Author's response to reviews

Title: The prevalence and characteristics of suicidality in HIV/AIDS as seen in an African population in Entebbe district, Uganda

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Author’s response to reviews: see over
To The Editor
BMC Psychiatry

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Dear Sir/Madam,

Re: Re-submission of a revised copy of the manuscript entitled, “Prevalence and characteristics of suicidality in HIV/AIDS as seen in African population in Entebbe district, Uganda”

We would like to re-submit a revised copy of the above manuscript. We have addressed the reviewers comments as follows:

**Reviewer's report 1**
Comment 1: “I’m not in favour of the term suicidality. Scholars worldwide now prefer suicide risk instead” . The authors of this article prefer to maintain the use of the word suicidality instead of suicide risk as suggested by the reviewer because we are not only referring to the risk for completed suicide but also to the risk associated the suicidal phenomena of both attempted suicide and suicidal ideation (what we refer to as suicidality in this paper) which in themselves are manifestations of psychological distress and in there own right independently associated with negative outcomes in HIV/AIDS.

Comment 2: “It is not clear from the paper when authors estimate suicide risk as related to a score or when they point to a previous suicide attempt. Authors should better report how MINI suicide assessment module works. There should be a note related to the assessment of suicide risk, that is who did that and if there was more than one person performing such task” We have now consistently described our dependent variable as ‘moderate to high risk suicidality’ (MHS) which under the section of statistical analysis page 6, we clearly define as a score of 9 and above on the B-items of the suicidality module of the M.I.N.I. neuropsychiatric interview (MINI Plus). We have added a sentence that describes the constituent items of the suicidality module of the M.I.N.I.

Comment 3: “Second, I would definitely strengthen the limitation of this report especially in light of the tool used to assess suicide risk. Moreover, I would report citations inserted in the limitations somewhere in the discussion” Under limitations we have included a sentence on the limitations of using ‘risk of suicidality’ instead of ‘suicidality’ as the dependent variable.

Comment 4: “I also suggest to mention the role of personality disorders in precipitating suicide risk, especially those of cluster B. Authors may discuss a paper by Pompili et (Ann IstSup Sanita 2004) for completeness” On page 3 under the section Background, second paragraph, we mention antisocial personality as a risk factor for suicidality in HIV/AIDS as reported in western literature.
Reviewer's report 2

Major Compulsory Revisions:

Comment 1: “Accurate interpretation of the study's findings requires data to estimate the potential direction and magnitude of any selection bias introduced by sampling. Can the authors give any impression of the proportion of PLWHA are served by these clinics or how those served by those clinics may differ from the population of PLWHA in Uganda? For instance, are there socioeconomic or geographic differences between those who are able to attend this clinics and those who do not attend? It seems conceivable that those attending these clinics would be more likely to have greater resources and less likely to be rural. The exclusion criteria might also suggest they are healthier though it would be helpful to know how many of those screened for eligibility were declined for being “too physically and mentally sick” or for other reasons (beyond failed to turn up for the interview)” We have better described the population served by the two health facilities where the sample for this study was drawn.

Comment 2: “Methodology, Study Design, Paragraph 2. The authors already indicate there is an ongoing longitudinal follow-up. To avoid any confusion, this paragraph can be deleted as it is not relevant to the study design as presented herein” As recommended by the reviewer we have deleted this section.

Comment 3: “Appreciation of the results also hinges on an understanding of the outcome. It would be helpful to include a table highlighting the dependent variable and its threshold. How did the authors arrive at the chosen threshold? Are there any studies which used a similar threshold? Was the threshold selected a priori?” The authors used the threshold for ‘moderate to high risk suicidality’ as specified a priori by the designers of the M.I.N.I. and has been used by a number of other authors. The fact that this threshold has never been validated in the African socio-cultural context has been stated as a limitation of this study.

Comment 4: “Is the dependent variable conceptualized as “suicidality” or “risk for suicidality”? The methods would suggest the latter, which would diminish the importance of the risk factors identified as being associated with a surrogate outcome, a risk index for suicidality rather than suicidality itself. Regardless, the authors should think about what they are measuring and describe consistently throughout” The dependent variable in this study is risk for suicidality, It is now consistently described throughout the manuscript. It has been stated under limitations that the use of the surrogate measure “risk of suicidality” instead of ‘suicidality” does diminish the importance of the risk factors assessed.

Comment 5: “I appreciated how the authors identified risk factors within sets, however, wonder if there were some potential problems with multi-collinearity in the multivariate models for each set” We built the model using a conceptual framework, to help avoid problems of collinearity. The selection of the final model was done in stages, at each level of the framework. Variables whose age- and sex-adjusted (as a priori confounders) association with the outcome was significant at p<0.10 were added to the model one by one, and those remaining associated at p<0.10 were retained. Thus, social risk factors were considered one at a time, adjusting for independent demographic predictors, and psychosocial factors were considered one at at time, adjusting for independent demographic & social predictors. In the final model, variables that were no longer associated with the outcome at p<0.10 were allowed to drop. We did not find collinearity to be a problem using this approach.

Comment 6: “The final model includes 11 variables (when considering need to enter two variables for each of the three level ordinal variables). Over-fitting is likely and should be addressed. This will require removing some variables from the final model”
We have re-done the final model, allowing all factors that were not associated at p<0.10 to drop, at each level of the framework, and in the final model The resulting model has 5 parameter estimates (4 variables).
In addition, we excluded the diagnoses of psychological disorders where the numbers were too small to obtain reliable adjusted estimates of effect.

Comment 7: “In a cross-sectional study without a control group, it would seem impossible for the authors to include suicidality “would have occurred anyway in psychologically vulnerable persons.” The authors should consider reframing how their results fit within “Marzuk’s hypothesis.” We have dropped reference to Marzuk’s ‘accelerator’ hypothesis of suicidality and instead interpreted our results in the light of the stress-vulnerability model, the basis of our conceptual framework.

Discretionary Revisions: 
Comment 8: “I did not find the conceptual model of Figure 1 to be particularly helpful. However, this framework was specified a priori and is subsequently worth including. Perhaps it would help if the boxes in the figures corresponded to the analysis: socio-demographic, social, psychological and clinical” The conceptual framework in it’s current form best depicts how the investigated factors fit in with the stress-vulnerability model.

Comment 9: “Consider rewording the title to “The prevalence and characteristics of suicidality among those with HIV/AIDS in Uganda” or something along this line to address the key findings of the paper and broader implications” Reworded as suggested.

Comment 10: “Under the methods section of the abstract, please indicate the goals of the regression analyses (e.g was undertaken to identify independent risk factors for suicidality)” We have reworded as follows: “Independent risk factors for suicidality were assessed using multivariable logistic regression”

Comment 11: “Marzuk’s ‘accelerator’ hypothesis does not seem to be so well established as to be the highlight of the conclusions of the abstract, particularly when the study is not designed to specifically test this hypothesis (as mentioned elsewhere)” Removed and replaced with the stress-vulnerability model.

Comment 12: “Background, Paragraph 1, can shorten sentence 3 to “The few African studies on suicidality in HIV/AIDS...” Done.
Comment 13: “Background, Paragraph 1, last sentence, please replace “incidental” with “secondary.” Done.
Comment 14: “Background, Paragraph 2, please state “a psychiatric disorder” in the place of “the psychiatric disorder.” Done.
Comment 15: “MHS is sometimes abbreviated MSH (e.g. in abstract). Please use consistently throughout” Done
Comment 16: “In Table 1, please make the religion singular. Did all participants belong to one of these two religious groups?” Yes

Comment 17: “In the Tables, there is a symbol over adjusted odds ratio. Presumably this is to indicate the variables adjusted for, however, these are not found on the table. The variables included in the multivariate models are also not apparent from the text.” We have added a footnote to explain that the odds ratios are adjusted for age and sex.

Comment 18: “the word “positive” can be dropped from the table titles.” Done.
Comment 19: “Discussion, Paragraph 1, last sentence. “psychiatric disorder” should read
“psychiatric disorders.” Done.

Comment 20: “Please move discussion of limitations from the Conclusion to the Discussion.”
Done.

Reviewer's report 3

Comment 21: “Methodology:
2.1 You named a follow-up study, but no data you reported in the results. Could you explain better the study design?” Removed from this paper as not relevant
Comment 22: “You haven't considered the clinic in the analysis and it is impossible to evaluate the potential confounding effect.” We have added clinic to the analysis, and present the results in Table 1 and the results text. We did not find clinic to be an important confounder of any of the observed associations, and state this in the results.

Comment 23: “Those who refused to participate in this study did not differ significantly....”. Can you provide a p-value?” Since we did not collect specific data on the sub-population that did not enrol we have reworded this sentence.

Comment 24: “The population served by two health centres was similar in terms of gender and age”. Could you upgrade the results with data (% respect to total) and a p-value in order to confirm this results?” We do not have population level data for the two catchments served by the two study clinics. We have however described the predominant population served by each clinic.

Comment 25: “Generalized anxiety disorder have a numerosity of only 5 patients and aOR is significant. How you have estimated the model without encounter prediction problems? Same considerantion is for "Alcohol dependency disorder and post traumatic stress disorder". We agree that there were very few individuals with a diagnosis of post-traumatic stress disorder, generalised anxiety disorder or alcohol dependency, and thus it is difficult to obtain reliable point estimates for the ORs of the association of these factors with the outcome, and the standard errors are large. We did not encounter convergence problems with our models. However, we acknowledge that the adjusted ORs are not very informative, given the small numbers involved, and thus have removed them from Table 3. In addition, we have re-done the final model, excluding these variables from consideration.

Yours faithfully,

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