

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Association between spectral characteristics of paraspinal muscles and functional disability in low back pain patients: a cohort study.
AUTHORS	Chiou, Shin-Yi; Koutsos, Ermis; Georgiou, Pantelis; Strutton, Paul

VERSION 1 - REVIEW

REVIEWER	Jean-Daniel Dubois Research professional Université du Québec à Trois-Rivières Canada
REVIEW RETURNED	23-Apr-2017

GENERAL COMMENTS	<p>General comments</p> <p>The current study is a well-thought-out research using continuous wavelet transform to determine if fiber type activation of lumbar erector spinae muscles during maximal voluntary isometric contraction correlate to disability in individuals with chronic low back pain. First of all the manuscript reads easily throughout and is a breeze to understand. While scientifically sound, there are still some points that I would like addressed or at least clarified before the article can be accepted for publication. These points are addressed hereby.</p> <p>General concern</p> <p>Even though the perceived effort by the patients was generally not different from perceived efforts from healthy controls, this does not ensure that the maximal voluntary isometric contraction was in fact a maximal contraction. Elevated pain catastrophizing, kinesiophobia or other psychological factors could mean guarded movement which would largely explain the higher ratio of low to high energy observed in individuals with elevated disability scores as observed in figure 3. Moreover, disability scores of the patients was very high (10.27/24 on the Roland-Morris scale) and since disability has frequently been associated with the aforementioned psychological factors, it is quite possible that the contraction was in fact submaximal.</p> <p>As a whole, I feel that the discussion section was mainly aimed at explaining EMG related results and stayed shy on the explanation of the link between disability and fiber activation ES muscles.</p> <p>Minor revision</p> <p>Globally, I thought that the back and forth between frequency characteristics (Hz) of muscle activation and the scale employed with the CWT framework was a bit hard to follow. I was glad to see the scale transformed to corresponding frequencies, then again,</p>
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	<p>maybe a table detailing corresponding scale to frequencies would be more indicated. Another possibility here would be to state in the data analysis section that even though scales were used, the rest of the manuscript would discuss the results by only stating frequencies. Again a table could be added for CWT enthusiasts to appreciate what was done in that regard.</p> <p>Minor comments</p> <p>Page 11 line 45, chronic CLBP is redundant, please choose either CLBP or chronic LBP.</p> <p>I really liked Figure 1 and feel that details of the data analysis are a breeze to understand. Still I would like for the experimental setup to be detached and maybe put into another figure if necessary to allow for more space to be allocated to data analysis explanation and appreciation by the reader.</p>
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REVIEWER	Paul Marshall Western Sydney University
REVIEW RETURNED	02-Jun-2017

GENERAL COMMENTS	<p>Interesting paper, and while from a broad context I was concerned about the subject matter (redundancy issue), the authors have professionally conducted a study to provide some interesting discussion points for those who use and consider the use of surface EMG for back pain patients (esp in fatigue based clinical tests). While the aspect of analysing a surface EMG signal to draw inferences about underlying function is fraught with error, the authors have carefully and conservatively discussed these issues within this paper and provided a coherent case regarding the clinical relevance and utility of not 'giving up' with these methods. Indeed, at my previous clinic we routinely applied surface EMG for our LBP patients, although simplistic frequency based measures did not appear useful in practice. The techniques discussed here, and the professional approach taken by the authors in discussing them, should be considered and the authors commended.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Jean-Daniel Dubois
Research professional, Université du Québec à Trois-Rivières, Canada
Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

General comments

The current study is a well-thought-out research using continuous wavelet transform to determine if fiber type activation of lumbar erector spinae muscles during maximal voluntary isometric contraction correlate to disability in individuals with chronic low back pain. First of all the manuscript reads easily throughout and is a breeze to understand.

Thank you for these positive comments.

While scientifically sound, there are still some points that I would like addressed or at least clarified before the article can be accepted for publication. These points are addressed hereby.

