Reviewer's report

Title: Predictors of Shoulder Pain and Disability Index (SPADI) and work status after 1 year in patients with subacromial shoulder pain.

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Reviewer: Wim Grooten

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ALL BELOWMENTIONED COMMENTS SHOULD BE CLASSED INTO CATEGORY C: Discretionary Revisions: i.e. recommendations for improvement which the author can choose to ignore.

My decisions are
- Accept after discretionary revisions
- The level of interest is somewhere between number 3 or 4: An article of importance in its field or an article whose findings are important to those with closely related research interests
- The quality of written English is acceptable
- The manuscript does not need to be seen by a statistician.
- 'I declare that I have no competing interests'.

Reviewer's comments

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I have tried to answer the abovementioned 9 questions:

1. Is the question posed by the authors well defined?
Yes, the questions are clear formulated. I believe it is a very important clinical issue to find predictors for long-term negative outcomes. Using data from an RCT again is a good approach.

2. Are the methods appropriate and well described?
Yes, it is easy to follow the structure of the paper and most of the methods are described appropriate or previously in one of the two papers describing the RCT (ref 12). This should perhaps be mentioned as one of the essential draw-backs to this paper. This paper does not contain all the information needed to understand
it: there is e.g. no “flow chart” showing the material of the study (the characters of the drop-outs). Moreover, this manuscript does not contain clear descriptions of many of the potential predictors: shoulder pain characteristics (which scales were used to measure intensity, duration?), occupational factors (which questions were used?), what is the range of the scales of the EQ-VAS, HBB, etc...

I believe the editor should decide if referring to the original RCT-study and other references (e.g. EQ-VAS, HBB) is good enough, or if the methods section should be expanded, in order to have this paper readable, i.e. free from checking in several other papers. Personally, I prefer to have more explanations of the predictors used in the manuscript, since the understanding of the results is difficult; eg. HBB: what does a median of 13 means?).

The description and choice of statistical methods seems appropriate to me, however the decision of adjusting for confounders ONLY in the final models can be discussed. Why did the authors not already use the confounders treatment, age and sex in the univariate analyses?

3. Are the data sound?
Yes, I have checked it against the data with the tables from the original RCT and it seems that no mistakes have been made.

I have one question about the predictor Work status that was highly significant in both univariate models, but did not appear in any of the final models. What happened? Perhaps this predictor was too high correlated with Education?

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
Yes, I believe it followed the standards used.

Table 1 is somewhat difficult to understand; what do the numbers represent? Some numbers represent means, and some are frequencies depending on the type of data used (continuous/categorical). Eg, it took me some time to understand that previous shoulder pain was dichotomized, while for “pain at rest” a mean was calculated. Perhaps how the data was treated should be described previously just before “statistical analysis”. One idea is to change that heading into “data treatment and statistical analysis” and use the first paragraph to describe this kind of issues there instead of the (too short) explanation in the footer of Table 1.

Table 2 contains a lot of information and perhaps it is a good idea to split that table into 2 separate ones; one for SPADI and one for Work status. Then, since there were some missing data especially on work status and the reader has no idea how many were not working (this should also be mentioned in the text), the exact number of subjects in each category can be provided. I would like to know for each category how many subjects were working/not working at time of follow-up. It will be also clearer then which category was used as the reference, eg gender).
In Table 3, the variable health status is described as (EQ5D) and not (EQ-VAS) as previously. Is that the same variable?

5. Are the discussion and conclusions well balanced and adequately supported by the data?
Yes, the discussion contains many important things and highlighted several ideas for further studies.

I don’t know if this is important but I just noted that the discussion starts with discussion the results concerning work status; in the rest of the paper the SPADI is mentioned first.

The authors mentioned that gender had no influence on the prognosis but there is no further exploring of this interesting issue. The large number of references shows that the authors have read many studies on this topic and I would like to have some opinion from the authors about this. Was the study-population to small to detect differences or is it a “real” finding the gender is not associated with a poor prognosis for women (the OR was increased although not significantly). Perhaps the word “sex” is better than “gender”, or were there any other aspects than “sex” taken into account?

Is there any information available about the treatment the subjects received during the follow-up period? The significant interaction between those with low SPADI scores and SE (supervised exercises) could perhaps be due to the fact that the subjects kept on with their training (at home), while those with rESWT or high SPADI scores did not. Was treatment group a predictor? Now, it was only used as a confounder, it should be interesting to know if the SE group had a better prognosis than the rESWT group. Can this be included in the table with the univariate analyses; gender and age are presented?

6. Are limitations of the work clearly stated?
Yes, the authors discussed the pre- and cons with their study in a special paragraph and they are aware of most of the important issues.

The authors state in the second paragraph under this heading that the small sample size and the 10% drop-outs could have biased the results, but they should also try to mention into what direction this bias works. For example the fact that Work status at baseline did not turn out to be an important predictor could be due to multicollinarity between some factors or because of those without jobs were lost to follow-up. These two facts could both LOWER the possibility to find significant predictors, because of the dilution of the ORs towards the one-value.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
Yes, the reference list is extensive, maybe too long?

It is clinical somewhat disappointing that only these factors turned out of importance, i.e. factors that are NOT the factors that usually are treated by
physiotherapists (pain levels, range of motion, functional capacity) or possible to influence by the patient him/herself e.g. exercise. What should the caregiver/patient do in order to improve long-term patient outcomes? Concerning the discussion on the predictors for work status one year after treatment, there is evidence that shows that only previous sickness absence and not any of the clinical signs of neck/shoulder pain could predict future sickness absence in a large population-based study. [e.g. Grooten W. Predictors for persistent neck/shoulder pain, medical care-seeking due to neck/shoulder pain and sickness absence. Clin Rehab 2007: 21: 648–659]. I believe that this reference is in contradiction to the results that showed that work-status at baseline was not an important predictor (only in the univariate analyses). Perhaps the authors could discuss this issue.

I believe that reference number 34 appeared after the wrong sentence. This number should not be placed after the sentence: “The reasons for not working may also depend on …” [see DISCUSSION, heading Predictors], but after the sentence just previous: “Lower health status may predict both work-related…”. Please check this once more.

8. Do the title and abstract accurately convey what has been found?
The title is more like a descriptive title and does not tell anything about the results, but still very appropriate and. I noticed small discrepancies between the aim of the study, the objectives of the study described in the abstract, and the title (conservative treatment is mentioned only in the aim of the study) but that didn’t bother me too much.

The abstract contains all necessary information. I would like to reformulate the last sentence of the results-paragraph into: “Adjustments for age, gender and treatment group were made”, since the results (OR=4.3) is the result from the multiregression model INCLUDING these confounders. Please correct me if I am wrong; I only quote the footers of Table 3 and 4.

9. Is the writing acceptable?
Yes! I think this is a very clear straight forward written paper. I am not a native English speaking person, but I had no problems with the readability of the paper, except for the sentence at the end of the paper (just before conclusion). In that sentence there are two negatives used: “The present study does NOT prove that shoulder-specific predictors are WITHOUT importance for outcome...”, please reformulate this sentence.

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

Quality of written English: Acceptable

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'