Author's response to reviews

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

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Author's response to reviews: see over
The Biomed Central Editorial Team,


Thank you for consideration of our manuscript for publication in your journal.

We have reviewed the above manuscript according to your reviewer’s comments.

Reviewer # 1 (Mark Hancock)

MINOR ESSENTIAL REVISIONS:

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.

We agree with the Reviewer in this point. So, we have deleted this conclusion both in the Abstract (p. 3) and in the last paragraph of the Discussion section (p. 20).

2. As a related point, make it clear in the introduction that the aim was to investigate children and adolescents together not separately.

To make clear this point, we have modified a sentence in the Introduction, in the following terms (p. 5):

“Thus, the main purpose of this research was to systematically review the prevalence of LBP obtained in studies with samples composed by children and/or adolescents.”

3. Consider referring to 1 week rather than 0.25 months in abstract and throughout

Done (see pp. 2, 9, 14, 35).

4. Page 5 line 3: change lesser to less

Done (see p. 5).

5. Page 6. Justify decision to limit studies to after 1980

We have added this sentence to justify our search period (p. 6):
“(c) studies had to be carried out between 1980 and April of 2011, as before 1980 there were not found studies that report LBP prevalence rates for children/adolescents.”

6. Page 7: Much of what is presented in the first half of the page should be in the results not the methods.

Following this recommendation, we have moved this paragraph from the Method to the Results section (p. 12):

“The sample size distribution was very skewed, with a median of 622 subjects and minimum and maximum values of 88 [19] and 34,423 [27], respectively. Most of the studies were carried out in Europe (42 studies; 73%), followed by North America (6 studies; 10.2%), Oceania and Asia with four studies each (6.8%), and Africa with two studies (3.4%). Only three of the 59 studies had not been published. Fifty-five studies were written in English, three in Portuguese, and one in Spanish.”

7. Provide more detail of the search process especially how many people were involved in screening for inclusion and how this was performed.

Done. In pp. 6-7 we have added this paragraph:

“Secondly, several electronic specialized journals were consulted such as, Spine, Spine Journal, Pain, European Spine Journal, Scandinavian J Public Health, European Journal of Public Health, and, finally, the reference lists of the studies recovered were also consulted.

Two reviewers independently: (a) screened the title and abstract of each reference to locate potentially relevant studies, and once hardcopies of the screened papers were obtained, (b) reviewed them in detail to identify articles that fulfilled the selection criteria.”

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.

We have modified this sentence and have changed reliability for reproducibility, in order to make easier its interpretation (p. 13):

“To collect the data, the studies used different instruments, such as questionnaires (79.7%), interviews (8.5%), and examinations (11.9%). The instruments had to be validated or at least its reliability had to be tested.”

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?

In order to not confuse prevalence rates with incidence rates, in longitudinal studies we only recorded the prevalence rate at the beginning of the study. In order to make clear this point, we have added the following sentence in the ‘Prevalence rates’ section (p. 9):

“When a study applied a longitudinal design, only the prevalence rate at the beginning of the study was recorded in our meta-analysis.”
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.

In the Methods section we refer to the delimitation of pain when we speak about how the methodological quality of the studies was appraised. In particular, in this sentence (p. 8):

“(g) the methodological quality of the study, assessed with an instrument used in previous systematic reviews on the prevalence of LBP [29-31]. This instrument contains 12 items grouped in three clusters focusing on the sample representativeness, the quality of the data collected, and the clarity of the definition of LBP, see Additional File 1.”

In the Additional File 1, the item 10 specifies how the delimitation of pain was coded. In order to not extend the paper, we think that it is not needed to make a more concrete reference to this point in the Methods section.

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?

We found a positive, statistically significant relationship of the publication year with the lifetime prevalence. We consider this result a very solid one, as publication year remained being statistically related to lifetime prevalence in the multiple meta-regression model. In order to make clear this point, we have added the following sentence in the Discussion section (p. 19):

“This result seems to be very solid, as in the multiple meta-regression model publication year was one of the two predictors that achieved a statistically significant relationship with the lifetime prevalence, once controlled the methodological quality of the studies, the delimitation of pain, and the mean age of the sample.”

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

We have tried to improve this section. The next paragraph summarizes the clinical implications of our results:

“The results of our meta-analysis have important consequences for professionals. Our finding of higher prevalence rates in the most recent studies suggests that LBP is a problem that is increasing in childhood and adolescence. As a consequence, more attention should be devoted to develop and apply prevention programs for young children in order to break this trend. On the other hand, our finding of higher prevalence rates in the studies with older subjects points to the need for efforts towards an early detection of LBP in children and adolescents. Finally, it is also important to monitor children with LBP to avoid recurrences that can increase the prevalence in adolescence.”

13. Re-word the first sentence in implications for future research to: Our results enable us to make recommendations for …
Done (p. 21).

**Reviewer 2 # (Lise Hestbaek)**

**DISCRETIONARY REVISIONS:**

1. *All the references in the background section are rather old. There are newer references that can be used.*

Done (see pp. 4 and 5).

2. *Reverse the order of Tables 3 and 4, so they appear consecutively and the effect of the moderator variables appear together.*

Done.

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.*

Done.

**MINOR ESSENTIAL REVISIONS:**

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.*

On pp. 6-7 there has been added a more detailed description of the sources consulted. With regards to not using Embase in the search strategy, we considered that this database will offer redundant information to that reported by the five electronic databases that we used in our search strategy (ISI Web of Knowledge, MedLine, PEDro, IME, LILACS, and CINAHL).

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

This question is similar to point 7 of Reviewer 1. With the new text that we have added in p. 7 we have addressed the points of the two Reviewers.

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”.*

We agree with the Reviewer in that this sentence is not correct. So, we have changed it and now the sentence is (p. 13):

“A further useful specification of the definition of LBP was applied in 52.5% of the studies by means of questions such as the frequency, duration or intensity, and character of the pain. 96.6% of the studies used recall periods that were clearly stated (1 and 2 weeks, 1, 3, 6 and 12 months or lifetime).”
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).

In the two first paragraphs of the Discussion we summarized the purpose and the methodology applied in our study. As this information is redundant with that presented in previous sections of the paper, we have deleted it from the Discussion. But the remaining paragraphs of the Discussion are dedicated to put our results in relation to those obtained in previous research. So, we have maintained this information in this section, as it constitutes a key element in our investigation. These are the paragraphs that we have deleted from the Discussion (p. 17-18):

“The target populations represented in the studies were children and adolescents (6 – 18 years old). When a study included adolescents and participants that were over 18 years old, only the prevalence rates for participants under 19 years old were recorded in the meta-analysis. The studies included in the meta-analysis were carried out in 24 countries and five continents. Both longitudinal and cross-sectional studies were included in the meta-analysis. As our purpose was to obtain prevalence rates, in longitudinal studies data from the first time point were used. The majority of the studies used self-report measures of LBP. The main recording instrument used in the studies was the questionnaire. For example, Balagué et al [42,43] used questionnaires that were answered at home by the parents of the younger children (8-12 years of age) while the older children answered them at school.”

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

This comment of the Reviewer is somewhat confusing for us, as the one month and lifetime mean prevalence rates were 0.183 and 0.339, respectively, and it is clear that they are very different among themselves.

MAJOR COMPULSORY REVISIONS:

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

Done. We have added the following text (pp. 10-11):

“In order to find the subset of moderator variables that can explain most of the prevalence rates variability, a multiple meta-regression model (by assuming a mixed-effects model) was adjusted. The moderator variables included in the model were selected taking into account the statistical significance achieved in the previous bivariate analyses. This regression model allowed us to identify the most relevant study characteristics to explain the variability exhibited by the prevalence rates.”

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.

Our conclusion about the increase of the prevalence rates in the most recent years is based on the results of the multiple meta-regression model (‘An explanatory model’ section). In particular, we found that, once controlled the methodological quality of the studies, the delimitation of pain, and the mean age of the samples, the publication year still maintained a statistically significant relationship with the lifetime prevalence rates. If the relationship between publication year and lifetime prevalence were explained by the confounding role of methodological quality of the studies and the delimitation of pain, then publication year should not have reached the statistical significance in the multiple meta-regression model. This is the reason why we consider that this is a solid relationship. In fact, we have added a paragraph to make more explicit this result (see p. 19, ‘Discussion’ section, and our response to point 11 of Reviewer 1).