Reviewer's report

Title: Nutritional status and HIV in rural South African children

Version: 2 Date: 12 March 2010

Reviewer: Alice Tang

Reviewer's report:

Overall, this is a very interesting and informative paper demonstrating results that could have important policy implications. There are a few weaknesses that should be addressed as detailed below.

Major Compulsory revisions:

The authors present p-values for differences in Z-scores between the total sample and the HIV free population, but it is not clear how these p-values were obtained. These two samples are not independent of each other.

Can the authors comment on the appropriateness of using the WHO 2006 standards for calculating the Z-scores? Are there more local references that could be used?

Related to the above point, is it possible that one reason for the higher prevalence of stunting in children living in villages mainly inhabited by people of Mozambican origin could be a genetic predisposition to being shorter? Therefore, the use of a different reference standard may remove the observed differences?

To what extent is the lack of association with food security in this study due to the lack of using a sophisticated tool for measuring food security? The authors state that Food Insecurity was defined as not having reported enough food to eat either in the last one month or in the last one year. Was this based on only one question? There are several measurement tools available that rigorously measure food insecurity with multiple questions addressing multiple dimensions. Perhaps this should be stated as a limitation in this study.

Discretionary revisions:

The authors state that “Children who were not tested had seemingly better nutritional outcomes generally than both HIV negative and HIV positive children”. Do they have an explanation for this? Do they suspect there was some bias in sampling or uptake of testing?

At the end of the fourth paragraph in the Discussion section, the authors state that the poorer outcomes related to WHZ in older children may be associated with weaning practices. They provide a reference for this, but it would be helpful to elaborate on this point for readers that are not familiar with weaning practices and how they might affect nutritional status.

In the 9th paragraph of the Discussion Section, the authors should further
elaborate on the point they make regarding the small proportion of HIV-positive children biasing the strength of the associations. What direction do they expect this bias to be in? Do they think that if the sample size were larger, the differences in HAZ and WAZ might go away?

Figure 1 does not add much, particularly since there are no significant differences by HIV status. The authors might consider incorporating this information into Table 1.

Consider dividing Table 1 into two tables – the first including just the nutritional outcomes (including binary stunting, underweight, and wasting variables) and the second including the other characteristics.

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests