A Contingency Model for the Design of Problem-Solving Research Programs: A Perspective on Diffusion Research

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Employing relevant research findings of the administration of research, we have developed guidelines for the effective administration of problem-solving research. Using the problem of the diffusion of medical technology as a case in point we sought to identify relevant steps in the research process. The initial step is to provide a bridge between practical and theoretical concerns. For example, the increasing federal role in health care delivery and the public demand for improvement of health care have led to the question of how social science may assist in the diffusion of medical technology and, therefore, improve further the quality of health care delivery. Only recently, however, has social science research in the diffusion of technology area begun to provide some information on potential inter- and intra-organizational factors that affect diffusion and that may facilitate the "reasoned" implementation of social policy. One way to view the lack of theoretical information relevant to policy issues is by understanding the nature of the problem-solving process. In this regard, a six-stage classification scheme for evaluating research sophistication and problem-solving capability was proposed.

This led to a conclusion that social science in general and diffusion research in particular were in a "pre-paradigm" stage of development and raised the question of what could be done to improve the quality of future research. The principal factors considered were internal and external evaluation criteria, disciplinary versus interdisciplinary research, types of institutional settings, and types of funding patterns. Given the constraint of limited knowledge in this area, it was suggested tentatively that institutes with an emphasis on interdisciplinary and group research and funded through contracts may be more appropriate for the further development of applied or mission-oriented research while continuation of individual research projects characteristic of university settings and funded through grants may be more appropriate for the development of discipline-centered research.

A contingency model for research administration was proposed to suggest more specific ways in which the effectiveness of problem solving in diffusion research could be improved. The concepts of urgency (the social need for rapid, applicable research results) and predictability (the extent to which researchers can predetermine the steps needed to reach their objectives) were used to develop administrative guidelines as well as predict the probability of tangential research, the emergence of anomalies, the probable sources of conflict, and the personality attributes required by researchers in different research settings.

An underlying assumption of this paper is that social science can and should play an important role in regard to social problems. Yet there is reason to question the effectiveness of such research. All too often social research in general, and health-related social research in particular, has reflected the whims and vagaries of social fad, social pressure, and changing support patterns. This has often resulted (as we shall show in regard to studies of the diffusion of medical technology) in research findings which are noncumulative, conceptually noncomparable, and of questionable relevance to social policy.

We feel that the relevance of social research to policy decisions would be increased if long-range systematic research programs aimed at specific social problem areas, such as the delivery of health care, were developed. Critical to the success of such programs is the fostering of cumulative research accomplishment. This objective, we believe, would be furthered if policy administrators and social scientists were encouraged to discuss science policy and administration not solely in terms of immediate concerns, but also from the perspective of the growing body of research on science administration policy.

The opportunity to implement this approach was created when the National Institutes of Health (NIH) asked us to plan a conference directed toward the development of policy in regard to research on the diffusion of medical technology. The conference was held in September 1972, at Cornell University. The eighty participants included government officials, physicians, and social scientists.

This paper follows our approach to the conference in that we first examine diffusion research from the perspective of policy imperatives and then present the issues and problems of medical diffusion research, assessing accomplishments to date, and outlining future objectives.

In the second part of the paper we review work on the general question of the administration of research which can facilitate the achievement of these objectives. In the third part we develop a contingency model for research administration and relate that model to diffusion research and the objectives of the conference.

Policy Issues in Studying the Diffusion of Medical Technology

The interest in examining diffusion of technology represents a widening of federal concern with the health system. Historically, the federal government has stressed the importance of establishing a scientific base for improving medical care. Consequently, a large share of federal health funds has been allocated to the development of medical technology and the support of research- and training-based institutions.

In recent years, however, the public has demanded a greater federal role, and, as a result, Congress has passed legislation to promote increased governmental intervention in health care delivery. This concern was summed up by a government participant at the conference (Tilson and Carrigan, 1972:2):

> The ever-enlarging Federal role, and the powerful public demand that this role be exercised to improve health care for all of the people, raises the question of whether conventional wisdom should continue to be the sole guide in designing Federal programs to intervene in the health system or whether systemic study of the diffusion of medical innovations might yield knowledge that could be used to make Federal health programs more effective.

The trend toward a greater federal role in health care is reflected in the development of the National Institutes of Health. In 1945, for example, the NIH annual budget was approximately $3 million; by 1972, it has reached $1.5 billion (Tilson and Carrigan, 1972:9). The growth of NIH is largely attributable to its broad mandate: it is responsible for developing new medical technology, and it shares responsibility with other federal agencies for facilitating the application of this technology. The Institutes' expanding role and their success in developing viable medical technologies have led to a concern within NIH with optimal diffusion and use patterns for the technologies already developed, as well as for future technologies. It was recognized that, if facilitating activities were to be planned, knowledge regarding diffusion in general and, more specifically, the manner in which technology is diffused among the organizations and individuals in the health care delivery system, is necessary.

The Conference on Diffusion had as its mandates to develop guidelines for a research thrust which would provide this information and also to provide the basis for a judgment as to whether such a thrust is warranted. This meant multiple concerns by participants with substantive questions and potential research strategies.

While our immediate task related to research on the diffusion of medical technology, our broad concerns and the programmatic design problems we encountered have implications for other problem-oriented social science research programs. In particular, since the bulk of the illustrations and supporting data relate to health research, we feel that our discussion bears directly upon many social problem areas and the delivery of care.

During the last four decades, interest in the development and use of innovations in medical technology has increased, primarily because medical science has been successful in solving problems, delineating new problems to explore, and discovering new areas where technology can be applied. These successes have led to the allocation of large sums of money and other resources for research on medical technology. Commenting on the need for such resources, Weinberg (1967:101) has said:

> We are, or ought to be, entering an age of biomedical science and biomedical technology that could rival in magnitude the richness of the present age of physical science and physical technology . . . of all the bases for claiming large-scale public support for a scientific activity, the possibility of alleviating human disease through such activity is obviously one of the most compelling.

The extent of funding for these research efforts is currently being questioned. One reason for questioning current and projected support levels is an uncertainty about the social effectiveness of biomedical research. This uncertainty is one of the factors leading to a concern to learn more about the diffusion of medical technology. But the question of the diffusion of technology transcends funding concerns. Given that the ultimate aim of medical research and development is improved medical care, evaluating technology's impact on care is, in and of itself, a central concern. If medical technology is presumed to be generally beneficial, access to the technology is one important way to improve the quality of care. The diffusion problem may be seen as the gap between the number of users who have adopted desirable technological innovations and the universe of appropriate users. Conversely, the problem may be seen in terms of premature acceptance, or the number of users who have adopted undesirable innovations. Both views imply the need for a high degree of quality control over the adoption of technologies, and stress the relationship between diffusion and the quality of medical care.

