Response to Referee 1

Thank you for commenting on our paper and for your suggestions. The following remarks reply to your comments and suggestions in the order in which they appear in your report (quotations from your report are shown in italic).

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.*

   For the comments on using Hodrick-Prescott filter on page 1, page 8 and Section 2, there are two points to mention. First, as a robustness check I have also used first-difference method to remove the trends and received similar results. Second, although there are some well-known drawbacks for the Hodrick-Prescott filter, it is still too soon to conclude that "*you should never use the hodrick-Prescott filter*"[1]. Thus, usage of Hodrick-Prescott filter does not, or at most slightly, hurt the main conclusions of the paper.

   The data in Section 5 was not HP-filtered before estimating the structural VAR model.

2. *On pages 9, 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.*

[1] Drehmann, Mathias and James Yetman (2018), "Why you should use Hodrick-Prescott filter – at least to generate credit gaps," BIS Working Papers, No. 744. (https://www.bis.org/publ/work744.pdf)

On page 9, the paper only says "… implies that residential investment does not Granger cause nonresidential investment," and this description is consistent with standard econometric language[2].

In the new version, I will add the word "Granger" in front of casualty test as "investigate the Granger causality effect between residential and nonresidential investment" to emphasize it only implies forecasting ability instead of true causality.

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.*

   In the new version, I will add cointegration test results into Table 3.

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.***

   Although there are some advantages to incorporate Section 5 into Section 2, adding Section 5 will help readers have better understanding of the theoretical results in Section 4. The order of the paper is similar as in the paper by Lorenzoni (2008)[3].

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.*

   In the new version, I will add some words about the prediction about positive correlation between expectation errors and the business cycle and the underlying mechanisms.

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*

[2] Please see Page 26 of the lecture notes by Dr. Eric Zivot at University of Washington (https://faculty.washington.edu/ezivot/econ584/notes/multivariatetimeseries.pdf).
[3] Guido Lorenzoni (2008), "A theory of demand shocks." American Economic Review, 99(5), pp. 2050-80.

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.

The paper is on a closed economy, rather than a small open economy. The identification scheme assumes forecast errors impact housing prices and then the output, not the other way around. In the new version, I will add more words to explain this assumption and the identification scheme. Again, Section 5 is only to help readers have a better understanding of the theoretical results in Section 4.

Comments on other parts:

1. *On pages 11, 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.*

   In the new version, I will use the terms "nonresidential firm on island i", etc.

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.?*

   There are three main reasons why I did not use an island-specific housing index: first, in the paper the housing price serves to affect the collateral values and further the output, and also as an information signal to the economy. The paper is to analyze the interaction of housing market and the whole economy, and not micro-level housing market characteristics. Second, there are nation-wide housing index available, which aggregate island-specific indexes. And third, the island-specific index is public information as in realty people can easily access to it and if we use island-specific index, we have to add more idiosyncratic shocks since it is reasonable to believe that one cannot extract full local information simply by reading island-specific housing index. In other words, the

benefit to add island-specific index to the model is marginal, rather than complicating the analysis.

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.*

   First, it is reasonable to assume the entrepreneur has more information than other economic agents. Second, the role of the representative entrepreneur in the model is more to clear the market, rather than other. Third, we use this type of information structure mainly for tractability purpose, which is similar as in Angeletos and La'O (2009)[4].

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.*

   The true economic fundamentals mean the real shocks to the economy. In the model, it means average technology shocks, average housing preference shocks, average bond-holding shocks.

   The information structure is outlined in Section 3.5. There are two types of information the agents should learn about the economy: first, the parameters and the model structure are common knowledge; second, the unknown shocks mentioned in the above paragraph. The representative entrepreneur has full information, and the island-specific agents have island-specific information except housing prices, which are observed by all agents. The agents' signal extraction problem is outlined in a similar way as in Graham and Wright (2010)[5].

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*

[4] Angeletos, George-Marios and Jennifer La'O (2009), "Noisy business cycles," NBER Macroeconomics Annual 2009, vol. 24, pp. 319-78.

[5] Graham, Lian and Stephen Wright (2010), "Information, heterogeneity and market incompleteness," Journal of Monetary Economics, vol. 57, pp. 164-174.

representative entrepreneur is assumed to be less patient but more risk-averse than private households. This assumption clearly requires some motivation.

There are three reasons to set the entrepreneur's relative risk aversion to be 2. First, we follow the setting in the literature, e.g. in Baxter and Crucini (1995)[6]. Second, the results are not sensitive to the choice of this parameter. One could set the entrepreneur's relative risk aversion to be 1 and get similar results. Third, it is not uncommon to see that firms have risk-control departments and at the same time actively invest.

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."*

   Again, the target of the paper is to analyze the interactions of financial frictions and information frictions and further the business cycle. The choices of the parameter values are for theoretical exploration. Graham and Wright (2010) have chosen different parameter values to explore results sensitivity to properties of idiosyncratic shock, which is in spirit in line with our approach.

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.*

   In the new version, we will explain the results in Table 4 in more details.

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).*

   The results are robust by taking average of 1,000 different simulation data. We will update the text and discuss significance at the 10% level.

[6] Baxter, Marianne and Mario J. Crucini (1995), "Business cycles and the asset structure of foreign trade," International Economic Review, vol. 36(4), pp. 821-854.

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.*

   You are right. The agents in my model solve a signal extraction problem in order to forecast values of the underlying fundamentals and form rational expectations under imperfect information about the state of the economy. Because there are two aggregate shocks (technology shock and housing-preference shock) and one public available signal (housing price), the agents cannot fully inform the underlying fundamentals, which means "agents are rationally confused about the sources of shocks".

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".*

    As answered in pervious question, agents cannot fully inform the underlying fundamentals and thus have imperfect information about the economy. In conventional models of signal extraction, imperfect information implies under-reacting to shocks if there is no interaction between agents. However, this is not true when there are interactions among agents. For instance, Bachetta and van Wincoop (2006) show how heterogeneous information could generate amplified exchange rate volatility[7].

    In Table 4, the full information case implies the amplification results are not purely from the "financial accelerator."

[7] Bacchetta, Philippe and Eric van Wincoop (2006), "Can information heterogeneity explain the exchange rate determination puzzle?" American Economic Review, vol. 96, pp. 552-576.