The Keynesian Method, Complexity, and the Training of Economists

by

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Keynes is dead; dynamic programming; Keynes is still dead. That's the way Stanford graduate economics students recently summed up what they had learned in their core graduate macroeconomics course. (Colander, 2007 pg. 152) Graduate students at other top US programs concurred, and in my recent interviews with them the strong feeling among students was that the core graduate macro course provided them little in the way of macro policy thinking and that the course had nothing to do with Keynes. It was a course in which, by design, students learn dynamic stochastic programming.[1] A comment of an MIT student was representative of student's view of what they learned about policy in macro. He stated: "Monetary and fiscal policy are not abstract enough to be a question that would be answered in a macro course." (Colander: 169)

As Solow (2007) points out the situation is a far cry from the core macroeconomics courses of the grand macro synthesis period of the 50s-70s when students studied "Keynesian synthesis" models and macro policy in their core courses. In the synthesis period students learned variations of IS/LM models, and how that IS/LM type reasoning underlay both large econometric macro models and macro policy. Compared to modern DSGE models, the synthesis models were technically simple, and macro graduate student training of the time was not highly technical. It involved a blend of institutional, historical, and policy training. In this grand neoKeynesian/neoclassical synthesis, macro theory, empirical work and policy were entwined in a superficially connected, but ultimately unsatisfying, set of models that built differences of policy position on slim reeds such as Pigou effects and wage and price rigidity assumptions. The models could be adjusted to "explain" just about any observation, which meant that, more often than not, researcher's judgments determined the results of the model. By that I mean that it was a synthesis in which one could predict the policy implications of models based on who was doing the modeling. It was hardly a situation that inspired confidence in the usefulness of the models.

As should be clear from the above description I am no fan of the grand synthesis macroeconomics. But I am also no fan of modern DSGE macro as a basis for policy analysis. Robert Solow has nicely captured my view of current macro theory when he wrote that modern macro is best seen as a "rhetorical swindle" that the "macro community has perpetrated on itself, and its students" (Solow 2008: 235)

In this paper I (1) explain why I am not a fan of either grand synthesis macro or modern DSGE macro and how both have deviated from the Keynesian method, which I believe should have been the most important legacy of Keynesian economics, (2) provide my explanation of how the profession moved from Keynes' writings to the modern

[1] At an AEA session on the core with top economists from Harvard, Chicago, MIT and Columbia, all agreed that macro as it is taught now could be eliminated from the core as long as the core offering were changed so that students would get the dynamic stochastic control theory elsewhere.

DSGE approach, and (3) explain why I believe the loss of the Keynesian method is of concern.

1.1 Losing Sight of the Keynesian Method

The grand synthesis and the DSGE models fail for me because they have given up a central element of the Keynesian method. The Keynesian method takes seriously Mill's famous half truths proposition (Mill, 1838) that, at best, any formal model will give us only a partial, highly imperfect, picture of the an economic reality. To move from formal models to policy one must have additional knowledge of economic reality that goes beyond knowledge of the economic model. The Keynesian method held that, at best, theoretical models in economics can only be used to guide researcher's judgment, not to arrive at policy conclusions directly from theoretical models. It is a method that accepts the limitations of theory, empirical work, and intuition in understanding the economy. It accepts our inability to develop a complete scientific model of the macro economy and sets it goals much lower—to provide a set of tools that may help answer certain questions about how the macro economy works.

The Keynesian method has a long history. While Keynes may have differed with Classical economists on many issues, he did not differ with them on what was the appropriate method to use, and his work is best seen within the Millian/JN Keynes/Marshallian methodological tradition. My problem with both the grand synthesis and the DSGE approach to macro is that they have given up this method and the advocates of both do not see the two as complements with different purposes, but as substitutes. This makes reasonable discussions of the advantages and disadvantages of the two types of models almost impossible.

Keynes' (1938) summarizes his method succinctly in the following quotation. He writes:

> Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world. It is compelled to be this, because, unlike the typical natural science, the material to which it is applied is, in too many respects, not homogeneous through time. The object of a model is to segregate the semi-permanent or relatively constant factors from those which are transitory or fluctuating so as to develop a logical way of thinking about the latter, and of understanding the time sequences to which they give rise in particular cases. Good economists are scarce because the gift for using "vigilant observation" to choose good models, although it does not require a highly specialized intellectual technique, appears to be a very rare one.

There are a number of aspects of this quotation that deserve highlighting. The first is that the reference to models is plural. In the Keynesian method, there is not a single model of the economy, or even of part of the economy; there are many. The second is that intuition and judgment need to be given a separate role in arriving at a policy conclusion. Keynes accepted that pure theory and analytic methods will only get us so far in understanding the events around us, and in arriving at policy conclusions. For Keynes, choice of models was an art, not a science with a well defined methodology.

