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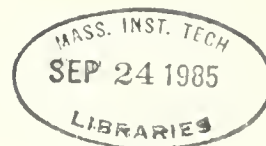






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WORKING PAPER  
ALFRED P. SLOAN SCHOOL OF MANAGEMENT

RESEARCH OPPORTUNITIES IN THE DECISION AND MANAGEMENT  
SCIENCES: REPORT OF THE DALLAS NATIONAL SCIENCE FOUN-  
DATION WORKSHOP

John D. C. Little  
Sloan School of Management  
M.I.T.

June 1985

WP #1703-85

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## ABSTRACT

The National Science Foundation established the Decision and Management Sciences Program (DMS) in 1982. Healthy and growing, DMS is likely to affect OR/MS significantly, not only by providing funds for basic research, but also through its vision of a combined theoretical and empirical science of operational and managerial processes, and by its policy of bringing together different disciplines within the program. An NSF workshop held in Dallas in April 1984 sought to identify research opportunities in the decision and management sciences. Promising areas, many of them inviting cross-disciplinary reasearch, appear in the development of measurement-based models for operational processes (both natural and designed and including organizational and managerial activities), choice theory (individual and group choice, values, judgement, and risk behavior), decision support, and the treatment of complexity. Increased knowledge in any of these areas would be likely to have a valuable long run impact on management practice.

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In 1982, with strong endorsement from the professional societies TIMS, ORSA, and AIDS, the National Science Foundation established the Decision and Management Science Program (DMS). During 1982 and 1983 the program started up with a modest but growing budget and awarded an initial series of grants. In April 1984, to develop the program further and to understand better its scientific potential, NSF sponsored a workshop at the University of Texas at Dallas under Frank Bass' chairmanship. The purpose was to identify research opportunities in DMS and make recommendations to NSF about directions for the program. This paper provides a summary of the research opportunities generated by the conference.

My belief is that NSF's Decision and Management Science program will substantially affect the evolution of our field. This will happen in part, of course, by the injection of funds for basic research, but even more because of the vision of the DMS charter and NSF's policy of deliberately bringing together different disciplines within the program.

### DMS Goals

A statement of DMS research objectives was written by its Advisory Subpanel early in the program's history. The statement emphasizes the creation of a theoretical and empirical science of managerial and operational processes. The processes are typically to be described by mathematical models derived from empirical observations or from a theory that is subject to empirical verification. To quote from the mission statement:

"Thus, the body of research supported by the program should possess generality, be based on empirical observation or be subject to empirical validation, and incorporate social and behavioral aspects. Processes should be characterized by models that are tested in operational contexts. Even though an individual project may not have all these characteristics, its evolution toward this end must be clear."

Although OR/MS has done much good in the world over the last 30 years, the field continues to fight a narrow image drawn largely from its long association with mathematical programming, queuing, decision analysis and their supporting mathematical methodologies. Although no one faults the payoff from these areas, most people in OR/MS wish to see the field attack managerial and operational problems broadly and, in fact, tend to be discontent with the slow pace at which useful new knowledge is created and put into practice.

From this point of view, the DMS statement serves as a highly visible reaffirmation of OR/MS's classical scientific roots and broad mission in models and measurements for live operations. The statement, reproduced in the Appendix, also recognizes explicitly the social and behavioral aspects of managerial processes.

Another significant move by NSF has been to involve, at the outset, not only the operations research, management science and decision science communities that were instrumental in establishing DMS, but also at least two others that are relevant but have not been closely connected. One is systems theory, which is based primarily in electrical engineering and has many

members who work on operational problems. The other, more distant in culture, is the branch of psychology that studies how people actually make decisions. This subject sounds relevant to DMS, and it is. The bringing of the three groups together establishes a broader disciplinary base than can be constructed from the mainline OR/MS community alone.

### Workshop on Research Priorities

The Dallas workshop on DMS research priorities consisted of two parts, each taking a day. The first contained papers and presentations: In the morning a series of speakers outlined their views of high-payoff research opportunities; in the afternoon people presented examples of research on operational phenomena and decision making. The second day built on these materials and sought to identify research topics with the potential for significant advance in the next 5-10 years.

The papers reporting research on operational phenomena and decision making were:

George Huber, "Decision Support Systems"  
Richard Larson, "Public Sector Applications of OR"  
Michael Cohen, "Computer Models and Organizational Design"  
Detlof von Winterfeldt and Ralph Keeney, "Value-Focussed Analysis"  
Alexander Levis, "Decision Making in C<sup>3</sup> Systems"  
Lola Lopes, "Psychological Theories of Risk"  
Subrata Sen, "Marketing"  
Andrew Whinston, "Decision Support Systems and Organizational Complexity"  
Robert Winkler, "Acquiring Information"  
Lofti Zadeh, "Expert Systems and Fuzzy Sets"

As may be seen from the topics, at least the three different disciplines mentioned earlier are represented. A selection of these papers is appearing in the IEEE journal, SYSTEMS, MAN AND CYBERNETICS, whose editor, Andrew Sage, chaired the session, and so they will not be covered here.

The session on high payoff research opportunities fitting the DMS mission, chaired by Alfred Blumstein, contained the following presentations:

Frank Bass: Management Science  
Richard Larson: Operations Research  
Kenneth Hammond: Psychology  
Hillel Einhorn: Behavioral Decision Making  
Edison Tse: Systems Theory

The material from that session along with the entire second day will be the subject of my summary on research opportunities. Two of the papers, Hammond on psychology and Tse on systems theory, provide good examples of thinking not usually brought to bear in OR/MS research and have been reproduced in a more extensive report on the workshop (Little 1985).

## On Forecasting Research Breakthroughs

I feel more than a little uneasy about our task of forecasting high opportunity research topics. The most important research is, almost by definition, unpredictable. For example, no one at the workshop would have dreamed of suggesting an improved LP algorithm for the high payoff list, but now we suddenly have one and the payoff looks big.

Nevertheless, the smug observation that breakthroughs cannot be predicted, often made by researchers, does not help resource allocation. This is true both at the personal level of individuals planning their own activities and at the institutional level for organizations like NSF. Whether we are good at it or not, we have to look ahead.

Another concern relates to the casual construction of wish lists. The popular press often asks: Why doesn't science cure the common cold (or cancer), do away with poverty, or turn garbage into gasoline. The reason is that good scientific research, and its subsequent practical application, require the assembly of many pieces. These include at least: (1) a problem that is ready, i.e., previous knowledge has accumulated sufficiently that the problem can be defined and is susceptible to solution, (2) a methodology or paradigm that is available or can reasonably be created that will solve the problem, (3) an intellectual or economic environment that makes the problem interesting or important so that the required effort will be expended, and (4) a person or persons who appear and have the requisite skills to perceive the problem and conceive of a way to solve it. The required skills will vary greatly from case to case. One situation may call for the fresh mind of a 23 year old mathematician, another for the entrepreneurial skills of a senior researcher running a large organization.

What follows, therefore, is not so much a wish-list as various thoughts by a set of scientists about where the DMS field might go, could go, or should go. In some cases the researchers are self critical and see the necessity of expanding their current paradigms; in others they see technology opening new vistas; and in still others a clear need for a specific pieces of work. In almost all cases they believe that something can be done, i.e., the indicated directions are feasible. In this sense I suspect the suggestions are conservative, although that does not mean that results will necessarily come easily. Our hope is that the articulation here will help start some set of individuals along a trail of ideas that will lead to valuable research. In addition, we wish to sensitize NSF to the emerging opportunities.

### HIGH PAYOFF OPPORTUNITIES BY DISCIPLINE

This section summarizes ideas that emerged from the disciplinary presentations.

Bass, in discussing management science, highlights opportunities surrounding the issues of individuals making choices, drawing illustrations from his own field of marketing. He observes that marketing measurements are uncovering fundamental disagreements with the assumptions used in many economic theories. Understanding what is going on at the individual micro level and then aggregating it to the macro level where economists normally

work offers a fruitful avenue for research. Also related to choice, understanding the linkages among information acquisition, preference for an alternative, intent to choose it, and final choice require deeper study by possibly new, empirical methods. Finally, real world decision makers in managerial roles use much judgment under conditions of uncertainty and risk. There is a need for what might be called "guessing systems" and therefore deeper fundamental theories of how people do and should employ judgment in actual managerial situations.

Larson, reporting opportunities in operations research, returns to the writings of early workers in the field, emphasizing the scientific study of operations and the need to develop identified regularities of behavior into natural laws. He notes, as does Bass, the need to go from micro observations to macro laws. In speaking of congestion processes, such as the distribution of traffic on a network, he observes that real traffic does not always behave according to the predictions of the equilibrium models devised to describe it. Similarly, most queuing systems in practice violate many of the standard assumptions of the existing theory. The discrepancy between theory and actual observation provides opportunities for new theories and new practice.

A neglected area is the development of tools for robust analysis. Larson cites a low-tech solution for an application, a "meals on wheels" food delivery service. Researchers devised a pencil-and-paper scheduling procedure that featured simplicity and robustness rather than sophistication and optimality. This provided excellent operating results when put to the stress test of field use by non-technical people. Larson expresses a desire for less research that refines existing models and more that invents new models for new operational contexts, pointing out that the historical strength of OR has been the ability to give structure to new problems. He conjectures that in recent years, OR/MS has spun off more in the way of subfields than it has acquired in the way of new problems to work on. Finally he sees great payoff from the use of super-micro computers and graphics as implementation vehicles for OR/MS models.

Hammond's provocative paper lays out high payoff opportunities in psychology within DMS. He first describes potential contributions to DMS from the psychological literature. Work carried out in the 1970's on how people actually make judgments and decisions has essentially demolished the validity of the expected utility model for describing people's decision making behavior. Many ingenious laboratory experiments have demonstrated that, under a variety of conditions, people will consistently make different decisions from those specified by the standard rational models of decision analysis. This discovery provides a rich set of research opportunities. For example, what are the costs of non-rational behavior in various contexts and what are potential remedies for such behavior?

Another branch of psychological literature compares the judgments of a person with the outputs of an empirical formula. It is found over many cases that simple linear models predict most task outcomes better than expert judgment. Further it has been found that these simple additive or weighted sum models predict judgments quite accurately. The main implication of this research is that, in many cases, linear models rather than a person should be given the information necessary for prediction, even if the person is an expert. This, too, opens up a variety of DMS relevant research, such as determining differential costs and benefits of the use of these models in



operational situations.

In contrast, psychological researchers from the field of problem-solving have found that people, especially experts, store large amounts of information and use them effectively, often in the form of short cuts. Notice that this research stream and the previous one are strangely contradictory about experts. The judgement and decision-making researchers usually find that experts fail and should be replaced by a simple linear processing of inputs, whereas the problem-solving researchers find that experts are superior problem-solvers. What are the differences in settings and tasks that give rise to these contradictory outcomes? What is correct? As Hammond says, it is hard to think of a more important problem for Decision and Management Science.

The DMS charter calls for generality of results, but Hammond observes that in all three of the research literatures just cited psychologists have found their results to be highly task dependent. Generality is disappointingly lacking. This means that DMS researchers must be cautious in building on the work of the psychologists, but therein also lies a major opportunity. Hammond feels that DMS can contribute much to psychology as well as itself by requiring generalizable results in operational settings. Traditional psychological research is laboratory-bound, often with inexpensive research projects and students as subjects. He cites examples of advances in psychological research that he feels cannot be made unless the work satisfies the DMS policy guidelines. These include understanding decision making over time, understanding expert judgment, and understanding group decision making among people in real organizations.

Einhorn, in his discussion of opportunities for DMS research in behavioral decision making, zeros in on a major issue raised by Hammond, namely, generalizability. Work in the psychological literature always seems to end up qualifying the results with "it depends." Behavior is found to be highly contingent on the environment. The environment is infinitely variable and complex. As a result there is a tendency to despair about obtaining general principles of behavior. He asks whether we are doomed to an enumeration of highly specific conditions.

His answer is that we need models, just as stated in the goals of DMS. He goes on to illustrate the idea by analyzing a particular situation. In communications research, subjects have been found to be persuaded most by information presented to them most recently - in some cases. In others, the information presented first has the greatest effect. In still other cases, the order of presentation does not matter. Einhorn suggests that the resolution of these contradictory results will lie in a model that explicitly accounts for differences in the conditions. For example, the model should contain at least two counteracting forces, which are then linked to explanatory variables describing the environment. He goes on to hypothesize such a model and show how it could work to explain the observed behavior. His point is clear: traditional psychological research has focussed on varying the conditions and observing the effects and not, as required by the DMS mission statement, on building models that would explain behavior across many experiments. Therein lie the research opportunities, both for psychology and Decision and Management Science.

Tse explores opportunities in systems theory for DMS objectives.

Although systems theory is closer to standard OR/MS in heritage than the psychology, Tse has some rather different perspectives. First he lays out a multi-stage view of decision making. In it he differentiates between a "generation" phase, which consists of problem recognition, issue identification, option generation, and preliminary option screening, and a "choice" phase, which contains selection and implementation. Although research opportunities exist at each stage, Tse points particularly to the study of intelligent activity in decision making as an area of promise opened up by developments in artificial intelligence and expert systems. Further special opportunities arise because of recent developments in man-computer interfaces. Moreover, he specifically calls for a new framework to handle the generation and choice paradigm. He sees an opportunity for finding a representational framework that handles uncertainty and copes with semantic ambiguity. One potential approach is the possibility theory of Zadeh.

Tse also sees the need for the development of methodologies to enhance the dynamic iteration between generation and choice. Such research is likely to require the integration of expert systems, user interface technology, systems analysis and human interaction. His vision is that, with the help of interactive computing, a simulation environment can be built in which the system equations are parameterized. Expert systems would contain rules for selecting equation structures to be simulated. Such an interactive simulation and expert environment would allow the user to change rules, model, and parameters interactively during system simulation. With the computer taking care of the analytical manipulations, the user can experiment with the implications of new options. This would permit the uncovering of new issues that in turn would redirect the search for alternative options in a dynamic iteration between the generation and choice phases of problem solving.

#### CROSS-DISCIPLINARY OPPORTUNITIES

A major goal of the workshop was to bring together individuals with quite different backgrounds to identify research topics in DMS. In terms of process, the group as a whole developed a list of topics. These were then coalesced under major headings, after which the conference split into mixed-discipline working groups, each of which sought to articulate the research opportunities under one assigned heading.

After simplifying some of the titles and rearranging them slightly, I have organized the content as follows:

1. Operational processes
  - a. Phenomenological processes
  - b. Organizational and managerial processes
2. Choice theory
  - a. Individual choice, values, judgment, and risk behavior
  - b. Group choice and decision making

### 3. Decision support and expert systems

### 4. Complexity

Within each heading I shall try to capture key ideas that percolated up during the conference. In doing this I am indebted to the people who summarized the discussions within each working group. In several cases their notes are reproduced almost verbatim. However, responsibility for editorial injustice is mine.

## 1. Operational Processes

### a. Phenomenological processes.

The word phenomenological is not only a mouthful but needs elaboration and example. First, we divide such processes into natural and designed. Natural processes are those that occur without an organized, conscious effort to make each individual event happen: for example, traffic on a road network, customers arriving at a store, or viewers switching TV channels. Designed processes have a structure deliberately put together by an organization: for example, a production line, airplane landings, or office administrative procedures.

Within natural processes three kinds can be identified that warrant modeling and empirical observation in multiple contexts. The first, "diffusion processes," includes propagation of information, technology transfer, diffusion of innovation, spread of epidemics, etc. The second, "homeostatic processes," refers to systems that tend to retain stable characteristics even in a changing environment. The third group, "evolutionary processes," encompasses growth and change in many entities, e.g., cities, corporations, and communications networks.

Designed processes of particular interest are of two kinds: "flow processes" and "response processes." The "flow processes" include a wide variety of situations where tangible items (such as partially completed products on an assembly line) or intangible entities (such as information) flow in some purposive way along paths that may be individually charted or collectively established but proceed with the intention of achieving some outcome. These processes may also suffer congestion. "Response processes" are exemplified by a wide variety of emergency service systems (e.g., fire, ambulance, police).

A research goal is to develop classes of models that capture a rich array of contexts in one or more of the categories mentioned. General approaches will be useful, some perhaps micro at the individual event level, others macro describing aggregate behavior. Ideal would be general models with parameters that can be measured in specific contexts to particularize the application. The task may not be easy but there are important commonalities between quite different phenomena and the discovery of general models that permit the determination of properties of various operations will be of significant value.

b. Organizational and managerial processes.

Three areas provide major opportunities: (1) organizational design for decision making, (2) organizational support systems, and (3) strategic management.

In organizational design for decision making the scientific and practical goals are clear: We would like to connect the decision making tasks to the design parameters of the organization, then relate the design to the eventual outcomes of the decision making, and finally relate those outcomes to relevant measures of mission performance. In format this is classical OR/MS and systems theory, but, as of today, we almost completely lack the quantitative linkages required. Not only are the relevant descriptors and parameters of organizational design large in number, but the relevant processes include such poorly modeled issues as managerial control and distributed decision making.

The members of the mixed-discipline group see organizational design as enormously important and, of course, difficult, but they are very clear and unanimous on the need to approach it with formal quantitative models. These are likely to require the development of new language and terminology. Unquestionably, theory-driven field experiments will be needed. These may be naturally occurring or specifically designed. Visualized is a multiple step research endeavor that starts with the qualitative knowledge already built up in organizational science, proceeds by devising mathematical theories, continues with the development of methods for making field measurements, and culminates with tests of the theories in the field with real organizations. Although this does not preclude laboratory tests, field testing should be the attained goal.

If our theories and measurements become good enough, we shall be able to consider computer-aided design of organizational systems.

A second major theme is organizational support systems. Organizations exist and function through a variety of information and communication processes. These have received considerable research although formal models of them are few. We are now in an era of rapid change in information processing and communications technology. This situation offers a remarkable opportunity for studying these processes because the changes are creating innumerable natural experiments. Part of the picture and a vital topic in its own right is the effect of specific kinds of computer and communications hardware and software on managerial processes.

