

Development of an Integrative Intervention Combining Emotion-Focused Couple Therapy and the Relationship Enhancement Model and Evaluation of Its Effectiveness on Family Resilience and Posttraumatic Stress in Couples Experiencing Marital Infidelity

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E d i t o r	R e v i e w e r s
Izet Pehlić [✉] Full professor for Educational sciences, Islamic pedagogical faculty of the University of Zenica, Bosnia and Herzegovina izet.pehlic@unze.ba	Reviewer 1: Zahra Yousefi [✉] Assistant Professor, Department of Psychology, Isfahan Branch (Khorasgan), Islamic Azad University, Isfahan, Iran. Email: Z.yousefi1393@khuisf.ac.ir Reviewer 2: Kamdin Parsakia [✉] Department of Psychology and Counseling, KMAN Research Institute, Richmond Hill, Ontario, Canada. Email: kamdinparsakia@kmanresce.ca

1. Round 1

1.1. Reviewer 1

Reviewer:

In the Study Design and Participants section, the sentence “The required sample size was estimated using GPower software. Based on an alpha level of .05, a 95% confidence level, and the specified effect-size assumptions, a minimum of 15 couples per group was determined to be adequate*” is insufficient for replication. The authors should report the specific statistical test selected in G*Power, whether the calculation concerned the within-between interaction in repeated-measures ANOVA, the assumed effect-size metric and value, statistical power, number of groups, number of repeated measurements, assumed correlation among repeated measures, and nonsphericity correction. The value referred to elsewhere as an “effect size of .99” also appears implausibly large and may instead represent statistical power. This issue should be carefully checked because an incorrectly reported power analysis undermines confidence in the sample-size justification.

In the participant eligibility paragraph, the criterion “absence of a major psychiatric disorder as determined through clinical interview” requires considerably greater methodological detail. The manuscript should specify who conducted the interviews,

the qualifications and training of the interviewers, whether a structured or semistructured diagnostic interview was used, which diagnostic framework informed exclusion decisions, and which disorders constituted exclusionary conditions. A general “clinical interview” without a standardized procedure introduces potential assessor subjectivity and reduces reproducibility. The authors should also indicate whether substance-use disorders, active suicidality, psychosis, bipolar disorder, and severe intimate partner violence were assessed, as these conditions are particularly relevant to the safety and appropriateness of conjoint couple therapy.

In Table 1, Session 7, the intervention includes “sexual intimacy education, including the importance of sexual relationships, the sexual response cycle, hormones associated with sexual functioning, and correction of misconceptions regarding sexual relationships.” The rationale for including this relatively broad psychoeducational material should be better linked to the theoretical integration of EFCT and Relationship Enhancement. The authors should explain whether these components emerged directly from the qualitative thematic analysis, from established RE protocols, or from expert recommendations. In particular, the inclusion of “hormones associated with sexual functioning” appears somewhat peripheral to the central attachment and communication mechanisms proposed in the manuscript and should either be theoretically justified or streamlined.

In the Data Collection Tools section, the Family Resilience Questionnaire is described as having 66 items and a total score ranging from 66 to 264, yet the means reported in Table 3 are approximately 100–126, and the listed subscales sum almost exactly to the total score. The authors should verify that the scoring procedure and subscale structure correspond to the specific version of the FRQ administered. It is particularly important to clarify whether the three reported dimensions—problem solving, social resources, and acceptance of problems—represent the validated factor structure used in the Iranian adaptation. Any discrepancy between the original Sixbey instrument, the translated/adapted version, and the dimensions analyzed in the present study should be explicitly resolved.

In the Posttraumatic Stress Questionnaire paragraph, the instrument attribution and nomenclature require verification. The sentence “The Posttraumatic Stress Questionnaire (PTSQ) was developed by Weathers et al. (1993)” appears potentially inconsistent with commonly used PTSD assessment instruments associated with Weathers and colleagues. The authors should provide the exact English name, original abbreviation, scoring structure, diagnostic framework, and bibliographic source for the 17-item measure. They should also clarify whether the three dimensions reported in Table 3 correspond to DSM-III-R/DSM-IV PTSD symptom clusters or to a validated factor structure of the Persian version. Accurate identification of the instrument is essential for methodological transparency.

In the descriptive findings paragraph, the statement “The experimental group demonstrated substantial improvements in family resilience following the intervention, whereas the control group showed little change across the assessment periods” is appropriate descriptively, but baseline differences between groups should be reported more formally. Although the pretest means appear reasonably similar, the manuscript should indicate whether baseline equivalence was evaluated for the primary outcomes and relevant demographic characteristics. Reporting standardized baseline differences or appropriate group comparisons would help establish that postintervention differences are unlikely to reflect preexisting imbalance, especially given the relatively small sample.

In the assumption-testing paragraph, there is a serious issue concerning the reported degrees of freedom. The manuscript states that only 28 couples remained, divided into 14 couples per group, but the Shapiro–Wilk results in the source material report $df = 28$ separately for each group, while Levene’s test reports a second degree of freedom of 54. These values imply a larger number of observations than 28 couples and raise the possibility that individual spouses were treated as statistically independent cases. Similarly, the mixed-ANOVA error term has $df = 108$. The authors must reconcile these degrees of freedom with the stated sample size and explain the analytic unit. This is a major statistical concern because treating two spouses from the same couple as independent observations can substantially underestimate standard errors and inflate statistical significance.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the Study Design and Participants section, the description of marital infidelity eligibility—“documented experience of marital infidelity by one partner”—needs an operational definition. The manuscript should specify how marital infidelity was defined, whether sexual, emotional, online, or combined forms were eligible, whether the extradyadic involvement had to be disclosed to both partners, and whether any minimum recency or duration criterion was applied. Because different forms and timelines of infidelity may produce markedly different levels of trauma, forgiveness difficulty, attachment injury, and motivation for reconciliation, lack of a precise definition creates substantial heterogeneity in the sample and limits interpretation of treatment effects.

In the qualitative intervention-development paragraph, the statement that “a total of 28 relevant sources were purposively selected and analyzed” requires a transparent description of the literature-selection procedure. The authors should report the databases or repositories searched, search terms, publication-year limits, language restrictions, inclusion and exclusion criteria, procedures for eliminating duplicates, and reasons for selecting exactly 28 sources. Because the intervention is described as being developed through inductive thematic analysis, readers must be able to assess whether the source corpus was sufficiently systematic and comprehensive. At present, the procedure resembles a narrative selection of sources rather than a clearly reproducible qualitative evidence-synthesis process.

In the same qualitative methods paragraph, the manuscript states that thematic analysis was conducted according to Attride-Stirling, but the actual analytic process is described only in broad stages of categorization and transformation into intervention domains. The authors should provide more methodological detail regarding coding. Specifically, they should clarify whether coding was conducted by one or multiple researchers, how basic themes, organizing themes, and global themes were developed, whether a coding framework was iteratively refined, and how disagreements were resolved. Some form of coder agreement, peer debriefing, audit trail, or consensus process should be described to enhance the credibility and dependability of the qualitative phase.

In the intervention-development and validation section, the sentence “The mean CVR and CVI values were .694 and .865, respectively, indicating adequate content validity” should be expanded and statistically justified. The authors should clearly indicate which CVR formula was used, how expert responses were converted into “essential,” “useful but not essential,” or “not necessary” categories if Lawshe’s method was applied, and whether item-level CVI or scale-level CVI was calculated. In Table 2, expert ratings appear to be values such as .60, .80, .90, and 1.00, which are not conventional raw expert ratings for standard CVR/CVI calculations. The manuscript should distinguish raw ratings from derived validity coefficients and explain precisely how the reported CVR and CVI values were obtained.

In Table 1 and its accompanying paragraph, the intervention is described as a 10-session protocol, but important treatment-delivery information is missing. The authors should report session duration, frequency, whether sessions were conducted individually by couple or in a group-couple format, therapist-to-couple ratio, therapist qualifications, supervision procedures, and whether a treatment manual was followed. The sentence “The intervention was organized as a progressive therapeutic sequence” is clinically plausible, but treatment fidelity cannot be evaluated unless the manuscript explains how adherence to the protocol was monitored. A fidelity checklist, session recordings, supervision records, or independent adherence ratings would materially strengthen the methodological rigor.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.