The reason this art/science distinction is important is that to usefully practice the art of policy economics one needs a different set of skills than are needed to be an economic research economist such as a theorist or an applied econometrician whose primary outputs are academic papers for other academics. To be a policy economist, one needs another skill, which might be called intuitive wisdom. It is a skill that includes a knowledge of how institutions actually function, a sense of history, a knowledge of what is happening in other fields of study, an understanding of real-world politics, and a practical sensibility. All these skills are central to economist's role in policy making. It is those skills that allow the policy economist to choose among models.

A follower of the Keynesian method would never move directly from a model to policy unless he or she believed that the model was the relevant one for the question at hand. He or she would keep a meta model of the relevance of various models at the back of his head, and adjust the specific model's conclusions to fit the particular circumstances. In the Keynesian method models are tools, not rules.[2]

Keeping thoughts in the back of one's head is definitely not formal science; the Keynesian method fully accepts that. For that reason it makes as strict a separation of policy from the formal or pure science of economics as it can.[3] This made what might be called the Policy Separation Proposition central to the Keynes's method: The policy separation proposition holds that if a researcher doesn't have expertise in the specific institutions and real world issues relevant to the problem at hand, then he or she should not make any strong pronouncements about the model's implications for policy as a scientific pronouncement. Such pronouncements should be left to others who have the appropriate training, or be made by the economist with the caveat that he is she is not speaking with the authority of science.

As I stated above, this Keynesian method did not begin with Keynes. It was a standard tenant of Classical economics, and in adopting it, Keynes was accepting Classical methodology. What he objected to in Classical economics was not its method, but its focus on a particular model that centered on long run equilibrium and did not consider potential problems in arriving at the long run equilibrium.

To see how central this method was to Classical economists, consider three Classical economists who took a strong interest in methodology: Nassau Senior, J. N Keynes, and Lionel Robbins. Nassau Senior writes:

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[2] Keynes writes "It is a great fault of symbolic pseudo-mathematical methods of formalizing a system of economic analysis…that they expressly assume strict independence between the factors involved…; whereas, in ordinary discourse…we can keep "at the back of our heads" the necessary reserves and qualifications…in a way in which we cannot keep. (Keynes, 1936: 237)

[3] There are many different definitions of science; Classical economists were referring to a very narrow definition, which A. C. Pigou called "light-bearing" science when they advocated strict separation. They had other work which Pigou called "fruit-bearing" science, that was designed for policy analysis. This work was more engineering than science, and was not to be separated. It was a tool to be used in policy analysis, but was not meant to be treated as fully scientific, but rather as a rough and ready guide for particular issues.

> (The economist's) premises consist of a very few general propositions, the result of observation, or consciousness, and scarcely requiring proof, or even formal statement, which almost every man, as soon as he hears them, admits as familiar to his thoughts, or at least as included in his previous knowledge: and his inferences are nearly as general, and, if he has reasoned correctly, as certain, as his premises.
>
> But his conclusions, whatever be their generality and their truth, do not authorize him in adding a single syllable of advice. That privilege belongs to the writer or statesman who has considered all the causes which may promote or impede the general welfare of those whom he addresses, not to the theorist who has considered only one, though among the most important of those causes. The business of a Political Economist is neither to recommend nor to dissuade, but to state general principles, which it is fatal to neglect, but neither advisable, nor perhaps practicable, to use as the sole, or even the principle, guides in the actual conduct of affairs. (Nassau Senior, 1836: 2-3)

For Senior and for most early Classical economists concerned with methodology, economic science of the time was a branch of logic. In the pure science of economics at the time one did theory, which meant that one developed theorems from almost self-evident principles. But, as Senior makes clear, economic theory was not meant to directly guide policy. To move from the theorems of the science of economics to the precepts of policy-relevant economics, they believed that one had to rely on common sense judgment and institutional knowledge which involved different skills than did economic theory.

This separation of theory and policy method was strongly advocated by J.M. Keynes' father, J.N. Keynes, in his famous summary of economist's methodology at the turn of the century. (Keynes 1891) Like Senior, J.N. Keynes saw the pure science of economics as a relatively narrow branch of economics that needed to be strictly separated from the branch of economics that dealt with policy prescriptions. He argued that the two branches of the field of economics had quite different methodologies. He writes that "a definitive art of political economy, which attempts to lay down absolute rules for the regulation of human conduct, will have vaguely defined limits, and be largely non-economic in character." (J. N. Keynes, 1891: 83)

Lionel Robbins was equally clear about the need to strictly separate theoretical models from policy. In his Ely Lecture, (Robbins, 1981) where he reflects back on how his famous 1932 essay (Robbins, 1932) was incorrectly interpreted by the profession, he states explicitly that the economics profession needs a separate branch, which he called political economy, to deal with policy. He writes that this policy branch of economics "depends upon the technical apparatus of analytical Economics; but it applies this apparatus to the examination of schemes for the realization of aims whose formulation lies outside Economics…." (Robbins, 1981: 8)

2.1 How the Profession Moved Away from the Keynesian Method

Economist's training up until the 1930s was designed to train economists in both the art and science of economics. This could be done because at the time the formal science of economics was underdeveloped, and for the most part, its theory was heuristic and verbal; formal, mathematically precise theoretical work was only done by a small number of economists. Up until the 1930s mathematical economics was the outlier, not the norm, since at the time an economist's training was not designed to create economic scientists. Similarly, economist's training at the time involved minimal statistical work. This was reasonable since at the time, empirical statistical work was in its infancy, and was not seen as central to economic science.

The underdeveloped nature of both theory and statistical work left more time open for training in literature, history, and institutions. It allowed Classical economists to develop an expertise in both the science of economics and in applied policy economics. Because of the limited techniques involved these economists could have a firm foundation in each branch of economics. As Robbins stated, even though there was a separation it was reasonable for economists of the day to be interested in both. He writes: "To me at least, it seems difficult to believe that recognition of the distinction between the two kinds of propositions will prevent any man of spirit from being interested in both. (Robbins, 1938: 345)[4]

3.1 Method Selection Mechanism within the Economics Profession

The acceptance that it was impossible to directly draw policy advice from models started to change in the 1930s, when economists attempted to more directly relate theory and policy. In micro, a separate branch of theory, welfare economics, developed, which attempted to merge the theoretical and policy branches of economics, drawing policy implications directly from microeconomic models. In macro there was a parallel movement with the development of the consumption function and the IS/LM models.

Explaining why this change occurred is beyond the scope of this paper, but my hypothesis is that the explanation is connected to the replicator dynamics of the economic profession, which in turn is connected to the training future economists receive, and how they advance in their careers. In my view, these replicator dynamics, far more than prescriptive discussions of methodology, determine economist's method, and it was a change in those replicator dynamics that led to a change in method.

As I stated above, up until 1930s economists were broadly trained, and the profession was not dominated by researchers who had been trained in economics specifically, but rather were trained more broadly in moral philosophy. They choose to specialize in economics after receiving a broad training and their method reflected that training. As economics as a separate discipline and separate academic subject developed, the training started to get more specialized. For economic science, this was an enormous advantage; it allowed the introduction of more advanced analytic techniques, which in

[4] Keynes is an example of an economists with interests in both branches of economics. He was highly involved in financial markets and in advising policy makers, but because theory was not highly technical, he would also be involved in theory. So he could reasonably discuss policy from an insider's perspective and simultaneously contribute to the cutting edge of economic theory.

turn required more technical training devoted to it. Hence, the 1930s and 1940s saw myriad developments in economic theory.

These improvements in technical training came at a cost, however. As economics training moved away from the general training in theory, policy, history, and institutions that built in the Keynesian method into economist's sensibility, it became more difficult for economists to specialize in both the science and the art of economics. Something had to give, and in the fights over method that occurred in the late 1800s those favoring the art of economics lost, which sewed the seeds for a change in method. It did so gradually over decades, through a change in the training economists received. Specifically, slowly, economics training moved away from institutional, literature, and historical studies and toward more technical training in the tools of modeling.

The change in training only became substantial in the 1950s, at which time it accelerated, until now, in 2008, it almost goes without saying that economics training is designed to train technical scientists in modeling techniques, not applied policy economists who have broad training in institutions or political sensibility that would be associated with the art of choosing models.[5] Today, it is just assumed that young economists will pick up the other information should they want to become involved in policy; it is not part of their formal training. This change in training has brought about a fundamental shift in method from the 1930s until now, which, because it has been gradual, has been little noticed by the normal economist.

The reason it has taken so long for change in economics to occur is connected to the working lifespan of economists. The productive lifespan of an economist is about 40 years, and their immediate students will also likely reflect the views. Since students of professors tend to reflect the method they learned from their professors, a well established method can continue to exist (at decreasing levels of intensity) for up to 80 years after that method is no longer the dominant method of cutting edge economists. Thus, while the Keynesian method started to die out in the 1930s, it took well into the 1980s for it to be largely eliminated as an acceptable method, although one still one hears small vestiges of it from older economists (such as myself and others at this conference) well into early 2000s.

I am not arguing that the movement away from the Keynesian methodology was a conscious movement by the profession. My argument is that few economists explicitly consider methodology; they do what they do. Their methods are guided by the incentives they face, not by an explicit choice. Students coming into economics look to see who are considered successful economists and then they copy and adapt those successful approaches into their approach, while simultaneously incorporating new analytic and empirical techniques into their work. Thus, the advancement procedures economists face are central to their choices about what to study and what methods they employ. Those

[5] The argument here is not that science is irrelevant to policy. It may well be. The argument is that the primary focus of science is on abstract understanding, and the applicability of the scientific work is secondary and not the driving force behind the research. Someone could be a wonderful economic scientist and a horrendous policy economist and visa versa.

who do well in the advancement procedures tend to reproduce their approach, and determine the direction of the field.

4.1 Academic Incentives and the Path of the Keynesian Revolution

The evolution of what is called Keynesian economics can be best understood as a result of the method-selection mechanism described above. If one reads Keynes' *General Theory,* 1936) one can come away with a variety of different views of what it might mean. At a minimum one will get a sense that an economy could get into trouble and could end up at an undesirable equilibrium. But how and why was not clear. It couldn't be made clear since it required a level of sophistication in the mathematics of complex systems that did not exist at the time. The coordination failure issues it raised could only be meaningfully considered within a highly complex dynamic framework, and the appropriate models would involve interacting strategic agents and would likely exhibit highly complex dynamics and multiple equilibria—models that the best mathematicians of the time had not yet developed. Moreover, the empirical tools available to economic researchers at the time were too weak to actually test any of these models in a reasonable scientific way. Even today economists are far from having the expertise to formally model our economy as a complex system, and most economists do not have the technical training to work on the cutting edge of complex systems analysis.[6]

Put simply, picturing the macro economy as a complex system and a depression as the result of the intricacies of complex dynamics may have been Keynes' visionary idea behind the *General Theory*, but it was not a vision that could survive within the evolving institutional framework of professional economics of the time when mathematical economics was only starting to emerge as a separate field, and professional journals that provided a forum for specialized technical discussions were in their infancy. Given such an institutional structure of the economics profession, it is not surprising that Keynes' presentation of his theory was heuristic and that he did not deal with how his ideas could be translated into a precise mathematical presentation.

The closest that economists came to beginning to model the economy as a complex system at the time was work by a small group of economists such as Richard Strotz (Strotz et al. 1953) and Richard Goodwin (1947).They started to work formally to cast Keynes' ideas in the mathematics of non-linear dynamics. The problem for them was that most economists of the time felt uncomfortable dealing with the complex mathematics needed to do so. Put simply, the task was beyond the level of most mathematicians of the time and beyond the level of almost all economists. The problems that theoretical macro posed were simply too hard to crack with the analytic technology then available. Thus, these mathematical economists generated few highly successful students to carry on work and the non-linear dynamic approach faded away.

Another group of heuristic economists, such as G.L.S. Shackle (1955) and Hyman Minsky (1975, 1986), Robert Clower (1965) and Axel Leijonhufvud (1968) worked on developing an intuitive understanding of macroeconomics within that complexity vision.

[6] See Colander, (2006) (ed.) for a discussion of this modern work.

Their heuristic work reflected what elsewhere I have called an educated "consumer's understanding" of the complexity approach. While their work was seen as visionary, it did not develop a significant following since it did not offer a clear path forward to advance it.[7] Their work did not fare well in the competition of ideas in the post Keynesian period because that heuristic work also did not offer a research path for students that would allow their students to thrive in the then-existing replicator dynamics of the profession. Once it was pointed out that the economy is complex, nonergodic, and fundamentally subject to uncertainty, there was not much more to say heuristically.

Thus, my argument is that instead of becoming a complexity revolution that started analyzing the economy as a complex system, the "Keynesian revolution" was quickly translated into a rather mundane set of models that were more amenable to the mathematics available to the cutting edge economists at the time. Only such mundane models could do well in the peer-review replicator dynamics of the time. Thus, Keynesian economics, which could have been the beginning of a complexity revolution in economics, evolved into grand synthesis macroeconomics, which modeled the economy as a unique equilibrium, comparative static, multi-market equilibrium system, in which the only problem was institutional rigidities or wealth illusion.

Elsewhere (Colander, 2006) I have discussed how Keynes's ideas were integrated with other developments in economics, and how what was called the Keynesian revolution was not a single directed revolution at all, but instead a multifaceted set of developments in theory, pedagogy, policy, and empirical work, each of which was part of an ongoing evolution in economics that was occurring independent of Keynesian economics, and which would have influenced economic thinking whether or not these developments had been placed under the Keynesian name.[8] These developments reflected the changing technology and changing institutions of the time, and often worked at cross-purposes with each other.[9] Because of its multifaceted nature it is best to think of the Keynesian revolution not as a revolution of like-minded individuals with common goals and views, but to think of it as a collection of developments in economic thinking that were classified under a broad umbrella term "Keynesian", but which could easily have been classified under various alternative headings

The first of these developments involved theory. In the 1930s there was the movement from a Marshallian partial equilibrium approach to a Walrasian general equilibrium approach. The differences in the mathematics between the two approaches were not all that great, but the difference in method, was enormous. Specifically, phrasing questions in general equilibrium involved a major movement away from the Keynesian method. It replaced the "many model" approach, in which models were aids to

[7] Leijonhufvud moved into computational economics which is currently an important element of the developing complexity approach to macroeconomics. (Colander, 2006)

[8] 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.

[9] It is precisely because the Keynesian revolution involves so many different elements, that there was always, and still is, enormous ambiguity about what precisely Keynesian economics was.

intuition, to a “single model” approach where the formal results of the model were given greater priority and not seen as at best a half truth. The back of economist’s minds were lobotomized, and the full truth had to be found in the model.

