W. Robert Reed, Raymond J.G.M. Florax, and Jacques Poot (2015). A Monte Carlo Analysis of Alternative Meta-Analysis Estimators in the Presence of Publication Bias. Economics Discussion Papers, No 2015-9, Kiel Institute for the World Economy. http://www.economics-ejournal.org/economics/discussionpapers/2015-9

Referee report:

The submitted manuscript evaluates the performance of various meta-analysis estimators by using a Monte Carlo analysis under various settings. The most important contribution is the extension of the analysis to the setting where multiple estimates per study are reported, which is what we typically observe in economics. The results are mixed and rather pessimistic compared to previous similar evaluations: the authors find that the estimators typically used in economics meta-analyses to correct for publication bias cannot be automatically expected to work better than simple estimators that disregard the bias. I believe the manuscript is an important contribution that significantly adds to our knowledge, and therefore can be published after minor revisions. I have several comments that the authors might want to consider when preparing the final version.

1) Terminology. The authors follow the typical meta-analysis methodology, usually used outside economics. For example, the use of "fixed effects"' is not consistent with what an economist would imagine under such a term. One of the estimators is called ``weighted least squares,'' but it seems that virtually all estimates evaluated in the paper are weighted, which is puzzling. I worry that many readers will be confused by these terms. Perhaps this is a good opportunity to introduce terminology that would more intuitive and consistent. In the present form the abstract of the paper will only be understandable to a few researchers narrowly specialized in the evaluation of the performance of various meta-analysis estimators. I am an empirical researcher who has written several meta-analyses in economics, but was confused by the terms used in the abstract.

2) Style. The paper is written competently, but I would consider rewriting some parts, especially the abstract, which should provide us with the results of the study. The performance of the estimators is evaluated under various settings, and the discussion of the numerical results is quite long; I worry that few readers will have the patience to go through all of this. Perhaps some parts could be moved to the appendix (especially the cases when only one estimate per study is assumed, which is not realistic in economics) and more space could be devoted to the interpretation of the results and implications for future use of meta-analysis.

3) Simulation design. The authors make several arbitrary choices in the design of their simulations; for example, see equation 3.B on page 5. Where do these numbers come from? I understand that arbitrary choices are inevitable, but would appreciate some justification of these parameters, and, potentially, evaluation how the results are robust to the choice of these parameters.

4) Focus. I think the authors should focus more on their contribution, which is the panel setting (each study reports several estimates). Almost all recent meta-analysis in economics work with several estimates from each study, so this is much more realistic and interesting than the rest of the analysis. The authors stress that the simulated meta-data should be as

close as possible to the actual data samples used in meta-analyses. They could pursue this problem further in their analysis. For example, an important issue in meta-analysis is the unbalancedness of data: some studies report many more estimates than other studies. Should we weight estimates by the inverse of the number of estimates reported in each study to give each study the same weight, such as in Havranek and Irsova (2015)? Another concern is precision-weighting with panel data. It is not clear how to interpret the results when weights (such as precision) are not constant within panels; for this reason, most Stata panel estimators do not allow precision weighting. Moreover, almost all economics meta-analyses include other moderator variables aside from the standard error. If the moderator variable is defined on the study level (the number of citations, for example), then precision weighting introduces artificial variation to that variable. In sum, I do not think we know precisely what it is that we estimate when we use precision weights for panel data, and the authors should consider non-weighted variants as well. Another crucial issue in meta-analysis is the potential (and likely) endogeneity of the standard error. When a method choice influences both the point estimates and standard errors in the same direction (think of using instrumental variables, for example), then SE is not exogenous to the point estimate. This is what I often observe in the data: tiny estimates are accompanied by tiny standard errors for no obvious reason other than methodology (but the precise causes are difficult to code). Such endogeneity leads to false detection of publication bias, so meta-analysts should instrument the standard error, preferably with something uncorrelated with method choices; for example, a function of the number of observations used in the original study (as in Havranek, 2015). If the standard error is not exogenous to the point estimate, then precision weighting is problematic even with one estimate taken from each study. Moreover, precision-weighted results are extremely sensitive to outliers. In each meta-analysis there are several estimates accompanied by implausibly large values of precision. What should we do with these values? Winsorize them as in Havranek et al. (2015)? While I know the present study cannot answer all of these questions, they could perhaps resonate in the manuscript, and I would like to know the authors' perspective on these issues.

Thank you for submitting your fine paper.

References

Havranek, T. (2015). Measuring Intertemporal Substitution: The Importance of Method Choices and Selective Reporting. Journal of the European Economic Association, forthcoming.

Havranek, T. and Irsova, Z. (2015). Do Borders Really Slash Trade? A Meta-Analysis. William Davidson Institute Working Papers Series wp1088, William Davidson Institute at the University of Michigan.

Tomas Havranek, Marek Rusnak, and Anna Sokolova (2015), "Habit Formation in Consumption: A Meta-Analysis." Czech National Bank and Charles University, Prague. Available at meta-analysis.cz/habits.