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THE STRUCTURALIST VIEW OF ECONOMIC THEORIES: A REVIEW ESSAY

The Case of General Equilibrium in Particular

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INTRODUCTION

Within contemporary philosophy of science there are many competitive approaches. Out of these different approaches those associated with the names of Kuhn, Lakatos, and Popper are the most familiar to economists. Even economists who have never written a word on "methodology" are often familiar with the central thesis of each of these metascientific views. Despite this familiarity, there are some approaches to the philosophy of science which remain relatively unknown to the economics profession. One such school is the "structuralist" approach associated with the work of Joseph Sneed and Wolfgang Stegmüller. The relative obscurity of this school among economists seems unfortunate since advocates of the structuralist view have, more than any other group of philosophers, actually applied their approach to economics.[1]

The purpose of this paper is to appraise the structuralist view of economic theories. The particular emphasis will be on general equilib-

1. These applications include Balzer (1982a,b), Diederich (1982b), Garcia de la Sienra (1982), Händler (1980a,b, 1982a,b), Haslinger (1982,1983), Kötter (1982), Pearce and Tucci (1982a,b), and Sneed (1982).

rium theory, which for reasons described below has received much of the structuralist attention. A positive appraisal of the program is in order if structuralist philosophy of science can help us better understand economic theory and/or if the behavior posited by the program is actually the behavior manifested by practicing economists. This is particularly important in the area of general equilibrium theory, where the metascientific theories most popular among economists do not seem to fit (or explain) the actual practice of economists very well at all. If the structuralist view "fits," if it can answer some of the methodological questions left unanswered by other metascientific views without creating new ones of its own, then a positive appraisal will be in order. If not, the program should be negatively appraised.

The paper is arranged as follows. The first two sections contain a rather long but necessary summary of the structuralist view. Section I is concerned with the *static* aspects of the program; that is, how the structuralist view characterizes scientific theories; while section II is concerned with the *dynamics* of theory change and "progress." Section III provides a structuralist reconstruction of Walrasian pure exchange general equilibrium theory. This reconstruction serves both as a concrete example of the presentation in the first two sections and as a reference for the rest of the paper. Section IV surveys some of the arguments in favor of the structuralist view, both with respect to economics in general and general equilibrium theory in particular. In section V several criticisms are discussed, again, of both a general and specific nature. The conclusion presents our final appraisal.

THE STRUCTURALIST PROGRAM: STATICS

For the purpose of this paper the "structuralist program" will mean the view of scientific theory structure and theory dynamics initially presented in Joseph Sneed's (1971) book on mathematical physics. This program has been further articulated by Wolfgang Stegmüller in his two books (1976a and 1979) as well as in numerous papers by Sneed, Stegmüller, and other authors.[2] In addition to Sneed's original application to mathematical physics, the program has been applied to equilibrium thermodynamics (Moulines, 1975) and certain biological theories (Beatty, 1980; Lloyd, 1984) as well as economics.[3]

2. For example, Balzer and Moulines (1980), Balzer and Sneed (1977), Moulines (1975, 1976, 1979, 1983), Sneed (1976, 1983), and Stegmüller (1975,1976b, 1978).
3. In this paper the term "structuralist" will mean *only* the *strict structuralist* view of Sneed, Stegmüller, and their school. This view is only one (restrictive) interpretation of the more general "semantic approach" to scientific theories, and many philosophers who are sympathetic to the more general semantic approach (e.g. Bas van Fraassen and F. Suppe) are in disagreement with many aspects of the structuralist

The structuralist program represents a significant break from the traditional view of scientific theories. The traditional approach characterizes scientific theories as sets of statements. These statements may be either true or false, and they may be corroborated or falsified on the basis of their relationship to the empirical data. According to the structuralist view, this traditional characterization is inappropriate and has contributed to the recent conflict between analytical philosophers of science and the advocates of a more historical approach (Kuhn, Feyerabend, etc.). The fundamental claim of the structuralist program is that scientific theories should be viewed as *structures* rather than as statements. These theoretical structures are related to their empirical claims by certain systematic logical relationships. Specifying these logical relationships is one of the fundamental research goals of philosophers working in the structuralist program.

Since the identifying structural characteristics of a scientific theory are most obvious when it is presented in its simplest and most concise form, the first step in a structuralist reconstruction of any scientific theory involves *axiomatization*. When the theory is presented in a formal axiomatized way the essential mathematical structure of the theory is manifest and the logical relations between the axioms of the theory and the sentences which constitute its empirical claims can be examined. While there are several ways to axiomatize a scientific theory, the structuralist school advocates the method of set-theoretic axiomatization, a tool which was originally applied to scientific theories by Patrick Suppes.[4] Because set-theoretic axiomatization is a fundamental part of the program, the structuralist approach has thus far been restricted to fairly mathematical scientific theories. For instance, in economics structuralist reconstructions have tended to focus on only the most mathematical fields – general equilibrium theory and Sraffa-based formal Marxian models. While most of its advocates express hope that the program can be extended to less mathematical theories, there is the candid admission that such extensions are a long way off.[5] With this

program. Arguments in favor of the more general semantic approach may be quite different from the arguments in favor of the structuralist view presented in Section IV, and this more general view is in no way indicted by the criticism in Section V.

4. It is not argued that set theory is the only possible formal framework which could be used, only that it is the most "practical" (Stegmüller, 1979, p.6). The traditional philosophical tool of first-order predicate calculus is available, but to use it for reconstructing real scientific theories is just "not humanly possible" (Stegmüller, 1979, p.5). While many economists may find structuralist set-theoretic reconstructions of major economic theories to be unnecessarily (or perhaps prohibitively) formal, it should be noted that only informal set theory is used. A formal language reconstruction would be significantly more complex.
5. "In the near future no more than a very fragmentary realization of this program will be possible, restricted to theories whose basic mathematical structure is sufficiently

brief introduction we turn to the details of the structuralist view of scientific theories.

The most fundamental proposition of the structuralist program is that a scientific theory *merely defines a predicate.* For example, the scientific theory T might define the predicate "is a P." The predicate "is a P" characterizes the formal structure of the theory T and it is true of only those entities which have this structure. Note that unlike the traditional view of scientific theories, a theory in the structuralist sense is not a statement about the behavior of empirical entities. In fact, theories are not statements at all. On this view it does not make sense to ask if a theory is true or false; theories just define predicates.

The simplest way to generate an empirical claim from the theory T is to formulate an "empirical hypothesis" of the form "x is a P," where x is simply the name of something which can be empirically described. For example, Newtonian mechanics defines the predicate "is a Newtonian system." A possible empirical hypothesis of the Newtonian theory might be "x is a Newtonian system" where x is "our solar system." As an economic example, the simple Keynesian income-expenditure theory (KT) could define the predicate "is a Keynesian economy."[6] A possible empirical hypothesis of the simple Keynesian theory might be "the United States economy in 1960 was a Keynesian economy." Such empirical hypotheses are simply empirical statements which may be either true or false.

Since Sneed and Stegmüller insist on set-theoretic axiomatization, the type of predicates defined in structuralist reconstructions are *set-theoretic* predicates. This simply means that the type of entities which the predicates are true of (or refer to) are set-theoretic entities, i.e., sets. Since sets may be composed of elements which are dissimilar in all but one respect, this set-theoretic characterization is extremely general. A particular theoretical predicate may denote (or apply to) concrete physical things like solar systems and billiard balls, or it may denote purely mathematical abstractions such as systems of differential equations. In terms of the previous economic example, if we say the simple Keynesian income-expenditure theory (KT) defines the predicate "is a K" (where K is a Keynesian economy), then the sentence "x is a K" may be true when x is a set of concrete objects such as a particular economy at a particular time, or where x is simply a set of algebraic equations (or both). While this referential flexibility has certain advantages, we shall see below that it is also a source of potential difficulty for structural reconstructions of formal economic theories.

clear to allow axiomatizations which meet the demands for rigor in the sense of Suppes" (Stegmüller, 1979, p.28). Many theories are not "mathematically elaborate enough to be clear candidates for structuralist reconstruction" (Sneed, 1983, p.361).

6. A structuralist reconstruction of this theory is presented in Händler (1982a).

Based on the logical/mathematical meaning of the term "model," the members of the class of entities for which "is a K" is true, are called *models* for KT. Thus, if some economy x satisfies the axioms of theory KT, i.e., if "is a K" is true of this x, then it is appropriate to say that x is a *model* for KT. The economist's claim that "the behavior of the United States economy in 1960 was accurately described by the simple Keynesian income-expenditure theory" is, in structuralist terms, simply that "x is a model for KT," where x is "the United States economy in 1960." Note that with the structuralist characterization, the empirical business of science is not "testing theories," but rather "looking for models."[7]

Having introduced the models of a theory as things which satisfy the axioms of the theory, it is now necessary to introduce the notion of a "possible model." Possible models (or potential models) for the theory T are things for which it makes some sense to attempt to apply T, things for which "is a P" could conceivably be true. Possible models are things that have at least enough structural similarity to the theory in question to qualify as "possibly" being models. Again returning to the above economic example, "the United States economy in 1985" is a *possible* model for KT; i.e., it may or may not be true that the United States economy in 1985 is accurately described by the simple Keynesian income-expenditure theory, but at least it makes sense to ask the question. On the other hand, "my cat Casey" or even "the United States automobile industry" are not possible models for KT. More formally, possible models are set-theoretic entities that can be characterized in the language of the theory, things that could conceivably be models. Of course, not all possible models *are* actually models. Thus, if the set of all models for some theory T is symbolized as $M(T)$, and the set of all possible models for this same theory is symbolized as $M_p(T)$, then we have the relation $M(T) \subseteq M_p(T)$. In general, scientists working in a particular theory T would only concern themselves with elements of $M_p(T)$, and the empirical activity of these scientists would consist of checking to see if various elements of $M_p(T)$ are also elements of $M(T)$.

In the above discussion there were several references to "finding out" or "checking to see" if "x is a P," but nothing has been said regarding how one would actually go about testing such claims. In fact, very little has been said regarding the *specifically empirical nature* of scientific theories at all. Everything presented so far could apply equally well to a purely mathematical theory. A mathematical theory

7. This use of the term *model* (while standard in mathematical logic) can be extremely confusing to economists since it is *not* the way economists normally use the term. As pointed out by Hausman (1981a, p.46), what most economists mean by "model" is actually what Sneed and Stegmüller call a "theory" (and to a certain extent vice versa).

could certainly be axiomatized to define a set-theoretic predicate.[8] A mathematical theory could also have possible models; these might include systems of equations, sets of numbers, geometric objects, matrices, or some other type of mathematical entities. And finally, these possible models might actually be models, i.e., they may obey the axioms of the theory. As Stegmüller (1979, p. 7) makes clear, "Informal set theory can be used equally well to define the predicate 'is a group' and to define the predicate 'is a classical particle mechanics.'" Surely something more must be added if we are to characterize *scientific* theories and "not only their mathematical skeletons" (Stegmüller, 1979, p. 13).

Explication of the *essentially empirical* element in the structuralist view of scientific theories requires the introduction of two additional concepts, the concept of "T-theoreticity" and the concept of a "set of intended applications." Both of these concepts are absolutely fundamental to the structuralist conception of the relationship between scientific theories and the empirical entities they "are about," and yet both concepts are problematic in various ways. We will consider "T-theoreticity" first and then examine the "set of intended applications." Any problems associated with these concepts will be deferred until Section V.

Consider a statement of the form "x is a P," where $x \in M_p(T)$. Structuralist philosophers of science argue that it is not possible to test this statement even if x appears to be an empirically describable entity as long as the description of x requires the use of *T-theoretical* terms. Structuralist authors define a term as "T-theoretical if all methods of determining its value presuppose that *this theory* T holds true in some of its intended applications" (Stegmüller, 1978, p.43). Testing such a sentence will always result in either a "vicious circle" or an "infinite regress." This problem is what the structuralist school calls the *problem of theoretical terms*.[9] It is extremely important to the school that this problem be solved since they claim "that every nontrivial physical theory T contains not only T-nontheoretical terms, but in addition, T-theoretical ones" (Stegmüller, 1979, p.9, and similar remark, p.87).

The most frequently cited examples of such T-theoretical terms are "force" and "mass" in classical particle mechanics. "We cannot discover the values of the mass and force functions to use in checking the claim for any application, unless we presuppose that the claim for some . . . application is true" (Sneed, 1971, p.38). As an economic example of the problem of theoretical terms consider the previously

8. Sneed (1971, pp.9–10) uses the example of group theory, which defines the predicate "is a group."

9. Sneed (1971, p.38), Stegmüller (1975, p.76; 1976b, p.151; 1978, p.43; 1979, p.21).

mentioned Keynesian income-expenditure theory (*KT*). The simple Keynesian theory predicts that there are forces in the economy which will cause the value of aggregate output (GNP) to be equal to (neglecting foreign trade) the sum of expenditures on consumption C, investment I, and government spending G. Suppose we desired to test this prediction for the current U.S. economy, or in structuralist terms, suppose we wanted to test the proposition that the current U.S. economy is (at least in this respect) a model for the Keynesian theory. We would find that this proposition is impossible to test. The reason is that the U.S. national income accounts were set up explicitly on the basis of the Keynesian theory. Gross national product, consumption, investment, and government spending are actually *defined* in such a way that the equation GNP $= C + I + G$ will always hold. This equation is *KT*-theoretical with respect to the Keynesian theory; it is a "law" which cannot be tested because the way we measure the entities involved presupposes that it is true.

The structuralist method of getting around the problem of theoretical terms is to consider only the set of *partial possible models*. The set of *partial* possible models (M_{pp}) is derived from the set of possible models (M_p) by "throwing out" (Stegmüller, 1979, p.22)[10] the T-theoretical terms from the descriptions of elements in M_p.[11] "Intuitively M_{pp} contains all structures which can be described in (or conceptualized in) the nontheoretical vocabulary of the theory" (Balzer, 1982b, p.19). Thus each element of M_{pp} (each $x \in M_{pp}$) is something which could conceivably be a model for the theory *and* can be described without using any T-theoretical terms.[12] By considering only $x \in M_{pp}$ as candidate models (or possible "instances" of the theory), the vicious circularity proposed by theoretical terms can be avoided. Stegmüller (1975, p.76) claims that this approach is the "only way out of this difficulty known at the present moment."

The problem of theoretical terms is certainly *one* barrier to an adequate structuralist characterization of the empirical nature of scientific theories. If every attempt to confirm a sentence of the form "x is a P" entails a vicious circle, then such claims cannot be considered empirical hypotheses.[13] The elimination of T-theroetical terms from such sen-

10. Or "cutting off" (Stegmüller, 1975, p.83), or "lopping off" (Stegmüller, 1979, p.25; Sneed, 1971, p.166; 1976, p.123).
11. In the technical language of the structuralist program this amounts to replacing each sentence "x is a P" with its corresponding "Ramsey-sentence."
12. "Roughly, M_p is the set of all possible models of the *full* conceptual apparatus of the theory including theoretical components, while M_{pp} is the set of all models obtained by simply 'lopping off' the theoretical components leaving only the non-theoretical part of the conceptual apparatus" (Sneed, 1976, p.123).
13. ". . . we call a statement empirical only if it is empirically testable" (Stegmüller, 1979, p.17).

tences (accomplished by considering only $x \in M_{pp}$) is certainly a *necessary* condition for such hypotheses to be empirical. But is it also sufficient? Can we say that "x is a P" is an *empirical* claim of the scientific theory T just because $x \in M_{pp}$, that is, just because x can be described without resorting to T-theoretical terms or functions?

The answer to the above question is no. One reason is that restricting attention to $x \in M_{pp}$ does not prevent x from being an entirely abstract (mathematical) entity rather than a concrete physical (or economic) entity. For instance, suppose we consider only $x_0 \in M_{pp}(KT)$ for the simple Keynesian income-expenditure theory discussed above. Does this guarantee that x_0 is a real economy? No, not necessarily; x_0 could be contained in $M_{pp}(KT)$ and thus be described without resorting to KT-theoretical terms, but still be a purely abstract entity such as a system of equations. The structuralist solution to this problem is to restrict the claims of the theory to elements of the *set of intended applications* (or interpretations).

The set of intended applications I is simply the set of concrete (as opposed to purely abstract/mathematical) entities which the theory "is about"–it contains only those "parts of reality to which we want to apply our theoretical apparatus" (Balzer, 1982b, p.20). At any particular moment in the development of a theory, I is an "open" set (Stegmüller, 1979, p.12) which is "loosely specified, but not completely unspecified" (Sneed, 1976, p.120). For instance, the original set of intended applications for Newtonian mechanics included such things as our solar system, the tides, free-falling bodies, and the motion of pendulums. For such a physical theory, the set of intended applications contains only *physical systems*.[14] For economic theories the set I would contain only the appropriate *economic* entities. The previously described theory KT would have a set of intended applications which contains particular economic systems at particular times; the theory of monopoly, on the other hand, would have a set of intended applications which contains only various firms. Since the set I contains only real-world entities, restricting the claims of the theory to elements of the set I circumvents the problem of characterizing a theory which only applies to abstract entities.

14. While the set I (of a physical theory) is said to contain only "physical systems," the members of the structuralist school are ontologically noncommittal regarding exactly what a "physical system" is. Sneed (1971, p. 183) is quite candid in this regard. "We have not, however, attempted to say what sort of things are admissible as intended interpretations of theories of mathematical physics–what sort of things physical systems are" on this point Sneed has "virtually nothing to say." The reason for this neglect is probably the same as the reason for neglecting to explain what "observational evidence" is. "I do this, not because I believe the notion to be unproblematic, but only because I have nothing to add to the solution of the problems connected with it" (Sneed, 1971, p.265).

The one restriction which is imposed on the set of intended applications is that its members "must, at least, have the same mathematical structure as the members of the set of possible applications" (Sneed, 1971, p.183). This means that the set of intended applications must be a *subset of the set of partial possible models* (that is, $I \subseteq M_{pp}$). The motivation for this restriction is that intended applications are described in "pretheoretical" or "everyday" vocabulary which is not necessarily the same as the non-T-theoretical vocabulary that describes the members of M_{pp}. Thus it is necessary to "'reconceptualize' the intended applications described in 'pretheoretical' language as models for the nontheoretical structures of the theory,"[15] i.e., to redescribe each $x \in I$ so that it is a member of M_{pp}. Generally "these redescriptions will not be unique" (Sneed, 1983, p.362).

According to the structuralist view of scientific theories the set of intended applications is a fundamental part of the theory itself. Instead of thinking of "the theory" and (separately) of those things in the world that might be used to discover if the theory is true (as in the traditional view), the structuralist view includes the set I in the basic definition of the theory. According to the structuralist program *a scientific theory is simply the ordered pair* $\langle C,I \rangle$. In this ordered pair, C is the *core* of the scientific theory and I is its set of intended applications. The core C is itself composed of three parts: the set of models of the theory (M), the set of possible models of the theory (M_p), and the set of partial possible models of the theory (M_{pp}). Thus we can write

$$T = \langle C,I \rangle = \langle M, M_p, M_{pp}, I \rangle.$$

Since each of the components of C is discussed above they are not individually reexamined here. It should only be noted that M, M_p, and M_{pp} capture the *essential structural characteristics* of the theory and define the fundamental predicate of the theory "is a P." The core C is related to the set of intended applications I by the previously discussed relation $I \subseteq M_{pp}$.

We are now in a position to state explicitly the structuralist view of the relationship between the logical structure of a scientific theory and its empirical claims. The theory $T = \langle C,I \rangle$ makes empirical claims in the following way. For any $x \in I$ we say that *"x is a P" if and only if there exist T-theoretical terms which can be added to the description of x such that x becomes a model for T*. To interpret this, recall that since $I \subseteq M_{pp}$, we have for each $x \in I$ that $x \in M_{pp}$. But each element of M_{pp} was defined by "lopping off" the theoretical terms from an element of M_p. Thus, if we could "add back" the theoretical terms to x (theoretically enrich x) we

15. Sneed (1976, p.361).

would obtain an $\bar{x}$ such that $\bar{x} \in M_p$. Now recall that $M \subseteq M_p$, i.e., the actual models of theory (the things for which the theory is actually true) is a subset of the possible models of theory (M_p). Therefore it *may* be that $\bar{x} \in M$; that the theoretically enriched element of I is actually a model for the theory. If such an enrichment is possible and $\bar{x} \in M$, then we say the empirical claim "x is a P" is true, and thus x constitutes a concrete application (or instance) of the theory.[16,17]

II. THE STRUCTURALIST PROGRAM: DYNAMICS

Thus far our discussion has been concerned with only the *static* aspects of the structuralist view of scientific theories. The last paragraph of the preceding section summarized the structuralist view of the relationship between the formal structure of a scientific theory and its empirical claims, but absolutely nothing was said regarding *progress* or *theory change*. How do advocates of the structuralist program view such dynamic questions? Certainly the structuralist program *must* address such dynamic questions if it is to be an influential voice within the philosophy of science. Since the early 1960s philosophy of science has become increasingly *historical* in approach and increasingly dynamic in its scientific concerns. The prominent figures within recent philosophy of science (Kuhn, Lakatos, Popper, etc.) have all, in one way or another, been concerned with the dynamics of theory change. Stegmüller in particular, claims that the structuralist program is attractive precisely because it promises "to bridge the gap between the historically and the systematically oriented approaches" (Stegmüller, 1976b, p.148).

In discussing scientific theory change it is often useful to distinguish between *evolutionary* and *revolutionary* theory change. Intuitively we know that evolutionary change should entail only a local distur-

16. Some structuralist authors (Balzer, 1982b, p.21 for instance) make the more global claim that *if* such theoretical enrichments exist *for all* $x \in I$ then it is appropriate to say that the theory itself is true. Such interpretations are consistent with Sneed's (1971, pp.69–71) "holistic" position that any scientific theory has *only one big empirical claim* about the set I, rather than a number of individual claims about the elements of I (as we have presented the structuralist view). While the structuralist view actually leaves open the question of the generality of empirical claims, we will restrict our discussion to the (possibly generalizable) case of individual empirical claims.
17. In order to describe an empirical claim of a theory T a bit more formally we can define e: $M_{pp} \rightarrow M_p$ as the *extension mapping* for the theory T. For each $x \in M_{pp}$ the function e maps x into the set of all members of M_p which have "the same configuration of non-theoretical functions" (Sneed, 1971, p.168) as x. Thus $e(x)$ is x with the theoretical terms and functions "added back in." Since there are a number of ways in which all or some of the theoretical terms may be added back, the function e will assign a *set* $e(x) \in M_p$ to each *element* $x \in M_{pp}$. Now suppose that $e(x) \cap M \neq 0$ for some $x \in I$; then it is appropriate to say that "x is a P."

bance, with the fundamental characteristics of the theory's core left intact. On the other hand, revolutionary change should entail a much more global (and fundamental) disturbance. This intuitive distinction is followed in the structuralist account of theory dynamics. At a *particular time* a scientific theory is characterized by the ordered pair $\langle C,I \rangle$. If there are only minor changes in either C or I (or both) the theory has *evolved* (the change is intratheoretic). If the change is such that the core itself is fundamentally altered or replaced, then the change is *revolutionary* (intertheoretic).

While a good portion of the total structuralist literature is concerned with *revolutionary* theory change – and in particular Kuhnian revolutionary theory change[18] – this topic is of little importance in structuralist reconstructions of economic theories. This is particularly true for structuralist reconstructions of general equilibrium theory, where there have been no revolutions to be explained. In addition to its lack of economic importance, the structuralist literature on revolutionary theory change is one of the least settled areas of the program. The relationship between the structuralist formalism and Kuhn's view of scientific revolutions often serves as a point of departure for philosophical critiques.[19] This controversy is particularly pronounced with respect to the question of "incommensurability."[20] For these reasons the topic of revolutionary theory change will *not* be examined in our survey of structuralist dynamics.

Unlike revolutionary theory change, the structuralist view of *evolutionary* theory change does contribute to the structuralist reconstruction of economic theories. Economic theories (even general equilibrium theory) certainly evolve through time and (hopefully) progress. If the structuralist view of theory evolution "fits" the evolution of general equilibrium theory it could contribute to our understanding of this theory in the same way as its advocates feel it has contributed to the understanding of certain physical theories.

According to the structuralist program nonrevolutionary progress occurs in basically three (non-mutually-exclusive) ways. Progress can be (a) *empirical*, (b) *theoretical*, or (c) *confirmational*. The first of these,

18. Though Stegmüller (1975, p.82; 1976b, pp.152–153; 1979, pp.1, 2, 13, 50, 57) repeatedly insists that defending Kuhn was *not* the motivation behind the development of the structuralist program.
19. For instance, the major criticisms of Feyerabend (1977), Pearce (1981, 1982), Tuomela (1978), and Kuhn (1976) himself seem to focus on this issue.
20. Feyerabend's comment (1977, p.363) is particularly indicative in this regard. "There is only one place where Stegmüller's virtues seem to be almost entirely absent and this is in his discussion of incommensurability. Apparently everyone who enters the morass of this problem comes up with mud on his head, and Stegmüller is no exception." Even Stegmüller (1979, p.55) now admits that the problems associated with incommensurability are much worse than he originally thought.

empirical progress, is the most intuitive. If the set of intended applications $\langle C,I \rangle$ of the theory expands, then the theory has made empirical progress. Thus, if subscripts are used to denote the set of intended applications at different times, we have that $I_t \subset I_{t+1}$ implies empirical progress between periods t and $t+1$. The structuralist position is simply that if the theory "applies to" more of the world, then empirical progress has occurred.[21]

The second type of progress, theoretical progress, depends on the structuralist notion of a *core specialization.*[22] If we have two cores, $C = \langle M,M_p,M_{pp} \rangle$ and $C' = \langle M',M'_p,M'_{pp} \rangle$, then C' *is a core specialization of* C if and only if $M' \subseteq M$, $M'_p = M_p$, and $M'_{pp} = M_{pp}$. The intuitive notion of a core specialization is that the specialized core has more laws and thus is more restrictive (more specialized) than the core it is derived from. This restrictiveness makes the models of the specialized core a subset (possibly a proper subset) of the set of models of the original core. As an economic example, Händler (1980a) describes what he calls "neoclassical static microeconomic equilibrium theory," which is an extremely general neoclassical/supply and demand characterization of an economy. He then discusses various "specializations" of this basic theory. These specializations include such things as an "equilibrium" economy, an economy with infinitely many agents, an economy with externalities, etc. Since a particular theoretical core can have a large (possibly infinite) number of specializations, the hierarchical introduction of specializations generates a *core-net* from the original core. In the case where the set of intended applications remains the same, the addition of each member "down" the net (each additional specialization) constitutes the second type of progress, *theoretical progress.* The intuition behind this criterion for theoretical progress is simply that specializations represent tighter theories, theories with increasingly accurate laws. If this accuracy comes at the expense of intended applications then it may not constitute progress. But if this accuracy can be obtained for the same set of intended applications, then it constitutes theoretical progress.

The final type of progress is *confirmational* progress. This is a type of empirical progress which does not require that I be expanded. It occurs when things which are suspected of being applications of the theory are actually confirmed as being applications of the theory, where "assumed elements are transformed into firm elements of I" (Stegmüller, 1979, p.33). The argument is most clear in Moulines (1979), where the original Sneed/Stegmüller position was modified to better capture the dynamics of Newtonian mechanics. Moulines (1979,

21. Stegmüller (1976b, p.157; 1979, p.33).
22. Balzer and Sneed (1977, p.201), Stegmüller (1979, p.26).

p.424) divided the set of intended applications I into two (mutually exclusive and exhaustive) subsets F and A,[23] where F is the set of "confirmed" or "firm" applications of the theory, while A is the set of "assumed" applications, that is, A is the set of things which the scientific community suspects as being applications.[24] Given this division of I, we will say that *confirmational progress* has occurred when those things suspected of being applications at time t have become confirmed applications at some future time $t + h$ (that is, $A_t \subset F_{t+h}$).[25] This definition makes a clear distinction between empirical progress and confirmational progress. Empirical progress expands the set of intended applications, while confirmational progress moves things around within the set of intended applications (moving things from A to F).

This discussion of the three different types of nonrevolutionary progress completes our survey of the structuralist view of scientific theories. We now turn our attention to the application of the structuralist method to the specific area of general equilibrium theory.

III. GENERAL EQUILIBRIUM THEORY

Economists use the term "general equilibrium" in a variety of ways, from the very general to the very specific. The term will be used here in the manner which is most consistent with the way it is used in structuralist reconstructions. Most structuralist philosophers of science equate general equilibrium theory with *strictly Walrasian* general equilibrium theory. This is the view of a competitive economy which was first systematized by Walras in the late nineteenth century, further articulated by a number of early twentieth-century economists (including Cassel, Pantaleoni, Pareto, and Wicksell), and finally put in its

23. This is not the only modification which Moulines proposes. See note 45.

24. The structuralist program formally defines the notion of a "scientific community" and the "holding" of a theory by an individual member of such a community. These concepts were introduced as part of the "pragmatization" of the structuralist view necessary to accommodate Kuhnian dynamics. These subtleties have been neglected in our exposition of progress. Stegmüller (1979, pp. 29–40) provides a detailed discussion.

25. Actually Moulines (1979, p.425) calls it a *progressive theory evolution* (proper) if and only if $F_t \subset F_{t+1}$, and a *perfect theory evolution* if and only if there exists an h such that $A_t \subset F_{t+h}$. Since Moulines's progressive evolution could be caused by empirical progress ($I_t \subset I_{t+1}$), we neglect this notion and define confirmational progress in the same way as he defines perfect theory evolution. This modification of Moulines (which was itself a modification of Sneed/Stegmüller) allows us to clearly distinguish between confirmational and empirical progress.

current rigorous form during the 1950s and 1960s by economists such as Arrow, Debreu, Hahn, and McKenzie.[26]

There have been numerous attempts to reconstruct either all or part of Walrasian general equilibrium theory along structuralist lines.[27] These attempts differ so significantly that it is difficult to accurately portray a "standard" or "representative" structuralist reconstruction of the theory. There seems to be disagreement about very general questions, such as the overall applicability of the structuralist program, as well as about more specific questions such as the set-theoretic details of the reconstructions themselves. Since it is beyond the scope of this paper to reproduce each of these individual reconstructions (and little would be gained by doing so), we will instead offer a version of our own. The purpose of this reconstruction is simply to provide the reader with a concrete general equilibrium application of the structuralist approach. This example, while not as detailed as some in the literature, highlights the essential characteristics of such reconstructions and provides a valuable reference for the appraisal contained in the following sections.[28]

Structuralist reconstructions of general equilibrium theory (or any other part of neoclassical economics) seem to dedicate an inordinate amount of attention to the utility functions of economic agents. This is because utility functions are generally considered to be the appropriate "primitives" for neoclassical demand theory. In the case of general equilibrium this emphasis on utility yields reconstructions which are unnecessarily complex and particularly subject to philosophical debate. The difficulty arises because utility is (and always has been) an epistemologically problematic notion.[29] Is utility observable? theoretical? *T*-theoretical? tautological?

In the case of the Walrasian general equilibrium theory of pure exchange it is possible to circumvent all of the questions that surround

26. The single best statement of Walrasian general equilibrium theory remains that of Arrow and Hahn (1971). With the exception of works concerned with a single author such as Debreu (Hildenbrand's introduction to Debreu, 1983), Samuelson (Brown and Solow, 1983), or Walras (Jaffé, 1983, Morishima, 1977), there have been surprisingly few general historical studies of the field (Weintraub, 1979, is an exception). On specific topics within the theory, there is Weintraub's (1983) excellent history of the "existence" literature as well as Chipman's (1965, 1966) survey of international trade theory.
27. For instance, Balzer (1982a, 1982b), Händler (1980a), Haslinger (1982, 1983), Kötter (1982), and Pearce and Tucci (1982b).
28. A more detailed version of our reconstruction is provided in Hands (1985a).
29. None of these problems evaporate by substituting "preferences" for "utility." Some economists apparently feel that simply using "preferences" as the primitive notion is sufficient to remove any philosophic objections regarding demand theory. This is not the case: any difficulties with one of these terms is inherited by the other.

the notion of utility. In a series of theoretical papers by Debreu, Mantel, McFadden, Mas-Colell, Richter, and Sonnenschein it has been demonstrated that under fairly general conditions, utility maximization on the part of individual economic agents does not imply any restrictions on market (or community) excess demand functions other than continuity, homogeneity, and Walras law (discussed below).[30] While the full methodological implications of this theoretical result have yet to be examined, it clearly implies that it is appropriate to reconstruct general equilibrium theory (at least for the pure exchange case) without any reference to utility (or preference) at all. Each *market* can be characterized by an excess demand function which must satisfy certain minimal conditions. These conditions are the axioms of the theory. The topics that constitute the theoretical concerns of practicing general equilibrium theorists, such as existence, stability, uniqueness, and optimality, can be (and are) fully addressed by using only these market excess demand functions.[31]

We offer the following axiomatization for Walrasian pure exchange economics (WPEE).

Definition D1. x is a *possible Walrasian pure exchange economy* ($x \in M_p$) if there exists an n, N, z, p, and D such that

1. $x = \langle n,N,z,p,D \rangle$,
2. $N = \{1,2,\ldots,n\}$, where n is a positive integer,
3. $D \subseteq R^n_{++}$,
4. $p \in D$,
5. $z\colon D \rightarrow R^n$, where z is a continuous function.

Note first that the *theory* is Walrasian pure exchange *economics* (WPEE). A *possible* model of the theory is then a *possible* Walrasian pure exchange *economy*. In Definition D1, N represents the set of commodity indices or the set of market indices (one market for each commodity), while n represents the number of different types of commodities (and markets); D represents the "price domain" and D is a subset of the strictly positive orthant of the n-dimensional real number space (R^n_{++}). It is of course always possible that $D = R^n_{++}$. p represents the price vector and $p \in D$ simply means that there is one and only one strictly

30. The principal papers are Debreu (1974); Mantel (1977); McFadden, Mas-Colell, Mantel, and Richter (1974); and Sonnenschein (1973). A summary of this literature is presented in Shafer and Sonnenschein (1982).
31. Haslinger (1983) criticizes Balzer (1982a) for using utility as the primitive concept in the reconstruction of pure exchange general equilibrium economics. Haslinger uses individual demands instead of utility as the basis for his own reconstruction. We go one step further from utility and use *market excess* demands.

positive price for each good and thus $p^T = (p_1, p_2, \ldots, p_n)$.[32] The continuous function $z(p)$ is the *excess demand function;* for any p in D the function z assigns to each commodity the excess demand (demand minus supply) associated with p. Thus $z_i(p)$ is the excess demand for good i at price vector p, and $z(p)^T = (z_1(p), \ldots, z_n(p))$. Excess demand will be the primitive notion in our reconstruction of WPEE.

Note that "if" is used in D1 rather than "if and only if." This is to emphasize that there are other perfectly reasonable ways of defining a Walrasian pure exchange economy. For instance, the price domain D could be specified in a number of alternative ways. One alternative would be $D = R^n_+$, i.e., prices non-negative rather than strictly positive. Another specification might be $D = R^n_+ - \{0\}$, i.e., prices non-negative but not all zero. In addition to alternative specification of D, excess demand could be specified differently as well. For example, excess demand need not be a function, it could be merely a correspondence. Alternatively, z could be a function but not continuous, or the definition could be made more restrictive by requiring z to be differentiable.

Now consider the *models* of the WPEE theory, those things which actually satisfy the laws or axioms of the theory.

Definition D2. x is a Walrasian pure exchange economy ($x \in M$) if there exists an n, N, z, p, and D such that

1. $x \in M_p$,
2. $z(p) = z(\alpha p)$ for all $p \in D$ and for all $\alpha \in R_{++}$,
3. $p^T z(p) = 0$ for all $p \in D$.

In Definition D2, condition (2) is a zero-degree homogeneity condition (H) on excess demands. This condition states that excess demand is unchanged when all prices are "scaled up" or "scaled down" by the same parameter. Under H only relative prices (price ratios) matter for excess demand. Condition (3) is Walras's Law (W). Basically W requires that all traders be on their budget constraints, that the total value of what they intend to buy be exactly equal to the income they can generate from what they intend to sell. Note that D1 and D2 imply that $M \subseteq M_p$ as the structuralist program requires.

While Definition D2 makes no mention of utility or utility maximization, the result discussed above (Debreu–Mantel–McFadden–Mas-Colell–Richter–Sonnenschein) guarantees that such maximization always lies "behind" any $x \in M$. Using the particular result in Debreu (1974), we know that if $x \in M$ there always exists a set of n agents, each with a well-behaved utility function, such that the utility-maximizing choice of these agents in fact generates the excess demand function z.

32. T indicates transposition, so $p^T z(p) = \sum_{i=1}^{n} p_i z_i(p)$.

Thus, while utility (or preferences) are not explicitly mentioned in either D1 or D2, our characterization of WPEE is thoroughly neoclassical, that is, the functions that characterize the economy are "based on" the rational utility-maximizing behavior of economic agents. Had we started with the "endowments" and the utility functions of traders (as in Balzer, 1982a; Haslinger, 1983) the homogeneity (H) and Walras Law (W) conditions of D2 would follow automatically from the constrained utility-maximizing behavior of the traders. Since excess demand rather than utility is our primitive notion, the conditions H and W must be included in D2.

Our definitions of D1 and D2 specify an entirely different relationship between the sets M and M_p than that which has been offered by other authors attempting to provide a structuralist reconstruction of Walrasian pure exchange economics. For instance, Balzer (1982a) uses utility as the primitive concept and thus the difference between members of M and members of M_p is drawn between agents who merely "have" utility functions (M_p) and those who maximize them (M). Since general equilibrium theorists never consider non-utility-maximizing individuals this seems to be a less than useful distinction. For Haslinger (1983), who uses "demand" as the primitive concept, the notion of equilibrium is used to demarcate M from M_p. Agents merely "having" demands characterizes M_p, while demand and supply being matched in each market (a p^* prevailing such that $z_i(p^*) = 0$ for all $i \in N$) characterizes M. Such a demarcation does not do adequate justice to the role of equilibrium in general equilibrium economics.

The way in which the concept of equilibrium is introduced *is* an extremely important aspect of any attempt to characterize Walrasian equilibrium theory. Given the above definition of WPEE, the equilibrium states should constitute a *core specialization* of the Walrasian theory. Recall from Section II that C' *is a core specialization of* C if $M' \subseteq M$ and $M'_p = M_p$. Since equilibrium states, where $z(p^*) = 0$, constitute an additional restriction on the models of WPEE, an *equilibrium specialization* of WPEE can be defined in the following way.

Definition D3. x is an *equilibrium Walrasian pure exchange economy* ($x \in M(p^*)$) if there exists an $n,N,z,p,D,$ and p^* such that

1. $x \in M$,
2. $p^* \in D$,
3. $z(p^*) = 0$.

