Reviewer’s report

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

Version: 1 Date: 24 January 2011

Reviewer: Rachel Jewkes

Reviewer’s report:

Thank you very much for asking me to review this paper. In recent years there has been a growing openness to the understanding the needs of MSM in sub-Saharan Africa, especially as they related to HIV, and there is an emerging body of literature on this previously neglected topic. This paper seeks to contribute with findings from a small venue-based sample from Cape Town. It is potentially of interest to others working on MSM issues in SSA.

Major essential revisions

The paper has a number of quite large problems that will need to be fixed before it’s really clear what the main findings from the study are. The abstract will then need to be revised to ensure that these are properly summarised and the statistics that have been found need to be included in the abstract. It may be considered unwise to place so much emphasis on bi-sexual practices.

The background to the paper is generally adequate but I have a few very specific suggestions that need to be addressed:

Para. 1: HIV prevalence studies have also been conducted in South Africa among MSM as well

Para 2: laws criminalising homosexual practices have not been in place for 16 years and I don’t think this is really “recent” and it can’t really account for why there has been limited research on MSM. You need another explantation.

Para 3: the N on the paper cited here was 1277 not 46 – there were 46 men with MSM experiences out of the 1277. Further the HIV prevalence was 11.5% among men who had had sex with a man and 2% among all men. This is 5.8x higher. The statistic cited in the paragraph is the adjusted OR for the association with HIV and that is a quite different statistic.

Lane’s study was much larger but I would caution against the use of ‘definitive’ with respect to a RDS study, especially one conducted in one area as there will be questions about generalisability.

The prevalence of HIV among MSM in the two larger studies cited is actually lower than that among men in the general population. This deserves a comment and needs to be returned to with respect to the findings of the current study in the discussion.

Para 4: introduces the issues of multiple partners and then discussed MSMW.
This may be less relevant if the changes I suggest later are pursued – I wonder if concurrency with men as well as women and condom use, transactional sex and other risk practices wouldn’t be better discussed here as a context for a paper primarily on HIV and HIV risk.

The paper doesn’t have a clear statement of aims and this should come just before the methods. I would suggest you focus on the HIV prevalence and associated factors as you can cover the points you want to raise within this and the current dual focus on HIV and bi-sexuality isn’t working well and would needs considerable further work to be able to retain in the paper. I suggest below that biases in your sample may make this a non-ideal population to comment on bi-sexuality in and so perhaps you should focus on HIV.

The paragraph just before methods presently is very unclear and emphasises the importance of studying MSM in Cape Town. As a reader this is puzzling as the authors have not told us why CT should be any different from elsewhere and the MSM population particularly interesting – either do so or de-emphasise the setting.

Please avoid the word ‘determinants’ as this is cross-sectional data and you do not know about causality. Also if you are going to mention human rights abuses here you need to work them into the results / tables more systematically and I suggest you mention you will describe their prevalence and association with HIV sero-status (not done at present).

Methods:

The biggest source of bias in this sort of study relates to the venues and sampling. To try and make sense of the results you need to present more about the venues, how they were distributed between townships, how many were used, how many people were recruited per venue and how the interviewers were instructed about who to recruit, and how many to recruit. How did you avoid interviewing the same person twice?

How many recruitment staff were used? Can you tell us a bit about them? What efforts were made to reduce bias in recruitment?

The sample generated by this sampling method is a form of cluster sample and it is important to have a term for venue and adjust for this appropriately in your multi-variable analyses.

It’s important to describe the items in the questionnaire – what was measured ?, how? where did the questions come from, what language was it administered in?

When was the study done?

What are the sensitivity and specificity of the OraQuick tests?

Please reference backward elimination at p=0.1 as commonly a more conservative p is initially recommended (p=0.2). I don’t understand the reference to the use of two levels of statistical significance as actually it didn’t seem to me that associations at p< or =0.1 were being discussed as significant. However, I
don’t think they should be and would like the convention of p< or =0.05 to be used. I know the sample size is small so power is limited but there is a risk of spurious conclusions being drawn, especially given the non-random sample. Please ensure the methods used in all analysis are described and as well as information given on candidate variables considered for multi-variable models.

Was written informed consent used – please mention.

Results:

There are quite a few problems with the results and a number of errors with the statistical analysis and I suggest that the presentation be somewhat changed and all statistics be re-checked.

Given that this is not a random sample I would suggest that the authors reduce their emphasis on prevalence (as it’s quite unclear how their sample related to the broader population of MSM in CT or even at these venues) and focus on associations.

The authors are primarily interested in HIV prevalence (and previous testing). I would suggest that you start by describing HIV prevalence and then present your tables giving a total column and then a column presenting the proportion (n & % or median) among HIV+ men and then another for HIV- men. A column presenting the chi squared p value for the association between the variables and HIV status would be informative.

When a median is used please give range as well as IQR. Given that the age data is apparently not skewed it’s not clear why a median is being used – a mean and 95% CI is an alternative.

Please revised table 1 according to this. Because of overlap between categories of disclosure please derive a ‘never disclosed’ variable and include this. Please also include a variable for race, marital status (if asked) and for township of recruitment.

I would like to suggest that table 2 be treated in the same way and that all HIV sexual risk behaviours be summarised in this way (so include variables now in table 4). Since you have data on human rights violations I suggest you include this in table 1 or 2 as well.

I would like to suggest that you present in a table with the multi-variable model of factors associated with being HIV+. This model must include all possibly causal or confounding candidate variables and given their strong association with HIV in other studies I suggest you retain age and race as well as sampling venue in the model.

The current tables 4 and 5 have some statistical problems and I suggest you drop them with the new approach I recommend. I have checked some of your bi-variable ORs for table 4 and they are not correct – e.g. for not always wearing condoms as well as age, unemployment and partner numbers. I think the
‘disease’ here should be HIV and ‘exposure’ the risk behaviour – which is different from table 5 where it’s the other way round (very confusing – especially as this isn’t explained in the methods section). If table 5 is kept its necessary to describe the other variables adjusted for in each multivariable model.

I am very confused about table 5 as the one bi-variable OR I checked was actually correct, but the description of the results shows that the multi-variable modelling approach is wrong and bi-sexuality has been modelled as an outcome. Bi-sexually isn’t “caused” by not always wearing condoms – the analysis is the wrong way round – the association of interest is a model where bisexuality is a dependent variable and condom use the outcome. Ditto for the other sexual behaviours etc.

The text of the results:
Start by discussing HIV prevalence. The current table is unclear and its best to handle different by race, age etc in a revised table 1 (as suggested above)

At present many of the paragraphs only describe (re-iterate) what is in the tables. This is not generally accepted in journal articles. Please use the text to provide extra information or to highlight statistics from the tables.

p.10 – when discussing condom use by partner sex please make it clear that you present use among men having that type of partner.

Were respondents about to report multiple human rights violations (and if not why not)– its just not clear from the statistics that they did this. The n’s don’t currently add up as 10+21+16=47 not 49.

I am not sure about the sentence about testing and being aware of their status – where does the denominator of 50 come from? Please don’t present % by township as your n here =3

Please follow the advice above about reporting and discussing factors associated with HIV as the current paragraphs are very hard to follow. Its not good practice to just present a p value and no effect size/95%CI and also its not good to introduce new stats here that are not in the table – such as association with rape by township status. The sample size is not large enough to describe and compare between townships with confidence. I prefer to focus the reader on the multi-variable model findings and not the bi-variable as these are potentially wrong due to confounding.

Discussion
The discussion needs to focus on the main findings of the paper – all of them and not just a few!

The prevalence is of interest and is higher than the other cited paper, but the reasons need to be discussed. Age and race of the men are the most likely explanations and the possibility that they explain it must be explored. Please don’t test the other study’s prevalence and your’s for significant differences – this
isn't valid.

On p.13 I find the second paragraph bizarre. The whole HIV epidemic in South Africa is massively affected by race because we still have relatively limited racial social (sexual) integration. This needs to be addressed first and foremost as the explanation. The discussion must not speculate, it must use evidence from your own work and that of others to offer explanations. The circumcision discussion is speculative and should be justified with evidence or removed.

p. 14 what do you think was meant by blackmail? what is the hypothesis linking blackmail causally to HIV status? Please offer some suggestions here (preferably informed by interviews with appropriate men)

p.14 – the discussion of rape shows exactly why you should not do extra sub-group analyses. You have relatively few men with HIV and rape was quite rare. When you go to different townships your cell size is minute = hence the huge confidence intervals. Please, the pooled rape and HIV association is about 3x higher. Stick to this.

I am concerned that this sampling method was not a good way of accessing MSMW and that the prevalence may have been much lower than would be found in the general population. It is particular types of MSM who will frequent venues and not others. I suspect we may learn little about bi-sexuality overall from this sampling approach and caution you against a weighty emphasis on these findings in the paper.

The discussion in para 1 of page 15 seems again speculative, please bolster with data or references.

P.15 – the main limitation of this study is the non-random sample. The range of biases introduced potentially by this need discussion with relation to what is known about the venues used. It’s a population based sample (not RDS) that is needed to tell us about MSM in the general population. Venue based sampling may have some value (apart from convenience) as MSM who frequent venues may actually be accessible for MSM-focused interventions in a way that those who don’t perhaps aren’t. This strength could be discussed too.

The discussion needs to conclude with reflections on what the results mean in public health terms and for HIV prevention interventions.

Level of interest: An article whose findings are important to those with closely related research interests

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 have no competing interests