Author's response to reviews

Title: Obesity and pronated foot type may increase the risk of chronic plantar heel pain: a matched case-control study

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Author's response to reviews: see over
Response to reviewers

Manuscript: ‘Obesity and pronated foot type may increase the risk of chronic plantar heel pain: A matched case-control study’

Thank you to both reviewers for providing considered and insightful feedback regarding our manuscript. Many important points were made by both reviewers which have been dealt with individually below.

Reviewer: Scott Wearing

<table>
<thead>
<tr>
<th>Comment</th>
<th>Response</th>
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<tbody>
<tr>
<td>1. Title &lt;br&gt;The title does not include the study design as requested in the Instructions for Authors</td>
<td>The title has been altered to include the study design</td>
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<tr>
<td>2. Page 5, Final Paragraph. &lt;br&gt;While the authors highlight the lack of prospective research designs in CPHP, the paragraph, as written, orientates the reader to expect that a prospective study design be used; which is not the case. It is not clear from the Background why another case-control study is required or why, in light of the factors listed in Table 1, only certain factors purportedly associated with CPHP were studied in this instance.</td>
<td>The paragraph has been reworded to orient the reader toward a case-control design. Content has been added to explain why the study is required and why only certain factors were selected in this particular study.</td>
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<td>3. Page 8, Paragraph 1. &lt;br&gt;The authors should indicate which reliability coefficient was reported. Reference 10 reported an ICC3,1 of 0.61, with 95%CI of 0.27-0.81.</td>
<td>Assuming that the author is referring to what was reference 9 (Redmond et al., 2006), it is not possible to specify the type of ICC used as this statement refers to several different studies in which different analyses were applied.</td>
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<td>4. Page 8, Paragraph 1. &lt;br&gt;Given the FPI only accounted for between 13% and 35% of x-ray measures (Ref 10) and.45% of midstance values (ref 9), recommend the sentence be changed to “The FPI has been shown to be moderately correlated with….”</td>
<td>The sentence has been changed as suggested.</td>
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METHODS

5. The authors should indicate the resolution to which all measures were

The authors are unsure what is meant by “resolution”. If the reviewer is referring
1. Normality tests – use with caution.  

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<td><strong>taken.</strong></td>
<td>to <em>units</em> of measurement, these have already been indicated in table 2.</td>
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<td>6. Given 3 measures of ankle dorsiflexion were recorded, an indication of the within-subject variability of the measures taken by each examiner would be useful, perhaps in the results.</td>
<td>The average absolute difference between the highest and lowest scores across the three trials of the WBLT was 2.2° for the case group and 2.8° for the controls. The coefficient of variation for these measures was 2.6 and 3.7%, respectively. We do not feel that this data is sufficiently important to rate a section in the results, however we are happy to be guided by the editor as to whether this should be included.</td>
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<td>7. The authors should relate the findings of the ankle dorsiflexion test to the literature. The values reported in the present study, particularly the control group, are somewhat less than those reported by reference 12 (Bennell et al., 1998).</td>
<td>The findings of the current study have now been related to the literature.</td>
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<td>8. Has the accuracy of the occupational rating scale other than content &amp; construct validity been published? Does the scale measure levels of work activity at the time of testing, over a week, a month? This may have important implications to the findings.</td>
<td>Other than in ref 16 (Barber-Westin et al, 1999), no other study has examined the accuracy of the occupational rating scale. The scale measures levels of work activity at the time of testing. The implications of this have now been noted in the discussion.</td>
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<tr>
<td><strong>DATA ANALYSIS</strong></td>
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<td>9. While it is recognised that t-tests are relatively robust, was a skewness &gt;1 the only criteria used to assess whether the data was normally distributed?</td>
<td>Normality of data was explored using the skewness statistic, and observations of the normal and de-trended Q-Q plots. Other tests of normality, such as the Kolmogorov-Smirnov test and Shapiro-Wilks test were not used, as (i) the K-S is sensitive to &quot;deviations&quot; in the midrange, which are not usually the kinds of departures that lead to problems in inference, and (ii) the S-W test is problematic when several scores in a dataset are the same.¹</td>
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¹ Normality tests – use with caution.
### 10. The authors should indicate which variables required transformation.

This was an error on behalf of the author. None of the variables required transformation so the statement has been removed from data analysis section.

### 11. The authors should provide greater detail regarding the use of logistic regression. In particular, how were underlying statistical assumptions tested?

The key assumption of logistic regression is that the independent “predictor” variables are independent. This was confirmed by non-significant chi-square tests between each of the three predictors (obesity, FPI≥4 and WBLT≥47). This statement has been added to the statistical analysis section.

### RESULTS

**12. Is it necessary to report P values to 3 places, given significance was set at P<.05?**

All P values have now been reported to 2 decimal places.

**13. In addition to the results provided for the logistic regression (Exp beta & confidence intervals), the authors should provide greater detail regarding the overall fit of the model. It would also be helpful to provide the beta values, their standard errors and the constant in Table 1, so that readers can construct the full regression model.**

Beta values, standard errors and Wald values have been added to Table 3, and the non-significant Hosmer and Lemeshow Goodness of Fit Index (indicating acceptable goodness of fit) has been added to the results section.

**14. It is not clear from the results as to which variables were entered into the model. Presumably these were BMI, FPI and Ankle Dorsiflexion. However, the methods indicate that variables would be included if statistical differences between groups were found at the univariate level. Bodyweight, however, does not appear to be included in the model, presumably because of its (multi)collinearity with BMI. It is also unclear if there is collinearity between foot posture and BMI or foot posture and ankle dorsiflexion. The authors should provide and indication of the extent of (multi)collinearity between predictors and how it was assessed.**

Clarification has been provided in the results section as to which variables were entered into the model.

Bodyweight was not entered into the model due to its obvious collinearity with BMI. As we have stated in response to comment 11, we have now included a statement explaining the use of chi-square statistics to check for collinearity before inclusion of the independent “predictor” variables into the logistic regression.

**15. The authors should clarify which population cut points (quartiles) for FPI & ankle dorsiflexion were determined.**

The lower cut-points are already provided in the second paragraph of the “data analysis” section, and we have now
added the lower and upper values of the upper quartiles to Table 3.

Table 2.  
16. Where 14 separate statistical procedures performed? Are there any implications that the reader should be aware of?  
The authors did not feel that adjustment of the alpha level was necessary as univariate comparisons were essentially undertaken to determine which factors to include in the multivariate logistic regression. Our conclusions are based primarily on the multivariate findings.

DISCUSSION

17. Page 13, Final sentence paragraph 1.  
The authors should indicate that BMI, FPI and Ankle dorsiflexion differed between groups at the univariate level.  
The paragraph now indicates that these factors differed at the univariate level.

While it is evident that the FPI has the greatest odds ratio, I am not convinced the authors provide sufficient information to state that it “is the most important CPHP predictor.” Would not the “importance” reflect the proportion of variance explained by the factor?  
The odds ratio is a measure of the effect size, and is therefore an indicator of the relative importance of each of the predictor variables in classifying participants into the case or control group. However, because the 95% confidence intervals for FPI and BMI overlap, we acknowledge that it may be overstating the case that FPI is the most important factor. We have therefore deleted this comment from the paragraph.

As highlighted by the authors, the study is cross-sectional in nature and, as such, conclusions regarding the stability of foot posture should be avoided, especially since at least one individual suffered heel pain for 8 years. The study cannot confidently conclude that foot posture plays a role in the development of CPHP. This sentence detracts from the paper.  
The authors appreciate this comment, and a reference has now been added to support the assertion that foot posture remains relatively constant from one decade to the next. We agree that the study cannot confidently conclude that foot posture plays a role in the development of CPHP, and have therefore only suggested that it ‘may’ also be a risk factor. We feel that the discussion section is an appropriate forum to speculate about risk.

