Author's response to reviews

Title: A cross sectional study of upper extremity strength ten days after a stroke; relationship between patient-reported and objective measures

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Author's response to reviews: see over
Dr Toby Cumming  
Associate Editor  
BMC Neurology

Dear Dr Cumming,

Thank you for the reviews comments on the manuscript entitled “A cross sectional study of upper extremity strength ten days after a stroke; relationship between patient’s perception and capacity measure”, MS 5303823527285372. After the suggestion from reviewer Dr Phu about changing the strength capacity measure to “objective measure”, we have changed the title to: “A cross sectional study of upper extremity strength ten days after a stroke; relationship between patient-reported and objective measures”.

We have tried to follow the suggestions given and think the manuscript has been improved by the process. Attached are our responses to the reviewers including indications to where changes in the manuscript have been made.

Looking forward to hear from you soon,

Yours sincerely,
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Reviewer 1

Dear Dr Lindmark,

Thank you for your comments which have helped us to improve the manuscript. Below are point-to-point answers to the specific questions raised.

**Major revision:**
The authors have found that the correlation between the patient’s perception and a clinical strength measure was quite good but not perfect and mean that this may be due to that the patients still were in hospital, resulting in reduced need or cause to use the upper extremity, which might be one reason. But they do not discuss for instance what impact reduced sensibility or reduced co-ordination might have.

*Thank you for making us aware of this. We have now in the discussion included a sentence about the possible impact of sensibility and co-ordinations, line 190. This is also mentioned as a limitation in the end of discussion, line 233-240.*

It is also unclear why the perception of arm strength is compared with a clinical measure of hand strength. Are there no clinical measures for arm strength? This has to be explained.

*We have now included a paragraph about this topic in the discussion, line 220 -228 where we discuss the use of other available measures of arm strength.*

*As the SIS and also handheld dynamometer often is used in patients with stroke, we thought it could be of interest to combine these two measures. Bohannon et al. (Are hand-grip and knee extension strength reflective of a common construct? Perceptual and Motor Skills, 2012) has shown that strength in the hand (as well as in knee extension) could be generalized to muscle strength in other muscles in the body.*

They should also stress more that the studied group was cognitively quite clear and that the results could only be generalized to that group.

*Thank you for pointing this out. In the end of discussion this was stressed out. We have now included this in the conclusion as well as in the conclusion in the abstract, line 38 and 245.*
Minor essential revisions:
Abstract: Describes the study quite clear. Last sentence under Backgrund: The objective was to investigate the relation between perceived strength and clinical measures…. But is it not just one clinical measure of strength? The objective stated here are not exactly the same as stated in the Background, page 4, bottom.

Thank you, we have changed this to clinical measure, instead of measures.

Conclusion: It should be added that this conclusion could only be generalized to patient who are not cognitively disturbed.

Yes we agree and have changed the manuscript accordingly.

Background: Short but gives enough information, why the study was important to accomplish.
First paragraph, line 61-62: Why is it important to study self reported measurements “in the case of stroke versus other diseases”?

This sentence is now clarified to, line 61-63.
Self-reported measures are rarely investigated in research compared to other diseases such as in Rheumatoid Arthritis and specific conditions (peripheral upper extremity). We argue that this is way self-reported measurements used in a population of stroke might be of even higher interest because it is rather unusual.

Methods: The design, the participants, the measurements, procedures and statistics are well described. Maybe the description of the arrival score on page 6, last paragraph should be presented before the main measurements at ten days.

We have discussed this, but thought that the main measurements could be presented first.
We would like to keep this structure, but could of course change it if the readability needs to be further improved.

Results: Short but the essential facts are presented. Page 8, 2nd paragraph, line 155-156:
”Correctly classified patients were (change was to were) in arm strength 0.81 and grip strength 0.84”. What do 0.81 and 0.84 stand for? Is it percentage or what?

Thank you for pointing this out. We have changed it to percentages.
Discussion: The authors discuss their results quite well and try to compare them with what other researchers have found. First sentence under Discussion, page 8: The authors should state which strength capacity measure they used.

  Thank you. Yes we agree, this has been clarified, see line 166.

Page 9, 1st paragraph, line 171-172 the authors refer to one study (29) who reported reduced self-perceived function although fully recovered based on strength measurements. Did they give any explanation?

  Unfortunately Domerick et al did not discuss explanations to their findings in detail. To continue the discussion and better connect to the following paragraph we now a have included “This confirms the need of a combined measurement strategy that is sensitive to changes and assess a broad range of performance (29)”, line 179-178.

Page 9, line 175-176: Is strength capacity and self-reported measurements within the same constructs? Strength measurement only measures one dimension while self-reported assessment is based on several dimensions.

  Thank you for pointing this out. We agree that self-reported measure include more than only the assessment of the strength. We have changed the sentence, line 182-186.

Next sentence in the same paragraph, line 177-179: The authors should discuss more reasons why the correlation between the two ways of measuring was not as high as they expected.

  Yes, we agree and have included that the patient might have problems with co-ordination and sensibility, line 189. We have also discussed it in the end of the section of the discussion, 233-240. It could also be due to that they in somewhat measuring different construct.

Page 9, 2nd paragraph, line 183-185: The sentence that starts with “The proportion of patients…” should be rewritten, as it is hard to really understand.

  Thank you for making us aware of this. We have re-written this sentence, line 193-194.

Line 187: What does 081-084 stand for?
It is percentages, and we have clarified this in the manuscript.

Tables and figures: Table 1: NIHSS, COG4, FMA-UE, Pre-BNIS: Is high or low score good. This is explained in the text but could be of help here.

