

Efficacy of Cognitive Behavioral Therapy Combined with Exercise in Patients with Chronic Pain: A Systematic Review and Meta-Analysis [Letter]

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Dear editor

We read with great interest the article entitled “Efficacy of Cognitive Behavioral Therapy Combined with Exercise in Patients with Chronic Pain: A Systematic Review and Meta-Analysis” by Zhang YZ et al¹. This study provides a systematic synthesis of randomized controlled trials evaluating the combination of cognitive behavioral therapy (CBT) and exercise for adults with chronic pain, a topic of growing clinical importance. The authors reported small-to-moderate improvements in pain intensity and functional disability, along with benefits in several cognitive-behavioral outcomes, mental health, and quality of life. While we commend the authors for their comprehensive effort, we would like to raise three methodological concerns regarding the handling of multi-arm trials, which may affect the reliability and interpretability of the findings.

First, we identified a point worth clarifying regarding the handling of multi-arm trials, particularly concerning the studies by Hrkac et al,² and McBeth et al.³ In the forest plot shown in Figure 3a, the two experimental groups of Hrkac 2022 (Hrkac 2022-A and Hrkac 2022-B) each had a sample size of 29 participants, while the corresponding control groups had sample sizes of 61 and 58 participants, respectively. According to the recommendations of the Cochrane Handbook, for multi-arm trials, the usual approach is to split the shared control group proportionally so that it can be compared with each experimental group separately, while the experimental groups themselves remain intact. This is consistent with the description provided in the methods section of the article. However, based on the results presented in Figure 3, it appears that the experimental groups of Hrkac 2022-A and Hrkac 2022-B may have been split, rather than the control group. The same issue appears in Figures 4, 8, 9, 10, and 11. A similar concern regarding group splitting is also observed in the analysis of the McBeth 2012 study, which employed a four-arm design (CBT alone, graded exercise alone, CBT + graded exercise, and usual care), as shown in Figures 7, 10, and 11. In the handling of multi-arm trials in meta-analyses, the shared arm may be either the intervention or comparator arm depending on the analytic contrast. As the authors do not clearly specify which shared arm from the original multi-arm trial was divided during the analysis, the implementation of the splitting procedure remains difficult to interpret, potentially raising concerns regarding the accuracy and reproducibility of the pooled results.

Second, in the forest plots shown in Figure 3, 4, 8, 9, 10, and 11, the authors labeled the Hrkac 2022 data as “Hrkac 2022-A” and “Hrkac 2022-B”. However, neither in the main text nor in figure legends did the authors specify the specific interventions corresponding to the two groups involved in each comparison. The same issue also arises for analysis of McBeth 2012 in Figures 7, 10, and 11. Greater transparency in reporting these labels would help readers better understand the studies by Hrkac et al,² and McBeth et al,³ and would also facilitate replication of the authors’ analytical process.

Third, the reporting of the splitting method for multi-arm trials could be further clarified. The authors stated in the methods section as follows: “When the sample size of a shared control group was odd, the additional participant was allocated to the control group with the fewest intervention components.” This statement is inherently ambiguous, as the control group itself generally has the fewest intervention components, while the term “fewest” lacks a clearly defined reference standard. The authors did not provide the rationale for this rule, nor did they indicate whether sensitivity analyses were conducted to test its robustness. We respectfully invite the authors to provide a more detailed explanation of this rule, including its methodological justification, as well as any sensitivity analyses performed to assess its impact on the pooled results.

In summary, we sincerely appreciate the authors’ valuable contribution to the field of chronic pain management. Addressing the methodological issues discussed above would further improve the methodological rigor and interpretability of the findings. Clarification of these aspects may help strengthen the evidence supporting the effectiveness of CBT combined with exercise for chronic pain management. We hope our comments will be helpful in further refining this important work.

Disclosure

The authors report no conflicts of interest in this communication.

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