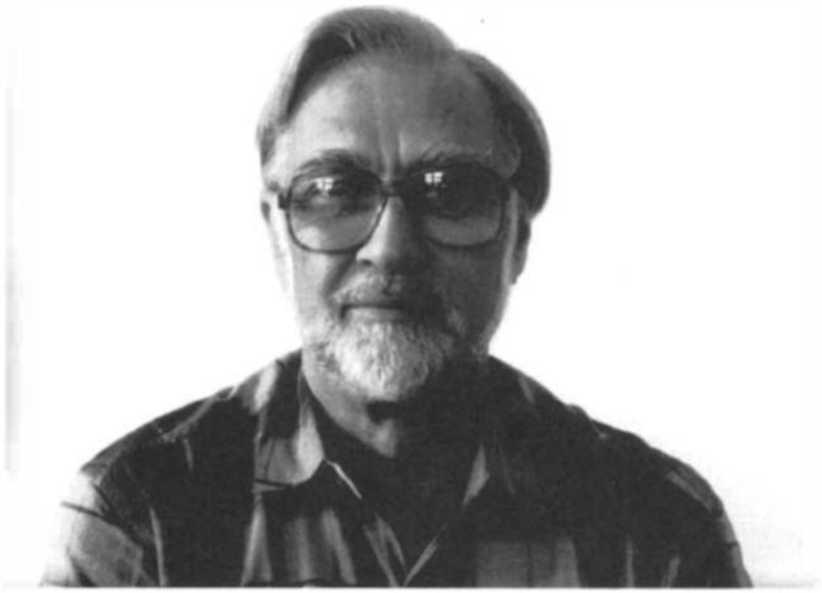

Annu. Rev. Psychol. 1995. 46:1–26

FOUR TENSIONS CONCERNING MATHEMATICAL MODELING IN PSYCHOLOGY

R. Duncan Luce

Institute for Mathematical Behavioral Sciences, University of California, Irvine, California 92717

KEY WORDS: dynamic versus static models, error versus structure, mathematical modeling, normative versus descriptive models, phenomenological versus processing models

CONTENTS

0066-4308/95/0201-0001$05.00

WHY SHOULD MATHEMATICS PLAY A ROLE IN PSYCHOLOGY?

Structure, Pattern, and Process

Mathematics studies structures and patterns described by systems of propositions relating aspects of the entities in question. Deriving logically true statements from sets of assumed statements (often called axioms), uncovering symmetries and patterns, and evolving and understanding general structures are the concerns of mathematicians.

Mathematics becomes relevant to science whenever we uncover structure in what we are studying. One should not underestimate the difficulties in isolating such structure and the even more difficult task of finding good ways to describe it. After all, it took several millennia—albeit with some fairly inactive periods lasting centuries—to get to our current elaborate understanding of physical structures and processes.

As psychologists, we seek structure in aspects of human (and sometimes, animal) behavior. No one holds that all true statements we can make about a person's behavior are independent of each other. Some propositions surely follow as a consequence of others. Otherwise, any prediction of behavior would be impossible, and obviously we continually predict the behavior of others. People count on others to behave in certain ways depending on the situation or on various indicators about social roles, mood, etc. Without some predictable behavior, our social environments would seem random, which clearly they do not.

The existence of psychological structure cannot be in doubt. But what that structure is, is another matter. As we psychologists gradually disentangle its aspects, we also begin to describe it in more formal terms and, in some of the simpler cases, mathematics begins to play a significant role.

What Keeps Mathematical Psychology from Being an Oxymoron?

The existence of psychological structure means that mathematical theories are, at least in principle, a possibility in psychology. Nonetheless, such theories may not prove realizable in a deep sense; the attempt may really prove to be a contradiction in terms, an oxymoron. To avoid that danger, we must attempt to satisfy the following sensible but demanding criterion: Knowledge of an explicit, falsifiable psychological theory should not provide the (unaided) knower with the means to falsify it at will in every empirical context. Put another way, psychological theories should not turn out to be nonfulfilling prophecies any more than they should be self-fulfilling. In practice this means that the scientist should be confident that an experimental or field design exists that allows the theory to be tested despite the subject's knowledge of the theory. I call this the non-oxymoron criterion.

Such a principle holds for any science, but it is particularly significant for psychology. In other social sciences, which typically deal with situations aggregated over large numbers of individuals, widespread knowledge of a theory is less likely to lead to its rejection. This is partly because the impact of any single person on the behavior of large groups usually is minuscule.

Any psychologist who has reflected on the issue knows that this criterion is exceedingly difficult to satisfy and probably impossible to do so if subjects are permitted suitable external aids. For example, current mathematical models of the perception of aperture colors agree that the data can be represented in a three-dimensional (3-D) geometric space (Indow 1982; Krantz 1975a,b; Suppes et al 1989, pp. 131–153). Can a person with normal color vision systematically fool us into thinking his or her perception of nonreflected colors is either 4- or 2-D? I doubt that anyone without benefit of a physical spectrum analyzer and a computer model can simulate 4-D behavior. Faking 2-D behavior is, in principle, simpler because it only involves ignoring a distinction, such as that between red and green. But as a matter of fact it is quite difficult for an unaided person to do so successfully—witness the failures to simulate color blindness to avoid being drafted during our mid-century wars.

FOUR DISTINCTIONS

Four major contrasts are useful in discussing current mathematical modeling. Any particular model can be identified as falling somewhere on each of the distinctions. Some examples will be mentioned in illustrating these distinctions, and I will repeatedly raise the question of how we attempt to satisfy the non-oxymoron criterion for these specific models.

Phenomenological versus Process Models: Unopened and Opened Black Boxes

Phenomenological models treat a person as a "black box" that exhibits overall properties, but with no internal structure specified within the model. This approach is like that of classical physics, in which objects have properties—e.g. mass, charge, temperature—but no explicit molecular or atomic structure is attributed to them. Many psychological theories, including most mathematical modeling of judgment and decision making, are of this type; they attempt to characterize aspects and patterns of behavior without asking about the underlying, internal mechanisms that give rise to the behavior.

Another type of psychological modeling, commonly called information processing, attempts to analyze the black box in terms of internal mechanisms of information flow. The attempt is, in a functional sense, to open the black box. As will be noted later, various versions of information processing differ in the degree to which they take neurobiological observations seriously.

Descriptive versus Normative Models

Many psychological issues, especially those having to do with measurement, are related closely to well-articulated normative theories that describe how we should reason, draw inferences, and make decisions. Everyday reasoning is loosely coupled to formal logic; the ordinary inferences we draw have some relation to the formalized inferences of probability and statistics; and human decision making is sometimes replaced by formal theories of decisions. Nevertheless, in our ordinary lives we often fail to be fully logical in our deductions, to behave like skilled statisticians in drawing inferences from data, or to optimize a criterion when making decisions.

These normative sciences, in some sense, characterize our collective, and improving, understanding of ideal reasoning, inference, and decision making. To follow these collective dictates requires training, self consciousness, and auxiliary aids, such as a computer. Most of us revert to everyday modes of behavior unless we explicitly elect to act like a logician, statistician, or decision analyst for the occasion at hand.

Despite differences between the normative theories and everyday behavior, the fact is that these normative sciences have grown out of our natural, if imperfect, skills in dealing with such issues. So it is plausible to anticipate some degree of overlap in some of the basic principles, if not in the actual execution of reasoning and decisions. Moreover, humans are able to address issues of reasoning, induction, and decision making that our present formalized normative theories find exceedingly difficult to confront. For example, we are all masters at dealing with ambiguity, which is anathema to logic, mathematics, and computers. This is where we have the greatest difficulty in interfacing people with computers. A few scientists are attempting to model ambiguous reasoning, but no consensus yet exists.

Dynamic versus Static Modeling

We change and our environments change. Little is static except many of our theories. Why is this? Every time we introduce a new variable, the scientific problems become appreciably more complex, and so if we can omit time, so much the better. Moreover, statistical issues are much confounded when we deal with changing behavior: it is counterproductive to average over trials, because that is where the change is to be seen, or over subjects, because the changes they exhibit may be qualitatively different. Furthermore, the main devices used in the physical sciences for dealing with dynamics—differential, difference, and integral equations—have not, so far, proved well suited to most psychological problems. It is unclear whether this reflects a deep difference in the nature of the sciences or only our incomplete understanding of the mechanisms of change. But the fact is that only small portions of our theories purport to be dynamic in character. Most assume a static phenomenon. An important issue is how best to increase the dynamic character of our models.

Noise versus Models of Structure

Theories are about structure, and to be tentatively accepted as a "correct" theory, we confront it with data to determine whether the proposed structure agrees sufficiently well with the empirical data. In practice, this evaluation is confounded by various forms of error, systematic and nonsystematic. This is true of any science, but it is an especially severe problem for psychology. In the macro-physical sciences, refinements of procedure and equipment typically reduce the magnitude of nonsystematic errors toward zero. In psychology, as is also true for quantum theory at a scale many orders of magnitude finer, the object of study itself seems to be the irreducible source of that error. This apparent fact must not be used as an excuse for poor experimental design, incautious procedures, or the inclusion of experimental artifacts. Nonetheless, after many years of careful methodology, it is probably safe to conclude that an irreducible amount of nonsystematic error—perhaps randomness—is inherent in human behavior. In that case, our options are to tack statistics onto the algebraic models, to develop probabilistic models of structure, or to interpret the error as arising in some way from complex but systematic processes. Each approach is to some degree unsatisfactory, and a fully satisfactory solution has not yet evolved.

EXAMPLES OF PHENOMENOLOGICAL AND PROCESS MODELS

Phenomenological Models

TRADE-OFFS AND MEASUREMENT All sciences study trade-offs, usually those among variables affecting an attribute (or dependent variable) of interest, in particular those combinations of independent variables that keep the attribute constant. Consider performance in signal detection (Green & Swets 1988, Macmillan & Creelman 1991). To improve detection, one must simultaneously increase hits and decrease false alarms. When an observer is operating below his or her performance limits, as shown in the hit versus false-alarm space of Figure 1, the observer can simultaneously improve both measures, in the region bounded by the horizontal and vertical lines from the point to the curve of

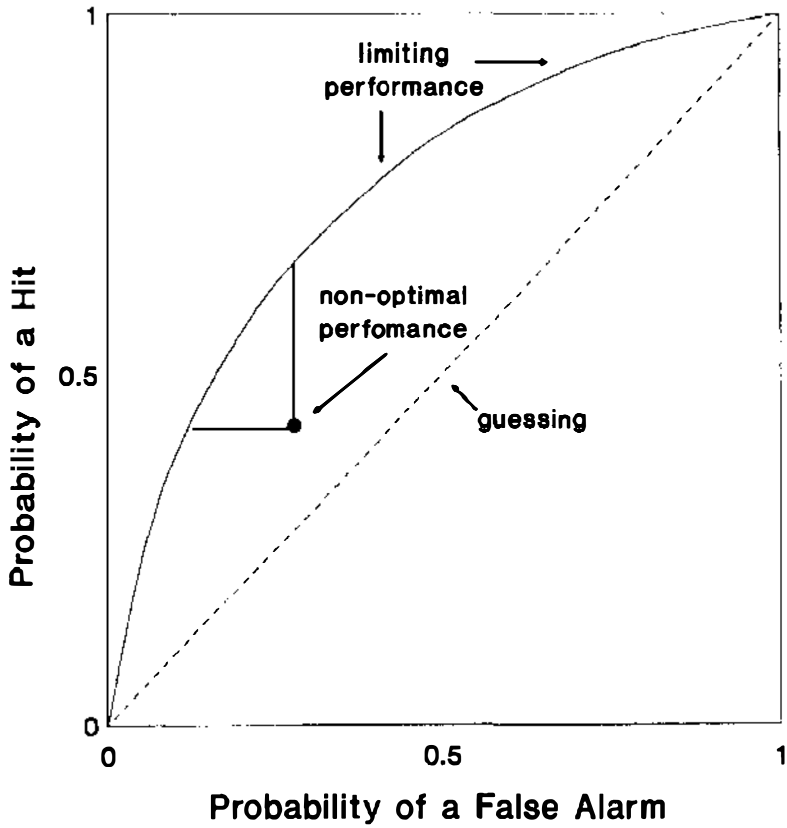

Figure 1 A plot of the probability of a hit (i.e. detecting a signal) versus the probability of a false alarm (i.e. saying a signal is present when it is not). The curve is the best performance possible for the signal in question. A point below the curve is always dominated by the points in the region under the curve bounded by the horizonal and vertical lines from the point to the curve.

limiting performance. But once the performance limit is reached, the only feasible movements while maintaining maximum detectability involve trading off an improvement in one dimension for a deterioration in the other. The limiting behavior is characterized by these being the only possible trade-offs that are not dominated by another feasible pattern of behavior. Such mathematical models of limits of behavior typically satisfy the non-oxymoron criterion because, without special aids, a person is incapable of overcoming these limits, even when fully familiar with the theory. This is one of several reasons why, when studying limiting performance, scientists often serve as their own subjects without the specter of experimenter bias being raised.

Another example where fairly sophisticated mathematical models have been developed is in studying speed-accuracy trade-offs in detection and discrimination contexts (see Luce 1986, Townsend & Ashby 1983). Once again, the focus is on the limits of performance, and so it is relatively immune to violations of the non-oxymoron principle.

REPRESENTATIONAL THEORIES OF MEASUREMENT Equal-attribute or indifference curves represent a third example of trade-offs. In this case, stimuli have two or more factors that affect an attribute. The trade-off studied is that between the factors that keep the attribute constant (e.g. intensity and frequency pairs that maintain constant loudness, delays in receiving and amounts of food that maintain constant motivation, the combinations of several relevant features of a job that yield equal attractiveness). Here subjects are free to mislead us, although they probably have little motivation beyond laziness to do so. The cautious scientist typically collects redundant data. For example, we expect the judgments of equal attributes to be, within the limit of error, transitive: If tone s_1 is equally loud as s_2 and s_2 is equally loud as s_3 , then if the subject is being consistent we expect s_1 to be equally loud as s_3 . Such checks are commonly made.

Trade-offs become a source of measurement scales when we collect not only equal-attribute data but also when we order the indifference curves by the attribute. Key measurement questions are 1. What are the properties exhibited by the ordering? 2. Are they such that one can construct a numerical representation of the empirical information? A numerical representation involves two distinct constructions. The first consists of numerical measures associated with each of the independent factors. These describe how each factor affects the criterion attribute. In psychological examples these measures often are called psychological scales. The second construction is a rule for combining the scale values that yields a numerical measure of the criterion attribute. The rule must be such that the numerical order exactly reproduces the empirical order. Such rules are often referred to as "psychological laws."

The simplest representational problem posed by these examples was formulated and solved by the economist Debreu (1960) and, independently and somewhat more generally, by Luce & Tukey (1964). We provided a list of properties about the qualitative ordering of the attribute being studied over the two (or more) factors that, when satisfied, imply the existence of a multiplicative representation[1] of the ordering into the positive real numbers. This is the representation found in many common physical examples (e.g. kinetic energy, momentum, density). It goes under the generic name of *additive conjoint measurement.*

1 Unlike physics, where measurement scales are into the positive real numbers and the combining rule is multiplication, social and psychological theorists often map into the entire real numbers and use an additive representation. The latter arises from the former by a logarithmic transformation.

