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edited by
Ronald E. Mickens
Department of Physics
Clark Atlanta University
USA

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Psychology and Mathematics

James T. Townsend
Department of Psychology
Indiana University
Bloomington, IN 47405, USA

and

Helena Kadlec
Department of Psychological Sciences
Purdue University
West Lafayette, IN 47907, USA

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A general theme of the present volume is to be "why and how mathematics works to describe reality". These questions are perplexing enough even in the physical sciences where mathematics has been used with the greatest success. The employment of mathematics in the biological sciences is perhaps somewhat less pristine. But the issue of mathematics vis-a-vis psychology is likely a provocative one in fields with longer scientific traditions. There are no doubt those who even question whether a science of psychology can ever be developed, much less whether one is presently extant. Since we are experimental psychologists, we obviously believe that such a science is possible, and only a little space will be devoted to this question. Rather our main concern here will be with whether and to what extent mathematics can be used productively in psychology. Needless to say, our search is not for some arcane analogue of astrology, but a rigorous apparatus that will serve psychology as fruitfully, if not perhaps in the same way, as it has so gloriously benefitted physics and other credible sciences somewhat further down in the pecking order of rigor.

But to return momentarily to the more general question posed for this project. Presumably we can all agree that there is order and structure in the universe, however complex that order may sometimes turn out to be. One is reminded of a statement attributed to Albert Einstein, paraphrased to wit: "When I was 16 years of age I set myself the task of putting down the laws of the universe in simple mathematical terms. I now consider that my self-imposed goal was doomed. I realize that the laws of the universe must be described in complex mathematical terms." Nevertheless, there are similarities of structure that are quite general in nature and that to some extent may attempt to mimic one another - sometimes in hierarchy and sometimes more horizontally. There may be allusions here to self-repairing and self-reproducing automata as foreseen by von Neumann (1958) and all the happy growths from that line of investigation. In any event, humankind's mimicking of universal structure seems to take the form of our attempts to describing the order that appears to us through our sensory organs (along with the instruments of modern science), in part by way of mathematics. Other paths are, of course, religion, philosophy and so on.

One suspects that other intelligent civilizations would (have?) put together stridently different linguistically (mathematically, etc.) coded accounts of nature, especially if their physical construction differs radically from ours. It seems reasonable to assume, however, perhaps as an article of faith, that there would exist metamathematical mappings that would carry

their quantitative descriptions into ours and vice versa; at least for those classes of phenomena that intersect both "observable" universes.

With this general, if rather terse, philosophical background in place, we move to the more specific goals in this essay. The three specific questions that will be considered are: 1. Can a science of psychology be developed? 2. If so, should mathematics be involved? 3. If so, how can mathematics best be employed?

The reader may not be surprised at our answer to the first question, as this is written by psychologists. Because the primary focus here is to deal with mathematics and psychology, not much time will be spent on this question. Suffice it to say that we take it for granted that a human is basically a system, albeit a complicated one, and that it therefore can be studied. Our personal predilection is to study the psychology of the individual (as opposed, say, to the study of social interactions) by varying the input, observing the output and drawing inferences about the internal systemic structure and its functioning from those observations. Such inductive inferences can be fed back into theoretical structures which can then be used to derive new predictions about input-output behavior, and these subjected to empirical test. This, in turn, leads back to revision of the theory, and thus repeating the inferential cycle. The clear emphasis is on behavioral research but physiological underpinnings should be taken advantage of where possible in order to provide direction and information about the "hard wiring" as well as its "programming".

We envision a sort of psychological systems theory which would perhaps be a special instance of general systems theory. This theory would possess its own peculiar characteristics that differentiate it from, say, that associated with the theory of automata, but might overlap and borrow from such disciplines, as in the theory of identification of canonical classes of automata. Although other scientific psychologists, as opposed to those engaged in clinical practice, might use a somewhat different language to express their scientific outlook for psychology, it seems safe to describe the modal viewpoint as close to the above.

Nevertheless, there is still massive disagreement as to the specifics of a science of psychology, even within the field itself. A prime example of this was the recent discourse on "methods and theories in the experimental analysis of behavior" by the venerable Burris F. Skinner, the legendary behaviorist (1984). This paper was published in *Behavioral and Brain Sciences*, an interdisciplinary journal, along with commentaries by a number

of interested scientists including the first author. A formidable diversity of views is encountered there, to put it mildly. The reader is referred to that article for further discussion of these and similar issues and more references.

