Author’s response to reviews

Title: Change in basic motor abilities, quality of movement and everyday activities following an intensive physiotherapy program in a group setting for children with cerebral palsy

Authors:

Anne Brit Sorsdahl (anne.sorsdahl@isf.uib.no)
Rolf Moe-Nilssen (rolf.moe-nilssen@isf.uib.no)
Helga K Kaale (helga.kaale@bergen.kommune.no)
Jannike Rieber (jannikerieber@bfsnett.no)
Liv Inger Strand (liv.strand@isf.uib.no)

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Author’s response to reviews: see over
Resubmitted manuscript:

Change in basic motor abilities, quality of movement and everyday activities following intensive, goal-directed, activity-focused physiotherapy in a group setting for children with cerebral palsy

Dear Editor

Thank you for the constructive and valuable comments on the manuscript. We found the comments and recommendations from the referees’ extremely encouraging, informative and useful, as you will see from our changes. We hereby hope that the article will be accepted for publication in the BMC Pediatrics.

Our changes in the manuscript are highlighted in coloured text.

Here are our amendments according to the referees’ suggestions:

<table>
<thead>
<tr>
<th>Reviewers’ comments to the authors</th>
<th>Authors’ response</th>
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<tbody>
<tr>
<td>Reviewer # 1</td>
<td></td>
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<tr>
<td>- Major Compulsory Revisions</td>
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<tr>
<td>1. Use of term &quot;intensive physiotherapy&quot; in the title and abstract: need to be more specific re: type and nature of physiotherapy intervention.</td>
<td>“goal-directed, activity-focused” is included in the title and “intensive physiotherapy” is specified in the abstract.</td>
</tr>
<tr>
<td>2. In methods of abstract: need to provide timing of two follow up assessments (one immediately after the three weeks, and specify the timing of the others).</td>
<td>Included in the abstract.</td>
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<tr>
<td>3. Statistical results are given in the abstract, but not the clinical significance of the magnitude of the change.</td>
<td>The conclusion of the abstract is rewritten to incorporate the clinical significance of the results. Also see our response in point 5 below.</td>
</tr>
<tr>
<td>4. Conclusion indicates reduction in caregiver assistance, but this was not provided in the results.</td>
<td>The p-value of significance in PEDI-results is corrected according to reviewer #2 comments on inter correlation of the scales in PEDI. Hence the number of test results that were significant was lower, and the results and conclusion sections in the abstract and the manuscript are altered accordingly.</td>
</tr>
</tbody>
</table>
5. Please provide a rationale for selection of the mean change in GMFM-66 score of 2.5 points. Is this considered clinically significant?  

As the change score of the GMFM-66 representing a minimal important change (MIC) in a group study in our opinion is determined by the children's ages and functional levels in the sample, we have not suggested a mean change in the primary outcome measure GMFM-66 as clinically significant. We have, however, in the revised discussion included parents' experiences expressed in the qualitative arm of the study, to shade light over the changes in function as observed in the children's home environment, in addition to the PEDI and GAS results. We have also included results from a study of Wang et al (2006) in the discussion, suggesting 3.71 point to differentiate between great and not great improvement in the GMFM-66.

6. The first time "medication" appears is on the top of page 12. The methods should contain a description of how these data were collected. Was it only about medication for spasticity management? Did it include medication for epilepsy or other co-morbidities? What is the rationale for collapsing across all medication categories, if this was done?

“anti-spastic” is added where “medication” appear, and how the data were collected is included in the description of the anti-spastic medication page 13.

7. Top of page 12 - reference 49 seems to be incorrect (I think you mean to reference number 50 here re: treatment of missing values). And at the top of page 15 ref 50 should be 51 (PLEASE CHECK ALL REFERENCE NUMBERING FROM 49 ON, but 52 and 53 are clearly correct)

The references at page 12 is checked carefully and corrected.

- **Minor Essential Revisions**

1. page 4: please indicate what is meant by "traditional training"

   Explained

2. page 7 - please provide a brief description of the children's "ordinary physiotherapy program"

   A brief description is included

3. on page 8 - please provide results of the fidelity checks on the intervention (i.e.

   Included
"supervision" of the therapists on three occasions)

4. Page 11 - unclear what is meant by "Performances of the selected activities for GAS were videotaped at 3. pretest and at 2. post-test"...Unable to reconcile with table.

The sentence is rewritten and the reference to Table 1 is removed.

- Discretionary Revisions

1. It would be "nice" for readers to see a table with a summary of the psychometric properties of these measures (including sensitivity to change and responsiveness)

A sentence regarding psychometric properties of each measure is included.

