Reviewer's report

Title: Prevalence of measured and reported multimorbidity in a representative sample of the Swiss population

Version: 3 Date: 1 October 2014

Reviewer: Concepcion Violan-Fors

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Title: Prevalence of measured and reported multimorbidity in a representative sample 1 of the Swiss population

Journal: BMC Public Health

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When assessing the work, please consider the following points:

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

As defined, the question has limited interest because some systematic reviews1 have already answered this question. That said, the most important problem in this article is the measurement of multimorbidity, which has an impact on how the question has been defined. This point is addressed in the comments made about the methods used.

Another basic consideration is that the background section is very short, and the authors must review the justification for the study. (Major Compulsory Revisions) The authors said that: “few studies have assessed the prevalence of MM in the general population.” The authors need to revise this assertion and to consider incorporating the bibliography suggested below:


Note: I suggested to the authors some articles which I’m co-author just in case they consider appropriate to review them.

2. Are the methods appropriate and well described? (Major Compulsory Revisions)

The Cohorte Lausannoise (CoLaus) study.

The study is based on the CoLaus study, but in the previous paper the author does not explain which codification system was used. Neither do they explain the professional profile of the interviewer(s). For these reasons, these aspects must be described in the methodology section.

Multimorbidity

The authors use 3 different kinds of measures to analyse multimorbidity:

“Multimorbidity was defined according to two sets of criteria [7, 14]. The first one [7] was chosen because of the large sample size it was built upon and availability of the criteria in our study. As both self-reported and objectively measured data were available, two definitions were considered: definition A included only self-reported morbidities, while in definition B all available objectively measured morbidities (either diagnosed or assessed by an existing medication) replaced the self-reported ones. Finally, a third definition (named C) was based on one of the most used scores to assess MM, the Functional Comorbidity Index (FCI) [14].”

One of these measures is used to analyse multimorbidity (MM) prevalence for 40 specific diseases [7] whereas the other assesses both MM and morbidity burden [14]. Nevertheless, each kind of measurement has a different indication for its use. Reference [14] was developed using two databases that included different diseases than those of reference [7]. In addition to MM prevalence, the second article [14] includes physical function and defines a Functional Comorbidity Index. Because reference [7] included more morbidities, as the authors show in Supplemental table 1. The authors have to explain, I wonder why not include all diseases as defined at the previous reference 7?

The authors need to explain each codification process more clearly in the methods section. This should include an explanation of the mapping process that was designed to permit comparisons between entries in the two data sources. The authors also need to explain why they used a smaller number of diagnoses than were used in the cited paper [7].

Other aspect I don’t understand is: Why is mental health the only diagnostic group specified as “reported”?

In a multimorbidity study, it is important to specify the process of reaching a diagnosis. The differences between prevalence could be explained only for the kind of measurement used. This is an important limitation because it is the primary endpoint of the study.
In the Supplemental Table 1, I wonder why “repeated diagnosis” and “reported” are listed together (e.g., Anxiety, reported). In column A, all the diagnoses are self-reported. Why do the mental health diagnoses are the only specified as self-reported?

3. Are the data sound?

The results of this article are similar to those obtained in other studies that have used electronic medical records (EMR). The conclusions are not novel, and do not increase the available knowledge on multimorbidity prevalence. However, it is the first study that compares self-reported diagnoses and those recorded according to the Functional Comorbidity Index.

Consideration of the differences between immigrants and the Swiss population should include a comparison by age group and sex between these two cohorts.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

Yes

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

DISCUSSION (Major Compulsory Revisions)

Multimorbidity Prevalence

The authors said: “Even using the same set of criteria, the prevalence varied considerably when self-reported or measured data was collected. A likely explanation is that many subjects are unaware of their status, as it has been shown for cardiovascular risk factors such as hypertension [15] or type 2 diabetes [16].” There are other reasons for these differences that the authors fail to comment on. I suggest a more careful review of the literature concerning this question.

The authors used 2 reference papers [7, 14]; a good analysis should compare the results of their own work with the referenced authors, analysing the differences or similarities obtained in the different studies.

Logically the prevalence of MM increases with the increased number of morbidities included!

The authors also should have added more comments about their results compared to other articles that analyse the differences between two multimorbidity measurements. I suggest some articles:


Discussion final comments

The discussion section should emphasize the bivariate analyses more, comparing the methods used and underlining that the best association is a higher number of reported conditions and the number of FCI diagnoses. Are the clinical history data overestimating the MM? Are the patients perhaps only self-reported diseases that they're really concerned about?

6. Are limitations of the work clearly stated?

The authors should expand this part of the discussion section to address the limitations pointed out above. (Major Compulsory Revisions)

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?

The authors need to improve this aspect. Previous work is properly acknowledged but not fully addressed, as noted above. (Major Compulsory Revisions)

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

The title is OK.

Abstract: Authors must rewrite according to the many changes I've recommended, mainly in the objective and methods. (Major Compulsory Revisions)

9. Is the writing acceptable?

Yes, but it needs proof reading/copy editing (Minor Essential Revision).

Please make your review as constructive and detailed as possible in your comments so that authors have the opportunity to overcome any serious deficiencies that you find and please also divide your comments into the following categories:

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

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

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

Level of interest: An article of limited interest

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'