It Takes a Village: The Impact of the New York City Community School Initiative on Student Behavior

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Abstract

School-based reforms have the potential to change student behavioral outcomes at scale. We provide causal evidence of the impact of the NYC Community School initiative—a multi-armed, multi-agency school-based reform—on student behavior and crime outcomes. We exploit the staggered roll out of the program, finding large improvements in student behavioral outcomes for elementary school students, particularly for bullying, but no discernible effect for older children. The results of a causal mediation analysis indicate that the lower bullying incidence in Community Schools plays a first-order role in improving test scores in these schools, notably for English.

JEL codes: H75, I21.

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1 Introduction

School-based interventions can be a cost-effective strategy to implement child-based program delivery at scale. Studies have shown that these interventions can positively impact a wide range of cognitive outcomes, such as improved academic performance and enhanced problem-solving skills. Additionally, they contribute to non-cognitive outcomes, including better social skills, increased emotional resilience, and improved overall well-being (Heckman and Kautz, 2012). These benefits are observed not only in the short term but also extend into the long run, indicating the lasting impact of such interventions (Galasso and Wagstaff, 2019; García et al., 2020; Deming, 2009). Research consistently shows that the timing of these interventions is crucial, with earlier interventions generally proving to be more effective (Currie, 2001).

In this paper, we examine the effect of the New York City Community School (NYC-CS) initiative on student behavior outcomes, such as bullying, and crime outcomes. Launched in 2014, the NYC-CS program has grown substantially, starting with an initial set of 45 schools in 2014/15 to covering 421 schools in 2022/23. The initiative takes a multi-armed, multi-agency approach to educational interventions. At the core of the Community School initiative are four key pillars: 1) integrated student supports, such as mental and physical health care; 2) expanded and enriched learning time and opportunities; 3) family and community engagement; and 4) collaborative leadership and practices. The NYC-CS initiative serves as a useful tool in studying the importance of program timing across the student life-cycle, as (i) the program is implemented in schools across all grade levels and (ii) the program contains the same core pillars irrespective of the grade level of the school in which it is implemented.

Using administrative data from the NYC Violent and Disruptive Incident Reporting (VADIR) between 2009 and 2017 and a difference-in-differences (DD) research design, we evaluate the behavioral impact of the NYC-CS Initiative for the first three waves of Community Schools. A recent literature on DD models with staggered treatment timing has highlighted the importance of properly accounting for heterogeneity in effects across treatment cohorts (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021a; Borusyak et al., 2024). Since our treatment is also staggered, we use various alternative estimators to ensure that our estimates are robust to the presence of this heterogeneity. In addition, we (i) provide support for the key identifying assumption of our design – the parallel trends assumption – and (ii) implement the Goodman-Bacon (2021a) decomposition to highlight why the various alternative estimators provide such similar results to our baseline two-way fixed effects (TWFE) model: clean comparisons (Community Schools versus untreated schools) comprise 98.9% of the weight used to form our baseline DD estimates.

We find that, when averaging across all grade levels, total crime and disruptive behavior incidents decrease substantially after a school becomes a Community School.[1] Our total incidents measure drops by 25%, driven primarily by large declines in bullying and disruptive behavior incidents. We document that the impact of Community Schools is extremely heterogeneous across grade levels. As has been found in many other domains, the timing of child-based interventions is crucial. We observe that the behavioral impacts of the Community School program are large and statistically significant for students of elementary school age, with no discernible impact on students in middle and high schools. In elementary schools that become a Community School, we observe a decrease of 62% in total crime and behavior incidents. Bullying incidents and disruptive behavior are the key drivers of this decline. Given the literature on the lasting impacts of being bullied (Powers and Bierman, 2013), and a large body of work documenting both short- and long-run consequences of disruptive peers on learning outcomes (Figlio, 2007; Carrell and Hoekstra, 2010; Carrell et al., 2018; Sarzosa, 2021), our findings suggest that the NYC-CS initiative will lead to lasting, positive benefits on the students in Community Schools.

Our next key finding emerges from our heterogeneity analysis, where we re-estimate our baseline DD model on various sub-samples of schools, splitting schools along key margins of the composition of their student body, such as racial and ethnic composition, English language proficiency, and poverty. We document a highly stable treatment effect of Community School status across the various student composition sub-samples. This finding (i) indicates that the observed decreases in crime and behavior incidents are unlikely demographic-specific and (ii) is an encouraging finding for school districts

[1] Given the data we have available, we are unable to separately identify treatment effects for the four pillars of the program – the treatment effect we estimate is thus a composite effect of the program as a whole.

in other parts of the country that wish to emulate the NYC-CS initiative – our finding of broad homogeneity of the treatment effect across sub-groups suggests the program is likely portable, even to settings with very different student composition to NYC schools.

We next move to examine parental responses to the NYC-CS initiative, analyzing both their stated and revealed preferences. This dual approach provides valuable insights, offering a comprehensive understanding of how parents react to the program. We make use of two independent data sources – (i) administrative data on student demographics at the school-year level and (ii) the responses of parents to an annual school survey – to provide evidence on how parents respond to the initiative.

The revealed preference evidence comes in the form of changing enrollment patterns, and the demographic nature of such changes along key dimensions of student intake. We find substantial parental sorting *out of* Community Schools – our snapshot measure of student enrollment declines by 9%. We also provide evidence that this parental response is not uniform across parental demographics. As a consequence, various aspects of student composition change post-policy including changes to the racial composition of the student body, a 7.1% rise in students with disabilities, and a 6.5% increase in students living in poverty. The combined effects of these compositional changes means that in absence of the NYC-CS initiative having any direct treatment effect, we would have expected a 6.3% rise in crime rates. We base this statement on the DD estimate for our "ex-ante crime risk score."[2]

The evidence on stated preferences relies on parental responses to an annual survey of schools in NYC. This data includes information on parental satisfaction with several key aspects of the school, including safety, communication, and various dimensions of educational quality. Our findings indicate that parents are not satisfied with Community Schools. Satisfaction with various aspects of the school and the education provided there fall, and most surprisingly, parents feel their children are less safe at school. We posit that the reason for this is that parents base their expectations of school safety not on crime

[2]We we construct an ex-ante crime risk score in order to conceptualize the ramifications of these demographic changes for crime outcomes. Using data from the pre-period of 2009-2013, we create a predicted crime risk score, based on student demographic variables. We make the assumption that the factor loadings on student demographics are stables across the 2009-2017 sample period. The risk score measure effectively enables us to reduce the dimensionality of student compositional changes to understand how changes in the student body may impact our estimated treatment effects.

outcomes in schools (which may be hard to observe contemporaneously), but rather on the easily observable demographic composition of students. This finding highlights the gap between true and perceived treatment effects.[3] The finding also emphasizes the importance of our constructed ex-ante crime score measure, which indicates that the demographic changes we observe are associated with an increased prevalence of crime and behavioral issues.[4]

We note that our estimates the crime- and behavioral incident-reducing impact of the NYC-CS Initiative serve as lower bounds of the true impact for at least two distinct reasons. First, a mechanical reporting effect: due to the program, students are around one another, under the supervision of teaching staff, for more hours of the school day. This means more time for interpersonal incidents to occur, and thus a mechanical increase in reported behavioral incidents. Second, based on the parental sorting changes that occur in response to the program, our ex-ante crime risk score measure increases in Community Schools. This means in absence of any crime-reducing benefit of the NYC-CS Initiative, based on demographic changes alone, crime would likely have risen in these schools. Both of these channels stack the deck against us finding a negative effect of Community Schools on behavioral incidents.

Our final key finding is based on the results of a causal mediation analysis. Given that (i) existing work documents that the NYC-CS initiative led to improved test scores, (ii) the large literature documenting the negative consequences of bullying on educational performance, and (iii) our findings of large reductions of bullying incidence in Community Schools, a natural question topic to consider is the mediating role that the Community-School-induced reduction in bullying plays in the total effect of the NYC-CS initiative on test scores. To make progress on this topic, we implement a causal mediation approach, which enables us to separate the direct effect of Community Schools on test scores from the indirect effect, as mediated through reductions in bullying in Community Schools. Using a shift-share instrumental variable and a causal mediation analysis strategy (Dippel et al., 2020; Huber, 2019; Celli, 2022), we document that the indirect bullying-reduction effect

[3]Furthermore, this discrepancy in parental preferences has also been noted in other domains (Adda et al., 2014).

[4]This explanation also applies to the academic quality of schools, where we observe that the NYC-CS initiative led to an improvement in test scores, but the resulting compositional changes are associated with a decrease in test scores.

of Community Schools accounts for 21% of the increase in Math scores, and 36% of the improvement in English scores.[5] The larger impact on English scores is driven in part by the greater penalty that bullying imposes on English scores in our sample.

Our work makes key contributions to two distinct literatures. First, by documenting the impact of Community Schools on crime and behavioral incidents, our work contributes to the literature studying the implication of school-based programs on student outcomes. The existing literature on Community Schools primarily focuses on educational outcomes (Covelli et al., 2022; Johnston et al., 2020).[6] A comprehensive report by the RAND corporation evaluates the NYC-CS Initiative and finds that it significantly improved academic outcomes, including attendance, math achievement, and graduation rates (Johnston et al., 2020). In contrast to Johnston et al. (2020), which find no significant effects on reading achievement and a positive effect on math achievement only in the third year of NY-CS implementation, Covelli et al. (2022) identify substantial increases in both math and English test scores by employing a different empirical strategy and analyzing longer-term data. While Johnston et al. (2020) consider a wide range of outcomes, including disciplinary incidents, our study complements their work in several ways. First, although Johnston et al. (2020) report a decrease in disciplinary incidents, they do not investigate into the specifics of these incidents, leaving it unclear whether the reduction pertains to bullying, other types of behavior, or criminal incidents. Second, we provide a more comprehensive examination of the community school initiative by not only considering its impact on children but also examining parental responses. Finally, we link the decrease in behavioral outcomes to test scores using a mediation analysis framework.

Second, by highlighting (i) the importance of the NYC-CS initiative on reducing bullying and (ii) the impact of bullying on test scores within a causal mediation framework, our work contributes to a body of work documenting the consequences of bullying, both in the short- and long-run. Prior studies have shown that the effects of bullying on educational achievement are comparable in magnitude to class size effects, and they also have negative

[5] Causal mediation analysis identifies and quantifies the mechanisms through which the NYC-CS initiative affects test scores by examining mediators (like student behavior and crime outcomes).

[6] There are other studies that study different variations of the community school approach implemented in different cities and countries. Figlio (2015) evaluates the Chicago's Communities In Schools partnership program, Dobbie and Fryer Jr (2011) studies the NYC Harlem Children's Zone, and Heers et al. (2014) studies Community Schools in the Netherlands.

long-term consequences on wages (Brown and Taylor, 2008; Carrell and Hoekstra, 2010; Carrell et al., 2018). Our causal mediation analysis results point to the importance of the indirect effect of Community Schools on test scores, particularly for English, as mediated through reduced incidence of bullying. This indirect effect is overlooked without the use of mediation analysis methods.

The rest of the paper is organized into the following parts. Section 2 provides some background of the Community School initiative in New York City and describes the data we use in this work. Section 3 outlines the empirical strategy and identification assumptions. Section 4 reports the main results for crime and behavioral incidents. Section 5 explores parental response to the Community School initiative while Section 6 quantifies the extent to which the reduction in bullying plays in the improvement of test scores. Section 7 concludes.

2 Institutional Setting and Data

2.1 Community Schools

The NYC-CS initiative is a multi-armed, multi-agency education intervention, providing the structure and support to attend to student's academic, medical, social, and mental health needs (Blank et al., 2012). Community Schools are borne from partnerships between the schools, community-based organizations (CBO), and the school district itself (Dryfoos, 2005). Community School approach has been adopted in both large school districts, such as New York City and Baltimore, and smaller urban and suburban communities, such as Buffalo, New York (Valli et al., 2016).

Usually, the CBO provides on-site coordinators to work with the school's leadership team to implement practices and structures that is developed based on the community needs. Although these practices and structures vary from community school to community school, there are four main features that are common across all Community Schools (Oakes et al., 2017). First, Community School provide wraparound specialized interventions and resources that are aimed to support student's academic, medical, social, and mental health needs.[7] Second, Community Schools provide extended learning time and enriching opportunities through academic and extracurricular learning.[8] Third, family and community are actively engaged in student's learning experiences.[9] Finally, Community Schools are multi-level collaborative partnership among the school administration, community non-profit organization, and the local government.

In 2014, former New York City Mayor Bill de Blasio launched the Community School initiative as part of his broader vision of a "whole child" approach to student learning and agenda to achieve equity across all New York City public schools (Office of the Mayor, 2014). By the school year 2022-2023, the initiative was the largest in the country with 421 public schools being Community Schools.

The initiative created and implemented practices and structures–such as health and wellness services, extended learning time, and deepened family engagement–aimed to improve students' attendance, academic performance, and social-emotional well-being (Johnston et al., 2020). A comprehensive impact study conducted by the RAND Corporation to evaluate the first three years of implementation of Community School initiative found that the NYC Community School improved student attendance, academic achievement, test scores, on-time grade progression, and high school graduation rates (Johnston et al., 2020).

2.2 Data

We use data from three different sources. First, we use Violent and Disruptive Incident Reporting (VADIR) data that provides information regarding school safety in NYC. Each school in New York State is required to submit annual counts of all violent and disrup-

[7]There is also a large body of work that studies the effect of various, predominantly implemented early in life, programs and interventions supporting students academic success (see Duncan and Magnuson (2013) and Duncan et al. (2023) for comprehensive reviews). For a review on the effect of spending on education in general, refer to Jackson and Mackevicius (2024).

[8]Prior studies have linked after-school programs with student academic performance (Drange and Sandsør, 2024) and bullying (Zimmer et al., 2010), and extra curriculum activities with student achievement (Lipscomb, 2007) and risky behaviours (Crispin et al., 2017). On the other hand, lengthening the school day has been associated with increases in academic achievement (Bellei, 2009).

[9]Some researchers have linked family engagement to increased student academic performance and motivation and decreased disciplinary infractions (Avvisati et al., 2014; Hill and Tyson, 2009; Kraft and Rogers, 2015).

tive incidents that occur on school grounds.[10] These incidents include a wide range of violent and disruptive incidents, such as serious assaults, minor altercations, bullying, harassment, and the possession of drugs or alcohol. We classify each VADIR incident into seven categories: violent crimes, property crimes, misdemeanor crimes, weapon possession, drug/alcohol possession/sale, bullying, and disruptive behaviors.[11]

For our main analysis, We use the VADIR data that covers academic year 2009-2010 to academic year 2016-2017. While VADIR data is available until 2019, the reporting system and the way incidents were classified and reported changed significantly on July 1, 2017. In particular, NYC Safe Schools Task Force provided a new set of definitions of incident categories, eliminated categories, such as robbery and burglary, and reduced the incident categories from twenty to nine.[12] This led to not only a drastic drop in incidents reported as seen on Figure A2, but also to confusion among schools how to classify incidents under the new categories.[13]

Second, we gathered a list of community schools in NYC from the academic years 2014-15 to 2016-17 from the New York Community Schools website.[14] We dropped charter and junior-senior high (grades 6-12) schools because very few of these schools have been Community Schools. We obtain school demographic data from Demographic Snapshots reports conducted by the NYC Department of Education (DOE). This data contains the gender, racial, and ethnic composition of the student body in each school as well as information on the percent of students who have disabilities, are English language learners, or are eligible for free or reduced lunch. We also obtained per-student school total expenditures from the School-Level Master File (SCHMA), a publicly-available dataset compiled by the Research Alliance for New York City Schools at New York University. Finally, we use test scores for students in grades 3,4, and 5 from the DOE English Language

[10] Federal law requires that each state determines which of their public schools are persistently dangerous. New York State bases this determination on the VADIR data. Each school in New York State is required to submit annual VADIR report that includes counts of all violent and disruptive incidents occurring on school property or on school buses. For more information, please refer to https://www.p12.nysed.gov/sss/ssae/schoolsafety/vadir.

[11] Please refer to Table A1 for a complete list of incidents included in each category.

[12] See https://www.regents.nysed.gov/sites/regents/files/916p12d2.pdf.

[13] In fact, another revision to the categories was implemented in 2021 to ameliorate some of the reporting struggles of the schools stemming from the 2017 change of VADIR. For more information about this most recent change, please refer to https://www.p12.nysed.gov/sss/documents/SSEC21-22memoFinal7.22.21.pdf.

[14] Figure A1 presents the growth of the Community School initiative over time.

Arts and Math State Tests data series.

We present summary statistics of our main estimation sample in Table 1. There are 100 Community Schools, roughly equally distributed across elementary, middle, and high schools. We observe that CS are different in terms of observable school demographics than non-Community School in that they tend to have a larger enrollment of students that are minority (Black and Hispanic), with disabilities and poor than non-Community Schools. In addition, Community Schools report, on average, more crime and behavior incidents per 1,000 students than non-Community Schools.

3 Empirical Approach

We exploit the staggered roll-out of Community Schools across NYC to estimate the causal effect of these schools on student outcomes. To estimate the causal effect of Community Schools on student crime and behavior outcomes, we use the following DD specification:

$$Y_{st} = \beta\, CS_{st} + \theta_s + \delta_t + \varepsilon_{it} \tag{1}$$

where Y_{st} is the crime and behavior outcome at school s during school year t. The crime outcomes include the number of violent, weapon possession, property, drug or alcohol related, and misdemeanor crime incidents reported while the behavior outcomes include bullying and disruptive behaviour incidents.[15] CS_{st} is a binary treatment variable equal to one if school s is a Community School during school year t. Our coefficient of interest, β, estimates the effect of Community Schools by comparing student behavior and crime outcomes between public schools that become Community Schools and those that do not, before and after treatment. β is the Average Treatment Effect on the Treated (ATT) of Community School status.

We include school fixed effects (FE), θ_s, to absorb time-invariant characteristics that could be correlated with Community School status. These school fixed effects are an

[15]In Table A1, we outline the incidents included in each outcome. Additionally, you can find detailed definitions for each incident category at https://www.p12.nysed.gov/sss/ssae/schoolsafety/vadir/glossary08aaug.html.

Table 1: Characteristics of Schools, by School Grades and CS Status

	(1)	(2)	(3)	(4)	(5)	(6)
	Elementary Schools		**Middle Schools**		**Senior High Schools**	
	Non-CS	**CS**	**Non-CS**	**CS**	**Non-CS**	**CS**
Number of Schools	707	35	192	35	277	40
Enrollment	652	549	674	386	755	819
	(311)	(288)	(473)	(182)	(897)	(942)
Schools Demographics						
% Female	48.6	48.0	49.0	47.0	49.8	46.3
	(4.2)	(2.5)	(5.2)	(4.5)	(13.5)	(9.3)
% Asian	14.1	3.0	14.0	3.7	11.1	4.9
	(19.4)	(5.3)	(18.2)	(6.4)	(15.7)	(8.0)
% Black	27.2	48.6	29.0	37.1	36.1	36.5
	(28.1)	(24.2)	(27.3)	(23.3)	(25.3)	(25.1)
% Hispanic	40.1	44.5	42.6	55.1	42.3	55.0
	(26.5)	(21.4)	(26.9)	(24.6)	(23.1)	(25.8)
% Other	1.9	1.2	1.0	0.9	1.5	1.2
	(2.4)	(1.3)	(1.6)	(1.5)	(1.8)	(1.2)
% Students With	19.2	22.6	19.8	25.1	17.3	19.7
Disabilities	(11.9)	(5.1)	(8.8)	(5.3)	(15.6)	(7.3)
% English	14.8	14.4	13.6	18.3	13.7	19.4
Language Learners	(11.8)	(11.8)	(14.1)	(9.9)	(21.4)	(18.6)
% Poverty	75.8	92.2	77.7	88.4	75.6	81.9
	(23.4)	(7.8)	(20.1)	(8.7)	(16.3)	(10.9)
Total Expenditure	20,818	22,098	19,128	21,127	19,524	18,876
Per Pupil	(10,424)	(3,888)	(6,868)	(2,935)	(9,451)	(3,455)
Crime and Behavioral Outcomes (per 1,000 Students)						
Total Crime	33.7	70.8	92.6	114.1	73.1	109.4
	(40.0)	(62.2)	(88.2)	(71.0)	(75.5)	(89.4)
Violent Crime	10.6	19.6	14.2	21.9	6.8	9.2
	(12.4)	(16.2)	(13.5)	(17.6)	(8.5)	(8.3)
Weapon	1.4	2.6	4.1	5.5	5.0	7.8
Possession	(2.1)	(2.9)	(4.7)	(4.8)	(6.2)	(8.1)
Property Crime	0.6	1.4	2.6	3.0	2.1	2.6
	(1.3)	(2.2)	(3.7)	(3.5)	(3.0)	(3.3)
Drug Crime	0.3	0.7	1.9	2.9	4.2	6.1
	(1.0)	(1.4)	(2.8)	(3.8)	(5.5)	(7.2)
Misdemeanor	0.6	1.8	2.2	2.5	1.7	2.3
Crime	(1.6)	(2.9)	(3.4)	(3.6)	(3.4)	(3.4)
Bullying	16.3	36.0	45.2	58.3	30.2	43.6
	(24.1)	(39.0)	(45.0)	(42.4)	(33.1)	(32.9)
Disruptive	4.0	8.7	22.4	20.0	23.2	37.9
Behavior	(9.4)	(14.0)	(39.4)	(23.3)	(36.5)	(56.6)

Notes: The table present means of key dimensions of student intake by school grade over the core estimation period of school years 2009/10-2016/17, with standard deviations in parentheses. CS =1 for our core community school sample, and 0 otherwise.

important component of our research design, as we intentionally exclude school-level demographic controls – these variables may change in response to the treatment implementation, thus rendering these as bad controls. The evidence that we provide in Section 5.1.2 validates this decision. The school FEs thus absorb key aspects of student intake, as well as dimensions of teacher composition and school leadership that remain fixed over our evaluation period. A corollary of this decision to exclude time-varying, potentially bad controls is that our treatment effect β should be interpreted as the total effect of community school status. δ_t are year fixed effects that capture capture time-varying city-wide changes in student outcomes. Finally, we cluster standard errors ε_{it} at the school level.

Recent developments in econometrics have shown that the standard TWFE design may be biased in staggered designs, such as the roll-out of the Community School initiative studies in our paper. A causal interpretation in the standard setting fails if treatment effects vary across cohorts (groups of schools that became Community Schools in the same year) or across years. We address this issue directly in Section 4.2, providing strong evidence in support of the TWFE design being valid in our setting.

We supplement our DD approach with an event study specification. This specification enables us to (i) gauge the dynamic impacts of Community School status and (ii) provides additional evidence on the validity of our parallel trends assumption. The event study specification takes the form:

$$Y_{st} = \sum_{\substack{e=-7, \\ e\neq -1}}^{2} \gamma_e (CS_s \times EY_e) + \theta_s + \delta_t + \varepsilon_{it} \tag{2}$$

where Y_{st} is the crime and behavior outcome at school s during school year t and CS_s is a dummy variable equal to one if school s has ever been a Community School. The event-year dummies EY_e represent 7 years before and 3 years after a school is converted to a Community School. The school and year FEs, respectively θ_s and δ_t play the same role as described when outlining our DD design. We continue to cluster the standard errors ε_{it} at the school level.

3.1 Support for the Parallel Trends Assumption

The key identifying assumption we require in order to be able to estimate the ATT of Community School status is the parallel trends assumption (PTA). We provide a battery of evidence in support for the PTA in our setting. First, we present the raw trends in Appendix Figure B1. The graphs themselves (i) allow one to make a visual inspection of the pre-trends for Community Schools and their control schools, and (ii) present the p-value from a test of equality of trend coefficients. In all cases we cannot reject the null of equality of trends.

Second, we implement a placebo analysis, where we lag our Community School status variable by three years. If there were differential pre-trends across treated and control schools, we would pick up significant placebo treatment effects in this setting. The placebo treatment assignment works as follows: schools first treated in 2014 are allocated a placebo treatment of 2011, those treated in 2015 are allocated a placebo treatment of 2012, and those treated in 2016 are allocated a placebo treatment of 2013. We then consider the placebo sample period of 2006-2015, and re-estimate Equation (1). We present the resulting DD estimates in Appendix Table B1. We find no evidence of a placebo treatment effect for any elementary school outcomes.

Third, we present a test of pre-trends following Borusyak et al. (2024) at the base of each set of grade-level results in table 2. We use a pre-trend testing period of 4 years pre-policy for this approach, following the advice of Borusyak et al. (2024) to use a constrained time period for pre-trend testing. For all elementary school outcomes, we cannot reject the null of no pre-trends.

Finally, we can inspect the event study coefficient estimates, $\hat{\gamma}_e$ for the event times $e \in (-7, -2)$. We present the event study estimates in Figure 3 below. We do not detect any meaningful pre-trends in any of our core elementary school outcomes. Based on this collection of graphical and statistical evidence, we conclude that there are no differential pre-trends for elementary school outcomes, hence we proceed with our DD design.

4 Crime and Behavioral Incident Results

In this section, we present our core results, detailing the impact of Community School status on crime and behavioral outcomes for students. Before presenting the results, it is worth noting that one of the four pillars of the NYC-CS initiative expanded learning time, allowing children at these schools to have extended time with teachers and other school staff before, during, and after school. Two consequences of this expanded learning time pillar is that (i) students will spend longer with one another under the supervision of school staff, and (ii) spend more of their day at school. Both of these changes to student time use at Community Schools should lead mechanically to an increase in all relational crime and behavioral outcomes (violence, bullying) – a 15% longer school day, assuming interpersonal conflict may arise at any time of the school day, should lead to a 15% increase in interpersonal conflict opportunities.

Given that schools are required by federal law to report all violent or disruptive events, these interpersonal conflict outcomes should be captured in our VADIR crime series. Hence, without any change in the intensity of student behavior incidents, we should find an increase in reported crime and behavior issues due to a mechanical reporting effect: students are at school, under staff supervision, for a longer period of time. The corollary of this observation is that any reduction in behavioral incidents that we document for Community Schools will be a lower bound of the true effect, given the offsetting mechanical report effect we discuss here.

4.1 DD Estimates for Crime and Behavioral Outcomes

We present evidence on the impact of Community School status on crime and behavioral outcomes by grade level in Table 2. We first present results for all grade-levels combined in panel (a) of Table 2. This allows an initial insight into the impact of Community School status on crime and behavioral outcomes for students. Across all grades, Community Schools show a statistically significant reduction in the annual total crime rate, with 14.2 fewer crimes per 1,000 students—a 24.5% decline compared to the control baseline mean. This overall decrease is primarily due to substantial reductions in bullying, as well as notable declines in disruptive behavior incidents and misdemeanor crimes.

Yet, given the differences in the incidence of crime and behavioral outcomes across student ages, the proceeding results, split by grade-level, offer a better account of the effect of Community School status.[16] The pattern of results across grade-levels is particularly pronounced – it is only in elementary schools, schools serving the youngest students, where we find consistent and statistically significant impacts of Community School status on student crime and behavioral outcomes.[17] For all crime types, the impact of Com-

[16]Another reason we focus our results on elementary schools is the presence of significant pre-trends across all grades, primarily driven by senior high schools.

