Responses to reviewer Childs:

We understand that reviewer Childs is fundamentally opposed to a biomedical model for LBP and we appreciate his open-mindedness in light of this. Indeed, the anticipation of this level of open-mindedness and scholarly integrity was the very reason we requested he be asked by the journal to review our MS.

Many of reviewer Childs’s concerns are the subject of future research and are not able to be addressed in this preliminary theoretical model paper.

We have tried to make it clear, and have tried to further clarify, that the approach to diagnosis we are presenting is not dependant on knowing the precise pain generating tissue in each case. It allows for speculation about the possible pain generating tissue, but treatment decisions can be made regardless of how confident one is in the pain source.

1. Reviewer Childs states, “The major remaining concern I have is that the title and ‘decision rule’ language throughout the paper…” Reviewer Childs did not report having a concern about this when he reviewed our MS the first time. Although perhaps not a perfect fit, we believe that “Diagnosis-Based Clinical Decision Rule” accurately reflects the approach we are presenting. We do not think it is scientifically inappropriate. In addition, this “decision rule” language has been used by us in a number of previous papers [1-3], so it has a precedent in the literature. None of the reviewers of those papers expressed concern about our use of decision rule terminology to describe the approach. The purpose of this paper is to propose an approach to diagnosis and management that may be useful to the clinician in helping make decisions. It is based on evidence that we present in more detail in a future paper. We realize, as reviewer Childs points out, the need for further investigation, and this is the topic of another future paper. Word count limitations did not allow us to go into this in greater detail. Nonetheless, we have tried to further clarify this in the text.

2. “…implies that the classification system may be useful for decision-making, when this is an early theoretical proposal.”

We have tried to make it clear that the DBCDR is not a “classification system”, but we have made revisions (within the imposed word count limitations) to the text to try to make this more clear.

Further, we do feel that this thought process may be useful to clinicians. Actually, clinicians are already using the various components if this approach. None of the individual approaches is new. We have merely found the commonalities of various approaches, examined the evidence behind these and developed a thought process by which these approaches can be applied.
3. Reviewer Childs states, “Much of the language in the discussion is overly enthusiastic when there are no data at this stage. This is not a fatal flaw by any means given the purpose of the paper. However, readers walk away from the paper wondering whether they should be implementing some of these ideas tomorrow in their practice. The tone should be adjusted to reflect the early development stage of this system.” We have tried to adjust the tone as much as possible, and just present the model as it is. However, as we stated earlier, the individual components of this model are not new, and clinicians are already using the methods in clinical practice. Clinicians can choose to wait until all the evidence has been gathered before attempting to treat patients with spinal pain, or they can apply the current knowledge to the best of their ability. We have simply examined the evidence and developed a thought process upon which clinicians can base their approach.

4. Reviewer Childs states, “This implies that there could be infinite combinations of patterns, each of which should be matched to a tailored approach. The problem of LBP is admittedly multifactorial, but there is far too much variability in clinical practice patterns for the same patient with LBP. How does this system propose to deal with the variability problem given the increasing evidence to support the ability to identify homogeneous sub-groups that can be matched to a finite number of optimal treatments?” The research that is attempting to identify homogeneous subgroups that can be matched to a finite number of optimal treatments may turn out to reveal the best way to approach patients with spinal pain and it may not. We feel that at this point in the evolution of that research, it is not certain. However, if it turns out that spinal pain patients cannot, in fact, be reduced to 4 subgroups, but, rather, spinal pain is more complicated than that, there will be the need for an approach that considers this complexity. While the subgroup research is being carried out, we do not see why research should not also be conducted that takes a different approach to spinal pain patients. The lead author of this paper is a full-time spine clinician; if the evidence ever becomes clear that the homogeneous subgroup system is best for managing patients with spinal pain, he will abandon the approach being presented here in favor of that one. But, again, it seems reasonable to investigate other approaches to the diagnosis and management of spinal pain patients, considering there is the distinct possibility that these patients (or at least a currently unknown percentage of them) cannot be fitted into 4 groups.

