Comments on the paper

"Macroeconomic Policy in DSGE and Agent-Based Models" by G. Fagiolo and A. Roventini

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From the very beginning (from the title, I should say) the authors (F&R hereafter) pursue the goal of comparing and contrasting the relative merits of the DSGE and ABM approaches, with reference, in particular, to policy implications. The comparison between DSGE and ABM is carried out almost everywhere in the text—sometimes ...between lines—and is made explicit especially in the introduction and in the concluding remarks. I think that F&R have brilliantly exposed the weaknesses of DSGE models (sections 2 and 3), have been successful in providing an overview of ABM (interpreted as a way out of the strictures of the DSGE approach, see section 4) but their comparative assessment of DSGE and AB models is not convincing. T*he authors' evaluation is that ABM beats DSGE hands down* but this assessment is clearly unbalanced. Let me reveal my priors before proceeding: I am aware of the limitations of DSGE models—which have been spelled out by many authors, especially since the onset of the Global Financial Crisis (GFC), and are thoroughly surveyed in the paper—and I am very much in favor (to say the least!) of ABM but I think that such an unbalanced assessment of the two streams of literature is not only unrealistic but also not useful, especially in terms of future developments of the AB literature. It may be overly optimistic and slow down the pace of development and refinement of ABM.

New Keynesian DSGE modeling has a honored and by now quite long history. This body of literature has grown over a span of more than two decades in the usual manner, *i.e.* by addition of missing elements (with respect to the three-equations model sketched in section 2 by F&R) and by twists and turns dictated by new macroeconomic evidence. For instance financial factors have been introduced in this literature since the end of the '90s (even if they have gained

center stage only after the onset of the GFC)[1]. It is true, as stated over and again by F&R, that these models cannot capture, almost by construction, some of the basic features of the GFC and therefore cannot be used to forecast the advent of a financial crisis. In many instances, well known proponents of this approach have recognized this limitation: DSGE models are useful in macro- economic forecasting "in normal times" but almost useless in the proximity (or during) a financial crisis and the ensuing recession. Therefore, if we want to capture at least some of the features of the GFC we have to go beyond DSGE macro models.

Are ABMs an alternative? F&R's answer to this question is a resounding yes! Mine is a more cautious: not yet.

Contrary to the DSGE literature, AB macroeconomics is still in its infancy.[2] It is true that, by construction, form a specific point of view ABM are better than DSGE models: There are research tasks, in fact, that can easily be carried out in ABM and are by construction out of the reach of DSGE models. In particular, one can generate artificial cross sectional evidence (through simulations) and compare the simulated evidence with the empirical one. For instance most of the ABMs mentioned in the references generate a power law distribution of the firms' size. This unique capability, however, is of limited use in assessing the emergence of a financial crisis.

As to the aggregate evidence, it is indeed true that all the ingredients which you may dream of to capture stylized facts of the crisis are already part and parcel of AB models (bounded rationality, non-linearities, bankruptcies and so on) as F&R correctly point out. But these models have been so far able to re- produce these stylized facts *only qualitatively*: instead of the "well behaved"—but terribly unrealistic—impulse-response plots of the DSGE approach, ABMs can reproduce the irregularly oscillating time series of GDP, generated from the bottom up, with ample room for booms and sudden busts of economic activity. This is all fine but there is a long way to go before implementing empirically these models for forecasting purposes: ABM can reproduce the "stylized facts" both at the cross sectional and at the

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1.   Bernanke, B., Gertler, M. and S. Gilchrist (1999), "The Financial Accelerator in a Quantitative Business Cycle Framework", in J.B. Taylor and M. Woodford (eds.), *Handbook of Macroeconomics,* vol. 1, Elsevier, Amsterdam. Curiously, this seminal paper does not appear in the references of F&R.

2.   AB models have been applied in a number of fields and have been around for decades now but applications to macroeconomics are only few and most recent, as one can infer from the list of references in F&R.

aggregate level but at the present stage of development they are not implementable for forecasting purposes; on the other hand empirically implemented NK-DSGE models are indeed used for macroeconomic forecasting (but they are reliable only in "normal times"). My impression is that so far ABMs and NK-DSGE models have been built and analyzed for different purposes, as answers to different research questions. Therefore they are not really comparable (and this is indeed the impression that one gets from the paper).

Potentially, once empirically implemented with the specific needs of macroeconomic forecasting in mind, ABM will, in my view, be used to generate macroeconomic forecasts (and therefore they will be truly comparable with NK-DSGE models). Moreover, *potentially*, ABMs can do much more than NK-DSGE, *i.e.* they can be used to generate early warning signals of an incoming crisis (because ABMs can "accommodate" domino effects and therefore systemic risk, issues that cannot be dealt with in standard NK-DSGE models). I'll make a bet: it will take years, not decades. But this is only an educated guess, it is not reality yet. We have to wait (and work) before verifying the guess.

Reply to Comments

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We thank the discussant for the very insightful and stimulating comments to our paper. About the current state of agent-based models (ABMs) vis-à-vis DSGE ones, we are a little bit more optimistic than him, because of three related reasons. *First,* we believe that macro ABMs such as the K+S (Dosi e*t al.*, 2010, 2012) and CATS (Delli Gatti *et al.*, 2011) models largely beat DSGE ones on the empirical validation side. *Second,* as empirical validation is a necessary condition to perform policy analysis and we have shown in the paper that DSGE models do not meet this criterion, we believe that policy implications drawn from DSGE models are logically inconsistent and should not be used by practitioners and policy makers. *Third,* ABMs allow for much more flexibility in the design of policy experiments than DSGE models, which are typically developed by patching them with ad-hoc fixes every time they receive incoherent feedbacks from empirical results. Having said that, we think that the discussant is right about the fact that ABMs especially in macro still miss some important features before being able to replace DSGE as "the" tool for economic policy. In particular, in addition to those described in the concluding section of the paper, we single out *five* of them here.

1. Expectation formation. ABMs in macroeconomics should pay more attention to the way agents form their expectations. More specifically, a lot of work is needed to endow agents with more sophisticated expectation formation procedures which allow them to learn from their past mistakes.

2. Prediction. As the discussant correctly notices, ABMs are mostly employed from positive and normative perspectives (*i.e.* to explain or reproduce, and to under stand what kind of policy measures could lead to certain desired outcomes). What is still missing is prediction. However, prediction requires to take seriously the issue of calibration,

which is again an issue that in the ABM literature requires more discussion.

3. Estimation. In principle, ABM models parameter can be estimated with the data, possibly with Bayesian techniques, thus leading to fully calibrated models that can challenge the predictive capability of DSGE ones. Again, a lot of work is required to fill this gap.

4. Welfare. More attention must be put in designing ABMs where one can easily evaluate the outcome of any policy measure in terms of social welfare. So far, in absence of a well developed theory of consumer choices, the outcome of policies is only evaluated through aggregate measures like output growth or volatility.

5. Comparability. Different DSGE models can be easily compared in their structure and in the results they produce because they are built following standard proce- dures. On the contrary, the extreme freedom one faces in developing an ABM from the bottom-up reduces the comparability among different ABMs. The ABM community should make additional efforts to develop some standard procedures which could allow different ABMs to "speak" to each other. An interesting effort in developing a common documentation guidelines is Wolf *et al.* (2011).

Despite all this room for future works, we still believe that ABMs are already a very good alternative to standard DGSE models. They are based, instead of DSGE, on relatively more realistic assumptions, whereas DSGE are built upon building blocks that are rejected by both experimental and empirical evidence. No one believes anymore in Friedman's instrumentalist tenets: if one wants to build models that explain reality, it is imperative to start by models that use approximations to reality as their assumptions, not false ones. In our view, this suffices to decree the winner of the contest: agent-based models.

References

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