

January 20, 2018

Dear Professors Daniels and Kakar,

I have now received reports from three reviewers regarding your manuscript, “Normalized CES supply-side system approach: how to replicate Klump, McAdam, and Willman (Review of Economics and Statistics, 2007),” (*Manuscript Number 2348; Discussion Paper Number 2017-70*).

Based on the feedback I have received, I am inviting you to submit a revision. Your revision should address the individual points raised by each of the reviewers. In addition, you should include a separate document that provides a point-by-point response to each of the reviewer’s comments. You are not required to do everything the reviewers say, but if you disagree with a recommendation, you should state your reason. A successful revision will include addressing the following:

1) Reviewer 2 raises a serious concern that your replication study has little value-added beyond Stewart (2017). I agree. The instructions for this Special Issue state “Contributors to the special issue will each select an influential economics article that has not previously been replicated” (<http://www.economics-ejournal.org/special-areas/special-issues/the-practice-of-replication>). The paper you chose does not satisfy this requirement. In your response to Reviewer 2, you state that your value-added is that you plan to replicate KMW using newer vintage data over the same time period examined by KMW. However, that is not a sufficient enough difference to justify another replication. Therefore, while you can still include a “narrow verification exercise” as part of your replication plan, you will need to expand your replication plan to address the robustness of KMW’s results. The most straightforward approach would be to check for robustness by extending the data set using more recent data, though you could consider other extensions as well.

2) Reviewer 2 also notes that you are inconsistent in your criteria for what constitutes a “successful” replication, alternately emphasizing “size and significance of parameter estimates” and later emphasizing (solely) statistical significance. Suppose your replication produced parameter estimates that were substantially smaller in size than reported by KMW, but were still statistically significant. Would that be a “successful” replication? There is no right answer here, but you do need to clarify your criteria of “replication success.”

3) Finally, while you are to be commended for giving detail about KMW’s model, there is too much detail. This is reflected in the fact that the conditions you identify for a “successful replication” consist of five separate sets of estimates covering a page and a half of your document. Furthermore, the criteria for a “successful replication” are far too general; for example, “the results should be similar to the unconstrained approximated model.” What does that mean? What is similar? What parameters are being compared? Do all of them need to be “similar?” This definition of a “successful replication” is too general to be of any use. Your revision should focus on a few key parameters in KMW. You should apply whatever criteria for “successful replication” you settle on (see previous comment”) for the estimation of these parameters so that the reader has a clear idea in advance of what a “successful replication” would look like.

A revision that satisfactorily addresses the points above, as well as responding to the individual reviewer’s concerns, is likely to result in a favourable publication decision.

Thank you for submitting your manuscript to *Economics: The Open-Access, Open-Assessment E-Journal*. I look forward to receiving your revision.

Sincerely,

W. Robert Reed
Co-Editor, *Economics E-Journal*