BANK FOR INTERNATIONAL SETTLEMENTS

In defence of central bank DSGE modelling

Luiz Awazu Pereira da Silva[1]
Deputy General Manager, Bank for International Settlements

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I am pleased to open the Seventh BIS Research Network meeting, centred this year on "pushing the frontier of central bank modelling". Let me say a few words, first, in defence of central banks' modelling work and about dynamic stochastic general equilibrium (DSGE) modelling efforts[2]. Then, second, from a policy perspective, let me suggest where I think the modelling research frontier has been making progress but could be pushed further to become even more useful to policymakers.

Introduction

But **why "in defence" of central banks' DSGE modelling work?** Because of the harsh criticism received by central banks' modellers and DSGE models after the Global Financial Crisis (GFC). What were the main criticisms?

They go like this: we – the community of central bank modellers – failed to see the GFC coming; central banks' models lost predictive capability; we were too focused on the success of our structural models and their capacity to forecast low and stable inflation during the Great Moderation; and we were too busy congratulating ourselves for our performance as inflation targeters.

In addition, DSGEs became the scapegoat for all the human shortcomings in foreseeing and preventing the GFC. Indeed, our failures originate from using DSGE models that are seriously flawed; with oversimplified microfoundations of consumers' and firms' behaviour; with a linear structure that overlooks non-linear drivers of financial stress such as shortage of liquidity, leverage and fire sales of assets; with excessive ad hoc parametrisation that drives results too much. So these DSGEs are a poor guide for policy, especially for assessing financial stability issues, and their results – unlike those from the structural models that some IT frameworks use – are difficult to communicate to the general public. Finally, and more importantly, they lack a proper modelling of financial frictions, and their transmission channels of financial factors into the real economy are therefore limited. In a nutshell, they provide an illusion of a "scientific approach" but are in fact a dead end for those who want to explore macro-financial linkages and derive operational practical policy recommendations from fundamental research.

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[1] Deputy General Manager, Bank for International Settlements (BIS). These remarks benefited from an initial draft by Giovanni Lombardo and comments from Pierre-Richard Agénor and Ingo Fender. However, the views expressed here are my own and do not necessarily reflect those of the BIS.

[2] Olivier Blanchard (2016) wrote a very concise policy brief that summarizes the controversy surrounding the topic, "Do DSGE Models Have a Future?" for the Peterson Institute for International Economics. The criticism has been widely publicized in journals and blogs since the GFC and involved many prominent names in the profession.

Now that's unfair to us; this **critique of central banks' modelling work is exaggerated**. Why? Because DSGEs are not that simplistic, because a lot of work on improving their structure and their modelling of financial imperfections started long before the GFC and accelerated significantly in its aftermath; and because we learned a lot about the links between macro and finance during the GFC precisely with improved post-GFC DSGEs. I fully subscribe to the key arguments presented in two recent pieces along these lines of defence by Olivier Blanchard (2016) and Ricardo Reis (2018).

On poor forecasting of the GFC, yes indeed, true, but as Ricardo Reis states, relative to what? Like medical sciences, we can do reasonably accurate conditional forecasts, not predict the exact time of someone's death. For a once-in-a century event, and immediately after Lehman we defined a set of probable associated factors. Then we moved swiftly, understanding the gravity and urgency of policy actions and avoiding another Great Depression. It was for me a sign that the profession has studied financial crises and learned, especially reflecting on the supervisory and regulatory responsibilities in the run-up to the GFC as exemplified by Alan Blinder (2013). A large body of regulations has emerged since, mostly for the purpose of precisely addressing these past failings.

Moreover, as explained by Christiano et al (2017), DSGE models fulfil a different, albeit important, task vis-à-vis conditional forecasting: they can help policymakers to assess policy trade-offs by conducting quantitative experiments that are unavailable in social sciences on actual economies.

Now there were some early warnings about systemic financial risks before the GFC. I am not talking about the heroic insights of Charles Kindleberger or Hyman Minsky. To be fair, a number of observers – among them the BIS – expressed long before the GFC concern with the systemic risks that financial excesses bring about (eg Borio (2007)), or the perverse role of wrong incentives (Rajan (2005)) and thus in particular with the financial euphoria and some policy neglect that preceded the crises.