Although Keynes followed Marshall on method, what were called Keynesian models quickly became tied in with Walrasian general equilibrium. This Walrasian general equilibrium modeling approach was quite incompatible with the Marshallian method, but it was a boon to academic researchers whose advancement depended on journal article publication. There were numerous models that needed to be developed, which meant large numbers of articles and dissertations exploring issues such as money neutrality, Pigou effects, multipliers, and alternative specifications of IS and LM curves flowed from this merging of Keynes’ ideas and general equilibrium.

A second of these developments was the blossoming of statistical work in the 1940s. Because of analytic developments in statistical theory and computational technology, a new field of econometrics was developing that allowed economists to relate their models to the data in previously impossible ways. This gave some economists the hope of developing models that could be used to actually control fluctuations in the economy. As is clear from his review of Tinbergen (Keynes, 1939) Keynes was not enthusiastic about this empirical work. He felt that it was too underdeveloped to shed much light on the problems. Nonetheless what was called Keynesian macroeconomics adopted it whole hog. The combination made Keynesian macroeconomics avant-garde, and gave the results of its model a scientific aura that it otherwise would not have had. It also increased its attractiveness to students enormously, providing both theoretical and empirical dissertations and articles to write.

A third development that was occurring was a change in the pedagogy of economics to accompany the changing method. Models were becoming central to the teaching of economics, and, whereas in the 1930s economics textbooks were broad treatises of reasonable insights about economic events, by the 1960s such books no longer fit the new methods that were becoming standard in economics. The change began in the 1940s. In the United States, for example, Laurie Tarshis' (Tarshis: 1947) was the first U.S. “Keynesian” text, but it was written in the older literary tradition, and it soon came under explicit attack by anti-Keynesians, such as members of the Veritas Society. It was quickly replaced by Paul Samuelson’s text (1948), which was more scientific-looking, and more structured around simple formal models.

Samuelson’s text became the prototype for modern economics texts. It created a separate macroeconomics, which led to the separation of the principles course into two courses: microeconomics and macroeconomics. It also introduced students to a macroeconomic framework centered on the AE/AP model and the IS/LM model, a model that was further developed and used in upper-level courses. These models served the purpose of introducing students to Keynesian ideas, but they did far more that that. For most students the textbook models became Keynesian economics, and in doing so replaced other more sophisticated interpretations of what Keynesian economics was about. These simple textbook models helped define the nature of how people understood

Keynesian ideas, providing a framework for macro that in many ways was significantly different than the framework that can be found in Keynes' *General Theory*.

A fourth development that was occurring during this period was a changing view about policy and the ability of government to control the economy. Before the Depression, laissez faire had strong support among economists, not as an implication of theory, but as a precept in the art of economics that combined broader insights from philosophy and political study with economic insights. The Depression challenged that view and led to discussions of the need for planning, more government control, and greater government involvement in the economy. Then came World War II, and Western democratic governments were seen as having saved our economic and political system. The end result was that, after the war, governments in general had a much better reputation in policy, while the market and laissez faire had a much worse reputation.

Economists were not immune to these changing social moods about government and, quite separately from Keynesian economics, they were exploring an expansion of government policies. Thus we had numerous calls from economists of all political persuasions, such as A.C. Pigou and Henry Simons (Hutchison, 1978, Davis, 1968) for activist fiscal policy to pull the economy out of the Depression. Initially these discussions were framed in the loose quantity theory/Say's Law framework of the earlier debates, but with the development of Keynesian models, and the Classical and Keynesian monikers, the policy discussion quickly changed from a discussion in which everyone started from the same quantity theory/Say's Law framework, and talked about nuances of institutions, into a discussion in which there were specific differences among economists based on the assumptions of their models. The nature of the policy differences, which were seen as separating Keynesians and Classicals, evolved over time but whatever the policy differences, they followed from the models that both sides used, which suggests to me that policy views were determining model selection and interpretation rather than the other way around.

Whatever their origins, the combination of Keynesian ideas, the analytic structure of a Walrasian type general equilibrium model, the developing empirical work, and the "Keynesian" multiplier and IS/LM pedagogical models made up an explosive combination, creating a fertile ground for a surface "revolution" in macroeconomics. It offered teachers models to teach; researchers research to do, and empirically minded economists enormous opportunities to apply newly developed statistical techniques. But the combination also presented a problem. Because the models were so closely tied to policy issues, in many ways the science of macro did not progress during this time period, and in some ways regressed, leading people to think they know more than they actually know.

Somehow, Keynesian economists always seemed to come up with models that showed that government intervention was good, and monetarists always seemed to come up with models that showed that government intervention was bad. This left some observers with the sense that many of the real issues that differentiated the various positions were going unstated because those real issues were based on reasoning that was too complicated to capture in the analytic models available to researchers at the time, and the empirical tests of the data were too weak to answer the questions being asked. Somehow, no one in the mainstream of the profession wanted to say the simple truth—the real problems of macroeconomic theory are beyond simple models.[10] One reason it could not say that is that it had given up the Keynesian multi-model method, which could have held that the synthesis models were simply ad hoc engineering models designed as tool to assist economist's judgment and intuition. Syntheses macro economists were never willing to admit that their models were not scientific theoretical models; but were instead ad hoc models that might be useful for macro if one kept a good intuitive understanding of the macroeconomy at the back of one's head.

