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 One

The Authors study an agent-based New Keynesian DSGE model with different social network structures to investigate the significance of network topologies to macroeconomic stability. I have a list of major comments and also some minor comments that I suggest the Authors to take into account in their revision.

1.
   - The Authors should emphasize a little bit more the importance of the representative-agent assumption in standard DSGE models and the relevance of heterogeneity in agent-based approaches. It is not entirely true that, as the Authors state at p. 6, "the difference between the stylized New Keynesian DSGE model and the agent-based DSGE models is the difference between the expectations of the output gap and inflation." Heterogeneity is another key factor that differentiate the two approaches.
   - Another one that the Authors overlook is out of equilibrium dynamics in ABM, whereas in standard DSGE everything happens in equilibrium. I would suggest the Authors to consider these points in their revision.
   - The assumption of a non-directed network is crucial, but is not fully justified. Why expectation formation and interactions in the real-world should be always reciprocated?

   - *The difference in heterogeneity has been emphasized (Section 2.2).*

- *Yes, the agent-based model, as well demonstrated by Stephen Wolfram (Wolfram, 2002), has four types of dynamic properties; however, even types III and IV are equally shared by general equation-based dynamics systems. Therefore, we cannot claim that agent-based models can demonstrate more than what the equation-based models can do in these dynamic properties at the macro level. Based on our understanding, their major difference lies in the fine details of the micro-dynamics as well as the micro-causality, which is completely absent in the DSGE models, be they linear or non-linear. Having said that, we admit that the DSGE model to which we refer (a system of linear stochastic difference equations) cannot lead to the Type-III and Type-IV phenomena, as correctly pointed out by the referee. It is, therefore, indeed possible that the emergent aggregate properties may constantly change with the evolution of the economy, and hence there is no time-invariant functional relation of macroeconomic variables. In this case, the effective time horizon which can legitimize our approximation cannot be infinitely long. Again, as we mentioned in the introductory section, what we pursue here is confined to a thought experiment under a controlled laboratory setting. At this stage, it is still too early to jump into an empirical study without knowing how these time-variant properties are handled first.*
- *The non-directed network is justified (Section 3.1, footnote 7).*

2. I am not totally convinced that the distinctions that the Authors make between small-world (SW) networks and scale-free (SF) networks is entirely sound. A SW network exhibits low average path length and high clustering, together with a bell-shaped degree distribution. A SF network displays a power-law degree distribution, but it can also have low average path length and high clustering, as it happens for example with the preferential attachment model. Therefore, the two classes are actually overlapping.

   Yes, we agree. It seems to be more informative to use the network characteristics, instead of the network generation mechanisms, to examine the effects of network topologies on economic stability. That is exactly what we did in Section 4.3. The reason why we start using the network generation mechanism as the first-stage (preliminary) design is because

earlier studies in agent-based economic models mainly focus on the network generation mechanisms rather than network characteristics. We believe that making the distinction between the two is important; therefore, both network generation mechanisms and network characteristics are presented in the analysis, while our major efforts are demonstrated in the latter. We now also clarify this point in the abstract and in Section 1.2. The referee also makes a good point regarding the shape of the distribution as another important characteristic; we have added Section 4.3.4 to address this issue.

3. Eq. 17: Why is the interaction strength inversely related to the number of neighbors? Please justify with an economic argument. Some more economic justifications for the chosen network parameters should be given.

   As we mentioned in the paper, the network topologies used in this article are mainly for us to conduct thought experiments. While these networks may potentially correspond to some real economic or social networks, there is no attempt to provide them with any empirical grounds or calibration, which require a framework very much different from the current setting. Hence, serious treatments of these networks in the parlance of economics or endowing the associated parameters with economic meanings can be superficial. Under this situation, to avoid making the model unnecessarily complex, the network topologies employed here are undirected and discrete (equal-weighted), serving as a starting point for this line of research. For pure thought experiments, it is certainly interesting to explore the directed and weighted network topologies; we leave this is to a further study and mention it in our concluding section.

4. Results in tables 3-6 are puzzling. Whereas there are differences in variances across different network topologies and parameters, these differences often occur at the 3rd digit. No indication on whether these differences are statistically significant are given. The Authors should provide a more in depth statistical analysis of these figures.

   The materials around the original Tables 3-6 have been substantially revised. Statistical tests have now been added to Tables 3 and 4 to make the numerical results more informative, while an extensive and rigorous quantitative analysis has been further performed in the larger-scale simulation in the later stage. The more intuitive but less established

discussions based on Tables 5 and 6 have been removed, including the removal of the two tables themselves.

5. Hypothesis 2 should be rephrased. A higher clustering may simply be due to a higher connectivity. Therefore, the net effect of a higher clustering is not "per se" clear. One should analyze what happens when clustering increases conditional to the same level of degree. Therefore results should be compared with null network models that take as given e.g. the level of local connectivity.

   Hypothesis 2 has been removed and, based on our more comprehensive econometric analysis, both the connectivity and clustering coefficients are not significant in the GDP equation. While they are still significant in the inflation equation, they are not robust to the non-linear variations of the model.

6. The discussion at page 19 below table 7 should be generalized by doing an experiment that checks whether the conclusions are true when one changes the rewiring probability, etc. Why the Authors have chosen the parameters in Table 7? Are regression results robust to changes in these parameters?

   The discussion as pointed out by the referee is actually on the effect of the scale-free network, and not the small-world network; therefore, the parameter rewiring rate is not involved. In addition, in the original version, we did not attach the statistical testing results. Now, they are attached in Tables 3 and 4. As demonstrated, the idiosyncratic effect of the scale-free network is not particularly sensitive to λ. As to the involvement of more samples from different rewiring rates, that is exactly what we did in the second-stage simulation (see Section 4.3). Table 7 (now Table 6 in the revised version) is not what we intend to choose as the controlled parameters, but the summary statistics of the networks which we randomly generated. We understand that we did not make this fully clear in the original version; hence, in this revised version we provide a more thorough description of the simulation design, including the network generation mechanism used here (Section 4.3.1 and Table 5).

7. Regression analyses are performed by drawing at random a network with different structural properties, computing its topological prop-

erties and see if they affect output gap and inflation in the DSGE model. I have two concerns with this strategy: (i) the analysis should be also performed for each given network class separately, to understand if apart from its topological properties, the structural features of the graphs, such as the shape of the degree distributions, affect the results; (2) we already know from the model the economic covariates that influence the two dependent variables, so why not controlling for them in the regressions? In fact the results obtained in table 8 may suffer from omitted variable biases.

As for the first point, we agree with the referee that the shape of the degree distribution can be another important characteristic to be included in the regression analysis. However, at this point, the literature has made no suggestion as to what kind of measure we should adopt. What we have done in this revised version is to invent two possible measures as detailed in Section 4.3.4, and we re-run our SURE regression again. We do find the statistical significance of the effect of both shape characteristics on the GDP volatility, but not on the inflation volatility. However, despite this significance, the original result built upon the five-variable SURE remains unchanged. Hence, our fundamental finding is robust. This has been detailed in Section 4.3.4. Regarding the second point, in this revised version, we follow the referee's suggestion to take into account the correlation structure of the error terms of the two equations and apply SURE to estimate the system of the two equations (see Section 4.3.2).

8. The Authors should discuss their results about the effect of connectivity on volatility in terms of the recent literature on interbank networks and their robust yet fragile properties, see for example Gai, P. and Kapadia, S. (2010), Contagion in financial networks, Proceedings of the Royal Society A: Mathematical, Physical and Engineering Science, Vol. 466, The Royal Society, pp. 2401-2423.

   As we have mentioned in Section 1.1, the social network which interests us in our analysis is the interpersonal network and its effects on information spread and expectation revisions. This kind of network is genuinely not the same as the interbank network which has the balance sheet as the basis for the network relation. Furthermore, as also mentioned in Section 1.1, the literature of the interbank network has

not been fully integrated into any agent-based macroeconomic models known to us. Most agent-based macroeconomic models either have no bank or just a single bank. The only exception with a large number of banks is Kinsella, Greiff and Nell (2011), which has 75, and the next one is Delli Gatti et al. (2011), which has only 10. Hence, the size has not come to a scale compatible with asymptotic analysis of the interbank literature as the one shown in Gai and Kapadia (2010) or the ones reviewed by Allen and Babus (2009). Nevertheless, since the non-linear effect of the connectivity has also been observed in Gai and Kapadia (2010), we still cite their paper during our discussion of the non-linear effect of network topologies (see Section 4.3.3).

9. There is a lot of work in agent-based macroeconomics on Keynesian macro models (without an explicit network microfoundation) that is not cited in the paper. For example:

   - Dosi, G., Fagiolo, G., Napoletano, M. and Roventini, A. (2012), Income Distribution, Credit and Fiscal Policies in an Agent-Based Keynesian Model, Journal of Economic Dynamics and Control.
   - Dosi, G., Fagiolo, G. and Roventini, A. (2010), Schumpeter Meeting Keynes: A Policy-Friendly Model of Endogenous Growth and Business Cycles, Journal of Economics Dynamics and Control, 34: 1748-1767.

   I urge the Authors to consider the following works and cite them consistently in the paper.

   Yes, the two contributions from Dosi et al. are cited in an appropriate place in the revised version. Indeed, there are many agent-based macroeconomic models. To the best of our knowledge, there are more than 25 now, including the above two provided by the referee. We do cite eight of them in this revised version in a place when they are related to the context under discussion.

10. Typos: page 4, "lagged" instead of "logged".

    The typo has been corrected.

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.