The existing comprehensive surveys of financial macro-modelling (Brunnermeier et al (2013)) or of the macro-financial linkages literature (Claessens and Kose (2018)) show that financial factors were present in macro-modelling long before the GFC. They start with the seminal work of Kiyotaki and Moore (1997) on collateral constraints and Bernanke et al (1999) on the financial accelerator that was itself based on the costly state verification model of Townsend (1979). Some of this early literature also used the double hazard model of Holmström and Tirole (1997). These approaches profited from these novel insights to explain credit booms and the role of assets (and their jumps in value) in determining borrowing behaviour as they affect either the prices (via the finance premium) or the quantity (via collateralisation) of funds borrowed. Nevertheless, they were not mainstream, and although their importance can easily be recognised ex post, they had limited impact on broader academic research and policy.

Nonetheless, central banks were at the forefront of this pre-GFC period of macro-modelling progress. Naturally, they benefited from the work of outstanding academics, Chris Sims being one of them (see Leeper and Sims (1994), Christiano et al (2005)), and following the trail opened by the seminal work of central bankers like Frank Smets and Ralf Wouters (Smets and Wouters (2003)). For example, among the most prominent forerunners in central banks' modelling was a DSGE model featuring a role for financial intermediation and financial shocks: the so-called CMR model developed at the ECB. Interestingly, this was a model inspired in large part by the Great Depression (eg Christiano et al (2003)).

These developments show that central banks participated very early on with academics in the "improvement" of DSGEs, especially their inclusion of financial factors. Hence recognising some of the shortcomings of DSGEs pointed out by critics does not mean that they could not be a useful starting point for macro-financial modelling by establishing solid foundations that are accepted by the profession. This is what Olivier Blanchard calls a "core structure around which to build and organize discussions".

DSGEs indeed constitute an important improvement in macro-modelling in the past few decades, allowing increasing adherence to scientific principles. The backlash against macro-modelling brought about by the GFC need not derail this modernisation process. For example, critics attacked DSGE models for being solved using linear approximations, thus misinterpreting technical/computing limitations as

desiderata of the new macro-modelling research agenda. In parallel, both academics and researchers at central banks have been striving to make progress on solution techniques, once more inspired also by the work of Chris Sims (Kim et al (2008)). Linearisation (both in theoretical models and in empirical research) is not a desideratum of the research agenda. We do have advanced methods to solve non-linear models, but the problem is about the size that these methods can handle.

Therefore, **the modelling research frontier has been pushed by academics and central banks**. In which directions has it been most useful? Before the crisis, Tovar (2008) discussed the main usage of DSGEs by central banks. In a more comprehensive way, Christiano et al (2011, 2017) review the usage of DSGEs for monetary policy purposes, showing how much progress has been made since the GFC. After early versions that implicitly assumed perfect and complete financial markets, in most central banks DSGEs' financial frictions and credit channels were included to analyse macro-financial interactions and shocks (eg Roger and Vleck (2011)). By doing so, this generation of post-GFC DSGEs were also capable of introducing new policy instruments in the analysis such as macroprudential tools (MaPs) that eventually affect the cost of financing.

I see two important policy contributions here. First, these new DSGEs are essential to discuss the policy mix between monetary policy and macroprudential policy, ie the best way to "lean against the wind", the complementarity of the two instruments to attain the objectives of both macroeconomic and financial stability. Several contributions, including meta-analysis of these DSGEs (Carré et al (2015)) that tests the relevance of augmented Taylor rules using MaPs, show their complementarity to monetary policy. When specific metrics for financial stability are defined, it is possible to define optimal levels of MaP interventions. This could be particularly important for small open economies that are subject to "sudden floods" of capital flows. These flows often generate an expansion in credit and activity, as well as asset price pressures. These new DSGEs can show that countercyclical MaPs (eg a Basel III-type capital rule based on credit gaps, or a countercyclical reserve requirement rule), can be highly effective in terms of promoting both macro stability and financial stability (Agénor et al (2014, 2018a)).

Second, these DSGEs can contribute to exploring in a more rigorous quantitative way the future of inflation targeting, by providing a framework that takes into account the expanded toolkit of instruments including MaPs, CFM measures and FX interventions especially to help achieve financial stability and prudential objectives in an open economy. That is particularly important for large middle-income countries, which tend to be subject to significant financial shocks and volatility (eg Agénor and Pereira da Silva (2013)).

What research area related to modelling needs to be pushed further? There are many important areas to remember: increasing agents' heterogeneity; improving adherence to data, for example by testing models' performance against other econometric techniques; and also enriching the models with other policy instruments in the vast domain of the macroprudential toolkit.

I would suggest pushing the modelling frontier to multi-country DSGEs with financial frictions and spillovers. This kind of setup could answer some important practical questions and convince policymakers of the benefits of international coordination, especially of macroprudential policies. Why is it important? Because we live in a financially integrated world with increasing evidence of these spillovers and spillbacks between advanced economies and large middle-income countries in the transmission of financial shocks (Bruno and Shin (2015), Agénor and Pereira da Silva (2018)).

Yes, it is true that the early empirical literature on the gains from international monetary policy coordination found only modest gains. But that's probably because it did not account for capital flows and largely abstracted from the financial system and its role in magnifying the response to shocks. Hence, capturing the implications of greater international financial integration through capital flows, as well as financial frictions at home and abroad, could potentially make the welfare benefits from policy coordination significantly larger. The area of macroprudential policy coordination has only recently begun to produce contributions, and more model-based contributions showing possible gains from international

macroprudential policy coordination are needed. This new view could support the current carefully prepared approach in policy communication by major central banks and/or recommend MaPs to be used cautiously given their spillovers into other jurisdictions. A non-cooperative reaction to these spillovers can produce an escalation of ad hoc MaP actions that eventually could lead to "regulatory wars". Now these new modelling efforts are just starting, here at the BIS with Giovanni Lombardo along the lines of Agénor et al (2018b). Indeed, the gains from international macroprudential policy coordination can be studied in a two-region, core-periphery model with a global bank and financial frictions, with periphery banks borrowing from the core global bank to fund domestic lending. This type of exercise has found that these gains can be significant for the world economy. However gains are not equally distributed across countries; depending on the origin and the nature of shocks, gains for the periphery can be larger than those accruing to the core region and vice versa. This could point to potential political economy obstacles to the implementation of cooperative policies.

Conclusions

There has been enormous progress with new post-crisis DSGEs since the GFC. But this progress is not purely of academic interest. It has allowed policymakers to think in a more rigorous way, using monetary policy and MaPs, about ways to achieve both macroeconomic and financial stability. These tools, in addition to other models and stress tests, also allowed our institutional arrangements, such as financial stability committees, to be designed and/or strengthened, and the analysis in our financial stability reports to be improved. In the future, further exercises with multi-country DSGEs might contribute to finding practical ways to promote international macroprudential policy coordination. As I have discussed elsewhere (Agénor and Pereira da Silva (2018)), given the challenges of our interconnected financial world a further strengthening of the current statistical, empirical and analytical work conducted by academia, central banks and international institutions, including the BIS, is warranted.

Macro models in general and DSGEs with their ongoing improvements will remain important and useful tools for policy trade-off analysis. Obviously, they will always carry shortcomings that will need to be addressed. For example, ideally we'd like our macro models to generate the endogenous build-up of financial imbalances and to assess the real consequences of disorderly unwinding instead of simply using exogenous shocks and scenarios. However, our tools already permit policy interventions, monetary or financial, to be simulated to assess the scope for action and results. Our macro models can already return valuable information for supervisors and regulators, alerting them of the systemic consequences of their prediction and suggesting the timing and location of more rigorous and precise policy action.

I hope that meetings like this one can be an opportunity for the academic and central bank community to take a critical "selfie", and spot limitations as well as promising avenues in central banks' modelling agendas. Let's keep pushing the frontier of macroeconomic research at central banks, and recognise without complacency – since much still has to be achieved – the great progress that has been made in the last few decades.

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