Author's response to reviews

Title: Actor-partner effects associated with experiencing intimate partner violence or coercion among male couples enrolled in an HIV prevention trial

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Author's response to reviews: see over
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Title: Actor-partner effects associated with experiencing intimate partner violence or coercion among male couples enrolled in an HIV prevention trial

To the editor of BMC Public Health,

We wish to thank you for the opportunity to revise and resubmit our article, and feel the incorporation of reviewer comments has greatly added to the clarity and value of our manuscript. Our comments, responses, and rebuttals to reviewer comments are indicated below in bold italics.

Best,

[Signature]

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Reviewer’s report
Title: Actor-partner effects associated with experiencing intimate partner violence or coercion among male couples enrolled in an HIV prevention trial
Version: 1 Date: 4 June 2013
Reviewer: Christian Grov

Reviewer’s report:
Overview: This study looked at 95 MSM couples (dyads, N = 190 individuals) to investigate factors associated with intimate partner violence. The authors used the actor-partner independence model, which is appropriate given their dyadic data. As the authors note (and to my knowledge), there is scant data on IPV using dyadic data, thus this manuscript has much promise, even though analyses were exploratory in nature and they were underpowered for some analyses. The manuscript is very well written, the study design seems solid, and the statistics seem accurate. This reviewer is familiar with the AIPM, but less so with the specific analytic steps needed to run a model. The results seem sound, but I would encourage the editor to seek out additional statistical review for verification. I have only minor concerns that the authors might consider, but would not say are “required” to.

Discretionary Revisions:
The methods indicate that participants were assessed separately. I would add more detail. Were they in separate rooms? (especially for something like IPV, it would be important that dyads couldn’t “intimidate” each other into false responses… which could happen if they were completing the survey “separately” but in the same space.. if that makes sense).

The individuals of each couple were assessed separately in that they completed the survey in opposite areas of one large room, generally at a distance of about 20 feet. Study administrators oversaw that the couples completed the surveys separately. This clarification has been added to the text.

I prefer to describe the association between variables as an “association” (not relationship). Especially in a paper about relationships between couples, perhaps best to use the term “association” when talking about variables and “relationship” when talking about people.

The term “relationship” has been reserved for people, and the term “association” for variables as per the reviewer’s suggestion.

On the first page of the discussion, the authors discuss unpublished data which suggest that different groups may define IPV differently. I am a little uncomfortable with the unpublished nature of these data. Is there anything more concrete/empirical that could be said instead? Or just say something to the effect that it is well known in the literature that variables/scales/experiences can be interpreted differently based on a person’s race (and cite appropriately).

The reference to the unpublished findings has been removed from the text.

Major Compulsory Revisions: None
Minor Essential Revisions: None
Level of Interest: High
Level of interest: An article of importance in its field
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’

Reviewer’s report
Title: Actor-partner effects associated with experiencing intimate partner violence or coercion among male couples enrolled in an HIV prevention trial
Version: 1 Date: 10 June 2013
Reviewer: Katie Edwards

Reviewer’s report:
Thank you for the opportunity to review "Actor-partner effects associated with experiencing intimate partner violence or coercion among male couples enrolled in an HIV prevention trial" submitted for publication to BMC Public Health. The manuscript was generally well-written and reports data on a critically important and timely topic. Below I list a number of suggestions-divided into discretionary revisions, minor essential revisions, and major compulsory revisions-that I believe would improve the manuscript.

Discretionary Revisions
1. The supplementary appendices seem unnecessary and in general there were too many details about the analyses.

Since we applied a relatively complex modeling method that many readers may not be familiar with, we believe that the appendices will be interesting and instructive to some readers.

2. Minor typographical errors and at times colloquial writing that if corrected would improve overall readability and sophistication of the paper.

We have revised the paper to correct typographical errors and improve the writing.

3. DISCUSSION: Consider mentioning some hypothesized reasons (e.g., money) why an abuser might coerce his partner to participate in research. I kept wondering this throughout the paper.

Given the lack of literature regarding why a partner may coerce another person to participate in a research study such as this, we declined to hypothesize reasons.

Minor Essential Revisions
4. ABSTRACT: Briefly define APIM, even if just in a phrase

The APIM has been briefly described in the abstract as “...an analytic framework to describe interdependent outcomes within dyads.”

5. BACKGROUND: give examples of "subgroups" (1st paragraph)

“Subgroups” could refer to young, black MSM, MSM couples, MSM IDUs, etc. We have decided not to specify and have removed this verbiage.

