

# **How Economists Got It Wrong: A Nuanced Account**

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## **Abstract**

This paper considers how economists failed society by not preparing society to expect and plan for a possible financial crisis. It argues that the story told by Paul Krugman in his recent *NYT Magazine* article was too black and white in that it made it look as if Classical economists who were blinded by the beauty of mathematics, are to blame and that Keynesian economics is the path of the future. This paper takes issue with both those claims. It reviews the evolution of economic thinking from Classical to modern times, and shows the Keynesian/Classical terminology misses many of the nuances of policy discussions. It suggests that the solution for the macroeconomics profession isn't the solution that Krugman suggests it is—to re-embrace Keynes. The solution is to re-embrace the broader Classical economic tradition, and to recognize that Keynes was an important part of that Classical tradition.

Key words; Keynes, Classical, Krugman, macroeconomics, crisis, depression

## **How Economists Got It Wrong: A Nuanced Account**

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Paul Krugman has become the voice of economists for many businessmen, politicians, and lay people. Thus, when he writes an article entitled “How Did Economists Get it So Wrong?” (Krugman, 2009), it’s big news. Krugman is a wonderful writer, and very bright, and there are some parts of the story he tells that are dead on. But there are other parts that, from my viewpoint as an historian of economic thought and an economist watcher, his story is misleading.

The overriding problem with the story Krugman tells is that it’s so black and white. There’s the good guys—the Keynesian gang—and bad guys—the Classical/Chicago gang. That, in my view, is seriously wrong. The real story is one of shades of grey. It’s a story that is full of nuances--a story in which it is hard to tell who are the good guys and who are the bad guys.<sup>1</sup> The real story is one of systemic failure of the large part of the academic economics profession that led them to pretend, and some of them to actually believe, that they understood a complex system that they did not, and still do not, understand. It’s a story of the modern economics profession’s failure to express its ideas and arguments with the humility with which they deserve to be expressed. It’s a story of a profession that has lost the enormous insights of past economists—both Keynesian and Classical.

The real story—the story that Krugman misses—is one of lessons lost. It is a story of many modern economists’ unwillingness to accept the common sense reality that the economy is complex—far too complex to fully understand with the analytical tools that have been, and currently are, available to economists. Earlier economists, both Keynesian and Classical, recognized that, and either avoided using formal models, or were willing to limit the implications they drew from formal models to highly tentative policy precepts, which they applied with educated common sense judgment. They called this application art, not science, because they recognized that formal models did not provide an answer, but only an aid to judgment. As analytic tools have improved, the ways in which models can assist judgment have increased, but we are still a long way from having a meaningful model of the macroeconomic economy that sheds significant light on the workings of the macroeconomy. This means that judgment remains key.

The systemic problem involves academic incentives gone wrong because they give far too little weight to common sense. The problem is twofold. The first is that for academics writing for other academics, common sense is too common, which means even those who have it learn to hide it. Why do academics hide common sense? Because it’s too obvious; it doesn’t lead to journal articles which are how academics advance. The second is that when writing for popular audiences, what sells are black and white stories,

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<sup>1</sup> I use guys, because almost all the participants in the debate have been male; women economists have tended to be more attuned to shades of grey.

and thus general audiences are presented with stories like the one Krugman tells. These black and white stories obscure the nuances and give a quite misleading interpretation to what is actually going on. With that overall introduction let me now consider three specific parts of the story where I believe Krugman gets it wrong in his discussion of the economics profession's failure to warn society about the recent financial crisis..

### **Krugman's Hatchet Job on Classical Economics**

The first part of Krugman's story that is misleading involves the hatchet job he does on Classical economics. The reason that it is misleading is that there are many different versions of Classical economics, so that the general term, Classical, is close to vacuous. There are traditional Classical economists, NeoClassical economists, New Classical economists, and many subgroups of each. Krugman's criticisms of "Classical economics" don't fit most of these groups. Specifically, they don't fit what historians of economic thought mean by Classical—the group of economists writing in the tradition of Adam Smith and John Stuart Mill who dominated English language economics from the 1700s to the 1940s.

Krugman lumps all these various types of Classical economists together. For example, he tells the reader that neoClassical economists were economists who "elaborated on the concepts of their "classical" predecessors" and that the Classical policy view was to "have faith in the market system." Even recognizing the need to simplify for a general audience, these descriptions are so far from the real story that if any of my history of thought students had written them they would have failed the course. (The likely explanation for why Krugman presents such an incorrect potted history is that just about all history of thought classes have been removed from graduate economics education, so Krugman may well have never taken a history of economic thought course.)

The more nuanced story is that Classical economics is fundamentally different from what became known as neoClassical economics, which in turn is fundamentally different from what is called New Classical economics. Classical economics is much broader than either neoClassical or New Classical; it used a fundamentally different methodological approach—an approach that was part of a larger philosophical tradition that has its origins in the Scottish enlightenment. Classical economists accepted that the economy was a complex system, which meant that it couldn't be controlled. Classical economists accepted the reality that there would always be unintended consequences that no one trying to control the complex economic system would be able to predict.

Classical economists were not clueless about financial crises as Krugman implies; they had a sophisticated understanding of how financial crises could occur. If one reads the two best known Classical macro/monetary economists—Henry Thornton's *Paper Credit* (1802) and Walter Bagehot's *Lombard Street* (1877)—one will see their deep understanding of financial policy issues and their understanding of how to deal with the financial crises that they felt were inevitable. Had modern students read their work, they would have had a much better understanding of the recent financial crisis than they got from their core graduate macro course.

Contrary to what Krugman writes, Classical economists did not argue for the market on efficiency grounds; rather they argued for the market on philosophical, practical, and common sense grounds—they saw the market as the least worst alternative for achieving the goals that they believed society felt was important. Those goals of economic policy in which Classical economists directed policy included much more than material welfare, which they saw as only a part of society’s welfare (and a declining part at that). The broader goals that they felt that society was interested in included basic freedoms for individuals and having a fulfilling life. Material welfare contributed to these goals, but was not the end goal. Amartya Sen’s work on capabilities (Sen, 1985) is a continuation of these broader Classical themes—themes that have been lost by a large majority of the modern economics profession.

Contrary to what Krugman states, most Classical economists did not claim that they had the answers to what government should do about policy, and they carefully presented their scientific arguments and models as only one input into policy. As Nassau Senior, one of the first Classical economists to spell out the Classical method, stated, an economist’s “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, pp. 2-3)

Contrary to what Krugman implies, Classical economists did not have a one-dimensional view of policy—they did not believe that the market was perfect or that government involvement was always unjustified. Just take a look at Steven Medema’s *The Hesitant Hand*, (Medema, 2009) or Denis O’Brien’s *The Classical Economists Revisited* (O’Brien, 2004) and you will see the enormous range of views that Classical economists had. As both these authors make clear, these early Classical economists held many different views—some even supported variations of socialism—even as they maintained a general laissez faire philosophy.

For the best Classical economists, the policy of laissez-faire was not a rigid prescription against any government involvement in the economy, but instead a warning to think five or six times before one advocates a policy that had to be implemented by government.<sup>2</sup> Laissez faire was not a theoretically derived precept. Classical economists

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<sup>2</sup> Even J.M. Keynes recognized this. In his “The End of Laissez Faire” (Keynes, 1926) he points out that the best Classical economists did not support a blanket laissez-faire approach. However, their subtle policy views did not get translated down into textbooks. He writes, “The dogma had got hold of the educational machine; it had become a copybook maxim. The political philosophy, which the seventeenth and eighteenth centuries had forged in order to thro down kings and prelates, had been made milk for babes, and had literally entered the nursery.” As evidence that laissez faire in not a fundamental theorem of Classical economics, Keynes cites a leading Classical methodologist, John Elliot Cairnes’ comment (1870) that “The maxim of laissez-faire has no scientific basis whatever, but is at best a mere handy rule of practice.”

did not claim it to be a product of scientific work; in fact, they specifically disavowed any scientific foundation for their laissez faire policy prescription. They arrived at their policy positions using a combination of their understanding of economists' theories, and their educated common sense, which involved their study of history, their sense of how government actually worked. From this study they developed a belief that policy interventions often had unintended and undesirable consequences. When the controller is often controlled by political intrigue, not by genuine concern for the welfare of society, it makes sense to limit the controller's control.

To prevent themselves from overstating the relevance of their arguments from science for policy, Classical methodologists, such as J.N. Keynes (J.M. Keynes' father) (1891) argued that it was necessary to maintain two branches of economics—the pure science of economics, which dealt with theoretical and scientific issues, and the art of economics, which dealt with policy issues. They held that the two branches had fundamentally different methodologies, and different outputs.<sup>3</sup> The pure science of economics' output was theorems and facts, neither of which, as Senior noted in the quotation above, had direct relevance for policy. It was only this pure science of economics in which the methodology was explicitly mathematical. Why mathematical? Because mathematics is the language of pure science; it forces researchers to be far more precise and careful, and at least get the logic of the argument right. In mathematical modeling, one develops theorems. If a logical theorem is to be accepted as a theorem relevant for policy—what classical economists called a policy precept—it would have to be shown to be empirically relevant. Classical economists didn't have the tools or data to empirically test their theorems, which is why the best of them were very modest in their claims for them as being relevant for policy.<sup>4</sup>

For Classical economists, the art of economics, which sometimes went under the name, political economy, had a much broader domain and a much looser methodology.<sup>5</sup> To do political economy one needed a knowledge of history, philosophy, and ethics. In arriving at a policy conclusion the political economist had to integrate all these with economic insights derived from theories and an educated common sense. Classical economists, such as J.S. Mill, were dismissive of abstract theory and models as a guide for

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<sup>3</sup> This need to separate out the two was reiterated by J.M. Keynes' contemporary, Lionel Robbins, whose work supposedly serves as the foundation for modern methodology in economics. (Robbins, 1932, 1981)

<sup>4</sup> As I stated above, Classical economists came in many different varieties. David Ricardo was probably the most important outlier of the position I am suggesting is the Classical tradition, and his attempt to merge theory and policy without empirical evidence acquired the name the Ricardian vice. The high point of Classical theory is found in the writings of John Stuart Mill. The Millian tradition was carried on by later economists such as Alfred Marshall and Lionel Robbins. These economists, writing at the high point of the Classical tradition, were careful about drawing inferences from scientific theory. They may have had strong policy views, but they made it clear that these views were not views that derived from the science of economics.

<sup>5</sup> The term science can be interpreted in many ways. Following J.N. Keynes, I am interpreting it narrowly, and it might be called pure science. Later economic writers distinguished between light-bearing science—pure science undertaken for the sole purpose of understanding, and fruit-bearing science—applied or engineering science—whose primary purpose was policy application. Applied science involved creating policy-relevant models, and then applying those models with judgment—as a rough guide. The models were not meant to provide definitive results.

policy. They saw models as presenting at best half truths (Mill, 1838). As J.N. Keynes (1891, p 83) put it “the art of political economy will have vaguely defined limits and be largely non-economic in character.”<sup>6</sup>

*From Classical to NeoClassical*

The evolution of economics from these broad Classical themes to a narrower set of what came to be called neoClassical themes started in the late 1800s and continued well into the 20<sup>th</sup> century. It happened as the study of economics moved to the university. As that happened, the study of economics changed from being a sidelight, which the people who were called economists did in addition to their real job, to being what people who called themselves economists considered their real job. University-based economists spent all their working time teaching, writing, and thinking about economics, and none of their working time actually taking part in the activities they were describing. As that happened, the metis that comes from living in a system faded in economists’ writing. That institutional change in the structure of the profession fundamentally altered economist’s background knowledge of the economy, and their incentives about what to study and how to express their ideas.

Instead of primarily writing for the broad lay public and emphasizing common sense, as earlier Classical economists had done, economists began to write more for themselves; they became more explicitly mathematical, more interested in theory per se, and less interested in real world issues. As that happened the field of economic narrowed, and the philosophical, historical, and institutional knowledge that was so central to Classical economists’ analysis of policy began to be lost.<sup>7</sup>

As publications became an increasingly important metric for advancement and success, economists began to see themselves more as scientists and less as artists who brought about a wide range of understanding to the table. As that happened, the study of economics became more focused on the pure science of economics. It became more technical and mathematical. It was this change in the institutional structure of the economics profession that led to the movement from Classical to NeoClassical.

This same movement happened in the natural sciences where it led to further advances in science. Unfortunately, that wasn’t the result in economics, and social science generally. I suspect that the reason why is that the social sciences involve a much

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<sup>6</sup> J.M Keynes continued in the Classical tradition, and did not see any particular model as the correct one. 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.” (Keynes, 1938)

<sup>7</sup> The process of change took decades. An economist’s working lifespan is about 40 years, so on average, their work reflects training they received decades earlier. Thus, even today, the economics training of most economists of Robert Solow’s or Paul Samuelson’s vintage reflects that Classical approach.

higher degree of complexity than do the natural sciences. The problem is that the basic units in social science, which economists call agents, are strategic, whereas the basic units of the natural sciences are not. Economics can be thought of as physics with strategic atoms that keep trying to foil any efforts to understand them and bring them under control. Strategic agents complicate formal modeling enormously; they make it impossible to have a perfect model since they increase the number of calculations one would have to make in order to solve the model beyond the calculations that the fastest computer one can hypothesize could process in a finite amount of time.

To even start to deal with formally modeling such a complex system requires a whole new branch of mathematics, game theory, which was only invented in the 1940s by John von Neumann and Oscar Morgenstern (1944). Inevitably, complex systems exhibit path dependence, nested systems, multiple speed variables, sensitive dependence on initial conditions, and other non-linear dynamical properties. This means that at any moment in time, right when you thought you had a result, all hell can break loose. As described in Waldrop's popular book, *Complexity* (Waldrop, 1992), the formal study of economics as a complex system only began in the 1980s with an innovative seminar at the Santa Fe Institute. Formally studying complex systems requires rigorous training in the cutting edge of mathematics and statistics along with a highly creative mind.<sup>8</sup>

#### *Different Branches of NeoClassical Economics*

Economists in the late 1980s and early 1990s did not have the technical expertise, the analytical tools, nor the computing power to even start dealing with these problems, so they, quite reasonably, simplified and studied problems that their analytical techniques could deal with. There were many ways to simplify, and what is called neoClassical economics was actually a multitude of competing approaches. At various times from the 1870s to the 1970s, when neoClassical economics was most prevalent, a variety of different neoClassical approaches dominated the English speaking world.<sup>9</sup>

One branch of neoClassical economics—the Marshallian branch, named after Keynes' mentor, Cambridge economist Alfred Marshall, did not lose its Classical roots; it is best seen as part of the broader Classical tradition. It was the dominant “neoClassical”

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<sup>8</sup> The fact that social science's basic units are people also gives social science an alternative approach to understanding—intuition and empathy with the unit of study. A physicist can't feel what it is like to be an atom. An economist can feel what it is like to be a person (at least sometimes). For Classical economists this alternative approach was central to their analysis. As neoClassical economics evolved and became more mathematical, that “intuition approach” was abandoned as economists strove for precision. It is only now, with the development of behavioral economics that this “intuition and empathy” is returning to modern economics and is being grounded in game theory and experiments.

<sup>9</sup> The group of economists who are described as neoClassical are so varied that the term's usefulness can be questioned. (Colander, 2000) Along with the competition within the neoClassical school, there was competition outside of the neoClassical approach. Two non-neoClassical groups, Historical and Institutionalist economists, carried through the Classical view that a knowledge of history and institutions was important; they argued strongly against an overreliance of economics on formal theory. These groups remained important through the 1950s.

approach in English speaking countries from the 1890s until the 1930s or 40s. Marshallian economics saw economics as a set of tools, not as a completed theory.<sup>10</sup>

A second branch of neoClassical economics—the Walrasian branch, named after French economist, Leon Walras, deviated more significantly from Classical economics; it saw economic theory as providing a unified model of the entire economy. It focused on developing a theory to the exclusion of reality, and is the branch of economics that Krugman rightly criticizes in his argument that economists got lost in the beauty of the mathematics. If Walrasian neoClassical economists had kept in mind that their model was so far from reality that little of practical relevance could be gained from it, it would have been a useful model. Economists using the Walrasian model helped clear up many logical confusions in the Marshallian branch of neoClassical economics.

Many Walrasian neoClassical economists made a fatal mistake that Classical economists had avoided and had strongly warned against. That fatal mistake was that they began directly drawing policy conclusions out of their models and theory. They gave up the strict separation of science and art that Classical economists had maintained. As they did that, they lost the insight that, at best, economic theory was far too simple to shed much light on policy issues in a system as complex as the economy. To the degree that Krugman is criticizing this subgroup, he gets it right, although in my view he overstates the argument.<sup>11</sup>

In conclusion, the larger Classical tradition is definitely not guilty of being blinded by the beauty of mathematics, nor is it guilty of arguing a policy position of “have faith in the market.” So, from a historian of economic thought’s perspective, Krugman is far too hard on Classical economics.

### **Is Keynesian Economics the Future of Macroeconomics?**

This leads us to a second place where Krugman’s story misses some important nuances—his discussion of Keynesian economics. Krugman presents Keynesian

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<sup>10</sup> Marshall retained the Classical vision of economics as a complex system, and in his *Principles*, (1890) he carefully warn readers about the limitations of his models. Unfortunately, the Marshallian sensibility as expressed in *Principles* did not survive, and later textbooks dropped the Marshallian addenda, and focused only on teaching the technical aspects of supply and demand analysis. As those addenda were lost, economists’ conception of economic policy switched from broad Classical “fulfilling life” goals to a single goal—efficiency, by which was interpreted to mean—producing the most goods at the least cost.

<sup>11</sup> The issues are of course more complicated, and there were differences in views even among the subgroup of “Walrasian” economists. Thus, any blanket criticism of even this subgroup is inappropriate; many who worked in the Walrasian framework fully recognized its limitation. For example, it was general equilibrium theorists Gerard Debreu (1974), R. Mantel (1974), and Hugo Sonnenschein (1972, 1973) who formally showed that the Walrasian approach of thinking about the macroeconomy from microfoundations was doomed. They proved that one cannot formally move from an analysis of individuals to an aggregate analysis in any smooth way, and thus the very way in which macroeconomics is conceived needs to be rethought. Alan Kirman’s new book on *Complexity and Macro* (forthcoming) shows how the precision of a general equilibrium theorist’s mind can lead to important advances in understanding. It was another of the developers of general equilibrium theory, Kenneth Arrow, who led the way in establishing the Santa Fe’s economics program that has offered the most likely future of macroeconomic theorizing.



economics as the correct path for economics, and suggests that the future of economics involves a return to Keynes. He writes that “Keynesian economics remains the best framework we have for making sense of recessions and depressions.” Before one can say whether or not Krugman is right in arguing that the Keynesian path is that path that modern macroeconomics should follow, one must decide what Keynesian economics is. That isn’t easy. Keynesian economics means many different things to many different people. The majority of differences of opinion about whether or not we should follow the Keynesian path reflect different conceptions of what the Keynesian path is, more so than they reflect differences of opinion of the path that macroeconomics should follow. So, as was the case with Krugman’s blanket criticism of Classical economics, the issue of whether we should follow the Keynesian path is nowhere near as black and white as Krugman presents it. There are so many different possible meanings of Keynesian economics that suggesting that we should follow the Keynesian path obfuscates more than it clarifies.

As I will explain below, if by Keynesian economics Krugman means the economics of the Classical tradition of which Keynes was an important part, I would totally agree with Krugman—that is the correct path. But if by Keynesian economics, he means what I suspect he means—the Keynesian branch of the neoKeynesian/neoClassical synthesis—which is the branch of macroeconomics that dominated the field from the 1950s to the 1980s, New Classical criticisms of Keynesianism are on much stronger ground.

*A Brief History of Macroeconomic Thought from the 1930s until Today*

To see the problem, it is helpful to look briefly at the history of macro economics from the early adoption in the late 1930s of “Keynesian macro” to its demise starting in the 1980s. Keynes (1936) was writing the *General Theory* in the 1930s--right at the time that economics was changing from the broad-based Classical approach to a narrower Walrasian neoClassical approach that emphasized comparative static mathematical models of multi-market equilibria. Keynes was not part of this Walrasian transformation. He was an economist of the Marshallian variety, which means that he eschewed mathematics. (Marshall is famous for his argument that economists should only use mathematics as a guide to their own reasoning, and should burn the mathematics before presenting their ideas. (Marshall, 1906) Marshall’s brand of neoClassical economics saw economic theory as providing a set of tools that economists could use to solve small microeconomic issues that he felt economists could have something concrete to say about. Marshall did not attempt to use economic theory as a foundation for large macro ideas. He felt that the theory just wasn’t up to the task. Thus, he stayed away from theoretically analyzing issues such as the aggregate efficiency of markets or the total level of employment. He stayed with what he called a partial equilibrium or one-thing-at-a-time approach, which involved looking at issues that he felt his supply-demand framework could shed some light on.

Although Keynes was a student of Marshall, he had little interest in the narrow themes that Marshall’s partial equilibrium supply demand analysis allowed economists to have something to say about. (His colleague, Gerald Shove, is reported to have remarked

that Keynes “never spent the half hour necessary to learn price theory,” by which he meant Marshallian microeconomics). Keynes’ interests were broad based. He was part of the Bloomsbury group and as an undergraduate he wrote a treatise on probability that challenged classical statistical theory. He was well versed in Classical economics, which is not surprising since, as I mentioned above, his father was a leading Classical methodologist. While Keynes was not interested in Marshall’s narrow themes, he maintained a Marshallian approach to thinking about the economy, and did not see models as providing policy answers. Thus, Keynes writes, “The theory of economics....is a method rather than a doctrine, an apparatus of the mind, a technique of thinking which helps its possessor to draw correct conclusions.” (Keynes, 1921)

A second difference between Keynes and Marshall is that Keynes was active in policy debates, and was a highly skilled debater. He was also quite willing to make use of rhetorical arguments to support a policy position that he believed was correct. This led him to allow his followers to blend theory and policy in a way that Classical methodologists had warned against without him objecting as long as they came close to the policies he favored. Thus, to achieve his policy goals, he was willing to sit back and allow his theory to be used as justification of a policy even though the theory wasn’t up to the task. This willingness gave him an advantage over other Classical economists, and accounts for some of the success of his theory. Let me explain.

When Keynes was writing *The General Theory* the Marshallian/Classical tradition was on the decline, and if the success of Keynesian economics had depended on supporters of Keynesian economics adopting that Classical/Keynesian methodology, it is highly unlikely that there ever would have been a Keynesian revolution in macroeconomics. There were a number of reasons why. The first is that while he audaciously called his work *The General Theory*, it was not. It was at best a rough sketch of a theory; which was more vision than model. In *The General Theory* Keynes did not present a full theory or even a model. As modern New Classical economist, Robert Lucas, suggests, the thought that Keynes’ work was an Einsteinian level revolution (as suggested by the title) was “just so much hot air.” It was only Keynes’ audacity and marketing skills the led him to call his book *The General Theory*.

The limitations of Keynes’ work were recognized early on. Contrary to what Krugman implies, Keynes’ *General Theory* was not greeted with major applause from the economics profession. In fact, most economists saw Keynes’ book as severely flawed. For example, reviews by Jacob Viner (1936) and Roy Harrod (1937) were supportive but critical. Keynes’ colleague, Dennis Robertson, who specialized in macroeconomic issues, was dismissive of it. Chicago economist Frank Knight’s comments were more than critical; he wrote the things in it that were new weren’t true, and the things in it that were true weren’t new. Even Alvin Hansen, who later became the face of Keynesian economics in the U.S., gave *The General Theory* what can at best be described as a lukewarm review (Hansen, 1936).

How then could such a flawed book lead to major revolution in economic thinking? The answer is a combination of right place-right time, marketing skills, and the ability of the “Keynesian” classification to morph into whatever fit with the major

changes that were taking place in the profession that were going on simultaneously in the profession. (Those changes included the change from Marshallian to Walrasian, the introduction of econometrics, the movement to a more policy activist sensibility, and the introduction of simple graphical models as central pedagogical devices.) The success of Keynesian economics depended on its abandoning some of its most important insights as well as its Classical methodological roots.

*The General Theory's* primary success and largest initial influence was among graduate students and younger economists who had less training in Classical economic thought, and were not familiar with cutting edge developments that were going on in Classical macro, as explained in my book with Harry Landreth on *The Coming of Keynes to America* (Colander and Landreth, 1996). They read in Keynes what beginning students often search for—seemingly easy answers to difficult questions. They were interested in policy, not fine points of theory or methodology. Faced with the Depression, they wanted a theory that told them that they should do what common sense told them they should do—get the government to undertake policies to end the Depression.

What this suggests is that the *General Theory* was more a marketing and policy success than it was an intellectual success; what was called the Keynesian revolution was a surface revolution about policy sensibilities more than it was a theoretical revolution. Its success was based on strong marketing and being in the right place at the right time.<sup>12</sup> It was not a revolution in scientific understanding.

But even if one limits the discussion to policy, the issues are more complicated, because what we now know as Keynesian policies were not reflective of Keynes' subtle understanding of policy. Consider the following: Keynes's *General Theory* is often associated with the use of monetary and fiscal policy. That association is problematic for two reasons. First, it suggests that Classical economists didn't support such policies when faced with a depression. That's wrong. Despite their hesitancy to support expansionary monetary and fiscal policy, given the severity of the Great Depression, a number of them, Keynes included, supported expansionary monetary and fiscal policies to help get the economy out of it. This support came well before Keynes wrote the *General Theory*.

Classical economists could support such expansionary policies even though theoretically those policies didn't fit their model, because they recognized that they did not have an adequate model of the aggregate economy, and that they would have to deal with the crisis using educated common sense and their knowledge of history, just as earlier Classical economists had done. As one of the strongest opponents of government intervention, William Hutt, reflected, "Once the persistent ignoring of "Classical" precepts has precipitated chaos and insurmountable political obstacles obviously block the way to noninflationary recovery, only a pedant would oppose inflation." (Hutt, 1979, p. 45)

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<sup>12</sup> The recession of 1937 was important in the success of *The General Theory* in the U.S. From 1933 to 1936, the U.S. economy had experienced strong growth, and advocates of alternative policies were losing adherents. With the recession of 1937, that changed, and many more economists were open to the proposition that a revolution in economic thinking was necessary.

The second reason associating Keynesian policies with expansionary monetary and fiscal policy is problematic is that Keynes' method of choosing the model appropriate for the situation, cited in a footnote above, was totally inconsistent with a blanket answer to any policy question. Consider the following: In the *General Theory*, there is no mention of fiscal policy at all. The reality is that Keynes took a much more nuanced view of fiscal policy as is clear by his reaction when one of his young followers, Abba Lerner, argued in favor of a deficit at a Federal Reserve Seminar that the both attended. Keynes responded to Lerner by jumping on him and telling him that he was totally wrong in supporting deficit spending (Colander, 1984).

I recount these instances to point out that Keynes' ideas can only be understood as being the ideas of a Classical economist who was writing in a Classical/Marshallian tradition that saw policy as not flowing from models. That Keynesian methodological nuance, rooted in his Classical methodological approach that models are only guides for thinking and do not lead to definitive policies, was lost by his followers, without Keynes vigorously objecting. By the 1950s Keynesian economics was seen as totally advocating the use of fiscal policy to steer the economy, and Keynesian models were seen as providing a justification for the use of activist monetary and fiscal policy.

Given this history, there is a natural confusion about what Keynesian policy is. One could never figure out what policy position Keynes would take on an issue, and he would change his views often.<sup>13</sup> The reason he could be so changeable in his policy views was that the policies he supported were quite separate from his theories, and as his father had argued, were based largely on non-economic grounds.<sup>14</sup> So, my sense is that if one is following a true Keynesian policy path, it is unclear whether or not one would have supported the recent stimulus package. Were Keynes living today, he may have supported these policies; he may not have, but whatever position he took, he would argue strongly and persuasively for it.

#### *Keynes' Hatchet Job on Classical Economics*

Keynes contributed to making *The General Theory* a marketing success, and leading his followers to draw policies from theory, in the way he wrote it. Rather than present the nuances of Classical thought, and the intricacies of analyzing the macroeconomic problem, Keynes did a hatchet job on early Classical economists, just as Krugman does on Classics in his essay. Such hatchet jobs sell well to the general public, but tend not to advance understanding. By doing such a hatchet job on Classical economics, Keynes helped perpetuate the belief that Classical economists were clueless about depressions, recessions, and unemployment. As I discussed above, that was far from the case.

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<sup>13</sup> It is reported, perhaps apocryphally, that when he was challenged on his tendency to change his views, he retorted: "When the facts change, I change my mind. What do you do?"

<sup>14</sup> At one point Alvin Hansen recalls Abba Lerner asking Keynes: "Mr. Keynes, why don't we forget all this business of fiscal policy, public debt and all those kinds, and have some printing presses?" Keynes replied, "It's the art of statesmanship to tell lies, but they have to be plausible lies." (Hansen in Colander and Landreth, 1996)

Let me give just one example of where Keynes was unfair to Classical economic thought. In *The General Theory*, Keynes centers his criticism of Classical economists on their belief in Say's Law that supply creates its own demand. He argues that Classical economists did not believe that general unemployment could exist, whereas his "theory" could explain general unemployment. The reality is much more nuanced. Consider the following quotation:

In the first place my attention is fixed by the inquiry, so important to the present interests of society: What is the cause of the general glut of all the markets in the world, to which merchandise is incessantly carried to be sold at a loss? What is the reason that in the interior of every state, notwithstanding a desire of action adapted to all the developments of industry, there exists universally a difficulty of finding lucrative employments? And when the cause of this chronic disease is found, by what means is it to be remedied? On these questions depend the tranquility and happiness of nations (Say 1821, p. 2).

As Per Jonsson (1995) has pointed out the person who said this was Classical economist Jean Baptiste Say, the very Say of Say's law, who supposedly didn't believe general unemployment could exist. The reality is that Classical economists knew quite well that unemployment and depressions could exist. In their theoretical discussion they were studying how coordination failures in markets could lead to depressions, and were struggling with a sequence analysis in which they attempted to analyze what happened when one industry affected another, which in turn affected others, creating a feedback loop. They recognized that such feedbacks could lead the aggregate economy astray, but could not formalize precisely how. They recognized that they didn't have the formal analytic tools to capture these feedbacks in a meaningful way. So they did not pretend to have a formal theory of the aggregate economy; it was just too hard a question for them to answer. They did, however, have insights to add in terms of policy some broad general relationships, such as the quantity theory of money and Say's Law, which seemed to hold true, and which would help people avoid fallacious reasoning.<sup>15</sup> (Remember, they separated theory from policy.)

### *The NeoKeynesian Diversion*

The combination of these "Keynesian" models into a Walrasian framework came to be called the NeoClassical/NeoKeynesian synthesis. The model of this synthesis consisted of multiple equations describing actions of actors in different markets—the labor market, the goods market, and the money market, which included no dynamics. In these models researchers would "solve" the model for an equilibrium, and then explore how changes in what they called exogenous variables would lead to a new equilibrium. They then compared the old and new equilibria to find the effect of policy changes.<sup>16</sup>

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<sup>15</sup> For further discussion of these issues, see my essay on the history of macroeconomics in Colander (2006).

