

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Differences among primary care patients with different mechanical patterns of low back pain: a cross sectional investigation
AUTHORS	Della Mora, Lauren; Perruccio, Anthony; Badley, Elizabeth; Rampersaud, Y. Raja

VERSION 1 - REVIEW

REVIEWER	Birgitta Widerström The department of Neurobiology, Care Sciences and Society, division of Physiotherapy, Karolinska Institutet, Sweden
REVIEW RETURNED	25-Jul-2016

GENERAL COMMENTS	<p>This study is well-written and interesting in its effort to develop a better understanding of the different phenotypes (clinically important subgroups) of LBP. The study used a large sample of patients with LBP from a large number of primary care physicians and nurses in Canada. As such it provides a good description of these patients with respect to patients reported demographics, BMI and co-morbidities and subgrouping approach. However, I have a few comments the authors might consider to increase the clarity of the paper and there are information that need to be added before its clinical value and implications can be judged.</p> <p>Abstract The result section lacks ORs and confidence intervals for “male, overweight and association with P3 and P4” and “age and P2and P4” line 27-30 What this study adds This section needs to be re-written. It is not new that LBP is a heterogeneous and complex condition and this study has not added information on improved health care outcomes.</p> <p>Introduction From a reader`s perspective there is a need for a better explanation of the Hall classification approach used in the study, content and purpose. Further, it needs to be clarified how this approach has implications for the care of LBP patients provided by other health care professionals such as physicians and nurses. To my understanding the Hall classification approach was developed for physiotherapy intervention and as such it has good clinical implications in being inclusive of most patients with LBP, simple and easy to understand and having a direct influence on physiotherapy management. I am fairly sure the authors have ideas on this, however, the implications that the presented four pain patterns may have on medical treatment or nursing need to be clarified.</p>
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	<p>Method</p> <p>How come/it needs to be clarified why/that patients with a LBP duration over 12 months were not likely to improve by involvement in the actual interdisciplinary health programme (ISAEC) (Line 27)? Inter-professional and multimodal interventions is to my understanding what is guideline endorsed in patients with longstanding pain disorders such as LBP. This needs to be better explained.</p> <p>The statement “a trained advanced practice clinician” needs to be clarified (Line 39). Was it one or several? What kind of training in the classification approach had been provided? If I remember it correctly the inter-rater reliability study referred to (ref no 30) included physiotherapists only. The patient assessment made by for example a physician is not necessarily similar to that made by a physiotherapist. Has there been any inter-rater reliability study made on other health care providers?</p> <p>All comorbidities were self-reported from a list that include some severe pathologies. Were the patients’ answers compared to medical journals or other sources of information?</p> <p>Please specify the statistical analyses used in relation to variables and not only as “appropriate” (Lines 35 and 38)</p> <p>Results</p> <p>For clarity it is good to denote all ORs instead of mixing ORs and % and “nearly twice” (Line 31 and 32)</p> <p>Discussion</p> <p>In general, I find the discussion is missing elaborations. For example, LBP and inference to patho-anatomical source is tricky. Imaging studies have indicated that LBP can occur although lumbar anatomy is considered normal, and in reverse, that abnormal lumbar anatomy is not necessarily associated with pain, and it’s unclear whether these abnormal findings are clinical important in LBP and sciatica. Intermittent leg pain classified to P4 may represent neurogenic claudication which may have several explanations apart from osteoarthritis. If inference to pathology is to be done I think the authors should broaden this discussion. Especially since the Hall classification was designed on recognition of patterns of pain with no direct reference to anatomic site or pathological process.</p> <p>As mentioned under my comments on the introduction section I am missing the implications for the Hall classification approach in areas outside physiotherapy. In my view this is important. If an approach is to be used outside its designed purpose this must have a clear rationale. What are the clinical implications for this subgrouping approach other than to physiotherapists (e.g. doctors, nurses, psychologists and other health care professionals) in an inter-professional setting? And what does practical patient phenotypes mean? (line 57). Practical for whom?</p> <p>The result discussion is insufficient. While the result certainly is correct and obviously precise with narrow CIs the associations between age and P2 and P4 is weak with ORs just above 1. The same goes for comorbidities and P2 where the CI includes 1. This needs to be discussed and maybe cautiously interpreted. Further, it is known that peripheral neurological signs and symptoms, e.g. leg pain, motor and/or sensory disturbances, indicative of nerve tissue damage, is associated with disorder severity, prolonged healing process, chronicity, work absence and higher health-care costs. Therefore, is the association between not having good health scores and P3 is not surprising. The co-variation between pain intensity and</p>
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	EQ-5D also need to discussed.
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REVIEWER	Siobhán Stynes Research Institute for Primary Care and Health Sciences, Keele University, United Kingdom
REVIEW RETURNED	03-Aug-2016

GENERAL COMMENTS	<p>The authors are to be commended on carrying out a study that explores the construct validity of a low back pain (LBP) classification system based on pain patterns. The authors have described different profiles within the groups but I am wary of their conclusion (abstract line 37) that this suggests "potentially unique risk factors and underlying etiologies". It is not clear how the findings from this study can advance the field and specifically address the justification of providing subgroup- specific care for LBP patients.</p> <p>Introduction: It would be good to see more background information on classification systems for LBP in the literature, and justification for use of the Hall et al method. Reference 27, where this system has been compared to usual care, is an observational double cohort design study, therefore effectiveness cannot be inferred, for that you need RCTs. Avoid starting too many sentences with authors first name et al (line 31,34).</p> <p>Methods: Clarification needed for terms such as "persistent pain"; "recurring and unmanageable pain"; and "possible pain disorder". The groups only apply to patients with symptoms up to 12 months. I suggest this should be made clear in the title and could you explain why no more than one year?</p> <p>More information is needed, either in the methods or an appendix, on how the patients were classified and if possible some accompanying details of clinical characteristics in the results. A flow chart is needed to illustrate the flow of patients for the study, to see the total pool of potentially available patients and how many were selected and classified. It is difficult to comment on potential selection bias as this information is not presented.</p> <p>"Previous history of back problems" (line 9 page 6) does not seem very informative to compare groups, was any more information available? Where there any psychosocial variables available to describe the groups e.g. anxiety/ depression questionnaires?</p> <p>Statistical analysis: It was not clear why the P1 (back flexion) group was chosen as the referent category for multinomial regression. Is there data from previous work to suggest that this is the least "severe" group? Otherwise would comparing groups with and without leg pain be more appropriate?</p> <p>Results: Lines 23-37. I suggest being consistent with reporting of odds ratios. Avoid phrases like "overweight individuals had 74% greater odds". No need to give p values when reporting confidence intervals.</p> <p>Tables: Table 1; it's unusual to report range with mean, I would expect mean and standard deviation. It's useful to give frequencies with percentages, e.g. for age categories and BMI categories. Be consistent with number of decimal places. For all p-values that are less than 0.001, round off as <0.001 instead of giving precise values. Table 2; Ideally overweight and obese should be analysed separately. Disability (ODI) is not addressed in the multinomial</p>
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	<p>regression.</p> <p>Discussion: Most of the differences between groups, and odds ratios are quite small; this is not pointed out in the discussion. Reasons for this could be explored e.g. does it suggest poor discriminant ability of the classification system? Were the right variables used to describe the groups? If the groups are different by clinical examination, what is the added benefit of them having a different profile in terms of demographics/ pain/ disability? Is it expected that this will effect matched treatments for these subgroups? I would expect to see some more comparison with other similar classification systems from the literature e.g. the Quebec task force classification. The results suggest that those with leg pain are worse on most characteristics, which is not surprising based on existing literature. This could be addressed in more detail.</p> <p>A pathoanatomical source of pain (line 14) is discussed, but if going down this route, why not address that P3 are considered sciatica and P4 stenosis according to Hall et al 2009.</p> <p>Good acknowledgement of limitations of sample source in discussion. This could be shortened to allow more discussion on interpretation of results.</p> <p>Line 8- what is meant by “group-specific risk management” in relation to your results?</p> <p>Based on my comments, I believe this manuscript requires substantial changes, focusing on the primary aim of the study which describes four groups using cross sectional data. The results should be interpreted in light of such findings.</p> <p>A prognostic study would be more informative than cross sectional data. It is hoped that the study team will be able to describe the clinical course of these groups and a prognostic study is strongly encouraged if not already underway.</p>
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VERSION 1 – AUTHOR RESPONSE

Author's Response to Decision Letter for (bmjopen-2016-013060)

Differences among primary care patients with distinct patterns of low back pain: a cross sectional investigation

Response to Editor and Reviewers

We thank the reviewers and editors for their time and comments. We have edited the manuscript in response to these comments and believe we have provided greater clarity and hopefully a better manuscript.

In those instances where our comments may have ‘reached’ beyond what the findings permitted, as kindly pointed out by the reviewers, we have removed these sections entirely and ensured commentary is restricted to the population and data at hand.

Thank you for the opportunity to revise and resubmit.

Reviewer: 1

This study is well-written and interesting in its effort to develop a better understanding of the

different phenotypes (clinically important subgroups) of LBP. The study used a large sample of patients with LBP from a large number of primary care physicians and nurses in Canada. As such it provides a good description of these patients with respect to patients reported demographics, BMI and co-morbidities and subgrouping approach.

RESPONSE: Thank you.

Abstract

The result section lacks ORs and confidence intervals for “male, overweight and association with P3 and P4” and “age and P2and P4” line 27-30

RESPONSE: We have added ORs and CIs to the results section. (p.2)

What this study adds

This section needs to be re-written. It is not new that LBP is a heterogeneous and complex condition and this study has not added information on improved health care outcomes.

RESPONSE: The reviewer is correct; thank you for pointing this out. We have removed the associated comment and added new text, (p.3)

- This study is the first to assess the discriminatory ability of the Hall mechanical LBP classification system in a non-rehabilitation primary care setting.
- In a primary care LBP population, the classification system, consisting of four mechanical low back pain patterns, appears to discriminate between clinically important subgroups with observed differences in demographic and specific health characteristics.
- In particular, this system was able to identify a distinct subgroup with likely spine osteoarthritis, with potential implications for more targeted management

Introduction

From a reader’s perspective there is a need for a better explanation of the Hall classification approach used in the study, content and purpose. Further, it needs to be clarified how this approach has implications for the care of LBP patients provided by other health care professionals such as physicians and nurses. To my understanding the Hall classification approach was developed for physiotherapy intervention and as such it has good clinical implications in being inclusive of most patients with LBP, simple and easy to understand and having a direct influence on physiotherapy management. I am fairly sure the authors have ideas on this, however, the implications that the presented four pain patterns may have on medical treatment or nursing need to be clarified.

RESPONSE: The second half of the introduction has been re-written, with some earlier text removed entirely, to provide greater clarity in this regard. (p.4)

Method

How come/it needs to be clarified why/that patients with a LBP duration over 12 months were not likely to improve by involvement in the actual interdisciplinary health programme (ISAEC) (Line 27)? Inter-professional and multimodal interventions is to my understanding what is guideline endorsed in patients with longstanding pain disorders such as LBP. This needs to be better explained.

RESPONSE: On re-reading this section, we agree it was not clear. We have edited and added to the text in response, (p.6)

“To be eligible for the ISAEC pilot program, patients had to be 18 years of age or older and experiencing LBP-related symptoms that were either persistent, lasting from 6 weeks to 12 months, or recurring (regardless of duration). Persistent pain was defined as constant daily pain

of varying intensity and recurrent pain was defined as episodic pain with a current episode no longer manageable by primary care measures compared to previous episodes. The lower time limit excluded patients experiencing acute LBP that had a favorable natural history, while the upper time limit excluded patients with persistent pain (as per our working definition) in excess of 1 year in duration as this group has a high likelihood of a concurrent pain disorder and poor outcome.⁽³⁴⁾ The principle focus of the ISAEC program is education and enabling patient and primary care providers to utilize chronic disease self-management principles to manage their LBP. Patients were ineligible if they had a work-based insurance claim, were in pain following a motor vehicle accident, had established narcotic dependency (i.e. those actively being managed by a pain specialist), were involved in active litigation, were pregnant/post-partum (<1 year), had emergent spinal presentations, or had an established diagnosis of a pain disorder”

The statement “a trained advanced practice clinician” needs to be clarified (Line 39). Was it one or several? What kind of training in the classification approach had been provided? If I remember it correctly the inter-rater reliability study referred to (ref no 30) included physiotherapists only. The patient assessment made by for example a physician is not necessarily similar to that made by a physiotherapist. Has there been any inter-rater reliability study made on other health care providers?

