UTILITY OF GAINS AND LOSSES

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GAINS AND LOSSES:

Measurement-Theoretical and Experimental Approaches

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To Carolyn and Aurora

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Preface

This monograph brings together in one place my current understanding of the behavioral properties people either exhibit or should exhibit when they make selections among valued alternatives, and it investigates how these properties lead to numerical representations of these preferences. The entire field has been under development, both theoretically and empirically, since the 1947 publication of the second edition of von Neumann and Morgenstern's *The Theory of Games and Economic Behavior,* and by now the literature is enormous. For relevant reviews from a psychological perspective, see various volumes of the *Annual Review of Psychology,* in particular, the latest one that includes a chapter by Mellers, Schwartz, and Cooke (1998). For an economic perspective, the recent *Handbook of Utility Theory, Vol. I Principles* (Barberà, Hammond, & Seidel, 1998) covers much of the theoretical area.

I shall report on this literature very selectively, focusing on those papers that strike me as involving decisive steps toward our current theories of utility and subjective weighting functions. The selection is biased, no doubt, toward the ideas that I have worked on during the past 11 years, which is why this is best thought of as a monograph, not a survey. My contributions are scattered over a number of journals and are interrelated in fairly complex ways. It is unlikely that anyone other than me actually understands all of the connections and modifications that have taken place as my understanding has developed. I hope it will prove useful to organize the material in a coherent fashion.

In an effort to increase the accessibility both to psychologists, who frequently respond with some revulsion to pages of mathematics, and to economists, who although typically quite mathematical often have a preferred style (differential equations, matrix analysis, and topology) different from mine (algebra and functional equations), I have relegated all but the simplest proofs to sections so marked.

The material has been presented and discussed in various forms in several graduate seminars on utility (one co-taught with Professor L. Robin Keller) at the University of California, Irvine (UCI). The most recent version was during the fall of 1998 in which the penultimate draft of this volume served as text. I appreciate the several roles that students in these seminars have played: audience while I tried out ways of organizing the material, critics of ideas and formulations, spotters of errors, and sources of results and ideas that I had overlooked. In particular in the last seminar, three of the students, Rolf H. Johnson, Bethany R. Knapp, and Robert K. Plice, and a visitor, Dr. Thierry Marchant, made numerous very helpful suggestions for improvements and dug out several errors or misstatements.

Most important, of course, have been my collaborators and former graduate students (whose degree dates and current positions are shown) who have worked on related problems during the development of the work. They are, in alphabetical order, János Aczél (Professor Emeritus of Pure Mathematics, University of Waterloo), Alan Brothers (UCI Ph. D., 1990, Battelle Pacific Northwest Laboratories), Younghee Cho (Ph. D. UCI, 1995, Assistant Professor, California State University at Long Beach), Peter C. Fishburn (AT&T Laboratories-Research), Gerald R. Fisher (Ph. D. UCI, 1999), Robert Hamm (Ph. D. Harvard, 1979, Department of Family Medicine, University of Oklahoma), A. A. J. Marley (Ph. D. University of Pennsylvania, 1965, Professor, McGill University), Barbara A. Mellers (Professor, Ohio State

University), Robert Sneddon (Ph. D. UCI, 1999, Postdoctoral Fellow, California Institute of Technology), Detlof von Winterfeldt (Professor, University of Southern California), and Elke U. Weber (Ph. D. Harvard, 1985, Professor, Ohio State University). Their contributions are many and without them the work, especially the experimental aspects, would be far, far less complete than it now is—however incomplete it may still seem.

There are others whose contributions to my thinking have been very considerable even though in some cases we have not collaborated directly on these materials. Continual conversations and e-mail correspondence with Michael H. Birnbaum (Professor, California State University at Fullerton), including very detailed comments on the penultimate version of this monograph, have kept me sensitive to what, from my perspective, often seemed at first to be unpleasant empirical realities. The late Amos Tversky (Professor, Stanford University), often in collaboration with Daniel Kahneman (Professor, Princeton University), has clearly had a major impact over the past 30 years on the way all of us look at decision problems, and at a theoretical level our work is intertwined, as will be apparent from the text. Marley followed much of the work on joint receipts and urged me to explore the important fully associative case that is discussed in Chapter 7. Moreover, as a result of his reading the previous draft, he and I came to collaborate on several results that are reported in Chapter 3, and he has commented in great detail on the previous version and parts of the present one. Louis Narens (Professor, UCI) has been a singular force in the development of modern measurement theory, and I have been greatly influenced by his ideas. Even though we have not collaborated directly on utility theory, his influence is clearly there. Peter P. Wakker (Professor, University of Leiden) has, in detailed critiques of two drafts, repeatedly shown me connections with his work and that of others, questioned ambiguities and misinterpretations on my part, and urged me to make as clear as I can the ways in which my point of view differs significantly from that of mainstream economics. And Elke Weber, commenting on an earlier version, tried hard, but probably not very successfully from her perspective, to push me to toward increasing its accessibility to psychologists with limited mathematical experience. In addition, I greatly appreciate specific comments and suggestions of Drs. J. Aczél, J.-C. Falmagne, Reid Hastie, L. R. Keller, Stephen Link, Barbara A. Mellers, Robin Pope.

The monograph is much better for the efforts of all these scientists. Nonetheless, as hardly need be said, its faults and errors can be charged only to me.

All of my work in this area (and indeed in all of the areas on which I have worked) has been supported in part by National Science Foundation grants. During the preparation of the monograph, it was by grants SBR-9520107 and SBR-9808057 to the University of California, Irvine. I am deeply indebted to the Foundation for supporting my research for well over 40 years.

Finally, producing such a volume involves a somewhat prolonged gestation and more than a few hours of intense labor—a total of over two years in this case—that imposes in various ways on those who are personally close, especially my wife Carolyn. I greatly appreciate her forbearance and support during this, her fourth, encounter with the birth of a book. Would that I could guarantee to her that it will never happen again.

Irvine, CA, September 1999

Chapter 1
INTRODUCTION

This monograph focuses on choices between (and occasionally among) valued entities or alternatives, and the properties that these choices seem to satisfy (descriptive ones) or should satisfy (normative ones). To a lesser degree, it also focuses on evaluations assigned to single entities or alternatives as occur, for example, when a store sets prices on goods. At least one of these topics is of lively importance to most of us, although only a few of us—including, presumably, the reader—find fascinating the formal (mathematical) attempts to model such choices. Such modeling is both more intricate and more extensive than one might anticipate.

There are several options about how to begin, and some of these early decisions tend to be reasonably decisive for the nature of theory that evolves. My aim in this first chapter is to lay out some of these options and make clear which I follow and why; others will arise as we move along. For other general perspectives on these issues, Edwards (1992a), Marley (1997b), and Mellers, Schwartz, and Cooke (1998) are useful.

1.1 Certain, Uncertain, and Risky Alternatives

One surprising feature of the pre-1979 literature was the assumption that choices are among states of wealth, not increments or decrements of it. Of course, no one who ran experiments ever tried to phrase it that way. The gambles involved changes from the status quo, some desirable, some not. Moreover, most of us, without the opportunity to do some tallying, probably do not know our total wealth to better than about 10% accuracy, and typically we do not think of our choices in that way. For example, if one is purchasing a briefcase and the choice is down to two, one's wealth is a factor only to the extent of determining whether both alternatives are feasible, but otherwise it is a background variable common to both and they are evaluated on their own terms relative to the status quo. Although a few authors commented upon the lack of realism of the standard interpretation (Edwards, 1954a; Markowitz, 1952), the major break in that tradition was the famous "prospect theory" paper (Kahneman & Tversky, 1979) that showed vividly that "absolute" gains and losses behaved differently. This monograph is based upon such a distinction, which is elaborated below.

So in some sense, there is a status quo, which often refers to our current if only partially known situation, and each alternative is evaluated relative to the status quo as either effecting an incremental gain or loss from it. Often one can interpret the status quo as the current situation, but that may not be useful if, for example, the decision maker defines the effective status quo as some reference level different from the current situation. However defined, the distinction between gains and losses as modifications in the status quo that are seen as better or worse, respectively, has a major impact on the nature of the theory one constructs, as we shall see in Chapters 6 and, especially, 7. For some contemporary views of economists about the role of the status quo, see Samuelson and Zeckhauser (1988).

1.1.1 Certain alternatives

A *certain alternative* or, more briefly, *consequence* is something that you value (positively or negatively) and which, if received, will be whatever it purports to be. Other commonly used terms are "riskless" and "pure" alternatives. Examples are most goods available in reputable stores. "Certainty" is, of course, an ideal concept. In various ways goods may fail to meet their specifications, and so there is uncertainty or risk involved in the sense that what you buy may fail to be what you thought it was: The car may be a lemon, the TV may be defective, the garment poorly made, the money counterfeit, and so on. Today in the developed countries many goods are made virtually riskless by various warranties and laws, and so the concept of certain alternatives is not vacuous. Nevertheless, certainty is an idealization because of the inconvenience when it fails to hold, even with warranties.

Typically, a domain of certain alternatives is modeled as a set C. It is useful to assume C includes money—both receipts and expenditures—as a special case, which is modeled as a subset $\mathcal{R}$ of the real numbers, $\mathbb{R}$. So $\mathcal{R} = C \cap \mathbb{R}$. Often, we assume $\mathcal{R} = \mathbb{R}$.

We assume that the set of certain consequences, i.e., changes from the status quo, includes a distinguished element, called *no change from the status quo*. Intuitively, this refers to a null consequence that does not alter the decision maker's current state. Rather than use 0 to represent no change from the status quo, which tempts one to think exclusively in monetary terms, I use e, which is often the symbol used in algebra for the analogue of an additive identify element. And, indeed, as we will see in Section 4.2.1, it is precisely that. It is a convenient abuse of terminology to speak of e just as "the status quo," without the modifying phrase "no change from."

The status quo plays two roles. Once we introduce the idea of preferences over consequences, it partitions C into *gains* and *losses*, where the former are consequences preferred to the status quo and the latter are consequences less preferred than the status quo. This distinction plays a crucial role in this monograph. The other use, as was mentioned, is as an identity element in the operation of joint receipt studied in Chapters 4, 6, and 7.

1.1.2 Uncertain alternatives

An *uncertain alternative* involves uncertainty about which consequence actually will be received, and that uncertainty is not resolved until some *chance experiment*, in the sense used in statistics, takes place. The outcome of the experiment determines which one of finitely many consequences from C one actually receives. (A formal definition is provided in Section 1.1.6.) The statistical use of the term "experiment" is considerably narrower than its use in the empirical sciences where experimental manipulations are key to the concept. Here it simply means carrying out some process whose outcomes have a random aspect. The context should make clear which use is intended. The limitation to finitely many consequences is convenient and, for the most part, realistic. However, a good deal of the theoretical literature addresses infinite cases and replaces the sums we shall encounter with integrals. I do not find these infinite idealizations particularly helpful.

