

Editorial Commentary: Book? ... Book Report? ... or Just a New Chapter in an Ongoing Story?: Knee Partial Meniscectomy Has Limited Benefit for “Nonobstructive” Meniscal Tears, but We Need to Know if Patients Have Osteoarthritis



Abstract: Knee partial meniscectomy has limited benefit for “nonobstructive” meniscal tears, but we need to know if included patients have osteoarthritis. Research on outcomes of arthroscopic partial meniscectomy versus nonsurgical treatment must consider not only signs and symptoms but also imaging findings, to determine the indications for surgical versus nonsurgical in a selected patient.

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Meta-analyses, when done comprehensively and well, have as their strongest advantage the ability to combine smaller—often underpowered—randomized investigations, to arrive at a pooled estimate of treatment effect that may achieve statistical significance. It is up to the reader to determine the clinical significance of such an estimate. This potential advantage assumes all the individual investigations are similar with respect to methodological rigor, study population, and outcomes. In the investigation “Arthroscopic Partial Meniscectomy or Conservative Treatment for Nonobstructive Meniscal Tears: A Systematic Review and Meta-analysis of Randomized Controlled Trials,” Van deGraaf et al.¹ pool the results of 7 randomized trials to demonstrate small, short-lived physical function benefits of arthroscopic partial meniscectomy (APM) over nonoperative treatment for predominantly degenerative meniscus tears. These benefits are not apparent beyond 6 months post-operation. Although this conclusion is not overly surprising to many of us, the precision of the estimate—derived from data obtained from almost 800 patients—should strengthen our confidence in inferring the limited effect of this treatment in arthritic individuals.

The 3 most common criticisms of any meta-analysis relate to heterogeneity among the individual studies,

inadequate quality and/or validity of the smaller investigations thereby leading to subsequent bias of a larger pooled estimate (garbage in—garbage out phenomenon), and the failure to consider the often unrecognized effects of publication bias. Poolman et al. have addressed each of these potential shortcomings as well as possible within the limits of current orthopaedic research and reporting thereof. For the most part, the studies are all similar with respect to demographic characteristics of the randomized patients. Important prognostic factors that are either unknown or imbalanced are the exact proportion of patients with arthritis, the extent of that arthritis, and the mechanical alignment of the limbs preoperatively. This is information that would be difficult, if not impossible, to ascertain on these patients retrospectively and the hope is that if other known factors are balanced, these will be also. After all, randomization is the best weapon we have in the fight against selection bias. In addressing the “garbage in—garbage out” criticism, a rigorous approach was employed to quantify the methodologic quality of the individual studies utilizing the Cochrane risk of bias tool.² This tool is likely the best we currently have to identify sources of bias at each level of randomized controlled trial design and in determining the extent to which such bias might weaken inferences that can be drawn from results. As for the possibility of publication bias, the authors searched for relevant articles outside the English language, which is admirable, but they did not report a search of relevant meeting abstracts (American Academy of Orthopaedic Surgeons [AAOS], American Orthopaedic Society for Sports Medicine [AOSSM], Arthroscopy Association of North America

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[AANA], European Society of Sports Traumatology, Knee Surgery, and Arthroscopy [ESSKA], International Society of Arthroscopy, Knee Surgery and Orthopaedic Sports Medicine [ISAKOS], etc.) over the study period, so we are left to wonder whether there may have been some unpublished investigations that were omitted. Somewhat curious is the exclusion of the first—and perhaps most famous—trial of knee arthroscopy by Moseley et al.³ as well as that of Kirkley et al.⁴ The former does lack a formal nonoperative control group and focuses more on arthroscopic treatment of degenerative disease, but includes a significant proportion of patients who underwent APM for “nonobstructive” tears in a large well conducted trial. Interesting too is the inclusion of the trial of Osteras et al.⁵ with a sample size of only 17 patients. This investigation is almost certainly underpowered and its inclusion, although comprehensive, has the potential to introduce bias and heterogeneity as a result of the small numbers.

In a previous editorial in this journal, it was suggested that reviews may be overly prevalent within orthopedics. Provencher et al.⁶ suggested that researchers might do better to “perform original scientific research and contribute an important new ‘book’ rather than another ‘book report.’” There are 2 previous meta-analyses that arrive at very similar estimates of treatment effect to that presented here.^{7,8} Although another investigation of the role of APM may not be overly beneficial in a large randomized controlled trial setting (there are likely some ongoing), there is perhaps some benefit of looking at the question in specific cohorts. It is pretty clear to most of us that partial meniscectomy is likely *not* helpful in the setting of established osteoarthritis—what is *not* clear is how many of the patients in each group in this study are arthritic. Although the cohort is derived from randomized controlled trials—in which prognostic factors should be balanced—that may not be true of the pooled group because of variations in inclusions, patient populations, etc. The 2 largest trials in the group demonstrate that — on average — a third of patients randomized to receive nonoperative care cross over. The reasons for the crossing over may be varied and controversial, but there must be some patients within that subgroup who just don’t do well with conservative treatment. Who those patients are, how to identify them, and whether

they do any better after operative care is still an unknown. Further work (perhaps an important new “book”) might help answer some of those questions.

Disconnect between what the evidence demonstrates and what is actually happening in clinical practice continues to plague us. APM is still the most commonly performed orthopaedic surgical procedure, and the numbers are rising.⁹ Perhaps future efforts should be directed toward elucidating the reasons for the ongoing gap in knowledge translation rather than pursuing another study to show us what we already know.

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