The Semantic Conception of Theories and Scientific Realism

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Prologue

The *Semantic Conception of Theories* is a philosophical analysis of the nature of scientific theories that has been undergoing development since at least 1948; anticipations as early as von Neumann 1932 and Birkhoff and von Neumann 1936 can be found. The Semantic Conception was offered as an alternative to the then-prevailing logical positivistic analysis of theories—the so-called Received View on Theories[1]—and proved to be one of many factors in the demise of the Received View. The Semantic Conception of Theories today probably is the philosophical analysis of the nature of theories most widely held among philosophers of science; it frequently is used to analyze or treat other philosophical problems including ones in the physical, biological, and social sciences,[2] and even has found its way into Ronald Giere's elementary philosophy of science textbook (1979), now in its second edition. Summarizing their discussion of the Semantic Conception, Lambert and Brittan (1987) write: "No clear objections, over and above those repetitions of their own positions that classicist and historicist defenders might urge, to the semantic view have yet emerged" (145–46). This acceptance and utilization have occurred despite the fact that the Semantic Conception's development is diffusely scattered throughout journals and edited volumes, and no single comprehensive work developing the Semantic Conception has been produced.

According to the Semantic Conception of Theories, scientific theories are not linguistic entities, but rather are set-theoretic entities.[3] This is in sharp contrast to many versions of the positivistic Received View on Theories, which construes theories as partially interpreted axiomatic systems, hence as linguistic entities. To say that something is a linguistic entity is to imply that changes in its linguistic features, including the formulation of its axiom system, produce a new entity. Thus on the Received View, change in the formulation of a theory is a change in theory. However, scientific theories have different individuation properties, and a given theory admits of a variety of different full and partial formulations. The Semantic Conception of Theories construes theories as extralinguistic entities which admit of different full and partial formulations. In this respect, theories are individuated on the Semantic Conception the same way science individuates them.

The Semantic Conception gets its name from the fact that it construes theories as what their formulations refer to when the formulations are given a (formal) *semantic* interpretation. Thus 'semantic' is used here in the sense of formal semantics or model theory in mathematical logic.

On the Semantic Conception, the heart of a theory is an extralinguistic *theory structure.* Theory structures variously are characterized as set-theoretic predicates (Suppes and Sneed), state spaces (Beth and van Fraassen), and relational systems (Suppe). Regardless which sort of mathematical entity the theory structures are identified with, they do pretty much the same thing—they specify the admissible behaviors of state transition systems. For example, in the case of classical particle mechanics, a system has n bodies, each characterized by three position and three momentum coordinate variables; the simultaneous values at time t of these $6n$ variables determine the state of the system, and the laws of the theory specify admissible change-of-state patterns. If one represents the states as points in $6n$-dimensional spaces, the theory structure is construed as a configurated state space (van Fraassen). One also can construe the state transition structure as a relational system consisting of the set of possible states as a domain on which various sequencing relations are imposed (Suppe). By defining set-theoretic predicates (Suppes, Sneed), one can specify either a state space, a relational system, or some other representing mathematical structure or class of structures.

When one propounds a theory, one specifies the theory structure and asserts a theoretical hypothesis claiming that real-world phenomena (or a particular real-world phenomenon) stand(s) in some mapping relationship to the theory structure whereby that structure models the dynamic behavior of the phenomena or phenomenon. The laws of the theory do not specify what that mapping relationship is. This is in sharp contrast to the Received View which construes theories as the conjunction of law statements and correspondence rules which specify how the laws manifest themselves in observable phenomena. Thus the Received View's linguistic analogues to the mapping relationships of the Semantic Conception are individuating proper parts of theories, and so any alteration in the correspondence rules produces a new theory. In particular, since the correspondence rules, inter alia, specify experimental methods and measurement procedures, on the Received View any improvements or breakthroughs in experimental design or measurement result in the replacement of the existing theory by an improved replacement theory.[4] In actual scientific practice, however, theories are not individuated that way. Improvements in experimental procedures, new measurement techniques, and the like, do not automatically alter the theory they are used with. On the Semantic Conception such experimental and measurement techniques are used to mediate or assess the mapping relationships asserted to hold between theory structure and phenomena, but are not specified by the theory structure and so are not individuating features of theories.[5] Thus, again, here is an instance where individuation of theories on the Semantic Conception corresponds to actual scientific practice, but individuation on the Received View does not.

Various versions of the Semantic Conception differ as to what is the strongest mapping relationship that can hold or be established between theory structures and their corresponding phenomena. Van Fraassen (1980) uses his version of the Semantic Conception to argue for what he calls an *antirealism* with respect to theories. In the chapters which follow I will develop a *quasi-realistic* version of the Semantic Conception. The differences here between van Fraassen and me concern the physical interpretation of theory structures on the Semantic Conception; and we take different stands on the issue of scientific realism—an issue which is highly controversial today. Thus the development of the Semantic Conception and the debates over scientific realism have become intertwined, and one cannot go very far developing the former without confronting the latter.

This book is concerned with developing a quasi-realistic version of the Semantic Conception of Theories. This requires taking positions on the contemporary scientific realism debates and also making progress in developing a quasi-realistic epistemology of science. These will be dealt with at length in subsequent chapters; the remainder of this chapter will provide a brief background to them and preview how later chapters contribute to the overall argument.

I. ORIGINS OF THE SEMANTIC CONCEPTION

Since this book focuses primarily on my own version of the Semantic Conception, it seems appropriate to say something about the other main versions of it and how my version relates to them. I think the most illuminating way to do this is to begin with a sketch of the history of the Semantic Conception's origins and the motivations behind its development.

During World War II, Evert Beth became increasingly dissatisfied with "the increasing discrepancy between science and philosophy, which is conspicuously demonstrated by the rejection, by well-known philosophers—such as H. Dingler, P. Hoenen, J. Maritain—, of fundamental conceptions quite unanimously accepted by men of science."[6] He viewed this as "one of the main causes of the downfall of contemporary philosophy which is manifest e.g. in existentialism, and which seriously menaces the future development of Western civilization" (Beth 1949, 178–79). In response to such developments and their antecedents in the history of philosophy which he traced back as far as Aristotle, Beth concluded that "a philosophy of science, instead of attempting to deal with speculations on the subject matter of the sciences, should rather attempt a logical analysis—in the broadest sense of this phrase—of the theories *which form the actual content of* the various sciences. The Semantic method, which was introduced by A. Tarski towards 1930 and which quite recently has been extensively studied by R. Carnap, is a very great help in the logical analysis of physical theories" (ibid., 180; italics added).

Combining the formal semantic techniques of Tarski and Carnap with von Neumann's (1932) work on the foundations of quantum mechanics and the work of Strauss (1938), Beth proposed a semantic analysis of Newtonian and quantum mechanics.[7] For Newtonian mechanics he postulated systems having states under the governance of Hamiltonian equations, introduced atomic statements of physical measurement, and defined how the state transition structure satisfies these atomic statements. He also gave the semantic interpretation for a consequence relationship as well as a similar Schrödinger equation treatment for quantum mechanics, which he used to provide a commentary on the Einstein-Podolsky-Rosen paradox. He characterized his analyses as not proposing new axioms of logic and said that it "only aims at clarifying and systematizing the methods by which physicists infer consequences from given or supposed observational data" (Beth 1949, 183–84). He was quite clear about the relevance of these structures to issues of quantum logic. So far as I know, the only subsequent work he produced on the Semantic Conception after his 1948 and 1949 publications was a very brief conference paper in 1961 containing nothing new.

The next important figure in the development of the Semantic Conception was Patrick Suppes. Suppes studied mathematics, physics, and meteorology as an undergraduate at the University of Chicago; he received a B.S. in the last subject in 1943 while in the activated army reserves. After the war he went to graduate school in philosophy at Columbia University, where he was influenced by Ernest Nagel more than anyone else. As a graduate student he continued to study mathematics and physics, including topology, group theory, and relativity theory. Of particular importance were the lectures in group theory and topology given by Samuel Eilenberg—a mathematician who came out of the same Polish logic school Tarski did. The elegance with which he used axiomatic methods throughout the course had a strong impact on Suppes. Equally important in impact was an informal graduate seminar on von Neumann and Morgenstern's theory of games. "Von Neumann's axiomatic approach, again, appealed to me, and I learned a great deal about how to think in axiomatic terms. Certainly there was little in my undergraduate education that taught me much about the axiomatic method" (Suppes, letter to author, May 21, 1987). Also influential was Ernest Nagel's excellent course on logic where everything was formulated with great care and rigor.

The axiomatic and foundational elegance Suppes enjoyed in these seminars was missing from the many physics courses he took as a graduate student. In these courses he found himself

> always searching for some way to put things in a completely organized, intellectually satisfactory way. . . .
>
> I can remember very well trying to organize the lectures in quantum mechanics I heard as a student. I typed the notes and made every effort to put them in as logically a coherent fashion as I could. I did not try to give them an axiomatic formulation. This was certainly much too difficult a task at this stage of my education and in terms of time I had available, but I was searching for some kind of foundational security. [ibid.]

