Author's response to reviews

Title: Scaling up combined community-based HIV prevention interventions targeting truck drivers in Morocco: effectiveness on HIV testing and counseling

Authors:

Hakima Himmich (h.himmich@gmail.com)
Lahoucine Ouarsas (ouarsas.lahoucine@gmail.com)
Fatima Zahra Hajouji (fatimazahra.hajouji@gmail.com)
Patrizia Carrieri (pmcarrieri@aol.com)
Perrine Roux (perrine.roux@inserm.fr)

Version: 3 Date: 18 March 2015

Author's response to reviews: see over
1. Is the question posed by the authors well defined? Yes

2. Are the methods appropriate and well described? There could be many other approaches for measuring the intervention impact. The method used in the paper in true sense it is a before–after design. Mathematical models or careful statistical methods are critical to measure the impact of intervention in such a design.

Though the before and after samples are considered for testing the effectiveness of intervention, the two samples cannot be considered as control and experimental groups in true sense. It is also not convincing to the readers that the two groups of 2012, one exposed to the intervention and other did not, really are similar.

Hence, it is difficult ascertain the impact of intervention without controlling the correlates and confounders. Even if this is a limitation, they should have used both the samples in the context of multivariate analysis. The authors mention about two control groups, but only one control group is used in most of the analysis.

A) We thank the reviewer for these comments. We realized that the inclusion of the 2007 group could be useful to assess the effect of the intervention and adjust for potential confounders.

It is important to remember that the two surveys (2007 -2012) were conducted using exactly the same methodology. Nevertheless we were mainly interested in the results of the 2012 survey in order to orient interventions to subgroups who were still less likely to be HIV tested (i.e. the fact that truck drivers who did not use condoms with sex workers was independently associated with a reduced likelihood of being HIV tested).

To identify possible differences between the two study samples (2007-2012) we created a new Table 1 according to the reviewer’s suggestions. To do this, we completely re-examined the 2007 database. As the sampling method was the same and each sample was representative of the population of truck drivers in the same geographical area in both 2007 and 2012, some differences observed may be due to the natural evolution of the population surveyed or to the effect of the intervention. This allowed us to identify potential confounders of the effect of the intervention on HIV-testing and counseling, as suggested by the reviewer.

In addition, after pooling the two surveys we compared the characteristics of individuals (e.g. HIV knowledge, sexual behaviors, etc..) which were presumed to be modified by the intervention. We also compared individuals unexposed in 2007, with those unexposed in 2012 and those exposed to the intervention in 2012, in order to better evaluate the effect of the intervention on the different outcomes (Table 2).

Finally, when analyzing factors associated with HIV screening and counseling, we performed a main analysis on the 2012 survey, taking into account confounders identified in Table 1, and a sensitivity analysis including individuals in the 2007 survey as an additional unexposed group.
The sensitivity analysis showed that after multiple adjustment no differences in HIV testing were found between unexposed in 2007 and unexposed in 2012.

We modified the methods section according to the new analyses performed:

“To compare the characteristics of the study samples enrolled in the 2007 and 2012 surveys, we used a Chi-square test as appropriate for categorical variables (table 1). To identify potential confounders or factors which were presumed to have been modified by the intervention, we compared specific behaviors or knowledge in the study samples in unexposed (2007, 2012) and exposed (2012) using a Chi-square test as appropriate for categorical variables and for sub-group comparisons (table 2).”

“Finally, we ran the same analysis (sensitivity analysis) on an extended dataset also including the 2007 study group (used as an additional unexposed group) to test the robustness of the results and the association between the intervention and VCT (table 3).”

I also noticed that in some places important statements are made without providing the data. For instance, "It is important to point out that the prevalence of HIV testing and counselling in those who had not been exposed to the intervention in 2012 (i.e. the control group) was comparable to that in the sample in 2007 (i.e. the historical control group)” (line numbers 276-279).

C) All these data are now available in the tables and referred to in the results. Please look at the sensitivity analysis in Table 3.

Moreover, this statement kind of contradicts the statement that “In the 2012 survey, the great majority of HIV testing had been performed on a voluntary basis (80%) compared with only 60% in the 2007 survey” (line numbers 289 and 290). That means even in 2007, before intervention, 60% them had undergone HIV testing. Similar statements can be observed on line numbers 294-295. I am wondering how come recent five years of duration did not have any impact on HIV testing as stated by authors’ online numbers 276-279.

D) The reviewer is correct about the lack of clarity of these sentences which have now been removed or rephrased. We also completely re-focused the analyses according to the reviewer’s comments.

The authors also make a statement on line numbers 263-264 such that “no significant differences was found among the characteristics presented in Table 1 when comparing participants in the 2007 and 2012 surveys (data not shown)”. I think it is important to present the data.

