Author's response to reviews

Title: HIV Risk and Associations of HIV Infection among men who have sex with men in Peri-Urban Cape Town, South Africa

Authors:

Stefan Baral (sbaral@jhsph.edu)
Earl Burrell (earl.burrell@ucla.edu)
Andrew Scheibe (andrew.scheibe@hiv-research.org.za)
Ben Brown (ben.brown@hiv-research.org.za)
Chris Beyrer (cbeyrer@jhsph.edu)
Linda-Gail Bekker (Linda-gail.bekker@hiv-research.org.za)

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Author's response to reviews: see over
Thank you for these constructive comments. I will embed responses.

**Reviewer's report**
**Title:** HIV Risk and Associations of HIV Infection among men who have sex with men in Peri-Urban Cape Town, South Africa
**Version:** 2 **Date:** 14 April 2011
**Reviewer:** Rachel Jewkes

**Reviewer's report:**

Review of submission to BMC on MSM and HIV in Cape Town
Thank you very much for asking me to review again this paper. A number of suggested improvements have been made by the authors need to make further changes before this could be published. This paper really would benefit from an epidemiologist as part of the drafting team. It has a myriad of little errors and some rather large ones that would have been corrected by an epidemiologist. I strongly suggest that the authors get the next draft of the paper checked.

** There are two faculty member epidemiologists on this team from the JHSPH. We feel sufficiently covered with the epidemiology presented within this paper given that it was also reviewed by Susan Buchbinder. To alleviate your concerns, we have further conferred on some of your specific points below with members of the STATEPI team that manages the MACS Cohort as well as Ron Brookmeyer who is a well-established HIV bio-statistician.

Abstract: the results presented here are incorrect and there are a number of words missing.

** Thanks and addressed with adding the word “the” to the second sentence.

I would suggest including the statistic of 0% consistent condom and water based lubricant use in the abstract

** Thanks for this, but given the limited space available for the abstract we are unable to include though it is prominently discussed in the paper.

Conclusions: I would suggest that these be redrafted to fit the study findings. The last sentence is very general and does not speak to the specific findings of this paper. I don’t believe the authors can say that HIV prevalence will continue to rise among MSM –it seem likely given the level of risk taking – perhaps the sentence could be tempered

** Thanks for this, but given the lack of targeted services available in the majority of country combined with a limited scale of service provision in places where services are available leads us to believe that incidence will be sustained and prevalence will increase.
Introduction: Changes are needed to the second paragraph
Line 9 – please revise this sentence to read “among these men, HIV positivity
was associated with men reporting same sex practices aOR 3.6 etc…”

**Thanks for this and has been grammatically corrected.
Line 13 – what is meant by ‘adjusted HIV prevalence’ – adjusted for what?

**IT refers to this being an RDS-weighted assessment, but have now made this more clear.

Line 14 – please rephrase this in the same style suggested for line 9
Line 18 – delete ‘overall’
Line 21 – change ‘significantly predictive’ to ‘associated with’

**Have addressed these issues.

First sentence of para 4 – this isn’t very scientific – what is ‘multiple times
higher’? – there is a bit of mixing of citations of prevalences from different age
groups etc – the <10% is for the general population over age 2 and its not
comparable with any of the prevalences cited that are not from this age group
I think this introduction shows the problem with the notion of ‘prevalence’ when
used in this type of research. There isn’t one study cited where a true population
prevalence has been derived. All of the samples are volunteer, snowball,
over-represent Whites or have some other design feature which makes it
impossible to properly compare the proportion HIV+ between them. The way this
is done is very unsatisfactory and I want to suggest that the authors revise the
introduction again. I would like to suggest that the first address the issue of
proportion HIV+ in different MSM study samples and devote one para to this
showing the range and commenting that because of the methodological issues
between studies these are essentially non-comparable and are NOT population
prevalences. Then I would suggest devoting a paragraph to what has been found
associated with HIV in these studies.

**Thank you for this comment. Some of these studies have been RDS studies which
provide unbiased estimates of disease though not probability samples. In addition, I do
not think that whites have been over-represented in the majority of these studies. This
is not intended as a systematic review of these studies, but I am happy to send the
reviewer the full citations for more complete assessment. Every sampling strategy has
its limitations—including population-based assessments. When addressing the needs
of a stigmatized population, the training of interviewers to be specifically sensitized and
competent to ask questions about same sex practices is vital. We recognize that there
are biases in any methodology—including large scale surveillance projects. My
understanding of limitations of surveillance, including telephone based studies, is
related to my training focused heavily on surveillance as well as the fact that I teach the
surveillance course in the department of Epi at the JHSPH. When we move towards
meaningful surveillance to develop population-based estimates of disease and risk
factors in South Africa, there will need to be a targeted component to ensure validity of data retrieved. This has been the experience of the public health agency of Canada, the US CDC, the EuroHIV Surveillance program, as well as in Australia. Simply phoning or visiting people in their homes surrounded by their families is also going to result in biased estimates of the prevalence of same sex practices as well as the prevalence of HIV among those men that do have sex with other men. I will add in a further comment about the limitations of these studies, but do not think it worthwhile to remove the use of the word prevalence or making some broad comparisons about trend even with the biases present in previous assessments.

Please note that the discussion around reference [6] needs to be differently handled as it’s the association between MSM and HIV – this statistic isn’t available from other studies as they are just “about MSM”

**I do not think that there is a significant difference in terms of the statistic that we are reporting. If the reviewer would prefer that we remove the mention of their study in this article, we are happy to oblige.

Methods
Questionnaire – there is not enough information about what was actually asked and where the questions come from. E.g. did you ask about “transactional sex” if so do you have any idea what men made of the question? Its important for interpretation of findings to know that they were standard measures, we cannot just assume that because the questions have been asked in many countries that they are good questions and valid.

** Formative work was completed in every site to assess the validity of each question in the survey. Every survey done in various countries shares common ideas, though the wording is different to reflect local values, preferences, or cultural realities. Plus, it is interview-administered to ensure that all questions can be explained as needed.

Statistical analysis – this is not correct. It doesn’t include enough detail about what was done. What were the candidate variables for the multivariable models? Why was the p value set at 0.1 for backwards elimination, please reference this. 0.2 is more commonly used. There are a range of things included in this section which do not appear to match the research.

** As mentioned in the last round if review, the number was chosen as a balance between having a parsimonious model and an inclusive one. There is no absolute standard for backward stepwise regression as confirmed with multiple members of the STATEPI team. Their preference is not to use backward stepwise regression as they feel that the accuracy of this system in contrast to a log binomial method is limited. However, for this study we chose to use a stepwise regression including independent associations that were both part of an a priori framework of potential risks and also significant at the univariate level as well as basic demographic characteristics including age and education.
Results
Please report table 1 with columns for HIV+ and HIVUnder sexual practices – what is 0.49 in the second sentence – is this the mean number of female partners? How many had any? What does the p value refer to? Condom use – how were the 52.4% and 39.5% proportions derived? What was the denominator?
Bisexual practices – please delete ‘in bivariate analyses’ from the fourth line of this paragraph.
The multivariable model is inappropriate here as what it models is ‘factors associated with being bisexual’ – it’s a putatively causal model. If you want to know whether condoms or disclose or HIV etc are associated with bisexuality after adjusting for the social and demographic variables you need to build models of bisexual practices that have the one risk behaviour (or HIV) of interest and the social demographic variables.

** We addressed this issue in the last round of review and do not feel that this model represents a putatively causal model.

You must do this separately for each riskbehaviour/HIV.
HIV prevalence – line five – what do you mean?
Associations with HIV infection – Please could you separate you list into those variables that were associated with a greater likelihood of having HIV and those that were associated with a lower one. As the ORs are shown in table 2 its not necessary to state them all in the text.
Please revise table 2 to put the social and demographic characteristics at the top.
I think the bivariable OR for not wearing condoms is incorrect – please check – it looks like it should be a protective factor
Last para please delete ‘data not shown ‘ because you are showing the data!
Discussion – this needs extensive revisions
Page 8 – you have not presented data to allow a reader to reach the conclusion of your second sentence. I strongly advise you not to get into a lengthy discussion of prevalence when what you have is a convenience sample and one that is non-comparable with any other data source in age and race distribution. The main findings of this paper are factors associated with having HIV and this should be the focus of the discussion.
Rates of bisexuality from this study are non-comparable for the same reasons that the HIV+ % is non-comparable. Please focus on associations which are all that is potentially comparable given your data source

** We have made some adjustments, but do feel that we can make some basic conclusions in terms of demonstrating a high burden of risk factors and disease in this population. As described earlier, all sampling methods, including population based estimates of disease have biases—we have been careful to explain the limitations of our conclusions and analysis and feel comfortable with what is currently described.

** Level of interest: An article of limited interest
**Quality of written English**: Not suitable for publication unless extensively edited

**We feel that this may be an overstatement about the quality of language in this report. We sent for review to two other faculty members in epi who made some minor suggestions but agreed that the quality of English was fine.**

**Statistical review**: Yes, and I have assessed the statistics in my report.

**Declaration of competing interests**:  
'I declare that I have no competing interests'