DIVISION OF THE HUMANITIES AND SOCIAL SCIENCES

CALIFORNIA INSTITUTE OF TECHNOLOGY

PASADENA, CALIFORNIA 91125

BOUNDED RATIONALITY IN INDIVIDUAL DECISION MAKING

Colin Camerer

SOCIAL SCIENCE WORKING PAPER 1029

March 1998

Forthcoming, Experimental Economics.

Bounded Rationality in Individual Decision Making*

Colin Camerer

Abstract

My goals in this paper are: (i) To give a pithy, opinionated summary of what has been learned about bounded rationality in individual decision making from experiments in economics and psychology (drawing on my 1995 Handbook of Experimental Economics chapter); and (ii) mention some promising new directions for research which would be included if that chapter were written today.

*This paper is based on a lecture at the Bonn Conference on Theories of Bounded Rationality, May 1997. Helpful comments were received from Charles Holt, and Angela Hung assisted with research. It was written in the warm incubator of the Center for Advanced Study for Behavioral Sciences, with support from National Science Foundation grant SBR 9601236. Direct correspondence to camerer@hss.caltech.edu

Bounded Rationality in Individual Decision Making

Colin Camerer March 1998

I. Utility theories

A. Economic life beyond expected utility

Expected utility was given an official birth in von Neumann and Morgenstern s seminal book on game theory (1944). The show that several simple, appealing axioms, characterizing preferences over risky gambles, imply that the utility of a gamble should be the probability-weighted average of the utilities of its possible outcomes.

From the very start (1952) the Allais paradoxes were enough to cast some doubt on expected utility as a completely general theory of how people value risky choices.[1] However, few serious efforts to develop formal, simple alternatives were undertaken for about 25 years. During this time, decision theorists were mostly busy working out important technical details of expected utility, like alternative axiom systems and how to measure risk-aversion, and applying the theory to areas like risk-sharing and asset pricing.

In the late 1970s, various scholars began to propose ways to generalize expected utility to explain data. Important work includes Chew and MacCrimmon s (1979) weighted utility theory, Quiggin s (1982) rank-dependent theory, Machina (1982), and of course the prospect theory of Kahneman and Tversky (1979).[2] See Starmer (in press) for a very recent review.

These new theories piqued the curiosity of psychologists and economists alike, and more theory followed (notably, implicit expected utility, disappointment theory, skew-symmetric bilinear, lottery-dependent, and SP/A (security potential/aspiration) theory). Many papers reported some data which were consistent with a new theory (and not with expected utility); others merely featured an obligatory discussion of how their theory could explain the Allais paradox.

In the 1980s, comprehensive experiments designed to test several theories at once were conducted. The efficiency of these designs is impressive and could serve as a model for researchers in other areas of how to test several theories which can all explain some basic phenomenon but can be distinguished by careful designs (e.g., learning theories in games, see Camerer and Ho, 1997). The first such paper is Chew and Waller (1986), who used an ingenious design suggested in Chew and MacCrimmon (1979). Their basic idea, extending Allais s approach, was to find sets of pairwise choices such that different theories predicted certain choice patterns would or would not occur. For example, if you are an expected utility maximizer then if you prefer getting Y for sure to a p chance of winning X (where X and Y can be any objects, with X preferred to Y), you should also prefer a q chance of getting Y to a pq chance of getting X, for any q in (0,1]. In fact, in this common ratio problem (due to Allais) people often shift preference toward the gamble with a pq chance of X, as q falls.

The Chew-MacCrimmon design enables one to use just four pairwise choices to test simultaneously for common ratio effects, similar common consequence effects , and violations of the axiom of betweeness (any probability mixture with a p chance of X and a 1-p chance of Y should lie between X and Y in preference). My 1989 and 1992 papers adopted this pattern paradigm and extended it, as did Loomes, Starmer, and Sugden, Gigliotti and Sopher, and several others. The general results showed that expected utility violations were systematic and replicable, but violations of some of the new theories could be generated as well.

Because there were many data sets but few clear conclusions, David Harless and I (1994) showed one statistical way that data from many different experiments with choice patterns could be added up to draw robust conclusions. Our technique exploits the fact that in experiments with k pairwise choices, and 2^k patterns (excluding indifference), different theories allow different subsets of those patterns. By allowing random error in choices, one can use the observed patterns of choices to estimate what fraction of subjects truly prefer each pattern, and their overall error rate. The technique gives a likelihood score to each theory. Likelihoods can be added across experiments, and adjusted for parsimony by subtracting a penalty for the number of patterns a theory allows from that theory s likelihood. We applied our technique to 23 data sets consisting of more than 2,000 choices. The end result is a menu of theories one could prefer. Any theory which is not on the menu is less accurate than an equally-parsimonious theory. Which theories are on the menus turns out to depend on whether gamble pairs have the same set of possible outcomes or different sets of outcomes. The same-outcomes menu is: Expected value (most parsimonious); expected utility; prospect theory (in its original form); and mixed fanning [3] (least parsimonious). The different-outcomes menu is: expected value; prospect theory and mixed fanning.

These menu results are powerful because they summarize dozens of comprehensive studies. The results are so statistically overwhelming that it would take a huge amount of new evidence, all with surprising new findings that are similar, to reverse the menu ranking. Furthermore, if a theory is not on the menu then it is either less parsimonious than an equally accurate theory, or less accurate than a more parsimonious theory, so there is no sound statistical reason for using it if one s goal is to describe how people choose.

Notice that expected utility is not even on the menu when gamble pairs have different sets of outcomes. That means that while expected utility is parsimonious (in the sense that few choice patterns are allowed), there is no price (in the sense of a penalty to log likelihood) which justifies using this theory instead of a more (prospect theory) or less (expected value) parsimonious one. If one leans toward expected utility on the grounds of parsimony, a logical statistical consequence is that you should choose expected value instead.

Many competing theories can be divided into two classes: Those which obey betweenness, and those which weight probabilities p nonlinearly in some way. The nonlinear weighting theories take a sum of outcome utilities weighted by some function w(p) rather than p. The betweenness theories imply a kind of local linearity in probability. Many clever theories use this axiom but in our menu analysis, betweeness-based theories are statistically dominated-- they are less parsimonious and predict worse than competing theories.

These pattern-based studies aggregate data across many subjects and ignore individual differences. Some researchers (e.g., Hey and Orme, 1994), have concentrated on fitting theories to individuals. To do so requires a theory of error in choices which has provoked some interesting work. These studies usually conclude that a large minority of subjects are best-fit by expected utility (adjusting for degrees of freedom) and substantial numbers are fit by rank-dependent or other theories.

There is some apparent conflict in findings from the two methods-- the aggregated pattern paradigm tends to reject expected utility in favor of theories with nonlinear weighting of probabilities like prospect theory, while estimates of individuals show that the most common individual-level theory is expected utility. One possibility is that since most of the individual-level estimation has not used gambles with low probabilities (say, below .10), they do not sample the region of the gamble space in which expected utility performs worst. A more interesting reconciliation comes from thinking about aggregation. If half the people in a population obey expected utility and the other half obey some theory with nonlinear probability weighting, the aggregate results observed in pattern-based studies will show some degree of nonlinearity and reject expected utility. For economic applications which require a single kind of representative agent it is best to assume an agent who obeys, say, prospect theory. But models which allow heterogeneity could still have a large number (perhaps a majority) of expected utility maximizers, along with some people who obey alternative theories.