E) The reviewer is correct and thanks to re-examining the whole 2007 database, we were able to compare more thoroughly the characteristics of the two study groups (2007 and 2012). As previously indicated, despite identical sampling procedures, some differences existed due to either the natural evolution of the population (Table 1) or to the effect of the interventions on knowledge and behaviors (as also shown in Table 2).

I am also not happy about using P-value of .20 without strong justification (line 249).

F) This is a standard approach suggested by Hosmer-Lemeshow (Applied logistic regression, strategies for model building - John Wiley & son 2000) to ensure the inclusion of variables whose
effect on the outcome is not significant in the univariate analysis due to confounding. We added the reference for the choice of this cut-off.

My main concern is regarding the rigorousness of data analysis. For example, the authors state on page 12 on line 307-308 that "these results were confirmed even after multiple adjustments for other potential correlates and confounders (Table 2)." I think they are referring to Table 3 not Table 2 here. The percentages in Table 2 are not adjusted for possible correlates and confounders, and hence the differences between the two sets of percentages can be tentative. Basing their discussions and conclusions on these differences are not quite correct. In my opinion the multivariate analysis should have taken the differences in the two surveys regarding the HIV testing and counselling as dependent variable especially when the differences are there (Figure 1). Another evidence of this can be seen from line numbers 294 and 295.

G) We have completely re-focused the analyses according to the reviewer's comments.

Table 1: A comparison of the characteristics of the two study sample (2007-2012). This table enabled us to identify potential confounders.

Table 2: Comparisons of indicators presumed to be modified by the intervention (two control groups – historical (2007 study sample), unexposed in 2012, exposed once, exposed more than once.

Table 3: Analysis of the effect of the intervention on HIV screening in the 2012 survey
Sensitivity analysis conducted on 2007 and 2012 survey.

We corrected errors related to tables in the text.

Alternatively, the authors should run two multiple regression equations: one for 2007 and another for 2012. Then they should decompose the mean difference in the dependent variables and demonstrate the effect of interventions on HIV testing and counselling after accounting for other correlates and confounders.

We preferred the previous suggestion with a main analysis on 2007 taking into account the confounders identified in Table 1, and a sensitivity analysis pooling the two surveys together. Again our thanks to the reviewer for the in-depth reading and pertinent suggestions.

3. Are the data sound? Yes
4. Do the figures appear to be genuine, i.e. without evidence of manipulation? Yes
5. Does the manuscript adhere to the relevant standards for reporting and data deposition? Yes
6. Are the discussion and conclusions well balanced and adequately supported by the data?

I do have some concerns regarding the discussion section. Some of the explanations that are provided in the discussion section for the results appears to be contradictory. For instance, the authors argue that those respondents with male sexual partners are receiving the HIV testing and counselling as they may perceive that they are at higher risk. How is it that with repeated education as well as behavioural interventions those respondents who are visiting commercial female sex workers do not perceive themselves at higher risk and thereby do not undergo HIV testing. Similar kind contradictory statements are found in the discussion section.

As in the new analysis we also introduced knowledge about confidentiality of HIV-testing, which was found to be a significant predictor of the outcome, the final model was different than that presented in the previous version of the manuscript.
Exposure to the intervention was significantly associated with HIV testing and counseling, after adjustment for self-employment, inconsistent condom use with sex workers and awareness of the confidentiality of HIV testing. This implied the need to discuss the new results and remove what was no longer found to be relevant in the multivariate analyses. Please see the changes in the discussion section. For respondents visiting sex workers, we modified the text as follows (see tracked changes in the attached manuscript):

"Individuals reporting inconsistent condom use with sex workers were less likely to report HIV-testing than those reporting consistent condom use or inconsistent condom use with occasional partners. This highlights the need for prompt, tailored and combined interventions for this group where the risk of sexual transmission of HIV is still underestimated. Previous studies have already underlined the effectiveness of sexual risk reduction interventions targeting female sex workers which potentially have an indirect effect on their clients (e.g. the decision to use condoms, or to go for testing, etc.) [18, 19]."

7. Are limitations of the work clearly stated? To some extent

We added the following sentence to the strengths and limitations section

“In order to confirm the robustness of the results and to include an additional unexposed group, we conducted a sensitivity analysis including the two surveys which showed that unexposed individuals - whether unexposed in 2012 or because they were interviewed before the intervention - had a similar likelihood of not being HIV-tested.”

8. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? Yes

9. Do the title and abstract accurately convey what has been found? To some extent

We completely reformulated the abstract following the new analyses

10. Is the writing acceptable? Can be improved

The new version of the manuscript has been thoroughly revised by a mother-tongue professional copyeditor with 10 years’ experience in the field.