RESPONSE: We have provided more information in this regard, including comments specific to the training. We have added, (p.6,7)

“A trained advanced practice clinician (APC) assessed patients. The dominant symptoms were classified by the APCs into one of four pain pattern subgroups: back dominant pain aggravated by flexion (P1), back dominant pain aggravated by extension (P2), constant leg dominant pain (P3), and intermittent leg dominant pain (P4). When applied by physiotherapists, inter-rater reliability for this system has been reported at 79% (kappa = 0.61).⁽³⁵⁾ Half of the ISAEC APCs were physiotherapists and the other half chiropractors. All APCs underwent a 13-week hands-on training curriculum (one full day per week) to ensure that the clinicians participating in the ISAEC program had the appropriate inter-professional training to understand the full continuum of LBP assessment and care from the context of primary and specialty care. This included education in the clinics of spine surgeons, a physiatrist, a rheumatologist, primary care doctors and a pain psychologist; inter-professional best practice education sessions; and a final examination that included assessment of the APCs ability to effectively utilize the Hall classification system and the ISAEC management protocols.”

Please specify the statistical analyses used in relation to variables and not only as “appropriate” (Lines 35 and 38)

RESPONSE: We have now specified the statistical analyses used in relation to specific listed variables. We added, (p.6)

“Statistical comparisons across subgroups were made by way of analysis of variance (age, BMI, number of chronic conditions, Oswestry percent disability and EuroQol-5D summary score), Fisher’s Exact test (age group) and chi-square test (sex, BMI category and history of back problems).”

Results

For clarity it is good to denote all ORs instead of mixing ORs and % and “nearly twice” (Line 31 and 32)

RESPONSE: Done.

Discussion

In general, I find the discussion is missing elaborations.

For example, LBP and inference to patho-anatomical source is tricky. Imaging studies have indicated that LBP can occur although lumbar anatomy is considered normal, and in reverse, that abnormal lumbar anatomy is not necessarily associated with pain, and it's unclear whether these abnormal findings are clinically important in LBP and sciatica. Intermittent leg pain classified to P4 may represent neurogenic claudication which may have several explanations apart from osteoarthritis. If inference to pathology is to be done I think the authors should broaden this discussion. Especially since the Hall classification was designed on recognition of patterns of pain with no direct reference to anatomic site or pathological process. As mentioned under my comments on the introduction section I am missing the implications for the Hall classification approach in areas outside physiotherapy. In my view this is important. If an approach is to be used outside its designed purpose this must have a clear rationale. What are the clinical implications for this subgrouping approach other than to physiotherapists (e.g. doctors, nurses, psychologists and other health care professionals) in an inter-professional setting?

RESPONSE: Thank you for pointing out these issues. On re-reading the associated sections of the Discussion we agree that more was needed in certain areas. We have revised the Discussion considerably in this regard adding new text, in addition to removing others we felt no longer contributed appropriately.

The result discussion is insufficient. While the result certainly is correct and obviously precise with narrow CIs the associations between age and P2 and P4 is weak with ORs just above 1. The same goes for comorbidities and P2 where the CI includes 1. This needs to be discussed and maybe cautiously interpreted.

RESPONSE: We respectfully disagree here, although we believe it may have been our lack of pointing out that the ORs for both age and comorbidities were per unit change that made it appear as if the associations were weak. To help clarify this point, we added, (p.11)

"The most clinically distinct observations were that increasing age and number of comorbidities were associated with the back pain with extension (P2) and/or intermittent leg pain (P4) groups. For every year increase in age, odds increased by 2% and 6% for P2 and P4, respectively. These are significant given that the narrowest age range spanned 60 years. Every unit increase in comorbidity count increased the odds of being in P2 by 14%, once again significant for this group as the number of conditions ranged from 0-10."

Further, it is known that peripheral neurological signs and symptoms, e.g. leg pain, motor and/or sensory disturbances, indicative of nerve tissue damage, is associated with disorder severity, prolonged healing process, chronicity, work absence and higher health-care costs. Therefore, is the association between not having good health scores and P3 is not surprising.

RESPONSE: The reviewer is absolutely correct, thank you. To make this point, we added, (p.11)

"Our finding that P3 pain (constant leg pain, typically termed 'sciatica') was associated with worse self-rated health status is not surprising, given the typically severe degree of pain and disability associated with the clinical syndrome of radiculopathy.(44)"

Reviewer: 2

The authors are to be commended on carrying out a study that explores the construct validity of a low back pain (LBP) classification system based on pain patterns. The authors have described different profiles within the groups but I am wary of their conclusion (abstract line 37) that this suggests "potentially unique risk factors and underlying etiologies". It is not clear how the findings from this study can advance the field and specifically address the justification of

providing subgroup- specific care for LBP patients.

RESPONSE: Thank you for your comments, we agree. We trust that the additional text, and deletions made, to the manuscript have provided greater focus and in turn greater clarity. Specific changes are noted above and below.

Introduction: It would be good to see more background information on classification systems for LBP in the literature, and justification for use of the Hall et al method. Reference 27, where this system has been compared to usual care, is an observational double cohort design study, therefore effectiveness cannot be inferred, for that you need RCTs.

RESPONSE: We trust that the whole of the changes made to the introduction in response to this and the other reviewer's comments have provided an appropriate background.

Avoid starting too many sentences with authors first name et al (line 31,34).

RESPONSE: Changes made accordingly, except to one sentence where we believed it necessary.

Methods:

Clarification needed for terms such as "persistent pain" and "recurring and unmanageable pain". The groups only apply to patients with symptoms up to 12 months. I suggest this should be made clear in the title and could you explain why no more than one year?

RESPONSE: We have expanded this section to provide greater clarity, adding, (p.5)

"To be eligible for the ISAEC pilot program, patients had to be 18 years of age or older and experiencing LBP-related symptoms that were either persistent, lasting from 6 weeks to 12 months, or recurring (regardless of duration). Persistent pain was defined as constant daily pain of varying intensity and recurrent pain was defined as episodic pain with a current episode no longer manageable by primary care measures compared to previous episodes. The lower time limit excluded patients experiencing acute LBP that had a favorable natural history, while the upper time limit excluded patients with persistent pain (as per our working definition) in excess of 1 year in duration as this group has a high likelihood of a concurrent pain disorder and poor outcome.(34) The principle focus of the ISAEC program is education and enabling patient and primary care providers to utilize chronic disease self-management principles to manage their LBP."

A flow chart is needed to illustrate the flow of patients for the study, to see the total pool of potentially available patients and how many were selected and classified. It is difficult to comment on potential selection bias as this information is not presented.

RESPONSE: Our failure to indicate that consecutive patients were enrolled within the time period specified may have caused confusion regarding selection. To clarify, we added, (p.5)

"This study analyzed data collected from consecutive ISAEC patients between November 2012 and February 2014."

"Previous history of back problems" (line 9 page 6) does not seem very informative to compare groups

Response: This data was captured to enable determination of whether the LBP was recurrent or

1st time episode. From a clinical perspective, it is relevant to management considerations for patients (e.g. what has or has not worked for the patient in the past).

Statistical analysis: It was not clear why the P1 (back flexion) group was chosen as the referent category for multinomial regression. Is there data from previous work to suggest that this is the least "severe" group? Otherwise would comparing groups with and without leg pain be more appropriate?

RESPONSE: The choice of P1 was based on statistical considerations only. Ultimately, it doesn't really matter which group is chosen, as long as one is aware of which category is the reference (we were careful to note in several instances, "...compared to the P1 group") The same results would be achieved no matter which is chosen; it is simply that the specific comparisons reported will differ. Two common options for choosing a category are using the normative category, if there is a norm, otherwise the largest category, as we have done. We also point out that no assumption was made as to severity – the outcome was nominal, with no implied order.

Results:

Lines 23-37. I suggest being consistent with reporting of odds ratios. Avoid phrases like "overweight individuals had 74% greater odds". No need to give p values when reporting confidence intervals.

RESPONSE: Thank you. The suggested changes have been made.

Table 1; it's unusual to report range with mean, I would expect mean and standard deviation. It's useful to give frequencies with percentages, e.g. for age categories and BMI categories. Be consistent with number of decimal places. For all p-values that are less than 0.001, round off as <0.001 instead of giving precise values.

Most of the differences between groups, and odds ratios are quite small; this is not pointed out in the discussion. Reasons for this could be explored e.g. does it suggest poor discriminant ability of the classification system? Were the right variables used to describe the groups?

RESPONSE: Thank you. The suggested changes have been made. The included ranges have been kept to facilitate interpretation of the ORs, in response to this and the previous reviewer's comments. We believe it may have been our lack of pointing out that the ORs for both age and comorbidities were per unit change that made it appear as if the associations were small. To help clarify this point, we added, (p.11)

"The most clinically distinct observations were that increasing age and number of comorbidities were associated with the back pain with extension (P2) and/or intermittent leg pain (P4) groups. For every year increase in age, odds increased by 2% and 6% for P2 and P4, respectively. These are significant given that the narrowest age range spanned 60 years. Every unit increase in comorbidity count increased the odds of being in P2 by 14%, once again significant for this group as the number of conditions ranged from 0-10."

If the groups are different by clinical examination, what is the added benefit of them having a different profile in terms of demographics/ pain/ disability? Is it expected that this will effect matched treatments for these subgroups? I would expect to see some more comparison with other similar classification systems from the literature e.g. the Quebec task force classification.

The results suggest that those with leg pain are worse on most characteristics, which is not surprising based on existing literature. This could be addressed in more detail. A pathoanatomical source of pain (line 14) is discussed, but if going down this route, why not

address that P3 are considered sciatica and P4 stenosis according to Hall et al 2009.

RESPONSE: The reviewer is absolutely correct, thank you. To make this point, we added, (p.11)

“Our finding that P3 pain (constant leg pain, typically termed ‘sciatica’) was associated with worse self-rated health status is not surprising, given the typically severe degree of pain and disability associated with the clinical syndrome of radiculopathy.(44)”

and,

“Patients in P2 and P4 present with mechanical symptom patterns which, in the absence of other symptoms or signs to suggest red flags (i.e. tumor, infection or fracture), represent symptomatic spine osteoarthritis (i.e. extension dominant symptom provocation) that are typical of symptoms of lumbar spinal stenosis (i.e. back pain and/or neurogenic claudication) which is due to facet osteoarthritic changes and posterior spinal element loading in extension.(45-48) ... It is noted by Hall et al. that P4 represents the clinical syndrome of neurogenic claudication. However, they did not speculate on the patho-anatomic etiology of P2 and also note that their classification by dominant pain pattern is not designed to make inference to a patho-anatomical source of pain.(27) Whether our findings truly indicate a greater likelihood of an underlying osteoarthritis etiology for the P2 and P4 groups would require advanced imaging and symptom correlation.”

Good acknowledgement of limitations of sample source in discussion.

RESPONSE: Thank you

Line 8- what is meant by “group-specific risk management” in relation to your results?

RESPONSE: We have provided an example for clarity, adding, (p.12)

“...group-specific risk management. For example, in addition to directional exercise and activity modification as suggested by Hall et al, a clinical diagnosis of spinal osteoarthritis for P2 and P4 patients can also allow for more targeted medical management, such as more aggressive use of non-steroidal anti-inflammatory drugs, or more appropriate surgical referrals for patients with P4 without the need for advanced imaging.”

A prognostic study would be more informative than cross sectional data. It is hoped that the study team will be able to describe the clinical course of these groups and a prognostic study is strongly encouraged if not already underway.

RESPONSE: Agreed. We have added, (p.13)

“Further longitudinal comparative and prognostic evaluation work is required and has begun concurrently to this study.”

VERSION 2 – REVIEW

REVIEWER	Birgitta Widerström Department of neurobiology, care sciences and society; division of physiotherapy. Karolinska Institutet, Sweden
REVIEW RETURNED	25-Sep-2016

GENERAL COMMENTS	The present manuscript is a revised version of the manuscript
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“Differences among primary care patients with distinct pattern of low back pain: a cross sectional investigation”. The authors have satisfactorily addressed all major questions raised and the several minor corrections suggested. There are still a few issues that authors may consider prior to publication

Abstract

The Hall pain pattern P3 and P4 (leg pain pattern) is described as constant or intermittent leg pain in the abstract and method sections without the aggravating movements (flexion and extension, respectively) whereas in the introduction (p. 4 lines 56-57) the movements are added with no mention of constant or intermittent pain. The used criteria need to be consistently described.

Method

I agree with reviewer 2 that the addition of a flow chart would make the study clearer for the readers.
Inclusion criteria 6 weeks -12 month are set to include those in a sub-acute and longstanding pain state. As the ISEC program is focusing on education and self-management, I still don't understand why people with longer duration than 12 months are considered ineligible. The described focus sounds exactly what patients with longstanding pain might need and likely would benefit from. Maybe better to state inclusion criteria 6 weeks to 12 months without inference likelihood of concurrent pain disorder and poor outcome such as done in p.6 lines 11-12?