Question number two, whether mathematics should be involved in a science of psychology, might spark a bit more contention than the first one. First, it should be said that almost (but not quite!) no one in psychology would argue against the use of statistics in psychology; such use being ordinarily to describe data characteristics or test a hypothesis of some sort. In contrast, however, the present focus is on the use of mathematics as it is used in physics, as mathematical structures to serve as theories of psychological behavior, when the proper correlative definitions are drawn between the mathematical and empirical terms. To be sure, a brief scan of the major experimental and theoretical journals of psychology should convince one of the general acceptability of mathematical modelling and theorisation. In psychology's most pristine subdisciplines, namely *psychobiology* and *psychophysics*, it would probably be taken for granted that a certain degree of mathematics is required for the advancement of the field.

Parenthetically, *psychobiology* (an older, and in some ways more expressive cognomen is *physiological psychology*) is that area of psychology that investigates psychological phenomena (especially overt behavior) through the study of germane biological aspects of the organism; for instance, physiology, anatomy, and biochemistry. *Psychophysics* is the study and description of sensory processes and perception particularly through the measurement of psychological reactions to different quantitative levels of stimulation on various dimensions (e.g., loudness as a function of acoustic energy; color perception as a function of wavelength of the stimulating light).

However, even in other areas of experimental psychology which pride themselves on their rigor, historically there have been two factions opposed to mathematics in psychology. The one faction would claim that psychology is simply not a "mathematisable" field. The second would maintain that psychology is still at an early inductive stage of its development, a stage where inductive empiricism and experimentation with regard to qualitatively expressed hypotheses is appropriate. Later, when a large body of orderly data is available, perhaps more mathematical theory may be adopted. The latter view appears close to that of Skinner, mentioned above (Skinner, 1984).

Interestingly, it is not rare to meet a devotee of a hard science (physics,

chemistry, etc.) whose belief in the applicability of mathematics to nature stops short of psychology. Often, the said devotee can accept the general line of thought in Freudian or Jungian psychoanalysis – branches of psychiatry are not always in the best of odor even in that field, much less academic psychology – yet abhor the attempt to numerically measure psychological characteristics or more especially to render psychological theory in mathematical formulae. Presumably, “psychic (wo)man” should remain ineffable from the point of view of ordinary mundane scientific method.

It may surprise many from the traditional sciences to learn that experimental psychology was largely instituted in the nineteenth century by physicists and physiologists. To be sure, their avenues of investigation were largely guided by the philosophy of the times; particularly the British Empiricism of Locke, Hume, Berkeley, James and John Mill and others. Nevertheless, their methods came straight from the laboratory and the standard mathematics, especially the calculus of Newton and Leibniz, although the probability notions of Laplace, the Bernoullis, and Gauss were also present virtually from the start. Such well-known nineteenth century scientists as Helmholtz, Weber, Fechner, and Mach were heavily involved in the emerging science of experimental psychology (see, e.g., the classic history of experimental psychology by E. G. Boring, 1957). As psychology evolved to have a life of its own, its practitioners began to more formally identify themselves with the field, rather than with their original discipline. Here we see the first real psychological laboratory established in Leipzig in 1879 by Wilhelm Wundt (as every student in introductory psychology must memorize). Wundt’s original training was in physiology and medicine.

In answer to the second question then, our opinion is that mathematics should indeed be involved in psychological investigation, at the level of theory construction and testing. In fact we would, as have certain others, turn the argument that psychological phenomena are too complex for immediate mathematical application around on the proponents of that view. It is precisely because the phenomena are so complex that we must have mathematics. Even in relatively simple (one might suppose) areas of psychology, a reader of the literature can easily be led down the primrose path through verbal argument. The logic seems impeccable. However, when the psychological principles on which the theory are based are put into mathematical form, the stated predictions may fail to follow at all. Moreover, just as in other sciences, the predictions are often rendered more testable by being derived as mathematical propositions or theorems.

