Reviewer's report

Title: The frail older person does not exist: development of frailty profiles with latent class analysis

Version: 1 Date: 30 Dec 2017

Reviewer: Jordi Amblas Novella

Reviewer's report:

This very interesting manuscript proposes a subdivision of the frail population into six subgroups by using the latent class analysis methodology, according to the physical, psychological, cognitive and social variables. The article is exposed quite clearly and with great methodological rigor.

GLOBAL COMMENTS: 1. The manuscript states that a frail group of population was analyzed. Given the global characteristics of the described patients they are certainly very likely to be patients with diverse degrees of frailty, frailty validated tools have not been used. For example, what does lead us to assure that subpopulation A is frail? It should be mentioned as a limitation and/or discussed. 2. The classification into subgroups was performed from the characteristics of the variables related to frailty and from a rigorous methodology. However, the subgroups validation and classification according to the frailty degree based only in qualitative methodology with a focus group with older people may imply some limitations that should be mentioned. For instance, why not to do it also with professionals? and/or why not use also a quantitative methodology comparing this with a Frailty Index or a Clinical Frailty Scale?. 3. Throughout the manuscript the population and individual visions are frequently overlapped. This may create some confusion among the readers, since when the population heterogenicity is mentioned there is no reference to the unique features of one person, but to the presence of subgroups in a sample. For instance, the subgroup analysis could be useful for research purposes or in the identification of population target that may benefit from a series of specific measures (a model of comprehensive care, for example); however, such analysis would very likely be insufficient for personalized decision-making processes, that is, for clinical practice purposes (https://doi.org/10.1186/s40985-016-0021-8). SPECIFIC COMMENTS: 1. According to the journal guidelines for contributors, the manuscript should have been presented with double space. 2. TITLE: The title is explicit and clear, although according to BMC regulations it would be advisable to include the type of the study design (e.g.: "A versus B in the treatment of C: a randomized controlled trial", "X is a risk factor for Y: a case control study", "What is the impact of factor X on subject Y: A systematic review"). 3. ABSTRACT: It's easy to understand, highlights the main results and conforms to the rules of the journal.* In the conclusion section of the abstract, the reviewer considers that the authors make some statements that go beyond the conclusions one can derive from the results of the study. For example, authors assure that "the conceptualization of frailty should be based on (a combination of) dimensions rather than on one dichotomy of frail-not frail, or on frailty scores". This statement is probably excessive and presents two problems. On the one side, it blends different concepts, the existing debate on HOW to identify frailty - dichotomy (Fried criteria) vs continuous (Frailty indexes) visions. On the other hand, the need of a multidimensional approach of the frailty concept. 4. BACKGROUND: The title of this section is missing. The background gives enough information to contextualize the research question. The reviewer wonders whether it would be
necessary to clarify the following:*Pg3/Line 12-13: In order to improve the background section, authors could consider that if determining the frailty degree from a perspective of population analysis can help to identify groups of population with specific needs, it is also true that from a perspective of individual assessment of the frailty degree one can obtain an estimation of the biological age of people, as stated in reference https://doi.org/10.1186/1471-2318-2-1 .*Pg3/Line 18 (and also in the abstract): What do authors really refer to when they talk of the complexity frailty? Its conceptualization? Its clinical approach? Please clarify*Pg3/Line 26: Authors mention the inclusion the psychosocial approach to the concept of frailty is more recent that the physical one, but reference 3 (from 1994), puts into value the psychosocial domain.*Pg3/line 34: The concept of accumulation of deficits was not yet described in 1994 (see reference 3).*Pg 4 /line 4-7 ("Our research…these interventions"): The reviewer considers this statement should not be included in the background section, since it makes reference to the results of the study and its possible implications. We suggest to add it in the discussion. A good alternative could be to add it as a work hypothesis.5.METHODS: The methods section adequately explains the aim, design and setting of the study, the characteristics of participants and the description of materials. The type of statistical analysis used, including a power calculation is appropriate. *Pg 5/line13: Please use formal language throughout your paper: that is avoiding shorthand abbreviations such as "i.e." 6.RESULTS: The results are clearly exposed and in a logical sequence. No substantial errors were detected in the statistic analysis. The explanation of the characteristics of the subgroups within the text, box 1 and tables 2 and 3 are probably quite redundant. I would suggest to unify all the information by leaving the data of the third step of the table as it is and describing in the text only steps four and five.7.DISCUSSION: The results are sufficiently discussed, the limitations of the study are adequately described and new lines of research are proposed for the future. The reviewer would only like to comment on the following:*Pg 10/line 2-4. The design of the study hardly allows to establish causal relations. The sentence should be modified or deleted.*Pg 10/line 52. The reviewer is agree to the limitation about the assessment of the social domain. But the sentence "the formulation of the social functioning item which may have affected our results slightly", is probably too "daring" because is hard to know the degree of interference (slightly or considerably) from the obtained results.8.CONCLUSIONS: According to the journal regulations, it would be advisable to propose the main conclusions and provide an explanation of the importance and relevance of the study reported.9. The BIBLIOGRAPHY is globally well referenced, with some exceptions (see the attached "reference checking" document).10FIGURES, TABLES & ADDITIONAL FILES: * Table 1: Please note that for each category the total sum up does not correspond to the global "n". For example, the "n" of the five self-reported categories is 40.163; and the "n" of the cognitive functioning is 38.827; … Could you please clarify if the rest of persons are missing data? On the other hand, the percentages should also be revised. For instance, 26009 women for a "n" of 43.704 corresponds to the 59.51%, not 59.6Finally: Please specify that SD stands for standard deviation.* Table 3: Please specify what do the numbers of the table refer to. Percentages?

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
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No

**Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?**
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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