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Examining Ohio Public High Schools' Smartphone Policies' Academic Efficacy

Alexander Ament

St. Ignatius High School, USA

ABSTRACT

The academic efficacy of smartphone policies in high schools has been a hotly debated topic. Previous studies have quantitatively examined this topic in Britain, Spain, Norway, and Sweden, but they have somewhat conflicting results. Additionally, such a study has never been conducted in the United States, which has an altogether different schooling system than Europe. In the present study, I investigate (1) whether schoolwide smartphone policies increase students' academic performance and (2) whether school technology programs, like 1:1 iPad or laptop or Bring Your Own Technology/Device programs, mitigate this impact, if it exists. I use public academic records from the Ohio Department of Education and Workforce and collect survey information from 192 public high schools in Ohio. Using a difference-in-difference analysis, I conclude that my hypothesis was partially correct: smartphone policies effectively increase students' academic performance, but the results were inconclusive regarding technology programs. I advise that high schools across the nation continue to implement and maintain smartphone policies because they effectively remediate the detriments of smartphone use in schools.

Introduction

Smartphones have become an indispensable part of life, especially for teenagers: in 2020, 95% of teenagers reported having access to a smartphone (Vogels et al., 2022). Teenagers commonly bring their smartphone with them to school, and the presence of smartphones in high schools has attracted considerable attention both in the United States and throughout the world. For example, in 2015, New York Mayor Bill DeBlasio lifted New York city's categorical prohibition against smartphones in schools, claiming that it would reduce inequality (Allen, 2015); in 2018, France's education ministry banned smartphones in schools through the 9th grade (Rubin & Peltier, 2018); and in 2023, Britain's education ministry also announced a ban on smartphone use in schools, requiring them to be turned off on school premises at all times (Weale, 2023). According to a 2023 United Nations Education, Scientific, and Culture Organization (UNESCO) report titled *Global education monitoring report, 2023: technology in education: a tool on whose terms?*, one in seven countries already bans smartphones in schools; the report recommended that schools should ban smartphones if they are not already doing so (Global Education Monitoring Report Team, 2023). Simply put, smartphones in schools are attracting international interest.

The Debate About Smartphones

Regard for smartphones in schools has become more contentious as smartphones have become more pervasive. Smartphone use, especially in schools, is negatively correlated with academic performance (Chaudhury & Tripathy, 2018; Global Education Monitoring Report Team, 2023; Junco, 2012; Lepp et al., 2016) and inattentiveness (Zheng et al., 2014). Many, like the education ministries of France (Rubin & Peltier, 2018) and Britain (Weale, 2023), argue that school systems should restrict or outright ban smartphones in schools because doing so will increase students' academic performance. On the other side of the debate, some students, parents, teachers, and educators believe that smartphones can be a beneficial asset in the school system if used correctly (Machmud, 2018; Mrazek et al., 2021). To date, there has been no clear consensus in the United States about what should be done.

For detractors, the most expedient method to solve problemsome smartphones is usually to either restrict their school-time usage or outright ban them from the premises. According to Tandon et al. (2020), around 91% of United States high schools have a smartphone policy in place that prohibits smartphone use during class time; that number has likely risen with the increased attention that smartphones in schools now receive. In essence, schools that perceive smartphones as a problem have begun doing something about it.

While smartphone bans have become a go-to solution for school systems and education departments, there is not strong evidence to suggest they improve students' academic performance in the United States even though such bans are often implemented to combat declines thereof. To date, only four studies have ever been conducted which test whether smartphone bans actually boost academic performance (Abrahamsson, 2022; Beland & Murphy, 2016; Beneito & Vicente-Chirivella, 2022; Kessel et al., 2020), the results of which are somewhat conflicting. In particular, while three of the four studies (Abrahamsson, 2022; Beland & Murphy, 2016; Beneito & Vicente-Chirivella, 2022) concur on smartphone bans' positive academic influence, Kessel et al. (2020) find that smartphone bans had no academic impact in Swedish school systems; one of their primary theories regarding this discrepancy is that Sweden's high density of technology and historical integration of it into the classroom mitigate any benefits that a smartphone ban would otherwise provide. Furthermore, these studies were all conducted in European countries and school systems, which are considerably different from United States school systems (Diehl, 2020; Nord Anglia, 2023), so the effects of smartphone bans in those European studies may not translate to the same effects in the United States.

Purpose

Thus, the benefit and purpose of the present study is threefold: (1) it seeks to generally contribute to the existing literature on the academic effects of smartphone bans, given the current deficiency, (2) it attempts to discern whether smartphone policies are effective particularly in the United States, and (3) it attempts to either support or refute Kessel et al.'s (2022) theory that technological integration into the classroom mitigates the academic benefits that a smartphone ban would otherwise provide.

Literature Review

Method

I began an initial survey of the existing literature by first searching the following terms with Google Scholar: (1) "Examining cell phone policies in high school," (2) "Smartphone use policies in high school," and (3) "High school smartphone policies." Google scholars was chosen for its wide indexing scope. The first ten articles from each search term, totaling thirty, were evaluated for relevance. Of the thirty articles, Beland and Murphy (2016) and Kessel et al. (2020) were the most relevant studies because they examine the academic effects of smartphone bans in schools; in other words, they are closest to what the present study attempts to do. I discovered two other highly relevant sources through a forward and backward citation search using Scopus and ResearchGate: Abrahamsson (2022) and Beneito and Vicente-Chirivella (2022). I then conducted another forward and backward citation search with Abrahamsson (2022) and Beneito and Vicente-Chirivella (2022), but discovered no further articles of interest. Therefore, these four sources, Beland and Murphy (2016), Kessel et al.

(2020), Abrahamsson (2022), and Beneito and Vicente-Chirivella (2022), comprise the current repository of articles that quantitatively examine the academic effects of smartphone bans in schools.

Review

These four studies have somewhat conflicting results. Beland and Murphy (2016), Abrahamsson (2022), and Beneito and Vicente-Chirivella (2022) all conclude that smartphone bans positively impact students' academic performance. Kessel et al. (2020) dissent in their conclusion, finding that smartphone bans in schools had no significant impact upon students' academic performance.

Study Similarities

The studies share similarities in their designs and executions. They all (1) were conducted in Europe and investigated European school systems and (2) used a difference-in-difference (DID) empirical design (Abrahamsson, 2022; Beland & Murphy, 2016; Beneito & Vicente-Chirivella, 2022; Kessel et al., 2020). The former point's implication is that smartphone policies have not yet been empirically evaluated in the United States; the latter point provides a grounded quantitative analysis method for the present study to utilize.

Kessel et al. (2022)

Kessel et al. (2020)'s Swedish study is the most curious of the four studies because unlike the other three, they do not find that smartphone bans boost students' academic performance. They theorize that Sweden's historic fusion between technology and education and the current high density of technology in Sweden mitigate any benefits that may be gained from a smartphone policy: "as noted by Ott (2017), Swedish schools have long made large investments in digital technology […] Schools are also encouraged to develop pedagogical uses for digital technology as tools for learning in school practice;" it is important to note, however, that "mobile phones have typically not been included" in the technology incorporated into the classroom, but "nevertheless, students have brought mobile phones to school and used them for schoolwork" (Kessel et al., 2020, p. 27). In the present study, I seek to support or refute this theory.

Research Question and Hypothesis

I now derive my driving research question and subsequent hypothesis: do smartphone bans in public high schools significantly increase students' academic performance, and if so, does technological integration mitigate or enhance this increase? My hypothesis to this question is: yes, smartphone bans significantly increase students' academic performance, but technological integration mitigates this increase.

Method

State Selection

In the United States, analyzing education at the national level is ordinarily not practical because many educational policies are implemented at the state level. Unlike Britain, Sweden, Norway, and Spain where previous studies on the topic have been conducted, the United States does not have a national repository of student academic information that would suffice for the present study. Furthermore, aggregating data from all 50 U.S. states is unfeasible because each state has different academic metrics and curricula. The present study is therefore practically confined in scope to a single state.

The Case for Ohio

While no state is a perfect representation of the entire U.S., Ohio has often been recognized as a relatively accurate national cross-section. Indeed, Ohio's diverse political, culture, and socioeconomic microcosm has earned it a certain aphorism in politics: "as goes Ohio, so goes the nation." Out of all 50 U.S. states, Ohio is the most accurate predictor for the United States' quadrennial presidential election: since 1900, Ohio has voted for the winning presidential candidate 90.3% of the time and voted for the winning candidate in every presidential election between 1964 and 2016 (Presidential election accuracy data, n.d.).

While I am not examining the political composition of the nation, scholars have long acknowledged that high school education influences political participation (Willeck & Mendelberg, 2022; Campbell, 2006). Thus, underlying Ohio's diverse and generally representative political sphere may be an equally diverse and representative educational system.

Empirical Design

I attempt to correlate two groups of data: high schools' smartphone policies and practices, and academic trends. Secondarily, I attempt to relate schools' technological integration practices to both of these groups.

High School Smartphone Survey

Collecting high schools' smartphone policies is challenging because there is no centrally compiled repository of smartphone policies. To collect these, I surveyed high schools across Ohio. According to the Ohio Educational Directory System (OEDS) (OEDS reports, n.d.), a data repository managed by the Ohio Department of Education and Workforce (ODEW), there are 814 schools in Ohio that are considered public high schools. I accepted the OEDS to be the authoritative source on the matter. I excluded 67 schools from the population because they were considered "online," "virtual," "digital," or "remote learning" and excluded another school, the Cardinal Autism Resource Education School, because it is a specialized school. The total number of schools in the population after adjusting for exclusions, then, is 746.

The OEDS (OEDS reports, n.d.), at the time of access, contained principals' emails for 323 of the 746 schools (43.30%). I individually retrieved another 342 (45.84%) email addresses, but I was unable to find principals' email addresses for 81 (10.88%) schools. In total, there are 665 (89.14%) high schools in the population for which a principal's email address is associated. I then developed a survey based on the one used by Beland and Murphy (2016) and emailed it to the principals' email addresses of those 665 schools. A follow-up email was sent two weeks after the initial survey to any principals who did not reply to the original. In total, 192 schools responded to my survey. A replica of the sent survey can be found in the supplemental files.

Academic Data

To collect data on schools' academic trends, I used the ODEW. The ODEW publishes an aggregated version of schools' academic results at the Ohio Department of Education report portal (n.d.). That page contains a subpage (Test results, n.d.) that includes student test results disaggregated by the school level, which are what I use to quantify a school's academic performance trends.

The test results data is aggregated, not raw, data. When a student takes the ODEW's annual standardized test, they receive a score. Based on that score, they are assigned an equivalent "scoring rank," which acts as an academic qualifier. From lowest to highest, the scoring ranks are: limited, basic, proficient, accomplished, advanced, and advanced plus. Descriptions for each of these ranks is tabulated in Table 1. The ODEW then tabulates the percentage of students, grouped by school, who score each rank for a given test. These percentages are the data which the ODEW publishes.

Table 1. Rank Descriptions for Ohio Standardized Tests

Proficiency Rank	**Description**
Limited	has trouble identifying important details of a story, understanding the meaning of common words and phrases in a text, and stating a clear written opinion supported by facts.
Basic	can recognize a main idea and several important details of a story, understand some common words and phrases in a text, and loosely organize general facts to support a written opinion.
Proficient	can describe the main idea of a story, support it with details, understand most familiar words and phrases as they are used in a text, and cite specific facts to support a written opinion.
Accomplished	explains how an author expresses a main idea using specific details, figures out the meaning of unfamiliar words in a text, and organizes facts into groups to support a written opinion.
Advanced	can ask and answer complex questions about the main idea of a story, tell the meaning of figurative language in a text, and organize facts in a logical order to support a written opinion.
Advanced Plus	[no description provided]

Descriptions are quoted verbatim, except for the Advanced Plus rank, for which the ODEW provides no description. Adapted from Ohio Department of Education. (2022). Understanding Ohio's state tests score reports 2022-2023. oh-ost.portal.cambiumast.com/resources/online-systems-resources/understanding-ohio's-state-tests-reports/2022-2023-understanding-score-reports

For student privacy, the ODEW obfuscates numbers for which there are less than ten students in the given category by expressing that particular value as “<10” in all its data. I treat these values in the dataset as missing, meaning they are not factored into any analyses.

In addition to downloading academic data from the ODEW, I also downloaded enrollment data from the ODEW’s Enrollment data (n.d.) webpage to use for analyzing sample fitness. I then recombined the academic and enrollment data with my survey data.

Difference-in-Difference Analysis

The four previous studies that examined smartphone policies used a DID method for their analysis, and for good reason: DID is a quasi-experimental design that utilizes longitudinal data to estimate the effect of an intervention. I find no reason to deviate from the current practice.

Treatment and Time Variable Creation

Implementing a DID analysis requires distinguishing between treatment and control observations and pre- and post-treatment observations. I first created two dummy variables to classify observations as either pre- or post-treatment and either the treatment or control group. Observations for which the year of policy implementation is prior to the reported year were assigned a value of 1 for the time variable; observations for which the year of policy implementation was after the reported year or the school reported not having a policy were assigned a value of 0 for the time variable; observations for schools that did not respond to the survey were assigned a value of ".".

Observations for schools that either had no policy or who had a policy that varied widely and discretionarily (not schoolwide) were assigned a value of 0 for the treatment variable. Observations for schools that had a policy were assigned a value of 1 for the treatment variable. Observations for schools that had not reported on the survey were assigned a value of "." on the treatment variable.

"As the treatment is staggered in time, schools that have not yet or never introduced a ban will serve as control groups" (Kessel et al., 2022, p. 13). The present study does the same.

Analysis Programming

I then conducted a difference-in-difference analysis using Stata's didregress command. Stata uses the standard DID model, $Y_{ist} = \gamma_s + y_t + z_{ist}\beta + D_{st}\delta + \varepsilon_{ist}$. Specifically, the Stata commands I used looked like this:

```
didregress ([rank] [covariate]) (treatafter), group(irn) time(after)
```

Where [rank] and [covariate] are placeholder keywords, the former for the proficiency ranks listed in Table 1 and the latter for various covariates mentioned later on; treatafter is the time and treatment interaction variable, irn is a unique school-level identifier, and after is the time variable. This command was iteratively repeated with the scoring ranks listed in Table 1 and the covariates described in Table 2 below.

Table 2. Analysis Covariates.

Covariate Name	Description
Method	Controls for how schools enforce their smartphone policies.
Leadership Change	Controls for whether there were any significant changes in faculty or staff at the time of policy implementation.
Compliance Level	Controls for principal's perception of how well students comply with the smartphone policy.
Previous Strictness	Controls for principal's perception of how relatively strict the school's previous smartphone policy was.
Tech Programs	Controls for whether the school has a 1:1 device program or a Bring Your Own Device program.

Results

Survey Results

In total, 28.87% (n=192) of principals to whom a survey was sent responded, representing 25.74% of the total population. I conducted several sample fitness analyses by the categories available from the ODEW, including enrollment by grade, ethnicity, gender; English learner, disability, and economic disadvantage status; and student rankings. The Figures below graph those fitness analyses.

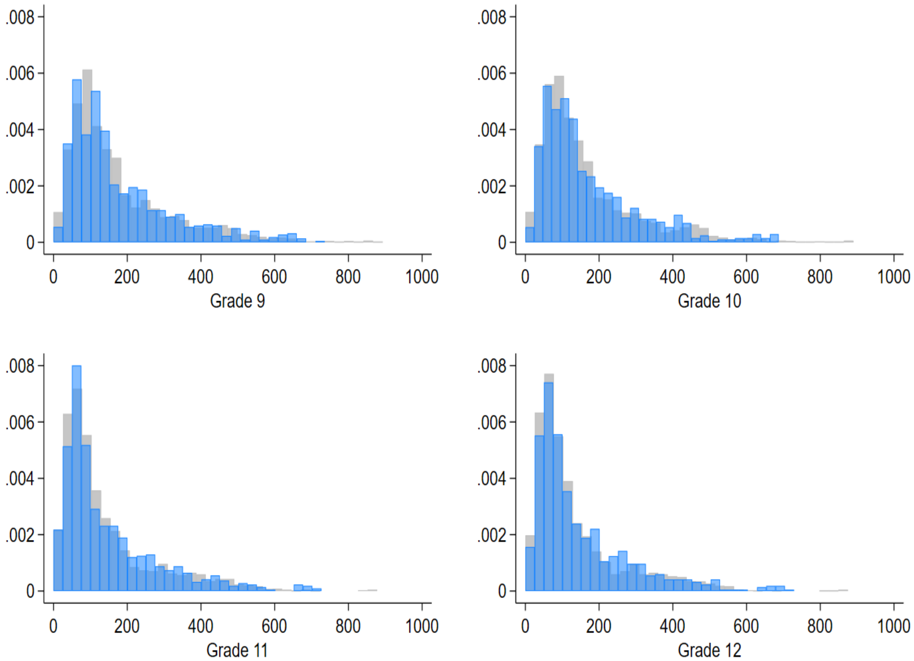

Figure 1. Probability Distributions for Number of Students in Ohio Public High Schools by Grade. The light-blue semi-transparent distribution models the sample while the solid gray distribution graphs the population. The sample Grade 9 distribution appears bimodal, but this may be caused by Stata's binning process.

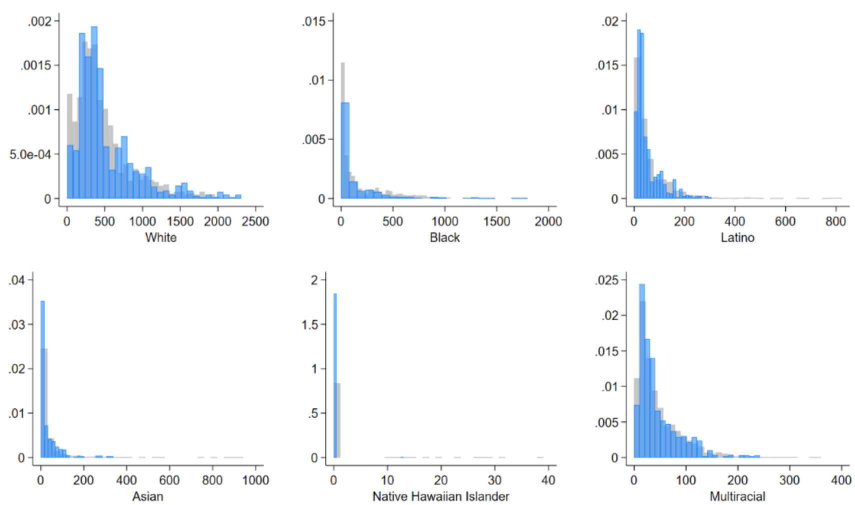

Figure 2. Probability Distributions for Number of Students in Ohio Public High Schools by Ethnicity. The light-blue semi-transparent distribution models the sample while the solid gray distribution models the population. The sample White distribution appears unusually leptokurtic.

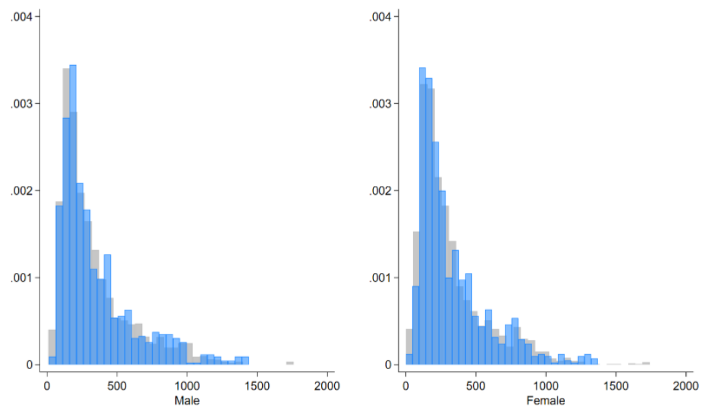

Figure 3. Probability Distributions for Number of Students in Ohio Public High Schools by Gender. The light-blue semi-transparent distribution models the sample while the solid gray distribution graphs the population.

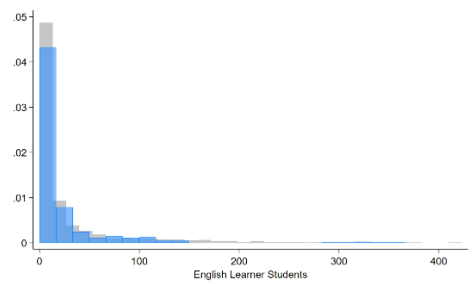

Figure 4. Probability Distributions Number of English Learner Students in Ohio Public High Schools. The light-blue semi-transparent distribution models the sample while the solid gray distribution graphs the population.

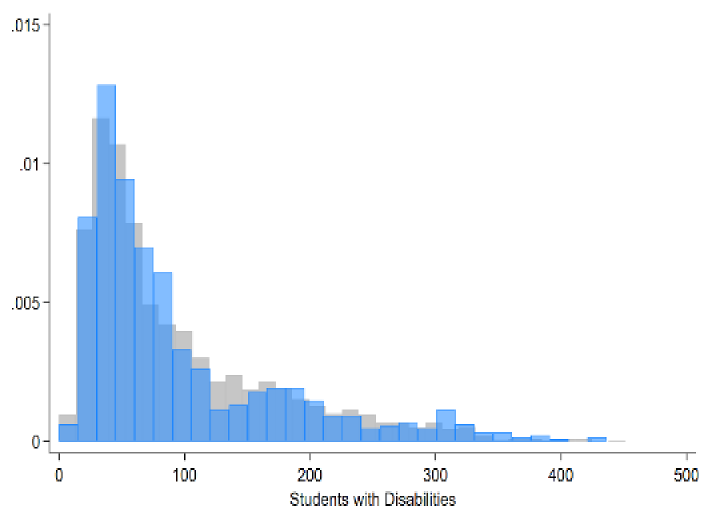

Figure 5. Probability Distribution for Number of Students with Disabilities in Ohio Public High Schools. The light-blue semi-transparent distribution models the sample while the solid gray distribution graphs the population.

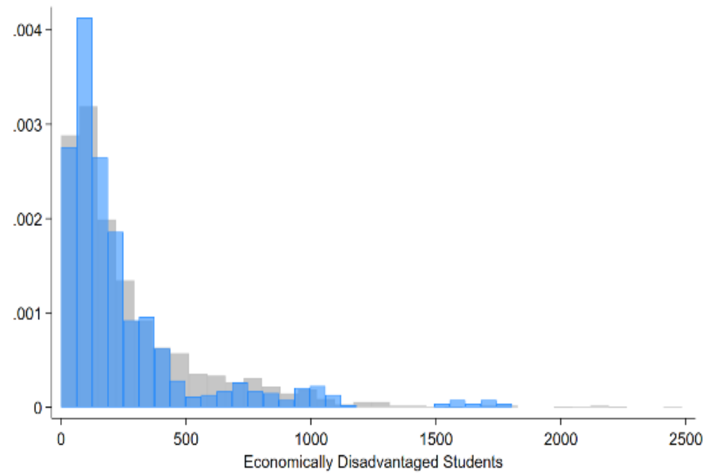

Figure 6. Probability Distribution for Number of Economically Disadvantaged Students in Ohio Public High Schools. The light-blue semi-transparent distribution models the sample while the solid gray distribution graphs the population.

Overall, the sample appears largely representative of the population.

Difference-in-Difference Results

The initial DID results suggest that smartphone policies do not significantly impact the percent of students scoring in any category given that only one result is statistically significant — Table 3 tabulates the predicted coefficients by rank and controlled covariate. Indeed, that only one rank in Table 3, limited, indicates statistical significance attenuates the credibility of that significance: because the percentages in each scoring rank are related it would be expected that a significant change happens in at least two ranks: the rank from which students are leaving and the one into which they are going.

Table 3. Difference-in-Difference Coefficients Matrix.

Controlled Covariate	Limited	Basic	Proficient	Accomplished	Advanced	Advanced Plus
No Covariate	1.9331	.6737	-1.4566	.6608	-1.2513	-.6898
Method	3.6364*	-1.656	-1.5166	.3073	-2.2233	-.6898
Leadership Change	1.9331	.6737	-1.4566	.6608	-1.2513	-.6898
Compliance Level	1.9331	.6737	-1.4566	.6608	-1.3513	-.6898

Previous Strictness	.1720	.3146	-1.6912	.7559	-1.1495	-1.0039
Tech Programs	1.9482	.6848	-1.4851	.6485	-1.2741	-.6898

* p < .05

I then disaggregated the data by several of the categorical variables gathered from the conducted survey: severity, method, and technology program. That data is tabulated in Table 4.

Table 4. Difference-in-Difference Coefficients Disaggregated by Policy Variables.

Disaggregating Variable	Limited	Basic	Proficient	Accomplished	Advanced	Advanced Plus
Aggregate	1.9331	.6737	-1.4566	.6608	-1.2513	-.6898
Severity 1	1.3653	.5364	-1.6513	.9888	-1.0644	-.416
Severity 2	2.1595	.6558	-.4443	-.2479	-1.4404	-2.2321****
Method 1	-.5020	1.7181	-2.8984*	3.2078**	5.5894**	—
Method 2	-4.0417	1.5626	-.9415	3.0292*	6.1398***	—
Method 3	5.8745*	.4991	1.1091	—	—	—
Tech Program	.6423	1.1861**	-4.2818****	-1.1403****	-1.4295	—

* p < .05, ** p < .01, *** p < .005, **** p < .0005

When interpreting Table 4, the disaggregating variable indicates the criteria for inclusion in that analysis; e.g., Severity 1 indicates the DID coefficients *only* for schools that prohibit students from being on their phones during class time. Severity 2 indicates the same for schools that prevent students from being on their phones the entire day. Method 1 indicates an out-of-sight, out-of-mind policy: students may keep their phones on them so long as they are silent and invisible. Method 2 requires that students remove their phones from their person during the day: either into a locker of phone caddy. Method 3 requires that schools actually collect students' phones.

The results indicate that while severity — that is, the timewise strictness of schools' smartphone policies — does not matter in a statistically significant way, both methods 1 and 2 in Table 4, when controlled for, increase in a statistically significant way the percent of students scoring basic, accomplished, and advanced, while less students scored limited and proficient, suggesting that the method schools use to enforce a smartphone policy is more important than the time periods during which students may not access their phones.

That the disaggregated data in Table 4 demonstrates smartphone policy efficacy does not seem to comport with the original data in Table 3; however, in Table 3, I defined the treatment group by the timewise restrictiveness of policies whereas Table 4 demonstrates statistical significance with regard to the method. I adjusted for this discrepancy by redefining the treatment variable: observations with a method of 0 (no school-wide smartphone policy) or "." (missing) were assigned a treatment value of 0 while observations with a method of 1, 2, or 3 were assigned a treatment value of 1. I then reanalyzed the data using the same DID technique used for Table 3; the results are tabulated in Table 5.

Table 5. Difference-in-Difference Analysis Coefficients Matrix, Redefined Methodwise.

Controlled Covariate	Limited	Basic	Proficient	Accom-plished	Advanced	Advanced Plus
No Covari-ate	-1.2030	1.6324	-2.1691	3.1387**	5.8141***	—
Method	-1.5345	1.5401	-2.1114	2.2950****	4.3043****	—
Leadership Change	-1.2030	1.6342	-2.1691	3.1387**	5.8141***	—
Compliance Level	-1.2030	1.6342	-2.1691	3.1387**	5.8141***	—
Previous Strictness	-7.3037****	2.8422****	-1.513	2.2828****	4.3807****	—
Tech Pro-grams	-1.1961	1.6441	-2.9141	3.1284**	5.8020***	—
All Covari-ates	-7.3160****	2.8542****	-1.5479	2.6968****	4.3669****	—

* p < .05, ** p < .01, *** p < .005, **** p < .0005

The results in Table 5 indicate that smartphone policies produce statistically significant positive effects. There was an average decrease of 7.316 percentage points for students scoring basic, a 2.8542 average point increase for students scoring limited, 1.5479 decrease in proficient, and a 2.6968 and 4.3669 point increase for advanced and advanced plus, respectively. In other words, students seem to be moving up the ODEW's scoring ranks. Thus, it seems smartphone policies work.

While the results in Table 5 are technically measured in percentage points, they do not represent the percent change but rather the actual difference, so I calculated the actual percent changes, and the results are noteworthy. Tables 6 calculates the percent changes by mean and median.

Table 6. Percent Change in Scoring Ranks by Mean

Rank	Limited	Basic	Proficient	Accomplished	Advanced	Advanced Plus
Point Change	-7.3160	2.8542	-1.5479	2.6968	4.3669	—
Mean	31.10	22.05	36.24	22.03	23.15	—
% Change (Mean)	-23.5%	12.9%	-4.2%	12.3%	18.9%	—
Median	25.60	21.21	-34.72	19.19	20.33	—
% Change (Median)	-28.6%	13.4%	-4.4%	14.1%	21.5%	—

The results in Tables 6 suggest that the two most impacted groups are the lowest performing students (the number of students ranking limited decreased by 23.5%) and highest performing students (the number of students ranking advanced increased by 18.9%) whereas it appears average students are least affected (the number of students ranking proficient decreased by only 4.2%). These percent changes conflict with the previous

European studies, which found that as students perform better, smartphone bans become less effective. I discuss possible theories and implications in the Discussion section.

While the present study aims to evaluate the impact of technological integration on the efficacy of smartphone policies, only one school in the sample reported not currently having a technology program. Because for the sake of brevity the sent survey did not include a question asking for a school's year of implementation for their technology program, I expected to establish a correlation by disaggregation; but such a correlation would be extremely tenuous with only one school in the control group. My determination on that matter is therefore inconclusive.

Discussion

Confirmation of Hypothesis and Previous Studies

My original hypothesis was not proven entirely wrong: smartphone policies do positively impact students' academic performance, but I was unable to test whether technological integration affects smartphone policies' efficacy. The present study confirms the general results of Beland and Murphy (2016), Abrahamsson (2022), and Beneito and Vicente-Chirivella (2022) in finding that smartphone policies positively impact students' academic performance. The present study also supports the idea that the effects of smartphone policies exist across continents and schooling systems, at least between the United States and Europe. It failed, however, to support or refute Kessel et al.'s (2020) results.

DID Assumptions

I review the assumptions necessary to conduct a DID analysis and evaluate their satisfaction. While a DID analysis is causal and therefore requires exchangeability, positivity, and a Stable Unit Treatment Value Assumption (SUTVA), it also uniquely requires parallel trends prior to treatment.

Exchangeability and positivity respectively require that (1) the sample is representative of the population and (2) that there exists a control group. The second is easily satisfied in that a DID analysis inherently requires a control group. In the present study, schools that do not have a smartphone policy are the control group. In total, the present study's data consisted of 3,648 observations for schools that do not have a smartphone policy and 35,772 observations for schools that have a smartphone policy.

Exchangeability is also satisfied. Figures 1-6 above illustrate sample fitness tests. Their conformity demonstrates that the values in the sample closely match the population and are therefore exchangeable.

SUTVA is more difficult to evaluate. It consists of three main assumptions: (1) there are no spillover effects between the treatment and control groups, (2) assignment of treatment is unrelated to independent variable, and (3) there are only two potential and mutually exclusive outcomes. The third point is obviously true: either students' test scores increase, or they do not.

The second assumption is tenuously satisfied. In the survey, the most commonly reported reason for a new policy implementation was a change in administrators, usually the principal. While it is possible that a principal may be ousted or decide to leave as a result of students performing poorly, it is not among the most common reasons for changes in a school's administration (Levin & Bradley, 2021). Therefore, at least for schools that reported a reason for policy implementation, such implementations were presumably unrelated to students' test scores.

Evaluating parallel trends also appears tenuous. Figure 7 displays scoring means with ranges from 2019 to 2023. Several of the graphs appear to violate the parallel trends assumption at times; however, such violations mostly occur in or around 2020, which was the year that COVID-19 was declared a public health emergency, and testing across the state of Ohio was interrupted. This suggests that the parallel trends violations seen in Figure 7 may not be substantive but rather caused by an interlocutory disruption in testing.

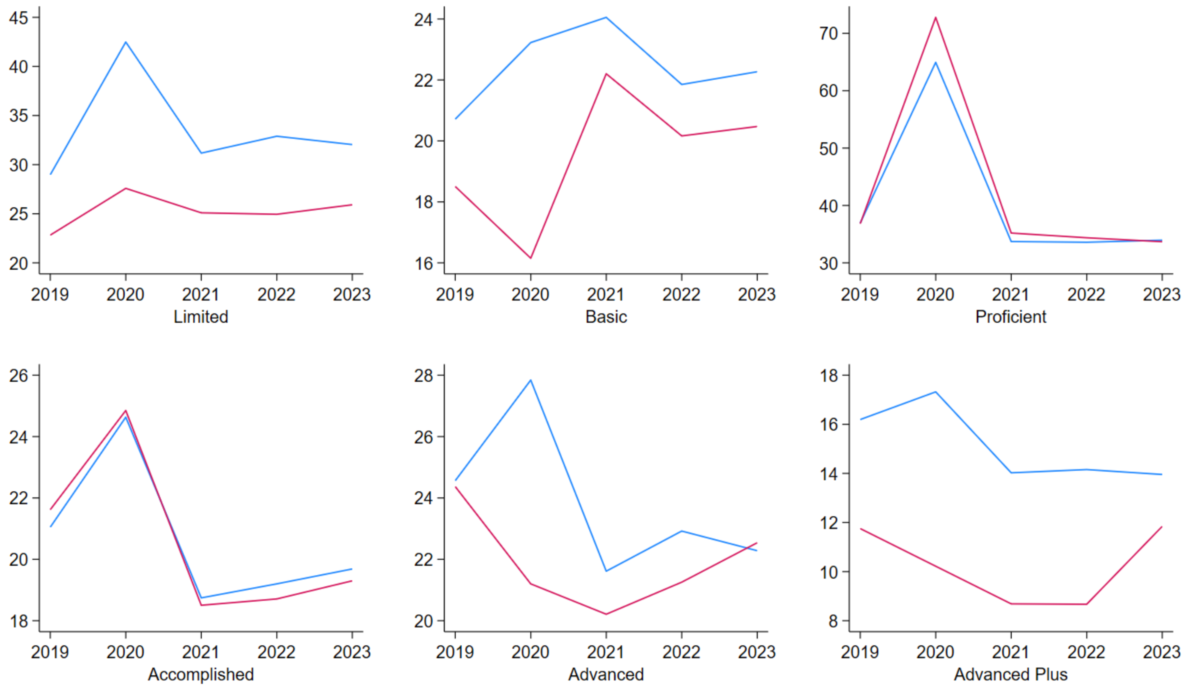

Figure 7. Percent Scoring Means 2019-2023. The blue line indicates control schools (those with no schoolwide smartphone policy) and the red line indicates treatment schools (those with a schoolwide smartphone policy).

Study Discrepancies and Theories

The present study found that the most impacted groups of students are the highest and lowest performing, whereas the number of average students changed very little. In other words, it seems students on the extreme ends of the academic spectrum benefit from smartphone policies more than students in the middle do. This contrasts with Beland & Murphy's (2016) findings that indicate smartphone bans provide negligible benefits to high achieving students.

The seeming lack of benefit to average students may be a consequence of the ODEW's data obfuscation. Because the ODEW only provides aggregated numbers for students scoring in each rank, it is not possible to track individual students. The percent changes in Tables 8 and 9 only indicate the overall change in the number of students in a particular rank. It does not actually indicate how many students changed ranks. Thus, it is possible that a large number of students moved into the proficient rank but a slightly larger number of students moved into a higher rank, therefore manifesting a deceptively underwhelming difference of around 4% that fails to convey the true change effected by a smartphone ban. The lowest and highest ranks cannot have this problem because it is not possible for students to move "up" into limited or "down" into advanced. If this theory is true, it suggests that smartphone bans actually have a much greater impact than can be represented by aggregate data. To get a true sense of how many students benefit from a smartphone ban, then, would require tracking individual students' improvements like Beland & Murphy (2016) were able to do.

This data obfuscation theory does not entirely explain the discrepancies between studies. While that theory may be able to correct the seemingly parabolic impact on students to a more linear concept as suggested by Beland & Murphy (2016), it does not explain why there was such a large increase in the number of students scoring accomplished and advanced when Beland & Murphy (2016) found that "banning mobile phones [...] has no significant impact on high achievers" (p. 17). This discrepancy regarding high achieving students may be explained by another potential phenomenological effect caused by the ODEW's aggregate data. When looking at the percent increases in Tables 8 and 9 for the accomplished and advanced ranks, those percentages indicate an increase in students achieving those ranks rather than an increase in the academic achievement of students already in those ranks. Therefore, the increases seen in the accomplished and advanced ranks may be more indicative of average students now scoring above average rather than high performing students performing more highly. Indeed, given the granularity of the ODEW's data, it is impossible to determine the academic progress of students who do not change scoring ranks and similarly impossible to actually determine whether students in the advanced rank improve since there is no higher rank to move to.

