Intellectual Autobiography, Part I, 1922-1978
(Written in 1978)

I have divided this autobiography into three main parts: education, research, personal reflections. The second part on research is the longest and most substantial.

I. Education

I was born on March 17, 1922, in Tulsa, Oklahoma, and grew up as an only child; my half brother George was born in 1943 after I had entered the army. My grandfather, C. E. Suppes, moved to Oklahoma from Ohio in the early part of this century. He and my father were independent oil men, intelligent and competent in their business but not well educated. My mother died when I was four and a half; I was raised by my stepmother, who married my father before I was six. She also had not had much formal education, but her interest in. self-improvement was strong, and she encouraged me in a variety of ways to pursue my intellectual interests, in spite of my father's ambition for me to follow him and his father in the oil business.

My interest in philosophy was generated early by my stepmother's devotion for more than a decade to the Christian Science of Mary Baker Eddy. From about the age of eight to fourteen years I attended Sunday school and church regularly and studied the works of Eddy as well as the Bible. The naive epistemological idealism of Eddy's writings stirred my interest, which turned to skepticism by the age of thirteen or so. I can remember rather intense discussions with fellow Sunday-school students about how we were supposed to reconcile, for example, the bacterial theory of disease with the purely mentalistic views of Eddy. No doubt our arguments were not at all sophisticated, but our instinct to distrust the flagrant conflicts with common sense and elementary science was sound.

I attended the public schools in Tulsa and was graduated from Tulsa Central High School in 1939. My public school education was more influential on my development than is often the ease, mainly because I was a participant in what is known as the Tyler eight-year study of the Progressive Education Association. On the basis of examinations given in the sixth grade, able students were selected to participate in a six-year experiment of accelerated education. In many respects the most competitive and ablest classes I ever attended were those in high school. One of the important aspects of this special educational venture was the extended attempt to get us as young students to talk about a wide range of current events and everything else that interested us. As is often the case, this led into some unusual lines of effort. I can remember very well being chagrined at fourteen if I were not able to name the senators from every state in the union.

The high school courses in mathematics, chemistry and history were excellent, but physics and English were relatively mediocre. The English course was so dominated by the idiosyncrasies of the teacher we had for two years that we became experts in her life and tribulations rather than in the more essential matters of grammar and composition.

I began college at the University of Oklahoma in 1939 but, after the first year, found the intellectual life too pedestrian compared to the much more exciting high school years. In the second year I transferred to the University of Chicago, but, under the laissez-faire approach of Chicago, neglected my academic work so completely that my family insisted on my attending the University of Tulsa for my third year, where I majored in physics. At the beginning of my fourth year I was called up in the Army Reserves (this was 1942) and returned to the University of Chicago as a senior in uniform. I undertook there an intensive course in meteorology and received a BS degree from the University of Chicago in 1943. Knowledge of meteorology has stood me in good stead throughout the years in refuting arguments that attempt to draw some sharp distinction between the precision and perfection of the physical sciences and the vagueness and imprecision of the social sciences. Meteorology is in theory a part of physics, but in practice more like economies, especially in the handling of a vast flow of nonexperimental data.

From Chicago I was sent to the South Pacific for two years of duty in 1944 and 1945. After a short period of adjustment, I found the isolation and serenity of the Solomon Islands quite attractive. I occupied myself with swimming, poker, Aristotle, and a couple of correspondence courses in mathematics and French. After a year of living on a small island, occupied only by military troops, I was transferred to Guam, which seemed relatively civilized but less conducive to intellectual work.

I was discharged from the Army Air Force in 1946, and after some months of deciding what to do, changing my mind any number of times, and spending more than half a year working for my father in the oil fields near Artesia, New Mexico, I entered Columbia University as a graduate student in philosophy in January of 1947 and received a PhD in 1950.

As an undergraduate I moved too often to be strongly influenced by any one teacher. I do remember certain impressive individuals, including Richard McKeon at Chicago, who lectured on Aristotle, Norman Steenrod, who taught my first course in calculus, and Professor Tanner at the University of Tulsa, from whom I learned elementary Greek.

The situation was different in graduate school. I was influenced by Ernest Nagel more than by anyone else and I still relish the memory of the first lecture of his that I attended in 1947. I came to philosophy without much background in the subject, since my undergraduate training was primarily in mathematics and physics. But Nagel's skeptical, patient and detailed analysis of F. H. Bradley and John Dewey, in the first course I took from him, won my attention and interest from the start. I have recorded these impressions in more detail in my account of Nagel's lectures on Dewey (l969e). In those days, interdisciplinary work was very much favored in the Department of Philosophy, and consequently I continued to learn a good deal more mathematics and physics. I still remember with great pleasure the beautiful and penetrating lectures of Samuel Eilenberg on topology and group theory, and I remember well the course in relativity theory I took from L. H. Thomas. Thomas was a terrible lecturer, in sharp contrast to the brilliance of Nagel or Eilenberg, but he knew so much about the subject and was in such total control of the theory and relevant data that it was impossible not to learn a great deal and to be enormously impressed by the organization and presentation of the lectures.

In those years Columbia was swarming with returning veterans, and in certain ways we veterans had our own ideas about what should be taught in graduate school. We organized in 1947 or 1948 an informal seminar on von Neumann and Morgenstern's theory of games, partly because we did not seem to be able to find a course on the subject, but also because Columbia's graduate school immediately after the war was so vastly overcrowded that the kind of individual attention graduate students now receive at Stanford, for example, was simply unheard of in almost all departments at Columbia. I felt myself extremely lucky to have as much personal contact as I did with Ernest Nagel. Friends of mine in the History Department saw their adviser only a few hours during their entire graduate-school career, including completion of the dissertation.

Considering my relatively extensive research efforts in psychology from about 1955 onward, it is somewhat surprising that I took no work in psychology either as an undergraduate or as a graduate student, but there was a feature of my education that made it easier for me to pick up what I needed to know without prior systematic training. As an undergraduate I wandered about in several different fields, and because of the easygoing policy of the Department of Philosophy in those days at Columbia I spent a good deal of time in nonphilosophical courses. I thus developed early the habits of absorbing a wide variety of information and feeling at home in the problem of learning a subject in which I had not had much prior training or guidance.

Because of my background and interests in physics I wanted to write a dissertation about the philosophy of physics, in particular to give a modern axiomatic treatment of some branch of physics, but as I got deeper into the subject I realized that this was not the kind of dissertation that was considered appropriate in philosophy. The Department at that time was primarily historically oriented, and

Nagel advised me to consider a more informal approach to the foundations of physics. What we finally agreed on was a study of the concept of action at a distance, and: a good deal of the dissertation was devoted to an analytical study of this concept in the works of Descartes, Newton, Boscovich, and Kant. I was able to come closer to my original interest in a chapter on the special theory of relativity, but certainly what I had to say in that chapter was no contribution to the axiomatic foundations of the subject. I did find the historical work absorbing and have continued over the years to retain and, on occasion, profitably to use the knowledge about the history of philosophy and science I first systematically acquired as a graduate student at Columbia. The part of my dissertation I published, with only minor modifications, was the material on Descartes (1954a). But I have relatively recently used much more of the dissertation material in a long article on Aristotle’s theory of matter (l974b), in which I also review the theories of matter of Descartes, Boscovich, and Kant.

Earlier, but still long after the dissertation was written, I used some of the material on Kant in a Festschrift article for John Herman Randall, Jr. (1967h). Randall’s lectures in the history of philosophy were a great occasion at Columbia, and it was a pleasure to say something in detail about Kant’s philosophy of science as an extension of Randall’s interpretation in the Festschrift volume. Randall, like a lot of other philosophers mainly interested in the history of philosophy, did not have a strong analytical turn of mind, but his lectures were a memorable experience. As I said in the opening paragraph of my Festschrift article for Randall, "The wit, the literary quality, and the range of learning exhibited in these lectures were famous around Columbia long before the time of my own arrival. As a young scientist turned philosopher, the most important general thing I learned from Randall was not simply to read the great modern philosophers in terms of a close explication of text, but also to realize that they must be interpreted and considered against the background of the development of modern science." The contrast between Nagel’s analytical and dialectical skill and Randall’s sympathetic and impressionistic account of ideas was dramatic, but I can rightly claim that I learned a great deal from both of them although I was clearly more influenced by Nagel.

Another person whose intellectual habits influenced me at Columbia and who was of quite a different sort than any of the others I have mentioned was Paul Oskar Kristeller. He was interested in the fine details of historical scholarship, famous of course for his work in Renaissance philosophy, but I was more influenced by the seminar on Kant that he and Randall gave. Kristeller’s meticulous insistence on a textual basis for every interpretation suggested in the seminar, and his decisive way of handling the text, were a model of scholarship that I admired and learned from, even though Kristeller always modestly insisted that he was in no sense a Kantian specialist.

I received my PhD from Columbia in June of 1950 but my education scarcely stopped there. Although I began teaching upon arrival at Stanford in the fall of 1950, where I have been ever since, almost as soon as I could I also began to think about research in the philosophy of science. My problem was that I did not really know much about how to do any serious research and writing, since my graduate education did not involve the personal supervision of research of the kind so familiar to graduate students today. I was neither forced nor encouraged to produce early in my graduate career a publishable paper nor to because acquainted with the "how-to-do-it" aspects of research.

I was, however, full of energy and brimming over with ideas. I thrashed around for a few months but fortunately I soon became acquainted with J. C. C. McKinsey, a logician who had recently joined the Department of Philosophy at Stanford. McKinsey served as my postdoctoral tutor. It was from him that I learned the set-theoretical methods that have been my stock in trade for much of my career. It was not, however, just set-theoretical methods as such that MeKinsey taught me but also a passion for clarity that was unparalleled and had no precedent in my own prior education. I remember well his careful red-penciling of the initial draft I gave him of my first article on the theory of measurement (195la). McKinsey was just completing a book on the theory of games and consequently also had some of the interests that were of importance to me as my own interests in the social sciences began to blossom.

But there were other people, at Stanford and Berkeley who continued my education in those early years at Stanford. With McKinsey's encouragement I attended Alfred Tarski's seminar in Berkeley. McKinsey always claimed that he had learned everything he knew from Tarski. This was not true, but after attending Tarski's seminar I understood why he liked to say this. Tarski was a ruthless taskmaster. I think he probably got the most out of his students of anyone I have known. His seminar provided perhaps the best example I know of vicarious learning. A student who made a poorly prepared seminar presentation was so ruthlessly and mercilessly questioned that the other students did not need any hints about the state of preparation they should achieve before making their own presentations. Tarski, as one of the great examples of the Polish school of logic, was unwilling to go forward on a single point unless everything covered thus far was completely clear—in particular, unless it was apparent to him that the set-theoretical framework within which the discourse was operating could be made totally explicit. It was from McKinsey and Tarski that I learned about the axiomatic method and what it means to give a set-theoretical analysis of a subject.

McKinsey died in 1953, and our collaborative work on the axiomatic foundations of empirical sciences was brought to a sudden stop. Another learning experience that influenced much of my later work was the summer research position I had early in the fifties for a couple of years with David Blackwell and M. A. Girshick while they were writing their influential book Theory of Games and Statistical Decisions (1954). As I remark later, I did not learn as much from them as I should have, but what I did absorb played an important role in a number of my early papers.

In later years I have learned a great deal from many persons who are mentioned below. Rather arbitrarily I have terminated this section on my education as of about 1954, but it seems important to mention those who have taught me much of what I know about psychology because, although my allegiance to philosophy had continued strong throughout all these years, for a considerable period I published more papers in psychology than in philosophy. Much of this work was due to the influence of William K. Estes, R. Duncan Luce, and Richard C. Atkinson. My joint work with each of them is discussed later at appropriate points.

II. Research

I have grouped the discussion of my research under seven headings: foundations of physics; theory of measurement; decision theory, foundations of probability, and causality; foundations of psychology; philosophy of language; education and computers; and philosophy and science. Where details warrant further organization, appropriate subheadings are also introduced. There is some arbitrariness in the classification, but I do not think any serious confusions will arise. The number of topics on which I have worked is large and it would be foolish to claim that I have contributed in a fundamental way to all of them. On the other hand, each of the main headings represents an area that has been the focus of my interests at some period over the past several decades.

