

## **Author's response to reviews**

**Title:** Effectiveness of physical therapy interventions for children with cerebral palsy: a systematic review

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**Author's response to reviews:** see over

to: John Kerr  
BioMed Central

Dear Editor,

Helsinki, 10<sup>th</sup> February 2008

We appreciate all the comments from the reviewers and have made changes to the manuscript accordingly. Below we respond in detail to the concerns of the reviewers.

### **Reviewer 1: Paul Uvebrant**

- 1) The cognitive impairments may be a factor that causes learning disabilities. To be coherent, we deleted the words "and learning disabilities" from the introduction.
- 2) The end of the inclusion period is February 2007, where the searches ended. This is stated in the methods section, specifically under the subsection "Literature searches" as follows: "from 1990 to February 2007".
- 3) The reviewer asks for a description of how we and the included trials dealt with children aged under 4. CM Drillien articulated a phenomenon called "transient dystonia" already 20 years ago and since then it's indeed well-observed that signs that mimic CP in early infancy can disappear. However, in none of the included trials an "amazing improvement or disappearance" of CP signs was reported. We are thus assuming that the patients included in these trials had permanent signs indicative of damage to the central nervous system and clinically diagnosable as CP.
- 4) The study by Wallen et al<sup>1</sup> had four groups (BTX-A plus OT, BTX-A alone, OT alone and no-treatment), of which we included only the two last groups (OT alone and no-treatment). This clarification is added to the text. Because the OT intervention techniques in this trial included similar activities to what a PT intervention could include (stretching, casting, splinting, motor training, environmental modification and practice of specific goal activities)<sup>1, page 3</sup>, it was included in the review.
- 5) The reviewer asked for an extension of Table 2 to include all 22 trials with some details on participants, interventions, study quality and results as classified according to the ICF. Such a table would be very informative indeed, but the amount of information in rows and columns becomes easily too large to fit the journals requirements for tables (max. 2 sides of A4 in length in portrait format). Thus, we are unable to do this, and ask the readers to look at the additional tables, where all this information is available.
- 6) We agree with the reviewer that measuring subjective well-being might be difficult for some children with CP, particularly for those with communication disabilities. We suggest that it "could be measured".

### **Reviewer 2: Adri Vermeer**

The reviewer points out the fact that we excluded all other study types than randomized controlled trials. We chose to rely only on randomized controlled trials. This choice was made for two main reasons:

- 1) The previous systematic reviews (as referred to in the introduction of the manuscript) have

already considered and reviewed a range of observational studies in this field. In a systematic critical appraisal of these reviews we found that due to the poor quality of primary studies, mostly of various observational designs, the majority of reviews drew no conclusions on the effectiveness of the interventions.<sup>2</sup>

2) RCTs are widely accepted as the gold standard for providing evidence of effectiveness. Observational studies are useful where there are consistent results with no plausible confounders.<sup>3</sup>

As our main question was "What is the effectiveness of various PT interventions", we chose to rely on the best evidence (i.e. studied in RCTs) that is available and published in this field.

### **Reviewer: 3 Jan Willem W Gorter**

1) The reason to include the Palmer et al. article from 1988 (ref 57)<sup>4</sup> was that it reports more outcomes of the very same study that was reported in the Palmer 1990 article (ref 38)<sup>5</sup>. A clarification is added to the text: "with more outcomes of one trial (ref 38)"

2) Yes, we excluded all adjuncts during the intervention, but in two trials there had been some adjuncts prior to the intervention. The text (in page 5) is clarified as follows: "In one trial all children had undergone multilevel surgery on lower extremities<sup>6,7</sup> and the PT intervention was designed as a postoperative treatment, and in one trial 18 children had had surgery and three botulinum toxin treatments 12 months prior to participation in the trial.<sup>8,9</sup>"

3) At the conception of this review we utilized the Mutch et al.<sup>10</sup> definition on CP. The most recent definition by Rosenbaum et al<sup>11</sup> is highly relevant and is thus added to the introduction.

### **Reviewer 4: Diane Damiano**

1) These comments are highly appreciated.

2) The judgment of the amount of evidence should indeed be noted earlier in the text, and not just in the discussion. Thus the following was added to the text, pages 9-10: "According to the levels of evidence (Table 1) we found no strong evidence on the reviewed interventions, only moderate, limited and conflicting evidence on some particular outcomes was established. The evidence synthesis of the available moderate and conflicting evidence is summarized in Table 2. Moderate evidence was established on the effectiveness of upper extremity NDT and CIMT, and both moderate and conflicting evidence on strength training. The other intervention categories provided only limited evidence (only one study per intervention)."

3) Previous reviews have addressed studies before 1990.<sup>2</sup> A methodological study by Siebes et al.<sup>12</sup> covering the period of 1990 to 2001 concluded that a more adequate methodology was applied more often than in the decade before. This qualitative analysis did not, however, report any results from the analyzed 50 studies. Thus we aimed to focus on trials from 1990 onwards. This is already referred to in the introduction. We had a limited number of databases readily available. It is a limitation of this study that we did not extend the searches to Embase or other possible databases. Embase indexes pharmaceutical studies, thus its value in the field of physiotherapy might be limited.

4) As suggested by the reviewer, the name of the category "upper arm NDT" is changed to the more descriptive "upper extremity treatment" throughout the text and tables.

- 5) Yes, we mean handgrip force, and the text and the tables are revised accordingly.
- 6) A description on how we rated the evidence is added to the beginning of the results section, as referred in the second point.
- 7) There was an unnecessary word "and" in the sentence referring to gait analyses (page 11). The sentence is corrected as "In one trial the strength training group performed better in gait analysis, particularly in analyses of the sum of ankle, knee and hip angles at mid-stance compared to the controls..."
- 8) The limitation of the searches is addressed in the third point.
- 9) All included trials were written in English, and this point is added to the beginning of the discussion.
- 10) The word "resolved" is changed to "addressed".
- 11) Page 17: we changed the wording of the sentence as suggested by the reviewer: "not been shown to be sensitive in detecting functional change"
- 12) Despite our categorization, no exactly similar intervention was studied in more than one trial. In Table 2, we however grouped some studies that had similar intervention components. The conclusion is thus reworded as follows: "Despite the categorization, no exactly similar intervention was studied in more than one trial, so clinical inferences can only be drawn from single studies."

All the authors have approved the final revised manuscript, and none of the authors have any financial or other relationships that might lead to a conflict of interest.

Yours sincerely,

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