

Parental Phubbing and Adolescent Loneliness: A Longitudinal Cross-Lagged Panel Model of Emotional Availability, Parent–Child Attachment, and Problematic Social Media Use

Angela. Villanueva¹, Samantha. Clarke^{2*}, Elena. Stoyanova³

¹ Department of Clinical Psychology, University of the Philippines, Quezon City, Philippines

² Department of Behavioral Psychology, University of Lethbridge, Lethbridge, Canada

³ Department of Psychology, Sofia University, Sofia, Bulgaria

* Corresponding author email address: samantha.clarke@uleth.ca

Editor

Sergii Boltivets^{id}
Chief Researcher of the Department of Scientific Support of Social Formation of Youth. Mykhailo Drahomanov University, Ukraine
sboltivets@ukr.net

Reviewers

Reviewer 1: Abotaleb Saadati Shamir^{id}
Assistant Professor, Department of Educational Sciences, University of Science and Research, Tehran, Iran. Email: psychology@iau.ac.ir
Reviewer 2: Abotaleb Saadati Shamir^{id}
Assistant Professor, Department of Educational Sciences, University of Science and Research, Tehran, Iran. Email: psychology@iau.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

In the Introduction paragraph beginning with “Parental phubbing is a specific manifestation of technofence,” the manuscript states that repeated phubbing “may communicate that the adolescent is less important than the parent’s digital activity.” This is theoretically plausible, but the proposed psychological meaning should be anchored more explicitly in a coherent theoretical framework. At present, attachment theory, emotional availability, mattering, social learning, and compensatory internet-use perspectives are all invoked, but they are not integrated into a unified explanatory model. The authors should identify one overarching framework or clearly explain how the frameworks complement one another. For example, parental phubbing could be conceptualized as a disruption of responsive caregiving that weakens emotional availability and attachment security, while problematic social media use represents a compensatory behavioral response to unmet belonging needs. A concise conceptual synthesis would make the hypothesized pathways less descriptive and more theoretically derived.

The paragraph introducing loneliness states that “Adolescent loneliness refers to the subjective experience that one’s social and emotional relationships are insufficient in quality, closeness, or availability.” The manuscript should distinguish emotional loneliness from social loneliness and explain which dimension is most relevant to parental phubbing. The current measure appears to produce a global loneliness score, yet the hypothesized mechanisms primarily concern emotional disconnection from a parent. If the selected loneliness instrument does not differentiate subtypes, this limitation should be acknowledged. The authors should also clarify whether loneliness was conceptualized as a relatively stable interpersonal disposition, a time-varying emotional state, or both. This distinction is particularly important in a cross-lagged model because autoregressive paths and cross-lagged paths may reflect different combinations of trait stability and occasion-specific change.

In the final paragraphs of the Introduction, the authors correctly identify the predominance of cross-sectional evidence, but the manuscript should formulate explicit directional hypotheses before stating the study aim. The sentence “The direction of influence is therefore a central unresolved issue” provides a strong transition, yet the reader is not told whether the authors expected a fully reciprocal relationship or an asymmetrical parent-driven effect. Separate hypotheses should be stated for the direct pathway from parental phubbing to later loneliness, the reverse pathway from loneliness to later parental phubbing, and the three indirect pathways through emotional availability, attachment, and problematic social media use. The hypothesized relative strength of the relational versus behavioral mediators should also be specified if this comparison was theoretically anticipated rather than identified post hoc.

In the Study Design and Participants section, the sentence “Participants were recruited from secondary schools located in urban and suburban communities in Ontario, British Columbia, Alberta, and Quebec to increase the geographical and sociodemographic diversity of the sample” requires substantially more procedural detail. The manuscript should report the number of school boards approached, the number that approved participation, the number of schools invited, the number of schools enrolled, and the number of classrooms sampled. It should also state whether sampling probabilities differed by province or school size and whether the achieved sample reflected the provincial distribution of Canadian adolescents. Without this information, the claim of geographical and sociodemographic diversity cannot be adequately evaluated, and the representativeness of the sample remains uncertain.

The manuscript reports that “The final analytic sample consisted of exactly 600 adolescents,” but no a priori power analysis is presented. The authors should explain how the required sample size was determined for a five-variable, three-wave cross-lagged panel model with indirect effects and multigroup comparisons. A Monte Carlo simulation would be preferable because power in structural equation modeling depends on the number of indicators, expected path coefficients, reliability, attrition, and missing-data mechanism. At minimum, the manuscript should identify the smallest standardized effect the study was designed to detect, the assumed retention rate, the target statistical power, and the alpha level. The term “final analytic sample” is also potentially misleading because all 600 participants were enrolled at baseline but only 524 completed Wave 3; the manuscript should distinguish the baseline cohort from the number of complete longitudinal cases.

The timing of the three assessments requires clearer justification. The Methods section states that data were collected at “three measurement occasions separated by six-month intervals,” but it does not explain why six months was selected as the theoretically appropriate lag. Cross-lagged estimates are sensitive to measurement intervals because an interval that is too short may fail to capture meaningful relational change, whereas an interval that is too long may obscure transient processes or combine multiple causal cycles. The authors should justify the lag using developmental or empirical evidence and report the actual mean and range of days between assessments. They should also clarify whether the intervals were approximately equal for all participants and whether deviations in assessment timing were modeled or ignored.

The missing-data analysis should be revised. The Findings section reports that “Little’s test of missing completely at random was also nonsignificant” and uses this result to support full-information maximum likelihood. A nonsignificant Little’s test does not prove that data are missing completely at random, particularly when statistical power may be limited for complex missingness patterns. Full-information maximum likelihood requires the less restrictive missing-at-random assumption, conditional on variables included in the model. The authors should report predictors of attrition, include auxiliary variables associated with missingness where possible, and compare baseline characteristics using effect sizes rather than relying only on nonsignificant probability values. The manuscript should also provide the number of participants contributing data at each combination of waves and report whether item-level and wave-level missingness were handled differently.

Table 1 reports a gradual increase in parental phubbing, loneliness, and problematic social media use and decreases in emotional availability and attachment, yet no formal test of mean-level change is presented. The narrative states that the changes were “modest” and “consistent,” but descriptive mean differences alone do not establish statistically or practically meaningful longitudinal change. The authors should test latent mean differences or use an appropriate repeated-measures model while accounting for missing data. Effect sizes and confidence intervals should be reported for changes between Waves 1 and 2, Waves 2 and 3, and Waves 1 and 3. If the study was not designed to test mean trajectories, the narrative should avoid implying that a systematic developmental trend was demonstrated.

Response: Revised and uploaded the new document.

1.2. Reviewer 2

Reviewer:

The participant eligibility criterion requiring adolescents to be “using at least one social media platform” may restrict the variability of problematic social media use and exclude an informative comparison group. The authors should explain why nonusers were excluded, particularly because the study aims to examine whether parental phubbing predicts problematic social media behavior. Excluding adolescents who do not use social media may produce selection bias and limit generalizability to the full adolescent population. The manuscript should report how many otherwise eligible adolescents were excluded for nonuse and discuss whether the findings apply only to active social media users. If the problematic-use instrument assumes prior social media exposure, this methodological necessity should be stated explicitly.

The Data Collection Tools section provides generic descriptions of the instruments but does not identify the exact scale names, original authors, number of items, response anchors, scoring procedures, or previously reported psychometric properties. For example, the paragraph beginning “Parental phubbing was assessed using an adolescent-report measure” should specify whether the Parental Phubbing Scale, the Parental Phubbing Scale–Mother and Father Form, or another instrument was used. Similar clarification is needed for loneliness, emotional availability, attachment, and problematic social media use. A manuscript cannot be replicated from phrases such as “a standardized self-report loneliness scale” or “a brief symptom-based measure.” The final version should report precise instrument details and justify the selection of each measure for Canadian adolescents.

The manuscript states that adolescents reported on “the parent or caregiver with whom they had the greatest amount of daily contact” and were instructed to select the same reference parent across waves. The authors should report whether participants consistently referred to the same caregiver at Waves 2 and 3 and how cases involving parental separation, changes in custody, caregiver loss, or changes in living arrangements were handled. Because 69.0% reported on mothers and 27.7% on fathers, reference-parent type may confound estimates if maternal and paternal phubbing have different meanings or frequencies. The analytic model should either control for caregiver type, test measurement and structural invariance across maternal and paternal reference groups, or provide a sensitivity analysis demonstrating that the main conclusions do not depend on the selected parent.

The bilingual administration procedure needs stronger documentation. The Methods section states that “All measures were administered in English or French according to the participant’s preferred language” and that translation and back-translation were used when validated versions were unavailable. The manuscript should identify which instruments already had validated French versions and which were translated specifically for this study. It should report the number of participants completing each language version and test measurement invariance across English- and French-language respondents. Without language invariance, observed differences or longitudinal paths may partly reflect translation-related differences in item interpretation. The authors should also clarify whether cognitive interviews included adolescents from both language groups and diverse cultural backgrounds.

The statistical model should be described with greater precision regarding the level of analysis and parameterization. The manuscript repeatedly refers to a “cross-lagged panel model,” but it is unclear whether the analysis used observed composite scores, latent variables with item indicators, or parcel-based latent constructs in the primary model. The Findings section later indicates that supplementary latent-variable models were conducted, implying that the primary model may have used observed

scale totals. If so, the manuscript should acknowledge that the conventional cross-lagged panel model conflates stable between-person differences with within-person change. Given the study's substantive claims about temporal influence, the authors should consider a random-intercept cross-lagged panel model or explain why such a model was not estimable or appropriate. Causal language should be moderated if stable between-family confounding was not separated from within-family processes.

The longitudinal measurement-invariance analysis reported in Table 2 requires additional methodological detail. The manuscript states that configural, metric, and scalar invariance were supported, but it does not explain whether residual invariance was tested, whether correlated residuals for identical items across waves were included, or whether ordinal estimators were considered for Likert-type indicators. Correlating the residuals of identical items across time is usually warranted because repeated item-specific variance may otherwise inflate longitudinal factor associations. The authors should state which parameters were constrained, the criteria used to judge invariance, and whether any modifications were data driven. If the structural model relied on observed total scores, the purpose and connection of the latent measurement-invariance tests to the primary analysis should also be clarified.

Response: Revised and uploaded the new document.

2. Revised

Editor's decision: Accepted.

Editor in Chief's decision: Accepted.