Reviewer’s report

Title: Effectiveness of physiotherapy exercise following hip arthroplasty for osteoarthritis: a systematic review of clinical trials.

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Reviewer: Martijn Steultjens

Reviewer’s report:

This is a thorough review of the literature on the effectiveness of post-surgery physical therapy for hip arthroplasty patients.

Nevertheless, there are a few issues that I feel need to be addressed:

Major compulsory revision

1) It is not entirely clear how study quality was defined. Based on their assessment, the authors state that only two studies were of sufficient quality to be meta-analysed. However, from their overview of study quality in Table 2, it is unclear by what criterion this decision was made. Specifically, the study of Suetta et al., 2004 appears to be of similar quality to the studies of Jan et al., 2004 and Nyberg and Kreuter, 2002. On what basis was the former deemed to be of sufficient quality and were the latter two judged to be of insufficient quality? Reading the author’s comments on the latter two studies, it appears that a lot of weight was given to the non-random nature of treatment allocation in these two trials. Conversely, blinded outcome assessment appeared to play only a marginal role in study quality assessment, as neither of the two allegedly best studies used this. In my opinion, assessing study quality is equal to assessing the risk of bias inherent in a study’s design and execution. I would argue that an outcome assessor aware of treatment allocation presents a far greater risk of bias than alternate assignment of subjects. I would therefore like to invite the authors to clarify their assessment of overall study quality based on the data in Table 2.

Discretionary revisions

2) I was surprised by the authors’ statement that pain outcomes were not generally part of the trials included in this review, although a quick look through these studies shows it to be correct. Nevertheless, pain is regarded as the primary symptom in hip OA. Therefore, having gone through all these studies in more detail, could the authors comment on the, to me at least, surprising absence of attention to pain in these studies?

3) Due to the heterogeneity of included studies and quality issues the authors have only done a limited quantitative data synthesis. Other systematic reviews in allied health care, dealing with the same problems, have instead opted for qualitative data synthesis based on identifying levels of evidence (such as provided by the Oxford Centre for Evidence-based Medicine at
http://www.cebm.net/index.aspx?o=1025, for instance) for treatment effectiveness. I would like to suggest that the authors do the same in this review, as this approach provides an excellent at-a-glance state-of-the-art of the field.

4) And finally, the mandatory spelling mistake: 'outpatient' is one word (p. 8, 3rd paragraph).

**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.

**Declaration of competing interests:**

I declare that I have no competing interests.