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

Version: 1 Date: 18 January 2014

Reviewer: Susan L Norris

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General comments

This manuscript addresses an important topic about which additional high quality research is needed. The authors have made use of administrative data to address their research question and their methods are generally valid. However, the intervention is so brief that the nonsignificant results are hardly surprising.

Major comments

1. The nonsignificant results are not surprising, given that the intervention was a total of only 12 hours. With such a brief intervention and no following, existing studies suggest that one would not expect major changes in behavior, health, or healthcare utilization.

2. The results presented in this paper are part of a study which also measured self-efficacy, health behaviors, and health status, however there is some lack of clarity between the parts of the study. The methods section on page 7 lists the outcomes related to self-efficacy, etc, but only presents those results in brief and there is no information on how they were analyzed. I suggest either leaving those results out of the methods and results sections and discuss them in the introduction (with the citation to the related manuscript), or supply more detail and add them to the objectives of this paper. Also, it might be helpful to refer to the main or initial study (both terms are used later in the manuscript) in the introduction, before introducing these terms later in the manuscript.

3. How were the 164 patients selected to be contacted from the cohort of 213? Was this random or were their criteria?

4. The 12-hour self-management intervention in this study is very different from the chronic disease management program described in the Hamar study on page 13, and I think the sentence at the top of page 14 alluding to the differences is understated. The Hamar study involved much more than just self-management. The two studies really can’t be compared.

5. The finding of differences between persons less than and greater than 66 years is overstated in the results and in the conclusions. This was a post-hoc analysis and should be labeled as such, noting the limitations.

6. The analysis does not appear to take into account clustering of participants at the 13 communities. This should either be explained or incorporated into the
Minor comments

1. Page 5: “There was a small but significant change for days hospitalized…” I suggest rather “There was a small but statistically significant decrease in days of hospitalization.” (assuming that the number of days was fewer).

2. Data in table 2 are repeated in the text page 11: this is unnecessary.

3. How are “nonrespondants” defined in Table 1 and on page 11? They seem to include both patients who were not contacted as well as patients who were contacted but did not supply HCNs.

4. Page 9: there appears to be an errant citation superscript “2”.

5. Page 12: “models were stratified by age based on the median (<+66…)”. Why was this the median? Are we not dealing with individual (versus population) data? Why nod stratify by individuals less than and greater than 66 year?

**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.