Referee report on *Economics* manuscript 2130

"Information heterogeneity, housing dynamics and the business cycle"

This paper introduces information heterogeneity of agents distributed across a continuum of island in a dynamic stochastic general equilibrium (DSGE) model with a borrowing constraint à la Kiyotaki and Moore (1997) in order to better explain two empirical observations: First, house prices are volatile and closely correlated with the business cycle, whereas rental prices are relatively stable and almost uncorrelated with the business cycle. Second, residential investment leads the business cycle while nonresidential investment moves contemporaneously.

While I think that the theoretical part of the paper makes a valuable contribution to the literature, it seems to improve upon standard models without information heterogeneity in terms of matching moments only in some dimensions. Moreover, I have several concerns regarding the empirical analysis in the paper, which I will describe in detail in the following comments.

Comments on the empirical parts:

1. On page 1 (last line) and on page 8 (last paragraph), the authors write that "all data are log-linearized and filtered using the Hodrick-Prescott filter". It is well-known that the HP filter introduces spurious dynamics into the data (see, e.g., James D. Hamilton, "Why you should never use the Hodrick-Prescott filter": https://www.nber.org/papers/w23429). While this is less problematic when matching moments and treating empirical data and model-simulated data equally, it is deficient when transforming the data prior to a regression such as in Section 2. Moreover, it is not clear whether the data in Section 5 was HP-filtered before estimating the structural VAR model. If the answer is yes, than none of the impulse response functions are reliable.
2. On pages 9ff., the authors use tests of Granger non-causality to "investigate the causality effect between residential and nonresidential investment". "Causality" refers to a link between cause and effect, whereas Granger non-causality refers to predictive power of one variable for another variable in a bivariate VAR. Since these are entirely different things, I suggest changing your wording (also in the title of Table 3) or discarding the Granger non-causality analysis altogether.
3. If the previous analysis is not discarded, the authors should clarify the specification of equation (3) for each country in the sample. Table 3 only indicates the lag length, while the reader is not informed about the presence of (co-)integration in the variables.
4. The discussion of "empirical evidence from survey data" in Section 5 is not well placed in the structure of the paper. Currently, you go from the empirical motivation to the theoretical model and back to an empirical paper. Try bringing the paper into a more sensible order.
5. On page 26, the authors write "A prediction of our model is that […] expectation errors should be correlated with the business cycle." It is not obvious that this follows from your model. Given your simulated sample size of ~140 observations, a correlation coefficient of 0.052 is not statistically significant at any conventional level. In any case, you should discuss this prediction (and the mechanisms underlying it) in the theoretical model part.
6. On page 26, the authors write that they "run a three-variable VAR with expectation errors of output, output, and house prices" without discussing their implicit or explicit identifying strategy. The impulse response functions in Figure 6 suggest that the authors use a recursive identification scheme (i.e. a Cholesky decomposition), where forecast errors are ordered first

and an orthogonal shock to forecast errors thus influences the other variables on impact. However, this kind of identification has no structural interpretation. Note that an exogenous change in output unrelated to economic agents' expectations, e.g. due to an external demand shock in a small open economy, affects both the level of output and the forecast error on impact. Yet, the direction of causality is from output to the forecast error rather than the other way around, as imposed by the authors. From a careful analysis of forecast data, it is obvious that the vast majority of the variance in forecast errors is due to unexpected fluctuations in fundamentals rather than unexpected changes in expectations. For these reasons, I suggest coming up with a more suitable identifying strategy or dropping Section 5 from the paper.

Comments on other parts:

1. On pages 11ff. index I denotes an island. Nevertheless, the authors also introduce "nonresidential firm *i*", "residential firm *i*", and "household *i*" on page 12. I suggest using the terms "nonresidential firm on island *i*", etc. instead to avoid confusion.
2. House prices are the same across all island and do not carry an island-specific index (see, e.g., p. 13). How does this square with the evidence of spatially separated housing markets in the U.S.?
3. Given that "the representative entrepreneur has full information", there is an information asymmetry between the economic agents interacting in the model that does not seem to be discussed anywhere in the paper. What are its implications? Would your results be different for a symmetric information structure? For example, entrepreneurs could use their information advantage over households to reap arbitrage gains by purchasing and selling houses across households.
4. On page 17, the authors write that "agents can only extract information about the true economic fundamentals from their idiosyncratic market activities." Describe what you mean by "true economic fundamentals". More generally, the agents' signal extraction problem seems to be outlined incompletely.
5. In calibrating the DSGE model (page 18), the authors set the entrepreneur's relative risk aversion equal to 2, while the household on island *i* has log-utility. Hence, the representative entrepreneur is assumed to be less patient but more risk-averse than private households. This assumption clearly requires some motivation.
6. In calibrating the DSGE model (page 19), the authors "choose the persistence and the variance of idiosyncratic shocks to maximize the effect of information heterogeneity on house prices". I am not sure whether this is the right target to choose when calibrating any model and claiming that the aim is to add realism. The appropriate question is whether the model with information heterogeneity can add to our understanding of house and rent price dynamics subject to a calibration in line with "empirical micro-level cross-sectional facts."
7. In Table 4, the model with "Hetero info" performs better than the "Full info" model along some dimensions but worse among others. Discuss in more detail.
8. In Table 5, the estimate of alpha_2 in simulated data from both the full info and the hetero info are very far of the theoretical value of 1. The fact that equality can be rejected in one case but not in the other seems to be a statistical fluke. Take a different simulation and you might obtain the opposite result. Also, it is bad practice to report 1 and 10% significance levels in Table 5 and discuss significance at the 5% level in the text (page 23).

9. I have not come across the terminology “agents are rationally confused about the sources of shocks” (pages 3 and 28) in the literature. From my point of view, the agents in your model form rational expectations under imperfect information about the state of the economy. As a consequence, they must solve a signal extraction problem in order to forecast future values of the underlying fundamentals.
10. Finally, it is not clear to me why the authors argue that “the model generates an amplified response of house prices to technology shocks”. In conventional models of signal extraction, imperfect information implies that economic agents hedge against the risk of misinterpreting the observable noisy signals of the state of the economy by underreacting rather than overreacting to shocks. Hence, the authors should clarify the mechanism behind this amplification result and whether it is driven by information heterogeneity rather than by the so-called “financial accelerator”.