Author's response to reviews

Title: Effect of Head and Limb Orientation on Trunk Muscle Activation During Abdominal Hollowing in Chronic Low Back Pain

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Author's response to reviews: see over
Reviewer: Phil Page
Reviewer's report:
Minor essential revisions:
Page 6, 2nd pp; maintain consistent tense when describing test procedures ("is", "was", etc)

Authors' response: We have modified the paragraph as suggested and the tenses are now all consistently in the past tense.

Pg. 7, bottom...is PBU a trademarked name? If so, should capitalize when calling out and then describe the manufacturer and source.

Authors' response: Done.

Discretionary Revisions:
Repeated the same sentence twice in manuscript; "A novel approach was that an inclusion criterion for the CLBP group was the presence of at least one PR."

Authors' response: We have deleted the replicated sentence in the discussion.

Recommend moving "Electromyography" section to just under "Procedures" heading to lead into "After EMG set up..."

Authors' response: Done

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Reviewer: Mark Lidegaard
In general I feel that the authors have answered the comment in the original review in a satisfactory manner and I only have some minor comments.

1. Minor essential revision:
This is comment nr. 9 from the first review “Why did you choose the frame? Furthermore it differs from the exclusion criteria pain free subjects (6 months)”. In the otherwise satisfactory authors’ response you state the following: “We have added the following citation for using a 6 month exclusion criteria. Turk DC, Okifuji A. Pain terms and taxonomies. In: Loeser. D, Butler SH, Chapman JJ, et al (eds.). Bonica’s Management of Pain, 3rd ed. philadelphia (PA): Lippincott, Williams &. Wilkins; 2001:18-25. 2.” However I can’t seem to find this reference neither in the text nor in the references.

**Authors’ response:** We have amended the exclusion criteria to provide greater consistency and clarity. We have also added the missing citation.

2. Discretionary Revisions:
This should be an “o” instead of a “0”

**Authors’ response:** Done.

3. Discretionary Revisions:
This is comment nr. 24 from the first review “Not important, leave out”. In authors’ response you state that this section has been deleted, however it still appears in the manuscript.

**Authors’ response:** The sentence has now been deleted as requested.

4. Discretionary Revisions:
In my opinion it would improve the reading experience if the numerous explanations/ reasons were highlighted. It can be done in several ways, fx. 1), 2), 3) etc., i) ii) iii) ect.

**Authors’ response:** We have added the numbers as requested.

5. Discretionary Revisions:
There is no information regarding Mr. Drinkwater

**Authors’ response:** Dr. Drinkwater’s biography information has been added.

6. Minor essential revision:
The following sections appear twice in the manuscript (First time on page 23)

**Authors’ response:** The additional replicated section has been removed.
Reviewer: Birthe Thomsen  
Reviewer's report:  
Major Compulsory Revisions:

1. Apparently, different inclusion/exclusion criteria have been applied to the two groups. If this is correct, then it is a very severe problem and invalidates any conclusion regarding an effect of the intervention.

Authors' response: We agree that this criteria was not properly explained. The two groups had the same exclusion criteria however since the control group did not have CLBP or apparent primitive reflexes, we did not explain well that the original list of exclusion criteria for the CLBP group also applied to the control group.

The exclusion criteria for the CLBP group was actually based upon them having non specific low back pain (no radicular Sx, no radiological diagnosis) or no known factors associated with primitive reflexes (severe postural abnormality, anti-depressant medication, opiate medication diabetes, previous neurological incidents, neurological conditions, heart surgery, diagnosed learning difficulties, withdrawal from alcohol or drug addictions or psychiatric conditions).

By virtue of not having back pain and excluding known causes of having primitive reflexes, these are the same except for the neck range of motion. This was not screened prior to coming (having neck pain was), but was screened on the day.

Revised paragraph:

“Inclusion for the CLBP group was identified by a score of over 12 on the Rolland Morris Disability Questionnaire (RMDQ) [27] and suffering from low back pain for greater than 12 weeks [1]. Subjects were excluded from the CLBP group if there was a presence of severe postural abnormality and/or signs and symptoms of specific LBP including: radicular symptoms, radiological diagnosis (specifically spondyloolisthesis or spondylolysis); limited neck range of motion or pain; known factors associated with primitive reflexes (severe postural abnormality, anti-depressant medication, opiate medication diabetes, previous neurological incidents, neurological conditions, heart surgery, diagnosed learning difficulties, withdrawal from alcohol or drug addictions or psychiatric conditions). A certified physiotherapist assessed the presence of PR. Intratester reliability of PR assessment has previously been established [28]. Exclusion criteria for the control group were any report of low back pain in the previous 6 months, limited neck range of motion or neck pain, or if they had any conditions (same as above) known to be associated with the presence of retained PR.”

2. Factors used for matching must be included as potential confounders in the analyses. Otherwise, bias may have been introduced by the design.
Authors’ response: That factors such as height, bodyweight and age were used for “matching” indicates that they do not need to be included in the analysis as confounders. To ensure the equivalency of the two groups, participant descriptors were statistically assessed and no substantial differences were observed between groups. Based on the presented means and SD’s in the participant’s section of the methods, we do not feel it necessary to present the results of statistically testing these variables.

3. The "novel approach" corresponds to using 50% confidence intervals instead of 95% confidence intervals. This corresponds to having a 50% chance of a Type 1 error, that is, falsely concluding that there is a substantial effect, when there is no true substantial effect. I believe that a Type 1 error rate of 50% is too large. Therefore, this "novel approach" should not be considered relevant unless the 95% confidence interval does not cover 0, and should then only be used as supplementary information in relation to defining the difference as "worthwhile".

Authors’ response: While our esteemed reviewer is clearly well-versed in statistics, we would like to take this opportunity to explain the philosophy underlying the expression of our results as magnitude-based inferential statistics are not as widely known as traditional p value (null hypothesis) based statistical approaches. We do not claim this as a “novel approach” however, since such a method has been published since the pioneering work of Liow & Hopkins (Med Sci Sports Exercise, 2003).

Clinical research should provide readers with information on the magnitude of result they can likely expect from a given intervention. Such results are best expressed with confidence limits and Cohen’s d (effect sizes). Our method of expressing results indicates that, while the true effect of any intervention is unknowable, we estimate that the true effect likely lies between the upper and lower limits. Then, by converting our absolute values of mean, lower, and upper confidence limits to Cohen’s d (effect sizes) we allow all variables to be compared on a common metric. Any effect with a substantial portion of the 95% confidence limit lying outside the range of -0.20 to 0.20 was considered to be unlikely to be trivial and therefore have a substantial likelihood (>75%) to be sufficiently large and consistent enough to be clinically meaningful. Such an approach makes questions of Type 1 and Type 2 errors, as well as confidence limits crossing the zero, moot since no inference is being made to statistical significance. Furthermore, the reviewer’s comment that 50% of the probability of type 1 error going in each direction (positive and negative) in a two-tailed null hypothesis test also becomes a non-issue as a null hypothesis is not being tested.

Such a method of data expression is much more information-rich to clinicians than the relatively uninformative “unlikely to be zero” result that comes from a null-hypothesis test of significance. We recommend reading the article from Iacobucci (2005, Journal of Consumer Research) for a commentary and outstanding reference
list of articles calling for alternative methods of expressing results in lieu of null-hypothesis tests. Other fields of research calling for statistical reform include:

- the American Psychological Association (6th edition, 2010);
- a host of medical journals (e.g. Hopkins, et al., Medicine and Science in Sports and Exercise, 2009; Ioannidis, PloS Medicine, 2005);
- statistical journals (e.g. Ziliak & McCloskey, Proceedings of the Joint Statistical Meetings, 2009; Armstrong, International Journal of Forecasting, 2007);
- and,
- even a ruling from the United States Supreme Court on statistical significance indicates that “This premise is flawed.” (Matrixx v Siracusano, 2011, p. 11).

In the past five years, our research team has extensively published this exact model in journals ranging from Medicine and Science in Sports and Exercise to Applied Physiology, Nutrition and Metabolism; from Journal of Athletic Training to Medical Problems in Performing Artists.

We therefore have sufficient justification and the necessary experience to publish such a method of expressing data.

4. How are the effects "standardized"? It should be specified which data are used to derive the standard deviation (SD) used in the standardization. Is it the control group in the current study? Or have the two groups been combined in some way? Or did the authors use some external standardization? Since the authors put quite a lot of emphasis on the specific size of the standardized effect, it is important to know which population the standardization refers to. Different choices of data/population can lead to quite different SDs.

**Authors’ response:** The calculation for a standardized effect size is outlined by Cohen (1988) as the observed effect divided by the variable’s pre-test standard deviation. We have revised the manuscript to reflect that “Qualitative descriptors of standardized (Cohen) effect sizes were calculated as the difference between means divided by the standard deviation of the control (supine) condition...”

5. The statistical model/analysis must be described. Did the authors apply standard analysis of variance techniques to the "standardized", but otherwise untransformed, measurements?

**Authors’ response:** We have expanded the statistical analysis section substantially. It now includes that “Precision of estimated (mean) differences between the control and CLBP groups were calculated using unpaired t-tests on log-transformed data …”

6. In general, all ratios should be log-transformed before analyzed using
standard, normal distribution based techniques, since ratios are highly likely to be very skewed distributed. Considering the small sample size, this may be very important. The results (including the confidence limits) should be back-transformed afterwards and presented either as the ratio CLBP/control or as the % difference = (CLBP/control - 1)*100%.

We emphatically agree with the reviewer and routinely log-transform all of our data to correct for non-uniformity of residuals that all-too-often happens in human performance data. The data for this manuscript was indeed log-transformed prior to analysis but, unfortunately, this step of our analysis was omitted from the manuscript. We have now included it in the revision.

Rather than presenting percent differences, we have presented differences as Cohen’s d (effect sizes). Cohen’s d effectively accomplishes the same thing as presenting results as a percent: results are shown on a common metric so the effect of the independent variable on all dependent variables can be compared.

**Authors’ response:**

Minor Essential Revisions:
7. To facilitate the interpretation of the results, please always present the effects using the control group as the reference group. Thus, higher values in the CLBP group compared to the control group should be presented as positive numbers, and lower values in the CLBP group compared to the control group should be presented as negative numbers. Right now the results are presented one way in Figure 2 and the other way round in Figure 3.

**Authors’ response:** The reviewer is correct. The figure legend was confusing. We have adjusted the figure legends to provide greater clarity.