Review of Marta Cota’s Dissertation Thesis

Marta Cota's thesis investigates household financial behavior concerning retirement savings (Chapter 1) and mortgage choices (Chapters 2 and 3). Through a combination of empirical analysis and advanced modeling, the thesis addresses current puzzles in the household finance literature and policy issues, such as the high level of inequality in retirement savings and various mortgage-related outcomes.
My overall judgment of Marta's work is very positive. She skillfully uses recent insights from behavioral economics to model household financial behavior. Often starting from interesting stylized facts that themselves contribute to the literature, Marta uses these as a foundation to structurally estimate household behaviors consistent with these facts. This approach allows her to conduct counterfactual policy evaluations aimed at improving financial outcomes among households, especially in the U.S.
Since the first paper is already at an advanced stage of the publication process (R&R at Review of Finance), my comments will focus on the other two chapters. These comments are primarily intended to improve the quality of the papers.

Comments on Chapter Two

Overall comment: The paper should be streamlined and better structured:

1. There are too many subsections, often only a few lines long (e.g., 2.7.1).
2. There are too many exhibits (tables and figures), which are poorly described and do not follow industry standards. For example, the notes at the end of the tables and figures lack sufficient content.
3. The data description is often lacking. It is unclear what each dataset (SCF and NSMO) contains and how the match between the two datasets is implemented, even after reading the related appendix. A more detailed description of the matching process, the assumptions required, and its robustness would enhance the credibility of the findings.
4. Regarding the matching process, if the matching has been done using the characteristics in Table 13, it is important to know how many observations per cell are available.
5. A more open discussion of the limitations of the current approach would also help the paper's credibility. For instance, the work primarily identifies correlations between financial literacy, search behavior, and mortgage outcomes, but does not establish causality because there is no proper identification strategy, nor are there mentions of the assumptions needed to interpret the findings as causal. The paper accounts for important covariates like age and education, which provides a more causal flavor to the findings. However, the absence of basic information in many tables prevents us from understanding how much of the variation in the data is explained by the included variables (especially Tables 11, 12, and 15).

Comments on Chapter Three

1. To a lesser extent, this paper suffers from the same issues reported for Chapter 2, but since the main contribution of the paper is its structural model, the data description is somewhat less critical. However, more information about the data would still be useful.
2. My main comment on the model is whether it is necessary to have an initial financial skill distribution or if it is sufficient to have different productivity levels of labor to determine varying financial skills over the life cycle. I have in mind the Lusardi et al. paper (JPE 2017), which suggests that for high-income individuals, the reward for investing in financial literacy rises because saving needs are relatively important.
3. The policy experiment of introducing a financial education training program is not clearly explained. The main text does not detail how this program should affect the financial literacy of renters. My interpretation is based on a reduction in the cost, but I suggest basing the effectiveness of this program on evidence from real experiments in the literature to estimate how much people usually learn from short courses like the one depicted in this paper.

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