<sup>16</sup> In these models, the primary difference between the neoKeynesian and neoClassical versions was the assumption of fixed wages in the Keynesian model, which would allow an unemployment equilibrium to exist, and flexible wages in the neoClassical model which would not allow unemployment. This made the primary difference between neoKeynesian economics and neoClassical economics the assumption of

This model served as a foundation for how macroeconomists were taught about the macroeconomy from the 1950s until the 1980s and it still remains in some macroeconomic textbooks today.

This neoKeynesian/neoClassical synthesis model had serious limitations as a micro-grounded scientific model; its primary advantage was that it seemed to fit the economy in a loose way; it was useful for structuring thoughts, and for pedagogical purposes. But it has serious limitations both for thinking about policy and for thinking about what was really going on in the macroeconomy other than for general guidance. One problem was that to make the models tractable, neoKeynesian/neoClassical models simply ignored the strategic behavior of agents, which is that aspect of economics that makes the formal study of economics so difficult. It pictured the macroeconomy as a system that could be modeled by mechanical laws without taking into account that their agents were strategic, and their strategic actions would undermine any knowable mechanical law as soon as you attempted to undertake policy based on those laws.<sup>17</sup> A second problem was that the model had no analysis of the underlying complex dynamics—somehow something changed and the entire system moved to a new equilibrium. The adoption of these simple models led economists to move away the thinking of complex dynamics as the cause of the depression—dynamics so complex that they could not be modeled—to a comparative static explanation for the depression—wages that did not adjust.<sup>18</sup>

These neoKeynesian models were operationalized with econometrics, which was a newly developing field of economics, making macroeconomics one of the hottest fields of study. These econometric models made the models seem impressive to lay people. Direct policy conclusions were drawn from these macroeconometric models. Here is how much fiscal policy we will need; here is how much monetary policy we will need. This was a totally different use of models than the way Classical economists had approached modeling, and does not fit Keynes' approach. Keynes was not a supporter of macroeconometrics and in his review of an early developer of macroeconometrics, Jan Tinbergen, Keynes (1939) challenged the basic premise of econometrics and had argued that “the method is one neither of discovery nor criticism.”<sup>19</sup> This makes it hard to see the macroeconomic path followed by neoKeynesian economists through the 1970s (the path that New Classicals would strongly challenge as Keynesian) as reasonably being described as Keynesian.

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fixed wages, even though Keynes had emphasized in *The General Theory* that that was not the primary difference.

<sup>17</sup> Pure general equilibrium theorists—which includes many of the best mathematical economists—have long ago abandoned any simplistic notion of the usefulness of the Walrasian model, and also would not be guilty. They are using the model not for policy but to understand the structural issues involved in thinking about the complex system that is our economy.

<sup>18</sup> In Colander (1988, 2006) I have suggested that the reason this happened was because the comparative static models fit the replicator dynamics of the academic profession, which was switching to a focus on articles. Exploring these models allowed young economists to get published and to advance without having a deep knowledge of past ideas that previously had been expected.

<sup>19</sup> This criticism has to be seen in the context of someone writing in 1939, with its highly limited computing power and data.

Why did neoKeynesian economics follow the path that it did? In my view, it had little to do with conscious decisions by researchers that studying these issues would shed most light on macroeconomics. (See Colander, 2006, 2009) Instead it was the unintended consequences of individual macroeconomic researchers following their own self interest, combined with incentives within the academic environment to eschew common sense. It was due to a systemic failure of the economics profession—a failure that remains today.

I present this history to demonstrate the ambiguity in the term Keynesian. In many ways what became known as Keynesian was the antithesis of Keynes. By that I mean that the entire neoKeynesian neoWalrasian approach was quite outside the British Classical tradition of which Keynes was a part. It was a blend of methods and approaches that is best seen as in the Walrasian neoClassical tradition, not the Marshallian Classical tradition of Keynes. This is not to say that the model that developed had no relationship to Keynes' arguments. It maintained an important aspect of Keynes' ideas—specifically that macroeconomic laws could be fundamentally different laws than microeconomic laws. This observation, which is a central identifying feature of complex systems, was a major change in vision, and was a major contribution to economic understanding. Unfortunately, it was a vision that obscured other central aspects of Classical thought.

The reason neoKeynesians followed this route was that in order to develop a formal Keynesian model with distinct macroeconomic laws without basing the analysis in complex dynamics, economists had to assume that those macroeconomics laws were definite, knowable, and unchanging laws, which in principle, could be derived from microeconomic relationships, although they admitted it was not quite clear how they were going to do that.<sup>20</sup> These assumptions of knowable and unchanging laws were very unKeynesian and were certainly unClassical. They went against the basic premise of Classical thought that with strategic agents, the system will be way too complex to derive knowable and unchanging laws. They obscured important elements of the policy debate.

In summary, neoKeynesian and neoClassical economists replaced a messy and imprecise “Classical” explanation of why a depression might occur—an explanation that suggested that the problem lay somewhere in the dynamics of the system that were closely tied with financial issues—that we did not have the mathematical tools to fully model, with a precise model that suggested that the problem lay in the fact that “exogenous variables” were not at the right level and that we could model how those exogenous variables affected the economy through mechanical multipliers and aggregate consumption embedded in a comparative static multi-market equilibrium model.

This is not to say that the models had no purpose. As long as they weren't taken too seriously, and were used with judgment, many of the models were extremely useful as a rough guide to policy issues, and as computational power and data availability advanced they became more useful. They provided the rough and ready guides for thinking about policy that policy makers needed. Had the neoKeynesian/neoClassical

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<sup>20</sup> When I asked Paul Samuelson about this, he stated that “We always assumed that the Keynesian underemployment equilibrium floated on a substructure of administered prices and imperfect competition. I stopped thinking about what was meant by rigid wages and whether you could get the real wage down; I knew it was a good working principle.” (Samuelson in Colander and Landreth, 1996, 160)

models been presented as such—rough engineering models that were useful as one tool in thinking about policy, which is how many practitioners saw and used them, they were a useful advance. But to the degree that they were presented as scientific models, and the only tool one needed—as something that could be used without enormous judgment, they had a negative effect on the practice of economics. That usage made it seem as if neoKeynesians had the answers to macroeconomic policy—add a steering wheel to the economy in terms of an activist monetary and fiscal policy--and that anybody who opposed such policies were dumb fools, who wanted to drive the economy without a steering wheel. Such usage served only a rhetorical function and stifled good policy discussion.

