Reviewer's report

Title: Effects and predictors of shoulder muscle massage for patients with posterior shoulder tightness

Version: 4 Date: 31 January 2012

Reviewer: Darcy Vavrek

Reviewer's report:

Thank you for an interesting paper. Comments are given with the hope of strengthening an already strong paper.

General comments:

The question posed by the authors is well defined. The data appear to be sound. The limitations of the work are clearly stated. The authors clearly acknowledge the work upon which they are building. The writing, in and of itself, is well done.

The methods, while mostly appropriate, could use some clarity in reporting. The data analysis, with clarity, could better adhere to tradition for reporting comparisons of groups within a clinical trial. Stronger comparisons of treatment groups will influence what is reported in the abstract, discussion, and conclusion.

The issues which are suggested for revision will be classified into major compulsory and minor discretionary revisions below.

Compulsory revisions

There appears to be a desire by the authors to use within-group analysis with this data. While this appears to be well done there are several points that could be clarified.

1. Page 7, Methods: The randomization method does not make sense. “Sixty patients were randomized by computer generated permuted block randomization of 5…” Blocks for permuted block randomization generally has more than one block, with each size being an even number so there are equal numbers per group. If the blocks are always the same size, for example 4, then the blind would be broken and no longer concealed for every 4th patient, because there is no permutation. The authors may want to clarify how the randomization list was generated.

2. Page 12, Methods, Data analysis: The choice of a 2 group, 2-factor ANOVA comparison results in within group comparison but does not compare the change from baseline between treatment groups. This choice of analysis dramatically underplays the statistical and clinical significance of the paper as well as substantially weakening the impact this paper might otherwise have.

Instead the authors may want to consider comparing the treatment groups to one...
another with a linear model testing for significance. This can be done with linear regression using the “post” result the outcome and the treatment group as the predictor while including the “pre” result as a covariate. Other covariates might be considered. The difference in group means from baseline, after adjusting for baseline, will be dramatic judging from Table 1 and worthy of citation in the abstract. This may also strengthen/influence the discussion and conclusion sections of the manuscript.

3. Pages 12-13, Methods, Data analysis: The description of stepwise logistic regression is unclear. The paragraph is poorly written and could be interpreted in a number of ways. For example:

Interpretation A: Responders versus non-responders within the massage group were compared with the chi-square or t test for all potential predictor variables, as appropriate. Predictor variables that had a difference with a p-value < .05 were entered into a logistic regression model. The variables with the least predictive value were then removed, one by one, in a backwards stepwise fashion until all predictors in the model had p-values < 0.10.

Interpretation B: Responders versus non-responders within the massage group were compared with the chi-square or t test for all potential predictor variables, as appropriate. Then ALL the predictor variables were placed into a logistic regression model. The variables were then removed in a stepwise fashion.

4. Page 14, Results, Lines 4-10, 16-17: These results show statistical significance within groups from baseline but do not compare groups. This is something that authors will do when there is no difference between treatment groups whatsoever, and the authors are trying to save the clinical relevance of the trial to show how everyone improved. This trick is entirely unnecessary, as it would seem there is likely to be a statistical significance between treatment groups. All of these results will need updating.

4. Page 14, Results, Lines 10-11: This statement makes it unclear as to whether the intention-to-treat data was presented or the raw data was presented. It is done both ways in the medical literature. The authors may simply want to say something like “similar results were found for intention-to-treat analysis” or “similar results were found when dropout data was excluded.”

5. Page 30, Table 3: The results in this table are inconsistent. The description in the title of the interpretation is well done for the first two predictors. For the third predictor, one unit difference in slope would result in the odds of being responsive as 300 times greater for someone who is unit 2.0 versus unit 1.0. The author may have erred in typing one or the other. Where does the 1.8 come from?

6. Page 30, Table 3: The results in this table are inconsistent. In particular, the results for the PD slope look to be off. With a beta coefficient at 5.71, and a SE of 5.40, one would expect the upper limit of the 95% CI to be over 60,000, not the exact number as the OR in the model. The whole model needs to be double checked for typos.
Minor essential revisions

1. Page 1, Methods, last line: Logistic is misspelled as Logistric throughout the manuscript.


3. Page 11, Methods, Procedures, lines 3-5: This sentence repeats itself almost exactly.

4. Page 15, Results, Line 4: Logistic is misspelled as Logistric throughout the manuscript.

5. Page 30, Table 3: Predictor is misspelled as Predicator in the first column heading of Table 3.

6. Page 30, Table 3: The authors may want to mention that all of these variables are at baseline. Though this may be considered obvious and left out.

Discretionary revisions

1. Page 0, Title: Consider adding the phrase “Randomized Controlled Trial” to the title.

2. Page 1, Abstract Methods: The authors may want to state that n=30 per group somewhere in the description of the sample for clarity.

3. Page 16, Discussion, Lines 1-2: This sentence reads awkwardly.

4. Page 16, Discussion, Lines 11-13: The sentence beginning on line 12 looks like an orphan phrase. These three sentences may need some re-writing.

5. Pages 28, Table 1: The % gender may be better represented within the first row of the table than as part of the title of the table. The title for Table 1 could instead have parenthetically (mean +/- standard deviation; n(%)).

6. Page 28, Table 1: The total number in each group may be better represented underneath the group title as a second line under “Massage group” and “Control group” instead of as part of the title of the table.

7. Pages 29, Table 2: The % gender may be better represented within the first row of the table than as part of the title of the table.

8. Page 29, Table 2: The total number in each group may be better represented underneath the group title as a second line under “Responsive group” and “Non-responsive group” instead of as part of the title of the table.

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Needs some language corrections before being published

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

Declaration of competing interests:
I declare that I have no competing interests