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When Time Is of the Essence: Averaging, Aspiration, and the Short Run

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A variety of experiments have demonstrated that people's choices among gambles differ according to whether the gamble is to be played just once (the unique case) or multiple times (the repeated case). The reason for this difference appears to be that people are acting in each case so as to increase the likelihood that the chosen alternative will leave them better off than the nonchosen alternative. This paper explores four themes that bear on the difference between unique and repeated gambles. The first traces the historical and theoretical role of weighted averaging in risky choice. The second defends the usefulness of probability-based rules for choices involving aspiration levels. The third presents the idea that choices under risk may reflect dual criteria, one based in weighted averaging and the other based in stochastic control. The fourth challenges the conventional criteria by which choices are judged to be rational or irrational.

Some years ago, I published a theoretical note (Lopes, 1981) that took issue with the proposition that rationality requires one's choices under risk to conform to the axioms of expected utility theory. My claim was that sensible people often base their choices on the probability of coming out ahead. In particular, I argued that sensible people might reject single plays of gambles with high expected utilities and low probabilities of winning but accept multiple plays of the same gambles if the net probability of winning were acceptably high.

The article was controversial and drew some fire (Tversky & Bar-Hillel, 1983). It also stimulated a conference (Beach, Vlek, & Wagenaar, 1988) and several experiments that compared people's gamble preferences under unique (one play) and repeated (multiple play) conditions. All found the distinction to affect people's choices. Keren and Wagenaar (1987; also Keren, 1991) found that violations of utility theory reflecting "certainty" and "possibility" effects observed under unique conditions disappear under repeated play. Wedell and Bockenholt (1990) found that nonnormative reversals in preference induced by different response modes (choice versus pricing) are reduced under repeated play. Joag, Mowen, and Gentry (1990) found that subjects in both a gambling task and a simulated industrial purchasing task combined information about probabilities and outcomes multiplicatively for decisions with multiple plays but additively for decisions involving a single play. Redelmeier and Tversky (1990, 1992) found that physicians make different decisions depending on whether they are deciding on a case-by-case basis or making aggregate decisions for groups of patients.

The foregoing experiments confirm that subjects choose differently under unique and repeated conditions. They also support the idea that subjects choose differently in the two situations in order to increase the likelihood that the chosen alternative will leave them better off. As Keren (1991) put it, the reason his subjects responded differently was "not necessarily the unique-repeated distinction *per se* but rather the degree of risk involved" (p. 301), an assessment his subjects appeared to base on their probability of not winning at all or of winning only the smallest amount. Similarly, Wedell and Bockenholt's subjects often justified their decisions by pointing out that the "chances of winning something" increased with more plays.

At one time, my note on the short run seemed isolated from my other research, a solitary jeer from the back of the crowd: "The Emperor has no clothes!" But now I see connections everywhere. One paragraph in particular stands out as prescient of views I would discover anew years later. In it I speculated on how the model of rational choice might be revised:

> First, the revised rationality criterion will be composite, reflecting multiple independent criteria. Among these, of course, will be the unquestionably important criterion of expected value (or expected utility). But other criteria may include vari-

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> ables such as efficiency and, what has been stressed in this paper, probability of coming out ahead. . . . Thus the conception of rationality will come to rest more squarely on essentially subjective judgments about how diverse—and sometimes competing—criteria combine or trade off with one another. (Lopes, 1981, p. 385)

Although time has qualified the details of these speculations, they foreshadow four themes that have persisted in my thinking: first, the importance of weighted averaging in risky choice; second, the role of probability-based rules for choices involving aspiration levels; third, the idea of dual or multiple criteria for risky choice; and fourth, skepticism concerning the conventional criteria by which choices are judged to be rational or irrational. These themes will provide the structure for the present article.

LINEARITY AND AVERAGING IN RISKY CHOICE

Five decades have elapsed since von Neumann and Morgenstern (1944) revived academic interest in the expected utility model. During the first 35 years, economists, statisticians, and decision analysts elaborated the theory and applied it to decision aiding. This was a time of theoretical complacency even though the most telling challenges to the theory had been issued relatively early (Allais, 1952/1979; Ellsberg, 1961) and even though behavioral experiments were regularly signaling the theory's descriptive inadequacy. The next 10 years saw intense theoretical activity directed at improving the model's descriptive accuracy and at better understanding the mathematical options for incorporating psychological variables into the model. The most recent 5 years have been marked by quiet acceptance of modeling approaches that might once have been rejected as irrational and by a highly productive blurring of the disciplinary line between economists and psychologists.

It is not necessary to review all the developments of the last 15 years. We need only trace the fate of one assumption that is central to both expected utility theory and subjectively expected utility theory (Savage, 1954). This is the assumption that people's preferences among gambles will be linear in probability (either objective or subjective). Theorists in the early years vied with one another to construct formal descriptions of the requirement (packaged as axioms bearing names such as independence, substitution, and the surething principle) that would appeal directly to intuition and thus provide normative support for the theory. Demonstrating such support was important because if linearity could be justified, entire distributions could be replaced by their means when complex gambles were analyzed.

Maurice Allais, a French economist, was also working in the early 1950s to describe linearity in a way that would be intuitively transparent. But his object was to demonstrate the inadequacy of the assumption. Allais saw that linearity implies that preferences among gambles should be invariant over additive and multiplicative rescaling of the probabilities. In other words, if we take a pair of gambles and reduce the probabilities of winning in both by either subtracting a constant or dividing by a constant, the relative attractiveness of the gambles should not change. The thought problems that Allais invented (1952/1979) showed clearly that people's preferences do change when probabilities are rescaled. When the probabilities of achieving desirable outcomes are large, people tend to be cautious and prefer certainty or as near to that as can be had. When the probabilities are small, people trade off small amounts of probability in favor of larger prizes. They say, in essence, "What the hell, I might as well go for it!"

Although Allais' paradoxes (as they are now called) were compelling in the sense that even staunch advocates of expected utility theory fell into their trap, they did not compel reexamination of the theory. Instead, blinders went on and the rhetorical volume was turned up in the form of "in-principle arguments" concerning the potential consequences of violating expected utility axioms. One of these—the Dutch book theorem—contended that if people's "degrees of belief" do not conform to the laws of probability, they will be liable to accept combinations of bets that guarantee a net loss. For example, someone who believes that each of two opposing candidates has a 60% chance of winning an election should be willing to bet at 4:6 on each, thereby losing no matter who wins. Another—the money pump argument—concerned intransitive preferences. Suppose that a person has the preferences, $C > B > A > C$. The person could be given A, then be offered B for A plus a penny, then be offered C for B plus a penny, then be offered A for C plus a penny, and so on until the unwary money pump was sucked dry.

It was not until the late 1970s that the linearity logjam was broken. In particular, 1979 saw the publication of an edited volume (Allais & Hagen, 1979) containing an English translation of Allais' original critique of linearity along with commentary by many well-known advocates of expected utility theory and further critique of the theory by Allais and others. A series of conferences (the biennial FUR series on the Foundations of Utility and Risk Theory) followed that has provided a forum for exploring nonlinear extensions of expected utility theory. Around the same time, Karmarkar (1978, 1979) and Kahneman and Tversky

(1979) proposed "weighted utility" theories that followed up on the earlier suggestion by Edwards (1962) that Allais-type preferences could be modeled by assuming that the psychological impact (or weight) of probabilities on decisions is a nonlinear function of probability magnitude. The version by Kahneman and Tversky (prospect theory) was especially influential in stimulating interest in descriptive models of risky choice.

Weighted utility was heavily criticized by economists, however, because the model predicted violations of dominance.[1] A more acceptable approach to nonlinear weighting was soon suggested independently by several economists (Allais, 1986; Quiggin, 1982; Yaari, 1987) and has since been explored extensively by others (e.g., Chew, Karni, & Safra, 1987; Luce, 1988; Schmeidler, 1989; Segal, 1989; Wakker, 1990). This approach, commonly called "decumulative weighting" or "rank dependent weighting," weights the probability of a potential outcome according to both the magnitude of its probability and the position of the outcome in the set of possible outcomes.

The difference between weighted utility and decumulative weighting is easily conveyed by example. Consider a gamble offering a 5% chance of winning \$1, a 90% chance of winning \$10, and a 5% chance of winning \$100. Computing the expected value in the ordinary way, we get: .05(\$1) + .90(\$10) + .05(\$100) = \$14.05. If we apply standard nonlinear weights to the gamble, the probabilities are replaced by weights that are a function of probability magnitude. Thus, the chance of obtaining the worst outcome is assigned the same weight as the chance of receiving the best outcome. Alternatively, we can compute the expectation by noticing that the gamble offers \$1 for sure *plus* a 95% chance of an additional \$9 *plus* a 5% chance of an additional \$90. Although the computation looks different— 1.00(\$1) + .95(\$9) + .05(\$90) = \$14.05—the answer is the same. In this case, however, a function assigning weights to "decumulative probabilities" would give quite different values to the chances of receiving the best and the worst outcomes.

Decumulative weighting models are technically superior to weighted utility models for representing behavioral nonlinearity. Consequently, Tversky and Kahneman (1992) now use a combination of cumulative and decumulative weighting functions in prospect theory.[2] But decumulative weighting is also descriptively superior to weighted utility for representing the cognitive sources of behavioral nonlinearity. For example, decumulative weighting can capture dispositional differences in the importance that different people attach to avoiding bad outcomes versus achieving good outcomes (Lopes, 1987, 1990). It also relates in a natural way to descriptions of risky choice that focus on risk dimensions and lexicographic processes (Lopes, 1995).

As Birnbaum, Coffey, Mellers, and Weiss (1992) pointed out, decumulative weighting is a form of differentially weighted averaging (Anderson, 1981; Birnbaum, 1972; Oden, 1971). Averaging models in general capture the integrated response of a decision maker to the individual elements of a complex stimulus. Expectation models constrain the expression of individual values to the utility function or to "subjective probabilities" that obey the quite restrictive axioms of probability theory. Even when probability weights are introduced descriptively (as in weighted utility), they tend to be interpreted normatively in terms of "underweighting" or "overweighting." With decumulative weights, however, deviations from linearity are not so likely to be attributed to errors in weighting. Although decumulative weighting functions can be interpreted perceptually as arising from probability distortion (Quiggin, 1982) or patterns of underweighting and overweighting (Tversky & Kahneman, 1992), they also lend themselves to nonperceptual interpretations such as representing attitudes toward risk (Yaari, 1987) or expressing a decision maker's preference for security (Allais, 1986; Lopes, 1990; Lopes, 1995).

ASPIRATION AND PROBABILITY-BASED DECISION RULES

An alternative approach to decision making under risk is suggested by a problem studied by Dubins and Savage (1976, p. 1):

> Imagine yourself at a casino with \$1,000. For some reason, you desperately need \$10,000 by morning; anything less is worth nothing for your purpose. . . . The only thing possible is to gamble away your last cent, if need be, in an attempt to reach the target sum of \$10,000. . . . The question is how to play, not whether.

Although Dubins and Savage knew they could answer this particular question in expected utility terms by

[1] For example, dominance would be violated if a person preferred the gamble {.01, \$100; .95, \$99; .04, \$0} to {.06, \$100; .90, \$99; .04, \$0}. Preferences like this can occur under weighted utility because the weights in weighted utility usually do not conform to the axioms of probability theory. Although weighted utility technically includes subjectively expected utility as a special case, the version of weighted utility needed to account for Allais-type preferences does not.

[2] Somewhat earlier, Luce and Fishburn (1991) axiomatized a rank-dependent and sign-dependent representation for choices among uncertain alternatives that includes cumulative prospect theory as a special case.

using a two-valued utility function,[3] they preferred to treat the problem as one of maximizing probabilities. What they found is that the decision maker cannot afford to play cautiously. If he hazards only small bets, the casino will eventually deplete his stake. Instead, he must play boldly, staking either everything on each bet or (if the target can be reached with a single bet) staking as much as needed to meet the target.

Procedures that maximize the probability of achieving a target outcome subject to stated constraints are studied in a field of mathematics called "stochastic control." Although this is a relatively new field in terms of technical development, probability-based solutions can be found among those that were suggested for the famous eighteenth century thought problem now known as the St. Petersburg paradox:

> A fair coin is tossed until it lands tails (on the nth toss) at which point the player is paid $\$2^n$. How much should a player pay for a single play of the game?

Probabilitists of the late seventeenth century believed that the worth of a gamble is given by its expected value. The St. Petersburg game showed this idea to be flawed, however, because it was immediately apparent to all that the game is worth much, much less than its expected value—which is, in fact, infinite.

Many scholars took up the challenge of explaining the discrepancy between the low apparent worth and the infinite expected value of the St. Petersburg game. The classical expected utility solution was offered by Daniel Bernoulli (1738/1967) and independently by Cramer (cited in Bernoulli, 1738/1967). Both made use of the idea that money has "diminishing marginal utility" and that substitution of utility for monetary value would appropriately diminish the value of the game. There were, however, a variety of other contemporary solutions (reviewed in Daston, 1980; Samuelson, 1977; Todhunter, 1865) not involving utility. D'Alembert, Buffon and Condorcet each proposed that the value of the game would be small if probabilities smaller than some critical threshold were ignored. Fontaine, Buffon and later Poisson proposed that the value of the game would be objectively small because the seller's fortune must be finite. Buffon also suggested that the value of the game would be small because the time available for play would necessarily be finite.

Although these solutions seem very different, they take a common approach to reducing the game's infinite value. If we write out the expression for computing the expected value, we find that each probability/outcome term in an infinite series contributes equally to the infinite result:

$$\begin{aligned} EV &= 1/2\ \$2 + 1/4\ \$4 + 1/8\ \$8 + 1/16\ \$16 \\ &\quad + \ldots 1/n\$2^n \ldots \\ &= 1 + 1 + 1 + 1 + \ldots 1 \ldots . \end{aligned}$$

If we then compare the various solutions, we find that each applies a mathematical operation that differentially affects terms with large outcomes and small probabilities. Diminishing marginal utility compresses large outcomes more than small outcomes. Probability thresholds effectively exclude all terms above the critical probability level. Solutions concerning the seller's bankroll cap outcomes at a finite level so that the contributions of terms beyond the cap diminish to zero. Solutions involving time limitations eliminate terms involving the later, larger outcomes. In other words, all four solutions focus operationally on terms representing events that in (almost) all likelihood will not occur.

