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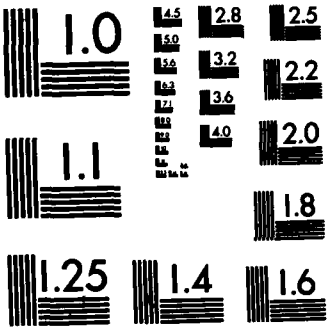

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POTENTIAL RESEARCH TOPICS IN DECISION ANALYSIS

by

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The paper "Research Directions in Decision Making Under Uncertainty" by Robert L. Winkler provides a comprehensive overview of challenging research areas for decision making under uncertainty. Hence, rather than try to extend the list of research areas identified, this note will attempt to embellish some that I feel are particularly important. In these areas, I feel the value of systematic research is particularly high.

For convenience, the discussion will be organized under the four research categories identified by Winkler with a couple of suggestions following in an "implementation research" category. The reader will note however that many of the suggested topics actually relate to more than one research category.

Model Formulation

The key to providing insight and assistance on many decision problems is a useful problem formulation. Basically, the problem is characterized by two lists: a list of alternatives and a list of objectives with their associated measures or attributes. Devices to promote creativity in generating these lists, which are essentially pulled from the minds of decision makers and others concerned about the problems, are certainly needed. But the interplay between the two lists seems particularly worthy of investigation, perhaps simply because it is so often not explicitly considered in the formulation of decision problems. Knowing the objectives might suggest alternatives that do not initially come to mind. Knowing measures clarifies the meanings of the objectives. Insights about which alternatives the decision maker feels are "good alternatives" suggests objectives that may be appropriate. Pushing the interrelationships between alternatives and objectives could lead to a richer and more appropriate specification of each.

Any analysis of a decision necessarily bounds the problem being considered. This is done for practical reasons related to the cost and perceived additional insights of a more involved analysis. However, in actuality, any particular decision problem is likely interwined with many others. The action chosen in one decision problem, as well as the

direct consequences of that action, could foreclose or open up options in other decision problems. They could also influence the likely consequences of any particular alternative chosen in other decision problems. Research is needed to better indicate how to incorporate such indirect decision interrelationships into a reasonable problem structure. One manner to include some of these major interrelationships between decision problems might be to create new attributes to measure objectives concerning resilience, flexibility, and adaptability. These objectives would relate to the ability of the decision makers to minimize the likelihood of "bad consequences" and maximize the likelihood of "good consequences" with either automatic or designed actions to meet future (often unanticipated) contingencies.

Modeling Uncertainty

Often a debate rages about the possible consequences of an alternative. Different experts are forecasting very different consequences and the decision makers and the public are necessarily confused. For important problems, it might be desirable to have an intensive conference with experts of diverse opinions either to forge a "consensus" estimate of consequences or to identify the "estimates of minimum variability" given current knowledge. These experts would have access to all past data, forecasting models, physical relationships, and whatever they needed to provide their estimates. Decision analysts would work with the experts to articulate the assumptions on which their forecasts were based and the grounds for those assumptions. Fundamental disagreements between experts could be aired. Some might be resolved and others partially resolved. Any such resolution would reduce the discrepancies between the judgments of various experts. In addition, the fundamental information on which the disagreements rested should be identified. One research topic would be to develop administrative procedures to conduct such "decision analysis conferences." In addition, applied research on all phases of such conference's is needed. Problems where an expert conference might be useful include future sales of a product, future prices for raw materials, or the health effects given various pollution levels.

A related research area would be to develop "official" estimates of the consequences of various situations. This would be especially important for generic problems where the consequences are common to many specific problems. For example, one might gather numerous experts and decision analysts to obtain an official probability density function for the health effects given exposures to various pollution levels of an air pollutant. Until a better probability distribution was found, the existing distribution could be used for analyzing any number of decision problems involving the consequences of that pollutant. This should both reduce the repetition of work and provide for better analyses. Such "official" probability distributions would typically relate to proxy variables that could be scientifically measured and that represented means to consequences of fundamental importance. For example, the proxy variable could be the pollutant level in parts per million and the fundamental attribute would be the health effects that result from such exposure.

Modeling Preferences

A useful research project could develop a public utility function for a problem felt to be important to the public. This research project might involve a theoretical part, to develop a reasonable formulation for such a utility function, and an empirical part, to put the theory into practice. The project would provide a learning experience to suggest how such utility functions might be obtained. Even if we could obtain reasonable "public utility functions," such utility functions should only be considered a useful piece of information to assist decision makers with responsibilities for making decisions on behalf of the public. It should not be directly utilized in an analysis to provide the answer to the problem. It is important to keep in mind that the purpose of analysis is to provide insights and not the answers.

Related to the two previous suggested research topics, it would be useful to obtain an official utility function to be used in evaluating certain problems. If the utility function is assessed over attributes of fundamental importance, rather than over proxy variables, the utility function could be used on a wide range of problems. Perhaps the best

example I see of this would be a utility function over two dimensions: government expenditures and loss of statistical lives, due to such causes as automobile accidents, exposure to pollution, and disease. It may be that such a utility function could be designed with a reasonable level of public input. It may also not be necessary to specify a precise utility function, but simply to provide some reasonable bounded "utility functions" which captured the range of values for a particular problem. With the "value of life" problem, it might reduce the likelihood of the persistence of the current situation, where some government programs de facto evaluate a statistical citizen's life at approximately $500,000,000 and other programs evaluate such lives (i.e., our lives) at close to nothing.

The interaction between modeling preferences and model formulation presents a potentially fruitful area for research. A research project to structure carefully the objectives and attributes for a decision problem first and then to assess a utility function before proceeding on other parts of the problem formulation and modeling of uncertainty could be very useful. Basically, the utility function suggests more precisely what it is that is important in the decision problem. This provides invaluable information for identifying, creating, and selecting reasonable alternatives to be considered, and for determining the usefulness of collecting or assessing data on possible consequences. Because the cost of specifying objectives and obtaining a utility function is much less than the cost of obtaining data, especially if expensive experiments are involved, the desirability of pursuing this research project is high.

Modeling Competitive and Group Decisions

Because of the myriad of research opportunities in this area, perhaps the biggest problem is where to begin. It would be useful to utilize the models we now have for competitive and group decisions to gain some insight on the fruitfulness on the various potential directions for further research. A few of the suggestions which I feel are particularly worthy of pursuit are outlined in the two sections above--the ones that relate to public and official probabilities and preferences.

Implementation Research

It would be useful to study partial utilization of decision analysis for problems where it is understood that no complete decision analysis will occur. It can be very worthwhile either to provide a model formulation, to model uncertainties, or to model preferences independent of whether any other steps in the analysis are carried out. Each of these steps can provide significant direction for understanding what the problem is about, for focusing discussion, and for providing a basis for more informal appraisal of alternatives.

Finally, a research project oriented at the implementation phase of decision analysis could provide very useful insights to both researchers and practioners in the area. It would be useful to have a better understanding of how decision analysis is perceived and received by decision makers and members of the public. If one assumes that the purpose of decision analysis is to provide a means to better decisions and hopefully better consequences, a better understanding of how and why decision analysis is or is not used could provide significant insights in ways to improve our techniques, our analyses, our presentations, and consequently our impact.