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PROBLEMS FOR JUDGMENT AND DECISION MAKING

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■ **Abstract** This review examines recent developments during the past 5 years in the field of judgment and decision making, written in the form of a list of 16 research problems. Many of the problems involve natural extensions of traditional, originally rational, theories of decision making. Others are derived from descriptive algebraic modeling approaches or from recent developments in cognitive psychology and cognitive neuroscience.

"Wir müssen wissen. Wir werden wissen." (We must know. We will know.)
David Hilbert, *Mathematische Probleme* (1900)

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INTRODUCTION

"Who of us would not be glad to lift the veil behind which the future lies hidden and direct our thoughts towards the unknown future . . . (Hilbert 1900, p. 437)." In 1900, Hilbert began a paper presented at the International Congress of Mathematicians in Paris with these words. (*Mathematical Problems*, an English translation

by Newson, was published in 1902). In this paper, Hilbert proposed 22 problems as the focus of mathematical research in the immediate future. His judgment was impeccable, and many of his problems turned out to be the most studied and productive in the next century. The present review attempts a modest version of Hilbert's feat for the field of judgment and decision making. First, I briefly consider what makes a good research problem; this is followed by an introduction to the field of judgment and decision making; and finally, a list of 16[1] problems designed to promote productive research on judgment and decision making in the next decade or two.

GOOD PROBLEMS

The two primary motives for research in the behavioral sciences are to develop scientific theories and to solve problems that occur in everyday life. In the case of research on judgment and decision making, there are three theoretical frameworks that provide the motivation for current and future research: (*a*) traditional expected and nonexpected utility theories, most prominently represented by von Neumann and Morgenstern's expected utility theory and Kahneman and Tversky's (cumulative) prospect theory, which focus on choice and decision-making behaviors; (*b*) cognitive algebraic theories, primarily concerned with judgment and estimation; and (*c*) cognitive computational theories of the mind's perceptual, inferential, and mnemonic functions. Each of these three theoretical frameworks provides a general image of the human mind, although sometimes, when research is focused on a specific issue within any one of the frameworks, it is unclear exactly what larger question about the nature of human nature is being addressed.

Most academic researchers seek solutions, usually in controlled laboratory experiments, that reveal basic behavioral and cognitive processes. The biggest rewards go to researchers who appear to have discovered something fundamental and general about human nature—for example, the general diminishing marginal utility tendency for rewards and punishments to have decreasing impacts on evaluations and behavior as the overall amount of reward or punishment increases; the loss aversion tendency, in which losses have a greater impact than gains of equal magnitude; the general habit of integrating separate items of information according to an averaging (linear, weight and add, or anchor and adjust) combination rule when estimating magnitudes of all kinds; the properties of capacity limits on working memory and selective attention; etc.

[1]My original list had 22 problems to be consistent with Hilbert's example. However, length constraints and lack of energy whittled the total down to the present 16. Anyone interested in reading the material excluded from this review should contact the author (e-mail: reid.hastie@colorado.edu); anyone who wishes to contribute additional problems to a master list of problems should send a suggestion to the author. I will maintain a current list and make it available to anyone who requests it.

The second strategy is to start with a phenomenon that is important in everyday life and independent of any scholarly tradition and then to study it with the primary motive of contributing to general social welfare. This is sometimes called the Pasteur heuristic, after the great French biologist's habit of starting with a phenomenon of practical importance, subjecting it to rigorous empirical analysis, and presenting solutions to major social and theoretical problems. On the practical side, the major "real world" problems that have been the subject of extensive study would include medical diagnosis and decisions by both health care professionals and their patients (especially relating to psychotherapy); financial and economic forecasting; legal, policy, and diplomatic judgments; and weather forecasting. The contributions of researchers in judgment and decision making to improved social welfare are more difficult to identify than the constantly increasing numbers of archived scientific papers, but I submit that many instances of applied decision analysis and aiding have had their origins in research by behavioral scientists (see Cooksey 1996 and Swets et al 2000 for many examples).

In the present review, I attempt to identify research problems that are justified with reference to either or both motives. Some of the attributes of good, productive research problems identified by Hilbert and others that apply to either theory- or practice-motivated research include the following:

1. The statement of the problem is clear, comprehensible, and succinct.
2. The statement of the problem is expressed in intuitively sensible and meaningful symbols.
3. The problem is difficult but accessible—it is challenging but not apparently impossible.
4. It is possible to evaluate the correctness (or at least the relative-correctness) of a candidate solution.
5. The answer to the problem connects to current knowledge; it has an important location in a chain of problems. A solution to the problem will have ramifications in many other theoretical and practical domains.

I have attempted to state problems that exemplify these attributes; although, unlike mathematical problems that can be proved or derived, most behavioral science problems have many answers, or at least answers with multiple, interrelated parts. As the adage states, almost any problem, however complicated, becomes still more complicated when looked at in the right way. However, that is not necessarily a bad quality for a scientific problem to possess.

THE PSYCHOLOGY OF JUDGMENT AND DECISION MAKING

What is the field of judgment and decision making about? The focus of research is on how people (and other organisms and machines) combine desires (utilities, personal values, goals, ends, etc) and beliefs (expectations, knowledge, means, etc)

to choose a course of action. The conceptual (perhaps defining) template for a decision includes three components: (*a*) courses of action (choice options and alternatives); (*b*) beliefs about objective states, processes, and events in the world (including outcome states and means to achieve them); and (*c*) desires, values, or utilities that describe the consequences associated with the outcomes of each action-event combination. Good decisions are those that effectively choose means that are available in the given circumstances to achieve the decision-maker's goals.

The most common image of a decision problem is in the form of a decision tree, analogous to a map of forking roads (Figure 1). The diagram in Figure 1 highlights the three major components: alternative courses of action, consequences, and uncertain conditioning events. For the sake of clarity, I will state a few basic definitions. Decisions are situation-behavior combinations, like the one

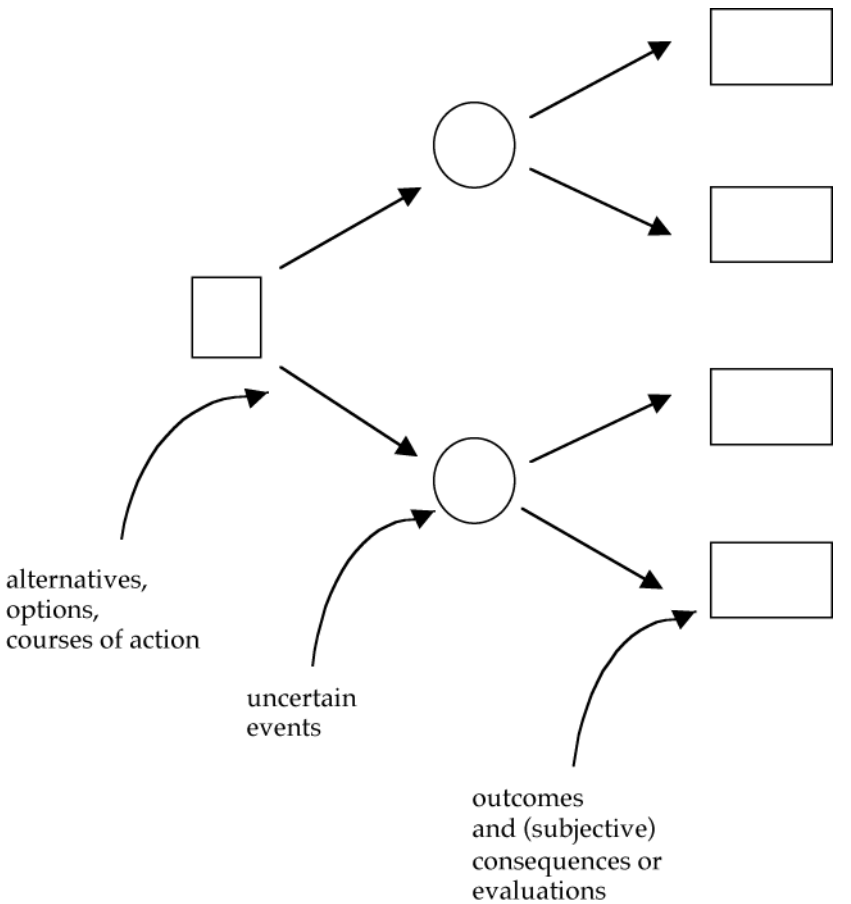

Figure 1 Definitional template for a decision.

summarized in Figure 1, which can be described in terms of three essential components: alternative actions, consequences, and uncertain events. Outcomes are the publicly describable situations that occur at the end of each path in the decision tree (of course, outcomes may become mere events if the horizon of the tree is extended further into the future). Consequences are the subjective evaluative reactions (measurable on a good-bad, gain-loss scale) associated with each outcome. Uncertainty refers to the decision-maker's judgments of the propensity for each of the conditioning events to occur. Uncertainty is described with several, sometimes competing, measures in various decision theories, including probabilities, confidences, and likelihoods (there are several prescriptions for precise usage of these terms, but for present purposes I lump together all measures of judgment of the propensity for events and outcomes to occur under the rubric "uncertainty;" see Luce & Raiffa 1957 for an introduction to traditional distinctions). Preferences are behavioral expressions of choosing (or intentions to choose) one course of action over others. Decision making refers to the entire process of choosing a course of action. Judgment refers to the components of the larger decision-making process that are concerned with assessing, estimating, and inferring what events will occur and what the decision-maker's evaluative reactions to those outcomes will be.

Historically, behavioral research has been divided into two separate streams: judgment and decision making (see Goldstein & Hogarth 1997 for an excellent history of recent research in the field). Research on judgment has been inspired by analogies between perception and prediction. For judgment researchers, the central empirical questions concern the processes by which as-yet-obscure events, outcomes, and consequences could be inferred (or, speaking metaphorically, "perceived"): How do people integrate multiple, fallible, incomplete, and sometimes conflicting cues to infer what is happening in the external world? The most popular models have been derived from statistical algebraic principles developed originally to predict uncertain events [especially linear or averaging models, fitted to behavioral data using multiple regression techniques; (Anderson 1982, Cooksey 1996)]. The primary standards for the quality of judgment are based on accuracy the correspondence between a judgment, and the criterion condition that was the target of the judgment (Hammond 1996, Hastie & Rasinski 1987). This approach has also been applied to study judgments of internal events, such as personal evaluations of consequences or predictions of evaluative reactions to possible future outcomes.

The second stream of research was inspired by theories concerned with decision making that were originally developed by philosophers, mathematicians, and economists. These theorists were most interested in understanding preferential choice and action: How do people choose what action to take to achieve labile, sometimes conflicting goals in an uncertain world? These models are often expressed axiomatically and algebraically, in the tradition established for measurement theories in physics and economics. Here the standards used to evaluate the quality of decisions usually involve comparisons between behavior and the prescriptions of rational, normative models, which often take the form of tests for

the coherence of expectations, values, and preferences or the achievement of ideal optimal outcomes.

Although the field is becoming more behavioral, more psychological, and more descriptive, its boundaries and major theoretical concerns are all related to the historically dominant expected utility family of theories. These theories (and there are many flavors) are exemplified by the subjective expected utility theory made popular by von Neumann & Morgenstern (1947) and Savage (1954; Friedman & Savage 1948). Several good recent introductions to expected and closely related nonexpected utility frameworks are available [e.g. Camerer 1995, Dawes 1998, the papers cited in Edwards 1992; and Luce 2000; see Miyamoto (1988) for a distillation of the formal essence of these theories]. At the center of all these frameworks is a basic distinction between information about what the decision maker wants (often referred to as utilities) and what the decision maker believes is true about the situation (often called expectations). The heart of the theory, sometimes called the rational expectations principle, proposes that each alternative course of action or choice option should be evaluated by weighting its global expected satisfaction-dissatisfaction with the probabilities that the component consequences will occur and be experienced.

The subjective expected utility framework was first applied analytically—if the preferences of a decision maker (human or other) satisfied the axioms, then the decision process could be summarized with a numerical (interval-scale) expected utility function relating concrete outcomes to behavioral preferences: first the choice behavior, then an application of the theory to infer what utilities and beliefs are consistent with those preferences. However, more recently, the theory has often been used synthetically to predict decisions: If a person has certain utilities and expectations, how will he or she choose to maximize utility? This second perspective on the theory seems more natural, at least to a psychologist; intuitively, it seems that we first judge what we want and think about ways to get it, and then we choose an action.

There are two important limits on the expected utility framework. First, it is incomplete. Many aspects of the decision process lie outside of its analysis. For example, the framework says nothing about how the decision situation is comprehended or constructed by the decision maker: Which courses of action are under consideration in the choice set? In addition, the theory says nothing about the sources of inputs into the decision process: What should the trade-off be between adaptive flexibility and the precise estimation of optimal choices by a realistic computational system (the human brain) in a representatively complex, nonstationary environment? Where does information about alternatives, consequences, and events come from in the first place, and how is it used to construct the representation on which the expected values/expected utilities are computed? Finally, how are personal values, utilities, and satisfactions inferred, predicted, and known?

The second limit on the expected utility framework is that it does not provide a valid description of the details of human decision-making processes. Today a myriad of qualifications is applied to the basic expected utility model when

it is used to describe everyday decision-making behavior. As the saying goes, Compared to the assumptions of the rational model, people are boundedly rational and moderately selfish, and they exercise limited self-control (Jolls et al 1998).

However, even with its limitations,[2] subjective expected utility has been the dominant conceptual framework for rational and empirical studies of decision making for the past 2 or 3 centuries. Therefore, many research problems concerning judgment and decision making are best conceptualized and stated in the context of this overarching theoretical framework. Furthermore, there is some sensible reason for most behaviors (Anderson 1990, March 1978, Newell 1982; see Oaksford & Chater 1998 for a sample of applications of rational models to cognitive achievements). Even when people appear to be making systematically biased judgments or irrational decisions, it is likely that they are trying to solve some problem or achieve some goal to the best of their abilities. The behavioral researcher is well advised to look carefully at his or her research participant's behavior, beliefs, and goals to discern "the method in the apparent madness" (Becker 1976, Miller & Cantor 1982).

The following problems are all concerned with aspects of the distinctive focus of research on decision making, the manner in which organisms integrate what they believe is true and what they expect will happen with what they want. Most of the problems can be interpreted as extensions of the enduring research program that produced the subjective expected utility framework, although the new directions are often more cognitive, more neural, and more descriptive than anticipated by that seminal framework. For each problem I attempt to provide an introduction to interest the reader in the subject matter and to initiate further scholarly and empirical research on the topic.

THE PROBLEMS

Overarching and Foundational Questions

Problem 1 (CK Hsee, RH Thaler[3]) What makes a decision good? The origins of the field of decision making lie in efforts to prescribe good, winning decisions in gambling and insurance situations. The original answers to the question of

[2]Many behavioral scientists would say that these limitations are advantages: At least there has been some agreement on the nature of the rational man that underlies traditional theories (see Problem 1), although there is, even today, no clear conception of and little consensus on the nature of the irrational person. At least at present, there are few clear theoretical ideas about how people comprehend and mentally represent decision situations (see Problem 5). What kind of a more psychologically realistic theory could be framed without answers to those questions?

[3]I have received too much constructive advice from too many sources to provide full acknowledgments. I have listed the names of those individuals I remember as being most influential on my selection or formulation of a problem in parentheses with each problem statement. I beg forgiveness from the many people whom I must have forgotten to thank.

what makes a decision a good one involved identifying the actions that would maximize desirable (and minimize undesirable) outcomes under idealized conditions. These rational solutions proved to be remarkably general and led to the enterprise of representing complex everyday situations as well-formed, regular gambles to allow the direct application of decision analysis (e.g. Clemen 1996, Hammond et al 1999). Recent analyses have shifted the emphasis to the robustness and generality of decision-making methods, especially when information is unreliable and incomplete, the environment is complex and changing, and mental computations are limited (in quantity and quality). Thus, there is a historical shift from criteria based on internal coherence [e.g. did the person's decisions satisfy the principles of rational thinking (usually the formal axioms of the probability theory or rational choice theories)?] to criteria based on measures of accuracy or success with reference to conditions in the decision-maker's external environment (correspondence) in the operational evaluation of decision goodness (cf Hammond 1996, Hastie & Rasinski 1987, Kahneman 1994). There are many conceptions of the nature of rationality. A useful introduction to modern views is provided by Harman (1995), who emphasizes the distinctions between theoretical (belief and judgment) and practical rationality (intentions, plans, and action) and between inference (psychological processes) and logical implication (relationships among ideal propositions). A common approach to identify rationality is to use simple, indubitable rules to detect irrationality; for example, if a logical contradiction is established within a set of beliefs, the beliefs and corresponding behavior must be irrational (Dawes 2000, Tversky & Kahneman 1974). Another solution is to rely on people's intuitions: If someone thinks hard about a decision (more often two or more decisions) and wishes that he or she had not made the decision, there is at least a hint of irrationality (especially if the thinker is someone whose credentials establish that he or she is an expert). Of course, the ability to recognize irrationality in decisions may not be directly related to the ability to reason rationally when making those decisions.

A second approach is to assess the success of various decision rules, algorithms, or heuristics in complex simulated choice environments. A precursor to this approach is the analytic study of the long-term survival of decision makers over a long sequence of decisions (Dubins & Savage 1965, Lopes 1982). The recent simulation methods have been pioneered by Axelrod (1984), Payne et al (1993), and Gigerenzer et al (1999). All have used simulation to evaluate the relative adaptive success of decision rules in abstractions of social and distributed-resource environments. An important property of robust computational systems is that they degrade gracefully, rather than fail catastrophically, when challenged. One of the surprises from these research programs has been the discovery that, under some representative conditions, "less is better." That is, more limited processing is actually more adaptive (Gigerenzer & Goldstein 1996, Kareev et al 1997).

A third approach is to study the adaptive success of decision habits in natural environments. This approach has been neglected by psychologists, although there are instructive examples in studies of optimal foraging by humans and

animals (Kaplan & Hill 1985, Krebs & Davies 1993, Real 1991, Stanford 1995), in studies of the success of economic strategies in naturally occurring markets (e.g. Camerer 1997), and in studies of global individual differences in everyday decision-making habits (Larrick et al 1993). Thus, the very definition of rationality, adaptive rationality, or ecologically fit rationality is under construction. One of the most interesting recent directions is an effort to reduce traditional definitions and precepts to a more fundamental, biological-evolutionary level—for example, Cooper's (1987) attempt to derive Savage's axioms from the concept of an evolutionarily stable strategy (an approach that assumes the bounded forms of rationality may still be adaptively optimal). Of course, additional work remains to be done on operational methods to measure the concept(s) of adaptive rationality, when it has stabilized.

Problem 2 (B Fasolo, JM Greenberg, JW Payne) What makes a decision difficult? There is no consensus on a comprehensive principle that predicts decision difficulty, provides measures of difficulty, or even provides a definition of difficulty. For example, consider an idealized choice set (the kind of stimulus display that would be presented to readers of a product review in *Consumer Reports* or to participants in a study of consumer choice on a MouseLab® information display board). What characteristics of the alternatives, their attributes, and their relationships to the choice maker's goals make the decision difficult? If the two decision problems are clearly described, how can we predict which will be more difficult? Are the choice sets that make us look smart in the Gigerenzer et al (1999) research program easy decisions, at least compared to market-driven consumer choice sets, in which the attribute values are often negatively correlated and where we are challenged by difficult attribute-value tradeoffs?

One answer is a very general "it depends"; most theorists presume that the number of elementary information processes executed to make a typical choice is a good index of difficulty, so that difficulty is defined on the basis of the relationship between the choice set and the decision-maker's capacities and strategy (e.g. Bettman et al 1990).

However, at least two aspects of cognitive load have been ignored in previous measures: the effort required to infer attribute values and to align attributes in the choice process (Medin et al 1995) and the demands on working memory imposed by choice strategies (Miyake & Shah 1999). Surely we can say more about the effects on difficulty of the number of alternatives, similarity of alternatives, number of important, goal relevant attributes, and, perhaps most importantly, the intercorrelations of the attribute values across alternatives.

A second approach to defining and measuring difficulty is to rely on choice makers' subjective evaluations of difficulty, effort, strain, or anxiety (e.g. Chatterjee & Heath 1996, Luce et al 1999). A third approach is to identify correct choice alternatives and to work backward from situations and conditions in which decision makers make many or few errors. These later approaches do not seem as fundamental as the proactive development of a theory of the causes of cognitive and

emotional difficulty. However, any progress toward a theory of decision difficulty will be a major contribution.

Problem 3 (KR Hammond, JM Greenberg, WM Goldstein) What are the roles of intuitive vs analytic modes of thinking in judgment and decision making? What are the relative roles of intuitive (e.g. implicit, associative, or automatic) vs analytic (e.g. explicit, rule-based, or controlled) cognitive processes in a decision? Both kinds of cognitive process are involved in any deliberate, goal-directed decision. Implicit processes seem to be fundamental, more likely to involve emotional reactions, and likely to be modified by slow, incremental learning processes. Explicit processes seem to be optional, more likely to involve context-independent abstractions, and likely to be modifiable by brief, even one-trial, learning episodes. If there is a signature distinction between the two kinds of processes, it is based on conscious awareness. If a process occurs outside of awareness, it is probably implicit; and any process that can be inspected and modified consciously is explicit. I suspect that it is useful to describe some fundamental cognitive functions as being performed by procedures that are essentially implicit: memory retrieval (including emotional reactions), familiarity and similarity judgments, registration and estimation of experienced frequencies, and probably some form of causal or contingency judgment. Obviously, these fundamental cognitive procedures will be employed in higher-order explicit, analytic, goal-directed strategies.