There was, however, a qualitatively different solution also contributed by Buffon (Todhunter, 1865). Buffon approached the problem of estimating the value of the game empirically by hiring a child to toss a coin 2000 times and record the sequence of outcomes. The experiment allowed him to conclude that the value of the game was likely to be quite small. Some 230 years later, not knowing about Buffon, I too became curious about the empirical value of the game and ran a similar experiment on a computer. The experiment, which constituted the original impetus for my note on the short run, approached the problem from the perspective of an inexhaustibly rich seller—I had in mind Scrooge McDuck. Using hundreds of millions of trials in a Monte Carlo simulation, I found that even if the game were sold at the discount price of \$100 to many, many buyers, it would constitute an almost certain money maker for Scrooge and no bargain at all for the vast majority of buyers (Lopes, 1981).

In discussing Buffon's strategy, Samuelson calls the experiments "childish ways to avoid the true infinity in the Petersburg expectations" (1977, p. 52). I do not suppose Samuelson would judge my replication any more kindly. Still, one can reach the same conclusion that Buffon and I reached by the more respectable (stochastic control) route of asking what price a buyer should pay if she wishes to hold her chances of taking a loss to some specified low value. Consider first what would happen if a buyer worth \$1 million relied on the expected value criterion and paid her entire fortune for one play of the game. In order to avoid a net loss, she

[3] For example, they could assign utility = 1 to values $\geqslant$\$10,000 and utility = 0 to values $<$\$10,000, in which case the expected utility of a gamble would be equal to the probability of winning $\geqslant$\$10,000.

would need to toss at least 19 heads before the first tail, an unlikely event having probability $1/2^{20}$. The vastly more likely outcome is that she would reduce her fortune to a very small sum (e.g., $\leqslant\$64$ with probability $1 - 1/2^6 = .984$). Alternatively, we can suppose that our potential buyer finds a discount seller (my Scrooge McDuck) who is willing to sell her as many plays of the game as she wants very cheaply. As I discovered belatedly after publishing my own work, this is a situation that Allais (1979) analyzed previously in terms of the "theory of ruin." He showed that a player with a bankroll of $1 million who keeps purchasing plays of the game for $33 each will be ruined with probability .9999 provided that settlement is made after every game (i.e., that purchases cannot be made on credit).

Although stochastic control problems are not much studied by psychologists, they provide an alternative model for explaining decision making under risk. For example, Bottom, Bontempo, and Holtgrave (1989) presented subjects with variations of the St. Petersburg game that were designed to allow a test between utility-based decision rules and a probability-based "expectation heuristic" suggested by Treisman (1983) by which subjects set bids by estimating the toss on which tails will appear. The results were generally supportive of the expectation heuristic or of a similarly motivated "bid protection heuristic" by which subjects set bids that give them an even chance of coming out ahead. More generally, the results supported the notion that, in assessing the worth of the St. Petersburg game, people attend mostly to events that are likely to happen rather than events that are unlikely to happen.

In my original note on the short run, I spoke more or less interchangeably about decision rules aimed at maximizing the probability of achieving some target level (for example, maximizing the probability of coming out ahead) and decision rules aimed at maximizing some percentile of a distribution (for example, maximizing the median outcome). Manski (1988) has recently described formalized versions of these rules as, respectively, a "utility mass model" and a "quantile utility model." Manski's models are interesting mathematically because they are consistent with the existence of a preference ordering over actions (and hence are candidate models for rational choice) and because their predictions are invariant under ordinal transformation of utility. They also lead to generalizations of conventional economic ideas about riskiness and risk preference that are psychologically plausible. In particular, they imply that people with greater preferences for risk will either set higher target levels (if they are utility mass maximizers) or maximize higher quantiles (if they are quantile utility maximizers) than people with lesser preferences for risk.

In introducing the utility mass and quantile utility models, Manski observed that "the atmosphere surrounding the expected utility model is highly charged. Attitudes seem to depend on an interplay of ideology, intuition, and pragmatism" (Manski, 1988, p. 82). I can only agree. Two years after the publication of my note, Tversky and Bar Hillel (1983) detailed a number of disagreements that they had with my stance on the difference between long-run and short-run decision making. In their paper, they endorsed the classic view (described above) that one can dispense with the St. Petersburg game by taking into account the seller's finite resources, and they reiterated the standard position that whatever objections one may have to applying the principle of expected value maximization to unique choices, those objections do not apply to the modern (axiomatic) principle of expected utility maximization. Suffice it to say these many years later that the intention of my note was explicitly and exactly to confront such views. I neither repent nor recant the opinions expressed.

There is, however, a point of disagreement that I would like to clarify concerning an anecdote from Samuelson (1963) in which he recounts offering some colleagues to bet $200 to $100 that the side of a coin they specified would not appear at the first toss. One of his companions replied to the offer roughly as follows (Samuelson, 1963, p. 109):

> I won't bet because I would feel the $100 loss more than the $200 gain. But I'll take you on if you promise to let me make 100 such bets. . . . One toss is not enough to make it reasonably sure that the law of averages will turn out in my favor. But in a hundred tosses of a coin, the law of large numbers will make it a darn good bet. I am, so to speak, virtually sure to come out ahead in such a sequence, and that is why I accept the sequence while rejecting the single toss.

