Electrical stimulation in cerebral palsy: are we asking clinically relevant questions?

Commentary

Professionals actively engaged in clinical trials involving patients with neurological damage must applaud researchers/clinicians who are willing to test popular clinical practices of equivocal or undetermined evidence. Kerr et al.'s study (p 870) is a major undertaking with sound scientific methodology. It is the fourth study published in DMCN in recent years that has failed to demonstrate the efficacy of neuromuscular electrical stimulation (NMES) in enhancing muscle force generation or improved functional ability of children with cerebral palsy (CP).

Whereas Kerr et al. speculate that a much larger sample size may yield significant results, a shortfall in their experimental design, an exceedingly high inter-participant variability, and possibly the interpretation of data management, may have contributed to masking the effect of the stimulation. Reviewing Table III raises a basic question: the NMES group included nine of 18 children whose locomotion ability depended on a posterior walker compared with only four of 20 and five of 22 children in the threshold electrical stimulation (TES) and placebo groups respectively. Basic biomechanical principle dictates that dependence on the upper extremity during ambulation minimizes the need to use the lower extremity muscles, particularly the plantar flexors and knee extensors. This statement holds whether the patient ambulates with a crouch gait (requires more quadriceps activity) or hyper-extended knees (requires very little quadriceps activity). Why the authors allocated patients to treatment arms without stratification (minimization) based on mobility deficits (the degree of dependence on upper limb support during ambulation), is not clear. It appears that children in the NMES group were less dependent on quadriceps strength and less likely to benefit from either the standard exercise therapy or stimulation protocols.

A second question (Table III data) relates to the very high inter-participant variability of quadriceps' strength (the co-efficients of variation seem to range between about 50 and 100%). To control for such inherently high variability (a common finding in patients with CNS damage) we can approach the analysis by calculating the individual patient's pre–post difference in quadriceps strength first and only then compare the groups. If such a priori treatment of the data precludes parametric analysis, one can use non-parametric ANOVA (such as Friedman's test) to test the data. I wonder why the authors' preference was to analyze the raw data and not the pre–post difference data. Would the latter yield different statistical results?

With the NMES procedure, the quadriceps contraction focused predominantly on the vastus medialis. I based my assumption on the elegant work of Vanderthommen et al.[1] who demonstrated that the effect of the stimulation occurs predominantly immediately under the stimulating electrodes. Thus, to induce contraction of as many motor units as possible in all four heads of quadriceps (all are active during locomotion) the electrodes should have covered a larger part of the quadriceps. Kerr et al. described the intensity of contraction as 'observable'. I took this to mean that the contraction did not result in an actual knee joint movement and, in fact, represented low intensity (not much different from the sub-motor TES group, yet the NMES was administered for only 1hr). We have shown that low level NMES given to healthy adults for 3 hours resulted in significant strength gain but not as much gain as intense contractions during 15- to 30-minute sessions.[2] Indeed the dose–response issue regarding NMES in children with CP remains unknown.

The rationale to stimulate the patients in the supine position is puzzling. If the authors meant to standardize the treatment to the required nighttime TES group, they artificially imposed on the NMES group a training condition that overlooked current knowledge of specificity of training.[3] It would be of interest to learn from the authors if they could offer clinically sustainable reasons why not to combine the NMES with task-specific or functional training. Previous clinical studies have shown that failing to match the stimulation with the desired outcome or with the clinical presentation may lead to questionable results. Hazlewood et al.[4] stimulated the dorsiflexors in sitting and reported significant increase in ankle range of motion but no change in ambulation variables. Van der Linden et al.[5] stimulated one gluteus maximus of children with diplegia, hemiplegia, and quadriplegia in a recumbent position and reported no benefit from NMES. Adding the Kerr et al. findings to these two studies, it appears that NMES is not an effective intervention when it is applied out of context of the clinical deficit(s), its known electrophysiological and biomechanical effects, and the desired clinical outcome measures.

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References

1. Vanderthommen M, Depresseux JC, Dauchet L, Degueldre C, Croisier JL, Crielaard JM. (2000) Spatial distribution of blood flow in electrically stimulated human muscle: a positron emission tomography study. *Muscle Nerve* **23:** 482–489.
2. Alon G, Taylor DJ. (1997) Electrically elicited minimal visible tetanic contraction and its effect on abdominal muscles strength and endurance. *Eur J Phys Med Rehabil* **7:** 2–6.
3. Dobkin BH. (2005) Clinical practice. Rehabilitation after stroke. *N Engl J Med* **352:** 1677–1684.
4. Hazlewood ME, Brown JK, Rowe PJ, Salter PM. (1994) The use of therapeutic electrical stimulation in the treatment of hemiplegic cerebral palsy. *Dev Med Child Neurol* **36:** 661–673.
5. van der Linden ML, Hazlewood ME, Aitchison AM, Hillman SJ, Robb JE. (2003) Electrical stimulation of gluteus maximus in children with cerebral palsy: effects on gait characteristics and muscle strength. *Dev Med Child Neurol* **45:** 385–390.