Author's response to reviews

Title: Correlates and outcomes of preterm birth, low birth weight, and small for gestational age in HIV-exposed uninfected infants

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Author's response to reviews: see over
Dear Editor,

We are grateful for the thoughtful review and the opportunity to resubmit our manuscript. Please see our responses to the Reviewers’ comments below, as well as our revised manuscript with tracked changes.

Reviewer #1:

Major Compulsory Revisions

1. Data are from 1999-2002.
   - Although old, these historic data provide an opportunity to examine correlates of preterm birth in HIV infected women, in an environment unconfounded by maternal ART, which was rarely available at the time of cohort accrual. We have added text to the limitations section to address this comment; please see page 14.

2. Should use CD4 count instead of CD4%. The latter is mostly used for pediatric patients.
   - During pregnancy blood volume increases dramatically, this results in what appears to be a precipitous drop in CD4 count in HIV-infected women. CD4 percent is less affected by this hemodilution, so we typically use CD4 percent to evaluate immunosuppression in pregnancy. We have added text to the methods section to explain why we have elected to use CD4 percent; please see page 8.

3. Since the authors restricted analysis sample for PTB to infants with Dubowitz assessment, why not also SGA. It could also suffer from misclassification if the gestational age was not estimated correctly.
   - SGA was calculated based on gestational age by Dubowitz and birth weight; so our analyses for SGA are restricted to those with both Dubowitz assessment and birth weight measured. We appreciate the comment and have clarified this in the methods section; please see page 7.

4. In the result, it says “there was a trend for increased risk of PTB in women with BV diagnosed at 32 weeks (OR=2.1, P=0.06). I assume you use trend because it is borderline significant. ‘a trend’ is not an appropriate word to use here. In discussion, the authors mentioned they found this ‘trend’ which is consistent with earlier analysis, but BV was not significantly associated with PTB in multivariate analysis.
   - We have made this correction to say that there was a non-significant increase in the odds for PTB in the univariate analysis, and deleted the corresponding sentence in the discussion, please see page 10 and 12.

5. It would be more interesting to present multivariate result in table 2, instead of univariate
   - We have added the multivariate results to Table 2.

6. In discussion, the authors stated that “These data demonstrate that PTB, LBW, and SGA may be major contributors to mortality in HIV-exposed uninfected children,” I don’t see how can the authors make this conclusion based on the analyses.
   - We have tempered this text to state more conservatively that we observed an association between PTB, LBW, SGA, and mortality; please see page 12.

Minor Essential Revisions

1. There is no page number in the manuscript
   - We have added page numbers.

Discretionary Revisions

1. In this analysis, BMI at 32 weeks was associated with outcomes. Do you have weight gain during the pregnancy for patients? It would be a better indicator for pregnant women’s nutrition status.
   - We agree that this would have been the ideal covariate for predictors of LBW, PTB, and SGA. Unfortunately we do not have pre-pregnancy weights on these women, as all of the women were enrolled during their third trimester.

Reviewer #2:
The authors have made all the suggested changes. I recommend that you accept it for publication without further revision.