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Predicting Emotional Divorce Based on Early Maladaptive Schemas and Frustration Tolerance in Married Women: The Mediating Role of Cognitive Flexibility

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1. Round 1

1.1. Reviewer 1

Reviewer:

The Introduction should better differentiate frustration tolerance from distress tolerance. The manuscript states that "Frustration tolerance refers to the ability to endure discomfort, delay gratification, manage unmet expectations, and remain psychologically regulated when goals or needs are obstructed," but several cited studies appear to concern distress tolerance rather than frustration tolerance. These constructs are related but not interchangeable. The authors should clarify their conceptual distinction, explain why Harrington's Frustration Discomfort Scale is appropriate for the present hypotheses, and avoid using evidence on distress tolerance as though it directly validates conclusions about frustration tolerance unless the conceptual bridge is explicitly justified.

The rationale for cognitive flexibility as a mediator requires greater theoretical specificity. In the paragraph stating, "Cognitive flexibility may be especially important in understanding how early maladaptive schemas affect emotional divorce," the proposed mediation is plausible, but the manuscript should explicitly describe the temporal and psychological sequence

assumed by the model. For example, the authors should explain whether maladaptive schemas are hypothesized to constrain alternative interpretations of partner behavior, which then reduces adaptive conflict responses and increases emotional disengagement. Similarly, the pathway from frustration tolerance to cognitive flexibility to emotional divorce should be described as a distinct mechanism rather than presented as an extension of the schema pathway.

The reported reliability values warrant scrutiny, particularly for frustration tolerance. The manuscript reports Cronbach's alpha = .995, composite reliability = .994, and AVE = .837 for the frustration tolerance construct. Such extremely high reliability values may indicate substantial item redundancy, duplicated content, common-method effects, or a misspecified reflective model. The authors should examine inter-item correlations, outer loadings, and possible redundancy among the 35 frustration-tolerance items. They should also clarify whether all 35 items were modeled individually as reflective indicators or whether subscale scores were used. Reliability values approaching 1.00 should not automatically be interpreted as superior measurement quality.

The assessment of discriminant validity is incomplete and should be expanded. The manuscript mentions the Fornell–Larcker criterion, but no discriminant validity results are presented in the Findings. Contemporary PLS-SEM reporting should include the heterotrait–monotrait ratio (HTMT), ideally with bootstrap confidence intervals, because HTMT is generally more sensitive than the Fornell–Larcker criterion in detecting discriminant validity problems. This is especially important in the present model because early maladaptive schema domains are conceptually related and the cognitive flexibility subdimensions may also exhibit substantial intercorrelation. The authors should report HTMT values and specify the threshold used.

The assumptions section should not rely primarily on the Kolmogorov–Smirnov test for a sample of 350. The statement, "The Kolmogorov–Smirnov test was statistically significant for… all variables (all p = .001), indicating significant departures from normality," is statistically correct but incomplete because tests of normality are highly sensitive to sample size. The authors should supplement this with skewness and kurtosis values, histograms or Q–Q plots, and, more importantly for PLS-SEM, an assessment of extreme non-normality and influential observations. They should also report the actual VIF and tolerance ranges rather than simply stating that multicollinearity was acceptable.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The Methods section should resolve the inconsistency between the study's stated sampling rationale and the inferential claims made later in the manuscript. The sentence, "Participants were selected using purposive, or judgmental, sampling," indicates a non-probability sampling strategy, yet the manuscript frequently speaks in ways that may imply population-level representativeness. The authors should explicitly acknowledge that the sample cannot be assumed to represent all married women in District 5 of Tehran and should revise any language implying population prevalence or generalizability. The implications of purposive recruitment from cultural centers, counseling centers, parks, kindergartens, residential complexes, and social-media channels should also be discussed because these recruitment sources may systematically overrepresent women with particular socioeconomic or help-seeking characteristics.

The sample-size justification should be revised using contemporary PLS-SEM principles. The Methods section states, "Based on the recommendation of approximately 5 to 20 observations for each estimated model parameter… a minimum sample of approximately 250 participants was considered necessary." Rules based on cases per parameter are relatively crude and are not the preferred basis for PLS-SEM sample-size determination. The authors should ideally provide an a priori statistical power analysis based on the maximum number of predictors of an endogenous construct, anticipated minimum effect size, alpha level, and desired power, or employ an inverse square-root or gamma-exponential approach where appropriate. The final sample of 350 is likely adequate, but its adequacy should be demonstrated through a method aligned with the analytical framework used.

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The manuscript contains an important numerical inconsistency in the data-cleaning description that must be corrected. The original procedural material reports that “378 valid questionnaires” remained after screening, whereas the final Methods and Findings use 350 as the analyzable sample. The authors should clearly report how many questionnaires were distributed, returned, excluded, and included in the final analysis, with specific exclusion reasons and counts. A transparent participant flow statement is needed so that the effective response rate and data-screening process can be independently evaluated.

The measurement description of the Emotional Divorce Questionnaire requires psychometric and conceptual clarification. The manuscript states, “A total of eight or more affirmative responses has also been suggested as an indicator that the marital relationship may be at risk,” while later descriptive results report a mean of 19.637 out of 24, suggesting that a very large proportion of the sample may fall in a highly elevated range. The authors should explain whether the questionnaire is validated as a continuous measure suitable for SEM or primarily intended as a screening instrument with categorical thresholds. They should also discuss the implications of using dichotomous items in a latent-variable model and explain how these indicators were treated in SmartPLS.

The description of the Young Schema Questionnaire contains scoring inconsistencies that should be addressed before publication. The manuscript refers to a “six-point Likert-type response scale ranging from 1 to 6,” whereas the source material initially also described a five-point format. The authors should verify the exact Persian version administered and report the wording and direction of all six response categories. They should also specify whether the analysis used the total schema score, the five domain scores, individual schema scores, or a hierarchical latent construct formed from the five domains. This is essential because the structural figure appears to model “Early Maladaptive Schemas” as a higher-order construct reflected by five domains.

The measurement model requires a substantially more rigorous description. The Methods section states that “indicators with inadequate loadings were removed during model refinement,” while an earlier statement indicated that “no items were removed.” The factor-loading table shows that several items were in fact removed, including items with loadings below .70. The authors should explicitly list all deleted items by instrument and explain the statistical and substantive rationale for deletion. They should also report whether item deletion was decided before or after inspecting the model and whether removal altered the content coverage of the constructs. Deleting multiple indicators solely to improve loadings can artificially inflate reliability and convergent validity and may compromise content validity.

Response: Revised and uploaded the manuscript.

2. Revised

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

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