Reviewer's report

**Title:** Secular trends in pediatric antiretroviral treatment programs in rural and urban Zambia: a retrospective cohort study

**Version:** 1  **Date:** 21 May 2010

**Reviewer:** Mary-Ann Davies

**Reviewer's report:**

This is an interesting and well written paper on an important topic. Analysis of secular trends in paediatric HIV service and ART scale up are critical to monitor and understand how to improve service delivery.

**Major Compulsory Revisions**

1. In the last sentence of the first paragraph under the heading “Statistical Methods”: a 90 day window used to consider measurements as being at ART initiation may be too long. For CD4% and CD4 count, it would be better to use a relatively short window after ART initiation as CD4 levels can increase rapidly after ART start. Measurements beyond 2 weeks to 1 month after ART start probably do not reflect measurements at ART initiation.

2. It would be nice to also see a figure of the number of children initiated on ART over time and the proportion of eligible children that were actually initiated. In this respect, were all eligible children initiated, and if not, what were the reasons for this? It seemed that in the urban clinics in year 4 and year 2, more children were initiated than were eligible, while in the rural clinics this was the case in year 4. Perhaps the additional children were identified as eligible in the previous year but initiated the following year?

3. The authors comment that there is a decrease in the proportion of deaths over time in the urban areas. While a detailed analysis of mortality risk factors is clearly not the focus of this analysis, the authors should mention as a limitation that they have not examined predictors of mortality so cannot comment on whether the change in mortality is programme-related or related to differences in the profile of children initiating treatment. For example, the authors state in the discussion that “it is unknown whether the observed decrease in the urban clinic was due to better survival or differential ascertainment of deaths in the clinics.” The authors could expand on this to talk about not having looked at whether the change in survival (if this is real) is explained by differences in baseline characteristics alone, or whether there is an independent secular trend.

4. The number of children in whom CD4 and WAZ measurements are available at 6 months is very small and not consistently the same proportion of those initiating ART across the different years. It would be helpful to know the percentage completeness for each measure (i.e. proportion of those in care at 6 months in whom a measure is available) and also for the authors to comment in the discussion on the possible factors that may determine which children have
measurements available and which do not – e.g. are younger or sicker children more likely to have their CD4 and weight measured at the 6 month visit?

5. Can the authors comment on follow-up of children not eligible for ART? In the Methods section, it states that children not on ART return for clinical and laboratory evaluation every 3 months. A key issue in many programmes is loss to follow-up of children not yet eligible for and initiated on treatment and the ability to keep these children in care so that treatment is initiated in a timely way when needed. Outcomes of these children would therefore be a useful indicator of programme performance, if the data is available.

6. In the Discussion, comparisons are made between the urban and rural programmes. For example: “Mortality was consistently higher in the rural clinics…” The Results section does not include a formal statistical comparison of the urban and rural programmes, but rather looks at secular trends within the urban and rural programmes separately. The authors should therefore be more guarded in the discussion about comparisons between the urban and rural programme. For example: “Mortality appeared to be higher at rural clinics…”

Minor Essential Revisions

Nil

Discretionary Revisions

1. In paragraph 1 under results, the authors should state in the text that the decrease in age at urban clinics over time is a trend, as the table shows this not to be significant.

2. Figure 1 could be simplified by rather showing the number enrolled per quarter or half-year, than the number per month, unless there is a particular seasonal variation that the authors wish to comment on.

Level of interest: An article of importance in its field

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