

Structural Equation Modeling of Emotional Divorce Based on Personality Maladjustments and Emotional Maturity: Examining the Mediating Role of Marital Intimacy

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E d i t o r	R e v i e w e r s
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1. Round 1

1.1. Reviewer 1

Reviewer:

In the Introduction, the paragraph beginning with “Emotional divorce refers to a condition in marital relationships...” provides a broad conceptual overview but lacks a stronger theoretical integration between emotional divorce and the proposed predictor variables. The authors should explicitly explain why personality maladjustments and emotional maturity were simultaneously selected within one structural framework, rather than presenting them as isolated correlates. A conceptual diagram or theoretical synthesis paragraph before the research question would substantially improve coherence.

The statement “Although precise and official statistics on the prevalence of emotional divorce are not available in many societies” weakens the epidemiological grounding of the study because no regional or Iranian prevalence estimates are

subsequently provided. The authors are encouraged to include at least approximate national or regional indicators from family or counseling research to demonstrate the practical urgency of the issue within the Iranian sociocultural context.

The reliability evidence for some subscales of the Bagarozzi questionnaire appears weak, particularly the “social–recreational intimacy” subscale with a reported reliability coefficient of 0.51. The authors should justify retaining subscales with relatively low reliability or report whether item purification, factor loading assessment, or subscale exclusion procedures were conducted before SEM analysis.

In the data analysis section, the authors state that “Kolmogorov–Smirnov test also confirmed the normal distribution of the data,” yet PLS-SEM is typically recommended precisely because it does not require strict multivariate normality assumptions. This creates methodological redundancy and conceptual confusion. The authors should explain why PLS-SEM was preferred over covariance-based SEM despite reporting satisfactory normality indices.

The manuscript reports that “HTMT ratios for all relationships were below 0.90,” but the actual HTMT matrix is not provided. Given the importance of discriminant validity in SEM studies, the authors should include the complete HTMT table either within the manuscript or as supplementary material to allow readers to independently evaluate construct distinctiveness.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The paragraph discussing personality maladjustment heavily relies on broad terminology such as “emotional instability,” “impulsivity,” and “psychological flexibility,” yet the manuscript later operationalizes maladjustment using the PID-5 brief form. The authors should explicitly connect the DSM-5 maladaptive trait domains (Negative Affectivity, Detachment, Antagonism, Disinhibition, Psychoticism) to marital functioning and emotional divorce in the literature review to ensure conceptual consistency between theory and measurement.

The discussion of emotional maturity contains a conceptual inconsistency because the manuscript repeatedly refers to “emotional maturity” as the predictor, while the scoring direction of the Singh scale actually measures emotional immaturity. This issue later creates interpretive ambiguity in the findings section. The manuscript should consistently distinguish between “higher emotional maturity” and “higher emotional immaturity scores” throughout the theoretical framework and results interpretation.

In the section beginning with “On the other hand, a review of the research literature shows that marital intimacy is one of the key components...,” the authors cite Sternberg’s Triangular Theory of Love but do not sufficiently explain why intimacy specifically—not passion or commitment—was selected as the mediator. A more rigorous theoretical justification is needed to establish why intimacy is expected to mediate the effects of personality maladjustment and emotional maturity on emotional divorce.

The manuscript states, “Most studies have examined pairwise relationships among these variables and have paid less attention to multivariate structural models,” yet no systematic literature gap analysis is presented. The authors should strengthen the novelty claim by explicitly comparing the current model with previous SEM or mediation studies and identifying which direct and indirect pathways remain underexplored in prior literature.

In the Methods section, the sentence “The present study was applied in terms of its purpose” is methodologically vague and should be revised. The term “applied” does not clearly communicate the epistemological orientation or practical implications of the research. The authors should instead describe whether the study is explanatory, predictive, or theory-testing in nature.

The sampling procedure requires more methodological transparency. The authors mention “purposive sampling based on inclusion criteria,” but they do not explain how participants were approached, whether recruitment occurred in classrooms or counseling centers, what the response rate was, or how many individuals declined participation. Without this information, sampling bias cannot be adequately evaluated.

The justification for sample size based on “Kline’s (1998, 2023) recommendations” is insufficiently detailed because the manuscript does not specify the number of estimated parameters in the model. Since SEM adequacy depends on model complexity, the authors should report the exact number of latent variables, indicators, and structural paths used in calculating the sample-to-parameter ratio.

The demographic information section states that “Demographic characteristics of the sample, including age, gender, duration of marriage, and level of education, are presented in Table 1,” but important contextual variables are missing. Variables such as number of children, employment status, income level, history of counseling, and type of marriage arrangement may significantly influence emotional divorce and should either be controlled statistically or acknowledged as limitations.

The description of the Bagarozzi Marital Intimacy Needs Questionnaire appears conceptually inconsistent because the manuscript interprets higher scores as “greater perceived need for intimacy” rather than actual experienced intimacy. This raises concerns regarding construct validity because the mediation model assumes marital intimacy as an experienced relational quality, not merely a perceived unmet need. The authors should clarify whether the instrument measures intimacy satisfaction, intimacy expectation, or intimacy need.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.