Suppes enjoyed L. H. Thomas' course on general relativity, but nevertheless found it intellectually disturbing.

> Thomas was not a good lecturer but knew everything there was to know at the time about the subject. He distributed notes which were in one sense clear and detailed but also completely nonaxiomatic. I appreciated the clarity of the notes but also, in contrast to the lectures in mathematics I was attending, I very much wanted to see what an axiomatic formulation would look like. I think it was my dissatisfaction with the rather casual approach to foundations in a systematic way in physics that pushed me to want to do a dissertation on the axiomatic foundations of some branch of physics. [ibid.]

He decided to write a dissertation about the philosophy of physics, wanting to give an axiomatic treatment of some branch of physics.

Such a dissertation was not to be written:

> 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 work 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. [Suppes 1979, 6]

While not done in a serious axiomatic fashion, there was an attempt to state axioms in an informal way.

Arriving at Stanford to teach philosophy, Suppes "became acquainted with J. C. C. McKinsey, a logician who recently had 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." Suppes also attended Tarski's seminar at Berkeley. "It was from McKinsey and Tarski that I learned about the axiomatic method and what it means to give a set-theoretic analysis of a subject" (ibid., 8).

Soon after his arrival at Stanford, Suppes combined the set-theoretic methods of McKinsey and Tarski with his desire, stifled at Columbia University, to do axiomatic studies in the foundations of physics. With McKinsey, A. C. Sugar, and H. Rubin, he published a number of papers that tried to do rigorous set-theoretic axiomatizations of existing branches of physics that had minimal sets of independent axioms (e.g., McKinsey and Suppes 1953; McKinsey, Sugar, and Suppes 1953; Suppes 1957, 1959; Rubin and Suppes 1954). "I really felt what I wanted to do in the dissertation was completed with the publication of my 1959 paper 'Axiomatics for Relativistic Kinematics [with or] Without Parity,' [Suppes 1959] in which I derived from very simple assumptions the Lorenz transformations" (1987 letter). In 1957, Suppes, Tarski, and Leon Henkin organized a symposium on the axiomatic method with special reference to geometry and physics (Henkin, Suppes, and Tarski 1959); Suppes was especially responsible for organizing and suggesting the speakers on the axiomatic foundations of physics. He also did work on the foundations of quantum theory, initially working from the von Neumann 1932 and Mackey 1963 approach, but later moving away from it because he became conceptually dissatisfied with it from a probabilistic perspective (Suppes 1979, 12). By 1959 Suppes had begun to shift his attention away from the concentration on axiomatic foundations of physics because he had come to view the subject as too special to put all of his energies into it—physicists were not much interested in the subject, and "very few philosophers had the background, training, or interest to be enthralled by the subject" (1987 letter).

Around 1950 Suppes had become interested in learning theory. In 1955, while a fellow at the Center for Advanced Studies in the Behavioral Sciences, he began working with William K. Estes on mathematical learning theory. This led to a rather extended period of doing psychological research on learning theory with Estes and Richard C. Atkinson (Suppes 1979, 26–29). "My interest in axiomatic methods did not stop, as can be seen from some of my work in learning theory at the time, but I did swerve away from the concentration on the axiomatic foundations of physics" (1987 letter).

The combination of his set-theoretic work on axiomatic foundations and especially his hands-on experimental research in mathematical learning theory led Suppes to write several papers which generalized from this work to theses about the nature of models and theories. In Suppes 1961 he distinguished three types of models in the sciences—what I call metamathematical models, iconic models, and the "theory" sense of model—and illustrated their similarities and differences with examples from his own and others' work. The thrust of the paper "was to ask was it possible to have the same kind of foundational clarity about the use of the concept of model in science that is possible in mathematics, when one had adopted Tarski's viewpoint. I tried to say that fundamentally the answer is yes" (1987 letter).

Suppes 1962 is a generalized analysis of how the mathematical learning theories he was developing related to phenomena in the context of his own experimental designs and setups for testing his, Estes', and Atkinson's theories. Instead of following the correspondence rule approach of the positivists, he introduces a hierarchy of models and theories semantically related to each other. As Suppes later explained it,

> The models of data paper was an effort to combine my interest in axiomatic foundations with the problem of understanding how to think about analysis of experimental data in a more formal way. The thrust here was not to give an alternative to mathematical statistics as applied to experiments but rather to ask what is the

> foundational framework within which one comes to experimental data ready to do appropriate statistical analysis. The models of data paper was an effort to show how much more is involved and what a high level of abstraction is required already at the level of data before any statistical analysis is appropriate. I also [tried] to bring out how issues of experimental design enter already. . . at an abstract level at the formulation of models. [1987 letter]

In a related paper, "What Is a Scientific Theory?" (1967), Suppes argued for the legitimacy of presenting theories via modeling rather than axiomatic methods and contrasted the approach emerging from his earlier papers with that of the Received View.

Joseph Sneed was a student of Suppes. Early in the 1960s he had decided not to complete a dissertation in physics at the University of Illinois but instead to get a doctorate in philosophy. After some graduate work in philosophy at Illinois he went to Stanford and worked with Suppes. His dissertation was a study of the structures of mathematical physics that exploited Suppes' axiomatic approach. He expanded his dissertation into his 1971 book. It had as one of its foci an attempt to characterize the nature of theoretical terms divorced from the positivistic observational/theoretical distinction—a problem posed by Putnam (1962). Sneed's solution utilizes correspondence rules, and so it seems to be far more positivistic in spirit than Suppes'.

The next work on the Semantic Conception to appear after Suppes' pieces and Sneed's dissertation was my dissertation (1967), which developed a version of the Semantic Conception. I now will try to sketch the main influences on my development of that version. Between high school and college, and during my undergraduate summers, I worked for General Electric Flight Test at Edwards Air Force Base on the research and development of jet engines. I began as an instrumentation technician, building, installing, and calibrating electronic and other data-gathering equipment. I witnessed the equipment progress from crude photo panels to fifty-channel oscillographs, from telemeters to airborne hundred-channel multiplexed pulse code–modulated digital tapes. Thus I was involved in state-of-the-art experimental research on military and civilian jet engines and aircraft, as well as playing a limited role in the work on Atlas missiles and the first flight of the NASA X-15 manned rocket.

Because I was in the later stages of obtaining a degree in mathematics, my last two summers at G.E. found me assigned to the new computer operation. Those were the very early days of using computers in military aircraft research and design, and we were pioneering experimental research uses for such machines as the IBM 650, the IBM 704, and the state-of-the-art IBM 709 and 7090. I worked on practical flight test data reduction problems and consulted on projects such as designing the control system for the LEM simulator, but I spent the bulk of my time doing exploratory and feasibility studies on three problems: (1) When we lost reference channels, could we use regression analysis to computer-generate dummy replacement data that would enable us to salvage our test data? (This involved fitting curves in up to one hundred-dimensional spaces, which was fairly ambitious for those days.) (2) Conversion of analog data (e.g., from flow meters) to digital involved electronic filtering, where the filters were chosen on a trial-and-error basis. Was it possible to develop an on-line, computer-generated dynamic filter which would exploit Norbert Wiener's "smoothing of time series" work (Wiener 1949) to produce a near-optimal transform function in a self-correcting manner? (3) Could we reduce the frequency of test flights by developing computer models that would simulate the operation of jet engines under a variety of circumstances and could be used as an alternative method of evaluating proposed design changes in, say, the core and annulus of a J-79 afterburner? The first problem involved representing dynamic systems as curves in hyperspace; the last two involved viewing phenomena as state transition systems. My colleague, who set me working on these problems, was Brandon Finney, a mathematician who had been a student of Reichenbach's; through him I learned a lot of Reichenbach's work. Thus, prior to graduate study in philosophy, I had already had an extensive background in engineering research—including instrumentation, measurement, and data reduction—and also had done a lot of work on dynamic computer modeling of physical phenomena. Through Wiener's work (1961, 1950) I had substantial exposure to the early developments in cybernetics, and they excited and influenced me a lot.

I was an undergraduate at the University of California at Riverside, which then was a small experimental honors campus. My degree was in mathematics, but I also studied physics, chemistry, biology, and philosophy. David Harrah had been my Western Civilization discussion leader, and I started taking courses in philosophy from him because he was a great teacher. I took a theory of knowledge course that paid a lot of critical attention to positivism (but not to philosophy of science), symbolic logic, an independent studies course on Toulmin 1953, and a course called "philosophy of science" from him. Harrah was a logician who had been an undergraduate student of Suppes at Stanford and a graduate student of Fitch at Yale. He was in the process of developing his erotetic logic and an associated formal model of information exchange and evaluation in the communication process (Harrah 1963). This work was introduced in some of his courses and had obvious links with my modeling interests at G.E.—it excited me very much. The philosophy of science course was actually a course in axiomatic set theory, taught out of the second part of Suppes 1957, and it presented Suppes' approach to the axiomatic foundations of theories. Before graduation I was fairly familiar with his approach and the papers he, McKinsey, Rubin, and Sugar had done, as well as the Henkin, Suppes, and Tarski (1959) volume resulting from their symposium on the axiomatic method.

