Reviewer's report

Title: Time Trends of Baseline Demographics and Clinical Characteristics of HIV infected Children Enrolled in Care and Treatment Service in Dar es Salaam, Tanzania

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Reviewer: Christopher Gill

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Review of Sando et al, BMC 2014

The paper describes what is essentially an ecological analysis, looking at changes in the population structure of pediatric HIV enrollees over an 8 year span in Dar es Salaam Tanzania. I had some small questions regarding methodology (why for example did they only report median outcomes, not means?), but what the paper seemed to be lacking was an overlay of context in which these changes were occurring. Over this period there has been a sea change in the availability of ART, scale of PMTCT services has expanded log fold, and the drugs used in treatment have shifted. New diagnostics for TB and other OI have emerged, and clinical algorithms and trainings of staff have evolved and matured. It seems that these external factors are very relevant to note in trying to write a story based on the numbers in this paper, but in this regard the authors have not made any real effort to place their results in this larger context. I recognize that in an analysis of this kind, it will not be possible to extract cause and effect relationships, but even so, context is critical.

Major comments:

1. Conversely, the year 2011 only included data to September, making it only ¾ of a year. That is not discussed in relationship to the numbers of enrolled children in that year, which was lower than in all other years. Presumably some of this is simply because they had 25% less time in which to enroll. Please take account of this in the analysis. Perhaps presenting the enrollment stats in terms of an averaged monthly or quarterly rate, instead of by year, would solve this problem.

2. Please also comment on whether some proportion of children were already on ART at enrollment, and how many were truly ART naïve at enrollment. This is relevant in regards to the rising Hb levels noted below. In a number of African countries, AZT was being phased out in favor of Tenofovir. The former is known to cause anemia, whereas the latter does not. A shift in the exposure to different ART regimens might help explain this seeming paradox.

3. In the results, you note that the proportion of patients with WHO class 3 or 4, and those with OIs at presentation, has been trending upwards. Surprisingly, the baseline Hb levels rose over the same period, which would seem to be a sign of better health.
4. I saw no mention of what ART regimens were being used, and whether different regimens were used for younger vs. older children, and whether the regimens shifted over time (which seems likely).

5. In your Table 1 it is noted that the % of pediatric enrollees declined markedly over the years, while the total # of pediatric enrollees followed a different pattern. Clearly this speaks to the total # of enrollees in the population at large, and while one could back calculate these #s from the data provided, it would be much easier to simply provide the total populations in each year. Please do so.

6. The p-values in Table 1 are not terribly helpful. Presumably they assess whether there is significant deviation from the central tendency across the full array of data by year, but from the reader’s perspective it is not at all clear what is the main point of difference driving these values. The age/IQR is a good example of this, since every year but two had the same median value, and yet the P was < 0.01. In other words, the choice of statistic has rendered the story a bit opaque.

7. Over this period, it can be assumed that two trends were at play. First, ART uptake was rising sharply due to greater availability of meds and wider screening of the population. Second, PMTCT services were expanding rapidly. The effect of the former is to increase the denominator of the population at large on ART. The second will have the effect of diminishing the cohort of pediatric HIV cases. It would be very helpful in your narrative of table 2 to describe these processes, which seems very likely to explain why the % of pediatric HIV cases declined so sharply over time. If available, please include data regarding the uptake of both phenomena.

8. The authors make note of the shift in TB diagnoses at baseline. Interesting, but again makes no sense absent a wider context. How was pediatric TB being diagnosed in 2005? That was years before GeneXpert came on line, so presumably was the way it is diagnosed worldwide: clinical suspicion without microbiological or molecular confirmation, which is neither sensitive nor specific. It is plausible that screening processes and criteria for declaring a true positive shifted over this 8-year span. In which case, the decline in TB caseload could reflect the wider adoption of more specificity in the diagnosis of TB, as opposed to an actual change in disease burden. Please explain this further.

Minor revisions

9. line 81: methods. The dates of collection include Oct 2004 – a partial year. The data presented in Table 1 start from 2005. This does not seem to be aligned. If the ‘2005’ data include the last quarter of 2004, please say so and amend the table to make this clear. Note also that this gives the first year 25% more time to accrue than the intervening years.

10. Line 88 describes this as a prospective analysis, but the study felt like a retrospective analysis of data that had been collected since 2004. Please clarify.

11. Please confirm that all the data described in lines 87-94 was collected at baseline, i.e., when the subjects first enrolled in the clinic.
12. I'm puzzled why only medians are provided. This would seem to obscure our ability to see more subtle changes over time, such as the average age of enrollment. For example, the Median age is 5 in every year except for 2006 and 2011, when it was 2 and 5 respectively, and yet the p value for this series of data is < 0.01. This does not seem to be very intuitive as presented, so unless there is a reason why means cannot be provided (e.g., data collected only in ranges), then it would be better in my view to provide the means instead with SD.

13. Why was diarrhea selected to report on separate from other OIs? How was this defined? Are you talking only about acute diarrheal disease? Or is this chronic diarrhea (and if so, how was this defined)?

14. Why is the proportion of subjects with wasting missing from 2011?

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