

BUILDING A SCIENCE OF ECONOMICS FOR THE REAL WORLD

HEARING BEFORE THE SUBCOMMITTEE ON INVESTIGATIONS AND OVERSIGHT COMMITTEE ON SCIENCE AND TECHNOLOGY HOUSE OF REPRESENTATIVES ONE HUNDRED ELEVENTH CONGRESS

SECOND SESSION

JULY 20, 2010

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**BUILDING A SCIENCE OF ECONOMICS FOR
THE REAL WORLD**

TUESDAY, JULY 20, 2010

HOUSE OF REPRESENTATIVES,
SUBCOMMITTEE ON INVESTIGATIONS AND OVERSIGHT,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
Washington, DC.

The Subcommittee met, pursuant to call, at 10:07 a.m., in Room 2318 of the Rayburn House Office Building, Hon. Brad Miller [Chairman of the Subcommittee] presiding.

Subcommittee on Investigations and Oversight

Hearing on

Building a Science of Economics for the Real World

Tuesday, July 20, 2010
10 a.m. – 12 p.m.
2318 Rayburn House Office Building

Witness List:

Dr. Robert M. Solow
Professor Emeritus
Massachusetts Institute of Technology

Dr. Sidney G. Winter
Deloitte and Touche Professor Emeritus of Management
The Wharton School of the University of Pennsylvania

Dr. Scott E. Page
Leonid Hurwicz Collegiate Professor of Complex Systems, Political Science, and Economics
University of Michigan

Dr. David C. Colander
Christian A. Johnson Distinguished Professor of Economics
Middlebury College

Dr. V.V. Chari
Paul W. Frenzel Land Grant Professor of Liberal Arts
University of Minnesota

HEARING CHARTER

**COMMITTEE ON SCIENCE AND TECHNOLOGY
SUBCOMMITTEE ON INVESTIGATIONS AND OVERSIGHT
U.S. HOUSE OF REPRESENTATIVES**

**Building a Science of Economics
for the Real World**

TUESDAY, JULY 20, 2010
10:00 A.M.—12:00 P.M.

2318 RAYBURN HOUSE OFFICE BUILDING

Purpose

The Subcommittee on Investigations and Oversight will hold a hearing on July 20, 2010, to examine the promise and limits of modern macroeconomic theory in light of the current economic crisis. The Subcommittee has previously looked at how the global financial meltdown of 2008 may have been caused or abetted by financial risk models, many of which are rooted in the same assumptions upon which today's mainstream macroeconomic models are based.¹ But the insights of economics, a field that aspires to be a science and for which the National Science Foundation (NSF) is the major funding resource in the Federal Government, shape far more than what takes place on Wall Street. Economic analysis is used to inform virtually every aspect of domestic policy. If the generally accepted economic models inclined the Nation's policy makers to dismiss the notion that a crisis was possible, and then led them toward measures that may have been less than optimal in addressing it, it seems appropriate to ask why the economics profession cannot provide better policy guidance. Further, in an effort to improve the quality of economic science, should the Federal Government consider supporting new avenues of research through the NSF?

Background

The implosion of the subprime mortgage market came as almost a total surprise to most mainstream economists. Five weeks after the investment house Lehman Brothers had filed for bankruptcy protection, former Federal Reserve Board Chairman Alan Greenspan called the financial crisis “much broader than anything [he] could have imagined.”² The chief steward of the U.S. economy from 1987 to 2006 said he was in a state of “shocked disbelief” because he had “found a flaw in the model that [he] perceived [to be] the critical functioning structure that defines how the world works.”³ Adherence to this model had prevented him from envisioning a critical eventuality: that the “modern risk management paradigm,” seen by Greenspan as “a critical pillar to market competition and free markets,” could “break down.”⁴

Greenspan's crumbled “intellectual edifice” depends on the “efficient market hypothesis” and the assumptions that underlie it.⁵ This hypothesis holds that the price of a financial asset traded on an exchange must indicate its true value because the market's efficiency is such that the price at any given moment reflects all pertinent information about the asset.⁶ It assumes that those trading on the market are considered to have rational expectations, which means that each possesses all available information about the market—indeed, all available information about the world—

¹Hearing of the Subcommittee on Investigations and Oversight of the House Committee on Science and Technology on “The Risks of Financial Modeling: VaR and the Economic Meltdown,” September 10, 2009, serial no. 111–48.

²Hearing of the House Committee on Oversight and Government Reform, “The Financial Crisis and the Role of Federal Regulators,” Oct. 23, 2008, preliminary transcript, p. 16, <http://oversight.house.gov/images/stories/documents/20081024163819.pdf> (last visited on July 14, 2010).

³*Ibid.*, p. 37.

⁴*Ibid.*, p. 18 and p. 34 respectively.

⁵*Ibid.*, p. 18.

⁶This assumption, it will be noted, would rule out the possibility of a price bubble on the exchange. The Subcommittee held a hearing on asset valuation issues in the wake of the Wall Street meltdown and the subsequent rescue packages. That hearing, held May 19, 2009, was titled “The Science of Insolvency,” serial no. 111–27.

and makes optimal use of it. The basis for the efficient market hypothesis, the “rational expectations hypothesis,” is a standard feature of modern macroeconomic models, which are concerned with the overall economy and its most important forces: growth, unemployment, inflation, monetary and fiscal policy, and the business cycle. “Whether we are talking about models of financial markets or of the real economy, our models are based on the same fundamental building blocks,” writes the economist Alan Kirman.⁷

The dominant macro model has for some time been the Dynamic Stochastic General Equilibrium model, or DSGE, whose name points to some of its outstanding characteristics. “General” indicates that the model includes all markets in the economy. “Equilibrium” points to the assumptions that supply and demand balance out rapidly and unfailingly, and that competition reigns in markets that are undisturbed by shortages, surpluses, or involuntary unemployment. “Dynamic” means that the model looks at the economy over time rather than at an isolated moment. “Stochastic” corresponds to a specific type of manageable randomness built into the model that allows for unexpected events, such as oil shocks or technological changes, but assumes that the model’s agents can assign a correct mathematical probability to such events, thereby making them insurable. Events to which one cannot assign a probability, and that are thus truly uncertain, are ruled out.

The agents populating DSGE models, functioning as individuals or firms, are endowed with a kind of clairvoyance. Immortal, they see to the end of time and are aware of anything that might possibly ever occur, as well as the likelihood of its occurring; their decisions are always instantaneous yet never in error, and no decision depends on a previous decision or influences a subsequent decision. Also assumed in the core DSGE model is that all agents of the same type—that is, individuals or firms—have identical needs and identical tastes, which, as “optimizers,” they pursue with unbounded self-interest and full knowledge of what they want are. By employing what is called the “representative agent” and assigning it these standardized features, the DSGE model excludes from the model economy almost all consequential diversity and uncertainty—characteristics that in many ways make the actual economy what it is. The DSGE universe makes no distinction between system equilibrium, in which balancing agent-level disequilibrium forces maintains the macroeconomy in equilibrium, and full agent equilibrium, in which every individual in the economy is in equilibrium. In so doing, it assumes away phenomena that are commonplace in the economy: involuntary unemployment and the failure of prices or wages to adjust instantaneously to changes in the relation of supply and demand. These phenomena are seen as exceptional and call for special explanation.

To what extent is this model, a highly theoretical construct that appears to bear little resemblance to everyday life, used in shaping policy that affects people and events in the real world? Prominent economists disagree. As long as a decade ago, John Taylor stated that it had migrated beyond the walls of the academy: “[A]t the practical level, a common view of macroeconomics is now pervasive in policy research projects at universities and central banks around the world. This view evolved gradually since the rational expectations revolution of the 1970s and has solidified during the 1990s. It differs from past views, and it explains the growth and fluctuations of the modern economy; it can thus be said to represent a modern view of macroeconomics.”⁸ In 2006 V.V. Chari and Patrick Kehoe, academic economists who are advisers to the Federal Reserve Bank of Minneapolis, echoed Taylor’s claim in an article titled “Modern Macroeconomics in Practice: How Theory is Shaping Policy.”⁹

Similarly, Michael Woodford argued in 2008 that there had been a convergence in the macro models used in the academic and policy spheres. He cited a number of central banks in the industrialized world that were using “fully coherent DSGE models reflecting the current methodological consensus,” adding that, in the cases of Canada and New Zealand, “these were not mere research projects, but models routinely used for practical policy deliberations.”¹⁰ The Federal Reserve Board’s main policy model, FRB/US, was developed before the recent trend toward DSGE,

⁷ Alan Kirman, “The Economic Crisis is a Crisis for Economic Theory,” February 2010 version, p. 2, http://www.econ.ed.ac.uk/papers/A_Kirman.pdf (last visited on July 14, 2010).

⁸ John B. Taylor, “Teaching Modern Macroeconomics at the Principles Level,” p. 1, from a speech delivered Jan. 7, 2000, <http://citeseerx.ist.psu.edu/viewdoc/summary?doi=10.1.1.88.7891> (last visited on July 14, 2010).

⁹ *Journal of Economic Perspectives*, Vol. 20, No. 4 (Fall 2006), Pp. 3–28.

¹⁰ Michael Woodford, “Convergence in Macroeconomics: Elements of the New Synthesis,” p. 17, from a speech delivered on Jan. 4, 2008, <http://www.aeaweb.org/articles.php?doi=10.1257/mac.1.1.267> (last visited on July 14, 2010).

but the Fed had “departed sharply from [its] previous generation” of models and had incorporated numerous assumptions and features consistent with DSGE.¹¹

A different view of the influence of the DSGE model outside academia has been put forward by Gregory Mankiw, who was chairman of the President’s Council of Economic Advisers from 2003 to 2005. “The sad truth is that macroeconomic research of the past three decades has had only minor impact on the practical analysis of monetary or fiscal policy,” he wrote in 2006. Still, despite this apparent expression of regret, he added: “The fact that modern macroeconomic research is not widely used in practical policymaking is *prima facie* evidence that it is of little use for this purpose.”¹²

What, then, are the opportunities in the U.S. for realistic macroeconomic policy guidance at this precarious time in the history of the national economy? Kirman, who is among the critics of modern macro models, suggests: “If the DSGE proponents have got it right, then they should be able to explain why their models do not allow for the possibility of a crisis of the sort that we are currently facing. Indeed this applies to all macroeconomic models, for if major crises are a recurrent feature of the economy then our models should incorporate this possibility.”¹³

Questions

Today’s troubled economic landscape is overflowing with ready tests of any model’s relevance to the real world.

- Last month’s G20 summit in Toronto produced a broad policy consensus behind “austerity” plans designed to reduce public debt. Practically speaking, that means governments made commitments to slash their public spending. The recovery is still shaky, and the possibility of a double-dip recession looms on the horizon. What might be the consequences of cutting government spending now? How can we determine when austerity policies make economic sense?
- The basic unemployment rate in the United States has been hovering at just below ten percent. Adding in the long-term unemployed who have become too discouraged to continue looking for work, as well as those who are working part time but would like to work full time, pushes the percentage of unemployed above 16 percent.¹⁴ Yet not so long ago the consensus figure among economists for the U.S. “natural rate of unemployment” was stable at between four and five percent. How do economists explain this high and lingering unemployment rate? What can and should be done about it?
- It has been suggested that one reason so many are staying unemployed is that they are lazy and enjoy receiving unemployment benefits. What can economics tell us about whether unemployment benefits have a large perverse effect of increasing the unemployment rate? If that is so, why was the “natural rate” of unemployment thought to be closer to four percent just a few years ago?
- Japan has been stuck in a deflationary spiral for almost 20 years. Relatively high unemployment, weak productivity gains and slack demand appear to have become permanent features of its economy. Some observers point to signs that a similar condition could await the United States. How do macroeconomists explain Japan’s lingering deflationary situation? Is the U.S. in danger of falling into a similar trap, and what might be done to avoid it?
- The mortgage housing bubble that expanded throughout the first years of this century was anything but inconspicuous. Why weren’t more economists able to identify it and to recognize its potential for doing broad damage to the U.S. and world economies? If economics cannot currently identify emerging conditions that could threaten the Nation’s economic well-being, what kind of work do we need to fund to receive such insights.

Policy makers wrestle with these issues every day. Does the current state of economic research offer reliable, robust answers? Is the reigning macroeconomic model trustworthy for policy-making purposes? If not, should the government consider

¹¹*Ibid.*, p. 16.

¹²Gregory Mankiw, “The Macroeconomist as Scientist and Engineer,” May 2006, p. 19, http://www.economics.harvard.edu/files/faculty/40_Macroeconomist_as_Scientist.pdfm (last visited on July 14, 2010).

¹³Kirman, *op. cit.*, p. 2.

¹⁴U.S. Bureau of Labor Statistics, Household Data Table A–15 “Alternative measures of labor underutilization,” <http://data.bls.gov/cgi-bin/print.pl/news.release/empst.t15.htm> (last visited on July 15, 2010).

funding different kinds of research that may provide more useful insights to real economic outcomes?

Witnesses

Dr. Robert M. Solow, *Professor Emeritus, Department of Economics, MIT*

Dr. Sidney G. Winter, *Deloitte and Touche Professor Emeritus of Management, The Wharton School of the University of Pennsylvania*

Dr. Scott E. Page, *Leonid Hurwicz Collegiate Professor of Complex Systems, Political Science, and Economics, University of Michigan*

Dr. David C. Colander, *Christian A. Johnson Distinguished Professor of Economics, Middlebury College*

Dr. V.V. Chari, *Paul W. Frenzel Land Grant Professor of Liberal Arts, University of Minnesota*

Chairman MILLER. This hearing will now come to order.

Good morning, and welcome to today's hearing entitled "*Building a Science of Economics for the Real World*." I know that economists must think that politicians are impossible to please. Harry Truman complained that he wanted a one-handed economist, and now we are complaining that we got very confident, unequivocal economic advice in the last decade or so but that one-handed advice proved to be wrong.

Unemployment is hovering at just under ten percent, more than 16 percent when you include the folks who have given up looking for work or who are working part time when they really want a full-time job. Banks have cash but aren't lending. The Federal Reserve can't lower interest rates any more without paying banks to take the money. There is worried talk of a deflationary spiral like the one that has dogged Japan for almost two decades now, and arguments about whether it is better to stimulate the economy or cut the deficit appear backed more by ideology—almost theology—gut feeling and election-year politics than by any evidence and honest analysis.

It would be great to have some reliable guidance to lead us out of this mess, but what we thought was authoritative guidance failed to see the mess coming and may actually have helped create the mess to begin with. Expert models of finance and the economy led to risk-taking at our largest financial firms and failed to warn leading economic policymakers that doom lurked in the housing market.

Because our experts' way of looking at the economy left them blind to the crisis that was building, we were unprepared to deal with the crisis. A few weeks after Lehman Brothers went bust, former Fed Chairman Alan Greenspan, the steward of our economy during the 20 years that culminated in the housing bubble, told our colleagues on the House Oversight and Government Reform Committee that his reaction to the financial crisis was "shocked disbelief." He had "found a flaw," as he put it, "in the model that [he] perceived [to be] the critical functioning structure that defines how the world works."

Greenspan's fallen model of the market shares many assumptions with the model that is favored today from academe to the world's central banks. The macroeconomic model is called the Dynamic Stochastic General Equilibrium model, mercifully called DSGE for short. According to the model's most devoted acolytes, the model's insights rival the perfect knowledge that Paul described in the First Letter to the Corinthians, but unlike the knowledge Paul described, DSGE's insights are supposedly available to us in the here and now. That overstates the case some, but if politicians can't exaggerate, who can?

To be fair, DSGE and similar macroeconomic models were conceived as theorists' tools, but why, then, do we continue to rely upon them for so many critical decisions, so much practical policy advice? And what has caused them to become, and to stay, so firmly entrenched? And, finally, the most important question of all: How do we get out of the mess we are in? What economic models, what tools are at our disposal to give us useful advice to deal with our urgent economic problems? If this approach to economics is

useless for the purposes of advising policymakers to lead to better economic outcomes, what are we getting out of the economic research we fund through NSF?

Besides raising these questions about the dominant model, we plan to have a look at the competition. What kinds of alternative models exist, and do we need to generate more still? Should we be using a variety of models in concert rather than relying on only one model or one kind of model, much the way meteorologists use a variety of models? Should the Federal Government use its funding of economic science to encourage the development of those alternative approaches?

We do have a very distinguished panel today to help us consider these issues. Dr. Robert Solow will tell us what is in the DSGE model, where it parts from the realities of the world, and what kind of advice it tends to deliver. Dr. Solow very modestly is not wearing today for this hearing his Nobel medallion. Dr. Sidney Winter will talk about the economic realities that DSGE and its macroeconomic cousins fail to take into account and about how to look for policy advice when there are important features of the economy that don't lend themselves to modeling. Dr. Scott Page will provide a glimpse of a new form of model that advanced computing power has made possible, the agent-based model, and make a case for the use of many and varied models. Dr. David Colander will explain why DSGE has achieved such a monopoly and outline a plan designed to open the floor to a broader spectrum of ideas. And Dr. V.V. Chari will state that while DSGE models are definitely capable of improvement, many of the criticisms leveled against them are inaccurate and, in any case, there is no other game in town. And I note that Dr. Chari is the minority witness and is a very useful addition to this panel today.

[The prepared statement of Chairman Miller follows:]

PREPARED STATEMENT OF CHAIRMAN BRAD MILLER

I know that economists must think politicians are impossible to please. Harry Truman complained that he wanted a one-handed economist. And now we're complaining that we got very confident economic advice in the last decade, but that one-handed advice proved to be wrong.

Unemployment is hovering at just under 10 percent—more than 16 percent when you include the folks who have given up looking for work or who are working part time when it's a full-time job they really want. Banks have cash but aren't lending, and the Federal Reserve can't lower interest rates any more without paying banks to take the money. There's worried talk of a deflationary spiral like the one that's dogged Japan for almost two decades. And arguments about whether it is better to stimulate the economy or cut the deficit appear backed more by ideology, gut feeling and election-year politics, than by honest evidence.

It would be great to have some reliable guidance to lead us out of this mess. But what we thought was authoritative guidance failed to see the mess coming and may actually have helped create the mess to begin with. Expert models of finance and the economy led to risk-taking at our largest financial firms and failed to warn our leading economic policy makers that doom lurked in the housing market.

Because our experts' way of looking at the economy left them blind to the crisis that was building, we were unprepared to deal with the crisis. A few weeks after Lehman Brothers went bust, Former Fed Chairman Alan Greenspan, the steward of our economy during the 20 years that culminated in the housing bubble's growth, told our colleagues on the House Oversight and Government Reform Committee that his reaction to the financial crisis was one of "shocked disbelief." He had "found a flaw," as he put it, "in the model that [he] perceived [to be] the critical functioning structure that defines how the world works."

Greenspan's fallen model of the market shares many assumptions with the model that's favored today, from academe to the world's central banks. The macroeconomic model is called the Dynamic Stochastic General Equilibrium model mercifully called DSGE for short. According to the model's most devoted acolytes, the model's insights rival the perfect knowledge Paul described in the First Letter to the Corinthians; but unlike the knowledge Paul described, DSGE's insights are available in the here and now.

To be fair, DGSE and similar macroeconomic models were first conceived as theorists' tools. But why, then, are they being relied on as the platform upon which so much practical policy advice is formulated? And what has caused them to become, and to stay, so firmly entrenched? And, finally, the most important question of all: What do we get when we apply the various tools at our disposal to the urgent economic problems we're facing today?

If this approach to economics is useless for the purposes of advising policy makers to lead to better economic outcomes, what are we getting out of the economic research funded through the NSF?

Besides raising these questions about the dominant model, we plan to have a look at the competition. What kinds of alternative models exist, and do we need to generate still others? Should we be using a variety of models in concert rather than relying on only one type? Should the Federal Government use its funding of economic science to encourage the development of these alternative approaches?

We have a distinguished panel to help us delve into these issues. Dr. Robert Solow will tell us what is in the DSGE model, where it parts from the realities of the world, and what kind of advice it tends to deliver. Dr. Sidney Winter will talk about the economic realities that DSGE and its macroeconomic cousins fail to take into account and about how to look for policy advice when there are important features of the economy that don't lend themselves to modeling. Dr. Scott Page will provide a glimpse of a new form of model that advanced computing power has made possible, the agent-based model, and make a case for the use of many and varied models. Dr. David Colander will explain why DSGE has achieved such a monopoly and outline a plan designed to open the floor to a broader spectrum of ideas. And Dr. V.V. Chari will state that while DSGE models are definitely capable of improvement, many of the criticisms leveled against them are inaccurate and, in any case, "there is no other game in town."

So my advice to you is to prepare for a lively discussion, and with that I yield back my time and call on the Ranking Member, Dr. Broun, for his opening statement.

Chairman MILLER. I yield back my time—actually I had no time to yield back and I now recognize the ranking member, Dr. Broun, for his opening statement.

Mr. BROUN. Thank you, Mr. Chairman.

Let me welcome the witnesses today, and I greatly appreciate you all being here with us.

Today's hearing on macroeconomic modeling continues this Committee's work on the role of science in economics. Not surprisingly, several of the topics addressed at our previous two hearings are also relevant today as we discuss macroeconomic modeling. Understanding the purpose and limitations of models is just as important in macroeconomic models as it is in financial risk modeling.

In general, modeling is also a theme this Committee has addressed several times in the past. Whether it is in regard to climate change, chemical exposures, pandemics, determining spacecraft survivability or attempting to value complex financial instruments, models are only as good as the data and assumptions that go into them. Ultimately, decisions have to be made based on a number of variables which should include scientific models but certainly not exclusively. As the witnesses of previous hearings have stated, "Science describes; it does not prescribe." No model will ever relieve a banker, trader, risk manager or policymaker of the responsibility of making difficult decisions.

This Committee struggles enough with the complexities of modeling, risk assessment and risk management regarding the physical sciences. Attempting to adapt these concepts to economics is even more complex. Despite the attempts of many to develop a scientific panacea for informing economic decisions, models are only a tool employed by decision makers and economists. They add another layer of insight but they are not crystal balls. Appreciation of this complexity and understanding the limitation and intended purpose of the economic models is just as important as what the models tell us.

We have an esteemed panel of witnesses here today who will discuss the appropriate roles and limitations of models such as the Dynamic Stochastic General Equilibrium, DSGE model. Mr. Chairman, maybe they will explain why they picked such a name. But I look forward to you all's testimony, and I yield back the balance of my time. Thank you.

[The prepared statement of Mr. Broun follows:]

PREPARED STATEMENT OF REPRESENTATIVE PAUL C. BROUN

Thank you Mr. Chairman.

Let me welcome the witnesses here today and thank them for appearing. Today's hearing on macroeconomic modeling continues this Committee's work on the role of science in economics. Not surprisingly, several of the topics addressed at our previous two hearings are also relevant today as we discuss macroeconomic modeling. Understanding the purpose and limitations of models is just as important in macroeconomic models as it is in financial risk modeling.

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We have an esteemed panel of witnesses here today who will discuss the appropriate roles and limitations of models such as the Dynamic Stochastic General Equilibrium (DSGE) model. I look forward to their testimony and yield back my time.

Thank you.

Chairman MILLER. The reason I said it out loud was that we would all be forgiven that and could all just say DSGE going forward.

I now ask unanimous consent that all additional opening statements submitted by members will be included in the record. Without objection, so ordered.

It is now my pleasure now to introduce our witnesses. Dr. Robert Solow is Institute Professor Emeritus at MIT, where he has been a Professor of Economics since 1949, and is currently Foundation Scholar at the Russell Sage Foundation as well as the President of the Cournot Center for Economic Study. I had more about you, sir. Dr. Solow did receive the Nobel Prize for Economics, as I men-

tioned earlier, in 1987, and the National Medal of Sciences in 1999. He is a member of the National Academy of Science. Dr. Solow was the Chairman of the Board of the Federal Reserve Bank of Boston for three years. Earlier in this career, he served as the Senior Economist on the Council of Economic Advisors during President Kennedy's Administration.

Dr. Sidney Winter is the Deloitte and Touche Professor of Management Emeritus at the Wharton School of the University of Pennsylvania. Before joining Wharton in 1933, he served for four years as chief economist of the U.S. General Accounting Office, now called the Government Accountability Office, our friends at GAO in Washington. He taught for more than two decades in the economics departments of Yale University and the University of Michigan.

Dr. Scott Page is the Leonid Hurwicz Collegiate Professor of Complex Systems, Political Science and Economics at the University of Michigan, and an External Faculty Member of the Santa Fe Institute. Dr. Page, you might want to come up with a shorter way to describe your job, just like DSGE is so handy. He is the author of three books and more than 50 scientific research papers and has won awards for teaching and service at four major universities.

Dr. V.V. Chari is the Paul W. Frenzel Land Grant Professor of Liberal Arts, Professor of Economics and Founding Director of the Heller-Hurwicz Institute at the University of Minnesota. He has served the Federal Reserve Bank of Minneapolis, for which he now consults as a Senior Research Officer and Economic Advisor. He has been elected Fellow of the Econometric Society.

And then finally, Dr. David Colander has been the Christian A. Johnson Distinguished Professor of Economics at Middlebury College since 1982. He has authored, co-authored or edited more than 40 books and 150 articles on a wide range of topics. His books include a Principles of Macroeconomics text and Intermediate Macro text. He is the former President of the History of Economic Thought Society.

Our witnesses should know that you each have five minutes for your spoken testimony. Your written testimony will be included in the record of the hearing. When you have completed your spoken testimony, we will begin with questions. Each member will have five minutes to question.

It is the practice of the Subcommittee, since this is an investigations and oversight subcommittee, to take testimony under oath. It does seem very unlikely that there would be any perjury prosecutions coming out of today's hearing. The prosecutor, the U.S. Attorney, would have to prove that you knew the truth and consciously deviated from it. Do any of you have any objection to taking an oath? The record should reflect that all the witnesses indicated that they had no objection. You also have the right to be represented by counsel. Do any of you have counsel here? Surprisingly enough, the record should reflect that none of the witnesses, or, all the witnesses indicated they did not have counsel. Please now stand and raise your right hand. Do you swear to tell the truth and nothing but the truth? The record should reflect that all of the witnesses did take the oath.

We will now start with Dr. Robert Solow. Dr. Solow, you are recognized for five minutes. I think you may need to turn on your—is your microphone on?

**STATEMENT OF ROBERT M. SOLOW, PROFESSOR EMERITUS,
MASSACHUSETTS INSTITUTE OF TECHNOLOGY**

Mr. SOLOW. Well, I start by thanking you and Dr. Broun for inviting me to this hearing. It is a little odd to be discussing an abstract question like how the macroeconomy works under circumstances like this, but it is pretty urgent. Here we are near the bottom, as the chairman said, of a deep and prolonged recession and the immediate future is very uncertain. We are in desperate need of jobs, and the approach to macroeconomics that dominates the elite universities of the country and many central banks and other influential policy circles, that approach seems to have essentially nothing to say about the problem. It doesn't offer any guidance or insight and it really seems to have nothing useful to say. And my goal in the next few minutes is to try to explain why it has failed and is sort of intrinsically bound to fail.

But before I go on, there is something preliminary that I want to make clear. I am generally a quite traditional, mainstream economist. I think that the body of economic analysis that we have built up over the years and teach to our students is pretty good. There is no need to overturn it in any wholesale way and there is no acceptable suggestions for doing that. It goes without saying that there are big gaps in our understanding of the economy and there are plenty of things we know that ain't true. That is almost inevitable. The national economy is a fearfully complex thing and it is changing all the time, so there is no chance that anyone is ever going to get it right once and for all. So it is all the more important to catch foolishness when you see it.

When it comes to things as important as macroeconomics, I think that every proposition has to pass a smell test: Does it really make sense? And I don't think that the currently popular DSGE models—I can say Dynamic Stochastic General Equilibrium without a lapse—I don't think that those models pass the smell test. They take it for granted that the whole economy can be thought of as if it were a single, consistent person or dynasty carrying out a rationally designed, long-term plan, occasionally disturbed by unexpected shocks but adapting to them in a rational, consistent way. I don't think that that picture passes the smell test. And the protagonists of this idea make a claim to respectability by asserting that it is founded on what we know about microeconomic behavior; but I really think that this claim is generally phony. The advocates believe what they say, there is no doubt, but they seem to have stopped sniffing or to have lost their sense of smell altogether.

So most economists are willing to believe that individual agents, consumers, investors, borrowers, lenders, workers, employers all make their decisions so as to do roughly the best that they can for themselves, given their possibilities and their information. They don't always behave in that fairly rational way, and systematic deviations are well worth studying. But it is not a bad first approximation in many cases.

The DSGE model populates its simplified economy with exactly one single combined worker, owner, consumer, everything else who plans ahead carefully, lives forever; and one important consequence of this representative-agent assumption is that there are no conflicts of interest, no incompatible expectations, no deceptions. This all-purpose decision maker essentially runs the economy according to its own preferences—not directly, of course, the economy has to operate through generally well-behaved markets and prices. Under pressure from skeptics and from the need to deal with actual data, DSGE modelers have worked hard to allow for various market frictions and imperfections like rigid prices and wages, asymmetries of information, time lags and so on. This is all to the good, and they have done very good work. But the basic story always treats the whole economy as if it were like a person trying consciously and rationally to do the best it can on behalf of the representative agent, given its circumstances. This cannot be an adequate description of a national economy, which is pretty conspicuously not pursuing a consistent goal. A thoughtful person faced with that economic policy based on that kind of idea might reasonably wonder what planet he or she is on.

The most obvious example is that the DSGE story has no real room for unemployment of the kind we see most of the time and especially now: unemployment that is pure waste. There are competent workers willing to work at the prevailing wage or even a bit less, but the potential job is stymied by a market failure. The economy is simply unable to organize a win-win situation that is apparently there for the taking. This sort of outcome is incompatible with the notion that the economy is in rational pursuit of an intelligible goal. The only way the DSGE and related models can cope with unemployment is to make it somehow voluntary, a choice of current leisure or a desire to retain flexibility for the future or something like that. But that is exactly the sort of explanation that does not pass the smell test.

To the extent that the observed economy is actually doing the best it can given the circumstances, it is already adapting optimally to whatever expected or unexpected disturbances come along. It cannot do better. It follows that conscious public policy can only make things worse. If the government has better information than the representative agent has, then all the government has to do is to make the information public. If prices are imperfectly flexible, then the government can make them more flexible by attacking monopolies and weakening unions, and actually even that proposition is dubious on its own.

The point that I am making is that the DSGE model has nothing useful to say about anti-recession policy because it has built into its essentially implausible assumption the conclusion that there is nothing for macroeconomic policy to do. I think we have just seen how untrue that is for an economy attached to a highly leveraged, weakly regulated financial system, as the chairman pointed out, but I think it was just as visibly false in earlier recessions and in episodes of inflationary overheating that followed quite different patterns. There are other traditions in macroeconomics that provide better ways to do macroeconomics, and I hope we will get a chance to talk about that soon. Thank you.

[The prepared statement of Dr. Solow follows:]

PREPARED STATEMENT OF ROBERT SOLOW

It must be unusual for this Committee, or any Congressional Committee, to hold a hearing that is directed primarily at an analytical question. In this case, the question is about macroeconomics, the study of the growth and fluctuations of the broad national aggregates—national income, employment, the price level, and others—that are basic to our country's standard of living. How are these fundamental aggregates determined, and how should we think about them? While these are tough analytical questions, it is clear that the answers have a direct bearing on the most important issues of public policy.

It may be unusual for the Committee to focus on so abstract a question, but it is certainly natural and urgent. Here we are, still near the bottom of a deep and prolonged recession, with the immediate future uncertain, desperately short of jobs, and the approach to macroeconomics that dominates serious thinking, certainly in our elite universities and in many central banks and other influential policy circles, seems to have absolutely nothing to say about the problem. Not only does it offer no guidance or insight, it really seems to have nothing useful to say. My goal in the next few minutes is to try to explain why it has failed and is bound to fail.

Before I go on, there is something preliminary that I want to make clear. I am generally a quite traditional mainstream economist. I think that the body of economic analysis that we have piled up and teach to our students is pretty good; there is no need to overturn it in any wholesale way, and no acceptable suggestion for doing so. It goes without saying that there are important gaps in our understanding of the economy, and there are plenty of things we think we know that aren't true. That is almost inevitable. The national—not to mention the world—economy is unbelievably complicated, and its nature is usually changing underneath us. So there is no chance that anyone will ever get it quite right, once and for all. Economic theory is always and inevitably too simple; that can not be helped. But it is all the more important to keep pointing out foolishness wherever it appears. Especially when it comes to matters as important as macroeconomics, a mainstream economist like me insists that every proposition must pass the smell test: does this really make sense? I do not think that the currently popular DSGE models pass the smell test. They take it for granted that the whole economy can be thought about as if it were a single, consistent person or dynasty carrying out a rationally designed, long-term plan, occasionally disturbed by unexpected shocks, but adapting to them in a rational, consistent way. I do not think that this picture passes the smell test. The protagonists of this idea make a claim to respectability by asserting that it is founded on what we know about microeconomic behavior, but I think that this claim is generally phony. The advocates no doubt believe what they say, but they seem to have stopped sniffing or to have lost their sense of smell altogether.

This is hard to explain, but I will try. Most economists are willing to believe that most individual “agents”—consumers investors, borrowers, lenders, workers, employers—make their decisions so as to do the best that they can for themselves, given their possibilities and their information. Clearly they do not always behave in this rational way, and systematic deviations are well worth studying. But this is not a bad first approximation in many cases. The DSGE school populates its simplified economy—remember that all economics is about simplified economies just as biology is about simplified cells—with exactly *one* single combination worker-owner-consumer-everything-else who plans ahead carefully and lives forever. One important consequence of this “representative agent” assumption is that there are no conflicts of interest, no incompatible expectations, no deceptions.

This all-purpose decision-maker essentially runs the economy according to its own preferences. Not directly, of course: the economy has to operate through generally well-behaved markets and prices. Under pressure from skeptics and from the need to deal with actual data, DSGE modellers have worked hard to allow for various market frictions and imperfections like rigid prices and wages, asymmetries of information, time lags, and so on. This is all to the good. But the basic story always treats the whole economy as if it were like a person, trying consciously and rationally to do the best it can on behalf of the representative agent, given its circumstances. This can not be an adequate description of a national economy, which is pretty conspicuously *not* pursuing a consistent goal. A thoughtful person, faced with the thought that economic policy was being pursued on this basis, might reasonably wonder what planet he or she is on.

An obvious example is that the DSGE story has no real room for unemployment of the kind we see most of the time, and especially now: unemployment that is pure waste. There are competent workers, willing to work at the prevailing wage or even

a bit less, but the potential job is stymied by a market failure. The economy is unable to organize a win-win situation that is apparently there for the taking. This sort of outcome is incompatible with the notion that the economy is in rational pursuit of an intelligible goal. The only way that DSGE and related models can cope with unemployment is to make it somehow voluntary, a choice of current leisure or a desire to retain some kind of flexibility for the future or something like that. But this is exactly the sort of explanation that does not pass the smell test.

Working out a story like this is not just an intellectual game, though no doubt it is a bit of that too. To the extent that the observed economy is actually doing the best it can, given the circumstances, it is already adapting *optimally* to whatever expected or unexpected disturbances come along. It can not do better. It follows that conscious public policy can only make things worse. If the government has better information than the representative agent has, then all it has to do is to make that information public. If prices are imperfectly flexible, then the government can make them more flexible by attacking monopolies and weakening unions. Actually this proposition is dubious on its own.

The point I am making is that the DSGE model has nothing useful to say about anti-recession policy because it has built into its essentially implausible assumptions the “conclusion” that there is nothing for macroeconomic policy to do. I think we have just seen how untrue this is for an economy attached to a highly-leveraged, weakly-regulated financial system. But I think it was just as visibly false in earlier recessions (and in episodes of inflationary overheating) that followed quite different patterns. There are other traditions with better ways to do macroeconomics.

One can find other, more narrowly statistical, reasons for believing that the DSGE approach is not a good way to understand macroeconomic behavior, but this is not the time to go into them. An interesting question remains as to why the macroeconomics profession led itself down this particular garden path. Perhaps we can come to that later.

Chairman MILLER. Thank you, Dr. Solow.
Dr. Winter, you are recognized for five minutes.

**STATEMENT OF SIDNEY G. WINTER, DELOITTE AND TOUCHE
PROFESSOR EMERITUS OF MANAGEMENT, THE WHARTON
SCHOOL OF THE UNIVERSITY OF PENNSYLVANIA**

Mr. WINTER. Thank you. Mr. Chairman and members of the subcommittee, this hearing explores some fundamental and relatively neglected questions related to the recent financial crisis, and I am pleased and honored to be asked to participate. And I am honored to be following Bob Solow on this panel because Bob was once my college honors examiner and not long after that my boss, but that was a long time ago.

As you mentioned, Mr. Chairman, I moved to the management department at the Wharton School of the University of Pennsylvania after previously having spent two decades teaching microeconomic theory at the Ph.D. level. One of the reasons that I shifted to a management department is that it offered me a more supportive environment for my research, which is more concerned than is the economics discipline with the realities of business behavior and of organizational behavior generally. The concern of this hearing, the shortcomings of the DSGE model, represents the tip of a very large iceberg, an iceberg which comprises by far the greatest proportion of model building and theorizing in the discipline, both microeconomic and macroeconomic.

