Reviewer's report

Title: Prevalence and pattern of HIV-related malnutrition among women in sub-Saharan Africa: a meta-analysis and meta-regression analysis of demographic health surveys

Version: 2 Date: 26 February 2008

Reviewer: Don Operario

Reviewer's report:

Manuscript poses a clear and important research question, on prevalence of malnutrition on HIV positive women in southern Africa. Strengths include meta-analysis techniques which allows for a more powerful estimate of HIV-positive women who are underweight, based on national representative surveys from 11 countries, and low heterogeneity among included studies suggesting appropriateness of pooling results from across studies. Implications for future research and for policy intervention are well noted.

Specific comments are:

1) A major concern concerns the study operationalization of malnutrition as BMI < 18.5. BMI in this sample of HIV positive women might reflect issues other than malnutrition, such as consequences of HIV illness or stage of disease progression. This limits the interpretability of BMI data as a valid reflection of malnutrition, which the literature generally defines in terms of dietary intake. It is recommended that a revised version of this manuscript state more explicitly the main outcome as prevalence of low body weight, which can be a proxy for malnutrition but lacking appropriate data on dietary intake and food/calory consumption this cannot be inferred directly.

2) The absence of significant findings in the meta-regression could be discussed further. Could this be associated with the extremely high homogeneity among studies, which might undermine ability to detect variability due to country characteristics? Moreover, the rationale for conducting the meta-regression could be better supported. Page 4 described the purpose for the meta-regression as a technique to "identify country-level factors that explain the variation among studies", but given the high homogeneity observed conducting this analysis seems inappropriate. Were there other reasons?

3) Background can provide a better justification for why it is meaningful to focus this analysis specifically on HIV-positive women. What are epidemiological, clinical, and policy implications for examining this specific population?

4) Thorough editing should address grammar and syntax issues.

5) Introduction should cite the revised UNAIDS estimates.
6) Table 1 needs clarification: DHS versus DHS+; definition of the final 3 columns (number of women, household, cluster).

7) Table 2, though descriptively interesting, is a summary of human development indicators (based on UNDP) and does not appear to present original data. Should this be summarized in text instead of reported as a Table? Also, justify why using 2004 HDI data rather than updated HDI--is this to reflect the year at which individual study data were collected?

8) Figures 1 and 2 can be omitted and reported in text only.

9) Figure 3 is difficult to read. Authors should consider reporting these data in a Table.

10) Figure 4 needs to clarify what is the outcome being reported (ostensibly prevalence of BMI < 18.5. I find it striking that confidence intervals for Cameroon and Lesotho or so wide (and that this does not affect the overall I-square).

11) Figure 5: does the forest plot really add anything here? Esp if the figure reports weighted prevalence and 95 CIs.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

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.