Reviewer's report

Title: Clinical Attributes for the Conservative Gait Pattern in Diabetes

Version: 1 Date: 11 June 2008

Reviewer: Anita Raspovic

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Major Compulsory Revisions

1. A significant limitation to interpreting the study findings is the measure of ‘conservative gait’ which was used. This is based on the single, discrete variable of ‘apropulsive foot function’, determined by observational gait analysis. This is not a validated model for measuring ‘conservative gait’ and can be confusing as it attempts to capture a multi-component gait pattern by measuring just one of its component variables. Not surprisingly the other components commonly associated with this gait style are then found to correlate. Does this really add to the body of work in the area? In addition, has this assessment been validated for its ability to detect propulsion / joint power or lack thereof? In regards to reliability for the observational gait analysis, kappa values are reported by the authors as being acceptable. It is unclear how the authors are able to support this claim with lower end kappa values reported at 0.43 for inter-tester reliability data.

Further details need to be provided by the authors justifying the approach used and acknowledging the limitations this creates.

2. A somewhat questionable approach to data analysis appears to have been selected by pooling left and right data. This would be particularly inappropriate given the study evaluates whole body gait variables, such as walking speed and stride length. If this is the case it is recommended that the data are re-calculated as n=152 individuals not n=304 limbs.

3. References to clinical recommendations around interventions for the diabetic foot that are simply not tested by this study. For example, suggesting that exercise training (stretching?) may be indicated in this population due to a small clinical and weak statistical difference between groups in static ankle joint dorsiflexion, is unfounded. The underlying assumptions are that a difference; exists, is clinically important and is compromised to the degree that it may be adversely influencing dynamic motion. This finding may simply be an unrelated correlation given the multiple co-morbidities of diabetes. More importantly however such recommendations incorrectly suggest that a small decrease in static ankle dorsiflexion may be pathologic, not a consequence of a compensatory change made to address a larger problem. A conservative gait is likely to be adopted for viable reasons. Suggesting that this should be altered clinically, without the support of solid evidence and a full understanding of the implications, is problematic. I strongly suggest to the editors that all references
which over extend the findings of this study be removed or re-phased to an appropriate context.

4. Conclusions are made from this research without due recognition of the limitations / design of the study. There are several issues which require acknowledgement including (but not limited to): that gait speed is directly linked to other gait measures such as stride length so will naturally correlate, the limitations with the method used to establish conservative gait, that lack of statistical power may be a reason for neuropathy not being a significant predictor in the model and that there could be a possible influence from other causes of apropulsive gait on the findings, given the clinical population recruited.

Minor Essential Revisions

Abstract

1. In the background, it should be clarified that people with diabetes and peripheral neuropathy (PN) are at a greater risk of falling (line 1). The acknowledgement of the influence of (PN) in this problem separates out who in this extensive clinical population is at higher risk.

2. The statement “Many people with diabetes also adopt a more conservative gait pattern…….” requires re-wording. To my knowledge there are no strong epidemiological studies measuring this phenomenon therefore this statement is potentially speculative and misleading.

3. The last sentence of the background indicates the process for analyzing the study (ie, multivariate approach) not the overarching purpose as suggested. Clarification of the purpose in the abstract is required to indicate the overarching aim to readers.

4. In the methods section it would be useful to include more information on the measures collected given the important place an abstract has in providing key information. I suggest that a list of the clinical and gait variables is included, given this is not too long. Particularly the use of observational gait analysis and plantar pressure measurement should be included. It would also be useful if it were made clear that static ankle joint ankle dorsiflexion was the variable measured (not to be confused with dynamic ankle joint kinematics).

5. The way in which participants were grouped for analysis (ie, into conservative gait pattern or not) and how this was determined should be noted here. Currently this is alluded to but is somewhat confusing until described later in the paper. (See also major revision 1)

6. The methods report n=152 however data reported appears to be on pooled analysis of left and right leg data (ie, n=304). The flaws of this approach are discussed later in this review, however is this approach to analysis is retained it needs to be reported up front in the abstract. In this way the results can be interpreted by readers accordingly. (See also major revision 2)
7. Please refer to JFAR’s protocols for data reporting. I suggest that reporting 4 decimal points for P values is not required and to 2 decimal points is sufficient.

8. The conclusion section of the abstract also requires revision in line with the major alterations suggested above.

Background

9. In the last part of paragraph one, are the authors suggesting that gait changes in diabetes are not associated with PN or is this attributed to the study methodology? It would be useful for the authors to be more specific in their critique and summary of the background literature to provide a more explicit summary of their views on related research. In addition, the discussion around diabetes, gait pattern alterations and falls requires consolidation to provide a clearer platform on which the study is based.

10. More substantiative statements on rationale, aims and potential clinical benefits of the study are required at the end of the background section. Please describe to readers the benefits of adopting a multivariate approach, what will this potentially add to what is already known?

Methods

11. Under general design and study population, it would be useful to know more about this sample. Given the patients presented to a podiatry clinic, what steps were taken to reduce the influence of unrelated lower limb pathology on the study findings. For example, were people presenting with other causes of apropulsive gait, such as active foot injury, Charots neuroparthropathy and Tibialis Posterior dysfunction, excluded?

12. The first sentence under clinical examination is not clear. By ‘norms’ do the authors mean ‘methods’?

13. Please provide a brief explanation of the ankle joint measurement technique used.

14. Please provide figures which indicate measurement error in degree units, for reliability data (ICC’s) reported on static joint motion measures.

15. Describe how the stride length measure was taken.

Statistical Analysis

16. I suggest that the approach to statistical analysis is described in a form which may provide more information to clinicians. Include a brief explanation for conducting several statistical tests on the same data.

17. The apparent decision to pool left and right data for analysis, rather then the individual, is discussed above.

18. I have recommended independent review of the statistics utilised in this
study, to provide additional expert opinion on this issue.

Results

19. Beware reporting statistically significant data without a discussion on whether this is likely to hold clinical significance. The two should not automatically be assumed as the same.

20. For variables reported in Table 1 which are not percentages, standard deviations would be useful.

Discussion and Conclusion

21. As detailed earlier, care should be taken to not draw conclusions which can not be supported by the study data. Eg: the second sentence of the discussion, has this technique for evaluating conservative gait been linked to falls risk? In the conclusion, the issue of unfounded clinical recommendations is raised again.

22. Full disclosure of study limitations should be detailed here.

23. The discussion indicates that the univariate analysis found callus to be associated with conservative gait, amongst other variables. The p value for this comparison reported in Table 1 however is 0.0793. This requires clarification.

Discretionary Revisions

1. (Abstract) In the abstract and throughout the paper, wording such as “neuropathy was significantly higher” at times reads strangely. The authors may wish to review this to reflect, for example in this case, that neuropathy was more severe or alternatively that peripheral sensation was lower. This describes findings in a more clinically applicable sense. Romberg’s sign was another example where this type of wording was used.

2. (Background, para 1) In the background it suggests that clinicians may view gait unsteadiness as trivial. I would suggest that this is not the case and that the implications of gait changes in diabetes, including unsteadiness, are taken as potentially very important clinical changes. I would also suggest that while the correlation with depressive symptoms is important, it is not the main reason why the importance of gait unsteadiness should be understood clinically. The authors may wish to consider these comments in future drafts.

3. (Background, para 1) The statement “fearful walkers adopted a slower gait speed” suggests they consciously elected to do so. I suggest the word ‘adopted’ be changed to something like ‘had’ or ‘presented with’.

4. (Background, para 1) The word ‘fearless’ sounds a little melodramatic and could be better phrased using wording like ‘those walkers not identified as fearful’.

5. (Methods, para 1) Consider amending the sentence “The exact methods have been previously described” to finish with “are overviewed below”. This pre-empts
for the reader that an overview will be provided and does not automatically look like the associated references need to be sourced.

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

**Quality of written English**: Needs some language corrections before being published

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