Author's response to reviews

Title: Segmental lumbar mobility in individuals with low back pain: In vivo assessment during passive and active motion using dynamic MRI.

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Version: 4 Date: 9 October 2006

Author's response to reviews: see over
Reviewer's report

Title: Segmental lumbar mobility in individuals with low back pain: In vivo assessment during passive and active motion using dynamic MRI.

Version: 2 Date: 5 July 2006
Reviewer: Alison Hazel McGregor
Reviewer's report: General

The Authors thank Dr. McGregor for her insightful comments. To improve readability, we copied Dr. McGregor’s comments into this document, and will keep them in ‘regular’ font. We will reply to her queries in ‘italics’ and the paragraph will be indented. The new text, will be presented in “bold”. Page and paragraph will indicate location of the new text within the revised version of the manuscript.

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

This paper looks at motion patterns in response to a PA mobilisation or press-up extension procedure using intersegmental angle as the principal measure in a group of subjects with sub-acute low back pain and controls. Although interesting the themes and focus of this paper change throughout.

Specific points:-
The paper starts with stating the impact of low back pain to the individual and society although it does not refer specifically to the sub population investigated.

We added a citation [McBride et al 2004] to the last sentence the first paragraph and a statement at the end of the first paragraph: “Even though the resolution of acute symptoms seems faster in younger person, one of four may continue having symptoms 12 weeks after the onset of symptoms resulting in pain and cost to economy (McBride et al 2004, Grotel at al 2005).

Paragraph 2 goes on to discuss mobility, I just feel this section lack robustness and would benefit from referencing to back up these statements.

We agree with Dr. McGregor’s recommendation and have added citations to these statements.

You discuss PA mobilisations briefly but do not let the reader in on what is known about PA mobs scientifically particularly what they achieve and how repeatable this technique is etc and on the clinical side how often it is used. This would help show how important studies are in this area. Also should you define what clinicians mean by the grading hypomobile normal etc.
We have modified the third paragraph to address your recommendations.

I think you need to justify why pain will affect spinal mobility.

We are thinking of pain and motion patterns as coexisting. We are not comfortable arguing a causative relationship.

Paragraph 5 first sentence, I am not sure how you are eliminating gravity. My concern here also with this statement is you are not looking at an active and then passive movement that are the same you are looking at 2 very different movements so not sure how relevant this all is.

We have altered the terminology to ‘manual test’ and ‘self-initiated test’.

I disagree with the statement that segmental motion of the spine during PA has been scarcely investigated this is misleading. It has been investigated a lot by Lee et al, McGregor et al, in vivo plus others albeit in cadavers. The problem is despite these studies we still do not fully understand what the manoeuvre is achieving.

We have made appropriate modification to the text of the manuscript.

I am confused as to how you are linking the PU and PA mobilisation and I think this need clarifying to me they are very separate movements/techniques.

We are thinking of these two clinical procedures as separate movements that may produce intervertebral motions, similar in direction, but perhaps different in quantity and pattern of intervertebral motion? We have modified the second paragraph on page 2. The test now reads: Quantity and pattern of segmental mobility may certainly vary with the type of movement tested, instructions to the subjects and their willingness to participate. For example, a self initiated test, such as a press-up (PU) maneuver may be influenced by the subject’s symptoms, thereby limiting spinal segmental motion. In contrast, application of a manual PA force to a lumbar spinous process sufficient in amount to reach end-range may produce more motion since it’s not self-limited by pain. Therefore, quantifying motion under these two conditions may provide a more comprehensive assessment of segmental mobility in persons with low back pain.

Study population/Methods
Can you explain why there were 45 patients and only 20 controls?

The data interpreted in this manuscript are from a larger intervention study. The patient group was randomly assigned to one of three groups (15 per group). The power analyses were performed on the patient groups. We feel that the uneven distribution of subjects in this analysis does not compromise our conclusions.
What were your power calculations for this study?

*The* power calculations were based of the 3 intervention groups mentioned above.

Also where and how were patient recruited.

*The* patients were recruited from a university campus with the use of flyers. *The* student and faculty

How much disability did the patient group have and was their global motion restricted?

*We* used the self-administered Oswestry disability questionnaire. *The* scores ranged between minimal and moderate disability.

Were these people at work or off work as a result of their back pain. *Their* VAS are low so I assume they were not too disabled.

*No, the subjects were not off work.*

When you performed your localisers scan did you screen for pathology such as disc prolapse or degeneration?

*Yes, we did pay attention to the morphological appearance of the lumbar spine when performing the localizer. One subject was excluded from the study based on the appearance of the fifth lumbar vertebra suggesting an anatomical variance in its osseous development.*

One concern is whether the force applied to each spinous level was the same how did you control for this other than subjective feel?

*We relied only on the subjective feel of an experienced operator (therapist). Which make the data clinically relevant, but less exciting and convincing biomechanically. We acknowledge that as a limitation of this study.*

How often did errors occur in terms of mobilising correct level page 8 paragraph 2.

