Author's response to reviews

Title: Thoracic spine pain in the general population: Prevalence, incidence and associated factors in children, adolescents and adults. A systematic review

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Author's response to reviews: see over
Dr Melissa Norton MD
The Editor in Chief
*BMC Musculoskeletal Disorders*

Re: manuscript 1408016664256083

Dear Dr Norton

Thank you for arranging peer-review of the above manuscript. We have considered the insightful comments prepared by the two expert reviewers and have addressed the issues raised in a point by point format below. Revisions to the manuscript are marked with ‘track changes’. A clean version of the manuscript without ‘track changes’ has also been uploaded.

I hope these revisions are to your satisfaction and enable publication of this paper in BMC Musculoskeletal Disorders.

Sincerely

Andrew Briggs
Chief Author.
Reviewer 1: Karen Grimmer-Somers

1. Pge 5: Line2: The evidence that the incidence of spinal pain among otherwise healthy adolescents is increasing is not clear cut. The collection of data on spinal pain amongst adolescents gained prominence from the 1980’s and therefore I believe, it is still too early to draw conclusions. Hakala et al’s (2002) study was focussed on questionnaire surveys carried out from 1985 through to 2001 in Nordic countries (Norway, Finland, Sweden, Holland). This study identified an increase in prevalence of reported spinal pain in adolescents from 1993/94, to 2001. Unfortunately similar epidemiological studies have not been undertaken on adolescent spinal in other countries to identify if this trend in reported pain is an international phenomenon. Alternatively it may be that the increased reporting of spinal pain in adolescents is a result of a more health conscious population (Sweeting and West 1998). I would say the evidence ‘suggests’ an increasing incidence, but it is by no means clear at present.

The reviewer raises an important point for clarification. The sentence has been revised as suggested.

2. Pge 6: Line 1,2: “that TSP is prevalent among healthy individuals and does impact on function” Please reference.

Suitable references have been added.

3. Pge 8: Line 15 to 17: I would suggest that this sentence be deconstructed for clarity. i.e. it currently suggests that cross sectional studies are an appropriate study design for investigating risk factors. This is obviously incorrect, but I think it just needs to be reworded to clarify this.

The sentence has been revised to improve clarity.

4. Pge 13: Line 5 – 10. I am concerned that whilst appropriate population characteristics were defined in the inclusion criteria (i.e. Pge 8:Line 1-7) to ensure no specific athletic/occupational groups etc., a number of the studies involved back pain specifically related to an activity i.e. carrying a backpack (12.1%) or pain after work (3%). The fact that the backpack study led to a point prevalence of 72% suggests that this group may not be typical of the community population. I would remove these from the study.

Although the reviewer raises an insightful issue, we have elected not to remove these studies. The primary aim of this review was to report epidemiologic characteristics of TSP in the general population. In a separate review we have reported prevalence by occupational group [1]. We feel that all studies meeting our operational definition of “general population” should be included in the review; consistent with the principles of a systematic review. We acknowledge the reviewer’s concern that some prevalence data may be influenced by the operational definition of TSP used in the various included studies. For this reason, we have extensively discussed the issue of variability of operational definitions in the Discussion and have presented the prevalence/incidence data for TSP by operational definition (Table 2) in order to explicitly demonstrate how TSP prevalence may vary according to the definition used. Moreover, the description of results on page 13 pertains to the definitions used in the studies; not the population. In the two examples cited by the reviewer, ‘backpack use’
can be considered an activity performed generally by children, rather than an occupation, and ‘work’ refers to an occupation in the general sense; this does not mean that the study focused on a specific occupational group. In the case of backpack use particularly, removing these 4 studies [2-5] from the review would significantly decrease the breadth of coverage of the review in term of prevalence and correlates of TSP in the younger population. In both circumstances, the populations reported were still part of the general population and the variability of prevalence data reflects differences in definitions of TSP used in the source papers, rather than differences in the populations relative to other studies included in the review.

5. Pge 14: Line 19-20. The description of TSP as a discrete and important clinical condition is, I feel, difficult to make without consideration of how the reported TSP was related to other spinal reports. If the prevalence and incidence of TSP was always or very highly associated with LBP or Cervical pain then its importance as a discrete clinical condition is questionable.

This sentence has been rephrased considering the point raised by the reviewer.

Reviewer 2: Sita M Bierma-Zeinstra

Thoracic spine pain has received much less attention compared to the lower back pain. The review gives a nice overview about the prevalence of thoracic spine pain for the different definitions and the different age categories. I have however some comments about the clarity of your manuscript.

Major compulsory revisions:
1. You describe the prevalence of thoracic pain in the abstract page 2 lines 23-page 3 line 3, but you do not distinguish between age and definition used. To describe your results transparent you have to make this distinction. Readers can get the wrong impression from the abstract. Please rewrite the results in the abstract and make a distinction between definition used and age.

We have revised the Abstract to reflect the reviewer’s recommendation within the 350 word limit for BMC Musculoskeletal Disorders.

2. To criticize the quality of the different studies you use the Critical Review Form-Quantitative studies. This appraisal tool has the advantage that it can be used for a variety of study designs, but not all the items evaluate the internal validity of the study. Is this really the best tool to assess the quality of the studies?

The question of the ‘best’ critical appraisal tool to use for observational studies is a matter of judgment, as no gold standard tools exist in this area [6]. In this review, the Critical Review Form was judged to be an appropriate tool based upon the justifications outlined on page 10 (lines 5 – 17). The fact that some items of the tool did not focus on internal validity is considered by the authors to be a strength, rather than a weakness of the tool, as the issues not pertaining to the actual conduct of the study (for example, whether clinical importance was reported) play an important role in determining the relevance of a study to related populations. Moreover, this tool has been used in other systematic reviews for the same purpose [7, 8], while similar tools have been used in other systematic reviews [9, 10].

3. You describe the Critical Review Form very extensively, but you do not use the score to evaluate the results of the articles. You only use this score in a few lines
of the discussion, page 19. The same goes for the NHMRC Evidence Hierarchy. Maybe you can use this tool to evaluate the results (sensitivity analysis for study quality), or you should describe the tools less extensive.

This is an important point and a critical element of a thorough systematic review. We have performed a sensitivity analysis using the study quality score. We performed this sensitivity analysis for both prevalence (page 17, paragraph 1) and incidence (page 19, paragraph 3) data. The reviewer refers to the incidence analysis and not the prevalence analysis. We did not perform a sensitivity analysis using the NHMRC Hierarchy of Evidence rank since the vast majority of studies were ranked as level IV, and therefore stratifying a sensitivity analysis by rank score would not be appropriate.

This issue concerning a sensitivity analysis by NHMRC rank has been added to the Discussion for clarification.

4. Table 2 is the main table in your article. It should be the table in which you can see a clear overview of the results, but it is not. The results of 1 article are sometimes described in more then 4 lines. Please re-arrange table 2. Some suggestions:
- Maybe you can arrange the table by author and describe the definition used in the article.
- If you describe the definition in a column of the table, you do not need subheadings in the table for the different definitions. Only the definitions ‘any backpain’ and ‘pain associated with backpain use’ are used in multiple articles, maybe you can put these articles among each other.
- At the end of each section you give a range of the prevalence. I think that this range is redundant.

These are useful suggestions and the authors had previously spent some time discussing the most appropriate format for the Table. We suggest that the Table in its current format is still the optimal way to present the data. We feel that presenting the prevalence/incidence data by author may be misleading as it does as clearly inform the reader about the variability of TSP by operational definition. The authors feel that this issue is critical to the interpretation of the prevalence/incidence data reported. Similarly, presenting by author does not as clearly demonstrate the variability of TSP prevalence with age. Finally, the purpose of Table 1 is to clearly describe study and cohort characteristics by study (ie by author).

The reviewer raises a good suggestion about the range presented at the end of each TSP definition category. The range data have been removed from the Table.

5. I have the same comment on table 3. You describe the results of 1 article in many lines, which makes the results unclear for the reader. Further, in table 3 you should describe whether the outcomes were adjusted or non-adjusted for major confounders and whether the relationship is positive or negative.

Consistent with our response to point 4, we feel that the associated and risk factors for TSP are most clearly presented by factor type, rather than by study design. Many studies report more than one factor (either risk or correlate) and therefore
interpretation of the overall trends and strengths of association between TSP and factors would become more difficult to interpret if this Table was formatted by author rather than by factor domain. Moreover, we feel that the presentation of data in the current format has more clinical meaning than if presented by author.

Some of the regression models used in source papers were adjusted for confounding variables. These adjustments are noted at the foot of Table 3.

The direction of the relationship between TSP and the factor is inherent in the size of the odds ratio (OR). An OR > 1 refers to a positive association with TSP, while an OR < 1 refers to a negative association. Where correlation coefficients are reported, positive values represent a positive association with TSP, while negative values represent a negative association. For statistical tests which do not test for association, a significant outcome (p<0.05) refers to the factor being greater among individuals with TSP.

A description of interpretation for outcomes of statistical tests has been added to the title of Table 3.

6. The design of figure 2 is good. It is nice to see the prevalences according to age-range. However, it is a pity that you cannot see which author belongs to the result. Is there a possibility to do so?

The reviewer raises an interesting possibility. Although possible, we elected not to add reference field codes to each symbol in the figure as we feel this would overly complicate the panels and make interpretation of the prevalence data more difficult for the reader. We feel with relative ease that the reader may cross-reference the figure with Table 2 in order to determine the source paper for a particular data-point.

7. Within this figure 3, the figure of the 3 month prevalence does not add much, because it is only one article. The same goes for the figure of lifetime prevalence. I recommend to delete these two figures.

The reviewer is correct in that a limited number of studies contribute to the 3 month and lifetime prevalence panels in Figure 2. Although these panels could be removed from the Figure, we feel that for completeness they should remain. Being able to visually identify each period prevalence may assist the reader with interpretation and highlight the most and least common period prevalences reported in the literature.

8. In figure 2 you can see that 1 month prevalence is much higher in the younger age ranges, but 1 year prevalence is higher in the older age ranges. Do you have an explanation for this result? You should discuss this issue.

The likely reason for the one month prevalence of TSP to be greater in youth compared to older age (and the reverse for 1 year prevalence) is due to the definitions of pain used in the various studies contributing to one month data. Data for one month prevalence of TSP in youth was sourced from four studies [11-14] while only one study was available for adult data [15]. The operational definition for TSP in the adult study was “frequent pain in the upper back” compared to “any pain” or “pain duration ≥1 day” in the youth studies. Fewer adults were likely to report frequent pain as compared to definitions that were unrelated to pain frequency. Similarly, for
the one year prevalence, six adult studies contributed to the data and reported a definition of “any pain” [16-19], “pain duration for \( \geq 1 \) week” [20] or “pain after work” [21], while one youth study reported a definition of “pain interfering with school or leisure” [22]. The lower prevalence in the youth study was likely related to a definition of pain which needed to be associated with a functional impairment.

This information has been added to the Discussion.

Minor Essential Revisions
9. Reference number 56 should be changed in Musculoskeletal pain in Malaysia: a COPCORD survey.

This reference has been changed.

References


