Author’s response to reviews

Title: Community mobilisation to prevent violence against women and girls in eastern India through participatory learning and action with women’s groups facilitated by Accredited Social Health Activists: a before-and-after pilot study

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RESPONSE TO EDITORS AND REVIEWERS

Dear Henry and editorial team

We are very grateful to you and to the reviewers for constructive feedback on our study.
We have provided a point-by-point response below and have highlighted changes in revised article. Please let us know if you require any further clarification or changes.

With thanks and kind regards

Audrey Prost, on behalf of the study team

Reviewer 1: Effat Merghati Khoel

The authors have not described the intervention and the participatory-based actions drawn out of conducting the study. Any action should be considered as a finding in the participatory inquiries. The research might say this is not PAR but approach has been defined in this manuscript bring PAR procedure in mind, so the intervention needs to be clearly described in the method section.

Response: We thank the reviewer for this important suggestion. We have added details of problems and strategies prioritised by women’s groups during the Participatory Learning and Action cycle on p.8. We agree that facilitating a Participatory Learning and Action cycle could be seen as a form Participatory Action Research. As the reviewer will know, Participatory Action Research, Participatory Rural Appraisal and Participatory Learning and Action include a wide range of overlapping concerns and approaches, though PLA is often located – at least aspirationally - on the more ‘action-focused’ end of the PAR continuum. We think that debating fine differences between PAR/PRA/PLA is not necessarily helpful as they can overlap considerably in practice (Chambers 2002). The reviewer rightly reminded us that describing the intervention transparently may be more important than invoking nomenclature. Therefore, and if the reviewer agrees, we would like to retain PLA and not discuss the distinction(s) between PAR/PRA/PLA at great length.

Reviewer 2: Agumasie Semahegn

1. The design is not well reflected and justified. Why chose a single arm for piloting the work? A single arm design is relatively weaker than other two or more arm designs. How can readers understand the changes are due to something related with time and or some policy changes in the country or a mass media exposure? It would be nice if a control group was there.

Response: We completely agree with the reviewer that having a control group (and ideally using randomisation) would have enabled us to obtain a much better estimate of the intervention’s effects. Sadly, we were not able to use randomisation or have a control group, as this was a pilot intervention and our aim was to assess its feasibility and preliminary effects in preparation for a more rigorous, controlled evaluation. Such piloting work is common in the development of complex interventions, as described in the Medical Research Council (MRC) guidance (see feasibility/piloting phase). We have now clarified this on p.4 (Objective and study design) and added a reference to the MRC guidance.

2. Eligibility criteria has to be briefly and exhaustively listed out.

Response: Thank you. We have added details of the inclusion criteria on p.6, as follows: “All women who were registered with the 39 women’s groups active in the 22 intervention villages were eligible to participate in the study, and we attempted to interview all of them before and after the intervention.”
3. There is also a confusion on the intimate partner violence and other family violence. Were they counted twice or what?

Response: We have not used the term ‘intimate partner violence’ other than in reference to the National Family Health Survey. We have consistently specified whether we refer to violence from husbands or other family members or both across all tables. For example, Table 3 focuses on violence from husbands while Table 4 focuses on violence from other family members, i.e. not husbands. These were recorded as separate instances of violence. It is of course possible that women experienced violence from both husbands and other family members, and these were recorded separately in Tables 3 and 4.

4. Ethical approval: the approval number and agency name should be clearly indicated. Local committee is some house doubtful for readers.

Response: Thank you, we have included a specific date and reference number for the local ethics committee’s decision on p.13.

5. Method of analysis: logistic regression models with random effects, to do what? What is the effect size did you computed? Was that Odds ratio, risk ratio, prevalence ratio, prevalence odds ratio, incidence ratio? As far as the outcome variable prevalence greater than 10%, adjusted prevalence ratio is recommended using the generalized linear model (GLM). Otherwise, you will reach a wrong conclusion.

Response: We have clarified that we used logistic regression models to compute odds ratios, as all our outcomes were binary. We are not aware of a restriction on using random effects models for outcomes where the prevalence exceeds 10%. We acknowledge Odds Ratios [OR] and Relative Risks [RR] will not have similar values since some of our outcomes, e.g. non-acceptance of any form of violence, are common. We also appreciate that many researchers advocate presentation of the RR in preference to OR. We prefer to use ORs partly for the superior statistical properties of logistic regression but also because we feel that for common outcomes, the OR and RR each have pros and cons. The advantage of the OR is that it takes a large value for a change such as from 90 to 99%, which is after all a substantial change, whereas the RR would be a modest 1.1. If the reviewer has specific references or guidance relating to the advantages of Generalised Linear Models, we would of course be very happy to revise our analyses, otherwise we prefer to keep our current approach.

We had included ‘Odds ratio [OR]’ and then subsequently ‘OR’ in all tables in the submitted article, but as the abbreviation seems to have caused some confusion we have now included ‘Odds Ratio’ in full in Tables 2, 3, 4 and 5.

6. Method of analysis: is it per protocol or intention to treat analysis? Not clear?

Response: we have followed an intention to treat principle, i.e. included all women surveyed whether they participated in the intervention or not. We have now clarified this in the methods section on p.7. However, we prefer not to use the phrase ‘intention to treat’ in the article as it is really only appropriate for RCTs, not pilot studies.

7. Results: Tables: UNADJUSTED or ADJUSTED 95%CI: unadjusted of what? Effect size measure is not specified?

All adjustments in Tables 2, 3, 4 and 5 were specified in notes below the tables, as highlighted in the
revised article.

8. Tables: should be designed a scientific format. Lines should be hidden and smartly designed to attract readers and illustrate the findings clearly.

Response: Thank you, we have edited the tables according to your suggestions, though we feel that deleting more lines is unlikely to improve attractiveness and readability.

9. Which guide is followed to write this manuscript? CONSORT or STaRI?

Response: We have not used CONSORT as this study is not a randomised controlled trial. We are not aware of STaRI, unless the reviewer was referring to STARD for diagnostic studies. Our pilot study does not fit into the categories covered by the EQUATOR network guidelines: it is not a randomised trial (CONSORT) or an observational study (STROBE), or a systematic review (PRISMA), study protocol (SPIRIT), diagnostic study (STARD), case report (CARE), clinical guideline (AGREE), qualitative study (SRQR). We have reported the methods as transparently as possible and provided extensive discussion of the limitations to enable readers to judge the quality of the study.