*There were three occurrences, among all 65 subjects tested, where the tester missed the L5 spinous process and moved from the sacrum to L4. The investigator, who viewed the data online, immediately prompted the operator (therapist) to repeat the test.*

Not sure how you used a hand held dynamometer to measure the force of the mobilisation. Are these levels of force in accordance with the published literature? It might be worth adding that studies have attempted to measure force in the scanner,
with non-electronic devices but these were unable to record the high magnitudes of force generated by the PA mob (Lee et al)

We decided to exclude this statement from the manuscript. Since the dynamometry experiment was not done in the magnet, this information does not seem to inform the reader what happened during our experiment. We have performed the experiment as a pilot experimentation, in preparation for this study.

Finally how did you decide which images were obtained at the end range?

During data collection, motion was performed slowly “on a count of 1001 to 1002”. We held the terminal position for a count of “1003 to 1008”. This gave us several images that were clear and easy to digitize. From our experience, motion produces artifact that become quite obvious in clarity of the image. Therefore, choosing an image at the end of the motion with the best clarity, gave us an assurance that this was the final position. To assure ourselves that this assumption was correct, we have randomly selected 5 studies and have digitized three consecutive images deemed to be at the end of the range. This experiment assured us that our prior choice of the final image was correct.

Results/Discussion
One concern is greater range of intersegmental motion appear to be achieve by the PU rather than the PA mob which is supposed to be the passive mobilisation to end of range.
Can you explain this?

Yes, that is a good observation. Even thought the differences in intervertebral motion were small, they were consistent in 4 out of 5 motion segments in the asymptomatic subjects. We can only speculate … that the PU provides more force (self-imposed and gravity assisted) in the direction of increased lordosis.

Much of the discussion focuses on hypermobility at one level, do you think the calculations etc in relation to this should be in results?

Upon reflection, on the reviewers’ comments, we have reviewed our data and decided to focus on the non-parametric analyses. Consequently, we moved the calculations to results.

Also how do this range of motion compare with other studies on intersegmental motion (so all of the videofluoroscopy work etc).

Considering the range and direction of motion, the quantities are similar.
You discuss age could you not investigate the correlation of age with motion directly using your data set.

_The subjects recruited for this study, represent a narrow age group. Therefore, we would not be able to look at our data from that perspective._

Also from the images taken could you make some assessment on the degenerative status of the discs?

_We screened for signs of pathology during the physical exam. We also screened for disc pathology during the online image analysis._

I also have concerns that the conclusion of this study hangs on the higher percentage of hypermobility when this is not presented or justified as clearly as it could be.

_We have focused the analyses on hypermobility._

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**Minor Essential Revisions** (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

**Discretionary Revisions** (which the author can choose to ignore)

**What next?:** Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** No

**Declaration of competing interests:** I declare that I have no competing interests
Reviewer's report
Title: Segmental lumbar mobility in individuals with low back pain: In vivo assessment during passive and active motion using dynamic MRI.
Version: 2 Date: 17 July 2006
Reviewer: Jim Dickey

The Authors thank Dr. McGregor for her insightful comments. To improve readability, we copied Dr. McGregor's comments into this document, and will keep them in 'regular' font. We will reply to her queries in 'italics' and the paragraph will be indented. The new text, will be presented in "bold". Page and paragraph will indicate location of the new text within the revised version of the manuscript.

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Reviewer's report:
General
This is an interesting paper describing the segmental lumbar spine extension angles which accompany passive and active motion. The authors apply dynamic MRI to study two groups: young individuals with non-specific central low back pain and asymptomatic subjects. They applied PA forces to spinous processes of each lumbar level starting caudally at L5 and moving cranially to L1. They limited their analysis to the one spinal motion segment caudal to the level where the PA force was applied. Similar analysis was performed for a press-up maneuver. They performed two-way ANOVA tests assessing differences in degree of spinal extension at the different lumbar levels between symptomatic and asymptomatic subjects (for the PA and PU maneuver separately). They failed to observe any statistically significant differences in their analyses. In the discussion, they go on to evaluate the proportion of subjects which were hypo-mobile and hyper-mobile in the symptomatic and asymptomatic groups.

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

1) the authors specifically assess young individuals; however, they do not provide a clear justification of whether this is a strength or limitation of their study. If it is a strength, then the authors must provide a more clear rationale for exclusively studying this group (they do acknowledge that younger subjects typically have a shorter duration of symptoms in low back pain, and that low back pain can begin its manifestations in the younger individuals, but this is not a compelling rationale for exclusively studying this group). If it is a limitation, then many of the references to age should be removed and there should be a paragraph within the discussion section where they acknowledge the limitation that their results are only applicable to young individuals.

