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Amos Tversky and the Ascent of Behavioral Economics

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Abstract

Amos Tversky investigated and explained a wide range of phenomena that lead to anomalous human decisions. His two most significant contributions, both written with Daniel Kahneman, are the decision-making heuristics—representativeness, availability, and anchoring—and prospect theory. Tversky's concepts have broadly influenced the social sciences. In economics, they gave rise to the burgeoning field of behavioral economics. This field, skeptical of perfect rationality, emphasizes validation of modeling assumptions, integration of micro-level data on decisions (including experimental evidence), and adoption of lessons from psychology. Tversky's contributions are reviewed, assessed using citation analysis, and placed in historical context. Fertile areas for behavioral economics research are identified.

Key words: Behavioral economics, decision theory, heuristics and biases, prospect theory, psychology and economics, rationality

JEL Classification: D81 (Criteria for Decision-Making under Risk and Uncertainty), A12 (Relation of Economics to Other Disciplines), C9 (Design of Experiments)

The assumption of individual rationality undergirds economics. This critical foundation stone permits the application of maximizing methods from mathematics, providing a parsimonious and extremely productive framework for empirical validation. The rational actor model makes sharp, testable predictions. Rationality also has important normative implications. In the absence of externalities, the confluence of rational, maximizing individual actions generates an efficient outcome for society as a whole.

As this analytically elegant and normatively sanguine theoretical edifice was being refined thirty years ago, Amos Tversky began to uncover flaws in the rationality foundation. Tversky—trained in psychology, with a strong background in mathematics and philosophy—was little noticed by the economics profession until 1979, when he and Daniel Kahneman published "Prospect theory: an analysis of decision under risk," in *Econometrica*, a journal steeped in rigor and prestige. That paper, which uses the language and models of economists, provides a well elaborated descriptive theory of decision

making which contrasts starkly with the normative theory due to Ramsey, Savage, and von Neumann-Morgenstern.

In the ensuing years, until his untimely death in 1996, Amos Tversky increasingly engaged and captured the friendship of the economics profession, a process made easier by his charm and extreme open-mindedness. Possessed of a brilliant mind, strong mathematical skills, and keen insights into economics, he was well equipped to challenge that discipline. But he sought instead to understand it; if he erred, it was in the modesty of his claims. In a field where boisterous intellectual jousting is common, Tversky, though no shrinking violet, let his ideas and his work speak for him. He did not seek to emulate economists when their approach might have put him on shaky ground. For example, he focused overwhelmingly on describing behavior, generally leaving prescriptive pronouncements to others. In addition, he was cautious in his extrapolations, say from established descriptive principles of laboratory choices to more general descriptions of economic behavior.

Today there is a burgeoning field called behavioral economics, which attempts to develop for economics the lessons set forth by Tversky and his intellectual comrades. Folk wisdom holds that "Prospect theory", with 1703 cites as of 1996, is the most-cited paper ever published in *Econometrica*. Our paper seeks to identify the specific contributions that Tversky and his coauthors have made to date, and to project possible contributions in the future. As important as these contributions will be, Tversky's greatest gift to economics will probably prove to be his demonstration of the potential of and need for behavioral economics. He showed that nonrational behavior can be identified and predicted, and that it has important implications for real world economics.

Amos Tversky awed economists with his powerful demonstrations of the gap between our theories and real behavior. He opened us to wonder, much as a good science museum does for a child. Though Tversky's results often questioned basic assumptions, he was neither a scold nor a proselytizer. He won adherents through the strength of his results, which were simply presented and whittled down to essentials. We turn now to the tale of his contributions to economics, past and prospective. Section 1 provides a brief survey of Tversky's major research findings. Section 2 scurries to his citation record to justify our selections and to quantitatively evaluate Tversky's impact on social science. Tversky's discoveries and contributions are weighed in historical context in Section 3, wherein we label him a cognitive archeologist. Section 4 discusses the central unresolved questions in the behavioral economics research program. Section 5 forecasts the next decade of research and the potential accomplishments of behavioral economics. Section 6 sums up. Each section can stand on its own.

1. Tversky's ideas

Perfect optimality serves as a benchmark for orthodox proponents of rational choice. They do not, however, presume that decision-makers always choose in perfectly optimal fashion. Actual decisions involve some error. The rational choice advocates assume that to

predict these errors is difficult or, in the more orthodox conception of rationality, impossible.

Tversky's work rejects this view of decision-making. Tversky and his collaborators show that economic rationality is systematically violated, and that decision-making errors are both widespread and predictable. This now incontestable point was established by two central bodies of work: Tversky and Kahneman's papers on heuristics and biases, and their papers on framing and prospect theory.

In this section, we briefly review these pivotal findings. We believe that these major subsets of Tversky's work represent the most important intellectual contributions that Tversky made to social science. Our assertion is substantiated by Tversky's citation record, which is assessed in Section 2.

First we hope to quickly familiarize or remind readers of the most important components of Tversky's research program. The remainder of our paper relates that research program to the rest of economics. Readers seeking a more extensive discussion of Tversky's research findings should consult Rabin (1996). Readers who are already very familiar with Tversky's work may want to skip immediately ahead to Section 2.

1.1. Heuristics and biases

In the early 1970's, Tversky and Kahneman published a pathbreaking series of papers that focus on judgments about the likelihood of uncertain events. This work is summarized in a 1974 *Science* article, "Judgement under uncertainty: Heuristics and Biases," whose dramatic impact has spread across the social sciences.[1] In this body of research, Tversky and Kahneman argue that people rely on a limited number of heuristic principles to simplify complex probability judgments. These heuristics usually work well, but in some contexts they lead to severe and systematic errors. Most of this research centers on three heuristics: representativeness, availability, and anchoring. Tversky and Kahneman document dozens of experimental decision-making anomalies that can be traced to these three simple decision-making short-cuts.

The representativeness heuristic captures the idea that probabilities are evaluated by the degree to which an event or object is representative of a class of events or objects. Here the word representative signifies meanings like "resembles," "is similar to," or "looks like."

> For an illustration of judgment by representativeness, consider an individual who has been described by a former neighbor as follows: "Steve is very shy and withdrawn, invariably helpful, but with little interest in people, or in the world of reality. A meek and tidy soul, he has a need for order and structure, and a passion for detail." How do people assess the probability that Steve is engaged in a particular occupation from a list of possibilities (for example, farmer, salesman, airline pilot, librarian, or physician)? In the representativeness heuristic, the probability that Steve is a librarian, for example, is

> assessed by the degree to which he is representative of, or similar to, the stereotype of a librarian. Indeed, research with problems of this type has shown that people order the occupations by probability and by similarity in exactly the same way." (1974 p. 1124)

When decision-makers use similarity judgments as a stand-in for probability judgments their probabilistic inferences will be biased. "Steve"—described above—is similar to a librarian, but he is unlikely to be a librarian if he was originally chosen randomly from a population of 95 physicians and 5 librarians. As predicted by representativeness, subject inferences are insufficiently sensitive to such underlying base rates. Analogous arguments imply that subject inferences will be insensitive to sample size.

> [Subjects] assess the likelihood of a sample result, for example, that the average height in a random sample of ten men will be 6 feet, … by the similarity of this result to the corresponding parameter (that is, to the average height in the population of men). The similarity of a sample statistic to a population parameter does not depend on the size of the sample. Consequently, if probabilities are assessed by representativeness, then the judged probability of a sample statistic will be essentially independent of sample size. Indeed, when subjects assessed the distributions of average height for samples of various sizes, they produced identical distributions. (p. 6)

Representativeness predicts many other documented anomalies of inference, including, a belief in the "law of small numbers," according to which even small samples are expected to be representative of the populations from which they are drawn; and misconceptions of regression to the mean, notably a failure to appreciate that an outlier event tends to be followed by a less extreme event, an outcome which does not well represent its precursor. For a complete list of representativeness biases—as well as the availability and anchoring biases discussed below—see Tversky and Kahneman (1974).

Tversky and Kahneman's second heuristic, availability, derives from the hypothesis that people assess the probability of an event by the rate or ease with which instances or occurrences can be brought to mind. For example, I infer the probability that I will die in a plane crash by recalling historical instances of deadly plane crashes. Whenever our capacity to imagine such instances is affected by factors other than the event's frequency, the availability heuristic will systematically bias the probability estimate. Consider salience or familiarity, which influence the retrievability of instances. Tversky and Kahneman read subjects a name list comprised of famous personalities of one sex and an equal number of less famous personalities of the other. The subjects concluded that the list was disproportionately comprised of the sex associated with the relatively famous names. Similarly, consider availability biases due to the differential effectiveness of a search technique or to differential imaginability. For example, because it is much easier to mentally search for words that begin with *r* than it is to search for words in which *r* fills the third position, subjects mistakenly conclude that the former event is more common.

Tversky and Kahneman's third heuristic, anchoring, is based on the observation that people solve problems by starting from an initial guess or salient starting point that is then adjusted to generate a final answer. Such adjustments are typically insufficient: the final

answer is biased (or anchored) toward the often arbitrary starting guess.[2] To assess anchoring, Tversky and Kahneman asked subjects to estimate various percentages (e.g., of U.N. countries that are African). Before determining their answer, subjects were shown a wheel of fortune which was spun and allowed to settle on an arbitrary value. Subjects were asked to indicate whether their answer was higher or lower than the value on the wheel and then asked to determine their final guess about the actual percentage. The median estimates of the percentage of African countries were 25 and 45 for groups that received spins of 10 and 65 respectively.

Tversky and Kahneman argue that anchoring explains several well-documented decision-making biases, including overestimates of the probability of conjunctive events and underestimates of the probability of disjunctive events: "The stated probability of the elementary event (success at any one stage) provides a natural starting point for the estimation of the probabilities of both conjunctive and disjunctive events. Since adjustment from the starting point is typically insufficient, the final estimates remain too close to the probabilities of the elementary events in both cases. (p. 1129)" Anchoring effects also explain overconfidence, the experimental finding that subjects' self-reported confidence intervals are invariably too narrow. For example, in most studies 30 percent of assessed quantities fall outside of subjects' 98 percent bounds. When forming their confidence bounds, subjects anchor on the 50th percentile, and fail to adjust adequately when forming their 1st and 99th bounds. Such an interpretation is supported by the finding that subjects actually display the opposite bias when asked to estimate the likelihood that true values will fall below a pre-given lower bound (the median of 10 percent confidence bound of other subjects). Now subjects exhibit conservatism, or underconfidence, since they anchor on even odds, and fail to adjust toward zero.

1.2. Framing and prospect theory

In the mid-seventies, Tversky and Kahneman changed directions and embarked on a new research program; it would prove to be as pathbreaking as their earlier work on heuristics. Thus they led a second intellectual revolution within a single decade. The original work on heuristics determined how probabilistic inferences are made. The new work took probabilities as an objective primitive, and asked how consumer choices are shaped by these probabilities and their associated outcomes. This new work was even more directly connected to economics. Indeed, its most fundamental document was published in *Econometrica* (1979): "Prospect theory: an analysis of decision under risk."[3]

In this body of research, Kahneman and Tversky demonstrate that subjects' choices of lotteries exhibit a wide range of anomalies that violate expected utility theory. Most importantly, they show that predictable and dramatic shifts in preference can be generated by changing the ways in which options are framed:

> It is often possible to frame a given decision problem in more than one way. Alternative frames for a decision problem may be compared to alternative perspectives on a visual scene. Veridical perception requires that the perceived relative height of two neighbor-

> ing mountains, say, should not reverse with changes of vantage point. Similarly, rational choice requires that the preference between options should not reverse with changes of frame. Because of imperfections of human perception and decision, however, changes of perspective often reverse the relative apparent size of objects and the relative desirability of options. (Tversky and Kahneman 1981, p. 453)

Many of Kahneman and Tversky's early framing experiments focus on manipulations that switch options from the gain frame to the loss frame. These experiments show that preferences exhibit risk aversion when lotteries are framed as gains, and risk seeking when lotteries are framed as losses. Consider the following, now classic, example (Kahneman and Tversky 1979). Subjects who have already been given \$1000 (hypothetically) are subsequently asked to choose one of two lotteries: a certain reward of \$500 or a 50% chance of earning \$1000. The overwhelming majority of subjects make the risk averse selection. A different sample of subjects are given \$2000, and also asked to choose a lottery: a certain loss of \$500, or a 50% chance of losing \$1000. Now an overwhelming majority of subjects make the risk seeking selection. But, the two scenarios are actually identical. By collapsing lotteries, one can see that both sets of subjects have been asked to choose between a certain reward of \$1500 and a lottery with a 50% chance paying off \$1000 and a 50% chance of paying off \$2000.

Two other fundamental anomalies of risky choice play central roles in prospect theory. First, the utility of risky lotteries is not linear in outcome probabilities. For example, certainty outcomes are special: changing probabilities from 0 to .01 or .99 to 1 has a disproportionate impact on preference, as compared with going from say .01 to .02 or .98 to .99. Finally, they showed that losses loom much much larger than gains, an asymmetry of such magnitude that it can not be explained by income effects or curvature in the classical utility function. Consider a lottery that offers a 50% chance of losing x dollars ($0 < x < 100$) and a 50% chance of earning $2x$ dollars. Despite the two-for-one payoffs, the typical subject assigns this lottery a certainty equivalent of \$0, exhibiting a preference that Kahneman and Tversky call loss aversion.[4]

Such paradoxical examples led Kahneman and Tversky to abandon the expected utility model in favor of a more behaviorally realistic alternative: prospect theory. Unlike traditional economic theories, which deduce implications from normative preference axioms, prospect theory takes an inductive/descriptive approach. It may be helpful to think of prospect theory as a parsimonious summary of most of the important risky choice anomalies.

Prospect theory assumes that lotteries, or 'prospects,' are evaluated in a two step process: an initial phase of editing and a subsequent phase of evaluation. In the editing phase, the lottery outcomes are coded as gains or losses relative to some reference point, which is usually the current asset position, but may be influenced by the presentation of the lottery or expectations of the decision-maker.[5]

The evaluation phase utilizes a value function $v(\cdot)$ and a probability weighting function $\pi(\cdot)$. Consider a lottery with three outcomes: x with probability p, y with probability q, and the status quo with probability $1 - p - q$. The prospect-theoretic value of the lottery is given by:

$$\pi(p)v(x) + \pi(q)v(y)$$

Note that the argument of the value function is the lottery payoff, which is the change in, not the level of, wealth. In prospect theory, the carriers of utility are gains and losses measured against some implicit reference point.

> Our perceptual apparatus is attuned to the evaluation of changes or differences rather than to the evaluation of absolute magnitudes. When we respond to attributes such as brightness, loudness, or temperature, the past and present context of experience defines an adaptation level, or reference point, and stimuli are perceived in relation to this reference point. Thus, an object at a given temperature may be experienced as hot or cold to the touch depending on the temperature to which one has adapted. The same principle applies to non-sensory attributes such as health, prestige, and wealth. (1979, p. 277)

The value function is assumed to be concave in gains and convex in losses, a pattern which is consistent with the experimental evidence on domain-sensitive risk preferences. The curvature of the value function is also consistent with the psychometric theory that as deviations from a reference point increase, those deviations are experienced with diminishing marginal sensitivity. Finally, to capture loss aversion, the value function is assumed to have a kink at the reference point, with a slope ratio of two to one. This value function is drawn in Figure 1. With this value function, it is easy to see how framing effects arise. The risk preferences of prospect theoretic consumers depend critically on whether good outcomes are framed as gains or as the avoidance of losses.

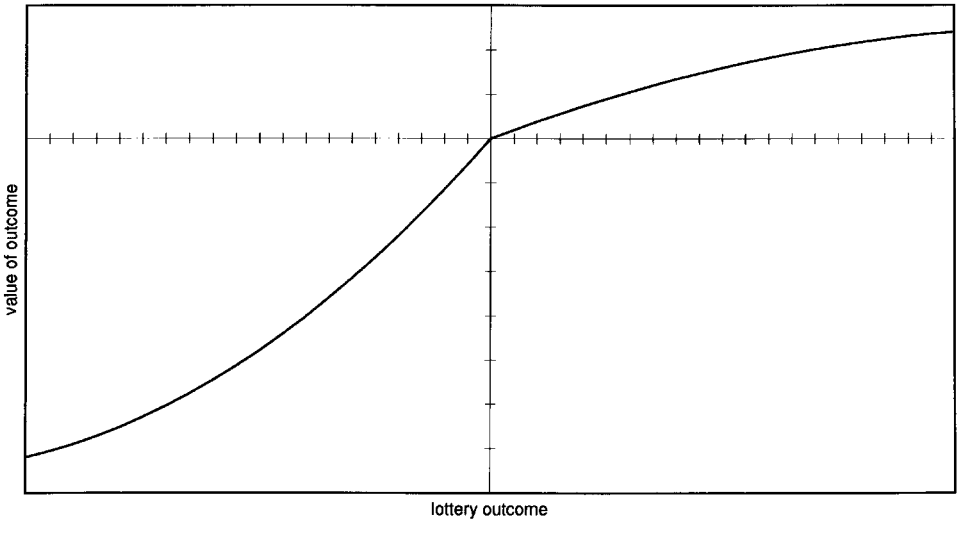

Figure 1. Prospect-Theoretic Value Function

The probability weighting function is the second major component of prospect theory. Kahneman and Tversky's experimental results imply an *S*-shaped curve (Figure 2). Like their value function, the probability weighting function should also be interpreted as exhibiting diminishing marginal sensitivity. For the probability weighting function, the diminishing sensitivity occurs with respect to the benchmark case of certainty. Note that the certainty effect applies at both probability zero and probability 1 events. As probabilities move further away from these end-points, the probability weighting function flattens out.[6] Finally, experimental results reveal that this curve tends to lie disproportionately below the 45 degree line, as shown in Figure 2.

Two important implications of the probability weighting function should be noted. First, the overweighting of small probabilities implies that decision-makers will make risk-seeking choices when offered low probability, high-reward lotteries. Second, the extreme underweighting of high probabilities makes complete insurance very attractive.

In Section 3 we discuss the ways that Tversky's findings have dramatically changed the field of economics. Before turning to that subjective analysis, however, we first consider a formal quantitative analysis of Tversky's impact.

2. A quantitative analysis of Tversky's intellectual contributions

Economists tend to quantify the things they study. With the advent of electronic citation databases, it is now possible to try to quantify the impact of particular academic ideas.

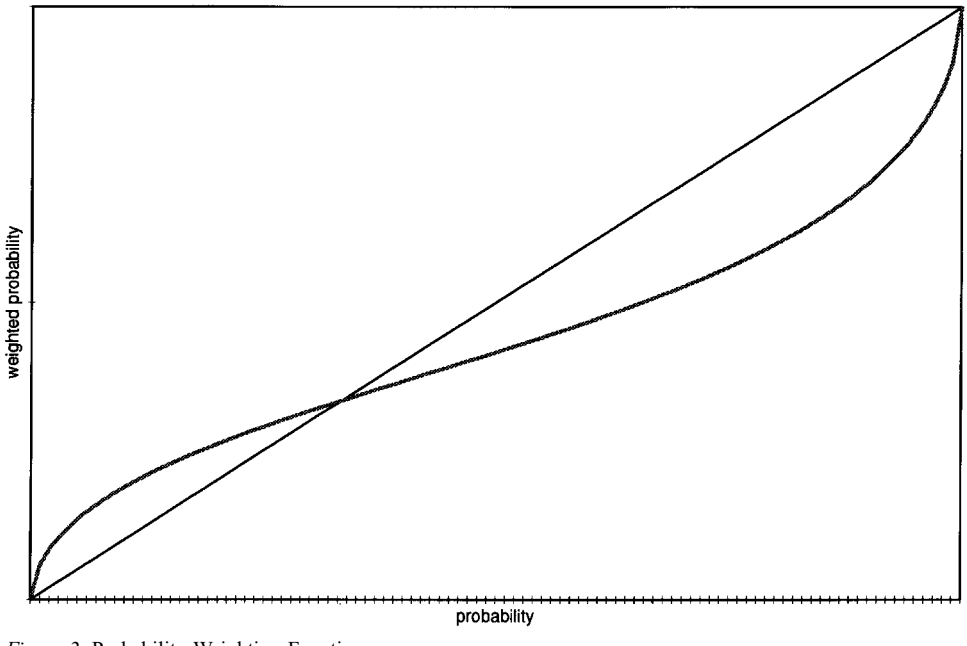

Figure 2. Probability Weighting Function

There are multiple reasons to be skeptical of such efforts at numerical measurement, but we believe that such analysis provides an objective complement to the subjective opinions that are the mainstay of academic evaluations.

To document Tversky's citation record, we use the Social Science Citation Index, compiled by the Institute for Scientific Information. The electronic version of this database contains entries for all articles published in social science academic journals since 1981. Each entry includes the bibliography of the published article, and these bibliographies can be searched for particular citations. Using this database, it is possible to count citations. The raw data for our analysis are the number of times each Tversky paper was cited from 1981 to 1996. Table A, presented in Appendix A, reports the annual citation record for Tversky's papers published by 1996. His bibliography provided to us by the Stanford Psychology Department and reproduced in Appendix B, lists 124 papers, of which 119 were published or forthcoming as of December 1997. In our tables and in this section, we identify papers by the number on Tversky's bibliography.

While no single index provides an ideal measure of intellectual impact, we believe that CITES PER YEAR is the least distortive simple measure of scientific impact.[7] Using CITES PER YEAR, we have ranked the top twenty articles in Table A. Within this ranking, the two articles discussed in Section 1 stand out by any measure: the 1974 *Science* article on heuristics and biases (paper number 24), and the 1979 *Econometrica* article on framing and prospect theory (paper number 33).

These articles were respectively cited 1851 and 1703 times from 1981–1996, implying 115.69 and 106.44 cites per year. (The next closest contender in the top twenty list had 63.38 cites per year.) These two papers identify the two research domains where Tversky made his most important contributions: identifying the biases that arise from the heuristics of representativeness, availability and anchoring; and analyzing the distortive role of framing and the predictive power of prospect theory. We label these domains HB (heuristics and biases) and F/PT (framing and prospect theory). These two domains are not always distinct. For example, Tversky wrote a number of papers on anchoring bias, which, though included here in the HB domain, relate as well to the F/PT domain. To make our definition precise, we use the 1974 and 1981 survey articles in *Science* as the respective definining documents for the two domains. ("Judgment Under Uncertainty: Heuristics and Biases," and "The Framing of Decisions and the Psychology of Choice.") To demonstrate the centrality of the HB and F/PT domains, we analyzed the twenty papers listed in Table 1. Seven of the top twenty papers fall in the HB domain (paper numbers 24, 21, 20, 17, 16, 52, 42). Eight of the top twenty papers fall in the F/PT domain (paper numbers 33, 38, 56, 64, 91, 84, 41, 44). Most of the remaining papers in Table 1 examine the ways that consumers compare, and choose among, multiattribute goods.

We are particularly interested in Tversky's impact on economics. Using the data in the last column of Table A—CITES PER YEAR (EC)—we identified the Tversky papers that are most often cited in the top four economics journals: *The American Economic Review, Econometrica, The Journal of Political Economy*, and *The Quarterly Journal of Economics*.[8] To reduce noise, we only included articles that were cited at least five times in these journals during the 1981–96 period. This slimmed our sample to seven Tversky articles. We report their ranking in Table 2.

Table 1. Top 20 Papers Ranked by Cities Per Year in Social Science

Paper	Year	Coauthors	Paper Title	Cities per year	Total cites
24	74	Kahneman	Judgment under uncertainty: Heuristics and biases	115.69	1851
33	79	Kahneman	Prospect theory: An analysis of decision under risk	106.44	1703
38	81	Kahneman	The framing of decisions and the psychology of choice	63.38	1014
21	73	Kahneman	Availability: A heuristic for judging frequency and probability	53.69	859
29	77		Features of similarity	48.31	773
20	73	Kahneman	On the psychology of prediction	46.06	737
56	84	Kahneman	Choices, values, and frames	34.38	447
64	86	Kahneman	Rational choice and the framing of decisions	25.91	285
19	72		Elimination by aspects: A theory of choice	25.31	405
17	72	Kahneman	Subjective probability: A judgment of representativeness	25.25	404
70	88	Sattath, Slovic	Contingent weighting in judgment and choice	23.78	214
91	92	Kahneman	Advances in prospect theory: Cumulative representation of uncertainty	21.20	106
16	71	Kahneman	Belief in the law of small numbers	19.31	309
8	69		The intransitivity of preferences	18.63	298
52	83	Kahneman	Extensional vs. intuitive reasoning: The conjunction fallacy in probability judgment	17.93	251
84	91	Kahneman	Loss aversion in riskless choice: A reference dependent model	16.17	97
41	82	Kahneman	The psychology of preferences	13.87	208
42	82	Kahneman	The simulation heuristic	13.47	202
44	82	McNeil, Pauker, Sox	On the elicitation of preferences for alternative therapies	12.13	182
78	90	Slovic, Kahneman	The causes of preference reversal	12.00	84

To judge Tversky's impact on economics, Tables A and 2 need to be evaluated in comparison to a benchmark. We tabulated the citation records during 1992–1996 of the 17 individuals who won Nobel Prizes in economics over the past ten years. Among them, the median number of first or sole-author citations in our four top economics journals during that period was 18,[9] whereas Tversky had 14. But, Tversky is disadvantaged in three ways relative to this benchmark. 1) His name comes late in the alphabet. 2) He gets no credit by this measure for his most cited-paper in economics, Prospect Theory, a Kahneman-

Table 2. Top 7 Papers Ranked by Cities Per Year in Top 4 Economics Journals

Paper	Year	Coauthors	Paper Title	Cites per year	Total cites
33	79	Kahneman	Prospect theory: An analysis of decision under risk	3.25	52
24	74	Kahneman	Judgment under uncertainty: Heuristics and biases	0.63	10
64	86	Kahneman	Rational choice and the framing of decisions	0.55	6
8	69		The intransitivity of preferences	0.50	8
56	84	Kahneman	Choices, values, and frames	0.46	6
38	81	Kahneman	The framing of decisions and the psychology of choice	0.38	6
41	82	Kahneman	The psychology of preferences	0.33	5

Tversky effort. 3) He's a psychologist. (For his 1981–1996 articles, he had 16 times as many cites outside the economics literature as within.) Independent of name order, Tversky had 39 top-four-economics-journal cites from 1992–1996.

Although the citations in the top four economic journals represent less than 1% of the general social science citations, the economics rankings in Table 2 bear a striking correspondence to the general social science ranking reported in Table 1.[10] The top two entries continue to be the 1974 *Science* article and the 1979 *Econometrica* article, though the latter comes first on the economics list. The remaining five articles in Table 2 are all also listed in Table 1. Hence, economists appear to be citing, and presumably reading, the same articles that are influencing other social scientists. However, it is interesting to note that the Tversky articles cited by economists are overwhelmingly in the PT/F domain (paper numbers 33, 64, 56, 38, 41). Only one of the seven papers in Table 2 falls in the HB domain (paper number 24). This imbalance in citation arises, we believe, in significant part due to the relatively greater mathematical formalism of the PT/F domain. Economics research usually develops and tests formal mathematical models, so one would expect the PT/F domain to be more influential in published economics research. We believe, however, that informal discussion of the HB domain has also been of great importance, particularly with respect to the intellectual development of the field of behavioral economics.

The correspondence between the citation patterns in economics journals and those in non-economic social science journals can also be demonstrated by computing the correlation between Table A columns SUM and SUM (EC)—the correlation is .69—and the correlation between Table A columns CITES PER YEAR and CITES PER YEAR (EC)—the correlation is .67. These high correlations reinforce our conclusion that economists and other social scientists generally identify similar sources of value in Tversky's work.

Tversky worked across a broad range of fields, often in collaboration with experts from those fields. His coauthors included psychologists, economists, mathematicians, statisticians, lawyers, and physicians. Of Tversky's 124 papers, 108 were written in conjunction with others. Citation evidence helps us evaluate the important role of collaborative work in Tversky's research program.

Tversky's most active collaboration, by far, was that with Daniel Kahneman; it stretched over a period of almost 30 years. Kahneman, a cognitive psychologist like Tversky, shared his deep interest in economic decision making. The citation evidence reveals a remarkable synergy between Tversky and Kahneman. We'll evaluate this synergy in several different ways. First, of the top 20 papers reported in Table 1—papers with the greatest impact throughout the social sciences—14 represent Tversky and Kahneman collaborations, and one additional paper was coauthored with both Kahneman and Slovic. (Of the remaining five papers on the top twenty list, three have no coauthors, and the final two were respectively coauthored with Sattath and Slovic, and McNeil, Pauker, and Sox.) Of the top 7 papers reported in Table 2—papers with the greatest impact within economics—6 are joint with Kahneman (the remaining paper has no coauthors). Kahneman's numerous appearances on these lists of high impact papers is particularly remarkable because Tversky and Kahneman actually coauthored only 23 papers.

Another measure of the extraordinary dynamism of this partnership is the ratio of collaborative citations with Kahneman to total citations. Table A lists 13,333 citations to Tversky papers during the 1981–1996 period; of these citations, 9148, or 68.6 percent, had Kahneman as a coauthor. By contrast, only 20.7% (23 of 111) of the papers published as of 1996 were collaborations with Kahneman. Finally, the papers that resulted from Tversky and Kahneman collaborations had 31.12 cites per year, while all other Tversky papers had 4.63 cites per years, a remarkable seven-fold difference.

To evaluate the synergy between Tversky and Kahneman, it is also necessary to look at Kahneman's citation record. From 1981–1996, Tversky and Kahneman collaborations, including papers in which Tversky and Kahneman were joined by additional authors, were cited 9148 times. Among papers that Tversky did not coauthor, Kahneman received 1480 citations as first author over this period. To get the synergy ratio (Collaborations with Tversky Citations)/(Total Kahneman Citations), we would also need to tally citations to papers authored by Kahneman on which Tversky is not a coauthor and Kahneman is not a first author, which would require a tedious paper-by-paper, year-by-year search. Absent the tally, the pattern of synergy is still clear: a significant majority of Kahneman cites are to papers written with Tversky.

3. Tversky's contributions in historical context

3.1. History of psychology and economics

Interactions between psychology and economics have a long if not intense history. Classical economists including Smith, Marshall, Pigou, Fisher, and Keynes acknowledged and carefully analyzed psychological foundations of preferences and beliefs.[11] The psychological content in mainstream economics began to slide starting in the 1940's due to the influence of the major new players in the economics profession, such as Samuelson and Hicks, who demonstrated the natural complementarity between technical virtuousity and rationality assumptions. But by the late 1960's economists were still relatively unorthodox when it came to the rational actor assumption. Microtheorists continued to worry about experimental anomalies, particularly those identified by Allais (1953) and Ellsberg (1961). Theories of bounded rationality (Simon 1957) were gaining increasing prominence. Macroeconomists typically adopted models featuring rules of thumb and adaptive expectations.

In the 1970's, strict rationality assumptions swept the profession, threatening to eliminate the modest psychological content remaining in economic analysis. The rationality takeover was spurred by five major developments. First, macrotheorists, including John Muth (1961), Robert Lucas (1972, 1975, 1976), Edward Prescott (1971 and 1974 papers with Lucas), and Roy Radner (1972), developed a formal framework—the rational expectations equilibrium model—that enabled economists to incorporate the full rationality assumption into stochastic macroeconomic models. Second, standard macroeconomic empirical relationships, notably the Phillips Curve, broke down in ways that were predicted by the new rational expectations theories (Lucas 1973). Third, the new field of information

economics expanded rapidly—e.g., Akerlof (1970), Spence (1974), Rothschild and Stiglitz (1976), and Grossman and Stiglitz (1980)—enabling economists to model fully rational behavior in settings in which actors had imperfect information about the consequences of their choices. Fourth, microtheorists increasingly embraced the equilibrium concepts of game theory, emphasizing correct beliefs and Bayesian updating.[12] Fifth, a growing body of evidence suggested that assets were efficiently priced in the market; asset returns seemed to be predicted only by covariation with market risk (e.g., Fama and MacBeth (1973) and Jensen (1969)). Each of these fields—rational expectations, game theory, information economics, and asset pricing—grew rapidly.

Three of the five developments listed above were purely theoretical, and the two empirical developments—the collapse of the traditional Phillips Curve and the lack of excess returns in asset markets—were both based on aggregate data. No significant evidence about the behavior of individual actors accompanied this dramatic intellectual shift within the economic profession. Despite lack of micro-level empirical documentation, by the end of the 1970's most nonrational models had been squeezed out of the mainstream literature. This transition is not surprising: the old quasi-rational models of the 1960's—such as adaptive expectations—were not themselves grounded in solid empirical or experimental evidence, and the experimental anomalies such as the Allais paradox and the Ellsberg paradox, though robustly demonstrated in the laboratory, had not been shown to be of import in the real world. Moreover, they had not yet generated a theoretical framework which could challenge the new hegemony of rationality.[13]

Just as economics was moving rapidly toward a fully rational model of economic decision makers, the rationality critiques made by Tversky and his many coauthors (notably Kahneman) were developing momentum in the psychology literature. The pro-rationality environment in economics may have made it difficult for Tversky to find a sympathetic audience there. Major contributions would be required to take hold in its uncongenial, pro-rationality environment. A big bang, or two, would be needed. The publication of "Prospect theory" in *Econometrica* (1979, co-authored with Kahneman) and of the volume of essays, *Judgement Under Uncertainty: Heuristics and Biases* (1982, co-edited with Kahneman and Slovic), which was organized around the 1974 *Science* article of the same name, provided the necessary thunder claps. These two publications altered the intellectual history of economics; they brought the behavioral economics research program into the mainstream.

The principal actors in the behavioral economics movement were Tversky and Kahneman, who ventured from psychology to economics, and Richard Thaler, who played unofficial host for the economics profession, communicating, encouraging, and extending the Tversky and Kahneman research program. Eric Wanner of the Sloan Foundation and subsequently the Russell Sage Foundation played a critical role identifying and then funding this new field. As it moves into adolescence, the behavioral economics movement emphasizes micro-level data on decisionmaking, including experimental evidence, validation of modeling assumptions, interchanges between psychology and economics, and skepticism regarding perfect rationality.[14] As a guiding force in this movement, Tversky played a central role in giving credibility to the discussions among economists about the

limits of rationality assumptions and the value of looking to psychology for the foundations of behavior.

Why did Tversky's critiques stick while other non-rational models were being squeezed out of the economics profession? First, Tversky's critiques were carefully validated experimentally. Concepts were often tested with a series of experiments and under a range of experimental conditions (e.g., varying experimental contexts, and reward characteristics—real vs. hypothetical, money vs. goods, large vs. small). Second, his critiques were accompanied by models that were relatively parsimonious—and hence, widely applicable. He showed that decision makers err, and he explained how to systematically predict their errors. Prospect theory provides a particularly strong case. The model has now been parameterized and calibrated (Tversky and Kahneman, 1992), making it more competitive with standard mathematical models of economic behavior. Third, Tversky mastered the relevant economics literature and employed the language of the profession in his writing. Fourth, Tversky's research had important economic implications. Tversky did not make a series of arcane observations about human decision-making in special settings (e.g., the Allais Paradox); rather, he argued that decision-making anomalies arise in a predictable way whenever Bayesian inferences are required or where frames and reference points vary.

Finally, Tversky has presented himself and his results in a winning style. Unlike economists, who are often imperialistic when applying their lessons to new domains, Tversky stated only what could be clearly and robustly demonstrated. He was well equipped to engage the economics profession, since he understood our models, presented himself as a scientist rather than a preacher, and did not challenge the central normative judgments of the profession. Many economists, including the authors, felt him to be a quiet ally in the battle for prescriptive rationality. Although Tversky's formal research publications with economists were not extensive, he mixed with and served with them on committees and research projects. Mutual affection and respect reinforced each other.

3.2. Tversky as archaeologist of cognition

Tversky issued his strongest challenge to rational choice in his work illustrating preference intransitivities and reversals. What are we to make of such behavior? Tversky's (1969) interpretation, anticipates the philosophy of many economists who have taken behavioral economics seriously:

> "When faced with complex multidimensional alternatives, such as job offers, gambles, or candidates, it is extremely difficult to utilize properly all the available information. Instead, it is contended that people employ various approximation methods that enable them to process the relevant information in making a decision.… In using such methods in making decisions we implicitly assume that the world is not designed to take advantage of our approximation methods.… This approximation may be very good in general, despite the fact that it yields intransitive choices in some specifically constructed situations." (p. 46)

Amos Tversky devoted much of his professional career to investigating anomalous behavior, intransitivities being its most salient form. Behavioral zealots use such anomalies to confirm their world view: Nonrational behavior is the universal norm for human beings. Rabid rationalists, by contrast, find anomalies potentially devastating. Thus, the anomalies themselves must be shown to be anomalous phenomena, say due to artful crafting of questions, with unrealistic conditions or low stakes. Tversky took an intermediate view of his work: "The main interest in the present results lies not so much in the fact that transitivity can be violated but rather in what these violations reveal about the choice mechanism and the approximation method that govern preference between multidimensional alternatives (p. 46)."

Just as shards of pottery tell archeologists about civilizations, so unearthed anomalies inform decision scientists about human choice processes. Amos Tversky pioneered the archeology of cognition. All of his work helps to identify the structures underlying human choice processes, and thereby explain the mechanisms—for better and for worse—that shape our decisions.

4. Unresolved questions: The next decade of research

This section discusses a series of issues that will influence the degree to which behavioral economics—Tversky's intellectual legacy—flourishes over the next decade.

4.1. Skepticism about laboratory evidence

From the beginning of his career, Amos Tversky's work drew heavily on experimental evidence. Economists are skeptical of experimental research because it generally fails to replicate the incentives and learning opportunities that decision-makers encounter outside the lab. Tversky was keenly interested in addressing critiques of this class, as much to find superior modes of investigation as to support his results.

Do disparities between the laboratory and the real world stem from lack of understanding or expertise? Slovic and Tversky (1974) shed some light. They experimentally evaluated Savage's Sure Thing Principle, found that it is usually violated, and that the magnitude of the violation does not change after subjects are given a learning opportunity composed of arguments for and against Savage's Principle. Tversky and Kahneman (1971) demonstrate that the biasing effects of the representativeness heuristic are found in the intuitive judgements of professional psychologists, participants at a meeting of the Mathematical Psychology Group and the American Psychological Association who were asked to make the kinds of statistical inferences that they routinely make in their research. McNeil, Pauker, Sox, and Tversky (1982), Redelmeier and Tversky (1990), and Griffin and Tversky (1992) also document anomalous decisions of experts.

The relevance of laboratory experiments has also been questioned because subjects answer hypothetical questions, or at best receive small rewards. Many of Tversky's most important studies involved hypothetical rewards (e.g., Kahneman and Tversky 1979 and

Tversky and Kahneman 1992), but Tversky was always sensitive to the potential importance of incentives. For example, Tversky and Kahneman's (1992) paper on cumulative prospect theory, includes a section on incentives.

> In the present study we did not pay subjects on the basis of their choices because in our experience with choice between prospects of the type used in the present study, we did not find much difference between subjects who were paid a flat fee and subjects whose payoffs were contingent on their decisions. The same conclusion was obtained by Camerer (1989), who investigated the effects of incentives using several hundred subjects. He found that subjects who actually played the gamble gave essentially the same responses as subjects who did not play; he also found no differences in reliability and roughly the same decision time. Although some studies found differences between paid and unpaid subjects in choice between simple prospects, these differences were not large enough to change any significant qualitatitive conclusions. Indeed, all major violations of expected utility theory (e.g., the common consequence effect, the common ratio effect, source dependence, loss aversion, and preference reversals) were obtained both with and without monetary incentives.

One study (Kachelmeier and Shehata 1992) verified one of Kahneman and Tversky's prospect-theoretic findings using Master's students at Beijing University; in the high-payoff condition, the students' average earnings were three times their normal monthly income.

Nevertheless, some observors argue that anomalies are sensitive to reward magnitudes. For example, Smith and Walker (1993) conduct a meta-study in which they find that higher rewards often shift subject responses toward the predictions of rational models, and almost always reduce the variance of subject responses. This suggests "that when rational models fail it can be attributed to low opportunity cost of deviations from the rational prediction. (p. 245)"[15]

Demonstrating correspondence between laboratory and real-world decision problems remains an important hurdle for the behavioral economics research program. Kahneman and Tversky's observations about incentives in their original prospect theory paper (1979) remain apt nearly two decades later:

> "The reliance on hypothetical choices raises obvious questions regarding the validity of the method and the generalizability of the results. We are keenly aware of these problems. However, all other methods that have been used to test utility theory also suffer from severe drawbacks. Real choices can be investigated either in the field, by naturalistic or statistical observations of economic behavior, or in the laboratory. Field studies can only provide for rather crude tests of qualitative predictions, because probabilities and utilities cannot be adequately measured in such contexts. Laboratory experiments have been designed to obtain precise measures of utility and probability from actual choices, but these experimental studies typically involve contrived gambles for small stakes, and a large number of repetitions of very similar problems. These

features of laboratory gambling complicate the interpretation of the results and restrict their generality (Kahneman and Tversky 1979, p. 265)."

Economists will remain skeptical of any of Tversky's findings that are not replicated with expert subjects, highly motivated subjects, and subjects with ample opportunity for learning. Such skepticism should serve to advance science, not merely to preserve a professional domain. Since it is always possible to find a reason to critique an experiment for not being sufficiently realistic, it is important to seek the appropriate limits of such critiques. Rabin (1996) describes this problem in a recent review of Kahneman and Tversky's work:

"Human experiments seem too artificial in their attempts to replicate analogous real-world situations to confidently infer real-world behavior from these experiments. Indeed, it seems clear that the levels of stakes and experience inherent in some economic situations are impossible to replicate in the laboratory. But using these criticisms as a justification for maintaining 'standard' assumptions is highly problematic, and begs the question of whether standard economic assumptions are themselves supported by conclusive evidence."

Does real world validity require more or fewer learning opportunities than current experiments offer? Some decision-makers are not experts, are not highly motivated, do not devote careful thought, and/or do not have opportunities for the learning that comes with repeated trials. (Academic readers, for example, should reflect on the inadequacy of their personal decision processes—many colleagues report taking fewer than five minutes—when making asset allocation decisions in their TIAA and CREF accounts.)[16] On the other hand, real world decision-makers frequently have external sources of advice and guidance, something that is almost always missing in the lab.

4.2. Dynamic and endogenous frames

An important theme of Tversky's work is that arbitrary frames influence subject choices. Most of Tversky's research focused on three kinds of frames. First, salient starting points or scale values "anchor" subjects' responses. Recall the wheel of fortune experiment, discussed in Section 1, in which an arbitrary number anchors subjects guesses about the percentage of UN countries that are African (Tversky and Kahneman 1974). Second, arbitrary frames determine whether a choice is perceived to lie in the gain domain or the loss domain. Recall the experiment, again discussed in Section 1, in which some lottery earnings are segregated, moving the subsequent lottery from the gain domain to the loss domain (Kahneman and Tversky 1979). Third, some arbitrary frames make certain attributes of a good salient. For economists, the most dramatic examples of this third kind of framing involve preference reversals, for example when subjects select lottery A over

B—since lottery A has a higher probability of winning—but put a higher monetary value on lottery B—since lottery B has a higher reward magnitude (Tversky, Slovic, and Kahneman 1990).

In all of Tversky's experiments with frames, the frame is manipulated exogenously by the experimenter. But many real world decisions involve frames set strategically, say by an insurance salesmen, or by the decision maker himself. We know very little about how frames evolve when they are not under the control of an experimenter. How easy is it for marketers to manipulate the frames that people use? Are not consumers watchful for and relatively wary of such manipulations by those who have a financial interest in their decisions? When decision-makers pick their own frames, do they pick them in ways that minimize distortions to their decisions? Does consumer frame selection get better—less arbitrary—with experience? Do consumers pick frames to induce feelings of well-being? If so, might endogenous frame selection be even more distortionary than exogenous frame selection? How should we evaluate such distortions normatively, if the decision-making distortion actually raises subjective well-being? Are consumers even aware of these framing issues, and do consumers have reasonable forecasts of their future reference points? Do consumers understand how their current choices affect their own future reference points? Do they make choices that capitalize on these intertemporal relationships?[17]

4.3. Mathematical formalization and parsimony

While Tversky's mathematical models make his experimental results, and particularly prospect theory, far more marketable to economists, his models remain in some ways dissimilar from classic economic models. Economic models usually provide complete mathematical representations of behavior; Tversky's models by contrast are partially or wholly linguistically based. For example, consider Kahneman and Tversky's verbal coding rules from prospect theory, which determine whether outcomes are perceived as gains or losses relative to a reference point.

> The reference point usually corresponds to the current asset position, in which case gains and losses coincide with the actual amounts that are received or paid. However, the location of the reference point, and the consequent coding of outcomes as gains or losses, can be affected by the formulation of the offered prospects, and by expectations of the decision maker. (1979, p. 274)

Similarly, we do not have formal mathematical statements of representativeness, availability, and anchoring. The heuristics are modeled in Kahneman and Tversky's papers, but these partially verbal models do not readily translate into the mathematical language of economists. (Given Tversky's mathematical and modeling skills, if they could be readily formalized, they would have been.)

Some of these models are not even translated into economists' "magic curves," say for supply and demand, which capture many qualitative mathematical properties without adopting a particular parametric function. This gap between the verbal and the formal

raises two critical questions: First, how important is mathematical formalization? Second, how important are parsimony and generalizability, two central attributes of most mathematical models?

Economists value mathematical formalization because it provides a precise language for communicating ideas and because it generates qualitative and often quantitative predictions. Such predictions are useful for model testing and for practical forecasting needs, such as policy design. How should one proceed if one only has a verbal model? Should the model be ignored by economists? Should it be shoe-horned into mathematical language? Should economists postpone consideration until it is satisfactorily translated into a mathematical representation? Or should they see what economic lessons flow from its qualitative implications?

We are not confident about the answers to any of these questions, but we think that the answers are closely related to parsimony and generalizability. How important are these qualities, how necessary to models that explain behavior? One can always translate a qualitative, verbal model into a quantitative, mathematical form—but one is likely to create a non-parsimonious, seemingly ad hoc mathematical representation with limited applicability beyond the narrow domain for which it was designed. This suggests that the important question is not whether we insist on mathematization, but rather whether we insist on parsimony and generalizability. The rational choice model is extraordinarily parsimonious and generalizable. By contrast, psychology provides no overarching paradigm, and is more like a kludge. It is a patchwork of ideas and conceptions, which, when combined with excellent professional judgment, yields good forecasts about human behavior. Will economists find psychological insights useful? And if so, how should economists incorporate these insights?

Imagine that you have been asked to pick an analyst to forecast the decisions of an emergency room physician. We believe that if you picked a psychologist fluent in Tversky's work, you would do much better then if you picked a first-rate economist who would assume that the doctors are skilled Bayesians. However, the psychologist probably would not be able to write down an equation or equations that summarize his forecasting strategy; he would simply base his judgments on Tversky and Kahneman's experimental results and their linguistic models of the heuristics. Would these judgments be improved if we forced the psychologist to represent his conclusions with parsimonious mathematical equations? Probably not in the short run, but over the long run it might.

Parsimony has proved to be an extraordinarily good organizing principle in the natural sciences. New and improved models have almost invariably been more parsimonious than the models they have replaced.[18] Hence, a commitment to parsimony seems reasonable in fields like physics. The empirical record is not nearly as clear in the social sciences. While economics has followed a path of increasing parsimony in the twentieth century, within anthropology, psychology, sociology, and political science, parsimony has been less of a central organizing goal; detailed description has frequently prevailed. It remains unclear whether these trends will continue, and what the fate will be within economics of non-parsimonious models imported from other disciplines.

There may be a waxing and a waning in the struggle for simple and broad explanation, depending on the state of knowledge. For example, medicine searches for a general theory

of cancer, but welcomes along the way detailed observations about the behavior of particular tumor types. Economics, like physics, must live with anomalies, unless and until it finds ways to explain them periodically in unifying theories.

4.4. The promiscuous prediction problem

To be tested, theories need to make sharp predictions. The rational actor model has excelled in this regard, generating easily testable implications. Nonrational behavior, by contrast, often has the disadvantage of permitting deviations up or down. For example, the anchoring heuristic (Tversky and Kahneman, 1974) predicts that consumers will fail to adjust adequately from a given initial guess. But we do not yet have a theory of where that guess comes from. To a psychologist who hasn't read Tversky's work it might seem just as sensible to theorize that the initial guess is associated with a base rate, as it is to imagine that the initial guess is associated with a similarity judgment. We now know that base rates tend to be ignored, and hence, do not provide cognitive anchors. But we lack a general theory of starting points. Until we have such a theory, anchoring will have limited real predictive power. Nevertheless, it will still be possible to tell a post hoc story that "explains" a particular judgmental bias as an anchoring effect. In the real world, we have dozens of possible anchors, and hence dozens of possible anchoring effects from which to choose when "explaining" an anomalous judgment.

Moreover, the number of post hoc explanations explodes when we let other heuristics enter the picture. As we write, the U.S. stock market has enjoyed extraordinary returns over the past 15 years. Will investors anchor on these extraordinary rates of return and overpredict future returns, or will investors apply the representative heuristic and predict negative future returns, because a random process should go down as often as it goes up? If the behavioral model can make either predicition, how can it be tested? By facilitating opposite predictions, behavioral economics often allows itself too many degrees of freedom. However, the promiscuous prediction problem also plagues mainstream economics. Both behavioral models and standard economics models are often so flexible that almost any outcome can be explained by them.

4.5. Field data validation, domains of advantage and disadvantage

Economic skepticism toward laboratory evidence (discussed above) is reflected in economists' keen interest in verification with real-world applications. No other challenge is as important for Tversky's work as field data validation, and this important stage in the research program has only barely begun.[19] Economists will be particularly interested in validation exercises that use market data, the canonical place where quasi-rational decision-makers supposedly suffer relative to their rational colleagues.

In this subsection, we describe where field data studies are likely to be most supportive and most critical of the Tversky and Kahneman research program. We build on Zeckhauser (1986), who argues that one can identify which environments are most likely to

engender behavioral effects, and which will engender rationality. Just as we admire the shotputter or miler, a theory need not win a predictive decathlon to merit acceptance and use. Those who wish to push the behavioral agenda initiated by Tversky should look for the field's best events.[20] We identify ten factors that affect its potential contributions. Factors that are less well understood receive more attention.

Stakes. Low stakes will generate hurried or heuristic decision-making (Smith and Walker 1993). However, as Camerer and Hogarth's (1997) meta-study concludes, large stakes—even very large stakes—rarely eliminate behavioral biases, though increasing the stakes sometimes reduces the magnitude of these biases.

Repetition. When learning opportunities are scarce, consumers will be more likely to fall back on suboptimal heuristics. For example, decisions about marriage and retirement savings do not offer us many opportunities for experimentation and low-cost learning. We may end up with bad outcomes, with little recourse.

Advice. Decisions will be relatively poor when consumers can not seek out the advice of others, or can merely learn vicariously by observing others' actions and outcomes.

Poaching and arbitrage. In some economic settings, third parties can profit from the decision-making errors of others, through a process called poaching. Poaching activities—arbitrage is the term used in financial markets—bring prices into line.[21] But, often poaching is costly or effectively impossible. For example, nobody benefits when a worker fails to accumulate a sufficient nest egg for her retirement.

Certainty, Risk, Uncertainty, and Ignorance. Decision making under risk is tougher than under certainty, and uncertainty—where the probabilities are ill defined—is much tougher still. It is no surprise that most, though hardly all, of Tversky's most salient demonstrations of deviations from rationality involve risk and uncertainty. Some situations involve what we label ignorance, contexts where even the set of states is not defined, or impossible to identify exhaustively. For example, what would be the categories of possibility with a question such as: "How will my relationship with my betrothed evolve over the next ten years?" With ignorance, nonrational behavior would be most likely, though also most difficult to document. Finally, ignorance can apply both to the future states of the world, and to immediate actions: "It didn't occur to me that I could use the Internet to find your phone number."

Non-market goods. Even staunch believers in rationality in the market sphere might admit to doubts in other arenas. Market norms encourage cool calculation and competition. By contrast, for many non-market goods, hard-headed techniques such as cost-benefit analysis are viewed skeptically. Big mistakes with love—half of new marriages end in divorce—seem more plausible than equivalent errors with money. Moreover, social norms like reciprocity and fairness, which may play some role in some markets (Fehr, Kirchsteiger, and Riedl 1993), dominate decision-making in non-market environments such as

friendship. It is perfectly appropriate for a graduate student to ask a colleague to give him a hand moving his belongings to a new apartment. However, it would be outlandish for the same friend to ask for a gift of $100 so he could hire a professional mover to assist him. The graduate student has a claim on his friend's time but not on his stipend.

Prices vs. Quantities. It is often a challenge to find the tracings of nonrational behavior in the market, because prices provide the principal data from markets, and well-financed arbitrageurs—profiting from the errors of nonrational participants—bring prices back to appropriate levels. By contrast, even in the presence of perfect arbitrage and the rational prices it brings, quantities held may still reflect the underlying irrationality of select market participants. Consider the uninformed investors who churn their portfolios in a hapless effort to pick winners or time markets, or the overwhelming majority of investors who are insufficiently diversified internationally.

Problem dimensionality. Multidimensional goods are difficult to compare, particularly when some of the dimensions cannot be readily priced (e.g., quality, ease of use, warranty protection, health risks, etc …), or otherwise put in commensurable units. The dimensionality problem is reinforced if performance on some dimensions cannot even be quantified.

Price taking. When agents act as price takers, their strategic roles are relatively clear. By contrast, negotiation and bargaining open a Pandora's box of strategic and normative complications, creating an environment where behavioral considerations flourish. Bargaining parties often have self-serving notions of what constitutes a fair settlement, or a fair negotiating process (Babcock et al. 1995, 1996). Moreover, negotiations introduce a personal and emotional dimension into any exchange, sometimes narrowing the scope for mutually beneficial gains from trade. Finally, bargaining introduces the dangers of speculating on the other side's behavioral propensities—e.g., how tied someone is to an "untenable" position—or of ignoring them, which can also be costly.

Aggregation. Most work within behavioral economics involves a single decision maker, or perhaps two. Yet most important economic decisions take place in arenas, principally markets, where dozens, thousands, or millions of decisions flow together to produce outcomes. There is no behavioral economics equivalent of Gresham's Law. Rational behavior by some may curb quasi-rationality, as with poaching. But it can also be overwhelmed, as short sellers learn when overheated stocks stay hot. Moreover, market interactions can exaggerate or amplify quasi-rational actions or beliefs. Consider herding models, or more generally any class of models in which it is optimal for actors to cluster their actions rather than spread them say because they deduce information from each other's actions, or secure protection against outside evaluators. Newspaper beauty contests are an oft-cited metaphorical example. In such herding games, a small number of irrational actors may tip the equilibrium from one focal point to another. Degeorge, Patel and Zeckhauser (1997) show how a small number of irrational investors can drive rational investors to treat arbitrary reference points—e.g., zero earnings growth—as economically

important hurdles. The knowledge that some irrational investors consider zero to be a significant reference point leads managers to manipulate earnings to meet or exceed last year's results. Moreover, the rational investor, knowing zero has become a hurdle that managers care about, will also come to treat it as an informative benchmark. The nonrational participant is catered to by rational participants, and the whole system may shift as a consequence to a behavioral equilibrium. The system loses efficiency due to distortion. Differential punishment in such contexts is inflicted not on the nonrational participant, but rather on the stubborn rationalist.

4.6. Paternalism and normative analysis

Tversky's work challenges the assumption that consumers are perfectly rational, seemingly encouraging paternalists and challenging economists' beliefs in the normative merit of consumer sovereignty. However, behavioral economics does not pose a threat to the principle of free choice (though it may make us less sanguine about the results); Tversky's work leads one to be no less skeptical of the rationality of the bureaucrat (or legislature) assigned to safeguard the consumer's interest, than of the consumer himself. And there is no argument of which we are aware that governmental processes will serve as a corrective to the quasi-rational behavior of governmental actors.[22] Moreover, if one believes that government actors are more rational than consumers, that rationality differential should raise other red flags. If the government actors are rational and not benevolent, empowering them may only make things worse for the quasi-rational consumer.

Despite these caveats, Tversky's work at least admits to the possibility that well-implemented government interventions could have normative merit, a possibility ruled out by the mainstream rational choice model when there are no externalities. Hence, Tversky's work raises important questions about the role of government intervention without ever taking sides in the contentious policy debate.[23]

The lessons are slightly clearer, however, about the value of improving decisions. Many Tverskyphiles, the authors included, would welcome the development of a private market for advice and debiasing services. For example, inexpensive computer-based assistance may be a sensible private market solution, particularly when the consumer suffers from an unmotivated, cognitive bias that she should want to eliminate once she becomes aware of its existence.[24] Consumer demand for debiasing technologies and services could be construed as a test of the Kahneman and Tversky research program.

Quite apart from the question of who should decide, there remains the question of on what basis to do so. Tversky himself provides little guidance. He repeatedly reminds his readers that his work is descriptive in nature, and only discusses normative issues in his conclusions. In obvious cases of invariance violation (i.e., preferences which do not exhibit invariance to framing) Tversky was willing to call the violation irrational, but, usually his analysis was relatively agnostic, as the following passages about particular violations illustrate:

> The normative status of the effects of mental accounting is questionable.… Regret, frustration, and self-satisfaction can also be affected by framing. If such secondary consequences are considered legitimate, then the observed preferences do not violate the criterion of invariance and cannot readily be ruled out as inconsistent or erroneous. On the other hand, secondary consequences may change upon reflection. The satisfaction of saving $5 on a $15 item can be marred if the consumer discovers that she would not have exerted the same effort to save $10 on a $200 purchase.[25] We do not wish to recommend that any two decision problems that have the same primary consequences should be resolved in the same way. We propose, however, that systematic examination of alternative framings offers a useful reflective device that can help decision makers assess the values that should be attached to the primary and secondary consequences of their choices (Kahneman and Tversky 1984, p. 348)

> Is loss aversion irrational? This question raises a number of difficult normative issues. Questioning the values that decision makers assign to outcomes requires a criterion for the evaluation of preferences. The actual experience of consequences provides such a criterion … We conclude that there is no general answer to the question about the normative status of loss aversion or of other reference effects, but there is a principled way of examining the normative status of these effects in particular cases (Tversky and Kahneman 1991, p. 1057).

In sum, Tversky's written work leaves major unanswered questions about the normative content of his findings. Are framing effects biases to be avoided, or do they sometimes reflect valid preferences?[26] Even if framing effects are distortionary, perhaps those distortions can be exploited to achieve desired outcomes. What if we find, as many suspect, that effective framing can raise subjective well-being? For example, if the subjective experience of losses really is convex while the subjective experience of gains is concave, shouldn't we dribble good news and deliver bad news all at once, say when dealing with a colleague or family member? If so, one should temporarily hide good news—at least some of the good news—from people whose interests we are trying to further. Might we even be able to use these hide-and-reveal techniques on ourselves?

Such normative ambiguities arise in areas across the Kahneman and Tversky research program. For example, consider the air passenger who reads about an airline disaster once a month, but only flies a few times each year. The availability heuristic suggests that this passenger will be irrationally afraid of airline accidents, leading her to avoid air travel whenever possible, or making the trips she does take irrationally unpleasant. Now consider a proposal to spend tens of billions of dollars in safety equipment which will effectively eliminate air accidents. These safety measures will save a couple hundred lives per year, and they will also dramatically improve the experiences of tens of millions of air passengers who will no longer fly in terror. How much weight should we give this fear effect when undertaking the cost-benefit analysis for this proposed new safety program? Is it normatively sensible to count the elimination of an exaggerated, irrational fear as a policy benefit?

5. Potential accomplishments for behavioral economics

Drawing considerably on the work of Amos Tversky, behavioral economics has made a mark. The National Bureau of Economic Research organizes conferences in behavioral finance and behavioral macroeconomics, graduate programs hold behavioral seminars, and a number of mainstream departments have made appointments in the field. Even skeptics must now make serious efforts to reject it. (The usual method is to construct rational explanations of widely observed but ostensibly anomalous activity.)

Behavioral economics is not just another field—say like environmental economics of a decade ago—that will blossom once it has trained enough new students, whose work in turn will give it empirical respectability, whatever the results. Behavioral economics has the potential to wither. Alternatively, it may ultimately challenge some of the most fundamental assumptions in economics, in much the same way that the theory of rational expectations challenged macroeconomics in the 1970's, and eventually become part of established wisdom. To do so, the field must win empirical battles.

It is too early to tell the fate of behavioral economics. If its empirical import cannot be demonstrated outside the laboratory, its central findings will be banished to the domain of anomalies, playing a role equivalent to optical illusions: intriguing, but irrelevant for our day-to-day activities. What achievements would be required to enable behavioral economics to play a central role in the profession? Five potential milestones may mark its progress:

1. *Solve one important empirical puzzle*. Computers, guided by their human masters, won respectability in mathematics by solving the Four Color Map Theorem. Similarly, we expect that behavioral economics will gain mainstream respectability when it convincingly explains an important phenomenon—e.g., the equity premium puzzle, exchange rate volatility, or nominal wage rigidities.[27]
2. *Routinely address everyday phenomena*. Behavioral economics has triumphed mostly in the laboratory, usually demonstrating departures from rational behavior in controlled choice problems. To prosper, it needs successes in the field, addressing the central subject matter of economics. Subjects such as demand elasticities, labor supply, and consumption functions need to be illuminated. Such successes will require that behavioral explanations compete against rational explanations of the same phenomena, using parsimony and explanatory power as criteria.
3. *Progress to a deductive science*. Economics is enamoured of deductions from theory that lead to nonobvious hypotheses that can be confirmed, or refuted, by empirical test. Such deductive exercises help demonstrate the validity of the theory by showing that the theory does not merely rationalize the data, but rather it predicts empirical relationships which themselves can be tested. The richer the deductive implications, the more powerfully a theory can be tested.[28]
4. *Have behavioral economics gain equal Bayesian footing with rational behavior*. Today, even practitioners of behavioral economics do not assert its superiority in domains where rational explanations offer equally compelling evidence. Thus, they search primarily for areas where rationality appears to have broken down. The behavioral field

will have arrived when mainstream journals regularly present econometric studies that offer neighboring columns comparing behavioral and rational explanations, and the competing explanations are judged solely on explanatory power.

5. *Provide a general theory of behavioral choice*. Tversky and his collaborators struggled for both generality and rigor. Over the years, they revisited and refined their central findings. Major principles emerged. But a general theory of behavioral choice, which would compete with the utility and profit maximization hypotheses of economics, has yet to be developed. Behavioral economists will likely wait a very long time until such a theory is formulated successfully. We conjecture that if such theories are ever developed, they will be a collaborative product of social and neuro-scientists.

Had this paper been written a quarter century ago, there would have been perhaps nine potential accomplishments to enhance the status of behavioral economics; Tversky and his compatriots have already tucked four under our belts. First, they documented systematic bias in the laboratory and to a lesser extent in the field. Second, they produced the first formal behavioral economics models. Third, they published the first widely cited behavioral economics paper in a leading economics journal. Fourth, they enticed a new generation of economists to enter this new subfield and nurtured their progress.

6. Conclusion

Amos Tversky leaves a rich legacy to economics. He and a small group of like minded psychologists and economists have mounted a challenge to a central assumption of the economics discipline—that individuals make choices on a rational basis.

If in response, economics were merely to abandon completely the rationality assumption, the Tversky legacy would prove to be more stultifying than rich. The economics field would flounder. We take a much more optimistic view. We predict that mainstream economics will ultimately meet the behavioral challenge by developing a new quasi-rational synthesis.[29] Such a synthesis, for example, will identify when and where the framing of choices dramatically affects what choices are made, and it will study how framing is conducted and countered in the real world. Better predictions, say of consumer choices, will be the result, but the standard framework of economic maximization will be for the most part preserved.

The present status of the theory of market performance and failure offers a happy metaphor for what behavioral economics can achieve. All mainstream economists recognize both the virtues of markets, and their limitations. The debate within the profession is one of degree: where the market should be extended, and where constrained. And even staunch free marketeers may welcome the theory of externalities undergirding the control of pollution, lest dirty skies and sludgy water cast a general pall on the market system.

In like spirit, behavioral economics has the potential to become a helpmate of rational choice theory, despite the apparent behavioral threat as a powerful alternative hypothesis in many domains. Say we were to discover that Americans, by their own standards, save far too little. One possible response would be to toss over economics, or abandon con-

sumer sovereignty in this domain. A more helpful response would be to identify and confront the particular behavioral phenomena that lead to this problem. To evoke decisions that sit well with individuals, we might learn to reframe long-term savings decisions. Or, since natural proclivities are not always optimal, we might undertake decision training, just as individuals now take lessons on how to ski.

But how should we train people for decision making? We admit to a strong preference for rational choice as a prescriptive theory, and believe that it would be the choice of most citizens for most contexts if they could deliberate on the arguments from all sides. If so, behavioral proclivities are sometimes natural tendencies to be overcome, not unlike leaning uphill when skiing. However, there are some contexts where careful framing can strongly increase utility. For those contexts, we believe the model of rational choice must bend to accommodate behavioral insights, giving them prescriptive weight. Rational choice theory will make even better predictions and better justified prescriptions if it can embrace and reflect the important lessons that emerge from behavioral economics.

In his remarkable career, Amos Tversky—often working with Daniel Kahneman—moved behavioral economics from a dismissed idea into an alternative hypothesis in addressing central choice problems within economics. In the future, we believe, behavioral explanations will gain equal Bayesian footing with rational explanations in addressing a range of core economic subjects, such as savings behavior and unemployment. In fair competition, behavioral explanations will ultimately be trounced in some areas, but triumph in others. The marketplace of ideas—an arena where Amos Tversky felt right at home—will pick the winners. And within that market, we predict contrary to rational expectations, the odds on Tversky ideas will shorten.

7. Appendix A

(Note for Table A.) The papers listed above are ordered chronologically by publication year; within each year the order is alphabetical by the name of the first author. The table includes all citations through 1996 for the 111 articles published by 1996. The numbers in column 1 correspond to the numbers on Tversky's vita (see Appendix B). Column 2 lists the first author for each paper, and column 3 gives the publication year for the first paper in a year. The next 16 columns show the number of citations of the paper in each year from 1981 to 1996. The 'SUM' column reports the total number of citations during the 1981-96 period. The 'CITES PER YEAR' column divides the entries in the 'SUM' column by the total number of years in which a citation could have occurred. (For example, for article #51, published in 1983, the denominator in the 'CITES PER YEAR' column is 14. For articles published before 1981, the denominator in the 'CITES PER YEAR' column is 16.) The 'SUM (EC)' column reports the total number of times the corresponding Tversky article was cited in the top four economics journals: *The American Economic Review, Econometrica, The Journal of Political Economy*, and *The Quarterly Journal of Economics*. The 'CITES PER YEAR (EC)' column is analogous to the 'CITES PER YEAR' column.

Table A. Tversky Citations

	First Author		'81	'82	'83	'84	'85	'86	'87	'88	'89	'90	'91	'92	'93	'94	'95	'96	SUM	CITES PER YR	SUM (EC)	CITES PER YR (EC)
1	Tversky	1964	0	2	2	1	1	0	0	0	1	0	0	1	3	5	2	1	19	1.19	0	0.00
2	Rapoport	1966	1	1	0	0	1	0	0	0	0	0	0	0	0	0	0	0	3	0.19	0	0.00
3	Tversky		0	0	0	0	0	0	0	0	0	0	2	0	2	1	0	0	5	0.31	1	0.06
4	Tversky	1967	2	2	1	1	0	2	0	2	2	0	1	2	0	0	1	0	16	1.00	3	0.19
5	Tversky		8	8	3	5	3	4	2	6	2	5	1	5	2	4	1	1	60	3.75	2	0.13
6	Tversky		6	3	1	2	0	0	0	1	0	1	1	0	0	0	0	0	15	0.94	0	0.00
7	Beals	1968	7	8	4	4	4	4	3	6	4	2	1	4	1	2	1	0	55	3.44	0	0.00
8	Tversky	1969	17	22	9	17	17	22	12	26	16	21	20	26	22	19	16	16	298	18.63	8	0.50
9	Tversky		1	3	0	1	0	3	1	2	0	1	2	1	0	1	0	0	16	1.00	0	0.00
10	Tversky		0	1	1	3	3	2	6	0	1	3	3	5	2	2	2	1	35	2.19	0	0.00
11	Pollatsek	1970	2	2	2	3	1	3	2	1	1	0	0	1	3	3	0	0	24	1.50	0	0.00
12	Rapoport		1	2	1	1	2	0	1	1	0	0	0	0	1	1	0	1	12	0.75	0	0.00
13	Tversky		3	4	1	1	2	3	1	1	1	2	1	5	1	2	0	1	29	1.81	0	0.00
14	Krantz	1971	6	10	12	10	4	3	4	6	2	7	3	5	2	2	0	5	81	5.06	0	0.00
15	Kubovy		0	0	0	0	0	1	0	3	0	0	0	0	0	1	0	1	6	0.38	0	0.00
16	Tversky		22	19	16	27	20	22	18	21	26	16	22	20	9	18	14	19	309	19.31	1	0.06
17	Kahneman	1972	15	30	27	39	18	30	23	21	33	24	27	25	22	19	30	21	404	25.25	0	0.00
18	Tversky		6	6	6	7	4	5	6	7	2	3	4	7	2	2	3	1	71	4.44	0	0.00
19	Tversky		24	26	23	37	19	30	31	29	24	19	15	26	32	21	30	19	405	25.31	2	0.13
20	Kahneman	1973	46	38	60	57	37	50	52	41	53	45	49	43	38	45	46	37	737	46.06	1	0.06
21	Tversky		32	38	53	61	46	53	60	57	57	45	61	49	55	72	60	60	859	53.69	1	0.06
22	Slovic	1974	8	8	6	7	5	8	10	6	7	6	7	11	6	7	6	3	111	6.94	4	0.25
23	Tversky		5	0	2	2	1	2	1	1	2	0	1	0	0	0	0	0	17	1.06	0	0.00
24	Tversky		92	111	104	104	100	115	116	114	139	96	112	133	122	120	144	129	1851	115.69	10	0.63
25	Krantz	1975	3	4	8	2	4	6	8	4	0	3	5	3	3	1	3	1	58	3.63	0	0.00
26	Tversky		1	1	2	2	3	1	6	3	2	1	3	2	2	1	0	2	32	2.00	2	0.13
27	Sattath	1976	0	0	1	0	1	0	0	0	1	0	0	0	0	0	0	0	3	0.19	0	0.00
28	Sattath	1977	8	8	9	13	3	10	9	8	8	2	12	9	5	4	10	8	126	7.88	0	0.00
29	Tversky		35	47	32	42	33	61	47	59	48	52	43	60	51	43	66	54	773	48.31	1	0.06
30	Tversky		0	2	3	2	1	1	0	0	2	1	1	2	2	0	1	0	18	1.13	0	0.00

Table A. Continued

	First Author		'81	'82	'83	'84	'85	'86	'87	'88	'89	'90	'91	'92	'93	'94	'95	'96	SUM	CITES PER YR	SUM (EC)	CITES PER YR (EC)
31	Tversky	1978	5	6	6	12	3	8	7	7	9	9	6	9	8	5	7	6	113	7.06	0	0.00
32	Kahneman	1979	4	7	5	5	0	7	6	2	8	3	2	7	5	5	6	6	78	4.88	0	0.00
33	Kahneman		31	46	57	58	67	85	95	120	105	120	117	164	129	153	196	160	1703	106.44	52	3.25
34	Lindley		6	7	8	2	8	9	1	4	4	6	4	2	3	1	2	4	71	4.44	0	0.00
35	Tversky		5	8	4	8	4	8	12	8	6	7	5	8	6	7	4	4	104	6.50	0	0.00
36	Schwarz	1980	0	2	2	0	0	1	0	0	1	0	1	1	0	0	0	0	8	0.50	0	0.00
37	Tversky		7	11	12	19	9	17	12	11	17	13	11	13	6	3	9	8	178	11.13	0	0.00
38	Tversky	1981	15	36	59	43	49	65	75	63	58	75	66	87	78	71	85	89	1014	63.88	6	0.38
39	Gati	1982		2	2	3	2	2	2	3	2	2	2	8	0	3	5	0	38	2.53	0	0.00
40	Kahneman			5	4	12	5	7	7	5	6	5	15	7	5	3	8	11	105	7.00	0	0.00
41	Kahneman			4	7	11	5	15	18	16	10	15	14	21	18	18	23	13	208	13.87	5	0.33
42	Kahneman			8	46	2	2	7	10	7	9	12	10	14	16	22	20	17	202	13.47	0	0.00
43	Kahneman			2	3	3	4	2	4	2	5	5	10	6	6	11	8	5	76	5.07	0	0.00
44	McNeil			0	9	15	5	10	8	5	10	14	14	11	15	13	23	30	182	12.13	1	0.07
45	Pruzansky			0	7	7	1	3	4	4	8	6	2	6	0	1	4	2	55	3.67	0	0.00
46	Tversky			1	5	6	7	12	8	8	4	4	7	12	5	8	8	1	96	6.40	0	0.00
47	Tversky			0	0	1	2	1	4	5	2	6	3	3	1	2	7	4	41	2.73	0	0.00
48	Tversky			0	0	3	0	9	6	3	1	4	5	4	6	5	2	5	53	3.53	0	0.00
49	Johnson	1983			0	4	13	11	16	15	11	9	12	6	15	12	10	18	152	10.86	0	0.00
50	Newman				2	0	0	1	0	0	0	0	0	0	0	1	0	0	4	0.29	0	0.00
51	Tversky				0	0	0	0	2	0	1	2	0	2	2	1	3	2	15	1.07	0	0.00
52	Tversky				2	5	13	24	16	19	21	19	26	18	24	18	27	19	251	17.93	0	0.00
53	Tversky				1	0	0	1	0	0	1	1	0	1	0	1	0	0	6	0.43	0	0.00
54	Gati	1984				0	0	3	6	6	3	3	2	6	1	6	2	4	42	3.23	0	0.00
55	Johnson					1	1	2	1	3	3	2	4	3	7	2	2	4	35	2.69	0	0.00
56	Kahneman					2	9	24	28	33	31	26	45	43	53	38	58	57	447	34.38	6	0.46
57	Quattrone					0	1	0	0	2	5	3	2	3	2	1	5	1	25	1.92	0	0.00
58	Gilovich	1985					0	3	3	5	5	2	5	10	1	6	7	7	54	4.50	1	0.08
59	Shafer						0	3	4	3	5	1	6	5	3	8	1	3	42	3.82	0	0.00
60	Corter	1986						0	2	1	5	2	2	5	3	5	6	1	32	2.91	0	0.00

