

The Mediating Role of Marital Burnout in the Relationship Between Attachment Needs and Couples' Tendency Toward Divorce


Elaheh. Khorshidi¹, Siamak. Samani^{2*}

¹ M.A in Clinical Psychology, Department of Psychology, Shi.C., Islamic Azad University, Shiraz, Iran



² Department of Psychology, Shi.C., Islamic Azad University, Shiraz, Iran

* Corresponding author email address: siamaksamani@iau.ac.ir

Editor

Anela Hasanagic
Full Professor, Department of Psychology, Faculty of Islamic Education, University of Zenica, Bosnia and Herzegovina
anela.hasanagic@unze.ba

Reviewers

Reviewer 1: Parvaneh Mohammadkhani
Professor, Department of Clinical Psychology, University of Rehabilitation Sciences and Social Health, Tehran, Iran. Email: Pa.mohammadkhani@uswr.ac.ir
Reviewer 2: Mohammadreza Zarbakhsh Bahri
Associate Professor, Department of Psychology, Tonekabon Branch, Islamic Azad University, Tonekabon, Iran. Email: M.Zarbakhsh@Toniau.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

In the Introduction, the sentence “A report by the National Organization for Civil Registration indicates that in the first seven months of 2024...” introduces a national divorce-statistics claim, but the citation appears incomplete as “National Organization for Civil, 2024.” The source name, report title, retrieval information, and exact statistical indicators should be corrected and fully documented. The authors should also avoid vague wording such as “a concerning level” unless they provide the actual divorce-to-marriage ratio and explain why it is interpreted as concerning.

In the Introduction paragraph beginning “The tendency toward divorce is a gradual and multidimensional process...,” the discussion is theoretically relevant but would benefit from a clearer operational distinction between “tendency toward divorce,” “emotional divorce,” “marital instability,” and “formal divorce.” These concepts are used closely together, yet they are not identical. The authors should define the dependent variable more precisely and explain whether the scale measures cognitive intention, emotional disengagement, behavioral readiness, or relational dissatisfaction.

In the Data analysis paragraph, the sentence “Pearson correlation test and multiple regression using the structural equation modeling method were used” is methodologically imprecise. SEM is not simply “multiple regression using SEM”; it is a latent or observed-variable modeling framework with model specification, estimation, identification, fit assessment, and

direct/indirect effect testing. The authors should describe the estimation method, missing-data handling, normality screening, outlier diagnostics, and whether the model used observed composites or latent constructs.

In the Findings section, Table 1 contains two rows labeled “Tendency toward divorce,” one with a mean of 37.04 and another with a mean of 15.44. This is a major table error that prevents accurate interpretation. The authors should verify whether the second row refers to “expression of feelings,” “loyalty,” or another subscale, and ensure all variables and subscales match the instrument description. Table numbering should also be corrected because the text refers to Table 4 when presenting Table 2.

In the assumptions-testing paragraph, the authors state that “the Kolmogorov-Smirnov test was used” and that all variables were normally distributed. For a sample of 200, normality tests can be overly sensitive or insufficiently informative depending on distributional conditions; therefore, skewness, kurtosis, histograms, Q-Q plots, and multivariate normality should be reported, especially for SEM. The manuscript reports skewness and kurtosis in Table 1 but does not connect these values to SEM assumptions or estimation choice.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the Introduction, the paragraph beginning “Contemporary research has shown that the quality of attachment in adulthood...” relies heavily on general attachment theory but does not sufficiently connect “attachment needs” as measured by Samani et al. to the broader literature on adult attachment styles. The manuscript should clarify whether attachment needs are conceptualized as a state-like relational need, a trait-like attachment orientation, or a culturally adapted marital construct. Without this distinction, the theoretical bridge between attachment theory and the selected instrument remains underdeveloped.

In the paragraph beginning “Despite the theoretical and practical importance of these variables...,” the research gap is stated too generally. The authors claim that “few studies have explained these relationships simultaneously within a mediating model,” but they do not specify which previous studies examined partial relationships, what methodological limitations they had, and why the present model advances the field. This paragraph should be strengthened by explicitly identifying the empirical gap, the cultural gap, and the methodological gap addressed by the current SEM design.

In the Methods section, the sentence “The sample size was determined to be 200 participants based on statistical power analysis using GPower software” is insufficiently reported. The authors should specify the test family, statistical test, anticipated effect size, α level, desired power, number of predictors, and assumptions used in GPower. Because SEM sample-size adequacy is usually not determined solely by generic regression power analysis, the authors should also justify whether 200 cases are adequate for the estimated model parameters.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.