Reviewer's report

Title: Relationships between the Foot Posture Index and foot kinematics during gait in individuals with patellofemoral pain syndrome

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Reviewer: Trevor D Prior

Reviewer's report:

This is a well written paper and of value to the readership. There are a number of points that require addressing / answering prior to publication and have been detailed below.

Abstract, conclusions

The authors state that the FPI was moderately associated with some parameters of dynamic foot function in both groups. In the results, this association is detailed as 23 & 28% in the PFPS group and 37% in the control group. Can these be described as moderate as these figures appear quite low, certainly for the PFPS group. Is there an appropriate reference which details a grading for this type of association? If there is no clear guideline, whilst perhaps a question of semantics, I would suggest this association is mild considering previous work evaluating the predictive value of the FPI for dynamic function was over 40%.

Procedures

The authors note that the tested limb was the symptomatic limb (or most symptomatic when bilateral) in the PFPS group and was randomly matched for the control group. Accepting that the aim of the study was to evaluate the relationship of the FPI to PFPS this, by nature, is evaluating the association of foot function to PFPS. By only evaluating one limb, the authors are automatically excluding the role of asymmetry of the two limbs and is a potential weakness of the study. This should be acknowledged / commented upon. I note that the authors are using the same group for at least one other study. Do they have data that compares symmetry of function?

Kinematic analysis

The Vicon system was used for motion analysis. The authors appear to have utilised a standard protocol although this would require expert opinion to confirm although an appropriate reference has been provided. However, I am unclear as to how the baseline rearfoot position was identified and the effect this may have on results. From the information provided, it would appear that the various markers were applied, a relaxed stance position adopted, a specific knee alignment device utilised and the baseline determined.

Given that two of the three significant parameters (timing of peak rearfoot angle in PFPS group and range of eversion in control group) were in relation to the
laboratory, this would appear particularly relevant. Was the rearfoot segment calibrated in relaxed stance or is the foot position controlled in anyway? Does this effectively ‘zero’ the relaxed stance position? I appreciate that whatever method used is based on a standard protocol but this may have some effect on outcome, particularly as the FPI uses the relaxed rearfoot position as one of the measures to determine foot position.

In the discussion, the authors note the lack of association of rearfoot eversion relative to the tibia compared to an observed association relative to the laboratory. This suggest this may be due to the influence of the tibia and individual variations. There are two other potential causes which should be considered:

1. The baseline setting of the system may alter the relative motion
2. The FPI has six discrete variables, the majority of which are direct foot measures. Whilst the fibula malleolus is one measure, there is no evidence that this is reflective of tibial position – thus the FPI is not reflective of tibial position and may not reflect tibial function. By contrast, at least two of the FPI measures are compared to the supporting surface (rearfoot eversion angle and arch height/profile).

In the abstract and conclusion, I feel it is important to note that the eversion findings are relative to the laboratory.

Statistical analysis

The author’s state that ‘…were calculated to determine the association between each of the FPI and kinematic measures during walking.’ This could be interpreted that the 6 individual measures within the FPI were compared to the kinematic measures whereas the results section indicates it was the total FPI score that was used.

There does not appear to be any detail as to the method of statistical analysis, particularly for the associations between FPI and specific gait parameters and the variance. This should be included although I do not feel adequately qualified to assess the statistics.

Discussion

The authors discuss the relevance of the timing of peak eversion in the PFPS group versus the range of eversion in the control group. This is a good review of the literature and points well made. However, they should also consider referring to the paper by Cornwall & McPoil (2009) which identified 4 patterns of rearfoot eversion which related to the timing of peak eversion. This is a potentially important finding in that any group could be further divided into the pattern of eversion which, as this current paper suggests, may be relevant whether it be cause or effect. Did the authors consider evaluating the duration of eversion?

The authors also highlight some of the limitations of the study. Given that PFPS is a generally a sport related condition, there was no evaluation of running, only
walking and this should be identified.

References
There is no date for reference 9.

Reference: Cornwall MW, McPoil TG, Classification of frontal rearfoot motion patterns during the stance phase of walking, JAPMA 2009, 99(5);399-405

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

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