Reviewer's report

Title: Modeling Predictors of Drug Risk among Male Street Laborers in Urban Vietnam

Version: 4 Date: 2 March 2013

Reviewer: Karen Wynter

Reviewer's report:

I congratulate the author/s on addressing most of the reviewers’ comments satisfactorily and substantially improving this manuscript since the first time I read it. It addresses an important gap in the literature, and as such I believe it is relevant to the audience of this journal. I do have some further comments and suggestions, however.

I have indicated with * revisions which I previously suggested which have not been addressed to my satisfaction as yet.

Major compulsory revisions

1. * The aim of the research is still unclear, in the Abstract, the end of the Background (p.5) and the Discussion. It needs to be clear to the reader that the study aimed to test a modified version of the IMB, which includes psychosocial stress, and compare this modified version with the original IMB model in terms of goodness-of-fit. The reader has no understanding of the modified model when reading the Abstract for the first time.

2. * The aim (p. 5) still states that the purpose of the model is to predict “HIV-related drug use behaviour”. While unsafe practices when using drugs certainly have implications for risk of HIV, the drug use behaviour itself is not related to HIV.

3. * I appreciate that the author/s may not wish to do further analysis and present “more” statistics such as percentages in specific response categories, but I still think that it is inaccurate to conclude that a mean score of 3 on a scale of 0 to 5 reflects that a behaviour is “common” while a mean score of 2 on a scale of 0 to 4 reflects that a behaviour is “inconsistent” (p. 9).

4. It is not clear whether one (p. 5) or two (p. 13) qualitative studies were conducted prior to this quantitative study.

5. * As I stated in previous reviews, I think the author/s need to elaborate “broader environmental and social factors” which are mentioned twice in the Discussion (p. 14). Some examples of such factors would indicate that the authors have thought carefully about which other factors might be important and how further research could be conceptualised.

6. It’s not clear how the sentence “Findings of the study may be generalized…” follows on from a discussion of Pearson’s product moment correlations between pairs of variables (p.14). Indeed, it is also not clear (as stated by another
reviewer) how Pearson’s product moment correlations are an indication of construct validity.

Minor essential revisions
7. There are still several typographical and language errors, and the manuscript would certainly benefit from an additional reading by an English-speaking person familiar with writing for a peer-reviewed journal. For example “risk drug use behaviour” is not meaningful; perhaps “risky drug use behaviour” might be better.

8. In the Background, it is stated that there are opportunities for the IMB model to be adapted / modified, but it’s not clear how the current modified model addresses the limitations of the original model. Does the current study (a) adapt existing constructs in the model? (b) validate the model in different populations? and/or (c) include broader social factors in the model, not just individual-level ones? (p4 – 5)

9. The sentence about how psychosocial stress is conceptualised (last sentence of Background p.6) belongs in the “Measures” section of the Methods.

10. The “goodness-of-fit” criteria (as they are commonly called, rather than “fitness”, p.8) include the #2 criterion, which is reported in the results but not included in the description under Data Analysis (only the CFI and RMSEA are mentioned).

11. * The p-value threshold is still inconsistent; mostly the author/s state that p<0.05 (for example) indicates statistical significance but Table 2 still cites the threshold as p # 0.05.

12. Please clarify what is meant by “the path from behavioural stills to behaviour significantly increased” (p.11). Is it the path coefficient which increased?

13. Figure 2, types of venues, number 8 reads “Small”. I think this text is incomplete.

14. Figure 3 has abbreviations in the footnotes which are not used in the diagram (Trans; Info)

15. Table 1 has a typographical error in the title (Original).

Discretionary revisions
16. I would caution against stating that a behaviour is “more likely” in men with certain characteristics than in other men, based on qualitative research (p.5).

17. Please clarify what you mean by “Men were selected because of their greater frequency…”. Do you mean that street laborers are more commonly men than women? (p. 6)

18. Presumably the participants were not screened (invited to participate?) during the interviews, rather before they were interviewed? (p. 7)

19. While I don’t recommend repeating detailed results in the Discussion, I think it would be good to remind the reader of the important overall findings, e.g. the % sharing needles in the current study, when comparing these findings with other “consistent” studies (p. 11).
Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests