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LETTER TO THE EDITOR

COMMENTARY ON: THE EFFECT OF A COMPREHENSIVE eREHABILITATION INTERVENTION ALONGSIDE CONVENTIONAL STROKE REHABILITATION ON DISABILITY AND HEALTH-RELATED QUALITY OF LIFE: A PRE–POST COMPARISON

We applaud the study by Brouns et al., 2021 (1), evaluating the effects of a comprehensive e-rehabilitation intervention, alongside conventional stroke rehabilitation, on disability and health-related quality of life. However, we would like to draw the attention of the authors to a few concerns.

The authors intended to assess "health-related quality of life", as stated in the introduction. But, in the objective of the study, the term "quality of life" is used. This is confusing, as "quality of life" and "health-related quality of life" are different; the latter being more specific to patients' health conditions. The authors should have specified if they were assessing generalized quality of life, health-related quality of life, or both (2).

For the majority of aspects, including the study approval and the recruitment of patients, the authors refer to a Stroke Cohort Outcomes of Rehabilitation (SCORE) study. However, the SCORE study, as mentioned, was an observational study, and in the current study the authors explored the effects of interventions. Hence, as per the ethics guidelines for medical research, the experimental study should have been approved by the ethics committee and the trial registered with the clinical trial registering authority of the country (3).

In the inclusion criteria, the authors mentioned an extremely broad range of patient's ages, but have not reported the sex of patients, type of stroke, and localization of stroke, although they have mentioned these in the results. They should have specified their inclusion criteria using these factors, as these are all major predictors of stroke recovery. The type of stroke plays a crucial role in patient care, as older patients with ischaemic stroke have worse outcomes, both in terms of mortality and disability (4). In addition, the authors did not mention whether consent to participation was obtained from the patients.

In the outcome measures, the Hospital Anxiety and Depression scale (HADS) and the Fatigue Severity Scale (FSS) were used to assess anxiety and depression, and fatigue, respectively, but the authors do not mention these factors in the selection criteria. These 2 factors are the most important ones to include in the study, as patients with post-stroke fatigue tend to have less energy than their pre-morbid levels. Patients feel tired all the time and require frequent rest periods during daily activities (5). Social restrictions and lack of meaningful activities lead to depression and, consequently, an additional deterioration in health-related quality of life (6). In the selection criteria, the authors state that patients were excluded if they were unable to communicate in Dutch, but the outcomes, including Stroke Impact Scale (SIS) and FSS, reported by the authors were in English. It was not specified if the included patients had an understanding of English.

The calculated sample size and number of patients included in the study are different, and the number of participants in the control and experimental group was not the same. This would have been a greater predictor of bias in the results. The Consolidated Standards of Reporting Trials (CONSORT) chart is not clearly defined.

In the treatment, the authors included exercises to improve physical and cognitive functions, but they did not include assessments of balance, coordination, mobility, stability, speech or aerobic capacity in addition to the assessment of cognitive functions. As this was a pre–post study, assessment of all these is essential to measure any significant improvement after the treatment. The authors also intended to provide rehabilitation for disability; however, they did not assess disability.

The authors have no conflicts of interest to declare.

Key words: stroke impact scale; rehabilitation; stroke.

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RESPONSE TO LETTER TO THE EDITOR FROM BERBER BROUNS ET AL.

We are honoured to receive Drs Kanika and Singh's interest in our paper and thank them for their remarks on our work.

Regarding the use of the terms "quality of life" and "health-related quality of life", it is indeed correct that our study measured health-related quality of life. The term "quality of life" was used once by accident in the Abstract.

With regards to the ethics approval, the study has been approved by the regional ethics committee, although as an ongoing observational cohort study (1–5). All patients provided written informed consent for their participation. The analysis in the paper concerned a comparison of the period before and after implementation of the e-rehabilitation intervention (pre–post or quasi-experimental design). Regarding the study design, the inclusion criteria were broad in order to allow inclusion of the full spectrum of stroke patients admitted for rehabilitation. Nevertheless, a wide range of important predictors of stroke recovery, as indicated by Drs Kanika and Singh, was included in the baseline measurements, in order to account or adjust for variation in recovery in any of the analyses performed in the context of the cohort study. Patients were thus not selected based on their levels of anxiety, depression and fatigue. However, precisely because these aspects were considered very important, they were included as outcome measures in our study.

With regard to sample size, the study used data from patients included in a cohort study that was conducted in a clinical setting. As stated previously, 2 time-periods were defined, prior to and after the implementation of e-rehabilitation, yielding approximately the number of patients that was needed according to the power calculation (318 vs the calculated number of 296; +7%). The flow chart of patients in our study does not strictly follow the CONSORT guidelines, since the research was conducted in the setting of a clinical cohort study rather than being an experimental, controlled clinical trial for which the CONSORT guidelines are designed. However, in its present form it provides a transparent and comprehensive overview of the flow of patients in our study.

Finally, the study did include a wide range of variables that are elements of the concept "disability", as defined by the World Health Organization (WHO) International Classification of Functioning, Disability and Health (ICF) (6), i.e. measures of physical, mental and social functioning. The suggestion of including other measures, e.g. balance, aerobic capacity, or cognitive functioning, is valuable and will be taken into account in future research.

Once more we would like to thank our colleagues Kanika and Singh for taking the time to share their critical remarks on our work.

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