A distinctive feature of economics among the sciences is the degree to which most economists, especially most theoretical economists, are oblivious to behavioral realities at the levels of the fundamental units of the complex system that they study: the business firms and households. Although many economists defy that descrip-

tion, they remain few compared to the mainstream and do not get much attention or carry much weight.

I was asked to discuss what is left out of the DSGE model. All theories must of course leave out almost everything, since the point of theory is to simplify reality in a way that tells the truth while not aspiring to tell the whole truth. But DSGE is an extreme example of the tendency to analyze hyperstylized versions of economic problems, thereby denying or suppressing quite observable and verifiable realities.

If improving the model is the problem, the challenge is to identify specific causal mechanisms in reality that should be in the model but are now excluded. However, in my view, improving model is not the only thing that deserves attention. We should really be talking about how to organize ourselves to meet the real needs for economic policy guidance, initially leaving open the questions about models and empirical inquiry.

I attempt three things in the remainder of my time. First, I mention a piece of economic reality that was fundamental to the recent financial crisis but was not reflected in the DSGE model or any macroeconomic model I know of. Second, I will suggest the difficulties and prospects of adding this piece to the prevailing models. And third, I will expand on the need to extend the quest for policy advice beyond models and their improvement.

The piece of reality I referred to is the process by which the residential mortgage business in the United States evolved into a system where nobody really cared whether the loans were going to be repaid. This meant, as you know, not attending carefully to the creditworthiness of borrowers and not seriously appraising the collateral. These practices developed slowly out of familiar mechanisms of self-interest, with attendant thoughtful advocacy, until at the end lenders in the traditional sense—with traditional lender incentives—had gone almost extinct as an economic species. I review this story in my written testimony. This is a pretty shocking thing in an economy in which self-interest is regarded as a fundamental and generally constructive guiding force. It may be particularly shocking to economic theorists because it beautifully illustrates the type of behavioral reality that most theorists tend to deny, since it seems sharply at odds with conventional, oversimplified images of economic rationality. What? Lenders didn't care about loan repayment? Most theorists would be so sure that couldn't happen that they wouldn't bother to check.

While there were other contributing factors, if you ask what distinguished this event from other economic crises, it becomes clear that residential mortgages and the business practices related to them were central to this crisis and to where the bailout money went. Without the mortgage-related practices, there might still have been a crisis at some point, but it would not have been much like the financial crisis of 2008 and it might have been a lot less severe.

So was this episode something that macroeconomic theory could or should make room for? These are all major macroeconomic events and they have a clear basis in long-sustained patterns of economic behavior among private-sector actors. So there might be a presumption that the causal mechanisms do belong in the model,

yet it is hard to imagine that much of the story of mortgage-market evolution is eligible for inclusion in macroeconomic theory as we conventionally understand it. The mortgage market by itself is far more complex than the DSGE represents the whole economy to be.

We could at least have a richer collection of partial models to inform us and we need in particular models based on business practice, an idea that does not appear in any mainstream economic theory text that I know about. Other key words to look for would include *habit, organizational routines, organizational capabilities, business systems, business processes*. They are all very much a part of the reality, and they can produce social outcomes very different from those anticipated in standard theory—and they are all absent from the textbooks. This is probably because they are all in some ways at odds with the theorists' assumption that businesses reliably get the right answer to the problems that they face.

Finally, I will return to my suggestion that we may need to look beyond the models and the theories to find the kinds of adjustments that are needed and appropriate, given the very large social stakes in macroeconomic problems. New research initiatives are needed in the regulatory agencies as well as in academe. Most fundamentally, we need to make sure that adequate intellectual resources are applied to the task of understanding what is happening in the economy as opposed to what is happening in the models. Those seeking that understanding must draw on the valuable body of knowledge that mainstream economics has accumulated but also on much broader sources. Historical perspective is particularly important. In the domain of modeling, we need more models that seek to capture systematic behavioral tendencies as they are and then assess the implied outcomes in terms of the service to private and social interests, rather than committing fully to the right answer framework from the very start.

Thank you, Mr. Chairman. Thank you for your attention.

[The prepared statement of Dr. Winter follows:]

PREPARED STATEMENT OF SIDNEY G. WINTER

Mr. Chairman and members of the Committee, this hearing explores some fundamental and relatively neglected questions related to the recent financial crisis, and I am pleased and honored to be asked to participate.

My name is Sidney Winter. I am the Deloitte and Touche Professor of Management, Emeritus, at The Wharton School of the University of Pennsylvania, where I spent 15 years in the Management Department. I am trained as an economist, and I previously was a tenured faculty member in two economics departments, those of Yale University (13 years) and the University of Michigan (8). One of my central roles there was to teach microeconomic theory at the Ph.D. level. Between Yale and Wharton, I spent four years as the Chief Economist of what was then called the U.S. General Accounting Office (now the U.S. Government Accountability Office).

One of the reasons that I wound up in a management department is that it offered me a more supportive environment for my kind of research, which is more concerned than is the economics discipline with the realities of business behavior, and of organizational behavior generally. It should be clear that my background and research do not qualify me as any sort of macroeconomist, theoretical or applied. What I offer here is a different perspective, which I hope the Committee will find useful in the context of this hearing.

The concern of this hearing, the shortcomings of the DSGE model, represents the tip of a very large iceberg, an iceberg which comprises almost all model-building and theorizing in the discipline, both macroeconomic and microeconomic. A distinctive feature of economics among the sciences is the degree to which most economists, especially most theoretical economists, are oblivious to behavioral realities at the levels of the fundamental units of the complex system they study.

In absolute number, there are many dissenters from that dominant view, and much constructive work is done from different viewpoints. I will take note of some it later on. One can reasonably argue that a slow trend has favored the dissenters for a few decades now. Relatively speaking, however, the dissenters are still few and their aggregate research effort is a fraction of what the mainstream tradition mounts, especially in core, policy-relevant domains like macroeconomics and public finance. Relative to the mainstream, the dissenters do not get much attention, and do not carry much weight.

I was asked to discuss “what is left out” of the dominant DSGE model. All theories must leave out almost everything, since the idea of theory is to try to tell the truth while not aspiring to telling the *whole* truth—because the latter ambition is hubris-ridden and ultimately counterproductive. But DSGE is an extreme case of the tendency to analyze hyper-stylized versions of economic problems, thereby suppressing or denying quite observable realities. In the DSGE case, the suppressed realities include the fact that economic actors are diverse and have diverse interests. Like many other economists, I would argue that the divergence of interests is one fundamental source of the difficulty society has in settling on good rules for the economic game. Macroeconomic dysfunctions like financial crises and involuntary unemployment are among the problems that good rules could help prevent—but for our difficulties in agreeing on enforceable ones. On this view, representing the macroeconomic problem as one confronting a single optimizing actor is an approach that is off target from the start.¹

It is useful to think of economic models as parables. True, the great teachers of history did not typically use mathematical notation when they used a parable to get a point across. Putting aside the notation issue, and also the level of professed concern with logical consistency, there are strong parallels between what those teachers sought to do and what economic modelers seek to do. The objective is not to tell “the whole truth,” but to get the point across. “When you think about this complex world we live in, or about how to get to heaven as your next stop, you might find it helpful keep this in mind: (insert parable here)”

Robert Solow put this very well when in characterizing his own approach to economic theory:

“My general preference is for small, transparent, tailored models, often partial equilibrium, usually aimed at understanding some little piece of the (macro)-economic mechanism.” (Solow 2008).

Arguably, almost all of what economic theorists “know” today about how the economy works can reasonably be thought of as a string of logically tight parables, some with a degree of empirical grounding, many not. The DSGE model is consistent with this broad approach to understanding the economy, but stands out for the ambitious scope of its subject matter, as well as for its high commitment to analyzing the optimal behavior of a single, fictitious type of actor.

Thus, if improving the model is the problem, the challenge is to locate the zone where there is an interesting case for an incremental adjustment, identifying specific things that should be in but are now excluded. However, in my view, improving the model is not the only thing that deserves attention. We should really be talking about how to organize ourselves to meet the real needs for economic policy guidance.

I attempt three things in this testimony. First, I will point to a piece of economic reality that was fundamental to the recent financial crisis but was not reflected in the DSGE model or in any macroeconomic model I know of. Second, I will suggest the difficulties and prospects of getting this piece of reality reflected in the models. Third, I will expand on the need to extend the quest for policy guidance beyond models and their improvement.

Building Toward Crisis: The Insidious Evolution of the U.S. Mortgage Market

The reality I speak of is the process by which the residential mortgage business evolved into a system where, when the loans were being made, *nobody really cared* whether the loans were going to be repaid. This meant not attending carefully to the credit-worthiness of borrowers, and not seriously appraising the collateral. These practices developed slowly, driven by familiar considerations of self-interest and opportunity, with attendant thoughtful advocacy—until in the end, traditional lenders, with traditional incentives, had almost gone extinct as an economic species. Those who remained presumably still cared, but they were largely replaced by new

¹To be fair, the economy-as-single-actor approach does have its own substantial history in the discipline, as illustrated by discussions of hypothetically perfect central planning.

species of players who, collectively, lost track of the problem of loan quality. At least some of those new players suffered great financial losses as a consequence of their errors, but the losses inflicted on the taxpayers and society as a whole were, and continue to be, much larger.

That is a pretty shocking thing to happen in an economy in which self-interest is regarded as a fundamental, and generally constructive, guiding force. It may be particularly shocking to economic theorists, because it beautifully illustrates a type of behavioral reality that most theorists tend to deny, since it seems so sharply at odds with conventional, oversimplified, images of economic rationality. What, lenders didn't care about loan repayment? Most theorists would be so sure it couldn't happen that they wouldn't bother to check.

This insidious transformation happened "sensibly," at least until quite a late stage (Jacobides 2005). Private sector actors responded to incentives in a largely familiar way, though with an unusually strong component of "financial innovation." (While we tend instinctively to celebrate "innovation," it should be remembered that "innovative" often means "untested and hazardous.") Government authorities and other observers commented on some of these developments, and there was some questioning and some level of warning was heard. Authoritative figures, however, largely pronounced the developments to be acceptable or even benign. (See for example (Greenspan 2002).)

What was involved in the evolutionary transformation that brought us to a regime where "the lender doesn't care"? It is a complex question for which I can only sketch an answer. Though there are some gaps, many of the relevant facts are well known by now. There remains in any case the problem of putting the facts in the required order to help make sense of the crisis, and that is what I attempt here.

To unravel this complex story, it is simplest and also immediately instructive to start with the role of the mortgage broker. The broker is effectively an "out-sourced" sales arm for financial institutions that originate mortgages, i.e., that advance the money in the context of the actual sale of a property. The mortgage broker role did not always exist; the job of finding and hand-holding mortgage customers was formerly a task for employees of the financial institution that made the loan. Brokers became particularly important to mortgage banks, non-depository financial institutions that originated mortgage loans and financed them through the capital markets. As of 1988, brokers were involved in about 10% of loan originations by mortgage banks. There was a jump to about 35% by 1991, partly because troubled savings and loan institutions were cutting payrolls in the context of an industry crisis. Released sales employees became independent contractors, initially for former employers, but ultimately performed the brokerage function in a wider market. By 1999 the broker-mediated fraction was over 60% and has remained at similarly high levels since. (Jacobides 2005).²

Like most brokers, a mortgage broker is paid on commission, a percentage of the value of the deal. Once the deal is done—meaning the financing arranged and the house purchase closed—the broker takes a commission and leaves that scene and looks to facilitate another deal. This means that the direct self-interest of the broker is to facilitate deals and collect commissions, and the quality of the collateral and the probability of repayment do not enter into that directly. In this sense, it is clear that the broker "doesn't care"—at least in his or her assigned theoretical role as a self-interested economic agent. (But is the mortgage broker "the lender"? Clearly not. We will look further for a true lender, one who might still have cared, even in the world with mortgage brokers.)

This is a huge example of what economists call an "agency problem"—the agent may not have the interest of the principal at heart. The solution to the agency problem, if it is not available in the incentives, is in controls. Mortgage applications typically involve the completion of a lot of forms that are supposed to provide whatever assurance is reasonably available about the collateral and the creditworthiness of the borrower. From the viewpoint of the broker, the problem is to get these forms completed, and *completed in essentially reassuring ways*, so the financing can be arranged and the commission can be collected. And that indeed was what happened, at least until a late stage when even the nominal defenses of loan quality crumbled and documentation-light loans became commonplace—all the way to the extreme of the NINJA loan. ("No Income, Job or Assets"). To interpret the evolution as a whole,

²An institution that used its employees in the sales function rather than brokers would have superior opportunities to control loan quality, but might choose to exert control in the "wrong direction"—a possibility dramatically illustrated in the case of Washington Mutual, which not only complemented its thrift business with a mortgage bank, but allowed the risky practices of securitized loans to become the norm in the rest of the organization, as the recent Senate hearings demonstrated.

it is important first to understand that if something was going to resist the degradation of loan quality, it emphatically was not the incentives operating on mortgage brokers.

We come next to the originator, the financial institution that initially advances the money. If the originator were going to hold the loan, there would be an incentive to actually read those forms describing the loan and assess the prospects of repayment. Here is where “mortgage backed securities” (MBS) and the “originate and sell” business model enter the story. Many originators made money by becoming, in effect, another kind of broker—taking a cut but not holding a continuing interest, or very little. They forwarded the mortgages to Wall Street firms, who packaged them into MBS. Thus the originator did not retain an interest in the asset and, like the broker, had little direct incentive to be concerned with loan quality. If the forms that accompanied the application were supposed to defeat the obvious agency problem at the broker level, we confront the question of who had the incentive to actually attend to that information. Under the “originate and sell” model, the originator is not that party. In fact, intense local competition among originators often deflected managerial attention away from loan quality and toward the increase in volume.

The securitization of mortgages is an important financial innovation. It has a substantial history that can for present purposes, be dated from its introduction in the 1970s by the government sponsored enterprises (GSEs), Ginnie Mae, Fannie Mae and Freddie Mac. Initially, the loans themselves were made under governmental loan guarantee programs (FHA, VA). That constraint was subsequently relaxed, and private sector securitizers, mostly investment banks, followed the governmental lead. All of this was widely celebrated for its benign effects on housing finance, even by some conservative economists who credited the government leadership with reducing informational imperfections in the market. As the bubble peaked in 2006, private sector securitization activity had risen above 40% of total securitization. As the crisis broke 2007–2008, it collapsed. Overall, securitization played an increasing role in the mortgage finance system over the long period, as Table 1 indicates.

The economic rationale of securitization is based on the reduction of investment risk through diversification and the related capacity to raise housing finance through the capital markets rather than individual financial institutions. Because individual borrowers face diverse circumstances affecting repayment, it is possible to improve things by pooling risks and offering an investor the opportunity to invest, in effect, in the average performance of the pool. The economic logic is sound, provided certain conditions hold. Unfortunately, the “certain conditions” are not very certain at all, if by that one means that it is objectively easy to determine the degree to which they obtain. One condition is that the repayment histories of individual loans do not respond too much to the causal factors they inevitably share, such as influences on the general level of housing prices. Another is that the quality of loans in MBS pools remains uncorrupted by the feedback from the securitization itself. That feedback includes not only a reduced incentive to look carefully at individual loans, but also the learning of self-interested agents about the exploitable weak spots in the control system. (The latter parallels a problem commonly noted in the context of government regulation: Both public and private “regulators” have trouble staying ahead in their games with the “regulatees.”)

In the end, of course, somebody has to be putting money at risk to finance mortgage lending. It does not follow, however, that these individuals or organizations are in a position to provide a secure anchor for the chain of agency problems, effectively insisting that everybody down the line to the mortgage broker has an eye on loan quality. We can indeed locate, in the history of the crisis, some people who seemingly had the “right incentives” and some of them should, in retrospect, have been more careful. Nevertheless, most of them are best called *investors* rather than lenders, because the actual apparatus of loan-making was very far removed from them. In effect, the parties who put up the money mostly had an investor interest comparable to that of a typical stock market investor, a role which generally does not entail delving into the question of whether, for example, corporate management is making a good decision about the location of the next plant the company builds. Similarly, investors in MBS and related derivatives did not delve into the quality of the actual mortgage loans behind those securities.

Their institutional distance from the action left most investors poorly positioned to make good investment choices, and in many cases—such as ordinary people with their retirement money invested through funds of various kinds—they did not remotely have practical incentives to attack the very large problem of understanding where their money went. The big investors did not fare that much better, for they did not get a lot of help with understanding what was happening to their money. Their perceived “needs”—to invest their money at a good return—were met by waves of financial innovation that took the form of ever-more complex repackaging

of underlying mortgage debt, plus new ways to place bets for or against particular securities.³ This process made the information gulf widen until, it appears, it even swallowed some of the parties who were creating it.

In sum: Between the investors, large and small, and the mortgage originators, there were first the securitizers and then other institutional actors who might possibly have played a role in maintaining attention to loan quality—but didn't. In these layers, the story became complex and even exotic, ultimately taking leave of the domain of "sensible" economic motivation.

While much of this detail can be left aside, it is important to take specific note of the role of the rating agencies. These for-profit organizations exercised quasi-governmental authority by virtue of regulatory requirements restricting insurance companies, pension funds and other significant institutional investors to invest only in "investment grade" securities—a determination left to designated rating agencies. These agencies, however, were customers of the securitizers. They naturally tended to have "customer satisfaction" at heart, as any respectable for-profit actor in a market economy tends to do. Like the mortgage broker role, the customer orientation of rating agencies toward issuers was not always a feature of the system. Here again we note the role of institutional evolution: The rating agencies used to have investors as their customers, not issuers. The very important change of the business model occurred in the early 1970s. (See (White 2009) on the evolution of the rating agencies.)⁴

In retrospect, it appears that the rating agencies took customer satisfaction a good deal too seriously. Their ratings, and the related regulatory restrictions on investments, served to sustain the demand for MBS and related derivatives in the face of disastrous weakness in the underlying loans, with extremely adverse consequences for investors in the U.S. and around the world.⁵

We can thus explain how the insidious transformation happened, how there gradually evolved a mortgage lending system that had lost track of the loan quality issue. Traditional mortgage lenders with traditional incentives became an endangered species as a consequence of a series of incremental changes in institutions and industry architecture, and hence in the operative incentives. Many of those changes were of a readily identifiable, datable kind, or were marked by measurable trends. Mortgage borrowers, and "lending" as an activity concretely manifested at real estate closings, became far separated from the investors who had the ultimate stake in loan principal. In that gap there evolved layer upon layer of related business practices that seemed to "work" in the prevailing context. Like most such practices, they were retained while they worked, or perhaps a bit longer.

It remains for me to place the business practices of the residential mortgage sector in context among the candidate causes of the crisis. One can find on the website of the Financial Crisis Inquiry Commission a list of the 22 topics and substantive areas of concern to the Commission, all of which can plausibly be colored as contributing "causes" of the crisis. Undoubtedly, it was a complex event, with numerous factors involved. Assigning weights among multiple causes of a complex event is intrinsically a difficult thing to do, and no one has a credible claim to having sorted this one out completely.

If, however, we examine the *aspects that distinguish this event from other historical episodes of bubble-and-crisis*, it is very clear that residential mortgages and the practices related to them were central to the distinctive features of THIS crisis—and to where the bailout money went. The collapse of Bear Stearns, Lehman, AIG and others largely resulted from practices related to mortgages and derived securities. While excessive leveraging of investments in those securities was a major factor, the risks of leverage depend in general on the resistance to price decline pre-

³Varying levels of detail about collateralized debt obligations (CDOs), synthetic CDOs credit default swaps (CDSs), tranches and the like are available from sources at varying levels of readability. One good source is (Pozen 2009). For the highly readable version, see (Lewis 2010).

⁴The importance of that change as a factor in the crisis is challenged by some who emphasize the overwhelming levels of demand for the securities, itself the result of other factors. Charles Calomiris points to the role of asset managers looking for yield on behalf of their clients—and afflicted by yet another agency problem inherent in the way they were rewarded (Calomiris 2008). Ultimately, it might be difficult to disentangle the underlying strength of demand from the influence of obfuscation and misrepresentation. An accurate forecast of the events of September 2008 certainly would have discouraged a lot of demand.

⁵See Michael Lewis's best-selling book, *The Big Short*, for particularly vivid testimony on the character and behavior of the rating agencies, as well as other matters. While academic norms should discourage me from citing a popular journalistic book as "evidence," I see a lot of face validity in this testimony. Hence, if there is genuine disagreement on its factual accuracy, it seems that it would be useful for somebody to orchestrate an orderly confrontation on whatever is said to be disputable. There are several excellent books on the origin of the crisis to which the same remark applies.

sented by the leveraged assets. Thus, when the fundamental weakness of the mortgage-related assets became apparent, the havoc wrecked by the excessive leverage was all the more extreme. Further back along the causal chain, laxity in underwriting practices not only produced the loans that underpinned flawed securities, but contributed to the housing bubble in a manner similar to the role played by low interest rates—a causal factor strongly emphasized by some economists (e.g., (Taylor 2009)). Because loans were made that shouldn't have been made, there was more demand for houses than there should have been, leading to higher prices, and thus more home equity to borrow against, further delaying the day of reckoning.⁶

To assess the “cause” of the crisis *without* reference to mortgage-related business practices would seem to be a bold exercise in hypothetical history. However sound and factual such an account might be with respect to interest rates, asset bubbles, speculative psychology and other matters, it has a weak claim to being about the Financial Crisis of 2008. Without the mortgage-related practices, there might still have been a crisis at some point, but it would not have been much like the Financial Crisis of 2008. It might also have been a lot less severe, and thus more in line with several previous crises in U.S. financial markets.

Does the Residential Mortgage Sector Belong in Macroeconomics?

Is the foregoing story about things that macroeconomic theory should or could make room for? The housing bubble, the financial crisis and the great recession are major macroeconomic events and ones with a clear (though partial) basis in long-maintained economic behavior patterns of private sector actors. The events were not basically “shocks” from technology or misguided public policies, though both of those did play a role.⁷ Given the importance of the events and their sources in economic behavior, it might seem that there is a presumption that the relevant mechanisms do “belong in the model.”

Yet, it is hard to imagine that much of the story that I have summarized here is eligible for inclusion in macroeconomic theory as we conventionally understand it. Deferring my discussion of the broader implications of that conclusion, I accept for the moment the conventional framing where progress is achieved through the accumulation of parables—partial models that each illuminates some little piece of the economic mechanism.

In that perspective, there remains abundant opportunity to improve macroeconomics by adding realism to the characterization of the problems faced by the different sorts of economic actors. Though not favored in the DSGE camp, this line has been vigorously pursued for a long time.⁸ There is a wide range of possibilities as to how exactly one goes about this; they differ particularly in the degree to which they seek to reconcile realism with a standing commitment to the traditional theoretical tools of optimization analysis.

In my view, the best path to further progress of this general kind is to develop models that are more securely grounded in an appreciation of the behavioral phenomena at the micro-levels—business firms and organizations, as well as individuals and households. By “grounded in an appreciation,” I mean, “attentive to the available evidence on the phenomena and prepared to concede it presumptive validity.” I emphatically do not mean that it is possible to avoid the trouble of thoughtful theorizing by somehow “copying” observed behavior directly into a model.

With respect to individuals and to a lesser extent households, there has been much progress of this kind in recent years. In their recent book, (Akerlof and Shiller 2009) review a number of areas where insights from behavioral research, combined with more conventional economic research greatly illuminate issues of macroeconomic significance—e.g., the origins of involuntary unemployment, saving behavior, and the role of speculative psychology. (As noted above, both speculative psychology and more considered speculative motives undoubtedly played a role in the housing bubble, but perhaps were less central to the eventual collapse than is some-

⁶For many homeowners, the threatened “reckoning” involved upward adjustments in mortgage interest rates on adjustable rate mortgages—with the result that the continued affordability of the mortgage was dependent on a continuing increase in the price of the house, generating equity that could be borrowed to pay the higher interest.

⁷Neither can the collapse be attributed to the occurrence of some highly improbable event, which however was more probable than previously expected because the relevant probability distributions had “fat tails.” Recognition of the empirical importance of fat-tailed distributions is long overdue, and was effectively promoted by (Taleb 2007). But fat-tailed distributions have little to do with the crisis. What happened was an extended process of “more of the same, only worse”—until in the end there was too much of the same, and it was much worse, and the system collapsed. It seems that Taleb emphatically agrees; see <http://www.fooledbyrandomness.com/imbeciles.htm>.

⁸I participated myself in one significant effort of that general kind (Phelps 1970).

times suggested.) Behavioral understanding has been furthered by experimental economics and by the work of the small band of researchers following the recently-opened paths to grounding behavioral understanding in human neurophysiology.

With respect to business firms and organizations, however, mainstream economics has shown little tendency to reach a *modus vivendi* with relevant lines of research, even to the limited extent that this is done with respect to behavioral research at the individual level. A basic fact is omitted from the mainstream models. Where there are plausible ways of dealing with this troublesome fact that are available from heterodox economic approaches, management and organization studies, and other social science disciplines, these opportunities tend to be ignored by the mainstream discipline.

The basic fact is that, in almost all real decision situations, neither the nature of the decision problem nor the list of available options is presented at the start with anything like the clarity posited in a mainstream model. Problems have to be discovered and framed; options have to be invented and designed. Consequently, it is far from the case that a mere optimization calculation (given some criterion) is all that separates the actor from a good decision—as mainstream modeling practice suggests. In the cases where this generalization does not hold—and there are important examples—the reason it does not hold is that the hard work has already been done in the past, and the power of systematic optimization techniques can readily be accessed to produce actionable results. Of course, that investment of “hard work” was itself an application of human ingenuity, and it may be flawed. The optimization may yield the right answer to quite the wrong problem, or fall short because of implementation difficulties within a frame that is basically sound.

The manifestations of the omitted fact are diverse, being quite different in the domain of high-level strategic decisions than they are in, say, pricing and inventory control in a department store or supermarket. Empirical behavioral research at the strategic level is often hampered by problems of access, and definitive results are also elusive because of the fog of uncertainty and complexity that is typical at that level. Lower in the hierarchy, however, the opportunities for observation and understanding by researchers are much greater.

It has been understood for a very long time that decisions about things like hiring, production techniques, output levels and pricing—the things featured in the economics texts as what firms decide about—are often not the subject of high-level managerial attention on a continuing basis (see, e.g., (Gordon 1948) or (Cyert and March 1963)). At least, they are not handled that way in the large organizations that account for the bulk of economic activity. It could hardly be otherwise, for there are just too many such decisions to be made.

Of necessity, and for a variety of specific reasons, firms commit for extended periods of time to systematic ways of doing things, including ways of making the “decisions” classically featured in the textbooks. These systematic ways often involve specialized equipment and personnel—computers and software, engineers and HR managers, for example. (Note that the personnel in these roles are “agents” as distinguished from principals, and incentives are not necessarily well aligned.) This decision apparatus is just as much an intermediate-term “given” in a typical firm as the plant and equipment is; it is open to reconsideration, but only over time and as the occasions warrant. For example, as noted previously, savings and loan institutions embraced the mortgage broker system initially in the context of crisis, as a cost control measure—not because it was identified as an “optimal” way to market mortgages. Once they had it in place, they stuck with it, it evolved on its own, and it seemed to succeed. In the financial markets, programmed trading provides an extreme example of the reality of systemization and automation in domains that economic theory treats as (intelligent? human?) “decisions”.

To explore this basic reality, we need instructive models based on “business practice”—an idea that does not appear in any mainstream economic theory text that I know about. Other keywords to look for in the index would include habits, skills, organizational routines, organizational capabilities, business systems, business processes. Such terms are commonplace in the discourse about business problems outside of economics, but all seem to be virtually absent from the economics texts. This is probably because they are in some ways at odds with the theorist’s standard assumption that businesses reliably get the *right answer* to the problems they face. As illustrated in the evolution of the mortgage market, business practices can produce social outcomes very different from those anticipated in standard theory.

While extending the theoretical parables in the “business practice” direction would be helpful, it remains true that parables are by nature limited in aspiration and effectiveness relative to the challenge of understanding the mechanism as a whole. The mechanism as a whole is a complex system with many tightly interconnected parts, and fragmentary analytical models are as unlikely to illuminate it

fully as they would be for a commercial airliner. You would not want to take the inaugural flight in a new type of airliner where the relevant experts explained merely that they believed they understood isolated fragments of its mechanism. But that is the sort of flight the whole U.S. economy took with its “new” mortgage market.

The residential mortgage system is far more complex than the DSGE model represents the economy as a whole to be. The DSGE model does not contain even a rudimentary representation of the financial sector at the level of the “IS–LM” model that has long been a staple of the macroeconomics textbooks, much less a reflection of the richer representations of asset markets and financial intermediation to be found in the broader research traditions of macroeconomic theory and financial economics. The DSGE economy cannot be brought low by the behavior of its brokers and bankers, because it doesn’t have any.

In the world of contemporary practical affairs, and on into many branches of pure science, extremely complex systems are effectively managed by complex organizations that seek to leave nothing to chance. Many of these systems are of extraordinary reliability—though we are recently reminded that big disasters can happen. This reliability is an accomplishment of social organization as much as it is of technology, and it involves effective integration of many different specialized skills and partial understandings. Although the stakes involved in macroeconomic policy management are much higher than in, say, space exploration, the ambition to surmount the challenge of complexity appears to be largely missing.⁹

I argue, therefore, that we are a long way from being able to understand the economy and generate macroeconomic policy guidance at a level commensurate with the stakes. The parables approach is constructive, and it can be more helpful in the future, but it is not adequate to the task. The discussion of the mortgage market and its role in the crisis suggests that it will be very difficult to correct this situation while staying within the frame of “improving the model.”

Meeting the Needs for Policy Guidance

I return to my suggestion that we may need to look beyond the models and theories, and beyond academic economics as practiced now, to find the kinds of adjustments that are fundamentally needed and appropriate.

There are, to begin with, issues about research funding and allocation, in particular, about the scale and character of projects that deserve public support. To devote more attention to how the system’s pieces fit together, as well as to what the pieces actually amount to in behavioral terms, we need research projects at a larger scale than has been typical. We also need intense and sensible (i.e., not theory-blinded) attention to economic phenomena. And we need these things on a continuing basis, enabling the tracking of the actual evolution of the system.

A panel of experts convened by the Pew Foundation commented as follows on the collective failure of the regulatory agencies to do that sort of tracking in the years leading up to the crisis:

“The crisis revealed both gaps in regulation and unanticipated interconnections among different types of financial institutions and markets. Yet no one was charged with understanding these interconnections, looking for gaps, detecting early signs of systemic threats and acting to mitigate them. During the years preceding the crisis, no regulator was tasked with monitoring and understanding the overall health of institutions and markets and the connections between them across the entire breadth of the financial system. Nor was any regulator charged with taking the lead in responding to any early signs of systemic risks. So, for example, several years ago there were widely recognized signs of unusual credit expansion and increases in leverage associated with an unprecedented rise in housing prices. These developments signaled the beginning of a bubble with the potential to destabilize the entire system. No action by any government agency was taken to address this.” (Pew 2009)

I argue that the economics discipline was complicit to a degree in this regulatory shortfall, since the task of “monitoring and understanding the overall health of institutions and markets and the connections between them across the entire breadth

⁹I must leave aside discussion of the applied side of macroeconomics represented by the econometric forecasting models. Although those models represent a higher ambition in terms of addressing the complexity of the system by assembling understanding of the pieces, the crisis of 2008 demonstrated that, for them too, far too much was evidently left out of the model. In particular, the dramatic events in the financial markets in the fall of 2008 were not significantly reflected in model forecasts by December 2008—there was only a continuation of a year-long trend toward a more pessimistic view of 2009 (as shown in the changing “Blue Chip consensus”).

of the financial system” is certainly one in which economists should be productively involved, but the prevailing research orientations of the discipline do little to support the development of competence at such an ambitious level. To improve the situation, change is needed not only in the regulatory agencies, but in academe. The two change agendas are inevitably closely related.

Given the highly individualistic way that economic research is organized in universities, the regulatory agencies may in fact be the most promising place to organize research of requisite scale and continuity. Given that the new financial reform legislation implies broadened responsibilities for the Federal Reserve, as well as the creation of a new Federal Stability Oversight Council, it may be an opportune time to reconsider the channels by which economic research can usefully inform policy and practice at the Federal level.

This suggestion, however, begs a number of important questions about the training, recruiting, pay and supervision of the government economists who might participate in such initiatives. The universities will continue to play the central role in the training of new economists and in doing so they will continue to impart an image of what is desirable in terms of style and focus in economic research. If, as I argue, some adjustment in style and focus is needed, then some of that adjustment must happen in universities or it will not happen at all. Beyond that, the universities compete with the government in the market for talent, and thereby constrain what agencies can do. My own impression is that the academic research model is more influential than it should be among economists in government, given that the latter should be oriented toward different objectives. My own experience tells me that this can be hard to resist, given the relative pay scales and the role of the promised job content in the recruiting process.

Especially in the market for well trained economists from the elite universities, there is a tendency to use the job requisite of “research freedom” as a recruiting feature. In practice, this may often mean freedom to try to lay the groundwork for a possible future career in academe, and such “freedom” entails acceptance in the short term of the research orientations of academe. Elsewhere in the government, such as among young lawyers in the Antitrust Division of DOJ, the use of government employment as a career stepping-stone seems to produce acceptable results at a relatively low cost. While the stepping-stone system is not necessarily a bad one in principle, I think it works relatively poorly for economists. The divergence in job content is too great, and would become even greater if my suggested reorientations should come to pass. This again underscores the need for some change on the academic side if there is to be any prospect of significant change overall.

One way or another, we need to make sure that adequate intellectual resources are applied to the task of understanding what is happening in the economy, as opposed to what is happening in the models. Those seeking that understanding must draw on the valuable body of knowledge that mainstream economics has accumulated, but also on much broader sources. Historical perspective is particularly important. In the domain of modeling, we need more models that seek to capture systematic behavioral tendencies *as they are*, and *then* assess the implied outcomes in terms of service to private and social interests, rather than committing fully to the “right answer” framework at the outset.

Once again, I thank the Committee for the opportunity to appear here today, and for your attention.

Table 1: US Home Mortgage Origination

(Billions of dollars)

Source: Inside MBS & ABS

Year	Total MBS Issuance (Dollars in Billions)	Total Mortgage Origination (Dollars in Billions)	% Securitized
1990	259	458	56.6%
1991	317	562	56.5%
1992	545	894	60.9%
1993	667	1,020	65.4%
1994	422	773	54.6%
1995	318	639	49.8%
1996	440	785	56.1%
1997	487	860	56.6%
1998	929	1,450	64.1%
1999	833	1,310	63.6%
2000	615	1,048	58.7%
2001	1,355	2,215	61.2%
2002	1,856	2,885	64.3%
2003	2,716	3,945	68.9%
2004	1,881	2,920	64.4%
2005	2,156	3,120	69.1%
2006	2,045	2,980	68.6%
2007	1,865	2,430	76.7%
2008	1,227	1,500	81.8%
2009	1,785	1,815	98.4%

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Chairman MILLER. Thank you, Dr. Winter.
Dr. Page, you are recognized for five minutes.

STATEMENT OF SCOTT E. PAGE, LEONID HURWICZ COLLEGIATE PROFESSOR OF COMPLEX SYSTEMS, POLITICAL SCIENCE, AND ECONOMICS, UNIVERSITY OF MICHIGAN

Mr. PAGE. Thank you, Mr. Chairman, and thank you to the committee for this opportunity to come and speak.

As mentioned, I am a Professor of Complex Systems at the University of Michigan and the Santa Fe Institute. Complex systems is probably unfamiliar to many of you, so I am going to begin with a simple definition if I may. Complex systems consist of diverse, connected, interdependent and adaptive actors who collectively produce phenomena that are difficult to explain or predict. So given this definition, an economy, traffic on the Beltway or even the stuff that goes on inside the Beltway, right, is going to be classified as “complex.”

In my comments today, I want to describe the benefits of having a variety of models when trying to understand a complex system. And I am going to show how complex-systems models themselves have an ability to generate insights that are going to be of interest to this committee, including the pace of innovation and market crashes.

So let me talk for a minute about the success of models as predictors. Models have proven almost, as Dr. Broun mentioned, almost unbelievably accurate in predicting some physical phenomena, such as the patterns in which planets orbit the sun. Yet as we all know, models have proven less adept at predicting the economy, and that is because the economy is complex. The solar system is complicated, it has got lots of connecting parts, but those parts aren't very diverse, right? They are little orbs, and they don't adapt, and because of that, it is predictable. So if you take something like a complex system, a single model can only cast so much light. Hence, we need multiple models, and this is an idea that goes back to Aristotle, who asserted that a multitude is a better judge than any individual.

Now, that is not just an intuition, that is something we can actually formalize. So my own work—I have written some stuff that basically says if I have a crowd of models and take the average of those predictions, then you can prove the following: that the crowd of models' accuracy is going to equal the average model's accuracy plus the model diversity. So this mathematical identity that I have framed here verbally shows the benefits of combining models. What you want is, you want a lot of models and you want those models to be diverse. But that is not to say that a group of models is accurately going to forecast the economy. It probably won't. The economy is too complex. But we can widen our lens and we can use a

crowd of models to predict bounds and the likely fluctuations in the economy, and to anticipate unintended consequences and riskiness of policy decisions—such as the expanding of use of sophisticated financial instruments such as credit default swaps.

So let me turn to my second point, the particular value of complex-systems models to help understand and guide the economy. This goes back to some of the things that Bob and Sid have mentioned. The economy consists of over 300 million people, 30 million organizations—and about 90 percent of those seek profits—and tens of thousands of government agencies. These actors are diverse. They have diverse beliefs and goals. They adapt as circumstances change, and they don't do so in lockstep. Some people spend, some save, some innovate, some people seek the comfort of routine. It is the aggregated, interdependent actions of these millions of actors—people, organizations and governments—that produce the macro-economic patterns that we are trying to explain and predict.

So how do we model this? The neoclassical approach assumes that individuals and firms make optimal choices subject to constraints of budgets, technology and time. Actors, be they firms, people or governments, accurately anticipate the future effects of their actions and the government's actions. And, in its simplest form, this model is going to produce a stable equilibrium with balanced growth. Now, modern variants, which Dr. Chari will probably talk about, of this model include technological shocks that reverberate throughout the economy. These variants also include frictions, such as wages that are slow to fall. This stickiness exacerbates the depth and length of the echoes caused by the shocks.

Now, this neoclassical model, this DSGE model, is stark. It assumes no sectors of the economy, no unemployment, no physical geography, no networks of connections, no learning—the agents are always optimizing—and little or no heterogeneity of income, wealth or behaviors. Further, almost all of the responses by the actors tend to equilibrate the system: So, for example, if you get an increase in demand for housing, this is going to increase the price of housing, therefore causing a reduction in future demand of housing. This is what we call in complex systems a “negative feedback.” The more you get of something, these negative feedbacks push things back. They tend to stabilize systems, and they lie at the core of neoclassical models.

Now, the complexity approach assumes individuals with diverse incomes and abilities who are situated in place and time. These actors don't necessarily maximize profits or utility. Instead, what they do is they follow rules that have survived or succeeded in the marketplace. So if a financial firm with greater leverage, such as Morgan Stanley, is making higher profits, other firms may follow their lead.

Now, note this effect: more leverage leads to greater leverage. This is a “positive feedback.” Positive feedbacks create what we call “correlated behavior.” Hence, systems that contain them can exhibit clustered volatility in large events like stock market crashes and home mortgage crises. These could be avoided if the agents in the model were capable of predicting the future and realizing they should be optimizing, not following other people. But they are not, and, unfortunately, neither are we. So I don't mean to imply that

complex systems can predict crashes. They probably can't. But they can provide an alternative lens to enable us to design rules, laws, incentives and institutions, as well as encourage the development of productive social norms, and it might reduce the likelihood and severity of financial collapses.

Complex systems also are analyzed using computational, what are called "agent-based techniques." This was mentioned. These techniques are capable of including sector-level details: financial markets, real estate markets and service markets. The ability of complex-systems models to include realistic detail creates the potential for new insights into causes and rates of innovation. So, for example, from a complex-systems perspective, the innovative potential of an economy depends on its building blocks: the ideas, technologies and basic science that sits out there that people work with. So innovation comes about by combining and recombining those building blocks.

Lest I make agent-based models seem like a panacea, I should add a word of warning: A model that contains too much detail can be as perplexing as the real world it was built to explain. Models should only include so much detail as necessary and no more. So it is an open question what necessary detail should be included in models of the economy, but I believe that the financial sector, unemployment and heterogeneous consumers probably fit the bill.

So to sum up, our goal is to understand an economy that is increasing in complexity. The neoclassical approach emphasizes optimization in the face of constraints. The complex-systems paradigm emphasizes diversity, networks, interdependencies—positive as well as negative—and adaptation.

So let me conclude with my first point. For non-complex systems, we can use single models. We can, for example, just multiply an object's mass by its acceleration and get a really good approximation of force. But if you have a complex system like an economy, no one model will likely work. We need a crowd. We actually need a crowd of diverse models.

I thank you for this opportunity to speak to the committee.
[The prepared statement of Dr. Page follows:]

PREPARED STATEMENT OF SCOTT E. PAGE

I thank you for this opportunity to address the committee.

My name is Scott E Page. I am the Leonid Hurwicz Collegiate Professor of complex systems, political science, and economics at the University of Michigan-Ann Arbor and an external faculty member of the Santa Fe Institute. I study diversity in complex social systems.

Complex systems may be unfamiliar territory to many, so I begin with a definition. Complex systems consist of diverse, connected, interdependent, and adaptive actors who collectively produce patterns that are difficult to explain or predict.¹ Complex systems are neither ordered nor chaotic. They lie in between.

Complex systems interest scientists because they are capable of producing emergent phenomena in which the whole differs in kind from the parts that comprise it. A brain differs in kind from a neuron. A society differs in kind from a person.

Given this definition, the economy, traffic on the Beltway, and the goings on "inside the Beltway" are all complex. Trying to make sense of and harness the complexity of the social world is what motivates my research efforts.

In my comments today, I first describe how, when we're confronted with complexity, we benefit by relying on a variety of models. I then show how complex sys-

¹Miller, J. and S. Page (2008) *Complex Adaptive Systems: An Introduction to Computational Models of Social Life*, Princeton University Press.

tems models, by including the diversity and interconnectedness of the economy, have a special ability to generate insights into phenomena of central interest to this committee, including the pace of innovation and market crashes.

My points both relate diversity to complexity. First, I'm saying that the economy is complex, not in some loose metaphorical way, but according to formal scientific definitions of complexity. As a result, we're never going to predict its future with much accuracy. Our best approach will be to encourage the creation of diverse models.

Second, I'm saying that we need to develop richer complex systems models of the economy because they embrace the diversity and interconnectedness that drive fluctuations, and because they may enable us to gain deeper insights into the causes of innovation. I'll argue that these models are much more flexible than standard neoclassical models.

I begin with a simple question: Why model? A standard response would be that models enable us to explain and predict empirical data—to make sense of the world. Models vary in their accuracy depending upon the domain. For example, in predicting physical phenomena—the rate at which objects fall, the patterns in which the planets orbit the sun, and so on—they're almost absurdly accurate.

Yet, as we all know, models have proven less adept at predicting the economy. That's because the economy is a complex system. The solar system may be complicated, i.e., have lots of connected parts. But the parts aren't that diverse, and they don't adapt. Hence, planetary orbits are predictable.

Prediction is only one of many reasons to encourage model building and interpretation. Models help us design policies and mechanisms. For example, the FCC spectral auction provides an excellent example of how models were used to anticipate shortcomings of traditional auction mechanisms.

Models also inform data collection, produce bounds on outcomes, explore counterfactuals, and explain whether a system will equilibrate, cycle, produce chaos, or generate complexity.

And perhaps most importantly, models help us identify the important parts and work through the logic of systems, especially complex, unpredictable systems like the economy or political systems.²

The complexity of the economy provides almost endless grist for our cognitive mills. An inquisitive person's head cannot help but develop theories and construct analogies about the economy. Many of these contain a grain of truth. Unfortunately, most also include logical inconsistencies.

The advantage of models is that they identify truths and reveal inconsistencies by forcing us to characterize the relevant parts of a system and to understand how those parts relate to one another.

However, when applied to a complex system, a single model can only cast light on some dimensions. Hence, we need multiple models. The advantage of combining diverse models was recognized by Aristotle, who asserted, "a multitude is a better judge than any individual."³

That's not just an intuition. With the help of a little mathematics, the claim can be made formal: My research has shown that if I have a crowd of models and take the average, then it follows that

Crowd of Models' Accuracy = Average Model Accuracy + Model Diversity.

The mathematical identity that I've framed verbally here shows the benefits of combining models.⁴ I want to reiterate that by no single model or even a group of models will accurately forecast the economy. It's too complex.

We can widen our lens a bit, though. And we can use a crowd of models to predict bounds on the likely fluctuations in the economy and to anticipate unintended consequences of policy decisions, such as allowing the expansion of sophisticated financial instruments.

I now turn to my second point: the particular value of complex systems models to help understand and guide the economy.

The U.S. economy consists of over three hundred million people, nearly thirty million organizations—about ninety percent of which seek profits—and tens of thousands of government agencies. These actors possess diverse beliefs and goals. They adapt as circumstances change, though not in lock step. Some spend and some save. Some innovate. Some seek the comfort of routine.

² See Bednar (2009) *The Robust Federation*, Cambridge University Press.

³ Aristotle, *Politics* (trans. Benjamin Jowett), Book Three, Part XV, available at <http://classics.mit.edu/Aristotle/politics.3.three.html>

⁴ See Page, S (2007) *The Difference: How the Power of Diversity Creates Better Groups, Firms, Schools, and Societies*, Princeton University Press.

The aggregated interdependent actions of these millions of actors—people, organizations, and governments—produce the macroeconomic patterns that economists seek to explain and predict.

How then, do we model this? The neoclassical approach assumes that individuals and firms make optimal choices subject to constraints on budgets, technology, and time. Both sets of actors accurately anticipate future effects of their actions and the government. In its simplest form this model produces a stable equilibrium with balanced growth.

Modern variants of this model include technological shocks that reverberate through the economy. These variants also include frictions, such as wages that are slow to fall. This stickiness exacerbates the depth and length of the echoes caused by the shocks.

The neoclassical model is stark. It assumes no sectors of the economy, no physical geography, no networks of connections, no learning (agents always optimize), and little or no heterogeneity of income, wealth, or behaviors.⁵

Oh yeah, and the only unemployment it includes is voluntary.

Further, almost all of the responses by the actors in the neoclassical model tend to equilibrate the system. An increase in demand for housing increases the price of housing, thereby causing a reduction in future demand for housing. This is an example of a “negative feedback.” Negative feedbacks stabilize systems and lie at the core of neoclassical economic models.

The complexity approach assumes individual agents with diverse incomes and abilities who are situated in place and time. Their actions influence those in their social and economic networks. These actors don’t optimize some hypothesized objective functions, be it a single period’s profits or lifetime’s income. Instead, they follow rules that have survived or are succeeding in the marketplace.

In a complex systems model, if financial firms with greater leverage are making higher profits, other firms may follow their lead even if the aggregate effect of all that leveraging is not sustainable.

This sort of effect—in which more leverage leads to even greater leverage—is called a “positive feedback.” Positive feedbacks produce correlation in observed behavior. Hence, systems that contain them can exhibit both clustered volatility and large events, for instance, stock market bubbles and home mortgage crises. These could be avoided if the agents in the model were capable of predicting the future consequences of their actions, but they are not. Neither are economists.

I do not mean to imply that complex systems models can predict crashes. They cannot. What they can do is provide an alternative lens to enable us to design rules, laws, incentives, and institutions—as well as encourage the development of productive social norms—that might reduce the likelihood and severity of financial collapses.

Adopting complex systems models requires a change in tools as well as a change in paradigm. Complex systems models are often analyzed using computational or what are called “agent based” techniques. These techniques are capable of including sector level details—financial markets, real estate markets, and service markets.⁶

The ability of complex systems models to include realistic detail has other advantages as well. It creates the potential for new insights into causes and rates of innovation. The innovative potential of an economy depends on its building blocks—ideas, technologies, and basic science. Innovation comes about by combining and recombining those building blocks.

Lest I make agent based models seem a panacea, I should add a word of warning. A model that contains too much detail can be as perplexing as the reality it was built to explain. Models should include only as much detail as necessary and no more.

In 1922, Georgia O’Keefe wrote that “details are confusing. It is only by selection, by elimination, by emphasis that we get to the real meaning of things.” She was right. That’s why standard macro models, which leave out so much information, can still be of great value. However, I would argue that to get at the real meaning of things in the economy, the necessary details should include the financial sector, unemployment, and heterogeneous consumers.

To sum up, our goal is to understand an economy that’s increasing in complexity. The neoclassical approach emphasizes optimization in the face of constraints and re-

⁵Narayana Kocherlakota, the President of the Minneapolis Fed, has written that “as far as I am aware, no central bank is using a model in which heterogeneity among agents or firms plays a prominent role”. Kocherlakota (2010) “Modern Macroeconomic Models as Tools for Economic Policy,” Federal Reserve Bank of Minneapolis.

⁶Farmer, D and D. Foley (2009) “The Economy Needs Agent Based Modelling” Nature 460: 685 686/

sponses to shocks, and sees macro level patterns as the re-equilibration of those shocks. The complex systems paradigm emphasizes diversity, networks, interdependencies (positive as well as negative feedbacks), and adaptation. Neither is right. Neither is wrong. They're both models. And both can be useful.

I'll conclude by reiterating my first point. For noncomplex systems, we can use single models. We can, for example, just multiply an object's mass by its acceleration to get a really good approximation of force. But for a complex system, like an economy, no one model will be accurate. We need a crowd, a crowd of diverse models.

I thank you for the opportunity to speak to the committee.

Chairman MILLER. Thank you, Dr. Page.

Dr. Chari, you are recognized for five minutes.

**STATEMENT OF V.V. CHARI, PAUL W. FRENZEL LAND GRANT
PROFESSOR OF LIBERAL ARTS, UNIVERSITY OF MINNESOTA**

Mr. CHARI. Thank you, Mr. Chairman.

It is an honor and a privilege to testify before this Committee. Let me begin with a disclaimer. Nothing I say here should be construed as reflecting the views of the Federal Reserve Bank of Minneapolis or the Federal Reserve System.

I want to make three points in this testimony. The first point is that macroeconomic research is a very big tent which accommodates a very diverse area of perspectives and is open to lots of different ways of thinking about the economy. The second issue that I want to raise is, why did the current generation of so-called DSGE models—there is not one, there are many—fail to see the crisis coming and what should macroeconomic research look like going forward in order to forestall future crises? And the third is, what can the public and Congress do to foster the kinds of macroeconomic research that is needed to ensure that we don't have catastrophes like the events of the last couple of years?

First, in terms of macroeconomic research: Macroeconomic research, as I said, is a very big tent and accommodates a very diverse set of viewpoints. There is a shared language and a shared methodology but not necessarily a shared substance in terms of policy issues. This openness and flexibility is best summarized by an aphorism that macroeconomists often use: "If you have an interesting and a coherent story to tell, you can do so within a DSGE model. If you cannot, it probably is incoherent."

Now, macroeconomic research has changed a lot in the last 25 years, and I want to emphasize the nature of that change and I believe that much of that change constitutes progress. The state-of-the-art DSGE model in, say, 1982 had a representative agent, no unemployment, no financial factors, no sticky prices and wages, no crises, no role for government. What do the state-of-the-art DSGE models of today look like? They have heterogeneity, all kinds of heterogeneity arising from income fluctuations, unemployment and the like. They have unemployment. They do have financial factors. They have sticky prices and wages. They have crises. And they have a role for government.

Given the limited amount of time, let me talk about financial factors. The best way of thinking about the important developments in theorizing about financial factors is to think about the career and accomplishments of Ben Bernanke. We have often heard the statement summarized: "Academic macroeconomists who are interested and active in policy routinely write down models with no role

for financial factors.” Nothing could be further from the truth. Ben devoted his career to developing such models. Was he a bit player, a heterodox person way outside on the sidelines of modern macroeconomics? No, he was chairman of Princeton’s economics department. He was right at the center, the heart, of macroeconomic debate and issues and improving models.

Second, let me talk about crises. Now, our brothers and sisters in international macroeconomics who study the economies of other countries have for the last decade or more been routinely developing DSGE models, quantitative DSGE models with crises. Why? Because they have been studying countries that have been routinely buffeted by these kinds of crises, so it is natural for them to study them. How about a role for the government? Kareken and Wallace in the late 1970s emphasized that deposit insurance together with government bailouts possibly creates strong private incentives for excessive risk-taking, and they emphasized the importance of government regulation in order to prevent these incentives for excessive risk-taking from going overboard. So macro models are very different from what they were. They can analyze a wide variety of policies. They are being used, particularly by central banks to guide monetary policy. They have been used in policy circles to analyze questions of fundamental tax reform, social security reform, and I believe they can and should be used for other policies and questions.

But all is not fine and dandy. Clearly, this class of models failed to see the crisis coming. Why? I offer three reasons. First, any model has got to be disciplined by historical data. That is a necessity. Now, modelers of the U.S. economy naturally tend to focus on the experience of the last 60 years, particularly of the United States. What has the experience of the last 60 years been? Well, relative especially to other countries, it has been remarkably stable except for the recent crisis, and so, in that sense, those kinds of models naturally tended to deemphasize these kinds of financial crises.

The sad thing about this is that, as I said, there are people in international macro writing down models, quantitative DSGE models, of crisis. What should we have done? We should have incorporated their insights. Why did we not? It is natural. Whenever I read a paper about, say, Argentina, I am tempted to say, “Oh, well, that is Argentina, we are in the United States, it can’t happen here.” What we have learned is, it can happen here and it is clear going forward that we need to incorporate those kinds of insights. It is clear going forward we need to incorporate the insights from the banking and deposit-insurance literature on incentives to take on excessive risk. Those are elements that were thought unimportant, they clearly are not unimportant. They are within our tool kit and can be used.

Final issue: What, if anything at all, can the public and Congress do about this? It is useful to put some numbers on the table here. NSF funding for economics overall is roughly \$27 million. Two point six million dollars of that goes to the PSID, a very worthwhile activity. About ten percent of the remainder goes to fund, in my judgment, my estimation, macroeconomic research, so we are talking about \$2.5 million. Now, compare \$2.5 million with the

NSF's budget of roughly \$7 billion and an overall basic research budget for the Federal Government of the order of about \$30 billion, and so we are talking about less than peanuts. We are talking about a tiny amount of money.

Now, would investing additional resources in macroeconomic research of the kind that is being practiced in the best universities and the best research departments across the Nation add substantially to our welfare? In my judgment, yes. All we have to do is reduce the probability of the next crisis by 100th of one percent, and if we quadrupled the amount of resources to NSF's macroeconomics research program, it would pay for itself tenfold. That is the kind of return that we are talking about. Now, can that return be realized? Not for sure. No one can offer guarantees, but I think that the odds are that we have got a bunch of very smart people, capable people who are open to diverse ideas. They can do it.

So let me conclude by trying to summarize three basic messages. First is a message to critics: These are not your father's models. These models are very different from the descriptions that critics often offer of these kinds of models, and so it is not helpful to advance the debate on the future of modern macro by caricaturing models from a generation ago. Message to my fellow researchers: Yes, the United States is not Argentina but we have a lot to learn from the experiences of other countries.

Third, the message to the public and Congress is that macroeconomic research of the kind that is being practiced at leading departments offers a very gigantic bang for the buck. Thank you.

[The prepared statement of Dr. Chari follows:]

PREPARED STATEMENT OF V.V. CHARI¹

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.

¹The views expressed herein are those of the author and not necessarily those of the Federal Reserve Bank of Minneapolis or the Federal Reserve System.

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 (often called a DSGE model). From this perspective, there is no other game in town. Modern macroeconomic models, often called DSGE models, all 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 heterogeneity 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 Ghanian 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 de-

regulating 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.

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 percent? 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.

2. Why did we not see the crisis coming and what should be done?

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 percent 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.

Chairman MILLER. Thank you, Dr. Chari.
Dr. Colander, you are recognized for five minutes.

**STATEMENT OF DAVID C. COLANDER, CHRISTIAN A. JOHNSON
DISTINGUISHED PROFESSOR OF ECONOMICS, MIDDLEBURY
COLLEGE**

Mr. COLANDER. Thank you very much for this opportunity to testify.

I am known in the economics profession as the economics court jester because I am the person who says what everyone knows but everyone knows better than to say in polite company. As a court jester, I see it appropriate to start my testimony with a well-known joke, a variation of a well-known joke. It begins with a Congressman walking home late at night. He notices an economist searching under the 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 over 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." That well-known joke is told by critics of economists a lot because it captures economists' tendency to be highly mathematical and technical in their research. On the surface, searching where the light is is clearly a stupid strategy. The obvious place to search is where you have lost the keys.

However, that in my view is the wrong lesson to take from this joke. I would argue that for scientific research the searching-where-the-light-is strategy is far from stupid. Where else but in the light can you reasonably search to find your keys or figure out in a scientific way what the system is? What is stupid is if the person who is there searching thinks he is going to find the keys under the lamppost. Searching where the light is only makes good sense if the search is not to find the keys—that is, to come up with practical policy recommendations based directly on models—but rather to expand theoretical knowledge: to understand the topography of the illuminated land and how that lighted topography relates to the topography of the dark, in the dark, where the keys were lost.

Most top economic theorists I talk to know that it is stupid to directly search for policy keys in the light. Then why do they often let people assume that is what they are doing? Because they believe that if they didn't appear to be doing so, they wouldn't get funded. The reality is that funders of economic research, such as NSF, all too often want immediate policy answers from abstract scientific models. Researchers respond to incentives, and if the researcher's livelihood is dependent on drawing policy conclusions from abstract formal models, they will do it. So if the economist had answered the Congressman honestly, he would have told him, "I am searching for the keys here because that is where you are funding me to search."

Keynes once said that policymakers are the slaves of some defunct economist. Economists like that story, by the way. To make the story complete, however, what he should have added is that, in turn, economists are the slaves of some defunct policymaker who established a funding system for research. The incentives inherent in that funding system play a central role in the kind of research that gets done.

The reason I am testifying here is I believe NSF can take the lead in changing the institutional incentive structure by implementing two structural changes in NSF programs funding economics, and I think these will change economists' incentives. The first proposal involves making diversity of the reviewer pool an explicit goal of reviewing process of NSF grants in social sciences. This would involve consciously including what are dissenting economists as part of the peer-reviewing pool, as well as reviewers outside of economics, such as physicists, mathematicians, statisticians and individuals from business and government who have real-world experience. They could put some sense of what Professor Solow said: "Does it pass the smell test?" Such a broader peer-review process would likely encourage research on a much broader range of models, promoting more creative work and providing that commonsense feedback from the real world that you need to figure out whether the topography of the models fits the topography of the land that you are trying to search for in the dark.

The second proposal involves increasing the number of researchers trained in relating models to the real world as opposed to just producing models. This can be done by explicitly providing some 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 economics component. This division would fund work on analyzing which of the many models are there being developed are appropriate for the real world.

The applied science work would involve a quite different set of skills than the standard scientific economics research requires. It would require researchers to have a solid consumer's knowledge of economic theory and econometrics but not necessarily a producer's knowledge of that. You often find that people who can be fantastic at producing models are not very good at interpreting them and relating them to the real world, and you can have more specialization than what we have. In addition, it would require a knowledge of institutions, methodology and previous literature as well as a sensibility of how the system works, and I think, you know, there are definitely economists who have that. Ben Bernanke I think does, Alan Blinder. But interestingly, when they were at Princeton, they weren't the one teaching macro theory: And when I talked to students there when I taught there, they said, "Oh, we wouldn't take it from him; that wouldn't prepare us to write articles." So they taught undergraduates as opposed to the graduates, and that to me is crazy.

The skills involved in interpreting models are the skills that are currently not taught in graduate economic programs. By providing grants for interpretive work, the NSF would encourage the development of a group of economists who specialize in interpreting models and applying models to the real world, making it less likely that fiascos like the financial crisis would occur. Thank you.

[The prepared statement of Dr. Colander follows:]

PREPARED STATEMENT OF DAVID COLANDER

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 is 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—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 U.S. 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.

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 "sensitivity test" that relates the topography illuminated

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

²Nassau 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)

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.

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 inter-related 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

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.

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 McGrattan, 2009, pg 243). Despite their agreement that their model does not fit the data, they are willing to draw strong policy implications from it. For example, they write “discretionary policy making has only costs and no benefits, so that if government policy-makers 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 U.S. 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 U.S. 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, 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 econ-

⁴For 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 1930s 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.

⁵Keynes 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.”

omy 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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Chairman MILLER. Thank you, Dr. Colander.

I now recognize Dr. Broun for a motion.

Mr. BROUN. Thank you, Mr. Chairman. I ask unanimous consent that Ms. Biggert, a member of the Full Committee, participate in this Subcommittee as if she were a member of this Subcommittee.

Chairman MILLER. And my colleague on the Financial Services Committee. Without objection, that is so ordered.

Mr. BROUN. Thank you, Mr. Chairman.

Chairman MILLER. We will now begin with questions, the first round of questions, and I now recognize myself for five minutes.

Dr. Chari, you testified that 30 years ago the models assumed no government action could improve things, and that was no longer the assumption, but there seems to be some disagreement among the panel about the extent to which policy can improve things. My own experience in financial crisis as a policymaker has been very much with a narrow, micro kind of point of view. When I was first elected to Congress in 2003, the advice I got was that most Members of the House—unlike the Senate, where they can hold forth on all matters—that most Members of the House labored in obscurity and I should pick some technical issue no one cared about or was paying any attention to. I would probably never be heard from again, but if I picked an issue that there was no one from my party with my point of view who had already claimed that issue, I would be doing useful work. And the issue I picked was mortgage lending.

My experience in dealing with mortgage lending was that the loans, the individual loans, were horrific. Dr. Solow talked about the assumptions that there were no conflicts of interest and no lack of information, no lack of knowledge; and middle-class homeowners and, really, subprime mortgage lending was not to purchase homes, it was people who owned homes and needed to borrow money. They were refinances overwhelmingly. They were handed a sheaf of documents, small print written by the bank's lawyer—by someone

else's lawyer, not by their lawyer—and they were getting advice from a mortgage broker who was actually being paid by the lender in addition to what they were being paid by the borrower, and were getting paid more by the lender the worse the loans were for the borrower. And the borrowers, the homeowners, were relying upon that advice and being told that “this is very complicated, I will help you through this, I am a mortgage professional.” Almost everything I said should be changed that with the argument, “Oh, that will result in unintended consequences,” and I would say, “What kind of unintended consequences?” “Well, we don't know, that is the point: they are unintended.” And it kind of became an epistemological test, you know: How could you possibly do anything without knowing that there would not be unintended consequences?

Do all of you agree that the models do now assume that there is government action, that government action can help? And how do we overcome the concern about unintended consequences, God only knows what they are? Any of you? Dr. Chari, since you were the one who said 30 years there has been a change from your father's economic models?

Mr. CHARI. Sure. Here is one way of illustrating the nature of that change: Earlier this month, the Society for Economic Dynamics, a hotbed of DSGE-style modeling, held its meetings. About 400 papers were presented. I flipped through the program. About 50 of those papers dealt with policy in macroeconomic models. Guess what? In none of the 50 was the best role for the government to get out of the way and stay out of the way. In every one of those 50 papers, every one of them analyzed ranging from monetary policy to fiscal policy to innovation, all across the line, they all had a role for policy. In terms of sort of thinking through the mortgage market, I think it is really important to understand that if you look at the people who held the debt issued by major financial intermediaries, ex post—that is now, after all these events—they have suffered very small losses, primarily because of various bailout programs. These debts that were issued by major financial intermediaries were backed in substantial part by subprime and other kinds of mortgages. It is rational in a world like that that the people who issue subprime mortgages will pay very little attention to the risk characteristics of the borrowers. I am not saying that is the only factor, but that is an important factor. Understanding the importance of that factor has obvious implications for the nature of financial regulation going forward. That is the kind of insight that comes out of thinking through a model in which even if everybody counterfactually behaves very rationally, you can create very perverse incentives, but you can create very bad outcomes and you can create an important role for government policy.

Chairman MILLER. Any of the others want to address the role of government under any of these models or whether—to the extent of which the models assume that government can play a useful role? Dr. Solow, you certainly touched upon this as you did, Dr. Winter. Dr. Solow?

Mr. SOLOW. Yes. Thank you. I have a lot of empathy for Dr. Chari and what he is trying to explain. This is very difficult to do. It is easy to say “you should include this aspect of reality, you

should include that aspect of reality.” To try to do it in a logically tight way is extremely difficult. But when he says that the more recent vintages of DSGE models have a role for government or allow for unemployment, then I think that is really a little misleading in the sense that, if you were to look closely at a DSGE model with unemployment and ask how does unemployment happen and what does it mean, it wouldn’t be the kind of unemployment that you see in your district, for instance. It wouldn’t be the case where there are workers who are unemployed workers competent to do a job because they did it six months ago or a year ago, prepared to work for a little less than the going wage, and no one will employ them because there is no market available for their output. Instead, if you read the pages on unemployment in a DSGE model, it is full of explaining little glitches in the labor market, little inefficiencies here and there, and there is a tendency to underestimate the cost.

Similarly, with the role of government, I said those models have lots of room for government. What the government should do is try to make the world more like the neat model by eliminating inflexibilities and rigidities and elements of imperfection and whatnot. The notion that the government might—when there is unemployment and excess capacity because there is not enough demand for goods and services to employ the whole economy at a reasonable level—that the government should try to find ways to fill that gap. That doesn’t appear even in recent-vintage DSGE models because there is no gap, there is not a gap of that kind.

Chairman MILLER. Thank you, Dr. Solow.

Dr. Winter, do you feel moved to—

Mr. WINTER. Yes, I would like to just comment briefly on what Dr. Chari said. I think in the attempt to understand what happened in the financial crisis, we should recognize that there are different lines of explanation for the behaviors we see up and down the system, and then Wall Street in particular. Enormous losses were inflicted on Wall Street firms and enormous personal losses on some Wall Street players, and there is a question of whether that happened because they didn’t understand the system that they had participated in creating or whether they were in some more rational way responding to the incentives of the system that presented themselves.

So that, in fact, is, I think, an important research question, you know: What exactly was the basis for the kinds of decisions that created these enormous financial vulnerabilities? And there are voices out there which I consider to be credible voices that say, basically, Wall Street confused itself in the end and created a system which it in turn did not understand at all. Now, I don’t know what the right answer is, but it is a very important question, and what I would argue for in the domain of economic research is that we try to do better at resolving some of those questions on a factual basis when they turn up. There is a whole list of questions like that about the financial crisis which could be investigated with high academic standards and systematically. It would tell us a lot about how the system failed us.

Chairman MILLER. Dr. Page.

Mr. PAGE. Yeah, I guess one quick comment, and this gets to, I think, some of the stuff David had said and also Chari as well. I agree that these DSGE models are powerful models and they have definitely changed over the last 20, 30 years, but there is this fundamental question of how do we think of the economy. So if I give you one word to describe the economy and you could choose between “equilibrium” or “complex,” you would probably vote “complex,” right? But yet where the streetlight is, as David was saying, and where we have built up all this knowledge, is in these equilibrium models. Now, there has been attempts, and I think they have been extremely successful, to introduce, you know, volatility. So there is this notion there is a shock to the equilibrium, and then, because of frictions and heterogeneity, that shock echoes through the system creating these complex patterns. But this equilibrium mindset, I think, can be complemented by a mindset that instead thinks of the economy as something we are probably never going to understand, but we will see that different sets of policies create different types of incentives, creating certain types of positive and negative feedback. So we did see this giant rush.

If you are just monitoring the economy and you don't think it is perfectly working and you suddenly see this huge rise in refinancing, a little bell should go off and you should say, “Let us think through the repercussions of this thing.” And what the finance people would have told you, they would have said, “Look, we are bundling all this risk, it is all going to be fine.” And they would have said looking back that—you know, past data—“this bundling is going to work.” But then you realize you are placing a lot of faith on a particular assumption about bundling that in fact didn't hold true. So I think that there is a fundamental question of this notion between do we think of the economy as an equilibrium? And if you do, then you are imposing a lot of logical coherence; and if you do want that logical coherence, then you are sort of stuck with something like DSGE.

There is an alternative approach which is based more on sort of complexity theory, which is not as advanced, which thinks of the economy constantly in flux. And one of the things that has been great about the NSF, I should say, is they have funded a lot of this very exploratory research into complex systems, right, to try and create alternative models of the economy.

Chairman MILLER. We have now gloriously exceeded my time, but Dr. Colander, you appear to be longing to address this question as well.

Mr. COLANDER. You know, I would like to reiterate, you know, what Scott just said, that is, really is, the point. DSGE models are wonderful and you can expand them and everything else, they are impressive, but they are one particular point of equilibrium that you are looking at. Then you are relating it to this world out there, which is extraordinarily complicated, which has this complexity, this diversity going on. And, yes, you can squeeze and push these DSGE models to make them explain things, but it is like telling people here, “yes, we can get a little roughness in the topography,” when there is actually a gigantic cliff, or there might be. You need a variety of other models. Now, I am not saying I know what other models are there, and the emphasis is that one needs a lot of diver-

sity within mainstream macroeconomics, and I consider myself a mainstream macroeconomist too but, you know, within that sense, everyone knew, and I think Dr. Chari has written, you have to start with a DSGE as your foundation so you have to start from this point and then move out as opposed to allowing you to search the entire lighted area and that just hasn't happened.¹ And that, I think, is what Dr. Solow is saying. There is nothing wrong with DSGE models, but there is a lot of topography out there, and we need more of that diversity—and, somehow, within the way academia works, it has not allowed that to happen. And that, I think, is sad.

Chairman MILLER. My time has expired, and I apologize to the other members of this Committee, and I will try to be reasonably lenient with others' time as well.

Dr. Broun is recognized for five minutes.

Mr. BROUN. Mr. Chairman, I ask unanimous consent that we let Ms. Biggert go out of order.

Chairman MILLER. Without objection.

Ms. BIGGERT. Thank you so much, Mr. Chairman and Ranking Member Broun. I had to come to this Committee because Dr. Solow was here, and I just wanted to say that he is my hero. I have been a longtime member of the Science Committee and a strong proponent of research and development, and so I have frequently, probably very frequently, reminded my colleagues of the importance of research and development, of the scientific and technological investments in the future of our economic competitiveness and security. So one of the ways that I have done this is always to remind them that science-driven technologies accounted for more than 50 percent of the growth of the U.S. economy in the last half-century, and this was a quote that I always used from Dr. Solow. So I really appreciate being here, and all of you, this has been a very interesting discussion.

And I am hope I am not straying off of the subject too much, but I would like to ask Dr. Solow about the factors that you see contributing to the U.S. growth in the first half of this century. And do you believe that scientific and technological investments will continue to contribute at the same level now that we are in another century?

Mr. SOLOW. Thank you. I don't know how a hero is supposed to respond. Before I taught at MIT, I was a Technical Sergeant in the U.S. Army, and if you just call me Sarge, I will settle for that. That is the way I spent my youth.

I do think that science and technology—first of all, let me say I don't believe that the current crisis and the long recession will fundamentally impair the long-run growth potential of the U.S. economy, although it is going to take a long time to shake off those effects. But they remain there because, just as you said, the basic

¹For the purpose of clarification, Dr. Colander has requested that his testimony from "Now, I am not saying I know. . ." in this paragraph to ". . .that just hasn't happened" be corrected to read as follows:

"Now, I am not saying that I know what the other models are. What I am saying is that one needs a lot of diversity within mainstream macroeconomics. I consider myself a mainstream macroeconomist too but do not believe, as Dr. Chari has written, that you have to start with a DSGE as your foundation. I believe mainstream economists should be out there searching the entire lighted area, and that just hasn't happened."

sources of that growth are in innovation of various kinds. What I think may have changed for the longer run is where in the economy that innovation takes place. We may have suffered from an excessive innovation in the financial services sector of the economy. I love Paul Volcker's remark that the best financial engineering innovation of all was the ATM machine. But I think, for instance, that the Committee possibly ought to think that if from now on—I suspect to be true—that the weight of the service sector in our economy is permanently bigger than it was in the first half of the 20th century or even in the second half of the 20th century, what does that say for the character of innovation that can affect the economy? I used to tell students and others that services are just like goods, the only difference being you can't inventory a service. I can't get three haircuts so that I don't have to go back again. But there may be differences in the way, in the kinds of science, the kinds of innovation that generate productivity in the service sector. There may be differences in the reception that service-sector firms can give to technological innovation, and I think that is a good subject for the Committee on Science and Technology to pursue.

I have no doubt that those potentialities for long-run growth or productivity are still there. During the buildup of the great stock of computers in the United States, most of those computers were being bought in the service sector: in retail and wholesale trade, in financial services and elsewhere. And some service sectors exhibit very rapid growth in productivity, but that is the kind of long-run change in the economy that may affect the role of science and technology. But that role remains fundamental for growth, that it is the generator of long-term growth I think is undoubtedly still true. Thank you.

Ms. BIGGERT. Thank you very much, Sarge. Now I have to return to Financial Services, where there is another hearing going on and I am due to be there. Thank you very much.

And thank you again for your indulgence.

Chairman MILLER. Thank you, Ms. Biggert.

I now recognize Ms. Dahlkemper for five minutes.

Ms. DAHLKEMPER. Thank you, Mr. Chair, and thank you all for your testimony today. It is a fascinating subject. I am a new Member of Congress, so it has been truly a fascinating time to join this wonderful body and I appreciate all of your expertise.

I wanted to ask you: I come from northwestern Pennsylvania, an area actually that has been suffering economically for a long time; but the Nation's current number of long-term unemployed is estimated at 6.8 million jobless, certainly a number that we have never seen before. So what do macroeconomics or any field or subfield have to say about the effects of the economy of extending unemployment benefits for these people versus just simply letting them fall off the rolls? And I don't know who might like to address that. Dr. Winter? I don't know. Maybe not. Dr. Chari? Who would like to—whoever would like to—

Mr. CHARI. Since you are from northwestern Pennsylvania and I received my Ph.D. in economics from Carnegie Mellon, I suppose I should be the person to try and help out. Let me just say one thing before I start. Professor Solow is not just a hero to Represent-

ative Biggert. He is a hero to all macroeconomists, modern or otherwise.

So the kinds of models of unemployment that people have written down, most notably the kinds of models that Mortensen and—Dale Mortensen at Northwestern and Chris Pissarides at the London School of Economics—have written down and a whole bunch of other people have written down emphasize the key tradeoff. The tradeoff is that we don't have very good insurance markets to protect people when they do get unemployed and so therefore there is a role for government policy in providing unemployment compensation because those markets are missing. The tradeoff is that providing unemployment benefits does tend to discourage people from looking as intensively for jobs. That is one effect. The second effect, which I think research has demonstrated is much more important, is that it tends to make them more unwilling to accept jobs when they do come up, and so that is the tradeoff. Now, making this decision requires understanding the quantitative assessments of those kinds of tradeoffs. Those quantitative assessments suggest that in times of relative prosperity, Congress has sensibly decided not to extend unemployment benefits for too long. In times of greater difficulty, Congress has done it.

So we have models that give us numbers. I don't have them immediately offhand, but we can certainly talk about it and I can communicate those kinds of numbers. But the ultimate decision about the size of those tradeoffs, how important is it to protect people from prolonged periods of economic hardship versus the effect on their own incentives, is something that Congress has to make the difficult decision on. The guidance that modern macro kinds of models can offer is a sense of the quantitative magnitudes of the tradeoffs. Both those effects are present. That is what the research seems to demonstrate fairly unequivocally, and so we need to balance those effects. But I don't have immediately offhand the exact numbers that—the latest version, of course, they are going to differ from model to model.

Ms. DAHLKEMPER. Would anyone else like to address that? Dr. Winter, do you want to comment? Dr. Solow, after Dr. Winter. Dr. Winter.

Mr. WINTER. Okay. Well, I just want to say again that in that area, there is a lot of rhetoric that goes around in the economics discipline about the causes of unemployment and the role of the incentives provided by unemployment insurance—and, unfortunately, I don't think this rhetoric moves forward very much over the years. It could be improved by closer study of these phenomena through research, and that would be a valuable thing to do. But on the face of it, it seems to me that, when you have the very dramatic changes in levels of unemployment that we have experienced and when you have the regional diversity that we have in levels of unemployment, that it is quite implausible that they are somehow fixed considerations of human nature underlying the phenomena that you are looking at. I think that people become unemployed because of circumstances beyond their control to a very large extent.

So I think again if there is doubt about that, and sometimes one hears comments that suggest there is doubt about it, I think it can be looked into carefully and should be.

Ms. DAHLKEMPER. Dr. Solow, would you like to comment?

Mr. SOLOW. Thank you. I just wanted to add to what Sidney just said, with which I agree, that if we simply look at northwest Pennsylvania, your area, there are a lot of long-term unemployed people for a number of reasons—for at least two big reasons, none of which has to do with a matter of refined incentives that are provided to them by the unemployment insurance system. One is that in every recession that follows a financial crisis, there is a tendency for unemployment to be very prolonged because the blow to business confidence and the blow to the confidence of lenders in the creditworthiness of businesses is such as to make businesses unwilling to make long-term commitments to employment. But secondly, western Pennsylvania is not an area full of rising industries, so that long-term unemployment, I suspect, has been a problem there that is antecedent to the financial crisis.

I think one has to ask, what are in fact the—and no simple model is going to do this for you—you have to ask, what are the possibilities for northwest Pennsylvania or for any other particular region of the country? What are the development possibilities that are there? If there are development possibilities, those are the things to pursue. And, in the meantime, I think it is simply a matter of humanity to support the 50-plus-year-old workers who clearly are not laying down on job search because they are getting UI benefits. They are not searching so actively because they know damn well there is nothing there to find. But one ought to focus, a) on the economic development of the region, and b), once that is in hand, or in the head at least, on preparing the labor force of the area for whatever kind of development seems promising. But looking at piddling little incentives, I think, is not going to help us at all.

Ms. DAHLKEMPER. Thank you. That is sort of the sense I get from talking to people in my district all the time.

Thank you very much. My time is expired.

Chairman MILLER. Thank you, Ms. Dahlkemper.

Dr. Broun is recognized for five minutes.

Mr. BROUN. Thank you, Mr. Chairman.

The famous philosopher Voltaire once said that “the perfect is the enemy of the good.” We use models such as weather forecasts every day in society knowing that they are not guaranteed to be correct all the time but accept this based on the importance of the utility of the information that the models produce.

Dr. Solow in his testimony argues that “the DSGE model has nothing useful to say.” I hope I quoted you correctly. I think so. Should the economists throw away the DSGE model approach outright or cautiously use the model with the understanding that there are limitations and shortcomings? For the panel. Dr. Solow, to begin with.

Mr. SOLOW. I think it is the latter. First of all, a lot depends on what you mean by the DSGE approach. If you simply mean that particular collection of assumptions, then I think there is nothing holy about that, and those assumptions can be discarded the way any set of assumptions can be discarded. If you mean by the DSGE approach being dynamic, being stochastic and being interested in general equilibrium, then I think that approach can be pursued, al-

though I share Scott Page's view—and, I suppose, the view of most of us here—that the presumption of equilibrium is a little excessive.

But I think there is a lot—I don't want to discard DSGE. The people who do it are among the brightest macroeconomists we have. They are not foolish. I do think they are neatness freaks like me and tend to be pushed by their neatness freakery into looking further at this, but I do think it has to loosen up.² I do think that one wants to give up the representative-agent presumption; I think that one has to give up the devotion to equilibrium and keep the broader methodology. And in the course—let me just add one more thing—in the course of meeting criticisms from sourpusses like me and from the data, the DSGE people have made a lot of modifications, and in the course of doing that they have done a lot of good work. I would like to focus on that and give up some more of the more egregious assumptions.

Mr. BROUN. Dr. Chari, do you want to comment?

Mr. CHARI. I think Professor Solow is exactly right. All the interesting policy questions inherently involved understanding the behavior of individuals who are confronted with making decisions that are inherently dynamic in the sense that, if I want to buy a refrigerator or a car or if I am a businessperson planning to invest in plant and equipment, I am making a decision about incurring cost today in return for future benefits. Dynamics is essential to lots and lots of decisions. Stochastic in the sense of handling uncertainty as we all know is central to decision-making. General equilibrium just means making sure that stuff adds up and things are consistent across the way. These are innocuous terms. It is hard at a conceptual level to disagree. Quite frankly, I don't know what the alternative is. There isn't any out there.

Now, within the class of DSGE models, there are a bunch of assumptions that people have made. As I illustrated earlier, early generations made very stringent assumptions. We have thrown away most of those kinds of assumptions. What Scott is asking for, for example, is that we describe individuals as computer programs that react in predictable ways. There is nothing in the flexibility of the methodology that automatically precludes that, no. It is open to that kind of thing. Here is the problem that modelers confront, and here is the difficulty: The difficulty is that every time we add another ingredient into the model, we want to try and make sure that it is disciplined in some fashion—that is, it is disciplined by historical evidence of the United States, other countries, things like that—both at the micro level and at the macro level. Absent that discipline, you have just got a bunch of idle theorists doing completely worthless stuff—and I am an idle theorist too, so I am all for idle theorists. But it is not the kind of stuff that is going to lead to fundamental improvements.

So every time we add a complication to the model, we discipline that complication by focusing on an additional piece of data. So, for example, early generations of these models had a representative

²For the purpose of clarification, Dr. Solow has requested that his testimony from “into looking further. . .” in this paragraph to “. . . to loosen up.” be corrected to read as follows:

“. . . into foolish conclusions. They can pursue a much looser version if they want to make sense.”

agent. Modern generations have all kinds of heterogeneity. How do we discipline that heterogeneity? Well, we have data on the wealth of individuals, how the wealth of individuals evolves over time, how the cross-sectional distribution of income evolves over time. That is the kind of stuff that we use to discipline that kind of activity. Early versions didn't have sticky prices or wages. Thanks to important work that Pete Klenow, who was at the Minneapolis Fed, did with Mark Bilts, we now have disciplined, interesting ways of introducing sticky prices and sticky wages. Right. Okay. So these kinds of things, every time things are added, as long as they are added with focus on data, with some discipline in the process of doing it, in general my reading of the macroeconomics profession is, "come on board, the water is"—"come on into the water, it is fine."

The main thing I complain about in my field is, you know, it is amazing how few of us there are. By my count, there are roughly 200—if I was being very generous and included a lot of people, 500—economists who are actively engaged in the production and the consumption and the interpretation of DSGE models. We are talking about—this is an economy of 300 million people with, you know, hundreds of thousands of economists. We are devoting a tiny fraction of our energy to this. Would we be better off if we could quadruple it? Absolutely. Would I be better off I had four times as many students? Yes. What holds me back? I just don't have the money. I wish I did.

Mr. PAGE. Can I just follow up very quickly and say that Chari is raising a really good point, in that there has been this push on these DSGE models to include all sorts of things, including heterogeneity and stickiness and frictions and that sort of thing? And I think this relates to your opening comments as well about studying climate-change models. I mean, one thing I think we really need is a lot—we don't need one big model. Every model is going to make mistakes, and one big model is just going to be really hard to understand and it is going to screw up. What we need is, we need lots of models that each include different parts of things. And then we need people who have really sound judgment who can say, "Okay, here is our 15 models of the economy, this one is really focusing on heterogeneity, this one has got lots of frictions, this one has got much better sort of firm-level detail, right, with sectors of the economy." And of all these models in play. . . . And then use sort of our judgment and our wisdom and sort of be willing to abandon the notion of stationarity—that the world next week is going to look like it was the week before—and have a dialog between those models to make good choices.

I think when we lock into a single model, which I think we have done with climate change and I think sometimes the Fed does—for political reasons, it makes a lot more sense to have the Federal Reserve Bank of Minneapolis or to have the IPCC say, "this is what is going to happen." And that is silly because it is going to be wrong: You are better off saying "here is a whole suite of models and there is a lot of disagreement about it because this thing is complex." I think we just have to be willing to accept that. I think that is politically difficult to do because people want answers.

Mr. BROWN. My time is expired, but Mr. Chairman, I would like to make a comment if I may. These models are certainly inter-

esting from a theoretical perspective and you have different economic theories, whether it is Keynesian or supply side, et cetera, and as policymakers, unfortunately we don't base the policies that we create and administrations both Republican and Democrat try to do the best they can to use whatever tools that they have to try to make this country a better place, and I appreciate you all's effort and what you all are doing in trying to give us some modeling and give us some kind of idea about how to proceed. And unfortunately, I think Republicans and Democrats alike don't pay as much attention to unintended consequences of decision-making that we make and hopefully in this Committee, the Science Committee, we have all agreed that science can't create policy but policy can be created based on scientific evidence and best evidence, and that is what I do as a physician, so I want to thank you all, and my time has run out and I will yield back.

Chairman MILLER. Thank you, Dr. Broun.

Dr. Broun quoted a philosopher as saying that the enemy of the good is not the bad but the perfect, which is probably the most quoted quotation from a philosopher in Washington, but never with attribution. It was Voltaire, and what politician wants to admit to quoting from a French philosopher?

Mr. BROUN. Mr. Chairman, I want to remind you, I just did.

Chairman MILLER. But not with attribution, never with attribution.

We are seeing the President sign tomorrow a new financial reform bill that will include agencies with new duties, including the Consumer Financial Protection Bureau and a Systemic Risk Council that is supposed to see systemic risk coming—although Alan Greenspan always thought that you could not recognize a bubble while you are in it, you could only recognize a bubble after it had burst. Obviously, we want to improve upon that and recognize bubbles as they are forming and not after they burst.

Dr. Winter, what sort of research will those new regulatory agencies need, would you urge them to undertake, to inform their decisions?

Mr. WINTER. The problem illustrated in the history of the crisis is that the institutional arrangements of the economy, and particularly in the mortgage markets, changed quite dramatically over a period of, well, some decades, and then rather rapidly in the final decade of the episode. And, basically, the regulatory agencies were to some extent unaware of the extent of these changes and did not have the capacity to try to estimate or understand what the implications of those changes would be. There was this—the Pew Foundation sponsored a very high-level expert task force on financial regulatory reform, which delivered a short report and remarked on this point. And I actually quote that remark in my written testimony, saying basically that the system changed a great deal, the regulators were not aware of the extent of this change and its implications. Now, to be aware of those changes, they would have had to had a lot of very high-quality economic research going on someplace, and presumably in-house, because I don't think it is very likely that that kind of research would be adequately supported elsewhere.

So this would bring me back to Dave Colander's remarks about the need to have some greater strength in the domains of applied economics, to use the understandings that we accumulate in academic research to address the really significant problems. So I think and suggest in my written testimony that these agencies—and I would include particularly the Fed staff in this respect—needs to have economists who have more of an orientation to the institutional context and the way that it is changing and are interested in trying to estimate the implications of that so that they can provide some useful guidance.

Chairman MILLER. Thank you.

Dr. Winter, I have heard Ben Bernanke say, not so much in defense of why they didn't see the bubble forming but why they did not act to adopt consumer protections—which the Fed had the authority to do since 1994—as saying that the abuses that became enormous happened fairly quickly: 2004 to 2006. Subprime lending went from eight percent of all mortgages to 28 percent of all mortgages in two years, and it was an almost entirely unregulated corner of the market that they didn't see happening. It was not depository institutions that were initiating the loans. They were a conduit to the securitization market, which was almost entirely unregulated, which were investment banks that were not depository institutions. And no one, no regulator, actually really saw what was happening. So your testimony just now is very consistent with what Chairman Bernanke has said as well.

Anyone else wish to—

Mr. WINTER. May I comment on that?

Chairman MILLER. Yes, Dr. Winter.

Mr. WINTER. Thank you. So I am familiar with the sequence of events in the Fed on the matter of subprime lending, and I think an important part of what happened was that there was discussion—there was not action but there was discussion—about the need to protect the borrowers in the context of that episode. Now, I think what was very largely missed was the fact that we had created a system in which the lenders were making dumb loans, that the effect of the securitization process and the general losing track of what was happening to the relevant information, created not only the abuse of the borrowers but a great vulnerability in the lenders.

Chairman MILLER. The loans would not have been dumb if housing had continued to appreciate the way it was appreciating, but that was. . . . Because even if a borrower couldn't pay it back when the reset came after just two or three years—and they had to pay 30 to 50 percent more in a monthly payment, which they couldn't begin to do, and then they had to pay a pre-payment in order to get out of that mortgage—if the house had appreciated 15 percent in those two years, there was no realistic possibility they wouldn't pay the full loan. They could either refinance or they could sell their house. But when it stopped appreciating, the music stopped.

My time has expired. I now recognize Dr. Broun for five minutes.

Mr. BROUN. Thank you, Mr. Chairman.

In Dr. Solow's testimony, he said there are other traditions with better ways to do macroeconomics. If macroeconomic models like

the DSGE are insufficient and should not be relied upon, what other tools should economists use? I will throw that out.

Mr. SOLOW. Thank you. I am glad to take a crack at that. In our discussion here, possibly because it is set up as a discussion of DSGE models, we tend to think and talk as if the choices are you can do DSGE or you can do something else. And Dr. Chari suggested, "Oh, well, DSGE can be a big raft, climb aboard." I don't want to climb aboard. I would rather have the DSGE people take a swim off the raft. There have been, and still are, long traditions of work, theoretical and applied, in macroeconomics which have had their ups and downs, but they are not all novelties or things that we ought to try for the first time.

My hero in macroeconomics, now alas dead, was James Tobin, Professor James Tobin of Yale University and a very dear and close friend of mine. Jim Tobin did macroeconomics in a way that paid, I think, just about the right attention to the microeconomic foundations of macroeconomics. He made sure that everything he did was compatible with the truth that what happens macroeconomically is the aggregation of millions of firms and individuals, and one wants only to say things that are compatible with that, and that give reasonable results that pass the smell test. Tobin, and not only Tobin but many others besides, left a large body of work which is now all of a sudden forgotten or ignored, and I think that is a terrible mistake. We should simply go—one of the things we should do is continue those traditions. They all worked in terms of aggregate supply and demand in one way or another, and they focused their research on learning more about the components of aggregate demand or learning more about the components of aggregate supply, and what happens in markets that fail to match demand and supply. And that is a perfectly sound tradition. It is not something new or untried. We could go back to do that or do more of that. I would like to see us do more of that. One of the problems is that the DSGE model is so attractive, as I said, to the neatness freak in many economists that it is very hard for anything else to get any traction, and I hope that we can change that. I hope that the misbehavior of the economic system in the past four years will help to change that.

But we don't have to do something brand new, although I am game to give agent-based models a run. I am all for that. There are some new things, but there are some basic existing traditions that could be revived.

Mr. BROUN. Dr. Chari?

Mr. CHARI. I want to go back to my aphorism: If you have got an interesting story to tell and you think it is a coherent story, nothing easier than or harder than to try to put it into a model and see if it comes out. If it does, then it is a coherent story. If it does not, then you haven't thought through the economic problem quite as well. These are very flexible models.

Let me give you an illustration of the sense in which they are very flexible. There is work by Mike Woodford that is now about seven or eight years old. And a lot of that work has been recently addressed by very prominent macroeconomists—John Taylor, Larry Christiano, Marty Eichenbaum, a whole bunch of people—all of whom were interested in the following question: when interest

rates are very low—we are in a zero-interest-rate world, so to speak, as we are now—what would be the consequences of big increases in government expenditures, what would be the consequences of various kinds of shocks to the economy? And those models delivered answers that I suspect Professor Solow had in mind when he said: “Can they generate environments in which there is unemployment because of insufficient demand?” Certainly, if you look at the output of those models, it looks that way. Those are models where apparently even wasteful expenditures in government can in certain circumstances be desirable. So that is the sense in which is an open, flexible tool. I would argue it does produce the kind of unemployment that Professor Solow thinks he sees. So all that is all fair game.

When I said “come on board,” what I meant was that if people have an idea—and Scott, for example, has been talking about agent-based modeling, and I think it is intriguing and interesting, and, as he recognizes, subject to computational limitations—there is really nothing in the logic of the basic method that prevents you from putting those kinds of features in. What are the kinds of things that then are left out? It is only ideas that are incoherent, arguments that are intended to obfuscate rather than clarify. It is a way of thinking about the world. It does not start off with any presupposition about what the outcomes are.

A final observation that is worthwhile keeping in mind whenever you think about all these things is, there are terms like *bubble*, *involuntary unemployment*, a variety of different kinds of things, which are theoretical constructs. They can be useful theoretical constructs, but they can also impede us from thinking through what the fundamental issue is. So the fundamental issue is the following: Housing prices in the United States rose dramatically over about a decade-long period and then collapsed dramatically. Is that a bubble or not? That is a hard question to tell. But an inarguable question to tell is, to address is, do you have a model that can produce dramatic increases in housing prices? If you do not have a model that can produce dramatic increases in housing prices, don’t come and tell me how we should regulate the housing market. You can’t have something interesting to say. Now, in that model is that dramatic increase in housing prices a bubble? It may or may not be. It depends on the details of the particular model.

So it is very important for models to be consistent with the data. It is very important for us to be able to write down models where the unemployment rate is sometimes 11 percent and sometimes four percent. It is less interesting, I think, to ask in that model, is that unemployed person involuntarily unemployed or voluntarily unemployed? Those are hard questions. It is valid and legitimate to ask yourself the following question: If you asked the actors in the model, “would you accept a job at the prevailing wage?” you better get a situation where sometimes 11 percent of them say yes and sometimes four percent of them say yes. So the way I describe this somewhat more succinctly is, we should judge models by the outcomes that they proceed, not by the particular language that we use to decide whether the outcomes are the outcomes that we want from the models.

Mr. BROUN. Thank you. My time is expired.

Chairman MILLER. Ms. Dahlkemper is recognized for five minutes.

Ms. DAHLKEMPER. Dr. Solow, your work highlighted the importance of technology as a key driver of economic growth, and Ms. Biggert had alluded to that earlier in her questioning. And we look for budget savings and, you know, we talked about western Pennsylvania where I am from and manufacturing-based economy in large part. How should we prioritize spending to guarantee that what we spend represents the best investment for our future?

Mr. SOLOW. You really want me to tell you how to do that?

Ms. DAHLKEMPER. We are looking for answers.

Chairman MILLER. You can use your full five minutes.

Mr. SOLOW. Are you speaking primarily of expenditures on science and technology or expenditures broadly?

Ms. DAHLKEMPER. Well, expenditures broadly.

Mr. SOLOW. I think that you ought to—there are two aspects of public expenditures, especially now, in the next couple of years. One is that public expenditures of any kind will provide some employment and some secondary expenditure as well and, perhaps at longer-term costs in terms of accumulating debt, would certainly improve the economy, would put idle resources to work. That is what I am trying to get at.

The second aspect of public expenditures, which would be true even if there were no idle resources to put to work, are that you have to think in terms: What is the social benefit? What is the benefit to society of spending money on object A as against spending an equivalent amount of resources, of real resources, on B? And in other words, sometimes the clichés are right; and I think that cost-benefit analysis, although a cliché, is the right answer to this question when all resources are reasonably fully employed. And in my view, and I have no personal interest in this, I think that expenditures on the promotion of innovation in science and technology, including social science and organization and things like that is, are resources well spent in our economy. On the other hand, right now, in 2010, I think that almost the dominant fact about public expenditures is that they have a good shot, when interest rates are low and staying low, of putting idle resources to work. That doesn't obviate the cost-benefit analysis, because you can put resources to work in one way or in another, and your job as a legislator is to make that judgment for people. Was it Dr. Broun that quoted someone, I don't know with attribution or without, that science describes but doesn't prescribe? Well, the same is true of economists and economics. It is your judgment what is the most socially beneficial use of a real dollar's worth or a real million dollars' worth of labor and capital in our economy, and that changes from time to time. So there is no reason to expect there to be an answer that is valid for five years.

Ms. DAHLKEMPER. Thank you.

Dr. Page?

Mr. PAGE. And one thing we have all mentioned is that we think of the economy as the sum of these 300 million people, right? And when you talk about the effects of some of these policies, you are taking about aggregating up over those 300 million people—saying if we spend, you know, X million dollars or a billion dollars, we are

going to get a one percent, two percent, three percent change in things. And I think it is incredibly important to focus not just on the mean but also on the variants. As we know, this economic downturn has affected people differently across groups by race, by age, by region, as you alluded to. It has been very different. And I think that we can sort of, with all good intentions, sort of naively assume sort of a linear view of the world: that, you know, if we put this amount of money in, we will get this nice, clean linear effect. But if I look through a tighter focused lens and look at the level of particular communities, whether it is by region or age group or by race, you can see that communities can fall apart, right? So there can be these sort of non-linear threshold-type phenomena where entire communities, regions, groups of people can really suffer, right? And so I think that it is very important we think about policy, not just to think at the macro level but also ask, "How are these things targeted in such a way to prevent sort of cataclysmic events at the micro scale?" We tend to focus on these sort of large events at the macro scale, but these same things are happening at the community level and at the group level, and we need to think of policy, I think, through a finer lens as opposed to just through these broad macro models.

Ms. DAHLKEMPER. Would anyone else like to comment? Dr. Winter.

Mr. WINTER. Yes, sort of the management perspective on the spending question. You know, one of the difficulties that we face in a circumstance like this, where we are asking what useful lines of government expenditure might be, is that some of them take a lot more time to plan and to get implementation than others do. So it is not just sort of a cost-benefit analysis in the abstract. You really have to look at the time phasing of the impacts and the requisite levels of administrative investment in order to make it happen. And that line of thinking brings me back to your question about the unemployment benefits, which is a seemingly very reasonable line of expenditure to pursue: to renew those benefits with the administrative apparatus to do that already in place and not requiring to be invented.

A similar point would apply, I think, to state and local government expenditures, where the state and local governments are doing many things that were deemed to be worthwhile in the past and they probably are worthwhile now, and the reason they are being cut back is because these governments are so strapped for revenue. So that is another area where in very short term and with very little new administrative investment you could make important things happen.

Ms. DAHLKEMPER. Thank you. My time is expired.

Chairman MILLER. Thank you.

We will now have a third round of questions, and I recognize myself for five minutes.

Dr. Solow, you used the phrase, in answer to a question, "the weight of the service sector," and I am not sure you intended that phrase the way I took it the moment that you said it: that, obviously, there are many parts of the service sector that make a useful contribution, but some does just feel like a weight. As a brand-new Member of Congress hearing Alan Greenspan testify, one Member

asked him the question, "Do you think it is important that our economy make things?" And like Ms. Dahlkemper, I represent a district where we lost a lot of manufacturing jobs so I listened intently, and he said he did not think it was important necessarily that we make things but the economy add value. And the distinction that I thought he was drawing was to bring in the service sector but also specifically the financial sector: that it didn't matter quite so much if we lost a lot of textile jobs if we were the world's financial capital and we were adding value. Being the world's financial capital now does not seem like such a great deal for us.

And some economists have pointed to the growth of the financial sector as a symptom of something wrong. It has gone from four percent to eight percent, but most notably the increase in the profitability of having gone from between 5 and 15 percent of all corporate profits to more than 40 with compensation almost twice what most Americans, the average American worker, is making. Should we regard that as simply the result of self-correcting forces in the current equilibrium or as symptomatic of something wrong? I am inclined to view it the way a doctor would view a swollen organ in the body. Anyone? Dr. Solow?

Mr. SOLOW. Yes. So am I. In fact, I was going to interject something along those lines earlier and then I thought I was talking too much so I shut up. I think the doctor's point of view is the right point of view here but I think there is a much broader point, especially in connection with financial services. When we were talking about what kind of research new regulatory bodies ought to do, one of the things that I would like, one of the kinds of research I would like to see done, is this: We have drifted into the habit of talking about the financial services sector as if it justified itself, as if to know and love and earn a lot of money in the financial sector is its own reward. The fact is, God created the financial sector to help the real economy, not to help itself, and one of the kinds of research I would like to see is a much deeper analysis of the way the financial part of the economy is related to the real economy, to the economy of employment and production and consumption and all that. I suspect, as it seems as if the chairman may suspect, that the financial services sector has grown relatively to the point where it is not even adding value to the real economy. It may be adding compensation to its members but it is not improving the efficiency or productivity of the real economy.

There are clear ways in which financial activity can and does do that. We know from lots of empirical study that, for economies at a lower stage of development than the United States and western Europe, financial depth really promotes economic growth. It allocates resources better. It allocates risk better. But I have the feeling, as you seem to have the feeling, that we have got to the point where the financial services sector is creating risk rather than allocating it. So I would like to see research aimed, as I said, at the relation between finance and the real economy, and particularly at what is the productivity in terms of the real economy of resources devoted to financial activity.

When I spoke earlier of the weight of the financial sector, I was really simply thinking of the service sector. I was really just thinking of the amount of employment that it generates and the fraction

of GDP that it generates. They are both very large and increasing. But I would not take for granted that because the financial sector is large and growing, that it must be profitable in the sense of beneficial to the efficiency and productivity of the real economy.

Chairman MILLER. Well, my time is expired, but I do have one other question that actually seems pertinent to the work of the Committee. If we assume that one of the reasons, perhaps the principal reason, we hold hearings is to inform the decisions that actually may come before us, one of the things that the Federal Government does is fund economic research. Dr. Solow just discussed some of the additional research he thought would be useful in informing economic decisions. Dr. Colander in his testimony talked about how we might reallocate the \$27 million that we spent in NSF for economic research, which doesn't seem like that much in the scheme of things, given the importance of the issues. Do any of you have a view as well on how we might reallocate those resources? Dr. Solow?

Mr. SOLOW. I will defer to anyone else who wants to say anything about this.

In a way, I have to disagree with David about his recommendation. Let me just say that first. I am not—in fact, I am a little appalled at the idea of appointing physicists and mathematicians and statisticians to review committees for the economics part of NSF. In my excessively long experience, physicists and mathematicians are capable of infinitely more stupidity about the economy than economists are, and not only capable of but they exercise that capacity frequently. I would—but I do have a small, tentative, extremely tentative suggestion of a way in which funds devoted to NSF's economics division might be profitably employed that I think David might agree with.

I spoke of earlier traditions in macroeconomics. It seems to me that as of now those traditions are most carefully practiced not in universities but in organizations, some of them for-profit firms, which do model building in order to do consulting for private businesses and for government agencies. I know the names of several of those, but that is unimportant. And the Council of Economic Advisors, the Federal Reserve Board, the Congressional Budget Office all make use of those commercially or otherwise-maintained macroeconomic models. I wonder whether NSF some—you might provide some funds for NSF that it could spend making grants for basic research done by some of those model-building enterprises. And there I would even like to see the selection committee have representatives from the Council of Economic Advisors, the Federal Reserve Board, the Congressional Budget Office, which have an idea of how to use these models, how they are used, how they might be improved, perhaps by the addition of altogether different agent-based sorts of things. I think that might be a useful thing to do because, as I said, this is where the older traditions in macroeconomics seem to be most practiced these days. Thank you.

Chairman MILLER. Dr. Chari?

Mr. CHARI. I have served on the NSF's review panel in economics and I have been a frequent reviewer for various kinds of proposals at the NSF. I have also performed similar tasks at the University of Minnesota, Northwestern University, for university-wide grant-

making activity. I have also participated in some private kinds of things. The NSF's process for allocating funds beats everything else I have seen by a mile. It is exceptionally balanced. It is very fair, very thoughtful. The most striking thing, and I think everybody who has served on an NSF panel always says, is the bent is always in two directions. The bent is young people. We have to make sure that young people who are coming into the profession get funded and so therefore we have to take big risks. The second bent is, we have to be sensitive to people who have ideas that seem outside the box. We should not be locked into our way of thinking about things. Of course we need to apply standards, but we do need to do that.

Given my experience dealing with the National Science Foundation, and in any event, given the approximate \$2.5 million for macroeconomic research, I don't think there is much to be done by way of reallocation. I would, however, reiterate the case I made in my original spoken testimony. I think given the importance of the issues at hand, given the centrality of these issues for important policymaking, this is an area—at the risk of being a pleader for special interests, this is an area where I do think the social returns to modest increases in funding are likely to be very substantial.

Chairman MILLER. Dr. Winter.

Mr. WINTER. First I would like to second that last remark of Dr. Chari's. I think given the stakes in these issues, I think it is very hard to imagine that we couldn't reasonably try to devote more resources to useful kinds of economic research.

But to follow up on Bob Solow's comment about the physicist and mathematicians, an interesting note is that younger generations of those physicists and mathematicians that he spoke of became the Wall Street quants, and so the model building that was going on in Wall Street was informed by very high levels of technical skill but very little of the broader perspective which one would at least hope that economists might have brought to that situation. And I am more sympathetic to Dave Colander's proposal about the review. I think he probably did not have in mind the physicists and mathematicians, but perhaps he did, but there are a lot of social scientists and even management scholars out there who think they know something about the way the world works, and I think their voices and some of those review processes would be helpful.

Chairman MILLER. Thank you.

Dr. Colander to defend yourself.

Mr. COLANDER. Yes. Let me just say that what I was trying to emphasize, I think you need diversity within there. I think as Dr. Chari said, there are 200 people, loosely, that are considered macroeconomists.³ If we really got down to it, if we were talking over beers, he would say there is probably about 12 that we take seriously and the rest, well, you know, they work on the edges. What happens is, it is a very small area, which means it can get inbred in the sense of here everyone starts to move in lockstep, and that is what I think Dr. Page is emphasizing. Diversity in and of itself is good, and somehow one needs a way of thinking, "How can we bring in as many diverse views as possible?" So I can say,

³For the purpose of clarification, Dr. Colander has requested that the word "macroeconomists" in this sentence be corrected to read "macroeconomic theorists."

“Well, there is a whole tradition in macroeconomics from Axel Leijonhufvud on coordination failures that all got moved out, that really didn’t get funded.” There is work being done now on saying, “Look, our knowledge of the macroeconomy is so minute that these models, that are formal models, are not helping us, so they use fractally cointegrated vector autoregression models.” Now, you can sort of add that and sort of have that. But they are essentially statistical models. They say, “Let’s pull everything we can from the statistics and use our judgment about theory to add in, and not have formal models.” So there is differences in methodology, and I think is that differences in methodology that hasn’t been allowed the diversity. Once you accept the methodology, I fully—and I have written that I think the profession is very diverse. You know, they don’t hold any particular views but you have got to accept this methodology.

If you don’t accept this methodology, you are not part of the economics profession. That is what I am saying is wrong, because the methodology is itself questionable, and the question is really here: It has to have microfoundation if it is going to be there, but of a particular type, of an equilibrium type. In other words, if you think of things broadly, every economist believes incentives are important. But the way they approach it is, here within the system that is created by the macroeconomy, you are operating within that—in which case the microfoundations have macrofoundations which have microfoundations, and they feed back, and you are not sure where it is. So therefore you have no distinct methodology that you allow.

Now, I don’t know what is right, but what I am saying is, the way to sort of have the system arrive at that is to maintain the diversity and that the current system is not allowing for that diversity. Thanks.

Chairman MILLER. Thank you, Dr. Colander. If we start using those other models that you describe or mention, could you come up with some initials for those?

I now recognize Dr. Broun for five minutes.

Mr. BROUN. I thank you, Mr. Chairman. Just for the sake of expediency, I am going to submit my further questions to the panel, and I appreciate you all’s written response to that, so I will—

Chairman MILLER. I am sorry. What was—

Mr. BROUN. I am going to submit my further questioning for written response to the panel and I will just yield back.

Chairman MILLER. Thank you. I think we are—Dr. Page, did you have anything to add to that last question. It is something that actually is within the jurisdiction of this committee, so perhaps we should talk about that a little bit if you have a thought on the direction in which the government-funded economics research should head.

Mr. PAGE. Yeah, I think it is always risky to sort of ask people off the top of their head, you know, “What should we do within an agency?” I think you always get better decisions if you bring in—if you sort of task people to think about it beforehand and bring sort of people with different vested interests and different training to think about it. But I think one thing that is interesting is that we haven’t within economics thought about could there be, you

know, sort of big new funding initiatives, you know. One thing that the NSF did that I think has been very successful is these IGERT grants, these Integrated Graduate Education Research Training grants, where you set of up sort of these interdisciplinary graduate training programs across the sciences. And these, I think, really transform graduate education in a lot of ways. We haven't done similar things within economics. I mean, there is some—I have one of these, which is partially within the economics department. The University of Michigan actually has two.

But we haven't sort of issued anything of, like a grand challenge or just real opportunities to say to the economics, you know, division within the NSF, "Suppose we gave you \$10 million, right, for a one-time-only event, how would you use the money." So rather than just sort of come up with a program, give them an opportunity and let economists and sociologists and people who know financial markets and people who study organizations, you know, task an interesting, diverse group of people with coming up with things—"What do you think we could give, and what would the bang for the buck be, and how large are the stakes?"—as opposed to sort of, you know, someone getting on a hobbyhorse and saying, "This is my idea, let's do it." Because I think that there is a lot of really bright people who care deeply about these things, and I think we have the resources—we could be very innovative. The reason we are not being as innovative as we could be now is, it is a very small pie and there is a lot of incredibly talented people and there is a lot of people with, you know, strong agendas. And I think that the economics division, like most of the divisions, does a very decent job of dividing that up, but there hasn't been a sense of: "Here is something interesting, here is an opportunity, you know, here is a pile of money, what could you do?" And if you brought, you know, some, I think, bright, diverse minds together, something interesting would probably come out.

Chairman MILLER. Thank you.

I think we are now to the end of our hearing.

Mr. BROUN. May I make a comment, Mr. Chairman?

Chairman MILLER. Dr. Broun.

Mr. BROUN. Just to go back to what Dr. Page was saying, I bet if we gave each one of you \$10 million, the money would be utilized in some manner or another, so I trust that that would happen.

Thank you, Mr. Chairman.

Chairman MILLER. Thank you, Dr. Broun. Dr. Broun, I will make an agreement with you that if you do not tell voters in North Carolina that I have gone to Washington and started quoting French philosophers, I will not tell voters in Georgia that you have as well.

Mr. BROUN. Are you asking for a unanimous consent request?

Chairman MILLER. I will agree. Thank you.

The hearings that I have grown to like the best are when there are really smart, thoughtful people whose testimony agrees with what I already think. I don't claim expertise in economics, but a great legal philosopher, Oliver Wendell Holmes, wrote that the life of the law has not been logic, the life of the law has been experience. And the lesson that I take from your testimony is that we do need logic as we need logic in law, but if experience pulls us up short for where logic appears to lead us, we should listen to the

logic, to the experience, whether it is the smell test that Dr. Solow—whether we call it a “smell test” or otherwise. If the logical, the result of the application of logic leads to an unacceptable result or one that does not make sense, then we should pause over it. And certainly a lot that went on in the economy in the last ten years, looked at in isolation, made no sense. And explaining that it could not be looked at in isolation, had to be looked at as a broader, part of a broader macroeconomy—in which the little pieces all made sense and there were self-correcting forces and there was an equilibrium and we couldn’t tamper with it or we would be tampering with mysterious forces that were beyond our knowledge—that proved not to be a good policy course. We would have been better dealing with the injustices, the things that in isolation appeared to make no sense.

I do appreciate very much this very distinguished panel coming today. It would be great if you could continue to be available to us as we have questions in this area. I appreciate all of you being here.

I now have a script for the closing. Before we bring the hearing to a close, I want to thank our witnesses for testifying—I think I already said that extemporaneously, but now let me also say it from the script—before our Subcommittee today. Under the rules of the Committee, the record will remain open for two weeks for additional statements from the members and for answers to any follow-up questions the Subcommittee may have for the witnesses. And having heard the testimony, I do not think that there is any possibility of any problems with possible perjured testimony.

The witnesses are excused and the hearing is now adjourned.
[Whereupon, at 12:20 p.m., the Subcommittee was adjourned.]