A third major opportunity for applying the DMS guidelines lies in strategic management. This field underwent great growth in the 1970's both as a consulting practice and as an internal corporate activity. Much of the work has rested on a few simple, but quite powerful conceptual frameworks. Since the world we live in seems to be increasing its rate of change and certainly is increasing in inter-connectedness and interdependence, the role of strategic management is secure. An organization must sense its external environment and internal state and then develop and execute short and long range plans. Needed are models to describe the environment and relate alternative strategic thrusts to outcomes and performance. Since most external environments contain competitors, our models must provide for competitive behavior and response. Developments in the field of differential

games may be useful. In any case we need new models of markets and organizations to support the overall planning process.

Only through interdisciplinary research will progress be made on these topics. OR/MS and systems theory can contribute mathematical modeling and organizational science can offer measurement skills and qualitative theories. It is often difficult to find people from different disciplines in the same organization, e.g., the same university department, but there may be ways to reduce the barriers. For example, NSF can encourage interdisciplinary conferences and interdisciplinary proposals.

Another obstacle is the very size and scope of the research tasks described, to say nothing of the difficulties of mounting research efforts on real organizations. An answer to this is to conduct research in phases. The first phase is to construct theory and models on the basis of current knowledge. The second is to devise measurement systems and methods of instrumentation within organizations. The third involves the actual field data collection. Finally, the fourth phase consists of data analysis and testing of the theories. Notice that each stage has stand alone interest and should produce important publishable research. However, the work should be shaped and driven by the larger research goals of the overall project.

Some of the supporting disciplines for addressing these major questions are organizational science, non-zero sum differential games, the cognitive side of expert systems, theories of distributed information systems, modeling concepts from large scale systems, and multivariate statistical methods, such as structural equations, that support model calibration.

## 2. Choice theory

### a. Individual choice, values, judgment, and risk behavior

Four important research areas with high promise are:

(1) Normative theory and human behavior. Many empirical studies have shown real decision making to be different from that prescribed by traditional rational models. While this can be interpreted as demonstrating irrationality, it can also be seen as challenging the traditional conception of irrationality, calling perhaps for new normative theories. Anticipated regret, costs associated with thinking, and alternative axiom systems for utility have recently been proposed to account for the observed anomalous behavior. Work that proposes and tests alternative or broader conceptions of rationality deserves high priority.

(2) Behavioral bases of decision/risk analysis. Formal methods for aiding decisions, such as decision analysis and risk analysis, depend critically on inputs from individuals. Judgments of probability have been extensively studied, although unanswered questions remain. Several other processes are far less studied and need attention. These include problem structuring and option generation, the elicitation and modeling of preferences, and the development of methods to extract information from experts.

A great deal of research has documented the existence of systematic biases in judgments under uncertainty. Research is now needed to find ways to reduce the biases. We need better understanding of risk preferences. Multiple criteria models should be improved, not only for simple decisions, but also for individuals faced with multiple decisions.

(3) Descriptive choice theory/customer behavior. As with decision analysis, we need research aimed at illuminating what goes on in early stages of choice. Topics such as framing and agenda effects and option generation are key here. Additional work should elaborate the specific cognitive processes that underlie choice behavior. The relationship between preference, intention, and choice is not well understood.

Also of importance are the possible effects of the new information and communication technologies on individuals' decision-making behavior. Unanswered questions remain about how decision makers cope with uncertainty. Do they view their problems as multi-outcome lotteries, or do they employ other models?

(4) Decision-making in crises. Crisis situations pose an extreme challenge to the abilities of decision researchers. Such situations, whether in military or non-military settings (accidents, swift changes in the business environment, etc.) are characterized by high stakes, severe time pressures, fear, and other elements of stress. Attempts to cope with potential crises usually take the form of contingency planning and simulation. Such activities occur in hypothetical settings, far distant in time from the actual crisis. We need research to help us understand the effects of stress, hypotheticality, temporal distance, etc., on decision making. For example, will utility functions for gains and losses change systematically as one gets closer in time to experiencing a crisis? How can realistic simulations be created for training purposes?

Besides the four primary research topics just discussed, two cross-cutting issues are germane to all of them: temporal factors and the methodological question of laboratory vs. field studies.

Temporal interactions add complexity to all the research questions posed above and are of obvious theoretical and practical importance. The role of time in crisis decision making has been noted. In addition there is the need to study the interplay between risk attitude and the decision maker's planning horizon (the modeling of risk preferences in multiple time periods). An important problem in consumer behavior is understanding the pattern of an individual's decisions over time. We need better methods to detect change and to model the stochastic processes involved.

Finally, both laboratory and field studies will play a role in addressing the topics described. Particularly promising is the use of a combined laboratory-field approach sequentially within the same project.

b. Group choice and decision making.

Under this title six principal research topics emerge:

(1) Structuring problems with groups. Needed research includes work on option generation, value structuring, and the issues raised by multiple criteria.

(2) Organizational information. Relevant are the generation of information, its representation, and how it affects interlocking and interdependent decision making.

(3) Conflict resolution. This is an important area which has received much attention in various forms but continues to elude a thorough scientific understanding. Theories and measurements for bargaining and negotiation are required. Related are more general considerations of multi-party decision making. Subproblems include the study of fair allocation and such phenomena as suppression of issues during negotiation.

(4) Values and judgments. As mentioned elsewhere we need to model the experts. Research is also required on value structures, preference, and risk assessment in groups. Beyond that comes the question of how these structures and assessments evolve over time. A neglected issue for the researcher is how the model builder's values may differ from those of the model user.

(5) Organizational implementation. We wish to understand not only how a group makes its choices and decisions but how it carries them out, for this undoubtedly feeds back into how the decision is made. Organizational structure is a variable here and questions arise over timing and various organizational interdependencies.

(6) Decision making over time. A recurring theme is that, as researchers, we start with circumscribed, usually static situations in our quest for understanding, but the world is dominated and driven by dynamic processes. These clearly feed back into the decision process and so deserve study and consideration for themselves.

Progress on these difficult topics requires interdisciplinary research, which is well-known to have many barriers. To help overcome these obstacles: (1) link research to real world problems; (2) follow the guidelines of DMS advisory panel; (3) develop postdoctoral programs for interdisciplinary training (stipends to be awarded for work outside the researcher's usual specialty); (4) focus grants on multi-disciplinary projects; (5) sponsor multi-disciplinary conferences on topics requiring such knowledge and skills.

### 3. Decision Support and Expert Systems

Eight topics have been collected under the heading of decision support and expert systems: (1) knowledge representation, (2) decision analysis with expert systems, (3) person/system interaction, (4) impact of real time information on decision making, (5) information requirements determination, (6) information systems impact on organizational structure, (7) generation of heuristics, and (8) limits of machine generated decision making.

To animate and focus the issues, subsets of these topics have been worked into suggested projects for NSF/DMS funding. Of course, given the

limited time at the workshop, numerous details remain to be developed. Nevertheless, the skeletons of three quite interesting projects emerge:

(1) Interplay between expert systems, decision analysis, models/algorithms, and humans

Decision makers face a spectrum of decision complexity, ranging from frequent, rather routine decisions in statistically regular environments to infrequent decisions with information profiles that may be unprecedented. Moreover, paralleling this spectrum are utilities or disutilities associated with outcomes, which may range from relatively small in absolute value for routine decisions to very high (perhaps with large life-and-death consequences) for non-routine, very rare decisions.

The research question focuses on the allocation of this spectrum to various techniques that are now available for decision aiding. A hypothesis is that expert systems, somehow married with models and algorithms, would be excellent, perhaps superlative decision aiders and/or decision makers for the statistically regular, high frequency decisions; that formalized decision analytic techniques would be appropriate for more difficult, less frequent decisions with high possible disutility; and that perhaps no formal techniques could be relied on for points near the end of the spectrum of statistical irregularity and highest potential disutility. Throughout the spectrum, even when it is envisioned that formal techniques may be superior to human judgment, a question remains as to the suitability for "human override" of the formal technique and the appropriate methods for evaluating the desirability and correctness of a selected human override technique.

The spectrum of decisions and decision complexities can be brought out by the study of real systems, such as emergency services (police, fire, and ambulance), and diagnostic operations (such as fault diagnosis, medical diagnosis, and analysis of medical tests). Response to a suspected nuclear attack would be an obvious candidate for examining the most difficult end of the spectrum.

Research questions abound under this umbrella of topics. For one, there is the question of the appropriate marriages between expert systems and traditional operations research models and algorithms. Expert systems often make use of detailed lists of conditions giving rise to a particular action; OR models and algorithms tend to rely on statistically regular physical operations and models of systems, and derive their normative implications from analysis of the model's mathematical properties. In some senses, expert systems parallel the thinking of the clinician, who conceptualizes his activities on a case-by-case basis; whereas OR models parallel the actions of a statistician, who relies on regularities derived from large populations.

Attributes that seem associated with expert systems and/or traditional OR models and algorithms are: structured, high in certainty, clear-cut, algorithmic, deterministic, repetitive, and systematic. Attributes toward the other end of the spectrum of decision making that seem to require extensive judgment and perhaps formal use of decision analysis are: unstructured, uncertain, ambiguous, judgmental, probabilistic, breaking of historical precedent, value laden, and life consequential.



The likely end-implementation of this line of research would be an allocation of the decision difficulty spectrum to machines and humans. A most critical type of situation would be one in which, say, 99.9 percent of the time, the machine provides the correct decision, but when an error is made, the results may be catastrophic. Obvious examples are: decisions regarding nuclear retaliation to presumed nuclear attack where the timeframe for decision making is 6-8 minutes, emergency maneuvers for aircraft attempting to avoid mid-air collisions, decisions on real-time control of nuclear reactors facing emergency conditions, etc.

(2) Person/machine as a decision making unit: effective organization, presentation, and exchange of information.

The overall objective of this project is to study alternative and improved mechanisms for information exchange from the machine (i.e., computer) to the user and from the user back to the machine. The work is motivated in part by the rapidly advancing technologies in computer graphics, which are making obsolete the simple alpha-numeric computer display of information. Today a rich assortment of window-oriented graphic displays, some using color, can provide a much richer environment for providing the user with information. In the reverse direction, the user can convey information and instructions to the computer in numerous ways other than the perhaps-to-become-outmoded keyboard; these include mouse, lightpen, joystick, touch sensitive screens, voice activation, and others.

To carry out the project requires one or more simulation laboratories in which to test hypotheses about information transfer between people and computers. The laboratories need state-of-the-art equipment in computers, computer graphics, etc. Research questions include alternative ways to present data (including use of color and graphics); presentation of only decision-relevant information (to avoid information overload); uses of sound, touch, and other senses for information conveyance; ways to design the person-machine unit so as to optimize the balance between the on-line computational power of the computer and the judgmental decision power of the human.

(3) Information systems impact on organizational (and decision making) structure

The key conjecture behind this project is that the revolution in networked local computation that we are currently experiencing will have marked effects on organizational structure and behavior. The organization as an organism may experience a dramatically swift Darwinian change (mutation?) as a consequence of a redesign of its "nervous system," (i.e., its network for information transmittal and storage). Since knowledge is power within organizations, it is possible that the traditional hierarchical organization will have difficulty in maintaining its hierarchical structure in an era of decentralized computation and information transfer.

One way to conduct this project is to study current organizations' responses to the introduction of networked information systems as a set of naturally occurring experiments.

Under the umbrella of the project, numerous related research questions arise: Are currently implemented and proposed MIS systems more

democratic or authoritarian in terms of organizational structure? Are new networked systems yielding a higher degree of decentralization than we have seen in the mainframe era? How are decentralized networked MIS systems affecting individuals, their incentives, reward structures, and the quality of their working life?

#### 4. Complexity

Complexity affects DMS research in three conceptually distinct forms. These can be characterized as prescriptive, descriptive and communicative complexity.

##### (1) Prescriptive complexity.

DMS researchers whose goal is to help decision makers make better decisions must cope with complexity in the decision setting. For example, in the design of complex systems there are many separate steps or processes each of which feeds back into and affects all the others. We call this structural complexity. Similarly, in problem domains that, in principle, can be modeled statistically, difficulties frequently arise when the processes under study are noisy or mutually dependent. This is statistical complexity.

The time is ripe for DMS researchers to tackle problems such as these. In general, enough is now known and methods are sufficiently well developed for research on these topics to be pursued fruitfully. There are at least two useful directions. One is for "outside persons" to observe and study interactions in complex settings. Thus, a behavioral scientist might observe users and system designers interacting in the development of a complex system, with intervention and experimental manipulation employed as appropriate. A second, complementary activity would be workshops focused on problems which are reasonably likely to be soluble at present but which, for whatever reasons, have received insufficient attention from relevant researchers. One such problem is the development of techniques for modeling complex statistical dependencies in statistical inference.

##### (2) Descriptive complexity.

Many DMS researchers are primarily interested in the description of the decision making processes of groups or individuals. Although current research on these topics has illuminated certain features (e.g., heuristic methods used by naive individuals, dynamic processes in groups), much needs to be done to understand the functioning of these processes in the complicated environments in which they naturally occur. In particular, strong theoretical work, perhaps involving simulation, is needed to define not only the characteristics of the processes (heuristics) themselves, but also the functional characteristics of the environments that control outcome-effectiveness.

Studies of this kind are feasible now, but generally would require, in addition to ordinary research investigator time, research assistants, and access to powerful computing equipment via medium-sized

computers or networks linked to supercomputers. NSF (in general, and also in conjunction with specific DMS programs) could clearly be of major importance in making these facilities possible.

(3) Communicative complexity.

DMS is a multi-disciplinary area. The need is for real hybridization not just a collection of disciplines. Interdisciplinary research, however, requires participants who have reasonable familiarity with one another's fields, including not only matters of the language in which problems are discussed, but also the goals and "world views" of the various practitioners. Such familiarity is not easily acquired across disciplinary boundaries. Encouragement notwithstanding, it is difficult for researchers from non-overlapping specialities to come up with interdisciplinary proposals without some preliminary interaction.

Two ways to foster such speculative interaction are (1) to develop support (i.e., for released time, administrative costs, etc.) for potentially interdisciplinary groups at campuses where the right mix of people already exists, but where no mechanisms exist to make it easy for such people to become acquainted and (2) to fund postdoctoral appointments or extended visits of DMS researchers from one field to laboratories where research from some other DMS field is being done. In both cases the intent is to make access to people in other fields easier than is currently the case.

Meaningful research along many of the lines reported in the workshop will not be quick. Much of it requires multiple stages, development of relationships with external organizations as sites, and specialized equipment. A 3-5 year time frame is realistic in many cases. Therefore, it is recommended that, for research involving operating systems in the real world, long-term support be sought that recognizes the special nature of the relationships required between researchers and the organizations being studied.

Small workshops focused on priority cross-disciplinary issues have demonstrated their effectiveness. It is recommended that a DMS workshop similar to this one be held on an annual or other periodic basis.

## APPENDIX I

## Workshop Participants

Participants in the Dallas workshop were:

Frank Bass, University of Texas at Dallas;  
Alfred Blumstein, Carnegie-Mellon University;  
Emilio Casetti, Ohio State University;  
Michael Cohen, University of Michigan;  
Rudolph Drenick, Polytechnic Institute of New York;  
Hillel Einhorn, University of Chicago;  
Peter Farquhar, Carnegie-Mellon University;  
Gregory Fischer, Carnegie-Mellon University;  
Kenneth Hammond, University of Colorado;  
George Huber, University of Texas at Austin;  
Richard Larson, M.I.T.;  
Alexander Levis, M.I.T.;  
John Little, M.I.T.;  
Lola Lopes, University of Wisconsin;  
Thomas Magnanti, M.I.T.;  
Trudi Miller, National Science Foundation;  
Laurence Moore, Virginia Polytechnic Institute and State University;  
William Pierskalla, University of Pennsylvania;  
Andrew Sage, University of Virginia;  
Subrata Sen, Yale University;  
Randolph Simpson, Office of Naval Research;  
Paul Slovic, Decision Research;  
Gerald Thompson, Carnegie-Mellon University;  
Robert Thrall, Rice University;  
Edison Tse, Stanford University;  
Willard Vaughn, Office of Naval Research;  
John Warfield; Andrew Whinston, Purdue University;  
Robert Winkler, Duke University;  
Detlof von Winterfeldt, University of Southern California;  
Lofti Zadeh, University of California, Berkeley.

## APPENDIX II

STATEMENT OF THE NATIONAL SCIENCE FOUNDATION SUBPANEL FOR THE DECISION AND  
MANAGEMENT SCIENCE PROGRAM

December 1983

Members of the Subpanel for the Decision and Management Science agree that the Program should emphasize basic research to develop a theoretical and empirical science of managerial and operational processes. Proposals that explore such common managerial processes as planning, control, selection, monitoring and evaluation; and such common operational processes as congestion, distribution, screening, and market responses, are encouraged. The panel is particularly interested in developing a body of knowledge that incorporates the social and behavioral aspects of these processes.

In the conduct of research, processes will typically be characterized by mathematical, logical, and statistical models. These models will be derived from empirical observation, or from theory that is subject to empirical verification. Empirical analyses should be pursued in some operational context, but the emphasis should be on theories, findings and methods that are generalizable to other contexts.

Thus, the body of research supported by the program should possess generality, be based on empirical observation or be subject to empirical validation, and incorporate social and behavioural aspects. Processes should be characterized by models that are tested in operational contexts. Even though an individual project may not have all these characteristics, its evolution toward this end must be clear.



THE UNIVERSITY OF TEXAS AT DALLAS

BOX 830668 RICHARDSON, TEXAS 75083-0668 (214) 690-2744

THE UNIVERSITY OF TEXAS SYSTEM  
EUGENE McDERMOTT PROFESSOR OF MANAGEMENT

July 5, 1984

Dr. Otto N. Larsen  
Senior Associate for the  
Social and Behavioral Sciences  
National Science Foundation  
Washington, DC 20550

Dear Dr. Larsen:

We regret that you were unable to attend our workshop on research priorities for decision and management science in Dallas. However, through various media, we will endeavor to make results available for your use in NSF planning.

The major workshop product will be a statement about research priorities that is being compiled by Dr. John D. C. Little, in-coming president of The Institute of Management Sciences (TIMS) and Area Head for the Behavioral and Policy Sciences in the Sloan School of Management at M.I.T. Dr. Little's statement will be sent to you as soon as it is released; it will also be printed in Interfaces, a joint publication of TIMS and ORSA. Parallel statements will appear in IEEE Transactions on Systems, Man, and Cybernetics and other relevant journals and newsletters.

This letter conveys supplementary recommendations about management policies that are consistent with the subject area priorities. These recommendations from the workshop were elaborated through comments by workshop participants and other leaders of the DMS research community on a first draft of this letter. As a former member of the DMS Advisory Panel, I know that these suggestions are compatible with NSF/BBS policies; indeed, many have already been adopted by DMS.

The widely circulated Statement of the Panel for DMS (attached) lies behind both the research priorities that Dr. Little will describe and these recommendations for program management. The features that program management should accommodate are its (1) focus on fundamental or general processes, (2) multi-disciplinary approach, (3) emphasis on research in operational contexts, (4) reliance on large-scale projects, and (5) utilization of the expanding capabilities of computers. Specific recommendations follow in these categories.



Dr. Otto N. Larsen  
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### Focus on Fundamental or General Processes

#### Recommendations:

DMS should support research on fundamental processes, which implies funding proposals that are "high risk" from relevant disciplinary perspectives - a couple of negative reviews should not be allowed to "kill" an otherwise promising project.

Research should rely on formal, especially mathematical, models; and projects that concentrate on formal theory should be encouraged, provided that they are subject to empirical verification in the long run.

Discussion: Too many theories and findings in decision-related fields can be summed up by saying: "It depends." In part, this lack of generality reflects the selection of research projects that will pay-off quickly in applications or, for basic research, in publications. Psychology's emphasis on controlled laboratory experiments also deflect research away from many questions that are fundamental for DMS. In contrast, the most powerful and useful theories of DMS are both abstract and generally applicable: they are mathematical representations of common processes, such as diffusion and propagation, selection and screening, evolution, and homeostatic processes. Also, multi-disciplinary efforts in DMS are most likely to succeed when disciplinary perspectives are translated into mathematical models, so that assumptions can be debated in the common language of science. To emulate these past successes, new research supported by DMS should employ formal models and address fundamental questions, even though some reviewers may favor narrowing inquiries to maximize internal validity and short-term pay-off. Also, the "escape clause" in the Panel Statement should be used to encourage theoretical research that concentrates on "first generation" (new, potentially high-payoff) models, provided that these abstractions are subject to empirical verification in the long run.

### Multi-Disciplinary Approach

#### Recommendations:

More than disciplinary programs, DMS should support conferences to aggregate and evaluate alternative disciplinary approaches to fundamental problems. Specifically, (a) the DMS panel should guide the planning of one Dallas-type workshop per year, and (b) the Program should encourage investigator-initiated conference proposals.

The DMS panel should also consider other mechanisms for providing "seed money" for promising cross-disciplinary efforts, including tutorials, multi-disciplinary reviews of research, post-doctoral fellowships in other disciplines, and cooperation with the Engineering Directorate in support of interdisciplinary centers.

Discussion: Interdisciplinary departments for "management" have emerged in universities, and their interest in science (as well as in practice) is represented by TIMS. Because generic management departments have student constituencies and control over faculty tenure, they provide a promising university base for scholarship in DMS. However, the focus of most management programs is on practice not science, and related professional associations (those grounded on electrical engineering and the social and behavioral sciences) have little overlap with TIMS, which is grounded on operations research and management science (OR/MS). Thus, especially in the short run, DMS should encourage cross-fertilization among disciplines.

### Emphasis on Research in Operational Contexts

#### Recommendations:

DMS should encourage projects that develop and verify general theories in real-world contexts, especially in collaboration with practitioners. University/industry projects to collect, update, and redefine data elements for calibrating and validating fundamental theories should have high priority.

Discussion: On the face of it, the DMS Panel's emphasis on both generality and operational contexts seems contradictory: How can models be abstract and general when organizations are diverse and solutions are tailored to specific conditions? In fact, the flagship theories (applications) of DMS are known to possess generality and to highlight the critical features of apparently diverse problems in varied settings. Generality and applicability combine when abstract models capture fundamental processes. Moreover, a current weakness of research in DMS is its focus on "fourth generation" problems, or refining existing models, to the neglect of "first generation" problems, or developing new models. Thus DMS should encourage scientists to study phenomena in real world contexts. Collaboration between practitioners and scientists in addressing pressing issues, such as the opportunities (and difficulties) of technological advancement, will facilitate the design, estimation, and validation of new models. Of special importance are practitioner/university efforts to collect, update, and redefine data elements as models evolve over time.



## Reliance on Large Scale Projects

### Recommendations:

DMS should encourage but not pay for the full cost of large scale, multi-disciplinary, collaborative projects between practitioners and scholars in operational contexts. Opportunities for joint funding with other Federal agencies, NSF programs for university/industry cooperation, and private sector organizations should be explored. For especially promising proposals, DMS should also invest considerably more than the average award amount in multi-investigator, empirical research.

Discussion: Past breakthroughs in DMS have generally involved multi-disciplinary teams working with practitioners in operational contexts to solve real problems, often with funding from the Defense Department. Large scale multi-disciplinary research on fundamental aspects of defense-related problems continues to receive support, primarily through the Office of Naval Research (ONR). Parellel efforts to address markets, strategic and long range planning, management controls, distributed decision-making, personnel requirements, and operational processes in domestic settings should be encouraged by DMS. However, DMS should seek joint funding for these multi-purpose, multi-actor efforts, not pay full costs. Also, because most successful models in DMS apply equally well to military and domestic processes, close coordination should be maintained with ONR and other Defense Department programs that support fundamental research.

## Utilization of the Expanding Capabilities of Computers

### Recommendations:

In collaboration with related NSF programs, DMS should encourage research on how humans can best employ the unfolding capabilities of computers. Relevant contexts range from using supercomputers to find numerical solutions for operational problems (machine-dominated decision making) to the use of expert systems interactively in decision support systems (man-dominated decision making).

Discussion: The decision-related sciences increasingly employ and address man-made systems. Agreement with NSF on the proper approach to constructing, validating, and applying scientific theories about man-made systems has yet to be achieved. In the interim, the interests of leaders in the three principal sub-fields of DMS (i.e., OR/MS, electrical engineering, and the social and behavioral sciences)

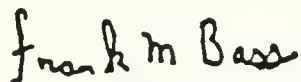
Dr. Otto N. Larsen

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are converging: all want to conduct multi-disciplinary research on human/machine combinations for decision making. Especially in this subject area, behavioral science methods that stress internal validity should be questioned and theory-driven research should be encouraged.

As I indicated above, these recommendations for DMS Program management were developed in consultation with representatives of the three broad disciplinary constituencies of DMS -- OR/MS, electrical engineering, and the social and behavioral sciences. I would be happy to bring leaders of these communities who work in the field of DMS together with you and others to talk about program management and research priorities.

Sincerely,

A handwritten signature in cursive script that reads "Frank M Bass".

Frank M. Bass  
The University of Texas System  
Eugene McDermott Professor of Management

/eas

Ask Not What Psychology Can Do for DMS;  
Ask What DMS Can Do for Psychology

Kenneth R. Hammond

Center for Research on Judgment and Policy  
Institute of Cognitive Science  
University of Colorado  
Boulder, Colorado

Decision and Management Science Workshop  
The University of Texas at Dallas  
April 26, 27, 1984

Ask Not What Psychology Can Do for DMS;  
Ask What DMS Can Do for Psychology

According to the directive from Alfred Blumstein each panelist "is expected to put on the table some specific suggestions of high payoff opportunities whereby his discipline can contribute to the DMS program objectives." I will try to meet that expectation by first indicating what I believe would be the current consensus view of psychologists regarding potential contributions from psychology to the DMS program. Because of my personal conviction that the NSF/DMS program can do more for psychology than psychology can do for it, in the second part of my paper I will indicate what I believe the contributions to psychology from DMS might be.

Potential Contributions from Psychology (Consensus View)

From the Literature of Indirect Comparisons

The most salient line of research in the field of judgment and decision making during the 1970's involved the comparison between a person's efforts to make rational choices with the choices produced by a rational model, for example, Bayes' Theorem. This line of research addressed the question of the rationality of information processing, particularly with regard to choice behavior.

The main result of this research (carried out largely by Daniel Kahneman, Paul Slovic, Amos Tversky, and their colleagues) was to demolish the descriptive validity of the expected utility model, apparently much to the surprise of economists, philosophers, and some psychologists. These results have received wide recognition among all three disciplines. For example, Kenneth Arrow (1982), in a paper entitled "Risk Perception in Psychology and Economics" notes that

The rationality or irrationality of choice has become a leading interest of the branch of psychology called 'cognitive psychology'. . . .in the last twenty years it has become a major field of psychological research, in contrast to earlier work which tended to emphasize either the role of emotions or mechanistic models for learning. (p. 1)

Arrow then indicates that "there has been renewed testing of expected-utility theory; one striking result has been the series of stunning experiments on the so-called 'preference reversal' phenomenon by Lichtenstein and Slovic (1971)" (p. 2). After drawing a series of analogies with various phenomena in the field of economics, Arrow points to the work of Tversky and Kahneman (1974, 1981) in which he notes that "several heuristic devices [have been discovered] by which individuals form cognitive judgements and while each has useful properties, each can also lead to biases in judgement" (p. 5). He then shows how a specific example from their work "typifies very precisely the excessive reaction to current information which seems to characterize all the securities and futures markets" (p. 5). Arrow concludes his paper by saying, "I hope to have made a case for the proposition that an important class of intertemporal markets shows systematic deviations from individual rational behavior and that these deviations are consonant with evidence from very different sources collected by psychologists" (p. 8). Attention of this sort from a Nobel Laureate in the discipline of economics to research in psychology certainly falls in the category of a rare event.

A second consequence of the work on indirect comparisons has been to encourage economists to turn to laboratory experiments (see especially Smith, 1982; Plott, 1979). Apparently inspired by the work of Kahneman and Tversky there now appears to be an established trend among economists toward the use of the general laboratory research paradigm commonly used by psychologists.

Research opportunities. The rich assortment of findings produced by psychologists regarding the irrationality of choice behavior provides many research opportunities of which I list only four general ones:

1. Discover which of the heuristics identified by psychologists will be used when, why, how, in which operational context.
2. Discover the costs of nonrational choice behavior in various operational contexts.
3. Discover remedies for nonrational choice behavior in operational contexts.
4. Discover the benefits of rational choice behavior, if it can be induced.

#### From the Literature of Direct Comparisons

A second line of research, somewhat older than the one above, directly compares the judgments of a person with an empirical criterion, and thus evaluates the empirical accuracy of judgments and/or predictions. There are two main results from this research effort:

1. Simple linear models predict most task outcomes very accurately (better than expert's judgment).
2. Simple additive (or weighted sum) models predict judgments very accurately.

The main conclusion from this research is that linear models, rather than a person, should be given the information necessary for a prediction, even if the person is an expert.

Research opportunities. This line of research also provides many opportunities for research within the Decision and Management Science context. For example,

1. Discover when, how, why, which operational contexts make the simple linear model inappropriate.
2. Discover how to improve the accuracy of judgments (especially among experts) in operational contexts.
3. Make direct comparisons of the accuracy of analytical, quasirational, and intuitive judgments produced by experts in various operational contexts.
4. Discover the differential costs and benefits of the use of these modes of cognition in operational contexts.

From the Literature of Problem Solving, Memory,  
and Text Comprehension Research

The main topics considered by researchers in this field include: (a) the sequences of heuristics used in problem solving, and (b) the role of

memory in the course of problem solving, and the ability to comprehend written material. One main result is that the course of problem solving can be simulated by a computer program. One main conclusion is that people (especially experts) store large amounts of information and use it effectively, often in the form of "shortcuts."

This field of research has also produced a rich variety of research opportunities for DMS. One salient opportunity is provided by the strange contradiction between the results achieved by judgment and decision making researchers and problem solving researchers regarding experts. That is, the judgment and decision making researchers generally find that experts fail, whereas the problem solving researchers generally find that experts are superior problem solvers. What causes the difference between these two conclusions? Where does the truth lie? It is hard to think of a more important problem for Decision and Management Science.

#### Results Common to All Three Research Areas

All three research areas have addressed the problem of the generality of results over task conditions. The main result is clear: Results from psychological research are highly task dependent; results do not generalize over tasks. That leads to a very important conclusion reached by all reviewers in the Annual Review of Psychology since 1977, namely "caveat emptor"! Researchers in the field of Decision and Management Science who wish to build upon the work of psychologists must be cautious; the result that indicates that people do this or that is, in all likelihood, restricted to the laboratory circumstances in which it was generated. For example, almost every result from psychological research is information restricted and time-restricted. If your "operational context" is one in which the actors will want to seek information and have the time



(hours, days or weeks) to do it, then the results that imply that people improperly process information are not likely to apply; at least, the burden of proof lies with those who wish to apply the results.

The primary research opportunity here is to find the road to the discovery of enduring generalizations. Can the Decision and Management Science Program of the National Science Foundation help with this problem? I hope so. And that hope brings me to my personal view of the contributions of Decision and Management Science Program to psychology.

#### Contributions to Psychology from DMS

First let me cite the DMS policy statement:

Thus, the body of research supported by the program should possess generality, be based on empirical validation, and incorporate social and behavioral aspects. Processes should be characterized by models that are tested in operational contexts. Even though an individual project may not have all these characteristics, its evolution toward this end must be clear.

This policy statement raises a number of important questions. Consider first the requirement of "generality." Does psychological research produce results that are general over a range of tasks? The answer to this must be "no." Can psychological research produce generalizable results? The answer to this, I believe, must also be "no." Why not? Because psychologists do what they can do within a traditional research paradigm. That is, they must carry out (literally) cheap research projects that involve minimal information displays, free subject time, problems cut to fit the college lecture hour, problems tailored to a methodology (uncritically inherited from agriculture) that inherently prevents

generalization, and the fact that promotion within psychology departments is based on quantity of publication, a practice which encourages the maintenance of cheap research.

A second set of questions is raised by the prerequisite of "operational context." Does the typical psychology laboratory study (complex problems cut to fit the sophomores 50-minute hour) meet the "operational context" criterion? No. Is there "evolution" toward research in an "operational context"? No; indeed, the trend is in the reverse direction as may be seen in the enthusiasm expressed by economists for laboratory research in the style of psychologists. My prediction here is that there will be an uncritical examination of the nature of psychological research, and that as a result, economists will reinvent the square wheel. I have considerable enthusiasm for Vernon Smith's (1982) suggestion regarding what he calls "parallelism," by which he (roughly) means the effort to find convergent validity of results from laboratory and "real world" research. But he should be made aware of the thorough treatment of the concept of convergent validity by the psychologists Campbell and Fiske (1959) who provide a method for examining both the convergent and discriminant validity of various measures.

### Barriers to Generalizable Research

In my view, the following barriers to generalizable research in psychology exist:

1. Most psychologists believe that research should not be carried out in an operational setting. Achieving rigor is more important to experimental psychologists than achieving complexity.

2. Most psychologists have a trained incapacity for research in an operational context. That is, most psychologists would not know how to do this type of research even if they wanted to.
3. Most psychologists must work in nonoperational contexts (that is the psychological laboratory); they have no choice if they wish to be promoted.

As a result of these barriers, results are exactly what we would expect them to be, namely, task dependent; enduring generalizations cannot be achieved under these conditions. At this point you may be wondering whether my personal views are shared by other psychologists. They are in fact shared by very few psychologists, but if the number is small, the quality is very high. One of psychology's most outstanding scholars, Paul Meehl, recently described the course of research efforts in psychology in this way:

There is a period of enthusiasm about a new theory, a period of attempted application to several fact domains, a period of disillusionment as the negative data come in, a growing bafflement about inconsistent and unreplicable empirical results, multiple resort to ad hoc excuses, and then finally people just lose interest in the thing and pursue other endeavors. (1978, p. 807)

Finally, Meehl (1978) observes that "the enterprise shows a disturbing absence of that cumulative character that is so impressive in disciplines like astronomy, molecular biology, and genetics" (p. 807).

### The Role of DMS

The DMS policy statement, if taken seriously, offers several research opportunities to psychologists, if DMS is indeed serious in its requirements for generalizable research in an operational context. I offer below five examples of advances that psychological research will never make unless they are carried out within the DMS policy statement.

1. Understanding decision making over time
2. Understanding expert judgment
3. Understanding the tension between logical consistency ("truth") and accuracy
4. Understanding group decision making among adults
5. Understanding the nature of causal inference

DMS/NSF can help break the tradition of "knowledge on the cheap" by requiring research methods that explicitly address the matter of generality, and by providing the dollar support for projects that will incorporate such methods. DMS can also help break the tradition of noncumulative research findings by requiring research to be done in an "operational context" or simulation thereof, and by providing dollar support for such research.

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## HIGH PAYOFF OPPORTUNITIES IN SYSTEMS THEORY FOR THE DMS OBJECTIVES

By

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Decision making is a dynamic multi-stage iterative process. Within it are sequences of "convergent" and "divergent" subprocesses that take the decision maker from the recognition that he has a decision problem to the implementation of his preferred choice. Fig. 1, shows a model of the decision process. The specific breakdown into different distinct phases is not unique, but it does allow us to distinguish the different divergent and convergent processes. Each divergence provides a scope for the following convergence; each convergence provides a focus for the following divergence. Nearly all research in the area of decision theory has been focusing on the choice process. While this is an important process, a thorough understanding of decision making so as to allow us to make "better" decisions requires a systematic study on the overall dynamic process of decision making, not just the choice process alone.

The lack of systematic research activity in the subprocesses prior to the choice process may be due to the fact that no convenient mathematical tool is available to allow us to study them; whereas choice theory can be formulated and studied under the statistical decision theory or stochastic control theory framework. One can broadly sub-divide the decision process into two main subprocesses: generation

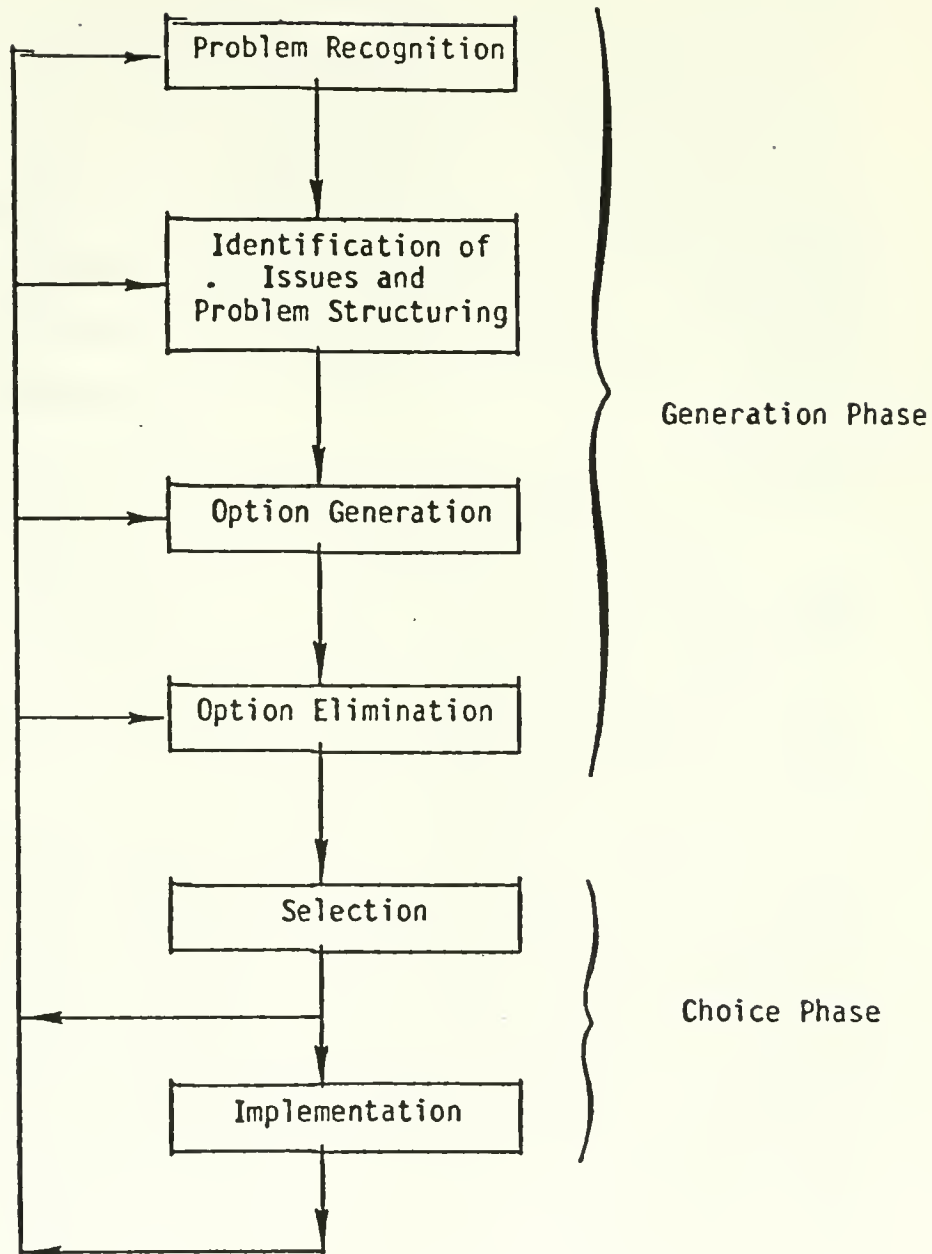


Fig. 1. A multi-stage process of decision making

and choice. The kind of activities that a decision maker is involved in each of these two main subprocesses are substantially different. In the generation process, the main goal is to create new alternatives--this is a divergent process. The main tools would be association, reasoning via analogy, adapting guidelines based on past experience, or expert knowledge. We shall call these intelligent activities. The choice process, on the other hand, requires focusing on the alternatives being generated, carefully studying the implications of these alternatives via analysis, and ranking these alternatives via certain criteria in order to recommend the final choice--this is a convergent process. The main tools would be a whole host of mathematical analyses including statistical decision theory, optimization, control and many others. We shall call these analytical activities.

In the past, we have been mainly researching on the analytical aspect of decision making because analytical tools have a long history of development; we are very familiar with them and we know how to use them well. The study of the intelligent activity in decision making has emerged in the fields of psychology and artificial intelligence (commonly referred to as AI). The recent development of rule-based expert systems within the AI discipline seems to offer a new efficient tool to allow us to study the generation process within decision making. [1] Recent developments in computer hardware will provide new dimension in decision making research. The ease in man-computer interaction through advancements in interface technology will offer a new perspective in research methodology. I perceive that the high payoff and exciting opportunities are surrounding the integration of new



technological development in expert systems and computer hardware with the familiar analytical methods to provide a coherent model of the decision making process that can be studied and validated in real operational settings. Moreover, such investigations will lead to the improvement on the decision process. In the following, I shall be a little more specific on the directions which I believe will lead to my vision beyond that long tunnel.

A new framework needs to be developed to allow us to handle the generation and choice paradigm--in particular, an uncertainty representation framework which is flexible enough to allow us to deal with ignorance as would be introduced in the generation process. Consider the example that we hear the noise of the jet engine which allows us to form a subjective probability of 0.9 that a jet plane has just flown by. The necessity to determine what type of a jet plane it is does not seem to be important. However, as the decision process unfolds, it may become apparant later that the knowledge of what type of plane, in particular whether it is DC-10 or 747, that had flown by is crucial in order to make the right choice. If we now generate the hypothesis space from

$$H = \{\text{plane, others}\}$$

to

$$H_r = \{747, \text{DC-10, other jet plane, others}\}$$

then how should the probability measure  $\text{Prob}\{\text{plane}\} = 0.9$  ,  $\text{Prob}\{\text{others}\} = 0.1$  "propagate" to  $H_r$  . Basic probability theory requires us to have knowledge on conditional probability

$$\text{Prob}\{747/\text{plane}\}, \text{etc.}$$

However, this may be too much to require since such knowledge may not come by easily without detailed statistical information on the number of planes in each type, flight schedules of all the planes, weather conditions everywhere that may cause delay, etc. To reflect ignorance, we may want to assign 0.9 to the set {747, DC-10, other jet planes} which is a subset of  $H_r$ . If additional evidence can be collected that can reduce our ignorance, we would certainly do so; however, if we are forced to make a choice, then we have to perform reasoning based on the uncertainty representation on  $H_r$  which incorporates the notion of ignorance. Now the uncertainty representation on  $H_r$  is not Bayesian, but rather a representation introduced by Dempster-Shafer. [2] Another uncertainty representation framework which accounts for semantic ambiguity is the possibility model of Zadeh. [3] This model has recently been widely discussed in the literature, but the role of such representation to decision making is not well studied.

While statistical decision theory is an appropriate theory based on Bayesian representation of uncertainty, new decision theories need to be developed based on different representations of uncertainty which incorporates ignorance and/or semantic ambiguities. How do we characterize risk under such representations of uncertainty? Can these models account for the seemingly "irrational" behavior of the decision maker?

Another important research direction is the development of a new tool, or a methodology that can enhance our understanding of the dynamic iteration between generation and choice processes. No coherent approach

exists in the literature because the two processes call for completely different reasoning procedures. I believe that such a research direction requires an integration of AI's expert systems technology, user interface technology, system analysis and human interaction. Just like linear programming is a very useful tool in the study of operations research, the tool to be developed that will be useful in the study of the decision process is an interactive simulation and expert environment.

We are all familiar with simulation models where a set of differential or difference equations is used to model the evolution of dynamic processes. With the advance in interactive computing, we can perceive a simulation environment where the equations used in the simulation are parameterized. Then the user can easily perform "what if" experiments to study the response of the systems dynamic to parameter variations by changing the parameter values in an interactive manner. With the advancement in expert systems technology, we can introduce rules for selection of the equation structure to be simulated. [4] For example, we can impose the following rules based on empirical evidences:

- An individual has vision → options will be generated to increase chance of fulfillment of vision
- An individual has little responsibility → less concerned with risk

Now, combining these rules

- An individual has a certain vision + has little responsibility → generating options that can allow him to increase his chance of goal fulfillment while not much concerning about the risk he will have to undertake.

The conclusion may be represented by a model

$$\max\{\text{Prob}\{\text{goal fulfillment}\}/\text{resource and timing constraints}\}$$

or a particular analytical model which can be represented by a set of dynamic difference/differential equations.

The interactive simulation and expert environment would allow the user to change rules, change model and parameters interactively during the simulation. Explanation capability is provided to allow the user to trace through the rationale for using certain models in the simulation. This establishes a dialogue between a user and the computer. With the computer taking care of the analytical manipulation, the user can experiment with the implications of new options that will induce changes in model structure in the analysis phase. The simulation environment can also be used to help us to discover new issues that may have been overlooked, which will redirect our search for new options, or modification of our old options in order to address such issues. Therefore, I perceive that such a tool can be extremely useful in developing a decision making model in order to obtain deeper understanding of the process, [5] as well as an integral part of a man-machine decision system that will enhance the process of decision making.

Analysis of non-classical dynamic systems which are representable by a mix of rule base and differential/difference equations triggered by discrete events and/or deliberate action can also bring deeper understanding to the decision process. The recent study on Discrete Event Dynamic Systems is an example of such types. [6] The research was motivated by a large class of dynamic systems which are driven by occurrence of discrete events--many opportunity driven decision making processes can be characterized as discrete event dynamic processes. Studies of these types of systems have been done in the context of queueing network. The application of it to the context of decision making would be worthwhile.

Another exciting research area is the strategic aspect of decision making in a "manipulative" environment. [7] The basic notion is that the decision making of an individual is done within an environment where there are many other decision makers who act simultaneously and/or responsively. However, there are constraints, which can be natural or artificial, that limit the options of each decision maker. In a stable environment, we can reformulate it as a game model. In realistic situation, such environment is subject to change even though the time constant of such change is very long. Many times, the change is due to strategic behavior of decision makers who consciously seek ways to improve their strategic positions by manipulating the environment such that the resulting "rules of the game" are to their favor. Examples are union movement, stockpiling, emerging companies and many others. If one examines the strategic behavior of decision makers, one may find that the awareness of other players plays an important role. The concept of

asymmetric relationships which leads to a limitation of one's options is the key to such analysis. Applying a tool like the interactive simulation and expert environment to the study of strategic behavior will provide further insight in the science of decision making.

As an example consider a manufacturing company which is manufacturing consumer goods in a growing market. The manufacturing of the goods depends crucially on the availability of certain input material which is produced by a handful number of firms.

Let the production function be represented by

$$\hat{Q}_t = f\left(\sum_{i=1}^{\lambda} Q_{it}\right) \quad (1)$$

where  $\hat{Q}_t$  is the output production at time  $t$ ,  $Q_{it}$  is the quantity of supply from the  $i^{\text{th}}$  supplier at time  $t$  and  $\lambda$  = number of suppliers. With production cost given by  $C(\hat{Q}_t)$ , and different supply prices  $P_{it}$ ,  $i=1, \dots, \lambda$ , profit at time  $t$  is

$$\pi_t = \hat{P}_t Q_t^a - \sum_{i=1}^{\lambda} P_{it} Q_{it} - C\left(f\left(\sum_{i=1}^{\lambda} Q_{it}\right)\right) \quad (2)$$

where  $\hat{P}_t$  is the price and  $Q_t^a$  is the actual quantity of goods sold at time  $t$ .

At the beginning of period  $t$ , the firm secures a standing order of  $Q_t^S$  for the goods, and if the production  $\hat{Q}_t$  is lower than  $Q_t^S$ , a measure of reputation of the company to be able to deliver,  $R_t$ , is "modified" via a model

$$R_t = R_{t-1} + \lambda^-(\hat{Q}_t - Q_t^S) \quad \lambda^-(x) = \begin{cases} 0 & \text{if } x \geq 0 \\ \lambda \cdot x & \text{if } x < 0 \end{cases} \quad (3)$$

The actual quantity sold is modeled as

$$Q_t^a = \begin{cases} \hat{Q}_t & \text{if } \hat{Q}_t < Q_t^S \\ Q_t^S + \phi(\hat{P}_t)(\hat{Q}_t - Q_t^S) & ; 0 \leq \phi(\hat{P}_t) \leq 1 \end{cases} \quad (4)$$

We further assume that

$$Q_t^S = Q_t^S(R_{t-1}, \hat{P}_t, Q_t^a, Q_{t-1}^S) \quad (5)$$

This reflects that, due to expectation of consumers, capability of obtaining standing orders depends on historical "average" delivery performance.

Given the profit model described by (1)-(5), and if  $\{Q_{it}\}$  are within the "control" of the company, then the rational decision maker will always buy from the supplier who has the lowest price; and depending on the objective function defined in terms of total discounted profit, an "optimal" purchasing plan can be determined for a specified time period. However, in realistic situations,  $\{Q_{it}\}$  are not controlled by the company. This may be due to the fact that supply is limited and there is more than one company that wants to buy the same material, either to produce the same product or to produce a different product. Thus, the company should conceive the situation where it is imbedded in an environment having different decision makers engaging in negotiation for allocation of supply.

A strategic move of the company is to engage in a contractual relationship with one or more suppliers in order to ensure a certain

level of supplier so as to maintain a certain level of reputation measure  $R_t$ . This can be achieved in different ways: e.g. merging (buying all or a fraction of a big supplier), diversification (maintaining business relationships with more than one supplier, sometimes paying a premium to maintain a relationship), exclusivity relationship, and many others. For each of the potential strategies to be adopted, the company will result a profit model different from the one represented by (1)-(5) in that the cost structure is different, the models for  $Q_t^a$  and  $Q_t^s$  are different due to consumer's expectation. Moreover, the plausible variations in supply from each supplier will be different depending on the specific strategy adopted.

The interactive simulation and expert environment can be a useful operational tool in studying the effectiveness of a specific strategy to be adopted. With a different strategy, one can select an appropriate profit model for simulation. Rules can be developed as guides to which model structure to use as well as what "range" of sensitivity studies to be carried out under a specific strategic option. The environment also allows experimentation with many different modeling representations (one may want to change the reputation model, actual sale model, etc.) together with many analytical studies based on each representation. Such studies will not only shed light on the understanding of strategic behavior, they will also enhance the process of strategic decision making.



NOTES

- [1] "Artificial Intelligence (AI) is the part of computer science concerned with designing intelligent computer systems, that is, systems that exhibit the characteristics we associate with intelligence in human behavior--understanding language, learning, solving problems, and so on" quoted from The Handbook of Artificial Intelligence, Vol. 1, edited by Arron Barr and Edward A. Feigenbaum, published by William Kaufmann, Inc., 1981. Expert systems, or knowledge engineering, has emerged as specific applications of AI techniques. Expert systems can be viewed as intermediaries between experts, who interact with the systems in "knowledge acquisition" mode, and users who interact with the systems in "consultation" mode. Expert systems also provide explanation capability, both to make the consultation more acceptable to the user and to help the expert find errors in the system's reasoning when they occur.
- [2] Dempster's uncertainty representation model was developed in the paper by Dempster, A.P., "Upper and Lower Probabilities Induced by Multivalued Mapping," Ann. Math. Statist, 38, 1967, pp. 325-339. A formal mathematical representation of the model and further existensions of Dempster's model were advanced by G. Shafer, A Mathematical Theory of Evidence, Princeton University Press, 1976. Such representation allows the treatment of ignorance through refinement.
- [3] L. A. Zadeh developed the possibility theory in representing uncertainty that may contain semantic ambiguity. This discussion can be found in L.A. Zadeh, "Fuzzy sets as a basis for theory of possibility," Fuzzy Sets and Systems 1, 1978, pp. 3-28; L.A. Zadeh, "Possibility Theory and Soft Data Analysis," Mathematical Frontiers of Social and Policy Sciences, L. Cobb and R.M. Thrall, eds.; Boulder: Westview Press, 1981.
- [4] A Simulation and Expert Environment (SEE) is now being developed within the Decision Systems Laboratory, Department of Engineering-Economic Systems, Stanford University. SEE is an environment in which one can describe problems both by numerical simulation models and by encoding expert knowledge about the problem. The environment had been used to study a simple team model where the emphasis was on the dynamic behavior of team cooperation in the situation where team members may have biased belief about their contribution to team performance. See P. Lounamaa and E. Tse, "Dynamic Model of Team Behavior," Proc: American Control Conference, San Francisco, CA 1983, 1340-1345.
- [5] Research on the use of SEE in studying organization and oligopoly problems, especially the influence of risk behavior in implicit or explicit cooperation among agents, is carried out by P. Lounamaa under the supervision of E. Tse and J. March at Stanford University.

- [6] Discrete event dynamic systems is an active research activity headed by Y.C. Ho at Harvard University. See Y.C. Ho and C. Cassadras, "A New Approach to the Analysis of Discrete Event Dynamic Systems," Automatica, Vol. 19, #2, pp. 149-167, 1983. A forth coming paper by Y.C. Ho, "On the Perturbation Analysis of Discrete Event Dynamic Systems," Journal of Optimization Theory and Application, Special G. Leitmann's Birthday Volume, scheduled to appear in 1985, gives a detailed account of the research activities.
- [7] A. Mir and E. Tse at Stanford University are doing research in modelling strategic decision behavior in a manipulated environment. A Ph.D. thesis by A. Mir supervised by E. Tse, "The Strategic Implications of Varying Environments: Aspects of Decision-Making under Instability, (Oct. 1983) developed a logical and systematic framework for strategic behavior. Some of these results will be submitted for publication.

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