We should never return completely to expected utility, even if many subjects are fit adequately by it, because new theories promise to explain those subjects <u>and</u> the ones who violate expected utility (and because the best representative-agent theory will not be expected utility). Indeed, research on alternatives has moved profitably toward trying to pin down details of alternative theory and apply them to economic problems.

There is much progress estimating specific functional forms for weighting of probabilities. Two promising forms are the one-parameter forms proposed by Kahneman and Tversky (1992), $w(p)=p^c/(p^c+(1-p)^c)^{1/c}$, and Prelec s (in press) axiomatically-derived form

$w(p)=1/\exp(b*(\log(1/p))^c)$, where b and c are constants (and exp(d) is the constant e raised to the power d). When b=c=1 both functions reduce to linear probability weighting, w(p)=p, and when b=1 in Prelec s form the function has a crossover point (where w(p)=p) at 1/e, which has a nice scientific ring and fits fairly well. There are other two-parameter forms, like the Lattimore, Baker, Witte (1992) form $w(p)=bp^c/(bp^c+(1-p)^c)$, but these parsimonious one-parameter forms may prove particularly useful for empirical work and theory.

Another new direction is similarity-based choice (e.g., Rubinstein, 1988). Early explanations of the common ratio effect focused on the fact that probabilities are similar in one pair of choices and different in another, which seems to shift attention or weight from payoff to probability. For example, in the common ratio problem many people choose (.20,$4000) over (.25,$3000) because the probabilities are similar, but they choose $3000 over (.80,$4000) because the probabilities are much different (and the difference between winning with .8 and 1.0 probabilities picks up a certainty effect). This violates expected utility because the ratios of winning probabilities in each pair of choice are the same, and only ratios should matter. The intuition that similarity judgment is driving these paradoxes is captured in nonlinear probability weighting, to some extent, by having a portion of the weighting curve in which probabilities that are close together have weights which are disproportionately close (that is, w(.20)/w(.25) is closer to one than w(.80)/w(1)).

A more direct approach is to model similarity judgment as a primitive which influences choice in some way. This has been done by Leland (1991) and Buschena and Zilberman (1995). The similarity-based approaches capture an important kind of psychological intuition which was put aside while people sought other kinds of theories, but it is worth reexamining because it promises to connect the generalizations of expected utility more closely to cognitive, attentional processes. So far, I have not mentioned a central principle of prospect theory for which there is much evidence: People value gains and losses from reference points, rather than final wealth positions.[4] (This idea goes back at least to Markowitz, in the 1950s, Duesenberry on concerns about relative income, and the psychology of adaptation in psychophysics.) Introducing a dependence on reference points allows the possibility that gain and loss utility functions have different shapes, perhaps reflecting a single principle of diminishing marginal sensitivity as one moves away from the reference point. Indeed, there is much evidence for convex disutility of losses, which implies taking risks over possible losses (though this evidence is less robust than concave utility for gains). It also allows for asymmetry between losses and gains (loss-aversion). Many studies suggest that in modest ranges, losses are about twice as aversive as equal-sized gains are pleasurable.

I conclude this section with a frank opinion about what economists should do now. For decades, economists who use expected utility in various applications, or simply prefer it, have resisted switching to an alternative. By now, every scientific argument against switching has been refuted so it is time to switch.

One argument against switching is that not enough evidence has accumulated about exactly which theory to switch to. The studies above suggest cumulative prospect theory with rank-dependent weights is a good alternative supported by the preponderance of evidence. I should add that while various other theories have proved analytically intriguing and useful for some purposes (e.g., Machina s local utility analysis, and betweenness-based theories), the full range of experimental evidence never seriously favored any of these other alternative theories over cumulative prospect theory. (That is, while these theories might have been popular for a while, the popularity was not caused by empirical accuracy.)

A second argument against switching is that we know how to use expected utility to do theory, and aren t sure exactly how to use the others. This sounds like laziness. It took decades of concerted effort to figure out how to use expected utility-- refining its axiomatic underpinnings, finding the right measure of risk (the Arrow-Pratt measure)-- and cumulative prospect theory will require such effort as well. A lot of progress has been made in a short time, particularly on weighting functions and the degree of loss-aversion. And in any case, the need for new tools is surely an challenge to be taken up by creative theorists rather than an excuse for using outmoded tools.

A third argument is that expected utility is a useful approximation; counterexamples do not undermine it because approximations are allowed (and even required!) to have counterexamples. This apologist claim misses the point that the counterexamples are meant to be raw material to construct new theory, not merely to disprove old theory. If the new theory can do everything the old theory can, and then some, then the new theory is equally useful and accommodates counterexamples; why stick with the old theory?

A strong case can be made for the idea that cumulative prospect theory has now been established as more useful, because it can do everything expected utility can do and then some. We should either move full steam to cumulative prospect theory, or at least treat it and expected utility as equally interesting competing theories when doing applied economics.

For example, the concept of risk-aversion embodied in expected utility purports to explain why there is risk-sharing between individuals and larger entities (like firms or families. But virtually any phenomenon of this sort that expected utility can explain can also be explained by cumulative prospect theory with loss-aversion (if choices have both possible losses and gains), because loss-averse people will behave a lot like risk-averse people. To the extent that expected utility explains sharecropping contracts, insurance purchase, and returns on risky assets (all of which may yield losses or gains), prospect theory can explain these regularities as well.

Furthermore, there are several well-established anomalies which can be explained by Cumulative prospect theory but not by expected utility. Simultaneous gambling and insurance by the same individuals at all wealth levels cannot be explained by expected utility but can be easily explained by Cumulative prospect theory (assuming one s current wealth level is the reference point). Indeed, a participant in the Bonn conference suggested, quite sensibly, that economists would take prospect theory seriously if it could explain or predict field phenomena which are anomalies for expected utility. In fact, Table 1 shows a list of nine patterns in field data which cannot be easily explained by expected utility, but which can be naturally explained by assuming either loss-aversion, reflection effects (convex disutility for losses) or overweighting of low probabilities. Most also require assuming a kind of decision isolation or narrow bracketing (segregation of decisions from a stream or portfolio of decisions they might naturally be included in; e.g. Read and Loewenstein, 1995), since otherwise an aversive loss can be absorbed by gains from other decisions in the portfolio (see Camerer, in press, or the cited papers for more details).

