

Child–Parent Relationship Therapy for Phubbing Symptoms in Primary School Children: A Quasi-Experimental Controlled Study

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**Child–Parent Relationship Therapy for Phubbing Symptoms in Primary School Children:
A Quasi-Experimental Controlled Study**

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Abstract

Background: Phubbing (phone snubbing) occurs when attention to a smartphone displaces attention to someone who is physically present. Among school-age children, family-based intervention may be relevant because patterns of technology use develop within everyday caregiver relationships. **Objective:** This study examined whether a phubbing-adapted Child–Parent Relationship Therapy (CPRT) program was associated with lower phubbing scores in primary school children. **Methods:** The quasi-experimental pretest–posttest study included 30 children aged 10–12 years with elevated phubbing screening scores. Children entered an intervention group ($n = 15$) or a waitlist control group ($n = 15$). Mothers in the intervention group attended 10 weekly CPRT sessions of approximately 2 hours each. Children completed the Persian Generic Scale of Phubbing at baseline and posttest. Separate ANCOVA models compared posttest scores after adjustment for the corresponding baseline score, and Holm correction was applied across the total score and four subscales. **Results:** Mean total scores decreased by 7.20 points in the intervention group and increased by 2.40 points in the control group. Adjusted posttest means were 42.13 and 51.73, respectively, $F(1, 27) = 29.73$, Holm-adjusted $p < .001$, partial $\eta^2 = .524$. Adjusted differences also favored the intervention for nomophobia, interpersonal conflict, self-isolation, and problem acknowledgement (Holm-adjusted $ps \leq .006$; partial $\eta^2 = .263$ –.423). Sensitivity analyses produced the same overall pattern. **Conclusions:** Participation in the program was associated with lower short-term child-reported phubbing scores. Larger randomized studies with prespecified screening rules, independent fidelity assessment, and follow-up are needed before drawing causal or long-term conclusions.

Keywords: child–parent relationship therapy; children; filial therapy; phubbing; problematic smartphone use; quasi-experimental study

Child–Parent Relationship Therapy for Phubbing Symptoms in Primary School Children: A Quasi-Experimental Controlled Study

Smartphones have become embedded in everyday communication, entertainment, education, and family coordination. Their portability and constant connectivity make them useful, but those same features allow digital activity to enter almost every face-to-face encounter. In homes, schools, and public settings, attention can therefore move repeatedly between a physically present person and a device. When attention to the device displaces attention to the person, the behavior is commonly described as phubbing, a term formed from “phone” and “snubbing” (Chotpitayasunondh & Douglas, 2016). Phubbing is not defined simply by the amount of time spent on a phone. Its defining feature is relational: a smartphone is attended to during an ongoing social exchange in a manner that communicates neglect, interruption, or disengagement. This distinction is important because an identical period of phone use may be harmless when a person is alone but disruptive when it occurs during a conversation, shared activity, or emotionally important interaction.

Context shapes how phubbing is interpreted. A quick glance at a phone may be reasonable in one setting and rude in another, but persistent checking during conversation can gradually become normal, particularly when the other person responds in kind.

Chotpitayasunondh and Douglas (2016) described this reciprocal process as one route by which phubbing becomes part of ordinary social behavior. Nazir and Pişkin (2016) likewise noted the tension between digital connection and diminished contact with the person who is present.

Phubbing is therefore more than an isolated choice by one individual. It changes the tone of an encounter, influences what the other person expects, and can prompt a matching retreat into

device use. Repeated often enough, a momentary interruption may develop into a familiar pattern that displaces conversation, shared play, and mutual attention.

Research has generally treated phubbing as a cluster of related experiences rather than a single habit. Karadağ et al. (2015) connected the behavior with several forms of technology involvement, including mobile-phone use, internet use, social media, messaging, and gaming. These associations do not make phubbing synonymous with any one digital addiction. Instead, several reinforcing routines may converge when a person turns away from an interaction and toward a phone. The Generic Scale of Phubbing (GSP) captures this breadth through four domains: nomophobia, interpersonal conflict, self-isolation, and problem acknowledgement (Chotpitayasunondh & Douglas, 2018b). Together, the domains address discomfort with disconnection, relationship friction, withdrawal from direct interaction, and recognition that phone use may be disruptive. Reporting both the total score and the domain scores can show whether change is broad or concentrated in one aspect of the construct.

The literature reviewed in the thesis identifies multiple antecedents of phubbing. Internet involvement, fear of missing out, social-media use, and habitual smartphone checking may increase the likelihood that attention is redirected toward a device (Blanca & Bendayan, 2018; Karadağ et al., 2015). These antecedents are especially relevant because smartphones provide rapid and intermittent rewards: a message, notification, social response, or new item of information can appear at any time. The result may be a repeated attentional orientation toward the device even when no urgent need is present. At the same time, personality, age, gender, social expectations, and sensitivity to rejection may influence how often a person phubs or how strongly the behavior is perceived by others. The available evidence therefore argues against a single-cause explanation. Phubbing can emerge from the interaction of device affordances,

learned routines, emotional needs, perceived social norms, and the quality of the immediate relationship.

Its consequences are most apparent in the interaction itself. Studies reviewed in the thesis found that smartphone presence or use can diminish enjoyment, perceived closeness, empathic involvement, and the overall quality of face-to-face exchange (Chotpitayasunondh & Douglas, 2018a; Dwyer et al., 2018). Phone snubbing may also convey exclusion. David and Roberts (2017) linked that experience with both social exclusion and stronger attachment to social media, raising the possibility of a feedback loop in which an excluded person seeks connection through the same digital environment that disrupted the encounter. For intervention, this matters because taking away the device may leave the relational need untouched. Lasting change may also depend on restoring attention, belonging, and reliable opportunities for direct engagement.

Children in late primary school occupy a particularly important developmental position. They are gaining greater independence, expanding their peer networks, and acquiring personal access to mobile devices, yet they continue to depend heavily on caregivers for emotional regulation, behavioral structure, and models of social conduct. Device-use habits established during this period may become intertwined with routines for managing boredom, discomfort, conflict, and connection. The interpersonal consequences may also be substantial because communication skills are still developing. A child who repeatedly shifts attention from a conversation to a smartphone loses opportunities to interpret facial expression, maintain eye contact, tolerate pauses, negotiate disagreement, and experience reciprocal attention. For that reason, elevated phubbing scores in children should not be reduced to a problem of screen duration alone. They may reflect a wider pattern involving emotion regulation, family routines, social learning, and the perceived availability of meaningful interaction.

Parents are central to this developmental environment in at least three ways. First, they establish rules and expectations concerning when and where phones may be used. Second, they model the behavior that children observe. Third, they provide, or fail to provide, alternative forms of attention and connection. Research included in the thesis shows that perceived parental phubbing is associated with later problematic smartphone use or mobile-phone dependency in adolescents (Geng et al., 2021; Liu et al., 2019). Xie et al. (2019) further reported that parent–child attachment helps explain the association between parents’ phubbing and adolescents’ mobile-phone addiction. Zhao et al. (2022) identified boredom proneness as another pathway and refusal self-efficacy as a protective factor. Although these studies focus mainly on parental phubbing and adolescent outcomes, together they support a relational and social-learning account: children’s device behavior develops within a family system in which rules, examples, emotional availability, and opportunities for shared activity matter.

A family-level perspective also avoids placing the entire burden of change on the child. Rules directed only at the child may appear inconsistent when adults continue to use their own phones during meals, conversations, or shared activities. Inconsistency can shift a practical concern into a power struggle, particularly if restriction is experienced as punishment rather than as a shared effort to protect family interaction. Technology interference has been associated with strain in parenting and coparenting contexts (McDaniel & Coyne, 2016), while qualitative work reviewed in the thesis indicates that parents may view social media as a threat to cohesion and parental authority (Lewis et al., 2015). These findings do not imply that all family technology use is harmful. Rather, they show why expectations are more likely to be credible when they are reciprocal, explained, and embedded in a responsive relationship. An intervention for child

phubbing may therefore benefit from teaching adults to combine clear limits with warmth, consistency, and visible changes in their own device behavior.

Emotional processes provide an additional rationale for a relational intervention. Negative affect during interaction can intensify conflict, whereas responsive and regulated exchanges can support closeness and cooperation. Research on interpersonal affect has shown that emotional exchange is closely connected with relationship functioning (Levenson & Gottman, 1983) and that the meaning and consequences of negative emotion depend on how it is expressed and managed (Sanford & Rowatt, 2004). Applied to phubbing, repeated criticism, shaming, or abrupt confiscation may increase defensiveness without teaching a child how to manage urges, boredom, or frustration. By contrast, acknowledgement of the child's feeling, calm limit-setting, and provision of an acceptable alternative may preserve the relationship while changing the behavior. This sequence is consistent with the therapeutic skills emphasized in filial approaches and offers a plausible route from improved interaction to reduced phone-related conflict.

Play is an especially appropriate medium for younger children because it allows emotional and interpersonal learning to occur through action rather than through abstract instruction alone. Samuelsson and Johansson (2006) described play and learning as closely integrated in early educational practice. Shared play creates repeated opportunities for turn taking, imaginative problem-solving, emotion recognition, sustained attention, and flexible response to another person. It also offers an offline activity with immediate relational reward. Within the literature reviewed in the thesis, child-centered and dyadic play interventions were associated with improvement in externalizing behavior, interpersonal understanding, or academic functioning among children facing different forms of risk (Blanco et al., 2019; Karcher & Lewis,

2002). These outcomes do not establish that play therapy directly reduces phubbing, but they support the broader proposition that structured relational play can influence behaviors maintained within parent–child interaction.

Filial therapy extends child-centered play principles by training the caregiver to conduct special play sessions under professional guidance. The parent is not positioned merely as an observer or rule enforcer; the parent becomes the primary agent of therapeutic interaction. In the approach described by Guerney and Gavigan (1981), acceptance, focused attention, empathic responding, and appropriate structure are combined so that the child experiences both emotional safety and behavioral limits. This combination is relevant to phubbing because the target behavior contains both a relational component and a self-regulatory component. Focused play may increase the child’s experience of being noticed and valued, while reflective communication may reduce escalation. Choice-giving can support autonomy, and consistent limits can establish boundaries around unsafe, disrespectful, or interaction-disrupting phone use. The intervention is therefore not based on the assumption that play competes mechanically with a smartphone; it uses play to reshape the relational conditions in which device habits occur.

The thesis literature also provides culturally relevant support for involving mothers in filial interventions. Soltani and Farhadi (2021) reported that filial therapy delivered to mothers was associated with reductions in computer-game addiction symptoms and aggression among Iranian preschool children. Although gaming and phubbing are not equivalent, both can involve persistent engagement with digital activity, conflict over use, and difficulty shifting attention to offline relationships. The relevance of that study lies in its demonstration that a parent-mediated play approach can be implemented in an Iranian context and may influence technology-related behavior as well as broader emotional or behavioral outcomes. It also suggests that the

intervention can be framed as skill development for caregivers rather than as treatment imposed directly on a child. However, transferring this rationale to phubbing requires empirical testing because the social interruption represented by phubbing differs from solitary or immersive gaming.

A phubbing-adapted Child–Parent Relationship Therapy (CPRT) program brings these ideas together in a practical format. Scheduled child-led play protects time for undivided attention. Reflective listening helps the parent address the feeling beneath a dispute, while structured choices and calm limits reduce avoidable power struggles. Family phone agreements make expectations reciprocal, and parent modeling keeps adult behavior consistent with the rules set for children. Home practice then moves these skills into ordinary family routines. The components are theoretically distinct, but they were delivered as one package. Because the study neither separated them nor measured the proposed pathways, the model offers an interpretive framework rather than evidence of mediation.

The two bodies of research rarely meet. Phubbing studies have largely described correlates, interpersonal consequences, or pathways linking parental behavior with adolescent smartphone outcomes. Play-therapy studies, by contrast, have usually focused on externalizing behavior, emotional problems, parent–child interaction, or educational outcomes. Little work has asked whether a structured relational program might alter children’s own phubbing scores. Correlational findings can identify plausible risks, but they cannot show what happens when family interaction is deliberately changed. A controlled pretest–posttest study provides an initial test of that question, although a small nonrandomized design cannot establish causation.

The present study therefore evaluated a 10-session, phubbing-adapted CPRT program delivered to mothers of primary school children with elevated phubbing screening scores. The

primary question was whether children in the intervention condition would have lower posttest total GSP scores than children in a waitlist control condition after adjustment for baseline.

Secondary questions concerned the four GSP domains: nomophobia, interpersonal conflict, self-isolation, and problem acknowledgement. It was expected that adjusted posttest scores would favor the intervention group across the total score and subscales. Because the allocation record did not document a reproducible random process, the study was reported as quasi-experimental, and all interpretations were framed as associations rather than definitive treatment effects. This qualification also reflects the broader function of hedging in academic discourse, through which writers calibrate their degree of commitment to a claim in relation to the available evidence (Farashaiyan et al., 2025).

Methods

Design and Setting

The study used a quasi-experimental pretest–posttest design with a waitlist control group. Participants were recruited through the Hedayat boys’ and girls’ primary schools in District 2 of Hamedan, Iran. The surviving records did not document a reproducible random sequence or allocation-concealment procedure, so the design is not described as randomized. The Research Ethics Committee of Bu-Ali Sina University approved the study (IR.BASU.REC.1402.033).

Each participant was a child–mother dyad. Mothers received the intervention, and children reported the outcome. The design therefore examined whether a caregiver-focused program and its accompanying family routines were associated with changes in children’s self-reported phubbing. Because the comparison group was waitlisted rather than given an attention-matched program, the study accounts for repeated measurement and time but cannot separate the

specific CPRT content from facilitator attention, peer support, expectancy, or the structure of weekly meetings.

Children completed the GSP before the first session and again after the 10-session program. During that interval, waitlisted families continued their usual routines and were offered the program only after posttest data had been collected. This arrangement provided a concurrent comparison while allowing every enrolled family eventual access to training. The records do not show that participants, the intervention provider, or those administering the outcome were masked. Children also knew whether their mothers had attended the sessions, so expectancy and response effects remain possible.

This report draws on the thesis, the final statistical manuscript, and the retained spreadsheet. When those historical materials did not preserve a procedure, the manuscript states the uncertainty instead of filling the gap with a plausible reconstruction. It therefore makes no unsupported claim of random allocation, allocation concealment, diagnostic selection, or assessor masking. The study is presented as an exploratory controlled evaluation of short-term group differences and as groundwork for a future randomized trial.

Participants and Recruitment

Students in Grades 4–6 completed the Persian Generic Scale of Phubbing (GSP) as a school-based screening measure. Of 52 students screened, 30 children aged 10–12 years met the eligibility requirements, assented to participate, and had a mother who was willing to join the program. Convenience sampling yielded 17 girls and 13 boys. Fifteen children entered the intervention group and 15 entered the waitlist group. Because the records contain no verifiable randomization procedure, the manuscript reports allocation without labeling it random or software-generated. Waitlisted families received the program after posttest.

Neither the original GSP nor its Persian validation provides a diagnostic cutoff, and the study records did not retain a prespecified numeric screening threshold. Participants are therefore described as having elevated phubbing screening scores rather than a clinical diagnosis of phubbing. Eligibility also required enrollment in Grades 4–6, ownership of a personal mobile phone, absence of a reported psychiatric diagnosis or ongoing psychiatric treatment, and willingness of both child and mother to participate. Families were considered non-completers if the mother missed more than two sessions or did not undertake the home practice. All 30 enrolled children provided pretest and posttest data.

Screening was followed by eligibility review and enrollment. Twenty-two of the 52 screened students were not enrolled because they did not satisfy all requirements or did not proceed with the child–mother participation process. The records do not give a complete, mutually exclusive reason for every non-enrollment, so an exclusion flow was not reconstructed. All 30 enrolled children, 15 in each condition, completed both assessments. The analysis therefore included the full enrolled sample and required no imputation for missing GSP outcomes.

The age range of 10–12 years corresponds to late primary school, when independent phone use and peer communication may expand while caregiver involvement remains substantial. The sample included boys and girls recruited from the corresponding Hedayat schools. Sex was described to characterize the sample but was not included as an additional covariate or tested as an effect modifier because the study was not powered for reliable subgroup analysis. Likewise, no subgroup claims were made concerning grade, sex, or baseline severity. With only 30 participants, such analyses would be unstable and could produce misleading differences driven by a small number of observations.

Convenience recruitment made an initial school-based evaluation feasible, but it also limits generalizability. Families who accepted a 10-week parent program may have had more concern about phone use, more flexible schedules, greater motivation, or more interest in a relational approach than families who declined or could not attend. Children with comparable GSP scores may therefore respond differently when their caregivers have fewer resources or less willingness to participate. Because mothers were the participating caregivers, the findings also cannot be assumed to apply unchanged to programs delivered by fathers, grandparents, or others.

Measure

Generic Scale of Phubbing

The GSP is a 15-item self-report measure of phubbing with four subscales: nomophobia (items 1–4), interpersonal conflict (items 5–8), self-isolation (items 9–12), and problem acknowledgement (items 13–15; Chotpitayasunondh & Douglas, 2018b). Children completed the questionnaire at both assessments. Mothers did not rate their children’s phubbing. The outcome therefore reflects each child’s account of his or her own behavior, not an observed count of device use or a caregiver’s judgment.

The original GSP uses a seven-point frequency format. The Persian version administered in this study used a five-point agreement format ranging from 1 (strongly disagree) to 5 (strongly agree). Under that format, total scores can range from 15 to 75. Higher values indicate stronger endorsement of phubbing-related experiences or behavior. Subscale scores were calculated by summing their designated items, and the total score was calculated as the sum of the four subscales. The measure was analyzed continuously. No score was interpreted as establishing a psychiatric condition, behavioral addiction, or formal diagnosis.

The four domains provide complementary information. Nomophobia reflects phone-related dependence or discomfort with disconnection. Interpersonal conflict captures friction created when phone behavior competes with social expectations. Self-isolation represents a tendency for device engagement to replace direct participation, whereas problem acknowledgement reflects recognition that the behavior may be excessive or disruptive. Because an intervention might reduce conflict without changing disconnection anxiety, or might increase awareness before producing behavioral change, the total and subscale results were both retained. The total score was designated as the broad summary outcome, and the subscales were analyzed as related primary components with multiplicity control across the five group tests.

Self-report can capture urges, discomfort, and awareness that are difficult for an observer to see, but it is also sensitive to recall, social desirability, reading comprehension, and knowledge of the study's aims. No phone-use logs, ecological observations, teacher ratings, or caregiver ratings were available to corroborate the scores. New reliability estimates were not calculated because coefficients based on 15 participants per condition would be too imprecise to be informative. The results should therefore be read as changes in child-reported GSP scores, not as verified changes in the frequency or duration of smartphone use.

Intervention

The intervention was a 10-session adaptation of filial-therapy and CPRT principles described in the thesis literature. Filial therapy trains a caregiver to use child-centered play, acceptance, empathic communication, and clear behavioral structure within repeated parent–child sessions (Guerney & Gavigan, 1981). The present protocol retained that relational foundation and added material concerning child phone use, interactional interruption, and family-wide device routines. The adaptation was informed by Iranian evidence that a mother-

delivered filial program could be associated with reductions in computer-game addiction symptoms and aggression (Soltani & Farhadi, 2021).

Mothers attended 10 weekly group meetings of approximately 2 hours each. The principal investigator delivered the training under thesis supervision. Sessions combined explanation, demonstration, rehearsal, feedback, and home practice. Mothers were taught to follow the child's lead during designated play, reflect the child's feelings and content, communicate acceptance, encourage effort, offer developmentally appropriate choices, and set limits through a calm sequence that acknowledged the feeling, stated the boundary, and offered an acceptable alternative. These skills were intended to reduce coercive cycles and strengthen the child's experience of focused parental attention.

Phone-related material was woven into the relational program rather than taught as a separate lecture on technology. Families observed when phones interrupted interaction, developed rules that applied to adults and children alike, and protected regular periods of parent–child time from competing device use. Consistency and modeling were emphasized over shame. For example, a mother could recognize that her child wanted to continue an online activity, hold the agreed limit, and offer a choice between two acceptable offline alternatives. This translated the usual CPRT limit-setting sequence into a familiar family conflict.

Home practice was essential because the target behavior occurred in daily life rather than in the training room. Mothers implemented special play periods, recorded observations, rehearsed emotional reflection, and practiced family device agreements. Repetition was expected to help the new interaction pattern become more predictable. Child-led play was also intended to provide an immediately rewarding alternative form of attention: the child had protected access to a responsive caregiver, some control over the activity, and repeated opportunities to

communicate through play. Prior literature reviewed in the thesis suggests that dyadic or child-centered play can support interpersonal understanding and reduce externalizing difficulties (Blanco et al., 2019; Karcher & Lewis, 2002).

The program combined therapist contact, peer-group participation, parent education, relational skills, scheduled play, behavioral limits, reciprocal device rules, and repeated home practice. None of these elements was tested separately, adherence was not measured component by component, and there was no technology-education-only comparison. The study also lacked an independent fidelity observer and a validated session-level fidelity measure. Its findings therefore concern the package as delivered and cannot identify the element that was necessary, sufficient, or most influential.

The waitlist control group received no active program during the assessment interval. Families in that condition continued their usual routines and were offered the intervention after posttest. No evidence in the retained records indicated that control participants received another structured phubbing or play intervention during the comparison period. However, usual-care activities, spontaneous family changes, or awareness produced by completing the pretest were not monitored. Table 1 summarizes the planned focus, core content, and home practice across sessions.

Table 1

Phubbing-Adapted Child–Parent Relationship Therapy Program

Session	Focus	Core content	Home practice
1	Orientation	Introduce CPRT principles; distinguish encouragement from praise; discuss observed patterns of child phone use.	Observe phone-use patterns and practice encouragement.
2	Emotional attunement	Identify core emotions and practice empathic, reflective responses.	Acknowledge the child's emotion before responding.
3	Play and device routines	Establish regular parent–child time and family-wide phone rules that apply to adults and children.	Implement one shared device-use rule consistently.

Session	Focus	Core content	Home practice
4	Choice-giving	Support age-appropriate autonomy and allow the child to lead special play sessions.	Practice structured choices and child-led play.
5	Limit-setting	Use a three-step model: acknowledge the feeling, state the limit, and offer an acceptable alternative.	Apply calm, consistent limits in daily routines.
6	Skill consolidation	Review child-led play, emotional reflection, validation, and firm but respectful limits.	Record observations from a home play session.
7	Communication	Practice emotional expression, self-regulation, and participation in the child's imaginative play.	Model clear, regulated emotional expression.
8	Confidence and problem-solving	Use nonpunitive responses to mistakes and support age-appropriate problem-solving.	Invite the child to solve a manageable problem.
9	Encouragement	Emphasize effort and process rather than outcome-focused praise.	Use process-focused encouragement each day.
10	Maintenance	Review skills, reflect on relationship changes, and plan continued use after the program.	Continue CPRT routines and family device agreements.

Note. CPRT = Child–Parent Relationship Therapy. The adaptation integrated phone-use content primarily into Sessions 1, 3, and 8 while retaining the standard relational and play-based structure.

Statistical Analysis

Analyses were conducted in SPSS version 27 and independently reproduced from the retained raw spreadsheet. As part of that audit, each total GSP score was recalculated from the four recorded subscales at pretest and posttest. Six values in the stored total column did not equal the relevant subscale sum: four intervention posttest totals and two control pretest totals. Those six totals were replaced with the reproducible sums. The underlying subscale values were not changed, so the correction affected only the derived total-score variable.

Data completeness and ranges were reviewed before inferential analysis. All 30 enrolled children had pretest and posttest values for the total score and four subscales, so the primary models used complete cases without imputation. Descriptive statistics were calculated separately by condition and assessment. Means and standard deviations summarize the score distribution, and change was defined as posttest minus pretest; negative change therefore represents lower phubbing scores. Independent-samples *t* tests were used to describe baseline group differences, but the decision to adjust for baseline did not depend on whether those tests reached statistical significance.

The principal analytic strategy was a separate analysis of covariance (ANCOVA) for each outcome. In each model, the posttest score was the dependent variable, condition was the fixed group factor, and the corresponding pretest score was the covariate. This specification estimates the posttest group contrast at a common baseline value and is more directly aligned with the research question than an unadjusted comparison of posttest means. Models were fitted for total phubbing, nomophobia, interpersonal conflict, self-isolation, and problem acknowledgement. Because allocation was not verified as random, baseline adjustment improves comparability on the measured baseline outcome but cannot remove bias from unmeasured differences between conditions.

Adjusted mean differences were calculated as intervention minus control. Because lower GSP scores indicate fewer endorsed phubbing-related experiences, negative values favor the intervention. Each model is reported with adjusted posttest means, a 95% confidence interval for the adjusted difference, the F statistic and degrees of freedom, the p value, and partial η^2 . Within a fitted model, partial η^2 describes the share of effect-plus-error variation associated with condition. It is not the percentage of children who improved and should not be read as a direct measure of clinical benefit in the population.

