



Letter to the Editor concerning “Comparison of combined anterior–posterior approach versus posterior-only approach in neuromuscular scoliosis: a systematic review and meta-analysis” by Shao ZX, et al. [Eur Spine J; (2018) 27(9): 2213–2222]

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Dear Editor,

We read with great interest the article by Shao et al. [1], which appeared in the September 2018 issue of *European Spine Journal*. The meta-analysis has brought several questions to our minds that we would like to communicate with the authors:

1. To our limited knowledge, the Newcastle–Ottawa scale (NOS) is recommended by most of the published studies to evaluate the qualities of observational studies (cohort, case–control, matched pair), so the authors should mention the consideration for choosing Methodological index for non-randomized studies (MINORS) as their quality assessing tool. On the other hand, with the use of MINORS, the author should explain the following issues: all studies scored 2 in item “prospective collection of data,” which is false in our opinion because some studies had mentioned their data collection in *Method* as this, “operative reports, outpatient clinical notes, and radiographs were reviewed [2]”; all studies scored 1 in item “loss to follow-up less than 5%,” which is inaccurate in our opinion too because three studies that did not report any relevant information should score 0. Additionally, it has been stated in the Cochrane Handbook [3] that funnel plots can be used for reviews with sufficient numbers of included studies (> 10), so there may be no need to use this methodology in the meta-analysis.
2. For the loss of Cobb angle in *Results*, we notice that the result in the no significant difference group was (MD, 6.4; 95% CI –0.19 to 13.0), which needs the authors to illustrate why they thought there was a significant difference in this group. Specifically, the result was affected obviously by the study *Teli 2006*. The authors should analyze in *Discussion* the similarities and differences between the three studies with special attention to aspects as patient type and surgical procedure of the third study, though there was no significant heterogeneity. This can improve the credibility of this important conclusion under the condition that there were only 3 studies and 166 patients included. In addition, the sentence “there was a significant difference in the significant difference group (MD, 6.4; 95% CI –0.19 to 13.0) without significant heterogeneity” reported in *Subgroup meta-analysis* seems erroneous.
3. With such large heterogeneities found in this meta-analysis (duration of operation, $I^2 = 69%$; blood loss, $I^2 = 52%$; duration of hospital stay, $I^2 = 75%$), subgroup analysis or sensitivity analysis had not been conducted to explain the source of heterogeneity. Different internal fixations and surgical techniques can result in different clinical outcomes [4]. We believe it is helpful to find the cause of heterogeneities when the authors perform subgroup analysis or sensitivity analysis according to the different internal fixations and publishing years, respectively.

We appreciate that Shao et al. provided us with an interesting meta-analysis focusing to compare different approaches in neuromuscular scoliosis and look forward to receive some responses about these issues.

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Compliance with ethical standards

Conflict of interest Fei Jia and Jianmin Sun declare that they have no conflict of interest.

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