Answer to the Letter to the Editor of F. M. Kovacs et al. entitled “Overviews should meet the methodological standards of systematic reviews” concerning “The evidence on surgical interventions for low back disorders, an overview of systematic reviews” by Wilco C.H. Jacobs et al.; Eur Spine J, doi:10.1007/s00586-013-2823-4

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An overview is not a review

We thank the authors for the interest they show for our systematic overview of the evidence on surgical interventions for low back disorders. We appreciate their critical

remarks on our study and we would like to elaborate on the issues raised.

First, the authors state that we overlooked reviews complying with the stated inclusion criteria. The referenced study of Urrutia et al. [3] was in our search results, but could not meet our inclusion criterion of extensive search. Urrutia et al. [3] used only five search terms without obvious variations, entered as text word ([tw]) only, which resulted in only 182 hits in two databases while there was no check on the cross-references. On replication of this search we could for example not retrace the study of Bogduk and Karasek [1] in this search. This might be explained by the slightly different terminology by Bogduk and Karasek (anuloplasty versus annuloplasty; IDETA versus IDET). The authors might have had knowledge of this study in other ways or used the search string slightly different, but this illustrates the low sensitivity of the search.

Second, the authors state that we did not distinguish between “quality of reporting” and “quality of the systematic review”. On the contrary, we evaluated only the quality of the included systematic reviews with the Amstar score. The quality of reporting is important, but for our purpose, evaluating the existing evidence, the right approach is to assess the quality of the methodology of the reviews.

Third, we need to disagree with the authors that both systematic reviews and overviews need to follow the same strict methodology. We believe that it is even more important for overviews to use accepted and evidence based methods because of the impact and the possible use of this evidence in guidelines or directly by care providers.

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Unfortunately these methodologies have not been developed and so far we have to rely on the methodology that was developed for systematic reviews. While these appear valid, there are many aspects that do differ between the two study designs. Interesting aspects concerning the methodology of overviews are outdated reviews, indirect comparisons, overlap of included studies, and different selection criteria. We agree with the authors that contacting authors could be a valuable addition to resolve inconsistencies as we have experienced that even in high quality systematic reviews these can still exist. However, the methodology of this effort, the associated effect on the risk of bias, and the peer-reviewed status of the information obtained are not yet clear.

Fourth, we are aware of the arguments put forward by Kovacs et al. [2] (page E1349, first two paragraphs) for having contradictory statements in their data presentation. Their main argument for rating compliance as “Yes” and blinding as “not applicable” is that meeting these quality criteria is difficult in surgical studies. We strongly oppose to such reasoning. The scientific community does a better job in accepting that randomized studies in surgical interventions have these limitations and explore the value of

other methodologies rather than over-qualifying the value of the randomized controlled trial by unjustly upgrading their quality.

In conclusion we believe we have, with the best available methodology, produced a comprehensive overview of the evidence on the surgical interventions for low back disorders. We concur with the authors that more work need to be done on evidence-based methodology for systematic overviews of the literature.

Conflict of interest None.

References

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