Five related condition effects were tested. To limit inflation of the familywise Type I error rate, Holm's sequential adjustment was applied across the total score and four subscales. The adjusted p values, rather than the unadjusted p values, were used for the primary inferential conclusions. The total score remained the broad summary outcome, while the domain analyses were retained to describe the consistency and composition of change. Confidence intervals were emphasized alongside p values because the small sample makes the precision and plausible range of each estimate particularly important.

Model assumptions were evaluated at the level of each outcome. Homogeneity of regression slopes was assessed through the condition-by-pretest interaction. Residual distribution was examined with the Shapiro–Wilk test and visual or standardized-residual review, and equality of residual variance was examined with a median-centered Levene procedure. Potentially influential observations were flagged when Cook’s distance exceeded $4/n$. These checks were treated diagnostically rather than as automatic deletion rules. The primary analyses retained all observations to avoid selecting a favorable subset after examining the results.

Sensitivity analyses were used to determine whether the direction and statistical conclusion depended heavily on influential cases or the non-normal self-isolation residuals. For each outcome, models were refitted after excluding observations flagged by the Cook’s-distance rule. For self-isolation, an additional model excluded the single case with an absolute standardized residual greater than 3. Supplementary paired pretest–posttest tests described within-group change, and independent comparisons of change scores provided another representation of the condition contrast. These analyses were not substituted for the baseline-adjusted models; they served as consistency checks.

All tests were two-sided. Exact p values were retained in the analytic audit and were displayed as $p < .001$ when smaller than .001. Statistical significance was not treated as proof of randomization, absence of bias, or clinical importance. The small sample, nonrandomized allocation, multiple outcomes, and lack of follow-up required attention to effect direction, confidence intervals, diagnostic checks, and design limitations together. No subgroup, mediator, moderator, or responder analysis was conducted.

Results

Baseline Scores

No statistically significant baseline differences were detected between the two groups on the total GSP score or any subscale (all $ps \geq .414$; Table 2). These tests do not establish equivalence, so all primary models adjusted for the corresponding baseline score.

Baseline total-score means were 49.47 in the intervention group and 49.20 in the control group, a difference of only 0.27 points. The subscale means were also similar in direction and magnitude, and none of the descriptive baseline tests suggested a marked separation between conditions. Nevertheless, the confidence that can be placed in baseline balance is limited by the small sample. A nonsignificant baseline test can occur even when a meaningful difference is present, and the absence of a documented random allocation process prevents a design-based assumption of exchangeability. Baseline adjustment was therefore retained for every outcome.

Table 2

Baseline Comparisons Between Groups

Outcome	$ t (28)$	p
Total phubbing	0.11	.912
Nomophobia	0.26	.800
Interpersonal conflict	0.54	.595
Self-isolation	0.51	.617
Problem acknowledgement	0.83	.414

Note. Two-tailed independent-samples t tests are shown. Absolute t values are reported.

Descriptive Change

The intervention group showed lower mean scores at posttest on every outcome, whereas the control group remained stable or increased (Table 3). The intervention group's total score decreased by 7.20 points (49.47 to 42.27); the control group's mean increased by 2.40 points (49.20 to 51.60).

The 9.60-point contrast in mean change reflected movement in opposite directions: the intervention mean fell after the program, whereas the waitlist mean rose. Change scores also varied within each group, with standard deviations of 5.00 in the intervention condition and 4.45 in the control condition. These descriptive changes do not adjust for baseline and were not the primary test, but they point in the same direction as the adjusted analysis.

Change was not confined to one subscale. In the intervention group, mean scores decreased by 1.93 points for nomophobia, 1.73 for interpersonal conflict, 1.87 for self-isolation, and 1.67 for problem acknowledgement. In the control group, nomophobia did not change, while interpersonal conflict increased by 1.20 points, self-isolation by 1.00 point, and problem acknowledgement by 0.20 points. Every domain therefore showed a more favorable descriptive trajectory in the intervention condition. The pattern is consistent with change across a multicomponent program, but it cannot reveal which program element contributed to a particular domain.

Table 3

Pretest, Posttest, and Change Scores by Group

Outcome	Group	Pretest <i>M</i> (<i>SD</i>)	Posttest <i>M</i> (<i>SD</i>)	Change <i>M</i> (<i>SD</i>)
Total phubbing	Intervention	49.47 (7.63)	42.27 (8.03)	−7.20 (5.00)
	Control	49.20 (5.17)	51.60 (8.21)	2.40 (4.45)
Nomophobia	Intervention	15.00 (3.12)	13.07 (3.03)	−1.93 (1.33)
	Control	15.27 (2.58)	15.27 (3.03)	0.00 (1.89)
Interpersonal conflict	Intervention	11.80 (2.54)	10.07 (3.13)	−1.73 (1.53)
	Control	11.20 (3.49)	12.40 (3.56)	1.20 (2.04)
Self-isolation	Intervention	12.13 (3.58)	10.27 (3.75)	−1.87 (1.60)
	Control	11.40 (4.32)	12.40 (4.00)	1.00 (3.14)
Problem acknowledgement	Intervention	10.53 (2.72)	8.87 (1.64)	−1.67 (2.09)
	Control	11.33 (2.55)	11.53 (2.07)	0.20 (1.74)

Note. Change = posttest minus pretest; negative values indicate lower phubbing scores.

Primary ANCOVA Findings

After adjustment for baseline total score, the intervention group had a lower adjusted posttest total score than the control group (42.13 vs. 51.73), adjusted mean difference = -9.60 , 95% CI $[-13.22, -5.99]$, $F(1, 27) = 29.73$, Holm-adjusted $p < .001$, partial $\eta^2 = .524$. All four subscale differences also favored the intervention group after Holm correction (Table 4).

The 95% confidence interval placed the adjusted intervention mean between 5.99 and 13.22 points below the control mean. The corresponding partial η^2 of .524 was large within the fitted model. Because allocation was not verifiably random, this statistic describes an observed baseline-adjusted separation rather than a bias-free causal effect. Even with that qualification, the estimate was sizeable relative to the 15–75 scale and agreed with the unadjusted change-score contrast.

