Author's response to reviews

Title: Pain relief is associated with decreasing postural sway in patients with non-specific low back pain

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Author's response to reviews: see over
We thank the reviewers for their comments and it has provided us with much stimulation in considering our manuscript and findings. Our responses are shown below after each reviewer comment and we hope that they are sufficient.

Reviewer's report

Title: Pain relief is associated with decreasing postural sway in patients with non-specific low back pain

Version: 1 Date: 17 November 2011

Reviewer: Masood Mazaheri

Reviewer's report:
Major Compulsory Revisions

While your study aims to answer an interesting and relevant research question regarding postural sway in patients with low back pain (LBP), two major concerns need to be addressed in your manuscript for improving the quality.

1) First of all, your study seems to be not the first one that “investigate(s) the association of altering pain levels and postural sway”. Below you can see a list of 5 articles assessing body sway following recovery of back pain and/or disability:

Response:
We thank the reviewer for providing the references and agree that this was not the first study to investigate the association between altering pain levels and postural sway as also outlined in our discussion. However, as we stated this is the first study to do so over a broader range of individual pain scores and "...with a best practice experimental setup for COP measurements" with regards to reliability [1]. We do detail this so we would like to stand by our statements as we believe it is true and accurate..


2) Irrespective of the type of intervention, the results are inconsistent. Three studies [1-3] report no effects of successful treatment on postural sway although other indicators of motor performance such as psychomotor reaction times and reflex delays of back muscles are found to improve. Two studies [4,5] do find a decrease in postural sway after successful treatment of LBP.

Therefore, the literature does not always support your results indicating decreased sway following reduced intensity of pain. You should discuss extensively about the conflicts between your findings and those of other researchers. Furthermore, based on this controversy, your statement about the stronger cause and effect relationship between pain and sway compared to impaired proprioception and sway is not always true.

Response:
That there are conflicting results on this question is true. However, we believe that these differences may be due to the studies themselves. So, the fact that in contrast to our study no decrease could be observed in association with the intervention(s) may be attributable to the following:

Luoto et al.: Only a single measurement of just 15 sec duration was conducted. This may explain why no difference between patients and controls was detected at baseline (for males). As gender is not to be considered a confounding factor for postural sway analysis [1], the fact that female pain sufferers showed a significantly increased sway may suggest that the experimental setup affected the results.

Unfortunately, the interpretation of their results is further complicated by the fact that Luoto et al. did not provide VAS or NRS scores for the pain sufferers. It is therefore not possible to interpret and discuss the pain reduction and possible effects of COP sway.

In addition, the fact that the many patients were suffering from neurological symptoms associated with possible demyelination prohibits a comparison to our patients where such pathologies were specifically excluded as no short-term improvement in terms of neurological healing was to be expected.

Leinonen et al.: Here only a single 20sec trial was conducted for each eyes open and closed condition. In association with the small sample size (n=20) and the resulting inter-subject variability, the low reliability expected for this measurement procedures [1] may explain why no difference was observed.

Again, the possible demyelination associated with the disc prolapses prohibits a comparison to our patients were such pathologies were specifically excluded. Even with the 3 months follow-up period applied by Leinonen et al., neurological recovery may not have been completed.


Mehling et al.: a) the small sample size (n=12) and the associated inter-subject variability, b) the minimal average decrease of less than 10mm on the VAS scale (pre VAS 5.15 (±2.04), post VAS 4.37 (±2.36)). This is not considered a clinically significant
improvement and our sway data did also not identify a significant change in postural sway between those pain scores (see also [2]).


Here, we may hypothesize that a significant difference would have been detected between pain sufferers and controls with a best practice measurement protocol.

However, we agree with the reviewer that this should have been discussed and have done so by adding the following section(s):

"Some studies do not support our observations as no decreasing postural sway was observed following pain reduction [31-33]. This may be attributed to the fact that patients with neurological impairments were enrolled where demyelination probably inhibited full recovery within the follow-up period [32,33]. Secondly, these studies employed prolonged follow-up periods of 3 and 6 months, while we investigated short-term changes over the course of around 2 weeks.

The chronic low back pain patients without neurological symptoms enrolled by Mehling et al. [31] experienced only a minimal average pain decrease (VAS 5.15±2.0 at baseline compared to VAS 4.37±2.36 post-intervention). This is not considered a clinically significant improvement and sway data published in an earlier study did also not identify a significant change in postural sway between those pain scores [1].

In addition, the results may have been affected by high inter-subject variability associated with the small sample sizes or single measurements of short duration [27]."

Secondly, you have founded your research question on the proposition that “increased center of pressure excursions are well documented in patients suffering from non-specific low back pain”. This is based on the results of the following review:


3) However, more detailed analysis of the literature shows that postural deficit in patients suffering from LBP may not be consistent as you found in your review and it depends on experimental conditions (e.g. sensory manipulation) in which patients with LBP have been assessed.

Response:
We agree that the experimental setup and conditions exhibit an important effect on COP measures. Please refer to the response and discussion above. We believe this covers this comment.
4) The finding of your study also verifies this notion since there is no significant difference between LBP patients and healthy reference group, even at the baseline where no intervention has been delivered.

Response:
In this instance the reviewers observation is incorrect. You may have misinterpreted our tables as we did not provide any p-values for what you described. As outlined in our previous study [1], statistically significant differences between pain sufferers and healthy controls started at NRS 3 (mVel AP) and NRS 4 (mVel ML). Significant differences between individual NRS scores occurred every 2 pain levels. These results also account for the baseline and follow-up data presented in this study as it is derived from the same patient cohort as enrolled in [1].


When COP excursion has not been changed in patients with LBP compared to control subjects, why we should think of recovery of sway in this group of patient? Please provide some lines of discussion.

Response:
Please see above.
Reviewer's report

Title: Pain relief is associated with decreasing postural sway in patients with non-specific low back pain

Version: 1 Date: 4 December 2011

Reviewer: Peter Vaes

Reviewer's report:
Major Compulsory Revisions
Clarity of abstract and paper

Abstract

1. Several denominations are insufficiently made clear to the reader. For example: centre of pressure excursions.

Response:
We agree and have added the following (Background, Para 2, Line 2): "COP excursions refers to the characteristics of the COP path with regards to sway area and velocity."

In the abstract we have changed "COP excursions" to "postural sway" (Para 1, Line 1).

2. “…linear relationship between pain intensity and postural sway exists.” Do you mean … increased postural sway?

Response:
Yes. We agree that this should have been explained and have added the following: "… whereby a linear relationship between higher pain intensities and increasing postural sway has been described" (Abstract, Para 1, Line 2).

3. “matching”, only for ‘age’? Add for which other criteria the matching was carried out.

Response:
Yes, matching was only conducted for age. However, as expected for larger sample sizes there were also no significant differences between controls and pain sufferers with regards to weight or height, two other factors that have also shown to affect postural sway (Table 1).

4. the sentence: “The cut-off age for both controls and symptomatic individuals was 50 years as after that age related impairments to postural stability could not be excluded [22-24].” is unclear.
We agree and have changed the sentence as follows (Methods, Para 2, Line 3): "The cut-off age for both controls and symptomatic individuals was 50 years as after that possible age-related sensory impairments may decrease postural stability [24-26]."

5. Specify “three manual interventions” in the abstract, this is unclear, state (in the abstract) at least that chiropractic techniques have been used.

Response:
We have added the following (Para 2, Line 5): "...(e.g. manipulation, mobilization or soft tissue techniques)."

6. define “A clinically significant decrease in pain score…”

Response:
For the purpose of an abstract I believe we have done so sufficiently by adding "... of four NRS scores". This is later defined and referenced in more detail (Discussion, Para 4, Line 1).

7. Although three references have been given, a “change in pain intensity” must be corrected to “clinically relevant change” which is set at 38 on a maximum of 100 (in a VAS pain score). This should at least be discussed.

Response:
We agree and have done so (Abstract, Para 3, Line 2 and Discussion, Para 4, Line 3):

8. “equally unaltered” is incorrect.

Response:
We have added the following (Abstract, Para 3, Line 3): "... postural sway remained similar compared to baseline."

9. in the conclusion of the abstract the term “mean sway velocity.” is suddenly used, this is unclear. There is confusion about terminology because of the use of numerous synonyms for the force platform measurements: (1) center of pressure excursions, (2) postural sway, (3) Center of pressure parameters, (4) postural sway measures, (5) center of pressure excursions, (6) mean sway velocity.

Response:
We agree that the number of synonyms used may be confusing. We have therefore replaced "center of pressure parameters" and "COP excursions" with "postural sway" throughout the manuscript.

10. there is insufficient data to support the strong conclusion: “Pain interference appears responsible for the altered sway in pain sufferers.”
Response:
We basically agree and have discussed the fact that this is based on current theories and this study was not designed to answer this question. Therefore we have phrased it carefully as "appears to be", which we believe is supported by the data, and would like to stand by our statement.

Paper

11. references have to be added following the phrase: “Analgesic effects have been described for a variety of manual therapeutic interventions such as spinal manipulative therapy (SMT), mobilization or soft tissue techniques.”. A description of effects is a very weak argument not supported by evidence.

Response:
We agree and have added the references.

12. the argument that: “activation of mechanoreceptors…” could be responsible is very superficial and insufficient. It is not argued how motor activity would be able to decrease pain and lower muscle spasm as suggested in stating: “…activate inhibitory interneurons to affect alpha motoneuron pools of the paraspinal musculature [17], breaking the pain-spasm-pain cycle.

Response:
We share the reviewer’s concern about the underlying mechanisms. Unfortunately, the precise mechanisms remain largely unknown and we therefore intended to only briefly outline to the current concepts and hypotheses (with all associated limitations). As this was not a main focus of our study, we have referred the interested reader to the included references for more detailed discussions (e.g. Pickar et al. [1]).


13. no information is given concerning the ‘number needed to treat’, nor about the number of participants necessary to reach sufficient power to acceptably.

Response:
We agree with the reviewer that this would have been interesting information. However, as this study was conducted in a private clinical setting where patients were measured during normal care, we could not enroll symptomatic participants as a control group for ethical reasons. Consequently, we were unable to administer "no treatment", placebo or "control treatments" (e.g. analgesics). As such a control group is required for NNT calculations, such data could not be provided.

In addition, the effectiveness of the intervention(s) was not a primary concern for this study. Instead, we were interested in observing whether postural sway is altered in association with whatever happens to the pain levels (e.g. pain remains the same,
decreases or increases). We assumed that the treatment potentially decreased the pain, however, the study design did not permit any comments on causality if lower pain scores were actually reported.

14. inclusion criteria are insufficiently specific: “…serious(?) spinal deformities or previous injuries(of what nature?).

Response:
We have added the following (Methods, Para 2, Line 9): "...pronounced spinal deformities or previous traumatic injuries such as spinal fractures or whiplash associated disorders."

15. the description of the intervention is vague and insufficiently detailed.

Response:
We basically agree. However, the intervention cannot be described in more detail as they did not follow a specific protocol and were aimed at the individual complaint. One patient may have received gentle segmental spinal mobilization while another was manipulated at multiple sites at spine and extremities. Yet another may have only been treated with muscle stretches. This is a pragmatic approach which is often seen in RCTs. It is not testing purely one modality but a usual care approach.

16. outcome measures are not described in detail: for example: authors describe they measured “postural sway velocity” at one instant and in another “center of pressure excursions” is used.

Response:
We agree, as outlined in our response to Comment 9. "Center of pressure excursions" was deleted throughout the manuscript. We have kept "postural sway" as it allows us to make general comments about changes in sway without having to refer to each of the two COP parameters individually while sway velocity describes one aspect of it, namely the speed in AP and ML direction in mm/s.

17. where the “three static bipedal standing tasks” identical? Then authors should specify that an identical task was repeated three times…

Response:
Yes, they were. We agree that this should have been clarified and have added the following (Abstract, Para 2, Line 2): "Center of pressure parameters were measured by three identical static bipedal standing tasks of 90sec duration with eyes closed in narrow stance on a firm surface."
Results

18. 25% of participating patients dropped out. This was described as ‘loss to follow’ up which seems incorrect. No follow up was described in the research protocol.

**Response:**
"Follow-up" refers to the two measurements (post-intervention) following the baseline measurement at 3-4 day intervals.

To clarify this, we have added (Methods, subjects, Para 2, Line 12)

"For inclusion, patients had to consent to participate in three measurement sessions at 2-3 day intervals that were scheduled around their treatment appointments."

and re-phrased Methods, Procedures, Para 3 slightly to

"Based on a physical examination, the participating NSLBP sufferers received a series of three non-specific therapeutic interventions at 2-3 day intervals consisting of a selection or a combination of all of the following: a) manipulation, b) mobilization, c) soft tissue techniques. The treatments were administered by two experienced chiropractors with 8 years of clinical practice each (TB and AS) and targeted the whole kinematic chain."

Discussion

19. It should be discussed or added to the conclusion that better performance during balance test can be due to a learning effect and not only to decrease pain scores.

**Response:**
We agree and have added the following (Discussion, Para 7):

"While learning effects cannot be excluded as an explanation for altered postural sway at follow-up, this appears less likely as similar effects would be excepted for those patients where no decrease in pain occurred. However, no such effect was observed."

We also conducted a pilot study (unpublished) to investigate effects of learning/fatigue/boredom both inter- and intrasession for the experimental procedures described in this manuscript. No such effects could be identified for 10 consecutive or three times 3 measurements at 3 day intervals. The reason may be that for longer sampling durations (90sec) any potential learning effects that may e.g. decrease sway during the initial adaptation period is equaling out.

20. When relating pain to postural sway it is imperative that the impact of this health problem on 'motor control' is discussed. No information is found in this paper related to the impact of low back complaints with or without ‘a history of low back pain’ and motor control deficiency. This should be considered a weakness of this study.
Response:
We thank the reviewer for this comment. However, we believe that we have discussed the aspects of impaired muscular control relevant to this study sufficiently in the introduction. We believe that a more detailed overview, while interesting, would exceed the scope of this study and not add much to the interpretation of our results.