Reviewer's report

Title: Obesity and pronated foot type may increase the risk of chronic plantar heel pain

Version: 1 Date: 20 September 2006

Reviewer: Scott Wearing

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General

1. In general the manuscript is well written and the findings, on the whole, corroborate those of previous studies. However, the background and rationale for the study is overly brief and needs to be substantially strengthened. The statistical procedures are generally under-reported and do not provide sufficient detail for replication. As highlighted by the authors, there are methodological and in interpretation issues regarding some of the reported clinical tests, particularly the accuracy ankle dorsiflexion test.

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

1. Title
The title does not include the study design as requested in the Instructions for Authors.

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.

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.

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...."

METHODS

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

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.

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.

8. Has the accuracy of the occupational rating scale other than content & 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.

DATA ANALYSIS.

9. While it is recognised that t-tests are relatively robust, was a skewness >1 the only criteria used to assess whether the data was normally distributed?

10. The authors should indicate which variables required transformation.
11. The authors should provide greater detail regarding the use of logistic regression. In particular, how were underlying statistical assumptions tested.

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

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.

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.

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

Table 2.
16. Where 14 separate statistical procedures performed? Are there any implications that the reader should be aware of?

DISCUSSION
17. Page 12, Final sentence paragraph 1
The authors should indicate that BMI FPI and Ankle dorsiflexion differed between groups 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?

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.

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.

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.

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.

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 (ie 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 8 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 paragraph.

Would not the same argument made at Page 13, Paragraph 1 hold for the occupational rating scale (ie 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 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.

Did the magnitude of ankle plantarflexion differ between groups during the calf endurance test? This may be important in individuals with heel pain.

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

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.

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.

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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)
1. Title
The title does not include the study design as requested in the Instructions for Authors.

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: Acceptable
Statistical review: No

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