

SELF-CONTROL GROUP COUNSELING REDUCES SOCIAL MEDIA ADDICTION IN EARLY ADOLESCENTS

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ABSTRACT

Problematic social media use can disrupt adolescents' learning, direct social interaction, and emotional regulation, yet evidence concerning brief school-based counseling responses remains limited. This study examined whether group counseling incorporating self-control techniques reduced social media addiction among eighth-grade students. A quantitative pre-experimental one-group pretest-posttest design was used. From a population of 256 students, 10 students with high pretest scores were selected purposively. Participants completed a four-response Likert-style social media addiction scale before and after six counseling sessions. Descriptive statistics, Shapiro-Wilk tests, and a paired-samples t-test were applied. The mean score decreased from 107.5 to 85.0, representing a 22.5-point or 20.9% reduction. The paired comparison was statistically significant, $t(9) = 6.945$, $p < .001$; a supplementary effect-size calculation based on the reported test statistic indicated a very large within-participant effect (Cohen's $d_z = 2.20$). These findings suggest that structured group counseling may strengthen adolescents' capacity to monitor urges, regulate access, schedule alternative activities, and use social media more adaptively. School counselors may use the intervention as a targeted early-response strategy, while avoiding the assumption that all intensive use is pathological. Future research should employ randomized or controlled designs, larger multisite samples, follow-up measurement, objective usage indicators, and fully reported instrument validity, reliability, and ethical approval information.

Keywords: adolescent counseling, group counseling, school intervention, self-control, social media addiction

INTRODUCTION

Digital Adolescence and Problematic Social Media Use

In the past decade, research on adolescents' digital lives has moved from asking whether young people use social media to examining how, why, and under what conditions use becomes disruptive. Social media platforms are now embedded in communication, friendship maintenance, identity exploration, entertainment, and school-related information exchange. Consequently, high frequency alone cannot be treated as evidence of addiction. A more defensible distinction separates intensive but functional engagement from problematic social media use, which is characterized by impaired control, persistent use despite negative consequences, preoccupation, conflict with important obligations, withdrawal-like discomfort, tolerance, relapse, and mood modification (Andreassen, 2015; Billieux et al., 2015; Kuss & Griffiths, 2011; van den Eijnden et al., 2016). This distinction is essential in educational settings because an intervention should target loss of control and functional impairment rather than punish ordinary digital participation.

The adolescent period is particularly relevant to this problem. Early adolescents are developing autonomy, peer identity, emotional regulation, and executive control while simultaneously encountering platforms designed around continuous social feedback and rapid reinforcement. The developmental importance of belonging and peer recognition can make notifications, comments, short videos, and algorithmically selected content difficult to disengage from. Contemporary reviews therefore emphasize that problematic use emerges through interactions among individual vulnerabilities, affective states, cognitive expectancies, platform affordances, and inhibitory-control processes rather than through exposure time alone (Brand et al., 2019; Leijse et al., 2023; Montag et al., 2024; Sun & Zhang, 2021). Within this interactional perspective, the school is not merely a location in which the consequences appear; it is a formative environment where self-regulatory competencies can be taught and practiced.

Empirical findings also counsel against simplistic claims. Meta-analyses indicate that the relationship between general social media use and depressive symptoms is typically small and heterogeneous, whereas studies that assess addiction-like or problematic patterns show more consistent associations with depression, anxiety, stress, sleep difficulty, and reduced well-being (Huang, 2017; Ivie et al., 2020; Marino et al., 2018; Mojtabai, 2024; Shannon et al., 2022; Woods & Scott, 2016). An umbrella review likewise concluded that effects differ according to the person, the form of use, the outcome, and the measurement strategy (Valkenburg et al., 2022). These findings make functional impairment and failed self-regulation more useful intervention targets than a universal screen-time threshold. They also imply that counseling should help students make deliberate, context-sensitive choices rather than impose total abstinence, which is rarely realistic in contemporary education.

The Indonesian school context intensifies the practical relevance of this issue. Local studies describe social media as a major influence on adolescent interaction, identity formation, and daily routines, while also documenting varying levels of addictive use and difficulties in controlling online behavior (Aprilia et al., 2020; Komariah et al., 2020; Rahayu et al., 2024; Regita et al., 2024). In the source study, preliminary observations at the participating junior secondary school indicated that more than 60% of eighth-grade students used social media for over four hours per day and approximately 45% displayed warning signs such as difficulty stopping during study, reduced concentration, and anxiety when access was unavailable. These school-level observations do not constitute prevalence estimates for the wider population, but they establish a locally meaningful service need and justify targeted screening.

Self-Control as a Modifiable Mechanism

Self-control refers to the capacity to align immediate impulses with longer-term goals. It includes noticing a desire, evaluating its compatibility with valued outcomes, inhibiting or delaying a response, modifying environmental cues, and selecting an alternative behavior. Trait self-control is associated with a broad range of adaptive outcomes, and it is particularly relevant to academic persistence because learning often requires resisting immediately rewarding alternatives (de Ridder et al., 2012; Duckworth et al., 2019; Tangney et al., 2004). Experience-sampling research further shows that self-regulatory success depends not only on willpower at the point of temptation but also on conflict recognition, motivation, and the management of situational cues (Hofmann et al., 2012). This broader understanding supports interventions that combine awareness, planning, stimulus control, self-monitoring, and reinforcement.

Digital overuse has long been interpreted as a problem of deficient self-regulation. LaRose et al. (2003) argued that repeated internet behavior may become habitual when users fail to monitor actions against personal standards and expected consequences. The I-PACE model extends this reasoning by proposing that addictive behaviors develop through recurring interactions among predispositions, affective and cognitive reactions, cue reactivity, expectancies, and executive functions (Brand et al., 2019). Social media-specific reviews similarly identify self-control as a reliable protective factor, and a recent meta-analysis reports a meaningful inverse relationship between self-control and problematic social media use (Şimşir & Akyürek, 2024). These perspectives provide a clear mechanism for counseling: students may reduce problematic use when they learn to identify triggers, interrupt automatic access, set concrete limits, monitor progress, and reinforce alternative actions.

Group Counseling as a School-Based Response

Schools can address problematic digital behavior through preventive education, targeted counseling, and coordinated support involving teachers and families. Systematic reviews of internet-addiction prevention recommend developmentally appropriate programs that involve the social environments surrounding children and adolescents rather than relying exclusively on individual prohibition (Vondráčková & Gabrhelík, 2016). Controlled school-based studies indicate that structured classroom programs can influence digital-use trajectories and related self-regulatory processes (Einstein et al., 2024; Walther et al., 2014), while a meta-analysis of psychological interventions for adolescent internet and smartphone addiction found that structured programs can reduce symptom severity, although effect estimates vary and stronger randomized trials remain necessary (Malinauskas & Malinauskiene, 2019). This broader evidence supports school intervention while also warning that modality, dose, comparison conditions, and follow-up matter.

Group counseling offers several mechanisms that are particularly suitable for early adolescents. First, normalization reduces shame and helps students recognize that urges and digital habits are shared and modifiable. Second, peer discussion exposes participants to alternative coping strategies and creates opportunities for observational learning. Third, public but supportive goal setting can strengthen commitment and accountability. Fourth, group feedback helps students distinguish between functional use and behavior that conflicts with learning, sleep, family interaction, or face-to-face relationships. Indonesian studies have reported promising reductions in problematic technology use following group guidance or counseling incorporating self-control techniques (Nija & Lianawati, 2020; Nugrahaeningtyas et al., 2024). However, the literature remains dominated by small, context-specific studies, and intervention descriptions are often less detailed than international reporting standards require.

