

Referee report  
"Theoretical Foundations of Buffer Stock Savings"

This paper provides a comprehensive theoretical investigation of savings behavior in a consumption-savings model in which consumers with CRRA preferences are subject to idiosyncratic shocks. My main concern is that most of the results that the author proves are not very surprising. Consumption-savings models have been analyzed extensively both analytically and numerically for the past 30 years and the profession has acquired a pretty reasonable understanding of how such models work. There was no rigorous theoretical analysis of CRRA preferences prior to this work, but there exists a large number of closely related studies that analyze either the same model numerically or closely related models with slightly different preferences/constraints analytically.

In my opinion, to merit publication in a top general interest journal such as *Econometrica*, a theoretical paper either needs to contain new insights or develop a novel methodology that can be applicable elsewhere. I do not think that this paper passes that standard. Most of the insights could be guessed from the existing body of work, and the methodology is not obviously new or very useful for other problems.

I also think that the exposition can be improved. Right now the paper reads like an encyclopedia. The author throws at readers one result after another, characterizes the consumption function for this set of parameters or that with no guide to what we are supposed to take from the analysis. I suggest that the author picks a couple of the most important results he wants to prove, explains to the reader why they are interesting or important, and leaves the rest for the appendix.