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Off the Books: School-Based Informal Diversion and Student and Community Outcomes

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Efforts to reduce school-based arrest may reshape policing beyond school doors, yet their downstream effects are largely unknown. We study a New York City policy that encouraged informal diversion in place of arrest, using a difference-in-differences strategy. The probability of a juvenile crime record near a school fell by 6.9 percentage points---concentrated among misdemeanor offenses and female students---likely independent of declines in school-based arrests. Yet, diversion increased by approximately 5 times more than arrests fell, consistent with net-widening. Within schools, suspensions increased despite unchanged behavior, suggesting schools substituted internal discipline for arrest. Attendance gains and test-score declines are suggestive.

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Abstract

Efforts to reduce school-based arrest may reshape policing beyond school doors, yet their downstream effects are largely unknown. We study a New York City policy that encouraged informal diversion in place of arrest, using a difference-in-differences strategy. The probability of a juvenile crime record near a school fell by 6.9 percentage points—concentrated among misdemeanor offenses and female students—likely independent of declines in school-based arrests. Yet, diversion increased by approximately 5 times more than arrests fell, consistent with net-widening. Within schools, suspensions increased despite unchanged behavior, suggesting schools substituted internal discipline for arrest. Attendance gains and test-score declines are suggestive.

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1 Introduction

Juvenile arrests, referrals, and other actions that generate a legal record for a juvenile negatively impact their educational trajectory (Sorensen et al., 2025; Aizer and Doyle, 2015). Beyond formal processing, police contact itself may harm student well-being and school engagement: direct police stops, arrests, and even witnessing police encounters have been linked to lower educational achievement, reduced school engagement, elevated psychological distress, and future delinquency (Del Toro et al., 2019; Gottlieb and Wilson, 2019; Jackson et al., 2019, 2022; Wiley and Esbensen, 2016; Wiley et al., 2013). These practices are also concentrated on historically marginalized groups, exacerbating educational inequities. Schools serving larger shares of Black, Latino, and low-income students are more likely to experience intensified police presence (Allen and Noguera, 2023; Holloway, 2021; Gleit, 2022; McGlynn-Wright et al., 2022; Williams III et al., 2023; King and Rodriguez, 2025), and Black, multiracial, and economically disadvantaged students are significantly more likely to receive juvenile complaints for the same school-based offenses (Sorensen et al., 2025). As such, the use of police in schools to address student behavior has become an increasingly contentious practice.

A growing body of research documents the consequences of police presence *in* schools, finding that school resource officers (SROs) and increased police presence are associated with higher rates of school-based arrests, referrals to law enforcement, exclusionary discipline, and criminalization of relatively minor student misconduct (Brady et al., 2007; Holloway, 2021; Na and Gottfredson, 2013; Homer and Fisher, 2020; Owens, 2017; Riesch et al., 2025; Sorensen et al., 2021, 2023; Theriot, 2009, 2016; Weisburst, 2019; Zhang, 2019).[1] However, these studies often apply to SROs—uniformed law enforcement personnel stationed within schools. The role of external police officers (i.e., "beat" officers) in interacting with students is often understudied, as well as the efficacy of implementing alternatives to arrest. Those studies that do exist are largely descriptive. As advocates and policymakers seek best practices for police engagement in schools, understanding how different policies and practices influence student academic and behavioral outcomes is of utmost importance.

The aggregate impact of relying on informal diversion by police is *ex ante* ambiguous. In theory, this practice reduces exposure to stigmatizing legal processes and the collateral consequences of

[1] Some studies also find evidence suggesting that SRO presence may deter serious violence or improve perceptions of safety (Jennings et al., 2011; Johnson, 1999; Soderstrom, 2024; Sorensen et al., 2021, 2023; Theriot, 2009; Zhang, 2019)

formal adjudication. One mechanism linking formal processing to downstream outcomes is labeling theory, which posits that official intervention can alter self-concept, institutional treatment, and future behavior. Wiley et al. (2013) and Wiley and Esbensen (2016) find that police contact increases deviance amplification and weakens school commitment and Vincent and Tobin (2011) describes how repeated low-level institutional sanctions create "labeling hype," whereby minor infractions escalate into cumulative criminalization, internalized criminalized identities, and diminished educational opportunity. If formal justice processing is the primary source of harm, reducing arrests and referrals should improve student outcomes.

Alternatively, if police contact itself is consequential through surveillance or labeling, informal diversion may produce smaller or null benefits despite reductions in formal processing. Further, informal diversion is "net-widening": because informal diversion may be viewed as lower stakes and more rehabilitative, authorities may draw more youth into police contact than they otherwise would have processed (Hoge, 2016; Schlesinger, 2018). In school settings, the presence of police officers may increase detection, documentation, and referral of behaviors traditionally handled by educators, thereby broadening the reach of law enforcement even absent formal arrest (Cocozza et al., 2005; Devlin and Gottfredson, 2018), which, as Sorensen et al. (2023) argue, increases the likelihood that school misconduct enters legal channels by extending surveillance.

In this study, we provide empirical evidence to better understand how informal diversion functions in schools. We examine a 2019 policy change that encouraged the New York Police Department (NYPD) in collaboration with New York City Public Schools (NYCPS) to reduce rates of school-based arrest for minor offenses and to instead rely on informal diversion (referred to as "mitigation" in NYC) to address these incidents. In these cases, a juvenile committed what would amount to a criminal offense but was instead released to the school for discipline rather than being formally processed via arrest. We leverage unique data from the NYPD on school-wide rates of informal diversion and arrest combined with student-level administrative records from NYCPS and publicly available community criminal incident data from the NYPD to understand the impact of relying on informal diversion by police in place of arrest on key student outcomes.

To estimate the impact of the policy, we leverage a dosage difference-in-differences strategy and variation in a student's and school's exposure to the policy—some schools had greater than predicted pre-policy arrest reliance (hereafter, "over-arresting" schools) while other schools had as expected or lower than predicted prior reliance on arrest (hereafter, "under-arresting" schools). Over-arresters were thus more likely to move toward informal diversion post-policy.[2] This measure was used previously in King and Rodriguez (2025) to examine the relationship between over-arrest and school demographic characteristics.

We examine outcomes across multiple domains: in-school behavioral outcomes (suspension and infraction rates), academic outcomes (attendance, math and ELA test scores), and external outcomes (juvenile crime in the immediate vicinity of schools). We find that the 2019 policy change increased informal diversion rates by a magnitude approximately 5 times larger than the corresponding reduction in arrests. We suggest that this phenomenon is consistent with net-widening, as other measures of student behavior within schools (i.e., a school violence index, office discipline referrals) remain unchanged under our primary and more conservative measures of treatment.

We provide evidence that the policy resulted in spillover effects to how police engage with juveniles *outside* of schools. We document a 6.88 percentage point (pp) reduction in the probability of a juvenile crime record in the vicinity of a school that is likely independent of the reduction in arrest rates within schools. We document no robust change in crime reductions for other age groups. This reduction is broadly shared across racial groups, is concentrated among female students, and is largest for minor offenses (misdemeanors and violations). This evidence is congruent with the policy reshaping how police interact with juveniles not only within schools, but also beyond school walls.

Our within-school findings reveal a shift in punishment rather than behavior. Suspension rates increased under both our most intense treatment definition (0.98pp, comparing terciles of over-arrest) and our most conservative (0.51pp, comparing schools with persistent pre-policy arrest reliance), while school violence and infraction rates were unchanged under conservative measures of treatment. We suggest this pattern is consistent with schools intensifying punishment, not necessarily students changing behavior.

Estimates for other within-school outcomes are sensitive to treatment definition: attendance gains, infraction increases, and test-score declines of approximately 0.10sd appear under tercile

[2] NYCPS and the NYPD refer to informal diversion as "mitigated" interventions. These involve an officer being called to address what would otherwise be criminal, arrestable behavior in school, but ultimately results in a student being returned to NYCPS for non-legal consequences. Common reasons for mitigation include marijuana possession, trespassing, or graffiti.

comparisons but do not survive persistent treatment classification, which selects schools on a stable state rather than a single-year peak and is thus less exposed to mean reversion. We therefore interpret increased suspensions as the robust within-school effect of the policy and treat the academic effects as suggestive.[3]

With this study, we provide some of the first causal evidence on the school and community impacts of informal diversion prior to arrest and add to a growing body of work on the consequences of police presence in schools. While prior research has documented harmful effects of SRO presence and school-based arrests on student outcomes (Weisburst, 2019; Owens, 2017; Sorensen et al., 2023), far less is known about the effects of non-SROs, interventions themselves, or the efficacy of alternatives to arrest. We provide some of the first causal evidence on how informal diversion—the most common such alternative—affects student outcomes. Within schools, the reform appears to have shifted how punishment is administered: raising suspensions with no measured change in student behavior, rather than producing robust academic gains. Second, this paper contributes to the literature on juvenile diversion and net-widening. A key concern with diversion programs is that they may expand the reach of the justice system rather than reduce it (Hoge, 2016; Schlesinger, 2018); our finding that mitigation rates increased far beyond the reduction in arrests is consistent with this concern.

We also provide implications for policymakers and practitioners interested in reforming the use of police within schools. These reforms have immense equity implications given that SRO placement and police interventions are disproportionately concentrated in schools serving students of color (Gleit, 2022; King and Rodriguez, 2025). We demonstrate both positive spillovers (reduced community-based arrests outside school) and potentially unintended consequences (net-widening and increased suspension rates) of police reform, such that policymakers may modify policy in the future to limit negative downstream consequences while maintaining the benefits of these changes for historically marginalized groups.

[3] We acknowledge that we are unable to directly parse mechanisms, but due to limited change in behavioral proxies we believe the student behavior response interpretation to be less likely.

2 Background

2.1 New York City Context

The relationship between the NYPD and NYCPS has long been governed by formal agreements that have, over time, reflected broader national shifts in perspectives on school safety and student discipline. For much of the late 1990s and 2000s, NYC school safety policy was anchored in the zero-tolerance disciplinary culture of the Giuliani era, formalized through a 1998 MOU that gave the NYPD broad authority over school safety and offered little guidance on limiting punitive responses to student misbehavior (Zimmerman, 2019). That agreement remained largely unchanged for over two decades despite growing evidence that police involvement in schools disproportionately harmed students of color and contributed to the school-to-prison pipeline (Na and Gottfredson, 2013; Owens, 2017; Weisburst, 2019; Sorensen et al., 2021, 2023).

2.2 The 2019 Memorandum of Understanding

After a three-year collaborative process involving NYCPS, NYPD, and a range of stakeholders, the City of New York finalized a new Memorandum of Understanding governing police engagement in NYCPS (Office, 2019). The MOU was announced on June 20, 2019 alongside a broader school climate initiative and took effect at the start of the 2019–2020 academic year. The agreement was described by the Mayor's Office as "memorializing best practices that the Police Department and the Department of Education have developed over the last twenty years," while the NYC Council Committee on Education characterized it as an important step toward ensuring "there will not be arrests in schools for low-level offenses" (Office, 2019).

The MOU contains several distinct types of provisions relevant to the shift from arrest toward diversion. It explicitly enumerates multiple goals including "eliminating the use of summons and arrests for minor school misbehavior" and "eliminating disparities and inconsistencies in the punishment of students" (MOU, Preamble, p. 3). Second, the MOU delineates roles between school staff and police, directing that "school personnel should not call upon SSD personnel and/or uniformed members of the NYPD to address or respond to non-criminal, minor misconduct where such school personnel can safely address misconduct" (MOU, Section 1, p. 4–5), and requiring that superintendents and principals address non-criminal misconduct using de-escalation and disciplinary supports rather than police involvement (MOU, Section 1, p. 4).

Third, and most consequential for the present study, the MOU contained a formal operational directive encouraging diversion for a variety of low-level school based offenses: low-level marijuana possession, disorderly conduct, consumption of alcohol, trespass, harassment, spitting in public, graffiti, and other low-level (but potentially criminal) offenses that could be handled by school administration. Functionally, this was implemented via the issuing of warning cards in consultation with school principals instead of arrest (Hobbs, 2019; Gould, 2019). Finally, the MOU established oversight mechanisms, including a Joint Committee of DOE, NYPD, and Mayoral designees charged with monitoring and evaluating the joint school security program on an ongoing basis and producing annual evaluations (MOU, Section 2, p. 6). Educators, students, and parents were also participants in this process via a School Safety Community Partnership Committee convened by the Mayor's Office of Criminal Justice (MOU, Section 2, p. 7–8).

2.3 Institutional Reception and Implementation

A key question and one directly relevant to the identification strategy employed in this paper is whether the MOU represented a genuine institutional shift that was adopted and implemented in practice, or whether it was a largely symbolic document that had limited effect on officer behavior. The available evidence suggests the former.

The MOU's durability as institutional policy is notable. The NYC Council's 2023 joint committee report on school safety initiatives explicitly summarizes its arrest-reduction provisions as actively governing school safety practice, including the requirement that School Safety Agents receive 17 weeks of training covering conflict resolution, crisis de-escalation, and collaborative problem-solving (Vasquez et al., 2023). As recently as the 2025–2026 school year, the NYCPS's District-wide Safety Plan continues to actively reference and incorporate the 2019 MOU's arrest-reduction and diversion provisions, listing it alongside Chancellor's Regulations as a key governing document (NYCDOE, 2025).

Taken together, while not legally binding, we suggest that the 2019 MOU was *not* merely a symbolic gesture. It reflected and reinforced an ongoing institutional shift in how both the DOE and NYPD conceptualized appropriate police engagement in schools, was operationalized through specific officer practices and training requirements, and produced measurable increases in diversion.

Indeed, we observe as articulated in King and Rodriguez (2025) and in Section 3.2 large increases in mitigation after the passage of the MOU as well as reductions in arrest, on average.

Crucially, despite the universality of the policy, not all schools were uniformly affected. Because the directive was to reduce arrest reliance broadly, schools that had previously relied on arrest beyond what student behavior alone would predict faced the greatest institutional pressure to change. Schools already operating with lower-than-expected arrest rates had little behavioral adjustment to make. This differential exposure is observable in the data: over-arresting schools drove virtually all of the post-MOU increase in mitigation and decline in arrests (Figure 1). This natural variation in pre-policy arrest reliance provides the basis for our identification strategy, which exploits the MOU as a source of policy-induced variation in the shift from arrest to diversion across NYC public schools.

3 Data Description

3.1 Data Sources

In this study, we rely on multiple sources of infraction-, student-, and school-level data. At the school level, we rely on data from the NYPD that became available in the spring of 2016 as a result of the School Safety Act, which required the NYPD to report on a quarterly basis every intervention by police that occurs in any New York City public school. These interventions include arrests, juvenile referrals, mitigated interventions, and psychiatric interventions ("child in crisis"). We aggregate this data to the year by school level and construct rates of intervention as the number of students experiencing a specific intervention type per 100 students enrolled within a school in a given year.[4]

We merge the NYPD school-level data with that from the citywide mandatory survey on School Safety and Educational Climate (SSEC), which provides an index of school violence as measured by a weighted average of the number of violent and disruptive incidents that occur in a school. We then connect this data with school- and student-level administrative data provided by the Research Alliance for New York City Schools (RANYCS) which contains information regarding student,

[4] This is the same data used in King and Rodriguez (2025). Please see King and Rodriguez (2025) Appendix for full details on data matching and cleaning procedure.

teacher, and staff characteristics. Our primary outcomes of interest from this data are whether a student received an office discipline referral (i.e., infraction) or suspension in a given year, as well as by the severity of the infraction.[5] We also investigate among students suspended in a given year the average length of suspension. This measure is available for students in grades 6-12. We also estimate impacts on annual outcomes, including attendance (available grades K-12) and math and English test scores (available grades 3-8).

Lastly, we use publicly available data on every arrest effected in NYC by the NYPD (including those outside of schools), which contain information on the age group, race, and gender of an offender, the type of offense (misdemeanor, felony, or violation), the crime committed, and the specific latitude and longitude of the offense. Using the latitude and longitude of a school's building, we identify the school that is closest to every documented arrest, specifically arrests for offenders under 18 years old and between 18-24. We further restrict to those arrests that occur within a 0.1km radius of the closest school, which is approximately the distance of a north-south block in Manhattan (0.08km).[6] We further restrict to arrests that occur on a school day, which we derive from publicly available NYCPS school calendars.[7] Ultimately, our final sample contains 67,281 unique arrests.

We then aggregate our arrest data to the offense category by demographic group by year by school campus level. We collapse all potential offense categories in the NYPD data to those identified by The Criminal Justice Administrative Records System: drug offenses, violent crimes, property crimes, public order crimes, traffic offenses, and other unspecified offenses. The authors and research assistants independently sorted these offenses, compared, and reconciled allocation of specific offense types into categories. See Table A1 for allocations to offense groups.