The nine phenomena cover a wide range of applied economics topics: Savings and consumption decision (consumption does not adjust downward when people receive bad news about future income shocks); the unusually-high return premium of stocks over bonds (equity premium); the tendency to hold losing stocks longer than winners before selling them; downward-sloping labor supply by cab drivers who set a daily income target and quit when they reach it; asymmetric elasticities for increases and decreases of the prices of consumer goods; the purchase of actually unfair insurance against telephone wire repair (toward which people should be approximately risk-neutral); the tendency of racetrack bettors to favor longshots disproportionately (especially in the last race of the day); scale-economies in state lotteries, reflecting overweighting of low probabilities; and the tendency for legal rulings to appreciate endowment effects by favoring incumbent owners over otherwise-identical newcomers (grandfather clauses, two-tier wage agreements allowing new employees doing identical work to be paid less, and rulings which award custody of a disputed good to one of two possible owners based on who has held it longer). Most of these patterns are well-established in field data and have a common explanation in decision isolation and some parsimonious combination of cumulative prospect theory ingredients.[5] In addition, at least two of the phenomena-- disposition effects, and downward-sloping labor supply of cab drivers-- were predicted before they were observed.

Given these observations, it is high time to stop ignoring Cumulative prospect theory in applied economics and begin investigating it usefulness (or at least, encouraging graduate students to do so, since as Max Planck said, science progresses funeral by funeral).

B. Subjective expected utility

In standard Ramsey-DeFinetti-Savage subjective expected utility, people choose among acts which yield consequences in uncertain states. The probabilities of states are assumed to be unobservable but are subjective (or personal , Savage s term) and are revealed by choices. If I prefer an act which has a good consequence X if state A occurs to an act which has the same consequence X if state B occurs, then in subjective expected utility my choice reveals that I think A is more probable than B. The difference between subjective expected utility and plain old expected utility is that subjective probabilities are revealed by choices among acts in the former, while they are assumed to be objectively given in the latter.

While the idea that choices among acts reveals subjective beliefs is useful for many purposes, it implies that people are not allowed to shy away from betting on events about which they have little information, unless their reluctance to bet is manifested in a low subjective probability. (Intuitively, you can t dislike betting on Italy in the World Cup because you just don t know much about soccer unless what you really mean is that your subjective probability of Italy s chance of winning is low.)

The Ellsberg paradox (conjectured earlier by Knight and Keynes) shows why this restriction can be too strong. In the Ellsberg two-color problem , people can choose to bet that either a red or black ball will be drawn from an urn with known composition (50 balls of each color) or an ambiguous urn with 100 balls in an unknown composition of colors. Many people prefer to bet on a red draw from the known-urn than a red draw from the ambiguous urn, and on a black draw from the known-urn instead of a black draw from the ambiguous urn. This is a paradox because in Subjective expected utility, preferring both known-urn bets means the subjective probabilities of red and black from the known-urn are both higher than the corresponding probabilities of red and black in the ambiguous urn. Since the probability of drawing a red or black is one in both cases, this creates a paradox (if probabilities are additive; more on this below): the known-urn probabilities P(red) and P(black) can t add to one <u>and</u> both be larger than corresponding ambiguous-urn probabilities which also add to one. (Also, the pattern can t be explained by risk-aversion because the dollar size of the prize is held fixed so concavity of utility for money doesn t vary across the two urns.)

The Ellsberg paradox demonstrates that subjective probabilities revealed by bet choices are a single beast forced to serve two masters-- they are betting weights which express one s desire to bet on events, but if they are also probabilities then must also express judgments of likelihood (and add up to one). In principle, I don t see why likelihood judgments should necessarily equal betting weights. A person could believe the Singapore stock market is equally likely to rise and fall tomorrow, but have a low weight for betting on either a rise or a fall because she is simply reluctant to bet on that class of events (compared to betting the same amounts of money on a better-understood situation like a coin flip).

The conceptual difference between likelihood and betting weight has been mentioned many times. (Savage referred to it but said he did not know how to capture it formally.) The important question is whether a reasonably parsimonious framework can generalize subjective expected utility to account for these paradoxes and introduce a way to disentangle likelihood and betting weight.

In my view, nonadditive probability is a reasonable way to go-- that is, allow P(A)+P(B) to be different from P(A or B)-P(A and B). As Schmeidler (1989) emphasized, the requirement of additivity is what catches the probabilities in the pincers of paradox in the Ellsberg problem. Additivity forces the complementary red and black probabilities to add to one for both urns separately, denying the red-state probabilities and the black-state probabilities the right to be different across urns

If additivity is relaxed it is easy to wriggle out of the paradox in a way which gets the psychology right. For example, the subjective probabilities of red and black in the ambiguous urn could be P(red)=P(black)=.4, while P(red or black)=1 (and of course P(red and black)=0 since they are mutually exclusive). These probabilities should then be interpreted as betting weights rather than expressions of subjective likelihood. Then where is the missing .2 probability? The missing probability is an expression of the extent of one s aversion to betting at all, or a reserved belief which could be allocated to red or black if more information were available. Seen this way, the fact that probabilities are not additive is in fact a modelling advantage, since it gives a way to measure the extent of one s aversion to uncertainty. More uncertainty-averse people will have more reserved belief. While nonadditive probability seems unruly compared to additive probability, a lot of progress has been made in some fields (e.g., game theory) in figuring out how to restrict the nature of nonadditivity and proving interesting results from it (e.g., Ghirardato, 1997).

Obviously, working with nonadditive probability requires some ingenuity when updating is involved, but as with non-EU toolmaking, I regard this as precisely the kind of challenge talented mathematical economists should live for, rather than a reason to cling helplessly to additivity. Modelling probability nonadditively creates a language for understanding some paradoxes in how people act when information is missing. Frisch and Baron (1988) have suggested that ambiguity is present when there is known missing information-- an agent knows there is relevant missing information, which makes her reluctant to act. (Perhaps knowing there is missing information creates an anticipation of greater regret if the choice turns out badly.) The composition of the ambiguous Ellsberg urn, or trends in the Singapore stock market, are examples of information which is known to be missing and hence reduces betting weight. For example, people often seem to demand information in situations where the decisions they would make would not change regardless of what the information turned out to be. (Medical overtesting may be an example.)

From a standard decision theory point of view, demanding information simply to scratch the itch of not-knowing is irrational because the value of information is solely derived from the possibility of changing your decision favorably (in expected value terms). But from a nonadditive probability point of view, it makes sense to demand useless information if it relieves aversive ambiguity, because that relief raising betting weights and raises subjective expected utility.

The presence of known missing information characterizes many situations in life; assuming people are averse to taking risks in those situations may therefore help explain lots of phenomena. Among them are: The home country investment bias (people in all countries invest too much in their own countries, compared to the diversification benefits of investing in internationally-diversified mutual funds); excessive brand loyalty (when the costs of experimenting with new brands is very low); coordination failures in games, where the actions of other players are ambiguous; and violations of the Groucho Marx theorem, in which common new information creates mutually-profitable trade. Camerer and Weber (1992) give details and other examples.

C. Probability judgment

Much research in cognitive psychology suggests that the way in which people form judgments of probability departs systematically from the laws of statistics and from Bayesian updating. (This should not be surprising, because there is no reason to think that evolution of brain processes like memory, language, perception, categorization, and reasoning would have adapted us to use a rule that Bayes only discovered a couple of hundred years ago.) Some research points toward systematic departures, or biases , which spring from a small number of heuristics , like anchoring, availability, and representativeness.

The heuristics-and-biases literature has been useful in forcing us to look beyond the Bayesian paradigm, but it has not so far produced the kind of unified, formal alternative to Bayesian updating that cumulative prospect theory is for expected utility, or nonadditive probability could be for subjective expected utility. Nonetheless, I think this is a ripe area for psychologically-informed theorists to produce a grand new theory and make a big splash.

As a descriptive theory, Bayesian updating is weakly grounded in the sense that there is little direct evidence for Bayesian updating which is not also consistent with much simpler theories. Most of the evidence in favor of Bayesian updating boils down to the fact that if new information favors hypothesis A over B, then the judged probability of A, relative to B, rises when the information is incorporated. This kind of monotonicity is consistent with Bayesian updating but also with a very wide class of non-Bayesian rules (such as anchoring on a prior and adjusting probabilities up or down in light of the information).

Furthermore, Bayesian updating is actually quite restrictive in two ways. First, a central feature of Bayesian probability is exchangeability -- the order in which evidence arrives should not matter. But usually order <u>does</u> affect judgments (there are well-known primacy and recency effects in memory, which affect judgments of probability), implying that people are not Bayesian. Second, Bayesian updating assumes a separation between prior probabilities P(A) and judgments of evidence likelihood P(evidence|A). But many experiments on motivated cognition suggest that prior beliefs bias likelihood judgments, in the direction of the prior. For example, in belief perseverance experiments, subjects with different prior beliefs interpret the same information differently (typically, people who believe something is true are more inclined to interpret information as consistent with their belief).

Thus, the case against Bayesian updating is that the Bayesian model has not won clear victories over simpler rules, and at least two basic properties of the Bayesian model are clearly wrong. As with the debates about expected utility, however, the challenge is to find a replacement for Bayesian updating which codifies the psychological heuristics in a way that is formal and analytically useful, but not too complicated (see Rabin and Schrag, 1997, for a start). I am optimistic that clever decision theorists can think of a way to do it, if serious attention were turned in that direction (as it has been in developing post-expected utility and post-subjective expected utility frameworks). Perhaps moving away from the concept of state spaces, in which information is represented by state-space partitions, toward something nonpartitional like modal logic, is the right way to go (e.g., Rubinstein, 1997).

I conclude this Handbook-recap section with a brief editorial. Expected utility, subjective expected utility, and Bayesian updating are wonderful normative theories, in the sense of providing good advice which is often surprising and counterintuitive. As a result, we should not expect people to use them in everyday decision making. If they described what we do naturally, they would not help us do better.

Furthermore, psychologists and behavioral economists have developed formal alternatives to these building block principles which are promising replacements. Expected utility, subjective expected utility, and exponential discounting of future utilities could be replaced, in the textbooks our children will learn economics from, by cumulative prospect theory, nonadditive probability, and hyperbolic discounting (more on the latter below).

Given the strong a priori arguments against descriptive accuracy of these theories, and the availability of interesting formal alternatives, it is amazing how much experimental work in economics has been concerned with defensively trying to account for artifactual explanations for apparent violations of the theories, or find conditions under which violations go away. Defensive research has made few dents in the basic findings and produced few surprises. At this point, other kinds of experiments are clearly more useful. Constructive experiments take alternative theories seriously and explore their implications or measure parameter values (e.g., Myagkov and Plott, 1998). Competitive experiments carefully explore which of several alternative theories is better than an old one (like the horse race experiments on expected utility mentioned above). Market-minded experiments ask whether individual-level phenomena are attenuated, or perhaps made worse, by various kinds of institutional aggregation like making decision in groups or trading in markets (e.g., Camerer, 1987; Ganguly, Kagel and Moser, 1998). Experimenters interested in studying bounded rationality of individuals should do more experiments in these latter three categories and do fewer defensive experiments.

II. New directions

This section is an addendum to my Handbook chapter, discussing recent research directions not covered there in much detail.

A. Choice over time

Most choices require people to weigh current costs and benefits against future ones. The standard model of such intertemporal choices assumes people have a discount rate r and apply a discount factor to time t utilities which is an exponentially declining function of t, $(1/1+r)^t$. As Loewenstein (1992) pointed out, this model compresses a long history of thought about the factors which affect intertemporal tradeoffs into a formula which is surely too simple. Many of these excluded factors have been shown to affect revealed discount rates. For example, people can delay gratification longer (lowering their revealed discount rates) if the gratifying object is not in front of them, or if they simply close their eyes or think about something else. People often exhibit negative discount rates, preferring to delay pleasurable outcomes (a hot date) and speed up bad ones (dentistry). Loewenstein (1987) attributes negative discounting to the pleasure derived from savoring the anticipation of something good, and the dread of worrying about something bad. And visceral factors like emotions, hunger, and fatigue change discount rates dramatically, and temporarily (Loewenstein, 1996).

The most striking, regular departure from exponential discounting is found in a large body of experimental research, mostly conducted with nonhuman animals but frequently replicated with humans, showing that the discount function is close to hyperbolic, $(1/1+ct)^{-b/c}$, rather than exponential (see Ainslie, 1975). (As c approaches zero this function approaches an exponential so it strictly generalizes the exponential approach.) The hyperbolic function is more steeply sloped for near-term tradeoffs than for long-term tradeoffs; that is, hyperbolic discounters act as if they are much more impatient delaying rewards from now to the near future than they expect to be for future delays of equal length. The evidence for hyperbolic discounting is overwhelming: There are no tests in which the exponential structure beats the hyperbolic form in a direct competition (when there is enough power to distinguish the two).

A one-parameter form which is approximately hyperbolic, and often easier to use analytically, is a model in which the discount factor is $b/(1+r)^t$ (see Phelps and Pollak, 1968). If $b<1$ this function discounts immediate delays more dramatically than exponential, because current utility gets a weight of one while utility one period from now gets a weight $b/(1+r)$. But this form discounts two different delays from the current period, t1 and t2, using exponential discounting because the discount factors are $b/(1+r)^{t1}$ and $b/(1+r)^{t2}$ so the <u>relative</u> discount factor is exactly the same as with exponential discounting (since the immediacy premium factor b divides out). Of course, if $b=1$ then the two-parameter form reduces to standard exponential discounting.

Besides its descriptive superiority, and reasonable parsimony (adding just one parameter), hyperbolic discounting provides a way to characterize problems of self-control: Since hyperbolic discounters are very impatient now, but act as if they expect to be patient in the future, they will indulge in current temptations which are wonderful now but costly later-- eating unhealthy foods, watching TV rather than exercise, putting off work-- because they expect to resist those temptations in the future. Obviously, hyperbolic discounters exhibit dynamic inconsistency because they will plan future behaviors they systematically do not carry out. While this is, of course, normatively undesirable, it is descriptively ubiquitous. (Many people you know behave this way-- probably including you!)

I think economists have resisted using hyperbolic discounting for three reasons: (i) Ignorance about the overwhelming empirical superiority and parsimony of hyperbolic discounting; (ii) confusion about the normative vs. descriptive appeal of dynamic consistency; and (iii) uncertainty about how to move away from the exponential model and still do analytical economics.

