

Predicting Marital Burnout Based on Negative Affect and Psychological Distress: The Mediating Role of Alexithymia in Couples

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R e v i e w e r s

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1. Round 1

1.1. Reviewer 1

Reviewer:

The psychometric description of the instruments should be revised for conceptual accuracy. For example, the Marital Burnout Questionnaire paragraph states that “for estimating validity through internal consistency, alpha coefficients were calculated.” Cronbach’s alpha is an index of internal consistency reliability, not validity. Similar conceptual confusion appears in the Psychological Distress Questionnaire section, where a Cronbach’s alpha coefficient is described as evidence of “validity.” The authors should clearly distinguish reliability from validity throughout the Measures section and report appropriate evidence for construct, criterion-related, convergent, or factorial validity separately from reliability coefficients. This correction is essential because accurate psychometric terminology is expected in an empirical behavioral-science manuscript.

The description of the PANAS requires careful verification because the manuscript states that, following confirmatory factor analysis, “six items were removed from the positive and negative affect dimensions” and subsequently refers to a two-factor model. It remains unclear whether the current study administered the original 20-item PANAS, a 14-item modified Persian version, or only the negative-affect subscale. Because negative affect is one of the core predictors in the structural model, the exact number of items used, response scale, scoring range, and source of the adapted version must be explicitly stated. If items

were removed in a previous validation study, the authors should identify which version was administered here and should not imply that item removal was performed in the present dataset unless it actually was.

The manuscript should report mediation using bootstrap confidence intervals rather than relying exclusively on statements such as “the indirect effect ... was significant ($\beta = .217, p < .05$).” Contemporary mediation analysis recommends estimation of the indirect effect with a sufficiently large number of bootstrap resamples and reporting the lower and upper bounds of the bias-corrected 95% confidence interval. The authors should specify the number of bootstrap samples used in AMOS, report confidence intervals for both indirect effects, and state that mediation was supported when the interval excluded zero. It would also be valuable to report the total effects and clarify whether alexithymia represents partial or complete mediation after accounting for the remaining direct effects.

The Discussion should be revised to distinguish empirical interpretation from causal inference. Statements implying that negative affect or psychological distress “increases vulnerability,” “contributes to,” or is “converted into” marital burnout should be qualified because the study is cross-sectional. The observed model establishes covariance patterns that are compatible with the hypothesized mediation model, but alternative temporal directions are equally plausible; for example, marital burnout itself may increase psychological distress, negative affect, and difficulty identifying emotions. The authors should explicitly acknowledge the possibility of reciprocal or reverse relationships when interpreting the findings and should avoid presenting statistical mediation in cross-sectional data as evidence of a demonstrated psychological mechanism.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The manuscript reports very high Cronbach’s alpha coefficients for several measures, including “.956” for marital burnout, “.929” for negative affect, and “.937” for psychological distress, but provides no item-level or construct-level reliability analysis. Because extremely high alpha values may sometimes indicate substantial item redundancy, the authors should consider reporting additional reliability estimates such as McDonald’s omega and, where appropriate, composite reliability from the measurement model. More importantly, because the study uses SEM, the authors should demonstrate that the latent constructs have adequate measurement properties rather than relying exclusively on coefficient alpha for total scores. Reporting standardized factor loadings, composite reliability, average variance extracted, and discriminant validity would substantially strengthen the psychometric foundation of the structural analysis.

The rationale for treating normality as adequately established solely because “the skewness and kurtosis values for all variables fell within the acceptable range of -2 to $+2$ ” is insufficient for maximum-likelihood SEM. Univariate skewness and kurtosis do not establish multivariate normality, which is a more relevant assumption for conventional maximum-likelihood estimation. The authors should report whether multivariate normality was evaluated, for example through Mardia’s coefficient, and explain whether any robust estimation or bootstrapping procedures were used if normality was violated. This is especially important because mediation effects often have non-normal sampling distributions and are more appropriately tested using bias-corrected bootstrap confidence intervals.

Table 2 appears to contain several possible transcription or significance-coding errors that should be checked against the original statistical output. For example, the correlation between negative affect and externally oriented thinking is reported as “.149*,” while substantially larger coefficients such as “.646*” and “.654*” are also marked with only one asterisk. With a sample of approximately 230, correlations of .646 and .654 would ordinarily yield p values far below .01, making the single-asterisk notation implausible. In addition, the correlation between marital burnout and difficulty identifying feelings is shown as “.671/” in the source table, suggesting a missing digit or formatting error. Every correlation and corresponding significance marker should be verified directly from the statistical output before publication.

The structural equation model is not described in sufficient detail to permit evaluation of model specification. The manuscript reports global fit statistics—“ $\chi^2 = 20.013, df = 16, CFI = .997, TLI = .995, NFI = .987, RMSEA = .033$ ”—but does

not explain which variables were modeled as latent constructs, which observed indicators loaded onto each construct, whether total scores or subscales were used, or whether any residual covariances were specified. Because the Method section explicitly justifies sample size in terms of latent variables, the reader reasonably expects a genuine latent-variable SEM rather than a path analysis based on observed total scores. The authors should provide a complete description of the measurement and structural models, including indicators, factor loadings, error terms, covariance specifications, and endogenous residuals.

The phrase “following model modification, the fit indices reached satisfactory levels” is methodologically important but insufficiently transparent. Any post hoc modification of an SEM should be fully disclosed, including the fit of the original hypothesized model, the modification indices consulted, the parameters added or removed, and the theoretical justification for each modification. Modifying a model solely to improve fit can capitalize on sample-specific characteristics and inflate apparent model adequacy. The authors should therefore present fit statistics for both the initial and modified models and explain why each modification was theoretically defensible rather than merely data driven. Ideally, the modified model should be described as requiring replication in an independent sample.

The reported path coefficients in Table 4 require statistical verification because some combinations of the unstandardized estimate, standard error, and p value appear internally inconsistent. For example, the direct effect of negative affect on marital burnout is reported as $B = .143$ with $SE = .078$ and $p = .011$. A conventional ratio of estimate to standard error would be approximately 1.83, which would not ordinarily correspond to a two-tailed p value of .011. Similar concerns arise for several other pathways. The authors should report the critical ratio or z statistic for each path and verify all coefficients, standard errors, and p values directly against AMOS output. Errors in this table would materially affect the conclusions regarding hypothesis support and therefore must be resolved.

The mediation results require particularly careful reconsideration because the reported indirect effects do not appear to correspond mathematically to the stated constituent paths. The standardized path from negative affect to alexithymia is reported as $\beta = .209$ and the path from alexithymia to marital burnout as $\beta = .388$; the product of these coefficients is approximately .081, yet the reported indirect effect of negative affect on marital burnout is $\beta = .217$. Conversely, psychological distress $\rightarrow$ alexithymia is $\beta = .559$ and alexithymia $\rightarrow$ marital burnout is $\beta = .388$, whose product is approximately .217, whereas the manuscript reports an indirect effect of $\beta = .081$. This strongly suggests that the two indirect effects may have been transposed. The mediation table, abstract, Results, and Discussion should all be checked against the original bootstrap/SEM output and corrected consistently.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.