6. Reviewer Childs asks, What are the proposed treatments for each pattern in this system? How many possible patterns are there?” The proposed treatments are provided on MS pages 14-17. The number of possible patterns is the question for which an answer is being sought in current cohort studies. It is the experience of the lead author that, while a large number of possible combinations of diagnoses are theoretically possible, there is significant overlap across patients and a more limited number of combinations is seen in reality. However, this has not been systematically evaluated and, again, is the purpose of current research efforts.
7. Reviewer Childs states, “Any classification system that is developed has to have an element of pragmatism that makes its application to busy clinical practice in an environment of constrained costs a reasonable endeavor.” We will reiterate that what is being proposed here is not a classification system. Also, the lead author has been a full-time clinician in a busy practice for nearly 20 years, and has been applying the DBCDR in a busy practice environment (and one with constrained costs) for most of that time. In addition, he has been teaching this approach to other clinicians for much of that time. It is his experience that this approach is quite practical. However, that remains purely on the anecdotal level and requires investigation. If, upon further investigation, it is discovered that the approach is not as practical as the lead author’s experience would suggest, the DBCDR will be modified or abandoned.

8. Reviewer Childs states, “It is unclear how this effort will overcome the futility of previous pathoanatomically systems.” Again, this is not a pathoanatomically based system. It is not necessary to know the pathoanatomical mechanism of pain in each case in order to apply the thought process being proposed with the DBCDR. Word count limitations do not allow us to elaborate on how dramatically different a diagnosis of, say, “centralization signs with dynamic instability and fear beliefs” is from a diagnosis of, say, “degenerative disc disease” or “spondylisis”. However, we expect that the difference will be clear to the reader.

9. Reviewer Childs states, “The costs associated with pathoanatomic classifications systems are not inconsequential”. We agree. Again, the DBCDR is not a pathoanatomical approach nor is it a classification system.

10. Reviewer Childs states, “The pursuit of ‘pathoanatomical lesions’ is also known to increase pain related fear, pain catastrophizing, and may predispose patients to an increased risk of unnecessary diagnostic and surgical procedures. I am not trying to oversensationalize this issue, but there are very legitimate concerns that pathoanatomically based classification systems for managing spinal patients actually result in more harm than good and contribute to [the] growing problem of chronic LBP. The authors should acknowledge these concerns and in their discussion and discuss how their system overcomes them.” The DBCDR is not expected to “increase pain related fear, pain catastrophizing”. On the contrary, the identification and management of fear and catastrophizing is an important component of the DBCDR. Also, preliminary evidence [1-5] suggests that when patients are treated according to the DBCDR, not only does this not result in “more harm than good”, but positive outcomes are found. In those studies in which the DBCDR was applied and fear beliefs were measured [2, 5], fear actually improved.

Nonetheless, if it is found upon further investigation that, contrary to the preliminary evidence, treatment according to the DBCDR does result in “more harm than good”, the approach will be abandoned. Our clinical experience and the current literature on the methods involved in the DBCDR strongly argue against this likelihood. But, objectivity and open-mindedness being an essential component of good science, we will examine our findings from future studies carefully.
11. Reviewer Childs states, “The authors should make it clear throughout the paper that their proposed system is addressing the non-surgical management of patients with spinal pain.” This is repeated a number of times throughout the text, but has been added to the title.

We greatly appreciate reviewer Childs’s thoughtful comments and questions, and we feel they have allowed us to significantly improve our paper.

REFERENCES

Responses to reviewer Donelson:

We appreciate reviewer Donelson’s enthusiasm for the McKenzie approach. We are enthusiastic about this approach as well, but we are also aware that it does not provide all the answers to all patients with spinal pain.

1. Reviewer Donelson states, “It is essential that manuscript readers receive a better balance between the evidence and the gaps in this model, even at this introductory stage.” Word count limitations do not allow us to elaborate on the evidence gaps in this model. It is for this reason that a part 2 was necessary.

2. Reviewer Donelson states, “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.” Again, word count limitations did not allow us to go into any detail about this. But this sentence has been changed.

3. Reviewer Donelson states, “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.” Reviewer Donelson only refers to studies on trigger point palpation that have shown lack of reliability, but does not acknowledge that there are other studies that have found good reliability. Perhaps he is not aware of these studies, but, we will be presenting all the evidence regarding not only reliability but also validity in a future paper. Word count limitations do not allow us to do that here.

In addition, we do not feel it is inappropriate in this presentation of a theoretical model to discuss treatment before everything that needs to be known about reliability and validity has been discovered. We note that later in his review Reviewer Donelson discusses McKenzie concepts such as “Dysfunction Syndrome”, “Postural Syndrome” and “Adhered Nerve Root”. These are widely written about and taught in spite of the absence of evidence of their validity (there is evidence of reliability, but this is only one aspect of investigation of diagnostic utility). In fact, reviewer Donelson discusses these concepts in his “evidence-based” book 1. We do not feel it is inappropriate for reviewer Donelson or the McKenzie Institute to do this, nor is it inappropriate for us to present treatment concepts here.

Again, investigating the gaps in the research knowledge is part of our long term plan.

4. Reviewer Donelson states, “Many patients with segmental pain provocation (SPP) are also centralizers.” This is the second time reviewer Donelson has made this claim without evidence. As we stated in our previous responses to reviewer Donelson,
the co-existence of SPP and centralization is consistent with our experience, but it requires investigation. In the meantime, we do not see why it is important to include this unfounded statement in our paper.

5. Reviewer Donelson states, “This is an important gap in this system, acknowledged by the authors (see their answer to my Q2).” We did not acknowledge that this was “an important gap in this system” and do not feel that it is, considering there is no hard evidence for it, except in the case of the sacroiliac joint, which we discuss in the paper.

6. Reviewer Donelson states, “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.” As we stated in our previous response to reviewer Donelson, we have found no data comparing centralization responses and SPP. This may or may not be “likely a large group”, but there is no evidence either way. As we also have already stated, investigation will be required to determine the relationship, if there is any, of SPP and centralization, and this is part of our research agenda.

7. Reviewer Donelson states, “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.” This is incorrect. There is evidence in the cervical spine to link SPP with facet pain and we have cited this evidence. This is not definitive, but we feel it is reasonable to speculate that this may be the case. Based on this, we also feel it is reasonable to speculate that SPP may be linked with facet pain in the lumbar spine as well, but, again, this requires investigation. Nonetheless, we have found no evidence to link SPP with disc pain or any other pain source.

8. Reviewer Donelson states, “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.” We do not feel that it would be appropriate to mention our clinical experience with regard to this and not with regard to every other aspect of the paper. In the interest of attempting to remain within word count limitations, we have eliminated all references to our experience.

9. Reviewer Donelson states, “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 especially 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.” As we stated in our previous responses, and in the paper, improvement in psych factors with a purely somatic approach has been demonstrated independent of the examination for centralization signs, contrary to reviewer Donelson’s insistence that this somehow is unique to the McKenzie approach. And there is no evidence that treatment according to centralization signs is “especially powerful in this regard”, despite our and reviewer Donelson’s biases. For this reason, we have elected not to include this anecdote in the paper.

10. Reviewer Donelson states, “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 each 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.” We feel that reviewer Donelson’s claim that patients who are centralizers do not have other factors that require attention is not supported by the literature. The RCT studies he cites are short-term and hardly definitive in terms of whether the subjects in the studies had other factors that were contributing to the clinical picture and that required treatment. Studies with two or four week follow up do not allow for conclusions to be drawn in this regard. The cohort studies he sites simply report observations. All these studies are useful in themselves, but do not support reviewer Donelson’s sweeping conclusions about them. Further research is required to evaluate this.

11. Reviewer Donelson states, “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 controversial, 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.” Reviewer Donelson is incorrect in stating that there is little or no evidence that
myofascial pain generation exists. There is a large body of such evidence. The problem with TrP examination in vivo is that there currently is no in vivo Gold Standard for this identification. But we do not feel that it is inappropriate to include in our model the basic science and clinical information that is available.

We find it perplexing that reviewer Donelson, on the one hand, claims that McKenzie practitioners find that myofascial trigger points often coexist with centralization and disappear with McKenzie treatment, and then, on the other hand questions the very existence of myofascial trigger points. It would appear that to reviewer Donelson myofascial trigger points exist when their presence supports his notions about the McKenzie method, but do not exist in any other context.

