

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

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Dear editor

We read with interest the randomized controlled trial by Canli et al titled “Effectiveness of Jaw Exercise Applied in Addition to Cervical Stabilization Exercise in Individuals with Chronic Neck Pain: A Randomized Controlled Trial.” The study addresses an important clinical question and contributes to understanding the potential interaction between temporomandibular interventions and cervical rehabilitation in individuals with chronic neck pain.¹ However, several aspects need clarification to strengthen the study’s impact.

First, the title may not fully reflect the outcome of the intervention used in this study. Although neck disability as measured by the Neck Disability Index was identified as the primary outcome, and the Rocabado exercise as the main intervention, these are not clearly emphasized in the title. A more descriptive title, such as Effectiveness of Rocabado Jaw Exercises Applied in Addition to Cervical Stabilization Exercise in Individuals with Chronic Neck Pain on Neck Specific Functional Disability, may provide clearer information regarding both the intervention and the principal outcome. A clear and informative title helps readers quickly understand the focus and clinical relevance of the research.²

Second, while the introduction presented a detailed background and rationale, a clearly defined study objective and research hypothesis are not explicitly stated. Explicit objectives and hypotheses are essential elements of research reporting and help readers interpret whether study findings support predefined expectations.³

Third, the broad inclusion age range of 18 to 65 years may introduce variability in pain perception and neck-specific disability outcomes, potentially skewing results. Age can influence musculoskeletal pain, functional capacity, and sensory function, and older individuals may present with additional comorbidities that affect disability scores and pain reporting. Therefore, the age range should be narrowed.⁴

Fourth, although stratified randomization and assessor blinding are mentioned, the procedure for allocation concealment during participant enrollment is not clearly reported. Proper allocation concealment is an essential methodological component of randomized controlled trials because it prevents selection bias during the recruitment and assignment process.⁵

Fifth, participant blinding was not feasible due to the nature of the intervention. However, since primary and secondary outcomes such as the Neck Disability Index and visual analog scale are patient-reported measures, the absence of participant blinding may introduce performance or expectation bias, potentially influencing perceived treatment effects.⁶

Finally, clarification regarding the sample size calculation would be helpful. Based on the reported parameter, an effect size of 0.22, $\alpha = 0.05$, power = 0.80, recalculated using G*Power, suggests a required sample size of approximately

652 participants. After accounting for a 20% dropout rate, the estimated total sample would be approximately 815 participants. Further explanation of the calculation procedure would improve methodological transparency.

We urge the authors to take these points into consideration, as addressing these concerns will strengthen the validity of the study.

Funding

There is no funding to report.

Disclosure

The authors report no conflicts of interest in this communication.

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