Research Problems

In any research area definitions pose a problem. What is meant by diffusion, innovation, technology, and quality of care? These are value-laden terms whose connotations are complex and diverse. Such terms are never completely adequate. The following definitions are offered as sensitizing rather than definitive.

Diffusion is the dispersion, or rate of dispersion, of ideas, techniques, practices, knowledge, information, and products to adopters at various distances from the innovation's point of origin. The literature defines *technology* in many different ways, but two elements are common to all the definitions: physical techniques, procedures, or programs to achieve desired goals; and a broad knowledge or skill base to achieve the goals. Medical technology which results from basic and applied biomedical research is a scientific body of knowledge underlying the techniques, procedures, or programs needed for effective medical diagnosis, therapy, or prevention. *Innovation* is a significant technological change. *Quality of care* refers to the continued upgrading of health facilities, the increased training of health professionals and paraprofessionals, and the coordination of health services to improve medical care. These definitions, in turn, create an awareness of several interrelated questions:

1. How do we select criteria for deciding whether a specific technological item should be diffused?
2. How much research evidence do we have on the types of diffusion patterns and the factors which cause them?
3. How can we identify the institutional and individual factors that lead to optimal diffusion?

The first problem, selection of criteria, is a matter of critical judgment, because there is neither an appropriate model nor a competitive marketplace to help us decide whether a specific item should be diffused. Given this situation, can a functional decision-making model be developed? Is it possible to evaluate current innovations, to reassess earlier innovations that are at various diffusion stages, and to examine the basic decision-making criteria on which the diffusion of these innovations were based? To be effective, the criteria must take into account judgments of experts in many fields, and must also be based on innovation characteristics that apply to many innovations, so that effectiveness can be measured, and diffusion rates can be accurately predicted, for a wide variety of innovations.

The second problem involves examining the currently available information on factors that affect diffusion patterns and adoption rates in medical organizations. Research is available on: (1) environmental pressures which encourage organizations to adopt innovations—

Rogers and Shoemaker (1971), for example, report on the effects of legislation and persuasion, and Thompson (1967) on power-dependence relationships with the task environment; (2) factors which determine adoption rates in different organizations—Aiken and Hage (1968) have studied the relationship between organizational diversity, resources, and diffusion, and Gordon et al. (1974) have approached the diffusion problem in terms of the locus of decision making in organizational structure; (3) innovation characteristics which facilitate rapid adoption—Thio (1971) has reported on the relationship between the characteristics of innovations and diffusion patterns, and Fliegel and Kivlin (1966) examined diffusion patterns in terms of the economic factors associated with innovation. Though one can point to other studies which contribute to our knowledge base, the research on factors that affect diffusion patterns in medical organizations is sparse. Not only do we need more knowledge, but that which we already have must be coordinated before viable intervention strategies can be developed to improve diffusion patterns.

A third problem is how to identify the institutional and individual factors that can lead to optimal diffusion. Given Perrow's (1965) suggestion that hospitals, as organizational structures, are dependent on medical technology, it is possible to use technology as a reference point and then determine the organizational structures and resource characteristics necessary for its effective use. Organ transplant technology, for example, is impossible in the absence of organizational structures that allow coordination of skilled personnel, necessary equipment, and operating rooms. Scarcity of specialized manpower, limited funds for technology, and lack of organizational support are assumed to limit diffusion. However, we have very little hard knowledge about the impact that each kind of scarcity (separately and in conjunction with one another) has on diffusion of technology.

The potential impact of organizational and individual factors can be seen in the relationship between practitioners and administrators within a hospital. Hall (1968:92–104) in his study of professions, found that problems resulted from different views of authority and autonomy held by professionals and administrators. These problems are exacerbated when, as in the case of medicine, the profession has a history of self-administration. For example, the AMA has played an important role in evaluating and certifying competency; through activities such as publishing acceptable medical practices, it has helped increase coordination within the medical profession while retaining authority over its membership. Furthermore, physicians have historically had sole responsibility for the coordination of patient care. But, as Glaser points out, the coordination, scheduling, and regulation of hospital resources have been increasingly determined by hospital administrators rather than practicing physicians, and this has led to tension between the two groups (see Glaser, 1963; Gordon and Becker, 1964). The question researchers must ask is: What is the effect of this tension on diffusion patterns?

Diffusion and the Health Care System

There is some movement toward an understanding of these problems. Rogers (1972) points out that the classical diffusion model has been of limited use in health field studies, because its treatment of organizational structures is inadequate. The classical model emphasized individual adopters (Coleman et al., 1966) and ignored organizational characteristics which affect either the diffusion process or the adoption process.[1] Recently, however, productive information exchanges have occurred between researchers in the diffusion and organizational fields.

In recent review of the literature on program changes in organizations, Aiken and Hage (1968) argue that a systems approach is needed for understanding the intra- and inter-organizational process of change. They say that changes occur at different organizational levels, and, in order to cope with multiple levels of analysis, we must view the basic organizational dimensions and their relation to performance in terms of social resources (e.g., technology), social structure (e.g., specialization), integration processes (e.g., communication), and social environment (e.g., inter-organizational linkages).

Currently, findings in the health and organizational fields are converging. Hospitals, like most organizations, have a complex network of relations with other organizations and individuals. Government agencies, national associations, regulatory bodies, local health organizations, community interest groups, and many individuals influence the hospital's functioning (see Glaser, 1963; Elling, 1963). Some research suggests that the relation of the hospital to outside groups influences its capacity to provide medical care; hospitals associated with community leaders, through the hospital's governing body, have better access to resources for expanding their services and adopting

[1]The *diffusion process* "refers to the spread of new product from its manufacturer to ultimate users or adopters. To model a diffusion process, the analyst works with a few macroparameters that will locate a curve to describe the spread of the innovation over time." The *adoption process* "refers to the mental sequence of stages through which a consumer progresses from first awareness of an innovation to final acceptance."

new technologies. Other researchers have demonstrated that the attributes of an innovation affect its diffusion. The more compatible an item is with an existing social system, for instance, the more rapidly it will spread.

Research results make it clear that inter- and intra-organizational factors, the nature of the delivery system, and the technology itself are all centrally related to medical diffusion. Facilitating optimal diffusion, therefore, requires the cooperation of experts in at least four distinct research disciplines: medicine, formal organization, medical sociology, and diffusion. This poses problems of communication and coordination among researchers with different orientations and research traditions, and it also raises a series of important questions. What problems need to be solved in each field? What facts do we need to solve them? What further knowledge would provide a bridge between theoretical concerns and practical applications? Are the problems and methodologies of each field well enough developed for interdisciplinary work to produce comprehensive directions, or would it just create a quagmire of misunderstandings? Do some fields need separate treatment in order to focus problems and tools and stimulate further growth? Can we use current knowledge and methodologies for diffusion and intervention purposes? Finally, what problems do we face, if we attempt to use interdisciplinary research to expand our predictive power in diffusion? Answers to these questions relate directly to the nature of problem-solving research.

The Problem-Solving Process

The term "problem-solving" implies fixed objectives and a relatively natural progression of activities aimed at achieving those objectives. This process can be analyzed in terms of six interdependent research stages, which, along with the research methodologies associated with each stage, are present in Table 1.

The first stage, *problem delineation,* involves determining which areas of concern can be fruitfully researched. Before we can develop effective research programs, we must assess certain factors, including, for instance, the diffusion patterns of current medical technologies, and techniques which are overdiffused or underdiffused. After this assessment, it is necessary to have a preliminary policy statement, highlighting relevant issues and available data, and defining the range and scope of the research problems. While statistical studies and secondary data sources are important, assessments are qualitative and relate to social judgments. In delineating a problem, the data selected for re-

TABLE 1.
A Classification of Social Science Research

Research Stages	*Research Purpose*	*Research Mode*
1. Problem delineation	To define what we are looking for and the extent to which it is a problem	Qualitative analysis
2. Variable identification	To identify variables which might be linked to the problem, and describe possible interconnections between these variables	Exploratory case studies
3. Determination of relations among variables	To determine the clusters of relevant variables required for prediction and to analyze their patterns	Cross-sectional studies
4. Establishment of causality	To determine which factors are critical in promoting or inhibiting the problem	Longitudinal studies, small-scale experiments
5. Manipulation of causal variables	To determine the correspondence between a theoretical problem solution and the controllable technical factors	Field experiments
6. Evaluation	To assess the advantages, as well as unanticipated consequences, of various programs before and after they are applied on a large scale, and to determine the effectiveness of such programs in overall problem solution	Controlled field comparisons

view, the weight given to a body of data, and interpretation are, more than in any other stage, the resultant of qualitative assessments.

The next stage is *identifying important variables*. This is an exploratory step taken to discover what variables are systematically linked to diffusion and to describe interconnections among them. The major research technique employed is the case study. Questions that arise during this phase include: What areas in the health care system are critical to understanding the diffusion problem? At what points do effective and ineffective innovations enter the health system, and how do these entry points affect diffusion and health care? What characteristics of potential adopting units affect adoption rates? How do those characteristics vary? What are the users' concerns about various technological innovations? How is the type of adopting unit related to the type of innovation? Do innovations spread outside, among, or only within medical centers? Who are the gatekeepers in the diffusion process, and what influences them? What roles do the media, voluntary associations, and government agencies play in medical diffusion?

The third stage involves *determining the relations among variables*. At this stage, researchers determine the strength of relationships among variables that have been associated with diffusion patterns.

This requires a shift in research mode, from case studies to cross-sectional techniques, in order to extend the comparative range of our research. The research range and results, however, will vary according to the level of analysis used and the field that furnishes the theoretical guidelines. Social scientists concerned with the individual level of analysis, for example, might examine the characteristics of adopters or the degree to which potential adopters are socially isolated from a professional group (see, for example, Coleman et al., 1966; Shepard, 1967; Schron, 1963). Social scientists concerned with structural variables, on the other hand, might examine the ways that organizational slack influences innovation (see Cyert and March, 1963). Differences among fields also exist. Researchers in the organizational field might assume that organizations resist innovations, whereas researchers in economics might assume that organizations have a common mechanism that allows them to adapt to their environment and adopt innovations. Despite differences in orientation the research has a central focus—the relation of a given factor to diffusion patterns.

At the fourth level, *establishment of causality,* research is directed toward systematic predictions. Most work conducted in the diffusion, health, and organizational fields has produced cross-sectional or case study material, so the data base consists primarily of correlated gross variables. Consequently, we know more about adoption patterns than we do about the factors that cause the pattern. Longitudinal studies are critical if we are to identify the behavioral or social factors that are causally related in the diffusion of medical technology. Longitudinal studies, however, are both expensive and time-consuming, which accounts for the dearth of longitudinal studies in the social sciences. This is a matter of concern, because understanding causal relationships is an important precursor of successful intervention. Unfortunately, many health and other types of social programs have been initiated on the basis of untested assumptions about causality. Experience has taught us the inherent perils of such assumptions. Recently, for example, the National Institutes of Mental Health sponsored two programs aimed at developing community mental health centers and regional medical programs, but, as Tilson and Carrigan (1970:2) state:

> . . .the [health centers] concept was launched by NIMH on a wholesale basis without pilot testing or evaluation of a model. The regional medical program concept sprang from the observation that, on the whole, better medicine was being practiced at the academic medical centers than in nonacademic settings, and that additional resources and new organiza-

tional arrangements could reduce this quality gap. There is at least some preliminary evidence that neither of these intuitive judgments—on the basis of which very large sums were committed—was based on an adequate appreciation of the complexity of the factors involved.

The fifth research stage, *manipulation of causal variables*, involves identification of causal factors that can be manipulated relatively easily. To illustrate this, let us assume that diffusion is causally related to the growth rate of the gross national product. Changing the GNP is not feasible, however, so we must find other, manipulable variables that affect diffusion. The level of available medical skills might be such a variable. Another concern related to the manipulation of variables is an understanding of the system implications of a given intervention. Field experiments, which enable investigators to introduce various changes into a part of a system, represent an effective way to study both concerns.

The Tavistock studies of British coal mines are illustrative of the field-experiment approach and provide a classic example of how technological change may be ineffective unless it is integrated with the existing social patterns of organizational groups. When technical changes were introduced into the coal-mining process, the traditional division of labor was changed from independent work groups responsible for a series of coordinated tasks, to individuals assigned to simple, singular tasks. Productivity and job satisfaction dropped, even though the new task divisions were supposed to be more efficient. The Tavistock researchers found that the cohesiveness, autonomy, and self-regulation of traditional work groups had produced "behavior which goes beyond specified role requirements and . . . advances the organization towards its goals" (Katz and Kahn, 1966: 441), but the social changes generated by the new arrangement had discouraged such productive behavior. When the traditional social organization was retained and the new arrangement was integrated into it, the expected benefits were derived; but this integration could not be sustained (Katz and Kahn, 1966:441):

> The thrust of the Tavistock group toward developing the best fit between the technological system and the social system met with only partial success. Its efforts were limited by its inability to gain entry to the top-power circles in the industry, the difficulty of communicating the research results to groups who had not themselves been involved in the experimental comparisons, and the threat to the larger social system of the implications of a thorough rational reform.

While the Tavistock experiments failed in implementation, the potential viability of integrating new methods into traditional structures was illustrated in a recent replication study of industrial organizations. The study found that "technological sophistication seems to operate as a conditioning variable in social change efforts directly through situational constraints on worker behavior, and indirectly through affecting interconnectedness of social subsystems" (Taylor, 1971).

