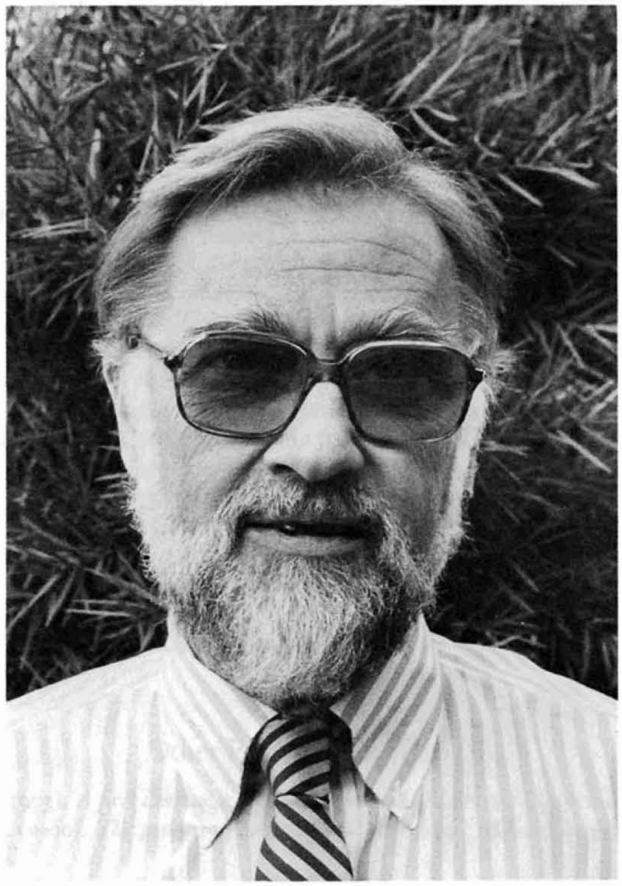

R. Duncan Luce

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 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 begin with the steps that led me into psychology. Next, I describe my research themes, giving little attention to the where, when, and with whom. The following section provides the actual chronology, citing professional highlights and the intellectually important events and people. Finally, I close with some musings about several general matters that strike me as important.

A draft of my autobiography was circulated in the early 1970's to a few people who figured large in it. For their helpful criticisms and comments, which I have in most cases used, I would like to thank Eugene H. Galanter, Henry Gleitman, David M. Green, the late Francis W. Irwin, Cynthia N. Luce, and Patrick Suppes. The present account is an abridged, lightly edited, and updated version of a chapter in T. S. Krawiec (Ed.), (1978), *The Psychologists, Vol. 3* (Brandon, VT: Clinical Psychology Publishing Company). Permission to use the original material has been generously granted by the editor and the publisher. My thanks to my wife, Carolyn A. Scheer, for suggestions and criticisms of this version.

Undergraduate and Graduate School

My parents, although both college educated and my father trained as a dentist, were hardly intellectuals, and as a child I never aspired to be one. As a teenager in Scranton, Pennsylvania, I preferred painting landscapes and still lifes 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 MIT in 1942, I opted for aeronautical engineering, mainly because of a romantic fascination with airplanes 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. After a few years, an increasing awareness of the risks and a wife who did not like the noisiness of a light plane led me to give it up.

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 that its officers, even during a major war whose outcome in 1943 still seemed uncertain, 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 the summer when 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. I was then 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 MIT as a graduate student in the Department of Mathematics.

As well as I can recall, I rejected physics on two grounds: its heavy involvement in weapons applications and its high level of formal development. I felt then 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 field was not clear to me. 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 MIT, 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, and another paper on the same topic followed shortly.

Although I didn't know it for sure, I was hooked. Still, the problem of a thesis remained—no one in the Department of Mathematics was interested in social networks, and at the time MIT did not have a psychology department. The nearest mathematical topic was cybernetics, but I had not attracted Norbert Weiner's notice. For reasons not wholly clear to me, a young algebraist named I. S. Cohen was assigned as my advisor on the then very unapplied topic of semigroups. At the time, it seemed a deflection. Some twenty years later, at a cocktail party, I ran into W. T. Martin, who had been chairman of the Mathematics Department at the time; and, to my surprise, he brought up the events surrounding my thesis, volunteering that the department had erred in not letting me pursue my psychological interests. Perhaps so, but probably not, since I later made considerable use of just this type of mathematics in working on the theory of measurement.

As work progressed on the thesis, 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 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 (RLE), who had some association with Alex Bavelas's Small Groups Laboratory, appeared at the Beacon Hill apartment I shared with Louis Osborne (a physicist later much involved with accelerators at MIT and Harvard) and Alan J.

Perlis (a well-known computer scientist, now at Yale). I had earlier met Bavelas through Festinger, just before Festinger and 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 expect these are the usual ingredients—only the mix varies.

During the next seven years I often questioned whether I had not made a foolish, irreversible decision. At that 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 then available at MIT—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, my research can be grouped into four general topics: group interactions (including game theory), probabilistic choice theory, psychophysics (including response times), 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.

Group Interactions and Game Theory

The work first stimulated by Festinger continued during my three years with MIT's Small Groups Laboratory. The main psychological idea was that many working groups have imposed upon them a communication 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 partitioned into wedges; 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. But even though we wrote a great deal and presented much data, I don't think we learned very much about communication in small groups.

In part through the skeptical questioning of some of the superb group of psychologists then being collected in MIT's new Lincoln Laboratory, initially located just down the hall in the "temporary" Building 20 (still heavily used) 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 not inherently very interesting. As a result, I became receptive to better approaches and during my last year at MIT began to study game theory as a possible model for some kinds of interactions. At least there were actors who made choices, not the propertyless nodes of the digraphs, as well as some communication among the participants.

My knowledge of game theory deepened after I moved to Columbia University in 1953. Howard Raiffa, then of the Department of Mathematical Statistics, and I agreed to write a short summary report on game theory designed primarily for social scientists. That short report evolved into the 600-page book *Games and Decisions* (1957), which remained in print for nearly 30 years; a reprint is forthcoming from Dover.

Before its publication, however, my interest in game theory and, indeed, in the whole area of modeling processes of social interaction had waned. I concluded that despite the obvious great importance of such interactions, neither our experimental nor our mathematical techniques were adequate to the problem. My view is little changed. 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 1954–55 when I was, for the first time, a Fellow at the Center for Advanced Study in the Behavioral Sciences (CASBS), Stanford, California. 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 which has dominated my intellectual life.

On returning 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—the assertion that choice probabilities behave like conditional probabilities from a much larger set of alternatives. That name was ill chosen, and I knew it at the time, because the "axiom of choice" exists in mathematics and is of much greater significance. The dilemma was that I could not think of a suitable alternative term for the intended interpretation: choice. I recall neither where the idea came from nor when I first wrote the axiom down, but probably it was during the winter of 1956–57. By the spring of 1957 a 100-page, red-covered mimeographed technical report had been distributed to some interested people. That summer a number of them met in a mathematical psychology workshop at Stanford, and the "red menace" was a major focus of discussion, including some controversy between Patrick Suppes and me. One consequence 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 John Wiley & Sons, who had published *Games and Decisions*, requested it, even though he knew that its sale would be marginal. It appeared under the title *Individual Choice Behavior* (*ICB*) (1959b) with, not by accident, a bright red jacket. The book had 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, which encouraged me to explore them further. 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. So I abandoned it, returning to psychophysics only some years later.

The work on learning in *ICB* was suggested by the linear operator models of Bush and Mosteller (1955). The choice axiom led naturally to nonlinear models that have the mathematically happy feature of being commutative, but thereby totally lacking the psychologically needed property of suppressing the distant past. Interest in it, along with other operator models for learning, waned.

The chapter on utility in *ICB* led to the curious prediction that probability of choice between certain gambles should vary as a step, rather than a continuous, function of the event probability. Elizabeth Shipley and I ran an appropriate experiment and found supporting, though not conclusive, evidence. The experiment has never been replicated, and it remains an isolated fragment that seems not to have affected any later developments.

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. A number of experiments, ranging from color perception to preference, 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.

The most interesting long-term consequence of *ICB* was Tversky's (1972) generalization known as choice by elimination, which explicitly takes into account that alternatives possess structure. The choice axiom is the special case in which there is no such structure.

Psychophysics, 1954–1963

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 MIT in the late 1940's and early 1950's in which information theory applications to psychology generally and psychophysics in particular were all the rage. Indeed, one of my first activities at Columbia was a long paper on information theory and its applications in psychology (Luce, 1960). But not until the year at CASBS, under the wise tutelage of Albert Hastorf, did I begin to delve carefully in psychophysical theory. Starting at the headwaters of the subject, I studied the Weber-Fechner problem, and two papers resulted.

One, with Ward Edwards (Luce and Edwards, 1958), pointed out the fact, surprising to me, 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, this method would have led to the wrong answer. The proper method is to solve Abel's functional equation. A small literature has resulted, which is summarized in Falmagne (1985). The second paper introduced what amounts to an algebraic approximation to the probabilities used by Fechner, but that is more appropriately discussed under measurement.

During the period from 1956 through 1961, when I was greatly preoccupied with choice models, much of what I did in psychophysics had to do with them. But not entirely. While at Harvard (1957–59), I spent a fair amount of time with the late S. S. Stevens—one either spent a fair amount of time with Smitty 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 earlier in support of neural quantum theory, the idea that the mental representation of stimuli 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 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. Among other things, neither model predicted the limits on information transmitted in absolute identification experiments (Miller, 1956), which seemed to me another key psychophysical phenomenon requiring a natural account.

Before leaving Harvard, I wrote "On the Possible Psychophysical Laws" (1959a), an obvious takeoff on Stevens's title. As my paper really concerned 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, and reprinted, I fear that it has rarely been understood. The fault is mine, for although the writing seems clear locally, it is misleading globally. In truth, it says nothing whatsoever about the form of psychophysical laws, but only explains why, except for power laws, laws should be formulated in terms of dimensionless signal and response variables. 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.

During the first half of my ten years at the University of Pennsylvania (1959–69), 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. The two Stevens topics, like thorns, were hard to ignore.

Neural quantum theory (Békésy, 1930; Stevens, Morgan, and Volkman,

1941) and the theory of signal detectability (Green and Swets, 1966) are completely inconsistent in their formulation of threshold phenomena. Supporters of each theory had data they interpreted as rejecting the other view. My attack on the problem was two-pronged. First, I attempted to demonstrate that ROC data (plots of the probability of saying a signal was present when it was present 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. Krantz (1969) gave the matter its most careful discussion. What seems to be evident now is that although the two-state model is wrong, no reasonable amount of ROC data can distinguish between a few states and a continuum.

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. There could be no doubt that enormous biasing effects were possible; nevertheless, tantalizing hints of discrete underlying structure showed through in studies of W. D. Larkin and D. A. Norman, then graduate students. Perhaps the most difficult data for the continuous theories to encompass are the piece-wise linear ROC curves obtained using two-alternative, forced-choice procedures and different payoff matrices. Data to test some choice models, collected by Shipley in W. P. Tanner's laboratory, showed that subjects failed to discriminate signal frequency when they reported no signal present. This made sense from a threshold point of view (with appropriate response bias), but not from that of the theory of signal detectability. W. A. Lindner, working under the direction of James Egan, replicated the study and 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, because the issue had not even been formulated at the time her experiment was performed.

To this day, I believe that the question whether or not signal representations are discrete remains unresolved. Most psychophysicists have been convinced not, but I am not convinced their reasons are adequate to that conclusion.

Stevens's second thorn was the inability of any of the discrimination theories—those, as he used to say, that "process noise" or, as we who have worked on them said, 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 responses about the mean value.

Suchsoon Mo and I ran an experiment collecting weight-lifting data, resulting in four main findings. (1) Many of the mean magnitude functions exhibited systematic deviations from power functions; in the case of loudness some deviations have been as large as 5 dB. This has been repeatedly replicated. (2) The "exponents," although averaging to values near those reported by Stevens, exhibited considerable variation, from 0.15 to 0.34 for loudness versus physical energy. Again, this seems typical of later data, except that the top of the range is more like 0.6. (3) The typical distribution of responses, which is sharply peaked and has high tails, was not really fit well by any of the familiar distributions we tried. (4) The variability of the responses was appreciably greater than that obtained using discrimination techniques, such as a two-signal absolute identification design.

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

Response time is not always thought of as part of psychophysics, but it is an integral part of any decision process. Any psychophysical theory, such as signal detection and choice theory, that fails to account for the time it takes the subject to respond is surely incomplete. The mere fact that response times form a continuous random variable warmly recommends their close examination, because each observation is potentially a richer source of information than is the typical binary choice data. (For references to reaction time papers, see the bibliography of my book *Response Times* [*RT*] [1986].)

My first foray into reaction times, which occurred in the mid-1950's with Lee S. Christie, made two points. The first, well known to mathematicians and statisticians, but then apparently overlooked by psychologists, was that certain familiar integral transforms 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 was later demonstrated by W. J. McGill (1973) and by Green and Luce in several papers, including the general theory one, Luce and Green (1972). The second was to remark that it is not very easy to distinguish between serial and parallel systems using overall time. At the time this did not attract much attention, but in the 1970's James Townsend carried out a great deal of research on the issue, confirming in considerable detail that serial systems can mimic locally independent parallel ones but not the converse.

Work resumed when Eugene Galanter and I interested a student, Joan Gay Snodgrass, in the area. 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 subjects could indeed track the band, but that the variability was a U-shaped function suggesting there is a natural reaction time and that the band is tracked by the subjects' introducing, in some fashion, delays that add to the variability. We also found that although the variability could be made as small as a 25-msec interquartile range, there was no advantage in using a band payoff much narrower than 20 msec. In later work and using a somewhat different procedure, A. B. Kristofferson reduced the estimate of variability even more, to as little as about 10 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 to various well-known distribution functions 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. Weak signals are another matter.

