Author's response to reviews

Title: Contributors to suicidality in rural communities: Beyond the effects of depression

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

Thank you for your feedback, and for the opportunity to revise our manuscript. We appreciate the time and effort that the reviewers have clearly put into our manuscript, and after addressing their comments, we feel that the quality and clarity of the paper have been vastly improved. The modifications that we have made to the manuscript are outlined point-by-point below.

Comments from the editor

1. Copyediting
The manuscript has been revised and the authors feel that the quality of written English is appropriate. If the editor is still not satisfied with specific sections of the manuscript we will happily make additional changes.

2. Competing interests
We have included a section declaring that the authors have no competing interests.

3. Tables as figure files
The tables have now been included within the text file of the manuscript after the references.

4. Figure titles
All figures now have a figure title listed after the references in the manuscript file.

Reviewer 1: Youngtae Cho

1. Relevance of rural populations
Authors argue that rural populations are valuable to examine suicidality as independent from depression, since suicide rate is generally higher in the rural, while mood disorder is about the same in the rural and the urban. Then, they hypothesized that suicidality might be developed by individuals with or without depression, as no depression would magnify the effect of other factors. This reviewer agrees with the authors that suicidality may not be necessarily preconditioned by disorder. But I am not convinced why rural area populations are more relevant study objects to test their hypothesis. Just as in rural areas, urban populations should also developed suicidality regardless of having depressive illnesses. If their findings are pertinent only in the rural areas, its reasons should be extensively explained in discussion, which is missing in the current manuscript.

From the outset, authors emphasized the use of rural populations as to test their hypothesis. Then, they should mention in discussion that how their findings would be differentiated or comparable to the urban populations. Do authors expect different findings to the urban populations?
The concept of suicidality as an independent construct from depression has been explored in previous research in an urban Australian population, the Fairweather-Schmidt et al. (2010) paper cited in the introduction. The aim of the current paper was to explore whether this concept was also relevant in a rural population. We feel that rural populations are a relevant sample for this analysis for several reasons; rural areas generally have a higher suicide rate than urban areas, and rural suicidality tends to receive less research attention. However, we did not mean to imply that rural populations are a more relevant sample in which to conduct this analysis, simply that they are relevant in their own right. We have re-worded sections of the introduction in an attempt to make our intentions clearer.

As mentioned above, the idea of suicidality and depression as separable states has been explored in only one Australian study that we are aware of, which used an urban sample. We did not necessarily expect our results to differ from the urban findings, but did wish to explore this concept in a rural sample. We have now discussed our results in relation to previous urban-based studies in the discussion section.

2. Details about data set
Authors should provide more information on the data set utilized in the study. In what year the data was obtained? If it is a longitudinal data set, authors used data from what year to what year? What are the response rates? Who are the people selected for CIDI? What was the sampling strategy and how the survey was taken? It is confusing if the survey was taken via post or telephone calling, since in page 8, authors mentioned that demographics are taken by postal survey, while a telephone-administered version of CIDI was utilized to determine the presence of suicidal ideation.

Information about the years in which data were collected has now been included in the Methods section under “participants.” We have also attempted to clarify which aspects of the data were collected by postal survey, and which were done by telephone interview. The sampling strategy for the overall ARMHS study is described briefly under “participants”:

“The sample consisted of New South Wales (NSW) residents aged 18 or over, who were randomly selected from the Australian Electoral Roll and resided in one of 60 Local Government Areas (LGAs) from the Greater Western, Hunter New England, or North Coast rural health service regions of NSW. These areas cover approximately 70% of the geographic region of non-metropolitan NSW. Metropolitan areas, including capital cities and other urban centres with populations greater than 100,000, were excluded.”

Additionally, a reference has been given to a descriptive paper that outlines the ARMHS study in detail. This referenced paper includes response rates for the overall study; as the present analysis only includes a sub-sample of ARMHS participants, we felt that readers who are interested in the entire ARMHS sample would find it more informative to be referred to a paper which utilises the whole sample rather than a subset.

For the current paper, response rates have been described in the Results section:
“Of the 2466 ARMHS participants recruited, 867 were selected for CIDI interview based on their K10 score, and 230 (27%) participants declined, therefore 637 (73%) were interviewed.”

The method by which participants were selected for CIDI has been described in the “Composite International Diagnostic Interview” section as follows:

“Participants were selected for CIDI interview based on their K10 psychological distress score; interviews were offered to 100% of those with a high-range score (25+), 75% of those with a moderate-range score (16-24), and one-sixth of those scoring in the low range (10-15).”

3. Order of presence of suicidality and depressive disorder
To examine suicidality non-preconditioned by depression, depressive symptoms should precede the presence of suicidality. However, according to authors’ description, both suicidality and depressive disorder were measured through the window of “life-time.” Although the age at the first and the most recent suicide ideation was taken into account, still there is no way to clarify the time order of presence of suicidality and depressive disorder.