It is a curiosity of the philosophy of science that the physicists, philosophers, and mathematicians who studied the bases of physical measurement at the end of the nineteenth century failed to come up with this result—or even to recognize that it was needed. Over half a century later, behavioral scientists saw its significance for measurement. The consequences have been considerable, not only for theory but also for widespread applications of the general idea of conjoint measurement in areas such as marketing (e.g. Green & Srinivasan 1990, Wittink & Cattin 1989).

Because the simple multiplicative rule is inadequate to describe many behavioral trade-offs, e.g. loudness, Narens and I generalized these results to far more complex rules (Narens & Luce 1976, Luce & Narens 1985; for a summary see Luce et al 1990). These representations continue to lead to numerical measures on the factors, but they combine in nonmultiplicative ways. Our models extend greatly the possibilities of measurement and are slowly finding applications in areas such as sensory psychology and decision making.

TESTING: PROPERTIES VERSUS REPRESENTATIONS There are two ways to test the adequacy of an explicit measurement model.[2] One way is to study the individual phenomenological properties that give rise to the representation. Data collection is limited to carefully contrived sets of stimuli that are well suited for the study of the property in question. For example, transivity is the simplest property assumed to hold in all such theories. Let $\succsim$ denote the attribute ordering over the stimuli and suppose *a, b,* and *c* are any stimuli such that $a \succsim b$ and $b \succsim c$. Then transitivity is satisfied if $a \succsim c$ also holds. Other properties exhibit a somewhat similar form, stating that if certain inequalities hold, then certain others must also hold. For example, a key property of trade-off (or conjoint) measurement necessary for a multiplicative representation to exist is called double cancellation. Consider stimuli having two independently manipulable factors with *a, b,* and *g* (not necessarily numerical) values on the first and *p, q,* and *x* on the second. If $(a,x) \succsim (g,q)$ and $(g,p) \succsim (b,x)$, then $(a,p) \succsim (b,q)$. Note that the entity *g* of the first component and the entity *x* of the second component each appear on opposite sides of the first two inequalities and so can be "cancelled," leaving the resulting assertion. This necessary property of structures having a multiplicative representation is discussed thoroughly from a psychological perspective by Michell (1990), and it appears in every book on representation measurement published since 1970 (Krantz et al 1971, Narens 1985, Pfanzagl 1971, Roberts 1979).

[2] I use measurement model to mean the special types of models found in the axiomatic, representational theory of measurement. I do not mean the extensive statistical and geometric models that have been widely used in testing abilities and attitudes.

A naive test for the transitivity of behavior can easily fail the oxymoron criterion. If we present the three pairs of stimuli—$\{a,b\}$, $\{b,c\}$, and $\{a,c\}$—in immediate succession to a subject who, for whatever reason, wishes to defeat the postulate of transitivity, he or she will have no difficulty in doing so. Two tricks are used to bypass this problem. First, suppose the stimuli have an intrinsic value to the subject, who is informed at the outset that after the choices are completed a few of the pairs will be selected at random and from each such pair he or she will receive the choice made. It thus behooves subjects to reveal their true preferences. The second trick is to separate the several related pairs widely over hundreds of trials. Subjects find it impossible to hold in memory many of their past responses. Once again, a person provided with suitable aids to memory can readily violate the theory. Enforcement of the non-oxymoron criterion depends in this case on addressing the self interest of the subjects and on producing sufficient experimental complexity that, coupled with the familiar limitations of human memory, make intentional violations unlikely.

The other testing option is to collect data that sample relatively unselectively the whole space of stimuli and do not focus on any one property, and then attempt to fit the representation, which typically has several largely unspecified functions, to the entire body of data. Anderson (1981, 1982, 1991) and Tversky & Kahneman (1992) offer good examples of such an approach. Again subject honesty is sought in the same ways. A major problem with this approach is the considerable freedom of the model and, therefore, how best to establish stringent goodness-of-fit criteria. To my knowledge, no satisfactory general solution yet exists. Both of these testing approaches typically do not make room for error or noise in the data, although the data are invariably quite noisy. This issue remains sufficiently problematic in mathematical modeling that I devote a later section to it.

Process Models

OPENING THE BLACK BOX Although a psychologist's interest lies primarily in behavior, process modeling attempts to explain some aspects of underlying mental and/or brain mechanisms and how they give rise to behavior. The attempt is to open the black box. The most extreme forms of opening it involve biological observations. Examples include readings of electrical spikes on individual neurons obtained from electrodes inserted into them, examination of the complex interactions taking place in a neural subnetwork, ablation techniques aimed at destroying a specific local region of a (nonhuman) brain to ascertain how behavior is affected differentially by the loss of that region, or the more-or-less passive EEG, CAT, and MRI scanning techniques that measure aspects of brain function under various tasks. Psychologists operate at these various levels.

In practice, most mathematical modelers, although sometimes inspired by neural data, postulate mechanisms far more abstract and functionally defined than are found at the neural level. Their strategy somewhat parallels the difference between understanding computer architecture and the actual detailed electronic connections among basic components. As with computer architecture, flow diagrams are a favorite device for communicating functional flows of information.

SOME FEATURES OF PROCESS MODELING A few general remarks about information processing models are appropriate:

1. Process modeling is popular among mathematically oriented psychologists. Perhaps as many as three-quarters of the mathematical-theoretical papers in psychology adopt such an approach.
2. It relies heavily on computer modeling and simulation, which most psychologists find easier to learn than they do mathematics.
3. The approach is very flexible, which is both a virtue and a fault. It can be exceedingly difficult to be sure what about a particular processing model is correct. This is especially true when the processes are entirely hypothetical as was true, for example, in the early stimulus sampling models (Neimark & Estes 1967) and in the vast majority of response-time (Link 1992, Luce 1986, Townsend & Ashby 1983) and categorization (Ashby 1992) models.
4. All behavior obviously must arise from some internal activity. But it has been difficult to establish plausible connections between standard information processing ideas and some types of regular behavior (e.g. such as are described below in the section on individual decision making). Although Busemeyer & Townsend (1993) devised a processing model of decision making, they focused little on explaining the simple (often rational) behavioral properties that have been of concern to most decision theorists.
5. Sometimes exactly the opposite is true. There are cases where insights about behavior arise from information processing concepts and for which phenomenological approaches seem helpless. The following is one such example.

TRADE-OFFS AND PROCESSING Direct recordings of electrical activity in the peripheral auditory (eighth) nerve, which departs the inner ear for the higher reaches of the brain, tell us that signal intensity is encoded, at least in part, by the rate at which electrical spikes occur. More intense signals yield higher rates. The observed values vary from about five spikes per second to hundreds per second. These observations are crudely analogous to heart rate, except for being much faster and varying over a far wider dynamic range.

If neural spikes are the information available to the brain about the signal—and current knowledge suggests that they may be—and if intensity (at least over a limited range) is encoded as spike rate, then a brain extracting intensity information has no option but to estimate the rate from brief samples of spikes. Assuming the brain includes the functional equivalent of a stopwatch and a simple counter—the evidence for which is entirely indirect—there are two extreme ways for the estimate to be made. One, called *timing,* is to see how long it takes to collect a prescribed number of spikes. The other, called *counting,* is to count how many spikes occur in a fixed time period. In both cases, the rate is estimated by dividing the count by the time.

When rates vary over a large range—a factor of about 100 in the neural case—the advantage of fixing the number of pulses is that the quality of decision, which varies with sample size, remains independent of signal level. The obvious disadvantage of that strategy is the time that it takes to achieve the sample depends significantly on signal strength. The organism can either maintain decision quality at the expense of slower responses to weak signals or maintain a fixed decision time at the expense of poorer quality performance. For weak signals, it cannot have high quality, fast responses. The problem either way—slow times or poor quality of information—is, of course, the reason why many predators employ a strategy of stealth coupled with a fast attack.

One empirical question is whether both options are actually available to human beings. Luce & Green (1972) showed mathematically that if both are available, then a dramatic difference should be evidenced in the resulting speed-accuracy trade-off. The two models result in differences in the slope of the hit versus false alarm (ROC) curve (see Figure 1) when replotted in z-score coordinates and in the resulting speed-accuracy trade-off. The slope of the ROC is considerably less than 1 for counting and considerably greater than 1 for timing.

To test this prediction, Green & Luce (1973) adopted a simple experimental procedure designed to induce the subjects to exhibit both modes of behavior, if they are available. Suppose in a detection situation we manipulate response times by imposing a fairly severe fine when a response deadline is missed. When the deadline applies to all trials, it is optimal to count the number of pulses in a fixed time, and when it applies only to signal trials, it is optimal to fix the count and measure the time. The latter is, of course, the payoff structure for potential prey relative to predators—it does not matter how long it takes to respond when the signal arises from nonpredator noise in the environment. The predictions were so clearly sustained by the data that no statistical test was needed. Wandell (1977) successfully replicated the study for visual intensity. In recent years, McGill & Teich (1991) have provided the main developments concerning such approaches.

MULTIPLE MODES OF BEHAVIOR This above case is an unusually simple example of a pervasive dilemma for psychologists. People typically have several qualitatively different ways of coping with a situation. If we are unaware of these multiple possibilities or elect to ignore some of them, we are likely to become confused by the data, which when averaged over subjects is perforce some unknown mix of these possibilities. Even if we are sensitive to the issue, we may have considerable difficulty either in identifying the mode being used, especially if the subjects shift among them easily and frequently, or in controlling which is used. The case mentioned in the previous section involved experimental control of the mode, and other evidence suggests that subjects do not move readily between the two modes.

The so-called fast-guess model (Ollman 1966, Yellott 1971) offers a different example of alternate modes of behavior. This model suggests that if, in the speed-accuracy situation, one presses a subject to faster and faster behavior, there is a limit beyond which the subject can no longer pay attention to which of the two signals has been presented. Urged to go faster through the judicious use of money rewards, the subject can either refuse to do so or can give up on trying to achieve any accuracy at all and simply respond to the signal onset, but not its identity. This shift of mode occurs in humans and in such animals as pigeons (for a summary see Luce 1986, p. 224, 286–294). It is a strategy of frustration, which psychologists need to be alert about and take into account. This model initially postulated that the mode is selected at random, trial-by-trial. A careful sequential analysis by Swensson (1972) and Swensson & Edwards (1971) showed that, in fact, subjects stay in each mode for a number of trials before switching, which made possible a fairly accurate partition of the data into the fast guesses and the slower, more attentive responses.

These two examples are misleadingly simple and clear; rarely is it possible to see the modes so clearly. Caution and ingenuity are the only solutions I know of for dealing with the dilemma of multiple modes of behavior. A theory alleging only one mode of behavior may be easily rejected by a person having two or more available. To pass the non-oxymoron criterion—that knowing a theory should not be sufficient for the subject to falsify it—the theorist must work out the full range of modes and figure out ways either to induce a single one, as Green and I did using payoffs, or to partition an individual's data, as was necessary with the fast-guess model. To the degree we exhaust these options, the non-oxymoron criterion will be satisfied; but otherwise it will not be.

Invariance of Mechanisms Across Situations

One feature of the physical sciences is that as mechanisms and phenomena are uncovered and modeled, they become available for use, with whatever constants have been estimated, in wholly new situations. For example, the laws of thermodynamics and electromagnetism and their corresponding dimensional constants or, for another example, the existence of biological mechanisms such as genes, chromosomes, DNA, and RNA, once isolated, are assumed applicable whenever they are relevant. Little comparable invariance has evolved in psychology. It is moderately rare to find a psychologist who, when confronted by a new set of data, invokes already known mechanisms with parameters estimated from different situations. Newell (1990) claimed to do so in his computer-based, unified theory of cognition called SOAR, but I am not persuaded by the claim. When each model is unique to a particular experimental situation, all of the model's free parameters must be estimated from the data being explained. Frequently the resulting numbers of parameters outrun the degrees of freedom in the data. This reflects a failure of the science to be cumulative, an unfortunate feature of psychology and social science that is widely criticized by natural scientists. I view it as one of the greatest weaknesses of modeling (and other theory) in our science.

A DESCRIPTIVE/NORMATIVE EXAMPLE: DECISION MAKING

An area of current interest to me is how descriptive and normative decision theories relate. The problem, from my perspective, is to discover which underlying principles of normative behavior are descriptive and which must be modified to get a correct description of behavior. These theories seem to have more in common than one might expect, but there are significant deviations. To this end, it is convenient to class the phenomenological assumptions into three distinct groups followed by some consequences.

Normative Principles of Preference

Consider a situation of uncertainty in which some chance event partially determines the outcome. As an example, consider an entrepreneur contemplating an investment in a country on the fringe of the old Soviet empire. The expected profit from the investment depends, in part, on whether hostilities break out in that region during the time period of the investment and, if they do, their exact scope and nature. A widely accepted normative postulate about such gambles—which, of course, is what an investment is—asserts that if for a particular state of hostility the amount of profit is increased but otherwise the entire situation remains unchanged, then the modified alternative will be seen as better than the original one.

This apparently innocent truism, called consequence monotonicity, is in fact quite a strong property. Some empirical studies had led many to question its universal applicability, but recent work suggests that it is strongly descriptive as well as normative (von Winterfeldt et al 1994).

Normative Principles of Framing

People were initially misled about monotonicity because the original experimental designs also presupposed another normative postulate, which for some reason decision theorists failed to question (Luce 1992). According to this postulate, two alternate descriptions of the same situation should be treated as indifferent in preference by the decision maker. This normative postulate together with transitivity of choices and consequence monotonicity go a long way toward establishing the now classical rational representation called subjective expected utility (SEU), in which utilities of consequences are averaged using subjective probabilities over the events (Savage 1954; see also Fishburn 1982, 1988; Wakker 1989).

But as we know from the familiar example of whether to call a glass half full or half empty, descriptions of situations can matter. The impact of framing, as it is called, has been explored by Tversky & Kahneman (1986), has been formulated explicitly in a particular case by Luce (1990), and has led to some striking discoveries in, for example, the realm of medical decisions (McNeil et al 1982). Others have shown major impacts of the framing of questions in public opinion polls.

A Descriptive Principle

Closely related to these framing effects is the possibility that people replace a complex alternative by something simpler than but not exactly equivalent to the original gamble. For example, people often partition uncertain situations into two parts, each examined independently of the other: the chance of gains arising and, separately, the chance of losses arising. Each aspect is separately appraised and the two evaluations are summed in some fashion to get an overall evaluation of the original situation. Indeed, such a decomposition forms the basis of many risk-benefit analyses. Nonetheless, it is not fully rational to invoke such a decomposition because the separate, independent analyses are not fully equivalent, in general, to the original situation. Only three studies have examined this decomposition hypothesis, but all sustain it (Cho et al 1994, Payne & Braunstein 1971, Slovic & Lichtenstein 1968).

Rank- and Sign-dependent Utility Representation

This descriptive, but non-normative, principle coupled with the rational preference hypotheses of transitivity and consequence monotonicity, along with the simplest rational framing properties, yields a mathematical theory that is closely related to SEU, but appears to be more adequately descriptive. It is called rank- and sign-dependent utility (RSDU) by Luce (1991, Luce & Fishburn 1991) and cumulative prospect theory by Tversky & Kahneman

(1992, Kahneman & Tversky 1979, Wakker & Tversky 1993). In the RSDU representation the utility of a gamble is the weighted utility of its gains subgamble and its losses subgamble, but with weights that fail to sum to 1, as do the subjective probabilities of SEU. This representational oddity simply reflects the nonrational decomposition mentioned above. The utility of the gains subgamble is a weighted average representation like SEU except it is rank-dependent (RDU) in the sense that the weights attached to an event depend not only on the event but also on the rank-order position of the consequence arising with that event as compared with the other consequences from the subgamble. This rank dependence arises quite naturally from the process of rational reframing. Quiggin (1993) provides a general discussion of RDU models. Research on this topic is active, and experience warns that, all too often, new data will surprise and perplex us. I fully expect the story to be somewhat different in a few years, but perhaps our empirical and theoretical knowledge will be cumulative rather than destructive in nature.

DYNAMIC AND SOMEWHAT DYNAMIC MODELS

Change is everywhere, and much of it is systematic. The major breakthrough in passing from Renaissance to modern physics was the creation of the calculus as a way to capture physical change. So far, these classical mathematical methods have proved of limited help in dealing with psychological change, which appears to be of at least two distinct types, both of which are often referred to as learning, despite considerable qualitative differences. One type involves small, systematic adaptations; tennis and other skilled performances are (relatively complex) examples. The other type has to do with the acquisition of concepts, their relation to previously existing concepts, and the representation of this knowledge in long-term memory. This is the sort of learning we associate with schools and textbooks, not tennis courts.

Models of Incremental Change

OPERATOR MODELS Two approaches to changing behavior were pursued in the 1950s. A phenomenological approach assumed that each experimental trial was fully characterized by a probability vector over the possible responses, and that this vector was altered systematically depending on the choice actually made and the payoff received. The most fully developed of this class of incremental change models assumed linear changes (Bush & Mosteller 1955, Norman 1972). A class of nonlinear models was also studied but was rejected because of its inability to neglect experience from the distant past (Luce 1959, Sternberg 1963).

SEQUENTIAL EFFECTS These simple operator models are ineffective in dealing with complex memory and learning processes (see below), but they have remained viable as descriptions of adjustment processes such as selecting the response criterion in signal detection. In particular, they seem somewhat useful for characterizing the sequential effects found in almost all psychophysical methods. The basic finding is that the response on trial *n* depends not only on the signal on trial *n* but on some of the past history of signals and responses. A central question is the actual depth of the dependency, which is not easy to decide because even if it only goes one step back, there will be an apparent dependency that goes back much further. These effects, which are seen in asymptotic behavior and can be quite large (on the order of 10–20% in response times), are mostly ignored by psychophysicists although they are found whenever they are sought. Attempts have been made to model them by incremental learning models (Luce 1986, pp. 292–298) and by linear regression (Ward 1979, Ward & Lockhead 1970); however, these models are inconsistent with the fact that the correlation between responses on successive trials has repeatedly been shown to depend on the signal separation, ranging from about 0.8 for repeated signals to 0 or even negative values for widely separated ones. Nothing adequate has yet been proposed.

NONLINEAR DYNAMICS Until the advent of high speed computing, no science was able to work effectively at a theoretical level with profoundly nonlinear processes. Attempts were made to approximate these processes by linear models, and there was some understanding of asymptotic results in certain cases. Faster computers made it possible to simulate nonlinear processes in great detail, resulting in considerable surprises. One such finding was that small changes in parameter values do not always lead to small changes in the final result. Qualitatively very different modes of behavior sometimes result. Another finding was that some of these modes are very irregular (i.e. chaotic) and appear superficially to be totally random despite being entirely deterministic. This seems to be the nature of turbulence in fluid flows. These qualitative facts about many nonlinear systems strike a receptive chord in behavioral and social scientists because much of the behavior under their scrutiny seems to undergo radical transitions and often has to be described as chaotic.

Even something as prosaic as psychophysics may be modeled in this fashion. Perhaps the major proponent of this view is Gregson (1988). Unfortunately, many find his presentations obscure and, as a result, they have had less impact than might be expected. Nonetheless, this may well be an important development, provided that we can arrive at sensible dynamic models. Because of the partitioning of behavior into trials for the convenience of data collection, even at the cost of considerable unrealism, the types of equations that arise are difference rather than differential. A simple example, which has been used in studying population changes for a century and a half, is:

$$Y_{j+1} = -aY_j(1 - Y_j),\ 0<Y_j<1,\ j = 1,2,\ldots \qquad 1.$$

In this example the variable Y is, in principle, observable or at least estimable because it is, for example, the relative proportion of predators to prey. In the psychophysical case, Gregson proposed that some sort of underlying but unobservable state variable controls the observable responses. By what appears to have been a trial-and-error approach, various nonlinear recursions (considerably more complex than the one above) were explored until the desired system behavior was achieved. Because the testing of such models raises general issues, I devote a separate section to it.

ENVIRONMENTS DEPENDENT ON BEHAVIOR Out of the operant animal literature has come an interesting development about choice behavior, one that appears equally applicable to human beings. Most modeling of choices in psychophysics and decision making assumes a totally static probabilistic environment. In the operant work on choice it is not static. The alternatives are designed to have features like those encountered by a foraging animal: the more uninterrupted time spent on one alternative, the lower the rate of reinforcements received, whereas the ignored alternatives become gradually richer.

Various schedules of this sort have been explored. One major finding is that subjects—ranging from rodents to humans—do not partition their time among alternatives to achieve a maximum total rate of reward, which requires equating all of the marginal rates of reward. Rather, they distribute their attention approximately so that each alternative yields the same average rate of reward (Herrnstein & Prelec 1991, Prelec 1982). At first, there was some doubt about this finding because, for most natural schedules, the difference between maximizing and averaging is comparatively small and, with data somewhat obscured by noise, it was difficult to be certain which was a more accurate description. Later studies, however, made the point unambiguously that it is averaging.

For example, consider the following two-choice design: Subjects received the same monetary reinforcement following every choice; however, it was only received after a delay that depended on which alternative was chosen, 1 or 2, and on the proportion of alternative 1 responses during the immediately preceding 10 trials. The functions used are shown in Figure 2. For each proportion, the delay for alternative 2 exceeds by 2 seconds that for alternative 1, and for each alternative separately, the delay increases linearly with the proportion of responses to alternative 1. The mean delay is shown by the dotted line in Figure 2. So, the optimal behavior is always to choose alternative 2, which yields the least mean delay. Yet, for any proportion of alternative 1

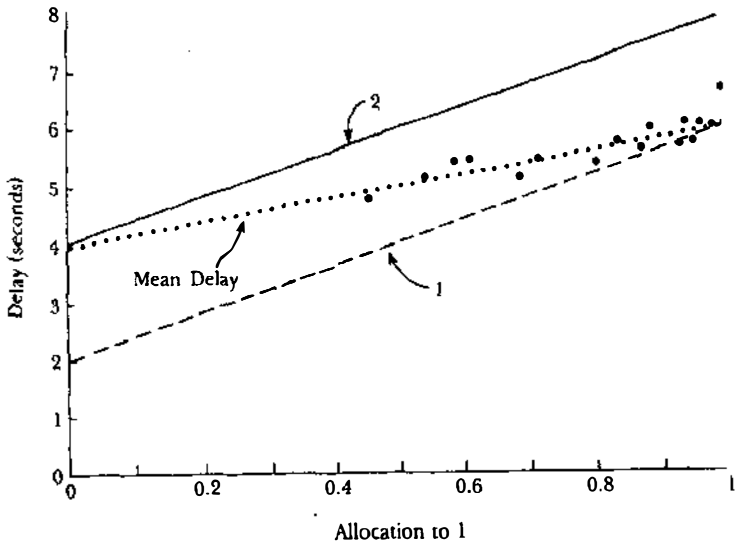

Figure 2 The delay for alternatives 1 and 2 as a function of the proportion of alternative 1 responses on the preceding 10 trials. The dotted line is the mean delay. The points are the behavior of 17 subjects in a 10 minute period after 100 practice trials. Reprinted from Figure 3 of Herrnstein & Prelec (1991).

responses, the delay is always 2 seconds less by choosing alternative 1. The dots show the behaviors of 17 subjects, and none is close to optimal.

An intuitively plausible mechanism, which Hermstein & Vaughan (1980) called melioration, underlies this result. Suppose the subject decides (in some currently unknown fashion) that on average, alternative 1 is paying off at a higher rate than alternative 2. Then it is postulated that the time devoted to response 1 is increased at the expense of time to response 2. But once the pattern of relative returns is reversed (which generally happens, although it does not in the payoff structure of Figure 2), then the pressure is reversed to increase the time devoted to alternative 2. This continues, oscillating back and forth, until each alternative appears to be equally rewarding and the pressure evaporates. This is the same mechanism that underlies the fixed-point theorem proof of Nash's famed equilibrium theorem in game theory (Luce & Raiffa 1957, p. 391).

Concepts and Memory

After much experimentation and controversy, the early incremental models were for the most part abandoned. They were deemed not applicable to the learning of complex concepts, although they are still used to model the fine tuning found in some types of skill learning. A second approach, called stimulus sampling, which was pursued at about the same time, postulated a simple associative process that develops with experience (see Neimark & Estes 1967 for a collection of papers on stimulus sampling models). These processing models gradually evolved into a class of models concerned with memory and concept identification (for summaries see Ashby 1992, Healy et al 1992).

NEURAL NETWORKS AND ADAPTIVE PROCESSING Some newer theoretical approaches to learning and memory stem from biological evidence that memories are not localized in single units but reside in larger neural networks. Minsky & Papert (1969) demonstrated the inability of the simplest type of network to acquire and retain concepts in the presence of competing ones. Computer simulation later showed that networks of greater complexity are able to acquire fairly complex concepts. (For more on this see Churchland & Sejnowski 1992, Grossberg 1982, McClelland et al 1986.)

3-D INFERENCES FROM SEQUENTIAL 2-D VIEWS A major conceptual problem of visual perception is how the brain takes the two-dimensional (2-D) display on the retina (binocular vision is not required) and infers from it a (usually) unique 3-D world populated with objects, some partially obscured by others. It has long been known that from a single view, a continuum of 3-D worlds could have given rise to the 2-D display. So some degree of dynamic input is needed. It has been established empirically that very little additional information is sufficient, at least in sparse displays, to permit a unique 3-D inference. A second view adds so little information that the mathematical 3-D possibilities, although more constrained, are still not unique. Yet, human subjects typically report a unique percept. The conclusion, therefore, is that the brain must have built-in constraints on the inferences being made (Bennett et al 1989, Marr 1982, Ullman 1979). The major task, and it is a difficult one, is to characterize as fully as possible the nature of these built-in constraints. Solving such problems is not only of significance to psychology, but also promises to have a significant impact on the design of visually perceptive robots.

Comparable problems exist in hearing. It remains unknown how the brain partitions the temporal sound wave form, as transduced by the ear and peripheral nervous system, into streams of speech, music, or noise. Again, one suspects that an important contribution is to be made by mathematical formulations of the constraints and processes involved.

Problems of Testing Models of Change

A characteristic of many approaches to change, including neural networks and nonlinear dynamics, is the unobservable nature of the basic underlying mechanisms. The attempt is made to evaluate the models qualitatively in terms of the overall behavior. Little can be done to verify the underlying dynamics directly. This becomes a tricky issue for evaluation. It is not like complex processes in much of physics, which are built up from applications of basic fundamental laws that led to explicit equations such as the Navier-Stokes equations in fluid mechanics or Maxwell's equations in electromagnetism. We simply do not know the underlying nonlinear dynamics of psychological behavior; so, we attempt to infer it, using trial and error, from overall behavior of the system. This observation applies equally well to the attempts some have made to attribute complex social behavior to some unknown dynamic processes leading to complex patterns of behavior.

Even when explicit processes are postulated, such as the incremental models of the 1950s, it is extremely difficult to test their adequacy. Consider a situation for which there are choice probabilities, $P_j(i)$, for choosing alternative i on trial j. How does one estimate the probabilities to be able to study directly the dynamic recursion from $P_j(i)$ to $P_{j+1}(i)$? Surely, one cannot average responses over trials because, by the very nature of the topic, they are changing. Only in the strictly linear case is averaging over subjects justified, and even then considerable care is required not to confuse oneself. In nonlinear cases, averaging is completely unjustified unless one is working with actual clones, which may soon be possible with animals. The only solution that I know of to the estimation and testing problem is to work out the probability calculations for sequences of responses and to compare those with the patterns actually observed. Coupling our lack of knowledge about local dynamic mechanisms with these statistical difficulties, it is hard to be optimistic about our ability to test these nonlinear models effectively.

STRUCTURE AND NOISE

As scientists, our primary interest is in the structure imposed by the mental processes under study, witness the models mentioned earlier. However, we are always faced with variability in the data, which often makes it exceedingly difficult to judge the adequacy of a structural model. Three approaches for dealing with variable data are discussed below; none is yet fully satisfactory.

Statistical Modeling

The most conventional approach to noisy data is the statistician's. A structural model—often a simple additive one, as in regression and analysis of variance—is stated and a random variable is added to the result to describe the errors in observing the process. Psychologists are well acquainted with this style of modeling, and its methodology is de rigueur if one is to publish an empirical paper. Anderson (1981, 1982, 1991) and his group have used this approach extensively in studying the structure of a variety of psychological attributes.

Some of us are deeply skeptical about this approach. Perhaps the most thorough critique is Gigerenzer & Murray's (1987). One criticism is that only rarely can we expect that the particular linear (or log-linear) model in question describes simultaneously both the numerical measures obtained in our study and the assumed statistical model. Usually some a priori unknown transformation of the data should be made to put the observations into the simple structural form. But we do not have any reason to believe that the assumptions of the statistical model (often Gaussian distributions or equality of variances) hold for any but a very special, unknown transformation and not necessarily the one that leads to the structural model. There is no automatic compatibility between structure and statistics.

We attempt to brush these difficulties under the rug. We do computer simulations and attempt to establish some degree of robustness, and we try various ad hoc transformations of the data, but we do not really have a truly satisfactory way to arrive simultaneously at the underlying structure and statistics.

In this connection, evidence of interactions is usually a signal of trouble. It tells us that the statistical assumptions are grossly violated or that the structure is not what we had hoped it might be or both. Sometimes we are led to a transformation that successfully rids us of the interactions (Folk & Luce 1987), knowing full well that at least one of the statistical tests surely violated the assumptions of ANOVA. All too often, in my opinion, the interactions are treated as a finding and not as evidence of a lack of understanding of the combining rule for measures of the independent variables.

Probabilistic Models

A second tack is to suppose that the basic structure is not at all algebraic in character, but rather that the observables are response probabilities. The area of psychometric testing falls into this camp. In an area of interest to me, decision making, one postulates a probability $P(a,b)$ of choosing alternative a over alternative b rather than supposing they are simply ordered by preference, $a \succsim b$. We have already encountered examples of such modeling in signal detection.

This tack treats the probability as an inherent aspect of the model, rather than as a statistical add-on (Falmagne 1985, Doignon & Falmagne 1991). The difficulty with the approach appears once we go beyond the simplest case of just orderings and attempt to incorporate additional structure. We seem to encounter highly intractable conceptual problems. The only cases for which we have had any success involve, in one way or another, either an assumption analogous to Weber's law (Falmagne 1980, Falmagne & Iverson 1979, Narens

1994) or replacing the random variables by their medians (Falmagne 1976). For example, suppose we are modeling choices according to stimulus intensity and have as part of our structure the fact that intensities can be physically added. Let a and b be two intensities and let $a \circ b$ denote the joint presentation or concatenation of a and b. The problem is to understand how various choice probabilities relate. For example, how does $P(a \circ b, c \circ d)$ relate to $P(a,c)$, $P(a,d)$, $P(b,c)$, and $P(b,d)$? If Weber's law is true, we can say something; otherwise, we do not have the slightest idea how to proceed.

Lack of a Qualitative Theory of Structure and Noise

In my view, the problem of meshing structure and randomness is a very deep one, one for which we do not seem to have a good idea about how to proceed. It is a matter of putting into a single mathematical framework the axiomatic ideas of measurement that describe how to go from qualitative algebraic observations to their numerical representation and the numerical ideas of probability or statistics. The difficulty in doing so resides partly in the lack of a qualitative theory of randomness. We can discuss randomness only numerically, in terms of random variables. Thus, we do not have any natural way of putting together the qualitative ideas of measurement with the numerical ones of statistics.

Let me outline the kind of theory I believe we need in the simple case of concatenation. Let A denote a (dense) set of stimuli and $\circ$ a concatenation operation over them. In the usual algebraic theories of measurement there is, in addition, an ordering $\succsim$ over A. In the noisy case, we are unable to impose properties like transitivity and monotonicity because any observation we make may be spoiled by noise, so such regular patterns cannot be expected in our observations. But I don't know what to substitute for it. The idea would be to axiomatize whatever we have in such a fashion that the representation would be into a family of random variables $\{X_a : a \text{ in } A\}$. For example, in this case a suitable representation would be into a family of gamma-distributed random variables with the property that $E(X_{a \circ b}) = E(X_a) + E(X_b)$, where E denotes the expectation operator. In such a representation the expectations act like the classical algebraic theory for what are known as extensive structures (e.g. mass measurement).

Our failure to make any progress on this problem since it was recognized several decades ago is discouraging. Until we get some insight into its nature, I do not foresee a satisfactory solution for coping simultaneously with structure and error.

Chaos

Chaos theory—the newest kid on the block—is based on the premise that the process under study is fully deterministic, a nonlinear dynamic system. As mentioned earlier, simple nonlinear processes can generate exceedingly complex behavior. In particular, small changes in the parameters of such systems can lead to vastly different patterns, some of which are literally chaotic. This has become the primary approach taken with many physical systems (e.g. aerodynamic turbulence) for which previous theoretical treatments were not satisfactory.

The natural question is whether chaotic human or social behavior can and should be thought of as arising from deterministic dynamic systems, rather than being thought of statistically or probabilistically. Social phenomena sometimes exhibit marked discontinuities, and there certainly is a good deal of apparent randomness in behavior. The difficulty with this approach is the crudity with which the dynamic processes are known. Until they are pinned down in much more detail, one cannot view this approach as more than an interesting speculation.

CONCLUDING COMMENTS

Tensions are rarely resolved, but are adapted to and modified. Is that to be expected of these four?

PHENOMENOLOGICAL VERSUS PROCESS MODELING In physics and applied physics they continue to co-exist, even when a detailed model at one level accounts, at least in principle, for the properties at a higher level. One does not predict the paths of space probes using particle physics. My guess is that any successful phenomenological model will always be seen as an explanatory challenge to process modelers, but that the latter will rarely supplant the former in all applications.

DESCRIPTIVE VERSUS NORMATIVE MODELING Aside from metaphysical considerations, this is not a distinction made in the natural sciences. I do not see how psychology can avoid dealing with both types of models. Surely reasoning, inference, and decision making will be guided by normative principles—indeed, they are well established disciplines independent of psychology—and psychologists cannot but be intrigued by how these activities are actually conducted in daily practice. In particular, it is important to understand exactly when and how people depart from normative principles.

DYNAMIC VERSUS STATIC MODELING If psychology is at all like the other sciences, it will tend increasingly toward dynamic descriptions. We are being held back from developing fully dynamic models not because we fail to recognize the importance of change. Rather, the data we deal with are inherently noisy, and the usual averaging procedures suitable in static situations are exceedingly difficult to use when subjects differ from one another either in the parameters of the process or by employing qualitatively different processes.

NOISE VERSUS STRUCTURE We simply do not know how to model randomness at the same qualitative level at which we can model structure. Our attempts to bypass this discrepancy are, in my opinion, less than satisfactory. Moreover, the findings of the past 10 or 15 years about nonlinear dynamic systems call into question whether the actual source of the noise is randomness or ill-understood dynamics.

These last two, interrelated tensions strike me as the most significant. Here profound changes in mathematical modeling could take place, and until they do, modeling will remain limited and, to a degree, unsatisfactory.

ACKNOWLEDGMENTS

I thank Donald Hoffman and Geoffrey Iverson for useful comments on this manuscript, which is based, in part, on the University of California, Irvine 1994 Distinguished Lectureship Award for Research.

Literature Cited

Anderson NH. 1981. *Foundations of Information Integration Theory.* New York: Academic

Anderson NH. 1982. *Methods of Information Integration Theory.* New York: Academic

Anderson NH, ed. 1991. *Contributions to Information Integration Theory,* Vols. 1–3. Hillsdale, NJ: Erlbaum

Ashby FG, ed. 1992. *Multidimensional Models of Perception and Cognition.* Hillsdale, NJ: Erlbaum

Bennett B, Hoffman D, Prakash C. 1989. *Observer Mechanics: A Formal Theory of Perception.* New York: Academic

Busemeyer JR, Townsend JT. 1993. Decision field theory: a dynamic-cognitive approach to decision making in an uncertain environment. *Psychol. Rev.* 100:432–59

Bush RR, Mosteller F. 1955. *Stochastic Models for Learning.* New York: Wiley

Cho Y, Luce RD, von Winterfeldt D. 1994. Tests of assumptions about the joint receipt of gambles in rank- and sign-dependent utility theory. *J. Exp. Psychol.: Hum. Percept. Perf.* In press

Churchland PS, Sejnowski TJ. 1992. *The Computational Brain.* Cambridge, MA: MIT Press

Debreu G. 1960. Topological methods in cardinal utility theory. In *Mathematical Methods in the Social Sciences, 1959,* ed. KJ Arrow, S Karlin, P Suppes, pp. 16–26. Stanford, CA: Stanford Univ. Press

Doignon J-P, Falmagne J-C, eds. 1991. *Mathematical Psychology: Current Developments.* New York: Springer-Verlag

Falmagne J-C. 1976. Random conjoint measurement and loudness summation. *Psychol. Rev.* 83:65–79

Falmagne J-C. 1980. A probabilistic theory of extensive measurement. *Philos. Sci.* 47: 277–96

Falmagne J-C. 1985. *Elements of Psychophysical Theory.* New York: Oxford Univ. Press

Falmagne J-C, Iverson G. 1979. Conjoint Weber laws and additivity. *J. Math. Psychol.* 86:25–43

Fishburn PC. 1982. *The Foundations of Expected Utility.* Dordrecht: Reidel

Fishburn PC. 1988. *Nonlinear Preference and Utility Theory.* Baltimore, MD: Johns Hopkins Press

