Reviewer’s report

Title: The Intersection Between Sex and Drugs: A Cross-Sectional Study of HIV Prevalence and Social Consequences among the Sexual Partners of Injection Drug Users in Chennai, India

Version: 1 Date: 23 September 2010

Reviewer: Linda M Niccolai

Reviewer’s report:

Overall, this manuscript addresses an important topic and the study is impressive. The investigators goal was to describe the potential for sexual bridging of infectious pathogens including HIV, HBV, and HBV among IDU in Chennai India. They also sought to examine what they call “social consequences” of IDU including interpersonal violence and economic deprivation. To do this, they recruited 400 spouses of IDU for an interview and blood draw for laboratory testing. The authors are to be commended for this impressive accomplishment. Other strengths of this manuscript include a focused and clear introduction, an appropriate, primarily descriptive analysis, and a thoughtful discussion.

I have three major comments that could strengthen the manuscript if addressed (major compulsory revisions).

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.

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?

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.

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”.

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

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?

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests