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# Characterizing the Psychological Patterns Associated With Toxic Peer Relationships During Adolescence

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| E d i t o r                                                                                                                                                                      | R e v i e w e r s                                                                                                                                                                                                                                                                                                                                                                                                                                                                         |
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| Ahmad Aman <sup>1</sup><br>Associate Professor, Counseling<br>Department, University of<br>Kurdistan, Sanandaj, Iran<br><a href="mailto:a.amani@uok.ac.ir">a.amani@uok.ac.ir</a> | <b>Reviewer 1:</b> Mohammad Salehi <sup>1</sup><br>Associate Professor, Department of Educational Management, Sari Branch, Islamic<br>Azad University, Sari, Iran. Email: <a href="mailto:drsalehi@iausari.ac.ir">drsalehi@iausari.ac.ir</a><br><b>Reviewer 2:</b> Kamdin Parsakia <sup>1</sup><br>Department of Psychology and Counseling, KMAN Research Institute, Richmond<br>Hill, Ontario, Canada. Email: <a href="mailto:kamdinarsakia@kmanresce.ca">kamdinarsakia@kmanresce.ca</a> |

## 1. Round 1

### 1.1. Reviewer 1

Reviewer:

In the Study Design and Participants paragraph, the authors state that adolescents were recruited “from public and private secondary schools located in several urban and suburban areas of France through a multistage cluster sampling procedure.” This description is too general for reproducibility. The manuscript should report the number of schools approached and participating, the geographical regions or départements represented, the number of classrooms sampled, the response rate at the school and student levels, and whether sampling probabilities differed across schools or grades. If the clustered sampling design was not incorporated into the statistical analyses, the authors should discuss the potential underestimation of standard errors arising from students being nested within classrooms and schools.

The ethical reporting in the Study Design and Participants section needs to be made more specific. The manuscript currently states that “ethical principles concerning research with minors, confidentiality, informed consent, and protection of participants from psychological harm were observed,” but this is not sufficient for a journal submission. The authors should identify the approving ethics committee or institutional review board, provide the approval number and date, describe the parental consent and adolescent assent procedures, and state whether participants were given referral information or access to counseling if questionnaire items concerning depression, distress, victimization, or potentially self-harm-related vulnerability caused discomfort.

The interpretation of Figure 1 would benefit from reporting the actual standardized indicator means used to generate the profile plot. The text states that the High-Toxicity profile showed “substantially elevated peer victimization, relational aggression, friendship conflict, and alienation accompanied by pronounced deficits in peer trust and communication,” but the numerical z-scores are not presented in a table. Because the scientific interpretation of latent profiles depends on the magnitude and relative position of each indicator, the authors should provide a supplementary table containing profile-specific standardized means and standard errors or confidence intervals.

In Table 3, the manuscript reports conventional F tests across latent profiles, for example “ $F(3, 482) = 116.28, p < .001, \eta^2 = .42$ ” for depression. This analysis may not adequately account for uncertainty in assigning individuals to latent profiles. The Methods state that “appropriate auxiliary-variable procedures” were used, yet the Findings present ordinary ANOVA-style statistics. The authors should specify whether BCH, DU3STEP, DCAT, or another distal-outcome method was used in Mplus. If participants were simply assigned to their most likely class and then compared using ANOVA, the estimates may be biased because classification error is ignored. The statistical procedure and reported test statistics must be made internally consistent.

The supplementary analysis states that “the overall multivariate effect of latent profile membership remained significant after adjustment for age and gender, Wilks’  $\Lambda = .49, F(15, 1319.62) = 25.84, p < .001$ .” The authors should explain precisely how age and gender were incorporated into the analysis and whether they were treated as covariates predicting distal outcomes, predictors of profile membership, or both. In latent profile research, covariates can alter class formation if included during enumeration, whereas three-step approaches preserve the latent class solution. The manuscript should clarify which approach was used and justify it.

The demographic comparison by gender requires more complete reporting. The Findings state that “girls [were] somewhat more frequently represented in the Alienated-Victimized profile and boys somewhat more frequently represented in the Conflict-Dominant profile,” but no frequencies, percentages, chi-square statistic, effect size, or confidence intervals are provided. Because gender-related peer experiences are discussed later in relation to identity and trauma, these descriptive claims should be supported numerically. The authors should report the profile-by-gender contingency table and an effect size such as Cramér’s V, while avoiding overinterpretation if the association is small.

In the Discussion, the sentence “The strongest psychological difficulties occurred when multiple adverse dimensions converged and positive relational qualities were simultaneously absent” is a persuasive interpretation, but the cross-sectional design does not establish whether these relational configurations caused the psychological difficulties. The discussion should consistently distinguish association from causation. Several passages currently use language such as “peer adversity may contribute to,” which is appropriately cautious, but other statements imply developmental effects more strongly. The authors should revise causal wording throughout and explicitly consider reverse or reciprocal pathways, such as depression or emotion dysregulation increasing perceived rejection, interpersonal conflict, or vulnerability to victimization.

Authors uploaded the revised manuscript.

## 1.2. Reviewer 2

Reviewer:

In the Data Collection Tools section, the manuscript states that the Peer Experiences Questionnaire was used to assess toxic peer relationship experiences, yet it is not fully clear which subscales or items were incorporated into the latent profile model. The authors should explicitly report the number of items used for peer victimization and relational aggression, the scoring range of each indicator, whether overt and relational forms were combined, and whether aggression represented perpetration, victimization, or both. This clarification is essential because “relational aggression” may reflect the adolescent’s own aggressive behavior, whereas “peer victimization” reflects exposure to others’ aggression, and these roles have distinct developmental meanings.

The measurement description should address the linguistic and cultural adaptation of all instruments for the French adolescent sample. For example, in the Data Collection Tools section the manuscript introduces the Friendship Qualities Scale, the Inventory of Parent and Peer Attachment, the DERS-SF, the Rosenberg Self-Esteem Scale, and the DASS-21, but it does

not state whether validated French-language versions were used. The authors should identify the specific French adaptations, describe any translation/back-translation procedure if validated versions were unavailable, and ideally report evidence of factorial validity in the present sample. Reliability coefficients alone do not demonstrate that the intended constructs were measured equivalently in this population.

The manuscript reports that “internal consistency was reassessed for the present sample,” and Table 1 provides Cronbach’s alpha and McDonald’s omega coefficients. This is appropriate, but the psychometric evaluation should be expanded because the main conclusions depend heavily on the validity of the profile indicators. The authors should consider reporting confirmatory factor analyses or at minimum factor-structure evidence for the principal scales, particularly the measures contributing directly to latent profile estimation. If composite scores were used without verifying dimensionality, the authors should justify this decision and explain whether item-level or scale-level indicators were selected to avoid introducing measurement error into the profile model.

In the Data Analysis section, the statement that “limited item-level missingness was handled using appropriate model-based estimation procedures” is too vague. The manuscript should specify the exact missing-data method used, such as full-information maximum likelihood, robust maximum likelihood, multiple imputation, or another approach, and should report the proportion and pattern of missingness. The authors should also indicate whether Little’s MCAR test or other diagnostics were conducted and whether missingness was associated with demographic variables or psychological distress. Because adolescents experiencing severe peer adversity may be more likely to omit sensitive items, the missing-data mechanism has substantive relevance.

The profile-enumeration procedure requires greater methodological detail. In the Data Analysis section, the authors indicate that progressively increasing numbers of profiles were estimated using AIC, BIC, SSA-BIC, entropy, LMR-LRT, and BLRT. However, the manuscript should also report the estimator, the number of random starts, the number of final-stage optimizations, convergence criteria, whether variances were constrained equal or allowed to vary across profiles, and whether covariances among indicators were fixed to zero. These decisions can materially alter the number and nature of identified profiles, and they are necessary for other researchers to reproduce the analysis.

Table 2 supports the four-profile solution, but the justification requires a more nuanced interpretation of the fit statistics. The manuscript states that “the four-profile solution yielded the highest entropy value” and that the five-profile LMR-LRT was nonsignificant. However, the AIC, BIC, and SSA-BIC continued to decrease in the five- and six-profile solutions, while the BLRT remained significant. The authors should explicitly acknowledge this statistical ambiguity and explain why parsimony, class size, interpretability, and LMR-LRT were weighted more heavily than continued reductions in information criteria. Providing a supplementary plot of BIC/SSA-BIC across models or reporting the percentage decrease between successive solutions would make the selection rationale more transparent.

In Table 2, the smallest class in the five-profile model contained 31 adolescents and the six-profile model contained 18 adolescents, and the text concludes that these classes were “not substantively distinct from neighboring classes.” This claim should be supported empirically. The authors should briefly describe how these additional classes differed from those in the four-profile solution, perhaps in supplementary material, rather than relying solely on a narrative assertion. Demonstrating that the additional classes represented minor severity splits rather than theoretically meaningful configurations would strengthen the case for retaining four profiles.

The manuscript states that the four retained classes were labeled “Secure/Low-Toxicity, Conflict-Dominant, Alienated-Victimized, and High-Toxicity.” These labels are intuitively understandable, but some may imply stronger substantive conclusions than the observed indicators justify. In particular, “Secure/Low-Toxicity” may be interpreted as an attachment classification, although attachment security was not directly modeled, and “High-Toxicity” could be perceived as a value-laden label. The authors should consider labels that more directly reflect the standardized indicator patterns, such as “Low-Adversity/High-Support” and “High-Adversity/Low-Support,” or explicitly justify the current terminology.

Authors uploaded the revised manuscript.

## 2. Revised

Editor's decision after revisions: Accepted.

Editor in Chief's decision: Accepted.