2. page 13 - with the GMPM - isn't it possible to score only if the GMFM score was 2 or 3, rather than > 0? How can quality of movement be evaluated with "just initiates"?

According to the GMPM manual, GMPM items can be scored when GMFM items >0. This may, however, blur the quality/quantity distinction as we have elaborated in the discussion of this revised manuscript.

3. Suggest removing the part about "a tendency of improved quality of movement". I'd also suggest removing, on page 14, the "tendency" to improve on the QUEST (and delete from discussion - most researchers would consider this an over-interpretation)

Although the changes were not statistically significant on GMPM and QUEST, they were all in a positive direction, therefore we find it informative to include this.

Reviewer #2

- Major Compulsory Revisions

Methods:

1. Not clear as to why the first author and PT videographer constructed the GAS based on goals set by parents/child and the PT who tested the child in collaboration. The authors should note why the goal levels were not set in the usual way, i.e., by the treating PT with the parent. Perhaps it was so that the goal levels would be amenable to measurement by observation rather than parent report? This does somewhat restrict the possible breadth of achievement options, and perhaps may have resulted in some measurement bias (activity vs performance types of

The procedure chosen for constructing the GAS-scales in this study is in line with Ottenbacher et al’s (1990) recommendations for using GAS as a research instrument. We do agree, however, that this procedure may be somewhat different from how GAS scales are – and should be - applied in clinical practice. However, most of the goals in our study were activity-type goals, and the parents presented early in the goal setting process their goals, hence type and content of goals might be influenced by the expectations of parents as to what kind of goals you could “present” for a three weeks
goals?). As we, in addition, have discussed, the standardized testing in the baseline period might have influenced the type of goals selected.

Results:

2. The method summarizing the GMPM is a bit unusual – the authors decided to create a Total quality score although this is not something that was done for the original GMPM and as such has not been evaluated within the validation work. It is unclear as to why the authors did not also report each of the GMPM’s 5 attributes since they noted in the analysis that they did summarize the results according to the attributes as well as their computed Total score. For example, it is relevant to know the extent that alignment or stability or co-ordination have changed. They may not all change!!!! A similar concern and need for clarification also exists for the QUEST.

The total scores of the GMPM and QUEST were calculated according to instructions in the manuals. In addition the items from GMFM which changed during the study period and the items that retained the same scores during the period, were manually identified from the GMAE database. We then calculated two new GMPM total scores for each child according to the instructions in the manual; one containing the attributes of the GMPM in the items of GMFM that changed during the study period, and one GMPM total score containing the GMPM attributes from items where the GMFM scores were stable. The premise of these calculations is that corrections for missing scores are built-in in the calculation of the GMPM total score.

A reliability study performed before this study (Sorsdahl et al 2008) showed large variability in reliability coefficients of the attribute/domain scores of the GMPM and QUEST. We decided therefore to include only the total scores in our intervention study, since both revealed satisfactory reliability.

3. The idea of looking doing a subanalysis for GMPM scores in relation to items that changed on the GMFM/those that did not change on the GMFM is an interesting one and fits with testing the authors’ hypothesis (which should have been stated at the outset) that GMFM items that are stable should be the ones that show changes in quality of movement. I also don’t understand why the two GMPM assessors’ scores were presented separately when the GMFM scores. While there were presumably several assessors (p. 10) for the GMFM (the authors noted that the same assessor did the pre- and

Our preconception of relationship between quality and quantity in movement development is described in the introduction section of this revised manuscript.

The GMFM is a commonly used and well known measure to the four assessors who assessed the children with the GMFM. Three of the assessors underwent the same training and the scoring of the fourth assessor’s scores was validated from video recordings. GMFM has shown high inter-tester reliability.
4. While the idea of looking at the relationship between improvement and function and quality is important, the results are difficult to interpret. Does this mean for example if the child improved in their 20 second stand by at least 1 point on the GMFM that the pre and post scores of the 3 attributes that were connected to that item would be used within a GMPM total score for that child? This needs to be clarified! Similar concerns and need for clarification exist for the QUEST.

See point 2.

### Discussion:

5. In the discussion of GMPM/GMFM change results (P. 18), the authors make a point about the possibility that “that assessors tend to judge quality of movement to be improved when abilities of basic motor function improve, could also be interpreted as if the assessors confuse ability with quality.” If however the assessor of the GMPM tape was looking at just a isolated clip of the child’s performance of a particular item (either pre- or post) this frame of reference-based scoring bias should not have happened.

More elaboration of this point is added in the discussion.

