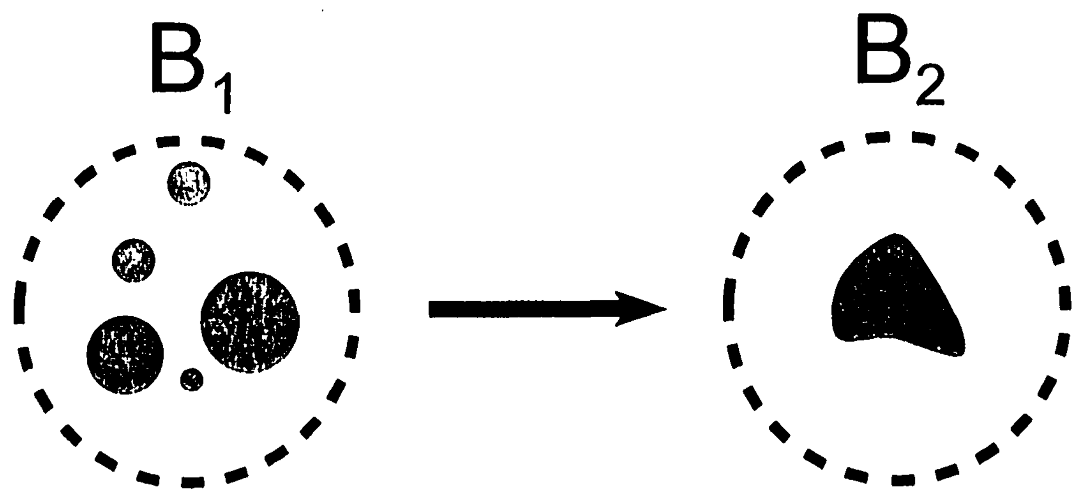


Figure E5. Evolution of group knowledge goals.

function of group effectiveness. "Corporate paradigms" are needed which can direct heterogeneous groups toward established group goals without restricting the individuals' or group's potential creativity (Bella and Williamson, 1976).

Problem Domains and Research Goals

Research problem domains, as depicted in Figure E6, are major research programs undertaken by individual scientists, research groups, or entire research organizations. Gieryn (1978) and Campbell (1987) referred to problem domains as "problem sets." A problem set is defined as the set of problem areas in which an individual scientist does research at a designated time. Problem domains contain problem spaces or problem areas (Campbell, 1987), defined as research efforts that can be carried out in a reasonable amount of time (Kleiner, 1985). A problem area is defined as the accepted knowledge and recognized questions associated with a substantive object of study and includes a number of related though discrete research problems (Gieryn, 1978). A number of related problem areas are said to make up a specialty. A scientific discipline is defined as a set of related specialties.

One example of a problem domain would be the ecological role of cryptogams in grassland ecosystems. Problem spaces within that domain might include 1) nutrient cycling by cryptogamic soil crusts, 2) soil stability in cryptogamic soils, 3) competition between grasses and cryptogams, and 4) age structure of cryptogamic crusts. Obviously, each problem space could generate several research questions or hypotheses and therefore support several separate but related investigations.

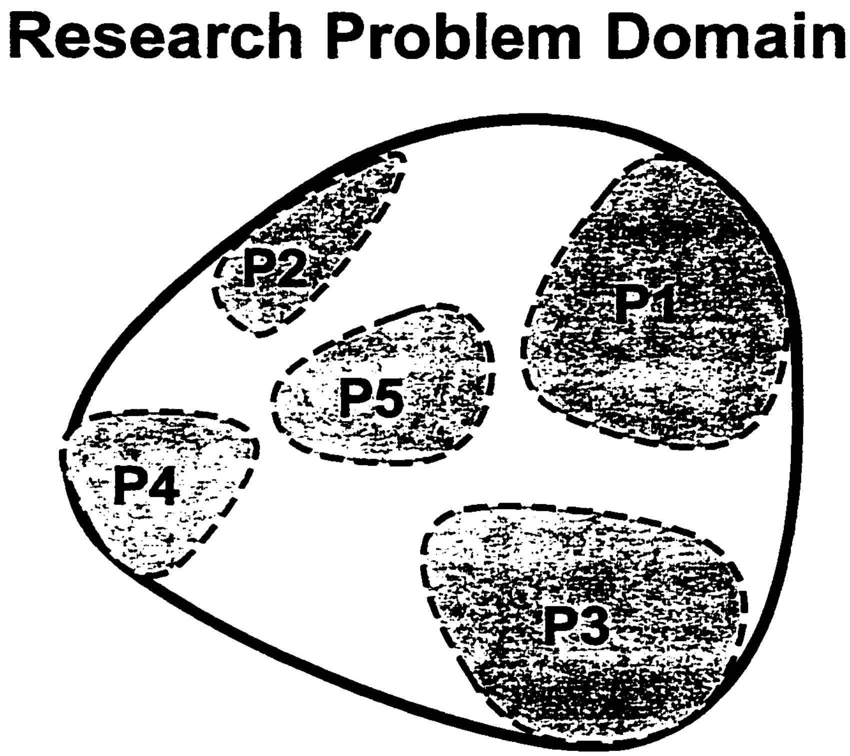


Figure E6. Problem spaces within a research problem domain.

Scientists usually choose a problem domain rather than a specific problem to work on (Ziman, 1987). Problem spaces are shown close to the domain boundary in Figure E6. This signifies that research problems often overlap into other, related problem domains. Different disciplines can and do occupy and contribute to the same problem space. I observed this occurring numerous times in research organizations in Europe and the U.S. One of the challenges for research groups is to define the problem space as a group. The tendency of a group is to expand the problem space beyond what can be accomplished in reasonable time. The group must be focused on client needs and practice goal alignment. Defining the problem space is closely related to establishing project goals.

Scientific activity occurs in discipline centers and at the peripheries of those centers. The periphery is the source of idea-hybridization, which leads to new problem fields and is characterized by cross-disciplinary interactions by scientists who have migrated from the disciplinary centers (Chayut, 1994). Quite often the boundaries of research systems are pushed outward when the client concept is broadened. This frequently happens with applied research and science that influences policymaking (Churchman, 1971). It is especially true in the case of natural resources and environmental management.

The clarification, formulation, and reformulation of the research questions being investigated is one of the functions of the research process itself. Problem definition is a vitally important phase of scientific inquiry and has been sadly neglected (Mitroff and Kilmann, 1978). Inadequate problem definitions often lead to a diversion of research direction and/or irrelevant research results due to researcher bias or political pressures

(Miller, 1993). Difficulties in problem definition may result in poorly prepared final reports which contain significant gaps or poorly related disciplinary components (Rossini, 1977). According to Kleiner (1985), the four conditions for a well-structured research problem are: 1) A well-defined problem space consisting of an initial state of knowledge and a final or desired state and all information relevant to the problem is included; 2) A method of inquiry that can effectively generate further knowledge states from the initial state; 3) An effective procedure for deciding at which state a solution has been obtained; and 4) A problem space in which the inquiry can be carried out in a reasonable amount of time.

Problem analysis consists of denoting the things on which to focus and framing the context in which to study them (Herrick and Jamieson, 1995). The critical link between science and management is through a sensitivity to management's needs in the process of problem selection and problem analysis. Thus, problem selection and analysis are the dual responsibility of managers and scientists and should be conducted in a collaborative manner. Research questions or preliminary hypotheses usually result from the problem analysis.

Miller (1985) found in expert groups a widespread resistance to spending time on discussing either the nature of the problem or the most appropriate strategy to use in seeking solutions. Instead, there was a cursory attempt at problem formulation followed by a retreat to the disciplines. A result of this may be that groups apply inappropriate methods to an inadequately formulated problem.

Table E1 shows a series of process steps that a research group may take in development of group knowledge goals. These process steps coincide with the model of scientific inquiry presented in Figure E3. Used together, Figure E3 and Table E1 offer a powerful blueprint of inquiry for a research group to follow.

Problem Spaces and the Science-Policy Interface

Defining problem spaces at the interface between science and policymaking is challenging. There is substantial support for science having a role in policymaking. However, it has never been clear what that role should be. According to Herrick and Jamieson (1995), science plays a crucial role in setting agendas, framing problems, and supplying concepts and vocabularies. It does not answer policy questions, but rather functions to provoke, structure, and inform debate. Increasingly, a principal role of science is helping society cope with increasing uncertainties in environmental issues (Funtowicz and Ravetz, 1990).

