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YOUTH MENTAL HEALTH AND SCHOOL SMARTPHONE BANS: EARLY EVIDENCE

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ABSTRACT

This paper is the first to examine the causal effects of school smartphone bans on the mental health of youth in the US. Time series data show that the mental health of youth has been declining for the past decade. Several researchers argue that easy access to social media and other internet sites provided by smartphones is to blame. To provide causal evidence of the effects of these bans, I rely on synthetic difference-in-difference models and the National Survey of Children's Health (NSCH) from 2016 to 2024. Currently, there are data for only one state with two post-ban periods and two states with one post-ban period, which makes the results preliminary evidence only. The outcome variables are screentime and measures of psychological wellbeing. Overall, these early results provide no clear evidence that the school ban policy reduced screentime or improved psychological wellbeing. Future studies with additional years of data, when they are available, are needed to increase power and to estimate the longer-term effects of school bans on youth mental health.

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1.Introduction

The declining mental health of teens has emerged as a critical public health concern. Time trends in youth mental health from the Youth Risk Behavior Surveys (YRBS) show an increase in persistent feelings of sadness or hopelessness from 2013 to 2023. By 2023, more than half the girls and more than a quarter of boys in the survey reported these feelings. The YRBS data also show that the number of both girls and boys making a suicide plan has increased. Data from the National Survey of Children's Health (NSCH) show that from 2016 to 2024, the percent of children aged 11 to 17 with a current diagnosis of anxiety increased from 9.8% to 16.4%. These surveys indicate a serious public health problem for teens.

An increasing number of public health researchers have argued that the use of social media is a causal factor in the decline of the mental health of young people. The Surgeon General (2023) argues that social media can have positive or negative impacts on youth depending on the content and the individual's strengths and weaknesses. The Surgeon General finds that youth who spend more than 3 hours a day on social media face an increased risk of mental health issues including experiencing symptoms of depression and anxiety and may also result in poor sleep quality. Singh et al. (2026) examined the relationship between social media use and mental health for youth in Australia. They found that moderate social media use was associated with the best well-being outcomes, while both no use and the highest use were associated with poorer well-being. This relationship was consistent across survey years and robust to sensitivity analyses. Pew Research (2023) reports that the share of US teens who say they are online almost constantly has roughly doubled since 2014 from 24% to 46% in 2023. This time is primarily on social media. While light social media use can be beneficial most American youth are heavy users. In a recent bestseller, Haidt (2024) argues that social media is having a destructive impact on the mental health of young people. This includes sleep deprivation, attention fragmentation, loneliness, and social comparison. Also, time spent on social media reduces time in independent real-world activity, which is important for developing social skills, navigating risks, and building resilience. This can lead to increases in anxiety, depression, self-harm, and suicide.

Alternatively, for some youth, causality could go from declining mental health to increased smartphone use. Over the past 10 years, parental mental health has been declining along with the mental health of children (Daw et al., 2025). This can lead to neglect of children, failure to help children feel secure, and to emotional or physical abuse. There are also external factors directly affecting children, including increased pressure and competition to succeed academically in order to have a secure financial future. There are also obstacles to obtaining mental health services, including long wait lists, insurance barriers, and a shortage of qualified specialists. These issues can lead to declining mental health for children and result in some young people turning to online contact with friends and others for support.

If social media and other online sites are harmful to some children, it is important to examine the approaches that are available to reduce these harms. One approach is a ban on social media accounts for youth. The federal "Kids Off Social Media Act" (S. 278) would prohibit children under 13 from creating social media accounts and ban algorithmic recommendation systems for users under 17. Enforcement would be the responsibility of the Federal Trade Commission (FTC). The bill also includes provisions that would require schools to block social media on their networks as a condition for receiving telecommunication discounts. The bill successfully advanced out of a senate committee in February 2025 and was put on the Senate Legislative Calendar in June 2025 but has gone no further. On the state level, 40 state attorneys general have filed lawsuits against Meta, claiming it is harming young people and contributing to the youth mental health crisis by deliberately designing features on Instagram and Facebook that addict children to its platforms. Another legal approach involves a set of lawsuits by parents that claim that the social media platforms deliberately addict and harm children. These lawsuits argue that the damage was done through deliberate design choices that sought to make the platforms more addictive to children to boost profits. The parents can ask the court for injunctive relief, which would require the companies to stop certain harmful practices. This could also end the companies' First Amendment shield, which protects them from liability for material posted on their platforms. The social media companies claim that they are not at fault and are very likely to contest all lawsuits that do not end in their favor.

In contrast, Australia has enacted a federal law prohibiting children under 16 from having accounts on select social media platforms. These include TikTok, Instagram, Snapchat, Facebook, X and seven other social media platforms. This new law cited concerns about online harms such as bullying and mental health impacts. The legislation requires tech companies to use age verification methods to deactivate existing underage accounts, with large potential fines on the social media platforms for non-compliance. About 4.7 million youth accounts in Australia have been closed thus far. Spain, France, Denmark, Norway, Greece, the United Kingdom and the European Union are all actively considering restrictions on social media accounts. How these age restrictions would affect youth mental health is unknown at this time.

Another approach centers on smartphones. These devices provide access to useful everyday information and help youth with learning and homework assignments. But they also provide an ever-present and easy accessibility to social media, artificial intelligence (AI), and other forms of online interaction and entertainment, which can be problematic for young people. The average age that young people get their first smartphone is around 11.6 years old, with a significant increase in ownership between the ages of 10 and 12. Over 40% of American children have a smartphone by age 10, rising to over 70% by age 12 (Harris Poll, 2025). A Pew Research Center survey conducted in June 2025 found that 74% of U.S. adults say they support banning middle and high school students from using smartphones during class.

School smartphone bans can be bell-to-bell or instructional time only. The bell-to-bell ban typically involves a special pouch for storage during the school day.[1] The student turns off their phone and places it in the pouch at the start of the day, which is then locked with a magnetic seal, keeping the device inaccessible but still in the student's possession. At dismissal, students use a designated magnetic unlocking base to open the pouches and retrieve their phones. For approved early departures or emergencies, schools provide specific unlocking points for students to open pouches before leaving campus. In states with instructional time only smartphone bans, students must put away their phones during class but can use them during non-academic times like lunch or free periods. Schools with the instructional time only bans have specific rules for storage that range from just storing them out of sight to a special magnetic pouch such as the Yondr pouch.

Schools have had some form of phone restrictions for many years. However, these older restrictions relied on simple teacher-led enforcement, such as verbal warnings or confiscating the phone for the remainder of the class or day. By 2025, 26 states had adopted school bans on smartphones, and more are considering adopting this policy. School bans are appealing to parents and to state legislatures but, at this time, there is no causal research on the effects of this policy on children's mental health. School bans are fairly easy to enact although there are some problems with enforcement. Some students try to use school provided Chromebooks to access internet entertainment and find ways to access their phones during the school day. Schools try to counter this by limiting access to entertainment sites on the school's Wi-Fi network. Bell-to-bell bans require funding to obtain secure storage solutions for student devices during school hours. School bans may also require schools to provide reliable methods for parents to contact their children

[1] The brand called Yondr is popular.

during the school day to address emergency concerns. Because these bans cannot be perfectly enforced and because there is ample time outside of school to go online school bans could have no effect on youth mental health.

This paper is the first to examine the causal effects of school smartphone bans on the mental health of youth in the US. To study the effects of bans I rely on synthetic difference-in-difference models and the National Survey of Children's Health (NSCH) from 2016 to 2024. These data are collected in every state and include extensive information on the mental health of about 18,500 children annually. At this time, there are post-ban data for only one state over two years and for two states over one year, which makes the results preliminary estimates. The dependent variables include a measure of screentime and eight measures of psychological wellbeing. Most of the outcome variables show evidence of parallel trends in the pre-period. Minimum detectable effect (MDE) sizes are also calculated with a simulation procedure. Because the estimated 95% confidence intervals for the ban effects encompass the MDE and zero, the study lacks the statistical power to distinguish between a null effect and a small significant effect. The results are consistent with an effect smaller than the MDE or no effect at all. If there is an effect and it is smaller than the MDE, the models are underpowered. Overall, these preliminary results provide no evidence that the school smartphone ban policy reduces screentime nor improves the psychological wellbeing of students.

2.Prior Studies

Prior studies of interest are those that investigate the link between smartphones and mental health and those that specifically investigate the effects of school smartphone bans on student mental health. These studies suggest that smartphone use has a negative effect on the mental health of youth. There are fewer studies of school bans on youth outcomes, and they are mostly from Europe. Overall, there is insufficient evidence in the literature to conclude that school smartphone bans have a causal effect on the mental health of youth in the US. Another area of interest is the short-term effect of smartphone restrictions. These studies find that there can be significant immediate effects.

Three causal studies from Europe exploit random differences in internet access as a source of exogenous variation in smartphone use. Golin (2022) examines the causal effect of broadband internet access on self-reported mental health and the gender gap in mental health. She exploits exogenous differences in the availability of broadband internet in Germany and finds that availability leads to a decrease in self-reported mental health for young women. Donati et al. (2022) exploit exogenous differences in the proximity of Italian municipalities to broadband internet access to show that access leads to an increase in diagnoses of mental disorders. Arenas-Arroyo et al. (2025) rely on variations in fiber optic availability in Spain to assess the impact of high-speed internet access on adolescent mental health. They find that access increases addictive internet usage and reduces time allocated to sleep, homework, and social interactions with family and friends. Access decreases mental health and contributes to an increase in adolescent suicide rates, particularly among girls.

Churchill and Johnson (2026) investigate the effect of increased internet access on youth mental health in the US. They use 2009–2019 Youth Risk Behavior Survey data and rely on the nationwide rollout of broadband internet as the exogenous change. The data show that adolescents in states with greater broadband internet access report spending more time online. They find that the increase in broadband internet access increased suicide ideation among youth, with more pronounced effects for girls. Greater internet access was also associated with increased cyberbullying and a reduction in adequate sleep.

Studies of school bans include Abrahamsson (2026), who examines the effects of a school smartphone ban in Norway. In Norway, schools decide whether to allow or ban smartphones. This resulted in variations in the timing of smartphone bans over the 10-year study period. The research design assumes that the timing of a school adopting a smartphone ban is uncorrelated with other determinants of student outcomes. Using detailed administrative data with survey data on middle schools' smartphone policies, plus an event study design, Abrahamsson finds that banning smartphones significantly improved mental health for girls. Additionally, girls' GPAs improved, and their likelihood of attending an academic high school track increased. Post-ban bullying among both genders also decreased.

A second Scandinavian study, Kessel et al. (2020), uses a difference-in-difference model with Swedish data to estimate the effects of a school smartphone ban. Data on educational results were obtained from the Swedish National Agency for Education. The data are school-level panel data on the educational performance of ninth-grade pupils over the period 1997–2017. In Sweden, schools choose their smartphone policy because there was no national policy. This led to a staggered adoption of bans that accelerated rapidly after 2015 and is called a "bandwagon effect". A bandwagon effect would make the adoption of bans exogenous to actual outcomes associated with smartphone use. They show evidence of parallel trends in the pre-period; however, they find no significant effect in the post-period. They conclude that in Sweden, there is no impact of smartphone bans on student performance.

Another paper on school bans is Goodyear et al. (2025), who used a sample of 30 English secondary schools. In this sample, 20 schools have a restrictive phone policy and 10 do not. The specific rules prohibiting phone use were left to the schools' discretion. They found no evidence that restrictive school policies were associated with overall smartphone and social media use or better mental wellbeing in adolescents.

A US study relies on the Florida smartphone ban in schools (Figlio and Özek, 2025). They note that smartphone bans in schools are increasing, but little is known about their effects on academic success. They use a difference-in-difference approach to examine the effect the Florida school smartphone ban had on student test scores using student-level data. The comparison groups are high-smartphone-activity schools and low-smartphone-activity schools in the periods before and after the policy took effect. This relies on an exposure design for exogenous variation. However, high and low smartphone use may not be exogenous to the outcome. Differences in family income could affect smartphone use and the trajectory of school achievement. Exogeneity requires the post-period effect to be related only to the treatment and not to a pre-existing difference in the trajectory of the outcome. They conclude that smartphone bans led to significant improvements in student test scores in the second year of the ban.

The other area of research relevant to this paper relates to whether short-term restrictions on smartphone use have an impact on mental health. This is relevant because school smartphone bans in the US are new, and effects can currently only be observed in the short run. Two US experimental studies address the short-term effects of a total ban, which is suggestive of the potential effects of the part-time ban resulting from school policy. The results from both of these studies suggest that if school bans have an effect on mental health, it should be evident even in the short run.

The first study is Hunt et al. (2021). They recruited 88 undergraduates who were randomly assigned to either: 1) limit Facebook, Instagram, Twitter, and Snapchat use to 30 minutes total per day while increasing social media interaction, 2) limit use to 30 minutes per day, or 3) continue to use social media as usual for three weeks. The baseline data showed that following more friends was negatively correlated with loneliness, whereas following more strangers was positively correlated with depression. Groups 1 and 2 showed significant reductions in depression compared to Group 3, the control group.

The second study is Elombe et al. (2025). They conducted a cohort study where participants completed a two-week observational baseline followed by an optional one-week social media detox intervention. Participants were aged 18–24. There was no control group, but over one week, it is reasonable to assume a control group would have no change in mental health (though there might be a participant effect). Data on social media use and mental health were collected over a two-week baseline period. The participants self-selected into the detox, which introduces bias. They found that reducing social media use for one week was associated with reductions in symptoms of depression, anxiety, and insomnia.

3. Data

The dataset I rely on is the National Survey of Children's Health (NSCH), which is conducted annually by the U.S. Census Bureau and is the largest annual national survey on the health of children ages 0–17. The NSCH is conducted as a household survey, and one child per household is selected to be the subject for a detailed age-specific questionnaire. The respondent is a parent or guardian who lives in the home and has knowledge of the sampled child. Survey participants complete either web-based or self-administered paper-and-pencil questionnaires. These data are well suited to this study because they include extensive information on the health of children as well as state identifiers, which allow a state's school smartphone policy to be linked to the respondent.

The survey collects data on indirect and direct measures of mental health. The NSCH is also important because it is fielded every year and includes all states plus DC, unlike the Youth Risk Behavior Surveys. The sample window is 2016 to 2024. I include data only for children aged 11 to 17 years old because smartphone ownership increases rapidly at about age 11. The sample includes over 167,000 interviews. Survey weights are available to generate nationally representative means. The NSCH also collects extensive demographic information, which is used for covariates.

Table 1 lists a set of variables derived from the NSCH data, a brief description of the question text, and weighted statistics. All of these variables have been defined as dichotomous except for screentime, which is measured in hours. The definition of screentime includes more than just smartphone time, is not limited to social media, is limited to weekdays and is reported by the parent. The data for this variable show a significant increase in screentime over the sample period, which corresponds to data from Pew Research (2024) that find nearly half of teens say they are online almost constantly, up from 24% a decade ago. Because this variable includes several media it should only be considered an indicator of time trends in social media use. The remaining variables in Table 1 are measures of psychological wellbeing and are the primary outcome measures. The data presented in Table 1 include the change from 2016 to 2024 and significance tests on these differences. These data show that seven of the eight measures of psychological wellbeing declined significantly over the sample period. The eighth measure is "Bullied," which has increased significantly. This is also a negative outcome for psychological wellbeing. These changes align with other data sources that also show that psychological wellbeing has declined and that screentime has increased for young people.

Table 2 presents correlation coefficients between all the psychological wellbeing variables in Table 1. These correlations are based on all observations over the nine years of data and are weighted by the population weights provided in the survey. Correlations of .3 or more are highlighted in bold. Excluding "Friends" and "Bullied," the other variables show strong correlations with each other, suggesting a common underlying construct relating to psychological wellbeing. Friends and Bullied appear to be separate from the others and are more specific measures of social function. Social function is also an important aspect of psychological wellbeing.

The NSCH also provides a number of demographic questions that can be used as covariates. These variables include the child's age, sex, race/ethnicity, marital status, and highest parent education level. The NSCH also includes data on the child's health insurance coverage. Table 3 provides the weighted means of all the demographic variables for 2024 along with the national means from the Current Population Survey. There is a close match between all of the variables from the two surveys that shows that the NSCH is nationally representative.[2]

The NSCH data are linked by state ID to state regulations on smartphone bans in schools. These bans are listed in Table 4. In May 2023, Florida adopted a statewide smartphone ban based on a model program in certain districts that showed that strict policies improved student focus and were relatively easy to implement. Florida's 2023 action sparked a trend followed in 2024 by four other states and by many other states in 2025. The primary drivers of these new state bans were: 1) the rapid increase in public perception that smartphones were a problem for young people, and 2) that the bans were relatively easy for states to enact. This makes the enactment of school bans more of a bandwagon phenomenon and arguably exogenous to the variation across states in the outcomes. Table 4 also shows that only Louisiana instituted a bell-to-bell ban.

To define the first post-ban period, we need to consider when the survey data was collected and when the school ban went into effect. The NSCH data are collected between July and December of each year. In Florida, the ban went into effect in July 2023. Thus, the first post-period for Florida is the 2023 NSCH data because, when the 2023 data were collected, the ban was in effect. In two other states, the bans went into effect in the spring of 2024, making the 2024 NSCH data the first post-period. This time pattern also creates a staggered adoption problem.

[2] Both surveys are conducted by the US Census.

4. Empirical Approach

The approach relies on the synthetic difference-in-difference (SDID) estimator (Arkhangelsky et al. 2021; Clarke et al. 2023), capitalizing on the quasi-natural experiments provided by the state smartphone bans in schools. The NSCH includes all states and DC for all years and thus satisfies SDID balanced panel requirement without dropping any states. The standard errors in the SDID estimations are based on placebo methods because the number of treatment states is small. Arkhangelsky et al. (2021) argue that SDID is the superior estimator when there are only a few treatment states because it achieves double robustness by combining the strengths of both standard Difference-in-Differences and Synthetic Control methods.

Specifically, in its basic form, a consistent causal effect of the school bans on a given outcome (Y_{st}, in a given state *s* at time period *t*) can be derived by estimating:

$$\left(\hat{\beta}^{SDID}, \hat{\mu}, \hat{\gamma}, \hat{\tau}\right) = \underset{\beta, \mu, \gamma, \tau}{arg\ min} \left\{\sum_{s=1}^{n} \sum_{t=1}^{t} (Y_{st} - \mu - \gamma_s - \tau_t - BAN_{st}\beta)^2 \hat{\omega}_i^{SDID} \hat{\lambda}_t^{SDID}\right\}. \qquad (1)$$

In equation (1) the causal effect of the treatment, represented above by $\hat{\beta}^{SDID}$, is estimated from a DID model with optimally chosen weights $\hat{\omega}_i^{SDID}$ and $\hat{\lambda}_t^{SDID}$. SDID relaxes the requirement of unconditional parallel trends by constructing weights that enforce approximate parallel trends in the pre-treatment period. Specifically, optimal unit-specific weights $\hat{\omega}_i^{SDID}$ are chosen to align pre-treatment trends across outcomes in the untreated vs. treated states, subject to a regularization parameter, which prevents overfitting while increasing the variance and uniqueness of the weights. SDID also introduces and optimally chooses time-specific weights $\hat{\lambda}_t^{SDID}$ to further remove bias from unobserved shocks and improve precision. These computations improve the robustness and precision of the SDID estimator, in addition to making the model more flexible in generating credible counterfactual comparisons (Arkhangelsky et al. 2021). SDID also separately calculates coefficients for sets of states based on staggered adoption.

5. Results

The SDID results are presented in table 5. The regression results need to be considered along with evidence of parallel trends in the pre-period and with evidence of sufficient statistical power. In order to assess pre-period parallel trends between the treatment and control states, I estimated event studies for each dependent variable. SDID event studies are time series plots of the difference between the treatment group and the control group. Because the average pre-period differential between the treatment group and control group is subtracted from each period's differential, the "reference group" is the average over the entire pre-period. Event studies for all nine dependent variables were estimated and the plots for these models are presented in figure 1. These event studies account for staggered adoption. For the one state, 2023 is the first post-period and for the other two states 2024 is the first post-period. The approach to staggered adoption in SDID is to treat each adoption cohort as a sub-experiment. The event study procedure used defines time as leads and lags relative to the treatment year. The leads represent the placebo periods (pre-treatment), and lags represent the treatment effects (post-treatment). In the SDID event study plots, effect_1 is the ITT after one year and is based on the three states with one post-period and the first period for the one state with two post-periods.[3] In the plots, effect_2 is based on the second year of the one state with two post-periods. Table 5 summarizes the results of these tests and reports a "yes" for parallel trends if all seven of the pre-period coefficients are statistically equal to zero. All but Calm and Concentration meet this definition of parallel trends.

The validity of the estimated ITTs in table 5 are also dependent on statistical power. The staggered SDID design is complex, and power cannot be easily determined with a simple formula.

