PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Intimate partner violence experienced by HIV-infected pregnant women in South Africa: a cross-sectional study
AUTHORS	Bernstein, Molly; Phillips, Tamsin; Zerbe, Allison; McIntyre, James; Brittain, Kirsty; Petro, Greg; Abrams, Elaine; Myer, Landon

VERSION 1 - REVIEW

REVIEWER	Allison K Groves American University, USA
REVIEW RETURNED	06-May-2016

GENERAL COMMENTS	Introduction The introduction spends too much time on IPV in general and not enough time on IPV specific to pregnancy/postpartum and particularly IPV specifically for HIV positive women. I would reduce the length of the first two paragraphs. Then I would suggest spending more time on the literature around IPV during pregnancy/postpartum in sub-Saharan Africa (you are missing our two studies on the topic in the introduction, which specifically examine prevalence of IPV in South Africa during pregnancy and then also examine whether or not IPV changes during this time for South African women (it doesn't). Also in our article in Maternal and Child Health, we highlight other reviews of IPV during this time in the region. 1. Groves A.K., McNaughton Reyes L., Foshee V., Moodley D., Maman S. Relationship factors and trajectories of intimate partner violence among South African women during pregnancy and the postpartum period. PLOS ONE. 2014; 9(9): e106929. 2. Groves A.K., Moodley D., McNaughton Reyes L., Martin S.L., Foshee V., Maman S. Prevalence, rates and correlates of intimate partner violence among South African women during pregnancy and the postpartum period. Maternal and Child Health. 2014; 19(3): 487-495. After establishing more about what we know about the prevalence of IPV and correlates of IPV specifically during pregnancy/postpartum in this context, I would then suggest spending more time developing your rationale for looking at IPV specifically among HIV+ women. Specifically, why do you think they warrant special attention? The last sentence of the second paragraph starts to get at this, but it seems out of place – why talk about it here, before you have talked about IPV in pregnancy/postpartum? Also, you cited one of our studies in that last sentence – while we appreciate citations, the

	particular study cited is not related to adherence among HIV+ women experiencing IPV. Lastly, your models focus on maternal and demographic characteristics and then psychological characteristics. I think the introduction would be stronger if you built up the rationale for looking at the association between psychological characteristics and IPV in pregnancy/postpartum in your introduction. Further, I do not understand why you modeled these effects separately. Aren't measures of psychological distress highly correlated with some of your key relationship characteristics (e.g., pregnancy intention agreement?) Methods 1. You need a citation for the last sentence of the first paragraph in the methods section. 2. Is the eligibility criteria for the parent study or for the study for which this analysis is based on? What is the sample of the larger study and what is the sample size for the study at hand? If there is a significant difference in these two groups, why is that the case? 3. What are 'consecutive HIV infected women?' I don't understand what this means. 4. How does alcohol use fit in under 'psychological characteristics' in your models? I don't see the conceptual rationale for this. 5. Under data analysis, I would use the word 'correlates' instead of 'predictor variables' since this is a cross sectional study. 6. Also, how did you decide which relationship variables to include in your bivariate analyses and which to include in your multivariate (b/c they differ in Table 2/3)? I would set up this rationale in the introduction, and then be consistent in which variables you choose for inclusion in your models across your models. 7. Had the women in your study learned their HIV status in this pregnancy? Or had they known it for some time? This becomes clear later in the paper and you show this data in your tables, but I think it should be discussed in the methods text as well so that your reader has a sense of when women were learning about their status relative to when they are reporting IPV. Results 1. In Table 1, you look at age as a categorical variable. In Table 2, you use IQR for age, and in Table 3, it appears that age is continuous. Why all these different approaches? Why not use the same age variable in all of your analyses? 2. For Table 1, what is the rational for stratifying your analyses into the different age groups? Given that there is no significant difference in any of the types of violence across the different age

	categories, I would simplify the table. I would present the prevalence estimates for each of the three types of violence and the confidence intervals for these estimates. And I would only present the estimates for the summary variable. I would give the reader the responses to all the individual items – too much to digest. Accordingly, in the text on the results section, I would steer away from talking about differences across age categories in types of violence given that there were no significant differences in these groups. 3. For Table 2, why does 'reports of another partner in last 12 months have a n=623? I thought that was the full sample size. 4. Table 4 comes out of nowhere for me – you should set us up as readers for this analysis (which seems exploratory) earlier in the paper. Also, why present the bivariate analysis here only? Discussion 1. Our study (26) is not a review of IPV in the region – but there is one: Shamu, S., Abrahams, N., Temmerman, M., Musekiwa, A., & Zarowsky, C. (2011). A Systematic Review of African Studies on Intimate Partner Violence against Pregnant Women: Prevalence and Risk Factors. PLOS ONE, 6(3), e17591. http://doi.org/10.1371/journal.pone.0017591 Would be good to use that study as a comparison for your findings. Also may want to comment in your discussion on the fact that your estimates were consistent with other estimates for HIV+ women – what does this mean about their vulnerability to IPV during this time in comparison to women without HIV? 2. The hypothesis presented in the discussion is not discussed previously in the paper – this hypothesis should be stated in the introduction and the rationale for the hypothesis should also be developed in the introduction prior to theorizing about why the hypothesis was not supported. Also, it seems from the paragraph in the discussion that you think that there might be an association between when HIV was diagnosed and IPV for women who have and have not disclosed their status – did you consider testing this as an interaction? 3. How pregnant were women at time of completing the survey? Your measure of IPV gets at IPV in the last year, which means that some of the IPV you are capturing is not specific to pregnancy. and also that you do not have a sense of whether IPV preceded HIV diagnosis or followed HIV diagnosis or both for women who received an HIV diagnosis in the current pregnancy. This limitation in terms of the measurement of your outcome variable dampens my enthusiasm for the manuscript at least as it is currently framed.

REVIEWER	Donna Hubbard McCree, PhD, MPH, RPh Associate Director for Health Equity Division of HIV/AIDS Prevention National Center for HIV/AIDS, Viral Hepatitis, STD, and TB Prevention Centers for Disease Control and Prevention

	1600 Clifton Road, NE MS D-21 Atlanta, Georgia 30333 United States of America The findings and conclusions in this report are those of the authors and do not necessarily represent the official position of the Centers for Disease Control and Prevention.
REVIEW RETURNED	18-May-2016

GENERAL COMMENTS	General Comments: The manuscript is well written and covers an important topic that is of interest to the field. The Reviewer's suggested edits follow. Abstract – (1) The Abstract does not include a Methods section perhaps because of the journal's formatting style. If allowed, the Reviewer suggests including a brief Methods section that describes the study procedures and data analyses. (2) Conclusions, Line 33: While no level of IPV is acceptable, given the results, the Reviewer is not sure that use of the word "high' to describe the level of IPV experienced by women in the study is supported by the data. Please consider editing the sentence as follows: HIV-infected pregnant women in the study reported experiencing multiple forms of IPV. Introduction - (1) Background, Page 5, Line 6: Please include a definition for IPV specifically because a goal of the study is to describe the prevalence of different forms of IPV during the past 12 months. (2) General Comment: Please consider reorganizing and editing the Introduction section for greater clarity. The current version is somewhat difficult to follow. The study population is HIV-infected pregnant women. The 1st paragraph of this section provides background on the prevalence of IPV among women in sub-Saharan Africa. The 2nd paragraph reviews the associations between IPV and specific psychological, drug use and biomedical outcomes including HIV infection. The 3rd paragraph is about IPV in the context of pregnancy and the data presented are not focused on HIV-infected women. The final paragraph provides a justification for and includes the purpose of the study. The authors state that prevalence data on IPV in HIV-infected pregnant women are few, but do not include information about these data. The Reviewer suggests editing the Introduction section to include a definition of IPV (as it relates to this study) in the 1st paragraph along with a brief review of the literature on experiences with IPV among women and pregnant women in sub-Saharan Africa. The 2nd paragraph could include a brief review of the literature on associations between experiences with IPV and key psychological, drug use and biomedical outcomes, including HIV. The final paragraph could include a review of the literature on IPV in HIV-infected women in sub-Saharan Africa and the purpose statement. Methods (1) Page 6, Line 1: Eligible consecutive HIV-infected pregnant women were enrolled in the study. The sample included women who were diagnosed prior to the current pregnancy and before the current pregnancy. This is the only information provided on the timing of the diagnoses, previous versus current pregnancy, and no

	significant associations were found between time of diagnosis and reported experiences with IPV. However, did the authors collect data on the length of time that women were aware of their infection and explore the associations between length of time and the study outcomes? If so, what were the results? (2) Measures, Page 6, Line 9: Please consider providing additional specifics about how the data were collected. This information may be useful in determining the effect of social desirability on the study results. Discussion (1) Page 8, Line: While no level of IPV is acceptable, as previously mentioned, the Reviewer is not sure the results suggest that IPV is a significant burden among this population based on the self-reported data in the manuscript. The 21% rate in the current study is less than the 30% rate among women in sub-Saharan Africa (WHO, 2013) and the 28% among pregnant women globally (WHO, 2012). Further, the results are compared to the general literature on IPV among pregnant women. Please consider including a comparison between these results and previous studies of IPV among HIV-infected women in sub-Saharan Africa. (2) Page 9, Lines 06-07: Please see my previous statement about use of the term “high” to describe the IPV prevalence rate. The references cited for this sentence are not data for HIV-infected pregnant women. Please consider how the study findings compare with the few studies that include HIV-infected pregnant women in sub-Saharan African. (1) Page 9, Lines 14-15: This sentence is the first time that readers are made aware of your hypothesis. I suggest including the hypothesis in the Introduction section. Additionally, the finding regarding disclosure is interesting. Sixty-four percent of the women reported not disclosing their HIV status to their partners. There may be an interaction between disclosure and length of time women were aware of their diagnoses that could potentially affect the association between disclosure and experiences with IPV. I suggest exploring this possibility if the data are available and including results in the manuscript to better inform the Discussion section. (2) Page 10, Line 4: Please consider including specifics about the study procedures utilized to reduce the possibility of underreporting. Tables – Please consider adding the location (or other appropriate identifier) and dates of the study to the Title of each Table.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Introduction

The introduction spends too much time on IPV in general and not enough time on IPV specific to pregnancy/postpartum and particularly IPV specifically for HIV positive women. I would reduce the length of the first two paragraphs. Then I would suggest spending more time on the literature around IPV during pregnancy/postpartum in sub-Saharan Africa (you are missing our two studies on the topic in the introduction, which specifically examine prevalence of IPV in South Africa during pregnancy and then also examine whether or not IPV changes during this time for South African women (it doesn’t). Also in our article in Maternal and Child Health, we highlight other reviews of IPV during this time in the region.

1. Groves A.K., McNaughton Reyes L., Foshee V., Moodley D., Maman S. Relationship factors and trajectories of intimate partner violence among South African women during pregnancy and the postpartum period. PLOS ONE. 2014; 9(9): e106929.
2. Groves A.K., Moodley D., McNaughton Reyes L., Martin S.L., Foshee V., Maman S. Prevalence, rates and correlates of intimate partner violence among South African women during pregnancy and the postpartum period. Maternal and Child Health. 2014; 19(3): 487-495.

After establishing more about what we know about the prevalence of IPV and correlates of IPV specifically during pregnancy/postpartum in this context, I would then suggest spending more time developing your rationale for looking at IPV specifically among HIV+ women. Specifically, why do you think they warrant special attention? The last sentence of the second paragraph starts to get at this, but it seems out of place – why talk about it here, before you have talked about IPV in pregnancy/postpartum? Also, you cited one of our studies in that last sentence – while we appreciate citations, the particular study cited is not related to adherence among HIV+ women experiencing IPV. Lastly, your models focus on maternal and demographic characteristics and then psychological characteristics. I think the introduction would be stronger if you built up the rationale for looking at the association between psychological characteristics and IPV in pregnancy/postpartum in your introduction. Further, I do not understand why you modeled these effects separately. Aren't measures of psychological distress highly correlated with some of your key relationship characteristics (e.g., pregnancy intention agreement?)

◊ We appreciate these comments and new references to add. We have reworked parts of the introduction to address these comments, including the rationale for psychological characteristics examined in the paper. Specifically, we have: reduced the length of the first two paragraphs; included the references to the reviewer's publications (which were cited elsewhere already); emphasized why we think examining IPV among HIV+ women is important in public health terms; and included rationale for examining psychological factors and IPV. And in response to the final point raised here, we did not find that psychological distress was correlated with pregnancy intention agreement.

Methods

1. You need a citation for the last sentence of the first paragraph in the methods section.

◊ Thank you for this. We have added a reference here.

2. Is the eligibility criteria for the parent study or for the study for which this analysis is based on? What is the sample of the larger study and what is the sample size for the study at hand? If there is a significant difference in these two groups, why is that the case?

We have clarified the relationship between this sample and the larger study – which involves follow-up measures that are not included here, but the Ns are similar.

3. What are 'consecutive HIV infected women?' I don't understand what this means.

◊ This means consecutive patients (ie, a total sample). We have clarified in the manuscript.

4. How does alcohol use fit in under 'psychological characteristics' in your models? I don't see the conceptual rationale for this.

◊ Alcohol dependence/abuse as used here is a psychological construct (eg, this is a disorder defined by the DSM). We have clarified here.

5. Under data analysis, I would use the word ‘correlates’ instead of ‘predictor variables’ since this is a cross sectional study.

◊ We appreciate this point and have made this change.

6. Also, how did you decide which relationship variables to include in your bivariate analyses and which to include in your multivariate (b/c they differ in Table 2/3)? I would set up this rationale in the introduction, and then be consistent in which variables you choose for inclusion in your models across your models.

◊ Following standard practice, independent variables were retained in models if they were persistently associated with IPV, and/or if their removal altered associations involving other covariates. We have added this this in the methods section to address the reviewer’s comment.

7. Had the women in your study learned their HIV status in this pregnancy? Or had they known it for some time? This becomes clear later in the paper and you show this data in your tables, but I think it should be discussed in the methods text as well so that your reader has a sense of when women were learning about their status relative to when they are reporting IPV.

◊ Table 2 describes the timing of HIV diagnosis vs the current pregnancy, but we agree and have added this information to the methods.

Results

1. In Table 1, you look at age as a categorical variable. In Table 2, you use IQR for age, and in Table 3, it appears that age is continuous. Why all these different approaches? Why not use the same age variable in all of your analyses?

◊ We have used age in different forms based on the context of the analysis – and in doing so hopefully provide more insight into the study results. If the reviewer feels strongly about changing this, we would be happy to do so but are not sure using a single form for a variable for its own sake will add insight.

2. For Table 1, what is the rational for stratifying your analyses into the different age groups? Given that there is no significant difference in any of the types of violence across the different age categories, I would simplify the table. I would present the prevalence estimates for each of the three types of violence and the confidence intervals for these estimates. And I would only present the estimates for the summary variable. I would give the reader the responses to all the individual items – too much to digest. Accordingly, in the text on the results section, I would steer away from talking about differences across age categories in types of violence given that there were no significant differences in these groups.

◊ We appreciate the reviewer’s comments on this. Age is an overarching factor that shapes social and biological risk related to IPV, HIV and many of the other factors evaluated here. We feel that presenting age-stratified results is not inappropriate at all in this context, and indeed it may be considered noteworthy that there are not strong age-related trends. So we would prefer to leave age-stratified results for Table 1. However if the editors feel strong about this we are happy to reconsider.

