

## Response to Letter to the Editor Regarding “Cognitive-and-motor Therapy After Stroke Is Not Superior to Motor and Cognitive Therapy Alone to Improve Cognitive and Motor Outcomes: New Insights From a Meta-analysis”



Dear Editor,—Thank you for the opportunity to respond to comments on our article, “*Cognitive-and-motor therapy after stroke is not superior to motor and cognitive therapy alone to improve cognitive and motor outcomes: new insights from a meta-analysis*”, published in *Archives of Physical Medicine and Rehabilitation*. We value the opportunity to address the points raised in your recent Letter to the Editor.

We acknowledge the heterogeneity present in our meta-analysis, both at the study and effect-size levels. This is most pronounced when comparing Cognitive and Motor Therapy (CMT) with motor therapy and when evaluating CMT dual-task or CMT integrated interventions against control interventions. Indeed, we anticipated this heterogeneity given the current dominance of pilot studies in CMT research, each employing diverse methodologies. To account for this in our statistical analysis, we used the Hartung-Knapp-Sidik-Jonkman method which is known to be robust in the presence of substantial heterogeneity.<sup>1</sup> We agree that results should be interpreted with caution, and hence we were transparent in reporting heterogeneity throughout the results section and discussed its effect on the interpretation of our findings in the limitations section. Over the entire Results section, we report heterogeneity alongside other statistical outputs, and in the Discussion (limitations), we explicitly note the highly varied rehabilitation methods used, underscoring its effect on the interpretation of our findings, and how this needs to be tempered. We also base our recommendations for future research upon this.

In response to the second and third comments regarding control interventions, we appreciate the careful observations made about the content of included control interventions. While a meta-analysis on 2 cognitive therapy studies may seem controversial, it provided a preliminary insight into the current research landscape. The inclusion of these 2 studies was not driven by selection bias but rather reflected a broader challenge in the field, namely, scarcity of Randomized Controlled Trials comparing CMT with control therapies like cognitive therapy. Our eligibility criteria were designed to achieve a robust selection process, without excluding studies based on the type/choice of control therapy received. The observation that only 2 studies used cognitive therapy as a control intervention clearly underscores the limited state of current literature. We also acknowledge restriction in the scope of motor interventions within our selected studies. Many studies labeled motor therapy as simply, “conventional physical therapy”, with little detail about their content. In short, there is, indeed, a dire need for greater transparency in reporting the specifics of control therapy interventions. For instance, interventions that involved VR were

often more aligned to the criteria of CMT rather than cognitive or motor therapy alone. A more nuanced description and understanding of specific treatment methods is needed to improve our comparison of specific modalities. These considerations align with our commitment to advancing the field of rehabilitation science and the ongoing discourse about effective interventions for stroke.

With respect to the assessment of study quality (and risk of bias), the PEDro scale is also widely used and generally more comprehensive than the Cochrane tool, encompassing a greater number of items. Moreover, there is typically an overlap between the 2 tools, as both PEDro and Cochrane evaluate randomization, allocation, blinding of therapists, patients, and assessors, as well as handling of missing data and selective reporting. PEDro also has 2 additional criteria: eligibility criteria and statistical reporting of means and SDs. To further enhance its rigor, we applied stringent cutoff criteria. We aimed for transparency, presenting distinct risk of bias results for each meta-analysis part (see Appendix of meta-analysis). Additionally, we assessed the influence of risk of bias on the results through a moderator analysis, revealing no significant effect. We acknowledge that inclusion of the Grading of Recommendations Assessment, Development, and Evaluation approach would have provided additional insights into the level of evidence. However, we believe its use is premature given that nascent state of current research in this field.

In conclusion, our meta-analysis provides an important snapshot of the current state of research in CMT after stroke. Our findings (despite the acknowledged limitations above) made an important contribute to the current and ongoing debate on optimal rehabilitation methods for stroke. Moving forward, we encourage continued collaboration and a concerted effort within the research community to address the identified gaps and enhance the quality and comparability of studies in this domain.

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## Letter to the Editor on “Rotator Interval vs Posterior Approach Ultrasound-Guided Corticosteroid Injections in Primary Frozen Shoulder: A Meta-analysis of Randomized Clinical Trials”



I commend the authors for their valuable contribution to the field<sup>1</sup>; however, I believe there is a significant issue with the inclusion of one specific study, namely the study conducted by Deng et al, titled “Comparison of Outcomes of Two Different Corticosteroid Injection Approaches for Primary Frozen Shoulder: A Randomized Controlled Study.”<sup>2</sup> My concern centers on the fact that the study by Deng did not employ ultrasound guidance for corticosteroid injections, which is a fundamental aspect of the meta-analysis’s title and research question.<sup>2</sup> Ultrasound guidance plays a pivotal role in comparing various injection approaches, as it influences the effectiveness and, most notably, repeatability and precision of the procedure.<sup>3,4</sup> The inclusion of a study that deviates from the core focus on ultrasound-guided injections introduces

inconsistency within the meta-analysis and has the potential to mislead readers.

Deng, instead of using ultrasound guidance for both the rotator interval and posterior approach groups, used injection techniques based on anatomic landmarks as advocated by Rijs et al. These techniques were reported to have a success rate of 94% for the new anterior approach and 78% for the posterior approach.<sup>2,5</sup> However, studies included in a meta-analysis comparing the effectiveness of similar injections in different injection sites should use imaging guidance. Furthermore, the incorporation of the study by Deng may significantly affect the overall findings and conclusions derived from the meta-analysis. Given that this particular study employed a different approach devoid of ultrasound guidance, its results may not be directly applicable or generalizable to the primary objective of the meta-analysis, which aims to assess and compare the efficacy of ultrasound-guided corticosteroid injections.

Although the authors made a diligent effort to address the issue of injection sites, they should also consider addressing this matter by either excluding the study by Deng from the meta-analysis or by providing a comprehensive explanation and discussion in the article regarding its inclusion and the potential ramifications on the overall results. It is imperative to uphold the integrity of the meta-analysis and ensure that it accurately represents the research question it seeks to address.

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