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APPRAISING GENERAL EQUILIBRIUM ANALYSIS

E. Roy Weintraub
Duke University

General equilibrium analysis is a theoretical structure which focuses research in economics. On this point economists and philosophers agree. Yet studies in general equilibrium analyses are not well understood in the sense that, though their importance is recognized, their role in the growth of economic knowledge is a subject of some controversy. Several questions organize an appraisal of general equilibrium analysis. These questions have been variously posed by philosophers of science, economic methodologists, and historians of economic thought. Is general equilibrium analysis a theory[1], a paradigm[2], a scientific research program[3], or a set of interrelated theories[4]? Is it not any of these but rather a branch of applied mathematics[5]? Is GE analysis associated with

This paper is drawn, with some changes, from the author's book *General Equilibrium Analysis: Studies In Appraisal* to be published in 1985 by Cambridge University Press. I have had many comments and criticisms on earlier drafts, and without associating any individual with my own views represented here, I wish to thank, for their help, Neil deMarchi, Wade Hands, Axel Leijonhufvud, Dale Stahl, David Nickerson, Craufurd Goodwin, Bob Coats, Bruce Caldwell, Marjorie McElroy, Colin Day, and Dan Hausman. Our remaining disagreements over some substantive issues have been important in helping me shape what appears here.

1. Mark Blaug, in his *The Methodology of Economics*, suggests in several places that general equilibrium theory is in fact a theory.
2. Sheila Dow finds that general equilibrium analysis can best be considered a Kuhnian paradigm; see her discussion in "Weintraub and Wiles" in the *Journal of Post Keynesian Economics*.
3. I argued this point briefly in my earlier book *Microfoundations* and Blaug appeared to argue in a similar way in some places in his book on methodology.
4. Coddington appeared to suggest this in his *Economic Inquiry* article "The Rationale of General Equilibrium," and Hausman seems to adopt this view in his paper "Are General Equilibrium Theories Explanatory?"
5. This appears to be the current position of the philosopher Alexander Rosenberg as expressed in his 1983 piece in *The Philosophical Forum* titled "If Economics Isn't Science, What Is It?"

the growth of knowledge, or does it waste intellectual resources? How is it related to other work in economics? Is it connected to, or is it apart from, the concerns of applied economists?

Any appraisal of general equilibrium analysis must provide answers to these questions. To compel attention, however, it must do more. A methodological reconstruction of the analysis must not only be consistent with the details of the analytic structure[6], but must explain the (historical) development of that structure. To explain relativistic mechanics we must not only describe the theory but explain the theory as a response to earlier theories. De Broglie's quantum mechanical theory must be linked, in an appraisal, to the earlier theories of Rutherford and Bohr.

The history that I shall reconstruct will be that which I developed in my article "On the Existence of a Competitive Equilibrium: 1930–1954" which appeared in the *Journal of Economic Literature* in 1983. I shall provide, with my appraisal, a rational reconstruction of that case study. That is, I shall reformulate the historical narrative as if it had been a series of rational choices by the economists involved; the history will corroborate the appraisal.[7]

HYPOTHESIS: THE NEOWALRASIAN RESEARCH PROGRAM

Since the days of Walras and Pareto the analysis of interrelated markets has been associated with the "Lausanne Tradition." Such theorizing was well presented in Cassel's *The Theory of Social Economy* which initiated one line of papers. In the late 1930's Hicks's *Value and Capital* promised to reinvigorate the "school" of Walras and Pareto and to blend with their work the capital-theoretic insights of Knut Wicksell. The resulting structure was designed by Hicks to integrate the theory of Keynes, particularly his monetary theory, with the older Lausanne tradition in value theory. Hicks's book was well received. Oscar Lange, Don Patinkin, and others in the 1940s refined and developed the integration of Keynesian monetary theory with the Lausanne value theory: the result is called the neoclassical synthesis.