3. For Table 2, why does ‘reports of another partner in last 12 months have a n=623? I thought that was the full sample size.

◊ We agree that reporting this N is not necessary, and have removed.

4. Table 4 comes out of nowhere for me – you should set us up as readers for this analysis (which seems exploratory) earlier in the paper.

◊ We appreciate this remark, and have done our best to present the table to reader so as to avoid surprise.

Discussion

1. Our study (26) is not a review of IPV in the region – but there is one: Shamu, S., Abrahams, N., Temmerman, M., Musekiwa, A., & Zarowsky, C. (2011). A Systematic Review of African Studies on Intimate Partner Violence against Pregnant Women: Prevalence and Risk Factors. PLoS ONE, 6(3), e17591. http://doi.org/10.1371/journal.pone.0017591

◊ Thank you for this note. We did not cite this as a review, but as an example of a study with a consistent finding. However we have changed the reference accordingly.

2. The hypothesis presented in the discussion is not discussed previously in the paper – this hypothesis should be stated in the introduction and the rationale for the hypothesis should also be developed in the introduction prior to theorizing about why the hypothesis was not supported. Also, it seems from the paragraph in the discussion that you think that there might be an association between when HIV was diagnosed and IPV for women who have and have not disclosed their status – did you consider testing this as an interaction?

◊ We appreciate this comment, and have attempted to enhance in the discussion the findings for the original hypothesis and the reasons for these.

3. How pregnant were women at time of completing the survey? Your measure of IPV gets at IPV in the last year, which means that some of the IPV you are capturing is not specific to pregnancy. and also that you do not have a sense of whether IPV preceded HIV diagnosis or followed HIV diagnosis or both for women who received an HIV diagnosis in the current pregnancy. This limitation in terms of the measurement of your outcome variable dampens my enthusiasm for the manuscript at least as it is currently framed.

◊ We agree with this comment and have added as a limitation to the discussion, noting the data on gestation at the time of the interview as presented in the results. While we appreciate that the reviewer may not be enthusiastic about this work, we feel that the manuscript is still valuable in shedding light on the burden of IPV in this population and its correlates.

Reviewer: 2

The manuscript is well written and covers an important topic that is of interest to the field.

◊ Thank you.

Abstract

(1) The Abstract does not include a Methods section perhaps because of the journal's formatting style. If allowed, the Reviewer suggests including a brief Methods section that describes the study procedures and data analyses.

◊ We have structure the abstract in keeping with journal guidance, but can rework if the editor would prefer.

(2) Conclusions, Line 33: While no level of IPV is acceptable, given the results, the Reviewer is not sure that use of the word “high’ to describe the level of IPV experienced by women in the study is supported by the data. Please consider editing the sentence as follows: HIV-infected pregnant women in the study reported experiencing multiple forms of IPV.

◊ We appreciate this comment and have made the requested change.

Introduction

(1) Background, Page 5, Line 6: Please include a definition for IPV specifically because a goal of the study is to describe the prevalence of different forms of IPV during the past 12 months.

◊ We have included the IPV definition used by the WHO for this.

(2) General Comment: Please consider reorganizing and editing the Introduction section for greater clarity. The current version is somewhat difficult to follow. The study population is HIV-infected pregnant women. The 1st paragraph of this section provides background on the prevalence of IPV among women in sub-Saharan Africa. The 2nd paragraph reviews the associations between IPV and specific psychological, drug use and biomedical outcomes including HIV infection. The 3rd paragraph is about IPV in the context of pregnancy and the data presented are not focused on HIV-infected women. The final paragraph provides a justification for and includes the purpose of the study. The authors state that prevalence data on IPV in HIV-infected pregnant women are few, but do not include information about these data. The Reviewer suggests editing the Introduction section to include a definition of IPV (as it relates to this study) in the 1st paragraph along with a brief review of the literature on experiences with IPV among women and pregnant women in sub-Saharan Africa. The 2nd paragraph could include a brief review of the literature on associations between experiences with IPV and key psychological, drug use and biomedical outcomes, including HIV. The final paragraph could include a review of the literature on IPV in HIV-infected women in sub-Saharan Africa and the purpose statement.

◊ In keeping with these comments and those of Reviewer #1, we have edited the introduction substantially. We feel that these edits address the requested changes, but are happy to consider further edits.

Methods

(1) Page 6, Line 1: Eligible consecutive HIV-infected pregnant women were enrolled in the study. The sample included women who were diagnosed prior to the current pregnancy and before the current pregnancy. This is the only information provided on the timing of the diagnoses, previous versus current pregnancy, and no significant associations were found between time of diagnosis and reported experiences with IPV. However, did the authors collect data on the length of time that women were aware of their infection and explore the associations between length of time and the study outcomes? If so, what were the results?

◊ We have included more information on the collection of data regarding timing of diagnosis. Further data on duration of HIV diagnosis was not available.

(2) Measures, Page 6, Line 9: Please consider providing additional specifics about how the data were

collected. This information may be useful in determining the effect of social desirability on the study results.

◊ We appreciate this comment and have attempted to strengthen the reporting of the methods. We are happy to include more detail beyond this, if the reviewer would like.

Discussion

(1) Page 8, Line: While no level of IPV is acceptable, as previously mentioned, the Reviewer is not sure the results suggest that IPV is a significant burden among this population based on the self-reported data in the manuscript. The 21% rate in the current study is less than the 30% rate among women in sub-Saharan Africa (WHO, 2013) and the 28% among pregnant women globally (WHO, 2012). Further, the results are compared to the general literature on IPV among pregnant women. Please consider including a comparison between these results and previous studies of IPV among HIV-infected women in sub-Saharan Africa.

◊ While we do feel that these levels are high in this population, we have avoided the term “high” to address the reviewer’s concern.

(2) Page 9, Lines 06-07: Please see my previous statement about use of the term “high” to describe the IPV prevalence rate. The references cited for this sentence are not data for HIV-infected pregnant women. Please consider how the study findings compare with the few studies that include HIV-infected pregnant women in sub-Saharan African.

◊ Per the response to the above point, we appreciate the reviewer’s comment and have attempted to remove the term “high” from the results, and to contextualise the data better in terms of the existing literature.

(1) Page 9, Lines 14-15: This sentence is the first time that readers are made aware of your hypothesis. I suggest including the hypothesis in the Introduction section.

◊ We agree with this point, and added accordingly to the introduction.

(2) Additionally, the finding regarding disclosure is interesting. Sixty-four percent of the women reported not disclosing their HIV status to their partners. There may be an interaction between disclosure and length of time women were aware of their diagnoses that could potentially affect the association between disclosure and experiences with IPV. I suggest exploring this possibility if the data are available and including results in the manuscript to better inform the Discussion section.

◊ We agree with that this is interesting, and would be worth exploring further. Unfortunately we do not have the necessary data for this, however, and have noted in the discussion as an interesting point for future attention.

(3) Page 10, Line 4: Please consider including specifics about the study procedures utilized to reduce the possibility of underreporting.

◊ We appreciate this comment, and have added details accordingly.

Tables – Please consider adding the location (or other appropriate identifier) and dates of the study to the Title of each Table.

◊ We agree with this point and have made the appropriate changes to the title of each table and figure.

VERSION 2 – REVIEW

REVIEWER	Dr. Donna Hubbard McCree Division of HIV/AIDS Prevention Centers for Disease Control and Prevention United States of America
REVIEW RETURNED	19-Jul-2016

GENERAL COMMENTS	The authors addressed my comments in the revised manuscript. My only remaining suggestion is to add study dates to the titles of each Table (if permitted by the Journal).