THE THEORY OF DECISION MAKING[1]

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Many social scientists other than psychologists try to account for the behavior of individuals. Economists and a few psychologists have produced a large body of theory and a few experiments that deal with individual decision making. The kind of decision making with which this body of theory deals is as follows: given two states, A and B, into either one of which an individual may put himself, the individual chooses A in preference to B (or vice versa). For instance, a child standing in front of a candy counter may be considering two states. In state A the child has $0.25 and no candy. In state B the child has $0.15 and a ten-cent candy bar. The economic theory of decision making is a theory about how to predict such decisions.

Economic theorists have been concerned with this problem since the days of Jeremy Bentham (1748–1832). In recent years the development of the economic theory of consumer's decision making (or, as the

economists call it, the theory of consumer's choice) has become exceedingly elaborate, mathematical, and voluminous. This literature is almost unknown to psychologists, in spite of sporadic pleas in both psychological (40, 84, 103, 104) and economic (101, 102, 123, 128, 199, 202) literature for greater communication between the disciplines.

The purpose of this paper is to review this theoretical literature, and also the rapidly increasing number of psychological experiments (performed by both psychologists and economists) that are relevant to it. The review will be divided into five sections: the theory of riskless choices, the application of the theory of riskless choices to welfare economics, the theory of risky choices, transitivity in decision making, and the theory of games and of statistical decision functions. Since this literature is unfamiliar and relatively inaccessible to most psychologists, and since I could not find any thorough bibliography on the theory of choice in the economic literature, this paper includes a rather extensive bibliography of the literature since 1930.

THE THEORY OF RISKLESS CHOICES[2]

Economic man. The method of those theorists who have been con-

[2] No complete review of this literature is available. Kauder (105, 106) has reviewed the very early history of utility theory. Stigler (180) and Viner (194) have reviewed the literature up to approximately 1930. Samuelson's book (164) contains an illuminating mathematical exposition of some of the content of this theory. Allen (6) explains the concept of indifference curves. Schultz (172) re-

cerned with the theory of decision making is essentially an armchair method. They make assumptions, and from these assumptions they deduce theorems which presumably can be tested, though it sometimes seems unlikely that the testing will ever occur. The most important set of assumptions made in the theory of riskless choices may be summarized by saying that it is assumed that the person who makes any decision to which the theory is applied is an economic man.

What is an economic man like? He has three properties. (*a*) He is completely informed. (*b*) He is infinitely sensitive. (*c*) He is rational.

Complete information. Economic man is assumed to know not only what all the courses of action open to him are, but also what the outcome of any action will be. Later on, in the sections on the theory of risky choices and on the theory of games, this assumption will be relaxed somewhat. (For the results of attempts to introduce the possibility of learning into this picture, see **51, 77**.)

Infinite sensitivity. In most of the older work on choice, it is assumed that the alternatives available to an individual are continuous, infinitely divisible functions, that prices are infinitely divisible, and that economic man is infinitely sensitive. The only purpose of these assumptions is to make the functions that they lead to,

continuous and differentiable. Stone (**182**) has recently shown that they can be abandoned with no serious changes in the theory of choice.

Rationality. The crucial fact about economic man is that he is rational. This means two things: He can weakly order the states into which he can get, and he makes his choices so as to maximize something.

Two things are required in order for economic man to be able to put all available states into a weak ordering. First, given any two states into which he can get, *A* and *B*, he must always be able to tell either that he prefers *A* to *B*, or that he prefers *B* to *A*, or that he is indifferent between them. If preference is operationally defined as choice, then it seems unthinkable that this requirement can ever be empirically violated. The second requirement for weak ordering, a more severe one, is that all preferences must be transitive. If economic man prefers *A* to *B* and *B* to *C*, then he prefers *A* to *C*. Similarly, if he is indifferent between *A* and *B* and between *B* and *C*, then he is indifferent between *A* and *C*. It is not obvious that transitivity will always hold for human choices, and experiments designed to find out whether or not it does will be described in the section on testing transitivity.

The second requirement of rationality, and in some ways the more important one, is that economic man must make his choices in such a way as to maximize something. This is the central principle of the theory of choice. In the theory of riskless choices, economic man has usually been assumed to maximize utility. In the theory of risky choices, he is assumed to maximize expected utility. In the literature on statistical decision making and the theory of games, various other fundamental

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views the developments up to but not including the Hicks-Allen revolution from the point of view of demand theory. Hicks's book (**87**) is a complete and detailed exposition of most of the mathematical and economic content of the theory up to 1939. Samuelson (**167**) has reviewed the integrability problem and the revealed preference approach. And Wold (**204, 205, 206**) has summed up the mathematical content of the whole field for anyone who is comfortably at home with axiom systems and differential equations.

principles of decision making are considered, but they are all maximization principles of one sort or another.

The fundamental content of the notion of maximization is that economic man always chooses the best alternative from among those open to him, as he sees it. In more technical language, the fact that economic man prefers A to B implies and is implied by the fact that A is higher than B in the weakly ordered set mentioned above. (Some theories introduce probabilities into the above statement, so that if A is higher than B in the weak ordering, then economic man is more likely to choose A than B, but not certain to choose A.)

This notion of maximization is mathematically useful, since it makes it possible for a theory to specify a unique point or a unique subset of points among those available to the decider. It seems to me psychologically unobjectionable. So many different kinds of functions can be maximized that almost any point actually available in an experimental situation can be regarded as a maximum of some sort. Assumptions about maximization only become specific, and therefore possibly wrong, when they specify what is being maximized.

There has, incidentally, been almost no discussion of the possibility that the two parts of the concept of rationality might conflict. It is conceivable, for example, that it might be costly in effort (and therefore in negative utility) to maintain a weakly ordered preference field. Under such circumstances, would it be "rational" to have such a field?

It is easy for a psychologist to point out that an economic man who has the properties discussed above is very unlike a real man. In fact, it is so easy to point this out that psycholo-gists have tended to reject out of hand the theories that result from these assumptions. This isn't fair. Surely the assumptions contained in Hullian behavior theory (91) or in the Estes (60) or Bush-Mosteller (36, 37) learning theories are no more realistic than these. The most useful thing to do with a theory is not to criticize its assumptions but rather to test its theorems. If the theorems fit the data, then the theory has at least heuristic merit. Of course, one trivial theorem deducible from the assumptions embodied in the concept of economic man is that in any specific case of choice these assumptions will be satisfied. For instance, if economic man is a model for real men, then real men should always exhibit transitivity of real choices. Transitivity is an assumption, but it is directly testable. So are the other properties of economic man as a model for real men.

Economists themselves are somewhat distrustful of economic man (119, 156), and we will see in subsequent sections the results of a number of attempts to relax these assumptions.

Early utility maximization theory. The school of philosopher-economists started by Jeremy Bentham and popularized by James Mill and others held that the goal of human action is to seek pleasure and avoid pain. Every object or action may be considered from the point of view of pleasure- or pain-giving properties. These properties are called the *utility* of the object, and pleasure is given by positive utility and pain by negative utility. The goal of action, then, is to seek the maximum utility. This simple hedonism of the future is easily translated into a theory of choice. People choose the alternative, from among those open to them, that

leads to the greatest excess of positive over negative utility. This notion of utility maximization is the essence of the utility theory of choice. It will reappear in various forms throughout this paper. (Bohnert [30] discusses the logical structure of the utility concept.)

This theory of choice was embodied in the formal economic analyses of all the early great names in economics. In the hands of Jevons, Walras, and Menger it reached increasingly sophisticated mathematical expression and it was embodied in the thinking of Marshall, who published the first edition of his great *Principles of Economics* in 1890, and revised it at intervals for more than 30 years thereafter (137).

The use to which utility theory was put by these theorists was to establish the nature of the demand for various goods. On the assumption that the utility of any good is a monotonically increasing negatively accelerated function of the amount of that good, it is easy to show that the amounts of most goods which a consumer will buy are decreasing functions of price, functions which are precisely specified once the shapes of the utility curves are known. This is the result the economists needed and is, of course, a testable theorem. (For more on this, see 87, 159.)

Complexities arise in this theory when the relations between the utilities of different goods are considered. Jevons, Walras, Menger, and even Marshall had assumed that the utilities of different commodities can be combined into a total utility by simple addition; this amounts to assuming that the utilities of different goods are independent (in spite of the fact that Marshall elsewhere discussed the notions of competing goods, like soap and detergents, and

completing goods, like right and left shoes, which obviously do not have independent utilities). Edgeworth (53), who was concerned with such nonindependent utilities, pointed out that total utility was not necessarily an additive function of the utilities attributable to separate commodities. In the process he introduced the notion of indifference curves, and thus began the gradual destruction of the classical utility theory. We shall return to this point shortly.

Although the forces of parsimony have gradually resulted in the elimination of the classical concept of utility from the economic theory of riskless choices, there have been a few attempts to use essentially the classical theory in an empirical way. Fisher (63) and Frisch (75) have developed methods of measuring marginal utility (the change in utility $[u]$ with an infinitesimal change in amount possessed $[Q]$, i.e., du/dQ) from market data, by making assumptions about the interpersonal similarity of consumer tastes. Recently Morgan (141) has used several variants of these techniques, has discussed mathematical and logical flaws in them, and has concluded on the basis of his empirical results that the techniques require too unrealistic assumptions to be workable. The crux of the problem is that, for these techniques to be useful, the commodities used must be independent (rather than competing or completing), and the broad commodity classifications necessary for adequate market data are not independent. Samuelson (164) has shown that the assumption of independent utilities, while it does guarantee interval scale utility measures, puts unwarrantably severe restrictions on the nature of the resulting demand function. Elsewhere Samuelson (158) presented,

primarily as a logical and mathematical exercise, a method of measuring marginal utility by assuming some time-discount function. Since no reasonable grounds can be found for assuming one such function rather than another, this procedure holds no promise of empirical success. Marshall suggested (in his notion of "consumer's surplus") a method of utility measurement that turns out to be dependent on the assumption of constant marginal utility of money, and which is therefore quite unworkable. Marshall's prestige led to extensive discussion and debunking of this notion (e.g., 28), but little positive comes out of this literature. Thurstone (186) is currently attempting to determine utility functions for commodities experimentally, but has reported no results as yet.

Indifference curves. Edgeworth's introduction of the notion of indifference curves to deal with the utilities of nonindependent goods was mentioned above. An indifference curve is, in Edgeworth's formulation, a constant-utility curve. Suppose that we consider apples and bananas, and suppose that you get

the same amount of utility from 10-apples-and-1-banana as you do from 6-apples-and-4-bananas. Then these are two points on an indifference curve, and of course there are an infinite number of other points on the same curve. Naturally, this is not the only indifference curve you may have between apples and bananas. It may also be true that you are indifferent between 13-apples-and-5-bananas and 5-apples-and-15-bananas. These are two points on another, higher indifference curve. A whole family of such curves is called an indifference map. Figure 1 presents such a map. One particularly useful kind of indifference map has amounts of a commodity on one axis and amounts of money on the other. Money is a commodity, too.

The notion of an indifference map can be derived, as Edgeworth derived it, from the notion of measurable utility. But it does not have to be. Pareto (146, see also 151) was seriously concerned about the assumption that utility was measurable up to a linear transformation. He felt that people could tell whether they preferred to be in state A or state B, but could not tell how much they preferred one state over the other. In other words, he hypothesized a utility function measurable only on an ordinal scale. Let us follow the usual economic language, and call utility measured on an ordinal scale *ordinal* utility, and utility measured on an interval scale, *cardinal* utility. It is meaningless to speak of the slope, or marginal utility, of an ordinal utility function; such a function cannot be differentiated. However, Pareto saw that the same conclusions which had been drawn from marginal utilities could be drawn from indifference curves. An indifference map can be drawn simply by finding all the com-
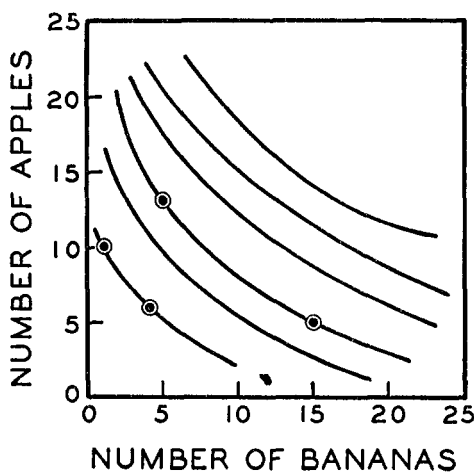

NUMBER OF BANANAS

Fig. 1. A Hypothetical Indifference Map

binations of the goods involved among which the person is indifferent. Pareto's formulation assumes that higher indifference curves have greater utility, but does not need to specify how much greater that utility is.

It turns out to be possible to deduce from indifference curves all of the theorems that were originally deduced from cardinal utility measures. This banishing of cardinal utility was furthered considerably by splendid mathematical papers by Johnson (97) and Slutsky (177). (In modern economic theory, it is customary to think of an n-dimensional commodity space, and of indifference hyperplanes in that space, each such hyperplane having, of course, $n-1$ dimensions. In order to avoid unsatisfactory preference structures, it is necessary to assume that consumers always have a complete weak ordering for all commodity bundles, or points in commodity space. Georgescu-Roegen [76], Wold [204, 205, 206, 208], Houthakker [90], and Samuelson [167] have discussed this problem.)

Pareto was not entirely consistent in his discussion of ordinal utility. Although he abandoned the assumption that its exact value could be known, he continued to talk about the sign of the marginal utility coefficient, which assumed that some knowledge about the utility function other than purely ordinal knowledge was available. He also committed other inconsistencies. So Hicks and Allen (88), in 1934, were led to their classic paper in which they attempted to purge the theory of choice of its last introspective elements. They adopted the conventional economic view about indifference curves as determined from a sort of imaginary questionnaire, and proceeded to derive all of the usual conclusions about consumer demand with no reference to the notion of even ordinal utility (though of course the notion of an ordinal scale of preferences was still embodied in their derivation of indifference curves). This paper was for economics something like the behaviorist revolution in psychology.

Lange (116), stimulated by Hicks and Allen, pointed out another inconsistency in Pareto. Pareto had assumed that if a person considered four states, A, B, C, and D, he could judge whether the difference between the utilities of A and B was greater than, equal to, or less than the difference between the utilities of C and D. Lange pointed out that if such a comparison was possible for any A, B, C, and D, then utility was cardinally measurable. Since it seems introspectively obvious that such comparisons can be made, this paper provoked a flood of protest and comment (7, 22, 117, 147, 209). Nevertheless, in spite of all the comment, and even in spite of skepticism by a distinguished economist as late as 1953 (153), Lange is surely right. Psychologists should know this at once; such comparisons are the basis of the psychophysical Method of Equal Sense Distances, from which an interval scale is derived. (Samuelson [162] has pointed out a very interesting qualification. Not only must such judgments of difference be possible, but they must also be transitive in order to define an interval scale.) But since such judgments of differences did not seem to be necessary for the development of consumer demand theory, Lange's paper did not force the reinstatement of cardinal utility.

Indeed, the pendulum swung further in the behavioristic direction. Samuelson developed a new analytic foundation for the theory of con-

sumer behavior, the essence of which is that indifference curves and hence the entire structure of the theory of consumer choice can be derived simply from observation of choices among alternative groups of purchases available to a consumer (160, 161). This approach has been extensively developed by Samuelson (164, 165, 167, 169) and others (50, 90, 125, 126). The essence of the idea is that each choice defines a point and a slope in commodity space. Mathematical approximation methods make it possible to combine a whole family of such slopes into an indifference hyperplane. A family of such hyperplanes forms an indifference "map."

In a distinguished but inaccessible series of articles, Wold (204, 205, 206; see also 208 for a summary presentation) has presented the mathematical content of the Pareto, Hicks and Allen, and revealed preference (Samuelson) approaches, as well as Cassel's demand function approach, and has shown that if the assumption about complete weak ordering of bundles of commodities which was discussed above is made, then all these approaches are mathematically equivalent.

Nostalgia for cardinal utility. The crucial reason for abandoning cardinal utility was the argument of the ordinalists that indifference curve analysis in its various forms could do everything that cardinal utility could do, with fewer assumptions. So far as the theory of riskless choice is concerned, this is so. But this is only an argument for parsimony, and parsimony is not always welcome. There was a series of people who, for one reason or another, wanted to reinstate cardinal utility, or at least marginal utility. There were several mathematically invalid attempts to

show that marginal utility could be defined even in an ordinal-utility universe (23, 24, 163; 25, 114). Knight (110), in 1944, argued extensively for cardinal utility; he based his arguments in part on introspective considerations and in part on an examination of psychophysical scaling procedures. He stimulated a number of replies (29, 42; 111). Recently Robertson (154) pleaded for the reinstatement of cardinal utility in the interests of welfare economics (this point will be discussed again below). But in general the indifference curve approach, in its various forms, has firmly established itself as the structure of the theory of riskless choice.

Experiments on indifference curves. Attempts to measure marginal utility from market data were discussed above. There have been three experimental attempts to measure indifference curves. Schultz, who pioneered in deriving statistical demand curves, interested his colleague at the University of Chicago, the psychologist Thurstone, in the problem of indifference curves. Thurstone (185) performed a very simple experiment. He gave one subject a series of combinations of hats and overcoats, and required the subject to judge whether he preferred each combination to a standard. For instance, the subject judged whether he preferred eight hats and eight overcoats to fifteen hats and three overcoats. The same procedure was repeated for hats and shoes, and for shoes and overcoats. The data were fitted with indifference curves derived from the assumptions that utility curves fitted Fechner's Law and that the utilities of the various objects were independent. Thurstone says that Fechner's Law fitted the data better than the other possible functions he considered, but

presents no evidence for this assertion. The crux of the experiment was the attempt to predict the indifference curves between shoes and overcoats from the other indifference curves. This was done by using the other two indifference curves to infer utility functions for shoes and for overcoats separately, and then using these two utility functions to predict the total utility of various amounts of shoes and overcoats jointly. The prediction worked rather well. The judgments of the one subject used are extraordinarily orderly; there is very little of the inconsistency and variability that others working in this area have found. Thurstone says, "The subject . . . was entirely naive as regards the psychophysical problem involved and had no knowledge whatever of the nature of the curves that we expected to find" (185, p. 154). He adds, "I selected as subject a research assistant in my laboratory who knew nothing about psychophysics. Her work was largely clerical in nature. She had a very even disposition, and I instructed her to take an even motivational attitude on the successive occasions . . . I was surprised at the consistency of the judgments that I obtained, but I am pretty sure that they were the result of careful instruction to assume a uniform motivational attitude."[3] From the economist's point of view, the main criticism of this experiment is that it involved imaginary rather than real transactions (200).

The second experimental measurement of indifference curves is reported by the economists Rousseas and Hart (157). They required large numbers of students to rank sets of three combinations of different amounts of ba-

con and eggs. By assuming that all students had the same indifference curves, they were able to derive a composite indifference map for bacon and eggs. No mathematical assumptions were necessary, and the indifference map is not given mathematical form. Some judgments were partly or completely inconsistent with the final map, but not too many. The only conclusion which this experiment justifies is that it is possible to derive such a composite indifference map.

The final attempt to measure an indifference curve is a very recent one by the psychologists Coombs and Milholland (49). The indifference curve involved is one between risk and value of an object, and so will be discussed below in the section on the theory of risky decisions. It is mentioned here because the same methods (which show only that the indifference curve is convex to the origin, and so perhaps should not be called measurement) could equally well be applied to the determination of indifference curves in riskless situations.

Mention should be made of the extensive economic work on statistical demand curves. For some reason the most distinguished statistical demand curve derivers feel it necessary to give an account of consumer's choice theory as a preliminary to the derivation of their empirical demand curves. The result is that the two best books in the area (172, 182) are each divided into two parts; the first is a general discussion of the theory of consumer's choice and the second a quite unrelated report of statistical economic work. Stigler (179) has given good reasons why the statistical demand curves are so little related to the demand curves of economic theory, and Wallis and Friedman (200) argue plausibly that this state

[3] Thurstone, L. L. Personal communication, December 7, 1953.

of affairs is inevitable. At any rate, there seems to be little prospect of using large-scale economic data to fill in the empirical content of the theory of individual decision making.

Psychological comments. There are several commonplace observations that are likely to occur to psychologists as soon as they try to apply the theory of riskless choices to actual experimental work. The first is that human beings are neither perfectly consistent nor perfectly sensitive. This means that indifference curves are likely to be observable as indifference regions, or as probability distributions of choice around a central locus. It would be easy to assume that each indifference curve represents the modal value of a normal sensitivity curve, and that choices should have statistical properties predictable from that hypothesis as the amounts of the commodities (locations in product space) are changed. This implies that the definition of indifference between two collections of commodities should be that each collection is preferred over the other 50 per cent of the time. Such a definition has been proposed by an economist (108), and used in experimental work by psychologists (142). Of course, 50 per cent choice has been a standard psychological definition of indifference since the days of Fechner.

Incidentally, failure on the part of an economist to understand that a just noticeable difference (j.n.d.) is a statistical concept has led him to argue that the indifference relation is intransitive, that is, that if *A* is indifferent to *B* and *B* is indifferent to *C*, then *A* need not be indifferent to *C* (8, 9, 10). He argues that if *A* and *B* are less than one j.n.d. apart, then *A* will be indifferent to *B;* the same of course is true of *B* and *C;* but *A* and

C may be more than one j.n.d. apart, and so one may be preferred to the other. This argument is, of course, wrong. If *A* has slightly more utility than *B*, then the individual will choose *A* in preference to *B* slightly more than 50 per cent of the time, even though *A* and *B* are less than one j.n.d. apart in utility. The 50 per cent point is in theory a precisely defined point, not a region. It may in fact be difficult to determine because of inconsistencies in judgments and because of changes in taste with time.

The second psychological observation is that it seems impossible even to dream of getting experimentally an indifference map in *n*-dimensional space where *n* is greater than 3. Even the case of $n = 3$ presents formidable experimental problems. This is less important to the psychologist who wants to use the theory of choice to rationalize experimental data than to the economist who wants to derive a theory of general static equilibrium.

Experiments like Thurstone's (185) involve so many assumptions that it is difficult to know what their empirical meaning might be if these assumptions were not made. Presumably, the best thing to do with such experiments is to consider them as tests of the assumption with the least face validity. Thurstone was willing to assume utility maximization and independence of the commodities involved (incidentally, his choice of commodities seems singularly unfortunate for justifying an assumption of independent utilities), and so used his data to construct a utility function. Of course, if only ordinal utility is assumed, then experimental indifference curves cannot be used this way. In fact, in an ordinal-utility universe neither of the principal assumptions made by Thurstone

can be tested by means of experimental indifference curves. So the assumption of cardinal utility, though not necessary, seems to lead to considerably more specific uses for experimental data.

At any rate, from the experimental point of view the most interesting question is: What is the observed shape of indifference curves between independent commodities? This question awaits an experimental answer.

The notion of utility is very similar to the Lewinian notion of valence (120, 121). Lewin conceives of valence as the attractiveness of an object or activity to a person (121). Thus, psychologists might consider the experimental study of utilities to be the experimental study of valences, and therefore an attempt at quantifying parts of the Lewinian theoretical schema.

APPLICATION OF THE THEORY OF RISKLESS CHOICES TO WELFARE ECONOMICS[4]

The classical utility theorists assumed the existence of interpersonally comparable cardinal utility. They were thus able to find a simple answer to the question of how to determine the best economic policy: That economic policy is best which results in the maximum total utility, summed over all members of the economy.

The abandonment of interpersonal comparability makes this answer useless. A sum is meaningless if the units being summed are of varying sizes and there is no way of reducing them to some common size. This

[4] The discussion of welfare economics given in this paper is exceedingly sketchy. For a picture of what the complexities of modern welfare economics are really like (see 11, 13, 14, 86, 118, 124, 127, 139, 140, 148, 154, 155, 166, 174).

point has not been universally recognized, and certain economists (e.g., 82, 154) still defend cardinal (but not interpersonally comparable) utility on grounds of its necessity for welfare economics.

Pareto's principle. The abandonment of interpersonal comparability and then of cardinal utility produced a search for some other principle to justify economic policy. Pareto (146), who first abandoned cardinal utility, provided a partial solution. He suggested that a change should be considered desirable if it left everyone at least as well off as he was before, and made at least one person better off.

Compensation principle. Pareto's principle is fine as far as it goes, but it obviously does not go very far. The economic decisions which can be made on so simple a principle are few and insignificant. So welfare economics languished until Kaldor (98) proposed the compensation principle. This principle is that if it is possible for those who gain from an economic change to compensate the losers for their losses and still have something left over from their gains, then the change is desirable. Of course, if the compensation is actually paid, then this is simply a case of Pareto's principle.

But Kaldor asserted that the compensation need not actually be made; all that was necessary was that it could be made. The fact that it could be made, according to Kaldor, is evidence that the change produces an excess of good over harm, and so is desirable. Scitovsky (173) observed an inconsistency in Kaldor's position: Some cases could arise in which, when a change from *A* to *B* has been made because of Kaldor's criterion, then a change back from *B* to *A* would also satisfy Kaldor's

criterion. It is customary, therefore, to assume that changes which meet the original Kaldor criterion are only desirable if the reverse change does not also meet the Kaldor criterion.

It has gradually become obvious that the Kaldor-Scitovsky criterion does not solve the problem of welfare economics (see e.g., **18, 99**). It assumes that the unpaid compensation does as much good to the person who gains it as it would if it were paid to the people who lost by the change. For instance, suppose that an industrialist can earn $10,000 a year more from his plant by using a new machine, but that the introduction of the machine throws two people irretrievably out of work. If the salary of each worker prior to the change was $4,000 a year, then the industrialist could compensate the workers and still make a profit. But if he does not compensate the workers, then the added satisfaction he gets from his extra $10,000 may be much less than the misery he produces in his two workers. This example only illustrates the principle; it does not make much sense in these days of progressive income taxes, unemployment compensation, high employment, and strong unions.

Social welfare functions. From here on the subject of welfare economics gets too complicated and too remote from psychology to merit extensive exploration in this paper. The line that it has taken is the assumption of a social welfare function (**21**), a function which combines individual utilities in a way which satisfies Pareto's principle but is otherwise undefined. In spite of its lack of definition, it is possible to draw certain conclusions from such a function (see e.g., **164**). However, Arrow (**14**) has recently shown that a social welfare function that meets certain very reasonable requirements about being sensitive in some way to the wishes of all the people affected, etc., cannot in general be found in the absence of interpersonally comparable utilities (see also **89**).

Psychological comment. Some economists are willing to accept the fact that they are inexorably committed to making moral judgments when they recommend economic policies (e.g., **152, 153**). Others still long for the impersonal amorality of a utility measure (e.g., **154**). However desirable interpersonally comparable cardinal utility may be, it seems utopian to hope that any experimental procedure will ever give information about individual utilities that could be of any practical use in guiding large-scale economic policy.

THE THEORY OF RISKY CHOICES[5]

Risk and uncertainty. Economists and statisticians distinguish between

[5] Strotz (**183**) and Alchian (**1**) present nontechnical and sparkling expositions of the von Neumann and Morgenstern utility measurement proposals. Georgescu-Roegen (**78**) critically discusses various axiom systems so as to bring some of the assumptions underlying this kind of cardinal utility into clear focus. Allais (**3**) reviews some of these ideas in the course of criticizing them. Arrow (**12, 14**) reviews parts of the field.

There is a large psychological literature on one kind of risky decision making, the kind which results when psychologists use partial reinforcement. This literature has been reviewed by Jenkins and Stanley (**96**). Recently a number of experimenters, including Jarrett (**95**), Flood (**69, 70**), Bilodeau (**27**), and myself (**56**) have been performing experiments on human subjects who are required to choose repetitively between two or more alternatives, each of which has a probability of reward greater than zero and less than one. The problems raised by these experiments are too complicated and too far removed from conventional utility theory to be dealt with in this paper. This line of experimentation may eventually provide the link which ties together utility theory and reinforcement theory.

risk and uncertainty. There does not seem to be any general agreement about which concept should be associated with which word, but the following definitions make the most important distinctions.

Almost everyone would agree that when I toss a coin the probability that I will get a head is .5. A proposition about the future to which a number can be attached, a number that represents the likelihood that the proposition is true, may be called a *first-order risk*. What the rules are for attaching such numbers is a much debated question, which will be avoided in this paper.

Some propositions may depend on more than one probability distribution. For instance, I may decide that if I get a tail, I will put the coin back in my pocket, whereas if I get a head, I will toss it again. Now, the probability of the proposition "I will get a head on my second toss" is a function of two probability distributions, the distribution corresponding to the first toss and that corresponding to the second toss. This might be called a *second-order risk*. Similarly, risks of any order may be constructed. It is a mathematical characteristic of all higher-order risks that they may be compounded into first-order risks by means of the usual theorems for compounding probabilities. (Some economists have argued against this procedure [83], essentially on the grounds that you may have more information by the time the second risk comes around. Such problems can best be dealt with by means of von Neumann and Morgenstern's [197] concept of strategy, which is discussed below. They become in general problems of uncertainty, rather than risk.)

Some propositions about the future exist to which no generally accepted probabilities can be attached. What

is the probability that the following proposition is true: Immediately after finishing this paper, you will drink a glass of beer? Surely it is neither impossible nor certain, so it ought to have a probability between zero and one, but it is impossible for you or me to find out what that probability might be, or even to set up generally acceptable rules about how to find out. Such propositions are considered cases of *uncertainty*, rather than of risk. This section deals only with the subject of first-order risks. The subject of uncertainty will arise again in connection with the theory of games.

Expected utility maximization. The traditional mathematical notion for dealing with games of chance (and so with risky decisions) is the notion that choices should be made so as to maximize *expected value*. The expected value of a bet is found by multiplying the value of each possible outcome by its probability of occurrence and summing these products across all possible outcomes. In symbols:

$$EV = p_1\$_1 + p_2\$_2 + \cdots + p_n\$_n,$$

where p stands for probability, $\$$ stands for the value of an outcome, and $p_1 + p_2 + \cdots + p_n = 1$.

The assumption that people actually behave the way this mathematical notion says they should is contradicted by observable behavior in many risky situations. People are willing to buy insurance, even though the person who sells the insurance makes a profit. People are willing to buy lottery tickets, even though the lottery makes a profit. Consideration of the problem of insurance and of the St. Petersburg paradox led Daniel Bernoulli, an eighteenth century mathematician, to propose that they could be resolved by assuming that

people act so as to maximize *expected utility*, rather than expected value (**26**). (He also assumed that utility followed a function that more than a century later was proposed by Fechner for subjective magnitudes in general and is now called Fechner's Law.) This was the first use of the notion of expected utility.

The literature on risky decision making prior to 1944 consists primarily of the St. Petersburg paradox and other gambling and probability literature in mathematics, some literary discussion in economics (e.g., **109**, **187**), one economic paper on lotteries (**189**), and the early literature of the theory of games (**31, 32, 33, 34, 195**), which did not use the notion of utility. The modern period in the study of risky decision making began with the publication in 1944 of von Neumann and Morgenstern's monumental book *Theory of Games and Economic Behavior* (**196**, see also **197**), which we will discuss more fully later. Von Neumann and Morgenstern pointed out that the usual assumption that economic man can always say whether he prefers one state to another or is indifferent between them needs only to be slightly modified in order to imply cardinal utility. The modification consists of adding that economic man can also completely order probability combinations of states. Thus, suppose that an economic man is indifferent between the certainty of $7.00 and a 50-50 chance of gaining $10.00 or nothing. We can assume that his indifference between these two prospects means that they have the same utility for him. We may define the utility of $0.00 as zero utiles (the usual name for the unit of utility, just as sone is the name for the unit of auditory loudness), and the utility of $10.00 as 10 utiles. These two

arbitrary definitions correspond to defining the two undefined constants which are permissible since cardinal utility is measured only up to a linear transformation. Then we may calculate the utility of $7.00 by using the concept of expected utility as follows:

$$U(\$7.00) = .5\,U(\$10.00) + .5\,U(\$0.00)$$
$$= .5(10) + .5(0) = 5.$$

Thus we have determined the cardinal utility of $7.00 and found that it is 5 utiles. By varying the probabilities and by using the already found utilities it is possible to discover the utility of any other amount of money, using only the two permissible arbitrary definitions. It is even more convenient if instead of +$10.00, −$10.00 or some other loss is used as one of the arbitrary utilities.

A variety of implications is embodied in this apparently simple notion. In the attempt to examine and exhibit clearly what these implications are, a number of axiom systems, differing from von Neumann and Morgenstern's but leading to the same result, have been developed (**73, 74, 85, 135, 136, 171**). This paper will not attempt to go into the complex discussions (e.g., **130, 131, 168, 207**) of these various alternative axiom systems. One recent discussion of them (**78**) has concluded, on reasonable grounds, that the original von Neumann and Morgenstern set of axioms is still the best.

It is profitable, however, to examine what the meaning of this notion is from the empirical point of view if it is right. First, it means that risky propositions can be ordered in desirability, just as riskless ones can. Second, it means that the concept of expected utility is behaviorally meaningful. Finally, it means that choices among risky alternatives

are made in such a way that they maximize expected utility.

If this model is to be used to predict actual choices, what could go wrong with it? It might be that the probabilities by which the utilities are multiplied should not be the objective probabilities; in other words, a decider's estimate of the subjective importance of a probability may not be the same as the numerical value of that probability. It might be that the method of combination of probabilities and values should not be simple multiplication. It might be that the method of combination of the probability-value products should not be simple addition. It might be that the process of gambling has some positive or negative utility of its own. It might be that the whole approach is wrong, that people just do not behave as if they were trying to maximize expected utility. We shall examine some of these possibilities in greater detail below.

Economic implications of maximizing expected utility. The utility-measurement notions of von Neumann and Morgenstern were enthusiastically welcomed by many economists (e.g., **73**, **193**), though a few (e.g., **19**) were at least temporarily (**20**) unconvinced. The most interesting economic use of them was proposed by Friedman and Savage (**73**), who were concerned with the question of why the same person who buys insurance (with a negative expected money value), and therefore is willing to pay in order not to take risks, will also buy lottery tickets (also with a negative expected money value) in which he pays in order to take risks. They suggested that these facts could be reconciled by a doubly inflected utility curve for money, like that in Fig. 2. If I represents the person's current income, then he is

clearly willing to accept "fair" insurance (i.e., insurance with zero expected money value) because the serious loss against which he is insuring would have a lower expected utility than the certain loss of the insurance premium. (Negatively accelerated total utility curves, like that from the origin to I, are what you get when marginal utility decreases; thus, decreasing marginal
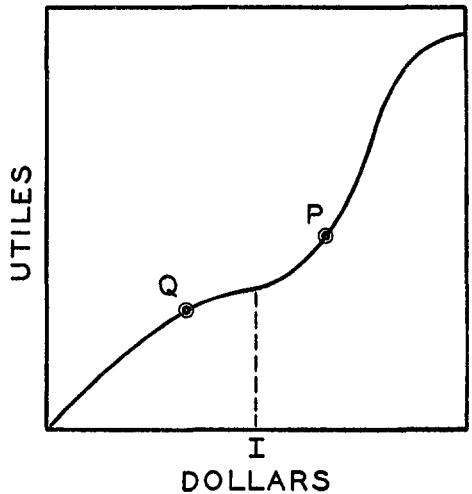

FIG. 2. HYPOTHETICAL UTILITY CURVE FOR MONEY, PROPOSED BY FRIEDMAN AND SAVAGE

utility is consistent with the avoidance of risks.) The person would also be willing to buy lottery tickets, since the expected utility of the lottery ticket is greater than the certain loss of the cost of the ticket, because of the rapid increase in the height of the utility function. Other considerations make it necessary that the utility curve turn down again. Note that this discussion assumes that gambling has no inherent utility.

Markowitz (**132**) suggested an important modification in this hypothesis. He suggested that the origin of a person's utility curve for money be taken as his customary

financial status, and that on both sides of the origin the curve be assumed first concave and then convex. If the person's customary state of wealth changes, then the shape of his utility curve will thus remain generally the same with respect to where he now is, and so his risk-taking behavior will remain pretty much the same instead of changing with every change of wealth as in the Friedman-Savage formulation.

Criticism of the expected-utility maximization theory. It is fairly easy to construct examples of behavior that violate the von Neumann-Morgenstern axioms (for a particularly ingenious example, see **183**). It is especially easy to do so when the amounts of money involved are very large, or when the probabilities or probability differences involved are extremely small. Allais (5) has constructed a questionnaire full of items of this type. For an economist interested in using these axioms as a basis for a completely general theory of risky choice, these examples may be significant. But psychological interest in this model is more modest. The psychologically important question is: Can such a model be used to account for simple experimental examples of risky decisions?

Of course a utility function derived by von Neumann-Morgenstern means is not necessarily the same as a classical utility function (**74, 203**; see also **82**).

Experiment on the von Neumann-Morgenstern model. A number of experiments on risky decision making have been performed. Only the first of them, by Mosteller and Nogee (**142**), has been in the simple framework of the model described above. All the rest have in some way or another centered on the concept of probabilities effective for behavior which differ in some way from the objective probabilities, as well as on utilities different from the objective values of the objects involved.

Mosteller and Nogee (**142**) carried out the first experiment to apply the von Neumann-Morgenstern model. They presented Harvard undergraduates and National Guardsmen with bets stated in terms of rolls at poker dice, which each subject could accept or refuse. Each bet gave a "hand" at poker dice. If the subject could beat the hand, he won an amount stated in the bet. If not, he lost a nickel. Subjects played with $1.00, which they were given at the beginning of each experimental session. They were run together in groups of five; but each decided and rolled the poker dice for himself. Subjects were provided with a table in which the mathematically fair bets were shown, so that a subject could immediately tell by referring to the table whether a given bet was fair, or better or worse than fair.

In the data analysis, the first step was the determination of "indifference offers." For each probability used and for each player, the amount of money was found for which that player would accept the bet 50 per cent of the time. Thus equality was defined as 50 per cent choice, as it is likely to be in all psychological experiments of this sort. Then the utility of $0.00 was defined as 0 utiles, and the utility of losing a nickel was defined as -1 utile. With these definitions and the probabilities involved, it was easy to calculate the utility corresponding to the amount of money involved in the indifference offer. It turned out that, in general, the Harvard undergraduates had diminishing marginal utilities, while the National Guardsmen had increasing marginal utilities.

The utilities thus calculated were used in predicting the results of more complex bets. It is hard to evaluate the success of these predictions. At any rate, an auxiliary paired-comparisons experiment showed that the hypothesis that subjects maximized expected utility predicted choices better than the hypothesis that subjects maximized expected money value.

The utility curve that Mosteller and Nogee derive is different from the one Friedman and Savage (73) were talking about. Suppose that a subject's utility curve were of the Friedman-Savage type, as in Fig. 2, and that he had enough money to put him at point *P*. If he now wins or loses a bet, then he is moved to a different location on the indifference curve, say *Q*. (Note that the amounts of money involved are much smaller than in the original Friedman-Savage use of this curve.) However, the construction of a Mosteller-Nogee utility curve assumes that the individual is always at the same point on his utility curve, namely the origin. This means that the curve is really of the Markowitz (132) type discussed above, instead of the Friedman-Savage type. The curve is not really a curve of utility of money in general, but rather it is a curve of the utility-for-*n*-more dollars. Even so, it must be assumed further that as the total amount of money possessed by the subject changes during the experiment, the utility-for-*n*-more dollars curve does not change. Mosteller and Nogee argue, on the basis of detailed examination of some of their data, that the amount of money possessed by the subjects did not seriously influence their choices. The utility curves they reported showed changing marginal utility within the amounts of money used in their ex-

periment. Consequently, their conclusion that the amount of money possessed by the subjects was not seriously important can only be true if their utility curves are utility-for-*n*-more dollars curves and if the shapes of such curves are not affected by changes in the number of dollars on hand. This discussion exhibits a type of problem which must always arise in utility measurement and which is new in psychological scaling. The effects of previous judgments on present judgments are a familiar story in psychophysics, but they are usually assumed to be contaminating influences that can be minimized or eliminated by proper experimental design. In utility scaling, the fundamental idea of a utility scale is such that the whole structure of a subject's choices should be altered as a result of each previous choice (if the choices are real ones involving money gains or losses). The Markowitz solution to this problem is the most practical one available at present, and that solution is not entirely satisfactory since all it does is to assume that people's utilities for money operate in such a way that the problem does not really exist. This assumption is plausible for money, but it gets rapidly less plausible when other commodities with a less continuous character are considered instead.

Probability preferences. In a series of recent experiments (55, 57, 58, 59), the writer has shown that subjects, when they bet, prefer some probabilities to others (57), and that these preferences cannot be accounted for by utility considerations (59). All the experiments were basically of the same design. Subjects were required to choose between pairs of bets according to the method of paired comparisons. The bets were of three kinds: positive expected value, nega-

tive expected value, and zero expected value. The two members of each pair of bets had the same expected value, so that there was never (in the main experiment [57, 59]) any objective reason to expect that choosing one bet would be more desirable than choosing the other.

Subjects made their choices under three conditions: just imagining they were betting; betting for worthless chips; and betting for real money. They paid any losses from their own funds, but they were run in extra sessions after the main experiment to bring their winnings up to $1.00 per hour.

The results showed that two factors were most important in determining choices: general preferences or dislikes for risk-taking, and specific preferences among probabilities. An example of the first kind of factor is that subjects strongly preferred low probabilities of losing large amounts of money to high probabilities of losing small amounts of money—they just didn't like to lose. It also turned out that on positive expected value bets, they were more willing to accept long shots when playing for real money than when just imagining or playing for worthless chips. An example of the second kind of factor is that they consistently preferred bets involving a 4/8 probability of winning to all others, and consistently avoided bets involving a 6/8 probability of winning. These preferences were reversed for negative expected value bets.

These results were independent of the amounts of money involved in the bets, so long as the condition of constant expected value was maintained (59). When pairs of bets which differed from one another in expected value were used, the choices were a compromise between maximizing ex-

pected amount of money and betting at the preferred probabilities (58). An attempt was made to construct individual utility curves adequate to account for the results of several subjects. For this purpose, the utility of $0.30 was defined as 30 utiles, and it was assumed that subjects cannot discriminate utility differences smaller than half a utile. Under these assumptions, no individual utility curves consistent with the data could be drawn. Various minor experiments showed that these results were reliable and not due to various possible artifacts (59). No attempt was made to generate a mathematical model of probability preferences.

The existence of probability preferences means that the simple von Neumann-Morgenstern method of utility measurement cannot succeed. Choices between bets will be determined not only by the amounts of money involved, but also by the preferences the subjects have among the probabilities involved. Only an experimental procedure which holds one of these variables constant, or otherwise allows for it, can hope to measure the other. Thus my experiments cannot be regarded as a way of measuring probability preferences; they show only that such preferences exist.

It may nevertheless be possible to get an interval scale of the utility of money from gambling experiments by designing an experiment which measures utility and probability preferences simultaneously. Such experiments are likely to be complicated and difficult to run, but they can be designed.

Subjective probability. First, a clarification of terms is necessary. The phrase *subjective probability* has been used in two ways: as a name for a school of thought about the

logical basis of mathematical probability (**51, 52, 80**) and as a name for a transformation on the scale of mathematical probabilities which is somehow related to behavior. Only the latter usage is intended here. The clearest distinction between these two notions arises from consideration of what happens when an objective probability can be defined (e.g., in a game of craps). If the subjective probability is assumed to be different from the objective probability, then the concept is being used in its second, or psychological, sense. Other terms with the same meaning have also been used: personal probability, psychological probability, expectation (a poor term because of the danger of confusion with expected value). (For a more elaborate treatment of concepts in this area, see **192**.)

In 1948, prior to the Mosteller and Nogee experiment, Preston and Baratta (**149**) used essentially similar logic and a somewhat similar experiment to measure subjective probabilities instead of subjective values. They required subjects to bid competitively for the privilege of taking a bet. All bids were in play money, and the data consisted of the winning bids. If each winning bid can be considered to represent a value of play money such that the winning bidder is indifferent between it and the bet he is bidding for, and if it is further assumed that utilities are identical with the money value of the play money and that all players have the same subjective probabilities, then these data can be used to construct a subjective probability scale. Preston and Baratta constructed such a scale. The subjects, according to the scale, overestimate low probabilities and underestimate high ones, with an indifference point (where subjective equals objective probability) at about 0.2. Griffith (**81**) found somewhat similar results in an analysis of parimutuel betting at race tracks, as did Attneave (**17**) in a guessing game, and Sprowls (**178**) in an analysis of various lotteries. The Mosteller and Nogee data (**142**) can, of course, be analyzed for subjective probabilities instead of subjective values. Mosteller and Nogee performed such an analysis and said that their results were in general agreement with Preston and Baratta's. However, Mosteller and Nogee found no indifference point for their Harvard students, whereas the National Guardsmen had an indifference point at about 0.5. They are not able to reconcile these differences in results.

The notion of subjective probability has some serious logical difficulties. The scale of objective probability is bounded by 0 and 1. Should a subjective probability scale be similarly bounded, or not? If not, then many different subjective probabilities will correspond to the objective probabilities 0 and 1 (unless some transformation is used so that 0 and 1 objective probabilities correspond to infinite subjective probabilities, which seems unlikely). Considerations of the addition theorem to be discussed in a moment have occasionally led people to think of a subjective probability scale bounded at 0 but not at 1. This is surely arbitrary. The concept of absolute certainty is neither more nor less indeterminate than is the concept of absolute impossibility.

Even more drastic logical problems arise in connection with the addition theorem. If the objective probability of event A is P, and that of A not occurring is Q, then $P+Q=1$. Should this rule hold for subjective probabilities? Intuitively it seems neces-

sary that if we know the subjective probability of *A*, we ought to be able to figure out the subjective probability of not-*A*, and the only reasonable rule for figuring it out is subtraction of the subjective probability of *A* from that of complete certainty. But the acceptance of this addition theorem for subjective probabilities plus the idea of bounded subjective probabilities means that the subjective probability scale must be identical with the objective probability scale. Only for a subjective probability scale identical with the objective probability scale will the subjective probabilities of a collection of events, one of which must happen, add up to 1. In the special case where only two events, *A* and not-*A*, are considered, a subjective probability scale like *S*1 or *S*2 in Fig. 3 would meet the requirements of additivity, and this fact has led to some speculation about such scales, particularly about *S*1. But such scales do not meet the additivity requirements when more than two events are considered.

One way of avoiding these diffi-

FIG. 3. HYPOTHETICAL SUBJECTIVE PROBABILITY CURVES

culties is to stop thinking about a scale of subjective probabilities and, instead, to think of a weighting function applied to the scale of objective probabilities which weights these objective probabilities according to their ability to control behavior. Presumably, I was studying this ability in my experiments on probability preferences (55, 57, 58, 59). There is no reason why such weighted probabilities should add up to 1 or should obey any other simple combinatory principle.

Views and experiments which combine utility and subjective probability. The philosopher Ramsey published in 1926 (reprinted in 150) an essay on the subjective foundations of the theory of probability; this contained an axiom system in which both utility and subjective probability appeared. He used 0.5 subjective probability as a reference point from which to determine utilities, and then used these utilities to determine other subjective probabilities. Apparently, economists did not discover Ramsey's essay until after von Neumann and Morgenstern's book aroused interest in the subject. The only other formal axiom system in which both utility and subjective probability play a part is one proposed by Savage (171), which is concerned with uncertainty, rather than risk, and uses the concept of subjective probability in its theory-of-probability sense.

The most extensive and important experimental work in the whole field of decision making under risk and uncertainty is now being carried out by Coombs and his associates at the University of Michigan. Coombs's thinking about utility and subjective probability is an outgrowth of his thinking about psychological scaling in general. (For a discussion of his views, see 43, 44, 45, 46, 47.) The
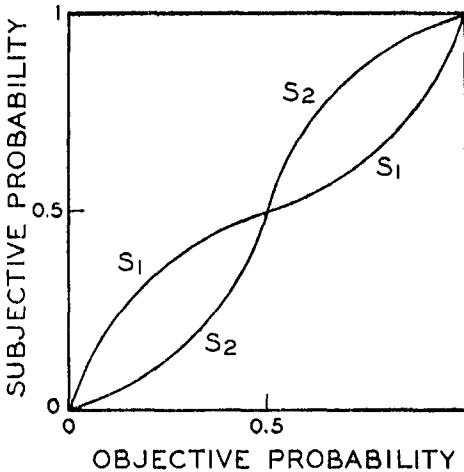
essence of his work is the attempt to measure both utility and subjective probability on an ordered metric scale. An ordered metric scale has all the properties of an ordinal scale, and, in addition, the distances between some or all of the stimuli can be rank ordered. Coombs has developed various experimental procedures for obtaining such information about the spacings of stimuli.

In the most important article on utility and subjective probability to come out of the Coombs approach, Coombs and Beardslee (48) present an analysis of gambling decisions involving three independent variables: utility for prize, utility for stake, and subjective probability. All three are assumed measurable only up to an ordered metric, although it is assumed that the psychological probability of losing the stake is one minus the psychological probability of winning the prize, an assumption that limits the permissible underlying psychological probability functions to shapes like those in Fig. 3. An elaborate graphic analysis of the indifference surfaces in this three-dimensional space is given, containing far too many interesting relationships to summarize here. An experiment based on this model was designed. Coombs is reluctant to use sums of money as the valuable objects in his experiments because of the danger that subjects will respond to the numerical value of the amount of dollars rather than to the psychological value. Therefore he used various desirable objects (e.g., a radio) as stimuli, and measured their utility by the techniques he has developed to obtain ordered metric scales. He used simple numerical statements of probability as the probability stimuli, and assumed that subjective probability was equal to objective probability. The subject from whose judgments the ordered metric utility measurement was constructed was then presented with imaginary bets involving these objects and probabilities, and it turned out that she almost always chose the one with the higher expected utility. This experiment is significant only as an illustration of the application of the method; the conclusion that subjects attempt to maximize expected utility cannot very comfortably be generalized to other subjects and to real choices without better evidence.

Coombs and Milholland (49) did a much more elaborate experiment in which they established ordered metric scales, both for the utilities of a collection of objects and for the subjective probabilities of a collection of statements (e.g., Robin Roberts will win 20 games next year). Statements and objects were combined into "bets," and the two subjects for whom the ordered metric scales had been established were asked to make judgments about which bet they would most, and which they would least, prefer from among various triads of bets. These judgments were examined to discover whether or not they demonstrated the existence of at least one convex indifference curve between utility and subjective probability (the requirements for demonstrating the convexity of an indifference curve by means of ordered metric judgments are fairly easy to state). A number of cases consistent with a convex indifference curve were found, but a retest of the ordered metric data revealed changes which eliminated all of the cases consistent with a convex indifference curve for one subject, and all but one case for the other. It is not possible to make a statistical test of whether or not

that one case might have come about by chance. No evidence was found for the existence of concave indifference curves, which are certainly inconsistent with the theory of risky decisions. This experiment is a fine example of the strength and weakness of the Coombs approach. It makes almost no assumptions, takes very little for granted, and avoids the concept of error of judgment; as a result, much of the potential information in the data is unused and rarely can any strong conclusions be drawn.

A most disturbing possibility is raised by experiments by Marks (**133**) and Irwin (**94**) which suggest that the shape of the subjective probability function is influenced by the utilities involved in the bets. If utilities and subjective probabilities are not independent, then there is no hope of predicting risky decisions unless their law of combination is known, and it seems very difficult to design an experiment to discover that law of combination. However, the main differences that Marks and Irwin found were between probabilities attached to desirable and undesirable alternatives. It is perfectly possible that there is one subjective probability function for bets with positive expected values and a different one for bets with negative expected values, just as the negative branch of the Markowitz utility function is likely to be different from the positive branch. The results of my probability preference experiments showed very great differences between the probability preference patterns for positive and for negative expected-value bets (**57**), but little difference between probability preferences at different expected-value levels so long as zero expected value was not crossed (**59**). This evidence supports the idea that perhaps only two subjective probability functions are necessary.

Santa Monica Seminar. In the summer of 1952 at Santa Monica, California, a group of scientists conferred on problems of decision making. They met in a two-month seminar sponsored by the University of Michigan and the Office of Naval Research. The dittoed reports of these meetings are a gold mine of ideas for the student of this problem. Some of the work done at this seminar is now being prepared for a book on *Decision Processes* edited by R. M. Thrall, C. H. Coombs, and R. L. Davis, of the University of Michigan.

Several minor exploratory experiments were done at this seminar. Vail (**190**) did an experiment in which he gave four children the choice of which side of various bets they wanted to be on. On the assumption of linear utilities, he was able to compute subjective probabilities for these children. The same children, however, were used as subjects for a number of other experiments; so, when Vail later tried them out on some other bets, he found that they consistently chose the bet with the highest probability of winning, regardless of the amounts of money involved. When 50-50 bets were involved, one subject consistently chose the bet with the *lowest* expected value. No generalizable conclusions can be drawn from these experiments.

Kaplan and Radner (**100**) tried out a questionnaire somewhat like Coombs's method of measuring subjective probability. Subjects were asked to assign numbers to various statements. The numbers could be anything from 0 to 100 and were to represent the likelihood that the statement was true. The hypotheses to be tested were: (*a*) for sets of ex-

haustive and mutually exclusive statements in which the numbers assigned (estimates of degree of belief) were nearly equal, the sums of these numbers over a set would increase with the number of alternatives (because low probabilities would be overestimated); (b) for sets with the same numbers of alternatives, those with one high number assigned would have a lower set sum than those with no high numbers. The first prediction was verified; the second was not. Any judgments of this sort are so much more likely to be made on the basis of number preferences and similar variables than on subjective probabilities that they offer very little hope as a method of measuring subjective probabilities.

Variance preferences. Allais (**2, 3, 4**) and Georgescu-Roegen (**78**) have argued that it is not enough to apply a transform on objective value and on objective probability in order to predict risky decisions from expected utility (see also **188**); it is also necessary to take into account at least the variance, and possibly the higher moments, of the utility distribution. There are instances in which this argument seems convincing. You would probably prefer the certainty of a million dollars to a 50-50 chance of getting either four million or nothing. I do not think that this preference is due to the fact that the expected utility of the 50-50 bet is less than the utility of one million dollars to you, although this is possible. A more likely explanation is simply that the variances of the two propositions are different. Evidence in favor of this is the fact that if you knew you would be offered this choice 20 times in succession, you would probably take the 50-50 bet each time. Allais (**5**) has constructed a number of more sophisticated examples of this type. However, from a simple-minded psychological point of view, these examples are irrelevant. It is enough if the theory of choice can predict choices involving familiar amounts of money and familiar probability differences—choices such as those which people are accustomed to making. It may be necessary for economic theory that the theory of choice be universal and exceptionless, but experimental psychologists need not be so ambitious. This is fortunate, because the introduction of the variance and higher moments of the utility distribution makes the problem of applying the theory experimentally seem totally insoluble. It is difficult enough to derive reasonable methods of measuring utility alone from risky choices; when it also becomes necessary to measure subjective probability and to take the higher moments of the utility distribution into account, the problem seems hopeless. Allais apparently hopes to defeat this problem by using psychophysical methods to measure utility (and presumably subjective probability also). This is essentially what Coombs has done, but Coombs has recognized that such procedures are unlikely to yield satisfactory interval scales. The dollar scale of the value of money is so thoroughly taught to us that it seems almost impossible to devise a psychophysical situation in which subjects would judge the utility, rather than the dollar value, of dollars. They might judge the utility of other valuable objects, but since dollars are the usual measure of value, such judgments would be less useful, and even these judgments would be likely to be contaminated by the dollar values of the objects. I would get more utility from a new electric shaver than I would from a new washing machine,

but because of my knowledge of the relative money values of these objects, I would certainly choose the washing machine if given a choice between them. Somewhat similar arguments can be applied against using psychophysical methods to measure subjective probability. A final point is that, since these subjective scales are to be used to predict choices, it would be best if they could be derived from similar choices.

Other approaches. Shackle (175) has proposed a theory of decision making under risk and uncertainty. This theory is unique in that it does not assume any kind of maximizing behavior. For every possible outcome of a decision made in a risky or uncertain situation, Shackle assumes that there is a degree of potential surprise that this, rather than some other, outcome would occur. Every outcome-potential surprise pair is ranked in accordance with its ability to stimulate the mind (stimulation increases with increasing outcome and decreases with increasing potential surprise). The highest-ranking positive outcome-potential surprise pair and the highest-ranking negative pair are found, and these two possibilities alone determine what the individual will do. Semi-mathematical methods are used to predict the outcome of consideration of possible lines of action. Although attempts have been made to relate it to Wald's minimax principle for statistical decision functions (see below), the fact remains that most critics of the Shackle point of view have judged it to be either too vague to be useful, or, if specified in detail, too conducive to patently absurd predictions (e.g., 201).

Shackle's point of view was developed primarily to deal with unique choices—choices which can be made only once. Allais (3) has similarly

criticized conventional utility theory's attack on this problem. Since the usual frequency theory of probability conceives of the probability as the limit of the outcomes of a large number of similar trials, it is questionable that notions which use probability in the ordinary sense (like the notion of maximizing expected utility) are applicable to unique choices. However, this seems to be an experimental problem. If notions which use ordinary probability are incapable of predicting actual unique choices, then it will be necessary to seek other theoretical tools. But so long as a generally acceptable probability can be defined (e.g., as in the unique toss of a coin), it is not necessary to assume a priori that theories based on conventional probabilities will be inadequate. When no generally acceptable probability can be defined, then the problem becomes very different.

Cartwright and Festinger (38, 41) have proposed a theory about the time it takes to make decisions which is in some ways similar to those discussed in this section. The main difference is that they add the concept of restraining forces, and that they conceive of all subjective magnitudes as fluctuating randomly around a mean value. From this they deduce various propositions about decision times and the degree of certainty which subjects will feel about their decisions, and apparently these propositions work out experimentally pretty well (38, 39, 61, 62). The Lewinian theoretical orientation seems to lead to this kind of model; Lewin, Dembo, Festinger, and Sears (122) present a formally similar theory about level of aspiration. Of course, the notion of utility is very similar to the Lewinian notion of valence.

Landahl (115) has presented a mathematical model for risk-taking behavior based on the conceptual neurology of the mathematical biophysics school.

Psychological comments. The area of risky decision making is full of fascinating experimental problems. Of these, the development of a satisfactory scale of utility of money and of subjective probability must come first, since the theory of risky decision making is based on these notions. The criterion for satisfactoriness of these scales must be that they successfully predict choices other than those from which they were derived. To be really satisfactory, it is desirable that they should predict choices in a wide variety of differing situations. Unlike the subjective scales usually found in psychophysics, it is likely that these scales will differ widely from person to person, so a new determination of each scale must be made for each new subject. It can only be hoped that the scales do not change in time to any serious degree; if they do, then they are useless.

Once scales of utility and subjective probability are available, then many interesting questions arise. What about the addition theorem for subjective probabilities? Does gambling itself have utility, and how much? To what extent can these subjective scales be changed by learning? To what degree do people differ, and can these differences be correlated with environmental, historical, or personality differences? Finally, psychologists might be able to shed light on the complex economic problem of interacting utilities of different goods.

The area of risky decision making, like the area of the theory of games, tends to encourage in those interested in it the custom of carrying out

small pilot experiments on their sons, laboratory assistants, or secretaries. Such experiments are too seldom adequately controlled, and are almost never used as a basis for larger-scale, well-designed experiments. Whether an ill-designed and haphazardly executed little experiment is better than no experiment at all is questionable. The results of such pilot experiments too often are picked up and written into the literature without adequate warning about the conditions under which they were performed and the consequent limitations on the significance of the results.

THE TRANSITIVITY OF CHOICES

In the section on riskless choices this paper presented a definition of economic man. The most important part of this definition can be summed up by saying that economic man is rational. The concept of rationality involves two parts: that of a weak ordering of preferences, and that of choosing so as to maximize something. Of these concepts, the one which seems most dubious is the one of a weakly ordered preference field. This is dubious because it implies that choices are transitive; that is, if *A* is preferred to *B*, and *B* is preferred to *C*, then *A* is preferred to *C*.

Two economists have designed experiments specifically intended to test the transitivity of choices. Papandreou performed an elaborate and splendidly controlled experiment (145) designed to discover whether or not intransitivities occurred in imagined-choice situations. He prepared triplets of hypothetical bundles of admissions to plays, athletic contests, concerts, etc., and required his subjects to choose between pairs of bundles. Each bundle consisted of a total of four admissions to two events, e.g., 3 plays and 1 tennis

tournament. In the main experiment, each bundle is compared with two others involving the same kinds of events, but in the better designed auxiliary experiment, a total of six different events are used, so that each bundle has no events in common with the other two bundles in its triplet. Since there are three bundles in each triplet, there are three choices between pairs for each triplet, and these choices may, or may not, be transitive. The subjects were permitted to say that they were indifferent between two bundles; consequently there were 27 possible configurations of choices, of which only 13 satisfied the transitivity axiom. In the main experiment, 5 per cent of the triplets of judgments were intransitive; in the auxiliary experiment, only 4 per cent. Papandreou develops a stochastic model for choices under such conditions; the results are certainly consistent with the amount of intransitivity permitted by his model. Papandreou concludes that at least for his specific experimental conditions, transitivity does exist.

May (138), using different kinds of stimuli in a less elaborate experiment, comes up with results less consistent with transitivity. May required a classroom group to make pairwise choices between three marriage partners who were identified only by saying how intelligent, good looking, and rich they were. Judgments of indifference were not permitted. The results were that 27 per cent of the subjects gave intransitive triads of choices. May suggests, very plausibly, that intransitive choices may be expected to occur whenever more than one dimension exists in the stimuli along which subjects may order their preferences. However, May would probably have gotten fewer intransitivities if he had permitted the indifference judgment. If subjects are really indifferent among all three of the elements of a triad of objects, but are required to choose between them in pairs and do so by chance, then they will choose intransitively one-fourth of the time. Papandreou's stochastic model gives one theory about what happens when preferences diverge just slightly from indifference, but presumably a more detailed model can be worked out. Papandreou's model permits only three states: prefer A to B, prefer B to A, and indifferent. It ought to be possible to base a model for such situations on the cumulative normal curve, and thus to permit any degree of preference. For every combination of degrees of preference, such a model would predict the frequency of intransitive choices.

In the paired comparisons among bets (57) described in the section on risky choices, quite elaborate intransitivities could and did occur. However, it is easy to show that any intransitivity involving four or more objects in a paired comparisons judgment situation will necessarily produce at least one intransitivity involving three objects. Consequently, the intransitive triplet or circular triad is the best unit of analysis for intransitivities in these more complicated judgment situations. I counted the frequency of occurrence of circular triads and found that they regularly occurred about 20 per cent of the total number of times they could occur. (Of course, no indifference judgments could be permitted.) The experiment fulfills May's criterion for the occurrence of intransitivities, since both probability and amount of money were present in each bet, and subjects could be expected to take both into account

when making choices. It might be supposed that the difference between the imaginary choices of the Papandreou and May experiments and the real choices in my experiment would lead to differences in the frequency of occurrence of intransitivities, but there were no substantial differences in my experiment between the frequencies of occurrence in the just-imagining sessions and in the real gambling sessions, and what differences there were, were in the direction of greater transitivity when really gambling. These facts should facilitate further experiments on this problem.

In one sense, transitivity can never be violated. A minimum of three choices is required to demonstrate intransitivity. Since these choices will necessarily be made in sequence, it can always be argued that the person may have changed his tastes between the first choice and the third. However, unless the assumption of constancy of tastes over the period of experimentation is made, no experiments on choice can ever be meaningful, and the whole theory of choice becomes empty (see 184 for a similar situation). So this quibble can be rejected at once.

Utility maximization will not work except with a transitive preference field. Consequently, if the models discussed in this paper are to predict experimental data, it is necessary that intransitivities in these data be infrequent enough to be considered as errors. However, from a slightly different point of view (54) the occurrence or nonoccurrence of transitive choice patterns is an experimental phenomenon, and presumably a lawful one. May has suggested what that law is: Intransitivities occur when there are conflicting stimulus dimensions along which to judge.

This notion could certainly be tested and made more specific by appropriate experiments.

A final contribution in a related, but different, area is Vail's stochastic utility model (191). Vail assumes that choices are dependent on utilities that oscillate in a random manner around a mean value. From this assumption plus a few other reasonable ones, he deduces that if the over-all preference is $1 > 2 > 3$, and if 1 is preferred to 2 more than 2 is preferred to 3, then the frequencies of occurrence of the six possible transitive orderings should be ordered as follows: $123 > 132 > 213 > 312 > 231 > 321$. This result is certainly easy to test experimentally, and sounds plausible.

THE THEORY OF GAMES AND OF DECISION FUNCTIONS[6]

This section will not go into the theory of games or into the intimately related subject of statistical decision functions at all thoroughly. These are mathematical subjects of a highly

[6] Marschak (134), Hurwicz (92), Neisser (143), Stone (181), and Kaysen (107) published reviews of *The Theory of Games and Economic Behavior* which present the fundamental ideas in much simpler language than the original source. Marschak works out in detail the possible solutions of a complicated three-person bargaining game, and thereby illustrates the general nature of a solution. The two volumes of *Contributions to the Theory of Games* (112, 113), plus McKinsey's book on the subject (129), provide an excellent bibliography of the mathematical literature. McKinsey's book is an exposition of the fundamental concepts, intended as a textbook, which is simpler than von Neumann and Morgenstern and pursues certain topics further. Wald's book (198) is, of course, the classical work on statistical decision functions. Bross's book (35) presents the fundamental ideas about statistical decision functions more simply, and with a somewhat different emphasis. Girshick and Blackwell's book (79) is expected to be a very useful presentation of the field.

technical sort, with few statements which lend themselves to experimental test. Rather, the purpose of this section is to show how these subjects relate to what has gone before, to give a brief summary of the contents of *Theory of Games and Economic Behavior* by von Neumann and Morgenstern (197), and to describe a few experiments in the area of game playing —experiments which are stimulated by the theory of games although not directly relevant to it.

The theory of games. The theory of games probably originated in the work of Borel (31, 32, 33, 34; see also 71, 72) in the 1920's. In 1928, von Neumann (195), working independently of Borel, published the first proof of the fundamental theorem in the theory, a theorem that Borel had not believed to be generally true. However, the subject did not become important until 1944, when von Neumann and Morgenstern published their epoch-making book (196). (A second edition, with an appendix on cardinal utility measurement, came out in 1947 [197].) Their purpose was to analyze mathematically a very general class of problems, which might be called problems of strategy. Consider a game of tic-tac-toe. You know at any moment in the game what the moves available to your opponent are, but you do not know which one he will choose. The only information you have is that his choice will not, in general, be completely random; he will make a move which is designed in some way to increase his chance of winning and diminish yours. Thus the situation is one of uncertainty rather than risk. Your goals are similar to your opponent's. Your problem is: what strategy should you adopt? The theory of games offers no practical help in developing strategies, but it does offer rules about how to choose

among them. In the case of tic-tac-toe, these rules are trivial, since either player can force a draw. But in more complicated games of strategy, these rules may be useful. In particular, the theory of games may be helpful in analyzing proper strategy in games having random elements, like the shuffling of cards, or the throwing of dice. It should be noted that the concept of a game is an exceedingly general concept. A scientist in his laboratory may be considered to be playing a game against Nature. (Note, however, that we cannot expect Nature to try to defeat the scientist.) Negotiators in a labor dispute are playing a game against one another. Any situation in which money (or some valuable equivalent) may be gained as the result of a proper choice of strategy can be considered as a game.

To talk about game theory, a few technical terms are necessary. A *strategy* is a set of personal rules for playing the game. For each possible first move on your part, your opponent will have a possible set of responses. For each possible response by your opponent, you will have a set of responses, and so on through the game. A strategy is a list which specifies what your move will be for every conceivable previous set of moves of the particular game you are playing. Needless to say, only for the simplest games (e.g., matching pennies) does this concept of strategy have any empirical meaning.

Associated with strategies are *imputations*. An imputation is a set of payments made as a result of a game, one to each player. In general, different imputations will be associated with different sets of strategies, but for any given set of strategies there may be more than one imputation (in games involving coalitions).

Imputation X is said to *dominate*

imputation Y if one or more of the players has separately greater gains (or smaller losses) in X than in Y and can, by acting together (in the case of more than one player), enforce the occurrence of X, or of some other imputation at least as good. The relationship of domination is not transitive.

A *solution* is a set of imputations, none of which dominates another, such that every imputation outside the solution is dominated by at least one imputation within the solution. Von Neumann and Morgenstern assert that the task of the theory of games is to find solutions. For any game, there may be one or more than one. One bad feature of the theory of games is that it frequently gives a large, or even infinite, number of solutions for a game.

The above definitions make clear that the only determiner of behavior in games, according to this theory, is the amounts of money which may be won or lost, or the expected amounts in games with random elements. The fun of playing, if any, is irrelevant.

The minimax loss principle. The notions of domination and of solution imply a new fundamental rule for decision making—a rule sharply different from the rule of maximizing utility or expected utility with which this paper has been concerned up to this section. This rule is the rule of minimizing the maximum loss, or, more briefly, *minimax loss*. In other words, the rule is to consider, for each possible strategy that you could adopt, what the worst possible outcome is, and then to select that strategy which would have the least ill-effects if the worst possible outcome happened. Another way of putting the same idea is to call it the principle of maximizing the minimum gain, or *maximin gain*. This rule makes considerable sense in two-person games

when you consider that the other player is out to get you, and so will do his best to make the worst possible outcome for you occur. If this rule is expressed geometrically, it asserts that the point you should seek is a saddle-point, like the highest point in a mountain pass (the best rule for crossing mountains is to minimize the maximum height, so explorers seek out such saddle-points).

Before we go any further, we need a few more definitions. Games may be among any number of players, but the simplest game is a *two-person game*, and it is this kind of game which has been most extensively and most successfully analyzed. Fundamentally, two kinds of payoff arrangements are possible. The simplest and most common is the one in which one player wins what the other player loses, or, more generally, the one for which the sum of all the payments made as a result of the game is zero. This is called a *zero-sum game*. In *nonzero-sum games*, analytical complexities arise. These can be diminished by assuming the existence of a fictitious extra player, who wins or loses enough to bring the sum of payments back to zero. Such a fictitious player cannot be assumed to have a strategy and cannot, of course, interact with any of the other players.

In zero-sum two-person games, what will happen? Each player, according to the theory, should pick his minimax strategy. But will this result in a stable solution? Not always. Sometimes the surface representing the possible outcomes of the game does not have a saddle-point. In this case, if player A chooses his minimax strategy, then player B will have an incentive not to use his own minimax strategy, because having found out his opponent's strategy, he can gain more by some other strategy. Thus the game has no solution.

Various resolutions of this problem are possible. Von Neumann and Morgenstern chose to introduce the notion of a *mixed strategy*, which is a probability distribution of two or more pure strategies. The fundamental theorem of the theory of games is that if both players in a zero-sum two-person game adopt mixed strategies which minimize the maximum *expected loss*, then the game will always have a saddle-point. Thus each person will get, in the long run, his expected loss, and will have no incentive to change his behavior even if he should discover what his opponent's mixed strategy is. Since A is already getting the minimum possible under the strategy he chose, any change in strategy by B will only increase A's payoff, and therefore cause B to gain less or lose more than he would by his own minimax strategy. The same is true of B.

Games involving more than two people introduce a new element—the possibility that two or more players will cooperate to beat the rest. Such a cooperative agreement is called a *coalition*, and it frequently involves *side-payments* among members of the coalition. The method of analysis for three-or-more-person games is to consider all possible coalitions and to solve the game for each coalition on the principles of a two-person game. This works fairly well for three-person games, but gets more complicated and less satisfactory for still more people.

This is the end of this exposition of the content of von Neumann and Morgenstern's book. It is of course impossible to condense a tremendous and difficult book into one page. The major points to be emphasized are these: the theory of games is not a model of how people actually play games (some game theorists will dis-

agree with this), nor is it likely to be of any practical use in telling you how to play a complicated game; the crux of the theory of games is the principle of choosing the strategy which minimizes the maximum expected financial loss; and the theory defines a solution of a game as a set of imputations which satisfies this principle for all players.

Assumptions. In their book von Neumann and Morgenstern say "We have . . . assumed that [utility] is numerical . . . substitutable and unrestrictedly transferable between the various players." (**197**, p. 604.) Game theorists disagree about what this and other similar sentences mean. One likely interpretation is that they assume utility to be linear with the physical value of money involved in a game and to be interpersonally comparable. The linear utility curves seem to be necessary for solving two-person games; the interpersonal comparability is used for the extension to *n* persons. Attempts are being made to develop solutions free of these assumptions (**176**).

Statistical decision functions. Von Neumann (**195**) first used the minimax principle in his first publication on game theory in 1928. Neyman and Pearson mentioned its applicability to statistical decision problems in 1933 (**144**). Wald (**198**), who prior to his recent death was the central figure in the statistical decision-function literature, first seriously applied the minimax principle to statistical problems in 1939. Apparently, all these uses of the principle were completely independent of one another.

After *Theory of Games and Economic Behavior* appeared in 1944, Wald (**198**) reformulated the problem of statistical decision making as one of playing a game against Nature,

The statistician must decide, on the basis of observations which cost something to make, between policies, each of which has a possible gain or loss. In some cases, all of these gains and losses and the cost of observing can be exactly calculated, as in industrial quality control. In other cases, as in theoretical research, it is necessary to make some assumption about the cost of being wrong and the gain of being right. At any rate, when they are put in this form, it is obvious that the ingredients of the problem of statistical decision making have a gamelike sound. Wald applied the minimax principle to them in a way essentially identical with game theory.

A very frequent criticism of the minimax approach to games against Nature is that Nature is not hostile, as is the opponent in a two-person game. Nature will not, in general, use a minimax strategy. For this reason, other principles of decision making have been suggested. The simple principle of maximizing expected utility (which is the essence of the Bayes's theorem [15, 198] solution of the problem) is not always applicable because, even though Nature is not hostile, she does not offer any way of assigning a probability to each possible outcome. In other words, statistical decision making is a problem of uncertainty, rather than of risk. Savage has suggested the principle of minimaxing *regret*, where regret is defined as the difference between the maximum which can be gained under any strategy given a certain state of the world and the amount gained under the strategy adopted. Savage believes (**170**, also personal communication) that neither von Neumann and Morgenstern nor Wald actually intended to propose the principle of minimaxing loss; they confined their discussions to cases in which the concepts of minimax loss and minimax regret amount to the same thing. Other suggested principles are: maximizing the maximum expected gain, and maximizing some weighted average of the maximum and minimum expected gains (**93**). None of these principles commands general acceptance; each can be made to show peculiar consequences under some conditions (see **170**).

Experimental games. The concepts of the theory of games suggest a new field of experimentation: How do people behave in game situations? Such experimentation would center on the development of strategies, particularly mixed strategies, and, in three-or-more-person games, on the development of coalitions and on the bargaining process. You should remember that the theory of games does not offer a mathematical model predicting the outcomes of such games (except in a few special cases); all it does is offer useful concepts and language for talking about them, and predict that certain outcomes will not occur.

A few minor experiments of this kind have been conducted by Flood, a mathematician, while he was at Rand Corporation. He usually used colleagues, many of whom were experts in game theory, and secretaries as subjects. The general design of his experiments was that a group of subjects were shown a group of desirable objects on a table, and told that they, as a group, could have the first object they removed from the table, and that they should decide among themselves which object to choose and how to allocate it. In the first experiment (**64**) the allocation problem did not arise because enough duplicate objects were provided so that each subject could have one of

the kind of object the group selected. The subjects were Harvard undergraduates, and the final selection was made by negotiation and voting. In the second experiment (65), in which the subjects were colleagues and secretaries, a long negotiation process eliminated some of the objects, but a time limit forced a selection by lot from among the rest. Further negotiations to solve the allocation problem were terminated by a secretary, who snatched the object, announced that it was hers, and then tried to sell it. No one was willing to buy, so the experiment terminated. Other experiments (66, 67) showed that coalitions sometimes form, that a sophisticated subject could blackmail the group for an extra side-payment by threatening to change his vote, and that the larcenous secretary, having succeeded once, had to be physically restrained in subsequent sessions to prevent more larceny. The general conclusion suggested by all these experiments is that even experts on game theory are less rational and more conventional than game theory might lead experimenters to expect.

Psychological comments. The most nutritive research problems in this area seem to be the social problems of how bargaining takes place. Flood's experiments left bargainers free and used physical objects, whose utilities probably vary widely from subject to subject, as stimuli to bargain over. This is naturalistic, but produces data too complex and too nonnumerical for easy analysis. A simpler situation in which the possible communications from one bargainer to another are limited (perhaps by means of an artificial vocabulary), in which the subjects do not see one another, and in which the object bargained over is simple, preferably being merely a sum of money, would be

better. Physical isolation of one subject from another would make it possible to match each subject against a standard bargainer, the experimenter or a stooge, who bargains by a fixed set of rules that are unknown to the subject. Flood (personal communication) is conducting experiments of this sort. For three-or-more-person games, Asch's (16) technique of using a group consisting of only one real subject and all the rest stooges might well be used. It would be interesting, for instance, to see how the probability of a coalition between two players changes as the number and power of players united against them increase.

The theory of games is the area among those described in this paper in which the uncontrolled and casually planned "pilot experiment" is most likely to occur. Such experiments are at least as dangerous here as they are in the area of risky decision making. Flood's results suggest that it is especially important to use naive subjects and to use them only once, unless the effects of expertness and experience are the major concern of the experiment.

SUMMARY

For a long time, economists and others have been developing mathematical theories about how people make choices among desirable alternatives. These theories center on the notion of the subjective value, or utility, of the alternatives among which the decider must choose. They assume that people behave rationally, that is, that they have transitive preferences and that they choose in such a way as to maximize utility or expected utility.

The traditional theory of riskless choices, a straightforward theory of utility maximization, was challenged by the demonstration that the mathe-

matical tool of indifference curves made it possible to account for riskless choices without assuming that utility could be measured on an interval scale. The theory of riskless choices predicted from indifference curves has been worked out in detail. Experimental determination of indifference curves is possible, and has been attempted. But utility measured on an interval scale is necessary (though not sufficient) for welfare economics.

Attention was turned to risky choices by von Neumann and Morgenstern's demonstration that complete weak ordering of risky choices implies the existence of utility measurable on an interval scale. Mosteller and Nogee experimentally determined utility curves for money from gambling decisions, and used them to predict other gambling decisions. Edwards demonstrated the existence of preferences among probabilities in gambling situations, which complicates the experimental measurement of utility. Coombs developed a model

for utility and subjective probability measured on an ordered metric scale, and did some experiments to test implications of the model.

Economists have become worried about the assumption that choices are transitive. Experiments have shown that intransitive patterns of choice do occur, and so stochastic models have been developed which permit occasional intransitivities.

The theory of games presents an elaborate mathematical analysis of the problem of choosing from among alternative strategies in games of strategy. This paper summarizes the main concepts of this analysis. The theory of games has stimulated interest in experimental games, and a few bargaining experiments which can be thought of in game-theoretical terms have been performed.

All these topics represent a new and rich field for psychologists, in which a theoretical structure has already been elaborately worked out and in which many experiments need to be performed.

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