Reviewer's report

Title: Community-based directly observed therapy (DOT) versus clinic DOT for tuberculosis treatment: a systematic review and meta-analysis.

Version: 2
Date: 3 November 2014
Reviewer: James Johnston

Reviewer's report:

1. Is the question posed by the authors well defined?
   Yes

2. Are the methods appropriate and well described?
   Generally yes, see below for further comments

3. Are the data sound?
   Yes

4. Do the figures appear to be genuine, i.e. without evidence of manipulation?
   Yes

5. Does the manuscript adhere to the relevant standards for reporting and data deposition?
   Generally yes, see below for further comments

6. Are the discussion and conclusions well balanced and adequately supported by the data?
   Yes

7. Are limitations of the work clearly stated?
   Yes

8. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
   Yes

9. Do the title and abstract accurately convey what has been found?
   Yes

10. Is the writing acceptable?
    Yes

Please make your review as constructive and detailed as possible in your comments so that authors have the opportunity to overcome any serious
deficiencies that you find and please also divide your comments into the following categories:

• Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)

Line 107: How many studies included family member based DOT? Please comment on this.

• Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Line 54: May want to add more detail re: barriers to clinic and community-based DOT

Line 130: there is no mention re: the WHO outcome “not evaluated” (which includes transfer of care). This may significantly alter results. I would at least comment on this, or perform a secondary analysis including transfer of care as an unsuccessful outcome to see if this significantly changes your results.

Line 183: I am concerned about criteria 6 (i.e. your definition of major selection bias). This seems somewhat arbitrary as there is not a fixed definition/criteria for this. In addition, I do not see a criteria for study quality, and note that several studies were of low quality. I would devote a bit more discussion to this issue and would recommend that you perform analysis by study quality as you did not exclude studies based on this.

Discussion: I would suggest discussing the value of patient preference, as some patients may prefer to attend clinic, rather than have a community health worker visit them at home (stigma).

• Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

None, see minor revisions above.

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests