Reviewer's report

Title: Herpes simplex virus type 2 seropositivity and relationship status among U.S. adults age 20 to 49: a population-based analysis

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Reviewer: Kenneth Fife

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This manuscript uses data from the NHANES database to examine an aspect of HSV 2 epidemiology not previously evaluated. The authors make an interesting (although not especially surprising) observation that relationship status (“partnered” vs. “unpartnered”) is a predictor of HSV 2 antibody positivity. One aspect that is novel is that the association differed in different age groups with “partnered” persons between ages 20-29 more likely to be HSV 2 positive and “unpartnered” persons between ages 30-49 more likely to be HSV 2 positive. Because age, race, and gender remain strong predictors of HSV 2 positivity, the specific contributions of each parameter are still somewhat murky, but relationship status seems to be a relevant component of the mix. There are a number of limitations to the analysis, but the authors have appropriately acknowledged most of them in the discussion.

Overall, I had no major reservations about the manuscript. That statement must be qualified by the fact that I lack sufficient expertise in statistical methods to be able to judge the statistical approach, so my opinion is based on the assumption that the statistics are valid. I had only a few minor (discretionary) comments that the authors should consider that I think would improve the manuscript.

1. Methods – For this study, the authors used data collected between 1999 and 2008. This is longer than any other previous NHANES/HSV 2 report. Presumably, the longer collection period was used to obtain enough individuals in the specific sub-categories for analysis. Although it is unclear what impact this may have on the current analysis, it may be worth pointing out this difference between this analysis and previous NHANES reports (probably in the discussion rather than the methods).

2. Discussion – The discussion is rather long and somewhat unfocused in places. Although length is not a particular consideration for this journal, it does obscure the more important points of the paper. For example, the discussion of the possible origins of the racial discrepancy in HSV 2 positivity rates, while interesting, contributes little to the discussion and the results presented in this manuscript contribute little new information on this topic.

3. Conclusion (also the conclusion of the abstract) – The reference to “the viral nature of HSV 2” is a little puzzling. It is less the fact that genital herpes is caused by a virus than it is caused by a pathogen that causes chronic/recurrent infection that is novel. The “viral nature” of influenza A does not lead to similar
problems.
4. Table 1 – Using percentages alone makes the table somewhat difficult to interpret. Is the fact that Black women between 40 and 44 have a lower percentage of HSV 2 positivity due to small numbers in that category? It cannot be determined. Consider adding some numbers (at least to, for example, the age categories) to the table.
5. Figure 1 – Consider extending the y-axis to 100%.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests