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POLICY BRIEF

PB 16-11 Do DSGE Models Have a Future?

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DSGE models have come to play a dominant role in macroeconomic research. Some see them as the sign that macroeconomics has become a mature science, organized around a microfounded common core. Others see them as a dangerous dead end.

I believe the first claim is exaggerated and the second is wrong. I see the current DSGE models as seriously flawed, but they are eminently improvable and central to the future of macroeconomics. To improve, however, they have to become less insular, by drawing on a much broader body of economic research. They also have to become less imperialistic and accept to share the scene with other approaches to modelization.

For those who are not macroeconomists, or for those macroeconomists who lived on a desert island for the last 20 years, here is a brief refresher. DSGE stands for "dynamic stochastic general equilibrium." The models are indeed dynamic, stochastic, and characterize the general equilibrium of the economy. They make three strategic modeling choices: First, the behavior of consumers, firms, and financial intermediaries, when present, is formally derived from microfoundations. Second, the underlying economic environment is that of a competitive economy, but with a number of essential distortions added, from nominal rigidities to monopoly power to information problems. Third, the model is estimated as a system, rather than equation by equation in the previous generations of macroeconomic models. The earliest DSGE model, representing an economy without distortions, was the Real Business Cycle model developed by Edward C. Prescott and focused on the effects of productivity shocks. In later incarnations, a wider set of distortions, and a wider set of shocks, has come to play a larger role, and current DSGE models are best seen as large-scale versions of the New Keynesian model, which emphasizes nominal rigidities and a role for aggregate demand.[1]

There are many reasons to dislike current DSGE models.

First: They are based on unappealing assumptions. Not just simplifying assumptions, as any model must, but assumptions profoundly at odds with what we know about consumers and firms.

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Go back to the benchmark New Keynesian model, from which DSGEs derive their bone structure. The model is composed of three equations: an equation describing aggregate demand; an equation describing price adjustment; and an equation describing the monetary policy rule. At least the first two are badly flawed descriptions of reality: Aggregate demand is derived as consumption demand by infinitely lived and foresighted consumers. Its implications, with respect to both the degree of foresight and the role of interest rates in twisting the path of consumption, are strongly at odds with the empirical evidence. Price adjustment is characterized by a forward-looking inflation equa-

1. While a "standard DSGE model" does not exist, a standard reference remains the model developed by Frank Smets and Rafael Wouters (2007). See Linde, Smets, and Wouters (2016) for a recent assessment with many references.

tion, which does not capture the fundamental inertia of inflation.[2]

Current DSGE models extend the New Keynesian model in many ways, allowing for investment and capital accumulation, financial intermediation, interactions with other countries, and so on. The aggregate demand and price adjustment equations remain central, however, although they are modified to better fit the data. In the first case, by allowing, for example, a proportion of consumers to be "hand to mouth" consumers, who simply consume their income. In the second case, by introducing backward-looking price indexation, which, nearly by assumption, generates inflation inertia. Both, however, are repairs rather than convincing characterizations of the behavior of consumers or of the behavior of price and wage setters.

Second: Their standard method of estimation, which is a mix of calibration and Bayesian estimation, is unconvincing.

The models are estimated as a system, rather than equation by equation as in previous macroeconometric models. They come, however, with a very large number of parameters to estimate, so that classical estimation of the full set is unfeasible. Thus, a number of parameters are set a priori, through "calibration." This approach would be reasonable if these parameters were well established empirically or theoretically. For example, under the assumption that the production function is Cobb-Douglas, using the share of labor as the exponent on labor in the production function may be reasonable. But the list of parameters chosen through calibration is typically much larger, and the evidence often much fuzzier. For example, in the face of substantial differences in the behavior of inflation across countries, use of the same "standard Calvo parameters" (the parameters determining the effect of unemployment on inflation) in different countries is highly suspicious. In many cases, the choice to rely on a "standard set of parameters" is simply a way of shifting blame for the choice of parameters to previous researchers.

The remaining parameters are estimated through Bayesian estimation of the full model. The problems are twofold. One is standard in any system estimation. Misspecification of part of the model affects estimation of the parameters in other parts of the model. For example, misspecification of aggregate demand may lead to incorrect estimates of price and wage adjustment, and so on. And it does so in ways that are opaque to the reader. The other problem comes from the complexity of mapping from parameters to data. Classical estimation is de facto unfeasible, the likelihood function being too flat among many dimensions. Bayesian estimation would indeed seem to be the way to proceed, if indeed we had justifiably tight priors for the coefficients. But, in many cases, the justification for the tight prior is weak at best, and what is estimated reflects more the prior of the researcher than the likelihood function.[3]

Third: While the models can formally be used for normative purposes, normative implications are not convincing.

A major potential strength of DSGE models is that, to the extent that they are derived from microfoundations, they can be used not only for descriptive but also for normative purposes. Indeed, the single focus on GDP or GDP growth in many policy discussions is misleading: Distribution effects, or distortions that affect the composition rather than the size of output, or effects of current policies on future rather than current output, may be as important for welfare as effects on current GDP. Witness the importance of discussions about increasing inequality in the United States, or about the composition of output between investment and consumption in China.

The problem in practice is that the derivation of welfare effects depends on the way distortions are introduced in the model. And, often, for reasons of practicality, these distortions are introduced in ways that are analytically convenient but have unconvincing welfare implications. To take a concrete example, the adverse effects of inflation on welfare in these models depend mostly on their effects on the distribution of relative prices as not all firms adjust nominal prices at the same time. Research on the benefits and costs of inflation suggests, however, a much wider array of effects of inflation on activity and in turn on welfare.