While the point the authors make is recognised, the discussion regarding weight versus height appears somewhat redundant given that BMI essentially represents a convenient (and useful) method of normalising body weight for differences in height.  
The discussion regarding weight versus height has been removed.
<table>
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<tr>
<th>Page 14, Paragraph 1, Final sentence.</th>
<th>21. Do the authors contend that increased plantar pressures (or forces) are associated with CPHP? If so, this statement should be clarified and supported by appropriate literature.</th>
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<tr>
<td>In this statement we (cautiously) infer that increased BMI may be related to CPHP via an increase in pressures beneath the heel. We are aware that some studies (eg: Wearing, Clin Orthop 2003) have shown that heel pressures are reduced in people with CPHP. However, these studies are also cross-sectional, suggesting that adaptations in gait patterns in response to CPHP lead to decreases in heel pressures, rather than reduced heel pressures causing CPHP. It is therefore still plausible that the initial development of CPHP may be related to increases in pressure caused by elevated BMI.</td>
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<td>22. Page 14, Paragraph 2. The paragraph spends considerable time developing the concept that increased fascial strain may only occur with a substantial deficit in ankle dorsiflexion. It is unclear, how this relates to the increased dorsiflexion noted in CPHP. I would recommend this paragraph be removed or substantially modified.</td>
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<td>The paragraph has been substantially modified, with some previous content removed and the remaining content reworded to achieve greater clarity.</td>
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<td>23. Page 14, Paragraph 3. The authors have provided no convincing rationale (mechanism) as to why increased ankle dorsiflexion may be associated with CPHP, other than it may represent an error associated with the test (i.e. foot pronation discussed in previous paragraph). Consequently, it is difficult to support the statement that “the findings of this study question the role of decreased ankle dorsiflexion ROM in the development of CPHP.” As highlighted by the authors, the study is cross-sectional, thus direction cannot be attributed. Is it possible that the increased ankle dorsiflexion in CPHP may be an outcome of conservative treatment? As indicated in Reference 10 (Young et al., 2006) at least some individuals with CPHP underwent a stretching program. This should be discussed and highlighted within the methods section. Recommend removal or substantial revision of Pearson’s r analysis was undertaken and no correlation was found between the FPI and dorsiflexion lunge scores (r=0.12, P=0.13) so the discussion regarding the influence of foot posture on the lunge test was removed from the discussion.</td>
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<td>Substantial revision of the paragraph has been undertaken. The authors wish to make the following points:</td>
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<td>We feel that our findings question the role of decreased dorsiflexion simply due to the fact that we didn’t find the expected association between CPHP and decreased dorsiflexion.</td>
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<td>The fact that case group participants were allowed to continue with stretching programs if they had commenced the program prior to 8 weeks before the study has been acknowledged in the methods section. However, it has also been noted</td>
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</table>
that at the time of the study none of the participants were undertaking a stretching program and therefore this point was not raised in the discussion.

24. Page 16, Paragraph 3. Would not the same argument made at Page 13, Paragraph 1 hold for the occupational rating scale (i.e. individuals with heel pain may have reduced their work activity secondary to pain?). The authors conclude (Page 15 paragraph 2) that “it is unlikely these two factors have a role in the development of CPHP” and “it appears that the stresses placed on the lower limb during an average working day do not play a role in the development of CPHP. However, this seems overly speculative. Given the cross-sectional nature of the study, is it not possible that people with heel pain reduced their activity to levels similar to that of controls? This paragraph should be modified accordingly.

The authors agree with this comment and the paragraph has been modified to acknowledge the fact that participants in the heel pain group may have reduced their activity levels after developing the condition.

25. Page 17, Paragraph 1. The authors should relate the findings of the calf endurance test to the literature. The values reported for heel pain and control groups in the present study are substantially less than those reported by reference 15, Svantesson et al & Lunsford & Perry. The authors should also indicate if individuals with CPHP experienced pain with this test.

The findings of the Standing Heel Raise Test have now been related to the literature, with an explanation for the fact that the values reported for the heel pain and control groups in the present study are substantially less than those reported by reference 15 and Lunsford & Perry. Svantesson et al was not discussed because the study sample consisted of ten healthy women with a mean age of 24 years.

Pain was not a limiting factor in the Standing Heel Raise Test for any of the participants.

The magnitude of ankle plantarflexion was not measured.

26. Page 16, paragraph 1. Recommend removal of the sentence “As such, calf endurance does not appear to play a role in the aetiology of CPHP.”

The sentence has been removed and replaced with, ‘as with occupational lower limb stress, it can be cautiously speculated from these findings that decreased calf endurance may not play a
27. The authors indicate that diagnostic imaging may have unduly limited the scope of the condition to a single entity, which although improving the rigor of the study, may detract from its generalisation given imaging is rarely used clinically. The point is valid. However, it was not clear (on first reading) that this was the point being made and seems somewhat at odds with the discussion (for the most part); which spends considerable time discussing the plantar fascia.

Plantar fasciitis is the most common cause of CPHP and also the most widely studied so invariably any discussion will focus more on it than other pathologies. However, in accordance with the reviewer’s comments some of the discussion on the plantar fascia was removed in the process of editing the ankle dorsiflexion discussion.

28. A statement comparing the prevalence of obesity in the study groups to national figures would also be useful to the reader and may help establish whether the samples were representative of the wider population.

The prevalence of obesity from the 2004-05 National Health Survey was added to the discussion.

Reviewer: Wendy Gilleard

<table>
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<th>Comment</th>
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<tr>
<td>1. Abstract: the term “univariate” does not describe t-tests</td>
<td>The term univariate was used to describe both t-tests and Mann Whitney U-tests in the abstract as the authors did not feel it was necessary to specify which test was used for each variable.</td>
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<tr>
<td>2. Aim is stated incorrectly as the test investigated differences between groups not an association. This should be corrected throughout the paper.</td>
<td>We disagree with this statement. Determining differences between groups is conceptually no different to determining associations, the only difference being that the variable being “predicted” is dichotomous. The use of the term association has been widely used in papers with similar methodology to our own (Riddle et al., 2003; Rome, Howe et al., 2001).</td>
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</table>
3. It is a major fault of the study that different raters were used with no indication of the agreement between them for the test results. Also given that evidence was presented on the basis of the literature for the inter-rater reliability for most tests the fact that this was not done for the six item FPI where there is no literature evidence of reliability is a problem.

The authors agree with this comment, but unfortunately no testing of the agreement between raters was able to be undertaken. However, the authors feel that the implications of this limitation have been adequately addressed in the manuscript.

4. Tests for normality of distribution based only on skewness are inadequate. At the very least Kolmogorov-Smirnov results should also be used.

Normality of data was explored using the skewness statistic, and observations of the normal and de-trended Q-Q plots. Other tests of normality, such as the Kolmogorov-Smirnov test and Shapiro-Wilks test were not used, as (i) the K-S is sensitive to "deviations" in the midrange, which are not usually the kinds of departures that lead to problems in inference, and (ii) the S-W test is problematic when several scores in a dataset are the same.²

5. How was the data transformed and which variables was this done for?

This was an error on behalf of the author. None of the variables required transformation so the statement has been removed from data analysis section.

6. While the use of independent samples t tests is not unreasonable, a large number of tests were performed. A correction should be made to the alpha value used to account for multiple tests.

The authors did not feel that adjustment of the alpha level was necessary as univariate comparisons were essentially undertaken to determine which factors to include in the multivariate logistic regression. Our conclusions are based primarily on the multivariate findings.

7. Table 2 should detail exact p values. Statistical test statement in the legend is not correct for all reported variables. I also find it difficult to understand how significant differences were found between groups when the Standard Deviations indicate a large overlap between groups eg BMI 29.8(5.4) and 27.5(4.9). Is this related to the transformation procedure?

As exact p values are reported in text, the authors felt that it was unnecessary to report them again in table 2.

The statistical test statement in the legend has been corrected.

BMI did differ significantly between the groups ($t = 2.85, P = 0.005$), which is certainly possible despite overlapping overlap.

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<tr>
<th>Reference and Discussion</th>
<th>Revised Content</th>
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<tr>
<td>8. Reference required to support the statement that foot posture is unlikely to alter after the onset of CPHP.</td>
<td>Reference has been added.</td>
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<tr>
<td>9. The discussion paragraph in relation to calf endurance results does not add to the paper and should be removed.</td>
<td>The discussion regarding calf endurance has been removed.</td>
</tr>
<tr>
<td>10. No discussion on the result that BMI and FPI were only able to classify participants 66% of the time even though the result was significant.</td>
<td>A section discussing the relatively low classification accuracy of the model has been added to the discussion.</td>
</tr>
<tr>
<td>11. The paper title is also misleading. Authors did not investigate “risk”.</td>
<td>We do not agree that the title is “misleading”, as we have stated that these factors may increase the risk of CPHP – we have not stated that they are risk factors per se. In response to Reviewer 1’s comments, the study design has now been acknowledged in the title (i.e.: “a case-control study”) which further qualifies the statement of “risk”.</td>
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