We agree with the reviewer that the use of lifetime data is an unfortunate limitation of this study, and share the concern that we cannot determine the order in which depression and suicidality occurred in the participants’ lifetime. We have included this as a limitation of the present study in the Discussion section.

4. CIDI interviews
This reviewer is not familiar with the CIDI interview. Thus, I am wondering how CIDI interviews can assess a lifetime diagnosis of depressive disorder. Does the CIDI ask respondent’s depressive symptoms in the past? If so, what is the time-frame that respondents should remember their past? If not, why did authors simply assume that the CIDI assessed life-time experience of depressive disorder?

The CIDI collects information to make diagnoses within three timeframes: the past 30 days, the past 12 months, and at any point during the participants’ lifetime. Due to the small sample size in our study, we do not have sufficient power to conduct our analysis using either 30-day or 12-month diagnoses, hence our decision to use the lifetime data. The potential limitations of this approach, primarily the higher likelihood of recall bias, have been included in the discussion. Additionally, information about the acceptability of the CIDI has been included in the Methods section.

5. Age group cut point
Clarification is necessary for the age group cut point. Why 45? If the cut point were to move to about the mean age (about 55), would the result be the same?

We have now changed the age group cut points; rather than two age categories we have divided the sample into three groups: under 45, 45-64, and 65+. This is more consistent with the reporting of age groups in other Australian community-based research, such as results
from the National Survey of Mental Health and Wellbeing. While this had only a minor impact on the results, we agree with the reviewer that this gives a more even distribution of participants between age groups.

6. Use of ROC curves
Authors' intention of utilizing ROC curve analysis is not clear. It is obvious that model fit will be improved with more controls. Comparison of a model that includes depressive disorder only with another that includes a number of other variables is not necessary, since the improvement of model is obvious.

The inclusion of the ROC curves was intended as a visual supplement to the findings of the regression analysis, and was not undertaken as an independent analytic strategy. We have attempted to clarify the intention of this analysis in the revised manuscript.

7. Multivariate models
Multivariate models for suicide ideation and attempts were carried out for overall sample only. Given the purpose of this study, multivariate models should be carried out and their coefficients should be compared between a model with depression and the other without depression. Odds ratios for overall sample do not make it possible to delineate the presence of suicidality independent of depression. If this reviewer misunderstands the authors’ intention, clearer explanation should be provided. At its present form, authors’ main discussion is simply based on the comparison of descriptive rates of with depression and without depression.

We apologise that the aim of the current paper was not made clear in the previous draft. While we did do univariate analyses to determine the presence of suicidality that occurred independently from depression, the purpose of the multivariate models was to explore the additive contribution of additional psychiatric diagnoses to suicidal ideation and attempts. That is, we did not specifically want to explore the impact of secondary diagnoses in the absence of depression, but rather to determine whether these disorders made a significant independent impact after depression had been controlled for. The interactions which were included in the multivariate models allowed us to determine whether the magnitude of the effect size of these additional disorders was different in participants with compared to without depression. After making amendment to the Introduction and Discussion sections, we believe that the aims of the paper, and the implications of our findings, have been clarified.

Reviewer 2: José Luis Ayuso-Mateos

1. Older age and reduced suicidality
How can the authors explain that older age reduced the likelihood older age reduced the likelihood of reporting a lifetime suicide attempt? Could it be recall bias?

We agree with the reviewer that this counter-intuitive finding may be due to recall bias. We have expanded on this potential explanation, as well as several other possibilities (e.g. cohort effect, change in perception), in the discussion section.
2. Telephone-administered psychiatric interviews

One limitation not mentioned is the nature of the psychiatric interview performed by telephone and covering a lifetime period. The authors should provide some evidence on the validity of diagnosis obtained through phone interview versus face to face interview.

We have now included a statement under the section “The Composite International Diagnostic Interview” giving reference to studies validating the use of telephone-administered diagnostic interviews.

3. Subthreshold depressive syndromes

Are the authors able to provide information on subthreshold depressive syndromes. With the CIDI questions already completed it maybe theoretically possible.

The manuscript does include subthreshold depressive syndromes (minor depression and dysthymia) in the “depressive disorders” category, with few participants meeting this subthreshold criteria. We acknowledge that the inclusion of these disorders may not have been clear in the previous version of the manuscript, and have attempted to make this more apparent to readers in the revised version. We have also now included a statement about the number of participants who have ever experienced any symptoms of depression in their lifetime, in order to give added context, however considering that these people did not meet diagnostic criteria for minor depression or dysthymia even with the “inclusive approach” which we utilised in this analysis, we did not feel that it was appropriate to categorise these people as having a depressive syndrome.

Thank you again for the opportunity to re-submit our manuscript.

Kind regards,

Tonelle Handley