Discussion

It seems my previous comment on the lack of elaborations in the discussion section was unclear.
What exactly have the distinct phenotypes of LBP found in this study added to guide treatment and management that the Hall system has not already addressed?
What implications do sex, age and overweight have in terms of management? What implications for management do the authors see for the increased odds of a number chronic conditions and P2, and for the confirmation of the association between poorer health and constant leg pain (P3)? What can be done to help these patients? I think it would be interesting for the readers if the authors could discuss these questions, in addition to the probable pathology track (p.12, lines 40-4) that authors refer to. This extended discussion is especially important as no pathology confirmation data is available for this study sample.

I and reviewer 2 both have highlighted that some odds ratios are quite small suggesting weak associations and that these results should be discussed. The authors' disagree but their response to this comment on p. 11(lines 21-25) is to me unclear. Either are these associations weak which just need to be acknowledged and discussed shortly or are the associations stronger than the result section depicts and should be explained in an understandable and clear manner.

There are no mentions about patients that had a mixed pattern and were dominance were ambiguous and how the clinical reasoning on how to classify these to one category was done. These

	<p>considerations would be interesting and valuable for the readers. It is probably true that a classification system with few categories is easy to integrate in clinical practice, but it is unknown that it also would be with good reliability (P.14, lines 20-21). It is known that reliability increase with familiarity. The examiners in this study were given quite extensive training (although the training, to my understanding, did not only include the ability to classify patients but also the ISAEC protocols on a whole). Does this imply that the implementation of the classification system need a training that may be unfeasible in clinical practice and consequently undermine good inter-examiner reliability?</p> <p>Finally, the sentence that has been added in p.12, lines 13-17 (“Our finding that P3 pain was associated with worse”) seems to be out of context and authors may consider placing this somewhere else. There might be a point in discussing each pain pattern and correlating findings one by one.</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

The authors have satisfactorily addressed all major questions raised and the several minor corrections suggested.

RESPONSE: Thank you.

There are still a few issues that authors may consider prior to publication

Abstract

1. The Hall pain pattern P3 and P4 (leg pain pattern) is described as constant or intermittent leg pain in the abstract and method sections without the aggravating movements (flexion and extension, respectively) whereas in the introduction (p. 4 lines 56-57) the movements are added with no mention of constant or intermittent pain. The used criteria need to be consistently described.

RESPONSE: The description in the introduction has been revised to be consistent.

Method

2. I agree with reviewer 2 that the addition of a flow chart would make the study clearer for the readers.

RESPONSE: A flow diagram has been added as Figure 1.

3. Inclusion criteria 6 weeks -12 month are set to include those in a sub-acute and longstanding pain state. As the ISEC program is focusing on education and self-management, I still don't understand why people with longer duration than 12 months are considered ineligible. The described focus sounds exactly what patients with longstanding pain might need and likely would benefit from. Maybe better to state inclusion criteria 6 weeks to 12 months without inference likelihood of concurrent pain disorder and poor outcome such as done in p.6 lines 11-12?

RESPONSE: While on the surface, the reviewer comment is correct, the ISAEC program does not provide the intensive psychological interventions to reverse entrenched maladaptive coping or pain management. We have removed the inference to possible pain disorder or poor outcome. The reviewer is accurate in this request as this can variably occur at any period after the onset of pain. We have also added an additional point to the sentence immediately following this exclusion statement to indicate that one of the principle foci of ISAEC is to mitigate chronicity (page 5, paragraph 2).

Discussion

4. It seems my previous comment on the lack of elaborations in the discussion section was unclear. What exactly have the distinct phenotypes of LBP found in this study added to guide treatment and management that the Hall system has not already addressed? What implications do sex, age and overweight have in terms of management? What implications for management do the authors see for the increased odds of a number chronic conditions and P2, and for the confirmation of the association between poorer health and constant leg pain (P3)? What can be done to help these patients? I think it would be interesting for the readers if the authors could discuss these questions, in addition to the probable pathology track (p.12, lines 40-4) that authors refer to. This extended discussion is especially important as no pathology confirmation data is available for this study sample.

RESPONSE: While we agree that these are interesting questions, this extended discussion is not possible as we have already exceeded the word limitations. Also, the focus of the study was to characterize and compare a persistent low back pain population based on four clinical pain patterns. Thus an extensive discussion would be purely speculative and not substantiated by our current methodology. However, we do discuss management implication for those with presumptive diagnosis of spine OA.

