






The Effectiveness of Acceptance and Commitment Therapy on Marital Inhibition and Emotional Intimacy among Women in Second Marriages

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1. Round 1

1.1. Reviewer 1

Reviewer:

The description of the sampling procedure is insufficiently rigorous for a quasi-experimental study. In the “Study Design and Participants” section, the statement “Using convenience sampling, 28 participants were selected and randomly assigned” lacks critical methodological details, including inclusion/exclusion criteria, diagnostic or screening procedures, marital duration thresholds, psychiatric comorbidities, concurrent psychotherapy or medication use, and attrition management. Without these details, the internal validity and replicability of the study are weakened.

The manuscript does not adequately justify the sample size statistically. The study includes only 28 participants, yet no power analysis is reported to determine whether the sample was sufficient to detect the reported effects. In the Methods section, the authors should report an a priori power analysis (e.g., using G*Power), including expected effect size, alpha level, and statistical power assumptions. The absence of such justification raises concerns regarding both Type I and Type II error risks.

The intervention description is conceptually rich but procedurally underdeveloped. In the “Interventions” section, the authors summarize ACT techniques in narrative form; however, the manuscript lacks a session-by-session protocol, therapist qualifications, treatment fidelity assessment, adherence monitoring, and information regarding supervision procedures. Given that ACT outcomes are highly dependent on implementation quality, these omissions substantially reduce methodological transparency.

The psychometric justification for the Marital Inhibition measure requires reconsideration. The manuscript identifies the “Spouse Controlling Behaviors Scale” as a measure of marital inhibition, but the reported Cronbach’s alpha coefficients (0.59–0.70) are relatively modest for research purposes. Furthermore, the manuscript cites older validation studies without discussing construct validity in Iranian remarried populations. The authors should provide updated psychometric evidence and explain why this scale appropriately captures the intended construct in the present cultural context.

The use of only the emotional intimacy subscale from the Personal Assessment of Intimacy Questionnaire raises concerns regarding construct completeness. In the “Measures” section, the authors do not explain why the emotional intimacy dimension was isolated while other intimacy dimensions (e.g., sexual, intellectual, recreational) were excluded, particularly given the multidimensional nature of remarriage adjustment. A stronger theoretical rationale for this selective operationalization is necessary.

The absence of follow-up assessment represents a major limitation that deserves deeper discussion. The intervention targeted deeply rooted relational schemas and emotional processes; however, only immediate posttest outcomes were measured. In the absence of longitudinal data, the manuscript cannot determine whether the observed changes in emotional intimacy and marital inhibition are durable or merely short-term intervention effects.

The manuscript insufficiently addresses potential confounding variables. Variables such as duration of first marriage, cause of divorce, time elapsed since remarriage, presence of stepchildren, socioeconomic status, and current marital duration may significantly influence emotional intimacy and controlling behaviors. These factors should either be statistically controlled or discussed more explicitly as threats to internal validity.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The reporting of descriptive statistics in Table 1 contains a substantial inconsistency regarding sample size. The table reports $N = 28$ for both the experimental and control groups separately, despite the manuscript stating that the total sample size was 28 participants divided into two groups of 14. This discrepancy creates confusion regarding the actual analytic sample and must be corrected immediately.

The ANCOVA results reported in Tables 2 and 3 contain apparent statistical inconsistencies that require correction. Specifically, the reported significance values are $p = 0.061$ and $p = 0.068$, both of which exceed the conventional alpha threshold of 0.05. Nevertheless, the manuscript interprets these results as statistically significant. This is a critical statistical error that undermines the study’s conclusions and must be addressed before publication.

The manuscript does not report the assumptions required for ANCOVA adequately. Although normality is briefly mentioned, there is no discussion of homogeneity of regression slopes, linearity between covariates and dependent variables, independence of observations, or homogeneity of variances (e.g., Levene’s test). Since ANCOVA validity depends heavily on these assumptions, their omission weakens the credibility of the inferential findings.

The effect size interpretation requires more sophistication. In Tables 2 and 3, effect sizes of 0.148 and 0.193 are reported, but the manuscript does not contextualize these magnitudes according to accepted benchmarks nor explain their practical significance in clinical terms. The discussion should address whether these effects represent clinically meaningful improvements in remarital functioning rather than merely statistical outcomes.

The discussion section tends to overstate causal conclusions relative to the study design. For example, in the paragraph beginning “The reduction in marital inhibition observed in the present study may be explained,” the authors make strong mechanistic claims regarding experiential avoidance and cognitive fusion despite not directly measuring these variables. Since mediation analyses were not conducted, these interpretations should be presented more cautiously as theoretical possibilities rather than empirically demonstrated mechanisms.

The manuscript would benefit from greater integration of remarriage literature specifically, rather than relying heavily on studies involving infidelity, marital conflict, or general couples. In several discussion paragraphs, the cited literature pertains to women affected by infidelity or extramarital affairs rather than remarried populations. The authors should more explicitly justify the transferability of these findings to second-marriage dynamics or incorporate more directly relevant remarriage scholarship.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.