Author's response to reviews

Title: Facility and home based HIV Counseling and Testing: a comparative analysis of uptake of services by rural communities in southwestern Uganda

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Author's response to reviews: see over
Dear Sir/Madam,

Re: Submission of Revised Manuscript (MS: 3546848974024230)

Please find attached a manuscript entitled “Facility and home based HIV Counseling and Testing: a comparative analysis of uptake of services by rural communities in southwestern Uganda”. We have again reviewed the second set of comments raised by the 3 referees and revised the manuscript addressing the residual “major” comments. The revisions in the manuscript have been highlighted (sky blue highlighter). Our responses to each comment are given here.

Reviewer: Susan Kiene

1. In the abstract the authors say that the objective of paper “was to compare predictors for uptake of facility and home based VCT in a rural context” but yet in the results and conclusions section of the abstract they don’t focus on comparisons. If there were no differences between the two approaches that should be stated.

We have revised the results and conclusion sections in the abstract to highlight the comparisons between the two VCT models.

2. The authors are confusing client initiated and provider initiated approaches. They say “Client initiated testing (VCT) can be offered through different innovative approaches such as in the work place, in mobile clinics and using the home based model.” Home-based testing is not a client initiated approach but is provider-initiated. This needs to be corrected.

We have adjusted this sentence to read “VCT can be offered.....” (page 5) and also included the reference that reports that client initiated testing can be offered in the home environment i.e. reference [14]. An extract from reference [14] states: “Client-initiated HIV testing and counselling (also called Voluntary
Counselling and Testing, or VCT) involves individuals actively seeking HIV testing and counselling at a facility that offers these services. Client-initiated HIV testing and counselling is conducted in a wide variety of settings including health facilities, stand-alone facilities outside health institutions, through mobile services, in community-based settings and even in people’s homes.”

3. Since the results in the present paper come from only the baseline data discussion of the details of methods of the follow-up should be removed except where initially mentioned in summarizing the context of the present paper.

This detail at the end of the 1st paragraph on page 9 has been removed.

4. With the multiple t tests and chi-square tests as presented in table 1 did the authors adjust p values required for statistical significance to prevent an inflation of the type II error rate?

We have noted the concerns of the reviewer, however although we had a common outcome (type of VCT model) the variables in column 1 of table were each independent and this analysis therefore does not constitute multiple t-tests.

5. I did not see the authors report the % of each study arm that accepted testing and received results. This is an important comparison point between facility and home-based testing.

A statement has been included in the first paragraph of the sub-section on background characteristics on page 12 that addresses this comment.

6. When discussing the results from Table 2 on page 13 the authors should focus on the findings from the model including all predictors (adj ORs) not the unadjusted OR values. Focusing on findings from multiple univariate analyses increases the Type II error rate because of the multiple comparisons being made.

We have noted the concern of the reviewer here, however the text under table 2 on page 13 is not a discussion but a summary of the result highlights in the table. In the discussion section we mainly focus on findings from the model including all predictors (adj Ors).
7. The results presented in Table 3 and 4 should only be reported in text as they are not major findings and are merely describing the sample as a whole, not comparing facility based to home based.

We concur with the reviewer that the results presented in table 3 and 4 are not major findings of this paper. In our view however, the readers are able to have a better perspective of the knowledge scores with the items as presented in table 3. We therefore propose that table 3 be maintained. We have however removed table 4 and reported the findings in text as suggested.

8. Since the authors state that their objective is (pg. 7): “This study therefore seeks to compare predictors for uptake of facility and home based VCT in a rural context” it would make more sense to have one table that is a combination of table 1 and 2 (and 5) that compares the two study arms reporting ORs from the univariate analysis and adjORs from the multivariate analysis. In fact, the analyses relating to their objective are buried under other more general descriptive findings that they report about the entire sample.

It is our view that Table 1 compares the background characteristics of the clients who received FB-VCT to those who received HB-VCT to assess whether the two groups are comparable. Table 2 assesses the association of various variables with the outcome (which is the different VCT models). The final table (now table 4) highlights the independent influence of the different significant variables on the outcome. In our view table 1, 2 and 4 should stand alone as is commonly the practice in scientific papers. We therefore propose that table 1, 2 and 4 be maintained in the paper.

Reviewer: catrin Evans 2

I still feel however that the overall objective, purpose and significance of the study needs to be more clearly stated. Given that the purpose was to compare predictors of home based vs. facility based VCT, you need to articulate very clearly whether the reported differences have any particular significance for service design. From my reading of this paper, I am still struggling to determine what implications the findings have - the 'so what' question? It seems that the predictors are very similar for both models of testing and that the differences are what one would expect given the very different nature of the models. But then each group was only offered one particular model rather than a choice. In fact, this is perhaps the main point (and the main limitation that needs to be more clearly addressed) - that on the whole there were few major differences in factors influencing VCT or home based uptake - why do you think this is? And if this is the case, what is the implication of that for service design? If the groups had had a choice of facility based vs. home based testing then the comparison of the predictors would have been more meaningful. My suggestion therefore would be to re-state the study's purpose, significance and implications for future service design and delivery (if any) in the introduction and again in the conclusion. For example - in the current abstract, the final paragraph refers only to the fact that uptake (of either model) is influenced by certain factors.
However - given that the purpose of the study was to compare the models, we need a conclusion that refers back to what you have found in comparing the 2 models rather than making a rather general comment. I still feel however that the overall objective, purpose and significance of the study needs to be more clearly stated. Given that the purpose was to compare predictors of home based vs. facility based VCT, you need to articulate very clearly whether the reported differences have any particular significance for service design.

We have revised the results (abstract) and conclusion sections in the manuscript to better reflect the differences between the models and significance for service design.

I'm afraid that I still do not understand the explanation for how the districts are average. What does "according to objective performance accomplishment scores" mean? I think this still needs more elaboration.

We have given an example of an objective performance measure in paragraph 1 on page to clarify this issue.

Likewise, I feel that the section on STI symptoms is still misleading. As previously suggested we highlighted this issue as one of the limitations of the study.

I am, not quite sure why the data from the 2 groups have been combined in tables 3&4 rather than comparing the two groups. The results in table 3 and 4 (original table 4 has since been removed) were combined mainly because they were not considered major of the paper.

Hoping for your kind consideration

Yours sincerely,

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