

**“How Did Macro Theory Get So Far off Track,
and what Can Heterodox Macroeconomists Do to Get it Back On Track?”**

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August, 2009

MIDDLEBURY COLLEGE ECONOMICS DISCUSSION PAPER NO. 09-11



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**How Did Macro Theory Get So Far off Track, and what Can Heterodox
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**Invited Paper prepared for the Research Network Macroeconomics and
Macroeconomic Policies' 12th conference on**

Macroeconomic Policies on Shaky Foundations - Whither Mainstream Economics?

Berlin (Germany),

31 October - 1 November 2008.

Abstract: This paper argues that the ideas that win out in economics are not necessarily those that a *representative researcher* would choose, but are rather the emergent result of the competition of ideas in which system replicator dynamics dominate. This means that those ideas that fit the analytic technology available to researchers at the time dominate, while “better” ideas that do not offer advancement to researchers lose out. This paper spells out that view. It differentiates a consumer’s understanding of theory from a producer’s understanding of theory, and argues that a consumer’s understanding of theory is often better suited to applied policy than is a producer’s understanding of theory. Because the replicator dynamics of the economics profession does not reward people for acquiring a consumer’s understanding of theory, that understanding is often neglected. Heterodox economists often have a better consumer’s understanding of theory than do mainstream economists but because they do not prepare students to be successful in economic institutional environment, their views do not receive the hearing they should in the profession. The paper offers a number of suggestions for heterodox European macro economists for competing and shaping the economic institutional environment.

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How Did Macro Theory Get So Far off Track, and what Can Heterodox Macroeconomists Do to Get it Back On Track?

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Over the past 80 years macroeconomic theory has gone through some remarkable transformations—the Keynesian revolution, the neo-Keynesian revolution, the New Classical revolution, the real business cycle revolution, the New Keynesian reaction to that New Classical revolution, and most recently the DSGE synthesis revolution, which essentially studies macro economy as a dynamic stochastic optimal control problem facing a representative agent. Currently, we have, as Robert Solow (2008) nicely summarized it, a dominant mainstream macroeconomic theory that tries to understand the macroeconomic problems as a “representative agent, infinite-horizon, intertemporal-optimization-with-conventional-constraints story with its various etceteras.” According to Solow, and I agree, this story is essentially a “rhetorical swindle” that the “macro community has perpetrated on itself, and its students” (Solow 2008: 235)

I have recounted the history of how we got to where we are in macro elsewhere (Colander 1988, 1998, 2006, Colander/Landreth 1996) so I am only going to provide a summary of my arguments here. Similarly, I have also written at length about the shortcomings of the current DSGE theory, (Colander 1996, 2006, Colander et. al. 2008) and have suggested that in its place macro economists follow what I call a Post Walrasian approach, which is an approach that sees the macro economy as a complex system in which the interesting policy issues stem from complex dynamics of adaptive interactive agents. This Post Walrasian approach makes learning dynamics, interactions of agents, and the dynamic feedback among decision makers—aspects that the DSGE model assumes away--essential elements of the theory.

The technical analytic tools to deal formally with such complex systems dynamics, such as ultrametrics and dimensional cluster analysis (Aoki/Yoshikawa 2006) are extraordinarily complicated—and even these tools are likely far too simple to shed much definitive light on complex systems. Thus, I have also advocated that that analytic approach should be supplemented by an ACE (agent-based computable economics) modeling

approach, (Tesfatsion/Judd 2006) in which one creates a virtual macro economy within one's computer, and studies the economy by experimenting with these virtual macro economies. Macro properties are seen as properties that emerge as discernable patterns that owe their existence to the system's complex dynamics. Work is progressing on these approaches, with European macro economists, who have been less affected by the "rhetorical swindle" than have US economists, leading the way. Unfortunately, both these approaches are in the beginning stages; they yield little in the way of policy insights and little in the way of formal analytic conclusions that are scalable to a real world economy.

1. Macro Theory's Role in Policy

Given the current state of macro theory, I think the most honest approach is to accept that we have to conduct macro policy without a formal scientific theory to guide us. I see the current field of macro as an engineering field in which the best we can hope for are loose laws to guide policy that follow from common sense and empirical studies. Currently, macroeconomics is not a scientific field in which we have a firm theoretical foundation.

This does not mean that there is no role for technical work in macro policy analysis. Some of these loose engineering laws can be discerned through statistical analysis, such as the cointegrated vector auto regression approach used by Katarina Juselius (2005). Her approach, which I find highly appealing, is based on careful statistical work and educated common sense application of macroeconomic theory. This Juselius approach is not an a-theoretical approach, but rather an approach in which the role of theory and empirical work are reversed from the normal "theory-first" way US macro economists think. By that I mean that it places empirical work first. It finds patterns in the data--and uses theory as a guide to interpret and discern those patterns.

1.1 An Intelligent Consumer's Understanding of Theory

The Juselius approach to macroeconomics sees macro policy analysis as a highly sophisticated art that requires high level statistical skills and a deep understanding of theoretical ideas. The needed deep theoretical understanding is, however, an "intelligent consumer's" understanding, not a

“producer’s” understanding of the formal theory. The two are quite different types of understanding. An “intelligent consumer’s understanding” is an understanding of not only the technical arguments, but also of its limitations both as a scientific theory and as a guide to policy. A consumer of theory is not concerned with the intricacies of theory, but rather with the applicability of the theory to policy. Economists with a consumer’s knowledge of theory are not economic scientists trying to make contributions to the technical development of the formal model, they are political economists who are directly interested in applying theory to policy.¹ Put another way an “intelligent consumer’s understanding” of theory is a Charles Goodhart understanding.² An intelligent consumer may or may not be able to produce the model, but he or she has the ability to understand it and its implications for policy.

A producer’s understanding of a theory is different; a producer doesn’t need to know the limitations of a theory for policy. Instead, he must know the technical aspects of the model intricately. In fact, it might be better if a producer doesn’t really know the limitations of the theory because that knowledge might reduce his or her incentive for producing the theory. It certainly did mine, and I suspect that it has kept many macro economists from working on DSGE models.

1.2 Heterodox Economists Tend to have a Consumer’s Understanding.

I suspect that most economists in this audience would generally agree with my assessment of DSGE models and of the need for a

¹ This distinction between economic science and political economy is to be found in Robbins’ work, and his famous essay that gave us the current definition of economics was a definition of economic science, not political economy. (Colander, forthcoming-a)

² By that I mean that while Charles Goodhart has not developed his arguments about money and uncertainty within a formal theoretical framework, his writings demonstrate a deep awareness of the insights that follow from formal economic theory, along with the insights of many other theoretical frameworks. In terms of drawing policy conclusions I would rely on his understanding of DSGE theory over that of most, if not all, producers of that theory. His writings convey wisdom. Goodhart is not the only economist who exhibits such wisdom. I could have, for instance, called it a Charles Kindleberger, Leland Yeager, or Herbert Stein type of understanding of the theory. Economists can, of course, have both a consumer’s and a producer’s understanding of theory. Kenneth Arrow and Robert Solow are examples of economists who excel in both areas. Such economists are rare.

complexity approach to macro, although, I am sure, that since this is an academic audience, they will likely have many complaints and problems with particular elements of my arguments. In any case, for sake of argument I assume that my description of the audience is right and that you share my assessment of the DSGE model's application to policy as a "rhetorical swindle." I also assume, for sake of this talk, that we are right. This means that I am asserting that the collective wisdom of a group of heterodox-leaning economists, such as we have at this conference, provides a better guide to policy than is being provided by the collective wisdom of the producers of the mainstream DSGE macro model.³ If that is true, and I think it is, it raises the question: Why aren't the ideas of the people in this room the dominant ideas in macroeconomics? Shouldn't the best ideas win out?

2. Macroeconomic Thought as an Emergent Phenomenon

My explanation for why the best policy ideas are not winning out in macro lies in my conception of the economics profession, a conception that differs from the way most economists think about the economics profession. The dominant approach to understanding the profession, used by almost all historians of economic thought and heterodox economists (I don't include mainstream economists because they don't really think about such methodological issues), is what might be called the *representative researcher* approach. It is an approach that sees macroeconomic thought as

³ To put some specificity to my argument I am using the Chari/Kohoe (2006) discussion of the policy implications of the DSGE model as representative of the policy thinking of producers of the modern DSGE mainstream. They write "The message of examples like these is that discretionary policy making has only costs and no benefits, so that if government policymakers can be made to commit to a policy rule, society should make them do so. (pp 7-8) and "Macroeconomists can now tell policymakers that to achieve optimal results, they should design institutions that minimize the time inconsistency problem by promoting a commitment to policy rules. However, to what particular policies should policymakers commit themselves? For many macroeconomists considering this question, quantitative general equilibrium models have become the workhorse model, and they turn out to offer surprisingly sharp answers. (page 9)

As I believe will be obvious to most, if not all, of the heterodox macro economists here, the "collective wisdom" bar to which I am comparing your collective wisdom to is not set too high. To be fair to mainstream economic theorists, had I chosen Michael Woodford or Robert Lucas, both of whom are far more circumspect about drawing policy implications from the DSGE model, the comparison would have been much more difficult.

if it were the product of a single representative researcher who is searching for the truth.

Within this representative researcher framework it is only natural to think of mainstream macro theory as representing, if not the truth, at least the best approximation of the truth that researchers have found. If that representative researcher doesn't choose the "truth" when it is available, it must either be because he or she is not searching for it because of an ideological bias, or because he or she is stupid. Most heterodox economists attribute both characteristics to mainstream macroeconomists.

I think both characterizations are wrong. In my view, mainstream macro economists who work on DSGE models are extremely smart and are no more, and probably less, ideological than the representative heterodox economists.⁴ (Heterodox economists generally only achieve an ideological neutrality by offsetting ideological views of the right heterodoxy with the left heterodoxy.) So neither stupidity nor ideology is my explanation. Instead, my explanation is that the representative researcher approach to thinking about the evolution of macro theory is not the way to understand the economics profession.

Just as I take a complexity approach to macro, I take a complexity approach to understanding the economics profession. I see the field of economics as a complex evolving system, consisting of many different competing ideas at all times. This means that in the mainstream, there is no single orthodoxy; there is, instead, a plurodoxy of ideas all competing within a set of rules that determine which researchers advance and which do not. The "mainstream view" thus is not a view held by any one in the mainstream, but is instead a composite view that emerges. Because good policy analysis does not enter significantly into the replicator dynamics of the profession, the policy view of the producers of theory does not necessarily imply a deep consumer's understanding of theory. We can ferret out a mainstream policy view, but that view is not necessarily the product of careful conscious thought of any individual producer since mainstream macroeconomic theorists are primarily concerned with science, not policy.

⁴ See Colander, (2005a) for a discussion of this issue.

As a way to do science I support this mainstream approach of separating policy from theory—if someone is going to get the science right, their policy views are likely to get in their way. Science is about understanding, not policy. This means that to the degree one is going to have a formal theoretical model, the model one uses is going to be driven by the available analytical techniques. In my view the DSGE model, just like the general equilibrium model, is a substantial intellectual achievement in the science of macroeconomics. The problem comes when the producers of this theory apply it to policy without taking into account the fact that its assumptions were chosen on the basis of analytic tractability, not policy applicability. It doesn't take a rocket scientist to know that the DSGE model is just not especially relevant for guiding macro economic policy, but it does take someone with a consumer's understanding of theory, so anytime I hear a producer of macro theory making pronouncements about theory based only, or even primarily, on the DSGE model, I know that they are not a trained consumer of economic theory.

Within my complexity approach to understanding the profession, the structure of scientific models being worked on at any particular time is largely determined by analytic and computing technology available to the researcher. Producers of theoretical macro models continually innovate, bringing in the latest analytical technological and computational technologies to the existing model. As they do, they succeed in the academic institutional environment. Succeeding means not only advancing but also getting graduate students or colleagues who continue their work. How well the scientific theory fits policy issues is only a minor factor in a scientific theory's success. Policy is not its purpose; its purpose is understanding. In judging the policy usefulness of current scientific theories, there is no reason why producers of scientific theories are better than non-producers, and they likely worse than educated consumers of scientific theories, who have focused their study on policy relevance of theory.

3. How Macro Policy Analysis got so Far off track

The crux of my argument of how macro policy analysis got so far off track is that modern economics institutions structures promote and advance economists on the basis of their success as producers of science, and not on the basis of their success as consumers of science. Within such

an institutional environment the training that is central to using a theory correctly gets shortchanged. Specifically, the institutional setting of economics in the US, (which alas, is becoming the institutional setting of Europe as well) which has a strong focus on a quality-weighted journal article publishing metric, does not reward and advance political economists. In fact, they are weeded out of the profession, or at least are not advanced in the profession. Some become heterodox economists. Others become minor mainstream economists.

