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*Mathematical Modeling of Perceptual, Learning, and Cognitive Processes**

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Introductory Outline

Since experimental psychology mostly involves discrete responses at discrete times, it had no great compatibility with the largely continuous mathematics of the nineteenth century. But this has changed radically with the growth of mixed discrete and continuous ideas: set theory, probability and stochastic processes, and ordered algebraic structures. In this paper, a variety of specific examples of modeling are drawn from sensation and perception, learning and memory, measurement and scaling, and cognition and decision-making. Four conclusions are drawn. (1) Not everything that looks both mathematical and psychological is actually very satisfactory modeling. (2) We have not been as successful as we would like in separating theories of the organism from the boundary conditions of specific experiments. And when we fail to do so, it is difficult for knowledge to accumulate. (3) Theories are either stated at just one level (behavioral) or at two levels (cognitive as well as behavioral), and behavior is explained in terms of mental or physiological concepts. The latter, although highly appealing, suffer from severe problems of

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nonidentifiability of explanatory concepts unless physiological data can be brought to bear. This has only really been done successfully in sensory work. (4) The apparently happy match of stochastic processes with experimental procedures has suffered from the need to cope with the control or strategy flexibility exhibited by subjects. This has led many to believe computer simulation is the easy way out. Some doubts are expressed.

Were one to take seriously the idea that scientific psychology began when Wundt founded a formal laboratory in 1879, one would have considerable trouble in understanding the initial interplay between mathematics and psychology. Among others, Fechner and Helmholtz would be banished, and that makes quite a dent in the early history of psychophysics. Of course, most of our graduate students, close adherents that they be of William James, would applaud the banishment of early psychophysics and would, no doubt, urge it for all of psychophysics.

The Growth of Mathematics Compatible with Experimental Psychology

The Nineteenth-Century Incompatibility: Continuous Mathematics and Discrete Psychology

In a way, it is somewhat surprising that mathematics and psychology were partners early on at all. Consider the mathematics available at the time, say, in the third quarter of the last century. Basically one had Euclidean geometry, which only a few mathematicians were aware was in the process of being dethroned as *the* geometry; various bits and pieces of algebra, especially linear algebra; analytic geometry, which is a lovely exploitation of structural parallels between algebra and geometry; a miscellany of results about the integers (number theory); and most important of all for applications, analysis—the calculus, ordinary and partial differential equations, theory of functions of a complex variable, Laplace and Fourier transforms, and the like—the basis on which physics had become mathematized, general, and predictive over the preceding two centuries. To a good first approximation, the mathematician or physicist who was likely to think about psychology was one who was familiar primarily with the mathematics suited to continuous or mildly discontinuous phenomena.

By contrast, the methods being evolved by experimental psychologists were highly discontinuous. Laboratory responses, then as now, usually were discrete events in time—a key pressed, a word spoken, a peck observed—and more often than not they were forced to be doubly discrete by making the times at which they could occur—trials—discrete as well. Even the moderately popular reaction times, which we now think of as continuous random variables, were not readily captured using the methods of analysis.

The widespread use of limited response alternatives occurring at prescribed times is a fact of laboratory psychology that has greatly influenced our history. From the point of view of this symposium, it prescribed rather closely what mathematics is compatible with our data. More generally, however, it has forced a radical idealization of what is involved in the interaction between organism and environment.

One Twentieth-Century Compatibility: Probability and Statistics

At the end of the last century and the beginning of the present one, the development of set theory, which very neatly put in the same framework finite, countable, and continuous sets, was an almost ideal match to the methods of the experimentalist. This became especially apparent with the creation during much the same period of probability theory, with its identification of chance events as sets, with the incorporation of the crucial concept of independence of events into the measure-theoretic ideas underlying the concept of an integral, with the careful isolation of the empirically essential concept of a random variable and its distribution function, and with the resulting evolution of ideas about estimation, inference, and correlation that constitute statistics. As long as one was willing to ignore response times as a measure of behavior, which it was easy to do since their accurate recording was rather awkward until recently, the observed data fit well the developing mathematics. With minor effort, relative frequencies could be extracted from data, and they looked like estimates of probabilities. True, there was the issue of whether the observations are actually independent and the strong possibility that the underlying probabilities sometimes are not fixed, as in learning, but a basic compatibility appeared to exist.

The earlier growth of analysis was symbiotic with both the field theories of physical force—gravity and electromagnetism—and the continuous theories of matter, in solid, liquid, and gaseous states; whereas, the later growth of probability and random variables was highly compatible with the largely discrete methods of psychology. It is, of course, true that statistics and probability have found widespread use throughout the social sciences and to a degree in the biological and physical ones, but I daresay that nowhere has this sort of modeling been more compatible than with experimental psychology.

Some Developing Incompatibilities

This clear compatibility of the probability models and the major methodological constraints of much of experimental psychology—discrete responses at discrete points of time—coupled powerful and developing mathematical methods together with vast aggregates of data and well-explored paradigms for getting additional data when needed. Such coupling was especially conspicuous in two areas: sensation and learning, topics which I discuss more fully later. During the time from 1925 to

1965, when much of the attempt to exploit the possibilities was under way, it was apparent that some limitations marred the picture. Like many obvious things, little was said about them in print, but much informal conversation together with subsequent events makes clear that the limitations were widely recognized. I shall cite three.

First, the great body of operant data was mostly ignored by model-builders. The reason was not so much Skinner's firm opposition, largely accepted by his disciples, to modelers (Skinner, 1950), although that was surely a factor, as it was the fact that the data were from free-response situations and so did not exhibit the familiar discrete trial structure.

Second, most of the models dealt with response probabilities but not response times. Yet a growing body of evidence suggested that these times exhibit an interesting, if complicated, structure. Moreover, the view began to be expressed quite explicitly that perhaps these continuous measures provide an important key to studying hypothesized internal decision processes.

Third, increasing evidence from cognitive experiments showed that subjects have varied strategies of coping with more-or-less complex perceptual, verbal, and memory tasks. And it has been less and less clear how to adapt stochastic processes to deal with them. Among other things, the paradigm of fixed, usually small, sets of response alternatives pretty well excluded any adequate study of verbal behavior and, more generally, of any responses that exhibit a degree of creativeness.

The past fifteen years have seen major attempts to break out of these limitations. The first two are a part of my story and I talk about them below. The third has more to do with the growth of modern linguistics and psycholinguistics, artificial intelligence, and parts of cognitive psychology about which I am not very expert, so I shall leave that to others.

A Second Twentieth-Century Compatibility: Abstract Algebra and Measurement

Before turning to more detailed matters, let me mention another part of my story, a second type of application of set theoretic methods to our problems. This century has seen a flowering of what is called "modern" or "abstract" algebra. It involves the isolation of structures that can be thought of as a collection of objects which are related to one another by means of operations—something like addition or multiplication—and/or by means of an ordering—something like greater than of numbers—and/or by means of other more complex relations. Such structures, whole classes of them, are studied by working out the mathematical consequences of axioms that describe how the operations and other relations behave. The axiomatic method is ancient—it was, after all, Euclid's method for organizing geometric results into a systematic statement of axioms and the development of their consequences as formal theorems—but it played almost no role in the flowering of analysis following the invention of the calculus by Newton and Leibnitz. By contrast, modern mathematics has seen a number of deep and beautiful axiomatic developments, among them group theory and topology.

It had been clear for much of the last century—at least since Helmholtz (1887/1930)—that certain classes of ordered algebraic structures have some bearing on the widespread use of numbers in science. True, numbers can enter just by counting instances, which is how we estimate probabilities. But that is not how classical physics got the numerical measures with which we are all familiar, the ones to which units such as meters, ergs, ohms, and so on are attached. Rather, the structure of the system of numbers somehow systematically mimics certain data structures: greater than of numbers more-or-less accurately reflects an empirical ordering such as the tipping of an equal-arm pan balance, and the operation of addition reflects what happens when two objects are combined by placing them together on the same pan. Indeed, one can argue the view that any science must take its start from crude, qualitative changes that can be unambiguously perceived with the unaided senses, and the first theoretical task is to formulate laws about such observations, from which it may then be possible to provide a convenient numerical representation. At least, this is what appears to have been done, almost unconsciously, in developing the number system and using it to represent basic physical phenomena. Thus, for physics, the only purpose in actually developing axiomatic theories of measurement is to understand exactly what had so successfully evolved over centuries of commerce, barter, and finally scientific systematizing.

Matters have not been so simple for the social and behavioral sciences, in particular, psychology. Measurement has not come easily, and it is frustrating. We all speak of endless attributes that seem to exhibit the most essential feature of a measure, namely, order. There is more or less of utility, of intelligence, loudness, hunger, aggressiveness, fear, and so forth.

Such are our variables, our subject matter; yet it is doubtful if we know how to measure any of them in a fully satisfactory manner. That fact, perhaps more than any other, has pervaded our science in its first century. To be sure, we have a multimillion dollar industry based on the "measurement" of intelligence, but I doubt if there are many scientific psychologists who have much confidence that we know a great deal about the concept of intelligence or how to measure it well. Loudness and brightness remain to this today problematic in psychophysics, with no real consensus on how best to measure them in the service of developing psychophysical theory. Hunger we continue to index by hours of deprivation, knowing full well that that is not true for ourselves.

Our measurement problems lie at two levels. First, there is the question of how to order the attributes empirically. How do we decide whether a particular white, middle-class, well-educated male is more or less intelligent than, say, a black, ghetto-educated school-drop-out female? How do we decide whether a rat is equally hungry on two different experimental occasions? For the most part—psychophysics may be the major exception—we have been unable to solve this empirical problem in an intellectually satisfying, principled way. And without a solution, measurement is blocked.

The second problem, the mathematical one, assumes an ordering is given, and it concerns itself with the properties exhibited by the ordering and with the classes of numerical representations that are compatible with those properties. A great deal of work has been carried out on this topic, and we understand quite fully the structures that have been important in physics and the generalizations that may prove important in psychology, once we get adequate empirical orderings of attributes of interest.

Psychophysics and Perception

Representation of Signals as Random Variables

Since mathematics was first applied in psychology to psychophysics, let me begin there. Aside from some curve-fitting techniques, little happened after Fechner until Thurstone (1927a,b,c), who I would say was the first mathematical psychologist in this century. Why the wait? I suspect it was primarily for the arrival of the formal concept of a quantity whose value varies somewhat from observation to observation but which, nonetheless, can be thought of as unitary—what we now call a "random variable." Thurstone did not use that term, but he exploited the concept. He (and almost everyone after him who has modeled psychophysical phenomena) assumed that when signals which vary in one dimension are presented, each may be treated as if it is represented in the mind by a single number which fluctuates a bit from presentation to presentation. By slicing up the scale of the representation into intervals corresponding to possible responses, the exact location of the representation on each trial determines the response on that trial. Although Thurstone was much interested in conceptual matters, his followers tended to be swept up with the complexities of estimation, fitting, and computation, which at the time were grave. He and they failed to note, or to make anything of, the fact that the model strongly suggests that the subject can, by varying the response criterion, effect a trade-off of response errors. It was another twenty-five years before the importance of that was first recognized.

Psychologists connected with the Second World War were brought into contact with two major ideas from engineering and one from statistics: the theory of signal detectability, information theory, and decision theory. All three are a part of our story.

Theory of Signal Detectability

The theory of signal detectability (Green & Swets, 1974; Swets, Tanner, & Birdsall, 1961; Tanner & Swets, 1954) gave a plausible account of how a vector representation of a signal, which probably exists since a number of peripheral neurons are excited by any signal, can lead to a normally distributed random variable much as Thurstone had assumed. But it also placed great emphasis on the error trade-off —called the ROC curve—resulting from criterion changes. And it applied decision theory to explain those changes. When psychologists looked, subjects did indeed exhibit error trade-offs somewhat, but not exactly, like those predicted by decision theory (Green, 1960; Swets et al., 1961). As a result of those discrepancies, attempts have been made to use learning models to account for criterion change (Atkinson & Kinchla, 1965; Dorfman & Biderman, 1971; Dorfman, Saslow, & Simpson, 1975; Luce, 1963). In part because the theory of signal detectability does not generalize in a very satisfactory way beyond two signals, numerous later models have provided alternative accounts of the Thurstonian random variables. One group of these assumes they arise from some sort of aggregation of neural pulses where pattern and/or rate are affected by the signal (Grice et al., 1979; Green & Luce, 1973; Luce, 1977a; Luce & Green, 1972, 1974; McGill, 1967; Siebert, 1968, 1970). Work along these lines was heavily influenced by the developing understanding of how information is encoded in the peripheral nervous system (Galambos & Davis, 1943; Kiang, 1965; Kiang et al., 1962; Rose et al., 1967, 1971).

Information Theory

Until the late 1950s, information theory (Shannon, 1948; Shannon & Weaver, 1949) was a major psychological fad: many papers resulted, but they had, I fear, little of lasting import for psychology. Since we all agree that much of our concern is information processing, why was this so? There are at least two reasons. First, the theory is concerned entirely with the statistics of messages—with, for example, how to encode a message to combat chance errors in the transmission—but it is not at all concerned with conveying or extracting meaning. Our concern is largely the latter and how that manifests itself in the behavior of organisms. Second, the theory makes much of a measure, the expected value of $-\log P$, called "entropy" or "uncertainty." Whenever a probability vector or distribution is collapsed into a single statistic—be it a mean, a variance, an entropy, or what have you—care must be taken to ensure that nothing much is lost. This was ultimately shown wrong for entropy in psychology. For example, much was initially made of the fact that mean-choice reaction time is linear with the entropy of the stimulus display—Hick's law—until Hyman (1953) carefully studied the component parts and showed their times were not simply related to $-\log P$.

As one would expect, there are some important residues (an early summary is Luce, 1960). One, I believe, is a result made famous by Miller (1956) as one of the three empirical bases for his concept of a magical number 7 ± 2 limiting our information-processing capabilities. One can state the result easily without reference to information theory. Two 1000-Hz tones 5-dB apart can be absolutely identified by anyone with normal hearing. But seven tones spread at 5-dB steps cannot be; the intervals have to be roughly tripled. Why is this important? Because it means that the Thurstonian random variable depends not only on the signal that is presented, but also on those that might have been presented. If a particular tone is presented, the variance of its representation is roughly an order of magnitude larger if it is in the context of identifying one of seven than if it is one of two tones (Braida & Durlach, 1972; Durlach & Braida, 1969; Gravetter & Lockhead, 1973). This strongly violates most peoples' intuitions about signal transduction, and therefore it poses a conceptual problem. Luce, Green, and Weber (1976) have attempted to resolve it in terms of selective attention affecting the neural sample sizes on which the representations are based, and Shaw (1980) has cited some supporting evidence in another area. But not everyone is convinced this explanation is correct.

Sensory Scaling

Another theme dating from Fechner and interlaced with the problems just discussed is sensory scaling. Fechner (1860/1966) thought he had solved it by postulating that a just-noticed sensory change is the same everywhere, no matter the size of the physical change required to produce it (see Falmagne, 1974, for a modern summary of the mathematics). Thurstone (1927b) simply had it as the expected value of his random variable representing the signal, but for computational reasons he wound up being forced essentially into Fechner's mold. Stevens (1957, 1961, 1975) said both were wrong in assuming any necessary interlock between the mean and the variance, citing physical mechanisms of various sorts as cases in point. He invented methods of so-called direct scaling to get at it, but showed little interest in how these measures related to the rest of psychophysics, where variability and error were the data. The debate still swirls actively today. Increasingly, however, scales play a role in theorizing about psychophysical phenomena of all sorts, and the so-called direct methods are more a problem to be explained than a direct insight into the mind.

Criteria for Psychophysical Modeling

For me, there are four criteria that must be met by any proposal to understand psychophysical data. First, the internal representation of a signal should be consistent with peripheral neurophysiological data, and in particular, while it must depend upon the stimulating conditions just before, during, and just after the signal presentation, it should not depend upon the experimental context within which the signal is embedded. Second, a model for any psychophysical experiment, from detection through cross-modality matching, should simply be an account of a plausible decision process carried out on the signal representation. Third, any experimental method, "direct" or not, does not, by fiat, provide a direct avenue to some truth. It requires a theoretical account just as much as any other experimental procedure. If it is shown that some statistic of some method is proportional to the expected value of the signal representation, then that is, indeed, a nice way to estimate a sensory scale. But that needs to be shown, not assumed. Finally, my fourth criterion, which is perhaps more controversial but I think equally valid, is that the theory should give a natural account of response times. In particular, it should explain speed/accuracy trade-off results. Any theory that starts by assuming a random variable representation does not easily meet this criterion. Rather, one must explicitly postulate how information accumulates with time, and this means that the representation must be a sequence of random variables—a stochastic process—in order to account for the times taken to respond. Examples of such models are Green and Luce (1974), Link and Heath (1975), Ratcliff (1978). Although I think these criteria are clear, and possibly philosophically sound, carrying out work along these lines has proved trickier than one might have contemplated, and we are far from a fully articulated comprehensive psychophysical theory that encompasses the best known procedures.

Miscellaneous Visual Models

Once one turns from general matters to specific sensory modalities, there is such an overwhelming amount of research that one hardly knows what to say. Let me, therefore, pick several examples with strong mathematical themes. The first theme is Fourier methods. The concern here is with alternative representations of a wave form, one as a function of time and the other as a function of frequency; or, in statistical terms, a distribution and its characteristic function. This duality has long played a role in audition, and in recent years has come to be highly dominant in vision where it is thought that one aspect of perception may be a spatial Fourier analysis (Graham & Ratliff, 1974; Robson, 1975). Such theories are inherently perceptual since the calculation of the transform is over a large region; it is, by its nature, not a local concept. This has led to a program of experiments involving sine wave gratings. It is premature to say where this is going to end, but it clearly has had several effects, among them that physical scientists have been attracted to work on visual perception, and there is a deepening grip of mathematical methods in these areas. Let no one approach these gratings who is unwilling to study advanced calculus.

The second theme is geometry, in particular, the geometric nature of visual perception. We view an outside world that physics tells us is three-dimensional and, at the speeds with which we can deal, locally Euclidean. It is projected on two two-dimensional surfaces which can be moved in certain ways, and out of that is somehow constructed an internal model of that physical world. What are the geometric properties of such a representation? This is no place to try to detail any results: suffice it to say that the question is less easy to answer than it might seem

(Blank, 1959, 1961; Indow, 1974; Luneberg, 1947; Suppes, 1977), and it continues to be under active investigation.

Continuing for a moment with vision, let me cite a final area, color vision, in which mathematical modeling has and continues to play a role. Empirical evidence has accumulated over the years that makes clear that there is an initial physiological three-color code which is rather radically altered for some behavioral purposes into a coding of opponent colors (DeValois & DeValois, 1975; Hering, 1878; Hurvich & Jameson, 1955; Jameson & Hurvich, 1955). Active work goes on in an attempt to understand mathematically what is involved here and how it relates to other aspects of vision; one lovely axiomatic study of this is Krantz (1975).

Learning and Memory

The attempts mathematically to model learning do not trace back nearly so far as do those for psychophysics. Hull (1943), and to a lesser degree Spence (1956), undertook early mathematical formulations, but today that elaborate program is largely dismissed as not very satisfactory modeling. More mathematically satisfactory work was carried out by Thurstone (1930) and Gulliksen (1934), but it suffered from the fact that it only tried to deal with average performances. As we now know, many quite different theories lead to the same or very similar mean learning curves, and one must provide a much more detailed understanding of the entire process. Just as the entropy measure often results in a combining of dissimilar things, so, too, the mean may cause interesting information to vanish from sight. For example, the mean may be the same whether the distribution of behavior is tightly centered about the mean or is U-shaped, with a fraction of the animals doing one thing and the rest doing something totally different.

The Rise of Markov Models

Serious modeling of learning awaited the development and infusion into psychology of knowledge about stochastic processes. Such processes are simply collections of random variables indexed by time. If the time variable is discrete—conventionally the integers—then it is said to be a "discrete-time stochastic process"; if time is continuous—conventionally the real numbers—then it is called a "continuous-time process." Although the case of independent random variables is of great interest in statistics, in learning, each random variable, which describes the propensity to select among the alternatives at an instant in time, is a function of the past events of the process, and so is by its very nature not independent of the past. The task is to describe the dependence.

In the 1950s two major ideas arose, both involving discrete time processes corresponding to learning experiments with trials. The one was the linear operators of Bush and Mosteller (1955). Basically, on each trial the organism is assumed to be described by a vector of response probabilities over the possible responses. A response is made according to the distribution, a reinforcement occurs, and together they determine a new vector for the next trial, with the changes all being linear. (Later, I suggested a class of nonlinear models in which the order of application of the operators was immaterial [Luce, 1959, 1964].) Note that such models have a very important, simplifying feature known as the "Markovian property": the current vector of probabilities depends on the preceding one, but not on any before that; it does not matter whatsoever how one gets to that vector; there is no memory other than that embodied in the previous vector.

At much the same time, Estes (see Atkinson & Estes, 1963, for a summary) was developing his theory of stimulus sampling for learning. Here the key idea was a mechanism of associative memory, involving what he called stimulus elements that are individually associated with responses. On each trial a sample of the elements is selected, the relative balance of associations thereby determining the response made. Following the response, the conditioning of the sample is or is not changed, depending on the reinforcement of that trial. This, like the Bush-Mosteller model, has the Markovian property, but unlike theirs, the possible probabilities are restricted to a finite set, and this led to what is technically called a "Markov chain." In a certain limiting case, it approached the linear operator model.

The Demise of Markov Models

Under the influence of Atkinson and Bower (for surveys see Atkinson, Bower, & Crothers, 1965; Atkinson & Juola, 1974; Greeno, 1974), it was not long before a family of Markov chain models for learning and memory processes were developed that were entirely independent of the stimulus-sampling interpretation. A characteristic difference between the operator models and the chain ones was that the former suggested the learning occurred in small steps, the latter, in rather discontinuous jumps in memory states. In a classic experiment, Bower (see Atkinson et al., 1965, Chap. 3) showed that of the two types of models, the discontinuous one was clearly the better. This led to a massive program at Stanford University of Markov chain modeling and to the group of people, sometimes more or less affectionately referred to as the Stanford Mafia, who have been major actors in developing the memory part of cognitive psychology. Curiously, as that developed, fewer and fewer mathematical models were involved, and there was more and more computer simulating. Let us examine what happened.

The routing of the operator model was relatively complete. True, Norman (1972) developed a beautiful general theory of them; they continued to play a minor role in psychophysics; and they have come to play a role outside psychology in engineering and elsewhere. Nonetheless, Markov chains won the day; and yet a decade later they were passé. As I see it, there were two related major reasons. The first was the Markov property itself. This made for easy analysis, at least for Markov chains, since their theory is very complete, but these were memory models that denied much real memory or else forced incredibly complex memory states. They could not account in a reasonable way for the fact that an animal resists extinction far longer after partial reinforcement than after 100 percent reinforcement. The models admitted hardly any cognitive power, no notion of analysis of patterns from the past, none but the most primitive memory.