A student of this same era, Robert T. Ollman, became interested in the speed-accuracy trade-off problem and developed and tested the fast-guess model, which was also independently developed by John Yellott, Jr. I was never taken by it conceptually and later worked out an alternative model with David M. Green. We provided data that showed the fast-guess model does not, by itself, account for responses to weak signals. I suspect that 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 behavior of last resort, in despair of complying with the instructions. Data of Richard Swensson and Ward Edwards strongly supported the idea of fast guesses, but with the new wrinkle that there are prolonged runs in the fast-guess mode alternating with runs in the attention mode. Also Donald Blough presented beautiful discrimination data for pigeons that clearly exhibited fast guessing.

Psychophysics, 1964–1981: Collaboration with David M. Green

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; the variability of magnitude estimates was not much understood; and I had failed to incorporate response times successfully 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.

The so-called method of free response, in which the signals to be detected are presented according to some haphazard temporal schedule with the subject free to respond whenever a signal seems to have occurred, appealed to me as being a far better idealization of natural detection problems than are the usual psychophysical procedures that delineate brief time periods during which a signal may or may not appear. However, the method of analysis then used, of treating the detection process as a sequence of fixed-interval yes-no decisions, did not appeal at all. The data consist of two interlaced time series—that of 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.

I worked out an idealized two-state, continuous-time model in which each signal presentation had some fixed probability of activating the detect state and the background had a temporally random (Poisson) tendency to do the same. David Green, who had come to the University of Pennsylvania, became interested in the model, and we decided to try to test it. 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 could have been completed. At first we attempted to model what might be going on, but the mathematical problems compounded until we decided it was better to change the experiment. Basically, there were two possibilities. One was to focus directly on the problem of how two temporally close signals interfere with each other; this, unbeknownst to us at the time, was the fruitful path followed by A. T. Welford, leading to his "single-channel" hypothesis (Section 5.4 of *RT*). The other, which we followed, was simply to rid ourselves of the interaction.

We wanted a design for which the onset of a signal is totally unpredictable while not having a second signal intervene before the response. This led us to a simple reaction-time design with random (i.e., 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. We worked out the two-state model, 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. A somewhat unusual data analysis, outlined in Section 3.2.4 of *RT*, showed the simplest model to be wrong and suggested the next approach.

This postulates that neural pulse rate serves as a surrogate for signal intensity, and the task for the brain is to estimate the local rate from small samples. Clearly, the greater the number of interpulse observations, the better the estimate. Equally clearly, larger samples from a single channel mean slower response times. The latter dictated some parallel acquisition from statistically independent channels. Our focus on rate estimates led to two distinct models: counting ones, in which the sample time is fixed, and timing ones, in which the sample size is fixed (Luce and Green, 1972). The former had been previously studied, but not the latter, which interested us because of their automatic account of the inverse relation between reaction times and signal intensity (the weaker the signal, the slower the pulse rate and hence the slower the decision time). However, they also led to the prediction that the yes-no ROC curves (in z-scores) should approximate straight lines with slopes considerably greater than one, unlike any data of the time. We were led to an auditory experiment, reported in Green and Luce (1973), which involved response deadlines. When the deadline applied to all trials, the ROC slopes were, as usual, less than one, agreeing with the counting model. Applied just to signal trials, however, the deadline led to slopes greater than one, agreeing with the timing model. Subsequently, Brian Wandell, a graduate student of mine at the University of California at Irvine, confirmed the finding in vision.

Further research with Wandell provided evidence that subjects aggregate information across neural channels by averaging rather than taking the maximum time. Further, Green and I observed that if timing is the natural mode of operation and assuming that the short duration signals of the usual psychophysical experiment invite counting, then one feature of laboratory training, leading to stable behavior, is reprogramming from timing to counting. Such reprogramming is slow.

One remarkable fact, not yet mentioned, is that all of our detection studies of acoustic intensity near threshold led to estimates of the Poisson rate parameter that grew approximately as a power function of intensity, with the exponent varying from 0.15 to 0.60 over subjects and averaging somewhere near, but below, 0.3. Stevens (1957) had shown this directly for the entire stimulus range by plotting average magnitude estimates versus signal intensity, and we found it to be true for individual subjects, especially if mean ratios of successive magnitude estimates are plotted against the corresponding signal ratios. 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.

Although the timing model gave a natural account of reaction times to weak signals, it was quickly shown not to be fully correct. As I mentioned earlier, the empirical distributions tend to be so peaked at the mode as compared to the rate of decay in their tails that it is impossible to fit them by any of the classic distributions, including those that arise from timing models. Stephen Burbeck, then a graduate student at Irvine, became challenged by this problem and came up with a plausible solution (Section 4.4 of *RT*). Reactions to weak signals are assumed to be triggered by a race between two independent processes, one having to do with perceived jumps or changes in the signal intensity, which is called a "change detector," and the other having to do with changed levels of activity, which is called a "level detector." The difference is that the change detector is sensitive to abrupt changes in the derivative of the wave form, whereas the level detector compares averages computed over successive periods of time. Functionally, the difference is that a change detector, when triggered, is fast; but, should it miss the change, as is possible with weak signals, it fails. The level detector is fundamentally more reliable, but at the expense of being much slower.

A particular transformation of the data known as the hazard function, first urged by McGill (1963) for use with response time data, is ideal for testing such a model because the hazard function of a race among several independent processes is simply the sum of the hazard functions of those processes. Reasonably strong evidence in favor of such dual detection was found. My guess is that the timing model, which is normally used for signal identification, is drawn into play in the pure detection situation and serves as the slower level detector. That results in the relatively long tails to the distribution. The basic change detector, which searches for rapid changes in the waveform, yields the highly peaked mode that is observed.

On my arrival at Harvard in 1976, Green and I gave a seminar on the use of time measures in psychophysics and later, from time to time, I gave it alone. Gradually, I learned more of the extensive literature, and my notes began to impose some structure on the material. In 1979, when thinking about what to do on my 1980–81 sabbatical, I decided to try to put it all together in a book, which became *Response Times*, completed in late 1984. During 1982–84 a number of seminars at the AT&T Bell Laboratories, organized by Saul Sternberg, critiqued drafts of chapters and led to substantial changes. The book is mostly a survey, with some original analyses and a plausible organization of a sprawling literature; it has received relatively kind reviews.

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

We quickly demonstrated that, in this simple form, both hypotheses are wrong. To improve the fit of the model to data, we next considered an "attention" hypothesis of the following sort: if a signal falls within an attention band, which we estimated to be about 10–15 dB wide for loudness, decisions are assumed to be based on a sample of interpulse intervals that is close to an order-of-magnitude larger than when the signal falls outside the band. This means that the standard deviation of the resulting estimates is smaller by a factor of about 3 (approximately $=\sqrt{10}$) when the signal is in the band than 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′ measure reported by Braida and Durlach (1972).

Increasingly, we became aware of the fact that both in magnitude estimation and in absolute identification there are very pronounced sequential effects. To ignore them is misleading. For example, in absolute identification, if one looks at the matrix of correlations between successive responses as a function of the signal pairs, it is found that roughly the same correlation obtains along the diagonals running from upper left to lower right. In other words, the correlation varies systematically with signal difference (measured in dB), being about 0.8 to 0.9 when the signal is repeated and dropping to zero or possibly a negative value when they are widely separated.