In an earlier book, *Microfoundations,* I suggested that the microfoundations of macroeconomics literature could be characterized as the

6. This line of attack has caught the attention of the followers of the philosopher Sneed; the recent book edited by Stegmuller et al., *The Philosophy of Economics*, contained several articles by philosophers who attempted to describe the formal structure of what they termed "general equilibrium theory."
7. In what follows, all declarative assertions concerning the history of the analysis are to be understood, without exception, to be drawn from my *JEL* paper; I shall not document each historical reference, but shall leave to the reader the task of joining the historical allusions to the appraisal developed here.

attempt to forge links between two scientific research programs in the sense of Lakatos. I called those programs "the Keynesian program" and the "neo-Walrasian program." For present purposes it is necessary to define the neo-Walrasian program with more care.[8]

A Lakatosian scientific research program, recall, is a complex of linked theories each of which may be subjected to the usual tests of empirical and theoretical science. That is, each theory must be falsifiable in principle, and have some corroborating instances. The program idea suggests that each theory is linked in specific ways to all the other theories in the program. That is, each shares certain elements; each can be developed from a "theory generator" called the heuristics of the program. These heuristics consist of the "rules" for constructing theories — rules not to modify some propositions, the "hard core," and suggestions for generating specific theories and models, the "positive heuristic." The rules and shared presuppositions of all the linked theories, together with those theories, constitute the research program. (See Blaug, 1980; Leijonhufvud, 1976; Weintraub, 1982 and 1985. The best and original presentation of these ideas is Lakatos, 1978.)

I assert that there is a neo-Walrasian research program, and it is characterized by its hard core, its heuristics, and its protective belts.[9] Without claiming novelty, or a definitive statement of the program's character, I assert[10] that the program is organized around the following propositions.

8. Unlike Lakatos, I do not like to give new names to old ideas. The term "neo-Walrasian" may not be the most appropriate choice. I have been motivated to use it out of impatience with the cavalier use of the word "neoclassical" which now seems to be used to describe all that is loved, or abhorred, by the particular analyst. "Neoneoclassical" is an abomination, "general equilibrium" will be shown to be too vague, and "standard economics" nondescript. Use of the term "neo-Walrasian" has the advantage of freshness, but it leaves much to be desired. For example, I can argue, to my own satisfaction, that Chicago economics is neo-Walrasian in the sense of my usage. This appalls Chicago economists, who believe that the term denies the efficacy of the partial equilibrium modelling strategies they employ. Coining a new word to encompass both Chicago and M.I.T. approaches is necessary. I cannot find a simple word that is sufficiently neutral to command assent. It is with reluctance then that I shall use "neo-Walrasian" and grieve that the nice term "neoclassical" has been so abused that its use now offends almost everyone.
9. I am adopting Lakatos's method, though as will be seen I shall argue that, for general equilibrium analysis, the methodology of scientific research programs must be modified, and interpreted, in ways that may appear to be hostile to the original Lakatosian conception.
10. I first presented some of these ideas in the article "Substantive Mountains and Methodological Molehills" in *The Journal of Post Keynesian Economics* in 1982. It is important to recognize that no simple statement of the program's core can be satisfactory; different scholars may, and will, quarrel with my selection of the hard core elements. This is of little concern to me. What is more useful than nit-picking the elements is the style of methodological argument that the Lakatosian framework imposes on the analyst.

HC1. there exist economic agents;
HC2. agents have preferences over outcomes;
HC3. agents independently optimize subject to constraints;
HC4. choices are made in interrelated markets;
HC5. agents have full relevant knowledge;
HC6. observable economic outcomes are coordinated, so they must be discussed with reference to equilibrium states.

The positive and negative heuristics of the program consist of propositions like:

PH1. go forth and construct theories in which economic agents optimize;
PH2. construct theories that make predictions about changes in equilibrium states;
NH1. do not construct theories in which irrational behavior plays any role;
NH2. do not construct theories in which equilibrium has no meaning;
NH3. do not test the hard core propositions; etc.

If these propositions define the program, practitioners will be puzzled and disturbed by those who ask questions about whether agents do, in fact, optimize and whether agents do, in fact, have full knowledge. It is not that these are assumptions that the neo-Walrasian analyst merely likes to make. They are tenets, overriding assumptions, that by the definition of the program are taken as "givens" by those who work in the program. The hard core propositions are only questioned by "outsiders." While the neo-Walrasian may be sympathetic with the concerns of the questioner, to ask questions about the validity of the hard core is to be outside the program.

How can asserting the existence of this program facilitate appraising general equilibrium analysis? Recall that the notion of a Lakatosian research program can be used to reconstruct a line of theorizing and generate a sequence of putative problem shifts. Suppose one were interested in the issue of how to explain the number of children that families have. Suppose that one is a neo-Walrasian. First one would assume that the number of children is an outcome or state that results from choice. One would assume that the agents are well-defined household units called families, and those families have preferences about the number of children. The families are constrained by income, time, etc. The optimizing choice of the number of children is made jointly with the other optimizing choices of the family. These choices are reflected in coordinated market outcomes. Research topics thus include examining how the number of children varies as the mother's income varies, as the father's number of hours worked varies, as the cost of housing varies, as race varies, etc. The resulting sequence of models[11] has an organic unity which results from the fact that the models are all constructed according to the rules, the heuristics of the program, which show how the hard core of the program may be developed into potentially falsifiable theories.[12]

HARDENING THE HARD CORE

In Lakatos's "Falsification and the Methodology of Scientific Research Programs" (1978) he noted that the "'core' is 'irrefutable' by the meth-

11. Appraising work in such an area requires one to identify the sequence of models (M) or theories and to show how each M(t), compared to its predecessor M(t − 1), did or did not predict a novel fact. Comparing M(t) to M(t − 1), did M(t) have excess empirical content, some of which was corroborated? Were M(t) and M(t − 1) linked by shared features which were drawn from the hard core of the program?

    For example, consider how general equilibrium analysis was used in the following situation. Until the mid-1970s, the Utility Commission of the State of North Carolina relied on engineering forecasts of growth in demand for electricity in the various service regions. Those predictions consistently, after the oil shocks of the early 1970s, overestimated demand growth. By the late 1970s, the Public Staff of the Utilities Commission developed a large econometric model, based on demand theory, which was used to forecast demand. This forecast generated a supply response in an associated model, which led to prices (in accounting models) that could earn the utilities a reasonable return; these prices were then compared to the prices that had been parameters of the original demand forecast model. This sequence of interrelated models was in fact a general equilibrium model and the equilibrium price analysis fully imitated the kind of fixed-point theorem that was commonly used to establish the existence of a competitive equilibrium price vector in more abstract general equilibrium structures.

    Since the price of electricity, given a nonzero demand elasticity, was the only restraining influence on demand growth, it had been shown that engineering forecasting models, which ignored prices, necessarily overstated demand growth. The Public Staff's excellent model analysis has thus led to a reining-in of system capacity growth, and has been the intellectual linchpin for the decision to abandon construction plans for several nuclear power plants in the service region.

    More importantly, perhaps, the models themselves can frame questions of how conservation and load-management programs can affect load growth and construction schedules. My own view is that such general equilibrium models have saved ratepayers billions of dollars. One who claims that general equilibrium analysis is not explanatory, or that the analysis is not empirical, is simply a prisoner of textbook presentations. It is almost a scandal that philosophers of economics do not seem to understand the role of applied economics, and the work that has been done in applied econometrics. (See Public Staff, 1979 and 1981; see also ICF, Inc., 1981; and Miedema et al., 1981.)
12. There is nothing hidden in an appraisal developed along Lakatosian lines. There are many difficulties with Lakatosian appraisals, however, which space limitations here prevent discussing. The fuller development of these objections, and my attempt to modify the Lakatosian framework, is presented in the book from which this article is drawn. For present purposes, however, it suffices to note that *if there are any Lakatosian programs in economics, the neo-Walrasian program must be one of them. Further, the Lakatosian approach to appraising work in economics is a starting place, not a final destination.*

odological decision of its proponents: anomalies must lead to changes only in the 'protective' belt of auxiliary, 'observational' hypotheses and initial conditions" (p. 48). Lakatos had a footnote to this sentence which noted: "The actual hard core of a programme does not actually emerge fully armed like Athene from the head of Zeus. It develops slowly, by a long, preliminary process of trial and error. In this paper this process is not discussed" (p. 48).

In "Schools, 'Revolutions,' and Research Programs" (1976)[13], Axel Leijonhufvud remarked: "'Hard cores' do not always spring fully armed from the brow of some venerated Thunderer. Surely, they usually take a considerable time to 'harden.'" Yet, Lakatos tells us little about this "hardening process." We probably need a theoretical account of it and criteria for recognizing it (before it is completed), for the process whereby a hard core hardens is apt, I believe, to bear at least some superficial resemblances to the activities mentioned by Lakatos as characterizing a "'degenerating' research programme" (p. 79).

It is my contention that the sequence beginning with the Schlesinger paper and continuing through those of Wald, von Neumann, Koopmans, Arrow, Debreu, and McKenzie should be recognized as hardening the hard core of the neo-Walrasian research program. This hypothesis makes sense out of the historical record.

The hard core of the program, as outlined above, contains a number of propositions. By having numbered the statements of this hard core I appear to suggest that the core *in that form* was present from the outset, that it indeed served to organize the sequence of models and theoretical analyses as early as 1930. This perception is inaccurate. The hard core as presented can be said to have existed only as early as the early 1950s. *The recognition that Arrow, Debreu, and McKenzie had accomplished a major feat was precisely the recognition that the hard core of the neo-Walrasian program was, by their work, no longer problematic.* This is a strong claim. What evidence supports it?

Consider the several propositions HC1–HC6. In different forms each of these could have been framed by Walras in the sense that he would

13. See *Method and Appraisal in Economics*, edited by Latsis, which collected essays on the application of Lakatosian methods to economics. It should be noted here that in the next few pages, I leave the idea of "hardening" undefined, and take it to mean a collection of activities associated with developing the hard core. This process will thus involve formal consistency analysis, interpreting the core propositions, and reinterpreting them, and refining the meaning of the linkages between the core and the heuristics, and the core and the belts.

I shall not present a full theory of the hardening process. Rather, I sketch the elements of this process as it appears to have worked in the neo-Walrasian program. It is more appropriate for professional philosophers to explicate the details of this process in general terms, and for other programs, and to assay the worth of this idea for reconstruction and appraisal of the growth of scientific knowledge.

not have had difficulty providing an interpretation of each so that each would have been an acceptable working hypothesis. Yet the Walrasian agent called the producer had few choices to make (with a fixed coefficient technology), utility functions and not preferences were basic, and the knowledge assumptions were only implicit. The final core proposition, that equilibrium states did in fact exist, was assumed by Cassel although there had been attempts by Walras and Edgeworth to argue that this proposition was consistent with the other propositions. They appeared to think it desirable to show that an equilibrium could exist and could be derived in models in which the other hard core propositions were assumed to hold simultaneously.

Look at the positive heuristic of the program, and recognize that it, in Lakatos's view, "consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research programme, how to modify, sophisticate, the 'refutable protective belt'" (Lakatos, 1978, p. 50). In the neo-Walrasian program this leads the analyst to construct theories based on optimizing choice and equilibrium outcomes, and to explore the effect of giving different interpretations, in different models, to the imprecise terms like agent, outcome, knowledge, equilibrium, and market. In order to believe that such a research strategy could produce any result at all (where results, recall, consist of propositions about equilibrium states) it is certainly required that the analysis not lead, necessarily and in every case, to nonsense. That is, it must at least be logically possible that the propositions about optimizing agents be consistent with the proposition about equilibrium. If they are inconsistent, then the positive heuristic's rallying cry, to go forth and construct models with equilibrium outcomes, leads to a quest for a mathematical economic unicorn.

The situation is quite similar to one that comes up in any formal system, and is recognizable to anyone who has studied geometry. Consider a set of axioms about lines and points which together define Euclidean geometry. These axioms, of course, contain words or phrases that are initially uninterpreted. Line is simply a word that is used in a certain way. Now drop the Euclidean line intersection postulate and replace it with the axiom "two nonparallel lines meet in exactly two points." The geometer is charged to go out and prove theorems about this system of axioms. Can any be proved? If the axioms are inconsistent, then all effort will be ridiculous, since any proposition whatsoever follows from an inconsistency. The test of consistency is well known: does there exist a model of the axioms? That is, does there exist a set of interpretations of the terms of the system such that the axioms are, in fact, under that interpretation, true? For the geometry just presented, interpret "point" as a point on the surface of a sphere and a line as a great circle passing through two diametrically opposite points on the surface. Then the intersection postulate is true, even though the "perfect sphere" does not exist in the world of our senses.

The situation is analogous for the hard core since the hard core functions much like axioms for a program; one must accept the Euclidean parallel postulate if one is doing Euclidean geometry, and one must accept the hard core proposition "agents optimize" to work in the neo-Walrasian program. The question then arises: are the hard core propositions of the neo-Walrasian research program consistent? A demonstration of consistency requires a model for the propositions in the sense that the terms of the propositions are interpreted in that model and that, for the model, the propositions are true. This entails the following: given the set of propositions HC1–HC6, the theorist must create a model in which terms like "agent," "preference," "optimize," "constraint," and "equilibrium" have well-defined meanings. One must then show that the propositions are true. For the neo-Walrasian program this requires a model such that HC1–HC5 are taken to be true and that HC6 is true when they are. In other words, consistency requires the production of a model in which a competitive equilibrium exists. The entire line of papers which culminated in those of McKenzie and Arrow-Debreu are exactly of this form. This can be seen from a rational reconstruction of the history of the existence theorems.

A LOGICAL RECONSTRUCTION

In or around 1930, the work in general equilibrium analysis had produced the following situation: there was a model of two classes of agents and teir actions in the sense that there was a partial model of factor demands and supplies, and a partial model of the supply and demand for final goods and services, and these two sectors were at least partially integrated. The choices of the agents were optimizing choices and the notion of equilibrium was understood as a balancing of forces. Equilibrium was not taken to be something that had to be proved, however. Instead it was a feature of the situation that was believed to be consistent with the various assumptions; asserting that there was a balance of forces (a primitive notion of equilibrium) did not appear to violate any of the other assumptions of the model.

The appearance was not the reality. In a series of papers, von Stackelberg and Zeuthan showed that the equilibrium notion could violate the coordination assumption in the sense that negative prices, which do not coordinate any market outcome, could not be ruled out as solutions of the model. Schlesinger was then able to show how the assumptions of the model could be rephrased to allow agent choice, at least in a primitive production model, to be based on an optimization framework. His choice structure precluded the possibility of negative prices. The Schlesinger model also extended the idea of markets and goods to allow free goods to result from market choices and equilibrium outcomes. His model analysis did not include a demonstration that equilibrium existed; it simply presented assumptions about the agents, their choices, the markets, and the equilibrium. In other words, this model interpreted the assumptions in a new way which made those assumptions mutually coherent.[14]

It was necessary, for this model, to show that the assumptions about agents, markets, and optimization entailed the existence of a competitive equilibrium. The proof was contained in the first Wald paper under the restriction that cross-commodity interrelationships were not present in the final demand for goods. The second Wald paper removed this restriction. The third Wald paper on exchange apparently made more coherent assumptions about the market structure by detailing the exchange context of the optimizing choices.[15]

Simultaneously, von Neumann presented a different model, based on a different tradition, which nonetheless made assumptions about agent choice, optimization, markets, and equilibrium. The von Neumann model was a disaggregated growth model, yet the issues were identical to those already identified. Von Neumann himself proved that there existed an equilibrium: the assumptions he had made were thus mutually consistent. By the mid-1930s there were thus two general equilibrium models, out of different traditions perhaps, which were nonetheless similar in the sense that each interpreted the components of the hard core of the neo-Walrasian program. Each provided interpretations of the imprecise terms of the core in such a manner that, by means of an existence proof, the propositions of the core could be seen to be mutually consistent; heuristics based on the core would not lead to incoherent theories.

