Author's response to reviews

Title: The Intersection between Sex and Drugs: A Cross-Sectional Study among the Spouses of Injection Drug Users in Chennai, India

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Author's response to reviews: see over
Shruti H. Mehta, Associate Professor, Department of Epidemiology

October 18, 2010

Dear Dr. Heimer,

Thank you for the opportunity to improve our manuscript. We have responded to comments of all three reviewers and have made the corresponding changes in a revised manuscript. We have also included our study questionnaire as requested.

Reviewer #1

Major compulsory revision:

1. The Background section can be strengthen by highlighting the need to understand HIV, HBV, HCV prevalence among wives of India/Chennai IDUs, as well as their risk behavior and risk factors. The discussion has some interesting points about risk behavior, implication to future epidemic and disease control strategies, but no related introduction up front. The current arguments for this paper need to be better organized to be strong and clear. For example, authors seems imply that prevalence data of this population is lacking by ONLY mentioning their own teams’ prior study results without giving more general background. The size of the target population and its implication to HIV epidemic and intervention efforts in the nation is insufficiently highlighted. Related to this comment, the Abstract Background section could be improved with more general background beyond the team’s prior study.

We have revised/expanded the background section to summarize other studies among the sexual partners of IDUs beyond our own. We have also reframed the objective of the paper to characterize the prevalence of blood-borne infections and the risk environment among this group – the spouses of IDUs in India. Within this new framework, we provide more context and rationale for this study. Unfortunately, there are not good estimates on the target size of this population. There are two estimates of the number of IDUs in India but they are very divergent as pointed out by reviewer #3. We have also included other relevant references of HIV prevalence among IDUs in India.

2. This reviewer is a little uncomfortable with the statement that “... making them [IDUs] an important bridge population” without sufficient support and that “Transmission is likely further exacerbated by...one sex partner, the epidemic is less likely to spread quickly to the rest of general populations, therefore the use of “important bridge” sounds stretching, and I can’t picture what this fact could “exacerbate” transmission?

We have revised these statements to remove the word “bridge population” and exacerbated. Our intention was to highlight the IDUs (not their wives) as a bridge population given that they are at high risk for transmission of HIV to their wives and sexual partners of IDUs. It is a good point that the wives of the IDUs are unlikely to transmit to other sexual partners since the majority of these women have only one partner. However, they are at risk for transmitting infection to their children. Nevertheless, we have softened this language as per the reviewer’s request.
3. The Abstract “Background” section need something broader than the team’s prior study. The Abstract “Methods” should point out that the sample is wives of prior study IDUs. The readers need to know that this sample was recruited years after the prior IDU sample. Otherwise, it is confusing to see that 13% were widowed.

We have revised the Abstract according to the reviewer’s recommendations. Due to the word limit, we could only make one statement of introduction but we have revised it to be more encompassing of the body of literature on this topic rather than the specific rationale for our own study. We have also provided an additional statement of clarification in the methods on the timeline of the study to reflect that the sample was recruited nearly three years after the sample of IDUs.

Minor Essential Revisions:

1. Each table title should have enough information describing the sample (e.g. Sample size, population from which the sample was drawn, location).

This additional information has been added.

2. Find a way to keep figure titles and the figure together.

The figures and figure legends are separated as per the requirements of the journal.

3. Needs proof reading to clarify some language problems

We have gone through the text in some detail to revise any perceived language concerns.

Discretionary Revisions:

1. What’s the point of the last sentence of the second paragraph? Sympathetic about wives of risk because they are “innocent”? or trying to point out an area that could be ignored by some public health professionals or researchers?

The point of this sentence is to convey the point that the wives of IDUs are a population of married monogamous women who do not perceive themselves to be at high risk for HIV. In India, it has been recognized that there are large groups of women whose only risk factor for HIV is sex with their husband. Because they are not a traditional ‘high risk’ group, prevention and treatment efforts are not typically directed at them. We have softened this statement.

2. In the middle of the second paragraph under “Study population”, how were IDUs encouraged to bring their wives? Did and how staff address IDUs’ fear?

There was no coercion in the recruitment of wives by their husbands. The IDUs were simply told that if they brought in their wives and their wives were eligible for the study, they (their spouses) could receive an HIV, HCV and HBV test. This has been clarified in the text. They were also told that staff would not share the HIV, HCV and HBV status of the IDUs with their wives unless requested by the IDUs themselves. As many IDUs reported to us that they wanted to disclose their status to their wives but did not know how to, all participants of the MIDACS were offered assistance with disclosure/couples counseling if it was so desired.

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3. In the 3rd paragraph of the same section, how the 400 were “pre-determined”? The first 400 staff able to contact? Or random selection of IDUs?

The sample size of 400 was pre-determined based on budgetary constraints, the timeline of the grant that funded the project and a sample size calculation. The sample size was chosen to based on precision of the HIV prevalence estimate and a convenience sample was accrued. We have revised the text to clarify this and now state that ‘a convenience sample of predetermined size was recruited.’

Reviewer #2

Major compulsory revisions.