Having looked in a recent paper (Blanchard, Erceg, and Linde 2016) at welfare implications of various policies through both an ad hoc welfare function reflecting deviations of output from potential and inflation from target and the welfare function implied by the model, I drew two conclusions. First, the exercise of deriving the internally consistent welfare function was useful in showing potential welfare effects I had not thought about but concluded ex post was probably relevant. Second, between the two, I still had more confidence in the conclusions of the ad hoc welfare function.

2. More specifically, the equation characterizing the behavior of consumers is the first order condition of the corresponding optimization problem and is known as the "Euler equation." The equation characterizing the behavior of prices is derived from a formalization offered by Guillermo Calvo and is thus known as "Calvo pricing."

3. In some cases, maximum likelihood estimates of the parameters are well identified but highly implausible on theoretical grounds. In this case, tight Bayesian priors lead to more plausible estimates. It is clear, however, that the problem in this case comes from an incorrect specification of the model and that tight Bayesian priors are again a repair rather than a solution.

Fourth: DSGE models are bad communication devices.

A typical DSGE paper adds a particular distortion to an existing core. It starts with an algebra-heavy derivation of the model, then goes through estimation, and ends with various dynamic simulations showing the effects of the distortion on the general equilibrium properties of the model.

These would indeed seem to be the characteristics of a mature science: Building on a well understood, agreed upon body of science and exploring modifications and extensions. And, indeed, having a common core enriches the discussion among those who actually produce these models and have acquired, through many simulations, some sense of their entrails (leaving aside whether the common core is the right one, the issue raised in the first criticism above). But, for the more casual reader, it is often extremely hard to understand what a particular distortion does on its own and then how it interacts with other distortions in the model.

DSGE models have to build more on the rest of macroeconomics and agree to share the scene with other types of general equilibrium models.

All these objections are serious. Do they add up to a case for discarding DSGEs and exploring other approaches? I do not think so. I believe the DSGEs make the right basic strategic choices and the current flaws can be addressed. Let me develop the two themes.

The pursuit of a widely accepted analytical macroeconomic core, in which to locate discussions and extensions, may be a pipe dream, but it is a dream surely worth pursuing. If so, the three main modeling choices of DSGEs are the right ones. Starting from explicit microfoundations is clearly essential; where else to start from? Ad hoc equations will not do for that purpose. Thinking in terms of a set of distortions to a competitive economy implies a long slog from the competitive model to a reasonably plausible description of the economy. But, again, it is hard to see where else to start from. Turning to estimation, calibrating/estimating the model as a system rather than equation by equation also seems essential. Experience from past equation-by-equation models has shown that their dynamic properties can be very much at odds with the actual dynamics of the system.

That being said, I believe that DSGE modeling has to evolve in two ways.

First: It has to become less insular. Take the consumption example discussed earlier. Rather than looking for repairs, DSGE models should build on the large amount of work on consumer behavior going on in the various fields of economics, from behavioral economics, to big data empirical work, to macro partial equilibrium estimation. This work is ongoing and should indeed proceed on its own, without worrying about DSGE integration. (Note to journal editors: Not every discussion of a new mechanism should be required to come with a complete general equilibrium closure.) But this body of work should then be built on to give us a better model of consumer behavior, a sense of its partial equilibrium implications, perhaps a sense of the general equilibrium implications with a simplistic general equilibrium closure, and then and only then be integrated into DSGE models. This would lead to more plausible specifications and more reliable Bayesian priors, and this is what I see as mostly missing. I have focused here on consumption, but the same applies to price and wage setting, investment, financial intermediation, treatment of expectations, etc. In short, DSGEs should be the architecture in which the relevant findings from the various fields of economics are eventually integrated and discussed. It is not the case today.

Second: It has to become less imperialistic. Or, perhaps more fairly, the profession (and again, this is a note to the editors of the major journals) must realize that different model types are needed for different tasks.

Models can have different degrees of theoretical purity. At one end, maximum theoretical purity is indeed the niche of DSGEs. For those models, fitting the data closely is less important than clarity of structure. Next come models used for policy purposes, for example, models by central banks or international organizations. Those must fit the data more closely, and this is likely to require in particular more flexible, less microfounded, lag structures (an example of such a model is the FRB/US model used by the Federal Reserve, which starts from microfoundations but allows the data to determine the dynamic structure of the various relations). Finally come the models used for forecasting. It may well be that, for these purposes, reduced form models will continue to beat structural models for some time; theoretical purity may be for the moment more of a hindrance than a strength.

Models can also differ in their degree of simplicity. Not all models have to be explicitly microfounded. While this will sound like a *plaidoyer pro domo*, I strongly believe that ad hoc macro models, from various versions of the IS-LM to the Mundell-Fleming model, have an important role to play in relation to DSGE models. They can be useful upstream, before DSGE modeling, as a first cut to think about the effects of a particular distortion or a particular policy. They can be useful downstream, after DSGE modeling, to present the major insight of the model in a lighter and pedagogical fashion. Here again, there is room for a variety of models, depending on the degree of ad hocery: One can think, for example, of the New Keynesian model as a hybrid, a micro-founded but much simplified version of larger DSGEs. Somebody has said that such ad-hoc models are more art than science, and I think this is right. In the right hands, they are beautiful art, but not all economists can or should be artists. There is room for both science and art. I have found, for example, that I could often, as a discussant, summarize the findings of a DSGE paper in a simple graph. I had learned something from the formal model, but I was able (and allowed as the discussant) to present the basic insight more simply than the author of the paper. The DSGE and the ad hoc models were complements, not substitutes.

So, to return to the initial question: I suspect that even DSGE modelers will agree that current DSGE models are flawed. But DSGE models can fulfill an important need in macroeconomics, that of offering a core structure around which to build and organize discussions. To do that, however, they have to build more on the rest of macroeconomics and agree to share the scene with other types of general equilibrium models.

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