General concern

Even though the perceived effort by the patients was generally not different from perceived efforts from healthy controls, this does not ensure that the maximal voluntary isometric contraction was in fact a maximal contraction. Elevated pain catastrophizing, kinesiophobia or other psychological factors could mean guarded movement which would largely explain the higher ratio of low to high energy observed in individuals with elevated disability scores as observed in figure 3. Moreover, disability scores of the patients was very high (10.27/24 on the Roland-Morris scale) and since disability has frequently been associated with the aforementioned psychological factors, it is quite possible that the contraction was in fact submaximal.

This is an interesting point. We asked patients to report their perceived efforts to attempt to overcome the issue of them guarding their movement. We reasoned that if patients did in fact have guarded movement, this would be reflected in lower rates of perceived effort (i.e. if the contraction is likely to induce pain, patients are less likely to contract as hard as possible). As part of the methodology, we asked every participant to perform 3 isometric back extensions. We chose isometric contractions as dynamic contractions would likely cause pain. Further, we found that the contraction in which the highest torque was obtained did not follow a clear pattern expected if the contraction induced pain. That is, if the patients were guarded, one might expect the first MVC to have the lowest torque. Conversely, if the first MVC induced pain, one might expect the first MVC to have the highest torque with subsequent MVCs being lower. Given that the rates of perceived exertion were not different between groups, plus there was no pattern to the torque produced during the MVCs, we make the assumption that the patients MVCs will be their maximum under the current experimental conditions. Since patients reported that pain was not increased during the experiment, it is likely that the data reflect true differences in muscle function in the patients.

As a whole, I feel that the discussion section was mainly aimed at explaining EMG related results and stayed shy on the explanation of the link between disability and fiber activation ES muscles.

Although alterations in fibre-type proportions of paraspinal muscles in low back pain have been reported, the changes are equivocal, some studies suggesting increased proportions of type II and others suggesting increased proportions of type I. Further, the relationships between fibre-types and back pain-related disability are unclear. Since we did not conduct a muscle biopsy study to directly investigate the relationship between disability and fibre-type proportions in this population, we stated in the original manuscript that a shift in the power spectrum could be related to alterations in motor control or in muscle fibre-type proportions. We have now included a paragraph in the revised discussion to put into context our results of a correlation between the ratio of energy at low to high frequencies and Roland-Morris disability questionnaire in subjects with LBP.

“Further, the correlation between the EMG spectral characteristics and back pain-related disability scores suggests that altered motor control or fibre-type proportions of paraspinal muscles has an impact on functional activities, which is reflected in higher disability scores. This highlights the importance of rehabilitation to restore muscle function in patients with LBP, which may reduce the levels of disability reported in these patients.”

Minor revision

Globally, I thought that the back and forth between frequency characteristics (Hz) of muscle activation and the scale employed with the CWT framework was a bit hard to follow. I was glad to see the scale transformed to corresponding frequencies, then again, maybe a table detailing corresponding scale to frequencies would be more indicated. Another possibility here would be to state in the data analysis section that even though scales were used, the rest of the manuscript would discuss the results by only stating frequencies. Again a table could be added for CWT enthusiasts to appreciate what was done in that regard.

Thank you for your suggestion. As the continuous wavelet transform was used, scales from 5 to 300 resulted in 296 corresponding frequencies. We feel that a table containing such a large amount of information might not be so informative to readers. Therefore, to clarify, we have now stated in the Data analysis section that *“the scales were then converted into their corresponding frequencies for further analyses.”*

Minor comments

Page 11 line 45, chronic CLBP is redundant, please choose either CLBP or chronic LBP.

Thank you for spotting this, we have corrected this in the manuscript; it now reads as “Further, given patients with chronic LBP have been shown...”.

I really liked Figure 1 and feel that details of the data analysis are a breeze to understand. Still I would like for the experimental setup to be detached and maybe put into another figure if necessary to allow for more space to be allocated to data analysis explanation and appreciation by the reader.

Thank you for your kind words about figure 1. We feel that the current arrangement in figure 1 shows a clear flow of the data collection and analysis that may help readers understand the methodology of the study. Nevertheless, we are happy to separate the information provided in the figure 1 into two figures if needed and will be guided by the editor in this regard.

Reviewer: 2

Paul Marshall

Western Sydney University

Please state any competing interests or state 'None declared': Nil

Please leave your comments for the authors below

Interesting paper, and while from a broad context I was concerned about the subject matter (redundancy issue), the authors have professionally conducted a study to provide some interesting discussion points for those who use and consider the use of surface EMG for back pain patients (esp in fatigue based clinical tests). While the aspect of analysing a surface EMG signal to draw inferences about underlying function is fraught with error, the authors have carefully and conservatively discussed these issues within this paper and provided a coherent case regarding the clinical relevance and utility of not 'giving up' with these methods. Indeed, at my previous clinic we routinely applied surface EMG for our LBP patients, although simplistic frequency based measures did not appear useful in practice. The techniques discussed here, and the professional approach taken by the authors in discussing them, should be considered and the authors commended.

Thank you for your positive comments about our professional approach and our discussion of the techniques used in the study. We acknowledge, in our paper, the difficulties in drawing inferences about function from analysis of EMG signals, but thank the reviewer for considering that we have provided a coherent case regarding the clinical relevance.

VERSION 2 – REVIEW

REVIEWER	Jean-Daniel Dubois Université du Québec à Trois-Rivières Canada
REVIEW RETURNED	24-Jun-2017

GENERAL COMMENTS	Even though the manuscript is still an excellent one, I was a bit disappointed by the authors responses to my original comments. I was glad to see their answer to my question about perceived exertion not being different between low back participants and controls (I seemed to have missed it the first time around in the
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	manuscript). Then again, I am still unsure that similar perceived exertion automatically means that patients performed their true maximal voluntary contraction. Then again, I will leave the decision to the editor although I feel as the article would benefit from discussing these limitations. Moreover, the added paragraph felt a bit generic.
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VERSION 2 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author: 1. Even though the manuscript is still an excellent one, I was a bit disappointed by the authors responses to my original comments. I was glad to see their answer to my question about perceived exertion not being different between low back participants and controls (I seemed to have missed it the first time around in the manuscript).

We would like to thank to the reviewer for the comment on the quality of the manuscript.

2. Then again, I am still unsure that similar perceived exertion automatically means that patients performed their true maximal voluntary contraction. Then again, I will leave the decision to the editor although I feel as the article would benefit from discussing these limitations.

We have now acknowledged the potential impact of chronic pain on the maximal voluntary isometric contraction (MVIC) in the Discussion.

'While factors such as fear avoidance or pain inhibition likely affect the values of maximal muscle activity, our subjects were given adequate warm-up time as well as asked to repeat the contractions 3 times, with the highest value taken.'

While this could be a limitation of the study, its impact on the results may be minimised using appropriate techniques (warm-ups and repetition of the MVICs 3 times), analysing data carefully (only the highest value of the 3 MVICs was taken) and recording subjective measures (pain scores and perceived exertion). We feel that, by using the appropriate methodology, which was highlighted by both reviewers in their previous comments, our results may reflect true differences in muscle function in the patients.

3. Moreover, the added paragraph felt a bit generic.

We have now clarified that altered motor control or fibre-type proportions of paraspinal muscles, which is related to the altered EMG spectral characteristics, may be reflected in self-rated disability scores, measured by back pain-related disability scores. We have also specified the type of alterations rehabilitation needs to target for to reduce the levels of disability.

'Further, the correlation between the EMG spectral characteristics and back pain-related disability scores suggests that altered motor control or fibre-type proportions of paraspinal muscles may be reflected in self-rated disability scores. This highlights the importance of rehabilitation to restore motor control and muscle function in patients with LBP, which may reduce the levels of disability reported in these patients.'