Reviewer's report

Title: Exclusive Breastfeeding among Women Taking HAART for PMTCT of HIV-1 in the Kisumu Breastfeeding Study

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Reviewer: Suzanne Penfold

Reviewer's report:

Thank you for the chance to read this manuscript again. I think the authors have made good improvements to this paper describing the rates of exclusive breastfeeding, and factors associated with EBF, in the context of a PMTCT trial in Kenya. I have a few more observations on the paper, and have repeated a few from before that I do not feel have been fully addressed:

Major compulsory revisions

Abstract

1. I previously mentioned “the conclusion seems overstated–these results are from a clinical trial with many contacts between health workers and mothers (at least 6 after delivery plus unknown during pregnancy). This is more than a simple counselling intervention.” While the conclusion has been improved, this comment has not been addressed

Introduction

1. I still feel that the introduction needs to present more information about what is already known with respect to breastfeeding practices among HIV women – can you pull out some details from references 6, 11 and 12 for example, for inclusion in the introduction?

Methods

1. I previously said “Please make the study design clear. It appears to be primarily cross sectional secondary analysis of clinical trial data”. You have explained further what the study design for this paper is in the comments, but it is not clearly specified in the methods. You say it is a secondary analysis of longitudinal study – a longitudinal study is not a specific study type and needs to be more clearly defined and stated as a cohort or panel study.

Results

1. You respond to a comment by another reviewer to say that some of the 480 women with breastfeeding data (immediately after delivery?) were lost to follow up by 5.25 months. In which case the N for the analysis at 5.25 months should be 462 surely? This is not biased analysis. Alternatively, if you classed those who dropped out as not doing EBF but still included them in the analysis, this should be clearly explained in the methods. Also, understanding how many participants dropped out at which time points re-emphasises the need for a flow diagram
specifically for the analysis done in this study, which is another comment I made previously.

2. Given the focus of the discussion and conclusion on the role of counselling to improve EBF, I agree with the co-reviewer that analysis of the association between number of visits and EBF should be presented. Even though there can be different ways to interpret the results (such is the limitation of cross sectional analysis), these results need to be seen and discussed.

3. I see from the plos med methods paper that babies were tested for HIV at 6 weeks of age. Given the HIV context it seems important to test for an association between HIV status of the baby at 6 weeks and EBF at 5.25 months, for inclusion in Table 2. Or to justify why this was not done in the methods.

Discussion
1. The authors should report and compare their results with any existing evidence of factors associated with EBF in HIV positive women and broader populations.

Conclusion
1. The conclusion should also refer to the factors found to be associated with EBF. While there are benefits of EBF to all mothers and babies, the wording of the conclusion should be made specific to HIV positive women as this is what the data relate to.

Minor essential revisions
1. Add the study design to the title – this previous comment has not been addressed

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

Quality of written English: Acceptable

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