Reviewer’s report

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

Version: Date: 21 November 2006

Reviewer: Scott Wearing

Reviewer’s report:

General
Thank you for the opportunity to review the revised manuscript. The authors have substantially modified the text and, with the exception of the concerns regarding measurement resolution, have largely addressed the queries raised in the original review. I have provided a definition of measurement resolution to help clarify my original concern on this matter. However, in modifying the manuscript a number of new issues have arisen. These primarily relate to the information contained within several newly included references and how they impact on the findings of the current study. I have marked these concerns as discretionary and the authors may wish to consider these matters should the manuscript be accepted for publication.

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

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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)

>> 1. The resolution of measurement refers to the smallest unit increment to which an attribute value is measured, whatever the units of measurement.

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Discretionary Revisions (which the author can choose to ignore)

1. Background: The authors have strengthened the rationale for the study. However, it is still remains unclear as to why Table 1 presents the level of evidence for only selected factors listed in the introduction and reference 9 (Table 1 appears to be a modified version of the summary table (table 9) from reference 9). For instance, why were MPJ extension, fascial thickness and presence/absence of a calcaneal spur not measured? Perhaps the study was restricted to clinical measures that are readily available to-, or routinely used by clinicians in the assessment of heel pain? While I appreciate that the tests employed were probably selected because of their simplicity, non-invasive nature and clinical relevance, I am not convinced that only factors that could be measured using reliable and valid techniques were examined. Certainly, based on the information provided by the authors, the validity of several measures used in the study (eg lunge test, occupational rating scale) does not appear to have been adequately established within the literature. Similarly, as highlighted by the authors, the reliability of the FPI-6 has not been established within the literature.

2. Outcome Measures – Foot Posture: Given the uncertainty as to which statistic the cited reliability coefficients refer, perhaps it may be more appropriate to cite reference 12 (Menz & Munteanu), which reported an ICC3,1 of 0.61, with 95%CI of 0.27-0.81?

3. Table 2. The authors may wish to reconsider presenting means and standard deviations for occupational rating scale variables, given that none of the data was (or could be) transformed into a normal distribution and non-parametric statistics were used in the analysis.

4. Discussion: The current study reported a 1.3 point difference in FPI between CPHP and control groups and the authors have provided a reference indicating that arch height essentially remains constant from one decade to the next. Intuitively, I would agree that this is likely the case. However, if I recall correctly, the cited study was conducted on non-pathological feet. The plantar fascia is theorised to play a role in supporting the arch. Would the authors draw similar conclusions if the condition being studied was rupture of the plantar fascia? Granted, it is not. Interestingly, the newly included reference 28 (Scott & Menz) reported a comparable (1.6 point) difference in FPI between younger (20 years of age) and older (80 years of age) age groups, which was hypothesised to represent an age-related flattening of the foot (albeit over a
60 year period). Of greater interest, however, was that mean FPI values for healthy young individuals cited in reference 28 (Scott & Menz) are remarkably similar to those of the CPHP group (mean age: 50 years), while control group values (1.1) are substantially lower than those of young individuals. This would seem to suggest that the CPHP group had a normal foot posture, while the control group had a much higher arch posture than healthy young individuals. Cain et al. (2006), suggest that FPI-6 values <2 points are representative of cavus foot types. If the authors contend that arch height does not change substantially in CPHP, then perhaps they may wish to address why the values reported for the control group appear at odds with those cited in reference 28 (Scott & Menz) and in light of the comments of Cain et al.?

5. Discussion: If the authors contend that plantar force or pressure is related to CPHP, then perhaps reference to the work of Spears et al (2005) may be helpful in supporting their statement.

6. The authors should be commended on examining the effect of foot posture (FPI) on ankle dorsiflexion (lunge test). However, there is evidence that foot posture does not necessarily reflect foot motion (at least during gait). Given that a major source of error associated with the lunge test is likely related to the amount of pronation occurring during the test (rather than foot posture, per se) it would seem premature to have removed the discussion regarding this limitation of the test.

7. The authors have indicated that participants were allowed to continue with conservative treatments but at the time of testing no participant was actively undertaking stretching. While I am aware that some studies have shown ankle ROM remains unaffected by stretching, there is also evidence that stretching may increase ankle dorsiflexion and that the effect may be maintained (or partially retained) for at least 3 to 4 weeks following its cessation (Brucker et al. 2005; Guissard & Duchateau, 2004). Are the authors suggesting that stretching is unlikely to have any effect on lunge test values? If so, some comment to the effect would seem warranted. Otherwise, it would seem reasonable, given the information provided by the authors, that stretching prior to testing may explain the increased ankle dorsiflexion noted in the CPHP group. The authors may wish to address this point within the discussion.

8. Discussion: The authors have indicated that, in contrast to previous research showing a weak association between ankle equinus and heel pain, the lunge protocol (employed in the current study) preferentially tested the soleus muscle. This of course raises the question as to whether a tightness of the gastrocnemius (as opposed to the soleus) muscle group could have existed in the CPHP which would remain undetected by the lunge test.

9. The authors suggest that the reduced ankle dorsiflexion noted in their study (relative to the literature) was likely due to the fact that participants were 30 years older. They also provide a reference indicating that ankle dorsiflexion (as measured by the lunge test) is significantly less in older (80 years of age) than younger (20 years of age) individuals (ref 28). However, the dorsiflexion values for the CPHP group are almost identical to the younger group of reference 28 (Scott & Menz), despite the CPHP group being ~30 years older. If the authors are to justify that the reduction in ankle dorsiflexion is age-related using reference 28 (Scott & Menz), they may wish to explain this apparent contradiction within the discussion.

10. I can understand that if one were to assume that activity levels remained stable across both groups, then it might be reasonable to speculate that CPHP was not related to occupational limb stress. Perhaps this is an issue of semantics, but at the very least I would recommend that “greater occupational limb stress” or “prolonged standing” be used instead of “increased occupational limb stress” as the latter, at least to me, implies a change over time. It is interesting that the overall score in both groups equate to ‘very light’ and that the CPHP group was restricted in performing the more vigorous activities (squatting and carrying load). The authors indicate that the lower scores are likely associated with the pain of CPHP. However, in the absence of information regarding co-morbidities and work practices (the scale was originally designed to assess knee function at work) it would seem difficult to attribute the lower scores solely to CPHP.

11. If pain was not a factor in the performance of the standing heel rise test, then perhaps the authors might wish to indicate this clearly within the text (intuitively one would expect pain to limit performance on the test) Reference 17 (on which the measure appears to be based) also raises the possibility that the test may not be sufficiently sensitive in certain clinical populations. Is it possible this is the case in CPHP?

12. Discussion: The authors state the proportion of the case (CPHP) group with obesity is 21%, which is inconsistent with the values cited in Table 3. The authors may also wish to clarify/confirm whether the 18% figure cited for obesity in the Australian population represent data from males only, all persons 35-44 years of age or all persons 45-54 years of age?
What next?: Accept after minor essential revisions

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:
'I declare that I have no competing interests