[3] The estimated effect should be considered an intent-to-treat (ITT) effect of the policy, where the ITT is defined as the effect of being subject to the law, regardless of individual compliance. Some individuals or schools may not comply with the law or may not comply fully with the law. This is a common occurrence with bans such as the ban on selling alcohol to people under age 21. The ITT is the likely real world effect.

Black et al. (2022) describe a simulation approach to power analysis that can be used with any research design. The simulation approach mirrors the actual estimation as much as possible. I use the 2016-2024 NSCH data set with the three actual treatment states deleted, which leaves 48 states. The simulation process randomly selects three states as treatment states and increments the dependent variable for these states in the post-periods by a chosen amount.[4] Then the regression model is estimated with covariates using placebo standard errors, and the p-value is saved. This process is repeated 1000 times and the percent of repetitions that estimate an effect with a p-values of .05 or less is the statistical power. The increment is adjusted in repeated estimation runs until the power increases or decreases to 80%. This increment is the minimum detectable effect (MDE) size and are presented for all dependent variables in table 5.

The ITT for screentime is insignificant, which suggests the ban didn't reduce total usage. Total screentime is a broad measure and the school bans could have shifted students away from high-stress social media interactions toward other activities without changing their overall time spent on devices. Thus, psychological wellbeing could still have been affected.

For all the outcome variables, the 95% confidence intervals for the ITTs include both zero and the estimated MDE. The MDE provides the smallest effect size that could be reliably estimated. The regression results are consistent with an effect smaller than the MDE or no effect at all. If there is an actual effect and, it is smaller than the MDE, the models are underpowered. Consequently, these results are statistically insignificant and are consistent with both a null effect and a very small significant positive or negative effect on screentime and psychological wellbeing. The point estimates are the “best guess" of the direction of the relationship, given that it is insignificant. Excluding the two variables without parallel trends and excluding Bullied, four of the

[4] Because the data set used by SDID is aggregated to the year-state level, the dichotomous dependent variables are converted to continuous percentages. An increment can thus be applied to each dependent variable.

five remaining psychological wellbeing variables have negative ITTs. The metric for Bullied is reversed, that is, less is better. The negative coefficient Bullied suggests less bullying. Also, the event histories show that in the second year of the ban in Florida, there was a significant decline in the number of students bullied and a significant increase in screentime. Overall, these early results do not provide clear evidence that the school ban policy reduced screentime nor improved the psychological wellbeing of students.

6. Robustness Tests and Extensions

Because no clear evidence was found in the primary models it is important to test alternative specifications to ensure that these results are not a consequence of the model specification nor are they obscuring important effects for population subgroups. An important extension of this research is the effect of school bans on education performance. The results from these added models and descriptive data on school performance are presented in the Appendix. Again, there is no clear evidence that the school ban policy had any positive effects on students.

6.1 Testing the effects for each treatment state individually

It is possible that treatment effects in each of the three treatment states are very different and that estimating an aggregate effect is misleading. To check for this, I estimated the models in table 5 for one state at a time. These results are presented in table A.1. In the Florida only models, interest is negative and significant which suggests that the bans reduced student interest. In Louisiana only and Indiana only models there were no significant treatment coefficients.

6.2 Testing demographic specific models

Both the Surgeon General (2023) and Haidt (2024) make the point that there are key gender differences in the type of interaction and impact of social media. Girls are more susceptible to the social pressures and body image issues prevalent on platforms like Instagram. Boys are impacted differently by video games, online gambling and other online content that can make the virtual world feel more comfortable than offline settings, potentially leading to a cycle of social isolation and a lack of offline social bonding. To test for additional demographic differentials the models were restimated for white only, age 10-13 only, age 14-17 only, and low and high parent education only. These results are presented in table A.2. In these models there are a few significant coefficients, which are mostly negative.

6.3 Academic achievement

Improvements in academic achievement may result from school smart phone bans and are typically correlated with mental health. There are descriptive data available from the National Assessment of Educational Progress (NAEP) that can provide some insight into the effect of the Florida smartphone ban on school achievement. The NAEP is conducted only every other year but is highly regarded and is also called the nation's report card. Figure A.1 presents data from NAEP for 8th grade Math in Florida and nationally. The data are simply time series plots that do not control for covariates and do not match a set of control states to Florida. The data in figure A.1 shows that math scores in Florida were typically lower than the national scores but trended in a very similar pattern to the national scores. From 2019 to 2021 both Florida and the national scores fell due most likely to Covid. In the 2024 survey, which includes the Florida smartphone ban, the national scores stopped falling, but scores in Florida continued to fall. Figure A.2 presents similar data for reading scores. Here we see the match between the national and Florida are not as consistent as the math scores. The Covid related downturn is also present in these data. In the period from 2017 to 2023 Florida was right at the national average. However, after the smartphone ban began Florida 8th grade reading scores fell relative to the national average. This is only one post-period and does not include careful matching with the control group. However, it does suggest that the ban did not improve 8th grade math scores nor improve 8th grade reading scores.

7. Conclusions

The empirical work in this paper finds no clear evidence of beneficial mental health effects from state mandated school bans on smartphones. This could be due to several reasons:

1) The recent study by Goodyear et al. (2025) suggests that while bans reduce in-school phone use by about 40 minutes per day, students typically increased use during evenings and weekends. School bans may not reduce total weekly use at all and thus have no impact psychological wellbeing. The empirical results for the screentime models presented above support the theory that school bans do not reduce overall screentime.

2) For some youth, causality could go from declining mental health to increased smartphone use. Declining mental health for children with limited parental support can result in these young people turning to online contact with friends and others for support. If the correlation between smartphone use and mental health for children is negative, but there is causality in both directions, then the measured causal effect of school bans could be zero.

3) Another reason for the null results may be the current availability of data, which limits this study to one state with two post-period years and two states with one post-period year. The effect of bans may take longer to become evident. The limited number of treated units and short post-treatment duration likely restrict our ability to detect small effects if they are there. An extension of this study with additional years of data that will include more treatment states and longer post periods will also increase power. This is an important avenue for future research when the data become available.

The early evidence presented in this paper suggest that school smartphone bans alone may not be enough to change the trajectory of youth mental health. Recall that 30 years ago eliminating cigarette smoking by young people was thought to be a very unlikely goal. However, today almost no youth smoke cigarettes. This goal took years and was achieved with the help of a wide variety of anti-smoking policies. Additional options that have been proposed to help reduce smartphone time are digital literacy and emotion regulation training to help students manage their technology use outside of school hours. More resources devoted to school mental health programs could be useful. Also, state or national level restrictions on social media platforms, AI sites and gambling sites have been discussed. Like the tobacco companies, the social media platforms are strongly opposed to this approach and argue that there are extensive safeguards to prevent misuse by young people and that use by young people is a parental prerogative. At this time, there is a need for more causal empirical evidence on the effects of each of these approaches.

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Table 1 NSCH Definitions, Weighted Percentages, Means and Standard Deviations of Outcome Variables for 2024 and for 2016						
Variable name	Definition	2024		2016		
		Mean	SD	Mean	SD	Δ in mean
Screentime	Hours spent in front of a TV, computer, cellphone excluding homework on weekdays.	2.82	1.1176	2.28	1.1873	0.55**
Psychological Wellbeing						
		Percent	SD	Percent	SD	Δ in percentage points
Interest	Shows interest and curiosity	79.24%	0.4056	98.54%	0.1200	-19.30**
Finish	Finishes what is started	80.23%	0.3983	96.49%	0.1839	-16.27**
Calm	Stays calm and in control when challenged	73.95%	0.4389	93.68%	0.2433	-19.73**
Concentration	No difficulty concentrating, remembering, or making decisions	87.95%	0.3256	89.89%	0.3015	-1.94**
Cares	Cares about doing well in school	84.55%	0.3615	96.60%	0.1813	-12.05**
Homework	Does all required homework	83.96%	0.3670	93.87%	0.2399	-9.91**
Friends	No difficulty making or keeping friends	94.59%	0.2262	95.42%	0.2091	-0.82**
Bullied	Bullied, picked on, or excluded by other	10.85%	0.3110	4.71%	0.2118	6.14**

[1] Data on bullied is from 2018 because the data 2016 and 2017 uses a different scale. Δ is the change from 2016 to 2024. **Indicates significance difference between 2024 and 2016 at 1% or less using a z test for proportions and z test for the mean.

Table 2 Weighted Correlations Data from 2016 through 2024 for the Psychological Wellbeing Variables							
	Interest	Finish	Calm	Concen-tration	Cares	Home work	Friends
Finish	0.43						
Calm	0.36	0.48					
Concentration	0.21	0.36	0.35				
Cares	0.38	0.49	0.40	0.31			
Homework	0.33	0.50	0.37	0.33	0.63		
Friends	0.18	0.23	0.26	0.36	0.21	0.22	
Bullied	0.07	0.01	0.00	-0.08	0.00	-0.01	-0.10

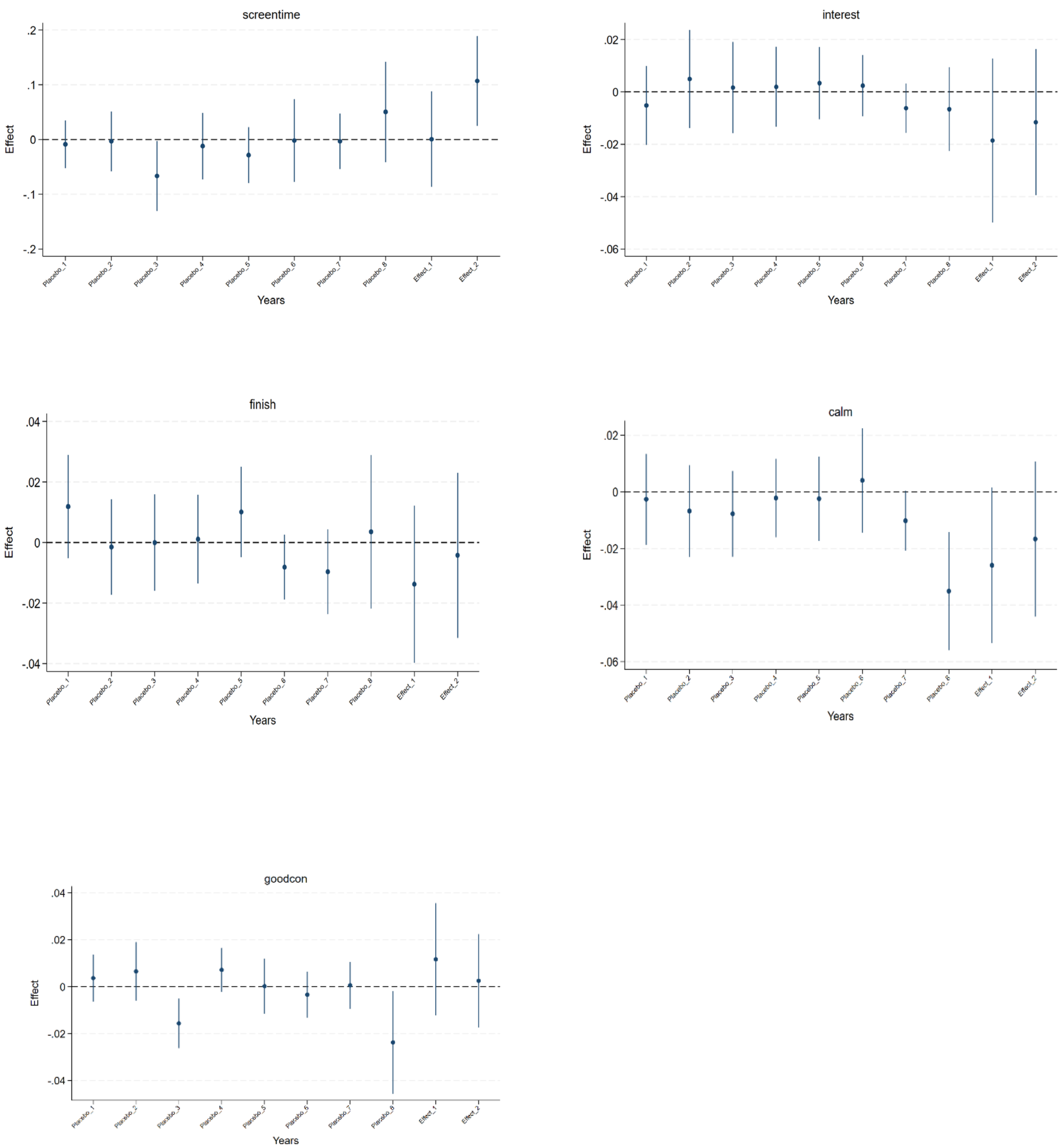
Table 3 Weighted Covariates			
		NSCH	US
Variable	Definition	Percent or Mean	Percent or Mean
White	White	71%	71%
Black	Black	15%	14%
Hispanic	Hispanic	27%	25%
Age	Age	14	14
Male	Sex	51%	51%
Married	Parents are married	76%	77%
Insurance	Child has health insurance	93%	95%
Education	Parent education in years	13.9	14.1
Hispanic includes all races. NSCH data for age 11 to 17. All states and DC included. US data from the U.S. Census Bureau, Current Population Survey. Census data for age 17 or less.			

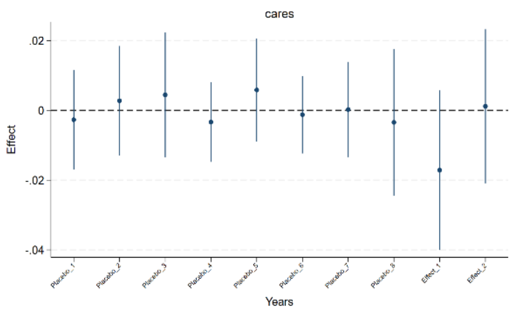
Table 4 State Level School Smartphone Policy		
State	Smartphone Ban Date Effective	Bell to Bell Ban
Florida (12)	July 2023	No
Indiana (18)	August 2024	No
Louisiana (22)	August 2024	Yes
Data from Ballotpedia. FIPS code in parentheses		

Table 5 SDID Results MDE and Pre-period Means						
Outcome Variable	ITT	SE	p-value	[1]Parallel Trends	MDE (+/-)	Pre-period Mean
Screentime	0.0256	0.0397	0.5201	Yes	0.1100	2.650
Psychological Wellbeing						
Interest	-0.0128	0.0118	0.2776	Yes	0.0330	0.8690
Finish	-0.0032	0.0126	0.7998	Yes	0.0340	0.8590
Calm	-0.0235	0.0128	0.0663	No	0.0350	0.8150
Concentration	0.0173	0.0117	0.1383	No	0.0300	0.8840
Cares	-0.0076	0.0096	0.4248	Yes	0.0280	0.8830
Homework	0.0055	0.0112	0.6205	Yes	0.0370	0.8730
Friends	-0.0121	0.0092	0.1901	Yes	0.0270	0.9400
Bullied	-0.0102	0.0114	0.3684	Yes	0.0360	0.3080

Treatment states are FL IN LA . SE are computed with the placebo method
[1]Parallel trends are "yes" if all pre-period coefficients in the event study are insignificant. The minimum detectable effect size (MDE) is the smallest tested increment with power of at least 80% using 1000 repetitions. The pre-period mean is calculated with unweighted aggregate data to match the data used in the SDID models. screentime is measured in hours.

