Reviewer's report

Title: Seroprevalence and estimated incidence of maternal Herpes Simplex Virus Type 2 Infection in semi-urban women in Kilifi, Kenya

Version: 1  Date: 24 January 2011

Reviewer: Tony Ao

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Major Compulsory Revisions

1. In the Background, there seems to be a discrepancy where the authors first claimed that neonatal herpes infection contributes to the public health importance of HSV-2 (first paragraph), yet in the second paragraph it was stated that vertical transmission is rare. The justification for the study is weakened by this discrepancy. Might the authors be able to strengthen the argument to conduct this study even if neonatal transmission of HSV-2 is rare?

2. Study Population section, second paragraph: The data is being used from the DSS and also VCT clinic in the same geographical area and time. Could it be possible that some participants were included in both data set by chance? Please comment on the likelihood of this concurrent enrollment.

3. Study Population section, third paragraph: “Acceptance of HIV-1 testing was an enrollment criteria” (Note: should be ‘criterion’). Although the authors indicated that the DSS sample was randomly selected, this criterion might also introduce bias into the sample, as those who refused HIV testing in the general population might be different from those who accepted. Do the authors have any data about the percentage of refusal for the DSS data set? I suggest to also discuss this point in the limitations of the study.

4. Statistical analyses section: The author employed a catalytic model what was used for the estimation of rubella infection, as the reference indicated. Has this model been employed elsewhere for any sexually transmitted infection, more specifically HSV-2? I have concern over one of the assumptions of this model, which states that the force of infection is constant across all ages and over time. We know that the risk for HSV-2 infection increases with age and thus the model might not be appropriate to estimate incidence of HSV-2.

Minor Essential Revisions

1. Statistical analyses, second and third paragraph: The authors alternate between the past and present voice in the description of the statistical analyses. I suggest to revise this section so one verb tense is used consistently through out.

2. Table 4 adds very little information and it is suggested it is removed.
3. Results: Incidence estimates of maternal HSV-2 infection and estimated risk of neonatal herpes, first paragraph: The author only reported the incidence for DSS subgroup only. Perhaps I have missed why the incidence for VCT subgroup was not calculated? I understand the DSS sample is meant to represent the general population, but it would have been interesting to also see the incidence calculation for this group.

4. Discussion, last paragraph, should be: "Women infected with HSV-2 have an increased risk of acquiring HIV-1, and HSV-2 infection may remain a driving force behind the HIV-1 epidemic in this semi-urban population, while mother to child herpes transmission is unlikely to be a significant public health problem in this population."

Level of interest: An article of limited interest

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

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.