Author’s response to reviews

Title: A multidisciplinary self-management intervention in patients with multimorbidity and the impact of socioeconomic factors on the results

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A multidisciplinary self-management intervention among patients with multimorbidity and the impact of socioeconomic factors on the results

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Dear editor and reviewers,

Thank you all for your comments that have helped us strengthen this paper. We have read the comments and short answers are presented below. The article has been modified accordingly. We hope the responses meet your expectations.

In addition, the paper has been professionally edited.
Reviewer #1:

1. The authors state that the results are largely similar to the main results already published on this RCT. This is not surprising because 281/332 patients from the full study have been included in this secondary analysis. This means that this paper is quite similar to the original in content.

Indeed, a large number of the patients are similar but the purpose of our study was to analyze a subgroup of patients with higher needs and look at the effect of the socioeconomic status, two objectives not covered in the original study. In addition, this secondary analysis will allow the inclusion of this study in systematic reviews on interventions for multimorbidity.

2. It would strengthen the paper to comment a little more in the Discussion about the limitations of using self-reported outcomes in a trial that is (by necessity) unblinded. To what extent does a change in these self-reported measures indicate a change in quality of life, cost of care, morbidity or mortality? In other words, on what basis is a change in these measures considered important (other than the fact that the original authors of the measure said it was)? Also, self-reported measures are subject to reporting bias when patients are aware of their intervention status. It may be that those who have undergone the intervention feel obligated to report better outcomes because of the effort that has gone into their care, even if no real change has occurred. In the absence of any objective outcome measures, this is an important potential flaw that should be discussed.

Our analysis focused only on the primary outcome of the original study. A significant change in our study was reported as mentioned by the creator of the tool. HeiQ is a validated tool and it was shown to be minimally affected by social desirability bias.

Cost of care, morbidity and mortality were not documented in our study or the original study and the outcome was solely based on a questionnaire on self-management. We could not find any publication reporting on the association between the heiQ results and these outcomes.

See the section in limitations that has been revised extensively (p.13)

3. I note that the outcomes were measured at 3 months. This is a short follow-up time. Is there any intention to follow-up for longer? What is the implications in relation to the importance of the results (or otherwise) given such a short follow up period?
The original randomized trial was limited to 3 months which is indeed a limitation. A before-after design with results at 12 months was included in the original study but was beyond the scope of this secondary analysis. Indeed, self-management can sometimes take longer to improve or the improvement may not be sustainable. We agree that this is a limitation and it is already mentioned in p. 14.

4. The discussion about whether or not SES is important in modifying the effectiveness of the results should be more nuanced. The extent to which an estimate shifts after adjustment for a covariate depends on the strength of the association of that covariate on the outcome, but also the extent to which there is imbalance between the two study groups in relation to that covariate. In the case of a RCT, the imbalance should, by definition, be fairly minimal. The fact there is some shift in the ORs, does not change this general argument that the extent of shift of the estimate is strongly related to how well balanced the study groups are (so this is not a good way to estimate the importance or otherwise of SES). To assess the impact of SES, it might be more logical to assess the impact of the intervention stratified by SEP in my opinion (to look for evidence of effect modification which is, I think, what you are arguing?).

We agree and thank the reviewers for this suggestion but after discussing this with our statistician, we concluded that our trial did not have the power for stratified analyses.

5. I suggest some brief information on the nature of the intervention within the abstract would be helpful to readers.

Information on the nature of the intervention has been added in the abstract on p.3.

6. Note reference 16 is no longer in press.

The reference has been updated.
Reviewer #2:

First, have any of the results presented here been part of a paper published before? If so, it should be mentioned.

As mentioned, the results of the initial study were published in the CMAJ Open, but results from the present study were not published before.

Second, there is little discussion about the question which implications these results have for further research and clinical practice. This should be amended.

This is now discussed further in p.13.

Third, do the author have any idea, why these dimensions and not any other dimension have improved and why some and not all dimensions related to SES? This also needs to be discussed.

No we do not have an explanation for this. Trying to explain would be highly speculative given the magnitude of the results. Further research is required.