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 in patients with multimorbidity and the impact of socioeconomic factors on the results
Eric Contant; Christine Loignon; Tarek Bouhali; Jose Almirall; Martin Fortin

Dear editor and reviewers,

Thank you for taking the time to review the article. We have read the comments and answered it below. We hope the comments will satisfy you.
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 part of the patients are similar but the purpose of our study was to analyse a subgroup of patients with higher needs and look at the effect of the socioeconomic status on the results.

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.

A significant change in our study was reported as mentionned by the creator of the tool. HeiQ is 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 and the outcome was solely based on a questionnaire of self-management.

"A "reliable improvement" for each domain of the heiQ is calculation developed by the creator of the tool. It is a soft outcome and we did not bind our results to other hard outcomes such as morbidity or mortality. Social desirability is a potential bias for every self-report questionnaire, the heiQ has been shown to have low social desirability bias[32] Bias was also reduced by having a designated research assistant conducting standardized interview.

See p.14
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 goal of this secondary data analysis was intended to have a look at the short-term improvement of patients with multimorbidity and was also part of Master's degree project, explaining the short term of the study.

Indeed, self-management can sometimes take longer for patients to improve and we will have to have a look at the 12 months mark that is undergoing.

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 thanks the reviewers for this suggestion but after discussion with our statistician, we concluded that our trial was not powered 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 on the abstract. p.3

6. Note reference 16 is no longer in press.

The reference has been updated. p.
Reviewer #2:

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

The results of the initial study have been published in the CMAJ Open. But results from the present study has not been published beforehand.

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

"It would have been interesting to receive the feedback of the patients on how the SES affected them during the study."

See comment p.15

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 don’t have an explanation. Trying to explain would be highly speculative given the magnitude of the results. Further research is required.

"Each domain of the heiQ is independent and seemed to have respond differently to the SES and it would be highly speculative to try to explain it."

See comment p.15