Reviewer's report

Title: A test for reporting bias in trial networks: simulation and case studies

Version: 2 Date: 27 May 2014

Reviewer: Dan Jackson

Reviewer's report:

This paper investigates a proposed test for reporting bias in trial networks, though both simulation studies and case studies. I found it to be interesting but have some concerns about the methodology.

Major Compulsory Revisions

1. I think including much of the material as supplementary is great but the first set of supplementary materials (Power calculations for binary and continuous outcomes) should appear in the main paper because this is central to the logic.

2. Related to 1 above, I am unclear how between-study heterogeneity and (in network meta) how inconsistency/incoherence are incorporated into the power calculations - surely we could get more or less significant results as the heterogeneity and/or inconsistency increase? The methods do not seem to allow for this. Heterogeneity is allowed in the simulation study (tau2=0.02, 0.08, 0.25) and we see in the results that the type 1 error rate depends on the simulation parameters. All of this suggests to me that the methods do not really address the problems associated with heterogeneity and inconsistency and I would like to see this point discussed.

3. The simulation studies seem to suggest that the method does not perform satisfactorily in general. This should be discussed more and ways to improve the method to retain type 1 error rates should be discussed.

4. For binary data, a normal approximation is used. In network meta exact binomial within-study distributions are usually used in WinBUGS. The limitations of using a normal approximation to binary data should be discussed more.

5. The notation b(i|N, E/N) is confusing - surely b(i, E/N) would be better or the authors should explain why they "condition on N" when evaluating the sums and hence explain their notation.

6. I do not really regard this as a method for network meta-analysis because this is treated as "J meta-analyses of nj trials each". Hence the network meta-analysis is treated as separate metas in the method, but in reality network meta considers the entire data as a single network, and so issues like consistency/coherence become important. I would like to see this issue receive more attention, how could the method be extended so it applies to network meta-analysis (as opposed to a network of trials, as it currently does).
7. The comment that the test may be applied to networks with or without inconsistency is rather blunt - yes the test could be applied but surely its operating characteristics depend on this, as well as the inconsistency?

8. The type of networks included in the simulation study/example should be made clear in the main text. Network diagrams should be included for both the simulation study and the example so we can see the types of networks involved.

All of the above lead me to conclude that this paper is of limited interest because the method does not seem to perform very well and it does not really tackle the challenge of network meta-analysis in view of my point 6. I think there are some interesting ideas in the paper however so I am not completely negative about it but I do think that the limitations of the method should be more openly and honestly discussed if a revision is invited.

**Level of interest:** An article of limited interest

**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