Reviewer's report

Title: Cognitive, fear-reducing information or individual symptom-based physical training in chronic LBP. A pragmatic, randomized, controlled trial with 1-year follow-up.

Version: 1 Date: 5 January 2010

Reviewer: Steven George

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This paper from Sorensen et al describes a well implemented and rigorous RCT comparing a CBT approach with a symptom based physical training group. Primary outcomes were comparable among the groups and in the secondary outcomes lower fear-avoidance was associated with the CBT group.

The paper’s primary strengths are its experimental design, use of validated measures, clearly stated primary and secondary measures, 1 year follow up point, and consideration of a relevant and important topic. There are, however, some areas in which this manuscript can be improved and I have outlined these below:

Major Revisions

1. The Introduction focuses too much on the role of the IVD and physical loading for this current study. There are no measures of IVD degeneration (or any other patho-anatomy) and physical loading in the current study, so this information could potentially distract readers from the primary aim of the current study.

2. The Introduction and Discussion would be enhanced by considering other CBT techniques that have been studied with LBP as the authors have primarily focused on the model proposed by Indahl. There are other cognitive models that have supporting data. Factors from these models that converge and diverge with the Indahl model would be of interest to readers.

3. Along the same lines the authors should highlight the lack of spinal manipulation in their physical training approach. Spinal manipulation is a physical intervention that has been recommended for patients with acute and chronic LBP (Chou et al, 2007). Considerable work has been done in identifying LBP patients appropriate for spinal manipulation and these studies have been associated with larger effect sizes in patients with acute and sub-acute LBP. Although not as much work has been done in chronic populations the lack of consideration of spinal manipulation deserves mention in this paper. There is potential that patients could have received additional benefit from receiving spinal manipulation in the physical training approach.

4. The symptom based physical training procedure needs to be better described. This section should have more references or the authors should have provided more detail to support the face validity of this particular treatment approach. A
major weakness of this paper is that this symptom based physical training approach has not been previously described in the literature and lacks supporting psychometric data. In my opinion there is not a lot of clinical evidence to support identification of these subgroups as described in the manuscript. For example, there is a preliminary clinical prediction rule for identification of subjects that would benefit from lumbar stabilization (Hicks et al, 2005), but this prediction rule was not included in the current study. Furthermore there is no empirically validated way of determining which patients with chronic LBP would benefit from the intensive dynamic exercise program. Centralization is the one exception because there is ample evidence for its use in a sub-group. However, more detail is needed than “…a complete MDT examination to find a possible directional preference – page 9” as even MDT examination is not standardized.

5. The ANCOVA models seem to focus on the main effects of treatment and time, when it is the interaction that is of primary interest for treatment analyses. Please clarify whether any of the p-values reported in the text or table were for the interaction.

6. There is no consideration or plan for missing data in the paper to determine how those not completing follow up affected results. The authors compare non-responders to responders in Table 4, but typically trials included an intention to treat or sensitivity analysis to determine influence of those lost to follow up.

7. There is no sample size estimation or power estimate provided for this paper, which is an important issue for a trial reporting null results.

8. The Discussion lacks a consideration of the study limitations, some of which have been previously highlighted in this review. I encourage the authors to consider adding those, and other relevant limitations. For example, the 2 group design permits the authors making conclusions about the absolute effect of any of these interventions. Therefore, their conclusions about reconsidering the role of physical training may be overstated, because they lack the data from the current study that either of these intervention has an absolute effect.

Minor Revisions

1. In the general intervention section (page 7) the authors note that both groups received “additional specific physical examination, particularly in the physical training group, explanations of the MRI scan, …” Please clarify if this procedure was exclusive to the physical training group or not. If it was not exclusive please provide the percentage of subjects in each group that received the additional physical examination.

2. Along the same lines the authors note that information on “patho-anatomy and physiology... emphasizing the positive aspects rather than focusing too much on possible abnormalities, unless they had particular significance.” Please clarify if this procedure was standardized for the CBT group and provide the percentage of subjects that received positive or negative information during this consult. Also please clarify that this information session was NOT part of the physical training intervention.

3. Typically in RCT’s the follow up period is based on the date of randomization,
not the duration of the treatment. The variation in the follow up period (ranging from 2-4 months as per page 10) should be noted as a limitation in the manuscript because the analysis is comparing patients at different time points.

4. The rationale for using an ANCOVA with baseline values needs to be presented. Typically there is an a priori determined rationale for use of co-variates or their use is based on preliminary statistical analyses. Justification of this analytical approach is warranted because this is an RCT with no obvious baseline differences.

5. The authors note that “No side effects were recorded – page 14”, but do not explicitly mention adverse events. Please clarify if any adverse events were recorded.

6. I would discourage the authors from using the term “state of art” to describe the physical training approach described in this study (Conclusion – page 16). As previously stated this approach is not empirically supported and therefore cannot be considered “state of art”.

7. The authors follow CONSORT guidelines for the most part, but I would encourage the authors to look at the CONSORT guidelines for non-pharmacological studies and providing a check list of those completed to facility review of the revised manuscript.

8. Tables – the authors should present mean (sd) in all tables to allow for calculation of effect sizes for future systematic reviews and also these data better match the central tendency that was used in their ANCOVA models. As previously mentioned please clearly indicate whether Table 2 reports any of the interaction effects for these models.

9. Figure 1 is not necessary for interpretation of this manuscript, and I recommend its removal, especially since the authors do not include measures for confidence, focusing, tension, and awkward movements.

10. Figure 2 does not include how many subjects were included in the analysis as recommended by CONSORT.

**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