

### Comment on Anterior Cruciate Ligament Reconstruction with Autografts Compared With Non-Irradiated Non-Chemically Treated Allografts

To the Editor:

We read with deep interest the recent article by Lamblin et al.,<sup>1</sup> "Anterior Cruciate Ligament Reconstruction with Autografts Compared with Non-Irradiated Non-Chemically Treated Allografts." This is an excellent job that we really appreciate. However, after reading the paper very carefully, we also found some worthwhile issues worth being explored.

First, this systematic review included 11 studies (Level of Evidence of I to III) in the final analysis. Unfortunately, according to their specific inclusion and exclusion criteria, one randomized controlled trial<sup>2</sup> was probably missing. The missing trial<sup>2</sup> was prospectively randomized into 3 groups, which were the autograft group (33 patients), the nonirradiated allograft group (34 patients), and the irradiated allograft group (32 patients). The average follow-up was 31 months (range 24 to 47 months), which met the minimum of 2-year follow-up. Also, it would not be excluded for providing insufficient outcome data or some other prestated exclusion criteria. Perhaps the authors need to clarify more clearly why this study<sup>2</sup> was excluded from the systematic review.

Second, 2 of the included studies were from the same particular trial.<sup>3,4</sup> One trial<sup>3</sup> indicated that their study was from May 1991 to August 1992, and the other one<sup>4</sup> was from May 1991 through November 1992. Both of them were conducted in the same sports medicine and orthopaedic center. The major difference between these studies was the different first author, which may have misled the authors. We have no idea whether these 2 duplicative studies will have an impact on the result.

Third, meta-analysis should only be considered when a group of studies is sufficiently homogeneous to provide a meaningful summary. In addition, sensitivity analysis is the study of how the uncertainty in the output of a mathematical model or system can be apportioned to different sources of uncertainty in its inputs. Furthermore, publication bias is a tendency on average to produce results that appear significant, because negative or near-neutral results are hard to publish. However, in this study, the heterogeneity and risk of publication bias were not reported, nor was the sensitivity analysis, which was not conducted. According to the Cochrane Handbook for Systematic Reviews of Interventions, version 5.1.0, assessing publication bias and conducting sensitivity

analysis are recommended, and approaches to addressing clinical heterogeneity also should be described.<sup>5</sup>

Finally, the authors did not retrieve gray literature. Gray literature is informally published written material that may be difficult to trace through conventional channels.<sup>6</sup> However, it is crucial for completing a systematic review and meta-analysis.

Above all, we respect the contribution of the authors and we are pretty sure the results of the data analysis are accurate.

Jie Wei, M.D.  
Tu-Bao Yang, Ph.D.  
Changsha, China

**Note:** The authors report that they have no conflicts of interest in the authorship and publication of this letter.

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<http://dx.doi.org/10.1016/j.arthro.2014.01.007>

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### Authors' Reply

First and foremost, the authors thank Dr. Yang et al. for their careful reading of our systematic review. After