Foundations of Physics

As already mentioned, my doctoral dissertation lay within the philosophy of physics. In particular, I studied the problem of action at a distance as it had occurred in 17th- and 18th-century physics and philosophy, especially in the writings of Descartes, Newton, Boscovich, and Kant. The final chapter dealt with the problem in the special theory of relativity. Working on it strengthened my earlier desire to give an axiomatic formulation of classical mechanics in the spirit of modern mathematics rather than ’physical’ axiomatizations common in physics. Serious joint work on this project began soon after my arrival at Stanford, in collaboration with J. C. C. McKinsey, and is represented in four papers we wrote on the foundations of physics prior to McKinsey’s death in 1953 (1953a, 1953b, l953c also with A. C. Sugar, and 1955b). Shortly thereafter I wrote with Herman Rubin a similar paper (l954c) on the axiomatic foundations of relativistic particle mechanics. It is a long and very complicated piece of work that has not been read, I suspect, by very many people.
My main interests soon turned to research on decision theory and related problems in the foundations of psychology but I have

continued through the years to have an interest in physics, and an irregularly spaced sequence of papers has reflected that interest.

Special Relativity

In 1957 I organized jointly with Leon Henkin and Alfred Tarski a symposium on the axiomatic method with special reference to geometry and physics. Working with Henkin and Tarski on the organization of this symposium was an exhilarating experience. My own paper (1959a) was concerned with the derivation of the Lorentz transformations of special relativity from an explicit but minimal set of assumptions. Essentially, the aim of the paper was to give an elementary derivation of the Lorentz transformations, without any assumptions of continuity or linearity, from a single axiom concerning invariance of the relativistic distance between any two space-time points connected by an inertial path or, put another way, from the assumption of invariance of segments of inertial paths. Important new results on the derivation of the Lorentz transformations have since been published by Walter Noll (1964), E. C. Zeeman (1964), and others.

The elegant aspect of Noll's paper is that he axiomatizes Minkowskian chronometry using coordinate-free methods. More importantly, Zeeman shows that it is not necessary to assume invariance of time-like intervals as I did, but that it is sufficient to assume the preservation of. order, that is, the relativistic partial ordering of one point being after another is sufficient. Like many simple and beautiful ideas, it is surprising that this did not occur to someone sooner. The key to the results is already present in the early work of Robb (1936), which shows that the binary relation of being after is a sufficient conceptual basis for the kinematical theory of special relativity.

In the final section of my 1957 paper (1959a) I discuss the possibility of introducing a relation of signaling in order to fix the direction of time. It is obvious that this can be done very directly in an *ad hoc* fashion. What is needed, however, is some natural approach that is fully satisfying from an intuitive and a conceptual standpoint. In his article, Noll makes some remarks about this, and he raises the question of whether his approach solves the problem I raised. Essentially, Noll introduces a directed signal relation that is asymmetric, and of course if we postulate that the numerical representation must preserve the direction of signals passing from earlier to later events, the direction of time is guaranteed. I find this approach unsatisfactory since this is an arbitrary stipulation in the definition of isomorphisrn, and we get just as good an isomorphism from a structural standpoint if the direction in time is reversed. At the time it did not seem feasible to give such an analysis, but recently I have been rethinking and, more importantly, learning a great deal more about optics. It now seems to me that natural qualitative postulates differentiating signals being received and being sent should be feasible, although such postulates must go beyond the purely kinematic aspects of the special theory of relativity to include some substantive assumptions, even if of a general nature, about physical optics.

After a gap of some years, my interest in classical physics (in which I include special relativity) was revived while working on Chapter 10 of Foundations of Measurement (1971a). This chapter is concerned with dimensional analysis and numerical laws. I think I made some contributions to the chapter, but almost surely I learned more than I contributed, especially from Duncan Luce. The rather technical material in this chapter on the algebra of physical quantities and on dimensional analysis, including the question of why numerical laws are dimensionally invariant, has not been much picked up by philosophers of science, but I think that in due time it will be.

Quantum Mechanics

Most of the effort that I have put in on the foundations of physics since 1960 has been devoted to quantum mechanics, and this continues to be a current active intellectual interest. Almost everything that I have written about quantum mechanics has been intertwined with questions related to the foundations of probability, especially as to how probabilistic concepts are used in quantum mechanics. My first paper on the subject (1961c) was concerned with the absence of a joint distribution of position and momentum in many standard cases. I shall not enter into the technical details of the argument here, but I do want to convey the basic philosophical point that I continue to find the real puzzle of quantum mechanics. Not the move away from classical determinism, but the ways in which the standard versions seem to lie outside the almost universal methodology of modern probability theory and mathematical statistics. For me it is in this arena that the real puzzles of quantum mechanics are to be found. I am philosophically willing to violate classical physical principles without too many qualms, but when it comes to moving away from the broad conceptual and formal framework of modern probability theory I am at once uneasy. My historical view of the situation is that if probability theory had been developed to anything like its current sophisticated state at the time the basic work on quantum mechanics was done in the twenties, then a very different sort of theory would have been formulated.

It is worth recording a couple of impressions about this because they indicate the kind of changes that can take place in one's attitudes as the years go by. Initially I was much impressed by the mathematical formulation of foundations given by von Neumann in his classical work and, later, by Mackey (1963), whose book has also become classical in its own way. No doubt I was originally struck by the mathematical clarity and sophistication of this work, but in later years I have become dissatisfied with the unsatisfactory conceptual basis from a probabilistic standpoint of the way in which the theory is formulated. I shall give here just two examples to indicate the nature of my conceptual dissatisfaction. Von Neumann stresses that we can take the expectation of the sum of any two operators, even though they are conjugate, that is, do not commute. But once this is said, the natural question is to ask about the underlying probability space that justifies the exact probabilistic meaning of the expectation. A similar question arises with respect to Mackey's treatment. Mackey takes as fundamental the concept of the probability that a measurement in a given state of an observable will lead to a given value. This seems innocent enough, but when the fundamental postulates of the theory are stated in these terms, what seems missing from what one would expect in a standard causal physical theory is any clarity about the relation between observables. The axioms he gives would seem to concentrate too deeply on the relatively simple properties of the probability of a given measurement on a given observable and not enough on the causal dependencies between observables. (It is important to remember that I am not really making a technical argument here but trying to give the intuitions back of arguments that I think can be formalized.)

A detailed analysis of the kinds of requirements that a satisfactory probabilistic theory of quantum mechanical phenomena should have is laid out for the simple case of the harmonic oscillator in a relatively recent paper by Zanotti and me (1976m). As I write this autobiography I am struggling to develop in a much deeper way than I have previously a thoroughgoing probabilistic interpretation of interference phenomena in quantum mechanics, especially as produced by the classical two-slit experiment.

Until recently I thought that the most important philosophical problems of quantum mechanics could be analyzed in the nonrelativistic framework characteristic of the early development of the subject. An effort to understand the place of probability concepts in relativistic quantum mechanics has abruptly changed my mind. The meshing of probability and relativity seems to be badly worked out for even the most elementary problems. One sign of the difficulty is the superficiality of the development of probabilistic concepts in relativistic quantum mechanics. An extensive search of the literature, for example, has not revealed a single discussion of particular distributions. The multivariate normal distribution is invariant under linear transformations, and the Lorentz transformations are linear, but the proper space-time hyperplane on which the distribution is defined needs to be correctly chosen to be Lorentz invariant as well. As far as I know, discussion of these 'first' questions does not yet exist, which I find very surprising.

Although I continue to be fascinated by the conceptual problems of quantum mechanics and I think of it almost as a responsibility of any philosopher of science with wide interests to know a good many of the details of what is surely the most important scientific theory of the 20th century, I find that my own work here is less satisfying to me than other areas I discuss later, just because I do not anticipate making a scientific contribution to the subject. In the case of the theory of measurement or psychology, for example, my contributions have as much a scientific as a philosophical flavor, and I find that this suits my temperament betterÑindependent of whether the scientific contribution is of greater or less significance than the philosophical one.

Theory of Measurement

In my first published article (1951a) I gave a set of independent axioms for extensive quantities in the tradition of earlier work by Hölder and Nagel. My contribution was primarily to weaken the assumptions of Hölder axioms and also to prove that both the axioms and the concepts used were independent. Looking around for other topics in measurement, and returning to the earlier interest in the theory of games and utility theory, it soon became apparent that there were more outstanding problems of measurement in psychology than in physics. One of my first efforts in this direction was a joint article with my student Muriel Winet (1955d). We gave an axiomatization of utility based on the notion of utility differences. The idea of considering such utility differences is a very old one in the literature, but an explicit and adequate set of axioms had not previously appeared. In 1956 I published two other articles which fell between decision theory and measurement theory. One was on the role of subjective probability and utility in decision making. In this article (1956b) I used the results of the joint work with Winet to provide an axiomatization alternative to that given by Savage in his book Foundations of Statistics (1954). And in the second article, my colleague Donald Davidson and I gave a finitistic axiomatization of subjective probability and utility (1956c).

Shortly after this I began to think more generally about the foundational aspects of theories of measurement and was fortunate to have as a collaborator the logician and mathematician Dana Scott, who was at that time a graduate student in mathematics. (Scott is also one of the Berkeley-Stanford persons from whom I learned a great deal, beginning when he was an undergraduate in a course on the philosophy of science I taught at Berkeley in 1952, along with Richard Montague. What a pair to have in such a course!) Scott and I tried to give a general framework for theories of measurement and to obtain some specific results about axiomatization. This article was published in 1958, a year or so after it was written. The framework that Scott and I set up has, I think, been of use in the literature, and probably the article with him has been the most important article in the theory of measurement that I have written, although the chapter in the *Handbook of Mathematical Psychology*, written with J. L. Zinnes and published in 1963, has perhaps been more influential, especially in psychology.

My most important recent effort has been the extensive collaboration with David Krantz, Duncan Luce and Amos Tversky in the writing of our two-volume treatise *Foundations of Measurement* , the first volume of which appeared in 1971. At the time of writing this autobiography, we are hard at work on Volume II. My present feeling is that when Volume II is published I shall be happy to let the theory of measurement lie fallow for several years. It is, however, an area of peculiar fascination for a methodologist and philosopher of science like myself. The solution of any one problem seems immediately to generate in a natural way several more new problems. The theory nicely combines a demand for formally correct and explicit results with the continual pursuit of analyses that are pertinent to experimental or empirical procedures in a variety of sciences but especially in psychology, where the controversy about the independent measurability of psychological concepts has been long and intense. The theory of measurement provides an excellent example of an area in which real progress has been made in the foundations of psychology. In earlier decades psychologists accepted the mistaken beliefs of physicists like Norman Campbell that fundamental measurement in psychology was impossible. Although Campbell had some wise things to say about experimental methods in physics, he seemed to have only a rather dim grasp of elementary formal methods, and his work in measurement suffered accordingly. Moreover, he did not even have the rudimentary scholarship to be aware of the important earlier work of Helmholtz, Hölder, and others.

The work of a number of people over the past several decades has led to a relatively sophisticated view of the foundations of measurement in psychology, and it seems unlikely that any substantial retreat from this solid foundation will take place in the future. I am somewhat disappointed that the theory of measurement has not been of greater interest to a wider range of philosophers of science. In many ways it is a natural topic for the philosophy of science because it does not require extensive incursions into the particular technical aspects of any one science but raises methodological issues that are common to many different disciplines. On the other hand, by now the subject has become an almost autonomous technical discipline, and it takes some effort to stay abreast of the extensive research literature.

Although important contributions to the theory of measurement have already appeared since we published Volume I of *Foundations of Measurement* , I do think it will remain as a substantial reference work in the subject for several years. What is perhaps most important is that we were able to do a fairly good job of unifying a variety of past results and thereby providing a general framework for future development or the theory.

Having mentioned the seminars of Tarski earlier. I cannot forbear mentioning that perhaps the best seminar, from my own personal standpoint, that I ever participated in was an intensive one on measurement held jointly between Berkeley and Stanford more than ten years ago when Duncan Luce was spending a year at the Center for Advanced Study in the Behavioral Sciences at Stanford. In addition to Luce and me, active participants were Ernest Adams, who is now Professor of Philosophy at Berkeley and was in the fifties my first PhD student, and Fred Roberts, who was at that time a graduate student in mathematics at Stanford and is now a member of the Department of Mathematics at Rutgers University. William Craig also participated on occasion and had penetrating things to say even though he was not as deeply immersed in the subject as were the rest of us. Our intensive discussions would often last well beyond the normal two hours, and it would not be easy to summarize all that I learned in the course of the year.

There is also a pedagogical point about the theory of measurement, related to what I have said just above about measurement in the philosophy of science, that I want to mention. The mathematics required for elementary examples in the theory of measurement is not demanding, and yet significant and precise results in the form of representation theorems can be obtained. I gave several such examples in my textbook in logic (1957a) and also in my paper 'Finite Equal-interval Measurement Structures' (1972d). I continue to proselytize for the theory of measurement as an excellent source of precise but elementary methodology to introduce students to systematic philosophy of science.

Decision Theory, Foundations of Probability, and Causality

Decision Theory

It is not easy to disentangle measurement theory and decision theory because the measurement of subjective probability and utility has been such a central part of decision theory. The separation that I make will therefore be somewhat arbitrary. My really serious interest in psychology began with experimental research on decision theory in collaboration with my philosophical colleague Donald Davidson and a graduate student in psychology at that time, Sidney Siegel. Davidson and I had begun collaborative work with McKinsey in 1953 on the theory of value and also on utility theory. We continued this work after McKinsey's death, and it is reflected in Davidson, McKinsey, and Suppes (1955a) and in the joint article with Davidson (1956c) on the finitistic axiomatization of subjective probability and utility, already mentioned. The article on the measurement of utility based on utility differences, with Muriel Winet, was also part of this effort.

Sometime during the year 1954, Davidson and I undertook, with the collaboration of Siegel, an experimental investigation of the measurement of utility and subjective probability. Our objective was to provide an explicit methodology for separating the measurement of the two and at the same time to obtain conceptually interesting results about the character of individual utility and probability functions. This was my first experimental work and consequently in a genuine sense my first real introduction to psychology. The earlier papers on the foundations of decision theory concerned with formal problems of measurement were a natural and simple extension of my work in the axiomatic foundations of physics. Undertaking experimental work was quite another matter. I can still remember our many quandaries in deciding how to begin, and seeking the advice of several people, especially our colleagues in the Department of Psychology at Stanford.

I continued a program of experimentation in decision theory as exemplified in the joint work with Halsey Royden and Karol Walsh (1959i) and the development of a nonlinear model for the experimental measurement of utility with Walsh (1959j). This interest continued into the sixties with an article (1960g) on open problems in the foundations of subjective probability. Then in 1961 I drew upon my interest in learning theory to try to create a behavioristic foundation for utility theory (1961a), and I also made an attempt in that same year to explain the relevance of decision theory to philosophy (196lb).

The most important effort in this period was the writing with Duncan Luce of a long chapter, ‘Preference, Utility and Subjective Probability’ (1965i), for Volume III of the *Handbook of Mathematical Psychology*. The organization of a large amount of material and the extensive interaction with Luce in the writing of this chapter taught me a great deal that I did not know about the subject, and I think the chapter itself has been useful for other people. It is also worth mentioning that large parts of the joint effort with Krantz, Luce and Tversky in writing our two-volume treatise on the foundations of measurement have been concerned with decision theory.

In the latter part of the sixties I wrote several articles in the foundations of decision theory, oriented more toward philosophy than psychology. Three of the articles appeared in a book on inductive logic edited jointly with Jaakko Hintikka, my part-time philosophical colleague at Stanford for many years.

One article dealt with probabilistic inference and the concept of total evidence (l966j). Here I advanced the argument that under a Bayesian conception of belief and decision there was no additional problem of total evidence, contrary to the view held by Carnap and also Hempel. According to this Bayesian view, which I continue to believe is essentially right on this matter, if a person is asked for the probability of an event at a given time, it will follow from the conditions of coherence on all of his beliefs at that time that the probability he assigns to the event automatically takes into account the total evidence that he believes has relevance to the occurrence of the event. The way in which total evidence is brought in is simple and straightforward; it is just a consequence of the elementary theorem on total probability.

A second article in the volume (1966e) set forth a Bayesian approach to the paradoxes of confirmation made famous by Hempel many years ago. I will not outline my solution here but much of the philosophical literature on the paradoxes of confirmation has taken insufficient account of the natural Bayesian solution, at least so I continue to think. A third article in the volume (1966f) dealt with concept formation and Bayesian decisions. Here I attempted to set forth the close relations between formal aspects of the psychology of concept formation and the theory of Bayesian decisions. I now think that the ideas I set forth here are the least interesting and the most transitory of those occurring in the three articles. The general idea of value in this article concerns the relation expressed between concept formation and the classical problem of induction. For those restricted settings in which no new concepts are needed but for which an induction about properties is required, a Bayesian approach is sound and can meet most, if not all, of the conceptual problems about induction that I regard as serious. On the other hand, a Bayesian viewpoint toward induction does not provide a general solution because it does not incorporate a theory of concept formation. Genuinely new inductive knowledge about the world requires not only a framework of inductive inference of the sort well worked out in the contemporary Bayesian literature but also a theory about how new concepts are to be generated and how their applicability is to be dealt with. This large and significant aspect of the general problem of induction seems to me still to be in a quite unsatisfactory state. In my own thinking, the problem of induction and the concept of rationality are closely tied together, and as I point out in the article on probabilistic inference mentioned above, the Bayesian approach still provides a very thin view of rationality, because the methods for changing belief as reflected in the introduction of new concepts or in the focus of attention are not at all adequately handled. The outlines of any future theory that will deal in even a partially satisfactory way with the central problem of concept formation are not at all visible, and it may even be that the hope for a theory that approaches completeness is mistaken.

Distributive Justice

For a variety of reasons, the literature on decision theory has been interwined with the literature on social choice theory for a very long period, but the focus of the two literatures is rather different and I have certainly had more to say about decision theory than about the normative problems of social choice or distributive justice. To a large extent, this is an accident of where I have happened to have had some ideas to develop and not a matter of a priori choice. I have published two papers on distributive justice (19661, 1977a). The main results about justice in the first one, which were stated only for two persons, were nicely generalized by Amartya Sen (1970). The other paper, which was just recently published, looks for arguments to defend unequal distributions of income. I am as suspicious of simplistic arguments that lead to a uniform distribution of income as I am of the use of the principle of indifference in the theory of beliefs to justify a uniform prior distribution. The arguments are too simple and practices in the real world are too different. A classical economic argument to justify inequality of income is productivity, but in all societies and economic subgroups throughout the world differences in income cannot be justified purely by claims about productivity. Perhaps the most universal principle also at work is one of seniority. Given the ubiquitous character of the preferential status arising from seniority in the form of income and other rewards, it is surprising how little conceptual effort seems to have been addressed to the formulation of principles that justify such universal practices. I do not pretend to have the answer but I believe that a proper analysis will lead deeper into psychological principles of satisfaction than has been the case with most principles of justice that have been advanced. I take it as a psychological fact that privileges of seniority will continue even in the utopia of tomorrow and I conjecture that the general psychological basis of seniority considerations is the felt need for change. A wide range of investigations demonstrate the desirable nature of change itself as a feature of biological life (not just of humans) that has not been deeply enough recognized in standard theories of justice or of the good and the beautiful.

Foundations of Probability

The ancient Greek view was that time is cyclic rather than linear in character. I hold the same view about my own pattern of research. One of my more recent articles (1974g) is concerned with approximations yielding upper and lower probabilities in the measurement of partial belief. The formal theory of such upper and lower probabilities in qualitative terms is very similar to the framework for extensive quantities developed in my first paper in 1951. In retrospect, it is hard to understand why I did not see the simple qualitative analysis given in the 1974 paper at the time I posed a rather similar problem in the 1951 paper. The intuitive idea is completely simple and straightforward: A set of 'perfect' standard scales is introduced, and then the measurement of any other event or object (event in the case of probability, object in the case of mass) is made using standard scales just as we do in the ordinary use of an equal-arm balance. This is not the only occasion in which I have either not seen an obvious and simple approach to a subject until years later, or have in fact missed it entirely until it was done by someone else.

On the other hand, what would appear to be the rather trivial problem of generalizing this same approach to expectations or expected utility immediately encounters difficulties. The source of the difficulty is that in the case of expectations we move from the relatively simple properties of subadditive and superadditive upper and lower measures to multiplicative problems as in the characteristic expression for expected utility in which utilities and probabilities are multiplied and then added. The multiplicative generalization does not work well. It is easy to give a simple counterexample to straightforward generalization of the results for upper and lower probabilities, and this is done in Suppes (l975a). I have continued to try to understand better the many puzzles generated by the theory of upper and lower probabilities, in joint research with Mario Zanotti (1977j).

Partly as a by-product of our extensive discussions of the qualitative theory of upper and lower probabilities, Zanotti and I (1976n) used results in the theory of extensive measurement to obtain what I think are rather elegant necessary and sufficient conditions for the existence of a probability measure that strictly agrees with a qualitative ordering of probability judgments. I shall not try to describe the exact results here but mention the device used that is of some general conceptual interest.

Over the years there have been a large number of papers by many different individuals on these matters. Essentially all of them have formulated conditions in terms of events, with the underlying structure being that of the Boolean algebra of events and the ordering relation being a binary relation of one event being at least as probable as another. The conditions have turned out not to be simple. The important aspect of the paper with Zanotti is our recognition that events are the wrong objects to order. To each event there is a corresponding indicator function for that event, with the indicator function having the value one when a possible outcome lies in the event and the outcome zero otherwise—as is apparent, in this standard formulation events are sets of possible outcomes, that is, sets of points in the probability space. We obtain what Zanotti and I have baptized as extended indicator functions by closing the set of indicator functions under the operation of functional addition. Using results already known in the theory of extensive measurement it is then easy to give quite simple necessary and sufficient axioms on the ordering of extended indicator functions to obtain a numerical probability representation.

Recently we have found correspondingly simple necessary and sufficient qualitative axioms for conditional probability. The qualitative formulations of this theory beginning with the early work of B. O. Koopman (1940a, l940b) have been especially complex. We have been able drastically to simplify the axioms by using not only extended indicator functions, but the restriction of such functions to a given event to express conditionalization. In the ordinary logic of events, when we have a conditional probability P(A|B), there is no conditional event A|B, and thus it is not possible to define operations on conditional or restricted events. However, if we replace the event A by its indicator function Ac, then Ac|B is just the indicator function restricted to the set B, and we can express in a simple and natural way the operation of function addition of two such partial functions having the same domain. The analysis of conditional probability requires considerably more deviation from the theory of extensive measurement than does the unconditional case: for example, addition as just indicated is partial rather than total. More importantly, a way has to be found to express the conceptual content of the theorem on total probability. The solution to this problem is the most interesting aspect of the axiomatization.

The move from events to extended indicator functions is especially interesting philosophically, because the choice of the right objects to consider in formulating a given theory is, more often than I originally thought, mistaken in first efforts and, as these first efforts become crystallized and familiar, difficult to move away from.

Apart from technical matters of formulation and axiomatic niceties, there are, it seems to me, three fundamental concepts underlying probability theory. One is the addition of probabilities for mutually exclusive events, the second is the concept of independence of events or random variables, and the third is the concept of randomness. I have not said much here about either independence or randomness. A conceptually adequate formulation of the foundations of probability should deal with both of these concepts in a transparent and intuitively satisfactory way. For any serious applications there is a fourth notion of equal importance. This is the notion of conditionalization, or the appropriate conceptual method for absorbing new information and changing the given probabilities. I have ideas, some of which are surely wrong, about how to deal with these matters and hope to be able to spend time on them in the future. However, rather than try to sketch what is still quite premature, I want to end with some general comments about the foundations of probability and decision theory.

It has been remarked by many people that logic is now becoming a mathematical subject and that philosophers are no longer main contributors to the subject. Because of its more advanced mathematical character this has really been true of probability from the beginning. The great contributions to the foundations of probability have been made by mathematicians—de Moivre, Laplace, von Mises, and Kolmogorov come quickly to mind. Although there is a tradition of these matters in philosophy—and here one thinks of Reichenbach and Carnap—it is still certainly true that philosophers have not had a strong influence on the mainstream of probability theory, even in the formulation of its foundations. On the other hand, I strongly believe in the proposition that there is important and significant work in the foundations of probability that is more likely to be done by philosophers than by anyone else. The various interpretations of foundations, ranging from the subjective view of the classical period through the relative frequency theory of the first part of this century to propensity and other views of late, have probably been discussed more thoroughly and more carefully by philosophers than by anyone else. I see no reason to think that this tradition will come to an end. The closely related problems of decision theory are just beginning to receive equal attention from philosophers after their rapid developnnent by mathematical statisticians in the two decades after World War II.

It is important for philosophers to be familiar with and to know the formal and technical developments by mathematicians and statisticians. It is unfortunate that there has been a tendency for philosophers to pursue their own special formalisms that do not relate well to the mainstream of work. Such formalisms tend to be, from a mathematical and conceptual standpoint, too elementary to come to grips with complex problems of applications or to offer sufficient complexity of structure to handle the main problems of interest to those pursuing technical issues. What seems to me to be the right role for philosophers in these matters is to be able to comment on and to use the concepts that are actively developed in most cases by others. I do not see as viable a special philosophical approach to probability, and my views on this matter are consonant with what I think about other issues in the philosophy of science or in philosophy generally.

Causality

Because my own approach to causality is probabilistic in character, I have included it in this section. It is hard to think of a philosophical topic that has received more attention historically than that of causality. It has already become clear to me that what I have had to say (1970a) has got to be extended, revised, and deepened, in order to meet objections that have been made by other people and to account for a variety of phenomena that I did not consider in any detail. Causality is one of those concepts that plays a major role in a variety of scientific disciplines and that can be clarified and enriched by extensive philosophical analysis. On some subjects of a probabilistic kind I find it hard to imagine how I, or another philosopher, could improve in a substantial way on what has been said with clarity and precision by probabilists and statisticians—the concept of a stochastic process is a good example. This is not true of the concept of causality. A good many statisticians use the concept in various ways in their research and writing, and the concept has been a matter of controversy both in the physical sciences and in the social sciences over the past several decades. There is a major place in these discussions for philosophical analyses of causality that join issue firmly and squarely with this extensive scientific literature.

A recent article by Woods and Walton (1977) emphasizes a point that is something of a minor scandal in philosophy. This is the absence of clear and definite elementary principles for accepting or rejecting a causal relation. The teaching of elementary logic depends upon extensive use of material implication and other truth-functional sentential connectives, in much the same way that beginning students of physics are taught Newtonian and not relativistic mechanics. We unfortunately do not at the present time have the same tradition in philosophy about a range of concepts that lie outside of formal logic. Causality is perhaps the prime example. I mention the point as a matter of pedagogy but in fact it is a matter of philosophy proper, because there has not been sufficient development or agreement about the developments that have taken place to provide a set of transparent systematic concepts that can be used in introductory teaching.

There are one or two systematic points about causality I would like to comment on here without entering into technical details The first is the objection to my characterization of causality in terms of probability. A standard remark about this characterization is that all kinds of spurious relations will satisfy the definition of prima facie cause. According to my formulation, an event A is a prima facie cause of event B if A occurs earlier than B and the conditional probability of B given A is greater than the unconditional probability of B alone. It is properly pointed out that many kinds of events are connected in the sense of this definition by a prima facie causal relation, for example, the lowering of the barometer and the rain that follows, and yet we all immediately reject the falling of the barometer as a prima face cause of the rain. I see the situation here as no different from that which applies to logical inference. The machinery is set up to be indifferent to our intuitive facts about the world, so that we can make logical inferences that seem silly. Standard examples are easy to give and are familiar to everybody. The same point is not as easily accepted about causality, but it is my claim that this is a virtue and not a defect of a general theory of causality. It should be universally applicable; intuitive counterexamples simply reflect the fact that the formal theory is indifferent as to what intuitive knowledge or substantive theory is being called upon.

Moreover, the full formal theory has appropriate devices for eliminating falling barometers as causes of rain. The standard notion to use here is that of a spurious cause. Showing that other events account for the change in conditional probability when the barometer is not present or broken provides the intuitive evidence we all accept for the absence of a causal relation between falling barometers and rainfall. The second point, related to this one, is that the notion of spurious cause itself and the closely related one of genuine cause must be relativized to a particular conceptual framework. This is made especially clear when one wants to prove a theorem about causality within the framework of a particular scientific theory. In my 1970 monograph I did not make the relativization to a particular framework an explicit part of the definitions. It is obvious how this can be done and perhaps in many cases it should be done. I do think that the insistence on relativizing the analysis of cause to a particular conceptual framework is a point on which to make a stand. Absolutists, who think they know the full truth and do not need such relativization, have the burden of providing forceful examples. I know of no interesting ones myself. I take this point as no different than the point that the systematic formal concept of truth is relative to a model and not in any sense appropriate to reality taken straight.

There is another and more interesting point raised in conversations on various occasions by Nancy Cartwright, Paul Holland, and others. It is that the full notion of causality requires a sense of experimental manipulation. There are many ways of formulating the idea. Holland likes to say that, from a statistical standpoint, without random assignment of individuals to experimental groups an unimpeachable causal inference cannot be made. My most immediate reply is that ordinary talk and much scientific experience as well does not in any sense satisfy these conditions of experimental design, that is, the causal claims that are made in ordinary talk or in much of science have not arisen from well-designed experiments but from quite different circumstances—in fact, from circumstances in which no experiments have taken place and in many cases are not possible. The great classical example is celestial mechanics. From the time of the Babylonians to the present, we have seen a variety of causal theories to account for the motion of the planets, the moon, and the stars. In the case of some terrestrial phenomena that are not themselves directly subject to experiment but for which an analysis can be built up in terms of experimental data, we are faced with a rather more complicated decision about what we regard as proper extrapolation from experiment. In fact, one underhanded way to meet the objections raised by Cartwright and Holland is to point out that the use of scientific theories outside the experimental domain and the power of the application of science depend upon sustaining causal claims in nonexperimental settings. Are we to conduct experiments on extendability in order to establish a justification of using the results of experiments in nonexperimental settings? It would not be difficult to set up a straw man of infinite regress by literal pursuit of this line of thought. My own view is that, rather than claiming that only in experimental settings can we really make proper causal claims, we should formulate theorems that are applicable to experimental settings but not to others. It seems to me one kind of theorem we might want to insist upon is that for experiments whose theory of design is adequate we should expect to be able to prove within a framework of explicit probabilistic concepts that all prima fade causes are genuine. We would not expect such a theorem to hold in general in nonexperimental settings.

Kreisel has pointed out to me that the general theory of causality is unlikely to be of much scientific significance once specific scientific theories are considered. Indeed, the interest of such theories is to provide a testing ground for the correctness of the general notions. On the other hand, not only ordinary talk but much highly empirical scientific work does not depend on a well-defined theoretical framework, and for these cases the general theory of causality can provide useful analytic concepts.

Foundations of Psychology

I have already remarked on my earliest experimental work in psychology in connection with the test of various concepts and axioms of decision theory. I shall not refer further to that work in this section. Because of my extensive work in psychology over the past two decades, I have organized my remarks under four headings: learning theory, mathematical concept formation in children, psycholinguistics, and behaviorism.

Learning theory
Either in my last months as a graduate student at Columbia or shortly after my arrival at Stanford in the fail of 1950—I cannot remember which—I developed my first interest in learning theory. As might easily be surmised, it began with trying to understand the various works of Clark Hull, not only the *Principles of Behavior* (1943) but also the relatively unreadable work written earlier in collaboration with the Yale logician Frederick Fitch and others (Hull, Hovland, Ross, Hall, Perkins, & Fitch, 1940). Part of my interest was stimulated by some very bright graduate students in psychology who attended my lectures in the philosophy of science. Probably the most influential was Frank Restle. I was a member of his dissertation committee, but I am sure I learned more psychology from him than he learned from me. My serious interest in learning theory began, however, in 1955 when I was a Fellow at the Center for Advanced Study in the Behavioral Sciences. Restle was there, but even more important for my future interests was the presence of William K. Estes. In his own and very different way, Estes has the kind of intellectual clarity I so much admired in McKinsey and Tarski. We began talking seriously about the foundations of stimulus sampling theory, which really began with Estes's classical paper (1950). It became apparent to me quite soon that stimulus sampling theory was from a conceptual and mathematical standpoint much more viable and robust than the Hullian theory of learning. No really interesting mathematical derivations of experimentally testable results could be made from Hull's axioms. The great virtue of stimulus sampling theory was that with variation of experimental conditions new experimental predictions could be derived in an honest way without the introduction of *ad hoc* parameters and with the hope of detailed experimental test.

Perhaps it will be useful to say something more about the contrast between Hull's theory and stimulus sampling theory. The mere use of mathematics and especially of mathematical symbols in a theory is no guarantee that anything of mathematical or scientific interest is being done. In Hull's theory the feeling is too much that each experimental result, or almost each remark about an experiment, is being given a direct translation into mathematical symbols. In contrast, no powerful and simple set of theoretical postulates from which specific results can be derived, once initial and boundary conditions are described, is even remotely approached. The translation from ordinary mathematical statements into the still more formal apparatus of mathematical logic as exemplified in the 1940 work of Hull and others cited above is still more mistaken if the only objective is a translation. One of the great lessons of logic in the 20th century is that formal systems themselves as deductive instruments are not of as much conceptual importance as the mathematical study of properties of such systems, but it is precisely the mathematical analysis of psychological theory that is not even touched upon in Hull's work. Hull, on the other hand, is in good company in this mistaken move. It has taken some time for the situation to become clarified as a result of the large body of important work in mathematical logic and metamathematics in this century. In Volume III of Whitehead and Russell's *Principia Mathematica* (1913) a detailed theory of measurement is developed; but from a formal standpoint their results are elementary, the notation is so forbidding and the welter of symbols so profuse that very little use has subsequently been made of the material.

In order not to seem too dogmatic about this point, it is worth noting that the interest in the use of formal systems has returned in new guise in the form of programming languages, but here the orientation is very different from that to be found, for example, in Hull's *Mathematico-Deductive Theory of Rote Learning*.

In contrast to Hull's theory, stimulus sampling theory works in a way very analogous to that of physical theories. The initial probabilities of response correspond to initial conditions in a physical problem, and reinforcement schedules correspond closely to boundary conditions. The fruits of extensive collaboration with Estes in 1955-1956 did not appear until later. In fact, we have published only two articles together (l959e, 1974q). The first article appeared as a technical report in 1957 and the second one first appeared as a technical report in 1959. The collaboration with Estes in writing the two long technical reports, later condensed into shorter papers, has been one of my most satisfactory research efforts in psychology. In a genuine sense these two reports combined a concern for axiomatic foundations with a focus on new scientific results.

While Estes and I were together at the Center, he introduced me to his former graduate student, Richard C. Atkinson, and we arranged for Atkinson to spend the following academic year at Stanford as a research associate. He and I undertoook an extensive series of investigations into the application of stimulus sampling theory to two-person interactions. The initial fruit of this collaboration was my first experimental article in psychology (l958a). (Unlike most psychologists, I published an experimental book (l957b) before publishing an experimental article.) Atkinson and I expanded this first effort into an extensive series of studies, which were published in our book, *Markov Learning Models for Multi-person interactions* (l960b). It was a great pleasure to me to work in this area of application of learning theory. It combined my fundamental interest in learning theory with my earlier interest in game theory, and I found that I had a natural taste for elaborate analysis of experimental data. (The book with Atkinson is much richer in data analysis and the testing of models than is the earlier book with Davidson and Siegel.) I think I work best with someone like Atkinson, who is extremely well organized and very good at designing and running experiments. I like to get into the action when the analysis of the data and the testing of theory are to be the focus. Working with Atkinson has the additional advantage that he is also an able theorist and has plenty of ideas of his own.

This was the period in which my theoretical interests in learning theory were flourishing. I also worked at this time on mathematical aspects of learning processes, particularly a study of their asymptotic properties in collaboration with John Lamperti, who was then a member of the mathematics faculty at Stanford. This work appeared in two publications (1959h, l960i).