The well-known existence proofs for Walrasian general equilibrium systems will guarantee that any model of WPEE as defined in D2 will have an equilibrium specialization as defined in D3.[33] While such an equilibrium price vector will always exist for any $x \in M$, economists working with the theory will not necessarily restrict their attention to equilibrium prices. It is often of interest, for example in the analysis of the "stability" of the equilibrium prices, to consider pure exchange economies which are not necessarily equilibrium pure exchange economies. Models in equilibrium are an important part of, but do not exhaust, the concerns of general equilibrium theorists.

Equilibrium is only one of many core specializations which are found in the general equilibrium literature. Another core specialization is the gross substitute specialization. For the *gross substitute* (GS) economy we have the following definition.

Definition D4. *x is a gross substitute Walrasian pure exchange economy* ($x \in M_{GS}$) if there exists an n,N,z,p, and D such that

1. $x \in M$,
2. z is differentiable on D,
3. $\partial z_i / \partial p_j > 0$ for all $i \in N$, $j \in N$, and $i \neq j$.

This GS specialization has been extremely important in the literature of general equilibrium theory. Among other things, this specialization is sufficient to guarantee that the equilibrium price vector p^* is unique (that there is only one equilibrium) and that under a somewhat general specification of disequilibrium dynamics the price vector will converge to p^* from any initial nonequilibrium p (i.e., p^* is stable).[34] There are of course numerous other core specializations of WPEE which have been used to analyze other specific topics. In fact, it is safe to say that much of the research on pure exchange Walrasian general equilibrium theory during the last thirty years has been to investigate the properties of various core specializations of WPEE.

Our final task of this section will be to provide an example of the theory which we have axiomatized. Consider the following example of a *possible* Walrasian pure exchange economy.

33. Given the conditions in D2, the existence of a p^* which satisfies D3 is guaranteed by Theorem 2.2 (p.28) of Arrow and Hahn (1971). Slightly different assumptions would require a slightly different proof, but existence theorems exist for Walrasian economies under very general assumptions. See Debreu (1982) for a formal survey and Weintraub (1983) for a more historical discussion.

34. For uniqueness see Arrow and Hahn (1971), Corollary 9.7 (p.223), and for (global) stability Theorem 12.4 (p.288).

$$\begin{aligned} x_0 &= \langle n, N, z, p, D\rangle = \langle 3, \mathrm{N}, z_0, p, R^3_{++}\rangle, \\ \text{where} \qquad z_0 &= (z_1(p),\ z_2(p),\ z_3(p)), \\ z_1(p) &= -10 + p_2/p_1 + p_3/p_1, \\ z_2(p) &= 9p_1/p_2 - 2 + p_3/p_2, \\ z_3(p) &= p_1/p_3 + p_2/p_3 - 2, \\ N &= \{1,2,3\}. \end{aligned}$$

Is this possible Walrasian pure exchange economy actually a Walrasian pure exchange economy? Is the possible model x_0 actually a model for the theory? To answer these questions it is only necessary to check the homogeneity condition [(2) in D2] and Walras's Law [(3) in D2]. If these two restrictions hold for the function z_0 on the domain $D = R^3_{++}$ then the possible model x_0 is in fact a model for the theory. Simple computation of $z(\alpha p)$ for $\alpha > 0$ and $p^T z(p)$ confirms that this is in fact the case: x_0 constitutes a Walrasian pure exchange economy; it is an "instance" or an "application" of Walrasian pure exchange economics. For x_0, the equilibrium specialization is given by $p^* = (p_1^*, p_2^*, p_3^*) = (3/11, 19/11, 1)$. Coincidentally, the model x_0 also satisfies the additional restrictions of the gross substitute specialization.

IV. ARGUMENTS IN FAVOR OF THE STRUCTURALIST VIEW

In this section we survey the structuralist response to such questions as: What are the advantages of viewing economic theory (particularly general equilibrium theory) from the perspective of the structuralist program? How do structuralist spectacles allow us to see formal economic theory in a more informative way than other metascientific views? In what ways does economics (particularly general equilibrium theory) "fit" the structuralist view of theories?

One approach to such questions would be to discuss the *general* advantages of the structuralist view of theories. After all, if structuralism helps us understand *science* in general, it should help with any particular science such as economics. This is not the approach that will be followed below. As much as possible the discussion will be restricted to economics and economic methodology. Of course it may be that some of the arguments in favor of the structuralist view of economics apply equally well to other sciences, but these implications will not be actively pursued. The arguments in favor of the structuralist view of economics will be divided into two (not necessarily distinct) parts. The first part will discuss general methodological problems in economics, while the second part will focus exclusively on Walrasian equilibrium theory.

With respect to economics in general, the structuralist program claims to offer a resolution to at least two important controversies within contemporary economic methodology. The first pertains to the issue of empirical testing or the "falsification" of economic theories. The second pertains to the closely related issue of the "theory-ladenness" of economic observations. Each of these issues will be considered separately.

The recently resuscitated methodological debate in economics has generated a consensus on only a very few points, but one of these points is that economists seldom if ever practice a falsificationist methodology. Despite the fact that most economists openly advocate the severe empirical testing of economic theories, they in fact almost never practice what they preach. Negative evidence, if acknowledged at all, is never quite sufficient to dislodge (or cause the rejection of) a professionally popular theory.[35]

Within the structuralist program such empirical immunity does not necessarily imply that the community of economic scientists is behaving incorrectly. Recall that for the structuralist program a scientific theory merely defines a set-theoretic predicate; theories are therefore not the kinds of things which might be true or false. The *empirical hypotheses* of the theory, statements of the form "x is a P" or, more formally, "there exists a theoretical enrichment of x which is a model for T," are the only types of things which might be true or false, not the theory itself. Suppose for example that an economist observes a particular economy x and then posits that this x is an instance (or application) of the economic theory T. Suppose further that the economist proves to be wrong; that is, suppose that there does not exist a theoretical enrichment of x which is a model for T. Does this failure mean that "the theory" is falsified and should be rejected? Certainly not. All that the falsification of an empirical hypothesis implies is that this *particular* x is not an instance of the theory. It does not necessarily imply that other things (perhaps many other things) might not be instances of the theory. Empirical hypotheses can be falsified and rejected, but the rejection of a scientific theory simply makes no sense within the structuralist view. As Stegmüller (1975, p.92) states, "Is a theory falsifiable? The correct answer to this has to be that strictly speaking the question is meaningless. The reason is simply that in our sense of the word a theory is not that kind of entity of which one could reasonably say that it is falsified or refuted."[36] Thus the empirical im-

35. While this opinion has been expressed in almost every recent work on economic methodology, it is particularly pronounced in Blaug (1980). Caldwell (1982, pp.124–138, 235–242) offers a number of nonstructuralist arguments regarding why falsificationism might not be appropriate in economics.

36. Stegmüller (1976b, p.162; 1978, p.56) makes similar statements. Stegmüller (1979, pp.53–54) provides a detailed discussion of the issue and lists three separate ways in which theories are immune to falsification in the structuralist view.

munity, so evident in the history of economic thought and so difficult to explain within the confines of most other metascientific views, becomes unproblematic within the structuralist view.[37]

The second methodological controversy on which the structuralist view claims to shed some new light is the so-called "theory-ladenness" problem. It is a relatively well-accepted position within modern philosophy of science that "brute facts" do not exist, that, in Popper's words, "observation statements and statements of experimental results are always *interpretations* of the facts observed; that they are *interpretations in the light of theories*" (1959, p.107, emphasis in original).[38] As long as a theory is present in every observation, then what appears to be a conflict between a "theory" and the "facts" actually amounts to a conflict between two theories, the theory itself and the theory implicit in the observation. As an economic example, consider a macroeconomic theory which predicts a particular inflation rate. Suppose a different inflation rate is actually observed. Some may argue that the theory (actually the empirical hypothesis that the theory applies to this economy) is falsified, but this is not the only possibility. Since the rate of inflation is not a brute fact but rather something based on a theory of index numbers, such a macroeconomic theory can always be protected. The defender of the macroeconomic theory can always say, "No, my theory is not falsified because my theory is about *inflation* and your data are based on a debatable theory of index numbers. My theory is in fact correct about inflation; it is your theory about how inflation should be measured that is falsified."

One way to view the theory-ladenness problem is to consider it to be merely a problem of drawing a line between "theoretical" and "nontheoretical." According to this view, such a demarcation solves the problem because theories (or theoretical hypotheses) may be refuted by nontheoretical entities (or statements about nontheoretical entities). The approach of traditional philosophy of science was to define "observational" as that which could be incorrigibly described by means of a theory-neutral observation language and then to simply equate nontheoretical with observational. The well-documented failure to solve all of the difficulties associated with such a language and its application left philosophers of science with the theory-ladenness problem.[39] If

37. Sneed, in the earliest presentations of the structuralist view (1971, p.152), argued that much of the program's attractiveness stemmed from the fact that it "allowed us to account for the putative fact that theories . . . sometimes appear to be tenaciously maintained in the face of recalcitrant data."

38. Again, as with the question of falsification, this topic is discussed in almost any recent work on economic methodology. Blaug (1980, pp. 14, 41–42) and Caldwell (1982, pp.45–49, 79–81) offer fairly concise statements of the problem.

39. This is well-documented in most surveys of contemporary philosophy of science, for instance F. Suppe (1977).

nothing is clearly observational, then nothing is clearly nontheoretical, and everything seems to be theoretical.

When theory-ladenness is characterized in this way, the structuralist program seems to offer a solution to the problem. Recall from Section I the structuralist definition of *T*-theoretical: a term or function is *T*-theoretical if its measurement requires theory *T* to be true in some application. This definition provides an unambiguous way of drawing a line between that which is theoretical and that which is nontheoretical. The distinction is "theory relativized" (Stegmüller, 1979, p.10); that is, theoreticity is only defined relative to a particular theory, but it is unambiguous nonetheless. Also recall that the empirical claims of the theory must be free of *T*-theoretical terms. Thus not only does the structuralist approach provide a clear distinction between theoretical and nontheoretical, terms and functions of the latter type do not enter into any tests of the theory's empirical hypotheses. While this may not be an exact solution to the general problem of theoretical terms, Sneed (1971, p.35) argues that "Despite its limited scope, I believe this distinction is adequate to deal with many, if not all, the problems for which some philosophers have called upon the more comprehensive distinction."

If we turn our attention away from economic methodology in general and toward the specific question of general equilibrium theory, there is at least one more point in favor of the structuralist program. This concerns the emphasis on mathematics and axiomatization. General equilibrium theory is a highly mathematical theory and the structuralist program has, from its beginnings in Sneed's (1971) reconstruction of mathematical physics, focused exclusively on the mathematical sciences. The important nexus, though, may be much deeper than merely the fact that both emphasize mathematics. After all, there are many other parts (in fact most) of economics which are couched in mathematics; what is unique about general equilibrium theory is that it is axiomatic in precisely the way that is most amenable to the structuralist approach. Debreu's work is particularly important in this regard. His *Theory of Value* (1959), probably the most important single book in the development of modern general equilibrium theory, has the revealing subtitle "An Axiomatic Analysis of Economic Equilibrium." In the preface Debreu is quite clear about his underlying axiomatic commitment: "The theory of value is treated here with the standards of rigor of the contemporary formalist school of mathematics" (p.x.). What contemporary formalist school? Given that Debreu was a French mathematics student during the 1940s, and given that N. Bourbaki is Debreu's standard mathematical reference in both the *Theory of Value* and much of his other work, it is safe to assume that Debreu was referring to the Bourbaki school.[40] This is extremely important since the overrid-

40. See Fang (1970) for a detailed discussion of the Bourbaki school.

ing goal of the Bourbaki group was to unify the many diverse fields of mathematics by building the edifice up from a fundamental (and set-theoretic) axiomatic "structure." It is not a coincidence that this sounds exactly like the goal of the structuralist program in physical science and economics. Stegmüller's major statement of the structuralist view (1979) is subtitled "A Possible Analogue of the Bourbaki Programme in Physical Science." Thus it seems that Debreu's early (and influential) work on general equilibrium theory was committed to exactly the same underlying Bourbakian philosophy of axiomatization as the structuralist view of scientific theories.

V. CRITICISMS OF THE STRUCTURALIST VIEW

This section surveys some of the criticisms of the structuralist view. Attention is focused on problems which are particularly relevant to economic applications rather than philosophical criticisms of a more general sort.[41] Since there seem to be a number of different problems and since these problems do not seem to be related in any systematic way, we will sacrifice style to clarity and simply list and discuss some of these problems one at a time. First we shall examine critically the supposed virtues of the structuralist approach discussed in Section IV and then we shall turn to some other deficiencies in the approach.

(i) Theory-Ladenness. There are a number of problems associated with the structuralist T-theoreticity "solution" to the theory-ladenness problem. First, there is simply the problem of converting the structuralist technical definition of T-theoreticity into a *usable* or *operational* form. This problem is of sufficient magnitude that it prevents even structuralist authors from agreeing on what is and what is not T-theoretical (at least outside the paradigmatic case of classical mechanics). For instance, three different structuralist reconstructions of general equilibrium theory, Balzer (1982a) and Haslinger (1982,1983), consider three entirely different parts of the theory to be T-theoretical.[42] Because it is so difficult to directly apply the structuralist definition of T-theoreticity several authors have offered more operational analogs of the concept. For example, Moulines (1975, p.106) defines a function f to be T-theoretical if it "has no clear meaning prior to T, or in other words, if the concept of f is rightly understood only inside T." On the other

41. Some of the general philosophical criticisms are contained in Agassi and Wettersten (1980), Diederich (1982a), Feyerabend (1977), Kuhn (1976), Pearce (1981, 1982), F. Suppe (1979), and Tuomela (1978).

42. For Balzer (1982a), "utility" is the only T-theoretical concept in the theory. For Haslinger (1983) utility is not, but the "demand function" is, while in Haslinger (1982), "equilibrium" is the only T-theoretical concept.

hand, Pearce and Tucci (1982b, p.92) merely equate *T*-theoreticity and *T*-nontheoreticity with the economist's notion of endogenous and exogenous. While such reinterpretations of the *T*-theoreticity criterion do make it much more operational, there is a real question as to whether they accurately reflect the original structuralist definition. Alternatively, *if* such simple criteria do reflect the *T*-theoretical distinction, then it is unclear what great contribution *T*-theoreticity makes. The notion of *T*-theoreticity is usually cited (as in the previous section) as a major accomplishment of the program. Such an honor seems undeserved if either the concept is not operational or it simply reduces to the rather common distinction between "before and after" or "exogenous and endogenous."

Second, *even if* we had an operational definition of *T*-theoreticity, it still does not really solve the traditional theory-ladenness problem faced by economists. Consider the example (from Section IV) of the macroeconomic theory which conflicts with observed inflation rates. How does even an operational definition of *T*-theoreticity help us respond to the theory's defender who argues that the observational theory of index numbers implicit in the observed inflation rate is incorrect rather than the macroeconomic theory? The answer is that it does not help us at all! Even if *T*-theoretical terms are eliminated from empirical claims there are still some theories embodied in the observations, and the structuralist program provides no guide regarding which to hold and which to reject. We could decide (by convention) that the observational theories are beyond doubt when the empirical hypotheses of the "higher level" theory are being tested, but this is simply the standard (Popperian) answer and it certainly constitutes nothing new or unique resulting from the structuralist program. In all fairness, it should be noted that some structuralist authors admit this difficulty. Stegmüller (1978, p.53; 1979, pp.70, 80–81) distinguishes between a "strong" version and a "weak" version of the theory-ladenness problem. The weak version says that *some* theories are always involved in observations, while the strong version says that *the theory itself* is always involved in any observations which could falsify its empirical hypotheses. Stegmüller fully admits that the structuralist *T*-theoretical "solution" only eliminates theory-ladenness in the *strong* sense. Regarding the weak sense, Stegmüller states only that (1978, p.53) "the thesis is connected with the important problem of how to reconstruct theories in the appropriate 'hierarchical order' " and "Only partial answers are known to this challenge." While elimination of theory-ladenness in the strong sense may shed some light on the "incommensurability" controversy,[43] it does not help with the concerns of economic methodology, since the

43. See note 20.

problems of theory-ladenness that confront economists are almost always of the weak form.

(ii) The Bourbaki Connection. It was argued in the closing paragraph of Section IV that a close connection (rooted in the Bourbaki school) existed between the axiomatic goals of important general equilibrium theorists (particularly Debreu) and the axiomatic approach of the structuralist school. While we will continue to maintain that such a Bourbaki connection exists, we would now like to argue that this connection may not do as much to "fit" general equilibrium theory into the structuralist framework as originally suggested. Stegmüller (1979, pp.3–12, 83–85) goes to great lengths to emphasize that earlier set-theoretic approaches (particularly that of P. Suppes) were aimed at *integrating* physical science into the Bourbaki program for mathematics, while Sneed and others who have applied the structuralist program to physical science desire an *analog* of the Bourbaki program for physical theories. This distinction is somewhat subtle but is very important. The aim of the Suppes approach "in axiomatizing a physical theory is to clarify the internal structure of the theory, meeting modern standards of mathematical rigor . . . the relation of the physical theory to something 'outside' itself is left out of consideration. On the other hand, Sneed's main point has to do precisely with the relations of mathematically described structures of theories to 'outside entities' " (Stegmüller, 1979, p.11). The Suppes approach axiomatizes only to make the purely mathematical aspects precise and clear; the structuralist approach axiomatizes to make clear the relationship between scientific theories and their empirical claims. A scientific theory axiomatized according to the structuralist program necessarily includes the set of concrete intended applications in the basic definition of theory (no I, no theory), while for the Suppes approach only the mathematical structure itself is relevant. The distinction between these two views of axiomatization is important because *Debreu's goals* for axiomatizing general equilibrium theory *seem to be much closer to Suppes than Sneed.* Not only is Debreu's important axiomatization of general equilibrium theory devoid of any concrete set of applications, the fact that the mathematical structure is "free" of any specific economic interpretation is considered to be "one of the sources of the power of the axiomatic method" (1983, p.5). By *not* specifying any intended applications, theory is given maximal interpretative flexibility. Debreu (1983, p.5; 1984, p.275) cites the term "commodity" as an example of this flexibility. By not restricting the term "commodity" to any particular interpretation, the theory was flexible enough to accommodate Arrow's extension to conditions of uncertainty. For Debreu the greatest benefits of axiomatization are obtained when the theory is formulated as a purely mathematical structure, following Suppes, rather than as a structure which is fundamentally "connected" to a

single set of concrete applications, following the structuralist school. In fact, Suppes (1961, p.170) cites Debreu's work as proof that "there is no systematic difference between the axiomatic formulation of theories in well-developed branches of empirical science and in branches of mathematics." This stands in stark contrast to Stegmüller (1979, p.7), who is concerned with "the fundamental question of what distinguishes a *theory of mathematical physics* from a mere *mathematical theory*" (emphasis in original). Thus, while there may be a Bourbaki connection, it seems overshadowed by fundamental differences regarding both the motivations for, and the final form of, appropriately axiomatized theories.

(iii) The Set of Intended Applications. As with T-theoreticity, the set of intended applications seems difficult to characterize appropriately for economics. According to the structuralist view, "empirical" progress is achieved when the set of intended applications is expanded ($I_t \subset I_{t+1}$).[44] In the case of economics empirical progress may occur in this way, but it also occurs in other (possibly more important) ways as well. For instance, according to the structuralist definition of I an increase in world population would increase the set of intended applications of consumer choice theory. This is because most economists consider any decision to purchase goods or services by an individual to be an intended application of consumer choice theory; thus more individuals, more choices, and more intended applications. While increasing I in this way technically satisfies the structuralist criteria, it seems to be a relatively "cheap" form of progress. On the other hand, if we could modify the structuralist view and define the set of intended applications not over individuals (or individual choices), but over the "classes" or "kinds" of individual choices made, then an expansion of I would seem more like real progress.[45] For example, most economists consider the "new home economics" of Gary Becker (1981) to be a "progressive" contribution to consumer choice theory.[46] What Becker and his followers have done is to extend the standard consumer choice theory to *types* of choices where it had not previously been applied, to things such as decisions about the number of offspring or marital partner. Most economists would argue that to explain the behavior of the family with economic theory where it had previously been explained only by sociological or psychological theory constitutes not only progress, but a more important form of progress than merely finding more new applications of the same basic type. It appears that the set of intended applications will need some (possibly

44. See Section II.
45. This is precisely the modification suggested by Moulines (1979, pp.420–421).
46. Though it is not necessarily progressive by Popperian standards, see Blaug (1980, pp.240–249).

substantial) modification to capture what many economists consider to be empirical progress within their discipline.

This difficulty involving the relationship between the structuralist definition of I and the economics profession's notion of progress pertains to economic theory in general; when we focus on general equilibrium theory in particular a different (perhaps more important) problem develops involving the set of intended applications. This is the problem that for Walrasian general equilibrium theory the set of intended applications is largely *empty*. As Balzer (1982a, p.41) states, "in economics we cannot point out a single real, concrete system which is commonly accepted by economists to be a standard example of PEE [pure exchange economics]."[47] The emptiness of I has prompted some structuralist authors to characterize general equilibrium theory as a *pure theory*, where "A pure theory does not intend to speak about reality. A pure theory is just a (sometimes very complex) picture of a possible world which does not actually exist" (Händler, 1982a, p.75).

Given the presence of many Leontief-type applied general equilibrium theories,[48] the "pure theory" claim ($I = \varnothing$) may be a bit of an overstatement, but nonetheless there does seem to be a fundamental difficulty in applying the structuralist notion of I to general equilibrium theory. Recall that I was introduced as a fundamental part of the definition of every scientific theory, and in order to guarantee that the theory referred to *concrete* (as opposed to purely abstract or mathematical) entities. Yet the standard examples or applications of Walrasian general equilibrium theory are entities such as the system x_0 described in the closing paragraph of Section III. Such applications are purely abstract/mathematical and not concrete in the sense that "our solar system" is a concrete application of Newtonian mechanics or "the U.S. economy in 1960" is a concrete application of a macroeconomic theory. While it may not be that general equilibrium theory is utterly devoid of such concrete intended applications, it is certainly safe to say "most" applications of the theory are not of this type. It seems that the sense in which general equilibrium theory is "empirical," that it has concrete applications, is much more complex than can be captured in the standard structuralist definition of the set of intended applications.

(iv) Dynamics. There are problems associated with "fitting" economics into the structuralist view of either revolutionary or evolutionary theory change. As discussed in Section II, the structuralist program has tended to characterize revolutionary theory change in a Kuhnian way. This is problematic since most economic theories, particularly

47. Similar comments are contained in Balzer (1982b), Hamminga (1982, pp.12–13; 1983, p.130), Händler (1980a, pp.50–51; 1980b, pp.154–155; 1982a, p.75), Kötter (1982, p.113), and Pearce and Tucci (1982, pp.99–100).

48. For a recent survey of this work see Shoven and Whalley (1984).

those formal enough to allow set-theoretic axiomatization, do not have histories of revolutionary upheaval. There are also problems with the structuralist characterizations of evolutionary change. Besides the previously discussed problem of defining the set of intended applications in a way which "fits" empirical progress in economics, there are also problems associated with the structuralist view of theoretical progress. Recall that theoretical progress only occurs through core specializations of the theory. The argument is that if the theory has increasingly restrictive laws – if its models must satisfy more conditions, while the set of intended applications remains the same – then theoretical progress must have occurred. There is little doubt that *some* of what the economics profession considers progress happens in this way. For example, the gross substitute specialization of Walrasian pure exchange economics (presented in Section III) was a "progressive" specialization from the viewpoint of general equilibrium theorists. The problem is that theoretical progress occurs in other ways as well. Often theoretical progress occurs in just the reverse manner; the theory is made *not more specific, but more general.* Much of the history of general equilibrium theory can be characterized as a search for increasingly more general conditions which preserve the basic properties of the theory.[49] This type of "generalizing" theoretical progress is outside the standard structuralist view of theoretical progress and thus represents one more way in which the fit seems less than perfect.

Because the existing structuralist characterization of theory change seems inappropriate for so much of economic theory, some authors have chosen to complement their static reconstructions of economic theory with dynamic frameworks not found in the structuralist literature on physical science. For instance, Haslinger (1982) characterizes theory change along the lines of L. Laudan's (1977) view of physical science, while Händler (1982a) provides his own evolutionary philosophy of science. While such modified views are probably a more accurate portrayal of theory change in economics, it is not clear that they can correctly be called structuralist. Alternatively, if we can correctly call all of these different views structuralist, then it seems that the structuralist program really does not imply any particular view of theory dynamics. If this is the case, and the structuralist program does not necessarily entail any particular view of theory change, then the methodological value of the entire program must be questioned. Theory change and theory dynamics are the fundamental questions of recent philosophy of science and especially of recent economic methodology. Failure to say something specific on this topic reflects badly on the value of the entire program.

49. This is the basis for Hamminga's (1982, 1983) criticism of the structuralist program and it forms the foundation for his alternative view, "plausibilism."

(v) Generic Economics. Thus far the entire discussion has focused on very specific economic theories (the Keynesian income-expenditure theory, Walrasian pure exchange economics, etc.). It might be suggested that the structuralist program would be more appropriate if all of economics were subsumed into one big theory. Of course, even attempting to properly characterize such a "generic" or "conglomerate" economic theory would be a major research project in itself, but it is possible to sketch some of the very general properties such a generic economic theory must have. For one thing the theory must be methodologically individualist; all "laws" must be based on the actions of individuals and only individuals.[50] For another thing it must be "intentional," since explanations in economics are always in terms of the beliefs and desires (i.e., intentions) of individuals.[51] Finally, these beliefs and intentions must be connected to specific actions on the basis of a "rationality principle" or "maximization hypothesis."[52] *If* such a generic economic theory could be characterized in structuralist terms then the set of intended applications would be fairly straightforward. The set I would simply contain any problem situation in which individual choices must be made under conditions of scarcity or constraint.

The difficulty is that such a generic economic theory is probably impossible to axiomatize. In this regard there seems to be a fundamental tension within economics. If the theory is specific enough and formal enough to axiomatize (as is general equilibrium theory), then the set of intended applications is difficult to specify. On the other hand, a more generic economic theory which has a clear set of intended applications seems impossible to axiomatize. Since the structuralist program requires *both* axiomatization *and* the specification of a clearly delineated set of intended applications, this tension always works against the structuralist characterization of economic theories.

VI. CONCLUSION

Comparing the arguments in favor of the structuralist view from Section IV with the negative arguments from Section V, one must conclude that a negative appraisal of the program is in order. The only

50. While there has been a great amount of controversy regarding whether economics "ought" to be, or "needs" to be, methodologically individualistic, almost every recent methodological commentator agrees that economics actually is methodologically individualistic. See for instance, Boland (1982, pp.27–43) or Hausman (1981a, pp.198–202).
51. See Hausman (1981a, pp.199–202) or Kötter (1982, pp. 104–107).
52. Thus generic economics must look much like Popper's "situational analysis"; see Hands (1985b).

positive argument that seems to hold up under further examination is that the structuralist program offers a novel explanation for why economic theories are not rejected every time they are faced with negative evidence, and even this argument is not entirely satisfying. Given the way in which the structuralist school uses the term "theory," their solution to the problem of falsification sheds almost no light on the problems associated with the falsification of what nonstructuralists call theories. And besides, since almost every recent metascience program offers a solution to the falsification question, the structuralist program needs more than this to justify its set-theoretic formalism. The bottom line seems to be that despite the best efforts of structuralist philosophers of science, economic theories (even highly mathematical ones) just do not seem to "fit" the structuralist view of scientific theories. We believe this difficulty arises because the structuralist program is based entirely on the structure of mathematical *physics,* and despite certain superficial mathematical similarities, economic theory (even in its most mathematical form) is simply not physics. The economic theories that are formally amenable to structuralist axiomatization are simply *not empirical in the same way that structuralist authors argue that physical theories are empirical.*

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REFERENCES

Agassi, J. and Wettersten, J.R. 1980. "Stegmüller Squared." *Zeitschrift für Allgemeine Wissenschaftstheorie* 11:86–94.

Arrow, K.J. and Hahn, F.H. 1971. *General Competitive Analysis.* San Francisco: Holden-Day.

Balzer, W. 1982a. "A Logical Reconstruction of Pure Exchange Economics." *Erkenntnis* 17:23–46.

Balzer, W. 1982b. "Empirical Claims in Exchange Economics." In *Philosophy of Economics,* edited by W. Stegmüller, W. Balzer, and W. Spohn. New York: Springer-Verlag, pp. 16–40.

Balzer, W. and Moulines, C.U. 1980. "On Theoreticity." *Synthese* 44:467–494.

Balzer, W. and Sneed, J.D. 1977. "Generalized Net Structures of Empirical Theories I." *Studia Logica* 36:195–211.

Beatty, J. 1980. "Optimal-Design Models and the Strategy of Model Building in Evolutionary Biology." *Philosophy of Science,* 47:532–561.

Becker, G.S. 1981. *A Treatise on the Family.* Chicago: University of Chicago Press.

Blaug, M. 1980. *The Methodology of Economics.* Cambridge: Cambridge University Press.

Boland, L.A. 1982. *The Foundations of Economic Method.* London: George Allen and Unwin.

Brown, E.C. and Solow, R.M. (editors). 1983. *Paul Samuelson and Modern Economic Theory.* New York: McGraw-Hill.

Caldwell, B.J. 1982. *Beyond Positivism: Economic Methodology in the Twentieth Century.* London: George Allen and Unwin.