*The Demise of neoKeynesian Economics and the Rise of New Classical Economics*

Starting in the 1960s and for the next fifty years economists learned that Classicals were not quite the fools that Keynes had made them out to be, and that neoKeynesian theories, which young economists, in their quick acceptance of simple theory, had accepted as truth, were far too simple to even come close to meaningfully model the macroeconomy. As the profession learned this, the black and white approach to understanding, which Keynes had used to his advantage, boomeranged on Keynesian economics. Instead of recognizing that there were important and complementary truths in both neoKeynesian and Classical insights, the black/white division was turned on Keynesian economics. The result was that Keynes and the neoKeynesians were treated as outrageously by many later critics as Keynes had treated Classical economists.

I totally agree with Krugman that the simplistic criticism of Keynesian economics that one hears among some macroeconomists today are misplaced. But that does not mean that these modern macroeconomists are stupid, or that the policy views of these Keynesian critics don't warrant serious consideration. The reason NeoKeynesian economics was replaced with New Classical economics in the 1980s was that that neoKeynesian model had serious logical problems. The "New Classical" economics in the tradition of Robert Lucas and Thomas Sargent made an important contribution to macroeconomics by showing the problems with that neoKeynesian model. They pointed out that strategic behavior on the part of individuals would undermine the basic laws, and any mechanistic policy prescriptions. In doing so they brought back important Classical insights.

In pointing these out, however, they made, in my view, two mistakes. The first, relatively minor, mistake is that they claimed that they were arguing against Keynesian economics and Keynesian policies. As I discussed above, that is unfair to Keynes. The second, much more serious, mistake is that same mistake that Keynes made. Like Keynes, they allowed less careful followers to draw policy implications from their models, when they should have pointed out that no model of the economy can be used as a direct guide to policy because the macroeconomy is too complicated to formally model in a satisfactory way. The New Classicals made their model tractable by essentially assuming away all coordination problems, and, hence, like the neoKeynesian models, New Classical models have no analysis of how complex dynamics might feed back on the economy and cause a structural breakdown in the entire economy. Just like Keynes did,



the New Classical model (and its modern dynamic stochastic general equilibrium offspring) cuts the Gordian Knot. It just cuts it in a different way—its proponents get around the strategic behavior of agents problem by creating models that eliminate the need for strategy. It's neat, but not especially helpful in guiding our macroeconomy.<sup>21</sup>

*Putting Keynes' Contribution into Perspective*

I provide this history of macroeconomics not to undermine Keynes' contribution but to put it in perspective. His contribution involved vision more than substance, and that vision was extraordinary. He went beyond earlier Classical economists who were worrying about coordination failures and small feedback effects; he recognized that, combined, these feedback effects could get the economy stuck in a deep rut from which it could not recover without government action. That was a vision that went far beyond previous Classical economists and involved a rethinking of government's role in the economy. He was the first well-known Classical economist to point out that in the aggregate there are just too many coordination problems to expect that the market will always provide the proper coordination.

What Keynes did not provide was a formal macroeconomic theory to explain how those coordination problems in a complex system could lead to a depression. He also did not explain why, generally, the economy worked reasonably well, even when on the face of it, it should not work at all—there should be chaos in a system as complex as an economy.<sup>22</sup> Until economists explain in a formal theory why the economy works, there is no hope of meaningfully explaining why it sometimes stops working. So as a scientific theory of macroeconomics, Keynesian economics failed, and it was that failure that led to what was called Keynesian economics fading away.

What does this history mean for Krugman's arguments? It means that regardless of how much respect one has for Keynes (and I am a strong admirer of Keynes), the reality is that Keynes did not solve the macroeconomic conundrum in terms of either theory or policy. To do that would have required untying the Gordian Knot of complex systems; Keynes simply cut it, and then convinced a group of students to accept the cut rope almost without question, and to believe that what came to be known as Keynesian economics was scientifically correct. That led macroeconomic theory down a dead end alley that blended policy and theory in an unsavory way. By that I mean that somehow, what were called Keynesian theories always seem to lead to policy activism; and what were called Classical theories always seemed to lead to no government involvement. That is hardly a prescription for good science, and it certainly did not reflect the subtle nuanced views of Classical economists or of Keynes. It is, instead, a description of how to muddle serious discussion of both theory and policy.

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<sup>21</sup> The rational expectations assumption which allowed them to do this actually came from a project to integrate process and learning into economics as part of the early work on artificial intelligence. (Colander and Guthrie, 1979).

<sup>22</sup> Economists still haven't fully explained that in an intuitively satisfying way, and it is only complex system theorists who are even trying to do so with their analysis of emergence.

## **Mathematics Is not the Problem**

This leads us to the third place where Krugman’s story loses the nuances—his discussion of mathematics and economics. He writes “the economics profession went astray because economists, as a group, mistook beauty, clad in impressive-looking mathematics, for truth. Until the Great Depression, most economists clung to a vision of capitalism as a perfect or nearly perfect system. . . . Unfortunately, this mathematicized and sanitized vision of the economy led most economists to ignore all the things that can go wrong.” The problem with Krugman’s argument about mathematics is twofold.

First it suggests that Classical economists supported the market because of some mathematical model. As should be clear from the above discussion, that’s incorrect. Classical economists, and the Marshallian branch of neoClassical economists, of which later “free market” economists such as Milton Friedman was a part, fought against the mathematicization of economics. Their goal in doing that was to keep the deeper philosophical insights that Classical economists held in the forefront of the analysis. It was less the Classical or even neoClassical macroeconomists who led macroeconomics down the mathematical path, and more the followers of Keynes, who developed simple mathematical macro models that they called scientific models, and then turned them directly to policy through the use of macroeconomic models. Used this way, models were no longer engines of discovery and analysis, as they were for Marshall, but pseudoscientific justifications for the policy that the economist supported.<sup>23</sup>

The second problem with the argument is that it seems to cast mathematics and mathematical models as the villains. That’s wrong. The problem is not, and never was, the mathematical models. The true cutting edge mathematical economists are pushing new frontiers, and have been doing so for decades. They deserve praise, not the contempt that Krugman seems to convey (Colander, Holt and Rosser, 2004). The future of the science of economics is mathematics—mathematical models that will likely be much more complicated than the current models.