I spent a fair amount of time on the generalization of learning theory to a continuum of responses. The first step was to generalize the linear model (1959b), and the second step was to generalize stimulus sampling theory (1960h). In collaboration with Raymond Frankmann, Joseph Zinnes, and later Henri Rouanet and Michael Levine, extensive tests of learning theory for a continuum of responses were made in publications between 1961 and 1964. With Jean Donio, who like Henri Rouanet was at that time a young French scientist working with me, I worked out the generalization in another direction to a continuous-time formulation of the theory. I am still pleased with the character of this work. It took a certain amount of mathematical effort to get things straight, and some of the detailed empirical predictions were quantitatively accurate and surprising. It is especially in predicting something like the continuous distribution of responses that untutored intuition fails in providing anything like an accurate idea of what the results will be. The need of theory to make non-trivial predictions becomes especially evident. On the other hand, I do not think that this work has had very much impact in psychology. The developments have not been followed up in the directions that could have led to more powerful results, but I do not think we were walking down a blind alley in this research effort. The kind of approach developed will almost surely turn out to be of use in several directions when a larger number of psychologists with strong mathematical training and quantitative empirical interests come onto the scene to study the theory of motor skills and a variety of perceptual phenomena in detail.

Mathematical Concept Formation in Children

In 1956 my oldest child, Patricia, entered kindergarten and my interests in applications were once again stimulated, in this ease to thinking about the initial learning of mathematical concepts by children. In collaboration with Newton Hawley, who was and still is a member of the mathematics faculty at Stanford, we began the following year, when our daughters were both in the first grade, the informal introduction of constructive geometry. At that time very little geometry was taught in the primary grades. A brief description of this first effort is to be found in Hawley and Suppes (1959g), but, more importantly, we went on to write two textbooks for primary-grade students in geometry, which have since been translated into French and Spanish (1960c, 1960d).

This practical interest in the mathematics curriculum in the schools almost inevitably led to trying to understand better how children learn mathematical concepts. Once again because of my continued collaboration with Estes, I was fortunate to get Rose Ginsberg, who was just completing a PhD at Indiana with Estes and C. J. Burke, to join me at Stanford as a research associate with the express purpose of studying concept formation in children. This work resulted in a number of publications with Ginsberg (1962d, 1962e, 1963c) and also a number of publications of my own, of which I mention especially my monograph *On the Behavioral Foundations of Mathematical Concepts*, appearing in the Monograph Series of the Society for Research in Child Development (l965e), and also my article on the same topic in the next year in the *American Psychologist* (1966g).

In this work in mathematical concept formation in children, Ginsberg and I were concerned to apply, as directly and as naturally as possible, stimulus sampling theory to the learning of such concepts. We met with more success than I initially expected. The ability of relatively simple models of stimulus sampling theory to account for the learning of simple mathematical concepts in great detail is, I think, a surprising fact and an important one.

In 1960, 1 was finishing my textbook on axiomatic set theory and the question naturally arose of the relation between the set-theoretical foundations of mathematics provided by Zermelo-Fraenkel set theory and the learning of mathematics by children. If I had not been finishing that text-book at the time I might well not have embarked upon a number of the experiments that Ginsberg and I undertook to test the formation of elementary concepts about sets—for example, identity and equipollence—as well as elementary geometrical concepts. A practical fruit of these investigations was the undertaking of a new series of elementary mathematics textbooks entitled *Sets and Numbers*, which was published over several years in the sixties.
In recent years the interest in mathematical concept formation has melded into my work on computer-assisted instruction, which I discuss in a later section.

Psycholinguistics
Another natural area of application of stimulus sampling theory is language learning. Again I was fortunate to get another former student of Estes and Burke from Indiana, Edward Crothers, to join me as a research associate at Stanford. We undertook a systematic series of experiments in second-language learning; this effort led to several publications but especially to a book in 1967, *Experiments in Second-language Learning*, which reported a large number of investigations on various elementary aspects of learning Russian.
At the same time, given my philosophical inclinations and training, it was natural for me to become interested in the broader range of controversies in psycholinguistics. To some extent the first chapter of the book with Crothers summarizes the kind of attack on the problems of language learning to be expected from a behavioral standpoint. I also attempted in the writing of this chapter to provide a

partial answer to the many criticisms that were being made of behavioral theories by psycholinguists.

Two years later I published a more thoroughly worked out answer in my article ‘Stimulus-Response Theory of Finite Automata’ (1969g). In this article I showed that from simple principles of conditioning one could obtain the kind of language behavior of which a finite automaton is capable. I made no attempt to relate the theoretical developments to the detailed and subtle learning that takes place in a child, but rather argued that the presumed theoretical limitations of stimulus-response theory were misunderstood by a number of linguistically oriented critics. I have continued to be involved in this controversy and some of my most recent articles are concerned with it (l975b, l977d).

I have emphasized in my writings on this subject that the challenge to psychological theory made by linguists to provide an adequate theory of language learning may well be regarded as the most significant intellectual challenge to theoretical psychology in this century. At the present time numerous difficult problems of providing a completely adequate scientific theory of language learning and language performance are enough to make even the most optimistic theorist uneasy. In very developed areas of science or mathematics, it is familiar to find the statement made that certain kinds of problems are simply far beyond the resources currently available but that certain more restricted problems are amenable to serious attack and likely solution. For example, in quantum chemistry there is, with present intellectual and computing resources, no hope of making a direct attack on the behavior of complex molecules by beginning with the first principles of quantum theory. A problem as easy to formulate as that of deriving from first principles the boiling point of water under normal atmospheric pressure is simply beyond solution at the present time and is recognized as such. Within mathematics there are classical open problems in elementary number theory, group theory, differential geometry, and in fact almost any developed branch of mathematics. Psycholinguistics will be a far happier and more productive subject when the same state of developed theory has been reached. A frontal attack on the problem of giving a complete analysis of the speech of even a three-year-old child is certainly outside the range of our conceptual tools at the present time. What seems essential is to recognize this fact and to determine the appropriate range of significant yet possibly solvable problems that should be studied.
One approach that has already been fruitful and will be significant in the future is the attempt to write computer programs that can understand and learn a natural language. Such enterprises must at present be restricted to a small fragment of a natural language, but the thorough investigation of such small fragments seems to me a promising arena for making progress in the way that is characteristic of other domains of science. I do not mean to suggest by this that study of natural language should be restricted to computers—

certainly not. There will continue to be a significant and important accumulation of fact and theory about the language learning of children. Our understanding of these matters will deepen each year, but what is not yet clear is the direction theory will take so as to be adequate to the limited domains of understanding we master. I recognize the inadequacies from an empirical standpoint of what can presently be said about language learning within a stimulus-response framework or a more sophisticated version of S-R theory in terms of procedures and internal data structure, but I also believe in emphasizing the theoretical thinness of any of the proposals about learning that come from the linguistic side of psycholinguistics. In fact, practically none of the ideas originating from linguistics about language learning has been sufficiently developed in a systematic fashion to permit any sort of theorem, asymptotic or otherwise, to be proved.

Some may say that it is scarcely required of an empirical theory that it be precise enough to permit the proving of asymptotic theorems, but in the present context it seems to me an important consideration. The actual empirical phenomena are too complicated for anyone coming at them from any theoretical viewpoint to provide a detailed account. One test of a theoretical proposal is whether its structure is rich enough to permit in principle the learning of language as the amount of exposure or experience goes to infinity. A good recent effort in this direction that does permit a theorem to be proved, even if the concept of meaning underlying the theory is not at all realistic, is to be found in Hamburger and Wexler (1975)—and I am pleased to claim Wexler as a former doctoral student of mine.

Toward the end of this period in the sixties I also got involved in the detailed empirical study of children's first language. The initial work in this area was in the construction of probabilistic grammars (1970b) followed by the construction of model-theoretic semantics for context-free fragments of natural language (l973e). I have been especially skeptical of the semantical concepts used by linguists. The long and deep tradition of semantical analysis in logic and philosophy provides, in my judgment, a much sounder basis for the analysis of the semantics of natural language. In my address as recipient of the American Psychological Association Distinguished Scientific Award in 1972, I tried to lay out the virtues of the model-theoretic approach to the semantics of children's speech (1974m). This is an issue that is still before us, and it would be too easy for me to enter into the substantive debate in this essay. I cannot refrain, however, from a few remarks.

The concept of meaning has a much longer history in philosophy and logic than it does in psychology or in linguistics. It is possible to begin the philosophical story with Aristotle, but Frege and Tarski will do as modern points of departure. The important thrust of this work has been to describe how the meaning of a sentence is to be built up from the meaning of its parts. In Tarski's case this takes the form of giving an explicit recursive definition of truth. One of the reasons for the large disparity between this developed logical literature and what goes under the heading of meaning in psycholinguistics is, I believe, the concern in psycholinguistics primarily for the meaning of individual words. In Roger Brown's recent book (1973) he talks a good deal about the meanings of individual words and even about the meaning of imperatives, but what he does not really face at any point is the Fregean task of trying to understand how the meaning of a complex utterance is built up from the meaning of its parts. Without this there can be no serious theory of meaning, and until this is thoroughly recognized by psycholinguists I am skeptical that a satisfactory psychological theory of meaning for use in studying the first language of children can be developed.

I have undertaken additional large-scale empirical work on children's language in collaboration with my former students, Dr. Robert Smith and Dr. Elizabeth Macken, as well as with Madeleine Léveillé of the Laboratory of Experimental Psychology in Paris. The first fruits of this collaboration are to be found in the reports by Léveillé, Smith, and me (1973g; l974t), and in two recent articles, one with Macken (l978f) and one by me (in press—b).

Of all my work in psychology, that concerning the syntax and semantics of children's first language has had the closest relation to broad issues that are current in philosophy, and for this reason alone I expect my interest to continue unabated. I have more to say on these matters in the section on philosophy of language.

Behaviorism

In spite of my recent interest in psycholinguistics I have certainly been identified in psychology with behaviorism, and I have written several philosophical pieces in defense of behaviorism. It should be clear from many other things I have to say in this essay that I do not believe in some Skinnerian form of behavioristic reductionism. In fact, the kind of methodological behaviorism, or what I have sometimes labeled neobehaviorism, I advocate is antireductionist in spirit, and wholly compatible with mentalistic concepts. The central idea of methodological behaviorism is that psychology as a science must primarily depend on behavioristic evidence. Such evidence is characterized in terms of stimuli and responses described in terms of psychological concepts. It is certainly possible to ask for physiological and, indeed, physical or chemical characterizations of both stimuli and responses. In some kinds of work, characterization of stimuli at a physical level is highly desirable, as for example in some detailed studies of visual perception. On the other hand, I strongly believe that a reduction of psychology to the biological or physical sciences will not occur and is not intellectually feasible. I am not happy with leaving the statement of my views at this level of generality, and I consider it an intellectual responsibility of methodological behaviorists like myself to reach for a deeper and more formal statement of this antireductionist position. What are needed are theorems based on currently reasonable assumptions showing that such a reduction cannot be made. I think of such theorems as being formulated in the spirit in which theorems are stated in quantum mechanics about the impossibility of deterministic hidden variable theories.

Given my earlier work, as reflected for example in my paper on the stimulus-response theory of finite automata (1969g), it may seem paradoxical for me to be arguing for such impossibility theorems, but the thrust to develop a psychological theory of computable processes is to be understood as an effort to bring the theory of language and other complex psychological phenomena within the framework of methodological behaviorism.

As I have argued more than once in the past, stimulus-response theory of behavior stands in relation to the whole of psychology in much the same way that set theory stands to the whole of mathematics. In principle it offers an appealingly simple and rigorous approach to a unified foundation of psychology. It is worth examining the extent to which a stimulus-response 'reduction' of the rest of psychology is feasible. The first difficulty is that most of the other parts of psychology are not formulated in a sufficiently general and mathematical form, contrary to the case of mathematics where what was to be defined in set-theoretical terms already had a relatively precise characterization. Because I am not persuaded that the reductionistic approach is of much interest at the present time, I turn to stimulus-response theory itself and its internal difficulties.
Although I shall not enter into the technical details here it is not difficult to show that given an arbitrary Turing machine a stimulus-response model of behavior with only simple principles of stimulus sampling, conditioning and reinforcement operating can be constructed that is asymptotically (in time) isomorphic to the Turing machine. The tape of the machine is represented by some potentially infinite sequence of responses, for example, responses to the numerals as stimuli. From this asymptotic representation for any Turing machine, we can construct a universal stimulus-response model corresponding to a universal Turing machine that will compute any partial recursive function. Thus in principle we can claim the adequacy of stimulus-response theory to give an account of the learning of any computable process, and presumably any human cognitive or affective behavior falls within this context.

From a sufficiently general philosophical viewpoint this stimulus-response representation of any behavior no matter how complex is of some interest. It shows, just as does the representation of all of classical mathematics within set theory, how simple the primitive concepts of a powerful theory can be when there is no severe limitation on the means of construction. In particular the representation provides an abstract reduction of all concepts of behavior to the simple set required for the formulation of stimulus-response theory, but the word abstract needs emphasis because we certainly have no idea how to carry out the actual reduction of most interesting behavior.

The basic representation of a universal Turing machine by a stimulus-response model brought to isomorphism at asymptote requires learning procedures that consist only of conditioning and change of conditioning of responses to given stimuli. But there is a severe weakness of these asymptotic results. Nothing is said about the learning rate. To match human performance or to be of real conceptual interest, the learning of appropriately simple concepts must not be too slow. Take the case of first-language learning in the child, for instance. A rather extravagant upper bound on the number of utterances a child has heard by the age of five is ten million. A learning rate that requires three orders of magnitude of exposure beyond this is not acceptable from a theoretical standpoint, but the highly simplified inadequate representation of the genetically endowed structures and functions the child brings to the task is evident. A richer theory is required to deal with them, but almost certainly there will be no fully satisfactory theory developed at any time in the foreseeable future.

Philosophy of Language

I have already said something about my interest in language in the section on psycholinguistics. Much but not all of my formal work on the theory of language has been related to psycholinguistics. Some overlap of the earlier section will be inevitable, but I concentrate here on the work that is more or less independent of psycholinguistics. My paper on model-theoretic semantics for context-free fragments of natural language (I973e) was partly generated by thinking about children's language but also by the formal problem of putting together the kinds of grammar that have become current in linguistics and computer science with the kind of model theory familiar in logic. The general approach has been anticipated by Knuth (1968) but he had not brought to the surface the model theory, and I did not become aware of his paper until I had worked out the essentials. My paper was first circulated as a technical report in 1971, and the ideas were applied with great thoroughness and extended by Robert Smith in his dissertation (1972), written under my direction. The basic philosophical point is that we can provide a standard model theory for context-free fragments of English directly without any recourse to the model theory of first-order logic. Because of my conviction that this can be done easily and naturally, I have continued to argue for the inappropriateness of first-order (or second-order) logic as an analytical tool for the study of the semantics of natural language. I summarize some of the further developments in later papers. Before doing so I want to note that the restriction to context-free grammars is not essential. One can work out a corresponding model-theoretic semantics for transformations that map trees into trees in the standard linguistic fashion, but because there is a great deal of work to be done just within a context-free framework I have not worked out many details from a wider perspective.

In my address (1973b) as outgoing president of the Pacific Division of the American Philosophical Association, I applied these semantical ideas to develop a general notion of congruence of meaning. I characterized this view as a geometrical theory of meaning because I developed the viewpoint that different weak and strong notions of congruence are appropriate to catch different senses of 'identity' of meaning. Moreover, it seemed to me then and it still seems to me that there is much to be learned from geometry about the concept of congruence and the related concept of invariance that is applicable to the theory of meaning. We have long ago abandoned the idea of one true theory of geometry; we should do the same for meaning.

The most important idea in the paper is to tie closely together the notion of congruence of meaning and the particular syntactical structure of expressions. Without such an explicit use of syntax I am deeply skeptical that any satisfactory general theory of meaning can he found. In particular, the features of similarity of meaning between two expressions seem to me lost in any translation into first-order logic, and just for this reason I am doubtful of the appropriateness of the standard notions of logical form. In fact, I suppose my view is that there is no serious notion of logical form separate from the syntactic form of an expression itself.

On the other hand, I accept as of great importance the crude notion of congruence characterized by logical equivalence of expressions. This extensional concept of congruence is the robust and stable one needed for much scientific and mathematical work. Of course, for most systematic contexts a weaker notion of equivalence is used, one in which the notion of consequence is broader than that of logical consequence, because of the assumption of various background theories—all of classical mathematics in the case of physics, for example.

There is a point concerning identity of meaning that I did not develop explicitly enough in that address. In geometry we have a clear notion of identity for geometrical figures but it is not a notion that receives any real use compared to the importance of the notion of congruence for the particular geometry under study. It seems to me that this is very much the case with meaning. I am not unhappy with a very psychological approach to meaning that takes the meaning of a term to be unique at a given time and place to a given individual. Thus in crude terms the meaning of a proper name in this sense might well be taken to be the set of internal programs or procedures by which the individual that uses or recognizes the proper name attaches properties or relations to the object denoted by the proper name. These procedures or programs internal to a particular language user are private and in detailed respects idiosyncratic to him. The appropriate notion for a public theory of meaning is a notion of congruence that is considerably weaker than this very strong sense of identity. If the viewpoint I am expressing is near the truth, the search for any hard and fast sense of identity of meaning is a mistake. It rests hidden away in the internal programming of each individual. What we are after are congruences that can collapse these private features across language users to provide a public and stable notion of meaning.

In fact, this way of looking at the matter is in a more general philosophical way very satisfying to me. I have come to be skeptical of the long philosophical tradition of looking for various kinds of bedrocks of certainty, whether in epistemology, logic, or physics. Just as the natural notion of a person is not grounded in any hard and definite realization, and certainly not a physical one because of the continual fluctuation of the molecules that compose the body of the person, so it is with the meaning of expressions. In terms of what I have just said about an ultimately psychological theory of meaning at the deepest level, I also disagree with Frege's attempt to separate in a sharp and absolute fashion logic from psychology.

From a formal standpoint, my work on the semantics of natural language has recently taken a more radical turn. I now believe that the semantics of a significant fragment of ordinary language is most naturally worked out in a framework that is an extension of relational algebras as developed earlier by Tarski, McKinsey, and others. Moreover, the notation for the semantics is variable free, using only constants and operations on constants, for example, taking the converse of a relation, the image of a set under a relation, etc. In a recent paper (1976c) I work out the details of such an approach to the standard quantifier words, whether in subject or object position. Such a view runs against the tide of looking upon quantification theory in first-order logic as one of the prime logical features of natural language. But as has been known implicitly since the time of Aristotle, much natural language can be expressed within Boolean algebra and it is not a large step from Boolean algebras to relational algebras of various sorts. One of the points of my 1976 paper is to prove that if we use the standard linguistic parsing that makes quantifiers part of noun phrases—so that we treat *all men* in the sentence *All men are mortal* as being a simple noun phrase and have a tree structure that reflects this—then it is not possible under obvious and natural conditions to have a Boolean semantics of the sort that has been familiar for a hundred years for such utterances. The previous history of Boolean semantics did not emphasize that the syntax and semantics had to go together. The proof of the theorem depends upon this intimate marriage of model-theoretic semantics and context-free grammars. The line of extension to quantifiers in object position brings in relational algebras in an obvious way, but the elimination of any quantifier notation in the underlying semantical notation is based on the same concept as in the case of quantifiers in subject position.

This same framework of ideas is developed in considerable detail in a paper on attributive adjectives, possessives, and intensifying adverbs, by Macken and me (l978f). The full set of subtleties to be found in the ordinary use of adjectives, possessives, and adverbs is beyond the competence of any theory to handle completely at the present time. I do think we make a reasonable case for the kind of model-theoretic semantics without variables that I have described as providing a more detailed and intuitive semantical analysis than any of the theoretical approaches previously published.

In a paper I am just now finishing I go on to consider logical inference in English. To my surprise I have been able to find practically no papers dealing with such inferences in a direct fashion. The reason for the absence, I suppose, is the slavish adherence to first-order logic in too much of the tradition of semantical analysis of natural language. A semantics that fits more hand in glove with the syntax of the language is required to generate the proper feeling for rules of inference, even though, as a technical tour de force, translation back and forth into a formal language is possible.

My own program of research in the philosophy of language is firmly laid out in broad outline, if not in all details. I want to understand in the same way I believe I now understand quantifier words the many other common function words in English. I have already spent some time on the definite article and the possessive preposition of and I would like to do the same for the other high-frequency prepositions like *to, in, for, with, on, at, by,* and *from*—I have listed these prepositions in their order of frequency of occurrence in a large corpus collected by Kucera and Francis (1967). The frequency of these simple prepositions is among the highest of any words in English but their semantical theory is as yet in very unsatisfactory state. Detailed analysis of their semantics is undoubtedly too empirical a problem for many philosophers deeply interested in language. I do not know whose work it is supposed to be—perhaps the empirical flavor of it seems antithetical to what philosophy should be like—but my own empirical bent in philosophy is nowhere more clearly reflected than in my attitude toward the philosophy of language. I do not think it is the only task, but for me it is a primary task, to provide a formal analysis that is faithful to at least the main function words in English when used in a serious and systematic way—I would of course like to have a theory for all possible uses but that seems out of reach at the present time. Frege himself had little to say about such matters and seemed rather suspicious of natural language as a vehicle for communicating exact thoughts. This same Fregean attitude has continued to hold an important place in the philosophy of language, but it should be apparent that I consider this aspect of Fregean philosophy a clear and definite mistake. I can think of no more appropriate task for philosophers of language than to reach for an exact and complete understanding of the prepositions I just mentioned, quantifier words, the tenses of simple verbs, and the like. As long as there is one definite intuitive usage that remains semantically unanalyzed, we have not completed the main task of any semantical theory of language. I do want to emphasize how complex and subtle I consider this task to be. I am sure it will continue to be an active topic in philosophy a hundred years from now.

Education and Computers

In the section on mathematical concept formation in children I mentioned the beginning of my interests in education in 1956 when my oldest child, Patricia, entered kindergarten. I cited there the work in primary-school geometry. An effort, also noted but briefly, that was much more sustained on my part was work in the basic elementary-school mathematics curriculum. This occupied a fair portion of my time between about 1956 and the middle of the sixties and led to publication of a basic elementary-school mathematics textbook series, *Sets and Numbers*, which was one of the more radical of the ‘new math’ efforts. Unlike many of my colleagues in mathematics and science who became interested in school curriculum after Sputnik, I had a genuine interest in the psychological and empirical aspects of learning and a traditional interest in knowing what had been done before.

When I began working on the foundations of physics after graduate school, I was shocked at the absence of what I would call traditional scholarship in the papers of philosophers like Reichenbach that I read, or even more of physicists who turned to philosophical matters such as Bridgman and Campbell. There was little or no effort to know anything about the previous serious work in the field. I found this same attitude to be true of my colleagues from the sciences who became interested in education. They had no desire to know anything about prior scholarship in education.

I found I had a real taste for the concrete kinds of questions that arise in organizing a large-scale curriculum activity. I shall not attempt to list all the aspects of this work here, but since, beginning in the mid-fifties, I have written a large number of research papers concerned with how students learn elementary mathematics and I have had a fairly large number of students from education or psychology write dissertations in this area. Most of the work in the last decade or so has been within the context of computer-assisted instruction, to which I now turn.

Computer-assisted Instruction

In the fall of 1962, on the basis of conversations with Lloyd Morrisett, Richard Atkinson and I submitted a proposal to the Carnegie Corporation of New York for the construction of a computer-based laboratory dedicated to the investigation of learning and teaching. The proposal was funded in January 1963 and the laboratory began operation in the latter part of that year as computing equipment that was ordered earlier in the year arrived and was installed. The laboratory was initially under the direction of an executive committee consisting of Atkinson, Estes, and me. In addition, John McCarthy of the Department of Computer Science at Stanford played an important role in the design and activation of the laboratory. In fact, the first computer facilities were shared with McCarthy and his group.

From a research standpoint, one of my own strong motivations for becoming involved in computer-assisted instruction was the opportunity it presented of studying subject-matter learning in the schools under conditions approximating those that we ordinarily expect in a psychological laboratory. The history of the first five years of this effort, through 1968, has been described in great detail—probably too much detail for most readers—in two books (l968a, l972a) and in a large number of articles. I shall restrict myself here to a few general comments.

