Reviewer's report

Title: Co- and multimorbidity patterns in primary care based on episodes of care: results from the German CONTENT project

Version: Date: 14 June 2007

Reviewer: Martin Fortin

Reviewer's report:

General
The subject posed by the authors is new but need to be better defined
The methods seem appropriate but need some more description
Data presented are good but some adjustments are required (see below)
The discussion and conclusion will need some improving
The title conveys what has been found. The abstract could be improved.
The writing needs some editing.

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

1. The authors should state the objectives of this study clearly. The reader is confused between the objectives of CONTENT and those of the study that is the object of this report. The objective should be placed at the end of the background section. The abstract will have to be modified accordingly.

2. The background could be improved by a short discussion on the concept of multimorbidity and its measure. Particularly, given that the authors will use throughout this study different ways of measuring multimorbidity, this discussion implies also some modification in the methods part where all the variables that will be used as a "proxy" for multimorbidity are presented and defined.

3. The originality and the need for this study deserve better attention in the background.

4. The methods is not described adequately. The readers want to know about the process in which the data are collected? Who? How? How many doctors in those practices? That's the only way for the reader to have an idea of the generalizability of the results. Again, a strong description of the variables under study is required.

5. The authors must pay attention that all the results presented are connected with the objectives. I question particularly the last paragraph of the result section that arrives completely unannounced.

6. The discussion should be developed further. The results of this study should be compared to existing papers on the subject. the references will have to be expanded accordingly.

7. Also, in the discussion, the contribution of this study should be stated clearly.

8. The limitations and the forces of this study deserve more attention.

9. The last paragraph of the discussion is not relevant to this study.

10. Finally the conclusion should be specific to the results and to the objectives of the study.

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

a) The authors define in the Background section of the text (page 4, last paragraph) the use of the word "episode". A short definition should also be provided in the Abstract.

b) The acronym ICPC should also be defined in the Abstract.

c) Description of the Methods in the Abstract should be all in past tense. The expression “The analyses are based on a total..." should be replaced by “The analyses were based..."
d) The Results section of the Abstract starts with the phrase “A couple of characteristics (number of parallelly active episodes, number of different codes documented in ICPC, number of different prescriptions, number of referrals) could be applied to indicate multimorbidity.” This phrase should be replaced by another more appropriate for the Results. The “couple of characteristics” are indeed the response variables in the text. Why not use the same name? Also, these “couple of characteristics” not only could be applied to indicate multimorbidity but they were used to indicate multimorbidity in the Results. A similar use of the word “characteristics” is found in the Results section of the text (page 6, two last paragraphs).

e) In the Results section of the text (page 6, paragraph 4) it says: “Figure 2 and Table 2 show that the correlation between these characteristics appears to be strong for both male and female patients (p<0.0001).” The p value shown at this place belongs to the multiple liner regression analysis and not to the description of Figure 2 and Table 2.

f) Results shown in Figures 1a to 1d and Figure 2 are all repeated in Tables 1 and 2. The authors could choose to show the standard errors and the n values also in the Figures and remove the Tables or to split data between the two ways of presenting the results, or even remove the Figures and leave only the Tables.

g) In the Discussion, the Cumulative Illness Rating Scale (CIRS) is mentioned as “a more detailed index” of multimorbidity and readers who are not familiar with the subject may be left with the impression that is “the other” (and only) index of multimorbidity. The existence of many other indexes of multimorbidity should be mentioned the Discussion without providing and exhaustive list. Probably just a reference such as the one of de Groot et al (de Groot V, Beckerman H, Lankhorst GJ, Bouter LM. How to measure comorbidity. a critical review of available methods. J Clin Epidemiol. 2003;56:221-9) could help to fill this gap.

h) The finding that despite gender differences in parallelly active episodes and ICPC codes the authors did not find differences in prescriptions should be discussed and not ignored in the Discussion. Indeed, the whole Discussion could be improved with more reference to the findings in this work.

i) All References should have a uniform presentation. References 5, 10 and 12 have the number of the issue, whereas the others do not.

Discretionary Revisions (which the author can choose to ignore)

In the Methods section of the Abstract and in page 6, 1st paragraph, the authors use the expression “interesting response variables”. There are variables that may also be interesting but that are not of interest for the authors or for this particular work. Probably, the expression “interesting response variables” should be replaced by “response variables of interest”.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

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