Author's response to reviews

Title: Age and gender differences in the prevalence and patterns of multimorbidity in the older population

Authors:

JOSE M ABAD (jmabad@aragon.es)
AMAIA CALDERON (acalderon.iacs@aragon.es)
ANTONIO PONCEL (aponcel@salud.aragon.es)
BEATRIZ POBLADOR (bpoblador.iacs@aragon.es)
JOSE M CALDERON (jmcalderon@aragon.es)
ANTONI SICRAS (asicras@bsa.cat)
MERCEDES CLERENCIA (mclerencia@salud.aragon.es)
ALEXANDRA PRADOS (sprados.iacs@aragon.es)

Version: 4 Date: 29 April 2014

Author's response to reviews: see over
Author's response to proposal of changes from the reviewers

Title: Age and gender differences in the prevalence and patterns of multimorbidity in the older population

Authors: JOSÉ MARÍA ABAD-DÍEZ, AMAIA CALDERÓN-LARRAÑAGA, ANTONIO PONCEL-FALCÓ, BEATRIZ POBLADOR-PLOU, JOSÉ MANUEL CALDERÓN-MEZA, ANTONI SICRAS-MAINAR, MERCEDES CLERENCIA-SIERRA, ALEXANDRA PRADOS-TORRES

Version: 3
Date: 29 April 2014
Author's response: see over
Age and gender differences in the prevalence and patterns of multimorbidity in the older population

JOSE M ABAD, AMAIA CALDERON, ANTONIO PONCEL, BEATRIZ POBLADOR, JOSE M CALDERON, ANTONI SICRAS, MERCEDES CLERENCIA and ALEXANDRA PRADOS

BMC Geriatrics

Comments from the reviewers and response from the authors

Thanks for the encouraging and valuable comments and suggestions made by the three reviewers that certainly will improve the quality and understanding of the manuscript.

In the following paragraphs we will address the suggestions and comments made by the reviewers. Our answer will be in italics.

Reviewer 1

Comments from the reviewer

p.4 Methods/Design: Please state the total number of primary care centers in your data set and the number of centers that have been excluded due to exclusion criteria.

Response from the authors

We have included data on the total number of centres included and the number of centres excluded based on the quality of the clinical information.

Comments from the reviewer

p.7 Results: You describe that the prevalence of the CM pattern is similar for males and females, but I find this interpretation rather subjective as 21.2% vs. 17.3% means that 22.5% more males than females suffer from this pattern. Maybe you could test for statistical significance and report its results? I would also suggest that you report the mean number of CM diagnoses you found in males and females. Both information would help to sharpen interpretation regarding these gender differences.

Response from the authors

We have included data on the prevalence of the different patterns and mean number of diagnoses, and the statistical significance of the differences by gender. We have restated the phrase according to the results. In fact we found
differences in the prevalence of the CM pattern, but we wanted to emphasize that the differences were greater in the MEC and PG patterns than in the CM pattern, and that these differences varied with age.

Comments from the reviewer
p.10 Discussion: You mention that gender differences in CM have been reported in other studies. How do you explain that you did not find these differences yourselves?

Response from the authors
As stated in the last paragraph, we have edited the discussion to better represent our findings.

Comments from the reviewer
p.10 Discussion/Strengths and limitations: Exclusion of health centers based on documentation quality could have introduced bias if centers with high patient traffic and/or more patients with worse health conditions and/or more patients from deprived areas also have worse documentation quality, e.g. due to higher workload. This should be discussed.

p.10 Discussion/Strengths and limitations: The study is limited to large cities and does not cover rural areas. This also should be discussed.

Response from the authors
We have included a new paragraph addressing the biases that could have been introduced by the exclusion criteria and by the limitation of the study to large cities.

Reviewer 2
Comments from the reviewer
1. Methods and Results section: The major weakness of the study is the validity of the conditions included in the different multimorbidity categories. Please justify inclusion of the diseases in each pattern. I would have expected conditions such as depression, anxiety and insomnia and other geriatric syndromes such as incontinence and falls to be included in the psycho-geriatric pattern. Congestive heart failure and cardiac arrhythmia appear in both the CM and PG pattern. This introduces collinearity and explains the significant association between the CM and PG patterns in the same patients (the same conditions are counted twice), thus invalidating the results. In the same vein, obesity appears in the CM pattern, but not the Obesity pattern. Although the authors argue in the Discussion section that dyslipidemia does not predict cardiovascular outcomes, dyslipidemia is still widely accepted to be part of the cardiometabolic syndrome. It has a strong association with hypertension and diabetes in cardiac and obese patients. Data from the previous study should be
presented to validate these choices – from a medical perspective the groupings are unclear.

Many patterns of multi morbidity have been described in previous papers that the authors reference. Concordance between grouping among different groups of researchers must be provided to lend validity to the findings.

**Response from the authors**

We have included in the Methods section references to the criteria used in the original paper for the inclusion and exclusion of disease in the multimorbidity patterns and the validity of them. We include also a web appendix describing the methodology of the previous study.

We have also addressed this question in the discussion section more thoroughly.

As to the problem of collinearity, we have decided to exclude the results relating to the association among patterns, as they may be biased and do not add much information to the main results and discussion.

We have broadened the discussion to address more clearly the issue raised by the reviewer with reference to the relationships among obesity, dyslipidemia and CM and MEC patterns. In the previous study, obesity was included in the so-called Mechanical pattern in the younger age groups (15-44 years and 45-64 years) but was not present in the pattern in the mayor older patients. So we have changed the acronym and denomination of the pattern to more clearly represent it.

We have also broadened the discussion to address the concordance among the patterns in our study and in other related bibliographic references.

**Comments from the reviewer**

1. Abstract: Results section, second sentence. Remove the word “with” from the sentence “women presented (with) more than one pattern...”

**Response from the authors**

We have made the suggested correction in the abstract and methods section.

**Comments from the reviewer**

2. Background and Methods sections: Single sentences do not comprise full paragraphs. Please edit to make sure that there are at least 3 sentences per paragraph.

**Response from the authors**

We have edited the paragraphs to make it more comprehensible and to include at least 3 sentences by paragraph.
Comments from the reviewer
3. Background: Some references are out of date. Consider updating some references to include the recent American Geriatrics Society guideline papers on multimorbidity or other recent high impact journal editorials and original research papers.

Response from the authors
We have included updated references in the bibliography in the background section.

Comments from the reviewer
4. Background: Last sentence. Would consider rephrasing the objective of the study to read, “The aim of this study was to describe gender differences in the prevalence and patterns of multimorbidity in elderly men and women”.

Response from the authors
We have rephrased the objective of the study according to the suggestion.

Comments from the reviewer

Response from the authors
We have made the suggested correction in the abstract and methods section.

Comments from the reviewer
6. Methods. The number of prescription medications was mentioned in the inclusion criteria. Were prescriptions accounted for at all in the analyses?

Response from the authors
We did not include data on prescriptions in the study. We used the percentage of prescriptions in uncoded episodes as an indicator of the quality of the information in the clinical records of the patients, in order to decide to include or exclude the centre from the study. We have included a mention in the methods section and discussion (limitations) to this issue.

Comments from the reviewer
1. Would consider changing the title of the paper to reflect the novelty of the findings: “GENDER DIFFERENCES in the prevalence AND PATTERNS of
multimorbidity in the elderly population: results of a study based on electronic medical records." Unless the journal requires specification of the methods in the title, I would also remove "results of a study based on electronic medical records".

**Response from the authors**

We have changed the title of the manuscript as we think that the one suggested by the reviewer may be more adequate to the main findings of the study.

