COMMENTARY

COMMENTARY ON "COMPARATIVE EFFECTIVENESS OF ROBOT-ASSISTED TRAINING VERSUS ENHANCED UPPER EXTREMITY THERAPY ON UPPER AND LOWER EXTREMITY FOR STROKE SURVIVORS: A MULTICENTRE RANDOMIZED CONTROLLED TRIAL"

We read with interest the article by Lin et al.: "Comparative effectiveness of robot-assisted training versus enhanced upper extremity therapy on upper and lower extremity for stroke survivors: a multicentre randomized controlled trial" (1).

The authors present a study with interesting findings, and conclude that robot-assisted training (RAT) is superior in reducing impairment in the lower extremities compared with enhanced upper extremities in stroke survivors. In addition, they conclude that RAT is non-inferior, but not better at reducing impairment of the upper extremities. However, we have some difficulty interpreting their findings (1).

First, the title should be a comparison of RAT versus therapist-mediated enhanced upper extremity therapy on the upper and lower extremities in individuals with stroke (2): a multicentre randomized clinical trial. As the authors compared the 2 groups the study design is a clinical trial.

Secondly, the introduction is unclear. The authors did not specify the study hypothesis and they calculated sample size via a 1-tailed hypothesis, which we consider to be a 2-tailed study. According to the study, the alternative hypothesis would be that RAT may have a better effect than therapist-mediated enhanced upper extremity therapy on the upper and lower extremities in individuals with stroke, and the null hypothesis would be that RAT may not have a better effect than therapist-mediated enhanced upper extremity therapy on the upper and lower extremities in individuals with stroke (3).

We have concerns about the study design. The methodology section reports that this is a multicentre single-blind randomized controlled design. However, if this is a controlled design why did the study deviate toward a clinical design? A randomized clinical design should have been used (4).

In the selection criteria, grades of spasticity are not mentioned; the authors describe the stretching protocol to inhibit spasticity in the intervention part. A score of 1 or 2 on the Modified Ashworth scale (MAS) should have been included for spasticity of the upper and lower limbs (5). In the conventional rehabilitation protocol they have omitted to mention the intensity, the number of repetitions, duration of stretching, range of motion exercises, strength training (6, 7) and gait training (8) for restoring functions, whereas the RAT group is well-presented and well-designed.

In their statistical analysis, the authors applied the Shapiro–Wilk test for normality, but since the sample size is >50 the normality test should be the Kolmogorov–Smirnov test (9).

The table and graphs included in the study are simple and easy to understand. In addition, the intention-to-treat analysis is appropriately applied to primary outcomes, which show significant results, although, for the secondary outcomes, the analysis of between-group difference in the Modified Barthel Index (MBI), and overall and proximal Fugl-Meyer assessment upper extremity subscale (FMA-UE), was not significant at mid-treatment and post-treatment.

The authors claim justification based on other literature, and state the limitations of their study. In conclusion, the authors have stated that RAT is not better and, in another context, also state that RAT was non-inferior; this could be confusing for readers. The authors should have stated that RAT is not better than enhanced upper extremity therapy (EUET), although it produces an almost similar result to EUET (1).

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The authors of the original article (Lin et al) were invited to reply to the commentary on their paper, but did not respond.