

General Comments

The paper aims to examine the factors that drive the effect of housing wealth on consumption. For this purpose the author provides a simple partial equilibrium OLG model as well as empirical evidence from a VAR. In particular the paper shows that the correlation between housing wealth and consumption is driven by demographics and the structure of financial markets.

The theme of the paper is very topical and the paper provides interesting empirical and theoretical insights to this field in the literature. The paper is well-written. Nonetheless the paper would greatly benefit from a sharper focus and from first providing empirical evidence which is then explained by a theoretical model. Please find below a few shortcomings that could be identified.

Detailed Comments:

1. The paper shows both empirically and theoretically that the sign of the co-movement between housing wealth and consumption changes. In my view the paper would greatly benefit from changing the presentation of the results. First, the author should show the empirical evidence that the co-movement of housing wealth and consumption changes over time. Given these findings the author should present his theoretical model that should be consistent with the identifying assumptions in the VAR and with the conditional correlations predicted by the model.
2. The author could also investigate the unconditional correlation of house prices/housing wealth and consumption. In a second step the author could then depict the time-varying correlation (say of a ten-year window for example) and show that over time this correlation changes from negative to positive.
3. The clarity in the Introduction should be improved. After a brief motivation of the paper the author should turn directly to present the main idea of the paper including both the model and empirical results. Then the paper can relate to the nascent literature that is currently quoted in the beginning of the introduction. Currently, I think the main contribution of the paper is expressed only in the last part of the introduction.
4. I think the empirical part of the paper should focus more on the VAR results and discuss the identification assumptions in more detail. Here the author could also try to link the empirical and theoretical contribution of the paper in showing that for example the identification assumptions are consistent with the implications of the theoretical model. All the remaining checks (unit root, Johansen etc.) and possibly parts of the lengthy data description should be moved to an appendix.
5. It seems that both VAR models are over-identified (see equation 28 and equation 29) due to the imposed zero-restriction in the lower left corner of the matrix. For example in equation 28: both the shock on financial wealth (fw) and net housing wealth (nhw) are equal to the reduced-form residuals. Why is this a plausible assumption? In equation 29 the assumption is even more extreme: The reduced form residual for house prices and the housing stock are interpreted as structural shocks. It would be helpful to convince the reader that the identified shocks are reasonable. For example, the author could also use the identifying assumption employed in the related literature that empirically identified house price shocks (e.g. Iacoviello (2005, AER)) or at

least to show that the shocks that previous papers have identified are highly correlated with those found in this study.

6. The author reports econometric tests of a break point in the data. However these tests seem to suggest a break point in different years (ranging from the 1970s to the early 1990s depending on the time series, the test statistic and the empirical VAR model) and the author eventually chooses (somewhat) arbitrarily to split the sample in the fourth quarter of 1984. I think the paper would benefit from a stronger motivation and economic intuition why this is a reasonable choice. In particular did demographics or the structure of financial markets have changed in this particular year? In addition, these two factors (demographics and financial markets) seem to have changed more slowly rather than ad-hoc. Therefore I think the explanation in the model is hard to reconcile with assuming a break-point in the data. If there exists specific narrative evidence of why there has been a substantial shift in 1984Q4 or the empirical results are robust to choosing alternative break-points then the overall results would be more compelling.
7. The model and the estimated VAR are both related to the contribution of Iacoviello (2005). The paper would benefit from discussing in more detail commonalities and differences. For example, the VAR and the identifying assumption seem to be similar to those in Iacoviello. If so, would that imply that when splitting the sample in Iacoviello around the years 1984/85 that the response of consumption changes its sign? This might be an interesting point to emphasize in the paper.
8. The paper derives analytical expressions for the model equilibrium. It would be useful to calibrate the model (either using conventional parameter values or to target certain moments in the data) to show how plausible a change in the correlation of housing wealth and consumption actually is. A calibrated model would also help to quantify the size of the different effects and channels emphasized in the paper.
9. I think the author could present some more sensitivity analysis with regard to the model. For example, what happens when the population size is matched to actual data rather than assuming equally large groups (which I think is fine for the benchmark economy). The benchmark economy assumes that the discount factor is the same for each generation. The discount factor strongly affects the marginal propensity to consume (see also equations 15-17 in the paper). However, it might be plausible that, for example, the old generation is more impatient. How sensitive are the results to using different discount factors for each generation.
10. The analysis would benefit from a general equilibrium analysis in which also the housing supply side is not fixed. The author could try to provide a brief intuition how the results might change when the supply side is not fixed.
11. The paper shows a large number of empirical impulse response functions for different shocks and variables. I think the paper would benefit from only showing the relevant impulse response functions. The remaining set of results could be moved to an appendix.