[17]We test the difference in effects across different grades in Table C1. We find a differential effect of Community Schools on bullying between elementary and senior high schools, weapon possession between elementary and middle schools, and misdemeanor crime between elementary and both middle and senior high schools.

munity School status declines monotonically with the age range of students served in the respective grade-levels. Such results are consistent with the idea that the timing of school interventions matter, with early interventions producing larger impacts (Nicolson et al., 1999; Ward, 1999; Grantham-McGregor et al., 2007; Landa et al., 2011; Guthrie et al., 2023).

In elementary schools, becoming a Community School leads to a statistically significant drop in the annual total crime rate by 21.6 crimes per 1000 students, a decline of 62% when compared to the control baseline mean. This overall drop in crime rate is driven primarily by large falls in the bullying rate in Community Schools, as well as significant falls in disruptive behavior incidents, misdemeanor crimes, and a small drop in weapon possession crimes.[18]

Moving down the table, we see same-signed impacts for the three main margins of behavioral response to Community School status – bullying, disruptive behavior, misdemeanor crimes – for the higher grade levels, but these effects are considerably smaller and far less precisely estimated. Due to the lack of significance of effects at the higher grade levels, we will focus our attention on elementary schools for the remainder of this section.

4.2 Are TWFE Estimates Valid in Our Setting?

Given the staggered nature of the Community School program in New York City coupled with the recent literature on the perils of implementing a standard two way fixed effects (TWFE) DD model in the face of staggered treatment implementation with treatment effects that may be heterogeneous, one may be concerned by our use of TWFE estimates in this setting. The key issue highlighted by papers such as de Chaisemartin and D'Haultfœuille (2020) and Goodman-Bacon (2021a) – that of "negative weighting" due to so-called "forbidden" comparisons of treated schools with already treated schools – means that TWFE estimates may not recover the true ATT of Community School adoption on crime and behavioral outcomes.

[18] Weapon possession crime includes firearms, knives, sling shots, martial arts instruments, explosive such as firecrackers, and dangerous chemicals among others. See Section 4a of https://www.p12.nysed.gov/sss/ssae/schoolsafety/vadir/glossary201718.html#Ft1 for the full listings of weapons included in the VADIR crime data.

Table 2: The Impact of Community Schools on Crime and Behavioral Incidents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Crime	Violent Crime	Weapon Possession	Property Crime	Drug Crime	Misdemeanor Crime	Bullying	Disruptive Behavior
(a) All Grade Levels								
CS	–14.2***	.804	.274	.0285	.233	-.808***	–8.85***	–5.93**
	(4.86)	(1.03)	(.454)	(.262)	(.364)	(.226)	(2.46)	(2.6)
$\overline{Y}^{NT}_{PRE}$	58.2	8.56	2.72	1.45	1.53	1.44	27.6	14.9
CS/ $\overline{Y}^{NT}_{PRE}$	-.245***	.094	.1	.0196	.153	-.561***	-.321***	-.397**
	(.0835)	(.121)	(.167)	(.181)	(.238)	(.157)	(.0893)	(.174)
Pre-Trends p-value:	.00056	.203	.887	.102	.459	.173	.0015	.112
Community Schools	119	119	119	119	119	119	119	119
All Schools	1,389	1,389	1,389	1,389	1,389	1,389	1,389	1,389
(b) Elementary Schools [Grades K-5]								
CS	–21.6***	-.0335	-.684*	-.34	-.0605	–1.37***	–15.6***	–3.5**
	(5.92)	(2.09)	(.369)	(.306)	(.203)	(.304)	(3.64)	(1.49)
$\overline{Y}^{NT}_{PRE}$	34.8	8.44	1.43	.654	.307	.789	18.4	4.72
CS/ $\overline{Y}^{NT}_{PRE}$	-.622***	-.00397	-.477*	-.52	-.197	–1.74***	-.848***	-.741**
	(.17)	(.247)	(.258)	(.467)	(.662)	(.385)	(.197)	(.316)
Pre-Trends p-value:	.19	.227	.546	.708	.926	.277	.339	.455
Community Schools	35	35	35	35	35	35	35	35
All Schools	742	742	742	742	742	742	742	742
(c) Middle Schools [Grades 6-8]								
CS	–4.43	3	1.17	.362	1.12	-.483	–7.35	–2.24
	(9.9)	(2.38)	(.758)	(.492)	(.91)	(.345)	(5.41)	(3.85)
$\overline{Y}^{NT}_{PRE}$	101	12	4	2.92	1.91	2.65	50.6	26.9
CS/ $\overline{Y}^{NT}_{PRE}$	-.0439	.25	.292	.124	.585	-.182	-.145	-.0834
	(.0981)	(.198)	(.19)	(.168)	(.476)	(.13)	(.107)	(.144)
Pre-Trends p-value:	.441	.461	.659	.327	.129	.447	.0969	.489
Community Schools	35	35	35	35	35	35	35	35
All Schools	227	227	227	227	227	227	227	227
(d) Senior High Schools [Grades 9-12]								
CS	–7.11	.804	-.105	.102	-.122	-.22	–1.43	–6.14
	(10.1)	(1.22)	(1.08)	(.46)	(.715)	(.48)	(3.97)	(5.91)
$\overline{Y}^{NT}_{PRE}$	78.8	5.51	4.91	2.16	4.09	2.06	32.1	28
CS/ $\overline{Y}^{NT}_{PRE}$	-.0903	.146	-.0215	.0471	-.0298	-.107	-.0446	-.219
	(.128)	(.221)	(.219)	(.213)	(.175)	(.233)	(.124)	(.211)
Pre-Trends p-value:	.0137	.22	.33	.42	.383	.0428	.00435	.146
Community Schools	40	40	40	40	40	40	40	40
All Schools	317	317	317	317	317	317	317	317

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the school level. The pre-trends p-value is obtained by implementing the approach of Borusyak et al. (2024) using the 4 years pre-treatment.

To assuage such concerns we provide two pieces of evidence. The first piece of evidence comes in the form of the Goodman-Bacon (2021a) decomposition, which we present in Table 3. The key message from this table is that the use of TWFE estimates in this setting is not meaningfully affected by "negative weighting" issues, as clean comparisons (Community Schools versus untreated schools) comprise 98.9% of the weight used to form our DD estimates. In Table 3 we present the relevant inputs into our TWFE DD estimates

– the clean DD estimate and weight, and the forbidden counterparts.

Table 3: Goodman-Bacon Decomposition of TWFE DD Estimates – Elementary Schools

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Crime	Violent Crime	Weapon Possession	Property Crime	Drug Crime	Misdemeanor Crime	Bullying	Disruptive Behavior
CS	–21.6***	-.0335	-.684*	-.34	-.0605	–1.37***	–15.6***	–3.5**
	(5.92)	(2.09)	(.369)	(.306)	(.203)	(.304)	(3.64)	(1.49)
Clean DD	–21.8	-.0424	-.687	-.346	-.06	–1.38	–15.7	–3.54
Clean Weight	.989	.989	.989	.989	.989	.989	.989	.989
Forbidden DD	–5.89	.797	-.337	.198	-.104	-.578	–6.35	.487
Forbidden Weight	.0105	.0105	.0105	.0105	.0105	.0105	.0105	.0105

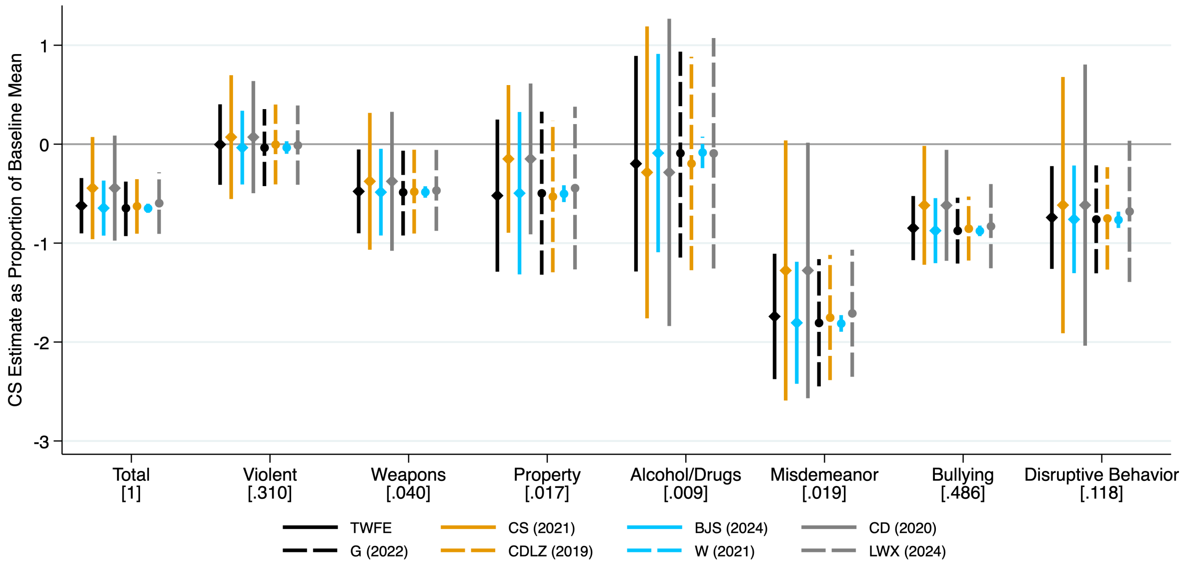
Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the school level. The Goodman-Bacon decomposition follows Goodman-Bacon (2021a). Clean DD is the component of the main DD term that comes from a clean comparison of treated schools vs. never treated schools, with clean weights denoting the weight that this estimate contributes to the main DD term. Forbidden DD is the component of the main DD term that comes from a forbidden comparison of already-treated schools vs. already-treated schools with different timings, with forbidden weights denoting the weight that this estimate contributes to the main DD term. Within DD is the component of the main DD term that comes from within treatment group variation that arises due to the inclusion of covariates, with within weights denoting the weight that this estimate contributes to the main DD term.

Next, we present the resulting estimates from a battery of alternative (DD 2.0) estimators, namely those from Callaway and Sant'Anna (2021), Borusyak et al. (2024), de Chaisemartin and D'Haultfœuille (2020), Gardner (2022), Cengiz et al. (2019), Wooldridge (2021), and Liu et al. (2024). In Figure 1, we present the estimated coefficients (as a percent of the control baseline mean) of the impact of Community School status on crime and behavioral outcomes in elementary schools using our baseline TWFE and seven alternative estimators. The key message we take away from this graph is that our findings are highly robust across DD estimators. The conclusion we draw from the two distinct exercises in this section is that the use of TWFE estimators are appropriate in our setting.

4.3 Heterogeneity Analysis

Having detailed the impact of becoming a Community School on crime and behavioral outcomes on average, we next turn to document heterogeneity in our estimated ATT. To do so, we dichotomize schools along four key margins of (pre-policy) student intake: racial and ethnic composition, English language proficiency, and student poverty exposure. The aim of conducting this heterogeneity analysis is to understand if Community

Figure 1: Alternative (DD2.0) Estimators



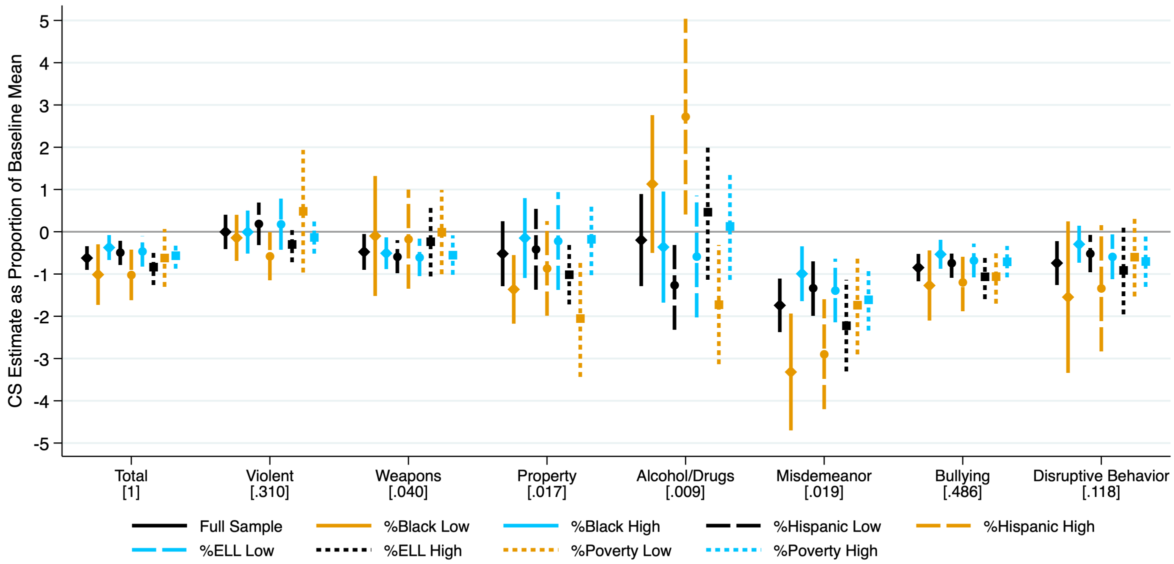
Notes: We present point estimates and 90% confidence intervals (based on standard errors that are clustered at the school level.) for our DD estimates from a variety of different estimators. These include: our baseline estimator [TWFE), and the estimators from Callaway and Sant'Anna (2021) [CS (2021)], Borusyak et al. (2024) [BJS (2024)], de Chaisemartin and D'Haultfœuille (2020) [CD (2020)], Gardner (2022) [G (2022)], Cengiz et al. (2019) [CDLZ (2019)], Wooldridge (2021) [W (2021)], and Liu et al. (2024) [LWX (2022)]. In square brackets under each category label, we present the proportion of our total crime and behavioral outcomes measure account for by each crime category.