No similar studies have been done on health organizations, but we can assume that such studies would offer many research payoffs, since similar problems are created when technological change is introduced into medical institutions. Hospital adoption of certain items, for example, will necessitate new kinds of social organization. The Tavistock research indicates that the benefits of change can be realized only if the changes are integrated with existing work procedures.

The phenylketonuria program illustrates how medical programs can gain large-scale acceptance and yet prove ineffective because of insufficient testing and control. Phenylketonuria (PKU) is a hereditary, metabolic disorder which "inhibits the synthesis of the liver protein . . . (so that) . . . mental impairment appears" (Bessman and Swazey, 1971: 49–76). After researchers discovered PKU's etiology and developed a dietary treatment program, forty-one states passed laws regulating or requiring tests and treatment for PKU and other metabolic disorders in newborns. In 1963, Massachusetts was the first to pass such laws, and, in the next four years, forty other states followed suit. In 1967, two federal bills, Kennedy-Prouty and Moss, were proposed in committee to standardize PKU testing and extend its principle to detect "other inborn errors of metabolism leading to mental retardation or physical defects" (Bessman and Swazey, 1971: 58–63). The scientific validity of PKU testing and treatment programs has not yet been established, however; and, according to Bessman and Swazey (1971:49–76), four major but false assumptions are made about PKU: (1) there are reliable, inexpensive mass-screening procedures; (2) the higher the level of phenylalaine in the blood the greater the degree of mental retardation; (3) dietary treatment programs effectively prevent PKU retardation; and (4) PKU generally produces retardation. Thus, the proprietary of making PKU testing and treatment compulsory is highly questionable. Yet the availability of techniques and the claims made for their effectiveness evidently precluded testing of the techniques themselves (Bessman and Swazey, 1971:64–71):

> These laws are better characterized as symptomatic of the conflict between the scientific commitment to unfettered research and the public commitment to social betterment, however construed. . . .In the case of PKU the ideological and the economic play almost no part, yet we find that the dilemmas of the scientists, the policy maker, and the citizen remain much the same . . . the doleful story of PKU teaches us that political methods are more likely to achieve conformity than knowledge, that consensus is not truth, and that action is not always better than inaction.

The final level in the research scheme, *evaluation,* involves controlled assessment of programs aimed at solving particular problems in order to understand their advantages as well as their unanticipated consequences. Evaluation, although often the most important, is the most neglected research stage. Even when evaluations are conducted, Weiss (1972: 332) points out: "they are beset by conceptual and methodological problems, problems of relationship, status, and function, practical problems of career and reward. To add to the perils . . . evaluation is now becoming increasingly political." Accordingly she notes that most evaluation studies stress discrepancies between initial and accomplished goals in single programs, rather than emphasizing the differences among programs or program components. The former is an all-or-nothing approach to isolated phenomena, whereas the latter involves a systems perspective in which goals are the sum of the subgoals (March and Simon, 1958) and are a function of the "effectiveness of other functions, such as recruiting resources, maintaining the structure [and] achieving integration into the environment" (Weiss, 1972:334). Moreover, failure to attain a goal may result from many factors (e.g., social science theory, a program's historical background, or the fragmented nature of program structure), and all those factors should be analyzed before any program is considered a total failure, discarded, and replaced by a quick substitute (Weiss, 1972:340).

As Suchman (1967: 152) noted, the administration of an evaluation program is very important. Public demand and cooperation, the available resources, the problems caused by role relationships and value conflicts, the definition of evaluation objectives, the evaluation research design and execution, and the use of research findings all affect the administrative structures and the outcomes of evaluative research.

The Administration of Problem-Solving Research

We are not implying that the progression of stages is deliberate or that the stages necessarily follow one another in sequence. We are suggesting that most research dealing with problem-related questions falls into one or another of these categories. Moreover, a prima facie case can be made that success in later stages is to some extent dependent upon adequate development in early stages. What we are seeking to do in this discussion is to make explicit the framework that we feel most research administrators have been implicitly employing. By doing so we hope to provide a focus for understanding the conference deliberations and for raising a number of questions bearing on research accomplishment. An initial question, for instance, is whether social scientists such as those attending the Conference on the Diffusion of Medical Technology can agree on the research level currently held by each discipline. Researchers in any given field may ask: Are we at the problem-defining stage? Are we seeking causal relationships? Are we ready to run field experiments? If we know the problems and the important variables, do we have the appropriate methodology for establishing causality, for experimenting, and for evaluating? How adequate is our data bank for developing appropriate research programs? In short, at what stage are we as we attempt to intervene in medical diffusion?

We have suggested that the nature, range, and certainty of a field's conceptual schemes reflect its level of development. What, then, is the developmental level of the social sciences as they relate to the study of diffusion? Barber (1962:37–38) offers the following evaluation:

> As for the social sciences, they tend to be still in quite an empiricist tradition, with few if any general conceptual schemes that are widely accepted . . . the fundamental cause of the difficulty is the high degree of indeterminancy in most social science knowledge. . .

Barber made this statement in 1952, and again in 1962. Others have made similar comments about social science progress (Kuhn, 1970; Masterman, 1970). Their criticisms indicate an increasing desire to maximize research success. Success is more likely when we accept the early stage of social science development, and recognize the factors that hinder progress.

Although we can evaluate the level of social science development by Barber's criteria, major problems remain. Assuming that a science with sophisticated conceptual schemes (and, thus, a high degree of certainty in its predictions) facilitates problem solving, how can we move social science, especially diffusion research, toward this goal? With a limited conceptual scheme, or no scheme at all, how do we proceed with scholarly work? Here, Kuhn's concepts of *paradigm* and *normal science* are most helpful.

According to Kuhn (1970:11–12, 59–60), a *paradigm* is an implicit body of law, theory, application, and instrumentation which is learned through example and practice. A paradigm guides scientific research by encouraging expert consensus on what problems and facts are important, what methods and techniques are appropriate, and what scientific standards determine proof of findings. Paradigms gain status through their promise of success in directing the search for solutions to acute problems; and *normal science* (Kuhn, 1970:24) is the

> actualization of that promise, an actualization achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions, and by furthering the articulation of the paradigm itself.

The maturity of a science, then, depends on the nature of its paradigm (Kuhn, 1970: 10). Research can and does occur without a paradigm, but in the absence of paradigm criteria for measuring relevance or importance, research very often resembles random fact-gathering. In pre-paradigm science, or in subfields with competing paradigms, the selection, evaluation, and criticisms of relevant facts is possible through "intertwined theoretical and methodological belief" (Kuhn, 1970:17) but the interpretation of these facts differs: (Masterman, 1970:74):

> Here, within the sub-field defined by each paradigmatic technique, technology can sometimes become quite advanced and normal research puzzle-solving can progress. But each sub-field as defined by its techniques is so obviously more trivial and narrow than the field as defined by intuition, and also the various operational definitions given by the techniques are so grossly discordant with one another, that discussion on fundamentals remains, and long-run progress (as opposed to local progress) fails to occur.

The development of normal, paradigm-based science is "usually caused by the triumph of one of the pre-paradigm schools which, because of its own characteristic beliefs and preconceptions, emphasized only some special part of the two sizable and inchoate pool of knowledge'' (Kuhn, 1970: 15–17). This triumph is based on a concrete scientific accomplishment which a scientific community acknowledges for a time as supplying the foundation for its further practice'' (Kuhn, 1970: 10). According to Kuhn (1970:178, 179) the social sciences are now at the pre-paradigm stage of development, while the natural and physical sciences are at more advanced paradigm stages.

Recent research supports Kuhn's concepts of paradigms and paradigm development. Studying the structure of scientific fields and the functioning of university graduate departments, Lodahl and Gordon (1972: 57–72) investigated the assumed differences in paradigm development between the physical and social sciences by asking faculty members from eighty departments to rank seven fields. The results were as follows: physics (higher development), chemistry, biology, economics, psychology, sociology, and political science (lower development). They further report that intrafield consensus in regard to teaching and research was much greater in fields ranked high in paradigm development.

Why has social science in general and diffusion research in particular remained in a pre-paradigm stage? To say a field must wait for a ''triumph'' based on ''concrete scientific accomplishment'' is somewhat self-defeating. We suggest that triumphs have been limited because social scientists tend to use internal referents or field-centered criteria in assessing research. Such criteria tend to reinforce prevailing research traditions.

The need for a scientific perspective that transcends a given field is supported by Ben-David's discussion of scientific growth and activity during the seventeenth, eighteenth, and nineteenth centuries. Ben-David (1964:475) found that growth and innovations in medicine occurred more frequently when scientific systems were open, diversified, and free to ''defenses against any external influences.'' He also reported that major conceptual breakthroughs during this period tended to occur from problem-oriented research. In a similar vein, Weinberg (1968:26–38) argued that resource allocation among fields should be based on external rather than internal criteria because:

> no universe of discourse can be evaluated by criteria that are generated solely within that universe. Means are established within a universe of

> discourse: ends—that is, values—must be established from outside the universe.

Internal criteria reflect the competence of scientists in terms of the standards of a given field, whereas external criteria measure the field's social importance. Weinberg (1964) claims that internal criteria constitute a necessary condition for potential scientific growth and external criteria constitute a sufficient condition.

The need for external criteria is reflected in the fact that in the 2,000-odd research studies on diffusion we reviewed in preparation for the conference we could find relatively little policy applicability. The lack of applicability was especially evident in the specific area of the diffusion of medical technology. Of the 2,000 studies, there were only eight that were both concerned with technology diffusion in medical organizations and had a sample size sufficient to permit generalization. At the time of the diffusion conference these eight studies constituted the core of our knowledge in the specific area of diffusion of medical innovation in health care organizations. Yet none of the studies is experimental in nature; one study is longitudinal and the rest are cross-sectional. Moreover, because of design or sampling limitations, imputations of causality are questionable.

The limitations of this research are a cause for concern and raise the question of what can be done to improve the quality of the data base. We believe that in stressing the external criteria implicit in problem solving, both research accomplishment and the social relevance of findings can be increased. In contrasting disciplinary criteria with problem-solving criteria, Coleman (1973:6) stated: "The criteria of parsimony and elegance that apply in discipline research are not important [in applied or policy-oriented research]; the correctness of the predictions or results is important [in applied research], and redundancy is valuable." The extent to which one or the other criterion is followed is contingent in part upon whether research is disciplinary bound or interdisciplinary, the setting of the research, and the patterns of funding research as well as its level of paradigm development.

Interdisciplinary Research

The problem of collaboration between researchers with different perspectives occurred often during the conference. The issue of interdisciplinary versus discipline-centered research had been approached by a number of researchers. Analyzing sixty-two major advances in the social sciences since 1900, Deutsch et al. (1971:450–459) found that since 1930 over half of the significant contributions have come from team research and that nearly two-thirds have come from interdisciplinary work. They concluded, as did the conference members, that teamwork and interdisciplinary work would become increasingly important during the next decade. Predicting the future of science, Dobrov (1966: 229) reached a similar conclusion:

> Science must nowadays combine the knowledge and efforts of scientists from several (often remote) specialized disciplines, utilize increasingly powerful and complex equipment, process a tremendous amount of information from various branches of knowledge and in various languages. All this can only be done by teams of scientists.

More philosophically, Campbell (1969) has argued that the ethnocentrism of disciplines, which results from present training and reward systems as well as present departmental decision-making and communication patterns, has precluded an "integrated and competent" social science. To achieve integration, Campbell suggests developing narrow but overlapping interdisciplinary specialists that together form a comprehensive whole.

Institutional Settings

Researchers have also examined the research setting and its effect on accomplishment. Ben-David (1964) found that decentralization and competition, among and within universities, were strongly associated with scientific discovery during the seventeenth, eighteenth, and nineteenth centuries. Decentralization avoided the inefficient monopolization of research facilities and resources, and competition encouraged creative approaches to difficult or tangential problems. The twentieth-century growth of science and the pressure for large-scale research have produced different organizational imperatives. Rossi (1964) examined the evolution of university-based research institutes and noted that large-scale research often requires an elaborate, hierarchical division of labor as well as a large resource investment. He concluded that the traditional departmental structure of the university, which rewards individual research and teaching, is organizationally incompatible with large-scale research. It promotes narrow disciplinary concerns and the reward system discourages long-term research. Doctoral research, for example, is usually planned to yield a completed product in one year, or, in rare cases, two years. Also, the tenure and promotion systems create pressure for publishable results every two to three years. Research institutes were established to promote the benefits of large-scale research as well as to alleviate the strain between individual and team-oriented approaches. (It is important to note that similar observations were made by members of the diffusion methodology panel.) Weinberg (1965: 1–14) agrees with Rossi and adds that, between universities and mission-oriented research institutes, the institutes should have the prior claim on resources, because they can coordinate interdisciplinary efforts to solve social problems. Thus, when large resources and applied interdisciplinary solutions are required, research institutes seem to be the most appropriate setting.

