

Thinking Outside the Heterodox Box:
Post Walrasian Macroeconomics and Heterodoxy

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As an historian economic recent economic thought I find classifications useful for students who need a quick entrée into a debate, or for nonspecialists who wants a quick and dirty summary of arguments. Used for that purpose, classifications— Classical/Neoclassical, Keynesian/Monetarist, heterodox/orthodox--can serve a purpose. However, when discussing issues among economists involved in a debate, those same classifications often serve a negative purpose; they tend to fix positions more firmly than they actually are, and loose dimensions and nuances of debates that don't fit the particular classification system used.

Consistent with that view about the usefulness of classifications, in my history of thought text (Landreth and Colander 2002) I use the heterodox economics classification, even while, in my writing for the profession, I strongly discourage the use of the term heterodox. Within the profession, "heterodox" is simply not a useful term for promoting conversation and understanding. Economists are bright and diverse and differ on multiple dimensions. Anyone involved in a debate with economists knows that. Classification systems, which obscure the multiple dimensions along which differences exist among economists, serve little purpose. Just about every interesting economist that I know is in some way orthodox, and in some ways heterodox. In my view, when used in a discussion within the profession, the term, heterodox, does not enlighten, but instead intensifies conflict, often creating an illusion of differences that are much larger than the actual differences. That illusion makes serious conversation, which is what I believe advances a profession, difficult, if not impossible.

A second problem that I have with the term "heterodoxy" is that it derives its meaning in relation to "orthodoxy" (by which most heterodox economists mean "neoclassical orthodoxy"), which, when I hear it described by heterodox economists, is in large part a straw man that is neither descriptive of what modern economists do or believe. (Colander, Holt and Rosser, forthcoming) The reality is that mainstream economists don't go around describing themselves as orthodox economists; they almost never use the term "neoclassical," and they are continually discussing issues, and using approaches, that do not fit a rigid neoclassical definition. That's why the term "neoclassical" long ago outlived its usefulness as a description of what economists currently do, if it ever did. (Colander 2000) I, quite frankly, have little interest in what people are in opposition to; my interest is in what insights their way of looking at an issue can provide, and if considering their views more carefully might be worth my time.

The Post Walrasian Classifier

I started using the term Post Walrasian with the strengths and weaknesses of classifiers in mind. I used it as a descriptor of a vision in macroeconomics that I believed has characterized many economists over the years, but which was not being presented in

the texts or receiving the attention within the mainstream that I believed it deserved. The Post Walrasian descriptor is neither orthodox nor heterodox. A Post Walrasian is simply an economist who is approaching the issue of multi-market coordination from a perspective that is slightly different from the approach being used by Walrasians, where the term Walrasian is interpreted as an approach that considers multi market coordination issues from a vantage point of rational agents operating in an information-rich environment.

A Post Walrasian approach asks the same questions as does the Walrasian approach, but it considers the questions from a vantage point of reasonable agents operating in information-poor environments.¹ Thus it does not jettison general equilibrium—or at least a multi market approach; it simply starts with a slightly different specification of what the puzzle is. I did not, and do not, see the economists following a Walrasian strategy—which most recently are those following a dynamic stochastic general equilibrium approach—as opposed to Post Walrasian macro. They are simply working on a different puzzle. My argument against that Walrasian work is not that it's illogical; it is that it is not especially relevant to the real issues that I'd like to understand about the macro economy, whereas I think Post Walrasian macro is relevant to those issues.

I decided that the Post Walrasian classifier would be useful because in talking with economists, I came to recognize that many economists shared with me a concern about the Walrasian research program that emphasized strict rationality and rich information environments. After reading in the field and meeting different economists, I came to believe that sufficient alternative work was developing with a common vision, to warrant the use of a Walrasian/Post Walrasian classification.

However, it is important to point out that there is no “we” vs. “them” in the Post Walrasian classifier. A researcher could be doing both Walrasian and Post Walrasian research at the same time. Moreover, the reason Post Walrasian macroeconomics developed is not necessarily that the Post Walrasian researchers thought the earlier Walrasian work wrong-headed or incorrect. It developed because of advancements in analytic and computing techniques that allowed researchers to start considering more complicated analytic techniques. Moreover, for many Post Walrasian researchers, it was because that Walrasian work had been explored, and implications drawn, that the Post Walrasian avenues seemed possible.

Post Walrasian macro emerged as a coherent research program in the early 2000s for three reasons. The first was that the Walrasian research program had structured the macroeconomic problem in a way that attempted to model it from first principles. This was quite different from the approach used by earlier economists who tended to regard such efforts as impossible (which they probably were given the analytic and computing techniques of the time). The second was the advancement in analytic technique such as evolutionary game theory and non-linear dynamics, which made it possible to formally

¹ It was because of the continuity of the questions being asked that I call the new work Post Walrasian. It is not opposed to Walrasian work; it builds on it.

analyze more complicated interrelationships. The third reason was advancements in computing power, which made analysis less dependent on analytic techniques and more dependent on simulation. These developments, combined with developments in statistical analysis, improved our ability to relate models to empirical evidence. Combined, these opened up research avenues that had not been available to earlier economists, and led to the emergence of the Post Walrasian approach to macroeconomics.

As I discussed above, a classification is most appropriately seen as a crutch for students and non-specialists who are not carefully following work on the forefront of macroeconomic theory. The Post Walrasian classifier serves little purpose for those working inside the field of theoretical macro; they know what's going on, and as I argued above, any classification system tends to obscure issues and suggest stronger differences than actually exist for the people being classified. On the research forefront Walrasian and Post Walrasian approaches blend together and are hardly distinguishable. But the subtlety of that blend is far beyond what can be conveyed to nonspecialists, which is why I find the classification system useful. But, like previous classifications, it, too, will likely be superseded as macroeconomics evolves.

Heterodox Criticisms of Purposeful Behavior and Equilibrium

Heterodox critics, such as Dutt, (this volume) have taken my exposition of Post Walrasian macroeconomics to task on a number of points. One criticism they make is that Post Walrasian economics is simply more of the same neoclassical (interpret bad) work, since it accepts that individuals optimize. I find this criticism quite unconvincing, since I don't think that the term "neoclassical" describes the economics done today. But if the critics mean that any type of purposeful behavior is optimizing, and therefore objectionable, then Post Walrasian economics is definitely objectionable since it does assume some type of purposeful behavior. What type of purposeful behavior to assume is a question in the Post Walrasian research agenda.

As I see it now, in the Post Walrasian approach the type of purposeful behavior that is assumed in a model depends on the structure of the model one is thinking of. From an evolutionary game theoretic perspective, such as in the work of Kenneth Binmore (1994) or Peyton Young, (1998) one might assume strict rationality of agents in order to get some sense of the development of norms, mores and institutions that develop which limit behavior in shorter runs. Alternatively one can have a "dial of rationality" such as they have in the work at CeNDEF (<http://www.fee.uva.nl/cendef>) in which one tries various degrees of rationality to check the robustness of the model. Theoretical work is calibrated to reproduce many features of real world data, but is based on heterogeneous agents with differing degrees of rationality, rather than on homogeneous agents. Their choice of assumptions is governed by experimental and econometric work using field data, not on a priori assumptions.

Abstract models, such as Young's and Binmore's are asking a quite different set of questions than are applied policy macro models such as van Ess and I (Colander and van Ess, 1996) discuss. In these models, a much looser sense of rationality is appropriate, and the bounded rationality that is assumed needs to be justified by a combination of

introspection, empirical evidence, and behavioral economics work. My point is, that depending on the purpose of the model, and the behavior one is trying to model, the degree of rationality, and the social limitations on actions one assumes, will necessarily be quite different. But behind all the models is an assumption of purposeful behavior.

For tractability reasons, one may specify an optimizing strategy but the nature of the optimization is continually related to individual's behavior—bringing the model to the evidence. In an information poor environment the decisions agents face are strategic and far too complicated to have unique solutions. In such environments abstract models don't provide descriptions of reality without enormous institutional detail that serve as equilibrium selection mechanisms. Eventually, one may not have to assume any rationality since in agent-based models, such as described by Leigh Tesfatsion (2002), the various strategies compete and the sustainable strategies emerge rather than being assumed.

If the use of such purposeful behavior assumptions is what heterodox critics are faulting me for, I plead very guilty, since I can hardly think that assuming some type of purposeful behavior on the part of agents in a model is objectionable. Since I can't think of how to design a model without some type of broad purposeful behavior, there may be a deep bias hidden in my approach—a bias that someone who does not share that bias will find objectionable. Within economics such people would definitely be heterodox.

I plead equally guilty to assuming some type of equilibrium. But in my view, equilibrium is not a characteristic of the world; it is a characteristic of a model; it is an analytic technique, not a statement about what one believes about reality. The nature of equilibrium one assumes, and even whether one is interested in equilibrium, depends on the model and questions being asked. For example, in evolutionary game theory, equilibrium is not an issue; one is searching for a sustainable strategy. Similarly, in a "multiple model" model (Brock, Durlauf and West (2003) one is searching for basins of attractions in order to understand how slight changes in assumptions might change the nature of the system. Agent based modeling is similarly not directly focused on equilibrium, but on sustainability. If, however, sustainability in this sense is seen by critics as a type of equilibrium that they oppose, then Post Walrasian economics will be objectionable to them.

The Tendency for Efficiency

A second objection of Dutt, and other heterodox critics, to the Post Walrasian approach is that it is ideologically tainted. As I understand it they are arguing that Post Walrasian economics is tainted because it makes the assumption that there is an inherent tendency for a system in which agents behave purposefully to operate better than a system in which agents do not so operate. I do believe that there is such a tendency in purposeful behavior models, so I plead guilty here. What else do we mean by purposeful behavior? However, I do not see that view about purposeful behavior and systems as an ideological bias about policy since I draw no policy conclusion about the need, or lack of need, for government intervention from that belief.

Here's my reasoning about the relationship of purposeful behavior agent models to policy. Purposeful agents try to make things better for themselves; often, (but not always) in doing so, they will make things better for more than just themselves. (Trade will often be Pareto improving in information-poor, as well as information-rich environments.) Where it does not do so, other purposeful agents will work to place restrictions on those actions that hurt others. If there is a consistency in the effects of certain actions over time, social mores, laws, and other social restrictions will develop that limit those actions, producing a better society. Purposeful agents trade on as many dimensions as they can; they develop institutions such as government, firms, and NGOs to allow them to do more complicated trades. In this sense everything that they do, including creating government and regulations to limit individual actions, is an expression of that tendency. All individual actions are assumed, in some sense, efficient on a local level, but can be enormously inefficient on a higher level. But if it is globally inefficient, agents will recognize it and will work to modify it through whatever means are available to them.

In an information-rich model without transactions costs, the connection between individual actions and higher-level efficiency is tight; in an information-poor environment with transactions costs, the connections is very loose, if it exists at all. In an evolutionary sense things that work will be saved and codified by purposeful agents; things that don't will be dumped. So over long periods of time, assuming underlying generating functions don't change, things will likely get better because agents are purposeful. Thus, if you had two systems, one with purposeful agents, and the other with random agents, the purposeful agent system will be a "preferable" system to agents who are allowed to choose among them.

The above argument may be wrong, but it is a working hypothesis that I have, and some day I hope that it will be tested with some agent-based models. I, however, see it as a reasonable working hypothesis, not as a reflection of an ideological bias. All it says is that the system reflects the efforts of the agents in the system on all levels—both in trying to change the system and in operating within the system. It tells me nothing about whether the system is optimal or whether I should support or oppose government intervention. It is simply a framework though which I view systems such as the economy.

Elsewhere (Colander 1991) I have discussed my view as one in which the economists are internal, not external to the system. A reasonable requirement for any model is that it be consistent with the rationality and information set available to economists and to agents in the model. In the Walrasian model, economists were assumed to understand the model, so it was only reasonable to assume that agents did too. In the Post Walrasian approach, agents don't know what the correct model is, so economists must not either. It is in that sense that policy becomes "muddling through" rather than control. (Brock and Colander, forthcoming a, b; Colander, 2004) In muddling through economic theory tells us nothing about whether the government should or should not intervene. That is something that economists study, and the fact that societies support a group of agents called economists suggests that society believes the economic system can be made better. So for me, the existence of economists in our economy precludes the

assumption that agents in the system believe that an economy with no intervention, on even an economy in its current state, is efficient. If it were, economists would have never developed as a profession. Neither does it say that government intervention is theoretically desirable or undesirable. It is simply a framework that develops when one thinks of agents as purposeful.

Macroeconomics as a Science

The Post Walrasian research program is unlike most previous research programs in macro in that it is neutral about policy. Most previous groups of macro researchers—Classical, Keynesian, Monetarist, New Classical, Austrian, Radical, Post Keynesian, Institutionalist— could be associated with some type of policy recommendation. Thus, much of the heterodox macro theory that I read connects theory to policy, and seems to support a specific policy approach based on theory. I have the same problem with that work that I have with any other economist who pulls policy out of an abstract theory. It is just unconvincing to me because the theory inevitably seems to arrive at the policy they support, with left wing heterodox economists coming up with theories that call for government intervention, and right wing heterodox economists coming up with theories that oppose government intervention. The problem is the same one that I have with much of the “scientific empirical support” for positions that I read. It seems that that empirical support follows policy proclivities of the researcher far more than would be the case were the study truly scientific. I’m much more likely to take a look at evidence that goes against what the researcher believes, and policy results of a model that go against what the researcher’s policy proclivities are.

Post Walrasian work cannot be associated with any particular policies. The reason is that, with the development of new analytic and computing techniques, macroeconomics is emerging as a formal science, not as the engineering approach that previously characterized pre New Classical Walrasian macroeconomic modeling, in which all models were seen as guides to policy. In that work macroeconomics could not exist as a science because the subject matter was beyond formal analysis. It is only with the development of new techniques and new much more powerful analytic methods that we can start thinking of macro as a science. However, it is not a traditional science, but belongs more appropriately as “new kind of science” such as that described by Stephen Wolfram (Wolfram 2002).

This view of macroeconomics as a science goes against the grain of many previous macroeconomists whatever their classification. Because macroeconomics was so closely tied to policy, there was always an “us versus them” mentality. The new Post Walrasian approach is eliminating that on the theoretical front. There will, of course, still be a branch of macroeconomics that is directly concerned with policy, but that will be more engineering than science. It will be messy, dirty and involved with value judgments; it will be muddling through, and will be conducted, not on the basis of economic theory, but on the basis of applied work. In Post Walrasian macroeconomics policy and theory are quite separate. In theory the discussion is “what’s going on here? Can I understand it?” Can I provide a satisfactory explanation for it?” It’s about fitting the theory to the empirical evidence. In policy, the discussion is quite separate from

theory, because formal theory does not provide answers for policy. At best it is suggestive.

This policy/theory divide, which classifies policy as muddling through and unrelated to formal theory, has a long history in economics; it was nicely articulated in J.N. Keynes's book on *Scope and Method of Political Economy*. (1891) In that book Keynes separated out a policy branch of economics—which he called the art of economics—that was quite separate from either positive or normative economics. He argued that policy issues involve politics, institutions, and issues that are outside the realm of science. While economists, as private individuals, can take positions on such questions, they should not use their position as economic scientists to suggest that they have some special insight into policy issues.²

Keynes's methodological dictums make good sense, but they have been systematically violated in macroeconomics. And, because mainstream macroeconomists have claimed the legitimacy of science as supporting some position, it is only natural that heterodox economists did also. The return of the science to macroeconomics, which is what I think is happening with Post Walrasian macro, will eliminate the need to distinguish economists as heterodox or orthodox on the basis of their policy views. Theory will be subjected to what Gibson calls the duck test (if it walks and talks like a duck, it is a duck) which is essentially the empirical test by which all science is judged.

What Should Heterodox Economists Do?

I raise the above issues because they may help to explain my lack of comprehension of comments by Dutt that I appear “to recommend that heterodox economists jump on the Post Walrasian bandwagon and join its challenge of orthodox Walrasian theory and its free-market policy implications” and that I want heterodox economists to “jettison their own approaches.” That is certainly not what I am recommending.

First, I do not see Post Walrasian as challenging an orthodox Walrasian theory, because I don't see any “orthodox” Walrasian theory to challenge. All I am saying to Walrasians is: “Hey, with the recent developments in analytics and computational technology, maybe we can broaden the assumptions a bit and approach macro issues in a slightly different way, with less emphasis on strict rationality and rich information environments and more emphasis on a subtler type of rationality and information poor environments.”

Second, I don't see any policy flowing from either Walrasian or Post Walrasian theory. Walrasian economics doesn't support free markets (even if I knew what free markets were) and Post Walrasian economics doesn't support policy intervention. They

² Marshall and Pigou backed off from that distinction, creating an approach that Pigou called realistic theory. Robbins attacked their approach, arguing essentially that realistic theory should be considered part of the art of economics. These issues are discussed in Colander. (2004)

are neutral on both. Policy is much more complicated and nuanced to pull any policy issues out of abstract models.

Third, I am not calling on anyone to jettison anything, but simply to do it better. My view is “whatever works”, where by “works”, I mean “adds insight on how the macro economy operates.” I see a gigantic puzzle out there, and I believe that to put it together will take many diverse types of work and techniques. I think it is best to think of the Post Walrasian approach as a description of the vision underlying the researcher’s work, since the work may be involved with only one small piece of the puzzle. I do not see Post Walrasian macro tied to any particular technique.

What I am arguing in regard to heterodox economics is that it is time to jettison the “us/them” mentality, and to get on with developing an understanding of the economy that fits the empirical evidence, and by fit I mean fitting one’s “aha” sense, a sense that one gets when an argument resonates with one’s vision of the world, which for a scientist is closely tied with the most sophisticated statistical tests available.³

I continue a dialogue with economists once know as heterodox because many of their arguments fit my “aha” sense. That’s true of heterodox economists on both the right and the left. I strongly hope that that work continues and gets a better hearing than it has to date. My call to abandon heterodoxy is not a call to stop the work heterodox economists are doing, but simply a call for heterodox economists to stop seeing their work as something that a good mainstream economist would not find interesting because of the mainstream’s ideological blinders. That isn’t the reason heterodox work isn’t being considered. Heterodox work isn’t considered because heterodox arguments have not struck a chord with mainstream economist’s “aha” sense of the world. The good mainstream challenges the mainstream all the time, and the mainstream will be much stronger if the strong critics of whatever is currently believed converse within the mainstream, rather than develop a separate conversation outside the mainstream.

Conclusion

In my view a good economist is a good economist; he or she is neither heterodox nor orthodox. He or she is always questioning, willing to consider new approaches, and trying to see whether he or she can gain a little more insight into what is going on by using this method, or that method, or structuring the organization of the facts in a slightly different manner. Sometimes the answer is “I don’t see how I can do that.” So one stays with the existing approach that one was taught. Other times the answer is: “Here’s a new way that seems promising? Let’s try it.” Ultimately, the test of the approach is whether it adds a bit more “aha” to ones understanding. When it does, it is an approach with “Oomph,” as Deirdre McCloskey would call it, and ultimately it is Oomph that an economic researcher is looking for.

³ I discuss the “aha” sense, which I call the “yeah criterion” in Colander (2000). Bob Gibson’s discussion of this issue (Gibson, this volume) said many of these issues better than I could have, and thus I will not develop them here; I thank him for his careful reading of my work.

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