3.2 Sample Descriptions

Panel A of Table 1 lists the relevant summary statistics for under-arrester middle and high schools (Column 1) and over-arrester middle and high schools (Column 2) at the student-by-year level. There are 1,733,186 student-by-year observations in under-arrester schools and 1,303,714 observations in over-arrester schools in our most expansive (attendance) sample. Across all years and

[5] Suspensions for level 1 and 2 infractions were prohibited in 2012.

[6] In the universe of arrests, the median distance to the closest school is 0.24km.

[7] We would ideally restrict to crime occurring within school hours. We do not have data on the time of arrest.

school types, under- and over-arresting schools have similar mean daily attendance rates (89.4% and 88.8%, respectively), average student enrollment durations (1.23 and 1.31 years, respectively), overall percent of students ever suspended (4%), and gender ratios (51% male). Under-arrester middle and high schools have 18% of students classified as IEP-SPED, compared to 20% in over-arrester schools. The racial composition of the student bodies in over-arresting schools does slightly differ from their under-arresting peers: in the former, 21% of students are Asian, 22% are Black, 38% are Hispanic, 17% are White, and 2% are listed as another race or ethnicity. In under-arresting middle and high schools, 17% of students are Asian, 24% are Black, 44% are Hispanic, 13% are White, and 2% are another race. Math and ELA test scores are similar across the treatment categories: under-arrester schools have mean z-scores of -0.01 s.d. in math and 0.00 s.d. in ELA, while over-arrester schools have a mean z-score of 0.01 s.d. in both subjects. All data sources span the 2016/17-2023/24 academic years. The 2020/21 academic year is excluded from all calculations. Due to COVID, math and ELA scores are not available for the 2019/20 academic year. Importantly, suspension and infraction data are only available through 2021/22. Attendance rates and crime outcomes are available through 2023/24.

Panel B displays school-level summary statistics. Unsurprisingly, the two treatment categories differ in the average rates of arrest and mitigation per 100 students. Over-arrester schools have a mean arrest rate of 0.21 students out of every 100, while under-arrester schools average 0.08 students per 100. Under-arrester schools have a mitigation rate of 0.74 students per 100, compared to 1.82 students at over-arrester schools. The probability of juvenile crime surrounding a school is nearly double for over-arresting schools relative to under-arresting schools, with smaller differences in crime probability for other age groups. There are limited differences in the school violence index between over- and under-arresting schools.

4 Empirical Strategy

Our goal is to identify the policy effect of moving towards mitigation and away from arrest on key youth behavior and academic outcomes. Estimation of a causal effect is complicated given that the policy was universal. As such, we construct a measure of treatment intensity based on a residualized measure of arrest rates which we elaborate on in the following section, and which was used in King and Rodriguez (2025). Using this measure, we then employ a dosage difference-in-differences (DiD) strategy comparing student outcomes prior to and after the policy change in over-arresting schools relative to under-arresting schools.

We recognize that this is not a canonical DiD setup with a clean comparison group; however, relying on variation in treatment intensity has emerged as common in the literature to derive causal effects (Ballis and Heath, 2021; King, Jo, 2024; Craig and Martin, 2025). Further, as noted in Callaway et al. (2024), dichotomizing treatment into high- and low-dose groups identifies effects under parallel trends, but estimates are interpreted as relative to the least-treated group rather than a true counterfactual of no treatment. Continuous treatment specifications recover clean causal parameters only under the stronger assumption that treatment effect heterogeneity across dose groups is ruled out, motivating our use of a dichotomized measure of treatment.

We outline our estimation strategy in the following sections. We first describe the construction of our measure of treatment. We then describe the difference-in-differences strategy.

4.1 Constructing a measure of over-arrest

Our goal is to construct a measure of treatment that identifies which schools in NYCPS were arresting an excess number of students prior to the policy change (i.e., "over-arrest"). Put simply, we want to remove the variation in arrest rates that is due to differences across schools in student behavior, characteristics, and baseline policing rates. We thus define "over-arrest" as the rate of arrest being higher than expected, given proxies for student behavior—that is, observable measures of youth delinquency across schools.[8]

To construct this measure, we use the last year prior to the policy change (2018/19 academic year) to residualize rates of arrest on school-level suspension and ODR rates, a school violence index, rates of other policing interventions, average school attendance, student income level, and percent of students new to a school. We do so for middle and high schools, and separately for K-5s and K-8s given large differences in the rates of interventions across these school types.[9]

[8]This approach was used in King and Rodriguez (2025) to understand over-policing generally in NYCPS as it relates to school racial composition.

[9]Ultimately, we only estimate impacts for middle and high school students given the limited number of interventions that occur in these schools at baseline and that middle and high schools were the only schools that increased mitigation in addition to reducing arrest. Further, we do not have access to suspension records for K-5 students and the use of suspension for grades K-2 is prohibited. Students 12 or younger (i.e., K-5 students) are also generally not allowed to be arrested.

Formally, we use 2019 data and OLS to estimate:

$$ArrestRate_s = \beta + \nu X_s + \epsilon_s, \tag{1}$$

and we predict residuals as:

$$\epsilon_s^* = ArrestRate_s - \hat{\beta} - \hat{\nu} X_s \tag{2}$$

For our final measure of interest, we dichotomize the residualized arrest rates as follows:

$$Overarrest_s = \mathbb{1}[\epsilon_s^* > 0] \tag{3}$$

with our ultimate "treatment" variable being an indicator variable for a school that has a higher than predicted rate of arrest in the year immediately preceding the policy.[10] We opt to dichotomize our measure to allow for a more flexible first stage and greater ease of coefficient interpretation. Further, there is no ex ante reason as to why we might expect residualized arrest rates to affect our outcomes of interest in a linear fashion. We also estimate effects comparing the top tercile to the bottom tercile of over-arrest.

4.2 Difference-in-Differences

We rely on a difference-in-differences strategy to derive the combined policy impact of reducing arrest and increasing mitigation in NYCPS. We rely on the fact that some schools relied on arrests above and beyond what would be commensurate with severe student behavior, and were thus forced to move away from arrest towards mitigation more so than schools that were less reliant on arrests. This is encapsulated by our over-arrest measure. We show that this is indeed the case in Figure 2. Specifically, middle and high schools more treated by the policy reduced their arrest rates by 0.14 per 100 and increased rates of mitigation by 0.77 per 100 enrolled than those under-arrester schools. In the average middle or high school, this corresponds to 4 more mitigated interventions and 0.75 fewer arrests.[11] We further show that there is no change pre- to post-policy in mitigation or arrest

[10] Because talks about updating the MOU had been occurring during the years prior, we elect to use the year immediately preceding the policy to capture those schools who are less likely to experience any anticipation of the policy (i.e., are still over-arresting).

[11] We opt to take a simple pre-/post- DiD design in place of an event study to increase power—our measure of treatment is relatively small. Nevertheless, we estimate event studies for all primary outcomes and present these

rates for under-arresting schools—increases (decreases) in mitigation (arrests) are monotonically increasing with residualized arrest rates (Figure 1).

Our primary specification is in line with the "binarized" estimator suggested by Callaway et al. (2024), which estimates the average treatment effect on the treated aggregated over the dose distribution under a parallel trends assumption. This requires only that pre-treatment outcome trends do not vary systematically with pre-policy arrest reliance.

Empirically, we estimate the following equation:

$$Y_{isgt} = \beta + \nu X_{isgt} + \phi \mathbb{1}[Overarrest_s \times (t > 2019)] + \gamma_t + \alpha_s + \epsilon_{isgt} \tag{4}$$

where Y_{isgt} is our outcome of interest, X_{isgt} are controls for student characteristics (i.e., race, sex, grade, disability status, and the number of years a student had attended a school), and γ_t and α_s are year and school fixed effects, respectively.[12] Our coefficient of interest is ϕ, which indicates the difference in outcomes between students in more treated schools and less treated schools, pre- to post-policy. We interpret ϕ as the effect of the policy on student outcomes.

We estimate a similar model at the school level to derive the impact on crime surrounding a school. We estimate:

$$Y_{st} = \beta + \psi \mathbb{1}[Overarrest_s \times (t > 2019)] + \zeta X_{st} + \gamma_t + \alpha_s + \epsilon_{st} \tag{6}$$

where all parameters retain their same meaning, but are at the school by year level, and X_{st} is a vector of school-level controls for student, school, and teacher characteristics.[13]

In addition to our measure of over-arrest, we estimate impacts using a persistent over-arrester definition—schools that over-arrested in every pre-policy year—minimizing the potential for reversion or anticipation driving results. This pattern may also be expected due to the fact that the MOU's creation was a three-year long collaborative process between the NYPD and NYCPS. As

results in Appendix B

[12]We also estimate event study models (Appendix B) to address the parallel trends assumption:

$$Y_{isgt} = \beta + \nu X_{isgt} + \sum_{k \neq 2019}^{2024} \phi_k \mathbb{1}[Overarrest_s \times (t = k)] + \gamma_t + \alpha_s + \epsilon_{isgt} \tag{5}$$

[13]We include student and teacher racial composition, sex composition, percent with disabilities, percent in poverty, pupil-teacher ratio, and teacher turnover.

such, schools and officers may have already started to practice warn and release prior to the official passage of the MOU.

Generally, our estimates reflect impacts of treatment measured at the school level (i.e., school-wide intervention rates) on student- and school-level outcomes. As such, we would expect small impacts as we are not simply measuring the targeted impact on a student who is intervened upon. Instead, we are measuring the average impact of the intervention across *all* students in a school, meaning diffuse effects are likely small.

5 Results

5.1 School-Based Arrest and Diversion

Figure 2 displays trends over time and difference-in-differences estimates for school-based police interventions. We observe in Panel A (interrupted time series) that, with the exception of the 2019/20 academic year, policing interventions generally—and mitigation specifically—increased significantly and persistently over time. Arrest, in contrast, experienced declines post-policy but, by 2024, is no different from pre-policy arrest rates.

When performing our difference-in-difference analysis (Panel B), we observe that over-arresting schools experienced persistent increases in mitigation rates (0.77 per 100) and reductions in arrest (0.14 per 100). While we observe decreases in arrest in the post-period, there is some evidence (Figure 2, Panel B) that this decline may be partially a product of mean reversion or anticipation of the policy, as arrest rates in over-arresting schools rise in the years immediately preceding the MOU. We address this concern directly in Sections 5.2.1 and 5.3.1. We discuss the presence of parallel trends in our primary outcomes in the ensuing sections and document evidence of their parallel trends in Appendix B.

5.2 Student-Level Outcomes

Table 2 presents estimates regarding the impact of the policy on attendance rates (Panel A) and test scores (Panels B and C). We find that students, on average, experienced increased attendance by 0.5 percentage points (pp) ($p<0.05$), which equates to $\sim$1 more day in school, on average, across all students. We observe that this effect is driven by Black students (0.6pp, or 1.08 days), male students (0.56pp), and students with disabilities (0.8pp, or 1.44 days), with a positive but imprecise estimate for Hispanic students (0.47pp). We find this unsurprising as students of color and male students are over-represented in police interventions in NYCPS (King and Rodriguez, 2025), and students with disabilities are also historically subjected to police intervention and exclusionary discipline (Hurwitz et al., 2021; Welsh and Little, 2018a,b). We also estimate impacts on math and ELA test scores, which, for our analytic sample, are only available in grades 6-8. We find suggestive but statistically insignificant evidence of potentially meaningful declines in math and ELA test scores (0.031sd and 0.010sd, respectively).[14]

We then estimate the impact of interventions on infraction (Table 3) and suspension likelihood and length (Table 4) for middle and high school students. We conceptualize any changes in infractions as an imperfect proxy for student behavior, whereas we conceptualize suspension as suggestive of changes to punishment.[15] We find that students in schools more affected by the policy experienced no significant change in suspension or infraction rates, regardless of severity level.[16] In fact, we find a precisely estimated null effect on suspension for the most severe offenses, which are largely reserved for criminal offenses. We further find no change in the average days a student is suspended. It should be noted, however, that when using a more intensive measure of treatment (i.e., terciles) we do observe increases in infraction and suspension rates.

5.2.1 Robustness

We conduct four primary robustness checks to lend credibility and provide nuance to a causal estimate. First, we assess robustness to three different measures of treatment. Second, we construct a treatment measure that identifies schools that only experienced arrest reductions, with no change in mitigation, which lets us try to parse which change (increased mitigation or arrest reduction) drives our results. Third, we assess the influence of policy-induced composition changes. Lastly, we assess the plausibility of parallel trends.

[14]We do find evidence of test score reductions when defining more intense measures of treatment, i.e., tercile comparisons, although these estimates are subject to racial composition changes.

[15]Both of these measures are subject to two forms of error: they fail to capture unreported forms of misbehavior/infraction and are reflective of subjective staff interpretations and decisionmaking processes of observed student behavior.

[16]NYCPS categorizes disciplinary offenses into 5 levels of severity: Level 1 (uncooperative/non-compliant); Level 2 (disorderly); Level 3 (disruptive); Level 4 (aggressive or injurious); Level 5 (dangerous or violent).

5.2.2 Treatment definitions

We define three additional measures of treatment—having at least one school-based arrest in 2019,[17] comparing schools in the top tercile of residualized arrest to the bottom tercile, and, lastly, comparing schools that were over-arresters in all pre-policy years to those that were under-arresters in all pre-policy years. Relying on schools with >0 arrests allows for a clean, easily interpretable comparison group, relative to our primary residualized measure. Terciles of treatment pull from the tails of our residualized arrest distribution, providing a more intense level of treatment relative to above/below zero. Lastly, our persistent over-arrester classification selects schools on a stable state, making it immune to anticipatory early-adjuster selection (these schools never adjusted during the MOU negotiation) and are minimally exposed to mean reversion. Results surviving under persistent-over vs persistent-under therefore bound both of these concerns. Figure 3 demonstrates the impact of these treatment definitions on post-policy arrest rates and mitigation rates. We observe for all alternative treatments a similar pattern as with our primary measure, where arrests decrease and mitigation rates increase.

We find that, generally, our primary results hold, increase in magnitude, and become more precise when using >0 arrests and tercile treatment. As shown in Columns 2 and 3 of Table 2, when defining a more intense treatment measure (i.e., comparing to zero arrests or top to bottom tercile of residualized arrest rates), effects on attendance (Panel A) are generally larger and more precisely estimated. For math and ELA scores (Panels B and C), we find that with more intense treatment measures we identify statistically significant reductions in math and ELA test scores by approximately 10% of a standard deviation, with magnitudes being relatively similar across all subgroups. However, we do observe that for schools that persistently over-arrested, relative to schools that persistently under-arrested, there are no changes in outcomes, suggesting that the effects that we observe may be due to an already existing trend or anticipation of the policy.

When using tercile treatment, we also detect increases in infraction rates (∼2.4pp), dominated by lower-level infractions (Table 3). Similar to our attendance and test score results, these findings are not consistent when using persistent over-arresting as our measure of treatment. Notably, however, when comparing persistent over-arresters to persistent under-arresters, suspension in-

[17] The median arrest rate in 2019 in NYCPS was 0.

creases remain positive (0.51pp) and about half the size of our tercile estimates (0.98pp). These increases are dominated by Hispanic students, students in poverty, and students with disabilities, especially for lower-level infractions. Because persistent classification selects schools on a stable over-arresting state rather than a transient 2019 peak, we read the suspension effect as robust to concerns of misclassification and mean reversion, whereas attendance, test score, and infraction effects are concentrated among schools classified at a peak and may partly reflect transitory disruption and recovery.

5.2.3 Decomposing the role of mitigation and arrest

We then construct a measure of "over-mitigation," which is analogous to our measure of over-arrest. Instead of residualizing pre-policy arrest rates, we instead residualize pre-policy mitigation rates. The intuition behind this measure is that schools that had greater than predicted rates of mitigation in the pre-policy period should be relatively unaffected by the MOU, as they are likely to not substitute further toward mitigation since these schools were already heavily reliant.

As shown in Figure 4, we observe that both under- and over-mitigators experienced no change pre to post policy in rates of mitigation. Yet, those schools that were heavily reliant on mitigation in the pre-period still experienced declines in arrest rates. We find this plausible as the directive from the MOU is also to generally reduce arrest reliance, so over-mitigators may still reduce their rates of arrest without changing mitigation. This exercise helps us understand how schools that experienced only reductions in police interventions may fare.

When examining these schools that are no longer arresting, but also not increasing mitigation, we show that these schools increase their use of within-school discipline. Table 5 presents these results. There are positive, but imprecise, changes in attendance, slight increases in low-level infractions and suspensions, on average, and reductions in the average number of days suspended in over-mitigator schools. Given observable increases in suspension and infraction rates in over-mitigators (i.e., when only reducing arrest reliance and not substituting mitigation), it may be the case that schools increase in-school disciplinary consequences when arrests are no longer occurring.

