Author's response to reviews

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

Authors:

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Version: 2 Date: 6 December 2012

Author's response to reviews:

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 Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?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 Embase 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

Reviewer's report
Title: Prevalence of low back pain in children and adolescents: a meta-analysis
Version: 1 Date: 7 November 2012
Reviewer: Mark Hancock

Reviewer's report:
This paper answers an important question, the study has been performed well and the manuscript is quite easy to follow. I have only a few suggestions for improvement.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
1. The conclusion in the abstract and at the end of the paper says “children have lower lifetime prevalence rates than adolescents”. I don’t think this is a good summary of the paper or the results presented. This sounds like a direct comparison between children and adolescents was both an aim of the study and performed as an analysis. This should be changed.
2. As a related point make it clear in the introduction that the aim was to investigate children and adolescents together not separately.
3. Consider referring to 1 week rather than 0.25 months in abstract and throughout.
4. Page 5 line 3: change lesser to less.
6. Page 7: Much of what is presented in the first half of the page should be in the results not the methods.
7. Provide more detail of the search process especially how many people were involved in screening for inclusion and how this was performed.
8. Page 13: the sentence ending in “validated or at least tested for reproducibility does not read well. This seems like it needs to be 2 separate sentences.
9. The authors included cross sectional and longitudinal studies but it seems only reported on cross sectional data. Would prospective longitudinal studies provide better estimates for period prevalence? Did some studies report this?
10. Page 18: the term delimitation of pain is used for the first time. Why is this not
defined in the methods, I found this confusing.

11. The authors interestingly found higher prevalence rates in more recent studies. Can they speculate about if this is a real change, a reporting issue or other?

12. I think the section on implications for practice is a weakness of the paper. I suggest re-writing this.

13. Re-word the first sentence in implications for future research to: Our results enable us to make recommendations for …

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