Reviewer's report

Title: Subgrouping patients on the basis of their individual course of low back pain over a six month period

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Reviewer: Ivan Steenstra

Reviewer's report:

This paper doesn’t seem to ready yet.

1. Is the question posed by the authors well defined?
   Yes the question is well defined, I am however not convinced by the relevance of it.

2. Are the methods appropriate and well described?
   They seem appropriate although my experience with Ward and K means analysis is that it does not always give stable solutions. And that's why other authors have opted for latent trajectory analysis. See also the recent study by Tamcan et al 2010 in Pain. And I think they purposefully talk about clusters and not subgroups.

3. Are the data sound? Yes, and collected in a very creative way. It was a lot of work, texting must have been a lot of fun and led to a ton of data.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
   Some have reported on the appropriateness of subgroup analyses. My issue with this study is, is that it is a description of a characteristic that can not be determined at start of the study. It is a description of “subtrajectories”, not so much of different characteristics of “subgroups”. In the end we learn that different people recover differently. The significance of the difference is clear, but the relevance of the difference is unclear.

   Reporting on the longitudinal cohort part of this study should comply with MOOSE standards or similar.

   I could not find Table one containing information on functional limitations, pain intensity, age, gender, duration of complaints (maybe even work status and job demands!) and radiating pain.

   Sun et al in 2010 have reported a number of criteria to assess the credibility of subgroup analyses

   Design:
   Is the subgroup variable a characteristic measured at baseline or after randomization?*: in this case the answer is NO, and this is my main problem with this kind of paper. What is the relevance of such findings if we can only wait and see how things unfold. I want to see the study that predicts the trajectories. You
present is as a data driven, exploratory study. Well, I think we explored this area and it is time for hypothesis testing. I do not see the clinical significance as well as the authors do. More studies like this will only lead to a messier table 3 as presented here. And does not lead to solving any issues.

Is the effect suggested by comparisons within rather than between studies?
Was the hypothesis specified a priori? No, it is presented as exploratory and data driven.
Was the direction of the subgroup effect specified a prior? No, the relevance of an effect was based on statistical significance.
Was the subgroup effect one of a small number of hypothesized effects tested? NO

Analysis
Does the interaction test suggest a low likelihood that chance explains the apparent subgroup effect? NA
Is the significant subgroup effect independent? NA

Context
Is the size of the subgroup effect large? NO, 23 is not big for a subgroup, the authors even present a solution where one of the groups is only 3 people.
Is the interaction consistent across studies? The authors report it is.
Is the interaction consistent across closely related outcomes within the study? The trajectories are based on the outcome, so NA
Is there indirect evidence that supports the hypothesized interaction (biological rationale)? No rationale is provided.
It is unclear what the flow of participants looked like (maybe in the earlier paper, but I would like to see it here) and I need that to know about generalizability. The 80% response rule seems questionable and it is not clear how many subjects are lost through this decision.

5. Are the discussion and conclusions well balanced and adequately supported by the data? It seems a bit short, the authors are mainly concerned about others concluding on effectiveness of chiropractic care, and I think the authors should not bother to address such concerns, because this paper is clearly not about treatment effectiveness since it is not an RCT. The clinical relevance of the findings are examined by univariate comparisons that are of limited value. Can a clinician predict the future trajectory of his/her patient base don characteristics present at first visit (the 4th week variable might be of interest if you want to know the trajectory at end of treatment only).

6. Are limitations of the work clearly stated? Limitations in the methods are not addresses. Biological rationale is not addressed. Recruitment rates are not reported and therefore also not discussed. Loss to follow up is not addressed.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? I think they are missing a few publications. For instance Chen et al in Spine.
8. Do the title and abstract accurately convey what has been found? YES
9. Is the writing acceptable? YES

Overall: So some recover fast some not so much and other keep on having it off and on. What's clinically relevant since these trajectories are based on information over the course (it is about sub trajectories, not so much about subgroups). These outcomes become relevant if we are able to predict who should be classified to which trajectory and it would be interesting if this information could be used to improve treatment. The only use of such a classification is patient education and improvement of care. Only if therapy will differ substantially in the different trajectories and in the end the treatment effectiveness of the entire population increases will this be relevant. These issues are not addressed.

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

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

I declare that I have no competing interests