1. First, the examination of interpersonal violence (“social consequences”) seems an afterthought throughout most of the manuscript. For example, it is not described in Background and no rationale is provided for this focus. In Methods, the measures are not described (e.g. “perceptions of the impact of their husband’s drug use behavior on family” is not further elaborated). I suggest a more thoughtful integration into the manuscript, including a review of the literature in the Introduction. It is better integrated in the Discussion, but perhaps more could be said earlier in the manuscript. There are also minor inconsistencies in the manuscript on this topic; for example, “social consequences” often refers to IPV, but occasionally there are mentions of economic deprivation. This should be clarified and consistent throughout. Finally, I suggest terminology other than “social consequences” as there is no evidence these factors result directly from the injection drug use of their partners despite the strong association. If the authors believe it is a direct consequence, this could be better explained.

Thank you for this comment. We agree and have tried to integrate a more focused discussion of this additional data into the introduction. We have change the term social consequences to risk environment to better describe the context in which these women live. The idea of the risk environment incorporates both violence and poverty. We have removed any statements that suggest that this is ‘caused’ by drug use as we agree that causality cannot be inferred from this study. We have also provided better detail on the measures used to collect this data.

2. Second, while I think the descriptive analysis is largely appropriate, there are some key descriptives that are missing. Even if it is only anecdotal, it will be important to present the other risk factors for HIV among the 10 women who were HIV positive. How many of these women reported multiple partners, transactional sex, or history of drug use (especially IDU but non-IDU as well)? In other words, could any of these women have acquired HIV from a means other than sexual transmission from their husbands? The fourth paragraph of the Discussion could be greatly enhanced with these results. Even though authors report these women had little risk of their own (e.g. <1% had ever injected, 85% were monogamous), it is possible that given n=10 HIV-infected women, these are the women with these risks (e.g. history of injection, multiple partners). Third, it seems to me that two key findings of this study are that 48% of women who knew their husbands were HIV+ did not use condoms and that 36% of women who knew their husbands were HIV+ had never been tested. These numbers are staggering and have direct and immediate implications for condom promotion and HIV testing programs. Yet this is not mentioned in the Discussion. Perhaps the authors could address this?

We have added the descriptive statistics of the 10 women who were HIV positive. We have also incorporated into the discussion the findings regarding low testing and condom rates despite knowledge of husband’s HIV status.

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Minor and discretionary revisions include the following:

Use of terms “spouse” and “sexual partners” both seem to refer to the same population throughout (the study population) and thus one or the other should be used consistently. If they are meant to refer to something different, this should be explicit.

**They refer to the same population and hence, we have corrected this to state “spouse” or “wife” consistently.**

The authors may want to consider excluding the 4 women who reported history of IDU so the focus of the manuscript can be clearly on non-IDU spouses of IDU. Partner risk behavior section: change “caught” (seems like slang) to “discovered” or “learned of”.

**We opted not to exclude these 4 women because an important objective of this analysis was to describe prevalence of infections and drug- and sexual-risk among this population. Including these four women speaks to the low prevalence of injection drug use in the population. We have clarified that none of the HIV positive women had a history of injection drug use. In the partner risk behavior section, we changed caught to ‘learned of.’**

Fix the Kumar reference in the second paragraph of the Discussion.

**Thank you. This reference has been fixed.**

Fifth paragraph of Discussion: suggest deleting “in some cases possibly even being driven to sex work”. No data are presented to support this. Could the authors look at timing of entry into sex work in relation to death of spouse to support this assertion?

**We have removed this language.**

**Reviewer #3**

Suggested Discretionary Revisions

1. Background section, second paragraph, line 2: the range of estimated number of IDU in India is enormous: 168,000 – 1.1 million. The reason for this huge range should be explained in a bit more detail so that the reader can understand why the range is so huge.

This estimate derives from two different papers that used fairly different estimators. The reality is that both estimates incorporate different assumptions and that the true estimate probably lies somewhere in between. We have included a sentence that the two estimates arise from two different methodologies in the text. We feel that further discussion of this range and the methods utilized would require several additional sentences and could potentially distract from the main message of the paper. If the reviewer feels strongly and the editorial board is agreeable, we would be happy to include more discussion of the methodologies used and the assumptions and limitations of the methodologies.

2. Methods section, 3rd paragraph: The authors state that 13% of the IDU in their cohort died before their wives could be contacted by study staff. What was the time period between the identification of the women to be contacted and the actual establishment of contact by the staff? The way it reads, it sounds as if the process of contacting these women took so long that the male IDU died of old age. Perhaps providing some information
about the stage of disease of these individuals (e.g., they were already in advanced HIV infection), other variables affecting their health (e.g., other infections, such as hepatitis), or cause of death (i.e., overdose, accidental injury, HIV disease) might help explain why they died so quickly.

The wives sample was recruited nearly three years after the original cohort of IDUs was recruited. We have clarified this in the description of study population. The leading causes of death in this population were drug overdose, HIV and tuberculosis. We have reference a paper that describes mortality in this cohort in detail and have also included some limited information to clarify why the mortality rate was so high in this cohort.

Suggested Minor Essential Revisions

1. The manuscript contains several instances of sentences that begin with numbers rather than actual words. In cases where percentages are being used to begin sentences, please write out the number; all instances if numbers within the same sentence can be left as numerals.

   We have made this revision.

2. Please ensure that there is appropriate subject and verb agreement when discussing data. The word “data” is plural, and thus should be followed by an appropriate plural verb (i.e., “data were collected”).

   We have revised this.

Thank you once again for your consideration. We look forward to hearing from you.

Sincerely,

Shruti H. Mehta, PhD MPH

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