Reviewer's report

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

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Reviewer: Ronald Donelson

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General

I congratulate the authors for addressing what many feel is the most important research need in the field of low back pain (LBP): an evidence-based means of examining and classifying individuals seeking care for LBP. This paper presents their ‘theoretical model of a diagnosis-based clinical decision rule”. They plan to present the evidence supporting this model in a subsequent manuscript as well as the additional research required to better support or refute their model.

Pg 6, par 2, li 4: I am in agreement with them that “there is good evidence that historical and examination procedures can allow one to identify certain characteristics in each individual patient that may be useful in making treatment decisions.” However, I have concerns about the basis for their classification system in a number of places.

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

Pg 6, Par 3, Li 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. To the contrary, there is extensive clinical experience that segmental pain provocation signs are elicited commonly in those whose pain centralizes or abolishes with directional testing and therefore exhibit a directional preference. Future studies that focus on this question will likely report what so many clinicians have already found. This overlap of findings has not been reported because any pain provocation signs are routinely found to be irrelevant in determining and carrying out treatment in patients whose pain centralizes.

Pg 7, Par 1: The authors properly reference Laslett’s work regarding pain provocation testing of the SI-joint, including his important finding of first examining for centralization, felt to be related to disc-generated pain, that then assists in interpreting any subsequent positive SIJ pain provocation signs. In the presence of centralization findings, any positive SIJ tests were felt to be false-positive since such patients then had positive discograms but negative SIJ diagnostic injections. This all suggested that pain-generating discs can cause “referred tenderness” to the SIJ as the basis for positive SIJ pain provocation tests yet negative SI diagnostic injections. This concept of “referred tenderness” is an important consideration in subsequent parts of this manuscript as well.

Pg 8, Par 2: In keeping with the “assessment-diagnosis-treatment-outcome research model” mentioned in para 2 of page 2, there should be discussion of the reliability testing for detecting trigger points, which has been reported as being poor. While the authors intend to save their discussion of the evidence for a subsequent manuscript, they should be cautious in, or even avoid, using tests and categories in their classification system that have demonstrated poor reliability. The clinical value of unreliable tests in classifying patients is nil.

Pg 8, Par 2, Li 1: “Muscle palpation signs” essentially mean tenderness, or detecting so-called trigger points. Not only is reliability reported to be suspect or poor, there is again anecdotal evidence amongst those who have examined for both trigger points and centralization in the same patient that trigger points commonly disappear when pain centralizes with repeated end-range testing. While no data have been published to my knowledge, “muscle palpation signs” may commonly represent just another application of “referred tenderness” from some central pain source, i.e. the disc, that beneficially responds to repeated end-range testing that centralizes the pain and simultaneously eliminates the trigger points. The authors may wish to rethink these “muscle palpation signs” in light of both the lack of test reliability and the possibility that trigger points are commonly a form of referred tenderness (false positive muscle tests)
that can be quickly eliminated in centralizing patients. In such patients, the disc may be a more likely pain-generator than the muscles themselves.

Pg 11, Par 2-4: There is also evidence that depression (Long 04 and Werneke 01), fear avoidance beliefs, passive coping, and overt pain behaviors (Werneke 01) 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 despite being reported as predictors in many other studies, none of which included testing for centralization at baseline. The irrelevance of these psychosocial factors in the presence of centralization identification and treatment is believed to be primarily due to the dominant, positive, and reassuring influence on patients when they discover they can learn how to, and that they can, control and eliminate their own pain.

Pg 12: The authors don’t report the extensive evidence of how commonly centralization and directional preference are elicited during assessment: 70-80% of acute patients and 50% of chronic patients. They likewise don’t cite how routinely these patients achieve good-to-excellent outcomes when treated according to their directional preference. There is value in doing so because of the very important consequences, as well as further research to be done, relevant to the formulation of this diagnostic classification system being proposed.

When further studies are completed, similar to Werneke’s on centralization’s interaction with psychosocial factors, and Laslett’s on centralization’s interaction with referred tenderness to the SIJ, these authors must be prepared to alter their classification significantly, either now or in the future, if, as anticipated, future research reports that a large number of pain centralizing patients, if treated appropriately, will have excellent outcomes despite commonly exhibiting one or more of the findings associated with each and every other category mentioned in this paper: segmental provocation, neurodynamic, and muscle palpation signs, or signs of dynamic instability, CPH, oculomotor dysfunction, or any of the psychosocial findings mentioned. Except for the work of Werneke and Laslett, none of these other clinical findings have been studied in patients were also examined for the presence or absence of centralization and directional preference to know either the validity or irrelevance of these various findings in those who could centralize and abolish their own pain.

Pg 12, Par 1, Li 4: 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 as clinical findings shown to be definitive and very objective (high reliability in numerous studies) and have validity (numerous observational cohorts and now 3-4 RCTs demonstrating their role in producing good or excellent outcomes), and they are very commonly found in both acute and chronic LBP.

Pg 13, Par 4, Li 1: This is a good example of a flawed conclusion drawn from unfamiliarity with how very commonly segmental provocation signs (SPS) coexist with centralizing pain. How can we justify manipulating a centralizer who has shown the ability to recover rapidly using self-treatment determined by directional preference principles? 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 in light of the patient empowerment for efficacious self-care so widely reported. Why unnecessarily risk dependency on manipulation?

And if SPS commonly co-exist in centralizers, then why would we conclude that these signs are reflective of zygapophyseal pain when we have strong evidence that centralization and directional preference are related to the disc? Why wouldn’t SPS occur as a result of moving a segment containing a pain generating disc rather than be relegated only to zygapophysyal joint pain, as implied by these authors?

Pg 14, Par 4, Li 2: 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, meaning that they can eliminate their leg pain with repeated mechanical loading in a single direction, often in a matter of a few minutes during an initial assessment session. If inflammation played such a big role in radicular pain, why would it abate so quickly in these individuals and as a result of a mechanical intervention? While there is no doubt some inflammatory reaction to the pain-generating mechanical process, don’t these common and rapid changes indicate a fundamental role of mechanical/positional, rather than inflammatory, changes?

In those whose pain cannot be centralized, one could make a case for trying anti-inflammatories, but they are unnecessary in most that can centralize and abolish their pain mechanically.

Pg 15-16: With every category mentioned, due to the high success rate with self-care in centralizers, there is great value in being certain that centralization and directional preference are tested first and, if present, then dictate initial treatment, independent of whatever other tests are positive. If one acknowledges the benefits of empowering patients to eliminate their own pain in order to avoid any
need for medication, manipulation, injections, imaging, and surgery, one must not only exhaust the efforts to identify centralization during the initial assessment, but, if found, place its treatment at the very top of a hierarchy of treatment options that, at least initially, ignores other physical signs like SPS, + SIJ tests (Laslett), and psychosocial findings (Werneke 01, Long 04) until pain-controlling treatments are carried out using individualized treatments and education of the patient. Those other co-existing findings so often either disappear and/or become completely irrelevant once the pain has centralized and abolished.

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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)

Pg 6, par 2, li 8: “Table 2” is an unclear reference for the authors’ “4 signs of greatest importance” since Table 2, as presented, pertains to five provocation signs.

Pg 7, Par 2, Li 7-9: the studies supporting centralization as an outcome predictor are numerous and there are now more RCTs than just the Long study that demonstrate the best treatment for this subgroup.

Pg 9, Par 1, Li 7: “Table 2” reference should be “Table 3” I think.

Pg 10, Par 3, Li 1: Some further background and definition of both normal oculomotor reflexes and impaired reflexes in chronic neck pain would be helpful.

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Discretionary Revisions (which the author can choose to ignore)

Pg 2, par 2, li 4: Perhaps the review and application of the “assessment-diagnosis-treatment model” is planned for a subsequent manuscript, but it is not mentioned again in this manuscript after it appears in the abstract.

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 importance 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:

1. I have been a paid consultant and faculty member with the McKenzie Institute, a non-profit educational and research organization.
2. I have recently published a book, entitled "Rapidly Reversible Low Back Pain", that comprehensively discusses the role of pain centralization and directional preference in classifying low back pain patients. One of these authors (D. Murphy) contributed to the Foreword to that book.