**Reviewer 3**

**Comments from the reviewer**

This study is based on the elderly population segment of the same data that this group reported in a previous paper (PlosOne, 2012). The methodology used in the present paper builds on what was found in the 2012 paper and a main difference is that it now focuses solely on the 65+ population, which is in the current report subdivided into three age segments (ranges). While the already published study documented clusters of multimorbidity by means of factor analyses, the current study is mainly characterized by descriptive analyses, although the risks for belonging to a certain multimorbidity cluster associated with sex or another multimorbidity cluster are computed. In comparing the two studies it is not entirely obvious to me what motivates the report under scrutiny this time.

This has to be clearly explained to the reader.

**Response from the authors**

We have rephrased the last paragraph of the background section to stress the justification for sending this manuscript to publication and we have addressed this issue also in the discussion section more thoroughly.

**Comments from the reviewer**

I lack a proper discussion of the age and sex disparities in prevalence figures studied here, in relation to the development of multimorbidity patterns that was studied in the previous study. In a lifespan perspective, what meaning do the varying prevalence figures in this more fine-grained study of the older age span have (lifespan development of multimorbidity was studied in the PlosOne paper).

**Response from the authors**

We have made changes in the discussion section to address more properly the differences in age and sex prevalence and in the evolution of these patterns in a lifespan perspective.
Comments from the reviewer
This might be a minor comment, but what kind of figures are presented in Table 4? Percentages? For example, the prevalence of dementia/delirium among men who are 65-74 years cannot be 31.1%. It actually says in the Results that the prevalence of diabetes and hypertension exceeds 80%, which is a figure that can be found in Table 4. I find that hard to believe.

Response from the authors
We have included a note in table 3 explaining the rates of prevalence presented in it. They refer to the prevalence of the diseases (in percentages) among the patients with specific patterns of multimorbidity, and not to the general population with multimorbidity. Among patients with a specific pattern, we expect that the prevalence of the diseases that define this pattern will be high. In Appendix I we present the prevalence of the diseases in the general population. We also address the figures of prevalence of specific diseases in some age-sex groups in the discussion section.

Comments from the reviewer
Abstract: The age range under scrutiny should be mentioned. The word “elderly” has many meanings.

Response from the authors
We have specified in the abstract the age range of the study.

Comments from the reviewer
Background: Third paragraph, lack of coordination. I assume it is between primary and specialist care?

Response from the authors
We have specified lack of coordination between primary and specialist care and between routine and emergency care.

Comments from the reviewer
Methods: “Methodos” should read Methods

Response from the authors
We have corrected the METHODS section title

Comments from the reviewer
Discussion: The directions of the CM patterns need to be discussed. The prevalence increases with age in men and decreases in women. It is not enough to say that gender disparities in diagnosis and treatment have been discussed elsewhere.

*Response from the authors*

*We have addressed more thoroughly the explanation of the gender differences in the evolution of prevalence of some diseases.*

*Comments from the reviewer*

About the structure of the coding system: for which health problems is there a risk for underestimation of the frequency (the authors mention this under strengths and limitations)? I also wonder whether there are administrative or economic motifs for registration of certain diagnoses? Registries of this kind are often motivated by economic incentives. Also, were the diagnoses registered as primary, secondary etcetera? Were there limits to how many diagnoses that could be registered for a given individual? I believe this is important information.

*Response from the authors*

*We have rephrased the statements about the risks of underestimation for certain diseases with some examples and explained that the registration of the diseases is not linked to economic incentives in our system, as they are not used in billing systems or financial incentives to the professionals. There is no difference among primary or secondary diagnoses as we refer to chronic diseases diagnosed to the patients along their health encounters with the primary care professionals and do not refer to specific clinical episodes. There is no limit to the number of diagnoses per patient.*

*Comments from the reviewer*

The term “frailty” is mentioned at several places throughout the manuscript without a proper definition. What do the authors mean by “frailty”?

*Response from the authors*

*We include the definition we use for frailty with an adequate reference.*