Author's response to reviews

Title: Effect of an institutional development plan for user participation on personnel's knowledge, practice, and attitudes. A controlled study.

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Author's response to reviews: see over
Dear Editor,

MS:1842982688540052. Resubmission and comments to reviewers.
"Effect of an institutional development plan for user participation on personnel’s knowledge, practice, and attitudes. A controlled study".

We are grateful for the possibility to resubmit this manuscript. Our responses to the comments from the reviewers are as follows:

Reviewer # 1

1. Thanks for the opportunity to review this manuscript. The topic is of considerable interest and relevance. I do have concerns with the writing style, I appreciate the authors are not native English speakers, but I found the manuscript a bit awkward and difficult to follow. I recommend the assistance of a proficient English speaking editor.

We have consulted a language editing service for the revised manuscript.

2. Major compulsory revisions:
The abstract needs to be rewritten to more clearly convey the background and the study undertaken. It is currently quite vague, for example: 'various activities', provides no information about the type of interventions. Also the statement 'controlled studies' is insufficient, more information is needed.

We have extended the Abstract to clarify and include more information on the background and methods. We have changed the term "controlled study" to "non- randomised controlled study" as well giving a more extended description.


We have extended the Background section, adding more literature on the concept user participation, and the proposed benefits and barriers. We have also used the literature suggested by the reviewer.
4. Methods:  
Some information about the interventions and why they were chosen should be included in the text with reference to the table.

We have added a sentence under Intervention in the Methods section to make it clear how, and by whom, the different initiatives in the plan were chosen. There is a reference to table 1 in this paragraph;

“The development plan included several initiatives to enhance user participation both on a system and an individual level (table 1). The initiatives in the plan were chosen by the project group based on their knowledge about user participation, and the services they wanted to provide after the reorganisation.”

5. How the adapted questionnaire was piloted prior to use needs to be included.

We have added a sentence in the Methods section;

“The translated questionnaire was not piloted prior to use, and we did not attempt to validate the Norwegian version of the questionnaire.”

6. Results: The first paragraph should be moved to the methods section.

Since response rates often are considered a part of the result we are accustomed to describing the sample participating in the study in the first paragraph of the Result section. We have however rewritten the paragraph to focus on the response rate (result) and not the number of questionnaire mailed.

“Eighty-nine of 184 (48 %) members of professionals responded at the intervention hospital at baseline (sample 1), and 93 of 179 (52 %) responded 16 months later (sample 2). At the control hospitals, 133 of 221 (60 %) professionals responded at baseline (sample 3), while 123 of 211 (58 %) responded 16 months later (sample 4). The total number of participants was 438, and the total response rate was 55 %.”

7. Discussion:
Some very useful discussion included here. More consideration needs to be given to organisational issues and attitudes of mental health professionals - again I recommend the references listed above.

We have added some text in the Discussion (and in the Introduction) on this, and have used the suggested literature.

8. There needs to be a heading limitation where all of these issues are considered together. The non-matching of questionnaires over the two time periods is a major limitation that is not acknowledged. Although the issues around confidentiality are understood, it is possible that an entirely different sample completed the two surveys therefore making attempts to compare meaningless.

We agree that a paragraph describing limitations is lacking. We have therefore extended the section Strengths and limitations;
“A major limitation in this study is that the samples were not matched, due to maintaining the participants’ anonymity. The lack of matching could have provided different samples at the two time periods, and therefore influence the findings considerably. Another limitation is that only professionals were studied. Investigating any changes in the users’ experience of user participation would complement the findings. We chose to invite all employees at the hospital to investigate any changes in the organisation as a whole. Including only those working directly with patients might have given different results. Although the study included all three district psychiatric centres in one hospital, providing good representation of professionals in this area, studying professionals in a different area or country could have provided different results. The lack of piloting and validation of the CPQ could be considered a limitation. However, since this was a study comparing the changes in the intervention group with a control group this would not have direct impact on the results.”

9. Conclusion
"No effect" should read "no significant effect".
We have changed this according to the reviewer’s suggestion.

10. Minor essential revisions: p. 3: Users' should read users
We have changed “users’ participation” to “user participation”.

Reviewer # 2

Major Compulsory Revisions:
1. Although the initiative to perform a controlled study after the effects of development plans for user involvement is honourable, the question is minimally elaborated. Unclear is precisely why and in which way an effect is to be expected of a development plan on knowledge, practice and attitude regarding user participation. I would like to advise the authors to explicate these aspects in their introduction. Additionally, an elaboration on the positive effects of user involvement, more than a few references, would be advisable.
We have added text in the Background section to argue why a development plan could have an impact on the professionals;

“The implementation of the development plan was intended to enhance user participation in the hospital. Since professionals are vital for exercising user participation in collaboration with the users, it would be reasonable to anticipate that the organisational changes should have a positive impact on the professionals’ knowledge, practice, and attitudes towards user participation. In addition, the comparison of any change with a control group would ensure that any impact was due to the development plan. The aim of this study, therefore, was to study whether implementing a development plan intending to enhance user participation in a mental health hospital had any effect on the hospital professionals’ knowledge, practice, or attitudes towards user participation.”
We have also added text in the Background section elaborating on the potential benefits and barriers towards user participation. (See also comment no 3, Reviewer 1.)
2. Although this was not a validation study on the CPQ, the validation of the CPQ was limited, when used in a different language. Only face validity has been assessed. As far as I can see, the original author has not been asked to assess the quality of the back-translated version. No factor analyses were performed to assess the subscales of the questionnaire. Reliability tests were not performed to assess the quality of the questionnaire after translation into Norwegian. Unclear is how the experience of Richter et al. (2009) is used, when they used this same questionnaire in their study. In the discussion section none of these limitations and their possible consequences for the results are mentioned.

Richter and colleagues’ article using the CPQ was published in April 2009, several moths after we started the data collection for this study, so we could not use their Norwegian version. In addition; since this is a comparison study the lack of piloting and validation will have not influenced the results as such. We have however added some text in the Methods section and the Discussion section to state this explicitly;

“(Methods): […] The translated questionnaire was not piloted prior to use, and we did not attempt to validate the Norwegian version of the questionnaire.”

“(Discussion): […] The lack of piloting and validation of the CPQ could be considered a limitation. However, since this was a study comparing the changes in the intervention group with a control group this would not have direct impact on the results.”

3. I consider the choice to perform post-hoc analyses with the total sample rather debatable. No rationale is given why the authors made this choice. Neither is it explained in the statistical analyses. The first paragraph of the discussion states that no effects were found of a development plan to enhance user involvement. But that a tendency was found towards a more positive development in the total sample. That is in itself a positive finding, but not congruent with the introduction and the objective of the study. The objective was a controlled study after the effects of a development plan and no significant effects were found. To mark the controlled study as a major strength of the study and concurrently devaluate this fact by taking the two groups together is questionable. The paragraph ‘Personnel’s views about user participation are generally evolving’ is not supported with literature or other research and based on the findings in the total sample. In my opinion too much value is attached to the findings based on the total sample. I would advise to delete table 5 and to reveal the rationale to attach so much value to the findings based on the total sample.

We agree with the reviewer and have omitted the results from the analyses of the total sample. Table 5 is also removed from the revised manuscript.

Minor Essential Revisions
1. I had some difficulties to understand the text. Rather uncommon words or phrases are used. I would strongly advise to ask a native speaker to edit the text.

We have consulted an English language editing service.

2. I would consider a different word for 'personnel'. Consider professionals or staff.

We have replaced “personnel” with “professionals” throughout the manuscript.
3. The word ‘cardinal’ (p. 3 background) is in my opinion uncommon. I would consider a different word.

We have changed the word “cardinal” to “vital”.

4. The word ‘sanctioned’ p.5 is rather uncommon. I would consider approved.

“Sanctioned” is replaced with “approved”.

5. P. 5 subheading implementation; I would consider to change ‘oversee’ in supervise.

“Oversee” is replaced with “supervise”.

6. Sentences do not begin with numbers usually. P 7; ’12 of the ….’ ; ’11 of the ....’; p. 8 ‘9 % ....’.

We have replaced the numbers in the beginning of sentences with words.

Discretionary Revisions
1. The samples were not matched, which could be seen as a weakness, because of the fact that is asked after private opinions regarding several aspects of user involvement. I would have expected that this was mentioned as a limitation of the study in the discussion section.

See comment no 8, Reviewer 1.

2. The paragraph at the end of page 7, starting with ‘12 questions…..’ is very detailed, but not all the information is relevant. I would advise to shorten or rewrite this paragraph.

We have shortened this paragraph considerably since the information is in the tables and in the literature.

3. P. 8 statistical analyses; I am used to assess potential confounders based on the change in the B-value, rather than to assess the p-value. A change in b-value > 10% is considered as a common cut-off for potential confounders.

We agree that this is also a valid approach, but have kept using the p-value as we had predefined using the bivariate analysis to select variables to control for.

4. There are 5 tables containing much similar text. I would advise to delete table 5 and try to combine table 3 and 4.

We agree that there is much similar text, but this is done to make each table self explanatory. We find it hard to combine table 3 and 4 and have not done so. However, we have deleted table 5 and only given the central values in the text.
5. In the result section in the paragraph ‘changes within the group’ is stated that there are no systematic differences between the two groups at baseline. I wonder if and how that is tested. I could not find this information in one of the tables.

The reviewer is right. There are no comparison tests of the groups at baseline in the tables. We have added a parenthesis in the text to state the test used;

“Comparing the distribution in the intervention hospital and the control hospitals at baseline indicated no systematic differences between the groups (tested using Pearson’s chi square).”

Reviewer # 3

Thank you for the opportunity to review this manuscript addressing the issue of user participation in mental health services and how an institutional development plan can impact on health care providers’ attitudes, knowledge and practices. The paper is interesting on an important topic, however before a decision on acceptance or rejection the authors will have to respond to the issues listed below.

1. Major Compulsory Revisions:
   Study background needs to pay more attention to clarification of the core components of user participation, what the objective with user participation is in mental health hospitals/organizations and how this relates to the current study. I recommend a more updated literature review including recent articles on implementation of service user involvement in mental health services. In addition the authors should reproduce more of the studies referred to as relevant to the current study. For example: what are the benefits of user participation and what are the potential barriers to implementation in mental health care.

   We have added text in the Background section. See also comment 3, Reviewer 1.

2. The authors also need to clarify key concepts in the study aim such as: ‘a controlled study’, ‘development plan’ and ‘personnel’.

   We have added text on this in the Abstract, the Background, and in the Methods section.

3. The methods section needs reorganization, and more information. The description of the study design is presented several places (in the first paragraph, as part of participants and data collection). I think this information needs to be presented under a heading ‘Study design’. Is this an ‘intervention study’, a ‘controlled study’ or both? As I find this confusing, the authors need to be clear on this.

   We have added a paragraph “Study design” in the beginning of the Methods section to clarify the design.

   “Study design: This was a non-randomized controlled study including professionals from three mental health hospitals in Central Norway. One of the hospitals (intervention hospital) implemented a development plan to enhance user participation, and two hospitals participated as control group. A non-matched sample of professionals from the three
hospitals filled out the same questionnaire two times, 16 months apart. The aim was to compare the change in the intervention hospital with the control hospitals. The study took place from November 2008 to December 2010, and was approved by the regional committee for medical and health research ethics in Central Norway, the Norwegian Data Inspectorate, and the hospitals’ management.”

4. Three mental health hospitals were included in the study. Why were these included? Where there any inclusion criteria and could these be reported?

We included all district psychiatric centres in one main hospital, and there were thus no inclusion or exclusion criteria. We have included a sentence in the Methods section to describe this;

“*The three hospitals are part of the same hospital trust, and include all district psychiatric centres in one of the trust’s main hospitals.*”

And what are potential biases with the study sample?

We have added a sentence in the Discussion under Strengths and limitations to elaborate on this;

“Although the study included all three district psychiatric centres in one hospital, providing good representation of professionals in this area, studying professionals in a different area or country could have provided different results.”

5. Could the authors also explain who the user representatives in the project group and implementation group are? Are they representing mental health service user organizations? Did the participants in the project group and implementation group take part in the two surveys? What impact might this have had on the study results?

We have added some words to the Methods section (in Intervention and Implementations) to state who the user representatives were;

“… user representatives (recruited from mental health user organisations)…”

Since the user representatives were not employees at the centre during the study they did not complete surveys. The number of user representatives at the intervention centre was small and would nevertheless not influence the study results.

6. The authors write under the sub-heading ‘Participants’: “The group of employees answering the questionnaire was thus not the same in the first and the second time of measurement.” When those responding to the two surveys are not necessarily the same people, the authors needs to address this issue and its potential consequences for study results in study limitations.

See comment no 8 to Reviewer 1.

7. In the results section the authors do not comment on the baseline results, but solely focus on the changes in scores from baseline to follow-up. Few quantitative studies have
been conducted on implementation of user participation. Therefore some comments on
the baseline results would be useful.

We are not sure we understand the reviewer on this point. We have commented on the
differences at baseline in the result section. But we can not see that further comments on the
baseline results are needed in a study on the effect of the intervention.

8. There is also information missing in the result section. When referring to the direction
of change from base-line to follow-up for the area: Knowledge about user participation
it was found an increase in knowledge for nine out of ten questions (page 10). What
questions? Table 4 only includes five questions for knowledge. Please be clear on this
also for the areas: Practice of user participation and Attitudes towards user
participation.

Changes within the groups for all questions are in Table 3. We can nevertheless see why the
reviewer have looked in Table 4 for this information. To avoid confusion we have moved the
text introducing Table 4 to after the description on the within-group changes.

9. In the last paragraph in the results section, I miss information in the text about how
many people reported that the unit’s attitude towards user participation is good or very
good. This information is not presented in table 5.

The paragraph and table 5 is removed from the revised manuscript.

10. In general the discussion needs to be rewritten to focus on viewing the study results
in relation to previous research using the CPQ and literature on implementation of user
involvement in mental health services.

Since the present study is about (and is the first to investigate) the effect of an intervention
compared to a control group, we do not find it pertinent to compare the results of this study
with the results of cross sectional surveys using the CPQ. We have however referred to
previous research using the CPQ in the section where we discuss the use of CPQ as an
outcome measure.

11. The last sentence under the heading “Personnel’s views about user participation are
generally evolving” is unclear. What are the general changes in society? This needs more
elaboration.

We have removed all results from analyses on the total sample.

12. A discussion of study limitations must be expanded and preferably be placed
towards the end of the discussion. What are potential weaknesses with the study design
and methods?

See comment no 8 to Reviewer 1.

Only service providers were approached in the surveys. This issue needs to be addressed
when studying user participation in mental health services.

We have added text in Strengths and limitations on the fact that this study is on professionals;
“Another limitation is that only professionals were studied. Investigating any changes in the users’ experience of user participation would complement the findings.”

13. It is commented that there are some concerns with the outcome measure CPQ. I believe that the authors need to be more specific about these and what the consequences are for the current study. Richter et al. (2009) has commented that the CPQ should be critically evaluated because of high interrelatedness among the items from the various topics. They also question if the items in the CPQ are valid indicators of the construct consumer involvement.

In addition to the discussion on the CPQ as an outcome measure, we have added a paragraph under Strengths and limitations describing the impact the use of CPQ might have had on the present study;

“The lack of piloting and validation of the CPQ could be considered a limitation. However, since this was a study comparing the changes in the intervention group with a control group this would not have direct impact on the results.”

14. The conclusion needs to be rewritten and include a paragraph on potential implications of this study to clinical practice in mental health services, and the relevance of the study results to future research in this area.

We have added a paragraph on clinical and research implications in the Conclusions section;

“Clinical and research implications: It is not possible to give any advice regarding the implementation of initiatives to enhance user participation based on the results from this study. More controlled studies should be conducted to build further knowledge on the effect on professionals from implementing initiatives to enhance user participation in health care.”

15. This is not a randomized controlled study. Therefore, I recommend the authors to use the term comparison group instead of control group throughout the article. I also believe that the word ‘effect’ used in the title and in general in the article is too strong, and suggest using the word ‘impact’.

To our knowledge the term “control group” and “effect” is commonly used both in randomized and non-randomized controlled trials. We have however emphasized that this is a non-randomized study several places throughout the manuscript.

16. Discretionary revision
Could the authors consider a subheading ‘Ethics’ in the methods section, including how study participants were ensured anonymity and the sentence about the approval of the study by the regional ethical committee for health and medical research. Was the study approved by the Privacy Ombudsman at the Norwegian Social Science Data Service?

We have included a subheading called "Study design and ethics". We have added that the study was approved by the Norwegian Data Inspectorate (by way of the Privacy Ombudsman at the Norwegian Social Science Data Service). We have kept the information about ensuring anonymity under the "Participants" section as we think it give a better presentation of the action taking to ensure anonymity.
17. The questionnaire was filled out twice. Why were there mailed four rounds of invitations (page 6).

To investigate any changes in the intervention hospital we mailed questionnaires before the development plan was implemented and repeated this after 16 months. At the control hospitals questionnaires were also mailed two times with 16 months apart, but a couple of months later than at the intervention hospital. We have however deleted the word “four” to avoid confusion on this.

Why was administrative staff included in the study sample?

We have added a paragraph in the Strengths and limitations section to explain why all professionals were invited;

“We chose to invite all employees at the hospitals to investigate any changes in the whole organisations. Including only those working directly with patients might have given different results.”

18. The authors write that there was some lack of consensus in the translation of the CPQ. Could these be reported?

There was not any lack of consensus after discussing the few differences occurring between the different translations. We hope that the sentence describing this is clear.

19. The paragraph describing the response categories for the items in the CPQ (page 7) is hard to follow. Could this information be presented in relation to table 3?

See comment no 2 - Discretionary Revisions - to Reviewer 2.

20. Could the information about the total study sample (page 8) be presented in table 2?

We have included "(total N=438)" in the introduction to the table.

Other editorial requirements:

1. Copyediting: Please note that BioMed Central journals are not copyedited prior to publication. We advise you to pay close attention to language during revision of this manuscript. If necessary, please seek the assistance of a fluent English speaking colleague, or have a professional editing service correct your language.

We have used a language editing service.

2. Tables as additional files: We notice that you have included tables as additional files. If you want the tables to be visible within the final published manuscript please include them in the manuscript in a tables section following the references. Alternatively, please cite the files as Additional file 1 etc., and include an additional files section in the manuscript.
We have added the tables to the main manuscript at resubmission.

We thank you again for the opportunity to resubmit our manuscript. We look forward to hearing from you in due course.

Yours sincerely

Marit By Rise /s/ Dr. Hilde Grimstad /s/

Dr. Marit Solbjor /s/ Dr. Aslak Steinsbekk /s/