To some extent those initial hopes have been realized of obtaining school-learning data of the sort one expects to get in the laboratory. Massive analyses of data on elementary-school mathematics have been presented in my own publications, including the two books listed above, and a comparable body of publications has issued from the work of Atkinson and his colleagues on initial reading. My own experience has been that even a subject as relatively simple as elementary-school mathematics is of unbounded complexity in terms of understanding the underlying psychological theory of learning and performance. Over the past several years I have found myself moving away from the kind of framework that is provided by stimulus sampling theory and that has been so attractive to me for so many years. The new ideas are more cognitive in character and organized around the concept of procedures or programs as exemplified, for instance, in a simple register machine, that is, a simple idealized computer with a certain number of registers and a small, fixed number of instructions (1973c). I think that the ideas of stimulus sampling theory still have importance in terms of learning, even in the context of such procedures or programs, but certainly there is a shift in conceptual interest characteristic not only of my own work but also of that of a great many psychologists originally devoted to learning.

One of my initial interests in computer-assisted instruction was the teaching of logic at the elementary-school level and subsequently at the college level. Once complexity of this level is reached, psychological theory is in a more difficult spot in terms of providing appropriate conceptual tools for the analysis of student behavior. Currently my work in computer-assisted instruction is almost entirely devoted to university-level courses, and we are struggling to understand how to analyze data from the sorts of proofs or logical derivations students give in the first logic course or in the course in axiomatic set theory that follows it.

Although there are many questions about the psychology of learning and performance in elementary-school mathematics that I do not understand, still I feel that I have a relatively deep conceptual grasp of what is going on and how to think about what students do in acquiring elementary mathematical skills. This is not at all the case for skills of logical inference or mathematical inference, as exemplified in the two college-level courses I have mentioned. We are still floundering about for the right psychological framework in which to investigate the complete behavior of students in these computer-based courses.

There are other psychological and educational aspects of the work in computer-assisted instruction that have attracted a good deal of my attention and that I think are worth mentioning. Perhaps the most important is the extent to which I have been drawn into the problems of evaluation of student performance. I have ended up, in association with my colleagues, in trying to conceive and test a number of different models of evaluation, especially for the evaluation of performance in the basic skills of mathematics and reading in the elementary school. Again I will not try to survey the various papers we have published except to mention the work that I think is probably intellectually the most interesting and which is at the present time best reported in Suppes, Fletcher, and Zanotti(1976f), in which we introduce the concept of a student trajectory. The first point of the model is to derive from qualitative assumptions a differential equation for the motion of students through the course, initially the drill-and-practice supplementary work in elementary mathematics given at computer terminals. The constants of integration of the differential equation are individual constants of integration, varying for individual students. On the basis of the estimation of the constants of integration we have been able to get remarkably good fits to individual trajectories through the curriculum. (A trajectory is a function of time, and the value of the function is grade placement in the course at a given time.) The development of these ideas has taken me back to ways of thinking about evaluation that are close to my earlier work in the foundations of physics.

Research on computer-assisted instruction has also provided the framework within which the large-scale empirical work on first-language learning in children has taken place. Without the sophisticated computer facilities available to me at Stanford it would not have been possible to pursue these matters in such detail and on such a scale. Even more essentially, the presence of a sophisticated computer system in the Institute for Mathematical Studies in the Social Sciences has led to the computer-based approach to the problems of language learning and performance mentioned earlier. One of our objectives for the future is to have a much more natural interaction between student and computer program in the computer-based courses we are concerned with. Out of these efforts I believe we shall also come to a deeper understanding of not only how computer programs can best handle language but also how we do, in fact, handle it. (Part of this search for naturalness has led to intensive study of prosodic features of spoken speech and how to reproduce them in computer hardware and software.)

I have not yet conveyed in any vivid sense the variety of conceptual and technical problems of computer-assisted instruction that I have tried to deal with in collaboration with my colleagues since 1963. This is not the place to undertake a systematic review of these problems, most of which have been dealt with extensively in other publications. I do, however, want to convey the view that the best work is yet to be done and will require solution of formidable intellectual problems. The central task is one well described by Socrates long ago in Plato's dialogue *Phaedrus.* Toward the end of this dialogue, Socrates emphasizes that the written word is but a pale image of the spoken; the highest form of intellectual discourse is to be found neither in written works or prepared speeches but in the give and take of spoken arguments that are based on knowledge of the truth. Until we have been able to reach the standard set by Socrates, we will not have solved the deepest problems in the instructional use of computers. How far we shall be able to go in having computer programs and accompanying hardware that permit free and easy spoken interaction between the learner and the instructional program is not possible to forecast with any reasonable confidence, for we are too far from yet having solved simple problems of language recognition and understanding.

At the present time we are only able to teach well skills of mathematics and language, but much can be done, and it is my conviction that unless we tackle the problems we can currently handle we will not move on to deeper solutions in the future. Because I am able to teach all my own undergraduate courses in a thoroughly computer-based environment, I now have, at the time of writing this essay, the largest teaching load, in terms of number of courses, of any faculty member at Stanford. During each term I offer ordinarily two undergraduate courses, one in logic and one in axiomatic set theory, both of which are wholly taught at computer terminals. In addition, I offer either one or two graduate seminars. As I have argued elsewhere on several occasions, I foresee that computer technology will be one of the few means by which we can continue to offer highly technical and specialized courses that ordinarily draw low enrollment, because of the budgetary pressures that exist at all American universities and that will continue unremittingly throughout the remainder of this century. Before I am done I hope to add other computer-based courses in relatively specialized areas, such as the foundations of probability and the foundations of measurement. The enrollment in one of these courses will ordinarily consist of no more than five students. I shall be able to offer them only because I can offer them simultaneously. My vision for the teaching of philosophy is that we should use the new technology of computers to return to the standard of dialogue and intimate discourse that has such a long and honored tradition in philosophy. Using the technology appropriately for prior preparation, students should come to seminars ready to talk and argue. Lectures should become as passé as the recitation methods of earlier times already have.

In 1967, when computer-assisted instruction was still a very new educational technology, I organized with Richard Atkinson and others a small company, Computer Curriculum Corporation, to produce courses in the basic skills that are the main focus of elementary-school teaching. In retrospect it is now quite clear that we were ahead of our times and were quite lucky to survive the first five or six years. Since about 1973 the company has prospered, and I have enjoyed very much my part in that development. I find that the kind of carefully thought out and tough decisions required to keep a small business going suits my temperament well.

I have not worked in education as a philosopher. I have published only one paper in the philosophy of education and read a second one, as yet unpublished, on the aims of education, at a bicentennial symposium. Until recently I do not think I have had any interesting ideas about the philosophy of education but I am beginning to think about these matters more intensely and expect to have more to say in the future.

Philosophy and Science

From the standpoint of research I think of myself primarily as a philosopher of science, but to a degree that I think is unusual among professional philosophers I have had over the period of my career strong scientific interests. Much of this scientific activity could not in fact be justified as being of any direct philosophical interest. But I think the influence of this scientific work on my philosophy has been of immeasurable value. I sometimes like to describe this influence in a self-praising way by claiming that I am the only genuinely empirical philosopher I know. It is surprising how little concern for scientific details is to be found in the great empirical tradition in philosophy. It has become a point with me to cite scientific data and not just scientific theories whenever it seems pertinent. I recently made an effort to find any instances in which John Dewey cited particular scientific data or theories in his voluminous writings. About the only place that I found anything of even a partially detailed character was in the early psychology textbook written in the 19th century. When it comes to data, almost the same can be said of Bertrand Russell. It is especially the case that data from the social and behavioral sciences are seldom used in any form by philosophers. In my monograph on causality (1970a) I deliberately introduced detailed data from psychological experiments to illustrate some subtle points about causality. In a recent paper on distributive justice (1977a) I went so far as to calculate Gini coefficients for the distribution of salaries at the various professorial ranks at Stanford and several other universities.

Set-theoretical Methods.

One of the positions in the philosophy of science for which I am known is my attitude toward formalization. In various papers I have baptized this attitude with the slogan "to axiomatize a scientific theory is to define a set-theoretical predicate." A large number of my papers have used such methods, and I continue to consider them important. I should make clear that I am under no illusion that in any sense this method originated with me. My distinctive contribution has been to push for these methods in dealing with empirical scientific theories; the methods themselves have been widely used and developed in pure mathematics in this century. To a large extent, my arguments for set-theoretical methods are meant to be a constructive criticism of the philosophical tendency to restrict formal methods of analysis to what can be done conveniently within first-order logic.

I do not think of set-theoretical methods as providing any absolute kind of clarity or certainty of results independent of this particular point in the history of such matters. They constitute a powerful instrument that permits us to communicate in a reasonably objective way the structure of important and complicated theories. In a broad spirit they represent nothing really new; the axiomatic viewpoint that underlies them was developed to a sophisticated degree in Hellenistic times. Explicit use of such methods provides a satisfactory analysis of many questions that were in the past left vaguer than they need to be. A good example would be their use in the theory of measurement to establish appropriate isomorphic relations between qualitative empirical structures and numerical structures.

The many recent results in the foundations of set theory showing the independence of the continuum hypothesis and related assertions are reminiscent of what happened in geometry with the proof of the independence of the parallel postulate. But, as Kreisel has repeatedly urged, the parallel postulate is independent in second-order formulations of geometry having a strong continuity axiom, whereas the continuum hypothesis is not independent in second-order formulations of set theory. The great variety of recent results in the foundations of set theory have not really affected the usefulness of set-theoretical methods in the analysis of problems in the philosophy of science, and I am certain such methods will continue to be valuable for many years to come.

At one time I might have been upset by the prospect of moving away from set-theoretical methods to other approaches, for example, the kind of deeply computational viewpoint characteristic of contemporary computer science, but now I see such developments as inevitable and indeed as healthy signs of change. It seems likely that the theory of computation will be much more fundamental to psychology, for example, than any development of set-theoretical methods.

Schematic Character of Knowledge

In 1974, I gave the Hägerström lectures in Uppsala, Sweden, entitled *Probabilistic Metaphysics* (1974a). In those lectures I took as my starting point Kant's criticism of the old theology; my purpose was to criticize various basic tenets of what I termed the new theology. The five most important are these:

1. The future is uniquely determined by the past.
2. Every event has its sufficient determinate cause.
3. Knowledge must be grounded in certainty.
4. Scientific knowledge can in principle be made complete.
5. The grounds of rational belief and action can be made complete.

It is not appropriate here to develop in detail my arguments against determinism, certainty, and completeness, but it is my conviction that an important function of contemporary philosophy is to understand and to formulate as a coherent world view the highly schematic character of modern science and the highly tentative character of the knowledge that is its aim. The tension created by a pluralistic attitude toward knowledge and skepticism about achieving certainty is not, in my judgment, removable. Explicit recognition of this tension is one aspect of recent historically oriented work in the philosophy of science that I like.

It seems evident (to me) that philosophy has no special methods separate from those of science and ordinary life and has no special approaches to problems of inquiry. What makes a problem philosophical is not some peculiar intrinsic feature of the problem but its place as a fundamental problem in a given discipline or in some cases the paradoxical qualities it focuses on and brings to the surface. I am sometimes thought of as a primarily formalist philosopher of science, but I want to stress that at least as much of my scientific activity has been spent on detailed data analysis as it has on the construction of formal theories. My attitudes toward induction and the foundations of statistics, for example, have been conditioned by the extensive work in applied statistics I have done as part of other research efforts in psychology and in education.

I pointed out earlier that I thought my work in the foundations of physics was not as significant as the work in psychology because of the absence of an original scientific component. It is one of my regrets that I have not been able to do more in physics, especially in terms of empirical data. I have together with my students pursued certain questions of data in physics with some persistence and great pleasure. If I had the time and energy to write my own ideal book on the philosophical foundations of quantum mechanics, it would present a rigorous and detailed analysis of the relevant data as well as of the theory.

I was especially pleased to receive in 1972 the Distinguished Scientific Award of the American Psychological Association in recognition of my activities as a psychologist. This dual role of philosopher and scientist would not suit everyone's taste but in my own case it has been a happy one for the vitality of my intellectual life.

III. Personal Reflections

My entire academic career has been spent at Stanford, so I have divided this section into three periods: the first five years at Stanford, the next ten, and the last twelve. What I intend is to make some more general and more personal remarks in this part of the essay and especially to comment, beyond the remarks made earlier, on some of the people who have had an influence on me.
I came to Stanford in 1950 immediately upon receiving my PhD from Columbia and I have remained here without interruption. I have had the usual sabbaticals and I have traveled a great deal, perhaps more than most of my academic colleagues, but still I have remained relatively fixed at Stanford and undoubtedly will do so throughout the remainder of my career.

1950-1955

I have already commented on the influence that McKinsey and Tarski had on me during my first years at Stanford. From another direction, as I have already mentioned, I was strongly influenced by working with David Blackwell and M. A. Girshick on their book, *Theory of Games and Statistical Decisions* (1954). Working with them I learned a lot about both mathematical statistics and decision theory that was very useful to me later. I also began at this time working with Herman Rubin in the Department of Statistics, and I learned a great deal from Rubin, who has perhaps the quickest mathematical mind I have ever had the pleasure to interact with in any extended way. His error rate is reasonably high by ordinary standards, but the speed and depth of his reactions to a problem posed are incomparably good. During this period I also learned a great deal from my colleague in philosophy, Donald Davidson, and began with him the joint work in decision theory I mentioned earlier. In many ways we nicely complemented each other, for he comes at philosophical problems from a different direction and from a different background than I do, but our common agreement on matters of importance was more than sufficient to give us a good basis for collaboration.

I remember these early years at Stanford with great pleasure. I was working hard and intensely and absorbing a great deal about a great many different things. On the other hand, it is useful to say something about how slow one can be in taking in new ideas. I learned much from Slackwell and Girshick and also from Rubin about the foundations of statistics, but as I look back on the splendid opportunity that was available to me in working with the three of them it does not seem to me that I got the kind of grip on the subject that I feel I have acquired since that time. I cannot help but feel that an opportunity was wasted and that a delay of years was imposed by my failure to reach a deeper understanding at that time. I think one of the difficulties with my earlier work is that I did not sufficiently appreciate the necessity of getting a strong intuitive or conceptual feeling for a subject. I probably tended to operate in too formal a manner, at least so it seems to me now in retrospect. All the same, some of my best papers were written in this period, and I do not want to sound overly negative about all that I had the opportunity to learn and do in those early years.

1956-1965

I have already mentioned the important influence of Estes during the year 1955-1956 at the Center for Advanced Study in the Behavioral Sciences. The continuation of the work in learning with applications to multiperson interactions and to mathematical concept formation in children was intellectually a major part of my life during the ten years ending in 1965. The work with Estes continued; we spent many summers together. We planned a monograph as the outgrowth of our work but for various reasons did not complete it. We did write the two long technical reports that were eventually published in shortened form as papers. But the extent of Estes's influence on my thinking during this period is underestimated by referring simply to the publication of two papers.

In the summer of 1957 there was a Social Science Research Council workshop, or rather collection of workshops, at Stanford. An outgrowth of the workshop on learning was the volume Studies in Mathematical Learning Theory (1959), edited by R. R. Bush and W. K. Estes, in which I published several papers, including the first paper with Estes. Perhaps the most important intellectual event for me that summer was the encounter with Duncan Luce and the famous 'red cover' report that later was published by Luce as his classical book *Individual Choice Behavior* (1959). He and I had great arguments about the exact interpretation of his axioms. I initially thought he had wrongly formulated his central choice axiom but he succeeded in persuading me otherwise, and out of those first encounters has grown a strong personal friendship and a large amount of collaborative work.

Although during this time I published two logic textbooks, *Introduction to Logic* (1957a) and *Axiomatic Set Theory* (l960a), in this ten-year period more than any other time in my career most of my effort was devoted to psychological research rather than to work in philosophy. I have already mentioned a good many of the individual psychologists I had the pleasure of working with in these years.

Another important influence on me was interaction with Dana Scott over several years, beginning with the period when he was an undergraduate at Berkeley and carrying through intermittently until he joined the faculty at Stanford some years later. Among mathematical logicians I have known, Scott is unusual in possessing a natural taste for philosophical problems and great interest in their analysis. We wrote only one paper together (1958b), but our conversations about a range of intellectual matters have extended over many years. Scott has the kind of clarity typical of logicians put at an early enough age in the Tarski mold. In some ways I am definitely less compulsive about clarity than I was in the days when I was working with McKinsey and later with Scott. Whatever one may say about the psychological healthiness of reducing compulsiveness of this kind, I am not at all sure it has been a good thing intellectually. It is perhaps an inevitable aspect of my widening intellectual interests since the late fifties.

In the last several years of this period, one of the strongest influences on my own work was the succession of able graduate students who wrote doctoral dissertations under my guidance and with whom I often collaborated. I mention (in chronological order) especially Helena Kraemer, Jean Donio, Barry Arnold, M. Frank Norman, and Paul Holland, all of whom took degrees with me in the Department of Statistics at Stanford and all of whom were concerned with mathematical or statistical problems in the foundations of learning theory. (Since 1960 I have had a joint appointment in the Departments of Philosophy and Statistics.) During the same period Michael Levine worked on a dissertation in psychology, which he completed in a formal sense a year or two later. Both Michael Levine and Frank Norman were as graduate students great sticklers for mathematical precision and correctness of formulation of theorems and proofs. I remember well the pleasure they took in correcting any mistakes I made in my graduate course on mathematical learning theory.

By the end of this period my attention was moving to the kinds of psychological questions, many of them applied, that arose in connection with computer-assisted instruction, and, on the other hand, I began to return to a more intense consideration of purely methodological problems in the philosophy of science. This does not mean that my interest in psychological research ended but rather that the ‘learning theory’ period running from 1955 to 1963 was reaching a natural end.

At the end of this period Duncan Luce and I undertook to write a long chapter on preference, utility, and subjective probability for Volume III of the *Handbook of Mathematical Psychology*. Writing this long article introduced me to Luce’s awesome habits of work. Very few people I know are able to meet deadlines for completing a piece of work on time; practically no one is able to complete an agreed-to assignment in advance of the deadline. Luce is one of the few that can; but it is not simply the meeting of the deadline that is impressive, it is his clear and relentless pursuit of the details of a particular theory or argument. I learned a great deal from him in writing this long survey article and have continued to do so. As the impact of his book *Individual Choice Behavior* has shown, he has a superb gift for simple formulation of quite general concepts and laws of behavior.

One important event for my own work and life that took place during this period was the founding, together with Kenneth Arrow, of the institute for Mathematical Studies in the Social Sciences at Stanford. Stanford was then administered in a sufficiently informal way that it is not easy to peg the exact date on which the Institute was formed. It was a natural outgrowth of the Applied Mathematics and Statistics Laboratory, which had been put together in the late forties by Albert H. Bowker, now Chancellor at the University of California at Berkeley, to provide an organizational framework and an intellectual home for a wide range of work in applied mathematics and statistics. My own research began there in the summers, starting with the apprenticeship to Blackwell and Girshick mentioned earlier. The forming of the Institute followed in the late fifties. I have continued to direct the institute since 1959, and it has been a pleasant and constructive home for most of my research efforts since that date.
It was also during this period that I had my one serious flirtation with university administration. I was a half-time Associate Dean of the School of Humanities and Sciences at Stanford for three years and during the last term, the fall of 1961, Acting Dean. During this period I had several opportunities to assume full-time administrative positions at Stanford and some offers to do so elsewhere. I enjoyed the administrative work during this period and think that I have some flair for it, but certainly one of the wisest decisions I have personally ever made was to move away from administration and back into a regular position of teaching and research.

During this period I would probably have left Stanford except for the continued support of Bowker, first when he was Chairman of the Department of Statistics and Director of the Applied Mathematics and Statistics Laboratory, and later when he was Graduate Dean. He more than anybody else was responsible for creating for me an intellectual atmosphere at Stanford and a context for constructive research that have been so attractive that for many years I have not thought seriously about leaving.

1966-1978

To continue this theme of administration as I move into the final period of these personal reflections, I have found that the large-scale computer activities on which we embarked in 1963 have turned out to be a sizable administrative problem in their own right. At the peak of our activities in 1967-1968 we had almost 200 persons, including staff, research associates and graduate students, involved in the

Institute. The activity is much smaller now and I am thankful for that, but it continues to be a relatively complex affair and demands, as it has demanded since 1963, a fair share of my time. I do not regret the time spent, for my temperament is such that I would be restless in a purely sendentary life of paper-and-pencil research. The complex problems of running a computer operation on the frontiers of the technology currently available have provided just the right kind of stimulation to keep me contented and not inclined to seek administrative outlets of a more extensive nature, except those already mentioned at Computer Curriculum Corporation.

The efforts in computer-assisted instruction during these last ten years have been in association with a very large number of people, too many to mention here. My numerous joint publications in this area provide partial evidence of the extent of this collaboration, but I should also mention the many extremely able young programmers and engineers with whom I have worked and who have contributed so much to our efforts. At first I did not rightly appreciate the important role that able technical people can play and indeed must play in any successful research effort involving complex hardware and software. I had in the past heard such stories from my physics friends, but this was my first opportunity to learn the lesson firsthand. It is humbling to direct a complex activity in which you know that you yourself are not competent in most of the aspects of the operation. Large-scale research and development work has this character; I am not entirely happy with it. Increasingly in parts of my own research I have enjoyed working alone, but this is certainly not true of the work in computer-assisted instruction as it is not true of my experimental work in psychology.

Also, there are certain kinds of extended efforts that I would simply not be capable of carrying through on my own. I have in mind especially the effort I have engaged in jointly with David Krantz, Duncan Luce, and Amos Tversky in the writing of our two-volume treatise, *Foundations of Measurement.* This has been my longest collaborative effort, and I am pleased to say that we are still all speaking to each other.

Friendships that extend over many years have not been a topic I have emphasized in this essay, but a number have been important to me. One sad fact is that after staying at Stanford so many years I find that all of the persons with whom I formed relatively close personal ties in the l950s have now departed. This includes Albert Bowker, now at Berkeley, Donald Davidson, now at the University of Chicago, William Estes, now at Rockefeller University, and Richard Atkinson, currently Director of the National Science Foundation. There are, of course, a number of individuals on the campus, especially colleagues in Philosophy and in the Institute for Mathematical Studies in the Social Sciences, that I work with and enjoy interacting with, but most of them are a good deal younger and are not friends of many years standing. Jaakko Hintikka has been my part-time colleague at

Stanford for many years. We have edited several books together and given a number of joint seminars that have been rewarding and pleasurable. On several occasions, Julius Moravcsik also participated in these seminars and I have benefited from my discussions with him about the philosophy of language. A third colleague in philosophy who has been at Stanford for many years but increasingly on a full-time basis is Georg Kreisel, and in the last few years we have developed the habit of talking extensively with each other about a wide range of topics. I value the regular round of talks with Kreisel and look forward to their continuing in the years ahead. Although Kreisel primarily works in the foundations of mathematics, he has a long-standing interest in the foundations of physics. He is especially good at giving broad general criticisms of the first drafts of manuscripts on almost any subject about which I write, and I try to do the same for him.

I close with some brief remarks about my personal life. I was married to my first wife, Joanne Farmer, in 1946, and together we had three children, John, Deborah, and Patricia, who at the beginning of 1978 are 17, 20, and 26 years of age; Joanne and I were divorced in 1969. In 1970, I married Joan Sieber and we were divorced in 1973. Since 1956 I have lived on the Stanford campus, and since 1961 in the house that I now occupy. It is a large, comfortable house, built in 1924, and is located in the old residential section, San Juan Hill, on a spacious lot. I feel completely anchored to Stanford and the Bay Area. It is unlikely that I will ever move anywhere else. All in all, I feel fortunate to have had the kind of life I have had for the past twenty-seven years at Stanford. Another twenty of the same sort is more than is reasonable to ask for, but I plan to enjoy as many as I can.

Stanford,
March, 1978.

Note

1. There is a certain amount of overlap in the content of this self-profile and an autobiography I wrote earlier (1978a), focused on my interest in psychology. I am indebted to Georg Kreisel for a number of useful criticisms. I also wish to thank Lofti Zodeh for the photograph which forms the frontispiece of this volume.

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