It was this failing in the synthesis models—the failure of claiming to be more than they were that led to the development of modern macro and the movement away from the syntheses models. Specifically, modern macroeconomics developed as a reaction to the claims of the synthesis models being a reasonable scientific model of the macro economy. What became known as New Classical researchers set out to build a such a scientific model. Undermining the synthesis was easy; they simply pointed out the many inconsistencies and problems with it. They pointed out that forward looking agents would act differently than the agents assumed in the synthesis model, and that expectations of policy changes would change the structural characteristics of the model. These were not new insights; they had been recognized before, but swept under the rug in an attempt to keep the models policy relevant. New Classical economics was not directly worried about model applicability. It was concerned with getting the scientific macro research program back on track.

The insights offered by the New Classical model were not the reason for its adoption. As I stated above, those insights—the advantage of rules, credibility, and the weaknesses of macroeconometric models—had been understood before. The reason it succeeded it that the New Classical research program offered institutional criteria for researcher success. By the 1970s journal articles had become central to economists, allowing for a more technical discussion among smaller subgroups of economists, and more technical models. Similarly, policy issues in macro were conducted by central banks, which had expanded their staffs and were dealing with policy issues on their own. This left the macroeconomists to explore the development of a scientific model.

The rational expectations assumption offered a method of solving models and introducing more technical analytics into the models and thus served the same purpose as

[10] The problems with the synthesis model were pointed out my many nonmainstream economists, but these were quickly left out of the debate. For example, there were Austrian economists Fundamentalist Keynesians, Post Keynesians (with and without hyphens), and coordination Keynesians. These dissenting Keynesians pointed out that the synthesis missed the central elements of Keynes' views. But the same institutional pressures that pushed toward the neoclassical synthesis worked against this group of dissidents. As I discuss in Colander (2004) for a view to develop, it must offer institutional advancement for the holders of that view; it must have dissertations for students to write, articles for assistant professors to publish, and textbook expositions that can spread the seeds of the ideas to students. The dissenters failed on almost all of these criteria. In terms of ideas about how the macro economy operated, which could be studied by a 'scientific researcher," (as economists had come to view themselves), the dissenters had little to say other than that the macro economy was too complicated to model, and that therefore the "neo" models, which assumed away the complications, were simply not adding much insight into the issues.

did the multi-market analysis of Walrasian economics fifty years earlier. Once the assumption and models became accepted, there were numerous variations of the model for researchers to develop. Just like the early advocates of the neosynthesis advanced fifty years earlier advanced with the development of synthesis models, young New Classical professors advanced in the 1980s. As the older synthesis macroeconomics retired, macroeconomics changed, and the teaching of graduate economics became the teaching of variations of the dynamic stochastic general equilibrium model, and the graduate course in macro became a course in introducing the students to the techniques need to solve such models. The specific models used evolved, going through numerous iterations from rational expectations to new classical to real business cycles to the current DSGE synthesis. But by the end of the 1980s, the approach was the only approach learned in graduate macroeconomics.

5.1 Modern Macro Policy and Macroeconomic Theory

The modern DSGE approach has had far less success on the undergraduate pedagogical front and policy front. As I predicted back in the 1980s (Colander, 1988) the synthesis models have held on in undergraduate macro, and there is now a large disconnect between undergraduate macro, where students are taught variations of the Keynesian synthesis model, and graduate macro, where students are told what they learned in undergraduate macro was all wrong and should be forgotten. They are only taught the DSGE model. The reasons why this bifurcation has occurred have to do with the strength of the alternative models—the synthesis model is seriously flawed as a scientific model, but because of those flaws, synthesis models are much more amenable to intuition being integrated back into them. This means that they can be usefully used as a framework to hang reasonable policy discussions onto the model, which is how they are used in undergraduate texts. (Colander, 2004).

My interest in this paper is not with pedagogy, but with what is happening with policy macroeconomics as practiced in central banks. Earlier (Colander and Daane, 1994) Dewey Daane and I observed that central bank policy macroeconomics was only being tangentially affected by the modern macroeconomic revolution and that we expected that limited effect to continue. While at the high-level policy level that remains largely true (For example, macro policy during the 2008 financial crisis reflected Keynesian and synthesis thinking much more than it did DSGE thinking.) as Bussiere and Stracca (2009) and Chari, Kehoe and McGrattan (2009) point out, it now seems to be changing at the research level at central banks. Today the DSGE model has worked its way into higher-level policy discussions. For example, implicit in estimates of the temporal effect fiscal policy being used by some high-level policy makers are Ricardian equivalence assumptions. The rising influence of DSGE thinking at the research level of central banks has been a surprise to me (although the recent financial crisis may slow or even stop that rising influence).