One needs to distinguish carefully the use of the terms "outcome" and "consequence." When the chance experiment of tossing a die is run, the outcome is the face that comes up, usually identified by a symbol or number on the face. To each outcome, some consequence may be attached, such as winning or losing an amount of money. Throughout the monograph I use the term *outcome* to refer only to what happens in the experiment and the term *consequence* to refer to any valued good that is arbitrarily associated to an outcome. Consequences can be from the set C of certain alternatives, in which case the resulting uncertain alternative is called *first order.* But sometimes the consequences are more complex objects, such as sets of certain alternatives or other uncertain alternatives. This aspect is discussed more fully below.

There can be various degrees of uncertainty in which the decision maker knows something about the likelihood of outcomes of the experiment but does not know a probability distribution over them. Events occur, but the decision maker does not have an objective appraisal of how likely they are. This can happen when we are dealing with situations for which repetitions of the experiment underlying the event are either impossible or are only very approximately possible. Most alternatives are of this character. Choosing among them is called *decision making under uncertainty.* If one is contemplating investing in drilling for oil, one does not have a firm probability distribution concerning the possible consequences of drilling in particular locations. Our knowledge about estimating the location and extent of oil fields simply is insufficient to reduce this to a probability. Or if one is thinking of setting up a business in Ukraine, there are many uncertainties that cannot be reduced to probabilities.

Of course, many examples of uncertain chance events are coupled with delayed resolution of the uncertainty. This interesting but difficult problem of time delay is not addressed in this monograph. For an introduction to such issues, see Loewenstein and Elster (1992) and Pope (1983, 1985, 1996/7).

1.1.3 Risky alternatives

In this literature, the word "risky" has a double meaning, the one specific and objective, the other vague and quite subjective. The specific meaning refers to an uncertain alternative for which the probability is known (or given) for each chance outcome that can arise when the experiment is performed. Such alternatives are called *risky.* The simplest *first-order risky alternatives* involve only consequences that are certain. An example is a bet on the toss of a fair die, with the consequences being a ticket to a ball game if either the 1 or the 6 face arises, or two tickets to the same ball game if the 3 arises, or the loss of \$10 otherwise. Given that the die is fair, the probability of a 1 or 6, and so of the consequence of a ticket, is $\frac{1}{3}$; that of a 3, and so two tickets, is $\frac{1}{6}$; and that of the balance, and so a loss of \$10, is $\frac{1}{2}$.

Given the concept of a first-order risky alternative, one can easily imagine more complex risky alternatives in which one or more of the consequences are themselves risky alternatives. When the probabilities are known at both levels, we speak of a *second-order risky alternative.*

A typical example is a state lottery in which winning on the first stage leads to an additional lottery, usually with a good deal of publicity for those who go to the second stage. Such *compound alternatives,* both risky and uncertain, are discussed in Section 1.1.6.5, and second-order gambles recur several times in the monograph.

The distinction between risky and uncertain alternatives dates at least to Knight (1921), and is reflected in the contrast between the theory of risk of von Neumann and Morgenstern (1947) and the theory of uncertainty of Savage (1954). See also the comments of Arrow (1951). It is a question of either having or not having known or constructible probabilities over the events. With Ellsberg (1961) the idea of ambiguity of probability—an urn with some fixed mix of white and red balls, but with no knowledge about the mix—began to play a role (§ 2.4.2). There is a small, growing literature concerning ambiguity and its avoidance (Curley, Yates, & Abrams, 1986; Fox & Tversky, 1995, 1997; Heath & Tversky, 1991). A relatively recent survey of uncertainty and ambiguity with more detailed references is Camerer and M. Weber (1992).

The other, subjective, meaning of "risky" is as an appraisal by either the theorist or by the decision maker that one alternative is "perceived as more or less risky" than another. The concept of "perceived riskiness" is treated as a primitive judgment on the part of the decision maker. Theorists have often tried to identify the riskiness of an alternative with one or another property that can be defined in terms of features of the alternatives. An example is the assumption that increasing the variance, while holding the mean of a money lottery fixed, increases the perceived risk. Several of these notions of risk manipulation and risk aversion are examined in Section 4.6. Other approaches, mine among them (Luce & Weber, 1986), have attempted to model judgments of relative risk, and Coombs (1975) developed an interesting theory of preferences based on trade-offs between risk and expected value. I do not attempt to cover this area, in part because of excellent review articles by Brachinger and M. Weber (1997), E. U. Weber (1997), and M. Weber and Camerer (1987).

1.1.4 Lotteries

A special, but important, class of risky alternatives is those whose consequences are all sums of money. I call them *lotteries.* The literature is not fully consistent on this language. Some authors treat the word "lottery" as interchangeable with the word "gamble," which term has, as we shall see, a far more general meaning in my lexicon. Examples of lotteries are many of the U. S. state lotteries [some of which are compound ones (see §1.1.6.5)], roulette, and any of the casino dice or card games. Because we are modeling money as a set (often idealized as an interval) of real numbers, it is possible, but not necessary, to view a first-order lottery as a probability distribution over that numerical set, i.e., as a *random variable.* Lotteries are often modeled in this fashion and, so far as I know, no difficulties are encountered provided one limits attention to first-order ones. But as we shall see (§§ 1.1.6.6, 2.3.1, and 2.4.3), extending this random variable formulation to second-order lotteries has very serious drawbacks, and as a result I strongly discourage using it in formulating general theories.

1.1.5 Gambles

The generic term *gamble* is used here to cover both risky and uncertain alternatives. Thus, a lottery is a special case of a gamble. [Other terms have been suggested: "act" (Savage, 1954) and "prospect" (Kahneman & Tversky, 1979) are the most common.] The set of gambles under consideration is denoted $\mathcal{G}$, sometimes with subscripts and/or superscripts to denote particular subclasses of gambles. It is convenient always to treat the set of certain consequences C under consideration as being a subset of $\mathcal{G}$. Ignoring timing differences about the resolution of gambles, as I do, the certain consequences can be viewed as arising either from degenerate gambles where the same consequence is attached to all outcomes of the chance experiment or from degenerate ones in which the consequence is assigned to the certain event, which of course occurs with probability 1 (see the first two definitions of § 2.1).

1.1.6 Alternative notations and representations of gambles

1.1.6.1 Savage's states of nature: Most economists today follow, more or less explicitly, the convention introduced by Savage (1954) that the uncertain or chance aspect of the situation is described as a universal set Ω, whose elements (or outcomes) are called *states of nature.* A gamble (he called it an *act*) is a function $g : \Omega \to C$, where if Ω is infinite then g has *finite support*[1] in the sense that its image in C is a finite subset of C. When Ω is finite, this formulation often is placed in matrix form where the columns are labeled by the states of nature $s_1, ..., s_j, ..., s_n$, the rows by the gambles $g_1, ..., g_i, ..., g_m$, and the i, j entry, which is denoted g_{ij}, is the consequence assigned to g_i when the state of nature turns out to be s_j:

$$\begin{array}{ccccccc}
 & s_1 & \cdots & s_j & \cdots & s_n \\
g_1 : & g_{11} & \cdots & g_{1j} & \cdots & g_{1n} \\
\vdots & \vdots & \vdots & \vdots & \vdots & \vdots \\
g_i : & g_{i1} & \cdots & g_{ij} & \cdots & g_{in} \\
\vdots & \vdots & \vdots & \vdots & \vdots & \vdots \\
g_m : & g_{m1} & \cdots & g_{mj} & \cdots & g_{mn}
\end{array}$$

Although this notation is very compact and an elegant theory can be formulated in terms of it, it has three crucial drawbacks. The first is that Ω tends to be very large indeed because very often multiple chance experiments are involved in decisions. For example, if one is considering a trip from New York to Boston, there are a number of ways that one might go. Probably the primary ones that most of us would consider are, in alphabetical order, airplane, bus, car, and train. Each of these instances of travel can be thought of as a chance "experiment," and so there will be four different ones under consideration. And certainly when we run laboratory experiments to study how people behave when choosing among gambles, often a number of distinct chance experiments must be considered. In the travel example, let A, B, C, and T denote, respectively, the spaces of possible outcomes of the airplane, bus, car,

[1] Theories for infinitely many consequences are possible, but I do not focus on them.

and train trips from New York to Boston. In each case it is not difficult to think of several reasonable outcomes having to do with the success or not of the travel by that mode, and in the successful cases the time of arrival can be partitioned into some units, such as half hours, and in the unsuccessful ones into various aspects about the severity of the breakdown or crash. It is certainly not unreasonable to suppose that each mode of travel entails, as a bare minimum, at least 10 distinct outcomes. To place this simple decision situation in the Savage framework we must set $\Omega = A \times B \times C \times T$, and so there are at least 10,000 states of nature. Make the problem a bit more complex and it is easy to see that millions or billions of states must be contemplated. I think very few of us are able or willing to structure decisions in this fashion. Rather, we contemplate each of the alternatives as something unitary.

The second drawback with the Savage framework is that it has some difficulty in talking about experiments that might be carried out, but in fact are not. So, in the travel example, the experiments A, B, and T occur whether or not the decision maker decides to use one of these modes of travel, but in general that is not true for C, especially if he or she would be the driver. This control by the decision maker of what experiments are actually realized is somewhat awkward in the Savage framework, although it can be dealt with by careful definition of the acts. It is dealt with far more naturally in the scheme we shall use because the events of one experiment never appear in the formulation of a different experiment.

The third drawback is that it is difficult, although again not impossible, to talk about compound gambles, and these will play a role in, primarily, Chapter 3. Moreover, they are not uncommon in ordinary decisions. Consider, as I often do, travel from Santa Ana (SNA or more popularly Orange County or John Wayne Airport), California to the East Coast, say, Washington. Because it is usually most convenient for me to go into National Airport (DCA) and because both DCA and SNA have relatively short runways, the range of fully loaded aircraft originating at either field is limited and so a refueling stop (and usually a change of planes) is needed. The most common option is to make a connection somewhere in the middle of the country, such as Dallas-Forth Worth (DFW). Such a trip is then a compound gamble in which the first flight from SNA to DFW is a gamble, one of whose consequences—the desired and most likely one—is that one will connect to the intended flight from DFW to DCA. Note that once one considers compound gambles the number of columns of the Savage matrix grows even more. Indeed, the number will have to be countably infinite in order to describe elements of $\mathcal{G}$ as I have defined it.

So, I will attempt to introduce a different formulation, one designed to capture explicitly the unitary character of uncertain alternatives.

1.1.6.2 Chance experiments: Let $\mathcal{E}$ denote a set of chance experiments that will underlie the gambles under consideration. For example, one experiment might be the toss of a pair of coins, another the spinning of a roulette-like wheel, and the third the draw of a ball from an opaque urn having some unknown composition of red and white balls. Each chance experiment, $\mathrm{E} \in \mathcal{E}$, has its own (universal) set Ω_{E} of its possible outcomes. So, for example, in the case of tossing the two coins it is the set of pairs $\{(H,H),(H,T),(T,H),(T,T)\}$, where H means a head occurs and T a tail and the order distinguishes the coins. In the spin of a roulette-like wheel it is the separately identified sectors. Or in the case of the travel example, the "experiments" would be trips of each of the four types mentioned.

To reduce the notation a bit, I will often write $E = \Omega_{\mathbf{E}}$, thus using the same letter for both the name of the experiment and the set of possible outcomes of a realization of the experiment, but place the former in bold face. Let $\mathcal{E}_{\mathbf{E}}$ denote a family of subsets of E, the set of possible outcomes of experiment $\mathbf{E}$. We suppose $\mathcal{E}_{\mathbf{E}}$ exhibits the following basic properties:

(i) $E \in \mathcal{E}_{\mathbf{E}}$.

(ii) If $C \in \mathcal{E}_{\mathbf{E}}$, then $\overline{C} \in \mathcal{E}_{\mathbf{E}}$, where $\overline{C} = E \backslash C$ is the complement of C relative to the set E.

(iii) If $C, D \in \mathcal{E}_{\mathbf{E}}$, then $C \cup D \in \mathcal{E}_{\mathbf{E}}$.

Note that it follows immediately that $\emptyset = \overline{E} \in \mathcal{E}_{\mathbf{E}}$ and if $C, D \in \mathcal{E}_{\mathbf{E}}$, then $C \cap D = \overline{\overline{C} \cup \overline{D}} \in \mathcal{E}_{\mathbf{E}}$. Such a family of subsets of a set is called an *algebra of sets*.[2] When E is finite, there is no loss in generality in assuming $\mathcal{E}_{\mathbf{E}}$ to be the set of all subsets of E, which is called the power set of E and is denoted 2^E. The members of $\mathcal{E}_{\mathbf{E}}$, i.e., (some) subsets of E, are called *events*.

For much of this chapter and the next, it suffices to work with a single $\mathcal{E}_{\mathbf{E}}$. But beginning in Chapter 3 we will be considering how several experiments relate to one another. From a purely mathematical point of view, one can think of all the experiments we wish to consider as being imbedded in master experiment $\mathbf{\Omega}$, much as in the Savage approach. That is to say, there is a single set Ω of all possible outcomes, and each chance experiment $\mathbf{E}$ we wish to consider has as its set of outcomes $E \subset \Omega$. Thus, the decision maker's isolation of E for special consideration is a form of conditionalization.

One must, however, be very cautious in following this approach. For example, it is not plausible to think of the experimenter's selection of $\mathbf{E}$ for use in a laboratory experiment or a traveler's choice of going by aircraft, bus, car, or train as in any sense a chance event governed by some master statistical structure over $\mathcal{E}_{\mathbf{\Omega}}$. A choice of when and how to travel differs deeply from the statistical risks entailed by that choice. The potential for confusion in trying to treat them in a unitary fashion is so great that I eschew this perhaps mathematically more elegant approach.

1.1.6.3 First-order gambles: Let $\mathcal{C}$ denote the set of certain consequences under consideration. For $\mathbf{E} \in \mathcal{E}$, let $\{E_1, ..., E_i, ..., E_n\}$ denote a partition of E $(= \Omega_{\mathbf{E}})$, i.e., for $i, j \in \{1, 2, ..., n\}$ with $i \neq j$, $E_i \in \mathcal{E}_{\mathbf{E}}$, $E_i \cap E_j = \emptyset$, and $\bigcup_{i=1}^{n} E_i = E$. Note that because the outcome of running $\mathbf{E}$ is exactly one outcome, i.e., one element of E, it follows that exactly one of the E_i occurs. I do not assume that, for each i, $E_i \neq \emptyset$ because on occasion such degeneracy is useful, e.g., Definition 2.1.2.

Suppose $\{E_1, ..., E_i, ..., E_n\}$ is a partition of the experiment $\mathbf{E}$. Many authors treat a *first-order* (or *simple*) gamble g as an assignment of a consequence to each event in the partition, i.e., a function

$$g : \{E_1, ..., E_i, ..., E_n\} \to \mathcal{C}. \tag{1.1}$$

[2] For any countable set of elements $E_i \in \mathcal{E}_{\mathbf{E}}$, $i = 1, 2, ...$, if in addition we have the property that $\bigcup_{i=1}^{\infty} E_i \in \mathcal{E}_{\mathbf{E}}$, then one calls the algebra $\mathcal{E}_{\mathbf{E}}$ a *σ-algebra*. We do not need this concept in the present formulation.

Often it is convenient to denote $g(E_i) = g_i$. Frequently such functions are displayed explicitly in the following fashion. Let $g_i = f(E_i)$, and then the function is a collection of such pairs (g_i, E_i), i.e.,

$$\{(g_1, E_1), (g_2, E_2), ..., (g_n, E_n)\}. \tag{1.2}$$

This mathematical model of a gamble is mathematically neat, but it does not really work as the basis of a theory of behavior for a very simple reason. A function can be presented in very many different formats, but people do not always perceive these mathematically equivalent formats as equally valuable. During the 1980s, psychologists became increasingly aware of these problems, which go under the generic name of *framing effects* (Tversky & Kahneman, 1986). So we must use a notation for gambles that is helpful in letting us formulate when such framing effects occur and, importantly, when they do not. I refer to the class of properties that say certain framings do not matter as "accounting indifferences" (§2.1).

A notation that imposes an ordering of the pairs in Display (1.2) has turned out to be useful in formulating theories. Thus, the gamble is thought of as an array of n ordered pairs. The ordering we encounter repeatedly arises from the assumption, discussed in some detail in Section 1.2 and a great deal more in Chapter 2, that there is a preference ordering over the consequences of gambles. The convention often followed is that the subscripts increase with decreasing preference of the associated consequences, i.e.,

$$g_i \text{ is preferred to } g_j \Longleftrightarrow i < j.$$

When emphasizing the order, typically we replace the commas between pairs by semicolons, drop the parentheses, and change from the set notation to an ordered n-tuple one, yielding

$$(g_1, E_1; ...; g_i, E_i; ...; g_n, E_n). \tag{1.3}$$

Although the notation of (1.3) is convenient for much of what we will be doing, one must recognize that it is different from the function of (1.1), because order plays no role in the concept of a function. No distinction exists between $\{E_1, E_2, E_3\}$ and, say, $\{E_2, E_1, E_3\}$, whereas in the ordered 3-tuple notation it is not automatic that

$$(g_1, E_1; g_2, E_2; g_3, E_3) \quad \text{and} \quad (g_2, E_2; g_1, E_1; g_3, E_3)$$

are perceived to be the same. We will introduce assumptions about preferences to the effect that the order is irrelevant for some of the simpler gambles while allowing it to be relevant for more complex ones.

Furthermore, issues of order arise in empirical studies because, necessarily, the several terms of a gamble must be presented in some ordered fashion. This aspect is discussed some in Section 1.1.7.

For risky alternatives, where there are known probabilities of the event occurring, and when it seems reasonable to assume that only these probabilities matter to the decision maker, then the notation is changed as follows. Let $p_i = \Pr(E_i)$ and Display (1.3) becomes

$$(g_1, p_1; ...; g_i, p_i; ...; g_n, p_n). \tag{1.4}$$

The notation for uncertain alternatives, Display (1.3), described above, which is what I will use in this monograph, is a variant on the notations first used by Herstein and Milnor (1953), von Neumann and Morgenstern (1947), and Pfanzagl (1959). It is substantially different from the one most commonly used by economists today, which was described in Section 1.1.6.1. It attempts to capture explicitly the fact that we deal with uncertain alternatives as unitary things contingent on the underlying chance experiment being carried out.

1.1.6.4 Binary gambles: In the case of first-order binary gambles, i.e., $n = 2$, with $x, y \in C$, it is often convenient to drop the subscripts and to abbreviate the notation even further to $g_1 = x$, $g_2 = y$, $E_1 \doteq C$, and $E_2 = \overline{C} = E \backslash C$. So the first-order, binary gamble becomes in several alternate notations

$$(x, C; y, E \backslash C) \equiv (x, C; y, \overline{C}) \equiv (x, C; y). \tag{1.5}$$

Some authors abbreviate it even further by omitting all punctuation, so it reduces to

$$xCy, \tag{1.6}$$

where one thinks of the event as a (mixture) operation between the consequences x and y, much as with numbers α and β we write the operation of addition as $\alpha + \beta$. In the case of risk, for $p \in [0, 1]$, these brief notations become xpy.

Perhaps the least satisfactory notation is the initial one of von Neumann and Morgenstern (1947), who wrote the risky gamble as $px + (1-p)y$. The potential for misunderstanding both the $+$ and the "multiplication" px is simply too great, and so no one has used this notation subsequently except when treating consequences as random variables (see §1.1.6.6 below).

I use Display (1.5) in the binary case and Display (1.3) in the general case except for risky alternatives, in which case I use Display (1.4).

1.1.6.5 Compound gambles: Let $\mathcal{G}_0 = C$ and let $\mathcal{G}_1$ denote the union of the set of first-order gambles and $\mathcal{G}_0$. We may define $\mathcal{G}_2$ as consisting of all assignments from an (ordered) finite event partition into $\mathcal{G}_1$. Thus, $\mathcal{G}_2 \supset \mathcal{G}_1$. Note that all of the experiments under consideration go into constructing these new gambles. One can continue this process recursively as follows: $\mathcal{G}_k$ is the set all assignments from an (ordered) finite event partition into $\mathcal{G}_{k-1}$. Of course, $\mathcal{G}_k \supset \mathcal{G}_{k-1}$. Any gamble in $\mathcal{G}_k \backslash \mathcal{G}_{k-1}$ is referred to as k^{th}*-order gamble*. Any gamble that is not a first-order one is called a *compound gamble*. For the most part we will confine our attention to first- and second-order gambles. Although all of the theories have formulations in terms just of certain alternatives and first-order gambles, it often becomes much simpler if second-order ones are admitted. An example will arise when we study separability in Section 3.4, Eq. (3.20).

Finally, let $\mathcal{G}_\infty$ denote the full countable recursion, i.e., $\bigcup_{k=0}^{\infty} \mathcal{G}_k$. From a purely mathematical perspective, for $\mathcal{G}_\infty$ one can omit explicit mention of the set C of certain consequences and simply begin with a set $\mathcal{G}$ of gambles and require that it be closed under the formation of gambles using elements of $\mathcal{G}$ as consequences. This was how von Neumann and Morgenstern (1947) formulated binary gambles. Because in this monograph, except for relatively incidental comments, we need only $\mathcal{G}_0 = \mathcal{C}$, $\mathcal{G}_1$, or $\mathcal{G}_2$, it is better to use the explicit inductive definition. The use of second-order gambles is similar to the modeling found in Anscombe and Aumann (1963) and Schmeidler (1989).

When dealing with compound gambles, the notation of Display (1.3) is used but with the g_i interpreted as gambles, not certain consequences.

In the case of binary gambles in $\mathcal{G}_\infty$, the operator aspect has sometimes been made more conspicuous by using a notation such as

$$g \otimes_C h, \tag{1.7}$$

where g and h are gambles. Since $g \otimes_C h$ is itself a gamble, this means $\otimes_C$ is a binary operation. I do not use this notation here. When the operation is defined only for $g, h \in \mathcal{G}_{k-1}$, then it is a partial operation.