5.2.4 Composition Changes

We then perform a student demographic balance test across the pre- and post-MOU periods to rule out composition changes driving the results. The pre-MOU period spans the 2016/17-2018/19 academic years, while the post-MOU period spans 2019/20-2023/24 academic years (the 2020/2021 AY is excluded from all outcome calculations). Table 6 presents the difference-in-difference estimates for over-arrester middle and high schools in the post-MOU period, as well as when comparing the top tercile of over-arresters to the bottom tercile. Being a post-MOU over-arrester school does not impact any student demographic characteristics except for a very slight decrease in the proportion of male students. Using our primary measure of treatment, we do not observe composition changes for our test score sample either.

We do, however, observe racial sorting when comparing top tercile of over-arresters to the bottom tercile. We find top terciles have 0.9pp more Black students and 1.19pp fewer White students in our full sample. In our test score sample, there are 1.65pp more Black students and 2.35pp fewer White students. Aside from the slight decrease in the proportion of male students also present under our primary measure, we observe balance on all other dimensions. As such, aggregate estimates regarding test scores derived from our tercile measure should be interpreted with caution, and more weight should be applied to our subgroup analyses.

5.2.5 Parallel trends

Lastly, we assess the plausibility of parallel trends via the event study estimates presented in Appendix B. Figures B1 and B2 show stable differences in trends for all outcomes except attendance, as such, we further interpret our results regarding attendance increase with caution.

5.3 Community Crime Outcomes

We then examine the impact of the policy change on crime surrounding a school (Figure 5). Our measures are at the school level, with our outcomes being the probability that at least one crime of a certain type occurred within a 0.1km radius of a school.[18] We first show that the probability

[18] We rely on indicator variables for two primary reasons. First, it is unclear how to convert the number of instances to a rate, given there is no direct mapping of population within 0.1km of a school. Second, arrests are low-incidence with 95% of schools having less than 3 near-school arrests, meaning we lose little information through dichotomizing and avoid issues with count variables.

of reported crime surrounding a school decreases most sharply for juveniles (<18; 6.9pp, $p<0.01$). We estimate a smaller, marginally significant decrease for the 18-24 age group (4.5pp, $p<0.05$); however, this estimate does not hold under any alternative treatment definition (Table 7, Columns 2-4). Decreases for older age groups are statistically insignificant. While juvenile crime is decreasing, at the same time there is no change in a school's violence index (SVI), which is determined by a school's rate of violent incidences occurring within a school. We suggest that this indicates a limited change in student behavior, with effects deriving from police- or school staff side responses to the policy.

We then explore heterogeneity by demographics and crime type in juvenile crime surrounding a school (Figure 6). We find that crime reductions are driven by Black youth (8.0pp, $p<0.001$), Hispanic youth (5.6pp, $p<0.01$), and White youth (3.0pp, $p<0.01$). Crime reductions are also concentrated on females (7.4pp, $p<0.001$). In terms of offense type and consequence, reductions are driven by lower probabilities of violent incidences (8.2pp, $p<0.001$), drug (3.1pp, $p<0.05$), and public order (3.6pp, $p<0.05$) offenses. We find reductions are entirely among misdemeanor offenses (9.4pp, $p<0.001$) and no change in felony offenses, which, given their level of severity, may not allow police to be as lenient with juvenile offenders.

In Appendix B, we demonstrate that these results are not a product of differential trends in juvenile crime. We observe flat pre-trends for most outcomes, and further, we observe that the reductions in crime are immediate and sustained for Black students, female students, misdemeanors, and violent offenses, pointing to the lasting impact of the policy on redefining the relationship between youth and police.

Together, we take these results as suggestive evidence that greater leniency in police interactions within schools may have spilled over into how police interact with youth outside of schools. The fact that reductions in interventions are driven entirely by females is in line with the documented phenomenon of lenient treatment for female offenders by police and throughout the sentencing process (Visher, 1983; Starr, 2015). Further, reductions are entirely for less severe offenses (i.e., misdemeanors and violations) which demand more leniency than felony offenses. Lastly, despite White juveniles being least likely to have an arrest record near a school, they experience the largest reduction in crime probability off the baseline. The gap in White juvenile crime probability between over- and under-arresting schools was 3.39pp in 2019, which is nearly eliminated in the post-policy period. In contrast, the gap for Black juveniles was 15.7pp, which was significantly reduced (8.0pp), but not eliminated, in the post-policy period. As such, we suggest that the policy influenced police behavior outside of schools, primarily through increasing leniency for specific "less-threatening" subgroups (female juveniles and White juveniles).

5.3.1 Robustness

We conduct four primary robustness checks to ensure that (1) the results we observe are indeed due to the policy change and (2) largely independent of changes to arrest rates within schools. First, we assess robustness to our three different measures of treatment. Second, we estimate two falsification tests by estimating impacts on crime further away from schools and on non-school days. Lastly, we conduct a placebo test that relies on the over-mitigation measure constructed in Section 5.2.1, which identifies schools that only experienced arrest reductions, with no change in mitigation. We also assess the plausibility of the parallel trends assumption.

5.3.2 Treatment definitions

We find our crime outcomes to generally be robust to different treatment definitions. We display these results using different treatment definitions in Columns 2-4 of Table 7. We find our result regarding reductions in juvenile crime to be robust across all specifications and of similar magnitude.[19] We still observe stability in reductions in arrest by racial group, with the exception of negative, but statistically insignificant decreases in crime for Black youth when comparing top to bottom terciles. Reductions in juvenile arrests for females are markedly stable, too. Reductions in misdemeanor offenses (and no change in felonies) also remain stable and reductions in violations remain the same magnitude but are less precise.

5.3.3 Falsification

We present results regarding our falsification exercises in Table 8, Columns 1 and 2. We find limited changes in crime greater than 0.1km from a school. Effect sizes are insignificant and small in magnitude for juvenile crime, on average. We still see reductions for groups most exposed to policing historically (Black juveniles), as well as for drug offenses. These reductions in general align

[19] The finding of reductions in crime for 18-24 year olds is not robust.

with community spillover impacts of greater leniency in within-school interactions with police. It is also in line with Jones and Karger (2023)'s investigation that finds reported juvenile crime increases surrounding school areas at the start of the school calendar year, with smaller, less significant increases in reported juvenile crime occurring at further distances from a school.

We find slight decreases in crime occurring on non-school days for juveniles (4.0pp, $p<0.05$) with a magnitude that is ∼60% of our primary estimate. We find this plausible as New York City often relies on schools as community centers, spaces where students may receive social services, and sites for extracurricular activities even when school is not in session. As such, crime may still decrease post-policy even on non-school days. Further, evidence suggests that police patrol patterns respond to "hot-spots" (Braga et al., 2019), with police resources being allocated to areas with concentrated incidents and higher levels of crime (Wu and Lum, 2017). Therefore, we may expect that given lower levels of police activity on school days, patrolling patterns may shift away from these areas generally, resulting in another mechanism for reductions in crime on non-school days.

We then move to assessing changes in crime in relation to our measure of over-mitigation. In the context of juvenile crime, this helps us assess whether these measures are simply picking up on school-based arrests. Because over-mitigators only decreased arrest with no increase in mitigation (Figure 4), these schools experienced limited post-policy change in how police interacted with students. If the NYPD crime-wide data were picking up on school-based arrests, we should still expect decreases in juvenile crime when using our measure of over-mitigation.

We display the results using over-mitigation treatment in Column 3 of Table 8. We find no change in the probability of juvenile arrest surrounding over-mitigating schools, with the exception of a small increase in the probability of an "other" offense (2.1pp, $p<0.01$). We suggest that, overall, this indicates the measure of arrest probability is primarily detecting arrests outside of schools—if this measure was also detecting within-school changes in arrest, we should expect reductions in NYPD arrest records even when using over-mitigation. This is not the case.

5.3.4 Parallel Trends

Lastly, we provide strong evidence for parallel trends in Appendix B. Across nearly all crime outcomes, we observe at minimum, statistically insignificant pre-policy difference-in-differences coefficients and, in many cases, these estimates being exceptionally close to zero. As such, we provide evidence of limited diverging trends that would account for our primary results.

6 Limitations and Directions for Future Research

This study is not without limitations. First, we are unable to derive a clean comparison group in that the policy affected all students. Instead, we must rely on an imperfect measure to identify those schools that were more treated by the policy. Our strategy of residualizing arrest rates assists in parsing the variation in arrests that is due to across-school differences in student propensities for delinquency. We create a better defined measure of treatment and, further, find our primary results regarding community crime are not sensitive to treatment definition.

Second, our measure of treatment is at the school level due to data limitations. As such, the effects that we are able to detect represent not only the direct effect of the policy on a student who, instead of being arrested, received a mitigated intervention, but also any spillover impacts of the intervention on other students in the school. The effects that we do detect are, thus, exceptionally small, but indicative of average effects on every student in a school. We find these estimates to be exceptionally policy relevant as they indicate aggregate effects. Future work should seek to identify targeted impacts of mitigation on student outcomes.

Third, we are limited by the coarseness of our arrest data—NYPD arrests are not disaggregated based on location. However, given the results from our robustness check with over-mitigators we suggest that this data is largely reflecting crime surrounding schools. Fourth, as with many studies regarding student behavior and academic outcomes, it is difficult to discern whether the effects we observe are due to changes in student, educator, or police behavior. We suggest that this be a fruitful direction for future research.

Lastly, our data is limited in its ability to assess impacts during COVID years as police interventions fell to nearly zero during this time. Additionally, this reform occurred at the same time as the post-2020 Defund the Police movement. Both COVID and the Defund the Police movement may intersect with trends in crime that could pose threat to identification. However, given that we do not see uniform change in crime rates across age groups or types, or in arrests further from the school, we find it unlikely that these events are driving the results documented in this study.

7 Discussion

In this paper, we show that cross-agency agreements that lack legal binding may still achieve their intended first-order purpose. Second, we further document the phenomenon of net-widening in a school-based policing context. Third, we document positive, cross-sector spillovers in policing behavior. And, lastly, we point to potential pitfalls of policing reform.

The 2019 MOU was not simply a symbolic gesture. Instead, it resulted in measurable changes in how student behavior was addressed in schools. Schools that had previously relied most heavily on arrest substituted toward mitigation following the policy and continue to do so. Importantly, this substitution occurred in a context that has historically been responsive to reforms to student behavior management. However, these results may not be generalizable to other contexts. Yet, this finding suggests that formal interagency agreements, even absent legal enforceability, can serve as credible commitment devices when accompanied by training requirements, oversight mechanisms, and institutional buy-in from both parties. For advocates and policymakers seeking to reform school policing without the leverage of legislation, this is an encouraging result.

However, the policy-induced substitution away from arrest towards mitigation is not a purely optimistic story. We find patterns of net-widening, with rates of mitigation increasing at a magnitude approximately 5 times larger than the corresponding reduction in arrests, with the overall rate of police intervention rising rather than falling. Rather than simply substituting arrest for mitigation, the policy appears to have expanded the reach of the justice system by drawing more students into police contact under the label of mitigation. Whether students who receive a mitigated intervention experience lasting consequences in terms of labeling, surveillance, or future contact with police is a question that we cannot answer in this paper, but one that future work may explore. If mitigation carries costs similar to, or even less than, those documented for arrest, then the expansion of mitigation to the level that we observe in this study may offset some or all of the documented benefits.

Despite net-widening, we also observe positive spillovers into how police interact with juveniles outside of the education context. We demonstrate that juvenile crime surrounding a school decreases substantially, notably for groups that may receive more leniency in policing (i.e., female juveniles) and for offenses that may allow more discretion (i.e., misdemeanors). As such, this work speaks to how reforms targeted at improving school-based policing may influence how police interact with juveniles more generally, even outside the schooling context and demonstrates the power of cross-agency collaboration in addressing the school-to-prison pipeline.

Lastly, the reform's within-school impacts appear to influence punishment rather than student behavior. Suspension rates rose robustly across more intense treatment specifications—including our most stable classification—while recorded infractions and school violence were unchanged. We suggest this to be congruent with discipline intensifying conditional on behavior. Two independent exercises point to the same mechanism: schools that lost arrest as a response, whether measured by persistent over-arrest or by over-mitigation, substituted toward internal discipline. Attendance gains, by contrast, are not robust to stable classifications of treatment. Students diverted from the justice system's books may thus land on the school's: the policy reduced formal legal consequences while expanding both police contact and, in some cases, exclusionary discipline, a trade-off policy-makers designing arrest-diversion reforms should weigh explicitly. These unintended consequences mirror those documented in settings of disciplinary reform (King, Jo, 2024).

These findings underscore the complex nature of police and disciplinary reform. The citywide policy change reduced severe forms of police interventions in *and* out of school. Yet, the increased informal contact with police via mitigation may come at the cost of increased disciplinary action, with limited benefit to academic outcomes. As such, we provide evidence regarding the within-school costs of net-widening, as well as directions for policy-makers wishing to reduce unintended consequences of a generally successful reform.

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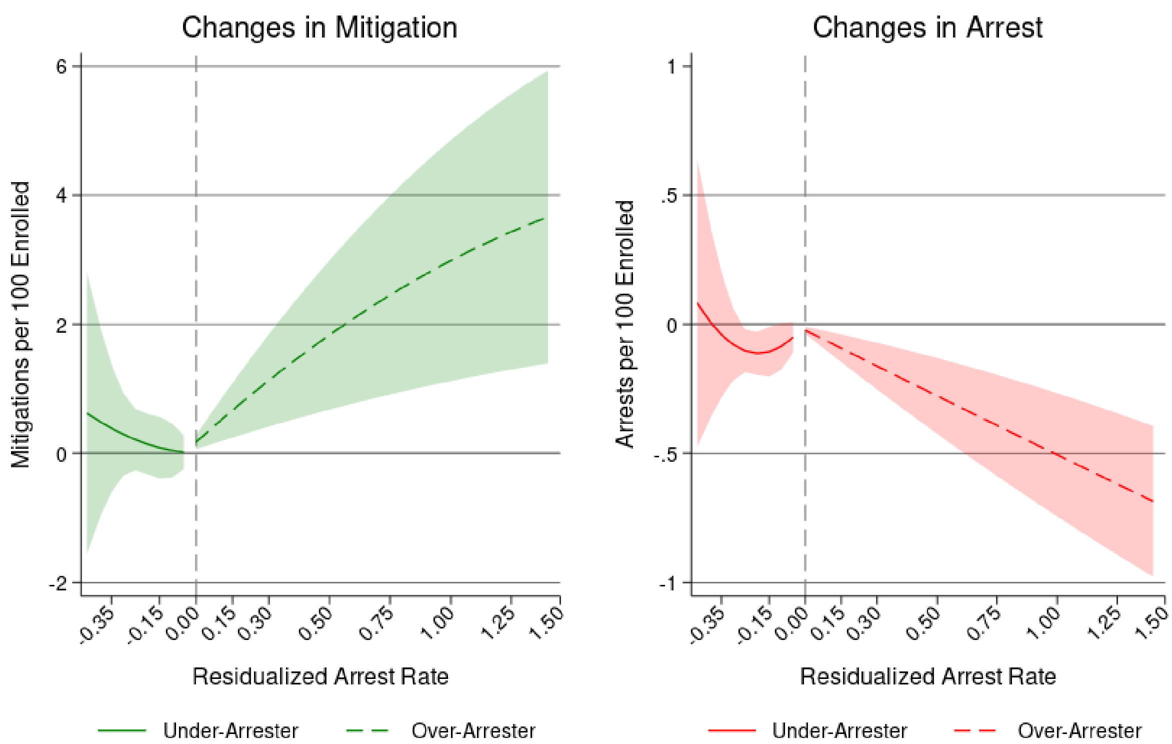
8 Figures and Tables

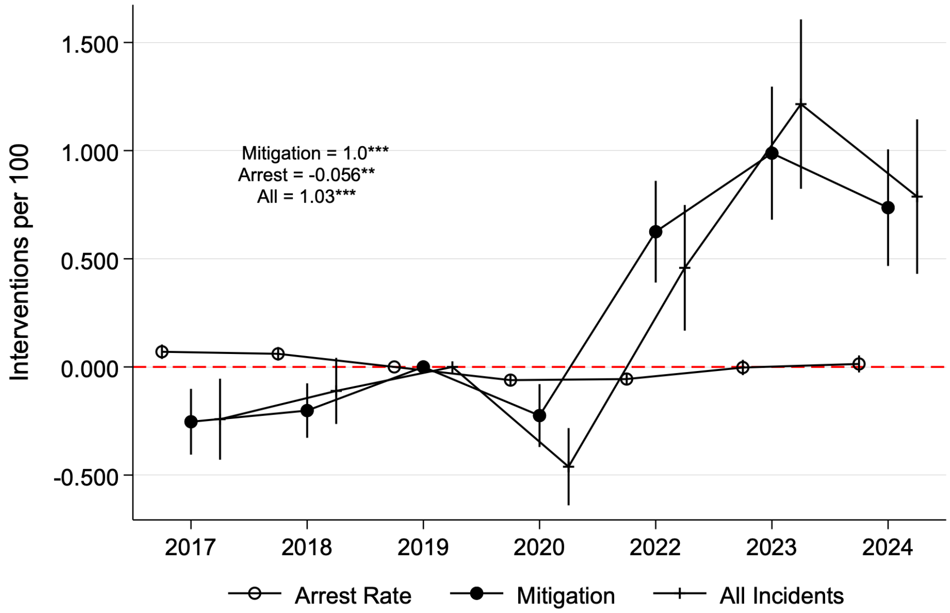
Figure 1: Continuous Treatment and Change in Mitigation and Arrest Rates



