Author's response to reviews

Title: Competition is not necessarily a barrier to community mobilisation among sex workers: A mixed method intervention planning assessment from Zimbabwe

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Author's response to reviews: see over
Dear Editor

Submission: *Competition is not necessarily a barrier to community mobilisation among sex workers: A mixed method intervention planning assessment from Zimbabwe*

We are pleased to submit a revised version of our paper, and are extremely grateful to all three reviewers for having provided feedback to strengthen our manuscript. We have addressed the comments provided by reviewers, as detailed below. We hope our revisions will satisfy the concerns raised, and look forward to moving forward toward publication.

**Reviewer 1 report (Lisa Lazarus)**

**Reviewer's report:**

Major Compulsory Revisions

Overall, this is an interesting paper. The main finding, that competition is not necessarily a barrier to community mobilization and support is of interest to those working in community-based practice. However, this manuscript requires major revisions before it can be considered for publication.

Background

Was bivariate analysis conducted? I'm not sure CIs are really needed for the descriptive statistics.

- The aim of this paper is to focus on qualitative findings, with the quantitative findings as contextual background. Further quantitative statistical analysis is ongoing and will be presented elsewhere. We have tried to make this clearer by shortening the quantitative results, and emphasising that this is a qualitative study in Methods.

- We have removed the CIs from all data in the narrative sections of the paper but have left them in the Table, in keeping with other studies.

Please consider the following references of successful SW mobilization interventions:


Methods

More details about the study are required. Even though you refer to a previously published report, it is helpful for the reader to have a brief description...
of the study setting and process. I would suggest subheadings for the Methods Section (Study design, Study Participants, Statistical Analysis, Qualitative Analysis).

- We have expanded the Methods section and addressed these points, although have opted not to include sub-headings. We have also emphasised that although this was a mixed methods study, we are presenting the qualitative study components in this paper.

How were sample sizes for each setting selected?

- See lines 151-155

Why was the decision made to not conduct bivariate or multivariate analysis? The paper would largely benefit from a more complex analysis of the survey findings.

- This will be done in a subsequent paper

How were interviewers’ selected? Were they members of the community? Were honorariums/gifts provided to participants? How long did it take to complete the survey or key informant interviews?

- Interviewers were professional research staff from the national HIV sex worker programme, and we have included this in line 158. Compensation was offered in keeping with national research ethics policy, see line 159. Line 160 mentions that interviews took roughly one hour.

Results
The statistical analysis requires some work. I'm not sure that reporting just the basic descriptive percentages is of enough interest to those working in the field of community mobilization.

- We agree, and as such have removed inclusion of the statistics throughout the text, except from the table and briefly in the newly added section on “Site Characteristics” (starting line 171) to provide background context. Further statistical analysis is underway and will be presented in future publications.

Discussion
The Discussion should refer back to previously published research and what is already known about community mobilization. Please also refer to: Reza-Paul, S., Lorway, R., O’Brien, N., ... & Steen, R. (2012). Sex worker-led structural interventions in India: a case study on addressing violence in HIV prevention. The Indian J of med research, 135(1), 98-106.

- We have added references in the discussion, particularly on the experience from the Avahan programme in India. The recommended Reza-Paul reference is included earlier in the Background section.

- We believe we have situated our findings more comprehensively within the existing community mobilization literature (within the confines of the word limit!) by demonstrating how our results support similar findings from India and discuss how our findings fit with concepts of social cohesion and social capital, which are often used as markers for successful community
mobilisation. In particular, we draw on a systematic review conducted in Africa that highlights the dearth of information on community mobilisation within the region to reflect on how best to proceed within our intervention to maximise its chances of success.

Study limitations should be addressed.

- We have added a paragraph on study limitations within the Discussion (starting line 382)

References
Please expand reference list.

- Done

Minor Essential Revisions
Please carefully review the grammar and language throughout. SW and SWs are used interchangeably. SWs is preferable.

- Corrected

- All the recommended edits in Minor Revisions have been made.

Reviewer 2 report (Monica Biradavolu)

Major Compulsory Revisions:

The review of literature shows that the verdict is not yet final on whether it is possible, or effective, to implement community mobilization strategies for HIV prevention among female sex workers. While this is a good point, it should not be overstated. Some of the referenced literature where such strategies did not work is somewhat dated: from 1996 [Asthana and Oostvogels], 2000 [Campbell et al.] and 2001 [Wojcicki et al.]. At minimum, the paper should balance this out by more recent literature that deepens our understanding of how community mobilization strategies for sex workers reduce HIV risk factors: e.g., see Blankenship et al 2008, AIDS; Erausquin et al 2012, Journal of Epidemiology and Community Health; Erausquin et al 2014, AIDS & Behavior etc.

Following from above, I urge the authors to consider more fully the literature on community mobilization efforts for HIV prevention. Just as an example, from the work in India, both Sonagachi and the Avahan program are mentioned but in too cursory a manner. My interest in making this point is to ensure that there is greater cross-cutting understanding of the very many similar efforts underway across the world. Since Avahan explicitly used a community mobilization strategy across 116 NGOs in 83 districts in 6 highest-affected states in India, it is important to provide a more substantial review of the findings from that effort than is currently the case in the literature review. See Wheeler et al. in the Journal of Epidemiology and Community Health 2012 for a detailed description of the program, and the citations therein.

