Reviewer's report

Title: Herpes simplex virus type 2 seroprevalence and risk factors among men who have sex with men from Rio de Janeiro, Brazil: a cross-sectional study

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Reviewer: Javier R Lama

Reviewer's report:

This is a well written and elegant manuscript describing the epidemiology of HSV-2 infection in high risk men who have sex with men in Rio de Janeiro who participated in a HIV-negative cohort designed to assess the incidence of HIV infection among other secondary endpoints.

However, the old nature of the data and mainly, methodological concerns regarding how the reported study population was chosen (a subset of participants of a high risk HIV-negative cohort), attempt with the extrapolation of the results and totally bias any of the conclusions in the way the manuscript is being reported by the authors.

First of all, given the nature of the study population (a subset of participants of a high risk HIV-negative cohort), the relationship of HIV infection, a well recognized associated factor HSV-2 seropositivity in many other epidemiological studies (as well is reported by the authors in the Background section), and its potential influence in other potential covariates is neither being addressed in the study nor commented as a main limitation in the Discussion section in order to generalize the results to most of the MSM population in Rio de Janeiro.

It had been worthy to analyze HSV-2 seropositivity not among those qualifying to enter in this HIV negative cohort, but also among those 1165 men who were screened. If the authors desire to publish their manuscript they must change the title and the approximation and analyses of the manuscript, including the description of the described main limitation in the Discussion section.

Beside these previous comments, there are other major and minor comments that the author must address in order to improve the quality of the manuscript:

1. The manuscript may be shortened in length. There is no word count for the manuscript text.

2. The manuscript must recognize, starting in the title, that this is a high risk population. The study population it is not just  “a sample of MSM from Rio de Janeiro” as stated in the first sentence of the Discussion. It is a sample of high risk MSM previously filtered and qualifying for a HIV negative cohort.

3. It would be of help to shortly describe in the Methods section, "the Projeto Rio" entry criteria to better know how this study population was selected. This
reviewer was directed by himself to review the entry criteria in a previously published manuscript. Further readers may not do the same.

4. No explanation is provided to know why only the first 403 participants (86.3%) out of the total 467 cohort participants underwent for HSV-2 testing. Based on this authors’ self-selection, baseline HSV-2 prevalence among cohort participants would be overestimated in the context that people at higher risk in need of health care services would be enrolled first than later. This limitation must be also addressed in the Discussion section.

5. Every time the Hepatitis B diagnosis is referred in the manuscript based on the seropositivity for anti-HBc, the authors must address that this represents past or current evidence of Hepatitis B infection and not active Hepatitis B infection at all.

6. The statistical analyses subsection at the Methods section states “potential risk factors for HSV-2 seropositivity were…” The authors must agree that through this study design, they cannot evaluate risk factors. They would evaluate “potential associated factors” for HSV-2 seropositivity”. Other sections of the manuscript made this mistake as well. I encourage authors to carefully review the manuscript wording for this item.

7. The second paragraph of the Result section, states “One hundred and eighty one men (46%) were defined as MSM”. This must be corrected. By definition, the entire population is MSM. What the authors are referring is to men self identifying themselves as homosexual according to Table 2.

8. Given that they are not described in Table 2, it would be of help to provide IQR and ranges for age of first sexual intercourse with men and women.

9. Table 2 must be shortened in length. For two-option variables (yes/no), only positive (yes) results with their correspondent totals must be reported. That would decrease the length of the table by its half.

10. As for syphilis conditions, to avoid confusions, Hepatitis B definition must be recalled in the table footnote for Tables 2, 3 and 4.

11. Point prevalences for variables of interests must include 95% confidence intervals.

12. Table 3 would be decreased in length. Only most important variables should be reported in the table. Others would be referred in the text as not shown data.

13. PR abbreviation is not defined in the article text.

14. Have the authors evaluated age as a continuous variable and its relation with HSV-2 seropositivity? It is not clear why the 26 year-old cut off was taken.

15. Discussion section is elegant, but would be abbreviated

Level of interest: An article of importance in its field
Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

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