THREE ESSAYS IN THE ECONOMICS OF EDUCATION

By

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ABSTRACT

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Chapter 1: School Cellphone Bans and Student Substance Abuse: Evidence From California Public High Schools

Following high profile school shootings and the September 11th terrorist attacks, public concern over school emergency preparedness prompted the California State Legislature in 2003 to overturn a statewide ban against student possession of cellphones on campuses. After the repeal of the prohibition, which had been established in 1988 to curb drug dealing, school districts were allowed individually to either continue banning phones or modify their device policies; most opted over time to accommodate usage during certain hours of the day. Using fixed effects regression analysis clustered at the district level, I exploit variation in the timing of district policies to estimate the impact on substance abuse from lifting school cellphone bans. Results provide evidence that allowing students to use cellphones at school increases opportunities to obtain and abuse controlled substances; this effect is particularly pronounced in the incidence of marijuana smoking among 9th graders, who exhibit a 1.3 percentage point higher chance of reporting past-month marijuana use in the year a ban is lifted. Factors involved may include the capability that the technology provides to negotiate high risk interactions in private and to seek out and contact a relatively small number of drug suppliers; as is thus to be expected, no impact is found on the consumption of cigarettes, which can be obtained legally by a large proportion of high schoolers.

Chapter 2: Impact of Internet Access on Student Learning in Peruvian Schools (with Leah Lakdawala and Eduardo Nakasone)

We investigate the impacts of school-based internet access on pupil achievement in Peru, using a large panel of 5,903 public primary schools that gained internet connections during 2007-2014.

We employ an event study approach and a trend break analysis that exploit variation in the timing of internet roll-out up to 5 years after installation. We find that internet access has a moderate, positive short-run impact on school-average standardized math scores, but importantly that this effect grows over time. We provide evidence that schools require time to adapt to internet access by hiring teachers with computer training and that this process is not immediate. These dynamics highlight the need for complementary investments to fully exploit new technological inputs and underscores the importance of using an extended evaluation window to allow the effects of school-based internet on learning to materialize.

Chapter 3: Discretionary School Discipline Policies and Demographic Disparities

In 2014, California passed the law AB 420, becoming the first state to limit the use of school suspensions and expulsions as punishment for "willful defiance" — a subjectively determined offense thought by state lawmakers to lead to racial disparities in discipline. In this paper, I overview the state's recent (from 2012-2017) progress in reducing exclusionary discipline and note effects on disproportionality, here characterized as the difference between a given group's proportion of discipline and its proportion of enrollment. Using identification by treatment intensity, based on schools' pre AB 420 proportion of discipline attributable to willful defiance, I also attempt to gauge the effectiveness of reducing punishment of defiance in mitigating disproportionality. School level administrative data from elementary schools (spanning kindergarten through 5th grade) indicate that exclusionary discipline has considerably declined throughout the period. On the other hand, it does not appear that AB 420, along with lower willful defiance related discipline, has reduced disproportionality.

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The opinions expressed are those of the author. All errors are my own.

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CHAPTER 1

SCHOOL CELLPHONE BANS AND STUDENT SUBSTANCE ABUSE: EVIDENCE FROM CALIFORNIA PUBLIC HIGH SCHOOLS

1.1 Introduction

In the United States today, educators and policymakers increasingly allow secondary school students to carry and use cellphones on campuses. In easily the most visible instance of this trend, the New York City Department of Education—the nation's largest school district by enrollment—lifted a decade-long campus cellphone ban in March 2015. The change, not without controversy, followed longstanding complaints by parents and civil rights advocates that the policy hampered emergency preparedness, reduced flexibility in scheduling after school activities, and unduly burdened low income students, for whom the fees charged by daytime phone storage services caused appreciable hardship (Harris and Schweber, 2015).[1] With mobile devices' increased ubiquity and role in household communication, issues of safety and convenience have elsewhere also weighed heavily in the determination of reasonable device policies.

Furthermore, the emergence of more sophisticated models featuring ever broader capabilities has fueled interest in using student devices to address unmet needs in learning. Hence, a growing number of teachers in Los Angeles Unified, the second largest school district by enrollment, sidestep official regulation by conducting class exercises that incorporate cellphones (Gerson, 2015). At a specially designated pilot school within LAUSD, software such as Google Classroom and the Remind app enhances student-teacher communication and promotes accountability by tracking course progress (Gerson, 2015). Thus, outright bans have also waned in popularity due to the technology's role as a platform for new and evidently promising pedagogical tools.

On one hand, greater preparedness for emergencies and access to modern education resources

[1]Truck and van based services, generally operated by individual owners, charged roughly a dollar per day or twenty dollars per month to securely store students' cellphones during the school day (Harris and Schweber, 2015).

offer compelling reasons to embrace mobile phones. However, it is also the case that many current cellphone bans came into existence in order to mitigate serious problems—in particular, drug dealing among students, which continues to challenge communities nationwide. In 2016, 9.8% of 10th graders and 14.3% of 12th graders across the country reported having used an illicit drug other than marijuana in the past year, including cocaine, methamphetamines, and inhalants (National Institute on Drug Abuse, 2016). While these figures in fact reflect a modest decline over previous years, use of marijuana remains ever prevalent; 38.3% of 12th graders in states with medical marijuana provisions reported use in the past year, compared to 33.3% even in states without such laws (National Institute on Drug Abuse, 2016). Furthermore, only 31.1% of 12th graders associate regularly smoking marijuana with harm, a proportion that has fallen substantially over time (National Institute on Drug Abuse, 2016). If cellphone use facilitates risky behaviors, then device policies may bear meaningful influence on human capital development through other channels than instructional resources and test performance.

In fact, cellphone communication very likely defeats several barriers to obtaining drugs. While it may not be uncommon that high schoolers, possibly handicapped by limited maturity and faced with questionable information from peers, develop some interest in health compromising behaviors, including drug and alcohol use, far fewer might be expected to actually have access to controlled substances. As such, effort is required to discover and trade with a relatively limited number of suppliers—tasks complicated by risk and social stigma. These obstacles are directly addressed by the ability to communicate and establish relationships before physically meeting and without the supervision of adults. This last concern also highlights the importance of the school environment; a high density of peers and the absence of watchful household members logistically offers unique opportunities to youth, who may otherwise exert less control over their movements and activities than adults.

This paper studies the effect of lifting school cellphone bans on alcohol and drug abuse among high school students in the state of California. As a result of the 2003 repeal of California's statewide school cellphone ban, district governance boards were allowed individually to either continue banning phones or to modify their device policies. Over time, most opted to accommodate usage during certain hours of the day. Using fixed effects regression analysis clustered at the district level, I exploit variation in the timing of district policies to estimate the impact on substance abuse from permitting cellphone use on school grounds. Results provide evidence that allowing students to use cellphones at school increases opportunities to obtain and abuse controlled substances; this effect is particularly pronounced in the incidence of marijuana smoking among 9th graders, who exhibit a 1.3 percentage point higher chance of reporting past-month marijuana use in the year a ban is lifted. Factors involved may include the capability that the technology provides to negotiate high risk interactions in private and to seek out and contact a relatively small number of drug suppliers; as is thus to be expected, no impact is found on the consumption of cigarettes, which can be obtained legally by a large proportion of high schoolers.

Student substance abuse outcomes are obtained from the California Healthy Kids Survey (CHKS), a project funded by the California Department of Education (CDE) and executed by the company WestEd. As a requirement for receiving Title IV funding under the No Child Left Behind Act,[2] the survey was administered to students in nearly every school of every California district at least biennially during 2003-2010, continuing afterwards to be required of school districts receiving state funds for certain drug prevention programs.[3] Administration of the CHKS is carefully planned to result in data representative of enrollment at the district level. Since there is no official, central database of school district governance codes, information on current and historical cellphone policies was obtained from individual district websites and offices during March through May of 2017. In cases where on-line records were unavailable or insufficient, off-line documents were requested from administrative offices by phone and email correspondence.

[2] Large districts may submit a sampling scheme that does not include all schools, and which must be approved as being representative of the district (WestEd, n.d.)

[3] The CHKS includes various core and supplementary modules which broadly ask students about health habits, lifestyle, risk behaviors, relationships (e.g. with peers, teachers, and community members), and beliefs and attitudes. The survey forms one part of the California School Climate, Health, and Learning Survey (CAL-SCHLS) system, which aims to provide educators with timely insights on school climate.

The rest of this paper is structured as follows: Section 1.2 overviews related literature, Section 1.3 describes the policy setting, data sources, and sample studied, Section 1.4 reports empirical analyses and results, Section 1.5 discusses mechanisms, Section 1.6 explores possible confounding influences on outcomes by non-policy factors, Section 1.7 examines heterogeneous effects that may bear policy implications, and Section 1.8 concludes.

1.2 Literature Review

Thus far, studies in economics and education concerning high schoolers' use of cellphones and other mobile technologies have exclusively or almost exclusively targeted outcomes directly related to learning (Beland and Murphy, 2016; Fryer, 2016; Hull and Duch, 2016). From a survey of policies and interventions involving various technologies, efficacy in this regard appears to depend on the degree to which usage is planned and supervised to ensure educational applications. In particular, evidence has associated few if any gains in learning to technology use that is not deliberately managed by educators for learning purposes.[4] Evaluating a more structured context, however, Hull and Duch (2016) conclude that a North Carolina school district's one-to-one laptop program effectively improved the math skills of fourth through eighth grade students over the course of four to five years following initial assignment.[5]

On the other hand, experimental and quasi-experimental literature that explores broader behavioral and health outcomes has focused on college students, with most current work underlining negative consequences of technology dependence and misuse (Jenaro et al., 2007; Lepp et al., 2013, 2014, 2015; Li et al., 2015; Thomée et al., 2011). [6]

[4]For instance, Fryer (2016) reports that providing cellphones to students in the Oklahoma City Public Schools produced no measurable changes in state test scores, attendance, student effort, or behavioral incidences (which refers to the total number of suspensions, irrespective of type of infraction). In fact, Beland and Murphy (2016) find banning the devices to be a highly effectively means of raising the standardized test scores of the lowest achieving quintile of students, equating the resulting gains to the average effect of extending the school year by five days.

[5]Improvements appear to rise over time and peak at 0.16 standard deviations after five years. Reading scores appear also to have improved to a lesser extent.

[6]Specifically, frequent usage has been associated with heightened anxiety and decreased life satisfaction (Lepp et al., 2014; Jenaro et al., 2007), boredom and lower receptiveness to challenges

As an effort to bridge the above bodies of research, this paper utilizes a quasi-experiment to identify the role of campus cellphone policies in controlling the health compromising behaviors of high school students. Whereas prior studies concerning high schoolers' health habits and use of technology has been largely descriptive, I aim to shed light on possible causal relationships between use of phones and health outcomes. In doing so, I also contribute the first quantitative analysis on the efficacy of a historically widespread education policy in the U.S., school cellphone bans, specifically with regard to one of its major original aims.

Pertinently, a large number of surveys and descriptive analyses may be taken to implicate cellphone use in enabling or exacerbating health compromising behaviors—including substance abuse—by high school aged adolescents. According to Sánchez-Martínez and Otero (2009), intensive phone use among youth between the ages of 13 to 20 correlates positively with drug abuse (i.e. amphetamines, hallucinogens, cocaine, ecstasy, or heroin), as well as cellphone dependence, school failure, and mental health issues.[7] Leena et al. (2005) link problematic alcohol consumption to cellphone use among 14 to 16 year old Finns.[8] Yang et al. (2010) also highlight apparent links between teen drinking and excessive cellphone use, further positing that cellphones "could provide private and instant communication that is convenient to get illicit drugs."

Furthermore, qualitative studies delving into classroom use of mobile devices as teaching tools

(Lepp et al., 2015), diminished fitness (Lepp et al., 2013), and poor sleep habits (Li et al., 2015; Thomée et al., 2011). Apart from experimental and quasi-experimental literature, Tao et al. (2017) note from a survey 2,376 college students in Anhui, China that an association between excessive mobile phone use and alcohol consumption persists after accounting for depressive symptoms, while Augner and Hacker (2012) find evidence of problematic usage among Austrian nursing students as characterized by psychological dependence and interruption of sleep.

[7] Findings are based on the authors' survey of 1,328 Spanish adolescents on mobile phone use and other lifestyle factors and behaviors.

[8] Studies also suggest heterogeneity across gender and socioeconomic factors in purposes and manner of cellphone use. For example, Sánchez-Martínez and Otero (2009) reports that girls are significantly more likely than boys to take cellphones to school. Additionally, Koivusilta et al. (2007) observe in a nationally representative survey of 12 to 18 year old Finns that problematic use of mobile phones occurs more commonly among children of parents with low socioeconomic status or education. Moreover, while excessive use of any form of information and communication technology (e.g. household computers) correlates with poor health outcomes, this is most apparent with girls' use of mobile phones.

have uncovered, along with opportunities, substantial challenges that highlight the extent to which cellphones impair the ability to supervise and control student behavior. While, for instance, Engel and Green (2011) note that a system for anonymous audience response benefited shy students who might otherwise not have answered questions posed by the teacher, they observe also that the ability to "backchannel," or have private conversations, necessitates a greater degree of trust due to heightened potential for disruptive behavior.[9] At a high school in its third year of a "Bring Your Own Device" policy (promoting the use of students' personal devices in instructional activities), Ross (2013) report that teachers who did not attempt to integrate cellphones viewed student distraction as a prohibitive barrier to exploiting the technology, and that the most common off-task behavior among students of teachers who permitted the devices was text messaging.[10]

Finally, from nationally representative survey data on 800 12-17 year olds, Wolfe et al. (2016) finds that school restrictions on student cellphone use significantly impact the incidence of "sext" messaging: teens attending schools that completely ban the devices have 48% lower odds of ever having received a sexually explicit text message. Notably, the extent of parental supervision appears unrelated to the likelihood of sexting, suggesting that the school environment, more so than the home and outside environments, presents opportunities for inappropriate device usage.

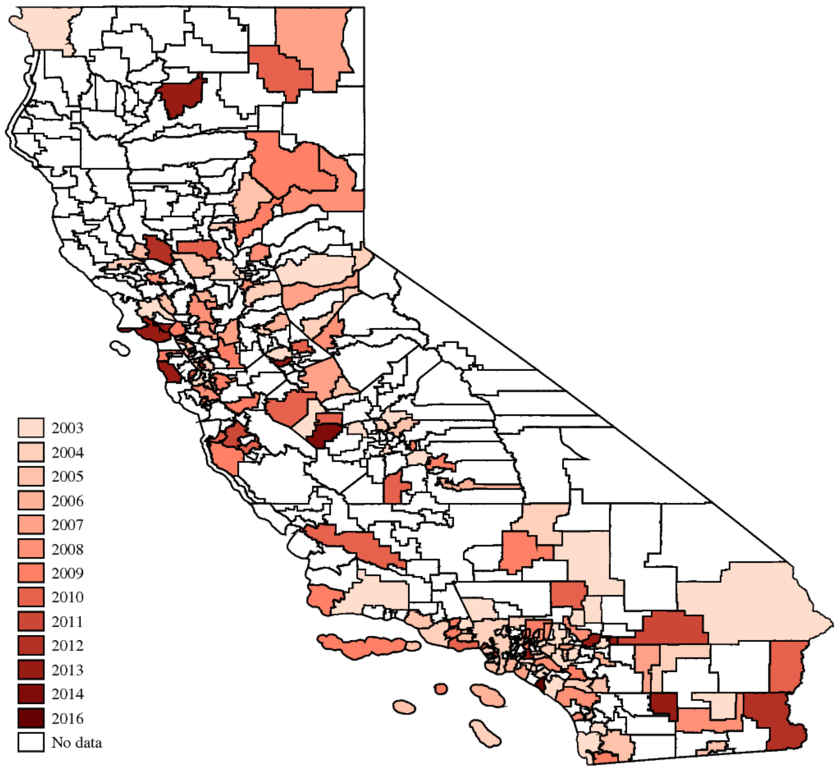
1.3 Policy Setting and Data

Data for this study was obtained from the California Department of Education, the company WestEd, and individual school district websites and offices. School-level enrollment and sociodemographic information is available from the CDE public data website.

[9]The authors study a pilot program that integrated cellphones into planned activities in a high school pre-calculus course with 18 students, whom they observe during class and from whom they collect feedback.

[10]For similar reasons, Thomas et al. (2014) find that slightly over half of 1,121 teachers in Kentucky and Tennessee (mostly in high schools) did not support the use of mobile phones in the classroom. Teachers cited concerns such as access to inappropriate information on the internet and difficulty regulating communication with parents during instructional time.

Figure 1.1: District policy revisions by year

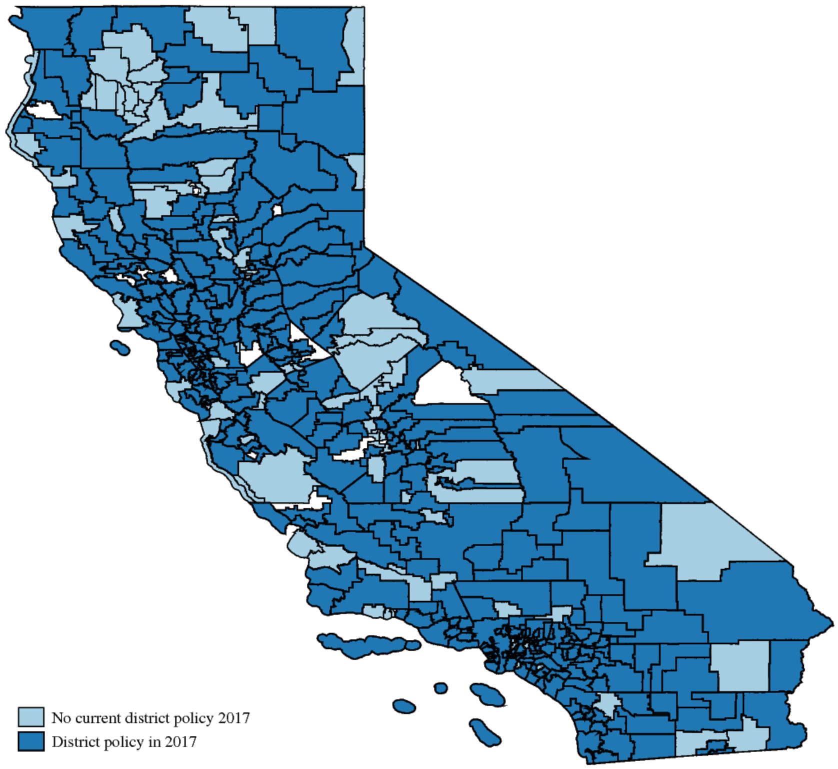


1.3.1 Repeal of Statewide Campus Cellphone Ban

Effective January 1, 2003, the California State Legislature repealed a prohibition against student possession of cellphones on school campuses (Education Code 48901.5). The ban, which had previously been in effect since 1988, was established due to the widely held belief by police and school officials that students were using the devices primarily for the coordination of drug dealing (Willon, 2002).

In conjunction with the spread of cellphone ownership, several high-profile incidents, such as the Columbine and Santana school shootings (Willon, 2002) and the September 11th terrorist attacks (Matthews, 2002*a*), led to increasing pressure from parents on school officials to allow cellphone communication with their children at school sites. Opting for a more flexible regulatory solution to accommodate varying concerns and circumstances in different communities, the new law allowed each school district's board of education to determine its own rules governing student cellphone use and possession. Figure 1.1 charts the timing of district cellphone policy revision

Figure 1.2: Current policies (2017)



among districts for which the date of the first revision after 2003 could be obtained.[11] Figure 1.2 illustrates that nearly all districts have since passed some manner of local policy on cellphone use.

This change in legislation was pushed largely by a handful of grassroots efforts (e.g. the principal of an LAUSD high school announcing in 2001 that his school would no longer enforce the state law (Matthews, 2002*b*)). In fact, Senator Liz Figueroa (D-Fremont), who sponsored the new bill, was initially approached to do so by a class of students at Logan High School in Union City (Matthews, 2002*b*). The legislation was also advocated by the La Canada school board (Matthews, 2002*b*).

By default, most districts almost certainly depended on the language of the 1988 CA Ed Code in their governance board policies until further revision at the district level. While it is not possible to confirm with certainty that some districts did not enter into a period free of device rules, district codes tend to contain or cite the language of relevant state codes. Additionally, Los Angeles

[11] Shapefiles detailing CA school district boundaries were obtained from U.S. Census Bureau (2017).

Unified, Irvine Unified, and Corona-Norco Unified, continued to enforce bans until local level revisions (Helfand and Hayasaki, 2003). The data furthermore suggests that the new state law affected little to no immediate change at the local level by itself—support for this is provided later in Section 1.4. Hence, later district-level policies that both restrict students from using cellphones at certain times (e.g. instruction time) while permitting use at others (e.g. before the first bell, after the last bell, during lunch, nutrition break, passing periods, etc) in fact produced a less restrictive environment by recognizing permissible usage times.

While degree of enforcement of the prior state law may certainly have varied, some facts suggest that it was generally taken seriously. Firstly, the state reform was largely driven by local level actors who would have had little incentive to officially challenge a toothless policy. Additionally, 77% of LAUSD high school principals predicted in a district survey that difficulties would arise from passing a district policy with relaxed restrictions (Helfand and Hayasaki, 2003), indicating a non-negligible degree of existing control that would be hamstrung by weaker rules.

1.3.2 California Healthy Kids Survey

The California Healthy Kids Survey (CHKS) is a youth risk behavior monitoring system funded by the California Department of Education (CDE) and managed by the company WestEd. The purpose of the survey is to provide both state and local level education officials with insights for creating safer and more effective learning environments, as well as mitigating youth problems beyond the classroom (e.g. drunk driving, unsafe sex, violence, and drug abuse). As such, the survey encompasses a large number of core and supplemental modules designed to collect information on a broad range of activities, health habits, relationships (e.g. teachers, other students, community members, etc), attributes of home life, and beliefs and attitudes. While certain elements feature consistently in the core modules during each iteration (i.e. substance abuse), other content has evolved over time to monitor current issues (e.g. cyber harassment, gang involvement).

Between 2003 and 2010, the CDE required all districts to administer the survey at least every two years in order to receive federal funds provided under Title IV of the No Child Left Behind

Act (Austin, 2013). As of 2013, the survey remained mandatory for districts receiving funds from the Tobacco Use Prevention Education (TUPE) program or the Safe and Supportive Schools grant (Austin, 2013). This requirement entails collecting data from all schools within each district, or else collecting data from all schools selected in an approved sampling plan (WestEd, n.d.). Resulting data is meant to be representative at the district level—WestEd provides local administrators district-level reports, with analyses disaggregated by grade level.[12]

1.3.3 District Cellphone Regulations

Current and historical district-level cellphone policies were retrieved mostly from district governance board websites. Key information consists of the date of a district's first code revision pertaining to student cellphone use following the state law change, as well as the particular time and/or location based restrictions and allowances for cellphone use. Where the nature of a district's policy changed again afterwards, the analysis to follow is based on the conditions laid out in the first policy. While board policies show only current rules along with dates when the document was edited, it was possible in many cases to use revision dates to search on-line archives of board meetings for specific changes made in the past. Naturally, this could only be done with districts having a current cellphone policy to trace backwards (including policies delegating regulation to individual school sites), and only if boards archived sufficiently old information. When on-line information was insufficient, attempts were made to obtain policy records from district offices and board archivists. In general, it is not common to retain or furthermore to digitize long-obsolete policies, and asking district office staff to recall details from unavailable historical documents proved largely ineffective; due to ubiquitously high turnover, there were few instances of staff members being able to recall rules from more than a few years in the past. Understandably, few staff were willing to retrieve non-digitized documents. Apart from being unlikely to report any older policies due to the above mentioned turnover, districts with no recent policy to trace back were also omitted

[12] In the case of sufficient data, such as districts that survey all students in all schools, school level reports can also be generated (WestEd, n.d.).

due to the likelihood of recalling only the latest rule—possibly reflecting some change stemming from dissatisfaction with a prior rule, and potentially thereby introducing bias.

Of the roughly over 400 public unified and high school districts active over the period 2002-2013, policy information was obtained for 295 districts. However, due to the above described limitations, complete historical information was unavailable for many of these districts. Thus, key historical policy dates and stipulations were only obtained for 170 public unified and high school districts that operated during the period (and are of course necessarily still active, since information cannot be retrieved from defunct districts).

1.3.4 Sample

The CHKS high school surveys from the 2001-2002 to 2012-2013 school years targeted 9th and 11th graders, with approximately 15% more 9th graders in the data.[13] According to WestEd's CHKS website: "It is important that schools collect grade-level data (rather than use a general high-school sample) because most health-risk behaviors increase or change with age. Understanding developmental differences is critical to implementing better programs that target each age group" (WestEd, n.d.). On the particular grade selection, the company cites the following rationale: "Grade 9 (age 14) is typically the first year of senior high school, and is a time when prevalence of AOD [alcohol or drug] use can increase to substantial levels," while "grade 11 was selected because research shows that virtually all students initiating AOD use in secondary school will have done so by the end of grade 11" (WestEd, n.d.).

The sample consists of repeated cross sections of student level observations with information for outcomes and controls, including past month use of cocaine/crack, marijuana, inhalants, meth, alcohol, and cigarettes, and various individual and school-level sociodemographic variables (e.g. age, gender, race, proportion of school eligible to receive free and reduced price lunches). Analysis

[13]While the data contain some 10th and 12th grade respondents as well, they are very few in number and not representative, as their inclusion is an incidental artifact of class based sampling. According to a WestEd employee, the presence of 10th and 12th grade students in classes with 9th and 11th grade students is not indicative of remedial/advanced status, and may be due to mixed grade activities.

Table 1.1: Introduction of District Policies

	(1) Students	(2) Schools	(3) Districts
2003	175216	178	42
2004	87930	134	21
2005	105806	86	21
2006	15372	22	6
2007	52942	38	9
2008	46768	43	16
2009	110460	91	23
2010	24579	22	13
2011	7794	10	3
2012	933	2	2
2013	20542	26	10
2014	11	1	1
Total	648742	654	168

Sample students and schools are in districts for which I could obtain policy dates, and which had information for all outcomes and controls.

is ultimately performed on 346,195 ninth graders and 302,547 eleventh graders from 654 schools across 168 districts.

Table 1.1 breaks down school districts, schools, and students by year in which the district board of education first revised its governance code concerning cellphones. While some bunching is evident immediately after the state repeal, considerable variation occurs in timing of the first district policy.

Tables 1.2 and 1.3 show within-sample comparisons of student and school characteristics before and after the first local level code revision. Drug use generally appears higher in the post period, and the analyses that follow will investigate these changes.

School and student summary statistics from the 2001-2002 school year are presented in Table 1.4 and Table 1.5. From Table 1.4, sample schools have generally smaller enrollment, a larger average proportion of Hispanic students, and a smaller average proportion of white students. Differences in school level characteristics are statistically significant at the 5 percent level in enrollment, proportion

Table 1.2: Student Summary Statistics: Pre and Post District Policy Revision

	9th grade			11th grade		
	(1)	(2)	(3)	(4)	(5)	(6)
	Pre-period	Post-period	(2)-(1)	Pre-period	Post-period	(5)-(4)
Cocaine/crack	0.018	0.030	0.012***	0.023	0.033	0.010***
	(0.133)	(0.169)	(0.001)	(0.150)	(0.179)	(0.001)
Marijuana	0.068	0.115	0.047***	0.127	0.177	0.050***
	(0.252)	(0.319)	(0.001)	(0.333)	(0.382)	(0.002)
Inhalants	0.026	0.043	0.017***	0.024	0.037	0.013***
	(0.159)	(0.202)	(0.001)	(0.152)	(0.189)	(0.001)
Meth	0.017	0.026	0.009***	0.020	0.027	0.007***
	(0.131)	(0.160)	(0.001)	(0.139)	(0.163)	(0.001)
Alcohol	0.124	0.180	0.056***	0.222	0.283	0.060***
	(0.330)	(0.384)	(0.002)	(0.416)	(0.450)	(0.002)
Cigarettes	0.049	0.073	0.024***	0.096	0.114	0.018***
	(0.217)	(0.260)	(0.001)	(0.295)	(0.318)	(0.001)
Age 14 or younger	0.728	0.663	-0.065***	0.004	0.003	-0.000
	(0.445)	(0.473)	(0.002)	(0.060)	(0.058)	(0.000)
Age 15 or older	0.272	0.337	0.065***	0.996	0.997	0.000
	(0.445)	(0.473)	(0.002)	(0.060)	(0.058)	(0.000)
Girl	0.521	0.515	-0.006***	0.519	0.516	-0.003
	(0.500)	(0.500)	(0.002)	(0.500)	(0.500)	(0.002)
Asian	0.235	0.212	-0.023***	0.236	0.216	-0.019***
	(0.424)	(0.409)	(0.002)	(0.424)	(0.412)	(0.002)
Black/AA	0.099	0.091	-0.008***	0.089	0.084	-0.005***
	(0.298)	(0.287)	(0.001)	(0.285)	(0.278)	(0.001)
White	0.579	0.482	-0.097***	0.594	0.501	-0.093***
	(0.494)	(0.500)	(0.002)	(0.491)	(0.500)	(0.002)
Other	0.087	0.215	0.128***	0.081	0.198	0.117***
	(0.282)	(0.411)	(0.002)	(0.273)	(0.399)	(0.002)
Obs	77134	269061	346195	63263	239284	302457

Sample students are in districts for which I could obtain policy dates, and who had information for all outcomes and controls. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.3: School Summary Statistics: Pre and Post District Policy Revision

	(1) Pre	(2) Post
Total enrollment	1897	1920
	(971)	(957)
Grade 9 enrollment	548	535
	(314)	(293)
Grade 11 enrollment	444	460
	(235)	(235)
Proportion Hispanic	0.404	0.443
	(0.248)	(0.261)
Proportion Black	0.079	0.083
	(0.108)	(0.118)
Proportion White	0.358	0.315
	(0.253)	(0.245)
Proportion FRPL eligible	0.367	0.442
	(0.234)	(0.265)
Proportion ELL	0.159	0.151
	(0.125)	(0.119)
Schools	420	592

Sample schools are in districts for which I could obtain policy dates, and which had information for all outcomes and controls.

Hispanic, and proportion White, but not in proportion Black, percent FRPL eligible, and percent English Language Learners (ELL).

Student summary statistics compare sample 9th and 11th graders to non-sample 9th and 11th graders. For both 9th and 11th graders, differences are statistically significant at the 5 percent level for all drug use outcomes, the age 14 dummy indicators, and all race dummies except those indicating Black/ African American (with sampled students being more likely to report being Asian). While sampled schools do not appear very different from other schools in proportion of FRPL eligible and ELL students, observable drug use behaviors appear lower among sampled students than in the population at large.

Table 1.4: Year 2002 School Summary Statistics

	(1) Sample	(2) Non-Sample	(3) (1)-(2)
Total enrollment	2401	1898	503***
	(920)	(952)	(87)
	159	468	627
Grade 9 enrollment	713	549	164***
	(339)	(317)	(30)
	159	468	627
Grade 11 enrollment	551	442	110***
	(215)	(221)	(20)
	159	468	627
Proportion Hispanic	0.374	0.327	0.047**
	(0.259)	(0.246)	(0.023)
	159	468	627
Proportion Black	0.086	0.070	0.016
	(0.130)	(0.105)	(0.010)
	159	468	627
Proportion White	0.373	0.458	-0.085***
	(0.241)	(0.271)	(0.024)
	159	468	627
Proportion FRPL elig.	0.325	0.290	0.035*
	(0.233)	(0.230)	(0.021)
	159	468	627
Proportion ELL	0.172	0.150	0.022*
	(0.135)	(0.131)	(0.012)
	159	444	603

Sample schools are in districts for which I could obtain policy dates, and which had information for all outcomes and controls. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.5: Year 2002 Student Summary Statistics

	9th grade			11th grade		
	(1)	(2)	(3)	(4)	(5)	(6)
	Sample	Non-Sample	(1)-(2)	Sample	Non-Sample	(4)-(5)
Cocaine/crack	0.010	0.014	-0.004***	0.013	0.016	-0.004***
	(0.099)	(0.119)	(0.001)	(0.113)	(0.127)	(0.001)
	13900	70659	84559	11516	59162	70678
Marijuana	0.038	0.085	-0.047***	0.078	0.139	-0.061***
	(0.191)	(0.279)	(0.002)	(0.269)	(0.346)	(0.003)
	13900	66923	80823	11516	54553	66069
Inhalants	0.013	0.021	-0.008***	0.012	0.016	-0.004***
	(0.112)	(0.143)	(0.001)	(0.110)	(0.124)	(0.001)
	13900	69603	83503	11516	58905	70421
Meth	0.010	0.015	-0.005***	0.014	0.018	-0.004***
	(0.101)	(0.123)	(0.001)	(0.117)	(0.133)	(0.001)
	13900	70647	84547	11516	59241	70757
Alcohol	0.069	0.131	-0.061***	0.143	0.244	-0.101***
	(0.254)	(0.337)	(0.003)	(0.350)	(0.430)	(0.004)
	13900	57481	71381	11516	44700	56216
Cigarettes	0.031	0.057	-0.026***	0.075	0.107	-0.032***
	(0.172)	(0.231)	(0.002)	(0.263)	(0.309)	(0.003)
	13900	67705	81605	11516	55760	67276
Age 14 or younger	0.773	0.748	0.025***	0.001	0.003	-0.001**
	(0.419)	(0.434)	(0.004)	(0.037)	(0.051)	(0.000)
	13900	72518	86418	11516	60673	72189
Age 15 or older	0.227	0.252	-0.025***	0.999	0.997	0.001**
	(0.419)	(0.434)	(0.004)	(0.037)	(0.051)	(0.000)
	13900	72518	86418	11516	60673	72189
Girl	0.532	0.535	-0.003	0.530	0.540	-0.009*
	(0.499)	(0.499)	(0.005)	(0.499)	(0.498)	(0.005)
	13900	72222	86122	11516	60361	71877
Asian	0.263	0.165	0.098***	0.260	0.155	0.105***
	(0.441)	(0.371)	(0.004)	(0.439)	(0.362)	(0.004)
	13900	42534	56434	11516	37959	49475
Black/AA	0.080	0.081	-0.001	0.077	0.074	0.003
	(0.271)	(0.273)	(0.003)	(0.267)	(0.262)	(0.003)
	13900	42534	56434	11516	37959	49475
White	0.610	0.697	-0.087***	0.622	0.720	-0.098***
	(0.488)	(0.460)	(0.005)	(0.485)	(0.449)	(0.005)
	13900	42534	56434	11516	37959	49475
Other	0.046	0.057	-0.010***	0.041	0.051	-0.010***
	(0.211)	(0.232)	(0.002)	(0.198)	(0.221)	(0.002)
	13900	42534	56434	11516	37959	49475

Sample students are in districts for which I could obtain policy dates, and who had information for all outcomes and controls. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.4 Analysis and Results

1.4.1 Drug Use Before and After District Policy Revision

This study exploits district level variation in the timing of policy changes in order to estimate the effect of relaxing campus cellphone rules on student substance abuse—separately from coinciding statewide factors that might confound analysis in a case study setting. First, I examine staggered comparisons of drug outcomes before versus after the first revision of applicable district code following the state repeal. By using multiple instances of local level policy changes in different calendar years, I am able to separately attribute part of the observed changes in drug outcomes to each district's post-policy period (policy effects) versus the corresponding calendar years (possibly reflecting statewide trends).[14] To this end, I estimate the following model:

$$Y_{isr} \quad = \quad \phi_1 \text{Post district revision}_{sr} + \gamma X_{isr} + \alpha_s + \theta_r + \varepsilon_{isr} \tag{1.1}$$

In the above equation, Y_{isr} is an indicator for past 30 day use of a given drug (the main outcomes are cocaine/crack, marijuana, inhalants, methamphetamines, alcohol, and cigarettes) by student i in school s during academic calendar year r. "Post district revision" indicates the years after the district to which school s belongs revised its cellphone policy. X_{isr} is a set of individual and school-level controls,[15] and α_s and θ_r are school and year fixed effects, respectively.

Table 1.6 presents the results of estimating Equation 1.1 on the samples of 9th and 11th graders, with the outcomes of past month use of cocaine, marijuana, inhalants, methamphetamine, alcohol, and cigarettes, as well as the composite drug use indicator. For 9th graders, the period following district code revision is positively and statistically significantly correlated at conventional levels with all drug use measures except for cigarettes. Among the non-cigarette specifications (1-6), the

[14] This analysis will use the same time frame, from five years before district revision to eight years after, as the disaggregated event study specification to follow (below in Subsection 1.4.2).

[15] Controls include age, sex, race, and school-level enrollment, racial demographics, proportion of student body eligible for Free and Reduced Price Meals, proportion of student body that are English Language Learners.