The subjects in this study were a ‘sample of convenience’. We did not specifically look for younger subjects, but since the study was done on a University campus, we captured a younger population. We consider this a limitation in terms of generalizability. We considered this an opportunity to capture lumbar kinematics in persons with early back problems, and find that a unique feature of this data.
2) the philosophy and analysis seem fundamentally flawed. The authors study segmental lumbar spine motion (angulation) between two groups at five spinal levels using two-way ANOVAs and determined that there is no statistically significant difference between the symptomatic or asymptomatic individuals, there is no significant difference between spinal levels, and that there is no interaction between these two variables for both the PA and PU conditions. This analysis precludes further investigation of differences at individual spinal levels; there is no statistical basis for further analysis of hyper- or hypo-mobile subjects. Accordingly, this aspect of the manuscript must be entirely removed. Alternatively, the authors could reframe their entire study to analyze hyper- and hypo-mobile subjects based around the chi-square analysis; however, this would clearly be a case for cherry picking the favorable results post hoc, which should be discouraged. Additionally, does the hyper/hypo distinction that you make in Tables 2 and 3 reflect the data was not normally distributed and that you should have used non-parametric statistics?

_The reviewer makes an excellent point. We have reviewed our data and decided that it is appropriate to analyze the data with non-parametric statistic. That said, we removed the ANOVA analyses and replaced it with the Chi-square analyses._

3) there are number of issues regarding how spinal mobility was assessed which need to be clarified. They applied PA forces to spinous processes of each lumbar level starting caudally at L5 and moving cranially to L1; the order was not randomized and therefore represents a systematic bias in the experiment. This needs to be identified as a limitation of this study.

_The PA procedure was performed in the manner it is carried out clinically. That is, the PA force is applied consecutively from the sacrum (as reference) to L1. This sequence allows the operator to palpate the consecutive spinous processes and apply the PA force in the center of each spinous process. Asking the operator to apply the PA force randomly, would likely have produced errors in identifying the spinous process. We agree that, from the experimental design point of view, there is a systematic bias. But, introducing a random order of PA application would have moved away from the clinical performance of the procedure and likely (speculation) introduced a random error in performance. We feel that with the existing limitations of not recoding the applied force, we had to rely on the experientially acquired skill of the therapist and conform to the clinical test._

The authors base all their measurements on two images: baseline, and maximum. However, it is unclear how the authors know that the frame they pick as maximum is really the true maximum since they do not analyze other frames.

_During data collection, motion was performed slowly “on a count of 1001 to 1003”. We held the terminal position for a count of “1003 to 1008”. This gave us several images that were clear and easy to digitize. From our experience, motion produces artifact that become quite obvious in clarity of the image. Therefore, choosing an image at the end of the motion with the best clarity, gave us an assurance that this was the final position. To assure ourselves that this assumption was correct, we have randomly selected 5 studies and have digitized
three consecutive images deemed to be at the end of the range. This experiment assured us that our prior choice of the final image was correct.

Similarly, the authors identified that the maximum amount of motion was always between the target vertebrae and the vertebrae caudal to it; however, it is impossible to evaluate this statement since they only quantify the segmental motion at this one level. The authors must provide data to substantiate these choices OR include a clear description of their qualitative procedures used to make these determinations.

We have performed analysis of each motion-segment on this dataset, but in order to improve focus of this interpretation we have decided not to present it in this manuscript. We have removed that statement from the manuscript.

4) organization of the paper: there are results in the methods section, and the discussion contains both methods and results. For example, they describe the Visual Analog Scale results (first paragraph), and the averaged angular position measurements (third paragraph on page 7) in the methods section. The discussion contains the methods and results regarding the chi-square analysis of the clinically most involved lumbar segments.

We have carefully reviewed the manuscript and have made the proper modifications in each section of the manuscript. We use the VAS as a description of the subjects’ symptoms.

Minor Essential Revisions:

It appears that the surface coil was repositioned between the two tests. If so, then this should be explicitly stated and the implications of this move should be discussed.

The text now reads: “For each of the two procedures, the surface coil was secured to the lumbar region using cloth tape. Careful care was taken to place the coil in the same region of the subject’s low back.”

The caption for Figure 2 states that the figure contains measurements of intravertebral motion when in fact it contains static images from dynamic MRI.

We have altered the terminology to intervertebral displacement. The beginning of the caption now reads: “Measurements of intervertebral displacement resulting from the posterior to anterior (PA) … “

Subject confidentiality should be maintained in Figure 1 by placing a bar over the subject’s face.

This old Figure 1 has been replaced by a new Figure 1 and a bar was placed over the subject’s face.

Tables 2 and 3 require additional detail in the table caption specifically describing the point made with the footnotes (specific subjects were hypermobile or hypomobile at more than
one spinal level and accordingly the tally in the right hand column is not merely the sum of the individual values within the table).

Tables 2 and 3 were modified.

Discretionary Revisions (which the author can choose to ignore)

On page 7, second paragraph: the author should include the manufacturer and model for the inclinometer.

The manufacturer and model of the goniometer were added.

Page 9: the first and second paragraph should be joined together.

The two paragraphs were joined together.

Page 9, 4th line before "data analysis" section: you state that the subjects placed their arms in front of them. Were their arms supported by the chair as shown in Figure? If so, then it should be mentioned in the text.

The arms were not supported on a chair, thank you for pointing that out to us. We have replaced this Figure with a new Figure.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions
Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Needs some language corrections before being published
Statistical review: Yes
Declaration of competing interests:
I declare that I have no competing interests