Reviewer's report

Title: Evidence for a general stiffening motor control pattern in neck pain: A cross sectional study.

Version: 1 Date: 13 November 2014

Reviewer: Markus Ernst

Reviewer's report:

General comments:
This study deals with a very interesting and comprehensive assessment battery in neck pain. It’s well written and uses clearly described data sampling and analysis methods and its publication is highly recommended. There are however, some points that need to be addressed in more detail within the methods, discussion and limitations, as this study partly contradicts current research within these topics.

Major compulsory revisions:

1. The main issue is that the lack of significant and meaningful group differences may have occurred, due to the sampling conducted?! Whether HC suffered from non-traumatic neck pain within the history remains unknown. Headache and dizziness conditions are not mentioned. Current and past (last month) pain recordings within Table 1 are missing for the HC group, and also the reader may assume that the HC are pain free, authors should address this issue or write N/A for not applicable or not tested. The reviewer doubts the correct sampling of healthy subjects, which is partly reflected in the JPE of approximately 5°, which is typical for a neck pain group, but not a HC group.

2. Authors should discuss the JPE, which lacks significance. According to the additional file, authors used the Absolut error (AE), why not the Variable error (VE) as more stiffening and probably less variability in the neck pain group might have been expected? Some studies recommend the use of at least 5 to 8 repetitions (Swait et al. 2007, Pinsault et al 2008), and found significant differences for one rotational direction. Please address a term, if applicable, like: “…as AE (VE) of both directions did not differ significantly from each other, they were summed up …” Authors are recommended to discuss this item in more detail, addressing the movement plane used (why not sagittal plane JPE, which involves gravity), and the error term, as mentioned before.

3. Trajectory movement control results contradict former research by Kristjansson et al. 2010, who not only found significant results for the Fly pattern A, but also within the other direction (less accuracy within the neck pain group). The author should discuss this difference in more detail.

Head steadiness: The authors discuss that the results by Woodhouse et al. (authors’ own research group) but not by other groups like Shahidi et al. 2012 or
Harris et al. 2005, the two the authors mentioned. A further study by Juul et al. 2013 measured holding time, but indication of fatigue was measured by a laser pointer, pointing towards the head of the subject. Authors should at least discuss the possibility of insufficient testing time for the high load test, as in both aforementioned studies, neck pain subjects achieved mean times of approximately 30 seconds, the time point the test ended in this study.

4. Postural sway: The postural sway results should be discussed too. The hypothesized stiffening strategy of the neck during the FOE test might have led to an overall stiffening of the spine, expressed by the decreased sway in neck pain. Another possibility might be that automatic control of the postural sway has been improved if subjects are distracted by another task, like in dual task conditions. Mazaheri et al. 2010 showed results in LBP using different levels of a cognitive dual task. The authors should discuss that possibility.

Minor essential revisions:
1. Background, line 94ff: you refer to the two articles by Treleaven and Kristjansson & Treleaven which do not “vary greatly”, but suppose the use of a similar testing battery. I would at least suggest deleting the word “arbitrary”.
2. Postural sway section, line 225: at the end there are some words missing? Like ...interval for...
3. Statistical analysis: line 241-243: I see the point of non-transformed values, but for regression models, the log-transformed values should be reported. Models with skewed data are not suitable. I would recommend changing the tables accordingly and reporting suitable measures of central tendency and spread.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests