History of Political Economy 20:1

CCC 0018-2702/88/$1.50

The empirical progressiveness of the general equilibrium research program

Arthur M. Diamond, Jr.

I. *Introduction*

The vast majority of economists and of economic methodologists believe that economics is in some sense an empirical science (Klamer, 344). One primary criterion for appraising theories is thus the extent to which the theory increases the empirical applicability of economics. We do not ask here whether empirical applicability *should* be a criterion for evaluating theories in economics. Nor do we ask whether other broad criteria, such as aesthetic beauty, are equally important in appraising theories. What we do ask is whether the general equilibrium research program has a record of empirical applicability.

Over the decades various methodologies have been popular in explaining how to appraise the empirical applicability of theories (see McCloskey 1986b, 6). We adopt the terminology of Lakatos' methodology of scientific research programs mainly because it is currently the most popular methodological position among economic methodologists.[1] The key advantage in adopting a popular methodology is the reduction in the space that must be given over to mainly definitional and context-setting issues. For example, those familiar with Lakatos will understand that 'empirical applicability' in his methodology is explicated by the expression 'empirical progressiveness' which measures the extent to which successive models within a research program explain more and more new phenomena.

Although 'empirical progressiveness' in a Lakatosian framework is arguably more precise than 'empirical applicability,' the former phrase is used in distinct senses that should be explicitly noted before we proceed.

Correspondence may be addressed to the author, Dept. of Economics, University of Nebraska at Omaha, Omaha, Neb.

1. See McCloskey 1986b, 6, and Hands 1985. The popularity of Lakatosian methodology among economists does not seem to have been diminished by Toulmin and Feyerabend's cogent arguments that Lakatos, despite his rhetoric, did not succeed any better than Kuhn in accounting for the rationality of science (see Toulmin 1972, 482, 1975, 384–91, and 1976, 665–75; and Feyerabend, 185–86). We do not concern ourselves with this issue, however, because empirical applicability is a robust concern of almost all serious methodologies of economics. The discussion here could thus have been expressed in the language of any of several other methodologies that in some sense accept empirical applicability as a criterion for appraising economic theories.

First, a research program can be empirically progressive in both a prospective and a retrospective sense. Even though a program has not resulted in successive models explaining an increasing range of phenomena in the past, an advocate of the program can consistently maintain that the program will do so in the future. Thus, a prospective claim that a research program will turn out to be empirically progressive is never subject to strict test although the credibility of the claim diminishes the longer it remains unconfirmed.

Second, a research program could also be judged empirically progressive in either an absolute or a relative sense. In the absolute sense any research program might be judged empirically progressive if successive models in the program explain an increasing range of new phenomena, even if the increase is minuscule. In the relative sense a research program might be judged empirically progressive if successive models in the program result in a greater increase in the range of new phenomena explained than did successive models in an alternative research program.

Finally, Lakatos himself argued that to be empirically progressive a research program had to explain new facts where 'new facts' were stringently defined as those that are "improbable or even impossible in the light of previous knowledge" (1970, 18). Even those generally sympathetic to Lakatos have recognized that his definition here is too stringent (Zahar, 103). De Marchi (p. 110), for instance, adopts the less stringent and more plausible position that "a known fact may be novel with respect to a given hypothesis or theory if it is accounted for by that theory *without* the theory's having been specifically designed with that end in view." Note that even by this less stringent definition, a research program could be empirically applicable without being empirically progressive. Evidence that a research program was not empirically applicable would be, a fortiori, evidence that the program was not empirically progressive (in the retrospective sense). On the other hand, evidence that the research program was empirically applicable would have to pass the additional screen that enough of the applications be 'new' before the program could be judged to be empirically progressive (in the retrospective sense).

We seek, then, to appraise the retrospective empirical progressiveness of the general equilibrium research program. Weintraub has emphasized the vagueness of 'general equilibrium theory' as the phrase is used by economists and methodologists (Weintraub 1985b, 43 and 121). We adopt the related phrase 'general equilibrium research program' because it is less awkward and is closer to conventional usage than the alternatives. Specifically, the phrase is used here to refer to research carried out since roughly 1950 that builds on what Weintraub calls "the ADM equilibrium notion" (p. 127) where ADM stands for Arrow-Debreu-McKenzie. In the sense that we adopt here, the 'general equilibrium research program' is concerned "largely with the analytic structures of theoretical economic models, often highlighting the formal similarity of these structures, and clarifying the conditions for consistency, equilibrium, stability and optimality" (Lindbeck 1985, 39).

The mathematical models that have resulted from the general equilibrium research program (GERP) are frequently criticized as being too complex, too abstract, or too unrealistic to be useful in explaining observed human behavior.[2] Most defenders of the GERP assert, to the contrary, that somehow the GERP is relevant to explaining such behavior. Hahn, for instance, reports that "the student of GE believes that he has a starting point from which it is possible to advance towards a descriptive theory" (1973, 324). Arrow, for another instance (1974, 4), boldly claims that the GERP must be judged by the empirical standards set forth in Friedman's 1953 'The methodology of positive economics.'

Even though most defenders of the GERP assert its empirical progressiveness, we should note that a few admit that the program does not provide explanations of human behavior. Hausman, for instance, suggests that the GERP (using our definition, not his) has failed to advance explanations of empirical phenomena, but instead has provided economists with a "sort of theoretical reassurance" (1981, 29).

