Letters to the Editor

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Surface Electrical Stimulation for the Affected Shoulder After Stroke: Reconsidering the Findings of Church and Colleagues

To the Editor:

We would like to raise serious methodological concerns with Church et al’s recent article, “Randomized Controlled Trial to Evaluate the Effect of Surface Neuromuscular Electrical Stimulation to the Shoulder After Acute Stroke.”1 First, we are perplexed by the authors’ choice for a primary outcome measure, the Action Research Arm Test (ARAT), and their use of the Frenchay Activities Index (FAI) as an outcome measure. Sixteen of the 19 ARAT items assess affected hand and finger movement. The FAI asks patients to rate how often they have undertaken valued activities (eg, preparing meals). Most items comprising these measures evaluate active movement in the distal regions of the affected arm. However, their intervention targeted shoulder movement. Thus, there is a mismatch between the movement abilities that the ARAT and FAI measure and the target of the study intervention.

The investigators also ignore the importance of careful patient selection. Numerous trials2,3 have evaluated sNMES as a treatment to improve pain-free lateral range of motion or reduce subluxation. Accordingly, these studies selected patients who already reported shoulder pain or were at risk for developing shoulder pain. However, the authors did not specify presence of pain, subluxation, spasticity, or stroke severity in their inclusion criteria, which likely diluted their sample. The clinically relevant question is “In what subgroup of stroke survivors is sNMES helpful?” By diluting the patient population, the study fails to address this question.

Given the goal of increasing recovery, the authors’ choice of electrical stimulation delivery is also perplexing. Task-specific repetitive training strategies, in which volitional affected limb use is encouraged, convey motor changes.4 However, Church and colleagues delivered cyclic, passive, sNMES. Because the end point of stimulation was muscle contraction and not joint translation, this further reduces the degree of afferent feedback and the potential for motor relearning. Surface electromyography (EMG)-triggered NMES has been available for over a decade, is cited by the authors, and offers the advantage of encouraging the patient to activate the affected musculature versus cyclic stimulation.5 It is odd that the authors would cite this work but not use this modality in their study.

We also have concerns regarding possible confounds. The conclusion that sNMES “may worsen arm function” is based primarily on 3-month outcomes. In general, though, treatment effect is most pronounced at the end of treatment. However, in the present study, end of treatment outcomes were remarkably similar, whereas statistically 4 weeks, we find it remarkable that the length of stay was 44% longer in the intervention group. Is it possible that the intervention group was more impaired, but the measures did not sensitively reflect this? Moreover, because subjects were all acute stroke survivors, many presumably received concomitant therapies during and after discharge. Is it possible that the control group received more follow-up therapy than the intervention group? Confounds are more plausible explanations for the lack of difference at end of treatment but a significant difference at 3 months.

Given these methodological limitations, the study results and conclusions are seriously flawed. Given the preponderance of evidence, sNMES remains a promising intervention for shoulder dysfunction for a select group of stroke survivors, especially with respect to pain.3 We are particularly concerned that results of this poorly designed study will negatively impact sNMES reimbursement and clinical use and be a major disservice to a subset of patients who might benefit from this modality.

Disclosures

None.

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