Reviewer's report

Title: Reliability of the TekScan MatScan(R) system for the measurement of postural stability in older people with rheumatoid arthritis

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Reviewer: Sarah Curran

Reviewer's report:

This is a well presented manuscript that will be applicable to a wide range of clinicians. I have provided comments below which are aimed at clarifying and improving the manuscript.

Major compulsory revisions:

Abstract:
1. In the background section the authors mention ‘…compared to healthy controls...’ and this may confuse readers in thinking that normal data would be collected and reiterated to further correlated with existing data. The authors should perhaps reword this sentence for absolute clarity.

2. In the methods section, the authors need to state the mean age and standard deviation of the RA sample investigated. It would also be useful if the authors state the speed of recordings (40Hz).

3. For the conclusion a further sentence should be added and the existing last sentence should be observed with caution given the wide range of the individuals – this is why the mean and standard deviation should be added to methods section. The range was fairly large at 60 – 80 as stated in your main methods section on page 10 (last paragraph, 2nd line from bottom).

Background:
4. The background section provides the relevant information. On page 4 the authors statement regarding moderate to good reliability of the Tekscan MatScan# is too descriptive and I would expect further details in terms intraclass correlation coefficients/SEM figures. I realise the present authors have used the Fliess guidelines, but not every reader / author uses this classification system, and moderate to good figures can differ.

5. If no reports exist on the reliability of the Tekscan MatScan# has not been undertaken in normals – why weren’t they examined in this study – why were only RA individuals explored? I realise this study is focussed to RA, but reliability data on normal / healthy individuals would be useful.

Methods:
6. Page 5 – the paragraph on clinical characteristics could be better presented - it is all in one sentence and reads like a list! Can the authors amend? For the
procedure – relevant information is provided and the inclusion of a template for foot position is key to enhancing standardisation and reliability of the measures obtained.

7. Was the step off the mat forward or backwards?

8. Data analysis – relevant information provided. It is good to see the inclusion of the Bland-Altman plots.

Results:

9. Relevant findings reports and supported with tables and figures. Table 1 – add ranges to the demographic and clinical characteristics.

Discussion:

10. The discussion picks up on relevant points. The ICCs appear to be good, but the SEM, SEM% and SRD were fairly high. Whilst this is acknowledged by the authors and reasons for measurement error are explored further discussion on the ICCs, SEM, SEM% and SRD should be explored with the latter three parameters having more clinical worth since they represent absolute values rather than the relative values of the ICCs.

11. On page 11, the authors mention that the consistency of the data gathered echoes that of previous studies - Zammitt et al. and Cousins et al., but these studies focussed on barefoot walking rather than postural sway. One study findings cannot give absolute confidence – reliability should always be viewed as a continuum. The findings are very interesting, but there is a gap which remains and lack of comparison for normal data, and to a certain extent there is and was an expectation of direct links to such literature (seen as it is yet to be reported) or normative (case matched ideally) in this study. Whilst this does not omit the paper from being accepted from publication it warrants discussion by the authors.

12. The authors collected data in the first session and then one hour later for a second session. This only represents day variability, and the authors need to provide a rationalisation as to why this was done. Why not measure one week apart? Is there a learned response from the subjects and would fatigue play a part? I doubt it given the amount of repetitions, but it must be acknowledged. I do however acknowledge the complexity of RA patients and from one week to the next, there situation may change which in turn could have a negative impact on the findings.

13. In terms of the number of repetitions of postural sway measurements and in order to improve the SEM, SEM% and SRD – were three repetitions enough? Keijsers et al. (2009) reported that 3.8 steps were required to obtain an ICC of 0.85. Although this is related to dynamic assessment rather than static this observation for the ICC does correlated well with the findings presented here (table 3), apart from from the ML sway EO (ICC at 0.84). As further assessment should the authors consider increasing the repetitions to enhance reliability? The full reference of the Keijsers et al. (2009) is provided below.


14. Was recording at 40Hz quick enough? It should be, but again this needs to be highlighted by the authors.

Conclusion

15. The authors explore the role of future investigations in the conclusion section – this does not really belong here and it should be stated within the main discussion section (albeit towards the end of this section). The authors should address this imbalance and amend accordingly.

Level of interest: An article of importance in its field

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.