Reviewer's report

Title: Meta-analysis and The Cochrane Collaboration: 20 years of the Cochrane Statistical Methods Group

Version: 2 Date: 4 July 2013

Reviewer: Edward Mills

Reviewer's report:

My review is unlikely to be well received by the authors but I feel I must make the following suggestions in order for this paper to meet the standards for unbiased articles in the scientific literature.

In general, this paper is very badly written and lacks any objectivity. I have no doubt that the Cochrane Library was an important contribution to the field of healthcare. However, I fail to understand the religious zeal that supporters of the style of research Cochrane does, feel the need to unquestionably praise it.

Cochrane was once an organization with the potential to influence healthcare. I think those days have passed and, in general, the quality of Cochrane reviews has gone down importantly in the last decade. I can honestly say that I do not trust most Cochrane reviews, mostly because they are conducted by non-experts in both the clinical and statistical fields. I realize this sentiment is rarely spoken, perhaps because it is such a large social network of people, with the usual big personalities and common opinions. Given my lack of zeal at the organization, my comments below reflect that I do not feel strongly either way on whether the organization should be promoted or not, and that includes the statistical methods group (who I might add, I do have a lot of individual respect for).

Major Essential Revisions

1) In general, you need to tone down the support for the organization and simply state the facts.
2) Please avoid making this about personalities and do not mention what individuals said what in meetings twenty years ago.
3) I similarly this quoting this from early guidance documents is an unprofessional style of writing. Page 4.
4) It seems a bit pointless to me to quote an entire section of a grant, especially one that was unsuccessful. Page 6.

Minor Essential Revisions

5) On page 8 and 9 you continue to state that the SMG made major contributions to meta-analysis, and indeed some of them did. But there were also major contributions to the field made by non-SMG that also deserve recognition. The work of Alan Donner, for example (who you reference) isn’t really SMG work. I think this reflects a complete absence in this paper of critical reflection on the
level of success of the collaboration.

6) Page 9. I agree that the I2 measure is the most widely used statistic for heterogeneity but it is probably also the most incorrectly used measure and many authors fail to note that the I2 works moderately well for relative measures, but extremely poorly for continuous measures where a high value is often taken as evidence of problematic heterogeneity that would not be found had a relative effect measure been used.

7) The fact that Revman doesn’t allow meta-regression is a huge drawback to the successful use of revman and is really a sign of the out of date approaches of most reviews.

8) Page 10. While I agree that some members of SMG have made massive contributions to the field of MTCs, I think it is a stretch to state that “account for the vast majority.” Jeroen Jansen and people within industry have published the most on MTCs (even of some of them don’t end up on pubmed).

9) Page 11. The fact that the SMG received 11 awards from their own organization doesn’t really convey confidence that this has been widely taken up outside of the collaboration.

10) Table 2 is useless.

**Level of interest:** Reject as not of sufficient priority to merit publishing in this journal

**Quality of written English:** Not suitable for publication unless extensively edited

**Declaration of competing interests:**

I have worked with two of the authors in small ways over the past 6 years.