Funding Patterns

Members of the conference questioned the relationship between type of funding and research accomplishment. In the past, however, researchers have paid little attention to the ways in which social science research is funded. Deutsch et al. (1971), Rossi (1964), and others have observed that the cost of doing effective research will increase as social science develops its methodologies and the range of behavior it can predict. Lodahl and Gordon (1972) have found that, in university departments, research quality is "not associated as strongly with levels of funding in the social as in the physical sciences." We have no evidence, however, on the funding levels and mechanisms which are most appropriate for different types of research. It would seem that funding levels should depend on criteria like scientific potential and social applicability, no matter what level of research activity is undertaken. The choice of funding mechanisms is more difficult. Reviewing national biomedical research agencies, Grant (1966:484) suggested that the NIH pattern of using grant review committees to determine "scientific excellence on a project-by-project basis" is satisfactory for discipline-centered problems, but does not "coincide with judgments on the most effective way to reach a particular scientific goal." Particular goals could be reached most effectively by establishing priorities among research problems and then contracting with scientists who can solve them. The funding mechanism issue cannot be completely resolved here, but we suggest that the interdisciplinary, applied work of university and other research institutes may warrant contract or similar funding mechanisms; while the individualistic, discipline-centered research characteristic of universities may call for funding through nonrestrictive mechanisms such as grants.

A Contingency Model

Administrative Perspectives

The question remains of how to systematically apply these findings so as to increase the effectiveness of problem solving in general and diffusion research in particular. In effect, we are stressing the need for developing a perspective to facilitate the application of the above and other findings to the administration of problem-solving research programs.

In attempting to develop a perspective for research administration, we ignored the distinction between basic and applied research, which refers to use or intent, and instead emphasized the research process and social imperatives. The importance ascribed to the basic/applied dichotomy is a deeply entrenched preconception in science, but it hinders our investigations of the relationship between organizational structure and scientific accomplishment. The dichotomy offers no clear-cut basis for categorization. More important, it is not directly related to the research process, and it discourages the recognition that all research activity is aimed at some type of problem solving.

The mystique which has grown up around the terms "basic" and "applied" has encouraged patterns of research administration based on misconceptions rather than empirical fact. Research findings indicate, for instance, that the following widespread beliefs are questionable:

1. Most scientific breakthroughs occur in universities, whereas exploitations of the breakthroughs occur in industrial and other applied research settings.
2. Basic researchers tend to be more creative or innovative than applied researchers.
3. Because basic researchers are highly dedicated, administrative controls over their work are unnecessary and tend to inhibit innovation.
4. The university structure is an optimal setting for all types of research activity.

Ben-David's (1960:828–843) findings challenge the first two assumptions. Noting the breakthroughs of Koch, Pasteur, Villemin, Devaine, Freud, and others, Ben-David has argued that, historically, breakthroughs have tended to occur in applied settings. Pelz (1964) and others have challenged the third belief by finding that certain organizational controls, particularly those associated with sources and evaluation, promote innovation. Kaplan (1964:463) has also questioned the assumption that universities are always optimal research settings:

> However adequate is the research environment in some universities, it is usually the top ten or perhaps even the top twenty that we are thinking about. And even in these top schools there is little question any more that everything is the best in the best of all possible worlds. Yet this is what some observers would have all of industry or government transpose to their own environment.

The distinction between basic and applied research might have been appropriate in the day of the lone researcher, but it is misleading when applied to modern research, which depends on expensive tools controlled by organizations and requires extensive teamwork. Modern research should be analyzed using concepts that refer to the research process itself and its organizational setting.

Two concepts which we found particularly valuable in analyzing patterns of research administration are *urgency* and *predictability*. *Urgency* is the social need for rapid, applicable research results, and it implies that large expenditures are justified to achieve these results. *Predictability* is the extent to which researchers can predetermine the steps needed to reach their research objectives. Predictability depends on the degree of paradigm development and the degree of involvement in paradigm-based, normal science. In Weinberg's terminology, urgency reflects social and scientific "importance," while predictability reflects a discipline's degree of "competence." By dichotomizing these concepts, we can construct four ideal situations:

U−P−	Little urgency, little predictability
U−P+	Little urgency, great predictability
U+P−	Great urgency, little predictability
U+P+	Great urgency, great predictability

Of course, research is always somewhat predictable, and there is always some urgency for the results. Also, the urgency and predictability associated with a research problem can change over time. Thus, the ideal types can be precisely matched with only a small range of studies. They are approximated in most studies, however, and using them can clarify design problems. Combining the ideal types with some findings from group dynamics and from the sociology of science produces the research administration guidelines that are outlined below.

Research Findings

1. Groups generally solve problems with predetermined answers (e.g., crossword puzzles) better than individuals; but individuals solve problems without predetermined answers (e.g., constructing crossword puzzles) better than groups (see Hare, 1962; Taylor et al., 1958).
2. In terms of absolute time, groups solve problems faster than individuals; but, in terms of man minutes per hour, individuals solve problems faster than groups (Hare, 1962).
3. Administrative control over resources stimulates scientific accomplishment. Nonpredictable research is stimulated by an administrator's evaluation of results and is hindered by an administrator's specification of research procedures (see Pelz, 1964; Gordon et al., 1966). Predictable research, however, is stimulated by administrative guidance in research procedures.
4. Effective organization of a group of research projects depends on the stage of research development. Highly developed research is facilitated by one or two concentrated projects; less developed research is facilitated by many dispersed projects (Sayles and Chandler, 1971).

Administrative Guidelines

1. Groups should be responsible for predictable (P+) research; individuals should be responsible for nonpredictable (P−) research.
2. Groups should suggest research approaches for urgent research (U+); individuals should suggest approaches for less urgent (U−) research. Also, when urgency is great (U+), different approaches should be tested simultaneously; when urgency is low, different approaches should be tested sequentially. This is especially true for nonpredictable (P−) research.
3. For nonpredictable research (P−), evaluative administrative control is advisable; for predictable research (P+), executive control is advisable.
4. Organizations should sponsor multiple projects for nonpredictable research (P−) but only a single project for predictable research (P+), because the procedures for goal attainment are predeterminable.

Classifying research in terms of urgency and predictability not only helps us develop administrative guidelines, but also allows us to predict the probability of tangential research, the emergence of anomalies, probable sources of conflict, and personality attributes required by researchers in different research settings.

Innovation

The pursuit of interesting tangents has long been considered a source of scientific innovations. A researcher's tendency to pursue a tangent is partly determined by the urgency and predictability of his research. In nonpredictable research, the procedures are determined by accepting or rejecting a series of alternatives. This selection process stimulates interest in areas tangential to the initial problem. In predictable research, procedures are usually specified in advance, which reduces tangential investigations. The urgency of the research also affects its sponsor's willingness to support tangential research: greater urgency leads to less support. We would therefore expect the U− P− situation to encourage tangential research, and the U+ P+ situation to discourage it. The U+ P+ and U+ P− situations should involve moderate constraints against tangential research. Another factor related to innovation is the anomalous finding. An anomaly is a finding that does not conform to paradigm-based expectations (Kuhn, 1970:52, 62):

> Normal science does not aim at novelties of fact or theory and, when successful, finds none. New and unsuspected phenomena, are, however, repeatedly uncovered by scientific research . . [thus] research under a paradigm must be a particularly effective way of inducing paradigm change. . .[Change] characteristics include: the previous awareness of anomaly, the gradual and simultaneous emergence of both observational and conceptual recognition, and the consequent change of paradigm categories and procedures often accompanied by resistance.

The probability of recognizing anomalies is greatest in the U+ P+ situation. Given social urgency and high predictability, the research paradigm is subject to external testing, which, as we have noted, increased the likelihood of uncovering anomalies. The recognition of anomalies may, in rare instances, induce a research crisis that "provides the incremental data necessary for a fundamental paradigm shift" (Kuhn, 1970:89) as well as the redefinition of the degree of research predictability.

Sources of Conflict

Administrator-researcher conflicts are more likely to occur in some situations than in others. One source of conflict that we have already noted involves organizational tolerance of tangential research. In the U+P− situation, where the researcher tends toward tangential investigation and the organization resists it, the potential for conflict concerning tangential research is high.

Resource allocation and procedure specification are also major sources of conflict in research settings. In the U−P− situation (where procedures are conceived of as nonpredictable by management) conflict between management and researcher over procedures should be minimal. On the other hand, since urgency is not great, support of the project might be limited. In this situation, therefore, more conflict would occur over resource allocations than over procedures. Conversely, where urgency is great and research considered predictable, much more conflict could be expected over procedures than over resources. In the U−P+ situation, conflict would tend to occur over both procedures and resources, and one would probably find a great deal of discontent among researchers.

Personality Attributes

The last predictions concern the personality attributes of the researchers. When procedures are not predictable, the researcher would seem to need a high tolerance for ambiguity; when urgency is great, he would need a high tolerance for pressure. Thus, it could be predicted that, for research personnel, the least psychologically demanding situation would be the U−P+ situation and the most demanding the U+P− situation.

The evidence we have presented to support our model is inferential in the sense that the studies cited were not designed specifically to test the assumptions implicit in the use of the predictability and urgency concepts for administrative purposes. In our study of research projects concerned with the social aspects of disease, we sought to directly examine whether urgency did affect outcome and whether predictable research (which we termed productive research) is fostered by a different administrative environment than unpredictable (innovative) research.

In our study, data were obtained by mail questionnaires sent to the directors of 245 projects (Health Information Foundation, 1954–1960). Questionnaires for 223 (91 percent) of the projects were returned. Detailed information was obtained on various aspects of the organizational structure within which each project was conducted. To determine research performance, a panel of experts was asked to evaluate specially prepared summaries of the major reports resulting from these projects.[2] The projects were evaluated in terms of both the innovativeness and productivity of the research:

> Innovation—the degree to which the research represents additions to our knowledge through new kinds of research or the development of new theoretical statements or findings which were not explicit in previous theory.
>
> Productivity—the degree to which the research represents an addition to knowledge along established lines of research or an extension of previous theory.

The panel consisted of 45 persons chosen as leaders in medical sociology by the members of the Section of Medical Sociology of the American Sociological Association. The summaries were standardized in form and efforts were made to conceal identities and publication sources.

Each evaluator was given 25 randomly selected and ordered studies to evaluate in terms of their innovativeness and productivity. The average number of panel members evaluating a project was 4.5. According to their ratings, the projects were divided into fifths—quintile 1 indicating the lowest 20 percent of the ratings and quintile 5 the highest 20 percent of the ratings on a given scale.[3]

One hypothesis following Ben-David and Barber was that in pre-paradigm sciences such as medical sociology, external pressures

[2]The decision to use summaries rather than major reports was based upon the recognition that time pressures upon rater would have precluded us getting multiple ratings for a given study if we used the major reports. To examine the tenability of using summaries in place of major reports, an experiment was run in which ratings based on summaries were compared with ratings based on major reports. The extent of agreement greatly exceeded change. For further information see Gordon (1963).

[3]As the interpretation of the intervals on the rating scale was found to differ from evaluator to evaluator, the 25 ratings for each evaluator were converted to *t* scores. The mean *t* score for each project was determined. According to the ranking of the mean *t* scores, the projects were divided into quintiles numbering 43, 43, 42, 43, 43. Nine of the projects which had no findings at the time of the evaluation, during a follow-up two years later, were found to have publications then available. Feeling that the evaluations of the projects therefore were inaccurate, they were removed from all analyses using the dependent variables.

(i.e., social urgency) were an important impetus to innovative research. We found, based on the assessments of 40 leading medical sociologists, that the research conducted in applied settings (hospitals, medical schools, health agencies), under conditions of evaluative administration (freedom in research decision and evaluation) was three times more innovative than similar research conducted in academic settings (university social science departments) under similar administrative conditions and was two times more innovative than research in applied institutions under conditions of executive administration.

Relating to our concepts of evaluative administration we found that where administrative practices were evaluative (i.e., gave researchers responsibility for research procedures but assessed outcomes) the extent of innovation was three times greater than where administrative superiors prescribed research procedures for project directors (Gordon et al., 1962:201).

The question remains, do the factors which stimulate innovative research also stimulate productive research? Judging from our study of research projects dealing with social aspects of disease, the answer is no. In that study, as we mentioned earlier, information was obtained both on the productivity and innovativeness of the research.

TABLE 2.
Percentage of Projects for Urgency and Authority Patterns in Most Innovative (Fifth) Quintile

Setting	AUTHORITY PATTERN			
	Evaluative*		Executive	
	N	%5Q	*N*	%5Q
Applied setting	62	31	45	13*
Academic setting	21	10	35	9

*Visibility + freedom

Significance of Differences (z—Tests)

	P
1. Academic—Evaluative versus Applied—Evaluative	0.03
2. Academic—Executive versus Applied—Evaluative	0.006
a. Academic—No discussion versus Applied—Evaluative	0.02
b. Academic—little freedom versus Applied—Evaluative	0.05
3. Applied—Executive versus Applied—Evaluative	0.02
a. Applied—no discussion versus Applied—Evaluative	0.05
b. Applied—little freedom versus Applied—Evaluative	0.06

Freedom to determine research procedures, hire staff, etc., which we found positively related to innovation, has a 0.31 negative correlation with productivity (Gordon et al., 1962). Decker (1967), in a further examination of how administrative factors affected productivity and innovation, found there was a significant relationship between administrative perceptions of their participation in allocation of research funds and the quality of professional administrative relationships. She concludes that:

> The factors correlated with productivity seem to be concerned more with the amount of administrative influence over the project director, either in the management of the project (allocation of funds) or the design of the research than with the conduct of the research, and they are also quite sensitive to the personal relations between the two levels of supervision. We have consistently seen this emphasis in productivity, especially in its need for strong administrative ties to management.

Decker's findings support the argument that predictable research is fostered by direct administrative control and support our assumptions in regard to predictive and nonpredictive research. This evidence in conjunction with prior evidence lends further viability to the categorization of research in terms of social urgency and predictability.

