Author's response to reviews

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

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Author's response to reviews: see over
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Dear Editor:

We appreciate the opportunity to respond to the reviewers’ queries. One commonality of all the reviews is the request for more detail as well as comparing and contrasting our diagnostic scheme with classification systems that have been previously published. In responding to the reviewers’ queries while using the fewest words possible, the word count of the manuscript increased from 2805 words to 4054 words. We realize that the current word count exceeds the limit placed on our original submission; however, we feel that the revisions based on the reviewers’ comments have greatly improved the paper. We hope you will agree. Thank you very much for considering our revised manuscript for publication.

Regarding comments from reviewer Donelson:

1. “No data has been published to support the authors’ implication that segmental pain provocation represents a different type of LBP than centralizing pain, or that segmental pain provocation occurs only in non-centralizing patients.” There are a number of studies that have demonstrated the presence of segmental pain provocation in patients with spinal pain, some of which we cite in this paper, and the remainder of which will be cited in our subsequent systematic review. Whether and how commonly this occurs in centralizing and non-centralizing patients remains to be determined, and this is part of our research plan.

2. “extensive clinical experience” suggests that segmental pain provocation signs are common in patients who exhibit centralization signs. This is consistent with our experience, but thus far remains on the anecdotal level. It is part of our research plan to investigate this impression.

3. “……there should be discussion of the reliability testing for detecting trigger points, which has been reported as poor.” It was not the intention of this paper to report reliability or validity data; this is the purpose of our subsequent systematic review. We will present the data on reliability of trigger point palpation in that paper.

3. “…there is again anecdotal evidence amongst those who examined for both trigger points and centralization in the same patient that trigger points commonly disappear when pain centralizes with repeated end-range testing.” This is consistent with our experience, but we did not find any data on this in the literature. Thus we did not include this anecdote in the paper.

4. “There is also evidence that depression, fear avoidance beliefs, passive coping and overt pain behaviors commonly co-exist with the finding of centralization, but, when centralization becomes the focus of treatment, all these other factors simultaneously
disappear and become non-predictors of long term outcomes...”. This is not unique to treatment that focuses on centralization, but has been shown with positive response to other somatic-based treatments, i.e., if somatic-based treatments successfully reduce pain, fear, catastrophizing, depression and pain behavior often improve. It is our opinion that treatment according to centralization signs is especially powerful in this regard, but we did not find any data that compared this phenomenon between directional preference-related treatments and other somatic based treatments. Nonetheless, we have added a general statement about this to the text.

5. “The authors write of ‘the absence of definitive objective diagnostic findings in the majority of spinal pain patients’. This statement disregards centralization and directional preference....” With this statement we were referring to special tests such as imaging and blood work. We have changed this accordingly.

6. “Pg 13, Par 4, Li 1: This is a good example of a flawed conclusion drawn from unfamiliarity with how very commonly segmental pain provocation signs (SPS) coexist with centralizing pain.” We did not find any evidence as to how commonly SPS coexist with centralization, other than in the sacroiliac area, which we discuss in the paper. This would be a very interesting study to do in other areas of the spine but, to our knowledge, it has not yet been done.

7. “Positive SPS, or any other signs described in this paper for that matter, should always be trumped by the presence of centralization and directional preference. Why unnecessarily risk depend dependency on manipulation?” It is our clinical practice to address centralization signs before the others, although we are not aware of any definitive evidence that this is the best approach. Nonetheless, we have added this to the text, and have changed the sequence of the signs to have centralization appear first, reflecting the commonness of this sign and our impression as to the importance of addressing this sign first.

8. “The authors state rather categorically that radicular pain is thought to be largely chemical as a result of inflammation. Yet published data in patients with sciatica reveals that a large percentage of patients (50%?) with sciatica are centralizers..... If inflammation played such a big role in radicular pain, why would it abate so quickly in these individuals as a result of a mechanical intervention?” Our statement that radiculopathy is thought to be largely chemical applied to the acute stage only. The only study of which we are aware that found centralization in patients with documented radiculopathy was that of Kopp, et al (Clin Orthop Related Res 1986). While this study did not provide the duration of symptoms for each case, subjects were only included if they had received at least 6 weeks of “conservative care”. As such, it seems unlikely that these subjects were in the acute inflammatory stage of radiculopathy. Nonetheless, we have made mention of the commonness of centralization signs in radiculopathy patients in that section.

9. “With every category mentioned, due to the high success rate with self-care in centralizers, there is great value that being certain that centralization and
directional preference are tested first...” Again, we have made changes in the manuscript to reflect this.

Regarding comments from reviewer Childs:

1. “The authors present a mixed message about the value of traditional pathoanatomic vs. treatment-based classification systems.” We have made alterations in the text to attempt to clarify this issue. We were unable to elaborate due to word count limitations, but this model was developed in a similar way to that of the International Classification of Functioning, Disability, and Health (http://www3.who.int/icf/) in which it is recognized that traditional tissue-specific diagnoses alone are inadequate for making treatment decisions, but this does not mean they should be completely forgotten about. There is some evidence (though much of it controversial) that clinical means can be used to identify pain arising from the SI joint, disc and nerve root. As research evolves, we may find that painful tissues can be detected with more certainty (but, of course, there is no guarantee of this). But this will not happen if we completely ignore the possibility of a detectible painful tissue. This model attempts to allow the clinician to be open to painful tissues, but to not be dependant on knowing for certain what the painful tissue is in order to make diagnoses upon which to base treatment decisions. This is certainly not a perfect way to go about it, but no approach that has been developed has proven to be perfect.

Thus this is a diagnostic strategy that focuses on how the patient is functioning and what the barriers are to their functioning “normally” or in a way that allows him or her to live a happy and productive life, but which is also open to the fact that, in the majority of patients, there likely is a painful tissue. How confident we can be in identifying the painful tissue may vary from patient to patient. Thus, how much emphasis is placed on the painful tissue must also vary from patient to patient. The model allows for this, because it allows for individual treatment decisions to be made.

2. “The authors appropriately add the consideration of psychosocial factors in their model, but these factors are mixed with other factors more commonly classified as physical factors. The authors should clarify their rationale for aggregating these factors together...Why not include this category within the second question related to ‘origin’?” The distinction between question #2 and question #3 is not one of physical factors vs. psychosocial factors. It is one of signs reflective of the purported source of nociception vs. factors that are suspected to perpetuate the chronic pain experience. In the model we are presenting, based on the literature, those factors that tend to perpetuate chronic spinal pain involve biomechanical (dynamic instability), neurophysiological (CPH, oculomotor dysfunction) and psychological (fear, catastrophizing, passive coping) processes. These are grouped together as suspected perpetuating factors, even though they encompass different dimensions.

We agree that the argument could be made that instability could fit into the “source” category, but it seems to be more sensible to us to consider it a perpetuating factor, at least until further research clarifies the role (if any) this condition plays in spinal pain.
We have added wording to the text that clarifies this within the bounds of word-count limitations.

3. “The authors ignore a large body of existing evidence supporting some of the very ideas that the authors propose as ‘novel’…”.

We are aware of this body of literature, and our intention was not to ignore it. However, word-count limitations did not allow us to review this literature. We agree that this would be important to do and have added a section that addresses this, while trying to avoid greatly exceeding the word-count limitations.

4. “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?” There are situations in which being able to have a suspicion about the painful tissue can be useful. For example, in a patient who does not exhibit centralization signs or neurodynamic signs but does exhibit segmental pain provocation signs, and who does not respond as expected to treatment, one might consider trying joint injection to determine the symptomatic response to this and possibly extended symptomatic relief. The speculation that segmental pain provocation signs may be reflective of joint pain would be useful in making the decision to recommend joint injection rather than, say, epidural steroid injection. Another example would be the patient who presents with orofacial pain and headache. It is thought that pain in the face and head can be produced by myofascial trigger points in the sternocleidomastoid muscle. A patient presenting with a pain pattern that fits the suspected referred pain pattern of this muscle may cause the clinician to examine this muscle for suspected trigger points. While there is no definitive proof that trigger points occur in the SCM muscle, the presence of the typical pain pattern may alert the clinician to examine this muscle.

Regarding comments from reviewer Manchikanti:

Abstract
Background
1. We have eliminated this sentence.

2. This sentence has been changed.

Body of Text
1. We have added this reference.

2. We agree that this would be a good idea, but word count limitations did not allow us to review these concepts.

Reviewer Manchikanti states, “The authors may also want to reference earlier in the manuscript many other authors who have attempted or continue to attempt to develop various diagnostic strategies but are referenced later in the manuscript.”
We have added a section that expands our review of other diagnostic strategies, although it seems that this best fits toward the end of the paper, after the DBCDR is presented.

Reviewer Manchikanti states:
“Discussion
The authors should describe Methods at this level, not the Discussion.” We have followed the format required by the journal, which only includes Background, Discussion and Summary sections (for the type of article submitted).

“Much of the material included in this Discussion section may be moved to the Background section and subsequently, a detailed explanation may be provided in the Discussion section if they desire.” We do not completely understand this, but we have revised the Discussion section in an attempt to make it easier for the reader to follow.

“This entire Discussion section is extremely confusing. The authors at times quote treatment modalities, for example sometimes making statements on page 14, paragraph 1, second sentence – ‘radiofrequency denervation is not a last-resort treatment for patients with segmental pain provocation signs.’” We are again not entirely clear on what the reviewer means here. We are not sure what the review finds confusing. Also, we do not state, “Radiofrequency denervation is not a last-resort treatment for patients with segmental pain provocation signs.” We state, “Radiofrequency denervation may be a last-resort treatment for patients with segmental pain provocation signs.” We discuss treatment modalities in the part of the paper in which we describes some of the treatments that are available for each of the clinical factors in the DBCDR. Again, we are not sure where the confusion lies, but we have revised the paper in an attempt to make it more easily understood in general.

“Summary
Following the Discussion section, they go into the Summary, which may be appropriate if they used the Methods section prior to going into the Summary.

This may be as a Conclusion in a shorter version” Again, we used the format required by the journal, which does not include a Methods section.

“The authors may want to mention the pitfalls of present diagnostic modalities including so-called objective tests as the authors describe in the future.” We are not sure what is meant by “…as the authors describe in the future”, but we have mentioned this within the bound of word count limitations.