Author’s response to reviews

Title: Reported oral and anal sex among adolescents and adults reporting heterosexual sex in sub-Saharan Africa: A systematic review

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2nd January, 2019

Dear Editor:

Re: Reported oral and anal sex among adolescents and adults reporting heterosexual sex in sub-Saharan Africa: A systematic review

Imran Morhason-Bello, MD, MPH, MSc; Severin Kabakama, MD; Kathy Baisley, PhD; Suzanna C Francis, PhD; Deborah Watson-Jones, MD, PhD

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We appreciate the editorial team and reviewers for their feedbacks which have improved the quality of our manuscript.

The team have revised the manuscript to address issues raised by the two reviewers and responded to their specific points. We revised the quality assessment of randomised controlled trials included in this review by using the same tool used for all observational studies. In addition, we have also added a sentence to the recommendation.

All the authors have read and approved the revised version.

We look forward to hearing from you.

Yours sincerely,

Dr. Imran Morhason-Bello
Response to specific comments of Reviewers

REVIEWER #1

1. Abstract p.2, line 2: 'reported' where/through what means?

Authors’ response: Thank you, we have revised this sentence as follows: “Oral and anal sexual behaviours in high income countries are increasingly reported in peer reviewed journals....”

2. p.2, line 37: 'Heterosexual oral and anal sex' - careful of framing the acts themselves as heterosexual - oral and anal sex between men and women - refers to the behaviour, rather than the sexual orientation of the individuals involved in the act

Authors’ response: Thank you. We have revised this sentence as follows: “Reported oral and anal sex between men and women are prevalent behaviours in sub-Saharan Africa”

3. Page 3, lines 3-5: re-phrase 'health problems' - negative health outcomes rather?

Authors’ response: Many thanks. This has been revised.

4. Page 3, lines 5-10: references for this 'available evidence'?

Authors’ response: Thank you. We used some of the references for this statement in the discussion (see # 140 & 141 references), but references are not allowed in the plain English language section of this journal. Nonetheless, we have rephrased the statement to provide clearer meaning as follows: “however, oral and anal sex are not always regarded as ‘hetero-normative sexual intercourse’, and often disregarded by researchers, programmers and policy makers. Importantly, both sexual acts are sometimes perceived to be safer than vaginal sex against pregnancy and STIs and are associated with lower reported use of condoms to prevent HIV and STIs.”

5. Page 3, line 24: define 'protection'

Authors’ response: We have qualified what we meant by ‘protection’ as barrier methods of contraception.
6. Page 4, lines 14-16: makes it sound like anal sex causes HIV - anal sex is not associated with HIV if both partners are HIV negative so this statement is not accurate that the act of anal sex itself is associated with HIV acquisition. It would be better phrased something about comparative risk of HIV transmission between vaginal and anal sex.

Authors’ response: Thank you for your suggestion. We have revised this sentence as follows: “The comparative risk of HIV infection transmission between condomless anal sex and vaginal sex is higher than oral sex, and also, the risk is higher among those engaging in receptive anal sex than insertive anal sex when other HIV prevention methods such as anti-retroviral treatment or preexposure prophylaxis are not used”

7. Page 4, line 27: the terminology around 'drug addicts' has not been changed as stated in response

Authors’ response: We have changed this to “people who inject drugs”

8. Page 4, lines 39-41: do these people actually self-identify as being 'heterosexual'? Better to comment on the behaviours reported rather than the sexual orientation of the individuals - or if this data was indeed gathered, then differentiate

Authors’ response: Thank you. We appreciate your concern, this is why we revised these sentences by qualifying our statements as “reported heterosexual act” which to our understanding will not suggest sexual orientation.

9. Page 9, line 23: write out the number 8

Authors’ response: Many thanks. We have written letter 8 in words.

10. Page 23, line 10: define 'hard drugs'

Authors’ response: Thank you. This was included in error, we have removed the term ‘hard drugs’ from the sentence. The independent variable of the model did not include hard drugs. We have deleted it.

11. Conclusion: In light of the challenges / issues with collecting data in terms of ambiguity of terms for anal sex in some languages, likelihood of under-reporting etc - not only should
recommendations include 'clearer definitions' (assuming these are possible), but also tools and techniques to encourage more honest reporting, reduce reporting bias, and improve comprehension of terms and reduce ambiguity. More details on these types of recommendations are in reference 161.

Authors’ response: Thank you for your suggestion. We have added a sentence to the recommendation: “Researchers should also consider using complimentary tools such as pictures/drawings and other visual aids, as well as possible triangulation of interview methods to elicit better response from participants[159]”

REVIEWER #2

1. Overall, the manuscript is much improved, and the authors are to be commended for all of their meticulous work.

Authors’ response: Thank you – we agree that the manuscript is improved from the reviewer comments.

2. I appreciate the authors' efforts, but I feel that my primary points about sampling classification and assessment of risk of bias for the quantitative studies in Table 1 ("Selected data from quantitative studies reporting on heterosexual oral and anal sex in sub-Saharan Africa by year of publication") have still not been addressed. The point that I was trying to make about the Fonck paper is that the authors need to more carefully assess the sampling strategy of how each study in this review ascertained its study population, not the design of each of the studies themselves, UNLESS the study was specifically designed to make population-level estimates about oral and anal sexual behaviors. Using Fonck as the example, Fonck conducted an RCT of antibiotic prophylaxis to prevent sexually transmitted infections (STIs) and HIV-1 in a cohort of Nairobi female sex workers (FSWs). It just so happened that they also collected data on sexual behaviors as part of an administered questionnaire, so they have this information about their study population. For the purpose of this review paper, it matters how Fonck ascertained the cohort of Nairobi FSWs, because that is the study population, and that is the target population that the authors of the current review are trying to make statements about the prevalence of oral and anal sex behaviors (e.g., among FSWs). So how did Fonck ascertain this cohort of FSWs? Did they go out to each FSW venue in the country? Did they use snowball sampling? Did they use convenience sampling? This reviewer is still unsure. The fact that the Fonck paper was an RCT is not relevant to this review paper because RCT was a study design used for another objective, NOT for the objective of ascertaining information about the prevalence of sexual
behaviors, but for Fonck's objective of seeing how well the antibiotic prophylaxis worked to prevent STIs. In fact, the randomization likely occurred AFTER participants were asked about their oral/anal sex behaviors. Another example of this can be seen with the following reference: Watson-Jones D, Weiss, H., Rusizoka, M., Baisley, K., Mugeye, K., Changalucha, J., Everett, D., Balira, R., Knight, L., Ross, D., Hayes, R., J. : Risk factors for Herpes Simplex Virus Type 2 and HIV Among Women at High Risk in Northwestern Tanzania: Preparing for an HSV-2 Intervention Trial. J Acquir Immune Defic Syndr 2007, 46(5):631-642. The aim of the Watson-Jones study was "To determine prevalence of and risk factors for herpes simplex virus type 2 (HSV-2) and HIV among women being screened for a randomized, controlled trial of HSV suppressive therapy in northwestern Tanzania." So for the purpose of this review, it matters how they selected their trial population. RCT does not matter because the RCT was conducted for the purpose of testing the HSV therapy, not for the purpose of measuring the prevalence of oral or anal sex. After reading the Watson-Jones methods, it looks like they used venue-based sampling to get their trial population, so "probability sampling" would be accurate in this case but it is unclear on whether the authors incorrectly based this on the "RCT" or correctly based this determination on the venue-based sampling design (which I argue is a form of probability sampling). As a final example, I read the methods for Cornman 2008 paper which was also included in Table 1 as "RCT" and "probability sampling". However, the methods of this paper state that, "HIV-infected participants were recruited at an urban HIV care clinic in KwaZulu-Natal, South Africa that provided highly subsidized treatment for PLWHA." This seems like convenience sampling and should be categorized as such. RCTs are typically conducted for the purpose of randomizing people to different exposure groups to prevent or reduce confounding of the exposure-outcome relationship. Because of this, I don't think that the Cochrane risk of bias tool is an appropriate tool to use for assessing risk of bias in this review paper, because the Cochrane tool will be focused on how well the randomization process worked for the purpose of measuring the exposure-outcome relationship, whereas the whole purpose of this review is to get at the population prevalence estimates of oral and anal sex. Similarly, I am also not convinced that the "validated tools for cohort, case control, and cross-sectional studies" were appropriately applied. Cohort and case-control studies are also often conducted to get a non-biased estimate of the effects of an exposure on an outcome. However, the authors of this review paper should be trying to estimate how biased the prevalence estimates of oral/anal sex are in the reviewed studies are, compared to the true population prevalence of oral/anal sex among adolescents and adults reporting heterosexual sex in sub-Saharan Africa.

Authors’ response: We appreciate the explanation of the reviewer and recognise the points as being very important for this review. Thank you.

We have addressed the two key issues that were raised by the reviewer: the sampling of the study participants in RCT studies and the risk of bias tool for quantitative studies (supplementary tables 1 and 3). The author raised 2 key issues here:
a) We appreciate the concern of the reviewer on RCT papers reviewed. It is true that we considered only baseline data of the selected RCT papers for this review, however, we did not base the categorization of sampling strategy on “randomization sequence”. We simply based our assessment on how participants enrollment (volunteer or venue or convenience) was described in each of the manuscript. For example, In the Fonk et al paper, the authors described the locations where FSWs were recruited to be Kibera district in Nairobi in the introduction section. We agreed that the selection of participants in the Cornman paper was convenience sampling and this has been revised accordingly.

b) We have now used the same risk of bias assessment tool for all quantitative studies (RCT, Cohort, Case controlled studies and the cross-sectional studies). The tool was revised to improve the quality of assessment. This tool assessed key components of research methods, operational definition of outcome measure (oral or anal sex), presentation of outcome (giving or receiving oral sex, and insertive and receptive anal sex) and ethical approval. Specifically, we removed two variables (information on research questions and sample size determination) from the risk of bias tools because the research questions and samples size were not relevant for extracting the prevalence of, or risk factors for, reported oral and anal sex. We included four additional variables: the definition of oral and anal sex in the methods; and reported prevalence of oral and anal sex by sexual behaviour role – given or received (see supplementary figures 1 and 3). We believed these additional variables further improve the quality of assessment.