**Draft of Written Testimony of David Colander**
**Submitted to the Congress of the United States, House Science and Technology Committee**
**July 20th, 2010**

Mr. Chairman and members of the committee: I thank you for the opportunity to testify. My name is David Colander. I am the Christian A. Johnson Distinguished Professor of Economics at Middlebury College. I have written or edited over forty books, including a top-selling principles of economics textbook, and 150 articles on various aspects of economics. I was invited to speak because I am an economist watcher who has written extensively on the economics profession and its foibles, and specifically, how those foibles played a role in economists' failure to adequately warn society about the recent financial crisis. I have been asked to expand on a couple of proposals I made for NSF in a hearing a year and a half ago.

## Introduction

I'm known in the economics profession as the Economics Court Jester because I am the person who says what everyone knows, but which everyone in polite company knows better than to say. As the court jester, I see it as appropriate to start my testimony with a variation of a well-known joke. It begins with a Congressman walking home late at night; he notices an economist searching under a lamppost for his keys. Recognizing that the economist is a potential voter, he stops to help. After searching a while without luck he asks the economist where he lost his keys. The economist points far off into the dark abyss. The Congressman asks, incredulously, "Then why the heck are you searching here?" To which the economist responds—"This is where the light is."

Critics of economists like this joke because it nicely captures economic theorists' tendency to be, what critics consider, overly mathematical and technical in their research. Searching where the light is, on the surface, is clearly a stupid strategy; the obvious place to search is where you lost the keys.

That, in my view, is the wrong lesson to take from this joke. I would argue that for pure scientific economic research, the "searching where the light is" strategy is far from stupid. The reason is that the subject matter of social science is highly complex—far more complex than the subject matter of natural science. It is as if the social science policy keys are lost in the equivalent of almost total darkness, and you have no idea where in the darkness you lost them. In such a situation, where else but in the light, can you reasonably search in a scientific way?

What is stupid, however, is if the scientist thinks he is going to find the keys under the lamppost. Searching where the light is only makes good sense if the goal of the search is *not to find the keys*, but rather to understand the topography of the illuminated land, and how that lighted topography relates to the topography in the dark where the keys are lost. In the long run, such knowledge is extraordinarily helpful in the practical search for the keys out in the dark, but it is only helpful where the topography that the people find when they search in the dark matches the topography of the lighted area being studied.

What I'm arguing is that it is most useful to think of the search for the social science policy keys as a two-part search, each of which requires a quite different set of skills and knowledge set. Pure scientific research—the type of research the NSF is currently designed to support—involves searches of the entire illuminated domain, even those regions only dimly lit. It also involves building new lamps and lampposts to expand the topography that one can formally search. This is pure research; it is highly technical; it incorporates the latest advances in mathematical and statistical technology. Put simply, it is rocket (social) science that is concerned with understanding for the sake of understanding. Trying to draw direct practical policy conclusions from models developed in this theoretical search is generally a distraction to these scientific searchers.

The policy search is a search in the dark, where one thinks one has lost the keys. This policy search requires a practical sense of real-world institutions, a comprehensive knowledge of past literature, and familiarity with history. While this search requires a knowledge of what the cutting edge scientific research is telling researchers about illuminated topography, the knowledge required is a consumer's knowledge of that research, not a producer's knowledge.

**How Economists Failed Society**

In my testimony last year, I argued that the economics profession failed society in the recent financial crisis in two ways. First, it failed society because it over-researched a particular version of the dynamic stochastic general equilibrium (DSGE) model that happened to have an easy, formal solution. That DSGE model attracted economists as a light attracts moths. Almost all mainstream macroeconomic researchers were searching the same lighted area. While the initial idea was neat, and an advance, much of the later research was essentially dotting i's and crossing t's of that original DSGE macro model. What that meant was that macroeconomists were not imaginatively exploring the multitude of complex models that could have, and should have, been explored. Far too small a topography of the illuminated area was studied.

What macroeconomic scientific researchers should have been working on is a multiple set of models that incorporated purposeful forward looking agents that the DSGE model was meant to add on to previous models. This would have included models with multiple equilibria, high level agent interdependence, varying degrees of information processing capacity, true uncertainty rather than risk, and non-linear dynamics, all of which seem intuitively central in macroeconomic issues, and which we have the analytical tools to begin dealing with.[1] Combined, these models would have revealed that just about anything could happen in the macro-economy.

The second way in which the economics profession failed society was by letting policy makers believe, and sometimes assuring policy makers, that the topography of the real-world matched the topography of the highly simplified DSGE models, even though it was obvious to anyone with a modicum of institutional knowledge and educated common sense that the topography of the DSGE model and the topography of the real-world

[1] I have called this research into more complex economic models, Post Walrasian macroeconomics, and have spelled out what is involved in Colander, 1996, 2006.)

macro economy generally were no way near a close match. It was as if you were telling someone that studying tic-tac toe models can guide one in playing 20th dimensional chess.

Economists aren't stupid, and the macro economists working on DSGE models are among the brightest. What then accounts for these really bright people continuing working on that simple DSGE model, and implying to policy makers that it was a useful policy model? The answer goes back to the lamppost joke. If the economist had answered honestly, he would have explained that he was searching for the keys in one place under the lamppost because that is where the research money was. In order to get funding, he or she had to appear to be looking for the keys in his or her research. Funders of economic research wanted policy answers from the models, not wild abstract research that concluded with the statement that their model has little to no direct implications for policy.

Classical economists, and followers of Classical economic methodology, which included economists up through Lionel Robbins (See Colander, 2009), maintained a strict separation between pure scientific research, which was designed to be as objective as possible, and which developed theorems and facts, and applied policy research, which involved integrating the models developed in science to real world issues.[2] That separation helped keep economists in their role as scientific economists out of policy. It did not prevent them from talking about policy. It simply required them to make it clear that they were not speaking with the certitude of science, but rather in their role as an economic statesman. An economic statesman has a well-tuned educated common sense and can subject results from models to a smell test that relates the topography illuminated by the model to the topography of the real world. Some scientific researchers made good statesmen; they had the expertise and training to be great statesmen as well as great scientists. John Maynard. Keynes, Frederick Hayek, and Paul Samuelson come to mind. Others did not; Abba Lerner and Gerard Debreu come to mind.[3]

The need to separate out policy from scientific research in social science is due to the complexity of the economic problem. Once one allows for all the complexities of intelligent interaction of forward looking agents and the paucity of data to choose among

[2] Nassau Senior, the first Classical economist to write on method put the argument starkly. He writes. "(the 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." (Senior 1836: 2-3)

[3] The fact that they were not great statesmen did not mean that they were not wonderful scientists. Gerard Debreu was clear about his work having no direct policy relevance; he did not try to play the role of policy statesman. Abba Lerner was not, which lead Keynes to remark about Lerner "He is very learned and has an acute and subtle mind. But it is not easy to get him to take a broad view of a problem and he is apt to lack judgment and intuition, so that, if there is any fault in his logic, there is nothing to prevent it from leading him to preposterous conclusions." (Keynes, 1935: 113) There are also economists who were great statesmen, but not great scientists. Ben Stein comes to mind.

models, it is impossible to avoid judgments when relating models to policy. Unfortunately, what Lionel Robbins said in the 1930s remains true today, “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.” (Robbins, 1927, 176)

**Why Economists Failed Society**

One of J.M. Keynes’s most famous quotes, which economists like to repeat, highlights the power of academic economists. He writes, “the ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed, the world is ruled by little else. Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back.” (Keynes, 1936: 135) What that quotation misses is the circularity of the idea generating process. The ideas of economists and political philosophers do not appear out of nowhere. Ideas that succeed are those that develop in the then existing institutional structure. The reality is that academic economists, who believe themselves quite exempt from any practical influence, are in fact guided by an incentive structure created by some now defunct politicians and administrators.

Bringing the issue home to this committee, what I am saying is that you will become the defunct politicians and administrators of the future. Your role in guiding research is pivotal in the future of science and society. So, when economists fail, it means that your predecessors have failed. What I mean by this is that when, over drinks, I have pushed macroeconomic researchers on why they focused on the DSGE model, and why they implied, or at least allowed others to believe, that it had policy relevance beyond what could reasonably be given to it, they responded that that was what they believed the National Science Foundation, and other research support providers, wanted.

That view of what funding agencies wanted fits my sense of the macroeconomic research funding environment of the last thirty years. During that time the NSF and other research funding institutions strongly supported DSGE research, and were far less likely to fund more esoteric macroeconomic research. Ultimately, successful researchers follow the money and provide what funders want, even if those funders want the impossible. If you tell funders it is impossible, you do not stay in the research game.

One would think that competition in ideas would lead to the stronger ideas winning out. Unfortunately, because the macroeconomy is so complex, macro theory is, of necessity, highly speculative, and it is almost impossible to tell a priori what the strongest ideas are. The macro economics profession is just too small and too oligopolistic to have workable competition among the diversity of ideas. Most top researchers are located at a small number of interrelated and inbred schools. This highly oligopolistic nature of the scientific economics profession tends to reinforce one approach rather than foster an environment in which a variety of approaches can flourish. When scientific models are judged by their current policy relevance, if a model seems temporarily to be matching what policy makers are finding in the dark, it can become built in and its premature adoption as "the model" can preclude the study of other models. That is what happened with what economists called the "great moderation," and the premature acceptance of the DSGE model.

The problem is not in the individual researchers; most, if pushed, fully recognized the limitations of formal models for policy.[4] But others, such as Chari and Kehoe and McGrattan (2009) were willing to draw direct policy conclusions from the DSGE models while arguing that other models which did not come the same conclusion their model came to, was inappropriate to use. The write "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."[5]

Individual economists make stupid statements all the time (Being a court jester, I make more than most.) But ideally, one makes them in a conference or in private, and then through discussion with other economists, reviewers, and editors, those stupid statements are transformed into nuanced statements that are more defensible. But Chari, Kehoe and McGratten's statement made it through all those profession filters and made it into print in an American Economic Association journal, without provoking the ire of the mainstream. Thus, my concern about the statement is not with the statement per se (and other like it), but with the professional elite of the macroeconomics community's response to those statements and others like them. Why didn't they enter in and say, "No, you can't make such a strong policy conclusion about real world policy from the model you are working with?"

I believe the reason why it happened involves a structural problem in the economics profession. The current incentives facing young economic researchers lead them to both focus on models that downplay the complexity of the economy and overemphasize the direct policy implications of their models.

**How the Economics Profession Can do Better.**

The reason I am testifying today is that I believe the NSF can take the lead in changing this current institutional incentive structure by implementing two structural changes in the NSF program funding economics. These structural changes would provide economists with more appropriate incentives, and I will end my testimony by outlining those proposals..

*Include a wider range of peers in peer review*

[4] For example, Robert Lucas one of the originators of the DSGE modeling approach, in some of his writings, was quite explicit about its policy limitations long before the crisis. He writes "there's a residue of things they (DSGE models) don't let us think about. They don't let us think about the U.S. experience in the 1930's or about financial crises and their real consequences in Asian and Latin America; they don't let us think very well about Japan in the 1990's." (Lucas, 2004) Even earlier (Klamer, 1983) Lucas stated that if he were appointed to the Council of Economic Advisors, he would resign.

[5]

The first structural change is a proposal to make diversity of the reviewer pool an explicit goal of the reviewing process of NSF grants to the social sciences. This would involve consciously including heterodox and other dissenting economists as part of the peer reviewer pool as well as including reviewers outside of economics. One might include physicists, mathematicians, statisticians, and even business and governmental representatives along with economists, on these reviewing committees for economic proposals. Such a broader peer review process would likely encourage research on a much wider range of models, promote more creative work, and provide a common sense feedback from real world researchers about whether the topography of the models matches the topography of the real world the models are designed to illuminate.

*Increase the number of researchers trained to interpret models*

The second structural change is a proposal to increase the number of researchers explicitly trained in interpreting and relating models to the real world, and reduce the number of researchers developing abstract formal models. This can be done by explicitly providing research grants to interpret, rather than develop, models. In a sense, what I am suggesting is an applied science division of the National Science Foundation's social science component. This division would fund work on the appropriateness of models being developed for the real world.

This applied science division would see applied research as true "applied research" not as "econometric research." It would not be highly technical and would involve a quite different set of skills than currently required by the standard scientific research. It would require researchers who had a solid consumer's knowledge of economic theory and econometrics, but not necessarily a producer's knowledge. In addition, it would require a knowledge of institutions, methodology, previous literature, and a sensibility about how the system works—a sensibility that would likely have been gained from discussions with real-world practitioners, or better yet, from having actually worked in the area.

The skills involved in interpreting models are skills that currently are not taught in graduate economics programs, but they are the skills that underlie judgment and common sense. By providing NSF grants for this interpretative work, the NSF would encourage the development of a group of economists who specialize in interpreting models and applying models to the real world. The development of such a group would go a long way towards placing the necessary warning labels on models, making it less likely that fiascos, such as the recent financial crisis would happen again.

## Bibliography

Chari, V.V., P Kehoe and E. McGrattan 2009. "New Keynesian Models: Not Yet Useful for Policy Analysis" *Macroeconomics,* (AEA) vol 1. No. 1

Colander, David, 1996. (ed.) *Beyond Microfoundations: Post Walrasian Economics*, Cambridge, UK. Cambridge University Press.

Colander, David, 2006. (ed.) *Post Walrasian Macroeconomics: Beyond the Dynamic Stochastic General Equilibrium Model*, Cambridge, UK. Cambridge University Press.

Colander, David, 2009. “What was ‘It’ that Robbins was Defining?” *Journal of the History of Economic Thought*, December. Vol. 31:4, 437-448.

Keynes, John Maynard, 1935. Letter to Lionel Robbins 1st May, 1935. Reprinted in Colander, David and Harry Landreth, 1997. *The Coming of Keynesianism to America*, Cheltenham, England. Edward Elgar.

Keynes, John Maynard, 1936. *The General Theory of Employment, Interest and Money*, London. Macmillan.

Klamer, Arjo, 1984. *Conversations with Economists: New Classical Economists and Opponents Speak Out on the Current Controversy in Macroeconomics*, Lanham, Maryland. Rowman and Littlefield Publishers.

Lucas, Robert, 2004. “My Keynesian Education” in M. De Vroey and K. Hoover (eds.) *The IS'LM Model: Its rise, Fall and Strange Persistence, Annual Supplement to Vol. 36 of History of Political Economy*, Durham, NC, Duke University Press

Robbins, Lionel, 1927. “Mr. Hawtrey on the Scope of Economics” *Economica*. Vol. 7, 172-178.

Senior, Nassau William. 1836. (1951). *An Outline of the Science of Political Economy*, New York. Augustus M. Kelly Pubs.