Author's response to reviews

Title: Development of spasticity with age in a total population of children with cerebral palsy

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Version: 4 Date: 21 October 2008

Author’s response to reviews:

Dear Editor,

We appreciate the positive response from the referees.

Referee 4 (Jan Willem Gorter) has some further questions and suggestions:

In para 2 Gorter writes „I also think that a longitudinal design (i.e. multiple assessments per child) would be the best methodology to answer the question.” „I strongly recommend that the authors make a comment on the limitations of the design in the discussion session and indicate that a study with a true longitudinal design and that long term follow-up is indicated to confirm their findings.”

Comments "As stated in the Background, for the present study, data was gathered on the spasticity of each child at least once a year. Data is thus clearly of a longitudinal nature and the limitation of the study does therefore not lie here. The limitation is rather in the limited amount of data available on the potential differences related to the development of spasticity of different birth cohorts. It is one of the key points made in the explanation of the analysis. It is therefore our opinion that further longitudinal studies, with different birth cohorts, are needed to confirm the results, rather than a ”true” longitudinal study. The present study is a longitudinal one and the expression “true” is therefore misleading.”

We have changed “further studies” to “further longitudinal studies” in the conclusion at the end of the Discussion.

In para 3 Gorter writes „I think, with the other reviewers, that the MAS as a measure of spasticity can be questioned with regard its validity and reliability” „I would recommend to have a paragraph on measurement issues in the discussion…”

Comments In the Discussion para 3 it is stated that ” Some studies have shown problems with interobserver reliability with the Ashworth scale [15]. The reliability is better with the original scale, as is measuring plantar flexion of the ankle compared with measurement of more proximal muscles [15,16]…”
In para 5 it is stated that “In the present analysis a low reliability would, in absence of systematic errors, only increase the observed variation in the population spasticity measurements, with increased confidence interval width. A low reliability should only affect our ability to correctly detect an actual change.”

In para 6 the validity of the Ashworth scale is discussed. We mention studies indicating good validity (ref 21, 22) and a study indicating problems with validity (ref 20).

Both pros and cons regarding the reliability and validity of the Ashworth scale are presented in the discussion. We have explained (para 5) that a low reliability would increase confidence interval width, and that the statistically significant changes found in the study can not be a result of a low reliability of the Ashworth scale. We consequently do not think that a more detailed discussion regarding the reliability of the Ashworth scale is relevant for the present study.

We have added the new reference from BMC2008;9 (ref 16).

Minor points:
1. Background: The first sentence is changed according to reviewers’ suggestion
2. Discussion: We do not agree to the suggested change of word from “showed” to “indicated”. The study showed a statistically significant increasing spasticity. Then the limitations of the study are discussed later.
3. Misspelled name corrected.
4. We agree that some children with unilateral CP may have some spasticity in the contralateral leg, and that the distinction between different subtypes of CP is difficult in some children. As the absolute majority of children with unilateral CP have normal muscle tone in the contralateral leg we decided to only include the “unilateral side” in the study.

We hope that our answers and our changes in the manuscript is OK, and we appreciate a further discussion if needed.

Yours sincerely

Gunnar Hägglund