Reviewer's report

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

Version: 3 Date: 13 March 2007

Reviewer: Scott Wearing

Reviewer's report:

General

Thank you for the opportunity to review the revised manuscript. In my prior review, I suggested a number of discretionary changes. With the exception of the concerns regarding measurement resolution (which remains unaddressed and will influence the number of decimal places to which data is presented), the authors have commented on the majority of the queries raised. However, some issues remain unresolved, 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 authors should indicate the resolution of all measures

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Discretionary Revisions (which the author can choose to ignore)

1. The FPI values noted for the CPHP group (2.4) are similar to those noted by Scott, Menz & Newcombe (2006) in healthy young individuals (2.5), while values for the control group are substantially different (1.1). This would seem to indicate that the control group used in the current investigation had an abnormally high-arched foot posture. The authors have provided no convincing rationale as to why they have not discussed the FPI scores relative to the literature. Given that the FP6 is a newly developed measure which has not been commonly used, it would seem even more important that comparisons to any existing literature be made.

2. The demonstration that static foot posture is correlated to midstance FPI scores merely indicates that individuals with low arches in stance are likely to have low arch while walking. It does not demonstrate that foot posture is in any way related to foot motion.

3. Again, if the authors contend that stretching is unlikely to have influenced lunge test values then some comment to the effect would seem warranted. It is inappropriate to imply the strength of a finding based on P values, which merely indicate whether the result is significant or not.

4. The authors previously indicated that, with the knee flexed, the lunge protocol employed in the current investigation preferentially tested the Soleus muscle. In the current version of the manuscript, they seem to argue that testing with the knee extended preferentially tests muscle tightness rather than joint stiffness. Intuitively, both positions (knee extended and knee flexed) would test muscle tightness and joint stiffness. Thus, neither test would discriminate between joint stiffness or muscle tightness and the argument made by the authors for employing the test does not hold. If it is possible that tightness in the Gastrocnemius muscle group may have gone undetected in the case group, then surely, this is an important limitation which should be recognised? This would not detract from the findings but would merely alert readers of the possibility.

5. The authors suggest that the reduced ankle dorsiflexion noted in their study is likely an age-related effect. In doing so, they cite a paper in which the ankle dorsiflexion values for health young controls are identical to those of the CPHP group (Scott, Menz & Newcombe, 2006). Surely, this raises the possibility that there is not an age related decrease in dorsiflexion in this instance?
6. The authors seem to argue that the finding that the CPHP group values were identical to healthy young adults indicates the CPHP group has an abnormal range of dorsiflexion as they expect an age related decrease in ankle dorsiflexion in all study participants. However, given that the values for the control group were substantially less than healthy young adults, it also raises the more alarming possibility that the control group has a significant limitation of dorsiflexion (rather than the CPHP group having greater dorsiflexion). Is it not possible that this is the case? If so, the authors should indicate this within the text. If not, the authors should explain the apparent contradiction to the findings of Scott, Menz & Newcombe (2006) within the manuscript.

7. While I appreciate the potential of an age-related decrease in Ankle range of motion, Reference 28 does not show that ankle dorsiflexion decreases with age. It merely demonstrates that elderly individuals (80yrs) have less ankle dorsiflexion than healthy young adults (20yrs). Clearly, when comparing FPI and ankle dorsiflexion data to that of Scott, Menz & Newcombe (2006) it would appear that the CPHP group have foot posture and ankle dorsiflexion values similar to healthy “normals”, while the control group has an abnormally high-arched foot posture with limited ankle dorsiflexion. If the authors are to use this reference, it would seem appropriate to address this possibility within the manuscript.

8. My concern with the occupational rating scale relates to the fact that it merely measures the time/frequency that participants spend doing each activity. In contrast to statements within the discussion, it appears that the scale is unable to discriminate between standing and walking, as these form a combined question (unless a modified version was used), nor does it report the properties of the surfaces involved. Thus, the measure merely supplies a self-reported indication of the frequency of lower limb loading of an individual at a given point in time. As such, it only partially assesses physical work practice. While the authors have provided some further information regarding co-morbidities that may influence the scale, there is no indication of the type of work performed (labouring verses desk) or whether participants were actually limited in performing these activities.

9. The use of the term “prolonged standing” throughout the manuscript to represent an item option entitled “standing/ walking” is somewhat misleading. The authors may wish to address this within the paper.

10. The authors should provide information within the manuscript regarding how data pertaining to co-morbidities was collected and what criteria were used to define co-morbidities (medical diagnosis, medicated, self-report etc).

11. Finally, the authors should indicate within the manuscript whether subjects were individually matched on age and gender or merely frequency matched. Clearly, the former would invalidate the majority of the statistical methods employed.

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, the manuscript does not need to be seen by a statistician.

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