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Expected Utility Hypotheses and the Allais Paradox.

Maurice Allais and Ole Hagen (eds.). Dordrecht, Holland: D. Reidel Publishing, 1979. vii + 714 pp. $86.85.

The early 1950's was a time of great excitement for economists, psychologists, and statisticians interested in the idea of numerical utility. Von Neumann and Morgenstern had only recently, in 1944, published their axiomatization of expected utility. L.J. Savage was at work on their suggestion that utility and subjective probability be jointly axiomatized at a similar level of rigor. The expected utility model seemed to offer exciting possibilities as a descriptive model in psychology, as a foundation for microeconomics, and as a basis for the theory of statistical inference.

For economists, the most important fact about von Neumann and Morgenstern's work was that it revived the idea of cardinal as opposed to mere ordinal utility. The 19th-century marginal utility theorists had talked about utility as if it could be measured on a cardinal scale—that is, as if utility differences could be compared. And the 19th-century German psychologist Fechner had claimed to find experimental bases for defining cardinal utility. But by the early 20th century, cardinal utility had been generally abandoned. Psychologists had become skeptical of Fechner's claims, and economists, under the prodding of Pareto, had recognized that the utilities of the marginal theory had only ordinal meaning. Von Neumann and Morgenstern's utilities, since they were derived in a probabilistic context and scaled by probabilities, did have a cardinal meaning.

In 1952, the French economist Maurice Allais mounted a direct challenge to the new cardinal utility. The axioms of the new theories, Allais declared, were wrong. They were wrong descriptively; people did not obey them. And they were wrong normatively; it was rational to reject them. Allais did believe in cardinal utility; he believed, like Fechner, that it could be defined operationally by direct comparison of differences in utility or scaled by taking minimum perceptible changes as unit changes. But since he rejected von Neumann and Morgenstern's axioms, he saw no reason why a person should rank probability distributions of utilities by expected utility alone. A person might want to consider the shapes of the distributions, perhaps by considering their variances and higher moments. Using various assumptions about how people might rank distributions of utilities, Allais developed his own theory of the economic phenomena associated with risk.

The most effective part of Allais's attack was his empirical demonstration that the tendency to prefer sure gains leads people to express preferences between gambles that are incompatible with expected utility. The idea, roughly, is that people will prefer a 100% chance of winning a large prize to a 99% chance of winning a much larger prize, but will not agree to nearly so great a reduction of the prize in exchange for a single percentage point of probability when that percentage point does not bring them to certainty. This phenomenon has come to be known as "Allais's paradox." For details, see the volume under review, Savage (1954, pp. 101–103), or Raiffa (1968, pp. 80–86).

Though Allais's paradox drew much attention, it did not have the effect that Allais had hoped. During a lunchtime conversation at the 1952 conference where he first presented his ideas, Allais showed his paradox to Savage, and Savage was startled to find himself expressing preferences violating his own theory. But after reflection Savage decided to change his preferences instead of his theory. Expected utility remained, Savage felt, normative. Other Bayesian statisticians have reacted similarly. Economists, who have a greater stake in the descriptive validity of expected utility, have also minimized the importance of Allais's examples, usually by arguing that they involve unrealistically large prizes.

Economists almost completely ignored Allais's own theory. (Some economists, such as Tobin (1958), have considered variances in ranking probability distributions, but the emphasis here has been on ranking distributions of money values, with hope of approximate agreement with ranking by expected utility.) For most economists, Allais's founding of cardinal utility on 19th-century psychology was unacceptable.

The volume under review represents an attempt to reopen the question. It reflects a feeling on the part of Allais and his coeditor, Ole Hagen, that the neglect of Allais's work is due in part to its comparative inaccessibility to those who do not read French. This is remedied here by the publication of 380 pages by Allais: a 120-page translation of his 1953 French monograph (only a 40-page version was published in English at the time) and, further, a 260-page review of the issues. Hagen contributes 30 pages on a theory similar to Allais's. About 40 pages are taken up by critics of Allais, and the remaining 200 pages are devoted to related issues. Altogether 17 authors are represented.

On the whole, the volume is difficult and not as rewarding as the reader might hope. Allais's contributions are well written but far too lengthy, and the quality of the other contributions is uneven. But there are several contributions that do deserve to be flagged:

- Oskar Morgenstern's brief defense of the von Neumann-Morgenstern theory is of historical interest if only because it shows how little objections to the theory impressed him. (He died in 1977.) He defends the descriptive value of the theory, conceding only that it, like Newton's theory, is only approximate and applies only to a limited domain. And he defends its normative status, asserting that people will correct their deviations from it just as they correct mistakes in long division.
- Cyert and DeGroot, in "Adaptive Utility," explore the idea that a person might be unsure about his utilities and use Bayesian methods to learn about them. Since Allais's paradox is sometimes presented as an inconsistency between choices at different times (with both prizes uncertain I will gamble on the larger, but I will choose differently once a favorable event assures me certain success if I opt for the smaller), this might offer a way to accommodate the paradox within expected utility theory. Ironically, though, it is easier to make sense of the idea of unknown utility if we define utility hedonistically, as Allais does, than if we derive it from an extensive set of preferences, as the expected utility theory does.
- Peter Fishburn's elegant contribution is one of his many recent studies of the consequences of relaxing various of the axioms of expected utility theory.
- MacCrimmon and Larson report empirical studies showing deviations from expected utility that go well beyond Allais's paradox. Some of their examples are similar to examples reported by Kahneman and Tversky (1979).

How should we evaluate today Allais's criticism of expected utility theory? To evaluate it adequately we must, I think, consider the full force of the results of recent investigations of the sensitivity of people's choices to context and presentation. Some of these results are reported in Wallsten (1980), Tversky and Kahneman (1981), and Hogarth (1982).

It turns out that choices can be presented in ways that will induce people to violate not only the axioms to which Allais objects but also the axioms to which Allais subscribes, such as the principle of stochastic dominance. In fact, seemingly innocuous changes in wording that do not even objectively change the options in a problem can systematically reverse the choices people make. Tversky and Kahneman (1981, p. 453) give an illuminating example. After describing a public health problem that is expected to cause 600 deaths if unchecked, people are asked to choose between different preventive programs on the basis of their probable consequences. People's choices, it turns out, depend on whether the consequences are described in terms of total deaths or in terms of the number of the 600 saved.

The commonsensical conclusion is that people have neither the extensive preferences required by von Neumann and Morgenstern's theory nor the psychological utilities required by Allais's theory. When offered a choice between gambles they do not query themselves about such preferences or utilities. Instead, they cast about for any argument or reason for a choice, often grasping at any ideas that may be suggested by the way the problem is posed.

Once this picture of the starting point is accepted, it becomes clear that decision is radically constructive. The problem is not to take an extensive set of preferences and correct a few inconsistencies, as Savage's language often suggested (see, e.g., Savage 1971). Rather, it is a matter of building up preferences, utility functions, or rankings of goals from much more limited beginnings.

Does the theory of expected utility have a uniquely "normative" claim to be our guide in this radically constructive process? Once the question is asked in this way, it becomes obvious that the answer is no. If the whole framework of preferences is lacking, the "coherence" arguments cannot be deployed to force a few errant ones into line.

Because of the new psychological work, the discussion of decision by statisticians and decision analysts will, I believe, change dramatically over the next decades. There will be much less talk about norms and coherence, much more flexibility in the choice of paradigms and languages used to formulate and resolve problems of choice.

The prospects for change within microeconomics are less bright. A few economists (e.g., Thaler 1980) have challenged their colleagues to face up to the facts the psychologists have described. But the myth of the rational man—the pretense that ordinary people have determinate preferences that rationally govern all their economic choices—is a fundamental element of neoclassical microeconomics. And it is the linchpin of the economists' claim to give value-neutral but relevant advice concerning the welfare implications of governmental decisions. The psychologists, like Allais before them, may have to continue to marvel at the ability of economists to ignore empirical facts about people's behavior.

Glenn R. Shafer
University of Kansas

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The Mathematical Experience.

Philip J. Davis and Reuben Hersh. Boston: Houghton Mifflin, 1982. xix + 440 pp. $9.95 (paperback).

In the preface, Philip Davis and Reuben Hersh state that their purpose in writing *The Mathematical Experience* is "to explain to nonprofessionals just what these people (professional mathematicians) are doing, what they say they are doing, and why the rest of the world should support them at it." It would seem that this task should be as difficult as explaining color to someone who has been blind from birth, and, as a professional mathematician, the reviewer is unable to assess how well the authors accomplish it. The *New York Times* (7 September 1981) seems to think they are quite successful.

The book consists of a collection of essays that to a large extent can be read independently of each other. Some are directly descriptive. To indicate the breadth of mathematics, for example, the authors first estimate that the average Ph.D. candidate reads approximately 60 books during his education, and they then continue:

> Thus we can think of our 60,000 books as an ocean of knowledge, with an average depth of sixty or seventy books. At different locations within this ocean—that is, at different subspecialties within mathematics—we can take a depth sample . . . that would represent the basic education of a specialist in that area. Dividing 60,000 books by sixty, we find there should be at least 1,000 distinct subspecialties. But this is an underestimate. . . . The fine structure would show mathematical writing broken down into more than 3,000 categories.
>
> In most of these 3,000 categories, new mathematics is being created at a constantly increasing rate. The ocean is expanding, both in depth and in breadth.

There are other essays that discuss the current issues of the mathematics profession. The reviewer learned here, for example, that federal funding agencies make no attempt to evaluate one area of mathematics relative to another, but rather fund each area according to the number of favorably refereed proposals. The authors discuss the perennial issue of usefulness as a justification of mathematical research. The opposing philosophies are referred to as Hardyism versus Mathematical Maoism!

There are also historical tidbits. A photograph of a clay tablet originating in southern Iraq in 1700 B.C. together with a translation reveal that the Babylonians were able to find x and y given $x+y$ and xy essentially by using the quadratic equation.

But some of the most valuable essays in this book are those that attempt to describe mathematics by explaining a branch of mathematics to the nonmathematician. There is, for example, an extremely illuminating example of what the authors call the "drive to generality and abstraction," given through a tracing of the evolution of the Chinese remainder theorem. To check for the obvious pitfalls of any such summary, the reviewer chose to investigate one of them. In a section called "The Coin of Tyche" the authors discuss the infinitely repeated fair coin toss:

> We should like a fair coin to have the *H*'s and *T*'s distributed, in the limit, in a 50%–50% fashion. But we should like more. We should like that if we look at the results of two successive tosses, then each of the four possible outcomes *HH, HT, TH, TT* occurs, in the limit, with probability 1/4. We should like, shouldn't we, something similar to occur for the eight possible outcomes of three tosses:
>
> *HHH, HHT, HTH, HTT, THH, THT, TTH, TTT*
>
> and so on for every conceivable consecutive length of toss. Such a sequence is called ∞-distributed.
>
> An infinite sequence $x_1, x_2, \ldots$ is called random in the sense of von Mises if every infinite sequence $x_{n_1}, x_{n_2}, x_{n_3}, \ldots$ extracted from it and determined by a policy or a rule R is ∞-distributed. Now comes the shocker. It has been established by Joseph Doob that there are no sequences that are random in the sense of von Mises. The requirement is logically self-contradictory.

The rule R referred to in this passage is a sequence of 0,1 valued functions $f_n(x_1, \ldots, x_{n-1})$ with the interpretation that x_n is selected for the subsequence exactly when $f_n = 1$. Given a specific sequence $y_1, y_2, \ldots$ (after recoding the y_i's to 0 and 1) we can define the rule $f_n(x_1, \ldots, x_{n-1}) = y_n$; when applied to the sequence $y_1, y_2, \ldots$, R selects only ones. This is the contradiction to the existence of random sequences.

The authors are unfair to von Mises to believe that he would be hoodwinked for several decades by such a patent trick. The problem is that when von Mises formulated this definition in 1919 there did not exist in mathematical logic an adequate understanding of what type of rules are effectively calculable (say by a machine). If the collection of possible rules is suitably restricted to calculable ones, then in fact almost every (in the sense of measure theory) sequence will be random in the sense of von Mises (see Billingsley 1979, p. 91 and p. 484 or Martin-Loff 1969). It is also true, however, that there are examples of sequences that are random in the sense of von Mises but that exhibit more subtle types of nonrandom behavior (see Martin-Loff 1969).

Von Mises called his random infinite sequences "collectives" and tried to develop laws of probability based upon the behavior of a single collective (or at most a few of them) under various types of transformations. For him, the law of large numbers and the futility of betting selection systems were properties built into the definition of a collective. This is in contrast to the current approach of developing probability based upon a measure on the space of all possible infinite sequences; the law of large numbers and the futility of betting selection systems become theorems. Von Mises and Doob advocated these different approaches before the Institute of Mathematical Statistics in 1940; the text of their talks, together with a discussion, appear in von Mises (1941). In his discussion, Doob acknowledges von Mises's role in stressing the importance of these two theorems to any adequate theory of probability. The reviewer feels that Davis and Hersh's account of the evolution of the analysis of the fair coin toss should have given a less trivial discussion of the existence of random sequences (collectives) and would have been greatly enhanced if it had been completed to include the more modern approaches.

Perhaps these lapses are unrepresentative of the quality of the math-