

RESPONSE TO: “SPINAL MANIPULATIVE THERAPY FOR ADOLESCENT IDIOPATHIC SCOLIOSIS: A SYSTEMATIC REVIEW”



To the Editor:

We read with interest the recently published systematic review on the effects of spinal manipulation for adolescent idiopathic scoliosis.¹ There appears to be a discrepancy between the risk of bias assessment text and Table 2 regarding Rowe et al.² The text appears mostly correct, but the table appears mostly incorrect.

In the text, the authors state that in the randomized clinical trial by Rowe et al, there is a low risk of bias for selection, performance, and attrition. But they incorrectly state that there is an unclear risk of detection bias. We indeed did blind the outcome assessors. Here is a quote from our published paper: “In this study, strict measurement protocols were used and intra- and inter-examiner reliability was measured using two independent orthopedic surgeons blinded to treatment allocation.”² Detection bias is a failure to blind the outcome assessor(s) to the treatment group allocation. We not only blinded our orthopedic surgeons as they assessed the spinal radiographs for the primary outcome, but also made their assessments independent. We suggest that Table 2 is incorrect regarding risk of bias for selection, performance, attrition, detection, and other source of bias.

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RESPONSE TO LETTER TO EDITOR: “SPINAL MANIPULATIVE THERAPY FOR ADOLESCENT IDIOPATHIC SCOLIOSIS: A SYSTEMATIC REVIEW”



To the Editor:

We welcome the comments that Feise and colleagues have made about our systematic review.¹ We had noted that Rowe et al² had examined intrarater and inter-rater reliability with blinded assessors. Intrarater and inter-rater reliability are commonly assessed through analyzing a subset of the data.³ As such, it was unclear whether blinded assessors had evaluated all outcomes, and hence the reason behind our attribution of an unclear rating for detection bias for the Rowe et al study. We appreciate the clarification that Feise and colleagues have provided. Nonetheless, whether the outcome assessors were blinded in the Rowe et al study has little bearing on the results of our systematic review. The primary limitation of the Rowe et al study was the low number of participants (n = 6), which meant that the effect size could not be established with any certainty, and the use of blinded outcome assessors is largely inconsequential in this regard.

We also appreciate Feise and colleagues noting the discrepancies between the results of the risk of bias presented in the text and risk of bias findings displayed in the table. Unfortunately, an error arose in the copyediting stage of the manuscript, and incorrect meaning was assigned to the +/- symbols in the legend of the table, that is, “+” should have meant “low risk of bias” and “-” should have meant “high risk of bias.”

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