Reviewer's report

Title: Prognostic factors of a favorable outcome following a supervised exercise program for soldiers with sub-acute and chronic low back pain

Version: 0 Date: 26 Sep 2017

Reviewer: Corey Simon

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This is a derivation CPR study to determine variables indicative of a 50% improvement in low back pain disability following a multi-stage exercise program. The primary finding was that five predictors determined patients most likely to benefit from the program: no pain lying down, no use of antidepressants, FABQ work score < 22.5, fewer than 5 physiotherapy sessions before entering the program, and less than 6 months' work restriction. Key strengths of the study are measurement of variables at baseline, as well as a conservative operational definition of success (50% ODI change). However, I have multiple concerns which prevent me from endorsing the study for publication in its current form:

Study design: The current consensus (Hancock 2009, Mistry 2014, Saragiotto 2016, Cook 2017) is that variables considered for prediction should not be tested in a single-armed trial; rather two-armed randomized trials with assessment of interaction effects. The authors note this as a limitation of the current study, yet still frame this as a CPR study. The need for an RCT isn't for validation; rather, to ensure the CPR is a predictor of treatment response and not a predictor of outcome, independent of treatment response (Haskins, 2015). Individuals who meet the CPR will always have a better outcome, regardless of treatment response. Further, Cook (2017) noted that for many musculoskeletal conditions, identified CPR variables are indication of natural history, where patients will recovery over time regardless of the treatment prescribed. This is logical, given that the five identified CPR variables in this study. Notably, your study sought to eliminate acute LBP symptoms through pre-PT. However, without a control, we're unable to determine whether the CPR variables are indication of treatment response or outcome.
Overfitting/collinearity: The justification of 10-15 variables per participant is to prevent model overfitting, not to adequately power your model. While each model iteration may have only included 5 variables, in truth, you had roughly 50 (Tables 1-3). Further, Haskins (2015) argues against using univariate screening to reduce predictors entered into the multivariate model as it does not prevent model over-fitting. As such, either the sample size would have to be commensurate with the total number of variables, and/or the tests would need to control for multiple comparisons. Another alternative is to only use variables which that can be justified a priori as having a treatment effect.

Generalization: My first concern here is that the study sample is made of military members; which do not represent the vast majority of the population seen for non-specific LBP. This is noted as a limitation by the authors; however, there are still multiple instances throughout the manuscript where generalization to patients is inferred. Second, participants received PT for Acute LBP prior to entering the exercise program. As such, at least part of the study sample received two different forms of intervention. I understand that the aim was to identify a CPR for a multi-stage exercise program; however, the sample included in the study is highly specific. Lastly, the discussion language seems to suggest that study findings can be used to determine clinical success. It's crucial that the language here is tempered, since the CPR in question is only at the derivation stage. Clinical utility can't be determined until the current study is subjected to first validation studies, and then impact analyses.

Rather than framing as a CPR, the authors may consider 1) reducing generalization inferences and just talk about the value of such a maintenance program following acute LBP for military personnel; and 2) examine only factors previously shown to influence disability change with rehab, and reporting the variance of those factors. This would be a viable work-around to the study design issues and provide important information which could then be used in a subsequent two-armed randomized CPR study.

Other Concerns:
1) You set your KMO threshold at 0.6, and then decided to include factors which didn't meet the threshold. Please provide rationale.
2) I did not see any mention of collinearity tests in your statistical analysis.
3) I understand categorical variables are more convenient clinically; however, partitioning continuous variables into categorical variables is based on your sample only. I'm concerned that such splitting of variables will not generalize to the target population.

Cited:

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.
No

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.
No

**Are the conclusions drawn adequately supported by the data shown?**
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Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?

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I am able to assess the statistics

**Quality of written English**

Please indicate the quality of language in the manuscript:

Acceptable

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