Chipman, J.S. 1965. "A Survey of the Theory of International Trade: Part 2, The Neo-Classical Theory." *Econometrica* 33:685–760.

Chipman, J.S. 1966. "A Survey of the Theory of International Trade: Part 3, The Modern Theory." *Econometrica* 34: 18–76.

Debreu, G. 1959. *Theory of Value.* New Haven: Yale University Press.

Debreu, G. 1974. "Excess Demand Functions." *Journal of Mathematical Economics* 1:15–21.

Debreu, G. 1982. "Existence of Competitive Equilibrium." In *Handbook of Mathematical Economics,* edited by K.J. Arrow and M.D. Intriligator. Amsterdam: North-Holland, Vol. II, pp. 607–743.

Debreu, G. 1983. *Mathematical Economics: Twenty Papers of Gerard Debreu.* Cambridge: Cambridge University Press.

Debreu, G. 1984. "Economic Theory in the Mathematical Mode." *American Economic Review* 74:267–278.

Diederich, W. 1982a. "Stegmüller on the Structuralist Approach in the Philosophy of Science," *Erkenntnis* 17: 377–397.

Diederich, W. 1982b. "A Structuralist Reconstruction of Marx's Economics." In *Philosophy of Economics,* edited by W. Stegmüller, W. Balzer, and W. Spohn. New York: Springer-Verlag, pp. 145–160.

Fang, J. 1970. *Bourbaki.* Hauppauge, N.Y.: Paideia Press.

Feyerabend, P. 1977. "Changing Patterns of Reconstruction." *British Journal for the Philosophy of Science* 28: 351–382.

Garcia de la Sienra, A. 1982. "The Basic Core of the Marxian Economic Theory." In *Philosophy of Economics,* edited by W. Stegmüller, W. Balzer, and W. Spohn. New York: Springer-Verlag, pp. 118–144.

Hamminga, B. 1982. "Neoclassical Theory Structure and Theory Development: The Ohlin Samuelson Programme in the Theory of International Trade." In *Philosophy of Economics,* edited by W. Stegmüller, W. Balzer, and W. Spohn. New York: Springer-Verlag, pp. 1–15.

Hamminga, B. 1983. *Neoclassical Theory Structure and Theory Development.* New York: Springer-Verlag.

Händler, E.W. 1980a. "The Logical Structure of Modern Neoclassical Static Microeconomic Equilibrium Theory." *Erkenntnis* 15:33–53.

Händler, E.W. 1980b. "The Role of Utility and of Statistical Concepts in Empirical Economics: The Empirical Claims of the Systems of Aggregate Market Supply and Demand Functions Approach." *Erkenntnis* 15:129–157.

Händler, E.W. 1982a. "The Evolution of Economic Theories: A Formal Approach." *Erkenntnis* 18:65–96.

Händler, E.W. 1982b. "Ramsey-Elimination of Utility in Utility Maximizing Regression Approaches." In *Philosophy of Economics,* edited by W. Stegmüller, W. Balzer, and W. Spohn. New York: Springer-Verlag, pp. 41–62.

Hands, D.W. 1985a. "The Logical Reconstruction of Pure Exchange Economics: Another Alternative." *Theory and Decision.* 17, forthcoming.

Hands, D.W. 1985b. "Karl Popper and Economic Methodology: A New Look." *Economics and Philosophy.* 1:83–99.

Haslinger, F. 1982. "Structure and Problems of Equilibrium and Disequilibrium Theory." In *Philosophy of Economics,* edited by W. Stegmüller, W. Balzer, and W. Spohn. New York: Springer-Verlag, pp. 63–84.

Haslinger, F. 1983. "A Logical Reconstruction of Pure Exchange Economics: An Alternative View." *Erkenntnis* 20: 115–129.

Hausman, D.M. 1981a. *Capital, Profits, and Prices.* New York: Columbia University Press.

Hausman, D.M. 1981b. "Are General Equilibrium Theories Explanatory?" In *Philosophy of Economics,* edited by J.C. Pitt. Dordrecht: Reidel, pp. 17–32.

Jaffe, W. 1983. *William Jaffe's Essays on Walras,* edited by D.A. Walker. Cambridge: Cambridge University Press.

Kötter, R. 1982. "General Equilibrium Theory–An Empirical Theory?" In *Philosophy of Economics,* edited by W. Stegmüller, W. Balzer, and W. Spohn. New York: Springer-Verlag, pp. 103–117.

Kuhn, T.S. 1976. "Theory-Change as Structure-Change: Comments on the Sneed Formalism." *Erkenntnis* 10:179–199.

Laudan, L. 1977. *Progress and Its Problems.* Berkeley: University of California Press.

Leinfellner, W. 1983. "Marxian Paradigms versus Microeconomic Structures." In *Epistemology, Methodology, and the Social Sciences,* edited by R.S. Cohen and M.W. Wartofsky. Dordrecht: Reidel, pp. 153–201.

Lloyd, E. 1984. "A Semantic Approach to the Structure of Population Genetics." *Philosophy of Science* 51:242–264.

Mantel, R.R. 1977. "Implications of Microeconomic Theory for Community Excess Demand Functions." In *Frontiers of Quantitative Economics IIIA,* edited by M.D. Intriligator. Amsterdam: North-Holland, pp. 111–126.

McFadden, D., Mas-Colell, A., Mantel, R., and Richter, M.K. 1974. "A Characterization of Community Excess Demand Functions." *Journal of Economic Theory* 9:361–374.

Morishima, M. 1977. *Walras' Economics.* Cambridge: Cambridge University Press.

Moulines, C.U. 1975. "A Logical Reconstruction of Simple Equilibrium Thermodynamics." *Erkenntnis* 9:101–130.

Moulines, C.U. 1976. "Approximate Application of Empirical Theories: A General Explication." *Erkenntnis* 10: 201–227.

Moulines, C.U. 1979. "Theory-Nets and the Evolution of Theories: The Example of Newtonian Mechanics." *Synthese* 41:417–439.

Moulines, C.U. 1983. "On How the Distinction Between History and Philosophy of Science Should Not be Drawn." *Erkenntnis* 19:285–296.

Pearce, D. 1981. "Is There Any Theoretical Justification for a Non-Statement View of Theories?" *Synthese* 46: 1–39.

Pearce, D. 1982. "Stegmüller on Kuhn and Incommensurability." *British Journal for the Philosophy of Science* 33: 389–396.

Pearce, D. and Tucci, M. 1982a. "On the Logical Structure of Some Value Systems of Classical Economics: Marx and Sraffa." *Theory and Decision* 14:155–175.

Pearce, D. and Tucci, M. 1982b. "A General Net Structure for Theoretical Economics." In *Philosophy of Economics,* edited by W. Stegmüller, W. Balzer, and W. Spohn. New York: Springer-Verlag, pp. 85–102.

Popper, K. R. 1959. *The Logic of Scientific Discovery.* New York: Harper.

Shafer, W. and Sonnenschein, H. 1982. "Market Demand and Excess Demand Functions." In *Handbook of Mathematical Economics: Volume II,* edited by K.J. Arrow and M.D. Intriligator. Amsterdam: North-Holland, pp. 671–693.

Shoven, J. B. and Whalley, J. 1984. "Applied General Equilibrium Models of Taxation and International Trade: An Introduction and Survey." *Journal of Economic Literature* 22:1007–1051.

Sneed, J.D. 1971. *The Logical Structure of Mathematical Physics.* Dordrecht: Reidel.

Sneed, J.D. 1976. "Philosophical Problems in the Empirical Science of Science." *Erkenntnis* 10:115–146.

Sneed, J.D. 1982. "The Logical Structure of Bayesian Decision Theory." In *Philosophy of*

Economics, edited by W. Stegmüller, W. Balzer, and W. Spohn. New York: Springer-Verlag, pp. 201–222.

Sneed, J.D. 1983. "Structuralism and Scientific Realism." *Erkenntnis* 19:345–370.

Sonnenschein, H. 1973. "Do Walras' Identity and Continuity Characterize the Class of Community Excess Demand Functions?" *Journal of Economic Theory* 6:345–354.

Stegmüller, W. 1975. "Structures and Dynamics of Theories." *Erkenntnis* 9:75–100.

Stegmüller, W. 1976a. *The Structure and Dynamics of Theories*. New York: Springer-Verlag.

Stegmüller, W. 1976b. "Accidental ('Non-substantial') Theory Change and Theory Dislodgement: To What Extent Logic Can Contribute to a Better Understanding of Certain Phenomena in the Dynamics of Theories." *Erkenntnis* 10:147–178.

Stegmüller, W. 1978. "A Combined Approach to the Dynamics of Theories." *Theory and Decision* 9:39–75.

Stegmüller, W. 1979. *The Structuralist View of Theories*. New York: Springer-Verlag.

Suppe, F. (editor). 1977. *The Structure of Scientific Theories*. 2nd ed.. Urbana: University of Illinois Press.

Suppe, F. 1979. "Theory Structure." In *Current Research in Philosophy of Science*, edited by P.D. Asquith and H.E. Kyburg. East Lansing: Philosophy of Science Association, pp. 317–338.

Suppes, P. 1961. "A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences." In *The Concept and the Role of Model in Mathematics and Natural and Social Science*, edited by H. Freudenthal. Dordrecht: Reidel, pp. 163–177.

Suppes, P. 1979. "The Role of Formal Methods in the Philosophy of Science." In *Current Research in Philosophy of Science*, edited by P. D. Asquith and H. E. Kyburg. East Lansing, Mich.: Philosophy of Science Association, pp. 16–27.

Tuomela, R. 1978. "On the Structuralist Approach to the Dynamics of Theories." *Synthese* 39:211–231.

Weintraub, E. R. 1979. *Microfoundations*. Cambridge: Cambridge University Press.

Weintraub, E.R. 1983. "On the Existence of Competitive Equilibrium: 1930–1954." *Journal of Economic Literature* 21:1–39.