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Causal Modeling of Emotional Exhaustion Based on Cognitive Avoidance and Psychological Flexibility with the Mediating Role of Mindfulness

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1. Round 1

1.1. Reviewer 1

Reviewer:

The paragraph describing sample-size determination states that “a sample of 360 students was considered adequate” based on “approximately 10 to 15 participants for each observed variable or model parameter.” This justification requires revision because the manuscript does not clearly specify which observed indicators or freely estimated parameters were used in applying this rule. The four questionnaires contain considerably more than 24–36 individual items, and therefore a literal application of the stated rule would imply a much larger sample. The authors should explain whether scale/subscale scores rather than individual questionnaire items were modeled as indicators, provide the exact number of indicators and estimated parameters, and preferably supplement the rule-of-thumb justification with an a priori power analysis or a recognized SEM sample-size rationale.

The sampling paragraph indicates that “proportionate stratified random sampling combined with cluster sampling was employed,” followed by selection of schools, “one class and one academic major” from each selected school. The procedure remains insufficiently reproducible. The authors should report the number of schools selected from the original 50, the number

of classes sampled, the number of students recruited from each academic-major stratum, and whether sex was also considered in proportional allocation. Because students were nested within classrooms and schools, the authors should additionally discuss the potential design effect created by cluster sampling and justify treating all 360 observations as statistically independent in the SEM analysis.

The Findings section reports reliability and convergent validity using Cronbach's alpha, composite reliability, and AVE, but it does not report indicator loadings. The statement that "AVE values ranged from .529 to .687, exceeding the recommended minimum criterion of .50" is insufficient for evaluating the measurement model because AVE alone cannot establish that individual indicators perform adequately. The authors should report standardized outer loadings for all indicators or, at minimum, their range for each construct and explain whether any indicators below accepted thresholds were retained or deleted. If item-level measurement was not used, the manuscript must clarify how AVE and composite reliability were computed from the available indicators.

The discriminant-validity paragraph states that "the diagonal value was greater than the absolute correlations of the corresponding construct with the remaining variables," relying exclusively on the Fornell–Larcker criterion. This assessment should be strengthened because Fornell–Larcker has limited sensitivity to discriminant-validity problems. The authors should additionally report the heterotrait–monotrait ratio (HTMT), preferably with bootstrap confidence intervals, and demonstrate that HTMT values fall below an accepted threshold. Cross-loadings may also be reported where appropriate. This is particularly important because mindfulness and psychological flexibility are theoretically overlapping constructs and their simultaneous inclusion appears to produce unexpected structural coefficients.

Table 3 reports a Pearson correlation of −.256 between cognitive avoidance and emotional exhaustion, .398 between psychological flexibility and emotional exhaustion, and .427 between mindfulness and emotional exhaustion. These directions require much deeper scrutiny. In particular, the positive correlation between mindfulness and emotional exhaustion conflicts with the conventional conceptualization of mindfulness as a protective factor, while the negative association between cognitive avoidance and exhaustion is also counterintuitive. The authors should verify whether the dependent variable or predictor scales are reverse-oriented. A full correlation matrix with p values should be provided, and the manuscript should explicitly distinguish zero-order correlations from partial structural effects. If these signs remain after verification, the authors should acknowledge that the empirical pattern challenges the proposed theoretical framework rather than treating the discrepancies primarily as statistical artifacts.

The manuscript reports that "cognitive avoidance and psychological flexibility jointly accounted for 43.3% of the variance in mindfulness" and that the model explained "64.6% of the variance in emotional exhaustion." These R^2 values are important but should not be interpreted as evidence of causal explanatory power in a cross-sectional design. The wording throughout the manuscript—particularly terms such as "causal model," "direct effect," and "influence"—should be moderated unless temporal precedence or experimental manipulation has been established. The authors may retain "causal modeling" if required by the methodological tradition, but they should explicitly state that the coefficients represent hypothesized directional associations rather than demonstrated causal effects.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The manuscript repeatedly refers to the primary outcome as "emotional exhaustion," yet the instrument described is the "20-item Multidimensional Fatigue Inventory developed by Smets et al. (1996)," with dimensions of general fatigue, physical fatigue, reduced activity, reduced motivation, and mental fatigue. These constructs are not interchangeable with emotional exhaustion as conventionally conceptualized in burnout research. This is a major construct-validity issue. The authors should either revise the outcome terminology throughout the title, abstract, introduction, hypotheses, findings, and discussion to accurately reflect multidimensional fatigue, or provide a rigorous theoretical and psychometric justification for treating the

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MFI score as a measure of emotional exhaustion. Without such clarification, the central dependent variable is conceptually misclassified.

The Data Collection Tools section states that the Multidimensional Fatigue Inventory is scored "from 1, indicating that the statement is completely true, to 5, indicating that the statement is completely untrue," but does not explain reverse-scored items or the direction of the resulting total score. This omission becomes particularly important because several structural coefficients are opposite to theoretically expected directions. The authors should present the exact scoring procedure for each scale, identify reverse-coded items, specify whether higher total scores represent greater fatigue/exhaustion, greater mindfulness, greater psychological flexibility, and greater cognitive avoidance, and confirm that all reverse-scoring was completed before model estimation.

The Methods section describes psychological flexibility using the Ben-Itzhak, Bluvstein, and Maor questionnaire and states that "higher scores indicate a greater capacity to adjust cognitive, emotional, and behavioral responses." However, the reported direct effect on emotional exhaustion is positive ($\beta = .243$), and the Discussion acknowledges that this "differs from much of the previous literature." Before interpreting this finding substantively, the authors should rule out a scoring-direction error. The original instrument structure, item coding, subscale composition, and any negatively worded items should be rechecked. The authors should also report the zero-order correlation between psychological flexibility and emotional exhaustion alongside the adjusted path coefficient to determine whether a suppression effect actually occurred.

A comparable concern applies to the statement that "mindfulness had a significant direct effect on emotional exhaustion ($\beta = .158$, SE = .069, $t = 2.477$)." The positive coefficient is theoretically counterintuitive given the questionnaire's stated scoring direction, and the manuscript currently explains this by suggesting that mindful students may "recognize and report their exhaustion more accurately." This explanation is speculative and should not precede basic statistical verification. The authors should first confirm scoring accuracy, inspect zero-order correlations, variance inflation factors, tolerance values, and construct correlations, and formally assess whether statistical suppression or multicollinearity produced the sign reversal. Only after these checks should a substantive explanation be offered.

The descriptive-results paragraph states that "the Kolmogorov–Smirnov statistics were statistically significant for all four variables, indicating departure from univariate normality," and then argues that the subsequent analysis was conducted "within a variance-based structural equation modeling framework." The manuscript should explicitly identify the software and estimation algorithm used, presumably PLS-SEM if variance-based modeling was employed. This information is currently absent. The authors should specify the software name and version, weighting scheme, bootstrapping settings, number of bootstrap resamples, treatment of missing data, convergence criteria, and whether the structural model was estimated from item-level indicators, subscale scores, or total scores.

Response: Revised and uploaded the manuscript.

2. Revised

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

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