Reviewer’s report

Title: Impact of a Chronic Disease Self-Management Program on Health Care Utilization in Rural Communities: A retrospective cohort study using linked administrative data

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Reviewer: Randall Fransoo

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Jaglal et al manuscript
Review by Randall Fransoo

This study takes advantage of an interesting opportunity: the fact that this intervention group already existed and could be contacted for consent to follow-up in admin data. Overall, the manuscript is well written, with a few exceptions noted. The statistical power is limited by the small sample size and the nature of the outcome measures used.

While I do not conduct research in this area, I was not surprised that the results showed no decrease in service use as a result of participation in the program. Indeed, I kind of expected that there might actually have been an increase, at least in the short term, in the kinds of outcome measures used in this study (brought on by the expanding interests and concerns of highly-engaged patients). I suspect and hope that other longer-term outcomes may well be reduced in frequency and/or severity (eg need for advanced care or surgeries, etc).

The data appear sound and relevant, though require more presentation and analysis using non-parametric tools and measures.

1) Major compulsory revisions

a. Participants – the manuscript states that 213 people participated in the original study, and that 164 were contacted again for this admin data study. What about the other 49 people? Was no attempt made to contact them? Are they different from the 19 listed as “could not be reached”? It would be good to know more about this group, as this may affect the interpretation of the results and limitations. (I believe they have been included in the ‘non-participant’ group, though again, they may be different from the 19).

i. BTW: the wording regarding this in the Recruitment section is a bit odd: it uses the phrase “164 participants contacted by the interviewer” but then notes that 19 could not be contacted…

b. Control group – the analyses in this manuscript compare the 104 original study participants who agreed to this follow-up admin data study to those who did not agree, or who could not be reached (plus the 49 not described). However, no mention is made of a true control group in the original study. This is important,
because without monitoring the same outcomes in a true control group that did not get the CDSMP intervention, we cannot know whether any of the changes observed could be attributable to the program. That is, people receiving usual care may also have improved their self-efficacy, behaviours, and health status over time (especially if their diagnosis was relatively recent). I understand that this manuscript cannot re-iterate everything in the original paper cited – but the existence of a control group is critical, and should be mentioned in this paper, because it affects interpretation of the current results.

c. Time period: from the descriptions provided, it seems to me that the researchers may well have available to them admin data for the full 18 months after program completion (perhaps even 24 months?) for all subjects. If this is the case, then the GEE modelling should likely be done on this dataset instead of the 12-month data, to include more events and gain more statistical power for this relatively small study with small differences.

2) Minor essential revisions

a. In many places in the manuscript and the results tables, means and standard deviations are reported for variables and/or outcomes that are clearly not normally distributed. I understand that non-parametric tests were used for some of these, which is good – so I am not overly concerned about the validity of the findings. However, it is still inappropriate to present non-normal results using means and SDs. Instead, they should provide the medians, along with inter-quartile ranges (or equivalent). Means and SDs are simply not meaningful with these kinds of values (eg when a mean +/- two SDs takes you into meaningless values).

b. It would be helpful to say a bit more about the age effect: they note that a significant interaction prompted stratification into two groups. This seems reasonable, but which variables/outcomes were used in making that judgment, and why was age 66 chosen?

c. I am also a bit concerned about the low frequency of Specialist visits and ED visits in the GEE models. It seems likely that many participants had zeroes for one or both of these outcomes in the study period. A zero-inflated type of model would address this concern, but may not be feasible in this situation.

i. Alternatively, they could compare the use rates for these measures, including only those participants who had at least one such visit (and comparing the percentage in each group that had 1+ visit). If the results are similar, then this would strengthen the argument; if not, it would prompt additional discussion.

d. Grammatical issues:

i. Page 6 top: “reduced followed” should likely be “reduced following”

ii. Page 6: First sentence of the methods section needs grammatical attention: perhaps just removing the word “an” after tele-CDSMP.

iii. Page 6: fifth sentence of methods: change “the course which used…” to “the course used…”

iv. Bottom of Page 7 – in the section on Variables collected: wording of the
sentence which includes “manage condition” need to be revised.
v. Also: participants’ confidence was described – but I didn’t see those variables mentioned as being part of the analysis, or in any of the tables. This should likely be clarified or removed.
vi. Page 9 mid: the sentence starting with “To examine the impact…” needs to be re-worded, or at least, add a comma after “adjusting for covariates”.
e. Privacy: mention is made of one participant who was removed because of outlier status. This was a necessary and good decision, but it struck me as a privacy issue that this person’s actual ED visit count was disclosed. I believe the same exclusion could be justified without mention of the actual number, which may be a privacy violation (certainly would be in Manitoba).

3) Discretionary revisions
a. The introduction states that the study has 2 main objectives, but they are so inter-twined that I don’t see them as separate. Both are before-after comparisons. Some extra analyses were done on the 12-month data alone, but I’m not sure there’s really two objectives here.
b. It would also be good to know if they have data on time since diagnosis – as those recently diagnosed would be expected to have more changes in a number of the variables studied than those who’ve already been living with their conditions for many years.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

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.