**Author’s response to reviews**

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

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**Author’s response to reviews:**

Response to comments from Reviewer 1:

This is an interesting review paper of partner notification for STD in developing countries. The paper is well written and will be interested in STD public health researchers, but there are a few things that would make it even easier to follow:

Response: Thank you for this comment; we have now addressed the other comments as below.

**Essential:**

Could the authors please clarify what specific STDs they are targeting? On page 3, the authors talk about PN for HIV, but the rest of the paper appears to be devoted to non-HIV STDs. On page 5, it says the search was limited to syphilis, Chlamydia and gonorrhea and then on page 6 they say it was limited to curable bacterial STDs. PN for syphilis and HIV are likely to have different impact and cost effectiveness strategies than for PN for Chlamydia and gonorrhea and may need to be considered separately.

Response: We targeted common curable STIs in this paper (gonorrhea, syphilis, chlamydia, and trichomoniasis) but not included HIV partner notification. We have eliminated the statement in HIV partner notification in page 4 (page 3 in earlier) and made it consistent throughout the paper that we have focused selected curable STIs.

While alluded to in some areas of the paper, it would be helpful if the authors could comment in gender differences in the outcomes since power differences in sexual partnerships in developing countries can be so great.

Response: Thanks for picking up this issue. Gender difference on PN outcome has been discussed in page 9 under the ‘Client reported barriers in notifying partners’ in page 12 under the discussion section.

Why is the information on patient-delivered medication approaches presented in
the discussion rather than the results and why is it not one of the main outcomes examined?

Response: Patient-delivered medication approaches are discussed under the research question ‘PN approaches that were evaluated in developing countries’ one the five research questions explored in this paper. It is now discussed in page 10 under the paragraph heading “PN approaches that were evaluated in developing countries’ in the result section, and also in the discussion section in page 12.

Trial is misspelled on page 5, last para.

Response: This has been corrected in last paragraph in page 5.

Discretionary:

It would be helpful to know what the policies are on PN in the countries included.

Response: We have discussed some of the relevant policies related PN in developing countries as a whole and some specific countries in particular in page 11, 12 of the paper. Detailed discussion of the policies in the countries is not possible in the context of the paper.

The tables are very nice, but it would be helpful if Table 1 was sorted either by continent or type of PN or some criterion.

Response: We have arranged the tables (table 1, table 2 and table 3) alphabetically by the first author’s name, this has been suggested by the ad-hoc reviewer earlier, and found to be useful.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Response to comments from Reviewer 2:

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.

Response: We have change STD to STI in the paper as recommended.

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:
Response: Reference of the systematic review on STD partner notification that was published by Mathews and colleagues is now cited as reference number 13 in page 3, but we have not cited the other paper in press.

Minor Essential Revisions
1. Page 5 – “with validation of last author (SK)”. How this was done should be better described.
   Response: We have now changed text saying ‘under supervision of the last author’ instead of ‘with validation of last author (SK)’ in page 5.
2. Page 7 typo – “Other study population”.
   Response: The typo is now corrected in page 7.
3. Page 7 typo – “…included community based men and women…” Perhaps this is just unclear in meaning and needs elaboration.
   Response: The statement of “…included community based men and women…” has been rephrased in page 7 as ‘….included men and women from general population…’
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?
   Response: Exactly, major drawback of syndromic management of STDs is that there are chances of false positive cases identified, where clients are medicated who do not need it. It is now rephrased in page 10.
5. Page 10 – “improving PN and referral”. In what manner are they improved?
   Response: We have rephrased the sentence as ‘two randomized trials investigated effectiveness of client-centered counseling on PN outcomes, instead of ‘two randomized trials investigated effectiveness of client-centered counseling on improving PN and referral’ in page 10.
6. Page 10 – last sentence, there is no reference to the statistical significance test for these findings.
   Response: Findings from the Zimbabwean study presented in this sentence is now referenced in page 10.
7. Page 11 – “promulgate provider-initiated notifications” – meaning is unclear.
   Response: We have rephrased the sentence as ‘overworked health care providers dealing with a large number of STI patients will not be suitable for provider-oriented notifications’ instead of ‘overworked health care providers dealing with a large number of STI patients will not promulgate provider-oriented notifications’ in page 11.
8. Page 11 typo – “4 out of 10 study clinics did not counselors in their”. This is
also an example of vote counting.

Response: We have rephrased the sentence saying that ‘almost half of the study clinics did not have counsellors in their clinics’ instead of ‘4 out of 10 study clinics did not counselors in their’. in page 11.


Response: typo has been corrected for trial in page 12.

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

Response: Cost-beneficial has been changed to cost effective in page 12.

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.

Response: We identified five research questions in this paper relevant to partner notification in developing countries, we have decided to pursue a descriptive review and thus the five cross cutting research question seemed reasonable to us in this purpose. The last research questions is ‘to evaluate PN approaches that were evaluated in developing countries’, we have identified the PN interventions that were studied in developing countries were reviewed and summarized in this paper.

(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.

Response: We have not included studies that dealt with HIV partner notification in this review, rather included common curable STIs, namely, syphilis, gonorrhea, chlamydia, and trichomoniasis. We have included both clinic based and population based studies if satisfy our other inclusion criteria. We have further narrated the inclusion criteria in page 5/6.

(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.

Response: Thank you for the comments about quality grading criteria. We have identified only 39 articles matched our selection criteria, and we have not used the quality grading for inclusion or exclusion of articles. Our grading exercises give an idea about quality of the studies conducted in developing countries on PN and found to be included in this review. We have included more discussion on quality grading of the studies in page 6, 7 and 13 in the paper.

(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.

Response: We opted a mixed review strategy combination of qualitative and quantitative review, because two out of the five research questions addressed in this study are descriptive in nature, other two were quantitative, while another one could be both. We have mention in the paper that formal meta-analysis was not feasible for a number of reasons as discussed in page 13. As suggested, we converted most of the vote counted number into proportion or descriptive approaches in page 7, 12, however, in some cases such counting is used to categorize the study characteristics. For example ‘reviewed studies had diverse study designs and populations. Only 3 studies were randomized trials, 4 studies were pre-test post-test evaluation etc.. We kept presentation of results and discussion according to the five research questions selected in this study, and we tend to remain consistent in this.

(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
Response: We admit that there are chances to introduce bias in the interpretation made in this paper, however, we identified five research questions a priori in for this review and presented results and discussion around these issues.

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.

Response: The table 1 presents the articles, where partner notification was studied as a primary outcome of interest, while the tables 2 presents the articles, where partner notification was not studied as a secondary outcome of interest, while their primary outcome was something else, e.g., prevalence of STIs, STD management etc.. We rephrased titles of the table 1 and table 2 to clarify this further.