Samuelson disapproved of his colleague's decision. Indeed, he considered it irrational and presented a theorem to that effect.

Samuelson stated the theorem as follows: "If at each income or wealth level within a range, the expected utility of a certain investment or bet is worse than abstention, then no sequence of such independent ventures (that leaves one within the specified range of income) can have a favorable expected utility. Thus, if you would always refuse to take favorable odds on a single toss, you must rationally refuse to participate in any (finite) sequence of such tosses" (p. 111). He also warned in a footnote (p. 112) against the "undue extrapolation" of the theorem that might occur if one were to ignore the restriction "if at each income or wealth level within a range."

My intent in discussing the case was to defend Samuelson's colleague against the irrationality charge and to cast my lot with those who sometimes choose to maximize the probability of a gain. Tversky and Bar Hillel seemed to suppose that my contrariness was occasioned by the sort of "undue extrapolation" against which Samuelson had warned. In this, they were mistaken. Samuelson's restriction applies to the range of asset positions that might be reached as a result of playing the proposed sequence of bets. In the case at hand, this would run from $10,000 below (100 losses) to $20,000 above (100 wins) the colleague's then-current asset position. Thus, the restriction entails only that the colleague's reaction to the single bet would have been the same had he happened on that fateful day to be a little poorer or a little richer than he actually was.

In arguing their case, Tversky and Bar Hillel (1983, p. 715) provided an example of a utility function that violates the restriction on the theorem and showed that it would also accept the sequence while rejecting the single bet. To the extent that they intended this to serve as a counterexample, their argument was with Samuelson and not with me since it was he who applied the theorem to his colleague. But personally, I see no reason to suppose that the economic reasoning of a "distinguished scholar" who lunches with Nobel laureates would be radically affected by minor shifts in wealth.

I should acknowledge, however, that Samuelson's colleague was not entirely blameless in having been labeled irrational for he reportedly assured Samuelson that he wished to "stand with Daniel Bernoulli, Bentham, Ramsey, v. Neumann, Marschak, and Savage" in "maximizing the expected or average value of the utility of all possible outcomes" (Samuelson, 1963, p. 110). I can well attest that it is not a comfortable experience to stand with the collection of money pumps, Dutch book victims, and assorted ne'er-do-wells who feel no obligation to obey the axioms and theorems of expected utility theory. Still, I hope Samuelson's colleague would join me in defending his choice. Indeed, one of the strongest arguments I can find against the principle of expected utility maximization is that it considers such an eminently sensible decision to be irrational.

CONFLICT AND THE IDEA OF A DUAL CRITERION

Having just argued the merits (both normative and descriptive) of probability-based rules for explaining the St. Petersburg paradox and Samuelson's offer, I must now fess up that people's responses in both of these situations could equally well be explained in terms of decumulative weighting. Thus, we face the spectre of indeterminacy—and maybe even an embarrassment of psychologically plausible models. On the other hand, being out on two limbs is less precarious than being out on one, and it may be that both models are needed to produce a descriptively complete picture of human risk taking.

To illustrate the point, it is useful to turn briefly to work I have been doing over the last decade on people's preferences among multioutcome gambles. My initial interest was in discovering how risk attitudes, particularly risk aversion, are related to preferences for different distributions of outcomes. Although most authors who have studied distributional variables have focused on point-measures such as variance or skewness, I preferred to think of my stimuli in graphical terms, as a kind of cumulative distribution that welfare economists use for comparing income levels across groups. This work showed that judgments of riskiness as well as preferences that would conventionally be labeled "risk averse" in the expected utility sense could be predicted equally well by the relative likelihood and severity of the worst outcomes in the distributions (Lopes, 1984).

These findings led naturally to the idea of modeling risk aversion in terms of the relative attention paid to the worst and the best outcomes of gambles. My main method was to explain subjects' preferences among various stimuli by relating their stated rationales (protocols) for preferring one gamble over another to the distributional characteristics of the stimuli. Although this work was done before I encountered the idea of decumulative weighting, it already contained the ideas that people think of gambles cumulatively (or as I would now say, decumulatively) and that "risk aversion" reflects differential weighting of bad outcomes rather than diminishing marginal utility (Lopes, 1986, 1987).

To get a sense of how subjects are reasoning, it is useful to let them speak for themselves. Below are four protocols from subjects who were choosing between the two multioutcome lotteries shown in Fig. 1. Each lottery has 100 lottery tickets (tally marks representing probability) distributed over approximately equally spaced outcomes ranging between zero and $200. The subjects in the experiment were graduate students recruited from various graduate departments. Each expressed preferences among a large set of gamble pairs and also provided written rationales at the point of choice for a subset of the gamble pairs. In the experiment, lotteries were unnamed: subjects referred to them by position on the page.

> S3: (chooses peaked): I like to be able to count on getting a moderate amount rather than choosing to get much higher or much lower.