These models provided only narrow interpretations of the uninterpreted terms of the hard core. Very few theorems could thus be created in the protective belt. Hicks's model expanded the set of permissible interpretations with explicit and general concepts of agent, optimizing choice (especially the choices of the households), commodities (by al-

14. Though not necessarily mutually consistent.
15. This paper was not published, however, and only the result, not the proof, was ever reported. It is worth noting that consistency merely requires production of a model in which HC1–HC6 are true. In fact all the papers are of the form "Assume HC1–HC5; is there a model in which HC6 can be deduced?" Why not try to show that HC3 is entailed by the other core propositions? The answer is that the production of such models is too easy. One can always "load" the information assumption with interpretations that entail existence of uninteresting equilibria.

lowing time and capital goods), and a form of money. He also provided a more general notion of equilibrium which respected the interpretations of the other hard core terms. Hicks did not prove that the resulting set of propositions was mutually consistent. Neither did Lange or Samuelson who themselves further extended the interpretations of the basic terms. Their work, however, permitted the hard core to generate theories in the protective belt of the neo-Walrasian program in such a way that those theories could be compared to the theories in the belt of the Keynesian program. Patinkin's work in the 1940s created this tradition. His interpretation of the terms in the core extended its monetary theoretic reach.[16]

The work of the late 1940s modified the interpretations of Hicks and Patinkin. Although both had a very rich conceptual framework of agents, their choices, and the markets in which those choices were made manifest, interpreting production choice was a problem. Koopmans's production choice model was developed at this time and was quickly incorporated into the standard general equilibrium model. That is, the emerging standard model involved Hicks's households with preferences and optimizing choices, Koopmans's production choice theory, Patinkin's rich framework of market interrelationships, and the idea of equilibrium as the potential prereconciliation of plans, a notion probably derived from Samuelson and Frisch. Those assumptions, which were really interpretations of the hard core propositions, led to a family of models with consistent interpretations. Within a short period of time Arrow and Debreu, and McKenzie, independently proved the existence of a competitive equilibrium in such models. Those models, now demonstrated to be consistent, have been the basis for further work up to the present. Recent studies have extended the interpretations of terms like money, knowledge, and market, allowing the theories in the protective belt to be directed to new sets of questions.[17]

This reconstruction of the series of papers on the existence of a competitive equilibrium corroborates my claim that there exists a neo-Walrasian research program in the sense of Lakatos. The sequence of papers represents a hardening of the hard core of that program.

16. Patinkin was aware that a full-scale existence proof was desirable and referred the reader to the proofs of Wald and von Neumann. The difficulty was that their proofs of existence were for much less complex models, the terms of which had more primitive, and restricted, interpretations than did Patinkin's.

17. For example, one cannot go forth and construct theories about inflation or persistent unemployment in the neo-Walrasian program unless the commodity concept subsumes a rich concept of money. One has to prove existence of equilibrium for such an interpreted model; if no such equilibrium can be found then the call to go forth to construct theories is hollow indeed.

"Hardening of the hard core" is similar to a progressive problem shift in the belt of the program."[18]

WHAT IS GENERAL EQUILIBRIUM THEORY?

I have argued that there is a neo-Walrasian research program and that the sequence of papers, from Schlesinger's to McKenzie's, constituted the hardening of the hard core of that program. The hardening process proceeded as any mathematical investigation; consequently, we have to appraise it as we would any research line in mathematics. We have to ask about the strength of the theorems that were proved, the counterexamples that were produced along the way, and how they were incorporated into the theorem structure as a sequence of lemmas. The line of theorems in the sequence of papers which have been seized upon by economists and philosophers alike as a testament to the unscientificness of economics, is thus rather a natural progression in the development of any scientific research program. Leijonhufvud alluded to the fact that the hardening of the hard core would, if we could identify it, look suspiciously like programmatic degeneration. His insight had been insufficiently heeded. The sequence has appeared to economists and philosophers alike as an example of everything that is wrong with economics.

Falsificationists[19] have argued that work on general equilibrium models is wasted energy that could better be spent developing falsifying instances of previous GE models. For such Popperians, the production of new existence proofs is itself proof that the theory lacks empirical content. In their view there is too much activity which substitutes mindless mathematizing for hard scientific analysis. *We can now see that this view is mistaken. What may seem like "more and more of the same old model" is rather a developing set of interpretative extensions of the imprecise terms of the programmatic hard core.* What may appear to be nonchalance about the empirical referents of the competitive equilibrium models now appears to be a sensible division of labor. Some theorists develop models that can frame the basic facts about our daily economic life: time matters, expectations matter, money matters, inflation matters, exhaustible resources matter, etc. Other theorists and applied economists test those frameworks. The fact that the analyst places such topics on the agenda for general equilibrium modelling is neither a sign of mental weakness nor an avoidance mechanism. It is, rather, an act of intellectual heroism.

18. This idea corresponds quite well to what Lakatos called a theoretically progressive problem shift. It does not, of course, correspond to the Lakatosian notion of progressivity which requires that there be empirical testing, corroboration, of the excess content or extended interpretation.

19. Blaug, for instance, appears to adopt this position in his *Methodology of Economics*.

It shows a willingness to expose the models to worldly tests which predict events with the theories from the protective belt of the neo-Walrasian program. It is nonsense to test a theory about the demand for electricity in the state of North Carolina, a theory dependent on the theory of demand for durable goods, if there is no model containing both capital goods and an equilibrium notion.[20]

Thus the arguments of the philosopher Rosenberg (cf. footnote 7), that GE analysis and much of economic theory is not empirical, are only partly correct. He has fairly characterized as a mathematical activity the creation of extended interpretations of the hard core of the program. He has accurately represented the sequence of papers on existence of equilibrium as a kind of applied mathematics. What he has failed to notice is that those activities form only a part of the program of neo-Walrasian economics. He has criticized the program for not being empirical by examining only the hard core of the program, thus conflating the axiom structure with the interpreted theorems.[21]

Hence there are two approaches which must be utilized in an appraisal of general equilibrium analysis. First, we use criteria appropriate for gauging mathematical progress to measure the growth of knowledge associated with hardening the core of the program. Second, we use traditional appraisal techniques to evaluate the work in the belt of that hard core. Such derived theories, like demand theory, like human capital theory, like the theory of effective protection, must indeed be tested and corroborated, since they form the sequence of theories or models in a particular portion of the protective belt. It is thus appropriate to ask whether the theory of demand is progressive in both theoretical and empirical senses. It is not appropriate to ask that the theory of general equilibrium be empirically progressive, since that "theory" is not a theory at all if one is referring to the various papers on the existence, unique-

20. Consider the effect of changing the electricity rate structure from declining block rates to time-of-day rates for residential users. Analysis requires that equilibrium usage will change in a predictable fashion as rates change; it is a general equilibrium model that allows that particular set of inferences to be drawn. That model must include interrelated demands by the various agents, which are usually taken to be households, manufacturing firms, commercial firms, and municipalities. It must also include the supply response under specific regulatory constraints, and the interlinked response of price to conservation to price. It is the neo-Walrasian program within which such partial equilibrium theorizing makes sense, and only within that program can such inferences be valid predictions. (See Miedma et al., 1981.)

21. When he does ask questions about predictions, he uses the case of predictions of large macroeconomic models, which are only remotely related to the neo-Walrasian program. A better case would have been along the lines I have suggested; for example, the sequence of papers on load forecasts for the electricity industry in a particular state, or the sequence of papers on black-white earnings differentials. This criticism is developed by Hands in his "What Economics Is Not: An Economist's Reply to Rosenberg."

ness, or stability of equilibrium in ever more richly interpreted models. *It is a category mistake to ask about the falsifiability of the Arrow-Debreu-McKenzie model.*

DUALISTIC PROGRAM APPRAISALS

There are two separate issues involved in an appraisal of the neo-Walrasian research program. First, there is the fact that work associated with hardening the hard core of the program is appropriately evaluated according to the rules of appraisal usually applied to the development of mathematical theories. There is no generally accepted way of producing such appraisals, however. The nature and development of mathematics is a subject that, like the nature and development of empirical science, has not been settled in the philosophical literature. That granted, however, I am content to use the framework for appraising mathematical work developed in Lakatos's *Proofs and Refutations* (1976). The idea of Lakatos's book is that theorem creation is an activity in which the mathematician simultaneously attempts to prove and disprove the theorem; proofs are based on lemmas while the disproof proceeds through a search for counterexamples to the main theorem. With this procedure, some counterexamples and exceptional cases are built into the revised versions of the theorem or into the lemmas that structure the proof itself. The process of proving a theorem is a process of discovery.[22] The theorem that emerges from the back and forth process between proof and preliminary theorem may bear little resemblance to the original conjecture.

For theories in the protective belt of the program, developed out of the hard core and the heuristics, appropriate appraisals will be more traditional. Thus it is entirely appropriate to ask that demand theory, or production theory, or the theory of household labor supply, be evaluated using the same criteria which are applied to physical theories. Likewise it is appropriate to ask whether the theory of black-white earnings differentials is progressive in the Lakatosian sense. Has it been theoretically progressive as successive variants have explained the corroborated content of the predecessor? Has there been excess content in the sense that successor theories have made new predictions? Progressivity requires that there be empirical progress as well, so it must be the case that at least some of the successor's excess content is corroborated.

It may be argued that this dualistic approach to appraisal replaces one problem, which has no accepted solution, with two problems, neither of which can be considered to be "solved." It may further be argued

22. This same framework appears to motivate the recent popular exposition of mathematical work, *The Mathematical Experience* by Davis and Hersh.

that the approach is too lenient, that it permits too much to be packed into the hard core and insulates too much analysis from the rigors of the testing-corroboration-falsification process. Is it not the case, some might argue, that what has been presented is fine for the period during which the hard core is hardening, but is no longer appropriate during the time periods, which are relatively longer, when the hard core is fixed and serves as an immutable generator of theories in the protective belt. The answer to this question is a simple one. The history of the papers on existence of equilibrium from 1930 to 1954 shows that the hard core may take a long time to form, to harden. The basic propositions of the hard core were not really developed, were not appropriately interpreted, until the early 1950s. It is certainly not the case that the hard core of the neo-Walrasian program had been around since the time of Walras. The terms of the core functioned in 1900 in a way that hardly resembled the way they functioned in the 1960s. It is inconceivable that the way Roy Radner introduced uncertainty into the structure of general equilibrium models was a possibility for even a clever analyst in the year 1910. The monetary theoretic models that were developed by Patinkin cannot be located in the organizing center of Cassel's book.

Consequently it must be the case that the hard core is not so fixed as a traditional Lakatosian appraisal may seem to suggest. One must not mistake the form of the hard core proposition "agents optimize" for its interpretation which is not fixed. As the core hardens, its propositions are interpreted in ever more flexible ways. This process should be familiar to those economists and philosophers who have read Thomas Kuhn, for Kuhn spent a great deal of time showing how concepts, when first presented, contained little of the packed content, the associated interpretations, of the finished version. The history of a concept like "agent," which must be interpreted in the hard core of the neo-Walrasian program, did not develop linearly. History takes time. Progress is not of an instant.[23]

23. The refinement of the hard core, the hardening as it were, was not even completed by 1954. It continues today. The new classical economics is a good example of how theories in the protective belt of the neo-Walrasian program are generated when the hard core is reinterpreted without being amended. If the hard core proposition "agents optimize" circa 1954 is now "packed" with the concept that one of the objects of the optimizing choice is the set of expectations of the future values of the choice variables, then the hard core supports the theories of rational expectations. The literature on rational expectations contains many existence proofs which, in outline, show that the idea of optimization in this extended interpretation is not inconsistent with the corresponding idea of equilibrium as a coordinated outcome for this kind of interpreted model. The hard core needs to be represented in a new model to test whether the newly interpreted terms of the core are mutually consistent. There is no reason why they must be so. Indeed if the richer hard core precludes a sensible concept of equilibrium, then rational expectations theories of the supply of an agricultural commodity, say, cannot be generated and redesigned upon falsification since the theory generator, the hard core, is logically flawed and incapable of supporting a theory sequence.

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