5. I and reviewer 2 both have highlighted that some odds ratios are quite small suggesting weak associations and that these results should be discussed. The authors' disagree but their response to this comment on p. 11 (lines 21-25) is to me unclear. Either are these associations weak which just need to be acknowledged and discussed shortly or are the associations stronger than the result section depicts and should be explained in an understandable and clear manner.

RESPONSE: We regret we were not clearer in our response. While the odds ratios are small in magnitude, this does not necessarily imply a weak association. We must consider the potential range of values that the predictor variable can assume when assessing a 'weak' versus 'strong' association. By indicating the age range of the sample, we were hoping to convey that even a 'small' per year impact can be quite significant incrementally over the age range. The age range of the sample was 18 to 93 years. So, for example, the odds ratio of 1.02 for each unit increase in age for the P2 group implies that the odds of P2 for a 60 year old individual are 81% higher compared to a 30 year old individual. Stated another way, and perhaps more in keeping with convention, an odds ratio of 1.02 per year is equivalent to an odds ratio of 1.22 per decade of age. How to characterize this estimate on a weak--strong continuum is arguable, but we don't think this suggests a weak association.

We have amended the corresponding section in the Discussion to read (page 12, paragraph 2), "For every year increase in age, odds increased by 2% and 6% for P2 and P4, representing a 22% and 79%, respectively, increased odds for each decade of age. Every unit increase in comorbidity count increased the odds of being in P2 by 14%."

6. There are no mentions about patients that had a mixed pattern and were dominance were ambiguous and how the clinical reasoning on how to classify these to one category was done. These

considerations would be interesting and valuable for the readers.

RESPONSE: This was not specifically captured. However, a protocol for this scenario does exist. We have indicated this in the discussion as a limitation. (page 13, paragraph 1): “Additionally, for cases where patients reporting of pain dominance was ambiguous or the pattern was potentially mixed, the APCs rephrased the symptom questions in multiple iterations to establish which symptom or aggravating factor was most functionally limiting and therefore determined the dominant pattern. The prevalence of this uncommon scenario was not objectively captured.”

7. It is probably true that a classification system with few categories is easy to integrate in clinical practice, but it is unknown that it also would be with good reliability (P.14, lines 20-21). It is known that reliability increase with familiarity. The examiners in this study were given quite extensive training (although the training, to my understanding, did not only include the ability to classify patients but also the ISAEC protocols on a whole). Does this imply that the implementation of the classification system need a training that may be unfeasible in clinical practice and consequently undermine good inter-examiner reliability?

RESPONSE: We thank the reviewer for this thoughtful comment regarding generalizability of the Hall classification. As noted by the reviewer, the APC were significantly trained, but more so for the broader operational aspects of the ISAEC program and to establish regional congruity with primary care practitioners and specialists within the ISAEC network. The simple answer to the reviewer’s question is no, however, the inter-examiner reliability of non-MSK trained primary care practitioners has not been evaluated. To partially address this important point we have amended the discussion to include the following (page 14, paragraph 2): “Although not the focus of this paper, in other collaborative work by Alleyne, Hall and Rampersaud (33), we have noted ease of clinical translation of the Hall classification system to primary care providers with minimal training (online or in person session) and / or the use of a simple one page clinical tool (Clinically Organized Relevant Exam (CORE) Tool for the Low Back Pain).”

8. Finally, the sentence that has been added in p.12, lines 13-17 (“Our finding that P3 pain was associated with worse ..”) seems to be out of context and authors may consider placing this somewhere else. There might be a point in discussing each pain pattern and correlating findings one by one.

RESPONSE: We agree that this sentence was ill-placed; it has been moved. Unfortunately, on account of the word limit, there simply is not sufficient space to elaborate further on each pain pattern separately in the Discussion. We have chosen to more so highlight P2 and P4 given our contention that these represent a relatively large proportion of the broader clinical population.

VERSION 3 – REVIEW

REVIEWER	Birgitta Widerström The Department of Neurobiology, Care Sciences and Society; division of Physiotherapy, Karolinska Institutet
REVIEW RETURNED	13-Nov-2016

GENERAL COMMENTS	I find that authors have addressed all my previous comments in an adequate way. I have no further comments.
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