

Comments on the article “Clinical and radiological outcomes of fixed- versus mobile-bearing total knee replacement: a meta-analysis”

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Dear Editors,

With interest I have been reading the article “Clinical and radiological outcomes of fixed- versus mobile-bearing total knee replacement: a meta-analysis”, by Smith et al. [10] recently published in this journal.

I would like to congratulate the authors with performing a systematic review with meta-analysis. The basic requirements are met for a systematic approach and the authors have an excellent data presentation. Having a strong heart for the quality of systematic reviews, I feel obliged to make a few remarks concerning some issues with the methodology and results of the presented material which made me sometimes wonder and sometimes merely confused.

The authors choose to include retrospective material. In retrospective studies, the data are not gathered with a specific question in mind and selection bias might be present because it is difficult to reconstruct the referral strategies to the experimental and control groups. I would be interested in a best evidence approach appreciating the study types.

The authors neglect the status of the posterior cruciate ligament in the knee arthroplasties in the included studies. As shown in the Cochrane review by Jacobs et al. [5], the clinical outcome depends on the existence of a post-and-

cam mechanism in cruciate sacrificing implants. Neglecting this effect introduces heterogeneity in the included studies which should be addressed in a subgroup analysis.

I can fully support the inclusion of grey literature in systematic reviews, if analysed accordingly. However, the authors used a very limited search strategy to identify relevant studies in the white literature to begin with. To my knowledge there are a few additional trials that have been missed. The addition of grey literature to an inferior primary search does not add to the quality of the review.

I wonder if the separately analysed AKSS and KSS might refer to the same (American) Knee Society Score. At least two studies (Price et al. [9] (AKSS) and Kim et al. [7] (KSS)) refer to the well known score of Insall et al. [4] These outcome scores were separately analysed, resulting in a significant difference in the functional KSS. If referring to the same score, the KSS and AKSS should be analysed together, possibly removing the effect.

One more problem with the found difference is the standard deviation (SD) used. Typical SD for KSS clinical and functional scores is about 10–15, as we can see in fig 3. However, for the study of Munoz et al. [8], the SD for functional KSS is only 2.3. Being this low, this study loads almost entirely on the pooled effect. Looking at the original paper of Munoz et al. [8], they report a range of functional KSS of 55–100 for the fixed type and 54–100 in the mobile group. The authors do not report their method of inferring SDs from ranges, but usually we can divide the range by 4, yielding an SD for the study of Munoz et al. [8] of approximately 11. Similarly, I also question an SD of 122.7 in the study of Biau et al.

I was finally rather confused by the differences between Tables 1 and 6 regarding the judgement of study type (RCT, observational or retrospective) for the studies of

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Aglietti et al. [1], Bhan et al. [2], Henricson et al. [3], Kim et al. [7], and Kim et al. [6].

I hope the authors can shed some light on these issues.

Conflict of interest I am the primary author of the Cochrane review: Jacobs W, Anderson P, Limbeek J, Wymenga A. (2004) “Mobile-bearing vs fixed-bearing prostheses for total knee arthroplasty for post-operative functional status in patients with osteoarthritis and rheumatoid arthritis” *Cochrane Database Syst Rev* 2: [10.1002/14651858.CD003130.pub2](https://doi.org/10.1002/14651858.CD003130.pub2) [doi]. This review is currently being updated.

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