Author's response to reviews

Title: Factors associated with the impact of quality improvement collaboratives in mental healthcare: an explorative study

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Version: 4 Date: 29 April 2011

Author's response to reviews: see over
Dear Prof Froy,

First we would like to thank you for providing us the opportunity to submit a revised version of our manuscript. We submitted our revised paper with the specific changes marked using the ‘track changes’ function in Microsoft Word. In this letter we explain how we integrated the helpful suggestions and comments of the reviewers in the revised manuscript. We will repeat their comments and suggestions for improvement and subsequently describe how we dealt with their remarks (bold text).

Reviewer #1 (Lisa Rubenstein):
The study investigated a large naturalistic implementation of quality improvement collaboratives. It also addresses an important area—mental health—and included both mental health specialist and primary care participants. In addition, unlike many papers on the subject of QICs, the authors defined the key elements of the QIC and determined adherence by QI teams to key elements of their model. The authors also carefully investigated a large number of factors often thought to influence the success of QICs.
The concerns about the paper are as follows:

1) Methods for dependent variables.

a. Without a believable outcome variable or variables, it is much harder to be interested in the factors predicting that outcome. As a naturalistic study, it would be unusual if the information were complete/completely specified; I would just want to know more about how complete (or incomplete) it was and the degree to which outcome data quality varied by team.

We agree with the reviewer that it is important to know how (in)complete the outcome measurements were. In the ‘methods (outcome measures)’ section we added more detailed information on the length of the registration period; the intended registration period for all teams was 12 months. A minimum of 2 months (the registration of 2 measurements) of data registration was chosen to be able to calculate outcomes that needed at least 2 measurements. The actual registration period and variation between teams was added to ‘results (impact of the QI team on the improvement)’ section. Only a minority of the teams registered data during the complete period. The actual measurement period of the teams varied, in anxiety disorders from 7 – 12 months, in schizophrenia 6 teams registered patient data for more than 8 months, and for dual diagnosis the registration varied from 2 to 8 months.

We agree with the reviewer that we cannot assess with certainty whether the number of patients in the practice was the appropriate denominator (except for screening/assessment in anxiety disorders). All the participating QI teams were part of a bigger organization; patients were included by QI team members if they fitted the criteria for inclusion. This point is now discussed in the strengths and weaknesses of the study (discussion section).

b. Being a team that collected data better could in itself be a part of a useful outcome, however, it wouldn’t translate necessarily into better clinical outcomes.

We added the following paragraph to the discussion section (strengths and weaknesses of the study). Generally, as the study proceeded the number of registered data available per patient decreased. We didn’t find a relation between the success of the team and the completeness of the data collection. The number of participating QI teams was small and only QI teams with a minimum of two months of data registration were included in the analysis. The handling of the missing data resulted in a conservative assessment of the successfulness of each QI team, which may have led to underestimation of their true impact. Also the outcome rates may have been affected by the range in the number of patients reached and the different number of registered measurements per patient.

c. Table 1 is not initially clear due to the wording whether the table was also reporting results.
We changed the title of the table 1, as it was not clear whether this table also included the reporting of results. This is not the case; the table only describes the indicators and the targeted standard to be reached according to the expert team.

2) Methods for Independent Variables:
   a. Methods related to the data on the teams are not clearly presented. We changed the title of the article and adjusted the wording into ‘mail survey’ and ‘questionnaire’.
   b. In the absence of information on the prior validation of the scales used, it would be helpful to include some information on scale performance such as Cronbach’s alpha in the paper.

   For the validated questionnaires a Cronbach’s alpha was calculated. A table with this information was added to the ‘method (study design and setting)’ section (table 1). The Cronbach’s alpha were also added to the ‘result (impact of the QI team on the improvement)’ section and discussed in the ‘discussion’ section.

3) Data Analysis:
   a. Here are a large number of hypotheses tested for the number of subjects (teams). Table 5 provides a lot of information but perhaps should be an appendix; a table that honed in on the key hypotheses and findings would be more helpful. To get to this, a clearer view of the conceptual framework or logic model for the analyses might help; it feels as if the authors felt compelled to put every possible variable into their model, rather than either pre-specifying a model or undertaking some sort of variable reduction process to winnow down what they found.

   We agree that the use of a conceptual framework, with the key elements of a QIC as a model of analysis could give a clear view of the possible associations between the key elements of the model and the outcomes. Nevertheless, we decided to use both detailed and a summarized model for analysis. We think using only a model with the 4 key elements as a model for analysis has limitations as the specific factors within the area of the composition; the participation, the functioning and the organizational context of the QI teams cannot represent the total domain.

   We feel the description of the results of the study become too superficial and do not justify our findings if we limit our description to this overall level of the model. For example to know which of the 9 factors related to the composition of the team determine the success of the QI team it is also necessary to look at these factors in detail. Table 6 summarizes the relationship between the 4 key elements of our model and the success of QI teams, whereas table 7 summarizes the relationship between 22 factors and the success of QI teams.

Reviewer #2 (Loes Schouten):
This is a good paper that adds to the growing literature about Quality Improvement Collaboratives (QIC) and QI methodologies. I recommend publication of this paper. I have some minor essential revisions/remarks:

1) The reviewer made a number of remarks on inconsistencies in word use in abstract, main text, and about unclear tables. **We have carefully checked the text and made the following adaptations.** We use now consistently the terms QI teams, QI team leader, inspirational QI team leader. We have now distinguished between organizational support en support of the QI team leader, and we explained what we mean by QICs in this study. The title of ‘Other measures’ was replaced by; factors possibly associated with the impact of the QI teams on the improvement and by ‘factors related to respectively the composition, the participation, the functioning and the organizational context of the QI team’. The table on the hypotheses (table 3) was adjusted. The table about characteristics of the participating QI teams is extended with data about response rates of the different QICs (table 4).

   a. References about the team climate for example should not be placed at the end of the paragraph but at the end of the text referring to references about team climate. **References were correctly replaced.**

2) Data analysis

   The authors state that ‘at the start of the study none of the QI teams worked in a structured way on the implementation of multidisciplinary practice guidelines. Consequently the baseline score of the QI teams was assumed zero.’ Although this might be true for scores/indicators regarding monitoring and screening and assessment (using specific instruments), this may not be the case for patient outcome measures. **In the ‘data analysis’ section we explained that the outcomes were a relative improvement compared to baseline, because a baseline measurement is lacking.** Although for patient outcomes the baseline score on CGI-S, MANSA and HONOS is known, the indicator is about the percentage of patients that showed an improvement on these clinical measurements. As guidelines are not implemented yet, also for patient outcomes it is justified to assume that the baseline performance of QI teams is 0%. We have no indication that one QI team performed better at baseline compared to other QI teams.

4) Discussion

   a. What do the authors mean with ‘professionals that are able to control the improvement’? This requires a clearer description/interpretation and some references or examples supporting this interpretation. **This part of the ‘discussion’ section was clarified.**
b. In the discussion section some general reflections about the operationalization of the items/concepts. Even the theory-based determinants did not consistently relate to success. This might be caused by range of factors (e.g., sub optimal operationalization, complexity of relations (not linear, etc.))

Having only one observation on the determinants of success is a limitation of the study and should be mentioned. **This point has been added to the ‘strength and weaknesses section.**

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Every care has been taken to strengthen the manuscript with the help of the reviewers’ advice. We look forward to your kind consideration of the manuscript.

Yours sincerely, on behalf of the other authors,

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