Table A. Continued

	First Author		'81	'82	'83	'84	'85	'86	'87	'88	'89	'90	'91	'92	'93	'94	'95	'96	SUM	CITES PER YR	SUM (EC)	CITES PER YR (EC)
61	Quattrone							0	0	0	0	1	1	0	0	0	0	1	3	0.27	0	0.00
62	Tversky							0	0	0	0	0	0	0	0	0	1	0	1	0.09	0	0.00
63	Tversky							0	3	3	7	2	5	9	4	2	5	5	45	4.09	0	0.00
64	Tversky							7	4	12	14	15	34	49	39	40	37	34	285	25.91	6	0.55
65	Gati	1987							0	1	1	2	1	1	0	0	1	0	7	0.70	0	0.00
66	Sattath								0	0	1	2	1	2	0	0	3	0	9	0.90	0	0.00
67	Bell	1988								0	2	8	8	16	10	13	9	15	81	9.00	0	0.00
68	McNeil									0	0	0	1	3	2	2	0	2	10	1.11	0	0.00
69	Quattrone									1	3	10	4	10	7	11	11	13	70	7.78	0	0.00
70	Tversky									0	13	13	16	43	36	30	30	33	214	23.78	2	0.22
71	Dawes	1989								0	0	0	0	0	0	0	0	0	0	0.00	0	0.00
72	Tversky										0	0	0	1	1	0	0	3	5	0.63	0	0.00
73	Tversky										0	0	0	0	0	0	0	0	0	0.00	0	0.00
74	Redelmeier	1990										0	4	2	5	3	2	6	22	3.14	0	0.00
75	Ritov											2	1	1	1	1	2	1	9	1.29	0	0.00
76	Ritov											0	1	1	0	0	0	1	3	0.43	0	0.00
77	Slovic											2	3	6	16	11	12	9	59	8.43	1	0.14
78	Tversky											4	9	17	8	23	10	12	84	12.00	4	0.57
79	Tversky											1	0	4	0	2	3	2	12	1.71	2	0.29
80	Gonzalez											0	0	0	0	1	0	0	1	0.14	0	0.00
81	Gerrig	1991											0	0	0	1	0	0	1	0.17	0	0.00
82	Heath												0	1	0	0	1	0	2	0.33	0	0.00
83	Tversky												1	0	1	3	0	0	5	0.83	0	0.00
84	Tversky												1	6	11	21	32	26	97	16.17	3	0.50
85	Simonson	1992												1	7	6	8	11	33	6.60	0	0.00
86	Griffin													2	4	10	7	18	41	8.20	0	0.00
87	Redelmeier													1	2	0	1	2	6	1.20	0	0.00
88	Tversky													2	4	5	6	3	20	4.00	0	0.00
89	Shafir													1	2	5	4	3	15	3.00	0	0.00

Table A. Continued

	First Author		'81	'82	'83	'84	'85	'86	'87	'88	'89	'90	'91	'92	'93	'94	'95	'96	SUM	CITES PER YR	SUM (EC)	CITES PER YR (EC)
90	Tversky													0	0	0	0	0	0	0.00	0	0.00
91	Tversky													4	6	24	37	35	106	21.20	4	0.80
92	Tversky													2	3	0	5	5	15	3.00	0	0.00
93	Liberman	1993													0	2	2	6	10	2.50	0	0.00
94	Gidron														0	1	3	5	9	2.25	0	0.00
95	Wakker														0	2	9	5	16	4.00	1	0.25
96	Tversky														0	2	8	7	17	4.25	0	0.00
97	Shafir														1	2	5	6	14	3.50	0	0.00
98	Tversky	1994														0	0	0	0	0.00	0	0.00
99	Tversky															0	2	14	16	5.33	0	0.00
100	Kahneman	1995															2	2	4	2.00	1	0.50
101	Tversky																1	4	5	2.50	0	0.00
102	Redelmeier																0	0	0	0.00	0	0.00
103	Fox																0	1	1	0.50	0	0.00
104	Shafir																0	0	0	0.00	0	0.00
105	Tversky																0	2	2	1.00	0	0.00
106	Tversky																0	0	0	0.00	0	0.00
107	Brenner	1996																0	0	0.00	0	0.00
108	Koehler																	2	2	2.00	0	0.00
109	Kahneman																	2	2	2.00	0	0.00
110	Redelmeier																	0	0	0.00	0	0.00
111	Tversky																	0	0	0.00	0	0.00

8. Appendix B

Tversky's bibliography, reproduced below, was provided by the Stanford University Psychology Department.

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Notes

1. The original papers, along with a substantial body of supportive and related research was gathered together in a volume of the same name co-edited by Kahneman, Paul Slovic, and Tversky (1982). A new edition with significant new material is due in 1998.
2. Slovic and Lichtenstein (1971, p. 712) initiated the development of this conceptual framework.
3. In a 1992 *Journal of Risk and Uncertainty* paper, Kahneman and Tversky generalize prospect theory. Their new formulation, cumulative prospect theory, adopts the rank-dependent or cumulative functional first proposed by Quiggin (1982). Unlike the original *Econometrica* version of prospect theory, cumulative prospect theory satisfies stochastic dominance and can be applied to lotteries with any number, or a continuum, of outcomes. All of the qualitative features that we discuss apply to both prospect theory and cumulative prospect theory.
4. This particular experiment is reported in Tversky and Kahneman (1992).
5. Other editing steps include combining states with identical outcomes, segregating the riskless component of the lottery, and cancelling components that are shared by all of the offered lotteries. See Kahneman and Tversky (1979) for more details.
6. This function has the same shape that Lichtenstein et al. (1978) discover relates subjects' perceived mortality risks to true risks. Viscusi (1989) links these results to Prospect Theory, as a form of homogenization of unknown quantities or regression toward the mean. He assumes that individuals treat stated hard probabilities as imperfect information.

7. Using total cites (i.e., SUM in the notation of our appendix) would bias the analysis against recently published articles. CITES PER YEAR tilts against articles published and actively cited in the 60s and 70s that were little cited in recent years, but we note that this measure may overweight recently published articles with short-lived impacts.
8. The *Journal of Risk and Uncertainty* has been the principal journal for the analysis of Tversky concepts in economics. Tversky articles published between 1981 and 1996 were cited 55 times in the *Journal of Risk and Uncertainty*, 18 times in the *American Economic Review*, 6 times in the *Quarterly Journal of Economics*, 4 times in *Econometrica*, and once in the *Journal of Political Economy*. While these numbers are relevant for cross-journal comparison, they are low in absolute terms as they do NOT include citations to Tversky's pathbreaking publications of the 1970's. This omission reflects a technical constraint that we faced in our database analysis.
9. Robert Lucas and Gary Becker were extreme outliers with 149 and 136 first or sole-author citations respectively. The next highest author had 36.
10. For the purposes of comparison it would be ideal to construct a general social science citation dataset that excludes the citations that appeared in all economics journals, not just the top four. That exercise would be extremely time-consuming and would do little to change our results. By any estimate, citations to Tversky in mainstream economics journals represent no more than 10% of the citations that appear in all social science journals. For example, using an electronic database compiled for this project by the Institute for Scientific Information, we calculate that only 6% of social science citations to Tversky occurred in economics journals. This database covers citations to Tversky articles that were published from 1981–1996.
11. Loewenstein (1992) and Thaler (1997) review some of this history.
12. Thomas Schelling's (1960) psychologically-sensitive and literary game theory was upstaged by more mathematically formal models. The latter models were motivated by parsimony and narrow economic conceptions of self-interest, trading reality for rigor and applicability for abstraction as the sources of inspiration.
13. During the 1980's, rational models that relaxed the independence axiom (Machina 1982, Schmeidler 1989) were proposed as explanations of the Allais and Ellsberg paradoxes.
14. For an excellent introduction to the behavioral economics research program, see Thaler's Anomalies' column, originally published in the *Journal of Economic Perspectives* and subsequently collected in an edited volume (1992). See also, Thaler (1991) and Rabin (1997).
15. Camerer and Hogarth (1997), in a subsequent meta-study, although reporting that strengthening incentives raises effort, finds that rational choice often requires more than increased effort.
16. See Samuelson and Zeckhauser (1988, pp. 31–33) for some evidence on the inadequacy of attention devoted to this important decision.
17. See Thaler (1985), Loewenstein and Adler (1995) and Loewenstein and Frederick (1997) for a discussion of some of these issues.
18. There are exceptions, such as the theory of punctuated equilibrium in evolution.
19. There are disappointingly few instances, positive or negative, of efforts to test Tversky's theories with field data. Consider the following examples. Benartzi and Thaler (1995) theorize that loss aversion engenders large psychic costs for equity holders, who frequently experience period-to-period paper losses in the nominal value of their portfolios due to the high-frequency volatility of equities. Payment for these costs explains the Equity Premium Puzzle. (See also related laboratory work by Gneezy and Potters 1997, and Thaler, Tversky, Kahneman, and Schwartz 1997.) Bowman, Minehart, and Rabin (1996) model consumption behavior for a rational prospect-theoretic consumer, and generate anomalous predictions that match the available consumption data. Samuelson and Zeckhauser (1988) examine a variety of individual choices in the laboratory and real world, and find that consumers are biased toward the status quo. Tversky and Kahneman (1991) utilize this field evidence when they develop their theory of loss aversion in riskless choice. Hardie, Johnson, and Fader (1993) test the multiattribute generalization of prospect theory (Tversky and Kahneman 1991), using supermarket scanner data. Rizzo and Zeckhauser (1997) find evidence of loss aversion in the business practices of young doctors. See also, Thaler (1985), Kahneman, Knetch, and Thaler (1986), DeBondt and Thaler (1985, 1987, 1990), Russell and Thaler (1988), Odean (1997), Barberis et al. (1997), and Camerer et al. (1997).

20. Behavioral economics has also managed to score some victories in areas, such as finance, where rationality seems to have a natural advantage. See DeBondt and Thaler (1985, 1987, 1990), Odean (1997) and Barbaris et. al (1997).
21. See Shleifer and Vishny (1996) for a discussion of the limits of arbitrage.
22. Government decisions designed to protect individuals against health risks are particularly troubling. Risk assessments, using methods alleged to be "conservative," overestimate different risks by varying orders of magnitude. Dollars spent by government are counted more heavily than dollars spent by others. The result is that some statistical lives are secured for billions of dollars per life, while others are let go to save mere hundreds of thousands. See Zeckhauser and Viscusi (1990) and Breyer (1993).
23. Centralized decision making may also have an economy-of-scale advantage, since it can carefully make a decision once, say on food risk levels, which can then be applied for many individuals.
24. Motivated biases, like wishful thinking, make us feel better—at least temporarily—while unmotivated biases, like anchoring, do not.
25. Pratt, Wise and Zeckhauser (1979)—cited by Tversky and Kahneman in the paper from which this quote was taken—find surprisingly great variability in the prices of standardized consumer goods, with little diminution in the variance/mean ratio for higher-priced items.)
26. Kahneman's current research program (Kahneman, Wakker, and Sarin 1997), which seeks to measure experienced utility, continues to pursue these questions.
27. Benartzi and Thaler (1995) attribute the equity premium puzzle to loss aversion and volatile equity prices (see footnote 19).
28. Working in the neoclassical tradition, Laibson (1997) examines the implications of hyperbolic discounting, which involves normal discounting between pairs of future periods but strongly overweights today relative to tomorrow. His model of savings behavior yields specific, nonobvious, and testable implications.
29. The term quasi-rational was coined by Thaler (1986). "Behavior can be (and is often shown in the laboratory to be) purposeful, regular, and yet systematically different from the axioms of economic theory. (p. 191)"

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