Table 1.6: Drug Use Pre vs. Post District Policy Revision

	(1) Any drugs	(2) Coc./crack	(3) Marijuana	(4) Inhalants	(5) Meth	(6) Alcohol	(7) Cigarettes
Grade 9							
Post	0.014***	0.004***	0.0123***	0.004**	0.002*	0.010**	0.003
	(0.004)	(0.001)	(0.003)	(0.002)	(0.001)	(0.004)	(0.002)
Pre-period mean	0.151	0.018	0.068	0.026	0.017	0.124	0.049
Obs	346195	346195	346195	346195	346195	346195	346195
Grade 11							
Post	0.033***	0.004***	0.026***	0.002	0.002*	0.024***	0.002
	(0.004)	(0.001)	(0.004)	(0.002)	(0.001)	(0.004)	(0.003)
Pre-period mean	0.260	0.023	0.127	0.024	0.020	0.222	0.096
Obs	302547	302547	302547	302547	302547	302547	302547
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Outcomes are indicators for any use of the substance in past 30 days. Controls include age, gender, race, school level enrollment, demographic composition, FRPL, and proportion English learners. Standard errors in parentheses, clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

coefficient on Post is significant at least at the five percent level in all except methamphetamine, in which it is nonetheless significant at the ten percent level. Hence, 9th graders are 1.4 percentage points more likely in the post period to report having used any substance in the past 30 days. At a glance, this appears to be driven by marijuana and alcohol. For 9th graders, the likelihood of reporting past month marijuana and alcohol use rises in the post period by 1.2 and 1 percentage points, respectively. These are relatively larger changes than those exhibited in cocaine/crack, inhalant, and methamphetamine use (which increase in the post period by 0.2 - 0.4 percentage points), and they are important in magnitude considering that only 6.8% and 12.4% of 9th graders in the pre period report past month use of marijuana and alcohol, respectively.

For 11th graders, the period following district code revision is positively and statistically significantly correlated at conventional levels with all drug use measures except for inhalants and cigarettes. Among the non-inhalant and non-cigarette specifications, the coefficient on Post is significant at the one percent level for all except methamphetamine, in which it is again significant at the ten percent level. Once again, the effect on the composite measure ("Any drugs") seems to be largely driven by marijuana and alcohol. Among 11th graders, the likelihood of reporting past month marijuana and alcohol use rises in the post period by 2.6 and 2.4 percentage points, respectively. These are again relatively important changes if compared to pre period reported use (12.7% and 22.2% of 11th grader reported marijuana and alcohol use in the period before the first relevant district code revision).

While marijuana and alcohol are by far more prevalent among both grade levels in the data than cocaine/crack, inhalants, and methamphetamine, the incidence of cigarette smoking is also fairly close to that of marijuana use. Moreover, the much larger incidence of cigarette smoking among 11th compared to 9th graders (9.6% vs. 4.9% in the pre period) parallels the differential use of marijuana and alcohol between the grades—contrast this with the other substances, for which there is a much less evident or even reversed difference. Though it may thus seem that cigarettes are in some manner equally popular or socially acceptable as marijuana and alcohol, the lack of any apparent impact on cigarette smoking should be expected under the scenario in which cellphones facilitate student drug use by lowering the effort and risk of discovery associated with obtaining controlled substances. In contrast with alcohol, which cannot legally be possessed by any person of high school age, and marijuana, which is a controlled substance even for adults at any age, cigarettes can be legally be bought and possessed by a large fraction of high schoolers. In that regard, private communication by cellphone likely does not contribute much or at all to the ease of finding and communicating with a potential supplier of cigarettes.

1.4.1.1 Multiple Outcomes

When testing multiple independent outcomes, inference is often adjusted by the Sidak or Bonferroni procedure to account for the higher likelihood of statistically significant results arising by chance. However, in the case of correlated outcomes, both of these result in highly conservative inference (Conneely and Boehnke, 2007) (e.g. consider the hypothetical case in which multiple outcomes are perfectly correlated—effectively a single test requiring no adjustment). The drug use outcomes in the sample analyzed here are indeed highly correlated. For example, the correlation between past month use of cocaine and past month use of methamphetamines is 0.79. Following Aker et al. (2012), I employ the Bonferroni correction for multiple tests, adapted to account for inter-variable correlation.[16] As the 9th and 11th grade analyses involve separate, non-overlapping samples, I consider the set of (individual) drug use measures for each grade to be a "family" of tests. Like Aker et al. (2012) and Gibson et al. (2011), I base testing on the ten percent significance level. For 9th graders, based on an average inter-outcome correlation of 0.47, the Bonferroni adjusted critical value is 0.040. All previous impacts remain significant at this level except for that on use of inhalants (which joins use of cigarettes in exhibiting no significant impact). For 11th graders, based on an inter-outcome correlation of 0.42, the Bonferroni adjusted critical value is 0.037. All impacts remain significant at this level except for that on use of methamphetamine (the change in use of inhalants was not statistically significant to begin with).

1.4.2 Endogenous Policy Timing and Event Studies

In order to check for possibly endogenous timing of local policy changes (which may manifest as trends in drug outcomes that begin prior to policy changes), I also disaggregate the comparison periods into a year-by-year event study centered on the event of first revising district student cellphone rules after the state repeal.

$$Y_{isr} = \sum_{t=-5}^{8} \beta_t \mathbf{1}\{E_{sr} = t\} + \gamma X_{isr} + \alpha_s + \theta_r + \varepsilon_{isr}, \quad t \neq -1 \tag{1.2}$$

Event time, here represented by E_{sr}, refers to time as counted relative to the first year a student cellphone policy is introduced. Hence, event time during the year of policy introduction is equal to zero, while event time during the year prior is equal to negative one, and event time during the year after is equal to positive one. Formally, $E_{sr} = r - R_s$, where R_s is the year that school s's district first changed its governance code concerning student cellphone rules in response to the new state law.

[16] The procedure is described in more detail by Sankoh et al. (1997).

The event window inspected spans from five periods prior to the event to eight periods following (the results of the main analysis that follow are robust to using other, shorter event windows, e.g. $t = -2$ to $t = 2$, not shown). I aim to estimate the β_t's (using $t = -1$ as the reference period) while accounting for X_{isr}, the set of observable individual characteristics and time-varying school characteristics as controls. As before, α_s and θ_r are school and year fixed effects, respectively.

District board policies may be changed both during the summer and also throughout the school year, which typically spans from late August or early September to May or June. CHKS survey administration has no set date, but must not start until at least 30 days into the school year, and must be completed before the end of June. I code school years in which the relevant policy change was made before April as being in event time zero ($t = 0$), and school years in which the change was made in April or later as being in event time one. Thus, some school years that count as $t = 0$ may only have had the new policy in place for two or three months before survey administration. Not many policy changes fell around the chosen cutoff, and the main results that follow are robust to using earlier cutoffs.

Figures 1.3 through 1.9 show the results of estimating Equation 1.2 on the samples of 9th and 11th graders (represented in each figure's upper and lower panels, respectively), using the outcomes of past month cocaine, marijuana, inhalant, methamphetamine, alcohol, and cigarette use, as well as the composite drug use indicator. Additionally, pooled results for both grades together are provided in Figure A.1 in the Appendix. For 9th graders, although many coefficients on the event times from $t = -5$ to $t = -3$ are negative, only one pre period in the composite and alcohol specifications is significantly so at the five percent level relative to the reference period $t = -1$. Visually, it appears that a pre trend in the composite measure is driven by the alcohol specification (Figures 1.3 & 1.8), as none of the other graphs exhibit a similar pattern. This is addressed in Section 1.4.3. Otherwise, the pre periods for 9th graders in general do not differ significantly in relation to $t = -1$. On the other hand, all periods from $t = 0$ onwards are positively and significantly correlated with marijuana use (Figure 1.5). The first year in which a district revises its own cellphone rules is correlated with a 1.3 percentage point increase in the likelihood of smoking marijuana, an effect which seems to rise before leveling off at around 2 percentage points from $t = 4$ onwards.

While effects are initially less clear with respect to consumption of alcohol, the periods from $t = 2$ onwards are positively associated with alcohol use (although the coefficients on $t = 6$ and $t = 7$ are not statistically significant) (Figure 1.8). Effects over this period lie between 1.3 to 2.4 percentage points greater likelihood of past month alcohol consumption compared to $t = -1$. As may be expected from the previous analysis, the graphs representing the use of cocaine/crack, inhalants, and methamphetamine specifications are visually indicative of an increase in 9th grade drug use brought on at $t = 0$, although the individual period coefficients tend to be very and mostly insignificant (Figures 1.4, 1.6, 1.7). Nothing suggests any particular significance of the policy event for incidence of cigarette smoking among 9th graders.

Results from the analysis of 11th graders differ most noticeably from the 9th grade analysis in that a visually apparent upward trend in marijuana use during periods $t = -5$ to $t = -1$ seems to precede the policy event (Figure 1.5). A subtler trend possibly also precedes alcohol consumption (Figure 1.8). As might be expected from the analysis in Section 1.4.1, coefficients on the post periods in the marijuana and alcohol specifications are roughly 2-3 times as large as for 9th graders. For instance, $t = 2$, the second year after the policy event is associated with a 3.2 percentage point increase in the likelihood of 11th graders' past month marijuana use compared to $t = -1$, while the effect among 9th graders is 1.4 percentage points. The same time period correlates with a 3 percentage point increase in 11th grade alcohol consumption and only a 1.3 percentage increase in 9th grade alcohol consumption (Figure 1.8). Otherwise, the analyses concerning cocaine and methamphetamines produces results fairly close to those from the 9th grade analysis, while the graph of the inhalants specification, like in the Section 1.4.1 model, does not suggest any particular impact of the policy event. In particular, the 11th grade methamphetamine graph (Figure 1.7), appears to more clearly suggest an upward shift in consumption attributable to the policy event. Once again, the cigarettes specification (Figure 1.9) seems to concur with the previous analysis in revealing no appreciable impact of the policy event on the incidence of cigarette smoking among 11th graders.

Figure 1.3: Composite: Any substance in past 30 days

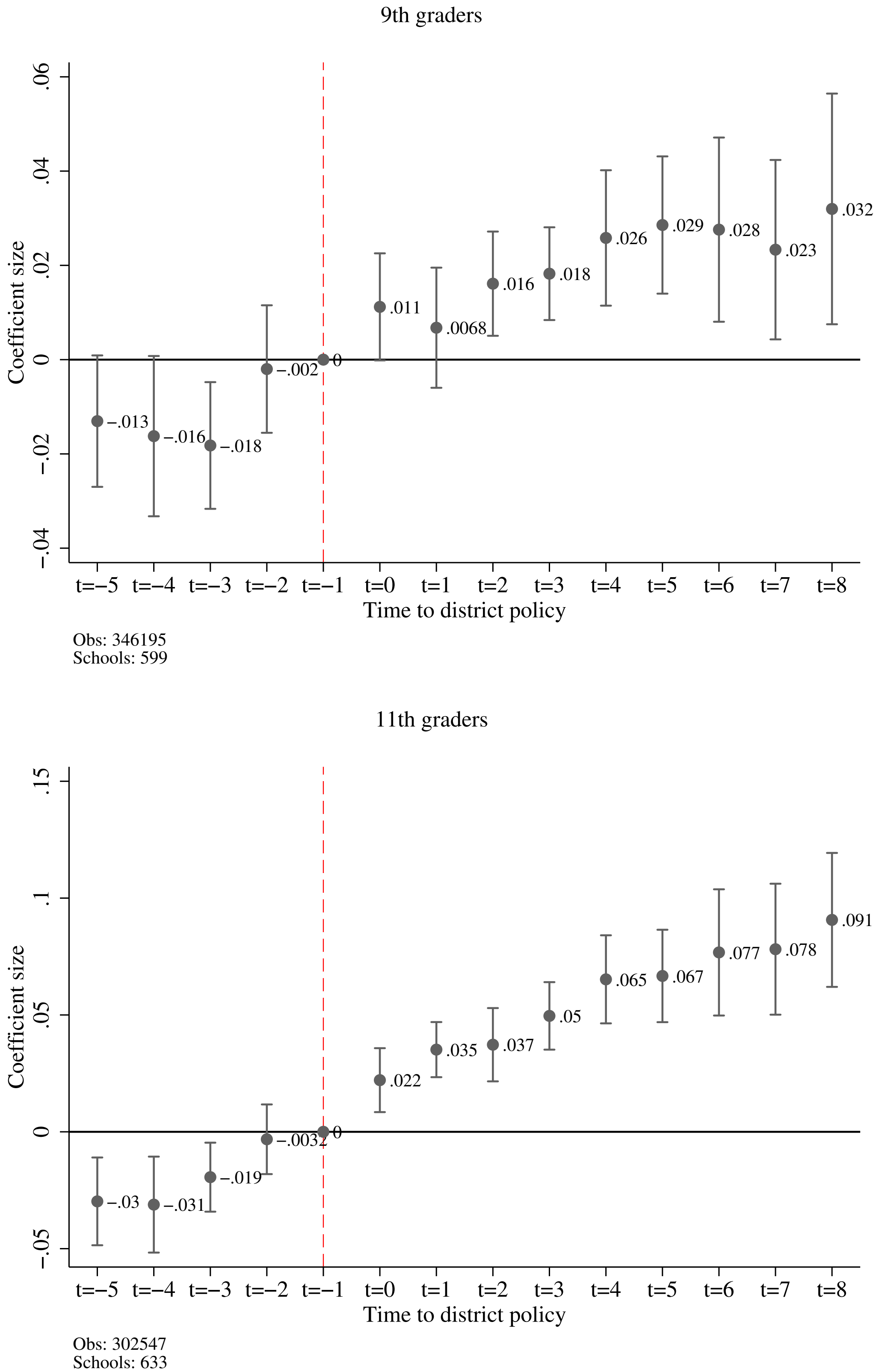
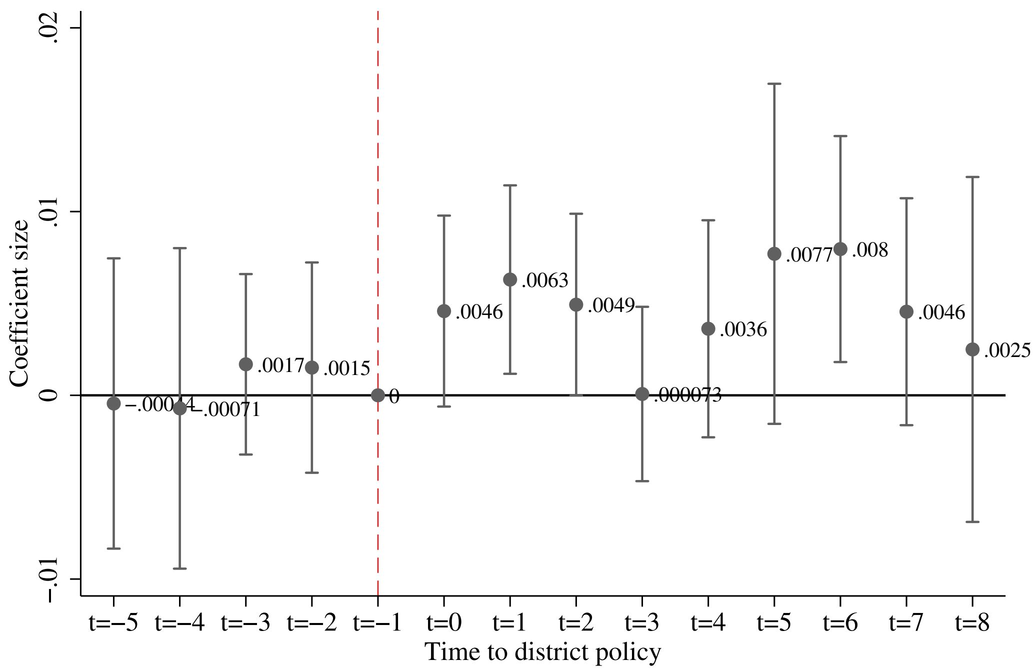


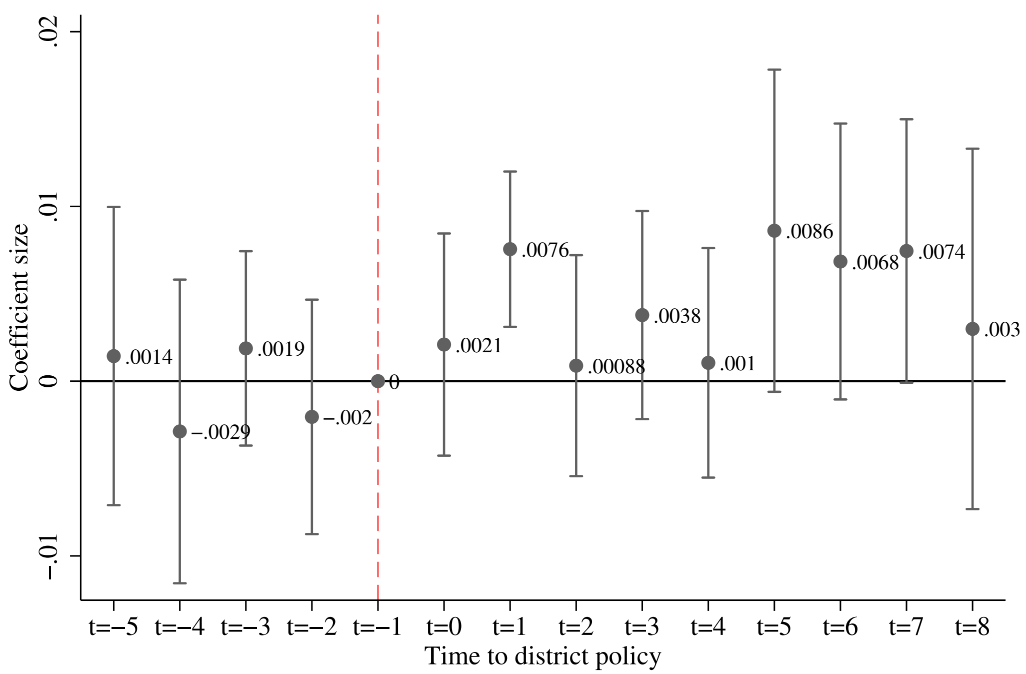
Figure 1.4: Used cocaine/crack in past 30 days

9th graders



Obs: 346195
Schools: 599

11th graders



Obs: 302547
Schools: 633

Figure 1.5: Used marijuana in past 30 days

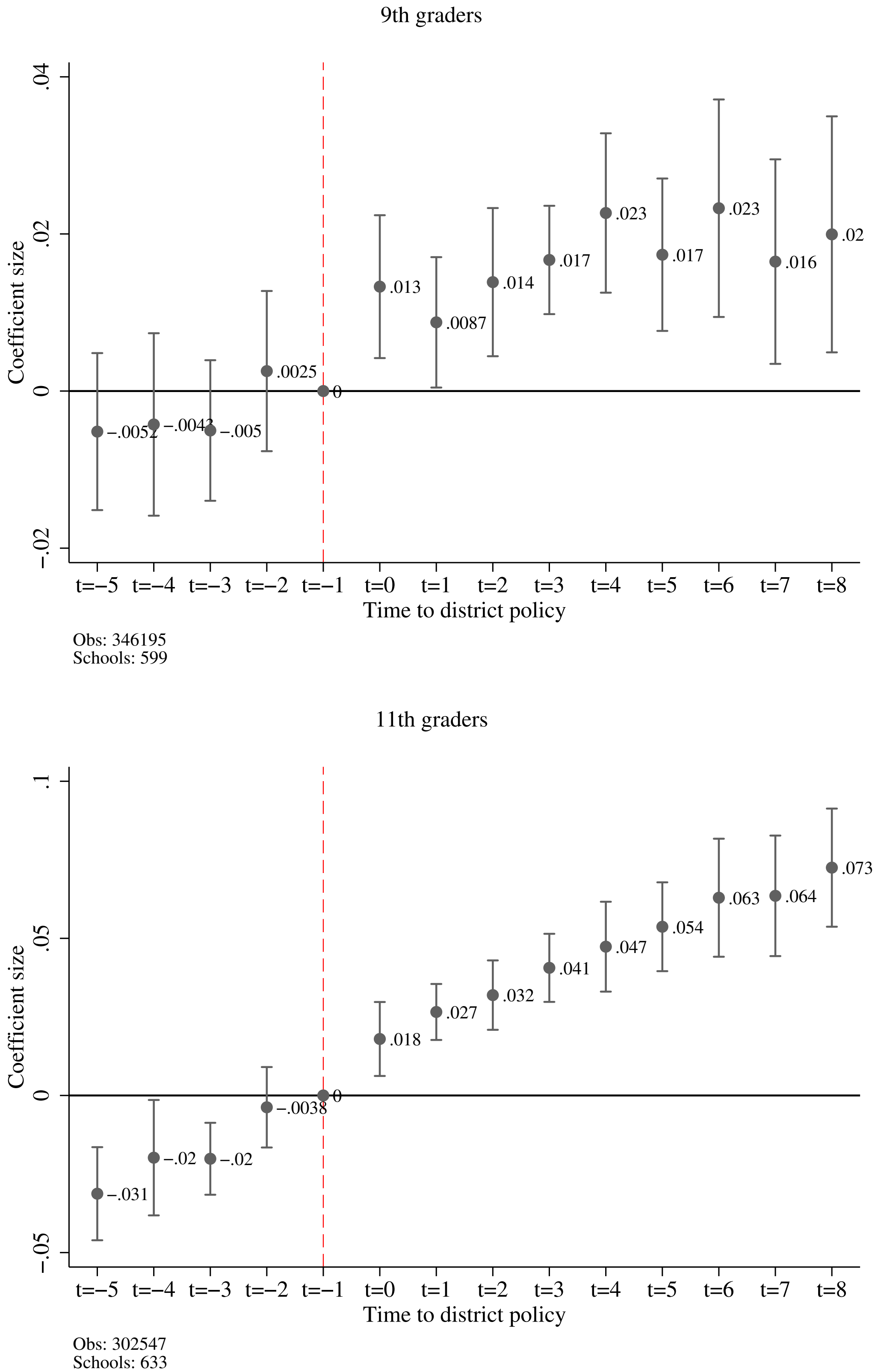
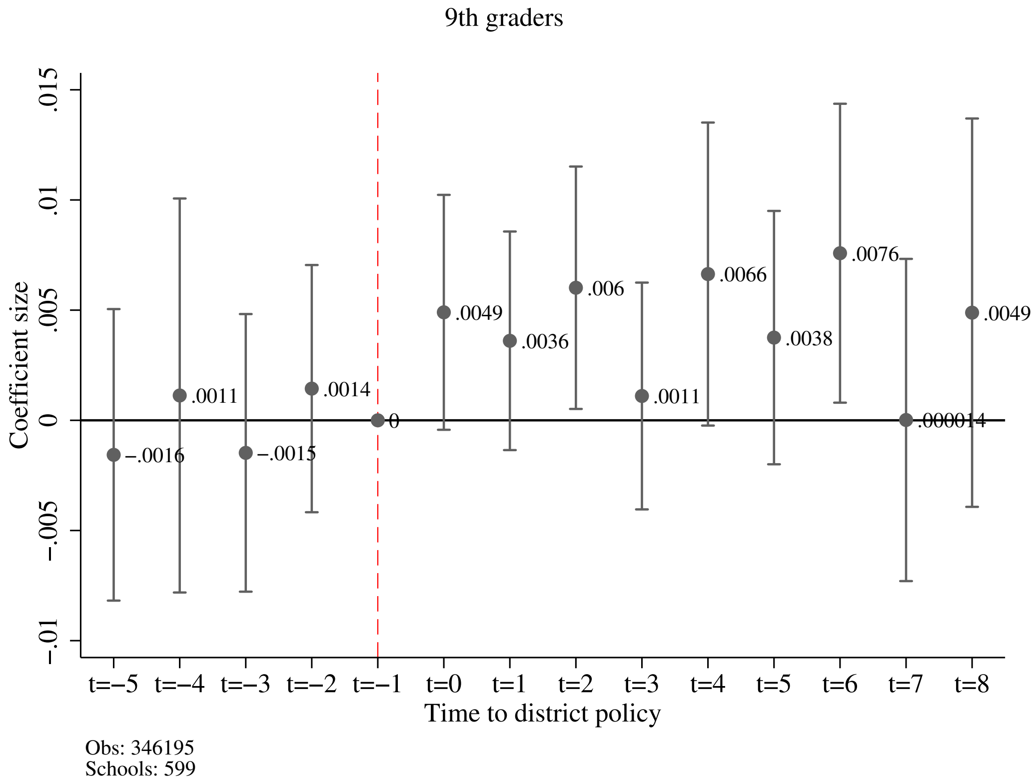


Figure 1.6: Used inhalants in past 30 days



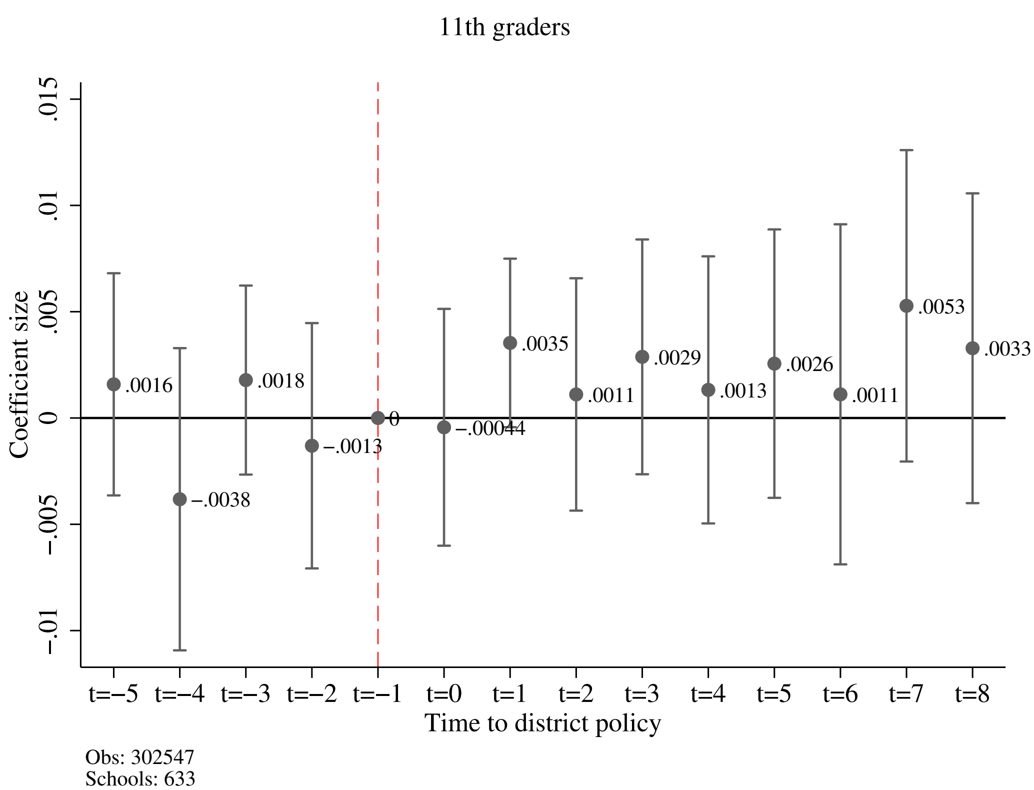


Figure 1.7: Used meth in past 30 days

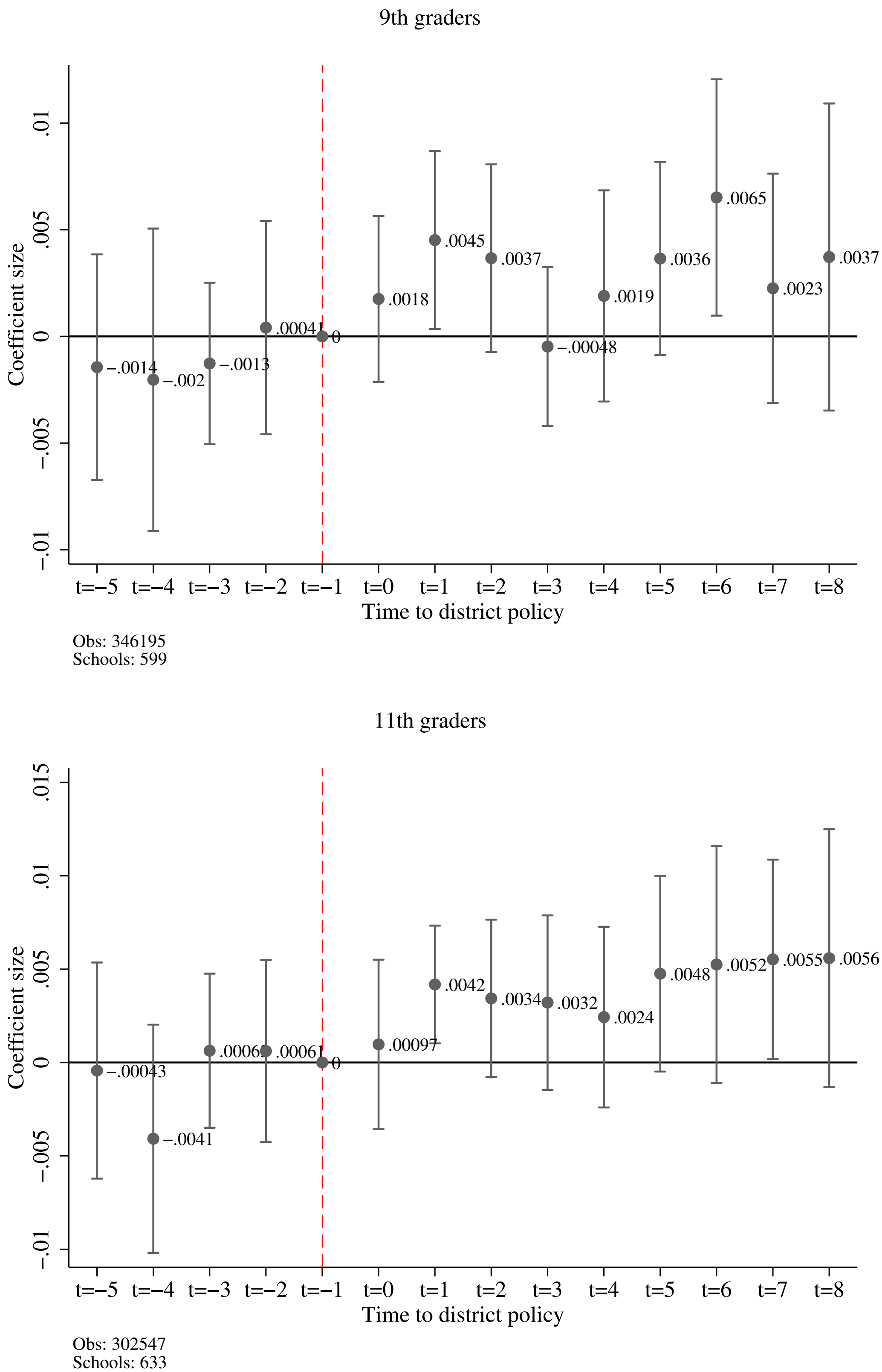


Figure 1.8: Used alcohol in past 30 days

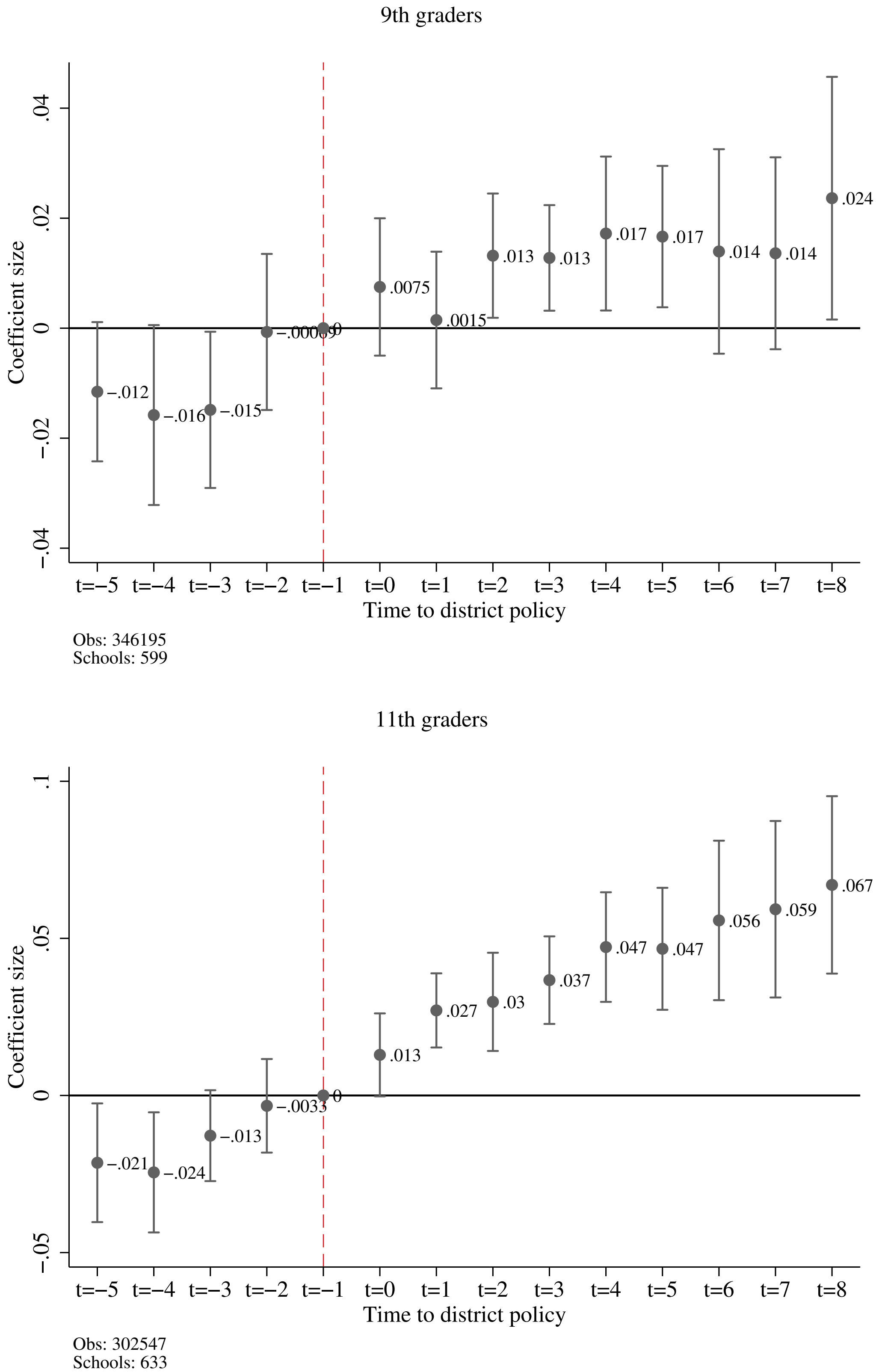
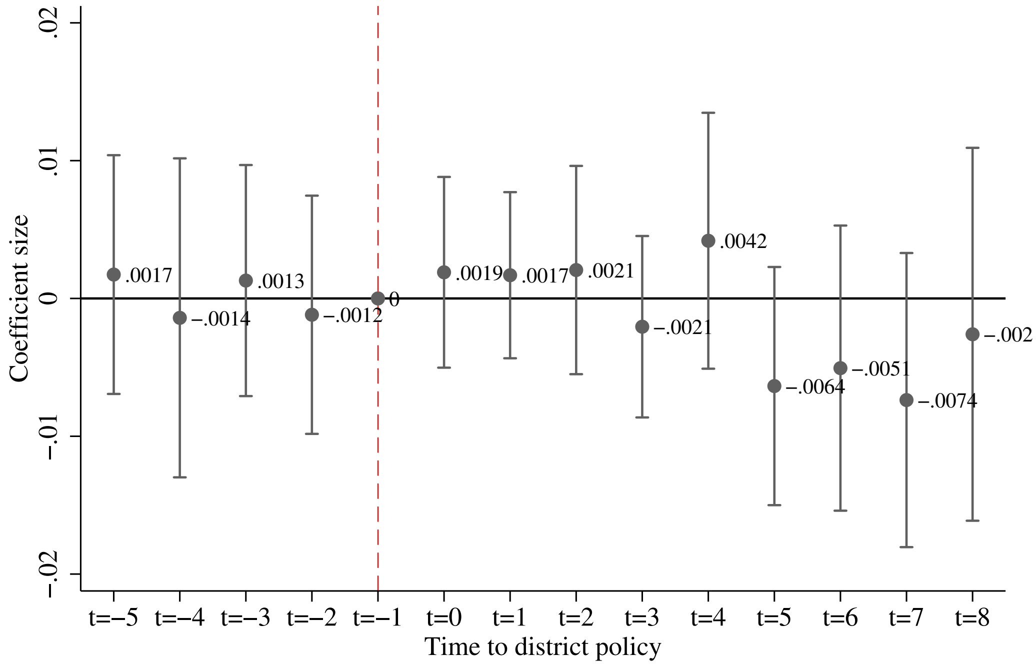


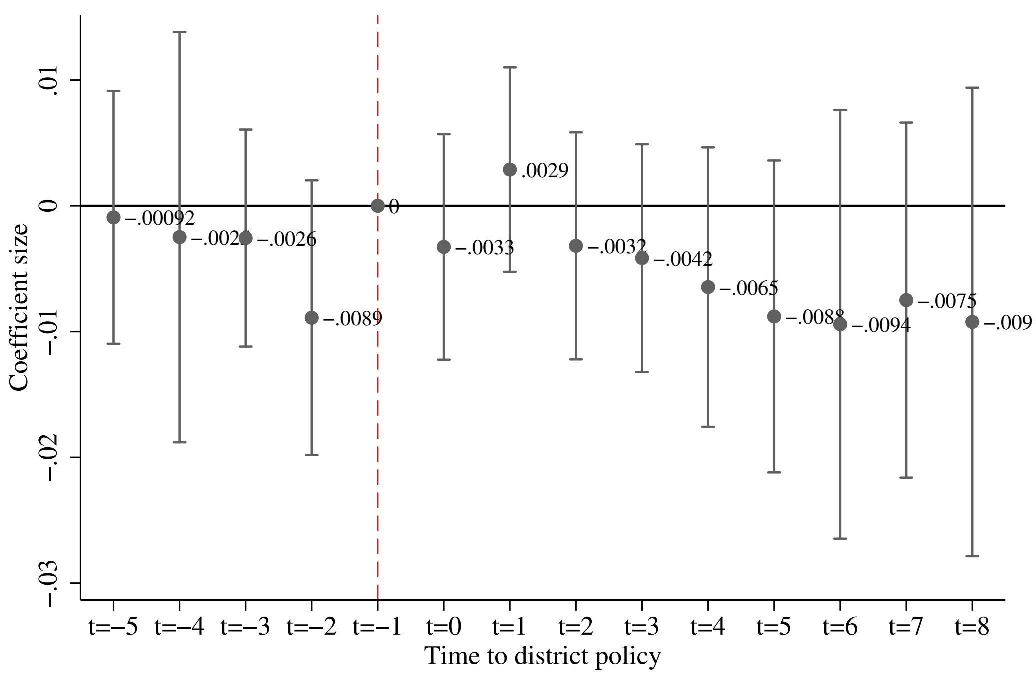
Figure 1.9: Used cigarettes in past 30 days

9th graders



Obs: 346195
Schools: 599

11th graders



Obs: 302547
Schools: 633

While it is not possible to determine whether in practice each and every school district by default continued enforcing the campus cellphone ban after the state law change (as opposed to allowing unregulated cellphone use by students), the data suggest this was generally the case. The top panels of Figures A.3 through A.9 show the transition from 2002 (a year prior to the state repeal) through 2005 for 9th graders in school districts that did not enact policy changes pertaining to campus cellphones until at least 2006. Drug behaviors do not appear to change as students enter an "interim" period between the state repeal and a response in local policy.[17] The bottom panels of these figures reproduce the event study graphs for this subset of districts, showing the change in drug use when going from the "interim" period into a most likely less restrictive environment with respect to campus cellphone use.[18] Positive and significant impacts of the change in cellphone policy emerge within a year for cocaine, marijuana, methamphetamines, and inhalants, though coefficients differ somewhat with the smaller and more select sample (e.g. the effect on marijuana peaks at 4.8 percentage points in $t = 6$ instead of 2.3).

1.4.3 Impacts As Shifts and Trend Breaks

In order to account for possible pre trends in the outcome measures leading up to district code revision (as well as possible changes in trends resulting from code revision), I also estimate the following model:

$$
\begin{aligned}
Y_{isr} \quad = \quad & \phi_1 \text{Post district revision}_{sr} + \phi_2 E_{sr} + \phi_3 \text{Post district revision} \times E_{sr} \\
& + \gamma X_{isr} + \alpha_s + \theta_r + \varepsilon_{isr} \qquad (1.3)
\end{aligned}
$$

In the above equation, ϕ_1 captures the effect of the district policy change in terms of a level shift in the outcome measure, while ϕ_2 accounts for any linear trend in substance use prior to

[17]This is robust to using the subset of students in districts not enacting a policy until 2004, as well as 2005, and also for 11th graders, not shown.

[18]Note that over the study period 2002-2013, districts that adopted policies in 2006 and onwards could not have a period $t = 8$ in the event window.

code revision. The parameter ϕ_3 captures any linear departure from that outcome trend in the post period, which this framework will attribute to the district policy event.

Table 1.7 presents the results of estimating Equation 1.3 on the 9th and 11th grade samples for all substance outcomes. Results using the pooled sample are also shown in Appendix Table A.1. As mentioned in the previous section, preexisting trends in alcohol consumption among 9th graders, as well as marijuana and alcohol consumption among 11th graders, appear to lead up to the district policy event. Additionally, the effect on marijuana use among 9th graders appears more positive over time, while any impact on cigarette smoking among 11th graders (Figure 1.9 bottom) appears almost to become negative over time.

Among 9th graders, estimating the model with linear pre and post trends leaves the coefficient on "Post" significant only in the marijuana specification, suggests a modest pre trend ("Evt Time") in alcohol, and reveals no other trends or level shifts. Results support the existence of significant upward trends in marijuana and alcohol consumption among 11th graders preceding the policy event ("Evt Time" in columns 3 & 6). After accounting for trends, a significant upward shift in 11th grade cocaine/crack, marijuana, and alcohol consumption is associated with the period following district governance code revision. All effects except for the level shift in 11th grade cocaine use remain significant after performing the multiple testing correction discussed above. Based on inspection of the prior event studies, estimation of linear trends is likely only appropriate in the specifications involving alcohol and marijuana among 11th graders, and perhaps only for alcohol among 9th graders. On one hand, it appears that the popularity of alcohol and marijuana may have risen in tandem with gradual environmental changes endogenous to district code revision. However, use of both substances additionally exhibits a level shift upon the policy change. Additionally, analyses of the other drug outcomes qualitatively suggest upward level shifts without mirroring the pattern in preceding trends.

In Appendix Tables A.2 and A.3, I further control for school-specific and district-specific time trends, respectively. With the additional sets of covariates, the estimated level shift in 9th grade marijuana use is no longer statistically significant, though still positive. The estimated level shift

Table 1.7: Trend Break Specification

	(1) Any drugs	(2) Coc./crack	(3) Marijuana	(4) Inhalants	(5) Meth	(6) Alcohol	(7) Cigarettes
Grade 9							
Post	0.006	0.003	0.009***	0.003	0.001	0.002	0.003
	(0.005)	(0.002)	(0.003)	(0.002)	(0.002)	(0.005)	(0.003)
Post x Evt Time	-0.001	-0.000	-0.000	-0.001	-0.000	-0.002	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Evt Time	0.004**	0.000	0.002	0.000	0.001	0.004**	-0.000
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Obs	346195	346195	346195	346195	346195	346195	346195
Grade 11							
Post	0.016***	0.005*	0.011***	0.002	0.002	0.012**	0.002
	(0.005)	(0.002)	(0.004)	(0.002)	(0.002)	(0.005)	(0.004)
Post x Evt Time	-0.000	0.001	-0.001	0.000	0.000	-0.000	-0.002
	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
Evt Time	0.009***	-0.000	0.008***	-0.000	0.000	0.006***	0.000
	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
Obs	302547	302547	302547	302547	302547	302547	302547
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Outcomes are indicators for any use of the substance in past 30 days. Controls include age, gender, race, school level enrollment, demographic composition, FRPL, and proportion English learners. Standard errors in parentheses, clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in 11th grade marijuana use remains positive and statistically significant when allowing for either school-specific and district-specific trends.

1.4.4 Academic Performance

The vast majority of relevant literature pertaining specifically to mobile phone use in the learning environment (e.g. Beland and Murphy (2016)) has shown cellphones to have a negative or ambiguous impact on student academic performance. Due to the great interest in this particular category of outcomes, I examine the impact of campus cellphones on self reported academic performance, reflected as having received mostly scores of C or worse over the course of the school year (a C in many settings indicates minimum proficiency required to proceed into a more advanced course in a sequence). No statistically significant effects are found for either 9th or 11th graders from estimating Equation 1.1 using this outcome. Figure A.11 in the Appendix presents the results from estimating Equation 1.2 with this outcome.

1.5 Mechanisms

If we assume that prospective buyers, in order to purchase drugs, must establish contact and negotiate with a relatively small number sellers, then removing the need to meet in person might increase trades by reducing the cost of building the requisite relationships. Firstly, cellphones may enable sellers to interact with more buyers at once than would otherwise be possible, as well as grant interested buyers more convenient access to sellers, who may otherwise be out of their way. Based on accounts of law enforcement involved in drug purchasing operations, drug users benefit from convenience, expedience, and—to an extent—safety from detection, while among sellers "cellphones and word of mouth to sell the product tends to be common practice" (Smith, 2014).

Particularly, both the real and subjective risk involved in trading illicit drugs may be reduced by the ability to communicate by text message before physically meeting. In addition to the need for buyers and sellers to establish mutual trust, both parties might be wary of drawing unnecessary attention during lengthy negotiations in public.

That the analyses in this study generally reveal no effects of a new district cellphone policy on student cigarette smoking may thus be considered consistent with both the risk and effort aspects of the cost reduction scenario described above. Since cigarettes may be legally possessed and purchased by a large fraction of the student body (anyone over the age of eighteen), baseline access to potential suppliers may very likely be great enough that cellphones offer little in the way of additional convenience. Moreover, the subjective risk or social stigma associated with using cigarettes might be lower than that associated with strictly illegal items. If being discovered while purchasing cigarettes incurs a relatively low cost, then a reduction in the risk of discovery might be less likely to pivotally influence the decision to smoke.

With regard to campus cellphone use in particular, the school environment entails a high density of peers and replaces the relatively personal supervision of parents and other household members with that of staff who are tasked with oversight of relatively larger populations.[19] Furthermore, heterogeneous impacts on drug outcomes based on amount of time during the school day when phone use is permitted provides supporting evidence that changes in drug consumption are in fact linked to ease of campus cellphone communication.[20] For this purpose, policies are categorized as follows: 1. those permitting use before first bell and after last bell (thus officially allowing for device possession on campus), and 2. those further permitting use during the school day, such as during lunch period, nutrition break, passing periods, or in class for instructional purposes (in addition to before and after school).

Table 1.8 presents the coefficients on "Post" from estimating Equation 1.3 using separate samples based on time of day when students may use their phones. If it is the case that unsupervised, private communication between students facilitates drug trades, then the samples of students in districts allowing the use of phones during lunch, nutrition breaks, and passing periods, should see larger/more significant impacts on substance abuse than students in districts only permitting use before and after the school day (e.g. 9th grade use of cocaine/crack, marijuana, and alcohol).

Among 9th graders, this appears to be somewhat true; in particular, students subject to more lenient policies seem to drive the focal marijuana result. However, this pattern does not hold among 11th graders. Overall, few differences are large or statistically significant between students under

[19] A nationally representative U.S. study conducted by Wolfe et al. (2016) finds that school cellphone restrictions effectively limit the incidence of sexually explicit text messaging between adolescents, while parental efforts specifically targeting inappropriate phone use prove largely irrelevant. The authors posit that certain risk behaviors are generally not attempted outside of school due to relatively more intense household supervision of adolescents' activities.

[20] In line with the intent of the state law change, which was to allow for policies appropriate for different local circumstances and environments, district policies regulating cellphone use vary not only in time of day permitted, but also in fairly specific stipulations (e.g. prohibition against the wearing of headphones).

Table 1.8: Heterogeneity: Time of Day When Cellphone Use is Allowed

	(1) Any drugs	(2) Coc./crack	(3) Marijuana	(4) Inhalants	(5) Meth	(6) Alcohol	(7) Cigarettes
Grade 9							
Before/aft. hours ONLY	-0.006	0.001	-0.001	0.004	0.001	-0.005	0.002
	(0.009)	(0.002)	(0.005)	(0.003)	(0.003)	(0.009)	(0.004)
	117293	117293	117293	117293	117293	117293	117293
School day ALSO	0.008	0.004	0.010**	0.002	0.002	0.003	0.003
	(0.006)	(0.003)	(0.004)	(0.003)	(0.002)	(0.006)	(0.003)
	228902	228902	228902	228902	228902	228902	228902
	P-values for test that coefficients equal across policy type						
	0.09	0.762	0.117	0.314	0.622	0.324	0.818
Grade 11							
Before/aft hours ONLY	0.020**	0.004	0.008	0.008**	0.003	0.019**	0.006
	(0.009)	(0.003)	(0.007)	(0.003)	(0.003)	(0.009)	(0.006)
	104090	104090	104090	104090	104090	104090	104090
School day ALSO	0.010	0.004	0.007	-0.001	0.001	0.005	-0.001
	(0.007)	(0.003)	(0.005)	(0.003)	(0.002)	(0.007)	(0.005)
	198457	198457	198457	198457	198457	198457	198457
	P-values for test that coefficients equal across policy type						
	0.507	0.429	0.27	0.019	0.144	0.477	0.145
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Outcomes are indicators for any use of the substance in past 30 days. Each cell shows the coefficient on "Post" from estimating the trend break regression specified in Equation 1.3, using only the sample specified in the row title. Controls include age, gender, race, school level enrollment, demographic composition, FRPL, and proportion English learners. Standard errors in parentheses, clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the different policy categories, and it is difficult to tell whether and how additional permitted usage time between/during classes affects student behavior.

1.6 Robustness to Concurrent Changes in Controls

While control variables representing various aspects of demographic variation are accounted for in all estimations presented, changes in the composition of either students sampled or the actual population enrolled in the public school system may take place concurrently with broader, unobserved changes that could be correlated with both student drug use and the school policy environment. Hence, if policies were altered to accommodate a changing body of students, this might introduce a positive or negative bias into the estimation of the impacts of the policy change, depending on whether or not these changes tend to result systematically in a student body that is more or less inclined to use drugs.

From estimating Equation 1.1 using each control as an outcome to be regressed on the others, nearly all controls apart from the enrollment variables, proportion of student body that is Hispanic, and age are statistically significantly related to the post period. However, in period-by-period event studies using the controls as outcomes, these changes over time occur as gradual trends with no apparent connection to the policy event of interest (not shown). Estimating the trend break specification (Equation 1.3) confirms this to be the case, except with respect to a level decrease in girl respondents and a level decrease in school wide proportion of Black students, for which the event study graphs are presented for the 9th grade sample in Figure A.10. Note that the heterogeneity analysis that follows (Section 1.7) does not appear to provide support for a scenario under which either of these dynamics contributes to the main patterns from the prior sections: with respect to 11th graders' consumption of alcohol, the estimated impact is significantly larger for girls, while the sample of Black students reveals generally large and significant impacts of the policy event on drug use. Of course, both of these variables are also included in all estimated specifications.

1.7 Policy Implications and Heterogeneous Effects

One key argument motivating the New York City DoE's removal of its campus cellphone ban centered on perceived inequity along demographic lines in enforcement, with schools having proportionally larger minority enrollment also employing stricter treatment of students. Hence, heterogeneity in impacts by demography may contribute relevant insight and additional context for qualifying these concerns. Previous literature has also possibly provided reason to believe that the role of cellphone use in fomenting problem behaviors may occur heterogeneously by socioeconomic factors, aspects of which are in many environments correlated with race.[21]

Table 1.9 presents the coefficients on "Post" from estimating Equation 1.3 on separate samples by self-identified race category (i.e. Black or African American, White, Asian, and Other). Joint testing of the effect of district policy revision across the samples suggests a degree of sociodemographic heterogeneity in the role of cellphones in health risk behaviors.

Among 9th graders, a large and significant "Post" coefficient in the marijuana specification can be found among the samples of students identifying as Black or one of the race options categorized under Other (e.g. American Indian/Alaska Native, Hawaiian/Pacific Islander, etc). Among students identifying as White, the marijuana specification also exhibits a significant effect (though not as large), while yet a smaller but still significant effect also emerges with respect to use of inhalants. The "Post" coefficient is not significant in any of the specifications within the sample of Asian students, except in the inhalants specification, where it is slightly negative. Among 11th graders, the sample of White students appears to drive the estimated policy effects in the marijuana and alcohol specifications.

1.7.1 Proportion of School Eligible for Free and Reduced Price Lunches (FRPL)

Table 1.10 presents the coefficients on "Post" from estimating Equation 1.3 on separate samples of students based on being surveyed in a school where the proportion eligible for free and reduced price

[21] Koivusilta et al. (2007) report that children with low socioeconomic status both exhibit high frequency phone use and are more likely to use their devices for playing games.

Table 1.9: Heterogenety: Race

	(1) Any drugs	(2) Coc./crack	(3) Marijuana	(4) Inhalants	(5) Meth	(6) Alcohol	(7) Cigarettes
Grade 9							
Black	0.026**	0.006	0.020*	0.002	-0.001	0.018	0.001
	(0.013)	(0.008)	(0.012)	(0.008)	(0.007)	(0.012)	(0.008)
	32060	32060	32060	32060	32060	32060	32060
White	0.004	0.002	0.008**	0.005*	0.002	0.002	0.004
	(0.006)	(0.002)	(0.004)	(0.003)	(0.002)	(0.006)	(0.003)
	174279	174279	174279	174279	174279	174279	174279
Asian	-0.006	-0.003	-0.002	-0.005*	-0.004	-0.007	-0.004
	(0.007)	(0.002)	(0.003)	(0.003)	(0.002)	(0.006)	(0.003)
	75257	75257	75257	75257	75257	75257	75257
Other	0.026**	0.014*	0.026*	0.010	0.008	0.014	0.017*
	(0.013)	(0.008)	(0.014)	(0.009)	(0.006)	(0.011)	(0.010)
	64599	64599	64599	64599	64599	64599	64599
	P-values for test that coefficients equal across race						
	0.03	0.102	0.012	0.151	0.082	0.14	0.07
Grade 11							
Black	0.001	0.012	-0.009	0.004	0.006	0.000	-0.003
	(0.015)	(0.008)	(0.013)	(0.007)	(0.008)	(0.014)	(0.008)
	25840	25840	25840	25840	25840	25840	25840
White	0.024***	0.003	0.017***	0.002	0.001	0.020***	0.004
	(0.007)	(0.003)	(0.005)	(0.003)	(0.002)	(0.007)	(0.005)
	157537	157537	157537	157537	157537	157537	157537
Asian	-0.001	-0.000	0.003	-0.003	-0.002	-0.004	-0.005
	(0.007)	(0.003)	(0.006)	(0.002)	(0.002)	(0.007)	(0.006)
	66619	66619	66619	66619	66619	66619	66619
Other	0.000	0.012	0.004	0.012	0.014*	-0.003	0.006
	(0.015)	(0.008)	(0.012)	(0.009)	(0.008)	(0.014)	(0.011)
	52551	52551	52551	52551	52551	52551	52551
	P-values for test that coefficients equal across race						
	0.028	0.266	0.151	0.153	0.277	0.047	0.606
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Outcomes are indicators for any use of the substance in past 30 days. Each cell shows the coefficient on "Post" from estimating the trend break regression specified in Equation 1.3, using only the sample specified in the row title. Controls include age, gender, school level enrollment, demographic composition, FRPL, and proportion English learners. Standard errors in parentheses, clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

lunches lies either above or below the median for that year.[22] While it may be perhaps unintuitive that 9th graders in schools with relatively lower meal program eligibility, and which may thus be better off financially, exhibit a greater increase in post period likelihood of drug use, none of these differences are statistically significant in either grade. It is furthermore possible that poverty exercises conflicting influences on drug use: while schools with higher average socioeconomic status may have more resources to expend on safety, their students may also be better able to afford illegal substances.[23]

1.7.2 Gender

With respect to gender differences in cellphone use, girls have been found not only to take their phones to school with them more frequently, but also to use them more frequently during inappropriate times (Sánchez-Martínez and Otero, 2009), and are more likely to develop psychological dependence and related health issues (Augner and Hacker, 2012; Roser et al., 2016). On the other hand, frequent phone use among Finnish teenagers has been found to correlate more strongly with excessive alcohol consumption and smoking for boys than for girls (Leena et al., 2005). In Taiwan, Yang et al. (2010) note conversely that high frequency cellphone use among girls under 15 is particularly associated with illicit drug use.[24]

Table 1.11 presents the coefficients on “Post” from estimating Equation 1.3 on separate samples of 9th and 11th grade boys and girls. Each cell in the table shows the shift associated with the period post district code revision in the outcome specified in the column header, for the sample specified by the row title. While, among 9th graders, boys appear to be more likely than girls to

[22] Quartiles are calculated at the school level: within each year, the median proportion FRPL eligible is determined among schools, which may thus alternate between being above and below the median form year to year.

[23] Patrick et al. (2012) in fact find teen alcohol and marijuana consumption to be linked with higher childhood socioeconomic status.

[24] Yang et al. (2010) also find, interestingly, that cellphone use among boys correlates with chewing betel nuts—an association that does not occur among girls. Used in a manner similar to caffeine, the nuts are often chewed as a part of socializing with colleagues. While not illegal, they cause oral cancer (Sui and Lacey, 2015).

Table 1.10: Heterogeneity: School is Above/Below Median Proportion FRPL Eligible

	(1) Any drugs	(2) Coc./crack	(3) Marijuana	(4) Inhalants	(5) Meth	(6) Alcohol	(7) Cigarettes
Grade 9							
Above	-0.003	0.002	-0.000	-0.002	-0.001	-0.004	-0.003
	(0.009)	(0.004)	(0.007)	(0.004)	(0.003)	(0.008)	(0.005)
	106996	106996	106996	106996	106996	106996	106996
Below	0.006	0.003	0.010***	0.005*	0.002	0.004	0.006**
	(0.006)	(0.002)	(0.004)	(0.003)	(0.002)	(0.006)	(0.003)
	239199	239199	239199	239199	239199	239199	239199
			P-values for test that Above = Below				
	0.363	0.763	0.184	0.147	0.378	0.394	0.121
Grade 11							
Above	0.009	0.004	0.004	0.002	-0.000	0.006	-0.003
	(0.009)	(0.004)	(0.007)	(0.003)	(0.003)	(0.009)	(0.006)
	92827	92827	92827	92827	92827	92827	92827
Below	0.020***	0.004	0.012**	0.003	0.003	0.017**	0.004
	(0.007)	(0.003)	(0.005)	(0.004)	(0.003)	(0.007)	(0.004)
	209720	209720	209720	209720	209720	209720	209720
			P-values for test that Above = Below				
	0.339	0.947	0.273	0.956	0.544	0.34	0.369
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Outcomes are indicators for any use of the substance in past 30 days. Each cell shows the coefficient on "Post" from estimating the trend break regression specified in Equation 1.3, using only the sample specified in the row title. Controls include age, gender, race, school level enrollment, demographic composition, and proportion English learners. Standard errors in parentheses, clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.11: Heterogeneity: Gender

	(1) Any drugs	(2) Coc./crack	(3) Marijuana	(4) Inhalants	(5) Meth	(6) Alcohol	(7) Cigarettes
Grade 9							
Girls	0.002	0.002	0.010**	0.004	-0.000	-0.003	0.002
	(0.006)	(0.002)	(0.004)	(0.003)	(0.002)	(0.006)	(0.003)
	178732	178732	178732	178732	178732	178732	178732
Boys	0.010*	0.005*	0.008**	0.002	0.003	0.008	0.005
	(0.006)	(0.003)	(0.004)	(0.002)	(0.002)	(0.005)	(0.003)
	167463	167463	167463	167463	167463	167463	167463
	P-values for test that Girls = Boys						
	0.217	0.204	0.776	0.717	0.367	0.063	0.467
Grade 11							
Girls	0.020***	0.004	0.013**	0.002	0.002	0.015**	0.001
	(0.007)	(0.003)	(0.005)	(0.002)	(0.001)	(0.006)	(0.004)
	156252	156252	156252	156252	156252	156252	156252
Boys	0.011	0.005	0.009	0.003	0.002	0.008	0.003
	(0.007)	(0.003)	(0.006)	(0.004)	(0.003)	(0.006)	(0.005)
	146295	146295	146295	146295	146295	146295	146295
	P-values for test that Girls = Boys						
	0.256	0.64	0.676	0.845	0.911	0.329	0.812
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Outcomes are indicators for any use of the substance in past 30 days. Each cell shows the coefficient on "Post" from estimating the trend break regression specified in Equation 1.3, using only the sample specified in the row title. Controls include age, race, school level enrollment, demographic composition, FRPL, and proportion English learners. Standard errors in parentheses, clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

report having used drugs, this is less evident for 11th graders, and none of the gender differences in either grade are statistically significant at conventional levels except for that with respect to 9th grade use of alcohol. At a glance, it may be reasonable to suspect that 9th grade boys may be driving the effects seen for 9th graders in alcohol consumption, a pattern which would align with the findings of Leena et al. (2005). Figure A.2 compares the results from estimating the event study model with this outcomes on the samples of 9th grade girls (top) and 9th grade boys (bottom).

1.8 Summary

This study finds supporting evidence that removing campus cellphone bans elevates the likelihood of student substance abuse, lending credence to the concerns of law enforcement and policy makers that in many cases gave rise to such rules. Although existing research in economics and education has assessed the benefits and challenges of applying mobile technology to improving academic outcomes,[25] few studies have applied similar methodology to exploring the devices' role in broader areas of high school students' lives, which may influence human capital development and later life outcomes.

In particular, cellphone rule changes in California districts appear to have had a clear and pronounced effect on 9th grade marijuana use, supporting the CHKS's focus on freshmen; the transitional first year of high school indeed appears to be a formative period with regard to health compromising behaviors. Effect magnitudes are important relative to the incidence of substance abuse before bans were lifted. Furthermore, allowing campus mobile phone use only clearly appears to exacerbate abuse of illegal or highly stigmatized substances, which is to be expected if cellphone communication reduces the transaction costs incurred in obtaining drugs.

Ultimately, recent trends towards more relaxed device policies may be justified by convenience, educational benefit, and safety in certain critical situations. However, it is likely also the case that without adequate preparation, including establishing trust and understanding between students and educators, these changes may compromise the safety of the school environment in other respects. While each of the above considerations will naturally bear more relevance in some settings than in others, this study suggests that effective policy will generally demand a broad view of consequences over a narrow focus on select metrics.

[25] For further reading on this subject, see Dietz and Henrich (2014), Ellis et al. (2010), Hawi and Samaha (2016), Junco (2012), Patterson and Patterson (2016), and Sana et al. (2013).

CHAPTER 2

IMPACT OF INTERNET ACCESS ON STUDENT LEARNING IN PERUVIAN SCHOOLS (WITH LEAH LAKDAWALA AND EDUARDO NAKASONE)

2.1 Introduction

In recent decades, developing countries have achieved large increases in school enrollment, particularly at the primary level. However, most remain far behind developed countries in terms of school quality as measured by student achievement (Glewwe and Kremer 2006). Traditional policies — such as hiring additional teachers or providing textbooks — do not appear to have improved student achievement in developing countries (Kremer et al., 2013). In turn, there has been an increasing interest in new approaches, such as Information and Communication Technologies (ICTs), to improve school performance. Their potential to boost modern-day digital competencies, promote interactive student-centered teaching models, and provide up-to-date learning materials even in remote areas (World Bank, 2018) has encouraged considerable investments in ICTs in schools developing countries (World Bank, 2018; Escueta et al., 2017; One Laptop per Child, 2016; UNESCO, 2012; Trucano, 2016; International Telecommunication Union, 2014).

Among ICTs, the internet in particular may have an important role as a pedagogical tool in developing countries. Internet access can provide students in understaffed schools with otherwise unavailable sources of information (Levin and Arafeh, 2002). Additionally, internet can expand teachers' access to references and teaching aids as well as their ability to share information among peers (Jackson and Makarin, 2016; Purcell et al., 2013). However, as with any new technology, benefits materialize only after a period of learning and adaptation, suggesting the importance of understanding the dynamic effects of ICT interventions over time.

Despite the potential of internet to improve learning, few studies have rigorously evaluated its impacts on student performance in developing countries. While previous research in developed countries has led to ambivalent conclusions on the effectiveness of internet access as a learning input (Belo et al., 2014; Faber et al., 2015; Gibson and Oberg, 2004; Goolsbee and Guryan, 2006; Machin et al., 2007; OECD, 2015; Vigdor et al., 2014), school-based connectivity can be potentially more important in developing countries due to lower levels of teacher skills, larger class sizes, and limited access to other conventional inputs.[1] Additionally, since the broader literature on ICTs (Escueta et al. 2017 and Bulman and Fairlie 2016) typically examines bundles of interventions such as computer access, learning software, and internet expansion[2], it is not yet clearly understood how internet on its own affects learning.[3]

Moreover, most prior studies of internet access — and of ICTs more generally — have been based on short-term observation of small samples, and are thus only prepared to detect large and immediate treatment effects. Importantly, such studies may overlook potential longer term impacts that may follow from an initial learning period, during which teachers, students, and administrators adapt to new technology. Hence, detecting gains in learning that may arise over such a learning period requires a longer evaluation window.

We examine the impact of internet access on student performance in the universe of public primary schools in Peru that initially acquired internet between 2007 and 2014, emphasizing its dynamic effects in schools over time. During our sample period, about 933,000 students gained access to internet. We link administrative data on school-based access to internet with school-

[1]In a recent paper, Malamud et al. (2018) investigate the impact of a home-based internet on students' school performance in Peru. The authors find no statistically significant effect on standardized test scores 9 months after the implementation of the program. The authors posit that the lack of impact might in part be due to relatively little time spent at home using computers. Additionally, children might use internet as a tool for entertainment rather than learning. Both of these problems might be reduced when internet is provided at school rather than home.

[2]Some notable exceptions analyze the individual impact of computer access (Beuermann et al., 2015; Cristia et al., 2017; Barrera-Osorio and Linden, 2009; Mo et al., 2013; Toyama, 2015; de Melo et al., 2013; Sharma, 2014; Meza-Cordero, 2017; Bai et al., 2016) or learning software (Bando et al., 2016; Banerjee et al., 2007; Carrillo et al., 2010; He et al., 2008; Linden, 2008; Muralidharan et al., 2016) in developing countries. However, there is little evidence on the impact of internet access.

[3]Previous work (e.g., Cristia et al., 2014; Bet et al., 2014; Sprietsma, 2007) has assessed the impact of programs that have provided school-based internet as part of larger schemes of ICT expansion. However, disentangling effect of internet from other technologies is not the aim of these papers.

average math scores from a large-scale national test that covers nearly the universe of second graders in public schools in Peru. We construct a panel dataset of 5,903 schools that gained internet during our study period. To fully exploit the longitudinal structure of our data and identify dynamic effects, we employ an event study framework in addition to a trend break analysis — approaches which also allow us to detect and control for pre-existing trends in student performance. Since we observe a large panel of schools over eight years, we are also able to assess how other determinants of school performance change over time, tracing out the dynamics of student, teacher, and school-level inputs. This allows us to discuss potential channels through which internet affects school performance, as well as to thoroughly explore the possibility that other confounding factors drive our results.

Using within-school variation in the timing of internet installation, we find that internet access leads to initial modest math score improvements of 0.042 to 0.076 standard deviations in the first 18 months after installation. Importantly, this advantage also grows significantly over time (at a rate of about 0.047 standard deviations per year on top of an initial level improvement), reaching 0.29 standard deviations five periods after installation.

We posit that this growth in our estimated impacts over time reflects an adaption period, during which schools must learn to integrate new technologies. Namely, we observe that schools respond to internet access by hiring teachers with formal training in digital skills, and that this process follows only gradually. In particular, schools are 2.1 percentage points more likely to have a computer-trained teacher in the first year after installing internet and 9.6 percentage points more likely by the fifth year after installation (a doubling of the pre-internet likelihood of having computer-trained teachers). Hence, the fact that the gradual growth over time in test scores shadows growth in the staffing of computer-trained teachers may suggest that complementary investment in staff computer proficiency is needed to fully exploit internet-enabled classroom capabilities.[4]

Furthermore, our data offers suggestive evidence for two potential channels through which

[4] Similarly, evaluations of laptop provision in the U.S. (Hull and Duch 2016) and computer assisted learning in China (Mo et al. 2015) estimate that the effects of ICT interventions grow over time.

internet access improves test scores. First, gains in test scores are predominantly driven by schools with high student-to-teacher ratios, suggesting that internet-related activities may supplement the limited individualized attention that teachers can provide in large classes. Second, gains in test scores are largest for schools with relatively low teacher qualifications — as measured by the per student count of teachers holding a pedagogical or university degree — which is consistent with internet resources compensating for or addressing deficits in teacher training.[5]

Additionally, our main findings are robust to a number of alternative explanations. Concerning potential endogeneity in the timing of internet access, we find that, conditional on year and school fixed effects and a set of time-varying school characteristics (e.g., school size, infrastructure, and resources), schools receiving access to internet do not exhibit positive pre trends in performance or have different pre internet scores compared with those that do not. Second, we also find that our results are not explained by concurrent changes in other inputs (e.g., infrastructure, textbooks, or computers) or by regional (and even school-specific) trends. Third, while our main specifications are based on an unbalanced sample of schools, our results are qualitatively similar across different sample specifications (including a balanced panel of schools). Lastly, analyzing student composition within schools shows that our findings cannot be driven by endogenous sorting of students.

We contribute novel insights and perspective to a nascent body of research in developing countries on the educational benefits of school-based internet access, as well as to a wider literature concerning ICTs as schooling inputs. Primarily, the size and time span of our data present opportunities to complement and contextualize existing knowledge from randomized control trials (RCTs), which largely comprise the current work relating ICTs and academic performance. Whereas RCTs are mostly constrained to observe short term effects (rarely beyond one academic year), we use our extended study period to analyze the effects of internet access up to 5 years after it is introduced to schools. Our results indicate that this longer evaluation window is highly relevant to understanding the impact of internet access, due to the dynamic effects of internet on learning over

[5] These findings complement evidence from the developed country context, where providing teachers with online access to "off the shelf" lesson plans improves students' math achievement and that benefits were larger for weaker teachers (Jackson and Makarin 2016).

time.

Additionally, the large scale of our sample — about 6,000 public schools — provides power to detect the short-run impacts of internet, which appear to be modest in size. Namely, we discover average math score gains of 0.042-0.076 standard deviations in the first year of internet access, statistically significant at the 5% level. While several evaluations of programs distributing computers in developing countries report similarly sized short-run effects (ranging from 0.052-0.088 standard deviations 5-22 months after initial access; Bet et al. 2014, Barrera-Osorio and Linden 2009, Cristia et al. 2017, Beuermann et al. 2015, Mo et al. 2013), none of these studies are able to statistically distinguish effects from zero, perhaps in part because they analyze far smaller samples of schools (ranging from 13 to 318 schools). Since we investigate a massive national policy (which affected a wide array of public primary schools serving roughly 900,000 children), we can assess conditions relevant for internet provision programs. We find that the effects of internet access are the largest in schools with low levels of existing resources, particularly those that are understaffed and with less qualified teachers.

Finally, we are able to explicitly identify the gains that internet access confers over hardware resources alone. Anecdotally, the usefulness of school computers without internet access has been limited by lack of access to information (National Public Radio 2012) and the inability to obtain routine maintenance and software updates — particularly in remote, difficult-to-reach locations (One Laptop per Child 2011). Indeed, our data suggest that computers alone (in schools without internet access) have only modest impacts on student learning. To the best of our knowledge, the scale, longitudinal length, and setting of this study, along with the comprehensiveness of the available data uniquely address important gaps in the existing literature. More broadly, our work contributes to understanding the role of internet access in economic development. In consideration of prior research connecting faster internet to higher employment, incomes, and wealth in African countries (Hjort and Poulsen 2017), increased human capital production may factor importantly in this progress.

The paper proceeds as follows. In Section 2.2 we describe the educational setting in Peru and

Plan Huascarán, the source of variation in internet installation for many pubic schools during our sample period. We also provide details about the two administrative datasets we merge for our analysis, the *Censo Escolar* and the *Evaluación Censal de Estudiantes*. Section 2.3 first describes our event study and trend break strategies and then presents the main estimates of the impact of internet access on test scores. In Section 2.4 we investigate the robustness of our results to a number of plausible confounding factors: changes in other school resources, potential regional shocks correlated with internet access and student performance, differential pre-trends in test scores, and changes in sample composition (both in terms of schools and students). In Section 2.5 we show that the dynamic patterns in test score impacts may be explained by an adaptation period, during which schools hire computer-trained teachers. We also shed light on two potential mechanisms through which internet access generates gains in learning by examining heterogeneity in the effects. Section 2.6 concludes.

2.2 Setting and data

2.2.1 Education and ICT Access in Peru

Education in Peru is compulsory and free through the public school system beginning at age 3 and continuing until the end of secondary school. In the past few decades, Peru has greatly increased access to primary school (grades 1-6, approximately age 6-11), raising the net enrollment rate from 85.6% in 1980 to 97.9% in 2015 (The World Bank 2016). At the same time, however, the education budget has seen little growth and greater enrollment over time has eroded per-student resources (Saavedra and Suarez 2002). The World Bank (2012) finds that, within Latin America, only the Dominican Republic has a lower education expenditure-to-GDP ratio than Peru.

This dearth of resources has limited the quality of education, as evidenced by Peru's performance in the OECD's Program for International Student Assessment (PISA) — an international standardized test among 15 year olds — in 2012 and 2015. In 2012, Peru ranked last out of 65 participating countries in all three evaluated subjects, with results revealing that most Peruvian students have serious deficiencies in math (75% deficient), science (69%), and reading (60%). In 2015, Peru jumped to the 64th place (out of 70 countries in the evaluation), nonetheless demonstrating that substantial progress remains to be made. Widespread under-preparedness is evident as early as primary school. In 2007, the Ministry of Education began administering yearly standardized tests, the National Student Assessment or *Evaluacion Censal de Estudiantes* (henceforth ECE, described below), to all second graders registered in classes with five or more students. The inaugural results of the ECE in 2007 showed that only 7% of students acquired skills mandated by the national curriculum in math (Appendix Figure B.1). Despite improvement since then in test scores and in the proportion of students meeting expected skill levels, the quality of schooling has continued to prove inadequate for many children; even by 2014, fewer than a quarter of second graders achieved proficiency in math.