- We agree with both Reviewers 1 and 2 that our literature review was outdated and did not cover key references on community mobilisation in other sex worker projects, particularly Avahan. As a result we have re-written our Background section, updating it with key literature, including the recommended references by Argento et al (2011), Reza-Paul et al (2012), Dixon et al (2012), Erausquin et al (2012 & 2014) and Galavatti and Wheeler et al (2012) among other studies.
• We refer to these and other texts in our Discussion, again to better position our findings within the broader literature.

From lines 97-100: Either the sentence or the reference is not correct: “Similarly, early efforts to replicate Calcutta’s successful Sonagachi model in Mumbai…” – the reference cites the Asthana and Oostvogels paper but the data from the cited paper is not from an intervention in Mumbai, but from an intervention in Madras, and this intervention did not use the Sonagachi model.

• This reference has been removed in the process of updating our literature review.

Can the authors provide some information on the three sites? What type of settings are these – urban/rural, types of sex work conducted etc.? They are written up as though they are the same - is this the case? What are the differences, if any? If there is space constraint, perhaps this could be in the discussion section? If we are to pay attention to local context, this seems an important point to highlight.

• We have added a section at the start of Results to provide some details about each study and intervention site, as requested (starting line 171).

• We have also limited presentation of our quantitative findings to this section, to make clear that we are using the descriptive statistics as background contextual information, and focusing on the qualitative findings in the bulk of the paper.

Minor Essential Revisions

No mention of limitations

• Now added the end of Discussion, beginning line 382.

Reviewer 3 report: KariKalan N

Major compulsory Revisions in Abstract and Introduction part.

The abstract doesn’t give the objective of this research paper. Without specifying objectives, the conclusion part of abstract looks detached from the abstract for any understanding.

• A sentence on the aim of the paper has been added to the abstract (lines 35-37)

The introduction part provides examples south African studies, Sonagachi Calcutta intervention and Avahan Intervention for explaining the importance of competition among FSWs in recruiting clients. (Line 94-104) . This needs to be more detailed and have to the discuss those study findings in detail. It needs to be explained that how this present study gains insight from those past findings and how the present study remains novel when compared to them. Introduction needs to be more detailed with more references and a proper justification for the study objectives.

• We have completely re-written our Background section, updating and expanding our references and making specific links to learning from the Avahan programme. As also recommended by Reviewers 1 & 2, we have included key literature, including the recommended references by Argento et al

- The Discussion also makes reference to findings from other programmes.

Major compulsory Revisions for Method section

Why RDS was used for this study? It needs to justified. RDS is used for hard to reach and highly hidden population like MSM and IDUs. But since FSW are not a hidden population, the authors need to justify the use of RDS specifically here.

Authors have hardly given any background details about the RDS process which involves RDS centre, compensation, seed recruitment etc. This needs to be given.

- We have now made clearer that this paper is focused on our qualitative investigation of SWs social networks, and that the survey results are presented as background data on our three study sites, and to illustrate what data were available to us prior to conducting the in-depth interviews. Thus only basic information on the RDS survey is included, but we have increased the information provided about the method, see lines 137-150.

- We disagree, however, that sex workers are not a hidden population. While this is true in some settings (particularly where sex workers operate out of specific venues like brothels, clubs, etc), in the Zimbabwean context, many sex workers find clients by mobile phone, from home, or work from market stalls and are not easily identifiable, making RDS an appropriate method for sampling.

There must be a detailed description of the three study settings

- A new section on the study site characteristics has been added to the beginning of Results, lines 171-190. We’ve moved all the descriptive statistics into this section as well, to make clearer that they are being used as background to the qualitative findings and that the primary aim of the paper is not to present the RDS survey results.

RDS usually involves incentives for the participants. Does this study participants were given incentives? If so that needs to be explained.

- Remuneration was provided for both the RDS and the qualitative interviews and this information has now been added to Methods, lines 141 and 159 respectively.

Major compulsory Revisions in Results and analysis:

The authors have given the results of quantitative and qualitative component of the study in a intertwined manner. This makes it appear like a discussion section rather than a result section. The authors need to separately provide the results for quantitative and qualitative analysis under separate headings.

- We have done this. The quantitative component has been significantly reduced and have moved all descriptive statistics together into a new section that describes the study sites.

- We have also explained that the RDS survey is undergoing additional analysis that will be included in future papers, and inserted references to justify the use of RDS for our study populations.
The quantitative results are plainly compared with the qualitative results and merely justified that competition and cooperation mutually exists among FSWs. This is hugely insufficient for such conclusion.

- **We agree, and as a result no longer compare the quantitative results with qualitative data. Instead, the quantitative results are presented briefly at the start of Results in order to provide a backdrop to discussion of the qualitative findings.**

Overall the RDS analysis must have more explanatory variables and must be assessed of those findings. Results could also be compared for site wise differences and consistency of the findings.

- **Further quantitative analysis is underway and more comprehensive statistical findings will be presented in a subsequent paper. We have now focused this paper on the qualitative research.**

We wish to thank all three reviewers for their time in commenting on the draft manuscript, which we believe has strengthened the paper.

Sincerely,

Sibongile Mtetwa (on behalf of all authors)

Corresponding Author