Reviewer's report

Title: The importance of adjusting for potential confounders in Bayesian hierarchical models synthesising evidence from randomised and non-randomised studies: an application comparing treatments for abdominal aortic aneurysms

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Reviewer: Paul Gustafson

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I find this to be interesting and well-executed work. I have the following comments.

p. 9: there is some confusion with notation and equations here. First, eq. (7) seems to be missing a summation sign (over m). Second, in the text m is referred to as a confounder but used as an index (of the M confounders). For instance, I think the intended meaning is that x_mTij and x_mCij are "the values of the m-th potential confounder..." (same issue arises on p. 10).

p. 9 and elsewhere: more interpretable (particularly for non-statistical readers) to report standard deviations instead of variances?

p. 9 and elsewhere: is "truncated to be positive" clearer than "truncated below zero"?

p. 10, 2nd sentence of 2.3: what variance? There are many floating around in your set-up.

p. 10: truncating the prior to force |mu-th1|<|mu-th2| makes intuitive sense, but also would seem to mess-up the interpretation of mu?

p. 12: Can't write sigma ~ N(0,???) etc. without being explicit that you mean half-normal, not real-normal.

p. 13 and forward, re: multiple imputation. Here is my biggest methodological concern. First, the standard interpretation of "multiple imputation" is that one imputes m>1 datasets, fits the complete-data model to each one, and then synthesizes the m inferences. However, the current manuscript reads as if multiple imputation produced 1 dataset, so I'm confused about what was actually done. Second, in a paper that is ostensibly about evidence synthesis, the 2-stage nature of the procedure (impute one (or more) datasets in R, dump into WinBUGS) feels inelegant. The more natural solution (though admittedly there may be devil in the details) would be to include the unobserved x values along with the unobserved parameters inside the MCMC. Of course this would necessitate modeling the joint distribution of x, but multiple imputation requires this as well. At the very least, more clarity and discussion is needed around this issue.
p. 16: a small point, but phrases like "estimated median value of their posterior distribution" disturb me. The "estimated" makes one think of statistical estimation, when of course you really mean "numerically approximated" (via MCMC). I find a lack of clarity between estimation error and numerical/simulation error is a problem in a lot of reported Bayesian analysis.

p. 18: I agree with the sentiment about better reporting characteristics of study populations. As a vaguely related point, and in the spirit of open-access, reproducible research, etc., I hope you will post your data on the web, so that others can reproduce your findings and try alternate analyses.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.