Unable to decide whether to be a mathematician, philosopher, computer scientist, or marine biologist, I went to graduate school in philosophy because I liked it, it was fairly easy, and philosophy of science afforded me the opportunity to combine my interests—in effect, saving me from having to choose what I wanted to be. (Computer science departments did not really exist in 1962, though there were a few graduate programs.) I chose the University of Michigan at Ann Arbor because its philosophy department was top-rated and Arthur Burks was there. He was a philosopher of science and had been a co-inventor of the ENIAC (the first electronic computer) and, with von Neumann and Herman Goldstein, had designed the first stored-program computers and laid the basis for computer programming languages (Burks, Goldstein, and von Neumann 1946). He has continued to make important contributions to computer science and philosophy to the present.

It seemed I could combine my diverse interests rather well at Michigan. There I took about 40 percent of my course work outside philosophy. I was particularly influenced by work in metamathematics and recursive function theory, foundations of mathematics with Raymond Wilder, automata theory studies with Burks in computer science, and work with John Holland on adaptive systems theory. Both the adaptive systems theory and the automata theory added richness and abstract structure to my earlier state transition modeling ideas from G.E. I also began to look at the Fisher-Kimura natural selection theory in biology from the state transition perspective as incorporated in Holland's adaptive systems work. And, I studied more traditional philosophy as well; of particular importance were Abe Kaplan's Kant course and many other courses in the history of philosophy.

My philosophy of science graduate course work was limited. I took a course called "philosophy of science" from Burks, which was based on the then-current draft of what became the first half of his *Chance, Cause, Reason* (1977), and so was a course in the foundations of probability theory and inductive logic. (I later worked on the book as a research assistant.) I sat in on a seminar Abe Kaplan gave on his new book on the philosophy of social science (1964), and I assisted Arnold Kaufman in a philosophy of social sciences course that used Nagel against Skinner. But I never systematically studied the then-dominant positivistic philosophy of science.

When it came time to do a dissertation, my old interests at G.E. predominated, and I proposed a dissertation on models in science with Burks as director. The thesis was organized around Suppes' (1961) three kinds of models and was to characterize and compare metamathematical, iconic, and "theory" models. In the end it concentrated mostly on theories. I formed my committee: Burks as chairman, Abe Kaplan, the mathematical biologist and game theorist Anatol Rapoport, and Joe Sneed, who had just come from Stanford.[8] After the committee was appointed and the thesis prospectus approved, I devoted my time to taking courses covering more adaptive systems theory, more logic, some anthropology, linguistics, and Sanskrit; learning quantum theory on my own from various books including von Neumann 1955, Mackey 1963, and Messiah 1961 with Burt Moyer of the Berkeley physics department serving as tutor; studying on my own other things like relativity theory, works in the history of science, and game theory economics from von Neumann and Morgenstern 1953; working with Burks in editing von Neumann's self-reproducing automata material after von Neumann's death and accompanying Burks in 1965 to India, where I finally wrote my dissertation in 1966.

Despite these competing activities, I thought a lot about my dissertation, especially about theories. I recall going to see Rapoport to try out the idea that "theories are just Turing machines"—which, if one allows for a wide enough class of extended probabilistic and deterministic Turing machines, is not that far off the mark as an analysis of theory structures. I thought it was a daring idea. Rapoport responded, "Of course," and went to the board and showed me how to construe a number of physical and biological theories as automata. It was exhilarating, as Rapoport had wonderful intuitions on the mathematical modeling of phenomena of all sorts. I also bounced ideas off Harrah back in California one summer.

My discovery that von Neumann's cellular automata model of self-reproducing automata intuitively was similar to his quantum theory model (both involve viewing their phenomena as fluid flows through space) was very exciting. Indeed, Birkhoff and von Neumann's (1936) treatment of observation spaces and phase space models of quantum theory was of crucial importance in developing my dissertation version of the Semantic Conception. Many features of my version of the Semantic Conception find their seminal beginnings there. These include the notion of phase spaces, the idea that theories idealize their phenomena, the notion of the logic of a theory, the interpretation of propositions as corresponding to subsystems of phase and observation spaces, and the notion of a physical system (also influenced by Margenau [1950]). Formal semantics study with David Kaplan was another important influence. He stressed the importance of formal models being faithful to the phenomena being modeled, and I learned not to let formal wizardry mask poorly thought out philosophy.

In the initial development of my version of the Semantic Conception, the main influences on my analysis were von Neumann's and Birkhoff's work, my simulation modeling experiences and work at G.E., Suppes' set-theoretic approach, the sharp distinction between syntax and semantic structures in formal semantics and associated set-theoretic techniques, and the automata theory/adaptive systems perspective, including Holland's treatment of the genetic theory of natural selection. My views on the physical interpretation of theories were strongly influenced by Suppes 1962 and, I suspect, to an extent by Toulmin 1953, which I had studied as an undergraduate.

One very strong belief of mine was that philosophy of science had to reflect actual scientific practice and theories. I don't recall anyone telling me this, though Abraham Kaplan might have; he certainly believed it, and it shows in his 1964 book. It certainly shows in Suppes' work (e.g., Suppes 1962). David Kaplan's cautions on irresponsible formalization, Rapoport's critiques of Richardson's war models, and Burks' careful attempts to use modal logic to model subjunctive conditionals in drafts of his 1977 work also contributed to my belief. In any case, I do know that I didn't know much positivistic philosophy of science, with its reconstructive perspective. I looked at a little of it (e.g., Braithwaite 1953, Carnap 1956, and Nagel 1961), but quickly concluded that it didn't have much to do with real science as I knew it.[9] So I wrote an eight-page polemic against what I knew of the existing positivistic philosophical literature on theories, dismissing its relevance to my dissertation (1967), and proceeded to ignore it after page 46 of a 396-page thesis. Only later did I come to study and appreciate that body of literature. I hope my later treatment of it (1974b) atones for my early brashness, although I am grateful that my committee let me get away with the polemical dismissal in my dissertation. Had they not, I might have been distracted from developing the Semantic Conception. Had I been indoctrinated into positivism, I doubt I would have been able to develop the Semantic Conception.

Sneed introduced me to Kuhn (1962). The way his work rang true to my scientific experiences, unlike positivistic accounts, excited me—I became a Kuhnian. Kuhn wasn't very clear about his metaphysics, but I read him as being in the tradition of C. I. Lewis (1929)—committed to a subjective idealism with intentional particulars being the subject matter of science. An embryonic version of this view informed the treatment of the physical interpretation of theory structures in my dissertation, and I tried to work out the subjective idealism in fairly fine detail in a series of lectures at Knox College in Galesburg, Illinois, in 1969. My eventual dissatisfaction with that detailed attempt, reinforced by some critical comments by David Schwayder, convinced me that subjective idealism was wrongheaded. I began to explore a realistic approach. In an extended series of weekly discussions with Wilfrid Sellars when he visited at Illinois, I began developing a correspondence theory of truth (Suppe 1973), a realistic approach to natural classification (see ch. 7 below), and a quasi-realistic physical interpretation for theories on the Semantic Conception (see chs. 3–5 below).

Bas van Fraassen is the final main developer of the Semantic Conception, and his version was initially stimulated by Beth. Van Fraassen had encountered Beth's 1959 book as an undergraduate at the University of Alberta and had become quite enamored with Beth. Over the next several years he read everything of Beth's he could get his hands on. He requested a copy of Beth's 1948 book and received it as a present on his birthday, April 5, 1965. During the academic year 1965–66, his last year as a graduate student at Pittsburgh, he taught a course at West Virginia University where he first wrote down a summary and explication of Beth's semantic view of theories.

In a letter (February 19, 1985), van Fraassen wrote me,

> I had read his *Synthese* article before, about the Einstein Podolsky Rosen Paradox, and had not properly understood it. It was I think after the book that I suddenly saw how his ideas actually worked and how they could be used to provide a semantics for quantum logic. During my last years of graduate school in Pittsburgh I presented a paper on Quantum Logic, which concentrated on the paper by Birkhoff and von Neumann. That paper I had already read and . . . [been] thinking about when I was an undergraduate, but had understood very imperfectly. I think probably that the main problem which exercised me and for which the semantic conception showed me the way out, was his question of how to provide a semantic analysis of quantum logic. There was a second factor, besides Beth's work that motivated me. I was writing my dissertation on the causal theory of time, and as part of that de-

> veloped a theory of events (I started with Reichenbach's theory, especially as given in his *Elements of Symbolic Logic*, but developed [a] somewhat different theory). Developing a theory of events when you're not allowed to use any spatio-temporal terms has special difficulties, and it was in connection with this that I mobilized the notion of logical space; reading that I then saw my views about logical space and its connection with modal logic (as developed in my "Meaning Relations among Predicates" *Nôus*, 1967) could be identified with the state space idea that was mobilized by Beth.