At least two alternatives to the GERP can be identified within the neoclassical tradition. The partial equilibrium alternative is defended on the grounds that the economist "must allocate the limited resources available to him for a particular study in the most efficient manner, which means considering just enough variables to obtain sufficiently accurate answers" (Becker 1971, 5).[3] In addition to partial equilibrium analysis, the alternatives to the GERP also include analysis in the Walrasian tradition that does not depend on the Arrow-Debreu-McKenzie equilibrium notion. This is important to emphasize, because criticisms of the GERP are often answered with defenses based on the fruits of the simpler Walrasian notion. When the debate takes this turn, the disputants are talking past each other (perhaps for rhetorical reasons, perhaps owing to genuine confusion about what the other side means by 'general equilibrium theory').

In the subtlest and most extended of the defenses of the GERP, Roy

2. Hands 1984, 122, includes among such critics Blaug, 257–59; Clower 1975, 10; Coddington 1972 and 1975; Handler 1980a, 50–51, and 1980b, 154–55; Hausman, 28 and passim; Hutchison 1976; Kaldor 1972; Latsis 1976; and Rosenberg 1980 (although they are not in Hands, we include some page references for the convenience of the reader). Hands might also have included Rosenberg 1983, 311, who, based largely on the high esteem in which the GERP is held within the economics profession, concluded that we should "give up the notion that economics any longer has the aims or makes the claims of an empirical science of human behavior."

3. That Becker's position is not uncommon is attested to by Frank Hahn, a participant in the GERP, who has remarked: "GE is frequently taken to task for being (say in comparison with the Marshallian tradition) too complicated and general to be useful" (1973, 326).

Weintraub argues that the GERP is the hard core of a Lakatosian scientific research program that has as one of its most fruitful results Becker's "new home economics" (1985b, 25). The defense has been further elaborated and refined in a paper by Weintraub that is to be published in a forthcoming volume edited by Neil de Marchi (Weintraub, 1985c). Partly because of the care of his historical account of the GERP, Weintraub's appraisal has deservedly received considerable careful consideration by economic methodologists. For that reason we briefly summarize and analyze his gambits.

Weintraub 1985b exhibits a split personality on the empirical progressiveness of the GERP. The first, a Lakatosian whom we will identify as Weintraub$_1$, argues that the GERP consists of the hardening of the hard core of a neo-Walrasian research program which is itself empirically progressive. The second, a McCloskey-Klamerian whom we will identify as Weintraub$_2$, argues that the GERP is a discourse whose confrontation with practical economics can only be analyzed with tools from rhetoric (1985b, 174). Weintraub$_1$ is not only the dominant personality in most of the book but is also the personality that Weintraub chooses to exhibit in his paper-length summary of what is important in the book (see 1985a) and in his elaboration and refinement of the argument in the book (see 1985c). In addition, Weintraub$_1$ is closest in spirit to Weintraub's earlier writings on this subject (see 1979). In the future we may see more of Weintraub$_2$ since that personality seems the dominant one in one of Weintraub's most recent working papers on equilibrium (see 1986). Here we focus on Weintraub$_1$.

Weintraub$_1$ defines the neo-Walrasian program so broadly that almost all of Western economics falls under the program. The only programs identified as falling outside of the neo-Walrasian are the neo-Keynesian and the Austrian. Weintraub$_1$ identifies six statements as constituting the hard core of the neo-Walrasian research program (1985a, 26; 1985b, 109; and 1985c, 2–3). These statements are:

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 references to equilibrium states.

The hard core propositions imply guidelines for fruitful research that are described as the positive and negative heuristics (which means: do's and don'ts):

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.

Weintraub$_1$ claims that the hard core statements and the heuristics "define" (1985a, 26; 1985b, 109) the neo-Walrasian research program. If so, then that program is very broad indeed. The breadth is a serious disadvantage if we are interested in appraising the ADM equilibrium notion (which we are calling the GERP). The problem is that all of the statements of the hard core and heuristics could easily have been accepted by an economist who knew nothing of work in the GERP since 1950. Accepting that the neo-Walrasian research program has been empirically progressive, we still may inquire how the "hardening of the hard core" has contributed to that progressiveness (see Hands 1985, 11).

The examples usually given of the empirical progressiveness of the program include the development of simultaneous estimation techniques in econometrics, Leontief's input-output analysis, and the elaboration of Leontief's analysis to consider the effects of tax and tariff policy (Shoven & Whalley 1984).[4] In addition to the usual examples, Weintraub$_1$ adds the examples of Becker's household economics (Weintraub 1985b, 25) and of extensions of Becker's household economics by McElroy & Horney (Weintraub 1985c, 3–11). Simultaneous-equations techniques and Leontief's input-output analysis depend on the simpler Walrasian notion but do not depend in any clear way on the Arrow-Debreu-McKenzie equilibrium notion that we are associating with the GERP. For that reason we will not consider these examples further, but will instead briefly consider the extension of Becker's household economics and the applied general equilibrium models developed by Shoven, Whalley, and others.

In his discussion of a series of three papers (the first two by McElroy & Horney, the third by McElroy) Weintraub$_1$ makes a credible case that the papers show the empirical progressiveness of the research program that they arose from. What Weintraub$_1$ does not show (and, in fairness, may never have intended to show) is that the research program exemplified by the papers is the GERP. Probably few would dispute that the gambits of McElroy and Horney are consistent with the positive and negative heuristics that Weintraub$_1$ has laid out as defining the neo-Walrasian research program. But Weintraub$_1$ does not undertake the more difficult task of

4. Hahn also lists a few policy issues that he claims can be better understood by those who are familiar with the results of the GERP (1973, 324).

showing how the papers of McElroy and Horney depend in an important way on the work derived from the Arrow-Debreu-McKenzie equilibrium notion.

Perhaps the most prima facie plausible example of the empirical progressiveness of the GERP is the work on applied general equilibrium models developed by Scarf, Shoven, Whalley, and others (see Shoven & Whalley 1984; Scarf & Shoven and Ballard et al. 1985). A 1984 volume edited by Scarf and Shoven, for instance, discusses techniques that in principle permit the estimation of prices in a general equilibrium system. Although the models that incorporate these techniques clearly make use of the Arrow-Debreu-McKenzie equilibrium notion, they are not fully consistent with all of the heuristics that we would associate with the GERP. We have not spelled out these heuristics for the GERP in the way that Weintraub$_1$ has for the simpler neo-Walrasian program. But surely somewhere on the list would be a positive heuristic that reads something like this: Be very careful to rigorously establish the uniqueness or non-uniqueness of the equilibria that result from your models.[5] Consider then that for the applied general equilibrium models, "there is . . . no theoretical argument that guarantees uniqueness" (Shoven & Whalley, 1015). That in itself might not be a violation of the heuristic, if finding a proof of uniqueness was high on the applied theorists' agenda. However, in their useful survey article Shoven and Whalley go on to note that "The current working hypothesis adopted by most modelers seems to be that uniqueness can be presumed for all of the models discussed here until a clear case of non-uniqueness is found" (p. 1015).

Even if we grant that the applied general equilibrium models are a part of the GERP, we still must determine whether or not they have been empirically progressive. One problem in judging the empirical progressiveness of the models is that their authors have so far seemed more interested in deriving the policy implications of their models than they have in demonstrating that successive models explain more and more new empirical phenomena. Shoven and Whalley confirm this when they observe: "Applied general equilibrium analyses . . . are attempts to assemble and use models for policy evaluation" (p. 1015). Earlier in the same survey they observe: "A question frequently addressed by these models is whether any particular policy change is welfare-improving" (p. 1013). Although the welfare conclusions of general equilibrium models can be compared with those of partial equilibrium models (see Whalley 1975) they are very difficult to test empirically. Therefore differences between models in their

5. Frank Hahn reports that as one who "only recently shifted from General Equilibrium Analysis" into macroeconomics, he was "dismayed" by the lack of rigor in rational expectations models. One sort of evidence for the lack of rigor of the models was the "assertions of the uniqueness of equilibria without proof" (1986, 276).

welfare implications are not by themselves sufficient to determine which models are better at explaining the empirical phenomena. In addition, the future usefulness of the applied general equilibrium models is still in doubt for several reasons—among them, the data requirements for estimating the prices, the assumption that the data were obtained from an economy in equilibrium, as well as the need to use elasticity estimates obtained either from intuition or from earlier partial equilibrium studies.[6]

We conclude that although Weintraub$_1$ has contributed to the history of the GERP, and although he has argued persuasively for the empirical progressiveness of the broadly defined neo-Walrasian research program, he has not yet established the empirical progressiveness of the GERP.

As we noted earlier, no strict test can be performed of the prospective empirical progressiveness of the GERP. Therefore, we propose a more direct approach to the question of progressiveness. The empirical method to be discussed in the next section measures the past empirical applicability of the GERP. As we noted in the introduction to this article, empirical applicability is a necessary but not sufficient condition for retrospective empirical progressiveness. If we find little evidence of empirical applicability, then we will, a fortiori, have little evidence of retrospective empirical progressiveness. If, on the other hand, we find significant evidence of empirical applicability, then a judgment concerning empirical progressivity will have to await a detailed examination of how many of the applications are *new* applications.

II. *Method*

The Coles have claimed (p. 384) that citations are one currency with which a scientist rewards those other scientists whose work has been important to his own. If so, then the relevance of the GERP to empirical work should be observable in the citations made by empirically oriented economists to the GERP economists. In order to measure the GERP's empirical applicability, we analyze the work of the economist whose name is most closely associated with the research program: 1983 Nobel Prize winner Gerard Debreu.[7]

6. For a candid discussion of these and other problems with the estimation techniques see Shoven & Whalley, 1018, 1033, and 1044; and Mansur & Whalley, 118–19.

7. In 1983 the Alfred Nobel Memorial Prize in Economic Sciences was awarded to Gerard Debreu "for having introduced new analytical methods into economic theory and for his rigorous reformulation of the theory of general equilibrium." According to the last will and testament of Alfred Nobel, the prizes in the sciences were intended to reward "those who, during the preceding year, shall have conferred the greatest benefit on mankind" (in Odelberg, 10). Although the prizes in economics were established in 1968 through separate funding from the Central Bank of Sweden, the intent remained to reward work that has "the eminent significance expressed in the Will of Alfred Nobel" (Statutes, section 1). Although the exact intent of Alfred Nobel may be subject to dispute, both Nobel Foundation President Stig Ramel and sociologist of science Harriet Zuckerman agree that Nobel

Werner Hildenbrand on p. 29 of his admiring introduction to Debreu's papers compares the GERP "to the great gothic cathedrals" and names Debreu as "the great master builder." Nicholas Kaldor (p. 1287) identifies Debreu as perhaps "the most prominent exponent" of those who have developed the notion of general economic equilibrium "with ever-increasing elegance, exactness, and logical precision." Nobel Prize winner George Stigler calls Debreu "the high priest of economic theory" (quoted in Barnhart & Hodge, 5). Hal Varian (p. 4) claims that "for at least two decades the name Debreu has been virtually synonymous with mathematical economics."

Examining the citations of Debreu is not the only test that can be imagined of the empirical progressiveness of the GERP. One might, for instance, examine the citations of a set of important GERP papers by several of the major writers in the program. Such a test would be just as sound as the one performed here, but would be much more costly to implement due to the increase in the pages of the *Social science citation index* (*S.S.C.I.*) that would have to be checked for citations. In defense of our test we would argue that if the GERP is empirically progressive, we would expect to see evidence of that progressiveness in the citations of the most prominent advocate of the GERP.

Reasons can be suggested why citation analysis might underestimate Debreu's impact. For instance Stigler has suggested that "successful scholarly work becomes a part of the corpus of the science, and its paternity is soon ignored" (1982b, 190). Elsewhere he mentions Marshall as a specific example, since Marshall is less cited today because his contributions have become part of the standard analytic tool box of economists (ibid.200).

"wanted to benefit mankind in a concrete rather than abstract, way" (Zuckerman, 18). However, according to Elisabeth Crawford (p. 164) who has studied the early years of the Nobel Prize in great detail, the "benefit mankind" provision was usually not the main criterion of the prize givers: "For the most part, the provision did not operate as a standard against which the candidates were measured but, rather more remotely, as a reminder to the committees not to stray too far from the course Nobel had laid down for the awards. It was only when work in technology was being considered that the utility of inventions or improvements became a criterion for choice. In most other cases, "benefit mankind" was taken to mean that it was preferable if the work rewarded had some utilitarian value."

According to press accounts the 1983 Prize Committee, although emphasizing the abstract form of Debreu's work, nonetheless asserted its relevance to applied problems by claiming that Debreu has had a major impact on the work of applied Nobel Prize-winning economists George Stigler and James Tobin (see "American . . .", 9; Anderson et al., 59: Barnhart & Hodge, 5). Stigler responded in an interview: "If I'm dependent on him, I'm glad to know it. I have no doubt that if I read Debreu, which I haven't, it might have been helpful" (quoted in Barnhart & Hodge, 5) Assar Lindbeck (p. 1) chairman of the committee, however, thinks that the Stigler-Tobin reference "is something that journalists have invented." In a recent article written for the internal consumption of the economics profession Lindbeck (p. 55) argues that the Nobel Committee has not used empirical applicability as a criterion for receipt of the prize, adopting instead an "eclectic approach."

Only twelve years, however, separate Debreu's earliest important contributions in 1954 and the beginning of the *S.S.C.I.* in 1966. Only seven years separate his most important early contribution, *Theory of value*, from the *S.S.C.I.* For work of such mathematical difficulty to have become so thoroughly integrated into the economist's consciousness in such a relatively short period of time is dubious on its face.

Those who disagree, should consider an additional difficulty for the integration hypothesis. We would expect, on that hypothesis, that theorists would be the first to understand and integrate Debreu's contribution with their own. As a result, we would expect, further, that theorists would be the first to stop citing Debreu. That theorists, as we shall see, still frequently cite Debreu, is further reason to doubt that Debreu's contributions have been thoroughly integrated into the economist's consciousness.

In any event, reasons can also be suggested why citation analysis might overestimate Debreu's impact. For example, an economist will sometimes cite a paper in order to add an ambience of class and erudition rather than because the cited paper had any impact on the economist's work. Our method may also overestimate the impact of Debreu's work in another way. Many papers in economics today are divided into model sections and empirical sections. Frequently there is very little relation between the two. Debreu might thus appropriately be cited in the model section of the paper without having influenced anything that is done in the empirical section. Yet by our method such a paper would be taken as evidence that Debreu had an impact on empirical work.

III. *Evidence*

A rough measure of an economist's importance to the economics literature is his total citation count as measured by the number of citations of his work that are reported in the *S.S.C.I.*[8] A 1976 article, for instance, used citation analysis to predict that Debreu, although not a leading candidate, was among those likely to receive the Nobel Prize (Quandt 752–54). Debreu's total citation count for the years 1966–1980 is 1,407, which

8. The citations used in this article were obtained by looking under Debreu's name in the citation volumes of the *S.S.C.I.* for all those articles and books that have quoted any publication for which Debreu was either the sole author or the first author. Several alleged defects of this sort of citation count are briefly mentioned in Diamond 1988. A more detailed discussion is provided there of the absence in the counts of citations to non-first-author publications. A tentative conclusion is that for some purposes, the absence of citations to non-first-author publications may not matter much. The most important paper of Debreu's that is neglected by our method is the 1954 paper with Arrow. In order to see if the neglect of this paper had biased our results, we examined a random sample of 10 of the 92 publications that are listed in the *S.S.C.I.* as having cited the paper from 1966 through 1980. Using the same criteria described in the paper we found that 8 of the articles were theoretical, 1 was empirical, and 1 was a review article. Thus, the omission of the Arrow-Debreu article appears not to have biased our findings.

places him fourteenth among the twenty-four economists to receive the prize through 1986 (see Diamond 1988).

