

The authors introduce a new agent-based model (ABM) with the aim of studying how the fiscal multipliers might vary conditionally on the state of the credit market and on the fiscal policy stance, offering some hints about policy implications.

The authors find that when credit markets are tight – and therefore the credit constraints bind the economic agents – a *deficit-spending* fiscal policy is effective as the fiscal multipliers tends to be larger than one; on the contrary, a *balanced-budgeted* fiscal policy stance is detrimental and unable to restore pre-shock income levels because it is associated with multipliers well below the unit.

I enjoyed reading the paper and I think it is an interesting and valuable contribution to the literature as it properly employs the greatest power of the small-scale ABMs: it clearly explains the transmission mechanism that allow a bankruptcy shock on the consumers side to affect the aggregate macroeconomic dynamics. This clarity in the exposition of the main propagation channels is in my opinion the first value added of the paper and shall help also non-ABM users to grasp that the results (i.e. the emergent properties regarding multipliers) are not a strange object without a clear economic explanation and that ABMs are not black-boxes. Furthermore, I believe that the second value added of the paper is the fact that the economic mechanisms at work in the model, are also well connected with the recent empirical literature. This confirms that the narrative of the model is also corroborated by econometric results and it is therefore relevant for better understanding some features that are observable in the real world. As such, I believe that the model is a useful exercise.

In what follows I enter into the details of my main concerns concerning the paper, which are either suggestion for further research or comments on some of the model equations or results.

1. The authors claim that “during booms households engage into standard permanent-income consumption smoothing” and “during economic downturns consumption is more sensitive to current income variations”. I wonder whether there is some verification of this consumer behaviour in the empirical literature and how these statements relate to the concepts of excess sensitivity and excess smoothness. In particular, during expansion periods income shall be more predictable (as there is low uncertainty) and you should observe some form of excess sensitivity in consumption levels. On the contrary, during recession periods uncertainty is larger and you shall however observe excess smoothness. I think that these are interesting implications that you can verify using your model. Further research might be dedicated to that by better specifying the consumption rules and taking into account the role of precautionary savings in order to match the empirical evidence of excess sensitivity and smoothness.
2. In the steady state section of the paper the authors claim that the model has multiple equilibria, however they do not investigate on their stability (either in a local or in a global sense). I think that a possible extension that shall be done to this model concerns the exploration of the parameter space, in order to verify for which values the initial steady state is stable (at least locally) and when instead it is not. This is another direction that the future research might take and it will allow to better connect the results on the size of the multipliers with the different statistical equilibrium that the model reaches, which in turn might represent different states (e.g. full-employment vs. recession) of the economy.

3. I think that to better link the model with the real-world implemented policies the authors shall include an additional scenario, one that mimic the stability growth pact (SGP) of the European Union. This would imply that the adoption of a deficit spending fiscal policy, but bounded within the 3% of GDP. It would be an interesting exercise to verify whether this policy is sufficient to get the economy back to the pre-shock equilibrium; if it is, it would be interesting to observe how long will it take with respect to an unconstrained fiscal policy rule.
4. In the results section, concerning the scenario with deficit spending policy, it would be interesting to include a graph with the dynamic of the public debt to GDP, to better understand (i) how much it shall increase in the short-run in order to sustain the economy and (ii) how many periods will it take before it gets back at its steady state level.
5. In figure 2, it is possible to observe that the peak multiplier can emerge at relatively late stage of the simulation, even after many periods after the shock hit the economy. In particular it goes from a minimum of 20 periods after the shock (for the case of 0.2 fiscal intensity), to a maximum of 60 lags (in the 0.02 intensity scenario). All in all, there is an apparent relation between the fiscal intensity and the lags after which the peak multiplier is reached: it might be worth to use the simulated data to estimate this relation and to verify whether greater fiscal intensity also implies a lower number of lags before the fiscal policy fully exploits its potential.

Coming to very minor, graphical, concerns instead, I think that:

1. In Figure 1 and Figure 2, the author shall pay attention in using the same colours for the same levels of fiscal intensity. At a first comparison, a reader might mix the results from two different experiments.
2. In figure 6 and 7, the confidence bands shall be dashed (as they already are) but also coloured. Otherwise it is impossible to distinguish the lower bound confidence interval for one scenario with the upper bound of the confidence interval for another scenario. This in turn is important because it determines whether two different scenarios are significantly different in a statistical sense.