Reviewer’s report

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

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Reviewer: Shauna Stahlman

Reviewer's report:

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

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.

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