Schools are more effective for reducing crime and behavioral outcomes for specific types of students. We base our heterogeneity analysis based on pre-policy realizations of student demographics, as the NYC-CS Initiative may directly impact student demographics. We present the results of this analysis in Figure 2.

The key finding that emerges from this heterogeneity analysis is that Community Schools are, for most outcomes, uniformly effective across key dimensions of student intake. There are a few statistically significant differences for specific demographic-behavioral outcome combinations, but these are rare.[19] What is encouraging about these results, with an eye on the further expansion of the Community School program that is occurring both within New York (see Figure A1) as well as in other states, is that the dampening effect of Community Schools on crime and behavioral outcomes is not demographic-specific. Whether or not this seemingly universal effectiveness of Community School in reducing negative behavioral outcomes is a consequence of the multifaceted

[19] In Figure C2 we examine whether these effects differ statistically for high vs low subgroups across four key demographic groups. Overall, the differences are not statistically significant, except for weapon, drug, and misdemeanor crimes. We attribute these findings to the rarity of such crimes in the data making our estimates noisy.

Figure 2: Heterogeneity Analysis – TWFE



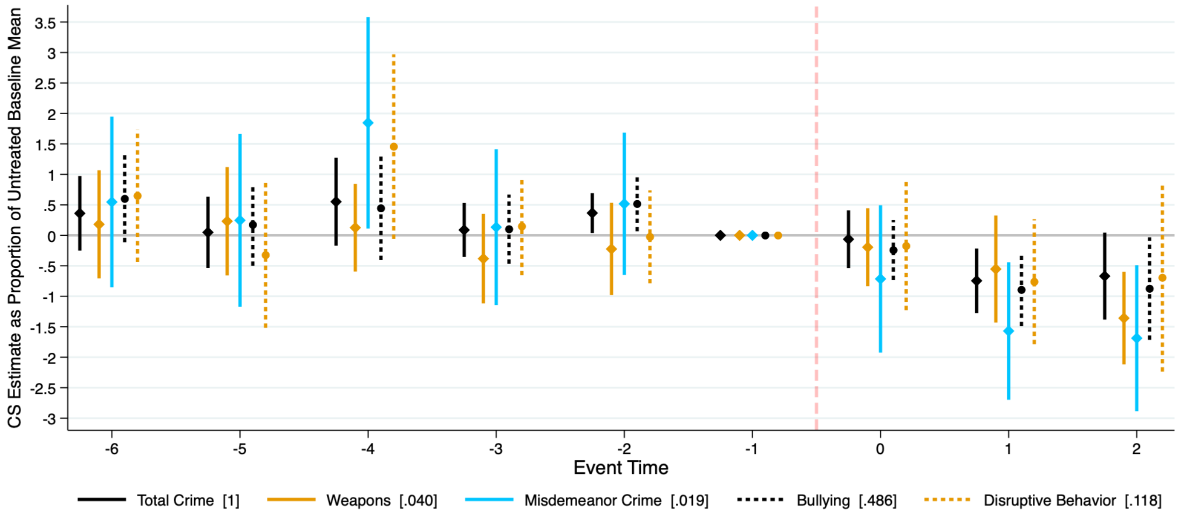
Notes: We present point estimates and 90% confidence intervals for our baseline TWFE DD estimates for a variety of different sub-samples. Standard errors are clustered at the school level. In square brackets under each category label, we present the full-sample proportion of our total crime and behavioral outcomes measure account for by each crime category.

nature of the program, or the in-built adaptability of Community Schools – bringing in local parents, local community organisations – is beyond the scope of what we are able to say with the data at hand.

4.4 Dynamic Effects

To get a sense of the dynamic effects of Community School status on behavioral outcomes, we present the resulting estimates from an event study analysis for key outcomes. We chose these outcomes as these are the margins that we detect a statistically significant impact in the static setting (i.e. a statistically significant DD estimated – see Table 2.). We start by noting the absence of any pre-trends in the outcomes we consider. This corroborates the other evidence that we present in Table 2 and Section B in support of the parallel trends assumption. Little happens in the first year of treatment – none of the event study estimates for the first year post-Community School status are statistically significantly different from zero. This changes however two and three years after becoming a Community School. The results for year two and year three are highly stable. With only up to three years post-treatment data, it is hard to say anything stronger about the lasting effects of becoming a Community School. However, we do document

Figure 3: Event Study Graphs – Key Outcomes



Notes: We present point estimates and 90% confidence intervals for our event study estimates for a sub-set of key outcomes. Standard errors are clustered at the school level. In square brackets under each category label, we present the full-sample proportion of our total crime and behavioral outcomes measure account for by each crime category.

evidence of a bedding-in period where key players within the school adapt to the changing organizational structure of the school, and then those changes begin to bear fruit.

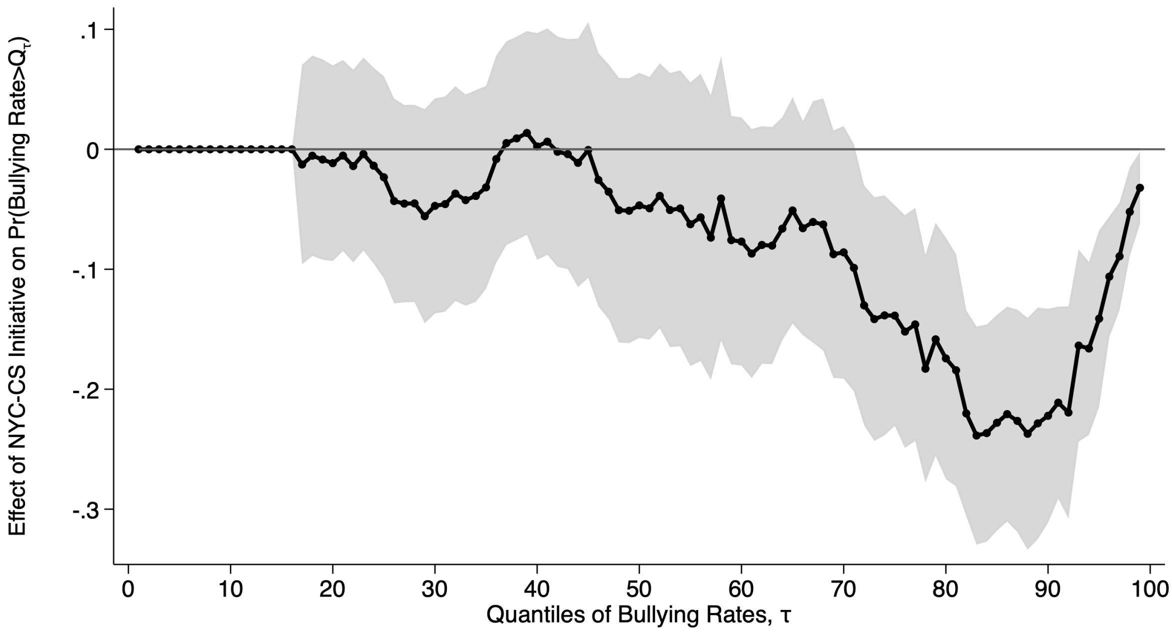
4.5 Distributional Effects

As a final exercise in this section, we consider the distributional effects of the NYC-CS Initiative. The results we present here focus on bullying, given that bullying incidents account for approximately half (48.6%) of all recorded incidents in elementary schools. We present the results of our distributional analysis in two different forms. In Figure 4 we present the results from our distributional DD regression in the form of an inverse cumulative distribution function (CDF) representation. In Appendix Figure C6 we present the estimates in unconditional quantile partial effect (UQPE) form. We discuss the pros and cons of this approach for our setting in Appendix Section C.6.2.

To operationalize the inverse CDF approach, we estimate our standard DD model as in Equation (1), but replace the dependent variable of bullying rate with a series of dummies indicating if the bullying rate is greater than a given quantile, Q_τ, of bullying rates in non-Community Schools pre-2014, for $\tau = [1, \ldots, 99]$, that is $y_{st} = \mathbb{1}[\text{bullying}_{st} > Q_\tau]$. This gives rise to 99 inverse CDF-based DD regressions, allowing us to trace the effect of the NYC-CS Initiative along the full distribution of bullying rates. An example of this approach can be seen in Goodman-Bacon (2021b).

The distributional DD results are informative regarding the distributional source of our baseline estimates – Community Schools reduce bullying rates at the mean by reducing bullying at the upper end of the distribution. It is only once we move above the 70th percentile of bullying rates that we document significant impacts of the policy. Part of the reason for this is likely mechanical – Community Schools have higher levels of bullying at baseline (Appendix Figure C5), thus if the NYC-CS Initiative is effective in reducing bullying, the effects of the program will be most prominent in the upper tail of the bullying distribution. The key finding here, however, is that the Initiative is highly effective at reducing the incidence of bullying in schools where this is a serious issue.

Figure 4: Distributional Effects of Community Schools on Bullying Rates



Inverse CDF Representation

Notes: We present point estimates and 90% confidence intervals for a the impact of Community Schools on bullying from a series of distributional DD regressions. The estimates come from a set of regressions where the outcome is $y_{st} = \mathbb{1}[\text{bullying}_{st} > Q_\tau]$ for $\tau = [1, \ldots, 99]$. Standard errors are clustered at the school level.

5 Parental Responses to Community Schools

5.1 Revealed Preference Evidence

In this section we document parental sorting responses to Community Schools, as evidenced by changing student demographics.

5.1.1 Constructing An Ex-Ante Crime Risk Score

Prior to presenting the impacts of gaining Community School status on student compositional changes, we first ask how student composition relates to crime outcomes in the cross section. Using the five years of our data prior to the introduction of the Community School program, we estimate the conditional correlation between student demographics and crime and behavioral outcomes, presenting the resulting partial correlations in Appendix Table C2. The purpose of this exercise is to create a predicted crime risk score, based on school level demographics.

Common patterns of correlation exist across the crime and behavioral outcomes – enrollment is typically negatively (partially) correlated with behavioral outcomes, while the proportion of students with disabilities typically correlate positively. As with all of these correlates, the correlation will reflect both engaging in crime behavioral outcomes (supply-side effects), and being the victim of these behaviors (demand-side effects). Based on the correlates of total crime and behavioral outcomes, we create an index based on the years 2009-2013, which combines all demographic inputs into a single score which we label the "ex-ante crime risk score". We will use this risk score to reduce the dimensionality of all the demographic variables we consider in the next exercise.[20]

5.1.2 Parental Sorting Responses

In Table 4, we present evidence of the impact of gaining Community School status

[20]Test scores are not the primary focus of this paper. However, to better understand the stated preference evidence presented in Section 5.2, we also create ex-ante predicted English and Math scores. These are generated using the same method outlined above for creating the ex-ante crime risk score, but with test scores as the target variable instead of the total crime rate. The factor loadings for the scores are presented in Table C3 It is worth nothing that the loadings for test scores tend to have opposite signs to those for crime and behaviour outcomes.

Table 4: The Parental Sorting Response to the NYC-CS Initiative

	(1) Enrollment	(2) % Female	(3) % Asian	(4) % Black	(5) % Hispanic	(6) % Other
CS	–58.7***	-.115	-.416**	.0789	.295	-.442***
	(14.7)	(.368)	(.209)	(.677)	(.581)	(.156)
$\overline{Y}_{PRE}^{NT}$	652	48.7	13.9	27.9	39.7	1.62
CS/ $\overline{Y}_{PRE}^{NT}$	-.09***	-.00237	-.0299**	.00283	.00744	-.273***
	(.0226)	(.00756)	(.015)	(.0243)	(.0146)	(.096)
Community Schools	35	35	35	35	35	35
All Schools	742	742	742	742	742	742
Observations	5,936	5,906	5,906	5,906	5,906	5,906

	(7) % Students With Disabilities	(8) % English Language Learners	(9) % Poverty	(10) Ex-Ante Crime Risk Score	(11) Ex-Ante Predicted Math Score	(12) Ex-Ante Predicted English Score
CS	1.26*	-.187	5.08***	2.26***	-.101***	-.115***
	(.667)	(.545)	(1.07)	(.566)	(.0281)	(.0346)
$\overline{Y}_{PRE}^{NT}$	17.7	14.9	78.5	36.1	.00722	-.0025
CS/ $\overline{Y}_{PRE}^{NT}$	.071*	-.0125	.0648***	.0627***		
	(.0377)	(.0366)	(.0137)	(.0157)		
Community Schools	35	35	35	35	35	35
All Schools	742	742	742	742	742	742
Observations	5,906	5,902	5,906	5,902	5,902	5,902

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the school level. The Ex-Ante Crime Risk Score is calculated based on the pre-period years of 2009-2013. We predict total crime using an OLS regression model, including all demographic variables as predictors. We then use the parameter estimates to predict crime risk score for the full period. We calculate the ex-ante predicted Math and English scores in precisely the same manner, except, due to data inconsistencies in the test score data, we only use the years 2012 and 2013 for the first-stage prediction regression. We discuss these data limitations in Section 6.1. The test score data we use is standardized by grade and year, and then collapsed to the school-by-year level. For this reason, we do not present proportional DD estimates for the predicted test score variables. As the variables are standardized, the DD is directly interpretable in standard deviation terms.

on student composition. These changes will reflect parental sorting responses to the NYC-CS Initiative. Given the large drops in crime and behavioral issues that follow from a school gaining Community School status, the results on enrollment – a drop in 59 students or a 9% reduction – are somewhat unexpected. Based on this measure, becoming a Community School makes schools less attractive to parents. One reading of this result is that parents are initially skeptical of the new organizational structure inherent in the Community School architecture. It is likely this skepticism would dissipate once parents learn about the improvement in behavioral outcomes in these schools.

Other changes occur along racial and ethnic lines – a 3% fall in Asian students, and a proportionally large, but small in absolute sense drop in other racial groups (.4 students fewer from other racial groups). Community School status leads to a 7% increase in the proportion of students with disabilities, and an economically meaningful and statistically significant 5.1pp (6.5%) increase in student's poverty exposure. In Column 10, we present the combined effect of these changes using our ex-ante crime risk score. Based on the demographic changes that result due to Community School status, we see a 6.3% increase in the crime risk score. This finding stands in sharp contrast to our results of a negative impact of Community schools on (realized) crime and behavioral outcomes.[21]

So, how should one read this final finding? The first conclusion we draw from this crime risk score finding is that the compositional changes that occur in response to gaining Community School status stack the deck against the treatment schools in terms of behavioral outcomes. Based on the correlates of total crime outcomes in the five years prior to the introduction of the program, the demographic changes that occurred in Community Schools should have *increased* crime and behavioral incidents in these schools. That we find substantially *lower* realized crime outcomes as a consequence of becoming a Community School suggests that we are very much capturing a lower bound of the crime-reducing effecting of the NYC-CS Initiative with our DD estimates. A second, related take-away from the analysis that we present in Table 4 is that the results within robustly validate our decision to not include time-varying school demographics as covariates in our DD model specification. As such a large proportion of these change in response to the program, these would have been bad controls.

5.2 Stated Preference Evidence

We next turn to the annual NYC School Survey, which is designed to elicit input from parents and teachers in grades K through 12 and students in grades 6-12 about the environment at each NYC school. Our focus is on the survey responses from parents of children in elementary schools. The reason to do so is to investigate if we find a congruence between the revealed preference evidence we document on enrollment, and the stated preference findings we can extract from survey data. We identify six key questions that closely match the dimensions of parents' perception of the school climate

[21] In the final two columns, we present analogous results for ex-ante predicted Math and English scores – based on demographic changes alone, we would expect Math and English to decline by 10.1% and 11.5% of a standard deviation respectively.

and satisfaction with various school aspects.[22]

We construct a dummy variable equal to one if the parent strongly agrees with a question and zero otherwise, and present the results from a series of DD regression specifications akin to Equation (1) in Table 5.[23] We document an unambiguously negative response from parents with children attending a Community School. These parents feel that their children are less safe at school, are less satisfied with their children's education, and are less satisfied with the school.[24]

Table 5: Parental Sentiment as Measured in the NYC School Survey

	(1)	(2)	(3)	(4)	(5)	(6)
	Parental Responses – Strongly Agree With the Statement:					
	My Child is Safe at School	School Kept Clean	Courses and Extra-Curricular Activities to Keep Interest	Satisfied with Communic-tion with School	Satisfied with Child's Education this Year	Satisfied with Child's Teacher Quality this Year
CS	-.0244*	-.021*	-.00913	-.0205**	-.0218**	-.0287***
	(.0128)	(.0122)	(.0122)	(.0096)	(.00924)	(.00977)
$\overline{Y}^{NT}_{PRE}$	.538	.521	.451	.475	.483	.551
CS/ $\overline{Y}^{NT}_{PRE}$	-.0453*	-.0403*	-.0203	-.0431**	-.0452**	-.0522***
	(.0238)	(.0235)	(.027)	(.0202)	(.0191)	(.0177)
Pre-Trends p-value:	.914	.563	.606	.296	.213	.146
Community Schools	35	35	35	35	35	35
All Schools	724	724	724	724	724	724

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the school level. The pre-trends p-value is obtained by implementing the approach of Borusyak et al. (2024) using the 4 years pre-treatment.

The key finding that we take away this exercise is that the stated preference and the revealed preference evidence that we document coincide – parents are not initially happy with the transition to Community School status. This is surprising, particularly the finding that parents feel their child is less safe at school once a school becomes a Community

[22] While the parent survey included additional questions, we were only able to explore six of them due to inconsistent reporting or significant changes over time. Unfortunately, because of similar issues we could not analyze how teacher perceptions changed in schools that transitioned to Community Schools. We do not analyze the student surveys given that we find null effect of Community Schools for middle and high school students.

[23] We decided to create this dichotomous measure because approximately 90% of parents agree or strongly agree with each survey question.

[24] We present the results from a corresponding event study in Appendix Figure C4. These results indicate that, for most outcomes, the harshest parental response occurs immediately after a school transitions to a Community School, with some reduction in the negative response two years later. This is suggestive of a bumpy transition to becoming a Community School.

School. This stands in direct opposition to our core finding of community school status leading to fewer student crime and behavioral outcomes.

One possible explanation for this counter-intuitive finding could be that parents base their expectations of school safety not on (contemporaneously hard-to-observe) crime outcomes, but rather the (easier to observe) composition of the students attending the school.[25] We posit that parents might heuristically forming a measure of our ex-ante crime risk score based on what they observe about the changing school composition in response to the NYC-CS Initiative. This observation on the gap between true and perceived treatment effects underscores the importance of the ex-ante crime score measure that we construct in Section 5.1.2. We note that such a disconnect between perceptions of outcomes and the actual realizations of these outcomes is documented in other domains (Adda et al., 2014).

6 Community Schools, Bullying, and Test Scores

In Section 4 we document the importance of the NYC-CS Initiative in reducing crime and behavioral incidents – primarily by reducing the incidence of bullying. Given that previous work has found that the NYC-CS Initiative leads to improved test scores (Covelli et al., 2022; Johnston et al., 2020), one may wonder: *given the negative impact of bullying on educational outcomes, what role does the reduction in bullying play in the improved test scores for Community Schools?* Answering this question is of first-order importance in understanding the long-run impact of the lower incidence of bullying risk faced by students attending Community Schools. If (i) Community Schools cause a reduction in bullying incidence, and (ii) this reduced bullying incidence causes grades to improve, then this would establish a causal pathway linking the NYC-CS Initiative and the determinants of future earnings via the reduction in bullying.

[25] A similar argument can be made for the discrepancy between stated preferences and revealed preferences for test scores. Specifically, parents might heuristically decide based on compositional changes brought by community school status. Note that the association between school composition measures and both the crime risk score and the two test score measures are generally opposite-signed. In essence, factors that lead to higher crime tend to result in lower test scores.

6.1 Causal Mediation Analysis

To investigate the causal linkages of the NYC-CS Initiative on test score outcomes, we conduct a causal mediation analysis. The core outcome equation models test-scores as a function of both Community School status and bullying:

$$TS_{st} = \gamma_1 \, CS_{st} + \gamma_2 \, \text{Bullying}_{st} + \theta_s + \delta_{B\times t} + \varepsilon_{it} \tag{3}$$

where TS_{st} are test-scores, CS_{st} is Community School status, and Bullying_{st} is the bullying rate. The terms θ_s and $\delta_{B\times t}$ are school and borough-by-year fixed effects respectively. The test-score series changed significantly in the academic year 2012/13. For this reason, our analysis runs from 2012/13 (the first year of consistent test score data) to 2016/17 (the final year of consistent crime data).

As we show in Section 4, Community Schools affect bullying rates. This means we cannot estimate Equation (3) by OLS as bullying rates will be a bad control, leading to biased parameter estimates. To circumvent this bad control problem we implement a causal mediation analysis (CMA) strategy (Huber, 2019; Celli, 2022). Such an approach is becoming increasingly common (Attanasio et al., 2020; Cattan et al., 2023; Nicoletti et al., 2023). The CMA approach amounts to two-stage least squares (2SLS) strategy, where we instrument for our bullying measure in a first stage, and then estimate this first stage and Equation (3) by 2SLS. The first-stage equation for bullying is:

$$\text{Bullying}_{st} = \alpha_1 \, CS_{st} + \alpha_2 \, Z_{st} + \sigma_s + \tau_{B\times t} + \mu_{it} \tag{4}$$

where Bullying_{st} is the bullying rate, CS_{st} is Community School status, and Z_{st} is a leave-one-out shift-share instrument. The share component, r_{s0}, is based on the school-level bullying rate mean for the years 2006-2008 ($R_{s0} = \text{Bullying}_{s0}/\text{Bullying}_0$), and amounts to the share of the NYC bullying rate that school s contributes. The shift component, F_t^{-i} is based on the annual, leave-one-out sum of NYC elementary school bullying rates. The reason to use the leave-one-out sum is that the total sum includes the own-observation contribution of school s in period t, thereby unnecessarily inducing endogeneity between the instrument and the error term in the first stage equation (Goldsmith-Pinkham et al., 2020). The resulting shift-share instrument is $Z_{st} = R_{s0} \times F_t^{-i}$. The terms σ_s and $\tau_{B\times t}$ denote school and borough-by-year fixed effects respectively and μ_{it} is an error term. We specify heteroskedasticity-robust standard errors. We provide evidence to highlighting that we satisfy the rank condition in panel (b) of Table 6.

Conditional Independence We additionally provide evidence in support in favor of the conditional independence assumption of our instrument. The evidence comes in the form of a test of conditional random assignment of the instrument, the results of which we present in Appendix Table C4. We regress the shift-share IV on our full set of demographic controls. The p-value associated with a test of joint significance of the demographic controls is 0.538 (Column (3), Appendix Table C4), informing us that, conditional on school and borough-by-year FEs, we are unable to meaningfully explain our IV with our full set of student demographics. We thus conclude that our shift-share IV is conditionally randomly assigned.

Monotonicity If the instrument has a heterogeneous effect on bullying rates, we additionally require the monotonicity assumption. In our case, this means the instrument must lead to monotonically higher bullying incidence in schools. To provide support for the monotonicity assumption, we follow the insight of Bhuller et al. (2020), who note that a testable implication of the monotonicity assumption is that the first-stage coefficient for our instrument should be non-negative for any sub-sample. We re-estimate our first stage on 24 sub-samples of the data, and present the estimated coefficients for the full sample and the 24 sub-samples in Appendix Figure C7. In every case, the coefficient is positive, providing strong empirical support in favor of the monotonicity assumption in our setting.

Decomposition To decompose the effect of Community Schools on test score outcomes, we use the parameters from the two stages of the 2SLS procedure. The direct effect is measured by γ_1 from Equation (3). The indirect effect traces the impact of Community Schools on bullying, and then from bullying on to test scores, and is therefore calculated as α_1 from Equation (4) multiplied by γ_2 from Equation (3).

Table 6: Decomposition of the Direct and Indirect Effect of Community Schools on Test Scores

	(1)	(2)	(3)
	Math	**ELA**	**Combined Score**
(a) OLS: Test Scores			
CS	.18***	.139***	.159***
	(.044)	(.0433)	(.0398)
(b) First Stage: Bullying Rate			
CS	–10.4**	–10.4**	–10.4**
	(4.33)	(4.33)	(4.33)
Shift-Share IV	.683***	.683***	.683***
	(.104)	(.104)	(.104)
First-Stage F-Statistic	42.8	42.8	42.8
(c) 2SLS: Test Scores			
CS	.134**	.0806	.107**
	(.0537)	(.0536)	(.0493)
Bullying Rate	-.00344*	-.00436**	-.00389**
Per 1,000 Students	(.00196)	(.00203)	(.0018)
(d) Decomposition			
Direct Effect	.134	.0806	.107
Indirect Effect	.0358	.0454	.0405
Total Effect	.17	.126	.148
Community Schools	34	34	34
All Schools	697	697	697
Observations	3,485	3,485	3,485

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Eicker-Huber-White standard errors in parentheses. School and Borough-by-Year FEs are included in all regression specifications. All test score measures are school-based weighted averages of grade level 3,4, and 5 math and English Language Arts (ELA) z-scores. To create z-scores, we first standardize the test-scores at the grade-year level. Slightly different numbers of pupils take the ELA and math tests within school. For this reason, the combined score measure is a school-based weighted average measure of the two scores. Due to changes in test scores in 2012/13 school year, the estimation sample is 2012/13-2016/17. The instrument for bullying is a leave-one-out shift-share instrument. The share component is based on the school-level bullying rate mean for the years 2006-2008. The shift component is based on the annual (leave-one-out) sum of NYC elementary school bullying rates for the years 2012-2016.

We present the results of the various components of our CMA analysis in Table 6. In panel (a) of Table 6, we corroborate the finding of other scholars – the NYC-CS initiative has a positive impact on test scores. In panel (b), we present key parameters from estimating the first-stage equation for crime, Equation (4). The F-statistics for our shift-share IV highlights that we satisfy the rank condition. In panel (c) we present the key parameters for our main outcome equation, Equation (3). The key finding from this analysis is that crime rates, and specifically bullying, negatively impact test scores, particularly English scores.[26] Finally, in panel (d), we present the decomposition of the impact of Community Schools on test scores. The indirect effect of Community Schools, mediated through reduced bullying incidence, typically accounts for a quarter to a third

[26]We estimated similar models for our other crime and behavioral outcomes. Bullying is the only behavioral incident to yield consistent negative effects on test scores in our elementary school setting.

of the total effect. From this we conclude that a key reason that grades improve in Community Schools is via reduced bullying rates in these schools.

7 Conclusions

New York State spends spends $250 million annually to support Community Schools, which represents a small fraction of its almost 32 billion education budget.[27] This paper provides evidence that the NYC Community School initiative—a school-based intervention that provides structure and support for student's academic, medical, social, and mental health needs—leads to a reduction in crime and disruptive behavior in schools, exclusively among elementary school students. These findings underscore the importance of early interventions, particularly those implemented during critical periods of child development, as they yield substantial long-term benefits. Our analysis reveals that Community School status leads to a significant decrease in total incidents, driven primarily by reductions in bullying and disruptive behavior. The robust and consistent treatment effects across diverse student demographic groups suggest that the benefits of the NYC-CS initiative are not limited to specific populations, making it a potentially valuable model for other school districts across the country. Despite the observed improvements in school safety and behavior, parents' perceptions of the initiative remain negative, as evidenced by decreased enrollment and lower satisfaction levels.

Our study contributes to the broader literature on school-based interventions by providing a rigorous evaluation of the crime and behavioral impacts of Community Schools, complementing existing research that primarily focuses on academic outcomes. Furthermore, our causal mediation analysis demonstrates that reductions in bullying play a crucial role in driving the impact of Community Schools on improvements in academic performance, particularly in English. This finding highlights the interconnection between behavioral and academic outcomes and suggests that addressing student behavior can be an effective pathway to improving educational achievement. In addition, it underscores that any evaluation on those standard academic outcomes, such as test scores, might undersell the benefits of this program, which targets to a great extent students' non-cognitive skills.

[27] https://www.budget.ny.gov/pubs/archive/fy22/ex/book/education.pdf

Future research should continue to explore the long-term impacts of Community Schools on various student outcomes, including labor market success and overall well-being. Additionally, understanding the factors driving parental dissatisfaction, despite positive behavioral outcomes, could provide insights for refining and enhancing the implementation of such initiatives. As educational reform efforts continue to evolve, the NYC-CS initiative serves as a promising example of how integrated, school-based interventions can foster safer, more supportive, and academically successful environments for students.

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Online Appendix

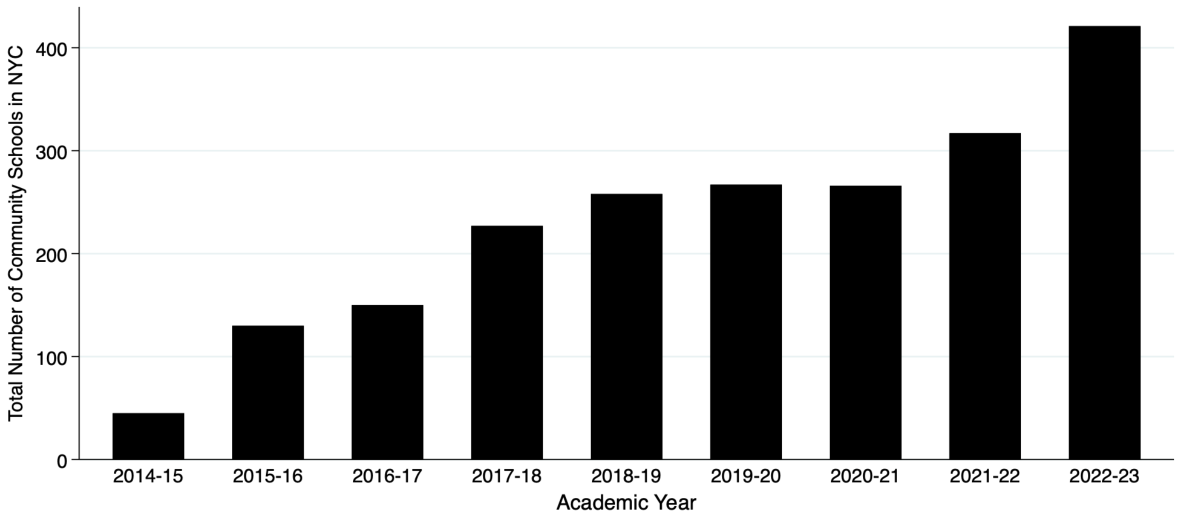
A Preliminaries

Table A1: Classification of VADIR Incidents

	(1)
VADIR Incident Category	Classfication
Homicide	Violent Crime
Sex Offenses	Violent Crime
Robbery	Violent Crime
Assault with Serious Physical Injury	Violent Crime
Kidnapping	Violent Crime
Assault with Physical Injury	Violent Crime
Reckless Endangerment	Violent Crime
Weapon Possession	Weapon Possession
Arson	Property Crime
Burglary	Property Crime
Larceny or Other Theft Offenses	Property Crime
Drug Use, Possession, or Sale	Alcohol/Drug
Alcohol Use, Possession, or Sale	Alcohol/Drug
Criminal Mischief	Misdemeanor
Riot	Misdemeanor
Minor Altercations	Bullying
Intimidation, Harassment, Menacing, Bullying	Bullying
Bomb Threat	Disruptive Behavior
False Alarm	Disruptive Behavior
Other Disruptive Incidents	Disruptive Behavior

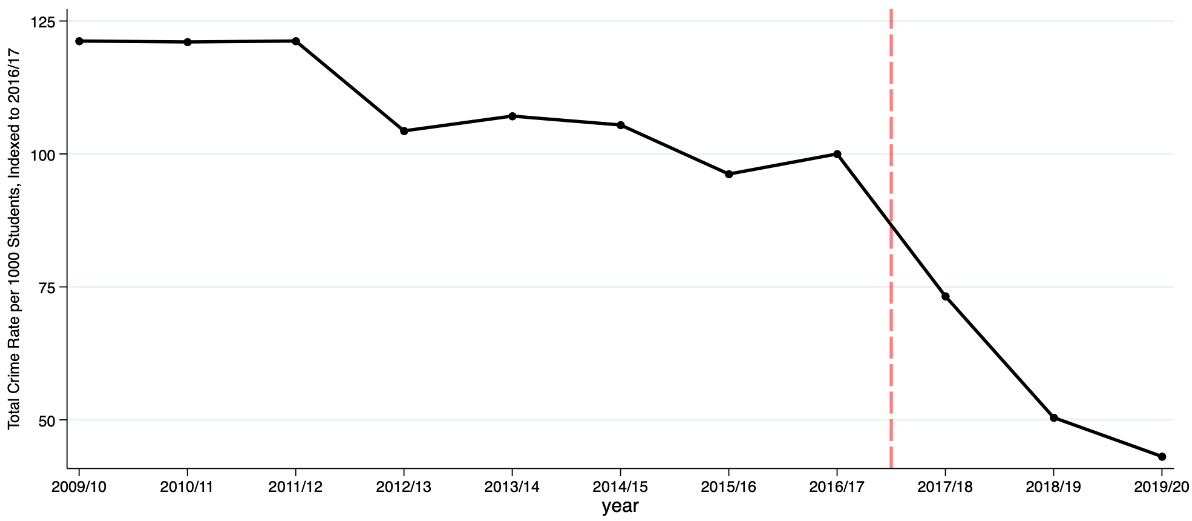
Notes: The category includes incidents that result in suspension, removal, referral to treatment/counseling, transfer to alternative education, or referral to juvenile justice system. For more information on this category and detailed definitions of the all the other VADIR incident categories please refer to https://www.p12.nysed.gov/sss/ssae/schoolsafety/vadir/glossary08aaug.html.

Figure A1: NYC Community Schools Over Time



Notes: We present the time-series of the count of all NYC schools part of the NYC-CS initiative.

Figure A2: VADIR Crime Data Over Time – Total Crime Rate per 1000 Students

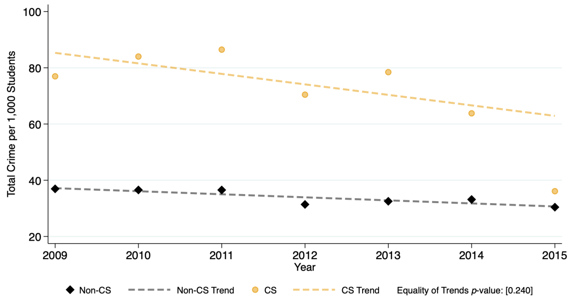


Notes: We present the indexed time-series of the VADIR-based total crime rate per 1,000 students in all NYC elementary schools.

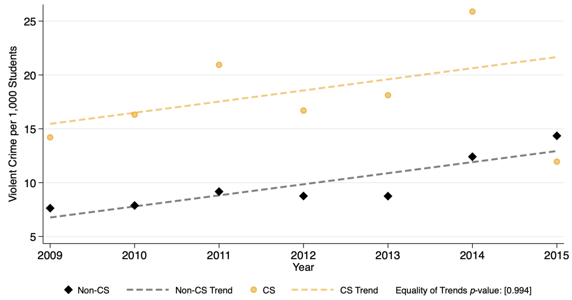
B Support for the Parallel Trends Assumption

B.1 Raw Pre-Trends

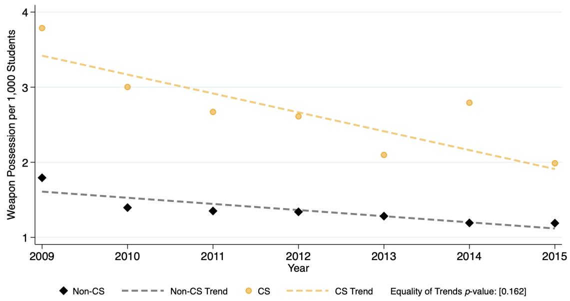
Figure B1: Raw Pre-Trends for Elementary Schools



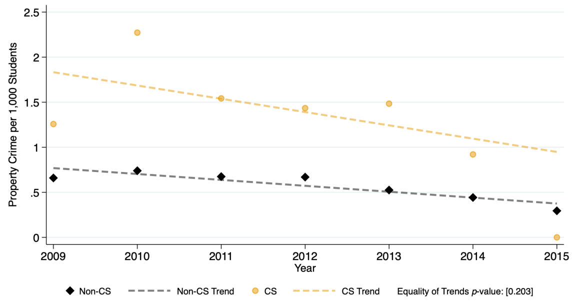
(a) Total Crime



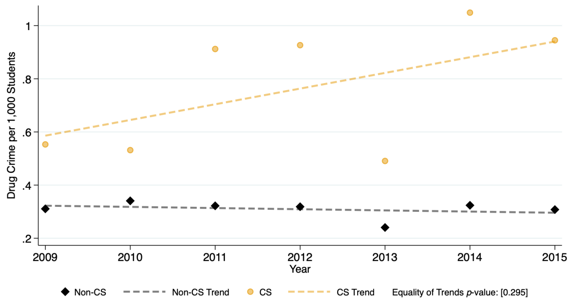
(b) Violent Crime



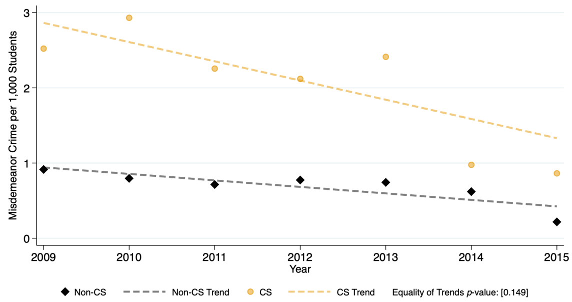
(c) Weapon Possession



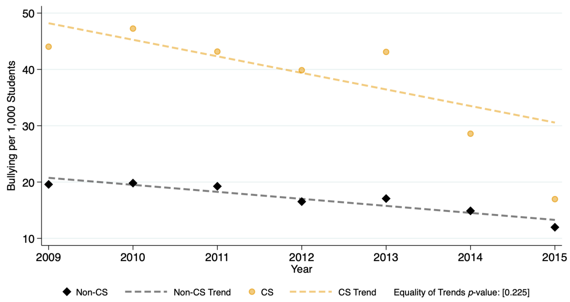
(d) Property Crime



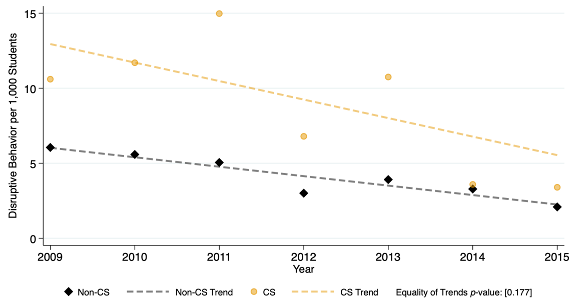
(e) Drug Crime



(f) Misdemeanor Crime



(g) Bullying



(h) Disruptive Behavior

Notes: We present the raw pre-trends for non-Community Schools and Community Schools, along with the associated *p*-value from a test of equality of pre-trends. The data covers all NYC elementary schools in the pre-policy period of 2009-2015.

B.2 Placebo DD-TWFE Results: 2006-2013

Table B1: Placebo Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Crime	Violent Crime	Weapon Possession	Property Crime	Drug Crime	Misdemeanor Crime	Bullying	Disruptive Behavior
All Grade Levels								
CS	–5.59	.764	.394	.056	.584*	-.212	–3.05	–4.13
	(7.6)	(1.16)	(.397)	(.27)	(.32)	(.299)	(4.04)	(3.65)
$\overline{Y}^{NT}_{PRE}$	57	8.82	2.91	1.42	1.33	1.37	26.2	15
CS/ $\overline{Y}^{NT}_{PRE}$	-.098	.0866	.136	.0395	.439*	-.154	-.116	-.276
	(.133)	(.131)	(.136)	(.19)	(.241)	(.218)	(.154)	(.244)
Community Schools	94	94	94	94	94	94	94	94
All Schools	1,266	1,266	1,266	1,266	1,266	1,266	1,266	1,266
Observations	10,128	10,128	10,128	10,128	10,128	10,128	10,128	10,128
Elementary Schools [Grades K-5]								
CS	7.24	3.27	-.592	.137	.171	.0924	4.3	-.139
	(12)	(2.02)	(.395)	(.378)	(.187)	(.472)	(8)	(2.83)
$\overline{Y}^{NT}_{PRE}$	34.9	8.06	1.68	.669	.274	.758	18.1	5.4
CS/ $\overline{Y}^{NT}_{PRE}$	.207	.406	-.353	.205	.624	.122	.238	-.0257
	(.342)	(.251)	(.235)	(.565)	(.685)	(.623)	(.442)	(.524)
Community Schools	34	34	34	34	34	34	34	34
All Schools	719	719	719	719	719	719	719	719
Observations	5,752	5,752	5,752	5,752	5,752	5,752	5,752	5,752
Middle Schools [Grades 6-8]								
CS	–5.5	.293	1.17	.633	.415	-.652	–7.83	.479
	(15)	(2.73)	(.74)	(.505)	(.439)	(.593)	(8.16)	(5.71)
$\overline{Y}^{NT}_{PRE}$	101	12.8	4.5	2.84	1.81	2.69	48.8	27.9
CS/ $\overline{Y}^{NT}_{PRE}$	-.0542	.0228	.26	.223	.23	-.242	-.161	.0172
	(.148)	(.213)	(.165)	(.178)	(.243)	(.22)	(.167)	(.205)
Community Schools	27	27	27	27	27	27	27	27
All Schools	216	216	216	216	216	216	216	216
Observations	1,728	1,728	1,728	1,728	1,728	1,728	1,728	1,728
Senior High Schools [Grades 9-12]								
CS	–3.34	.048	1.3	-.195	1.57	.339	–3.17	–3.23
	(13.3)	(1.07)	(.94)	(.605)	(1.06)	(.514)	(4.55)	(9.22)
$\overline{Y}^{NT}_{PRE}$	77.3	6.26	5.04	2.24	3.88	1.95	29.6	28.3
CS/ $\overline{Y}^{NT}_{PRE}$	-.0432	.00766	.258	-.0871	.405	.174	-.107	-.114
	(.173)	(.171)	(.186)	(.27)	(.272)	(.264)	(.154)	(.326)
Community Schools	28	28	28	28	28	28	28	28
All Schools	247	247	247	247	247	247	247	247
Observations	1,976	1,976	1,976	1,976	1,976	1,976	1,976	1,976

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the school level.

C Additional Analysis

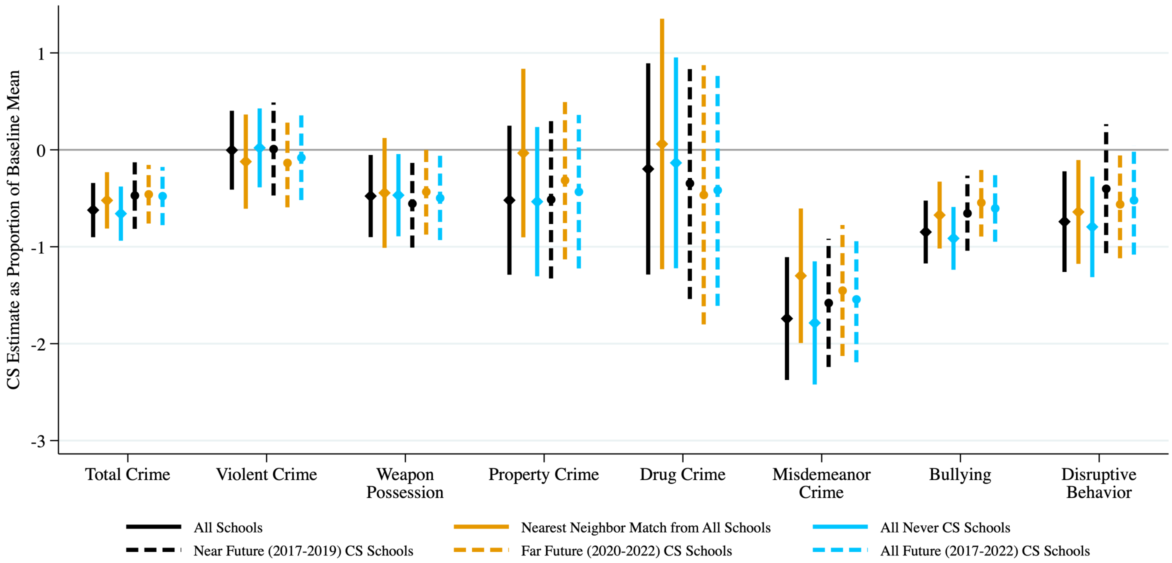
Table C1: Testing The Impact of Community Schools Across Grade Levels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Crime	Violent Crime	Weapon Possession	Property Crime	Drug Crime	Misde-meanor Crime	Bullying	Disruptive Behavior
(a) DD Estimate for Elementary vs. Middle Schools								
$\hat{\beta}_{CS}^{\text{ES}} - \hat{\beta}_{CS}^{\text{MS}}$	–17.2	–3.03	–1.85**	-.702	–1.18	-.89*	–8.3	–1.26
	(11.5)	(3.16)	(.841)	(.578)	(.93)	(.459)	(6.5)	(4.12)
(b) DD Estimate for Elementary vs. Senior High Schools								
$\hat{\beta}_{CS}^{\text{ES}} - \hat{\beta}_{CS}^{\text{SHS}}$	–14.5	-.837	-.578	-.441	.0614	–1.15**	–14.2***	2.64
	(11.7)	(2.42)	(1.14)	(.552)	(.742)	(.567)	(5.38)	(6.09)

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the school level.

C.1 Sensitivity Analysis – Alternative Controls

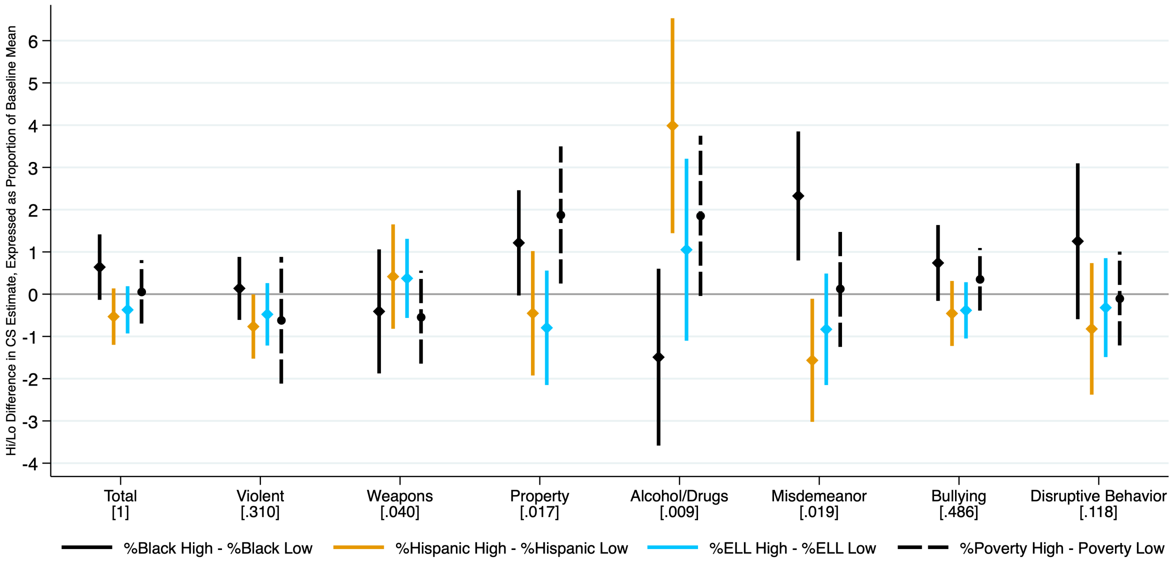
Figure C1: Alternative Controls – TWFE



Notes: We present point estimates and 90% confidence intervals for our DD estimates using a variety of different control schools. Standard errors are clustered at the school level.

C.2 Heterogeneity Analysis

Figure C2: High-Low Differences in DD Estimates Across Demographic Sub-Groups



Notes: We present the difference in point estimates between the high and low demographic sub-groups, and 90% confidence intervals of this difference for our baseline TWFE DD estimates. Standard errors are clustered at the school level. In square brackets under each category label, we present the full-sample proportion of our total crime and behavioral outcomes measure account for by each crime category.

C.3 Ex-Ante Crime Risk Score

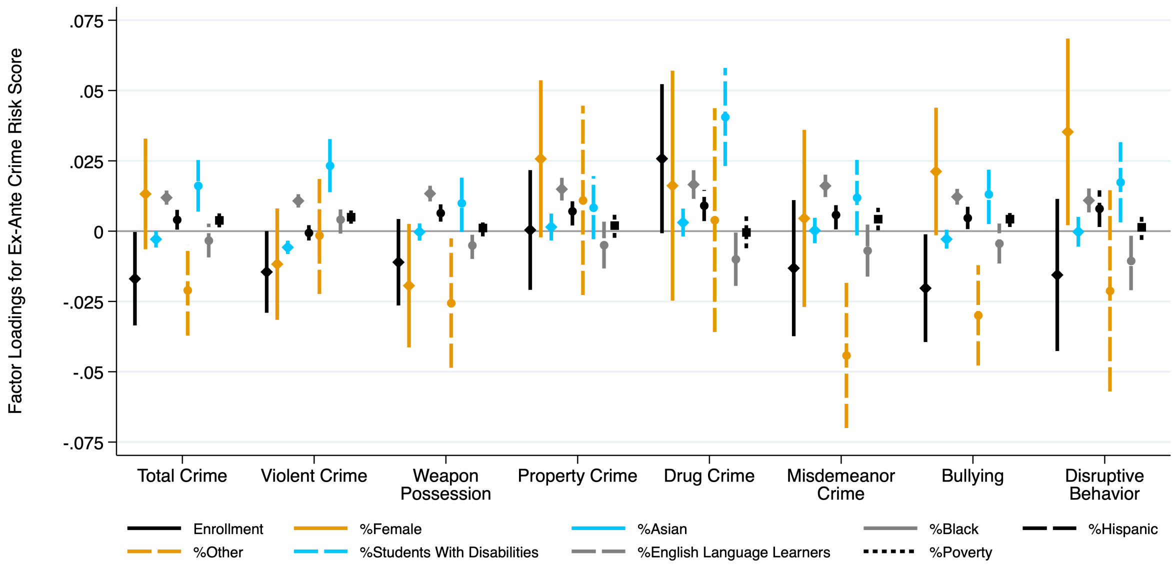
In this section we present the factor loadings on the inputs into our ex-ante crime risk score. Table C2 presents the raw factor loadings for the inputs, whereas Figure C3 presents the factor loadings rescaled by the dependent variable mean in the pre-period for the non-treated – this rescaling allows one to view all the factor loadings on a common scale.

Table C2: Ex-Ante Crime Risk Score Inputs and Factor Loadings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Crime	Violent Crime	Weapon Possession	Property Crime	Drug Crime	Misde-meanor Crime	Bullying	Disruptive Behavior
Enrollment	-.624*	-.128	-.0166	.00028	.00836	-.0114	-.398*	-.0782
	(.372)	(.0782)	(.014)	(.00904)	(.00523)	(.0128)	(.229)	(.0825)
% Female	.488	-.104	-.0291	.018	.00525	.00393	.416	.177*
	(.441)	(.107)	(.02)	(.0119)	(.00806)	(.0166)	(.271)	(.101)
% Asian	-.106	-.051***	-.00044	.00102	.001	.00021	-.0559	-.00109
	(.0656)	(.0129)	(.00278)	(.00203)	(.00098)	(.00238)	(.04)	(.0162)
% Black	.44***	.0953***	.02***	.0104***	.00538***	.014***	.24***	.0548***
	(.0566)	(.0128)	(.00254)	(.00172)	(.00101)	(.0021)	(.0335)	(.013)
% Hispanic	.15*	-.00524	.00966***	.00495**	.00295***	.00502*	.0922*	.0402**
	(.079)	(.0146)	(.00281)	(.00215)	(.00109)	(.00272)	(.0475)	(.0199)
% Other	-.775**	-.0135	-.0383*	.00764	.00127	-.0383***	-.587***	-.106
	(.362)	(.112)	(.021)	(.0143)	(.00785)	(.0136)	(.213)	(.109)
% Students With	.595***	.206***	.0149	.00584	.0132***	.0103	.258**	.0872**
Disabilities	(.205)	(.0508)	(.00938)	(.00477)	(.00344)	(.00707)	(.127)	(.0435)
% English	-.123	.0366	-.00758*	-.00346	-.00324*	-.006	-.0864	-.053*
Language Learners	(.135)	(.027)	(.0044)	(.00353)	(.00187)	(.00488)	(.0849)	(.0319)
% Poverty	.143**	.0443***	.00174	.00141	-.00013	.00373*	.0846**	.00707
	(.0563)	(.0127)	(.00275)	(.00186)	(.00112)	(.00211)	(.0337)	(.0138)
$\overline{Y}_{PRE}^{NT}$	36.9	8.85	1.5	.698	.324	.867	19.6	5.02
R^2	.148	.207	.0959	.0402	.0442	.06	.115	.0335
Observations	3,676	3,676	3,676	3,676	3,676	3,676	3,676	3,676

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. We cluster standard errors at the school level. The Ex-Ante Crime Risk Score is calculated based on the pre-period years of 2009-2013.

Figure C3: Demographic Determinants of Crime Risk – 2009-2013



Notes: We present OLS-based point estimates and 90% confidence intervals for each input into our ex-ante crime risk score measure for the pre-policy period of 2009-2013. Standard errors are clustered at the school level.

C.4 Ex-Ante Predicted Test Scores

In this section we present the factor loadings on the inputs into our ex-ante predicted test score measures. Table C3 presents the raw factor loadings for the inputs. In both columns, the outcome variable is a Z-score, so all the factor loadings have the interpretation: $\hat{\beta}_k$ is the standard deviation change in the outcome variable when control k changes by a unit – for this reason we do not also present scaled coefficients as we do for the ex-ante crime risk scores in Figure C3.

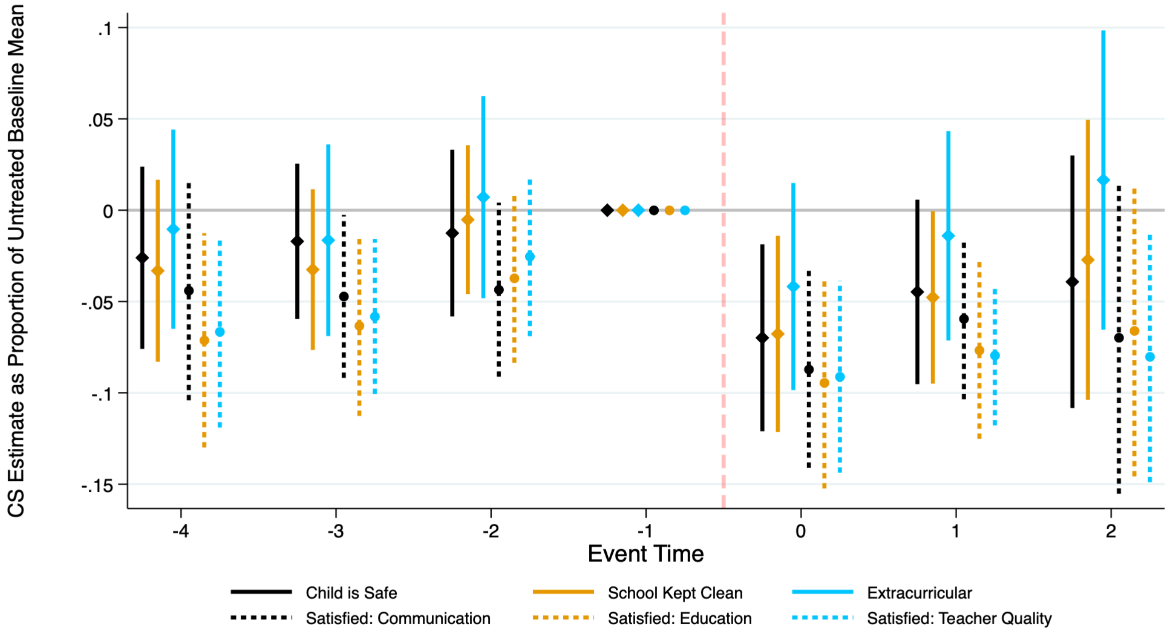
Table C3: Ex-Ante Predicted Test Score Inputs and Factor Loadings

	(1)	(2)
	Math Z-Score	English Z-Score
Enrollment	-.00292	-.00521
	(.00549)	(.00586)
% Female	.0292***	.0429***
	(.00664)	(.00636)
% Asian	.0107***	.00623***
	(.00135)	(.00131)
% Black	-.0157***	-.0138***
	(.00099)	(.00103)
% Hispanic	-.00916***	-.0102***
	(.00118)	(.00118)
% Other	-.00317	-.00032
	(.00592)	(.00539)
% Students With Disabilities	-.0279***	-.032***
	(.00334)	(.00322)
% English Language Learners	-.0117***	-.0157***
	(.00217)	(.00233)
% Poverty	-.0117***	-.0138***
	(.00097)	(.00104)
$\overline{Y}^{NT}_{PRE}$	.0115	.00902
Adjusted R^2	.779	.78
Observations	1,427	1,427

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the school level. The Ex-Ante Predicted Test Scores are calculated based on the pre-period years of 2012-2013.

C.5 School Survey Event Student Results

Figure C4: School Survey – Event Study Graph

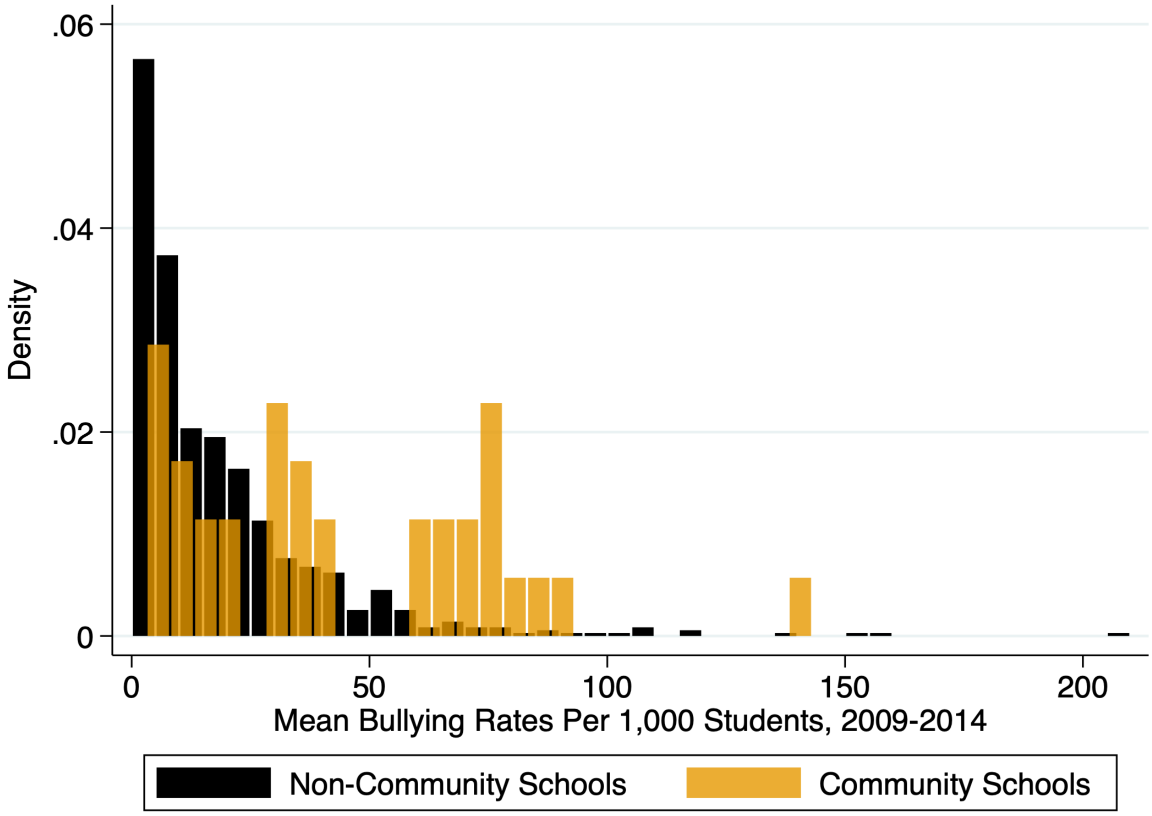


Notes: We present point estimates and 90% confidence intervals for our event study estimates for each of our core school survey outcomes. Standard errors are clustered at the school level.

C.6 The Distribution of Bullying

C.6.1 The Distribution of Bullying Across School Types

Figure C5: The Distribution of Bullying Across School Types



Notes: We present the distribution of school level means of the bullying rate for the period 2009-2014, separately for our control and treated schools.

C.6.2 Distributional Effects of Community Schools on Bullying Rates – Unconditional Quantile Partial Effect Results

The UQPE approach to measuring the distributional effects of the NYC-CS Initiative on bullying takes a very similar form to those that we document in Section 4.5. In this case we estimate a DD regression with $y_{st} = \mathbb{1}[\text{bullying}_{st} < Q_\tau]$. We rescale the resulting quantile-specific DD estimates by the minus one times the density of bullying at the τ-th quantile, $f_{PRE}^{NT}(Q_\tau)$, where once again we use the distribution of bullying rates in non-Community Schools pre-2014. This gives us a local linear approximation of the UQPE at a given quantile.

A caveat to the UQPE approximation is that, as noted by Dube (2019), the local linear approximation is best suited for cases where the treatment is continuous and has substantial variation in treatment intensity, and is less well suited for discrete treatments as in our case. As we show in Appendix Figure C5 above, there are key differences in the baseline distribution of bullying in Community Schools and non-Community Schools. For this reason, the use of the never-treated distribution for the density estimates – $f_{PRE}^{NT}(Q_\tau)$ – will be an imperfect approximation. We present the results UQPE results in Appendix Figure C6 nonetheless, as the scaling of such estimates allow us a better comparison with our core DD results in proportional form, i.e., $CS/\overline{Y}_{PRE}$ in Table 2. We additionally rescale the estimates by dividing by the quanitle-specific cutoff, $c(\tau) = Q_\tau$ for non-treated schools at baseline, in order to facilitate a proportional representation.

Figure C6: Distributional Effects of Community Schools on Bullying Rates

Notes: We present point estimates and 90% confidence intervals for a the impact of Community Schools on bullying from a series of distributional DD regressions. Standard errors are clustered at the school level. The estimates come from a set of regressions where the outcome is $y_{st} = \mathbb{1}[\text{bullying}_{st} < Q_\tau]$ for $\tau = [1, \ldots, 99]$. We apply two scaling factors to the estimates. The first is $1/-f^{NT}_{PRE}(Q_\tau)$. The second is $1/Q_\tau$. This gives the estimates a proportional UQPE representation. The red dotted line in the graph is the baseline (mean) DD estimate, scaled by the mean of bullying for non-Community Schools in the baseline period, and serves as a reference point for the UQPE estimates.

C.7 Empirical Support for the IV Assumptions

C.7.1 Conditional Randomization Test

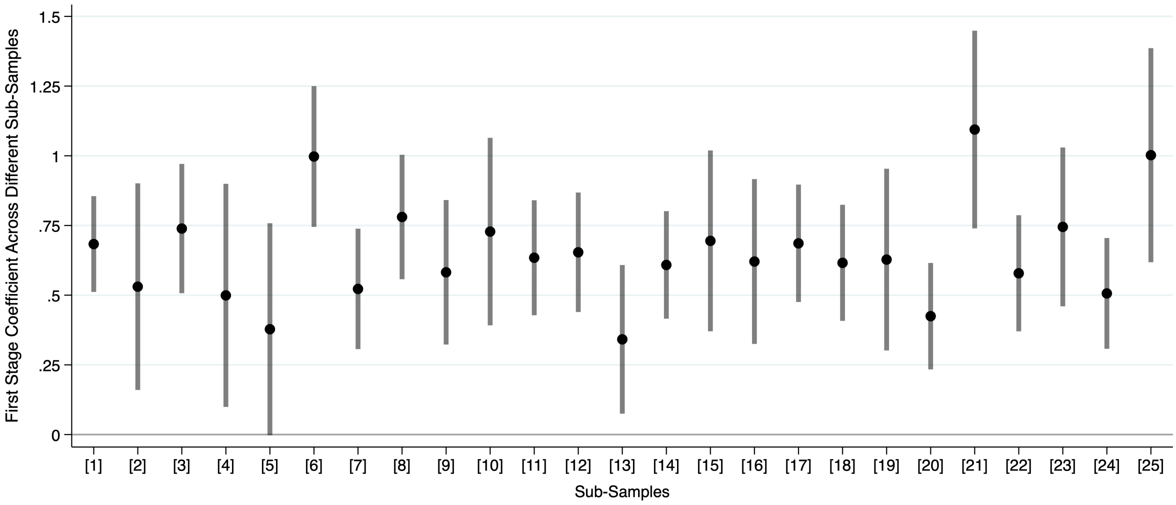
Table C4: Testing for Conditional Random Assignment of our Shift-Share IV

	(1)	(2)	(3)
	Unconditional	**School and Year FEs**	**School and Borough-by-Year FEs**
Enrollment	-.00291**	.00285**	.00253*
	(.00136)	(.0014)	(.00139)
% Female	.453*	-.0417	-.039
	(.235)	(.0435)	(.0426)
% Asian	-.0782***	.0582	.0285
	(.0257)	(.0372)	(.0368)
% Black	.117***	.0918**	.0474
	(.0238)	(.0373)	(.04)
% Hispanic	.00996	.0435	.00403
	(.0265)	(.0382)	(.0379)
% Other	-.0495	.0531	-.00376
	(.104)	(.0523)	(.0522)
% Students With Disabilities	.382***	.0347	.0324
	(.147)	(.0334)	(.0335)
% English Language Learners	.0827*	.0568**	.0282
	(.0483)	(.0264)	(.0264)
% Poverty	.121***	-.00281	-.00167
	(.0234)	(.00362)	(.00359)
Total Expenditure Per Pupil	.596***	-.0545	-.0464
	(.221)	(.0467)	(.0486)
F-Statistic for Joint Test	43.5	1.89	.883
p-Value	[0]	[.0419]	[.549]
Observations	2,091	2,091	2,091

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Eicker-Huber-White standard errors in parentheses. The dependent variable in all specifications is our instrument for bullying – a leave-one-out shift-share instrument. The share component is based on the school-level bullying rate mean for the years 2006-2008. The shift component is based on the annual (leave-one-out) sum of NYC elementary school bullying rates for the years 2012-2016. School and Year FEs are included in Column 2. School and Borough-by-Year FEs are included in Column 3. Due to changes in test scores in 2012/13 school year, the estimation sample is 2012/13-2016/17.

C.7.2 Support for the Monotonicity Assumption

Figure C7: Support for the Monotonicity Assumption – Shift-Share IV



Notes: We present point estimates and 90% confidence intervals for the first stage coefficient on our shift share IV for a variety of sub-samples:[1] Full Sample, [2] Predict Bullying Quartile 1, [3] Predict Bullying Quartile 2, [4] Predict Bullying Quartile 3, [5] Predict Bullying Quartile 4, [6] Enrollment High, [7] Enrollment Low, [8] % Female High, [9] % Female Low, [10] % Asian High, [11] % Asian Low, [12] % Black High, [13] % Black Low, [14] % Hispanic High, [15] % Hispanic Low, [16] % Other High, [17] % Other Low, [18] % Students with Disabilities High, [19] % Students with Disabilities Low, [20] % English Language Learners High, [21] % English Language Learners Low, [22] % Poverty High, [23] % Poverty Low, [24] % Per Pupil Expenditure High, and [25] % Per Pupil Expenditure Low. High signifies above median average for the sample period, low signifies below median. Eicker-Huber-White standard errors in parentheses.