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Effectiveness of Group Couples Therapy Based on Transactional Analysis on Conflict Resolution Styles and Marital Adjustment among Women Seeking Divorce

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1. Round 1

1.1. Reviewer 1

Reviewer:

The demographic description requires correction because the reported age statistics appear internally inconsistent. The manuscript states that "the mean age of the participants was 42.80 years (SD=4.20), with an age range from 20 to 60 years." A standard deviation of only 4.20 years is difficult to reconcile with a 40-year observed age range in a sample of 30 unless the extreme values were highly isolated. The authors should recheck the raw data and verify the mean, standard deviation, minimum, and maximum. If the reported SD is correct, the age distribution should be shown or explained; if it is a transcription error, the corrected value should be reported consistently in the abstract, methods, and findings.

There is a substantial inconsistency between the stated inclusion criteria and the reported educational distribution. The "Study Design and Participants" paragraph specifies that eligibility required "possessing at least a high school diploma," whereas the findings report that "53.3% of the participants had completed lower secondary education." These two statements cannot both be correct. The authors should verify the original eligibility criteria and demographic coding. If participants with lower secondary education were included, the inclusion criterion should be revised accordingly; if the inclusion criterion was

indeed a high school diploma or higher, the demographic percentages should be corrected. This inconsistency directly affects reproducibility and raises questions about adherence to the study protocol.

The paragraph describing the participants' "birth order or position among siblings" appears conceptually unclear and potentially mislabeled. The manuscript reports that "50.0% were only children, 43.3% were first-born children, 3.3% were second-born children, and 3.3% were the youngest children in their families." The Persian source appears to refer to "number and order of children," which could be interpreted as participants' own children rather than the participants' birth order. The authors should determine precisely what variable was collected and rename it accordingly. If this variable concerns the participants' own children, the current English interpretation is incorrect; if it concerns birth order, the rationale for including it should be stated and the categories should be mutually exclusive and clearly defined.

The assumptions section includes a statement that "for marital adjustment, the assumptions were likewise satisfied: the group-by-pretest interaction was non-significant... and Levene's test was also non-significant," but it additionally reports "the variance–covariance assessment for marital adjustment was also non-significant (p=0.317)." Box's M is not applicable to a single dependent variable ANCOVA in the same way it is to a multivariate outcome model. The authors should clarify what test produced p=0.317 and remove any inappropriate reference to covariance-matrix homogeneity for the univariate marital-adjustment ANCOVA. For the single outcome, normality of residuals, linearity, homogeneity of regression slopes, homoscedasticity, and independence are the more relevant assumptions.

The normality assessment should be strengthened. The manuscript reports that "the Kolmogorov–Smirnov test indicated that the distributions of all six dependent variables at both pretest and posttest did not significantly deviate from normality." With a total sample of 30 and only 15 participants per group, reliance solely on non-significant Kolmogorov–Smirnov tests is insufficient because such tests have limited power in small samples. The authors should examine normality of model residuals within groups using Q–Q plots, standardized residuals, skewness and kurtosis, or Shapiro–Wilk tests. It is particularly important to assess residuals rather than simply raw variables because ANCOVA assumptions primarily concern residual distributions conditional on the model.

The manuscript should report whether multicollinearity and excessive correlations among the five ROCI-II outcomes were assessed before conducting MANCOVA. Because the styles represent related but theoretically distinct conflict-management tendencies, very high correlations could compromise interpretation of the multivariate test, whereas negligible correlations would reduce the value of a multivariate model. The assumptions paragraph currently mentions Box's M, Levene's test, regression slopes, and linearity, but not intercorrelations among dependent variables. A correlation matrix or a concise report of the correlation range would improve the statistical transparency of the analysis.

The interpretation of the multivariate effect size should be made more precise. The findings state that "the partial eta squared value of 0.60 indicated a substantial multivariate intervention effect" and earlier describe this as approximately "60% of the variance in the linear combination" being explained. Partial η^2 in multivariate models should not be interpreted too literally as the proportion of total outcome variance explained, particularly in small samples. The authors should use more cautious language, such as stating that the effect size was large according to conventional benchmarks, while acknowledging that effect estimates may be unstable because of the small sample.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the ROCI-II paragraph, the manuscript states that the instrument "consists of 28 items" and that each of the five conflict styles was treated as a separate dependent variable. The authors should provide the exact number of items assigned to each subscale, the response format, the possible score range for each style, and the direction of interpretation. This information is necessary because the descriptive table shows different numbers of items across subscales, which makes direct comparison of

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raw means across styles inappropriate. It would also be useful to report internal consistency coefficients for each ROCI-II subscale in the present sample rather than relying exclusively on previous validation studies.

The description of the Dyadic Adjustment Scale contains a potentially serious scoring issue. The manuscript states that "scores above 101 generally reflect relatively higher marital adjustment" and that "total scores below approximately 100 have frequently been used as an indicator of relational distress," yet the experimental group had a pretest mean of 30.40 and posttest mean of 42.80. These values are extraordinarily low relative to the conventional DAS range of 0–151 and suggest either an unusually severely distressed sample, use of a modified scoring procedure, incomplete scoring, or a possible data-entry problem. The authors should verify whether the total DAS score was computed according to Spanier's original scoring rules. If an adapted version or different scoring convention was used, this must be fully described and appropriately referenced.

The intervention paragraph should provide substantially more information about treatment fidelity and implementation. The manuscript states that "the experimental group participated in eight sessions of group couples therapy based on Transactional Analysis," but does not report session duration, session frequency, group size, therapist qualifications, therapist training in Transactional Analysis, supervision procedures, use of a manual, or any fidelity assessment. These details are essential for replication and for assessing whether the observed effects can reasonably be attributed to the intended intervention. The authors should also clarify whether only the women attended the sessions or whether spouses participated, because the term "couples therapy" generally implies participation of both partners.

The terminology "group couples therapy" requires clarification throughout the manuscript. The participants are consistently described as "women seeking divorce," and the methods do not indicate that their spouses were recruited or attended treatment. If only women participated, the intervention should not be labeled "couples therapy" without qualification, because this may mislead readers regarding the unit of treatment. The authors should specify whether the intervention was a group-based Transactional Analysis program for married women, a couple-focused group intervention delivered to one partner, or actual couples therapy attended by both spouses. The title, abstract, methods, and discussion should use terminology consistent with the actual treatment format.

In the "Data Analysis" paragraph, the description of the MANCOVA is statistically ambiguous. The manuscript states that MANCOVA was conducted "for the five conflict resolution styles" while "corresponding pretest scores" were entered as covariates. A conventional MANCOVA with five posttest outcomes and five corresponding covariates differs materially from a model with a single composite covariate, yet the manuscript does not specify exactly which covariates entered the model. The authors should explicitly state whether all five pretest conflict-style scores were entered simultaneously as covariates, whether each posttest outcome was adjusted only for its corresponding pretest score, or whether another specification was used. The reported degrees of freedom should also be checked against the exact model.

Response: Revised and uploaded the manuscript.

2. Revised

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

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