Reviewer's report

Title: Implementation Outcomes of Evidence Based Quality Improvement for Depression in VA Community Based Outpatient Clinics

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Reviewer: Gerdien Franx

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Fortney et al. Evidence-based Improvement for Depression in VA Community based Outpatient Clinics

No major compulsory decisions required, some minor essential revisions suggested:

1. The research question posed by the authors is important and relevant, but not easily identifiable. In the introduction the authors state that the objective was to test the feasibility of EBQI as an implementation strategy. It would be helpful if the research question is repeated at the end of the introduction. Also, a brief description of the characteristics of CBOC and its patient population is helpful for the international audience and for comparisons between countries.

2. The uncontrolled data are sound.

3. The interpretations of some of the findings seems overly positive. The authors interpret an adoption rate of 69% as excellent, which is understandable. However the definition of adoption was not very strict (having referred at least one patient to the telemedicine CCM program within a time frame of 12 months). In this definition, a doctor having referred a patient once and never again, is considered to have adopted the program which might not be the case at all. The reach into the patient population of the program was 9%, with a wide range of 1.1-49.1%. The authors qualify this as ‘relatively’ low. The question is to what number this figure is related and what expectation did the researchers have at the start of their work? It seems to me that the reach of the program is the most important outcome of the implementation, and that a 9% reach should make the researchers worry a little bit about how to augment this. The high fidelity scores to the program were indeed impressive, although baseline data are not presented and the influence of the NetDSS was very high according to the authors. Was it mostly this tool that produced this outcome or the whole EBQI method?

4. Discussion and conclusion. The authors conclude that EBQI is a strong strategy for implementing the telemedicine intervention into the CBOCs, considering the fact that several barriers mentioned by Rogers were overcome. Some of the barriers cited by the authors seem a bit ‘far fetched’. For instance the barrier observability and the statement that depression severity can not be observed (barrier) and therefore should be measured (EBQI). Apart from the fact that this statement could be debated (a severely depressed patient does not look
the same as a mildly depressed patient), this selective use of the barriers mentioned by Rogers, to stress the strength of the EBQI method does not seem useful.

5. The level of institutionalization was moderate. Can the authors explain how this outcome was generated since this is a very hard thing to accomplish in most implementation projects.

6. Although qualitative data were not collected, considering the goal of the study (assessing the feasibility of the EBQI method), the discussion could be strengthened with a more balanced discussion about the experiences of the participants or the pro- and cons of the EBQI method in the CBOC context. The implementation strategy used here was costly, it demanded a buy-in from clinical leaders and from a multidisciplinary team, the latter was not possible due to regulations. It needed supportive tools and training manuals and demanded specific skill from the researcher, such as a delicate balance between top-down and bottom-up elements, which is a difficult task. The discussion could elaborate a bit more on these issues and the generalisability of this strategy to other settings outside the VA.

7. The overall conclusion at the end of the document that the EBQI method can be used successfully does need some refinement (see argumentation under 3). The EBQI method, as described in the text, consisted of different elements, such as the EBQI team, leadership buy-in, PDSA, local tailoring of the ccm model. Also, the training and decision support tools seemed to have had an important impact on some of the reported outcomes. What exactly are the ingredients meant here that can be used successfully? New information about the NREPP is presented at the end of the conclusion, which seems a bit out of place.

8. The methods are appropriate, adequate (RE-AIM) and sufficient details are provided to allow others to evaluate and/or replicate the work? Statistical analyses have been carried out but do not need to be assessed specifically by an additional reviewer with statistical expertise. Some more specific questions:
   • Can the authors explain why the program was initially launched in the CBOC considered to have the highest chances for success, and only later in the other CBOCs and how these clinics were selected (buy-in of leadership)?
   • Are the Level of Use interview and the Level of Institutionalization survey based on validated questionnaires or developed for this study?
   • Why are the authors talking about ‘trials’ when they describe the fidelity measures. Were the patients in this study enrolling in a trial or was this a naturalistic context?
   • How was remission defined?

9. Strengths and weaknesses of the methods.

The authors make understandable comments on limitations but not on strengths. The VA infrastructure for reliable measurement is a clear strength of the study, as well as the elaborate execution of the EBQI method. The lack of qualitative information on the adoption of the intervention and the reasons for poor reach might be considered an additional limitation.
Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests