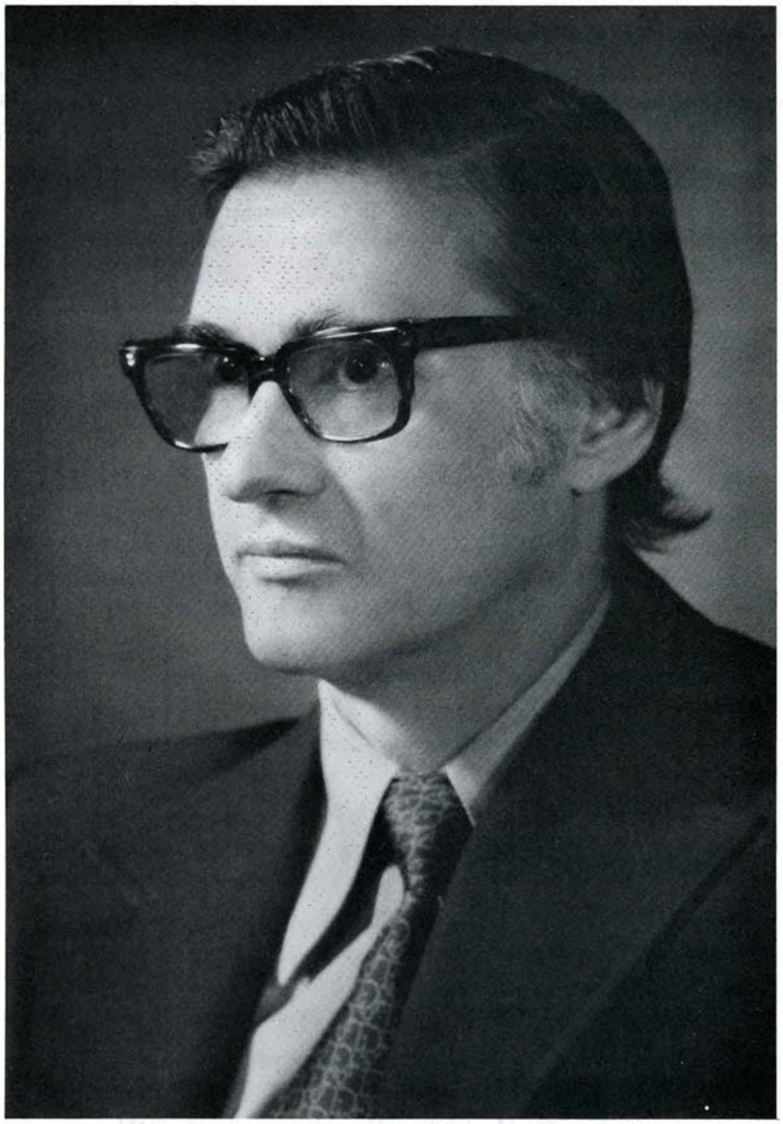

5

A Mathematician as Psychologist

R. DUNCAN LUCE

A scientific autobiography is, I suppose, a chronicle of the intellectual highlights of a scientist's career, the persons, places, and events that went along with them, and some attempt to suggest how one thing led to another. Presumably, the last interests a reader the most — how did an idea, an experiment, or a theorem arise? Yet it is this for which one is least able to provide an account. I have never read an autobiography, short or long, that gave me any real sense of the intellectual flow; nor as I sit down to contemplate my own intellectual history do I sense that flow very well. The actual work is too slow, too detailed, and too convoluted to be recounted as such. I believe I see some recurrent themes and intellectual convictions which probably have marked what I have done, but little of that seems causal. Therefore, I shall not attempt to impose much of a logic on my development beyond some grouping into themes and some mention of convictions.

I shall begin with the steps that led me into psychology. Next, I describe my research themes with little attention as to where, when, and with whom. Following that, I provide a brief chronology in which the highlights are cited and the intellectually important events and people are mentioned. Finally, I close with some musings about several general matters that strike me as important.

UNDERGRADUATE AND GRADUATE SCHOOL

My parents, although both college educated and my father professionally trained as a dentist, were hardly intellectuals, and as a child I never aspired to be one. As a teenager I preferred painting to science or mathematics, and I applied to college with some reluctance even though my high school record made it an obvious thing to do. When I arrived at M.I.T. in 1942, I opted for aeronautical engineering, mainly because of a romantic fascination with aeroplanes and flying. That passion did not die easily as evidenced by the fact that during one summer in Palo Alto, at age 39, I obtained my private license and a year later bought a light plane.

I soon discovered that engineering, at least as then taught, was not very congenial to me, but physical theory and mathematics were fascinating, even if difficult. By the summer of 1943 I was in the Navy V-12 program — the snobbery of the Navy being such

An initial draft was circulated to a few people who figure large in this account and they have all given helpful criticisms and comments which I have in most cases used. I would like to thank: Eugene H. Galanter, Henry Gleitman, David M. Green, Francis W. Irwin, Cynthia N. Luce, and Patrick Suppes.

that its officers, even during a major war whose outcome in 1943 seemed problematic, must have college degrees — and so I was no longer free to transfer out of engineering. That had to await graduate school. In 1945 I graduated and was elected to both the honorary engineering society Tau Beta Pi and to the scientific one Sigma Xi.

Following Midshipman School at Notre Dame, during which summer the war ended, I spent a brief, intense period in the Catapult and Arresting Gear School at the Philadelphia Navy Yard, as did all V-12 aeronautical engineers that year as a result of some clerical quirk. Then I was assigned as a catapult officer to the USS Kearsarge, which was receiving her final fitting out at the Brooklyn Navy Yard. In the isolation of her shakedown cruise I decided on applied mathematics rather than physics, and in 1946 returned to M.I.T. as a graduate student in the Department of Mathematics.

As well as I can recall, I rejected physics on two grounds: its very heavy involvement in weapons applications and its very high level of formal development. I felt that there must be other fields in which one could contribute in more peaceful ways and have the excitement of working in more virgin terrain. Just which fields were not clear to me. As a student, I was formally committed to a minor in hydrodynamics, but informally I exposed myself to some of the other intellectual ferment in the Cambridge area. At first I knew little of psychology, and economics seemed the more obvious choice. Indeed, I recall some early and feeble attempts to write down economic equations, but chance ultimately led me to concentrate on psychology.

The actual start of my career in psychology was, in a sense, sharply defined. One afternoon Albert Perry, a graduate student in Electrical Engineering at M.I.T., and I were modifying a military surplus radio into what then passed for high-fidelity equipment, when my roommate, William Blitzer, returned from Leon Festinger's class in social psychology. He described to us some of the combinatorial problems they faced in dealing with social networks. Soon Perry and I were busy trying to translate these into questions about matrices, and a few days later Blitzer introduced us, with some theorems in hand, to Festinger. By the end of the summer we had a paper ready for submission (Luce & Perry, 1949). Another paper on the same topic followed shortly (1950b).

Although I didn't know it for sure then, I was hooked. Still, the problem of a thesis remained — no one in the Department of Mathematics was interested in social networks and M.i.t. had no Department of Psychology. The nearest mathematical topic was Cybernetics, but I had not attracted Norbert Wiener's notice. For reasons not wholly clear to me, a young algebraist, I. S. Cohen, was assigned as my advisor on the, then, very unapplied topic of semigroups. It seemed a deflection; however, this experience in algebra has since proved invaluable in my work on the theory of measurement. Some twenty years later, I ran into W. T. Martin, the ex-Chairman of the Department, at a cocktail party and, to my surprise, he brought up the events surrounding my thesis and volunteered that the Department had erred in not letting me pursue my psychological interests. Perhaps so, perhaps not.

As work progressed on the thesis (1950a), a significant career decision had to be faced. Should I attempt the standard academic route in mathematics, largely suppressing my interest in applications to the social sciences, or should I attempt a major commitment to psychology or some other social science? My taste was for applied mathematics in spite of a pure mathematics thesis, and I was convinced, probably correctly, that I would not become a very distinguished pure mathematician. But knowing little about psychology, I was not at all sure how to go about entering the field, and being rather shy I was not especially adept at finding out.

It was all resolved by an accidental social meeting. Oliver Strauss, an M.D. working in the Research Laboratory of Electronics, who had some associations with Alex Bavelas's

Small Groups Laboratory, appeared as a guest of someone at the Beacon Hill apartment I shared with Louis Osborn, a physicist who has since been much involved with accelerators at M.I.T. and Harvard, and Alan J. Perlis, who is a well-known computer scientist now at Yale. I had earlier met Bavelas through Festinger, just before Festinger and the rest of the Center for Group Dynamics moved to the University of Michigan following Kurt Lewin's death, and I had done a little work for him. Strauss and I talked about my interests, and he soon arranged a position for me as Bavelas's captive mathematician. So a major career decision was reached through some blend of ignorance, predisposition, and chance; I suppose these are the usual ingredients and only the mix varies.

During the next seven years I often questioned whether I had not made a very foolish, irreversible decision. At this time departments of psychology hired statisticians, but not mathematicians with absolutely no psychological qualifications who aspired to do psychological theory. I had taken no courses in either psychology or statistics — few of the former and quite possibly none of the latter were available at M.I.T. at the time — but even as I picked up some statistics in self-defense I was convinced that I did not want that to be my major teaching role. So the initial stages of the career were rocky and I was often apprehensive.

RESEARCH THEMES

Aside from a few minor excursions, not usually of my own volition, my research can be grouped into four general topics: group interactions (including game theory), probabilistic choice theory, psychophysics, and the foundations of measurement. The first preceded and is rather independent of the other three, which have been closely interlocked both temporally and intellectually. Because of its length and because there is a natural break, I have subdivided the section on psychophysics into two parts.

Group Interactions

The work first stimulated by Festinger continued during my three years with M.I.T.'s Group Networks Laboratory. The main psychological idea was that many working groups of people have imposed upon them a communications structure which presumably affects their ability to carry out tasks. To study this in its simplest form, Bavelas had groups of five subjects sit around a table which was partitioned into wedges, and they passed notes to one another through slots in the center core. Any network could be imposed simply by closing the appropriate slots. The highly stylized notes provided a permanent, if clumsy, record of the communications. A number of empirical papers, including two long technical reports that were never rewritten into journal articles, were the output (1952c, 1953a, c, 1956d). Although we wrote a great deal and presented many data, I don't think we learned very much about communications in small groups.

The theory concerned the use of digraphs and matrices to represent the imposed structure. For a modern summary of such work, see Harary (1971) and Harary, Norman, and Cartwright (1964). My contributions included 1952a, 1952b, 1953b, and 1955b.

In part through the skeptical questioning of some of the superb group of psychologists then being collected in M.I.T.'s new Lincoln Laboratory, initially located just down the hall in the rickety "temporary" building in which we were housed, I gradually began to realize both that the graph theoretic models were not relating in any important way to the data we were collecting and that the data themselves were inherently not very interesting. As a result, I became receptive to better approaches and during my last year at M.I.T. began to study the theory of games as a possible model for some kinds of interactions. This model at least had in it actors who made choices, not the property-less nodes of the digraphs, and it included some considerations about communications among the participants. Of course, as I have pointed out several times (1954, 1955a, c, 1957), von Neumann and Morgenstern's concept of a solution for n-person games suffered from its failure to incorporate any of the structural constraints on communication that had earlier been my main focus.

My knowledge of game theory deepened after I moved to the Behavioral Models Project at Columbia University. Howard Raiffa, of the Department of Mathematical Statistics, was loosely associated with the project, and we agreed to write a short summary report on game theory designed primarily for social scientists. His interest was mostly in statistical decision theory and two-person games and mine in utility theory and n-person games. That short report evolved into the 600-page book, *Games and Decisions* (1957).

Before its publication, I had lost interest in game theory and, indeed, in the whole area of modeling social interaction processes. I concluded that in spite of the obvious great importance of such interactions, neither our experimental nor our mathematical techniques were adequate to the problem. To this day, I feel that the study of interacting human groups has failed to achieve a satisfactory meld of structural and psychological assumptions at the theoretical level and has also failed to capture much of significance at the experimental level. The fact that a problem is important does not make it tractable, and a scientist can be foolish to hammer at it as if it were. Furthermore, I had also begun to be tempted by other research topics in individual psychology.

Probabilistic Choice Theory

The shift of focus began during 1954-1955 when I was at the Center for Advanced Study in the Behavioral Sciences, Stanford, Calif. I had become fascinated with the von Neumann-Morgenstern theory of expected utility, with the Weber-Fechner problem of psychophysical scaling, and with their relation, if any. This started an interplay between algebraic and probabilistic approaches to choice and between utility and psychophysical scaling ideas which has dominated my intellectual life. I shall try to write about the three areas of probabilistic choice, psychophysics, and the foundations of measurement as if they were separate activities, but there can be no doubt that knowledge of each area has affected what I have done in the other two.

On returning from the Center to Columbia in late 1955, I divided my time between work on *Games and Decisions* and on the development of what I called the choice axiom.[1] This was a poor choice of terms, and I knew it at the time, because the axiom of choice exists in mathematics and is of ever so much greater importance. The dilemma was that I could not think of a suitable, alternative term for the intended interpretation: choice. I do not recall exactly where the idea came from, except as a natural probabilistic version of the concept of independence from irrelevant alternatives, first clearly isolated by K. J. Arrow (1951) in connection with his Nobel Prize winning work on social choice and which, in one form or another, has played such an important role in decision theory even though it is almost certainly wrong as a description of behavior. I am also not sure when I first wrote the axiom down, but probably it was during the winter of 1956-1957. By the spring of 1957, a 100-page, red-covered mimeographed technical report on it had been distributed to some interested people. That summer a number of them met for a mathematical psychology workshop at Stanford, and the "red menace" was a major

1. In essence, if we denote by $P(x;X)$ the probability that x is selected when the choice set X is offered, then the axiom asserts that for x and y in both X and Y and provided none of the probabilities is O,

$$\frac{P(x;X)}{P(y;X)} = \frac{P(x;Y)}{P(y;Y)}$$

focus of discussion, including some controversy between Patrick Suppes and me. One result of that was the beginning of our friendship. I rewrote the manuscript during the next academic year with an eye to publication as a Psychometric Monograph, but Gordon Ierardi of Wiley, who had published *Games and Decisions*, requested it even though he knew that its sale would be marginal. It appeared in 1959 under the title, *Individual Choice Behavior*, and again, not by accident, with a bright red jacket.

The book has four main chapters: the axiom and some of its direct consequences, followed by applications to psychophysics, learning, and utility theory.

The psychophysical models led to numerical results very close to those of Case V Thurstonian models,[2] which encouraged me to explore them further (1959d, f, 1961, 1962c,b, 1963c,d,e, 1964a,b). However, after some years of effort, I concluded that, except possibly as approximations in certain cases, this approach had not resulted in satisfactory psychophysical models for four reasons:

1. It did not lead in any natural way to asymmetric Yes-No ROC curves, which arise both in visual and auditory detection data.
2. The growth of the underlying v-scale with intensity was at least an order of magnitude faster than the scales obtained by magnitude estimation and other methods, although they were of the same power function form.
3. In spite of efforts to account for the limits on the information transmitted in absolute identification designs as the number of signals is increased — the magical number 7 ± 2 phenomonon (Miller, 1956) — no satisfactory explanation was forthcoming (1963c, pp. 171-177).
4. I was unable to work out any satisfactory way to incorporate response times into the model (1959f).

It was primarily these failures that led me to abandon this approach to psychophysics and to take up the neural mechanisms which are described later.

The work on learning was suggested by the linear operator models of Bush and Mosteller (1955). Once the ratio v-scale underlying the choice model was seen (see fn. 1) it was obvious to consider multiplicative operators on that scale. In terms of the probabilities, the operators are nonlinear, but with the mathematically happy feature of being commutative. This feature is probably not a psychologically happy one since it means that the saliency of a past event is independent of when it occurred relative to other events. Put another way, the model admits no suppression of the distant past. For a period I investigated this model (1959d,h) but ultimately Sternberg (1963) convinced me it was not satisfactory and interest in it, as in the other operator models for learning, waned. Work on this model did lead me, however, to study the class of all possible commutative operators (1964c), which involved me for a second time[3] with functional equations (Aczel, 1966) and contributed indirectly to my work on measurement.

The chapter on utility in *Individual Choice Behavior* introduced a probability axiom for certain pairs of gambles which says, in essence, that judgments about preferences are independent on judgments about likelihoods of events. That postulate coupled with the choice axiom was shown to lead to a curious prediction. A psychometric function for preferences between two gambles, with different outcomes associated with a common event, can be generated by varying the probability of that event. The prediction is that this psychometric function should be a step function rather than a continuous function of the event probability. Shipley and I (1962b), modifying the methods of Mosteller and Nogee (1956), ran an appropriate experiment and found supporting, though not con-

2. Models in which signals are represented as normally distributed random variables with the same standard deviations, and decisions are determined by the value of the random variable relative to fixed boundaries.
3. The earlier brush with a functional equation was in connection with work on the Weber-Fechner problem, which is described in the next section.

clusive, evidence. The experiment has never been replicated, and it remains an isolated fragment that seems not to have affected any later developments.

As my interest in the choice models diminished, I turned to the study of more general probabilistic models of choice. Some of that work was summarized by Suppes and me in 1965a and the dissertation of my student A. A. J. Marley was devoted to these same general ideas (Marley, 1965 a,b). He has continued to study them with success.

The coup de grace for choice theory (and many related approaches) was provided by Krantz (1964) and Tversky and Russo (1969) who showed that any (binary) choice model in which the choice probabilities can be expressed as a fixed function of scale values on the two alternatives is equivalent to several simple properties of the choice probabilities. Perhaps the simplest is this: Let $P(x,y)$ denote the probability of selecting x in favor of y. Then, for all alternatives x, y, and z, $P(x,y) \geq P(y,z)$ is true if and only if $P(x,y) \geq 1/2$ is true. A number of experiments, ranging from color perception (Krantz, 1967) to preference (Coombs, 1958), carefully designed to maximize the possibility of difficulties, showed that these properties can be violated, and so models of this type cannot be generally correct.

Investigating these choice ideas was probably a useful experience for me, and there may be limited contexts in which they can be employed. For example, Bush, Luce, and Rose (1964a) and Luce (1964g) showed how choice models can arise asymptotically as the result of certain linear learning operators. But as a general constraint on the response mechanisms, as I once thought the choice axiom might be, there is little doubt that it is not descriptive. I am now persuaded that attempts to characterize the response process axiomatically are futile, although the axiomatic approach appears to be quite useful in trying to understand the central tendency of the transformation of physical energy into a neural representation of it on which sensory judgments are based. To characterize the full process one seems to be forced to consider in greater detail the nature of the stimulus coding in the nervous system and exactly what it is that the decision processes are trying to do. For a general discussion of these matters, see Luce (1974j); this approach is illustrated below by my psychophysical work with Green.

Psychophysics: 1954-1963

Psychophysical Laws: Fechner's and Stevens's. My interest in psychophysics derives, in part, from the fact that mathematics has, from the start, played a significant role in the development of this field. I had been dimly aware of this from meetings at M.I.T. in the late 1940s and early 1950s in which information theory applications to psychology generally and psychophysics in particular were all the rage. Indeed, as one of my first activities in late 1953 for the Behavioral Models Project, I drafted a long paper on information theory and its applications in psychology; for somewhat obscure reasons the publisher delayed its release for a number of years and so its impact was never very great (Luce, 1960a). But it was not until 1954-1955 at the Center for Advanced Study in the Behavioral Sciences that I began to delve carefully into psychophysical theory under the wise tutelage of Albert Hastorf. Starting at the headwaters of the subject, I studied the Weber-Fechner problem, and two papers resulted.

The one with Ward Edwards (1958b) pointed out the surprising fact that Fechner's derivation — the one usually presented in texts — of his "law" from Weber's law was technically incorrect and that for any Weber function other than Weber's law[4] this method would have led to the wrong answer. The proper method is to solve Abel's functional equation.[5] Moreover, when the problem is stated so as to give an appropriately u-

4. Weber's law states that the relative change in the physical scale required for a fixed level of discriminability is a constant.

5. Abel's equation: For given functions f and g, find those functions u, if any, such that for all x and y,

$$u[f(x,y)] - u(x) = g(y).$$

nique answer, it becomes apparent that it is equivalent to the probabilistic notion that equally-often noticed differences are equal. Some later studies that delve more deeply into these and related problems are Luce and Galanter (1963d), Falmagne (1971, 1974), and Levine (1970, 1972).

The second paper introduced what amounts to an algebraic approximation to the probabilities used by Fechner, but that is more appropriately discussed under measurement.

As described above, during the period from 1956 through 1961 I was greatly preoccupied with choice models and much of what I did in psychophysics had to do with them. But not all. While at Harvard from 1957-1959, I spent a fair amount of time with the late S. S. ("Smitty") Stevens — one either spent a fair amount of time with him or none at all, for his intellectual style, although intense and persistent, was leisurely and was often intermixed with skiing in one way or another. In his firm way, he ground my nose into two sets of data: those collected some years ealier in support of neural quantum theory — the idea that the stimulus representation is discrete rather than continuous — and those he had recently been collecting using magnitude estimates and cross modality matches. His classic paper "On the psychophysical law" (Stevens, 1957) had just appeared.

Although I was not really happy with the way either body of data had been collected, I eventually became convinced that any psychophysical theory worthy of the name had to be able to account naturally for both sets of data. In particular, it slowly became clear to me that neither my choice models nor the theory of signal detectability, with which I had familiarized myself at Columbia, were satisfactory. I also found the theory of signal detectability wanting in another, extremely important respect: it did not generalize in a satisfactory way beyond two stimuli except as Thurstone's discriminal dispersions; in particular, the emphasis on likelihood ratios, which made it distinct from Thurstone's model in the case of two stimuli, could not be maintained without introducing more parameters than there were data to explain. Moreover, neither the Thurstone nor the choice models predicted the limits on information transmitted in absolute identification experiments, which seemed to me another key psychophysical phenomenon requiring a natural account.

Before leaving Harvard, I wrote a paper (1959e) whose title, "On the possible psychophysical laws," was an obvious takeoff on Steven's basic paper. As this paper really concerns dimensional analysis, it belongs in the section on measurement, but as its impact really was in psychophysics, I discuss it here. Although widely referenced, criticized (see Rozeboom, 1962, and Luce, 1962e), and reprinted, I fear that it has rarely been understood. The fault is mine, for although I think the writing is clear locally, it is misleading globally. In truth, the paper says nothing whatsoever about the form of psychophysical laws, but only really explains to psychophysicists why, except for power laws, they must formulate laws in terms of dimensionless signal and response variables. Since it has been the custom to do so with signal intensity (e.g., dB scales), the paper really only urges him to do so also with other signal dimensions and with the response scale. However, this point is made most obliquely, and many have interpreted the paper as saying that Stevens's results on the psychophysical function were, somehow, mathematically foreordained, which is not true. It is ironic that later when I worked on magnitude estimation, I did not for a long time take my own advice which, it has turned out, was quite good advice.

Another Harvard paper, with Bruce Finnie (1960c), was never published because, for some reason that I never understood, referees for the *Journal of Abnormal and Social Psychology* found it unacceptable on grounds of significance. We simply took items that Thurstone and Chave (1929) had used, had Harvard students rescale them both by pair-comparison methods of the original study and by magnitude estimation and we showed that the results were very similar to those found in psychophysics. Finnie did his Ph.D. thesis under Stevens's direction on this problem, others have made extensive use of magnitude estimation in social contexts (Galanter, 1962; Sellin & Wolfgan, 1964); and Stevens later gave it considerable emphasis (1972).

Neural Quantum Theory. During the first half of my ten years at the University of Pennsylvania, my psychophysical work centered on the last gasps of the choice models, already discussed, the two topics stimulated by Stevens (neural quantum and magnitude estimation data), and reaction times.

Neural quantum theory (Bekesy, 1930; Stevens, Morgan, & Volkman, 1941; Corso, 1956; Luce, 1963c) and the theory of signal detectability (Tanner & Swets, 1954; Green & Swets, 1966) are completely inconsistent in their formulation of threshold phenomenon. Supporters of each theory had data which they interpreted as rejecting the other view. My attack on the problem was two-pronged. First, I attempted to demonstrate (1963a) that ROC data — plots of the probability of saying a signal was present when it was versus the probability of saying it was present when it was not — which had been interpreted as devastating evidence against the threshold idea really only clearly rejected what has come to be known as high thresholds, not low ones. A tricky debate ensued as to whether or not ROC data, especially those collected using rating scale methods, also reject the low threshold model. The most careful discussion of the matter that I know of is Krantz (1969). What seems to be evident now is that no reasonable amount of ROC data can distinguish between a few, but more than two, states and a continuum; however, the two-state model is probably wrong for any experimental design.

