Author's response to reviews

Title: The Mini-BESTest - a clinically reproducible tool for balance evaluations in mild to moderate Parkinson's disease?

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Author's response to reviews: see over
Dear reviewers, thank you for your valuable comments and questions. As you probably will notice we have made some changes, primarily with regards to definitions. The reason for this is primarily the confusion with terminology that occurs within this field of research (something that has also affected our group).

Before we respond to your respective comments of the manuscript, we would like to present the major changes that we have added to the revised manuscript

- We have now adopted the umbrella term: reproducibility (previously referred to as reliability) as well as its separation between agreement (previously referred to as absolute reliability) and reliability (previously referred to as relative reliability) [1-3]. We now also use test-retest (instead of intra-test) because this is the correct term with regard to our design [4].

- We no longer present the Standard error of measurement (SEM) as a measure of agreement on group level. Instead we define the SEM (including the difference between SEM\textsubscript{agreement} and SEM\textsubscript{consistency}), and present how calculations were performed to obtain the SEM.

- We have added the formula used to calculate the Smallest Real Difference (SRD)

- To calculate the SEM\% and SRD\%, Flansbjer et al. [5] suggested the calculation: SEM (or SRD)/ the mean score of all observations. However, we believe that it is more interesting to know the proportion of the measurement error in relation to the maximal score of the Mini-BESTest, and therefore we now use the following calculation: SRD/total score of the Mini-BESTest (28).

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Dear reviewers, please see our point by point responses to your concerns below.

**Reviewer 1**

**Reviewer’s concern:**

- My major concern with this paper is the possible influence of motor fluctuations on the results obtained. Indeed, the authors admit that motor fluctuations may represent a bias to the interpretation of the results, however they do not provide any information on the prevalence of fluctuators, neither on whether test were performed in the "on" or "off" condition. To my opinion, these informations are crucial for the correct interpretation of the results, and should be given.

**Authors’ response:**

We agree with the reviewer that the possible influences of motor fluctuations might have affected the results. We particularly acknowledge that we should have been clearer that all subjects were in their “ON” state (this was the major reason to why we tested the subjects at the same time of the day at all occasions). We have now added this information to the paragraph describing the participants “…and were tested during the on-phase with regards to their medication scheme” (page 5, line 83-84).

However, we did not assess fluctuations nor did we identify any fluctuators in this study. Although such an assessment might have been beneficial, this study aimed to be similar to clinical conditions (where balance is assessed also on fluctuators), hence we believe that controlling for fluctuations might have been contra productive.

**Reviewer’s concern:**

- A further issue is the lack of information on the cognitive status of the patients. I understand from Table 1 of a disease duration between 1 and 15 y in the subjects enrolled. Again, it would be interesting to know the presence and severity of cognitive dysfunctions, in particular dysexecutive symptoms in these patients.

**Authors’ response:**

We agree that the cognitive status of the participants within the studied population is of particular importance. We have indeed screened their cognitive status with the Mini Mental State Examination (all participants scored at least 24 points). Similar to the response to the reviewer’s previous concern, as our aim was to assimilate clinical circumstances where also individuals with cognitive impairments are assessed with balance tests, we did not want to use...
MMSE as an inclusion criteria. However, we acknowledge that question marks are likely to be raised if the cognitive status of the participants is not included. We have therefore added this information in the paragraph describing the participants as well as reference 27 in the manuscript “All participants had a Mini Mental State Examination [6] score of at least 24 points (indicating adequate cognitive function to occur in this sample)…” (page 5, line 81-83).


Reviewer’s concern:

- The authors choose a 1 week interval for test-retest procedure. This interval appears adequate to avoid biases due to changes of the disease stage, but may be short enough to expose to bias of recalling the previous experience (both by patients and by raters). The authors might wish to discuss this issue.

Authors’ response:

We acknowledge this and have added this as a potential limitation of the results in the discussion section “Furthermore, it is possible that the less-experienced test administrator experienced a learning effect during the course of data collection, a form of bias that also might have occurred with the participants as well as with the experienced test administrator at the test-retest assessments (that occurred 7 days after the initial assessments)” (page 13, lines 243-246).

Reviewer 2

Reviewer’s concern:

- The question of the study is well defined and described. The data are well collected and analysed and methods are well clarified, even if a more detailed description of the MiniBESTest would be helpful.

Authors’ response:

We have now added a table of the Mini-BESTest and its subcategories. (Table 2)

Reviewer’s concern:

- I think it would be helpful if authors clarified better the difference between group and individual level.

Authors’ response:
Thank you for your remark, which led us to thoroughly review our reference (1) regarding this issue. The consequence of this review is that we do no longer feel confident in presenting our results in terms of group level, instead we now follow the suggestions by Terwee et al. (2) and de Vet et al. (3) with regards to terminology as well as analysis.


**Reviewer´s concern:**

- Authors could also clarified better why the studied elderly patient with mild-to-moderate PD and not some younger patients.

**Authors´ response:**

We have added information as well as a reference to the background “The incidence of PD, most common after the age of 60 years, rises with age…” (page2, lines 2-3, reference number 3 in the manuscript).


Moreover, although it would indeed be valuable to investigate younger people as well, the typical patients with PD that are treated by physical therapists in the clinic are mainly elderly. In addition, the majority of research studies within the field of balance interventions are performed in elderly with mild to moderate PD.


Reviewer´s concern:

- I would also write that further studies on more heterogenous patients would be recommended

Authors´ response:

A sentence regarding this is added to the discussion "Future studies, preferably on a more heterogeneous sample, need to investigate…" (page 12, line 226-227)