In that *Nôus* article, van Fraassen (1967) introduced the idea of a semi-interpreted language and announced that he intended to exploit those developments in the analysis of scientific theories in a subsequent paper. I read this, saw the relevance to my own work, and wrote to him. We corresponded, he read my dissertation, and then I visited him in New Haven, and we had extended discussions about the Semantic Conception of Theories. He then published his 1970 work, in which he put forth his extension and generalization of Beth's approach. In a footnote (325) he expressed his debt to me "for stimulating discussion" and said that my dissertation developed "a point of view closely related to Beth's." My recollection of those early exchanges is that they were exciting and stimulating, and that we both profited greatly from them, but that each of us had pretty well worked out his basic position prior to initiating contact. It was through van Fraassen that I first learned of Beth (1948, 1949) and saw the significance of his work.[10] In a number of subsequent works van Fraassen has made important further contributions to the Semantic Conception (e.g., 1972, 1974, 1980).

As I look over the foregoing history, I am struck by how heavily intertwined the lines of influence are and the extent to which the influences of von Neumann, Tarski, and set-theoretic approaches have dominated us. I am also struck by how little attention we paid to positivistic views during the formative stages of developing our views and the extent to which a concern with actual science predominates. For the most part, we did not pay much attention to the Received View until our own views were fairly well developed.

New developments along the lines of the Semantic Conception have continued. For example, the Kimura multiple-locus, multiple-allele generalization of Fisher's genetic theory of natural selection in evolutionary biology was one of my paradigm cases in developing my version of the Semantic Conception. A number of other philosophers have been active in utilizing and further developing the Semantic Conception in biological contexts; most of these efforts have focused on evolutionary theory, especially population genetics. For example, a long-standing issue in philosophy of biology has been whether evolutionary theory has genuine laws, for it is claimed that scientific laws must hold universally over all space and time, but that evolutionary laws seem to be specific to our planet or similar planets in similar solar systems.

John Beatty wrote a doctoral dissertation (1979), directed by Ronald Giere, which turned to the Semantic Conception to help resolve that issue. As an outgrowth of his dissertation, he published his 1981 work on the universality (or generality) issue and his 1980 work on the issue of the cognitive status of optimal design models in evolutionary biology. Central to his treatment of both issues is his claim that according to the Semantic Conception, "[t]heories do not consist of empirical claims, much less general, empirical laws of nature. . . .[The] empirical claims of science are not considered to be components of theories" (1980, 542–43). By my lights, this constitutes a serious misconstrual of the Semantic Conception (certainly I would deny these claims under my version of it), and so I have not found Beatty's efforts terribly convincing. (See Sloep and van der Steen 1987 for other criticisms of Beatty and see Beatty's 1987 reply.) Nevertheless, Beatty is correct that the Semantic Conception is capable of shedding light on both issues. In chapter 8, I show how my version of the Semantic Conception handles the generality issue so as to render evolutionary and other "local" biological theories genuine scientific theories with scientific laws. And developments in subsequent chapters allow me to use the Semantic Conception to characterize optimal design models as conceptual devices—which yields a superior defense of much the same account of optimal design models as the one Beatty attempted to defend on questionable grounds. The sort of account I would give is sketched briefly in the Epilogue.

Elisabeth Lloyd defended a dissertation at Princeton in October 1984 entitled, "A Semantic Approach to the Structure of Evolutionary Theory," which was directed by Bas van Fraassen. In it Lloyd looked at the special case modern evolutionary theory presents for questions about scientific explanation and theory structure, utilizing the semantic view of theories (as developed by Suppes, myself, and van Fraassen). In particular, she used it as a framework for analyzing some major topics in evolutionary theory, including population genetics, group selection, and species selection models. After publishing articles containing material from her dissertation (1984, 1987, 1988), Lloyd revised it and expanded it into book form (1988a).

Lloyd tends to use van Fraassen's state space, antirealist version. She shows how closely Semantic Conception state space models conform to the way evolutionary biologists such as Lewontin actually formulate population genetics. She also provides classifications for population genetics models, discusses the confirmation of ecological and evolutionary models, and analyzes the "unit of selection" controversy (whether the gene, the entire phenotype, or something else should be considered the unit of selection). Her book focuses especially on the structure of evolutionary theory, concentrating on population genetics, and contains chapters on group, kin, and organismic selection models, and other chapters on species selection and genic selection, the "unit of selection" controversy, and the confirmation of evolutionary theory. One merit of her treatment is the detailed attention paid to specific theories and models: Lloyd's book does not merely use the Semantic Conception, it furthers its development.

Sloep and van der Steen (1987) mount some rather bizarre criticisms of some of Lloyd's work which are rooted in part in the misguided supposition that the only function of a philosophical analysis of theories is to help scientists solve the scientists' own problems, and hence an analysis which coheres well with actual scientific practice has no philosophical contribution to make. Although there is little substance or merit to their objections, Lloyd's rejoinder (1987a) is not terribly effective.

While a graduate student in philosophy at the University of Toronto, Paul Thompson studied with van Fraassen and became familiar with his work on semi-interpreted languages (van Fraassen 1967), but not his exploitation of those ideas in his 1970 paper on the Semantic Conception. Around 1977, as Thompson began to work toward a dissertation in philosophy of biology directed by Thomas Goudge, he learned of the Semantic Conception from John Beatty, who was then writing his dissertation using the Semantic Conception. Around the same time, Thompson encountered some of Giere's work with the Semantic Conception of Theories and the brief description of it in my 1974b. This initial exposure led him to work through the literature, including van Fraassen's work, my dissertation, and my published writings (incorporated into the present volume). Thompson's work exploits details of my quasi-realistic version of the Semantic Conception to a greater degree than Lloyd's or Beatty's work does. His 1983 paper defends the superiority of the Semantic Conception over the Received View for foundational research in evolutionary biology and presents an analysis of Hardy-Weinberg law–based population genetics theories. Of particular note in his account is his view that laws of coexistence specify the sets of states of which a system is capable, and then laws of quasi-succession are used to specify state transition behavior or histories of members of a population. His defense of the superiority of the Semantic Conception includes the claim that it naturally corresponds "to the way in which biologists expand, employ, and explore the theory" (227). Further, it also provides a particularly clarifying perspective for approaching the debate between the classical and balance theories of population structure in evolutionary theory.

In his 1985 work, Thompson addresses controversies surrounding the testability of sociobiological theories, arguing that "a semantic conception of theories provides a more thoroughgoing analysis of this problem" than the Received View, because the Semantic Conception "more accurately represents the relation between a theory and phenomena" (201). In his 1986 work he discusses the roles of laws of interaction in evolutionary theory from the perspective of the Semantic Conception. Many of these themes are developed further in other works of his (1988, 1988a, n.d.) especially in his new book (1989).

Thompson's defense of the superiority of the Semantic Conception for foundational studies of evolutionary theory is criticized in Sloep and van der Steen 1987. His rejoinder (1987) is very much to the point in showing how seriously confused their understanding of the Semantic Conception is; it provides a convincing defense of Thompson's views—Sloep and van der Steen's reply (1987a) notwithstanding.

Kenneth Schaffner (1980) is of the opinion that evolutionary theory is quite untypical of biological theory, and thus philosophy of biology's focus on evolutionary theory as the paradigmatic biological theory is misleading and distorts our understanding of biological science. He argues that biological theories more typically resemble what he calls interlevel theories of the middle range. He has attempted to look at such theories from a number of perspectives including the Semantic Conception (see my treatment of them in chapter 8, where I argue that the Semantic Conception straightforwardly accommodates and illuminates such middle-range interlevel theories).

A number of other philosophers have attempted to analyze theories as set-theoretic predicates or structures; the best-known analysis is Sneed 1971. Moulines 1975 exploits Sneed's approach, and Stegmüller 1976 is an exposition of Sneed 1971 together with an attempt to exploit Sneed's analysis to provide an improved analysis of Kuhn's notion of incommensurability. (Sneed was involved in the development of Stegmüller's attempt.) This attempt seems to me somewhat bizarre, since a key idea of Kuhn's is the rejection of correspondence rules, and Sneed's analysis of theories retains certain explicit correspondence rules in the form of Ramsey sentences (used in Sneed's analysis of theoretical terms). By retaining some explicit correspondence rules, Sneed seems to concede far too much to positivism—indeed, far more than Kuhn would, or should, be happy with. Further, Stegmüller 1976 displays a cavalier disrespect for actual scientific practice—which, I'm afraid, is all too characteristic of this general approach. The problems Sneed and Stegmüller choose to address all too frequently seem to me to be philosophical impositions on the science rather than arising out of it. Sneed, Moulines, and Stegmüller have collaborated on various works relating to their semantic analysis of theories.

In Europe one finds other philosophers who are attempting to wed a set-theoretic structure approach to analyzing theories with a neopositivistic approach that employs explicit correspondence rules. Prominent among these are M. L. Dalla Chiara, G. Toraldo di Francia, Marian Przełecki, and Ryszard Wójcicki.[11] While I am uncertain of the historical background behind these attempts, my impression is that they essentially try to employ Tarski semantic techniques within a positivistic approach to theories. These approaches, like those of Sneed, Stegmüller, and Moulines, are sufficiently different in their treatment of the physical interpretation of theories from the approaches of Beth, Suppes, van Fraassen, and myself that it is appropriate to construe them as advancing a different analysis. For one of the key distinguishing features of the Semantic Conception of Theories, as developed by Beth, van Fraassen, Suppes, and myself, is the absence of anything like correspondence rules—and this absence is crucial to the way the individuation of theories is handled. Subsequently throughout this book, I will confine my attention to semantic conceptions which eschew correspondence rules as integral components of theories.

This concludes the historical account of the origins of the Semantic Conception of Theories. To follow the subsequent development of views of the main developers of the Semantic Conception who have been featured here, one may consult the philosophical literature. I now turn to other matters of background.

II. SCIENTIFIC REALISM

The first known scientific revolution arose over the replacement of Eudoxus' theory of celestial movements with Aristotle's. Aristotle objected to Eudoxus' account on the grounds that it did not make mechanical sense—that his mechanisms could not be the actual mechanisms producing the celestial motions. Aristotle figured out how to combine Eudoxus' various mechanisms for the specific planets (as reworked by Callippus) to produce a single mechanism responsible for all celestial mechanisms. The resulting mechanism had fifty-six nested crystalline spheres concentric on the earth, whose motions were driven by the outermost spheres (see Aristotle's *De Caelo* and *Physica*). Aristotle's theory replaced Eudoxus' and Callippus' despite the fact that the two theories were *provably* equivalent in their accounts of the apparent motions of the planets and stars.[12] Aristotle's theory won out because it was a realistic theory, whereas Eudoxus' was an instrumentalistic theory, and Aristotle had convinced people that adequate scientific theories had to be realistic.

The dispute between Eudoxus and Aristotle marks the beginning of the realism-versus-instrumentalism disputes that have surfaced repeatedly in the history of science. Such disputes were at the heart of Copernicus' rejection of Ptolemaic astronomy—Osiander's preface to *De Revolutionibus Orbium Coelestium* notwithstanding. And they played a central role in the atomism debates of the nineteenth century (see Gardner 1979), as well as issues surrounding the Copenhagen interpretation of quantum mechanics during this century. Even within logical positivism and the Received View, one finds realism-versus-instrumentalism disputes between, for example, Gustav Bergmann and allied behaviorists such as B. F. Skinner on the instrumentalist side, and Hempel and the later Carnap (1937 and onward) on the realist side. Hempel's "Theoreticians' Dilemma" (1958) is the classic positivistic statement of scientific realism.

Modern post-positivistic debates over scientific realism surround the work of W. V. Quine and Wilfrid Sellars and the ontic realisms they championed. Although Cornman ultimately came out in support of what he called "compatible common-sense realism," he challenged Sellars' brand of scientific realism (Sellars 1959, chs. 7, 12, 14, 15; 1963, chs. 2, 3; 1965) and in the process took instrumentalism seriously and gave it a good run for its money (see, e.g., Cornman 1975). The two of them had a number of go-arounds over scientific realism, and later (e.g., at the 1976 Philosophy of Science Association meetings) van Fraassen joined in championing an "antirealism" as opposed to an instrumentalism (Sellars 1977; Cornman 1977; van Fraassen 1977). Van Fraassen summarized his objections to Sellars' scientific realism in his 1975.

Van Fraassen's antirealism took on a life of its own and was challenged by Glymour and Boyd, among others (see Glymour 1976; Boyd 1973, 1976; van Fraassen 1976, 1980). Hesse (1977), Laudan (1981), and others have joined in the fray. The results are what MacKinnon (1979) calls "the New Debates" on scientific realism. MacKinnon's 1979 article and his earlier book (1972, 3–71), provide an excellent introduction to, and survey of, the older and newer modern debates over scientific realism (see also Leplin 1984).

It is not my purpose in this volume to attempt any systematic or comprehensive survey or critique of the recent or current controversies over scientific realism. My concern is limited to their impact upon the development of the Semantic Conception of Theories. Thus I am concerned primarily with van Fraassen's incorporation of his antirealism into his version of the Semantic Conception and with my own quasi-realistic version. In effect, my concerns with the scientific realism issue are twofold: I am interested in how theory structures are to be interpreted on the Semantic Conception and in learning what sorts of mapping relationships between theory structures and phenomena are epistemologically knowable. To my knowledge, Suppes has not taken a position on the scientific realism issue. MacKinnon (1979) considers the implications of Sneed's and Stegmüller's program of using Sneed's work on theories to rework Kuhn's view of science (Sneed 1971, 1976; Stegmüller 1976), and concludes

> the Sneed program, so interpreted, is neutral with respect to the problem of scientific realism. It can either accept or disregard the pre-critical realism implicit in functioning science. In neither case does the new method really treat the philosophical problems [concerning scientific realism] considered earlier. If this S-type formulation were to be generally accepted, however, it would certainly favor instrumentalism over realism. There remain, as I see it, serious obstacles in the way of such general acceptance: practical problems, stemming from the difficulties involved in reading Sneed and Stegmüller; theoretical problems concerning the use of Ramsey-reduction sentences and the instrumentalist interpretation of theories; and, perhaps, the most formidable difficulty of all, the Sneed-Stegmüller acceptance of Kuhn's descriptive account as an empirically adequate basis for the dynamics of theory development.[13]

Since I already have decided to exclude Sneed's and Stegmüller's approach from my consideration of the Semantic Conception, I will not pursue its possible implications for the scientific realism controversy any further.

An antirealism essentially involves the claim that there are epistemological limits to what can be known, and the physical interpretation of theories ought to be restricted to those which make theories knowable. Thus the mapping relations which constitute the physical interpretations of theories on the Semantic Conception should be restricted to ones that can be known. Actually, a realist can accept this line of reasoning, too, as the primary difference between a realist and an antirealist is over how much can be known. Van Fraassen's antirealism is based on the adoption of the following empirical adequacy condition for theory acceptance: All of the measurement reports of phenomena are to be isomorphic to one of the models of the structure (van Fraassen 1976).

Van Fraassen (1980) characterizes scientific realism as follows:

> Scientific realism is the position that scientific theory construction aims to give us a literally true story of what the world is like, and that acceptance of a scientific theory involves the belief that it is true. Accordingly, antirealism is a position according to which the aim of science can be well served without giving such a literally true story, and acceptance of a theory may properly involve something less (or other) than belief that it is true. [9]

Expanding on this, he says

> The idea of a literally true account has two aspects: the language is to be literally construed; and so construed, the account is true. This divides the antirealists into two sorts. The first sort holds that science is or aims to be true, properly (but not literally) construed. The second holds that the language of science should be literally construed, but its theories need not be true to be good. The antirealism I shall advocate belongs to the second sort. [10]

Van Fraassen's above adequacy conditions for theory acceptance are a reflection of his antirealism. If one accepts his characterizations of realism and antirealism,[14] then the position I defend in this book is an antirealism of the first sort he mentions. Theories typically are formulated in formulae or sentences that are in the indicative mood, but on my version of the Semantic Conception they are interpreted as giving a counterfactual (subjunctive) characterization of phenomena. (This is done via the mapping relationship between theory structure and phenomena, which is counterfactual.) Thus theories are not literally true. Further, although science can and does aim to accept theories as nonliterally but counterfactually true, often theories are propounded as conceptual devices whose epistemic claims are weaker—for example, that the theory is a simplification or a promising first approximation worth pursuing, and the like.

While my position thus fails to qualify as a scientific realism, instead qualifying as an antirealism on van Fraassen's characterizations, it seems to me that the spirit of my enterprise is decidedly realistic and that what I am defending is a slightly attenuated kind of realism. Thus I call the position I defend a *quasi-realism*. Since my quasi-realism is for van Fraassen an antirealism and since his arguments against scientific realism do not favor his second sort of antirealism to the exclusion of the first sort, which I support, I will not rehearse or critique his arguments. Rather, I will concentrate on the positive development of his *constructive empiricism* form of antirealism. The plausibility of this strategy is enhanced when one notes that while van Fraassen argues against scientific realism and in favor of his antirealism, he virtually ignores the first sort of antirealism (including the sort of quasi-realism I will develop and defend here) and does not give it serious consideration in his arguments. For example, in discussing the "conjunction of theories" objection to antirealism, he maintains that "if one believes both T and T' to be true, then of course (on pain of inconsistency) one believes their conjunction to be true" (1980, 83). Granting that this is true for realists,[15] it generally will be false for antirealists of the first sort: It is not generally true that the conjunction of two nonliterally true theories is itself a nonliterally true theory.[16]

In barest outline, van Fraassen's antirealism maintains that

> *[s]cience aims to give us theories which are empirically adequate; and acceptance of a theory involves a belief only that it is empirically adequate.* This is the statement of the anti-realist position I advocate; I shall call it *constructive empiricism.* . . . In addition it requires an explication of 'empirically adequate'. For now, I shall leave that with the preliminary explication that a theory is empirically adequate exactly if what it says about the observable things and events in this world, is true—exactly if it 'saves the phenomena'. A little more precisely: such a theory has at least one model that all the actual phenomena fit inside. I must emphasize that this refers to *all* the phenomena; these are not exhausted by all those actually observed nor even by those observed at some time, whether past, present, or future. [van Fraassen 1980, 12, italics original]

This adequacy condition determines the strongest mapping relation between theories and phenomena allowed by his version of the Semantic Conception and thus delimits the extent of theoretical scientific knowledge. For "what the antirealist decides to believe about the world will depend in part on what he believes to be his, or rather the epistemic community's, accessible range of evidence" (18).

The persuasiveness of van Fraassen's attack on scientific realism ultimately depends in large part on the cogency of his constructive empiricism. At the very heart of the latter is his observable/nonobservable distinction (van Fraassen 1980). The burdens this distinction carries are great indeed:

> To accept the theory involves no more belief therefore than what it says about observable phenomena is correct. [57]

> To the antirealist, all scientific knowledge is ultimately aimed at greater knowledge of what is observable. [31]

> To be an empiricist is to withhold belief on anything that goes beyond the actual, observable phenomena, and to recognize no objective modality in nature. To develop an empiricist account of science is to depict it as involving a search for truth only about the empirical world, about what is actual and observable. . . . [I]t must involve throughout a resolute rejection of the demand for an explanation of the regularities in the observable course of nature by means of truths concerning a reality beyond what is actual and observable, as a demand which plays no role in the scientific enterprise. [202–03]

> . . . according to constructive empiricism, the only belief involved in accepting a scientific theory is belief that it is empirically adequate: all that is both actual and observable finds a place in some model of the theory. So far as empirical adequacy is concerned, the theory would be just as good if there existed nothing at all that was either unobservable or not actual. Acceptance of the theory does not commit us to belief in the reality of either sort of thing. [157]

These quotations pretty well exhaust what van Fraassen has to say about the import of the observable/nonobservable distinction for constructive empiricism.

Van Fraassen has surprisingly little to say when it comes to precisely characterizing what it means to be observable, perhaps because he believes that "if there are limits to observation, these are a subject for empirical science, and not for philosophical analysis" (1980, 57). He does tell us that

> [a] look through a telescope at the moons of Jupiter seems to me a clear case of observation, since astronauts will no doubt be able to see them as well from close up. But the purported observation of micro-particles in a cloud chamber seems to me a clearly different case—if our theory about what happens there is right. . . . [W]hile the particle is detected by means of the cloud chamber, and the detection is based on observation, it is clearly not a case of the [p]article's being observed. [16–17]

And in discussing the hermeneutic circle, he says:

> To delineate what is observable, however, we must look to science—

> and possibly to that same theory—for that is also an empirical question. This might produce a vicious circle if what is observable were itself not simply a fact disclosed by theory, but rather theory-relative or theory-dependent. It will already be quite clear that I deny this; I regard what is observable as a theory-independent question. It is a function of facts about us *qua* organisms in the world, and these facts may include facts about the psychological states that involve contemplation of theories—but there is not the sort of theory dependence or relativity that could cause a logical catastrophe here. [57–58]

Van Fraassen's statements suggest that what is observable is what can be seen with unaided vision; this is how most critics and commentators in Churchland and Hooker 1985 interpret his position. For van Fraassen this observable/nonobservable distinction is supposed to mark a fundamental epistemological distinction. In chapter 11 I will establish that, so drawn, the observable and the nonobservable are equally problematic epistemically, and so they mark no fundamental epistemological distinction. Churchland (1985), Glymour (1985), and Gutting (1985) urge the same conclusion. Given the deep affinities between my arguments in chapter 11 and the criticisms that Churchland (1985) raises on the basis of a thought experiment concerning a man for whom absolutely nothing is observable (42–43), it is clear that none of the rejoinders van Fraassen (1985) offers in rebuttal to these various objections constitutes an effective rejoinder to the criticism that will be raised later in chapter 11. Thus if the "unaided vision" reading is the proper interpretation of van Fraassen's position, we have every reason to reject his constructive empiricism—hence his antirealism.

We thus turn to the consideration of other possible interpretations of van Fraassen's observable/nonobservable distinction that might enable him to salvage his constructive empiricism. Despite the claims of the passage last quoted, a considerable amount of text by van Fraassen (1980) suggests that his observable/nonobservable distinction *is* a theory-dependent one:

> [1] Nor can the limits [of what is observable] be described once and for all. . . . To find the limits of what is observable in the world described by theory *T* we must inquire into *T* itself, and the theories used as auxiliaries in the testing and application of *T*. [57]

> [2] . . . the empirical import of a theory now is defined within

science, by means of a distinction between what is observable and what is not observable drawn by science itself. [81]

[3] . . . what counts as an observable phenomenon is a function of what the epistemic community is (that *observable* is *observable-to-us*). [19; italics original]

[4] Not only objectivity, however, but also observability, is an intrascientific distinction, if the science is taken wide enough. [82]

[5] For science itself delineates, at least to some extent, the observable parts of the world it describes. Measurement interactions are a special subclass of physical interactions in general. The structures definable from measurement data are a subclass of the physical structures described. It is in this way that science itself distinguishes the observable which it postulates from the whole it postulates. The distinction, being in part a function of the limits science discloses on human observation, is an anthropocentric one. But since science places human observers among the physical systems it means to describe, it also gives itself the task of describing anthropocentric distinctions. It is in this way that even the scientific realist must observe a distinction between phenomena and the transphenomenal in the scientific world picture. [59]

These five claims by van Fraassen virtually exhaust what he has to say by way of characterizing observability; typically these claims are asserted rather than defended. Based on such statements, Wilson (1985) interprets van Fraassen as holding an "internalist" theory-laden view of observation. Van Fraassen (1985) rejects this interpretation, saying that he wants "to draw a sharp distinction between the use of science to help delineate what is observable, and Wilson's program, which can issue only . . . in a theory-relative notion of observability" (304–05). Unfortunately, his rejoinder leaves unclear what sense we are to make of the passages above.

I did not find a coherent viewpoint in the above five quotations. The first quotation suggests two distinct interpretations:[17] (1) What is observable is theory-dependent and is determined by a given theory *T* alone, and (2) What is observable is theory-dependent and is determined by a given theory *T* and any auxiliary theories used to test and apply *T*. Although the third quotation suggests that this combination of *T* and its auxiliaries might be construed as the body of accepted theory and background knowledge accepted by a scientific community, the fourth quotation (among others) suggests a third interpretation: (3) What is observable is determined by science in some quite broad but unspecified sense of the term 'science'.

The first interpretation regarding the observable/nonobservable distinction is just false, since many theories have nothing to say about what is observable—especially in the anthropocentric ways outlined in the fifth quotation. The second interpretation is more plausible, but it is incompatible with a key doctrine of van Fraassen's antirealism—namely, that anything nonobservable is a gratuitous metaphysical addition. In other words, "there is no general physical difference to which it corresponds," and recourse to such nonobservables is to be avoided. On the contrary, in a very straightforward sense, it plausibly can be maintained that there are components of theories that correspond to nothing observable which nevertheless affect the sorts of auxiliary hypotheses that can be used in conjunction with the theory. For example, Ralph Kenat has argued that Planck's and Einstein's different stances with respect to Planck's blackbody radiation "permitted different auxiliary hypotheses to be employed; only if we accept the corpuscular nature of light does it make any sense to employ the conservation of momentum in analyzing the interaction of light and electrons."[18] Now, if the auxiliary hypotheses are part of what determines what is observable, as the second interpretation indicates, then one can get different observability boundaries depending on what nonobservable components one tacks onto one's theories. Van Fraassen has no basis for objecting to tacking on such nonobservable components, so long as one does not believe in them. This leaves open, then, the possibility that what is observable, hence what is empirically adequate, depends on nonobservables incorporated into one's theories. This surely is unacceptable to van Fraassen.

The way to block this possibility is to modify (2) to restrict the auxiliary hypotheses which determine observability to those that depend only on observable theory constituents for their applicability. However, doing so would seem to be incompatible with the ways in which the boundaries of observability in fact are expanded in science. For example, in the Planck-versus-Einstein blackbody radiation case, Einstein's assumption that light is in fact propagated in individual particles was about nonobservables originally, but it allowed the use of auxiliary hypotheses, which led to predictions that Compton could then experimentally test. Compton's experiments established the truth of Einstein's hypothesis and thereby expanded the range of what was observable to include light being propagated in individual particles.[19] To restrict auxiliary hypotheses to observables alone would preclude such developments and thus is incompatible with the ways science in fact does determine what is observable. Therefore, the second interpretation is unacceptable.

The third interpretation (expressed in the fourth quotation) is unacceptably vague. What constitutes this science "taken wide enough"? Van Fraassen gives us no idea, and so this interpretation is difficult to assess. Moreover, van Fraassen gives no arguments in support of the notion that science decides what is observable.

Shapere (1982), however, has provided arguments in support of this claim, if observability is construed as direct observation. On the other hand, he also argues that current notions of direct observation in astrophysics make observation involving human receptors be a special case of the more general analysis. Indeed, observation generally involves interactions between the observable phenomena and suitable detectors, with a given branch of science defining what a suitable detector is. Such a view enjoys some affinity with van Fraassen's anthropocentric interpretation (in the fifth quotation), but if this is what van Fraassen has in mind, note that much of what empiricists and antirealists have objected to as unreal fictions now qualify as observable. His notion of observability is indeed extremely wide now, seemingly designed primarily to exclude entities such as the space-time manifold in relativity theory from the class of observables.

More generally, van Fraassen's observability notion seems to become the idea that what is observable is simply a function of the measurable interactions. Let us distinguish between (a) those measurable interactions that in fact are possible in the world and (b) those that are believed to be measurable interactions by, say, a scientific discipline. Under the third interpretation, (b) seems to represent his position, but elsewhere he has made such claims as "observation is a special species of measurement" (1980, 59), which suggests that not all measurable interactions are observations. More importantly, he claims,

> When the hypothesis is solely about what is observable, the two procedures [acceptance as true and empirical adequacy] amount to the same thing. For in that case, empirical adequacy coincides with truth. [72]

> I would still identify truth of a theory with the condition that there is an exact correspondence between reality and one of its models. [197]

Van Fraassen thus seems to be claiming under (b) that what is observable is what is true in a correspondence sense, and thus that what is true in a correspondence sense depends on what are *believed* to be measurable interactions by a scientific discipline. This is incoherent, though, for what is true in a correspondence theory does not depend on scientific belief. What a scientific discipline believes to be true in a correspondence sense is a doxastic, not a correspondence, matter; and what is believed may be constrained by what the discipline believes to be measurable interactions or to be observable. But this is a position quite unlike (b); indeed, it is compatible with (a). In a fundamental sense, what is *observable* is determined by the actual real-world measurable interactions which enable observers to detect effects of things or processes.

This fundamental limitation imposes an ultimate boundary on scientific knowledge. One cannot know what one is incapable of interacting with, even in torturously remote and involved ways. I can, and do, accept something like this as a fundamental limitation on scientific knowledge, which my quasi-realism must accept—call this *ultimate observability*. By contrast, on the present reading, van Fraassen tries to limit science not to knowledge of the ultimately observable, but rather to what presently is believed to be observable—to the *doxastically observable*. However, the boundaries of the doxastically observable are variable, changing, and expanding over much of the history of science. The Planck-Einstein–Compton case sketched earlier is fairly typical: By taking seriously what is not doxastically observable, we thereby expand the limits of what is doxastically observable. Thus the doxastically observable is not a fundamental epistemic limit on science—contrary to van Fraassen's antirealistic position. Moreover, although it does strongly constrain what a scientific discipline will accept as empirically true, it does so in ways that allow one to test the correctness of statements about the doxastically nonobservable. Without this, the limits of doxastic observability cannot expand; thus van Fraassen's position makes the limits imposed by doxastic observability too severe.

I conclude that van Fraassen has not coherently presented his observable/nonobservable distinction. None of the plausible readings is satisfactory for his purposes and capable of escaping my arguments in chapter 11, while being faithful to actual scientific practice—thus the case for his constructive empiricism and antirealism is far from convincing. The world imposes limits of ultimate observability, which do constitute limits on what science can know. Science, of course, does not presently know, and may never fully know, what these limits are, but at a given time a scientific discipline's body of accepted belief defines a range of phenomena that it is confident its methods do enable it to know. Whatever the limits of science are, they probably are at least as broad as that. By seriously countenancing the reality of things that presently do not qualify as knowable, a scientific discipline is capable of expanding its ranges of doxastic observability and of phenomena it confidently claims to know. I think something very like this is a correct picture—it is a more plausible picture than van Fraassen's antirealistic one, and it allows for a more robust science than his constructive empiricism does. Indeed, it is a picture that my quasi-realism embellishes and enriches. In the chapters that follow I will develop it as an alternative to van Fraassen's antirealistic version of the Semantic Conception of Theories.

Finally, there is an important point to be made about philosophical method. Suppose van Fraassen were correct that empirical science need not countenance or pay attention to the doxastically nonobservable in theorizing about or explaining phenomena, or in any of its other characteristic activities, and suppose that empirical adequacy is the closest approximation to truth that science need concern itself with. From this it does not follow that an antirealism is correct. While it is the case that philosophical analyses are empirical in the sense that actual scientific theories or practices can refute them, it does not follow that scientific realism and the antirealisms are themselves empirical scientific theories. Indeed, the problems philosophical analyses attempt to solve typically are not scientific problems; thus there is no guarantee that what is adequate for doing science is adequate for doing philosophy of science.

To provide an adequate philosophical analysis of science—to explain and provide an understanding of what science does and why it has the epistemic or other philosophical attributes claimed for it—may require philosophical theorizing about things that science itself need not countenance. For example, let us grant that scientific realism concerns what is ultimately observable and that this typically is distinct from what a branch of science deems doxastically observable and is typically more expansive in scope. In order to defend a realism or antirealism, the issue of ultimate observability must be addressed, as it was, for example, in the discussion of the Einstein-Planck-Compton case. Or, granting that empirical adequacy is sufficient for science to do its job, defending that claim may require recourse to a stronger correspondence notion of truth that relates to ultimate observability.

The specifics of these examples are not important. What is important is the point that the ontic commitments of working science may be different than—indeed may be weaker than—the ontic commitments that philosophy of science must make in providing *its* analyses and answering *its* questions. And the issues of scientific realism really concern what sorts of ontic commitments are essential to a *philosophical* understanding of science, not what sorts of ontic commitments are essential to the scientists in going about the business of science. Thus, for example, the argument that a correspondence notion of truth is of no help to working scientists, since they can only assess truth on the basis of coherence with data and a body of background belief and theory accepted by their scientific discipline—hence that working scientists can eschew a correspondence notion of truth—says nothing whatsoever about whether a correspondence notion of truth has a substantive role to play in a philosophical analysis from the perspective of scientific realism or quasi-realism. To think otherwise is simply to confuse philosophy of science with science.

III. PLAN OF THE BOOK

My quasi-realistic version of the Semantic Conception employs no observational/theoretical or observable/nonobservable distinction (though it does respect the limits of ultimate observability). In chapter 2 I use a consideration of the Received View as a foil to argue that anything like the positivistic observational/theoretical distinction is unnecessary. I do so under the guise of evaluating how successful Achinstein's and Putnam's attacks on the Received View have been. The chapter, inter alia, argues that their attacks on partial interpretation miss the mark. The model-theoretic analysis of partial interpretation I give there (sec. I) is divorceable from the positivistic observational/theoretical distinction, and when so divorced it has a minor technical role to play in the developments of chapters 4 and 5. Chapter 2, section III, introduces a heuristic version of the Semantic Conception which is intended to be fairly intuitive.

That intuitive version of the Semantic Conception is developed in the next three chapters (chs. 3–5). Chapter 3 introduces the basic components of theories and explores the semantic relations between the linguistic formulation of theories and theory structures, physical systems, and phenomena. In the process, the chapter also argues against the operational imperative. Chapter 4 explores more fully the relations between theories and phenomena, including the roles of observation, measurement, experimental design, and standard inductive confirmation techniques. Chapter 5 investigates a number of different kinds of theoretical laws, including laws of succession, interaction, and coexistence; teleological and functional laws; and laws of quasi-succession. In the process, it is argued that approximate truth notions are unnecessary for understanding the truth status of laws and theories on the Semantic Conception.

An important test of a philosophical analysis is its ability to deal successfully with problems and issues that were not key to its development—that is, its robustness. The robustness of the Semantic Conception is extensively explored throughout the book. Chapters 3 and 5 consider the implications of the Semantic Conception for the operational imperative and Scriven's view that laws are fundamentally inaccurate. Chapters 6 through 9 further test the power of the Semantic Conception by examining its implications for a range of other issues. Chapter 6 focuses on theoretical explanation. Chapter 7 takes on issues about what makes a biological taxonomy natural instead of artificial, presents a solution, and explores the relationships between theories and taxonomies on the Semantic Conception. Chapter 8 explores the robustness of the Semantic Conception by showing how it can accommodate what Schaffner calls interlevel theories of the middle range in the biomedical sciences—theories which appear to be quite different in structure than what philosophy of science typically has considered. Chapter 9 shows how the Semantic Conception's treatment of teleological laws (in ch. 5) enables one to better understand public scientific controversies (such as the one concerning nuclear power) which become entangled in moral and public policy disputes, and it also sheds light on engineering applications of scientific theories. The ability of the Semantic Conception to deal productively with all these issues adds to the evidence supporting it. Since its treatment of each of these issues turns centrally on features of its quasi-realistic treatment of the physical interpretation of theory structures, these chapters provide considerable support for adopting that quasi-realistic version rather than some weaker antirealist or instrumentalist one.

