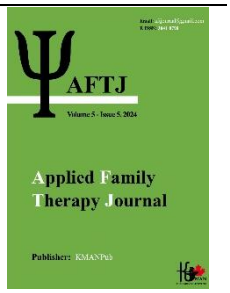
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Hierarchical Moderated Regression Analysis of Marital Burnout Based on Work–Family Conflict, Emotional Exhaustion, Spousal Support, and Psychological Flexibility

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the Introduction paragraph beginning “Marital burnout is clinically important because it may represent an intermediate condition between persistent relational distress and more severe outcomes,” the conceptual status of marital burnout requires greater precision. The manuscript alternates among the terms “marital burnout,” “couple burnout,” “marital boredom,” “relational burnout,” and sometimes broader constructs such as marital dissatisfaction. These concepts overlap but are not necessarily psychometrically or theoretically interchangeable. The authors should define marital burnout using the specific conceptual framework underlying the Couple Burnout Measure and distinguish it explicitly from marital dissatisfaction, relationship distress, emotional disengagement, and divorce intention. This clarification is particularly important because several cited studies concern “marital boredom” or “parental burnout” rather than marital burnout itself. The Introduction should explain why findings from these adjacent constructs can legitimately inform the proposed model without implying construct equivalence.

The demographic description states that the sample included "222 women (53.9%) and 190 men (46.1%)." The manuscript should clarify exactly how gender was assessed and coded. If the questionnaire included additional gender identities but none were selected, this should be stated transparently; if only binary response options were provided, that limitation should be acknowledged. This issue is particularly relevant because the manuscript cites research documenting gender differences in work–family processes and because gender was included as a regression covariate. The coding used in the regression model should also be reported, for example, 0 = men and 1 = women, so that the sign and interpretation of the coefficient can be reproduced.

The eligibility criterion requiring participants to be "currently married or living with a common-law spouse for at least one year" warrants conceptual reconsideration. The article is framed consistently in terms of "marital burnout," yet common-law partnerships were apparently included without reporting how many participants were legally married versus cohabiting. The authors should either restrict terminology to "couple/relationship burnout," justify combining legally married and common-law couples, or provide subgroup information and test whether marital status meaningfully affects the outcome. In Canada, common-law relationships are socially and legally important, but they should not automatically be treated as identical to marriage when the dependent construct is explicitly labeled marital burnout.

In the Data Collection Tools section, the description of the Couple Burnout Measure states that "after appropriate reverse scoring of positively phrased items, item scores are averaged to produce an overall marital burnout score." The authors should provide the exact scoring procedure used in the present study, including which items were reverse-scored, whether all 21 items were retained, whether any recommended scoring transformations were followed, and how missing item responses were handled. The manuscript should also specify the version and language of the instrument administered. These details are necessary because the principal dependent variable must be replicable precisely, particularly when regression coefficients are interpreted in raw-score units.

The measurement of emotional exhaustion requires careful revision. The manuscript states that emotional exhaustion was assessed using "the emotional exhaustion subscale of the Maslach Burnout Inventory–General Survey" and that it "contains five items." The authors should verify the official subscale name, number of items, response format, scoring, and licensing requirements against the exact MBI version used. More importantly, because emotional exhaustion is conceptually close to marital burnout, the authors should address discriminant validity between the predictor and criterion. A high correlation between two exhaustion-based constructs may partly reflect overlapping wording or generalized negative affect. At minimum, the Discussion should acknowledge construct overlap; preferably, the Methods or supplementary analyses should provide evidence that the measures represent distinguishable constructs rather than alternative manifestations of generalized exhaustion.

The spousal-support measure is currently underdescribed. The manuscript states that support was evaluated using "spouse-specific items adapted from the social support framework developed by Caplan and colleagues," but adaptation of an established measure can alter its psychometric properties. The authors should identify the precise original instrument, cite its original development source, reproduce or characterize the four items sufficiently for conceptual evaluation, explain the adaptation procedure, and specify whether emotional and instrumental support were intentionally collapsed into a single score. Evidence of factor structure or at least an exploratory/confirmatory test in the present sample would strengthen confidence that the four items form a unidimensional moderator. Reporting alpha alone is not sufficient to establish construct validity.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The Introduction paragraph beginning "One of the most consequential pressures on contemporary marital relationships is work–family conflict" would benefit from a more rigorous treatment of the directionality of work–family conflict. The manuscript describes the broader construct as involving interference in both directions but subsequently measures only work-to-family conflict. Because family-to-work conflict is excluded empirically, the theoretical discussion should consistently refer

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to work-to-family conflict when discussing the variable actually analyzed. Alternatively, the authors could justify why the work-to-family direction was selected as the theoretically relevant component for predicting marital burnout. As currently written, readers may incorrectly assume that the total bidirectional work–family conflict construct was measured. The distinction should also be reflected consistently in the title, research hypotheses, tables, and discussion if only the work-to-family component was operationalized.

In the paragraph stating that "spillover refers to the transfer of strain from one life domain to another, whereas crossover refers to the interpersonal transmission of stress between individuals," the distinction is theoretically useful but insufficiently connected to the actual study design. The present study collected data from only one member of each couple, meaning that true crossover effects between partners cannot be empirically evaluated. The authors should revise this part of the theoretical framework so that crossover is presented as a contextual mechanism supported by previous dyadic literature rather than as a mechanism directly tested in the present data. This distinction should also be carried into the Discussion, where some statements currently approach a dyadic interpretation despite the individual-level design.

The Introduction paragraph introducing psychological flexibility states that this construct involves "the capacity to remain in contact with present experiences, including uncomfortable thoughts and emotions, while adapting behavior in ways that remain consistent with important values and situational demands." This section needs a stronger theoretical bridge between psychological flexibility and moderated regression. At present, evidence for its moderating role is inferred primarily from Acceptance and Commitment Therapy intervention studies. Intervention efficacy does not by itself demonstrate that dispositional psychological flexibility statistically moderates stress–burnout relationships. The authors should articulate a more explicit mechanism, for example, that psychological flexibility reduces experiential avoidance, cognitive fusion, and rigid behavioral responding under high stress, thereby attenuating the translation of work–family interference and emotional exhaustion into relational burnout. This would provide a stronger theoretical rationale for the interaction terms.

In the Methods paragraph reporting that "the final analytic sample therefore consisted of 412 participants," the manuscript should provide an a priori sample-size or statistical-power justification. This is particularly important because moderated multiple regression generally requires more power than tests of main effects, especially when four interaction terms are entered simultaneously. The authors should specify the anticipated effect size for the incremental interaction block, alpha level, desired power, number of predictors, and resulting minimum sample size, preferably using an established power-analysis procedure. Without this information, it remains unclear whether N = 412 was determined analytically or simply reflects the number of usable responses. A power justification would materially strengthen confidence in both statistically significant and nonsignificant effects.

In the Study Design and Participants section, the manuscript states that participants were recruited "from Ontario, Quebec, British Columbia, and Alberta through workplace mailing lists, professional associations, community organizations, social-media advertisements, and online participant-recruitment networks." This recruitment description is too broad to permit assessment of selection bias or reproducibility. The authors should report the approximate number or percentage recruited through each channel, the recruitment period, whether advertisements were targeted geographically, whether compensation was provided, and how duplicate participation was prevented. Because recruitment channels can substantially affect occupational composition and socioeconomic characteristics, these details are important for judging representativeness. The manuscript should also acknowledge that the sample is a convenience rather than probability sample unless a probabilistic sampling strategy was actually employed.

Response: Revised and uploaded the manuscript.

2. Revised

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

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