Point (ii) reflects a common methodological prejudice in positive economics (mentioned above): Many of the bedrock rationality assumptions economists cling to as modelling principles complete preferences, expected utility, Bayesian updating, exponential discounting, rational expectations-- are wonderful normative principles; people who understand them and manage to apply them will live better lives and make fewer big mistakes. But precisely because these principles are normatively useful, we should not expect them to be universally obeyed in everyday choices or even to be conveyed to consumers by social influences and by markets for advice. For example, the fact that people would like to avoid temptations, and plan or hope to do so in the future, is hardly a reason to think they do or that social influences, education, and market forces will necessarily solve their self-control problems.

Hyperbolic discounting addresses the how to do economics? concern (iii) by offering a two-parameter functional form which can account for individuals who are dynamically consistent and for those who aren t. Recent research demonstrating that the quasi-hyperbolic mode can be used to do economic theory includes Laibson (1997) and O Donoghue and Rabin (in press).

B. The adaptationist (or evolutionary psychology) program

Evolutionary psychology is a new (or rekindled) approach which has some implications for decision research. Evolutionary psychologists ask: Why might decision rules have evolved or adapted as they did? This approach has, so far, been mostly an exercise in post facto rationalization of observed patterns. For example, the special properties of face recognition (compared to other kinds of object recognition) can be explained as an adaptation in hunter-gatherer economies which allowed primitive people to share with (recognized) friends and avoid enemies. Some studies with the Wason 4-card logic problem suggest that people are better at solving logic problems when they are cloaked in a context of cheating-detection, which is often taken as indirect evidence that people have some specialized cheating-detection module which adapted to solve hunter-gatherer exchange problems. In the Wason problem, there are four cards which have a letter on one side and a number on the other. Subjects see cards which read A, K, 4, and 7. Subjects are asked which cards they would turn over to discover any violations of the rule If there is a vowel on one side of a card, there must be an odd number on the other side . The common pattern is to turn over A and 7. However, turning over 7 does not matter, because the rule is not falsified even if there is no vowel on the other side. And subjects should turn over 4, because the rule is false if there is a vowel on the other side, but most subjects do not realize this. However, the problem can be recast as a test of the rule If a person drinks alcohol in a bar, they must be over 21", and the objects to be inspected are a person drinking ginger ale, a person drinking vodka, a 19-year old, and a 23 year-old. (This is logically equivalent to the 4-card problem). Then subjects immediately realize that they should check the age of the vodka drinker, and what the 19-year old is drinking.

Differences in male and female pair-bonding, violence toward children, and sexual behavior also have obvious potential explanations as adapted outcomes (For example, the prediction is that men will invest less in their children than women do, and pair-bond with the mothers of their children only reluctantly, because it is obviously easier to verify who a child s mother is than to verify who the father. In addition, stepfathers will harm their stepchildren at much higher rates than their natural fathers do, which seems to be true).

While evolutionary psychology is a fruitful way to rationalize phenomena we observe, predicting new phenomena is much harder. Prediction requires one to understand the environment in which adaptation took place, to understand (to some extent) the cognitive mechanisms which resulted (including perhaps the time scale on which selection and genetic transmission took place), then predict how the adapted mechanism will perform in a modern environment. This chain of reasoning is very hard to do by working forward, and incredibly easy to do in reverse. The reverse postdictive strategy often results in just-so stories which explain a little too glibly why a behavior adapted, often ignoring constraints or negative side effects of the adaptation. Furthermore, in the end evolutionary psychology will explain only a small portion of variance in economic behavior if brain structure adaptations are swamped by cultural adaptations, socialization, individual differences, etc.

Having sounded those pessimistic notes, I think the adaptationist program is well worth exploring. The adaptationists simply prefer to focus on the half-full part of the glass of cognitive ability, asking how intelligent simple rules can be, rather than the half-empty part, looking at shortfalls from full rationality as a way of discovering simple rules. Both perspectives are useful.
A related development is mathematical exploration of the evolutionary foundation of preferences. The idea in this work is to assume that decision rules evolve to solve some specific objective (e.g., maximize reproduction of genes), and are transmitted by genetic or cultural evolution, or imitation, then determine mathematically which rules will survive (e.g., Canning, 1997; Cubitt and Sugden, in press). The contrast between this approach and traditional decision theory is remarkable. A decision rule is traditionally justified or explained by the set of axioms which imply it; if the axioms seem plausible then the rule is too, by implication. To an evolutionist this reasoning reflects a kind of creationism-- a rule s existence is not explained until its ability to survive natural selection has been established.

C. Case-based decision theory

I am a fan of the case based decision theory of Gilboa and Schmeidler (1995). To convey the spirit of case-based theory, imagine that you never learned about expected utility and subjective expected utility theories which value choices by sums of probability-weighted outcome utilities. Now consider how you might decide to hire a colleague, buy a house or choose a movie to see. In case-based theory you do so by comparing the current group of options (or case) to previous groups. The value of options is computed by considering how well different actions that were chosen in the past actually performed, and weighting those historical outcomes by the similarity of those actions to the current action under consideration. To do this, you compare a possible colleagues to others who are like her, taking an average of the previous successes and failures weighted by similarity of those previous people to her.[6] In buying a house, you compare the house to others in the neighborhood (comparables in real estate jargon) or to other houses you have seen or lived in.

Standard subjective expected utility looks ahead-- parsing the world into possible states with different consequences and weighing those states by their likelihoods. In contrast, case-based theory emphasizes the past as a way of guessing the future, parsing the world into actual consequences and weighting them by similarities. For many types of decisions, the logic of similarity-based historical comparison is just more plausible as a description of how people think (and respects the large literature in cognitive psychology on reasoning and similarity). When you choose a movie, house, or colleague, do you think about possible consequence states and weigh their likelihood? Or do you instinctively compare each movie, restaurant, or colleague to others you have seen and liked or disliked? You almost surely do some of the latter.

There is surprisingly little research on case-based theory other than a series of papers by Gilboa and Schmeidler extending the theory (allowing similarity between acts, as well as cases), showing the conditions under which it will converge to expected utility, and pointing to applications like consumer choice. Indeed, case-based decision theory is both a general language for thinking about the components of choice, and a theory of how preferences are formed over time (since preferred choices will change with one s historical experience). It seems ripe for application to decision making in domains where guessing state probabilities and consequences is awkward but recalling similar past cases is natural. For example, lawyers have staunchly resisted the introduction of probabilistic reasoning into legal judgment. Instead, they tend to think about legal cases by judging how well threads of a woven fabric of precedent apply to a current case. Since similarity to previous cases plays such a central role in legal reasoning, case-based theory could be very useful in characterizing how judges and juries make decisions or helping them do so more systematically (see Sunstein, 1997).

D. Hedonics

In centuries of philosophical thought, before the revealed preference approach came to dominate economics, the concept of utility had various meanings which would seem strange or even nonsensical to modern economists. For example, many philosophers thought of utility as sensation of momentary pleasure (and pain), rather than a number used to index choices of commodity bundles. Reopening the exploration of these distinctions among types of utility is the goal of research on hedonics , or the Benthamite program begun by Daniel Kahneman in various collaborations (e.g., Kahneman, Sarin and Wakker, 1997).

