





Predicting Behavioral Problems of Sixth-Grade Elementary Students Based on Digital Media Use and Parent-Child Interaction Quality with the Mediating Role of Emotion Regulation Level

Maasoumeh. Afzooni¹, Reza. Abbasi Bakhtiari^{1*}, Maryam. Afshari¹, Safoura. Narimani¹



¹ Department of Educational Sciences, Mey.C., Islamic Azad University, Meymeh, Iran

* Corresponding author email address: re.abbasi@iau.ir

Editor

Izet Pehlić¹
Full professor for Educational sciences, Islamic pedagogical faculty of the University of Zenica, Bosnia and Herzegovina
izet.pehlic@unze.ba

Reviewers

Reviewer 1: Abolghasem Khoshkanesh¹
Assistant Professor, Counseling Department, Shahid Beheshti University, Tehran, Iran.
Email: akhoshkonesh@sbu.ac.ir
Reviewer 2: Ali Khodaei¹
Department of Psychology, Faculty of Educational Sciences and Psychology, Payam Noor University, Tehran, Iran. Email: alikhodaei@pnu.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

In the Introduction, the sentence “sixth-grade students, as the final stage of this sensitive period, stand at the threshold of transitioning from childhood to adolescence” is conceptually appropriate, but it needs stronger theoretical anchoring. The authors should expand this paragraph by explaining why sixth grade is developmentally distinct in terms of emotion regulation, autonomy, peer influence, academic pressure, and digital independence. This would make the rationale for selecting sixth-grade students more scientifically persuasive.

In the Introduction, the paragraph beginning “Research findings indicate that approximately one-fifth of children and adolescents...” presents several prevalence statistics from different studies, including “10 to 20 percent,” “23 percent,” and “30 percent.” These figures should be contextualized more carefully because they may refer to different populations, age ranges, instruments, and definitions of behavioral problems. The authors should avoid presenting them as directly comparable prevalence estimates unless methodological differences are explained.

In the Introduction, the paragraph beginning “Given the importance of the topic, after explaining the classification and prevalence...” introduces digital media as a major predictor, but the manuscript sometimes uses “digital media,” “electronic

media,” “social media,” and “internet addiction” interchangeably. These constructs are not equivalent. The authors should define the exact predictor as measured in the study and distinguish between general screen exposure, problematic media use, internet addiction, social media use, and digital device duration.

In the Methods section, the paragraph stating that “questionnaires related to intrapersonal variables... were completed by the students themselves, while... observable behaviors... were completed by the parents” is useful, but the authors should discuss informant effects. Parent reports of behavioral problems and parent-child interaction may share method variance, potentially inflating associations between these variables. The authors should acknowledge this and, ideally, justify why teacher reports or multi-informant behavioral measures were not included.

In the instrument description, the section titled “Child Symptom Inventory (CSI-4) (Parent Form)” should be carefully revised because the Findings table reports CBCL-like dimensions such as anxiety-depression, withdrawal-depression, somatic complaints, social problems, thought problems, attention problems, rule-breaking behavior, and aggressive behavior. These subscales correspond more closely to the Child Behavior Checklist than to CSI-4 DSM symptom groups. This mismatch must be resolved before publication.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the Introduction, the sentence “Watching educational media content, having screen exposure of less than one hour per day, and having clear media rules at home are significant protective factors” is important but requires better integration with the present study’s measurement strategy. If the current media questionnaire measures internet, foreign media, and domestic media consumption, it may not directly assess educational content, screen duration, or parental rules. The authors should align the literature review more closely with the operational definition of digital media use in this study.

In the Introduction, the paragraph beginning “On the other hand, the quality of parent-child interaction is recognized as the cornerstone...” would benefit from distinguishing between the CPRS dimensions of conflict, closeness, dependence, and overall positive relationship. The current text discusses parent-child interaction broadly, but the instrument contains conceptually different subdimensions. The authors should explain whether “quality” refers to the total positive relationship score or whether conflict, closeness, and dependency were separately analyzed.

In the Introduction, the sentence “The answer to this important question should be sought within the child and in the construct of emotion regulation” is theoretically useful, but the manuscript should clarify whether emotion regulation is being conceptualized as a mediator, an internal protective factor, or a learned family-socialized process. Since the model includes both parent-child interaction and media use as predictors, the authors should explicitly justify why emotion regulation is modeled as a mediator rather than as a moderator or outcome variable.

In the Introduction, the sentence “In contrast, weakness in emotion dysregulation skills creates a smooth path for the formation and exacerbation of internalizing and externalizing behavioral problems” needs conceptual refinement. “Weakness in emotion dysregulation skills” is linguistically inaccurate; the authors likely mean “weakness in emotion regulation skills” or “emotion dysregulation.” This sentence should be revised for scientific precision and to avoid conceptual redundancy.

In the Methods section, the paragraph stating that the population included “3419 individuals (1449 girls and 1700 boys)” contains a numerical inconsistency because 1449 plus 1700 equals 3149, not 3419. This is a serious reporting error in the sampling frame. The authors must correct the total population, gender distribution, or both, because these numbers affect proportional stratified sampling and the credibility of the sample-size procedure.

In the Methods section, the paragraph reporting “200 girls and 170 boys” as the sampled proportion conflicts with the final sample size of 343 reported elsewhere. The manuscript should clearly explain how many students were initially invited, how many questionnaires were returned, how many were excluded, and how the final sample of 343 was obtained. A participant flow description would substantially improve transparency.

In the Methods section, the sentence “A combined (random stratified-multistage cluster) sampling method was used” conflicts with the later statement in the assumptions section that “the sampling process was conducted using convenience and individual sampling.” These two descriptions are mutually inconsistent. The authors should revise the assumptions section and, if cluster sampling was used, consider whether clustering by school/class required adjustment in the analysis or at least acknowledgment as a design limitation.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.