If the notion that mathematics can be helpful in psychological theory is tentatively accepted, the third question posed is how should it be employed to best advantage? Although mathematics has been employed in psychology for over a hundred years, Clark Hull may perhaps be credited with the first serious program to formally model a substantial body of (mostly) animal behavioral data concerned with learning and motivation. He believed that much of the theory presumably confirmed with laboratory rats could be generalised to human behavior (e.g., Hull, 1943, 1952). Originally trained in electrical engineering, Hull self-consciously attempted to implement the so-called hypothetico-deductive method, an approach adopted from his comprehension of Russell and Whitehead's *Principia*. His approach also seems to have borrowed much from the Vienna Circle logical positivists and Bridgman's operationalism, at least in its general philosophy.

Thus, Hull's idea was to provide a system of primitive undefined terms, succeeding definitions and then axioms and theorems. Next, the theory would be related to reality by offering correlating definitions from the primitive terms, or more realistically and more often, from later axioms or theorems to empirical objects or measurements. Now the theory was in a position to be tested by interpreting the remaining theorems as predictions, given the necessary initial conditions.

This approach is pretty much standard today although usually in a less self-conscious fashion. Actually, many of Hull's predictions and experiments may now seem more like elaborated curve fittings than true attempts at theory testing, but his efforts set a new standard at the time. Following his death in 1952, much psychological/mathematical theorising has been more modest, but often more rigorous. Certain developments have been relatively ambitious, for instance Estes' stimulus sampling theory (see, e.g., Estes, 1950; 1959; 1962; Atkinson and Estes, 1963) and Luce's individual choice behavior (Luce, 1959) attempted formulations to describe a wide variety of learning situation or decision situations, respectively. However, at that time, most mathematical theorising had developed models for fairly limited experimental paradigms.

In approaching the third question, one might anticipate that once one accepts the notion of the usefulness of mathematics in psychology, a uniform mode of application would be forthcoming. This is far from the case. However, the diversity of mathematics presently employed may also be viewed as beneficial at this stage of development, as psychology searches

for the appropriate mathematical descriptions within its various domains.

One way to view the spectrum of approaches is along the type of mathematics used; for instance, discrete versus continuous, deterministic versus stochastic, geometric versus algebraic, and the like. Although that has some advantages, it seems less useful than a depiction along the dimension of "process". That is, the types of quantitative theorising in psychology can be placed somewhere on a continuum representing the extent to which the theory details the hypothetical inner workings of the psychological processes. If a theory or model is low on the process dimension, it would tend to be high on the descriptive dimension; thus, "process" and "descriptive" tend to be polar opposites. What follows is a brief overview of the various areas in psychology which have fruitfully employed mathematics in their theorising. Hopefully this will convince the reader that mathematics has been useful in psychology, and will continue to be so even as we search for new mathematical models to guide our enterprise. We make no attempt to treat each area with equal space, but references are provided for those interested in more detail.

We begin with the area of *foundational measurement*, since one of the important questions one may ask is how can we measure psychological attributes. Foundational measurement takes as its goal the establishment of a rigorous theory of the assignment of number to real world qualities, especially psychological qualities. Because the traditional measurement of physics, which almost always presupposes the existence of extensive measurements with its associated ratio scale, is often too strong for the behavioral sciences, the theorists in this area have discovered other weaker types of measurement (e.g., Krantz, Luce, Suppes, and Tversky, 1971; Suppes and Zinnes, 1963; Narens, 1985; Pfansagl, 1968; Roberts, 1979; Stevens, 1946, 1951). Oversimplifying for the purpose of exposition, it may be said that physicists can rely on the existence of a unique origin or zero point on their scales; something that is rare in the behavioral sciences. Only the unit can typically be altered. This cannot be taken for granted in the behavioral sciences where, more often, an interval scale (in which both the zero point and unit are non-unique) or perhaps an ordinal scale (in which only the order of objects on the scale is unique) would be appropriate for measurement. Moreover, it seems fair to say that quite basic questions and some resolution to them, have arisen through such enterprises, with regard to the fundamental nature even of the so-called extensive (ratio scale) measurement case associated with physics (e.g., Luce and Narens, 1987).

The area of foundational measurement has been closely tied to psychophysics because the latter tries to measure the sensory and perceptual reactions of human beings to physical stimulation as in loudness, pitch, and brightness as unidimensional examples. Color perception is a classical example of a multidimensional measurement, or scaling, problem (e.g., Burns and Shepp, 1988; Krantz, 1974), and although it has usually been rather descriptive in nature, in some cases it has been related to more detailed process models of a psychological or physiological nature (e.g., DeValois and DeValois, 1975; Hurvich and Jameson, 1957; Livingstone and Hubel, 1988). As in other applications of mathematics to psychology (psychometrics has been a partial exception), it is closely aligned with a class of experimental techniques and associated data.

