

**Author reply to  
“Regarding ‘Operative  
Versus Nonoperative  
Treatment of  
Femoroacetabular  
Impingement Syndrome:  
A Meta-analysis of Short-  
Term Outcomes’”**



We thank Kim et al. for their comments regarding our article.<sup>1</sup> We sought to perform a meta-analysis of treatment outcomes in patients with femoroacetabular impingement syndrome (FAIS) comparing operative and nonoperative treatment. When possible, we focused on 12-month outcomes given that this is the time frame of important clinical decision making such as evaluation of surgical success and return to sport.<sup>2,3</sup> However, the included study by Palmer et al.<sup>4</sup> only reported outcomes after 8 months of randomization. These results were included in our meta-analysis after a post-protocol decision, and we do agree that it induces a degree of clinical heterogeneity in our analyses. Although we also agree a 6-month pooled analysis would have been a complementary adjunct to our article, we would argue that 6-month outcomes do not provide significant value in clinical decision making postoperatively.<sup>5</sup>

Kim et al. also questioned our pooling of adjusted and unadjusted mean differences in International Hip Outcome Tool 33 (iHOT-33) scores. We agree with the authors that under ideal circumstances, we would not have combined unadjusted and adjusted data. However, Palmer et al.<sup>4</sup> reported only adjusted mean differences between treatment groups, whereas Mansell et al.<sup>6</sup> reported only unadjusted mean differences. Griffin et al.<sup>7</sup> reported confidence intervals only for their adjusted mean differences and gave an unadjusted point estimate, which was higher than their adjusted mean difference. If we had used the unadjusted mean difference in our analysis, the pooled treatment effect would have more substantially favored surgery over nonoperative care, not the opposite, as Kim et al. suggest. If estimates and confidence intervals had been available for all studies, we would have included only unadjusted differences.

Kim et al. also critique our use of a fixed-effects model for pooling results. We do not share the same view that a random-effects model is always more appropriate, and in fact, their stated consensus of avoiding fixed effects in medical research is not borne out in the literature.<sup>8-13</sup> Previous statistical guides have advocated for the use of fixed-effects models provided that the  $I^2$  value indicates low heterogeneity across studies, as well as when the relative number of subjects included in the meta-analysis is small.<sup>14,15</sup> Additionally, it is well documented that the

use of random-effects models will disproportionately give higher weight to studies with smaller sample sizes,<sup>16</sup> which in our study would be the least generalizable trial with the most methodologic concerns.<sup>1,6</sup>

Ultimately, our meta-analysis found statistically significant improvement in patient-reported outcomes among surgically treated FAIS patients, as shown by their pooled iHOT-33 scores. The iHOT-33 is a robustly developed, valid, reliable, and responsive patient-reported outcome measure for health-related quality of life specifically for patients with FAIS.<sup>17</sup> Given the breadth of the iHOT-33 and its consistent performance with external psychometric evaluation,<sup>18</sup> we do believe that these results are generalizable to patient-reported hip-related outcomes. However, these results are inclusive of only 3 studies with relatively small sample sizes and do not include objective clinical measures such as strength and stability—a limitation well stated within our article. Clarity as to the true treatment differences, especially with long-term outcomes, will be derived only from future, well-powered randomized controlled trials.

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## Can't See the Right Forest Plot for the Wrong Trees!



We have with great interest studied the meta-analysis and forest plot in Dwyer et al.<sup>1</sup> which is supposed to illustrate the effect of operative versus nonoperative treatment of femoroacetabular impingement syndrome. The meta-analysis, based on three randomized controlled trials,<sup>2-4</sup> shows a between-group difference on the International Hip Outcome Tool-33 (iHOT-33; 0 points [worst] to 100 points [best]), favoring operative treatment (mean difference: 3.46 points; 95% confidence interval [CI] 0.07 to 6.86).

We congratulate the authors on the first meta-analysis for this very important topic, but unfortunately the forest plot presented by Dwyer et al.<sup>1</sup> is flawed owing to incorrect use of the iHOT-33 value derived from Palmer et al.,<sup>2</sup> published in 2019 in the *British Medical Journal* (BMJ). Dwyer et al.<sup>1</sup> have used the between-group difference of 2 points on the iHOT-33 reported by Palmer et al.<sup>2</sup> The iHOT-33 score is normally measured on a visual analog scale from 0 to 100 mm, where each millimeter represents 1 point.<sup>5</sup> When Palmer et al.<sup>2</sup> was published, we were surprised to see that a 2-point between-group difference could yield statistical significance with only 180 subjects. We contacted the authors at BMJ Rapid Response with our concern (see <https://www.bmj.com/content/364/bmj.l185/rr-4>), who informed us that they had used a centimeter scale (0 to 10 cm) instead of the usual millimeter scale (0 to 100 mm) (see <https://www.bmj.com/content/364/bmj.l185/rr-5>). Unfortunately, this is not clear in the BMJ paper.<sup>2</sup>

Furthermore, Palmer et al.<sup>2</sup> informed us in their Rapid Response that Dwyer et al.<sup>1</sup> have used these incorrect values in their meta-analysis. This means that Dwyer et al.<sup>1</sup> unknowingly have combined different scales (0 to 10 cm and 0 to 100 mm) in their meta-analysis, mixing absolute millimeter points from 2 studies,<sup>3,4</sup> with absolute centimeter points from Palmer et al.<sup>2</sup> The correct iHOT-33 value to include in the meta-analysis from Palmer et al.<sup>2</sup> should have been 20.4 points (95% CI 13.2 to 27.6), not 2 points (95% CI 1.2 to 2.8) (<https://www.bmj.com/content/364/bmj.l185/rr-5>). This has, based on what seems to be incorrect reporting using the iHOT-33 scaling by Palmer et al. in BMJ,<sup>2</sup> led to (1) an incorrect data point in the meta-analysis<sup>1</sup> and (2) an effect on the accompanying editorial commentary.<sup>6</sup>