

PEER REVIEW HISTORY

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This paper was submitted to the STI but declined for publication following peer review. The authors addressed the reviewers' comments and submitted the revised paper to BMJ Open. The paper was subsequently accepted for publication at BMJ Open.

ARTICLE DETAILS

TITLE (PROVISIONAL)	High HIV Seroprevalence, Rectal STIs and Risky Sexual Behavior in Men who have sex with men in Dar es Salaam and Tanga, Tanzania
AUTHORS	Ross, Michael; Nyoni, Joyce; Ahaneku, Hycienth; Mbwambo, Jessie; McClelland, Scott; McCurdy, Sheryl

VERSION 1 - REVIEW

REVIEWER	Orroth, Kate London School of Hygiene and Tropical Medicine, Infectious Disease Epidemiology Unit
REVIEW RETURNED	25-May-2014

GENERAL COMMENTS	<p>This paper presents important data to add to the literature regarding HIV and STI prevalence among MSM in Tanzania. The study design and methods were appropriate to address the study question to understand HIV and STI co-infection among MSMs in a large and small city. However, more attention to detail and explanation of the methods, data analysis and discussion of the study limitations are necessary.</p> <p>Methods</p> <ol style="list-style-type: none">1. The authors planned a sample size of 200 for Dar es Salaam and 100 for Tanga. They should give the rationale for this choice, whether it was for statistical power based on hypothesized prevalence levels for HIV and STI or some other logistical reason?2. P1, Methods – What characteristics of the seeds were compared to assess sample equilibrium, age and area of city, or some other characteristics?3. In the procedures section the authors should describe data collected in the study questionnaire. For example, what instrument or questions were used to give the depression score or internalized homonegativity score. The reader needs a reference for these measures. <p>Results</p> <ol style="list-style-type: none">1. The tables should all be black and white and double spaced. They are difficult to read in their current form.2. The denominators (or Ns) should be given for table 2. I'd also put the DES data first as in table 1 to maintain consistency for the reader.3. In table 3, the lifetime number of partners should have a mean and standard deviation reported. Also, table 3 is not referenced in the results section of the manuscript.
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	<p>4. P2, Results – It seems results were only reported in table 4 for the data from DES or if these are the combined data it should be stated explicitly. The sample size is about 200 instead of 300. What are the reasons for missing a third of the data?</p> <p>Discussion</p> <p>1. Any limitations of the study or data should be highlighted. They are not currently discussed.</p> <p>2. P6, Discussion – Given the high rates of STI in the population, treatment of STI among MSM would benefit the population regardless of impact on HIV transmission and the authors should advocate or make recommendations regarding this policy.</p> <p>References</p> <p>1. The format for the citations should be numbered and at least one is missing, Grosskurth et al.</p> <p>2. Johnston, et al. is incorrectly referred to in the text as Johnson et al.</p> <p>3. All the references should be checked for accuracy.</p>
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REVIEWER	Hawkes, Sarah Reader in Global Health at University College London
REVIEW RETURNED	16-Jun-2014

GENERAL COMMENTS	<p>The paper is a good description of an STI and HIV survey among MSM in Tanzania. I think that the paper is clear as it currently stands, but suggest the following points should be addressed in order to enhance the understanding of the paper:</p> <p>1. The accompanying papers make it clear that there are additional individual behavioural and more community and structural level attributes that were studied for all participants. Exploring their association with STI and HIV rates will add to our understanding of community, social and structural variables and their influence on risk. So, for example, there is an accompanying paper that looks at violence and stigma and HIV rates. It is not clear why these findings were not combined into this paper to look at the association between violence and STI rates too. In this paper there is some mention of internalised homonegativity - but this needs explaining in the text as it is currently without any explanation (the explanation seems to lie in the accompanying paper). Similarly, there is another paper that looks in depth at perceptions of reasons and rationale for sexual motivations and sexual behaviours. This paper has studied the association of these individual variables with HIV but not with STIs. Is there a reason why this could not be done?</p> <p>Overall, I think that using the full data set to understand STI risks would be a major advance in our understanding of motivations, risks and influencing variables that are associated with STI risk transmission. For now it looks as if we are getting a rather "piecemeal" selection of studied variables without any detailed explanation of how those variables were actually selected. I think that if the authors could be persuaded to look at the full range of variables available to them and study their association with STI status, this would greatly improve the salience of the paper.</p> <p>2. I think there could be a more considered exploration of the sexual behaviour variables. For example, age at first sex is significantly lower among HIV+ men. I think this needs addressing in the</p>
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	<p>discussion; suggests the need for sexual health interventions at any early age, and promotion of comprehensive sexuality education in schools. Likewise, the "lifetime number of men had sex with" figures are significantly higher among HIV+ men. Is this representative of social desirability bias, or is there a chance that the men having first sex earlier, and with more partners, are actually a "core population" of more sexually active men with higher risk?</p> <p>3. The STI/HIV interaction (or lack thereof) is an interesting one and the authors quote some well known studies of HIV transmission to support their work. However, I wonder if they might also want to consider whether these infections simply represent different periods of exposure? HIV, hep B and syphilis are lifetime; GC and CT are acute. Is it not simply possible that we don't see an association because behaviours and risks change with time?</p>
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- This manuscript received three reviews at the STI but the other reviewer had declined to make his reviews public.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1.