In developing his antirealist version of the Semantic Conception, van Fraassen (1980) maintains that "philosophy of science is not metaphysics" (82) and that "we cannot settle the major questions of epistemology *en passant*" (19). In passing, no—but the quasi-realistic version of the Semantic Conception developed in chapters 2 through 5, and elaborated and applied in chapters 6 though 9, has taken a number of stands that have significant metaphysical and epistemological implications. In particular, it imposes rather strong constraints on what sort of epistemology of science one can adopt in conjunction with it. There are two main approaches one can take here: One can develop a realist or quasi-realist view that has such implications and then try to finesse them—as Glymour tries to do in his *Theory and Evidence* (1980). Alternatively, one can view these implications as incurring an obligation to develop and defend an epistemology/metaphysics that must be met. I prefer the latter approach.

Many years ago Carnap commuted once a week by train from Chicago to a university some hours away to guest-teach a graduate seminar. A young faculty member sat in on Carnap's seminar and soon sought him out. He told Carnap that he was very interested in the problem of induction and hoped it would be possible to meet with Carnap some time during the semester and discuss it. Carnap listened, then asked, "Tell me, Mr. ______, do you have your own theory of inductive logic?" The young professor answered, "No, but I'm very interested in induction and would like very much to discuss it with you." Carnap replied, "Well, Mr. ______, when you have your own theory of inductive logic, I'd be more than happy to discuss induction with you." Carnap's reply to the young professor simply was that it was pointless to discuss induction if you don't have a position. And for him, to have or defend a position was to develop a theory of inductive logic.

My quasi-realistic version of the Semantic Conception commits me to a kind of position on some basic epistemological issues. Carnap's view on having a position on induction is generalizable to the issue of scientific realism: To take a position on scientific realism, especially to defend a realism or quasi-realism, is to make some fairly strong metaphysical and epistemological commitments. To defend them adequately requires developing an epistemology and associated metaphysics in fair detail. Chapters 3 and 7, inter alia, present key metaphysical and ontological assumptions which underlie my quasi-realism and use these to sketch a correspondence account of factual truth and empirical truth for theories (based on my 1973 article). The treatment of natural versus artificial biological taxonomies in chapter 7 provides a general quasi-realistic construal of the difference between natural and merely artificial classification. Thus it provides the *antinominalism* which van Fraassen (1989) so rightly insists must underlie any plausible realistic construal of science. Chapters 10 through 13 are intended to be a fairly detailed prolegomenon to such a fully developed epistemology, and the issues taken up there should give a good idea of the epistemological lines I wish to take in defending my quasi-realistic version of the Semantic Conception. The introductory section to part IV provides an overview of the specific contents and objectives of each of those chapters.

To me, the question of scientific realism is how strong an epistemology can be and still be defensible. I do not believe much progress can be made by the recently fashionable practice of delimiting an ever-growing variety of realisms, antirealisms, instrumentalisms, and so on, characterizing them in a few sentences or paragraphs, and then mounting abstract arguments for or against each. Far more appropriate, it seems to me, is for the various proponents to attempt to develop in fine detail epistemologies that are faithful to their realist, quasi-realist, antirealist, or instrumentalist beliefs.

The strongest defense I know of for a scientific realism or quasi-realism is to develop a realistic or quasi-realistic epistemology that works in fine detail and proves robust in solving problems other epistemologies and philosophies of science have floundered on. This is precisely the strategy I take in chapters 10 through 13 where I develop an underlying epistemology for my quasi-realistic version of the Semantic Conception of Theories. I welcome criticisms from those who do not have their own competing epistemologies, but, like Carnap on induction, I think the most productive debates will be with those who have developed their own epistemologies. For the viability of a realism or a quasi-realism, or any other approach, ultimately depends on the *details* of how knowledge is analyzed and handled. And it is precisely those sorts of details that contemporary debates over scientific realism too often ignore—and must ignore if the protagonists do not have their own detailed theories of knowledge.

The Epilogue attempts to place the development of the Semantic Conception into the larger picture of scientific theorizing and explores the connection between developments here and other issues in the philosophy of science. Problems about scientific theories and theorizing needing further research are indicated.

NOTES

1. For an extensive discussion of the Received View, see Suppe 1974b, 3–118, and Suppe 1977a, 619–32. See also ch. 2 below.
2. For example, Beatty 1979, 1980, 1981, 1982; da Costa and French n.d.; Edelson 1984; Giere 1988; Hardegree 1976; Hausman 1981; Horan 1986; Lloyd 1984, 1987a, 1988, 1988a; Moulines 1975; Sneed 1971; Schaffner n.d.; Stegmüller 1976; Thompson 1983, 1985, 1986, 1988, 1988a, 1989, n.d.; Wessels 1976; as well as various writings of its main developers (e.g., Beth, Suppes, van Fraassen, and myself).
3. For the distinction, see Suppes 1957, 232.
4. A few versions of the Received View do not identify theories with their linguistic formulations, instead requiring that theories be such that they can be given canonical linguistic formulations meeting the requirements of the Received View (see Suppe 1974b, 57–62). On such versions it still remains the case that the content of the correspondence rules are individuating components of the theory, and so substantive changes in correspondence rules produce new theories.

5. Sneed (1971) and Stegmüller (1976) maintain that in some instances such measurement procedures are individuating characteristics of theories and in others they are not. Specifying when they are is a central task of their analysis and is offered as a solution to the "problem of theoretical terms" (theoretical terms, roughly, being those terms or variables whose measurement presupposes the very theory in question). Their theoretical terms exclude most of what the positivists counted as theoretical terms. Later I exclude such approaches from the Semantic Conception.

6. Beth 1949, 178. Beth 1949 is a journal article in English summarizing his 1948 book, which has never been translated from the Dutch.

7. He also cites Destouches 1942 as influential.

8. By the time I had done much work on my thesis, Sneed had left Michigan for Stanford. He was replaced by Richard Boyd on my committee. I vaguely knew about Sneed's thesis, though I never looked at it then, and I do not think his thesis had much influence on my development of the Semantic Conception. In particular, I was not in residence at Michigan in the winter of 1966 when he gave a course of lectures on his dissertation, and did not learn of that series of lectures until recently. But Sneed *was* influential in turning me on to Kuhn 1962—which he still is high on—and in bringing Mackey 1963 and Messiah 1961 to my attention. Around the summer of 1964 he arranged for me to meet Suppes at Stanford; from that contact I gained access to a prepublication version of Suppes 1967, which influenced my dissertation a lot.

John Holland was not on my committee (the philosophy department objected to the number of nonphilosophers I wanted on it, just as it objected to the amount of course work I wanted to do outside philosophy), but he served unofficially and was extremely helpful to me.

9. Specifically, its treatment of correspondence rules bore no resemblance to my experience at G.E. with how theories related to phenomena.

10. I had seen Beth's 1961 work previously, but it hadn't said much to me then.

11. See Dalla Chiara 1976; Dalla Chiara Scabia and Toraldo di Francia 1973; Przełecki 1969 and 1976; Wójcicki 1974, 1974a, and 1976.

12. Aristotle preferred Callippus' version but actually provided his replacement versions for both Eudoxus' and Callippus' theories. See Aristotle *Metaphysics* Λ, 8. Since Callippus' versions were refinements of Eudoxus', it is standard to refer to it as Eudoxus' theory.

13. MacKinnon 1979, 518. See also Feyerabend 1978.

14. In ch. 11 I will argue for a reconstrual of the scientific realism issue that implicitly rejects these characterizations of van Fraassen.

15. Since on a realistic construal of quantum theory, such a principle does not hold for statements in general, it is not clear that it holds for theories on a scientific realism.

16. These difficulties are discussed in chs. 4 and 5.

17. The following analysis and line of objection is based in part on the ideas of my student Ralph Kenat.

18. Ralph Kenat, unpublished paper, "Some Critical Problems with van Fraassen's *Scientific Image*," written for a seminar I gave on the Semantic Conception.

19. It certainly made light being propagated in individual particles part of the phenomena, and for van Fraassen the phenomena are just the observables (1980, 56). Thus it follows that it is observable. Michael Friedman (1983a) has argued that assessing the reality of the space-time manifold in general relativity theory allows for a richer range of inductive inference that affects the testability of the theory. It appears that his line constitutes another example that could pose problems for the second theory-dependent interpretation of van Fraassen's account of observability.