Author's response to reviews

Title: The effectiveness of motorized lumbar traction in the management of LBP with lumbo sacral nerve root involvement: A feasibility study.

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Author's response to reviews:

Reviewer: Christopher McCarthy

Authors response:

Many thanks for your comments; my apologies for not clearly addressing these queries within the article, I hope I have adequately addressed your points this time.

General

Point 3: I asked the authors to justify why they excluded two treatments which can be used with patients with nerve root pain. You have given me a response but not put this justification in the paper itself. I would expect to see a reasonable criticism of a paper addressed both by response to the reviewer and also in the manuscript.

Response: hopefully I have now adequately addressed this within the methods section under excluded treatment pg8 ¿ added in ¡ These were excluded as the UK wide survey [3] informing this trial had not indicated a common use of these modalities in conjunction with traction.

Point 5: It is vital to discuss the issue of differential contact time for both groups. I asked you to iterate this. Your response is inadequate and gives me no idea if and where you have adjusted your manuscript.

Response: added statement on the number of treatments per week per group being similar and that treatments were not in excess of 30 min into section on compliance with treatment at the beginning of the results section pg9, I hope this addresses it clearly.

Point 6: Not indicating the scale of the maximum scale of a questionnaire is not particularly rigorous science. I consider this type of illustration as open to abuse
as very small changes can be presented as large looking change with the tweak of the scale. To simple say you would prefer not to address this point is not acceptable.
Response: Graphs corrected, point noted.

Reviewer: Julie Fritz
Authors response: Thank you for your encouraging comments.

General
The authors have done an excellent job of revising this manuscript. The revised version reflects what was done in a more scientifically appropriate manner and acknowledges the shortcomings of the approach much better. I have a few additional comments that I believe will strengthen the manuscript further.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
In the Discussion section the authors characterize there study as "high quality". While I agree that the approach used in this study represents important advances over some of the previous studies in this area, I do not believe it is appropriate to characterize this study as "high quality" considering the shortcomings primarily related to sample size. I recommend removing this term and simply characterizing how this manuscript advances previous work.
Response: completed as requested pg12.
The authors consider the use of more specific treatment parameters in this study as one way this research advances prior studies. I agree with this characterization, however the authors appear to be concluding that there treatment parameters were "appropriate" based on a survey of practitioners. We do not know if these parameters are the "best". The reason they represent an improvement over previous studies in my mind is that they were well-defined, which is important, not that they are "better" -we simply do not know this. In addition, the treatment parameters used in this study are still somewhat broad do the authors know what parameters were actually used or have any comment on this? My recommendation is that the authors present a more nuanced and thorough discussion of the issue of treatment parameters in the Discussion section of this paper because this is an important issue for future research in this area.
Response: I have added this into the discussion section I hope I have dealt with it adequately pg 13.

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

Page 4 - was the SLR considered positive if symptoms were reproduced at any angle, or was a threshold used?

Response: As long as leg pain was reproduced below 90 degrees it was assumed positive. I have added this into the inclusion criteria Pg4.

Please clarify if all treatment components (education, exercise, manual therapy) were required for all patients, or if therapists used their judgment to include or omit components for individual patients.

Response: Therapists could use their own judgment as to how to treat the patient according to their own clinical reasoning, not necessarily using all components of the protocol if they felt it was not appropriate. see treatment groups MT Pg 6. However therapists tended to use all the components of the protocol at some point in the patients management and I have included a comment on this towards the end of the discussion Pg 13.

Reviewer: Helen Frost

Authors response: Many thanks for your comments I hope I have addressed them to your satisfaction.

General

The authors have addressed most of the comments. The paper would be useful reference for other researchers considering a trial of traction but the study is small and therefore has limitation. I am not convinced a large study is justifiable looking at the results of this feasibility project. A larger trial is unlikely to demonstrate clinically significant differences between groups and would require an expensive multi-centre trial. In my view it would only be justifiable with the addition of an adequate control.

Response: The introduction of a control group receiving medication and advice could be introduced see conclusion Pg 14

The authors have used non parametric descriptive statistics and now report that the data were skewed. If this was the case why did they use parametric statistics?

Response: Advice from my statistician at that time, however in reconsidering the
comments from reviewers I agree that non parametric statistics are probably more appropriate for this study.

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

1) My main concern is the reporting of intention to treat analysis as the figures still do not add up. On the consort flow diagram the authors report that in the traction group 2 patients failed to complete treatment and one failed to return outcomes yet in the intention to treat analysis only 13 rather than 15 patients were included. The same mistake has been made in the manual therapy group. Intention to treat analysis should include all patients who return outcome questionnaires regardless of the treatment they did or did not receive. The last value carried forward method is used to replace missing data and this should be explained. I am still not sure whether an intention to treat analysis has been carried out as the consort flow diagram implies that it hasn't. Insertion of the number of patients analysed in each group on Table 3 and 4 would clarify this.

Response: I apologize for the confusion over this intention to treat analysis and I agree that the addition of numbers in Table 3 and 4 would clarify this and have done as requested. I have also added in a little more description to the forward method on Pg 8 under statistical analysis. The consort flow diagram was used to illustrate what actually happened with drop outs.

2) I would prefer to see a measure of variability around the median points on the graphs

Response: I attempted to add to this to the graphs but it made them very unclear I hope the measure of variance noted in the table will suffice.

3) A reference for the method of randomization would be useful.

Response: this has been added pg5.

4) Justification for a larger trial should be discussed relating to other recent trials bearing in mind that this feasibility study suggests that no differences would be found between groups.

Response: Pg 14 conclusion and already mentioned re small trial problems at the bottom of pg 11.

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

1) As the focus of the paper is a feasibility study it would be useful to have more details of the power calculation and a reference for choice of the method used.
Response: reference added

2) Grammatical errors on page 1 (conclusion line 1, page 12 (2nd line), page 14 (line 8), page 3, para 3 (repetition), page 11, line 15.
Response: I have dealt with these but not the Pg 14 line 8 as there are only 2 lines on this page and this may be a typo.

Discretionary Revisions (which the author can choose to ignore)

Page 12, para 2. I would argue that using traction in isolation is not a limitation in the design of other traction studies.
Response: added in the word “possible” limitation. I take your point if you are looking at traction in isolation then this is not a limitation. However my argument was that traction is not used in isolation so why look at its effectiveness on its own. I accept though they are 2 different research questions. See Delitto comment added, suggesting we need to look at treatments in the clinical setting and the survey I had conducted did not demonstrate that traction was used in isolation. I hope this is clear enough within the text but thank you for your comments.