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# Effectiveness of Emotion-Focused Couples Therapy on Sexual Satisfaction, Extramarital Relationships, and Marital Frustration

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|---|---|
| Monika Szczygieł<br>Department of Psychology, Jagiellonian University, Krakow, Poland<br>monika.szczygiel@uj.edu.pl | **Reviewer 1:** Abolghasem Khoshkanesh<br>Assistant Professor, Counseling Department, Shahid Beheshti University, Tehran, Iran.<br>Email: akhoshkonesh@sbu.ac.ir<br>**Reviewer 2:** Thseen Nazir<br>Professor of Psychology and Counseling Department, Ibn Haldun University, Istanbul, Turkey.<br>Email: thseen.nazir@ihu.edu.tr |

## 1. Round 1

### *1.1. Reviewer 1*

Reviewer:

In the same participant paragraph, the exclusion criterion stating that couples were excluded if either partner had “a severe psychiatric disorder that could interfere with participation in couple therapy, including psychotic disorders or severe depressive symptoms” requires specification of the assessment procedure. The manuscript does not indicate whether psychiatric disorders were determined through structured diagnostic interviews, medical records, therapist judgment, or participant self-report. This is particularly important for severe depression, substance misuse, suicidality, and other psychiatric conditions that may affect both safety and treatment outcomes. The authors should describe who conducted the screening, which diagnostic criteria or instruments were applied, and whether intimate partner violence or risk of harm was assessed, as these considerations are especially important in couple therapy research.

The statement that “the 24 eligible couples were randomly allocated using a computerized randomization procedure” should be expanded to describe allocation generation and concealment. A rigorous randomized design requires information on who generated the random sequence, what software or algorithm was used, whether simple or blocked randomization was employed,

and how allocation was concealed from recruiters until enrollment was complete. The later statement that "the groups were examined with respect to major baseline characteristics…to ensure adequate comparability" should not substitute for rigorous randomization procedures. The authors should report the randomization mechanism in enough detail to allow assessment of selection bias and should avoid implying that post-randomization baseline significance testing establishes equivalence between groups.

The methods paragraph states that participants completed posttest measures "within one week after completion of the final therapeutic session," but the study contains no follow-up assessment. This is a major limitation for an intervention that aims to alter attachment processes, trust, sexual satisfaction, and extramarital tendencies, because immediate post-treatment effects do not demonstrate maintenance of change. The manuscript should clearly frame the findings as short-term post-intervention effects rather than sustained treatment efficacy. The authors should also reconsider statements in the Discussion suggesting broader or enduring improvements unless supported by longitudinal data.

The paragraph describing the Larson Sexual Satisfaction Questionnaire contains a potentially important inconsistency in the measurement strategy. The manuscript states that scores are categorized into four levels and that, according to the earlier study description, individuals with dissatisfaction or low satisfaction were coded as 1 and those with moderate or high satisfaction as 2. However, the statistical analysis reports ANCOVA/MANCOVA and continuous effect estimates, which normally assume a continuous dependent variable. The authors should clarify whether raw continuous scores or dichotomized categories were actually entered into the MANCOVA. If scores were dichotomized, conventional MANCOVA would be inappropriate and substantial information would be lost; if continuous scores were analyzed, the coding description should be removed or explicitly identified as an interpretive convention not used in inferential analysis.

In the "Data Collection Tools" section, the paragraph beginning "The Tendency for Extramarital Relationships Questionnaire, originally developed by Drigotas et al. (1999)" requires more complete psychometric and scoring information. The manuscript identifies 11 items and refers to emotional, sexual, and mixed forms of infidelity, but it does not provide the response scale, possible score range, direction of scoring, subscale composition, or interpretation of higher scores. Since this instrument is a primary outcome measure, the reader must understand precisely what constitutes a reduction in "tendency toward extramarital relationships." The authors should also report reliability for the present sample rather than relying exclusively on coefficients from previous studies, particularly because the instrument assesses a culturally sensitive construct.

The reported inferential results require verification because the manuscript presents values such as "$F(1, 22) = 26.14$" despite describing a sample of 24 couples/48 individuals and three pretest covariates. The denominator degrees of freedom should be reconciled with the actual sample size, analytic unit, number of groups, and number of covariates in the model. If couples were the unit of analysis, the degrees of freedom may be plausible under a particular model; however, if individuals were analyzed, they appear inconsistent with the stated sample size. The authors should provide the complete MANCOVA model specification, including the multivariate test statistic (e.g., Wilks' Lambda, Pillai's Trace, Hotelling's Trace, or Roy's Largest Root), associated F value, degrees of freedom, p value, and effect size before presenting the follow-up univariate ANCOVAs.

The Discussion opens with the statement that "The magnitude of these effects indicates that the intervention was associated with substantial changes across all three relational outcomes." Although partial $\eta^2$ values of .48, .36, and .42 are numerically large, interpretation should be made more cautiously given the small sample and immediate posttest design. Effect sizes estimated in small randomized samples can be unstable and upwardly biased. The authors should avoid treating these estimates as precise indicators of population-level treatment magnitude and should ideally report confidence intervals for adjusted mean differences and effect sizes. Presenting adjusted posttest means with standard errors or confidence intervals would also substantially improve interpretability.

Response: Revised and uploaded the manuscript.

### *1.2. Reviewer 2*

Reviewer:


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The measurement section is inconsistent in its treatment of reliability. For the Marital Frustration Questionnaire, the authors report that "In the present study, the internal consistency of the questionnaire was reassessed using Cronbach's alpha and yielded a coefficient of .74," whereas no corresponding current-sample reliability estimates are presented for sexual satisfaction or extramarital tendencies. Reliability should be reported systematically for all primary outcome measures in the analyzed sample. Ideally, internal consistency coefficients should be presented separately for pretest and posttest if feasible, because reliability can vary across measurement occasions. Selective reporting of reliability for only one instrument weakens confidence in the measurement quality of the remaining outcomes.

The "Intervention Protocol" paragraph states that the experimental group received "10 weekly sessions of Emotionally Focused Couple Therapy, with each session lasting approximately 90 minutes," whereas the earlier abstract describes "eight structured sessions." This discrepancy must be corrected throughout the manuscript because intervention dose is a fundamental methodological characteristic. The title, abstract, methods, intervention description, participant flow, and Discussion should consistently describe the same protocol. If the intervention was in fact 10 sessions, the abstract should be revised accordingly; if eight sessions were delivered, the detailed session descriptions in the Methods must be corrected.

In the intervention paragraph, the sentence "The eighth session addressed relational injuries associated with betrayal or infidelity when applicable" raises concerns about intervention standardization. The phrase "when applicable" suggests that some couples may have received different session content even though the study population was described as having a history of extramarital involvement. The authors should clarify whether session eight was delivered uniformly to all couples or tailored according to individual clinical presentation. If treatment was individualized, the extent and nature of adaptation should be described, because differential content across couples affects treatment fidelity and reproducibility.

The manuscript states that "All intervention sessions were delivered by licensed clinical psychologists with at least five years of clinical experience in Emotionally Focused Couple Therapy" and that fidelity was supported through "review of recorded therapy sessions by senior clinicians." This is encouraging, but the fidelity methodology remains insufficiently specified. The authors should report the number of therapists, their formal EFT-C training or certification, whether therapists treated multiple couples, the proportion of sessions reviewed, the fidelity checklist or rating system employed, and any quantitative adherence results. Therapist effects should also be considered because outcomes may cluster by therapist if several therapists administered the intervention.

In the "Data Analysis" paragraph, the authors state that assumptions relevant to MANCOVA included "normality of score distributions and homogeneity of variances," but this is incomplete. MANCOVA additionally requires consideration of multivariate normality, linear relationships among dependent variables and covariates, absence of problematic multicollinearity, homogeneity of variance-covariance matrices, and, critically in ANCOVA, homogeneity of regression slopes. The authors should report the specific diagnostic tests used, such as Box's M for equality of covariance matrices and Levene's test for individual outcomes, and state whether the relationship between each pretest covariate and posttest outcome was homogeneous across groups. Given the small sample, assumption violations could materially affect the validity of the inferential results.

Response: Revised and uploaded the manuscript.

## 2. Revised

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


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