Adjusted subscale estimates also favored the intervention. The intervention-minus-control difference was -1.96 for nomophobia, -2.89 for interpersonal conflict, -2.71 for self-isolation, and -2.28 for problem acknowledgement. All Holm-adjusted p values were .006 or smaller. Partial eta squared values ranged from .263 for self-isolation to .423 for problem acknowledgement. The largest adjusted domain difference in scale points occurred for interpersonal conflict, whereas the smallest occurred for nomophobia. Because the subscales contain different numbers of items and score ranges, raw differences should not be used to rank their substantive importance without caution.

Lower adjusted scores appeared across domains involving disconnection, conflict, withdrawal, and acknowledgement, so the posttest contrast was not confined to awareness or phone-related anxiety alone. All four domains, however, came from the same self-report instrument administered at the same two time points. Shared method variance and knowledge of

the intervention could have contributed to their parallel movement. The findings show a coherent pattern within one measure, not four independent replications across different methods.

Table 4

ANCOVA Results for Adjusted Posttest Group Differences

Outcome	Adj. <i>M</i> Int.	Adj. <i>M</i> Ctrl.	Difference	95% CI	<i>F</i> (1,27)	Holm <i>p</i>	Partial η^2
Total phubbing	42.13	51.73	−9.60	[−13.22, −5.99]	29.73	< .001	.524
Nomophobia	13.19	15.15	−1.96	[−3.19, −0.73]	10.69	.006	.284
Interpersonal conflict	9.79	12.68	−2.89	[−4.26, −1.52]	18.61	< .001	.408
Self-isolation	9.98	12.69	−2.71	[−4.49, −0.92]	9.64	.006	.263
Problem acknowledgement	9.06	11.34	−2.28	[−3.33, −1.23]	19.83	< .001	.423

Note. Adjusted means were estimated at the grand-mean value of the corresponding pretest outcome. Difference = intervention minus control. CI = confidence interval; Int. = intervention; Ctrl. = control.

Assumption and Sensitivity Checks

The group-by-pretest interaction was not statistically significant for any outcome ($ps \geq .066$), and median-centered Levene tests were nonsignificant ($ps \geq .201$). Residual normality was acceptable for four outcomes. Self-isolation residuals departed from normality (Shapiro–Wilk $p < .001$); however, the adjusted group effect remained significant after removal of the single observation with an absolute standardized residual greater than 3, $F(1, 26) = 11.22$, $p = .002$, partial $\eta^2 = .301$. Cook’s-distance sensitivity models also retained statistically significant group effects for all five outcomes. Supplementary paired and change-score analyses converged with the primary ANCOVA findings (Supplementary Tables S1–S4).

Slope tests gave no clear evidence that the pretest–posttest association differed by condition, although the total-score interaction was close to the conventional threshold ($p = .066$) and merits re-examination in a larger sample. Non-normal self-isolation residuals were the main diagnostic concern. Removing the one extreme standardized residual left the group contrast in the same direction and statistically significant, making it less likely that a single observation produced the result. Cook’s-distance analyses likewise retained significant condition effects after

flagged cases were removed. These checks strengthen confidence in the direction of the estimates, but they cannot correct selection bias, measurement limitations, or unmeasured confounding.

Discussion

Children whose mothers completed the phubbing-adapted CPRT program reported lower posttest phubbing scores than children in the waitlist group after baseline adjustment. The adjusted total-score difference was 9.60 points, and all four subscales favored the intervention after Holm correction. Descriptive trajectories, change-score comparisons, paired analyses, and influence sensitivity models agreed with the primary ANCOVA. This convergence supports a short-term association between program participation and lower child-reported phubbing, but it does not show that the program alone caused the change.

The size of the total-score contrast is noteworthy, but it needs a restrained interpretation. Partial η^2 was .524 within the fitted model, and the confidence interval excluded a null adjusted difference. Estimates from a small nonrandomized study, however, may be unstable or inflated. Motivation, caregiver availability, expectancy, unmeasured family characteristics, and changes occurring outside the program could all have contributed. The result justifies a more rigorous trial; it does not establish the efficacy of CPRT for child phubbing. Replication with a larger sample, concealed random allocation, an active comparison, and longer follow-up is needed.

A relational account of phubbing offers one way to understand this pattern. Phubbing is defined by the disruption that occurs when attention shifts from a person to a device. Earlier studies linked it with poorer perceived interaction quality, less enjoyment, and feelings of exclusion (Chotpitayasunondh & Douglas, 2018a; David & Roberts, 2017; Dwyer et al., 2018). CPRT works in that same interpersonal space by increasing focused attention, emotional

acknowledgment, child-led exchange, and predictable limits. More rewarding and less conflictual offline contact could reduce the appeal of turning to a device during family interaction. That explanation is plausible, but the study did not test it directly.

The nomophobia result may reflect change in the child’s relationship with disconnection, family routines, or both. The adjusted difference was smaller in raw scale points than for several other domains but remained significant after multiplicity adjustment. Family-wide agreements may make periods without a phone more predictable and less punitive. Scheduled play can also provide an alternative activity and source of attention during device-free time. However, the program did not include a specific exposure-based treatment for anxiety about disconnection, and the study did not measure fear of missing out, withdrawal-like distress, or actual periods without the device. The nomophobia finding should therefore be interpreted as lower endorsement of the GSP domain, not proof that a defined anxiety mechanism changed.

The interpersonal-conflict finding is closely aligned with the intervention content. Phone-related disputes often involve repeated commands, delayed compliance, criticism, and inconsistent enforcement. CPRT teaches the caregiver to acknowledge emotion, state a clear boundary, and offer an acceptable choice. That sequence can reduce escalation even when the limit itself remains unchanged. Reciprocal family rules may also reduce the perception that adults demand behavior they do not model. Technology interference has been associated with strain in parenting and coparenting (McDaniel & Coyne, 2016), and parents may perceive social media as challenging family cohesion or authority (Lewis et al., 2015). The present result suggests that a structured relational response to such conflict deserves further study, ideally with direct observation of parent–child exchanges.

Lower self-isolation scores may reflect greater participation in shared offline activity. Child-led play calls for sustained engagement with another person while preserving the child's agency, which is different from simply replacing a phone with an adult-chosen task. The child receives attention and also helps shape the interaction. Dyadic developmental play has been associated with better interpersonal understanding and fewer externalizing behaviors (Karcher & Lewis, 2002), and Samuelsson and Johansson (2006) described play, learning, and interaction as closely intertwined. Neither study tested phubbing, but both help explain why repeated shared play might strengthen reciprocal attention and reduce withdrawal from immediate social contact.

Problem acknowledgement also decreased, which may initially appear counterintuitive if lower scores represent less recognition of a problem. Within the GSP scoring framework, however, higher endorsement reflects stronger identification with problematic phone-related behavior. A child whose behavior becomes less disruptive may have fewer problems to acknowledge. An alternative possibility is that participation changed willingness to report or label the behavior. Because the study did not include item-level qualitative interviews, it cannot distinguish reduced problematic behavior from a change in self-perception or response style. Future work should examine item-level trajectories and combine the GSP with behavioral indicators so that lower acknowledgement scores can be interpreted within a broader pattern of observed change.

The pattern across domains also fits the family and social-learning literature reviewed in the thesis. Longitudinal and mediation studies suggest that perceived parental phubbing can precede problematic smartphone use in adolescents, with dependency intentions, perceived norms, attachment, boredom, and self-efficacy among the possible pathways (Geng et al., 2021; Liu et al., 2019; Xie et al., 2019; Zhao et al., 2022). The current program approached the issue

from the other direction by training caregivers to provide focused attention, model reciprocal rules, and respond differently during conflict. Children were the measured phubbers, but family life may shape both expectations about phone use and access to rewarding alternatives.

The results also extend the filial-therapy literature reviewed in the thesis. Filial approaches treat caregivers as capable of delivering therapeutic interaction when they receive structured training and supervision (Guerney & Gavigan, 1981). Previous child-centered or dyadic play studies reported benefits in domains such as externalizing behavior, interpersonal understanding, and academic functioning (Blanco et al., 2019; Karcher & Lewis, 2002). In Iran, maternal filial training was associated with reductions in computer-game addiction symptoms and aggression (Soltani & Farhadi, 2021). The present findings are consistent with that broader evidence while addressing a distinct behavior: the interruption of real-time social interaction by a phone. The study therefore contributes preliminary evidence that filial principles may be adaptable to a contemporary technology-related concern.

More than one process may have been involved. Focused play could strengthen warmth and belonging, reflective listening could support emotion regulation, choices could reduce oppositional responses, consistent limits could reduce uncertainty, and parent modeling could alter perceived norms. Group meetings may also have offered mothers support and accountability. None of these processes was measured, and no mediation analysis was conducted. It would therefore be unwarranted to attribute the score changes to attachment, self-esteem, boredom, or family cohesion. A future trial should select a small set of mechanisms in advance and measure them at suitable intervals.

The use of child self-report is both a contribution and a limitation. Children directly reported their own urges, conflicts, isolation, and acknowledgement, avoiding sole reliance on

adult perceptions. This is valuable because adults may underestimate covert device preoccupation or overestimate behavior during conflict. Yet children also knew whether their mothers attended the program and may have inferred the desired response. Objective device-use data would not replace self-report because phubbing depends on social context, but it could complement it. Future assessment could combine the GSP with device logs, structured observation of family interaction, ecological prompts asking whether phone use occurred during an interaction, and separate caregiver or teacher ratings.

The cultural and service context should also be considered. The intervention was delivered through schools in Hamedan and involved mothers who were willing to attend 10 sessions lasting approximately 2 hours each. The relational skills may be broadly applicable, but expectations concerning parental authority, children's autonomy, gender roles, play, and appropriate phone use vary across families and communities. Adaptation should preserve the core principles of acceptance, child-led engagement, and consistent limits while allowing examples and routines to fit local family life. Implementation studies should assess feasibility, attendance burden, facilitator training, fidelity, and acceptability before assuming that the full format can be transferred to other schools or clinical services.

In practice, concern about child phubbing need not be framed as a diagnosis or managed only through prohibition. A school counselor or family practitioner could examine when phone use disrupts relationships, help caregivers establish reciprocal rules, and teach interaction skills through structured play. Screening scores are better used to guide discussion and monitoring than to label a child as addicted or clinically phubbing. A brief parent program is not a substitute for comprehensive assessment when severe anxiety, depression, safety concerns, family violence, or wider functional impairment is present.

The findings also have implications for the design of a definitive trial. Eligibility should use a prespecified operational threshold or clearly defined risk criterion. Allocation should be generated and concealed through a reproducible procedure. An active comparison should match contact time and group attention. Outcome assessors and analysts should be masked where feasible, and fidelity should be independently rated. Assessments should include post-intervention and follow-up time points, objective or observational measures, and prespecified adverse-event monitoring. A sample-size calculation should be based on a conservative effect rather than the large estimate observed here. These design features would determine whether the short-term association is reproducible, specific to the intervention, and sustained.

Cross-Disciplinary Methodological Considerations

Methods developed in other fields can suggest useful directions for later phubbing research without implying that the subject areas are equivalent. Elizondo-González and Jahanbin (2026), for example, used a Linear Logistic Test Model and a prespecified Q-matrix to relate item difficulty to explicit cognitive and linguistic features. Similar item-level work with the GSP could examine whether reading demand, wording direction, or developmental language affects how children of different ages respond. The present study had only 30 participants and retained data at the subscale level, so such modeling was not possible here. The relevance of this work is prospective: it points to a more informative measurement strategy for future validation rather than supplying evidence about the validity of the scores in this sample.

Jahanbin (2025a; see also the accompanying reproducible materials, Jahanbin, 2025b) used an explainable, reduced-feature modeling framework to identify informative predictors. A larger phubbing study could adopt a similar approach when integrating device logs, observations, and self-report.

Strengths and Limitations

Several features strengthen the study. The controlled pretest–posttest design retained every enrolled child in the outcome analysis and used a multidimensional measure completed by the children themselves. Baseline-adjusted models were reported with confidence intervals, effect sizes, familywise error control, and multiple consistency checks. The session-by-session program description also makes the intervention easier to understand and reproduce. Although a waitlist does not control for attention, it allowed the comparison families to receive the program after posttest and offers more information than an uncontrolled pretest–posttest design.

The statistical audit is another strength. Recalculating each GSP total from its four subscales uncovered six inconsistencies in the stored total-score column. Correcting those derived values did not alter any recorded subscale score. The main analyses were then reproduced independently from the spreadsheet, and diagnostic findings were reported even when they raised a concern, as with the self-isolation residuals. This process improves reproducibility and prevents arithmetic errors in a derived variable from carrying into the manuscript.