6. While the authors then go on to note that “The difference between the twin instruments GMFM and GMPM may, in addition, not be straightforward since GMFM clearly captures change in some aspects of quality of movement like weight shift (item 12/13) and stability (item 56)”. they could capitalize more on this observation. Indeed, what I have been

A comment is included in the discussion.
observing as a PT researcher who is currently doing work on refinement of the GMPM, is that it may be the changes in balance or alignment that actually permit the child to move up to the next level of GMFM ability, i.e., if they are stronger and straighter and their stability is better, then they are more likely to be able to do the 20 second stand for longer. This possibility could be noted in this paper.

- **Minor essential revisions:**

1. Abstract – Suggest that evaluation of the impact of the intensive intervention on quality of movement should also be noted (as the authors clearly are looking at the dual effect on function and quality of movement).

   “Quality of movement” is included.

2. The authors should define ‘intensive’ physiotherapy, e.g., three hours of training, five days a week in a three weeks period in a group setting) in the abstract.

   Defined in the abstract.

3. The reporting of GAS scores as mean change is a bit unusual – more typical with this system to look at the final attainment score and see if it is in the 45.0 to 55.0 range that represents accomplishment of goals at the targeted level (i.e., the 0 level). Indeed the post-test GAS T-score was 51.3 so satisfies this criterion without having to look at P values.

   The reporting of GAS scores is altered.

4. The P value for the PEDI’s 6 scales should be adjusted (0.05/6 subscales), and the number that had P values less than this reported as all are highly correlated! According to Table 4, 3 of the scales showed significant change at a P < 0.01 level.

   The p-value is adjusted and the results altered.

5. The two aims of the study are clearly presented on p. 5. Research hypotheses could also have been presented – this would have been particularly useful for the question pertaining to the relationship between changes in function and quality of

   Our clinical preconception of relationship between quality and quantity in movement development is described in the introduction section of this revised manuscript.
6. Am I correct in thinking that the GMFM (1e outcome measure), GMPM and QUEST were evaluated at all 5 assessment points, while the PEDI and GAS (p. 11) were just evaluated at baseline 3 and follow-up 2?

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Table 1 shows the assessment points for the measures; 5 GMFM assessments; 4 GMPM and QUEST assessments, and GAS and PEDI were scored in the baseline phase and once after the intervention. The test times for PEDI are further outlined in the “results” section p.15 and the test times for GMFM are shown in Fig 1.
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7. I am not clear as to why the PEDIs were scored by experienced PTs and OTs at the various centres (were these the treating PTs and OTs - I assume this was the case as they would be the only ones who would have sufficient knowledge of the child)? Why were the parents not the respondents to the PEDI? They would have a much better awareness of exactly what their child was doing in a variety of real-life environments, and this is what we really need to know. This limitation at least needs to be noted in the discussion so that the reader is fully aware of it!

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The PEDI was administered to the parents in an interview performed by PT and OT from the habilitation centres, who were not involved in the training. This is further clarified in the manuscript.
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8. A brief comment should be made on p. 9 on the published evidence of the psychometric strength of each of the QUEST, GMPM, PEDI and GAS when used with young children with CP.

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A sentence regarding psychometric properties of each measure is included
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9. Analysis: The authors explained in considerable depth including the approach for handling missing data (and in the results on p. 13, the extent of missing data was noted for GMPM, GMFM and QUEST – the extent of missing data in the PEDI should be noted as well). As noted earlier, the setting of the P value at 0.05 for the multiple scales within each of the measures may be problematic. Did the authors consider some type of alpha adjustment to account for correlated outcomes? A Bonferroni adjustment was made for the repeated measures testing (Table 3) but not for the correlated scales.

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The missing data for PEDI is included.
The p-value for the PEDI has been adjusted.
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**- Discretionary revisions:**

<table>
<thead>
<tr>
<th>Suggestion</th>
<th>Change Description</th>
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<tbody>
<tr>
<td>1. On p. 13, suggest using the word “adherence” rather than ‘compliance’. Less negative association with that word.</td>
<td>Changed</td>
</tr>
<tr>
<td>2. On p. 15, I am confused by the word ‘action’ goals – would the word ‘activity’ goals be more appropriate?</td>
<td>The term “action” used by Gentile and Larin is in our opinion similar to “activity” as used in ICF. The term “action” is changed in the manuscript to avoid confusion.</td>
</tr>
<tr>
<td>3. Probably better to refer to the GMPM as a sibling measure to the PM, not a twin measure (P. 18)</td>
<td>Changed</td>
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</table>

For the authors,

Sincerely,

Anne Brit Sorsdahl