Yes, we agree, and have clarified this in the table. Please see table 1.

Figure 1: The text in the figure is impossible to read.

We have tried to improve the figure to increase the readability.

References: There are some small errors in some of the references. The first letter in the names of the Journal should be a capital: Check ref. 1, 18, 26, 30, 31 and 33.

Thank you for making us aware of these mistakes.
We have tried to up-date the references in accordance to BMC Neurology guidelines and reference style.
Reviewer 2

Dear Dr Phu

Thank you for your comments which have helped us to improve the manuscript. Below are point-to-point answers to the specific questions raised.

**Minor essential revisions:**

1. Abstract (last sentence of conclusions): you suggested a combination of self-reported and strength capacity assessment (replace with objective strength capacity measures) is needed to increase knowledge of impairment early after stroke (replace with to enhance the setting of realistic goals with patients).

   Thank you for your suggestion to improve the conclusion. We agree that “enhance the setting of realistic goals with patients” will clarify the conclusion, and have changed the conclusion accordantly. We have also included that the conclusion is valid in persons after a stroke without severe cognitive disabilities.

   We have also changed “strength capacity assessment” to objective strength capacity measure as suggested.

2. I suggest similar correction in the Conclusion (last sentence): In addition to your sentence “…to increase focus on the patient’s perspective…” add the following: …and to ensure that a treatment goals are set on the basis of the objective outcome measures.

   Thank you for your suggestion. In accordance to our answer to question 1, we do not agree to this. According to modern views of goal setting, this is a negotiating process between the professionals and the patient (Siegert R J, Levack W M.M. Rehabilitation Goal Setting – Theory, Practice and Evidence. Taylor and Francis Group; 2015).

   We changed and added in a sentence of the conclusion about that the results might enhance the setting of realistic goals.

   We also added that our results are valid in a group without severe cognitive impairments.

3. Methods (design and participants): Please clarify the third exclusion criteria: short life expectancy. It is not clear part of which exclusion criteria are severe communication disorders, cognitive deficits and fatigue, on the basis of which you excluded 18 patients.
We have in the methods now included the SALGOT-studies full exclusion criteria, line 92-95. We have also clarified the reasons for way patients from the SALGOT-population were not included in the present study, indicated by numbers of patients, line 97.

4. Table 1. is not totally clear, especially where multiple parameters are quoted in one line.
   Thank you for making us aware of this. We have tried to improve table 1 in general.

5. Figure 1: you should increase the size of letters and numbers.
   Yes, we agree, and have tried to change it accordingly.

Major compulsory revisions:
1. Methods (measurements and procedures): Please describe the position of the patients (sitting position, position of the arm) during measurement with a dynamometer. “Patients rested their arm and hand on a table during the measurement“ does not describe the measurement position in the repeatable way, and the elbow could also be extended; which is not in accordance to the position described in the studies by Mathiowetz et al. (1984, 1985).
   Yes, we are aware of this. In the methods, line 105-106 we have included that the reason to that this position was used to increase the possibilities for patients to participate even if low muscle strength in arm/hand.
   We have now also included a paragraph in the discussion about this, line 219-227.

2. Further, you set a cut off 80 % normative strength values to delineate normal strength versus reduced strength, which you mentioned in the manuscript several times. However, you did not quote which normative values you used.
   We have used the normative values that are given in the JAMAR manual, (Mathiowetz V et al. Grip and pinch strength: normative data for adults. Arch Phys Med Rehabil. 1985) in this manuscript reference 21. We have tried to make it more clear in the manuscript, that this reference was used (methods and discussion).

3. Also in the discussion section (limitations) you indicate that “using percentage of normative values of JAMAR is constrained by the size and background of the reference group.” It is necessarily, that you clarify which normative values you used.
Thank you for making us aware of this, and we have clarified this in the manuscript, in the section of methods and of discussion.

Discretionary revisions:
1. Results and discussion: You performed a screening of cognitive function (COG4, BNIS), FMA of the upper extremity motor and sensory function, and ARAT (as an inclusion criteria), but in the discussion (second paragraph) you are explaining the differences in the present study only with the fact that “the patients were still in hospital, resulting in reduced need or cause to use the upper extremity in the activities of daily life.” Why didn’t you perform some multifactorial analysis with your data to see if there is any pattern? Difference in hand awareness might be also a problem of hemi neglect and/or sensory problems. You should consider this in the discussion section and you may perform some additional calculation with your data.

Thank you for pointing this out. We have changed that sentence with reasons for why differences between the measures, and now also included sensitivity and co-ordination as possible confounders.

We agree that a multifactorial analysis could increase the knowledge of this group of patients, which we have also mentioned in the end of discussion. We decided not to include a multifactorial analysis, and instead use the 80% cut off when we presented the data in the manuscript. We had a long discussion with statistical experts since we first analysed our material using a multiple logistic regression analysis, but there were several difficulties with this analysis: not enough patients within each category in the primary outcome and/or in the confounders (minimum number of 5). This problem remained even after the categories were collapsed. Another problem could be the psychometric properties of for example FMA-UE which not is fully studied in different stroke severities using the original manual (Woodbury et al; Rasch analysis staging methodology to classify upper extremity movement impairment after stroke. Arch Phys Med Rehab 2013, Crow et al; Hierarchical properties of the motor function sections of the Fugl-Meyer Assessment scale for people after stroke: A retrospective study. Phys Ther 2008). Therefore, we decided not to include the results from regression analysis. We have also tried to clarify the reasons for this in the discussion, line 233-237.