Author's response to reviews

Title: Physical therapy treatments for low back pain in children and adolescents: a meta-analysis

Authors:

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Author’s response to reviews: See over
Dear Editor,

We are very grateful for tentatively accepting our manuscript MS: 9918092448819109 for its publication in *BMC Musculoskeletal Disorders*. My co-authors and I greatly appreciate the time and effort that you and the reviewers put into the review of our manuscript. We have revised our paper, taking into account the useful recommendations made by one of the reviewers. His suggestions have improved the quality of our paper.

In the following paragraphs, all of the suggestions from the reviewer are addressed, including specifications of the changes that we have made in the text.

Thank you for considering the revised manuscript and I look forward to hearing from you.

Sincerely,

Inmaculada Calvo-Muñoz, PT
Antonia Gómez-Conesa, PT, Ph.D.
Julio Sánchez-Meca, Ph.D.

**Reviewer # 2 (Wolfgang Viechtbauer)**

**Minor Essential Revisions:**

*p. 4: I would replace "has been proven" with "has been demonstrated" (proven is a bit strong).*

Done.

*p. 4: I am not sure I understand what the authors are trying to say with: "In any case, the impact of LBP in terms of quality of life is far from being a major problem [1]." This sentence implies that LBP is *not* a major problem, but I believe the authors are trying to make the point that it is.*

We agree with the reviewer in that this sentence is not the best way to close this paragraph. So, we have deleted it and have added this one in order to make clearer our position:

“Although about 33.6%-56% of adolescents with LBP have limitations for some activities, Pellise et al [1] found that 9 out of every 10 adolescents reporting LBP can be considered healthy, while in a 10% of them LBP can be considered as a symptom of a multidimensional health problem.”

*p. 10: As the correlation between pre- and post-test scores was not reported, the authors assumed a value of 0.5 for all studies. Ideally, one should then conduct a*
sensitivity analysis to ensure that the results are robust when assuming other values (e.g., 0.2 or 0.8). Given the results, I would not expect any of the conclusions to change, but it would be nice to check.

We have carried out this sensitivity analysis. As a consequence, we have added this sentence to the section ‘The effect size’ (p. 10):

“In order to check whether the value of the correlation coefficient can affect the meta-analytic results, a sensitivity analysis was carried out consisting of calculating the sampling variances of the effect sizes by assuming \( r \) values of 0.2 and 0.8.”

As expected, the meta-analytic results were not affected by changes in the correlation coefficient. Thus, we have added this sentence to inform about the results of this sensitivity analysis (p. 15):

“The statistical analyses presented in Table 3 were obtained by assuming a pretest-posttest correlation coefficient of \( r = 0.5 \) in all of the studies. In order to check whether the value of \( r \) can affect the ANOVA results, these analyses were repeated twice: assuming \( r = 0.2 \) and \( r = 0.8 \). The results exhibited negligible differences in comparison to those obtained for \( r = 0.5 \). Another sensitivity analysis consisted in repeating the ANOVAs using the REML estimator of the residual heterogeneity variance in place of that based on the method of moments. This change did not affect the meta-analytic results. The ANOVA results obtained for \( r = 0.2 \) and 0.8 as well as those for the REML variance estimator are not presented in this paper, but they can be obtained from the corresponding author upon request.”

p. 11: What estimator did the authors use for the amount of (residual) heterogeneity?

We used the DerSimonian and Laird estimator based on the method of moments. There are other estimators of the residual heterogeneity variance and its selection might affect the meta-analytic results. Out of the different estimators proposed in the literature, one based on restricted maximum likelihood (REML) seems to offer good properties. In order to check whether the selection of the heterogeneity variance estimator can affect the results, we carried out a sensitivity analysis consisting of repeating the analyses with the REML heterogeneity variance estimator. Thus, we have added the following sentence in the section ‘Statistical analysis’ (p. 12):

“The residual heterogeneity variance was estimated by the method of moments proposed by DerSimonian and Laird. There are other heterogeneity variance estimators proposed in the literature. In order to check whether the selection of the variance estimator can affect the meta-analytic results, the analyses were repeated by using a variance estimator based on the restricted maximum likelihood (REML) method.”

As expected, the results were not affected by the variance estimator. The results of this sensitivity analysis have been explained in the same paragraph than those for \( r = 0.2 \) and 0.8 (see above).

p. 14: Please remove the extra "time" in: "regards to time, context, time, etc.". I would replace "no studies carried out reliability analysis" with "none of the studies carried out reliability analyses".
p. 19: I would replace "and intent-to-treat analysis included" with "and intent-to-treat analyses should be carried out".

Done.

p. 19: I think "is" is a bit strong in: "and manual therapy is the most effective". Maybe change this to: "and manual therapy appears to be the most effective" and then start the next sentence with "However".

Done.

Table 1: Under "Objective" for Fanucchi et al., 2009 [31], change "To investigate whether that exercise" to "To investigate whether exercise".

Done.

Table 1: Under "Objective" for Jones et al., 2007 [29], change "as an treatment" to "as a treatment".

Done.

Table 3: It is not quite clear to me what the "overall" outcomes represent. Are these based on first combining multiple standardized mean changes within a single group? Or are these based on a single overall improvement measure within each group?

To make clear this point, we have added this sentence in the section ‘The effect size’ (p. 11):

“In addition, an overall effect size was calculated in each single group by averaging the effect sizes for the different outcome measures reported in the study.”

Table 4: Based on the data shown in Figure 1, I can reproduce the results in Table 3 (for pain) exactly. However, when I try to do the same for the results in Table 4 (for pain), I get a value for $d^+ = 1.03$ (which is close), but 95% confidence interval bounds 0.62 and 1.43 (which are quite a bit different from what is reported in Table 4 and which can't be due to rounding errors). I also find $I^2 = 33.33\%$. However, if I compute the standard errors of the standardized mean differences (i.e., the square root of the sampling variances), but then (incorrectly) treat those values as if they were the sampling variances in the analysis, then I get $d^+ = 1.02$ with CI bounds .48 and 1.56, which matches the authors results (and now $I^2 = 0\%$). Therefore, I suspect that the results in Table 4 are not quite correct.

The reviewer is right. We have made a mistake in the calculations in table 4. So, we have repeated the analyses and modified Table 4 (see p. 30). Now, the width of the confidence intervals is smaller than that of the previous ones, but the interpretation of
the results did not change, but the mean effect sizes remained very similar. We have also made the corresponding changes in p. 16.

Table 4: Change "were nonstatistically significant" to "were not statistically significant".

Done.

Figure 1: If possible, it would be nice to adjust the figure so that the lower CI bound for Harringe et al. (2007) does not overlap with the text.

Done.