Research Gap, Objective, and Novelty

It remains unclear how much change can be attributed specifically to self-control group counseling when interventions are tested without a comparison group, when samples are selected because they begin with extreme scores, and when instrument psychometrics or follow-up outcomes are incompletely reported. The gap is therefore not the absence of any intervention evidence; rather, it lies in the need for context-sensitive school studies that report their design accurately, distinguish statistical change from causal proof, connect intervention components to self-regulation theory, and identify the limits of generalization. The source manuscript addressed a practical gap by applying a six-session group intervention to screened eighth-grade students in a real school counseling context. Its original contribution is the integration of group dynamics with explicit self-control exercises directed at addiction-like dimensions of social media use, followed by quantitative pretest-posttest evaluation.

The purpose of this study was to determine whether social media addiction scores decreased after students participated in group counseling using self-control techniques. The directional hypothesis stated that posttest scores would be significantly lower than pretest scores. The study's novelty lies in translating self-regulation principles into a brief, school-deliverable group format for early adolescents and in interpreting the resulting change through both statistical and practical lenses. The scope is deliberately narrow: the study evaluates short-term within-participant score change among 10 purposively selected eighth-grade students from one school. It does not establish population prevalence, platform-specific effects, long-term maintenance, or causal superiority over alternative counseling approaches. These boundaries are central to an accurate interpretation of the findings.

METHOD

Research Design and Approach

This study employed a quantitative pre-experimental one-group pretest-posttest design. All participants were measured before the intervention, received the same six-session counseling program, and were measured again after the intervention. The design is represented as O1–X–O2, where O1 denotes the pretest, X denotes group counseling with self-control techniques, and O2 denotes the posttest. The design is suitable for an initial school-based effectiveness test because each participant serves as his or her own pre-intervention reference. Nevertheless, the absence of random assignment and a comparison group means that the design estimates temporal change associated with participation rather than definitive causal impact.

Population and Participants

The study population comprised 256 eighth-grade students at the participating junior secondary school. Ten students were selected through purposive sampling because their screening or pretest scores indicated a high level of social media addiction. The source manuscript does not report participants' age distribution, gender composition, socioeconomic characteristics, platform preferences, or comorbid concerns; therefore, these characteristics are not inferred. The analytic sample represents students identified for targeted counseling and should not be interpreted as a representative sample of all eighth-grade students.

Data Collection Techniques and Instrument

The primary instrument was a Likert-style social media addiction scale with four response alternatives: very appropriate, appropriate, inappropriate, and very inappropriate. The same scale was administered before and after treatment. Observation and documentation were used as supporting data-collection techniques. The intervention was delivered over six meetings following the conventional stages of group counseling: formation, transition, core activity, and termination. Across the meetings, the source description indicates attention to salience, withdrawal-like discomfort, tolerance, mood modification, relapse, and conflict. Students were encouraged to recognize triggers and consequences, delay urges, limit access time, construct activity schedules, increase direct social interaction, monitor their own behavior, evaluate progress, and use self-reinforcement.

The exact number of scale items, possible score range, item-development procedure, content-validity evidence, construct-validity evidence, and internal-consistency coefficient were not reported in the source manuscript. No unsupported psychometric values are introduced in this revision. These details should be inserted from the original research records before final submission because they are necessary for evaluating measurement quality and reproducibility.

Data Analysis Procedures

Descriptive statistics summarized the highest, lowest, and mean pretest and posttest scores. Shapiro-Wilk tests were reported for the two measurement occasions because the sample contained fewer than 50 participants. The primary hypothesis was tested with a paired-samples t-test at an alpha level of .05. The original output expressed the difference as pretest minus posttest with a negative sign in one table; for interpretive consistency, this article reports the reduction as a positive value of 22.5 points while retaining the absolute t statistic, degrees of freedom, and probability value. Statistical software may display $p = .000$, but the correct scholarly notation is $p < .001$.

A supplementary within-participant effect size was calculated from the reported *t* statistic and sample size using Cohen's $d_z = t/\sqrt{n}$. Because this value is derived from summary statistics rather than raw partici

pant-level data, it is presented as an interpretive supplement. A 95% confidence interval for the mean reduction was also derived from the reported standard error and the critical *t* value for nine degrees of freedom. Levene's homogeneity test was not used to judge the paired comparison because equality of variance between independent groups is not an assumption of a paired-samples *t*-test. Ideally, the normality of the difference scores should be inspected directly; the available source only reports normality tests for pretest and posttest scores separately.

Validity, Reliability, and Ethical Considerations

Internal validity is constrained by history, maturation, testing effects, regression toward the mean, expectancy effects, and the absence of a comparison group. Statistical conclusion validity is also limited by the small sample. The source manuscript does not provide a formal research-ethics approval number, assent and consent procedures, confidentiality protocol, or school-permission documentation. These ethical details must be completed from the original study records. The present revision therefore limits its claims to the evidence actually reported and treats the findings as promising preliminary effectiveness evidence rather than conclusive proof.

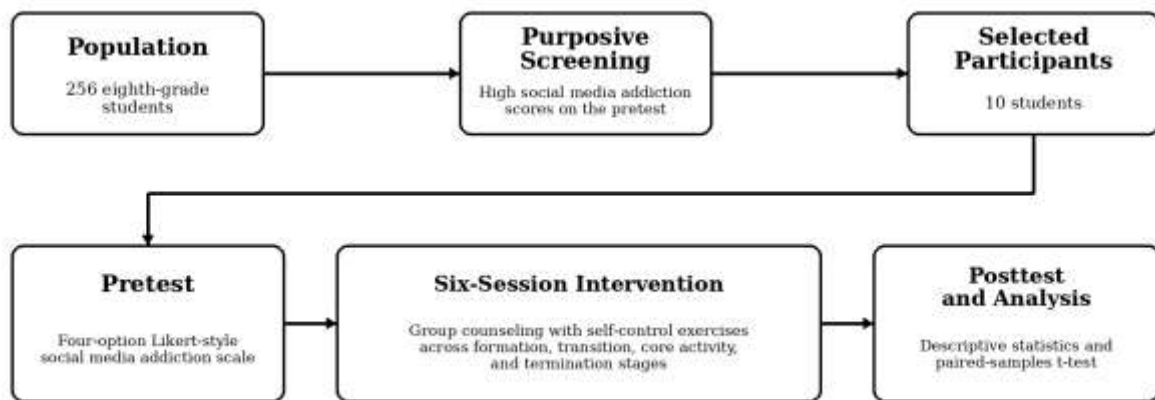


Figure 1. Research design and intervention flow

RESULTS AND DISCUSSION

Descriptive Change After the Intervention

The findings show a clear downward shift in group-level social media addiction scores after the six-session intervention. The mean score decreased from 107.5 at pretest to 85.0 at posttest, an absolute reduction of 22.5 points. Relative to the pretest mean, this change represents a 20.9% decrease. The highest observed score declined from 119 to 88, while the lowest observed score declined from 90 to 79. Thus, both ends of the observed score range were lower after counseling.

Table 1. Pretest and posttest descriptive statistics

Statistic	Pretest	Posttest	Change
Highest score	119	88	−31
Lowest score	90	79	−11
Mean	107.5	85.0	−22.5 (20.9%)

The descriptive results are educationally meaningful because they indicate that the average participant moved substantially away from the high pre-intervention level. However, aggregate maxima, minima, and means cannot establish that every individual improved. Participant-level change scores were not available in the source document, and the observed narrowing of the range may reflect different individuals at the minimum and maximum on each occasion. The results therefore support a group-level conclusion but not an individual responder analysis.

Assumption Checks and Hypothesis Test

The Shapiro-Wilk statistics reported for the pretest and posttest were nonsignificant. The pretest yielded $W = .866$, $p = .189$, and the posttest yielded $W = .905$, $p = .251$. These values provide no statistical evidence that either set of scores deviated from normality. With only 10 participants, however, normality tests have low power to identify non-normal distributions. Moreover, the paired-samples t -test assumes approximate normality of the difference scores, not equality of variance between two independent groups. Because the source does not provide raw difference scores, this assumption cannot be inspected directly. The inference should therefore be considered reasonably supported but not exhaustively diagnosed.

Table 2. Shapiro-Wilk normality tests

Measurement	W	df	p	Interpretation
Pretest	.866	10	.189	No significant departure from normality
Posttest	.905	10	.251	No significant departure from normality

The paired-samples t -test identified a statistically significant pretest-posttest difference. The mean reduction was 22.5 points, the standard deviation of the paired differences was 10.244, and the standard error was 3.240. The reported test statistic was $t(9) = 6.945$, $p < .001$. The derived 95% confidence interval indicates that the population mean reduction compatible with these data lies approximately between 15.17 and 29.83 points, subject to the assumptions of the design and analysis. The directional hypothesis was therefore supported: the posttest mean was significantly lower than the pretest mean.

Table 3. Paired-samples t -test and supplementary effect estimate

Comparison	Mean reduction	SD of difference	SE	t(df)	p	Cohen's dz
Pretest–posttest	22.500	10.244	3.240	6.945 (9)	< .001	2.20

Magnitude and Practical Meaning of the Effect

Statistical significance alone is not sufficient to judge intervention value, especially in a small sample. The supplementary effect-size calculation produced Cohen's $dz = 2.20$, conventionally interpreted as a very large within-participant effect. The confidence interval around the mean reduction also excludes trivial change. These indicators suggest that the score difference was not merely detectable but substantial relative to the variability of individual change scores. At the same time, effect sizes from uncontrolled pilot studies are often inflated by purposive selection of participants with initially high scores, regression toward the mean, common-method measurement, and expectancy effects. The effect should therefore be interpreted as evidence of strong short-term

change within this group, not as an unbiased estimate of the intervention's causal effect in a broader population.

Theoretical Interpretation: From Automatic Access to Deliberate Choice

One explanation for the observed decline is that the intervention disrupted automatic access routines. Problematic social media use is sustained when cues such as boredom, notifications, academic stress, or social uncertainty rapidly activate habitual checking and when the immediate reward outweighs distant goals. Deficient self-regulation theory predicts that behavior becomes difficult to control when individuals do not monitor it against standards or when self-reactive consequences are weak (LaRose et al., 2003). The counseling activities described in the source—monitoring urges, evaluating consequences, limiting access, and creating schedules—directly strengthen these regulatory functions.

The I-PACE framework provides a complementary interpretation. According to Brand et al. (2019), cue reactivity, affective responses, cognitive biases, expectancies, and executive functions interact over repeated cycles. A student may initially open a platform to relieve boredom or obtain peer reassurance; repeated reinforcement can then make the response increasingly automatic. Self-control counseling can intervene at multiple points in this cycle. Trigger identification increases metacognitive awareness; delayed responding weakens cue-response immediacy; alternative activity scheduling changes the behavioral environment; and self-reinforcement increases the value of goal-consistent action. The intervention therefore does more than ask students to “use less.” It provides a behavioral architecture for choosing when, why, and how to use social media.

The very large within-group change is also consistent with broader evidence that self-control is inversely associated with problematic social media use (Şimşir & Akyürek, 2024). Self-control is not a single act of suppressing desire. Meta-analytic and daily-life research indicates that successful regulation involves habits, standards, monitoring, avoidance of tempting situations, and goal commitment (de Ridder et al., 2012; Hofmann et al., 2012). The source intervention included several of these components, particularly self-monitoring, scheduling, and self-reinforcement. This component alignment strengthens the plausibility of the theoretical explanation, even though mediation was not directly tested.

Contribution of Group Processes

The group format may have amplified the self-control content. Adolescents' social media practices are socially embedded; peers, family support, health literacy, and perceived self-control shape the contexts in which problematic use develops (Lahti et al., 2024; Leijse et al., 2023). Group counseling allows participants to compare experiences and discover that difficulties in disengaging are not unique personal failures. This normalization can reduce defensiveness and increase willingness to experiment with change. Peer-generated strategies may also appear more realistic than adult directives, particularly when participants discuss how to manage class time, homework, sleep, and group-chat expectations.

Group processes can create accountability. When students articulate a personal limit, review progress, and receive feedback, the intended standard becomes more salient. Social learning is also likely: participants can observe peers describing successful cue management, replacement activities, or relapse recovery. These mechanisms are compatible with the positive findings reported in Indonesian group guidance and counseling studies using self-control techniques (Nija & Lianawati, 2020; Nugrahaeningtyas et al., 2024). The present study extends that practice-based literature by linking score change to an explicit self-regulation mechanism and by reporting a supplementary effect-size estimate.

Comparison With International Intervention Evidence

The direction of the finding is consistent with international evidence that structured psychological and educational interventions can reduce problematic digital behavior. Vondráčková and Gabrhelík (2016) concluded that prevention is most defensible when it targets at-risk young people and includes relevant social systems. Walther et al. (2014) found that a brief school-based media-literacy intervention altered trajectories of gaming-related behavior, demonstrating that school delivery can influence digital habits without requiring clinical placement. Malinauskas and Malinauskiene (2019) reported overall benefits of psychological interventions for adolescent internet and smartphone addiction, while also noting heterogeneity and the need for stronger trials. The current result falls within this pattern: brief, structured, school-based work appears promising, but confidence depends heavily on design quality.

The study also supports the literature's shift from raw duration to impaired control. Reviews by Andreassen (2015), Kuss and Griffiths (2011), Sun and Zhang (2021), and Montag et al. (2024) emphasize that addiction-like use is multidimensional. A student may spend many hours online for learning or social coordination without experiencing severe impairment, whereas another student may spend less time but repeatedly fail to stop despite academic and interpersonal consequences. By addressing salience, withdrawal, tolerance, mood modification, relapse, and conflict, the intervention focused on the pattern of regulation rather than a single time quota. This is a conceptually stronger basis for counseling and reduces the risk of overpathologizing ordinary adolescent behavior, a concern raised by Billieux et al. (2015).

The mental-health literature adds urgency but also nuance. Shannon et al. (2022) found moderate associations between problematic social media use and depression, anxiety, and stress, whereas Ivie et al. (2020) reported a small and heterogeneous association between general adolescent social media use and depressive symptoms. Marino et al. (2018) similarly found links between problematic use, psychological distress, and well-being. Taken together, these findings suggest that school counselors should screen for function and distress, not assume that use causes mental-health problems. Reduced addiction-scale scores may indicate better regulation, but this study did not measure depression, anxiety, sleep, academic achievement, or relationship quality. Claims about broader benefits must therefore remain prospective rather than demonstrated.

Educational and Counseling Implications

For school counseling practice, the result supports a tiered response. Universal services may teach digital literacy, attention management, sleep hygiene, and respectful online interaction to all students. Targeted group counseling can then be offered to students who show persistent loss of control or interference with learning and relationships. The six-session format is potentially feasible within a school term and may be integrated into guidance schedules without requiring long-term clinical treatment. Counselors should nevertheless use validated screening instruments, avoid stigmatizing labels, and explain that the objective is balanced, purposeful use rather than complete abstinence.

The intervention components can be translated into routine counseling tools. Students can keep brief trigger-and-response records; identify high-risk situations such as homework, bedtime, or emotional discomfort; disable nonessential notifications; define device-free study intervals; place devices outside immediate reach; schedule offline alternatives; set specific rather than vague limits; review lapses without self-condemnation; and reinforce incremental success. These techniques reflect environmental restructuring and self-management rather than moralizing. They are also more compatible with the academic value of digital technology because they preserve beneficial access while reducing automatic or conflict-producing use.

At the school-policy level, individual counseling should not carry the entire burden. Consistent classroom expectations, protected learning periods, teacher modeling, parent communication, peer support, and referral pathways are needed, because social and health-literacy resources may buffer problematic use across adolescent contexts (Lahti et al., 2024). Schools should distinguish discipline violations from signs of dysregulation and should avoid policies that merely confiscate devices without teaching replacement skills. Because problematic use can coexist with anxiety, depression, bullying, family conflict, or academic difficulty, counselors should assess whether social media is the primary problem, a coping strategy, or both. Referral to mental-health services is appropriate when impairment or emotional symptoms exceed the scope of school counseling.

Alternative Explanations and Unexpected Considerations

Several non-intervention explanations could contribute to the score reduction. Participants were selected because they began with high scores, making some regression toward the mean likely on repeated measurement. The pretest itself may have increased awareness and changed behavior. Participants may have reported lower posttest scores because they understood the desired outcome or wished to satisfy the counselor. Events occurring during the intervention period, maturation, or temporary changes in school demands could also influence use. Without a wait-list or active comparison group, these alternatives cannot be separated from the effect of counseling.

The range data also deserve caution. The maximum declined by 31 points, whereas the minimum declined by 11 points, but these are not necessarily the same students across occasions. The pattern may indicate that students with the most severe initial scores benefited more, yet raw paired data are required to test this possibility. Future studies should report individual change plots or reliable-change indices. Such analyses would reveal whether improvement was consistent, concentrated among a few participants, or accompanied by nonresponse in some students.

Limitations and Reliability of the Findings

The primary limitation is the one-group pre-experimental design. A significant paired t-test demonstrates that scores differed across time, but it cannot establish that the intervention alone caused the difference. Second, the sample of 10 purposively selected students limits statistical precision and external validity. Third, the study was conducted in one school and one grade level, so local counseling culture, implementation quality, peer composition, and school policy may have shaped the result. Fourth, only immediate posttest data were reported; maintenance after several weeks or months is unknown.

Fifth, the outcome was based on self-report. Objective logs of platform access, screen time, nighttime use, or app switching could complement subjective addiction scores, although objective duration should still be interpreted alongside impairment. Sixth, instrument details were incomplete. Without item count, scoring range, validity evidence, and reliability coefficients, readers cannot fully judge measurement sensitivity. Seventh, the source reports Shapiro-Wilk tests for each occasion rather than the paired differences, and raw data were unavailable for a more complete diagnostic analysis. Finally, ethical approval, consent, assent, and confidentiality procedures must be documented explicitly before publication.

Despite these limitations, the result remains useful as preliminary effectiveness evidence. The reduction is large, statistically clear, theoretically plausible, and aligned with prior school and intervention literature. The appropriate conclusion is not that the program has been definitively proven, but that it warrants a more rigorous trial. Future research should use randomized assignment or matched comparison groups, preregistered hypotheses, larger multisite samples,

validated instruments, treatment-fidelity checks, facilitator competence measures, follow-up assessments, and mixed methods. It should also test mediators such as improved self-control, reduced cue reactivity, and changed social norms, as well as moderators such as baseline severity, gender, platform type, family monitoring, and mental-health symptoms.

CONCLUSION

This study aimed to determine whether six sessions of group counseling using self-control techniques were associated with reduced social media addiction among purposively selected eighth-grade students. The mean score decreased from 107.5 to 85.0, a 22.5-point reduction, and the paired comparison was significant, $t(9) = 6.945$, $p < .001$, with a very large derived within-participant effect. The findings contribute preliminary evidence that school counseling can translate self-regulation principles—trigger awareness, delayed response, access limits, activity scheduling, self-monitoring, and reinforcement—into a feasible group intervention for early adolescents. Practically, the program may support targeted school services that promote balanced and purposeful digital behavior; theoretically, it is consistent with deficient self-regulation and I-PACE explanations of problematic use. Because the design lacked a control group, involved only 10 students from one school, and did not report follow-up, complete instrument psychometrics, or formal ethical documentation, future studies should use controlled multisite designs, objective and self-report indicators, treatment-fidelity assessment, mediator analysis, and longer-term evaluation.

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