Reviewer's report

Title: Selecting long-term care facilities with high use of acute hospitalizations: Issues and options

Version: 1 Date: 29 March 2014

Reviewer: Rebecca Hubbard

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Major Compulsory Revisions

1. The manuscript makes no connections nor references to the substantial existing literature on provider profiling which contains many similar analyses of the comparative role of, e.g., fixed versus random effects and adjusted versus unadjusted approaches to identifying poorly performing providers. Some appropriate references should be added to connect and contextualize this manuscript with respect to the broader literature on provider profiling.

2. The first sentence of the section “Methods of ranking” states that “only four combinations of the dimensions shown in Table 1” were included in the analysis. I think this is reasonable, but the contribution of this manuscript toward building a framework for choosing a method of analysis would be greatly strengthened if each of the four methods were clearly linked back to the considerations presented in Table 1. In general, the manuscript would be improved by a more systematic presentation of the considerations related to the question of interest and the corresponding method that should be used.

3. The description of how the model estimates from the GLMM are used to provide facility-level estimates for Method 3 is not described clearly. Stating that, “model estimates effectively became a predictive risk score” does not explain whether facility-level random effects were included in the predictions. (I believe they were not but this is unclear.) It also does not explain what values were used for patient-level predictors when obtaining fitted values. (I believe facility means were used, see Minor Essential Revision #3 below, but this is not stated.) Without a clearer description this method is basically not replicable.

4. Related to #3 above, the description of the model building procedure for the GLMM is not clear. The authors state that covariates were eliminated from the model “if the effect size was closer to zero and the p-value closer to 1.” Closer than what? At what point was this process stopped?

Minor Essential Revisions

1. In the abstract, Method 1 is described as “simple event counts,” when it is really an event count per resident. This should be corrected.

2. The fourth paragraph of the Introduction presents information on acute hospitalizations among long-term care residents in New Zealand. However,
paragraph two of the Introduction says that there are no reliable reports of acute hospitalizations among long-term care residents in New Zealand. This apparent contradiction should be resolved.

3. The last sentence of the section “Classification of potentially avoidable hospitalizations” refers to “facility-level analyses.” However, as currently written it is not clear what this is referring to. I believe the authors mean that these were used to obtain predicted values from the GLMM for Method 3. However, as mentioned above under Major Compulsory Revisions #3, this is not clearly described.

4. The last sentence of the “statistical methods” section describes Kendall’s tau but this statistic is not reported in the Results.

5. In, the second sentence of the Results, it should be clarified that 34 PAH events per 100 person-years of follow-up was the overall event rate across the entire sample (assuming this is a correct interpretation of this statistic).

Discretionary Revisions

1. On pg. 7, the authors state that the list of conditions on discharge classified as PAH are available from the authors. Perhaps these could be provided as an appendix.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**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