Author's response to reviews

Title: Effect of a peer-educational intervention on provider knowledge and reported performance in family planning services: a cluster randomized trial

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Author's response to reviews: see over
Dear Nina Tirmus,

First I want to thank both yourself and the reviewers a lot for all valuable comments. I am so sorry for the long delay to respond, which was due to my personal problems. I hope you can accept my apology. In the following I have provided detailed point-by-point responses to both yours and the reviewers’ comments:

Response to your comment on information about ethical approval

I had forgotten to write about ethical approval of the study. I added the text below as last paragraph of Study design in the Methods section.

Permission to undertake the study was obtained from the National Ethics Committee of the Ministry of Health and Medical Education in Tehran and from the Ethics committee at the Tabriz University of Medical Sciences. Approval was also received from authorities at the district health centre.

Response to the comments by reviewer 1 (Danette W McKinley)

Reviewer’s comment:

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore):

I think I understood the results in Table 3 - were these predicted means based on the regression analysis (regressed on age, facility, and degree)? If so, then standardized mean differences can’t be calculated. Reporting the r-squared values from the regression analysis may be appropriate in the text. Standardized mean differences (effect size d) can provide the readers with a practical index of the potential effect of the intervention.

Authors’ response:

There was no compulsory revision and only one suggested discretionary revision. We appreciate the concerns for clarity in the table of the main results (now table 2). We have modified the table to make the information about the differences more visible and added some information to make it clearer. However, we have chosen not to add the more extensive discretionary revisions suggested by the reviewer.
Response to the comments by reviewer 2 (Mark A Albanese)

Discretionary revisions

Reviewer’s comments:

1. The topic is guidelines for family planning services. It would seem like this is a minimum competency situation. The instruments were created by eliminating items that in-charges answered correctly at 80% or more. This seems incompatible with a minimum competency situation and probably underestimates the true knowledge level. However, as a means of making the survey less lengthy and focusing on areas for change, it was probably a practical thing to do. Some comment on why this was done would be useful.

2. All the details about how the authors ended up with 19 items in the first section and 22 items in the second section are confusing. It would be better just give a paragraph indicating that out of 26 initial items, 19 remained after removing 7 items that had either >80% correct or….. and of 36 items in part 2, 22 remained after ….

Authors’ response:

Based on these valuable comments we have revised the second paragraph of “Data collection instruments” in the Methods section.

To make the survey less lengthy and focusing on areas for change, seven items from part I and eight from part II, which were answered correctly by more than 80% of the participants at the needs assessments, were excluded at the post-tests. Also, two items from part I were excluded due to their unssuitability, as mentioned by the participants, and five questions from part 2 were excluded due to discrepancies between the national and WHO guidelines. The performance questions were excluded at follow-up I due to the short time (only one month) available to make any change of performance. Two items about the weak points of the providers’ reported performance were added to part 1. Therefore, the post-test questionnaire consisted of three parts; 19 items in part 1 (general knowledge, see Annex), 22 questions in part 2 (eligibility of the methods), and five open ended questions (part 3). The same post-test questions plus the fourth part of the needs assessment questionnaire were used at follow-up II. The questionnaires also included some questions on characteristics of the providers, and at follow-up I there was also a question about the providers’ views on the usefulness of the educational programme.
Reviewer’s comment:
3. Tables 2 and 4 could be put in an appendix to make the presentation simpler and allow those with a more abiding interest to see the details if they so desired.

Author’s response:
We have moved table 2 to an appendix, and simplified table 3, which is now table 2 (see also response to Reviewer 1).

Reviewer’s comment:
4. A very interesting feature of the study is that the authors know that some of the intervention facilities did not implement the intervention, so the results are really diluted by the non-implementers. It would be interesting if they could run a separate analysis in which they removed the non-implementers from the analysis.

Author’s response:
We have not run a separate analysis as we consider it more appropriate to present the results for all facilities in the intervention arm.

Compulsory revisions

Reviewer’s comment:
1. Table 3 is the most critical results presentation. However, it needs to add the sample size and SD with the means reported. This could be done by having a separate line for in-charges and all providers rather than both across the top.
Also, the reliability (internal consistency) of the sections needs to be reported.

Author’s response:
SD of means has been added in the previous table 3 (current table 2) and some revisions have been done on the format of the table (see also response to Reviewer 1).
The reliability of the instrument is reported in the Methods section, third paragraph of Data collection instruments, which now reads:

*Content validity of the instrument was identified by the experts and reliability by Cronbach’s alpha, separately for different parts, showing more than 0.7 in each part.*

Reviewer’s comment:
2. It is not clear what the motivation was for in-charges or anyone else in the study to participate in the intervention. It is addressed to some degree in the discussion, but it needs to be brought up to the methods.
Author’s response:

We have added a paragraph explaining the lack of obvious incentives and possible motivational factors, at the end of Study design in the Methods section:

*There was no special incentive for participation in this study. The in-charges were asked to participate by their supervisors, who have responsibility for their continuing education. In order to ensure the implementation of the educational program at the interventional health facilities, a written report signed by all participants had to be sent to the research team. There were also some questions about the quantity and quality of the educational sessions in the questionnaire at follow-up I.*

Reviewer’s comment:

3. These are pretty remarkable results for such a limited intervention. It could be that there any cultural aspects that might make it more effective than it might be elsewhere. Some discussion of this is needed.

Author’s response:

Thank you! We have added a paragraph to the Discussion (paragraph 3) explaining possible effects of environmental factors on the results of the study:

*The positive effects of the intervention may seem relatively high for such a limited intervention. There is no obvious cultural or environmental factor that can be considered an explanatory factor for a better outcome in this context. At the time of the study, some quality promotion programs, like quality monitoring and conducting counseling educational workshop, including client rights, had been implemented in the facilities. Although this took place in both the intervention and control facilities, the providers in the intervention facilities may have been more receptive to the type of quality improvement method used in the study than they would have been otherwise. It might be a factor to consider when drawing conclusions about how this method can be used in other contexts.*

Reviewer’s comment:

4. In the retrospective power analysis, the use a "design effect for follow-up I=1.74". Please define what is meant by this value in more specific terms. If it is an effect size, meaning the differences in means divided by an estimate of the within group SD, that is a massive effect size to expect. Why would they have expectations for such a large effect
size? If it is the actual effect size obtained in the study, this should be reported in table 3 and discussed in the text as a remarkable impact for such a limited intervention.

**Author’s response:**

As the allocation unit in the intervention or control unit was facility (cluster random allocation), the design effect mentioned in the calculation of sample size is the design effect due to cluster random allocation. This is explained in paragraph 7 in the Discussion, where we have now further clarified the information:

*However, after the study, the calculated statistical power for the total percentage score of knowledge was 81 and 90 per cent for follow-up I and II, respectively; considering m1=50, m2=60, sd1=sd2=17, α=0.05, and a design effect due to cluster sample allocation for follow-up I estimated as 1.74 and for follow-up II as 1.44.*

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