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1 Assessment

The three chapters of the dissertation are original and interesting. Martin demonstrates great creativity, excellent modeling ability, and strong execution skills. My overall assessment is that this is a strong dissertation that very comfortably meets the standards for awarding a PhD degree. I also think that the first two chapters have a high potential for publication in good to very good journals. Nevertheless, these papers need, in my view, to be improved before being submitted to such journals. My comments below point toward what I think are the main areas for improvement. I gave a particular focus to the first chapter because I think it has the highest potential.

2 Specific comments

2.1 Chapter 1

The first chapter is very interesting and has a very good potential. The model is rich and well balanced (that is, given the ambitious agenda, some simplification assumptions were needed and I think they were chosen with flair). The results are intuitive and well explained. Still, Martin may find the following comments useful to improve the paper.

2.1.1 Execution

The analysis seems well executed. However, some important elements are missing to make the paper self-contained. (Since the results are intuitive, I have little doubts on them being correct.) Also, some easy adjustments would make the paper easier to read.

- There is no definition of an equilibrium until we study the recursive problem. A deterministic steady state is mentioned but is not defined.

- In the case with adverse selection, the description of interactions borrows from game theory. This is of course fine, but it would be then useful to formalize the game. More generally, it would be good to make very clear what is observable and what is not (are sellers able to split their sales without the buyers noticing, etc). Again, I do not think that there are “problems”, but it would clarify the analysis for the reader.

- It would be useful to describe the functioning of asset markets. Competition among sellers is mentioned at some point, and it is stated that the buyers FOC pin down the prices, but what does guarantee that these “buyers” do indeed always buy at equilibrium? Are we talking about the same kind of steady state, or all along?
• Some variables are not named, which do not facilitate the reading.
• This chapter is very (too) long. For instance, the main result is only presented on p32.
• I think it is a good idea to have most of the math in the appendix, but more intuition about agents behavior would be useful.

2.1.2 Economics

In line with the last point above, I have a series of comments on the substance:

• My guess is that firms with good investing opportunities always sell first any kind of legacy assets they may hold. What does guarantee that they do not have enough wealth to finance all productive investment in the economy (that is, reaching the efficiency level)? It is fine to restrict initial parameter values so that the skin-in-the-game constraint is biding, but in these kind of models there is a strong incentive for agents to accumulate enough assets so that at some point the (endogenous) probability that the constraint binds in the future is negligible (See the discussion of Suarez 2010 in the IJCB). There may be reasons why it does not happen in this environment, but they are not obvious.

• “Implicit recourse” are studied in a general way first. Then, they are interpreted as a way to alleviate adverse selection. However, they also interact with the skin-in-the-game constraint. This is very interesting per se, and would perhaps be worth more investigation. For instance (assuming a more general borrowing constraint), will it necessarily increase total pledgeable income and total investment? If, as is suggested, competition on the recourse decreases the profitability of investment, it could go the other way around. If it increases investment (and decreases the equilibrium risk-free interest rate), it is likely to increase the price at which constrained firm can sell their legacy asset and alleviate the constraint further, etc.

• What is presented as the main result (the endogenous switch from separating to pooling equilibrium) is nice and very interesting. However, it is not clear to me that we need such a complicated model to generate it. It would also be nice to discuss this result in the light of Chari, Shourideh, and Zetlin-Jones (forthcoming AER), which studies reputation in secondary loan markets together with economic fluctuations.

• It is stated that agents cannot insure against the investment opportunity shock, but in fact they do use markets for this purpose. I assume that what is meant is that the realization of those shocks are not observable / contractible.

2.2 Chapter 2

Chapter 2 is very interesting and topical as well and has a good potential for publication. However, I have some potential reservations on the methodology as I explain below.

2.2.1 Approach / methodology

The model is even richer than the one in chapter 1, which makes the project extremely ambitious. Again, it seems well executed, but essential steps are missing in the text.

• What is the equilibrium concept? How is the steady state defined?

• It would be nice to explicitly solve the full model for the steady state. Under potential adverse selection, there are feedback effects between saving decisions and adverse selection (see my 2014 JF paper), which can lead to multiplicity. It is therefore not clear at all to me that the steady state is unique. In case of multiplicity, what is the selection method that is used? Is the intuitive criterion relevant here?
• Martin uses Foerster et al.’s method, which makes sense to deal with a MSDSGE. However, adverse selection itself is often associated with strong non-linearities (reinforced by the strategic complementarities identified in my paper mentioned above). Is Foerster et al. relevant in this case, I genuinely don’t know, but that should be discussed together with the multiplicity of equilibria.

2.2.2 Link with literature

• It would, in my view, also be worth discussing the results in more details and comparing them with Eisfeldt (2004), Kurlat (2013), Chari et al. (forthcoming), and the adverse selection literature based on search frictions (see papers from Guerrieri and Shimer and Chiu and Koeppl for instance).

2.3 Chapter 3

With chapter 3, Martin demonstrates further impressive skills. Together with his coauthor, they blend two strands of the literature and develop a tractable model which they then estimate. The paper is original, very clear, and well executed. Unfortunately, I am afraid that the potential for publication in a top journal is lower than the first 2 chapters because the results are not really surprising and the contribution to our understanding of the underlying issues is not very large. To be sure, “negative” results are important too because they allow researchers to close unpromising avenues, but these days they rarely find their way towards top publication outlets.

As an additional comment, “habit formation” does indeed help to “generate” an equity premium with a lower coefficient of risk aversion, but they still imply a very high level of effective aversion to consumption risk (see “The Equity Premium Puzzle in Retrospect” by Mehra and Prescott, and the link the risk-free rate puzzle in Weil 1989).