Author's response to reviews

Title: Normative values for the Foot Posture Index

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Author's response to reviews: see over
Dear Dr Menz

Re: manuscript “Normative values for the Foot Posture Index”

Thanks to your reviewers for their comments. I have addressed the comments item by item below and have made the relevant amendments in the text of the manuscript.

Reviewer 1. PJB

Discretionary Revisions

- Provide a statement on what the Rasch model is (don’t assume the reader knows what this term means).

We have amended the text to read “
A more recent study has also demonstrated good internal construct validity and fit of the scoring system to the Rasch model, a useful statistical model defining the degree of the uni-dimensionality (capacity to measure a single construct) and scale stability (or linearity across a range of values) of a measure [13].”

- Some issues over terminology i.e. table 1, supposed “normals” are identified, but how are these actually defined in each study 9ie asymptomatic??). By what criteria, the reader cannot determine.

Our normals were the control or comparator arm in each of the studies cited. The inclusion and exclusion criteria are separate and independent for each of 16 studies and it is not considered appropriate to list them all individually. For the purposes of this quasi ‘meta-analysis’ the normals are simply comparator groups considered by the original authors to represent a group of people free of major pathology and against whom a valid comparison could be made. We cannot satisfactorily detail a full and singular list of criteria against which their various profiles can be defined and so take the reviewer’s point but do not propose to address this discretionary comment further. A particularly interested reader could refer to the original criteria detailed in each publication.
Pg 6 then starts to discuss this point i.e. criteria of normal, 1 SD, potentially abnormal 2 SD, pathological = > 2 SD. The authors are reminded this is an “arbitrary construct”. ..... I’m (personally) always a little concerned at attempts to “classify” feet as normal/abnormal etc, based on a morphological criteria (be i.e. Root, Kirby, Dannenberg, or FPI).

We agree with the reviewer’s comments that this is an area that has caused significant debate in the literature and among practitioners. The definition of normal/abnormal is complex (and, we agree, essentially arbitrary) and can be approached from many perspectives including clinical, statistical and anatomical perspectives. There is however a fairly substantive body of literature that advances a definition of ‘normal’ for a range of physiological and anatomical parameters based on 2SD’s difference from the population mean, even if in the context of foot posture evaluation this is perhaps a little conservative. Other perspectives, employing more clinical definitions are usually based on n=small studies and are limited as such. These other perspectives where they apply to foot posture and function are also, almost without exception less conservative. In the main, narrower definitions of ‘normal’ foot postures form the basis of much clinical decision making in this area.

In this paper we present the first data from a cohort large enough to define a population-derived measure of normal and so have opted to make the most of this strength. Mindful however that in this particular area of work, a classification system that excluded only the worst extremes from the normal would have little clinical utility, we have attempted to reconcile the conservative statistical definition with a classification that is also clinically meaningful, hence the inclusion of a category “Potentially abnormal” (note the careful phraseology). We again appreciate the reviewer’s point but assert that a paper such as ours must have clinical relevance and that a pragmatic definition is required, rather than more abstract discussion on the many and, as the reviewer points out, arbitrary ways in which definitions might be made.

Difference between “pathological groups” (pg 8) would mean this current paper, could only be generalised to those groups identified (in limited detailed, i.e. probably very heterogeneous sub groups i.e. “those with miscellaneous local musculoskeletal symptoms”.

The data in the paper relates only to the normal populations, firstly as a whole and later sub classified by age. We have made no attempt to provide reference range values for pathological groups, simply describing the differences between normals and those other groups to illustrate the sensitivity of the FPI to systematic variations in foot posture. We would support the reviewer’s comments and would even take them a stage further. Differences between pathological groups should not be generalised at all from this paper.

The discussion around neutral vs. pronated (pg 8 – 9) is perhaps more meaningful, and could be explored in more detail? Second paragraph on page nine is a good discussion of this point.

We thank the reviewer for his comment and trust that we have addressed the point in our response on the definition of normal above.

Reviewer 2. JS

Discretionary Revisions
1. In the abstract-results section on page 3 the authors state ‘.....indicating some sensitivity of the instrument to detect a pathological population’. Would it be more precise to say ‘certain structural pathological populations’?

The authors state in the results section under the sub-heading of ‘Differences between
pathological groups’ on page 8 that certain conditions (pathologies) not normally associated with structural change were comparable with the normal population, where as the neurogenic cavus and idiopathic cavus group (structural pathologies) were clearly different and represented a pathological population. I think there is a difference between the two (structural and non-structural pathologies) and for the sake of clarity it would help to be specific on the instrument’s sensitivity to detect which type of pathology.

We agree with the reviewer’s comment and have amended the text to read “indicating some sensitivity of the instrument to detect a posturally pathological population.”

2. In the background section page 4 paragraph 3–what is the Rasch model? A brief explanation would help the reader.

This has been addressed in the response to reviewer 1. We have amended the text to read “A more recent study has also demonstrated good internal construct validity and fit to the Rasch model, a useful statistical model defining the degree of the uni-dimensionality (capacity to measure a single construct) and scale stability (or linearity across a range of values) of a measure [13].”

3. Page 10 paragraph 3-would it read better ‘Such sampling methods are extremely resource intensive and financially costly, where as the retrospective compilation of a large sample from existing sources covering both normal and pathological subgroups was felt to be a realistic compromise between impact and resource.’ Rather than ‘......financially costly however, and the retrospective compilation....’

We thanks the reviewer for his comments. We have amended the text to read: “financially costly whereas the retrospective compilation”

4. Page 11-3rd paragraph. The authors stated ‘In summary, this study has provided a set of definitive normative values for FPI scores’. In light of the study numbers (619) and the study limitations highlighted on page 10- 3rd paragraph is the evidence/data strong enough to be deemed definitive? If not then another wording should be used.

We agree with the reviewer’s comment , the word definitive has been deleted.

Please let me know if I can provide any further information
Kind regards

Anthony Redmond
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