Dear Editor,

Re: “Relationship between exposure to the Avahan intervention and levels of reported condom use among men who have sex with men in southern India.”

We would like to thank you for your consideration of our manuscript, and for the opportunity to submit a revised version. The reviewers raised some helpful points of clarification and discussion, which we have responded to, as detailed point-by-point below. We would like to take this opportunity to thank the reviewers for their comments.

We hope that you will find the revised version suitable for publication in BMC Public Health.

Yours sincerely,

Kate Mitchell

Responses to reviewers’ comments (page numbers refer to the revised manuscript)

Reviewer 1: Bea Vuylsteke
1. p5, exposure variable. The authors describe various exposure variables: contact with project, reception of condoms, assistance to condom demonstration, duration since first contact with project, number of contacts during last month, number of condoms received at last contact, number of condom demonstrations seen. These variables are not independent from each other, in fact they are probably overlapping and intertwined to a great extent. Examples: Receiving condoms by the project includes a contact, condom demonstration goes hand in hand with condom distribution, etc…

Is there any reason to keep the analysis of the different exposure variables separate? Was it not possible to create a general exposure variable taking into account all these variables, eg in a “exposure score”?

P. 19-20, table 1 and table 2 See also previous comments. Would it be possible to examine a “summary exposure variable”

Response: The different binary exposure variables are certainly not independent from each other, and we have now expanded our description of how much they overlap in the section ‘Intervention exposure’, adding the following text: (p8 lines 170-174) “There was considerable overlap between these exposures; all of those who had received condoms or seen a condom demonstration had also been contacted by the intervention, and 55% of MSM reported all three of these exposures (they had been contacted, received condoms and seen a condom demonstration). Almost all (98%) of those who had seen a condom demonstration also reporting receiving condoms.”

There was no overlap between the binary and their related continuous exposure variables, as the continuous variables excluded those who reported not experiencing that type of contact. Less overlap was seen between the continuous exposure variables (only duration since initial contact and frequency of contacts were related), as already described on p8 (lines 174-179).

One of the reasons we undertook this analysis was to identify the components of contact with the intervention which were associated with an increase in condom use, as a previous study amongst female sex workers had shown that contacts involving condom demonstration were more strongly associated with condom use than contacts per se. We also wanted to explore the importance of receiving condoms, which was not assessed in the FSW study. For this reason, we have not considered a general exposure score, but have looked at each measure of intervention exposure separately. We have expanded upon this motivation in the introduction section: (p4 lines 60-64) “Since the previous study amongst FSWs suggested that different exposure measures, reflecting different aspects of the intervention, may vary in their relationship with condom use [15], we looked at a number of different measures of intervention exposure to identify the particular components of contact with the Avahan intervention which were associated with increased self-reported condom use by MSM.”

2. Line 151: “positive correlation between duration since first contact with
Sangama and number of contacts in the last months” This is a potential interesting finding which may need some more discussion. Do the authors have any hypothesis why the older contacts would also be the most active participants of the project?

Response: We checked whether this association was also seen over the period (the previous 11 months) for which Sangama had been offering Avahan services, and found that it it was. We have added this result on p 8, lines 176-178: “this association persisted when only those who had first had contact with Sangama within the previous 11 months (since Sangama began offering Avahan services) were considered ($r = 0.211$, $p = 0.03$, $n = 106$).”

We have added a hypothesis for this association in the discussion section: (p12, lines 271-275): “Although neither factor was associated with condom use, we found that MSM with longer exposure to Sangama also reported more frequent contacts, including over the eleven months for which Sangama had delivered Avahan services. One plausible hypothesis for this association is that MSM who contacted the intervention earlier on were more pro-active and more likely to contact the intervention repeatedly.”

3. P. 8. Associations with socio-demographic and behavioural variables. Because of the separation of the different exposure variable, this paragraph is a bit confusing. See also previous comments. I guess the main question should be what is the profile of MSM who are reached by the project? Or: what is the profile of those MSM who are most exposed to the project?

Response: We have rephrased the first part of the paragraph, relating to factors associated with binary exposure variables, to describe the profile of MSM who are reached by the project, as suggested. We have maintained the separation of different exposure variables, as discussed above. The paragraph now reads: (p9 lines 181-186) “Men who had ever been contacted by Avahan were more likely to have ever sold sex than those who had never been contacted (odds ratio (OR) = 2.17, $p = 0.010$). MSM who had ever (versus never) received condoms from Avahan were more likely to have ever sold sex (OR = 2.23, $p = 0.006$) and less likely to have ever had sex with a female sex worker (OR = 0.51, $p = 0.083$). Men who had ever (versus never) seen a condom demonstration were also more likely to have ever sold sex (OR = 2.26, $p = 0.005$), and less likely to have ever had sex with a female sex worker (OR = 0.50, $p = 0.038$).”

4. P.12 Limitations, line 269. The social desirability bias is the main limitation of the study, potentially resulting in a huge overestimation of the true impact of the intervention. This should be extensively discussed here.

Response: We have added further discussion of this: (p14 lines 322-325) “Social desirability bias is likely to have led to an overestimate of the true impact of the intervention upon condom use in this study, since those exposed to the intervention should have heard more messages promoting condom use than those not exposed, and so may feel a greater pressure to report high condom use.”
5. P.12, before conclusions. The paper describes mainly program evaluation results and its public health importance should be more extensively discussed. Could the authors add some recommendations to the Avahan programme?

Response: We have added a paragraph to the discussion which discusses the public health importance of the results, in relation to HIV transmission (p14, lines 313-318): “The high levels of condom use among MSM contacted by the Avahan intervention, coupled with the high levels of contact with the intervention reported, suggest that the Avahan intervention could have a large impact upon HIV transmission amongst MSM in Bangalore. No significant change in HIV prevalence was found amongst MSM in Bangalore between two sero-prevalence surveys carried out in 2006 and 2009 [34], but this does not necessarily mean that Avahan had no impact, as it is possible that HIV prevalence could have risen in the absence of the Avahan program.”

We have added a recommendations section before the conclusions (p15, lines 339-342): “In light of our findings, we recommend that the Avahan program, and other programs following this model, focus upon providing high-quality contacts with MSM, which wherever possible should include demonstrations of correct condom use and distribution of condoms, as well as communication about risk reduction.”

Minor essential revisions:

1. P. 6, line 105: “double deckers”. This term could be stigmatising. Is this an official name/category or translated from a local language? Is this term accepted by the community as name?

Response: Double deckers is a term which is widely used by members of the community, (including for self-identification), as well as in the sociological literature (e.g. Phillips et al 2008); we do not believe that it carries stigmatizing associations in this context.

2. P.9, line 205: who had ever been married Please specify married “to a woman” or “to a man”

Response: We have added “to a woman” as requested, both here and throughout the manuscript.

3. P. 19-20, table 1 and table 2 Please include the number of MSM in each category (with all partners, with main partners, with casual partners)

Response: We have included the number of MSM answering questions about condom use with each partner type in tables 1 and 2, and added a note below the table to explain that numbers are smaller in models for continuous exposure variables, where those never reporting the exposure are excluded.
Reviewer 2: Denton Callander

Minor Essential Revisions:

Language and writing

1) p 6, line 127 suggests significance of p<0.1

Response: We follow the fairly common practise of allowing a higher p-value cutoff for inclusion in the multivariate models. We have reworded this sentence to try and avoid the suggestion of significance at this level: (p7 lines 141-143) “Socio-demographic or behavioural variables were considered for inclusion in the multivariable models if the p-value for their association with either the exposure or the outcome variable was less than 0.1.”

We have also added a line at the end of the methods to make it clear that we consider a p-value of <0.05 to be statistically significant. (p7 line 152) “Results were considered to be statistically significant if p<0.05.”

2) p 10, line 206 suggests a significant relationship in spite of a p>0.05. The value is marginal so if the authors are going to make that leap then they need to be explicit

Response: At the end of the methods, we have now made it clear that we report results with p<0.1 where these will be considered for inclusion into the multivariable models: (p7 lines 152-154) “All associations of the main exposure and outcome variables with socio-demographic and behavioural variables with a p-value <0.1 are reported, as these variables were considered for inclusion in multivariate models.”

3) Most of the time exact p values are given but in a few cases the authors provide ranges (e.g., p 10, line 207; p 10 line 10). Just a small thing but somewhat distracting.

Response: We have now given separate p-values wherever p-values are reported (page 10 and elsewhere), apart from the abstract where it is not practical to give p-values for all of the analyses.

4) If a sentence starts with a number then the number must be spelt out. e.g., "Forty one per cent reported sex work..." vs "41% reported sex work..." (e.g., p 7 lines 142 and 143)

Response: This has been changed throughout the manuscript.

Information and detail

5) p 5, line 78; the authors indicate that only MSM with a new/unknown partner in the past week were asked about condom use with casual partners. This cut-off seems a bit short and I wonder if the authors could add a sentence about why
they chose to exclude men who may have casual partners but not within the week before the survey.

Response: This analysis was restricted by what was asked in the survey, and questions about causal partners were only asked to men who reported a new/unknown partner in the past week.

This has been clarified in the text: (p5 line 91-93) “Only MSM who reported having sex with new or unknown partners in the past week were asked about condom use with casual partners in the survey.”

6) The proportion of recruited MSM who reported sex work as primary income seems very high (41%). It would help provide some context for readers if the authors would add information about sex work in India, possibly by citing representative data around the proportion of men who report sex work.

Response: We have now reported figures on this from the MSM surveys mentioned in the introduction, and added the following sentence: (p3 lines 34-39) “Surveys of MSM across southern India have suggested high levels of engagement in commercial sex, [7-9], with 40-68% of MSM in different surveys reporting ever receiving payment for sex [7, 9]. Much smaller numbers of MSM (3-7%) report sex work as their main source of income, excepting Karnataka state where 57% of MSM surveyed reported sex work as their main income source [7, 10]. It should be noted that these surveys tend to capture higher-risk MSM, and so they are likely to overestimate the true proportion of MSM who sell sex [11].”

7) A finding of the multivariate analysis was that a larger number of condoms provided seemed to predict less consistent condom use among men and their main partner, albeit with a small AOR. Aside from describing this finding, the authors never return to it in the discussion. It would be interesting for the discussion to include a sentence or two with possible explanations for this finding or at least some recognition that it runs counter to what one would expect.

Response: We have not thought of a plausible explanation for this finding, but have now highlighted it in the discussion: (p13 lines 291-295) “Counter-intuitively, we found a slight decrease in reported condom use (both CCU and condom use at last sex act) with main partners with increasing number of condoms received. While this is of concern, the effect is relatively small (AOR 0.99 per additional condom), and likely to be outweighed by the positive impact on condom use with casual partners, who are more frequently reported by MSM in this sample.”

Reviewer 3: Heidi Reynolds

Minor essential revisions

1. the data are 8 years old at this point. The authors should provide justification why they are still relevant.

Response: Although later data is available for this setting, the earlier data
contains a larger proportion of MSM unexposed to the intervention and so has
greater ability to detect the effect of intervention exposure on condom use. We
have now added a paragraph about this in the discussion section: (p13 lines
305-311) “The data used in this study were collected eight years ago, and while
this means that they may not reflect current behaviour (which may be affected
by, for example, increased access to ART), they were collected at a time
sufficiently soon after the beginning of the Avahan intervention to have a large
enough population still unexposed to the intervention allowing for comparisons to
be made between exposed and unexposed interventions. In more recent surveys
amongst this population [34], the proportion of MSM never contacted by the
intervention was greatly reduced, limiting the usefulness of such comparisons.”

2. The authors should also add a limitation in the discussion about possible
selection effect by the intervention. In other words, it’s possible that the same
factors that resulted in exposure to the program are the same as those that result
in higher condom use.

Response: We have added the following to the discussion, in the limitations
section: (p14 lines 328-332) “There may have been a selection effect by the
intervention, i.e. those reached by Avahan may have been more likely to use
condoms anyway. We attempted to control for this by including in our multivariate
models measured factors associated with exposure to the intervention, but there
may have been other unmeasured factors which could account for both a higher
likelihood of intervention exposure and higher condom use.”

Discretionary Revisions

3. Provide a bit more information about the Sangama program in the intro. It is
not until the discussion that the points about Sangama role prior to Avahan and
the time since becoming involved in HIV prevention are presented. These are
important points for the reader to consider while assessing the methods and
results.

Response: We have now included the following information about Sangama in
the introduction section: (p4 lines 55-58) “In Bangalore, Avahan services were
delivered to MSM by a local non-governmental organisation (NGO), Sangama.
Sangama had been working with local MSM for some years previously,
promoting sexual minority rights, and had been delivering Avahan HIV-prevention
services for about eleven months prior to the survey used here.” We have
removed some of this information about Sangama from the methods and
discussion to avoid duplication.

Other changes made

1. Reference 18 has been updated to cite a published paper (Panovska-Griffiths
et al 2014, PLoS One) rather than an earlier conference presentation on the
same work