Reviewer’s report

Title: High prevalence of HSV-2 among rural pregnant women in Tanzania

Version: 2 Date: 8 October 2007

Reviewer: Lewis John Haddow

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- Major Compulsory Revisions

The title does not accurately convey what has been found: the HSV-2 prevalence is not that high in this sample, for a sub-Saharan African study.

The discussion and conclusions are well balanced and adequately supported by the data, but are over-long. The discussion is too wide-ranging for this report. The inclusion of information about circumcision is of low relevance. Likewise the following paragraph on valaciclovir etc should be removed. There are other bits that could be cut. The discussion should not go much further than comparing and contrasting this paper with other Tanzanian studies (there are several in the authors’ reference list).

- Minor Essential Revisions

What is a polygamous or monogamous relationship? These terms may be culturally-specific and need to be defined.

What HSV-2 prevalence was considered relevant in the sample size calculation?

Minor grammatical errors.

HIV prevalence and overall characteristics of the sample are not described in the abstract.

- Discretionary Revisions

In the objectives, the plan to estimate the impact of exposure variables is not stated.

The data are sound and well controlled, although the Basotu clinic seems to stand out as being much lower prevalence than the rest, and perhaps the analysis for odds ratios should be done excluding this clinic, or at least a comment in the discussion to explain what is different about that clinic.

It is stated that HSV-2 seropositivity is related to history of sexually-transmitted
infections. But what were the criteria for this exposure variable? I assume in this area that STIs are managed syndromically and there are no diagnostics. Did the women self-report STIs, or was it based on medical records? Did this exposure variable amalgamate genital ulcers, discharge and pelvic pain syndromes as a single factor?

In the results, do not say “young” and “middle-aged” women, instead state their ages.

The discussion makes the risk of mother-to-child transmission of syphilis sound important in late latent syphilis. I think it is actually quite low. What is the actual risk of mother to child transmission in late latent syphilis?

Quality of written English
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Suggest having a native English speaker go through the manuscript, as there are minor grammatical errors and non-standard forms.

Statistical review
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The manuscript does not need to be seen by a statistician, but I have two questions:

1. Why was it decided to do age-adjusted analysis first? (especially for syphilis where age was not associated with prevalence)

2. With such a low prevalence of syphilis, is it valid to do multivariable analysis at all?

What next?: Accept after minor essential revisions

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

Declaration of competing interests:

I declare that I have no competing interests