Reviewer’s report

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

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Reviewer: Bregje van Spijker

Reviewer’s report:

This paper describes the results of a cross-sectional survey conducted in Haiti after the 2010 earthquake as a first attempt to map the prevalence rates of depression and suicidality, as well as associated factors. The results are interesting and a welcome addition to the relatively limited data on mental health in low-income countries.

Major Compulsory Revisions

1) A first general comment is that I feel that the introduction of this paper needs more work as the literature used in this section does not appear the most relevant to this paper. For example, citing studies that found that schizophrenia and impulsive disorder are associated with suicide does not add to the aim of the study, as neither are directly relevant to the present paper. A similar argument applies to some of the depression literature used. In both instances, the authors should be able to find literature that provides a better background to their research question. Additionally, or alternatively, they might be able to demonstrate the relevance of these papers more clearly.

On the other hand, I would be interested to know more about what the study among expatriate military forces found, and what the contrasting findings are on the impact of natural disasters on suicidality (or briefly explain why you don’t describe these issues further so that I’m not curious).

2) Analogous to the previous comment, the discussion needs more attention. For example, it is not clear to me how it is relevant to hypothesise in the discussion that rates of suicidal behaviour (not measured in the study) are higher in rural areas compared to urban areas (also not studied), and what this implies about the 6.13% prevalence rate for suicidal thoughts found in the study. Another example is the statement that the point prevalence found in this study is twice as high as in Ethiopia. This is not particularly useful if the authors don’t explain why only Ethiopia would be relevant to mention here (as opposed to e.g. comparing the rates to several low-income countries). I trust the authors to inspect both the introduction and discussion on similar issues and either clarify the relevance or omit/replace them.

3) Reading this paper, I found it difficult to keep track of all the different variables that were examined as new variables keep popping up in the various sections and the tables. Specifically, I suggest the authors consider the following:
3a) It would be accommodating to introduce the variables you are going to study in the introduction by providing background on them (which could subsequently be used in the discussion to reflect on results).

3b) It would be helpful to describe the survey that was used in more detail in the method section. Quite a few variables mentioned in the results section and the tables are not described in this section, or are lumped together in one term (e.g. daily stressors), which makes it unclear and up to the reader to figure out under what term e.g. ‘spirits causing sadness/anxiety’ and ‘last time sick went to hospital’ would fall. Although for variables such as ‘distance to work’ or ‘number of children’ the measure would be fairly straightforward, this is not the case for variables such as ‘trauma related to earthquake’, ‘general trauma’, ‘mental illness of household members’, and ‘perceptions of mental illness’. For example, if no standardised questionnaires are used but one relies on the opinion or judgment of the participants, levels of mental health literacy could influence answers. In general, it is customary to describe the instruments used to measure different variables, as this facilitates interpretation, comparison of results with other (future) studies and replication of studies.

4) The BAI is described in the methods section, but no results are provided for this scale. Would the authors be able to explain/address this?

5) One of the aims of the study formulated in the introduction (last paragraph) is to look at help-seeking factors. Results on this aspect are not clearly presented in the paper or discussed in the paper. Would the authors be able to address this?

6) In the discussion, third paragraph, distance to health care is described as one of the associations with depression. In the results section only distance to work is described (under “factors associated with BDI scores for both genders”), whereas both appear to be part of the combined model when looking at table 2. It is therefore not clear to me why the authors chose this construction, as it leaves it up to the reader to figure out how they are linked.

7) In the discussion, under “implications for treatment”, the authors describe how their qualitative data indicate that suicidal ideation is culturally normal, whereas completed suicide is the result of a sent spirit via a Vodou priest. Presumably, this data was described in a different paper, but this is not very clear. Moreover, this is a new concept that is relevant to the paper and the results which is introduced in the discussion, and the authors may therefore want to consider describing this in the introduction.

8) In the method section, the authors describe the use of a SES scale based on possessions: is this a common method? And could the authors provide a reference?

9) Table 2: would the authors be able to explain why the N for the combined model in this table is 396 and not 408 as would be expected? Also, there seem to be several variables that are described as significant in the results section, but are not marked as such in the table (e.g. ‘last time sick went to hospital’ for
males, and ‘disaster cause sadness/anxiety’ for males). Finally, it is unclear to me why the spaces for education for males and marital status for both males and females are left empty. It seems to make sense to mark them either “n.s.” or “n/a” (presumably?), as was done for other variables.

Minor Essential Revisions

1) In the results section the authors state that 18 was the median score on the BDI, whereas in the discussion 18 is described as the mean. Although both could be true, it seems unlikely, and I would encourage consistency.

Discretionary Revisions

1) The authors could consider adding separate headings in the results section for the prevalence results as this is lumped together with demographics despite being their main research question. Also, a separate heading for the results of the fit of model analyses could be inserted.

2) Throughout most of the paper, depression is described first, but in the discussion suicidality is described first. For consistency, the authors could consider restructuring the discussion in line with the rest of the paper.

3) Reading the paper I got confused when I read the “factors associated with BDI scores for both genders” as results were described for males and females separately whereas I was expecting the results for the combined model. The authors may want to consider changing the way this is presented.

Minor issues not for publication

1) Table 1: remove superscript ‘b’ for ‘distance to work’ variable as this doesn’t seem to refer to anything.

2) Table 2: superscript ‘c’ appears twice below the table: last one should be ‘d’.

3) In the discussion, under “implication for treatment”, at the end of the first paragraph, the authors reference papers [30-48]. Just wanted to check whether this is correct or was meant to be [30, 48].

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests