Letter regarding Liu et al.'s study entitled “A systematic review with meta-analysis of posterior interbody fusion versus posterolateral fusion in lumbar spondylolisthesis”

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Dear Editor,

In a recent issue of *European Spine Journal*, Xiaoyang Liu and his workmates published an article [1] entitled “A systematic review with meta-analysis of posterior interbody fusion versus posterolateral fusion in lumbar spondylolisthesis”. In this study, the investigators performed a meta-analysis of four randomized controlled trials and five comparative observational studies on the clinical effectiveness of posterior lumbar interbody fusion (PLIF) and posterolateral fusion (PLF) for lumbar spondylolisthesis. The investigators concluded that PLIF can improve the clinical satisfaction and increase the fusion rate compared to PLF. No superiority was found between the two fusion methods in terms of complication rate, amount of blood loss, and operating time for the treatment of lumbar spondylolisthesis. However, we have several opinions that we would like to communicate to the investigators.

1. The investigators used a random-effects model to pool the data in all the forest plots (Figs. 2–8). However, different effect models may result in different results. Therefore, we would like to know the reason that the

investigators chose the random-effects model for all analyses. In this meta-analysis, the investigators used a random-effects model to pool the data in evaluating clinical satisfaction ($I^2 = 0\%$, Fig. 2), postoperative back pain ($I^2 = 20\%$, Fig. 3), fusion rate ($I^2 = 0\%$, Fig. 5) and reoperation rate ($I^2 = 15\%$, Fig. 6). All the I^2 values were below 25%, which showed no heterogeneity and the effect model should be fixed. Therefore, we suggest that “random-effects model” should be replaced by “fixed effect model”.

2. The results of the meta-analysis suggested that there were significant heterogeneities between studies in Fig. 4 ($I^2 = 58\%$), Fig. 7 ($I^2 = 92\%$) and Fig. 8 ($I^2 = 81\%$); meanwhile, the studies were combined by using the method of inverse variance (IV) or Mantel–Haenszel (MH) with the assumptions of a fixed-effects model, rather than DerSimonian and Laird’s random-effects model. In our opinion, the studies should be combined with DerSimonian and Laird’s random-effects model, which considers both within-and between-study variations [2].
3. The investigators included four RCTs and five comparative observational studies. In our opinion, the five non-RCTs should be excluded from the meta-analysis to enhance its credibility. Finally, the difference between studies was large, which may cause heterogeneity. In our view, subgroup analysis and sensitivity analysis should be conducted to explain the source of heterogeneity. Moreover, more carefully and scientifically designed studies with large samples are still needed to provide much stronger evidence for clinical decision-making in the future. We believe that these remarks will contribute to further, more accurate elaboration and substantiation of the original results presented by Liu et al. [1].

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Conflict of interest None.

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