Editorial

Interbody versus posterolateral fusion

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Systematic reviews and meta-analyses have become increasingly popular methods of summarizing similar clinical trials to obtain a better understanding of management options. Systematic reviews identify and critique relevant research studies, discuss factors that may affect heterogeneity, and synthesize the information in textual format. Meta-analyses allow one to identify relevant research studies according to a defined protocol, statistically test study heterogeneity and investigate explanatory variables, and statistically summarize results to estimate an overall treatment effect. The advantages of meta-analysis include the use of a protocol to choose studies that avoid selection bias, the inclusion of a statistical test to determine if variability between studies could be due to random variation (heterogeneity), and the calculation of an overall effect estimate. These factors provide increased statistical power and precision of confidence intervals, and results are often more generalizable than those from single studies.

In the article by Zhou and colleagues,14 the authors examine nine randomized controlled trials and comparative observational studies in an attempt to discern outcome differences between an instrumented posterolateral fusion (iPLF) and an instrumented posterior lumbar interbody fusion (iPLIF). They provide a credible summary and subanalysis and conclude that interbody grafting is associated with a higher fusion rate and improved restoration of alignment. They found no significant difference between the 2 techniques in terms of clinical outcome, complication rate, operating room time, or blood loss.

In general, Zhou and colleagues’ study is methodologically credible. The authors include a random-effects model for the assessment of heterogeneity, although Cheong and van Gelder1 have questioned whether this is the most suitable technique when the causes of heterogeneity remain unclear, particularly for neurosurgical data.

Additionally, one may argue that the outcomes of interest in this study represent imperfect reference standards due to the lack of uniform consensus on what constitutes relevant outcomes. In such circumstances it has been proposed that meta-analysis may not be a suitable tool for summarizing findings and that a simple description of study results may be preferable until suitable statistical methods to adjust for imperfect reference standards are established.1

The authors correctly address the issues of the publication bias and the general limitations of the data available for inclusion. Meta-analyses are particularly prone to publication bias because the results of positive studies are more likely to be published than comparable investigations with negative findings. The authors address this in the discussion, but the likelihood of this bias remains. The quality of the available data is likely the major limitation of the study. The meta-analysis comprises a total of 9 studies including 3 randomized controlled trials,2,6,8 2 prospective studies,5,9 2 retrospective comparative trials,7,9 and 2 listed as “unclear.”10,13 The level of evidence provided in either systematic reviews or meta-analyses is only as good as the lowest level of the studies selected for inclusion. Alternatively stated, a complex analysis of biased or low-level data will not lead to better conclusions than those of the original data.

In summary, the authors should be commended for their detailed analysis and attempts to contribute to the ongoing debate on optimal care of patients with low-back pain. The inclusion of a broad spectrum of data may improve the generalizability of a meta-analysis, but it may also limit the ability of evidence-based study groups to incorporate the results due to the absence of data addressing specific disease processes rather than a particular surgical technique.

The American Association of Neurological Surgeons and Congress of Neurosurgeons—endorsed document, “Guidelines for the performance of fusion procedures for degenerative disease of the lumbar spine. Part I: interbody techniques for lumbar fusion”11 concluded at the option level:

1) It is recommended that both PLF and interbody fusion (PLIF, TLIF [transforaminal interbody fusion], or ALIF [anterior lumbar interbody fusion]) techniques be considered as treatment options for patients with low-back pain due to DDD (degenerative disc disease) at one or two levels. 2) Placement of an interbody graft is recommended as a treatment option to improve fusion rates and functional outcome in patients undergoing surgery for low-back pain due to DDD at one or two levels. The surgeon is cautioned that the marginal improvement in fusion rates and functional outcome with these techniques is associated with increased complication rates, particularly when combined approaches (that is, 360°) are used. 3) The use of multiple approaches (anterior and posterior) to accomplish lumbar fusion is not recommended as a routine option for the treatment of patients with low-back pain without deformity.”
The North American Spine Society–generated document “An evidence-based clinical guideline for the diagnosis and treatment of degenerative lumbar spondylolisthesis”12 included the question: “How do outcomes of decompression with posterolateral fusion compare with those for 360° fusion in the treatment of degenerative lumbar spondylolisthesis?” The authors concluded that “Because of the paucity of literature addressing this question, the work group was unable to generate a recommendation to answer this question.”

Given the low-level recommendations possible in these 2 comprehensive documents, it is clear that additional data is badly needed to improve the quality of the recommendations that are critical to guiding optimal care of the back pain patient. Unfortunately, the heterogeneity of the study populations included in the current study limits its contribution to either of these evidence-based documents.

Disclosure

Dr. Traynelis has a financial relationship with Medtronic and United Health Care.

References


Response

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Despite many studies on iPLIF and iPLF, the preferred fusion method for the degenerative lumbar spine remains uncertain. Our meta-analysis, with the best evidence derived from 9 identified studies,2,4,7–11,15 was performed to compare the outcomes of these two fusion techniques, following the recommended method guidelines.6,12,14 Our meta-analysis provided moderate-quality evidence that iPLIF had the advantages of higher fusion rate and better restoration of spinal alignment over iPLF. No significant differences were identified between iPLIF and iPLF concerning clinical outcome, complication rate, operating time, and blood loss.

Although heterogeneity was statistically absent or mild in 5 of 8 meta-analyses (5 of 6 for binary data), we used a random-effects model rather than a fixed-effects model because we observed a number of differences in study design, specific surgical techniques, and odds ratios among the individual studies.5 Additionally, we performed subgroup analyses in an attempt to determine the causes of heterogeneity. We expected that would be a beneficial complement.

Although we collected the best evidence from 3 randomized controlled trials (RCTs) and 6 comparative observational studies, we admit that the quality of these available data is probably the major limitation of this study. As Drs. Traynelis and Ryken mentioned, the level of evidence provided by either systematic review or meta-analysis is only as good as the lowest level of the studies. Given that point, we conducted subgroup analyses for RCTs and comparative observational studies, beyond the analyses for the overall materials in all these meta-analyses. These analyses revealed consistent effect trends for the main results abstracted from mere RCTs and from all the included studies. Moreover, we assessed
the evidence quality of the results only by using the RCTs rather than by all of the selected studies, following a rating system with 4 levels recommended by the Grading of Recommendations Assessment, Development and Evaluation Working Group. Therefore, we treated the level of evidence provided by our study as being as good as that of the RCTs, although the quality was generally not high because of the limitations of the study design, or inconsistency, indirectness, and imprecision of results, or publication bias of the identified RCTs.

Given the potential incorporated heterogeneity and publication bias in the current study, as well as the diminishing quality of evidence due to the small number of trials included, the pooled odds ratios should be treated with caution. Additionally, this study was limited to contribute to evidence-based documents concerning optimal care of patients with low-back pain of a broad disease spectrum rather than of a specific entity such as DDD or degenerative lumbar spondylolisthesis, due to the lack of original data addressing the specific disease. For all this, we believe our meta-analysis would be a valuable addition to the literature. Future studies of high quality, in particular RCTs, are urgently needed to add to current data.

References
3. Dantas FL, Prandini MN, Ferreira MA: Comparison between posterior lumbar fusion with pedicle screws and posterior lumbar interbody fusion with pedicle screw in adult spondylolisthesis. Acta Orthop Belg 70:578–582, 2004