For risky alternatives, such compounding of gambles at the level of the probabilities does not seem problematic. But for uncertain ones, it may be. The reason is that some of the properties we will examine depend upon compounding gambles based on independent replications of the same underlying experiment. Here "independent replications" is used as a primitive undefined concept; it cannot in general be formulated in probability terms because, by assumption, such probabilities are unknown. Indeed, replication many times is contrary to uncertainty because relative frequencies can be used to estimate probabilities, thus transforming the problem into one of risk. In practice, of course, even statistical independence, such as in selecting a random sample, is often based on procedures, such as repeated tosses of a coin, not on actual probability estimates. For example, in the utility literature, where experiments are realized by some device such as choosing one ball from an urn of balls of several different colors, independence is typically achieved by replacing the ball, re-randomizing by thoroughly shaking the urn, and drawing a second time.

But in many situations of uncertainty, such replication simply is not possible. For example, if the consequences of an investment in Ukraine vary as a function of whether or not the country is at war in the next 10 years, that uncertain event obviously cannot be replicated. So, to a degree, the assumption of compounding a particular uncertain experiment is at variance with the assumption of uncertainty. For example, as Peter Wakker has noted,[3] if one is confronted with a single toss of an unknown coin, it is plausible to assume that the faces are equally likely to arise, despite the fact one may feel great uncertainty about that hypothesis. However, after 1,000 tosses in which heads always comes up, one's hypothesis on the next toss is likely to be very different from what it was initially.

The problem of independent realizations of uncertain events is mitigated to a degree, but not entirely, by the fact that the only compounding that we will deal with is second order—those gambles whose consequences are either certain ones or first-order gambles.

1.1.6.6 Compound lotteries and random variables: As was noted earlier, many economists model lotteries, first-order and compound, as random variables, which are first-order by definition. This formulation has, depending upon how one views it, the advantage or dis-

[3] Personal communication, April 13, 1998.

advantage of automatically reducing any compound lottery to an equivalent first-order one.

So, to model lotteries as random variables one must either (i) avoid compound gambles or (ii) assume a probability reduction to its equivalent first-order form. I take up both points.

(i) Some researchers may claim that they do not work with compound lotteries, but such claims tend to be violated regularly. Frequently, one sees decision trees of the sort described below in Section 1.1.7, and these can only be viewed as compound lotteries. And certainly the discussions of what is called the "independence axiom" (§ 2.3.1) implicitly require second-order lotteries. So, I do not believe it is realistic to assume that such compound lotteries can be avoided.

(ii) Assuming that some compound lotteries are involved, then modeling them as random variables introduces the following problem. Random variables are defined only in terms of a distribution over ultimate (in this case, money) consequences, so the compound lottery

$$((x,r;y),p;(z,s;w)),$$

where $p,r,s \in [0,1]$, automatically reduces to its probabilistically equivalent first-order form

$$(x,pr;y,p(1-r);z,(1-p)s;w,(1-p)(1-s)).$$

Typically, the notation used in such a case is

$$\begin{aligned} p(x,r;y)+(1-p)(z,s;w) &= p(rx+(1-r)y)+(1-p)(sz+(1-s)w) \\ &= prx+p(1-r)y+(1-p)sz+(1-p)(1-s)w. \end{aligned}$$

To be more concrete,

$$((\$100,.30;0);.20;(\$50,.60;-\$20))$$

automatically reduces to:

$$(\$100,.06;\$50,.48;0;.14;-\$20,.32),$$

where the consequences are listed in descending order.

However, it is by no means clear that people necessarily perceive these two descriptions of the situation as the same—indeed, we will see evidence strongly suggesting that, in general, they do not. Yet, modeling the domain as random variables simply precludes the option of the two descriptions being treated as non-equivalent. I much prefer to make such a reduction assumption explicit as an axiom rather than build it into the initial formulation of the decision situation. von Neumann and Morgenstern (1947) did exactly this. Such assumptions are called "reduction (of compound gambles) axioms." They are discussed below.

1.1.7 Empirical realizations of gambles

The possible mathematical notations for gambles are far fewer than the potential ways of describing gambles to people in experimental settings. I will mention only the most commonly used presentations.

- Present the gamble more or less explicitly as text:

  There is a 60% chance of gaining \$50, a 30% chance of losing \$10, and a 10% chance of losing \$100.

- Or present it in one of the mathematical notations, e.g.:

$$(\$50, 0.60; -\$10, 0.30; -\$100, 0.10)$$

or

$$\begin{pmatrix} 0.60 & 0.30 & 0.10 \\ \$50 & -\$10 & -\$100 \end{pmatrix}$$

- Or as a tree diagram such as shown in Fig. 1.1:

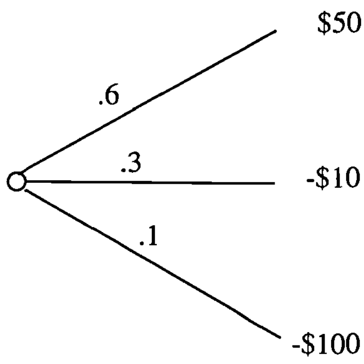

Figure 1.1. Representation of a gambles as a tree diagram.

- Or as a pie chart such as shown in Fig. 1.2:

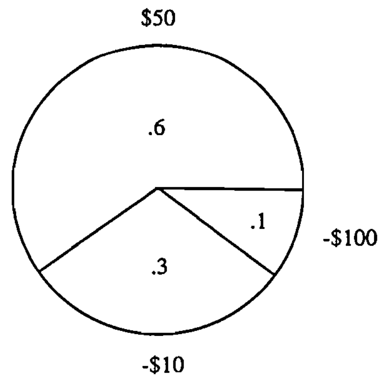

Figure 1.2. Representation of a gamble as a pie diagram.

- Or as a cumulative distribution function such as shown in Fig. 1.3:

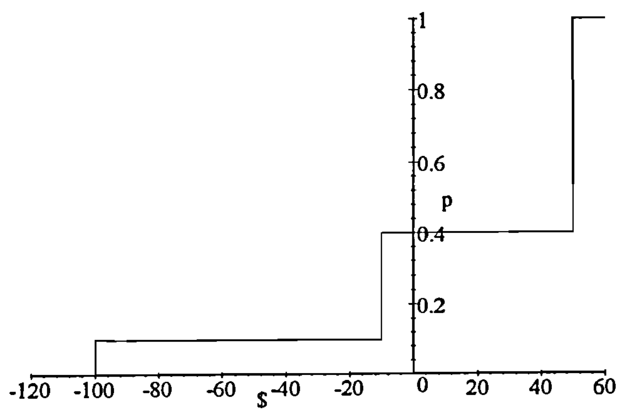

Figure 1.3. Representation of a lottery as a cumulative distribution function.

Many other modes are possible; see Keller (1985b) for a study in which several alternatives were examined in connection with her work on the hypothesis of the reduction of compound gambles (§ 3.4). In each of the presentation modes shown, several additional options exist, e.g., the order in which the outcomes are listed, the orientation of the display, color coding, etc. None of these things should matter, but probably they all do to some minor degree, as Keller found in her work. One of the realities of empirical behavioral research is that experimenters have not arrived at a consensus about how best to represent the gambles, and equally competent experimenters will disagree on procedural details. In describing experiments, I will usually mention the type of display used.

1.2 Preference and Its Determination

1.2.1 Choice and its problems

1.2.1.1 The algebraic view: Because we are presuming that the consequences and gambles under consideration are valued—either positively or negatively—by the decision maker, we can think of there being relative values between any two of them. If one gamble is seen as more valuable than another, it seems reasonable to assume that, in choosing between them, the more valued one will be selected, and conversely. In this literature, this is often thought of as an operational definition of *preference* or, as it is sometimes called, *revealed preference* (Samuelson, 1948; Kreps, 1990).

There is a subtle and complex philosophical literature on the meaning of preference; see, e.g., McClennen (1990). Here I treat it in a highly operational fashion; even so, as we shall see, ambiguity remains. When a person assigns numerical evaluations to gambles, we do not necessarily find that the ordering of the gambles induced by these numbers always agrees with the persons's ordering of them by choice (see § 2.2.5).

Let $\mathcal{G}$ be the set of gambles under consideration, usually $\mathcal{G}_2$. So, if $g, h \in \mathcal{G}$, then we assume that either g is seen as preferred to h, which we denote by $g \succ h$; or h is seen as preferred to g, denoted, of course, $h \succ g$ (or equally well by $g \prec h$); or g and h are seen as indifferent in preference, denoted $g \sim h$.

Often it is convenient to work with the relation[4] of "more or equally valued," i.e., $\succsim$ means either $\succ$ or $\sim$ holds, which relation is often called *weak preference*. Note that $\succsim$ is a *connected* or *complete relation* in the sense that for each $g, h \in G$ either $g \succsim h$ or $h \succsim g$ (or both, in which case $g \sim h$). This observation rests, of course, on the initial assumption that just one of the three possibilities, $g \succ h$, $h \succ g$, or $g \sim h$, occurs. Some authors have contemplated situations where for some pairs no response is, or can be, given to the question: Which of the pair do you prefer or are you indifferent? Such orderings are called "partial." Anand (1987) summarizes a number of the reasons for doubting that people always have existing or constructible preference orders among alternatives.

[4] A mathematical, binary relation $\succsim$ on a set $\mathcal{G}$ is simply a subset of $\mathcal{G} \times \mathcal{G} = \{(g,h) : g, h \in \mathcal{G}\}$. Rather than write $(g,h) \in \succsim$ to mean g is prefered or indifferent to h, we write $g \succsim h$, just as with numbers rather than $(x,y) \in \geq$ we write $x \geq y$. As we shall see, we will be interested in the special class of relations known as *orderings*. More about this later.

Mainly because the theory becomes appreciably more complex with partial orders, I will postulate only situations in which resolving the choice is always possible. Moreover, in most experiments respondents[5] are required to express preference or indifference and usually without providing a "no choice" option. Generalizations to theories with partial orders might well be useful for some applications where some consequences are seen as incomparable.

1.2.1.2 Are the respondents in experiments serious? A question repeatedly raised, especially by economists, is: Do respondents in our experiments take seriously the many choices they are forced to make when we try to elicit $\succsim$ over a somewhat large set of gambles? They could, after all, simply respond randomly or always choose the right-hand display or the like. One line of evidence that tends to impress psychologists is that we can rather easily spot those respondents whose responses exhibit total consistency, such as always choosing one response key, or total chaos, for which the data are patternless. Usually such data, which are in fact rare, are dropped from further consideration. Other respondents typically exhibit patterns of behavior of the sort described in Chapter 2 and elsewhere, which seems to suggest that at least some thought has been given to their choices.

Some theorists have expressed the concern that some of the observed anomalies reported in Chapter 2 may result from insufficient attention, which they believe can be focused by introducing substantial monetary consequences to be won or lost by the respondents. The evidence is to the contrary.[6] For example, in connection with the preference reversal phenomenon (§ 2.2.5), two studies were aimed at getting rid of it by making the monetary outcomes serious. Perhaps the most striking was the study of Lichtenstein and Slovic (1973) in which the phenomenon was exhibited by ordinary gamblers in Las Vegas playing for high stakes. Grether and Plott (1979) carried out a series of systematic manipulations in an attempt to eliminate preference reversals, including increasing the monetary stakes, and again they found the phenomenon robust. Comparable robustness with the magnitude of consequences was reported by Kachelmeier and Shehata (1992) when they carried out studies in China using money amounts that, although modest by western standards, amounted to several months' salary for the Chinese respondent. [See also Hsee and Weber (1999) and Weber and Hsee (1998).] Camerer (1989) reported no significant difference between having respondents play some gambles at the end of the experiment and not doing so; however, the stakes involved were modest.

In many, but by no means all, of the empirical studies I shall mention, the experimenter randomly selected some of the choice pairs presented and then, after all choices were completed, ran off those gambles selected by the respondent. The outcomes of these chance experiments determined a financial payment beyond the respondent's assured hourly rate for participating in the experiment. Everyone agrees that any such running of gambles should be carried out

[5] It is common to speak of the people from whom judgments and choices are solicited as "subjects." That term, it seems to me, makes sense when these people are subjected to experimental manipulations, such as number of trials of exposure to a stimulus in a memory experiment, but it is less appropriate when we are attempting to elicit some information about what the person feels or thinks. Better terms seem to be either "informant," "participant," or "respondent." I have elected to use the last term.

[6] This comment applies to the anomalies discussed here and does not cover others in the literature. Some of the latter do seem to attenuate, and in some cases evaporate, when real payoffs are used.

after all of the choices have been made in order to avoid the possibility of varying endowment effects, of a changed sense of status quo from choice to choice.

1.2.1.3 Context inconsistencies: MacCrimmon, Stanbury, and Wehrung (1980) made a case against the assumption of consistent preferences. Their idea was simple: Devise two sets of five lotteries that have two lotteries in common and that each involves only changes in one of the variables. The sets they chose are shown in Table 1.1. Note that both subsets have the same first certain consequence and the same fourth lottery. The respondents, 40 middle-aged business executives, some presidents and CEOs and some mid-level executives of large companies, rank ordered each set by preference under a scenario, designed to capture their attention, involving stock market behavior. Thirteen of the 40 respondents had opposite orderings of the common alternatives, the first and the fourth, in contexts B and C. The specific pattern is shown in Table 1.2.

Table 1.1 Binary lotteries used by MacCrimmon, Stanbury, and Wehrung (1980) to show context effects. It is part of their Table 9.1. The probability of the worst outcome = 1— probability of best outcome. Note that B1 = C1 and B4 = C4.

Lottery	Best Outcome	Probability of Best Outcome	Worst Outcome
B1	$ 5.00	1.0000	$ 5.00
B2	20.00	.0692	3.90
B3	20.00	.2752	-.70
B4	20.00	.6185	-19.30
B5	20.00	.9046	-137.00
C1	$ 5.00	1.0000	$ 5.00
C2	10.00	.6185	-3.10
C3	15.00	.6185	-11.20
C4	20.00	.6185	-19.30
C5	25.00	.6185	-27.40

Thus, for these respondents, apparently the context had partially controlled their preferences, which were otherwise quite coherent within each set. This finding, if the data were correctly interpreted, is disturbing. The issue, as we shall discuss in the next subsection, is that choices in this area are notoriously noisy, and no attempt was made to determine if that could explain the findings.

Although there is certainly reason for concern, I will act here as if the invariant preference approach makes sense at least for judgments between pairs of gambles.

Table 1.2. The numbers of respondents exhibiting each choice pattern in MacCrimmon et al. (1980).

		Context C		
		1>4	4>1	Total
Context	1>4	23	3	26
B	4>1	10	4	14
	Total	33	7	40

1.2.1.4 Inconsistent choices: A major difficulty with the algebraic formulation of preferences and choices is that the empirical world does not seem to be so simple. If, during the course of an experiment, we present a particular pair (g, h) several times, but each presentation is some tens of trials from its predecessor, we do not necessarily find that one gamble is consistently chosen over the other. Such inconsistency seems to occur even when experimenters go to great effort to keep the state of the respondent the same except, of course, for the fact that he or she will have encountered more total choices at the time of the later choice than the earlier one. Although this fact is unavoidable, it really should not matter.

Given that the observed choices are inconsistent, an obvious idea is to postulate not an algebraic relation $\succsim$ for choices but rather an underlying probability $P(g, h)$. There is a substantial literature on such probabilistic models of choice, and summaries of it may be found in Suppes, Luce, Krantz, and Tversky (1989, Ch. 17) and in the several articles of the issues of Volume 23, 1992, of *Mathematical Social Sciences.* That being so, why do I not use it here? The reason is simple if unfortunate. The stimuli, gambles in our case, are rather highly structured objects with components involving events and consequences, and our algebraic models will draw heavily on that structure. The difficulty is that no one has very successfully combined that type of structure with a probabilistic choice structure. Although there are continuing efforts in this direction (e.g., Marley, 1997a), the general lack of success is a major failing of the field—actually a failing with far broader implications than just for utility measurement. Given this failure, we have to adapt as best we can.

The following approach is taken by many experimentalists. Estimate $P(g, h)$ from repeated presentations of the pair (g, h) and then define

$$g \succsim h \iff P(g,h) \geq \frac{1}{2}. \tag{1.8}$$

As we shall see (§ 2.2.3), using this definition and checking properties of $\succsim$ in terms of it can be a fairly subtle matter, and significant mistakes appear to have been made in the literature.

1.2.2 Matching and its problems

If one is going to have to use Eq. (1.8) to determine $\succsim$, it clearly becomes expensive and demanding on the respondents to do so. Suppose the experiment involves m gambles, then $\frac{1}{2}m(m-1)$ pairs have to be examined. If, to estimate any one $P(g,h)$, we make k separate presentations, then a total of $\frac{1}{2}m(m-1)k$ observations are required. In other areas of psychology, such as psychophysics, k is rarely less than 100 and sometimes samples are as large as 1,000. If $m = 10$, which is actually a very modest stimulus set, and $k = 100$, we see that 4,500 observations are required. In the kinds of experimental procedures commonly used in decision making, a choice on average every 15 seconds is rather fast, i.e., 240 per hour, which comes to almost 19 hours as a minimum. That is a lot of sessions for what really is a very simple experiment. So one must seek more efficient methods. The basic idea is to select some one-dimensional subset and use that to evaluate the value of gambles.

1.2.2.1 Certainty equivalents: The most obvious idea for evaluating a gamble g is to establish its *certainty equivalent*, which is defined to be that amount of money, $CE(g) \in \mathcal{R} = \mathcal{C} \cap \mathbb{R}$, such that

$$CE(g) \sim g. \tag{1.9}$$

This is a "match" between money and the gamble. Because preference between CEs, i.e., money, is expected to agree with the numerical order, i.e.,

$$CE(g) \succsim CE(h) \Longleftrightarrow CE(g) \geq CE(h),$$

this device reduces the problem from determining $\frac{1}{2}m(m-1)$ to m values—in our example from 45 to 10. Of course, other one-dimensional valued attributes can be used instead of money.

Now, if we can also reduce k substantially, the whole issue of evaluating preferences becomes vastly simpler. The lower limit of replications, $k = 1$, is achieved very simply by having a respondent tell us just once his or her CE for each gamble, and such estimated CEs are called *judged certainty equivalents*, denoted JCEs. There are at least three difficulties with this solution.

- First, given the fact that choices are not stable, we can hardly expect JCEs to be. The only data that I know of on repeated judgments of CEs for the same gamble are the repeated pairs of judgments of von Winterfeldt, Chung, Luce, and Cho (1997) and Weber and Hsee (1998). As one would expect, they exhibit substantial variability. A systematic study of the distribution of such responses has not been conducted.[7]
- Second, and more subtle, is the fact that JCEs do not appear always to agree with estimates of CE based on at least some choice procedures or, for that matter, with choices themselves. The latter point is carefully explored by Tversky, Sattath, and Slovic (1988). Whichever procedure—judgments or choices—one believes provides basic preference information, the other must be considered to be biased. This difficulty is encountered later (§§ 2.2.5 and 2.3.5).

[7] Clearly, the judgments about one gamble would have to be fairly widely separated by trials on which other gambles are presented so that the subject does not simply recall what he or she said earlier.

- Third, however CEs are estimated, comparing a gamble to an amount of money rather than to another gamble may induce a systematic effect because people respond very differently to certainty than to uncertainty, e.g., due to some utility of gambling. This, if correct, again may mean that CEs give biased estimates of the worth of gambles. I will deal with such bias issues as they come up. For now, it is sufficient to recognize that if CEs can be determined in some reasonably efficient way, they provide a very efficient way for indirectly evaluating choices.

1.2.2.2 Probability equivalents: Another method sometimes used is to hold the consequences (usually, amounts of money) x, y fixed, with y in the interval bounded by 0 and x, and to ask for a probability equivalent PE such that $y \sim (x, PE; 0)$. Depending upon whether the PE is a judged value or is determined by some sort of choice procedure, it is called a JPE or CPE. Note that PEs are not useful in ordering pre-chosen gambles in the way CEs are, but as we will see in Section 6.3.4, they can be very useful in other ways.

1.2.2.3 Consistency of certainty and probability equivalents: Hershey and Schoemaker (1985) raised the question whether judged certainty equivalents and judged probability equivalents are consistent in the following sense. Suppose one first determines $y = CE(x,p;0)$ and then the PE such that $y \sim (x, PE; 0)$. The responses are called *consistent* if $PE = p$. Equally well, one can begin with a given $x > y > 0$ or $x < y < 0$ and ask for PE such that $y \sim (x, PE; 0)$ and follow that with a CE such that $CE \sim (x, PE; 0)$. Here consistency means $CE = y$.

To study this empirically they obtained judged responses from 147 undergraduate students who had been exposed to expected utility theory in a business course. Eighty-three answered questions involving gains, of which 41 were asked to make the judgments in the order CE-PE and 42 were given the order PE-CE. The other 64 respondents were split equally between the CE-PE and PE-CE versions for losses. The two types of judgments were made a week apart. The values of y were \$100, \$500, \$1,000, and \$5,000, with the corresponding values of $x = 2y$. The losses were the corresponding negative values. The questions were presented on paper with text descriptions of the gambles and task

The data exhibited two notable features. First, there were huge individual differences. In particular, estimated utility functions exhibited all four combinations of concavity and convexity for money gains and losses. Second, the consistency hypothesis—that the points fall on the diagonal—failed in systematic ways as shown in Fig. 1.4. Without going into detail, the authors considered six possible hypotheses about what might be causing the inconsistency. Among these, they suggest that perhaps the respondents tended to recast the judgments as follows. Suppose that when asked to report JPE such $y \sim (x, JPE; 0)$, the respondents actually did not make this judgment directly, but rather recast it as the judgment $0 \sim (x - y; JPE; -y)$. This account, if true, probably destroys any chance of consistency because, as we shall see, under certain reasonable assumptions to be introduced later, gambles of mixed gains and losses are evaluated differently from gambles of just gains (compare Chapters 4, 6, and 7). Indeed, this proposal is formally the same as treating y as a buying price as developed Sections 6.4 and 7.4, and as we shall see the resulting formulas are complex. In the next subsection, other data will cast doubt on this explanation.

