Reviewer's report

Title: A Randomized Trial of Practice Facilitation to Improve the Delivery of Chronic Illness Care in Primary Care: Initial and Sustained Effects

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Reviewer: William Hogg

Reviewer's report:

This is an excellent study that adds to the evidence that practice facilitation is an effective means to improve the delivery of evidence based care in community based primary care practices. The study extends what is known about facilitation by showing the approach to be effective as an intervention to improve the delivery of chronic illness care and that the improvements are sustained. The sustainability of the approach is important in that it distinguishes it from alternative approaches such as learning collaboratives and it dramatically improves the cost effectiveness of the approach.

Discretionary Revisions:

1. The paper is well written. The research question is important. The intervention is well described and incorporates approaches to facilitation that reflect what has been learned from previously published trials on facilitation. The toolbox for the practice facilitators was very sophisticated and precisely targeted to the problem. The facilitators only visited the practices just over 6 times on average. This is less than many comparable trials. Is this what was intended? It is not also clear how many practices were assisted per facilitator.

2. The research question is clear and the methods are appropriate. Randomization succeeded in that the main outcome measure was the same at baseline in the two groups. The manuscript adheres to the relevant standards for reporting of randomized controlled trials. The most important literature in the field is referenced. It is not clear if the facilitators and clinicians in the practices knew the main outcome measure came from the survey.

3. The section on the strengths of the study could be expanded. The low dropout rate and high completion rate for the surveys are indicative of how well the study was implemented. It is important that this study occurred in the real world and succeeded despite some practices undergoing computerization and moving to different locations. That 16 of 19 practices showed improvement in one of the subscales of the outcome measure demonstrates the impact of the approach.

4. The section on weakness could also be expanded. This section could address the problem that practices that voluntarily participate in quality improvement
Research studies are higher performing at baseline than those who do not participate. While this may make it more difficult to improve performance and increase the strength of the conclusion of the study it also means that the approach is failing to reach the practices that need it the most. Some jurisdictions in Canada are now mandating practice participation in quality improvement initiatives. It is a strength of the design from a research methods perspective that only one chronic disease was targeted but a limitation in terms of its applicability to practice. Could it be that other aspects of service delivery decreased while the care of diabetics improved? The main outcome variable is self-reported by the FPs and clinical staff in the practices. Some clinical processes data was collected at the beginning of the trial and feedback to the practices but I gather this data was not collected at the end of the study. It would have strengthened the trial if as a secondary measure clinical outcomes were included. This would have however added to the cost of an already expensive trial. Similarly it would have strengthened the study if the practices had been randomly recruited from all practices in a particular region but getting a high recruitment rate also incurs great expense.

5. The most important comment I have relates to the discussion about “Why should PF result in such sustained improvement?” The speculation of how the intervention might work is interesting but there is rather more of it than is usual for a scientific report about the effectiveness of an intervention. The methods describe a patient survey but I do not understand which finding come from it or what the response rate was. At times the speculation is grounded in communications from a person I guess to be a facilitator. There is a mention for the first time in the discussion of a focus group. Were their call reports or exit interviews with the facilitators and clinicians who participated in the trial that support the points made in the discussion? If so the manuscript could be framed as a mixed methods study. It is important that researchers who are so well informed about practice facilitation share their opinions about how and why the intervention works but the extent to which this happens in this paper is exaggerated and might be better in a separate commentary.

6. The section on the clinical importance of the findings could be expanded and include the thinking at the time when the sample size was calculated.

7. There should be a discussion of cost and the impact of their sustainability finding on cost effectiveness.

8. The authors state that practice change can be especially challenging “in small, autonomous primary care practices….“ In our experience larger practices are more challenging.

9. The conclusion might address next steps.

Minor issues:
• Add “in” to last sentence on page 4
• bottom of page 9 “clinicians and nurses….“ I think of nurses as clinicians. Perhaps another term can be used than clinicians.
• Remove the 9 on top of page 13
This is a well written and an extremely important and interesting study. I recommend the manuscript be accepted after minor revisions which the authors can choose to ignore.

**Level of interest:** An article of outstanding merit and interest 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.