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The Effect of Digital Literacy on Perceived Interactivity in Learner–Artificial Intelligence Interaction: The Moderating Role of Digital Stress

Mohamad. Porfarzollah[1], Samira. Pali[2*]

[1] Department of Educational Management, To.C., Islamic Azad University, Tonekabon, Iran
[2] Department of Educational Sciences, To.C., Islamic Azad University, Tonekabon, Iran

*** Corresponding author email address**: samira.pali@iau.ac.ir

Editor	Reviewers
Davood Feiz Affiliation: Professor of Management Department, Semnan University, Iran feiz1353@semnan.ac.ir	**Reviewer 1:** Marco Yamba-Yugsi Unidad Académica de Posgrado, Universidad Católica de Cuenca, Azuay 010101, Ecuador Email: marco.yamba@ucacue.edu.ec **Reviewer 2:** Manijeh Haghighinasab Assistant Professor, Department of Management, Alzahra University, Tehran, Iran Email: haghighinasab@srbiau.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

In the paragraph discussing digital literacy, the manuscript states that “Digital literacy extends beyond basic access to digital devices or the ability to operate software.” The construct is subsequently operationalized with only two dimensions, technical literacy and digital skills. The authors should justify this relatively narrow operationalization because the conceptual definition includes broader elements such as critical evaluation, information literacy, problem-solving, and communication. A clearer explanation is needed regarding whether the adopted scale captures digital literacy comprehensively or only its functional/technical components.

In the Method section, the paragraph stating that “The statistical population consisted of all elementary school teachers employed by the Department of Education in Rudsar during the 2025–2026 academic year” does not report the total population size. Because the sample size was determined using the Krejcie and Morgan table, the exact population size should be reported so readers can verify whether a sample of 234 participants was appropriate and whether the sampling fraction was reasonable.

The structural model section states that "all path coefficients were statistically significant at the .05 level, as the calculated t values for all paths exceeded 1.96." The manuscript should specify the bootstrapping procedure used to derive the t statistics, including the number of bootstrap resamples, whether confidence intervals were bias-corrected, and whether testing was one-tailed or two-tailed. Reporting p values and 95% bootstrap confidence intervals would substantially strengthen the inferential reporting.

In the structural model results, the reported direct path from digital stress to perceived interactivity in Figure 1 is −0.282, but this relationship is not included in Table 5 or explicitly discussed in the findings. The authors should clarify whether this path was hypothesized, statistically significant, and included as a control/direct effect within the moderation model. Omitting a modeled structural relationship from the hypothesis table creates inconsistency between the figure and the reported results.

In the moderation analysis, the manuscript states that "the interaction effect of digital stress and digital literacy on perceived interactivity was statistically significant ($\beta = -0.131$, $t = 2.660$)." The authors should go beyond significance testing and provide a simple slope analysis or interaction plot showing how the relationship between digital literacy and perceived interactivity changes at low, medium, and high levels of digital stress. This would make the moderation effect substantively interpretable rather than merely statistically significant.

The paragraph discussing R^2 states that "38.8% of the variance in this variable was explained by the predictor variables included in the model." This interpretation is acceptable, but the manuscript should clarify which predictors are included in this explained variance—digital literacy, digital stress, and the interaction term. In addition, the authors should interpret the magnitude of R^2 using recognized PLS-SEM conventions and avoid presenting 38.8% as inherently strong without an explicit benchmark or context.

The Q^2 value of 0.237 is interpreted only as evidence that "the model possessed predictive relevance." This interpretation is incomplete. The authors should indicate which procedure was used to calculate Q^2, such as blindfolding, specify the omission distance, and interpret the magnitude rather than only its positivity. If predictive assessment is central to the study, the authors might also consider reporting PLSpredict results to distinguish in-sample predictive relevance from out-of-sample predictive performance.

Authors revised the manuscript and uploaded the new document.

1.2. Reviewer 2

Reviewer:

In the same Method paragraph, the manuscript reports that "234 participants were selected using stratified random sampling," but the stratification criterion is not specified. The authors should identify the strata used, such as gender, school, geographic district, employment status, or another characteristic, and explain how participants were proportionally or disproportionately allocated to each stratum. Without this information, the sampling procedure cannot be replicated.

In the instrument description, the manuscript states that the questionnaire developed by Ren et al. (2025) "comprises three main constructs." The authors should report whether the questionnaire was originally developed in English or another language and describe any translation and cultural adaptation procedures used for the Iranian teacher sample. If translation was required, forward translation, back-translation, expert review, and pilot testing procedures should be documented to establish linguistic and cultural equivalence.

In the paragraph reporting content validity, the manuscript states that "The content validity of the research instruments was evaluated and confirmed through expert review by university faculty members and specialists in the field of management." This statement is insufficiently specific. The number of experts, their areas of expertise, evaluation criteria, and whether quantitative indices such as the content validity ratio or content validity index were calculated should be reported. Simply stating that experts confirmed validity does not provide adequate methodological transparency.

The pilot reliability assessment described in the sentence "the reliability of the questionnaires was assessed using a pilot sample of 15 participants" requires reconsideration. A pilot sample of 15 is very small for stable estimation of Cronbach's

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alpha, especially for multidimensional constructs with numerous items. The authors should either justify this pilot size methodologically or place greater emphasis on the reliability coefficients estimated from the full sample of 234 participants, which would provide substantially more robust evidence of internal consistency.

In the data analysis paragraph, the manuscript states that the Kolmogorov–Smirnov test was applied and that non-normality justified using PLS-SEM. This rationale should be refined. PLS-SEM should not be selected solely because of non-normal data; its use should also be justified based on the study's predictive orientation, model complexity, sample characteristics, and distribution-free estimation properties. The authors should provide a methodological rationale aligned with established PLS-SEM guidelines.

In Table 3, Cronbach's alpha, composite reliability, and AVE are reported for all first-order dimensions, but discriminant validity is not presented. The measurement model evaluation is therefore incomplete. The authors should report an appropriate discriminant validity criterion, preferably the heterotrait–monotrait ratio (HTMT), and demonstrate that construct pairs meet the recommended threshold. Cross-loadings or the Fornell–Larcker criterion may also be presented as supplementary evidence.

In the paragraph following Table 3, the manuscript concludes that "the convergent validity of the study constructs was also confirmed" based on CR and AVE alone. The authors should also report standardized outer loadings for all indicators and indicate whether any items were removed due to insufficient loadings. Because Figure 1 displays several loadings at the dimensional level, the text should clarify whether the model is reflective-reflective, reflective-formative, or another hierarchical component model and explain how higher-order constructs were estimated.

Authors revised the manuscript and uploaded the new document.

2. Revised

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

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