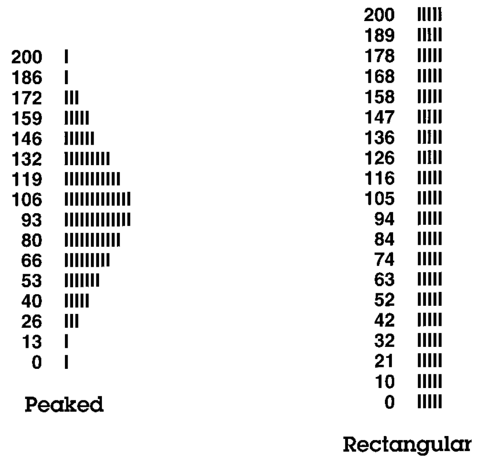

FIG. 1. Example of a pair of multioutcome lotteries. Each lottery has 100 lottery tickets (represented by tally marks) and an expected value of $100. The amounts at the left of each row represent the prize that is paid if a ticket from that row is selected. The names below the lotteries are for expository purposes only. Subjects referred to lotteries by position on the page (e.g., left or right).

> S7: I choose the [peaked] because it offers a majority of chances of at least an amount somewhere between $40.00 and $159.00. The odds of winning are best between those dollar amounts, and the odds indicate an excellent chance of winning an amount between $80.00 and $119.00. The other lottery does not offer as good of odds.
> S8: (chooses peaked): Too many chances of getting a lower prize in the [rectangular] lottery.
> S11: I'll take the [peaked]. It looks like there's a better chance to get at least something.

There are several points to be noticed in these protocols. First, all four of these subjects chose the distributionally less risky peaked lottery. In general, most subjects were risk averse (or "security-minded" in my terms) for gain pairs. Second, subjects often signaled concern with avoiding zero or low outcomes. This was especially true for pairs involving asymmetric gambles in which there were large differences between the numbers of tickets for bad and good outcomes. Third, there was often explicit reference to decumulative reasoning (e.g., "at least an amount," "at least something"). Fourth, there was also often explicit reference to the likelihood of achieving particular target or aspiration levels (e.g., "able to count on getting a moderate amount," "an excellent chance of winning an amount between $80.00 and $119.00"). Aspiration levels were especially likely to be mentioned when one of the available choices provided a clear reference point such as a $100 sure thing or a $70 minimum.

In reading gain protocols such as these it is often difficult to discriminate between statements that signal decumulative weighting (i.e., focus on low outcomes and reasoning in inequalities) and statements that signal probability-based reasoning (focus on likelihoods of achieving particular targets). Indeed, although I often found myself struggling with this indeterminacy verbally when I was writing about subjects' choices, the theoretical indeterminacy was not apparent to me until Sandra Schneider and I (Schneider & Lopes, 1986) investigated the risk preferences of subjects who were preselected for extreme risk aversion. Our data indicated very clearly that there was an asymmetry between gain and loss preferences in which preferences for losses were neither identical to those for gains (as decumulative weighting suggests) nor mirror reflected (as suggested by the S-shaped utility function of prospect theory).

The reasons for the gain/loss asymmetry can be found in the subjects' protocols for loss pairs. Below are protocols from the four subjects we encountered above but this time choosing between versions of the rectangular and peaked lotteries in which the outcomes are all losses.

> S3 (chooses rectangular): This is a somewhat arbitrary choice. They're so similar, but I might as well make even my chances for high or low losses.
> S7: This was a difficult choice—I finally chose the [peaked] because it seemed "safer" than the other lottery. Since it seems likely I would lose some amount, I decided to opt for the lottery that favors a loss of less than $135.00. The other lottery is evenly weighted and somehow that seems "riskier" to me.
> S8: It is very hard to select one or the other. I selected the [rectangular] because it gives you more chances of paying zero or even under $50. I'm still unsure that I selected the right one.
> S11: I go back and forth on this, the gain on improving the chances on a low loss increases the chance of a higher loss. I pick the [peaked] to try to reduce the higher loss.

Several points of difference can be discerned between these protocols and the analogous gain protocols. First, choices for these pairs were not uniformly risk averse. Two subjects chose the peaked and two chose the rectangular. Over the entire group of 14 subjects, 13 chose the peaked for gains whereas only 6 chose it for losses. Second, some subjects focused on avoiding large losses (consistent with heavier weighting on the worst outcomes), others on achieving low or zero losses (consistent with maximizing the chances of achieving an aspiration level), and some (e.g., S11) focused on both. Third, subjects often expressed conflict and difficulty in choosing or lack of confidence in their choices for losses. Comments of this sort were extremely rare in gain protocols.

Without the data on loss choices, the indeterminacy between decumulative weighting and probability-based reasoning might be unresolvable because both classes of rule prescribe the same choices for the generically risk averse person. In the choice pair at hand, for example, security-minded decumulative weighting would amplify the differences at the low end (5 zero tickets versus 1 zero ticket) and consequently favor the peaked lottery. Similarly, a risk averse utility mass maximizer would set a low aspiration level and a risk averse quantile utility maximizer would choose a low quantile leading both to prefer the peaked to the rectangular (Manski, 1988). With the loss data, however, the indeterminacy disappears. The conflict that is expressed directly in protocols and indirectly in choices that veer unpredictably from risk averse to risk seeking strongly suggests that subjects attend to both criteria and find it difficult to choose between them.

Corroborating evidence concerning the asymmetry between gains and losses may be found in a variety of experiments. Parametric studies of reflection comparing people's preferences for gain gambles with preferences for analogous loss gambles have often found that gain preferences exhibit strong risk aversion whereas loss preferences are weaker (nearer to risk neutrality) or more variable (Cohen, Jaffray, & Said, 1987; Hershey & Schoemaker, 1980; Weber & Bottom, 1989, experiment 3). Parametric studies of framing have also found that preferences are predominately risk averse for problems described in terms of gains but variable across problems described in terms of losses (Fagley & Miller, 1987; Maule, 1989; Miller & Fagley, 1991; Schepanski & Kelsey, 1990; Schneider, 1992). In addition, Budescu and Weiss (1987) found that systematic cycles of intransitivity that sometimes occur for gains (Montgomery, 1977; Ranyard, 1982; Tversky, 1969) do not reflect for losses even when analysis is restricted to subjects whose preferences are reliably reflective.

My conclusion is that an adequate descriptive theory of risk taking will need to be a dual criterion theory. Elsewhere (Lopes, 1990; Lopes, 1995) I have proposed one such theory—called SP/A theory—that combines a decumulative weighting process (the SP or security-potential part of SP/A) with a process that maximizes the probability of achieving an aspiration level (the A or aspiration part of SP/A). Although a detailed exposition of SP/A theory falls outside the scope of what I want to accomplish here, suffice it to say that this particular dual criterion theory does a creditable job of describing both preferences and reasoning patterns across a wide variety of behavioral phenomena.

Description aside, however, dual criterion models are also normatively attractive for the many business and public policy decisions in which risk and return trade off. It often seems sensible in such contexts to construe return in terms of an expected outcome or average benefit and to construe risk in terms of the probability of an unacceptable outcome. For example, Petty and Scott (cited in Payne, Laughhunn, & Crum, 1980) surveyed the chief financial officers of Fortune 500 firms and found that the most common definition of investment risk was the probability of not achieving a target rate of return. Similarly, risks in medical, industrial, and environmental contexts are often assessed by measures such as death rates and accident rates.

Although it is rarely noted, a unifying theme of Allais' approach to risk taking is that prudent decision makers will trade off the expectation of monetary gains (or utility gains) against the probability of failing to achieve important goals. For example, Allais (1952/1979, p. 53) called on the concept of bold play in discussing a traveler who must get to Paris but finds himself stranded in Marseilles with only $10. He argued that for decision makers facing the immediate necessity of turning a small sum into a large sum, gambling at the best available odds may provide the only rational avenue to success even if the odds are objectively disadvantageous. On the other hand, he described his professional recommendations to a firm prospecting for oil in the Sahara by saying: "there is little point in entering a game with a positive expectation if there is from the outset a high chance of being forced out of it by ruin" (1952/1979, p. 506). Indeed, one may find an echo of this concern with the trade off between mathematical expectation and probability of success in his defense of the Allais paradox: "Just as it may be conceived that a very great effort is justified to perfect a whole, where only a detail remains to be remedied, . . . it may similarly be considered that *it is in no way irrational to accept a heavy reduction of possible gain as the price of achieving certainty, although this same reduction would not be felt acceptable for the same increase in the probability of gain if that increase is far removed from certainty*." (1952/1979, p. 102).

The canons of good science dictate that simple models are to be preferred to complex models, all else being equal. But all else is not equal if different models shine in different spheres. It is a false economy to choose between probability-based models and models based on weighted averaging when observation and good sense suggest that both apply. To borrow some sentiments that Manski expressed in defending his explorations of the utility mass and quantile utility models, "it seems quixotic to think that any model as simple as expected utility, quantile utility, or utility mass should be able to explain [the] diverse and complex aspects of behavior under uncertainty" (1988, p. 82). Likewise, it is simplistic to suppose that any one model is the royal road to rationality. Instead, each model "illuminates a possible mode of rational behavior under uncertainty" (Manski, 1988, p. 82).

RATIONALITY AND HUMAN JUDGMENT

The two classes of decision rule discussed in this essay conflict with the requirements of expected utility theory. One may ask, however, what warrant there is for concluding from this that the rules are irrational. If an Allais-type person conserves small probabilistic edges when security can be had but doesn't sweat the details otherwise, what is the harm? If Samuelson's colleague rejects his offer—or Scrooge McDuck's—by reasoning that neither is likely to leave him better off, what dire consequence will follow? Why must a scientist who prefers to evaluate data sets by medians rather than means be branded "irrational" if she applies the same policy in evaluating personal risks? Would any of these decision makers be better off practically if they gave in to fifty years of browbeating and entrusted their fates to expected utility?

According to Hammond (1994), most judgment research has espoused one of two complementary metatheories about rationality: "coherence" which stresses the internal consistency among a person's beliefs and preferences or "correspondence" which stresses the empirical accuracy of a person's judgments. Whereas coherence values reasons, correspondence values rightness. Thus, someone whose judgments flow logically from an empirically impoverished but internally consistent set of beliefs would be considered rational by the coherence standard no matter how poorly her judgments match reality. On the other hand, someone with a rich hodgepodge of beliefs, each individually chosen for usefulness in solving some particular problem, would be absolved by the correspondence standard for inconsistencies lurking in the mix.

As Hammond points out, research in the coherence framework tends to conclude that people are judgmentally incompetent without addressing the practical impact of the observed or inferred inconsistencies. One reason for this is that coherence is commonly portrayed as a precondition for correspondence. Dawes (1988), for example, describes the relation between consistency and correctness as follows: "By the logical *law of contradiction*, reasoning processes based on the same evidence that reach contradictory conclusions are irrational. And because reality cannot be characterized in contradictory ways, irrational thinking is erroneous thinking. A proposition about reality cannot be both true and false" (p. 9).

According to the coherence metatheory, people whose decision processes lead to contradictory conclusions live in peril of encountering a statistically astute scoundrel who will turn them into a money pump or trap them with a Dutch book. Kyburg (1978) has countered, however, that arguments of this sort rest on the (usually unstated) behavioral assumption that people feel compelled to act on their beliefs. That a person might maintain inconsistent views but refuse to act inconsistently is implicitly denied. My own sense is that money pump and Dutch book arguments are logical bogeymen. Permitting irrational behavior to occur in principle is not sufficient cause to invalidate a decision rule. Instead, it is necessary to demonstrate that the rule in question leads to observably irrational behavior.

Coherence theorists also tend to characterize reality discretely so that it will fit neatly into the absolute "trues" and "falses" of classical logic. Although some (like Dawes) are willing to acknowledge the appeal of logics that treat some propositions as "indecidable," few are comfortable with the idea that propositions can be true to varying degrees (Lukasiewwicz, 1930; Zadeh, 1965). Discrete logics have their problems, however, when they are applied to propositions that are not cut and dried. One such example is the "paradox of the heap" described by the Greek philosopher Eubulides. The paradox runs as follows: (1) One pebble does not make a heap; (2) Adding one pebble to something that is not a heap will not make it a heap; (3) Therefore, no number of pebbles makes a heap. If we take (1) and (2) to be true absolutely, (3) follows by mathematical induction. But (3) is clearly absurd.

As Oden (1988) pointed out, the proof Samuelson offered for the theorem concerning his colleague has the same form and is liable to the same problem.[4] Samuelson's logic ran as follows:

> If you will not accept one toss, you cannot accept two—since the latter could be thought of as consisting of the (unwise) decision to accept one plus the open decision to accept a second. Even if you were stuck with the first outcome, you would cut your further (utility) losses and refuse the terminal throw. (Samuelson, 1963, p. 4)

The proof is by mathematical induction:

R_1 :: you prefer not to take one bet.
and $R_1 \rightarrow R_2$ :: even if you have one bet, you prefer not to take a second.
and $R_2 \rightarrow R_3$ :: even if you have two bets, you prefer not to take a third. . . .
and $R_{n-1} \rightarrow R_n$ :: even if you have $n - 1$ bets, you prefer not to take one more.
$\therefore \forall\, n\, \mathrm{R}_n$ :: for any n you prefer not to take n such bets.

Although the proof form is logically sound when applied to premises that are absolutely true, it fails when the truth of one or more premises is not absolute. Thus, even if we take it as absolutely true that "one pebble does not make a heap," the truth of the premise that "adding one pebble to something that is not a heap will not make it a heap" cannot be absolute—for otherwise the induction would be good. Likewise, even if we stipu-

[4] Tversky and Bar-Hillel (1983) offer a variant on Samuelson's proof that is subject to the same critique.

late with Samuelson that his colleague rejects the single bet absolutely, we may side with Oden and Eubulides in supposing that the truth of Samuelson's second premise is a shade shy of one-point-oh.

Paradoxes such as Eubulides' heap, the St. Petersburg paradox, the Allais paradox and many others have played significant roles in challenging and enriching our understanding of formal reasoning and its application to the world. Advocates of expected utility theory tend to be impatient with paradoxes and are prone to accept "solutions" that fix the problem while saving the rule. It is my view, however, that paradoxes should not be fixed and forgotten as we might correct some careless bug in a computer program. Nor should they be dismissed as unrealistic and unimportant for the workaday world—if the inexhaustibly rich Scrooge McDuck is a fiction, so too is the robotically compliant money pump. Instead, paradoxes should be savored, debated, and recited to each new academic generation along with the theorems and axioms of normative theories. Although each paradox and axiom is attractive in its own way, none compels unthinking allegiance. Rather, each enriches our understanding of the evolving and imperfect constructions that humankind has created to describe and to aid decision making.

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