Folk MD, Luce RD. 1987. Effects of stimulus complexity on mental rotation rate of polygons. *J. Exp. Psychol.: Hum. Percept. Perf.* 13:395–404
Gigerenzer G, Murray DJ. 1987. *Cognition as Intuitive Statistics.* Hillsdale, NJ: Erlbaum
Green DM, Luce RD. 1973. Speed-accuracy trade off in auditory detection. In *Attention and Performance,* ed. S Kornblum, 4:547–69. New York: Academic
Green DM, Swets JA. 1988. *Signal Detection Theory and Psychophysics.* Palo Alto, CA: Peninsula
Green PE, Srinivasan V. 1990. Conjoint analysis in marketing: new developments with implications for research and practice. *J. Mark.* 54:3–19
Gregson RAM. 1988. *Nonlinear Psychophysical Dynamics.* Hillsdale, NJ: Erlbaum
Grossberg S. 1982. *Studies of Mind and Brain.* Dordrecht: Reidel
Healy AF, Kosslyn SM, Shiffrin RM, eds. 1992. *Essays in Honor of William K. Estes,* Vol. 1–2. Hillsdale, NJ: Erlbaum
Herrnstein RJ, Prelec D. 1991. Melioration: a theory of distributed choice. *J. Econ. Perspect.* 5:137–56
Herrnstein RJ, Vaughan W Jr. 1980. Melioration and behavioral allocation. In *Limits to Action: The Allocation of Individual Behavior,* ed. JER Staddon, pp. 143–76. New York: Academic
Indow T. 1982. An approach to geometry of visual space with no a priori mapping functions: multidimensional mapping according to Riemannian metrics. *J. Math. Psychol.* 26:204–36
Kahneman D, Tversky A. 1979. Prospect theory: an analysis of decision under risk. *Econometrica* 47:263–91
Krantz DH. 1975a. Color measurement and color theory. I. Representation theorem for Grassman structures. *J. Math. Psychol.* 12: 283–303
Krantz DH. 1975b. Color measurement and color theory. II. Opponent-colors theory. *J. Math. Psychol.* 304–27
Krantz DH, Luce RD, Suppes P, Tversky A. 1971. *Foundations of Measurement,* Vol. I. New York: Academic
Link SW. 1992. *The Wave Theory of Difference and Similarity.* Hillsdale, NJ: Erlbaum
Luce RD. 1959. *Individual Choice Behavior.* New York: Wiley
Luce RD. 1986. *Response Times: Their Role in Inferring Elementary Mental Organization.* New York: Oxford Univ. Press
Luce RD. 1990. Rational versus plausible accounting equivalences in preference judgments. *Psychol. Sci.* 1:225–34. Reprinted (with minor modifications) 1992. In *Utility Theories: Measurements and Applications,* ed. W Edwards, pp. 187–206. Boston: Kluwer
Luce RD. 1991. Rank- and sign-dependent linear utility models for binary gambles. *J. Econ. Theory* 53:75–100
Luce RD. 1992. Where does subjective-expected utility fail descriptively? *J. Risk Uncert.* 5:5–27
Luce RD, Fishburn PC. 1991. Rank- and sign-dependent linear utility models for finite first-order gambles. *J. Risk Uncert.* 4:29–59
Luce RD, Green DM. 1972. A neural timing theory for response times and the psychophysics of intensity. *Psychol. Rev.* 79:14–57
Luce RD, Krantz DH, Suppes P, Tversky A. 1990. *Foundations of Measurement,* Vol. 3. San Diego: Academic
Luce RD, Narens L. 1985. Classification of concatenation measurement structures according to scale type. *J. Math. Psychol.* 29: 1–72
Luce RD, Raiffa H. 1989. *Games and Decisions: Introduction and Critical Survey.* New York: Dover
Luce RD, Tukey JW. 1964. Simultaneous conjoint measurement: A new type of fundamental measurement. *J. Math. Psychol.* 1: 1–27
Luce RD, von Winterfeldt D. 1994. What common ground exists for descriptive, prescriptive and normative utility theories? *Manage. Sci.* 40:263–79
Macmillan NA, Creelman CD. 1991. *Detection Theory: A User's Guide.* New York: Cambridge Univ. Press
Marr D. 1982. *Vision.* San Francisco: Freeman
McClelland JL, Rumelhart DE, PDP Research Group. 1986. *Parallel Distributed Processing. Explorations in the Microstructure of Cognition,* Vols. 1–2. Cambridge, MA: MIT Press
McGill WJ, Teich M. 1991. Auditory signal detection and amplification in a neural transmission network. In *Signal Detection,* ed. M Commons, J Nevin, M Davison, pp. 1–37. Hillsdale, NJ: Erlbaum
McNeil BJ, Pauker SG, Sox HC Jr, Tversky A. 1982. On the elicitation of preference for alternative therapies. *N. Engl. J. Med.* 306: 1259–62
Michell J. 1990. *An Introduction to the Logic of Psychological Measurement.* Hillsdale, NJ: Erlbaum
Minsky M, Papert S. 1969. *Perceptrons.* Cambridge, MA: MIT Press
Narens L. 1985. *Abstract Measurement Theory.* Cambridge, MA: MIT Press
Narens L. 1994. The measurement theory of dense threshold structures. *J. Math. Psychol.* In press
Narens L, Luce RD. 1976. The algebra of

measurement. *J. Pure Appl. Math.* 8:197–233

Neimark ED, Estes WK, eds. 1967. *Stimulus Sampling Theory.* San Francisco: Holden-Day

Newell A. 1990. *Unified Theories of Cognition.* Cambridge, MA: Harvard Univ. Press

Norman MF. 1972. *Markov Processes and Learning Models.* New York: Academic

Ollman RT. 1966. Fast guesses in choice-reaction time. *Psychonomic Sci.* 6:155–56

Payne JW, Braunstein ML. 1971. Preferences among gambles with equal underlying distributions. *J. Exp. Psychol.* 87:13–18

Pfanzagl J. 1971. *Theory of Measurement.* Würzburg: Physica-Verlag. 2nd ed.

Prelec D. 1982. Matching, maximizing, and the hyperbolic reinforcement feedback function. *Psychol. Rev.* 89:189–230

Quiggin J. 1993. *Generalized Expected Utility Theory: The Rank-Dependent Model.* Boston: Kluwer

Roberts FS. 1979. *Measurement Theory.* Reading, MA: Addison-Wesley

Savage LJ. 1954. *The Foundations of Statistics.* New York: Wiley

Slovic P, Lichtenstein S. 1968. Importance of variance preferences in gambling decisions. *J. Exp. Psychol.* 78:646–54

Sternberg S. 1963. Stochastic learning theory. In *Handbook of Mathematical Psychology,* ed. RD Luce, RR Bush, E Galanter, 2:1–120. New York: Wiley

Suppes P, Krantz DH, Luce RD, Tversky A. 1989. *Foundations of Measurement,* Vol. 2. San Diego, CA: Academic

Swensson RG. 1972. The elusive trade-off: speed versus accuracy in visual discrimination tasks. *Percept. Psychophys.* 12:16–32

Swensson RG, Edwards W. 1971. Response strategies in a two-choice reaction task with a continuous cost for time. *J. Exp. Psychol.* 88:67–81

Townsend JT, Ashby FG. 1983. *Stochastic Modeling of Elementary Psychological Processes.* Cambridge: Cambridge Univ. Press

Tversky A, Kahneman D. 1986. Rational choice and the framing of decisions. *J. Bus.* 59:S251–78. Reprinted 1986 in *Rational Choice: The Contrast Between Economics and Psychology,* ed. RM Hogarth, MW Reder, pp. 67–94. Chicago: Univ. Chicago Press

Tversky A, Kahneman D. 1992. Advances in prospect theory: cumulative representation of uncertainty. *J. Risk Uncert.* 5:204–17

Ullman S. 1979. *The Interpretation of Visual Motion.* Cambridge, MA: MIT Press

von Winterfeldt D, Chung N-K, Luce RD, Cho Y. 1994. Tests of consequence monotonicity in decision making under uncertainty. *J. Exp. Psychol.: Learn. Mem. Cogn.* In press

Wakker PP. 1989. *Additive Representations of Preferences: A New Foundation of Decision Analysis.* Dordrecht: Kluwer

Wakker PP, Tversky A. 1993. An axiomatization of cumulative prospect theory. *J. Risk Uncert.* 7:147–76

Wandell BA. 1977. Speed-accuracy tradeoff in visual detection: applications of neural counting and timing. *Vis. Res.* 17:217–25

Ward LM. 1979. Stimulus information and sequential dependencies in magnitude estimation and cross-modality matching. *J. Exp. Psychol: Hum. Peform. Percept.* 5: 444–59

Ward LM, Lockhead GR. 1970. Sequential effects and memory in category judgments. *J. Exp. Psychol.* 84:27–34

Wittink DR, Cattin P. 1989. Commercial use of conjoint analysis: an update. *J. Mark.* 53: 91–96

Yellott JI Jr. 1971. Correction for guessing and the speed-accuracy tradeoff in choice reaction time. *J. Math. Psychol.* 8:159–99