6. BACKGROUND: Consider including new national prevalence data from NISVS on IPV among LGB individuals. This is more recent than NVAW data. See:
http://www.cdc.gov/violenceprevention/nisvs/specialreports.html

We appreciate being directed to this reference and agree that this report contains a wealth of more recent data. However, upon consideration of including these more recent numbers, we have decided to keep with reporting the NVAWS given that the NISVS did not have enough sample size to estimate sexual violence among either MSM (bisexual or gay), or among heterosexual males (Table 4, http://www.cdc.gov/violenceprevention/pdf/nisvs_sofindings.pdf). The only reportable estimates of sexual violence were inclusive of physical violence and stalking behavior.

7. BACKGROUND: The paragraph beginning with "A limited number of cross-sectional studies have identified factors" would be improved if previous research was more integrated. Rather than listing sentence after sentence a different study, the authors should integrate main findings across different studies.
We chose to list the studies instead of integrating the findings to highlight the differences in the definitions of IPV used in each study. As discussed in the text, we believe this is a major impediment to the comparison and integration of study findings.

8. BACKGROUND: Define serostatus the first time it is mentioned in the paper.

This definition has been added.

9. METHODS: Please include information about the socioeconomic status of the sample.

We did not collect data on participant income. Our primary proxy measure of socioeconomic status, as reported in the manuscript, is education.

10. METHODS: What exactly does "couples in which either partner reported these exclusionary criteria were informed, without explanation, that they would receive individual testing in the study" mean? Who was tested together? What was the rationale for separate versus individual testing (e.g., safety?), and rationale for no explanation? This is confusing and should be clarified and justified.

If either partner reported IPV or coercion, they were not eligible for randomization due to potential risks associated with violence and threats to human subjects. These couples were not tested and counseled jointly but were instead given individual testing, and were not informed that one or both partners had reported IPV or coercion. They were not informed due to safety concerns. This clarification has been made in the text.

11. METHODS: Why was a single item used rather than the entire CTS2?

We used two items from the CTS to assess recent physical or sexual violence ("In the past 3 months, has ___ hit you, kicked you, or physically hurt you?" and "In the past 3 months has ___ ever used force (hitting, holding down, or using a weapon) to make you have oral or anal sex?"). These two questions, in addition to the coercion item, were considered to be sufficient to assess for IPV/coercion as exclusionary criteria for the study. As these were not the main study outcomes, we did not more thoroughly or comprehensively assess for IPV. This has been added to the study limitations section.

12. METHODS: "There is no meaningful way to distinguish outcomes between individual members of the dyad" - what does this mean?

The APIM can accommodate both indistinguishable and distinguishable dyads, and the modeling approaches for each differ. Identifying dyads as indistinguishable or distinguishable is one of the first steps in the APIM modeling approach. For example, when the partners are heterosexual couples, one natural way to distinguish the dyad is gender (e.g., husband and wife). In the distinguishable dyad APIM, a distinguishing factor is included in the analysis, and tests of distinguishability are conducted. In indistinguishable dyads, partners are not considered to have a natural distinguishing factor, are considered exchangeable, and are statistically indistinguishable.

13. RESULTS: Descriptions of grouping races is confusing. Throughout the paper it appears as if the variable was Black/African American compared to All other Races, but in other places (p. 11), Caucasian/White appears to be the reference group? Please clarify. Along these lines, the authors' state race analyses, but they never indicate (until Table 1) how many people of the different races are included in the study.

On page 11, we are justifying combining the “white/Caucasian” race group with the “other” race group. The number of different races included in the study is provided on this page as well. White/Caucasians are never used as a reference group and are combined with the “other” race group for the entirety of the analysis.
14. RESULTS: Why are the authors using p<0.1 for their cutoffs? This seems like the authors did not find anything at the p<0.05 level, so they went more generous. Please clarify and justify.

As described in the manuscript, we decided (a priori) to use a p value of 0.1 as these were exploratory analyses and we were willing to accept weak signals. The fact that these results should be interpreted with caution, given the exploratory nature of the analysis and since the study was not powered to detect the outcomes of IPV or coercion, has been expanded on in the discussion section.

15. DISCUSSION. The section on screening tools and support service development seems loosely connected to the current study's findings. Either more directly connect or eliminate from the paper. Along these lines, I thought the Discussion could be generally improved if there were more direct connections between study findings and research/intervention implications.

Our rationale for the section on screening tools is that most tools have not been designed or validated among MSM. Understanding factors associated with IPV and coercion among MSM via studies such as this is critical to the development of better tools. Similarly, we feel that MSM violence support services are inadequate and could also benefit from a better understanding of the types of MSM couples experiencing violence. We conclude that “This preliminary understanding actor-partner effects and dyadic differences related to IPV and coercion suggests that screening tools and support services can benefit from an understanding of both actor and partner effects, and that they may benefit from targeting younger, less educated MSM.” Though our findings are preliminary, we believe that other, larger studies designed to better understanding these associations should be conducted to inform the development of screening tools and support services. This has been added to the discussion, and we have tried to better develop the role of our findings to inform future research and interventions.

