

Comparative Efficacy of Canggung Tanxue Acupuncture Combined with Scapular Stabilization Training versus Scapular Stabilization Training Alone for Rotator Cuff Injury: A Retrospective Cohort Study [Letter]

Manoj Joshi , Rittu Sharma , Nikita Vaid 

MM Institute of Physiotherapy and Rehabilitation, Maharishi Markandeshwar (Deemed to be University), Mullana, Haryana, Ambala-133207, India

Correspondence: Rittu Sharma, MM Institute of Physiotherapy and Rehabilitation, Maharishi Markandeshwar (Deemed to be University), Mullana, Haryana, Ambala-133207, India, Email ritubhardwaj3105@gmail.com

Dear editor

We read with interest the study by Geng et al titled “Comparative Efficacy of Canggung Tanxue Acupuncture Combined with Scapular Stabilization Training versus Scapular Stabilization Training Alone for Rotator Cuff Injury: A Retrospective Cohort Study”.¹ While the study explores an important clinical topic, several critical statistical and data-related issues require clarification, as they may substantially affect the validity and interpretation of the findings.

First, multiple outcomes (ASES, VAS, range of motion variables, SF-36 domains, and efficacy rates) were compared without a well-defined primary outcome. This heightens the chances of type I error and selective reporting.² Differences in the reported numerical values, especially in the results section and Table 2 in ASES scores, are of concern in terms of data integrity. These discrepancies must be closely checked, and datasets must be audited to guarantee internal consistency.

Second, the statistical significance interpretation does not seem to be appropriate. The overall efficacy rate (92.5% vs 81.25) is reported as positive, although the P-value is not significant ($P = 0.061$). The non-significant results should not be taken as evidence of superiority and should be reported with due caution.³ The reported effect sizes (Cohen $d > 1.4$) are too large to be considered as the results of a non-randomized study and can be attributed to overestimation by bias or insufficient adjustment. Adjusted effect sizes should be reported with confidence intervals to give more accurate estimates.⁴

Third, the data presentation is inconsistent, as some variables are reported as mean \pm SD and others as median (IQR) without any explanation. This implies that normality assumptions might not have been evaluated. Statistical test selection should be informed by appropriate testing and reporting of data distribution.⁵ Advanced statistical methods, including ROC analysis, LASSO regression, and nomogram modeling seem to be overused in relation to the sample size and study design. Such techniques are susceptible to overfitting, especially with small datasets and must be properly validated before interpretation.⁶

Fourth, ROC analyses seem to be performed on the same data without validation, which brings about the issue of circular analysis and inflated predictive performance. The post-hoc power analysis is methodologically inappropriate and can be misleading because it relies on observed effect sizes and not on the assumptions of the study design.⁷ The calculation of a rate of clinical efficacy based on VAS and its statistical analysis is redundant and can be circular. Use of validated and independent outcome measures is recommended.⁸

In conclusion, the above statistical and data-related issues, especially the problems with the definition of outcomes, data consistency, statistical significance interpretation, and the use of more complicated analytical tools, can have a significant impact on the reliability and interpretation of the research results. These points could be improved by transparent reporting, proper statistical approach, and validation plans to increase the scientific rigor and clinical relevance of future studies.



We sincerely appreciate the authors' efforts in addressing an important and clinically relevant topic. Their work contributes to the growing interest in integrative rehabilitation approaches and provides a valuable foundation for further investigation. With more rigorous methodological and statistical refinement, future studies in this area have the potential to offer more robust and clinically applicable evidence.

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Disclosure

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