Author's response to reviews

Title: Intra-household evaluations of alcohol abuse in men with depression and suicide in women: A cross-sectional community-based study in Chennai, India

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Author's response to reviews: see over
Dear Dr. Pikhart,

We are grateful to the editor and reviewers for considering our submission and for providing critical inputs to improve this manuscript. We have revised the tables and text in accordance with suggestions by the reviewers, and look forward to your feedback on the updated manuscript.

We have revised the title of the manuscript to, ‘Intra-household evaluations of alcohol abuse in men with depression and suicide in women: A cross-sectional community-based study in Chennai, India’.

We have addressed each of the general and specific comments from the editor and the peer reviewers in our set of responses, with the respective changes in the revised version of the manuscript for your kind perusal:

Editors’ Comment:

This is an interesting paper focusing on important association between levels of drinking in men and the levels of depression in men and women and suicidal attempts in women in a community sample from Chennai, India. Both reviewers highlighted the importance of the topic but also commented on range of potential problems in current version of the manuscript. In particular sections on methodology and results require authors' attention. Both reviewers highlighted several issues related to variable description as well as description of analytical methodology. Discussion also needs authors' attention. I would highlight an issue of cross-sectional design and possible bi-directionality of the relationship.

Based on both sets of comments it is not possible to make any decision at this stage as the paper will require further review if resubmitted.

Thank you for your insightful overview on the changes required to improve upon the submitted manuscript. Per reviewer suggestions, the manuscript has undergone major revisions, which addresses concerns expressed, both more broadly and specifically. We would like to assure the respected Editor and reviewers that all the comments have now been addressed and are well incorporated into the manuscript. Look forward to your reconsideration of the paper and the reviewers’ decision.

Reviewer(s)’ Comments to Author:
Reviewer: 1
Comments to the Author

The author must respond to these before a decision on publication can be reached.

**MAJOR REVISIONS**

1. Please report/compare the scores of PHQ-9 between women living in households with and without abusive drinkers. There is a slight hint to such important findings only in the discussion section, line 189.

   *Thank you for the comment, we conducted these analyses but did not include them in the previous draft, as the conclusions remained the same. We have now reported this important comparison in the Results section (line no. 216-209), which supports the associations we observed for dichotomous outcomes of yes/no depression.*

2. Please refer to depression as affective symptoms since the measure used is not a valid diagnostic tool for depression.

   *Thank you for this important point, we would like to highlight that studies have shown that the PHQ-9 to be a reliable and valid measure of depression for the purpose of diagnosis. A meta-analysis by Gilbody, Richards, Baley, and Hewitt (2007) showed that the pooled sensitivity of PHQ-9 as a diagnostic tool for major depression was .80 (95% confidence interval [CI]: .71-.87) and specificity was .92 (95% CI: .88 .95) (Gilbody et al., 2007). We have now mentioned this in the Discussion section (line no. 282-85). Also, PHQ-9 parallels the major American Psychiatric Association’s Diagnostic and Statistical Manual of Mental Disorders (DSM-IV) criteria for the screening of major depression, and is a dual-purpose instrument that, with the same nine items, and can establish provisional depressive disorder diagnoses as well as grade depressive symptom severity (Kroenke et al., 2001).*

3. The writing of the manuscript should be substantially improved. The title and the abstract are not illustrating well the nature of the analyses performed especially the associations between drinking in men and affective symptoms & suicidal attempts in women living in the same household. The authors missed to state the aim of this study in the abstract and therefore the results section of the abstract is very confusing. The abstract’s conclusion does not reflect the findings of this study.

   *We have conducted a detailed review of the manuscript in an attempt to improve the quality of writing and elaborating on the nature of information collected and analyses conducted, to remove any concerns and doubts. The title of the manuscript has been revised as follows, ‘Intra-household evaluations of alcohol abuse in men with depression and suicide in women: A cross-sectional community-based study in Chennai, India’. Aim of the study has also been appropriately added in the abstract (line no. 30-32) with relevant conclusion highlighting the findings of the study (line no. 45-49).*

4. Could the authors explain why they choose not to control their analyses for the levels of physical activity, since they had this info available and there is a strong evidence suggesting that regular participation in exercise has been shown to decrease overall levels of tension, anxiety and depression, elevate and stabilise mood and improve self-esteem?
We agree on the evidence suggesting that regular exercise can decrease overall levels of tension, anxiety and depression, particularly from high-income settings. In low- and middle-income settings such as India, high levels of physical activity are usually associated with occupational activity (eg, manual labor, farming), which are associated with lower socio-economic status and other socio-economic determinants of health that are not necessarily associated with improved health. However, it is important to consider so we adjusted for physical activity using the GPAQ questionnaire, and we derived quartiles for the hours spent in vigorous and moderate occupational and recreational physical activity. We have reported this new variable in the Methods (line no. 115-118), in the descriptive statistics (Table 1) and as a covariate in the Results (Table 2,3,4).

5. There were several cut-off points for the PHQ-9. The authors made a decision to group mild and severe depression symptoms together. Why not have severe symptoms separately grouped?

We combined the moderate and severe depression together as the prevalence of severe depression was too low to analyse separately, particularly for multivariate regression (now shown in Table 1). Moreover, we have now separated mild depression from moderate-severe depression as well as a combined category of all three into ‘any depression’, which takes into account all the 3 levels of depression together (Tables 2,3).

6. Please present descriptive statistics by level of anxiety/depression chosen to be included in the main analyses.

Thank you for the suggestion. We have now presented the descriptive statistics by depression as seen in Table 1.

7. Was suicide attempts measured also for men? if yes, why not reported analysed.

Thank you for raising the query. Yes, a single question on suicide attempt was asked to all members of the family, ‘Did you ever attempt to commit suicide in the past 1 year?’ (Yes/No), which is now presented in Table 1. There is not enough power to analyse the association in men, in whom less than 5% of the men answered ‘yes’; moreover, the primary research question is the association of alcohol abuse in men and depression/suicide attempts in the female family members.

8. Table 1. page 17 reports alcohol use in terms of weekly consumptions but the manuscript describe the use of AUDIT score 1 to 5. Why is this not reported in the descriptives table?

Thank you for pointing this out, and we have now included both frequency of alcohol use and distribution of the AUDIT score in Table 1.

MINOR ESSENTIAL REVISIONS

The author can be trusted to make these corrections:

9. Replace labels in Table 2 and 3 from “any depression” to “mild depression”
The original label was correct as ‘any depression’, but as a result of the reviewer’s suggestion, we have decided to also include ‘mild depression’ as a separate analysis in the Results section. Tables 2 & 3 now display results for 3 categories of depression: 1) mild; 2) moderate-severe; and 3) any depression.

10. Change title to reflect the exact associations conducted and replace the first sentence line 56 to be different than the first line of the abstract page 2, line 30.

Thank you for the suggestion. We have now edited the title in order to emphasize the nature of the evaluations, particularly at the household level, and have made the suggested corrections accordingly.

11. The idea of intra-household mechanisms (how the alcohol affects the spouses of drinkers) should be introduced in the background section.

Thank you for the important comment, and we have mentioned intra-household mechanisms, such as IPV, by which alcohol consumption patterns in men have various effects on their spouses and other family members; see Background (Line no. 76 to 79) and Discussion sections (Line no.256-259). We have also included this in the title, and accounted for it in the clustered standard error approach (see #6 below).

12. Page 9, line 189 repetition - Page 10, line 212 typo “report on”

Thank you for pointing it out. We have omitted the repetition.

Reviewer 2:

MAJOR COMPULSORY REVISIONS

1. There needs to be a clearer rationale for considering all female relatives as this paper does – at present the Background only refers to spouses.

Thank you for the suggestion. We have now clearly highlighted the rationale of including all females in the household the Background. (Line no. 82-86)

2. The description of PHQ-9 is unclear, particularly see page 6 line 123-124.

Thank you for the suggestion. We have elaborated on the PHQ-9 tool in the methodology section (Line 144-145) and have clearly stated that it is a tool for screening depression and not for the purpose for disease diagnosis.

