Comments of
Ken Judd
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I have often attended and enjoyed John Taylor's annual conference on monetary economics. This year I decided to take a look at the main papers in the NK literature so that I could better understand the presentations. I soon realized that I had many comments to make on the presentations and the background literature. Time limitations will prevent me from making substantive comments during the conference, so I decided to send these comments to participants before the conference. My comments are definitely in a rough draft form, consisting of several distinct observations. Please inform me if you see errors. I will be at the conference and will welcome your reactions to these observations.

Of course, the first paper I looked at was "The science of monetary policy." This title struck me as a bold statement. I searched the web to see if there were any papers with the title of "The science of tax policy" or "The science of trade policy" or "The science of antitrust policy". No hits. I was impressed that there was a science of monetary policy. So I dove into this literature to learn about the scientific tools used by the science of monetary policy.

Computational methods

After looking through many papers, I was struck by the fact that this science is generally based on linear quadratic models. The common justification is that these are linear approximations of an underlying nonlinear model, but I was still puzzled why better methods were not used. The NK models are usually of low dimension. Basic projection as well as higher-order perturbation methods were already well-known in 1999. The set of methods that could solve low- to moderate-dimensional problem grew over the next several years — for example, Kubler introduced Smolyak polynomials to economics, Serguei and Lilia Maliar and I developed two methods, GSSA and EDS, that could solve higher-dimensional models, and, more recently, Thomas Mertens and I developed an arbitrary-order perturbation method for infinite-dimensional models.

Monetary policy models often need to address time consistency issues when they compare commitment and discretionary policies. Papers in the 1980’s and 1990’s, by Williams, Wright, Kotlikoff, Ha, Sibert, Miranda, and others, showed how to use projection methods to solve for time consistent policies. Therefore, there is no need to limit models to linear ones in order to analyze discretion.

Despite the progress in computational methods since 1999, along with a thousand-fold increase in computer speed, the papers at this conference indicate that the science of monetary policy still relies largely on linear approximations of nonlinear models.

Model realism

Some will argue that linear approximations are satisfactory for the insights that these papers are presenting. However, if the science of monetary policy is to be used by monetary authorities, then shouldn’t the science look at models beyond the toy models and simple techniques that generate insights? The literature on the science of monetary policy examines many different issues, but each issue is examined in isolation. This reminds me of the parable of a group of blind men examining an elephant.

I agree with Rick Mishkin, in his October 2007 comments, on where the science of monetary policy should go:

“Scientific principles are all well and good, but they have to be applied in a practical way to produce good policies. The scientific principles from physics or biology provide important guidance for real-world projects, but it is with the applied fields of engineering and medicine that we build bridges and cure patients….Tractability has led to models based on microfoundations, such as DSGE models, to rely on representative agents, which is a serious drawback. I have a strong sense that what drives many macroeconomic phenomena that are particularly interesting is heterogeneity of economic agents. Building heterogeneous agents into macroeconometric models will by no means be easy, but it has the potential to make these models much more realistic. Furthermore, it may

allow us to understand the link between aggregate economic fluctuations and income distribution, a hot topic in political circles. Heterogeneity of economic agents is also crucial to understanding labor market frictions. In some DSGE models, all fluctuations in employment are from variation in hours per worker, and yet in the real world, changes in unemployment are a more important source of employment fluctuations. Bringing the search and matching literature more directly into microfounded macroeconometric models will make them more realistic and also allow better welfare comparisons of different monetary policies."

I am happy to hear of Mishkin's support for more realistic models, but I am not aware of any followup. Mishkin was a Governor of the Fed for ten months after his 2007 comments regarding where the science for monetary policy should go. I do not know about the inner workings of the Fed, but I don't see any evidence of any Governor of the Fed pushing the science of monetary policy in those directions.

At a conference a few years ago, John Williams said there was a need for models that considered both monetary and fiscal policies. I was glad to hear this support for the development of more realistic models. I said to Williams "As President of the San Francisco Fed you control resources which could be used to build such models. Are you doing anything in that direction?" His response sidestepped the issue by referring to the many people building many models and his hiring of Thomas Mertens.

I am aware of the activity on building models but I see no coherent pattern in terms of model development. Research at the Fed appears to be as disorganized as at any economics department. This chaos is typical in academia, but I would expect an organization like the Fed to at least nudge its researchers to contribute to some coherent, organized line of research.

I met Mertens at a workshop where he presented a very impressive poster on solving multidimensional option pricing problems using methods he learned as an applied math major at Bonn University. I am glad that Mertens has found a good job, but in looking at the Mertens-Williams paper, I see no evidence that his presence has raised the technical sophistication of research done at the SF Fed.

I like some of the speeches I hear but at my age I have heard many such speeches. My question to Williams today is whether he still feels that there should be development of models integrating monetary and fiscal policy, and if he will use the influence and resources of his now far more important position to push the science of monetary policy in that direction.

Linear approximations and monetary policy

A presentation by Clarida a few years ago at this conference revealed the attitude of this science towards model complexity as well as why linear models are not reliable.

A few years ago at this conference Clarida began his presentation by describing just how hard it was to analyze the models was using to model monetary policy. He dealt with those difficulties by looking only at linear approximations.

I pointed out two facts. First, I told him that the equations behind the design of airplanes, such as the one he flew from NYC to Hoover, are far more complex, far more difficult to solve, than the macroeconomic models he was trying to work with. If engineers can solve difficult computational problems when they design an airplane, why can't macroeconomists solve the computational problems they face without relying solely on linear approximations around a stationary point?

Second, I pointed out to him that linearization was not adequate for his analysis. His presentation argued that optimal policies would lead to unit root behavior for some economic variables. Random walks are stable in the sense that they are not explosive. However, his analysis is supposed to be an approximation of some more realistic nonlinear model. It is well-known that if the linearization of a nonlinear system has a unit root then the underlying nonlinear system could be unstable, and that local approximations must go beyond the linear terms to determine the local stability of the nonlinear system. I reminded him that I learned this in the late 1970s in a macroeconomics course where I read papers by Jess Benhabib.

Clarida lamented the difficulty of solving nonlinear monetary models. This is a problem in many sciences, and the usual response is to help younger scholars improve their skills so that they can push the science further. So, I then asked him what he was doing to make it so that his students could solve complex nonlinear models that he could not solve. His response was that he would send his students downtown to talk with Benhabib. There were many chuckles in the room, but I was puzzled by the response. If a Columbia student is going to spend a lot of time shuttling between Harlem and Greenwich Village then perhaps he should just enroll at NYU.

Clarida gave no response to the bifurcation issues I raised. There really is no question about the underlying mathematical issues. A unit root in the linearization of a nonlinear system says nothing about the stability of the nonlinear system. This renders invalid any conclusions Clarida had about the stability of the nonlinear economic model he is approximating. He never contacted me to discuss this issue. Nor did the journal editors that published this paper ever asked me for any comments. I guess that is how the science of monetary policy deals with mathematical comments. More recently, I became aware of work by William Barnett that had raised the bifurcation questions even before I did in my comments to Clarida. In particular, Barnett published "Hopf Bifurcation in the Clarida, Gali, and Gertler Model," in 2013.

Clarida is now the vice chairman of the Fed. So today my question is what is he doing to help the hundreds of economists in the federal reserve system acquire the tools and skills they need to solve models that are more realistic in the representation of real economic processes.

Why can’t economists be more like engineers?

My presentations regarding macro and computational methods often begin with a video of the collapse of the Tacoma Narrows bridge in 1940. I am sure many of you have seen it. The bridge, nicknamed Galloping Gertie, oscillates both vertically and horizontally, eventually tearing itself apart and falling into the river. Where did the engineers go wrong? At that time, they used linear approximations around the rest state to determine the stability of a bridge. As is typical in these cases, a board of inquiry was convened. Theodore van Karman, a world renowned expert on aerodynamics, was asked to participate. The conclusion was that the linear approximations were not sufficient to ascertain stability.

Engineers who design bridges learned much from that engineering disaster. Is there any evidence that macroeconomists learn from policy failures?

Macroeconomists versus computational science

Macroeconomists have repeatedly expressed their interest in modern scientific and computational tools. Wieland believes that Matlab is a suitable tool for macro economic modeling, pointing out that Matlab is used in engineering and science. Wieland is correct when it comes to small problems solved on small computers, but he ignores the fact that when it comes to serious computing challenges in engineering, physics, etc., Fortran, C++ and other languages and software tools are used instead of Matlab.

The Fed however, apparently believes that the economy can be simplified to the point where it can be modeled on a laptop using Eviews, a software package designed to do time series econometrics. I mention this because this is how FRB/US is solved at the Fed. I once asked the author of FRB/US software why they use Eviews; his response was that Eviews allows them to compute model solutions and display them graphically in one piece of software. This was puzzling to me because even in 1996 when FRB/US was first constructed it was easy to take the output of a solver, such as GAMS, and then import the solution into Matlab to do the graphics. It is also the case that in 1996 GAMS was well-known in the applied general equilibrium community and used for solving dynamic economic models. In fact Nordhaus used GAMS in his early versions of DICE. Why didn't the Fed use GAMS? Was it because games originated at a competitor, the World Bank. The IMF at that time was using computational tools far more sophisticated than FRB/US. The IMF is at least 20 years ahead of the FRB/US today, and while looking at their website today it is clear that the gap is widening.

The applied general equilibrium community has used GAMS for decades, and GAMS has strong connections with the computational mathematics community. Therefore, the AGE community is able to use the best algorithms and software available.

The Fed says “No thanks, we will do things on our own.”

The Fed has (and wants) many responsibilities for regulating the US economy, and also wants to be the big player in the world economy. It limits itself to “tractable” mathematical and computational tools. I did a google search for “tractable”; the definition is “easy to deal with”. Government agencies are often given mandates by our national leaders. I remembeer the mandate JFK gave NASA in 1962: “We will go to the moon in this decade…not because it is easy, but because it is hard.” The Fed has mandates far more challenging than putting a man on the moon, but it seems that they also heard a different voice telling them that, whatever they do, keep the math easy.

What is holding back the science of monetary policy?

Why isn't the Fed or the community of macro scientists in general using the full range of scientific tools that could be used in a science of monetary policy?

In his 2010 Congressional testimony, Chari explained why the models used by macroeconomists are simple: “Abstracting from irrelevant detail is essential given scarce computational resources, …”

That is not true today, nor was it true in 2010. Macroeconomists have never been constrained by any scarcity of computational resources. In 2008, a PhD student of mine defended his thesis which used a hundreds of networked workstations to solve dynamic programming problems from economics. The gap today between what the macro scientists do and what has been done by other economists has grown even wider. For eight years, I had worked on a paper that relied on millions of core hours of supercomputer time to solve models far more complex than anything examined in the science of monetary policy. The computational work was completed five years ago. It will finally appear in a top five journal by the end of the year.

Chari’s first explanation for the simplicity of macro models is clearly false. There are enormous computational resources available for economists and I was able to obtain a small fraction of what's available, overcoming fierce opposition from economists and others.

Chari’s full comment in 2010 was

"Abstracting from irrelevant detail is essential given scarce computational resources, not to mention the limits of the human mind in absorbing detail."

I once asked Chari which human minds was he referring to? He said nothing. I had hoped he would at least say he did not mean to include my mind, but he wouldn't even say that.

Finally, there was the journal editor who told me:

"Ken, it is nice to write papers for smart people, but that is not what referees and readers want."

The academic world is what it is. What people want from the Fed is responsible and effective monetary policy that helps them achieve and maintain the standard of living justified by their hard work. They don't care about the stylistic preferences of the readers, referees, and editors of academic journals. They do not want their economic lives endangered by academics' limited abilities to analyze complex systems.

People want to fly in safe airplanes. The aircraft industry has spent the money that allows them to use modern mathematical and computational methods. The record is not perfect, but it is pretty good.

People want to ride in safe and fuel-efficient cars. The automobile industry has spent the money that allows them to use modern mathematical and computational methods. The record is not perfect, but it is pretty good.

People want effective monetary policy. They would appreciate it if the Fed at least tried.