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Book Review

"Rational" Decision Making versus "Rational" Decision Modelling?[1]

MAURICE ALLAIS AND OLE HAGEN (Eds.). **Expected Utility Hypotheses and the Allais Paradox: Contemporary Discussions of Decisions under Uncertainty with Allais' Rejoinder.** (Theory and Decision Library, Vol. 21). Dordrecht: Reidel, 1979. pp. vii + 681 + indexes. $86.85.

Reviewed by MARK J. MACHINA[2]

Maurice Allais is Professor of Economic Analysis at the Centre d'Analyse Économique, École Nationale Supérieur des Mines de Paris, from which he received his Ph. D. in 1949. He has received numerous awards and honors, including Officier Légion d'honeur. He wrote the five-volume *Traité d'Économie Pure, Imprimerie Nationale*, 1952, and his research interests include the economics of uncertainty, theory of general economic equilibrium, and capital theory. Ole Hagen is an Associate Professor at the Norwegian School of Management, Oslo, having obtained degrees in Economics and Law from the University of Oslo. His research interests include economics of uncertainty and inventory control, one recent article being "Utility and Morality," in R. Henn and O. Moeschlin (Eds.), *Mathematical Economics and Game Theory, Essays in Honor of Oskar Morgenstern*, Springer-Verlag, 1977.

The reviewer, Mark Machina, received his Ph. D. from the Massachusetts Institute of Technology and is currently Assistant Professor of Economics at the University of California, San Diego, and (1981–1982) Visiting Assistant Professor of Economics at Princeton University. This review was written during a visiting appointment to the Department of Applied Economics, University of Cambridge. His primary research interest is decision making under uncertainty.

I. AN EXTRAORDINARY CONTROVERSY

It is hard to think of a more notorious, long-standing, and often outright confused controversy in modern decision theory than the continuing debate on the meaning of "rationality" in choice under uncertainty. Centered around the "expected utility" theory of risk taking first proposed over two centuries ago by mathematician Daniel Bernoulli, this debate has seen the dramatic reversal of Samuelson's and others' opinion of the theory from "logically arbitrary" to "logically compelling," repeated charges and countercharges of "nonscientific theories," "anti-scientific attitudes," and

[1] A list of references at the end of the review includes all the sources implicitly mentioned in the text.

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circular definitions of "rationality," the appearance of a major article in the debate with an editorial warning to the reader that it was being published "on the author's responsibility," and the reaction of Savage who, upon being shown that the preferences he expressed in a survey violated his own "rationality" postulate, concluded upon reflection that it was his preferences, and not the postulate, which were in error!

Although this debate has at one time or another engaged some of the most respected mathematicians/statisticians, psychologists, and economists of our time (de Finetti, Edwards, Friedman, Marschak, Morgenstern, Samuelson, Savage, Tversky, Wold,...), the individual most responsible for its origin and continuation over 30 years is the French economist Maurice Allais. Although the revival of expected utility theory in the forties due to its axiomatization by von Neumann and Morgenstern generated much controversy, much if not most of this reflected a confusion over the meaning of the conclusion of their (logically valid) argument—it was Allais who led the opposition to the *premises* of their theory of "rational" behavior under uncertainty. Similarly, it was Allais' famous "Paradox" which until recently continued to provide the major refuting evidence to the theory, and which is still discussed in textbooks on the subject, either as a classic example of scientific refutation or as an accidental and correctable example of "irrational behavior," depending on the particular author's outlook.

On the other hand, Allais, primarily through sins of omission, has done much to hinder the debate, especially in regard to the propagation of his own views. While it was he who initiated and organized the famous 1952 Paris conference on risk taking where seminal contributions of Arrow, Savage, and Samuelson were first presented, the proceedings of this conference were published only in French (and even then given limited distribution), and while many of the other participant's contributions (including the above three) eventually found their way into English, Allais' equally important criticism of the "neo-Bernoullian" or "American" school represented by the above authors was published in the United States in summary version only, again in French. Yet while the theoretical critique he presented in that summary received at least some distribution, the empirical results and analysis of his extensive survey on risk preferences, which were to provide the empirical support for his arguments and were due to be published "shortly" in 1952, (to my knowledge at least) have still not appeared.

The present volume thus constitutes a most welcome addition to the literature on risk taking and "rational" behavior under uncertainty. Its 681 pages contain (i) the first English translation of the full 1952 Allais "memoir" expounding his views and criticisms of the American school and his own alternative theory, (ii) current views (pro and con) on the debate by some of the original participants (de Finetti, Marschak, Morgenstern) as well as more recent entrants, and (iii) a lengthy statement by Allais of his current views, including a partial analysis of the results of his 1952 survey. While both mammoth and scholarly (Allais' contributions alone have 486 footnotes and cross-references), it provides a fascinating and well-balanced combination of mathematical analysis, philosophical discussion, new empirical evidence, personal reminiscences, and heated debate. I shall try to give some of its flavor by presenting a critical, but I hope fair, treatment of some of the key issues of the debate and the contributions of the present volume.

II. The Expected Utility Hypothesis and the Allais Critique

The controversy is best understood by beginning with the case of individual choice under certainty, where there is almost universal agreement on the meaning of "rationality." If A is a set of conceivable alternatives, say alternative bundles of n commodities so that $A \subseteq R^n$, then a (complete, reflexive) binary relation $\succcurlyeq$ on $A \times A$ (read "is at least as preferred as") is considered "rational" if and only if it is transitive. In such a case (and granted an additional technical assumption of a topological nature) it is always possible to mathematically summarize or "represent" $\succcurlyeq$ by a real valued "preference function" $V(x_1,..., x_n)$ on A, in the sense that $(x_1,..., x_n) \succcurlyeq (x'_1,..., x'_n)$ if and only if $V(x_1,..., x_n) \geqslant V(x'_1,..., x'_n)$, so that a rational individual may be modelled *as if* trying to maximize the value of $V(\cdot)$ over the currently attainable (e.g., affordable) subset of A. Besides (essentially) guaranteeing its existence, however, "rationality" places no further restrictions on the representation $V(\cdot)$: any further restrictions are either testable hypotheses on preferences which a rational individual may or may not satisfy, or else assumptions, such as differentiability, made for analytic convenience. It is not "irrational," for example, to hate asparagus.

Although weak, the assumption of rationality is clearly not without implications. An increase in disposable income, for example, can never make the individual worse off, but it is important to note that this is *not* because $V(\cdot)$ must be increasing in all or any of its arguments, merely that the maximum value of any function can never decrease as the attainable (i.e., affordable) set increases. It is also crucial for our purposes to note another aspect of preference functions. In the previous century, economists originally assigned a psychological reality to the function $V(\cdot)$, calling the units it was measured in "units of satisfaction" or "utils." Since "satisfaction," like temperature or location along a line, has no natural origin or unit of measure, $V(\cdot)$ was regarded as a "cardinal" function: by proper choice of origin and units we might equally legitimately represent the individual's satisfaction by any positive affine transformation $a + bV(\cdot)$ of $V(\cdot)$ $(b > 0)$, however, $V(\cdot)$ could not be subjected to nonlinear transformations without changing some "real" aspect of the individual's preferences. Economists have since come to realize that, as a representation, $V(\cdot)$ is actually "ordinal:" any monotonically increasing transformation $f(V(\cdot))$ will represent the same preference ranking $\succcurlyeq$ and hence the same choice behavior, and indeed, given the individual's awareness of $\succcurlyeq$, there is no need to assume that he or she consciously thinks in terms of any actual preference function at all.

It is possible, and would seem natural, to extend this approach to the case of choice under uncertainty, where the natural objects of choice are probability distributions or "lotteries" over outcomes. Consider the set P of all alternative lotteries over the set $\{\$i\}_{i=0}^{M}$, so that a typical element in P may be represented by $(p_0,..., p_M)$, where p_i is the probability assigned by the lottery to the outcome of winning $\$i$ (so P is the unit simplex in R^{M+1}). By analogy with the certainty case, "rationality" would appear to require only that the preference ranking $\succcurlyeq^*$ over P be transitive, and hence (given the above-mentioned topological assumption) representable by a preference function $V^*(p_0,..., p_M)$. Actually, since we are now working with money directly rather than with commodity bundles, we would also want to impose the "more money is better" implication of the previous paragraph and require that any shift of probability mass from an outcome $\$i$ to a higher outcome $\$j$ be preferred. This property, termed "monotonicity," is equivalent to $dV^*(p_0,..., p_M)/dp_i \leqslant dV^*(p_0,..., p_M)/dp_j$ whenever $i < j$, and is ordinal in that it is preserved under monotonically increasing transformations of $V^*(\cdot)$. Beyond this, however, rationality would again seem to warrant neither further restrictions on $V^*(\cdot)$ nor that the individual aware of his or her $\succcurlyeq^*$ ranking think in terms of any particular $V^*(\cdot)$ at all.

What then is the "expected utility hypothesis"? It is that $\succcurlyeq^*$ be representable by a preference function which is linear, and hence of the form $V^*(p_0,..., p_M) \equiv \sum u_i p_i$ for some fixed set of coefficients $\{u_i\}_{i=0}^{M}$. The phrase "expected utility" comes from the fact that if we call the coefficient u_i of p_i the "utility" of receiving the outcome $\$i$, then $V^*(\cdot)$ consists of the mathematical expectation of "utility" implied by the lottery $(p_0,..., p_M)$. A useful account of the expected utility and related models is given in the present volume by Günter Menges.

Now while linearity is typically a useful first approximation to any function, economists and statisticians were at first hard put to see why a rational individual must *necessarily* have a linear $V^*(\cdot)$ (see the elegant statement of the "pre-1950 Samuelson" in this regard). Yet today most professionals indeed *do* view linearity as a *sine qua non* of rational behavior toward risk, so much so that Pratt could with full conviction write "I am all in favor of any argument which will convince anyone not already convinced that maximizing expected utility is the only behavior worth rational consideration."

Although it is possible with strong enough additional assumptions to interpret expected utility maximization as a "rule of long run success" (a derivation is given in the present volume by A. Camacho), the primary reason for this direct about face on what decision modellers viewed as "rational" behavior was the discovery by Ramsey, von Neumann and Morgenstern, Marschak, Rubin, Samuelson, Savage, and others that "linearity in the probabilities" was equivalent to what has been termed the "strong independence axiom." One of several equivalent statements of this axioms reads "a lottery $(p_0,..., p_M)$ will be preferred to $(p'_0,..., p'_M)$ if and only if the lottery $\lambda(p_0,..., p_M) + (1-\lambda)(p''_0,..., p''_M)$ is preferred to $\lambda(p'_0,..., p'_M) + (1-\lambda)(p''_0,..., p''_M)$, for all $\lambda \in (0, 1)$ and $(p_0,..., p_M)$, $(p'_0,..., p'_M)$, and $(p''_0,..., p''_M)$ in P." The argument for the "rationality" of this prescription is straightforward: the choice among the latter pair of prospects is equivalent in terms of final probabilities to being presented with a coin which has a $(1-\lambda)$ chance of landing tails (in which case you will "win" the lottery $(p''_0,..., p''_M)$) and being asked *before the flip* whether you would prefer to win the lottery $(p_0, \ldots, p_M)$ or $(p'_0, \ldots, p'_M)$ in the event of a head. Now, either the coin will land tails, in which case your choice won't have mattered, or else it will land heads, in which case you are in effect back to a decision between $(p_0, \ldots, p_M)$ and $(p'_0, \ldots, p'_M)$ and you should clearly make the same choice as you did before. There is no doubt that the principle enunciated here is compelling. Indeed, Friedman and Savage felt that "the Greeks must surely have had a name for it" and Marschak (present volume) comes close to suggesting that it be taught in curricula along with the principles of arithmetic and logic.

Besides the discovery and interpretation of the independence axiom, one other factor had a bearing on the eventual acceptance of the theory. Recall that the preference relation $\succcurlyeq^*$ of an expected utility maximizer can be represented by the set of linear coefficients $\{u_i\}$. In this case, it is straightforward to show that any positive affine transformation $\{a + bu_i\}$ $(b > 0)$ will represent the same preference relation, but that no nonlinear transformation of the u_i's will. Thus the economists of the late forties and early fifties, who had none too recently cast off the mistaken notion of cardinality in the certainty case, were once again asked to believe in it as part of a new and self-proclaimed "rational" theory of choice under uncertainty! This quite understandably caused some confusion and resistance until it became generally understood that the objects of choice were not outcomes but rather lotteries over outcomes and (thus) that even linear preference functions over P could be subjected to nonlinear (but monotonic) transformations of the form $f(\sum u_i p_i)$ without changing preferences, and that in any event the independence axiom was defined directly on the ranking $\succcurlyeq^*$ so that there was still no need to posit the actual psychological reality of any $V^*(\cdot)$ function, much less the set of coefficients $\{u_i\}$. The theory in its final form thus consisted of the beliefs that (i) satisfying the independence axiom is a necessary condition for rationality and the preferences of such a rational individual could be represented by the expectation of a cardinal "utility index" $\{u_i\}$, but that (ii) there was no reason to assume that any cardinal index actually exists in the mind of the individual. Imagine then the profession's reaction upon being told by Allais that *both* these views were wrong!

Allais' views are complex and multi-faceted, and the otherwise well-read reader will be amazed to see how much of the subsequent debate, as well as the theory of behavior toward risk in general, is anticipated in his 1952 memoir. His main points, however, are:

(i) the actual psychological reality of a cardinal index $\{s_i\}$ (distinguish from $\{u_i\}$) giving the "psychological values" of the outcomes $\{\$i\}$ (and more generally, of nonmonetary outcomes as well), and which "can be defined operationally by considering either psychologically equivalent variations... or minimum perceptible thresholds (Weber–Fechner),"

(ii) in choosing among actual lotteries, individuals take into account not only the expectation of psychological value implied by each lottery, but the variance and possibly higher moments of s_i as well, so that while the u_i's of an expected utility maximizer may be inferred directly from choice over lotteries, the s_i's of an "Allais-type" individual in general cannot be, and

(iii) it is *perfectly rational* for individuals to take into account more than just the mean of the s_i's, and indeed, except for monotonicity (which he calls "absolute preference") and transitivity, "rationality" as such imposes no restrictions whatsoever on preferences over lotteries.

Many of the differences, and most of the misunderstanding, between Allais and his critics stem from this fundamental difference over whether preference rankings or psychological values are the underlying "real" generators of choice, and some of the deeper philosophical and linguistic aspects of this difference of approach are discussed in the present volume by Werner Leinfellner and Edward Booth. One particularly long-lived and well-known argument has concerned whether an aversion to risk (which both sides agree is consistent with rationality) may result in a "rational" individual choosing to violate linearity in the probabilities. To the Americans, an individual is "risk averse" if he or she always prefers receiving the expected monetary value of any lottery to the lottery itself. This condition on $\succcurlyeq^*$ is completely consistent with the independence axiom and (given the latter) is equivalent to the condition that the utility indices derived from $\succcurlyeq^*$ form an increasing concave sequence (i.e., $0 \leqslant (u_{i+1} - u_i) \leqslant (u_i - u_{i-1})$ for all i). Thus to the Americans risk aversion is completely compatible with, and requires no deviation from, linearity—the individual's aversion to risk is completely captured by the shape of the $\{u_i\}$ index. To Allais the $\{s_i\}$ index exists independently of and logically prior to risk preferences, and since it is the s_i's rather than the actual monetary values \$$i$ which measure the true psychological benefit of the outcomes, a risk averter would naturally choose to take into account the dispersion as well as the mean of the s_i's in ranking lotteries. Thus the $\{s_i\}$ index reflects nothing about attitudes toward risk, and it is perfectly rational for an individual to want to maximize something other than the linear form $\sum s_i p_i$. Furthermore, since Allais argues that no (Allais-type) individual can satisfy the independence axiom *except* by maximizing $\sum s_i p_i$, it follows that rational individuals may, for reasons of risk aversion, choose to depart from expected utility maximization.

However, since introspection reveals that I would choose among alternative lotteries by conjuring up neither a well-defined preference ranking *nor* a cardinal index of psychological value, I turn from the above argument to those aspects of the debate which are of practical importance to (i) the decision modeller, who as descriptive scientist is concerned only with the differing observable implications of the two models and the available evidence, and (ii) the decision maker, who would like to be thought of as "rational," but who wants to know if it is okay, in the words of the pre-1950 Samuelson, to "satisfy his [or her] preferences and let the axioms satisfy themselves."

III. Implications for Decision Modellers

Allais has sought to operationalize his notion of psychological value by means of either minimum perceptible thresholds or "psychologically equivalent variations" in wealth. On the former approach, it would seem that the minimum perceptible difference between two sums of money would depend on whether they were presented as two piles of bills and coins or two account balances. In the nonmonetary case (say slices of pie) it would seem that the same fuzziness of perception that resulted in imperceptible differences would also render these "minimum thresholds" themselves too fuzzy and unstable to be of use in deriving his index. In a contribution to the present volume, Peter Fishburn has shown that much of expected utility theory may be derived in a model that allows for fuzziness of perception (e.g., intransitive indifference) and it may similarly be possible for Allais to do the same with his theory.

