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Paradise lost?

A brief history of DSGE macroeconomics*

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Abstract

Since the Global Financial Crisis, academic economists and policymakers have had to deal with uncomfortable questions about the quality of their models and the state of macroeconomics as a profession. This note offers a summary of this discussion, focusing on the Dynamic Stochastic General Equilibrium (DSGE) framework and its underpinnings. This class of models reflects both theoretical advances and perennial modeling challenges. While DSGE modeling developed in times of scarce micro data and limited computational resources, it has much room for improvement given progress along these dimensions and advances in other branches of economics. Key tasks on the to-do-list for model improvement include the modeling on the financial sector, departures from the representative agent and rationality, as well as clarification of the empirical relevance of the Lucas critique. The framework is likely to remain a major research and policy tool, although its limitations call for greater robustness, validation and open recognition of uncertainty in drawing real-life quantitative conclusions.

Keywords: macroeconometric models; DSGE; Lucas critique; financial crisis

JEL Classification: B22; E13

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1 Introduction

Is macroeconomics in trouble? Are its current problems only related to applied work such as the types of models used for policy advice, or are they more fundamentally tied to the available theories offered to explain economic phenomena? Since the Global Financial Crisis of 2007–2008, such fundamental questions have generated a lively debate among academics and economic practitioners and become a frequent topic in professional conferences.[1] Several elaborated opinions on the subject are collected in a special issue of the *Oxford Review of Economic Policy* (2018) and the summer issue of the *Journal of Economic Perspectives* (2018). The purpose of this note is to highlight some of the main threads in these discussions. In the following limited overview, I focus on a crucial aspect of the debate related to applied macroeconomic modeling and the Dynamic Stochastic General Equilibrium (DSGE) framework.

While the DSGE paradigm surely does not constitute the entire basis of modern macroeconomics, the DSGE class of models quite arguably has become *the* workhorse of mainstream macroeconomics. It is used extensively as a tool in supporting policy decisions of central banks and widely taught in graduate programs. I devote my attention mainly to the theoretical assumptions of these models, touching as needed on relevant econometric and computational issues.

The term "applied" deserves particular emphasis. One message from the literature and the current discussions is that the academic profession has been partly aware of many of the limitations and weaknesses of modern macroeconomic models and offered some remedies. Unfortunately, many of these improvements remain within the academic domain, percolating only slowly to applied work and to the core of the framework.

Mainstream economists rarely raised questions about the state of macroeconomics before the outbreak of the Global Financial Crisis. Robert Lucas' conclusion that the "...central problem of depression prevention has been solved, for all practical purposes, and has in fact been solved for many decades." (Lucas, 2003) is now routinely cited as evidence of the profession's pervasive blind side. However, the data available before 2008, indicating low, stable inflation conditions and a Great Moderation (i.e. permanent reduction in business cycle volatility) since the mid-1980s at least partly justify such claims.

The discipline's relative self-confidence was shattered in 2007 by worldwide economic turbulence.

[1] See e.g. the 2018 Nobel Symposium on "Money and Banking" organized by the Swedish House of Finance.

Queen Elizabeth II famously asked why nobody had noticed the financial crisis coming. Warnings and words of caution had been raised occasionally, e.g. by Raghuram Rajan in the 2005 Jackson Hole conference (published as Rajan, 2006), but for the most part the depth and breadth of the crisis came as a genuine surprise to the profession. The mainstream cutting-edge policy models of the time, based on the DSGE paradigm, failed to predict the extent of the economic slump before the crisis or capture the subsequent economic dynamics.

The forecasting performance of macroeconomic models relates closely to the set of assumptions that underlie them and to their general ability to (at least approximately) mimic the empirical data-generating process. These model qualities are especially relevant in economic policy making as they are used for numerical predictions regarding the future path of the economy and alternative policy scenarios.

Most complex DSGE models before the crisis, frequently labeled as "New Keynesian," focused mainly on the inability to adjust prices and wages instantaneously (see e.g. Christiano et al., 2005). This "stickiness" means that the same good is priced differently across sellers and buyers at any point in time with the consequence that the resources in the economy are allocated suboptimally. This gives central banks their *raison d'être*, the perpetual quest to correct these persistent misallocations through active interest rate manipulation. A second message of these models is that welfare losses from high, volatile inflation rates are orders of magnitude greater than losses from fluctuations in output. This has become a prime argument for keeping inflation and inflation targets low. It has also signaled that inflation merits a vast research effort, even at the expense of work on other phenomena such as financial markets and economic crises.

This framework seemed sufficient in normal times when the short-term rate was the sole instrument of monetary policy. It became a straitjacket after 2007 when the urgent need arose for studies on the role of financial markets and effectiveness of "unconventional" policy measures. But before discussing how macroeconomic modeling should change, it is important to understand how the discipline arrived at the New Keynesian benchmark as it was at the onset of the crisis.

2 Early days

First attempts to develop large models of the whole economy started with the pioneering contributions of Jan Tinbergen (see Tinbergen, 1939). This work not only earned him the first Nobel

Prize in economics in 1969, but established the research agenda carried forward by Tinbergen's disciple, Tjalling Koopmans. Koopmans rose to head the influential Cowles Commission, based in Chicago. That research agenda was later on taken over by Lawrence Klein and Arthur Goldberger, who developed consecutive versions of their macroeconomic model throughout the the 1950s and 1960s. Models in the spirit of the Cowles Commission aimed at capturing economic relationships of key aggregate variables and included equations such as those describing the Keynesian consumption function, aggregate demand and supply, market clearing restrictions and accounting identities. Because of their mathematical construction, they were referred to as "simultaneous equations models" (SEMs).

The equations in these models were intended to capture fundamental relationships stemming from economic theory. They were viewed as stable, i.e. unaffected by the passage of time or exogenous shocks. Accordingly, these relationships were labeled "structural," and the SEM acronym came to stand also for "structural econometric model."

Although the construction of SEM models is guided by theory, the theoretical underpinnings are hardly rigid. SEM models can involve arbitrary or *ad hoc* assumptions, particularly those related to expectations. Final specification, such as inclusion of certain variables and time lags, also depends on statistical relevance tests.

This elastic approach allowed the models to have many blocks capturing various sectors of the economy, e.g. households, banks and other financial institutions, various goods and production sectors, the central bank, as well as the government, which had taxes and other policy instruments at its disposal. Households were heterogeneous with respect to the broad range of demographic cohorts and degrees of financial constraints. Such detailed description of the economy resulted in several hundred equations, as was the case with the Brookings model (see Duesenberry et al., eds, 1965).

Given their good forecasting performance, these models were initially considered a great success. By the late 1960s, however, they faced increasing attacks on both empirical and methodological grounds (e.g. Wren-Lewis (2018). Amid the rising inflation in the late 1960s, the oil crisis of 1973--1974 and the resulting stagflation (i.e. recession combined with high inflation), the forecasting performance of SEM models substantially deteriorated, failing to match even single-equation, a-theoretical and purely statistical models (see Nelson, 1972). These events also cast doubt on the traditional short-run Phillips curve tradeoff that postulates an inverse relationship between

economic activity (and employment) and the rate of inflation. If interpreted as a causal rather than only a statistical relationship, the curve implied that inflationary policy could be expansionary. For example, if agents are subject to money illusion, they would take higher nominal wages (inflation) as a sign of higher real wealth. The resulting increase in aggregate demand would then generate higher economic activity and lower unemployment. Stagflation, however, did not fit this simple story. Rather, it lent support to the expectations hypothesis postulated by Phelps (1967) and Friedman (1968), who argued that because people are forward-looking and learn from past mistakes, they can avoid being repeatedly fooled by money illusion. Thus, repeated money injections into the economy ultimately are expected to result in higher inflation without generating higher economic activity.

3 The Lucas critique

The initial reaction to Friedman's and Phelps's groundbreaking argument was to include inflation expectations as an independent variable in macroeconometric models. However, these expectations were backward-looking or "adaptive," which means that current inflation expectations of agents are formed solely on the basis of observed past inflation. This approach raised the objection by Robert Lucas (see Lucas, 1976) who argued that such models are useless in policy analysis. Specifically, any conditional forecasts from SEM models that involve policy changes relative to the past are fundamentally flawed as such models are estimated on past data and capture aggregate relationships that are only valid historically. Crucially, when the policy rule changes, these relationships no longer hold. For example, the marginal propensity to consume may change when a new tax is introduced. This problem is most acute for the formation of expectations that are likely to change along with policy, and thus cannot be modeled in an *ad hoc* way based on past data only. Otherwise, they would be decoupled from the true data-generating process of the economy and, according to Lucas and Sargent (1979), unreliable.[2] The Lucas critique points out that SEM models are not truly structural as the relationships in them are vulnerable to policy changes.

Lucas saw the pervasive instability and sample-dependence of parameters in the estimated contemporaneous models as empirical evidence for his critique (although, as discussed below, this may be a symptom of problems other than policy regime changes). To overcome this, the model-maker

[2]From an econometric perspective, the assumption that expectations are only backward-looking and cannot be affected by economic policy changes would be an exclusion restriction. In principle, such restrictions are helpful in identifying econometric models, but in this form they were deemed "incredible" (Sims, 1980).

needs to know and model explicitly those structures and agents' objectives in the economy that may plausibly be considered stable and invariant to policy changes. For example, classical Marshallian microeconomic theory assumes households seek to maximize their lifetime utility while firms maximize profits using a given technology. In practice, this means that macroeconomic models should be built from the bottom up, starting with explicit modeling of all agents in the economy. Given an assumed-stable specification of preferences and technology, it should be possible to deduce how optimal behavior and expectations adjust following a policy change. The requirement of "microfounding" earned this approach the label "new classical macroeconomics"and became, along with the rational expectations assumption, the backbone of DSGE models.

The Lucas critique is based on the concept of rational (as opposed to adaptive) expectations, originally put forward by Muth (1961). If agents have rational expectations, they know and behave according to the rules of the economy they inhabit. Specifically, they know the distribution of the shocks that might hit the economy, so they form expectations that cannot involve systematic or "large" mistakes. The notion that the agents "know how the world works" has been attacked since its onset for treating people as hyper-knowledgeable supercomputers. Muth's initial motivation, however, was simply to acknowledge that the econometrician cannot know more about the economy than the agents who participate in it. The scientist learns about how the world works by estimating the true parameter values that describe the economy.[3]

Rational expectations can be understood as an outcome of a learning process, and learning theory emerged largely as an answer to this specific critique (Evans and Honkapohja, 2005). For example, suppose agents in the economy hold a deeply flawed view about how the economy works. This would mean that they will make systematic forecasting mistakes. But, assuming that they use all available information, and, importantly, the economy's parameters do not change in the meantime, they will learn from past mistakes and gradually come to understand the true mechanisms. Thus, rational expectations is an equilibrium in which agents have converged to the truth about the data generating process and their private expectations are consistent with the expected path of the model economy itself.[4]

[3]The fact that agents in a rational expectations model know the true data-generating process is itself a form of econometric identification restrictions ("cross-equation restrictions") by which the parameters that govern agent expectations are precisely the same as the parameters that govern the mathematical expectations of the economy itself. See Hansen and Sargent (1991) for details.

[4]The learning literature has identified conditions under which this process will actually converge to rational expectations equilibrium. See Evans and Honkapohja (2001).

The rational expectations approach enjoyed strong support in the 1970s from research on asset markets. Following the paper by Fama (1970), the finance literature was heavily influenced by the efficient market hypothesis, which (depending on the exact formulation) states that the prices of financial assets traded on the market reflect all information available at the time. If the price of an asset changes, it must be due to previously unavailable information, so the change would been impossible to forecast ex ante.[5] This theory squared well with the assumptions of the new classical framework in which the agents know how the economy works and in which they efficiently process all available information.

4 Initial assumptions

Early rational expectations models often involved simplifying assumptions made for technical tractability and elegance. Some were relatively uncontroversial such as the assumption that the model should be dynamic with no end-of-time horizon. This required specification of the agents' longevity. The technically simplest, and most prevalent in practice, way of modeling households was to assume agents are infinitely lived. The assumption does not mean that agents are literally immortal. It merely says that current generations care about themselves as much as future generations care about themselves (corrected for time discounting) and that intergenerational transfers of wealth among household members are frictionless.

This infinite-life-of-households assumption was frequently combined with a representative agent paradigm. Again, this does not mean that all households in the economy are identical clones. Rather, the representative agent is intended to serve as an artificial device to represent the behavior of the actual households in aggregate. However, the representative agent does not necessarily represent any particular household, which is referred to in microeconomic theory as the aggregation problem. As discussed at length in Kirman (1992), utility maximization of individual households generally does not imply utility maximization of the representative agent. As the responses of individual households after a policy change cannot be linked to those of the representative agent, it defeats the purpose of microfounding the model to address the Lucas critique. Moreover, some theories may be statistically rejected because agent heterogeneity has been wrongly suppressed by the representative agent rather than because they are wrong *per se*. To represent the sum

[5] In technical terms, stock prices would follow a random walk.

of households' preferences with preferences of a single representative agent requires that they are subject to extremely stringent assumptions about the underlying households' utilities and essentially identical.[6]

Hartley (1997) provides a lengthy discussion on the origins of the representative agent. He asserts that the new classical research agenda initiated by Lucas was heavily inspired by the Walrasian tradition of building general equilibrium models. A natural starting point, therefore, is the modern formalization of general equilibrium due to Arrow and Debreu (1954). This turns out to be essentially impossible for technical reasons. Explicit modeling of multiple agents all trading a variety of commodities and financial assets implies that the number of economic interactions quickly succumbs to the curse of dimensionality. The representative agent paradigm in turn eliminates trade entirely, at least in a closed economy. Financial markets were also simplified by assuming them to be "complete." This technically attractive assumption omits the need for explicit modeling of different asset classes and portfolio compositions. Market completeness also implies that financial markets provide insurance for all possible states of the world. In this ideal world, all idiosyncratic (i.e. applying to a single agent) risk is diversified away and only aggregate risk matters. Both simplifications are convenient in making welfare theorems operational and finding market equilibrium allocations of these models through solving a central planner's problem where the allocation is identical. They also make these models amenable to dynamic programming techniques.

The theoretical problems underlying the representative agent approach led to the development of models with richer sets of agents. Following Campbell and Mankiw (1989), the simplest formulation assumed two agents: savers and borrowers. Departing from the contributions of Truman Bewley, Aiyagari (1994) proposed a growth model with a continuum of infinitely-lived households who only face idiosyncratic income risk that is uninsurable, i.e. markets are incomplete. This arrangement forces people to save for a rainy day and creates a meaningful and non-degenerate distribution of wealth. This simple form of heterogeneity has aggregate consequences because the additional precautionary savings motive lowers the equilibrium interest rate in the economy relative to the economy inhabited by the representative agent.

Extending this framework with aggregate shocks to study aggregate dynamics in the presence of heterogeneity is a daunting task, due to the need to track the evolution of wealth and income distributions over time. Krusell and Smith (1998) showed that it is possible to approximate these

[6] A further problem is that the revealed preferences axioms do not aggregate. See Shafer, 1977.

models in a satisfactory manner by looking only at the mean of distribution. Their main message is that the representative agent framework is only a good approximation as long as the heterogeneity of the true model is unrealistically small (in terms of Gini coefficient) and stems only from income. Adding heterogeneity in preferences goes a long way in matching the data but also significantly widens the gap relative to the predictions of a representative agent model. This heterogeneous agent approach has attracted considerable interest recently (see Kaplan et al., 2018).

