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Testing a Structural Model of Social Media Addiction, Sleep Quality, Academic Burnout, and Psychological Well-Being among High School Students

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the paragraph beginning “One of the most significant changes affecting adolescent development in recent years has been the rapid expansion of digital technologies and social media platforms” (p. 2), the discussion remains relatively general. The authors should provide more recent prevalence estimates of social media addiction among Canadian adolescents specifically. Since the study focuses on Ontario high school students, incorporating national or provincial epidemiological data would strengthen the contextual relevance and justify the importance of the research problem.

The paragraph stating that “Researchers have increasingly conceptualized problematic engagement with social networking platforms as social media addiction” (p. 2) requires deeper theoretical grounding. The manuscript would be strengthened by explicitly discussing behavioral addiction frameworks, including Griffiths’ components model of addiction, and explaining how the Bergen Social Media Addiction Scale operationalizes these theoretical dimensions.

The authors argue that “sleep quality may represent a critical mechanism through which social media addiction influences adolescent psychological outcomes” (p. 3). Although this rationale is plausible, the manuscript does not sufficiently explain why sleep quality was modeled as a mediator rather than a moderator. A theoretical justification for the directional assumptions

embedded in the structural model should be provided, particularly because alternative pathways are equally plausible in cross-sectional designs.

In the paragraph discussing academic burnout, the authors note that "academic burnout is influenced by a wide range of personal, academic, technological, and environmental factors" (p. 3). However, the study excludes potentially important confounding variables such as academic achievement, socioeconomic status, parental monitoring, and school climate. The authors should explain why these factors were omitted and discuss the implications of their exclusion for model validity.

Figure 1 presents the structural model; however, the figure does not display residual variances, error terms, or R^2 values for endogenous variables. Including these parameters would provide a more complete representation of the SEM results and allow readers to better evaluate the explanatory power of the proposed model.

In Table 3, the authors report standardized path coefficients but do not provide confidence intervals for these estimates. Since confidence intervals are particularly informative in SEM analyses, especially when evaluating practical significance, the authors should report 95% confidence intervals alongside each direct effect.

Authors uploaded the revised manuscript.

1.2. Reviewer 2

Reviewer:

The final paragraph of the Introduction claims that "relatively few studies have investigated them simultaneously within an integrated structural framework among high school students" (p. 4). This statement requires stronger evidence. The authors should provide a more systematic synthesis of previous structural equation modeling studies examining similar variables and explicitly identify the specific knowledge gap that distinguishes the current study from prior work.

In the Methods section, the description "several school districts were randomly selected from different regions of Ontario" lacks sufficient methodological transparency (p. 4). The authors should specify the exact number of districts, schools, and classrooms sampled, as well as the selection probabilities and participation rates. Such information is essential for evaluating the representativeness of the sample and the external validity of the findings.

The paragraph reporting that "742 students participated in the research" and that "718 questionnaires were retained for final analysis" (p. 4) requires greater detail regarding data screening procedures. The authors should specify the criteria used to identify multivariate outliers, the number of cases removed for each exclusion reason, and whether removed participants differed systematically from retained participants.

The Measures section provides descriptions of previously reported psychometric properties but does not report reliability and validity evidence obtained from the current sample. For example, the paragraph describing the Bergen Social Media Addiction Scale should include Cronbach's alpha, composite reliability (CR), average variance extracted (AVE), and item-factor loading statistics calculated from the present dataset.

The same limitation applies to the PSQI, MBI-SS, and Ryff Psychological Well-Being Scale. The manuscript repeatedly cites reliability coefficients from previous studies rather than reporting sample-specific psychometric evidence. Given that the measurement model is central to the SEM analysis, reliability and construct validity indices for all latent variables should be presented in a dedicated table.

In the Data Analysis section, the authors indicate that "Maximum likelihood estimation was employed to estimate model parameters" (p. 5). However, there is no discussion of whether multivariate normality assumptions were formally tested. The authors should report Mardia's coefficient, critical ratios, or alternative robust estimation procedures if normality assumptions were violated.

The statement that "missing data were below 2% for all variables and were handled using expectation-maximization procedures" (p. 6) requires further clarification. The authors should indicate whether the missingness mechanism was evaluated (MCAR, MAR, or MNAR) using Little's MCAR test or similar diagnostics before applying expectation-maximization imputation.

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Table 1 presents descriptive statistics and correlations; however, the authors do not report confidence intervals for the correlation coefficients. Providing confidence intervals would improve statistical transparency and allow readers to evaluate the precision of the observed relationships. Furthermore, reporting variance inflation factors (VIFs) would help demonstrate the absence of multicollinearity concerns.

In the CFA results, the authors state that "all factor loadings were statistically significant and ranged from .63 to .88" (p. 7). This information is insufficient for evaluating measurement quality. A complete measurement model table should be added reporting standardized loadings, standard errors, t-values, composite reliability, AVE, and discriminant validity indices for each latent construct.

Authors uploaded the revised manuscript.

2. Revised

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

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