Author's response to reviews

Title: Evaluation of Stepping Stones as a tool for changing knowledge, attitudes and behaviors associated with gender, relationships and HIV risk in Karnataka, India

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Author's response to reviews:

Reviewer's report (1)

Title: Evaluation of Stepping Stones as a tool for changing knowledge, attitudes and behaviors associated with gender, relationships and HIV risk in Karnataka, India

Version: 3 Date: 16 December 2010

Reviewer: Nthabiseng A Phaladze

Reviewer's report:

1. Is the question posed by the authors well defined?
The description of Stepping Stones (SS) training package is well described. However, the purpose of the evaluation in the 40 villages does not come out clearly. It would have been helpful to have the aims of this project stand out e.g. have bullet points objectives followed by the hypotheses. The hypotheses should clearly spell out the endpoints/outcomes.

RESPONSE – this has been fully addressed.

2. Are the methods appropriate and well described?
The methods section needs to be organized differently e.g. have headings such as: study design; sampling technique; data collection methods etc.

As it is it makes reading a bit difficult. Data analysis methods for both qualitative and quantitative data should be specified. This is important for readers in assessing how authors came to conclusions for instance. We need to know how the sampling of the villages was done? what was the eligibility criteria?

RESPONSE - this has been fully addressed. The sampling has been described for each survey method.
3. Are the data sound?
The data is relatively sound

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
Yes.

5. Are the discussion and conclusions well balanced and adequately supported by the data? The discussion section only has one reference which is highly inadequate. The findings need to be supported by the literature. The authors need to revisit this section. There is no conclusion following the discussion. This needs to be addressed.

RESPONSE – the discussion section has been revised, and more references included, as well as a conclusion.

6. Are limitations of the work clearly stated?
Yes.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
Yes, they did acknowledge previous evaluations of Stepping Stones.

8. Do the title and abstract accurately convey what has been found?
9. Is the writing acceptable?
Yes.

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.

Declaration of competing interests:
I declare that I do not have competing interests

Reviewer's report (2)

Title: Evaluation of Stepping Stones as a tool for changing knowledge, attitudes and behaviors associated with gender, relationships and HIV risk in Karnataka, India

Version: 3 Date: 17 November 2010
Reviewer: Rachel Jewkes

Reviewer's report:

Thank you for inviting me to review this paper which presents a mixed methods evaluation of the HIV prevention intervention Stepping Stones. Overall the article is of interest as Stepping Stones (SS) is very widely used globally and it is essential that there is a better understanding of its effectiveness and impact in different contexts.

The following are major compulsory revisions:

p. 2 The diffusion of ideas from SS is an important theme of the paper. It would be useful to know what proportion of participants in the workshops were chosen because of their potential as change agents and what proportion were see as ‘high risk’. Readers need to be able to make an assessment of how realistic it was to expect broader change to flow from ~34 people per village being recruited into SS, especially given that ‘high risk’ people in most other settings are usually in other ways very vulnerable and may be particularly poorly suited for becoming change agents.

RESPONSE - We agree that the expectations of KHPT for SS need to be spelled out – we have made changes.

A further point that is missed in this paper and should be incorporated is that the model which is intended to enable social norm change in communities is one where there are multiple (at least 4) Stepping Stones groups conducted with different ages/genders within on community. This is apparently not the model used by KHPT given that only 34 people on average per community was trained. This is an important limitation for any reasonable expectation that SS would have a broader community impact beyond the immediate participants.

RESPONSE – there were separate groups (trained separately but also they had group meetings about what to do in the community) as described above and there was an expectation that there would be community impact – we feel that in fact this WAS an SS expectation in Karnataka. Dr Jewkes' work limits itself to change in the trained group, but this was not the case in India where the implementers expected 34 people to make a change in their villages. The median village size was 2100 people – or approximately 1000 sexually active adults. KHPT definitely planned that the trainees would be able to affect change in this population. This has been clarified.

Furthermore, the authors must state the average size of SS village – it is important to get a sense of how reasonable diffusion through a village is from the ratio of SS participants to village population.

RESPONSE – We agree this needs showing as readers may over-estimate the size of the villages. The median size was 2100 (see page 2. KHPT thought training approximately 30-40 people would be enough to make changes at the
community level.

What is known about the % of sessions of SS attended by participants? Given that only 61% of SS trainees say they saw a condom demonstration it begs the question as to whether condoms were indeed demonstrated in SS sessions, or whether levels of attendance were less than 100%. This is important for understanding the results.

RESPONSE – As we conducted the research 1-3 years after the training, we do not know whether in fact the trainers covered this topic adequately, even though it is in the curriculum. The attendance records are also not available so we do not know what % of sessions were attended. This is a limitation we have mentioned in the limitations section.

p.2 – at the end of the page the reference to the RCT of SS has not mentioned that there was a significant reduction in incident herpes infections in female trial participants (not just male).

RESPONSE – Corrected

p.3 – there is a discussion of diffusion of SS training effects in the recently published paper with qualitative findings from the South African trial

RESPONSE – we have added a reference to the Gambia paper. Thank you for noting the recently published South African paper, ref to which is now included.

p.4 – information about the characteristics of the qualitative research participants should be provided

RESPONSE – This has all been included although it adds to the word count

p.4 – how was the sample selected for the PBS surveys? “using random cluster sampling” is not sufficient as a description. What was the response rate for the PBS survey? For the Stepping Stones sample? Is there information on how responders differed from non-responders?

RESPONSE – the methodology has been explained more. The response rate, i.e. the number of those invited who came to the sessions, has been added to the text. Of course there is bias in the fact that some people are not available when we do the survey and therefore not invited, but this is unavoidable. We do not have data on who did and did not respond as personal details are not collected in a PBS, rather they are rapid assessments with pooled data. The SS PBS sample was convenience sampling so we did not monitor who was not available. We agree this is a limitation, and have noted as such.

How were the interviews conducted? Self-completion? Interviewers?

RESPONSE – Trained interviewers...added to the text.

p.5 what was the origin of questions in the PBS – some look like GEM scale questions – were they? If so it must be acknowledged. Were they validated for
use in this area? What language was used?
RESPONSE – We have included this reference and language. The questions were not “validated” as such but were questions commonly used in PBS in Karnataka.

p.5 the data analysis is very poorly described and doesn’t seem to have been appropriately conducted. The sample was clustered and this needs to be taken into account in the data analysis – this cannot be done in Excel. The data from the SS group should not have been adjusted. If the differences in socio-demographic characteristics of the sample are considered to have influenced results this must be taken into account by adjusting for age, marriage and sex variables in a multi-variable regression analysis.

RESPONSE – In order to assign cluster weights to the data, we need to know the sampling probabilities. This is based on the size of the segments in which the clusters were situated. The nature of the PBS is that it involves rapid selection and no data are collected on the total size of the target population in those segments. However we do have response rates associated with those contacted, and these have been added to the text. We could not carry out multi-variable regression analysis, since the PBS gives us group-level and not individual-level data, making the number of observations small. Moreover, we do not have the socio-demographic characteristics of each individual participant, but only the characteristics of the groups - in terms of male/female, married/unmarried, and young/older.

We adjusted the SS group mainly because this was very much different from the other groups studied. Since multi-variable standardization was not possible, we used direct standardization method to adjust for the different group profiles.

So, most of our analysis limitations arise out of the limitations of the PBS as a method...we have changed the limitations section.

Table 1 – the differences between the three samples should be tested for statistical significance, and only significant differences commented on in para 1 of the results (p.8). Throughout all tables exact p values should be presented. Where the attitudes questions are from scales (or where they form scales) then an overall score should be calculated for the 3 groups and the scores presented and compared. Where questions were not answered by all PBS participants, the N actually asked and the n responding should be given for the item.

RESPONSE –
The differences and p values have been included. The attitude questions in PBS are not based on any scale and hence we cannot get the overall score as suggested by the reviewer. In a PBS, respondents must answer all questions – they have to place a token in a box. This has been clarified in the text. There are
2 cases only where the respondents left before the attitude questions were asked, and so the n is lower (412 vs. 414).

Table 4 – the reference to anal sex – was that with a man or a woman partner?

Discussion

In many settings women are so unused to being asked their opinions that they are considered likely to state the social norm as the answer to a gender attitude question even if their own views somewhat differ. I have had recent conversations about this with researchers in Bangladesh and wonder if the problem was considered in this study and whether it may have influenced the results (and their interpretability).

RESPONSE – We agree this is a problem and have noted this in the study limitations.

The data analysis needs to be redone using the most appropriate methods and this may influence results. However, on the face of the current results, which are quite good, it’s unclear to me why paragraph two of the discussion focused on what was not achieved and sustained 3 years after Stepping Stones. What evidence do the authors have that the explanation for not getting overwhelming and sustained change was insufficient factual information? I was under the impression the strength of SS was argued to be its participatory methods and this was perhaps why it worked better than factual information giving based interventions.

RESPONSE – We think we have used the most appropriate statistical tests for this kind of data and so the results have not changed. We agree that the discussion is rather too negative and that the sustained changes are in fact quite impressive – have changed the discussion to reflect this. However, the issue of factual information remains – having observed training, we think that participants get more out of the process and the conceptual issues than factual information.

Paragraph 3 of the discussion is completely speculative – what evidence do they have that 40 people would be sufficient to galvanise change in a community – who would they have to be? How education? Empowered? Influential? How small a village? I think the authors should focus on the facts which appear to be that there was some dissemination to the social network (shown in the qual research). It’s impossible to know whether the community differences examined are due to SS or due to other factors (problem of attribution – must be discussed). If interventions are intended to be community-transformative they need to be set up in a realistic way and research is needed (or references need to be cited) to indicate what that actually constitutes.

RESPONSE – KHPT’s intention was that the SS trainees would indeed spread the SS messages to a wider community. The average village size was quite small with adult populations less than 1000 people, so it does seem feasible that
a group of 20-30 people could have some impact by joining the village health committee and holding events and talking to people one on one. The problem was more that the training did not include enough information on exactly how they might do this. As a result of the study, the manual has been changed to include more sessions on how to be a change agent. I have tried to strengthen the discussion to reflect this. The issue of attribution has been included in the limitations section.

The concluding paragraph of the paper does not sit well with the rest of the discussion. I would suggest that the discussion be extensively revised. It needs to relate back to the hypotheses addressed in designing the study and also to the findings of research on Stepping Stones from other settings – the Gambia evaluation has been published and should be discussed here too.

RESPONSE – OK, changes have been made to reflect this.

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:
I declare that I distribute the South African version of Stepping Stones from my office but it is on a not for profit basis and charge only for bulk orders. I have no competing interests.