Reviewer's report

Title: Partner Notification for sexually transmitted diseases in developing countries: a systematic review

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Reviewer: Michael Sweat

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"Partner Notification for Sexually Transmitted Diseases in Developing Countries: A Systematic Review"

Lead Author: Nazul Alam

Discretionary Revisions

1. The authors opt for “STD” rather than “STI”. The field has tended to move towards use of STI as it includes treatment of people infected with sexually transmitted pathogens, yet perhaps not yet at a disease stage.

2. There are two important citations from the literature that are not referenced. One is a systematic review on STD partner notification that was published by Mathews and colleagues in the International Journal of STDs and AIDS (2002;13:285-300). There is also a Cochrane review in press systematically reviewing STD Partner Notification, also led by Mathews (see: http://www.cochrane.org/reviews/en/ab002843.html).

Minor Essential Revisions

1. Page 5 – “with validation of last author (SK)”. How this was done should be better described.

2. Page 7 typo – “Other study population”.

3. Page 7 typo – “…included community based men and women…” Perhaps this is just unclear in meaning and needs elaboration.

4. Page 9 – “were reported to over-diagnose and over medicate”. The meaning of over-diagnose is not clear. Does this mean that there are false positives identified and treated? Is over medication meaning that clients get too much medication, or that people are medicated who do not need it?

5. Page 10 – “improving PN and referral”. In what manner are they improved?

6. Page 10 – last sentence, there is no reference to the statistical significance test for these findings.

7. Page 11 – “promulgate provider-initiated notifications” – meaning is unclear.
8. Page 11 typo – “4 out of 10 study clinics did not counselors in their”. This is also an example of vote counting.


10. Page 12 typo – “circumstances suggested PN to be cost-beneficial”.

Major Compulsory Revisions

1. As the analysis is a systematic review, it is subject to standards in 5 key areas using conventions widely accepted in the field. Each is reviewed here:

   (a) Framing of the research question – The authors framed a very broad set of 5 questions, some of which are not easily measured or summarized in a consistent manner. Two of the research questions address barriers, to client’s notifying partners of their STI, and those related to infrastructure which may impede notification of partners. One of the five questions is to examine “PN approaches that were evaluated in developing countries”. This last research question is very broadly defined, making it difficult to succinctly summarize the findings. The analysis would thus benefit from a more focused research agenda, and selection of outcomes that can be consistently measured.

   (b) Identifying relevant work – The criteria for inclusion are specified in the methods section, but some detail is not clear. For example, what qualifies as a STD (is HIV included?), and what types of intervention programs qualify? It appears that the authors focused on clinic-based interventions, but this was not a stated inclusion criterion. Otherwise the methods for identifying relevant work are fine. The paper would be enhanced with addition of more detail on the specific inclusion criteria, preferable listed clearly in a summary statement.

   (c) Assessing the quality of the studies – the authors have an acceptable strategy for grading study quality. However, they do not use this to triage of studies, and seem to accept all studies regardless of study quality. There is little discussion on study quality provided.

   (d) Summarizing the evidence – the authors opted for a mixed strategy, conducting both a qualitative review, and using a vote counting method for some outcomes. The qualitative review tends to cherry pick results, and not summarize across studies. In reviewing findings with this strategy the authors also tend to focus on discrete results from select studies, and not comment on the presence of the outcome across studies (no denominator for the frequency of the outcome is commented on). The vote counting approach is used for some outcomes, such as for “Willingness of index patients to self-notify partners”. See for example page 7 where they state “Six of them reported that a majority….”. Vote counting in systematic review is known to frequently bias the interpretation of results, as it ignores the effect size and sample size from studies. In some instances the vote counting is also not reflecting on the statistical significance, which is a further weakness of this approach. In Table 3 the authors provide a nice summary of the
results across studies, and in many cases there are descriptive results shown. A superior manner of summarizing these cross-study results in the text would be to present a weighted average and range of the proportions presented in the table. In general, the paper does not summarize enough, tends to pick and discuss discrete results, and does not fully exploit the available quantitative data from the original studies. Additionally, the characteristics of interventions used in the included studies were not coded, and thus it is difficult to get a sense of what the various interventions assessed in the original studies actually were.

(e) Interpretations of the findings – The interpretation of the findings identify some interesting findings. Yet based on bias likely introduced from the way the review was conducted lead to some concern that the interpretation is likewise biased.

2. Tables 1 & 2 are stratified by “primary” and “secondary” outcomes. It is not clear what the definition of primary and secondary is for the study.

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

**Quality of written English:** Needs some language corrections before being published

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

**Declaration of competing interests:**

I declare that I have no competing interests.