The second problem is far more subtle, but, in my opinion, crucial. Let me state it this way. These models failed to meet the following criterion, which I consider to be essential for successful modeling of behavior: that there should be a theory of the impact of stimuli and reinforcement on the organism separate from a description of the environment within which the organism is placed, and together these two generate a model of the organism in that environment. For those familiar with physics, it is analogous to having a theory of the relevant physical variables embodied in a system of equations and a set of boundary conditions describing the particular context within which the process is unfolding. In particular, parameters about the organism that are estimated from two distinct experiments should agree to within the error resulting from the data. Whenever one has only models of experiments, with no separable theory of the organism, then one has a hopeless feeling that no information is accumulating. The Markov chains were models of the combined subject and experiment, and no separation was suggested. This, by the way, was not really true of Estes's original stimulus-sampling model.

Another fact about these models, although I think it did not especially bother those at Stanford, was that they were limited to discrete trial experiments and did not encompass free responding. There was a little work (Donio, 1969; Norman, 1966) to generalize them to operant situations, but it did not attract much attention. During the past five years or so, however, operant psychologists, largely under the leadership of Herrnstein, have begun to develop mathematical models for various operant procedures (de Villiers, 1977; Herrnstein, 1974). Two major lines are being pursued. One is to try to view the behavior as resulting from some sort of continuous-time stochastic process that is being affected by the experimental reinforcement schedule which is itself a continuous-time stochastic process that may or may not depend on the behavior (Heyman & Luce, 1979; Staddon & Motheral, 1978). The other thrust is to try to involve ideas from economics, treating the rats and pigeons as if they are "economic men." The preliminary evidence is that animals fit that model rather well, better than human beings do (Rachlin, 1979; Rachlin & Burkhard, 1978; Shimp, 1975), providing that one does not insist on maximization of overall reinforcements.

Memory and Response Time

As the emphasis shifted from learning and Markov chains to a concentration on memory, another important change occurred. The focus came to be less on accuracy of performance—the studies were mostly designed so that performance was nearly perfect—while the emphasis was on the time to carry out the information processing (Audley, 1974). Perhaps the earliest and clearest example of that was the technique evolved by Sternberg (1969a, b). The basic idea is that by manipulating various stages of the internal processing, one affects the response times. The problem is to infer from the times the nature of the processing involved. This continues to be a very appealing idea, one that should continue to receive a great deal of attention, in particular drawing on the forms of the time distributions rather than just their means. But as Grossberg (1978), Townsend (1976), Theois and Walter (1974), and others have been at pains to point out, it is not without problems. Without information other than the time, it is probably impossible to infer any unique structure generating those times. Added constraints are needed. We run into the same problem in certain psychophysical modeling, but there at least we can draw upon peripheral neurophysiology to limit our choices; it appears to be less easy to do something comparable in cognition.

Measurement and Scaling

I have already made some general remarks about the, to me, important problems of axiomatic measurement, and I shall not pursue that much more except to mention what I consider the two or three most important developments.

Decision Theory

The earliest measurement theory totally distinct from those of physics, the one which convinced many of us that psychological measurement might ultimately prove tractable, was expected-utility theory and its subjective variants resting on the ideas of qualitative probability (Fine, 1973; Fishburn, 1970; Krantz et al., 1971, Chaps. 5, 8; Luce & Raiffa, 1957; Ramsey, 1931/1964; Savage, 1954; von Neumann & Morgenstern, 1944). The main idea was to study choices among alternatives where the outcome is partially under the control of chance. These concepts, together with both the multivariate ideas mentioned below and the use of Bayes's theorem to incorporate information in probability assessments, has spawned an area of some applied value called decision analysis (Bell, Keeney, & Raiffa, 1977; Pratt, Raiffa, & Schlaifer, 1965; Raiffa, 1968; Schlaifer, 1969).

In a sense, this can be viewed as one of the first attempts to provide a cognitive analysis of human responses—it is cognitive because the situation itself is subjected to analysis rather than being dealt with in some relatively reflexive or associative way. The resulting theory appears to provide a fine analysis of what is a rational approach to such problems, one that is partially descriptive of what people do. However, after a good deal of probing experimentation, it is now clear that people are doing something different. The evidence is overwhelming that context affects the decisions in a way we do not yet understand (Coombs, 1969; Coombs & Huang, 1970, 1976; Grether & Plott, 1979; MacCrimmon, 1968; MacCrimmon, Stanbury, & Wehrung, 1980). The area is so important, the data are so tantalizingly regular, and the modeling so elegant, I have no doubt that we will persist in trying to crack this nut.