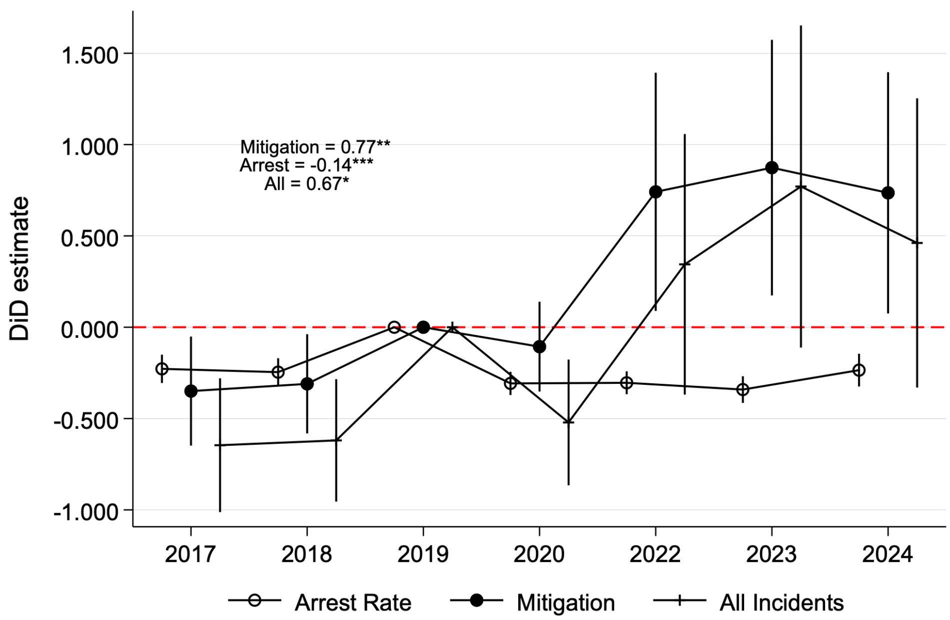
Notes. This figure presents estimates of the change pre- to post-policy in rates of mitigation or rates of arrest. 2019 residualized arrest rates are on the x-axis, with schools of residuals less than 0 being considered underarresters and those with residuals greater than 0 being considered over-arresters. Estimates are derived separately for under- and over-arresting schools. 95% confidence intervals are provided.

Figure 2: Changes in Arrest and Mitigation Rates

(a) Interrupted Time Series

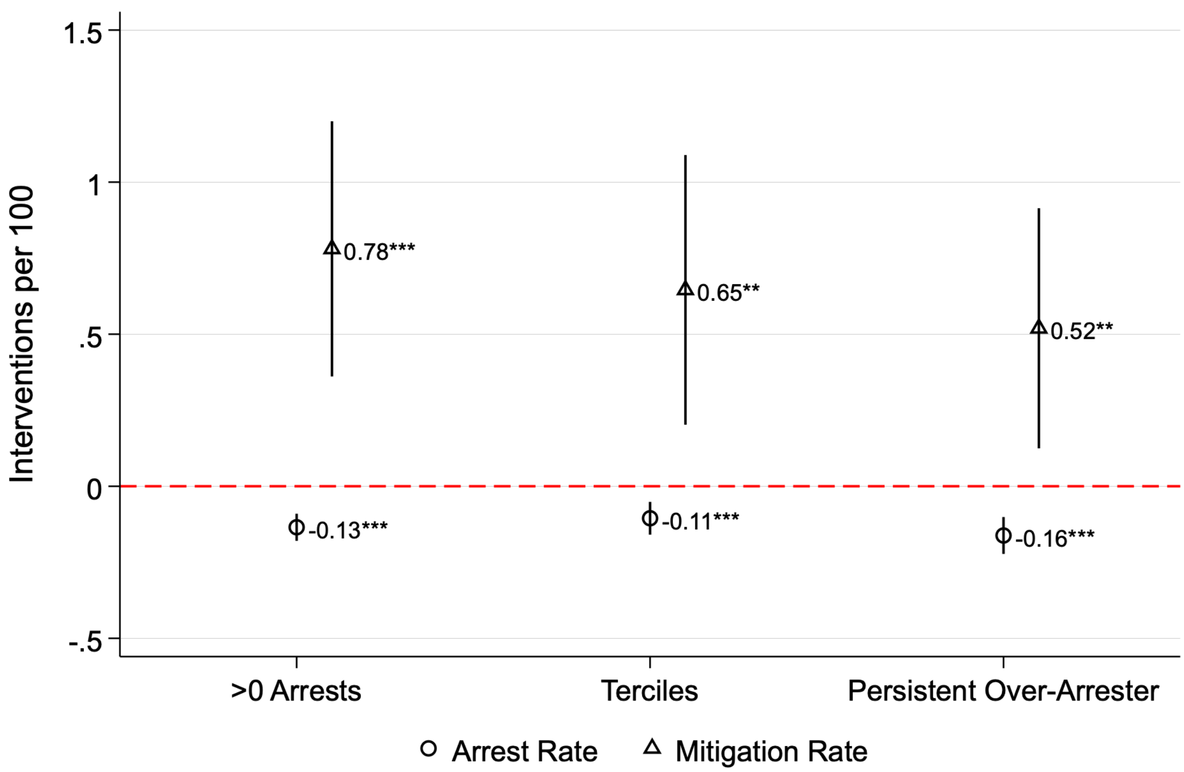


(b) Event Study: Grades 6-12



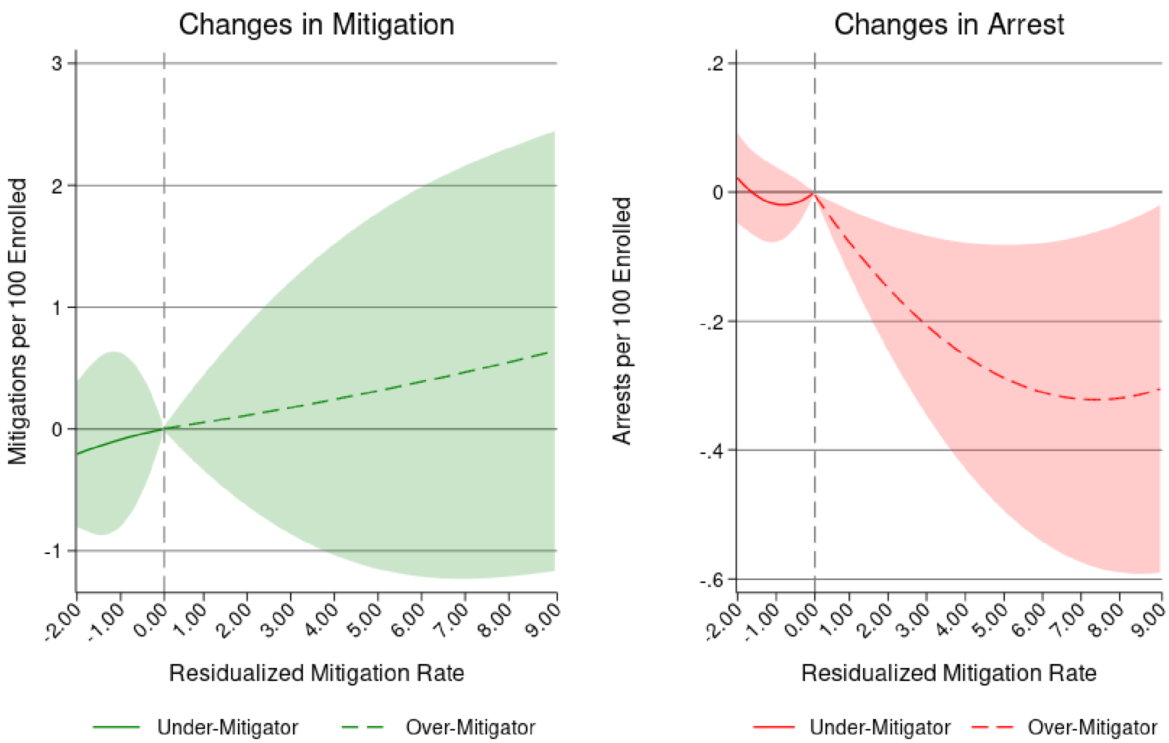
Notes. Panel A presents interrupted time series estimates of the citywide average change in rates of arrest, mitigation, or any intervention pre- to post-policy. Panel B estimates indicate the average in each year relative to the 2018/19 academic year between over- and under-arresting schools. Rates are measured as the number of interventions per 100 students enrolled. All estimates exclude the 2020/21 academic year and condition on school-level characteristics and school and year fixed effects. $^{***}p < 0.001,^{**}p < 0.01,^{*}p < 0.05$ and 95% confidence intervals are provided.

