Author's response to reviews

Title: A cross-sectional study of knowledge of sex partner serostatus among high-risk Peruvian men who have sex with men and transgender women: implications for HIV prevention

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Author's response to reviews: see over
Dear Members of the Editorial Board:

Thank you for your thoughtful and thorough comments on the following article:

A cross-sectional study of knowledge of partner serostatus among Peruvian men who have sex with men and transgender women: implications for HIV prevention

Per the editor’s requests, I have made the following changes to the manuscript:
• The article length has been reduced from 3,296 words to 2,414 words (from Background through Conclusions).
• The article has been edited by two native English speakers with scientific expertise, and I am also a native English speaker.
• I have added sections on Competing Interests (none exist) and Acknowledgements.
• Tables now conform to the journal’s formatting guidelines (vertical lines have been removed).

Please find my revisions and explanations for the reviewers’ comments detailed below:

David Dolling

• The [high-risk] behavior which led to recruitment should be described in a supplement.
  I agree this is important and I believe the enrollment criteria under Methods, Participant Selection, 2nd paragraph describe the enrollment criteria which select for high-risk persons. I would like to clarify if you were requesting more information about the type of high-risk behavior which led to recruitment, or requesting that the enrollment criteria be specified in a supplement instead of the article?

• The article should be explicit in that it only describes high-risk Peruvian MSM and TW in the title and elsewhere.
  Excellent point, thank you. The study population is now described as high-risk in the title and throughout the article where appropriate. Also, under Discussion, 5th paragraph, we have written that “MSM at higher risk for HIV and/or STI acquisition may have been more willing to participate or selected for in this survey based on eligibility criteria,” and comment that this is not necessarily representative of Peruvian MSM and TW as a whole.

• It seems it would be useful to compare knowledge (positive) and knowledge (negative) to unknown separately since one would expect these to have different directional impacts on likelihood of UAI which could be hidden when combined.
  This is a good point. The results of the analysis comparing the likelihood of UAI in HIV positive and HIV negative participants to those with unknown serostatus are now presented. Interestingly, they have similar directional impacts (both are associated with a lower likelihood of UAI, and as one might expect HIV positive serostatus has a stronger association) but these results were not statistically significant.

• 1347 partnerships with unknown HIV status and 230 with knowledge (page 9). Yet 1643 in total, is the rest missing data?
Thank you for identifying this mistake. That number was from a previous analysis where I had incorrectly excluded a number of partnerships, but has now been corrected – there were 1413 partnerships where the partner’s HIV serostatus was unknown.

- **In Discussion** Ref 30 compared to paper however different recruitment has led to very different populations. Ref 30 did not restrict participants to be at high risk.
  You are correct. This reference to a non-high-risk MSM population has been removed.

- **Would it be possible to look at strategic positioning evidence where partners were known to be HIV positive? It seems like there exists active/passive data which could be explored.**
  This is an interesting topic to bring up, however strategic positioning data was not analyzed because the low number HIV-positive sex partners (13) would not provide an adequate sample size for this analysis, and also it was not the main focus of our paper.

- **Not clear what the bivariate analysis used as the dependent variables - the last two sexual encounters?**
  I appreciate your comment here, I have tried to clarify the dependent variable under Methods, Analysis. In both bivariate and multivariate analyses, the dependent variable was knowledge of sex partner serostatus in each of the participant’s last 3 partnerships.

- **The 89.9% of participants reported alcohol use is repeated on pages 8 and 9.**
  Thank you for catching this mistake – this result is now reported only in the table.

Hanne Thiede

- **The author should explain and justify in the Methods section why they chose to combine [MSM and TW] for analysis.**
  I appreciate your comments on this matter – we were also interested in analyzing these two populations separately, but unfortunately the small sample size of TW in this study – roughly 100 persons – would be inadequate to achieve statistical significance. This is now clarified under Methods, Analysis, 2nd paragraph.

- **Please add more information about recruitment including venue selection and eligibility, whether potential participants were approached and recruited in a systematic manner, and whether they received an incentive.**
  Thank you for identifying gaps in our description of the surveillance protocol. We now describe convenience sampling and snowball sampling for recruitment, venue selection as “previously mapped,” and the reimbursement given to participants under Methods, Participant Selection, 1st paragraph. Eligibility criteria are described in the following paragraph.

- **The authors need to state specifically and clearly what associations they assessed. Results of multivariate analyses should be presented in the same order as described in the Abstract, Background and Methods.**
  We appreciate your comments here and have tried to clarify the aims of our study. The associations being studied – i.e. factors associated with knowledge of sex partner serostatus, the association of unprotected anal intercourse with knowledge of sex partner serostatus, and the association of unprotected anal intercourse with sex partner serostatus – are now clearly and consistently stated and presented in the same order throughout the article.
• Please clarify the reference period for the sexual behavior variables. As written it sounds like it is the last month.
   This is an good point since the reference periods do differ for each of the sexual behavior variables. They are now clarified under Methods, Data Collection, 1st paragraph.

• Please explain whether or not the survey was anonymous.
   The survey is now stated to be anonymous under Methods, Data Collection, 1st paragraph.

• Analyses were conducted at both the individual and partnership levels, not just the partnership level.
   You are right in that we did conduct an individual level analyses for our descriptive statistics, however our bivariate and multivariate analyses were conducted at the partnership level because the outcome or dependent variable was a partnership level variable. I tried to make this more clear under Methods, Analysis, 1st paragraph.

• Please explain how predictor variables were selected and why the models were not controlled for city. Please provide more information on how the logistic regression models were constructed and why variables that were not significantly associated with the outcome in bivariate analysis were included in the final models.
   Thank you for seeking more detail about our methods. We now offer the following explanation of our analysis under Methods, Analysis, 2nd paragraph: “We developed a conceptual framework for our multivariate model prior to conducting the analysis. An extensive review of the literature and available data from the survey database guided our choice of predictor variables for our conceptual framework. As such, the final multivariate model included some variables which were not significantly associated with the primary outcome in bivariate analyses.”
   Many variables, such as participating city, although important and interesting, were not included in the final logistic regression because doing so would potentially destabilize the model. Although we did analyze data according to city, we did not present these results in full nor did we include the participating city in our logistic regression model as it was thought to be of greater internal use and of lesser interest to the broader international scientific community. To be sure, we checked that our primary outcome – knowledge of partner serostatus – did not vary with statistical significance between cities.

• It would be easier to understand if the descriptive analyses were presented separately from the multivariate analyses and the multivariate analysis were presented under descriptive headings and in the same order they were mentioned elsewhere in the manuscript.
   Excellent suggestion. Descriptive analyses are now presented separately from multivariate analyses, and multivariate analyses are presented under descriptive headings.

• It is not clear why receptive anal intercourse is presented here and why a 5 year referent period was used when much more recent referent periods were used for other sexual behaviors?
   I appreciate your comment noting the variable time periods. I believe the 5 year reference period was used to for this sexual behavior since an “all-or-none” response was being assessed, which may be easier to recall than a frequency of that behavior or behavior with a specific partner. I have decided to remove that result since it was not a major finding in our study.

• Table 1: Number of male sex partners: Change >10 to 10 or more or #10; Expand table titles to be more descriptive; Use consistency in description of variables and variable categories; Include a definition of Secondary education and Superior/Technical in a footnote; It seems redundant to list all the variables in the model in the footnote?
Excellent suggestions. We changed the labeling for male sex partners as suggested, descriptions of variables are now consistent from table to table, a footnote description of secondary and technical education in Peru has been added to Table 1, and the footnote describing variables in the multivariate models has been removed.

- **The authors need to discuss representativeness of their sample considering the recruitment method the limited geographic areas, and that the participants were selected based on their high-risk status.**
  Thank you for this suggestion. We have added the following discussion of this issue: “This study recruited a convenience-based sample of high-risk MSM in three cities and may not represent the Peruvian MSM and TW population as a whole. Although large samples representing a diverse range of MSM subcultures were included, men who participated in this study consisted mainly of those who visited socialization venues where study recruitment was conducted and who voluntarily accepted participation. MSM at higher risk for HIV and/or STI acquisition may have been more willing to participate or selected for in this survey based on eligibility criteria.”

- **The authors should also discuss the implications of participants with previously diagnosed HIV infection self-reporting as HIV negative or as HIV unknown status.**
  Thank you for this suggestion. We have added the following discussion to the first paragraph of the section: “The implication of HIV-positive persons reporting their HIV status to their sex partners as negative or unknown, whether intentional or not, is that the ability of sex partners to effectively utilize sero-adaptive and self-protective behaviors may be diminished.”

- **Conclusions last sentence: The last part of the sentence is not clear. Do the authors mean ‘... HIV treatment resulting from identification of previously undiagnosed cases via partner notification?**
  We would agree that this last sentence is not clear, and in the interest of being concise and focused, it has been removed.

I look forward to your comments on this latest version of the manuscript. Thank you very much for your time and consideration.

Sincerely,

Sharita Nagaraj, MD