Reviewer’s report

Title: The ankle brachial index in people with and without diabetes: Intra-tester reliability

Version: 0 Date: 15 Jan 2020

Reviewer: Anita Raspovic

Reviewer's report:

This is a well-conceived and important piece of research, which provides valuable clinical information on the reliability of Ankle Brachial Indices (ABIs) in a specific population (i.e., people with diabetes). The researchers recruited a relatively large sample and have taken care to design a high quality experiment. I offer the following comments for the authors consideration as they finalise the paper for publication:

Major Compulsory Revisions

1. Consider removing the discussion on predictors of low ABI and reporting these results in a separate paper if there is sufficient data to do so. This is a sizeable topic on its own, which is relatively disparate from measurement reliability. Given both areas require introduction of relevant literature and a dedicated discussion on methodology, results and findings, it would seem more fitting for the respective topics to be addressed in stand-alone publications. The lack of participants presenting with low ABIs also raises questions about statistical power, in addition to other methodological considerations about that part of the study. For example, I query whether an ABI of .99 can be considered borderline, particularly when accounting for measurement error. Separating the topics into different papers would allow greater space to explore each area in more detail, including further explanation and elaboration on the reliability study findings in this current paper (see suggestions below).

2. The authors have done very well in presenting the data however the narrative describing the reliability of ABIs reads as inconsistent at times. Specifically, the ICCs are described as excellent, however interpretation of the absolute measures such as the LOA's, MDC and the 95% confidence intervals of the ICCs, suggest a more moderate picture (for e.g., the lower end of the 95% confidence interval for ABI reliability in people with diabetes is 0.62). Consider altering the descriptors used for the reliability coefficients, clearly acknowledging that the descriptors are an indicative term only, with the interpretation of the ICC depending on several factors, including the proportion of the overall measure which can be attributed to error and how the results are to be applied clinically. For example, instead of 'excellent reliability' for an ICC &gt; .75 as used by Fleiss (1999), Portney and Watkins (2009) use the term 'good reliability' for the same coefficient. In addition, when discussing ICCs, remember to reflect the 95% confidence interval as an indicator of precision of the ICC. Ensure this is then contextualised against the indicators of absolute reliability, for a well-rounded discussion. Whichever way you decide to address this issue, ensure there is a cohesive, consistent and accurate approach to interpreting the reliability data, so that readers understand the position you are taking on the measure (also see points 3 & 4).
3. The authors have done an excellent job in operationalising the reliability co-efficients (i.e., ICCs), through the use of statistics reporting absolute measurement error in the units of measurement, i.e., LOAs, SEMs and MCD. It would be useful for the readers to have each of these statistics clearly explained in simple terms however, including why the three measures were reported, how the measures differ and what each adds clinically. Specific examples of how each may be applied clinically, by applying the data to a scenario, would be very useful. I suggest selection of a mixture of best and worst case scenarios for these clinical examples, so that readers gain an appreciation of the range of error that needs to be considered and how the error can be quantified in practical terms based on a given ABI measure.

4. Be specific when describing the clinical implications of the work. On one hand the paper states that reliability is excellent however on the other hand the results should be interpreted with caution. What does 'interpret with caution' actually mean in practice? Are there any specific recommendations or guiding suggestions arising from the work to assist people applying the measure clinically?

Minor Essential Revisions
5. Consider describing the degree to which the clinician taking the measures has past experience taking ABIs on the population studied,
6. Ensure that minimal detectable change is not conceptually mixed up with minimal clinically important difference (see line 79 in the background),
7. Please clarify the inclusion criteria regarding (line 98). It is unclear whether you recruited participants with pre-existing PAD using ABIs or some other approach. If PAD was an inclusion criteria, make it clear early in the paper that people with PAD, and people with PAD with diabetes, were recruited. Or are you saying that people who met the guideline for testing for PAD with ABIs were recruited? Either way, please double check the wording as I read this a few times to try and establish the inclusion criteria that were used. Also clarify, is it people 65 years and over were included, OR or 50 years and over if they had a history of diabetes?
8. Reference the standardised ABI techniques followed in the methods,
9. Line 61 - add a reference for the statement "The ankle brachial index (ABI) is typically calculated as the ratio of the highest of the dorsalis pedis and posterior tibial artery systolic pressure to the highest of the left and right brachial systolic pressures".
10. Consider adding to limitations, one person only assessed ABIs only therefore generalisability to other clinicians may be limited, that the clinician was not blinded to the measures therefore may remember some patients measures (especially unusual measures) and that using ABI to recruit participants may involve some inaccuracy (due to the measurement error you went on to establish),
11. You have some very rich data therefore consider exploring it in more depth. Some ideas, to take or leave as you wish, are: that the brachial systolic measures are less reliable than the ankle systolic measures - what might this mean to the ABI reliability results and might it be an area to address the improve reliability of the coefficient? How might the differences between groups in age and BMI be relevant? Did some participants have wounds and was this even between groups? How might this be relevant to the study findings? Why was there only a small difference in ABI reliability between the diabetes and non-diabetes participants? Was this what you expected and how do you explain this finding? What are the implications of the study in term of developing the ABI measure - is there room to improve accuracy, what are the likely
sources of error and can some be worked on? And finally, I read it that you undertook paired t-tests on time 1 measures v's time 2 measures. What was the purpose of these comparisons and what is your interpretation of the result? Where you looking to establish systematic versus random error? What did you find?

12. Consider acknowledging the broad clinician concern that exists, questioning the validity of ABI's in diabetes (due to medial calcific sclerosis) as an important issue, albeit not what is being investigated in this study. One sentence would suffice, but it just shows an appreciation of the scope of accuracy issues in the area, for completeness.


14. Provide reference for line 79 statement "There has also been limited investigation of the intra tester reliability of the ABI in people with diabetes, with only one previous study, which reported a coefficient of variation of 8%.

Discretionary Revisions

15. In line 154, would the wording "no ABI's...." read clearer than "not ABI's...."?

16. Note, there are 2 full stops in line 195

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