Author's response to reviews

Title: Reliability of capturing foot parameters using digital scanning and the neutral suspension casting technique

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Author's response to reviews: see over
Re: Revision of manuscript, Reliability of capturing foot parameters using 3D non-contact digitisation and the neutral suspension casting technique (MS: 7690958364226012)

Dear Dr Munteanu,

Please thank the reviewer for his comments. We have addressed each of his comments both within the main body of the text and below. Revised text in the manuscript has been highlighted in red text and track changes.
Editorial Comments and responses

1. All references amended in accordance with PubMed abbreviations.

2. All text wording of ‘et al’ amended to ‘et al.’

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MAJOR COMPULSORY REVISIONS

1. With respect to the author response #3 regarding ‘word limitations’ – the Journal of Foot and Ankle Research does not stipulate a word limit except for the abstract (350 words). Can the authors please provide a brief statement putting these studies into context and to justify why the current study was undertaken.

Response: We have added the following statement: “In summary, the current evidence suggests that there is conflicting evidence relating to the inter-rater reliability of traditional casting techniques and that not all foot parameters produce consistently reliable results. With the advancement of technology, especially in the field of digital foot scanners....”

2. Page 4 – 4th paragraph. Could the authors please attempt to specify the actual reliability values obtained for Burns et al [7] and Pierrynowski [2] by placing the values in brackets after the words ‘acceptable’ and ‘poor’ inter-caster reliability, respectively.

Response: We added the ICC values for Burns and generalisable coefficient estimates for Trotter.

3. With respect to author response #5 regarding the terminology of ‘within-method’ techniques – it is still unclear to the reader what this means. Could the authors please add some brackets and define what is meant by ‘within-method techniques’?

Response: It is difficult to make assumptions when reviewing the literature. The assumption we have made is that the authors are describing intra-rater reliability. We added in brackets intra-rater reliability. However, the authors may quote differently if our assumptions are incorrect. We have also found that Trotter and Pierrynowski [2] also use a term inter-caster reliability and have added the term ‘inter-rater’ reliability.

4. With respect to author response #11 regarding the raters, please indicate whether rater training was performed, and if it was, please describe the nature/duration of the training. If there was no rater training, please simply state this.

Response: We have added the following statement under a new sub-heading:

Examiners

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Casting and digital scanning was performed by an undergraduate podiatry student of AUT University (rater 1) and an experienced clinician of 13 years (rater 2). To ensure consistency each examiner undertook a single training session prior to data collection.

5. The authors have not adequately responded to comment #14 [Page 7 – Statistical analysis. Could the authors please clarify in the manuscript whether; (i) the data was screened for normality, and (ii) whether hypothesis testing was undertaken to determine whether systematic bias was statistically significant]. It is not a sufficient response to state that the paper was discussed with a statistician.

Response: We have undertaken normality testing for the data. We have added the following sentence; ‘All data was tested for normality.’ We do believe that is not whether the data is tested for normality, but what matters is uniformity of the residuals (error) in the analysis that resulted in the ICC. Or to put it another way, were the change scores between repeated measurements of similar magnitude, regardless of the magnitudes of the scores themselves, which we have addressed in the manuscript.

The second comment relating to hypothesis testing is that we do not believe you need to test for zero correlation in a reliability study, because ICCs are seldom that bad. What matters is the uncertainty in the ICC: how good or bad could the ICC be?

6. Page 10 – 3rd Paragraph. It appears that the comment regarding ICCs becoming ‘improperly high with low inter-subject variance seems to be a typographical error in the study referenced [10] – when in fact the opposite is true. Higher inter-subject variance increases the reliability coefficient. Could the authors please review this comment and modify/delete it if necessary. Portney and Watkins (2000) suggest, “we can demonstrate that as the true variance in a set of scores decreases, the reliability coefficient will also decrease” (page 559). “Variability among subjects’ scores must be large to demonstrate reliability (page 566).” (Portney LG, Watkins MP. Foundations of Clinical Research: Applications to Practice, 2nd Ed. New Jersey: Prentice Hall Health; 2000).

Response: We thank the reviewer for his observation and have deleted the comment.

7. Acknowledgements section. Could the authors please specify the contribution that Virtual Orthotics Limited (Sydney, NSW, Australia) made to the study, as it is not sufficient to simply list their name under the acknowledgements. Please note that if Virtual Orthotics contributed funding

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or in-kind support to the study, this does not qualify as a conflict of interest, merely a contribution that needs to be acknowledged. A conflict of interest would only be evident if the authors have financial or personal relationships with the company that could have potentially influenced the content of the paper.

Response: We have deleted the acknowledgement sub-section and declared: “Virtual Orthotics Limited (Sydney, NSW, Australia) contributed to the loan of the equipment over the data collection period.”

Yours Sincerely

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Reviewer 2 comments and responses

1. My understanding is the more common terminology for the device the authors used is ‘digital scanning’ and the authors have called it ‘3d non-contact digitisation’, which is probably correct, but as a reader it was not until well into the manuscript did I get to understand that they were using a ‘digital scanner’. I misunderstood that the ‘3d non-contact digitisation’ was actually something different, which it wasn’t. Perhaps the authors need to better explain this early in the manuscript to avoid misunderstanding or confusion.

In the interests of avoiding reader misunderstanding all references to 3D non-contact digitisation have been replaced by digital scanning throughout the entire manuscript.

2. The only other minor point is the statement on page 12 of the manuscript that "It would also be of benefit to investigate a full cost analysis study to ascertain the clinical viability of the technique". I have actually done that study: Payne CB. Cost benefit comparison of plaster casts and optical scans of the foot for the manufacture of foot orthoses. Australas J Podiatr Med 2007;41(2):29-31

Reference to the article has been added into the discussion and reads as below.

“It would also be of benefit to reinvestigate a full cost benefit analysis based on the initial work completed by Payne [16], as the cost of digital technology has significantly decreased over the past 5 years”.

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