A minor aside. From time to time I have been charged with proposing seriously that there are only two states of, for example, loudness. It had not occurred to me that anyone would think me so stupid (or so deaf) as to believe that. A more subtle version of this same charge is the supposition that I would predict that rating data should also follow the two-limbed ROC function predicted from the two-state model for Yes-No detection. The issue is really observable discreteness versus apparent continuity, not 2 versus n states.

One innovation of the 1963 paper was its relatively successful attempt to account for the location of the response bias as the asymptotic value of a simple learning process described by a linear operator model. Some such mechanism seems more plausible and gives a better prediction of the data than does maximization of expected value, which Green (1960) had earlier shown was wrong for the theory of signal detectability and I showed was wrong for the threshold model. Dorfman is currently having considerable success in melding the learning idea with detectability theory (Dorfman and Biderman, 1971).

The second prong of the attack was to see whether response biases, whose existence had been so clearly demonstrated by ROC plots, could account for the difficulties some experimenters had in replicating the neural quantum results. Two first-year graduate students, William D. Larkin and Donald A. Norman, working with Eugene Galanter and me pursued this; the initial results obtained in 1960 were reported by Larkin and Norman (1964) and the later ones in Norman's dissertation (1962a,b, 1963, 1964). There could be no doubt that enormous biasing effects were possible; nevertheless, tantalizing hints of a discrete underlying structure showed through. Later Joseph Markowitz (1966), working under Green's direction but also the recipient of advice from me, attempted to exploit the sequential effects predicted from the postulated slow variation in the quantal grid, but his work was ultimately inconclusive because of response bias problems.

Another bit of evidence arose from data that Elizabeth Shipley (1961, 1965) had collected in W. P. Tanner's laboratory at Michigan to test some choice models. Among the conditions she ran was a Yes-No design in which there were two equally detectable signals of different frequencies. Each of three subjects was required to report, independent of his detection response, which frequency he thought had been used. There was no evidence whatsoever that subjects could discriminate the frequency when they said that no signal had been presented. This made sense from a threshold point of view (with appropriate response bias) but not from that of the theory of signal detectability. After I published this analysis (1963c), Lindner (1968), working under the direction of James Egan, replicated the study at various points on the ROC curve; he got exactly the opposite results. I have no idea why there was the difference, especially since both Lindner and Shipley are careful experimenters. I do not believe it can be attributed to experimenter bias on Shipley's part since the issue had not even been formulated at the time her experiment was performed.

To this day, the question remains unresolved for me whether or not the neural quantum idea, i.e., discreteness of the representation of the signal, is correct. Most psychophysicists have been convinced that it is without merit by one or another failure to produce a function of some form — a rectilinear psychometric function from Yes-No data, or a atwo-limbed ROC function from Yes-No or rating data, or a more subtle analysis of the form of the rating data. But it is easy in each case to see why artifacts, having nothing to do with the underlying discrete representation of the signal, might account for the rounding of these functions. Perhaps the most difficult data for the continuous theories to encompass are the ROC curves obtained using two-alternative, forced-choice procedures and different payoff matrices. Various experimenters (Shipley as reported in Norman, 1964, pp. 116-118; Atkinson and Kinchla, 1965, p. 192) report that they are straight lines of slope 1. This prediction follows from a variety of discrete models, but no one has derived it from a continuous one. Moreover, as I have several times pointed out, for such ROC curves, the psychometric function $p\ (1/\langle s, n\rangle) - p\ (1/\langle n, s\rangle)$ versus the intensity of the signal s is free from any response bias and so should be rectilinear with 2 to 1 intercepts if the neural quantum theory is correct. So far as I know, no one has ever carried out this simple experiment.

I have dwelt on this issue at such great length because I remain so uneasy about it and because the answer affects work I am currently doing with Green. More on this when we come to it.

Magnitude Estimation. Stevens implanted a second thorn, namely the inability of any of the discrimination theories — those, as he used to say, that "process noise" or, as we who worked on them said, that are "local in character" — to encompass magnitude estimation and absolute identification data when the range of signals is sufficiently large. To know how deep that thorn went, I had to examine two features of the data that Stevens typically ignored: the mean response of individual subjects and the variability of the individual's responses about the mean value.

In an attempt to incorporate magnitude estimation within the choice-theory framework, Galanter and I (1963e) used functional equations techniques to derive a distribution of responses for magnitude estimates. It was an odd distribution, unlike any of those familiar from standard probability theory, composed of two power functions, back-to-back. Following the suggestion of John Tukey, we called it the double monomial. Mo and I (1965d) ran an experiment collecting weight-lifting data from six subjects and loudness data from another six. There were 100 magnitude estimates at each of 20 signal levels. We found:

1. Many of the mean magnitude functions exhibited systematic deviations from power functions; in the case of loudness some deviations were as large as 5 dB. This has

been found by others (Schneider & Lane, 1963; Stevens & Guirno, 1964; Green & Luce, 1974b,e). Stevens did not, however, interpret the deviations as systematic because he used only a few signals and only a few responses to each signal, but I think there can be no longer be any doubt about the systematicness of the deviations.

2. The "exponents," although averaging to values near those reported by Stevens, exhibited considerable variation, from .15 to .34 for loudness versus physical energy. Again, this seems typical of later data, except that the top of the range is more like .6.

3. Although the distributions of responses, which are sharply peaked and have high tails, are better fit by the double monomial than either the normal or log normal distributions, they were not really fit well by any of the distributions we tried. Again, later data (Green & Luce, 1974b) support the belief that the distributions are odd, but we now think we know why.

4. The variability of the responses was appreciably greater than that obtained using discrimination techniques, such as a two-signal absolute identification design. We did not run a large absolute identification design over a wide stimulus range to see if its variability agreed with that of magnitude estimates, but as later work has shown (Braida & Durlach, 1972) the correspondence, although improved, would not have been perfect.

It was not clear to me where to go next with magnitude estimation, and so I dropped it for nearly a decade until a better theory led to a better understanding, new predictions, and additional experiments.

Simple Reaction Time. Reaction time is not always thought of as a part of psychophysics, but I believe it to be an integral part of any decision process. So any psychophysical theory that fails to account for the time it takes the subject to respond is surely incomplete.

My first foray occurred in the mid 1950s with Christie (1956a). That work arose in part from some time data we had collected on our interacting groups, but the paper itself was entirely theoretical. It made two points. The first, well known to mathematicians and statisticians but then apparently overlooked by psychologists, was that transforms such as the Laplace (moment-generating function) and the Fourier (characteristic function) take the distribution of the sum of independent random variables into the product of the transforms of the separate distributions. This fact can be exploited, as McGill (1963) did and as Green and I did years later (1971a, 1972a). The second was the observation, which followed readily from the plots of the transformed distribution, that it is not very easy to distinguish between serial systems in which the total time is the sum of the independent component times and parallel ones in which it is the slowest of the independent component ones.

Some years later, Galanter and I agreed that, in spite of results such as this, the reaction-time distribution probably contains information about sensory processing, if only we could figure out how to extract it. In fact, we failed to figure it out, but later Green and I began to. One fact that warmly recommended close examination to reaction times was that they apparently form a continuous random variable, and so each observation is potentially a richer source of information than is the typical binary choice data.

Work began when we interested a student, Gay Snodgrass, in the area, and the output was our joint paper (1967d) and her dissertation (Snodgrass, 1966, 1969). The key idea in our approach was to apply information feedback and payoffs to reaction times, just as to choices, to find out how malleable the reaction times are. We had two initial questions: Could the subject be made to track a narrow band of payoffs over a range of times, and to what degree could we reduce the variability by narrowing the band? The results showed that they could indeed track the band, but that the variability was a U-shaped function, suggesting that there is a natural reaction time and that they tracked the band by some form of trickery which introduces the added variability. We also found that while the variability could be made small — an interquartile range of about 25 msec — there was no advantage in using a band payoff much narrower than 20 msec.

Perhaps the most perplexing thing in these data was the form of the distribution of reaction times. In contrast to the rather rounded mode often reported, usually from less careful experiments and frequently from data averaged over subjects, we found very peaked distributions. Attempts to fit them with various well-known distributions and with the little known double monomial were not very successful.

I am now convinced that any data, like these, based on strong signals are incapable of telling us much about the psychological decision process because its duration is too brief relative to other delays in the system, such as sensory transduction, neural transit times, muscle innervation, and so on. My reasons for saying this will become clear later. Nevertheless, these data remain perplexing, since if we are observing a sum of random times, then the mode of the distribution should be rounded except either if a single peaked time dominates or if times are not independent, as would occur if there were some sort of feedback mechanism forcing the system toward a particular time. There is no direct evidence for the latter; we must entertain the former and ask what might be the source of peakedness in the dominant time. The only mechanism that I am aware of which might lead to it is the distribution of the slowest of several similar, parallel processes (Gumbol, 1958). I do not know if there is a suitable physiological explanation in these terms.

A student of this same era, R. T. Ollman, became interested in the speed-accuracy tradeoff problem and developed and tested the fast-guess model (Ollman, 1966, 1970); it was independently worked out by Yellott (1967). I was never very taken by it conceptually, and later worked out an alternative model with Green and we provided data that showed the fast-guess model does not, by itself, account for responses to weak signals (1973a). I suspect the fast-guess model may well be correct, or approximately so, when the experimenter drives the subject beyond the range of his ordinary decision mechanisms; it is his mode of behavior when he despairs of complying with the instructions.

Psychophysics: 1964-1974

By 1963 my work in psychophysics had lost direction. I had abandoned the choice models, my efforts at deciding whether or not there is anything to neural quantum theory were indecisive, I had not gained much understanding of the variability of magnitude estimates, and I had failed to incorporate response times into any model. Moreover, I lacked an overall theoretical scheme in which I had any faith. The way out of this unhappy state was totally unclear and, in all honesty, I clumsily backed into my next attack on psychophysics without knowing where I was going.

Free-Response Detection. Two papers, Egan, Greenberg, and Schulman (1961) and Broadbent and Gregory (1963), attracted my attention to the so-called method of free response in which the signals to be detected are presented according to some haphazard temporal schedule and the subject is free to respond whenever he believes one has occurred. The method appealed to me as being a far better idealization of natural detection problems than the usual psychophysical procedures that delineate brief time periods during which a signal may or may not appear. What I did not like about either paper, or about the literature on vigilance in which similar methods are often used, was the method of analysis. In one way or another an arbitrary temporal subdivision was employed to estimate response probabilities analogous to those used in fixed interval designs (see Broadbent & Gregory, 1965). In effect, they were trying to treat the free response situation as a series of fixed interval ones. The data, however, actually consist of two interlaced time series — that of the signal presentations and that of the responses — and the theoretical problem is to understand the probabilistic structure of the response series and its relation to the signal one. This has to do with continuous stochastic processes, not discrete ones.

For lack of a better idea, I decided to try a two-state model in which each signal presentation had some fixed probability of activating the detect state and the background had a temporally random, i.e., Poisson, tendency to do the same. When a detect state occurred a response followed after a random delay of unspecified distribution. By restricting attention to signals of brief duration, I decided to treat the occurrences of detect states as point events, and for mathematical convenience I elected to limit the analysis to a Poisson schedule of signal presentations. The model and a number of its properties, including some I thought could be used to estimate parameters, were presented in Luce, 1966a.

Before this paper appeared, David M. Green, who was then at the University of Pennsylvania, became interested in it and we decided to try to test it. As the experiment and its analysis progressed, we soon discarded my estimation schemes as unsatisfactory and substituted others (1967c, 1970b). After a bit we began to realize we were being plagued by the fact that, under a Poisson schedule, the signals tend to occur in bursts (because the most probable time between two signals is zero), and so a second and even a third signal could occur before the response to the first had been completed. For a time we tried various assumptions about what happened under these circumstances — independent response mechanisms, a signal lockout until the response underway was completed, storage of signals and delayed responses, suppression of earlier by later signals — but the mathematical problems compounded until we decided it was better to change the experiment.

Simple Reaction Times. We wanted a design in which the onset of a signal is totally unpredictable while not having the problem of second signals occuring. This led us to a simple reaction-time design with random (exponentially distributed) foreperiods and weak signals. We also found the modeling to be much simpler if we used response terminated signals rather than ones of fixed duration. The model remained the two-state one, with the occurrence of states governed by one Poisson process before signal onset and by a different one, with a larger parameter, during its presentation. In essence, the problem for the subject was to decide when the parameter of the process had changed value. We were assuming that a single pulse would, with some fixed biasing probability, initiate a response process which, following a random delay, ended in the response.

We made no assumptions about the distribution of these residual delays except that they are bounded. Although we had no direct interest in the distribution of residual times, we in fact estimated it as a way of testing the model. The method involved parameter estimation, which depended on the boundedness assumption, and the transform techniques earlier suggested by Christie and me (1956a) and now feasible because of suitable computers and programs (in particular, the Cooley-Tukey Fast-Fourier Transform Program). The "distribution" ground out from the computation exhibited two features which told us the model was wrong. First, it was not a density function: after a sharp cutoff at about 500 msec, which agreed with our assumption of boundedness, it became negative for about 100 msec, which is impossible. Second, its mode was at about 300 msec, whereas we know from data using strong signals that it could not exceed about 170 msec. Our conclusion, drawn in 1971a, was that an analysis in terms of the occurrence of detect states (pulses) would not work.

Timing and Counting Models. We were led to consider the next more complex model, namely, that information about the signal is encoded as the time between successive pulses. Indeed, in a Poisson process with intensity parameter μ , the time between pulses is distributed according to the exponential density $\mu e^{-\mu t}$ and so the expected time between pulses is $1/\mu$. And if μ varies monotonically with signal intensity, then the reciprocal of the time between pulses provides an estimate of intensity. And the more such interpulse observations, the better the estimate.

As we began to work on this model, we became aware of the work along these lines, both of other model builders, especially McGill (1967) and Siebert (1965, 1968, 1970), and of neurophysiologists, especially Kiang (1965, 1968) and Rose *et al.* (1967) who were recording neural pulse patterns on the peripheral auditory fibers. Our theory, initially presented in Luce and Green (1972a), differed from the other theoretical work in at least two important, related respects. First, it emphasized the time between pulses rather than the number of pulses to occur in a fixed time — what we have called timing rather than counting models. Second, it focused attention on response times, including both the muddying effects of the residual times — those other than the decision time — and the need to accumulate information about the signal from several channels simultaneously. One reason for our excitement about the timing models was the natural account they gave of the inverse relation between reaction times and signal intensity, since the weaker the signal, the slower the pulse rate and hence the slower the decision time. On the other hand, certain timing models led to predictions, such as that Yes-No ROC curves when plotted in normal-normal coordinates (z-scores) should be approximate straight lines with slopes considerably greater than one, totally at variance with most published data. This led us to an experimental investigation, reported in Green and Luce (1973a), in which applying a response deadline to all trials led to the usual result of slopes less than one, as predicted by a counting model, whereas applying it just to signal trials led to slopes greater than one, as predicted by the timing model.

I currently suspect that the timing model we proposed for this experiment is correct in spirit only, not in detail. I have three reasons for saying this. First, this model is not really the natural analogue of the one used for the experiment with randomized signal onsets, which in its simplest form involves deciding when a short interpulse time has occurred rather than actually measuring the time. Second, vigilance data recently reanalyzed by Angus Craig do not really make sense until one considers analogues of the reaction-time model rather than of the Yes-No one. Third, later Yes-No data, not yet published, do not fit the simple timing model. We are currently working on these problems using computer simulation of more complex decision processes.

When the usual fixed-interval psychophysical procedures are employed, we believe that subjects are driven to counting rather than timing procedures. For some reason, not yet explained, it takes a lot of trials with information feedback to switch subjects from one mode of operation to the other. This probably accounts for the long training periods required for observers to stabilize in fixed interval procedures. For such procedures, counting analyses are likely to prove more successful. So, for example, when we gave 1974c a detailed analysis of both intensity and frequency discrimination of pure tones (ΔI and Δ f as a function both I and f), we found the counting model to be quite satisfactory and the timing one unacceptable. This paper includes an interesting modification of the Poisson model to an interpulse distribution that is geometric in integral multiples of the period of the pure tone signal, but somewhat smeared by variability. This model was suggested by the data of Rose *et al.* (1967). In essence, it provides a way for both intensity and frequency information to be encoded on single fibers, and so it permits us to assume that the entire role of parallel channels is to build up samples from which estimates of intensity and frequency can both be made. This view contrasts sharply with the more prevelant ones either that the subset of active fibers codes intensity and frequency information or that some form of correlation over fibers is involved.

Global Psychophysics. Another line of inquiry sparked by this model was into the global psychophysics of magnitude estimation and absolute identification. In our 1972 paper, Green and I suggested that the pulse rates estimated from the sum of a fixed sample of interpulse times could serve to account for both experiments: a constant times the rate being the number emitted in magnitude estimation, and the rate being a Thurstonian random variable underlying the categorization made in absolute identification.

We now know that in this simple form both hypotheses are wrong. The one for magnitude estimation predicts that the reciprocals of magnitude estimates to a signal should be gamma (=chi-square) distributed, and this is not so (1974b). A better model, suggested by Ward's (1973) proposal for data analysis and a rather natural idea in the light of Luce (1959e), supposes that the ratio of successive responses is proportional to the corresponding estimated pulse rates. This predicts a beta distribution, which is more adequate although still not perfect. To improve it, we are now considering an "attention" hypothesis of the following sort. If a signal falls within a loudness band, which we estimate to be about 15 dB wide, it provides the decision mechanism with a sample of interpulse intervals that is close to an order of magnitude larger than when the signal falls outside band. This means the standard deviation of the resulting estimates is smaller by a factor of about 3 ($\cong \sqrt{10}$) when the signal is in the band then when it is outside it. Not only does this hypothesis seem to account for some anomalies in the magnitude estimation data, but it provides a natural account for the asymptotic form of the function relating information transmitted to number of signals in absolute identification (Miller, 1956) and to the form of the cumulative d^1 measure reported by Braida and Durlach (1972) (Luce & Green, 1975b).

A mathematical study (Luce & Green, 1974a) of the response ratio hypothesis for magnitude estimation has led us to the peculiar view that whenever an estimate of a pulse rate is used, it is lost. Thus, when an estimate must be used twice in an experiment, as in this response ratio hypothesis, the sample of information about the signal has to be partitioned into two, and two independent, separate estimates must be made. Without this assumption one does not, and with it one does, obtain the drift and sequential effects found by Holland and Lockhead (1968), Ward and Lockhead (1970), Ward (1972, 1973), Cross (1973), and Luce and Green (1974a, e). One effect of this is to predict that the variability in magnitude estimates should be larger by a factor of $\sqrt{2}$ than in absolute identification, which seems to be the case (Braida & Durlach, 1972).

Stevens's Law and Physiological Data. One remarkable fact, which I have not yet mentioned, is that all of our studies of acoustic intensity seem to show that the Poisson parameter grows approximately as a power function of intensity, with the exponent varying from .15 to .60 over subjects and averaging somewhere near, but below, .3. As was emphasized by Stevens (1957) this is shown directly by plotting average magnitude estimates versus signal intensity, and we find it is true for individual subjects, especially if we plot mean ratios of successive magnitude estimates against the corresponding signal ratios. We also find it directly for the first 10 dB above absolute threshold by fitting predicted exponentials to the tails of the distributions of response times to weak signals (Luce & Green, 1972a); indirectly as an assumption that works in our analysis of discrimination of both intensity and frequency (Luce & Green, 1974c); and indirectly as an assumption in our account of absolute identification results (Luce & Green, 1975b). The parameter has been estimated in all these cases, in magnitude estimation, and in the Yes-No deadline experiment (Green & Luce, 1973a) as well. Moreover, Levelt *et al.* (1972), using additive conjoint measurement to analyze the summation of loudness over the two ears, found the resulting scales to exhibit the same growth.

This law — Stevens's law — seems to describe a central tendency of the transformation of acoustic intensity into the pulse trains that enter into sensory decisions. Note that

I have said "transformation . . . into the pulse trains that enter into sensory decisions" and not "transduction into the pulse trains of the peripheral nervous system." This is important. If one looks at the data from single, peripheral fibers, nothing so simple as a power function obtains. Roughly, the activity can be described as follows. In a plot of log intensity versus log frequency, each fiber corresponds to a V-shaped wedge that is about 15 to 20 dB thick in the intensity dimension and whose right limb is about twice as steep as the left one. For tones in the region below the wedge, the fiber fires at its resting rate — the rate when no signal is imposed. Above the wedge, it fires at its maximum rate. The growth from resting to maximum follows some ogival function over the 15-20 dB range of change. Different fibers have the minimum of the V at different frequencies; most seem to have it at the behavioral threshold, although some are at a much higher level. Somehow — we do not yet know how — the full dynamic range is pieced together from such local fibers.

Two points are of interest. First, the dynamic range of these peripheral fibers is about the same as the width of the loudness band that we were led to postulate to account for some aspects of global psychophysics. Moreover, the dynamic range of the individual fibers measured in frequency rather than intensity seems to correspond roughly to some estimates that have been made of the critical frequency band. We therefore suspect that our loudness bands and critical frequency bands are just two behavioral aspects of the same phenomenon, namely, that at any one time the brain monitors intensively one group of similar fibers and much less intensively all others, and signals falling in the heavily monitored region are better represented and so responded to with less variability than signals falling outside that region. Second, if the whole dynamic range of intensity is being spanned by all of the fibers that are judged active, for example each contributing pulses to a common channel, then neural quantum effects could arise naturally. A neural quantum would correspond to the change in the number of fibers that is interpreted as a real change in the signal and not a random fluctuation in estimated activity. This possibility raises in me once again the desire to see a clear resolution to the neural quantum question.

Much work remains to fill out and test these ideas and to incorporate other auditory phenomena, such as masking, various harmonic phenomena, and so on. We are by no means satisfied that we have found the correct decision rules, and we are uncertain how the channels of the model relate to the peripheral neurons, but we are encouraged that the general approach has merit. It is likely to preoccupy me for some time to come.

Measurement

In contrast to my work in choice behavior and psychophysics, where the models are entirely probabilistic, that in the foundations of measurement is algebraic. My training strongly favored this approach, and I have always found algebra more esthetic than analysis; however, such models are usually difficult to relate satisfactorily to experiments. I suspect that the best way to look at them is as descriptions of some central tendency of a process that is best thought of as probabilistic. When the latter is quite complex, however, it may be best to begin with just the central tendency.

Semiorders. My first paper in the area (1956c) was devoted to an axiomatization of the algebraic concept of a threshold; I called such systems semiorders. The axioms were a natural, and surprisingly simple, generalization of those for a linear order, the main difference being that the indifference relation is nontransitive. An example on the real numbers is the relation R defined by xRy if and only if $x + 1 \geq y$; so any two numbers within unit distance are judged indifferent. A small literature, including my later contribution (1973b), well summarized by Fishburn (1970) and Roberts (1970), has followed up on this concept. Another summary will appear as Chapter 15 of Volume II of Krantz, Luce, Suppes, and Tversky (1975e).

Additive Conjoint Measurement. The next contribution did not appear until my joint paper with John Tukey (1964e). Work on that began in the summer of 1961 at an informal seminar held in Tukey's study at the Center for Advanced Study in the Behavioral Sciences where he proposed that measurement additive over components might serve for the social sciences in a way analogous to that served by extensive measurement (e.g., additive over a combining operation) for the physical ones. We were then unaware of the earlier work of Adams and Fagot (1959), Anderson (1959, 1962a,b), and Debreu (1960) on the problem, but knowledge of these papers would not have affected us greatly since the first two did not provide sufficient conditions for a representation to exist and the latter did so only in the context of partially topological assumptions, whereas we wanted purely algebraic ones. Given the later, much simpler and more revealing proofs (e.g., Chapter 6 of Krantz, *et al.,* 1971e) which neatly relate additive conjoint measurement to extensive measurement, it is difficult now to realize how laborious our first proofs were.

My next efforts were on what may be called local measurement axioms. In 1966b I showed that additive conjoint measures can be constructed using a local form of solvability which is empirically much more reasonable than the strong version Tukey and I had used. And Marley and I (1969b) studied extensive measurement with a local concatenation operation and introduced a possible axiomatization of relativistic velocity. Krantz (1968) followed up this idea of a local concatenation by producing a very useful local version of Holder's theorem, which is one of the basic theorems employed in Krantz *et al.* (1971 e). That book includes improved versions of both of the above papers, especially of the relativistic velocity model.

Foundations of Measurement. During the early and middle 1960s, Patrick Suppes and I participated in and organized a number of conferences where questions in the theory of measurement were frequently discussed. In spite of his chapter with Zinnes (1963) in the *Handbook of Mathematical Psychology,* Vol I, we increasingly felt the need for a systematic presentation and integration of the materials on measurement which had appeared in a wide range of literatures, including economics, management science, mathematics, operations research, philosophy of science, physics, psychology, and statistics. Eventually we decided to undertake a book, but as we outlined it we became acutely aware of areas in which we had not made contributions and were not especially expert. These topics were nicely covered by two brilliant and industrious young men, David H. Krantz and Amos Tversky. I had known Krantz from the time he was a graduate student at Pennsylvania, where he worked with Leo Hurvich and Dorothea Jameson, and I had met Tversky when, as a graduate student at Michigan, he was working on a dissertation under Clyde Coombs on finite conjoint measurement. We invited them to join the project. Our initial outlines suggested a book of about 20 chapters, and so it has remained except that their length soon expanded the one volume into two. The first of these volumes is rather closely integrated, whereas the second will be somewhat more of a miscellany of topics. We titled it *Foundations of Measurement.*

One major problem was to find the minimum number of basic mathematical results that describe how to pass from an algebraic structure to a numerical representation of it, from which we could derive all of the results in the theory of measurement. Ultimately, we found that three theorems would do (see Ch. 2 of Krantz *et al.,* 1971e). But doing it this way meant that virtually every result in the literature had to be reproved to fit into our scheme; in the process of doing that, we uncovered some new results (e.g., the theory of conditional expected utility) and improved many other therorems. Our hope was that by integrating and systematizing the results this way, we would make it easier for others to build new structures and better integrations. The latter has already begun to happen.

My measurement papers during the late 1960s and early 1970s — on extensive measurement (with Roberts, 1968d; and with Marley, 1969b, 1971b), subjective probability (1967a, 1968c), conditional expected utility (with Krantz, 1971c, 1972c), and dimensional analysis (1971d) — all arose from work on Volume I of the *Foundations of Measurement.*

Much of my work on the first volume was carried out during 1966-1967 when I was again at the Center for Advanced Study in the Behavioral Sciences and during the following year back at Pennsylvania. At that time Fred Roberts, a Stanford mathematics Ph.D., was a postdoctoral fellow with me, and we worked on a paper (1968d) which gives not only an interesting axiomatization of entropy, but also what I believe is the neatest characterization of closed extensive measurement yet presented.

Dimensional Analysis. The following year, in Rio de Janeiro, Brazil, my major efforts went into psychophysics and into what turned out to be Chapter 10 of the *Foundations of Measurement.* Since I first ran into it in graduate school, dimensional analysis has fascinated and perplexed me, and two early contributions to it were 1959e and 1964f. It is a method whereby physicists, engineers, and biologists often can arrive at the form of a physical law simply by knowing which variables are relevant, which of course is a great deal to know. Although intriguing and useful, the subject seemed to me conceptually slippery. Read carefully the introductory chapter to any book on dimensional analysis, e.g., Sedov (1959), and you soon realize that something mysterious is going on; only when you get to the applications does it begin to make sense.

My attention was again focused on the problem by listening to Robert Causey during three long sessions of a Stanford-Berkeley seminar on measurement (1966-1967) which were devoted to his dissertation on physical similarity. Part of the reason these sessions ran long was my inability to understand exactly what was involved. During the following year his paper was published (Causey, 1969), and we corrresponded at length about it, until I finally got straight what I found objectionable. My modified, and much simpler, version of his result appeared as 1971d. An important aid in clarifying matters was Whitney's (1968) very nice axiomatization of the space of physical quantities. This helped in distinguishing clearly between the concept of a physical quantity and its non-unique numerical representation, which confusion plagues most discussions of dimensional analysis because an attempt is made to formulate matters in a real vector space.

So part of the problem had been clarified, but I now realized there remained a major, apparently unnoticed, lacuna. No one ever gave any reason why physical scales (e.g., mass, length, time, velocity, etc.), which arise from the theory of extensive measurement, should have anything to do with the representations of physical quantities discussed in dimensional analysis. The latter structure was axiomatic in character, and no one ever showed how to construct it from the former, although everyone obviously believed such a construction to be possible. So I undertook the task.

There were two keys to the construction. First, one must assume that, in addition to extensive structures, there are additive conjoint ones (conventionally written as products rather than sums over the independent components) and that some physical quantities, although not all, are measured both extensively and conjointly. Second, in physics these two kinds of scales are always related by power transformations, and there is a neat qualitative way of characterizing that transformation by either of two kinds of qualitative laws, called laws of similitude and of exchange, which relate the conjoint and extensive structures. Part of the argument appeared in 1965b, but the only full discussion is in Chapter 10 of *Foundations of Measurement.*

It has always struck me as an odd curiosity of history that although some physical scales obviously have an internal additive structure and some a multiplicative decomposition into other scales (e.g., length is additive and momentum is the product of mass and velocity), philosophers of physics during the latter part of the last century and the first half of this one axiomatized only the additive aspect. Not until behavioral scientists, who for their own reasons, axiomatized additive conjoint measurement was the other half of physical measurement properly formalized. With that done it became possible to provide a natural account of dimensional analysis.

Generalized Extensive and Conjoint Structures. Most of the measurement literature has focused on structures that have additive representations. There are exceptions to this statement, but they tend to be isolated and little has been done to understand the full range of nonadditive representations that can still reasonably be called measurement representations. During the summer of 1974 I began to collaborate on this problem with a brilliant younger colleague, Louis Narens, who received his training under the late Abraham Robinson, the father of nonstandard analysis. We agreed that the criterion for measurement is this: when a representation into a particular numerical operation exists, then that representation should be unique once one value is specified. So the first question became one of finding fairly general conditions under which a nonassociative,[6] ordered operation can be represented uniquely by some nonassociative numerical operation. It turns out that the solution to this problem pretty much provides the solution to the general question of representing a conjoint structure in terms of some function of scales on its two components. A third question we have dealt with is what is possible when one has a general conjoint system with an associative operation on one component. Under apparently weak conditions there is a surprising amount of structure, and the results give considerable insight beyond that of Chapter 10 of the *Foundations of Measurement* into the use of addition and multiplication in the representations of physics. We still do not understand fully certain cases typified by the "addition" law for relativistic velocity (Luce & Marley, 1968; Luce & Narens, 1975d) in which one of the variables receives a bounded representation. This general type of structure appears to be exactly what is relevant for psychophysical measurement of subjective attributes such as loudness and pitch, and so we will undoubtedly continue to work on it.

What exactly are measurement models good for? In my A.P.A. Distinguished Scientific Award address (1972b), I argued that even within psychophysics there is no evidence that we can construct a system of variables and measures comparable to that of physics. The main difficulty is that while (approximate) power relations abound, the exponents seem to vary considerably from subject to subject. If not that use, then what? Recently (1975a), I have pointed out that the successful applications of the measurement models to psychophysical problems can best be described as formalizing the structural relations involved in some central tendency of the sensory transducer. They permit us an economical characterization of the average information reduction effected by the transducer as revealed in the various tradeoffs among stimulus variables that yield, on the average, the same internal representation. Krantz (1972) has also argued forcefully that measurement methods are a means to begin to get basic relations among variables as well as to measure them.

6. An operation o is nonassociative if for some x, y, z it fails to satisfy
$$x \circ (y \circ z) = (x \circ y) \circ z.$$

M. I. T., 1950-1953

Within six months of my joining Bavelas's laboratory, he left to work on a classified project for the State Department. He turned the management of the laboratory over to Lee S. Christie and me. I was hardly qualified for a position of leadership in a psychology laboratory, but that did not seem to matter in the Research Laboratory of Electronics, of which the Group Networks Laboratory was a small segment. In fact, I think it had unfortunate consequences in that I neither trusted my judgment sufficiently to oppose the momentum of the group on an expensive subproject nor was able to face squarely the weaknesses of our research. The subproject, well underway when I joined the laboratory, was to build a special purpose computer — of relays, tubes, and tape — to "automate" Bavelas's card-passing experiments. In fact, by its very design it was less flexible than his partitioned tables and cards and, of course, orders of magnitude more expensive. Worse still, it was plagued with technical problems and, in spite of the heroic efforts of Josiah Macy, Jr. and the technicians under his direction, it was never completed. After being exposed to it and being privately persuaded that it was worthless, I ignored it, feeling too insecure to try to terminate the brain child of Bavelas, Strauss, J. C. R. Licklider, and Jerome Wiesner (then associate director of R. L. E. and now President of M.I.T.). It was aptly named "Octopus."

The second form of momentum was our attempt to continue the funding of the laboratory at a relatively high level even after I felt that what we were doing was not very significant. That irresponsibility was blocked by the Ford Foundation and its advisors.

Probably the most important intellectual experiences for me during this period were two groups of seminars. One was a regular luncheon meeting in R. L. E. involving various groups interested in behavioral and information theoretic projects. The other, and more interesting one, was evening sessions of hard-headed Cambridge psychologists which meetings were called the Pretzel Twist. I learned a good deal of psychology informally from what has turned out to be a quite illustrious list of tutors, including among others Bert Green, J. C. R. Licklider, William J. McGill, George A. Miller, Walter A. Rosenblith, and Warren Torgerson.

Although it is difficult to recall many specific events of that era, I can recount three that stick in my memory. On a train ride to New York, Licklider pointedly questioned me about the empirical and psychological significance of the graph theoretic ideas we were then investigating; I recall vividly my discomfort at the inadequacy of my replies. It is perhaps ironic that today Licklider is primarily interested in computers with little regard to psychology per se and I am deeply involved with experimental psychology. A second event was really a series of persistent demands, lasting long after I left M.I.T., from Rosenblith that I pay attention to the neurophysiological results that bear on psychophysics; eventually I did and with the benefit he predicted. The third was a brief discussion with Miller at the height of his passion for information theory during which I remarked that I found it difficult to believe one could view language statistically (in particular, as a finite-state Markov process) without regard to semantics; his poohpoohing of this later amused me when first structural linguistics and then semantics became the focus of his interests.

Columbia University, 1953-1957

In the winter of 1952-1953 I began to realize fully that the Group Networks Laboratory was going to fold and that another position was imperative. Apparently, I had not impressed the Cambridge psychologists sufficiently to generate an offer there, where I would have preferred to stay. In the spring of 1953 I received one from the Department of Mathematics at the Stevens Institute of Technology, but that was not my intended route and Hoboken repelled me as a place to live. At the last minute, Paul F. Lazarsfeld of the Department of Sociology of Columbia University contacted me and arranged to hire me as Managing Director of the Behavioral Models Project. Just how he learned of me I do not know. The project, a brainchild of an interdisciplinary seminar and led by T. W. Anderson, C. H. Graham, Lazarsfeld, Howard Raiffa, E. Nagel, Herbert Solomon, and W. Vickrey, was funded by the Office of Naval Research and administered by the Bureau of Applied Social Research. It was charged with preparing expository documents on models relevant to the social sciences, although research was not entirely precluded.

Our small group was housed in an ugly, dirty-green apartment, which we shared with Fred Ikle, now in charge of the U. S. Arms Control program, in one of the imposing brownstone houses on 118th Street. We were mostly left in isolation except for occasional directives from Lazarsfeld, sometimes gruffly communicated by the official Director of the Project, Solomon. Those members of the group that I remember best are Sidney Morgenbesser, now Professor of Philosophy at Columbia, who while clearly brilliant was most reluctant to write; Ernest Adams, now Professor of Philosophy at Berkeley; and James Coleman, an ex-chemical engineer then a graduate student in Sociology and now a Professor of Sociology at Chicago, who is famous for his report on educational interventions with culturally deprived groups.

Lazarsfeld was involved in founding the Center for Advanced Study in the Behavioral Sciences at Stanford, and he attended the opening year, 1954-1955; he also arranged for me to be invited. He had a European view of the academic hierarchy, and argued that the more junior fellows should assist the senior ones. Fortunately, the director, Ralph Tyler, vetoed this idea of two classes of fellows and the precedent has been followed that each fellow decides exactly what he will do — a wise decision.

The year at the Center was productive, including the drafting of portions of *Games and Decisions*. Much of the rest was written the next year when I was back at Columbia and Raiffa was at the Center. So although it appears as if we wrote the book while together, in fact we were apart. I have always felt that we would have not written it had we been together because it would have been too easy to talk.

Raiffa left the publishing negotiations to me, and I soon reduced the choice to McGraw-Hill or Wiley. The former made a better offer on the basis of a projected lifetime sale of 7500 copies versus Wiley's 4000; however, I was strongly attracted to the attitudes and evident integrity of the late Gordon Ierardi of Wiley, and finally we agreed on a retroactive version of McGraw-Hill's offer provided that the sales exceeded 5000 copies in the first two years. By publication date, they dropped the retroactive clause on the basis of field reports, and sales have far exceeded either estimate and continue moderately strong today.

My last two years at Columbia were brightened considerably by numerous weekend discussions with Eugene Galanter, then an Assistant Professor at the University of Pennsylvania. He was at this time an ebullient, outspoken young Turk who outraged many experimentalists, since they tend personally to be a rather conservative lot. But Galanter's quick, reactive mind was impressive to many others, including me. We had met in 1955 in San Francisco at the American Pyschological Association meetings, and although our styles were very different, we found each other's company agreeable and intellectually stimulating. He systematically tutored me in psychophysics, and he first introduced me to Stevens's work. I taught him something of the mathematics I was developing for *Individual Choice Behavior*. There is no doubt that our dialogues affected that book, were influential in my deciding to go to Pennsylvania, and continued to affect my work into the middle sixties.

In connection with the publication of *Individual Choice Behavior,* I learned the important lesson to demand that publishers limit their copy editors — often very inexperienced English majors — to marking the manuscript for the printer. I had carefully prepared the text so that "we" was used when I was leading the reader through an argument and "I" when I was expressing a belief or an opinion. Evidentally the copy editor knew that one should never under any circumstances use the first person singular in a scientific monograph and also never use a passive construction. That created problems with sentences of the form, "I believe that X is true," which he or she resolved simply by deleting "I believe that."

At some point either just before or after leaving Columbia, I was invited to participate in the exclusive, Eastern, Under-40 Psychological Round Table. In a sense I took this to be a semi-official annointment of me as a psychologist in lieu of a Ph.D. in the field.

Harvard Univeristy, 1957-1959

I went to Harvard on a five-year appointment as Lecturer on Social Relations, but stayed only two years. This position was arranged by Frederick Mosteller, who was then jointly in Social Relations and Statistics. I had first met him, along with Robert R. Bush, in the early fifties; evidentally he was taken by my work on choice models or game theory or both. Aside from teaching jointly with him in his undergraduate statistics course and working with a group of junior faculty on a methodology course, my contacts with members of the department were marginal. Most of my intellectual activity was with students — including Merrill Carlsmith, Bernard Cohen, Saul Sternberg, and Wayne Wickelgren — ; with Elizabeth Shipley, a research assistant introduced to me by Galanter and later my Ph.D. student; with S. S. Stevens of the Department of Psychology; and with Bush and Galanter, with whom I had a small grant from the American Philosophical Society which permitted us to meet frequently on weekends.

Among Stevens's other influences, discussed under Research, he toiled over my writing. English was a continuing problem for me as a child and as an adult. I found it difficult to master: spelling plagued me and still does to a degree; my vocabulary remains modest; I am unable to this day to pronounce a new word on sight; and compositions of only a few paragraphs were hideously hard work and the results mostly absurd. A high school English teacher warned me that if my compositions did not improve I would fail in college; M.I.T. did immediately place me in a remedial composition class; and I was often marked down for poor writing.

As an undergraduate, I eventually came to accept the importance of written communication and became increasingly sensitive to the elegance of such authors as Bertrand Russell. As a graduate student I tried more and more to write, often writing up lecture notes with some care. Over the years I have slowly improved it, helped in part by trying to analyze the writing of authors such as George A. Miller and in part by the careful editing and rewriting of Stevens, to whom I shall always be indebted.

At Harvard there began a most satisfactory funding relationship with the National Science Foundation which, except for my three years at The Institute for Advanced Study, has been continuous. At first the grants were from the Division of the Social Sciences and later from the Program in Psychobiology. Taking into account inflation and increased salary levels, the level has remained roughly constant; a half-time secretary, one or two graduate students, summer research, limited travel, and a modest amount of equipment. One of the joys of dealing with NSF has been the flexibility permitted the researcher. I rarely see where I am going beyond the next study and, depending on what ideas arise and what opportunities present themselves, I shift about, pursuing leads where they take me, sometimes returning to old themes after years and sometimes starting new ones. One needs a sympathetic agency to understand the nature of such unprogrammed research, and I can only hope NSF continues to be one in spite of the myriad pressures on it to be otherwise.

University of Pennsylvania, 1959-1969

As Galanter, Mosteller, and I (1974i) have related in an obituary about Bush, the idea of introducing Bush's name as a candidate for chairman of the Pennsylvania department was hatched by Bush and Galanter on a train ride from New York to Cambridge to visit me. Surprisingly, he was made chairman in 1958 and I joined the department a year later as a professor.

For the first time in my career I now held a senior position, indeed one with considerable local influence because of my close advisory role to Bush. I found some features of academic politics to my taste, but never sufficiently appetizing to lead me seriously to consider the chairman-dean-provost-president route, or any segment of it. I enjoy the private and policy aspects of helping to run a department or a school, especially one in a new growth phase, but I detest the unrelenting routines and public performances required of most official administrative positions.

Aside from helping to reconstruct the department, which effort we always viewed as quite successful, Bush, Galanter and my main joint activity was the three volume *Handbook of Mathematical Psychology*. The 1950s had seen a great deal of activity in mathematical psychology, much of it stimulated by summer workshops supported by the Social Science Research Council (for a summary, see Mosteller, 1974), and we felt that the time was ripe for some sort of summary. At first we thought in terms of an integrative text, but it soon became clear that this was likely to be exceedingly costly in time. So we elected to edit a moderately high level collection of expository chapters which could be used for advanced undergraduate and graduate teaching and could also serve as a source for those writing undergraduate texts. It began as a single volume and grew into three simply because of the length of the contributions and by our adding topics. I was not only heavily involved in the editorial work — witness the order of the editors — but in writing for it, contributing to five chapters. In retrospect, among our other errors of judgment, of which there were several, the material on psychophysics probably should have been spread among various authors. We didn't do it that way because Galanter and I had a point of view that we wanted to formulate consistently for the whole field. However, it was premature and the field would have benefited from other points of view.

Associated with the *Handbook* was the two volume *Readings in Mathematical Psychology*, whose content was largely determined by the authors of the corresponding articles.

During this same period, I was involved in two other activities also designed to foster mathematical psychology. A group consisting of R. C. Atkinson, R. R. Bush, C. H. Coombs, W. K. Estes, W. J. McGill, G. A. Miller, P. Suppes, and myself founded the *Journal of Mathematical Psychology*. We did this largely as a response to the difficulties we were having in finding suitable outlets for our articles. None of the usual psychological journals were terribly happy with our articles, either because they included too much mathematics or because of our different and, to them, unacceptable analyses of data. At that time Academic Press was rapidly expanding its stable of journals, and they agreed to publish it. I have remained active in the management of the journal over the years, and effective in 1975 William Batchelder and I will be joint editors of it.

Although we have never formed a Society of Mathematical Psychology — personally I see no need for the added bureaucratic structure when a field has so few practitioners — the Journal's board of editors, which is self-perpetuating, has run annual meetings of about 150 participants. From time to time, I and others have made attempts to effect some sort of union with the Psychometric Society, but as a group the mathematical psychologists have been uninterested in the idea. However, the issue is not dead.

When the SSRC terminated its committee on mathematical social science, a number of us — in particular, Bush, Estes, Coombs, Suppes, and myself — felt that the summer training activities and workshops supported by SSRC had been extremely effective and should not only be continued but expanded, especially in the social sciences other than economics and psychology. We prepared a proposal to the National Science Foundation which in reduced form (at a level of about $250,000 per year) was granted, with the Center for Advanced Study in the Behavioral Sciences having fiscal responsibility and the Mathematical Social Science Board having intellectual responsibility. I have been closely associated with this Board over the years, both as a member and twice as its chairman, and have been intimately involved in writing the three grant proposals it has submitted to NSF and with its recent transfer to the National Research Council - National Academy of Sciences in Washington.

In 1963 I was elected to membership in the Society of Experimental Psychologists — the national, more elderly, and far more staid counterpart of the Psychological Round Table — and in 1966 to the Boston-based American Academy of Arts and Sciences.

Returning to life at the University of Pennsylvania, let me remark that, except for the *Handbook,* the collaboration I had anticipated with Bush and Galanter never worked out. Bush was caught up in his administrative position and, in any event, the overlap of our intellectual interest was unfortunately not large. After Bush resigned as chairman in 1964, my relationship with him waned, for now we had neither politics nor research in common, and there was little else that bound us. I did not see him often in the ensuing seven years during which time his health deteriorated leading to his untimely death in 1971.

I continued some work with Galanter, especially jointly with students, but it never evolved into the working relationship I had hoped it might. Because I am convinced that collaborations between theorists and experimentalists are important, I will expound on some of the problems involved in the final section, Musings.

The one person with whom throughout my ten years at Pennsylvania I maintained a steady, largely luncheon-based, friendship was Francis W. Irwin. He is a splendid example of a gentleman and scholar, of the sort one reads about in turn-of-the-century novels but does not expect to know. He has spent his entire academic career — undergraduate, graduate, and professorial — at the University of Pennsylvania, retiring this year. Obviously I could, and did, learn much from him of the history and traditions of the University, which he dearly loves. But more than that, I learned much conceptually about psychology. As anyone who had read his book (Irwin, 1971) knows, he demands that basic psychological concepts be logically organized so that one can establish by a series of qualitative experiments whether or not an organism exhibits the concept in question. This is moderately straightforward for discrimination and preference, but it becomes far more subtle for aversion, expectation, intention, and the like. Although this was his research focus during the years we saw each other regularly, he had a residual interest in psychophysics from his earlier work and he found problems of measurement philosophically fascinating, and so we had many areas to talk about. Many of our lunches involved more people; I especially recall some with the exceedingly knowledgeable Richard L. Solomon and the vivacious biologist, Vincent Dethier, who were then collaborating on the difficult question of whether or not a fly can be conditioned operantly. It was a question perfectly suited to Irwin's analytic approach.

At about the time Galanter left the Department to become Chairman at the University of Washington, David M. Green joined it. We did not begin to collaborate right away, but we had run our first free-response detection experiment before he moved to the University of California at San Diego. As I have already made clear, I judge this collaboration to be nearly ideal and it has proved to be a durable one, capable of withstanding the vicissitudes of many changes in location. He is an extremely efficient person who, apparently effortlessly, manages a comparatively large laboratory while teaching, writing, editing a journal, and consulting extensively. Nevertheless, whenever we get together for a few days, he seems to have nearly full time for me. Ours is an easy relationship in which ideas are quickly understood, bad ones promptly shot down, and experiments rapidly realized, especially since his laboratory is fully integrated with an on-line computer. Moreover, we are friends who enjoy each other's company.

Another important relationship, that with Patrick Suppes, deepened at about this same time. We had known one another for some time and had already collaborated on a chapter for the *Handbook* (1965a) and on two articles for the *Encyclopedia of the Social Sciences* (1968a,b), but our planning and work on the *Foundations of Measurement* drew us closer and we became personal friends. One reason I elected to spend my 1966-1967 sabbatical as an NSF Senior Postdoctoral Fellow at the Center for Advanced Study in the Behavorial Sciences, aside from its inherent quiet and beauty and its good general intellectual stimulation, was to be able to collaborate more closely with Suppes on questions of measurement. Among other things, together with Ernest Adams, we set up a joint Stanford-Berkeley seminar on measurement which met regularly throughout the year.

The most frustrating thing about collaborating with Suppes is trying to get him to spend time on your problem, rather than on one of a dozen others he is also involved in. His mind is as quick as any I know, his memory prodigious, and his breadth of interest staggering. It includes everything I have worked on and at least twice as much again: logic, philosophy of physics, learning, computer-assisted instruction, perception, semantics, and more. Moreover, for many years he has run a very large research establishment at Stanford, at times numbering more than 100 people, and he has maintained worldwide speaking, administrative, and research commitments. I have never understood how he has withstood the onslaughts on his time and energy and maintained, into his fifties, a jovial curiosity about all ideas and a youthful intensity. In any event, one has to be devious or persistent or both to get his attention. As Volume I of the *Foundations of Measurement* neared final form and certain parts required his concentration, I simply moved in with him and his wife for three weeks until the work was done.

Bush was succeeded as chairman by Henry Gleitman. Although they differed greatly in style and research interests, I continued serving in an advisory role much like the one I had with Bush. At the time, Gleitman and I had little opportunity for intellectual exchange — department politics can be remarkably compelling — but after I left Pennsylvania and he stepped down as chairman we continued our friendship and developed an intellectual exchange, often a three-way one with his brilliant wife, Lila, a linguist, which continues to this day.

Gleitman was very influential in arranging that I be honored the year following my Center stay by being made Benjamin Franklin Professor of Psychology at the University of Pennsylvania, one of their six University professors at the time. As a name chair was in many ways ideal for me, especially since the teaching obligations were minimal, it must seem odd that after spending the next year on leave I left Pennsylvania. To account for this, I must bring in a personal matter. I do not believe one's personal life belongs in an intellectual history unless it bears directly on it; here it does.

My first marriage to Gay Gaer Luce — known to many psychologists for her expository work on sleep, dreams, and biological rhythms — ended in divorce in 1967. Shortly thereafter I married Cynthia Newby. A number of my professional decisions since then have been seriously, and quite reasonably, influenced by her preferences, which include a love for mild climates and artistic people, some distaste for the more pretentious elements of the academic establishment, and a strong aversion to large, noisy, cold, and smoggy cities. Philadelphia was anathema to her. Brazil, where she had emigrated for several years prior to our marriage, was most satisfying. I agreed to try Brazil for a year to see how I reacted to it, and we spent 1968-1969 in Rio de Janiero, which although both large and noisy is mild and where we lived it was free of smog because of ocean breezes and relatively quiet because our apartment was at the end of a deadend street. I was an Organization of American States Visiting Professor at the Universidad Catolica de Rio de Janiero, a guest of Aroldo Rodrigues, a social psychologist trained at UCLA. Although I responded to some of the appeal of Brazil, I could never live there permanently. One reason was that I found it nearly impossible to pick up Portugese — my difficulties with languages date back to early childhood when, in a private grammar school, I was virtually unable to learn French and had difficulties with English. Another was that no one there was really interested in the sort of work that I do.

The Institute for Advanced Study, 1969-1972

Shortly before I departed from Brazil, Carl Kaysen, the recently appointed Director of the Institute for Advanced Study, Princeton, N.J., inquired about my joining their faculty. Although the conditions — a visiting appointment for two years with his personal assurance that it would become permanent once some political problems were overcome — would not normally have been acceptable to me, in many ways the Institute seemed an agreeable compromise between my needs and those of my wife, and so I leapt at his proposition. Given that I would be away so long and given Cynthia's aversion to Philadelphia, I resigned my appointment at Pennsylvania even though the administration urged me not to.

Although the Institute appears to be a perfect scholarly paradise for a theorist and during the last of my three years there, when the social science group occupied a handsome new building, certainly an elegant one, in fact it seemed more like purgatory to me. There was political opposition, mounted primarily by the mathematicians and later joined by the humanists, against Kaysen, against the social sciences, and against me in particular. The battle between Kaysen and the faculty erupted in the public press the year after I left when he forced the appointment of a social scientist against a majority of the permanent faculty; it was an ugly atmosphere and it remains to be seen if a first-rate scientific social science faculty can be assembled.

In spite of my enormous discomfort and frustration at my situation, I was able to complete work on Volume I of the *Foundations of Measurement,* to write a number of papers, and to maintain the research program with Green. Moreover, I had some enjoyable intellectual contacts with various social scientists who were there at the time, especially in several informal seminars I ran on measurement and information processing for psychologists in the area.

While at the Institute I received two very major honors. First, in 1970 I was awarded one of the three annual Distinguished Scientific Awards of the American Psychological Association (APA). And in the Spring of 1972 I was elected to the National Academy of Science (NAS). Both had their consequences in responsibilities. The APA subsequently appointed me to the Scientific Awards Committee for the 1971-1974 period. And the NAS asked me to become a member of the fifteen-person Executive Committee of the newly formed Assembly of the Behavorial Social Sciences of the National Research Council (NRC). The NRC does the scientific and policy advisory work of the NAS, and those parts that are primarily social science in character come under the direction of this

Executive Committee. This has proved to be an interesting experience and there is the possibility that the Assembly may have some significant long-run impact on both the development and application of social science research.

University of California at Irvine, 1972 —

At the start of my third year at the Institute, I finally accepted fully what I had begun to suspect early on, namely, that permanency would be possible, if at all, only after a bruising battle. Moreover, although this may be a matter of sour grapes, I began to doubt whether the atmosphere would ever prove congenial to a psychologist of my breed. So I began to explore alternatives, especially ones in mild climates.

One of the visitors I had invited to the Institute was John Yellott, originally from Minnesota but now at the University of California at Irvine. Soon after he learned of my interest in a position, I received an attractive offer in the School of Social Sciences at Irvine which I accepted.

This campus was founded only in 1966, and the School was the brain child of its first dean, James March, who favored both interdisciplinary opportunities for social science research and mathematical approaches to such problems. As a result, the School is composed of people who tend to have one or both of these traits, and it is not subdivided into departments in an attempt to promote unusual interactions. The School is controversial at UCI because of various factors — including the intellectual style of some faculty, a strong tradition not to mimic traditional structures, and the fact that much of its approach is not very appealing to average quality undergraduates — and so it has been under some attack by the rest of the campus. Furthermore, it has its own self-doubts. It is in a period of, as yet unresolved, crisis, and I am probably too caught up in what is going on to discuss it objectively.

The Future

Writing about one's career as one approaches age 50 is not a wholly happy activity. Considerably more than half of the career is complete, thinking about people and events brings up some bittersweet memories, the work while satisfying is neither as good nor as extensive as I had once hoped it might be, and inevitably I am forced to consider: what next? This is accentuated by the collapse of my second marriage.

Should I continue on the track I have been on — research in psychophysics and measurement — or should I seek out a more-or-less new career? I have long felt that while my research continues to prosper and interesting ideas continue to bubble to the surface no compelling reason exists to stop doing what has brought considerable satisfaction. The difficulty, however, is in knowing when it has stopped going well. Even at the height of one's productivity there are longish fallow periods during which one becomes convinced that the well has dried up. At the moment this is not a problem as I find myself busy in two productive collaborations. But I do worry a bit because they are in areas in which I have long worked, and perhaps it would be well to shift at least some of my effort into mathematically less developed and, for me, new areas. With this in mind, last academic year, together with Rochel Gelman and C. R. Gallistel, a married couple from the University of Pennsylvania then visiting Irvine, I was involved in a seminar on how children acquire basic numerical concepts and operations. It is a fascinating area, but I have not yet seen how to do effective modeling of it.

At the moment, then, I continue to see modeling and research as the most likely activity for some years. But I do not foreclose the idea of quite different activities for at least limited periods. My involvement during the last few years with social science policy matters in Washington has made clear that there are interesting possibilities here when one is ready to close down his research activities.

MUSINGS

As the first draft of this spilled out of my typewriter, it included a number of asides prompted by something in the research or the chronology. Some were brief enough that I have let them stand. Others I decided to excise and bring together, sometimes in expanded form, into this final section. Each is an isolated fragment, not related to the others.

Teaching

Since I have spent most of my adult life in university settings with professorial titles, I cannot but be a teacher in some sense. But certainly not a TEACHER; rather I am a RESEARCHER-teacher. To the extent feasible, I prefer to blur the roles. So I feel more at home in an advanced seminar or working with an individual student than teaching a large lecture class. Since I do not get my kicks on the lecture platform, I do as little lecturing to large groups as possible, mostly only at invited talks devoted to my research.

Part of my discomfort with large classes is that the quality of my lectures varies a great deal and I don't seem to know how to make them more even — simple preparation is not the answer. One factor to which I may be overly sensitive is the physical environment: the acoustics, lighting, adequacy of the blackboards, and general feel of the room. A second factor is the background of the audience. It is usual for psychological audiences, both student ones and colloquia, to vary enormously in the knowledge and appreciation of mathematical psychology and psychophysics. All too often I feel that only a handful are sensitive to what I am presenting and the rest are dead weight, and often my presentation loses its force when the mass seems too great.

My greatest contribution to teaching is not as a classroom lecturer, but as an author. Raiffa and I have "taught" thousands about elementary game theory, and my other books — especially the *Handbook of Mathematical Psychology* and the *Foundations of Measurement* — were also designed in part to instruct students and peers.

In the current environment of "relevance," "accountability," and "number of students contacted" my style is a bit out of fashion. Universities have failed to rationalize sensibly the various teaching roles that are possible, and so they find themselves in very awkward stances toward faculty who do not conform closely to the current attempts to make education "efficient." To their credit, they do protect their good research scientists, but not without having to distort the rules about teaching.

I remain an elitist who believes strongly, based on watching and reading, that knowledge is gained and theories are developed by a relatively few, relatively unusual people, and this intellectual base is not greatly increased by having large numbers of relatively indifferent undergraduates about. Keeping millions of young people off the job market and certifying them as appropriate for certain classes of clerical and managerial positions by sending them through four years of a university is almost certainly not an efficient operation, and it may well be one, if we are not very careful, that will do great damage to our ability to generate and transmit advanced knowledge. Such students mostly belong in colleges, not universities.

Especially pernicious is the challenge that a substantial portion of research carried out and all work required of students should be shown, rather directly, to be relevant to some social problem. Admittedly, universities have in various ways failed to shoulder responsibilities they should have taken on, but I believe that the responsibility they must treat as primary is the generation and transmission of knowledge that gives a better understanding of the world. When I hear the demand for more relevant education I am reminded of how engineering schools in American heeded the call in the 1930s and students learned highly specific and narrow procedures suited to specialized industries (much of my training in aeronautical engineering was of this sort) and so when the demands of World War II led to novel engineering problems, the work often had to be taken over by irrelevantly but more basically trained physicists and mathematicians. But that was thirty years ago, and society's long-term memory is, at best, a few years.

Brightness and Creativity

What sorts of people become creative scientists? One thing seems clear: sheer brightness is neither necessary nor sufficient. I have known extremely bright, clever, and sophisticated people who have done little of real interest, and slow, plodding, and unsophisticated ones who have done a great deal of importance. But I have also seen the other two combinations, so I would not claim that brightness is a handicap and slowness really a virtue. What seems to ail some of the very bright is their hypercriticalness which destroys every idea before it is adequately nurtured. I have the impression that the successful scientist lets an idea mature privately, or at least within the confines of a very sympathetic environment, until he has had a chance to explore it thoroughly before exposing it to unmerciful criticism. At least I find that approach congenial.

One aspect of creativity is recognizing where the "body is buried." One needs intellectual good taste or good luck to realize which among all the bits and pieces of information reported by experimenters and theorists are important and to see what coherent view makes sense of them. It is not necessary that one hold an accurate or complete view provided it is one that gets the field to focus on something of importance. In my personal experience, a fine example of a person who, although not unusually quick, had this talent was Stevens. Almost everything he did could be and was attacked by the very smart, and yet in at least three very important cases he had a sense of the significant issues before others did.

In selecting among graduate student applicants we seem relatively poor at spotting this quality, and we are usually unwilling to ask those whom we later discover to lack it to leave. In the 1950s and 1960s our pattern was to admit to graduate school a number of bright people in the hope that a few would also be creative. The current economic limitations make this strategy unfeasible, and probably will remain true for some time to come. So it is much more imperative than it was a few years ago to devise ways to spot creativity as early as possible.

Collaboration of Experimentalists and Theorists

An applied mathematician doing psychological theory is always in danger of losing contact with empirical reality, and he must continually force himself to consider the testability as well as the depth and generality of his ideas. Otherwise, he is likely to become a pure mathematician of indifferent quality. One solution is for him also to run experiments. This is often the solution urged upon him by his experimental colleagues; for example, Stevens was most vociferous about it. This is fine when the equipment and data collection are both simple — and I have had students and assistants run several such studies — but it is a strenuous strategy when the experimental techniques and apparatus are complex. Although there are exceptions (in vision F. Ratliff and D. H. Krantz are two examples), one is likely to stop doing theory and become a second-rate experimentalist. My feeling in such cases is that, as in physics, theorist and experimenter should collaborate closely. This can happen only if the theorist understands well the problem facing the experimenter who, on his part, must understand well the language of the theory; they should complement, not compete. Of course, this does not mean that the theorist should have no ideas for experiments or the experimenter no ideas for theories — quite the contrary — but that each should work out the details of what he does best and, presumably, finds most congenial.

If such collaborations are really desirable, why do so few exist in psychology? Perhaps the major reason is that only recently, and then in only a few areas of psychology, is the equipment becoming so complex as to warrant it. In any event, for me at least, it seemed clear that auditory psychophysics had achieved the level of complexity that I did not want to run my own laboratory.

Of my two collaborations of this sort, one with Galanter and later with Green, the latter developed into a longer lasting, more productive program. One searches for reasons. One may be the question of proximity — though not in the direction one might anticipate. My major collaborations — with Raiffa on *Games and Decisions,* and with Krantz, Suppes, and Tversky on the *Foundations of Measurement,* and with Green on numerous papers on psychophysics, though not yet a book — have throughout the major part of the relationship all been at some physical distance. We have maintained contact by correspondence, telephone, and most importantly, brief, intense meetings. It may be that my personality is such that some kinds of work cannot be sustained with a peer when we have daily contact. Unlike many mathematicians, I do not much like to work at a blackboard with other people, although my recent collaboration with Narens is a sharp exception to that tendency. Usually, I prefer to meet and discuss what needs to be done and then go do the work in private, where I can mull over my fuzzy thoughts and crazy intuitions. Only after I get it down in some written form and I am willing to interact again, and then I would just as soon if my collaborator first read and pondered it before talking. This style is ideally suited to distant collaboration and is relatively unsatisfactory for those nearby, especially those who like Galanter prefer to talk through the work.

Statistical versus Scientific Inferences

Psychology is one of the heavier consumers of statistics. Presumably the reason is that psychologists have become convinced that they are greatly aided in making correct scientific inferences by casting their decision making into the framework of statistical inference. In my view we are witnessing a form of mass deception of the sort typified by the story of the emperor with no clothes.

Statistical inference techniques are good for what they are developed for, mostly making decisions about the probable success of agriculture, industrial, and drug interventions, but they are not especially appropriate to scientific inference which, in the final analysis, is trying to model what is going on, not merely decide if one variable affects another. What has happened is that many psychologists have forced themselves into thinking in a way dictated by inferential statistics, not by the problems they really wish or should wish to solve. The real question is rarely whether a correlation differs significantly but usually slightly from zero — such a conclusion is so weak and so unsurprising to be mostly of little interest — but whether it deviates from unity by an amount that could be explained by errors of measurement, including nonlinerarities in the scales used. Similarly, one rarely cares whether there is a significant interaction term, but wants to know whether by suitable transformations it is possible or not to get rid of it altogether (e.g., one knows it cannot be removed when the data are crossed). The demonstration of an interaction is hardly a result to be proud of, since it simply means that we still do not understand the nature and composition of the independent factors that underlie the dependent variable.

Model builders find inferential statistics of remarkably little value. In part this is because the statistics for most models have not been worked out, to do so is usually hard work, and by the time it might be completed interest in the model is likely to have vanished. A second reason is that often model builders are trying to select between models or classes of models, and they much prefer to try to ascertain where they differ maximally and to exploit this experimentally. This is not easy to do, but when done it is usually far more convincing than a fancy statistical test.

Let me make clear several things I am not saying when I question the use of statistical inferences in scientific work. First, I do not mean to suggest that model builders should ignore basic probability theory and the theory of stochastic processes — quite the contrary, they must know this material well. Second, my objection is only to a part of statistics, in particular it does not apply to the area devoted to the estimation of parameters. This is an area of great use to psychologists, and increasingly statisticians are emphasizing it over inference. And third, I do not want to imply that psychologists should become less quantitative and systematic in the handling of data. I would urge more careful analyses of data, especially ones in which the attempt is to reveal the mathematical structure to be found in the data.

Computers: A Personal Scientific Dilemma

My career has pretty much coincided with the growth of digital computers, and I have been repeatedly urged by some whom I respect to involve myself deeply with them on the grounds that computers will be a theorist's main tool in the future. I have resisted, thereby probably branding myself a scientific conservative, if not a reactionary. To discuss my position, let me list some of the ways a computer can play a role in psychology and how I have related to each.

1. To compute. This means what it says, and I am delighted with the power it gives us. Much of what Green and I have done would not have been possible without such aid.

2. To simulate. For many stochastic processes that arise in psychology it is difficult to derive analytic expressions for statistical quantities of interest, in which case one may try to estimate them by simulating the process. Although I have and am doing simulations, it is with reluctance. The method is cumbersome and expensive when more than one or two parameters are involved, and one can be misled easily because of sampling variability, and one always fears that some interesting region of the space has not been explored.

3. To control experiments. The use of a modest sized, on-line computer to control stimulus presentations, provide information feedback, and record responses is a godsend for any complex laboratory which is largely electronic rather than mechanical. This flexibility can be a danger since the experimenter can easily run complex stimulus patterns that, unless he is very careful, he is unlikely to be able to analyze.

4. To teach. All sorts of teaching now involves computers, particularly when there are standard routines to be mastered as in elementary mathematics and statistics. In general this strikes me as a good thing, although because I have not taught elementary classes for some time I have not made use of it. In addition, there are large systematic efforts in computer-assisted instruction involving fancy contingencies depending on the progress of the student; a prime example is the laboratory of R. C. Atkinson and P. Suppes. Only time will tell how beneficial this is.

5. As a conception of the brain. This is not really a use of the computer as such, but an attempt to conclude that the brain must be organized much as a computer is. For a time, one heard the argument phrased at the mechanical level, attempting to equate the binary language of the computer with the binary pulses of the central nervous system. This is basically wrong. The presence or absence of pulses carries information in a computer, whereas it is almost certain that temporal patterns of pulses carry it in the brain and so the brain is more of an analogue device than a digital one. Also, the physiological evidence is suggestive that information is not stored in the brain in single locations, but somehow is more diffusely represented.

Another argument centers around concepts of universal machines and all computable functions. There might be something to this were one willing to accept the present basis of mathematics as the ultimate one, in which case the brain must indeed operate within those limits. But wouldn't it be odd if, as of now, all basic mathematical concepts were in hand and all that remained was to elaborate them? But if some mathematical concepts are not developed, those appropriate to brain function may be among the missing, in which case I am convinced we should study the brain and not the computer. In the process some genius may invent — albeit, sloppily at first — some new mathematics which, conceivably, might lead to a better computer.

6. To formulate psychological theories. It is here that my dilemma lies. Roughly, the idea is that interesting psychological phenomena — language production and comprehension, perception of complex patterns and arrays such as pages of print, problem solving, concept formation, theorem proving, game playing, etc. — are processes far too complex to state in any ordinary mathematical fashion, but they can be embodied as computer programs. The test of a theory so formulated ranges anywhere from its ability to solve problems that some human beings can solve (artificial intelligence) to far more detailed comparisons of step-by-step protocols. Probably the most extreme proponents of this approach are Minsky (1969), Newell and Simon (1963), and Simon and Newell (1974); the last two have been especially persuasive to some psychologists.

Although this approach is at present very imperfectly realized, I sometimes wonder if it may not prove to be the avenue of the future. Perhaps the effective mode of encoding psychological theories will be deeply different from anything we have known previously — specifically, maybe computer programs will replace mathematics as the language of theory.

But two aspects of this approach disturb me sufficiently so I have been unwilling to undertake the labor of pursuing it in my work. The first is the current inability of its proponents to articulate clearly the psychological principles that underlie the construction of the programs they write. To the extent that such principles exist, they seem to reside only in the brains of those who write the programs and they can be communicated to others only in the most hazy way. The second is the number of untested assumptions, functions, decision rules, and the like that, together, form a program. I know both from my work, e.g., the psychophysical models with Green, and from that of others how difficult it can be to isolate and test simple, well-articulated principles and assumptions. A good chastening example is the elaborate set of studies sparked by Sternberg's (1969) attempt to decide whether the search of short-term memory is self-terminating or exhaustive. If such limited, apparently sharply formulated questions cannot be decided readily, how can we possibly test large complexes of such ideas strung together as a program? And if no systematic tests are possible about the components that make up the program, then is this psychological theory or artificial intelligence? The latter is fine, but it does not happen to be my area of interest.

So, I have elected the more conservative, more plodding route. The problems I can tackle are not so glamorous to the average person and the building and testing of ideas is slower, but I have some limited hope that a fragment or two will survive as a permanent part of psychology. I am not as optimistic about any attempt I might make at writing a program for something really complex.

Mathematics in Psychology

When asked my profession by strangers, I usually say "psychologist" or "mathematician" and only rarely "mathematical psychologist." I used to, but experience has made me wary. Too often I have been told in no uncertain terms that mathematics has nothing whatsoever to do with psychology or been skeptically asked to explain the connection. For a while I hope the question meant an open mind and I would try to discuss the matter. At first I used to illustrate applications by example, but we always bogged down in technical detail — usually both experimental and mathematical. That failure led me to try some form of the clever-question gambit. For example, to the wife of one physicist I agreed to try to answer her question if she would explain to me why mathematics had anything to do with physics. I fear that the cleverness of my strategy escaped her notice for, to her, the prima facie evidence seemed a sufficient answer. Another tack I have tried is to ask if all factually correct things one might say about a person are independent of one another, and then to suggest that the study of how one set of statements can be deduced from a set of other statements, taken as primitives, was in fact mathematics. At best this tends to draw a sympathetic, but pained, expression and at the worst the more or less explicit suggestion that I belong under the care of a good (presumably clinical) psychologist.

Nonetheless, there are two serious questions lurking near the surface. First, has mathematics as yet played a serious role in the development of any areas of psychology? Second, is it conceivable that the mathematics we now know, molded as it has been by the development of physical science, is especially appropriate to psychological problems?

At the risk of offending some colleagues, aside from the special use of statistics in much psychological research, I believe that there are only two areas where mathematical modeling can be shown to have had a profound impact: the study of sensation and perception and psychological testing. In the former, the modeling appears to be cumulative, to have led to empirical discoveries, and to be essential to the ongoing life of the subject. In the latter, modeling appears to be essential in handling the masses of data involved, and while I have my doubts about how deeply it gets at questions such as what intelligence is, there can be no doubt about its social significance. Psychological testing is the one large-scale technology spawned by psychology, and it is more mathematized than most people realize.

In other areas, success — either conceptual or practical — is far less apparent. For example, studies of preference and motivation have resulted in a number of careful mathematical analyses — I have contributed several of them — but mostly the results are negative and not very cumulative. Ingenious experimental studies have knocked down one general idea after another without making very clear where to go next. Perhaps we are just witnessing the initial interplay between theory and data that tends to sharpen both and to accumulate a body of solid empirical findings that make it increasingly difficult to formulate a theory that cannot be rejected out of hand. This stage preceeds the one where we begin to feel we have a good first approximation to a correct theory.

In learning, hundreds of papers studying and testing operator and Markov models have, in my opinion, come to very little. True, we can set up models that give surprisingly accurate descriptions of certain sets of experimental data, but this seems to have provided us with little depth of insight into the learning process — witness the inability of modelers to account well for certain basic phenomena such as the effects of partial reinforcement and reversal learning or to predict the outcomes of new experiments. In recent years, work has shifted away from such models and experiments towards more schematic formulations of information processing and memory in which mathematics plays a decidedly auxiliary role.

One difficulty in much psychological modeling is in separating the theory of the human being from the boundary conditions that model the context (experiment) in which the person is placed. This separation is characteristic of all physical theory and pretty much accounts for the different use of the words "theory" and "model" in science (though not philosophy); it has not been very characteristic of most mathematical work in psychology. To the degree it is achieved, one begins to see both cumulative improvements in the theory and the ability to predict new experiments; to the degree it is not achieved, one sees only models of specific experiments in which the role of the person and that of the experimental design are not clearly separable.

I suspect that much of our problem in using mathematics effectively arises from the state of conceptualization in psychology rather than from the appropriateness of mathematics in formulating psychological theory. But there does remain the haunting fear that the existing mathematics is not, in fact, particularly suited to the problems of psychology. Consider, for example, the representation of uncertainty in decision making. I can never get over the feeling that the attempt to cast it into probabilistic terms is misguided; intuitively, I sense that however it is that human beings handle uncertainty, their calculus is different from the probabilistic one. Or take memory and learning; can it be the troubles we are having has to do with the fact that memories seem to be diffusely represented in the brain and so may not be very amenable to our usual set theoretic formulations?

Perhaps only rarely — psychophysics may be the prime example — is the existing mathematics well suited to the phenomenon; in other areas we may have to become involved in the creation of new sorts of mathematics. If so, our time perspective had better be a long one, for we await a genius.

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