Reviewer's report

Title: Dual use of VA and non-VA hospitals by veterans with multiple hospitalizations

Version: 2
Date: 13 October 2014

Reviewer: Ann Borzecki

Reviewer's report:

1. Is the question posed by the authors new and well defined?

The question posed by the authors is reasonably well-defined – their objective was to determine whether patients with multiple admissions, particularly those who with both VA and non-VA hospitalizations had unique characteristics that would help in resource allocation and better coordinated care efforts. I presume this is a novel question although they don’t explicitly state this.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?

I am not sure how reliable/complete their method of obtaining info on non-VA hospitalizations is. Perhaps they could provide a bit more info on this? Otherwise I think the methods are well described.

3. Are the data sound and well controlled?

Please see my comments about the methods above. I wonder about the comparisons of comorbid diagnoses and how much this is a function of differences in coding or number of secondary diagnoses that are coded between the VA and non-VA hospitals.

There seem to be a couple of analytic methods that show up in the results for the first time (e.g., Tukey's test). These should be under methods. The tables don’t include statistical results, e.g., significant differences. I presume most of the differences are significant as indicated in the text and the numbers are quite large but perhaps something could be included in the table legend(s)?

4. Are the discussion and conclusions well balanced and adequately supported by the data?

It’s not clear where the discussion starts since results and discussion is a single heading. Is this part of this journal's standard formatting? It makes it a bit harder to follow.

I think the discussion and conclusion are supported within the limitations of their data as they note. However, I think they should discuss any implications/impact on their findings associated with the fact the data is 7 years old. Presumably there may have been non-VA changes in coding practices – we know that some
comorbidities that go into the CMS readmission measures are relatively undercoded in the VA, e.g., malnutrition. It’s possible the number of data fields for diagnostic codes in the non-VA setting may also have changed, e.g., increased over this time, i.e., increased.

5. Do the title and abstract accurately convey what has been found?

The title adequately describes the study’s topic. The abstract seems to accurately convey their findings.

6. Is the writing acceptable?

The paper is well-written. It is somewhat difficult to follow the results at times due to the multiple comparisons but the summary paragraphs help with that.

- Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

none

- Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

I would like them to address my comments above regarding data issues under sections 2, 3 and 4, i.e., to: provide additional information on the reliability/completeness of their method of obtaining information on non-VA hospitalizations. In the discussion they should discuss the implications of the age of the data and possible differences between non-VA and VA claims data with respect to the number of codes available, and how this may affect the validity of their findings.

- Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)

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

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