THE REVIEW OF
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STUDIES

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Philadelphia, January 9, 2018

Prof. Alexis Akira Toda
University of California, San Diego

Manuscript # 26107: An Impossibility Theorem for Wealth in Heterogeneous-Agent Models with Limited Heterogeneity

Dear Alexis,

Thank you for submitting the above paper to the *Review of Economic Studies* for editorial review. I have now received four mostly informative referee reports from experts in the area of macroeconomic theory that have all worked on wealth distributions in heterogeneous agent models. In addition, I have looked at the paper myself, and have been in touch with a 5th referee.

I am afraid the message I have to convey is not good. All referees are sceptical about the suitability of the paper for the *Review*, and referees 1-3 recommend I should reject it at the *Review*. Referee 4 had reviewed the paper before for AERI and remains sceptical about the suitability of the paper for a top 5 journal. I have access to the previous report by this referee but discounted her/his editorial recommendation when making my decision. In any case, since s/he is the least negative of the referees, using the report, if anything, works in your favour. After reading the manuscript myself alongside the reports and cover letters I do not necessarily agree with all detailed comments the referees offer, but I do share their overall final assessment about the suitability of the paper for the Review. Therefore, I must conclude that we should not proceed further with your paper.

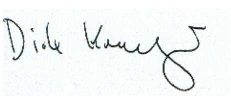
Let me briefly summarize the main reactions the referees have about the paper and my interpretation of them. The overriding concern of all referees is that although the paper makes a very useful theoretical contribution to the theoretical literature on the canonical Bewley model, this contribution is distinctly more suitable for a good field journal in theoretical (macro-)economics that for a top 5 general interest journal. This assessment comes in two incarnations. First, straightforward extensions of the canonical model are known to break the result, e.g. extending the model to include idiosyncratic investment risk, heterogeneity in discount factors etc. It is useful that the paper proves these extensions are not just sufficient, but also necessary, but since the applied literature has already moved on from the canonical model for quite some time, the impact of the result is bound to be limited, according to the referees. To a large degree, this is the view of referee 1 (comment 1), referee 2 ("Framing"), referee 3 (item 4) and referee 4 (comment 1). This consideration leads all but referee 4 (who is on the fence) to a rejection recommendation, and ultimately leads me to my negative decision since I cannot fundamentally disagree with this view.

The second incarnation of this concern, which might potentially be important even at more specialized journals, is just how novel your theoretical result is, taking as given what it intends to show. I think it is fair to say that the papers by Benhabib, Bisin and co-authors anticipate (or you might say, conjecture) your result. Of course, actually providing a formal proof under the most general conditions is important and worthy of publication, but not necessarily in a top 5 general interest journal. Clearly footnote 3 belongs in the main text, in my view. Second, the Benhabib and Bisin papers build very significantly on Grey (1994), e.g. their Theorem 3 in the recent JEL paper. They are very explicit about this, and it seems to me the current paper, given its results on the tail behaviour of the wealth distribution, should probably discuss the relation of your main results to Grey's results. Of course, there again is a difference between assuming a certain savings behaviour on the household side and giving general conditions under which this assumption is actually true, under the appropriate assumption on the utility function and the product between interest rate and time discount factor. It is also very useful to have a paper that synthesizes the results from the literature and extends it to the unbounded income case, but I still think that the Grey (1994) work ought to be given credit in addition to referring to the Benhabib and Bisin papers that use it extensively. My informal discussions with the 5th referee who does not provide a report and abstains from making a recommendation helped me appreciate this point much better.

Overall, my own reading of the paper is that it belongs in one of the leading journals in (macro-)economic theory such as *JET*, and I can see how it might become as influential as, say, the Chamberlain and Wilson paper in *RED*. I had a fairly precise idea about this after my first read, and in retrospect I probably should have made this editorial decision without consulting referees, saving you valuable time. For this I sincerely apologize.

To conclude, I thank you again for letting us review your work. I do regret that the news I have to convey is not more positive, and hope you find the comments of the referees useful for your future work on this topic. Also, please keep in mind that the *Review* publishes less than five percent of all submissions and that, consequently, many good papers are not accepted.

Sincerely yours,



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