Reviewer’s report

Title: Human immunodeficiency virus-associated tuberculosis care in Botswana: evidence from a real-world setting

Version: 1 Date: 14 Apr 2019

Reviewer: Patrick Cudahy

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The addition of a cox proportional-hazards model has significantly strengthened this analysis. The manuscript identifies several areas where healthcare delivery could be strengthened, highlighting the benefit of this programatic data.

One area that does still require revision is that without CD4 data, the conclusion that ART naive patients were inappropriately not commenced on therapy is not supported. Most deaths were before two months, which is before ART should have been started in those with CD4 >50 per WHO guidelines, of CD4 > 100 per Botswana guidelines. During the time period of this study, persons with HIV with higher CD4 counts (>350) without a TB diagnosis were more likely to not have been started on ART, so the ART naive group may have had high CD4 counts. In addition, the Cox PH analysis showed that another significant predictor of death was major side effects (as defined as "requiring first-line TB treatment to be discontinued") which would have also appropriately delayed ART initiation. In your reply to my prior comments you state that "we should also note that both integration of ART into TB treatment and timing of ART initiation during TB treatment have been shown to improve survival among HIV-associated TB patients". I do not know what the distinction is. I am not aware of studies that have looked at concurrent initiation of both TB and HIV therapy at the same time. Based on existing evidence, CD4 count and timing of ART are both very important in the survival benefit. Since you do not have the CD4 data, the conclusions you can draw about this are weaker than what you state.

I also would suggest not reporting mean survival times (lines 208-210) since most patients were only followed for 6 months and the cases with bone or CNS disease that were followed longer would bias this. In general, median survival is better to report but when I look at the Kaplan-Meier curves, the survival function is never less than or equal to 0.5 for either group, so there is no median survival. I would omit these summary statistics, but keep the kaplan-meier figure.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No
Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
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I am able to assess the statistics

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