Most thoughtful economic scientists recognize the problems with the policy pronouncements of the less-than-thoughtful economic scientists, but they generally stay out of the debate since they know their training is as an economic scientist, not as a political economist. This means that the perceived composite mainstream view of policy is skewed, and does not reflect the thoughtful mainstream position of policy since that thoughtful mainstream position does not draw their policy conclusions directly from the latest theory. Instead, it reflects the views of the “less-than-thoughtful” economic scientists.

The situation was not always like this. The journal article metric is a recent phenomenon, and it became central in the US only in the 1970s, and is only now becoming central in Europe. Before that time a consumer’s knowledge of theory was more highly valued than it is today, and was less separated from a producer’s knowledge. One reason for this is that the technical aspects of theories were less sophisticated earlier, so less technical specialization was needed.

But in the 1960s because of both technological changes requiring much more research specialization, and institutional changes that gave more relative weight to producer’s knowledge, the replicator dynamics of economics in the US began to change. The institutional structure starting leaning toward selecting and advancing economic scientists with more producer’s knowledge over political economists with a consumer’s knowledge. Economic science pushed out political economy. Training in economics similarly changed; the core of graduate training in the US became more focused on teaching technological skills appropriate to science. As that occurred, the selection process of who became an economist changed, and students with leanings toward consumer knowledge became less likely to be admitted to top schools, and those whose

proclivities were in political economy, and who still made it into the top programs became less likely to advance. In other words, there are fewer Charles Goodhart-type economists being created today than there were 40 years ago.

4. How Institutional Forces Shaped the History of Macro

Thus, my argument is that to understand the prevailing macro theory one must understand the forces at work in the advancement and promotion of economists. Elsewhere (Colander 1988, 2006) I have explained how macroeconomics evolved over the last 70 years in reference to these forces, and have argued that what is called the Keynesian revolution was not a *macro* revolution at all. The so-called Keynesian revolution was kept alive only by its chameleon nature—its willingness to change to fit the technology of the day. My argument is that Keynes had at best, a hazy scientific vision of an aggregate economy that was driven by complex dynamics, a vision that he felt that the “Classical” macro vision of the time had lost. A true scientific Keynesian revolution would have emphasized uncertainty, complex dynamics, emergence, nonergodicity, and the fallacy of composition central to its models. It would have had, as a core belief, the proposition that macroeconomics that was fundamentally different from micro. It would have been a macro that had different laws and different rules of motion than those we used in microeconomics. There would have been no attempt to develop the type of comparative static models that became known as Keynesian macro models. That scientific Keynesian revolution was stillborn, and was quickly lost.

Instead of revolutionizing the field, the Keynesian revolution was quickly subsumed by and subverted by two other analytic technological revolutions—the general equilibrium revolution and an econometric revolution both of which pulled theory away from complex dynamics because neither were then able to deal with such complex issues. They required a much simpler underlying model to be applicable to macro. These revolutions, which were part of an ongoing evolution in economics that was occurring independent of Keynesian economics would have influenced economic thinking whether or not these developments had been placed

under the Keynesian name.⁵ They diverted macro economic theory from dealing with issues of complex dynamics, and instilled in macro a vision of a system with a unique attractor and simple linear dynamics that Marshall had used for partial equilibrium micro analysis. In brief, the science of macro economics was overpowered by a desire to have policy-relevant models that fit the analytic techniques available at the time.

The result was a macro theory that most macroeconomists recognized was not a scientific theory but which was, at best, a loose framework with which to frame macro questions. Because the resulting models were concerned with policy, all too often those supposedly scientific models became immersed in ideology. For example, the mid 1970s were the time of the *money matters* wars. Somehow, Chicago always came to one conclusion, and Yale came to another, both using the same data set. The fact that the dynamical foundation of the model didn't exist or that microfoundations were selectively used didn't bother these macroeconomists of the time. They were too busy developing more and more complicated models based an illogical core. Doing so advanced their careers and led to more people doing similar things.

Let me be clear to what I am objecting to here. It is not the macro models of the time. Much of what was being done in macro at the time could have been justified as rough and ready engineering economics designed to give policy makers some sense of how to deal with something as complex as the macro economy. But macroeconomic theory wasn't presented that way. It was presented as science. That, I think, was fundamentally wrong, and it didn't take a rocket scientist to recognize that it was wrong. Anyone with an "educated consumer's knowledge" of economic theory would have realized it.

It was at that height of the neosynthesis period that I became interested in heterodox economics on both the right and the left. Reading their work, it seemed clear to me that they had best kept a consumer's understanding of macro, and had not been fooled by neosynthesis pretense of science. Heterodox macro economists such as Paul Davidson, kept telling

⁵ Why all these disparate developments came under the Keynesian moniker is a difficult question in social thought, and one that I will not deal with here. I point it out here because in order to understand the history of macro one must recognize these disparate elements of the Keynesian revolution and that many of these developments worked at cross purposes.

the world that the neoclassical/NeoKeynesian synthesis had no clothes, and should be abandoned. But no one listened. Why?

My brief answer is that heterodox macro economists failed to influence the profession because they did not take into account in their decisions the way in which the profession works, and the way in which ideas advance and develop within the profession. They thought that the truth would win out just because it was the truth. Then, when the truth as they saw it did not win out, they separated themselves from the profession and created their own subgroups—Post Keynesian, Austrian, and Institutionalists--and started communicating solely within their subgroup. In short, they became heterodox. As that happened, they no longer influenced the profession, allowing an even larger deviation between a consumer's understanding and a producer's understanding of theory.

5. What Can European heterodox economists do to get it back on track?

Let me now expand on that brief answer and provide my answer to the second question—what can European heterodox macro economists do to get it back on track? My answer to this second question is embodied in the above arguments—to get macro back on track one has to develop an institutional framework that values a consumer's understanding of theory as well as a producer's understanding of theory. To do that one has to play a role in shaping the quickly changing mainstream European economics institutional environment. This is the essence of my argument today. Success of ideas depends on success within the profession, and it is far more important for the heterodox economics community to work on succeeding in the mainstream institutional structure than it is to further develop their specific ideas.

Let me give some specific suggestions.⁶

- **Be Economists, not Heterodox economists**

There is one important conversation in economics, and that's the mainstream conversation. Almost by definition, heterodox economists don't

⁶ A more substantial discussion of these suggestions, along with some other ones can be found in Colander (forthcoming-b).

compete in the mainstream environment. They create their own conferences, their own journals, their own graduate programs and their own conversation. I have nothing against starting a separate conversation per se. As an incubator for new ideas, it can be helpful. But if it develops a separate conversation, and research program, it had better provide jobs for the young researchers either by colonizing an ever larger portion of the teaching area, or by making its ideas desirable to the mainstream. The reason this is fundamentally important is that success of a program takes multiple generations—it is incumbent on any group to not just make arguments but to develop an institutional structure in which their students and others who agree with them can flourish. This is something that heterodox economists have failed at. Essentially heterodox economists have eaten up their seed capital, leaving an institutional environment within which their followers have an increasingly difficult time of propagating.

I am, of course, sympathetic to those heterodox economists who want to stay out of the mainstream debate. Ideas need nurturing, and the environment for ideas within mainstream economics is unfriendly. Its requirement that ideas be formally modeled makes it hard for novel ideas to develop. Heterodox economics communities provide an incubator environment within which ideas can germinate and sprout. They are wonderful idea incubators, which allow people to have more friendly critics around, who treat their ideas more gently than they would be treated in mainstream economics. This gentle treatment gives the ideas a chance to germinate and perhaps even to sprout.

The problem is that ideas cannot remain in the incubator forever, and for the heterodox communities to serve the function of incubator, it must transfer the idea, developed in heterodoxy, up to the mainstream. All too often, what happens with ideas developed in heterodox economics is that they remain in their incubator and do not cross-pollinate with mainstream ideas. Both heterodox and mainstream economics are worse off for it.

To make the transfer of a scientific idea from the heterodox incubator to the mainstream, the ideas must be developed in a formal model and buttressed by technical empirical work. Transferring the often vague heterodox ideas to such models is difficult; often, the reason the mainstream has shied away from the complicated issues that heterodox economists see

as important is that mainstream economists thought the issues were intractable given the existing tools. This is an important point—tools govern what can be done scientifically, and the fact that mainstream economists are not looking at an issue does not mean that they believe such issues are unimportant. It just means that the analytic technology does not yet exist to deal with such questions. This leads me to my second suggestion.

- **Give the Mainstream its Due.**

I often hear from my heterodox friends that the economics profession is closed to their views. I totally disagree. As I stated above, the mainstream profession is what I call a plurodoxy, not an orthodoxy. They are open to ideas *if* those ideas show a complete understanding of the reasons the mainstream is doing what it is doing, and of the theory. Good mainstream economists know the problems with their theory; but they also operate within the methodological rules of the profession. You are not telling them anything new when you make arguments that the macro economy is nonergodic, that one needs a non-linear dynamic model to describe events, that stochastic is different than uncertain, or that the representative agent model misses important elements of reality. If you think you are, then you are fooling yourself.

The problems that heterodox economists point out are recognized by the good mainstream—they are not new insights. If one has ever talked with Robert Lucas or Tom Sargent, you will know that they are deep thinkers. Just take a look at Sargent's latest work on self-confirming equilibrium. (Sargent 2007) The good mainstream economists have thought of most of the problems that heterodox economists have thought of and then some. But they are also playing by producer's of theory rules, which makes them limit their formal work to available techniques. They are pushing the empirical and analytic limits as far as possible. Moreover, good mainstream macroeconomists have an enormously impressive consumer's understanding of theory. (Some, of course, do not.) The good ones who don't have that consumer's understanding could likely acquire it if they were pushed. Generally, they are not pushed because those heterodox economists who could push them don't enter the mainstream conversation.

- **See mathematicians and technical economists as your allies, not your nemesis.**

Mainstream economic science is a formal modeling field; it is not going to change. It has chosen to study the issues it has because the tools it has available could be used to shed light on those issues. Advanced mathematicians can bring in new ideas because they have new ways of looking at issues that mainstream economists know are important, but shy away from because they don't have the techniques to handle them. Thus, there is a natural symbiosis of heterodox economics with advanced applied mathematicians and statisticians. That symbiosis has not been developed, in part because heterodox economists have been anti-math. In my view heterodox economists should be precisely the opposite—they should welcome higher and higher levels of mathematical and statistical formalization into economics because that is what will allow the formal consideration of the issues they want considered.

Most heterodox economists don't have the skills to do that formal mathematical work, and, for those who don't have them, I am not arguing that they should develop them. They should see themselves as political economist, not economics scientists. But I am arguing that all heterodox economists who want to take part in the scientific debate in economics should have a consumer's understanding of what is going on in high-level mathematics and statistics, with an eye to see if new analytic techniques may be able to address some of the issues they believe should be addressed. They should be looking to do joint work with ultra mathematicians and ultra statisticians, because only they have the skills to do the work formally. These "ultras" often find mainstream economists far too rigid for their tastes, and have a deep interest in entering the economics debate. Working with heterodox economists provides an entre for them into economic debates.

- **Worry less about methodology.**

Most heterodox economists don't work with formal mathematical models; instead they focus on methodological issues. I think that is a mistake. Unless he or she is a philosopher specializing in methodology, just about everything to be said about methodology has been said. To think that anyone but a specialist is going to have much to add on methodology is similar to a neophyte thinking he can do better than an index fund in investing.

Instead of complaining or discussing methodological problems, a heterodox economist could be working on specific institutional problems that both underlie and affect methodology, such as creating an alternative ranking system. If the current ranking system does not put heterodox research in an appropriate light, he or she could develop a research agenda designed to create an alternative ranking system that does, and explain why it is a better system. There are many foibles with the current ranking systems, especially as a ranking system for economists who are primarily teachers of economics, or whose interests are in hands-on applied policy and political economy. Heterodox interests fit much better into what undergraduate teaching needs and were a separate “teaching-oriented” ranking system developed, heterodox economists would come out much better in the rankings.

- **Don’t dwell on unfairness.**

If there is to be a dialog between heterodox and mainstream economists it has to originate from heterodox economists. The mainstream has the power, and has little incentive to give it up, and for the most part is totally unaware of the existence of a heterodoxy even existing. Heterodox economists today find themselves in precarious positions, and are being squeezed out institutionally both in the U.S. and in Europe. Is it fair that most of the effort toward communication will have to be on the heterodox economist’s side? Absolutely not. But so what? Regardless of how unfair the profession is to you, it does not help to feel sorry for yourself.

I fully agree, heterodox economists are discriminated against and ill-treated. But complaining about it will not change the situation when the other side has the power. So, I see no other option than to live with it. If you define your role in a way that allows you to succeed within the institutions that exist, you have more of a chance of changing the institutions than you do if you are marginalized. That’s why I do not see it as especially helpful to distinguish oneself as a heterodox economist, and not just as an economist who has certain beliefs. To differentiate oneself as heterodox places one in opposition to an orthodoxy that the mainstream doesn’t believe exists, and thereby reduces the possibility of communication with the very people who I believe heterodox economists should be communicating with.

- **Concentrate on areas where you can make a difference.**

As I have argued in Colander (2005a) economics textbooks are far from consistent with much of the latest thinking in the profession. One of the reasons textbooks are inconsistent with the best thinking in the profession is that the mainstream does not focus on teaching or pedagogy. This lack of concern about teaching by the mainstream leaves an opening for economists who are interested in a consumer's understanding of theory, which includes most heterodox economists. By addressing their arguments to the narrow issue of what economists teach in their textbooks, and how they can do it better, heterodox economists are on much firmer ground, and can get a better hearing from mainstream journals. Economists receive much of the funding they do because of their teaching. Given its importance, there should be ten times as much research on what it is we teach, and why we teach it, than there is. It is a wide open area where heterodox economists to make a contribution without placing themselves in opposition to mainstream economists.

I don't try to make any contributions to the science of macroeconomics. It is out of my area of expertise. But I do try to make contributions to political economy, and to the teaching of economics. I study the organization of the economics profession. For the most part, the mainstream has accepted that area of specialization; they don't see me in opposition to the mainstream—they just see me as a little bit weird—what one reviewer called the mainstreams economists' court jester—who is allowed to say what everyone knows but has better sense than to say it. Thus, when they need a token heterodox economist, they often call on me, just as when heterodox economists need a token mainstream economist they often call on me. (I am the only white male protestant token that I know.)

- **Think carefully about how your approach can succeed for both you and your students in the institutional environment you are in.**

I saved my most important recommendation for last. Within my emerging complex system understanding of the profession, success depends less on the truth and insightfulness of an idea—many ideas can be true and insightful--and more on the replicator dynamics of the profession. Ideas that develop are the ideas of those economists, and their students, who are institutionally successful. Thus a necessary precondition of furthering one's

ideas is figuring out what leads to success in the environment. One must have a reasonable plan for doing well within that environment. For European economists at this time, I believe this issue is especially important.

The reason I say this is because, while the US economics institutional environment is relatively fixed, the European environment is not. It is in the process of change. Any group of economists interested in spreading their economic ideas should, in my view, be spending as much time figuring out how to influence that environment, as they do working on extending their ideas. That means that they should become actively involved with the emerging European economics organizations; they should work to combine the regional economic organizations with the European economics organizations. From talking with European economists, I believe that only a minority want to adopt US institutional structures. But that is what is happening. All European heterodox economists should be working together collectively to have their voice heard as a necessary voice in mainstream economics.

For example, here in Europe there is a new European Economic Association. It is, in principle, controlled by the members, but in practice it is controlled by a small group of mainstream economists whose goal seems to be to make European economics much more like US economics. These are good economists who care about economics. They are open in their views. They are also, almost all, economic scientists who have forgotten that political economy is an important subcomponent of the field of economics, and needs to be nurtured as well. Their sense is that heterodox economists, and others who do not want to go along with their changes, simply do not understand science or do not want to subject themselves to the rigors of intellectual competition.

I think they are wrong. There are many economists in Europe who want to make European economics better, but who disagree with their strategy. The problem is that this group has essentially pulled out of EEA. Pulling out is, in my view, precisely the wrong strategy, because it removes the voice of reason in applying economic theory to policy. Instead of pulling out, my view is that heterodox economists should whole-heartedly enter into the EEA. They should devote time and energy into creating sessions for the meetings and in recruiting members to the organization.

They should volunteer to serve on committees and continually be providing suggestions for new programs to “broaden” the education and training of economists that could get support in the broader mainstream community. They should also exercise their right to vote, if need be conducting organized write in campaigns for a candidate who shares your views.

I am not arguing that heterodox economists will be welcomed with open arms. But they will be allowed in, and if they are truly the majority, they will be given a strong voice. Whereas individual departments don't have to be pluralistic, these organizations of economists must at least appear to be.

6. Conclusion

My talk has deviated significantly from macroeconomic theory. I did so because I think the outlines of what a reasonable modern macro theory should be is clear. It is a science of macro that sees macroeconomics as falling into the science of a complex systems. Formally analyzing these is the domain of ultra mathematicians.

Modern macro policy has two dimensions. One is a technical policy dimension, which is highly dependent on statistical tools. This technical policy dimension requires a deep knowledge of cointegrated vector auto-regression to determine what potential patterns are in the data, along with a deep consumer's knowledge of macroeconomic theory, to determine which of those patterns we should be using to guide out policy suggestions. Like the science of economics, it is highly technical and requires enormous statistical expertise simply to play at it, and then enormous creativity and concentrated focus to be able to actually make a contribution.

For those without that expertise, there is another dimension--the general policy dimension. This requires a solid consumer's understanding of modern macro theory, and a consumer's knowledge of modern statistical tools. It is where most professors of economics who are teachers and advisors to government will most likely fit. There is much work to be done by this group, both in macroeconomic pedagogy, and in policy advice, and it complements the developments in the science of economics. The problem facing European macroeconomics is developing an institutional structure where this each group can specialize in what they are best at.

References

- Aoki, M., Yoshikawa, H. (2006): *A Reconstruction of Macroeconomics: A perspective from Statistical Physics and Combinatorial Stochastic Processes*, New York: Cambridge University Press.
- Chari, V.V., Kehoe, P. (2006): Modern macroeconomics in practice: How theory is shaping policy, in: *Journal of Economic Perspectives*. Winter.
- Colander, D. (1988): The Evolution of Keynesian Economics in Omar Hamouda and John Smithin, *Keynes and Public Policy after 50 Years*, Cheltenham: Edward Elgar.
- Colander, D. (ed.) (1996) *Beyond Micro Foundations: Post Walrasian Macroeconomics*, Cambridge: Cambridge University Press.
- Colander, D. (1998): Beyond new Keynesian economics: Post Walrasian economics in *New Keynesian Economics Post Keynesian Alternatives* (Roy Rotheim, ed.), Cheltenham: Edward Elgar.
- Colander, D. (2003): Post Walrasian macroeconomics and heterodoxy: Thinking outside the heterodox box, in: *International Journal of Political Economy*, 33(2): 68-81.
- Colander, D. (2005a): Economics as an ideologically challenged science, in: *Revue de Philosophie Economique*, 1: 3-24.
- Colander, D. (2005b): *Stories Economists Tell*, New York: McGraw Hill Publishers.
- Colander, D. (ed.) (2006): *Post Walrasian Macroeconomics: Beyond the DSGE Model*, Cambridge: Cambridge University Press.
- Colander, D. (forthcoming-a): What Was “It” that Robbins was Defining?, in: *Journal of the History of Economic Thought*.
- Colander, D. (forthcoming-b): Moving Beyond the Rhetoric of Pluralism: Suggestions for an “Inside-the-Mainstream Heterodoxy”, in: *Economics and Pluralism* William Garnett, (ed.) London/New York: Routledge.

How Did Macro Get so Far Off Track...

Colander, D., Landreth, H. (1996): *The Coming of Keynes to America*. Cheltenham: Edward Elgar.

Colander, D., Howitt, P., Kirman, A., Leijonhufvud, A., Mehrling, P. (2008): Beyond DSGE models: Toward an empirically based macroeconomics, in: *American Economic Review*, May, 98:2.

Juselius, K. (2005): *The Cointegrated VAT Model: Methodology and Applications*, Oxford: Oxford University Press.

Sargent, T. (2007): Evolution and intelligent design, New York University Mimeo.

Solow, R. (2008): The state of macroeconomics, in: *Journal of Economic Perspectives*, Winter.

Tesfatsion, L., Judd, K. (2006): *Handbook of Computational Economics*. Handbooks in Economics Series, Elsevier.