A second phenomenon is that the ratio of the standard deviation to the mean response to a signal as a function of the dB separation between that signal and the preceding one is decidedly V-shaped. Responses to a signal that is repeated are less variable than when the preceding signal is more distant.

A small theoretical and experimental literature has developed around these problems, but no really satisfactory model seems yet to have resulted. From the point of view of experimentalists, the situation is (or should be) deeply frustrating, because we do not know how to gain real control of the sources of these sequential effects. As a result, it is virtually impossible to draw any firm conclusions from the variability in magnitude estimation and absolute identification since any estimate of it is so thoroughly contaminated by sequential effects as to be meaningless. Much the same problem exists in using distributions of response times, as is summarized in Section 6.6 of *RT*. I am not sure how widely this dilemma about global psychophysical methods is fully appreciated.

At this point I found myself not working actively with an experimental group, and I lacked any new idea, so I stopped working on the problem.

Measurement, 1955–1972

For specific references, both to my work and that of others, in measurement, see the bibliographies of the three volumes of the *Foundations of Measurement* (*FM*) (Krantz, Luce, Suppes, and Tversky, 1971, 1989).

In contrast to my work on choice behavior and psychophysics, where the models are probabilistic, that in the foundations of measurement is algebraic. My training strongly favored this approach, and I have always found algebra more aesthetic 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 or ill understood, however, it may be best to begin with just the central tendency.

My first paper (1956) in the area was devoted to an axiomatization of an algebraic concept of threshold called "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 not transitive. Important later elaborations were made by Peter C. Fishburn and Fred S. Roberts.

The next contribution did not appear until my joint paper with John W. Tukey (1964), which was the lead paper in the newly founded (see below) *Journal of Mathematical Psychology*. That research began in the summer of 1961 at an informal seminar held in Tukey's study at CASBS, 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 axiomatized it. Given the later, much simpler and more revealing proofs of E. Holman and David H. Krantz (*FM I*, chapter 6) that neatly relate additive conjoint measurements to extensive measurement, it is surprising how tortured our first proofs were.

My next efforts concerned more realistic idealizations in which solvability is assumed only locally, both for the conjoint case and, with A. A. J. Marley, for the extensive one, including the bounded case (e.g., relativistic velocity). Krantz followed this up by producing a very useful local version of Hölder's theorem, which is one of the basic theorems employed in *FM I*. Improved versions of both the above papers are included in *FM I*.

During the early and middle 1960's, 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 J. Zinnes (Suppes and Zinnes, 1963), we increasingly felt the need for a systematic presentation and integration of the materials on measurement, which spanned a wide range of disciplines including economics, management science, mathematics, operations research, philosophy of science, physics, psychology, and statistics. As we outlined a book that could do this, 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 the University of Pennsylvania, where he worked with my friends and colleagues Leo Hurvich and Dorothea Jameson, and I had met Tversky, who was working on a dissertation under the late Clyde Coombs on finite conjoint measurement. We invited them to join the project. Our initial outline suggested a book of some twenty chapters, and so it has remained, despite the fact that early on it fissioned into two volumes and, in 1987, into three. (The additional two introductions raised the chapter count to 22, and some of the topics also changed over the years.) We titled it *Foundations of Measurement*. Although I had expected the work to be completed within a few years of the first volume, it was not finished until the end of 1987. The delay is discussed below.

Much of Volume I was completed during 1966–67, when I was again a Fellow at CASBS. A major task was to find the least number of mathematical results that describe basic algebraic structures which have additive numerical representations and from which we could derive all of the results in the additive theory of measurement. Ultimately, we showed that three theorems would do (*FM I*, chapter 2). This meant, however, 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 and improved many other theorems. My eight measurement papers during the late 1960's and early 1970's arose from this effort. 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. That has happened.

Chapter 10 on dimensional analysis was particularly troublesome. Dimensional analysis is a method whereby physicists, engineers, and biologists often can arrive at the form of a physical law simply by knowing exactly which variables are relevant—of course, that is a great deal to know. Since I first encountered the method in graduate school, it has fascinated and perplexed me. Although useful, the subject seemed conceptually slippery. Read carefully the introductory chapter to any book on the subject, and you soon realize that something mysterious is going on; only when you get to the applications does it begin to make sense. During that year at CASBS three long sessions of a Stanford-Berkeley seminar on measurement were devoted to Robert Causey's dissertation on physical similarity, which is a major aspect of dimensional analysis. Part of the reason these sessions ran long was my inability to understand exactly what was involved in the concept of dimensional invariance. During the following year Causey's paper was published, and we corresponded at length about it, until I finally got straight what I found objectionable.

Although part of the problem had been clarified, I came to realize, as I was drafting Chapter 10 in Rio de Janeiro, that there remained a major, apparently unremarked, lacuna. No one ever provided a serious 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 had 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 conjoint ones, written as products, and that some physical quantities, although not all, are measured both extensively and conjointly. Second, in physics, these two kinds of measures are always related by power transformations. I found a reasonably neat qualitative way of characterizing that transformation by what I called laws of similitude and of exchange, which relate the conjoint and extensive structures. Subsequent generalizations, involving a qualitative notion of the distribution of a measurement structure on a component of a conjoint one, have greatly improved those results (1978a, 1988, *FM III*, chapters 20 and 22).

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. Their failure to axiomatize the multiplicative decomposition was not for lack of technical power (O. Hölder, who axiomatized extensive operators, was an accomplished mathematician) but apparently lack of motivation to do so. 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.

Measurement, 1973–1988: Collaboration with Louis Narens

I break the discussion of measurement not at the time I left Irvine for Harvard, but at the time I began to collaborate with a brilliant younger colleague Louis Narens, who received his training under the late Abraham Robinson, the founder of nonstandard analysis. This collaboration, which still continues, has been one of the most fruitful of my career. I feel fortunate to have been able to work with someone whose mind is a marvelous mixture of creativeness, fantasy, philosophical demandingness, and mathematical power. Through his impact, I have done better work than I had done earlier. Some is summarized in Narens (1985) and some in chapters 19, 20, and 22 of *FM III*.

Most work on measurement, to this point, focused on structures with additive or averaging representations. The exceptions to this statement were isolated, and we understood little about the full range of qualitative structures with non-additive representations. The first question we tackled was to find fairly general conditions under which a general (non-associative) operation can be represented uniquely by some numerical operation other than +. It turned out that the solution to this problem pretty much provided the solution to the general question of representing a conjoint structure in terms of some function of scales on its two components. Our results, however, left it unclear how the different representations of the same structure relate to one another. For example, in the classical additive measures of physics, we do not simply say that a unique representation is singled out once a unit is assigned, but rather that the set of all representations forms a ratio scale in the sense that any two are related by a multiplicative constant. We could not say, at the time, how the several representations of non-associative operations were related.

After coming to Harvard in 1976, I encountered a maverick graduate student Michael A. Cohen, whose mathematical skills were just what was needed for these measurement problems. In a term paper for my seminar on measurement he came up with the, to me, surprising result that the family of transformations relating the representations was in fact very simple—namely, isomorphic to some subgroup of the positive numbers under multiplication. At first I didn't believe it, especially since the proof he turned in was, like his personal style, disheveled, but eventually I became convinced and put him in touch with Narens, who had also been working on the problem. Narens's approach was directed at seeing what happens when you impose a property to the effect that the family of transformations is rich in the same sense that a ratio scale is. Put another way, no element is distinguishable from the others solely by its behavior, a property called homogeneity. Coupling that restriction with Cohen's result led to a remarkably simple characterization of non-associative, ratio scale representations, one that I believe may prove useful in psychological theorizing. It was only fully worked out in Luce and Narens (1985) and further generalized in Luce (1988).

During our July 1980 collaboration, Narens became obsessed with the question of classifying the scale types of all measurement structures that can be represented by real numerical systems. He arrived at a partial solution, which included in it some of the ingredients for the general solution. That was achieved in 1984 by Theodore Alper, who became aware of the problem during my measurement seminar, in his senior mathematics thesis under the guidance of his advisor, Andrew Gleason.

Independent of that, Narens and I classified a broad class of structures with operations, and we worked out much of the theory for the two distinct types of structures that can arise—generalizing additive and averaging representations. In particular, this led to an interesting generalization of subjective expected utility that seems capable of dealing with several of the empirical difficulties that have been encountered.

Subsequently, Narens and I have pursued and solved a number of related questions, including what to make of the fact that there are a number of different notions of commensurability of measurement scales when an operation exists; how to generalize such concepts of commensurability when there is no operation; and what in that case is the generalization of the Cohen and Narens result about homogeneous operations. It turns out that in the homogeneous case, the situation is ever so much neater than we had any reason to expect, with everything fitting together in a beautiful fashion. Closely related is the general definition of distribution that is needed to put these structures together in a fashion suitable for dimensional analysis. Again, the result is very neat, but this is not the place to attempt a detailed exposition.

I was also able to establish that the elusive idea of dimensional invariance is just a special case of a general notion of meaningfulness, much like the one that arose in nineteenth century geometry and that S. S. Stevens (1951) raised in asking about how statistical practice should be affected by the scale type of the measure. Everyone who has thought about this at all agrees that we do not fully understand why we demand such invariance. Intuitively, one would like to say that something is meaningful in a structure provided it can be defined in terms of the primitives of that structure.

The problem is to formulate, in a philosophically well justified fashion, what exactly is meant by that. This, as one might expect, has turned out to be extremely elusive. For the past four or five years Narens has worked very hard on the problem, and he has many interesting results forming a large book manuscript, but the core problem remains unresolved as I revise (March 1988).

What exactly are measurement models good for? In my APA Distinguished Scientific Award address (Luce, 1972), 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? I later (Luce, 1985) 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 trade-offs among stimulus variables that yield, on 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. Falmagne (1985) illustrates this for psychophysical models. Increasingly, however, as we have uncovered the generalizations mentioned above, I have come to believe that the major significance is to lay out, as completely as we can, the possibilities for numerical measurement. This provides a chart for the behavioral and biological sciences of what is potentially possible by way of one-dimensional measurement and, in particular, of adjoining new ratio scale measures to the structure of units developed by the physical sciences. Whether we will be able to take advantage of the opportunities that are now understood remains to be seen—it is far too early to make a judgment—but at least we now know that additive operations are definitely not the end-all of measurement.

Persons, Places, and Events

MIT, 1950–1953

Within six months of my joining Bavelas's Small Groups Laboratory at MIT, he left to work on a classified project for the State Department and 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, and that had unfortunate consequences. I neither trusted my judgment sufficiently to oppose the momentum of the group on an expensive subproject, nor could I 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. 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 despite the heroic efforts of the late 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 brainchild of Bavelas, Oliver Strauss, J. C. R. Licklider, and Jerome Weisner (then associate director of the Research Laboratory for Electronics and later President of MIT). It was aptly named "Octopus."

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

Columbia University, 1953–1957

In the winter of 1952–53, I began to accept fully that the Group Networks Laboratory was going to fold and that another position was imperative. In the spring of 1953, I received an offer 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 hired me as managing director of the Behavioral Models Project, which 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 one of the imposing brownstone houses on 118th Street. We shared an ugly, dirty-green apartment with Fred Iklé, who largely ignored us and later ended up as a high official in the Department of Defense. We were mostly left in isolation except for occasional directives from Lazarsfeld, sometimes gruffly communicated by the official director of the project, Herbert 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, a former chemical engineer who was then a graduate student in sociology and is now a professor of sociology at Chicago, famous for, among other things, his report on educational interventions for culturally deprived groups.

Lazarsfeld, who was involved in founding the Center for Advanced Study in the Behavioral Sciences at Stanford, attended its opening year, 1954–55, and he arranged for me to be invited. His was a European view of the academic hierarchy: the more junior fellows should learn from the senior ones by assisting them. Fortunately, director Ralph Tyler and the Center board vetoed the idea of two classes of fellows, and the precedent has been maintained that each fellow decides exactly what he or she 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 of that book was completed the next year when I was back at Columbia, and Howard Raiffa was at the Center. I have always felt that we would never have written it had we been together, because it would have been too easy to talk.

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 an ebullient, outspoken Young Turk who outraged many experimentalists (who tend to be a rather conservative lot). But Galanter's quick, reactive mind was impressive to many others, including me. Although our styles were very different, we each found the 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 developoing for *ICB*. There is no doubt that our dialogues affected that book, were influential in my deciding to go to Pennsylvania, and continued to influence my work into the middle 1960's.

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 anointment of me as psychologist in lieu of a Ph.D. in the field.

Harvard University, 1957–1959

I went to Harvard on a five-year appointment as lecturer on social relations, but stayed only two years. Frederick Mosteller, of the Departments of Social Relations and Statistics, arranged the position. 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 the late 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 Robert R. Bush and Galanter, with whom I had a small grant from the American Philosophical Society, which permitted us to meet frequently on weekends.

In addition to Stevens's other influences, which I discussed above, 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, although it is now alleviated by an automated spelling checker; 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; MIT 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, helped in part by trying to analyze the writings of authors such as George A. Miller and in part by careful editing and rewriting by Stevens, to whom I shall always be indebted. I try to repay them by now and then rewriting passages of students' and colleagues' manuscripts.

At Harvard, there began a most satisfactory funding relationship with the National Science Foundation (NSF), which, except for my three years at the Institute for Advanced Study, has been continuous. 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.

University of Pennsylvania, 1959–1969

In 1956 and 1957 the chairmanship at the University of Pennsylvania Department of Psychology became open. On a train ride from New York to Cambridge to visit me, Bush and Galanter hatched the implausible—given that Bush was not a mainstream psychologist—idea of proposing him. Surprisingly he was made chairman in 1958, and I joined the department a year later as professor.

For the first time in my career, I held a senior position, 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 except for once being a rotating chairman (see below). 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, the main joint activity carried out with Bush and Galanter was the three-volume *Handbook of Mathematical Psychology* (*HBMP*) (Luce, Bush, and Galanter, 1963–65a) and the associated two volumes of *Readings in Mathematical Psychology* (Luce, Bush, and Galanter, 1963–65b). During this same period, I was involved in two other activities also designed to foster mathematical psychology. First, several of us active in the area (R. C. Atkinson, R. R. Bush, C. H. Coombs, W. K. Estes, W. J. McGill, G. A. Miller, P. Suppes, and I) founded the *Journal of Mathematical Psychology*. We did this largely as a response to our difficulties 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.

The second arose when the Social Science Research Council (SSRC) terminated its Committee on Mathematical Social Science. A number of us—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 social sciences other than economics and psychology. We persuaded NSF to fund the project (at a level of about $250,000 per year) with CASBS having fiscal responsibility and the newly created Mathematical Social Science Board having intellectual responsibility. Later, responsibility was transferred to the National Research Council (NRC) of the National Academy of Sciences (NAS). I was closely associated with this board over the years, as a member and twice as its chairman. Eventually this program was terminated, being seen as too elitist, in favor of ordinary peer-reviewed proposals, but that has not worked well. In my opinion, this has been a significant loss for mathematical social science because much in the way of energy and direction was achieved at these summer institutes. One recommendation of an NRC report (Gerstein, Luce, Smelser, and Sperlich, 1988) on prospects for the behavioral and social sciences is that such activities, in a variety of areas, be resumed, but with evaluation handled separately from individual research grants.

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.

Except for *HBMP*, 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 interests was not really large. After Bush resigned as chairman in 1964, my relationship with him waned, for we then 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.

Work with Galanter continued, 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 my final section.

One person with whom throughout my ten years at Pennsylvania I maintained a steady, largely luncheon-based friendship was the late Francis W. Irwin. He was 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. Many of our lunches included other people as well; I especially recall those with the exceedingly knowledgeable Richard L. Solomon and the vivacious biologist Vincent Dethier, who were 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.

Shortly after Galanter left Pennsylvania, David M. Green arrived. We ran our first free-response detection experiment just before he moved to the University of California at San Diego. This collaboration was productive and nearly ideal for about fifteen years, quite capable of withstanding the vicissitudes of many changes in location.

Another important relationship, that with Patrick Suppes, deepened about this same time. We had known one another for some time and had already collaborated on a chapter for *HBMP* and on two articles for the *Encyclopedia of the Social Sciences*, but our planning and work on *FM* drew us closer and we became personal friends. One reason I elected to spend my 1966–67 sabbatical as an NSF Senior Postdoctoral Fellow at CASBS, aside from its inherent quiet, beauty, and 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 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 ran a very large research establishment at Stanford, at times numbering more than 100 people; he has maintained worldwide speaking, administrative, and research commitments; and he founded and has led a substantial company, Computer Curriculum Corporation, which sells computer-based learning systems to elementary and high schools. I have never understood how he has withstood the onslaughts on his time and energy and maintained, into his mid-sixties, a youthful intensity and a jovial curiosity about all ideas. In any event, one has to be devious or persistent or both to get his attention. As *FM I* 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. Gleitman was very influental in arranging that I be honored the year following my Center stay by being made Benjamin Franklin Professor of Psychology, one of their six University Professors at the time. Since a named chair was in many ways ideal for me, especially with its minimal teaching obligations, it may seem odd that after spending the next year on leave, I left Pennsylvania in 1968. 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 there is a direct connection; here there is.

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 after that were seriously, and quite reasonably, influenced by her preferences, which include a passion 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 lived for several years before our marriage, was most satisfying. I agreed to try Brazil for a year to see how I reacted to it, and we spent 1968–69 in Rio de Janeiro, which although both large and noisy is mild; where we lived was free of smog because of ocean breezes and was relatively quiet because our apartment was at the end of a dead-end street. I was an Organization of American States Visiting Professor at the Universidad Católica de Rio de Janeiro, 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 Portuguese—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 problem 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 left for Brazil, Carl Kaysen, then 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. It turned out to be a form of purgatory. There was strenuous 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 attempted to force the appointment of a social scientist against a majority of permanent faculty; it was an ugly atmosphere, and it resulted ultimately in collecting a social science faculty far more humanist than scientific in orientation.

In spite of my enormous discomfort and frustration at my situation, I was able to complete work on *FM I*, to write a number of papers, and to maintain the research program with David Green. Moreover, I had enjoyable intellectual contacts with various social scientists who spent a year during that time—among them Robert Audley, Peter Fishburn, Tarow Indow, W. J. M. Levelt, David Rumelhart, and John Yellott Jr. Especially valuable to me were several informal seminars I ran on measurement and on information processing for psychologists in the area, which included the superb group at Bell Laboratories.

While at the Institute I received two very major honors: in 1970 one of the three annual Distinguished Scientific Awards of the American Psychological Association and in 1972 election to the NAS. Both led to new responsibilities. The APA subsequently appointed me to the Scientific Awards Committee for the period 1971–74. And the NAS almost immediately asked me to become a member of the fifteen-person Executive Committee of the newly formed Assembly of the Behavioral and Social Sciences (ABASS) of the NRC.

As my third year at the Institute began, I finally accepted fully 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 me. So I began to explore alternatives, especially ones in mild climates. Yellott, who had moved to the University of California at Irvine (UCI), arranged an attractive offer in the School of Social Sciences which I accepted.

University of California at Irvine, 1972–1975

The UCI campus was founded only in 1966, and the School of Social Sciences was the brainchild of its first dean James March, who favored both interdisciplinary opportunities for social science research and mathematical approaches to such problems, both of which are congenial to me. As a result of his initial direction, the School is composed of people who tend to have one or both of these traits; and, in an effort to promote unusual interactions, it was not then subdivided into departments. The School was controversial at UCI because of various factors—including the intellectual style of some faculty members, a strong intention not to mimic traditional structures, and the fact that much of its approach is not very appealing to average quality undergraduates—and so it was under some attack by the rest on campus. Furthermore, it had its own self-doubts. It was then in a period of unresolved crisis. After some political exploration, I concluded that shy of becoming dean there was little I could do to alter its path in the short run. In the ensuing years, it has gradually matured (if for no other reason than the aging of its initially very young faculty).

Aside from personal matters, by far the most important event of my experience at UCI was meeting Louis Narens, then an assistant professor. As a person, he can be unusual and, to some, he is disconcertingly intellectual and all-too-often oblique if not obscure. My experience is that, more often than not, the obscurities are ultimately transformed into remarkable theoretical insights. Working with him for the past twelve years has been the single most rewarding intellectual experience of my life. At times it has been taxing, for his standards are higher than most of us aspire to, but the rewards of discovery (outlined earlier) have been very great.

The period of UCI involved two outstandingly important personal events. First, in the fall just before I moved to California, my mother died, and my father, then 90, could not manage for long on his own. With surprisingly good spirits and adjustment for someone who seemed very set in his ways, he moved with us to California. He became my responsibility until his death in 1978. Second, in the fall of 1974 my second marriage, which had for some time been pretty ragged, collapsed. This was especially hard on my father and sad for me because Cynthia insisted on moving our daughter Aurora back to Brazil.

Once David Green, who had moved from San Diego to Harvard in 1973, became aware of my changed circumstance, he convinced his colleagues to recommend my appointment, which was approved. I accepted, and I made the unusual decision to come in January of 1976 because at my father's age a delay of six months could matter greatly. He accepted the change, though not happily, because he had found a congenial social life in his hotel in Laguna Beach, and the coldness, both of people and climate, in the Northeast, while familiar, did not please him.

In that last year in California I made a number of friends and found life at Irvine far more agreeable than it had been, so it was with some feeling of ambivalence that I left.

Harvard University, 1976–1988

Cambridge had always been a magnet for me, and I came very close to moving to Harvard in 1966. I did not accept that offer for two reasons, both of which now seem most inadequate. The one was that I had just moved in Philadelphia. The other was the incredibly aloof attitude of both the department members and the administration—something often said of Harvard. When the dean knowingly offered me $500 less than my Pennsylvania salary, I said to myself, "Who needs this?" So, there I was, a decade later, moving to Harvard, the department nearly as aloof as before but the dean, Henry Rosovsky, far more persuasive, among other things awarding me one of the IBM Alfred North Whitehead chairs. (Only later did I discover it was a five-year "folding chair," and for a couple of years in the early 1980's I was, as it were, without a seat until honored with the Victor S. Thomas Professorship of Psychology.)

Life in the department has been a mixed experience. Perhaps the most uncomfortable aspect was the gradual collapse of my collaboration with David Green. It was, no doubt, placed under considerable strain by Green's becoming chairman in 1978 for three years, which (no matter how efficient an administrator one is—and he is an exceedingly accomplished one) is a very consuming burden; by the tragic, agonizing illness and death of his first wife; and by my becoming chairman in 1981. But probably the most telling reasons were shifting intellectual interests and frustrations with the work. Had the phenomena proved more tractable, who knows? Another factor, no doubt, was my increasing attention to other matters: measurement theory, the NAS and NRC, and my book *Response Times*.

As was remarked earlier, *FM II* long remained unfinished while numerous other projects were completed—which, of course, was part of the problem. Four authors, each seriously over-committed and no one of them really feeling priority to complete it, is a recipe for delay. Another part of the problem was Krantz's view that nearly everything we had written could be appreciably improved, either in exposition or in substance or both. The problem was to effect the revisions. In an effort to overcome this dam, I took a leave of absence in 1984–85 to be at the AT&T Bell Laboratories, where he then was. Progress was made, especially on the chapters concerning the latest work on non-additive structures and scale types, but not as much as I had hoped for. Little progress was made during the next year, but in the fall of 1987 I returned for a third time as Fellow at CASBS, and Suppes and I forced completion of the project.

The National Academy of Sciences / National Research Council, 1972–1988

In 1976, after three years as a member, I became chairman of ABASS. Few of my activities—chairing meetings sometimes involving touchy problems and people, evaluating proposed projects, reading and criticizing draft reports, and appearing six times a year at meetings of the NRC Governing Board to defend ourselves from attacks by "hardheaded" engineers and physical scientists—are worth relating in any detail except for one observation. Once the hardheaded enter into areas of social implications of technology, which is true for most important NRC reports, a surprising number stop being rigorous and become quite softheaded. Suffice it to say that under the talented, if sometimes imperious, direction of David Goslin, its executive director, ABASS prospered and became a widely respected part of the NRC. My tenure with ABASS was climaxed in 1979 by one of the warmest send-off parties I've seen.

Probably the most exciting part of my chairmanship was two trips to the Soviet Union. The first, in 1976, was to Moscow, leading a group of about a dozen psychologists and half as many spouses to establish a seminar series in experimental psychology. Only two seminars occurred, one at UCI in 1978 on physiological psychology and the other the next year, on mathematical models of decision making, chaired by William K. Estes, in Tbilisi, Soviet Georgia. Both were nearly canceled over Soviet attempts to deviate from the agreement we had carefully and painstakingly crafted to avoid just such difficulties. In the case of the Irvine meeting they cabled us announcing last minute changes in several delegates, which was their all-too-common practice at the time. The NAS reply was simple: the group agreed upon or no meeting. The meeting was, in fact, relatively successful. For the Tbilisi meeting, two of our scientists were permanent resident non-citizens of the United States. We had raised this possibility in 1976 and, with considerable reluctance, the Soviets agreed to language permitting a few. Their clear preference was to restrict participants to citizens. They objected to one, a prominent Israeli citizen, but eventually a solution was found, and he received a visa. This meeting, while personally interesting because Soviet Georgia, being almost Mediterranean in quality, is so utterly different from Soviet Russia, was judged pretty much a failure by our delegation. The Soviets had little of interest to offer and seemed excessively reluctant to provide us with any real details of their work. The rest of the series was terminated when the NAS stopped all group arrangements with the Soviet Union as long as Sakharov was held in exile. He has been released, but the current NAS policy is to hold seminars only in fields where there is obvious parity in the level of research expertise; psychology is not one.

After that I was not terribly active for the NRC except for participating in the overall quality control of reports carried out by the Academy's Report Review Committee (RRC). Ultimately I began to wonder if my impression of being a capable committee chairman was all a private delusion, but that fear was dispelled when in 1983 I was appointed co-chair, with Neil J. Smelser, of the Committee on Basic Research in the Behavioral and Social Sciences. This standing committee had just accepted the tricky task of preparing an appraisal and outlook for basic research in these sciences. That report was released in March 1988, after a year of revisions (Gerstein, Luce, Smelser, and Sperlich, 1988). I am much too close to it to evaluate it dispassionately.

In addition to these NRC activities, I have served in various capacities in the NAS as chair of the Psychology Section and then chair of the Class of Behavioral and Social Sciences. These tasks are not especially important or exciting, but they do tend to get one involved in other committees: Nominating, Structure of the Academy, Bylaws, and the RRC. Beyond a doubt, the intellectually most interesting of these is the RRC, which is the oversight committee of Academy members who coordinate the reviews of important, controversial reports.

University of California, Irvine, 1988–

The academic year 1987–88 saw my third fellowship at the Center for Advanced Study in the Behaviorial Sciences, where Narens and I had organized a special project on measurement. It involved Jean-Claude Falmagne, Kenneth Manders, and de facto Mark Machina, an economist from the San Diego campus of the University of California much interested in utility theory, as well as numerous visitors for brief periods.

In November, Narens raised the question of submitting my name to UCI for the annual campus-wide Distinguished Professorship. I was willing for the following reason. When Green had introduced the Research and Training Group (RTG) structure in the department at Harvard, I had not recognized the profound effect it was going to have on resource allocation, and I erred in not creating an explicit RTG in mathematical psychology. Coupled with the fact that our small cadre of assistant professors was gradually being reduced, there was little opportunity to strengthen my areas of interest, and so I was contemplating early retirement and retreating to our country place in New Hampshire. The UCI plan, which as it unfolded involved the creation of an Irvine Research Unit (IRU) in Mathematical Behavioral Science and a 10-year period of faculty and student growth, seemed far more attractive than actual retirement. In January I was told that I would receive the Distinguished Professorship, and after a few months of the usual processing it was effected, the IRU was approved, and I was made director of it. At that point I did take early retirement from Harvard, but the next five to ten years promise not to be idle.

