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The Effectiveness of Schema Therapy on Forgiveness and Marital Adjustment in Married Women

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1. Round 1

1.1. Reviewer 1

Reviewer:

The description of participant flow should be revised for transparency. The manuscript reports that 36 eligible participants were initially assigned equally to the experimental and control groups, followed by attrition of three control participants and three experimental participants, resulting in 15 participants per group. The authors should provide the timing and reasons for each exclusion more systematically and clarify whether the participant who was found to be divorced had been incorrectly enrolled despite “being married” being an inclusion criterion. This apparent inconsistency raises questions about eligibility screening and should be explicitly resolved.

The statement that the inclusion criteria included “having the ability to attend the schema therapy sessions regularly” and that exclusion occurred following “absence from more than two intervention sessions” is reasonable, but adherence should be reported more rigorously. The manuscript should specify the actual number of sessions attended by retained participants, whether any participants missed one or two sessions, whether missed content was compensated for, and whether treatment adherence was monitored. Without such information, it is difficult to determine the actual therapeutic dose received by the experimental group.

The intervention paragraph beginning "The experimental group received a schema therapy intervention based on Young's schema therapy model" provides a useful session-by-session overview, but essential intervention fidelity information is missing. The authors should report the therapist's professional qualifications, training in schema therapy, whether sessions were delivered individually or in groups, the frequency of sessions, the setting, whether a treatment manual was used, whether supervision occurred, and whether fidelity or adherence was independently assessed. These details are necessary for reproducibility and for evaluating internal validity.

The demographic section contains a numerical inconsistency that should be corrected. In the source material for the age distribution, the total row was reported as 20 for each group despite each group containing only 15 participants. Although the narrative correctly refers to 15 participants, the manuscript should ensure that every table and textual description reflects the actual sample size. All demographic frequencies and percentages should be cross-checked against the final analytic dataset before submission.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

The manuscript states that the control group "did not receive any psychological intervention during the study period." The nature of this control condition should be described more precisely, including whether participants received usual services, were placed on a waiting list, had contact with researchers only during assessments, or received any nonspecific attention. An inactive control group does not control for therapist attention, expectancy, group support, or time spent in treatment, and this limitation should be acknowledged when attributing observed differences specifically to schema therapy.

The Data Collection Tools section should revisit the scoring and interpretation of the Dyadic Adjustment Scale. The manuscript reports very low total marital-adjustment means such as 11.45, 16.35, and 13.00 while simultaneously describing the instrument as having a possible total score of 0–160. These values appear inconsistent with conventional scoring of the 32-item Dyadic Adjustment Scale and may instead represent a subscale, transformed score, or data-entry error. This issue is critical because it directly affects the validity of the findings and must be checked against the original dataset and SPSS output before publication.

A similar and potentially serious discrepancy exists for the forgiveness scores. The Heartland Forgiveness Scale is described as having a total possible score from 18 to 126, yet Table 1 reports means of 14.35, 20.40, and 17.90, which fall below the stated minimum possible total score. The authors must determine whether these values represent subscale means, item averages, transformed scores, or incorrectly transcribed statistics. Until this discrepancy is resolved, the descriptive findings and interpretation of intervention effects on forgiveness cannot be considered psychometrically coherent.

The paragraph reporting assumptions appropriately addresses normality, homogeneity of variance, and sphericity; however, the statistical assumption testing should be aligned more closely with the actual repeated-measures model. Reporting Shapiro–Wilk tests only for pretest scores is insufficient to establish distributional adequacy across all measurement occasions or residuals. The authors should clarify whether residual distributions were examined, and they should report whether influential outliers were assessed. In addition, given the small sample size of 15 per group, graphical diagnostics such as Q–Q plots would strengthen the assessment of model assumptions.

The manuscript reports that Mauchly's test was significant and appropriately applies Greenhouse–Geisser corrections. However, the statistical reporting should be standardized throughout. For example, the paper should consistently report exact p values when $p \geq .001$ and use "$p < .001$" rather than "$p = .000$." Similarly, effect sizes should be clearly labeled as partial eta squared rather than simply "effect size," and degrees of freedom should be reported consistently to an appropriate number of decimal places after Greenhouse–Geisser correction.

The interpretation of the repeated-measures results should prioritize the Time × Group interaction rather than the main effects of time and group. The sentence "These findings indicate that schema therapy was associated with significant changes in both forgiveness and marital adjustment" is generally appropriate, but the strongest evidence for differential treatment

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response is the significant interaction between time and group. A significant main effect of time alone does not establish treatment efficacy, and a between-subject group effect averaged over all time points may partly reflect baseline differences. The Discussion should therefore make the interaction effect the primary inferential basis for claims of effectiveness.

The Bonferroni post-hoc results require substantial verification because the reported pairwise mean differences do not correspond to the descriptive means in Table 1. For instance, the experimental forgiveness mean changes from 14.35 at pretest to 20.40 at posttest, a difference of approximately 6.05, yet Table 3 reports a pretest–posttest mean difference of 18.067. Likewise, the marital adjustment descriptive means do not reconcile with the reported pairwise difference of 26.733. These discrepancies suggest that post-hoc statistics may have been copied from another scoring metric or analysis. The authors should re-run the pairwise comparisons and ensure that all tables derive from the same variables and scoring conventions.

Authors revised the manuscript and uploaded the document.

2. Revised

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

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