Author's response to reviews

Title: Treatment for illegal drug use disorders: the role of comorbid mood and anxiety disorders.

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Author's response to reviews:

Simon Harold
Executive Editor
BMC Psychiatry

RE: Treatment for illegal drug use disorders: the role of comorbid mood and anxiety disorders.

Dear Dr. Harold,

Thank you for reviewing our manuscript and giving us the opportunity to resubmit it to BMC Psychiatry. As described below, in response to the second round of comments made by the two reviewers, we have made additional changes to the manuscript. We hope that you will be satisfied with the revised version of our work and look forward to hearing from you.

Sincerely,

Maria Melchior

Reviewer 1:

1. We are asked to “change the first line in the methods section in the abstract ("Data come from...") to past tense ("Data came from...")”.

The abstract has been changed.

2. The reviewer points out that “In the discussion, the term "health services use" is still used to refer to treatment for drug use disorders”.

This term has now been replaced by “treatment for illegal drug use disorders”.

3. The reviewer points out that “A limitation that should be noted is that the two main outcome variables are both measured with a single unvalidated item (having received treatment and having an unmet treatment need).”

To account for this, we added the following text to the discussion section (page 14): “Third, access to treatment for illegal drug use disorders and unmet need for treatment were ascertained using a single measure specific to the NESARC study, previously used by other researchers [1].”

Reviewer 2:

4. The reviewer questions the way in which we defined the study population. “All those answering the first question about treatment are also asked the second question about needing but not seeking care, this is true in the wave 1 and the wave 2 interviews. Note that the first question (w2s3dq1) with a “no” response skips to the second question 3A (w2s3dq3a) but all “yes” responses are also asked.

We see below that there are counts for treatment use (w2s3dq1=1) and need but not seeking treatment (w2s3dq3a=1). This is also likely true in those meeting criteria for the authors' specified abuse/dependence group. Even in a scenario where the treatment group was not asked about unsought treatment they should arguably be considered missing not zero from any analysis involving the needing but not seeking care group, which the counts from the authors' tables suggest the treatment group was not considered missing”.

The reviewer is right in indicating that all NESARC study participants were interviewed about perceived unmet need for treatment of illegal drug use disorders. However, as indicated in our manuscript, we only studied unmet need for treatment among study participants who had not received treatment (n=764). Since we combined data from wave 1 and wave 2 of the NESARC study, our outcome measures ascertaining treatment receipt and unmet need for treatment span across both times of measurement. To clarify this, we made the following changes: 1) we corrected the methods section (p 7), which now reads: ‘Moreover, participants were asked about perceived unmet need for treatment:…’; 2) we specify in the Statistical Analysis section that ‘Analyses examining the relationship between the presence of mood and anxiety disorders and a) treatment for an illegal drug use disorder (n=1114) and b) perceived unmet need for treatment among participants who never received treatment for an illegal drug use disorder (n=764) were conducted separately; 3) we corrected the n reported in Table 4 (n=764).

5. The reviewer indicates that “the authors want to […] just look at those that have NOT received treatment and among this group examine the association of anxiety and mood disorders with needing but not seeking care”.

This is the way the statistical analysis is performed.

6. We are asked to “respond to the request for the variance estimation method
used, that would be helpful. Also was Stata's subpop specified?

We used Taylor series estimation methods for variance estimation (STATA `svy' commands) to obtain proper standard error estimates for the cross-tabulations and logistic regressions[2]. This has been added to the description of our statistical methods (page 8). Stata subpop commands were used to restrict the analyses to the subpopulation of interest.

7. The reviewer points out that “A section of the text on limitations reads: “Fourth, we did not account for psychiatric comorbidities such as posttraumatic stress disorder, psychosis, and personality disorders which may also influence service use [29,30]”. This is not a limitation. Either these are relevant comorbidities or not. If the authors really don’t think they are relevant to the analysis then don’t say it is a limitation. If they are relevant include the disorders from the diagnostic instrument in your analysis. Note there are two ‘Fourth’ limitations.”

As suggested, we deleted the potential limitation relative to the role of psychiatric disorders other than mood or anxiety disorders.

Additionally, we corrected the typo and there is now only one ‘fourth’ limitation.

8. The reviewer indicates that “If you do decide to acknowledge the data was collected by federal employees, census workers with no previous diagnostic interview experience, then you probably should cite the reference”. We added to the text on page 13 that prior to fieldwork, Census employees received extensive training on the AUDADIS, which was designed to be administered by non-clinicians, similar to several standardized epidemiological interviews [3]. Moreover, the reliability and validity of the AUDADIS have previously been verified and described [4].

9. The reviewer questions the confidentiality of NESARC study interviews and the reliability of the AUDADIS, which we refer to on page 13: “The NESARC was not as confidential as it could have been; it was a face-to-face interview not a computer assisted self-interview. Respondent were not protected by a certificate of confidentiality to my knowledge. Please re-examine this “the reliability of the AUDADIS-IV is good to excellent” and cite original sources. The services utilization component of the AUDADIS have not been tested to my knowledge, it would be good to learn it was. It is also not accurate to say that a test even with excellent test-test reliability would not be subject to differential reporting of illegal activity, drug users that do not report drug use tend to reliably not report drug use (excellent test-test reliability still biased). The same reference (Grucza 2007) suggests suppression of substance use disorder reporting is not as severe as suppression of reporting of simple illegal substance use. Consider using the reference in this fashion”.

Although the reviewer is right in pointing out that NESARC study interviews were conducted face-to-face and not via computer assisted self-interview, we disagree with his statement that data collected this way are less reliable than through self-completed questionnaires. There are advantages to both methods, the key
aspects of a face-to-face interview being that interviewers are able to explain questions that are difficult to understand for some participants, that the coherence between different questions can be checked, and that the likelihood of overreporting is limited.

We now provide additional references indicating that the AUDADIS is reliable [3,4].

In response to the comment made by reviewer 1 (point 3), we now indicate that lack of testing of the validity of measures of treatment for illegal drug use disorders and unmet need for treatment is a limitation of our study (page 14).

The reviewer rightly indicates that the reliability of an instrument does not guarantee its validity, however reliability is a prerequisite for validity and that is the way in which we described our data. In order to temper our conclusion, we modified the text on page 13, which now reads: “therefore the influence of information bias on our measures should be limited”. As suggested by the reviewer, we now specify that measures of substance use disorders are likely to be precise.

10. The reviewer asks us to restate a phrase on page 4: “[Instead of] “In contrast, access to treatment for illegal drug use disorders appears to be highest among individuals who use multiple substances [20]”, ‘access to’ should probably read as ‘utilization of’ as severity of abuse/dependence is most probably on the causal pathway to services (triaged to service use as opposed to a discretionary accessing of an available service)”.

The text was modified as requested.

11. The reviewer asks us to modify the “First paragraph of Sample and Procedures “All participants were interviewed at home by experienced lay Interviewers…” ‘experienced’ should be removed (see 5 above)”.

We replaced ‘experienced’ by ‘trained’.

12. The reviewer asks about the significance of “Table 1 reports a p-values, from what statistical test?”

The p-value reported compares the three treatment groups – this has been added to the Table 1 legend.

Reference List