Musings

As the first draft of this spilled out of my typewriter (many years ago), 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, a RESEARCHER-teacher. To the extent feasible, I prefer to blur the roles. I am more at home in an advanced seminar or working individually with students 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. In recent years I came to violate this rule to the extent of giving a Harvard CORE course called Sound and Hearing. This was one of the basic science offerings, and it tended to be populated by students not in the sciences who, left to their own devices, would stay as far from science as possible. They didn't like it, and I refused to make it a "gut" course. It was fun for neither them nor me.

My greatest contribution to teaching is not as a classroom lecturer, but as an author. Howard Raiffa and I have "taught" tens of thousands about elementary game theory, and some of my other books—especially *HBMP*, the previously mentioned *Contemporary Developments in Mathematical Psychology* (Krantz, Atkinson, Luce, and Suppes, 1974), *FM*, *RT*, and the new edition of the *Stevens' Handbook of Experimental Psychology* (Atkinson, Herrnstein, Lindzey, and Luce, 1988)—and many expository articles were designed in part to instruct students and peers.

Collaboration of Experimentalists and Theorists

An applied mathematician doing psychological theory is always in danger of losing contact with empirical reality, and one must continually force oneself to consider the testability as well as the depth and generality of ideas. Otherwise, one is likely to become a pure mathematician of indifferent quality. One possible solution is also to run experiments. This solution is often urged by one's experimental colleagues; for example, Stevens was 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, Floyd Ratliff and Edward Pugh 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 problems facing the experimenter who, on his or her 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 none for theories—quite the contrary—but that each should work out the details of what he or she 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 such a level of complexity that I did not want to run my own laboratory.

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 have witnessed 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 were 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 to 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 rarely is 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 nonlinearities in the scales used. Similarly, one rarely cares whether there is a significant interaction term; one wants to know whether by suitable transformations it is possible or not to get rid of it altogether (e.g., 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 limited 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 have emphasized 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 introduction and later widespread availability of digital computers, and I have been repeatedly urged to involve myself deeply with them on the grounds that computers will or should be a theorist's main tool. 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. I am delighted with the power the computer gives us. Much of what Green and I did would not have been possible without such aid.

2. To simulate. For many stochastic processes that arise in psychology, there are no analytic expressions for statistical quantities of interest. One may then try to estimate these quantities by simulating the process. Although I have used simulations, most recently in *RT*, it is with reluctance. The method is cumbersome and can be expensive when sufficiently many parameters are involved; one can be easily misled because of sampling variability; and one always fears that some interesting region of the parameter space was missed.

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 laboratory which is largely electronic rather than mechanical. Care is needed, however, to avoid complex designs we are incapable of analyzing.

4. To process words, as I am now doing. Marvelous! It has eliminated for me almost all of the frustrations I once had with typewriters, revisions, and secretaries. And that can only improve. To be sure, there are new frustrations, like hard-disk crashes.

5. To teach. All sorts of teaching now involves computers, particularly when there are standard routines to be mastered as in elementary math-

ematics and statistics. In general this strikes me as a good thing. Systematic efforts toward better computer-assisted instruction, involving contingencies that depend on the progress of the student, have the potential for altering significantly the labor distribution in the teaching profession.

6. As a model 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 or a computer program is. For a time, attempts were made 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 synchronized 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 far more of an analog device than a digital one. Also, the physiological evidence suggests 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 yet developed, those appropriate to brain function may be among the missing, which convinces me that we psychologists should study the brain and behavior, not the computer. In the process some genius may invent—albeit, sloppily at first—some new mathematics which, conceivably, might lead to better computers.

7. To formulate psychological theories. Here is the focus of my dilemma. The proposal is that interesting psychological phenomena—language production, 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 abilty to solve problems that some human beings can solve (artificial intelligence) to far more detailed comparisons of step-by-step protocols.

With the advent of considerable computer power and much hard work, this approach is increasingly being more fully realized. As this section was written, Allen Newell was giving the 1987 William James Lectures at Harvard University, describing over the course of eight well-attended lectures both the philosophy and the realization of his current version of a universal psychological theory, called SOAR. He argued that this is the effective mode of encoding psychological theories—that computer programs, designed as knowledge systems, are replacing mathematics as the language of theory.

Two aspects of this approach have, all along, disturbed me sufficiently so that I have been unwilling to undertake the labor of pursuing it in my work. The first was the difficulty its proponents initially had in articulating clearly the psychological principles underlying the programs they write. I can no longer, however, make this charge, for Newell spent considerable time on such principles and on how he has arrived at them from a consideration of a wide range of empirical data. Still there is a problem. The principles are very general and correspondingly nonspecific in specific situations. Their realization seems not uniquely determined by the situation but to rest heavily on the intuitions of the person formulating the program. The second, closely related, point concerns the number of untested assumptions, functions, decision rules, and the like that, together, form a program. I know from my own work as well as from that of others how difficult isolating and testing simple, well-articulated principles and assumptions can be. A chastening example is the elaborate set of studies sparked by Sternberg's (1969) attempt to decide whether searches of short-term memory are 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?

Newell argues that the great mass of psychological data so constrain the theory that the problem is simply one of finding something that works, not to worry about whether it is correct. I find this somewhat unpersuasive, since the theory seems to be little more than a great "kludge" of numerous small theories, all structured in a similar fashion, but individually no more overdetermined than has been any previous, well-specified theory. If no systematic tests are possible on 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. Newell clearly feels it is decidedly a psychological theory.

So, despite some fear that I am missing a major intellectual development, rather than avoiding a fad, 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." When young I did, but experience 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 had hoped 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 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 three areas where mathematical modeling can be shown to have had a profound impact: the study of sensation and perception, psychological testing, and patterns of preferences. In the sensory area, the modeling appears to be cumulative, to have led to empirical discoveries, and to be essential to the ongoing life of the subject. In testing, 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. Studies of preference and motivation have resulted in a number of careful mathematical analyses (of which I have contributed several) followed by ingenious experimental studies that show difficulties. I believe the latter is an example of the initial interplay between theory and data that tends to sharpen both and also helps 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 precedes 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 stochastic operator and Markov models have, in my opinion, come to very little. True, models can be set up 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 the 1970's work shifted away from such models and experiments towards more schematic formulations of information processing and memory in which mathematics plays a decidedly auxiliary role. Recently, however, the modeling has become active and some developments are promising.

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. One area where such a separation is made very clearly is the modeling of the past ten years concerning schedules of reinforcement (Herrnstein, 1982).

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 human beings handle uncertainty, their calculus is different from probability. Or take memory and learning: can it be that the troubles we have had have 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? Recent work of the connectionist school is pursuing an alternative approach.

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, as I believe, this is the case, our time perspective had better be a long one, for we await a latterday Newton.

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