

We are grateful for the very painstaking efforts made by the editor and the three referees in providing us with very valuable suggestions and comments. These have enabled us to think of some fundamental issues which can help us extend the current research into the future. We believe that we have taken all the suggestions and comments into account in this revised version. To enable the editor and the referees to quickly see what we have done, we summarize how we have responded to each comment or suggestion below. We replicate the editor's and referees' points using ordinary font, and show our responses in *italics*.

1 Response to Referee Two

This paper proposes a DSGE model with heterogeneous expectations. The main idea of the paper is interesting but the paper is written in a rather imprecise way. There seem to be several major inconsistencies and inaccuracies.

1. The structure of the paper is unclear. The research question is not presented in the introduction but in section 1.2, page 4: 'We shall simulate the macroeconomy using the agent-based DSGE model augmented with the Ising model, which is embedded with different network topologies. We then examine the effect of these different network topologies on the observed macroeconomic stability in terms of the output and inflation dynamics'. This part should go in the introduction.

Done. The main research question is posed at the very beginning of the first section.

2. On page 4, second paragraph, the authors refer to 'big names'. What exactly are 'big names'? In the same paragraph the authors claim that some network topologies are more stabilizing or destabilizing. It is not entirely clear what the authors mean by stabilizing/destabilizing. A specific definition would be advisable.

The original presentation is somewhat imprecise, and we have rewritten this part. The term "big names" has been removed and the stabilizing or destabilizing role will become clear in Section 4, and hence those terms are avoided here for the moment.

3. In section 2, at the end of page 6, the authors write that 'in this model, agents have different expectations'. This is somewhat confusing. In standard New Keynesian DSGE models, equations (1), (2) and (3) are derived under the assumption of homogeneity. Once the model has been derived, the authors introduce heterogeneous expectations by assuming that agents are either optimistic/pessimistic with regard to their inflation expectations. But under heterogeneous expectations equations (1), (2) and (3) would not hold in the first place. If the authors wish to work under the assumption of heterogeneity, they should start from the basic utility maximization problem assuming that expectations are heterogeneous and then derive equations (1), (2) and (3). It is far from clear that the equations they would arrive at would be identical to the equations (1), (2) and (3) that the authors write in their paper. This would be equivalent to derive the $e = mc^2$ formula under the assumption that the speed of light is constant, and then, once the formula has been derived, to assume that the speed of light is not constant any more.

We have addressed this issue with a new subsection "Remarks on Theoretical Foundations" (Section 2.2.4). The reviewer has correctly pointed out that we are actually building our agent-based model partially upon an equation-based model. We, however, have to say that this is probably the current practice in many agent-based macroeconomic models. In fact, building agent-based macroeconomic models by simultaneously taking into account heterogeneity and optimization may not even be computationally tractable. A search of up to 15 recently published agent-based macroeconomic models shows that none of them has actually adopted this research strategy. In our response, we, therefore, provide an alternative way of looking at the three equations. We basically assume that these equations are the possible approximation to the emergent properties of an agent-based macroeconomic model, of which the details will not bother us. Our starting point is then simply to work directly on the emergent functional relation between the meso-structure and the aggregate variables. This research strategy allows us to move ahead at a cost of some imperfections, which we also mentioned in Section 2.2.4.

4. In section 3.1.1, the authors give a rather lengthy presentation about the different network models (fully-connected network, circle and regular network, small world and random network and scale-free network).

These are standard network models and therefore it would suffice to just name them or describe them just briefly. This is also true for the different measures used by the authors to characterize the networks (average degree, average clustering coefficient, average path length and centrality indices).

Since the use of the social network in macroeconomics is just the beginning, considering that a large number of readers may have limited exposure to it, to make this paper self-contained, we have therefore kept this introductory part, as the editors seem to agree upon it. We, however, do make some modifications to make the presentation more efficient.

5. The description of the equations is sometimes not precise. On page 12, the authors present the equation (15) and (16). In the paragraph immediately after the equations the authors write that according to these equations two variables affect the behavior of each agent: $(m_{1,i}, m_{2,i})$ and λ . As far as I can judge, λ is not a variable but a parameter. In other places, relevant information is missing. In equation (17), for instance, the authors define the variable $w_{i,j}$ as being a function of j where $(j \in \vartheta_i)$. No formal definition of the set ϑ_i is given. It would be highly advisable to write the model in more precise way.

These two equations, now numbered (9) and (12), are now carefully written. All the symbols or variables, including ϑ_i , $m_{i,y}(t)$ (originally, $m_{1,i}$), $m_{i,\pi}(t)$ (originally, $m_{2,i}$), and λ , are carefully defined and even discussed.

6. In table 3 the variances of the output gap are presented. The authors write that the variance of the output gap is the smallest under the the fully connected network. The numbers presented in the table are extremely similar. They are so close to one another that it is hard to believe that the differences have any economic relevance. Furthermore, are these variances statistically different from one another? Something similar happens with the variances presented in table 4. The authors never comment on the relevance of the parameter λ . How does this parameter affect the results? If the macroeconomic model has three state variables, why do the authors analyze here only to the output gap and the inflation?

The statistical test is now added to both Tables 3 and 4. The referee is correct that some of our original arguments do not stand well in

terms of its statistical significance so that we have removed those claims. In fact, the entire discussion of Tables 3 and 4 has been rewritten. The effect of λ (the intensity of choice) is also included. Generally speaking, the effect of λ is rather uncertain; it differs among networks and also differs from one volatility to the other. Very much because of this uncertainty, in our second-stage simulation we have fixed this value to the middle one ($\lambda = 0.5$) to avoid the possibility of obtaining some extreme results, which have been observed in Table 3 and Table 4. Finally, since the behavior of the central bank is not part of our social network modeling, the volatility due to the Taylor rule is not brought into the analysis. It certainly would be an interesting subject if in the future the interbank network were to be integrated into the agent-based macroeconomic models.

7. In section 4.2.2, the authors regress the (log) variance of the output gap and inflation on a series of variables relating to the network structure. It is not clear why the authors assume that the relationship between the dependent and independent variables is linear. The variables on the right hand side of equation (20) and (21) affect the variances nonlinearly in my view, as they affect the macroeconomy through the Ising model, which determines the probabilities of being an optimist/pessimist in a nonlinear fashion.

The revised version has been substantially modified to include a subsection (Section 4.3.3) to deal with the possible presence of the non-linear effect. Indeed, as the referee correctly pointed out, we do find evidence for the nonlinear effect. We also perform a robustness check of our first-order (linear) effect with the presence of the nonlinearity.

8. It would be nice if the authors would include a footnote under each table providing an explanation about the information displayed in the tables.

A footnote for each table has been added, except for the last two in the Appendix, i.e., Tables 11 and 12. These two tables already cover the whole page; the information required for reading the two tables is, therefore, given in the main text. In addition, Tables 1 and 7 are self-explanatory; hence no footnote is added.

References

- Allen F, Babus A (2009) Networks in finance. In: Kleindorfer P, Wind Y, Gunther R (eds.) *The Network Challenge: Strategy, Profit, and Risk in an Interlinked World*. Upper Saddle River, NJ: Wharton School Publishing, pp. 367-382.
- Delli Gatti D, Desiderio S, Gaffeo E, Cirillo P, Gallegati (2011) *Macroeconomics from the Bottom-up*. Springer.
- Kinsella S, Greiff M, Nell E (2011) Income distribution in a stock-flow consistent model with education and technological change. *Eastern Economic Journal* 37:134-149.
- Wolfram S (2002) *A New Kind of Science*, Wolfram Media.