A Contingency Model for Research Administration

We have emphasized the need for a coherent framework to facilitate a unified approach to research administration. If the administrative guidelines are used in combination with the problem-solving approach, they may provide the beginning of such a framework. The interface between the problem-solving approach and administrative guidelines involves the predictability of procedures. The problem-solving approach is based upon the assumption that in the earlier stages of the research process, procedures are relatively unstructured (i.e., case study procedures) and that, as we learn more about the problem, procedures become more predictable. For example, once causality has been established, procedures for field experiments are relatively straightforward. Consequently, guidelines related to low predictability (P−) of procedures apply more in the early stages of problem solving, and guidelines associated with high predictability (P+) are more applicable in the later steps. For instance, individual-based research teams which facilitate nonpredictable research are more appropriate in the early stages of problem solution, whereas institutional structures

TABLE 3.
Contingency Model for the Administration of Problem Solving

Degree of Predict-ability*	Research Classification: Problem-Solving Stage	Research Classification: Prime Research Mode	Research Administration: Guidelines	Research Administration: Little Urgency (U−)	Research Administration: Great Urgency (U+)
			Initial Problem-Solving Stages		
Low	1. Problem delineation	Qualitative analysis	*Administrative*		
			Suggestion of approaches	Individual	Group
			Number of approaches	Multiple sequential	Multiple simultaneous
			Responsibility for research decisions	Individual	Individual
			Administration authority pattern	Evaluative	Evaluative
			Funding	Nonrestrictive	Nonrestrictive
P−	2. Variable identification	Exploratory case studies	*Innovation*		
			Toleration of tangents	High	Moderate
			Stimulation to tangential research	High	High
			Recognition of an anomaly	Low	Low
	3. Determining of relations among variables	Cross-sectional studies	*Personality characteristics*		
			Tolerance for ambiguity	Needed	Needed
			Ability to tolerate pressure	Not needed	Needed
Moderate			*Prime Conflict Areas*	Resources	Tangential research

	Later Problem-Solving Stages				
Moderate	4. Establishment of causality	Longitudinal studies small-scale experiments	*Administrative*		
			Suggestion of approaches	Individual	Group
			Number of approaches	Single	Single
			Responsibility for research decisions	Group	Group
			Administrative authority pattern	Executive	Executive
			Funding	Restrictive	Highly restrictive
	5. Manipulation of causal variables	Field experiments	*Innovation*		
			Toleration of tangents	Moderate	Low
P+			Stimulation to tangential research	Low	Low
			Recognition of an anomaly	Moderate	High
	6. Evaluation	Controlled field comparisons	*Personality characteristics*		
			Tolerance for ambiguity	Not needed	Not needed
			Ability to tolerate pressure	Not needed	Needed
High			*Prime Conflict Areas*	Resources Procedures	Procedures

*The degree of predictability is the resultant of paradigm level and problem-solving stage.

facilitate research in the more predictable later stage. Thus, research funds may be channeled primarily through grants during earlier stages of development of a problem and through other, more restrictive research support mechanisms after immediate objectives have been articulated.

It is important to note that the presence of a viable paradigm not only leads to increased predictability throughout the problem-solving stages, but also may enable researchers to skip certain stages in the process. For example, the manned space program had to solve a number of problems involving heat transfer. Since many of the factors affecting thermal quality of materials are known, all that was necessary in many instances were evaluations of promising solutions. We are, in effect, advancing not an absolute, but a contingency model of research administration that is dependent on social urgency, problem-solving stage, and paradigm development. The contingency model and the administrative recommendation derived from it are presented in Table 3.

To use the contingency model viably, we must be able to determine degrees of urgency and predictability. We can never be completely sure of research predictability, but experts in given disciplines can assess the state of the art in their respective fields and, in conjunction with a determination of the problem stage, they should be able to estimate predictability with some accuracy. Determining urgency is more difficult, since urgency varies with social values. This can be seen in the attitudinal change regarding availability of medical technology. Medical care, or access to a specific medical technique, can be considered a privilege, a right, or a mandate. When access to a technique is considered a privilege, the technique is not available to everyone, and special considerations (e.g., money) are prerequisites for access. Types of care which are considered a matter of right are available to everyone, so use depends primarily on individual choice. Techniques that are mandated involve little or no individual discretion; under a positive mandate, a procedure is imposed on people (e.g., no abortions after six months). Before 1900, medical care was considered a privilege except in some cases of communicable disease, and governmental resources were only marginally involved in medical care. During the last seventy years, however, we have increasingly come to think of medical care as a right rather than a privilege. This attitude change has been accompanied by a dramatic increase in governmental involvement in health activities as reflected in increased public expenditures for health. The government has become directly involved in providing services, training professionals, and developing new techniques. Today, no one would deny the right of all people to have the best medical treatment available. But, in competition with other rights, how much of our scarce resources should be allocated to providing the best available medical technology? Moreover, how much do we expend on improving technology and how much on the distribution of existing technology? While some of these questions require a determination of the extent to which diffusion of medical technology is a problem, the ultimate decision is a political one relating to social values and aspirations. It has to be recognized that, given its political nature, perceptions of urgency vary over time.

We expected that the diffusion conference, in addition to determining problem stage and assessing the "state of the art," would provide a dialogue between governmental representatives and potential diffusion researchers on the nature of the diffusion problem and its perceived urgency. Before determining guidelines for research on medical diffusion, conference participants had to consider several questions: Is diffusion of some medical innovations more important than diffusion of others? Can we establish criteria for evaluating the level of urgency? The contingency model for research administration which we have presented is only a starting point in a dialogue that we hope will focus discussion for the diffusion and similar conferences. Ideally, the model will also provide a thrust for the systematic application of existing knowledge to the development and administration of research programs.

Admittedly, the base upon which to apply such knowledge is sketchy and identification of relevant factors is incomplete. Even employing such relatively simple distinctions as predictability and urgency presents major problems. Research urgency tends to change in response to success and failure, and projects may require state-of-the-art methodologies at one stage and innovative procedures at another stage. Though difficult, such decisions will have to be made; and we should recognize that the same types of decisions are constantly being made under such rubrics as "basic" and "applied," "fundamental" and "problem-oriented." In making such distinctions, we have too often failed to treat social research as a problem-solving process. Consequently, we have emphasized men and organizational structures, but neglected the prerequisites of science. By prerequisites of science, we mean more than just a determination of the nature of the problem; rather, we are referring to a broad view of the science system including the steps necessary for problem solution. Instead of treating research as a continually changing process, we have too often applied rules and precepts about research willy-nilly to all projects within an institution or program.

In conclusion, we raise this question: Would the 2,000 diffusion studies we reviewed have yielded more compelling findings if they had been done in the context of unifying administrative and support structures designed to facilitate the various phases of diffusion research? Given increasing funding pressures and social needs, it is becoming imperative that this question be dealt with in a systematic fashion by research and policy makers.

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