There are many neuroscientific findings that imply a distinction between implicit, intuitive processes and explicit, analytic processes (e.g. Alvarez & Squire 1994, McClelland et al 1995, papers in Schacter & Tulving 1994). In addition, some decision processes are deeply ingrained in the nervous system at a level at which they are unlikely to be consciously penetrable (e.g. Knowlton et al 1994, Lieberman 2000, Nichols & Newsome 1999, Platt & Glimcher 1999). However, there is little consensus in cognitive and social psychology on the relationship between implicit and explicit processes or on standard operations to separate implicit from explicit aspects of responding [Smith & DeCoster (2000) provide a nice summary of diverging views]. The most popular methods in empirical studies of implicit vs explicit processes study the unconscious effects of prior experience on the perception or comprehension of a stimulus (i.e. priming; e.g. Bargh & Chartrand 2000, Leeper 1935).

An interactive model that assumes a constant mixture of intuitive-analytic modes (e.g. Anderson 1983, Jacoby 1991) seems more plausible than an independent channel model (e.g. Bargh & Chartrand 1999, Schacter 1994, Sloman 1996). I suspect that many phenomena that have been described as a race between an implicit and an explicit process, both of which finish, [sometimes these processes promote competing responses (e.g. Sloman's S-criterion)], are actually examples of sequential processing. However, I have little confidence in this speculation, and a good answer awaits advances in the methods to identify the existence and outputs of different process types. I agree with the common assumption that implicit

processes rely less, if at all, on immediate, working memory capacities than do explicit processes, and this assumption does imply that the cognitive resources for simultaneous processes are available.

I strongly endorse Hammond's cognitive continuum framework, which proposes that pure intuition and pure analysis anchor a descriptive scale in which most cognitive performances are quasi-rational mixtures of the two ingredient modes of processing (Hammond 1996) and also describe the characteristics of the different modes of judgment and the task conditions that promote one process rather than the other. I suggest that the cognitive continuum framework should be further developed in terms of a successful cognitive architecture [e.g. JR Anderson's ACT family of information processing models (1983); NH Anderson's information integration theory (1996); or perhaps a connectionist formulation, e.g. Rumelhart et al (1988)]. In my view, one weakness of research on the differences between implicit and explicit processes has been the tendency to settle for empirical demonstrations of the mere existence of implicit processes followed by piecemeal theoretical assertions. What is sorely needed now are comprehensive process models for the overall cognitive performance in the experimental tasks under study that assign relative roles to implicit and explicit processing modes. Only within the framework of a comprehensive process model can we achieve conceptual clarity about the nature and interrelations between the two or more types of processes and also develop useful empirical methods to study the relationships between the processes.

For example, the roles of implicit and explicit processes are going to change in relation to the following events: an emotional reaction to one or more of the choices affecting the larger decision strategy, the retrieval of remembered cases and analogical reasoning, the matching of the current situation to an abstract prototype or narrative schema, or the deliberate enumeration of reasons for and reasons against each imagined course of action. I would speculate that the role of intuition would be more dominant in the first two strategies and the least dominant in the last strategy; but again, I have little confidence in any hypothesis without empirical research and theories that identify the roles of implicit and explicit processes within the context of a comprehensive model of the larger decision-making process.

Problem 4 (WM Goldstein, T Kameda, TK Landauer, R Tourangeau) What are the alternatives to consequentialist models of decision processes? Many apparent decisions are not conceptualized as such by the decision maker; for example, "I'm tailgating because I want to get to work as fast as I can" (not "I made a choice between driving at a reasonable speed and hurrying."), "I'm having sex because I can," "I'm taking the drug because the doctor told me to," "I'm signing the contract because I can't stand any more of this irritation," etc. Many such decisions are the result of applying a plan, a policy, or a self-concept formed in the past to the current situation; for example, "I didn't decide whether or not to vote by considering means and consequences; I voted because I am a good citizen,"

"I didn't decide whether or not to recycle; it's just an expression of my attitude about protecting the environment," "I didn't decide whether or not to overfish the stream; I just acted the way a Native American would act," "I didn't marry Donald because I reasoned through the means and ends; he's just the right kind of husband for me."

March & Simon have discussed the reliance on social roles and personal identity in decision making (i.e. decision making as rule following; March & Heath 1994, March & Simon 1993), and Baron (1994), and Baron & Spranca (1997) demonstrated the impact of protected, obligatory values on behavior. However, clear models of nonconsequential decision processes are still needed (see also Goldstein & Weber 1995). Cognitive and production system models would seem to be natural candidates to describe these processes, and similarity is likely to play a key role (cf Medin et al 1995). Furthermore, many decisions are made by imitation of the behavior of others; for example, important financial decisions appear to be well described by herd instincts. We know of almost no discussion or research on the role of social imitation in decision making.

Problem 5 (B Fischhoff, EJ Mulligan, DA Rettinger) How are deliberate decision-making problems represented cognitively (e.g. causal explanations and naïve conceptualizations of random processes), and what are the major determinants of the representation of situations? How do people comprehend the decision situation; in other words, how do they perceive, retrieve, or create alternative courses of action; represent the events that will condition which outcomes occur; and evaluate the consequences they might experience? If they do create a mental representation of actions and consequences that is something like the decision tree from traditional decision theory (Figure 1), what determines which branches, contingencies, and components are included; the temporal extent or horizon of the tree; and the point at which a person stops looking ahead and begins to reason backward about what will happen and what he or she will receive as a consequence? What little we know about the generation and representation of alternative courses of action suggests that decision makers are myopic and do not consider many options (Fischhoff 1996, Keller & Ho 1988), although obviously this habit may not be a handicap if the focal options are good ones (Klein et al 1995).

Far more is known about the consequences of alternative decision problem representations (e.g. gain vs loss frames and summary vs unpacked event descriptions) than is known about the determinants of the representations. Thus, one key problem is understanding the determinants of decision frames (Levin et al 1998) and event descriptions (Johnson-Laird et al 1999, Macchi et al 1999, Rottenstreich & Tversky 1997, Tversky & Koehler 1994) and the impact of these differences on evaluations and judgments when a person is uncertain.

This is a problem that seems to invite applications of the cognitive theories and methods to describe knowledge representations (Markman 1998).

Recent work applying graphical plus algebraic representations to evidence evaluation and causal reasoning points the direction to useful hybrid structure-process

representations (Pearl 2000, Schum 1994). Some applications of this approach have been attempted in studies of judgment and decision making (e.g. Hastie & Pennington 2000, Klayman & Schoemaker 1993, Weber et al 1991), although almost all of the psychological research on decision problems with which we are familiar is concerned with the representation of evidence but not of alternative courses of action, consequences, and evaluations (some example exceptions might include Fischhoff et al 1999, Kintsch 1998, and Rettinger & Hastie 2000). For ideas about how to proceed, there are suggestions from economists, game theorists, and management scientists (e.g. Ho & Weigelt 1996, Manski 1999). One reason that research on decision making has been so closely connected to normative theories in economics and statistics and so little influenced by research on problem solving, reasoning, language, and other higher cognitive functions is because the cognitive methods are less developed and much more labor-intensive than the methods used to test algebraic process models. A major challenge for cognitive approaches is to standardize and simplify methods for the measurement of knowledge structures involved in decision processes.

Problem 6 (RD Luce, MC Mozer) It would be useful to develop a theory that provides an integrated account of one-shot, well-defined decisions (current theories) and sequences of linked decisions in a dynamic, temporally extended future. Most current decision theories are designed to account for the choice of one action at one point in time. The image of a decision maker standing at a choice point like a fork in a road and choosing one direction or the other is probably much less appropriate for major everyday decisions than the image of a boat navigating a rough sea with a sequence of many embedded choices and decisions to maintain a meandering course toward the ultimate goal (Hogarth 1981). This is exactly the image that has dominated analysis in research by psychologists and computer scientists concerned with problem solving and planning: a problem space composed of a series of problem states with connecting paths, with the problem solver navigating from start to goal and relying on evaluation functions for guidance (Newell & Simon 1972). Each move from one state to the next can be treated as a decision, with the evaluation function serving to define expected utilities for the alternative moves to each available next state.

The temporal dimension enters the decision process in many ways. The decision process takes time, and some theories describe the dynamics of the temporally localized decision process (e.g. Busemeyer & Townsend 1993). Sometimes the anticipated outcomes are distributed over a future epoch or located at a distant point in time; if so, the decision maker has to project his or her goals into the future to evaluate courses of action in the present (Loewenstein & Elster 1992). Sometimes the theory attempts to incorporate the future via a temporal horizon by supposing that the decision maker anticipates some possible consequences by constructing scenarios or decision trees as extensions of his or her mental situation model (although almost nothing is known about the psychology of these constructive planning processes). Sometimes choice in the current situation involves a

sequence of decisions that are dependent on each other and on changing future circumstances.

This issue of projection and, especially, controllability of the anticipated sequence of decisions is frequently mentioned in strategic managerial contexts; however, we do not have much factual knowledge about these decisions or a clearly specified theoretical framework to account for empirical findings (Brehmer 1999, March & Shapira 1992, papers in Shapira 1997). Some recent developments in behavioral game theory include an especially promising source of research paradigms and theoretical concepts to study people's reasoning about choices and contingencies in an extended temporal frame (e.g. Camerer 1997, Ho & Weigelt 1996).

Problem 7 What is the nature of value-expectation (payoff-probability) interactions, and where do they occur? The existence of wishful thinking (the valence of an event has an impact on expectations of its probability of occurrence) or Pollyanna effects (the probability of an event's occurrence has an impact on judgments of its valence) is assumed in many discussions of judgment and decision-making phenomena. However, there is little hard evidence for the reliable occurrence of these phenomena, and there are contrary hypotheses with some support (e.g. simple pessimism and defensive pessimism). Nonetheless, research literature exists, mostly concerned with judgments of health and medical risks, that assumes that optimistic thinking is the norm and is adaptive (Taylor & Brown 1988; Weinstein 1980, 1989; but see Colvin & Block 1994).

An instructive recent example comes from research on physicians' estimates of patients' longevity. Initial results suggested that the estimates were much too optimistic; it appeared that the physicians overestimated the patients' longevity by a factor of 5, but careful follow-up studies revealed that in most cases the physicians' overestimates were deliberate errors and that their true estimates were much more accurate (Lamont & Christakis 2000). Similar effects are prevalent in many apparent demonstrations of optimistic and overconfident judgment: When incentives are increased for accuracy or when disincentives are increased for errors, wishful thinking decreases or disappears.

My reading of this literature suggests that there are only two situations in which unrealistic optimism has been reliably established: judgments of cost and time to complete future multicomponent projects (e.g. Griffin & Buehler 1999) and early judgments of the longevity of personal relationships, such as in dating and marriage (MacDonald & Ross 1999). Results in gambling situations and results that require responses on well-defined probability-of-occurrence scales have been mixed over the past 50 years [Slovic's 1966 dissertation summary (p. 22) still seems apt: "Desirability was found to bias probability estimates in a complex manner which varied systematically between subjects and between estimation trials . . . some subjects were consistently optimistic. Some were quite pessimistic."] Nonetheless, I believe that the quest for reliable value-expectation interactions is a productive problem for research. The results of careful research on this problem

will be useful in practice and will illuminate the relationships between value and belief that lie at the center of the decision-making process.

Questions About Values, Utilities, and Goals

Problem 8 (D Kahneman, GF Loewenstein, RD Luce, B Fischhoff, JW Payne) If people don't have preferences like those postulated by expected utility theories, what do they have instead? Economists postulate the existence of preferences, and psychologists interpret some data as evidence of unstable or nonexistent preferences. In psychology, the most popular conception of values and preferences supposes that they are like attitudes. This analogy between values and (other) attitudes is useful, but it needs further development: What kind of attitudes? It would be interesting to apply the general belief-sampling model of attitudes that has been developed by researchers concerned with the quality of survey responses (Tourangeau & Rasiniski 1988); this seems close to what Kahneman and others have in mind in the context of contingent valuation processes (Kahneman et al 1999b). This belief-sampling model assumes, with some empirical support, that a person carries around a store of relevant ideas in his or her long-term memory that can be retrieved in response to an attitude question or object. The attitude (evaluation) on any one occasion is a function of the number and affective-evaluative qualities of the ideas retrieved at the time when a question, object, or situation is encountered. Because the memory retrieval system is variable, attitudes (evaluations) will be labile. The degree of lability can be predicted by the statistical properties of the long-term memory store, the sampling-retrieval process, and the response scale characteristics (Tourangeau et al 2000).

What are the implications of constructed, and highly contingent, preferences (values) for such important applied problems as the measurement of consumer preferences for new products and for the design of future decisions [e.g. helping a person make a decision about medical treatment or assessing preferences as inputs into public policy decisions (Payne et al 1999)]? Research on the construction of preferences had some of its origins in practical applications such as the design of methods to elicit reliable preferences for nonmarket goods (e.g. Fischhoff 1991 and Slovic 1995). The original embedding, framing, and other context effects (oversensitivity and undersensitivity) are now well documented, and sophisticated theories of their sources have been proposed (Hsee et al 1999, Tversky et al 1988). However, the normative implications remain to be explored: Can we make a distinction between better-constructed and more poorly constructed preferences? What advice can we give people to help them construct better preferences? What are the implications of differences in quality of preferences for the practices of social choice?

Another important aspect of preferences and values concerns the construction of summary evaluations from memory and the prediction of future values and evaluative reactions to anticipated outcomes (see many of the papers cited in Kahneman et al 1999a, 1997). In addition, there is always the need to verify that

alternative expressions of preferences and values agree and, more importantly, to understand why they do not (e.g. Bernstein & Michael 1990).

Problem 9 (RD Luce, CF Camerer) What is the form of the function(s) that relate decision outcomes to personal values, satisfactions, and utilities? The best-known models of choice among uncertain alternatives have assumed (and have been tested to a degree) properties that, when suitably combined, imply a utility representation. Specifically, behavior can be represented in terms of a utility function over consequences and a weighting function over events. The utility function of a gamble (i.e. an uncertain alternative) is some sort of weighted sum of the utilities of the individual consequences. A common proposal is that the utility function is linear in utilities and linear in some function of the weights (i.e. bilinear, a multiplicative relationship), although there is contradicting evidence from some empirical studies (see Birnbaum 1999, Chechile & Butler 2000, and Luce 2000 for entry points into this literature). Theorists must confront this challenge to their hypotheses and try to understand what is going on. A major distinction is whether the consequences are treated as homogeneous or are sharply partitioned into gains and losses. Most current proposals assume the dual bilinear form: two bilinear functional systems with different forms in gains and in losses (Luce 2000) [This general algebraic model was probably first introduced in psychology under the label "configural weight model," (e.g. Birnbaum & Stegner 1979)].

Problem 10 (CF Camerer, A Chakravarti) How are reference points chosen and changed when expected values are inferred (within the context of a two-value function, mixed gains/losses theory)? To a psychologist, the traditional economic assumption that choices are among states of wealth, not increments or decrements of it, is counterintuitive. Some of the earliest psychological contributions to decision making commented on this unrealistic interpretation (Edwards 1954). The major break with that tradition occurred with the prospect theory's many demonstrations of the failure of the absolute gains and losses assumption, accompanied by the proposal of a labile reference point (Kahneman & Tversky 1979).

Many economists are hesitant to use the prospect theory (and other psychological models) because the central framing process forces the theorist to specify a reference point, usually the 0,0 point in an objective outcome by subjective value space. However, psychological principles about plausible reference points provide little guidance; as Heath et al (1999, pp. 105–106) noted: "We are proposing that whenever a specific point of comparison is psychologically salient, it will serve as a reference point." An open question remains: When is a reference point set and then reset? This freedom may be liberating to an empirical psychologist, but it is off-putting to a theoretical economist.

There have been many follow-up demonstrations of the lability of reference points via framing manipulations. The most common values hypothesized to be reference points are related to current, status quo conditions. However, various

authors have proposed that norms, expectations, levels of aspiration, foregone alternatives, and social comparisons also function as reference points (locations in a value function where the slope changes suddenly; Heath et al 1999, Kahneman 1992, Luce et al 1993, Thaler 1999). In addition, if multiple reference points are chosen, is their selection motivated by hedonic considerations? Do people pick the point of comparison that makes the decision maker happiest? This seems unlikely, because then we would all compare ourselves to the worst status or to the most pathetic people we could imagine to make ourselves feel good.

A closely related problem is partly solved by considering the contextual, comparative, and referential properties of the judgment of well-being or satisfaction: Many studies show that people in dramatically different life circumstances (e.g. paraplegics vs people in normal health or rich vs poor, etc) report similar levels of well-being. In many cases, however, poor people exert considerable efforts to become rich, paraplegics would pay a lot to regain use of their limbs, and those who are rich and in good health make strenuous efforts to remain in these conditions. How can these observations be reconciled (GF Loewenstein)? The answer to this question seems to be partly related to the solution of the reference point problem: Because a person's sense of well-being is heavily determined by their frames of reference, what are the comparisons they make to evaluate how well they are doing?

Questions About Uncertainty, Expectations, Strength of Belief, and Decision Weights

Problem 11 (JW Payne, AG Sanfey) What are the sources of the primitive uncertainties and strengths of beliefs that apply to our estimates, predictions, and judgments? Uncertainty is an essential element of our relationship with the external world; members of very different species also perceive and forage in an uncertain subjective world (i.e. their brains are "wired" to encode and manage uncertainty). Most research by psychologists (and others) has been directed at the updating and integration of several primitive strengths of belief, after they have been inferred (e.g. Anderson 1996, Schum 1994, Tversky & Koehler 1994, papers in Shafer & Pearl 1990). However, another challenging problem is concerned with the sources of primitive uncertainty—or, more basically, how does primitive uncertainty moderate or mediate our behavior?

One possibility is to address the cognition of uncertainty as a secondary impression associated with a primary judgment. We can be very confident of a judgment or estimate [e.g. "I'm sure that my answer, 'Hamburg has a greater population than Bonn,' is correct"; "I'm certain (in my belief that) my leg is broken"; or "I'm confident about my estimate that it will take me about 4 h to drive to Aspen today."] or not so confident. Under this interpretation, uncertainty is derived from aspects of the primary judgment (e.g. evaluations of the completeness, coherence, diversity, and credibility of evidence for the primary judgment or perhaps from

the feelings associated with the judgment process: Did it feel fluent, fast, and effortless or not?). This approach implies that several subtheories of uncertainty are needed because the secondary evaluation of certainty will differ for different primary judgment processes (see discussions of certainty conditions in Collins & Michaelski 1989 and Pennington & Hastie 1991).

A related theoretical proposal is that there are alternative modes or strategies for representing uncertainty: singular (episodic, intensional, or inside) vs distributional (aleatory, extensional, or outside). These representations are hypothesized to occur with different reasoning strategies or heuristics. For example, the representativeness and simulation heuristics seem distinctively singular, whereas the availability and anchor-and-adjust heuristics are possibly more distributional, as evidenced by their associated biases. There are also many interesting connections and (possibly misleading) relationships among different situations in which uncertainty is apparent: Is it just a coincidence that uncertainty about whether an event will occur has a similar impact on behavior as does uncertainty about when an event will occur (e.g. Mazur 1993)? Is uncertainty a fundamental metacognitive realization that our mental models of the world must be poor analogies to reality because they are models? Will a comprehensive psychological theory of inductive strength be the answer to our questions about primitive uncertainties, or is there something more involved in the psychology of uncertainty (Osherson et al 1990)?

Problem 12 (RD Luce) In the context of decisions, what are the determinants and the common forms of the decision-weighting function that allow probabilities and propensities to moderate the impact of consequences on decisions? The most common forms of expected and unexpected utility models assume that the central process in decision making is the combination of scaled value and expectation information via a multiplicative rule: for example, utility $=$ probability $\times$ value. In the most popular kinds of utility theory, fairly natural properties place severe constraints on the form of the utility function (for values). Specifically, the functions are from the one- or two-parameter family over (usually monetary) gains, and the same holds true for losses, with one parameter linking the two domains. Basically, these are exponential functions, linearly transformed so Utility$(0) = 0$, of a positive power of money. If the exponent of the power is taken to be 1, as it often is, then they are one-parameter functions; otherwise, they are two parameter functions.

The situation for decision weights, derived from expectations of occurrence or probabilities, in non-expected utility theories is much more poorly defined. One can deduce from a rationality assumption that the weights should be powers of probability (Luce 2000). In early studies, the results are reasonably consistent with power functions, but later studies, since Kahneman & Tversky (1979), suggest an asymmetric, inverse S-shaped function. We need to concentrate on finding out more about what limits the form of these weighting functions. Prelec (1998) has suggested plausible constraints on the function form, and one of his candidate functions is consistent with the best relevant empirical data (Gonzalez & Wu 1999).

However, there are still many open questions concerning the stability of these functions within individuals and across decision-making domains and even the range of alternative permissible functions (cf Weber 1994). It is important to note that the family of permissible functions should at least include power functions as a special case, because our theories should not preclude the possibility of some people behaving rationally.

As an aside, I am particularly enthusiastic about the theoretical justifications for the weighting functions and aspiration levels in the security-potential/aspiration theory (Lopes & Oden 1999). This theory neatly combines individual differences (security vs potential orientations) and situational attention factors (aspiration criteria) to yield psychologically plausible hypotheses. It has fared well in competitive tests against other rank- and sign-dependent models, at least in the domain of multioutcome lotteries.

Questions About Emotions and Neural Substrates

Problem 13 (F Loewenstein, E Peters, P Slovic, EU Weber) What is the role of emotions in the evaluation of experienced outcomes (consequences) and in the evaluation of expected outcomes? A major obstacle to the study of the role of emotions in decision making is that there is little consensus on a definition of emotion. A recent survey volume, *The Nature of Emotion: Fundamental Questions* (Ekman & Davidson 1994), does not provide a definition and notes that there was disagreement on virtually every fundamental question addressed. [In a summary section, "What Most Students of Emotion Agree About," the authors comment: "We originally did not include the word 'most' in the title of this section . . ." (p. 412); see discussion in Larsen & Fredrickson 1999.] For present purposes, I think three concepts will be useful: emotion, mood, and evaluation. I would propose the following definition of emotion: reactions to motivationally significant stimuli and situations, including three components: a cognitive appraisal, a signature physiological response, and phenomenal experiences. I would add that emotions usually occur in reaction to perceptions of changes in the current situation that have hedonic or valenced consequences. Furthermore, I propose that the term "mood" be reserved for longer-duration background states of the physiological (autonomic) system and the accompanying feelings. Finally, I suggest that the expression evaluation be applied to hedonic, pleasure-pain and good-bad judgments of consequences.

There seems to be agreement that an early, primitive reaction to almost any personally relevant object or event is a good-bad evaluative assessment. Many behavioral scientists have concluded that the reaction occurs very quickly and includes emotional feelings and distinctive somatic and physiological events (Bechara et al 2000, Loewenstein 1996, Zajonc 1980). Neuroscientists have attempted to describe the properties of the underlying neurophysiological response (Damasio 1994, LeDoux 1996, Rolls 1999), addressing such issues as: Is the reaction located on a unitary, bipolar, good-bad dimension; or are there two neurally independent

reactions, one (dopamine mediated) assessing positivity and one (acetycholine mediated) assessing negativity [the so-called "bivariate evaluative response system" (Ashby et al 1999, Cacioppo & Berntson 1994, Coombs & Avrunin 1977, Gray 1971, Ito & Cacioppo 1999, Lieberman 2000, Russell & Carroll 1999)]? There must be a discernable physiological difference at some point before the responses diverge into an approach or avoid reaction; but is it a deep, central difference or is it peripheral, at either the stimulus-processing or response-generation ends of the system?

The primary function usually attributed to the fast good-bad reaction is to guide adaptive approach-avoidance actions and, collaterally, to winnow down large choice sets into smaller numbers of options for a more thoughtful evaluation (e.g. Damasio's somatic marker hypothesis). The alternative functional interpretation in the decision-making literature is that emotions serve a crucial override function that operates when it is necessary to interrupt the course of an ongoing plan or behavior sequence in order to respond quickly to a sudden emergency or opportunity (Simon 1967; see also LeDoux 1996 on learned triggers).

The analysis so far is vague on the role(s) of the traditionally designated palette of emotions in the decision process (Ekman & Davidson 1994, Roseman et al 1996, Yik et al 1999). An important distinction is between emotions that are experienced at the time the decision is being made (decision-process emotions or just process emotions) and emotions that are anticipated or predicted to occur as reactions to consequences of a decision (consequence emotions) (GF Loewenstein, EU Weber, CK Hsee, ES Welch, unpublished manuscript). Thus, studies of mood or emotional state at the time a decision is being made fall into the first category [e.g. research on the effects of mood on estimates and judgments (DeSteno et al 2000, Forgas 1995, Johnson & Tversky 1983, Mayer et al 1992, Wright & Bower 1992) and research on stress or the effects of difficulty with trade-offs involved in the decision process (e.g. Luce et al 1999)], and studies of the effects of anticipated consequence-related emotions on choices fit into the second category (e.g. Loewenstein & Schkade 1999, Gilbert & Wilson 2000, Mellers et al 1999).

Problem 14 Are there different basic decision processes in different domains of behavior, or is there one evolutionarily selected fundamental decision module? To what extent are various habits or behavioral tendencies that are relevant to decision making context independent and general across behavioral contexts? Is there a central neural module or organ that computes decisions across different domains of activity, perhaps a neural unexpected utility calculator? Do people who are risky in recreational situations (e.g. rock climbing or skiing under extreme conditions) also take more risks in sexual, financial, or other domains? Is there an underlying evolutionary implication such that decision domains that pose related reproductive survival problems (e.g. mate selection, parental investment, health, shelter, social exchange, and within-species combat) elicit similar risk attitudes and decision strategies?

The evolutionary psychology research program pursued by Cosmides, Tooby, and colleagues is suggestive of domain-specific reasoning and decision modules (Barkow et al 1992, Cosmides & Tooby 1996). However, the question of how to define and evaluate claims about the generality vs context specificity of decision processes is still open. It would not be as interesting to discover that the peripheral perceptual or response aspects of the decision process are different in different domains as it would be to discover that that central information integration principle was different. Would it surprise anyone to learn that we attend to different attributes of potential mates and of potential shelters? However, it would be very important to learn that the unexpected utility principle applies in some domains (e.g. mate selection and competition) but not in others (e.g. health and social exchange).

Questions About Methods

Problem 15 (CF Camerer, B Fischhoff, KR Hammond, BA Mellers) What methods will allow us to best apply results from one situation to another, especially in generalizing from a simplified, controlled situation to a complex, uncontrolled nonlaboratory situation? How can we gain an understanding of the relationship between the conditions in our experiments and the situations in which decisions are actually made? The questions of generality of results and the scope of theoretical principles are, of course, critical for any experimental science. In the behavioral sciences, the usual argument for the generality of a finding begins with the interpretation of the existence of a phenomenon (e.g. a cause-effect relationship) as a prima facie case for its generality. Then the projectability of the result is systematically evaluated by examining each conceptual dimension along which variation occurs from one setting to the other target settings of interest (Hastie et al 1983). Thus, typically, progress on the problem of generalizability, projectability, or scope is made by discovering interesting empirical limits and testing feelings of skepticism in order to control the tendency to overgeneralize. This tactic identifies boundary conditions, the manipulation of which changes the prevalence of a response pattern. However, such an incremental process leaves us vulnerable when asked (by ourselves or others) to make summary statements about, say, the quality of human judgment or how people usually behave. Those statements are meaningless without specifying a universe of observations. However, all we usually have is a biased sample of anecdotal or experimental evidence. The laboratory should be a place to complement field studies and dissect theory, but it should not be a substitute for looking at other types of data (Fischhoff 1996).

There are two common methodological strategies used to address the generality problem: First, sample phenomena (subjects, tasks, stimuli, etc) as representatively as possible in research, and only generalize (from the original study) with confidence when you are replicating the previous situation (e.g. Gigerenzer et al 1999, Hammond et al 1986). Of course, there is still a crucial open question: What are the attributes or dimensions of situations that afford projection from a past finding to a future result? Second, identify fundamental causal mechanisms and forces that will

support inductive generalization. Under this imperative, research should focus on the identification of generative causal mechanisms—perhaps by studying unrepresentative situations, preparations, or samples—and then generalize based on deeper causal theoretical principles (Dawes 1996). Psychologists might well heed the thoughtful commentaries on the experimental method from practitioners who do not take it for granted [Lopes (1994); experimental economists Friedman & Shyam (1994), Kagel & Roth (1995); and experimental political scientists Kinder & Palfrey (1993)].

At present, the largest mutual enterprise in the field of judgment and decision making is the development of measurement-theoretical extensions of traditional utility theory and associated empirical research on evaluations of monetary gambles and lotteries [Luce (2000) provides a summary of this endeavor]. Many of the present problems are best understood within the context of that research program. The measurement-theoretical approach looks like a model scientific research program that is making visible progress on both theoretical and empirical fronts. However, this research program has one major weakness: How much can one generalize from the results of gambling studies in a laboratory and the resultant theoretical principles to important nonlaboratory decisions? Ironically, the primary research problem for this approach may be the methodological generalizability problem. Perhaps the enormous impact of prospect theory owes more to its strong case for the generalizability of its theoretical principles to real decisions in everyday social, economic, and political life than to its theoretical elegance and originality.

Problem 16 (NH Anderson) How can useful methods be developed to measure variables such as psychological uncertainty and personal value on true linear, equal interval scales? Linear, equal interval measurement is fundamental because of the multiple determinants of behavior: Virtually all perceptions, thoughts, and actions result from the integration of multiple determinants. Prediction and understanding both require linear scales. With even three competing response tendencies, monotone (ordinal) scales cannot generally predict even the direction of a response. This problem has been around for over a century, and it is central to the study of judgment and decision making because the measurement of values is central. However, measurement has been neglected in empirical work. Makeshift measurement is a common practice, notably in applied multiattribute analysis, research on social judgment, and in many studies of cognitive processes. Makeshift measurement is not necessarily wrong, and it is sometimes useful, especially in early research, to identify the major environmental and individual difference causes of behavior. Current conceptual frameworks have become successful by focusing on issues that can be attacked without true measurement. This was reasonable at first, but it is an inadequate foundation for general theory. Anderson (e.g. 1982, 1996) and Michell (1990, 1999) provide introductions to this profound and daunting problem.

AFTERTHOUGHTS AND CONCLUSIONS

There have been a few dramatic intellectual events in the recent history of the field of judgment and decision making. One is the sudden acceptance of non-expected utility theories with labile reference (inflection) points, separate value functions for gains and losses, and nonadditive probability weighting functions (e.g. see Luce 2000 and Birnbaum 1999 for overviews of these developments; and see Tversky & Kahneman 1992 for the best-known formulation). Another is the sudden popularity of cognitive heuristics models for judgment (Tversky & Kahneman 1974) and choice (summarized in Payne et al 1993). As an aside, the most notable historical event to occur during the years covered by this review was the untimely death in 1996 of Tversky, the greatest researcher of judgment and decision making of the twentieth century. (See Laibson & Zeckhauser 1998, McFadden 1999, and Rabin et al 1996 for assessments of Tversky's impact.) However, reactions to these important events have not been as dramatic as, for example, the response would be to a major mathematical proof or to a breakthrough discovery of the genetic code structure.

In 1995, when Andrew Wiles publicly presented his proof of Fermat's last theorem, it is likely that every major mathematician in the world knew of his achievement within 24 h. Is there any plausible analogous scenario in the scientific field of judgment and decision making? Would the solution of any of the problems we have posed result in the same kind of immediate, worldwide reaction? The sole possibility might be a genetic or neuroscience result (problems 13 and 14). For example, the identification of a specific genetic source of cross-situational general risk-taking habits or the localization of a neural circuit that computes probability-weighting or evaluation functions such as those proposed in expected utility theories might elicit a dramatic reaction. However, empirical and theoretical results in the decision sciences tend to disperse gradually, more like the conclusions of a Newton or an Einstein than like those of Watson and Crick, or a Wiles.

There are some aspects of the proposed 16 problems that are worth noting. First, some of the problems are already "on the table" and under investigation. The purpose of this review of these problems is to introduce them to readers who are not knowledgeable and to provide some recent context for those to whom these problems are already familiar. For the most part, these are the problems that I predict will be solved sooner," such as problems 1, 3, 8, 9, and 12–14. Other problems are my personal picks; they have received some attention but, in my view, not enough. My goal in mentioning these neglected problems is more exhortatory; my hope is that the present review will focus more attention on these issues in the future, especially problems 2, 4–7, and 11.

One of the most persistent metatheoretical issues for the field of judgment and decision making concerns the relationships among theoretical approaches. What are the relationships among the alternative theoretical descriptions, including connectionist-neural computational processing (e.g. Grossberg & Gutowski 1987,

Leven & Levine 1996), information processing (symbolic production systems; e.g. Lovett 1998, Payne et al 1993), traditional cognitive algebra (Anderson 1981, Birnbaum 1999), and measurement-theoretical algebra (Luce 2000, Tversky & Kahneman 1992)?

Are these actually different descriptions of different phenomena, or are they simply alternative notations to describe the same phenomena? One popular answer is that the cognitive process descriptions of phenomena are at a finer resolution than the algebraic models; that the cognitive descriptions actually capture individual thought processes at a causal level, whereas the smoother, continuous, quantitative, algebraic representations describe averages of those cognitive processes (e.g. Lopes 1996 and Oden & Lopes 1997; but see Anderson 1996 for a different view). I believe that further consideration of the relationships among theoretical frameworks is a worthy metaproblem and would not be merely an exercise in academic hairsplitting. In addition, an even greater issue concerns the selection of a behavioral theoretical representation that will effectively guide and incorporate developments in cognitive neuroscience.

"As long as a branch of science offers an abundance of good problems, so long is it alive. . ." (Hilbert 1900, p. 444). By Hilbert's abundance of problems criterion, the field of judgment and decision making is in excellent health. The fundamental obstacle in preparing this review was the difficulty of choosing the problems to exclude from among the diversity of inviting prospects. The review is organized around problems for future research because I believe that research in our young scientific field will be improved by thinking harder about the nature of the problems we should be attacking. Perhaps the most important step in successful research (as in writing a good review) is the selection and definition of the research problem. I believe that the wise fictional detective Father Brown could have been describing many scientists when he commented: "It isn't that they can't see the solution. It's that they can't see the problem" (Chesterton 1951, p. 949). Researchers on decision making, of all people, need to be reminded that they are placing a bet when they choose their research direction.[4]

Moreover, there is evidence from research that evaluations of uncertain prospects are better when they are made in the context of alternative courses of action than when made in isolation (Hsee et al 1999, Read et al 1999).

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[4]It is interesting (and humbling) to remind ourselves that although David Hilbert had a wonderful eye for good problems, his own bet on the development of a general, uniform method for mathematical proof was a loser, but perhaps it was still a good bet in 1900 (cf Gödel 1934).

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