Reviewer's report

Title: Depression, Suicidal Ideation, and Associated Risk Factors: A Cross-Sectional Study in Rural Haiti

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Reviewer: Mary C Smith Fawzi

Reviewer’s report:

Review of manuscript entitled: “Depression, Suicidal Ideation, and Associated Factors: A Cross-Sectional Study in Rural Haiti” submitted to the journal “BMC Psychiatry.”

I have reviewed the manuscript, and there are some changes I think that need to be made before the article can be accepted for publication.

Major compulsory revisions:
1) In the results section of the abstract, the authors report a median as a proportion; it should only be reported as a number.

2) In the last sentence of the second to last paragraph of the background, the authors mention that only 17% of people from low-income countries present to providers in the year prior to suicidal behavior. The authors should mention how this general estimate may reflect the situation in Haiti, in terms of access to mental health care. For example, do the authors think that the burden may be higher in Haiti, first in terms of the level of trauma experienced in Haiti and also the very limited mental health services as well, even compared to other low-income countries?

3) In the last paragraph of the introduction, the authors state it is the first study on depression, suicidal ideation, and associated risk and help-seeking factors in Haiti’s Central Plateau. However, in terms depression and suicidal ideation, there have been prior studies. I think the authors should not make the claim that it is the ‘first study’ of this nature.

4) In the methods section, the authors may consider describing with some detail about how the culturally adapted Kreyol BDI and BAI were adapted, since the article that they cite is ‘in press.’ It would be good to offer summary statistics on validity and reliability of the scales.

5) In the first paragraph of the implications section, the authors state that working partnerships between allopathic care and traditional healthcare networks are important. The authors should state what they hope to see come from these partnerships, and how that may decrease suicidal ideation. There can be not only positive but negative outcomes that come from these partnerships and I think the authors should make this cautionary note.
6) It’s unclear why, if suicidal thinking is common, it would be considered “normal.” Unless there are data to support this, I would remove this statement.

7) The authors refer to qualitative data in the discussion, but do not present these results. Since the qualitative data are not presented in the results, then these statements should be removed from the discussion.

8) It’s unclear why there is a focus on Vodou in the discussion, when there are other factors that are equally (or more) important in predicting suicidal ideation. In that regard, I’m not sure why this issue related to Vodou is mentioned first in the section on ‘implications for treatment.’ For example, what are the implications locally if someone does not have anybody to take care of them if they are sick? Use of alcohol and elevated depression scores are also risk factors. It would be good for the authors to comment on the service implications of these findings as well. Focusing too heavily on the role of traditional Vodou healers weakens the discussion.

9) In the limitations and strength section, the authors state that the 4 zones excluded from the study may have been systematically different from the 13 sampled. Please provide information about how they would be expected to be different.

10) Also, in the limitations section, a strength is clearly the evaluation and referral by a social worker, but there is not enough evidence to state “confirming the specificity of our suicide screening tool.”

11) In the last paragraph before the conclusion, the authors refer to an “ethnographically-valid” BDI. Unless there is clear evidence provided in the paper to support this, I would take this phrase out.

12) The conclusion section can be stronger if it is summarizes the clear implications of results in Tables 2 and 3 for treatment, such as importance of increasing access to screening and treatment for depression and suicidality. For example, from the risk factors of depression outlined in Table 2, what are the treatment implications? If SES (among women), months without food, and travels over one hour to see the doctor are predictors of depression, what would be the potential role of poverty and how could this be addressed? Women are at greater risk than men. If both SES and gender are structural factors influencing depression, they should be mentioned in terms of treatment and broader program implications (e.g. programs that focus on increasing economic security and reducing gender inequality may also have a broader impact on mental health in this context). The roles of depression, alcohol, lack of instrumental support, as well as accessing traditional healers as predictors of suicidality should be discussed and summarized briefly in terms of treatment implications.

Discretionary revisions:

13) Based on the point above in #12, the authors should consider having a clinical provider of mental health care in Haiti review their paper from a clinical /
treatment implications perspective, in terms of what concretely can be done as a follow-up to these findings. Ideally, this person, if he/she informs the discussion significantly, could be included as a co-author.

Minor essential revisions:
14) In addition, the manuscript should be carefully copy edited before resubmission.

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**
I declare that I have no competing interests.