Written Testimony of David Colander
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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 (letting available analytic technology guide one's technical research), 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—arguably far more complex than the subject matter of most natural sciences. 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—ideally involves searches of the entire illuminated domain, even those regions only dimly lit. It should also involve 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 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, familiarity with history, and a well-tuned sense of nuance. 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 a tractable formal solution, whereas more realistic models that incorporated purposeful forward looking agents were formally unsolvable. That tractable DSGE model attracted macro 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, and far too little focus was given to whether the topography of the model matched the topography of the real world problems.

What macroeconomic scientific researchers more appropriately could have been working on is a multiple set of models that incorporated purposeful forward looking agents. 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 complex models are just that—

[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.)

complex, and just about anything could happen in the macro-economy. This knowledge that just about anything could happen in various models would have warned society to be prepared for possible crises, and suggested that society should develop a strategy and triage policies to deal with possible crises. In other words, it would have revealed that, at best, the DSGE models were of only limited direct policy relevance, since by changing the assumptions of the model slightly, one would change the policy recommendation of the model. The economics profession didn't warn society about the limitations of its DSGE models.

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 macro economy generally were no way near a close match. Telling policy makers that existing DSGE models could guide policy makers in their search in the dark was equivalent to telling someone that studying tic-tac toe models can guide him or her in playing 20th dimensional chess. Too strong reliance by policy makers on DSGE models and reasoning led those policy makers searching out there in the dark to think that they could crawl in the dark without concern, only to discover there was a cliff there that they fell off, pulling the US economy with it.

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 simple versions of the DSGE model, and implying to policy makers that these simple versions were useful policy models? 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.

[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

It did not prevent them from talking about, or taking positions on, policy. It simply required them to make it clear that, when they did so, they were not speaking with the certitude of economic science, but rather in their role as an economic statesman. The reason this distinction is important is that being a good scientist does not necessarily make one a good statesman. Being an economic statesman requires a different set of skills than being an economic scientist. An economic statesman needs a well-tuned educated common sense. He or she should be able to subject the results of models to a "sensibility 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 policy 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 economic policy problems. Once one allows for all the complexities of interaction of forward looking purposeful agents and the paucity of data to choose among models, it is impossible to avoid judgments when relating models to policy. Unfortunately, what Lionel Robbins said in the 1920s 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 this 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.

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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] Gerard Debreu is a great economic scientist who is clear about his work having no direct policy relevance; he did not try to play the role of policy statesman. Abba Lerner was less clear about keeping the two roles separate. This 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 whom I consider great statesmen, but not great scientists. Herbert Stein and Charles Goodhart come to mind.

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 alternative macroeconomic research. The process became self-fulfilling, and ultimately, all macro researchers knew that to get funding you needed to accept the DSGE modeling approach, and draw policy conclusions from that DSGE model in your research. Ultimately, successful researchers follow the money and provide what funders want, even if those funders want the impossible. If you told funders it is impossible, you did 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 supporters of a wide variety of ideas and alternative models. 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.

Most researchers; if pushed, fully recognize the limitations of formal models for policy.[4] But more and more macroeconomists are willing to draw strong policy conclusions from their DSGE model, and hold them regardless of what the empirical evidence and common sense might tell them. Some of the most outspoken advocates of this approach are Vandarajan Chari, Patrick Kehoe and Ellen McGrattan. They admit that the DSGE model does not fit the data, but state that a model neither “can nor should fit most aspects of the data” (Chari, Kehoe and McGratten, 2009, pg 243). Despite their agreement that their model does not fit the data, they are willing to draw strong policy

[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.

implications from it. For example, they write "discretionary policy making has only costs and no benefits, so that if government policymakers can be made to commit to a policy rule, society should make them do so." (Chari and Kehoe, 2006; pg 7, 8)

While they slightly qualify this strong conclusion slightly later on, and agree that unforeseen events should allow breaking of the rule, they provide no method of deciding what qualifies as an unforeseen event, nor do they explain how the possibility of unforeseen events might have affected the agent's decisions in their DSGE model, and hence affected the conclusions of their model. Specifying how agents react to unexpected events in uncertain environments where true uncertainty, not just risk, exists is hard. It requires what Robert Shiller and George Akerlof call an animal spirits model; the DSGE model does not deal with animal spirits.

Let's say that the US had followed their policy advice against any discretionary policy, and had set a specific monetary policy rule that had not taken into account the possibility of financial collapse. That fixed rule could have totally tied the hands of the Fed, and the US economy today would likely be in a depression.

Relating this discussion back to the initial searching in the light metaphor, the really difficult problem is not developing models; they really difficult policy problem is relating models to real world events.[5] The DSGE model is most appropriate for a relatively smooth terrain. When the terrain out in the dark where policy actually is done is full of mountains and cliffs, relying on DSGE model to guide policy, even if that DSGE model has been massaged to make it seem to fit the terrain, can lead us off a cliff, as it did in the recent crisis. My point is a simply one: Models can, and should, be used in policy, but they should be used with judgment and common sense.

DSGE supporter's primary argument for using the DSGE model over all other models is based on their model having what they call micro foundations. As we discuss in Colander, et al. (2008) *what they call micro foundations are totally ad hoc micro foundations.* As almost all scientists, expect macroeconomic scientists, fully recognize, when dealing with complex systems such as the economy, macro behavior cannot be derived from a consideration of the behavior of the components taken in isolation. Interaction matters, and unless one has a model that captures the full range of agent interaction, with full inter-agent feedbacks, one does not have an acceptable micro foundation to a macro model. Economists are now working on gaining insight into such interactive micro foundations using computer generated agent-based models. These agent based models can come to quite different conclusions about policy than DSGE models,

[5] Keynes recognized this. He wrote (1938) "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."

which calls into question any policy conclusion coming from DSGE models that do not account for agent interaction.

If one gives up the purely aesthetic micro foundations argument for DSGE models, the conclusion one arrives at is that none of the DSGE models are ready to be used directly in policy making. The reality is that given the complexity of the economy and lack of formal statistical evidence leading us to conclude that any particular model is definitely best on empirical grounds, policy must remain a matter of judgment about which reasonable economists may disagree.

How the Economics Profession Can Do Better.

I believe the reason why the macroeconomics profession has arrived in the situation it has reflects serious structural problems in the economics profession and in the incentives that researchers face. The current incentives facing young economic researchers lead them to both focus on abstract models that downplay the complexity of the economy while overemphasizing the direct policy implications of their abstract models.

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

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 what are often called heterodox and other dissenting economists as part of the peer reviewer pool as well as including reviewers outside of economics. Along with economists on these reviewer panels for economic proposals one might include physicists, mathematicians, statisticians, and individuals with business and governmental real world experience. 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. 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.

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