Author’s response to reviews

Title: Sagittal plane gait characteristics in subjects with hip osteoarthritis with mild to moderate symptoms compared to healthy controls: A cross-sectional study

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Author’s response to reviews: see over
Details of revision

We thank both the Editor and the reviewers for their thorough evaluation and valuable comments to our original manuscript. Reworking the manuscript in accordance with their suggestions has clearly improved the quality and readability of our paper. We have responded to each comment made by the Editor and the two reviewers successively in this report. Our responses and explanations to the changes made are given in italics.

Author’s responses to the Editor

Editor’s comments:

Thank you for your manuscript. Both reviewers have provided valuable feedback and suggestions for improvement, and all comments of both reviewers should be addressed in a revised manuscript. Please provide an abstract and also more clearly discuss the implications of your findings for clinicians in the discussion.

We have tried to meet the suggestions by the Editor by rewriting and extending clinical implications that in the originally submitted manuscript only were mentioned as part of the conclusion. In the revised manuscript, clinical interpretations and suggestions for future studies appear in the last paragraph of the discussion. The conclusion has as a consequence been shortened. In addition, the changes made to the discussion in response to suggestions from the reviewers, hopefully further contribute to clarify our results and their potential impact.

Abstract: Please ensure that you include an abstract in the manuscript file, and that the abstract is identical in the manuscript file and on the submission system. Abstracts should not cite references, nor refer to figures or tables. Please check the instructions for authors to ensure that your abstract follows the correct structure for this journal and article type.

An (identical) abstract with the required structure is now included both in the manuscript file, and separately uploaded in the submission system.

Also, please add details of the related trial and the TRN to the methods section.

As required, we have added more information on the overall RCT in the first paragraph of the methods section, and also included the reference number from www.clinicaltrials.gov for the overall study.
Author’s responses to the reviewers

Reviewer’s report

Title: Gait characteristics in hip osteoarthritis patients with mild to moderate symptoms compared to healthy controls

Version: 1 Date: 15 September 2012
Reviewer: Yong-Hao Pua

Reviewer’s report:
The authors have conducted a cross-sectional study to compare people with and without mild-to-moderate symptomatic hip OA on several physical impairments (hip ROM and muscle strength) and gait variables. The study methodology was generally appropriate and the sample size was relatively large (when compared with sample sizes of most previous gait studies). Furthermore, the paper is clearly written and it should be of interest to the readers of BMC Musculoskeletal Disorders. That said, the authors have made some statistical and methodological choices that I cannot fully agree with, and my concerns are detailed below.

- Major Compulsory Revisions

Background

The secondary aim of the study was to evaluate the influence of radiographic OA severity on the various gait parameters. Can the authors justify why they have chosen to use a minimal joint space (MJS) cutpoint to dichotomize their patients into 2 groups instead of analyzing MJS as a continuous variable? To be sure, the authors admitted (on page 11) that there is no absolute consensus on subcategories for radiographic severity based on MJS in hip OA. Given the arbitrariness and statistical limitations (e.g., the biggest of which is low statistical power) associated with categorizing patients into subgroups, the authors need to provide clear and strong justifications for dichotomizing continuous variables.

As noted by the reviewer, we agree that there is a lack of an overall consensus regarding a cut-off for severe and less severe radiographic OA. Another option for our paper would, as suggested here, be to use MJS in mm and evaluate associations on a continuous scale. However, in line with Jacobsen et al. (2004) who defined a MJS of ≤2 mm to be the radiographic criterion most strongly associated to self-reported hip pain; ≤2 mm MJS is also the established cut-off used by the orthopedic surgeons at our clinic when categorizing patients with hip OA. Therefore, rather than trying to identify a new cut-off from our material, we aimed at investigating differences based on our existing clinically anchored criteria. We acknowledge that the rationale for defining our two subgroups of patients was not described clearly in the manuscript, and have now emphasized the above reasoning both in the Background and Discussion paragraphs in the manuscript.

Results
The body-mass adjusted muscle strength values are disproportionately large and I suspect that these are unadjusted values. Please provide the body mass-adjusted data and results.

The muscle strength values presented in Table 1 are normalized to bodyweight in kilograms (peak torque/kilogram bodyweight*100; values given as Nm/BW*100, specified in the abbreviations). Arokoski et al. (2002) published data on isokinetic hip muscle strength in male subjects with and without hip OA, in a comparable age range and measured at the same velocity (60°/second) as our data. Their unadjusted peak values were higher than ours for both extension and flexion, which is plausible due to the fact that we have included both males and females in our cohort: Arokoski et al found hip OA patients to have a mean peak torque of 161±51 Nm at hip extension and 106±31Nm at hip flexion; whereas corresponding values for controls were 186±40 Nm and 130±29 Nm. In our cohort, the hip OA patients had unadjusted mean peak torque values of 145±58 Nm at extension and 88±33 Nm at flexion, whereas our corresponding values for controls were 164±58 Nm for extension and 98±35 Nm for flexion. Further, Danneskiold-Samsøe et al. (2009) published age- and gender specific normative values for isokinetic hip muscle strength at extension and flexion at 60°/second, that for both men and women within the same age range are comparable to our control subjects. Thus, our strength values do not seem to be abnormal. However, we have normalized our data to bodyweight in kilograms. We are aware that in some studies, torque values are normalized to bodymass in Nm; which will naturally give another output.

One plausible explanation that our subjects still might have somewhat higher muscle strength levels than reported in some other studies concerning hip OA, may be that our inclusion criteria were patients with mild to moderate symptoms; not eligible for total hip replacement. This is in contrast to the majority of existing biomechanical studies on hip OA, where patients have severe symptoms and in most cases are candidates for surgery. These patients, with more severe symptoms, are probably less active; with consequently reduced muscle strength. Further, our subjects also overall revealed a relatively low BMI, which may further support the possibility of them being at a higher level of physical fitness than the population in other studies concerning hip OA.

References for isokinetic hip muscle strength values:


Discussion

The authors are at pains to explain why velocity is not a covariate in their regression analyses (they argue that velocity is the mediator of the relationship under study and hence, its inclusion in regression models may lead to over-adjustment). With that said, however, consistent with previous studies, I feel that the authors should still perform the velocity-adjusted analyses and issue the
appropriate caveats. To my mind, these analyses would potentially allow us to identify OA-related gait parameters which are NOT attributed simply to a slow walking speed.

We acknowledge these concerns. In the original manuscript we included linear regression models in an attempt to disclose the explanatory value of velocity to the observed variances. We agreed that this was not an ideal approach. In the revised manuscript we have therefore replaced these analyses with simple logistic regression models where the key kinematic and kinetic parameters identified as significantly different between patients and controls, and between the patients with severe and less severe ROA, are adjusted for velocity. These adjusted analyses are incorporated in the text in the Results and Discussion.

The authors recognize that multiple testing increases the false discovery rate in their secondary analyses. What of the main analyses?

Biomechanical gait analyses in general reveal an abundant amount of data, often with large standard deviations. As emphasized in the introduction, numerous variables have been reported in previous biomechanical studies on hip OA. At the same time, the number of patients included in biomechanical studies is typically relatively low, given the demanding nature of motion analyses with regard to both data collection and analyses. This may cause a concern with regard to multiplicity when interpreting results – a concern that grows when introducing sub-group analyses. From the literature, we have only identified one other biomechanical study on hip OA with a sample size comparable to ours (Dujardin et al., 1998). We consider the large sample size – from a biomechanical point of view – of our main analysis to reduce the chance of misinterpreting random variables to be true statistical findings. For the secondary analyses, the problem of multiplicity is larger, as the number of subjects in each group is considerably lower. We still chose to perform the analyses also for the secondary, as we think the hypotheses that can be generated are of considerable clinical interest. However, we chose for this part to explicitly emphasize in the Discussion that these results should be interpreted with caution.

The concept of motion discontinuity is interesting but it is abruptly introduced only at the end of the paper. Can the authors reword the paragraph to include a clearer topic sentence? Or, the authors may want to consider the analysis of motion discontinuity as part of their secondary analyses.

We agree with the reviewer that this concept is introduced without a direct link to our own analyses and results. As both reviewers had similar concerns with regard to this part of the discussion, we have chosen to delete this paragraph, in line with the suggestion from Reviewer 2. Even though we find the concept of motion discontinuity interesting, we do agree that we should address this topic first when we have performed the same analyses on our own data. However, we regard this to be out of the scope of this paper. We will rather consider performing these analyses on our own data and present the results in a future paper.

- Minor Essential Revisions

Outcome measures, gait analysis section
Please explain why a p<0.05 was used to support the statement that “there were no significant systematic differences between these patients and the patients with unilateral involvement on any of the outcome variables.”

This is simply a typographical error; the symbol should obviously be “>” and not “<”. This is corrected in the revised manuscript. We apologize for this confusion.

Results
The authors sensibly state that their findings related to the secondary aim of this study should be regarded as an explorative supplement to the main analysis and hence, they should be interpreted as hypothesis-generating rather than conclusive. For this reason, please consider re-organizing the results section such that the findings of the primary and secondary analyses are presented in separate sections.

The results have been re-organized in line with the suggestion from the reviewer. As a consequence, Table 2 and 3 have been switched with regard to content. In addition, to improve the thread in the manuscript, the discussion has also been re-organized, so that the paragraph containing most of the discussion related to the ROA subgroup analyses has been moved to a later part of the discussion.

Discussion
page 12, analysis of variance: Shouldn’t that be an “analysis of co-variance” given the main thrust of Wilson’s paper (referenced by the authors) was that gait velocity should not be included in the regression analysis as a covariate?

As previously explained we have replaced the original linear regression models with a binary logistic regression model for each biomechanical variable found to be significantly different between patients and controls and between patients with MJS ≤/> 2 mm. Thus, the above addressed concern is no longer present in the manuscript.

Table 1
• Please include gait velocity in your variable list
• Please include the 95% CI for the “mean difference” column

Changes performed in accordance with the suggestions from the reviewer, for all Tables. Further, all numbers have been double-checked and typographical errors corrected. Finally, we have added one decimal to the SDs, so that all given SDs and CIs have one more decimal than the value it is calculated from.

• I am a little troubled by the fact that the SD ratio between the 2 groups is greater than 2 for some of the variables (e.g., hip flexion and external rotation ROM). Please consider the use of the Welch t-test. Do note that the limitations of the Student t-test are inherited by the Mann-Whitney U-test when the equi-variance assumption is violated. In the P-value column, please indicate which tests which are used.

All outputs from the t-tests for the normally distributed data were checked with the Levene’s test for variance when the original analyses were done. In accordance with the
suggestions from the reviewer, it is now specified in the Tables whether the Students t-test or the Welch t-test is reported; based on whether we can assume equal variance or not for the variables. For the non-normally distributed variables, use of the Mann-Whitney test is specified. We have included this information in all tables to make the results consistent.

Grammatical and typo errors

Background
o pg2: “had focus” should read “had focused”
o pg3: Please remove the repeated word, “this”

Materials and Methods
o pg4: “Subjects that” should read “Subjects who”

Changes performed in accordance with the suggestions from the reviewer.

- Discretionary Revisions

Study Title
This paper has a fairly ambitious title (gait characteristics) but the authors only reported sagittal plane gait data. Please consider re-wording the title. Also, per STROBE guidelines, please consider re-wording the title to include the study design.

We agree with the reviewer that adding “Sagittal plane” to the title would give a more precise notion as to what we have done. We also agree that it would be informative to add “A cross-sectional study” to the title. If the Editor approves the length of the title these changes will imply, we are happy to change the title in accordance with the suggestion from the reviewer.

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:
I declare that I have no competing interests
Reviewer's report

**Title:** Gait characteristics in hip osteoarthritis patients with mild to moderate symptoms compared to healthy controls

**Version:** 1  **Date:** 14 September 2012

**Reviewer:** Joaquin Barrios

**Reviewer's report:**

MAJOR Compulsory Revisions

There is no abstract in the materials given!

*The abstract was somehow left out of the original submission, for which we cannot do anything but apologize. An updated abstract is included in the revised submission.*

Background, P1: Provide citations when mentioning the studies evaluating early stage hip OA

*Citations included as suggested by the reviewer (page 1).*

It is questionable whether Background, P2, Sentences 4-6 are needed in your intro - these sentences seem to detract from the idea of using radiographically driven subgroups. It begs the question of why symptom-driven subgroups were not also explored.

*We agree with the reviewers’ comment. Sentences have been removed from manuscript.*

M&M, P1, Sentence 1: Stay in past tense, please address throughout paper

M&M, P1: Please include the anchor scores for the HHS in your description

M&M, P1, Exclusion criteria: "resent" should be "recent"

M&M, Gait analysis: How was 50% of stance determined methodologically?

M&M, Radiography assessment: What software was used to view and make measures on the digital films?

M&M, Analysis: Replace "nonparametric" with "non-normally distributed"

*Changes performed and additional information provided as suggested by the reviewer for all comments.*

M&M, Analysis: Please provide more detail for how the MLR was conducted. It doesn't appear from the reading that the MLR "controlled" for velocity. It sounds like it was conducted as a corollary analysis to observe the R-square value between walking velocity and the given key variables. Unclear from the reading.

*We agree that this came out somewhat confusing. In the original manuscript we included linear regression models in an attempt to disclose the explanatory value of velocity to the observed variances. This was not an ideal approach. In the revised manuscript we have therefore replaced these analyses with simple logistic regression models where each of the kinematic and kinetic parameters identified as significantly different between patients and...*
controls, and between the patients with severe and less severe ROA, are adjusted for velocity. These new data are incorporated in the text in the Results and Discussion.

Results: When describing group differences, rather than repeating what is in the table (means, SDs and p-values), please given the difference between groups in the appropriate units. Example: "Hip excursion was lesser in the patient group by 5 degrees on average." Same for joint space difference. Otherwise, it is redundant to the well-presented tables.

Changes performed in accordance with the suggestions from the reviewer.

Results tables: Add walking velocity data

Information added in accordance with the suggestions from the reviewer.

Discussion, P3: How do your findings compare to the Watelain study?

Sentence included on the findings from Watelain et. al (page 13).

Discussion, P6: This paragraph is not well presented and very long. No actual topic sentence, and subject matter is all over. Starts out talking about loading, but finishes with passive energy storage. Break up and re-work.

Paragraph shortened and rewritten in accordance with the suggestions from the reviewer (page 15).

Discussion, P8: This entire paragraph seems to come out of the blue. You present a concept that you neither fully describe nor actually did in the study. If incorporating this MD concept is important to the authors, that is fine, but it comes across as agenda-driven. Please reduce the emphasis on this if keeping is important - otherwise delete.

We agree with the reviewer that this concept is abruptly introduced. As both reviewers had similar concerns with regard to this part of the discussion, we have chosen to delete this paragraph, in accordance with the suggestion from Reviewer 2.

MINOR Essential Revisions

Background, P2, Sentence 3: Change "between" to "within"
Background, P3, Sentence 1: Delete double word
Results: 2nd to last P: reword "significant reduced" and "deviations was observed"
Discussion, P1, S1: "manifest" should be past tense
Discussion, P4: Sentence 1 is a run-on sentence. Also fix the comma
Discussion, P4: Reword "inferior level of disease in our material"
Discussion, P4, last sentence: Reword "velocity do have"
Changes and rewording performed as suggested by the reviewer for all comments. With regard to the last comment, this paragraph has been rewritten and the original sentence is no longer included.

Level of interest: An article of importance in its field
Quality of written English: Needs some language corrections before being published
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I declare that I have no competing interests