Conjoint Measurement

Another major measurement development, dating back nearly twenty years (Debreu, 1960; Luce & Tukey, 1964), is conjoint measurement and its close relation, multiattribute utility (Keeney & Raiffa, 1976). This exploits the fact that an ordering of multifactor stimuli is really rather more structured than one first realizes, provided the factors can be manipulated independently. There is a trade-off between factors that can be studied and exploited. Physics has long done so—e.g., saying that kinetic energy is proportional to mass and to the square of velocity describes those trade-offs that leave the energy unchanged—but physicists and mathematicians failed to work out the corresponding qualitative theory. This has now been done not only for the cases of interest to physics, but also for a number of other cases that may be pertinent to psychology, especially ones for which the representation involves distributive mixtures of addition and multiplication (Krantz, 1972, 1974; Krantz & Tversky, 1971; Krantz et al., 1971; Narens, 1976; Narens & Luce, 1976). Elaborate computer programs now exist which make the applications of these methods quite practical, provided that the data are not too plagued by error (Young, 1972). The task of generalizing the models to handle error, important as it is, has only just begun with the work of Falmagne (1976) and Falmagne et al. (1979).

Before turning to other matters, I cannot refrain from mentioning that the development of theories relating additive conjoint and extensive measurement has provided an adequate qualitative account of the entire structure of classical physical quantities (Krantz et al., 1971, Chap. 10). Moreover, by bringing to bear the attempts to clarify the idea of meaningful measurement statements (Stevens, 1946, 1951), there has developed a satisfying explanation why natural laws are dimensionally invariant (Adams, Fagot, & Robinson, 1965; Krantz et al., 1971; Luce, 1978; Pfanzagl, 1971; Suppes & Zinnes, 1963). The upshot is a better understanding of why the useful method of dimensional analysis works.

Three Types of Scaling: Probabilistic, Functional-Measurement, and Multidimensional

From the point of view of the working psychologist, much of this work in measurement has seemed esoteric, and three other scaling methods enjoy far wider use.

The oldest are probabilistic—sometimes inappropriately called "stochastic"—models of choice which assume an underlying scale from which the probabilities arise. Thurstone's model, viewed in a wider context than psychophysics, is of this character, as is my 1959 choice-axiom model (Luce, 1959, 1977b) whose relations to Thurstone have been so neatly developed by McFadden (1974) and Yellott (1977). The data have made clear its limitations, which first Restle (1961) and later Tversky (1972a, b) have attempted to overcome by working out explicit notions of the similarity of stimuli which appears to play a crucial role in people's choices.

Next is the work of Anderson (1974) and his students on what they call "functional measurement." The context is similar to that of conjoint measurement, but instead of working with orderings and axioms, they begin with numerical data and an assumed representation and they fit the representation to the data using analyses of variance methods. The range of application has been impressive and influential, especially in the so-called "soft areas" of psychology.

The last and probably the single most successful area of scaling is that of multidimensional scaling, especially the nonmetric version (Carroll & Wish, 1974; Shepard, 1962, 1974; Torgerson, 1952, 1958, 1965). The input data concern the similarity of stimuli, and even numerical data are treated as providing only ordered information about similarity. The model is usually assumed to be n-dimensional, Euclidean space, with the ordinary distance metric reflecting the ordering of the data, although other spaces have been looked at. The procedure is to find that monotonic transformation of the ordering that yields the smallest number of dimensions providing an adequate account of the data. The mathematics of what is involved has never been fully worked out although there has been some partial work (Beals, Krantz, & Tversky, 1968; Tversky & Krantz, 1970); but computer software for doing what I have described is well developed. In a number of applications, the results have been most impressive, almost always giving a much more comprehensible representation of the data than does its major competitor, factor analysis.

Similarity and Categorization

As I said earlier, the major problem in all of these measurement and scaling applications is getting the empirical ordering of the attribute of interest. In the three scaling methods just discussed, this is resolved largely by getting subjects to establish the order. The subject tells the experimenter about the similarity of stimuli. This is fine as far as it goes, but one suspects that in the long run there needs to be developed a theory of similarity or, what I believe to be the same thing, categorization. Tversky (1977) has offered one interesting analysis, discussed by Krumhansl (1978), but perhaps the most striking developments along these lines right now are not theoretical but empirical. Especially important is the work on natural categories of Cerella (1979), Herrnstein (1979), Herrnstein and de Villiers (1980), and Rosch et al. (1976, 1978). It is not unusually difficult, by showing a pigeon slides of trees and branches which are reinforced and nontrees which are not, to draw forth the concept of tree so that the pigeon henceforth responds appropriately to new tree and new nontree slides. More striking, one can do the same thing using underwater shots of fish in natural habitats and fish-free water habitats, an environment not much a part of the recent experience of pigeons. Moreover, pigeons agree with us about what is a typical fish, making errors just when we do. We know little about this area, but it is clear that the instances of natural categories are complexly related to one another, and it invites much, much more work. This is something that seems basic, important for a behaving organism, and approachable in the laboratory. It will be an enormously important conceptual challenge to mathematical psychologists.

Concluding Remarks

In closing, I am supposed to assess where we are. That is complex, and anything I say is bound to be incomplete, unsatisfactory, and, probably, misleading. Nevertheless, I shall try.

1. Not everything published in psychology with equations or mathematical terms in it is, necessarily, a serious or satisfactory attempt to involve mathematics in theory development. Some cases are now easy to recognize—e.g., Lewin's (1935) hand waving of topology or Hull's (1943) ponderous learning theory. Other more recent cases strike me as more subtle, and I approach them with some concern that I am wrong. All the cases I have in mind exhibit a pattern. First, they note that some branch of mathematics, often a highly respected one, exhibits qualities not unlike the empirical ones of some branch of psychology. Second, to suggest that there is something to the analogy, attempts are made to identify certain of the mathematical concepts with usually informal psychological concepts. However, this step is notable for the lack of any detailed identification between the principal terms of the mathematical theory and specific empirical objects or relations. Third, the level of abstraction is usually very high, comparable to that of advanced physical theories such as quantum mechanics or the general theory of relativity, but this is in areas which, unlike physics, have not yet seen detailed, low level, empirically testable theories from which to generalize and abstract. Fourth, the sponsors are usually well-trained and often respected mathematicians whose knowledge of psychology and whose empirical experience, even in the physical sciences, is very sketchy.

At the risk of making enemies across the board, let me cite several specific examples of what I have in mind: catastrophe theory for any phenomenon that exhibits discontinuous jumps and hysteresis (Kolata, 1977; Poston & Stewart, 1978; Saari, 1977; Smale, 1978; Sussmann & Zahler, 1978; Zeeman, 1977); tolerance spaces as models of the mind (Zeeman, 1962); fuzzy set theory for anything involving an apparently imprecise boundary; and lie groups as a way to account for visual illusions (Paillard et al., 1977).

2. My second general observation is that we have failed more often than we have succeeded to construct theories of the organism rather than models of an organism in a particular experiment. Only to the extent that we begin to do that will our work achieve a cumulative character. To a degree, this has happened in sensory psychology, and I think that is one of the reasons that mathematics is such an integral part of that area.

3. Our theories are of two types, as in physics. One involves concepts and constructs at only one intellectual level—in our case, observed macro behavior. The operator models for learning and much of measurement and scaling is of this type. The other involves concepts and usually mechanisms at one level, often a postulated, unobserved mental process, to account for observations at a different level. The sensory random variables, the stimulus elements sampled in Estes's early learning theory, and the stages of memory encoding and processing of Sternberg's theory of memory are all of this character. These reductionistic theories seem far richer and intuitive than the wholly behavioral ones, which is both their appeal and their weakness. Only to the degree that one can bring to bear data appropriate to the explanatory level is it possible to avoid endless arguments about the identifiability of concepts. So far, this has been done successfully, and there only to a very limited degree, in sensory psychology. Other concepts that one hopes have a solid physiological basis, such as memory stores of various sorts, have so far eluded physiological isolation. Nonetheless, extremely interesting and deep investigations along these lines are beginning to appear, e.g., the current work of Grossberg (1980).

4. Although there is at least a superficial happy match between stochastic processes—i.e., time-indexed random variables—and our experimental procedures, whether with trials or free response, not all is well. The development of cognitive psychology has departed from that mode. Part of it, that having to do with grammar, draws on ideas of recursive functions and logic, and that having to do with memory has drifted more and more toward computer simulations, where complex options are easy to build in. Subjects seem to have available many alternative ways of behaving and it seems less stressful to most theorists to try to embody this in computer programs (Simon & Newell, 1974). I am yet to be convinced that this use of computer programs is really solving any problem. The difficulties we are having in cognitive psychology may be conceptual or experimental or both. We may be asking the wrong questions, given our current understanding, and we may not be getting under experimental control enough of what goes on in the typical cognitive

experiment. For example, understanding natural categories may be a necessary, though hardly sufficient, precursor to understanding semantic memory, yet many more scientists have been working on memory than on categorization. In any event, I think some cognitive psychologists may have thrown over mathematics for computer programs a bit too fast. Some—those whose knowledge of mathematics is largely restricted to Markov chains—may have found this the easy route, but others—most notably Simon—have made a conscious and well-informed choice. My own hope—and it is little more than that—is that cognitive psychology will be the source of a new interplay of mathematics and psychology, and perhaps in the long run the source of some new mathematics.

My conclusion that mathematics in psychology is here to stay will come as no surprise. To the degree that a scientist thinks there is some redundancy in what he of she observes and reports, that not everything is independent of everything else, then he or she is dealing with structure. And that is exactly what mathematics is the study of. The only problem is to isolate those structures appropriate to psychological phenomena. Our success in doing so, while considerable, is rather less than I should like to be able to claim. Still, mathematics and computer simulation are really the only games in town if you want to understand and to predict data.

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[1] It is quite impractical to give comprehensive references; the compromise I have followed, albeit imperfectly, is to cite both seminal papers and review articles and books, which will permit the reader to recover as much of the earlier history as desired.

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