Author's response to reviews

Title: Community stigma endorsement and voluntary counseling and testing behavior and attitudes among female heads of households in Zambezia Province, Mozambique

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Author's response to reviews: see over
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Jimmar Dizon
C/o Victor Minichiello
Journal Editorial Office
BioMed Central

Dear Jimmar Dizon and Victor Minichiello:

RE: MS: 2065612729512881 “Community stigma endorsement and voluntary counseling and testing behavior and attitudes among female heads of households in Zambezia Province, Mozambique
Abraham Mukolo, Meridith Blevins, Bart Victor, Heather N Paulin, Lara M. E. Vaz, Moshin Sidat and Alfredo Vergara”

Many thanks for your letter of May 16th, 2013 in which you invited us to resubmit the manuscript. We have made all the change you suggested together with the changes that were suggested by the reviewers. We thank the reviewers for helpful suggestions. We look forward to the acceptance of the manuscript.

Sincerely,
Abraham Mukolo

RESPONSE TO REVIEWERS

REVIEWER #1
Thank you for the opportunity to read and review the manuscript titled Community stigma endorsement and voluntary counseling and testing behavior and attitudes among female heads of households in Zambezia Province, Mozambique. The manuscript addresses an important topic, stigma and other correlates of engagement in and endorsement of VCT. Stronger understandings of this topic could have important implications for the development of interventions to strengthen engagement in VCT in Mozambique, which is particularly important given the low percentage of participants of the current study who had ever engaged in VCT. This topic, paired with the methodology of the study (i.e., general household survey of female heads of households), are significant strengths of the current work. Despite these strengths, however, there are several aspects of the manuscript that limit my enthusiasm for it.

Response:
We thank the reviewer for the favorable observations and for the balanced review provided below. We have endeavored to accommodate the recommendations from Reviewer#2 (see below), particularly where there were slight differences in details requested to be added or removed from the original submission.

Major Compulsory Revisions
1. The focus of the paper is unclear. The title, abstract, and questions addressed by the paper (described in last paragraph of introduction, on p. 5) are focused on the associations between stigma and VCT. However, the results and discussion are much broader than stigma and don’t seem to highlight the role of stigma. For example, stigma is not even mentioned in the first
paragraph of the discussion. This paper appears to be about correlates of VCT more generally. Relatively, the questions specified within the introduction do not all appear to be answered in the results section (e.g., where are the unadjusted associations between stigma and VCT use behavior and attitudes presented in the results to answer the first question? It appears that only multivariate/adjusted analyses are presented).

Response:
We agree with the reviewer’s observations. We have reviewed the manuscript to focus on stigma as the primary concern. We have provided information about the unadjusted associations between stigma and VCT use behavior and attitudes in the text and in Table 1. As noted by Reviewer #2 (below), some of the stigma relationships that we tested were weakly supported by the data. Hence, in the discussion we have referenced this and also highlighted, albeit briefly, other covariates that were also associated with VCT use and endorsement. See also our response to comment #4 below regarding the rationale for the statistical analysis.

2. The use of stigma theory and constructs are inconsistent, and at times ill-defined. The authors rely on Link and Phelan’s definition of stigma to specify domains of stigma. This definition specifies domains that are necessary to create and maintain stigma at the societal level. Therefore, it is problematic for the authors to attempt to map these domains onto the individual level outcomes of engagement in and endorsement of VCT. The authors choose to focus on stereotyping/negative labeling and social exclusion but do not present a rationale for why these domains, in particular, are important to examine. What about the others? Further, the language describing the domains is inconsistent. At some points, the authors refer to social exclusion (e.g., tables, results). At other points, they refer to discrimination (e.g., stigma measurement on p. 7). Given that discrimination is experienced at the individual level, I recommend adopting discrimination throughout. Finally, the authors never define community stigma. What is this and where does the term come from?