Major Compulsory Revisions

16. A significant weakness of the paper is the lack of theoretical underpinning connecting IPV, MSM dyads, and HIV prevention. In the BACKGROUND section of the paper, the authors discuss HIV and MSM and also IPV and MSM but never connect the three of these phenomena together, which is needed. What is the importance of studying IPV within the context of HIV prevention among MSM?

We appreciate this reviewer’s comment and feel that by addressing it, we have strengthened the manuscript considerably. Though data is limited, the reporting of experienced IPV by MSM has been strongly correlated with HIV/STI risk and more generally sexual risk-taking. Particularly, reporting experiencing of any form of IPV [1-4], of physical IPV [4], of sexual IPV [4, 5], and of psychological/emotional IPV [6] is correlated with recent unprotected anal intercourse (UAI). This underscores the potential role of IPV as a significant risk factor for HIV among MSM. This information has been added to the background section.

17. In in the BACKGROUND section, the authors review coercion in relationships more broadly, but in the METHOD it becomes clear they only assessed research participation coercion. This should be introduced in the introduction and another reason the specific aims and study variables should be integrated into a more cohesive conceptual framework.

Most definitions of coercion are generally defined as behavioral or mental coercive control, and one aspect of coercion may be related to coercion to participate in research or obtain health services. We agree with the reviewer that this point should be discussed in the introduction.

18. More on the APIM is needed in the BACKGROUND section of the paper. Exactly how has it been used with other health outcomes? Why is the APIM important for MSM, HIV, and IPV? I think stating WHY and HOW the APIM could be useful to inform research, theory, practice, etc. is more convincing than just stating it has not been studied regarding IPV among MSM.
The APIM has been used to study dyadic relationships and outcomes related to dyads. Actor-partner effects among MSM have been evaluated for various health outcomes related to HIV risk including UAI within and outside the relationship [7], agreements about sex outside the relationship [8], and main and casual partner selection related to sero-sorting [9]. We do not feel that the APIM is important specifically for studying MSM, HIV, or IPV. We feel that it is a generally important methodology for studying all dyadic relationships and important outcomes that are influenced by the characteristics and actions of different members of a dyad, as well as measurements of their relationship to each other. We have tried to clarify the importance of the APIM in dyadic level modeling in the introduction.

Level of interest: An article of importance in its field
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

Reviewer's report
Title: Actor-partner effects associated with experiencing intimate partner violence or coercion among male couples enrolled in an HIV prevention trial
Version: 1 Date: 9 September 2013
Reviewer: Paul Hewson
Reviewer's report:
This is a well written article, and is clearly on an important topic. The problem is the small sample size, and I believe this is actually a subset analysis which is always a risky business. I'm not sure much of the inferential results have the validity claimed. Unless I'm mistaken, this is not a study "190 individuals (95 couples)", it's a study of a much larger number of individuals from whom this subset has been selected after other analyses have taken place.

We appreciate the reviewer's comments. The “parent” RCT was, in fact, comprised of 190 individual (95 couples). Therefore this is not a subset analysis. The results of the RCT have recently been published and that reference has been added to the manuscript in order to clarify the point.

I'll let the subject matter experts comment on the point, but given the inclusion criteria seem to include "reported willingness to complete a 3 month follow-up survey" I don't understand how one of the outcomes could logically have been partner coercion in completing study. Doesn't this put these respondents outside the inclusion criteria and hence they should not form part of the dataset for analysis.

If respondents reported being coerced, they were provided individual counseling and testing (instead of couples’ counseling and testing), and were still administered the 3 month follow-up survey. These individuals were not eligible for randomization and did not form part of the primary analysis. However, they were eligible for this analysis.

Major revisions.
The article needs to be rewritten with the CONSORT statement in view.

We did not write this paper or conduct the analysis using the CONSORT framework because we are not presenting the results of an RCT.

Presumably the primary outcome of the parent study is the one powered for analysis. I accept that the data are somewhat complex to describe any other way, but feel subset analyses cannot be presented as
if it were designed to see whether education levels were associated with IPV. I don't see how any findings from this study can be anything other than speculative. For myself, I would have preferred to do a complete analysis of the dataset, and examine whether the subset examined in this paper clearly differed from the rest of the population.

The reviewer is correct. The RCT was powered to detect the study’s primary outcome and not the outcomes of IPV or coercion. Preliminary findings from this RCT have recently been published (Sullivan et al. Safety and Acceptability of Couples HIV Testing and Counseling for US Men Who Have Sex with Men: A Randomized Prevention Study). The fact that these results should be interpreted with caution, given the exploratory nature of the analysis and since the study was not powered to detect the outcomes of IPV or coercion, has been added in the discussion section.

Level of interest: An article of limited interest
Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:
I have no competing interests.
REFERENCES


