

The Effectiveness of Gottman-Based Conflict Management Training on Perceived Stress, Quality of Marital Life, and Cognitive Harmony in Conflicted Couples

Rasul. Rezaei Mollajegh¹, Zohreh. Ghanbari², Hossein. Alamdarian³, Mostafa. Tashvighi^{4*}

¹ Rasul Rezaei Mollajegh, Assistant Professor of Department of Psychology and Counselling, Tehran, Iran

² M.A. in Family Counseling, Faculty of Educational Sciences and Psychology, Al-zahra University, Tehran, Iran

³ Hossein Alamdarian, M.A. in Family Counseling, Faculty of Humanities, Islamic Azad University, Hamedan, Iran

⁴ Ph.D. in Counseling, Faculty of Educational Sciences and Psychology, University of Mohaghegh Ardebili, Ardebili, Iran

* Corresponding author email address: mostafa.tashvighi@gmail.com

Editor

Monika Szczygiel^{ORCID}
Department of Psychology,
Jagiellonian University, Krakow,
Poland
monika.szczygiel@uj.edu.pl

Reviewers

Reviewer 1: Mohammad Hassan Ghanifar^{ORCID}
Assistant Professor, Department of Psychology, Birjand Branch, Islamic Azad
University, Birjand, Iran. Email: ghanifar@iaubir.ac.ir

Reviewer 2: Azade Abooei^{ORCID}
Department of Counseling, Faculty of Humanities, University of Science and Art,
Yazd, Iran. Email: a.abooei@tea.sau.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

In the introduction, the rationale for selecting the three dependent variables—perceived stress, quality of marital life, and cognitive harmony—should be strengthened. The current theoretical framing explains marital conflict broadly, but it does not sufficiently justify why these three outcomes were chosen together. A clearer conceptual model is needed to show how Gottman-based conflict management is expected to reduce perceived stress while simultaneously improving marital quality and cognitive harmony.

In the introduction, several citations concern adolescent outcomes of parental marital conflict, such as “parental marital conflict and adolescent anxiety” and “adolescent self-perception.” While these studies are relevant to the wider consequences of marital conflict, their connection to the current sample of conflicted couples should be made more explicit. The authors should avoid overextending evidence from adolescent or parenting contexts unless they clearly explain how such findings support the clinical significance of intervening with couples.

The sentence “The present study employed a quasi-experimental design with a pretest-posttest control group and a three-month follow-up period” should be expanded to specify whether random allocation was performed at the individual level or couple level. Because this is a couple-based intervention, randomizing spouses independently would be inappropriate. The manuscript should clarify whether both members of each couple were assigned together to the same study condition.

The inclusion criterion “couples with marital conflict” is insufficiently operationalized. The manuscript should specify how marital conflict was identified, whether a standardized screening instrument or clinical interview was used, and what cutoff score or diagnostic criterion determined eligibility. Without an operational definition, the sample may include couples with heterogeneous levels of distress, weakening internal validity.

The interpretation of Mauchly’s test appears statistically incorrect. The manuscript reports that Mauchly’s test was significant for the variables, for example “ $p = .001$,” which indicates violation of the sphericity assumption. However, the text states that “the F index in the analysis of variance required no correction,” while later it refers to the Greenhouse–Geisser correction. The authors should correct this inconsistency and clearly report corrected degrees of freedom when sphericity is violated.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The exclusion criterion “absence of severe physical or psychological disorders according to the participants’ records” requires further detail. The authors should indicate what type of records were reviewed, who assessed the presence of severe disorders, and whether any structured screening or clinical diagnostic procedure was used. This is particularly important because severe psychopathology, substance use, or domestic violence could affect both treatment response and ethical suitability for group couple intervention.

The manuscript states that “the control group received no intervention during this period.” This raises methodological and ethical concerns. The authors should clarify whether the control group was placed on a waiting list, received treatment after the follow-up assessment, or had access to usual counseling services. A no-treatment control group may introduce expectancy and attention effects, so this limitation should be acknowledged.

The intervention protocol is described as “Gottman-based conflict management training,” but the content is closer to Gottman Method couple therapy principles. The authors should clarify whether the intervention was an educational training program, group psychoeducation, or formal couple therapy. This distinction matters because therapist qualification, session structure, clinical supervision, and fidelity monitoring differ between psychoeducational and therapeutic interventions.

In the intervention protocol paragraph, the manuscript should report who delivered the intervention, their academic degree, clinical training, and experience with the Gottman method. Without this information, the replicability and credibility of the intervention are reduced. The authors should also specify whether an intervention manual was used and whether treatment fidelity was assessed through checklists, supervision, audio/video review, or independent raters.

The description of the Perceived Stress Scale states that it was developed in 1997 by Cohen and colleagues. The authors should verify the date and bibliographic information of the PSS-14, because the commonly cited Perceived Stress Scale was originally introduced earlier. Any discrepancy in the psychometric description of the instrument should be corrected to maintain scientific accuracy.

The manuscript refers to “Busby’s Marital Quality of Life Scale (RADS),” but the scale described appears to correspond to the Revised Dyadic Adjustment Scale, commonly abbreviated as RDAS, not RADS. The authors should verify the exact name, abbreviation, scoring range, and construct measured by this tool. Misnaming the instrument can create confusion regarding whether the study measured marital quality, dyadic adjustment, or quality of life.

In the measurement section, the Couples’ Mutual Understanding and Cognitive Harmony Questionnaire is described in detail, but the manuscript should more clearly explain how total scores and subscale scores are interpreted. Specifically, the

authors should indicate whether higher scores consistently represent greater cognitive harmony after reverse scoring and whether the total score or subscale scores were used in the main analyses.

The findings report that “the two groups were homogeneous in terms of education,” but no table, chi-square test, frequency distribution, or significance level is provided. The authors should present demographic comparisons for education, age, marriage duration, number of children, and other relevant marital variables. In couple intervention studies, baseline comparability is essential for evaluating whether posttest differences can reasonably be attributed to the intervention.

In the results section, the assumptions of repeated-measures ANOVA are not reported with adequate precision. The sentence “the results of Box’s M test indicated that the covariance matrix assumption was met in the multivariate condition” should include the Box’s M statistic, F value, degrees of freedom, and p-value. Similarly, the Shapiro–Wilk and Levene’s test results should be reported in a more complete and transparent manner.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.