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LETTER TO THE EDITOR

Letter regarding "The effects of arthroscopic joint debridement in the knee osteoarthritis: results of a meta-analysis"

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Dear Sir,

The study by Spahn et al. [5] aimed at evaluating the clinical effects of arthroscopic joint debridement in patients with knee osteoarthritis. We note the following problems, which hamper the review's validity.

In the abstract, the authors state that "no randomized study that compared conservative and arthroscopic treatments for knee osteoarthritis was found". Among the included studies, however, we identified two randomized trials [3, 4]. One of these trials [4] was criticized because of "a number of faults", including the randomization process itself. We recently contacted the author group of this trial, and a co-author confirmed to us that sequentially numbered opaque sealed envelopes were used to conceal allocation. This shows that high-quality evidence is, in fact, available, thus negating the need to include non-randomized studies and even simple case series into a meta-analysis on effectiveness.

Contrary to the principles of evidence-based medicine, Spahn et al. analyzed the baseline versus follow-up data for the arthroscopically treated patients only. By doing so, all comparative evidence was denuded of its control group results. The lack of a control group greatly limits conclusions about changes attributable to treatment. It is therefore problematic to conclude that arthroscopic joint debridement "results in an excellent or good outcome in approximately 60 % of patients", because exactly the same success rates could have been observed with conservative treatment. According to Spahn et al.'s meta-analysis, the rates of knee replacement after arthroscopy averaged 22 % after 3 years. However, according to other studies, a rate of 22 % may well represent the natural course of the disease [1].

It is a good meta-analytical practice to plan an exploration of between-study heterogeneity at the protocol phase [2]. When planning to include non-randomized studies, the authors should have anticipated the inconsistent results and should have planned stratified analyses by design, treatment and patient characteristics. Simply pooling results across studies when visual inspection of the forest plot and the I-squared of 97.6 % indicate very large between-study heterogeneity is invalid and therefore misleading.

Lastly, we regret that the authors did not use any of the available papers providing guidance to reviewers on the interpretation of the overall body of evidence, as provided, for example, by the Cochrane Collaboration [2]. The overall quality of evidence is "very low" according to accepted standards due to the observational and non-comparative nature of the data, the associated high risk of bias and the observed inconsistency across studies [2]. The overoptimistic conclusions fail to reflect this.

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