Reviewer’s report

Title: Prevalence of diagnosis and direct treatment costs of back disorders in 644,773 children and youths in Germany

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Reviewer: Nicholas Henschke

Reviewer’s report:

The authors have presented a descriptive analysis of a very large population-based data set and reported the prevalence of low back pain related diagnoses and their costs in Germany. While the objectives of this study are worthy of research and the size of the data set used is very impressive, I have a number of points which I hope can be addressed to make this manuscript even better.

Major Compulsory Revisions

While the methods are described well and appear to be appropriate to answer the objectives, there are a number of details regarding the data set which I think would be important for readers and should be included.

1. It is not clear in which health care setting the diagnoses based on ICD codes were made. As the cost data included inpatient and outpatient costs, presumably the diagnoses were provided by a mixture of doctors working in primary and secondary care. Please elaborate on this point, as the prevalence of different diagnoses would differ based on health setting.

2. How was the definition of “youths” chosen? Generally in the low back pain literature persons aged > 18 years are considered adults. This may prove confusing for some readers as they may not consider persons aged 18-24 to be “youths” and therefore should not be compared to “children”.

3. For the cost analysis, it is described that only where the relevant musculoskeletal diagnosis was found to be the only diagnosis (singular diagnosis), were patients included in the analysis. Was this also the case when determining the prevalence of the diagnoses? A large proportion of adult low back pain sufferers are known to have multiple complaints and this could also be the case in children and adolescents. A clear explanation of whether this was the case is needed to gauge the generalizability of the study.

4. Were there many instances of missing data? How was this handled in the analysis?

The discussion and conclusions are generally well balanced and supported by the data, however, the implications of the results need to consider the limitations of the study, which could be quite substantial.

5. The main limitation of the study appears to be the diagnosis by ICD codes.
While the low reliability of coding and diagnosis with the ICD system are mentioned in the discussion, these do not appear to be incorporated in the conclusions. Without information on the accuracy of ICD diagnosis in children and adolescents for low back pain related diseases, the prevalence rates reported must be considered with caution. More discussion on these limitations may provide more balanced conclusions which are supported by the available data.

6. As an example on the above point, the conclusion that: “we may conclude that doctors’ diagnoses of cases of scoliosis are in large parts those of cases with an angle over 10° as compared to the self-reported cases of scoliosis” is not supported at all by the data analysed and is only an assumption based on comparison with selected literature. Please revise.

7. In paragraph 7 of the discussion, “Apart from that, disorders like scoliosis are well-defined and therefore not as prone to misclassification as other, more vaguely defined disorders” this statement is not supported by the data in the analysed dataset, or by references. Please provide support for this statement or remove it from the discussion.

8. There are repeated statements in the discussion and conclusion (e.g. first line of conclusion) that this study provides information on the health care utilization of children and youths. This is incorrect as only information on the prevalence of diagnoses and costs have been reported. There is no information (apart from assumed health care use) which supports this. Please revise.

9. Extrapolating the results to health care utilization using data from a separate study (Roth-Isigkeit 2004) must be performed with caution as no details regarding the study population of the earlier study are provided. This also appears to be the study with the highest proportion of children seeking care (compared to the others described in the introduction) and may not accurately reflect care seeking behaviour.

10. The conclusion that “our results again stress the importance of prevention strategies for musculoskeletal disorders even at an early age” is not supported by the data and should be reconsidered. This study does not provide any reason to think prevention strategies at an early age would change diagnoses or costs.

11. The authors should consider the implications of this study on future research and propose directions which could improve descriptive population based studies. While the direct clinical implications may be small, improving the collection and analyses of these large data sets is warranted and important. With such a large data set indeed being the major strength of this study, many readers would welcome suggestions to improve such studies in the field of low back pain.

Minor Essential Revisions

In general this manuscript is readable, but in parts is informal and metaphorical. I would consider revising the manuscript carefully to ensure the written English is of an academic standard.

1. For example, in the introduction the phrase “And while it has been known for a while now...” is slightly informal and assumptive.
2. Similarly, in the conclusion “In a nutshell,” is too informal and not an adequate reflection of what was found.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

**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.