Theory

If the seeming increase in students scoring advanced is not solely a phenomenological illusion and does indicate to some degree academic improvement by high-performing students, then the results of the present study are truly in conflict with Beland & Murphy (2016). My propounded explanation theorizes that these discrepancies are caused by differences between American and European schooling systems.

This theory first requires some understanding of why smartphone policies cause the academic increase that they do. Logically and theoretically, smartphone policies only work when students actually use their phone in school. Phone use in school causes academic decline; in turn, smartphone policies remediate this effect, giving the appearance of an academic boost, but smartphone policies only increase a student's academic performance to the extent that a smartphone damages it: smartphone policies do not make students "smarter," only less distracted. A smartphone policy can be, though is not necessarily, more effective on students who use their phone more and therefore suffer the detriments of smartphones more. High performance students tend to use their smartphone less than lower performing students (Lepp et al., 2017). Therefore, smartphone bans are likely less efficacious with high-performing students, and this decreased effectiveness accords with Beland and Murphy's (2016) findings, but begs the question of the present study: why are high-performance Ohio public high school students receiving such a large benefit? In other words, what significant difference exists between this study and the previous studies that would generate such effects? I argue that it is because of differences between American and European schools, given that the previous four studies were all conducted in Europe.

Generally, American school systems are considered to be more social at the cost of a focused and more efficient European education. As Snowden (1959) notes, "[t]he American secondary school, then, [...] has a decidedly social character, whereas, the European has an emphasis, largely cultural" (p. 346). This "social character," Snowden claims, comes at the expense of a more elite education; whether Snowden's argument is true, the United States does lag behind other nations when it comes to schooling (Palla et al., 2018). A lack of sociality among European secondary schools also seems to be evident among non-scholarly sources, with many particularly noting a lack of sports teams and other school-sanctioned extracurriculars (Duli, 2019; Grajera, 2023; Nord Anglia, 2023; Vesterlund, 2023; Yucel, 2021).

Given the more social nature, the detriments conferred to high performing students in the United States may be a result of peer pressure. Peer pressure is a known contributor to problematic smartphone use among teens (Huang et al., 2021). Thus, high-performing students, while individually resistant to smartphone overdependence, may nevertheless feel inclined to use their smartphones more because of peer pressure. As a result, smartphone policies remain effective for high-performing students; but because European school systems lack the sociality found in American school systems, they may similarly lack the peer pressure that causes high-performing American students to overuse their smartphones. This lack of smartphone use by high-performing students may then lead to a less pronounced effect by smartphone policies in European high schools as seen in Beland and Murphy (2016).

Conclusion

Summary

Smartphones are becoming a topic in schools that must be dealt with, given their pervasiveness and proven negative effects. While prior research has made it clear that smartphones cause problems in school, up until now, there has not been a clear solution because only four studies have ever actually tested whether smartphone policies in schools work (Abrahamsson, 2022; Beland & Murphy, 2016; Beneito & Vicente-Chirivella, 2022; Kessel et al., 2020); but not only have these studies generated conflicting results, none of them were conducted in the United States, which has remained until now an unexplored frontier in schools' smartphone regulations.

In the present study, I sought to bridge this gap and determine (1) whether smartphone policies increase or decrease students' academic performance and (2), if such an effect exists, whether technology programs enhance or mitigate it.

To do this, I conducted a DID analysis using data I obtained from a survey modeled off of Beland & Murphy's (2016) and sent to 665 public Ohio high schools and publicly available test results from the ODEW's Test results (n.d.) page. After 192 schools completed the survey, I conducted a difference-in-difference analysis, the results of which strongly indicate that smartphone policies do increase students' academic performance, therefore effectively remediating the detriments caused by smartphone use in schools, though the findings were inconclusive on which smartphone policy is most effective.

Policy Advice

Because the present study affirms that smartphone policies increase students' academic performance, I advise that all high schools continue to implement and enforce smartphone policies. Additionally, more research should be conducted to further hone the efficacy of smartphone policies.

Limitations and Future Research

The present study has several limitations. First, it was conducted in Ohio and with public schools. While Ohio is a relatively diverse state, it cannot possibly represent the entire United States, so further studies should nevertheless seek to confirm the results of the present study by replicating it in other states. Additionally, the study exclusively used public schools, so the results may not translate to private and charter schools. Second, because the ODEW obfuscates statistics for which the number of students are less than ten, minorities tend to be underrepresented in the dataset. Third, the present study only uses data starting in 2019; the results are relatively short-term, so the literature would benefit from a more longitudinal study. Fourth, participants were not randomly selected, so there may be unobserved volunteer bias, although the sample fitness analyses conducted help mitigate this. Fifth, because there are so few schools which have not implemented a technology policy, trying to establish a correlation through disaggregating results will likely fail in the future as it has here. Future researchers may need to conduct a DID analysis for technology programs using data from years prior to 2019 and with a larger or different sample so that they can adequately capture enough control data. Sixth, while the data altogether was sufficient to use a DID analysis, there was not enough data for each individual smartphone policy implementation method to run separate DID analyses in figuring out which policy is best. This is in part a violation of SUTVA, which requires consistent treatment across all groups and further attenuates the results to some degree. Future researchers should more closely examine specific policies to determine whether some are more effective than others.

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