Reviewer's report

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

Version: 3
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Reviewer: cecile cames

Reviewer's report:

Dear editors,
Dear authors,

Dakar, 02/24/2014

I reviewed the paper entitled 'Exclusive Breastfeeding among Women Taking HAART for PMTCT of HIV-1 in the Kisumu Breastfeeding Study' of Okanda et al. This is a sub-analysis of the KiBS that aims to assess the prevalence of exclusive breastfeeding among HIV-infected mothers participating in a PMTCT study in Kenya. The authors also focused their analyses on the risk factors associated with the practice of exclusive breastfeeding at 5.5 months postpartum.

A/ Major Compulsory Revisions

1/ There is a confusion throughout the manuscript, abstract and tables between the study population of the KiBS, and the study population of this particular sub-analysis.

2/ Methods: because this is a sub-analysis, it's important to replace the work in its context and make the difference clear between information, population size and procedures relative to the KiBS and those relative to this sub-analysis for the readers, a suggestion would be:

• ‘KIB Study’ : introducing study setting and procedures used to provide counseling and collect data, and ethical considerations

• ‘Study population and definitions for this analysis’ : describing inclusion criteria in the present analysis, and presenting key definition (EBF, PBF, MF).

• ‘Statistical analysis’

3/ Methods: in the present version, this key-information is given in the wrong order, or not mentioned at all. Method section is not clearly structured; data collection and analysis look like distinct sections.

4/ Methods, paragraph 1: the 2 last sentences should be deleted (and placed at the beginning of the results section).

5/ Methods, paragraph 2: the 2 sentences about ARV should be put together in
With regards to the study objective: details on the following points (6-9) are expected by the reader:

6/ Methods: should describe extensively the modalities, frequency of counseling and explain to the readers why this counseling intervention is “unique” as presented in the discussion. Give reference of the WHO guidelines used in your study.

7/ Methods: Who were the counselors? Profile, training. Who were the home visit surveyors?

How long did the ‘home visits’ last? What was the study procedure when/if a home visit shows a predominant/mixed breastfeeding practice?

8/ Methods: should describe extensively the modalities, frequency of the data collection,

Who were the surveyors? What were the study tools used for both counseling and data collection?

9/ Methods: should indicate if the counselors and the study staff for data collection were distinct persons. If it was not the case in the KiBS, this would have important implications in the results and should be discussed in details in the discussion section.

10/ Data collection: this paragraph deals with KIBS procedures, only the two last sentences present data collection.

11/ Data analysis: delete first sentence

12/ Data analysis: sentence “…stratified by maternal and infant characteristics at baseline and 2 weeks postpartum”. When reading the tables, it is unclear whether baseline in this sentence is inclusion or delivery. What are the arguments for ‘2 weeks postpartum’? Only the maternal BMI at 2 weeks is relevant in my opinion.

13/ Data analysis: Sentence: “the event of interest was time of first mixed feeding”: I disagree with this statement. The objective of this analysis is to study prevalence of EBF, so the event of interest is time of first predominant breastfeeding. The authors should be clear on their hypothesis, if the question is the prevalence of full breastfeeding (EBF and/or PBF), or strictly EBF.

14/ Results section: should start with a paragraph on the study population flow: from the 522 to the 480.

15/ Discussion section: Starting by qualifying the support provided as “unique”, at this step one is quite surprised of such high breastfeeding rate at 5.25 month and feels uncomfortable with the lack of details in the method section about what type of support these women actually received and who provided versus who collect the data.
Discussion section: Limitations of the study should include the absence of qualitative approach to sustain the high declared rate of EBF, as we know mothers may declare what they think they should do and not what they actually do.

I feel that the results could be more questioned by the authors; the absence of association (even a trend!) with any of the traditional and major risk factors of EBF failure is quite surprising and would worth discussion. As a result, the discussion sounds a bit poor, and finally repeats the study conclusion throughout the text. I think the discussion could be alternatively strengthened by a pertinent review of recent publications on the same topic (as an example, this section does not mention any of the several and recent PROMISE EBF study group’s papers). The result would gain to be better compared with the previous studies on the same population, since this would highlight the specificities/interest of this study (that does not appear clearly in this version).

Table 1

I suggest to correct the title to: “characteristics of the study population of 480 HIV-infected mothers included in the ….”

No need to mention ARV in the title.

Title line should contain 2 parts: first “characteristics” second “study population N (%)”. The authors could refer for presentation to the table 2 in the recent paper “Infant feeding practices among HIV exposed infants using summary index in Sidama Zone, Southern Ethiopia: a cross sectional study », Haile et al, BMC pediatric, Fev 2014.

Delete % from the table as it is indicated in the title line

BMI at inclusion is not pertinent, the classification used is unknown to me and there is a gap of category between 17 and 18 (!).

The thresholds of international classification are:
BMI <18 ; 18<= BMI <25; 25<= BMI < 30; 30<= BMI

Moreover, BMI is specified “at enrollment” what about the other variables?

Minor Essential Revisions

Manuscript: use a consistent term for ARV throughout the paper, ex: put (ARV) at first mention of antiretroviral prophylaxis in introduction/methods

Statistical analysis: did the authors investigate further the risk factors via a Cox model/ logistic regression, as a complement to the KM analysis?

Results section, paragraph 2, first sentence: would replace by “…. to the recommendation to exclusively breastfeed up to 5.5 months”

Results section, paragraph 3: is not the subject of the paper
Table 1
1/ Suggestions to improve the table:
Because none of the covariates is associated with your dependant variable, they should be presented in a more synthetic way.
Example “income” regular/irregular”, if not simplified, at least presented directly in USD ?
“Viral load detectable/undetectable” with a foot note indicating the threshold of 400 copies. By the way, you could also indicate the device used (400 copies is not so usual…)
These simplifications could be applied to almost all variables.
The table 1, once simplified, could group together maternal and infant characteristics, as authors refers to the latter in the table 3.
The table 1 could mention first maternal characteristics at enrollment, with a foot note for BMI at 2 weeks and in a second part the infant characteristics at delivery.
Tables 2 and 3:
1/ For 95% CI : a dash rather than a coma
2/ As no association was observed with any covariate, my opinion is that table 2 and 3 are really optional. If the authors want to keep a table, they could gather together tables 2 and 3, and select a few major maternal and infant covariates (major here means ‘covariates which were expected to have a strong association with EBF practice at 5.25 months’).
3/ WHO HIV stage brings very few information when given again at 2 weeks (and was not presented in the table 1)
4/ Categories of infant characteristics should also be simplified.
5/ Table 4, title: delete “with any BF data”.

C/ Discretionary revisions
Table 1
1/ I think it is common in East Africa to collect and then mention ethnic group in the characteristics ….. My feeling is that ethnicity is not necessary and is not relevant in biomedical research, unless there is evidence of its association with breastfeeding practices?
2/ “Median people in house” is not necessary
3/ It would be interesting to know if a proportion of these women had already a previous infected child, if this variable is available in the data base.

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

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

No competing interest