12. Reviewer Donelson states, “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.” This is incorrect, as we point out in the paper. We cite the systematic review of the literature by Fishbain, et al which concluded that one of the few things that Waddell’s nonorganic signs have been found to be associated with is pain hypersensitivity. Again, word count limitations did not allow us to elaborate on this.

13. Reviewer Donelson states, “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.” We can appreciate this, and agree that the word “diagnosis” and the term “diagnosis –based clinical decision rule” are not perfect. But we feel that it is the best term available that allows us to communicate this approach. The term has been used in other publications (without objection from the reviewers or editors of those papers) and we do not feel it is necessary to try to find a different term at this point.

14. Reviewer Donelson states, “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
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.” This may or may not be true, and we do not feel that reviewer Donelson’s confidence is appropriate in light of one observational study and one very short-term RCT. Further research is required to determine if reviewer Donelson’s enthusiasm for this notion, and for the McKenzie Method in general, is well founded. Nonetheless, even if it turns out that it is true with regard to patients who exhibit centralization signs, this would mean that it applies to, at most, 50-70% of low back pain patients and an unknown number of cervical and thoracic spine patients. It would not apply to the remainder of spinal pain patients.

15. Reviewer Donelson states, “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.” We agree that this needs to be investigated. In the meantime, we do not feel it is inappropriate to suggest that, at least in the case of centralizers whose directional preference is extension, distraction manipulation may be a reasonable thing to try. We base this on the fact that biomechanical studies have found that distraction manipulation affects the disc in the same way that extension does. Nonetheless, we have acknowledged in the text that the utility of distraction manipulation in this regard requires investigation.

16. Reviewer Donelson states, “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.” We state regarding treatment in those with centralization signs, “…these signs are always addressed first, regardless of the presence of other signs.” Nonetheless, at the risk of further expansion beyond word count limitations, we have added a statement regarding this in the section on segmental pain provocation signs.

17. Reviewer Donelson states, “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.” As we have stated, and will review in a subsequent paper, there are data regarding the reliability of segmental pain signs in the cervical spine and SI joints. In the lumbar spine, this has not yet been fully assessed. We do not feel that it is premature
to discuss this now, given the fact that this paper is the presentation of a theoretical model requiring investigation; we are not presenting this model as if all the “A-D” research has already been done. Word count limitations do not allow us to elaborate further on this.

18. Reviewer Donelson states, “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-ht. pain clinically, with controversial success at best.” See our response to the previous statement.

19. Reviewer Donelson states, “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.” A large number of studies have found that nerve root pain with radiculopathy is largely chemical in nature, for example 15-25. Some of these studies found that direct compression or increased pressure within the IVF can play a contributing role, but there are a great number of patients who have compressive lesions demonstrated on imaging who have no pain at all. Nonetheless, it is unclear as to why this is such an issue, as we do not see it as threatening McKenzie concepts. If a patient centralizes, even if acute, he or she is a centralizer, regardless of the purported mechanism of radicular pain. We have attempted to clarify this.

20. Reviewer Donelson states, “Centralization in acute sciatica is actually very common, just not widely reported yet.” If this is true, future research should bear this out. Until then, we see no reason to discuss this unfounded, anecdotal statement.

21. Reviewer Donelson states, “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.” We cannot find anywhere in our paper in which we implied that centralization signs are absent in patients with “acute sciatica”. In fact, we do not use the term “sciatica” in our paper at all. “Sciatica” is not a diagnostic term in our view. If reviewer Donelson means to use this term as synonymous with radicular pain, we would not feel this would be appropriate. Not all pain the leg (i.e. “sciatica”) arises from nerve root pain.

22. Reviewer Donelson states, “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.” We have found no evidence to support the statement that neural mobilization is an inappropriate treatment for most non-centralizers and reviewer Donelson does not provide any evidence to support this claim. The papers that reviewer Donelson cites are reliability studies, not treatment outcome studies. Furthermore, as these are reliability studies they do not allow for any evaluation of the validity of the McKenzie concepts of “irreducible derangements”, “dysfunctions” and “adhered nerve roots”. Because 2 examiners can agree on their perception of the presence of an “adhered nerve root” does not mean that an “adhered nerve root” exists. This is the difference between reliability and validity.

23. Reviewer Donelson states, “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).” We cannot find anywhere in our paper in which we state that research on treatment should precede research into reliability.

24. Reviewer Donelson states, “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.” We have changed the wording to make it less speculative. Actually, speculation is not necessary here, as evidence supports our statements, as we have cited in the text.

25. Reviewer Donelson states, “And shouldn’t the current final statement in that same paragraph instead be the first statement in the paragraph?” No. There is evidence that somatic treatment provided in a cognitive-behavioral context may allow for management of patients without referral for specialized psych intervention. We have changed the wording to reflect this.

26. Reviewer Donelson states, “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’. ” Again, at the request of reviewer Donelson to be consistent in this regard, we have eliminated reference to our clinical experience. The fact remains that, in spite of reviewer Donelson’s unsupported claims about the experience of MDT clinicians, no evidence of the commonness of centralization signs in patients with cervical and thoracic pain was found in our search of the peer-reviewed, indexed literature.

27. Reviewer Donelson states, “P. 20, line 2: “are less uncommon” should be “are less common”.” This has been eliminated.

28. Reviewer Donelson states, “P. 20, para 2, line 2: “many” should be “most,” given the prevalence of centralization alone.” We have altered this sentence.
29. Reviewer Donelson states, “Remember that non-centralizers include irreducible derangements, dysfunctions, and posture syndromes (see above), all shown to be reliably identified in both lumbar and cervical patients (Clare 2005).” We have not found any evidence for the validity of the McKenzie Method concepts of “irreducible derangements”, “dysfunctions” and “posture syndromes”, and reviewer Donelson does not offer any. The paper he cites is a study of reliability, not validity.

30. Reviewer Donelson states, “Your “experience” that a “quite large” percentage of cervical and thoracic patients do not have derangements does not reflect data from experienced MDT clinicians. Their experience is reported in an audit of 170 cervical and 32 thoracic patients where derangement prevalence was 80% and 67% respectively (May S, Classification by McKenzie’s mechanical syndromes: report on directional preference and extremity patients, International Journal of Mechanical Diagnosis and Therapy, Vol 1, No 3:6-10). This also does not include the other reliably identified MDT subgroups found by using end-range repeated test movements while monitoring patterns of pain response. No other methods of assessment are needed for these patients that comprise the great majority of lumbar, cervical, and thoracic pain, with distinct and identifiable treatment needs.” Again, we have eliminated reference to our experience, as well as that of other practitioners who have training in the McKenzie method. While the experience of “experienced MDT clinicians” is interesting, no evidence was found in our systematic review of the peer-reviewed, indexed literature to support the claim that 80% of cervical and 67% of thoracic patients exhibit centralization signs. But investigating this is part of our research agenda.

31. Reviewer Donelson states, “No other methods of assessment are needed for these patients that comprise the great majority of lumbar, cervical, and thoracic pain, with distinct and identifiable treatment needs.” As has been the case repeatedly throughout his review, this sweeping statement is not supported by the scientific evidence.

32. Reviewer Donelson states, “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).” Because a certain percentage of patients exhibit centralization signs on examination does not in any way mean that this one clinical factor is the only one that is involved in any individual case. And while the results of a few short term RCTs are promising, they do not support the notion that
because a patient is a centralizer there are no other factors that could possibly be significant enough to require assessment and management. One simply cannot draw the kind of sweeping conclusions about the McKenzie method that are being drawn by reviewer Donelson on the basis of a single RCT that had a two week follow up period.

We appreciate reviewer Donelson’s input regarding the McKenzie Mechanical Diagnosis and Treatment method. Again, we are also enthusiastic about the McKenzie approach and the promise it holds as an important tool in the management of patients with spinal pain. If our investigation of this clinical model leads us to compelling evidence that no other method of assessment and treatment is required in helping patients overcome spinal pain, the remainder of the model will be abandoned. We do not feel the current evidence supports this. In the meantime, we will set about the process of investigating this entire model of approach for patients with spinal pain.

REFERENCES