

The Mediating Role of Perceived Stress and Coping Styles in the Association Between Sleep Quality and Job Burnout Among Physicians at Medical Centers Affiliated with Mashhad University of Medical Sciences


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1. Round 1

1.1. Reviewer 1

Reviewer:

In the paragraph beginning with “One of the most critical yet often overlooked determinants of occupational well-being is sleep quality,” the discussion is conceptually rich but would benefit from a clearer operational distinction between subjective and objective sleep quality measures, especially given that your study uses PSQI, which is subjective; this distinction is necessary to align theory with measurement .

The sentence “Shift work, in particular, disrupts circadian rhythms and has been consistently linked to poor sleep quality and reduced work ability” requires more precise integration with your sample characteristics; since not all physicians may be shift workers, clarify whether this contextual factor applies uniformly to your participants or only to a subset .

In the paragraph discussing perceived stress, the statement “perceived stress captures the cognitive appraisal process” is accurate but insufficiently elaborated; it would be beneficial to explicitly link this to Lazarus and Folkman’s transactional model of stress to provide a theoretical anchor for your mediation hypothesis .

The introduction claims that “research that simultaneously investigates perceived stress and coping styles as parallel mediators is limited,” yet no systematic review or meta-analytic evidence is cited to substantiate this gap; you should either provide stronger empirical justification or rephrase this claim more cautiously .

In the methods section, the sentence “Sampling was conducted using convenience, non-random sampling” raises concerns about external validity; you should explicitly discuss potential sampling bias and its implications for generalizability already in the methods, not only in the limitations section .

The description “a final sample size of 367 participants was determined” using Cochran’s formula is appropriate; however, you should provide the exact parameters used (p , q , d) and the finite population correction formula to ensure reproducibility of the sample size calculation .

The paragraph stating “the mediating role of perceived stress... provides a more nuanced understanding” would benefit from quantitative reporting of indirect effects within the discussion (e.g., effect sizes, proportion mediated) to maintain consistency with results reporting .

In discussing coping styles, the sentence “effective coping requires cognitive clarity... compromised by inadequate sleep” is theoretically valid but lacks direct empirical linkage to your data; consider explicitly connecting this claim to your observed path coefficients .

The limitations section appropriately mentions self-report bias, but it omits discussion of common method variance (CMV), which is particularly relevant given that all variables were collected via self-report instruments; you should address this explicitly and, if possible, report any statistical tests (e.g., Harman’s single-factor test) .

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the measures section, the statement “Cronbach’s alpha... generally exceeding 0.70” is too generic; you should report the actual reliability coefficients obtained in the present study for each instrument rather than relying on prior literature .

The sentence “Statistical assumptions, including normality and outliers, were examined” is insufficiently detailed; you should specify the exact tests used (e.g., Kolmogorov–Smirnov, skewness/kurtosis thresholds, Mahalanobis distance) and report their outcomes .

In reporting Table 1, while you state that CR and AVE values are acceptable, the interpretation would benefit from referencing specific thresholds (e.g., Fornell–Larcker criteria) and explaining why AVE values slightly above 0.50 are considered adequate .

In Table 2 interpretation, the sentence “the negative correlations between problem-focused coping and other variables... align with theoretical expectations” is appropriate, but the magnitude of these correlations (e.g., -0.523) suggests moderate relationships; a more nuanced interpretation of effect sizes should be included rather than simply confirming theoretical alignment .

In the structural model results, the reported path coefficient “sleep quality → perceived stress ($\beta = 0.989$)” is extremely high and approaches unity, which raises concerns about multicollinearity or measurement overlap; you should critically examine and justify this unusually large coefficient .

Similarly, the coefficients “sleep quality → problem-focused coping ($\beta = -0.993$)” and “emotion-focused coping ($\beta = 0.969$)” are near-perfect effects, which is statistically implausible in behavioral research; you should verify model specification, potential common method bias, and discriminant validity issues .

The statement “problem-focused coping shows a small negative effect on burnout ($\beta = -0.031$)” suggests a negligible effect size; this should be interpreted cautiously, and you should clarify whether this path is statistically significant and practically meaningful .

In the discussion, the claim “sleep disturbances may function as an early warning indicator of burnout” is conceptually appealing but not directly testable within your cross-sectional design; you should avoid causal or temporal language that exceeds your study design .

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.