Methods

1. The authors planned a sample size of 200 for Dar es Salaam and 100 for Tanga. They should give the rationale for this choice, whether it was for statistical power based on hypothesized prevalence levels for HIV and STI or some other logistical reason? **Paragraph on statistical power calculations is added.**
2. P1, Methods – What characteristics of the seeds were compared to assess sample equilibrium, age and area of city, or some other characteristics? **Details of equilibrium characteristics added.**
3. In the procedures section the authors should describe data collected in the study questionnaire. For example, what instrument or questions were used to give the depression score or internalized homonegativity score. The reader needs a reference for these measures. **Details of the IH and depression questionnaires added.**

Results

1. The tables should all be black and white and double spaced. They are difficult to read in their current form.
2. The denominators (or Ns) should be given for table 2. I'd also put the DES data first as in table 1 to maintain consistency for the reader. **Done.**
3. In table 3, the lifetime number of partners should have a mean and standard deviation reported. Also, table 3 is not referenced in the results section of the manuscript. **Done.**
4. P2, Results – It seems results were only reported in table 4 for the data from DES or if these are the combined data it should be stated explicitly. The sample size is about 200 instead of 300. What are the reasons for missing a third of the data? **This is due to listwise deletion of missing data in a logistic regression. Explanation added in footnotes of Table.**

Discussion

1. Any limitations of the study or data should be highlighted. They are not currently discussed. **This is now added.**
2. P6, Discussion – Given the high rates of STI in the population, treatment of STI among MSM would benefit the population regardless of impact on HIV transmission and the authors should advocate or make recommendations regarding this policy. **Very good point – we have added this.**

References

1. The format for the citations should be numbered and at least one is missing, Grosskurth et al. **Grosskurth now added. We will change referencing format to fit the journal where this is accepted.**
2. Johnston, et al. is incorrectly referred to in the text as Johnson et al. **Corrected.**
3. All the references should be checked for accuracy. **Done.**

Reviewer: 3

1. The accompanying papers make it clear that there are additional individual behavioural and more community and structural level attributes that were studied for all participants. Exploring their association with STI and HIV rates will add to our understanding of community, social and structural variables and their influence on risk. So, for example, there is an accompanying paper that looks at violence and stigma and HIV rates. It is not clear why these findings were not combined into this paper to look at the association between violence and STI rates too. In this paper there is some mention of internalised homonegativity - but this needs explaining in the text as it is currently without any explanation (the explanation seems to lie in the accompanying paper). **We have explained this in the text as requested.**

Similarly, there is another paper that looks in depth at perceptions of reasons and rationale for sexual motivations and sexual behaviours. This paper has studied the association of these individual variables with HIV but not with STIs. Is there a reason why this could not be done? **This paper is primarily an epidemiological one about the biological and clinical tests and HIV/STI prevalence, broken down by city and HIV status (n=300). The paper about MSM discrimination has almost no overlap with this one and nor does the one on sexual behavior motivations in this sample. Further, they are just on the DES data (n=200). If we were to combine them, the resulting paper would be about 10,000 words long and very disjointed.**

Overall, I think that using the full data set to understand STI risks would be a major advance in our understanding of motivations, risks and influencing variables that are associated with STI risk transmission. For now it looks as if we are getting a rather "piecemeal" selection of studied variables without any detailed explanation of how those variables were actually selected. I think that if the authors could be persuaded to look at the full range of variables available to them and study their association with STI status, this would greatly improve the salience of the paper. **We describe the significant variables after a preliminary bivariate analysis, as is usual in constructing regression models – many variables were not significant and didn't make it into the final model. A few crucial demographics are left in the Tables if they were not significant because their lack of significance is theoretically important.**

2. I think there could be a more considered exploration of the sexual behaviour variables. For example, age at first sex is significantly lower among HIV+ men. I think this needs addressing in the discussion; suggests the need for sexual health interventions at any early age, and promotion of comprehensive sexuality education in schools. Likewise, the "lifetime number of men had sex with" figures are significantly higher among HIV+ men. Is this representative of social desirability bias, or is there a chance that the men having first sex earlier, and with more partners, are actually a "core population" of more sexually active men with higher risk? **Good points, and we have added these into the discussion.**

3. The STI/HIV interaction (or lack thereof) is an interesting one and the authors quote some well known studies of HIV transmission to support their work. However, I wonder if they might also want to consider whether these infections simply represent different periods of exposure? HIV, hep B and syphilis are lifetime; GC and CT are acute. Is it not simply possible that we don't see an association because behaviours and risks change with time? **Yes, we thought about this, and we think that as GC and CT are often not treated in MSM, especially when rectal, they may also represent lifetime rather than acute infections.**