Response:
We have reviewed the manuscript as suggested by the reviewer, to ensure consistency in terminology, conceptual frameworks and definitions. We relied on conceptualizations of HIV-related stigma that have been advanced in the HIV literature (see Mahajan et al, 2008; Nyblade, 2006; Aggleton & Parker, 2002; Earnshaw & Chaudoir, 2009). It is the view of the first author that the majority of these frameworks are based on the modified labeling theory (associated with Bruce Link and colleagues). The modified labeling framework also seems to have informed the measure of HIV-related stigma that was used in the survey reported in this manuscript (Pulerwitz, J., et al, 2008). We have tried to highlight, through citations, the HIV-specific sources of our conceptual framework. We have described the factor analysis that led to the 2 domains of stigma that we used. We have also added text that informs the reader about the source of the 2 domains in both the abstract and the main body of the manuscript. We have elected to use the term “social exclusion” and its acronym “SoE”, rather than discrimination, to be consistent with our measure of stigma. As rightly noted by the reviewer, community stigma, as a social norm, is “create(d) and maintained at
the societal level", hence our earlier reference to its existence in the public mind. Our rationale is that an individual’s endorsement of a social norm about a particular public service/entity is likely to influence his/her engagement with and attitudes towards that service/entity. Hence, an individual’s endorsement of community stigma might explain his/her VTC use and endorsement characteristics. We believe that this approach is similar to the established approach of relating perceived stigma to the use of HIV services by individuals living with HIV/AIDS.

3. The measures are inconsistently described. Information regarding whether measures are from validated scales or were developed from the study, how many items were in each scale, response options for items, reliability, and other details are inconsistently reported for different measures. As an example, the Quality of Life scale is described in great detail whereas there is very limited information reported on the experience/familiarity with HIV infection measure.

**Response:**
We have endeavored to balance the information provided across the measures used. We have also taken Reviewer #2’s recommendations by giving more details about VCT outcomes and stigma. We have provided an appendix that lists items for those measures that are briefly referenced in the text, as per Reviewer #2’s Major Essential Revisions #4.

4. Rationale for statistical analyses is often lacking. Perhaps of primary concern, there is no rationale provided for breaking up the stigma measures (75 vs. 50 points for NLD; 50 vs. 25 points for SE). Why were these not treated continuously? Why are they broken up around different points? Without a rationale provided, one can only assume that they were broken up in this manner because this is where the authors found statistically significant results. The introduction and guiding questions are focused around stigma, therefore it is unclear why the authors go into great detail about how their covariates are associated with their outcomes. The authors also add covariates to analyses without explaining their rationale. An example is on p. 13 (“an additional variable considered in the analysis was...”). All analyses should be explained to readers as the rationale may not be obvious to the readers.

**Response:**
We thank the reviewer for calling this to our attention. The statistician has added considerably to the methods section to better explain the analyses. Table 2 also includes a footnote explanation for the points selected. The reviewer is right that the contrast presented was to highlight where the effect size was greatest, though our intent was not to overstate the results. The p-value is an overall test of the continuous variable, so we felt it would confuse readers to show a confidence interval which overlapped 1 from a region where there was more data (i.e., the 25th and 75th percentiles). We hope by acknowledging this point in the footnote, that readers and reviewers do not feel mislead.

Minor Essential Revisions
1. Within the discussion, results were discussed that were not presented in the paper. An example is on p. 15 (“in separate analyses (results not shown”)}. The authors should show all results of analyses reviewed in the discussion. If the analyses are important enough to be detailed in the discussion, they are important enough to be described in the results.

**Response:** Done as per reviewer's advice.
2. I strongly suggest that the authors do not use acronyms for “NSE” and “SE”. I found this very confusing, especially because SE typically stands for standard deviation. Please use the full terms throughout.

Response:
We thank the reviewer for the suggestion. We have opted for SoE instead of NSE and SE. We have tried to use the full terms as much as possible.

3. This article does not appear to have been proofread thoroughly. For example, there are many instances of inconsistent uses of quotation marks (p. 13: “I don’t know’ with double quotation followed by single).

Response:
The manuscript has been proof read to the extent possible. We hope the reviewers’ concerns have been addressed.

References:

REVIEWER #2
This article has the potential to make a useful contribution to the literature on the influence of stigma on the uptake of HIV voluntary counseling and testing. The underlying assumption that different domains of stigma may have a different association with HIV testing uptake, and therefore may require different types of intervention, is a reasonable and interesting one. However, the results were not particularly convincing in this regard, and thus the conclusion that “domain-specific stigma reduction interventions might be warranted” is relatively weak. The remaining points in the conclusion are adequately supported by the data.

Response:
We are grateful to the reviewer for the balanced review of our manuscript and for the useful suggestions about ways to improve it. We agree with the reviewer that domain-specificity was weakly supported by these data. We have acknowledged this finding in the discussion section and also used it as basis for encouraging further testing of this assertion in subsequent studies.
Nevertheless, overall this is a well-structured manuscript with a clearly defined research question, and appropriate and well-described methods that analyse sound data. The background section reads very well, and clearly frames the research questions within the context of the existing knowledge base on this subject. However, the methods section could be readily shortened without losing any information necessary for the reader to understand how the study was undertaken. Similarly, too much time is spent in the discussion section repeating the results with too little emphasis on considering their implications. The key limitations of the research are stated. The title of the manuscript and the quality of the writing are appropriate for publication (though careful editing is required as there are several grammatical errors and missing words in the text, some of which are detailed below).

Response
We have reviewed the manuscript to include the suggested changes. We have followed the recommendation by Reviewer # 1 to focus primarily on the relationship between an individual’s endorsement of community stigma and his/her use and endorsement of VCT. We have removed those sections in the discussion that were repeating results and focused on likely implications of the results. We have also attended to the grammatical errors, highlighted by the reviewer (see below).

DISCRETIONARY REVISIONS

1. Results: p12. It would be interesting to know whether an individual’s HIV status was asked – it seems that this was the case given that “2% reported that they were HIV positive”. However, presumably this would have posed some ethical issues?

Response:
As rightly noted by the reviewer, HIV disclosure was one of the major ethical issues throughout the survey process. The question--- “Do you have HIV?”---was asked only after carefully structured, IRB approved skip patterns, and mainly among participants who attended PMTCT services. However, the question regarding chances of getting infected had a response option---“already infected”---which also indicated HIV status among those choosing to disclose. We tried to estimate HIV status by a positive response to any of these two items. This is a highly unreliable estimate of HIV prevalence, hence we do not report it. More reliable province-wide estimates are available from other studies.

2. Add % after number to sentence beginning “Respondents with missing data...”

Response: Done

MINOR ESSENTIAL REVISIONS

1. Background paragraph 3: Missing word VCT before “use behaviour” in sentence starting “Therefore, women are an ideal...”

Response: This word was initially added, but this whole sentence was later removed in response to Major Essential Revisions #s 2 and 3 below.
2. Methods, paragraph 1 under section “VCT outcomes” Missing word in the sentence “The skip pattern...asked only those...”

Response: Done

3. Results, paragraph 1: Report the percentage (after the number) of female heads of households with data on VCT use. “Ie/ Of the 3749 female heads of household interviewed, 3407 (X%) had....”

Response: We thank the reviewer for highlighting this as we realized an error in reporting the numbers. The total sample (n=3,749) was used in the analysis of VCT use, but analysis of VCT endorsement was based on the 1,069 respondents who knew about VCT services and so could provide an informed opinion about its importance. We have provided these explanations in the methods and results sections.

4. Missing word between “regarded” and “isolated” in 3rd sentence of paragraph 1 of results section.

Response: The word was inserted

5. Too much time is spent restating the results in the first paragraph of the discussion section – this section should be tightened up.

Response: Done. Recount of results have been minimized, except to support a discussion about the implications of different factors.

6. In the discussion (p14), the authors state that their findings are consistent with the literature from other settings (plural), but only cite one reference from Brazil. It would be more appropriate to indicate findings from a range of settings (preferably other African settings eg/ http://www.ncbi.nlm.nih.gov/pubmed/22943376), or state more precisely that the findings correspond with those from a study in Brazil.

Response: We thank the reviewer for this observation and the suggested reference. We have added that reference, plus one more from Sub-Saharan Africa.

7. P16: The statement that “not receiving HIV test results ...is associated with HIV status knowledge” goes without saying, and does not merit being spelled out in the discussion!

Response: We thank the reviewer for highlighting this redundancy. We have removed the statement.

8. P17 “…our measure of community stigma may have missed out other important domains of stigma that have been identified in the literature.” The authors should give examples here and cite appropriate references.

Response: We have edited and qualified this statement.
MAJOR ESSENTIAL REVISIONS

1. There is a problem with the formatting of the references – no titles are shown in the bibliography list – and this made it difficult to assess the relevance of certain articles that were cited.

Response: We were not able to identify the problem raised by the reviewer. The reference list included titles of all cited manuscripts. We have reviewed this list to ensure that it follows the recommended format. We will be happy to comply with other more appropriate formats.

2. Background, para 3: The statement that women are more likely to be familiar with HIV/AIDS (due to their caring roles) should be supported by data, eg from the Madagascar DHS (HIV knowledge indicators for men and women) as this certainly isn’t the case in all settings).

Response: We agree with the reviewer. We have removed this statement because it is not entirely accurate.

3. Furthermore, it doesn’t necessarily follow that women are an "ideal segment of the community among which to investigate effects of HIV stigma ...". It is also reasonable to assume (unless the authors can present evidence to the contrary) that men are also strongly affected by the effects of stigma in relation to VCT use. There may however be other reasons for choosing to conduct this study among women, but further explanation and/or justifications are required.

Response: Again we couldn’t agree more with the reviewer that this clause in the statement is problematic. We meant to highlight that having attitude and behavior data from female heads of household who fall within that age range is a boon, especially if one wants to answer the kinds of questions that we were interested in getting answer for. We hope the reviewer will be pleased with the corrections and qualifications we have provided.

4. Methods: Too much detail is provided on the wording of questions that were used in the data collection tool. The definitions for the main outcome variables do need to be clearly explained, but less detail is required for the other variables included in the analysis (eg/knowledge of HIV transmission, perceived risk of infection, experience of HIV infection etc etc). The questions used to generate these variables could however be presented in a box that could be referred to within the text.

Response: We have tightened the methods sections. We have tried to balance the reviewer’s need for short descriptions with Reviewer #1’s request for more details. Thank you for the suggestion to include an Appendix. We have provided one as a result.

5. Table 1 is poorly formatted and cluttered making it difficult to read. The column entitled “total” is particularly confusing. The first table should focus on describing the characteristics of the respondents by providing the number and % for i) the total population, and ii) for each outcome. In other words, as follows:

Response: We have reformatted this table as advised, and added other details that Reviewer #1 wanted added, i.e., unadjusted association between stigma and VCT use and
endorsement. We still wanted the reader to see the difference between VCT user/endorsers and those who don’t.

6. Tables 2 and 3 refer to adjusted (rather than crude) odds ratios, but this should be indicated in the table headings. If p-values are being provided in addition to 95% confidence intervals, then these should be for each category of each variable.

**Response:** By including a p-value for each covariate, we present the overall test of significance. Additionally we believe the 95% CI is essential to show the precision of the OR estimate. Tests of significance within categories may be underpowered, and do not contribute additional information beyond the 95% CI. If the 95% CI excludes 1, then there is a significant association between that category and the outcome.

We thank the reviewer for the balanced review and all these useful suggestions.