

**Testimony before the  
Committee on Science and Technology, Subcommittee on Investigations and  
Oversight, U.S. House of Representatives**

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Mr Chairman, Ranking member and Honorable members of the committee. It is an honor and a privilege to testify before you. The purpose of this hearing, as I understand it, is to examine the promise and the limits of modern macroeconomic theory in providing advice for policy. Much of modern macroeconomic theory is centered around a class of models known as Dynamic Stochastic General Equilibrium models. The vast majority of modern academic developers of such models use a common methodology and share a common language. These economists are exceptionally diverse in their politics and the policy conclusions from their models often differ. That is, modern macroeconomics is a very big tent. Academic macroeconomists do, however, use very similar tools. In the wake of the recent financial crisis, some critics have argued that we need a radical new agenda in macroeconomics. They have advanced the view that since DSGE models failed to predict the depth and magnitude of the recession, we need to adopt models that are very different from the ones currently in use. I will argue that variants of existing models need to be improved in important ways but should be done so within the shared methodology of modern macroeconomics. I also argue that critics who argue for a new agenda have made a number of factual assertions about modern macroeconomic models that are, quite simply, wrong. Let me begin with the assertions made by the critics.

## **1. Unfounded, Uninformed Criticisms**

Critics have made the following factually incorrect statements.

1. The models do not have a role for financial factors and imperfections in financial markets.
2. The models have no role for sticky prices or wages.

3. The models have no role for unemployment or for frictions in labor markets
4. Equilibria in the models are efficient (in the sense that as a society we cannot do better than the outcomes of the models.)
5. The models have a "representative agent."
6. The models have no role for crises.
7. Mainstream Macroeconomic Theory did not warn us of the bad side effects of deregulating financial markets.

Take the first claim of the critics. I reformulate it more baldly. Here is the claim. "Academic Macroeconomists who are interested in policy and active in policymaking write down models with no role for financial market frictions." Nothing could be further from the truth. The career and accomplishments of Ben Bernanke show why this claim is simply wrong. Ben was one of the most prominent academic macroeconomists well before he came to Washington, where he played a key role in the years before the crisis and after. What is Bernanke famous for as an academic? Starting with a famous paper in the *American Economic Review* in 1983, through his work with Mark Gertler in 1989 and subsequently also with Simon Gilchrist in 1999, he has devoted his career to incorporating financial frictions in quantitative dynamic stochastic general equilibrium models. The famous Bernanke Gertler paper was published two decades before the current crisis. It was an attempt to understand the greatest economic crisis in U.S. history: the Great Depression. Was Ben a heterodox, bit player on the sidelines of modern macroeconomics? Absolutely not. He was chairman of the Princeton economics department, a leading center of modern macroeconomics. It is simply laughable to assert that our models do not have crises driven by financial frictions. The first claim is false.

Take the second claim: The models do not have sticky prices or wages! Where do people get this idea? The baseline DSGE model used by a large number of central banks is the so-called New Keynesian model. The central features of this model are sticky wages and prices. The second claim is false.

Take the third claim: The models have no unemployed people. The baseline model used in the analysis of labor markets in modern macroeconomics is the Mortensen-Pissarides model. The main point of this model is to focus on the dynamics of unemployment. It is specifically a model in which labor markets are beset with frictions. The third claim is false.

Take the fourth claim: Equilibrium outcomes are efficient in these models and the models have no role for policy. The Society of Economic Dynamics held its meetings in Montreal earlier this month and about 440 papers were presented. This society typically has a large number of members who develop DSGE models.. About 50 dealt specifically with policy in macroeconomic models. In *none* of these 50 papers was the best policy by the government to do nothing and simply get out of the way. The critics should get out of their ivory towers and attend the SED conference, Minnesota macro week and the meetings of the National Bureau of Economic Research's Economic Fluctuations and Growth group

Take the fifth claim: The models have a representative agent: Please look at the seminal work by Rao Aiyagari, Per Krusell and Tony Smith, Tim Kehoe and David Levine, Victor Rios Rull, Nobu Kiyotaki and John Moore. All of them are (or were, in the case of Rao, who is unfortunately deceased) prominent macroeconomists at leading departments and much of their work is explicitly about models without representative agents. The fifth claim is false.

Take the sixth claim: The models have no crises: An important branch of modern

macro is international macroeconomics. A huge fraction of this literature led by Tim Kehoe at Minnesota and Guillermo Calvo at Columbia has explicitly focused on financial crises. In terms of domestic macro, Lee Ohanian and Harold Cole explicitly attempt to develop DSGE models of the Great Depression. The sixth claim is false.

Take the sixth claim: Macroeconomic Theory did not warn us of the bad side effects of deregulating financial markets. In 1979, Kareken and Wallace at Minnesota pointed that deregulated financial markets with explicit deposit insurance or implicit government guarantees would lead to an orgy of risk taking. Gary Stern, President of the Minneapolis Fed, inspired by Kareken and Wallace and other researchers at Minnesota and elsewhere wrote a book titled "Too Big to Fail" which laid out specific proposals to regulate banks and financial markets. The seventh claim is false.

So, what are DSGE models and is all well in DSGE land?

## **2. The Uses and Limits of DSGE Models**

I begin with a simple message about all models: Models are purposeful simplifications that serve as guides to the real world, they are not the real world.

This message comes from understanding that policymaking and policy advice necessarily must use models. Policymakers need to understand the rough quantitative magnitudes of the key tradeoffs and they need to understand the economic forces that drive the tradeoffs. A hugely complicated model that no one understands cannot convey an understanding of the key tradeoffs. Large models simply have too many moving parts. A macroeconomic model of monetary policy will surely leave out the Cotton Exchange in Minneapolis! By construction, a model is an abstraction which incorporates features of the real world thought important to

answer the policy question at hand and leaves out details unlikely to affect the answer much. Abstracting from irrelevant detail is essential given scarce computational resources, not to mention the limits of the human mind in absorbing detail! Criticizing the model just because it leaves out some detail is not just silly, it is a sure fire indicator of a critic who has never actually written down a model.

DSGE models in macro do share common features. All of them make sure that they are consistent with the National Income and Product Accounts. That is, things must add up. All of them lay out clearly how people make decisions. All of them are explicit about the constraints imposed by nature, the structure of markets and available information on choices to households, firms and the government. From this perspective DSGE land is a very big tent. Also from this perspective, there is no other game in town. A useful aphorism in macroeconomics is: "If you have an interesting and coherent story to tell, you can tell it in a DSGE model. If you cannot, your story is incoherent."

It is true that the vast majority model individual actors as purposeful agents who do not lightly forgo profit opportunities if they can profitably exploit the opportunities given their constraints. There is nothing explicitly in DSGE modeling that excludes the possibility that we can think of individuals as little behavioral automatons who follow fixed decision rules and routinely leave \$1,000 bills on the sidewalk. The traditional modeling style is certainly that people make the best decisions they can, given their constraints and their information. The advantage of the traditional modeling procedure is that it imposes discipline on the modeler. Give me the freedom to make up decision rules based on dubious evidence from psychology labs in which the subjects are college sophomores and I can explain pretty much anything. The problem is that my dubious model will surely give the wrong answer to any

interesting policy question.

Thomas Sargent, a distinguished macroeconomist has written a number of papers modeling agents as learning about the economy over time in otherwise conventional DSGE models. Nothing in the discipline or structure of the methodology forces one to use conventional rational expectations as the only way of modeling belief formation. DSGE land is, indeed, very welcoming to innovations.

DSGE models in macro are designed to answer quantitative questions. What would be the effect on GDP of changing tax rates on capital income by 10 percentage points forever and raising labor tax rates to make up for the revenue? What would be the consequences of a monetary policy which raised the Federal Funds Rate by 10 basis points if the stock market goes up by 1 per cent? Answering the first question requires in part pinning down elasticities of intertemporal substitution in consumption for households and intertemporal substitution in consumption for production of firms. We pin down these parameters using historical time series and cross sectional evidence. A variety of econometric methods, estimation, calibration and the like are used to ensure that the model is consistent with key features of the data. This methodology often implies that the models are not well suited to analyze extremely rare events. But then I know of no method that is well suited for this purpose. Answering the second question requires developing quantitative models of stock market fluctuations.

All is not, however, well in DSGE land. For example, we do not have a satisfactory model to analyze the kinds of regulation of the financial markets recently legislated by Congress. We do not fully understand the sources of the various shocks that buffet the economy over the business cycle. We do not know what would happen if we required banks to hold T Bills to back all their deposits. So, how should policy makers use advice from

DSGE models. I would suggest that they should do so in exactly the way that central bank policy makers use the advice that their research departments give from such models. It is one ingredient, and a very useful ingredient, in policy making. It is a useful ingredient because it offers a disciplined way of reasoning through the quantitative importance of various economic forces. The reason that they do not rely exclusively on such models is because they understand that the point of the models is to make a point or teach a lesson, not to make policy in real time. As such the models are guides to the real world but they are not the real world.

Clearly DSGE models failed to predict the recent financial crisis. More precisely, they failed to emphasize the risks to which the economy was exposed in the period before the crisis. Was this failure because we did not have the right tools in our toolbox or because we did not know how to use our existing tools? I will argue that we had all the ingredients to see the problem. Macroeconomists who focus on the economies of the rest of the world have long understood the need to model financial crises and have actively been developing such models. They have understood this need because many countries in the rest of the world have been buffeted by financial crises. An important ingredient in understanding our crisis is to understand why financial intermediaries like banks have incentives to take on excessive risks. At a theoretical level, since Kareken and Wallace's work in the late 1970s, we have understood that with deposit insurance or the prospects of government bailouts, private actors have strong incentives to take on excessive risk.

Why then did our models of the U.S. economy fail to incorporate the insights from the study of other countries or the theoretical insights from the literature on deposit insurance? I offer three reasons. First, all useful models must be consistent with key features of the



historical data. The history of U.S. economic performance since World War II is remarkable because economic fluctuations have been relatively small and have not been dominated by severe fluctuations in financial markets to the extent seen in the recent crisis. A focus on U.S. historical performance leads modelers to develop models in which severe financial crises are the exception, not the norm. The obvious implication for academics is that we need to ensure that our models are consistent not just with U.S. experience but the experience of countries in the rest of the world.

The second reason is that we deemphasized the insights of the theoretical literature on the perverse effects of government bailouts because understanding these effects requires that we impute even more rationality and foresight to economic agents than we currently impute. The theoretical insight from the literature on deposit insurance is that debt holders must rationally see that they will be protected in the event of crises. They then have limited incentives to charge higher prices for risk taking. Stockholders then have strong incentives to reward managers of financial intermediaries to take on excessive risk. Whenever I lay out this argument, many distinguished economists have dismissed them because they are skeptical that financial market participants are that sensitive to bailout prospects. The lesson of the recent crisis is that financial markets are far smarter than economists credited them to be. The lesson for academics is that we should be skeptical of those who would argue that people are not very smart and those who would argue that imposing irrationality on market actors is a useful modeling device.

The third reason is that, as a society, we have devoted far too little by way of resources to modern macroeconomics. We have too few people working on modern macroeconomics, we have too few students and we devote too little in the way of other resources to this area.

I would argue that the United States devotes shamefully little to economic research. For example, the NSF's budget for economics is a pitiful \$27 million out of which \$2.6 million goes to the worthwhile activity of supporting the Panel Study on Income Dynamics. Twenty five million dollars for an activity that is deemed fundamentally important by the people of the United States? Even if it does seem like special interest pleading, I would argue that if we want to prevent the next big crisis, the only way is to devote substantially more resources to modern macroeconomics so that we can attract the best minds across the world to the study and development of mainstream macroeconomics.

The recent crisis has raised, correctly, the question of how best to improve modern macroeconomic theory. I have argued we need more of it. After all, when the AIDS crisis hit, we did not turn over medical research to acupuncturists. In the wake of the oil spill in the Gulf of Mexico, should we stop using mathematical models of oil pressure?

The best way of using the power in the modeling style of modern macroeconomics is to devote more resources to it, not to pursue elusive chimera dreamt up in remote corners of the profession.