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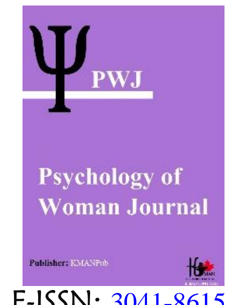

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The Effectiveness of Mindfulness-Based Cognitive Therapy on Marital Burnout and Social Anxiety in Married Women with Family Problems

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the paragraph beginning "Marital burnout is one of the major psychological consequences of prolonged marital stress," the construct of marital burnout is treated as conceptually homogeneous, whereas the literature commonly conceptualizes it as multidimensional (physical exhaustion, emotional exhaustion, and mental exhaustion). The manuscript should explicitly explain whether the Couple Burnout Measure (CBM) total score or its subdimensions were used analytically. If subscales exist but were ignored, the rationale for collapsing dimensions into a total score should be clarified because different dimensions may respond differently to mindfulness interventions.

The paragraph discussing mindfulness as a protective factor cites multiple correlational studies but insufficiently distinguishes between trait mindfulness and mindfulness cultivated through therapeutic intervention. For example, the sentence "individuals with higher mindfulness skills experience lower levels of emotional exhaustion" conflates dispositional mindfulness with intervention-induced mindfulness. The authors should theoretically clarify whether the study targets state

mindfulness acquisition through MBCT or preexisting mindfulness tendencies, because these constructs differ substantially in mechanism and clinical interpretation.

The manuscript repeatedly uses the phrase “women with family problems,” yet the construct “family problems” is never operationally defined. This is a major methodological ambiguity. The authors should specify the inclusion criteria used to identify family problems (e.g., marital conflict, emotional disengagement, domestic violence, communication dysfunction, infidelity concerns, economic conflict, or family dissatisfaction). Without an operational definition, the sampling procedure lacks reproducibility and threatens internal validity.

In the Methods section, the sentence “30 participants who obtained high scores on the marital burnout and social anxiety questionnaires were selected” raises serious concerns regarding cutoff determination. The manuscript does not specify what constituted a “high score,” whether normative cutoffs were used, whether percentile thresholds were applied, or whether clinical diagnostic criteria informed participant selection. This omission undermines the transparency and replicability of the sampling strategy and must be corrected.

The study describes the design as “quasi-experimental,” yet participants were “randomly assigned into an experimental group and a control group.” Random assignment generally reflects a true experimental component. The authors should clarify why the study is categorized as quasi-experimental rather than randomized controlled. If convenience sampling is the reason, this distinction should be explicitly justified using methodological terminology.

In the Intervention section, the description of the mindfulness-based cognitive therapy protocol is overly generic and insufficiently standardized for scientific replication. For example, the manuscript does not identify whether the intervention followed the original Segal, Williams, and Teasdale MBCT manual or an adapted culturally modified version. Session-by-session therapeutic content, mindfulness exercises, homework structure, therapist qualifications, adherence procedures, and treatment fidelity assessments should be reported in greater detail to improve reproducibility and methodological rigor.

The manuscript does not report whether any therapist effects were controlled. Because psychotherapy outcomes are often influenced by therapist competence, experience, therapeutic alliance, and adherence to protocol, the omission of therapist-related variables represents an important methodological weakness. The authors should clarify whether the same therapist conducted all sessions, whether supervision occurred, and whether therapist fidelity was evaluated.

In the Measures section, the description of the Social Anxiety Scale is insufficiently detailed. The authors state that the instrument consists of 16 items and has three factors, but the name and original developer of the scale are not clearly identified. This creates ambiguity regarding the exact instrument used. The manuscript should include the original citation, theoretical dimensions, psychometric evidence, and sample items to facilitate scholarly transparency.

The manuscript reports that “the Kolmogorov–Smirnov test confirmed the normal distribution of the data,” but no statistics, skewness values, kurtosis values, or exact p-values are presented. Given the small sample size ($n = 30$), reliance solely on the Kolmogorov–Smirnov test is statistically insufficient because normality tests have low power in small samples. The authors should supplement this analysis with skewness/kurtosis indices, Q-Q plots, or Shapiro–Wilk tests.

The manuscript lacks follow-up assessment, which significantly limits interpretation of treatment durability. MBCT is often conceptualized as a relapse-prevention and maintenance-oriented intervention, yet the study only assesses immediate post-test outcomes. The absence of 1-month, 3-month, or 6-month follow-up evaluations prevents conclusions regarding the sustainability of reductions in marital burnout and social anxiety.

In the Discussion, the sentence “mindfulness-based interventions target fundamental cognitive and emotional processes” is theoretically broad and insufficiently integrated with established frameworks such as emotional regulation theory, attachment theory, cognitive decentering, or self-compassion models. The manuscript would benefit from a more sophisticated theoretical integration explaining precisely how MBCT modifies interpersonal and intrapersonal processes relevant to marital burnout.

The study exclusively recruited women, yet some theoretical statements throughout the manuscript generalize findings to “couples” and “spouses.” For example, several cited studies involve couple dynamics broadly rather than women specifically. The authors should avoid overgeneralization and explicitly delimit the applicability of findings to married women with family problems rather than marital relationships universally.

Authors revised the manuscript and uploaded the document.

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1.2. Reviewer 2

Reviewer:

The ANCOVA procedure reported in Tables 2 and 3 lacks evidence regarding critical statistical assumptions. Specifically, there is no report of homogeneity of regression slopes, homogeneity of variances (Levene's test), independence of covariate and treatment effects, or multicollinearity diagnostics. Because ANCOVA validity depends heavily on these assumptions, the omission substantially weakens confidence in the inferential findings.

In Table 2, the reported effect size for group membership on marital burnout is $\eta^2 = .59$, which is exceptionally large for a psychosocial intervention with only eight sessions. Similarly, Table 3 reports $\eta^2 = .56$ for social anxiety. Such large effects warrant cautious interpretation and further justification. The authors should discuss whether the magnitude may have been inflated due to the small sample size, lack of active control group, expectancy effects, or regression toward the mean.

The control group received "no specific intervention," which introduces a major internal validity limitation. Without an active or placebo control condition, it is impossible to determine whether observed improvements resulted specifically from mindfulness mechanisms rather than nonspecific therapeutic factors such as attention, group support, expectancy, or social interaction. The authors should acknowledge this limitation more explicitly and discuss how future studies could employ active comparison conditions.

In the Findings section, the post-test mean for social anxiety in the control group increased from 50.45 to 52.68, whereas the experimental group showed a sharp decline to 39.38. The manuscript does not discuss the possibility that worsening symptoms in the control group may have artificially amplified the intervention effect. This pattern deserves interpretation, particularly given the absence of follow-up data and the potential influence of external stressors during the study period.

The Discussion section repeatedly interprets findings causally using phrases such as "mindfulness-based cognitive therapy helps individuals become aware of their emotional reactions" and "mindfulness enhances present-moment awareness and empathic responsiveness." However, the study did not directly measure mindfulness, emotional regulation, empathy, or cognitive flexibility. These mechanisms are therefore speculative rather than empirically demonstrated. The authors should moderate causal language and distinguish clearly between observed outcomes and inferred mechanisms.

Authors revised the manuscript and uploaded the document.

2. Revised

Editor's decision: Accepted.
Editor in Chief's decision: Accepted.

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