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Decision science as a by-product of decision-aiding: A practitioner's perspective

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ABSTRACT

Decision aiding practice has stimulated much research on neglected scientific issues. The author spent 40 years alternating between consulting and diverse faculty appointments in management, statistics, economics, psychology, information technology and public policy. His decision science research, with applied decision theory at its conceptual core, is published, often with academic co-authors, in the literatures on decision theory, cognitive psychology, philosophy, and organization design. A number of the author's ideas and insights, grounded in "anecdotal evidence" stemming from over 100 cases, are discussed here. These ideas cover, for example, hybrid judgment, decomposing estimation error ideal judgment, organization fit and evaluating decision aids. Avenues for future research are outlined. Settled science will require more definitive research.

Behavioral scientist Fischhoff (2013) illustrates the relationship between decision theory and practice with the following question: "The real world: What use is it?" My answer is: Plenty, if you want to use decision aids to improve that world. I have spent a long career trying to do that. Decision aiding – and applied decision theory in particular – promises to greatly enhance the effectiveness of individual and institutional decisions, but it is still quite primitive and needs countless refinements (perhaps even radical changes) before it comes close to realizing that promise (Brown, 1992). Effective decision technology (broadly interpreted) calls for significant advances in psychological normative and organizational theory. Decision-aiding practice can contribute fruitful direction and seed promising research. The following is my view on a wide variety of research issues that could make a real difference. I will discuss some projects, cite others and reference papers that give more detailed treatment.

1. Background

1.1. Unorthodox scope

This paper will, no doubt, be unlike others in this special issue, in that it does not conform to what is commonly expected in a scientific journal. It does not address a single, well-defined issue, sharply focused on a particular disciplinary topic. Instead, it reports on exploratory inquiries, whose unifying theme is a class of practical problems. It does not purport to report findings that are universally and eternally applicable, nor grounded firmly in theoretical or empirical research. Instead, it reports on the experience, albeit extensive and varied, of one decision aider-researcher – myself – and it takes research ideas no further than I have needed to address client dilemmas cost-effectively.

Environmental policy scholar Morgan (1978) has persuasively argued that good policy calls for bad science. In other words, for policy purposes, the science only needs to be "good enough" to support decision tools that are used by – and useful to – a decider. Findings do not need to be firm enough and thoroughly enough documented so that a scientific audience can confidently accept them and expect that experimental results can be replicated. Many policy-makers and other deciders may not be willing to pay the cost and delay associated with such verifiable certitude. To illustrate this point, consider the following case. (Terms: A "case" really happened; an "example" is hypothetical.)

Clean air legislation. The Environmental Protection Agency (EPA) was mandated by Congress to evaluate whether the Clean Air Act was worth its cost. My company, Decision Science Consortium, Inc. (DSC), a small decision consulting and research company, was charged with orchestrating the study (Brown, 1991). We constructed a "macro-model" whose input was to be provided by various scientific bodies. Major research organizations such as national labs and the US Geological Survey were to provide us with expert input, such as what the Act's impact would be on

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agricultural interests or auto industry economics. Without exception, they refused to provide us with any findings until these met tests of scientific publishability. Before any of these subcontractors were satisfied, control of Congress passed from Democrat to Republican and the whole project was scrapped.

Philosopher Good (1963) advocated a "Journal of Partly-Baked Ideas". As a decision aider, I would be an avid consumer of such ideas, though Good may have valued them more for their contribution to definitive science than to practical decision aiding. The present paper should fit well in a journal with such a name. Practical necessity may have led me to these partly-baked ideas in the first place; but they may now also serve to advance conventional scientific objectives.

1.2. Autobiographic basis

The basis of these thoughts is purely autobiographic: My fifty year career interleaved decision consulting with university teaching and research. As a consultant, I applied decision-aiding methods, mainly derived from applied decision theory, to over 100 diverse dilemmas, throughout government and business, often at the highest levels, including heads of agencies. As an academic, I served on various normative and descriptive university faculties. I do not have the cognitive psychology training or academic orientation of, I expect, most readers of this essay. My main formal training was in statistical decision theory, but real world experience has taught me that it is more important to understand how people *do* behave than how they *would* behave if they obeyed logical norms. I have acquired a selective smattering of cognitive psychology by conferring and collaborating with academics in that field.

I am a technologist, not a scientist. I apply whatever science I have available, however tentative, to a practical problem; in this case, to make wise decisions in an organizational context. In the course of so doing, I may stumble on gaps or inadequacies in the scientific canon, which I can formulate as questions for the scientific community to pursue, along with guesses of my own – hypotheses if you will – about where a scientific solution may lie.

2. Prescriptive research needs

2.1. Distinctive features of prescription-oriented research

Effective prescription, or decision adding, may be advanced by mono-disciplinary research; be it purely descriptive or purely normative (Bell, Raiffa, & Tversky, 1988). In these cases, the nature of the research is not essentially different from the research normally carried out in traditional departments, such as psychology, organizational behavior and statistics, and is governed by similar career incentives (although the topics may differ quite markedly from those currently pursued).

Much prescription-oriented research, however, is more radically distinctive in being interdisciplinary, and less readily accommodated by existing academic institutions. We decision technologists have a pervasive interest in evaluating the performance of candidate decision aids in terms of both their logical rigor (a normative issue) and their human implementability (a descriptive issue). Wise aid design requires trading off one consideration against the other (an interdisciplinary issue).

Suppose a decider is interested in updating probabilities in the light of new evidence. Should he assess the required "posterior" probability directly or use Bayes theorem to compute it from the information required by this theorem? Descriptive research may find (as I suspect) that humans are not very good at making imaginary assessments (such as the probability for obtaining the evidence, if a hypothesis is or isn't true). Research may find that, *given sound input* assessments, some Bayesian algorithm outperforms unaided intuition. Interdisciplinary research is needed to judge whether or not this improvement is outweighed by loss due to unsound input.

2.2. Deciders' avoidance of applied decision theory models

Deciders' use of quantitative applied decision theory and other formal decision aid has, so far, been surprisingly small, given the initial optimism of the 1950s (Brown, 1992). A number of highly trained decision analysts, who later became deciders in organizations, confessed that they *never* use a quantified model when making their own decisions. These deciders include: Grayson (1973), author of what was probably the first published case study in applied decision theory and former chairman of the Federal Price Control Board; Andrew Kahr, a theoretical pioneer in the original applied decision theory group at Harvard (of which I was a junior member), who became a highly successful financier; and Jim Edwards, an ex-student of mine at Harvard Business School, who became chairman of ICF, Inc. a billion dollar corporation. Even I myself almost never use a quantified decision model on my private decisions and rarely put them before my consulting clients, at least as a decision aid. Although decision modeling is commonly used in business (Ulvila & Brown, 1982), government (Brown, 1987), medical and other organizations, according to my experience the motivation is hardly ever to help deciders to make better decisions. Instead, the main purpose seems to be to validate or explain proposed and past decisions to others (see chapter 2 of Brown, 2005b). Nevertheless, deciders have uniformly credited their decision modeling training with honing their intuition and informal reasoning (Brown, 2012).

2.3. Dearth of prescription-oriented research

I attribute the deciders' neglect of applied decision theory models, at least partly, to inadequate state-of-the-art of prescription-oriented science, that is, science that is targeted at improved practical decision-making. In the mid-1980s I became concerned about a possible mismatch between current decision science and decision practice. DSC arranged a short conference (funded by the Office of Naval Research) at the National Academy of Sciences, where scientists and decision aiders discussed synergy between them (Tolcott & Holt, 1988). The scientists included leading academics, such as Amos Tversky and Herbert Simon. The aiders were commissioners of decision aid in the military. Conference participants could identify (at least on-the-spot) only a couple of research efforts over the previous 20 years that had been used in operational decision aiding practice, such as influence diagrams (Howard & Matheson, 1984).

To be used and useful, decision aid has to be interdisciplinary. It needs to be logically sound, cognitively realistic, organizationally appropriate, and take advantage of the best practical skill and knowledge available in the particular application domain, such as business or medicine. Professional and academic institutions, however, reward the mono-disciplinarian. This difference is akin to the situation in medicine, where general medical practitioners arguably make the greatest difference to the health of the population, while prestige and financial rewards go to the specialists. A contrary and more encouraging analogy would be music, where the conductor is rewarded more highly than the first violin.

I was not able to lure some of the keenest scientific minds into interdisciplinary prescriptive science. When I tried to interest Herbert Simon, Amos Tversky and James March into redirecting their impressive intellects into decision aiding all three agreed that that this activity was important and encouraged me to pursue it. (Over weekly lunches, Simon would quiz me on how his theories fared in