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MANAGEMENT SCIENCE AND SCIENCE MANAGEMENT: PROSPECTS FOR THEORY DEVELOPMENT

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PREFACE

From 1973 to 1981 I attempted to apply a background of management science training and practice to duties in science management within two international scientific organizations, the East-West Center in Hawaii and the International Institute for Applied Systems Analysis (IIASA) in Austria. The experience was stimulating in that both organizations were supportive of the application of scientific principles in decisionmaking and in the design of their respective management systems. It was also frustrating in that I found the problems encountered in the management of these two organizations intractable by the formal methods with which I was familiar. During these eight years, I began to gain a deep appreciation both for the exceptional nature of science management as a problem context indicative of most problems in management science applications elsewhere, and for the clarity and visibility which science management offers for new descriptive modeling of decision systems facing rather perplexing conditions.

In the summer and fall of 1980, IIASA held two workshops which helped crystalize my ideas of science management as a problem context for the further development of management science theory. The first was on Rethinking the Process of Systems Analysis, and was the initial workshop in a proposed series examining the potential role of the behavioral sciences in systems analysis. The second was on the Role of Analysis in Decisionmaking and was focused on the selected context of siting decisions for large storage facilities of liquefied energy gases. Throughout this paper are references to both these workshops and to the overall work of IIASA as well.

ABSTRACT

Current trends in the development of management science are characterized by increasing usage of advanced mathematics and by large scale computer programs and data structures based on simplistic formulations conforming to the requirements of traditional analytical techniques. What was originally conceived as management aids for practical operational problems is rapidly becoming an esoteric branch of mathematics ill-suited to most management concerns and incomprehensible to most managers. Rather than striving to critique and improve management science's basic assumptions, formulations, and concepts to reflect more accurately the problem contexts addressed, management scientists have tended to maintain traditional formulations and concepts, and concentrated instead on adding new and more complex analytical procedures to their repertoire. As a consequence, the history of management science is filled with both successes and failures, and some consider the record rather dismal compared to both expectations and potential.

In a second, unrelated context of science management, developments in theory are virtually non-existent in the West, the science of science is still in its infancy in the Soviet Union and Eastern European countries, and the practice of science management remains largely a craft dependent on personal skill and intuition.

These two fields have much to offer each other, though little interaction and mutual interest currently exists. For the development of management science, science management offers a rich empirical base of problems and discernible decision processes both requiring and encouraging the emergence of new analytical formulations for descriptive and prescriptive studies in decisionmaking. For improvements in science management, though management science may offer little usefulness as a

technique-laden scientific approach to rational decisionmaking, it may be quite helpful as a source of concepts and strategy alternatives to science managers in particular problem situations.

The purpose of this paper is to identify the prospects that science management offers as a context for further developments in management science theory and analytical formulations. These prospects are abundant largely because of the exceptional nature of science management as a field clearly incompatible with the assumptions and focus of traditional management science frameworks. Additionally, current trends in management science formulations are reflected in many critical issues of science management, and thus may gain support from studies and approaches to those issues. Also, other science management problems require novel analytical approaches and challenge management scientists to create them. Why has science management been so neglected by management scientists? How can the obstacles it poses be turned to opportunities for new theory development? What kinds of developments can be expected to emerge from a focus on science management problems? These and other questions are answered in this brief introduction of science management to management scientists.

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MANAGEMENT SCIENCE AND SCIENCE MANAGEMENT: PROSPECTS FOR THEORY DEVELOPMENT

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INTRODUCTION TO PART I

Management science in the 1960's and 1970's gained considerable acceptance as a useful discipline for addressing diverse management problems in a wide variety of settings.[1] Surveys of usage (cf. Markland 1979,p.17 and Wagner 1970,p.25) attest to the application of management science techniques in the petroleum, paper, chemical, metal processing, brewing, food manufacturing, aircraft, rubber, transportation and distribution, mining, textile, cement, glass, computer, electronics, farm and industrial machinery, motor vehicles, and pharmaceuticals industries, as well as commercial banks, insurance companies, merchandising companies, and public utilities. Within government, applications are made in military affairs; health, education, and welfare; air and highway traffic control; air and water pollution; police and fire protection; voter and school redistricting; and program budgeting. Early business applications of management science techniques tended to be found in the management functions of production scheduling, inventory control, and forecasting, though current applications are also found supporting management decisions in plant size and location, new product development, marketing,

[1] Management science is used here in the generic sense also to include operations research, systems analysis, cost/benefit and cost/effectiveness analysis, systems engineering and engineering economics, decision theory, cybernetic management, and much of applied economics. Science management refers here to the functions and responsibilities primarily of research directors, sometimes called science managers, and of science policy advisors and administrators.

advertising, cash management and finance, investment portfolio management, mergers and acquisitions, and short-term and long-range corporate planning.

Notable in its absence from these lists of applications is the field and functions of science management, including both research management and science policy. Moreover, reasons are not completely obvious for the apparent immunity of science management to the penetration of management science applications occurring in so many other management functions and settings.[2] However, evidence of this immunity is readily available and may be associated with a number of circumstances. For example, science managers are still far more likely to have been educated in the field of their respective natural or social science discipline than in management (Wolfe 1974 and Gibbons 1963). Also, in the United States, attitudes of science managers and scientists alike tend to reflect disbelief in the applicability of management science techniques to critical research management concerns, or even in the idea that science efforts could be managed in any way comparable to the management of production or service delivery. What are the characteristics of science management that appear to prevent or at least resist application of management science approaches?[3] What does science management have to offer as a problem context?

Reverse Images

Science managers may well know the value of rigorous scientific methods within the substance of their own fields, yet find themselves in management positions requiring seemingly new and different tools (a) to shape consensus on plans and priorities among independently minded colleagues; (b) to fight off encroaching bureaucracy within their science organization; (c) to maintain the active support of funding sources and collaborators; and (d) to attract and motivate highly skilled staff using only persuasion, personal contacts, and moral commitments to institutional goals. In this particular context, science managers may view the intellectual and quantitative nature of management science as clinical, detached, simplistic and naive, and not focused on the more critical aspects of research management. Scientific methods, though fundamental to the conduct of scientific research, may seem useless in the management of it. Similarly,to the practicing management scientists, the realities of research management or of science policy may seem a morass of goals that are ambiguous, unoperational, and often hidden; dominant personality types; situation resolutions that emerge slowly more from complex interpersonal interactions or major social movements than from any conscious decisionmaking by science managers or science policy makers; and loose and fluid organizational couplings that

[2]Two significant exceptions are applications of management science techniques to research and development (R&D) project selection and to R&D program planning, both found within large scale industrial or military research efforts.

[3]Many authors identify unique characteristics of R&D management (cf. White 1975,p.2,252 and Freeman 1977), but do not assess the appropriateness of management science approaches to its problems.

defy hierarchy or the identification of the locus of authority.

Yet in the contrast of these images lies the source of potential mutual insight. Research management and science policy offer to management scientists application contexts that provide greatest exposure to the inherent difficulties, ambiguities, and overly simplifying assumptions of present management science methodologies and applications. Equally important, the reconceptualizations and broadening of perspectives that are now occurring in management science offer to the science manager insights into the complexities of managing his science organization and to the alternative strategies available to him as manager. Some of the potential contributions that management science and science management offer to each other, especially with regard to theory development in management science, are explored in the following chapters.

Part I

MANAGEMENT SCIENCE AND SCIENCE MANAGEMENT: EXAMINING THE ROOTS OF MUTUAL DISINTEREST

CHAPTER 1

MANAGEMENT SCIENCE: AN OVERVIEW

Hundreds of texts and articles on topics of management science are now available, each with its own set of terms defining the nature of management science and many claiming definition a futile exercise. This paper will make no attempt to resolve the differing opinions on definition nor will it be comprehensive in presenting all points of view. Rather, it will consider the totality of approaches under the generic label of management science and focus on fundamental characteristics as presented in basic texts published in the United States and Great Britain.

Fundamental Concepts

Management science is described as being, or being concerned with, the application of the *scientific method* to problems in management. Text authors have included in their definition that the problems addressed by management science are embedded in *systems*, and that this systems approach is fundamental to the management science method.[1] Some have also stressed the *interdisciplinary* nature of management science as an important component, especially in the operations research context.[2] Some also argue that a fundamental component of the scientific method and therefore of management science is the use of *models* as

[1]Emphasis on systems is found, for example, in the introductory books of Ackoff and Sasieni (1968,p.7); Markland (1979,p.7); and Ignazio and Gupta (1975,pp.10-11).
[2]See, for example, Markland (1979,p.6) and Ackoff and Sasieni (1968,p.7).

representatives of the system addressed,[3] though others are quick to point out that model-building, though essential, is not the central focus of the approach (Tomlinson 1979). Finally, most all introductions to management science texts have portrayed the purpose of management science as an *aid to decisionmaking or problem solving*[4]

Some authors of management science texts argue strongly that management science is not simply a collection of tools, techniques, or methods, but that it is a perspective or approach to solving problems, and that to separate the methods from the perspective or approach would be to relegate the management scientist to the role of technician rather than analyst[5] However, according to Drucker (1974,p.507), most managers continue to view the management sciences as a set of tools, and few have acquired the skills to use them effectively.

Traditional Steps in the Management Science Approach

Text authors also differ in the detail in which they describe the requisite steps or stages in applying management science as a method, but usually include the following four in one manner or another:

[1] *Formulating the Problem:* determining relevant characteristics or measures of the problem or of system performance relating to the problem; determining who are the relevant decisionmakers, and what choices they have or system variables they may effect; and determining what measures would indicate effects of system performance and problem solution.

[2] *Constructing a Model* of the system in which the problem is embedded.

[3] *Using the Model* to test hypotheses or derive solutions; including activities of data collection and manipulation, sensitivity analysis, and model validation.

[4] *Identifying Solutions* or conclusions and communicating them; including in some cases, testing the associated solutions in the real system rather than only in the modeled one, establishing controls to maintain system stability during implementation, and recommending to decisionmakers the course of action suggested by the analysis.[6]

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[3] See, for example, Wagner (1970,p.9); Littlechild (1977,p.23); Ackoff and Sasieni (1968,p.9); Quade and Boucher (1968,p.11); Markland (1979,pp.8-11); and Stainton (1977).

[4] Decisionmaking and problem solving are sometimes used interchangeably. However, emphasis on management science as an aid to decisionmaking is found, for example, in Wagner (1970,p.4) and Quade and Boucher (1968,p.2); and emphasis as an aid to problem solving is found in Markland (1979,p.3) and Ackoff and Sasieni (1968,p.6).

[5] Rejection of management science simply as a collection of techniques is found, for example, in Ignazio and Gupta (1975,pp.10,25); Markland (1979,p.7); and Quade and Boucher (1968,p.2).

[6] Implementation, evaluation, and subsequent recyclings are additional components of the decisionmaking process, and many authors include implementation as part of the management science approach. However, because implementation responsibilities are predominantly those of the decisionmaker rather than of the analyst (though the latter may assist), implementation is not included here as one of the traditional steps.

The applicability of management science to various contexts, including those of research management and science policy formulation, depends to a large extent on how well these steps are able to be performed within the relevant context. For the second step, that of constructing a model, this is determined primarily by the availability of techniques and model forms which match the problem and system characteristics. One management science text (Buffa and Dyer 1978) illustrates this matching by confining the characteristics of the problem/system to include (a) the nature of the environment as either certain or risky; (b) the nature of relationships among problem/system elements; and (c) whether there is a clearly defined, quantifiable function of the decision variables to be maximized or minimized. The corresponding management science approaches or models useful in various combinations of these problem/system characteristics are reproduced in Table 1 on the following page. This illustration is neither comprehensive nor does it imply that there is a best model framework for any situation. If anything, it portrays the severe limits on problem/system variation within which most available model frameworks lie.

Traditional Assumptions Underlying Management Science

Three fundamental assumptions underlie the management sciences as traditionally practiced. (1) Management decisionmaking or problem solving is basically a ***rational process*** and accordingly seeks and uses processes of rational analysis. Management science is therefore presented as a rational approach to identifying problem solutions or evaluating decision alternatives. (2) Management science's two primary criteria for evaluating alternatives have traditionally been those of ***efficiency and effectiveness***. Efficiency is simply a measure of output per input, or productivity, in either monetary or physical terms. Management science applications in industry, military, and government settings have traditionally focused on identifying solutions which are efficient. Effectiveness is the degree of attainment of output or accomplishment of objectives. This may include using multiple and also conflicting objectives, and by considering the trade-offs among them, determining an overall most effective solution. (3) Management science has traditionally been ***prescriptive*** in nature.[7]

Challenges to the usefulness of management science are often based on challenges to the realism or appropriateness of these underlying assumptions. Chapter 4 describes some of the current trends in management science toward adopting alternative assumptions.

[7]Chacko (1976,p.123) emphasizes, "An OR/SA (Operations Research/Systems Analysis) study is not an academic exercise in which the pursuit of truth is an avowed objective. The end-product is a recommendation; preferably to do something, or not to do something, which was not otherwise obvious." Specifically, Chacko defines "Operations Research is the heuristic art of prescriptive application of the scientific method to executive-type problems..."(p.12). Wagner (1970,p.5) adds that the principle results of management science studies must have "direct and unambiguous implications for executive action."

Table 1

Matching Management Science Approaches to Specific Objectives and Assumptions[a]

Management Science Approach:	Objective:	Assumption About the Environment:	Relationships Among Variables Can Be Characterized By:
Probability and Expected Value; or Decision Trees	Maximize Expected Value	Risky[b]	Outcomes derived from predictive models, optimizing models, or subjective estimations
Evaluative Models Based on Utility Functions	Maximize Expected Utility	Either Certain or Risky	Outcomes derived from predictive models, optimizing models, or subjective estimations
Forecasting Techniques	Minimize Forecast Error	Risky	Statistical Measures
Mathematical Models to Predict System Performance (Markov Chains, Queuing Theory, Simulation)	Understand Dynamics; Test Sensitivity to Changes	Certain	State Transition Probabilities; Single and Multiple Waiting Lines and Service Facilities
Optimizing Models (Linear Programming, Regression, etc)	Maximize or minimize a function of the decision variables	Certain or Risky	Linear (or more complex) algebraic expressions
Network Models (PERT/CPM)	Maximize or minimize a function of the decision variables	Certain	Networks of activities linked by prerequisite relationships

[a] Adapted from Buffa and Dyer (1978, Table 16-1, p.478) where a similar table is used to identify text chapters corresponding to different management science approaches described in the text.

[b] Risky is meant to imply both that more than one future state may occur and that through past experience or judgment, meaningful probabilities to each can be assigned.

Basic Limitations

Researchers and practitioners of management science, as well as users, are quick to point out the inherent limitations of its approach. Some of the more basic limitations are described below:

[1] Management science results cannot be the sole basis for decision-making or problem solving in any real situation no matter how sophisticated the analysis. Results of rational analysis are only part of the total input to any decision or problem situation involving people, institutions, and social contexts. Management science is no substitute for managers, nor can its analytical frameworks take everything into account.

[2] Solutions found to the modeled problem/system are not necessarily solutions to the real situation. The important question is whether any improvements to the real situation can be generated by actions based on solutions to the model.

[3] Despite the tremendous number of instances of management science applications to date, there are still few standard model frameworks or techniques for ready application. Most model formulations are tailor-made to a large degree (Wagner 1970,p.8).

[4] Tailor-made models emphasize that much of management science is still an art or craft requiring creativity, ingenuity, and competency in model design. Talent needed to develop or adapt appropriate models may not be readily available in any problem situation, or may be available only at high cost.

[5] As a consequence of [3] and [4], applications of management science usually involve fairly large efforts and corresponding costs. A survey in the early 1970's of the top five hundred U.S. corporations found that the average duration of management science projects was about ten months, and that an average of 2.5 people were assigned (Markland 1979,p.632). In another source (Wagner 1970,p.538), most operations research projects are estimated to require two to three man-years and three to four months time. However, when relevant computer programs are available, and relevant data are accessible, and problem formulations adequately fit the models programmed, then costs of analysis may be only a few hundred dollars (Buffa and Dyer 1978,p.474).

[6] Models using subjective or probabilistic inputs generate outputs which are also only probabilistic at best. No systems involving human interactions are purely deterministic. Consequently, in any social system, there are no guarantees of anticipated system performance, despite the scientific method of analysis employed.[8]

[7] Problems in social systems, particularly in human services areas including that of research, often pose additional difficulties for management science applications. These include: (a) unavailability of data critical to the systems analysis; (b) poor validity or reliability of

[8] One of the most delightful footnotes regarding reasonable probabilities associated with human behavior is in Quade and Boucher (1968,p.355). In true Damon Runyan fashion, we are advised, "Nothing what depends on humans is worth odds of more than 8 to 3."

the data over time; and (c) problems of confounding effects caused by multiple influences on and by the group of people involved in the system (Hershkowitz and Spindler 1975,p.396).

[8] Management science approaches to situations involving many independent decisionmakers are still not formulated well, though situations of multiple objectives for a single decisionmaker or decision group have been addressed by many authors in the last few years (cf. Bell et.al. 1977). For cases of multiple decisionmakers, the critical problem is to determine whose trade-off preferences to use, or more generally, how to construct aggregate preference functions.

[9] Further limitations to problem formulation, often severe in complex social systems, occur when goals and objectives are imprecise, unoperational, and unable to be quantified, or when measures of system performance or effectiveness are lacking. Psychological concepts, for example, pose severe measurement problems.

[10] Finally, there are limitations within the management science profession itself, especially with regard to attempted applications in social system contexts. These include: (a) tendencies of analysts grounded in quantitative manipulation of hard data to avoid, or to treat with suspicion, social science concepts and measures; (b) tendencies of analysts to use sophisticated mathematical methods whether required by the particular problem context or not; (c) predispositions of analysts toward precision in quantitative measures, when decisionmakers may be more concerned with and sensitive to the non-quantifiable factors; and (d) tendencies of analysts to lack familiarity with the subject matter of the social system context and with social science methodology (Hershkowitz and Spindler 1975,p.395).

As noted in the previous section, the applicability and appropriateness of management science methods in various management contexts depends to a large extent on the relevance of the traditional assumptions listed earlier. Additionally, applicability of management science depends on the degree to which the methodological limitations listed above are significant factors in the problem context.

CHAPTER 2

SCIENCE MANAGEMENT: AN OVERVIEW

The term science management is used here to refer to two rather separate activities. The first is the management of scientific activity within a laboratory, a research-related corporate division, or a scientific institution. This is commonly referred to as research management or R&D management. Of interest in this paper is the person at the head of the research unit; his management problems, and the methods or knowledge available to him to assist in problem resolutions. The second activity is the formation of national or international policy governing or at least influencing the growth, distribution, funding, and focus of scientific activity among the many scientific institutions affected by that policy. The relevant managers for this paper in this case are the science agency administrators and policy advisors who shape science policy. Both research management and science policy strive for the advancement of knowledge and its application to human needs. Both are also concerned with incentives, capital resources and their allocation, and the productive use of scientific talent, among other indirect objectives such as economic development and national security. Few applications of the methodologies of management science are evident in either of these two kinds of activities, at least within the West, and with the exceptions footnoted earlier. The following sections examine the major characteristics of each activity as an aid to understanding why.

RESEARCH MANAGEMENT

Research is search for the unknown. The generation of knowledge happens mostly by meticulous applications of scientific methods, sometimes by serendipity, frequently by the support of large numbers of independent approaches, sometimes by the effort of large coherent national programs, and sometimes not at all despite huge investments and elaborate forethought. For these reasons, research management has been compared to gambling, or worse, "taking incalculable risks for unassessable rewards" (Bernal quoted in White 1975,p.11). Science as a creative process may be seen as impervious to control or management. Arguments for such views are based on the nature of research as a productive enterprise, and the resulting problems of research management which differ from problems in other management contexts. Among these are the allegiance of scientists to a discipline and its career stability outside the employing organization; difficulties in evaluating progress, probability of success, usefulness of results, and required resources; and the tendency to reject bureaucratic authority as the proper basis for decisionmaking in scientific matters (White 1975,pp.2, 252).

In scientific organizations, traditional management functions such as marketing or aspects of finance are either handled by host organizations, are substituted by other science related activities such as publication and proposal writing, or are considered necessary but not priority items for management attention. As yet there is no clear and comprehensive listing of the functions or responsibilities of research managers. However, different listings from different perspectives offer insight into the complexities and ill-defined nature of the research management process.

Functions and Responsibilities of Research Management

Some articles on research management highlight the role of the research manager as the bridge between top-level goals and concerns of either the corporate host, the board of governors, or the institution founders on the one hand; and the individual interests and initiatives of the scientific staff on the other (Churchman 1970 and White 1975,p.184). According to Churchman, such a role requires the research manager to be "sanely schizophrenic" by holding simultaneously two partially conflicting value systems and views of the organization. Within this perspective, the functions of research management may be divided first into those responsibilities related to overall institutional concerns, such as (1) keeping higher level management informed of the scientific climate of relevant substantive and methodological advances, of contextual opportunities for new research, and of the organization's related technical competitive position; (2) developing research plans to meet institutional goals; and (3) maintaining good working relations with other institutional divisions or collaborating organizations. In contrast are those responsibilities related directly to scientific personnel, such as (1) hiring, firing, and promotion; (2) establishing an organizational structure and set of task assignments; (3) developing policies for recruiting and training; (4) determining or recommending appropriate renumeration and other incentive systems; and (5) developing a style of management conducive to individual creativity, initiative, enthusiasm, and focus on project objectives, time constraints and financial limitations.

Activities of science managers within national funding agencies provide a second, more procedural view of research management focused primarily on the program of research rather than on the organizational setting or the interactions among scientific performers. In one typology (Wirt et.al. 1975), the research management process is divided into three categories of activities: program planning, program management, and program evaluation. Program planning is composed of (a) determination of priorities among programs; (b) detection and incubation of new program ideas; (c) determination of an initial program strategy, intraprogram objectives, and the relative priorities among objectives; and (d) continual reassessment and readjustment of intraprogram objectives and priorities. Program management consists of project generation, project selection, project monitoring, project utilization, and project evaluation. Program evaluation consists of assessment of substantive and managerial accomplishments and deficiencies; and recommendations for changes in program objectives, priorities, management, and organization.

A third perspective is related to the special problems of managing international cooperative research, where attention to interpersonal interactions is especially important. Wilson (1980) defines these problems as (a) those arising from national ethnocentrism about definition of science terms, relevant problem dimensions, and professionally appropriate ways of investigation; (b) those relating to appropriate involvement of international team members so that research is a cooperative and reciprocal enterprise rather than a form of academic imperialism; (c) those arising when team members are spread geographically and have unequal financing and scientific resources available to them; and (d) those arising from social and cultural differences which affect perceptions of what constitutes effective work arrangements. In the highly problematic context of international research, effective management requires attention and appropriate responses to situations that may emerge from these many sources of conflict and that may affect research performance adversely.[1]

Finally, whether within a host organization or heading a separate scientific institution, research managers often engage in both entrepreneurial activity and in activity to restrain, avoid, or cope with crises. More time and effort may be spent on creating opportunities for research efforts; building contacts with prospective funding sources, potential users of results, and potential collaborating institutions; and containing crises brought on by overly influential outside non-scientific elements, fear of loss of financial or technical base, or any number of reasons, than on all the routine procedural elements of research management listed earlier. All the different views expressed above are combined in brief form in Table 2.

[1]Some of the situations Wilson describes as requiring effective management response in international cooperative R&D projects are: situations in which project members are uncooperative in taking others' views into consideration during the problem formulation stages; situations in which project members differ in how they assign accountability to each other for the tasks they have to do; and situations in which project members expect different kinds of professional recognition for their work in the project.

Table 2

Aspects of Research Management
Functions and Responsibilities[a]

Entrepreneurial Aspects

- Creating Opportunities for Research by Matching Interests of Supporters to Capabilities of the Organization
- Attracting and Hiring the Key Research Staff
- Building Cooperative Links with Others

Executive Aspects[b]

- Maintaining Effective Liaison with the Board or Host Organization
- Conceptual Planning of Major Research Programs
- Budgeting and Major Resource Allocation Decisions
- Designing the Basic Organization Structure
- Evaluating Progress and Continuing Potential of Major Programs
- Establishing Policies on Recruiting, Publications, and Job Security

Psychotherapeutic Aspects

- Inspiring Staff to Creative Work
- Motivating Staff to Hard Work of High Standards
- Providing a Supportive Environment for Creative Scientific Work
- Resolving Major Disagreements among Key Scientific Staff, Research Leaders, or Collaborating Partners

Crises and Perennial Problems Aspects

- Responding to or Preventing External and Internal Crisis Situations
- Responding to Perennial Concerns (expressed by those outside the research organization) of costs, efficiency, equities, etc.[c]

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[a]This is a highly condensed outline of functions and responsibilities either listed in the text or implied by it. Little if any literature is available on either the entrepreneurial aspects or the crisis and perennial problem aspects of research management, though personal experience within various research organizations leads me to believe that the bulk of time spent by the research director is on these two aspects, especially the latter.

[b]Planning, budgeting, resource allocation, and evaluation are the functions traditionally thought most amenable to support from the management sciences. In fact, few applications are made within the context of research management, with exceptions noted in the text. Wirt et.al. (1975) provide additional detail on actual management practices used within the executive aspects, for some U.S. federal research agencies.

[c]Responses may include, for example, conducting comprehensive internal administrative reviews, cooperating with an assortment of external reviewers, and initiating structural reorganizations to symbolize attention focus on the concerns expressed.

Required Competencies of the Research Manager

What does it take to fulfill these disparate functions and responsibilities? A business magazine article listed the attributes of research managers seen as most important. They included technical knowledge and ability; ability to get along with people; and conceptual ability, with the relevant importance of each depending partly on the level of management. To this list were added eight other attributes selected by the Industrial Research Institute (IRI) Research Management Study Group as being most important to a research director: decisiveness, leadership (inspirational rather than coercive), flexibility and understanding in order to deal with the unpredictable; tempered optimism; independent drive; energy; articulateness; and ability to initiate and to project into the future (Bowie 1964).

In the context of international collaborative research, characteristics noted by Wilson (1980) as contributing to effective research management are flexibility; patience; openness to change and to others' points of view; ability to deal with ambiguous situations, different communication styles, and interpersonal conflict; self-confidence and professional security; and ability to show little tension in difficult relations with others and to manage personal stress well.

Research managers themselves sometimes have lists of characteristics required of them in their jobs of of others in similar roles. Levien (1980), listing desirable qualifications for his successor as director of the International Institute for Applied Systems Analysis (IIASA), includes: (a) dedication to the ideals and goals of the institution; (b) having a vision of the research organization's further development that is consistent with that of the governing board or host institution; (c) experience with the scientific subject content; (d) experience with managing research (applied and interdisciplinary in this case); (e) combining a dedication to excellence with a tolerance for diversity; (f) leadership without egoism (ability to help others flourish rather than overshadowing them); (g) experience as a manager of funds and facilities; and (h) willingness to serve as research manager for an extended term.

Others searching to fill a position of research director may seem extreme in the qualifications they seek. Sproull et.al. (1978) cite a memo from the U.S. Assistant Secretary for Health, Education, and Welfare stating that the first director for the U.S. National Institute of Education (NIE), responsible for coordinating all U.S. educational research, should be:

> [1] A humanist...who never loses sight of the gravity and immediacy of the human problems whose solutions our research seeks.
>
> [2] An eminent scientist...who can elicit the support of researchers and practitioners, as well as direct the attention of other intellectuals currently outside the education research area to the solution of education's important problems...
>
> [3] A capable administrator and representative...with

experience in administering large scale research projects or similar undertakings...and able to articulate (NIE's) mission and its value to the remainder of the government and the general public.

[4] Politically neutral or independent...(pp.115-116)

In these and other listings of required skills in research management, little mention is made explicitly of ability in rational analysis in general, and of knowledge of or use of management science in particular.[2]

SCIENCE POLICY

A clear definition of science policy is as elusive as one for management science. For example, former U.S. Science Advisor Edward David views science policy as concerned with the infrastructure of science as an enterprise; with science manpower, education, technological project management, science communication, etc. (David 1973). Salomon of the Organization for Economic Cooperation and Development (OECD) defines science policy as the collective means or interventions taken by government both to encourage the development of science and to exploit its results for general political objectives (Salomon 1977,p.45). A similar view is expressed by Soviet Deputy Chairman of Science and Technology Gvishiani (1973,p.175) when defining science policy within the Soviet perspective as the system of government measures aimed at directing scientific activities. In these views, developing and implementing science policies addresses similar basic concerns at the national or international level as research management does at the level of the individual scientific organization.

Attention to science policy has gained importance only within recent decades. Science policy goals and roles within different national settings have also shifted substantially during this time, and continue to do so in response to changes in social and economic concerns, and issues of national security. At different times and to different degrees, U.S. science policy goals have emphasized military preparations (weapons development), national standing (space race), national economic growth and productivity (including related responses to energy needs), improvements in health and disease control (establishment of the National Institutes of Health and the National Cancer Institute), and both viability of the nation's scientific community and protection of the nation's population against risks and misuses of science. This last concern of public protection from science is seen by Nelkin (1979,pp.10-15) as arising from: (a) public unease and fear of potential health and environmental risks resulting from scientific advances (such as those stemming from nuclear and recombinant DNA research); (b) fear of misuse or harmful applications of

[2]Wolf (1974) observes the difficulties that scientists or engineers turned science managers face, and recommends mid-career training, for those who make this transition, in both broader aspects of science and science policy as well as managerial skills in organization, budgeting, and personnel.

science (as from genetic engineering and social or behavior control); and (c) protection of individual freedom of choice against exposure to possible harm or inconvenience in order to benefit a larger community or society as a whole (as in regulations regarding fluoridated water, air bags, and prohibitions of saccharin).

Elements and Issues of Science Policy

Like research management but on a national scale, science policy is concerned with allocation of resources, provision of a supportive environment for creative work, building cooperative links, assigning priorities for major research thrusts, attracting, training, and motivating scientists, and responding to prevailing concerns and crises. Also, as in research management, science policy instruments and interventions are largely indirect. Incentives in science policy usually operate on organizations in forms of tax policies, federal standards and regulations, differential financial support, and the creation of mission-oriented scientific institutions.[3]

No comprehensive listing of science policy issues, elements, or major questions exists in the literature, nor may one be possible given the rapidly changing nature and focus of science policy even within a single nation. However, Table 3 on the following page provides a sample of some of the more important issues or elements which have been addressed either explicitly or implicitly by science policies as background to later assessments of the possible role of management science concepts in contributing to the formation of science policy.

Basic Assumptions Underlying Science Policy

Four basic assumptions give rise to the many issues of Table 3 and explain why science policy has gained the importance it has today. The first is that applications of science can indeed be ***directed to practical needs***. The history of science is full of evidence to this effect. However, for reasons listed earlier regarding public risks and fears, whether science has and always will be directed toward positive ends with positive results is a matter or public contention, at least in the U.S. Nevertheless, science continues to be regarded, according to former U.S. congressman Emilio Daddario (1973), as a natural national resource.

The second basic assumption of science policy is that science is an ***important*** national resource. In other words, science has become sufficiently important and useful to make significant differences in matters of economic development, military superiority, and world standing, or that it can help solve important problems or help meet important national goals. Again, history offers much evidence in support of this assumption.

[3]Incentives may also be applied to the overall class of scientists within a nation. In the U.S.S.R., added indirect incentives include the high prestige and privileges given to scientists relative to other occupational groups. In the U.S., relative salaries and perceived status of scientists is not as high, though scientists generally do enjoy relative autonomy in their work compared with professionals in profit-oriented organizations.

Table 3

Science Policy Issues,
Elements, and Questions Addressed

How Much and What Kinds of Science Are Needed

[1] In what directions and to what extent should science be developed; e.g. related to military objectives, energy goals, space and aeronautics, medicine, and related to basic and applied research.

[2] What should be the proper balance or allocation in manpower and funds for science objectives and other social objectives.

[3] What kinds of research should not be done.

[4] To what international goals should international scientific activities be directed, and with what priority.

[5] What supply and training of scientific manpower is required.

How Are These Decisions to be Made

[1] Are the crucial policy choices to be made by experts serving as science advisors, or through the participatory processes of public representation, or by government bureaucrats or appointed science agency heads.

[2] Should the directions of research be left to the community of individual scientists funded by grants from general research budgets, or should it be directed toward specific national needs.

[3] What are the criteria and procedures to be used for selecting and funding major scientific programs (big science) as well as small research grants (little science) and international activities.

What Mechanisms Are Appropriate

[1] What institutional arrangements are best for science policy formulation; for providing scientific information, consultation, and advice to government; and for coordinating scientific activities.

[2] Which methods for deciding budget allocations are most appropriate to overall science goals: formula allocations, distributions based on individual merit as judged by peer review systems, or other methods.

[3] What forms of relations should exist between government and academia and what mechanisms are most appropriate to the relations: e.g. research grants or contracts, advisory systems, cross flows of personnel.

Table 3 (cont.)

[4] Should government support of basic research in universities be through institution grants, or grants to individuals, or both.

[5] When and to what extent should research be done by government-owned laboratories and research institutions.

[6] What forms of relations should exist between industry and academia, and what government mechanisms are most appropriate to develop them.

[7] What kinds of mechanisms or strategies (such as tax incentives or standards) should be used to direct scientific effort without stifling its development.

[8] What organizational mechanisms are best for planning, coordinating, and implementing international scientific programs.

What is the Proper Role of the Federal Government

[1] Who should pay for what kinds of science efforts (in particular, in what conditions is government funding justified).

[2] What aspects of science development should be centrally planned* and what organizational forms are best suited for science planning. Also, to what degree should government support civilian industrial research (to help cover high-cost risks).

[3] What is the proper balance of centralized support and control of science versus pluralistic and decentralized support and control.

[4] How important is the geographic placement of scientific institutions to both scientific and other goals of regional development and equity.

[5] To what extent should scientific relations with other countries be supported.

[6] To what extent should science policy itself be well formulated (how desirable is a strong coordinated government-industrial-academic complex).

[7] How far should government go in financing applications and commercializations of research results in efforts to stimulate industrial innovation.

*Naturally, these and related questions of centralization versus decentralization and autonomy in science planning are dependent upon national political contexts.

The third assumption is that without the government support and interventions which comprise science policy, integration of science efforts and their focus on national priorities would not be achieved, and so the *usefulness of science* as an important national resource *would be less* than it would otherwise be with effective science policies. This is the heart of the Soviet rational for centrally planned science and the assumption underlying such U.S. programs as Resources Applied to National Needs (RANN) within the National Science Foundation. Corollary arguments are made to support science policies as the basis for effective information exchange and accumulation across national sectors, for international pooling of science experience, for standardization of methods and definitions, and for effective utilization of expensive equipment which without proper external coordination of interests would neither be made available to any one lab on the basis of its own needs nor fully utilized.

The final basic assumption leading to the issues of Table 3 is that the principle mechanisms for shaping and developing scientific contributions toward national goals are the indirect influences of institutional arrangements, funding procedures, tax and regulatory provisions applied to scientific activity, and other forms of incentives. In other words, scientific advances and applications *cannot be legislated or managed in the traditional direct manner* used in the creation of laws or the production of physical goods.

Certainly these are not the only assumptions underlying U.S. or other nation's science policy, but they are probably the most fundamental ones. Other assumptions, however, may help explain more clearly the particular funding patterns or justifications of public support for science efforts, such as the controversial "spin-off" and "trickle down" assumptions of U.S. supported defense and space research efforts in the 1960's. All underlying assumptions in any national context are important in that they shape the issues addressed and may also determine to a great extent the science choices made.

Formulation of Science Policy

Many of the characteristics of research management listed earlier are reflected at the national level in the formulation of science policy. Research managers rely on their ability to solicit and understand the diverse interests and perspectives of the scientific staff and others, and to develop agreements which accommodate both these and institutional objectives. Similarly, science policy formulation involves the solicitation of opinions from throughout the scientific community and the coordination of views into an expression of policy. Coordination, persuasion, and agreement are integral parts of both processes. Making and implementing major decisions in both research management and science policy is usually a lengthy process of multiple discussions and approvals. Science policy formulation may require years. Big science projects in the U.S. involving millions of dollars may take three to thirteen years to pass successfully through the stages of receiving support from the scientific community, acquiring design specifications and cost estimates, being accepted into the national budget request, receiving authorizations and appropriations for funding, and beginning actual construction (York

1973). Science policy postures regarding the use of nuclear energy, for example, may require even longer to stabilize, with several changes in government stance along the way.

Part of the reason for the length of the process is that science policy is basically a *political process*. The amount of national funds to be provided in support of science is a political decision. Similarly, the federal support of science applications to specific military development projects or major alternative energy projects are also political choices. Two additional characteristics of decisions of science policy add further length in time and substantial controversy to the political processes involved. The first is that science policy decisions usually involve highly technical considerations and that current knowledge of the technologies employed is less than perfect. This is particularly true in the cases of advanced technology projects with low probabilities of very high cost and harmful accidents. In these situations, technical assessments from various interested parties may differ widely in the estimates they provide, lending a great deal of confusion and mistrust to the political procedures involved. The second characteristic adding to the length of the process, at least in the U.S., is that science policy formulation is often a form of adversary politics in which parties are arguing about different and often incomparable things. In these situations, scientific advice, national agency objectives, societal interests, and local community concerns create such a confusion of interests and assessments that those involved in making policy choices are hard pressed to find either rational processes on which to rely, or procedures politically acceptable to all parties with which to reach some consensus position.[4]

Processes of science policy formulation are by no means uniform across nations or across situations within any one nation. A process of referendum for deciding continued construction of nuclear power plants is used by some countries and not by others, and by some U.S. states and not by others. The role and importance of technical analyses, policy analyses, environmental impact assessments, and hazard analyses varies widely from case to case. Even mechanisms for routine decisions are subject to challenge. Periodically within the U.S., attacks are made on peer review as the primary process within national science agencies for reviewing individual science proposals.[5] The substance of these attacks may be on peer review as a form of inbreeding of ideas and of focusing support to only a limited set of prominent individuals, or for supporting science for its own sake to too great an extent (Price 1976,p.103). Even the need for scientific advisors to government is continually challenged as, in the U.S., the position of Science Advisor to the President and various Science Committees are created, disbanded, and created anew.

[4] One example of science policy formulation exhibiting all these characteristics and for which research is currently being conducted at IIASA concerns the siting of liquefied energy gas facilities in relation to nearby residential communities (Lathrop 1976).

[5] A description of peer review as practiced by the U.S. National Institutes of Health is given in Wirt et.al.(1975). Essentially, panels of scientists in fields related to the research review proposals and assign numerical ratings along various dimensions. Projects are then funded largely on the basis of these scores. Reviews and score assignments may be done either together as a group or individually by mail.

Problems in Science Policy Today

Science policy in the U.S. became fraught with frustrations and difficulties in the 1960's and 1970's largely due to attempted applications of science strategies and funding patterns, proven successful in military and space programs, to major social objectives as means to counter emergent social ills.[6] Salomon (1977,p.60) describes a number of the problems encountered in this attempted transfer. First, the technological objectives in military and space achievements drew on substantial knowledge, techniques, and practical experience. In contrast, the social objectives to which science was subsequently directed could rely on no proven techniques, nor were the objectives themselves clear and operational. Secondly, the social objectives were tied to a variety of social, economic, and political interests and so had to be approached in the difficult context of multiple, divergent, and often conflicting goals reflective of a large collective constituency. As a consequence, science programs toward social objectives were continuously adapting to new compromises among supporting interest groups, new understandings of the problem and of the viability of alternative approaches, and changing standards such as those represented in changing legislation on pollution control. In these cases, the role of research and development in the lessening of social ills is not at all clear, and recent acknowledgments of lack of successes have reversed a wide range of science policy positions.

Determining the proper role of the scientific community or science advisors within the formulation of science policy has been an additional continual problem. The basic controversy surrounds the requirement for sound scientific assessments and understandings on the one hand, and the arguments, summarized in Bozeman (1977,p.63) on the other, that scientists are ill prepared and unsuitable for participation in the political processes entailed.[7]

A third major problem in U.S. science policy today is the public attention and reaction given to issues which place national needs against local community concerns or individual beliefs. Local concerns may involve issues of environmental quality, personal security threatened by national computer data banks, or the nearby siting of a large facility of potential danger. Individual beliefs may simply be in contradiction to the support of a science activity such as nuclear experimentation. In these contexts, arguments and analyses are brought to bear by opposing factions, usually with quite different conclusions, to support respective positions. Furthermore, resources for conducting sophisticated analyses are often unbalanced among factions, and so resorting to argumentation

[6] For a concise and compelling account of this attempted science strategy transfer, see "On Escaping the 'Moon Shot' Metaphor" (Rettig and Wirt 1977).

[7] Bozeman refers to arguments that "scientists are ill prepared for a role in politics because they lack the lawyer's training in bargaining, compromise, manipulation of symbols, and negotiation of favors." Greenburger et. al. (1976,pp.332-333) argue further that scientific approaches are incompatible with the political processes of policy formulation. They cite that political authorities "resent research studies that produce contention, attract widespread notice, accentuate disagreements, and aggravate conflict." Scientific expertise can thus serve to inhibit resolution rather than foster it. This is especially true when various interest groups provide their own sources of scientific expertise and the experts disagree.

based on highly technical analysis becomes a political tactic to shift the basis of decision influence to one of greater power inequality.

Perhaps the most perplexing problem facing U.S. science policy today is how science and technology policies can be used to help increase the competitiveness of American products in international markets. Bueche (1973) believes that some of the current U.S. policies may result in exactly the opposite effects as intended. He cites government bail-outs of major technology-based private corporations, greater funding of and use of R&D by the federal government for a broad spectrum of potential applications, and government induced R&D cooperatives among private industries, all in support of international competitiveness, as liable to reduce, in actual effect, both the incentive for private R&D and the domestic competitive nature of American industry. The effect of the latter may be, in turn, to decrease competitiveness in either foreign or domestic markets.

Overall, the formulation of science policy in support of national goals remains an enigma. Schmandt (1975,p.192) calls the massive use of science and technology for the deliberate planning and building of society an art that man has not yet mastered. Turkevich (1977,p.40) concludes that the Soviet centrally planned system apparently has also not yet solved the problem of how to plan science and technology development. Yet support for the establishment of a science of science continues in the U.S.S.R. (Gvishiani 1973) and in other countries[8] for the purpose of developing scientifically planned, organized, and directed national science efforts, and for the application of advanced techniques of management and control to promote greater science effectiveness.

[8]Evidence for a growing international interest in developing a science of science is given by the recent establishment of two journals *Science of Science:* An International Journal of Studies on Scientific Reasoning and Scientific Enterprise, published by the Polish Academy of Sciences, Wroclaw Poland; and *Scientometrics:* An International Journal for All Quantitative Aspects of the Science of Science and Science Policy, published by Elsevier Scientific Publishing Company, Amsterdam. Scientometrics is defined as the study of the quantitative laws and regularities in the dynamics and structure of science.

CHAPTER 3

MATCHING MANAGEMENT SCIENCE AND SCIENCE MANAGEMENT: AT FIRST GLANCE

To date, management scientists have given little attention to science management as a field of potential application. Within the many aspects of research management, applications of management science techniques tend to be found only within the executive functions of project selection, project planning, and capital budgeting for R&D projects, and here only for large institutions with substantial numbers of projects to consider (Wirt et.al.1975,Appendix 3 and Souder 1978). Within science policy formulation, management science applications are even less evident.[1] It seems then, at first glance, that management science and science management have little to offer each other as approach and context. To a great extent, this is explained by an apparent mismatch in favorable contextual conditions.

[1]Analytical tools for improving the effectiveness of national science policies are described in Long and Wright (1975,pp.9-10) as including budget analysis, manpower studies, analysis of the character and productivity of different fields of science, and assessments of the impacts of technology. Other formal techniques such as cost-benefit comparisons or technology assessment techniques are also found in science policy formulations. None of these, however, represents the full management science approach as described earlier.

Favorable Conditions for the Application of Management Science

In any problem context, conditions affecting the suitability of management science and the probability of successful implementation of findings pertain to: (a) problem characteristics, especially problem definition clarity; (b) attributes of the system in which the problem is embedded; (c) attributes of the system environment; (d) attitudes and values of those to whom the management science analysis is directed; and (e) characteristics of the organization or social context within which the problem is to be resolved. Major favorable characteristics are described below:

(a) Characteristics of the Problem or Decision Favorable to Management Science Application:

[1] The problem or decision is *well-defined;* i.e. characteristics determining successful outcome or desired degree of improvement in system performance can be stated a priori and can also be measured.

[2] The problem or decision is perceived by or is the responsibility of an *individual or single interest group*.

[3] The nature of the problem or decision *does not change* during the search for a solution or the deliberation among alternative approaches.

[4] The problem is sufficiently *important* (worthy of management science efforts and costs) and does *not require an immediate solution* (time for application of a management science approach is available).

Additional favorable conditions which further characterize the well-defined nature of a problem or decision are that: (1) the alternative actions or choices are known, and new alternatives are not generated during the approach process; (2) the consequent outcomes of each action or choice alternative are also known or can be measured; and (3) the degree of system improvement or successful outcome can be expressed in a quantitative objective function or single rank ordering of action outcomes.[2]

(b) Characteristics of the System in which the Problem or Decision is Embedded Favorable to Management Science Application:

[1] The system, or relevant parts of the system in interaction with relevant parts of the environment, is relatively *bounded* (or closed) and of *few kinds of relevant parameters and variables* (system elements).

[2] Relationships among system elements are of *known causal direction* and can be *represented by mathematical expressions* or programmable statements. This tends to exclude most psychological constructs as system elements, and most human behaviors as system variables.

[2] Simon (1955) first identified these conditions as requirements for the "classical" concepts of rationality in decisionmaking.

[3] Conditions [1] and [2] may apply only after considerable analysis and experimentation has occurred, involving the testing of alternative system structures in models and the estimating of model parameters. Accordingly, conditions favorable to management science application in the immediate absence of the previous two conditions are those identified by Beck (1981) as helpful in model structure design. These include: (a) that *experiments* can easily be conducted, or outcomes of natural experiments observed, to identify system behavior; (b) that a priori *theory* is capable of predicting accurately the system behavior that is observed; and (c) that data accumulated through system behavior observation are *data relatively error free*. Such enabling conditions allow successive model testing and design modification for developing (or acknowledging the absence of) the earlier conditions [1] and [2].

[4] The *system remains unchanged* during the period of analysis; i.e. is not naturally evolving or undergoing transformations in unpredictable ways, and is not changing as a consequence of the analysis performed on it. This tends to exclude many social systems in which changes in key personnel have major effects on system behavior, and many institutions for which rapid development and maturity alter system structure and relative importance of performance criteria.

[5] *Data* on system behavior and performance are *available at low cost*.

(c) Characteristics of the System Environment Favorable to Management Science Application:

[1] The most favorable conditions are if the system environment can be forecast with *certainty*, and the relations between environmental change, if any, and system performance can be represented by *logical or algebraic expressions* of system and environmental variables.

[2] If the environment cannot be forecast with certainty, and environmental changes cannot be related to performance effects directly, then the next most favorable condition is if *probability distributions* of possible environmental changes and their effects can be determined.

(d) Prevailing Attitudes, Values, and Motivations Favorable to Management Science Application:

[1] Attitudes and values of those to whom the analysis is directed reflect *reliance on expertise, rational analysis, and a system-wide perspective* as the appropriate basis for problem solving or decisionmaking rather than personal experience, tradition, dogma, cultural imperatives, coercion, or interorganizational rivalry (Vertinsky et.al.1975,p.254 and Smith 1966).

[2] Motivations of the parties involved *reflect or support the goals of the system or organization* in which the problem is embedded, rather than personal objectives or interest group positions on important issues of the problem.

(e) Favorable Organizational Conditions[3]

[1] High level of managerial support, understanding, and acceptance of management science activities;

[2] Location of the management science group within the organization conducive to effectiveness (in data gathering and implementation, for example);

[3] Adequacy of resources made available;

[4] Enabling nature of the charter given to the management science group;

[5] Low strength of opposition to management science activity within the organization;

[6] Existing high reputation of management science activity within the organization, and general perception of high level of success of efforts.

Neither research management nor science policy readily provides these conditions favorable to the application of management science. Rather, they seem to present the worst case situations in many ways. Conditions within various research organizations naturally vary widely according to size, host environment, and nature of the research addressed. Similarly, conditions surrounding the formulation and choice of science policy alternatives vary widely according to social and political context, national perceptions of the role of science and science policy, and the nature of the contextual issues addressed. However, despite these variations, some general characteristics of the problems, systems, environments, and prevailing attitudes of those who might apply management science approaches in science management settings can be described to portray the apparent contrasts to the favorable conditions mentioned earlier.

Decision Characteristics in Science Management

Research management entails a wealth of both routine decisions on hiring, publications approvals, and expenditure authorizations; and important design decisions regarding the organization and management of research efforts. Some of the design decisions are indicated in Table 4. Generating alternative design choices is usually not a major difficulty; diversity in management approaches is quite evident and various management procedures and structures are well documented (cf. Wirt et.al.1975, Levien et.al.1971, Smith 1966). What makes these design decisions ill-defined is that there are no dominant management solutions, even for specific situations. Each alternative has its many advantages and disadvantages, and the relevant criteria for judging are often diverse and conflicting. Furthermore, overall organizational criteria for assessing

[3]According to studies at Northwestern University by Rubenstein (Markland 1979,p.633), the organizational conditions listed here determine success or failure of management science groups. In addition to these organizational conditions, Rubenstein stresses both high technical and organizational competence of the individuals in the management science group, and relevance and practicability of the projects they undertake as important factors in determining success of the group.

success may themselves be conflicting, unoperational, or even hidden. Some of the many relevant criteria for judging research management approaches to decisionmaking or problem solving are given in Table 5.

The great difficulty in measuring or even making subjective assessments according to most of these criteria is what makes research management decisions particularly unfavorable for application of management science approaches such as those using ranking models for choosing among decision alternatives. Additionally, because of lack of resources, time, and confidence, research managers may feel that attempts to apply management science approaches simply aren't worth the effort in most research management problem situations. Nor is there any evidence that using a formal model which combines assessments on all criteria deemed relevant to the situation produces choices which research managers would agree are better or equal to those they would make based on personal experience, intuition, or expert advice.

Science policy problems pose different though even stronger obstacles to management science applications. Certainly science policy problems are sufficiently important to warrant analyses of the scope and effort usually found in management science applications elsewhere, and usually time for these efforts is potentially available. Impeding applications in the U.S., however, are at least two major problem characteristics of science policy least favorable to the management science approach: (1) the political nature of science policy as manifest in multiple interest groups presenting various perspectives, criteria, and political power; and (2) the changing nature of the problem context and problem definition as experience with science policies accumulates and is reassessed within changing social concerns.

The political nature of science policy formulation, including the political determination of the extent to which science policy should be a politically influenced process, is discussed throughout the science policy literature (cf. Schmandt 1975,p.196; Price 1976,pp.107-111; Bueche 1973; and Shenin 1978,pp.330-331) and explains much of the difference between national approaches, as for example that of the U.S. and U.S.S.R. Within a political context where differing perspectives, aims, and prohibitions draw public attention, as in the U.S. where government agencies, citizen groups, and industry representatives participate in political debate on science issues, it becomes highly unlikely that any particular science policy problem can be defined with clarity, or that alternative options can be evaluated with non-conflicting measures of success. Rather than presenting well-defined problems to be solved with specified measures of success, science policy in the U.S. presents a host of issues and concerns for which satisfactory responses, largely perceptual, are hard to find.

Table 4

Alternatives in the Design
of Research Management Systems

Organizational Decisions and Alternatives

[1] Structure of lines of authority:

pyramidal (conventional hierarchical); or matrix[a]

[2] Major direction of span of interest:

horizontal (encompassing related or complementary research areas or topics); or

vertical (building on core research by adding supportive activities of dissemination, service and training, and interaction with users)

[3] Locus of decisionmaking and degree of collaboration with others:

centralized, internal decisionmaking; or decentralized, collaborative decisionmaking

[4] Basis of organizational subdivisions:

by discipline; or

by problem addressed or subject area; or

by R&D component function (planning, research, experimentation, development, applications); or

by organizational unit (geographical units, specialty units)

[6] Structure of administrative services:

centralized; or decentralized

Management Decisions and Alternatives

[1] Management Style:

directive; or intervening; or laissez-faire.[b]

high degree of participation in decisionmaking by host, client, governing board, or constituency; or low degree of participation.

[a]Matrix structures in research organizations are described in Wirt et.al.(1975); Stuki (1980); and Levien et.al. (1971). Their common benefits and problems are discussed in Davis and Lawrence (1978) and Greiner and Schein (1981).

[b]These styles of management are reflected respectively in the management approaches to programmatic, service-oriented, and basic research, as illustrated in Wirt et.al.(1975).

Table 4 (cont.): Management Decisions and Alternatives

[3] Balance of management focus:

toward achieving research of high technical quality;

toward increasing relevance of the research to important practical problems;

toward applying results of the research;

toward building a supportive constituency;

toward attending to auxiliary organizational goals.

[4] Allocation of Management Effort

on development aspects of identifying, selecting, funding, and planning of research projects;

on oversight aspects of coordinating, monitoring, evaluating, and modifying of approaches and methods;

on recruitment of key personnel;

on relations with external collaborators and the host/client institution or governing board

[5] Approach to budget allocation decisions:

comprehensive (zero-based); or incremental.

based on bottom-up requests; or top-down allocations.

competitive; or colleagial.

focused on outputs (program accomplishments and interest in products); or on inputs (line-item expenditures relative to of other research units').

[6] Uses made of advisory and liaison committees:

substantive criticism and recommendations (programmatic functions); or

dissemination and communication links with scientific or user communities (outreach functions); or

sources of potential personnel and potential funding (capacity development functions); or all of the above.

[7] Balance of continuity and flexibility:

fixed-term short contracts for scientific staff; or permanent or tenured staff positions.

finite-length programmatic research efforts; or ongoing knowledge-accumulating research efforts.

Table 5

Criteria for Judging Research Management Approaches to Decisionmaking or Problem Solving[a]

Criteria Related to the Technical Quality of the Research Produced (tendency of the research management approach to:)

1. Promote interaction among scientists from various parts of the organization and with the scientific community
2. Concentrate or focus research activity into coherent strands of work
3. Attract quality scientists and research leaders
4. Remain open to new ideas
5. Provide adequate technical and contextual information for effective decisions on project selection and methodological approach

Criteria Related to the Extent to which Results will be Applied by Others (tendency of the research management approach to:)

6. Incorporate interests and concerns of potential users during the stages of problem formulation and project selection
7. Encourage participation by potential users in the performance of the research
8. Provide effective communication linkages to user communities and multiple avenues for implementation of results

Criteria Related to External Support and Flexibility (tendency of the research management approach to:)

9. Attract a supportive constituency or to maintain good relations with the host or client institution
10. Maintain flexibility and responsiveness to changes in priorities
11. Keep financial costs of administration low
12. Avoid undue bureaucracy
13. Process proposals for new projects or changes in plans quickly

[a]Much of this material is adapted from earlier unpublished work conducted in association with Wirt et.al.(1975) in support of evaluating alternative management approaches for the then proposed U.S. National Institute of Education.

Table 5 (cont.): Criteria for Judging Research Management Approaches

Criteria Related to the Overall Goals of the Research Organization
(tendency of the research management approach to:)

14. Reflect in its procedures the goals and values set for the organization as a whole (This is especially important for research organizations with auxiliary goals such as international cooperation, prestige attainment for the host organization, or public service regarding information dissemination or field assistance.)

Criteria Related to Matters of Procedure[b]
(tendency of the research management approach to:)

15. Employ expert advice on important decisions
16. Search for additional information when warranted prior to commitment to choice
17. Identify alternative courses of action and assess probable outcomes prior to commitment to choice
18. Experiment, test assumptions, and modify choices when reasonable to do so on the basis of further knowledge gained.

Further limiting the application of a management science approach, public issues and concerns surrounding major science policy questions tend to change significantly over time reflecting prevailing political, economic, and social conditions. In describing the history of science policy, Salomon (1977,pp.51-57) focuses on changing public attitudes toward the role of science and associated perceptions of science policy issues in three distinct periods of time. The first he calls the starting period of euphoria following World War II, reflecting faith in science to resolve post-war problems. The second is the age of pragmatism from 1955 to 1967 in which science policy was directed toward strategic concerns, economic advance, and social problems. The third is the age of questioning and disenchantment arising from the threat of nuclear war, environmental deterioration, awareness of limits to economic growth, and the war in Vietnam. As a consequence of these changing social climates, the way in which science policy problems are formulated and the indicators of successful problem resolutions have themselves undergone considerable change, making any analytical approach toward a stable science policy posture quite difficult if not impossible.

[b]Arguments for criteria focused on the procedures for decisionmaking or problem solving rather than on outcome or solution merits are found in Simon (1976 and 1978).

System Characteristics in Science Management

System conditions described in an earlier section as favorable to the application of management science rarely exist either for the system of scientific inquiry found within a research organization, or for the system of science advancement within a nation. For research management, the relevant system is comprised of two parts. The first is the system of interpersonal interactions among the scientific staff, and between them and collaborators and information sources elsewhere. The second part of the system are the effects of organizational conditions, management style, and the nature of the research substance addressed, on productivity and the coherent aggregation of individual efforts among the scientific staff. In this system, boundaries are purposefully vague (communication and cooperation with outsiders encouraged, except in cases of confidential industrial processes or military secrets); system parameters and variables numerous, psychological, and ill-understood (as in creativity); and difficulties in expressing relationships among system elements by mathematical or programmable statements exceedingly great. Consequently, models of research management systems are rare, at least in the West.

Natural experiments in research systems, observed as behavior changes following major reorganizations within scientific institutions or the implementation of new research strategies, are documented in rather abstract or general terms.[4] Few specific hypotheses are examined in these histories regarding how the changes or decisions discussed affected the complex relations of system elements, and how this in turn affected system behavior. Consequently, only few lessons can be learned from these histories for system modeling. Furthermore, little if any a priori theory is capable of predicting accurately what system behavior will result from specific decisions of organization or management. Theory which is relatively applicable generally pertains to contexts considerably different from those of research organizations, focusing instead on organizations of hierarchical structures, reliance on authority of position, and functional specialization.

Additionally, in the context of research management, the system of scientists, their interactions, and the influence on them by organization and management decisions, changes continually and often in unpredictable ways. New personnel either on the scientific staff or in the research management often have profound and unpredictable influence on system behavior and output. Even without any external source of change, institutional maturity may alone bring about increases in bureaucracy, avoidance of risky research, and subunit insularity, and decreases in morale, flexibility, and overall quality (Argyris 1965). Both lack of relevant theory and continual system change pose added difficulties for modeling efforts.

[4]See, for example, Smith (1966) in connection with the history of the Rand Corporation, Levien (1977) on IIASA development, Stuki (1980) on structure and management within a large pharmaceutical lab, and Wirt et.al. (1975) on research management evolutions within certain U.S. federal research agencies.

In the context of science policy, the relevant system is the national network of science institutions and the effects on the behavior of this network made by government interventions. Science policy is viewed as the mechanism for encouraging advances in science and for directing those advances toward other national goals. To understand this system requires that scientific research as a whole be submitted to a systems analysis. The purpose of such an analysis would be to find principles or at least develop hypotheses for the subsequent improvement of science policy. Within the U.S.S.R., such a pursuit led to the beginnings of psychological and sociological studies in the 1920's aimed at the planning of science. These efforts were abandoned in the early 1930's and then revitalized in the 1950's (Graham 1975,pp.21-23). Within the West, Price of Yale in 1963 first proposed the quantitative analysis of scientific data and productivity as an approach to the analysis of scientific institutions in what he termed a science of science (Salomon 1977,p.51). The belief that science development can be made subject to principles which are ascertained through an integration of a wide variety of social science disciplines is today the backbone of Soviet science policy formulation and the special branch of Soviet research on the science of science. Gvishiani (1973) states the purpose of this branch as follows:

> ...to work out both the theoretical foundations for organizing, planning, and directing scientific activity and a concrete set of measures dictated by the objective logic of the development of science and technology. Thus, it ensures the optimum rate of scientific and technological progress and the increased effectiveness of scientific investigations and development, forming a theoretical foundation for working out science policy priorities.(p.175)

To date, results from the Soviet experience are mixed. Certainly the tremendous increase and maturation of the U.S.S.R. science system in the last two decades must be acknowledged. Yet the same system problems plaguing Western science seem to be also plaguing Soviet science today, described by Graham (1975) as tensions:

> ...between political administrator and researcher, between central direction and autonomy, between pure science and engineering, between research and teaching, between industrial advancement and conservation, between expansion of knowledge and satisfaction of immediate social needs, and between research for military or strategic purposes and research for the improvement of human life.(p.54)

Whether or not any national system producing science advances can ever be modeled and subjected to approaches such as those of management science today remains controversial, though much may be learned from current Soviet attempts. In any case, the system conditions listed earlier as most favorable to application of management science are certainly absent, and attempts at modeling and system control, such as those of the U.S.S.R., are proving to be Herculean tasks.

System Environment Characteristics in Science Management

Major elements of the system environment facing managers of research organizations include: (a) relevant scientific communities and the flow of new knowledge and talent emerging from them; (b) similar and competitive research organizations (outside the collaborative network considered as part of the system); (c) the host, client, or sponsoring institutions for which system performance is monitored and appraised, and other supporting and user organizations; and (d) the social, economic, and political milieu. None of these environmental elements is particularly certain, and each in unpredictable ways can have major impacts on the behavior and performance of the research system of a particular organization. Emerging ideas and progress on research results elsewhere, communicated through publications, professional meetings, or personal contacts within the scientific community, may cause research managers to adopt new approaches or switch priorities in the work of their own organizations, or may help them skip a number of research steps based on the accomplishments of others. The important impacts on research that these environmental elements often make force research managers to stay closely in touch with developments in the scientific community..

Similarly, comparable and competitive research organizations may also have profound effects on the system structure, behavior, and performance of a specific research organization. Emulation of organizational arrangements, management style, substantive focus, and of goals, practices, tactics and strategies is seen by Vidmer (1979) as one of the main ways organizations learn from their environment, especially about innovations that seem successful. Such emulations may explain the recent adoptions by so many research organizations today of a matrix structure, problem rather than discipline orientation, a think-tank general purpose, use of formal advisory committees, and an interdisciplinary character. Competitive organizations (or key personnel in competitive organizations) also may have strong influence on the behavior of a research organization, as dramatically illustrated in the competition between Crick and Watson of the Cavendish labs in England and Linus Pauling and his staff in the U.S. for finding the structure of DNA.[5] Environmental interactions of these kinds simply cannot be represented by probability distributions of possible events and impacts. The process of scientific discovery is based too much on unanticipated events and serendipitous findings to allow it.

Host, client, and sponsoring institutions play an especially important role in determining system behavior and even survival of the research organization also often in unpredictable ways. For example, changes in the strategy of an industrial enterprise from concentration on new product development to cost savings on existing product lines may have profound effects on the nature of research and the system structure of the company R&D organization. Changes in key personnel in the host institution or client institution may alter or even lead to the demise of a supporting R&D organization. A case in point is the short life and rapid demise of the New York City-Rand Institute following the succession of New

[5] An account of this scientific competition is found in *Double Helix* from the viewpoint of Crick and Watson (Watson 1968).

York Mayor Lindsay by former City Comptroller Beame, described by Greenberger (1976, Chapter 7) as an important lesson in the potential impacts of changes in supportive constituencies.

Changes in the social and political milieu, as well as changes in the economy, are also important and usually unpredictable elements in the environment of R&D organizations. For example, between 1967 and 1972, negotiations between the U.S. and the U.S.S.R. for the establishment of IIASA represented the newly prevailing interest in detente emerging from the Cold War era. The negotiations were concluded successfully and the Institute continues to flourish. But certainly the political climate has changed recently. While celebrating IIASA's second triennial conference in 1980, first IIASA director Raiffa lamented that were negotiations for IIASA to begin then rather than thirteen years earlier as they had, IIASA would probably never have been created.[6]

Science policy environments reflect this changing political climate even more strongly as indicated in the periods of different public attitudes toward science mentioned earlier. As in research management, unanticipated exogenous events often play a crucial role in shaping major decisions and policies. Kunreuther (1980,pp.14-17) emphasizes the important role of exogenous events in throwing substantial support to some interest groups involved in influencing science policy formulation even though the events may be basically irrelevant to the specific issues. He cites a number of unanticipated and sometimes irrelevant events occurring in the context of siting decisions for liquefied energy gas facilities which resulted in reversals or reinvestigations of decisions not yet finalized. Kunreuther's contention is that parties involved in science policy decisions scan and interpret environmental events for their own purposes and by doing so can trigger new coalitions and new legislation imposing additional constraints on policy alternatives under consideration. Both changing political climates and use of unanticipated exogenous events to reshape constituencies, booster support, and affect legislative context contribute to the highly uncertain nature of the environment within which many science policy problems must be resolved.

Attitudes and Values in Science Management

In principle, most research managers would naturally favor expertise, rational analysis, and a system-wide perspective as the proper basis for decisionmaking. However, in routine matters or with regard to problems of no great consequence, few research managers can afford the time or expense required by typical management science approaches nor might they think the benefits worth the costs. In these situations, personal experience or tradition may generate satisfactory solutions at little or no cost of analysis. Examples are in publications ordering and inventory management, cash management, internal charges for common services, equipment ordering and distribution, and location of facilities used by many units. Sophisticated management science approaches exist for handling all these routine matters. Few research organizations apply

[6]Keynote speech published in IIASA's newsletter *Options* 1980, Volume 2, p.2.

them. Costs of data collection and analysis are usually not justified, and reliance on experience and standard heuristics are judged adequate.

For matters of great consequence in research, however, management science approaches are generally less relevant, or are at least judged so on the basis of the characteristics listed earlier as unfavorable for management science application. Sensitivity to the various perspectives and interests of scientific staff, host or client organizations, or other constituent groups also tends to limit the political feasibility of purely rational approaches to important decisions. Finally, not all parties involved in a research organization necessarily reflect or support all the institutional goals to which the research is directed, and so would not necessarily support a research program rationally deduced or structured from those goals. Individual careers may be paramount to some of the staff and loyalty of scientists is generally to their discipline rather than to their employing institution (White 1975,p.2).

Within science policy formulation, strict reliance on expertise and rational analysis is highly suspect. Arguments for a science court in which debate is limited to questions of fact, and scientists are seen as removed from political considerations are long-standing and viewed by Nelkin (1979,p.19) as anachronistic and dangerous. Often, the questions of science are not what the political debates are about in the first place. Nelkin contends that protests by community groups against science and technology advances or by industry against government regulations are actually protests against prevailing power relationships among them. These situations are obviously better handled through participation, public debate, and political evaluation of policy alternatives than through reliance on rational analysis.

The Match Resisted

Within the relatively sparse literature on research management, almost no mention is made of management science, outside the specific problem contexts of project selection and capital budgeting for R&D projects.[7] This in itself is one indication of the prevailing negative attitude or attitude of indifference toward management science held by research managers. As an example, Levien, director of IIASA and author of many articles on systems analysis and its applications, recently received suggestions by IIASA's U.S. advisory committee to apply systems analysis to IIASA's own operations and exert a stronger management role in the selection, planning, organization, and guidance of research, and in the support of area managers' own determination of priorities and scope of activities. His reply may typify the attitude of many research managers

[7]Within these two contexts, attitudes are mixed. Results of quantitative models for comparing projects and project sets are used as guides only, and even so with mixed support. Souder (1970) recommends against the use of capital budgeting methods for project selection decisions, against the use of cost prediction formulae for estimating required investment as a basis for selection, and against scoring and ranking techniques also for selection decisions, despite the favor in them found among some research managers, especially in industry. Only resource allocation models and PERT/CPM project scheduling techniques appear to pass Souder's standards of formulation without serious defects.

toward the applications potential of management science in research management:

> The proper management style for IIASA would be a topic worthy of considerable study and it warrants far more discussion than is possible here. While I accept these comments in the helpful spirit in which they were presented, I feel they miss the reality of an Institute that strives to have high quality and distinguished leaders and staff (who cannot simply be ordered around), that is subject to many sources of advice and recommendations (17 National Member Organizations, 6 Advisory Committees, evaluation groups, distinguished visitors, policymakers, senior staff), and that draws non-permanent staff from disciplines, settings (academia, industry, government) and nations having widely differing traditions of research management. To make IIASA work, it is absolutely essential to find a management style that balances overall guidance with independence and delegated responsibility, single-mindedness with flexibility, and coherence with tolerance of diversity. There is no doubt that we have not yet evolved an optimal management system, but the style of systems analysis appropriate here is more that of "adaptive management" than of PERT charts and centralized control. (From IIASA Draft Research Plan 1981-85: Comments and Responses, September 1980; unpublished)

In the context of science policy, negative attitudes toward management science approaches are more pronounced. Reviewing the use of models in policy making, Greenberger et.al.(1976,pp.23-24) document that a large fraction of models are never put to use; that from one-third to as much as two-thirds of modeling efforts fail to achieve their avowed purposes in the form of direct applications to policy problems; and that when used, models of analysis, guidance, and problem solving tend to be directed toward providing political validity to specific positions or alternatives. Freeman (1977) also cites ways in which formal techniques and sophisticated methods of management control are manipulated for political purposes and warns of the dangers of "naive, technique-based approaches, such as systems research" being substituted for a more integrated approach of social sciences in cooperation with technologists (p.265). Even within the system of U.S.S.R. research where applicability of quantitative methods in science policy matters is examined and encouraged, quantitative analysis in the science of science is judged by Shenin (1978,p.21) as "having an ancillary role to play for the time being."

Science management's lack of conditions favorable to management science accounts for much of the absence of attempts of applications there. But management science has generated both disappointment and criticisms even in contexts of much more favorable conditions. To a great extent, these same criticisms may be reflected in the attitudes of science managers. In the next chapter, some of these common criticisms are described.

CHAPTER 4

GENERAL CRITICISMS OF MANAGEMENT SCIENCE APPROACHES

Criticisms of management science, many of which are what Quade and Boucher (1968) refer to as common pitfalls of analysis, focus both on the tools and techniques employed in management science, and on the behavior of management scientists in overworking the tools they use. Extensive unsatisfactory experiences with management science have led Drucker (1974) to consider it a disappointment: "It has not lived up so far, to its promise. It certainly has not revolutionized the practice of management. Few managers, indeed, pay much attention to it." (p.508) Many of the faults attributed to management science and its applications fall into five categories:

[1] Management science as practiced and researched is often seen as overly mathematical and attentive more to the model created for analysis than to the problem addressed.

[2] Management science tends to ignore the unmeasureable variables even though they may be highly important.

[3] Management science is seen as preoccupied with economic rationality as the basis for prescriptive results.

[4] Management science tends to ignore the social sciences, and

[5] Management science is criticized as being simplistic, oversold, politically naive, and uncritical of itself.

These categories of criticism, though clear and explicit, convey little of the diversity and strength of perceived weaknesses within the categories. Following are references given to convey some of this diversity.

Overly Mathematical

McLean (1978,p.973) observes "the tendency of systems researchers to use elaborate mathematical formalisms in their work," cites others' criticisms of "mutual incomprehension between the policy-maker and the analyst," and argues that "preoccupation with technicalities has been observed to deflect attention from assumptions and issues." He refers to these and other behaviors of management scientists as "tendencies towards complexity and mystification" (p.975). Bonder (1979) questions the relevance of mathematical developments characteristic of current operations research and relates this to individuals "who have more of a disciplinary allegiance to mathematics and economics than to operations research" (p.64). Ignazio and Gupta (1975,p.23) warn that some practitioners of management science see the concentration on mathematical theory development as "the development of complex, esoteric solutions to simple or non-existent, hypothetical problems." Lastly, Quade and Boucher (1968,p.348) cite the common pitfalls of much systems analysis including overconcentration on the model, excessive detail, and overworking concepts of statistical uncertainty (in attempts to transform ignorance into states of subjective probabilities).

Ignoring the Unmeasureable

Minzberg (1979,p.121) criticizes, "In fact, analysis cannot handle the soft data, which is critical." He argues further that it takes time for events to be recorded as facts and that some facts important to managers such as emotional responses, political positions, group dynamics, and personality traits never do become recorded as facts. In policy formulation, this neglect of unmeasureables may be disastrous; as indicated by Minzberg quoting Wilensky on the role of analysis in the Vietnam War:

> ...analysis of the easy-to-measure variables (casualties suffered by the Viet Cong and the South Vietnamese) was driving out consideration of the hard-to-measure variables and long-run costs (the nature of popular support for a South Vietnam government, the effect of the war on the Western alliance and on domestic civility, the effect of the bombing on the will to resist)... (p.122).

This bias toward the easily quantified and neglect of the intangible and unquantifiable is also observed by Majone and Quade (1980), Quade and Boucher (1968), and Dror (1971a,1971b). Furthermore, the proxy measures commonly used in management science for the quantification of unquantifiable attributes (utility, benefits, effectiveness) are criticized by Bonder (1979,p.66) and Majone and Quade (1980,pp.27-29) as poor.

Preoccupation with Economic Rationality:

Criticisms of management science's heavy reliance on economic rationality focus on three arguments. First is that such reliance incapacitates management science to deal with conflicting, noncommensurate values; with problems that are ill-defined and lack clear criteria for choosing among alternatives (Dror 1968,1971b) or which are basically ambiguous (Sproull et.al. 1978 and Minzberg 1979) and with how to retain future options and develop "versatility" or adaptability to future conditions instead of simply choosing the best alternative at the time (Bonder 1979,p.69). Secondly, economic rationality is not supported very well by empirical studies of decisionmaking behavior. It gives inadequate treatment of intuition and other modes of thinking and action bases (Minzberg 1979,pp.133-148) and fails to consider explicitly the psychological stresses and induced biases of individual decisionmakers, or the group dynamics and institutional contexts which shape issues and restrict alternatives, or the political realities which interpret situations and constrain decision processes (Lynn 1980). Third, economic rationality is seen by Minzberg (1979,pp.123-130) as a mask of supposedly value-free, objective, open-minded, impartial and amoral analysis for what is in actual practice found to be sometimes filled with "deliberate distortion and partisan bias," advocacy, systematically favoring operational goals (often economic in nature) over other less operational goals (often social in nature), hard data over soft data, and actually creating and altering goals and alternatives as part of the analytical process.

Ignoring the Social Sciences

This is a common theme of Dror (1968,1971a,1971b). The separateness of systems analysis from organizational theory and decision theory from general systems theory tends to give "fragmented and disjointed views" of decisionmaking which reflect a "dichotomy between behavioral and normative approaches." This increases the difficulty of embedding greater understanding about reality in decisionmaking into prescriptive approaches or at least into improved practices, and is seen as a major weakness in management science overall.

Simplistic, Oversold, Politically Naive, and Uncritical of Itself:

Drucker (1974,p.509) finds in management science an emphasis on "techniques rather than on principles, on mechanics rather than on decisions, on tools rather than on results, and above all, on efficiency of the part rather than on performance of the whole." He also faults management science applications in business for focusing narrowly on where to use a given technique rather than on the more fundamental questions of what the enterprise should be; and for having as an implicit goal the elimination or minimization of risk when risk-making is what supports economic progress. Additional pitfalls cited by Majone and Quade (1980) as common in management science include overambition, irrelevance of the model, unquestioned acceptance of stated goals and constraints, inflexibility, disregard for limitations, and an underemphasis on adequate problem formulation. Dror (1968,1971a) views management science as young, still narrowly focused in its field of application (predominantly military and industry), concerned too much with selling itself in order to be a part of the decisionmaking system, neglectful of institutional context and political needs for consensus-building and avoiding confrontation, inadequate in its explication of value assumptions, incapable of handling primary uncertainties or major shifts or reversals of strategy (focusing instead on incrementalism), inadequate for analysis of very large or complex systems as a whole (without resorting to decomposition and suboptimization), and fixating on conventional research methods rather than embedding new knowledge or engaging in social experimentation. McClean (1978,pp.976-977) comments on the "relative lack of genuine criticism of system methods by those working in the field" and recommends methodological pluralism as a necessary ingredient of all systems research. Minzberg (1979) adds that management science approaches usually fail to consider dynamic factors such as delays in information and recycling in problem approaches, and that pressures toward superficiality and premature closure (by time constraints, emphasis on verbal media, action orientation, discontinuity in problem context, inability to handle unmeasureables and nonoperational goals, and lack of extensive direct experience with the problem context) lead to management science studies that are "little more than a hasty compilation of platitudes swathed in mathematical macrame" (Minzberg quoting Hoos, p.145).

In addition to the criticisms of the categories above, management science has also been attacked as an avenue for greater centralization and bureaucracy (Vertinsky et.al. 1975,p.265; and Minzberg 1979,pp.130-133); and subject in practice to "tunnel vision" by tending to make precise estimates for a small number of alternatives or future states rather than providing partially accurate information on many options and consequences (Bonder (1979,p.67); or by considering only the narrow range of alternatives for which changes can be measured or which relfect the user's preferences or point of view (Majone and Quade 1980). Finally, management science is accused of being misnamed as a science. Minzberg (1979,p.155) argues, "By no stretch of the imagination do OR/MS practitioners practice a science." Bonder (1979) supports this view by bemoaning insufficient knowledge to teach analysis or how to perform many of the steps and activities basic to management science.

To a large degree, all these perceived deficiencies and the ensuing reputation of management science help explain why science managers as well as managers in other contexts have not adopted management science approaches; or at least have limited attempts at application to those areas where experience with management science has been generally positive and where economic payoffs are clearly foreseeable. The situation is not, however, necessary pathological, though the trend in management science development may be pointing in that direction. What is needed are opportunities for trend reversals, and these in turn require new bases for empirical observations and insights. The field of science management may offer such a base, as discussed in the next section.

Part II

TOWARD RESPECTING REALITIES OF PROBLEM CONTEXTS

INTRODUCTION TO PART II

It will not be easy for practitioners of management science to overcome the criticisms and limitations outlined in the previous chapters, and to have their analytical approaches gain acceptance as credible management tools for the more social and less well-defined problem contexts such as science management. To do so, management scientists must reject many of the tools and conceptual frameworks formulated for well-defined problems. This is basically a step toward greater humility in the management science approach. It requires respecting the realities of application contexts and attempting to deal with them, rather than assuming them away to fit exiting conceptual frameworks. These are not easy developments, and so examples which clarify their necessity and provide contexts for new insights may be helpful. In this regard, *science management offers a rich empirical base of visible problems and discernible decision processes requiring and encouraging the emergence of new analytical formulations for both descriptive and prescriptive studies in decisionmaking.*

Adopting a More Humble Perspective

Earlier chapters of this paper stressed that management science makes extensive use of models, and that models are based on simplifying assumptions that help limit the complexity of the problem addressed to levels at which the problem can be analyzed with known techniques and tools. It was also stressed that solutions found in this approach are actually solutions to the modeled problem rather than to the real problem, but that often these solutions help point the way to actions or decisions which would lead to improvements in the real problem situation as well. However, many of the criticisms of management science listed earlier stem from allegations that analysts pay too little attention to the realities of problems they face and accept too readily that the simplifying assumptions they make do not invalidate their analyses.[1] Greater humility is obviously needed when management scientists or users act as if they really know what is to be done based on results of analysis (when realities of actual problems make no alternative best with absolute certainty); or when analysts believe that what they are doing is correct, i.e. applying appropriate scientific methods (when in actuality the absurdity of some of the simplifying assumptions is obvious). In most real problem contexts, there are no a priori correct answers or actions, only guesses backed differentially and often obscurely by data and analysis. A healthy skepticism and sense of humility is basically in order.

Realities of many of the problem contexts which management science is asked to address often reach beyond complexity into perplexity. Respecting these realities requires rejecting some of the earlier held notions and simplifying assumptions that have been the bases for conceptual frameworks in much of management science. Advances in understanding and new techniques have already allowed analysts to avoid many of the simplistic concepts of classical decision theory and to acknowledge some of the complexities of actual problem contexts. Table 6 indicates most of these transitions in its first two columns. Despite these advances, however, analytical capabilities have not kept pace with the growing understanding of actual problem contexts. Column 3 of Table 6 indicates that even the more sophisticated and complex natures of problem formulations used today are in fact gross over-simplifications of reality. Furthermore, little is known about how to handle the realities that are recognized.

The resulting perplexities have led some management science theorists to reexamine the nature of goals, decisions, decisionmakers, and problems as terms meaningful for existing analytical approaches, or at least to question the way they have been operationalized in management science practice. Some new ideas and analytical formulations have emerged as a consequence and are described later in Part III.

[1] Validation of model framework, structure, and parameterization; and of data relevance are discussed at length in *Issues in Model Validation* by A. Lewandowski (IIASA Working Paper WP-81-032 March 1981).

Table 6

Management Science: Progress in Respecting Realities of Problem Contexts*

From Rejecting Simplistic Concepts of Classical Decision Theory	To Acknowledging Complexity (Current State of Management Science)	To Facing Perplexity (Adopting a More Humble Perspective)
Goals are explicit and decomposable to expressions of uni-dimensional dependent variables (e.g. profit, market share, expected value, and other effectiveness measures).	Goals may be multiple and even conflicting, and are influenced by current levels of attainment.	Goals are often ambiguous, opaque, hidden, and changing; a dynamic mixture of knowns and unknowns; or only post-hoc abstract statements to explain behavior but not guide it.
Alternatives are known or are easily deduced.	Additional alternatives are sought or are purposely generated through analysis.	Alternatives may disappear and new ones emerge out of happenstance.
Decisions are guided by economic rationality (as in optimization techniques).	Decisions are guided by bounded rationality (as in goal adaptation and search heuristics).	Decisions are guided by bias, mistake propensities, intuition, consensus, and context.
Analysis is objective and useful primarily for prescriptive purposes.	Analysis aids understanding through testing options and effects of changes.	Analysis is subjective in part, advocative in use, and only one input to any decision situation.

*Continued on the next page.

Table 6 (cont.)*

From Classical Decision Theory	To Acknowledging Complexity	To Facing Perplexity
Decisions are made by people in positions of authority (decisionmakers) who are influenced by analyses.	Institutional arrangements influence and limit decisionmakers' behavior and must be taken into account in the analyses.	Decisions (in the classical sense) are rarely made; the decisionmaker is largely a myth; the role of analyses is unclear.
Action or choice automatically follows knowledge of best alternative (information is key).	Care must be given to presentation of results for effective implementation (argumentation is key).	Knowledge, acceptance, and intent may still result in no action or unrelated actions.
Analysts operate outside the decisionmaking system.	Analysts must interface with the decision-making system for greater understanding and effective implementation.	Analysts become part of the decisionmaking system and thus change it (as in psychoanalysis).
All required information is available with calculable error; all variables are certain or can be expressed as statistical measures from past data.	Information is usually not all available or with calculable error but can be purchased or improved according to "value of information" techniques. Some variables need to be expressed as subjective probabilities based on related knowledge or expert judgments.	All information is perceptual and subject to bias; attempts to improve information are influenced by attitudes toward uncertainty. Ignorance abounds; there may be no grounds for meaningful subjective probability estimates for certain key variables.

*Also continued on the next page.

Table 6 (cont.)

From Classical Decision Theory	To Acknowledging Complexity	To Facing Perplexity
Accountability is in the decision consequence; was the decision the correct one (subjective rationality approach).	Decision effects may be too complex to measure; accountability is in the procedures followed; better made decisions will generally be better decisions (procedural rationality approach).	There is no empirical evidence that better procedures lead to better decisions in consequence (though those involved may feel more comfortable with their choice).
Analysis is a service for the decisionmaking client (and is amoral).	The ultimate client (social service beneficiary) may differ from the (bureaucrat) decision-maker. (The analyst is then faced with the moral issue of whom to serve.)	There may be no obvious client and no immediate decision-maker. (The analyst is then faced with his own motivations for analysis.)
"Best" choices are sought by analysts for real problems confronting decision-makers.	Some problems and choices are basically perceptual; the analyst must under-stand the perspective of the decisionmaker.	The meaning of choice may vary; sometimes choices seek problems so that they may be implemented, issues seek decision situa-tions so that they may be aired.

Facing Realities Squarely: The Use of the Science Management Context

How can the field of science management help in the development of new analytical formulations more accurately reflecting the realities of problem contexts? Development of theory or of new analytical frameworks benefit from a rich empirical base, where data is accessible, processes are discernible, and the challenges to existing frameworks are clear. Science management provides all three characteristics.

The nature of goals, decisions, and decisionmaking in research management and especially in science policy are characterized most clearly by Column 3 of Table 6. The next three chapters illustrate how the realities of science management in all these regards may serve as a basis for new model frameworks and new analytical formulations, especially in descriptive studies of decisionmaking.

CHAPTER 5

REALITIES OF GOALS

Most management science efforts rely on an unambiguous operationalization of goals or objectives as part of their formulation. These operationalizations serve either as the basic function to be optimized or as evaluation mechanisms by which alternative actions or decisions are compared. The complexities and ambiguities of many real problem contexts, however, may make traditional operationalizations seem quite naive. In these cases, the whole notion of stated goals as being susceptible to rational means-ends analysis, or even as determinants of behavior or rational choice becomes suspect.

The Exceptional Nature of Science Management Goals

The following examples in science management illustrate the perplexing situations often encountered which so far have eluded management science formulations:

- *Science Management Offers Clear Cases of Unclear Goals.*

  Goals in science policy are often purposely vague (to attract broad constituent support), purposely ambiguous (to avoid explicit choices which would undermine consensus), and are continually changing (reflecting changes in environmental conditions and institutional developments).

Goals which typically are stated in a way to have greatest chance for obtaining consensus support are also those typically least operationalizable. This permits various constituents to interpret the goals as supportive of their own interests or at least does not make explicit that they are not. Such strategy, common in science policy as well as in education, health, welfare, military, and economic policy, serves well to get programs going, but provides little guidance to program administrators for what to do, how to do it, or how much success is either sufficient or desirable.

Goals of this unoperationalizable type usually are stated in non-specific terms such as:

> to improve; to enhance; to reinforce; to strengthen; to raise the quality of; to promote; to upgrade; to service; to expand; to coordinate; to stimulate; to augment; to assist; to develop; and to make viable[2]

At least two things are missing in each of these cases. One is the designation of the level or condition which would be considered the successful accomplishment of the goal. The second is the specification of measures or evidence which would verify attainment of that condition. How do research organizations behave in these situations of unclear goals? How is overall performance directed? Studies of the behavior of science organizations facing such clearly unclear goals may lend insight into the development of alternate model formulations of organizational behavior which differ substantially from current goal-oriented ones.

o *Science Management Offers Problem Issues That Resist Trade- Off Analysis.*

  In traditional management science formulations, operationalized goals are usually expressions of utilitarianism; in science policy matters, utilitarian tradeoffs may appear to be both unacceptable and yet unavoidable.

Rights and moral traditions believed to be non-negotiable to parties in a science policy debate are antithetical to management science notions of trade-offs and overarching utility functions by which conflicting objectives are handled. The sacredness or value of life, for example, is held by some to be incapable of measurement in dollar terms. Trade-offs for expenditures on estimated lives saved, lives lengthened, and quality of life improvements are rarely explicitly addressed. A growing interest in risk assessment represents the search for acceptable levels of risk for different hazards, yet some simply reject the notion that a calculatable acceptable level of risk can be determined (Starr and Whipple 1980 and Otway and von Winterfeldt 1981). Yet in fact, trade-offs of lives and dollars, and lives and life improvements are made either implicitly or by default in a wide variety of public and private settings. These are the realities of many science policy issues. When are rights to be treated as constraints and when as competing objectives? When will trade-off analysis

[2] These are listed as "fuzzy terms" in goal specification by the U.S. Agency for International Development (1973, pp.12-13).

help lead to an acceptable compromise and when will explicit statements of trade-off retard the resolution process? Traditional notions of goals have largely ignored issues of rights and moral traditions treated as non-negotiable. Real problem contexts of science policy are rarely without them.

- *Science Management Offers Cases Where Satisfactory Measures of Goal Attainment are Unlikely.*

  Some goals of science policy or research management are simply impossible to quantify satisfactorily and thereby remain immune to management science approaches.

Science related goals concerned with quality of life, for example, often recognize that material well-being is not the only measure relevant to populations affected by policy options. Community groups potentially affected by hazardous facilities may argue strongly that freedom from risk, or at least from potentially dangerous situations, defines their quality of life to a great extent. How can such notions be adequately quantified?

In the related context of research management, project goals similarly unoperationalizable reflect research as an uncertain search for greater understanding. Traditional academic measures of number of publications, quality of science produced, and number of citations simply fail to capture adequately how much greater understanding has actually occurred. As an extreme case, most international research organizations include as a major goal that of achieving international cooperation, or mutual understanding and better relations among countries. Problems of satisfactory measures associated with such goals are usually seen as insurmountable and substituted instead with expert testimony on accomplishments.[3]

- *Science Management Offers Cases of Goal Uncertainty and Search Uncertainty Different from the Event Uncertainty of Traditional Management Science Formulation.*

[3]For example, goals of programs of scientific exchange include such difficult to quantify notions as: "to promote better relations between the United States and the nations of Asia and the Pacific" (actual goal of the East-West Center in Hawaii as legislated by the U.S. Congress); "to strengthen international collaboration; to contribute to the advancement of science and systems analysis; and to achieve applications to problems of international importance" (actual goals of the International Institute for Applied Systems Analysis as stated in its charter); and "to understand other nations better," "to see ourselves as others see us," "to help other nations develop," "to bring about a flow in two directions of personalized information, experience, and understanding," "to broaden the outlook of educators and leaders," "to advance knowledge and to strengthen the world community of education, science, and culture," and "to develop abroad an understanding of U.S. culture and institutions" (all these and many more are goals attributed to the U.S. Fulbright Program of scholarships abroad). In each of these cases, individual testimony has dominated as the critical evaluative mechanism for continued budget support. Quantitative studies have tended to focus on input measures (dollars, participants by disciplinary field and country, etc.) and not on outcomes.

> In research management, project goals are often actually loose depictions of what *might* be attainable, without clear means-ends chains or search procedures available a priori to guide actions and choice, or even to test whether achievement of the goal is feasible.

Research project goal statements are notoriously vacuous and often with good reason. Scientists often simply do not know in advance what their final outcome, conclusion, methodology, or product will be, or even it if can be produced. Consequently, they tend to couch their goal statements in terms of what activities they are going to do, rather than in what those activities are intended to accomplish. Research managers are then faced with such goal statements as:

> to study; to investigate; to attempt to find; to test alternatives; to analyze; to explore the nature of; to conduct a workshop on; and to prepare a paper on.

In these situations, the scientists involved may simply be unwilling to state , and with good reason, that they *will* develop a methodology, or find an answer, or even achieve a better understanding of a phenomenon. Instead, they are willing to commit themselves only to the conduct of the research and to specified formats of activity, but not any results. Facing realities in science management problem contexts requires not only the operationalization of ambiguous and complex goals, but confronting the uncertainty in goal attainment as well.

o *Science Management Offers Visible Cases of Hidden Goals.*

> Hidden political goals (unstated yet often dominant) in science policy are common in many political settings, and usually ignored by management science formulations.

For example, geographic placement of large scientific facilities may have less to do with optimal natural environmental conditions than with providing employment opportunities for specific locations. Dror (1980) focuses on different goal orientations of management science and politics in the following manner. Management science approaches focus on goal attainment in an "open mind" approach to alternatives; with a single decision focus; a substantive solution orientation; concern for the issues involved and in a time orientation reflecting the natural life cycle of the issues; and in a clinical, detached manner. In contrast, Dror sees many matters of policy being determined according to concerns of "high politics" which focus instead on practice and doctrines essential to maintain consensus and save time in problem solving; on policy packaging for consensus and balance rather than a single decision orientation; on the symbolic and expressive aspects of solutions rather than strictly on their substantive aspects; on political time cycles as well as the life cycles of issues involved; and on achieving an emotional zeal for policy acceptance rather than on a detached and objective analysis.

Political concern for consensus, packaging, and symbolism are not necessarily opposing the concerns represented by stated goals; often they are aimed at promoting political acceptance of decisions and actions analyzed as optimum for goal attainment. Furthermore, management scientists are often aware of the political processes that will be brought to bear on their analyses and recommendations, and wish to incorporate political concerns somehow in their analytical formulations. How to do so is still rather a mystery. Dror's concepts and principles of policy science (1968, 1971a, 1971b) offer a step in this direction. However, translations of these concepts and principles into management science frameworks is yet to be done.

In research management, individual goals of key scientists are often never explicitly revealed because of their personal character, involving for example, career advancement or personal visibility. Yet these individual goals and motivations play no small part in shaping the work of the respective organizational unit.

New Frameworks for New Notions About Goals

Modern organizational theory suggests some interesting and plausible alternatives to the traditional management science frameworks of goal seeking decision behavior. Yet none has been developed to the point of supplanting the traditional normative frameworks. Simon (1970), for example, argues that individuals and organizations don't really pursue particular goals; instead, they seek in their actions and decisions to satisfy a whole set of requirements or constraints. Singling out any one of them as the "main" objective is largely arbitrary. In this regard, goals act more as criteria, either in testing the satisfactoriness of proposed solutions or helping in alternative generation.

A second framework is suggested in the context of organizations as coalitions of individuals with widely varying preference orderings. One is tempted to suppose that the goals of the organization are then represented by some sort of joint preference ordering. But Cyert and March (1975) show that this is not well supported by evidence. "Studies of organizational objectives suggest that to the extent to which there is agreement on objectives, it is agreement on highly ambiguous goals. Such agreement is undoubtedly important to choice within the organization, but it is a far cry from a clear preference ordering. The studies suggest further that behind this agreement on rather vague objectives there is considerable disagreement and uncertainty about sub-goals; that organizations appear to be pursuing one goal at a time and another (partially inconsistent) goal at another; and that different parts of the organization appear to be pursuing different goals at the same time." (p.79) As an alternative to the traditional framework, Cyert and March explain the nature of goals by referring to the process by which they evolve, including (a) bargaining among the participants which form the coalition; (b) internal organizational processes by which objectives are stabilized (in budgets, allocation of functions, and adherence to precedents); and (c) responses to experiences of environmental change.

In a third perspective, Cohen, March, and Olsen (1972) and March and Olsen (1976), accept goals as ambiguous or unknown or even discovered through action, and postulate a model in which organizations make choices without consistent or shared goals. In their model are different streams of problems (concerns requiring attention); solutions (somebody's answer looking for concerns to satisfy); participants (who come and go and offer varying amounts of energy for supporting solutions); and choice opportunities (occasions when it is expected to produce decision behaviors). Solutions are accepted to problems not in any framework of overall goal support, but as a "consequence of different rates and patterns of flow in each of the streams and different procedures for relating them."

How goals function to influence, guide, or determine decision behavior (if they do) is still a mystery. To sort it out, management scientists need empirical studies as a base for new model development. Science management, in which goals are clearly ambiguous, or hidden, or unmeasureable, or disparately supported, yet in the context of which decisions are visibly made, may provide an extremely useful empirical base for this exploration.

CHAPTER 6

REALITIES OF DECISION PROCESSES

In complex, social situations, decisionmaking in the sense of rational, analytical determination of choice as a paradigm of goal-directed behavior is largely a myth. So is the concept of a decisionmaker (an individual identifiable as the one responsible for a particular major decision). In reality, when a person identifies a decision that he or someone else has made, he usually is referring either to a formal statement, document, or other symbolic form which represents commitment to a particular action or choice, or the action or choice itself which infers the decision made. Yet the person or persons responsible for enacting the symbolic representation or even carrying out the choice or action may not necessarily be the one(s) who formulated the commitment and/or ordered its execution. Nor does choice or action necessarily mean conscious decisionmaking except in a tautological sense.

For management scientists, the concepts of decisions and decisionmakers have been both useful for defining and delimiting the role of management science, and blinding to the realities in which actions and choices are often made in actual problem situations. Management scientists willing to be accountable for the rational formulation of their recommendations according to principles of scientific method and the limitations of data and resources, but not willing to be accountable for the consequences of the actions taken on the basis of their recommendations need to separate the role of analyst/consultant from that of accountable official. By referring to others as decisionmakers, analysts provide themselves with justification for focusing their own attention on problem analysis, and avoiding questions of who is responsible for the commitment to action, and how that commitment is formulated (the realities of the decisionmaking process). This also serves to separate identification and communication of the recommended choice (the primary role of management science) from acceptance and implementation of the recommendations made (often a matter of organizational or national politics). Some management scientists, however, have focused specifically on the latter processes of argumentation, participation of interest groups, consensus development, and formulation of action commitments. Their published experience has added significantly to the management science literature regarding realities of decisionmaking in social and political contexts. Some of those realities are listed below.

Aspects of Decisionmaking Realities

[1] The dynamic reality of the formulation of commitment

In complex situations, there is usually no single act of conscious deliberation leading to an intended commitment, but many deliberations by many interested groups, each requiring resolution of differences, trade-offs of interests, and persuasion of others. Unanticipated events, new actors in the problem situation, and new formations of coalitions, positions, and alternatives, all influence the dynamics of the processes leading to decision representation.

[2] The political reality that no single choice may dominate

In many social situations, there are often legitimate disagreements on goals and legitimate differences on the importance placed on various criteria. Whose values are to be used in making trade-offs? How are equity considerations to be handled? How are long-term impacts to be valued and compared to short-run gains when long-term interests may not adequately be represented? Rational analysis may have little to offer in these situations of differing values.

[3] The cultural/legal reality that processes of decisionmaking or policy setting (at government levels) are shaped by cultural and legal traditions influencing who can participate, how arguments are to be presented, and how power is employed

The usual notion of a "best" solution does not reflect the role of power or institutional traditions in the way commitments are formulated in real contexts. For example, in the science policy questions of development of standards and regulations, or in the site and safety decisions for hazardous facilities, the cultural heritages of different nations impose structure to the political processes by which decisions are made. Table 7 by Jerry Ravetz[4] shows how these cultural styles set the tone and form of many government decision processes.

[4]Dr. Ravetz was a participant in an IIASA seminar in 1980 on Risk Management: Siting of Liguid Energy Gas Facilities. The presentation of material in Table 7 was impromptu and no published version is believed to exist.

Table 7

Influences of Cultural and Legal Heritages on Policymaking Processes Regarding Hazardous Facilities*

	Rules and Regulations	Style of Regulatory Power	Style of Rule Setting
U.S.A.:	Formalistic; Encyclopedic	Participatory (function of history)	Litigious; Use of experts
U.K.:	Informal; Iterative (responding to events)	Paternalistic (reflecting a sense of fair play)	Consultative among agencies (citizens have no legal power)
France:	Informal (though deductive)	Elitist (dependent on experts)	Consensus in Private (e.g. in Tribunals)
Economic European Community:	Rationalistic	Elitist	Bureaucratic source of Arguments; Rules negotiated along with other issues

* Ideas and assessments in this table are attributed to Jerry Ravetz.

[4] The human reality that any particular commitment to action may be more a response to cognitive limitations, stress, and anxiety than a response based on rational evaluation and selection among alternatives

Limits to rationality in human decisionmaking is seen by March and Simon (1964) as being a result of cognitive limitations in humans as information processing devices. In Simon's concept of "bounded rationality," alternatives and their effects are not all known in advance but are discovered through information search procedures. Pure rationality would require a complete preference ordering of all the possible alternative actions based on evaluations of their respective effects. Bounded rationality in humans is instead characterized by satisficing (searching until a satisfactory solution is found and then adopting it and ending search) rather than optimizing (requiring generation and evaluation of all possible alternatives). Cognitive limitations, stress, and anxiety also lead humans to adopt informal rules or heuristics which govern how they search for alternatives and their effects, and a system of concepts and cues influencing what they perceive, and how they interpret resulting information. Decisions made or information communicated not on the basis of the evidence itself but on the basis of inferences drawn from the evidence and influenced by the concepts and cues created by an individual are products of "uncertainty absorption" according to March and Simon (1964, p.165). Dror (1980) refers to similar phenomenon as "mistake propensities" and "uncertainty distortions and repressions" caused by continually reinforced ways of perceiving situations which, though functional in some settings, may be dysfunctional in others.

Avoiding decisions is a further means for controlling stress. Decisions imply discretion on the part of the individual, at least discretion and/or responsibility to choose among available options. The exercise of discretion itself may be stress inducing. For example, Thompson (1967) suggests general plausible propositions about discretionary behavior in organizations in response to anxiety:

> When the individual believes that his cause/effect resources are inadequate to the uncertainty, he will seek to evade discretion. (p.119)

This may occur by his perceiving the situation as one to be based on precedent or formula and therefore not requiring a decision identifiable as his own. A second proposition is the following:

> The more serious the individual believes the consequences of error to be, the more he will seek to evade discretion. (p.120)

Here, discretion is substituted, for example, by objective testing, as in personnel placement or educational promotion.

Within organizations, precedents, institutionalized rules, standard operating procedures, routinized programs of action and classifications of situations for determining what is the proper rule, procedure, or program to use, are all common ways to limit the exercise of discretion and thereby limit deviation in organizational response to normal situations encountered. Of course, discretion is not completely evaded, since using one of these institutionalized mechanisms is a decision in itself. The point is that institutionalized rules for response often impose severe limits on the considerations of alternative actions and on the generation of new alternatives. Stress and anxiety may often lead individuals to resort to these rules rather than proceed with information search and action choice based on principles of rational analysis.

[5] The social/interaction reality that achieving consensus for commitments to a course of action may be based less on the identification of a most preferred alternative and more on the acceptable conduct of a participative, open-minded process of fact-finding, exchange of views, and compromise

In complex government policy matter, decisionmaking has become somewhat of a social ritual. Participation by groups of affected or even only interested groups are now mandated in the U.S. in some circumstances. Various government agencies and levels, all affected by the interdependent consequences of their separate policies and programs, consult, coordinate, interact, and in other ways engage in the decisionmaking processes of each other. Studies may be ordered which then spawn further studies to examine the conflicting or uncertain results of earlier ones. Social experiments may be conducted, polls or referenda taken, and political positions formulated. The entire process may take years before a policy is finally adopted or action program begun.

Much of this ritual is a reflection of the cultural/legal/political traditions and changes to those traditions that are now occurring. Objections and demands by citizen groups in the U.S. are often more directed to the processes of government agencies in making policy decisions than to the actual policies that are adopted. To many interested parties, participation may symbolize power or standing, or at least acknowledgment of their particular viewpoint. All the ritual and inclusion of so many groups may indeed benefit the search and evaluation of alternative policy choices, or may instead bog it down or redirect it toward more politically acceptable but less analytically optimal solutions. The evidence that fair and open procedures actually lead to "better" solutions is hard to come by, even in a post hoc decision review.

First Order Responses by Management Scientists

As a response to these and other realities about decisions and decisionmaking, some authors have proposed alternatives to the rational model of goal-directed behavior as a description of decisionmaking processes actually used. Most of the following alternative models have an organizational context, with the relation between the individual and his organization influencing decisionmaking behavior strongly.

Organization Process Model: This is a model proposed in Allison (1971) and expands on the notion of decisions in large organizations being formulated less on rational analysis and more on the basis of standard operating procedures and routine patterns of behavior (programs and repertoires). Similar and earlier arguments were made by Downs (1966) and March and Simon (1964).

Bureaucratic Politics Model: When more than one person or group is involved in the decisionmaking process, differences in goals, perceptions, and resources plus territorial interests and quests for increased power tend to introduce bargaining behavior, formation of coalitions, and situations requiring conflict resolution. This model is used to help understand decisionmaking in large organizations and bureaucracies as a competitive bargaining game among players positioned hierarchically. March and Simon (1964) view bargaining and politics as two ways organizations handle conflict (along with problem solving and persuasion). At its current state of development, game theory may help explain some decision behavior, but evidence that people in real decision situations involving conflict act in accordance with gaming solutions is sparse.

Disjointed Incrementalism or the "Muddling Through" Model: More closely consistent with reality in advanced countries where organizations have achieved relatively stable relations with their environment is Lindbloom's theory of "muddling through" first proposed in 1959 and developed over the following decade. In this theory, decisionmaking leads usually to incremental changes toward agreed-upon policies closely based on past experience. Lindbloom, like Simon, basis his arguments on the limits of information processing capabilities in humans, and similarly concludes that all alternatives will not be considered, but only those near the status quo. The critical test of decision merit is if all parties agree to it. Criticisms of Lindbloom's theory by Dror (1971b, pp.257-263) focus on two aspects. First, Dror argues that for a strategy of incremental change to be useful, present policies must be generally satisfactory and the nature of problems encountered and ways of dealing with them relatively constant, conditions not usually found in developing countries or in others facing rapid change. Secondly, Dror sees the "proinertia and antiinnovation" aspects of the theory as blocks to stimulating administrations to seek new ideas and attempt novel solutions. This latter argument is interesting in that it points out the differences between descriptive and prescriptive models and how they might be related. Kickert (1980, p.48) interprets Dror's arguments as indicating "prescriptive (normative) models should indeed by based on a description (model) of reality but the descriptive model cannot simply be equalized to a normative one." The major differences evident in the model of rational analysis, which requires evaluation of all possible alternatives, and the model of muddling through, which severely limits alternative consideration, indicates the current polarities of models of prescription and others of reality.

Intuition: Though no formal theory of intuition as a decisionmaking process has yet been formulated, many authors do include discussions of intuition or other "extrarational" processes such as judgment, sentiment, and moral values in their descriptions of how decisions are made.[5] To a great extent, the descriptions of what intuition is or how it works are formulated in rather imprecise terms. What is made clear, however, is that models of rational analysis seem inadequate in describing common recognizable acts of creativity in alternative generation, or insight into a preferred solution, or seemingly direct leaps to an appropriate conclusion. Certainly the acknowledgment of these phenomena is helpful in maintaining a proper perspective and sense of humility when approaching such an unclear, complex phenomenon as human decisionmaking with the simplistic decision concepts currently available to us.

[5] See for example Cowan (1975) and Morris (1968, pp.199-207).

Organized Anarchy or the "Garbage Can Model of Organizational Choice: Cohen, March, and Olsen (1972) have focused on universities as characteristic of organizations with inconsistent and ill-defined preferences; ill-understood processes by which they operate, learn, and cope with emerging problems; and fluid participation, to develop a model of organized anarchy. In situations characterized by the above, decision opportunities are ambiguous stimuli for "choices looking for problems, issues and feelings looking for decision situations in which they might be the answer, and decision makers looking for work" (p.1). Principles of the model are used by Sproull et.al. (1978) to help understand decisions regarding the early history of the U.S. National Institute of Education, and by March and Olsen (1976) regarding the selection of a dean, the location of a college, and other decisions within educational contexts.

Other models offered as alternatives to the model of rational analysis include "System Models," "Policy Models," "Models of Cognitive Style," and "Policy Content Approaches" all discussed briefly in Lynn (1980, pp.9-22) plus the "Mixed Scanning Model" proposed by Etzioni and the "Cybernetic or Homeostatic Model" of Steinbrunner based on principles of Ashby and Simon discussed briefly in Kickert (1980, pp.48-50). Kickert also presents a more detailed survey of most of the alternative models listed above. Diversity among these models provides a clear indication of the complexity and uncertainty surrounding actual decisionmaking processes. In some of the perspectives represented by these models, rational analysis has only a small part to play if any at all.

The Exceptional Nature of Decisionmaking in Science Management

What can the field of science management offer? First, better understanding of the complexities of real decision processes requires a rich source of decision behaviors indicative of the complexities involved. Secondly, these processes must be highly visible or discernible so that analytical or modeling efforts are feasible. Science management offers an empirical base of both characteristics.

- *Science Management Offers Visible and Discernible Complex Decision Processes.*

Science management, including both research management and science policy formulation, is illustrative of most of the decisionmaking realities described above, and of many of the perspectives represented in the alternative models of decisionmaking. Table 8 summarizes many of these realities and points out where each is reflected in key science management decisions. Some of the latter items (in italics and double brackets in Table 8) are clarified below in examples of actual processes used and described in published documents.

Table 8

Decision Processes in Science Management

Realities of Decision Processes

[a] *complex dynamics:* many steps each requiring resolution of differences and influenced by unanticipated events, new actors, and new problem formulations

[b] *no single correct or optimal decision:* legitimate differences in goals and importance placed on various criteria make no dominant solution possible

[c] *decision processes are shaped by cultural/legal traditions*

[d] *cognitive limitations, stress, and anxiety* all limit the number of alternatives examined

[e] *decision processes as social ritual:* reflecting acceptable conduct of search and evaluation procedures

[f] *reversions to standardized operating procedures:* avoiding decisions by using institutionalized rules and response behaviors instead

[g] *bargaining:* and coalition formation which influence decision outcomes

[h] *incrementalism:* in decisions introducing change

[i] *reliance on intuition*

[j] *choices looking for problems:* and issues looking for decision situations, and solutions looking for issues, all as more representative of actual situations than classical decision theory suggests

Research Management Decisions and Decision Process Realities (indicative of the listing above)*

(1) Choosing major topics of the research agenda: [[*a*]], [b], [d], [[*e*]], [g], [[*i*]], [j]

(2) Choosing key research staff: [[*b*]], [c], [d], [e], [g], [[*i*]], [j]

(3) Planning a research program: [[*a*]], [b], [d], [[*e*]], [[*f*]], [g], [i], [[*j*]]

(4) Project selection and budgeting: [a] or [f], [[*d*]], [[*e*]], [g], [[*h*]], [[*i*]], [j]

(5) Designing the organizational structure: [[*a*]], [b], [c], [d]

*Items in italics and double brackets are discussed in the text.

Table 8 (cont.)

Science Policy Decisions and Decision Process Realities (of the same listing above)

(1) Choosing priorities for science development (e.g. basic versus applied research; and the relative support of science for military, energy, space, health, etc.): [a], [b], [c], [e], [g], [h], [j]

(2) Setting the science budget (i.e. allocations to science agencies; to manpower training; and to institution building; etc.): [a], [b], [c], [e], [g], [h], [j]

(3) Funding of specific science projects (both large and small): [a] or [f], [b], [d], [e], [i]

(4) Choosing proper mechanisms for directing and encouraging science development (e.g. tax incentives, formula allocations to regions, and peer review processes for proposal acceptance): [b], [c], [f], [h], [i]

(5) Choosing and/or designing organizations for science policy advice: [a], [b], [c], [g], [i]

Decisionmaking realities within the context of research management are illustrated in the practices employed in the following processes:

(1) Choosing major topics of the research agenda (i.e. identifying the priority disciplines, topics, or problems on which to focus the research program). Characteristic of this process are:

Complex dynamics and decision processes as social ritual: The major topics or problems on the research agenda of a scientific organization are rarely if ever deduced directly from the organizational charter or mandate. Enabling legislation or founding statements provide notoriously little guidance on what specifically is to be addressed in making contributions toward abstract or lofty goals. Instead, determination of the research agenda is usually a complex social process of solicitation of priorities from interested parties and mediation among them.

For example, in setting the initial research agenda (list of projects) for the International Institute for Applied Systems Analysis, Levien (1977, pp.7-9) mentions discussions with the more than a dozen national member nations as the process used for developing a balanced portfolio responding to the multiple interest groups (national member scientific organizations), multiple goals, and the capabilities and interests of the research staff. For the U.S. National Institute of Education, Sproull (1978,pp. 36-59) describes the variety of activities of outside consultants, special planning units, and congressional hearings contributing to the alternative listings of program initiatives (research agenda items) proposed, and (on pp.162-201) the many forms of deliberations and reformulations of the agenda that occurred. For the Rand Corporation, major topics of research are arrived at in a variety of ways. According to Smith (1966, pp.165-171), many arrive through formal and informal negotiations with a client leading to agreements reflecting both client interest and Rand interest and capability. Others are self-initiated based solely on the interests of research staff, and still others are some combination of the two. In all three cases, processes used stress extensive discussion, a style of consensus rather than authoritarian decisionmaking, and attention to the interests of constituent groups.

Reliance on intuition: Also in all three cases above is the lack of any analytical way to predict that the topics chosen would yield to research efforts successful in promoting overall organizational goals. Here, as in other science decisions, the judgment and intuition of the single top manager of the organization (research director) is often critical.

(2) Choosing key research staff (i.e. identifying candidates and choosing among them for research leader positions):

No single correct or optimal decision, reliance on intuition: Personnel decisions, especially for key positions, are laden with uncertainties, incomparable dimensions (personality traits), and varying views of what is important. As in marriage, there is no best choice out of all possible choices; only choices that end up working out well or not well. In key personnel decisions, reliance on intuition and efforts for ensuring a proper search and evaluation process are common. These latter efforts promote fair decisions, but not necessarily optimum ones.

(3) Planning a research program (i.e. not the approval of the plan, but the input, analyses, and drafting involved in the plan preparation):

Complex dynamics, social ritual, and issues looking for decision situations: Wirt et.al. (1975) document the wide range of activities for program planning found in U.S. federal research agencies, including planning workshops and conferences; use of advisory councils; internal staff planning meetings and generation of issue papers and program proposals; studies of needs assessments, program alternatives, social experiments, and policy research into underlying causes of problems, conducted by outside consultant groups or special staffs; use of formal planning techniques (frameworks of objectives and decision criteria); and variations in the role of the program officer in formulating plans. For specific research organizations, planning conferences are routine and plans are prepared in relation to specific prestated criteria such as multidisciplinary character, clarity of accomplishable objectives, interrelations with other research units within the organization and with external cooperating institutions, and clear relevance to institutional goals. Yet the process of program planning may be anything but straightforward. For planning the initial programs of the U.S. National Institute of Education, Sproull et.al. (1978) portray a process fraught with ambiguity, recycling of drafts, and inability to reach closure, all despite a desire to implement a rational model of decisionmaking for the planning process:

> Rather than consider alternatives simultaneously, the NIE system examined them sequentially. As each alternative cycled through its iteration, events external and internal to the institute continually reshaped the criteria against which the solution was judged. The development of alternatives, the desire for and belief in deliberation, and the press of events led to situations in which the alternatives under consideration were out of phase with the criteria used to evaluate them. The processes we have described came to closure only when an important external deadline required it - in the case of the budgets...or when staff exhaustion and a deadline impelled it - in the case of the planning process. In each case the final decision was made despite desire for further deliberation. (p.200)

(4) Project Selection and Budgeting:

Cognitive limitations and anxieties limiting alternatives examined; decision processes as social ritual; and reliance on intuition: Processes for selection of R&D projects and for their funding comprise a major proportion of the literature on research management. What is available for review ranges from detailed descriptive accounts of specific selection decisions to highly mathematical formulations offered as general prescriptive aids. In most R&D settings, the number of possible projects of potential relevance is unlimited. Consequently, organizations either conducting research or funding it elsewhere develop ways for encouraging project proposals in areas in which they have programmatic interest. These include Requests for Proposals, Calls for Papers, publication in external media or in research plan documents of organizational interests in funding research of specified focus, and in the case of organizations conducting research, hiring scientists with established reputations in research fields of interest. Project generation processes are important components in project selection decisions because they help create the alternatives considered for funding.

Some of the current diversity in both project generation and selection processes among U.S. federal research agencies is presented in Wirt et.al. (1975). These processes range from reliance on the scientific community to submit unsolicited proposals, and use of structured panels of peer scientists to review and select projects recommended for funding; to processes dominated by the personal knowledge and initiatives of program officers or of internal program management committees. Unlimited project alternatives, major uncertainties about project success or usefulness of findings, and the personal consequences on individual careers often resulting from major selection decisions, all have contributed to the rather elaborate selection processes found in the U.S. These included use of science panels, of formal project evaluation forms, and of advisory boards and multiple authorizing authorities, especially when government funds are involved. Even where more quantitative formulations are suggested (cf. Cardus et.al. (1980), Sigford and Parvin (1965), and Wirt et.al. (1975, Appendix C)), the determination of probability of success, probability of utilization, and valuation of potential benefits require some measure of intuition backed where possible with consensus and/or enthusiasm for support.

Incrementalism: Managers of research rarely act in the characteristic manner of a military officer deciding for himself the proper course of action, then leading the troops to battle; rather, their actions are more like that of a sheepdog running around the edges of its herd and making loud noises just to keep everybody moving in the same general direction.[6] Approval of projects and setting of budget levels are two of the few management tools available for influencing the direction of research (another is hiring) and are usually focused on marginal adjustments rather than trend reversals or other major discontinuities (To do otherwise may lead to the herd dispersing in confusion or simply ignoring the irritating barks). Major research programs do not usually conclude and disappear; they spawn other related research, revive in other guise, and hopefully cumulate toward desired results beyond the scope of any one project. In this way, research capabilities, contacts, and investments are not lost following the scheduled completion of a program. Few research organizations could survive continued major shifts in programmatic focus or internal budget swings. Equally important, project approvals and budget levels often connote status in the organization. Unequal or changing distributions of internal funds may be interpreted as symbolic of different or changing status of people, disciplines, or problems, or of the arbitrary exercise of power on the part of the research manager. Consequently, many research organizations, especially international or interdisciplinary ones, exhibit not only incrementalism in decisions of program budgeting, but also equality in funding (program areas increment equally) despite lack of evidence that research in one area, even if as important as research in another, is equally costly.

(5) Designing the Organizational Structure:

Complex dynamics and cognitive limitations: Published accounts of the issues and circumstances surrounding the initial organization of a research institution, and its many reorganizations are also available (cf. Smith (1966) regarding the history of organizational structures of the Rand Corporation, Levien (1977) on the initial and current structure of IIASA, and Wirt et.al. (1975) on structures in

[6] A characterization voiced by Roger Levien, director of IIASA.

U.S. federal research agencies). Other accounts of organizational history are readily found in internal documents, published reviews by outside agencies (cf. the U.S. Government Accounting Office's published report on the East-West Center (1978)), and consultants' reports, obtainable from the individual research organizations. In many cases of organizational change, the resulting structures are reflections of the dynamics and complexities of constituents or supporters, and are symbolic of changes instituted in response to crises (cf. Selbst (1978), Smith (1966), Government Accounting Office (1978)).

Similarly, the realities of decisionmaking listed in Table 8 are illustrated throughout the context of science policy formulation. Overall, probably more has been written about science policy than about research management, both in general terms and with regard to specific examples. The general literature on science policy includes material on science and technology policy history, developments, and perspectives; on methods for policy analysis and formulation; and on the relation of science and technology policy to society. Specific policy studies include analyses of science policy formulation processes within specific nations, or comparing processes across selected nations; and the legislative histories of specific science projects. This combined literature is a rich base of detailed information surrounding science policy decisions and decisionmaking in a wide variety of contexts. Regarding science policy formulation, perhaps the most information is available on the processes used in the U.S. and the U.S.S.R.[7] All the realities of decisionmaking processes listed in Table 8 are clearly evident in both these cases, and most likely in all cases of industrialized nations.[8]

[7]For example, an article by York (1973) titled "The Nuts and Bolts of Science Policy" describes the lengthy U.S. process of science policy formulation in most cases, from ideas generated in the science community, through the relevant national science agencies, then through the complex Executive and Congressional budget and appropriations processes, and finally to the agencies responsible for carrying out the policy or program. In a similar fashion, Turkevich, in an article titled "Soviet Science Policy Formulation" (1977), describes Soviet science decisionmaking involving the First Secretary of the Party, the Politburo of the Central Committee, the Central Committee itself, the State Committee on Science and Technology of the Council of Ministers, the State Planning Committee (Gosplan), the Presidium of the U.S.S.R. Academy of Sciences, the relevant ministries, etc. Both of these articles are brief and readable, though others may be more exact in their descriptions.

[8]Rather than attempt here to exemplify the obvious and similar characteristics of decisionmaking realities for science policy formulation as was done earlier for research management, I focus on science policy formulation in the context of the roles of scientific analysis, as described in the next chapter.

CHAPTER 7

REALITIES OF THE ROLES OF ANALYSIS

The role of scientific analysis within either management or policy formulation in any setting is shaped to a great extent by both the nature of scientific analysis itself, and the nature of the context in which it is employed. This section focuses on the realities of scientific analysis in general (including management science analyses) and the roles it currently plays shaped by the realities of its present nature. Examples in science policy formulation are used to provide illustrations of the nature, limitations, and productive uses made of scientific analysis.

Earlier chapters of this paper addressed the nature and limitations of management science. Additional characteristics of scientific analysis in general pertinent to its role in science policy include the following.

Realities of the Nature of Analysis in Policy Related Roles

(a) Within science policy contexts (and elsewhere), scientific analysis is no longer viewed by participating parties as being value-free or purely objective.

Not only is it becoming increasingly recognized that analysis is a process incorporating subjective judgment and selective perception in problem formulation, choice of analytical framework and evaluative criteria, data, and perspective for interpretation of results, as well as cognitive distortion and tendencies to prefer hard data over soft and measureable characteristics over hard to measure ones; it is also becoming recognized (as argued by Ravetz) that these craft and creative aspects of scientific analysis cannot be disassociated from the alleged objective and impersonal results and knowledge derived from the analysis. In other words, as products of a subjective and value-laden process, analytical results necessarily carry the subjective and craft characteristics of those processes.

(b) Scientific analyses on any particular policy issue usually occur in multiple amounts, focusing on various aspects of complex decision or policy contexts.

Rarely can one analysis encompass all aspects of a complex issue, and rarely do alternative analyses on the same aspect produce identical conclusions. Consequently, policy issues are typically fraught with competing and conflicting analyses on similar issues, or with analyses which focus on different and incomparable issues supporting competing general conclusions. Even when many analyses are performed on various different and complementary aspects, there is usually no overarching or integrative analysis (or methodology for performing one) pulling all the analyses together.

(c) In practice, scientific analyses differ in breadth of scope, depth of study, and employment of sophisticated techniques, according to the kinds and amounts of resources provided; and parties to policy issues usually have unequal financial resources or political standing with which to support their analyses.

Competing scientific analyses therefore are not usually competing in their application of scientific principles alone. As indicated in an earlier chapter, referral to sophisticated scientific analysis may be one way of shifting the argumentation to a base whereby the inequalities in resources may be exploited to political advantage.

(d) Decision structures assumed by analyses may not match the actual institutional structures responsible for decisionmaking.

Lathrop (1980) argues that most analyses infer a single decisionmaking process to make tradeoffs among conflicting objectives and that in reality (as in cases of siting decisions for liquid energy gas facilities in California), the institutional structure responsible for decisions is largely disaggregated and diffuse in focus.

(e) Despite the social and value-laden aspects of most policy issues, scientific analyses are usually focused on technical or engineering aspects.

For example, scientific analyses of energy futures tend to focus on the technically feasible rather than the politically or economically feasible (cf. IIASA's energy study and the WAES study). Similarly, for space exploration, analyses are most often focused on what is technically possible rather than what is socially desirable, relative to other social improvements. Perhaps most striking in this regard is the focus of analyses conducted to measure or reduce the risks surrounding hazardous facilities. Here, analysis is conducted predominantly on the engineering and materials reliability of systems developed to prevent or handle accidents, when recent events of nuclear reactor accidents, liquid energy gas leaks, and off-shore oil blowouts have shown that system failures often occur in management systems as well as in engineering systems.[8] Only recently has much attention been directed toward analysis of the adequacy of training in crisis management, of the procedures employed, and of personnel selection practices for these situations of potential hazard.

(f) Scientific analysis in usage is commonly advocative.

In their book *Models in the Policy Process*, Greenberger et.al. (1976, p.xv) view policy modeling (employing mathematical formulations of system interactions) as "becoming more widespread and more patently political in character." Various political functions served by analysis in this regard are listed below.

Given the nature of scientific analysis as partly subjective, diffused in focus, biased toward the technical and engineering aspects, and advocative in usage, what roles are suited to this nature and evident in practice? Also, if the complexities, uncertainties, and value conflicts of most policy settings prevent analysis from playing a prescriptive role in practice, what roles are useful to the policy formulation process?

Different Roles Played by Scientific Analysis in Policy Formulation

[1] Information generating roles of scientific analysis, especially within certain components of a decision or policy context:

Analysis performed on specific aspects of a policy context are commonplace and provide valuable information relevant to issues clarification and alternatives examination. Typical in science policy formulations are:

> *technical analyses:* focused on engineering feasibility, availability of required materials, predictions of system performance, and required advances in science or improvements in technology.
>
> *economic analyses:* focused on costs or on effects on employment, economic growth, or distribution of income.

[8]Fischer (1980) compares the nuclear reactor accident at Three Mile Island and the North Sea Bravo oil blowout of 1977 to suggest patterns of accident formation, including the failure of management or crisis-handling teams.

commercial analyses: focused on questions of transfer of scientific advances to commercial applications, profitability of resulting products in relevant markets, patent restrictions of commercialization, etc.

social or environmental impact analyses: focused on possible or probable effects of a program or policy on environmental quality, social institutions and interactions, social or political development, or any of a wide range of influences on the quality of life for an effected population.

risk or hazard analyses: focused on measuring associated risks and comparing them to risks of approximately equal magnitudes in nature or in other man-made settings[10], or focusing on the reliability of components (human and technical) in determining overall risk measures.

Analyses performed to generate information are educative in purpose. They are not necessarily meant to prescribe what course of action to take, but rather to lend greater understanding or insight into the problem addressed or the alternatives available. For example, by making assumptions and factual bases explicit and visible, analysis can at least help prevent use of wrong information or misinformation about the nature of the problem of the existence of considered choices. Similarly, sensitivity analyses can provide information on the effects of changes in assumptions or choices made on various aspects of the problem formulation. Analysis can further clarify who the stakeholders are within a policy issue and how they might react to positions along various dimensions of the problem; or conversely, why certain parties may be supporting particular positions. Greenberger et.al. (1976, pp. 24-25) describe three common ways analyses are used to produce relevant information or understanding: (1) *unconditional forecasting:* arriving at predictions about conditions that policymakers are likely to face in the future (as in econometric modeling); (2) *conditional forecasting:* formulating likely consequences of policy decisions (or the absence of policy changes, as in the Global 2000 Report to the President); and (3) *simulations and scenario writing:* articulating or focusing attention on policy problems, issues, or options and their potential consequences.

[2] Process-supporting roles of scientific analysis:

Scientific analysis is supportive of movement or closure in at least three phases of policy formulation:

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[10] See for example Eng (1980) including comparisons of days of life expectancy loss, by age, in different industries such as coal mining, construction, and railroad work; and probability of deaths per year for various environments such as road transport, air transport, and factory settings, compared with probability of death by natural causes, also by age.

constructive function of analysis (helping to get deliberations started and/or initial policies stated): in this role, analysis serves to clarify issues, define concepts and measures, focus attention, and explicate opposing viewpoints.

critical function (exposing arguments and their supporting evidence or logic to criticism): here, scientific analysis is used to test or evaluate the foundations of opposing views, and to bring the results of these tests to public scrutiny.

ritual function (ensuring proper procedures): analysis of environmental impact, or effects on employment across certain segments of the population, are now legislated in certain contexts and become part of the normal ritual surrounding policy or decision formulation and review.

Whether scientific analysis actually produces new relevant information when performing the third of these functions is not paramount in this particular role. Rather, analysis is used to ensure that due process is being taken in allowing any new information to emerge.

Lathrop (1980) provides two examples of scientific analysis serving these functions in relating the role of risk analysis in the siting of hazardous facilities in California. He describes analysis useful in the drafting of initial legislation (constructive function) and in the presenting of cases before a hearing process (critical function). In the former example, analysis is used to produce specific numbers as legislated limits to action (e.g. no facility within 10 miles of a city; nor more than 100 units of radiation per time period). In the latter example, analysis is used to support arguments of opposing parties within an appropriate public setting.

[3] Political Roles of Scientific Analysis:

Analyses are used in a variety of ways to promote political ends, including but not limited to the marshaling of support for particular positions. Greenberger et.al. (1976, pp.45-46) describe three different political functions:

providing political validity: earning a place for the issue in question on the political agenda, especially important when unusual decisions are required and when the number of interested parties is large. Scientific analysis (policy modeling) is useful here for focusing attention and converting dimly perceived problems into defined political issues.

submerging or defusing issues: pressures for immediate action may be evaded by referring hot issues to study teams for detailed analysis. This tactic has almost become a cliche; yet its persistence is indicative of its usefulness in forestalling action until a better understanding of the situation (or the positions of interested parties) is known.

> *marshaling support for a particular political position:* Greenberger and his colleagues find evidence that policy research, including modeling as one form, is shifting away from a focus on the identification of policy problems and the invention of policy options, and toward the systematic testing of particular policies (Greenberger et.al. 1976, p.23). As a result, they are increasingly being used as "instruments of political advocacy."

As political instruments, scientific analyses carry the prestige and status of objectivity and rational reasoning. Yet highly quantitative analysis offered as "scientific" may be anythings but reasonable or objective in its approach. For this reason, scientific analysis serves another role in raising political debates to more reasoned levels of contention.

[4] Role of scientific analysis in promoting maturity in argumentation:

With experience in the introduction and use of scientific analysis in policy formulation, sophistication in the analyses themselves is also growing. For example, early analyses in the context of hazards of nuclear plant meltdowns or other possible manifestations of advanced technology were often either "Valium reports" based on probability of accident estimates lower than normal life risks, or "Doomsday reports" ignoring probability of occurrence and focusing instead on worst case scenarios. Proper treatment of very low probability events with catastrophic costs is still not well formulated (cf. Kunreuther 1980). Yet experience with blatantly one-sided analyses, and with more objective analyses which attempts to synthesize information from each side, helps, but does not ensure, proper reaction to such biased analyses and thus maturity in subsequent analytical support of argumentation.

Attending and adapting to the realities of goals, decisionmaking processes, and roles of analysis in management and policy formulation are some ways in which scientific analysis, including management science, can develop improvements in its approach and applicability. The context of science management has been shown as a rich empirical base illustrating these realities in a visible and discernible manner and offering a wide variety of problem contexts for testing alternative analytical formulations that reflect realities more accurately. Additionally, trends in the problems and issues of science management point toward directions for useful methodological advances in scientific analysis and encourage their development. In the next part, these trends are described and related to emerging and still needed developments in management science.

Part III

ENCOURAGING CURRENT TRENDS IN MANAGEMENT SCIENCE

INTRODUCTION TO PART III

Acceptance of the need to reconceptualize management science formulations to conform to problem realities is a necessary step in the further development of management science. It is in this step that science management is exceptional as a problem context for management scientists to address and to use in developing new formulations. To science managers, goal ambiguities, performance unmeasureabilities, considerable use of intuition, and the complex, dynamic, and interpersonal nature of decisionmaking are long accepted facts of life. To a large extent, the recognition of these realities is what may have prevented traditional management science formulations from appearing as relevant to science management. Where better to turn, then, for learning how decisions are made in the face of these acknowledged realities than to science management as a problem context.

Secondly, science management focuses both on the individual scientist; his characteristics, motivations, and behaviors, as well as on the organizational, political, and social systems in which and for which science efforts are directed. In addressing needed developments in management science formulations, Dror (1971,p.51) calls for a merging of the management sciences with the behavioral sciences partly in recognition of the differences between traditional management science approaches and the realities of decisionmaking discussed earlier. Science management is a context well suited for incorporation of behavioral aspects; indeed, much of the literature in research management is based on empirical findings concerning behavioral phenomenon.[1]

[1] See for example the excellent text *Scientists in Organizations:* Productive Climates for Research and Development (Pelz and Andrews 1976).

Thirdly, certain of the problems and issues of science management listed earlier in Tables 2 and 3 relate directly to current trends in analytical formulations emerging in the management sciences. Addressing these science management issues may then aid in the developments of the emergent analytical trends. In the following section, these trends and related science management problems and issues are discussed.

Trends in management science reflect some, but not most, of the realities of goals, decisionmaking processes, and roles of analysis described previously. In some cases, these trends represent a positive adaptation to realities and a corresponding broadening of perspective and scope of potential contribution. In other cases, they represent an enlightened retreat in expectations associated with management science applications and a restatement of appropriate aspirations.[2] Table 9 indicates some of the trends in management science formulations. Most are highly interrelated and are various manifestations of a few basic shifts in focus discussed in the following chapters.

[2]The retreat in expectations refers to changes away from an orientation of management sciences focused on providing answers or solutions to problems, and toward an orientation focused on increasing understanding and insight, sharpening intuition and judgment, and gaining familiarity with the complexities of real problems (cf. Tomlinson 1979 and Quade and Miser 1980). In all fairness, I should add that the authors named above and others (Charles Hitch, Alec Lee, Alain Enthoven, and Gene Fisher to name a few) have insisted all along on the latter orientation, despite the more enthusiastic advocates of management science in their wake, and the vail of higher mathematics and optimization routines prevalent in today's management science journals.

Table 9

Recent Trends in
Management Science Formulations[a]

Earlier Theoretical Focus and Analytical Formulations (circa 1940's through 1960's)	Current Theoretical Focus and Analytical Formulations
Economic considerations of efficiency or effectiveness are paramount	Focus on broader social goals as context for economic considerations[b]
Emphasis on quantification of analysis for specific issues	Emphasis on systems-behavior and interactions among issues[c]
Focus on "best" or optimal solutions (via maximization or optimization approaches)	Emphasis on "what if" questions and adoption of contingency or situational approaches
Emphasis on solutions analytically derived from well-defined problem formulations	Focus on postulated systems with no analytical solutions
Emphasis on hard models (in the style of the physical sciences)	Adoption of soft model elements (incorporating behavioral and attitudinal aspects of the social sciences)
Focus on prescription (from a theoretical framework)	Focus on description (from an empirical base of case studies and social science findings)[b]

[a]This list is neither exhaustive nor are the bifurcations into earlier and current focus as sharp as implied. The intent of the table is simply to provide an overview of some of the highly related trends in focus evident in management science literature, often as a response to failures in the earlier mode. The table continues on the following page.

[b]Each of these the trends marked with superscript b (representing changes in the concepts of, or importance of, efficiency and effectiveness, rationality, and prescription) reflects an alteration from the traditional assumptions underlying management science listed in Chapter 1.

[c]Management science in this paper includes a systems orientation as a fundamental concept. This particular trend represents a recognition of the systemic interactions among the economic, political, and social systems in which the systems addressed are embedded.

Table 9 (cont.)

Earlier Theoretical Focus and Analytical Formulations	Current Theoretical Focus and Analytical Formulations
Emphasis on substantive rationality (evaluation of decision consequences)	Emphasis on procedural rationality (evaluation of decision processes)[b]
Focus on the choice situation: selecting from among alternatives	Additional focus on the structures in which decisions are made, and post-decision problems of implementation
Focus on deductive reasoning (applying "laws" for stable and well-regulated systems to specific instances)	Focus on inductive reasoning (developing new alternatives, new models, and new understandings for less stable, less regulated systems)
Focus on the individual decisionmaker (or the organizational unit as if it were an individual)	Focus on larger and more complex institutional forms in which multiple decision units are recognized
Emphasis on multiple character of objectives or criteria	Toward frameworks of analysis for multiple decisionmakers[d]
Interaction with user limited to early phases of needs assessment and problem formulation, and late phases of implementation	Emphasis on interaction with user throughout entire process

[d]The word "toward" is significant. As yet, no frameworks reflecting realities of coalition, alliance, and cooperative/competitive behaviors for n>2 persons exist. Game theory and oligopoly theory do not reflect or explain behavior well. Some progress toward multiple decisionmaker analysis is being made, however, as discussed later in the text.

CHAPTER 8

ADOPTION OF A BROADER AND MORE SYSTEMIC PERSPECTIVE

In both the public and private sectors, experience with analysis using traditional management science formulations has shown those formulations to be too narrowly focused to capture all the important dimensions of the problems or issues addressed, or not attentive to critical issues of the larger systems in which the systems under study are embedded. For example, in the public sector, Dobell (1980,pp.12-14) argues, "The importance generally attached to economic criteria in government decisions has, in practical terms, declined substantially." Why? "First, broader social goals increasingly dominate economic considerations, calling for measures which protect people and enterprises from the misfortunes of the market and even from the pressures for mobility, adjustment and adaptation to which market signals might otherwise lead." In the face of these broader social concerns, "representations based on the criterion of economic efficiency are no longer very persuasive."

Non- Economic Criteria

Experience with management science approaches as aids to decisionmaking in hospitals, libraries, blood banks, and other public service institutions has similarly argued for a broadening of measures, criteria, and constraints beyond those of economic considerations. Though no new frameworks or analytical procedures have stabilized in practice for dealing with these broader perspectives, many current management sciences approaches to public service problems are forced to deal with them in one way or another (e.g. introducing additional variables representing social concerns, performing impact analysis on various additional issues, or employing multi-objective decision frameworks).

In the private sector, the situation is similar, though perhaps even more difficult because of the long history of profit maximizing formulations. Empirical evidence for profit maximizing behavior remains rare. "Moreover, such empirical evidence as we do have on the decision-making processes of business executives largely contradicts the hypothesis that business firms behave explicitly as profit maximizers" (Simon 1980,p.74).

Partly as a consequence of this shift away from economic efficiency or effectiveness as the one main criteria for choice, management science has redirected itself toward examining the effects of a variety of system changes on a wide range of system performance measures. Here, the trend is away from maximization/optimization frameworks and toward sensitivity analyses, scenario writings, and contingent/situational solutions. "Contingency theory is a reaction to the 'one best way' approach in the 'it all depends' direction" (Kickert 1980,p.191). Why is this occurring? First, conflicting values and consequently different weightings of importance of different objectives or criteria may defy an acceptable formulation of an optimal solution. Secondly, conditions change; what may be optimal at one time may be unpreferred at another, requiring a highly adaptive approach instead. Thirdly, the complexities of real problems may present numbers of alternatives far too many for assessment of each, so that only heuristic-based rather than optimal choices are possible. Ignorance, exogenous influences, and unanticipated events may further prevent optimal solutions from being specified. Accordingly, management scientists have begun to shift from formulations focusing on optimal solutions to frameworks in which system behavior can be better understood and assessed within larger contextual concerns.

Scenario writings convey internally consistent portrayals of possible futures, often, as is done in sensitivity analysis, to point out major changes that may follow from critical choices or developments.[3] Sensitivity analysis is also neither used commonly for prediction nor optimality (though it can help in the latter). Instead, it allows addressing the "what if" questions. For example, what if an assumption is proved false, a parameter is estimated differently, or the system structure is altered or

[3] The Global 2000 Report to the President (Barney 1980) is basically a scenario of the world future under the (relatively improbable) condition of no changes in U.S. policy. IIASA's Energy Study (Haefele 1981) provides two scenarios of world energy futures, one in a high energy consuming world and one in a low one. None of these scenarios are predictions. They do, however, help our understanding of the impact of system changes (or non-changes).

represented in another way? Here, the focus is clearly on the system, its behavior, and its performance along a wide range of dimensions, rather than on any particular solution and its optimality.

A third, highly related trend in this regard, is in the examination of issues in interaction with each other rather than in isolation. Traditional management science formulations tend to focus on specific issues, such as inventory management, production scheduling, and facility siting. A more common approach in contemporary applications is a systems perspective allowing a broader view of what issues are related or more suited to the problem addressed.[4]

Critical questions encountered in this emerging shift in management science toward adopting a broader, more systemic perspective are given below:

(a) If economic considerations of efficiency and effectiveness fail to capture broader social concerns, *what kinds of criteria* seem to appear as important in a wide variety of settings and in relation to broad social contexts, and how are these criteria measured and incorporated within decisionmaking processes?

(b) What are the socio-economic-political *conditions* in which certain broad criteria seem to be more important than others? How can these contingencies be modeled?

(c) What is the *role of individual perception and evaluation* in the assessment of choices with respect to various broad social criteria (as in risk assessment)?

(d) What *analytical frameworks* seem best suited to these broad social issues and the relations among them, and how can these frameworks be strengthened?

Relevant Examples in Science Management

Science management problems and issues provide a rich and relevant context for addressing these critical questions. For example:

o *Concern in science management has always been in support of broader non- science objectives of the host organization or government*

In both the management of research and the setting of science policy, a major focus of activity is on meeting or contributing to the objectives of the sponsoring organization and of society (Dean and Goldhar 1980,p.1 and White 1975,p.4). For research management, two factors make this process of science in support of non-science objectives highly

[4]For example, Dror (1971,p.13) indicated, "...by taking a broader systems view, we can often reformulate the decision problem in a way permitting better solutions. Thus, instead of asking ourselves how to improve peak-hour transportation, we may reformulate the question to how to reduce peak-hour traffic demand - which may bring up for consideration completely different alternatives (e.g., staggering work hours, changing physical planning to have people live nearer their place of work, or encouraging shopping by mail or through wired-television)."

problematic. The first is the multiplicative and confusing nature of goals associated with the R&D function. The second is the highly uncertain connection between R&D activity and the goals assigned to it. Radnor and Rich (1980,p.122) describe four highly interactive aspects of R&D goals which together give rise to their multiplicative and confusing nature: (1) variations in goal sources, including sources internal and external to the R&D activity, to the host organization, and to the sponsorship; (2) variations in goal content, pertaining to product, process, service, policy, etc.; (3) variation in the process of goal determination, regarding how goals are set and changed and what negotiations are included; and (4) variations in the target for R&D results, including the type of impact sought on other functions, on policymakers, on ultimate users, and the various evaluation criteria used in judging R&D performance. Specific goals for R&D thus result from contextual determinants within which the R&D activity is embedded.

In this regard, Radnor and Rich argue that questions of "how" to motivate R&D personnel (for which there is much knowledge available) are useful only in connection with "for what" R&D activity is to be motivated, and the conditions under which it is to take place. Few authors discuss the question of "for what" in terms of direct economic considerations. For example, in industrial R&D, the focus may be on improvements in the marketing, production, service delivery, or planning functions.

In a purposive and contextual approach to R&D management (as opposed to traditional context-free and goal-free internally focused studies of R&D organization design, information flow, and leadership), the question of how R&D relates to improvements in other functions is both paramount and quite uncertain.[5] Studies of economic returns to R&D are not uncommon. The important point is that depending on the purpose and context of R&D, economic considerations may be irrelevant. Actual purposes may have more to do with building growth capacity, market share, quality improvements, or survivability of the host organization.

In the arena of science policy, the "for what" and the "how does R&D relate" questions are even more apparent. In modern societies, the advancement of science and technology for its own sake is not well supported. On the contrary, industrialized nations are increasingly questioning whether they really desire the benefits of science in light of its perceived costs, and whether or not there are alternative ways to science for meeting national objectives. These issues arise most often in the annual development of science budgets in the federal government. Table 10 summarizes the issues of "for what" and "how does R&D relate" found in analyses of the U.S. federal budget for R&D for the years 1977 through 1979 (Shapley 1976, Shapley, Phillips, and Roback 1977, and Shapley and Phillips 1978).

[5]For example, Radnor and Rich (1980,pp.119-120) state,"The returns [to R&D] are frequently difficult to identify with any precision or confidence, especially given the longer time horizon involved in the activity, as compared to the more immediate returns to be observed from operational production and delivery (marketing functions). The literature has not been definitive in demonstrating the macro-economic benefits of R&D. Similarly, studies of its impact on public policy-makers have produced quite equivocal results."

Table 10

Problems and Issues of Science Policy in the Service of Non-Science Objectives*

[1] Are the nature and importance of the external objectives (external to science development) of federally supported R&D understood and properly assessed? Are science supported objectives of high priority?

[2] How does science help? Are the relations of federally supported R&D to the external objectives understood and properly assessed, including the questions of:

- (a) The potential contributions of the R&D to the specific external objectives;
- (b) The potential for side-benefits and spin-offs;
- (c) The relevance and assessment of national interest criteria (defense, international policy, national prestige, long-term economic growth, etc.);
- (d) The availability and merits of alternative non-R&D approaches to the same external objectives.

[3] Whose assessments are to be used for [1] and [2] (the relevant R&D agency, prospective users or beneficiaries, or a third party such as the Office of Management and Budget (OMB) in the U.S.)?

[4] What roles are to be taken by government as opposed to private industry in R&D for external objectives?

- (a) What factors will govern the degree to which industry will invest in R&D?
- (b) If current restraints that inhibit R&D and innovation were removed or mitigated, would industry actually perform more R&D than projected? Would more innovation be introduced?

[5] What is the relation of the use of science (and technology advance) in support of external objectives and in the generation of negative side-effects in the social fabric of society (e.g. in the centralization of social control, alienation, changes in values, effect on the environment, and threats to world survival)?

*Most questions in this table are taken directly from the texts of Shapley et.al. 1977 pp.35-38 and 1978 pp.66-83.

Questions of how R&D relates to these objectives are basically unanswered. Shapley and Phillips (1976, Chapter 4) review the literature on How Does R&D Help the Economy, and come to the following methodological conclusions:

> Economists trying to measure how and how much R&D helps the economy have not come to clear conclusions... Nevertheless, oversimplified models of basic research, and R&D in general, as *causes* of advances in economic welfare are pervasive in current discussions of R&D policy. (pp.72-75)
>
> The fact seems to be that conceptual, modelling, and data limitations have prevented the development of a generally accepted way to measure the economic benefits of federal support of R&D. (p.78)

The reasons:

> First, the horrendous complexity of the problem. Secondly, the absence of an adequate theoretical structure which reflects the realities of the present U.S. economy and the ways in which technological changes affect it. Third, the difficulty or impossibility of getting the data needed - accurate data, current data, and data that are disaggregated in ways needed for meaningful analysis. Fourth, the conceptual and practical problems of finding ways to measure outputs of R&D other than by cost or other inputs. (pp.78-79)

In science policy problems, how science advances relate to the generation of negative side effects is often seen to be as important as how science advances relate to meeting national objectives. In this regard, the broader, more systemic perspective is focused on the social and political impact of science advances as indicated in Table 10 item 5. Associated criticisms of modern technology (and science as its major producer) include the following:

> *technology tends to destroy human values;* i.e. technology imposes demands inconsistent with individual preferences (and seen as inferior to them);
>
> *technology alienates the individual;* producing resentment of forces beyond his comprehension and control;
>
> *technology thrives on a hedonism promoted by business for profit;* thus discouraging religion and undermining the viability of society;
>
> *technology is not "natural";* and interferes in the relation of man and nature;
>
> *science is a limited and misused tool for understanding self and environment;* it describes reality only in terms of objects within a time-space set (unlike religion or art); is an instrument of power; and serves the self-interest of its institutional networks. (Raffaele 1978, pp.77-89)

The purpose of this section is to illustrate that science management is already concerned with broad, systemic aspects. Interactions between science advances and the economy, social change, and individual values are critical issues demanding new analytical frameworks. The clarity and visibility of problems in science management in this regard make them appear unique; yet they parallel problems of analysis in business and other settings. The latter may simply be hidden by the history of approaching them through the narrowly focused perspective of traditional management science frameworks.

- *Criteria in science management have generally not focused on economic considerations.*

Criteria used in science management, both for evaluating alternative actions or decisions and for evaluating alternative management systems, focus only very little on economic considerations. For example, Table 11 illustrates non-economic criteria used (explicitly or implicitly) in project selection decisions. Earlier in chapter 3, Table 5 lists criteria for judging research management approaches to decisionmaking or problem solving. Of the 18 criteria listed, only one pertains to fiscal considerations. Finally, in the design, operation, and focus of the national science budget system, most criteria are concerned with issues other than economics (Table 12).

These tables support the contemporary notions of goals held by Simon, Cyert and March, and others, that organizations seek to satisfy a whole set of objectives and considerations rather than optimize on any one of them. Few of these goals may be economic in nature. This means that frameworks for expressing the system effects and interactions indicated in the tables cited need to be developed, as well as ways of relating decision alternatives to them. So far, management science approaches to project selection have addressed these system effects and considerations only in simplistic schemes of subjective ratings (checklists, weighted ratings, or delphi consensus).

- *Individual perception and evaluation play a large part in the assessment of alternatives along science management criteria.*

The relation of specific decisions or actions by individuals, organizations, or governments to broad social concerns is largely perceptual. Even in retrospect, it is extremely difficult to assess their influences. To a large extent, these influences are perceived differently, evaluated differently, and provoke different responses among various groups and interested parties.

Table 11

Non-Economic Criteria in R&D Project Selection Decisions*

Importance of the Project

[1] Importance to the R&D organization or its sponsor of the problem or opportunity addressed by the project

[2] Importance of the anticipated project results to the resolution of the problem addressed or to being able to take advantage of the opportunity encountered

[3] Importance of the project to maintaining organizational expertise in the area of the project; to maintaining the reputation of the organization; to maintaining cooperative ties with other institutions; and to attracting additional support to the organization

Probability of Technical Success

[1] Relative to the state-of-the-art of technical knowledge

[2] Relative to the state-of-the-art of supporting knowledge

[3] Availability of required manpower, technical resources, and funds all within the project schedule

[4] Existence of a detailed technical plan

[5] Length of project duration as planned relative to time available

Probability of Utilization

[1] Relevance to host organization goals

[2] Ease of understanding by prospective users

[3] Ease of implementation

[4] Compatibility with existing norms, past experience, and procedures and facilities available to users

[5] Accessibility of research results or products to potential users

[6] Observability or demonstratability of usefulness

[7] Trialability on a small scale before major or irreversible commitments must be made

[8] Credibility of anticipated findings

[9] Relative advantages over existing products or practices

*Many different criteria and systems of criteria are found in organizations throughout the R&D communities in different countries. Most of the ones listed here are taken directly from Cardus et.al. (1980) as illustrative of those found in government R&D settings.

Table 11 (cont.)

[10] Patentability of anticipated results

[11] Potential sales of anticipated products resulting from successful product development, or market share from product introduction (items 10 and 11 are particular to profit oriented R&D)

Fit Within the Project Portfolio*

[1] Fit of time demands on resources and equipment; dovetailing of research results from other projects

[2] Support for other projects and support by other projects

[3] Effect on geographic distribution of total project budget, or distribution among R&D units

[4] Balance achieved or distorted with regard to sponsor interests and multiple R&D objectives

*Compatibility fit rather than economic fit.

Table 12

Non-Economic Criteria in Science Policy Formulation*

Criteria Related to the Fit of the National Science Budget System Within the National Government

[1] The federal R&D budget decision system must function within the framework of the overall political system of the country (i.e. within the normal processes of planning and coordinating among component political structures)

[2] R&D budgeting must also fit into the overall pattern of the federal budget process

Criteria Related to the Operation of the National Science Budget System

[1] The system should be designed to minimize "single point failures" from a bad decision at any level (through suitable provision for consultation and review - in both Eastern and Western systems - and - in Western systems - public appeals and debate)

[2] The system should provide for a relatively quick reaction capability in restructuring budget allocations

[3] The system should avoid the misuse of plausible decision criteria such as relevance tests, cost-benefit analysis, or marketplace tests of value, in contexts where these tests are not applicable or where the underlying assumptions of the tests are not valid

Criteria Related to the Context of the R&D Programs

[1] The system must give visibility to future implications of current budget decisions (which restrict response to unanticipated future events) and provide capability for timely control of major R&D commitments

[2] The system for R&D budget decisions should give appropriate consideration to the broader implications of the R&D programs (implications for the national economy, national security, international cooperation, international competition in science and technology, and other national non-science objectives)

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*These sets of criteria are derived from the textual material of Shapley et.al.(1976, 1977, and 1978)

Table 12 (cont.)

Criteria Related to the Current and Future Productivity of the National Science Community

[1] Both the science budget system and the science policy formulation process should encourage the development and maintenance of a supportive science constituency among the population

[2] The science budget system and the decisions deriving from it should reflect the need to preserve and enhance the nation's capabilities to perform R&D (including the need to maintain effective institutions and facilities for R&D both in and outside of government support) and a continuing supply of well qualified scientific, technical, and management personnel.

[3] The science budget system should provide incentives and rewards for high quality work and creativity in R&D

For example, attitudes about the value of alternative energy sources, and particularly about nuclear energy, are based on a wide variety of beliefs about their likely effects and the nature of the risks involved. In a series of reports based on interviews with members of the Austrian public, and subsequent interviews with Austrian policymakers (Otway and Fishbein 1977 and Thomas et.al. 1980a and 1980b), attitudes for and against nuclear energy were shown to be reflections of differences in strength along 39 different belief dimensions. These include beliefs about the economic benefits offered, the likely social changes resulting from adoption of nuclear energy, the risks to the environment, and the nature of associated hazards as being uncontrollable, a threat to mankind, arising from a technology not well understood, and affecting large numbers of people at the same time (categorized as psychological risks). The follow-up study tested to see whether the beliefs of policymakers accurately reflected public beliefs about nuclear energy, and whether policymakers when asked to do so could accurately describe the public beliefs of both those for and against nuclear energy. Conclusions were given as follows:

> ...the difference in overall attitudes between policymakers and the public was primarily due to the fact that, for the public, psychological risks were strongly associated with the use of nuclear energy, while environmental risks made only a minimal positive contribution toward their attitude. A similar analysis of the policy makers' own personal responses showed that here psychological risks were associated only to a small extent with the use of nuclear energy, whereas environmental

> issues were perceived as a substantially positive aspect. (Thomas et.al. 1980b,p.23)

Could the policymakers reproduce accurately the general attitudes held by the portion of the public in favor of nuclear energy and the portion against it when asked to do so? Generally, yes, with the exception of a tendency to overestimate the positive attitudes of the group in favor of nuclear energy, and a tendency to underestimate the influence of perceived psychological risks:

> The accuracy of the policy makers' perceptions was somewhat diminished, however, by their failure to recognize the extent to which issues of psychological significance contributed negatively to the public's attitudes, irrespective of whether they were in favor or or against the use of nuclear energy. The policy makers underestimated the public's negative evaluation of psychological risks and they also underestimated the public's belief that the use of nuclear energy would lead to such risks. (Thomas et.al. 1980b, pp.24)

Evaluations of risk may be based on belief structures such as these, on comparisons to accustomed hazards, or in relation to an array of considerations such as whether the risk is voluntary or involuntary, of immediate effect or delayed, with no alternatives available or many, encountered occupationally or not, or with reversible consequences or irreversible ones. In science policy (including technology adoption), these evaluations are often critical. In U.S. cities, for example, public referendum has been used to decide the issue of public water flouridation, and in Austria, the first nuclear power plant, a 730 Megawatt facility at Zwentendorf near Vienna remains unoperational as a result of a November 1978 public referendum.

Here are cases that typify the power of broad social concerns in deciding matters of policy, and that explore ways to measure aspects of those concerns and examine differences in public and policy settings. Practitioners of traditional management science frameworks pay no attention to them, except in indicating that policymakers must take them into account in addition to the solutions or choices resulting from their own analyses. Current approaches in management science may attempt to address them by incorporating representative dimensions in a multi-dimensional objective function approach. Alternatively, by examining both decision processes in science policy and studies such as the ones given above with respect to risk-related beliefs, these broad social concerns may be more appropriately integrated into a framework for descriptive analyses of how these kinds of issues are resolved in practice.

- *Science policy questions are basically "what if" questions rather than those suited to optimization frameworks.*

Earlier chapters presented many reasons why science management has resisted the traditional management science frameworks of optimality. In both research management and science policy formulation, substantial focus is on the design and functioning of the relevant systems, and on the anticipated effects on system performance of changes in resource input, interactions, knowledge gains, and demands for contributions to non-science objectives. These are basically "what-if" questions, and usually are made with respect to incremental changes, such as changes in funding, project portfolio, and estimations of probability of success for various program components. No where is the what-if nature of questions so clear as in considering alternative budget levels for individual major R&D programs, or for the overall R&D effort. Shapley et.al. (1977, p.94) list the following general questions associated with U.S. federal budgeting for R&D:

> If the R&D budget were at one level rather than another, or oriented toward certain non-science objectives rather than others, *what would be the implications for:*
>
> (1) the goals and objectives of the R&D programs?
>
> (2) the goals and objectives the R&D program is intended to serve?
>
> (3) the achievement of science and technology?
>
> (4) the national economy?
>
> (5) the position of the country in the world vis a vis national security, competitive position in world trade, international cooperation in science, and international competition in science?
>
> (6) the institutions that perform R&D including universities, national laboratories and other government R&D installations, contract research centers and nonprofit institutions, and industry R&D?
>
> (7) the future capabilities of the country in science and technology?

In this what-if framework, two characteristics of the decisionmaking process emerge as critical. First, the decisionmaking system for budget setting in practice is based on incrementalism and the cumulative history and consequences of previous incremental decisions, rather than on any overall planned budget configuration. This does not mean that planned incremental changes (5-year and 10-year plans) are not evident; only that de novo plans are not. Secondly, advocacy rather than rationality is the dominant decision process in practice. Arguments are based on answers to these what-if questions simply because there exists no way of rationally deducing any single best budget configuration from scratch.

Furthermore, the influences of these argumentations on decisions made (budget allocations) are documented in the Congressional Record, in committee hearings, and in committee reports. This is important in at least two ways. First, the science management context may provide guidance to management scientists of crucial what-if questions about system behavior and performance. The empirical record in science may show, for example, relations between contextual conditions and the prevailing balance between concern for system resilience, sustainability, or stability, and concern for meeting relatively short-term or medium-term performance objectives. Or, the record may show that in certain conditions, what-if questions about system structure (e.g. what if the government were to encourage a more active role by industry in R&D through tax incentives?) may dominate argumentation rather than questions about the annual setting of system parameters (e.g. what if the federal budget for R&D were increased/decreased by a certain amount?). Secondly, current trends in management employment of decision support systems and structured information systems aimed at providing capability for what-if analysis represent a focusing on *improved decision inputs* and not on a descriptive or prescriptive decision process. How the improved decision inputs are used in the making of decisions is still a matter of interest to management scientists. The documented cases of budget setting and other decisions in science management (siting of hazardous facilities, use of nuclear energy, setting of standards and enactment of regulations) may provide both insights and an empirical base for development of new models of decisionmaking incorporating the incremental and advocacy aspects of argumentation based on what-if analyses.

CHAPTER 9

SYSTEMS FOR WHICH ANALYTICAL SOLUTIONS ARE NOT POSSIBLE

Traditional management science approaches employ frameworks for which solutions can be analytically derived (e.g. the linear programming framework has a solution found by the simplex method; maximization frameworks may be solved using principles of calculus). More contemporary management science applications have found these frameworks for which solutions can be analytically derived deficient in being able to capture the richness of problem characteristics. As a result, current management science is increasingly employing systems formulations, "soft system" modeling, and heuristic-based models. Actually, these are all quite different developments, though one is tempted to lump them all together into a general loosening of rigor in favor of apparent relevance to realities. Such interpretation is quite incorrect, and this brief section will attempt to sort out what these developments are.

Simulated Systems Behavior

The shift from a predominant reliance on analytically derived solutions to the investigation of systems for which there are no "solutions" but only system behaviors and multiple measures of performance is perhaps the most fundamental shift. In the typology of management science approaches of Table 1, this reflects an increased concentration of the use of simulation models to understand system dynamics and to test sensitivity to changes, as opposed to finding optimal solutions.

This systems orientation represents recognition of the realities of complexity in real problems, including the importance of side effects, interactions among system components, interest in multiple performance criteria, and effects of exogenous influences and unanticipated shocks. Simulation models do not necessarily imply any softening of science or explicit introductions of uncertainties. Environmental systems, ecological systems, climatic systems, and materials flow systems may be modeled with the same scientific rigor as found in the physical sciences elsewhere.

However, related to this shift in focus toward systems analysis are methodological adjustments reflecting the soft character of many of the systems addressed. These adjustments represent a qualitative change in the reliability and verifiability with which representational systems can be developed. This does not necessarily follow from the introduction of the vagaries of human behavior, but from problems of data, theory, and experimentation, found in physical as well as social systems. Beck (1981,p.234) defines "hard systems" as those in which "experiments can easily be conducted to identify the behavior of such systems, and a priori theory is capable of predicting accurately what the nature of that behavior should be." In contrast, "soft systems" are ones for which "a priori theory is strongly colored by the opinions of the analyst, existing theory is unlikely to lead to accurate prediction of future behavior, and planned experiments with the system are particularly difficult, if not impossible, to implement." Though Beck was concerned with physical, environmental systems (water quality - ecological systems), his characteristics of soft systems certainly apply to economic systems, energy demand systems, food and agricultural systems, regional development systems, health care systems, urban dynamics systems, and others. Methodological adjustments to soft systems include incorporation of uncertainty, attention to model structure design (model calibration becomes not simply a problem of parameter estimation based on field data; data must first be used in calibrating system structure), and consideration of alternative theories or model frameworks to represent system behavior (e.g. in economic systems one can use either input/output analysis or econometric frameworks).

The third related development began with the introduction of behavioral science/political science variables and postulated relationships into management science formulations representing the soft social systems of business and government. "Models turned softer as a higher percentage of effects were attributed to behavioral factors" (Starr 1974,p.78). Additionally, models based on heuristics (rules of thumb) rather than tested scientific findings, and adjusted by Bayesian statistical treatments allowing management participation in the setting of probability estimates, became commonplace. Increasingly, management science frameworks focused on decision situations in which behavioral, attitudinal, and interorganizational interactive elements were addressed, though crudely. "Thus, over time management science has become less similar to physical sciences roles and more similar to social science" (Starr 1974,p.86). Still, the modeling of behavioral and political processes leaves much to be desired.

Two critical questions are often encountered in this second shift in management science toward soft systems modeling, especially for systems involving human behavior and attitudes, perceptions, and varying degrees of knowledge in the making of decisions:

(a) What empirical bases are there for developing *descriptive models of decision behavior under uncertainty*? What heuristics or decision procedures are used in different situations? What are the consequences for system performance?

(b) What kinds of models can be developed to represent *systems of progressingly decreasing uncertainty* (learning systems)?

Relevant Science Management Examples

Here again, science management processes provide a rich source of behaviors of individuals and systems for use in developing improved model frameworks. For example:

- *Science management decisionmaking incorporates a variety of behaviors in the face of uncertainty;*
- *Science management provides cases of explicit estimation of probabilities through integration of multiple subjective judgments; and*
- *Integral to science management is the implicit incorporation of Bayesian type adjustments on the basis of new information.*

All three of these characteristics identify science management as the management of exploring and learning systems in the face of uncertainty. Cancer research, for example, is abundant, but whether cures for cancers can be found is uncertain. Research for development of new methodologies or for confirming or rejecting abstract hypotheses embark in the face of uncertainty regarding whether they will be successful in accomplishing their aims. Yet behind these and most other kinds of science initiatives are a host of procedures and mechanisms through which subjective judgments of technical quality, problem relevance, and probability of success are widely solicited, compared, and revised on the basis of incomplete and often preliminary information about previous work, work-in-progress, related work elsewhere, and the general reputation of the investigators contributing to the effort. As a response to the general uncertainty inherent in science, managers of science often make use of mechanisms both for the solicitation of judgment and input from throughout the scientific community, and for group assessments and recommendations based on collective judgment as a basis for decisionmaking. These mechanisms include peer reviews and mail reviews both for input (quality assessments) and for decision recommendations (on project funding); assessment workshops; consultant meetings; delphi-type procedures for coalescing around probability estimates or likely consequences; annual reviews for updating estimates of the probability of project success based on progress-to-date and annual funding patterns to allow changes in support on the basis of those reviews; and pilot projects, multiple-path approaches, and the funding of duplicative programs to increase the chance of success somewhere among the efforts.

These science systems are purposely redundant, search oriented, collectively guided, and attuned to new information and knowledge gains from throughout disparate fields of endeavor. Though some literature exists about the nature of such learning or inquiring systems (cf.Churchman 1971), few decision models if any simulate their behavior or internal processes. Yet models of this nature may reflect more accurately the realities of decision systems in business and government than do those of traditional decision analysis. New model frameworks and analytical formulations recognizing and incorporating these features may lead to greater understanding of real decision processes and improvements in both descriptive and prescriptive analysis. Science management offers a wide variety of case examples of such systems, many of which are well documented with respect to the procedures they use.

CHAPTER 10

FROM SUBSTANTIVE RATIONALITY TO PROCEDURAL RATIONALITY, PRESCRIPTION TO DESCRIPTION, AND DEDUCTIVE REASONING TO INDUCTIVE REASONING

Fundamentals of management science listed in Chapter 1 include rationality as a basic assumption. The rationality assumed in traditional management science formulations is the rationality of utility maximization. Simon and others have indicated that experiments conducted to confirm empirically behavior according to this rationality have failed. "A fair summary of the findings of these experiments is that actual human choices depart radically from those implied by the axioms [of subjective expected utility theory] except in the simplest and most transparent situations" (Simon 1980,p.75). As a result, Simon poses a different kind of rationality, called procedural or bounded rationality, to better explain behavior observed. The difference is clarified in the following definitions. "Behavior is substantively rational when it is appropriate to the achievement of given goals within the limits imposed by given conditions and constraints." In the classical economic goal of utility maximization, there is usually only one correct substantively rational solution. In contrast, "Behavior is procedurally rational when it is the outcome of appropriate deliberation" (Simon 1976,pp.130-131). Appropriate deliberation is related to the thinking process of reasoning and recognizes "...man as an organism of limited computational ability and possessing limited information and limited imagination, seeking to survive in a world rich in complexity" (Simon 1980, p.75).

Assessing the New Trends

Real world complexities and uncertainties combine with human limitations in information processing capacity to favor a procedurally rational approach to problem solving over the search for a substantively rational one. Procedurally rational behaviors indicated by Simon include using target values to reflect satisfactory levels of goal achievement (satisficing); means-ends analysis to guide search for appropriate actions; experience to identify important problem features; and intelligence activities, buffers, and diversification strategies to reduce harmful effects of uncertainty.

Not everybody finds this trend toward procedurally rational approaches acceptable. Dobell (1980,p.15) cites Gordon's review in *The Journal of Political Economy* contrasting procedural principles of ethics (good actions emerge from proper procedures) with the consequential principle (an act is good which yields good consequences independently demonstrated to be so), and arguing that "end-state" tests for desirable outcomes are unavoidable even if it is not clear what tests are appropriate. But as Dobell remarks, "Gordon is swimming against the tide."

Closely linked with the emergence of procedural rationality as an approach increasingly used in management science is a shift away from prescription and toward description. Moreover, authors of management science literature have great difficulties in sorting out the role of descriptive analysis in the formulation of prescriptive statements, and even in the nature of prescription within a scientific approach. For example, Kickert (1980, pp.255-257) argues, "Science serves to formulate explanatory theories on empirical reality. Consequently, methodology of science is restricted to the procedure of obtaining explanatory theories. In other words, there is no explicit methodology of prescriptive science." Kickert views classical organization science as an example wherein theories do not arise out of empirical reality but are only tested against it. "From 'practical experience' with organizations, prescriptive statements are derived that should be applied to solve practical problems. In extreme form these prescriptions are given without any underlying empirical confirmation. In still more extreme form almost universal validity is moreover attributed to these statements. Apparently, this has little to do with science, not withstanding the fact that the proposed prescriptions might appear to be very practical, useful and effective."

Changes in approach from prescription to description and substantive rationality to procedural rationality are both recent and pervasive. Dobell (1980,p.14) comments, "In public administration, the literature of the sixties filled with formulae, matrices, and algorithms describing how complex decisions should be made in government, has given way to the literature of the seventies, describing how decisions are made." Part of this change is an emergence from naivete, especially in regard to the recognition that managers and government officials have goals of their own that they pursue not necessarily identical or even consistent with the professed objectives of the institutions within which they make decisions. Accordingly, Dobell characterizes this change as one from "normative optimism" to "descriptive pessimism" and refers to "... the burgeoning literature insisting that the process of policy decisions in governments is so bound up in games and bureaucratic tangles that economic rationality is a fool's dream." But if the descriptive and procedural approaches have represented a retreat from analytically derived optimal solutions as one set of guides to problem solving, they have also created other guides.

Associated with the descriptive and procedural approaches is an orientation toward inductive rather than deductive thinking. Deductive thinking is predominant in the application of postulated laws to specific instances. In the management sciences, these laws are actually the laws of calculus or computational analysis. Inductive reasoning uses available facts to develop new forms of model frameworks, or new alternatives to consider. Commenting on the change in emphasis in management science from deductive reasoning to inductive reasoning, Starr (1974, p.79) indicates, "The creative ability of the model builder is less mathematical and more architectural." Actually, this shift in orientation may be the most fruitful for management science as a practical aid to decisionmaking. Increasing the ability or even the tendency to think up new alternatives may be a far more important practical contribution than increasing the ability to find a "best" alternative among a given set.

Further improvements in descriptive decision models, in understanding rational procedures and their impacts, and in creating new model frameworks may build on answers to the following critical questions:

(a) *How do decisionmaking procedures evolve*? How do constraints and their systemic natures affect their development and performance?

(b) What *mechanisms are used for new alternative generation*? Can these mechanisms be modeled?

In these regards, the study of science management has much to offer.

Potential Case Studies in Science Management

- *Science management offers a variety of decisionmaking procedures in a variety of settings for investigating both general and specific features of decision systems and their relevance to other non-science contexts.*

- *Often in contexts of science management are discernible histories of the evolution of the decisionmaking systems and of the conditions shaping those evolutions.*

Perhaps the greatest aid to understanding how a management or decisionmaking system works or why it is structured as it is and employs the procedures that it does, is knowing about the history of challenges, responses, and adaptations occurring during its development. Decision systems, especially those in large organizations, rarely spring full blown and stable (unless they are direct imitations of systems elsewhere) and usually carry with them procedures and heuristics which were adopted over the years in response to crises and criticisms particular to the organization. Most of the management science theory focused on describing the structure and functioning of decision systems (Cyert and March 1963, March and Simon 1964, Lindblom 1959) tend to take a snap-shot approach of the management system and observe its simulated behavior in response to environmental inputs over time. Some theorists provide hypotheses of how certain subsystems or heuristics develop in decision systems (Thompson 1967) but do not attempt to simulate an evolution of the whole decision system structure.[6]

Models and analytical frameworks used in the field of self-organizing systems (Allen et.al.1981, Haken 1978, and Beer 1966) may be applicable in modeling the evolution of complex management systems, though no one has yet made the attempt to do so. Such exercises may help understand why decision procedures and common targets of attention have developed as they have. Certainly, a basic requirement for the modeling of decision system evolution is a rich base of empirical data encompassing both a wide variety of decision systems and a wide variety of environments associated with their respective histories. Secondly, these data must be accessible; and thirdly, the systems themselves should be relatively simple or discernible so that modeling efforts can focus more easily on alternative perspectives and frameworks without getting mired in complexity. In these regards, science management offers many case history evolutions of research management systems (Table 13).

- *Science management offers a wide range of strategies and procedures for developing new alternatives as well as for choosing among them.*

Traditional management science frameworks have focused on ways for selecting among alternative choices (narrowing the options to be considered by decisionmakers); while current trends in management science seem to be oriented in part toward gaining understanding of system behavior in order to develop new or improved alternatives (widening the scope of perceived options). Accordingly, better understanding of mechanisms used for generating new alternatives through analysis or through search and scan procedures is needed to understand their operation and influence in decision systems. Some literature in management science already exists in this regard. For example, Thietart and Vivas (1981) describe the use of sales personnel for obtaining strategic information on market changes and the activities of competing organizations as input to research and new product development functions; and Wilensky (1967) describes a variety of similar organizational intelligence mechanisms.

[6]Development hypotheses posed by Thompson (1967) include the following: Organizations: (1) "seek to buffer environmental influences by surrounding their technical core with input and output components" (p.20); (2) "seek to smooth out input and output transactions" (p.21); (3) "seek to anticipate and adapt to environmental changes which cannot be buffered or leveled" (p.21); (4) "The organization facing many constraints and unable to achieve power in other sectors of its task environment will seek to enlarge the task environment" (p.37); (5) "Organizations with capacity in excess of what the task environment supports will seek to enlarge their domain" (p.46); (6) "The organization component facing a stable task environment will rely on rules to achieve its adaptation to that environment" (p.71); and (7) "When the range of task-environment variations is large or unpredictable, the responsible organizational component must achieve the necessary adaptation by monitoring that environment and planning responses, and this calls for localized units" (p.72).

Table 13

Samples Histories of the Evolution
of Research Management Systems

Rand Corporation

Smith (1966) and Dickson (1971) provide detailed accounts of the growth and development phases of Rand in its organizational structure, management team structure, relation to sponsors, and patterns of diversification in disciplines, areas of expertise, and sources of income.

Research Services of the U.S. Department of Agriculture (USDA)

Numerous fascinating and published accounts exist of the four-step evolution of attempts to manage research utilization by the USDA.[a] This history of trail-and-error appears to be repeated in fields of national health and education as well.

U.S. National Institute of Education (NIE)

A recent and compelling account of the initial years of the NIE (Sproull et.al.1976) describes the evolution of tactics, responses to external demands and changes, and attempts at rational decisionmaking by the early research management teams, especially with regard to the management of planning.

[a]In brief, shortly after its inception in 1862, the USDA constructed Agricultural Extension Stations throughout the country. These stations were to be responsible to the needs and questions of farmers and were to promote the implementation of achievements from federally supported agricultural research. In practice, farmers simply did not interact with the stations as anticipated. Party, this was because they did not trust the federal experts; and partly they were not willing to risk the use of their land on alleged improvements in seed or farming methods without proof of payoff. In any case, few major changes in the practice of agriculture resulted from the institution of experiment stations in the 1880's. The next step of USDA was to adopt a strategy of sending people from the stations out to the farmers. Dissemination was still treated separately from R&D performance in research labs, but was now a main focus of USDA management. Station personnel traveled throughout the country in whistle-stop tours spreading the word on research findings and new farming methods developed in the labs. The problem here was that farmers were unable to understand the experts of to translate what they had to say into meaningful changes for themselves. Also, they were still reluctant to believe that results produced in the labs would also work on their own lands. USDA again responded with a totally new approach, but one which again treated utilization as a function managed separately. The new method of dissemination was based on a flood of pamphlets and practitioner guides on how farmers could apply the results of R&D to their own farms. Additionally, agricultural libraries and outposts were built to help distribution in massive programs of pamphlets, brochures, and guidebooks. Again, few major changes in the agricultural practice occurred as a result. USDA's most recent and successful evolutionary step involves the use of extension agents which couple utilization with R&D performance. Extension agents work among agricultural researchers, and county agents work among farmers and with extension agents to mobilize research efforts to specific farmer needs and support experimentation directly on farmers' lands.

Table 13 (cont.)

International Institute for Applied Systems Analysis (IIASA) and the East- West Center (EWC)

These are two academic, international, interdisciplinary, and problem-oriented institutes. The first focuses on international scientific collaboration between Soviet-bloc nations and western nations, and the second focuses on scientific cooperation between the U.S. and the nations of Asia and the Pacific. Together they provide a wide range of (some) published and (mostly) internal documents on the evolution of their respective research management systems. Moreover, the many similarities and differences of the two organizations help identify the advantages and disadvantages of their respective management systems as they have evolved.

Bell Labs[b]

Reich (1980) presents a concise account of the early years of American Bell Telephone Company research based on a defensive strategy of patent coverage and achievement of long-distance telephony to protect a monopoly position in telephone service (against competitors and radio transmission approaches); then a strategy of expansion of research into non-telephonic fields to promote technological innovations and leading to the incorporation of a separate Bell Labs. The influence of specific research managers and of corporate objectives for research is emphasized.

U.S. National Institutes of Health (NIH); National Cancer Institute (NCI); Office of Naval Research (ONR); Cooperative State Research Service of the U.S. Department of Agriculture (USDA); and other federal research agencies

Brief histories of the evolution of many U.S. federal research agencies and the research management systems they employ are found in Wirt et.al. (1976). Each case also has extensive literatures on their individual histories and management procedures.[c]

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[b]Histories of other industrial research labs are also published, for example see Kendall Bier's Pioneering in Industrial Research: The Story of the General Electric Research Laboratory (Washington D.C. 1957).

[c]Wirt, Lieberman, and Levien (1976) actually cover nine U.S. federal research agencies in their comparative study, including the National Science Foundation, the National Institute of Mental Health (NIMH); the Office of Economic Opportunity (OEO); the NASA/Goddard Space Flight Center, and the U.S. Air Force. Brief historical material concerning the development of the research management systems used, however, is included only for some of these.

Science management uses these and a variety of other mechanisms for search and scanning of the environment. In non-profit R&D organizations, decision systems make extensive use of intelligence mechanisms for finding out what research ideas and progress is being make throughout the relevant scientific community, and for incorporating these ideas into their own programs. These mechanisms include calls for papers; science conferences; visits and short-term assignments of scientific staff to other institutions (within the scientific community, in government, in business, or in sponsoring organizations); professional journals, newsletters, and annual meetings; advisory committees, liaison committees, and science boards; external reviews of draft publications; joint sponsorship of meetings and workshops; and direct collaboration with other institutions in a variety of manners. Profit oriented industrial R&D organizations make use of many of the same mechanisms, plus formal and informal interactions with marketing, production, and planning departments in their host organizations.

This tremendous amount of input sources for decisionmaking on program direction, project selection, staff hiring, and research focus provides a constant stream of potential alternatives for research managers to consider. Some of these mechanisms reflect problem-initiated search as discussed by Cyert and March (1963,pp.120-122); and others reflect the mixed-scanning approach of Etzioni mentioned earlier. Responses of decision systems to a strong flow of alternatives has not been the focus of much analysis in modeling. In traditional frameworks, either alternatives are fixed and given at the time of decisionmaking, or decisionmaking is purposely postponed while search for new alternatives is initiated. The exceptional work to the contrary is that of Cohen,March, and Olsen (1972) in which streams of problems and solutions interact in a rather random way. New model frameworks are needed incorporating this feature of streams of input occurring both independently of and as a response to decision input needs, and the responses of decision systems to them. The many visible cases of such input streams in science management, as indicated above, may provide a basis for beginning these modeling efforts.

CHAPTER 11

FOCUSING ON LARGER AND MORE COMPLEX INSTITUTIONAL CONTEXTS

Applications of management science approaches to larger and more complex institutional contexts are evident throughout both management science literature and new literatures pertaining to planning and problem solving within particular institutional contexts. For example, urban planning literature now includes an extensive body of management science approaches and case applications in such areas as design of transportation networks, legislative redistricting, location of public facilities, urban center planning, and establishment of garbage collection routes (cf. Ignazio and Gupta 1975, p.9 and Mesarovic and Reisman 1972). In many respects, these applications are similar to earlier ones of inventory control and production scheduling for specific business managers or organizational units in that they generally assume the framework of a single decisionmaking entity maximizing some utility function or multi-attribute objective function. At the sub-national regional level, management science analyses are more likely to recognize the conflicts among decisionmaking entities (e.g. between cities and rural areas; among municipalities sharing a common facility yet benefiting unequally; and among economic sectors within a region) and reformulate the structure of analysis to clarify value differences and examine methods to deal with them in ways other than multiple-criteria decision frameworks.[7]

[7]Young et.al.(1980) address the regional-level problem of how to determine a "fair" or "just" allocation of cost for a common water resource such as a multipurpose reservoir among participating municipalities which vary in benefit from the resource, ability to pay, etc. They

National and International Models

System models and simulations at the national level are fairly common today, though usually restricted to specific aspects. National models of the economy are probably most common and are used to examine the consequences of trends or alternative governmental economic policies on the simulated national economy. National models of energy demand and supply have also become widespread in response to concerns over foreign dependence on oil. National models of food and agriculture sectors, forest industry sectors, and others are also emerging. In these national models, interactions among sectors and between nations is usually addressed in forms of export/imports, movements of resources, and transfers of capital or finished goods, though without incorporating notions of overt competition or cooperative behavior among disaggregated decisionmaking units, other than that implied by normal market mechanisms (in Western based models) and by the meeting of planned levels of production (in Eastern based planned economy models). Here again, the purpose of the models is to test potential government policies against a simulated national system, or to examine the possible effects of major shocks or discontinuities as in another abrupt rise in oil prices, a major drought, or a critical loss or gain (through innovation or discovery) of capacity in production.

In a few models of national systems, interactions between the economic sector and the political sector are emphasized (cf. Frey 1978 and Ward and Cusack 1981). In these politico-economic models, a driving force of model behavior is the desire of the government to remain in power by maintaining a high popularity which in turn is a function of economic variables such as inflation, unemployment, and growth in disposable income. Economic conditions drive government policies which effect economic conditions in a feedback loop constrained basically by the inability of government to produce economic advances to match rising public expectations thereby producing reversals in government popularity. These models are not simply fanciful or purely speculative; they are structured on "empirically based behavior equations" relating popularity as judged by public opinion polls and voting records with economic variables such as growth in real disposable income and changes in the rates of inflation and unemployment; and on changes in government policies such as changes in spending levels and in tax rates, as functions of current popularity measured by polls and election returns (policies also depend on exogenously introduced government ideology such as expansionary or restrictive). Modelers of the GLOBUS model (Ward and Cusack 1981) include a domestic political-economic interactive system such as the one described above and claim good success in historically predicting support for the government as a function of gaps in public expectations and actual performance along macroeconomic indicators.

compare seven different approaches in the project evaluation and game theory literature, in terms of both advantages and disadvantages of each approach. For example, one method they discuss is the separable costs - remaining benefits (SCRB) method actually used in the U.S. and other countries for multipurpose reservoir projects. This method allocates certain costs directly to each participating member unit, and then divides the remainder on the basis of some single numerical criterion such as use, population served, or level of benefit.

Modeling at the international, or global, levels has also developed rapidly in recent years. Table 14, taken from Guetzkow and Valadez (1981, Table 9.1) lists a subset of these models, selected to indicate that at the international level, a focus on the interaction among separate decisionmaking entities (nations) again surfaces in many, but not all, model frameworks. At the international level, modeled conflict often arises out of competition for scarce resources and may escalate in action and reaction spirals. Here again, the models involve feedback loops and empirically based relations among variables. For example, the GLOBUS model recognizes that international relations between any two countries are composed of a mixture of both competitive behaviors at different levels (diplomatic conflicts involving disagreements on issues; serious disputes involving threat of force; and use of force) and cooperative behaviors also at different levels (normal cooperative actions in exchanges and joint efforts; coalition behaviors; alliance behaviors; and foreign assistance). Empirical data is abundant on all these levels and may be linked to both domestic economic and political conditions and to situations of international competition for scarce resources.

Additionally, global or international models based on linked systems of national models (recognizing as in the regional level example earlier the disaggregation of decisionmaking units) may help clarify the nature of pervasive global problems such as famine in certain parts of the world as a consequence of system interactions regarding world markets and not easily solvable through classical government actions.[9]

Currently, management science approaches to the multi-actor decision situations inherent in large and complex institutional contexts is basically limited to game-theoretic formulations and to multi-attribute decision theory reoriented to fit a multi-actor framework (representing multiple interests or values). Additionally, students of negotiation practices and principles are making significant contributions to multi-actor decisionmaking, though with little attention to the role or use of analysis. These and other developments are described below.

[9] An illustration of the systemic character of some world problems is given in a recent test run on the IIASA linked system of national food and agricultural models. It was previously estimated that the undernourishment of the world's 460 million people in this condition could be greatly reduced with the addition of only 30 million tons of wheat per year delivered to those suffering malnutrition. In the test run, this amount was simply added to world markets at no costs or production capacity utilization. Within a relatively brief period of simulated time, the effect of the additional wheat on famine essentially disappeared, as price adjustments in response to the added supply depressed wheat production and prices, shifted use of wheat from human consumption to animal feedstock for increased consumption of meat by those who could afford it, and drove those originally in famine conditions back to them.

Table 14

International Interaction Models*

Model Name Year, Author	Disciplines Used	Central Concepts	General Format	Number of of Nations
Inter-Nation Simulation [INS] 1956 Guetzkow, Noel	political science, and social psychology	war games, international politics	person-computer game	5 to 9 nations
International Processes Simulation [IPS] 1967 Smoker	political science	national and international politics, economics, military science	person-computer game	6 nations
Project Link [LINK] 1968 Klein and Hickman	economics	national and international economics, economic growth and stability	all computer econometric model	2 to 10 nations per region, 25 regions
Simulated International Processor [SIPER] 1969 Bremer	political science, economics	goal and budget based decisionmaking; national and international political (arms races) and economic (trace and aid) processes	all computer model	5 to 25 nations
Nations-In-Conflict [NIC] 1972 Choucrit and North	diplomatic history; political science, economics	military conflict and violence behaviors, alliances and other interactions	econometric model	6 nations

*Based on Table 9.1, pp.334-336 in Guetzkow and Valdez (1981) and limited to models of multi-actor (nations) interactions.

Table 14 (cont.)

Model Name Year, Author	Disciplines Used	Central Concepts	General Format	Number of of Nations
Simulation Model of Political, Economic, and Strategic Interactions [SIMPEST] 1974 Luterbaches, Allen & Imhaff	diplomatic history, political science, calculus	political, economic, strategic interactions among superpowers	all computer model	7 actors
World Politics Simulations [WPS] 1975 Bennett and Alker	diplomatic history, political science, artificial intelligence	goals, strategies, & foreign policy outputs in cybernetic configurations	all computer cybernetic model	10 nations
Generating Long-Term Options by Using Simulation [GLOBUS] 1980 Bremer et.al.*	political science (including international politics), economics, sociology	government decision-making per domestic/ international political (including military) and economic (including trade and monetary) processes of cooperation and conflict	all computer model	25 nations

*The description of GLOBUS is not found in Guetzkow and Valdez (1981) Table 9.1 but is taken from a separate unpublished addendum provided by Guetzkow and dated April 1981.

Multi- Attribute Approaches to Multi- Actor Decisionmaking

Whether multi-attribute decision theory can be used effectively in either descriptive or prescriptive analyses of multi-actor decisionmaking is still in contention. For over two decades, Arrow's (1951) famous "impossibility theorem" diverted efforts away from attempts to construct aggregate preference orderings or social welfare functions by combining individual differences in utilities. More recently, Edwards (1977) suggests a new role for multi-attribute utility theory in cases of multi-actor decision situations, focusing on making explicit the differences in values held by the different decision participants.[9] This approach does not lead to a particular solution, but prescribes a process in which information on differing underlying values are used as the basis for finding compromises or alternative solutions. Peschel and Riedel (1977, pp.111-113) suggest an alternative method in which two groups' or individuals' criterion vectors are coordinated by a third party in an hierarchical system organized to prevent contradictions among aims. Uses of third parties as arbitrators or mediators is again discussed later under negotiation approaches. A third model by Bauer and Wegener (1977, pp.362-363) combines objective functions according to the principle of balancing the trade-off ratio between each criterion with the ratio of values of decisionmakers on each criterion.

None of these models represent an adequate basis for descriptive analysis (since none are supported by evidence that group decisions are actually made in these manners); nor for prescriptive analysis (for choice, though Edward's model may help in formulating new options). In effect, they are all perturbations of single decisionmaker frameworks rather than formulations based specifically on group decision processes or analyses.[10]

Process Approaches to Multi- Actor Decisionmaking

A second related approach to multiple decisionmaker frameworks particularly for descriptive analyses parallels the "organizational process" approach of Allison described earlier. In it, the first step is the identification of participants in the decision process, often including (in policy cases) government agencies, local citizen groups, interest groups, and various factions within each of these. A second step is the identification of the concerns, values, and objectives of each decision participant group and an understanding of the way each group senses, interprets, and processes information relating to the decision. Thirdly, analysts develop a

[9]Edwards (1977, p.249) argues, "Multiattribute utility measurement can spell out explicitly what the values of each participant (decision maker, expert, pressure group, government, and so on) are and show how and how much they differ - and, in the process, it can frequently reduce the extent of such differences. The exploitation of this technique permits regulatory or administrative agencies and other public decision-making organizations to shift their attention from specific actions to the values these actions serve and to the decision-making mechanisms that implement these values."

[10]An excellent state-of-the-art survey on these and other multi-attribute decision models and their applications is found in Hwang and Masud (1979). Major problems with models of this kind are discussed in Cobb and Thrall (1981).

framework (usually a flow diagram with feedback loops) illustrating how the course of events and the nature of conflicts and options are influenced by the dynamics of interaction among the groups, by differing interpretations of fact and external and sometimes unanticipated events, and by the development of new alternatives. Linnerooth (1980) uses this approach to describe the decision process followed in the Oxnard, California liquid energy gas site denial case. Her methodology is then generalized in Kunreuther (1980). Braybrooke (1974) suggests an even more extreme process approach in his conception of group decisionmaking according to the "issue-machine approach." In this framework, decisions evolve from distinguishable and successive "rounds," each round consisting of phases of issue-circumscribing, proposal-initiating, interacting, and decision-taking in which an authoritative decision is made or at least a report completed on some of the issues addressed in the round. The machine characterization of Braybrooke's model arises from his conceptualizing the processing of issues as would be done by computer-like programs which apply successive test questions to all policies proposed in connection with an issue addressed, and then output either adopted policies which pass all the tests or dead issues. Braybrooke's issue-machines are called station-stable if the people or groups holding certain recognized positions (as officials, or experts, or group spokesmen) operate (apply their tests) in the same sequence in each successive round; and program-stable if regardless of sequence the same reasons (test results) count with the same weight for or against a proposal that is introduced. The approach is used by Braybrooke to analyze the actual handling of proposals related to reducing traffic congestion in London within the period 1963-1968.

Process approaches such as these are inherently descriptive rather than prescriptive. They serve to highlight the importance of dynamics, changes in decision participants, effects of external events, interpretations and presentation of arguments, the sequence in which sub-issues are addressed or tests applied, and the role of varying information processing procedures used by different decision participants. In this regard, they also serve to help understand and improve decision processes both in avoiding stalemates and in identifying or developing new options.

Game Theory Approaches to Multi- Actor Decisionmaking

The literature on game theory approaches to situations involving multiple decisionmakers, especially with respect to decisions which are economic in nature, is vast, based principally on utility theory, and rarely ventures beyond a two-person situation.[11] Game theory encompasses both situations of conflict as zero-sum games, and situations of a cooperative or mixed nature as non zero-sum games. It is generally prescriptive rather than descriptive[12], and requires a powerful set of

[11] Two outstanding texts in the field are John von Neumann and Oskar Morgenstern, Theory of Games and Economic Behavior, Princeton: Princeton University Press, 1944; and Thomas C. Schelling, The Strategy of Conflict, Cambridge, Mass.: Harvard University Press, 1960.

[12] According to Young (1975, p.37), game theory frameworks are basically static models in that they focus on outcome rather than process, by being concerned with the ultimate divi-

assumptions highly unlikely to be found in the real world.[13] Even with these assumptions accepted, game theoretic models of interdependent decisionmaking are not able to arrive at determinate solutions because players may still find themselves in an "outguessing" regress (Young 1975, p.24).

Contributions from Negotiation Practice and Principles

In contrast to the outcome oriented framework of game theory, and related to the process approach described earlier, recent research in negotiations attempt to find principles which help reformulate apparent conflict situations into cooperative or mixed cooperative-competitive ones and lay the groundwork for "side-by-side" rather than advocative negotiation, or encourage joint problem-solving approaches to conflict situations.

In analyzing cases of negotiation, especially with regard to the social settings in which they are embedded, Strauss (1978) focuses on various subprocesses of negotiation including making tradeoffs, obtaining kickbacks, compromising toward the middle, and paying off debts, all influenced by the negotiation context. This context includes (1) the number of negotiators, their relative experience in negotiation, and whom they represent; (2) whether the negotiations are one-shot, repeated, sequential, serial, multiple, or linked; (3) the relative balance of power exhibited by the respective parties; (4) the respective stakes in the negotiation success; (5) the visibility of transactions during negotiations; (6) the number and complexity of the issues involved; (7) the clarity of legitimacy boundaries of those issues; and (8) the options to avoiding or discontinuing negotiation perceived as available (pp.237-238). Other analysts of negotiations have focuses less on contextual conditions and more on processes by which parties influence each other's expectations, assessments, and behavior during the search for a negotiated settlement. These include "cognitive models that purport to explain interaction in terms of the

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sion of payoffs among players rather than with the sequence of actions and reactions through which players arrive at that division. Even as prescriptive aids, game theory models have a poor record as good predictors in empirical terms, even in carefully controlled experiments.

[13] Young (1977,p.23) summarizes these as follows: "Specifically, all game-theoretic models incorporate at least the following assumptions. First, both the number of players and their identity are assumed to be fixed and known to everyone. Second, all the players are assumed to be fully rational, and each player knows that the others are rational. Third, the payoff function of each player is assumed to be fixed and known at the outset. This assumption subsumes several subsidiary points. Each player's range of alternatives (or strategies) is fixed and known. And under conditions of risk arising from natural (that is, inanimate) sources, the results of each player's expected-utility calculations are fixed and known. It is this complex of assumptions which makes it possible to specify the characteristic payoff matrixes of game-theoretic models. Forth, the formal models of game theory restrict the role of communication among the players in a highly stylized fashion. In particular, communication can never affect either the form or the content of a game's payoff matrix once it is initially established." Additionally, "...in real-world situations, bargaining often involves lumpy or indivisible goods...in which one side or ther other gains sole possession of the good" (p.393) and "...while most of the existing models deal with bargaining about a single, well-defined issue, bargaining in real world situations often involve several distinct issues at the same time" (p.394). Both situations cause severe difficulties for game theoretic frameworks.

assumption that each party's actions depend on his perception about the future results of those actions," "learning models in which it is assumed that each party's actions are largely dependent on his experience of the results of past actions by the two parties," and "reaction process models in which a party's actions are assumed to be an almost automatic response to his opponent's last action and to be based on the party's own characteristics and propensities" (Gulliver 1979,p.49).

Gulliver's own model of negotiation sidesteps unprovable assumptions and focuses instead on "how and how far negotiators tolerate and cope with unavoidable ignorance, how they assess and reassess expectations and preferences about quantifiable and nonquantifiable components, and how in practice they make some kind of choice under risk and uncertainty" (pp.46-47). His is a simultaneous two-process model composed of a cyclical process of repetitive exchange of information, its assessment, and the resulting adjustments of expectations and preferences; and a developmental process of overlapping phases toward a negotiated outcome. The latter has eight phases such as formulation of an agenda and working definitions of the issues in dispute, preliminary statements of demands and offers, and narrowing of differences. In both processes, learning and change are emphasized. Gulliver's conception of negotiation illustrates this viewpoint:

> Negotiations comprise a set of social processes leading to interdependent, joint decision-making by the negotiators through their dynamic interaction with one another. These processes involve the exchange of information (and its manipulation), which permits and compels learning by each party about his opponent, about himself, and about their common situation: that is, about their expectations, requirements, strengths, and strategies. As a result of learning, there is modification of expectations and requirements such that the negotiators may shift their demands to some point at which they can agree. Negotiators continue to exchange information and to explore possibilities so long as they consider that they may gain an outcome that is more advantageous than the status quo. Negotiations are thus a dynamic process of exploration in which change is intrinsic: changes in each party's assessment of his requirements, in his expectations of what is possible, preferable, and acceptable, and changes in his understanding of the opponent's assessments and expectations. (p. xvii)

Changes in expectations, demands, positions, and strategies are also emphasized in mediation and arbitration involving third parties. Fisher (1978) presents a working guide for practitioners of international mediation in order to "loosen up a conflict situation" or move negotiations from irreconcilable fixed positions to "new operating assumptions" about the negotiation situation itself, including its goals, processes, and nature of substantive options. All these negotiation models differ substantially from the game theory formulations of fixed moves according to an advance strategy, or the bargaining formulations of offers and counter-offers. In particular, the negotiation literature offers ideas relevant to understanding the basically cooperative joint decisionmaking that occurs most often in decision situations. Here, information is shared for the purpose of achieving joint benefits and building effective relationships. Still, human interaction in this regard, is not well understood.

Examples of Cooperative Group Decisionmaking in Science Management

Science management is usually non-authoritative and sensitive to the concerns and judgments of interested parties. It seeks consensus across many lines, and thrives usually on cooperative rather than competitive behavior. It is in this last aspect that science management may provide the greatest insights into the development of improved management science frameworks for multi-actor decision analysis.

In the decision interactions of research managers, their key scientific staff, liaison committee members, and host organization users, different perspectives certainly exist but the overall character of interaction is generally cooperative and directed to the best interests of the R&D organization. How these interactions differ from the more competitive or advocative ones found in traditional case studies of group decision processes or suggested by game-theoretic frameworks may help develop new models of cooperative decisionmaking. Examples of cooperative decisionmaking readily found in research management include situations in which new programs are selected, collaborating institutions are chosen, and research plans are formulated. In these situations, decision participants contribute their respective expertise by providing new information and offering assessments on proposals with ultimate payoff to all dependent on the overall performance of the R&D organization within the constraints and context created by their cooperative decisions. Models based on processes with these characteristics may be more accurate descriptors of actual situations found outside science management as well and may help contribute to prescriptive aids for improved joint decisionmaking in general.

CONCLUSIONS: SCIENCE MANAGEMENT CONTRIBUTIONS TOWARDS IMPROVEMENTS IN THE THEORY AND PRACTICE OF MANAGEMENT SCIENCE

The central argument in this text is that research management and science policy offer to management scientists application contexts that provide clear exposure to the inherent difficulties, ambiguities, and overly simplifying assumptions of present management science methodologies and applications, and thus may serve as a base for developing improvements in analytical frameworks. Previous chapters have focused on realities of most application contexts and have shown how science management offers an empirical base for new descriptive modeling. In this chapter, the focus is on the particular characteristics of science management and their implications for new perspectives in analyses.

The Special Nature of Science Management

Earlier chapters describe the peculiar nature of science management goals and decisionmaking realities. These characteristics pose severe problems both in management and in analysis. Some of the more difficult are summarized below:

(a) Challenges to Science Management

[1] Science Management is often characterized by decisionmaking under a wide set of ambiguous unoperational goals and with decision consequences either unmeasureable or of unknown probability.

Faced with this perplexing situation, science managers find typical management science methods of means-ends analysis, optimization, and expected value computations both impossible and naive.

[2] Expectations for science are high.

Large expenditures for science activities are justified by past experience and hopes for future technological breakthroughs, cures, and trend reversals (e.g. in reversing lagging industrial productivity or military balance of power through sophisticated weaponry). Attainment of science advances consistent with these high expectations is uncertain at best, as is what process to follow to ensure a high probability of success.

[3] Traditional management methods employing bureaucratic authority, hierarchical organizational structures, and career advancement and institutional incentives are inappropriate in most science management contexts.

Science managers seem to react to these challenges by behaving in ways not too differently from those of managers in other settings, but with some identifiable tendencies:

(b) Behaviors of Science Managers in the Face of These Challenges

[1] Science managers appear to focus their efforts not directly on institutional goal achievement so much as on general institutional development[1].

The operational focus of concern in this regard is on (a) achieving and maintaining a good institutional reputation for quality and relevant work; (b) achieving a respectable rate of production of publications or patents; (c) responding quickly to changes in the science environment and in the interests of sponsors; (d) achieving sustainability in funding, staffing, and program effort; and (e) providing an occasional major piece of evidence of contribution toward to institutional mandated goals.

[2] Science managers tend to employ defensive strategies (protecting institutional integrity and comparative advantages) rather than offensive ones (concentrating resources on major, novel programs of uncertain success).

Ensuring patent coverage, using diversified project portfolios, employing multiple methods and measures, and encouraging frequent interactions throughout the relevant scientific communities are strategies for protection from surprise and ignorance. Few science organizations bet their existence on single-line research approaches or on any one big program. The uncertainties of science prohibit it in most cases.

[1] This and subsequent statements about behavior of science managers are based on personal observations rather than on systematic collection of data.

[3] Science managers appear sensitive to a multiple time frame, focusing simultaneously on progressing through current program milestones or phases, and on retaining some current resources for opportunistic investment in unanticipated special projects.

[4] Science managers tend to create institutional mechanisms to help prepare them for an uncertain future rather than for meeting a forecasted or most probable one.

Science managers rarely forecast the future state of knowledge in a field, the specific interests of their sponsors, or the exact composition of their scientific staff beyond the next few years. Yet they must continually adapt and respond to changes in these elements, especially to the occurrence or threat of major shocks or discontinuities in them. To help them prepare for these occurrences, science managers use liaison committees, advisory boards, visits, conferences, professional meetings, and a host of other mechanisms to ensure resiliency in the face of an uncertain future.

Trends and New Problems in Science Management

In addition to the challenges above and the behaviors of science managers faced with these challenges are public concerns regarding science and its effects on society. These concerns have produced a new set of perspectives and criteria imposing further demands on science management. Among them are:

[1] Growing demands for linking science policy, technology policy, and industrial policy to other national social goals.

Science for its own sake is currently unfundable in most countries. Yet almost nothing is known about how science contributes to social and economic improvements except in hindsight and even then subject to widely diverse interpretation. Few models of interaction between science and technology and their effects on society exist, and the few notions that have surfaced (such as the theory of "spin-offs" from science projects to commercial applications, or the "trickle-down" of science benefits from large federal programs to the community of independent scientists) have been largely discredited. The belief in goal linkage is now one of the few arguments for federal support of science that remains politically compelling (another is international leadership in technology, especially for military superiority) and yet little evidence or logic supports it.

[2] Public desire to improve and expand the processes of participation and consensus in formulating science policy.

Incorporating different value perspectives, providing legitimacy to various interest groups, and ensuring public awareness of science decisions of potential risk to the populace are substantial concerns expressed in most western industrialized nations evident in court hearings and media coverage. The substance in these cases varies from nuclear energy to chemical waste disposal, atmospheric pollution, occupational hazards, depletion of the ozone layer, health effects of dietary measures, and environmental effects of acid rain. The issues are liability, regulation, citizen rights, perspectives in making trade-offs, and the proper role of government. At odds are pressures for due process and pressures against bureaucratic delay, values of technological progress and values of anti-technocracy, national needs and local preferences, and requirements or economies of scale for high technology enterprises and the involuntary risks they sometimes pose for the surrounding populace.

[3] Shifts away from economic analysis in evaluating science programs or selecting science projects (if ever used in the first place)

The growing requirement to incorporate values, systems interactions, and societal effects in evaluating science options, and the failure of cost/benefit and other economic analyses to capture these elements satisfactorily, has led to experimentation with a number of new approaches to evaluation. These include scenario writing, gaming frameworks, and process models. Unlike most prescriptive economic approaches, these new frameworks serve to aid understanding of complex situations or underlying causes rather than provide direct decision recommendations.

[4] Trends toward an expanded science system perspective including both federally supported science and privately supported science.

Particularly in the U.S., there is a strong interest in providing incentives for private industry to broaden its support of science and thus allow a decreased dependency on federal support. The apparently successful federal-private support model in Japan is portrayed often as one model incorporating this broader perspective. In eastern industrialized nations, central planning in science already includes an integrated federal and local perspective. The general trend in the direction of this increased systems perspective in science support is strong, yet models or even gross understanding of integrated science systems is severly lacking.

New Perspectives in Analytical Frameworks

The challenges, concerns, and trends in science management and the apparent responses of science managers to them offer new perspectives for analytical frameworks more indicative of realities in science management, and perhaps elsewhere, than is provided by traditional management science approaches. Table 15 summarizes some of the changes in frameworks they suggest. Together they pose a challenge and a guidepost to theory development in management science.

Table 15

New Perspectives in Analytical Frameworks
Suggested by Science Management

From frameworks oriented toward goal attainment or progress toward long-term stated objectives	To frameworks focused on developing viable, productive, and resilient systems consistent with stated objectives
From time independent, time value-discounted, or equilibrium frameworks	To dynamic and transitional (non steady-state) frameworks
From a multi-dimensional, single-decisionmaker framework	To a multi-dimensional multi-party cooperative decision framework
From frameworks of decisionmaking based on facts	To frameworks of decisionmaking incorporating values
From frameworks of analysis based on rationality and logic	To frameworks acknowledging the craft aspects of analysis

Refocus on Objectives

"The age of decision is over; the era of implementation is passing; the time to modify objectives has come." Wildavsky (1979,p.32) states the above in describing a widespread retreat from objectives prevalent in U.S. public agencies in health, crime, and education, and in response to the failures of those agencies to achieve the objectives set for them. The failures were predetermined according to Wildavsky as the objectives, while politically attractive and forged in a time of social service optimism, were beyond the capacity of government to achieve. The current institutional response Wildavsky observes is the displacement of goals from external effects on people (rehabilitation of criminals, health improvements in local populations, increases in cognitive achievement by students) to internal organizational processes (equalizing access to social services or funds spent per person, or responses given).

> Since organizations wish to be regarded as successful, they try to replace objectives whose achievement depends on variables either unknown or outside of their control with objectives than can be attained by manipulating the instruments that those groups do control. (p.38)

Science organizations follow these tendencies routinely. Rather than face the impossible task of measuring achievement of unoperational goals, science managers focus on productivity, reputation, ability to respond to external changes, and on operating in a manner consistent with their organization's unique nature or comparative advantage. Success is equated with achievements in these controllable measures. This does not make science organizations goal-free; the goals of their mandates are treated instead as constraints in that the substance and process of work must remain consistent with them.

This suggests that for descriptive modeling, the objectives to be built within a framework for analysis are those inferred from behavior rather than those taken from the institutional mandate at face value. Identification of operational objectives thus becomes a central component of decision analysis. Such a behaviorist approach may not be appealing to many, yet it reflects a definite trend in management science today towards decision aids (decision support systems) and understanding (system models, simulations, and scenarios) rather than prescriptive approaches. These are all oriented to helping managers do what they do rather than prescribing what they should do.

Systems in Transition

Most economic theory treats equilibrium conditions as basic and non-equilibrium conditions as perturbations to be reduced in a return to the steady-state. In contrast, social organizations are constantly in transition. Science organizations in particular are charged with creating change in their own state of knowledge and in accelerating the dynamics of their environment through innovation and discovery. For these situations, new frameworks for the analysis of developing or transitioning systems are needed. The literature on self-organizing systems cited earlier provides one example of systems analysis focused on transitions from one state to another and the role of perturbations and fluctuations in system evolution. As of yet, these developments have not been incorporated in management science frameworks. Most management science applications are still oriented to identifying the best or optimal future state or option. Learning how to improve in getting from one state to another may be equally beneficial.

Value-Laden, Multi-Party, Cooperative Decisionmaking

Chapter 11 described various approaches toward analytical frameworks of this type currently found in the literature. None yet provide either accurate descriptions or forecasts of behavior, or prescriptions of high credibility. Furthermore, multi-part frameworks rarely focus on cooperative rather than conflict situations, though the former may be more common in practice.

Craft Aspects of Analysis

Chapter 1 defined management science as the application of the scientific method to problems of management. The succeeding chapters illustrated the variety of ways in which the practice of the scientific method is itself a craft. Formulating the problem, selecting simplifying assumptions consistent with a framework for analysis, selecting the kinds of data to examine, and many other steps of model development, testing, and experimenting, are activities of analysis requiring knowledge, skill, and a commitment to excellence on the part of the analyst if the analysis is to be of high quality. Like other crafts, these arise from talent, training, and experience. The subsequent calculations that follow precisely defined and proven mathematical relations should not obscure the craft aspects of the preliminary steps or of the interpretation and application of results. Especially for systems involving human behavior, scientific analysis cannot expect to provide conclusions, only conjectures; not forecasts, only possible outcomes; not credible prescriptions, only aids to understanding. In the perspective of the practice of the scientific method as a craft, management science is educative, not only about the substance addressed but also about limits to understanding and the usefulness of the tools employed.

Lessons from Science Management

Three themes run throughout this text: First, that management science has not made many inroads in science management, partly for reasons related to the particular characteristics of science management. Second, that realities of problems and decisionmaking behavior in science management as well as in most problem contexts are poorly reflected in contemporary management science frameworks. And third, that by addressing the particular context of science management, management scientists may gain insights for the development of improved analytical frameworks. Additionally, the behaviors of science managers in facing their own perplexing problem situations provide valuable lessons for management scientists. These are lessons of humility, of looking beyond quantification for analytical assistance, and of providing safeguards for excellence.

Humility

[1] "First, many analyses are plainly not wanted" (or sought by decisionmakers)

[2] "Second, many analyses are failing to meet the legitimate needs of the policymakers of the broader requirement of the society."

[3] "Third, analysis can be viewed as little more than politics with an 'objective' veneer."

[4] "It is not clear that analysis 'helps.' In the really difficult policy problems, even the best analysts can arrive at contrary positions and, by implication, policy recommendations."

[5] "Most decisions are made by common sense, 'ordinary knowledge' with little formal analysis."

[6] "Analysis cannot eliminate judgments about uncertainties and values."

[7] "Poor analysis may be worse than none."

[8] "Analysis can be used and misused in the adversarial process."

However:

[9] "Analysis can help with incremental choices."

[10] "Analysis can generate creative alternatives."

[11] "Analysis can raise the level of discourse."[2]

Science managers, and some business managers according to a recent McKinsey survey of well managed U.S. business companies (Peters 1980), avoid analyzing issues to death and avoid complicated procedures for developing new ideas. Analysis may not yet be in general disrepute, but neither is it without skeptics. Historically, the expectations of management science practitioners for acceptance of their results and even for their ability to provide significant assistance has been overly high. Science managers are generally highly sensitive to the uncertainties surrounding their work and to their own limitations to guide others to effective action. Their perspective is one for management scientists to emulate.

Looking Beyond Quantification

In applying the scientific method, management scientists seem to have grasped onto quantification and the exotic mathematical manipulations possible with quantified data as the hallmark of their approach. The criticisms of Chapter 3 indicate that this heavy reliance on quantification has led management science away from salient aspects of problems and decision processes at a high cost of loss of relevancy, credibility, and usefulness. Because of the particular nature of science management, science managers have developed methods and processes of analysis and decisionmaking which rely less on quantified data and more on the pooling of knowledge and judgment from a wide variety of sources. Modeling these processes may suggest alternative strategies for management science development.

[2] Statements such as these are found throughout the policy science and management science literature. In this particular set, [1] through [4] are from deLeon (1981,pp.3-4), and [5] through [11] are from Raiffa (1981, Transparency 5).

Providing Safeguards for Excellence

Majone (1980) presents two different arguments for the need to develop standards or safeguards for analysis:

[1] "If it is no longer possible to believe in the objective validity of the conclusions of an analytic study, and if even the criteria of success of the decision it supports are ambiguous, then evaluation by results becomes meaningless, and must be replaced by such process-oriented criteria as internal consistency and professional (or even political) consensus" (p.39)

[2] "...many policy studies in fields like energy, risk assessment, or education are 'designed to influence congressional debates and to affect the climate of public opinion, not guide decisions within individual corporations.' The effectiveness of such analysis can only be measured in terms of their impact on the ongoing policy debate: their success in clarifying issues, in introducing new concepts and viewpoints, even in modifying people's perceptions of the problem. Here analysis is no longer separable from social interaction as a problem-solving device, but becomes an integral part of the process by which public issues are raised, debated, and resolved." (p.42)

Science managers rely primarily on peer review of their published works and third-party analysis of their patentable ideas as safeguards for excellence. In the latter case, the two main parties are the scientists creating the ideas and the company divisions interested in patent exploitation; third-party analysts are patent attorneys and others judging the usefulness, requirements, and novelty of the ideas. For most management science studies, however, outside or third-party reviews are rare. Greenberger (1980) indicates why:

> Modelers mostly build and run their own models: that is where the credits lie. Very few modelers run and analyze the other fellow's model in any systematic way... Modelers are synthesizers and refiners more than analyzers, particularly analyzers of other modeler's models. When possible at all, such secondary analysis is too difficult and unrewarding an activity to generate much interest. As a result, the inner workings of a policy model are seldom understood by anyone but the builders of the model (and not always by them). This is a weak foundation for gaining the reliance and trust of policymakers. (p.93)

The safeguard Greenberger proposes is the development of institutions or groups of analysts for model analysis. Their work would be directed toward "making sensitivity studies, identifying critical points, probing questionable assumptions, tracing policy conclusions, comprehending the effects of simulated policy changes, and simplifying complex models without distorting their key behavioral characteristics." Two such institutions already exist in the U.S.: The Model Assessment Group at the Massachusetts Institute of Technology and the Energy Modeling Forum at Stanford University.[3] Their work and the growing research on model

[3]See Sweeney and Weyant (1979) for a more complete description of the Energy Modeling Forum than is provided in Greenberger (1980).

validation at IIASA and elsewhere is indicative of the efforts of self-criticism, and the willingness of some management scientists to expose their analyses and models to external evaluation by peers, that may provide some safeguards for preserving excellence in the development of management science theory and practice.

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