Psychometrics, which develops tests of various psychological attributes such as intelligence, aptitudes, psychological pathologies, personality attributes and so on, is relatively far towards the descriptive end of the continuum. Historically, this line perhaps began formally with Spearman's two-factor theory of intelligence (Spearman, 1904; 1927), and the method continues to enjoy considerable popularity in the social sciences. This was later developed into the general methodology of factor analysis (Thurstone, 1947).

Psychometrics was originally also very closely connected with measurement in psychophysics as well as with assessment of individual differences (e.g., Galton, 1883), but it later went its own way. Today there is, as most academics, clinicians and industrial psychologists are aware, a large industry based on various psychological tests. The way in which psychological attributes that appear on the psychometric scales are defined (typically implicitly rather than explicitly) have usually been rather static. Some attempts to point out certain problematic mathematical or methodological aspects of psychometrics seem to have gone largely unheeded (see e.g. Schönemann, 1981). Also, until recently there has been little attempt to develop an experimental and theoretical underpinning that might offer independent and explicit definitional status and confirmatory plexus to these attributes (as notable exceptions see, e.g., Embretson, 1985; R. Sternberg, 1984). Without such underpinning, the concepts tend to have a somewhat circular demeanor, in the view of the present writers.

An area of numerical research with wide applications that is historically related to the above approaches, is that of *multidimensional scaling* (e.g., Torgerson, 1958). It attempts to employ structure within a data

set, often ordinal in nature, to retrieve a plausible geometric configuration of points in a multidimensional space, associated with some metric, usually a member of the Minkowski family (e.g., Attneave, 1950; Eisler and Roskam, 1977; Carroll and Chang, 1970; Kruskal, 1964a, 1964b; Shepard, 1964). There has been some interchange between traditional psychometrics and multidimensional scaling but the latter has also been employed in an extremely wide diversity of other basic and applied settings.

In another domain, in the 1940's and 1950's the developments of cybernetics, theory of automata, mathematical communications and detection theory, statistical decision theory, and information and coding theory terrifically influenced the behavioral sciences by providing new analogies for the study of behavior. In particular, they helped lead the field back from a rather stark behaviorism (even in the modified neobehaviorism of Hull, Tolman, Spence and others), to a more centralised, cognitive perspective of psychological processes. Once again experimental psychologists became interested in what might be going on inside the cogniser - we could say, from a systemic point of view, not necessarily explicitly physiological. The 1950's saw a heavy spate of studies employing information theory developed by Shannon (Shannon, 1948; Shannon and Weaver, 1949) to study the number of bits of information processed in various tasks and other related concepts (e.g., Attneave, 1959). It was said, not without foundation, that psychology tended to confuse information theory as a measuring tool with a substantive theory, which it was not at least for psychology (see e.g. Luce, 1959).

Another important development of the 1950's was *mathematical learning theory* (e.g., Bush and Mosteller, 1955; Estes, 1950, 1959; Atkinson and Estes, 1963; S. Sternberg, 1963). The Bush and Mosteller (1955) line tended to be somewhat less process oriented than Estes' stimulus sampling theory (e.g., Estes, 1959), but both were heavily dependent on relatively simple first order (and usually linear) differential equations to represent learning dynamics. Elementary combinatorics also played a role, especially in the stimulus sampling theory. Although out of vogue for a number of years, mathematical learning theory continues to make an important contribution to the understanding of human cognition (e.g., Raaijmakers and Shiffrin, 1981; Riefer and Batchelder, 1988).

Almost coincident with the appearance of mathematical learning theory was the invention of *signal detection theory*, an amalgamation of principles of statistical decision theory and notions of signals and noise from

electrical engineering and applied physics (Peterson and Birdsall, 1953). It was almost immediately applied to human psychophysics and perception (Tanner and Swets, 1954; see also Green and Swets, 1966). Indeed, the prime originators were composed of electrical engineers (Birdsall and Peterson) and psychologists (Tanner and Swets) at the University of Michigan.

