Reviewer’s report

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

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Reviewer: Di Sarfati

Reviewer's report:

This paper describes the results of a RCT relating to a self-management support intervention for patients with multimorbidity in primary care.

The care of patients with multimorbidity in clinical practice is an increasingly important issue, and the authors are to be congratulated on evaluating a possible intervention with the aim of improving outcomes for these patients.

I do have some points to raise, which I think should be addressed:

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.

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.

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?
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?).

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

6. Note reference 16 is no longer in press.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

No

**Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?**
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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