Reviewer's report

Title: A Diagnosis-Based Clinical Decision Rule for Patients with Spinal Pain. Part 1: Theoretical Model

Version: 3 Date: 7 January 2007

Reviewer: John Childs

Reviewer's report:

General

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

The authors present an overall well written paper that covers an important topic. I have 2 primary concerns with the paper as written:

1) The authors present a mixed message about the value of traditional pathoanatomic vs. treatment-based classification systems.

A common theme throughout the paper is that the traditional medical diagnosis of patients with spinal pain is multifactorial and that efforts to develop pathoanatomic classification systems have been mostly futile and do not accurately influence decision-making for the large majority of patients with back pain. In the same vein, they propose to develop a ‘diagnosis-based’ clinical decision rule that presumably guides treatment-related decision-making based on a ‘specific diagnosis’ (presumably a pathoanatomic one). On the other hand, the authors criticize the value of pathoanatomic (and I agree). The authors appear to be advocating the development of an alternative quasi pathoanatomic/treatment-based classification system, which sends a contradictory message at times to the reader. Please address.

As an example, after ruling out the possibility of a serious medical condition, the authors include determination of the pathoanatomic origin of the patient’s symptoms as a central tenant of their proposed system (Question #2: ‘From where is the patient’s pain arising?’). However, the authors readily acknowledge that virtually no diagnostic evidence exists that can accurately answer this question (p. 6: ‘methods that allow clinicians to make an unequivocal tissue-specific diagnosis have been elusive.’), and even those that have do not necessarily accurately influence the best treatment decisions. They go on to say that ‘historical factors and examination procedures can allow one to identify certain characteristics in each individual patient that may be useful in making treatment decisions.’ This is an accurate statement but does not answer the central question about the pathoanatomic origin of the patient’s symptoms, thus why include this as a central tenant?

The authors appropriately add the consideration of psychosocial factors into their model, but these factors (catastrophizing, fear, depression, etc.) are mixed in with other factors more commonly classified as physical factors (ie, dynamic instability, oculomotor dysfunction, etc.). The authors should clarify their rationale for aggregating these factors together. As an example, there is some evidence that the presence of clinical signs and symptoms are predictive of outcome from a matched stabilization exercise treatment approach (Hicks, Arch Phys Med Rehab, 2005). Why not include this category within the 2nd question related to ‘origin’? Also, how is that the authors propose actually ‘new’?

2) The authors ignore a large body of existing evidence supporting some of the very ideas that the authors propose as ‘novel’ in this paper.

The authors appear to be advocating for a treatment-based classification system that downplays the importance of understanding the precise etiology of the patient’s pain except for serious medical conditions. This is clearly articulated on p. 12 when they say that:

‘the diagnosis using the DBCDR is not a traditional diagnosis, in which a diagnostic label is given to a disease entity based on the unique pathophysiology of the particular entity. Rather, it is a collection of signs, sometimes single and sometimes multiple, from which the clinician can make treatment decisions.’
However, in doing so, the authors do not compare and contrast their ‘new’ system with several existing treatment-based classification systems (Delitto, McKenzie, etc.) that have already been developed to overcome the limitations of pathoanatomic models. The authors should clearly articulate how their system will improve upon contemporary treatment-based classification systems for which mounting evidence is available to support their effectiveness in improving decision-making and patient outcome, regardless of the precise underlying etiology. Except for so called muscle palpation signs (ie, trigger points), the key examination procedures advocated within the authors’ proposed system (segmental palpation, centralization, and neurodynamic signs) are readily incorporated within existing treatment-based classification systems that have already been proposed and in many cases, already have a growing research foundation.

In essence, what is new about what the authors propose and how is it better that what has already been done in this area? For example, the authors advocate manipulation in the presence of positive segmental pain provocation signs, which is reasonable. However, it does not make sense to hold this up as a ‘new’ recommendation when evidence already exists that manipulation is effective in patients with segmental hypomobility (and who have acute symptoms, symptoms above the knee, low fear avoidance beliefs, etc. – Childs, Ann Int Med, 2004). Entire comprehensive classification systems have already been developed (Fritz, Spine, 2003; Brennan, Spine, 2006, Childs, Ann Int Med, 2004, Hicks, Arch Phys Med and Rehab, 2005, Long, Spine, 2004, etc.). Given the large overlap in the ‘new’ system proposed here with existing treatment-based classification systems, a ‘compare and contrast’ discussion is highly relevant for this paper and should be added.

The authors appear to have a related manuscript to follow that lays out the existing evidence for their approach. Presuming this secondary paper includes the evidence inherent within other existing treatment-based classification systems, this further highlights the need for the authors to differentiate how their system will improve upon existing treatment-based research efforts to improve non-surgical treatment-related decision-making in patients with spinal pain. Many of the psychometric properties of the procedures that the authors propose to review in this second paper have been discussed in detail elsewhere, in addition to their value to improve decision-making. Overall, it is unclear how this system is in fact ‘new’.

In summary, it is difficult to judge the theoretical merits of how this system is ‘new’ and different from existing treatment-based classification systems. A substantive debate about how the authors’ proposed system will improve upon existing treatment-based classification systems that similarly downplay the relevance of the pathoanatomic diagnosis is necessary to accurately judge the merits of this ‘new’ system.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Why is it ‘interesting, and sometimes useful, to speculate about the precise pain generating tissue responsible for producing pain with segmental palpation’ unless this understanding can improve decision-making. Statements like this throughout the paper give the impression that the authors are trying to develop a ‘middle ground’ system that considers both pathoanatomic and treatment-based diagnosis simultaneously.

The authors should make it clear throughout that their proposed system is addressing the non-surgical management of patients with spinal pain.

In general, the paper appears to be over-referenced with 122 references.

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Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable
**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.