

Comparing the Effectiveness and Durability of Acceptance and Commitment Therapy, Cognitive Behavioral Therapy, and Their Combination with Repetitive Transcranial Magnetic Stimulation on Sleep Quality in Patients with Multiple Sclerosis

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E d i t o r	R e v i e w e r s
Seyed Hamid Atashpour Associate Professor, Department of Psychology, Isfahan (Khorasgan) Branch, Islamic Azad University, Isfahan, Iran hamidatashpour@gmail.com	Reviewer 1: Hajar Torkan Assistant Professor, Department of Psychology, Islamic Azad University, Isfahan Branch (Khorasgan), Isfahan, Iran. h.torkan@khuif.ac.ir Reviewer 2: Farhad Namjoo Department of Psychology and Counseling, KMAN Research Institute, Richmond Hill, Ontario, Canada. Email: farhadnamjoo@kmanresce.ca

1. Round 1

1.1. Reviewer 1

Reviewer:

In the PSQI description, the manuscript states that “A global score above approximately 5 is commonly interpreted as indicative of clinically relevant poor sleep quality.” The cutoff should be specified precisely rather than described as “approximately 5,” and the interpretation should be aligned with the scoring convention of the cited validation literature. More importantly, because the discussion interprets the CBT + rTMS posttest mean of 4.53 as clinically meaningful, the manuscript should distinguish statistical significance from clinical significance. Reporting the proportion of participants crossing from above to below the accepted PSQI cutoff in each group would provide substantially more clinically interpretable evidence than comparing group means alone.

In the CBT intervention paragraph, the treatment is described as a general 12-session cognitive-behavioral program containing cognitive restructuring, relaxation, behavioral modification, and relapse prevention. However, the manuscript repeatedly interprets the intervention as if it were specifically CBT for insomnia. The authors should clarify whether this was

a sleep-focused CBT/CBT-I protocol or a broader CBT program delivered to patients with MS. The sentence in the Discussion stating that “CBT may be particularly appropriate when sleep problems are strongly associated with dysfunctional beliefs, maladaptive routines, excessive pre-sleep arousal, or behavioral dysregulation” is plausible, but the intervention protocol does not appear to include several core CBT-I components such as stimulus control, sleep restriction/compression, circadian scheduling, or systematic sleep-diary monitoring. The terminology and causal interpretation should therefore be revised to reflect the actual intervention delivered.

In the Data Analysis paragraph, the manuscript describes the main analysis as “a repeated-measures covariance-based approach, with baseline sleep-quality differences controlled where appropriate,” whereas the Findings present a mixed repeated-measures ANOVA including pretest, posttest, and follow-up as three levels of time. These are not the same analytic strategy. The authors should revise the Methods so that it exactly matches the reported analysis. If pretest is modeled as one of the repeated observations, it is not simultaneously being used as a conventional covariate. If an ANCOVA framework was actually used for posttest and follow-up, the reported degrees of freedom and interaction model should be reconsidered. Analytical transparency is essential.

In the assumption-testing paragraph of the Findings, the manuscript states that Shapiro-Wilk tests showed deviations from normality but that parametric analysis was retained because group sizes were equal. This rationale is broadly defensible, yet the manuscript should report assumption tests at the appropriate level of the model rather than emphasizing minimum p values across multiple cells. Repeated-measures ANOVA is primarily concerned with the distribution of residuals rather than raw scores in each group-time cell. The authors should consider providing residual diagnostics and should avoid treating multiple Shapiro-Wilk tests as a decisive model-selection procedure. Given the bounded 0–21 PSQI scale and small group sizes, a sensitivity analysis using a robust or generalized mixed-effects approach would strengthen the conclusions.

In Table 2 and its accompanying paragraph, the key Group $\times$ Time interaction is reported as $F(6.08, 106.36) = 15.90, p < .001$, partial $\eta^2 = .476$. This is the most important inferential result and should be interpreted carefully. The authors should avoid statements suggesting that partial $\eta^2 = .476$ means that 47.6% of all change in sleep quality was “caused” by differential intervention effects. Partial eta-squared is conditional on the model and represents the proportion of effect-plus-error variance attributable to a factor, not a direct proportion of total clinical improvement. The discussion should therefore describe this as a large interaction effect rather than translating it into a causal percentage of sleep improvement.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the ACT intervention paragraph, the protocol includes several established ACT processes, but treatment fidelity is not reported. The sentence “The protocol was based on established ACT resources and focused on enhancing psychological flexibility” should be supplemented by information concerning therapist qualifications, formal ACT training, use of a treatment manual, supervision, session checklists, and any independent assessment of adherence. This is especially important in a comparative trial in which differential therapist competence or allegiance could contribute to differences between ACT and CBT. Ideally, the manuscript should report whether the same therapist delivered both psychological interventions and, if so, how cross-contamination between therapeutic approaches was minimized.

In the rTMS intervention paragraph, the authors describe a 10-Hz left-DLPFC protocol at 120% of resting motor threshold with 3,000 pulses per session. The technical description is relatively detailed, but important reproducibility information remains absent. Please report the manufacturer and model of the stimulator, coil type and geometry, exact procedure used to establish motor threshold, whether electromyographic or visually observed motor responses were used, the method used consistently for DLPFC localization, and whether neuronavigation was actually used in any participants. The current wording—“where neuronavigation was available”—implies possible variation in localization procedures across participants, which could introduce unwanted heterogeneity into stimulation exposure.

In the rTMS safety paragraph, the manuscript states that participants were screened for seizure history, metallic implants, pacemakers, and related contraindications. However, no adverse-event results are presented. Because safety is an important component of any rTMS study, the Results should report the number and type of adverse events, whether any sessions were interrupted, whether stimulation intensity had to be reduced, and whether there were between-group differences in tolerability. Even if no serious adverse events occurred, this should be explicitly stated rather than leaving safety information confined to the protocol description.

In the combined-intervention paragraph, the manuscript indicates that “rTMS was administered first, followed by a short interval of approximately 20 to 40 minutes before commencement of the CBT [or ACT] session.” A 20-minute range is relatively broad for a protocol that implicitly relies on temporal coupling between neuromodulation and psychotherapy. The authors should explain why this interval was selected, whether it was standardized or recorded for each participant, and whether variation in delay was associated with treatment response. If a hypothesized priming or state-dependent neuroplasticity mechanism underlies the combined treatment, timing is not a trivial procedural detail and should be justified with relevant evidence.

In the control-condition paragraph, the control group received usual medical care but no study-specific intervention. This design does not control for therapist attention, treatment expectancy, group participation, repeated clinical contact, or the sensory experience of rTMS. Consequently, statements implying that observed changes represent specific effects of ACT, CBT, or rTMS should be moderated. The manuscript should explicitly acknowledge that comparisons with the control group estimate the effects of the entire treatment package, including nonspecific factors. A stronger design would have incorporated an attention-control psychological condition and sham rTMS, and this limitation should be emphasized more prominently.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.