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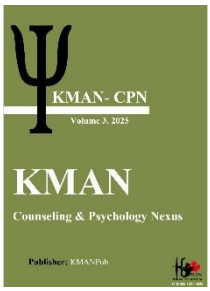

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Comparison of Mentalization-Based Therapy and Attachment-Based Therapy on Sleep Quality in Individuals with Borderline Personality Disorder

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1. Round 1

1.1. Reviewer 1

Reviewer:

The statement “Sleep disturbance is another central but relatively under-addressed feature of BPD” provides an excellent rationale; however, the paragraph includes multiple directions (psychoneuroimmunology, public health, and exercise). The reviewer recommends reorganizing this paragraph by synthesizing these strands and directly linking them to the theoretical frameworks of mentalization and attachment.

The claim “In people with BPD, sleep problems often involve difficulties initiating and maintaining sleep, nightmares…” would benefit from additional specification regarding prevalence percentages or epidemiological data to give the reader a clearer sense of magnitude.

The final paragraph clearly states the aim, but the transition sentence “Despite the strong theoretical rationale linking attachment, mentalization, and sleep regulation…” would benefit from explicitly mentioning the gap in comparative data, further strengthening the argument for a head-to-head comparison.

The sentence “To determine the sample size, various criteria exist…” appears vague. Please specify which power estimate, effect size assumptions, or statistical standards were considered. Relying solely on Dalavar (2017) may be insufficient for a robust sample justification.

The inclusion criterion “not using medication” should be clarified. Since many individuals with BPD are prescribed mood stabilizers or SSRIs, the authors should explain whether the exclusion of medicated individuals limits external validity or introduces a selection bias.

You report “Cronbach’s alpha reliability coefficient was .76” for BSL-23 in the present study. Given that prior research consistently reports $\alpha > .90$, the authors should discuss possible reasons for this discrepancy (translation issues, sample characteristics, test administration).

The description states “A global score above 5 indicates poor sleep quality.” It would strengthen methodological transparency to report the proportion of participants whose baseline PSQI exceeded 5, establishing that sleep disturbance was clinically relevant at pretest.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The section describing Iranian studies—“In the Iranian context, several studies have provided preliminary support for MBT…”—is valuable but overly condensed. The reviewer suggests clarifying which methodological differences (sample types, treatment length, outcome domains) may account for variations across studies to deepen the justification for including MBT in the present comparison.

The statement “Evidence indicates that attachment-focused interventions can reduce depressive, anxious, and adjustment symptoms…” lacks discussion of potential mechanisms. Since the current study’s theoretical framing emphasizes mechanisms, elaborating on why attachment-based interventions may affect sleep (e.g., via reduced hypervigilance) would strengthen the logic.

The therapeutic protocol is described in detail, but the paragraph “Session 8 introduced family-based stress-management techniques…” suggests strong involvement of family dynamics. The manuscript should clarify whether participants attended individually or with family members, as this affects intervention fidelity.

In the description “Session 4… offering psychoeducation about depression and borderline personality disorder,” it is unclear whether psychoeducation was standardized or delivered flexibly. Standardization matters for reproducibility; please clarify whether a manual or checklist was used.

The sentence “Data were analyzed using mixed analysis of variance (mixed ANOVA)…” should specify whether assumptions (normality, homogeneity of variances) were tested beyond Mauchly’s test. This is important because small samples increase vulnerability to Type I/II errors.

The descriptive values indicate notably low standard deviations (e.g., SD = 0.430). Such small SDs may indicate restricted range or potential scoring errors. The authors should re-check data entry and discuss whether intervention effects appear inflated due to limited variability.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.
Editor in Chief’s decision: Accepted.

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