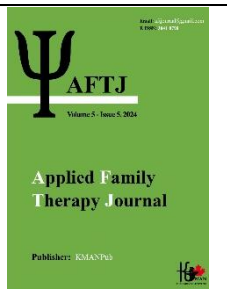
KMAN PUBLISHING INSTITUTE
Journal Website

Article history:
Received 11 May 2026
Revised 16 August 2026
Accepted 25 August 2026
Published online 01 September 2026

Applied Family Therapy Journal

OPEN PEER-REVIEW REPORT



E-ISSN: 3041-8798

Structural Equation Modeling of Marital Burnout Based on Childhood Emotional Neglect, Attachment Anxiety, Emotion Dysregulation, and the Moderating Role of Partner Support

Reem. Al-Sabah[1], Ryan. MacDonald[2*], Tharindu. Jayasinghe[3]

[1] Department of Psychology, Kuwait University, Kuwait City, Kuwait
[2] Department of Sports Psychology, Acadia University, Wolfville, Canada
[3] Department of Clinical Psychology, University of Peradeniya, Peradeniya, Sri Lanka

*** Corresponding author email address**: ryan.macdonald@acadiau.ca

Editor	Reviewers
Habib Hadianfard Affiliation: Professor, Department of Psychology, Shiraz University, Iran hadianfd@shirazu.ac.ir	**Reviewer 1:** Farhad Namjoo Department of Psychology and Counseling, KMAN Research Institute, Richmond Hill, Ontario, Canada. Email: farhadnamjoo@kmanresce.ca **Reviewer 2:** Manijeh Daneshpour Department of Couple and Family therapy, Alliant International University, California, United States of America. mdaneshpour@alliant.edu

1. Round 1

1.1. Reviewer 1

Reviewer:

In the paragraph beginning “Attachment anxiety is particularly relevant in this regard,” the theoretical explanation appropriately invokes hyperactivation of the attachment system, yet attachment avoidance is omitted from both the conceptual rationale and the model. The manuscript should provide a more explicit justification for selecting attachment anxiety alone from the two-dimensional adult attachment framework. The sentence “because the theoretical model specifically focused on attachment anxiety” in the Methods is not itself a sufficient rationale. The authors should explain whether prior evidence suggests that anxiety is more closely related to marital burnout than avoidance, whether avoidance was excluded for parsimony, or whether the study was designed around a specific hyperactivation-based mechanism.

The conceptualization of partner support requires greater precision. The Introduction defines it broadly as “the emotional, informational, instrumental, and relational assistance perceived as available from one's spouse,” whereas the Methods operationalize it using only four partner-focused items adapted from the Multidimensional Scale of Perceived Social Support. The authors should clarify whether this measure adequately captures the multidimensional partner-support construct described theoretically. If the measure primarily assesses perceived emotional availability and general support, the conceptual definition

should be narrowed accordingly. Alternatively, if the authors intend to retain a multidimensional conceptualization, they should acknowledge that the operationalization captures only a limited portion of that construct.

The preliminary data-screening description should be expanded because the manuscript currently reports criteria without presenting corresponding empirical results. The Methods state that multivariate outliers were assessed using Mahalanobis distance and that missing values were handled using expectation-maximization, but the Findings do not report the number of multivariate outliers detected, whether any cases were removed, the criterion used for Mahalanobis distance, or the exact proportion and pattern of missing data. A complete report should indicate whether missingness was examined for randomness, whether Little's MCAR test or a comparable procedure was used, and how the analytic sample of 412 changed, if at all, following multivariate screening.

In Table 1, reliability is reported using both Cronbach's alpha and composite reliability, but no evidence of convergent or discriminant validity is provided. Because composite reliability is typically reported in the context of latent measurement models, the manuscript should also provide average variance extracted and, preferably, HTMT ratios or another appropriate discriminant-validity criterion. This is particularly important for attachment anxiety and emotion dysregulation, which correlated at r = .55 and are theoretically related constructs. The authors should demonstrate empirically that these measures capture distinguishable latent dimensions rather than overlapping manifestations of generalized psychological distress.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The Study Design and Participants section reports that “participants were recruited through community organizations, family and relationship support centers, social media advertisements, university community boards, and online research participation platforms.” This recruitment strategy needs substantially greater methodological detail. The authors should report the provinces or regions represented, the approximate contribution of each recruitment channel, whether recruitment was national or concentrated in specific metropolitan areas, whether any incentives were offered, and whether duplicated participation was prevented. These details are particularly important because the paper later emphasizes the relevance of conducting the study “in a Canadian sample,” yet the present description does not demonstrate geographic or demographic representativeness of Canadian married adults.

The sample-size justification in the Methods is currently too general. The manuscript states that “this sample size exceeded commonly recommended requirements for structural equation modeling and provided an adequate ratio of participants to freely estimated parameters while also offering sufficient power for detecting small-to-moderate interaction effects.” This should be replaced with an explicit a priori or sensitivity power analysis. The authors should specify the assumed effect size, alpha level, desired statistical power, number of predictors or free parameters, and the software or simulation procedure used. This is especially important because moderation effects in SEM are typically smaller than main effects and can require substantially larger samples than models containing only direct paths.

The description of the measurement model is insufficient for a study presented as structural equation modeling. The Data Analysis section moves directly from reliability and correlations to “the hypothesized structural model was estimated using maximum-likelihood estimation,” but it does not report whether a confirmatory factor analysis was conducted before testing structural paths. The manuscript should clearly describe the latent-variable measurement model, identify which items or parcels served as indicators, report factor loadings, convergent validity, discriminant validity, and measurement-model fit, and explain whether the same indicators were subsequently used in the structural model. Without this information, it is difficult to determine whether the analysis is genuinely latent-variable SEM or essentially path analysis based on observed composite scores.

The operationalization of marital burnout needs greater psychometric detail. The Methods state that “the total score was used as the primary indicator of marital burnout,” yet the Results and Figure present marital burnout as though it were part of a structural equation model. If a single total score was entered as an observed variable, the authors should describe the analysis

AFTJ Applied Family Therapy Journal
E-ISSN: 3041-8798

as observed-variable path modeling rather than full SEM unless other latent components were modeled. If marital burnout was represented as a latent factor, the manuscript must report its indicators and loadings. This same issue applies to childhood emotional neglect, attachment anxiety, emotion dysregulation, and partner support, all of which are currently described primarily in terms of total scores.

The moderation procedure requires methodological clarification. The Data Analysis section states that "predictor variables were mean-centered before interaction terms were created" and that these interaction terms were "entered into the structural model." If the model used latent variables, conventional multiplication of mean-centered scale scores would not constitute latent moderated SEM. If observed composites were used, then the analysis is closer to moderated path analysis. The authors should explicitly state whether moderation was estimated using observed product terms, latent moderated structural equations, product indicators, or another approach. The estimation method and handling of non-normal product terms should also be justified.

Response: Revised and uploaded the manuscript.

2. Revised

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

AFTJ Applied Family Therapy Journal
E-ISSN: 3041-8798