Reviewer's report

Title: Time trends in single versus concomitant neck and back pain in Finnish adolescents: results from national cross-sectional surveys from 1991 to 2011

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Reviewer: Steven Kamper

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This study uses survey data to describe the prevalence of self-reported back and neck pain in adolescents over a 20 year period. The manuscript is generally clearly written but would benefit from some additions. The Discussion section fails to put the findings in the context of previously published literature and needs better explanation of the limitations, particularly potential bias introduced by the low response rates.

Major compulsory revisions

Introduction

1. The justification for the study appears to be based on an hypothesis regarding the aetiology of separate and concomitant back and neck pain. It strikes me that this study is a rather indirect investigation of this theory. This is not say that the link is untenable and this would represent a worthwhile point in the Discussion section, but I think at present it receives too great a focus in the paper.

2. I would then recommend some thought be given to better outlining the justification for the study/research question in the Introduction.

Methods

3. I would question why the surveys from 1993, 1995 and 1997 were not used for this study? If the goal is to describe the trends then it would seem such information is valuable, particularly given that the trends are not necessarily extremely clear for each strata. This is important because the conclusions rest very strongly on the figures reported from 1991, additional information from years proximal to this would increase confidence in the findings. Further, it seems that there were often changes between 1991 and 1999 so exploration of this is important.

4. Can the authors confirm that surveys used exactly the same sampling methods and questions for all the included years? As mentioned above there seems often a difference between 1991 and 1999 and it is important to establish this is likely to be a ‘real’ time effect or a measurement issue.

5. I have little confidence that the method used for analysis of non-responders is of much value. Why would the authors assume that people who needed a reminder were more like non-responders? It would seem much more likely that they are more like (and actually are) responders. It is of little surprise that no difference was observed.
6. More information regarding the analysis of linearity of trends needs to be provided.

Results

7. The Results section focuses on change that occurred during the 90s and change that occurred during the 2000s. That being the I would suggest odds ratios that compared prevalence in 1999 to 1991, and prevalence in 2011 to 1999 would be more informative that ORs for the comparison 2011 to 1991. The same goes for the tests of linear trends, and again points towards the value of include data from the other surveys in the 1990s.

Discussion

8. The response rates are very low for some of the surveys, as low as 31%, and as worryingly, quite different over the different years. This has a number of implications for the study which are insufficiently addressed n the Discussion. When considered in addition to the apparently poor reliability of the questions (kappa 0.56), legitimate questions can be asked about the validity of the prevalence estimates.

9. Following from the point above, the OR results hang quite heavily on the reliability of the prevalence figures from 2011, the point at which response rates are lowest and consequently at most at risk from bias.

10. The authors need to interpret the results in the context of other studies that have been published. The single paragraph that essentially says nothing else is comparable is inadequate.

Minor essential revisions
Nil

Discretionary revisions
Nil

Level of interest: An article of limited interest

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
None