The early 1960's found a new paradigm emerging, the advent of the so-called *information processing approach*. Past issues of *Perception & Psychophysics* give a realistic picture of the evolution of this approach. This journal has closely followed the research in the area of information processing, especially those research problems that deal with perception. Charles Eriksen, the editor of *Perception & Psychophysics* over much of the period since its inception, has also influenced the information processing approach through his research contributions. It should be noted that this approach does not confine itself to information theory (see e.g., N. H. Anderson, 1981), and in fact rarely uses it. To the extent that it can be defined at all, it has tended to take as a metaphor a von Neumann type of digital computer (e.g., Arbib, 1964; Newell and Simon, 1963; von Neumann, 1958), if not something more simplified (e.g., S. Sternberg, 1966; see Roediger, 1980, for a discussion of the role of metaphors in the study of memory and Massaro, 1986 for a general treatment). Thus, the human information processor was often viewed as a set of processors arrayed in a series.

It was typically supposed that information entered by way of sensory apparatus and proceeded through the various processors (which varied with the particular cognitive or perceptual task under study) in a strictly serial fashion. Strictly serial here means that each subtask carried out by a process was completed before the information (used in a nontechnical sense) was sent on to the successive processor. Various psychological data were adduced to support seriality (e.g., S. Sternberg, 1966; Smith, 1968) and experimental and statistical methodologies were developed that depended on the truth of seriality of the processors for their success (e.g., S. Sternberg, 1969; cf. Townsend, 1971a, 1972, 1974). Other studies provided support for parallelity (e.g., Egeth, 1966; Egeth, Jonides, and Wall, 1972; Murdock, 1971). More recently, these methodologies have been developed for more general classes of processors including parallel and more complex networks (Fisher and Goldstein, 1983; Schweickert, 1978, 1983; Schweickert and Townsend, 1989; Townsend, 1984; Townsend and Ashby, 1983; Townsend and Schweickert, 1989).

Most of the work in the general domain of information processing has not been heavily mathematical, but where it has been, the primary tool has been elementary probability theory and stochastic processes and the main observable variables have been reaction time (e.g., McGill, 1963; Pachella, 1974) and accuracy measures (e.g., Townsend, 1971b, 1981). In more complicated systems, computer simulation and computation that was sufficiently complex to require computer calculation began to see more and more use. A seminal paper in the latter vein was presented by Atkinson and Shiffrin (1968), work that proposed an information process type of human memory model. While most investigations over the years have employed the information processing framework, many have relied on more qualitative and intuitive approaches to testing hypotheses rather than a strictly quantitative strategy or theory (e.g., Craik and Lockhart, 1972; Massaro, 1972; Posner, 1973).

The 1970's witnessed a growing amount of more rigorous attempts at mathematical modeling (e.g., Krueger, 1978; Link and Heath, 1975; Luce and Green, 1972; Ratcliff, 1978; Snodgrass, 1972, Theios, 1973; Theios, Smith, Haviland, Traupman and Moy, 1973; Townsend, 1974, 1976a, 1976b; Townsend and Ashby, 1978; Wickelgren, 1970), and at separating classes of mathematical models based on opposed psychological principles, through the derivation of experimental tasks by mathematical means (e.g., Pachella, Smith and Stanovich, 1978; Snodgrass and Townsend, 1983; S. Sternberg and Knoll, 1973; Yellott, 1971).

One topic that received a good deal of attention, both theoretical and empirical, was parallel versus serial processing of multi-element visual displays (see Schneider and Shiffrin, 1977, for an excellent review of this literature). Shortly after some influential papers appeared that argued for seriality (e.g., S. Sternberg, 1966), the author (Townsend, 1969, 1971a) and Atkinson, Holmgren, and Juola (1969) pointed out the existence of stochastic parallel models which could mimic the standard predictions of serial models. Over the last twenty years, we and our colleagues have striven to map out the regions of the model spaces where experimental discriminability was impossible; that is, where the models make identical predictions in experimental paradigms. Townsend (1976b) and Vorberg (1977) generalized the equivalence theorems to large classes of distributions. Much of this work and further theoretical developments, including results showing how parallel and serial mechanisms could be experimentally discriminated, are reported in Townsend and Ashby (1983) and Townsend (1990). Luce

(1986) reviews a great deal of the quantitative work on response times over the past thirty years or so. A terse example representative of how mathematical theorising can be used to develop tests of opposing psychological concepts will be given at the end of the paper.

In the meantime the dominant paradigm in *cognitive psychology*, which had by dint of fashion become an umbrella term (and still is) for virtually anything in psychology which was not strictly behavioristic, clinical, social or directly physiological (and sometimes, even those!), had evolved an even more fervid infatuation with computer simulation, and models based on digital computer ways of doing mental things. Thus, the birth of "cognitive science". Certain pioneers in artificial intelligence in general and computer science in particular, such as Simon, Newell, McCarthy, Minsky, Feigenbaum, Selfridge and many others, had always been interested in problems also considered as content matter for psychology, for instance problem solving, pattern recognition, speech and language production and processing, and so on. Many artificial intelligence theorists would be (and are) pleased when their models appear to be plausible candidates for human thought, while psychologists have also ventured into artificial intelligence (e.g., Hunt, 1975). However, when psychologists such as J. R. Anderson and Bower (J. R. Anderson, 1983; Anderson and Bower, 1973) began to get heavily involved in this type of work, experimental verification with human subjects became more of a daily integral part of such theorising. Norman and Rumelhart and their colleagues (Norman and Rumelhart, 1975) were also prolific contributors to this literature with an emphasis on psychological realism and laboratory and natural data. To jump to the present, perhaps the most ambitious project now in the offing in this general sphere is SOAR developed by Allen Newell with the aid of many top rate experts in artificial intelligence and computer science (Newell, 1988).

Inevitably there have been many facets of cognitive science *a la* computer models that have been almost tropistic to philosophers and philosophers of science. This cross-disciplinary mixing of computer scientists, physiologists, psychologists and philosophers has served up a fascinating brew of theory and sometimes experiment, and in some cases, pure invective! An intriguing theory has been developed by Lefebvre (1982, 1987; see also Townsend, 1983) which is inherently psychological, yet quite unconventional with a strong dose of concepts not typically considered in good odor in psychology (i.e., sufficiently reducible to experiment, etc.). Nevertheless, he has made strides in mathematically defining such notions as

conscience, ego, and good versus evil, and he is beginning to achieve a degree of experimental testing of his predictions.

One region of applied science with psychological implications is that of *decision making*. It has always (at least three hundred years!) had close ties with probability and statistics. Statistical inference is, of course, simply decision making under specified conditions. The Bernoullis, Laplace, Gauss, and many others provided the underpinnings of present day treatments, as later developed by Pearson, Fisher, Neyman, Savage and others. In the 1940's and 1950's, von Neumann and Morgenstern (1953) invented a brilliant axiomatic, and algebraic, machinery to deal with the concept of utility in uncertain environments. Their work has spawned an unbelievable number of papers and books over the past forty years or so. It seems to remain practically inviolate in the pertinent subfields of economics. Almost from the start, however, various of its axioms of predictions have evoked criticism from both economists (e.g., Allais, 1953; Ellsberg, 1961; Savage, 1954) and psychologists (e.g., Edwards, 1953, 1954, 1961, 1962) if interpreted as descriptors of human behavior.

Many modified theories, especially ones with less stringent conditions, have been proposed, especially by mathematical psychologists. A few of the latter are Luce (1959), Restle (1961), Fishburn (1976), Krantz (1967), Tversky (1967, 1969, 1972a, 1972b), Kahneman (Kahneman and Tversky, 1979; Tversky and Kahneman, 1981), and recently Goldstein (Goldstein and Einhorn, 1987), Miyamoto (Miyamoto, 1988; Miyamoto and Braker, 1988), and Busemeyer (1985), among others. Some, such as Coombs (1964) have developed alternative axiomatic approaches. All these latter approaches attempt to make the theories conform better to human propensities, including their decisional frailties!

Along the way, many nonquantitative experimental psychologists have been involved with proposing verbally based models and contributing data that have influenced the more mathematical endeavors (e.g., Payne, Bettman, and Johnson, 1988). Learning and motivational psychologists have also long been interested in decisions (e.g., Bower, 1959; Estes, 1976; Hull, 1952; Lewin (who was most of all a social psychologist), 1935, 1936). These investigators were inclined to take a more biological, or at least psychological, stance with regard to the background for decisions, with more attention to physiological substrates and environmental influences, especially those making themselves felt through learning and reinforcement (e.g., Hull, 1952; Tolman, 1959). Lewin (1935, 1936) observed that moti-

vational variables often seem to act in a style suggestive of physical field theory, and this in turn was taken up by Hull (1943, 1952) and Miller (1959). However, the mathematical work on such a theory, which would be appropriate for psychology, was quite limited.

