

**Testimony before the
Committee on Science and Technology, Subcommittee on Investigations and
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*The views expressed herein are those of the author and not necessarily those of the Federal Reserve Bank of Minneapolis or the Federal Reserve System.

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. In this testimony, I will make three major points. First, I will argue that macroeconomics has made huge progress, especially in the last 25 years or so. Second, I will address why our models failed to see the recent crisis coming and how our research in the future must change so that we can forestall such crises. Third, I will argue that macroeconomic research is severely underfunded and that devoting greater resources to macroeconomic research will have huge social benefits.

1. Progress since the early 1980s

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.

All the interesting policy questions involve understanding how people make decisions over time and how they handle uncertainty. All must deal with the effects on the whole economy. So, any interesting model must be a dynamic stochastic general equilibrium model. From this perspective, there is no other game in town. Modern macroeconomic models, often called DSGE models in macro share common additional 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. The only alternatives are models in which the modeler does not clearly spell out how people make decisions. Why should we prefer obfuscation to clarity? My description of the style of modern macroeconomics makes it clear that modern macroeconomists use a common language to formulate their ideas and the style allows for substantial disagreement on the substance of the ideas. 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."

What progress have we made in modern macro? State of the art models in, say, 1982, had a representative agent, no role for unemployment, no role for financial factors, no sticky prices or sticky wages, no role for crises and no role for government. What do modern macroeconomic models look like?

The models have all kinds of heterogeneity in behavior and decisions. This heterogene-

ity arises because people's objectives differ, they differ by age, by information, by the history of their past experiences. 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. Any claim that modern macro is dominated by representative agent models is wrong.

In terms of unemployment, 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.

In terms of a role for financial factors, the career and accomplishments of Ben Bernanke show that mainstream academics have been intensively interested in financial factors. 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. Others, including Nobu Kiyotaki, Hugo Hopenhayn and Tom Cooley have dramatically improved our understanding of financial factors. 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. Mainstream macroeconomic models do have crises driven by financial frictions. Any assertion to the

contrary is false.

In terms of sticky prices and wages, the baseline DSGE model used by the European Central Bank, the Federal Reserve and by other central banks is the so-called New Keynesian model. The central features of this model are sticky wages and prices.

In terms of financial 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.

In terms of a role for government, let me use papers presented at the recent meetings of The Society of Economic Dynamics held in Montreal earlier this month as an example of the changes in macroeconomic modeling. 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. Critics who assert otherwise 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. Also in terms of a role for the government, macroeconomic theorists have long warned 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.

Such improvements have made it possible for us to understand macroeconomic forces much better. In spite of our difficulties in conducting monetary policy during the recent crisis, I would argue that, in general, the conduct of monetary policy has been much better over the last two decades across the world than over the preceding two decades. We have much better models for analyzing the consequences of fundamental changes to the tax system, improved models to think of pension reform, and better models to analyze the challenges of health care reform. Obviously, we need to improve on these models, but we are getting closer to an era of policymaking informed by a clearer understanding of the quantitative consequences of alternative policies and the key tradeoffs that must be made in formulating policy.

So, what are DSGE models? Is all well in DSGE land. Most important, why did we not see the crisis coming?

2. The Uses and Limits of DSGE Models

A common criticism of macroeconomic theory is that the actors in our models are typically rational and forward looking. In the vast majority of our models, individual actors are 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. His style of modeling imposes considerable discipline on the way people learn. Nothing in the 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.

Other criticisms fail to appreciate the extent to which historical data plays, and should play, a central role in developing models. To see this role, note that 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? 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. A second tool we had was our understanding of how policy affects risk taking incentives. 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. Excessive risk taking played a central role in the recent crisis.

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? Out of that 25 million dollars, my best estimate is that only about 10 per cent goes to macroeconomics. Compare \$2.5 million to an overall NSF budget of \$6 billion or to the federal government support of basic research of roughly \$30 billion. I should emphasize that, in my judgment, the NSF's peer review process in economics is exceptionally fair and thoughtful. Expanding resources to the NSF's economics program will surely result in much better economic research and will result in very little waste. 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 to do so 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? Rather than pursuing elusive chimera dreamt up in remote corners of the profession, the best way of using

the power in the modeling style of modern macroeconomics is to devote more resources to it.