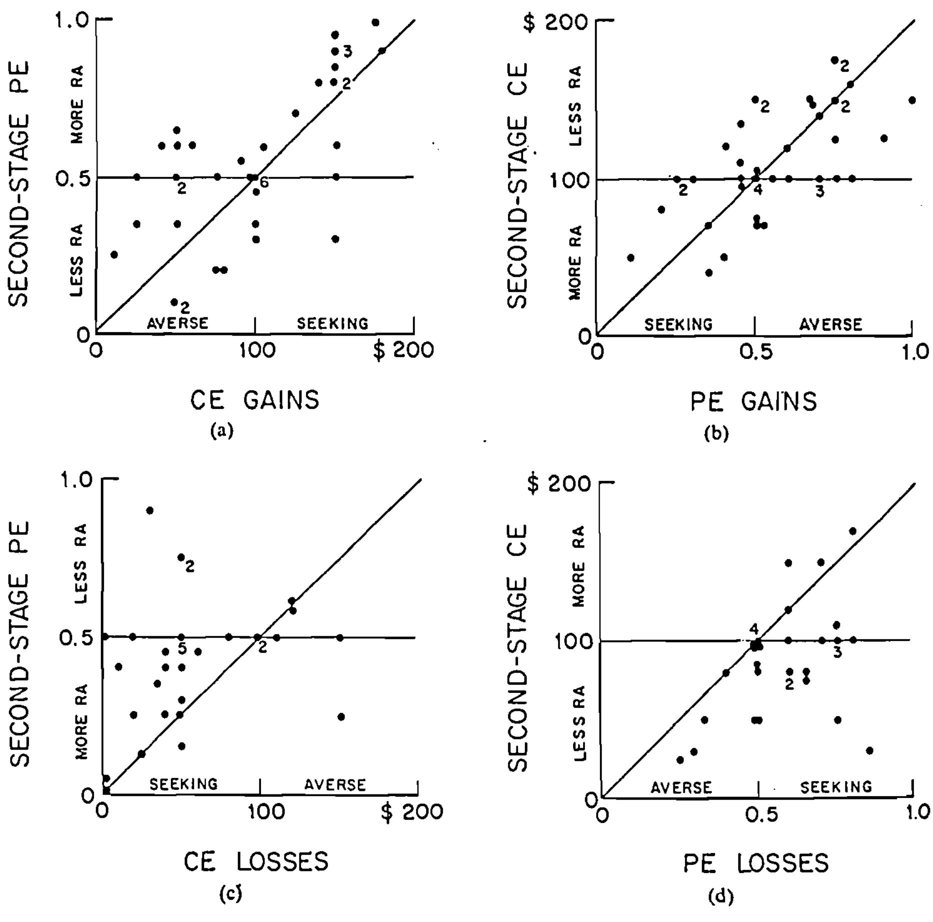

Figure 1.4. Failure of estimated CEs and PEs to be consistent. This is Fig. 1 of Hershey and Schoemaker (1985). Reprinted by permission, Hershey and Schoemaker, "Probability versus certainty equivalence methods in utility measurement: Are they equivalent?" *Management Science*, Volume 31, Number 10, October 1985, 1218. Copyright 1985, The Institute of Management Science (currently INFORMS), 901 Elkridge Landing Road, Suite 400, Linthicum, Maryland 21090-2909, USA.

Johnson and Schkade (1989) ran a somewhat similar study, which agreed with Hershey and Schoemaker (1985) for gains but obtained biases opposite to theirs for losses.

Something the latter authors did not comment upon directly, which in 1985 was not so widely recognized as it is now, is the inconsistency of choice and judged certainty equivalents (see §§ 2.2.5 and 2.3.4). Presumably, a similar inconsistency may exist between choice and judged probability equivalents, but I do not know of any data on this issue.

In any event, it would be interesting to know whether or not choice-determined CEs and PEs are consistent. However, the results of the next subsection do not make one sanguine and, according to Peter Wakker,[8] the conclusion among those studying medical decision making is that the PE estimates are too high. This was also found in Wakker and Deneffe (1996).

1.2.2.4 Lottery equivalents: Many are uncomfortable about the asymmetry between a gamble and a certain sum of money involved in making CE and PE evaluations and have suggested that one should work only with lotteries. So, for example, one can be given p, q, x and ask for the solution LEO, which stands for lottery equivalent outcome, to the equivalence $(LEO, q; 0) \sim (x, p; 0)$, or given q, x, y one can ask for LEP, for lottery equivalent probability, to $(x, LEP; 0) \sim (y, q; 0)$. The LEO reduces to CE and LEP to PE when $q = 1$. The analogous consistency issue arises:

1. Find LEO and LEP, in that order, such that

$$(LEO, q; 0) \sim (x, p; 0) \quad \text{and} \quad (x, LEP; 0) \sim (LEO, q; 0),$$

then consistency means $LEP = p$.

2. Find LEP and LEO, in that order such that

$$(y, LEP; 0) \sim (x, p; 0) \quad \text{and} \quad (LEO, q; 0) \sim (y, LEP; 0),$$

then consistency means $LEO = x$.

The first study of such consistency was by McCord and de Neufville (1986), who used judged values and found inconsistencies. More persuasive is the very systematic study by Delquié (1993), who used a procedure that he described as a "converging sequence of choices." Presumably this is some variant of the PEST procedure described in the Appendix C. He ran five distinct experiments that involved between 10 and 47 MBA students except for one experiment with 38 engineering students. His conclusion was that they all "show, in matching objects $[X, Y]$, trading off X against Y is not equivalent to trading off Y against X" (p. 1390). The overall result is that in method 1 the LEP lies between p and q, rather than equaling p, and in method 2 the LEO lies between x and y, rather than equaling x.

Because these findings do not entail any certain consequences, they undermine Hershey and Schoemaker's (1985) explanation in terms of recoding the PE gamble into one of gains and losses. Moreover, in the domain of losses, the biases exhibited by Delquié's respondents agree, as noted earlier, with those of Johnson and Schkade and disagree with those of Hershey and Schoemaker. Delquié concludes that it is unlikely that a common explanation exists for both sets of biases. He discusses a number of possible sources for these effects, including a

[8] Personal communication, April 13, 1998.

form of statistical error propagation, but rules each out as a single source explanation.

The unsatisfactory conclusion that I draw is that we do not understand these effects, but there is little doubt they exist.

1.3 Possible Additional Primitives

So far, the underlying preference structure to be studied is $\langle \mathcal{C}, \mathcal{E}, \mathcal{G}, \succsim, e \rangle$, where $\mathcal{C}$ is a set of riskless consequences, $\mathcal{E}$ is a set of chance experiments, $\mathcal{G}$ is the set of gambles under consideration (often $\mathcal{G}_2$), $\succsim$ is a binary (preference) relation on $\mathcal{G}$, and $e \in \mathcal{C}$ is the status quo. This, or a variant of it such as Savage's (1954) system, is the classic framework for studies of decision making under risk and uncertainty, and much of the well-developed theory has been limited to just these primitives.

Within this framework it is, however, difficult to study preferences over certain consequences $\mathcal{C}$ in any self-contained fashion independent of the complexities of the gambles. Yet preferences over $\mathcal{C}$ seem, in a sense, far more basic than preferences over $\mathcal{G}$. To study the certain consequences by themselves, some other basic primitive appears to be needed. I know of four distinct and independent approaches. Of these, I will use only one, joint receipts. For completeness, however, let me make explicit the other three.

The first, worked out in considerable detail by Keeney and Raiffa (1976) and which has led to an extensive subsequent literature under the name "multiattribute utility theory," is to suppose that the elements of $\mathcal{C}$ are multidimensional in the sense that they have several attributes that can be combined in various ways. Most goods have distinct attributes that are of pertinence to the decision maker. For example, cars vary on many dimensions—type, color, horsepower, amenities of various sorts, etc. Focusing on the attributes permits an analysis using the measurement theory methods of conjoint analysis (see Krantz, Luce, Suppes, & Tversky, 1971, Chs. 6 and 7; Luce, Krantz, Suppes, & Tversky, 1990, Chs. 19 and 20; Michell, 1990, 1999; and Appendix D). To some degree, the 2-attribute theory is similar to joint receipts.

A second approach is to introduce an additional ordering primitive, $\succsim^*$, defined over $\mathcal{G} \times \mathcal{G}$ rather than over $\mathcal{G}$. It is interpreted as follows: For $g, h, g', h' \in \mathcal{G}$, $(g, h) \succsim^* (g', h')$ means that the decision maker judges that the "preference distance" or "interval" in going from g to h is at least as large as that in going from g' to h'. Of course for $h = h'$, one assumes that $\succsim^*$ restricted to the first component coincides with the preference ordering $\succsim$ on these gambles. Examples of such theoretical work on judged "strength of preference" are Alt (1936/1971), Barron, von Winterfeldt, and Fischer (1984), Bouyssou and Vansnick (1988), Dyer and Sarin (1982), and Keller (1985c).

Such an approach of comparing pairs provides a good deal of extra mathematical structure (Suppes, Krantz, Luce, & Tversky, 1989, Ch. 14), and it has been widely used in psychological studies of stimulus similarity. Nonetheless, to me, it seems rather artificial in the context of uncertain alternatives. When and where in ordinary life do people make such judgments, that gamble g is "closer to" h than is g' to h'? It is true that if you ask respondents in the laboratory to make such judgments, they will; but that may be an experimental snare and delusion. People are surprisingly flexible about doing unusual things for an experimenter even though they have had no experience in life with such judgments. So I am skeptical about the value of this approach.

It is certainly the case that we do make judgments of strength of preference such as considering whether good g is worth the extra cost above that of a lesser good h. As we shall see, such judgments can be recast naturally in terms of the primitive, joint receipts, that we shall investigate.

Third, within economics a good deal of attention has been devoted to aspects of time discounting and attempts to use it to infer something about the nature of preferences. As it stands, I view this more as a challenge to incorporate into the kind of work represented here. Some additional comments are made about it in Section 1.4.5.

The fourth approach, the one I adopt here, is to introduce a binary operation into the situation. I call it *joint receipt* and denote it by $\oplus$. For $g, h \in \mathcal{G}$, $g \oplus h$ is interpreted to mean having or receiving both g and h. Certainly such an operation is totally familiar; we encounter it daily. The mail brings bills and checks, holidays bring multiple gifts, purchases in the store leads to the joint receipt of goods, etc. Often, as these examples make clear, joint receipt involves more than two things. For all gains or all losses, no problem seems to arise in extending the theory to any finite number of jointly received goods because associativity is satisfied. But as we shall see in Chapter 6, the most common utility models for gambles are not consistent with associativity, which generates a difficulty. However, recent studies suggest that these utility models are empirically incorrect in the mixed case. Chapter 7 approaches the issue by assuming associativity of joint receipts and derives what that must mean for the utility of gambles. These models appear to be more satisfactory for a substantial majority of respondents.

Strength of preference can be recoded in monetary differences using joint receipt. For example, suppose g is preferred to h, and that they cost, respectively, x and y. Then the question of strength of preference becomes one of deciding if $g \oplus (-x)$ is or is not preferred to $h \oplus (-y)$.

So Chapters 4, 6, and 7 investigate the joint-receipt, preference structure $\langle \mathcal{C}, \mathcal{E}, \mathcal{D}, \succsim, e, \oplus \rangle$, where $\oplus$ is a binary operation on $\mathcal{D}$, where $\mathcal{D}$ is the closure of the set of gambles being considered, $\mathcal{G}$, under $\oplus$. Thus, $\mathcal{D}$ includes not only compound gambles, but also the recursive formation of joint receipt of gambles. When $\mathcal{G}_2$ suffices within the domain of gambles, its closure under $\oplus$ is denoted $\mathcal{D}_2$. The main task is to understand how these primitives inter-relate—the putative behavioral laws holding among them—and to discover the degree to which the structure of preferences over gambles and their joint receipt can be summarized numerically. We turn to that general possibility next.

1.4 Numerical Representations

The concept of utility is a numerical one—the idea being that there is a strength of preference that in some sense can be measured in a way similar to the measurement of various physical attributes. Indeed, the aim of this monograph is to see just what numerical representations can be justified from (potentially) observable behavioral properties. So, a number of the assertions (theorems) will take the form: Given that some behavioral properties $P_1, P_2, \ldots, P_r$ are true in some fairly general context, then there exists a numerical function U over the domain of gambles (or pure consequences) such that for $g, h \in \mathcal{G}$,

$$g \succsim h \Longleftrightarrow U(g) \geq U(h), \tag{1.10}$$

and U also reflects numerically aspects of the structure of the gambles, their compounding, and their joint receipt. Several comments about such representations are needed.

1.4.1 What behavioral properties?

Behavioral properties are assertions about the nature of the behavior of the decision maker. They are to be distinguished from structural conditions that say something about the nature of the gambles involved, which is discussed in Subsection 1.4.3. One can often distinguish between behavioral and structural properties in terms of the logical quantifiers that are used. The behavioral ones often involve only universal quantifiers whereas as structural ones always involve existential ones as well. But sometimes, as in Section 4.4.7, a behavioral property entails the existence of something, so existential quantifiers are not a reliable way to tell them apart.

Certain combinations of behavioral and structural properties can be shown to have a numerical representation. When that happens, it is usual for the behavioral properties, whether descriptive or normative, to be *necessary conditions* of the representation. In general, the structural conditions are not necessary, but only sufficient for the representation. As an example of a necessary condition, any representation for which Eq. (1.10) holds, which is true of all the representations we consider, implies that the behavior must be transitive in the sense that:

$$\text{If } f \succsim g \text{ and } g \succsim h, \text{ then } f \succsim h. \tag{1.11}$$

This follows immediately from the Eq. (1.10) and the transitivity of real numbers.

1.4.2 Descriptive versus normative properties

The behavioral properties can be approached in either of two ways, corresponding very roughly to the approaches of psychologists and decision theorists. The one is whether or not the property seems to describe the behavior of fairly ordinary people. The other is whether or not the property is rational or desirable in some generally agreed upon sense. These properties are called descriptive and normative, respectively, and the two kinds of resulting theories have description or prescription of behavior as their goal.

Much attention will be paid to properties that are both normative and descriptive, and so are common to the resulting descriptive and normative theories. Great efforts have been made to decide whether certain fairly simple behavioral properties are both. Perhaps the simplest is transitivity of preference, Eq. (1.11). We will explore some of this literature in Section 2.2.

Of course, for there to be a real distinction there must be some properties that are descriptive and not normative—e.g., duplex decomposition (§ 6.2.2)—and others that are normative but not descriptive—e.g., the universal accounting indifferences (§ 3.2.2). It is equally important to know about these, for it is here where prescriptive training can come into play.

On the other hand, I consider it highly desirable for any descriptive theory to include the corresponding normative theory as a special case. It does not seem reasonable for the descriptive theory to exclude from its scope the, admittedly rare, rational decision maker. This view seems not to be widely accepted by psychologists, who often seem to feel that the two domains are quite distinct (Edwards, 1992b; Tversky & Kahneman, 1986).

1.4.3 Structural properties

In proving such representation theorems, typically we also invoke properties that do not follow from the representation, but that we find useful in the proof. For example, with risky gambles, we may find it useful to assume that any probability can arise in a gamble, so in the binary case if $p \in [0,1]$ and $g, h \in \mathcal{G}_1$, then $(g, p; h) \in \mathcal{G}_2$. Such properties are referred to as *structural*. For example, in this example the underlying set of outcomes of the chance experiment is assumed to be what is called *nonatomic*. We like to invoke only structural properties that do not seem greatly at variance with what may be true of the situations to which the theory applies. We try as much as we can to reduce the structural assumptions, but the domain under consideration must be reasonably rich, like a continuum, or regular, like the integers, in order to get results.

1.4.4 Axiom systems

Because we can look at the behavioral and structural properties relating the underlying primitives as forming an axiomatic mathematical system, it is common to refer to the assumed properties—both behavioral and structural—as *axioms*. But it should never be forgotten that such systems are primarily proposed for their scientific, not their mathematical, interest. And they are scientifically interesting only if two conditions are met: First, the behavioral properties have either empirical or normative support. Second, the resulting theory is useful in making either predictions or prescriptions in one or another discipline.

As in any axiomatic system, many different sets of axioms can yield the same representation. This is illustrated in Chapters 3 and 5, which both provide several axiom systems for exactly the same representation. Which system to use is, to a considerable degree, a matter of mathematical taste and the empirical feasibility of evaluating the behavioral axioms. From time to time I will express personal judgments when comparing systems.

1.4.5 Who knows the axioms and the representation?

It is important at the outset to get straight the answer to this question. A number of people have seemed confused about it. For example, anyone who says that these theories of choice "*postulate* the maximization of utility" probably is confused. The representation theorems go in only one direction: from behavioral and structural properties to numerical representations. The behavioral properties embody the scientific information that we, as scientists, have about the decision maker. It is the scientist, not the decision maker, who formulates both the properties (axioms) and the representation. The decision maker exhibits the behavior which, presumably, arises from some fairly complex neuronal processes, but no one is saying that either the axioms or the representation are related in any direct way to these internal and largely unknown and currently unknowable processes.

An analogy may be useful. Consider tossing a sphere of some homogeneous material in a vacuum. A physical analysis of its trajectory is formulated as a differential equation in terms of several variables such as mass, time, displacement, velocity, acceleration, and gravitational force. Given some initial conditions, the solution to that equation describes the motion as a function of time and spatial location. Here we have no trouble recognizing that the variables, the differential equation, and the solution are all products of the scientist, not the sphere. They serve to describe its macroscopic behavior. They do not in any way describe what play of forces exists among the molecules, let alone atoms, composing the sphere. Moreover, it is possible to recast this physical phenomenon as one of minimizing a function, but no one will claim that the sphere is carrying out any explicit minimization calculations.

Likewise, we have absolutely no reason here either to impute to the decision maker any knowledge of the numerical utility function or to suppose that he or she is carrying out any maximization calculations.

1.4.6 Other modeling approaches

The approach to utility measurement we are taking is thus a very classical one—purely behavioral. Within the psychological, but not the economic, community, such behavioral approaches are decidedly out of fashion, and have been ever since the so-called "cognitive revolution." The fashionable models are of two other types.

One, called *information processing*, attempts to describe hypothesized internal processing that starts with input information provided by the situation (including the experimenter or his or her surrogate, the computer), passing through various stages of information processing (some in parallel, some serial) until a response is made. Frequently, the outlines of hypothetical processing architectures are presented as flow diagrams and then either systems of equations representing the postulated processes or computer simulations of them are developed to arrive at predictions of behavior. An example of such mathematical modeling in the decision making area is Busemeyer and Townsend (1993).

These information-processing models have one major advantage over the behavioral models now formulated in that they typically provide a fairly natural account of the time it takes to respond as well as characterizing the response made (Link, 1992; Luce, 1986; Newell, 1990; Townsend & Ashby, 1983). A major challenge facing behavioral decision theorists is to incorporate at least two temporal aspects into our work, namely, time to decision and the impact of delays in the resolution of decisions. Some work on the latter can be found in the psychological literature on operant procedures and some beginning fragments in the economic literature (Chew & Ho, 1994; Davison & McCarthy, 1988; Herrnstein, 1997; Loewenstein & Elster, 1992; and Pope, 1983, 1985). But so far, little rapprochement exists between that approach and the kind of utility theory I am reporting here.

On the other hand, the information processing models have two decided disadvantages.

First, it is exceedingly difficult to devise sensitive and isolated tests of the several underlying components. No one has figured out how to do this at the behavioral level (although W. H. Batchelder and his colleagues have made some progress, e.g. Batchelder and Riefer, 1990), and the level of processing being postulated seems, at present, to have little to do with the neuroimaging that can be done with EEG, PET, or fMRI techniques, let alone with results about firing patterns of single neurons, which data are, in any event, pretty much limited to animals.

Second, these models tend to proliferate numerous free parameters (or free programming choices) that often have to be estimated from the data that are to be explained. To be sure, if the data have many more degrees of freedom than there are parameters, the model can be tested for goodness of fit. But what the parameters mean is something else. And, typically, these estimated parameters do not seem to exhibit much invariance from situation to situation. Such invariance is needed if we are to be confident that we are onto something important, and it makes it possible to estimate them once and for all and to use them to predict in new situations. A similar charge can be made against behavioral utility theories as they are often formulated because they typically have unknown utility and weighting (over events) functions—both entailing a continuum of parameters. However, as we shall see, there are principled ways of adding behavioral properties that, on the one hand, can be studied empirically and, on the other hand, determine these functions up to a mathematical form with very few parameters. Examples limiting weighting functions are in Section 3.4.3.4 and utility functions in Sections 4.4.6 and 4.5.

Another class of popular models in psychology goes under the name of *connectionism* or *distributed processing*. Typically, these models involve the idea of input nodes, output nodes, and hidden nodes that are linked by weighted links to the other two classes of nodes. The model entails some learning mechanism that, with experience, alters the strengths of the links in such a way that the device responds not only to the inputs on which it has been trained but also to new stimuli. Such modeling of decision making is beginning to be developed by, e.g., Jerome Busemeyer at the University of Indiana and by members of Stephen Grossberg's group at Boston University.

1.5 Empirical Evaluation of Models

There are two extreme ways one can approach the testing of axiomatic models of the type we will be encountering. One can examine the axioms individually and ask in specifically designed experiments whether the observed behavior accords with them. The experiments one designs are highly focused on the individual properties, although often a single study will attempt to examine several axioms within the same overall design. Alternatively, one can ask how well the overall numerical representation accords with global preference data that are not selected with any specific property in mind. Here, the scope of the experiments should be broad in an attempt to encompass the full range of possible behaviors.

Studies that examine axioms usually are not concerned with choosing one axiom over another closely related one—although we will encounter that occasionally (e.g., §7.3)—but with a Yes-No decision on whether or not the axiom in question seems to be correct. Doing this is made difficult by the fact that choices appear to be in part statistical—or, at least, repeating the same choice does not always result in the same response (§1.2.1.4). Thus, one is confronted with devising or selecting some sort of statistical test, and very often it is unclear what it should be. We will encounter a number of ad hoc solutions that have been used, some of which have been demonstrated to lead to error (§2.2.3).

The great advantage of examining individual axioms is that in improving theories it helps to know which axioms appear valid and which are in need of change. Knowing that an entire representation fails to fit data usually fails to focus on what is wrong and may, indeed, lead theorists in the wrong direction.

When there are several competing theories, studies that focus on representations have the great advantage of pitting them against one another. Usually it is easier to decide that one theory fits data more closely than another than it is to evaluate how well a single theory fits data.

Often, however, there are difficulties that make this approach problematical. First, most of the theories are rather underdetermined in the sense of having numerical representations with unspecified utility and weighting functions—in some sense, infinite degrees of freedom. Of course, from finite data sets one can only estimate them approximately and often there are many combinations of functions that will account for the behavior. For example, much data on risk aversion can be explained either in terms of the form of the utility function or in terms of the weighting function or both. In my opinion, the existence of very unspecified functions simply means that the theory is not adequately characterized and is in need of a more complete behavioral analysis leading to additional axiomatic structure. The purported advantage of having relatively unspecified functions is the relative ease with which they can be adapted to different bodies of data.

The typical approach has been to select, often without much reason, particular families of functions with one to three free parameters that have to be estimated from the data. We will encounter numerous examples of this approach. This leads to the second problem of comparing theories, namely, how to deal with different numbers of parameters. Statistical correction procedures for estimated parameters have been suggested but, except for the simplest textbook models, they continue to be debated. (See, for example, a forthcoming issue of Volume 44 (2000) of the *Journal of Mathematical Psychology* where papers from a 1997 miniconference on general techniques of model evaluation will be published.) Moreover, for me at least, not all parameters are "equal." Consider three types: (i) Sometimes a data analysis estimates certain parameters to be approximately equal, and so the theorist "argues" for their equality and reestimates on that assumption. That is fine if explained, but sometimes it does not appear to be acknowledged. Should one receive any penalty for trying different parametric families and simply reporting the most parsimonious of the lot? (ii) The theory only partially specifies a function and the theorist picks, often out of the blue, a function with one or two parameters. (iii) A principled (axiomatic) argument leads to a function with one or two parameters. Are the fits to data using (ii) and (iii) to be thought of as being on a par scientifically? Of course, they are statistically.

The third issue is the nature of the probabilistic structure to be imposed. None of the theories considered here comes with a natural source of randomness. Thus, it is somewhat arbitrary what we assume about it. For example, Harless and Camerer (1994) postulated that each choice is a probabilistic mix of the algebraic model and what amounts to the toss of a coin, independent of whether the algebraic difference is large or small. In contrast, Hey and Orme (1994), at the same time and in the same journal, postulated a model in the spirit of the psychological tradition that began with L. L. Thurstone (1927, 1959) of assuming that if X and Y are the numerical values associated to two alternatives x and y, then the probability of choosing x over y is determined by whether $X - Y + \epsilon > 0$, where ϵ is some random variable. Note that the impact of the randomness varies greatly with the magnitude of $X - Y$. Carbone and Hey (1997) compared the two models and concluded that neither model is clearly correct, and I know of no principled reason for choosing either. Indeed, they concluded that there may well be individual differences underlying the probabilistic behavior. Thus, any attempt at an overall evaluation of theories is seriously confounded both with the choice of a statistical model and with the parametric specification of functions.

Which approach one takes depends somewhat upon one's goals. If they are scientific—trying to understand what is going on—then I think there is reasonable agreement that testing axioms individually is the way to go. There is a purposeful double meaning to what I just said: Each axiom should be tested in as much isolation as is possible, and it should be done in so far as possible for each person individually, not using group aggregations. Whenever we look, we find substantial individual differences, so aggregation must be done with a good deal of restraint and care. Thus, this monograph, which attempts to deal with scientific questions, focuses attention whenever possible on individual axioms tested on individual people. (Because the literature is often otherwise, it is not always possible to report individual data.)

If, on the other hand, one wants the theory primarily for purposes of prediction, either of behavior in new situations or as the grounding for microeconomic theory, then global testing of representations is the correct way to go. It tells one that with a particular representation coupled with a particular statistical add-on, one will do as well in predicting choices among uncertain alternatives as currently we know how to do. Because I will not be making use of this approach, it may be well to list some of the key references here. In addition to the two just mentioned above, there are: Carbone (1997), Carbone and Hey (1994, 1995), Daniels and Keller (1990, 1992), Hey (1995, 1997a, 1997b, 1997c), Hey and Carbone (1995), Loomes and Sugden (1995), Selten (1991), and a forthcoming (2000, Vol. 44) issue of the *Journal of Mathematical Psychology* on model evaluation.

Of course, the empirical world is rarely dealt with solely using one extreme approach or the other.

1.6 Outline

One might expect me to begin with the simplest case of certain alternatives and then move on to gambles. To do so encounters a difficulty. The behavioral linkages that seem to exist between certain consequences and gambles are such that for gains alone or losses alone one can have the mathematically simplest available numerical representation of preference either in the domain of certain consequences or in the domain of gambles, but they cannot both be simultaneously simple except in special cases. So one must choose which representation to make more complex, and when one knows the results it is reasonably clear that it is better to impose the greater complexity on the representation of certain alternatives. So, we take up gambles first.

Chapter 2 describes a series of fundamental, underlying properties that are common to the preference structures $\langle \mathcal{C}, \mathcal{E}, \mathcal{D}, \succsim, e, \oplus \rangle$, with or without $\oplus$, of rest of the monograph. These basic behavioral axioms, which are summarized in Section 2.7 (and also Appendix B), are postulated in almost all theories that have been proposed. One of these assumptions is that a gamble with three or more distinct consequences of both gains and losses is decomposed into an equivalent binary gamble whose consequence are two subgambles, the one being based on all of the events leading to gains, the other on all of those events leading to losses. The monograph is structured accordingly. The study of the gains subgamble is given in Chapters 3-5.

Chapter 3 focuses on a particular numerical representation of binary gambles of gains (or equally well, losses) relative to the status quo that are of the form $(x, C; y, E \backslash C)$. It explores two necessary trade-off relations—that between x and C with y fixed at the status quo e, and that between x and y with (C, E) held fixed—as well as a behavioral condition called event commutativity. Data on these are reported. This representation includes, as special cases, the classical (binary) theories of subjective expected and expected utility. Conditions characterizing these special cases are laid out, and it is made experimentally clear how the more general subjective expected utility representation fails descriptively. The mathematical forms of the weighting and utility functions that are estimated from data assuming the representation are discussed. Going the other way, each behavioral trade-off can be used to generate a numerical representation of the underlying preference order. From these, two axiomatizations of the representation are provided.

Chapter 4 introduces into the situation the additional primitive of the joint receipt of gambles and consequences. Some argument is given for supposing this operation on certain gains (and by symmetry, losses) satisfies properties akin to those of mass measurement, which therefore means it has an additive numerical representation. The major issue of the chapter is to work out the relation between this measure of value over the certain alternatives and the utility measure that was arrived at in Chapter 3 for binary gambles. A very simple and locally rational,[9] accounting indifference that links them is introduced. It leads to either a proportional or an exponential relation between the two measures. Ultimately, this provides us with an axiomatization of the binary rank-dependent representation of Chapter 3. A variety of empirical issues are explored in connection with this and the related linking laws.

Chapter 5 turns to general gambles of more than two gains consequences. The binary theory was studied in the previous two chapters, and so what remains is to understand how to generalize that theory. We explore one axiomatization based on properties holding for what are known as comonotonic gambles and two other axiomatizations that involve inductive behav-

[9] The term "rational" is tricky to use in talking about aspects of models. Some scientists identify it with a very specific overall representation, whereas I want to be able to class some specific behavioral properties as rational in certain well-defined senses. I will speak of these as "locally rational."

ioral laws that begin with the binary representation of Chapter 3. At least one of the inductive principles, coalescing, is very compelling—so much so that some authors do not view it as even testable—although it is most likely not descriptive. All three axiomatizations lead to a representation that goes under various names, the most common being rank-dependent utility (RDU) and Choquet expected utility (CEU). The latter term arises from the fact that the weighting functions that arise are examples of Choquet's (1953) capacities. There is a fair amount of inconsistency in how these terms are used. Some authors use RDU for risk and CEU for uncertainty, but others use RDU for both. Here I will take RDU to apply to both risk and uncertainty. Various sets of data are described, some of which favor RDU and some of which cast it into considerable doubt. Some alternative and historically important ideas about the representation of general gambles of gains are described. The conclusion that I draw is the situation is very much in flux and there is no agreed upon satisfactory model for more than binary gambles.

Chapters 6 and 7 extend the study of gambles to the cases of mixed consequences. This is done by separating the gains from the losses and assuming that it can be reduced to the binary case of one gain and one loss. So the main focus is on the binary mixed case. Two fundamentally different approaches are considered that lead to substantially different representations. The first, in Chapter 6, assumes a simple additive utility representation for mixed joint receipts which, via either of two possible linking "laws," leads to familiar bilinear representations of mixed binary gambles. I call the resulting class of models "rank- and sign-dependent utility" (RSDU), and Tversky and Kahneman (1992) called it "cumulative prospect theory" (CPT). The major theoretical difficulty of this approach is to understand why the utility functions found in Chapters 3 and 5 agree with those found this way. The story, while fully understood mathematically, is not very intuitive. Moreover, some recent data cast into great doubt the kind of bilinear model that arises in this way. The other approach, in Chapter 7, follows the simplest possible extension from the gains and losses theory for joint receipts, which turns out to lead to a somewhat complex utility representation for mixed gambles. Despite its complexity, it appears to accord appreciably better with the data that reject the models of Chapter 6. Data analyses to discriminate further between the two approaches are also presented.

Chapter 8 provides a brief summary of the main theoretical ideas and lessons learned, and it lists some open problems. It also raises the possibility that perhaps we are on the wrong track in assuming choice is the basic primitive and that instead we should be concerned with how respondents establish reference levels for choice situations and then assume that the analysis is carried out on deviations from that reference level.

This is followed by five appendices concerning various matters. Appendix A summarizes the basic notations that were introduced in this Chapter. Appendix B summarizes the basic behavioral assumptions of Chapter 2. Appendix C describes the nature of the PEST procedure used by me and my colleagues to estimate certainty equivalents of gambles. Appendix D describes the measurement results concerning additive conjoint measurement that are first used in Chapter 3. And finally, Appendix E pulls together in alphabetic order all of the major definitions of the monograph.

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