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Effectiveness of Emotionally Focused Couple Therapy Training on Alexithymia and Perceived Stress in Women Affected by Infidelity

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the “Methods and Materials” section, the description of the research design contains inconsistencies regarding the sampling strategy and sample size. The abstract reports 45 couples divided into three groups, whereas the methods section later reports 30 women assigned to two groups of 15 participants each. This discrepancy raises concerns about the validity of the methodology and whether the article contains remnants of another manuscript. The authors must clarify the actual number of participants, whether the participants were individuals or couples, and how group allocation was performed.

The paragraph beginning with “The present study was applied research in terms of purpose…” lacks sufficient detail regarding the cluster sampling process. The authors state that three centers were selected from sixteen counseling centers, but no information is provided about the randomization procedure, selection criteria for centers, or whether the centers differed demographically or clinically. This omission limits reproducibility and introduces potential sampling bias. A detailed explanation of cluster selection and participant recruitment procedures is necessary.

The inclusion and exclusion criteria require greater operational precision. For example, the phrase “first experience of marital infidelity” is ambiguous because it does not specify whether this refers to confirmed sexual infidelity, emotional

infidelity, or suspected betrayal. Furthermore, the manuscript does not clarify whether infidelity was self-reported, clinically assessed, or verified through standardized diagnostic criteria. Given the centrality of betrayal trauma to the study, a clearer operational definition is required.

Table 1 requires substantial refinement. The presentation of descriptive statistics lacks confidence intervals and effect estimates, which are important for interpreting clinical relevance. Moreover, the "positive perception of stress" subscale appears conceptually confusing because higher scores typically indicate lower stress perception in the Cohen scale. The authors should clarify scoring direction and explain why higher scores post-intervention were interpreted positively.

The assumptions testing section reports both the Shapiro–Wilk and Kolmogorov–Smirnov tests simultaneously, which is unnecessary given the small sample size. For samples below 50 participants, the Shapiro–Wilk test alone is generally recommended. Additionally, normality statistics should be reported for each dependent variable separately rather than broadly summarized.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

The intervention section insufficiently describes the actual structure of the emotionally focused therapy sessions. While the manuscript summarizes three general phases of EFT, it does not provide session-by-session therapeutic content, therapeutic exercises, therapist qualifications, or fidelity monitoring procedures. Since the study claims therapeutic effectiveness, treatment fidelity is critical. The authors should include a detailed intervention protocol or at minimum a summarized therapeutic framework table.

The manuscript does not report whether therapists delivering the intervention received formal certification or advanced training in Sue Johnson's EFT model. Therapist competence is particularly important in emotionally focused interventions because therapeutic alliance and emotional attunement substantially influence outcomes. Without information regarding therapist qualifications and supervision, the clinical validity of the intervention implementation remains uncertain.

The psychometric discussion of the Toronto Alexithymia Scale is overly descriptive and repetitive. Several reliability coefficients from prior studies are reported redundantly without critical evaluation of construct validity within the present sample. The authors should instead focus on psychometric indices directly relevant to the current study, including confirmatory factor structure, cultural adaptation procedures for the Persian version, and reliability coefficients obtained specifically from the present sample.

The statistical analysis section contains methodological inconsistencies. The authors report using "univariate analysis of covariance and multivariate analysis of covariance," yet the study design is clearly pretest-posttest with repeated measurements. A repeated-measures ANCOVA or mixed-model analysis would appear more appropriate. The manuscript should explicitly justify why MANCOVA was selected instead of repeated-measures MANOVA and clarify how pretest scores were controlled statistically.

The paragraph reporting demographic findings includes inaccurate percentage calculations. For example, the educational frequencies reported for the experimental group (4, 6, and 8 participants) exceed the stated sample size of 15 participants. Similarly, the percentages sum beyond 100%. These numerical inconsistencies substantially weaken confidence in data accuracy and require immediate correction.

Authors revised the manuscript and uploaded the document.

2. Revised

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

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