Reviewer’s report

Title: High burden of STI and HIV in male sex workers working as internet escorts for men in an observational study: a hidden key population compared with female sex workers and other men who have sex with men.

Version: 1
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Reviewer: Kristen Underhill

Reviewer’s report:

This paper aims to identify disparities in HIV and STI incidence between internet-based MSWs, FSWs (unclear if internet-based or other?), and MSM. It is timely and interesting, but I am concerned about a few methodological issues that should be addressed before publication.

**Double-Starred comments are Major Compulsory.**

*Single-Starred comments are Minor Essential.

Abstract

Abstract - Background: Referring to MSWs as “a hidden reservoir for STI” seems somewhat stigmatizing. Does “organized” mean that they are actually working together as a collective on the internet, or does it mean that individuals are simply using the Internet as their preferred venue for meeting clients?

*Abstract - Methods: MSM are described here as those “who did not report having paid for sexual contacts.” This means that they are not clients of sex workers – but not necessarily that they are not sex workers themselves. Is this what the manuscript intended? Judging from Table 1, which reports that 3% of this group had sex with a commercial sex worker, I assume this phrase is not correct. Was the intention to say that they “did not report being paid”?

Abstract – Conclusions: Typo in line 46 (“reduces”).

Background

**The background neglects the seminal article on male sex work by Baral et al. that was e-published in the Lancet last year. The Baral article also cites a 2013 report published by the UN General Assembly Special Session, which reports HIV prevalence among MSWs in 10 European countries, with a median of 8.9%. Baral et al. also cite numerous papers comparing MSWs to FSWs. Another recent paper on internet-based sex work by MacPhail et al. may also add to this background. There are also several new papers on male sex work in Europe that should be incorporated.


*This background and discussion sections tend to discuss MSWs using potentially stigmatizing language such as “source of transmission,” “subculture,” "exhibit high sexual risk behavior," "sustain their high-risk behavior," “spreading … to the heterosexual population” and “increasing the risks of disease transmission throughout Europe.” The emphasis at times seems to be more on containment and/or protecting others from male sex workers, rather than improving the prevention and treatment services available to sex workers themselves. The context of male sex work (reasons why MSWs may engage in transactional sex) is also missing, and providing some of this contextual information could help to focus the discussion on MSWs as a population in need of services, rather than a threat to others. If the manuscript intends an approach more focused on containment, it is up to the editors to decide if this fits with the values of the journal.

**The sentence in lines 87-8 suggests that there have not been studies comparing HIV or STI prevalence in MSWs to prevalence in MSM or FSWs. As the Baral article makes clear, this is not correct; studies comparing prevalences should be cited here.

Line 71 refers to “sex work on the streets or in venues.” “Venues” should be clarified.

Lines 75-76 refer to differences between Asian countries and Western countries, suggesting that each culture is homogenous – which may not be true. This should be more nuanced, e.g., discussing the need for research given unique features of the Netherlands context, or settings characterized by male sex workers who have migrated from Eastern European countries.

“Positivity rate” in line 89 may not be the most appropriate terminology -- although new infections were reported, incidence rates (cases per person-years) were not reported.

Methods

**My principal concern regarding the methods is the strategies for recruitment for MSWs compared to MSM or FSWs. MSWs were recruited via respondent-driven sampling, with initial seeds from online contacts. There is no information available for how FSWs and MSM were recruited; what outreach activities took place? Were these primarily street-based FSWs? Were there efforts to recruit MSM who were especially at-risk, or was this a general sample of MSM? Perhaps FSWs or MSM who meet clients/partners online would be more similar
to the MSWs in this study. If the study compares MSWs recruited principally online to FSWs recruited principally in street-based venues, then there are two differences between the groups – gender and venues for meeting clients. This would confound efforts to compare the two samples on gender alone. The same goes for comparisons between MSWs and MSM – if they differ not only by sex work engagement, but ALSO by use of online venues to meet partners, then any conclusions about sex work are confounded.

**Also – the manuscript does not say whether all the MSWs were internet-based, or exclusively internet-based. Internet is just one means of meeting clients – did some of these MSWs also encounter clients in street-based or entertainment venues? Is there a way to compare those who were exclusively internet-based, compared to those who used both strategies for meeting clients? Judging from the statement on lines 132-133, it looks like MSWs who were exclusively street-based were excluded – was this true?**

**My second biggest concern about this study is the double-enrollment of participants. In lines 153-156, the difference in the percentage of MSWs who had a repeat visit vs. percentages of FSWs and MSM should be addressed as a potential source of bias, since the consultation is the unit of analysis for both demographics and STI prevalence. How may double-enrollment have affected the results? This may be problematic. Could someone be counted in more than one category? For instance, if a man reports recent sex work at Time 1, but then doesn’t report sex work at Time 2, would his data be in both categories? And what effect would double-enrollment have on the demographics described in Table 1? For example, were younger people more likely to have repeat tests, and therefore biased the demographics toward a younger age? (This is especially problematic because the multivariate analysis shows that age makes a significant difference in the odds that a consultation will have a newly diagnosed STI.) Were riskier people or those diagnosed with an STI more likely to come in repeatedly? Again, this is especially problematic because Table 3 shows that a prior STI history was associated with higher odds of new infection. Why not report demographic and risk data (table 1) using the individual as the unit of analysis? One way to test how double-enrollment could have biased data is to repeat the analysis using individuals instead of consultations as the unit of analysis, and for participants who were tested multiple times, use only the results of their most recent consultation. This might yield very different results.

*In lines 107-115, the manuscript suggests that not all cases used the same types of testing (e.g., self-collected swabs, urine, clinician-collected swabs). Why was this? Is there any difference in the accuracy of these tests? Was any difference in HIV/STI prevalence observed for participants who got one type of test compared to another?*

I’d recommend using common names for infections throughout the remainder of the manuscript, rather than the abbreviations Ct and Ng.

*In lines 132-133, more information is needed about the questions for identifying MSWs. The background correctly notes that MSWs may not self-identify as sex
workers or escorts, including MSWs visiting the study clinic. How did they self-identify as male sex workers and involved in internet escort services for men? Is there a potential bias here in the men who were comfortable reporting this in a face-to-face interview with a nurse, compared to the men who were not comfortable doing so?

In lines 145-46 – was informed consent written or oral?

Results

*In various places throughout the results, MSW and MSM are mixed up. For instance, see lines 158, 174, 178.

Typo in line 167 (6f).

*Does “sexual contact with a commercial sex worker” mean that the participant was a paying client of a sex worker? Or does it also include casual, nonpaying sex with someone who is a sex worker? This should be clarified in text and Table 1.

Discussion

Line 185 overstates the contribution of this paper – there have been other studies that have compared MSWs to MSM, and that have compared MSWs to FSWs. See the Baral review paper for references.

**Throughout the discussion and even the results, the unit of analysis tends to be neglected. For example, it isn’t quite right to say that “more than 40% of MSWs were diagnosed with new infections” (line 187) when in fact, the result was that a new STI was found in more than 40% of consultations with MSWs, where 39% of MSWs had at least 2 consultations.

The manuscript suggests in line 215 that the sampling represents MSWs, MSM, and FSWs “in the Netherlands.” Does this mean that there is no street-based male sex work in the Netherlands? This group was excluded from the sample based on the inclusion criteria reported in lines 132-33.

**The manuscript suggests in lines 218-220 that multiple consultations with the same person may cause some confounding, but it does not explain why this confounding is unlikely to impact main results. This is important, given that 39% of MSWs, 68% of FSWs, and 65% of MSM enrolled more than once. Perhaps repeating the analysis using individuals as the unit of analysis would help to back up that statement.

**A larger limitations section is needed commenting on the following issues: face-to-face collection of demographic and risk behavior data, potential unwillingness of MSWs to report that they are in fact MSWs or involved in internet escorting, different recruitment strategies for the different subgroups, using different methods of testing some individuals, and the exclusive focus on internet-based sex workers compared to those who meet clients in other venues (instead of or in addition to the internet).
In line 223, how did the study discover that MSWs travel among European countries? Was this part of data collection?

Tables
Table 1 should clarify the lookback period (past 6 months?) for sex with men, sex with women, and sex with a commercial sex worker.

*Tables 1 and 2 need to clarify in Row 1 that they refer to “Consultations with Male Sex Workers,” “Consultations with Female Sex Workers,” and “Consultations with Men who have Sex with Men.”

*Table 3 also needs to specify that these are determinants associated with newly diagnosed STI in consultations with MSW, FSWs, and MSM.

Sometimes the reference group is specified for two-group comparisons (e.g., age), but sometimes it is not (e.g., sex with women, sex with a commercial sex worker, STI history, known HIV-positive). These table entries should all be reported in a similar way.

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

Quality of written English: Needs some language corrections before being published

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

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