Appraising Weintraub's *Appraisal*

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This book is motivated by the the need to defend general equilibrium analyses from the attacks of critics. What is interesting is the resort to defending this kind of work not by reference to its degree of empirical correspondence but by the extent to which it conforms to some theory of science. The main purpose of Weintraub's book is to provide such a defence of general equilibrium analysis. The first section of this article is devoted to summarising Weintraub's aims and arguments. Remaining sections are used to examine the extent to which Weintraub answers the questions frequently directed at work on general equilibrium analysis.

Weintraub's Appraisal of General Equilibrium Analysis

In *Studies in Appraisal,* Weintraub sets out to define what he calls a neo-Walrasian research program, relying heavily on the Lakatosian concept of a scientific research program (SRP). In doing so, Weintraub hopes to determine "whether and in what sense, neo-Walrasian economics is good economics" (p. 55). Lakatos (1970) argued that isolated individual theories are not the appropriate units of appraisal. What should in fact be the subject of appraisal are clusters of interconnected theories defined by Lakatos as 'scientific research programs'. All scientific research programs can be characterised firstly by their 'hard core' containing a set of axioms or irrefutable "overriding assumptions that by the definition of the program command assent" (p. 109). This hard core is in turn surrounded by a protective belt of auxiliary hypotheses or theories which bear the brunt of empirical tests. Along with these hypotheses, the protective belt contains a 'positive heuristic' or investigative approach consisting of a "partially articulated set of suggestions or hints on how to...modify, sophisticate the 'refutable' protective belt" (Lakatos 1970). A negative heuristic may also be included providing guidelines as to what should *not* be done.

Weintraub tries to portray economics as being dominated by the "neo-Walrasian program" which is organised around the hard core propositions listed by Weintraub as below:

HC1. There exists economic agents.

HC2. Agents have preferences over outcomes.

A Review Article of E.R Weintraub's, **General Equilibrium Analysis: Studies in Appraisal** *(1985) Cambridge, Cambridge University Press.*

HC3. Agents independently optimise 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 'theory generators' or the positive and negative heuristics of the neo-Walrasian program consist of propositions such as:

PH1. Go forth and construct theories in which economic agents optimise.

PH2. Construct theories that make predictions about changes in equilibrium states.

NH1. Do not construct theories in which irrational behaviour plays any role.

NH2. Do not construct theories in which equilibrium has no meaning.

NH3. Do not test the hard core propositions.

The positive and negative heuristics of the program generate theories in the protective belt of the hard core and it is these "theories in the protective belt of the program, theories developed out of the hard core by the heuristics, (which) are appraised by the method that is appropriate for any empirical science" (p. 120). The centrepiece of Weintraub's argument however is the idea that the hard core is not to be so evaluated.

The hard core, as outlined above in a numbered form, Weintraub believes can only be said to have existed from the 1950's. Weintraub provides a history of the development of general equilibrium analysis from the 1930's through to the early 1950's which shows the hardening of the hard core of the neo-Walrasian research program. He argues that general equilibrium analysis is a process of refinement of the proof of existence of a general equilibrium. This analysis is very significant as it suggests that general equilibrium analysis, being a component of the hard core, is insulated from attacks addressing its lack of empirical correspondence: "The hard core propositions are questioned only by 'outsiders'" (p. 109). Despite its lack of empirical content, the hard core of the neo-Walrasian program, in other words general equilibrium analysis, is still to be appraised but only in terms of theoretically progressiveness:

> progress for this sequence, is a sequence of interpretations of the undefined terms of the hard core such that
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> (1) each successive interpretation is manifested in a consistent model,
>
> (2) each successive interpretation contains the interpretation of the predecessor and
>
> (3) each allows a concept uninterpreted by that predecessor to now be interpreted ... we use criteria appropriate for gauging mathematical progress to measure the growth of knowledge associated with the hardening of the core of the neo-Walrasian program" (pp.117, 119).

In summary then, Weintraub suggests a 'dual approach' to appraising the neo-Walrasian program. Firstly, the theories in the protective belt of the program should be appraised on the basis of their empirical progressivity using traditional falsificationist appraisal techniques. Importantly however, general equilibrium analysis, which forms the hardening of the hard core of the neo-Walrasian program, should not be subjected to the same approach but rather appraised, in a similar fashion to appraisal of other mathematical theorems, in terms of its theoretical progressivity. General equilibrium work passes such an appraisal with flying colours according to Weintraub.

An Appraisal of Studies in *Appraisal*

Studies in Appraisal is an entertaining and at times persuasive piece of work centered on both an important and topical issue. The book is well structured and easy to read and Weintraub appears to make a conscious effort to make the ideas presented accessible to a wide range of reader from undergraduates to academic economists. Despite the entertaining nature of the book and the potential importance of the ideas presented, Weintraub leaves many questions unanswered and at times, while appearing to be on the right track, does not carry arguments through to the extent that would be needed to make a fully persuasive case.

The first and most obvious shortcoming of Weintraub's book is that it does little to defend neoclassical economics against many of the main arguments that have historically been directed against it, other than to hide it behind a methodological shield. Weintraub appears to have the belief that the be all and end all of determining the worth of general equilibrium analysis is to appraise it in light of the SRP approach first formulated by Lakatos.

With credit, when deciding what would constitute an appropriate method of appraisal, Weintraub does not simply "opt for an easy answer provided by his favourite philosopher of science". Instead Lakatos' methodology is extended in an attempt to allow a fuller and 'improved' rational reconstruction of the history. In spite of this extension, Weintraub seems content to maintain Lakatos' claim that "the hard core contains propositions taken as given...the core is never subjected to testing" In using this approach, however, Weintraub seems to brush aside the earlier criticisms of equilibrium economics with a cursory call of 'look to Lakatos'. Any criticism of general equilibrium analysis by economists however renowned (e.g. Robinson, Kaldor, Kornai) is dismissed as being "intellectually shoddy" (p.45) or due to a "flawed understanding" (Weintraub 1985c, p.146). This response by Weintraub seems less than convincing when one considers the nature of the serious indictments put forward by these and other economists.

Robinson, for example, faulted the models abstraction from historical concerns. Kaldor (1978) criticises general equilibrium assumptions as being patently unrealistic at best, and explicitly falsified at worst. The pure general equilibrium analysis is "shown to be valid only on assumptions that are manifestly unreal, that is to say, directly contrary to evidence and not just 'abstract'" (Kaldor, 1978). Kaldor argues, for example, that the assumption of constant returns to scale is simply not justified for any analysis of real world occurrences due to the overwhelming evidence suggesting the dominance of increasing returns to scale. Once the presence of increasing returns is acknowledged, however, Kaldor (1978) suggests that "change becomes progressive and propagates itself in a cumulative way" and "When every change in the use of resources creates the opportunity for a further change *which would not have existed otherwise*, the notion of an 'optimum' allocation of resources...becomes a meaningless and contradictory notion" (Kaldor, 1978).

Many other economists find cause to fault general equilibrium analysis, Hahn

(1981), while a supporter of the neoclassical framework, lists several of these arguments as follows: the lack of an explicit role for money and credit; inadequate treatment of intertemporal considerations; exclusion from the model of oligopoly or imperfect competition; unrealistic assumptions of the presence of a market for all goods and information symmetry between all agents. Despite the serious nature of the above, Weintraub is satisfied with the defense of the assumptions of the general equilibrium analysis as "irrefutable propositions" of the hard core of the neo-Walrasian research program. On this basis, criticisms of general equilibrium analysis on the grounds of the unrealism of it's assumptions are dismissed by Weintraub as misguided or owing to a misunderstanding of the role played by general equilibrium analysis in mainstream economics.

Weintraub regards general equilibrium as a logical starting point - the necessary conceptual framework - for any attempt at explaining how a decentralised economic system works. General equilibrium analysis should be regarded as the foundation for a long process of discovery, not as the end or final destination of past efforts of discovery. In his mind, many of the criticisms of general equilibrium analysis arise due to a misunderstanding of just what the role of general equilibrium work is. A major fault of those methodologists who spend time criticising general equilibrium analysis, is that they have to date, been unable to provide a description of general equilibrium analysis that commands assent among all or even most methodologists (p. 48). Blaug (1980) for example, is criticised for attacking the unfalsifiability of general equilibrium 'theory' as it's main problem. It is Weintraub's suggestion however (p. 47), that it is *theories* that provide falsifiable predictions, not frameworks for analysis. General equilibrium analysis, according to Weintraub's methodology, of course falls into the latter of these two groups.

Blaug's criticisms of general equilibrium analysis lend support to Weintraub's view that the basis of many of the methodological attacks on general equilibrium analysis is a confusion in the methodologists mind as to exactly what he is appraising. Blaug (1980) seems to accept that general equilibrium analysis may be a framework or program of analysis rather than a theory. Nonetheless, he goes on to appraise the analysis on the basis of it's production - or more correctly, lack of production - of falsifiable predictions. Blaug suggests that the general equilibrium analysis, if a framework "lack(s) any bridge by which to cross over from the world of theory to the world of facts" (Blaug 1980, p. 119). This suggestion unfairly biases Blaug's appraisal according to Weintraub, who believes that the "logic or set of strategies or rules for constructing theories are not the same thing as the theory itself" (p. 46). Consequently, for Blaug to look for such a 'bridge' indicates that he is appraising general equilibrium as if it should have some relation to facts. That is, in Weintraub's view, as a theory.

Weintraub argues that development of the general equilibrium framework is a sequence of mathematical theories building off each other, and intended to provide the basic hard core assumptions with consistency. As such, appraisal of this work, according to Weintraub, should follow the criteria laid down in Lakatos' examination of method in mathematics: *Proofs and Refutations* (Lakatos 1963). Weintraub suggests that "progress for this sequence, is a sequence of interpretations of the undefined terms of the hard core...(which) represent a natural progression in the development of any scientific research program" (p 117-118). Others, such as

Samuels (1979), however, argue that this does not answer the questions relating to realism. In a review of Weintraub's (1979) *Microfoundations*, Samuels stresses his belief in the value of mathematical models, but argues that the verification of models by other theoretical constructs is no verification at all. In Samuels words, "I am not saying that models cannot be tested for coherence and consistency; rather, verification by congruence with other theoretical constructs is not verification with respect to economic reality" (Samuels 1979, p 1024).

It would seem, upon reflection, that while Weintraub has effectively deflected some of the major criticisms of economic methodologists such as Blaug's criticisms of its lack of empirically testable hypotheses, he leaves unanswered some of the criticisms suggested by economists such as Robinson, Kaldor and Kornai. Devising an elegant methodological structure into which general equilibrium analysis can be nicely fitted is all very well, but as others before have asked, can general equilibrium analysis increase the economists understanding of a decentralised economic system and consequently improve on policy making decisions? It seems that Weintraub's argument that general equilibrium work is simply a starting point that focuses research in economics, is nothing new. Kaldor (1978) acknowledged that it is the deep underlying belief of all economists of the 'neoclassical' school that "general equilibrium theory is the one and only starting point for any logically consistent explanation of decentralised economic systems" (p. 178). Kaldor believed however, that this 'starting point' did not appear to be leading anywhere. The conversion of what Kornai referred to as an "intellectual experiment" (Kornai 1971, p11) into a set of theorems directly related to observable phenomena, did not, and many would argue, does still not, appear to be happening.

There are another set of issues not addressed by Weintraub. Robinson (1979) argued that the neo-Walrasian program biased the kinds of questions that economists can ask, and answer. In particular, the neo-Walrasian approach "blurs the distinction between income from work and income from property and leaves no room for the classical problem of the distribution of the produce of the earth between the classes of the economy" (Robinson 1979, p. 51). Samuels (1979) says of the neo-Walrasian program "I am principally concerned that it neglects power" referring to the power over price and the transfer of power from one market to another. Samuels goes on to criticise the competitive assumption in general equilibrium analysis for fulfilling a "self-assumed ideological burden of the profession". Many of the critics of general equilibrium analysis "wish to formulate the theory in such a way that class conflict and power become central and explanatory variables. It is perfectly true that general equilibrium theory is not suitable for this project" (Hahn 1979, p. 128).

Other observers are less than happy with the tendency of economists, whenever they feel it necessary to justify their theories, to claim that "they only provide us with some sort of logic of economic phenomena or that they are merely bags of tools into which we dip when convenient" (Hausman 1984, p. 352). This claim does not resolve the problem of justification of theories. If an economic theory is simply a logic or guide to which concepts are central, or as Weintraub would claim, an "organising feature" to focus research (Weintraub 1985b, p. 148), then the question still needs to be asked, is it a good logic or a good guide? Weintraub spends little time on this, focusing only on the question of how it can be used as a guide. If the role of economics is to explain reality, then many (e.g. Hausman 1984) would argue that it must be necessary for them to be able to claim that their models apply to reality. Without this, their models will have little predictive and no explanatory worth. Weintraub in turn argues that such corroborative demands are more correctly placed on the theories constructed in the 'protective belt' of the research program and that any criticism of general equilibrium for its presumed lack of realism "only identifies (those involved) as one who works in a different program" (p. 148).

From another perspective, Rosenberg (1986) criticises economists in general for lagging behind philosophers of science in the assimilation of differing philosophical viewpoints. This leads to the ironic situation in which economists are to be found desperately trying to establish their disciplines scientific credentials while, according to Rosenberg philosophers some time ago realised the futility of hoping to formulate such credentials. "Economists fixation with Lakatos (is) philosophically dated. (They have, as it were attached themselves to a degenerating research program)" (Rosenberg 1986, p. 136). This situation, if correct, immediately casts doubt on Weintraub's *Studies in Appraisal.*

Sassower (1988) displays some irritation towards the tendency of economists to "opt out of studying the history of their own field" and instead hope that the ideas of philosophers of science can be applied directly to their concerns. He praises Weintraub, however, for not taking this approach but instead modifying Lakatos' MSRP in what he feels is an appropriate manner and for his detailed account of the history of GE theory. Others (e.g. Toruno 1988) feel that any attempts to defend economic theories by reference to some theory of knowledge, is a sign in itself of the degenerate nature of the theory in question. Leaving aside these questions, it is interesting to instead turn to the question of what are the problems commonly seen in the application of Lakatos' methodology of scientific research programs to an appraisal of general equilibrium economics.

One of the main difficulties seen when applying Lakatos' methodology to any field is the perceived overflexibility and leniency of this approach. The very nature of Lakatos' methodology, while painting a very believable picture, leaves plenty of scope for manipulation to suit ones purposes. For example, Lakatos claims that a previously successful program may enter a degenerative phase and that currently degenerative programs may always stage a comeback. Obviously with such a claim one is left with no methodological guide for the determination of which competing theory is better. For any program accused of being degenerative it can simply be argued that before too long it will be a progressive program once again and so patience and perseverance are called for. Diamond (1988, p.120), makes this distinction claiming that a research program can be empirically progressive in either a prospective or retrospective sense. A prospective claim that a research program will turn out to be empirically progressive sometime in the future is never subject to strict test although the credibility of the claim diminishes the longer it remains unconfirmed.

Additionally, Lakatos suggested that it may be some time before a new research program is able to show the results of its progressivity in the form of empirically corroborated predictions. Rosenberg (1986) and others argue that such claims only serve to reduce the usefulness and applicability of Lakatos' methodology in terms of comparative choice between two competing research programs. If the neo-Walrasian program is as young as Weintraub says it is (dating from the early fifties) it is further insulated from assessment by Lakatos' dictum that "one must treat budding research programs leniently, programs may take years before they get off the ground and become empirically progressive" (Rosenberg 1986, p. 134).

Koopman (1957) suggests that good science always starts off by purposefully developing abstract and simplistic models which will subsequently be replaced by more complex versions incorporating those elements of reality unexplained by the initial models. Hahn (1973, p. 324) supports Weintraub and general equilibrium analysis along these lines: "the student of general equilibrium believes that he has a starting point from which it is possible to advance towards a descriptive theory". The fear that many have however, as put forward by Toruno (1988, p.128) is that the test of the theory is always postponed to some future date, in the meantime, secure in the belief that future work will vindicate their faith in the theorem, some of the best minds in economics devote their time to "endless formalisations of mathematical models" (Blaug 1980). Rosenberg (1986) dates the current research program in economics from 1874, when Walras' *Principles of Pure Economy* was published, implying that perhaps the neo-Walrasian research program has had long enough to mature. He says that: "Thirty years is not a long time in the life of a scientific research program, but a hundred years is not a short time either" (p. 135).

Rosenberg (1986) argues that the Lakatosian paradigm is not sufficient to distinguish science from non-science. Such non-scientific disciplines as literary theory, painting or music are all disciplines which can as easily satisfy the MSRP criteria, according to Rosenberg, as can General Equilibrium. Rosenberg believes that it would be easy for "able philosophers with time on their hands to cook up a research program, replete with hard cores, heuristic and belts, that satisfy Lakatos' dictum" (p. 136) even for disciplines for which it would be nonsensical to describe as sciences. Toruno (1988, p. 131) lends support to this idea by claiming that Weintraub has misapplied Lakatos' MSRP, highlighting the apparent flexibility of this methodological appraisal. In defence, Weintraub (1987) argues that at no stage did he adopt the MSRP framework "in order that, by getting economics to conform to it, he could claim scientificness for economics and thus 'value' for the enterprise of doing economics" (p. 140). Weintraub claims that economists are not so unsophisticated as to think that calling economics a "science" says any thing about what economists do or should do.

Rosenberg (1986) argues that Weintraub's use of Lakatos simply aims to prevent doubters looking at the principals (which Weintraub identifies as the hard core) for empirical failures. Weintraub is also criticised on the grounds of his overemphasis of the role mathematics or the formalisation of economics should play: "Showing that the neo-Walrasian paradigm is a Lakatosian research program is no refutation of the claim that economics is mainly an exercise in applied mathematics" (Rosenberg 1986, p. 136). Boland (1987) considers it remarkable that Weintraub's "criteria of 'good' is not necessarily what economists think as good but rather what professional mathematicians think is 'good economics'" (p. 662). Toruno (1988) adds "Weintraub seems to have forgotten that economists are interested in assessing general equilibrium theory as an economic not a mathematical theory" (p. 137). He also says "the mathematical success of general equilibrium theory is totally irrelevant if its usefulness as an economic theorem is questionable".

Weintraub does in a later paper (Weintraub 1987), add further weight to his claims that the neo-Walrasian program is empirically progressive. However, he suggests that the the Popperian view of falsification is a poor guide to actual empirical work in economics. Heijdra and Lowenberg (1988) support this claim arguing that "methodologists are often guilty of using overly restrictive falsificationist criteria to appraise the scientificity of economics" (p. 275). Weintraub argues that empirical work in economics is more complex than people believe. Empirical research in the neo-Walrasian research program is easier to identify if the role of econometric analysis is understood as different from statistical hypothesis testing. Theorists do not create theory which econometricians then test, rather economic analysis itself is a blend of logical and empirical analysis and the idea of progression in economic analysis has both theoretical and empirical components. Yet he does acknowledge that: "I have nothing to add to the view of the philosophers about the testing of the theories (in the protective belt) except to observe that I sense too little of it in economics; and what little there is is too often innocuous." (p. 141).

Conclusion

It seems that despite the entertaining and interesting ideas presented by Weintraub, he does not explore the issue fully enough to persuade sceptics of the worth of general equilibrium. Weintraub homes in on Lakatos' MSRP as the saviour of general equilibrium analysis without providing any support of his choice of this particular methodological appraisal guide over other possible appraisal guides. In reading the book one can only guess that Weintraub hopes to portray the study of general equilibrium as a worthwhile activity and worth supporting at the margin. For the various reasons outlined above, however, it seems that he has not convinced many commentators of this.

Expecting Weintraub to cover all the points above may at first appear overly harsh. In making his claim to show the true worth of general equilibrium however, Weintraub implicitly accepted this responsibility. In summary, *Studies in Appraisal* does not provide a comprehensive guide as to the validity and worth of general equilibrium analysis, as Weintraub leaves too many questions unanswered. It does however provide very enjoyable reading along with the presentation of a possible way to view the structure of orthodox economics. This structure may in turn allow a more complete appraisal of economics in the future. Weintraub's work does in fact provide many suggestions as to current faults in methodological appraisal techniques and possible means of correcting these faults, and so may itself form part of the foundation for a more complete appraisal of general equilibrium analysis.

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