Figure 1 Event Studies

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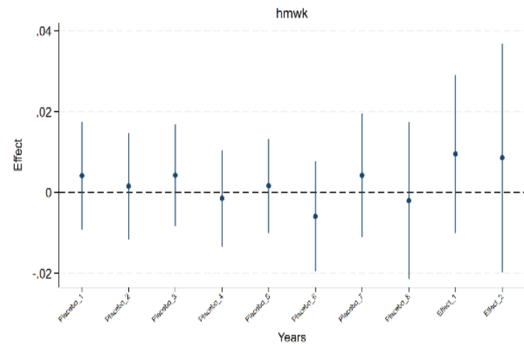
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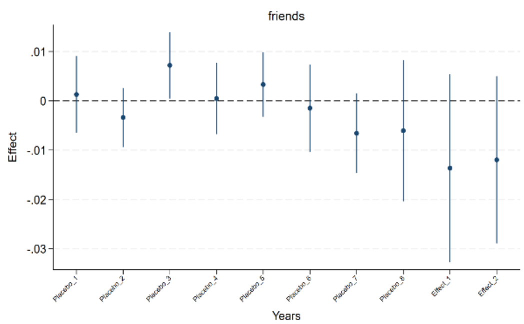
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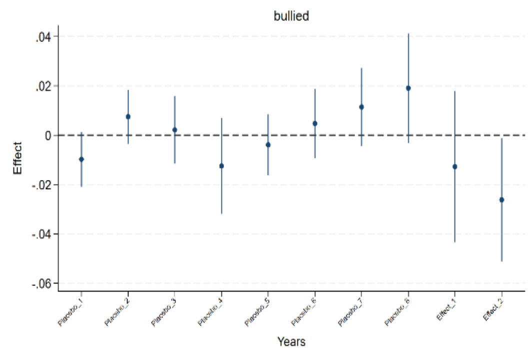
Years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Years

Appendix Tables

Table A.1 SDID Results Treatment states are FL IN LA, covariates included								
	FL only		IN only		LA only[1]		IN and LA	
Outcome	ITT	p-value	ITT	p-value	ITT	p-value	ITT	p-value
Screentime	0.0279	0.6121	0.1001	0.1247	-0.0524	0.4215	0.0232	0.6099
Psychological Wellbeing								
Interest	-0.0305	0.0698	0.0142	0.5027	-0.0034	0.8725	0.0049	0.7601
Finish	-0.0105	0.5421	0.0022	0.9298	0.0082	0.7461	0.0041	0.8082
Calm	-0.0226	0.1877	-0.0353	0.0790	-0.0104	0.6036	-0.0244	0.1257
Concentration	0.0233	0.1741	0.0003	0.9858	0.0148	0.4374	0.0112	0.4098
Cares	-0.0131	0.3409	0.0057	0.7463	-0.0085	0.6341	-0.0022	0.8800
Homework	0.0008	0.9558	0.0043	0.8469	0.0153	0.4921	0.0103	0.5033
Friends	-0.0245	0.0782	-0.0077	0.6504	0.0098	0.5646	0.0003	0.9814
Bullied	0.0053	0.7877	-0.0209	0.3241	-0.0305	0.1501	-0.0258	0.1019
[1]LA is a bell-to-bell ban state.								

Table A.2 Demographic Specific Models SDID Results, covariates included								
	Male		Female		White		Parents Married	
Outcome	ITT	p-value	ITT	p-value	ITT	p-value	ITT	p-value
Screentime	0.0498	0.2867	0.0398	0.4619	0.0215	0.6430	0.0244	0.4809
Psychological Wellbeing								
Interest	-0.0208	0.2731	-0.0122	0.5073	-0.0122	0.4594	-0.0142	0.3212
Finish	0.0206	0.1836	**-0.0336**	**0.0308***	-0.0053	0.7237	-0.0027	0.8366
Calm	-0.0143	0.4209	-0.0172	0.3526	-0.0207	0.1668	**-0.0304**	**0.0158***
Concentration	0.0239	0.0863	0.0100	0.5832	0.0210	0.1169	**0.0228**	**0.0384***
Cares	-0.0049	0.7358	-0.0102	0.4638	-0.0027	0.8068	-0.0128	0.2219
Homework	0.0226	0.1352	-0.0033	0.8185	0.0118	0.4293	0.0065	0.5948
Friends	-0.0112	0.3072	-0.0100	0.4067	-0.0083	0.3693	-0.0149	0.1209
Bullied	-0.0058	0.7000	-0.0189	0.2828	**-0.0238**	**0.0435***	-0.0180	0.1315
Relevant covariate is dropped in each specification. Coefficients in bold are significant at 5% or less.								

Table A.2 Continued								
	Age 11 to 13		Age 14-17		Low Ed Parent		High Ed Parent	
Outcome	ITT	p-value	ITT	p-value	ITT	p-value	ITT	p-value
Screentime	-0.0129	0.5647	0.0444	0.2802	0.0372	0.4024	0.0385	0.3897
Psychological Wellbeing								
Interest	-0.0051	0.3865	-0.0079	0.6707	-0.0130	0.4383	-0.0147	0.3803
Finish	-0.0003	0.9805	-0.0095	0.5231	-0.0111	0.4585	-0.0113	0.4483
Calm	-0.0214	0.0697	-0.0347	0.0599	-0.0182	0.2253	-0.0179	0.2298
Concentration	0.0139	0.0997	0.0121	0.3851	0.0127	0.2816	0.0126	0.2821
Cares	-0.0023	0.7580	-0.0058	0.6312	**-0.0311**	**0.0127***	**-0.0312**	**0.0121***
Homework	0.0002	0.9849	-0.0028	0.8504	-0.0233	0.0765	-0.0232	0.0757
Friends	-0.0044	0.5242	-0.0169	0.0917	-0.0105	0.3655	-0.0111	0.3363
Bullied	-0.0105	0.2755	-0.0159	0.2042	-0.0094	0.4925	-0.0094	0.4936
. Coefficients in bold are significant at 5% or less.