In the early 2000s, the Peruvian government launched *Plan Huascarán*, which produced much of the variation in school internet access observed during our sample period. This project aimed to "incorporate information and communication technologies to increase the coverage, quality, decentralization, democratization, and equity of the Peruvian education system." Project planners ambitiously aimed to install hardware and internet in 32,000 schools and to train 180,000 teachers by 2020. *Plan Huascarán* targeted primary, secondary, and integrated schools (i.e., those that teach both primary and secondary classes) under public management, particularly in rural (or peri urban) and high poverty areas. Officially, selection into the program was rationed, with each Local Educational Management Unit (UGEL) allowed to submit a set number of its schools, adhering to a set proportion of primary, secondary, and integrated schools (see Appendix Figure B.2 for an excerpt of the official Ministry of Education flow chart that outlined the specific prioritization protocol under *Plan Huascarán*).[6] The largest allocated proportion (50%) was set aside for primary schools. As prerequisites for program selection, schools needed to have electricity and a computer lab (also called an "innovation classroom") with anti-theft measures (i.e., perimeter fencing). Within each UGEL and level (e.g., primary), prioritization among qualified schools was officially based on the size of the student population with larger schools receiving higher priority. Lists

[6]An excerpt from the translated Ministry of Education directive regarding the prioritization protocol for *Plan Huascarán* is provided in Appendix Figure B.3.

of eligible schools were aggregated to the regional level and then submitted to *Plan Huascarán* headquarters, accompanied by data sheets on the characteristics of each school listed, a sketch and description of each school's computer facilities, and the discussion minutes from each UGEL.[7] Officially, no school was integrated into the project without all required information.

As a consequence of initiatives such as *Plan Huascarán* and the One Laptop per Child program (OLPC, undertaken by the Peruvian government in 2008)[8], the ratio of students to computers in primary schools fell dramatically from 240 to 6 between 2000 and 2014 .

In parallel, the government has steadily increased access to internet in schools (as described in Section 2.2.2.1). In 2013, the Ministry of Education announced plans to triple the number of schools with internet access in the country.

2.2.2 Data

Our analysis uses data from two sources administered by the Ministry of Education: the *Censo Escolar* (CE), an annual census of schools, and the *Evaluación Censal de Estudiantes* (ECE), an annual standardized test of second graders' skills.

2.2.2.1 Censo Escolar (CE) and School-based Internet Access

Each year, all school principals are required to submit two forms with their updated information to the Ministry of Education. Between April and July, principals complete a form on enrollment (by grade and age), teachers (by qualification), available supplies and materials (e.g., books, computers, and laboratories), and infrastructure (e.g., access to utilities, building characteristics, and internet connectivity). Between December and February, another form is completed on year-end pupil

[7] A translated version of the school data sheet is provided in Appendix Figure B.4.

[8] Peru has been the single largest buyer of OLPC laptops and to date has distributed close to one million laptops, mainly targeting school children in poor areas of the country. For a discussion of the OLPC program in Peru, see Trucano (2012). In general, impact evaluations of OLPC in Peru suggest that the provision of laptops did not improve student performance (Beuermann et al. 2015; Cristia et al. 2017).

outcomes (e.g. number of pupils transferring to other schools).[9] We refer to the CE for data on school characteristics such as internet access, enrollment, teachers, educational materials and resources, and physical infrastructure. Between 2007 and 2014, around 29,500 public primary schools reported administrative information in the CE annually.

We use information from the CE to determine the timing of initial internet connection among schools in our sample. Administrators report in the first semester of every year whether their school currently has access to internet. Though some schools report gaps in internet access the data do not allow us to distinguish between temporary outages and longer-term disruptions to connectivity.[10] Based on this information, we determine the first year in which a school reports gaining access to internet and interpret this as the time of connection. In our estimation framework, this implies a conservative estimate of the impact of internet access because we treat schools that might have permanently lost their connections as still being connected. Another benefit of using initial internet connection rather than current access is that we avoid bias due to endogenous changes in access. We estimate that 7,089 schools — and the 933,000 students in these schools — at some point gained internet connectivity between 2007 and 2014.[11] This implies that the rate of internet connection in schools increased from 5% to 30% and that the share of students with internet connection in their schools jumped from 23% to 66% (Figure 2.1).

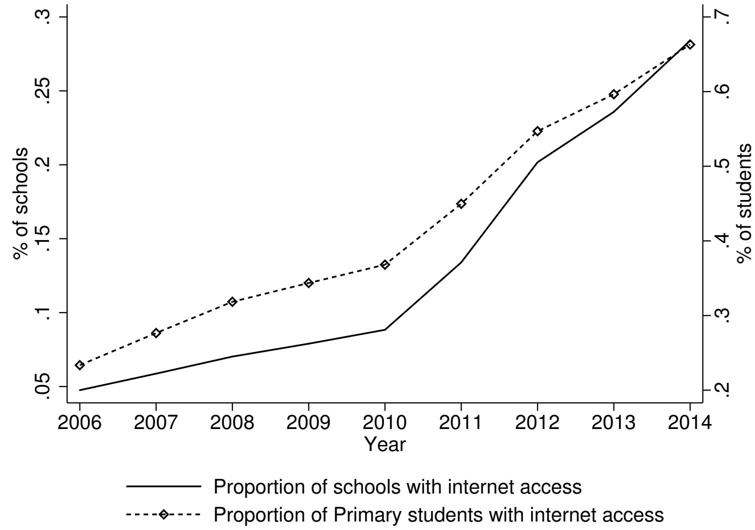
Most of the observed expansion in internet connectivity during this period was due to *Plan Huascarán*. In Appendix Table B.1 we verify that the official qualification and prioritization rules (set by the Ministry of Education) predict actual installation in practice. Schools received priority primary based on quotas by province (Local Education Management Units, UGELs), high poverty status, location in a rural versus urban area, public versus non-public management, the presence of required infrastructure (including electricity, a computer lab, and anti-theft measures such as

[9]The school year in Peru runs from March to December.

[10]Out of the 30,338 public primary schools with at least one year of data in the CE between 2006 and 2014, 13.2% report not having access after having access in a previous year; about 20% of those schools regain internet access at a later point.

[11]Note we do not observe test scores at least twice during our sample period for all 7,089 schools, so some are not included in our estimation sample. See Section 2.2.2.3 for more details.

Figure 2.1: Internet Connectivity in Primary Public Schools, 2006-2014



NOTE: The stock of schools that gained internet connections is based on the first year in which they report internet access in the Peruvian *Censo Escolar*.

perimeter fencing), and enrollment. Column 1 includes these characteristics and year effects to control for aggregate trends in internet connectivity. To approximate the status of "adequate infrastructure, in good condition," we include indicators of whether the school has a library and administrative offices. To capture high poverty status, we include district-level fixed effects. We also include UGEL fixed effects to account for the UGEL-specific quotas. As expected, most prioritization characteristics positively predict internet access, though location in urban areas is not statistically significant. In column 2, we add school fixed effects to match our main specification (described in Section 2.3), thus dropping terms for prioritization characteristics that are time-invariant within schools (e.g., UGEL, district, and location). Even with school fixed effects, facilities such as the existence of a computer room, administrative offices, and a library positively predict internet access. This pattern is consistent across columns 3 and 4, which add perimeter fencing (available only for 2010 and later) and information from school data sheets (number of computers used for instruction, number of computers used for administrative purposes, and number of teachers), respectively. Since these factors predict internet access and are also likely to influence student performance directly, we control for all of these measures (except perimeter fencing due to data limitations) in our main specifications.

2.2.2.2 Evaluación Censal de Estudiantes (ECE)

The Ministry of Education also mandates the *Evaluación Censal de Estudiantes* (ECE), a yearly standardized assessment of second graders' skills, which is administered in late November or early December (before the end of the school year). In order to ensure uniform testing environments — and to prevent content leaks or influence from school personnel — the Ministry hires independent staff to administer the test in all schools simultaneously. Furthermore, the ECE was designed for comparability of results over time: experts defined the current and future skill categories prior to the test's first administration. Hence, since its inauguration in 2007, the ECE has assessed the same skill sets with consistent relative focus. To account for differences in difficulty across cohorts, we standardize ECE scores across the universe of tested schools within each year.

The ECE allows us to gauge the academic performance of the vast majority of second graders in Peru, targeting all public and private schools that meet two criteria: 1) having at least five second graders enrolled during the test year, and 2) using Spanish as the primary language of instruction. The rationale for the first criterion is entirely budgetary, as smaller schools are often in remote areas and would take considerable resources to reach. As it stands, the ECE already requires about 40,000 field workers each year. Schools teaching in indigenous languages are covered under a separate testing schedule.

In total, 16,000 – 19,000 primary public schools participated per year (55% to 65% of all primary schools; see Figure B.5a). About 27% – 39% of schools were exempt under the minimum enrollment or language criteria. The remaining schools (between 4% and 10%) were not tested due to logistical problems. The coverage of the test was nonetheless very broad: because the smallest schools were excluded by definition and because schools in native language tend to have modest enrollments, between 83% and 90% of all second graders in the country were tested in the ECE in a given year (Appendix Figure B.5b).

2.2.2.3 Estimation Sample

Our empirical strategies exploit the timing of internet connection within schools. Therefore, we restrict our sample to only those schools that help identify the effects of internet access conditional on school fixed effects — i.e., those who had internet installed during the study period (2007-2014) — and exclude all schools without changes in internet access during this period — i.e., those that already had internet before 2007 and those that did not gain access by 2014. This leaves us with 7,089 schools, roughly a quarter of all public primary schools in Peru. We then merge this information with annual school-level average math scores from the ECE. All in all, there are 5,903 schools that were tested in the ECE, that gained internet in our period of analysis, and that are observed at least twice during our window of analysis.

Appendix Table B.2 presents summary statistics from 2007 (or each school's earliest available year in our sample) for the 25,624 schools that appear in both the CE and the ECE in our sample period. We divide the sample into schools that already had access to internet before 2007 ("early adopters"), those that became connected between 2007 and 2014 (our estimation sample), and those that had not gained access by 2014 ("non-adopters").

We highlight two key observations from Appendix Table B.2. First, only 1,359 (5.3%) of schools were internet-equipped by 2007. While 17,738 (69.2%) remained unconnected by 2014, 6,527 (25.5%) of schools gained access during our study period. The sharp expansion in internet connectivity during this period allows us to form insights from a large number of schools despite our sample restrictions.[12] Second, schools that gained internet from 2007-2014 generally fall "between" the early adopters and non-adopters in various measures of school quality. Namely, early adopters appear to be schools with higher performance, larger enrollment, and endowed with better infrastructure and educational inputs (e.g., piped water, libraries, administrative offices,

[12] Within the group of schools that gained connection between 2007-2014, there is considerable variation in the timing of access for our analysis: 4,915 schools are observed for at least one period prior to internet connection, 5,424 are observed 1-2 years after internet connection, and 3,316 are observed 3-5 years after internet connection. This allows us to implement the event study approach described in Section 2.3.1.

teachers, classrooms, computers, and textbooks). Conversely, non-adopters systematically appear worse in these areas. Consequently, our estimation sample focuses neither on the best nor on the worst performing schools.

In Figures 2.2b-2.2i below, we plot each treatment "cohort's" (by year of initial internet connection) average math performance over time.[13] Additionally, Figures 2.2a and 2.2j represent the performance of schools that gained access prior to 2007 (the start of our sample period) and that had not gained access to internet by the end of our analysis period, respectively. Generally, schools that connected later or remained unconnected exhibit lower average test scores, indicating that variation in internet access *across* schools is not random.

However, *within* a cohort of schools becoming connected in a given year (2007-2014), there do not appear to be trends in scores prior to internet access. This suggests that within cohorts of treated schools, the timing of access is unrelated to test score trends on average. Furthermore, Figure 2.2 suggests that performance gains among treated schools are modest initially and only become sizable in the medium term. In contrast, the relative math performance of schools that never connected to the internet appears to have stagnated over the period of analysis. Furthermore, it appears that schools with internet connectivity prior to 2007 continued to experience increases in average test scores during our period of analysis.[14] This pattern motivates the strategies employed in our main analysis (Section 2.3) to identify the dynamic effects of internet.

2.3 Empirical strategies and results

2.3.1 Event study specification

In order to analyze dynamic impacts of internet access over time, we estimate the following event study specification:

[13] Recall that within each year, school level averages are normalized across *all* schools giving the ECE, including those not in our main estimation sample.

[14] Unfortunately, we cannot determine the timing of internet connectivity prior to 2007. While we can identify schools that had internet installed by 2007, we do not have information about the specific year in which they gained connectivity.

Figure 2.2: Standardized Average Math Scores over Time, by Year of Initial Internet Access

Figure 2.2a plots the standardized test scores for all public schools that had an internet connection prior to 2007. Figures 2.2b-2.2i plot the standardized test scores over time separately for groups of schools based on the year of initial internet connection. The sample includes all public schools that initially gained internet access between 2007 and 2014 and with at least two observations within the same period. Figure 2.2j plots the standardized test scores for all public schools that did not have an internet connection by 2014.

$$Y_{ir} = \sum_{t=-3}^{5} \beta_t \mathbf{1}\{E_{ir} = t\} + \gamma X_{ir} + \alpha_i + \theta_r + \varepsilon_{ir}, \quad t \neq -1 \tag{2.1}$$

Our primary outcomes of interest are averages of standardized math scores for second grade students in school i in year r (Y_{ir}) (normalized across the universe of Peruvian schools within each year). α_i and θ_r are school and year fixed effects that capture school-specific fixed determinants of and aggregate changes in student performance over time, respectively.[15] X_{ir} is a set of time-varying characteristics that includes total school enrollment, number of second grade students scheduled to take the test, facilities (piped water, library, administrative offices), and resources per student (classrooms, computers and teachers).

Let I_i denote the year in which school i gains internet connection (the first year in the dataset in which i reports internet access in the CE). E_{ir} represents time relative to internet access for each school; specifically, $E_{ir} = r - I_i$. The coefficients on the set of event study dummy variables β_t capture the path of test scores relative to the year before a school receives internet access (i.e., relative to $t = -1$). It is worth highlighting one important feature in the timing of the two datasets we use. The CE reports internet access in the beginning of the school year, while the ECE is a year-end test. Any school that installs internet after the CE (April-July) does not report internet access until the following calendar year. If internet installation occurs before the ECE exams (end of November - December), students are (at least partially) exposed to internet access in the year *prior* to reporting initial access in the CE. Therefore in merging internet information from the CE to test scores from the ECE, we match test scores from the ECE to the internet status in the CE of the following calendar year. This means that some schools acquire internet access in $t = 0$ (if installation occurred *before* submitting the CE information) while others acquire it in $t = 1$ (if installation occurred *after* submitting the CE information). Unfortunately, school-level information

[15]A regression that includes school fixed effects, event study time dummies, and a full set of calendar year fixed effects results in perfect multicollinearity. We therefore pool two year effects (which should be close to zero, given that school-level scores are normalized within each year to a mean of zero and standard deviation of one). Results are robust to our pooling choice, and are similar when pooling two pre-internet event study indicators (e.g., t=-3 and t=-2) or dropping year effects altogether.

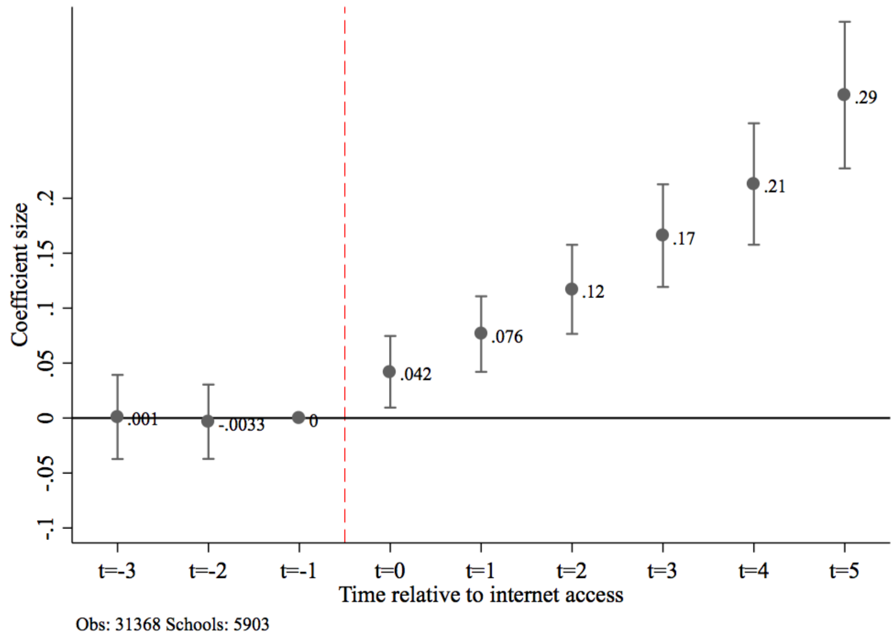
is not available for either the month of installation or completion of the CE so we are unable to tell how many schools receive internet in $t = 0$ and $t = 1$. Thus, in interpreting estimates of β_t it is important to keep in mind that $t = 0$ is a partially treated year for some schools and a pre-treatment year for others, while $t = 1$ is a partially treated year for some schools and a (fully) treated year for others.

By exploiting variation in the timing of internet access *within* schools (as well as additionally controlling for aggregate year effects and a set of time-varying characteristics), we aim to identify the effects of internet access separately from potential confounders that are fixed at the school level. We consider this a refinement over Hopkins (2014), who also examines the relationship between internet access and test performance in Peru — but compares internet-connected schools to non-connected schools (including those that never become connected). We use the event study framework to examine both pre-treatment trends and dynamic effects in up to five periods following internet access in a non-parametric fashion. Standard errors are clustered at the school level to allow for arbitrary serial correlation in ε_{it}.

Figure 2.3 displays the results of estimating Equation 2.1 on our main sample. The full set of coefficients for the event study dummy variables are reported in Appendix Table B.3. We find that, prior to internet access ($t < 0$), schools' relative math performance compared with their peers was roughly constant from year to year (Figure 2.3). Importantly, there is no apparent trend in math scores prior to internet access, indicating that the timing of internet access within schools is unrelated to pre-trends in student performance. In particular, we rule out the case in which internet installation is budgeted endogenously as a reward for steadily improving test performance. While relative math performance rises in all years following initial connectivity, immediate gains are small in magnitude (0.042 standard deviations in the first partial year of access). The improvement does not surpass 0.1 standard deviations until 2 years after installation. By year 5, scores are 0.29 standard deviations higher compared with other schools than before internet installation.

On the surface, this finding stands in contrast to other studies in developing countries that find limited or no impacts of ICTs on test scores. However, our short run estimates are in fact similar in

Figure 2.3: Impact of Internet Access on Test Scores



The above figures plot the coefficients and 95% confidence intervals from estimating equation 2.1. Full regression results are reported Table B.3. Scores are standardized to have mean zero and standard deviation of 1 across the universe of schools reporting scores within each calendar year. Coefficients capture the increase in test scores relative to the year prior to a school receiving internet access ($t = -1$). Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Control variables include total school enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers, and teachers). The sample includes all public schools with at least two observations within the sample period (2007-2014). Standard errors are clustered by school.

magnitude to those from several studies of hardware-only ICTs —which range from 0.052 to 0.088 standard deviations, 5 to 22 months post-intervention (Bet et al. 2014, Barrera-Osorio and Linden 2009, Cristia et al. 2017, Beuermann et al. 2015 Mo et al. 2013) — though these other studies are unable to statistically distinguish estimates from zero (based on smaller samples of schools that range from 13 to 318).[16] Results from Figure 2.3 suggest that though classroom internet is beneficial to learning, improvement in the initial years post-intervention is small. The majority of the studies in this literature focus on impacts within the first 18 months post intervention, an early stage in which impacts may not be statistically detectable in smaller samples. The fact that our estimates grow over time, at least through the medium-term, is also consistent with the only

[16]Other studies (Angrist and Lavy 2002, Meza-Cordero 2017, Sharma 2014) find negative — though not always statistically significant — effects of hardware introduction on test scores.

other two longer-term studies of ICTs in education — which have also supported the need for an adaptation period to fully utilize new technologies (Hull and Duch 2016; Mo et al. 2015).

In the medium-run (3-5 years), the increase in math scores is sizable, though somewhat smaller than those typically found in evaluations of computer assisted learning and related interventions (0.18 to 0.59 standard deviations) (e.g., see Bando et al. 2016, Banerjee et al. 2007, Carrillo et al. 2010, He et al. 2008, Linden 2008, Muralidharan et al. 2016). Our smaller albeit statistically significant estimates may owe partly to the fact that introduction of internet into Peruvian schools was unaccompanied by any particular pedagogical software service, pre-specified uses, or complementary interventions.[17] Unlike in many of the interventions associated with relatively larger impacts, teachers and students here would have needed to find effective uses of the internet on their own.

2.3.2 Trend break specification

Though the shape of Figure 2.3 suggests a steadily increasing effect of internet access on math test scores over time, it does not explicitly test for a break in the trajectory of scores at the time of internet installation. To do so, we estimate a linear trend break specification as follows:

$$\begin{aligned} Y_{ir} &= \phi_1 \text{Post-internet Access}_{ir} + \phi_2 \text{Event Time}_{ir} \\ &\quad + \phi_3 \text{Post-internet Access}_{ir} \times \text{Event Time}_{ir} + \gamma X_{ir} + \alpha_i + \theta_r + \varepsilon_{ir} \end{aligned} \tag{2.2}$$

Here, Post-internet Access is a dummy variable that is equal to one in all periods after internet installation ($t \geq 0$). Event Time is a linear term for time relative to the year prior to access, $t = -1$. The control set is otherwise identical to that described in Section 2.3.1. In this specification, ϕ_1 captures the level shift in test scores in response to internet access; ϕ_3 represents the change in the linear time trend in math scores after schools gain internet access; and ϕ_2 accounts for any

[17] Though many public schools gain access to the internet through *Plan Huascarán*, which also aims to increase computers in schools, our main specifications control for the number of computers per student so that the estimated effects of internet access account for differential access to computers. We discuss the relationship between the effects of internet access and computer availability in more detail in Section 2.4.

pre-existing linear trend. Based on the results in Section 2.3.1, it is unlikely that there are any existing pre-trends. However, one benefit of this specification is that even in the presence of any linear pre-trends in test scores, ϕ_3 measures the impact of internet access on the growth in test scores *apart* from any such trends.

Results from estimating equation 2.2 are displayed in Table 2.1. The specification in column 1 controls only for year fixed effects. Though the estimate of the trend break in scores starting in the year of internet installation is positive and significant, we also observe a (statistically insignificant) pre-trend. This suggests that cross-sectional variation across schools may not account for selection into internet access following test score growth. In column 2, we add school fixed effects to account for time-invariant school-level unobservables. Basing identification on only the within-school variation in internet connectivity reveals a positive trend break at the time of installation, with no pre-trend. This specification also shows a larger level improvement in scores upon installation (0.029), but we are unable to statistically distinguish this effect from zero. Finally, we present our preferred specification in column 3, which includes additional controls for time-varying school resources. Based on this specification, estimates of both the level shift and trend break are positive and statistically significant, while the estimate of the pre-trend is close to zero and fairly precise.

Using our preferred specification to linearly approximate the dynamic effects of internet access, we find a level improvement of 0.036 standard deviations upon installation and an additional 0.047 standard deviation gain in each later year. We take care to note a particular limitation of our analysis that stems from evaluating only the short and medium run effects of internet installation: from Figure 2.3, it is unclear when exactly the positive effects of internet on math scores level off (as opposed to continuing to rise at the rate estimated in Table 2.1). It may therefore not be appropriate to extrapolate these results over much longer term time spans.

2.4 Robustness checks

In this setting, identification of the impact of school-based internet access on student performance may be confounded if the timing of internet access is non-random within schools. However,

Table 2.1: Internet Access & Test Scores: Trend Break Results

	Dependent Variable: School Average Standardized Math Score		
	Only Year Fixed Effects (1)	Adding School Fixed Effects (2)	Adding Time-varying Controls (Baseline) (3)
Post-internet Access	0.017	0.029	0.036*
	(0.021)	(0.020)	(0.020)
Post-internet Access	0.046***	0.043***	0.047***
× Event Time	(0.011)	(0.011)	(0.011)
Event Time	0.014	-0.008	-0.001
	(0.010)	(0.010)	(0.010)
Observations	31,368	31,368	31,368
Number of Schools	5,903	5,903	5,903
Year Fixed Effects	Yes	Yes	Yes
School Fixed Effects	No	Yes	Yes
Time-varying Controls	No	No	Yes

The sample includes all public schools that gained internet access between 2007 and 2014 and are observed at least twice. Standard errors are clustered by school. Math scores are standardized to have mean zero and standard deviation of 1 across the universe of schools reporting scores within each calendar year. Post-internet access is a dummy variable for whether a school has gained internet access (i.e. $t >= 0$). Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Event time is years relative to internet access. Control variables include enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers, and teachers).

Significance levels denoted by: *** p< 0.01, ** p< 0.05, * p< 0.1.

conditional on school fixed effects, our analysis in Section 2.3 offers no signs that the timing of internet access relates to prior test performance. In this section, we address several other potential challenges to identification, namely endogenous changes in sample composition in terms of both schools and students (including non-random attrition), concurrent changes in school resources, and differential regional shocks and school-level pre-trends.

2.4.1 Changes in sample composition

Due to utilizing an unbalanced panel (schools are included when they participate in the ECE) observed over a limited window of time (2007-2014), it is possible that our estimated treatment effects to some extent reflect changes in sample composition. Namely, identification of pre-trends and treatment effects might rely on largely different samples of schools. In Section 2.4.1.1, we find that our estimates are not contaminated by this issue or by school-level attrition. Additionally, we consider also that the composition of students *within* schools may change in response to internet access. For instance, internet access may attract a different pool of students to a school (either from other schools or from non-school activities). Section 2.4.1.2 presents evidence that this manner of endogenous student sorting does not occur in our sample.

2.4.1.1 Unbalanced panel and attrition

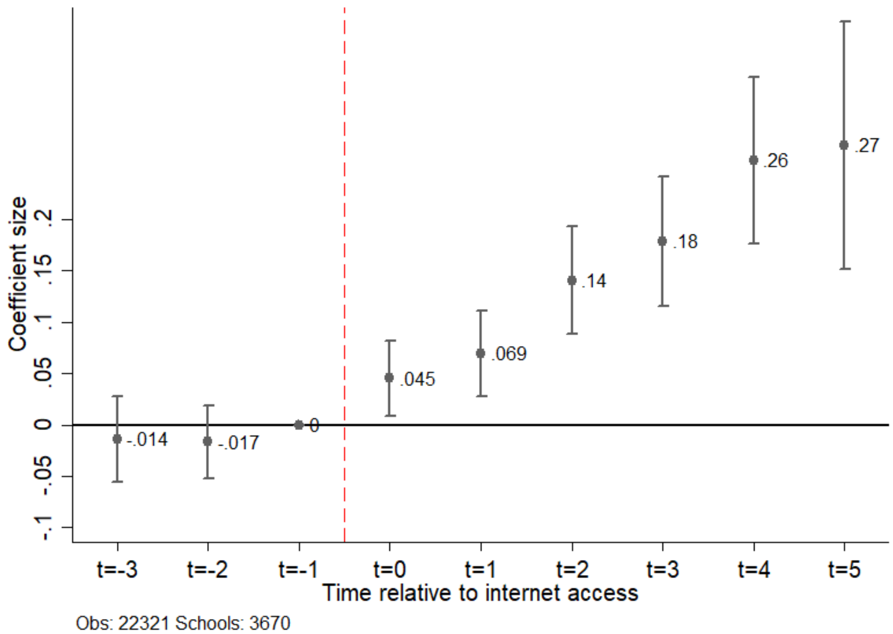
Because some schools in our main sample are observed only prior to internet installation while others are observed only after (fixed effects estimation only precludes including schools without at least two years of data), it is possible that the pre-internet coefficients and trends are identified from a different set of schools than those identifying the post-internet treatment effects. Unfortunately, the likelihood of observing a school's pre-internet years versus post-internet years is furthermore naturally influenced by the date of internet installation (in the extreme case, we of course do not observe the pre-period of any school that installed internet in 2007). If these schools in actuality experienced non-zero pre-trends, our estimates will not take these into account. On the other hand, schools that installed internet in 2014 are not observed post-internet and can only be used to identify pre-internet coefficients/trends. Even though these schools may show no pre-trends, it is possible that they also go on to experience zero (unobserved) effects of internet access.

Figure 2.4 and column 2 of Appendix Table B.5 suggest that our main findings are not driven by these school-level sample composition issues. Specifically, we restrict the sample to schools that appear at least twice prior to and twice following internet installation, i.e. schools for which we observe both pre-trends and treatment effects.[18] In this sample, which we refer to as the "2 Pre, 2 Post" sample, we find no statistically significant trends in performance prior to internet access, and the estimated effects are similar in magnitude and show similar dynamics as those using the

[18]This limits our sample to schools that gained access to the internet during a span of four (rather than the full eight) calendar years. Our restricted sample includes only 3,670 schools versus the 5,903 in the main sample, but is highly comparable along many observable dimensions — including student achievement (see Appendix Table B.4).

full sample.

Figure 2.4: Effect of Internet Access in the Sample of Schools Observed in at least Two Periods before and Two Periods after Internet Access: Event Study Results



The sample includes all public schools that gained internet access between 2007 and 2014 and are observed at least twice prior to and twice after internet access. Coefficients capture the increase in test scores relative to the year prior to a school receiving internet access ($t = -1$). Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Control variables include enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers and teachers). Standard errors are clustered by school.

Attrition from the panel may pose another compositional issue. Overall attrition in our sample is 18.2%. Attrition can happen for several reasons. First, as mentioned in Section 2.2.2.1, only schools with at least 5 second grade students and in which the language of instruction is Spanish are required to administer the ECE exam. Therefore, observations will be missing when schools fall below the threshold of 5 students (or which switch to an instructional language other than Spanish). Some schools might have experienced permanent reductions in their second grade enrollment (and drop from the sample at some point) and some others might "bounce" around the ECE enrollment threshold (for example, a school might have five second graders during a year and only four during the next year). Appendix Table B.6 shows that about half of overall attrition is likely due to a school dropping below the enrollment threshold.[19] The remaining attrition is either due to missing ECE scores for another reason or missing CE (covariate) information. Only a very small portion of attrition is due to school closures.

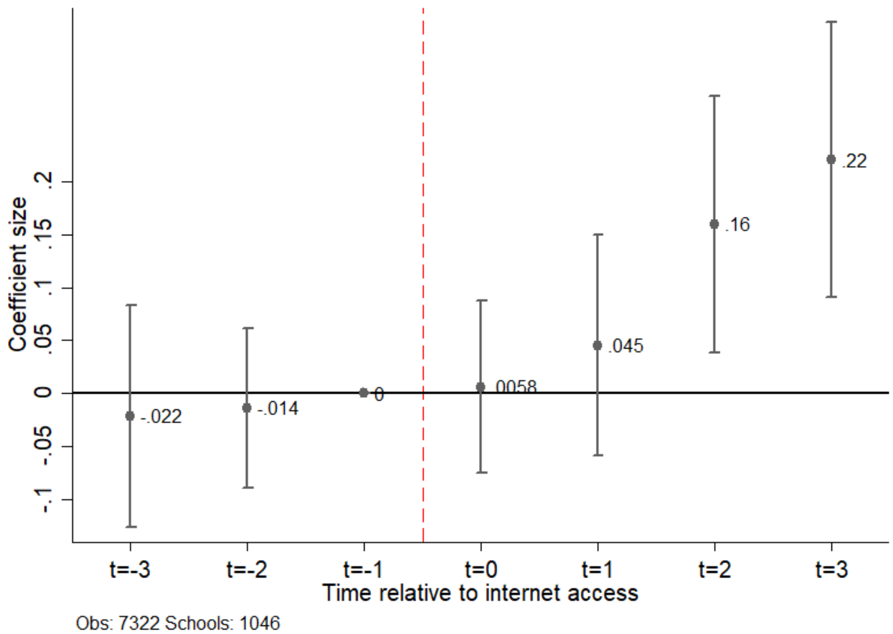
To illustrate that attrition does not affect our results, we estimate equation 2.1 on the restricted sample of schools that are observed for the entire sample period, i.e., the fully balanced sample. The results are displayed in Figure 2.5 and Appendix Table B.5. It is clear that using the restricted sample makes the estimates much less precise overall and that the short-run estimates are somewhat smaller than in the baseline results. However, the pattern of effects is otherwise very similar. In fact, the 2- and 3-year post installation effects appear even larger in this sample (Figure 2.5).

There are several important caveats to using the fully balanced sample. First, we are only able to identify effects for an evaluation window that spans seven periods, $t = -3$ to $t = 3$. This considerably limits our ability to study the dynamic path of effects, compared with our main results. Second, this limits the identifying variation to only schools gaining access in 2009 and 2010. Third, the balanced panel restriction shrinks the sample of schools shrinks considerably, from 5,903 to 1,043. Schools in the balanced sample appear to be higher achieving and larger compared with the full sample overall (see Appendix Table B.4). In Appendix Table B.5 we display the results of estimating equation 2.2, first reproducing the results for the full sample using the restricted evaluation window (column 3) and then using the balanced sample (column 4). In the balanced sample, the trend break is large but imprecisely estimated. In line with the event study results in Figure 2.5, the immediate impact (level shift) in this sample is small (the point estimate is actually negative) and not statistically significant.[20]

[19]A third is due to a school having fewer than 5 second grade students and an additional sixth is explained by having enrollment "near" the threshold (defined as having 5-7 second grade students).

[20]The data are not well suited to other methods of accounting for non-random attrition. For example, Lee bounds are not appropriate in this context because it is not clear whether the internet access would affect attrition monotonically . We do not consider inverse probability weighting because around 25% of attrited observations are missing covariate information.

Figure 2.5: Effect of Internet Access in the Sample of Schools Observed in All Periods: Event Study Results



The sample includes includes all public schools that gained internet access between 2007 and 2014 and are observed for the entire sample period. Coefficients capture the increase in test scores relative to the year prior to a school receiving internet access ($t = -1$). Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Control variables include enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers, and teachers). Standard errors are clustered by school.

2.4.1.2 Student composition and endogenous sorting

Another related issue is that the composition of students *within* schools may change in response to internet access. A priori, it is hard to tell the direction of the bias that this would entail. If we consider that gaining internet access might signal an increase in the quality of education in a school, two things can happen. It might be that parents who would not have sent their kids to school (at all) decide to enroll their children in a school connected to internet. If internet connectivity attracts students that would have otherwise performed poorly in school, then our estimates of treatment effects are likely conservative. Alternatively, motivated parents seeking learning opportunities for their children may decide to transfer students from schools without internet to schools that gained connectivity. If these new students are better achievers on average, then our findings of positive treatment effects may owe to upward bias from changes in student composition.

Overall, it does not appear plausible that an influx of high-achieving transfers or re-entrants explains the performance gains in our main results. To rule out this possibility, we first analyze the response of grade 2 transfers and re-entry to internet access in columns 1-2 of Table 2.2. Transfers are students enrolled in the current year who were enrolled in a different school in the previous year. Re-entrants are students that are currently enrolled but who were not enrolled in any school during the previous year (i.e., dropouts who come back to school). It appears that schools gain about 0.293 second grade transfers in the year that internet is introduced and that transfers increase by about 0.167 students in every subsequent year (column 1). However, these increases are small relative to total grade 2 enrollment (enrollment in grade 2 was 31.5, on average, prior to internet). For example, the results in column 1 predict that 5 years after internet, there will be in total about 1.1 additional transfer students. Given that 31.5 students on average take the ECE each year, it is unlikely that one additional student can substantially contribute to the observed increase in average test scores.[21] There are no apparent effects of internet access on grade 2 re-entry, though there are very few re-entrants to begin with (column 2).

Nevertheless, even if total enrollment remains relatively unchanged in response to internet connectivity, the makeup of the students that take the test could still change. For example, if internet availability induces attendance, then a different set of students will be present to take the test after a school gains internet access. We investigate this possibility in columns 3 and 4 of Table 2.2. Column 3 examines the effect of internet on the number of students scheduled to take the ECE, conditional on the total number of grade 2 students enrolled. It appears that, after internet is introduced to a school, the number of test takers actually declines by 0.334 students. Not only is this effect likely too small (and, for this scenario, in the "wrong" direction) to drive the estimated effects of internet access, but this also represents a one-time decrease in the number of test takers — which is unlikely to explain gradual performance gains that occur over time. These

[21] Additionally, the number of grade 2 transfer students is unrelated to test scores (point estimate = 0.0003, p-value = .619) in a regression including school and year fixed effects and the controls listed in Section 2.3.1.

results are consistent with Cristia et al. (2017) and He et al. (2008), who find that neither hardware nor CAI/CAL interventions have any significant effects on attendance.[22] In column 4, we further explore whether student background changes in response to internet access. The only information on the background of students in the *Censo Escolar* is the proportion of native Spanish speakers enrolled.[23] To the extent that native language captures student background, it does not appear that internet access attracts more advantaged students. Overall, the evidence in Table 2.2 does not seem to indicate that endogenous student sorting drives our estimated impacts of internet access.

2.4.2 Concurrent changes in school resources

Timing of internet access may also possibly correlate with changes in other school resources. For example, it might be that internet provision is bundled with other inputs in a multifaceted approach to improve quality of schooling.[24] If this is the case, the improvement of students' performance that we observe might be due to increases in these other resources. In general we do not find that the timing of internet access is correlated with increases in other observable inputs such as teachers, classrooms, or textbooks (Table 2.3). Though teachers per student rises slightly after internet access, this effect is small both in absolute terms and relative to the pre-internet mean (column 1). Therefore we find it unlikely that such a small shift in teachers per student explains our main findings. Classrooms per students actually *fall* very slightly after internet access (a one-time shift, column 2). In column 3, we report small and statistically insignificant changes in the number of textbooks per student in schools that gain access to internet.

[22] Relatedly, Cristia et al. (2014) find no effects of computer and internet access on enrollment, grade repetition, or dropout in secondary public schools in Peru.

[23] The proportion of Spanish-speaking students is positively related to higher test scores, even after conditioning on school and year fixed effects and the controls listed in Section 2.3.1.

[24] It could also be the case that internet access at schools is highly correlated with alternative sources of internet. For example, it might be that students who gain internet access at school already have internet connections at home or go to cyber-cafes. However, we find that only 23% (32%) of students with access to internet at school also use it at cyber-cafes (at home) according to the 2014 Peruvian National Household Survey (ENAHO). Additionally, our results are unchanged if we include a control for whether the town nearest the school has a cyber cafe; results available upon request.

Table 2.2: Effect of Internet Access on Grade 2 Transfers, Re-entry, Test Taking, and Student Composition

	Grade 2 Transfers (1)	Grade 2 Re-entry (2)	Grade 2 Students Scheduled to Take Test (3)	Proportion of Native Spanish Speakers in Grade 2 (4)
Post-internet Access	0.293**	0.028	-0.334*	-0.002
	(0.120)	(0.031)	(0.179)	(0.005)
Post-internet Access X Event Time	0.167***	0.005	-0.133	0.000
	(0.061)	(0.017)	(0.104)	(0.002)
Pre-internet mean of dep. variable	2.866	0.372	30.89	0.850
Observations	31,368	31,368	31,368	31,357
Number of Schools	5,903	5,903	5,903	5,903
Year Fixed Effects	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes

Transfers are students enrolled in the current year who were enrolled in a different school in the previous year. Re-entrants are students that are currently enrolled but who were not enrolled in the previous year. The sample includes all public schools that gained internet access between 2007 and 2014 and are observed at least twice. Standard errors are clustered by school. Math scores are standardized to have mean zero and standard deviation of 1 across the universe of schools reporting scores within each calendar year. Post-internet access is a dummy variable for whether a school has gained internet access (i.e. $t >= 0$). Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Event time is years relative to internet access. Control variables include enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers, and teachers). Columns 1-3 controls for both second grade enrollment and enrollment in other grades separately, not including transfers or re-entrants when specified as an outcome variable. Column 4 also controls for the number of second grade students scheduled to take the test.

Significance levels denoted by: *** p< 0.01, ** p< 0.05, * p< 0.1.

However, in column 4, we note a positive and non-negligible level increase in computing resources at the time of internet installation (on the other hand, the estimate of the trend break is near zero and statistically insignificant). To better understand the potential for increases in computers to confound our estimates of the impact of internet access, we perform some back-of-the envelope calculations. Note that, by definition, internet connection is complementary to computer access (i.e., generally students cannot use the internet *without* computers). However, students might nevertheless benefit from computers without access to internet (e.g., using preloaded software and resources installed from flash drives / DVDs, etc.). Joint increases in computer and internet

Table 2.3: Effect of Internet Access on School Resources

	Teachers per Student (1)	Classrooms per Student (2)	Textbooks per Student (3)	Computers per Student (4)
Post-internet Access	0.003***	-0.005*	-0.132	0.045***
	(0.001)	(0.003)	(0.088)	(0.006)
Post-internet Access X Event Time	0.002***	-0.001	-0.063	-0.001
	(0.001)	(0.001)	(0.046)	(0.003)
Pre-internet mean of dep. variable	0.0552	0.0721	3.932	0.0676
Observations	31,368	31,368	23,636	31,368
Number of Schools	5,903	5,903	5,857	5,903
Year Fixed Effects	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes

In column 3, the dependent variable is the number of 2nd grade textbooks per 2nd grade student. In column 4, the dependent variable is the number of instructional computers per student. The sample includes all public schools that gained internet access between 2007 and 2014 and are observed at least twice. Standard errors are clustered by school. Math scores are standardized to have mean zero and standard deviation of 1 across the universe of schools reporting scores within each calendar year. Post-internet access is a dummy variable for whether a school has gained internet access (i.e. $t >= 0$). Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Event time is years relative to internet access. Control variables include enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers, and teachers). Significance levels denoted by: *** p< 0.01, ** p< 0.05, * p< 0.1.

availability might thus possibly imply that our estimates capture (at least partially) the utility of computers themselves apart from internet access.

By our calculations (displayed in Appendix Table B.8), we find that the increase in computers alone explains very little of the observed rise in test scores (at most, 6%). For these calculations we use two pieces of information: (i) the impact of internet access on computer resources and (ii) the impact of computer resources alone (i.e., without connections to the internet) on student test scores. Our estimate of (i) comes from column 4 of Table 2.3 above. The pre-internet yearly increase in computers is 0.034 (not shown in the table), suggesting that the approximate rise in computers t years after internet access follows the formula $0.045 + 0.034 \times t$. To approximate (ii), we use the sample of schools that do not gain access to the internet during our sample period and regress math scores on computers per student (including school and year fixed effects as well as all of the same controls listed in Section 2.3.1). These results are displayed in Appendix Table B.7. In each successive column, we add in lags of computers per student to allow for dynamic effects of computing resources on student performance. Using both (i) and (ii), we can calculate the approximate gain in math scores that is due to increases in computing resources (*without* internet access) that occur around the introduction of internet.

Appendix Table B.8 displays these calculations. For $t = 0$, this calculation is relatively straightforward: we see a rise in computers per student of 0.045, from which we would expect a gain of $0.045 * 0.031 = 0.001$ standard deviations (relative to $t = -1$) based on the largest estimate of contemporaneous computing resources' impact on test scores (0.031, from column 1 of Appendix Table B.7). From our event study specification, our overall estimated effect of internet access at $t = 0$ is 0.042 standard deviations. Therefore, we estimate that the increase in computers per student at $t = 0$ explains only about 3.3% of the rise in test scores that we observe in response to internet access. The calculations for other post-internet periods are more complex when the effect of computers is allowed to be dynamic. However, our most "generous" estimates indicate that, at most, acquisition of more computers explains about 6% of the estimated gains in math following internet access.[25]

2.4.3 Differential pre-trends

Another possibility is that access to internet is correlated with pre-existing trends at the regional level. For example, states with faster growing economies might be better able to finance internet expansions, increase public spending on education, or otherwise improve student learning. Hence, in Table 2.4 we re-estimate equation 2.2 including state-specific trends to allow for differential

[25]To get the "most generous" estimates, we take the largest individual estimated effect of computers for each lag across all specifications in Appendix Table B.7, regardless of significance level. Reassuringly, results with and without controlling for computers are virtually identical (post-internet event study coefficients are slightly - but not statistically significantly - *smaller* when we do not control for computers); results available upon request. These confirm the conclusion of our calculations in Appendix Table B.8; namely, that concurrent (one-off) increases in computing resources at the time of internet installation do not affect our estimated impacts of internet access.

pretreatment trajectories in academic performance driven by such state-level factors. Our results are robust to the inclusion of state-specific pre-trends (column 2), as well as to allowing pre-trends to differ by state-sector (i.e. urban/rural) (column 3).

Table 2.4: Allowing for State, State-Sector, and School-specific Pre-trends

	Dependent Variable: School Average Standardized Math Score			
	Baseline (1)	State-specific Pre-trends (2)	State-Sector Pre-trends (3)	School-specific Pre-trends (4)
Post-internet Access	0.036*	0.039**	0.038*	0.024
	(0.020)	(0.020)	(0.020)	(0.029)
Post-internet Access X Event Time	0.047***	0.045***	0.041***	0.031
	(0.011)	(0.010)	(0.010)	(0.038)
Observations	31,368	31,368	31,368	22,321
Number of Schools	5,903	5,903	5,903	3,670
Year Fixed Effects	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes

In columns 1-3, the sample includes all public schools that gained internet access between 2007 and 2014 and are observed at least twice. Standard errors are clustered by school. In column 3, the sample is further restricted to schools that are observed at least twice prior to and after gaining internet access. Math scores are standardized to have mean zero and standard deviation of 1 across the universe of schools reporting scores within each calendar year. Post-internet access is a dummy variable for whether a school has gained internet access (i.e. $t >= 0$). Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Event time is years relative to internet access. Control variables include enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers, and teachers) as well as the additional fixed effects and/or time trends indicated in each column heading (columns 2-4). Standard errors are clustered by school.

Significance levels denoted by: *** p< 0.01, ** p< 0.05, * p< 0.1.

To also examine whether school-specific trends may confound our identification, column 3 of Table 2.4 augments our equation 2.2 specification with linear trends that are specific to each school. Note that the inclusion of school-specific pre-trends limits our sample to those schools within which we can identify both pre- and post-internet trends: namely, our "2 Pre & 2 Post" sample. Accounting for differential pre-trends produces positive, albeit smaller and statistically insignificant, estimates of the trend breaks and level increases from our main analysis. It is worth emphasizing that the estimation of school-specific linear trends — which add an additional 3,670 covariates to the model — likely absorbs much of any potential exogenous variation in test scores and internet access. As expected, precision drops considerably in this specification. Thus, while we are unable to reject the null hypothesis of zero effects when including the additional trends, we are also unable to reject that these estimates are equal to our baseline estimates.

2.5 Explaining dynamics and identifying potential mechanisms

In this section, we present some suggestive evidence to aid interpretation of our main results. First, we show that the dynamic pattern of effects of internet access in the years following installation may be explained by the need for complementary investments in resources: namely, teachers with formal training in digital skills. We then explore two potential mechanisms by which internet access improves student learning: supplementing individualized teacher attention and mitigating low teacher quality.

2.5.1 Why do the effects of internet access take time to emerge?

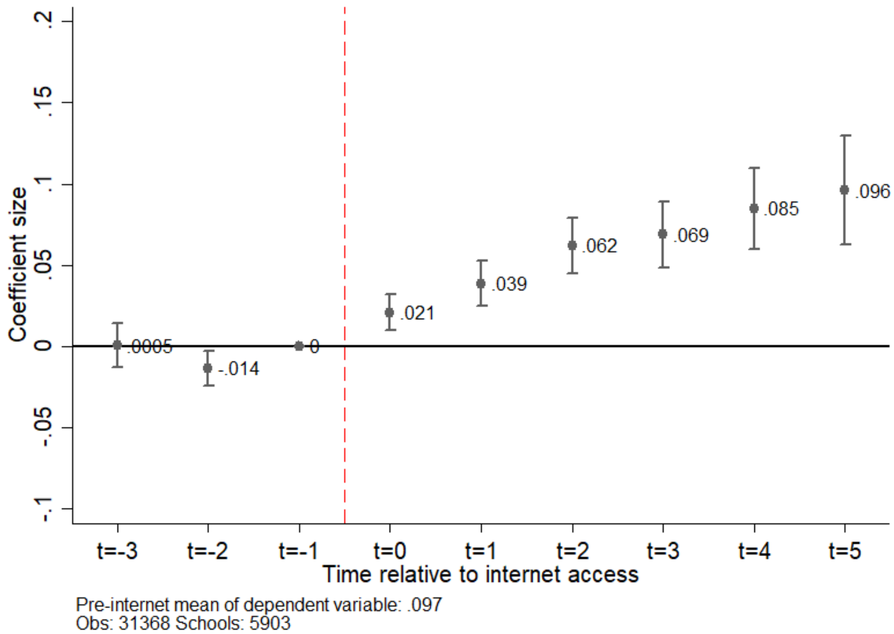
One explanation for why we observe delayed impacts of internet access may be that schools require teachers with digital and internet skills in order to incorporate the new technology into the classroom. To investigate this possibility, we study whether schools respond to internet access by hiring teachers with expertise in "computer and information technology." This includes both teachers trained to teach computer skills, as well as teachers who themselves underwent advanced education relating to computers; hereafter, these are referred to as "computer teachers." We estimate equation 2.1 using an indicator for the presence of a computer teacher as the outcome.[26]

Figure 2.6 shows that internet access is accompanied by a slow but steady increase in the likelihood a school has a computer teacher; by year 5, this results in a doubling of the pre-internet likelihood. When taken together the findings for computer teachers and test scores are consistent with the idea that schools may need time to make complementary investments to fully exploit new

[26]We use a dummy for the presence of a computer teacher rather than the number of computer teachers because less than 3% of schools have more than one computer teacher in any given year.

classroom technologies, such as teachers with computer training.

Figure 2.6: Internet Access and Presence of a Computer Teacher



Coefficients capture the increase in the likelihood of having a computer teacher on staff relative to the year prior to a school receiving internet access ($t = -1$). Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Control variables include enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers, and teachers). The sample includes all schools with at least two observations within the sample period (2007-2014). Standard errors are clustered by school.

2.5.2 Increased individualized attention

Previous work suggests that ICTs may enable providing students with more individualized attention than they would otherwise receive from teachers. To explore whether increased individualized attention might explain our results, we follow Barrow et al. (2009) and examine heterogeneity along the lines of class size. In theory, teachers divide their time in classrooms between group and individualized instruction. If ICTs reduce the time teachers spend in group activities, they might be able to increase the time they allocate to individualized instruction. Teachers might be more constrained in providing individualized instruction when they have more students, so gains from

ICTs should be larger in large classes relative to small classes. Therefore, we expect the effects of internet access to matter more for schools with high versus low student to teacher ratios (STR).

Indeed, we find that the positive effects of internet access are concentrated among schools with high STRs when schools are split by the pre-internet median STR. We define "high STR" and "low STR" groups as follows. First, we calculate the total number of teachers per second grade student. We do not use the number of teachers exclusively dedicated to second grade, as many smaller schools have teachers that cater to multiple grades. Then, we calculate each school's pre-internet average STR (time-invariant). Finally, we divide the schools into high and low STR groups based on having a pre-internet average STR above or below the median.[27] In Figure 2.7a, the high and low STR trends in test scores prior to internet access are nearly identical, but diverge once internet is introduced. In low STR schools, the effects are much smaller though the estimates are not very precise in either subsample. Appendix Table B.10 (Columns 1-2) confirms that the trend break in test scores is larger and only statistically significant in high STR schools, though the point estimate of the immediate effect of internet access is larger in low STR schools.[28] Thus, these results are qualitatively consistent with increased individualized attention as a causal pathway through which internet access improves student performance.[29]

2.5.3 Substitution between internet access and teacher qualifications

Another possibility is that ICTs generate gains in student learning because they compensate for the lack or low quality of other inputs. Relatedly, some have found that the success of ICT

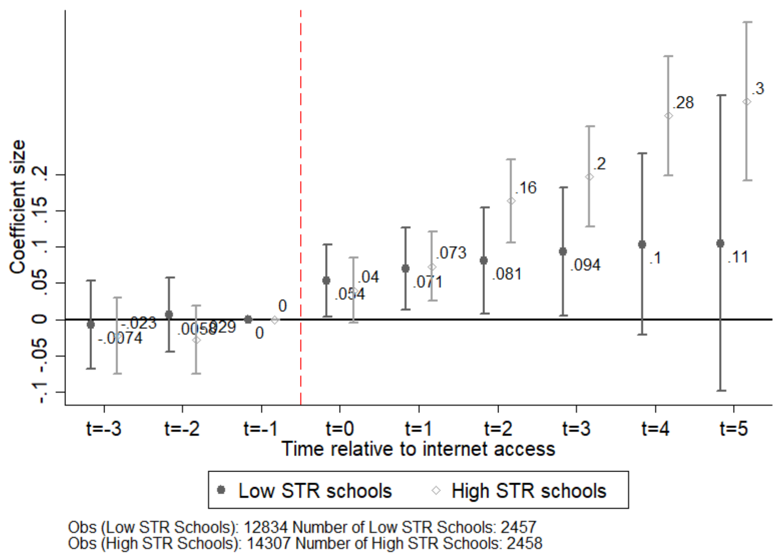
[27]The average 2nd grade student-teacher ratios in the high and low STR groups are 8.28 and 2.77, respectively.

[28]The difference in the estimated trend break across the two samples is is marginally statistically significant; p-value = 0.139. The difference in level shifts is not statistically significant; p-value = 0.866.

[29]Another way to investigate the mechanism of individualized instruction would be to look for heterogeneity by student attendance (see Barrow et al. 2009). However, we do not have data on attendance or any other student characteristics (e.g., prior achievement as in Bai et al. 2016, Barrow et al. 2009, Linden 2008, He et al. 2008, Mo et al. 2015, and Muralidharan et al. 2016 or age and gender as in Linden 2008) that would allow investigating heterogeneity along individual dimensions.

Figure 2.7: Heterogeneity in the Impact of Internet Access on Test Scores: Event Study Results

Heterogeneity by Student to Teacher Ratios



Heterogeneity by Teacher Qualifications

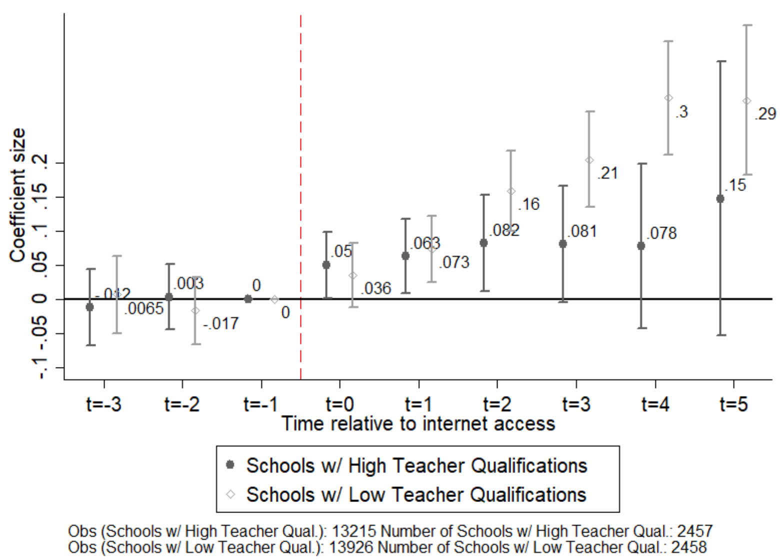


Figure (a): The sample is split based on each school's pre-internet average ratio of second graders to total teachers (STR) — high (low) STR schools fall above (below) the median pre-internet STR. Figure (b): The sample is split based on each school's pre-internet average number of teachers with a pedagogical or higher education degree per student over the sample period relative to the median of all schools' sample averages. Coefficients capture the increase in test scores relative to the year prior to a school receiving internet access ($t = -1$). Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Control variables include enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers, and teachers). The sample includes all schools with at least two observations within the sample period (2007-2014). Standard errors are clustered by school.

interventions may depend on whether they displace traditional instruction or constitute additional learning activities outside of traditional classroom hours (as part of an after school tutoring program, for example, as in Linden 2008). In cases where ICTs substitute for traditional instruction, impacts may depend on the quality of instruction that the new technology is displacing. Such is hypothesized in Bulman and Fairlie (2016, p. 20), "[...] Interestingly, evidence of positive effects appears to be the strongest in developing countries. This could be due to the fact that the instruction that is being substituted for is not as of high quality in these countries."

To shed some light on whether internet access may substitute for low teacher quality in the context of our study, we examine heterogeneity in results by the level of qualification that a school's teachers have obtained. In Figure 2.7b, we see that schools with low teacher qualifications experience relatively larger gains in test scores over time, though all schools see a moderate rise in scores immediately following access. The trend break among low teacher qualification schools is large (0.063 standard deviations per year after internet access) and statistically different from the trend break among high qualification schools (columns 3 and 4 in Appendix Table B.10). Here, we measure teacher qualification as the per student number of teachers with a pedagogical or university degree. We estimate the average ratio of qualified teachers-to-students by school using the pre-internet period, and split the sample in two groups based on the sample median across schools. Those with ratios above (below) the sample median are classified as schools with "high" ("low") teacher qualifications.[30]

These results align with findings from other contexts, such as Jackson and Makarin (2016), who determine that the benefits — in terms of math achievement — of providing teachers with online access to "off the shelf" lesson plans were larger among students with weaker teachers. Importantly, if technological tools can substitute for teacher quality, ICT interventions such as school-based internet may help poor schools narrow or close achievement gaps vis-à-vis wealthier

[30]Though STR and teacher quality measures are related, schools with high and low teacher qualification are distinct from schools with high and low STR; about a quarter of high qualification schools are high STR schools and around 18% of low qualification schools are low STR schools. The analysis in Figure 2.7b and Appendix Table B.10 also control for the overall STR.

schools.

2.6 Conclusions

We find evidence that the introduction of internet to Peruvian primary schools produces economically meaningful improvements in student performance (as measured by standardized test scores for grade 2). Gains increase over time, growing from 0.042 standard deviations in the year of installation to 0.29 standard deviations 5 years after installation. Importantly, there are no apparent pre-existing trends in math test scores prior to internet access, suggesting little role for reverse causality. Using a trend break specification, we confirm that there is a level shift and (linear) trend break in test scores that occurs at the time of internet access. In the medium term, the yearly gain in test scores is about 0.047 standard deviations. These results, which are representative of about one quarter of all public primary schools in Peru, are robust to a number of potential confounding factors, including changes in sample composition with respect to either schools or students, changes in school resources, and endogenous timing of installation with respect to prior trends in test performance. In our setting, the nationwide scale of roll-out, large sample of schools, and extended time frame uniquely enable the analysis of this technology's application at the farthest-reaching level of policy.

On the one hand, previous research on ICTs has found that providing hardware with few or no complementary learning tools has little immediate impact on student performance (Bet et al. 2014; Barrera-Osorio and Linden 2009; Cristia et al. 2017; etc.). Our short run results (based on up to 1 year after internet installation) confirm that any effects are small in magnitude — and thus perhaps impossible to detect in the small samples used by this literature's many RCT studies. On the other hand, medium run gains are sizable, pointing towards the necessity of a longer evaluation window for understanding the effectiveness of ICT interventions. Ultimately, our estimated effects of internet access for years 2-5 still fall below prior estimates of the impact of computer assisted learning and instruction. Even so, while school-based internet does not fully confer the benefits of individualized pedagogical tools, it may provide access to learning resources that are otherwise unavailable to many students in developing countries.

We provide supporting evidence that achievement gains are slow to emerge because schools need time to adapt to new technologies. Specifically, after installing internet public schools require time to augment their staff with teachers experienced in computers and information technology. We thus concur with several prior studies finding that student achievement begins to increase only as teachers learn to integrate new technology into their curricula (Hull and Duch 2016, Mo et al. 2013, Sprietsma 2007).

Our data also yield evidence suggestive of two channels through which school-based internet access facilitates human capital accumulation: allowance of greater individualized instruction and substitution for low teacher qualifications. Gains in math scores are concentrated among schools that have high student-teacher ratios and in which relatively few teachers hold pedagogical or university degrees. Hence, school-based internet may generate important gains in learning particularly when individualized instruction and teacher quality are constrained below the optimum.

However, interpretation of the results presented is subject to a number of limitations. Perhaps most notably, school-level analysis may mask important individual-level dynamics. We are largely unable to explore heterogeneity in the effectiveness of school-based internet based on student characteristics. Indeed, previous work suggests that individual heterogeneity - especially with regard to initial achievement - significantly determines how technology affects the learning process (Bai et al. 2016; Barrow et al. 2009; Linden 2008; He et al. 2008; Muralidharan et al. 2016). Future research on heterogeneous impacts of internet in education could bear broad implications for inequality within and across learning environments.

CHAPTER 3

DISCRETIONARY SCHOOL DISCIPLINE POLICIES AND DEMOGRAPHIC DISPARITIES

3.1 Introduction

Despite ostensibly race neutral disciplinary policies, large racial disparities in student suspensions and expulsions persist throughout the U.S., causing Black students in particular to miss more days of school than their peers. During the 2011-12 school year, Black students nationwide comprised only 16% of enrollment, but 32% of students suspended and 42% of students expelled (Green, 2015). Furthermore, this magnitude of disproportionality in disciplinary outcomes manifests as early as preschool: in the same year, Black students accounted for 18% of preschool enrollment nationwide, but fully 42% of preschoolers suspended once and 48% of preschoolers suspended multiple times (Smith and Harper, 2015). Recent research also indicates that punitive measures (e.g. suspensions, expulsions, and juvenile justice referrals), as opposed to restorative justice practices and medicalized approaches, form a higher proportion of disciplinary measures faced by Black and Hispanic students compared with peers (Ramey, 2015; "Trey" Marchbanks III et al., 2016; Welch and Payne, 2018).[1]

Evidence suggests that exclusionary school discipline and the associated loss of instruction time adversely impacts recipients' academic performance, as well as later life labor outcomes and contact with the criminal justice system (i.e. the oft referenced "school-to-prison pipeline"). From a large scale study of thousands of middle and high schoolers, Morris and Perry (2016) estimate that roughly a fifth of the black-white academic achievement gap can be attributed to the gap in

[1] "Trey" Marchbanks III et al. (2016) analyzes statewide data from the Texas Education Research Center, establishing a positive association between school minority composition and juvenile justice referrals. Based on nationally representative data, Ramey (2015) and Welch and Payne (2018) determine that schools with greater minority composition are more likely to employ punishment and deterrence over milder or corrective methods.

suspensions.[2] In particular, Aucejo and Romano (2016) find that the penalty in math and reading scores resulting from absences is larger in magnitude than the benefit resulting from extra days of instruction; this implies that extrapolating from studies of the effects of extended school years leads to an underestimate of the impact of the discipline gap on achievement.[3] In terms of non-cognitive outcomes, students who are suspended or expelled are "placed at risk of delinquency and incarceration" due to removal from the structured school environment (Krezmien et al., 2014). Perhaps unsurprisingly, dropouts who report feeling "pushed out" of school due to disciplinary issues ultimately commit more property and drug crimes and record more frequent arrests than peers who are similar in a broad array of home and community observables — including childhood household income, family structure, having had a teenage mother, having attended schools with gangs present, and urban location (Bjerk, 2012). Moreover, comparing pairs of siblings in the NLSY79, Campbell (2015) observes lower incomes and fewer weeks worked in a year among dropouts, even after accounting for sibling fixed effects.

Principally for these reasons, the Department of Education (DOE) and the Department of Justice (DOJ) during the Obama administration together issued a guidance redefining and expanding their efforts to enforce Title IV of the Civil Rights Act of 1964, which prohibits racial discrimination in schools. Issued on January 8th, 2004, the guidance introduced the concept of *disparate impact*, stating that "a disproportionate and unjustified *effect* on students of a particular race" would constitute unlawful discrimination even if "the policy itself does not mention race — and is administered in an evenhanded manner."[4] Schools and districts investigated for unlawful discrimination can be compelled to engage in time consuming collection of evidential data, and those found to have discriminated can be taken to court and ordered to invest in costly remedies,

[2]Morris and Perry (2016) observe a three-year panel of over 16,000 6th through 10th graders across 17 schools in the Kentucky School Discipline Study.

[3]Aucejo and Romano (2016) find that extending the school year by ten days raises math and reading scores by 0.017 and 0.08 standard deviations, respectively. On the other hand, eliminating ten days of absences raises math and reading scores by 0.055 and 0.029 standard deviations.

[4]For reference, see *Dear Colleague Letter on the Nondiscriminatory Administration of School Discipline* (2014).

such as teacher training programs.

In response, California in 2014 passed the law AB 420, becoming the first state to limit the use of school suspensions and expulsions as punishment for "willful defiance."[5] Unlike other offenses listed in the state education code (e.g. having "stole or attempted to steal school property or private property"), there is no concrete definition of what constitutes "disrupting school activities" or "otherwise willfully defying the valid authority" of school personnel. As such, administrators must determine at their own discretion whether or not a given student's behavior constitutes a suspendable/expellable offense — a situation thought by state lawmakers to open the door to racial bias and hence disparities in disciplinary outcomes.

While other work has extensively studied the relationships between school discipline, race, and various academic and social outcomes, this paper examines a state legislated effort aimed at curbing disproportionality through reducing a certain application of discipline. As the afore described policy is both recent and the first of its kind, similar initiatives in the future may benefit from understanding the results and context of this specific situation. In particular, I aim to provide the first retrospective analysis of how disciplinary disproportionality has responded over the period.

In this paper, I provide an overview of the state's recent progress in reducing exclusionary discipline and disproportionality, here characterized as the difference between a given group's proportion of discipline and its proportion of enrollment, over the period 2012-2017. I also gauge the effectiveness of reducing punishment of defiance in mitigating disproportionality, using identification by treatment intensity, characterized as a school's pre AB 420 proportion of discipline attributable to willful defiance.

Based on school level data on elementary school discipline and enrollment from the CA Department of Education (CDE), exclusionary discipline has consistently and considerably declined throughout the period. On the other hand, it is unclear that AB 420, along with lower willful defiance discipline, have actually reduced disproportionality. Hence, there is so far no indication that removing willful defiance from the toolbox of California educators has brought the state's

[5] See California (2014).

schools closer to compliance with the DOE/DOJ guidance.

The rest of this paper is structured as follows: Section 3.2 reviews literature examining bias in school discipline, Section 3.3 describes the recent legislative efforts surrounding disparate impact in California, including SB 607, proposed as a replacement for AB 420 after it expires July 2018, Section 3.4 breaks down recent state trends in discipline and disproportionality, Section 3.5 explores the impact of banning willful defiance on disproportionality, and Section 3.6 summarizes findings.

3.2 Disparities and Bias: Review of Theories and Evidence

Although not firmly establishing bias as a leading cause, recent large scale, quantitative studies reveal that racial disparities in school discipline are not entirely explained by differences in actual student conduct. On the one hand, students who engage in more frequent delinquency and drug use, and who exhibit lower academic engagement and aspirations, are indeed more likely to encounter exclusionary discipline (Mizel et al., 2016). However, Mizel et al. (2016) find that after controlling for self reported fighting, theft, and vandalism, African Americans are still suspended and expelled at rates higher than would be expected based on enrollment.[6] Even when accounting for teachers' own ratings of various disruptive student behaviors, Rocque (2010) still observed that teachers gave out a disproportionate number of office referrals to African American students.[7]

Some literature suggests that congruence between student and staff demographic background may affect both disciplinary and academic outcomes. Using statewide longitudinal data on Texas middle and high school students, Blake et al. (2016) note that both Black and Hispanic students experience significantly elevated odds of discipline when moving to schools with more racially dissimilar faculty.[8] Broadly pointing to cultural differences as a source of classroom misunderstandings, the authors posit that "teachers may unknowingly apply media-driven stereotypes about Black culture to understand the ambiguous actions of Black students that are distinct from

[6] Mizel et al. (2016) collects survey data from a panel of 2,539 10th through 12th graders in 16 schools across 3 districts in Southern California.

[7] Rocque (2010) study a sample of over 28,000 students in 45 Virginia elementary schools, using data from official school records and teacher reports.

[8] This phenomenon varied by gender, and was particularly pronounced for Black males.

White, middle-class culture" (Blake et al., 2016). In terms of academic performance, Dee (2004) estimates that being taught for one year by a teacher of the same race lead to math and reading score improvements of 2 and 4 state percentiles,[9] respectively, for both Black and White students.

Evidence of implicit bias also emerges in large sample studies of faculty and administrator subjective perceptions of minority students. For instance, it has been documented that Black teachers in the National Educational Longitudinal Study of 1988 are more likely to rate Black students favorably in terms of behavioral characteristics and career prospects (Ehrenberg et al., 1995). Conversely, White teachers in the ECLSK are more likely to give Black students poor behavioral ratings (though Black kindergartners do not for their part appear to differentially prefer teachers by race) (Downey and Pribesh, 2004).[10] In a smaller study based on direct observation of instructional time in a Louisiana school district, Casteel (1998) reports that White teachers both called on Black students less frequently to answer questions, and also gave Black students less praise for correct answers.[11]

In particular, from Figlio (2006), it is apparent that ad hoc discrimination in the modern era has occurred on a large scale at least in some areas: throughout the period 1996-2000, school districts in Florida selectively gave low achieving students longer suspensions — despite conduct policies not explicitly differentiating between high and low performers — in order to prevent them from influencing school performance in statewide high stakes assessments.

On the one hand, the above findings in general would suggest that minimizing faculty and administrator discretion in disciplinary processes should lead to more equitable outcomes. On the other hand, however, it is not clear that race gaps in discipline stem entirely from staff bias. At least across schools, Kinsler (2011) finds in administrative data from North Carolina that principal and

[9] Dee (2004) study test score data from the Tennessee STAR program.

[10] Similarly, Dee (2005) find that teachers are more likely to deem students of other racial/ethnic designations disruptive, regardless of whether they are White, Black, or Hispanic. Their study also determines that this occurs across genders as well: both male and female students are face worse assessments from teachers of the other sex.

[11] For an overview of smaller sample, mixed methods studies on classroom implicit bias, refer to Irvine (1988).

teacher race does not explain away differences between Black and White students' likelihood of suspension or length of suspension. According to Kinsler (2013), the greater use of exclusionary discipline by principals in predominantly minority schools is furthermore in fact "consistent with achievement maximizing behavior on the part of principals": as schools with proportionally more minorities contain a higher concentration of individuals at risk of misbehavior, harsher punishments are necessary to deter harmful disruptions. Note that in this case, since all students within a school face the same schedule of punishments, large disparate impacts can arise throughout a district absent any implicit bias by educational personnel. Importantly, one implication of this scenario is that the use of concrete, highly specific rules for discipline within a given school should not by itself affect within-school gaps.

3.3 AB 420 and Willful Defiance in California Public Schools

In 2014, the California State Legislature passed AB 420, the second bill proposed since 2012 to curb the use of suspension and expulsion as punishment for "willful defiance." The new law, approved in September 2014 by Governor Jerry Brown, eliminated the option to expel students for "disrupting school activities or otherwise willfully defying the valid authority of supervisors, teachers, administrators, school officials, or other school personnel engaged in the performance of their duties." Brown had previously vetoed a similar bill, AB 2242, due to the fact that it would have additionally banned willful defiance suspensions for all grades (Washburn, 2018). Unlike AB 2242, AB 420 only banned willful defiance suspensions for students in Kindergarten through 3rd grade. Sponsored mostly by democrats, AB 420 reflected a concern that the subjective judgments involved in determining students to be "defiant" or "disruptive" opened the door for implicit bias, thereby leading to differential punishment (Washburn, 2018). Henceforth until July 1, 2018, suspensions and expulsions could only be issued to students found to have committed certain specifically defined acts, such as drug or weapon possession, theft, and assault.

With AB 420 set to expire prior to the 2018-19 school year, state lawmakers have recently (as of this writing) considered implementing SB 607, the "Keep Kids in Schools Act," to both extend the current bill's provisions on defiance related expulsion and also expand the ban against defiance suspensions to all grades, this time for the next decade. As many educators view the large decreases in exclusionary discipline associated with such recent efforts to have increased the difficulty of dealing with genuinely disruptive behavior, it is important to understand whether or not this policy change has in fact reduced disproportionality. With proponents of SB 607 arguing for school discipline to rely more on restorative justice (e.g. mediation)[12] and Positive Behavioral Intervention and Supports (PBIS),[13] the California Teachers Association (CTA) has indicated that its support of the bill will depend on increased funding for teacher training (Washburn, 2018). Despite mixed evidence as to the academic benefits of reducing punishment on its own, such an expense by the state may still be justified by proven gains in equity (and hence assuring Title IV compliance, a factor in federal education funding).

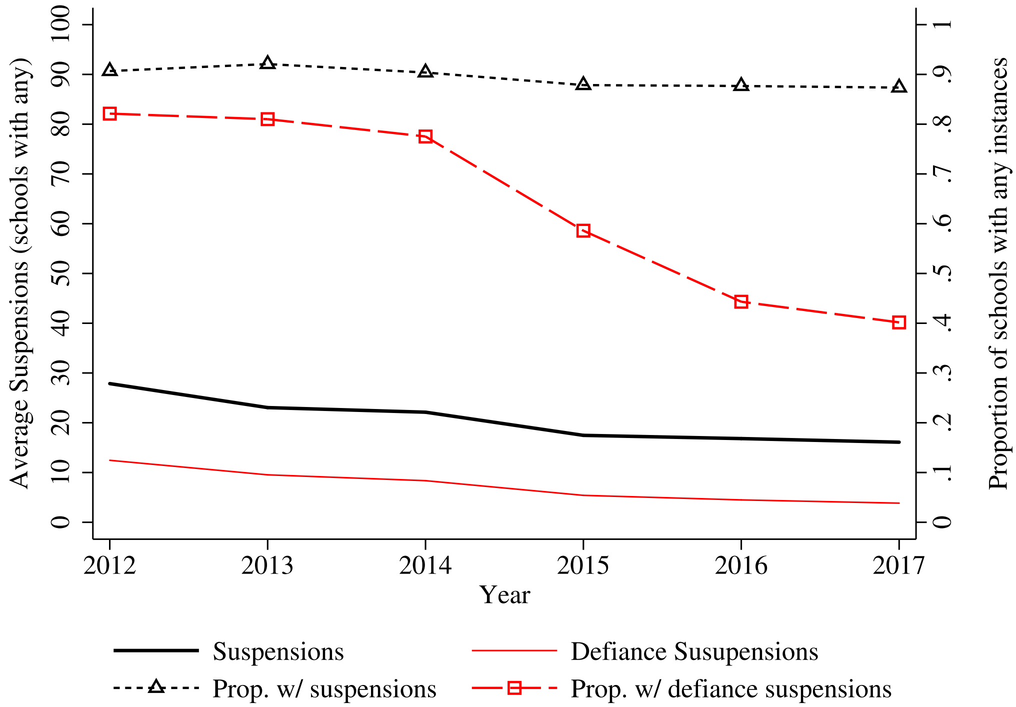
Although we might not expect to attribute a large share of suspensions to students in grades K-3 traditionally, it appears that AB 420 did nonetheless correspond with a substantial decline in defiance related suspensions. Figure 3.1 plots the average number of suspensions at schools whose pre AB 420 proportion of suspensions attributable to defiance (averaged over the 2011-12 through 2013-14 school years) was above the median. Note that the solid black (thick) line plots average total suspensions in each year only among schools having any suspensions in that year, and that the solid red (thin) line does the same for average defiance related suspensions. While a gradual decline in willful defiance suspensions appears to drive a decline in total suspensions throughout the entire period, the proportion of schools issuing any willful defiance suspensions clearly exhibits a sharp decline corresponding with the passage of AB 420.

It is possible that the passage of AB 420, particularly on the heels of the DOE/DOJ guidance earlier in the year, discouraged citing students for willful defiance in general by a number of means.

[12]While varying by implementation, mediation generally entails convening a circle of peers of the parties in conflict to determine steps necessary for resolution, with faculty acting in an advisory capacity (Richmond, 2015). Resolution is intended to target root causes of specific behavior, rather than the blunter aim of deterrence (Richmond, 2015).

[13]PBIS entails "implementing a multi-tiered approach to social, emotional and behavior support" (for more details, see `https://www.pbis.org/`).

Figure 3.1: Suspensions Over Time Among Schools With Large Initial Proportion of Defiance Suspensions



This graphic represents average suspensions over time among schools that gave out more than the median pre-period average (2012-2014) proportion of willful defiance suspensions (as a proportion of total suspensions).

Firstly, the bill's passage may have caused administrators to anticipate that defiance suspensions would soon be removed entirely from their toolkit, and they may thus have used this period to adjust to other behavior management strategies for 4th and 5th graders as well. Additionally, Babcock (2009) provides evidence that "state-level judicial-legal climate does appear to influence administrators' discipline policies." Examining data from 132 middle and high schools across the country, Babcock (2009) finds that student rights lawsuits brought by public interest law firms led to subsequently less harsh punishments within states where the suits took place.

As AB 420 was brought about in the midst of both national and state-level coordinated shifts towards less punitive approaches to discipline, the next section summarizes and breaks down trends in exclusionary discipline in California from the 2011-12 through 2016-17 school years.

3.4 Recent CA Suspensions and Disproportionality

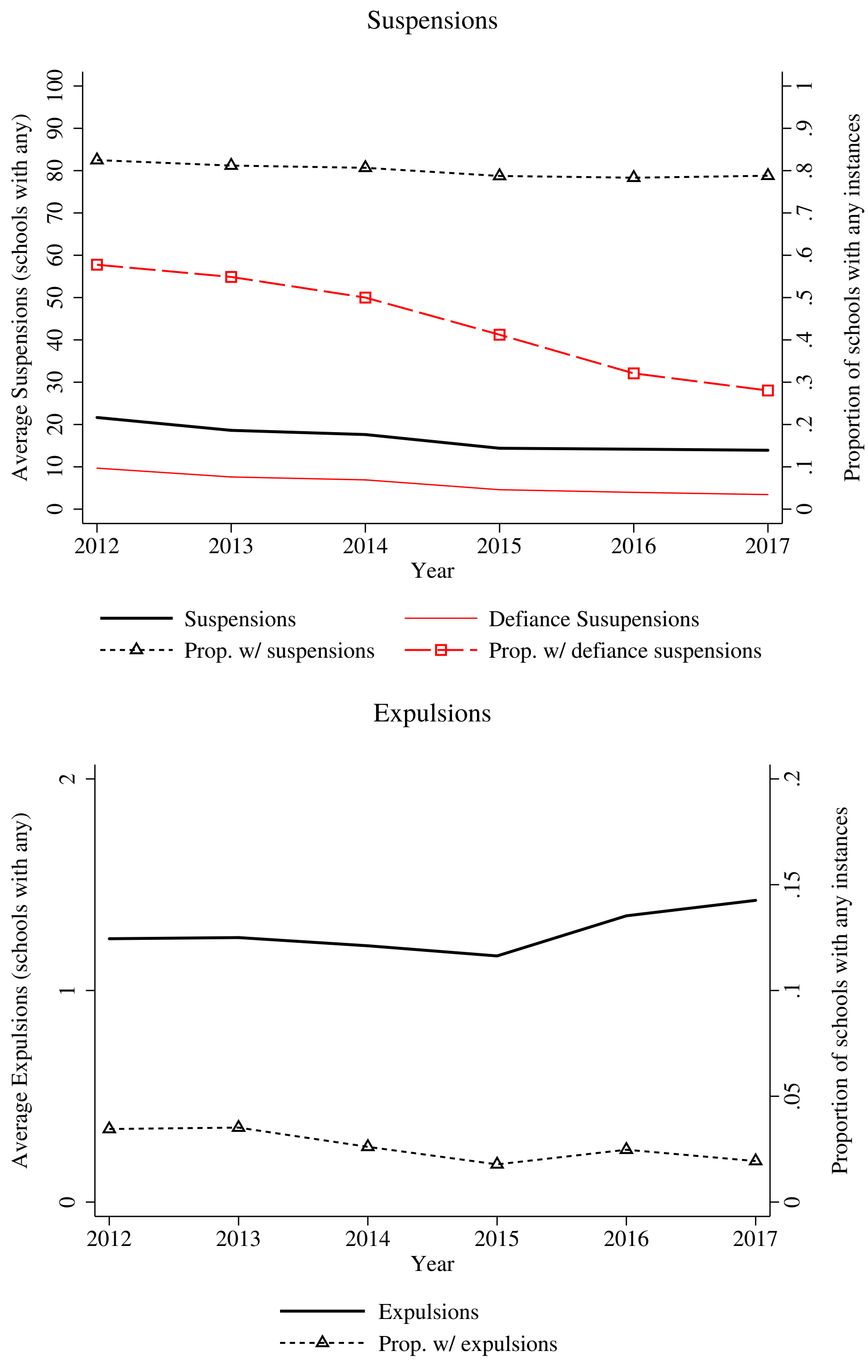
As of this writing, administrative data on suspensions and expulsions in all California public schools (approximately 10,500) are made available by the CA Department of Education for the academic years from 2011-12 through 2016-17. Information includes each school's number of suspensions and expulsions citing defiance and non-defiance reasons, broken down by racial/ethnic designation. However, expulsions among elementary school students are exceedingly rare (as shown below) and are therefore set aside in the analysis to follow.

As can be seen in panel (a) of Figure 3.2, school suspension fell significantly in overall volume over the period surrounding the passage of AB 420. While the proportion of schools issuing any suspensions remained more or less constant (at around 80%), the average number of suspensions among those schools dropped by roughly a third, from 21.7 in 2011-12 to only 13.9 in 2016-17. In the case of suspensions specifically for willful defiance, a decrease in the proportion of schools employing this method of discipline is evident (going from 57.8% in 2011-12 to 28% in 2016-17). While we might expect this to be driven by administrators abandoning the practice who previously had not relied heavily on it to begin with, Figure 3.1 suggests that the opposite is true: it is in fact the schools which previously leaned most heavily on defiance suspensions (as a proportion of total suspensions) that stopped. Hence, among schools that issued any defiance related suspensions, the average number of these fell from 9.7 to 3.4.

In panel (b) of Figure 3.2, the proportion of schools issuing expulsions fall as well, albeit following a notably different pattern: though the proportion declines slightly in the pre-period, this trend is sharply broken upon the 2015-16 school year (one year after the passage of AB 420). Similarly, the very slight pre-period downward trend in the average number of expulsions (among schools issuing any) appears to reverse upon the 2015-16 school year. Among such schools, expulsions overall actually rise from an average of 1.2 in 2011-12 to 1.4 in 2016-17. Ultimately, the proportion of schools issuing expulsions falls from 3.5% initially to 1.8% in 2015-16, before slightly climbing again to 2.5%.

It is unclear from the following analysis why these particular patterns emerge. While suspensions

Figure 3.2: Average Suspensions and Expulsions Over Time



In the above graphics, averages of suspensions/expulsions in each year are calculated based on schools that gave out suspensions/expulsions in those years. For example, each year composing the solid thin (red) line in panel (a) is based on schools giving out any suspensions specifically for willful defiance in that year.

have recently become less popular among elementary schools, it may be the case that expulsions, as the most severe punishment — undertaken as a last resort — have reached a functional floor: the remaining instances might be generally necessitated by extreme situations, and therefore cannot be reduced further. Here it is perhaps worth noting again that movements in the volume of exclusionary discipline are not necessarily related to any particular student group's relative proportion of citations.

In the graphs in either column of Figure 3.3, we may visualize disproportionality in suspensions for each race category as the distance between its respective red (dashed) and black (solid) lines. For example, the left panel of row (a) shows that Black students throughout the study period have accounted for roughly 15-16% of suspensions, while comprising roughly 6-7% of enrollment. Hence, overall, White students as a group are slightly overrepresented in suspensions, and Hispanic students are underrepresented in suspensions. Like elsewhere in the country, Black students are substantially overrepresented with respect to their enrollment in suspensions.

3.5 Role of Willful Defiance

3.5.1 Treatment Intensity: Initial Use of Willful Defiance

As noted previously, simply reducing the overall volume of exclusionary discipline does not necessarily change disproportionality, and hence inequity as thought of by the DOE (and may even be undesirable absent any gains in "fairness"). Thus, this paper also aims to shed light on whether or not reducing defiance-related punishment alleviated racial disproportionality in school suspension, as measured by the difference between a given group's proportion of total suspensions and its proportion of total enrollment.

To this end, I compare pre and post AB 420 disproportionality in exclusionary discipline across elementary schools based on their pre-period (2012-2014) proportion of suspensions that cite the offense of willful defiance. With a one-time "treatment" and no control group, this approach may be thought of as identifying effects based on variation in treatment intensity. Explicitly, I assume that elementary schools with a high pre-period proportion of discipline citing willful defiance are ultimately more affected by the law than those whose disciplinary citations mostly already involved

Figure 3.3: Disproportionality in Suspensions Over Time

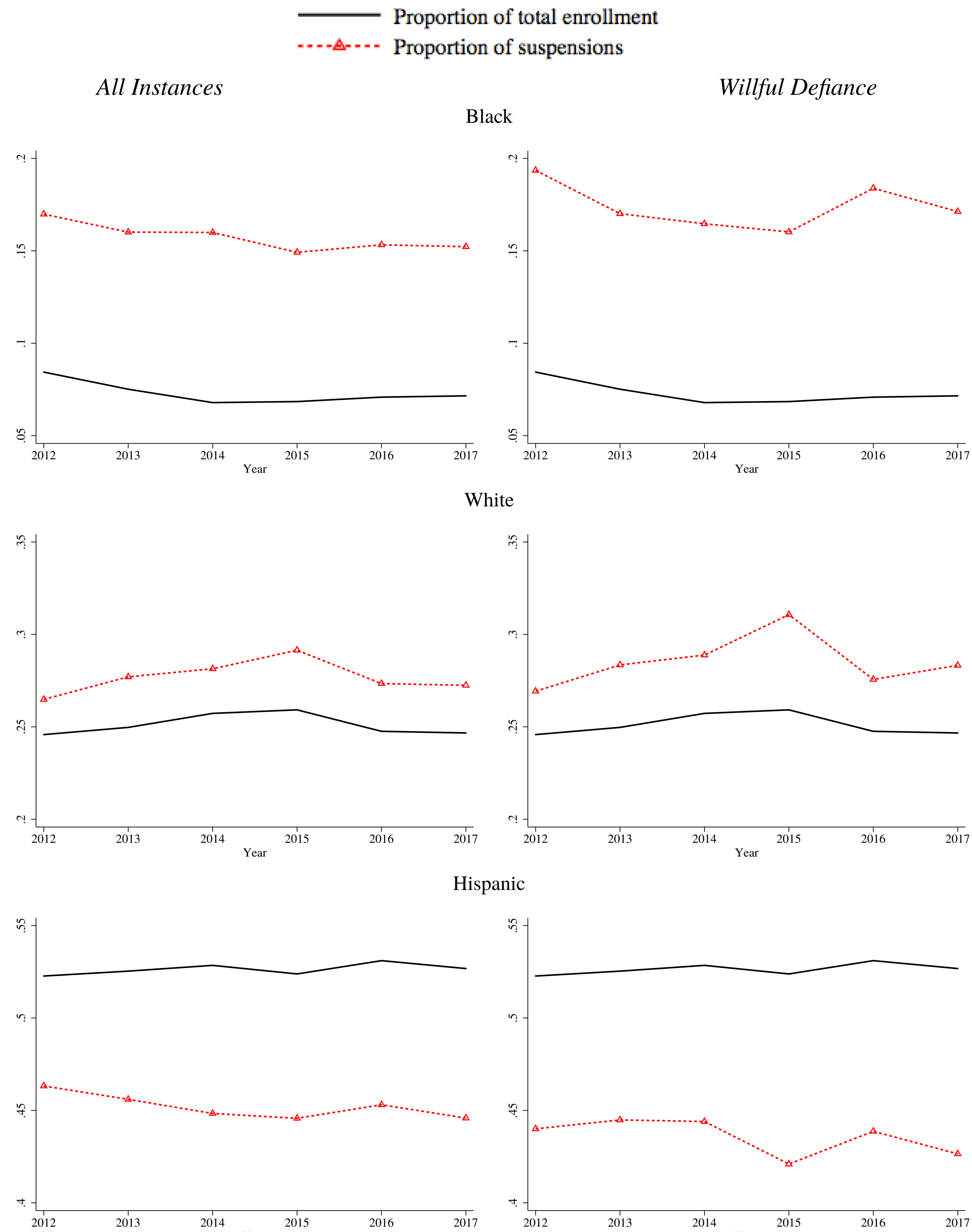


Table 3.1: Effects of “Willful Defiance” on Disproportionality in Suspensions

	(1) Black	(2) White	(3) Hispanic
Difference between proportion of suspensions and proportion of enrollment			
AB 420 X Initial defiance related proportion of discipline	0.018 (0.014)	-0.035 (0.025)	0.023 (0.023)
Initial prop. of suspensions	.159	.273	.455
Initial prop. of enrollment	.069	.261	.509
Observations	12100	12541	12671
School FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Outcomes are the differences between each race category’s proportion of total suspensions and its proportion of total enrollment. Controls include school level enrollment, proportion FRPL eligible, and proportion English learners. Standard errors in parentheses, clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

concrete offenses to begin with. Some evidence of this is shown further down in Figure 3.4. Without suspensions by grade level, this analysis is agnostic as to whether changes in disproportionality reflect schools being actually constrained by the law from issuing suspensions, versus responding to politics or adjusting in preparation for further anticipated changes.

$$Y_{rit} = \beta_1(\text{AB } 420_t \times \text{Initial W.D. prop. of discipl.}_i) + \gamma X_{it} + \alpha_i + \phi_t + \varepsilon_{it} \tag{3.1}$$

In the above equation, Y_{rit} represents the difference between group r’s proportion of total suspensions and its proportion of enrollment in school i during year t. “Initial W.D. prop. of discipline” is school i’s pre-period (2012-2014) average proportion of suspensions citing willful defiance as the offense being punished. Finally, X_{it} is a set of time-varying school level controls,[14] and α_i and ϕ_t are school and year fixed effects, respectively.

Table 3.1 shows the results of estimating Equation 3.1. As previously determined based on Figure 3.3, overall disproportionality does not change notably from the pre period to the post period

[14]Control variables include overall enrollment, proportion eligible for free and reduced price lunches (FRPL), and proportion English learners (ELL).

for any of the groups by race. Inspecting the coefficients on the interaction term, it seems also that the passage of AB 420 did not elicit differential responses in the overall disproportionality facing any group based on schools' initial proportion of discipline citing willful defiance.

Considering the law at face value, this is not necessarily surprising. Suppose that reducing the volume of discipline entails pardoning a random selection of students from the pool of those who would have been punished: although some groups would certainly see a larger number of students pardoned, no group would mechanically achieve any gains in likelihood of discipline relative to the others (their chances of discipline will fall by the same factor). The results of Table 3.1 may indicate that administrators did not respond to AB 420 with particular focus on reducing the punishment incurred by any one overrepresented group.

3.5.2 Robustness

As a check that the proportion of willful defiance citations is not influenced by other factors, besides AB 420, that might also impact disciplinary disproportionality, I regress the contemporaneous proportions of defiance-related suspensions on various school socio-demographic variables and on their interactions with the AB 420 indicator. In Table 3.2, the fraction of discipline citing defiance appears to be mostly unrelated to these other factors, at least as included on their own. However, the fraction of English learner students as well as the interaction terms involving meal assistance, English learners, and Black enrollment are statistically significantly correlated with the proportion of suspensions citing defiance. In other words, schools' proportion of ELL students may be linked with the use of willful defiance suspensions, and schools' proportions of FRPL, ELL, and Black students may be linked with their change in willful defiance suspensions following AB 420. As noted previously, this paper's analyses include these school characteristics as controls.

As a check for pre-trends, Figure 3.4 plots event studies of the school-level proportion of suspensions citing defiance (with controls for school socio-economic characteristics), using separate subsamples of schools by pre-period (2012-2014) proportion of suspensions citing defiance. On the one hand, schools in the second quantile, which initially cited defiance more heavily, did indeed

Table 3.2: Proportion of Defiance Suspensions Regressed on School Demographics

AB 420 onwards	-0.105***
	(0.034)
Proportion FRPL Eligible	0.067
	(0.052)
Proportion English Learners	-0.241***
	(0.078)
Total Enrollment	0.000
	(0.000)
Proportion Black	-0.196
	(0.166)
Proportion White	-0.065
	(0.143)
Proportion Hispanic	0.042
	(0.100)
AB 420 X Prop. FRPL	-0.153***
	(0.033)
AB 420 X Prop. ELL	0.177***
	(0.051)
AB 420 X Tot. Enrol.	-0.000
	(0.000)
AB 420 X Prop. Black	0.145***
	(0.053)
AB 420 X Prop. White	-0.008
	(0.042)
AB 420 X Prop. Hisp.	0.056
	(0.038)
Observations	12362
School FE	Yes
Year FE	Yes
Controls	Yes

Outcomes are the contemporaneous proportions of total suspensions citing willful defiance. Standard errors in parentheses, clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

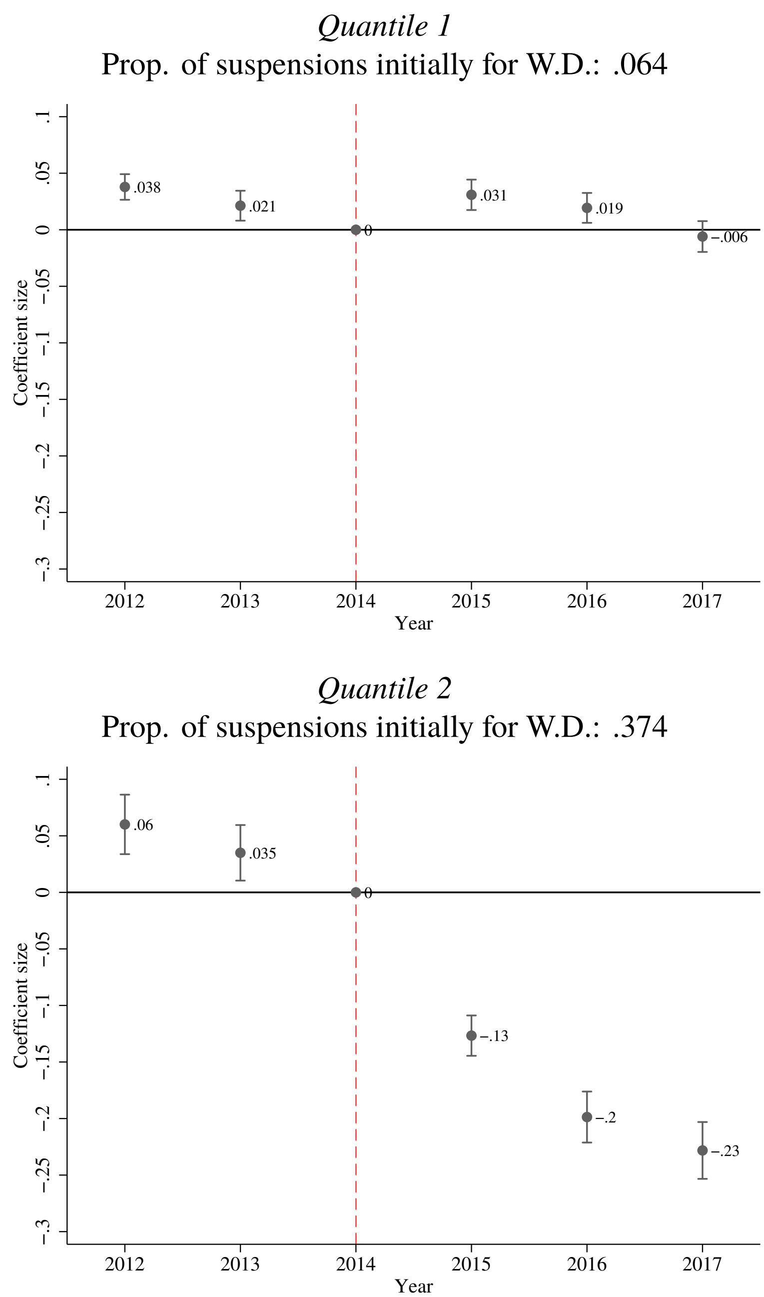
see a large decrease in their defiance-related proportion of disciplinary actions over the period (as opposed to schools in the first quantile, which did not). There furthermore appears to be a large drop in these schools' defiance-related proportion of suspensions coinciding with the passage of AB 420. As we might suspect, however, considering the political climate surrounding the law and the previous attempt to pass AB 2242, willful defiance citations look to have already been trending downwards in the pre-period. If steep downward trends in defiance-related discipline coincide with additional efforts to reduce racial disproportionality, then the estimates of the coefficient on the interaction term in Table 3.1 may be biased downward. As these estimates are nonetheless not statistically significant, this does not appear to be so (barring the case in which exogenously reducing punishment for willful defiance otherwise leads to greater disproportionality).

3.5.3 Separate Quantiles by Initial Use of Willful Defiance

Figure 3.5 plots event study regressions of disciplinary disproportionality (with controls for school socio-economic characteristics) on separate subsamples of schools, characterized by issuing an above or below median pre-period average proportion of their total suspensions as punishment for defiance (2nd quantile versus 1st quantile, respectively). Similar to the prior analysis reported in Table 3.1, Figure 3.5 offers little indication that disproportionality going from the pre to post AB 420 period changes any differently based on initial proportion of discipline citing willful defiance.

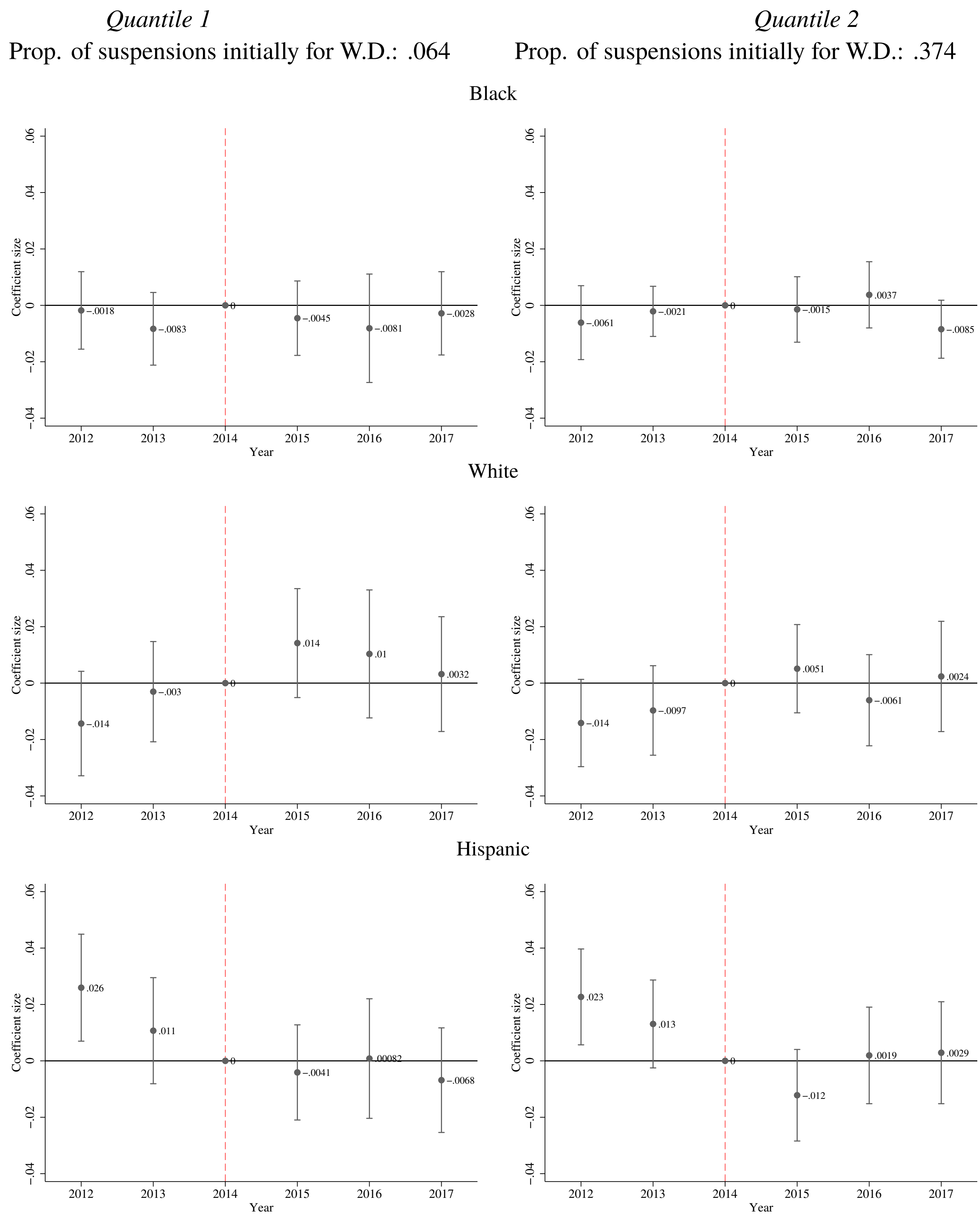
Table C.1 additionally presents the results of estimating Equation C.1 (see Appendix) on the above described quantile subsamples. In this specification, the pre and post period years are aggregated into a dummy indicator for the post period, while the year dummies are replaced with a continuous time variable. Perhaps what is most interesting in the summary figures of Table C.1 is the fact that initial disproportionality (the difference between each group's pre-period average proportion of discipline versus enrollment) appears to be nearly similar between quantiles by initial defiance proportion of discipline. That is to say, that schools initially issuing many of their suspensions for defiance (on average 37.4% of all their suspensions) do not seem to have exhibited greater racial disproportionality than schools issuing only 6.4% of their suspensions for defiance. If

Figure 3.4: Proportion of Discipline Related to Defiance by Initial Proportion of Discipline Related to Defiance



Outcomes are the contemporaneous proportions of total suspensions citing willful defiance. Standard errors clustered at the district level. Controls include school level enrollment, proportion FRPL eligible, and proportion English learners.
For each type of discipline, schools are divided into quantiles based on initial proportion of citations for willful defiance (averaged over the period 2012-2014). Hence, the graph under "Quantile 2" illustrates the change in the proportion of defiance related suspensions over time among schools which initially issued an above-median proportion of suspensions citing willful defiance.

Figure 3.5: Disproportionality in School Discipline by Initial Use of “Willful Defiance”



Outcomes are the differences between each race category’s proportion of total suspensions and its proportion of total enrollment. Standard errors clustered at the district level. Controls include school level enrollment, proportion FRPL eligible, and proportion English learners.

anything, Black students may be slightly less overrepresented in discipline among schools initially leaning more on defiance citations. Consistent with the prior analysis, the afore described statistics are unsurprising in the case that punishment of defiance is not any more disproportionate than punishment of more concretely specified behavior.

3.6 Summary

In recent years, California educators have significantly reduced their use of exclusionary discipline for managing problem behavior among students. School suspensions have fallen markedly over the period 2012-2017, in terms of both the fractions of schools issuing any suspensions and the volume of occurrences recorded within these schools.

Counter to a popular narrative among many proponents of AB 420, trends in disciplinary proportionality in the years around the passage of the law do not particularly support the notion that punishment of willful defiance subjects students to greater implicit bias. Though defiance related discipline naturally fell more sharply in the schools initially issuing the largest proportion of their citations for defiance, the demographic distribution of discipline in these schools did not change distinctly compared with other schools.

One caveat concerns the downward trend in defiance related discipline surrounding the passage of AB 420; the decrease in punishment of defiance may correspond with other factors influencing disproportionality. However, if those factors promote more evenly distributed discipline (as seems likely when considering the recent political climate), then the absence of any evident link between willful defiance and racial disparities is consistent with there truly being no "equalizing" effect of requiring administrators to concretely specify offending acts.

With debate underway on whether or not to replace AB 420 — set to expire in July 2018 — the above results offer no evidence of gains in disciplinary equity (as currently conceptualized within federal oversight) to justify any losses in achievement from limiting educators' tools for behavior management. Indeed, many are quick to oppose efforts targeting disparate impact in general, on the grounds that prioritizing such metrics hamstrings teachers faced with genuinely disruptive behavior (Green, 2015). According to the 2015 Education Next poll, only 23% of teachers nationwide support “federal policies that prevent schools from expelling or suspending black and Hispanic students at higher rates than other students,” with 59% opposed (Green, 2015). There is certainly evidence to support their concerns: Figlio (2007) finds that adding a single disruptive child to a class of 30 students reduces peers’ math scores by roughly 4 national percentiles, while also increasing their likelihood of being suspended for five or more days by 3 percentage points. Kinsler (2013) additionally notes that “losing classroom time as a result of suspension has a small negative impact on performance, whereas exposure to disruptive behavior significantly reduces achievement.” Depending on the educational benefit of removing troubled students from the learning environment, future top-down efforts may benefit from more explicitly aiming to correct the distribution of punishment, rather than regulating its volume.

APPENDICES

APPENDIX A

CHAPTER 1 APPENDIX

Table A.1: Trend Break Specification on Pooled Sample (9th & 11th Graders)

	(1) Any drugs	(2) Coc./crack	(3) Marijuana	(4) Inhalants	(5) Meth	(6) Alcohol	(7) Cigarettes
Grade 9 & 11 combined							
Post	0.009**	0.004*	0.009***	0.002	0.001	0.006	0.002
	(0.004)	(0.002)	(0.003)	(0.002)	(0.001)	(0.004)	(0.003)
Post x	-0.001	0.000	-0.001	-0.000	-0.000	-0.001	-0.001
Evt Time	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Evt Time	0.007***	-0.000	0.005***	0.000	0.001	0.006***	0.000
	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.002)	(0.001)
Obs	648742	648742	648742	648742	648742	648742	648742
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Outcomes are indicators for any use of the substance in past 30 days. Controls include age, gender, race, school level enrollment, demographic composition, FRPL, and proportion English learners. Standard errors in parentheses, clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.1: Event Study Regressions Using Pooled Sample of 9th and 11th Graders Together

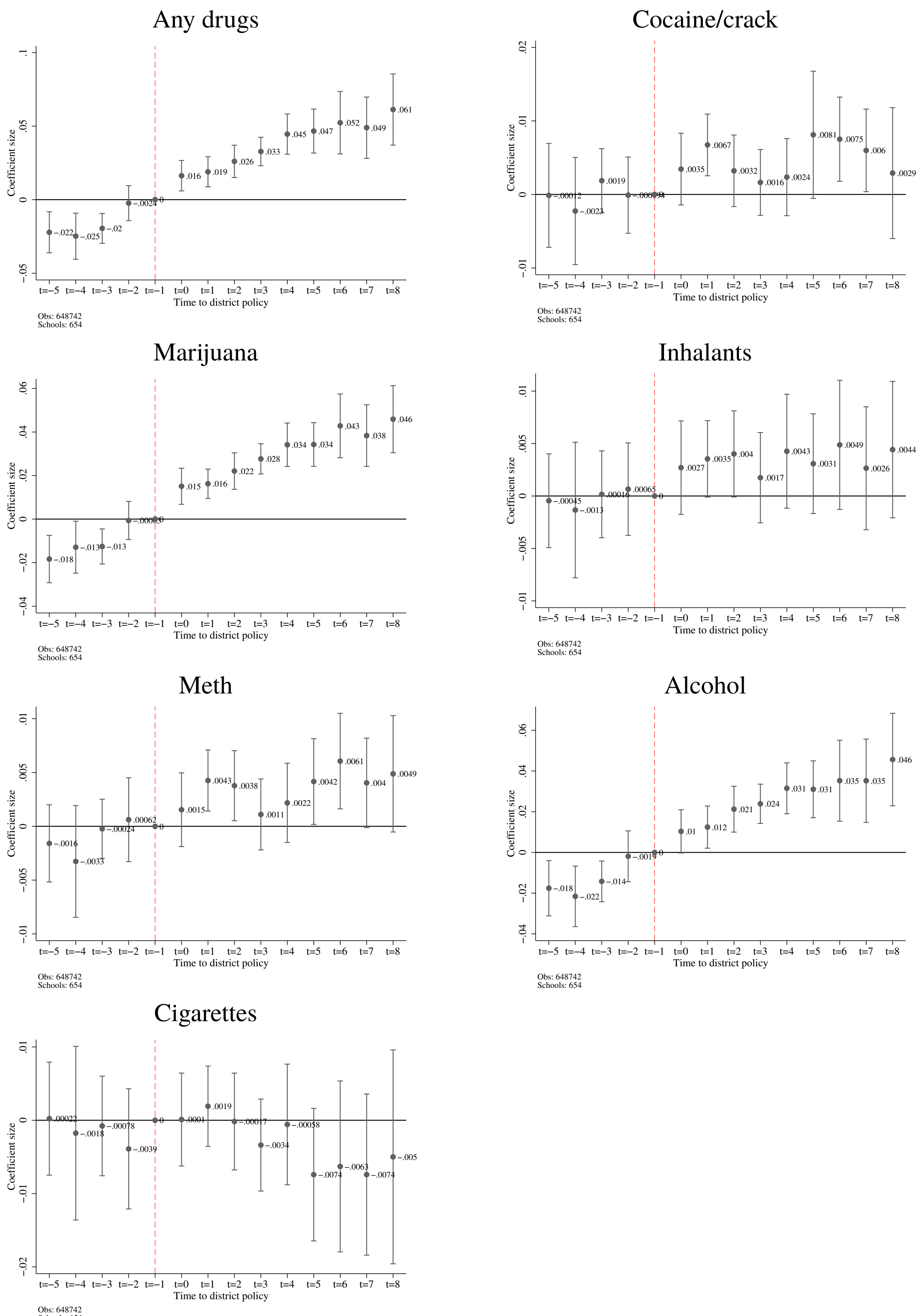
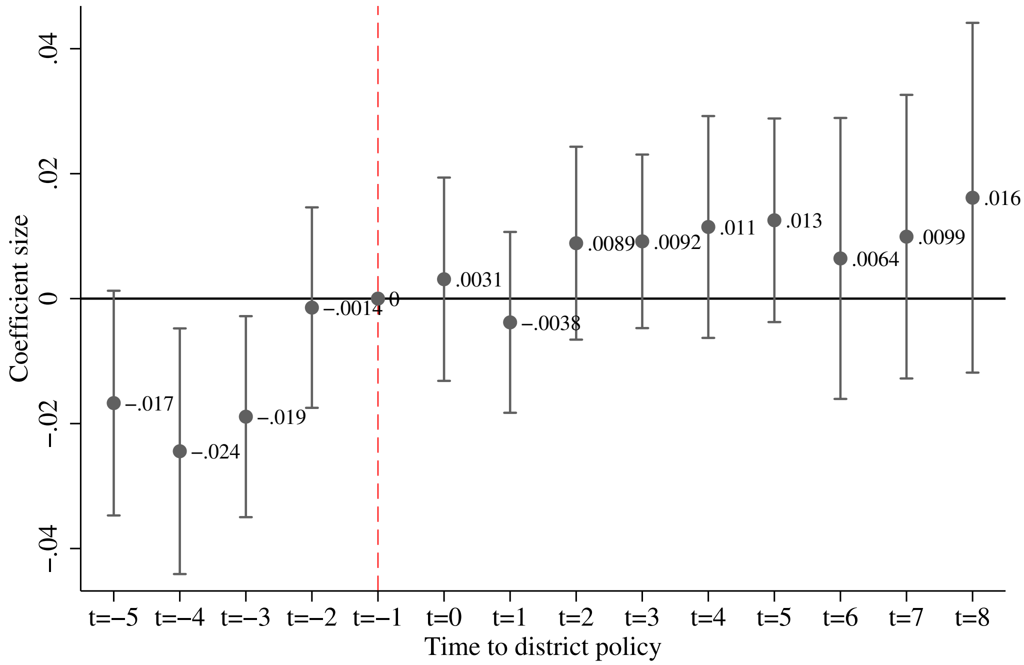


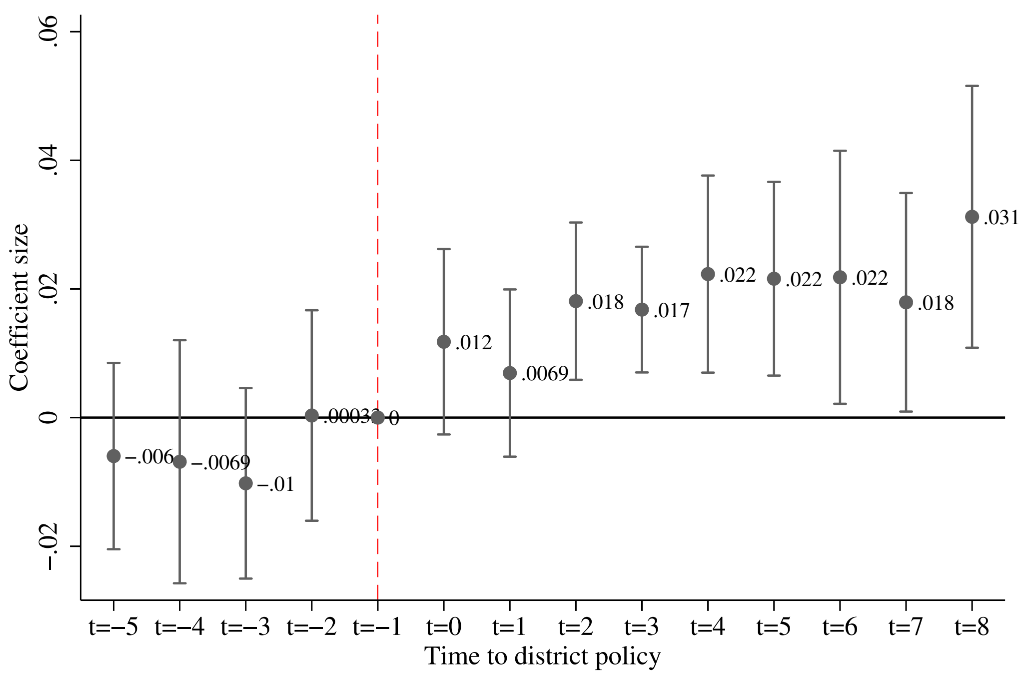
Figure A.2: 9th Grade Girls vs. Boys: Alcohol

Girls



Obs: 178732
Schools: 577

Boys



Obs: 167463
Schools: 588

Figure A.3: Districts not revising policy until 2006: Composite: g9 Any drugs

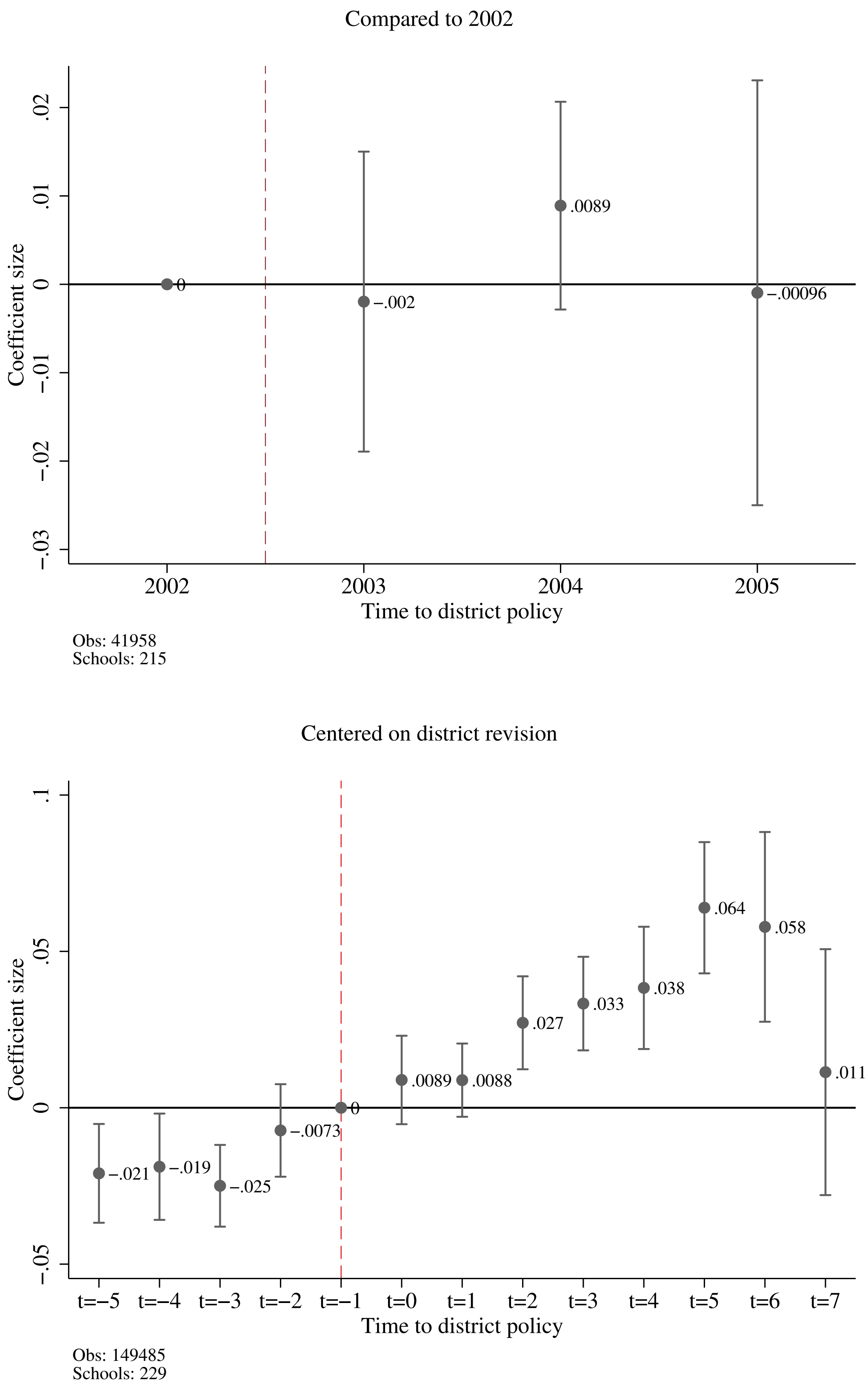


Figure A.4: Districts not revising policy until 2006: g9 Cocaine/crack

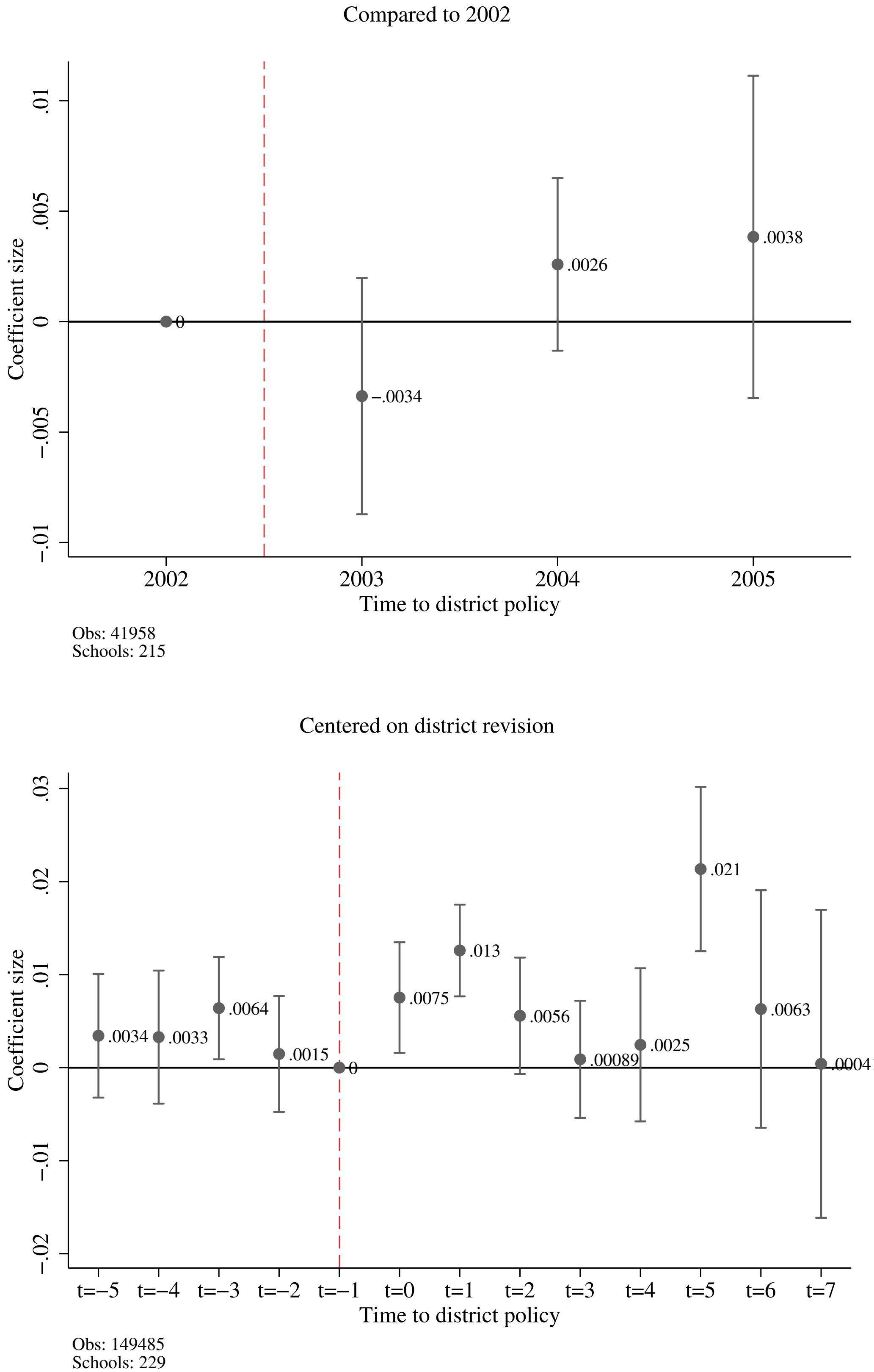
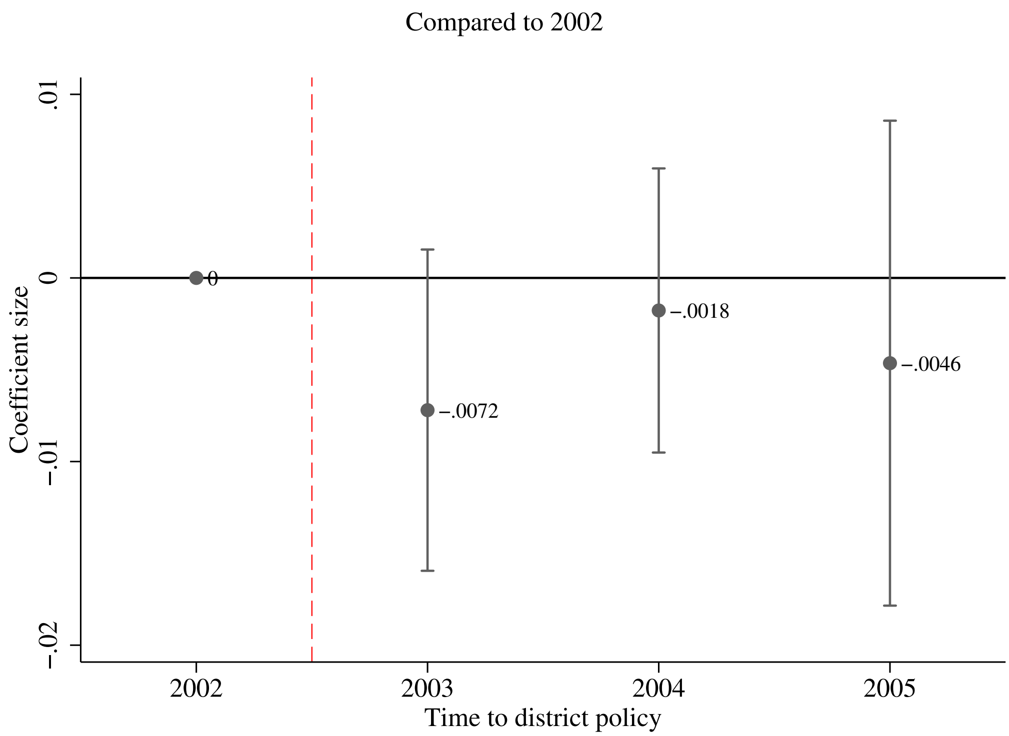
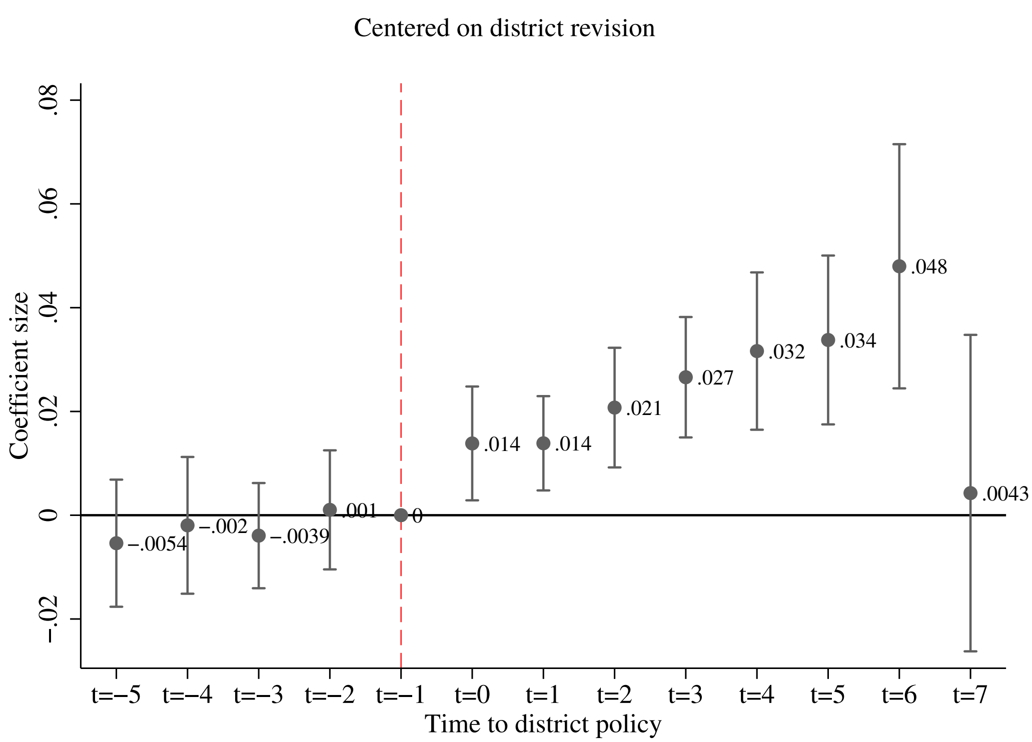


Figure A.5: Districts not revising policy until 2006: g9 Marijuana

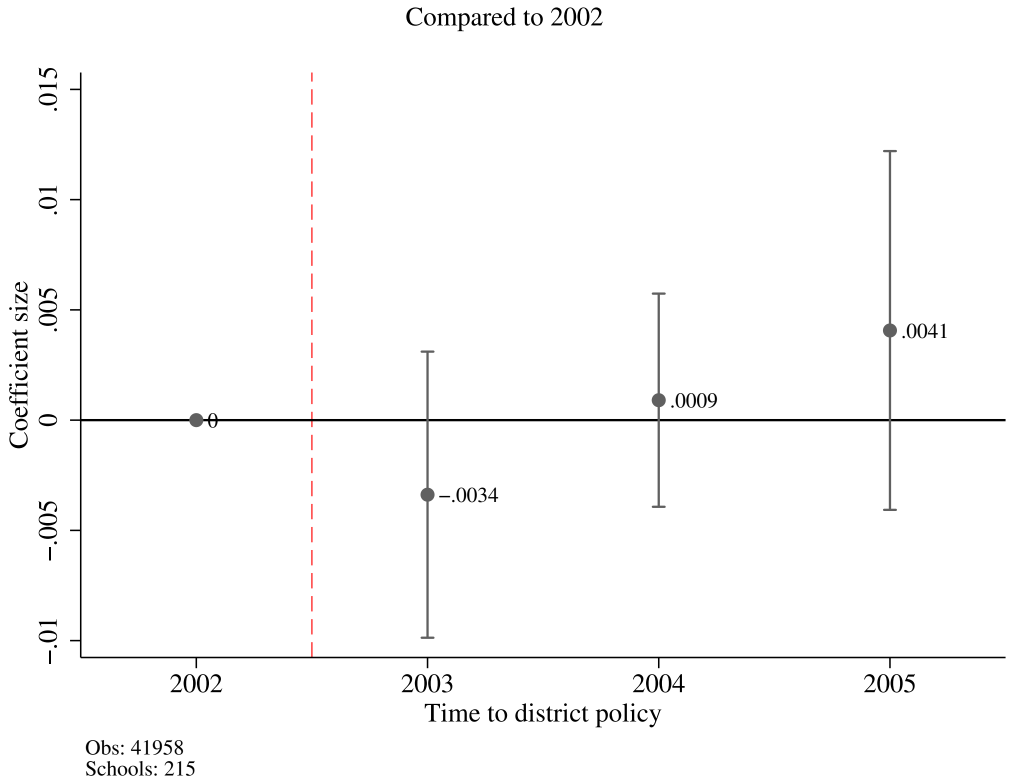


Obs: 41958
Schools: 215



Obs: 149485
Schools: 229

Figure A.6: Districts not revising policy until 2006: g9 Inhalants



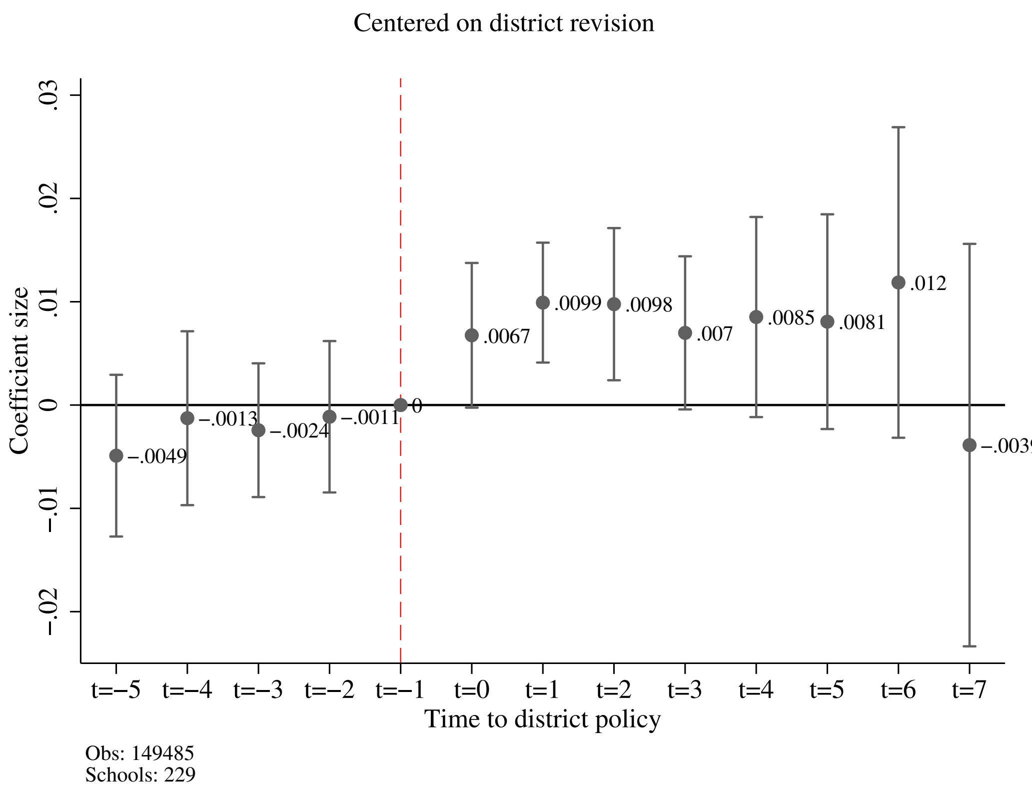
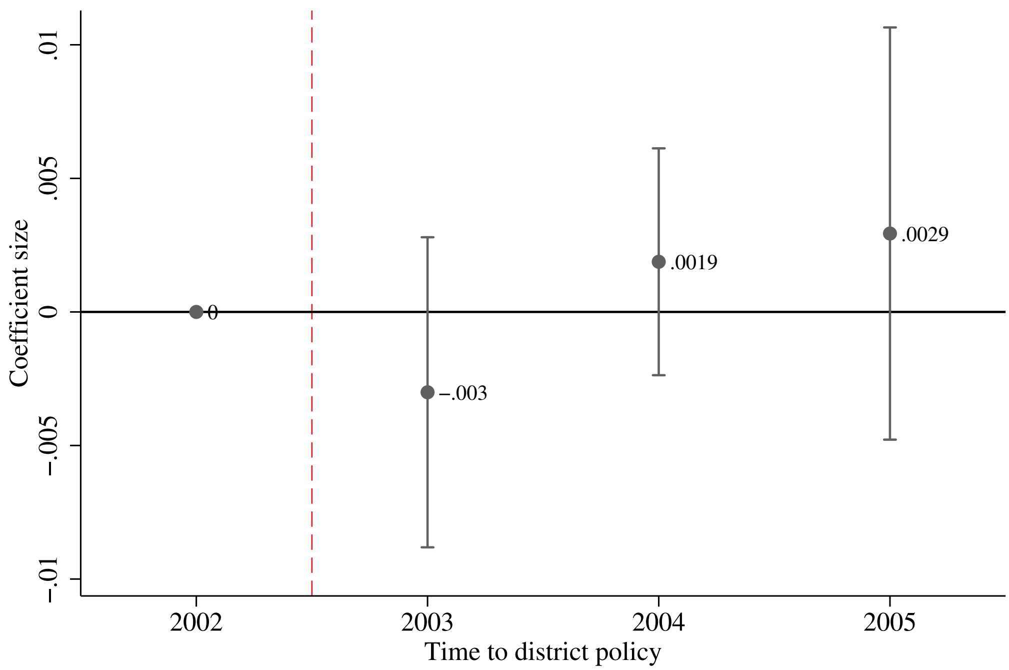


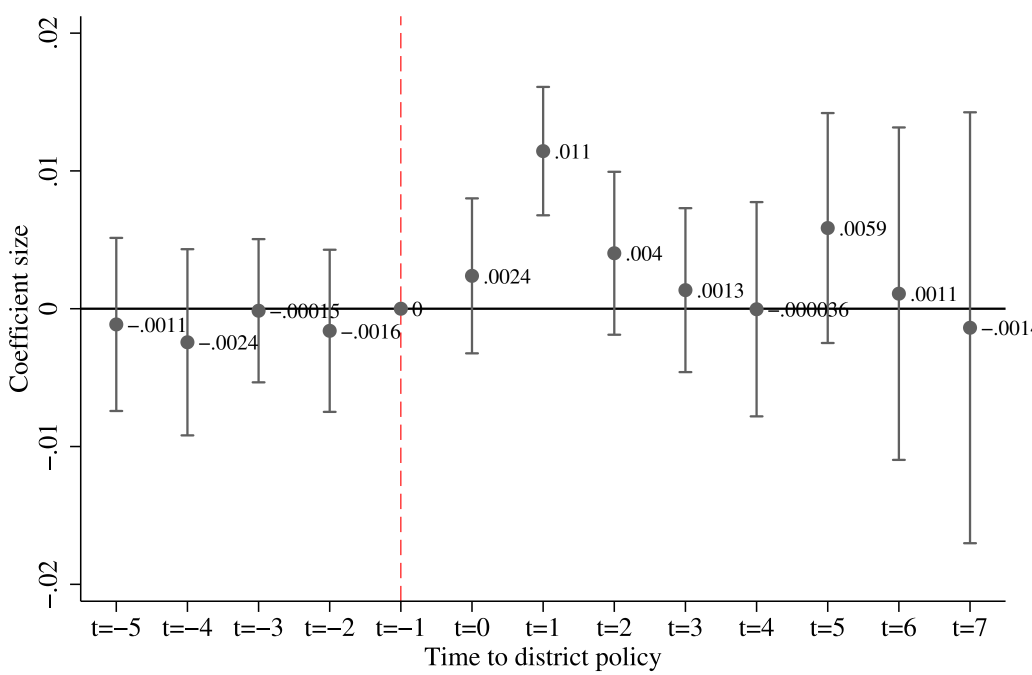
Figure A.7: Districts not revising policy until 2006: g9 Meth

Compared to 2002



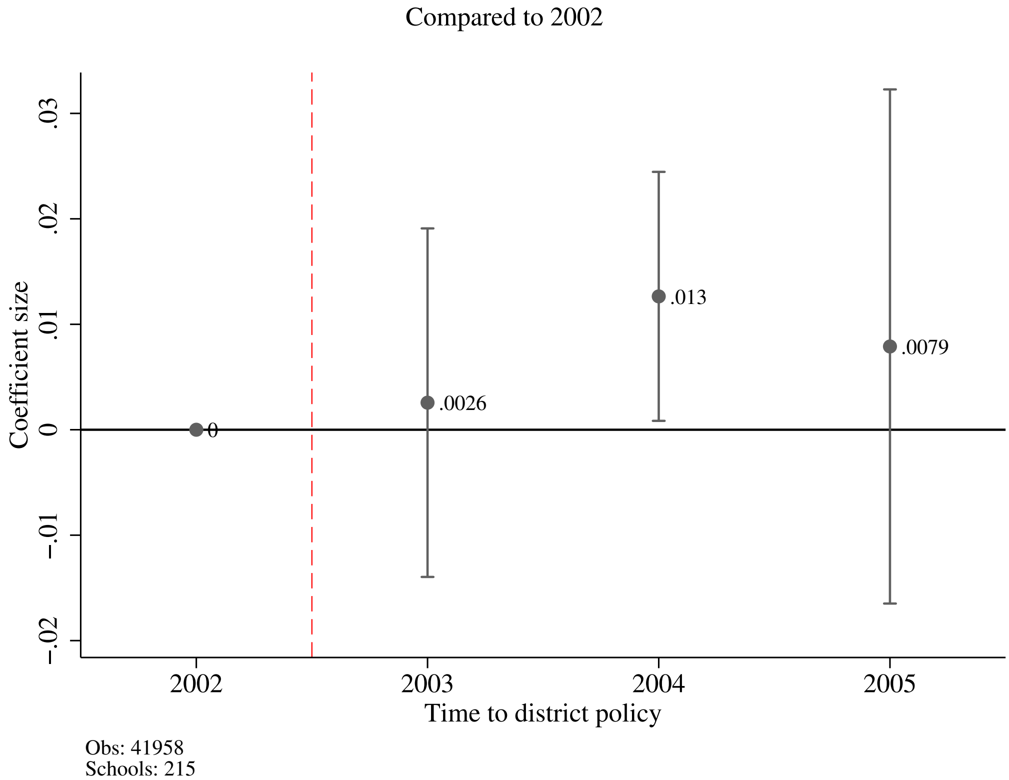
Obs: 41958
Schools: 215

Centered on district revision



Obs: 149485
Schools: 229

Figure A.8: Districts not revising policy until 2006: g9 Alcohol



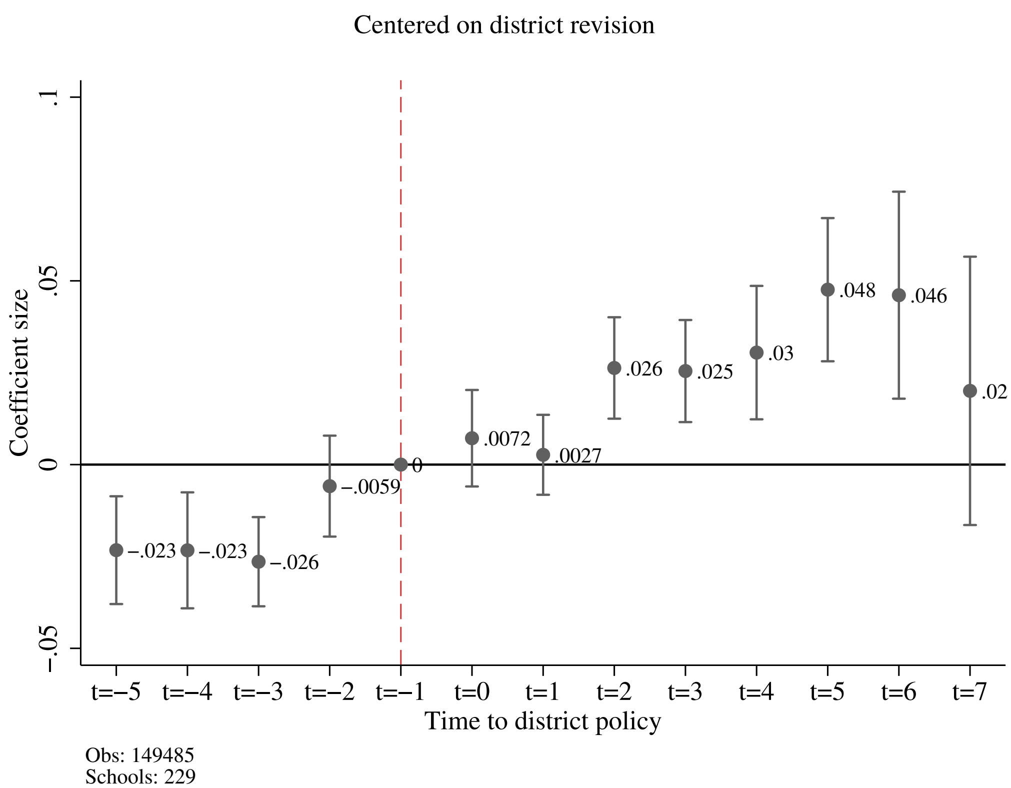
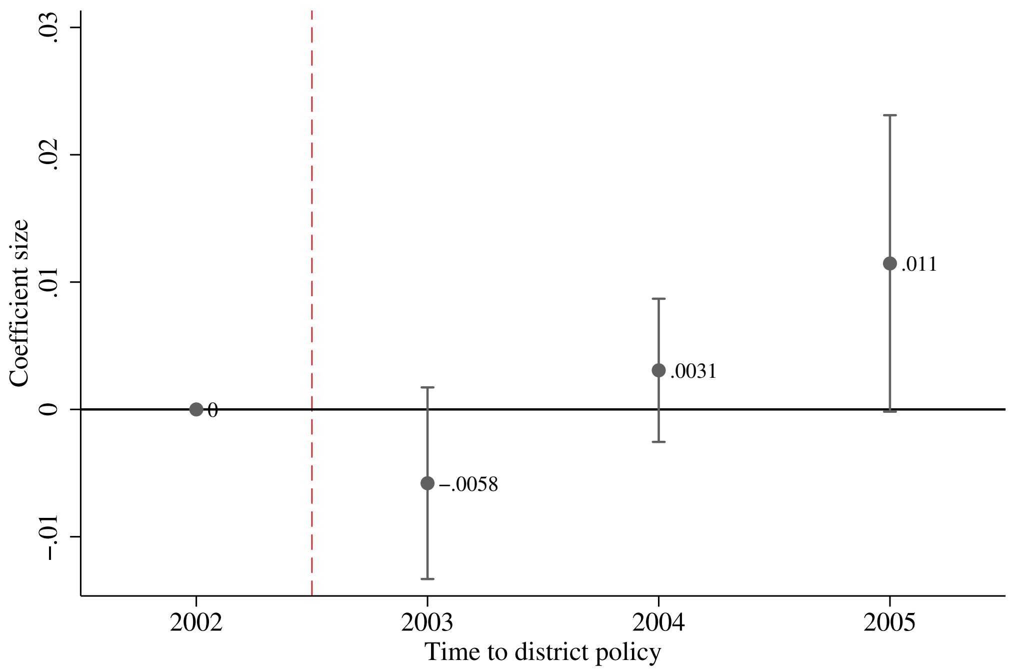


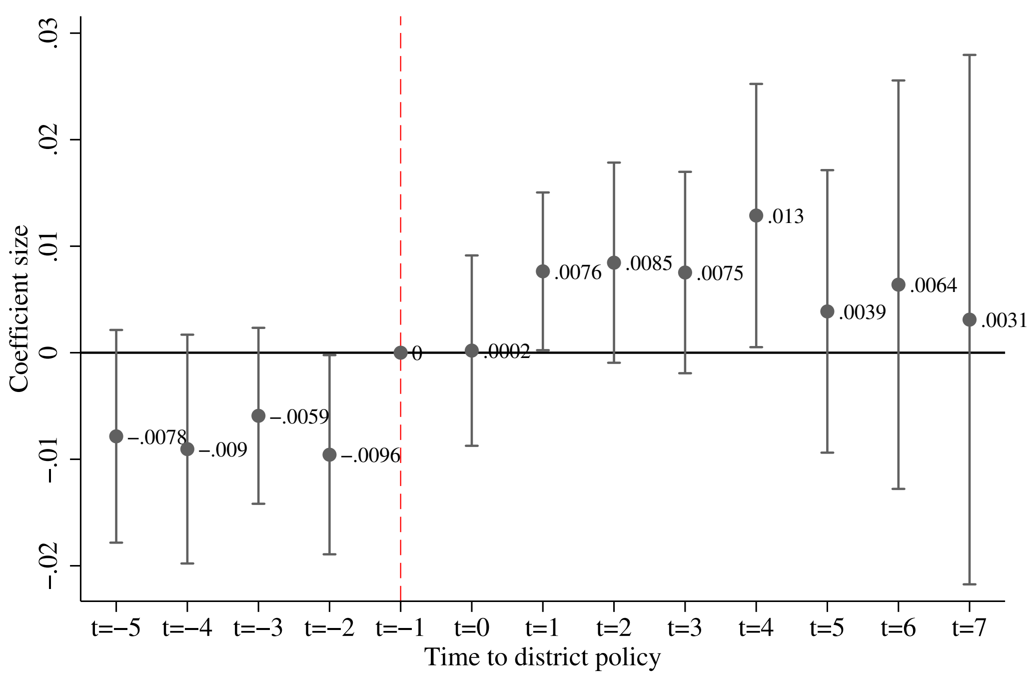
Figure A.9: Districts not revising policy until 2006: g9 Cigarettes

Compared to 2002



Obs: 41958
Schools: 215

Centered on district revision



Obs: 149485
Schools: 229

Figure A.10: Grade 9 Robustness: Gender and Proportion Black

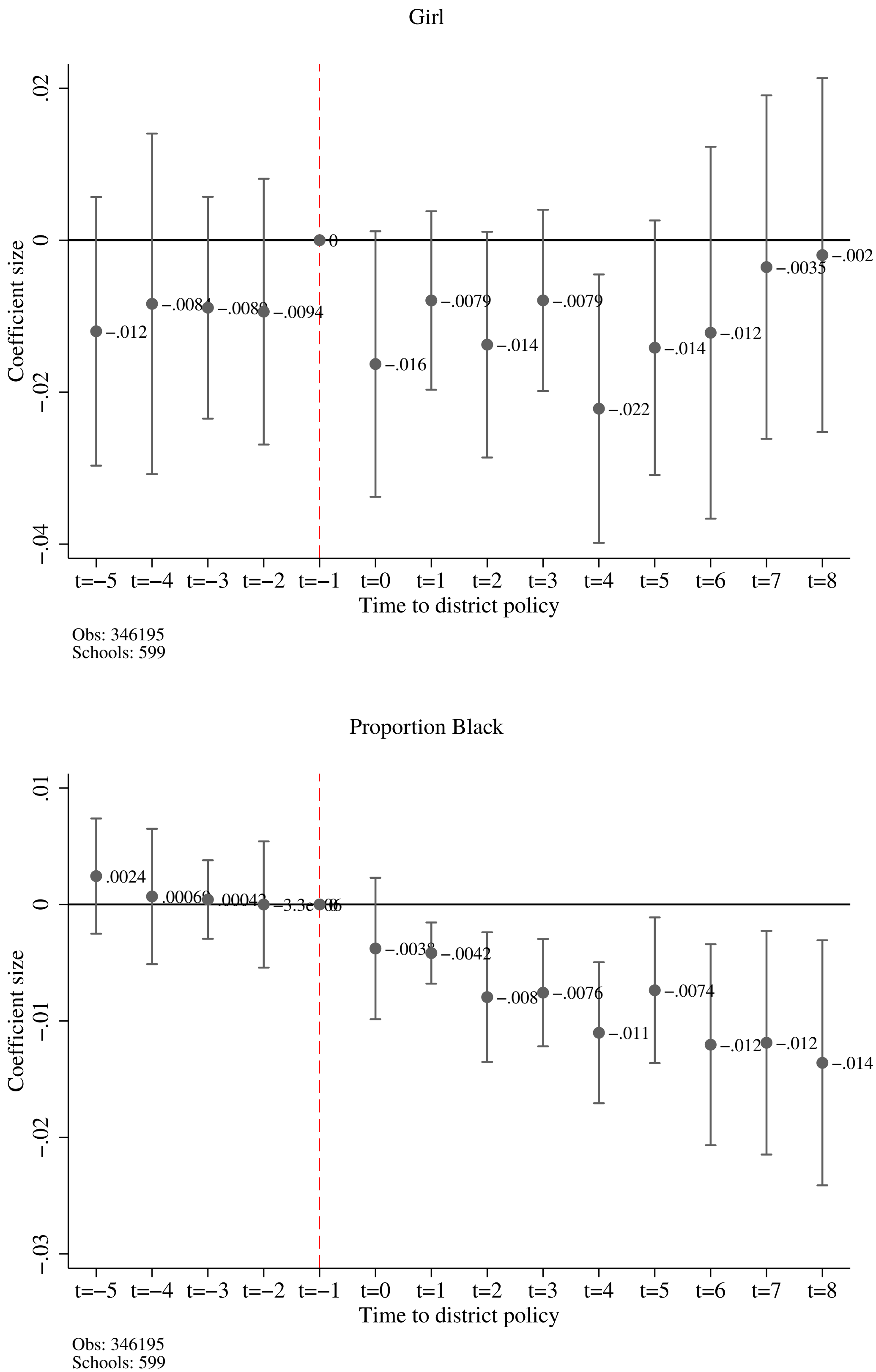
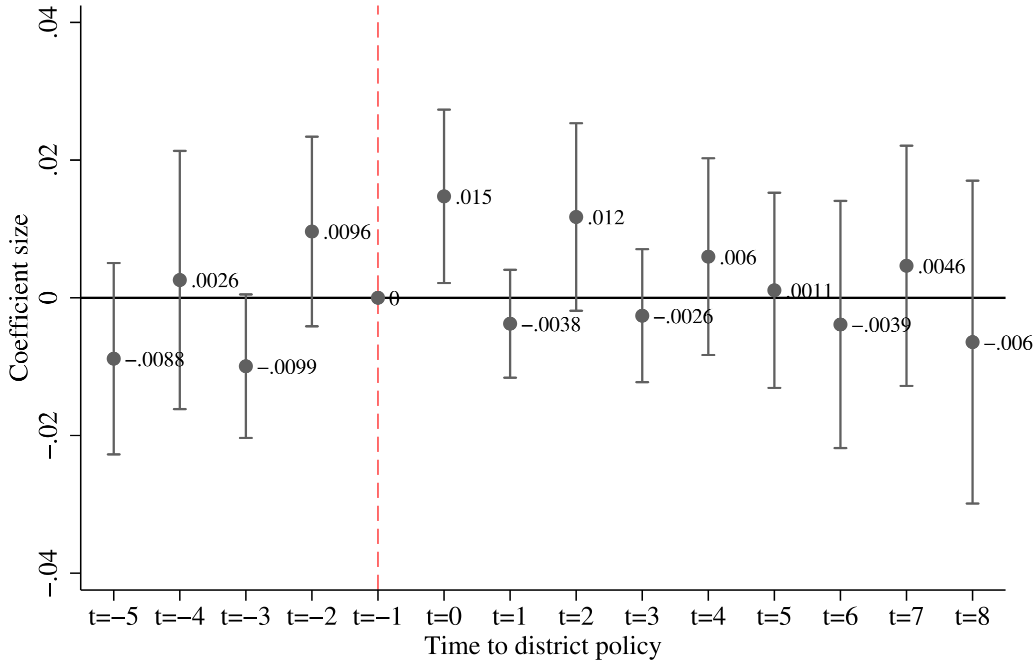


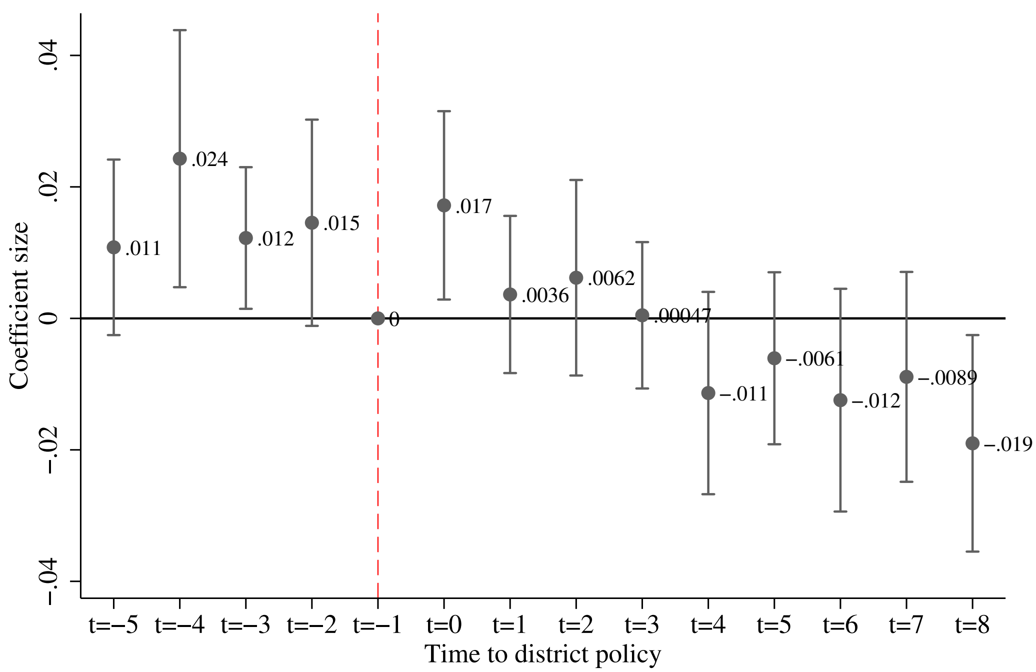
Figure A.11: Mostly C's or worse this past year

9th graders



Obs: 323570
Schools: 590

11th graders



Obs: 286816
Schools: 624

Table A.2: Controlling for School Specific Time Trends

	(1) Any drugs	(2) Coc./crack	(3) Marijuana	(4) Inhalants	(5) Meth	(6) Alcohol	(7) Cigarettes
Grade 9							
Post	-0.001	0.001	0.004	0.004	0.002	0.001	0.003
	(0.006)	(0.003)	(0.004)	(0.003)	(0.002)	(0.006)	(0.003)
Post x	0.000	-0.002	0.002	-0.000	-0.002	0.000	-0.002
Evt Time	(0.004)	(0.002)	(0.003)	(0.002)	(0.001)	(0.003)	(0.003)
Obs	346195	346195	346195	346195	346195	346195	346195
Grade 11							
Post	0.009	-0.000	0.008**	0.001	0.001	0.008	0.004
	(0.006)	(0.003)	(0.004)	(0.003)	(0.002)	(0.006)	(0.004)
Post x	-0.001	-0.001	0.002	-0.000	-0.001	-0.002	-0.004
Evt Time	(0.005)	(0.002)	(0.004)	(0.002)	(0.001)	(0.004)	(0.003)
Obs	302547	302547	302547	302547	302547	302547	302547
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Outcomes are indicators for any use of the substance in past 30 days. Controls include age, gender, race, school level enrollment, demographic composition, FRPL, proportion English learners, and school-specific linear time trends. Standard errors in parentheses, clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Controlling for District Specific Time Trends

	(1) Any drugs	(2) Coc./crack	(3) Marijuana	(4) Inhalants	(5) Meth	(6) Alcohol	(7) Cigarettes
Grade 9							
Post	-0.001	0.001	0.004	0.004	0.002	0.001	0.004
	(0.006)	(0.003)	(0.004)	(0.002)	(0.002)	(0.006)	(0.003)
Post x	0.000	-0.002	0.002	-0.000	-0.002	0.000	-0.002
Evt Time	(0.004)	(0.001)	(0.003)	(0.002)	(0.001)	(0.003)	(0.003)
Obs	346195	346195	346195	346195	346195	346195	346195
Grade 11							
Post	0.009^{*}	0.000	0.008^{*}	0.001	0.001	0.007	0.004
	(0.005)	(0.003)	(0.004)	(0.002)	(0.002)	(0.006)	(0.004)
Post x	-0.001	-0.001	0.001	-0.000	-0.000	-0.001	-0.004
Evt Time	(0.004)	(0.002)	(0.004)	(0.002)	(0.001)	(0.004)	(0.003)
Obs	302547	302547	302547	302547	302547	302547	302547
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

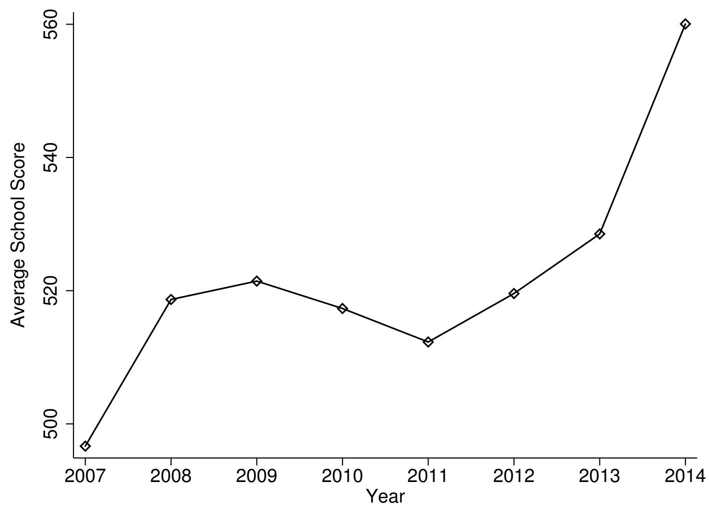
Outcomes are indicators for any use of the substance in past 30 days. Controls include age, gender, race, school level enrollment, demographic composition, FRPL, proportion English learners, and district-specific linear time trends. Standard errors in parentheses, clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

APPENDIX B

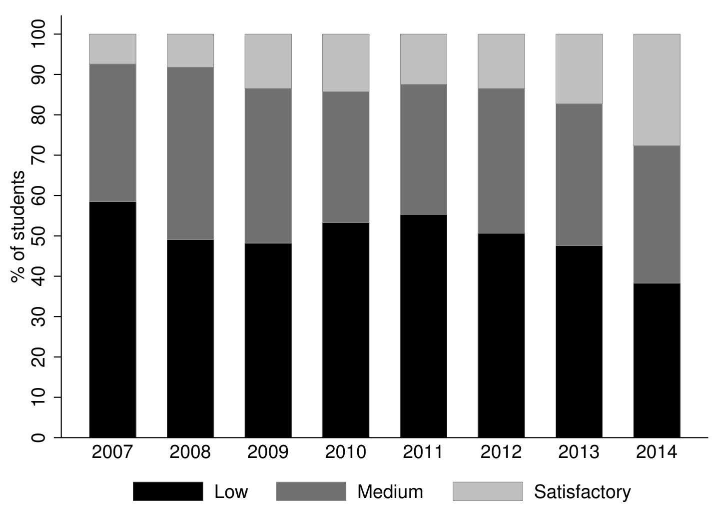
CHAPTER 2 APPENDIX

Figure B.1: Performance of Grade 2 Students in Public Schools on the ECE (2007-2014)

Average Test Scores in Math

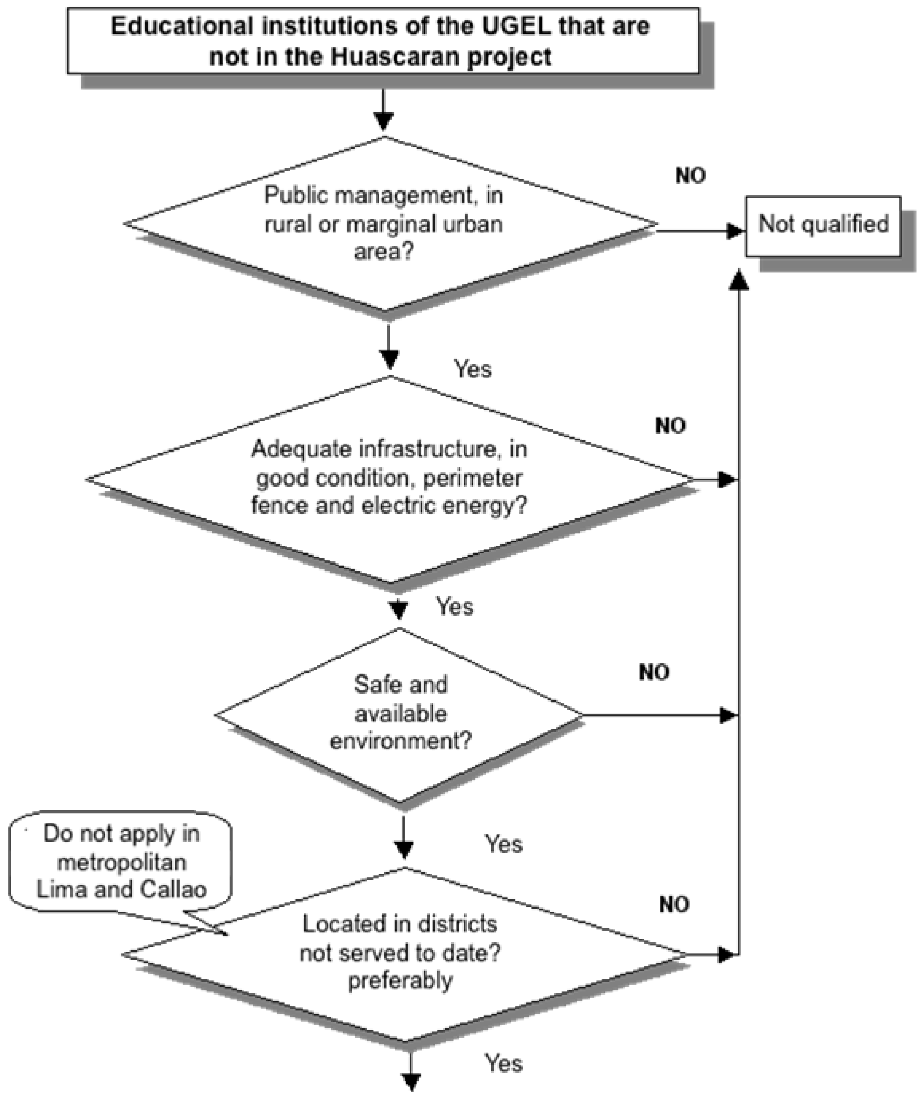


Percentage of Students with Low, Medium, and Satisfactory Performance in Public Schools



Source: Authors' calculations based on the Evaluación Censal de Estudiantes, 2007-2014

Figure B.2: Translated Excerpt of Flow Chart for Prioritization under *Plan Huascarán*



Authors' translation. Original document in Spanish can be found here: `http://www.minedu.gob.pe/normatividad/directivas/Dir083VMGP2003.php`. <Accessed October 4, 2017>

Table B.1: Predictors of Internet Access

	Dependent Variable: School Has Gained Internet Access			
	Including Characteristics Prioritized by *Plan Huascarán*	Adding School FE	Adding a Control for for Fencing (2010 and Later)	Adding Characteristics in School Data Sheets
	(1)	(2)	(3)	(4)
School Has a Computer Room	0.092***	0.033***	0.025***	0.034***
	(0.006)	(0.005)	(0.006)	(0.005)
School Has a Library	0.039***	0.014***	0.019***	0.015***
	(0.005)	(0.005)	(0.007)	(0.005)
School Has Administrative Office(s)	0.014**	0.014***	0.008	0.013***
	(0.006)	(0.005)	(0.006)	(0.005)
Total Enrollment (in 100s of students)	0.033***	0.028***	0.030***	0.027***
	(0.002)	(0.007)	(0.011)	(0.007)
School Is in an Urban Area	0.008			
	(0.010)			
School Has a Full Perimeter Fence			0.014	
			(0.009)	
Number of Computers for Instruction				-0.001***
				(0.000)
Number of Computers for Administration				0.000
				(0.000)
Total Number of Teachers				-0.003***
				(0.001)
Observations	31,368	31,368	22,669	31,368
Number of Schools	5,903	5,903	5,766	5,903
UGEL and District FE	Yes	No	No	No
Year Fixed Effects	Yes	Yes	Yes	Yes
School Fixed Effects	No	Yes	Yes	Yes

The sample includes all public schools that gained internet access between 2007 and 2014 and are observed at least twice. Standard errors are clustered at the school level. Prioritized characteristics include enrollment and facilities (computer room, electricity, library, administrative offices), Local Educational Management Unit (UGEL) fixed effects (to capture UGEL-level quotas), district fixed effects (to capture poverty status) and year fixed effects. Information on the existence of a perimeter fence is only available for 2010 and later. Additional characteristics on school data sheets include number of computers used for instruction, number of computers used for administrative purposes, and number of teachers.

Significance levels denoted by: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table B.2: Summary Statistics (2007 or Earliest Available Year) of Public Primary Schools in ECE, by Year of Internet Access

	Internet before 2007	Internet 2007-2014	No Internet by 2014
School Average Standardized Math Score	0.230	-0.040	-0.270
	(0.620)	(0.910)	(1.160)
School Is in an Urban Area	0.940	0.490	0.110
	(0.240)	(0.500)	(0.320)
One Teacher per Grade	0.940	0.540	0.130
	(0.240)	(0.500)	(0.340)
Total Enrollment (Grades 1-6)	553.7	199.6	62.62
	(347.8)	(233.9)	(69.83)
Enrollment in 2nd Grade	89.73	34.62	12.23
	(58.62)	(39.82)	(12.62)
School Is Connected to	0.870	0.570	0.320
Public Water Network	(0.340)	(0.500)	(0.470)
School Has Library	0.700	0.370	0.250
	(0.460)	(0.480)	(0.440)
School Has Administrative Office(s)	0.800	0.470	0.270
	(0.400)	(0.500)	(0.440)
Number of Teachers	6.140	5.570	5.270
	(7.880)	(7.950)	(7.780)
Number of Classrooms	20.53	8.180	3.360
	(11.60)	(6.830)	(2.920)
Computers in School	17.92	3.200	0.490
	(19.57)	(7.260)	(2.590)
Second Grade Textbooks	345.8	131.2	44.30
	(279.6)	(181.1)	(54.97)
Number of Schools	1359	6527	17738

When 2007 data are not available, we present data from the earliest available year in the sample. Samples include all schools with at least one evaluation in the ECE between 2007 and 2014. Note that the estimation sample includes all schools in column 2 with at least *two* evaluations in the ECE and with data within our analysis window (described in Section 2.3.1). Average school test scores have been standardized to mean zero and standard deviation one across all tested schools within each year. In the Peruvian school system, schools might be *unidocente*(only one teacher in the school teaches all grades), *multigrado* (more than one teacher, but each might teach more than one grade in the same classroom), or *polidocente completo* (there is one teacher per grade in the school). Standard deviations in parentheses.

Table B.3: Effect of Internet Access on Standardized Test Scores

	School Average Standardized Math Score (1)
t = -3	0.001
	(0.020)
t = -2	-0.003
	(0.017)
t = 0	0.042**
	(0.017)
t = 1	0.076***
	(0.018)
t = 2	0.117***
	(0.021)
t = 3	0.166***
	(0.024)
t = 4	0.213***
	(0.028)
t = 5	0.294***
	(0.034)
Joint Test of Significance for All $t < 0$:	0.968
Joint Test of Significance for All $t \geq 0$:	0.000
Year Fixed Effects	Yes
School Fixed Effects	Yes
Time-varying controls	
Observations	31,368
Number of Schools	5,903

The sample includes all public schools that gained internet access between 2007 and 2014 and are observed at least twice. Standard errors are clustered by school. Math scores are standardized to have mean zero and standard deviation of 1 across the universe of schools reporting scores within each calendar year. Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Control variables include enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers, and teachers).
Significance levels denoted by: *** p< 0.01, ** p< 0.05, * p< 0.1.

Figure B.3: Translated Excerpt of Prioritization Directive for *Plan Huascarán*

VI. Specific rules

6.1 The prioritization criteria are as follows:

a. Educational institutions of public management, preferably located in areas with higher poverty, rural or marginal urban areas.
b. Primary, secondary and integrated educational institutions.
c. Establishment of a quota of educational institutions by UGEL (See Annex No. 1).
d. For each level of education (only primary, secondary only and both levels) a percentage will be applied that will allow to set the maximum number of educational institutions for each level in the UGEL and that in total will be equal to the quota set for each UGEL. The percentages are as follows:
    - Primary: 50%
    - Secondary: 30%
    - Integrated: 20%
e. It will benefit educational institutions of larger school populations.
f. Infrastructure of the school premises in good condition and with electricity service.
g. Available environment for the Innovation Classroom and with appropriate security measures to prevent theft.
h. Educational institutions located in districts not served to date, preferably.
i. The Ministry of Education, through the Huascarán Project, will establish the criteria and procedures for attending educational entities that do not have electricity.

Authors' translation. Original and complete directive in Spanish can be found here: `http://www.minedu.gob.pe/normatividad/directivas/Dir083VMGP2003.php`. <Accessed October 4, 2017>

Figure B.4: Translated School Data Sheet for *Plan Huascarán*

ANNEX N ° 2

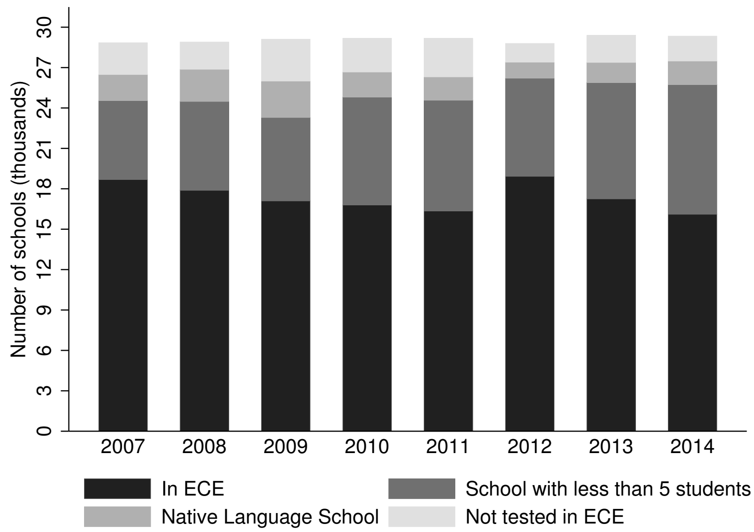
DATA SHEET OF THE EDUCATIONAL INSTITUTION		
Name of Educational Institution		
School Site Code		
Address		
Department		
Province		
District		
Town Center		
Phone		
Principal's name		
Direct intermediate organ		
Geographical area (urban, rural)		
Type of Management (State, Parish, Cooperative, Supervised, etc.)		
Number of computers for school use (only Pentium I or more)		
Number of computers for administrative use (only Pentium I or more)		
Do you have electricity?		
Number of hours of electricity		
Number of students and teachers per level	Students	Teachers
Initial		
Primary		
High school		
Number of students and teachers per shift	Sections	
Morning		
Late		
Night		
Number of sections per shift	Sections	
Morning		
Late		
Night		
Number of sections per level	Students	Teachers
Initial		
Primary		
High school		
Is there home-based telephone in the locality?		
Native language of students		
Distance to the nearest Huascarán Program educational institution		

Source: Authors' translation of Annex No. 2 available in Spanish here:
http://www.minedu.gob.pe/normatividad/directivas/Dir083VMGP2003.php

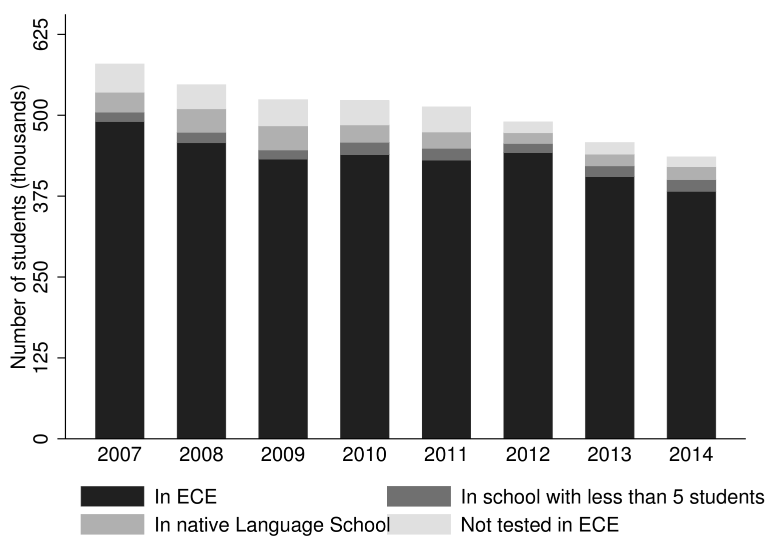
Authors' translation. Original document in Spanish can be found here: `http://www.minedu.gob.pe/normatividad/directivas/Dir083VMGP2003.php`. <Accessed October 4, 2017>

Figure B.5: Schools and Students in ECE, 2007-2014

Primary Schools tested in ECE



Primary Students tested in ECE



Source: Authors' calculations based on the Peruvian *Censo Escolar* (CE) and *Evaluacion Censsal de Estudiantes* (ECE), 2007-2014.
* Note: Some schools both have fewer than five second graders and teach primarily in native languages. For simplicity, the graph includes these under "Fewer than five students."

Table B.4: Summary Statistics for Alternate Estimation Samples

	Baseline Sample (1)	2 Pre & 2 Post Sample (2)	Balanced Sample (3)
School Average Standardized Math Score	0.069	0.076	0.110
	(0.924)	(0.917)	(0.878)
Years of Internet Access	1.119	0.851	0.857
	(1.448)	(1.227)	(1.125)
Number of Students Scheduled to Take the ECE	35.689	33.475	38.410
	(38.955)	(34.180)	(34.447)
School Has a Library	0.446	0.433	0.460
	(0.497)	(0.496)	(0.498)
School Has Administrative Office(s)	0.480	0.458	0.540
	(0.500)	(0.498)	(0.498)
Ratio of Classrooms to Students	0.076	0.075	0.068
	(0.076)	(0.079)	(0.047)
Ratio of Computers to Students	0.192	0.189	0.148
	(0.329)	(0.319)	(0.252)
Ratio of Teachers to Students	0.057	0.056	0.053
	(0.040)	(0.033)	(0.029)
Total School Enrollment	209.530	195.701	224.555
	(227.649)	(197.098)	(199.243)
Observations	31,368	22,321	7,322
Number of schools	5,903	3,670	1,046

The baseline sample includes all public schools that gained internet access between 2007 and 2014 and are observed at least twice. The "2 Pre & 2 Post" sample includes schools observed at least twice prior to and twice after internet access. The balanced sample includes schools that are observed throughout the entire sample period and restricted event window $t = -3$ to $t = 3$. Math scores are standardized to have mean zero and standard deviation of 1 across the universe of schools reporting scores within each calendar year.

Table B.5: Understanding the Role of School-level Compositional Changes: Trend Break Results

	Dependent Variable: School Average Standardized Math Score			
	Full Event Window: $t = -3$ to $t = 5$		Restricted Event Window: $t = -3$ to $t = 3$	
	Baseline Sample	2 Pre & 2 Post Sample	Baseline Sample	Balanced Sample
	(1)	(2)	(3)	(4)
Post-internet Access	0.036*	0.031	0.041**	-0.005
	(0.020)	(0.022)	(0.021)	(0.053)
Post-internet Access	0.047***	0.043***	0.044***	0.061
X Event Time	(0.011)	(0.013)	(0.012)	(0.042)
Event Time	-0.001	0.007	-0.002	0.011
	(0.010)	(0.011)	(0.010)	(0.027
Observations	31,368	22,321	28,392	7,322
Number of Schools	5,903	3,670	5,836	1,046
Year Fixed Effects	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes

Column 1 reproduces the baseline results using all public schools that gained internet access between 2007 and 2014 and are observed at least twice. Column 2 presents the results using the restricted sample of schools observed at least twice prior to and twice after internet access. Column 3 presents the results using the baseline sample for the restricted event window $t = -3$ to $t = 3$. Column 4 presents the results using the sample of schools that are observed throughout the entire sample period and restricted event window. Standard errors are clustered by school. Math scores are standardized to have mean zero and standard deviation of 1 across the universe of schools reporting scores within each calendar year. Post-internet access is a dummy variable for whether a school has gained internet access (i.e. $t >= 0$). Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Event time is years relative to internet access. Control variables include enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers, and teachers).

Significance levels denoted by: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table B.6: Attrition (Number of Observations)

	N	%
All Information Observed (Not Attrited)	31,368	81.78
Attrited	6,989	18.22
Missing ECE Scores		
Second Grade Enrollment less than 5	2,340	6.10
Second Grade Enrollment between 5 and 7	1,022	2.66
Other	1,915	4.99
Missing Census (CE) Information		
Missing Information on Infrastructure	1,467	3.82
Missing Information on Resources	218	0.57
School Permanently Closed	27	0.07

Based on each school's initial year of internet connection and our event study window ($t = -3$ to $t = 5$), we determine all the periods that should be included in our panel dataset. Enrollment is measured from the CE (reported at the beginning of each year). Only schools with five or more second graders by the end of each year are tested in the ECE. Schools that have 5-7 students at the beginning of the year might not have been included in the ECE if they fell below the 5-student threshold by the end of the year. CE information is used to calculate infrastructure (and, importantly, internet access) and school resources (control variables in our regressions).

Table B.7: Effects of Computers in Non-internet Schools

	School Average Standardized Math Score					
	(1)	(2)	(3)	(4)	(5)	(6)
Ratio of Computers to Students	0.031**	0.030*	0.026	0.019	0.016	0.024
	(0.015)	(0.018)	(0.019)	(0.022)	(0.027)	(0.037)
Ratio of Computers to Students		0.022	0.015	0.011	0.023	0.006
1 Period Lag		(0.018)	(0.020)	(0.021)	(0.026)	(0.037)
Ratio of Computers to Students			0.031	0.037**	0.028	0.001
2 Period Lag			(0.023)	(0.019)	(0.021)	(0.032)
Ratio of Computers to Students				-0.021	-0.034	-0.017
3 Period Lag				(0.023)	(0.024)	(0.042)
Ratio of Computers to Students					-0.036	-0.051
4 Period Lag					(0.036)	(0.060)
Ratio of Computers to Students						-0.049
5 Period Lag						(0.056)
Observations	80,288	65,596	51,450	39,351	29,727	21,852
Number of Schools	17,464	16,198	14,903	12,732	10,509	9,516
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes	Yes	Yes

The sample includes all schools that report ECE scores during 2007-2014 and that had not gained internet access by 2014. Math scores are standardized to have mean zero and standard deviation of 1 across the universe of schools reporting scores within each calendar year. "Ratio of computers to students" is the number of instructional computers per student. Control variables in all columns include school facilities (piped water, library, administrative offices) and resources per student (classrooms and teachers). Standard errors are clustered by school.
Significance levels denoted by: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table B.8: Effects of Concurrent Computer Investments: Back of the Envelope Calculations

Time Relative to Internet Access (1)	Tot. Predicted Rise in Computers (per Student) (2)	Tot. Dynamic Effect of Computers on Scores (3)	Est. Effect of Internet on Scores (4)	% of Tot. Internet Effect Explained by Computers (5)
$t = 0$	0.045	0.001	0.042	3.32%
$t = 1$	0.079	0.003	0.076	4.58%
$t = 2$	0.113	0.007	0.117	5.97%
$t = 3$	0.147	0.009	0.166	5.61%
$t = 4$	0.181	0.010	0.213	4.79%
$t = 5$	0.215	0.009	0.294	3.16%

Column 2 gives the total predicted rise in computers in each period using the parameter estimates from the trend break regression of computers per student on Post-internet access, event time, and the interaction between the two (from Table 2.3). Column 3 calculates the total dynamic effect of computer as of time t using (i) the largest parameter values from regressing ECE scores on computers per students (and lags) using the sample of non-internet connected schools (from Appendix Table B.7), without regard to significance level and (ii) the total predicted rise in computers from column (2). Column 4 displays estimated effects of internet access on test scores from the baseline event study specification (Appendix Table B.3). Column 5 expresses the total effect of computers as a percent of the total effect of internet access (column 3 divided by column 4).

Table B.9: Internet Access and Computer Teachers: Trend Break Results

	Dependent Variable: Presence of a Teacher with Formal Computer Training
Post-internet Access	0.027***
	(0.007)
Post-internet Access X Event Time	0.015***
	(0.004)
Event Time	0.000
	(0.003)
Observations	31,368
Number of Schools	5,903
R-squared	0.035
Year Fixed Effects	Yes
School Fixed Effects	Yes
Time-varying controls	Yes

The sample includes all public schools that gained internet access between 2007 and 2014 and are observed at least twice. Standard errors are clustered by school. Math scores are standardized to have mean zero and standard deviation of 1 across the universe of schools reporting scores within each calendar year. Post-internet access is a dummy variable for whether a school has gained internet access (i.e. $t >= 0$). Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Event time is years relative to internet access. Control variables include enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers, and teachers).
Significance levels denoted by: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table B.10: Heterogeneity in the Impact of Internet Access across Schools by Student to Teacher Ratios and Teacher Qualifications: Trend Break Results

	Dependent Variable: School Average Standardized Math Score			
	High STR (1)	Low STR (2)	High Teacher Qualifications (3)	Low Teacher Qualifications (4)
Post-internet Access	0.025	0.050	0.036	0.043
	(0.027)	(0.031)	(0.029)	(0.030)
Post-internet Access × Event Time	0.046***	0.009	0.063***	0.006
	(0.015)	(0.020)	(0.016)	(0.018)
Event Time	0.012	0.004	-0.002	0.006
	(0.013)	(0.015)	(0.014)	(0.014)
	p-value for test that High STR = Low STR		p-value for test that High Qual. = Low Qual.	
...for Post-internet Access	0.552		0.866	
...for Post-internet Access × Event Time	0.139		0.022	
Observations	14,307	12,834	13,926	13,215
Number of Schools	2,458	2,457	2,458	2,457
Year Fixed Effects	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes

High and Low STR schools are defined based on each school's pre-internet average ratio of grade 2 students to total teachers in the school (STR) over the sample period relative to the median of all schools' sample averages. High and Low Teacher Quality schools are defined based on each school's pre-internet average number of teachers with a pedagogical or higher education degree per student over the sample period relative to the median of all schools' sample averages. Note that degree information is not available for all schools. Samples includes all public schools that gained internet access between 2007 and 2014 and are observed at least twice. Standard errors are clustered by school. Math scores are standardized to have mean zero and standard deviation of 1 across the universe of schools reporting scores within each calendar year. Post-internet access is a dummy variable for whether a school has gained internet access (i.e. $t >= 0$). Note that due to the timing of the *Censo Escolar* relative to the ECE exam, some schools receive internet access in $t = 0$ while some receive it in $t = 1$. For more details, see Section 2.3.1. Event time is years relative to internet access. Control variables include enrollment, number of second grade students scheduled to take the ECE, facilities (computer room, library, administrative offices), and resources per student (classrooms, computers, and teachers).
Significance levels denoted by: *** p< 0.01, ** p< 0.05, * p< 0.1.

APPENDIX C

CHAPTER 3 APPENDIX

$$Y_{rit} = \beta_1 \text{AB 420}_t + \phi \text{Year}_t + \gamma X_{it} + \alpha_i + \varepsilon_{it} \tag{C.1}$$

Table C.1: Disproportionality in Suspensions Over Time by Initial Use of "Willful Defiance"

	(1) 1st Quantile	(2) 2nd Quantile
Schools' initial willful defiance proportion of suspensions	.064	.374
Black: Difference between proportion of suspensions and proportion of enrollment		
AB 420 onwards	-0.004	0.001
	(0.010)	(0.008)
Year	0.001	-0.000
	(0.003)	(0.003)
Initial Black proportion of suspensions	.165	.152
Initial Black proportion of enrollment	.069	.068
Observations	5657	6443
White: Difference between proportion of suspensions and proportion of enrollment		
AB 420 onwards	0.012	-0.000
	(0.012)	(0.010)
Year	0.001	0.003
	(0.003)	(0.003)
Initial White proportion of suspensions	.256	.291
Initial White proportion of enrollment	.245	.273
Observations	5906	6635
Hispanic: Difference between proportion of suspensions and proportion of enrollment		
AB 420 onwards	0.006	-0.008
	(0.011)	(0.010)
Year	-0.007**	-0.002
	(0.003)	(0.003)
Initial Hispanic proportion of suspensions	.468	.442
Initial Hispanic proportion of enrollment	.526	.503
Observations	5992	6679
School FE	Yes	Yes
Controls	Yes	Yes

Standard errors in parentheses, clustered at the district level. Controls include school level enrollment, proportion FRPL eligible, and proportion English learners. Outcomes are the differences between each race category's proportion of total suspensions and its proportion of total enrollment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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