Recently, the first author and Jerome Busemeyer have begun work on a dynamic field theory for decision making, which mathematically attempts to describe real space-time behavior by people in decision making situations (Busemeyer, You, and Townsend, 1988; Townsend and Busemeyer, 1989; see also Nakagawa, 1987). One of the important aspects of decision making not captured by other theories is the "gravity-like" effect that positive attraction towards a choice object (e.g., candy) increases as a person (or animal for that matter) gets closer to it. Similarly, if the object also has unpleasant aspects associated with it (e.g., it makes one fat!), the negative attraction (i.e., repulsion) also becomes greater, the closer one is to it. It is fascinating, and well-studied with animals if less so with humans, that the slope of the negative attraction function, with the argument being distance from the object, tends to be steeper close to the object than is the slope of the repulsion function (e.g., Miller, 1959; but see also Hearst, 1969).

We would be remiss not to mention a major trend in experimental, particularly cognitive, psychology, namely *parallel distributed processing* (PDP; e.g., McClelland and Rumelhart, 1986; Rumelhart and McClelland, 1986). There are many features that help to define this area, some of them more critical and some of them quite fussy (as with any real-life definition). One of its most important aspects is that it helped psychology to self-consciously move away from the digital computer metaphor toward models that may work 'more like the brain', in some sense. The latter phrase is in single quotes because no one knows for sure how the brain works although much knowledge has been gained in the last 100 years. In particular, we still do not know for certain how memories are formed physiologically, though ideas abound; although the jury is still out, it seems that Hebb's (1949) idea that it is due to some kind of synaptic modification may not have been that far off the mark (e.g., Brown, Chapman, Kairiss, and Keenan, 1988).

Ironically, if naturally, memory is one of the favorite topics of the PDP approach. Within psychology, two major lines have evolved in modelling associative memory, both of which use vectors to represent various features of stimuli to be stored; one employs convolutions for storage and correlations for retrieval (e.g., Cavanagh, 1976; Eich, 1982; Murdock, 1982;

Pribram, Nuwer, and Baron, 1974; but see also Schönemann, 1987), while the other uses linear matrix operations (e.g., Anderson and Hinton, 1981; J. A. Anderson, 1973; Hintsman, 1986; Humphreys, Bain and Pike, 1989; Willshaw, 1971; Kohonen, 1977; see, e.g., also Pike, 1984; Willshaw, 1981). Nevertheless, we can say that PDP models seem to qualitatively, and perhaps globally, pick up some of the flavor of how the brain appears to operate more naturally than has been typical with models relying heavily on notions from digital computer land. For instance, they tend to be soft-fail, to go down as a continuous function of extent of damage (e.g., neural damage in real life), rather than in all-or-none fashion. Sometimes, they can act in a holographic manner, each part of the structure possessing almost all of the stored information; something that Lashley discovered to be approximately true for mammalian brain over fifty years ago (e.g., Lashley, 1929, 1933; see also Beach, Hebb, Morgan, and Nissen, 1960). As the name indicates, the operations are also often more parallel than serial, the latter being another holdover from early digital computer analogies.

Although the primary use of these "neural network" models in psychology has been in the modelling of human memory, they have also been extended to other and wide-ranging psychological and physiological phenomena. An extensive theoretical framework based on such neural nets has been developed by Grossberg and his co-workers, which, in addition to memory structures, attempts to account for such psychological properties as perception, motivation, and decision-making (e.g., Grossberg, 1980, 1988). Other extensions include the modelling of vision and pattern recognition (e.g., Fukushima, 1986; Koch, Poggio, and Torre, 1982; Marr, 1982; Poggio, Torre, and Koch, 1985) and various kinds of problem solving (e.g., Hopfield and Tank, 1986; see also Levine, 1983; Scott, 1977 for reviews). It could be said that this direction grew out of the "perceptron" line which has its roots in the early stages of artificial intelligence that was based on simplified physiological principles of the day (e.g., McCulloch and Pitts, 1943; Rosenblatt, 1962), but was shown to be inadequate to compute relatively simple problems (Minsky and Pappert, 1969).

One of the problems that may plague this area, a problem that began to surface with the advent of computer simulation models, is that the models readily become so complicated that one could spend one's life striving to work out the laws of the functioning of the model. A number of investigators have voiced a concern about the testability of these neural net models (e.g., Ratcliff, 1981; Massaro, 1986; Schneider, 1987), and many

consequences remain to be worked out. No doubt as with any new approach with promise in diverse areas, there will be much "noise" published. However, there is much that is intriguing with the work going on here and there is hope that rather a lot of "signal" will manifest itself as well.

Maybe it should be a capital offense to be writing about a partly mathematical subject without any observable mathematics. In order to avoid prosecution, we will limn in a little example of modelling in human information processing.

Consider the situation where a person is presented two objects to perceive, call them A and B , both shown at the same moment. If perception is relatively fast, it may not be easy to determine whether the person is capable of processing both objects simultaneously (in parallel) or must perceive them one at a time (i.e., serially). Suppose that if processing is parallel, it is statistically independent on the two objects. We do not assume that processing is equally fast on the two objects; that is, the distributions on processing time can differ. If processing is serial, we permit either object to be processed first, say A is first with probability p and B is first with probability $1 - p$. Now, it turns out that in order to have the serial and parallel models equivalent here, it is basically necessary and sufficient for them to be equivalent on the minimum processing time. (It is certainly necessary and the equivalence mapping for the "second stage" duration is trivial.) Townsend (1976b) established this background and then proved some theorems about serial-parallel equivalence and uniqueness. It was actually done with relaxed independence conditions but the assumption of independence permits brevity of exposition here. Note also, that this construction is general to many kinds of processing situations.

With regard to the serial model, let the conditional density on the first processing time when the object is A , be $f_a(t)$ and for B when it is processed first, $f_b(t)$. Then there are two kinds of trials depending on whether A or B happens to be processed first. The probability density on the minimum processing, including the probability of it being A , is just $p f_a(t)$ whereas when the object is B , the density is $(1 - p) f_b(t)$. Observe that this order of processing may be under control of the subject, but this does not affect our theoretical results or their implications.

If processing is parallel then the minimum time density when A is done first is $g_a(t)G_b(t)$ where $g_a(t)$ is the actual density on A 's processing time and $G_b(t)$ is the probability that B has not been completed by time t . $G_b(t)$ is equivalent to one minus the cumulative distribution function on

B's processing time. It is known in actuarial statistics, quality control and reliability theory as the survivor function. Similarly, *B* can only be done first at exactly time *t* if *A* has not yet been completed so that its density is the mirror image of the other, namely, $g_b(t)G_a(t)$.

Now, if we are to have parallel-serial equivalence then the *A*-first serial functions must equal the *A*-first parallel functions for all positive *t* and similarly for the *B*-first functions. This leads to the pair of functional equations

$$\begin{aligned} p f_a(t) &= g_a(t) G_b(t), \\ (1 - p) f_b(t) &= g_b(t) G_a(t), \quad (\text{for } t > 0). \end{aligned}$$

It was shown that for any well-defined parallel functions, there exists an equivalent serial model, in the sense that p , $f_a(t)$ and $f_b(t)$ exist and are well-defined probability functions of the parallel expressions. On the other hand, there exist serial models, that is, functions $f_a(t)$ and $f_b(t)$ and a p such that no well-defined parallel functions can equal the left-hand side of the above equations (Townsend, 1976b). This means that for experiments that would meet the present conditions, the class of serial models is more general than that of the parallel models. Therefore, no data could satisfy the parallel model without satisfying the serial model as well. However, in principle, there could exist serially produced data that could not be replicated by any parallel models.

Ross and Anderson (1981) developed some statistical tests based on the above results and employed them with some human memory data. We cannot go into details of the experiment here, but the authors were able to conclude that their subjects' data satisfied the conditions for parallelity. That is, parallel processing could not be ruled out in favor of serial processing. On the other hand, because in this case, the serial class encompasses the parallel class, the process could also have been serial. Only if the test had rejected parallelity could a strong conclusion have been drawn in that particular study. Nevertheless, because the authors' theory of memory access predicted parallel processing, the outcome was a propitious one for their theory.

We have developed a number of other experimental designs, based on uniqueness results analogous to the above, so that it may not be necessary to rely wholly on one type of test. In some cases, the parallel models are more general than the serial and in others they are equally general, but each

possessing regions of uniqueness that enhances testability (e.g., Townsend and Ashby, 1983; Townsend, 1984; 1989; 1990).

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