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Author response—nonoperative treatment of lateral epicondylitis: a systematic review and meta-analysis

In reply:

We accept that lateral epicondylitis may be a degenerative condition and that inflammation may or may not be present. Although “lateral elbow tendinopathy” is acceptable, the term “lateral epicondylitis” remains in widespread use in the medical literature and in trials included in this study, and the term we chose to describe the condition for the purposes of our study.

We did not state that Chesterton et al was a systematic review, but rather that past systematic reviews did not reach definite conclusions. Given that Chesterton et al was a review paper that summarized 17 systematic reviews and 6 meta-analyses, we felt that this was an important citation to include.

We completely agree that the term “physiotherapy” has been used as a catch-all phrase to denote many different modalities and this has contributed to the confusion in interpreting the efficacy of these different treatments. Our desire to distill strengthening from other physical therapy modalities formed the basis of the current study.

We are not sure whether the author is suggesting that stretching cannot be used as a passive treatment.

Although we accept the terminology the author suggests, the term ‘massage’ is widely used in the literature and reproduced here given that it was the term used in some of the original comparative trials.

Only by comparing strengthening with a true control (no active treatment) can one determine whether strengthening has a significant treatment effect. With respect to the author’s question about the natural history of the condition, unequivocally, tennis elbow is self-limited in 90% of patients.[1] However, symptoms do not resolve in all patients and the reasons for this are not well understood and likely multifactorial.

Although we recognize the value of supervised programs, there is little evidence that they are more effective than home programs for lateral epicondylitis. The study cited by the author was a nonrandomized study. The results, although interesting, must still be interpreted with caution given the potential for selection bias in this type of study design.

McQueen et al was a ‘randomized controlled trial’ as described in the study methodology. We agree that the strategy used to generate the random allocation sequence increased the potential risk of bias; using the Cochran Risk of Bias tool, we appraised this study’s risk of bias as “serious.”

We compared strengthening to no active treatment. We excluded trials that compared other physiotherapy modalities (eg, stretching vs. no active treatment, massage vs. no active treatment). The fact that a few of the studies allowed stretching in addition to strengthening, if anything, would bias the results in favor of physiotherapy. The fact that no differences were found lends further confidence to our conclusion that no difference exists between strengthening and no active treatment.

Similarly, the addition of deep friction massage to strengthening would, if anything, bias the results in favor of physiotherapy. The fact that no differences were found lends even greater confidence to our conclusion that no difference exists between strengthening and no active treatment.

We would be very interested in seeing the evidence that demonstrates that proprioception and neuroplastic training is superior to a strengthening program alone, especially given that our study did not demonstrate any benefit to strengthening alone compared with a ‘wait and see’ approach.

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Peter Lapner, MD, FRCSC[a]
Jonah Hebert-Davies, MD, FRCSC[b]
J. Whitcomb Pollock, MD, FRCSC[a]
Ana Alfonso, MD[a]
Jonathan Marsh, MD, FRCSC[c,d]
Graham J.W. King, MD, FRCSC[e]
On behalf of the Canadian Shoulder and Elbow Society (CSES)
[a]Division of Orthopaedic Surgery, The Ottawa Hospital, University of Ottawa, Ottawa, ON, Canada
[b]Orthopedic Trauma Surgery Clinic at Harborview, Seattle, WA, USA

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[c]Pan Am Clinic, Winnipeg, MB, Canada
[d]Section of Orthopaedic Surgery, Department of Surgery, University of Manitoba, Winnipeg, MB, Canada
[e]Roth|McFarlane Hand and Upper Limb Centre, St. Joseph's Health Care, Western University, London, ON, Canada

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