From the point of view of hedonics, revealed preference concentrates obsessively on decision utility -- the utility revealed (tautologically) by what is chosen, or decided. But in principle, one could also distinguish this from experienced utility -- sensational measurements of on-line, real-time pleasure and pain-- which recalls an older use of the term utility . The two kinds of utility will differ before people have learned what they like (children eating too much candy or reaching toward an open flame). Decision and experienced utility also differ when people routinely choose things they do not take physical pleasure from, like people with compulsive disorders washing their hands obsessively. This is not to say that people will stupidly choose goods which make them miserable over and over (though some might), but simply that a distinction can be drawn between deciding and experiencing, and explored empirically. Having established a possible distinction between decisions and experiences, one can also distinguish forecasted utility (a forecast of experienced utility, which is likely to be closely related to decision utility but conceptually different) and remembered utility .

Kahneman makes a persuasive case that we should distinguish these types of utility and explore them empirically. For example, forecasted utility is important because virtually all choices require people to forecast the utility they will get from an experience they will have in the future, rather than immediately. The time gap between choice and consumption varies from minutes (ordering at speedy Baja Fresh) to hours (deciding to rent Casino later tonight) to days (planning a weekend outing to Santa Anita racetrack) to months (choosing what to teach next year) to years (building a house) to decades (planting an oak tree, getting a tattoo, bearing a child). Seen this way, the problem of forecasting what you will want in the future is the essence of choice. There is simply no reason to believe that revealed (decision) utilities, perhaps based on remembered utilities, will necessarily be unbiased forecasts of experienced utility. Evidence has already accumulated that there are special errors in forecasting future tastes (see Loewenstein and Schkade, in press), and this crucial problem for economics should certainly be explored further.

E. Neurobehavioral economics

Imagine a group of astronomers who theorize about the moon, using only observations from a weak telescope, or geologists who theorize about the earth s core using only evidence from earthquakes and volcanos. Suddenly they have a spaceship, or a huge drill. Should they use these tools to check whether the assumptions they make about the moon and the earth-- previously beyond their observational reach-- are correct or not? Of course they should!
I think economists will soon be in a similar position with respect to the human brain. Tremendous advances in genetics and brain scanning are making possible a profound leap in understanding the details of brain mechanisms.

A standard mantra in economics is that assumptions about individual rationality like completeness of preference, linearity of probability weights, constrained maximization, exponential discounting, and rational expectations are only as if stand-ins for some (incompletely) specified theory in which learning, advice, or market forces create prices and quantities like those which would result if individuals obeyed these assumptions. But why not take these assumptions seriously and ask whether brain mechanisms exist which lead to the assumed behavior and, if not, what behaviors do those mechanisms cause? [7] Some kinds of economic rationality may turn out to be consistent with well-established brain mechanisms, and in other cases neuroscientific evidence will suggest mechanisms which imply different assumptions. For example, I am confident we will see neuroscientific evidence for the hyperbolic model of discounting, or something akin to it, rather than for dynamically-consistent exponential discounting, as described in section II.A above.

Two examples will illustrate, and perhaps whet the reader s appetite.

(i) Research on the neural computation of utility tries to determine the neural mechanisms which encode liking and compare liking of two different rewards to determine which is better (given prices). Evidence from rats (and some earlier evidence from humans) implicates , as the neuroscientists say, electrical brain stimulation reward and dopamine neurotransmitter levels in the limbic system as elements of such a system (e.g., Shizgal, 1997). Brain stimulation reward does not seem to satiate, substitutes easily for both food and drink (which do not substitute well for each other[8]), and seems to affect brain centers downstream from the centers which are activated by food and liquid. Thus, brain stimulation reward has ideal properties to be a common currency or brain money which can be used to compare two rewards which are physiologically different (like food and drink), like a kind of neurochemical utility.

(ii) Neuroscientific evidence is likely to improve economic theories of addiction. In the view which appeals to most economists, addiction is simply an extreme case of an intertemporal, intrapersonal spillover (or internality) in which past consumption of a good influences present utility from consuming it (Becker and Murphy, 1988). The brain evidence is roughly consistent with this view, but much more precise (and surprising) about the details of the internality.

Most addictions produce a combination of craving or withdrawal (displeasure associated with coming down from using a drug--or a disutility from not consuming) and enhanced tolerance (or reduced utility from a fixed dose). These effects can be observed in great neural detail (e.g., measured dopamine levels) in rats and other animals who, interestingly, can become addicted to all the chemical substances humans become addicted to. (This parallelism alone suggests that physical addiction is a primitive process which evolved in animal brains a long time ago and still exists in the old part of our brains.)

A more neurally detailed version of the economic model could incorporate these processes, allowing three wrinkles: First, there are substantial individual differences in addictiveness. Second, it is not clear whether addicts realize they are becoming addicted or act rationally , in a sense of stable foresightful preference, during the times when they are either high or craving (as is assumed in the Becker-Murphy view).

The third wrinkle is truly amazing. Recent research indicates that while addicts develop a dependence on the drug itself (which results in unpleasant craving and perhaps painful withdrawal), they can also learn to associate drug use with environmental cues like drug use rituals and the place, time, and people with whom they use. The brain learns to expect a drug dose to follow when these cues are present (as in classical Pavlovian conditioning, in which dogs learn a bell is followed by food, so that the bell produces salivation). In a homeostatic opponent process, like a thermostat, when the cue is observed the brain knows opiates are coming and turns off naturally-produced opiates, which produces craving. As a result, simply seeing an advertisement, an old drug friend, or a piece of drug paraphernalia, can activate craving and spontaneously increase demand for a drug. For example, my colleague Mr. G. used to smoke only in his car. After quitting, he would crave a smoke <u>when he got into the car</u>, but not at other times. Similarly, many Vietnam veterans who were addicted to heroin during the war were able to quit easily when they returned home to America (where the environmental cues surrounding them during their wartime heroin use were absent).

Many drugs are easy to withdraw from slowly (e.g., a clinician can get a user off heroin painlessly by tapering doses downward over 30 days) but long-term abstinence is hazardous because environmental cues can trigger a craving which induces relapse into drug use. Cue-based conditioning greatly complicates the Becker-Murphy view, because it means that a desire for the drug (expressed as increased marginal utility) can come from sources other than one s own previous consumption (see Laibson, 1996). Past-consumption internalities are accompanied by externalities, which takes some responsibility for consumption out of the addict s hands and undermines the addict s ability to consume addictive substances rationally.

III. Conclusion

Happily, the exploration of procedural (or bounded) rationality of individuals as they make economic decisions is an idea whose time has finally come. Interesting research is happening in many different areas. This paper has mentioned a few but left out many others.

Influenced by computer science and automata models, theorists explore models in which agents rationality bounds are computational or result from limited memory (e.g., Rubinstein, 1997).

Computational economists interested in very complex systems, liberated by computing power from the shackles (and discipline?) of analytical tractability, posit many types of limitedly-rational agents and study how their simple behavior leads to emergence of something more complex and possibly lifelike.

Game theorists have essentially abandoned the naive idea that equilibration arises from mental tatonnement by hyperrational players who figure out an equilibrium in their heads, and have turned instead to the formal details of evolutionary and adaptive equilibration by players who evolve (Weibull, 1995), or learn from experience (Camerer and Ho, 1997) or from each other.

Macroeconomists are once again interested in rule-of-thumb consumption (cf. Keynes s consumption function) and habit formation, and various ways to model learning (Sargent, 1994).

Business is booming in behavioral finance , which seeks to explain price and volume movements which strain credibility of efficient-markets explanations, using models in which traders are not always utility-maximizing and Bayesian (e.g., Thaler, 1995).

Environmental economists are energetically trying to measure consumer preferences for nonmarket contingently valued goods like clean air. Standard theory gives no reason why the value people place on these goods should be any harder to elicit than, say, their home equity or college GPA. But eliciting consistent, reasonable contingent valuations from people seems as slippery as asking them about whether they d prefer to visit Mars or Venus, or the pH of the dirt in their yards. The difficulty of eliciting reasonable valuations has made many economists realize people often do not have well-formed preferences as standard theory assumes. Instead, they "construct" a preference; they try to answer difficult questions about their own valuations like they approach the problem of figuring out an unknown quantity, like guessing the distance to the moon.

All these developments in important areas of economics mark progress away from a purely rational-choice model which is normatively appealing but descriptively incomplete, toward a more general conception (which, of course, should include rational models as a special case when possible). It means we can spend less time attacking and defending overly simplified rational-choice theories and spend more time doing what we had in mind all along-- better economics.

References

Ainslie, George. Specious reward: A behavioral theory of impulsiveness and impulse control. Psychological Bulletin, 82 (1975): 463-509.

Becker, Gary S. and Murphy, Kevin M. A Theory of Rational Addiction. Journal of Political Economy. 96 (1988): 675-700.

Benartzi, Shlomo and Richard Thaler. Myopic Loss Aversion and the Equity Premium Puzzle. Quarterly Journal of Economics, 101 (1995): 73-92.

Bowman, David, Debby Minehart, and Matthew Rabin. Loss aversion in a savings model, University of California, Berkeley working paper, 1996.

Buschena, David and Zilberman, David. Performance of the similarity hypothesis relative to existing models of risky choice. Journal of Risk and Uncertainty 11 (1995): 233-254.

Camerer, Colin. F. Do biases in probability judgment matter in markets? Experimental evidence? American Economic Review 77 (1987): 981-997.

Camerer, Colin. F. An experimental test of several generalized utility theories. Journal of Risk and Uncertainty 2 (1989): 61-104.

Camerer, Colin F. Recent tests of generalized utility theories. In Utility Theories: Measurement and Applications, ed W. Edwards. Cambridge:Cambridge University Press, 1992.

Camerer, Colin F. Behavioral economics in the wild: Field evidence for prospect theory. In Choices, Values and Frames edited by D. Kahneman and A. Tversky. in press.

Camerer, Colin F.; Babcock, Linda; Loewenstein, George; and Richard Thaler. Labor Supply of New York City Cab drivers: One day at a time. Quarterly Journal of Economics, 103(1997): 407-441.

Camerer, Colin F. and Ho, Teck-Hua. Experience-weighted attraction learning in normal-form games. Caltech working paper, 1997.

Camerer, Colin F. and Martin W. Weber. Recent developments in modeling preferences: Uncertainty and ambiguity. Journal of Risk and Uncertainty 5 (1992): 325-370.

Canning, David. Evolution, preferences, and choice under uncertainty. Queen s University of Belfast working paper, 1997.

Chew, S. H. and K. R. MacCrimmon. Alpha utility theory, lottery composition and the Allais paradox. University of British Columbia Faculty of Commerce and Business Administration #686. 1979.

Chew, S. H. and W. S. Waller. Empirical tests of weighted utility theory. Journal of Mathematical Psychology 30 (1986): 55-72.

Ciccheti, Charles and Jeff Dubin. A microeconometric analysis of risk-aversion and the decision to self-insure. Journal of Political Economy, (1994) 102, 169-186.

Cohen, David and Jack L. Knetsch. Judicial choice and disparities between measure of economic values. Osgoode Hall Law Journal, (1992) 30, 737-792.

Cook, Philip J and Clotfelter, Charles T. The peculiar scale economies of Lotto. American Economic Review 83 (1993): 634-643.

Cubitt, Robin and Robert Sugden. The selection of preferences through imitation. Review of Economic Studies, in press.

Frisch, Deborah and Jonathan Baron. Ambiguity and rationality. Journal of Behavioral Decision Making 1(1988):149-157.

Ganguly, Ananda; John H. Kagel; and Donald V. Moser. Do asset market prices reflect traders judgment biases? University of Pittsburgh working paper, 1998.

Ghirardato, Paolo. On independence for non-additive measures, with a Fubini Theorem. Journal of Economic Theory 73(1997): 261-291.

Gilboa, Itzhak and David Schmeidler. Case-based decision theory. Quarterly Journal of Economics 110 (1995): 605-640.

Hardie, Bruce G. S., Eric J. Johnson and Peter S. Fader. Modeling loss aversion and reference dependence effects on brand choice. Marketing Science, 12(1993): 378-394.

Harless, David and Camerer, Colin F. The predictive utility of generalized expected utility theories. Econometrica 62 (1994): 1251-1290.

Hey, John D. And Orme, Chris. Investigating generalizations of expected utility theory using experimental data. Econometrica 62 (November 1994) 1291-1326.

Jullien, Bruno and Salanié, Bernard. Estimating preferences under risk: The case of racetrack bettors. IDEI and GREMAQ Toulouse University working paper, 1997.

Kahneman, Daniel and Amos Tversky. Prospect theory: An analysis of decision under risk.

Econometrica 47 (1979): 263-291.

Kahneman, Daniel and Amos Tversky. Advances in prospect theory: Cumulative representation of uncertainty. Journal of Risk and Uncertainty 5 (1992): 297-324.

Kahneman, Daniel; Sarin, Rakesh; and Wakker, Peter P. Back to Bentham? Explorations of experienced utility. Quarterly Journal of Economics 112 (1997): 375-406.

Laibson, David I. A cue theory of consumption. Harvard University Manuscript, 1996.

Laibson, David. Golden eggs and hyperbolic discounting. Quarterly Journal of Economics 112 (1997): 443-478.

Lattimore, P. K., J. R. Baker and A. D. Witte. The influence of probability on risky choice: A parametric investigation. Journal of Economic Behavior and Organization, 1992, 17, 377-400.

Leland, J. A theory of approximate expected utility maximization. Carnegie-Mellon Department of Social and Decision Sciences, 1991.

Loewenstein, George. Anticipation and the valuation of delayed consumption. Economic Journal 97(1987):666-684.

Loewenstein, George. The fall and rise of psychological explanation in the economics of intertemporal choice. In Choice Over Time edited by G. Loewenstein and J. Elster. New York: Russell Sage Foundation, 1992.

Loewenstein, George. Out of control: Visceral influences on behavior. Organizational Behavior and Human Decision Processes, 1996, 65, 272-292.

Loewenstein, George and Schkade, David. Wouldn t it be nice? Predicting future feelings. In Foundations of hedonic psychology: Scientific perspectives on enjoyment and suffering edited by E. Diener, D. Kahneman, and N. Schwarz. New York: Russell Sage Foundation Press. In press.

Machina, M. J. Expected utility analysis without the independence axiom. Econometrica 50 (1982): 277-323.

Morgenstern, O. Some reflections on utility theory. In The Expected Utility Hypothesis and the Allais Paradox, edited by M. Allais and O. Hagen. Dordrecht: D. Reidel. 1979.

Myagkov, Mikhail and Plott, Charles R. Exchange economies and loss exposure: Experiments exploring prospect theory and competitive equilibria in market environments. American Economic Review 87 (1997): 801-828.

Odean, Terrance. Are investors reluctant to realize their losses? Journal of Finance, in press.

O Donoghue, Ted and Matthew Rabin. Doing it now or later. American Economic Review, in press.

Phelps, E. S., and Pollak, R. A. On second-best national saving and game-equilibrium growth. Review of Economic Studies 35 (1968): 201-208.

Prelec, Drazen. The probability weighting function. Econometrica, in press.

Quiggin, John. A theory of anticipated utility. Journal of Economic Behavior and Organization 3 (1982): 323-343

Rabin, Matthew and Schrag, Joel. Confirmation bias. Unpublished University of California, Berkeley Department of Economics working paper. 1997.

Read, Daniel and Loewenstein, George. The diversification bias: Explaining the difference between prospective and real-time taste for variety. Journal of Experimental Psychology: Applied 1 (1995): 34-49.

Rubinstein, Ariel. Similarity and decision-making under risk (Is there a utility theory resolution to the Allais paradox?). Journal of Economic Theory, 46 (1988): 145-153.

Sargent, Thomas J. Bounded rationality in macroeconomics. New York: Oxford, 1994.

Schmeidler, David. Subjective probability and expected utility without additivity. Econometrica 57 (1989): 571-587.

Shea, John. Union contracts and the life-cycle/permanent-income hypothesis, American Economic Review, 85 (1995): 186-200.

Shizgal, Peter. Neural basis of utility estimation. Current Opinion in Neurobiology 7 (1997) 198-206.

Starmer, Chris. Developments in non-expected utility theory: The hunt for a descriptive theory of choice under risk. Journal of Economic Literature, in press.

Thaler, Richard H. Mental accounting matters. In Choices, Values and Frames edited by D. Kahneman and A. Tversky, in press.

Thaler, Richard H. (Ed.), Advances in Behavioral Finance. New York: Russell Sage Foundation Press, 1995.

Rubinstein, Ariel. Modeling Bounded Rationality. Cambridge: MIT Press, 1997.

Sunstein, Cass. Behavioral analysis of law. University of Chicago Law Review 64 (1997) 1175-1189.

von Neumann, J. and O. Morgenstern. Theory of Games and Economic Behavior. Princeton: Princeton University Press, 1944.

Weibull, Jörgen W. Evolutionary Game Theory. Cambridge: MIT Press, 1995.

Endnotes

1 Morgenstern (1979) said he and von Neumann never intended expected utility to apply to gambles with low outcome probabilities (For example, the probabilities used must be within certain plausible ranges and not go to .01 or even less to .001, then be compared to other equally tiny numbers such as .02, etc.). He gives no hint, however, what kind of theory should apply there.

2 Incidentally, the Kahneman and Tversky (1979) paper on prospect theory is one of the most widely-cited papers ever published by Econometrica.

3 Fanning out refers to Machina s (1982) clever conjecture that many choice paradoxes can be explained by indifference curves which fan out when plotted in the Marschak-Machina triangle. This means that people act as if they get more risk-averse when choosing among better gambles. Mixed fanning allows them to get more risk-averse in one range, then less risk-averse after that. It ends up on the menu largely because it allows almost any patterns and is therefore least parsimonious.

4 A related statement, of interest in studies on savings and consumption, is that different categories of wealth may be mentally accounted for differently rather than combined into a single net worth figure (see Thaler, in press).

5 It is true that in each case, special modifications to the standard expected utility approach could conceivably explain the anomaly-- survivorship bias at the market level might explain the equity premium, participation bias among cab drivers might explain downward-sloping labor supply, unobserved heterogeneity among consumers might explain asymmetric price elasticities, and so forth. But these modifications are truly ad hoc because a special modification to expected utility is needed for each phenomenon, which leads to an applied version of expected utility which is crusted with special features like a boat s hull is crusted with barnacles. Decision isolation plus prospect theory can explain them more parsimoniously.

6 Technically, I am allowing similarity between acts as well as between cases, which seems much more natural for examples like these.

7 The evolutionary-foundations approached referred to at the end of section II.B seems to have already captured the curiosity of many economic theorists. This suggests (by revealed preference!) that in principle, economists are open to study the foundations or origins of preference. But by asking which behavioral mechanisms might have survived natural selection, the evolutionary approach leapfrogs back one long causal leap. Neurobehavioral economics stops in the middle of that causal leap to simply ask how the brain works, before asking why it might have evolved to work that way.

8 Substitutability is measured in the standard way, by a cross-price elasticity, where the price of

food is the number of lever taps is the amount of work an animal is required to perform to receive a reward.

Table 1: Nine **field phenomena** inconsistent with EU, consistent with cumulative prospect theory

DOMAIN	REGULARITY	DESCRIPTION	TYPE OF DATA	ISOLATED DECISION	INGRED-IENTS	REFERENCES
Macro-economics	**Insensitivity to bad income news**	Consumers do not cut consumption when they get bad income news	Teachers earnings, savings	No isolation	Loss-aversion, reflection	Shea (1994); Bowman, Minhart and Rabin (1996)
Stock market	**Equity premium**	Stock returns are too high, relative to bond returns	NYSE stock, bond returns	Single yearly return (not long-run)	Loss-aversion	Benartzi and Thaler (1995)
Stock market	**Disposition effect**	Hold losing stocks too long, sell winners too early	Individual investor trades	Single stock (not portfolio)	Reflection effect	Odean (in press)
Labor econ	**Downward-sloping labor supply**	NYC cabdrivers quit around daily income target	Cabdriver hours, earnings	Single day (not week or month)	Loss-aversion	Camerer et al (1997)
Consumer goods	**Asymmetric price elasticities**	Purchases more sensitive to price increases than to cuts	Product purchases (scanner data)	Single product (not shopping cart)	Loss-aversion	Hardie, Johnson, Fader (1993)
Insurance	**Buying phone wire insurance**	Consumers buy overpriced insurance	Phone wire insurance purchases	Wire risk (not portfolio)	Overweight low p(loss)	Cicchetti and Dubin (1994)
Horserace betting	**Favorite-longshot bias**	Favorites are underbet, longshots overbet	Track odds	Single race (not day)	Overweight low p(win)	Jullien and Salanie (1997)
Lottery Betting	**Demand for low-p lotteries**	More tickets sold as top prize rises, p(win) falls	State Lottery sales	Single lottery	Overweight low p(win)	Cook and Clotfelter (1993)
Law	**Grandfather clauses, two-tier wages, time-held rules**	Endowment effects: Owners protected compared to identical newcomers	Legal rulings	Single item or income stream	Loss-aversion	Cohen and Knetsch (1992)