A random sample of articles that cite Debreu was obtained by selecting every fifteenth source listed in the *S.S.C.I.* as having cited Debreu during the period 1966–1980.[9] The sampling procedure resulted in 110 distinct entries (112 entries if you double count the two articles that appeared on the list twice). Of the 110 original entries, 5 were eliminated for being books, 4 others were eliminated for being either review articles[10] or book reviews ad 9 were eliminated because the article was not available in the Ohio State University library system.[11] We then obtained photocopies of the remaining 92 articles.

The *S.S.C.I.* includes citations from all of the social sciences, not just economics. Debreu, however, has been cited mainly by economists: in our random sample, 76 of the 92 articles that cited Debreu clearly fell within the standard domain of economics journals. Several of the remaining articles had substantial economic content.

We classified articles from the sample into two categories: empirical and theoretical. Empirical articles were defined as those making more than casual use of real-world data (as opposed, say, to simulations). Any paper in which a regression was estimated, for example, automatically would be counted as empirical even though the empirical analysis constituted a minor part of the paper. Any articles that were not judged empirical were considered to be theoretical. Of the 92 articles, 87 were theoretical and 5 were empirical. Full references to the 92 articles, and the classification of each, can be obtained from the author upon request.

Since only five of the articles in the random sample turned out to be empirical, we sought to obtain a larger collection of empirical articles in order to learn how Debreu's work influences the empirical articles that cite him. The *Journal of Political Economy* (*J.P.E.*) has a reputation for emphasizing empirical work, so we expected that by examining all the *J.P.E.* articles that had cited Debreu from 1966 to 1980 we would substantially expand our set of empirical articles. We found, to the contrary, that the

9. All citations to Debreu's work were first classified according to the journal of the citing article in order to obtain a broad overview of the sorts of articles that were citing Debreu. An Appendix available upon request from the author reports the journals in which Debreu was heavily cited during 1966–70, 1971–1975 and 1976–1980 and the ten journals that contain the most citations to Debreu over the whole period

10. Review articles were those whose primary aim was to summarize existing literature rather than to make a substantial contribution.

11. The articles for which we were unable to obtain copies appeared in the following journals: *Operations Research, New Zealand Economic Papers, Economie Appliquée* (two articles), *Rivista Internazionale di Scienze Economiche ę Commerciale* (two articles), *Review of Radical Political Economics, Journal of Agricultural Economics*, and *Mathematical Social Sciences*.

sixteen articles obtained from the *J.P.E.* were all theoretical by the criteria discussed above.

In order to further test the impact of Debreu on empirical work, the five empirical articles in the random sample were examined in detail in order to learn whether the references to Debreu were (a) cosmetic, (b) relevant to the model section of the paper where the model is loosely related to the empirical work, or (c) relevant to the model section of the paper where the model is closely related to the empirical section. Here we summarize in chronological order the contents of each of the five articles, making particular note of the context in which Debreu is referenced.

The first article is a 1970 paper on macroeconomics by Morris Copeland. The paper develops a fifteen-equation "Walras-Hicks type model" of unemployment and is classed as empirical (although marginally so) because brief mention is made (p. 57) of calculating some of the variables in the model using data from the July 1967 *Survey of Current Business* and the April 1967 *Federal Reserve Bulletin*. Arrow and Debreu are referenced when the author assumes "for the sake of argument" that the fifteen equations are consistent and unique and determine real, non-negative values for the variables. In the footnote reference to Arrow and Debreu, the author observes that they "establish the conditions for the existence of a meaningful solution for a general equilibrium model" (p. 58). This seems to be the extent of Copeland's use of Debreu's work.

In a highly mathematical 1971 paper, T. Bergstrom studies the existence and optimality of competitive equilibrium in a slave economy. The paper is classed as marginally empirical because it includes a table on rates of manumission and because rates of return for infant slaves are calculated based on prices estimated by Meyer and Conrad. The table is used to test between three verbal hypotheses, independent of the main formal apparatus, concerning why rates of manumission were low. The rate-of-return data are used to test the startling implication of the apparatus that slavery would not have existed if the present value of an infant slave were negative.

Another 1971 paper, this one by Aurelio Mattei, formulates "a dynamic version of the theory of the consumer, based on the introduction of stocks into the utility function" (p. 251). In the empirical section of the paper he estimates a system of fourteen demand equations using time series data. His only reference to Debreu is in the theory section of the paper where he notes that instead of assuming the existence of a utility function, he could have derived it from other assumptions.

In a 1975 article John Whalley compares the reliability of partial equilibrium models with that of a general equilibrium model. The paper is classed as marginally empirical because in order to perform simulations of the various models it makes use of production elasticities calculated by Nerlove. The only reference to Debreu (p. 299) is to a 1951 paper in which

Debreu suggested a measure of welfare gains in commodity space. Whalley concludes that for his purposes the measure is "ill-defined."

In her 1979 article, Christina H. Gladwin argues that economic maximization models cannot explain the adoption of new farming practices in Puebla, Mexico. The paper is empirical because she estimates a production function for maize using data from Puebla. Gladwin refers to a 1954 Debreu paper as part of a growing literature that advocates the use of lexicographic ordering models. She constructs a lexicographic model of the adoption of new farming techniques and claims (without sharp test) that it is superior to a production function model.

A few generalizations can be made on the basis of the "empirical" articles just summarized. The papers tend to be only marginally empirical and the references to Debreu tend either to be a general reference to a minor paper (Gladwin), a rejection of an empirical measure (Whalley) or else in the context of a restrained apology that the model is not more mathematically rigorous and complex (Mattei and Copeland). Only in Bergstrom's paper were the references to Debreu important to the substance of the paper. But even there, the references were in a mathematical theory section that was only tenuously connected to the brief, marginally empirical pages at the end.

The evidence presented so far indicates that Debreu has had a negligible effect on empirical work. A sceptic might, however, object at this point that all we have shown is that Debreu has had little direct effect on empirical work. The real impact of Debreu, the sceptic might argue, would be indirect through less abstract, though still theoretical, intermediaries. We tested the sceptic's argument by randomly selecting ten of the eighty theoretical articles in the original random sample. We then obtained a list of all the citations made to these ten articles from the date of the article through 1984. Full references to the ten articles along with the number of times each article is cited may be obtained upon request from the author. Using the same sample inclusion criteria as previously sketched for the random sample, we finally obtained photocopies of 79 articles that cite the ten articles that cite Debreu. Of the 79 articles only five were empirical, indicating the indirect effect cannot be large.

IV. *Conclusions*

Citation evidence indicates that the writings of Gerard Debreu have had a negligible impact on empirical work. The evidence is thus consistent with the claim that the GERP does not have a strong record of empirical applicability.[12] As we noted earlier, since we looked for applicability rather

12. Although Debreu would not agree with our conclusions that retrospectively the GERP has not been empirically progressive, he is worried about the *prospective* empirical progressiveness of the program: "This lecture has credited the mathematical form of theo-

than just *novel* applicability, evidence of the absence of empirical applicability is, a fortiori, evidence of the absence of retrospective empirical progressiveness.

Some, however, remain hopeful that in the future the GERP will be more empirically progressive than in the past. The most promising work in this direction may be that done by Scarf, Shoven, Whalley, and others on applied general equilibrium models. We have already briefly mentioned some of the problems with such models, most notably the difficulty in establishing their empirical progressiveness. If these problems can be overcome, then in the future it may be possible to make a strong case that the GERP is empirically progressive in the absolute sense. A judgment on whether the GERP is also empirically progressive in the more important relative sense would then have to await a systematic evaluation of the empirical progressiveness of alternative research programs. Such an evaluation would include not only the partial equilibrium research program but also research in the general equilibrium tradition that does not depend upon the results generated by the GERP.

In addition to this issue, further research can address several other questions. One would be whether the GERP can be favorably appraised by some criterion besides the empirical progressiveness. Weintraub, for instance, has suggested that the GERP should in part be appraised on the basis of the "proofs-and-refutations model of the growth of mathematical knowledge."[13] Other criteria for appraising the GERP have been expressed in terms such as simplicity, rigor, elegance, generality, and clarity. An important issue is whether the presence of these characteristics is sufficient for a favorable overall appraisal or whether the characteristics count as second-order criteria for comparing research that has already been favorably appraised in terms of empirical progressiveness.

Those whose overall appraisal of the GERP is unfavorable must then confront what to them must seem the puzzle of why work in the GERP is so highly esteemed within the profession. One possible answer is Grubel & Boland's view that the demand for work in the GERP is attributable to rent-seeking behavior on the part of mathematical economists. Alternatively, economists may receive psychic benefits from the contemplation of

---

retic models with many assets. Their sum is so large as to turn occasionally into a liability, as the seductiveness of that form becomes almost irresistible. In its pursuit, research may be tempted to forget economic content and to shun economic problems that are not readily amenable to mathematization. I do not intend, however, to draw a balance sheet, to the debit side of which I would not do justice. Economic theory is fated for a long mathematical future, and at other World Congresses of our Society Frisch Lecturers will have the opportunity, and possibly the inclination, to choose as a theme "Mathematical Form vs. Economic Content" (Debreu 1986, 1268–69).

13. Weintraub 1985b, 173. For more on the model, consult Lakatos' tour de force *Proofs and refutations*.

elegant mathematical structures and rigorous proofs (see Hausman, 29). Debreu argues along similar lines, that the GERP's pursuit of simplicity is "strongly motivated by aesthetic appeal" (1986, 1267; see also Debreu 1984). Or perhaps the complex mathematics and the rigorous logic are useful to economists in marketing their discipline as a science (see McCloskey 1986a, 4).

Dae-hyun Baek, Ching-wei Lien, and Kathryn L. Williams spent long hours carefully collecting and analyzing the data. Other useful research assistance was provided by Jack Julian, Steven Oetken, James Thomas, and Ann Wertz. I have received helpful comments from Dae-hyun Baek, Neil de Marchi, Douglas Hands, Luis Locay, Andrea Maneschi, Aurelio Mattei, Hajime Miyazaki, Christian Schmidt, George J. Stigler, and E. Roy Weintraub. An early version of the article was presented as a paper to the 1985 annual conference of the History of Economics Society at George Mason University. A recent version was presented to the 1986 annual meeting of the Society for Social Studies of Science in Pittsburgh.

References

"American Awarded Nobel in Economics." 1983. *Chicago Tribune*, Section 1 (18 Oct.): 1 and 9.

Anderson, Harry, Nadine Joseph, and K. Mortensen 1983. 'Explaining the "invisible Hand."' *Newsweek* (31 Oct.): 59.

Arrow, Kenneth J. 1974. 'Limited knowledge and economic analysis.' *American Economic Review* 64:1–10.

———, and Gerard Debreu 1954. 'Existence of equilibrium for a competitive economy.' *Econometrica* 22:265–90.

Ballard, Charles L., Don Fullerton, John B. Shoven, and John Whalley 1985. *A general equilibrium model for tax policy evaluation*. Chicago.

Barnhart, Bill, and Sally Saville Hodge. 1983. 'No, No Nobel.' *Chicago Tribune*, Section 3 (26 Oct.): 5.

Becker, Gary S. 1971. *Economic theory*. New York.

Bergstrom, T. 1971. 'On the existence of optimality of competitive equilibrium for a slave economy.' *Review of Economic Studies* 38:23–36.

Blaug, Mark 1980. *The method of economics or how economists explain*. Cambridge.

Clower, Robert W. 1975. 'Reflections on the Keynesian perplex.' *Zeitschrift für Nationalökonomie* 35:1–24.

Coddington, Alan 1972. 'Positive economics.' *Canadian Journal of Economics* 5:1–5.

——— 1975. 'The rationale of general equilibrium theory.' *Economic Theory* 13 (Dec.): 539–59.

Cole, Stephen, and Jonathan R. Cole 1967. 'Scientific output and recognition: a study in the operation of the reward system in science.' *American Sociological Review* 32.3 (June): 377–90.

Copeland, Morris A. 1970. 'On unemployment and overemployment, assuming price and wage stability.' *Journal of Economic Issues* 4:40–59.

Crawford, Elisabeth 1984. *The beginning of the Nobel Institution: the science prizes, 1901–1915.*'

Debreu, Gerard 1959. *Theory of value*. New Haven.

——— 1984. 'Economic theory in the mathematical mode.' *American Economic Review* 64 (June): 267–78.

——— 1986. 'Theoretic models: mathematical form and economic content.' *Econometrica* 54 (Nov.): 1259–70.

de Marchi, Neil 1976. 'Anomaly and the development of economics: the case of the Leontief paradox.' In S. J. Latsis, ed., *Method and appraisal in economics* (Cambridge, 1976), 109–27.

Diamond, Arthur M. Jr. 1988. 'Citation counts for Nobel Prize winners in economics.' *History of Economics Society Bulletin* 10 (Spring).

——— 1986. 'What is a citation worth?' *Journal of Human Resources* 21 (Spring): 200–215.

Dorfman, Robert 1983. 'In theory: a Nobel quest for the invisible hand.' *New York Times,* Section 3 (23 Oct.): 56.

Feyerabend, Paul 1975. *Against method*. London.

Friedman, Milton 1953. 'The methodology of positive economics.' In *Essays in positive economics* (Chicago), 3–43.

Garfield, Eugene (chairman) 1966–. *Social science citation index*. Philadelphia.

Gibbard, Allan, and Hal R. Varian 1978. 'Economic models.' *Journal of Philosophy* 75:664–77.

Gladwin, Christina H. 1979. 'Production functions and decision models: complementary models.' *American Ethnologist* 6:653–74.

Grubel, Herbert G., and Lawrence A. Boland 1986. 'On the efficient use of mathematics in economics: some theory, facts and results of an opinion survey.' *Kyklos* 39:419–42.

Hahn, Frank H. 1984. 'The winter of our discontent.' *Economica* 40 (Aug. 1978): 322–30. Reprinted in Frank Hahn, *Equilibrium and macroeconomics* (Cambridge, Mass.), 133–44.)

——— 1986. 'Review of Arjo Klamer's *Conversations with economists*.' *Philosophy and Economics* 2 (October): 275–282.

Handler, E. W. 1980a. 'The logical structure of modern neoclassical static microeconomic equilibrium theory.' *Erkenntnis* 15:33–53.

——— 1980b. 'The role of utility and of statistical concepts in empirical economic theories: the empirical claims of the systems of aggregate market supply and demand functions approach.' *Erkenntnis* 15:129–57.

Hands, Douglas W. 1984. 'The role of crucial counterexamples in the growth of economic knowledge: two case studies in the recent history of economic thought.' *History of Political Economy* 16 (Spring): 59–67.

——— 1985a. 'Second thoughts on Lakatos.' *History of Political Economy* 17 (Spring): 1–16.

——— 1985b. 'The structuralist view of economic theories: a review essay.' *Economics and Philosophy* 1 (Oct.): 303–35.

Hausman, Daniel M. 1984. 'Are general equilibrium theories explanatory?' In Joseph C. Pitt, ed., *Philosophy in economics,* vol. 16 in Robert E. Butts, ed., The University of Western Ontario Series in Philosophy of Science (Dordrecht, 1981), 17–32. The essay is also reprinted in Daniel M. Hausman, ed., *The philosophy of economics* (Cambridge), 344–59.

Hildenbrand, Warner 1983. Introduction in Gerard Debreu, *Mathematical economics: twenty papers of Gerard Debreu* (London), 1–29.

Hutchison, T. W. 1976. 'On the history and philosophy of science and economics.' In S. J. Latsis, ed., *Method and appraisal in economics* (Cambridge), 181–205.

Kaldor, N. 1972. 'The irrelevance of equilibrium economics.' *Economic Journal* 82:1237–55.

Klamer, Arjo 1985. 'Review of appraisal and criticism in economics.' *Economics and Philosophy* 1 (Oct.): 342–49.

Kuhn, Thomas 1962. *The structure of scientific revolutions*. Chicago.

Lakatos, Imre 1970. 'Falsification and the methodology of scientific research programmes.' In Imre Lakatos and Alan Musgrave, eds., *Criticism and the growth of knowledge* (Cambridge), 91–96.

——— 1976. *Proofs and refutations: the logic and mathematical discovery.* Cambridge.

Latsis, S. J. 1976a. 'A research programme in economics.' In S. J. Latsis, ed., *Method and appraisal in economics* (Cambridge), 1–41.

——— 1976b. *Method and appraisal in economics*. Cambridge.

Lindbeck, Assar 1984. Personal communication dated 10/22/84.

——— 1985. 'The Prize in Economic Science in Memory of Alfred Nobel.' *Journal of Economic Literature* 23.1 (March): 37–56.

McCloskey, Donald N. 1986a. *The rhetoric of economics*. Vol. 1 in the Series on the Rhetoric of the Human Sciences. Madison, Wis.

——— 1986b. 'Thick and thin methodologies in the history of economic thought.' Xerox draft, University of Iowa, April. To appear in *The Popperian legacy in economics, and beyond* Cambridge, forthcoming.

Mansur, Ahsan, and John Whalley 1984. 'Numerical specification of applied general equilibrium models: estimation, calibration, and data.' In Herbert E. Scarf and John B. Shoven, eds., *Applied general equilibrium analysis* (London), 69–127.

Mattei, Aurelio 1971. 'A complete system of dynamic demand functions.' *European Economic Review* 2:251–76.

Odelberg, W., ed. 1972. *Nobel: the Man and his prizes*, 3d ed. New York.

Quandt, Richard E. 1976. 'Some quantitative aspects of the economics journal literature.' *Journal of Political Economy* 84:741–55.

Rosenberg, Alexander 1980. 'A sceptical history of microeconomic theory.' *Theory and Decision* 12:79–93.

——— 1985. 'If economics isn't science, what is it?' *Philosophical Forum* 14: 296–314.

Scarf, Herbert E., and John B. Shoven, eds. 1984. *Applied general equilibrium analysis*. London.

Shoven, John B., and John Whalley 1984. 'Applied general-equilibrium models of taxation and international trade: an introduction and survey.' *Journal of Economic Literature* 22 (Sept.): 1007–51.

Stigler, George J. 1982a. 'The citation practices of doctorates in economics.' In *The economics as preacher and other essays* (Chicago), 192–222.

——— 1982b. 'The pattern of citation practices in economics.' In *The economist as preacher and other essays* (Chicago), 173–91.

Toulmin, Stephen 1972. *Human understanding*. Princeton, N.J.: 1972.

——— 1975. 'Commentary.' In Robert S. Westman, ed. *The Copernican achievement* (Berkeley).

——— 1976. 'History, praxis and the "Third World."' In R. S. Cohen et al., eds. *Essays in memory of Imre Lakatos*. Dordrecht.

Varian, Hal R. 1984. 'Gerard Debreu's contributions to economics.' *Scandanavian Journal of Economics* 86:4–14.

Weintraub, E. Roy 1979. *Microfoundations*. Cambridge.

——— 1985a. 'Appraising general equilibrium analysis." *Economics and Philosophy* 1 (April): 23–37.
——— 1985b. *General equilibrium analysis: studies in appraisal*. Cambridge.
——— 1985c. 'The neo-Walrasian program is empirically progressive.' Preliminary draft presented in December of a paper to appear in Neil de Marchi, ed., *The Popperian legacy in economics, and beyond* (Cambridge, forthcoming).
——— 1986. 'On the brittleness of the orange equilibrium.' Xerox draft, 30 Jan.
Whalley, John 1975. 'How reliable is partial equilibrium analysis?' *Review of Economics and Statistics* 57:199–310.
Zahar, E. 1973. 'Why did Einstein's programme supersede Lorentz's? (I).' *British Journal for the Philosophy of Science* 24:95–123.
Zuckerman, Harriet 1977. *Scientific elite: Nobel laureates in the United States*. New York.