Reviewer's report

Title: Prevalence of low back pain in children and adolescents: a meta-analysis

Version: 1 Date: 20 November 2012

Reviewer: Lise Hestbaek

Reviewer's report:

I am very pleased with this manuscript. The authors have done a very thorough job in summing up the literature in this area. Furthermore, the manuscript is well written and appears clear and understandable. I only have a few comments below, which I believe are easily attended to.

1. Is the question posed by the authors well defined? Yes
2. Are the methods appropriate and well described? Yes
3. Are the data sound? Yes
4. Does the manuscript adhere to the relevant standards for reporting and data deposition? Yes
5. Are the discussion and conclusions well balanced and adequately supported by the data? Almost – see review
6. Are limitations of the work clearly stated? Yes
7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? Yes
8. Do the title and abstract accurately convey what has been found? Yes
9. Is the writing acceptable? Yes

- Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)

1. All the references in the background section are rather old. There are newer references that can be used.
2. Reverse the order of Tables 3 and 4, so they appear consecutively and the effect of the moderator variables appear together.
3. One week does not equal 0.25 months as written throughout the manuscript. To be exact, you should write one and two weeks rather than 0.25 and 0.50 month.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Method section:
4. I would like a better explanation of the selection process. Why wasn’t Ebase
used? 32 articles were identified from other sources (fig 1) – which? “Electronic specialized journals” as written in the 3rd paragraph does not explain sufficiently.

5. Was the first screening based on titles or abstract and what were the exact inclusion and exclusion criteria?

Results section:

6. I don’t understand the last sentence in the 9th paragraph: “96.6% of the studies used recall periods that were clearly stated by means of questions such as the frequency, duration of intensity, and character of the pain”

Discussion:

7. A little more than two pages is used for summary of results. This includes details which are not reported in the result-section. I think this should be moved to results and the summary in the discussion kept to a minimum (one paragraph).

8. You could make a comment about recall. I find it noteworthy that the one month prevalence is so similar to lifetime prevalence.

- Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

9. Methods: The explanatory model, which is reported in the result section, is not described in the method section.

10. Discussion: Under “Implications for Clinical Practice” you write that the higher prevalence rates in recent studies suggest that the problem is increasing. However, recent studies also have higher quality scores and you report that higher quality studies are associated with higher prevalence rates as well. Therefore, I don’t think you can make the assumption about increasing prevalence rates, since it might be due to better studies.

**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