Allais' other method of deriving the cardinal $\{s_i\}$ index, which he actually uses in his 1952 survey, consists of direct questions of the form "for what value of i is your intensity of preference for \$$i$ over \$100 the same as your intensity of preference for \$100 over \$50?" Personally, I would respond to this question by asking what it meant. Would I rather obtain \$100 after having hoped for \$$i$ or obtain \$50 after having hoped for \$100? Surely the former—\$100 is better than \$50 regardless of i. Would I prefer \$100 to an even chance of \$$i$ or \$50? Since an Allais-type individual would consider the variance of psychological value and not just the mean in this situation, this also will not work.

Yet Allais' subjects did provide answers to such questions, and while these questions do not seem to correspond to any actual or hypothetical choice behavior, they nevertheless do elicit verbal (or written) behavior. Nor is this theory of psychological value irrefutable: if I really had such an $\{s_i\}$ index this would place restrictions on my answers to such questions. Although Allais does not seem to have tested these restrictions directly, he concluded that subjects responded "consistently" to these questions, revealing $\{s_i\}$ indices which were approximately log-linear over large ranges of outcome values.

However, Allais' theory of psychological value is only a theory of risk taking to the extent that the $\{s_i\}$ index is specifically linked to a preference ranking $\succcurlyeq^*$ or a preference function $V^*(\cdot)$, and the exact nature of this link has caused some confusion. One the one hand de Finetti (present volume) repeats the earlier argument that nothing besides the expectation of the index ought matter, since risk preferences are already captured in the shape of the index. This is true for the u_i's of an expected utility maximizer, which are derived from $\succcurlyeq^*$, but not for Allais' s_i's which are derived from verbal behavior in a riskless context. On the other hand, Allais' assertion that any individual satisfying the independence axiom must exhibit $\{u_i\} = \{a + bs_i\}$ $(b > 0)$ also deserves careful scrutiny: in response to de Finetti's 1952 counterexample $V^*(\cdot) \equiv \sum f(s_i) p_i$, Allais has reproven the result via the addition of an additional "axiom of isovariation." The amazing strength of this result, linking non-risk-related survey behavior to preferences over lotteries, leads one to wonder whether "isovariation" might not be a lot stronger than it first appears.

Yet while the link between the survey behavior and risk preferences remains to be fully explored, I feel that the more fruitful approach to the psychology of risk would be to concentrate on the nature of $\succcurlyeq^*$ or $V^*(\cdot)$ directly. Over the years Allais has been charged with providing little in this regard, and hence of fostering an "unscientific" theory (this was Friedman's view in 1950 and is repeated by Yakov Amihud (present volume)). It is true that Allais offers many "psychological factors of choices involving risk" without always suggesting how strongly, and in which direction, he expects them to operate. Yet he does offer at least some well-defined refutable hypotheses (among them absolute preference), and his primary empirical assertion, namely that rational agents will not always choose according to the independence axiom even after it has been explained to them, is clearly scientifically legitimate (not to mention important, if verified).

Since the present volume still only offers "selected findings" of Allais' extensive 1952 survey, his primary (though not sole) empirical contribution to the debate remains his well-known counterexample to the independence axiom, the so-called "Allais Paradox." Before proceeding, the reader may wish to note his or her preferences over:

$$a_1: \{100\% \text{ chance of } \$1{,}000{,}000 \qquad \text{versus} \qquad a_2: \begin{cases} 10\% \text{ chance of } \$5{,}000{,}000 \\ 89\% \text{ chance of } \$1{,}000{,}000 \\ 1\% \text{ chance of } \$ \qquad\qquad 0, \end{cases}$$

and

$$a_3: \begin{cases} 10\% \text{ chance of } \$5{,}000{,}000 \\ 90\% \text{ chance of } \$ \qquad\qquad 0 \end{cases} \qquad \text{versus} \qquad a_4: \begin{cases} 11\% \text{ chance of } \$1{,}000{,}000 \\ 89\% \text{ chance of } \$ \qquad\qquad 0. \end{cases}$$

Allais and several researchers since him have found that the modal (if not majority) choice of subjects has been for a_1 over a_2 and a_3 over a_4, which can be shown to violate the independence axiom (i.e., there is no set $\{u_i\}$ of utilities which can generate these choices). This was one of the examples with which Allais "tricked" Savage (who initially chose a_1 and a_3) and similar examples were offered in the early fifties by Allais and George Morlat.

The main objections to the Allais Paradox as "evidence" have been (i) that individuals would always, like Savage, change their preferences upon being shown how they violate the axiom, and (ii) that the example in question is an isolated case, and examples involving less extreme payoffs and probabilities would result in less if any violations of expected utility. On the first point, although experimenters (especially ones who believe in the rationality of the axiom themselves) typically are able to talk subjects out of violations, Slovic and Tversky as well as MacCrimmon have found that when subjects were presented with written arguments for *and against* conforming with the axiom, there is an about equal propensity for preferences to change in either direction.

On the second point, recent experiments have shown that, not only are such Allais-type violations replicable with less extreme payoffs and probabilities, but that the nature and direction of such departures from linearity are both systematic and predictable. The current volume contains two major studies of this type. Coeditor Ole Hagen extends and formalizes many of Allais' ideas, offering well-defined refutable hypotheses on risk preferences as well as theoretical implications and empirical tests of them. In a characteristically careful and illuminating piece, Kenneth MacCrimmon (with coauthor Stig Larsson) provides an exhaustive compendium of the various alternative axiomatizations of expected utility as well as alternative decision rules, and presents evidence both new and old on the different types of violations of expected utility and the dependence of these violation propensities on the parameter values (probabilities and payoffs) involved. The outcome of these and other studies suggests that the two most systematic types of violations are (i) that, relative to linearity, individuals are more sensitive to (i.e., proportionately overweight) the probability of the most extreme outcome when this probability is small than when it is large (called the "common ratio effect"), and (ii) that the nature of the "common consequence" on the tail side of the earlier coin example *does* influence individuals' choices over which lottery they would prefer in the event of a head, with a more preferred common consequence leading to a more risk averse choice (the "common consequence effect"). The conclusion is clear: preferences systematically depart from linearity, and if the proportion of the above types of violations steadily drops as less extreme probabilities and payoffs are used, this simply reflects the fact that linear functions provide better approximations to nonlinear ones "in the small" than they do "in the large."

Elsewhere I have extended this last idea to show how much of what has been termed "expected utility" analysis in fact does not require the independence axiom (i.e., linearity) at all. Recall the earlier result that risk aversion is equivalent to the sequence $\{u_i\}$ of utilities ("linear coefficients") being concave. Taking an arbitrary *non*linear $V^*(\cdot)$ and defining the cardinal sequence $\{dV^*(p_0,..., p_M)/dp_i\}$ (i.e., the "local linear coefficients") as the "local utilities" at the point $(p_0,..., p_M)$ in P, it may be shown that $V^*(\cdot)$ is made worse off by all mean preserving increases in risk if and only if the *local* utilities form concave sequences at all points in P. Similar generalizations of "expected utility" results may be obtained, as well as a simple condition on the functions $\{dV^*(\cdot)/dp_i\}$ which generates both the "common ratio" and "common consequence" effects.

The implications for decision modellers? To Allais, whose 1979 contribution consisted primarily of a restatement of his 1952 views, I would say "Your disciples and intellectual descendents have gone beyond criticizing the "rationality" of expected utility and are now formalizing your thoughts into well-defined and testable alternative models. Join them!" To the neo-Bernoullians: "The evidence against the independence axiom is mounting. You have always admitted, when pressed, that expected utility was a prescriptive and not a descriptive theory. Take that admission seriously and join in this search for better predictive models."

IV. Implications for Decision Makers

Having already presented the main argument in favor of the rationality of the independence axiom, I offer here what I feel to be the strongest counterargument, drawing on the arguments of Allais as well as comments by Tversky, Dreze, and Samuelson (the latter two, however, do not necessarily disagree with its rationality).

Consider the decision problem in Table 1, where the (mutually exclusive) outcomes α, β, γ, and δ denote *completely specified consequences*, that is, exhaustive descriptions of every observable aspect of the world which would be attained under the given outcome. In particular, we assume that each of the descriptions α, β, γ, and δ pertain solely to what *will* be true if they occur, and not to aspects of any of the other infinite number of conceivable states of the world (that is, to what might *otherwise* have happened). In this case the prescription of the independence axiom is clear: since the possible outcome γ is common to actions b_1 and b_2, and δ is common to actions b_3 and b_4, I should choose over $\{b_1, b_2\}$ and $\{b_3, b_4\}$ solely on the basis of α and β, so that I ought to prefer b_1 to b_2 if and only if I prefer b_3 to b_4.

Now assume these four alternative consequences are identical in all respects except for the following: in each, your best friend has been in the hospital for an operation, and in

α: comes out with a permanent limp, and receives flowers and a sympathy card from you,

β: comes out with a permanent limp, and receives champagne and a box of cigars from you,

γ: comes out in perfect health (no limp), and

δ: dies during the operation.

If I were in a position where I had to choose *ex ante* between b_1 and b_2, I would choose b_1 since in this case the 0.05 probability event would, relative to what I had reasonable cause to hope for, be a most unfortunate and unhappy outcome. On the other hand, if my choices were between b_3 and b_4 I would certainly specify champagne and cigars in the unlikely and near miraculous event that my friend did not die. Furthermore, I do not feel that these choices would be "irrational."

TABLE 1

	Outcome	
Action	0.05 Probability	0.95 Probability
b_1	α	γ
b_2	β	γ
b_3	α	δ
b_4	β	δ

I have constructed this particular example to highlight what I feel to be the key objection to the independence axiom: namely, that my attitudes toward ("utility of") a particular outcome need not be independent of what might otherwise be expected to happen and how likely these other possibilities are. Indeed, even if I were allowed to make my choice after learning the outcome of the operation, my decision between α and β might "rationally" depend on whether I had expected γ or δ to have almost certainly happened instead, and if such "complementarity" across mutually exclusive outcomes is legitimate *ex post*, it could hardly be irrational *ex ante*.

One objection to this example is that it does not really violate the axiom because α and β are not really the "same" outcomes in the $\{b_1, b_2\}$ decision as in the $\{b_3, b_4\}$ decision: the complete description of a consequence must include not only its physically observable aspects but also my "state of mind" if it were to occur, and clearly my state of mind in both α and β would depend on whether I had been expecting γ or expecting δ. Arguing this, however, is to defend the axiom by rendering it observationally irrefutable: it allows me to defend *any* pair of choices in situations like the above table, the coin example, or the Allais Paradox.

A more useful objection might be that "rationality" would at least require that the axiom be satisfied in the "ethically neutral" case where the outcomes are purely monetary payoffs. However, this seems to fly in the face of the economist's typical view that money is only valued for the nonmonetary outcomes it affords us. In any event, even if the outcomes were purely monetary, say α a lottery ticket and β, γ, and δ sure payments with γ very large and δ very small, my preferences for bearing further risk (i.e., α versus β) may well depend, ex ante or ex post, on whether the alternative outcome would be (or would have been) γ, in which case not getting γ would be a disappointment and I might be inclined not to gamble further, or whether it would be (would have been) δ, in which case I would consider myself lucky if I don't get it, and possibly feel willing to bear the uncertainty of an addition bet (α). It is important to note that it is not my estimation of the respective probabilities of the gamble α which are affected here, merely my willingness to bear them.

The argument over the rationality of the independence axiom may well go on forever. The implications for decision makers? "Make sure you understand the argument for the independence axiom, and the usefulness of structuring decisions to highlight common consequences as in Table 1. But if you truly feel that your enjoyment of outcomes such as α or β will depend on what might have otherwise happened (or more to the point, what might still otherwise happen), then don't let anyone who doesn't happen to share these preferences convince you that you're 'irrational.'"

V. An Important Volume

Besides those mentioned above, the contributors to this collection include Oskar Morgenstern on the need to continually be looking beyond our scientific theories (of risk taking or anything else) toward richer and more complete descriptions of reality, Karl Borch on the usefulness of the "stochastic dominance" and related criteria in ordering uncertain prospects, Richard Cyert and Morris DeGroot on adaptive behavior when the individual cannot completely determine the "utility" of an outcome without experiencing it, and Samuel Gorovitz on the neglect of very low probability events and the St. Petersburg Paradox. Each of these papers make original, if more specialized, contributions to the field of decision making under uncertainty.

As one of the many who were looking forward to this volume, I have only two regrets. The first is that it didn't include contributions by some of the other original participants in the debate (Kenneth Arrow, Milton Friedman, Paul Samuelson) or some of the psychologists who have made important theoretical and empirical contributions to the field (Ward Edwards, Amos Tversky). The second was that Allais' 1979 contribution consisted more of a repetition of his 1952 memoir than of new views and ideas or a detailed analysis of his 1952 survey. Nevertheless, this volume will someday be an important source for the historian of thought, though since the controversy is if anything heating up again, this will not be for some time yet.

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