

Examining the Predictive Role of Computer Game Use, Impulse Control, and Quality of Family Relationships in the Occurrence of High-Risk Behaviors Among Upper Secondary School Students

Ilmaz. Sheibani¹, Ali Mohammad. Rezaei^{2*}

¹ Department of Psychology, Shi.C., Islamic Azad University, Shiraz, Iran

² Department of Psychology, Ars.C., Islamic Azad University, Arsanjan, Iran

* Corresponding author email address: dr.rezaie@iau.ac.ir

E d i t o r	R e v i e w e r s
Seyed Hamid Atashpour ^{id} Associate Professor, Department of Psychology, Isfahan (Khorasgan) Branch, Islamic Azad University, Isfahan, Iran hamidatashpour@gmail.com	Reviewer 1: Hajar Torkan ^{id} Assistant Professor, Department of Psychology, Islamic Azad University, Isfahan Branch (Khorasgan), Isfahan, Iran. h.torkan@khuisf.ac.ir Reviewer 2: Farhad Namjoo ^{id} Department of Psychology and Counseling, KMAN Research Institute, Richmond Hill, Ontario, Canada. Email: farhadnamjoo@kmanresce.ca

1. Round 1

1.1. Reviewer 1

Reviewer:

Relatedly, the paragraph beginning “The results presented in Table 3 show the regression coefficients for predicting high-risk behaviors based on impulsivity” interprets $\beta = .351$ as evidence of the predictive effect of impulsivity. However, because this coefficient comes from a simple regression, β is mathematically equivalent to the bivariate correlation ($r = .351$) and does not demonstrate the unique contribution of impulsivity after controlling for gaming addiction and family functioning. Indeed, using the correlation coefficients reported in Table 2, the simultaneous standardized coefficients would be expected to differ substantially from the bivariate coefficients; impulsivity in particular may have a considerably smaller unique contribution because it correlates .533 with gaming addiction. The authors should therefore avoid describing Tables 3–5 as evidence of independent predictive effects and should base conclusions about unique predictors on the simultaneous model.

In the Abstract, the sentence “Furthermore, excessive computer game use significantly increased the likelihood of being classified at a high level of high-risk behaviors” appears unsupported by the analyses presented. The manuscript reports Pearson correlations and linear regressions involving continuous high-risk behavior scores, but no binary or ordinal outcome, odds ratio, logistic regression, classification threshold, or risk-category analysis is reported. The expression “increased the likelihood”

implies a probabilistic or logistic analysis that was apparently not conducted. This statement should either be removed and replaced with language appropriate for linear regression—such as “higher gaming addiction scores were associated with higher high-risk behavior scores”—or supported by a clearly described logistic regression analysis with the categorization criterion, odds ratios, confidence intervals, and model diagnostics.

In the Methodology paragraph, the sentence “The statistical population consisted of all 15,000 male and female upper secondary school students in District 1 of Shiraz... In the present study, 200 participants were selected... based on... the Krejcie and Morgan sample size table” requires correction. For a population of approximately 15,000, the conventional Krejcie–Morgan table yields a sample substantially larger than 200—approximately 375 participants under its standard assumptions. Therefore, a sample of 200 cannot simply be attributed to that table without further justification. The authors should either provide the actual sample-size calculation, including confidence level, margin of error, anticipated effect size, statistical power, number of predictors, and expected attrition/nonresponse, or revise the statement about the Morgan table. A priori power analysis for multiple regression would be more appropriate and scientifically defensible.

The same Methodology paragraph states that “200 participants were selected through purposive sampling,” yet the study concerns a clearly defined school population and makes inferential claims about students in District 1 of Shiraz. Purposive sampling substantially limits population-level generalizability and may introduce selection bias, particularly if recruitment depended on willingness to complete an online questionnaire. The authors need to explain exactly how schools and students were approached, who distributed the questionnaire, how eligible students were identified, how many students were invited, how many declined or provided incomplete data, and why purposive rather than random, cluster, or stratified sampling was employed. Because grade, sex, school type, and academic field may be related to both gaming and risk behavior, the representativeness of the final sample should also be evaluated.

In the Methodology paragraph, the sentence “The data for the present study were obtained through both library research and questionnaire administration” is methodologically imprecise. Literature review is not a method of empirical data collection for the variables analyzed in this correlational study. The manuscript should distinguish clearly between the literature used to construct the theoretical background and the primary empirical data obtained from participants. Similarly, statements such as “data were collected to confirm or reject hypotheses” should be revised because null-hypothesis significance testing does not literally “confirm” hypotheses. The Methods section should focus on design, setting, participants, sampling, instruments, procedure, ethics, and statistical analysis, while information about literature searches belongs in the introduction only if a formal review methodology was actually used.

In the statistical-analysis paragraph, “At the inferential level, Pearson’s correlation coefficient and regression analysis were used to test the research hypotheses,” the assumptions underlying these analyses are not addressed. For Pearson correlation and multiple linear regression, the authors should examine linearity, approximate normality of residuals, homoscedasticity, independence of errors, influential observations, and multicollinearity. This is particularly important because gaming addiction and impulsivity correlate at $r = .533$, indicating appreciable shared variance. Please report diagnostic indices such as tolerance and VIF, Durbin–Watson if appropriate, residual plots or tests relevant to model assumptions, and procedures for detecting influential cases (e.g., Cook’s distance). The manuscript should also describe treatment of missing data, incomplete questionnaires, outliers, and any screening performed before inferential analysis.

Table 2 contains an incorrect statistical footnote: “ $p \leq 0.01$ $p \leq 0.01^*$.” Both one- and two-asterisk symbols are defined using the same threshold, which is inconsistent with standard table notation. If only coefficients significant at $p < .01$ are marked, one symbol is sufficient; alternatively, the conventional format is typically “ $* p < .05$; $** p < .01$.” Exact p values should preferably be reported where possible. Furthermore, APA formatting should be standardized throughout: use decimal points rather than Persian-style slash notation, italicize statistical symbols (r , p , t , F , R^2), omit leading zeros for correlations and standardized coefficients, and report $p < .001$ rather than $p = .000$.

The manuscript requires a clearer distinction among “family functioning,” “quality of family relationships,” “family cohesion,” and “family adaptability.” Table 1 reports cohesion, adaptability, and a total “family functioning” score; Tables 5 and 6 call the predictor “quality of family relationships”; the title uses the same phrase; and the discussion shifts among these constructs as though they were interchangeable. They are not necessarily synonymous. The authors should select terminology based on what FACES-III actually operationalizes and maintain that terminology throughout the title, abstract, hypotheses,

methods, tables, and interpretation. If a composite FACES score is retained, it should be consistently named and its construct validity established.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the ethics portion of the Methodology paragraph, the sentence “First, parental participation was entirely voluntary” is inconsistent with the stated participants, who were adolescents rather than parents. The authors should clarify whether parental consent and adolescent assent were obtained, whether any participants were 18 years of age and therefore provided their own informed consent, and how consent was implemented for online participation. Because the IHRAS assesses sensitive behaviors including substance use, risky sexual behavior, suicidality, and self-harm among minors, the ethical procedures require considerably more detail. The manuscript should report the institutional ethics approval body and approval code, procedures for confidentiality and data security, whether responses indicating imminent self-harm risk were identifiable or anonymous, and whether participants were provided with counseling/referral information when responding to potentially distressing items.

In the paragraph describing the Iranian Adolescents Risk-Taking Scale, the sentence “The questionnaire... assesses seven major domains... including... suicidal or self-harming tendencies” raises an important measurement issue that should be reflected in the Results. The manuscript analyzes only a total high-risk behavior score, although the seven domains are conceptually heterogeneous and may have very different relationships with gaming, impulsivity, and family functioning. For example, associations with aggression may differ substantially from associations with substance use or suicidal behavior. The authors should justify use of the total score psychometrically and conceptually and, if possible, provide subscale descriptive statistics and supplementary analyses. At minimum, the Discussion should acknowledge that the aggregated outcome prevents conclusions about which specific types of high-risk behavior drive the observed associations.

The paragraph introducing the “Barratt Impulsiveness Scale (BIS-11)” requires bibliographic and psychometric verification. The manuscript attributes the instrument to “Barratt et al. (2004),” whereas the widely cited BIS-11 publication and its conventional factor structure should be checked carefully against the exact version administered. The authors should provide the original source of the specific translated version, the Persian validation reference, translation/adaptation procedure if applicable, scoring range, number of reverse-scored items, and evidence supporting use of the three reported dimensions in Iranian adolescents. More importantly, all instrument sources—including Mohammadkhani, the BIS-11 source, Ekhtiari et al., Olson et al., and Wang and Chang—must appear in the final reference list. At present, several measurement references discussed in the Methods are absent from the supplied bibliography.

The paragraph describing FACES-III states that “Higher scores on each subscale indicate more desirable levels of interaction and adaptability within the family context” and subsequently combines cohesion and adaptability into a “total family functioning score.” This interpretation requires stronger justification. Within Olson’s Circumplex framework, extremely high or low levels of certain dimensions have historically not always been conceptualized as linearly equivalent to better or worse family health, and scoring conventions differ across FACES versions. The authors should cite the scoring manual or validation study supporting the use of a simple 20-item total score and the assumption that higher scores uniformly represent healthier family functioning. If the instrument is intended to yield separate cohesion and adaptability scores, combining them into a single predictor may obscure theoretically meaningful distinctions.

In the paragraph describing the “Wang–Chang Computer Game Addiction Questionnaire (W-C),” the authors report four dimensions—“compulsion and urge to play,” “withdrawal symptoms,” “tolerance,” and “gaming-related problems”—but provide no original psychometric evidence, Persian adaptation information, or validity evidence for the present population. Because gaming-disorder conceptualization has evolved considerably, the manuscript should establish exactly what construct this scale assesses and whether the term “addiction” is appropriate. Please provide the full original citation, evidence for factor structure, scoring procedures, possible cutoffs if any, Persian-language validation, and internal consistency of subscales where

relevant. The authors should also explain why this older measure was selected rather than a measure aligned with contemporary gaming-disorder criteria, particularly because the Introduction cites more recent adolescent-specific measurement work.

Response: Revised and uploaded the manuscript.

2. Revised

Editor's decision after revisions: Accepted.

Editor in Chief's decision: Accepted.