Another popular alternative for modeling agents was proposed by Diamond (1965). In this framework, agents are finitely-lived and care only about their own utility. Generations overlap so that there is a demographic structure with young and old agents at any point of time. Generation overlapping, in turn, gives rise to the most basic form of agent heterogeneity. This approach has been highly successful, finding application in academic work and in policy models for analyzing taxation, pension systems and related issues. It has been used much more sparingly in mainstream models designed to guide e.g. monetary policy decisions.[7]

Tractability considerations have always plagued the development of rational expectations models. Nonlinear relationships are prevalent and modeling frequently calls for discontinuities that restrict the use of calculus. This complexity, openly acknowledged by Kocherlakota (2010), forces researchers to make tough modeling choices. One solution, recently advocated by Wright (2018) is not to include a mechanism or friction in macro models unless it can be modeled and microfounded in a satisfactory manner. A much more popular approach, however, is to take reduced-form modeling shortcuts. The current vintage of DSGE models includes many such examples. Convex cost functions, for example, are used not because they are rigorously microfounded or even consistent with the micro evidence, but because they are mathematically convenient. As an illustration, Hartley (1997) and Caballero (2010) use a capital adjustment cost function that describes capital production in an "as if" proxy manner. Since this function is of reduced form and not microfounded, its parameters are also non-structural. There is no guarantee that they are robust to policy changes or immune to the Lucas critique.

Similar doubts can be raised about other parts of the model. For example, the staggered price-setting mechanism of Calvo (1983), now a standard element of New Keynesian DSGE models, is commonly regarded as non-microfounded (partly based on theory and partly because some pre-

[7]Kilponen and Ripatti (2006), Almeida et al. (2013) and Marchiori and Pierrard (2015) provide examples of overlapping generations models used by central banks in the euro area. These models were developed, however, for purposes other than policy-setting and lack an autonomous monetary policy block.

dictions of the Calvo lottery are at odds with micro data).[8] The academic literature, which has devoted considerable attention to the problem of pricing, offers alternative theories in which infrequent price adjustments would be an optimal decision for a firm facing e.g. menu costs (see Golosov and Lucas, 2007). The popularity of these alternatives outside pure academic work has been much lower, presumably due to their low tractability. A final example of a non-microfounded element of DSGE models is the Taylor rule itself (Lubik and Surico, 2010), an overlooked fact given that it shares many features of optimal monetary policy (Woodford, 2001).

The problem of mathematical complexity of rational expectations models has forced researchers to make simplifications not only at the assumptions level but also while solving them.[9] A common approach, log-linearization, involves replacing the actual complex nonlinear model with its approximate linear version in which all relationships can be depicted as straight lines. Log-linearization makes it possible to work with much simpler mathematical objects, albeit at the cost of losing potentially important non-linear dynamics of the model. For example, in a log-linearized model the responses to positive shocks are simply mirror images of those to negative shocks. Similarly, large shocks become simply scaled-up small shocks. All asymmetric dynamics as well as threshold and scale effects are ignored. Therefore, the modeler could make inference mistakes not necessarily because of imperfect model's assumptions but because of its poor approximation. Solutions based on a full unapproximated model, so-called "global methods," are available, but usually involve a huge computational burden. Even today they are only applied to relatively small, stylized models. They have seen very limited use in larger policy models that attempt to capture many mechanisms concurrently.

The process of taking models to the data and their quantitative assessment creates its own set of complex problems. The failure of early attempts to estimate rational expectations and real business cycle models using maximum likelihood (see e.g. Evans and Honkapohja, 2005) was not only because these models are too stylized to statistically mimic the data, but also because the likelihood functions they generate are highly nonlinear and poorly behaved. In particular, the

[8] Empirically, the hazard function is decreasing, i.e. the longer a firm keeps prices unchanged, the higher the probability it will continue to do so. This is not the case for the Calvo price-setting mechanism. Whether a firm can adjust prices in a given period is a random variable independent of the past, so the hazard function is constant. From a microfoundations perspective, it is also problematic that the firm cannot optimally choose the time at which it readjusts prices.

[9] A "solution" is a mathematical expression by which optimal decisions of agents such as how much households should save or consume in a given period are expressed only in terms of shocks or variables known from the past. They no longer depend on other decision variables.

likelihood may exhibit local (as opposed to global) maxima, or cliffs and ridges in the parameter space along which it barely changes. It is therefore quite easy to end up with a wrong estimate of the model parameters, even if the model is correctly specified. As the complexity of DSGE models has increased over time, so have these issues. Canova and Sala (2009), for example, report poor identification of Calvo pricing parameters. Frustration with these problems fueled the popularity of calibration in the 1980s (Hansen and Heckman, 1996). Calibration, in turn, was supplanted by Bayesian techniques in the late 1990s as computing power soared (Herbst and Schorfheide, 2016).

5 Post-crisis challenges

As is clear from the discussion above, many of the problems of DSGE models were well recognized before the Global Financial Crisis. The fact that the advanced economies entered the period of Great Moderation created a false impression that the modeling assumptions, even if debatable on theoretical grounds, were sufficient for applied work and that policies based on these models were successful. The Global Financial Crisis not only forced the profession to seriously rethink these assumptions but also brought new empirical challenges questioning some old truths.

A common assumption in DSGE models developed before the crisis was the lack of frictions in the financial sector. Finance worked as a "veil," meaning all agents in the economy could insure themselves against idiosyncratic shocks at all times due to the assumed market completeness. Such models were poorly suited to the study of financial panics, crises or liquidity dry-ups. Indeed, perfect insurance implied that a serious financial crisis was impossible in such model economies.

While it may be fair to say the role of the finance in macroeconomics was under-emphasized before the Global Financial Crisis, phenomena such as sovereign debt crises and defaults (Eaton and Gersovitz, 1981, bank panics (Diamond and Dybvig, 1983), and financial market rigidities due to moral hazard (Holmström and Tirole, 1997), asymmetric information between borrowers and lenders (Bernanke and Gertler, 1989) and credit constraints in the form of an outright borrowing limit (Kiyotaki and Moore, 1997) were actively studied. The problem was that these academic insights were rarely incorporated into cutting-edge policy DSGE models. Oliver Blanchard (Blanchard, 2017), acknowledging the lack of attention, commented: "I remember telling Bengt [Holmström] that, while I admired his work on liquidity with Jean [Tirole], I was not sure how central it was to macro."

Financial crises in developed countries were largely considered a thing of the past, and following the events in East Asia, Latin America and Russia in the 1990s, associated primarily with less institutionally sound emerging economies. The experience from the financial liberalizations and crises in Finland in Sweden in the early 1990s received relatively little notice beyond the Nordics and fell largely into oblivion.[10]

Interest in the pre-crisis models with financial financial frictions revived after 2007. But these studies were only partly suited to address the new questions. For example, the popular financial accelerator model by (Bernanke et al., 1999) focused on the frictions between the borrowing non-financial corporations and the lending households. Several recent papers focus in turn on the role of banks and bank balance sheets in propagating shocks (e.g. Gertler and Kiyotaki, 2010). Others stress the role of frictions arising from the mortgage market. For example, in their discussion on the state of macroeconomic modeling, Hendry and Muellbauer (2018) argue for an explicitly distinct role for the housing sector and its indebtedness. They point to the perils of boiling down household assets and liabilities to a single net worth measure. This is because of different degrees of liquidity, elasticities and wealth effects associated with various asset classes (cash, house, pension fund, etc.).

Despite widespread agreement on the necessity of modeling financial frictions explicitly, there is as yet no consensus regarding the methodology or the sectors on which to focus. The picture is further clouded by the roles of various rigidities at different times. For example, bank runs and panics occur only in extraordinary circumstances, whereas asymmetric information and moral hazard are concerns that persist throughout the business cycle.

The benchmark New Keynesian DSGE model was ill-suited to study unconventional policy measures, jointly referred to as "quantitative easing." The model relies on a single class of assets (a one-period nominal bond), and does not allow for longer-term assets of varying degrees of riskiness, duration or liquidity.[11] Broadening of asset classes traded by the Fed was at the heart of each of the three quantitative easing rounds and Operation Twist.

Another unconventional monetary policy tool, i.e. forward guidance, was studied already before the crisis (see Eggertsson and Woodford, 2003). The New Keynesian DSGE model predicted a very strong impact of this policy, which was due to its previously discussed assumptions, specifically

[10]The US experienced the occasional financial turbulence (e.g. the S&L crisis, Black Monday in 1987, the demise of LBOs, the bankruptcy of Long-Term Capital Management and the bursting of the dot-com bubble) but all these events were isolated rather than systemic. They were also considered mild by historical standards.

[11]This asset homogeneity was partly due to a theoretical result of Wallace (1981) according to which the central bank's asset composition is irrelevant.

rational expectations (i.e. people have no uncertainty regarding how the economy or policies work), the representative, infinitely-lived household paradigm (which puts considerable weight on future periods and generations), and market completeness (which allows smoothing of consumption over time). In practice the effectiveness of these policies was much more muted, a discrepancy labeled as "forward guidance puzzle".[12]

The assumptions behind the pre-crisis DSGE models are also closely related to their forecasting ability. It is widely acknowledged that these models were unable to predict the financial crisis of 2007-2008, i.e. the collapse of the housing market in the U.S. and the following solvency problems of financial institutions investing in it. It is less obvious whether economists working with the DSGE framework should take full blame for this failure. After all, the original motivation for building microfounded or DSGE models was to analyze alternative policy scenarios, not improve short-term forecasting.[13] Traditional SEM models (and other econometric models built for forecasting purposes) failed to foresee the crisis as well. This failure was implicitly conceded by Wren-Lewis (2018).[14]

Lindé et al. (2016) run a conditional forecast exercise and ask whether the macro models could have forecasted the Great Recession given a strong negative shock coming from the financial sector, i.e. without explaining how these shocks arose. If one takes a benchmark pre-crisis New Keynesian DSGE model like Smets and Wouters (2007), the answer is a resounding no. That model extended with financial frictions and non-Gaussian shocks performs somewhat, although not sufficiently better. Del Negro et al. (2015) and Del Negro and Schorfheide (2013) argue that the economic slump was largely predictable by a DSGE model with financial frictions fed with asset price data until the third quarter of 2008, i.e. including the collapse of Lehman Brothers. That exercise beats the consensus of professional forecasters, even if it sidesteps the critique that the economics profession as a whole (including professional forecasters) generally do a poor job of forecasting.

A separate question is whether DSGE models have much use as forecasting tool more generally. The strong growth of popularity of the New Keynesian framework before the crisis, as mentioned, was due in part to its good forecasting performance relative to Bayesian vector autoregressions (see Smets and Wouters, 2003). However, Gürkaynak et al. (2013) obtain more mixed results regarding

[12] A more detailed summary on modeling unconventional monetary policies can be found in Granziera et al. (2018).
[13] In fact, Lucas (1976) explicitly acknowledged the good forecasting accuracy of contemporary econometric models.
[14] Wren-Lewis blames this on the fact that SEM models had fallen out of fashion after the DSGE revolution, so they were never developed to incorporate financial frictions and other factors.

the relative performance of DSGE models versus purely statistical methods. The jury is still out.

Inflation and inflation expectations, another area of concern for forecasting performance, is of particular interest to central banks. The high degree of uncertainty regarding inflation dynamics, present already before the crisis (Atkeson and Ohanian, 2001, Stock and Watson, 2008), increased with the implementation of the unconventional policy measures. These programs initially created fears of looming inflation in some circles.[15] Other economists, including Krugman (2018), noted the "missing" disinflation that should typically be present in a situation of high unemployment. Neither of these gloomy predictions materialized. In fact, inflation seems to have been more stable and insulated from the rest of the economy, in particular from output and unemployment, than any theory at the time predicted.

The discussion concentrates on a modern version of the Phillips curve, which remains a key relationship embedded in New Keynesian DSGE models. Some authors (see, e.g., Blanchard, 2016) argue that the Phillips curve relationship has merely "flattened" (i.e. inflation has become less sensitive to variations in output in unemployment), although the extent of such flattening is still uncertain (see e.g. Gilchrist et al., 2017 and Dotsey et al., 2017). The inability to capture precisely the price dynamics poses a special challenge for New Keynesian DSGE models. This is because the study of price rigidities, inflation dynamics and monetary policy designed to offset these inefficiencies was, if anything, the key reason for which they were designed in the first place. McLeay and Tenreyro (2018) interpret the flattening as evidence that monetary policy has followed the Taylor rule very closely. Lack of significant departures from the Taylor rule makes it difficult to identify exogenous monetary policy variation in the data that would allow to identify the Phillips curve.

6 What needs to change

The list of disputes around the current vintage of macroeconomic models is longer than the examples discussed so far.[16] As argued by Reis (2018), this diversity of opinions should be interpreted as a sign of vigor and strength in the field of macroeconomics, even if outsiders do not see it that way. Krugman (2018) notes that the world avoided calamity similar to the Great Depression in the 1930s in the period after 2008. This may suggest that scientists and policymakers understand the

[15] See, for example the open letter to Ben Bernanke (Asness et al., 2010), signed by, among others, Michael Boskin, Charles Calomiris, Ronald McKinnon and John Taylor.

[16] For example, macroeconomists disagree on the effectiveness of fiscal policies during the crisis (i.e. size of the "fiscal multiplier"), the role of social transfers and many other issues.

economy better than they did 90 years ago. Krugman argues that the absence of "utter disaster" further suggests that the theoretical foundations of economics are less likely to crumble, even if he remains critical of many aspects the DSGE paradigm. Nevertheless, there is a growing consensus that the way in which applied policy macro models are built and what elements they should include has to change. Recognizing the role of finance, as discussed previously, is commonly mentioned, but there are other issues to consider.

A widespread view in the wake of the crisis of DSGE modeling is that although microfounding models is the right strategy, the current forms of microfoundations need revision. Historically, relatively little attention was paid to the empirical microeconomic evidence while constructing these models, largely because of its limited availability. This started to change in the 1990s (see e.g. Browning et al., 1999 and Fernández-Villaverde, 2008). It is now more widely recognized that micro data need to play a much bigger role. A key message from microeconometric work is that heterogeneity of agents' preferences, financial wealth and constraints, as well as productivities, all matter. This is the motivation behind recent modifications of the New Keynesian DSGE model (see e.g. Kaplan et al., 2018). Ghironi (2018) proposes focusing on diversity between firms and sectors. Another lively research agenda involves revisiting the behavior of prices and their stickiness. Recent contributions by e.g. Alvarez et al. (2018) and Nakamura et al. (2018) question the empirical predictions of Calvo pricing and its strong welfare implications.

These research efforts all seek to remedy the aggregation problem and depart from the representative agent paradigm, in which, as argued before, there is really no distinction between micro and macro levels. Other possible ways to model households involves utilizing some versions of the overlapping generations framework. This is a potentially appealing way to accommodate different groups of households, e.g. young indebted versus older savers, without having to resort to full-fledged (and more challenging technically) distributions. For example, recent empirical study by Wong (2018) suggests that it is mainly indebted young people who react to monetary policy changes. The overlapping generations approach also easily handles the situation in which the interest rate falls below zero (or at least below the rate of growth of the economy), as shown in Eggertsson et al. (2017). This is an attractive feature given that negative interest rates observed in recent years (see e.g. Barsky et al., 2014) is another empirical fact that does not fit in conveniently with the representative agent framework.[17]

[17]In a representative agent framework, the steady state interest rate is the inverse of the discount factor. The

There are also grounds for revisiting the Lucas critique. Given the soundness of Lucas' theoretical argument, the profession has accepted it almost as orthodoxy or an axiom (Rudebusch, 2005). However, the early proponents of microfounded models viewed the problem of structure stability as a purely empirical, not theoretical (Lucas and Sargent, 1979). The initial econometric tests performed in a series of papers by David Hendry and his coauthors (see e.g. Favero and Hendry, 1992) were based on the idea of superexogeneity and tended to reject its relevance.[18] Lindé (2001) then found the Lucas critique to be quantitatively relevant, and questioned previous testing methods demonstrating their low power. Lubik and Surico (2010) also pointed to the necessity of looking at second moments. Rudebusch (2005), on the other hand, reached opposite results by working with a different model specification than Lindé (2001). Fernández-Villaverde (2008) and Lubik and Surico (2010) found that it is difficult to separate structural breaks in the data (which may be due to a policy regime change) from other shocks. This may be interpreted not only as a sign of powerless tests (as in Lindé, 2001) but also as evidence that the empirical relevance of parameter instability may be relatively modest as compared to the volatility of stationary shocks. Overall, however, it is fair to say that this list of contributions on this issue is surprisingly short given the immense impact the Lucas critique has had on the discipline.[19] This point, made long ago by Fischer (1983), remains valid today. For example, the introduction of forward guidance and quantitative easing policies can be regarded as the largest policy regime switch since the Volcker contraction. It provides an attractive environment to study this question, but has so far remained unexploited. More generally, existing contributions have rarely reached beyond the application of the Lucas critique to monetary policy.

Second, the line between structural and reduced-form parameters and model parts remains insufficiently clear. Since the original article of Lucas (1976), it has been widely assumed that stable parameters and structures pertain to preferences and technology. However, the appropriate distinction is more likely to depend on the problem at hand and the precise application. For example, the share of labor income in GDP, a parameter widely assumed to be stable, evolves over time (see Karabarbounis and Neiman, 2014) and may well be affected by changes in policy, in particular taxation of labor and capital. These remarks beg the question as to how structural

latter has to be less than unity, which implies that interest rates can become negative only temporarily after a shock that makes the economy deviate from its steady state.

[18] See also Ericsson and Irons (1995) for a review of this literature.

[19] Other papers include Estrella and Fuhrer (1999) and Collard et al. (2002).

and robust to the Lucas critique are modern DSGE models. In an early attempt to answer this problem, Fernández-Villaverde and Rubio-Ramírez (2008) found considerable instability of wage and price stickiness parameters which may, however, be also a symptom of their poor identification, as reported by Canova and Sala (2009). Difficulties in detecting parameter instability are also discussed in Inoue and Rossi (2011).

In their assessment of DSGE modeling, Caballero (2010) and Wren-Lewis (2018) argue that it is not always clear whether the gain from internal consistency of DSGE models always outweighs the cost of model misspecification and whether a wrong structural model is better than one that ignores the Lucas critique altogether. Indeed, the problem of misspecification and the strength of the Lucas critique are interlinked in complicated ways. Lindé (2001) and Cogley and Yagihashi (2010) remind that structural parameters remain stable in estimation only if the model is correctly specified. Therefore, exercises which highlight instability of structural parameters may in fact signal misspecification of a particular DSGE model, rather than the fact that DSGE models are subject to the Lucas critique in general. This puts in doubt Lucas' original empirical argument that unstable parameters are a signal of wrongly ignored behavioral changes due to policy regime changes. For example, Chang et al. (2013) show that if a heterogeneous agent economy is modeled with a misspecified representative agent model, the result may be instability in preference and technology parameters. Canova et al. (2015) explore the misspecification issue further, distinguishing between parameter variations that can be interpreted as exogenous shocks and those that look more as an omitted endogenous mechanism. They show that not allowing for time variation in the latter case (i.e. when the true process includes endogenous time variation) may result in large distortions in the results.

The problem of misspecification can also be viewed through the lens of shocks that hit the model economy. Modern microfounded policy models include several, even dozens of shocks that are assumed to be independent and primitive driving forces of the economy. In practice, however, their structural interpretation is questionable, a problem figuratively described by Romer (forthcoming). Instead, these shocks are increasingly treated as a "measure of ignorance". This means that they are interpreted more in the spirit of "wedges" (Chari et al., 2007, i.e. indicators of dimensions in which the model lacks fit or is misspecified. This is not a problem *per se*, but such interpretation is clearly at odds with the initial motivation for building microfounded models. In terms of diagnostics, such shocks exhibit distinct patterns when backed out from the data using a misspecified model. For

example, they exhibit strong in-sample correlations, despite the assumed independence. They also show up in variance decompositions whereby each variable is largely driven by its own shock and there is little empirical interaction between the variables that the model captures (Andrle, 2014).

It should also be stressed that the Lucas critique is based on the assumption that expectations are rational and agents have full information. There is a growing understanding of the need to move beyond these assumptions. Empirical tests since the 1980s have repeatedly reported deviations from full information rational expectations (see Coibion et al., 2017 for a discussion and recent evidence). What are the options? One relatively mature theory is the previously mentioned learning (by least squares or Bayesian updating). Other frameworks developed before the crisis assume that agents are still rational but face some costs or frictions in obtaining information. Rational inattention (Sims, 2003) and information stickiness (Mankiw and Reis, 2002) frameworks are good examples. More recent approaches depart from rationality and include k-level thinking and higher order beliefs (see Granziera et al. (2018) for a broader discussion) or frameworks motivated by psychology or neuroscience, for example, perceptual biases (Khaw et al., 2017) or diagnostic expectations (Bordalo et al., 2016). Nevertheless, most of these approaches are still in the early development stage. The relevance of the Lucas critique when full information and rational expectations are relaxed remains an open question. Early evidence of Gabaix (2016) suggests that the critique largely loses its bite when agents become myopic.

Finally, an important aspect is the frequency domain of the data under study. Some variables, such as stock prices or currencies tend to be volatile at high (e.g. daily frequencies), while others tend to move slowly, with cycles lasting years or even decades. An important example here involves stabilizing inflation or its volatility. Policy actions may have different consequences at different frequencies and there may be tradeoffs involved (see e.g. Brock et al., 2008). Simple rules disregarding the frequency domain may be suboptimal given that textbook economic theory suggests that monetary policy is most effective over business cycle frequencies. Another example where frequency analysis might be helpful is business cycle analysis. Traditionally, business cycle fluctuations, i.e. oscillations of frequency up to eight years, have been treated separately from lower-frequency movements. The latter would traditionally be in the scope of interest of growth theory. In other words, movements of output would be split by an artificial frequency cutoff and analyzed separately by two distinct subfields rarely talking to each other. Such approach, although useful at times, precludes understanding issues such as permanent effects of recessions ("hysteresis") or exploring the possible

long-run effects of monetary policy (money non-neutrality) (Blanchard, 2018b).

7 Concluding remarks

The current discussion about the state of macroeconomics may appear to outsiders as a sweeping critique. However, the breadth of work discussed above actually demonstrates that some of the problems were well known in academia and worked on before the Great Recession. This research has permeated to applied policy work and teaching too little and too slowly. This is partly due to tractability constraints and partly to the fact that many frictions were considered no longer relevant in the era of Great Moderation. In any case, a post-crisis textbook core model has yet to be created.

Despite its limitations, it seems unlikely that the broadly understood DSGE framework will or even should be abandoned altogether. As put succinctly by Korinek (forthcoming), the critique of DSGE models is not about the fact that they are dynamic, stochastic or general equilibrium, but about other specific assumptions that can be or are subject to change. It also has the advantage of the incumbent or perhaps benefits from the "tyranny of the status quo." Evolutionary changes made to the existing framework are easier to communicate and absorb than vastly different approaches, not only by decision-makers but also by academics. Moreover, frameworks such as agent-based models postulated by Haldane and Turrell (2018), are still insufficiently developed and not yet a viable alternative.

In any case, it is quite clear that the task of building a satisfactory general equilibrium macroeconomic model is likely to remain an ongoing project for the foreseeable future. To some extent the to-do-list above can be seen as a daunting task, at times even as a self-contradictory wish list that cannot be fully addressed even with unlimited human resources, which is a skepticism expressed by Stiglitz (2018). Given the complexity of the real world, it is probably impossible to incorporate all desired changes into a single new benchmark model. It is not even clear whether such a project is desirable in the first place. The fact that macroeconomic models have to simplify many aspects of reality also means that macroeconomists have to be modest in their pursuits and put a stop to what Caballero (2010), following von Hayek (1989), referred to as the "pretense-of-knowledge" syndrome. According to Caballero it is better to be realistic in the single dimension of interest and simplify the remaining aspects of the economy. This is against the current logic of DSGE models that try

to be a bit good in everything and really good in nothing. Macroeconomics is a reductionist field by nature and simplification is unavoidable.

The list of limitations and simplifications discussed throughout this review also calls for much more model validation than is usually reported in applied work. This concerns diagnostic tools and tests but not only. Equally important are sensitivity to chosen priors, sample uncertainty as well as the choice of estimation and solution techniques (see e.g. Canova, 2007). It is also the problem of robustness to various assumptions and specifications. One step in this dimension is the work on robust control theory which attempts to provide decision rules when the researcher recognizes that the model is only an imperfect approximation of the true data generating process (see Hansen and Sargent, 2008). More generally, DSGE models are not and cannot be the only tool used in policy making. Instead, as postulated byBlanchard (2018a), a more diversified approach to applied macroeconomics is desirable, one in which various types of models coexist and serve as validating devices against themselves. As discussed by Tenreyro (2018) using the example of the Bank of England, decisions are made based in practice on a broad set of information sources and classes of models. Any model, even a very good one, is only one of these sources.

The advantage of DSGE models relative to other tools is also not always clear. Changes in the policy regimes studied by Lucas (1976) involve systematic changes of rules (e.g. inflation targeting vs. nominal GDP targeting), rather than temporary deviations from extant rules. Such policy shifts occur infrequently, as noted already by Sims (1980) and, in a different guise, more recently by Kocherlakota (2018). According to Kocherlakota, practical policy making most of the time involves decisions based on private information of the policymaker. It does not lead the private sector to think that the rules of the game have changed. As a consequence, optimal choices in these situations can be based on simple regressions on past data rather than on microfounded structural models. In the terminology of Leeper and Zha (2003), policy interventions may be considered "modest" if they appear as a shock that is small by historical standards. Such shocks do not change the formation of expectations about future policy. Finally, upcoming policy regime changes are much better messaged than in the past. Major policy reforms tend to be publicly debated for extended periods, even years, before they happen. This is a very different situation than the Volcker disinflation, the textbook example of an unforeseen regime change. This predictability suggests that, depending on the specifics regarding the information and expectations of the agents, the regime change permeates the structure of the economy only gradually.

At the most general level, the need for a deeper understanding of finance and of human behavior and expectations make it imperative to intensify the dialogue between macroeconomics and economists working in other branches, especially microeconomics and financial economics, or even scientists representing other disciplines. This lack of communication and disregard of micro and financial data was apparent before the crisis. In the mid-2000s, macroeconomists were mainly concerned about global imbalances, the savings glut (Bernanke, 2005) and the inattentiveness of long-term interest rates to changes monetary policy stance (the "Greenspan Conundrum"). Financial economists, in contrast, were worried about rising systemic risk and shadow banking, as stressed by Rajan (2005). Looking back, these phenomena were arguably two sides of the same coin.

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