Letter to the editor concerning “Independent evaluation of a clinical prediction rule for spinal manipulative therapy: a randomised controlled trial” (M. Hancock et al.)

Jeffrey J. Hebert · Stephen M. Perle

Received: 13 May 2008 / Accepted: 13 July 2008 / Published online: 27 August 2008
© Springer-Verlag 2008

To the Editor:

We congratulate Hancock and colleagues for undertaking a randomized trial which in part, examined the effectiveness of an eclectic approach to manual therapy for non-specific low back pain (LBP) [8]. These results were reported elsewhere and demonstrate that individuals with non-specific LBP, who receive paracetamol and advice from a general medical practitioner, do not experience a shortened time to recovery with the addition of diclofenac or an assortment of manual therapy techniques.

The authors carried out a secondary analysis of this data [9] to evaluate the performance of a clinical prediction rule [3], which identifies individuals who have a high probability of achieving clinical success with a combination of a spinal manipulation technique and therapeutic exercise. It appears in part, that the authors sought to determine whether the prediction rule would also identify patients with LBP who experience clinical success with treatment consisting of a diverse collection of predominantly mobilization techniques, in the absence of therapeutic exercise, and when applied in a non-standardized manner. Curiously, while the authors felt the need to highly standardize the medical treatment (e.g., drug, dosage, frequency) in this trial, there was no like effort to standardize the manual therapy approaches. The inevitable result was that the clinicians

utilized a diverse mix of manual therapy techniques, which may be an explanation for the lack of clinically important change in those receiving this therapy. Indeed, a recent metaanalysis [10] investigating manual therapy as a treatment for non-specific LBP, observed smaller treatment effects in trials where clinicians were not restricted as to the type of treatment technique. Given these differences in treatment type and application, it is clear that the treatment employed in this trial differs greatly from those used to develop [3] and validate [1] the clinical prediction rule, on which this analysis was purportedly evaluating.

The terminology used to describe manual therapies has been identified as problematic [4]. Only 5% of all manual treatments in this study involved spinal manipulation, and yet Hancock and colleagues chose to use the term “spinal manipulative therapy” to describe the treatment evaluated in these papers. This is confusing to the biomedical community who may not understand the subtle differences in terminology used by clinicians and researchers who employ these treatments. While we feel confident that this was not an attempt at obfuscation by the authors, this appears to be the result nonetheless. Secondary sources such as Medscape®, have cited the author’s work as evidence arguing against the effectiveness of spinal manipulation [11]. We respectfully suggest that in future work, researchers differentiate between manipulation (i.e., thrust or high-velocity passive joint movements) and mobilization (i.e., non-thrust or low velocity passive joint movements) to avoid on-going confusion over this issue.

The analysis conducted by Hancock et al. does contribute to a greater understanding of the performance of the clinical prediction rule, when a very different treatment package is utilized. In other words; a prediction rule which identifies individuals likely to experience clinical success with manipulation and therapeutic exercise, does not

A reply on this paper is available at doi:[10.1007/s00586-008-0728-4](https://doi.org/10.1007/s00586-008-0728-4).

J. J. Hebert (✉)
College of Health, University of Utah,
Salt Lake City, UT 84112, USA
e-mail: Jeff.Hebert@utah.edu

S. M. Perle
College of Chiropractic, University of Bridgeport,
Bridgeport, CT 06604, USA

identify individuals who experience clinical success when receiving a variety of mobilization techniques directed at the thorax, lumbar spine, pelvis and hips. Unfortunately, Hancock and colleagues chose to frame their results by questioning the external validity of the clinical prediction rule. We find this curious, as the prediction rule is part of a *treatment-based* approach to sub-grouping individuals with LBP [7]. It appears that the treatment received by individuals in this trial is very different than the manipulation and exercise techniques which have been the basis of previous investigations of the prediction rule [1–3, 5, 6]. Given these differences, we fail to understand how the results of this secondary analysis can be used to shed light on the clinical utility of the prediction rule, when used in a manner for which it was intended; that being the application of therapeutic exercise and a manipulation technique to individuals with specific prognostic indicators. We would make the argument that the findings of Hancock and colleagues do not test the validity of the prediction rule, but make the case for the implementation of the rule in the same manner as it was developed, validated and examined in other clinical settings.

References

- Childs JD et al (2004) A clinical prediction rule to identify patients with low back pain most likely to benefit from spinal manipulation: a validation study. *Ann Intern Med* 141(12):920–928
- Cleland JA et al (2006) The use of a lumbar spine manipulation technique by physical therapists in patients who satisfy a clinical prediction rule: a case series. *J Orthop Sports Phys Ther* 36(4):209–214
- Flynn T et al (2002) A clinical prediction rule for classifying patients with low back pain who demonstrate short term improvement with spinal manipulation. *Spine* 27:2835–2843
- Flynn TW et al (2008) Manual physical therapy: we speak gibberish. *J Orthop Sports Phys Ther* 38(3):97–98
- Fritz JM, Childs JD, Flynn TW (2005) Pragmatic application of a clinical prediction rule in spinal manipulation intervention primary care to identify patients with low back pain with a good prognosis following a brief spinal manipulation intervention. *BMC Fam Pract* 6(1):29
- Fritz JM, Brennan GP, Leaman H (2006) Does the evidence for spinal manipulation translate into better outcomes in routine clinical care for patients with occupational low back pain? A case-control study. *Spine J* 6(3):289–295
- Fritz JM, Cleland JA, Childs JD (2007) Subgrouping patients with low back pain: evolution of a classification approach to physical therapy. *J Orthop Sports Phys Ther* 37(6):290–302
- Hancock MJ et al (2007) Assessment of diclofenac or spinal manipulative therapy, or both, in addition to recommended first-line treatment for acute low back pain: a randomised controlled trial. *Lancet* 370(9599):1638–1643
- Hancock MJ et al (2008) Independent evaluation of a clinical prediction rule for spinal manipulative therapy: a randomised controlled trial. *Eur Spine J* (in press)
- Kent P et al (2005) Does clinician treatment choice improve the outcomes of manual therapy for nonspecific low back pain? A metaanalysis. *J Manipulative Physiol Ther* 28(5):312–322
- Vega CP (2008) NSAIDs and Manipulation ineffective for acute low back pain: a best evidence review. *Best Evid Rev*. Available at <http://www.medscape.com/viewarticle/572783>. Accessed 10 May 2008