Reviewer's report

Title: Socioeconomic factors and other sources of variation in the prevalence of genital chlamydia infections: A systematic review and meta-analysis

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Reviewer: Guy Harling

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This study presents a quantitative analysis of the literature on Chlamydia infection prevalence and how it varies by key socioeconomic covariates in high-income countries, based on population-based samples. The analysis is a potentially important one for identifying key at-risk populations, and targeting interventions. Nevertheless, there are several areas of the study which could be strengthened, particularly in terms of clear descriptions of the methods and what new evidence this review provides.

Major compulsory revisions

1. Geographical target of paper. At various points, the manuscript links findings back to the UK situation, however this is not an explicit goal of the paper. I would recommend either that the authors explicitly aim to present an analysis that can inform UK policy, or make their target international and not country-specific. This is linked to the study’s restriction of eligible papers for analysis to higher-income countries (line 96; line 116-7), which is not justified scientifically.

2. Exposure definition. The authors’ use of a “most” vs “least” disadvantaged groups methodology is potentially very problematic. First, the authors need to be very explicit about the values of each comparator group for each study and each exposure of interest (e.g. what does “least disadvantaged” mean for education in study X, etc.). This might usefully be included in the table providing an overview of the studies included. Second, the authors need to carefully consider whether heterogeneity in exposure categories within an exposure (e.g. within educational attainment) invalidates any efforts to conduct meaningful meta-analysis (over and above any heterogeneity in outcomes found). Linked to this, I worry that mixing countries with high levels of inequality with those of low inequality may again make comparisons between the most and least disadvantaged groups incomparable across countries.

3. Analytic target of paper. The Results section of this paper presents both analyses of variation by socioeconomic factors, and more general heterogeneity by other factors. Since the rest of the paper focused on socioeconomic factors only, it was unclear to me where the other heterogeneity analyses fitted into the paper. I would recommend that the authors integrate the latter analyses into the socioeconomic findings to strengthen the Discussion section, or drop them altogether.
4. Unmeasured potential confounders. As the authors note in passing (p292-3), race/ethnicity and urbanicity is a vital component of sexual health inequalities in the United States (quite possibly the most important one). This should be made very clear in interpreting the findings from the US studies, and elsewhere; more exploration of the possible impact of not including these variables in their analyses is needed.

5. Validity of meta-analysis/regression. I am not an expert in these methods, so cannot easily judge whether they added anything meaningful to the review. So I would ask the authors to clearly lay out to what they add to the qualitative descriptions of study findings. I would also very much like to see a clear statement of what this study substantively adds to the previous reviews of this literature, notably one published as recently as 2013 [ref 10].

Minor essential revisions

1. Line 80. References need fixing.

2. Line 109. Please specify which years are covered by “all years”

3. Line 119. The restriction to public-sector schools seems strange if this is an international study, since this will introduce differential biases depending on how different countries provide schooling. It would be helpful to know if this restriction led to the exclusion of many (any?) studies. Additionally, the term “state schools” is unlikely to translate internationally.

4. Line 120. Please explain what a GP register is for non-UK readers.

5. Line 142. It would be helpful to explain what the variation in question is here – I was not clear from my reading of the article.

6. ~Line 129. Many study results were presented stratified by gender, which seems eminently sensible. However I was not clear from reading the methods whether this was a systematic decision. Could the authors clarify their approach?

7. Line 187. If this subheading is used, similar ones are needed for employment, area deprivation, etc.

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

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 interests