

Commentary

JUDGMENT AND DECISION MAKING: A Personal View

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The large number of textbooks recently published identifies the field of judgment and decision making as one of the areas of psychology in which research activity grew most rapidly during the past two decades. The enthusiasm is easily explained: The topic has much to make it appealing to investigators. Its focus is a large puzzle that will not go away—a search for the bounds of human rationality. It includes a deep normative theory that offers criteria for rational action. It is also rich in amusing anecdotes and challenging brain teasers. The study of judgment and choice occasionally sheds light on events in the real world, including the decisions of world leaders, the foibles of the market and the pitfalls of medical diagnosis. The doubts that psychologists have raised about the rationality of human agents are having a modest effect on neighboring disciplines, such as economics and political science, in which the assumption of human rationality is often used to predict the outcomes of competitive interaction. The detailed study of bounded rationality also has implications for the human engineering of information systems, decision aids and organizational procedures. The following observations sketch a personal view of this exciting field, its history, accomplishments and limitations, and possible future.

LOOKING BACK

My involvement in the study of judgment and decision making began in 1969, in a fortunate collaboration with my colleague Amos Tversky, who was then a young star in the field. Many features of today's intellectual landscape already existed; the field had been shaped fifteen years earlier, by a striking conjunction of important contributions in a three-year period. Ward Edwards defined the domain in a classic *Psychological Bulletin* article in 1954, by bringing together concepts from economics, decision theory and psychology. Edwards also led a vigorous research program that compared actual performance

to idealized models of decision and inference, and trained several of the leaders of the next scientific generation in that program. Luce and Raiffa offered in 1957 a crisp introduction to the logic of decision under uncertainty. In 1955 Herbert Simon introduced the concept of bounded rationality, which in effect defines the problem that the field has been trying to solve ever since. Paul Meehl's famous study of the superiority of actuarial over clinical prediction, published in 1954, dramatically illustrated both the limitations of clinical intuition and the unreliability of subjective confidence as a measure of accuracy. At about the same time, Kenneth Hammond extended to clinical judgment the lens model that Egon Brunswik had previously applied to depth perception, by comparing the correlations of a set of cues with a clinical criterion and with clinicians' predictions of the same criterion.

From its inception in the 1950's the psychological study of decision making and judgment under uncertainty has been characterized by the following features: (i) The critical role of the normative theory of rational belief and choice. This theory sets standards to which subjects' behavior will be compared, and provides null hypotheses for behavioral research; (ii) The emphasis on risky choice, and the relative neglect of the problem of decisional conflict, which had preoccupied Kurt Lewin and was also central to the work of Leon Festinger; (iii) The preference for analyses that invoke cognitive or psychophysical terms, and the relative neglect of emotional and social factors.

The development in which I took part, along with Tversky and many other friends and colleagues, is sometimes called the "heuristics and biases" approach. It was characterized by two new features: (iv) The emphasis on cognitive processes described at an intermediate level of generality (heuristics of judgment, framing operations). Characteristics of these processes are used to predict and explain diverse phenomena of judgment and choice, usually departures from the rational model; (v) A research method that favors simple between-subjects experiments drawing on subjects' life experience. Key demonstrations, included in the text in the manner of the Gestalt psychologists, provide readers with personal experience of the phenomenology of error.

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HEURISTICS AND BIASES

Two decades of research on heuristic and biases by many investigators have yielded a substantial list of explanatory processes or mechanisms, and a longer list of empirical generalizations or "effects." The more useful explanatory notions have been linked to multiple effects. An example is the representativeness heuristic, which has been used both to explain beliefs about chance and sampling and to account for predictions of the behavior of individual persons. Another example is loss aversion, the asymmetry in the valuation of losses and gains, which has been used to explain both the extreme reluctance to accept fair bets on even chances and the general bias in favor of the status quo. Other effects, such as hindsight, overconfidence, optimistic bias, and escalation of commitment (the sunk-cost effect), probably reflect the operation of multiple mechanisms. And in some cases the distinction between effect and process is moot. An example is the concept of compatibility, which is used, in the domain of judgment and choice as well as in the domain of stimulus-response relations, both to summarize and to explain results (Tversky, Sattath and Slovic, 1988).

The research agenda has been shaped jointly by predictions from analyses of heuristics and by the idealized models of rational performance that define standards for the measurement of biases. In studies of judgment, intuitive assessments have been compared to standards of Bayesian inference, sampling statistics, and regression analysis. In the domain of choice, standards are usually derived from expected utility theory. The scope of the field has expanded in recent years by applying to new domains the general strategy of testing null hypotheses derived from a normative model. Such developments are in progress in the context of riskless choice (Tversky, Sattath and Slovic, 1988; Kahneman, Knetsch and Thaler, 1991), choices about delayed or extended outcomes (Loewenstein and Prelec, 1991; Varey and Kahneman, 1991), behavioral game theory (Camerer, 1990) and negotiations (Neale and Bazerman, 1991).

Progress in the study of heuristics and biases has been made not only in breadth, by the identification and demonstration of new effects, but also in depth, by increasing the precision with which concepts are used. An early example of such progress was a distinction that Ajzen (1977) drew between two types of base rate information. Statistical base rates, which merely characterize the population from which a case is sampled, are relatively neglected in judgments and predictions about individual cases. In contrast, causal base rates strongly affect predictions: the proportion of students passing an exam reflects its difficulty, and this information will be used to predict whether a particular student passed it. Another

example of progress by differentiation is the distinction between off-line judgments, which depend on retrieval from memory and are therefore susceptible to availability biases, and on-line judgments which are modified in real time as the information is received (Hastie and Park, 1986). Kunda and Nisbett (1986) showed that people are most likely to use statistical reasoning in thinking about chance events, rather less prone to apply the same ideas to repetitive events such as competitive sports, and least likely to do so in making predictions about personal traits of individuals. A noteworthy aspect of these studies is that they are characteristic of a cumulative discipline, where useful early ideas are preserved, even as they are qualified and improved. An influential contribution to this "third generation" research approach is the concept of contingent weighting that Tversky has applied to a broad class of preference reversal effects, where different ways of eliciting a ranking of alternatives (for example, choice versus pricing) yield systematically different preference orders.

A common complaint against the field is that it lacks theory, but in my opinion such complaints exaggerate the difference between theories and less orderly collections of facts and concepts. I take the distinctive feature of theory to be a commitment to completeness (within reason) and a consequent commitment to critical testing, in a specified *domain of refutation*, which is often quite narrow. For example, the domain of refutation for a theory of choice that Tversky and I proposed was restricted to gambles with at most three outcomes and stated numerical probabilities; some important hypotheses were further restricted to gambles with monetary outcomes. Prospect theory does well in empirical competition against similarly restricted theories (Camerer, 1989), but the success must be seen in perspective: A successful theory of three-outcome monetary gambles with stated probabilities would be quite useless except for the light that it may shed on *other* situations. When applied outside its domain, however, prospect theory becomes a collection of processes and empirical generalizations: rules of framing, the certainty effect, loss aversion and others. We should distinguish between the context of refutation and the context of use: A theory can only be refuted in its specified domain, but it is likely to be most useful outside it. In this light, too much is made of the special virtues of theory.

TOO MANY BIASES?

A salient characteristic of current research in judgment and decision making is its emphasis on anomalies that violate normative standards of belief or choice. A common strategy is to identify a factor that should *not* affect judgment or decision, then design an experiment in

which it does. Thus, an uninformative message can produce anchoring: subjects exposed to different anchors will make different estimates of, say, the population of Turkey or the likely selling price of a house. Similarly, the framing of outcome statistics in terms of mortality or survival can affect the attractiveness of medical treatments: 10% mortality is more frightening than 90% survival. In these cases and in many others, subjects are influenced by a factor that they would wish to ignore, or in some way fail to meet a standard that, upon reflection, they would accept.

There is no denying that errors of judgment and choice are often quite interesting. However, the focus on error in current research is not due to this fact, but to two standard features of psychological methodology. The first is that the determinants of normal performance are commonly studied by inducing failures: Memory is understood by investigating forgetting, and visual illusions contribute to the understanding of visual constancies. In the present context, the heuristics of judgment and choice are identified by the biases they tend to produce. The second reason for the emphasis on errors is that the proximal objective of most psychological research is the rejection of a plausible or otherwise respectable null hypothesis. We study errors because the logic of belief and choice is a rich source of null hypotheses, and because the prestige of the rational model makes these null hypotheses sufficiently interesting to deserve refutation.

The extensive literature on base rate neglect suggests that the urge to refute is stronger than any urge to find fault in human nature. An early paper on the psychology of prediction suggested that information about the base rates of outcomes has little or no weight when individualizing information about the case at hand is available. This claim provided subsequent investigators with an attractively implausible null hypothesis: that subjects are *completely* oblivious to a factor that the normative theory designates as relevant. Twenty years and dozens of studies later, articles rejecting this hypothesis are still appearing. The null hypotheses derived from rules of rationality have not been attacked with greater zest.

The refutationist spirit of psychology is hardly an un-mixed blessing. Its most dubious manifestation is that our professional standards assign greater originality to a study that claims the total destruction of an earlier position than to one that only restricts or modifies an existing idea and thereby improves it. This perverse incentive system spawns caricatures of refutation—studies that refute caricatures of competing positions. A unique advantage of the field of judgment and decision making is that it offers the luxury of refutation without the costs of destructive controversy. The normative ideas that define most of the null hypotheses that we test are not destroyed by the rejection of these hypotheses; their normative sta-

tus is immune to psychological observations. I attribute the rapid and cumulative progress of the field in the past two decades at least in part to the coincidence that directs our refutationist tendencies to the discovery of new phenomena. It is ironic that the cumulative advance of research may also benefit from the scarce supply of powerful and explicit behavioral theories in the field, and of the null hypotheses that such theories imply.

I have attributed the emphasis on errors and biases to the method of psychological research, rather than to a generally negative view of human nature. Even if this interpretation is accepted, a critical question can still be asked: Has recent research on judgment and choice drawn a caricature of its subject and unjustly denigrated human reason? There are several stock arguments to that effect, and stock answers to each: (i) A philosophical critique is that it is improper to describe people as making systematic errors, either because shared intuitions are the sole criterion of rationality, or because the normative theory is itself controversial. This criticism appears to have little substance, because the psychological analysis of observed effects does not depend on labelling them as errors, and because it is often easy to show that people not only violate abstract rules, but that they behave in ways that do not achieve their own goals and do not meet their own standards; (ii) From economists we often hear the comment that our subjects are not paid enough to induce them to think correctly. However, incentives only eliminate careless errors, not the more severe cognitive illusions (Arkes, 1991). The limited effects of high stakes and of social accountability in inducing rationality are also demonstrated, unfortunately, by many decisions about which we read in newspapers; (iii) Another critique is that the deficiencies of human reasoning and choice could be corrected by learning. This is undoubtedly true in some cases, but it is surely of interest to note that at the time of testing the subjects have not *yet* learned to make regressive predictions or to ignore sunk costs, even if they could perhaps be trained to do so in a particular context. Furthermore there is much evidence that experts are not immune to the cognitive illusions that affect other people; (iv) Finally, it is sometimes said that the demonstrated biases of judgment and choice are laboratory curiosities, but the implied general claim that cognitive illusions vanish in the real world is simply false. The persistence of biases in the judgments and decisions of people in their natural environment has been richly documented (Gilovich, 1991).

LOOKING FORWARD

Much of what we know about risky choice has been learned by examining how people choose between simple gambles with stated probabilities. This simple experi-

mental paradigm captures an essential aspect of many decisions: Breaking up with a companion, accepting a job, even buying a house—all are aptly construed as gambles because the consequences of these actions and of their alternatives are uncertain. However, other potentially important features of real-world decisions are not represented: There is more scope for hindsight and regret when choices depend on guesses about a complex situation than in a game of chance; many choices are made sequentially, rather than in isolation; important choices often represent a commitment to a prolonged game of skill rather than to a one-shot roll of the dice; significant decisions are made in a social and emotional context, rather than in experimental anonymity. Studies of these variables require more complex designs than we have used in the past, but the difficulties appear tractable and the effort is essential. In the spirit of what I have called "third-generation" research, an early objective might be to investigate the robustness of the findings obtained in the simpler situation. We should expect to be surprised by many of the results.

It is to be expected that future research will be less exclusively concerned with cognitive factors in judgment and choice. A striking indication of the current bias is that the field treats irrationality as a failure of reasoning; the debate on human rationality focuses on such topics as the Allais paradox or overconfidence in judgment, rather than on impulsive or self-destructive behavior. We are therefore ill-equipped to understand the actions of a national leader caught in a contest of will, the contraceptive choices of a teenager, or indeed the behavior of a compulsive gambler. Current textbook coverage implicitly suggests that the analysis of such problems belongs to other areas of psychology, but this could change.

The cognitive interpretation of judgmental biases was in part a reaction to earlier treatments that viewed beliefs as motivated, but the reaction went too far. Thus, the most important judgmental bias may well be the unreasonable optimism with which most people approach their future and the consequences of their actions (Taylor and Brown, 1988). Although cognitive factors contribute to this bias (Kahneman and Lovallo, 1991), a satisfactory account of wishful beliefs will surely involve emotional and motivational factors. It makes little sense for the boundary of the field to pass through the middle of this problem.

The segregation of cognitive and psychophysical from emotional and social factors in the study of choice is in large part an accident of research traditions. However, the paradigm of contrasting behavior to a rational model is readily extended to the study of questionable emotional priorities in decisions. Examples are situations in which

people are affected, more strongly than they would wish to be, by envy, embarrassment, or the fear of regret. In this spirit, Tversky and Shafir (1991) have documented the absurdities of procrastination and other manifestations of conflict avoidance. In a different approach to the role of emotions, Kahneman and Snell (1990) have discussed the relevance of hedonic psychology to rational decisions. To take but one example, a sensible decision about a renal transplant requires not only an assessment of the odds of organ rejection, but also a prediction of future emotional adjustment to dialysis. Accurate prediction of future emotions and tastes is required for rational action, and preliminary results suggest that this prediction task can be quite difficult. As these isolated examples illustrate, the next generation of texts in judgment and decision making will probably cover a substantially expanded field.

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