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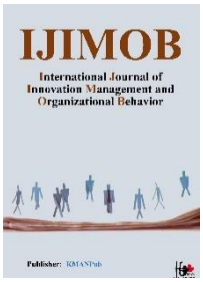

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The Effect of Artificial Intelligence–Induced Stress on Employees' Individual Creativity: Examining the Roles of Stress-Coping Strategies and Management Commitment

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1. Round 1

1.1. Reviewer 1

Reviewer:

The paragraph introducing management commitment defines it as "the degree of support, participation, and dedication demonstrated by managers toward organizational objectives and employee well-being." This definition is broad and potentially overlaps with perceived organizational support, top-management support, managerial support, organizational commitment, and change-management commitment. The authors should provide a construct-specific definition that corresponds exactly to the five questionnaire items used in the study. They should also explain whether management commitment refers specifically to commitment to AI implementation, commitment to employee welfare, commitment to innovation, or general managerial commitment. This clarification is necessary because the proposed moderating mechanism depends on what employees believe management is committed to.

The method paragraph reports that "after estimating the standard deviation as 0.428, the required sample size was calculated as 121 using Cochran's formula." This sample-size calculation requires further explanation because Cochran's formula for

estimating a population proportion generally does not use the standard deviation in the form reported. If a formula for estimating a population mean was used, the authors should present the equation, identify the outcome variable on which the standard deviation was based, and justify the selected margin of error. The finite population size of 211 employees should also be incorporated explicitly. More importantly, because the primary analysis is PLS-SEM, the adequacy of the sample should be evaluated through an a priori statistical power analysis based on the maximum number of predictors, anticipated effect size, significance level, and desired power rather than relying only on a general survey-sampling formula.

Table 1 contains a serious reporting ambiguity. The text states that "the data-collection instrument comprised four questionnaires," but the source column attributes only the AI-induced stress questionnaire directly to Tong et al. (2025), leaving the sources of coping strategies, individual creativity, and management commitment unclear. The authors should identify the original source of every scale, report whether translated or adapted versions were used, describe the translation and back-translation procedures, and provide sample items and response anchors. If all constructs were adopted from Tong et al. (2025), this should be stated explicitly. The authors should also report whether permission was obtained to use or adapt the instruments.

The demographic paragraph reports that "84% were men and 16% were women," that 94.4% were married, and that a large proportion had more than 21 years of work experience. These pronounced sample imbalances may influence perceptions of technological threat, coping, and creativity. The authors should discuss the representativeness of the sample relative to the full organizational population and report whether the population itself has a similar demographic composition. Sex, age, education, tenure, and occupational category should be considered as control variables in the structural model, particularly because age and tenure may be associated with AI learning stress and perceived replacement risk. At minimum, robustness analyses should determine whether the primary path coefficients remain stable after relevant demographic controls are included.

The mediation paragraph states that "the mediating role… was examined using the Sobel test" and concludes that coping strategies demonstrated full mediation. The Sobel test is not optimal for PLS-SEM because it assumes normality of the indirect-effect distribution, whereas indirect effects are typically asymmetric. The authors should test mediation through bootstrapping and report the indirect coefficient, bootstrap standard error, *t*-value, *p*-value, and bias-corrected confidence interval. The term "full mediation" should also be used cautiously. A nonsignificant direct effect combined with a significant indirect effect is better described as indirect-only mediation, provided that the theoretical model is correctly specified and alternative mediators or omitted confounders have been considered.

The moderation analysis is underdeveloped. The sentence "management commitment did not moderate the effect of artificial intelligence–induced stress on stress-coping strategies" reports only the interaction coefficient and *t*-statistic. The authors should explain how the interaction term was constructed in SmartPLS, such as through the product-indicator, orthogonalizing, or two-stage approach, and whether the predictor and moderator were standardized. They should report collinearity diagnostics for the interaction model, the change in (R^2), and the interaction effect size (f^2). Although the interaction was nonsignificant, plotting conditional slopes at low, mean, and high levels of management commitment could help demonstrate the form and practical irrelevance of the estimated interaction.

Authors revised the manuscript and uploaded the new document.

1.2. Reviewer 2

Reviewer:

The paragraph discussing the Kolmogorov–Smirnov and Shapiro–Wilk tests concludes that two variables were non-normal and two were normal. This analysis is of limited relevance to PLS-SEM, which does not require multivariate normality in the same manner as covariance-based SEM. Moreover, formal normality tests are highly sensitive to sample size and should be supplemented with skewness, kurtosis, histograms, and examination of influential observations. The authors should explain how the non-normality findings affected their analytical decisions. They should also report the bootstrapping procedure used in SmartPLS, including the number of bootstrap subsamples, confidence-interval method, whether the test was one- or two-tailed, and whether sign changes were permitted.

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Table 4 labels the column as “Number of Dimensions/Items” and reports values of 3 for coping strategies and 2 for AI-induced stress, although these constructs contain 13 and 6 observed items, respectively. This creates confusion about whether reliability and AVE were computed at the dimension level or item level. If higher-order constructs were estimated using three and two first-order dimensions, respectively, the authors must explain the higher-order modeling procedure, such as the repeated-indicator, two-stage, or disjoint two-stage approach. If the statistics were computed using all individual items, the table should report the actual number of items. The dimensional structure must be consistently represented in the methods, measurement model, and structural model.

The paragraph stating that “the discriminant validity of the model was confirmed” solely through the Fornell–Larcker criterion is insufficient according to current PLS-SEM reporting standards. The authors should additionally report the heterotrait–monotrait ratio of correlations and its bootstrap confidence intervals. HTMT values below the selected threshold, commonly 0.85 or 0.90 depending on conceptual similarity, would provide stronger evidence of discriminant validity. Cross-loadings may also be reported as supplementary evidence. This is particularly important because AI-induced stress, coping strategies, management commitment, and creativity are all self-reported psychological constructs and may share substantial method variance.

The paragraph following Table 6 states that “all factor loadings exceeded 0.40” and therefore construct validity was confirmed. A loading threshold of 0.40 is too permissive for established reflective scales, especially when several items fall below the commonly recommended level of 0.708. For example, Q12 has a loading of 0.573, Q11 has a loading of 0.716, and Q6 has a loading of 0.747. The authors should justify retaining low-loading items by examining their effect on composite reliability and AVE, as well as their substantive content validity. They should also distinguish between confirmatory factor analysis in covariance-based SEM and reflective measurement-model assessment in PLS-SEM, because the current terminology may incorrectly imply that a conventional CFA was conducted.

The model-fit section relies heavily on the statement that “the GOF value of 0.441… demonstrated strong overall fit.” The global goodness-of-fit index used here is generally not considered a reliable criterion for validating PLS-SEM models because it does not adequately distinguish valid from invalid model specifications. The authors should replace or supplement this index with contemporary PLS-SEM criteria, including the standardized root mean square residual, exact model-fit measures where appropriate, predictive relevance using Stone–Geisser’s (Q^2), effect sizes (f^2), and out-of-sample predictive assessment such as PLSpredict. The continued use of GOF benchmark values should be theoretically and methodologically justified if retained.

The paragraph interpreting the coefficients of determination states that (R^2=0.37) for coping strategies and (R^2=0.17) for individual creativity “indicat[ed] an acceptable overall fit of the model.” This interpretation is imprecise because (R^2) measures explanatory power rather than model fit. Moreover, an (R^2) of 0.17 indicates that the model explains only 17% of the variance in individual creativity, which should be described as relatively limited rather than uncritically characterized as good. The authors should interpret each (R^2) in the context of prior research and the complexity of behavioral outcomes. Reporting adjusted (R^2), (f^2) effect sizes, and (Q^2) predictive relevance would provide a more informative evaluation of the model’s explanatory and predictive performance.

Authors revised the manuscript and uploaded the new document.

2. Revised

Editor’s decision after revisions: Accepted.
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