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RESPONSE TO LETTER

Effectiveness of Jaw Exercises Applied in Addition to Cervical Stabilization Exercises in Individuals with Chronic Neck Pain: A Randomized Controlled Trial [Response to Letter]

Mehmet Canli[1], Anıl Özüdoğru[1], Halil Alkan[2], Basak Cigdem-Karacay[3]

[1]Department of Physiotherapy, Kırşehir Ahi Evran University, School of Physical Therapy and Rehabilitation, Kırşehir, Turkey; [2]Department of Physiotherapy Rehabilitation, Muş Alparslan University, Faculty of Health Sciences, Muş, Turkey; [3]Department of Physical Medicine and Rehabilitation Faculty of Medicine, Kırşehir Ahi Evran University, Kırşehir, Turkey

Correspondence: Mehmet Canli, Department of Physiotherapy, Kırşehir Ahi Evran University, School of Physical Therapy and Rehabilitation, Kırşehir, Turkey, Tel +90 541 550 13 46, Email canlimehmet600@gmail.com

Dear editor

We read with great interest the letter titled "Effectiveness of Jaw Exercises Applied in Addition to Cervical Stabilization Exercises in Individuals with Chronic Neck Pain: A Randomized Controlled Trial [Letter]" regarding our recently published study. First and foremost, we would like to express our sincere gratitude to the editor for their valuable contribution. We believe that the ideas raised in the correspondence with the editor will help us examine the literature more deeply, and we are pleased to take part in this academic dialogue.

In the comment regarding the title, it was stated that the study's title does not adequately reflect the specific intervention used and the primary outcome measure. However, we respectfully disagree with this view. Our study's title clearly and concisely conveys the main intervention (jaw exercises applied in addition to cervical stabilization exercises), the target population (individuals with chronic neck pain), and the study design (randomized controlled trial). In scientific publications, it is recommended that titles succinctly reflect the key components of the study—namely the intervention, population, and study design.[1] The current title includes these elements clearly and provides readers with sufficient information about the scope of the study. Therefore, we believe that the title appropriately reflects the content and design of the study. Indeed, scientific writing guidelines also indicate that titles should be as clear and concise as possible while reflecting the main elements of the study.[2]

We thank the authors for their evaluation regarding the aim and hypothesis of our study. In the letter, it was noted that the study's aim and hypothesis were not clearly stated in the Introduction section. However, we partially disagree with this assessment. The aim of our study is explicitly stated in the Introduction as investigating the effects of jaw exercises applied in addition to cervical stabilization exercises on pain, functional status, and related clinical outcomes in individuals with chronic neck pain. In this context, the main research question and the evaluation of the intervention are presented within the text. The Consolidated Standards of Reporting Trials (CONSORT) guidelines for reporting randomized controlled trials also emphasize that clearly defining the research question and the intervention is a fundamental requirement.[1] These elements were provided in the Introduction of our study, and the research framework for evaluating the intervention's effects was clearly established. Nevertheless, we acknowledge that the authors' suggestion is valuable from a scientific writing perspective, and that expressing the study aim more explicitly could further enhance clarity for the reader.

We thank the authors for their evaluation regarding the age range used in our study. In the letter, it was suggested that the broad age range of 18–65 years could lead to variability in pain perception and functional outcomes. However, we partially disagree with this view. The primary reason for selecting the 18–65 age range in our study is that chronic neck pain is a common musculoskeletal condition affecting a substantial proportion of the adult population.[3–5] This age range represents the

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adult population who typically receive physiotherapy and rehabilitation services in clinical practice, thereby enhancing the generalizability of the findings to real-world settings. Moreover, one of the key methodological advantages of randomized controlled trials is that randomization helps balance potential confounding variables between groups. Therefore, the influence of demographic variables such as age is minimized through the randomization process.[1,6] Indeed, similar adult age ranges are frequently used in randomized controlled trials conducted in the field of musculoskeletal rehabilitation.[7–10] In this context, the age range selected in our study represents the clinical population appropriately and supports the generalizability of the results.

We thank the authors for their evaluation regarding the randomization process and allocation concealment in our study. In the letter, it was noted that the method used for allocation concealment during participant assignment was not described in sufficient detail. In our study, participants were assigned to two groups using stratified randomization to ensure balance in age and sex. Following stratification, participants were randomly allocated to either the Rocabado exercise group or the control group. Moreover, all baseline and post-treatment assessments were performed by a researcher blinded to group allocation. We acknowledge that reporting the allocation concealment process in greater detail could enhance methodological transparency. Clear reporting of randomization procedures and blinding is also recommended in the CONSORT guidelines for randomized controlled trials.[1,11] Therefore, while randomization and assessor blinding were implemented in our study, providing more detailed information would further improve clarity for readers.

Although we acknowledge that the lack of participant blinding is a limitation, we do not agree with this critique. Due to the nature of the intervention, participant blinding was not feasible.[12] However, our primary and secondary outcomes, including the Neck Disability Index (NDI)[13,14] and the Visual Analog Scale (VAS),[15] are participant-reported and validated instruments. The reliability of these measures helps minimize the risk of significant bias, and the consistency observed in our primary and secondary outcomes further supports the robustness of our findings.

We appreciate the valuable feedback regarding the sample size calculation. The difference between the reviewer's calculation and ours appears to be due to the type of statistical test selected in the G* Power software.[16] Our study's sample size was calculated based on our primary outcome, the NDI, using the "*t*-tests: Means: Difference between two independent means (two groups)" protocol.[17] Entering an effect size of 0.22 (d), a significance level of 0.05 (α), and a power of 0.80 (1-β) into this protocol yielded a total required sample size of 54. Accounting for a potential 20% dropout rate increased this to 66, indicating that the study, which was completed with 62 participants, retained adequate statistical power.

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Disclosure

The authors have no conflicts of interest to declare for this communication.

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