Reviewer's report

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

Version: Date: 14 April 2007

Reviewer: Ronald Donelson

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General
In addition to the revised manuscript, I also had the benefit of the authors' responses to my initial questions from which I could gain further insights into this paper. Their addition of the section comparing their DBCDR model with other classification systems is helpful, but it also raised new issues relative to their misunderstandings of some important data and their inferences regarding MDT studies.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
The authors stated that this first manuscript is intended to merely present the theoretical model for their DBCDR model and that their subsequent manuscript(s) will present the evidence and the gaps in evidence. In their response to Q3 in my first review, they stated it is not the intention of this paper to report reliability and validity data. Yet they then cite 129 references throughout their article, most of which represent evidence in support of the reliability and validity of their “theoretical” DBCDR model.
That is fine, but it leaves readers with the incorrect impression that this entire model has evidence to support it. As they then acknowledge in their responses to my questions in my first review, there are some important evidence gaps not acknowledged in the manuscript. It is essential that manuscript readers receive a better balance between the evidence and the gaps in this model, even at this introductory stage.
Further, throughout the manuscript, the authors acknowledge their own clinical “experience”. I don’t have problems with that in this introductory paper, but, again, these acknowledgements are almost exclusively in support of their model. There is again an absence of their “experience” regarding evidence gaps and the need for further research, which they also acknowledged when responding to my first review questions. So if supportive evidence is to be cited, then the research gaps and therefore the gaps in their model likewise need to be mentioned, at least briefly, with details to come in their subsequent manuscript(s).

For example:
1. Re. reliability research: In the opening, it is stated that “one finds a variety of methods for detecting many of the factors that are believed to be of importance for which reliability and validity have been demonstrated.” This implies to many that good reliability evidence exists throughout their model. But this is not true. Given the fundamental importance of reliability evidence, the authors need to acknowledge when and where this initial step in diagnostic research is lacking or even unsupportive. In several spots, they fall into a common trap of ignoring the topic of reliability evidence and going directly to treatment evidence. The reliability of trigger point elicitation is an example. This is a very controversial topic with many studies showing a lack of reliability. To omit this and move on to discussing treatment is inappropriate in my view.
2. Re. the overlap of their four physical signs:
a. Many patients with segmental pain provocation (SPP) are also centralizers. This is an important gap in this system, acknowledged by the authors (see their answer to my Q2). If this holds up in future research, as the authors and I expect it will, this would greatly alter their model, the treatments of this important subset, as well as the interpretation regarding the likely anatomic source of pain. The authors appear to have removed references in the text to facet pain in patients with SPP but have for some reason maintained it within Table 2, which should be altered accordingly or perhaps even eliminated. For those with both centralization and SPP, likely a large group of LBP patients, disc pathology could be the likely pain source. There’s no evidence to link SPP with facet any more than with a disc source. Either is feasible and the biomechanical cadaver and imaging evidence that centralization comes from disc pathology is pretty strong.
b. Centralization and the presence of trigger points. The authors also acknowledge their experience that trigger points disappear when centralizing pain is treated successfully, but only in their response to my review questions. Though no one has any data on this, here is where the authors’ experience with this should be mentioned in the manuscript.
c. Centralization and the presence of depression, fear-avoidance beliefs, passive coping, etc. Again, the authors state in answering Q4 that it is their “opinion that treatment according to centralization signs is
especialy powerful in this regard” with the Werneke et al (2001) and Long et al (2004) data confirming their, and my, clinical experience. That needs to be mentioned in the manuscript.

P. 5: “By utilizing many of the various diagnostic procedures available to the spine clinician, one can develop a specific working diagnosis that encompasses all of the dimensions for which there may be contributing factors and from which a management strategy may be designed that addresses each of the most important factors in each individual patient.”

Published data reports are precisely to the contrary of this statement. Those patients in whom multiple factors are found that need to be managed in some way actually comprise a small LBP subset. This is based on the evidence that most LBP patients are centralizers (comprising 75% of acutes and 50% of chronics), whose pain generator is sufficiently characterized by its Dir. Pref. and these reversibility signs – centralization. These findings then guide how the pain can be eliminated, even long term, with all secondary outcome variables also improving without direct care focused on these other factors (see RCTs: Schenk 03, Long 04, Brennan 06, Browder in print; and many cohorts, Werneke 99 and 01 especially). No other clinical findings, including any co-existing factors listed in this DBCDR model (psychosocial, segmental pain provocation, trigger points, neurodynamic signs, etc.) needed any specific attention in this very large subgroup of centralizers.

P. 5: Muscle palpation signs “have typically been thought to implicate the presence of myofascial trigger points (TrPs) [33].” In my opinion, “has typically been thought” is hardly sufficient to justify attributing muscle palpation signs to myofascial pain generation. That is pure theory or speculation. Myofascial pain generation is very controaversial, with little or no evidence that such a pain-generating pathology even exists. In the subsequent paragraph, the authors correctly acknowledge the lack of evidence to validate the use of muscle pain referral maps but the lack of evidence of reliability in identifying muscle palpation signs or any link between them and myofascial pain generation should be acknowledged.

No data is presented to support the discussion of Waddell signs on page 10. Since there is apparently nothing in the author’s anecdotal experience to justify these statements, this seems to be pure speculation that these signs could be the basis for identifying or even screening for the presence or absence of CPH. This huge research gap should be identified or this discussion removed altogether.

P. 12: I struggle with the authors’ use of the word “diagnosis” when merely describing a combination of clinical findings that may or may not be influencing the LBP symptoms. Their three examples do not illustrate “a diagnosis”, nor even a particular subgroup of patients, but merely list three findings that may or may not be relevant to the presenting symptoms. On page 22, they themselves state: “The DBCDR is different from these other systems in that there are no classifications in which patients are placed.” Their use of the term “diagnosis” seems inappropriate as their system unfolds throughout this article.

P. 13: Again, the authors state: “An important concept of management in this model is that none of the important factors that may be present in any given spinal pain patient occurs in isolation.” While this is certainly true, it again brings up the point made earlier: not all co-existing factors are relevant, important, or need direct treatment. Considerable data exists that parallels the clinical experience acknowledged by these authors, that when so many of these factors co-exist with centralization, they turn out to be unimportant and in need of no specific attention, providing the patient is treated with centralizing directional exercises and posture strategies (Werneke 01, Long 04). In other words, in the presence of centralization, most, if not all, other factors “melt away,” becoming irrelevant and therefore unimportant when centralization is treated appropriately.

P. 14: Treatment for centralization is always determined by patient clinical assessment, i.e. what means of mechanical intervention is required to centralize the pain? Therefore, distraction manipulation is never a treatment for centralization unless, after a full MDT evaluation was completed with no centralization elicited, centralization occurs with a test application of distraction-manipulation techniques. Therefore, the first step in the future research of distraction-manipulation would be to determine whether non-centralizing patients, determined after a full MDT evaluation, and when then "tested" with distraction-manipulation techniques, turned out to be centralizers after all. There may be a few, in which case distraction-manipulation treatment would be indicated, but no data exist to support this as yet. Anyone determined to be a centralizer during the application of a routine, thorough MDT evaluation would not be a distraction-manipulation candidate.

P. 14: “It seems reasonable to consider manipulation as a treatment of choice in the presence of segmental pain signs, and to explore this from a research perspective.” Such treatment would not be reasonable in any one in whom centralization and SPP co-exist. And I have discussed that I and the authors agree that this is likely a large subset of those with SPP. The authors’ suggested treatment-outcome research, but this would
be premature before establishing adequate assessment/reliability data for segmental pain signs and whether they are distinct from, or commonly accompany, the reversible pain source responsible for centralization.

P. 15: Why is Z-jt injection efficacy research indicated before Z-jt reliability research is done? Laslett is the only one who has been working to discover how to diagnose Z-h.t. pain clinically, with controversial success at best.

P. 15: Re. "Neurodynamic signs. In the acute stage, especially with disc protrusion, radicular pain is thought to be largely chemical, as a result of inflammation [97]."
But Chen's work showed the inflammation was due to disc nuclear material being placed on the nerve root. The inflammation was clearly secondary to the initial mechanical placement (or displacement) of nuclear material onto the nerve root. So the root cause was this (dis)placement. Therefore, mechanical correction or removal of that nuclear insult stops what is a secondary inflammatory response.
Centralization in acute sciatica is actually very common, just not widely reported yet. To imply its absence in acutes however would be unwise. Acute sciatica was included in the Donelson et al (1990) original centralization paper as well by Skytte et al (Skyte et al, Spine 2005) who reported a 42% prevalence in subacutes.

P. 15: Re. neural mobilization. Many chronic patients that are non-centralizers are reliably categorized into other MDT subgroups (Razmjou 00, Kilpikoski 02, Clare 05) such as irreducible derangements, dysfunctions, etc. Therefore, contrary to the authors’ statement, neural mobilization is actually inappropriate treatment for most non-centralizers, with the exception of an adherent nerve root, a subset of the MDT dysfunction subgroup.

P. 16-17: Just like with the Z-jt, why does research into the treatment of muscle pain, dynamic instability, or CPH precede reliability research into the presence or absence of any of these findings? Reliability must be established before any subgroup treatments and outcomes are studied (Spratt ADTO, 03).

P. 17: “It appears that fear and catastrophizing are directly related to CPH.” What is the basis for this statement? The subsequent statements in that paragraph seem to be pure speculation. And shouldn’t the current final statement in that same paragraph instead be the first statement in the paragraph?

P. 19: Every experienced MDT clinician estimates that the prevalence of centralization in neck pain is just as great as in the lumbar spine. (See data below on cervical derangements) If the authors are satisfied with estimates in other places in their paper, why not here? And what if the authors’ experience differs in this regard from the fully trained MDT clinicians? Point that out, but it would not be wise to ignore the more experienced clinicians’ estimates and go with the authors’.

P 22: Once again, the authors state that “many patients have a variety of factors involved in his or her clinical picture”. This statement again contradicts the data. Again, most have been shown in multiple studies to be centralizers, i.e. reducible derangements. Their pain centralizes or abolishes with a directional preference, independent of any other clinical signs found, including so many of the other findings listed and described, and also independent of pain duration (acute, subacute, or chronic) and location (LBP only, vs. sciatica, even with neural deficit) (Long 04). If their pain centralizes and/or they have a dir. pref., they’re going to do well with the indicated mechanical self-care, as shown in at least 8 cohorts and now four RCTs (Schenk, Long, Brennan, and Browder).
What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of outstanding merit and interest in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I have been a consultant for the McKenzie Institute International in the past and have recently published a book on centralization of low back pain entitled: "Rapidly Reverisible Low Back Pain". (I do not feel either of these represents competing interests.)