Author's response to reviews

Title: The effect of mode of delivery on HIV-1 disease progression and mortality in a Kenyan cohort

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Author's response to reviews: see over
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RE: Revised Manuscript for BMC Pregnancy and Childbirth; MS 2023872777110212

Dear BMC Pregnancy and Childbirth Editorial team:

Please accept the third revision and resubmission of the original research paper: The effect of mode of delivery on HIV-1 disease progression and mortality in a Kenyan cohort.

We appreciate the reviewers’ questions and comments and are happy to address the issues raised.

It was requested that we make the following revisions:

#1 “In particular, the incomplete data around the CS (limited data on reasons, method, anesthesia, complications during or after surgery, blood loss etc) and VD (blood loss, use of ventouse or forceps etc) make the observations not very strong.”

Response: Thank you for this observation. As an obstetrician-gynecologist I agree with the concerns brought forth by this reviewer. As was noted by the reviewer, we do not have sufficient data to examine factors surrounding surgical technique or practices of vaginal delivery. However given the majority of deaths in this analysis occurred distant from delivery (>3 months) suggests that these difference likely did not play a major role. Additionally all women were delivered at the same institution and were managed under the same practice guidelines.

#2 “In addition, the finding that the increased rate of mortality was not related with HIV progression markers, raise the feeling that other confounding factors may have played a role.”

Response: It is correct that increased rate of mortality was not related to traditional markers of disease progression in the non-scheduled cesarean section (NSCS) group as a whole. This finding is similar to other studies that have examined this same question (Navas-Nacher EL, et al, 2006). It is in fact very likely that other factors influenced the rate of mortality and these were not measured by this study. Our plans are to pursue investigation of these factors in a study designed to specifically examine the effect of non-scheduled cesarean section on disease progression and mortality in the era of ART in Africa. The data do, however, suggest that further investigation is warranted.

#3 “As a minor concern, I am afraid that some readers may conclude from the observations of this study that the best clinical method for delivery in HIV women may be scheduled CS, as this group had the lowest rate of maternal deaths. Therefore, I suggest that when the editors decide to accept this study for publication, they may ask the authors to try to find more clinical relevant data (if available) in order to minimize the risk of confounding factors. And they may ask to include some lines in the discussion paragraph about whether the observations in this study may be strong enough or not to chance clinical practices regarding the mode of delivery.”

Response: Thank you for this very important comment. We have now included more specific wording in the discussion to clearly state that these findings do not warrant a change in clinical practice regarding mode of delivery. “These findings may influence postpartum surveillance in the non-scheduled cesarean section population but given
the limitations of the data it is not possible to make recommendations regarding mode of delivery or a change in labor management.” Please see page 10 of the manuscript.

We appreciate your comments and assistance in crafting this article. We believe it is an important study to suggest there should be further investigation into non-scheduled cesarean section in HIV-1 positive women in low-income countries. This question has not been examined previously in this setting.

We look forward to a positive response

Sincerely,

Jennifer Unger, MD, MPH