The reason it is a surprise to me is because I saw research policy macroeconomists, such as those at central banks, as facing different incentives than did academic economists. By that I mean that previously, central bank economists had incentives to be expert consumers of the latest macro theory, not expert producers. As I discussed above I saw the scientific revolution in macro as largely driven by academic incentives. I saw policy economists at central banks advancing by providing useful advice, or at least advice that policy makers saw as useful. The different incentives created a real-world reasonableness in central bank economists that was far less likely to exist in pure academic economists. The best of the policy macro economists, such as Robert Solow, David Laidler, or Charles Goodhart, crossed the lines between academia and policy, and, like Keynes, provided a blending of the two. For central bank policy macro economists the academic economist's models were simply used as a rough framework to structure reasoned common sense. The models were considered something to think about, but not to use as the full truth. The mid-level policy advisers/researchers at the central banks were those subset of economists who had the "vigilant observer" skill that Keynes said was quite rare.

I had assumed that since those differing incentives that had worked throughout the neosynthesis period to differentiate academic and central bank economists, would remain, and make central bank economists far less susceptible to the modern theoretical work in macro because those models made so many assumptions to arrive at their results that a "vigilant observer" would not take them seriously for anything other than for their general ideas. I was wrong in that assumption.

I now realize that the central bank reasonableness worked well with the IS/LM synthesis models precisely because the synthesis models maintained a foot in both camps. While this attempt to have it both ways undermined the IS/LM model's scientific value, it left economists entering central banks open to retraining. Central banks would train incoming research economists straight from graduate school in its own variation of the Keynesian method (as opposed to any Keynesian model). That training essentially involved teaching the incoming research economists that while the models they learned in graduate school were useful, they contained only half truths, and that the models they had learned needed to be used with intuition and judgment that was based on institutional knowledge and experience.[11] As the head of research at a major central bank told me in the 1970s, it took central bank research economists about 2-years to "train" incoming economists and separate them from the academic mentality that they had picked up in graduate school. The result of this central bank training was that there was essentially a separate central bank macroeconomics that was different than academic macroeconomics, even in the synthesis period.

As I stated above the reason the academic method and the central bank method could exist simultaneously is because central bank economists faced a different replicator dynamics than did academic economists. Specifically, advancement in central banks did not depend on journal article publication. It depended much more on central bank researcher's ability to pull out relevant insights about policy into briefings and memos. Put another way central bank advancement depended on not only mastery of the technical models, but also on judgment and intuition—precisely the characteristics that Keynes

[11] In many ways, the only way I can understand the success of the synthesis model is as a half-way model that allowed economists to massage the model with different judgments. The model could be shaped to arrive at a reasonable policy conclusion.

said were important for practicing economic policy.[12] I had assumed that this "central bank filter" would continue, and would prevent the DSGE training that students received in graduate school from significantly influencing central bank policy in any direct way.

But I am now seeing that my reliance on the reasonableness of central bank economists to instill the Keynesian method into central bank economists was probably not warranted. What I did not take into account was the possibility that that ascendency of the scientific method in graduate school would lead to a change in the replicator dynamics of central bank research economists. The earlier central bank training approach worked only as long as those trained in the older methods remained in charge of central bank research departments. But over time, that earlier cohort has left the bank, and we now have research departments in central banks where more and more researchers do not seem to have been acclimated in the same way that earlier researchers were. Some central bank policy researchers don't even concentrate their research on monetary policy or macroeconomics generally, but instead worry about publishing in academic venues on a variety of topics that have little to do with macro.

What I am suggesting is happening is that the moderating influence on theory imposed by central banks is decreasing, and that the introduction of the DSGE model is changing the internal replicator dynamics and incentives of central bank economics research staffs. This means that the graduate training of economists is tending to eliminate the Keynesian method not only in academia but also in policy researchers at the central banks.[13] As more scientifically oriented economists, who are only trained in the intricacies of the DSGE model and are not trained in institutions or earlier literature, are coming into the bank, it is harder to retrain them into central bank economists who follow the Keynesian method.

Were advancement in central banks still based primarily on memo writing and briefings, eventually, the new research economists coming in would be converted to the Keynesian method, but the selection mechanism of advancement in central bank research departments seems to be changing. More and more, central banks are starting to judge researchers by their ability to publish papers in academic journals, and less by their overall understanding of the macro economy or their ability to write a coherent policy memo. Whereas before central banks took academically relevant show dogs and turned them into policy-relevant hunting dogs, more and more they are simply changing there advancement policy within central banks to emphasize show dog abilities. When I asked older heads of some central bank research about this change, they agreed that it was

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[12] It isn't only in macro where this intuition and judgment is needed. It is also needed in microeconomics as Little pointed out when he summed up his message of his study of applied micro welfare economics. He wrote: "Economic welfare is a subject in which rigour and refinement are probably worse than useless. Rough theory, or good common sense, is in practice, what we require. It is satisfying, and impressive, that a rigourous logical system, with some apparent reality, should have been set up in the field of the social sciences; but we must not let ourselves be so impressed that we forget that its reality is obviously limited; and that the degree of such reality is a matter of judgement and opinion." (Little 1950, p 279)

[13] Businesses which have a bottom line to consider use macroeconomists in quite different ways; business economists still rely much more on judgement and intuition than do academic economists.

happening, and that is was of concern. But they explained to me that it is the only way that they can attract the top academic economists coming out of graduate school.

Now, I am sure some will argue that the DSGE model is not only a useful model for advancing the science of macroeconomics (and I agree; the DSGE model is a much better scientific model than is the IS/LM model); it is also the more useful model for policy guidance. I find that suggestion hard to swallow for two reasons. The first reason the DSGE model is such a poor guide for policy is the same reason that it is a better foundation for a serious scientific model than is the IS/LM model. It is such a highly abstract model that is so far removed from reality that the thought that it could shed much direct light on reality is almost beyond comprehension. Somehow a model of a representative agent, who is infinitely rational and who faces no model uncertainty, is supposed to shed light of something as complex as the macro economy is on the face of it absurd. It is that absurdity that underlies Solow's "rhetorical swindle" comment at the beginning of this paper. The absurdity is, however, not the model—the model is reasonable; the absurdity is its direct use for guiding policy, rather than just a background information for guiding policy. The DSGE model does pass even a low level common sense hurdle as a guide for policy. Yet, with the development of computer software, such as DYNARE, the DSGE model's influence is spreading.

The second reason is that it does not meet reasonable empirical tests. (If it could be shown that the DSGE model predicted better than alternatives, the fact that it doesn't meet common sense requirements wouldn't matter.) But it doesn't. As Juselius, Johansen, and Franchi (Johansen, S. 2006, Johansen and Juselius. 2006, Juselius, K. and M. Franchi. 2007) have shown, the DSGE models are not being brought to the data in a reasonable way, and that were care is taken in relating the models to the data, claims for their relevance, such as Ireland (2004) vanish.

The fact that younger macroeconomists see the DSGE model as the only methodologically acceptable approach is, in my view, scary.[14] It represents a major movement away from the pragmatic educated common sense that used to be the hallmark of central bank economists. It leads to grossly overstated policy conclusions such as the following made by Chari and Kehoe and McGrattan (2009):[15]

> 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

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[14] Chari et al. (2009) summarize this generally accepted methodological view when they write "an aphorism among macroeconomists today is that if you have a coherent story to propose, you can do it in a suitably elaborate DSGE model."

[15] Individuals make stupid statements all the time, but ideally, one makes them in a conference such as this one first (as I am doing) and then through discussion with one's betters, reviewers, and editors, those stupid statements are transformed into nuanced statements that are more defensible. But Chari and Kehoe's statements made it through all those profession filters and made it into print is AEA journals, without provoking the ire of the mainstream. Thus, my concern about the statement is not with the statements per se, but with the professional elite of the macroeconomics community's response to those statements and others like them. Only Robert Solow made the appropriate response from a Keynesian methodological position.

> considering this question, quantitative general equilibrium models have become the workhorse model, and they turn out to offer surprisingly sharp answers. (p. 9)

Anyone trained in the Keynesian method would be seriously bothered by such a statement. The fact that such statements, and others like it, appeared in the inaugural issue of the American Economic Association’s new *Journal of Macroeconomics*, and, other than from Solow, did not bring about an outcry from the macro policy community is a statement to how far the economics profession has deviated from the Keynesian method.

6.1 Conclusion

Incentives and training matter. Keynes once said that policy makers are all the slaves of some defunct economist. That statement does not go far enough. Economists are themselves slaves to the incentives and training in the system, and thus policy makers are ultimately the slaves of the institutional structure which trains and advances economists.

It is time to change that training. It is strange that economists whose models demonstrate the significant benefits of specialization and division of labor, seem unwilling to admit and incorporate the need for specialization and division of labor in their own field. Somehow, in their training there is a “one training fits all” mentality. They just assume that someone who is great as a producer of scientific models also has the qualities to draw policy inferences from those models. While that may be true for polymaths, it is unlikely to be true for the large majority of economists.

Because of increases in the technical tools of economics, economists can no longer rely on a single type of training. Much more specialization is needed; the training for an economist going into policy economics should differ from the training of someone going into scientific research. Do we really care whether a top policy economist can create a DSGE model or work out the conditions under which a rational expectations equilibrium might exist?

Graduate economic programs today have made preparing scientific researchers as their single goal. Consistent with that goal economic graduate students aren’t taught the limitations of models nor are they taught the need for judgment and intuition when applying abstract models to policy. For a long while that didn’t matter, since policy institutions selected for those macroeconomists who followed the Keynesian method. That filtering process is now disappearing, and the result is that central bank research departments are now edging away from the Keynesian method. That, in my view, is cause for serious concern.

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