The problem is not the mathematics, the problem is the way the mathematical models are used. If mathematical models are used with an understanding of their enormous limitations, they are useful.<sup>24</sup> If they are used to help support, to or come to a policy that the developer of the model believes is the correct policy, then the models are probably worse than useless, because they give pseudoscientific support for policies and thereby stifle relevant debate about the policies.

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<sup>23</sup> Krugman is very much a part of that “simple model” tradition; he made his reputation within the economics creating simple mathematical models that came to policy conclusions that non-mathematical economists had long been arguing for. So for him to be arguing against mathematics seems quite disingenuous at best.

<sup>24</sup> Many of the economists who developed the Walrasian general equilibrium model, such as Kenneth Arrow or Frank Hahn, did not use it to directly guide policy, but instead to clear up their understanding of the logic of the model. As general equilibrium theorist, Frank Hahn, stated back in the 1970s; the general equilibrium model was worth exploring but was not as useful as they had hoped. (Hahn, 1981) So they moved on—precisely what you would expect scientists would do.

An actual unified scientific model of the macroeconomy—which takes into account its full complexity—does not exist. In fact, we are so far from having such a model that if being a science depended on such a unified model, economics would have no scientific branch. But it doesn't; scientific study is not dependent on having a unified model. All it requires is a critical approach, a focus on understanding for the sake of understanding, and a commitment to reporting the results of that work with the modesty that our lack of understanding demands. Lionel Robbins made this quite clear in his essay on the appropriate method for economists to follow. He writes:

What precision economists can claim at this stage is largely a sham precision. In the present state of knowledge, the man who can claim for economic science much exactitude is a quack. The problems of human motive we have to analyze with the “vast amorphous phantoms” of psychology at their back, are nebulous enough in all conscience. It is not because we believe that our science is exact that we wish to exclude ethics from our analysis, but because we wish to confine our investigations to a subject about which positive statement of any kind is conceivable. (Robbins, 1927, 176)

As Robbins makes clear, the blame should be directed at the way the mathematical models are used, and the belief that the models could be used without judgment. As science, the macroeconomic models that Keynes' followers developed were unsupportable. Those neoKeynesian models could have (and should have) been supported as engineering models that were back-of-the-envelope guides to thinking about very complicated issues. Had they been presented as such, and had applied macroeconomists focused their energies on dealing with the nuances of policy that were where the true differences lie, the history of macro would have been quite different.

Despite the current problems in economics, progress is being made and my prediction is that a future historian of economic thought will eventually look back and see the most recent development in macroeconomics as highly positive. She will explain the “New Classical/Real Business cycle interlude” as the inevitable result of that macro pseudo science inherent in the “Walrasian neoClassical/neoKeynesian interlude” which had become dominant in the 1960s and 70s. At least New Classical economics got the logic of the model clear and dealt with the central problem of strategic agents, albeit in a way that shed little light on the macroeconomy.

This future historian will also point out that eventually, macroeconomics returned to its Classical roots, modernized to take into account the enormous advances in analytic and computational power that changed the way empirical data could be integrated with the mathematics of complex systems involving interacting strategic agents, to provide insight into the economy. She will describe a highly mathematical science of macroeconomics--one of beautiful and elegant models, that a small group of complex system theorists have developed. She will point out how macroeconomic practitioners keep the insights of these models in the back of their minds to frame their thinking about the macroeconomic economy, but how they use a wide variety of less elegant, but more useful engineering models combined with educated common sense and judgment for specific policy guidance.

## **Conclusion: The Systemic Failure of the Economics Profession**

Why is such a description not a description of modern macroeconomics? The reason is that there is a systemic problem in the economics profession. We have been unwilling to admit that the economy is far too complex to be captured by any unified model. True, when pushed in private discussions we recognize that complexity, and admit it among ourselves, but we don't add the warning labels to our models that our lack of understanding would demand. We pretend we understand more than we do. I suspect the reason why we do that is that most of us are not good enough mathematicians to make a real contribution to the analysis of complex systems, and after you've said that the economy is complex, unless you are an ultra-mathematician, there's not much more to say in terms of pure science. Only recently are analytic and computational tools being developed which allow even ultramathematicians to say something useful. I have predicted that ultimately this work will lead to a new Post Walrasian macroeconomics (Colander, 2006) that is neither Keynesian nor Classical. It is simply macroeconomics.

If we give up the pretense of full scientific understanding, we free the majority of economists to return to that branch of economics that Classical economists saw as the most important branch for policy—the art of economics. This art of economics is the applied policy branch of economics. It is the branch where most of us normal academics can make contributions.

What's important in this applied branch is common sense, an ability to understand (but not necessarily to produce) high theory, a knowledge of institutions, and a knowledge of history. The systemic failure in economics is that policy economists are not trained in any of these and they're not even trained that these issues are important. Because they are not, what you get are black and white positions, and unnuanced discussions such as Krugman's, when what would further understanding is an economics profession trained to recognize and highlight the shades of grey. Once one recognizes those shades, one recognizes that government policy actions have both costs and benefits.

To use expansionary monetary and fiscal policy may help expand the economy temporarily, but it will likely stop the larger necessary structural changes from taking place. When Chicago economists point this out, they are making a valid point. They deserve better than the ridicule Krugman subjects them to when he writes that “comments for Chicago economists are the product of a Dark Age of macroeconomics in which hard-won knowledge has been forgotten.” (Krugman, 2009) Similarly what is called the Keynesian position deserves better than to be described as reflecting “schlock economics” that are “based on discredited “fairy tales.” That verbal word slinging reminds me of unruly seven year olds. Don't we deserve better from tenured professors of economics at major universities?

To get better, we need to change the structure of the profession. We need more economists trained in the subtlety of policy issues and institutional realities. We need far fewer economists trained as macroeconomic theorists (but those few who are trained as macroeconomic theorists need much stronger training in mathematics) and far more economists trained to consume macroeconomic theory. These applied macroeconomists

need to know how macroeconomic and financial institutions really work, and they need to know the history of economic ideas and of the economy.

So the failure of economics is not a failure of Classical or Keynesian economists. Instead, as we argued in Colander et al (2008), it is a systemic failure in the entire economic profession. It follows that the solution for the macroeconomics profession isn't the solution that Krugman suggests it is—to re-embrace Keynes. The solution is to re-embrace the broader Classical economic tradition, and to recognize that Keynes was an important part of that Classical tradition.

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