The most important limitation is the absence of a verifiable record of randomization or allocation concealment. The groups may have differed in motivation, family functioning, parental concern, availability, or other characteristics that baseline GSP scores did not capture. ANCOVA adjusts for the measured baseline outcome, but it cannot recreate randomization or remove unmeasured confounding. Because the waitlist group also lacked an attention-matched activity, expectancy, facilitator contact, and group support remain plausible alternative explanations. The findings should not be expressed as causal effects.

The sample was small, recruited by convenience from one school context, and limited to children whose mothers could participate. Small samples yield imprecise and potentially unstable effects, reduce the ability to detect assumption violations, and prevent reliable subgroup analysis. The lack of attrition is useful, but it does not resolve selection into enrollment. Generalization to other regions, socioeconomic contexts, age groups, fathers, or families with more severe difficulties should be cautious.

Measurement was also limited. The records did not preserve the numeric screening rule used for recruitment, and the GSP has no diagnostic cutoff. The same child self-report measure was administered twice, with no device logs, direct observations, caregiver reports, teacher reports, or follow-up assessment. Durability therefore cannot be evaluated. Nushi and Jahanbin (2024) addressed a related timing problem in a small quasi-experimental educational study by adding a two-week delayed posttest to the immediate assessment. Future CPRT research would benefit from the same repeated-assessment logic, although behavioral maintenance would require a much longer interval. Neither participants nor the intervention provider were documented as masked, and program fidelity was not rated independently. Expectancy, changes in reporting, and variation in delivery therefore remain entangled with behavioral change.

Finally, the study did not measure the mechanisms proposed in the conceptual interpretation. Parent–child relationship quality, parental phubbing, family rules, play engagement, boredom, self-efficacy, attachment, and objective interaction time were not assessed. The data therefore cannot demonstrate that relationship improvement mediated the effect or that family-wide rules altered social norms. The intervention should be evaluated as a package, and all mechanism statements should be treated as hypotheses for future research.

Conclusion

Among these primary school children, participation in a 10-session phubbing-adapted CPRT program delivered to mothers was associated with lower short-term child-reported phubbing scores. The total GSP score and all four subscales favored the intervention, and the direction was consistent across baseline-adjusted, change-score, paired, and influence sensitivity analyses. The study provides an initial indication that a relational, parent-mediated play approach may be relevant when phone use disrupts face-to-face interaction.

The evidence remains preliminary. Allocation was not verifiably randomized, the sample was small, the control condition was a waitlist, and outcomes came from one self-report measure with no follow-up. A preregistered randomized trial should use a prespecified screening rule, concealed allocation, an active comparison, independent fidelity assessment, multimethod outcomes, and longitudinal follow-up. Until such evidence is available, these findings support further evaluation rather than a definitive claim of efficacy.

Declarations

Ethics Approval and Consent to Participate

The study was approved by the Research Ethics Committee of Bu-Ali Sina University (IR.BASU.REC.1402.033). Written informed consent was obtained from parents, and assent was obtained from participating children. Participation was voluntary, responses were treated confidentially, and participants could withdraw without penalty.

Consent for Publication

Not applicable; the manuscript contains no identifiable individual-level information.

Competing Interests

The author declares no competing interests.

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Data Availability

The data include information collected from minors and are not publicly posted. A de-identified dataset may be available from the corresponding author upon reasonable request and subject to applicable ethical and institutional requirements.

Author Contributions

Atiyeh Choobkar was responsible for conceptualization, methodology, investigation, data curation, formal analysis, and preparation, review, and approval of the final manuscript.

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Supplementary Analyses

The following analyses were prespecified as consistency and robustness checks. They were not treated as the primary inferential tests.

Table S1

Within-Group Pretest–Posttest Comparisons

Outcome	Int. <i>t</i> (14)	Int. <i>p</i>	Int. <i>dz</i>	Ctrl. <i>t</i> (14)	Ctrl. <i>p</i>	Ctrl. <i>dz</i>
Total phubbing	5.57	< .001	1.44	2.09	.056	.54
Nomophobia	5.61	< .001	1.45	0.00	1.000	.00
Interpersonal conflict	4.38	< .001	1.13	2.28	.039	.59
Self-isolation	4.53	< .001	1.17	1.23	.238	.32
Problem acknowledgement	3.08	.008	.80	0.45	.663	.11

Note. Cohen's *dz* = absolute mean change divided by the standard deviation of change scores. Raw *p* values are shown; these within-group tests were supplementary.

Table S2

Between-Group Differences in Change Scores

Outcome	Int. Δ	Ctrl. Δ	Difference	95% CI	<i>p</i>	Holm <i>p</i>
Total phubbing	−7.20	2.40	−9.60	[−13.14, −6.06]	< .001	< .001
Nomophobia	−1.93	0.00	−1.93	[−3.16, −0.71]	.003	.009
Interpersonal conflict	−1.73	1.20	−2.93	[−4.28, −1.58]	< .001	< .001
Self-isolation	−1.87	1.00	−2.87	[−4.73, −1.00]	.004	.009
Problem acknowledgement	−1.67	0.20	−1.87	[−3.31, −0.43]	.013	.013

Note. Δ = posttest minus pretest. Difference = intervention Δ minus control Δ ; negative values favor the intervention.

Table S3*ANCOVA Diagnostic Checks*

Outcome	Slope interaction <i>p</i>	Shapiro–Wilk <i>p</i>	Levene <i>p</i>
Total phubbing	.066	.578	.957
Nomophobia	.852	.505	.353
Interpersonal conflict	.352	.129	.201
Self-isolation	.222	< .001	.270
Problem acknowledgement	.302	.182	.474

Note. Levene tests used median-centered absolute deviations of ANCOVA residuals.

Table S4*Influence Sensitivity Analyses*

Outcome	Cases removed	Reduced <i>n</i>	Reduced <i>F</i>	Reduced <i>p</i>	Reduced partial η^2
Total phubbing	3	27	48.14	< .001	.667
Nomophobia	1	29	17.20	< .001	.398
Interpersonal conflict	3	27	80.75	< .001	.771
Self-isolation	2	28	25.26	< .001	.503
Problem acknowledgement	2	28	18.87	< .001	.430

Note. Cases with Cook's distance $> 4/n$ were excluded only in sensitivity models. Primary models retained all observations.