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International Journal of Surgery

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Letter to the Editor

Letter to the editor on the article “Is opening-wedge high tibial osteotomy superior to closing-wedge high tibial osteotomy in treatment of unicompartmental osteoarthritis? A meta-analysis of randomized controlled trials”


Dear Editor,

We took great interest to read the article by Zhan Wang et al. [1] entitled “Is opening-wedge high tibial osteotomy superior to closing-wedge high tibial osteotomy in treatment of unicompartmental osteoarthritis? A meta-analysis of randomized controlled trials” published online November 2018 in the International Journal of Surgery. They performed a systematic review and meta-analysis to investigate whether opening-wedge high tibial osteotomy (OWHTO) was superior to closing-wedge high tibial osteotomy (CWHTO) in treatment of unicompartmental osteoarthritis. It is a very interesting study. However, this review has brought several questions to our minds that we would like to communicate with the authors.

The review is reported according to the Preferred Reporting Items for Systematic Review and Meta-Analysis (PRISMA) format. The authors state the inclusion criteria as: 1) Patients were treated with CWHTO or OWHTO; 2) English randomized controlled trials. However, the inclusion criteria are ambiguous because the PICOS (P: participants, I: intervention, C: comparison, O: outcomes, S: study design) format did not sufficiently followed. The other components of inclusion criteria like specific outcome measures and which is intervention or control group did not clearly reported in the part of eligibility criteria. Running a review without full knowledge about the inclusion criteria can lead to problems with assessing the validity, applicability, and comprehensiveness of the review [2].

With such large heterogeneities found in this meta-analysis (HKA angle at < 5 years, $I^2 = 51.9\%$; HKA angle at > 5 years, $I^2 = 73.8\%$; tibial slope angle, $I^2 = 54.9\%$; complications, $I^2 = 78.9\%$), sufficient subgroup analysis had been conducted to explain the source of heterogeneity. However, sensitivity analysis is also the core component in a meta-analysis. Although the authors state: “A sensitivity analysis was performed to identify the source of the heterogeneity; we performed sensitivity analysis to identify the robustness of our meta-analysis.” Nevertheless, the sensitivity analysis for outcomes with high heterogeneities was not found.

Finally, only nine RCTs were included in this meta-analysis with all the studies were conducted in Europe (six from Netherlands), therefore, the ethnic bias should be considered carefully. We suggest that the authors should mention it in the discussion. It should be noted that most outcomes and subgroup analysis were performed with only

two or three included studies. Thus, the conclusion should be interpreted cautiously.

We appreciate that Zhan Wang et al. provided us with an interesting meta-analysis. However, more studies based on scientifically designed RCTs with long-term follow-up and large samples should be carried out to clear these issues.

Provenance and peer review

Not Commissioned, internally reviewed.

Data statement

Not Applicable.

Ethical approval

Not Applicable.

Source of funding

None.

Author contribution

All authors read and approved the final version of the letter to the Editor.

Conflicts of interest

None.

Research registration number

Not applicable.

Guarantor

Feng Wang.

<https://doi.org/10.1016/j.ijjsu.2018.12.017>

Received 16 December 2018; Accepted 20 December 2018

Available online 02 March 2019

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Med. 6 (2009) e1000097.

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