3. I would also like to see a clearer description of the covariates used in the analyses, possibly as a separate paragraph in the measures as this is now solely included in the statistical analysis section. For e.g. what measure of quality of life was used; the tables refer to level of pain which is not referred to in the analysis section

Thank you for this important suggestion, we have described each of the covariates in a separate paragraph as suggested, including level of pain, in the section on Data collection (Line no 111-123).
4. How was the information on suicide assessed? The statistical analysis section of the results also needs to provide further detail on the analysis carried out for suicide attempts.

Information on suicide was assessed through a single question i.e. ‘Did you ever attempt to commit suicide in the past 1 year?’ (Yes/No). We have now clearly stated this in the methodology section (line no.1561-163) and have also described its analysis in detail in the result section. (line no.223-227).

5. Related to a previous point, I would prefer to see the analysis examining the association between male drinking and spouse’s mental health/suicide attempts rather than for all family members as I think this would be a clearer piece of work. If not, it might be useful for the analysis to additionally consider type of relation.

Thank you for the suggestion. In India, as with other LMIC’s, often the family/household units are defined by multiple generations, extended families, constrained resources and traditional cultural norms whereby decision-making by one person can have major consequences on other members of the same household (Obot and Room, 2005; Nandkarni et al., 2011). Moreover, the sample size and power is significantly reduced if we focus on spouses only. In light of these points, we conducted the analysis on all females of the household. This has also been highlighted in the Background section (Line 82-56).

6. Based on the numbers presented, it appears that in some cases mental health for more than one female family member is assessed. Is this correct? Were multilevel models used?

We thank the reviewer for raising this valid point. We did observe depression occurring in some instances among multiple members of the household. As a result, we re-ran the regression models accounting for clustering within the household for depression, thereby adjusting the standard errors accordingly. We have updated the methodology and analyses sections to reflect this (Line no.38-39).

7. Table 1 should present values/category of AUDIT score as well as the categories for PHQ-9. As of now, it is unclear how many individuals are in each of the depression categories.

Thank you for the suggestion. We have now reported the categories of AUDIT score and PHQ-9 score in Table 1.

8. While the analysis section refers to linear regression analysis on PHQ-9 scores, the results section does not provide results relating to this. Also, it would be useful to see the analysis carried out with category of PHQ-9 as the outcome, i.e. possibly an ordinal model.

Thank you for the comment, we had conducted linear regression with PHQ-9 scores as an outcome, but did not report them as the results were similar to those from categorizing scores into dichotomous depression outcomes. We have now included the results for PHQ-9 continuous scores in the Results section (Lines 220-2222; text only). The main focus of the analyses continues to be on the dichotomous outcome as these are easier to interpret through
standardized cut-offs for different levels of depression. Though we understand the advantage of the ordinal approach in maximizing data, efficiency and statistical power.

9. I would like to see some further consideration in the discussion about the bi-directional relationship between alcohol consumption and depression (e.g. see Boden & Ferguson, 2011, Addiction, 106: 906-14; also see comments on the article in the same issue). Indeed, the Background could also go further in explaining this.

Thank you for this important insight, we have now elaborated on the existing evidence for bi-directional relationship and causal linkages between alcohol abuse disorder and depression in the Background (Line no.79-80) and Discussion sections (Line no. 263-265). Thank you for referring us to the article. It was very helpful.

10. The limitations of the cross-sectional design should also be acknowledged.

We agree and have acknowledged our inability to infer causality & direction of the relationship due to the limitation of a cross sectional design adequately in our limitations section. (Line no.287-288)

MINOR REVISION

11. In the tables, should ** refer to AUDIT score?

Thank you for pointing it out. We have now made the necessary corrections in all the tables. (Tables 2,3,4)

We are grateful for the valuable inputs by the editor and the reviewers. We are hopeful for a positive reply from the editor on our revisions to the submitted manuscript and are happy to provide any further clarifications as required.

Kind regards,

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