One of the reasons that the science estate has remained separate and distinct from the management-policy estate is that science was thought to be rational and linear and management or policymaking was chaotic and nonlinear. Hanley (1994) asserts that research and management are fundamentally different kinds of endeavors and have different roles in both science and policy matters. The tension between science and politics is deeply rooted. They occupy different estates and serve different purposes. Politics, or the policy domain, aims at the responsible use of power, while science, or the cognitive domain, aims at funding truths (Lee, 1993). Science tries to tip the balance

Table E1. Process steps for reaching the knowledge goals of interdisciplinary research groups, based on Klein (1990) and Parker (1993).

1. Define and bound the problem space by scanning for important research issues and by identifying and acknowledging all disciplinary perspectives.

2. Identify the client who will receive and use the research results.

3. Formulate a working hypothesis in the form of an overarching question.

4. Determine all knowledge needs, including appropriate disciplinary representatives and literatures.

5. Develop an integrative conceptual framework based on patterns of relationships among the research issues being addressed.

6. Review current theory and existing knowledge relevant to the research issues.

7. Develop multiple working hypotheses based on existing knowledge of each researchable issue.

8. Determine knowledge gaps and search for new information.

9. Engage group members in role negotiation, goal alignment, reciprocal learning, and development of a common vocabulary.

10. Specify particular studies to be undertaken, plan the collection of data, and get commitments from the individuals responsible for each contribution.

11. Collate and integrate all contributions.

12. Develop a solution space and proposed solutions.

13. Compare results, proposed solutions, and working hypotheses.

14. Revise hypotheses if necessary.

15. Develop inferences and generalizations with appropriate confidence limits.

16. Determine whether each group member is able to achieve expanded. knowledge about his/her discipline.

17. Decide about future management or disposition of the project and/or research group.

between uncertainties and relevant facts in the direction of facts. Within the policy domain, uncertainty is either manipulated to one's advantage or ignored or suppressed. Scientific information is uncertain and probabilistic and policymakers have trouble dealing with that kind of information (Hammond et al., 1983).

Brewer (1981) used three-dimensional space to describe the primary activities involved in problem-solving. The three dimensions are politics, analysis, and science (Figure E7). Until now I have framed the science × policy interaction in two-dimensional space, either as a science-analysis plane (Figure E8) or a science-politics plane (Figure E9). The science-policy interface is actually found in three-dimensional space (science × politics × analysis). Natural resource and environmental policy issues should be approached in this three-dimensional space. With any one of the dimensions missing, policymaking will be incomplete. Research that advances theory applicable to management and policy is ultimately the most useful research for managers and policymakers (Hanley, 1994). Such research must be anticipatory ("upstream" research) and integrative over several disciplines. Figure E10 shows an upstream-downstream model of research delivery. The different tributaries represent streams of knowledge from different disciplines integrated over time.

Policy-oriented research should result in 1) a narrowing of the probability density for anticipated outcomes or 2) the discovery of alternative outcomes as depicted in Figure E11.

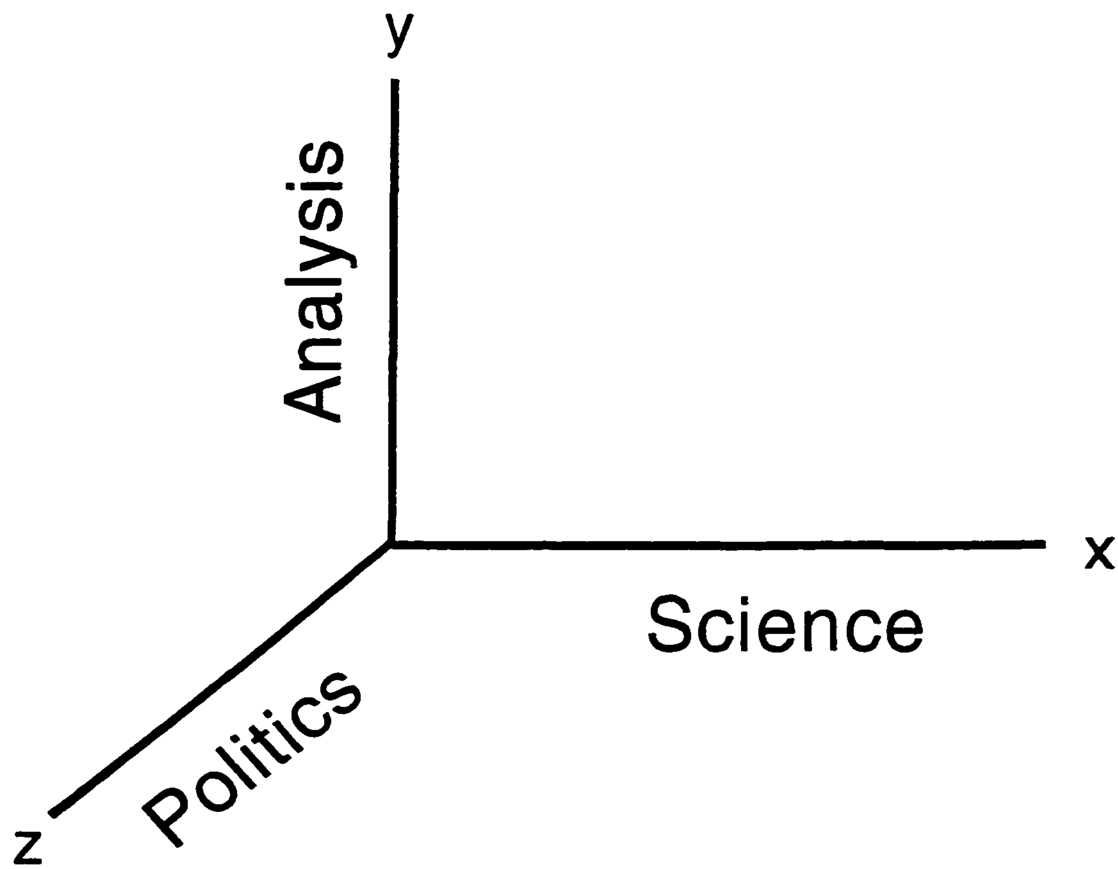


Figure E7. The three-dimensional space of problem-solving, after Brewer (1981).

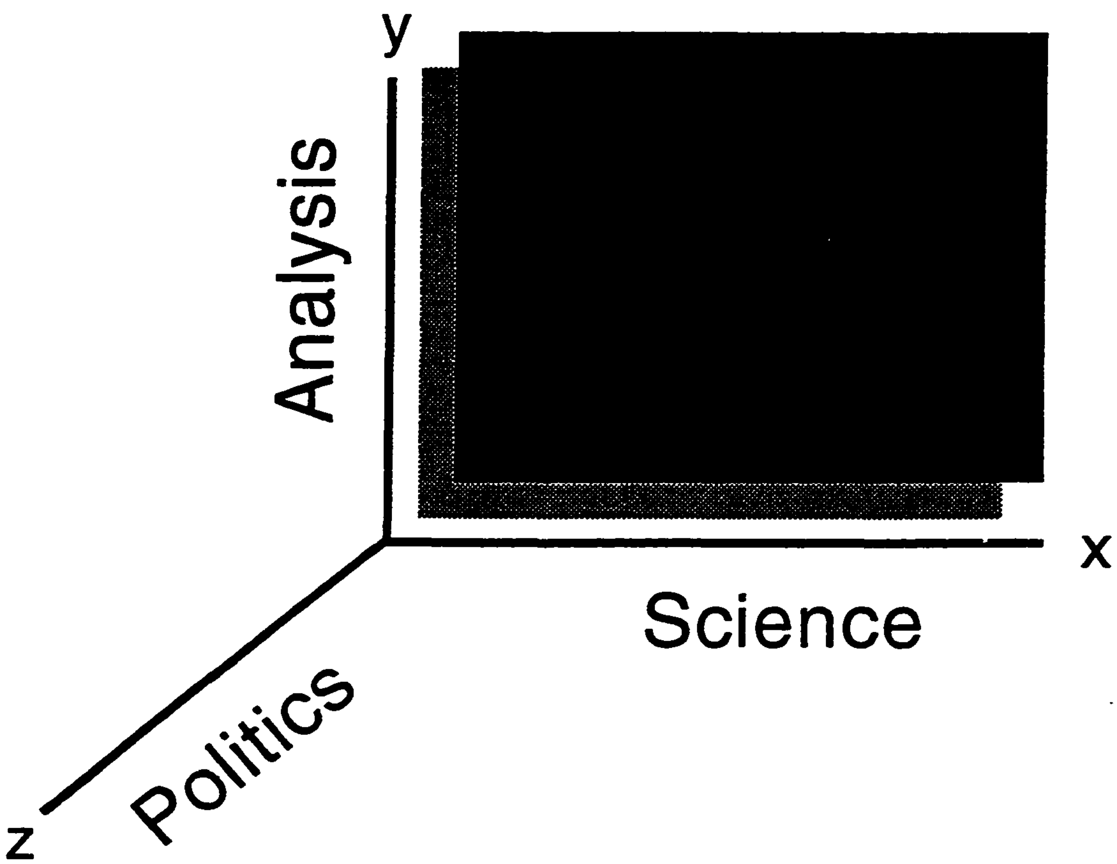


Figure E8. The science-analysis interaction in problem space, after Brewer (1981).

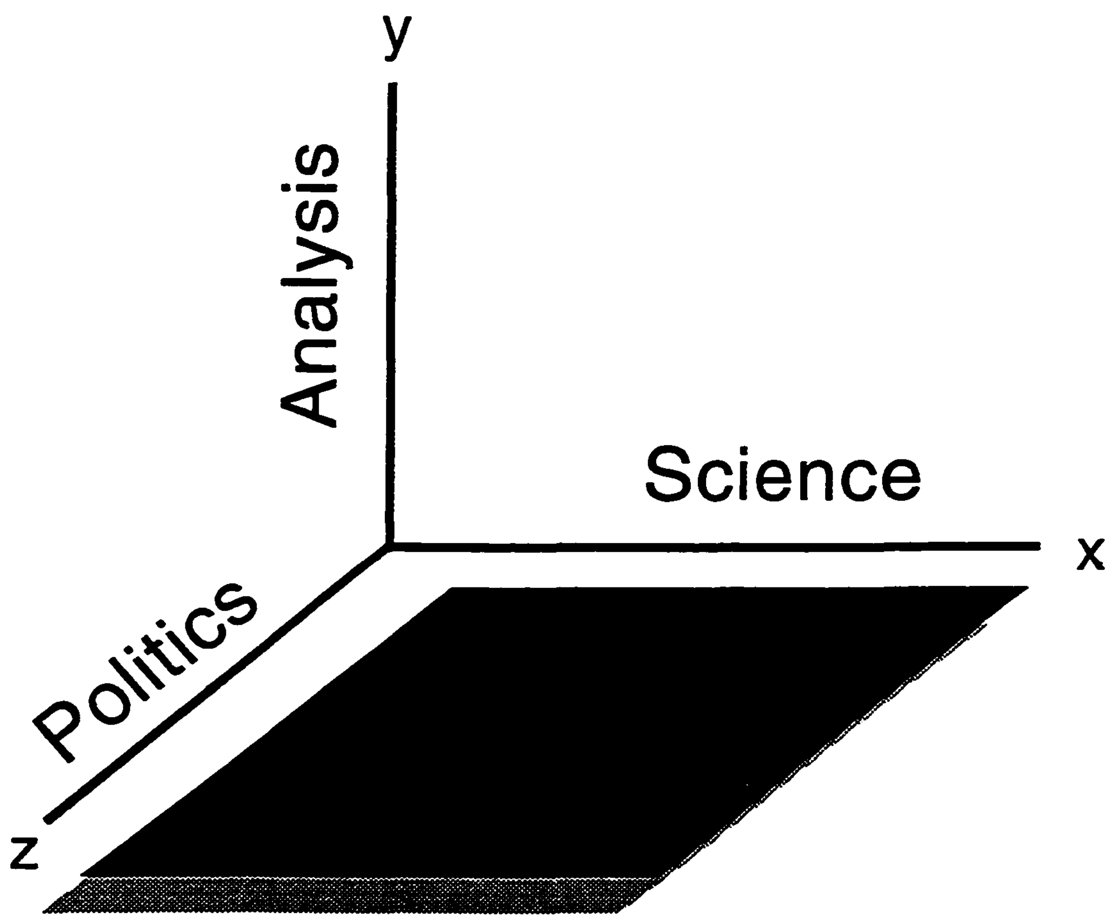


Figure E9. The science-politics interaction in problem space, after Brewer (1981).

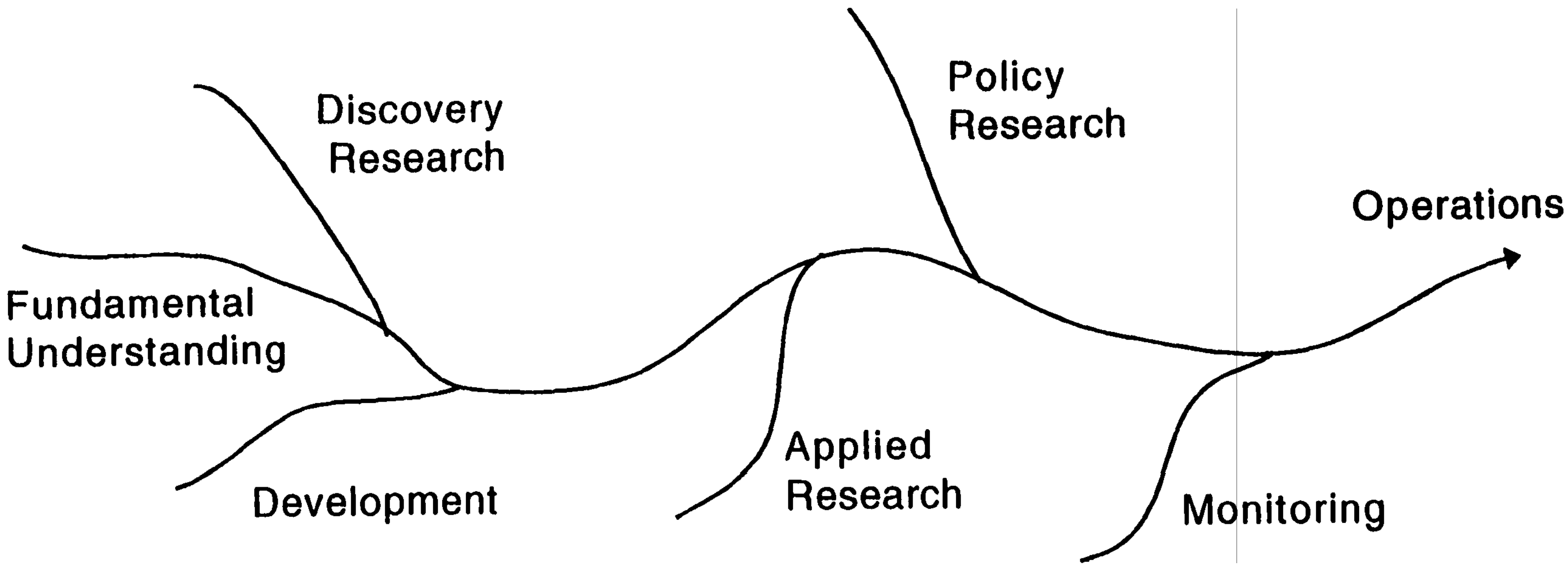


Figure E10. The upstream-downstream model of natural resources and environmental research.

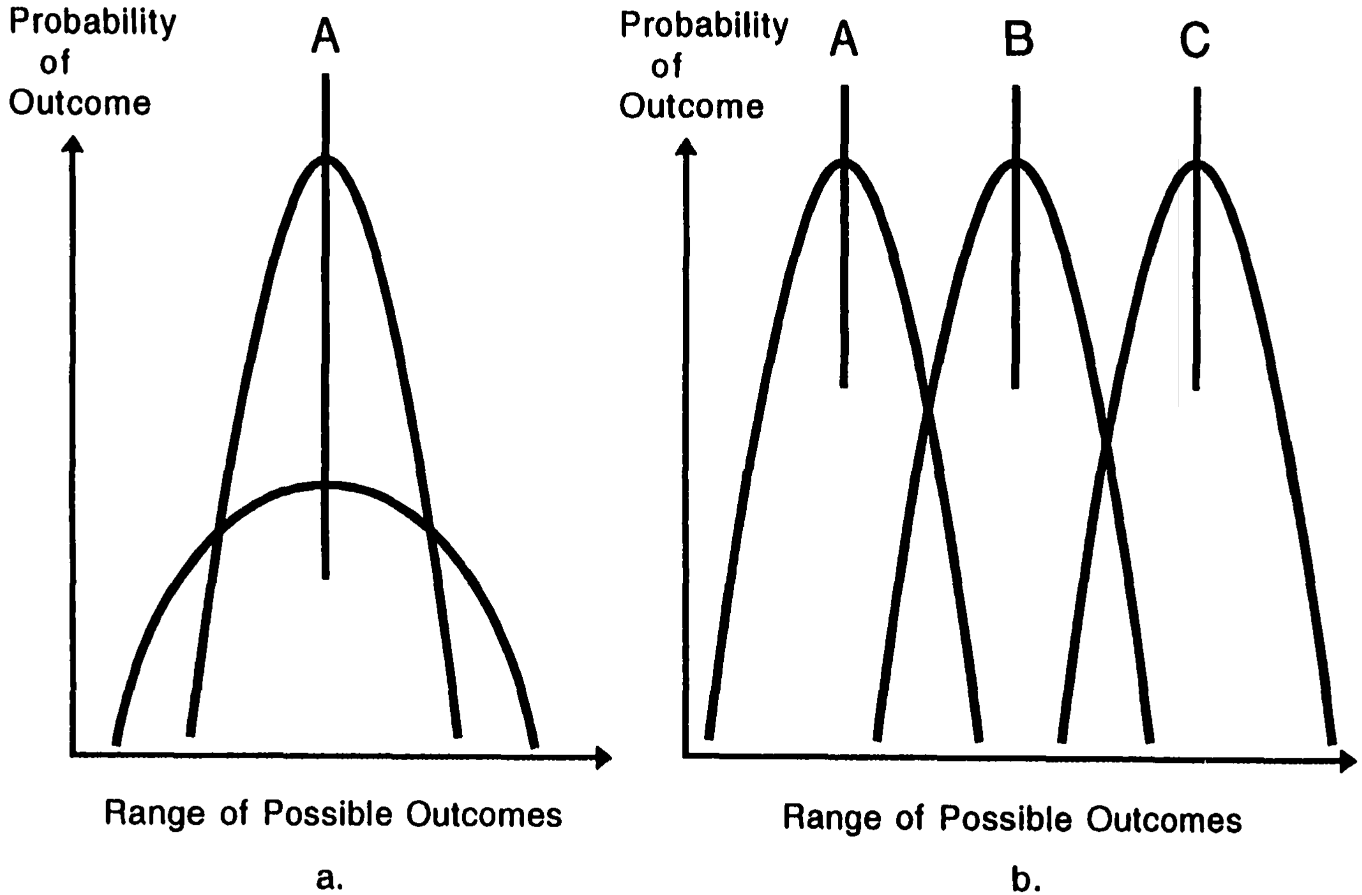


Figure E11. Possible outcomes of policy-oriented research: a) narrowing of the probability density for anticipated outcomes; b) discovery of alternative outcomes. Based on Ince (1989).

Herrick and Jamieson (1995) use the term, "science/policy assessment," to refer to the practice of enlisting science as the foundation for public policy decisions. They further argue that it is a mistake to assume that good science will always provide a right answer for science-based policy disputes. The National Atmospheric Precipitation Assessment Program (NAPAP) received high marks for scientific quality but not as an exercise in science policy assessment.

Social problems that involve an interaction of scientific discovery and policymaking require solutions found through synthesis and assessment at the intersection of science, politics, and analysis (Scavia et al., 1996). Stratospheric ozone depletion was a good example of science-policy convergence success (Sarewitz, 1996).

According to Kuhn (1970), disciplinary boundaries are helpful in normal science because scientific disciplines provide the frameworks through which phenomena are constituted into specific research problems. However, disciplinary boundaries are a hindrance in science/policy assessments because they lead to disputes concerning the strength and validity of scientific evidence. Policy-oriented research is improved by using interdisciplinary approaches. Mathisen (1990) favors the use of "problem-solving communities," groups of researchers and users, to define research problems and conduct problem analyses of policy-relevant research.

Theory Construction and Hypothesis Generation by Research Groups

Hypotheses are rules which govern our expectation of future experience; they are designed to enable us to anticipate the cause of our observations (Ayer, 1952). Kitchener (1995) asserted that primitive theories are used to select and direct observation. For most research efforts, primitive theories probably arise during problem selection and definition. As Beveridge (1957) states, the main function of the hypothesis is to suggest new experiments and new observations. Hypotheses may be used as tools to uncover new facts, rather than as ends themselves. In that sense, then, all hypotheses are "working" hypotheses. Since hypotheses are primitive theories, the hypothesis formulation, evaluation, and revision stages shown in Figure E3 also represent a theory-building process. Since the H-D-deductive approach involves testing hypotheses against experience in the form of observations, it is also a hypothesis confirmation or inference-building process.

Hypothesis generation begins with a researchable problem that requires a solution, proceeds with a framing and analysis of the problem, involves diffuse, nonlinear and intuitive thought, and is not subject to any methodological rules. Hypothesis generation consists of cognitive activities which yield a tentative explanation of a scientific problem (Rachelson, 1977). Thus, the steps involved in generating a hypothesis are largely inductive rather than deductive processes. A tentative or working hypothesis evolves from and forms a part of the research problem statement.

In the physical sciences, according to Lee (1973), observed data combined with a problem or question suggest a working hypothesis. Working hypotheses are used primarily to suggest specific lines of inquiry (Chamberlin, 1965).

Investigation of existing knowledge about the problem may reveal several alternative hypotheses. For any scientific problem there may be many hypotheses that can provide an explanation for the problem. Multiple working hypotheses, defined by Chamberlin (1965) as a family of tenable, competing hypotheses, prevent a single hypothesis from becoming a controlling idea. Controlling ideas may lead to premature explanations or a tentative theory of a problem, which in turn may become the adopted theory and eventually a ruling theory. According to Chamberlin (1965), investigations, observations, and interpretations are often controlled and directed by favored or adopted theories.

Hypotheses are formed by induction. This is the creative or discovery stage of scientific inquiry, an imaginative or intuitive preconception of what might be true in the form of a declaration with verifiable deductive consequences. The creative phase of scientific inquiry, formulating a hypothesis, is taken for granted, underemphasized, and tends to be forgotten. Induction does not follow logical rules but comes from insight, imagination, inspiration, and intuition. This is an aspect which has been neglected in science. Inspiration and intuition were viewed by the inductivism and hypothetico-deductivism schools of thought as being too closely related to religious and mystical ways of knowing. Preliminary or tentative hypotheses may be based on initial observations, the scientific literature, or other existing knowledge about the research problem. There are different types of hypotheses. Poincaré (1913), for example, presented three kinds of hypotheses:

1. *Natural and necessary hypotheses* (the basis of theory in the physical sciences). These are very valuable to science because they are either verifiable or else refutable through

empirical testing.

2. *Indifferent hypotheses* (cannot be proved or disproved). These are quite valuable in spite of the fact that they cannot be confirmed nor refuted. They are devices of understanding and can be used to give conceptual unity and connectedness to certain types of phenomena. In the Kantian sense, they are a form of *a priori* propositions that help us to organize our interpretation of experience. Examples are often found in sciences that deal with the past, like geology, archaeology, and history.

3. *Real generalizations* (generalizations from observation). These are similar to the conjectures arrived at through discovery phase research (Leary, 1991). They may or may not be testable. Many hypotheses are not testable (Kitchener, 1995).

It is obvious from Figure E3 that the H-D method of scientific inquiry relies heavily on inductive thinking. A group approach benefits the discovery phase of inquiry through greater illumination of the problem, many more possible hypotheses, and the benefits of shared intuition and inspiration. An effective research group expands the available pool of ideas, approaches, hypotheses, and solution spaces.

Interfield Theories

Theories are typically built within disciplines, but it is also possible to build theories across disciplines. Cole (1979) advanced a null hypothesis with regard to the unity of science. He claimed that the logic and semantics associated with observation, theory construction, and hypothesis testing can be standardized across all of science. Cole's null hypothesis is essentially challenged by Kuhn (1970), who defined scientific specialties on the basis of paradigms which he says includes not only a cognitive framework but also a sociological dimension. Kuhn's (1970, 1977) disciplinary matrix consists of 1) symbolic generalizations, 2) shared commitments in the form of beliefs in certain models, analogies, metaphors, and language, and 3) values.

I have observed in just a few very effective research teams a capability and propensity to set aside disciplinary paradigms, create a new language, new values, and new models to deal with the research problem at hand. Bechtel (1988), in discussing cross-discipline theory building, referred to this group phenomenon as "boundary-bridging." Darden and Maull (1977) defined interfield theories as theories that bridge two or more fields of science. They are useful when two or more fields share an interest in explaining different aspects of the same phenomenon (for example, a complex problem space) or when questions arise in a field that are not answerable using the concepts and techniques of that field alone.

The solution to an interfield or interdisciplinary research problem is an interfield theory. Darden and Maull (1977) suggest that the unity in science is a complex network of relationships between fields effected by interfield theories. Much of the progress of modern biology results from the development of interfield theories and the progressive bridging of biological and physical sciences, especially chemistry. Thus, another potential advantage of research groups is that several primitive interfield theories, or working hypotheses, may be formulated (see Figure E3).

Hypothesis Evaluation and Revision

Pirsig (1974) warns that the number of hypotheses should never grow faster than the experimental method can handle. We need to have reliable methods for refuting or eliminating implausible hypotheses. Hypothesis pruning is at the heart of Popperian logic and the H-D method. A plausible hypothesis requires verification or elimination through the test of experience. Hypothesis testing consists of cognitive activities which expose the hypothesis to experience, usually by experimentation. Working hypotheses are evaluated against existing knowledge. The eight criteria of Kitchener (1995) for determining a "good hypothesis" may also be used. An *a priori* evaluation of competing hypotheses results in one or more hypotheses suitable for testing.

The *a posteriori* testing of hypotheses is a deductive process. Hypothetico-deductivism is a model of scientific inquiry devoted to the reasons for accepting or refuting a hypothesis once it is formulated. The H-D method has not been concerned as much with the generation of hypotheses as it has with the testing of hypotheses (Popper, 1961; Medawar, 1969; Rachelson, 1977). This is a weakness of the H-D method. According to Weimer (1979), Popper did not consider how conjectures or hypotheses to be refuted are created in the first place. Popperian logic needs to accept some amount of induction. Most of the steps involve inductive processes and therefore lend themselves to group research approaches.

According to Popperian logic, all testable hypotheses must be falsifiable. Popper (1961) offered four ways to deductively test theories: 1) logical comparison of conclusions among themselves; 2) investigation of the logic of the theory; 3) comparison with other theories; and 4) empirical applications of the conclusions derived from the theory.

Figure E3 assumes that scientific inquiry is a cyclical process: data gathering, description, explanation, theory testing, and back to data gathering (Simon et al., 1981). The H-D process involves continuous feedback from inference to hypothesis (Medawar, 1969). Further analysis of the relevant data yields a modified hypothesis. There may be several cycles of data analysis, *a priori* hypothesis evaluation, and hypothesis revision, as shown in Figure E3, before a hypothesis is tested deductively. Scientific inquiry is a self-correcting, revisionary system.

Chamberlin (1897, 1965) first proposed the method of multiple working hypotheses to replace the "ruling theory" paradigm in science. He claimed that the method of multiple working hypotheses was an approach particularly well-suited to sciences such as geology. Platt (1964) favored the use of multiple hypotheses, because it distributes the inquiry effort and divides the affection that some scientists have toward a single pet hypothesis. He suggested that the use of multiple hypotheses leads to intellectual excitement and teamwork, especially when they are coupled with strong inference.

According to Wilson (1998a), the method of multiple competing hypotheses is also known as strong inference. Strong inference is similar to shaping a tree by pruning--undesirable paths for future growth are eliminated by tests that falsify competing hypotheses. This is an explicit logic of inquiry. Scholarly research has a greater chance of identifying alternative approaches to land management because it seeks to challenge multiple competing hypotheses (Lee, 1991). Mature theories have developed from a progression of ruling theories, working hypotheses, and multiple working hypotheses. These stages represent a succession from the defense of favored ideas to the utilization of strong inference.

At the earliest stage of methodological development, plausible explanations are adopted as theories and when rigorously defended become ruling theories. The use of multiple competing hypotheses coupled with the logic of strong inference represent the most advanced stage of research.

Poincaré (1913) believed that several conclusions were possible in any scientific inquiry. Popper (1966) likewise thought that scientific results are relative--they have the character of hypotheses. There needs to be some principle of selection of explanatory theories (which limbs to prune), which Poincaré (1913) said was simplicity. Simple facts had the greatest chance of recurring. However, this is an inductive approach. Popper (1961) gave four ways to deductively test theories: 1) logical comparison of conclusions among themselves, 2) investigation of the logic of the theory, 3) comparison with other established theories, and 4) empirical applications of the conclusions derived from the theory. Any theories or hypotheses tested and shown to be false are pruned. An alternative to hypothesis elimination, as shown in Figure E3, is to revise hypotheses that do not stand up to scrutiny.

According to Kitchener (1995), there are two methods for evaluating competing hypotheses--logico-conceptual or *a priori* and empirical or *a posteriori* evaluations. A research group working as an effective team could be very helpful in the *a priori* evaluation of hypotheses. Logico-conceptual or *a priori* evaluations may be based on clarity, testability, and plausibility of hypotheses or compatibility of the hypotheses with accepted scientific knowledge. An *a priori* evaluation of hypotheses by a research group may result in 1) hypotheses being accepted as plausible and worthy of testing; 2) hypotheses being rejected as implausible; 3) hypotheses being modified and re-evaluated; or 4) hypotheses being replaced with new hypotheses.

According to Toulmin (1967), research managers should be concerned with both the content (or quality) and the volume of intellectual variants involved with any research endeavor. Research groups can be used to both increase the number of intellectual variants (ideas, hypotheses, explanatory theories) and to improve the quality of the conclusions and inferences resulting from scientific inquiry.

<u>Solution Spaces and Inference</u>

The sphere of potential problem solutions should be as large as possible. In theory, individual solution spaces will be smaller than the solution space a group derives. Figure E12 theorizes that a group solution space will be larger than the sum of solution spaces of all individual members of the group. Interdisciplinary team research should have the effect

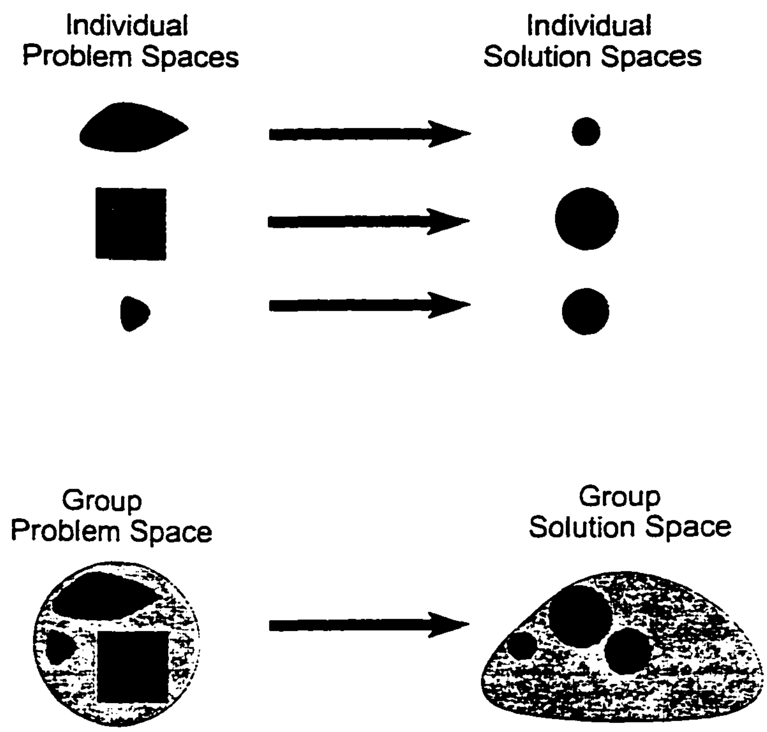


Figure E12. Individual vs. group problem spaces.

of increasing the pool of solution ideas (Toulmin, 1967). In other words, synergy of solution spaces is realized in an effective group. Constraints on the solution space include disciplinary paradigms, uncertainty about the initial knowledge state, and ineffective evaluation of whether or not the goal state has been reached.

The applied, policy-oriented nature of much forestry research necessitates the adoption of more explicit rules for drawing inferences from observations (Lee, 1991). Policy-oriented research seldom reaches the strong inference stage of scientific inquiry. Research controlled by land and resource management organizations often focuses on discovering the best methods with which to support policy decisions, not on the policy decisions themselves, and seeks to provide means for predetermined ends. This type of research may be closer to the stage of ruling theory. In such management agencies, the understanding of basic biological, physical, social, or economic processes is less important than knowing how information can be used to serve organizational objectives. An explicit logic of inquiry is essential to avoid groupthink and self-deception. Actually, interdisciplinary group research may be the panacea for avoiding self-deception and ruling theory.

Assumed policy decisions are often challenged by scholarly research. Research in natural resources and environmental science should satisfy three methodological requirements to be considered scholarly: 1) researchers must be committed to making predictions and providing explanations rather than issuing or implementing prescriptions for management actions; 2) theories should permit falsification; and 3) researchers must adopt strong inference in their work. In addition, the social mores of scientific inquiry demand that there be 1) free criticism, 2) expression of theories in such a way that they can be tested by others, and 3) publicity of methods (Popper, 1966).

If followed using the accepted rules of logic, the method of scientific inquiry shown in Figure E3 will result in strong inference. Strong inference consists of the following steps: 1) devising alternative hypotheses; 2) devising a crucial experiment with alternative possible outcomes, each of which will exclude one or more of the hypotheses; 3) conducting the experiments so as to get clean results; and 4) recycling the procedure, creating subhypotheses or sequential hypotheses to refine the possibilities that remain (Platt, 1964). Scientific inference is the movement of thought from the known to the relatively unknown (Lee, 1973) or the bridge between observation and theory (Howson and Urbach, 1993). Strong inference is similar to shaping a tree by pruning-- undesirable paths for future growth are eliminated by tests that falsify certain competing hypotheses. This is an explicit logic of inquiry.

Mature theories have developed from a progression of ruling theories, working hypotheses, and multiple working hypotheses. These stages represent a succession from the defense of favored ideas to the utilization of strong inference (Platt, 1964). At the earliest stage of methodological development, plausible explanations are adopted as theories and when rigorously defended become ruling theories. The use of multiple, competing working hypotheses coupled with the logic of strong inference represent the most advanced stage of research (Chamberlin, 1897, 1965).

A key goal for research groups, particularly in the areas of natural resources and environmental research, is to reach consilience, both conceptually and methodologically. The most appropriate methods of inquiry chosen and used by research groups are those that will both achieve consilience and maximize the solution space.

Consilience and Research Groups

The epistemological focus in this dissertation is on how small research groups acquire knowledge and solve research problems. Whewell (1847) introduced the concept of "consilience of induction" to explain how one class of facts obtained inductively coincides with another class of inductive facts. The dictionary definition of consilience is the "occurrence of generalizations from separate classes of facts in logical inductions so that one set of inductive laws is found to be in accord with another set of distinct derivation" (Webster's Third New International Dictionary). Wilson (1998a,b) recently redefined consilience as the linking of facts and fact-based theory across disciplines to create a common groundwork of scientific explanation and a webwork of established cause and effect.

Consilience represents the growth of knowledge across and between disciplines--a sort of filling in of the interstices of a knowledge webwork. I will expand the meaning of consilience to include an interlocking of both the theoretical (conceptual) and the methodological (empirical) aspects of interdisciplinary inquiry. Within most research problem domains there is the potential for consilience of theory, or *a priori* consilience, and the potential for consilience of fact, or *a posteriori* consilience among two or more disciplines.

Implications for Research Managers

Research managers have the responsibility to frame research problems, to organize research groups, to help groups coalesce individual goals into group-defined goals, and to encourage groups to maximize their solution spaces. In addition, managers can encourage research groups to develop and mutually agree on a unique system of inquiry built on the principles of strong inference as suggested in Figure E3.

The two cardinal virtues of science, as espoused by Karl Popper (1961), are freedom of conjecture and severity of criticism. They operate to both enlarge the pool of intellectual variants and enhance the degree of selective pressure. In the case of research groups, the leaders may wish to emphasize the freedom of conjecture and reserve the selective pressure until group members know each other well and mutual trust has been established. There are psychological, disciplinary, and epistemological barriers facing any research group (Chubin et al., 1986). In a classic study of interdisciplinary research, Chubin et al. (1979) uncovered the following four epistemological problem areas:

1) disputes over the use of formal analytical techniques;

2) inability to move beyond firm data to speculate about the future;

3) difficulty of interaction with economists; and

4) the role of social scientists.

There are significant epistemological distances between natural and social scientists. The social sciences are not as fully developed as the natural sciences because of the complexity of the data and because questions of fact have not been as successfully distinguished from questions of value in the social sciences. The organizational design of a research group should reflect this reality. Managers may be able to control epistemological distances by carefully selecting research group members. Managers must also allocate the time necessary for research groups to work through the problem identification, problem analysis, and goal-setting stages.

Institutional factors are typically cited as impediments to interdisciplinary group research. Bauer (1990) says that institutional factors are the not the cause but rather the symptom. Disciplines differ in epistemology, in what is viewed as knowledge, in opinions over what sort of knowledge is possible, and over what is interesting and valuable. Different sciences tolerate different balances between fact and speculation. A discipline's knowledge, methods, and theoretical approaches cannot be separated from its practitioners. The real barriers to interdisciplinary group research are not institutional but epistemological. Science, especially the natural resource and environmental sciences, cannot be understood from a single disciplinary viewpoint. A great deal of time and energy must be focused by research groups at the problem identification and problem analysis stage.

THE CONCEPT OF VALUE IN SCIENCE

Quality and Value in Scientific Research

Quality is an elusive concept and difficult to define (Pirsig, 1974). Moed et al. (1985) believed that it is virtually impossible to operationalize the concept of research quality, because quality may refer to a variety of values. They defined research quality by three dimensions: cognitive quality, methodological quality, and aesthetic quality. They called this "basic quality" and state that an evaluation of basic quality is based on criteria intrinsic to scientific research and known only to peer researchers. Thus, quality is an intrinsic property of a scientific effort or product.

Good science is described in terms of combinations of aspects and attributes and there is general agreement among academic scientists for those factors related to scientific quality (Montgomery and Hemlin, 1987). Impact, on the other hand, refers to the actual relationship of a scientific paper with the existing literature or the influence a paper has on the surrounding research activities at a given time. Therefore impact is an extrinsic property. It can be used to assess the extent to which a research group exerts itself at the research front and forms a part of the research community (Luukkonen-Gronow, 1987).

It is clear that there are both intrinsic and extrinsic values associated with scientific research. Value means the assessment of the impact of the research in terms of some value criteria with respect to either social or scientific value (Stankiewicz, 1980). According to Shils (1968), the science community has accepted two independent and incommensurable criteria: scientific value and practical value. The evaluation of research has focused primarily on intrinsic values--the quality of research as viewed by peer scientists. Intrinsic values include actual or potential contribution to knowledge, cognitive progress, originality, cogency, strong inference, consilience, explanatory power, and plausibility or truth. Scientific truth is dynamic and relative and constantly evolving and replacing itself (Tykociner, 1966; Toulmin, 1967; Kitchener, 1996). According to the scientific community, if research has these attributes, it is quality research. Internal value criteria have traditionally been evaluated using peer review (Luukkonen-Gronow, 1987). Bibliometric indicators are an effort to measure these internal criteria more systematically (Van Raan, 1989).

Less attention has centered on extrinsic values--the impact that science has on society. Extrinsic values include such things as goals met, problems solved, impact on the scientific community, and impact on society in general. New scientific knowledge is neither good nor bad. Value is derived from the philosophic, social, and ethical significance of science (Weisskopf, 1972). Philosophic or epistemic significance is derived from progressively deeper and more comprehensive insight into the workings of nature. Social significance comes from the increasing ability to change or adapt to our environment and to improve the quality of life by applying the results of science. Ethical significance derives from the recognition that the evolution of life and humans on earth is predicated on a most precarious balance of physical conditions on this planet, which produces a human responsibility to protect the natural environment.

Intrinsic values tend to compete against extrinsic values in science. The competing values model of effectiveness (Quinn and Rohrbaugh, 1983; Hall, 1987) stresses that there are external and internal facets to effectiveness. The same applies to scientific value. Research seeks to satisfy internal or intrinsic scientific goals and needs as well as external or extrinsic goals and needs. The question of who benefits from investments in scientific research suggests that there are competing values in science and results in a tension (for example, the classic fundamental vs. applied research tension).

Polanyi (1962) discussed the concept of scientific merit and concluded that scientific merit depends on 1) a sufficient degree of plausibility of the research, 2) scientific value of the work, and 3) originality of the discovery. Thus, scientific merit appears to be a mix of quality and value. Toulmin (1961) said that scientific merit is a function of survival value--the better ideas will survive. Polanyi (1966) further asserts that the scientific value of a contribution is formed by three factors: a) its exactitude or accuracy (proximity to the truth), b) its systematic importance, and c) the intrinsic interest of its subject matter. Scientific journal referees are engaged in eliminating contributions offered to science which lack an acceptable scientific value on the basis of those three factors.

Intrinsic values may compete with one another. The criterion of plausibility tends to promote conformity while the originality criterion tends to encourage dissent and competition. Both conformity and originality are necessary in science.
Weinberg (1972) referred to the study of values in science as the axiology of science--questions of scientific value, including the problem of establishing priorities within science. These choices involve value instead of truth. Although these issues appear to be internal to science, they clearly transcend science and involve investment decisions for research sponsors.

According to Ziman (1987), scientific progress is made not by the accumulation of routine results but rather by discoveries--research results that make a significant change in what we thought we already knew. As measured by citations, an important, paradigm-breaking research result may prove itself to be hundreds of thousands of times more scientifically valuable than a routine research result because of the difference in impact. Citations are a measure of quality, impact, penetration, visibility, or the utility of the research work (Luukkonen-Gronow, 1987). Van Raan (1990) calls impact a measurable aspect of scientific quality operationalized by the number of citations received by publications within a certain time period. Luukkonen-Gronow (1987) concluded that substitution of impact for quality represents an improvement in understanding the citation process. According to Lindsey (1989), citation counts may be the most convenient measure of scientific quality, but it is probably not the most robust measure. Citation counts do not assess the essential cognitive and intellectual core of scientific contributions. However, citation counts remain our most popular measure, or a useful approximation of that measure, of quality in science.

It is very difficult to measure the quality of interdisciplinary research because of the lack of a peer structure. There are no consensus standards for judging interdisciplinary research (Kash, 1988). The same could be said about consilience. How can we measure consilience? New concepts and measures of scientific quality and impact are needed for interdisciplinary research, which involves such goals as problem-solving, consilience, and interfield knowledge growth.

Cogency in Scientific Research

Novak (1964) presented a conceptual model of scientific inquiry in which two parallel spirals represented the empirically-formulated and hypothetically-formulated paths of inquiry leading from ignorance to truth. Novak and Gowin (1984) introduce Gowin's epistemological "V" in an attempt to distinguish between two classes of epistemic elements--conceptual/theoretical (ideas) and methodological (facts). The base of Gowin's V (Figure E13) represents events and objects in nature. The conceptual/ theoretical leg is composed of concepts, a conceptual framework, and relevant theory. The methodological leg begins with observations and records, includes data transformations, and concludes with knowledge and value claims. In addition to the temporal progression up each leg of the "V" there is active interplay, in the form of questions, between the conceptual and methodological sides. Novak (1964) refers to this interplay as the epistemic bridge, or two-way interaction that attempts to correlate the two worlds, and the process of deduction, or one-way interaction leading from the hypothetical world of ideas to the empirical world of facts.

The purpose of the epistemological V is to ensure cogency in research efforts. It forces the scientist to have a cogent approach to the planning and conduct of research. Scientific cogency is defined as the maximum organization, clear identification, and precise statement of ideas and constructs in a discipline or research project (Leary, 1991). It is very much a cognitive activity. Leary (1991, 1993) modified Gowin's V for natural resources research wherein a H-D-deductive approach to inquiry is desired (Figure E14). He suggests using the modified Gowin's V as a research planning tool to increase cogency in natural resources research.

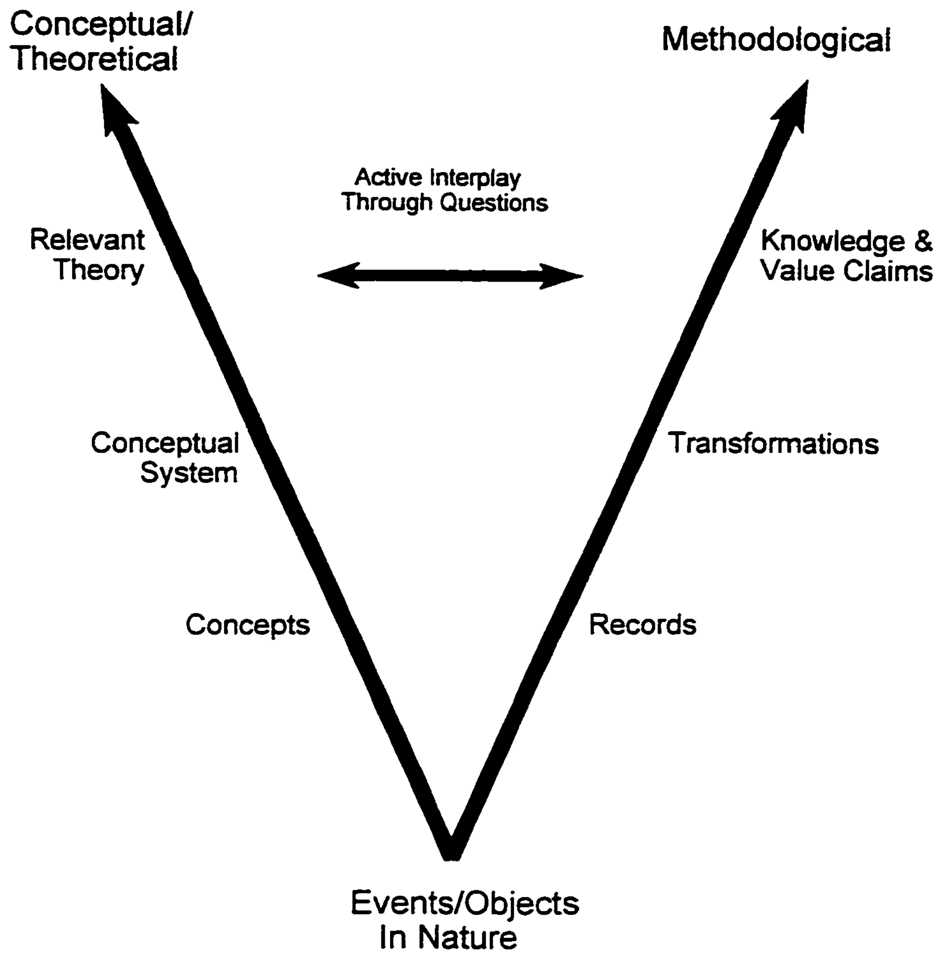
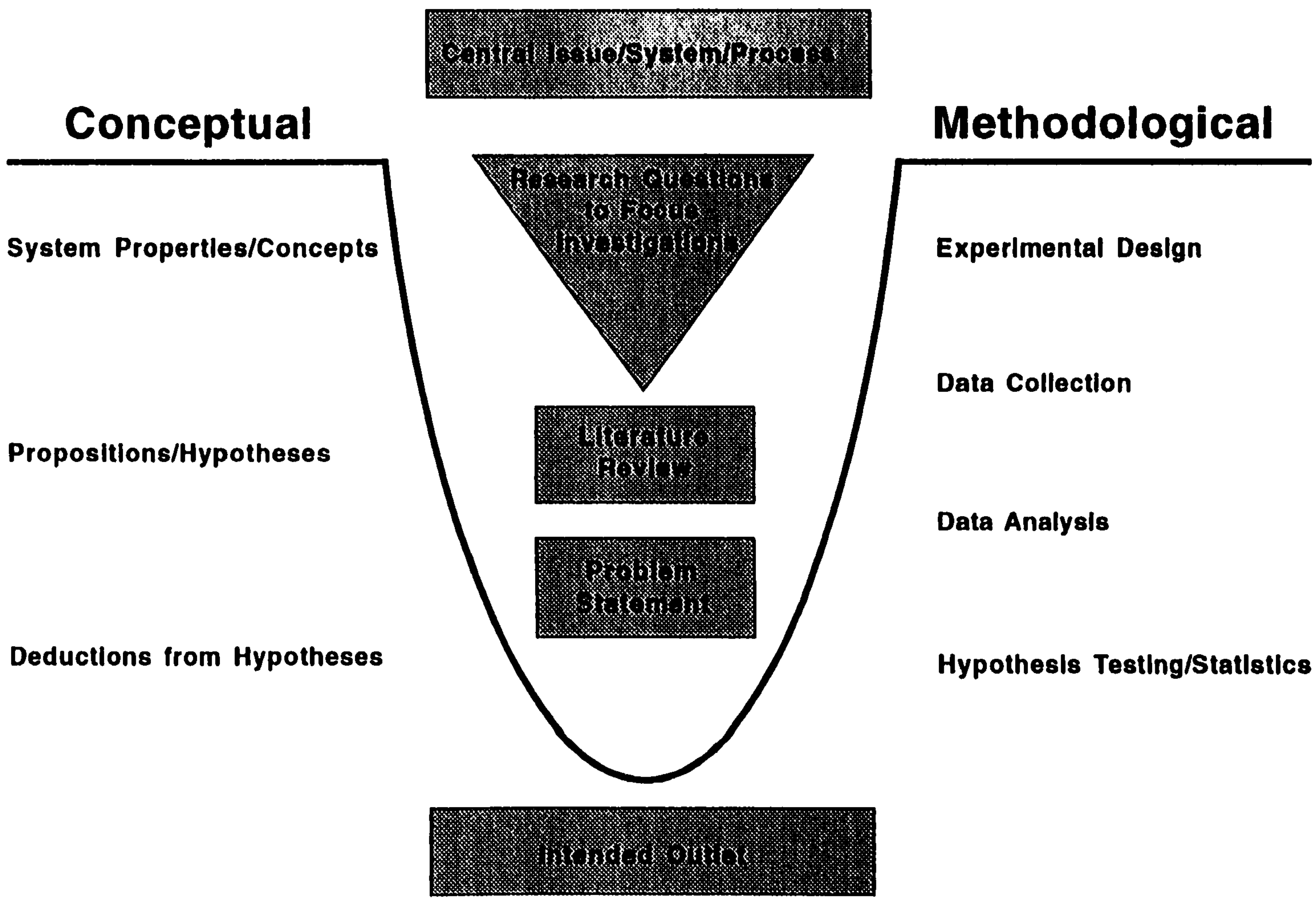


Figure E13. Gowin's epistemological "V" from Leary (1991) and Novak and Gowin (1984).

Figure E14. Leary's epistemological "V" modified for natural resources and environmental research, based on Leary (1991, 1993).


Central Issue/System/Process
Conceptual
Methodological
Research Questions to Focus Investigations
System Properties/Concepts
Experimental Design
Data Collection
Literature Review
Propositions/Hypotheses
Data Analysis
Problem Statement
Deductions from Hypotheses
Hypothesis Testing/Statistics
Intended Outlet

The conceptual models of scientific inquiry promoted by Novak (1964), Novak and Gowin (1984), and Leary (1991, 1993) contain both empirically-formulated and conceptually-formulated paths of inquiry. Both paths are needed for a cogent approach to scientific inquiry. A cogent approach, such as that found in the epistemological V, forces the scientist to develop a problem statement and ideas about how the problem can be approached and solved. Unfortunately, a cogent approach that includes both the conceptual/theoretical and methodological sides is often missing from natural resources research (Leary, 1991). Cogency adds considerable value to any research activity and should be a principal process goal of a research group. Brooks and Grant (1992) argued for cogency as well as a systems approach in forest research.

Criteria for a Good Hypothesis

It is obvious from Figures E3 and E14 that hypothesis formulation is a critical step in the H-D-deductive method. Their construction deserves careful attention. Kitchener (1995) gave the following eight criteria for a good hypothesis:

1) relevancy (to the phenomenon to be explained)
2) logically possible
3) testable as to validity
4) compatible with accepted scientific knowledge
5) simple and clear
6) plausible
7) adequate scope of explanatory power
8) provides a deep (vs. superficial) explanation

Working hypotheses are meant to be tentative and are usually not testable. In fact, they may not meet several of Kitchener's criteria. However, by the time working hypotheses reach the evaluation stage in Figure E3, they should meet all eight of the criteria and are ready to be tested against experience on the methodological side of Leary's V (Figure E14).

Uncertainty in Science

If the goal of scientific inquiry is to produce scientific knowledge, what specifically comprises knowledge goals? Certainly, scientists are after truth--what really exists, how nature really works, how humans have altered nature. Besides truth, scientists are also interested in accuracy, precision, and certainty--accurate, precise answers to research questions and a high degree of confidence that the answers will approach the truth.

Wynne (1992) defines risk as how well you know the odds and uncertainty as not knowing the odds but knowing the main parameters. Ignorance, on the other hand, is essentially that we don't know what we don't know. There are technical, methodological, and epistemological sorts of uncertainty. These correspond to inexactness, unreliability or lack of precision, and "border with ignorance."

Modern science includes the management of uncertainty (Funtowicz and Ravetz, 1990). Quality control in science is now an urgent and threatening topic due to the Ch-Ch Syndrome (Chernobyl/Challenger), thought to represent the collapse of mega-technologies due to political pressure, incompetence, and cover-ups. The lesson of Ch-Ch is that ignorance and error (inexactness and unreliability) may interact negatively with knowledge and power. Uncertainties, assumptions, and compromises must be fully exposed to public scrutiny (Newby, 1992).

Science has been seen as achieving ever greater certainty in our knowledge of natural systems. However, science cannot always provide well-founded theories based on observation and experimentation for explanation or prediction of natural events and anthropic influences. A principal role of science now is helping society cope with increasing uncertainties in environmental and natural resource issues. Quality assurance of scientific information and the management of uncertainty will require a new approaches based on a new philosophical foundation.

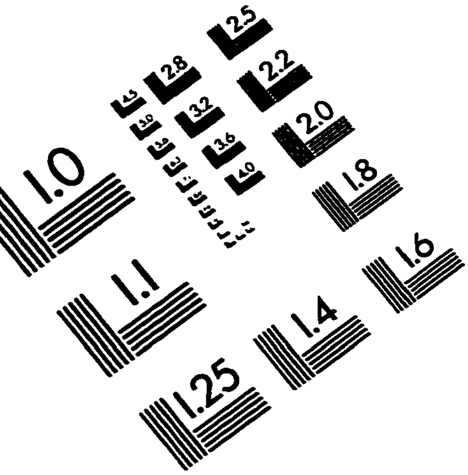
Implications for Research Evaluation

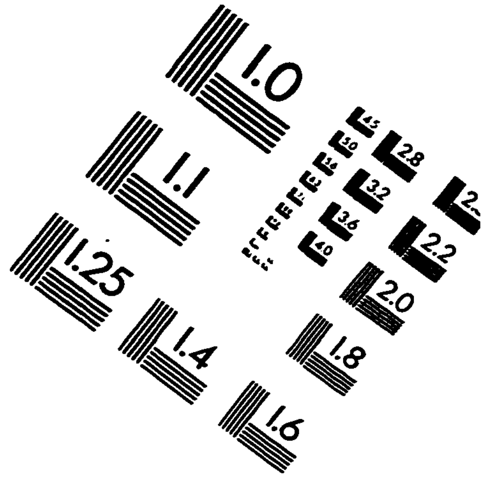
The evaluation of research has focused primarily on intrinsic values--the quality of research as viewed by peers. Less attention has centered on extrinsic values--the impact that science has on society. Both intrinsic and extrinsic values have to be considered in evaluating the quality and value of research.

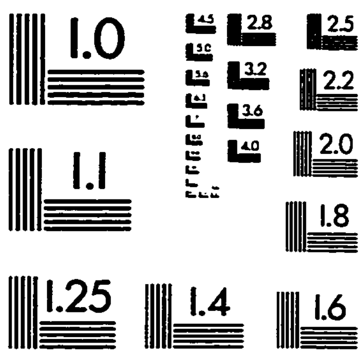
Leary (1985) argued for the consideration of research problem difficulty in evaluations of scientific productivity. According to Ziman (1987), science could be evaluated according to the concept of importance:difficulty, which is similar to the concept of benefit:cost. Cognitive difficulty or complexity has to be introduced into research evaluation because it is inaccurate to measure the productivity of research simply in terms of the number of scientific papers produced.

Unfortunately, we do not have much empirical information about group scientific inquiry--what kinds of knowledge interdisciplinary research groups actually produce vs. what they are supposed to produce, the inquiry systems they use to produce scientific knowledge, or the quality and value characteristics of group-generated knowledge.

IMAGE EVALUATION TEST TARGET (QA-3)

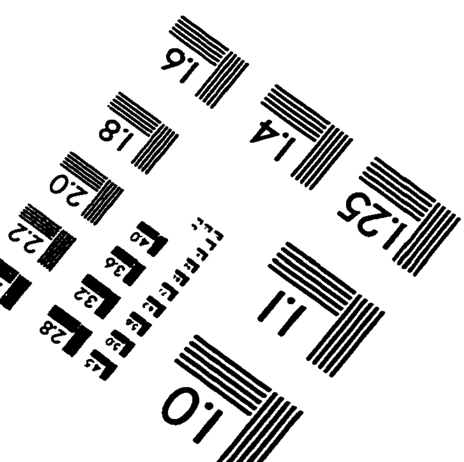


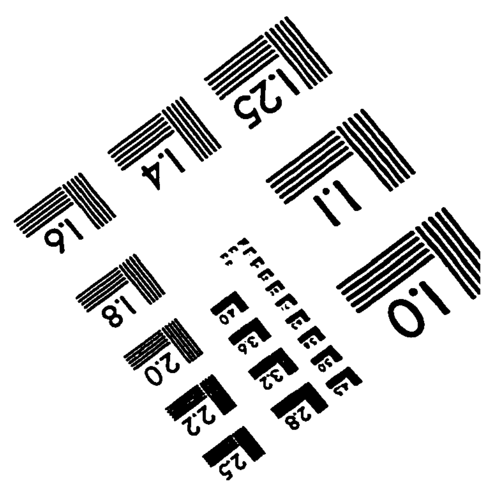




150mm

6"





APPLIED IMAGE, Inc
1653 East Main Street
Rochester, NY 14609 USA
Phone: 716/482-0300
Fax: 716/288-5989