**Author’s response to reviews**

**Title:** Prevalence and correlates of depressive symptoms among adults living with HIV in rural Kilifi, Kenya

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**Author’s response to reviews:**

Reviewer #1

1. Under study participants you note that you will exclude those above 60 years but the reasons is not clear. Please clarify why those above 60 years were excluded. Line 7-17 page 5

We thank the reviewer for pointing out this. This work is part of data collected from a larger project looking at different outcomes of adults living with HIV. One of the main outcomes was health-related quality of life which may be affected by diseases of the elderly (mainly cardiovascular; see Hunger M, Thorand B, Schunk M, Döring A, Menn P, Peters A, Holle R. Multimorbidity and health-related quality of life in the older population: results from the German KORA-age study. Health Quality of Life Outcomes. 2011 Jul 18;9:53). The inclusion criteria in the larger project therefore only considered adults up to 60 years. We have now rephrased the methods section under “study participants” sub-topic to clarify this. (see Page 5, lines 5-6 and 10-12).

2. One of your findings having an additional chronic illness was associated with high odds for having depressive symptoms. In this case how did you differentiate the cause of depressive symptoms i.e. were depressive symptoms as a result of HIV or due to the comorbid chronic illness or old age? See lines 37-49 page 12
We thank the reviewer for this comment. Our model of analysis was prognostic than causal. Prognostic models can include any variables that are predictive, the causal relation with outcome is not considered. For our case, we were interested in understanding potential predictors of depressive symptoms among adults living with HIV. Also, the study design used limits investigations of causality and we acknowledge this in the limitation section (page 14, line 24). Since we only recruited adults living with HIV without a comparator/control group, it is difficult to know whether depressive symptoms were because of having HIV or not. Picking up from our findings, future studies recruiting a control group can explore this research question further. We fixed the age (and sex) variable in the multivariable analysis (because these have been shown to predictors of depressive symptoms in other studies from Africa) and we did not include the elderly population (over 60 years) because of the above-mentioned reason. With this approach variables that were significant predictors of depressive symptoms among adults living with HIV in our setting included comorbid chronic illness. We can only state that comorbid chronic illness is a predictor of depressive symptoms but whether depression is caused by comorbid chronic illness or vice versa requires a different study design (perhaps longitudinal study) with a different analysis approach (causal modelling).

3. On page 13 lines 1-12 you state that travelling long distances or having difficulty accessing the clinic was associated with depressive symptoms. Did you find out if difficulty accessing the clinic associated with non-adherence to ART hence non-adherence may be responsible for presence of depressive symptoms?

We thank the reviewer for this comment. We did not check this. As explained above, the basis of our analysis was purely prognostic modelling. The pathway could be that clinic inaccessibility among adults living with HIV leads to depressive symptoms which then leads to ART non-adherence. This is a potential area of investigation that can be addressed using a different analysis approach such as a path analysis using a partial mediation approach. Depressive symptoms has consistently been shown to reduce the likelihood of achieving good adherence. Uthman et al (https://www.ncbi.nlm.nih.gov/pubmed/25038748) discuss this in detail, in other words non-adherence is a consequence than an antecedent of depressive symptoms.

4. Review the whole document to remove extra spaces all through.

We thank the reviewer for picking this up. We have carefully reviewed the whole manuscript to remove extra spaces between words.

Reviewer #2:

1. **GENERAL COMMENTS:** This is an interesting and well performed study. Authors have been successful in selecting samples, data collection and analysis. Discussion should be improved by explaining the clinical application of findings.
We would like to thank the reviewer for the positive appraisal of this work. The key clinical implication of our finding is the need to integrate mental health screening in HIV comprehensive care clinics, a point we emphasize in the conclusion statement. However, for successful integration, a first step will be to train the health care providers at these clinics on detection and basic management of common mental disorders like depressive symptoms. We have added the following statement on page 13, lines 2-5, discussion section:

“To successfully integrate mental health screening into HIV primary care in our setting and similar settings, we call for the training of healthcare providers at HIV clinics on detection, management of, or referral for common mental health problems. Early detection will facilitate early management or referral for specialized care, hence better outcomes”.

2. I expect to see the specific rational for performing this study in a regional center in Kenya. What is different in this region about HIV?

As pointed out in the methods section, Kilifi County mostly consists of a rural population. HIV prevalence in this setting almost nears the reported national estimate according to Kenya’s National AIDS Control Council report of 2016 (http://nacc.or.ke/wp-content/uploads/2016/12/Kenya-HIV-County-Profles-2016.pdf). There is a high level of socio-economic disparities with majority of residents in this rural setting being illiterate and near two thirds live below the poverty line. These are some of the potential contributing factors to HIV infection and prognostic patterns in this setting. Therefore, conducting studies looking at outcomes such as mental health, in such a setting, may help inform policy makers on priority areas and better ways of delivering care to those most in need.

We have rephrased the “study setting” sub-topic in methods section to be clearer (see page 4 lines 15 – 23).

3. How the 450 samples were selected from all eligible samples? Were samples selected randomly or not?

We thank the reviewer for pointing out this. For clarification, we used consecutive sampling than random sampling from an existing sampling framework. This sampling method involves selecting and recruiting every consenting subject meeting the criteria of inclusion, until the required sample size is achieved. Because of regular client transfer-in and -outs, coupled with mostly non-electronic record keeping and a shortage of manpower to update records on a regular basis, having an up-to-date sampling framework for this kind of study populations in our resource limited settings is challenging. Consecutive sampling is preferred and has been used in previous studies involving people living with HIV in a similar setting (https://www.ncbi.nlm.nih.gov/pmc/articles/PMC5833144/). We now include a statement stating:
“Selection was done through consecutive sampling of eligible patients on arrival at the clinic until the required sample size was achieved” on page 5, lines 17-18

Considering the study population and the fact that recruiting such participants is only feasible for those who are seeking regular treatment, our study findings may only be generalizable to adults living with HIV actively seeking care in our setting and similar settings. Also, our findings do not apply to the elderly people living with HIV above 60 years. We add the following statement in the limitation section in view of the sampling approach used:

“Because of the sampling approach and selection criterion, the study findings may not be generalizable to the adult population living with HIV who are not actively attending HIV-related services/care and the elderly people (above 60 years) living with HIV.” Page 14/15, lines 25, and 1 -2

4. More description of medical and cognitive exclusion criteria is required.

As a criterion set a priori, participants who were very sick or reported not feeling well because of a medical condition that brought them to the clinic on the day of recruitment were to be excluded, despite meeting the inclusion criteria. Additionally, participants who showed obvious signs of difficulties comprehending what was told to them during consenting or at administration of study instruments (based on their responses and interviewer judgement) were also to be excluded. This is what we describe as signs of “acute medical illness and cognitive difficulties”. Important to note is that no formal medical examination or cognitive assessments were carried out, other than reliance on self-report (by client) and interviewer judgement (our assessors have been administering different cognitive tests and we relied on their experience to pick out any cognitive difficulty). However, none of the approached clients were excluded because of these two reasons. For clarity, we have now removed this statement and left only the exclusion criteria that was applicable (see page 5 lines 10 -14).

5. What was study power?

Sample size estimation was based on a previously reported prevalence of depressive symptoms among adults living with HIV in sub Saharan Africa (15.5%; see https://www.ncbi.nlm.nih.gov/pubmed/27008895) . We used single population proportion formula, setting our precision around this prevalence estimate at 3.5%. Considering non-response, systematic missing data, or other factors that tend to reduce the final sample size, a sample size of 450 would give reliable prevalence estimates in our setting. Such a sample is &gt;95% powered to detect a proportion of ≥ 0.10 given a null proportion of 0.05 at 5% level of significance. We now include a sub-topic called “Sampling and sample size estimation” to describe the sampling, sample size calculation and study power (see page 5, lines 16 -25)
6. How the collinearity was tested?

Because the assumptions of least-squared regression model do not apply in logistic regression analysis, we first checked that continuous variables to be moved into multivariable analysis did not highly correlate using Pearson’s moment correlation. None of the continuous variables correlated.

7. Why they kept BMI in final model?

We kept BMI in the final model because during model building (backward stepwise approach), this variable had p<0.05, implying it made meaningful contribution to be retained in the final model.

8. Did they check departure from linearity in continuous variables?

Indeed, we checked for departure from linearity for all continuous variables by visually inspecting the frequency histogram with normal overlay graph. To be clearer, we have added a statement stating:

“Departure from linearity in continuous variables was checked through visual inspection of the frequency histogram with normal overlay graph” Page 8, lines 11 - 12

9. Please report AUC for final model.

To report this, as suggested by the reviewer, we have now added the following statement:

“The cross-validated mean area under the curve (cvMean AUC) for the final multivariable model was 0.75 (bootstrap bias corrected 95% CI: 0.66, 0.80)” (see page 11, lines 9 – 11)