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Letter to the Editor

Letter to the Editor regarding: Reliability and validity of a mobile tablet for assessing left/right judgements

Dear Profs Jull & Moore

Further to the recently published study by Williams et al. (2019) in *Musculoskeletal Science and Practice*, we were very disappointed to read the authors' misrepresentation of our own study published in the same journal last year (Alazmi et al., 2018).

The authors write:

"A recent report from a small study in healthy participants doing LRJ of back images, showed a negative relationship between mental movement amplitude and RT (Alazmi et al., 2018), which is opposite to that observed for limb LRJ. We did not detect this relationship in a previous study of over 1000 participants performing LRJ for trunk images (Bowering et al., 2014), nor in a study of over 1300 participants performing LRJ for neck images (Wallwork et al., 2013). Notably however, the Alazmi et al. (2018) study failed to provide practice trials, used both first and third person images and was underpowered, which leaves the matter unresolved". (Williams Et Al., 2019; page 49).

To take each point in turn:

1. Although the authors are correct to state that we showed a negative relationship between movement amplitude and response time, it is misleading to suggest that this was in contrast to the studies by Bowering et al. (2014) and Wallwork et al. (2013) as neither considered the issue or were designed to investigate it.
2. The authors are incorrect to state that we failed to provide practice trials. Our study did include these and we state this explicitly in the paper (page 57).
3. Our study presented both first and third person images, but unlike the authors suggest, this was entirely consistent with Bowering et al. (2014) and wallwork et al. (2013). Wallwork et al. (2013) confirm this (page 226). Bowering et al. (2014) provide very few details about images used but we have acquired Bowering's related project report (Bowering, 2011) and it is evident that both first and third person images were also used in this study.
4. Our study was not *underpowered*; we detected a change in response time based on the amplitude of movement that was highly significant. There was no Type II error. This point made by Williams et al. (2019) appears to include the suggestion that the value of any particular study is directly related to its sample size. We don't have space to expand in detail on this issue here, but would warn against such a simplistic view. Also, we draw the reader's attention to the following. While Williams et al. (2019) were prepared to criticise our study (n = 29) on the grounds of *power*, their paper accepts findings from Parsons (1987) seminal work in the field (n = 11). Do the authors also consider Parsons' research to be underpowered?

While we have previously questioned the theoretical understanding that has driven the recent development of left/right judgment tasks for clinical practice and some of the methods used (Alazmi et al., 2018; hartnoll And Punt, 2017; Punt, 2017), our letter here is solely concerned with addressing the misrepresentation of one of our contributions.

Yours sincerely.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.msksp.2019.07.004.

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