Reviewer's report

Title: Multimorbidity Patterns and Health Service Use in Swedish 85 year olds: An Exploratory Study

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Reviewer: Dave Kerby

Reviewer's report:

This is a paper on multimorbidity among older adults at age 85, and the impact of multimorbidity on ER visits and hospitalization.

A big challenge to publishing this paper, and to reviewing it, is that the language is an impediment to understanding. This version is not suitable for publication unless extensively edited. For example, on page 4 in the third paragraph is this sentence: “Difference according to gender needs to be concerned.” The meaning is not clear. On page 5 on the second line: “The letter invited the individuals born two months after their 85th birthday.” Clearly, no one is born two months after they turn 85 years old.

Major Compulsory Revisions:

1. The authors are not clear on how the presence of a chronic disease was measured. On page 5, the authors note that chronic disease was assessed in two ways: by self-report, and by medical records. But there is no mention of how these two sources were combined. The paper should state how chronic disease was defined when the two sources did not agree. For example, what if an older adult reports suffering from arthritis, but this diagnosis is not in the medical record? As another example, what if the medical record reports a diagnosis of dementia, but the older adult reports no memory of such a diagnosis? Also, the paper should state the nature of the self-report – whether it was a checklist, or a free response question.

2. On page 6 at the end of the first paragraph, the authors say, “we chose a prevalence >5% as the criteria for a common morbidity.” This statement describes how chronic disease was measured; as such it should not be in the data analysis section of the paper, but in the section describing how chronic disease was measured (see point 1 above).

3. The authors are awkward in their description of the distance measure used. On page 6 at the top of the page: “A hierarchical cluster dendrogram was formed using Yule’s Q measurement.” In the context of cluster analysis, the usual term used is not “measurement” but “distance measure” or “similarity measure.” A better wording would be something like the following: “. . . was formed using Yule’s Q as the similarity measure between clusters, with a higher value indicating greater similarity.”
4. The authors are imprecise in their description of the method of agglomeration. On page 6 on the third line, the method of agglomeration is referred to as “average linkage.” In fact, there are two types of average linkage: a) within groups, and b) between groups. Figure 2 states that the method used was average linkage between groups, so this should be made clear in the text. A simple statement of fact should do it, such as “the agglomeration method was average linkage between groups.”

5. On page 6, line 6: “A cluster feature can often be overrepresented when one or a [sic] just a few cases are investigated.” I am not sure what the authors are trying to say here. I am puzzled about the meaning of the word “cases”, because the cluster analysis reported in the paper is not of cases but of variables. I also am not clear on the meaning of “cluster feature.” The authors should drop this sentence or reword it in some way that clarifies the intended meaning.

6. On page 7, in the second full paragraph: “A five-cluster structure was derived by a cut-off distance of 15 units. . .” The cutoff value based on visual inspection seems reasonable to me. However, there is a lack of clarity in the reporting, because the method section reported that Yule’s Q was the similarity measure. What the authors report here in the results section is the SPSS rescaled value, which has no intrinsic meaning. I strongly recommend that the authors report the Yule’s Q value that goes with the rescaled value of 15. This Yule’s Q value can be obtained by an inspection of what SPSS calls the agglomeration schedule. For example, consider Figure 2. Cluster 3 joins with Cluster 4 at a rescaled value of 15; the SPSS agglomeration schedule will state the Yule’s Q value at the point when these two clusters joined; this value will be in the column labeled “coefficient”, because Yule’s Q is the coefficient of similarity. I would also recommend that the odds ratio associated with this Yule’s Q value be reported. In this way, the cutoff value is stated in terms that will be understood by most readers – namely, the odds ratio.

Minor Essential Revisions:

1. The authors switch between the term “morbidity” and “chronic diseases”. The paper would be much easier to read if just one term was used, and my personal preference is “chronic disease.”

2. Table 1 reports inferential statistics, comparing men and women, but all that is reported are the p values. I commend the authors for reporting exact p values for the non-significant results and not merely putting NS. In addition, I think it would be far more informative to have column three report the appropriate statistic (the t statistic, the chi-square, or the Mann-Whitney U) along with the p value. I would also prefer to see a fourth column with an effect size that goes with the statistics – the Pearson r for the chi-square, the rank-biserial correlation for the Mann-Whitney U test, and either Cohen’s d or the Pearson r for the t test.

3. The paper reports five clusters for men and five clusters for women. I think that a paragraph or two at this point could compare the results with some previous
studies. The clusters that emerged in this study of the very old seem similar to clusters that have appeared in previous research. A discussion of these similarities would be informative.

4. In Figure 1, one chronic disease is “osteoposis”, which needs correcting whether this is retained as a figure or converted to a table.

Discretionary Revisions:

1. Figure 1 does not work well for me, for two reasons. First, Table 1 does a good job of describing gender differences on a number of variables, so a second table (not a figure) to describe gender differences in chronic diseases seems sensible to me. Second, the focus of the study is on the topic of multimorbidity, not gender differences, so I do not see a need to highlight gender differences in a figure. Though figures are nice and often easy on the eyes, I would much prefer to see these data in a table and not in a figure.

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

Quality of written English: Not suitable for publication unless extensively edited

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests.