Comment on Tian et al.: Minimally invasive versus open transforaminal lumbar interbody fusion: a meta-analysis based on the current evidence

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To the Editor,

In this meta-analysis, Tian et al. [1] compared the clinical and radiological efficacy of minimally invasive transforaminal lumbar interbody fusion (MI-TLIF) and conventional open transforaminal lumbar interbody fusion (open-TLIF) for degenerative lumbar diseases. They reached an important conclusion that MI-TLIF was associated with less blood loss, shorter hospital stay, and a trend of better functional outcomes when compared with open-TLIF. MI-TLIF seemed to be a promising procedure, but extra efforts are needed to reduce its intraoperative radiation exposure. It is a valuable study. Nevertheless, there are some comments we would like to raise related to this article.

1. A literature search was conducted up to July 2012 using MEDLINE database by the authors. The small number of required papers would be an important limitation of the review. We hope that more electronic databases can be systematically searched.

2. In this meta-analysis, the authors included randomized controlled trials and non-randomized comparative studies (prospective and retrospective). They also defined a eligibility criteria that a minimum sample size of ten was required for MI-TLIF group and open-TLIF. We are eager to know why they set such a criteria and the theory evidence.

3. Two study by Wang et al. [2, 3] were both included. Obviously, Wang et al. [3] and his team publish there prospective comparative study once again in 2011. To strength the credibility of meta-analysis, the study by Wang et al. [2] should be excluded from the meta-analysis.

4. It is not appropriate that summary mean difference (MD) estimate with corresponding 95 % CIs were derived by using the method of Inverse variance (IV) with the assumptions of a random-effects model (Fig. 2). However, studies should be combined by using the DerSimonian and Laird random-effects model [4]. Meanwhile, the results of the meta-analysis further suggested that the heterogeneity (I^2) between studies in Fig. 2 was found to be 87–100 %. This showed significantly higher variations between studies and cannot be comparable, which could potentially bias the results of this study. Moreover, I^2 values above 90 % are very rare in any meta-analysis. We are eager to know the authors' opinion about this.

5. Publication language was limited to English in the meta-analysis. Therefore, the authors should mention the potential importance of language bias in the limitations of their meta-analysis. It is not sufficient that publication bias was only assessed by visual examination of funnel plot (Fig. 5). Funnel plot symmetry should be further assessed by statistical tests (e.g. Egger's linear regression test or Begg's rank correlation test). Moreover, in the meta-analysis, publication bias was only assessed for complication rate. Actually, publication bias should be assessed for other comparisons (such as, blood loss, hospital stay, the

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intraoperative, X-ray exposure operative time, and re-operation rate). Therefore, publication bias may be present, distorting the meta-analysis.

We agree on the following conclusions of the authors: MI-TLIF resulted in less blood loss and shorter hospital stay, but was associated with more intraoperative X-ray exposure. Both MI and open-TLIF obtained similar operative time, complication rate, and re-operation rate. Our findings suggest that MI-TLIF is a promising procedure, but more effort should be conducted to reduce intraoperative radiation exposure in the future. More carefully and scientifically designed randomized controlled trials are required to further prove the claim.

Conflict of interest We have no competing interests to declare.

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