

Structural Modeling of the Relationship Between Attachment Styles and Expectations of Marriage: The Mediating Role of Gender-Role Orientations Among University Students Approaching Marriage

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1. Round 1

1.1. Reviewer 1

Reviewer:

The sample-size justification requires reconsideration. The manuscript states that, “Based on Kline’s (2016) recommendation regarding the adequacy of approximately 15 participants per observed variable in SEM analyses, a total sample of 322 students was considered sufficient.” The authors should identify exactly how many observed variables and freely estimated parameters were included in the tested model and demonstrate how the value of 322 followed from the stated rule. More importantly, contemporary SEM sample-size justification should ideally consider model complexity, number of parameters, anticipated effect sizes, estimator, distributional characteristics, and statistical power rather than relying exclusively on a general participant-to-variable ratio. A model-specific power analysis or Monte Carlo justification would considerably strengthen the methodological rigor.

There is a major mismatch between the scoring description of the Marriage Expectation Scale and the way the variable is analyzed. The Methods section initially emphasizes that the instrument yields scores between 40 and 200 and explains the

interpretation of a total score, whereas the Findings section explicitly treats “realistic,” “pessimistic,” and “idealistic” expectations as three independent variables and states that no total score was calculated. Because the authors have correctly emphasized that none of the multidimensional variables should be represented by a total score, the Methods section must be rewritten accordingly. The exact items belonging to each marriage-expectation subscale, the possible score range of each subscale, reverse-scoring procedures within each component, and evidence for the reliability and validity of each separate component should be reported.

A similar conceptual problem appears in the description of attachment. The manuscript states that the Collins and Read instrument assesses “three styles of secure, anxious, and avoidant attachment,” but then describes its original dimensions as “Close, Depend, and Anxiety.” These are not automatically equivalent to three independent secure, avoidant, and anxious subscale scores. The authors should explain precisely how the scores labeled “secure attachment,” “avoidant attachment,” and “anxious attachment” were derived from the 18-item Revised Adult Attachment Scale. If Close and Depend were combined or transformed to represent security or avoidance, the scoring algorithm and supporting psychometric reference must be given. Otherwise, the labels used in Table 1, Table 2, Table 4, and Figure 1 may not correspond to the actual measurement structure of the instrument.

The psychometric treatment of the attachment measure is particularly concerning because the reported Iranian Cronbach’s alpha coefficients include .55 for Depend and .48 for Close. The paragraph reports these coefficients but does not address their implications. Internal consistency below approximately .60 is generally weak and raises questions about measurement error and the stability of structural coefficients. The authors should calculate and report reliability coefficients for all attachment components in the present sample rather than relying solely on earlier Iranian estimates. If similarly low reliability is observed, sensitivity analyses, alternative reliability estimates such as McDonald’s omega, or a latent-variable measurement model that explicitly accounts for measurement error should be considered.

The mediation analysis requires more complete bootstrap reporting. Table 4 states that “indirect-effect confidence intervals are based on bootstrap estimation,” yet the Methods section does not specify the number of bootstrap resamples, whether confidence intervals were percentile or bias-corrected, or whether standardized or unstandardized indirect effects were bootstrapped. These details are necessary for reproducibility. The authors should specify, for example, that 5,000 bootstrap samples were used with 95% bias-corrected confidence intervals and should report unstandardized indirect effects alongside standardized estimates where possible. The significance of mediation should be based primarily on confidence intervals excluding zero rather than on *p* values alone.

The interpretation of partial mediation should be made more precise. The Discussion states that “the mediating effects were partial rather than complete” because direct paths remained significant after inclusion of the mediators. This is broadly correct, but the manuscript should quantify how much of each attachment–expectation relationship was transmitted through each mediator rather than relying only on significance. For instance, the total indirect effect of secure attachment on realistic expectations was described as approximately .124, but the total effect and proportion mediated were not reported. The authors should present direct, specific indirect, total indirect, and total effects for each attachment–marriage-expectation pathway and avoid inferring the strength of mediation merely from statistical significance.

Authors uploaded the revised manuscript.

1.2. Reviewer 2

Reviewer:

The manuscript must report the internal consistency of every variable as measured in the present sample. The instrument paragraphs currently cite historical coefficients such as “Cronbach’s alpha coefficients of 0.79 for the masculinity dimension and 0.83 for the femininity dimension,” but reliability is sample-dependent and should not be inferred from previous studies. Cronbach’s alpha and preferably McDonald’s omega should be presented for secure, avoidant, and anxious attachment; realistic, pessimistic, and idealistic marriage expectations; and masculine and feminine gender-role orientations. If sexual self-

concept and emotion-regulation components remain in the manuscript, their reliability coefficients should also be reported. This information is essential for evaluating whether the observed structural relationships are interpretable.

The appearance of sexual self-concept and emotion-regulation variables in Table 1 and Table 2 is unexplained and should be resolved. Table 1 contains “sexual self-concept,” “suppression,” and “reappraisal,” although these variables are absent from the title, study aim, principal theoretical model, stated data-collection tools, and mediation hypothesis. Their inclusion creates the impression that parts of the Findings section may have been imported from a different model or that important study variables were omitted from the Methods. The authors should either remove these variables entirely from the manuscript or revise the theoretical framework, Measures section, hypotheses, structural model, and discussion to explain their prespecified role. Variables should not appear only in descriptive and correlational tables without a clear analytic purpose.

The manuscript needs to state unequivocally whether the SEM was an observed-variable path model or a latent-variable structural equation model. The Findings section refers to “latent or observed constructs,” while Figure 1 shows only rectangular observed variables and no item-level indicators or latent factors. If the components were entered directly as observed variables, the analysis is more accurately described as path analysis within the SEM framework. If latent constructs were modeled, the manuscript must report a measurement model, factor loadings, residual variances, factor correlations, convergent validity, discriminant validity, and confirmatory factor analysis before testing structural paths. This distinction is not merely terminological; it determines the appropriate analytical assumptions and interpretation of the reported coefficients.

The preliminary assumption testing is underreported. The Findings section states that “examination of the distributions did not reveal severe departures from normality” and that “the Shapiro–Wilk statistics were within an acceptable range,” but no actual statistics are presented. With a sample of 322, Shapiro–Wilk can be highly sensitive to minor deviations, and SEM additionally requires attention to multivariate normality rather than only univariate normality. The authors should report skewness and kurtosis for each component, identify any outliers, describe the handling of missing data, assess multivariate normality—for example, through Mardia’s coefficient where appropriate—and state what estimator was used in AMOS. If normality assumptions were violated, bootstrap standard errors or another robust estimation strategy should be documented.

Table 2 should be revised to correspond strictly to the prespecified model and should include a clearer analytical rationale. The paragraph currently interprets correlations among 11 variables, including sexual self-concept, suppression, and reappraisal, although the structural model contains only eight principal components. Moreover, the statement that “the pattern of correlations therefore provided preliminary support for the proposed mediating mechanism” is too strong. Correlations can establish bivariate association but cannot substantiate mediation. The authors should restrict Table 2 to theoretically relevant components unless additional variables are explicitly modeled and rephrase the interpretation so that the correlations are described as satisfying preliminary association conditions rather than providing evidence of mediation.

The model-fit reporting in Table 3 should be strengthened and aligned with current SEM reporting standards. The table provides χ^2 , χ^2/df , GFI, AGFI, CFI, TLI, IFI, NFI, RMSEA, and SRMR, but omits the chi-square p value and does not explain whether these statistics refer to the hypothesized model before or after modification. GFI, AGFI, and NFI are now less emphasized in contemporary SEM reporting than CFI, TLI, RMSEA with confidence intervals, and SRMR. The authors should report the χ^2 test with its exact p value, clarify the estimation method, state whether correlated residuals or modification indices were used, and, if model modifications were made, provide a theoretical justification rather than presenting the final model as though it had been entirely prespecified.

Authors uploaded the revised manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.