Figure 3: Alternative treatment definitions



Notes. This figure presents estimates of the change pre- to post-policy in rates of mitigation or rates of arrest for each alternative measure of treatment. > 0 arrests is an indicator for a school having at least one arrest in the 2018/19 academic year. Tercile treatments are assigned based on the 2018/19 residualized arrest rate. Tercile treatment compares the top tercile of residualized arrest rates to the bottom tercile. Persistent over-arrester is defined as a school that, in all years prior to the policy were over-arresting. Persistent schools are compared to schools that under-arrested in every year. 95% confidence intervals are provided.

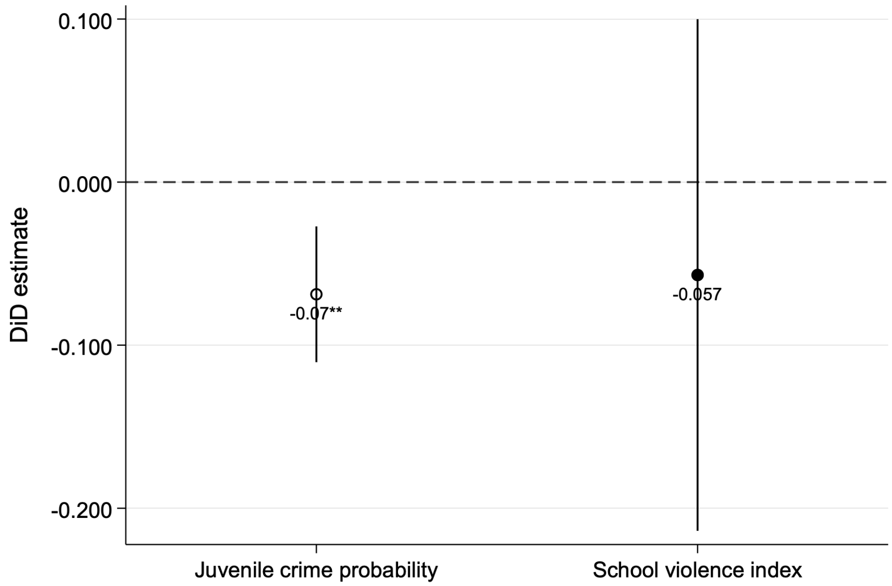
Figure 4: Placebo Treatment



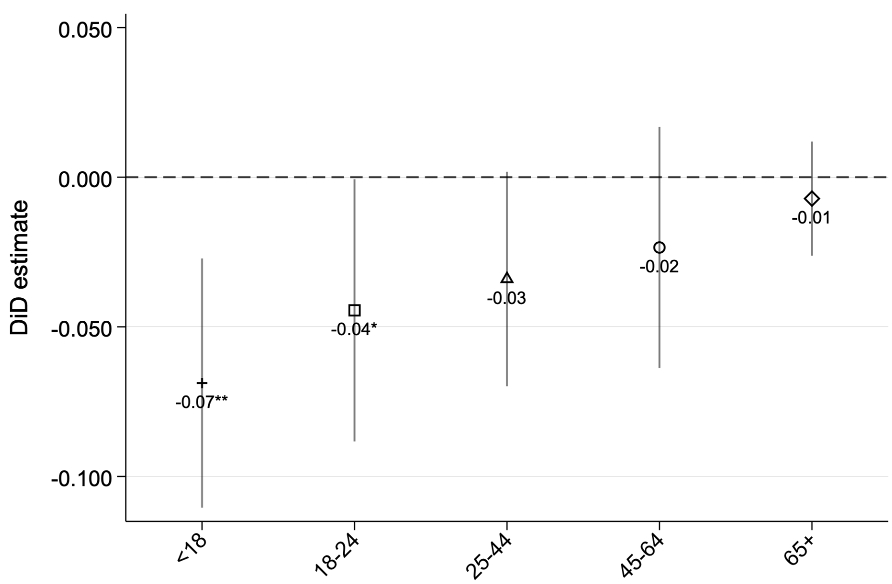
Notes. This figure presents estimates of the change pre- to post-policy in rates of mitigation or rates of arrest. 2019 residualized mitigation rates are on the x-axis, with schools of residuals less than 0 being considered under-mitigators and those with residuals greater than 0 being considered over-mitigators. Estimates are derived separately for under- and over-mitigating schools. 95% confidence intervals are provided.

Figure 5: Effects on Crime Surrounding Middle and High Schools

(a) Arrests and SVI



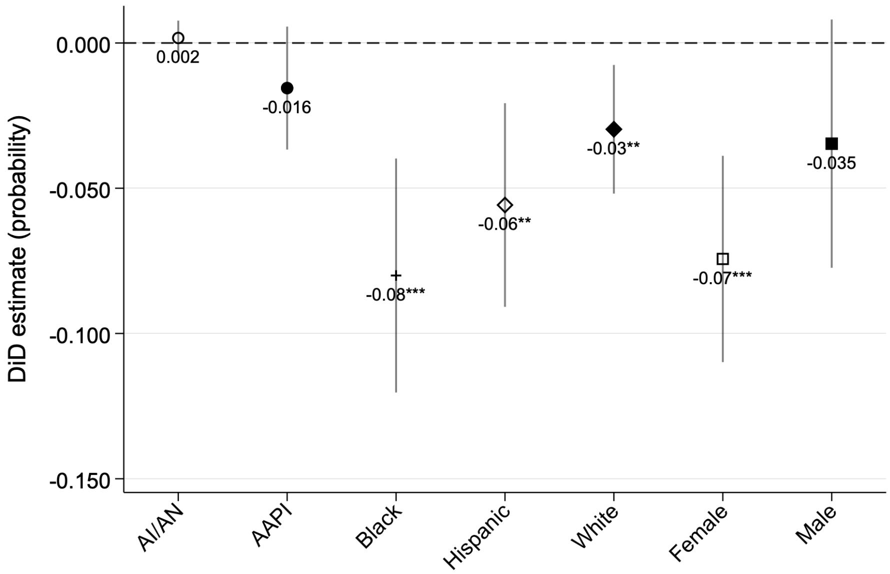
(b) Arrests by Age Group



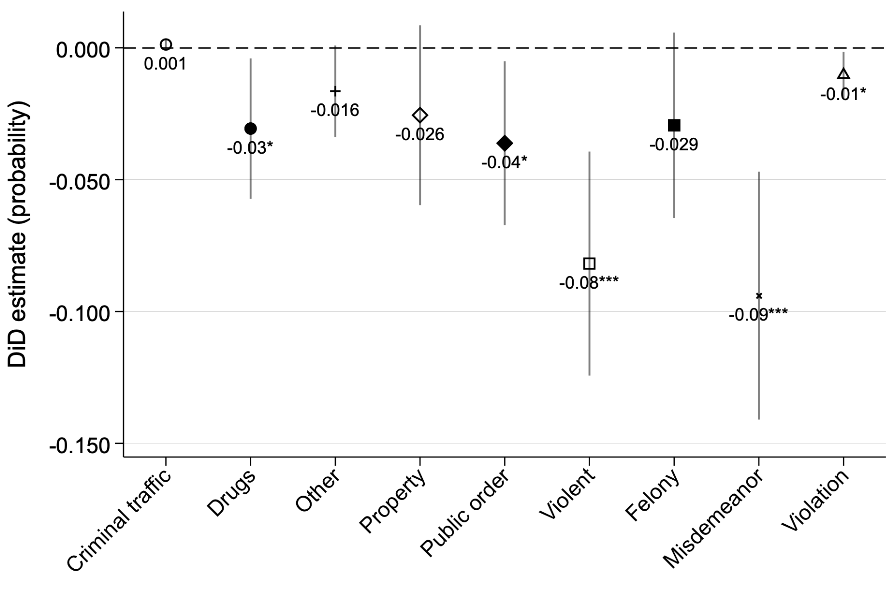
Notes. This figure presents estimates of the change pre- to post-policy in the probability of arrest or changes in school violence index between over- and under-arresters. Coefficient estimates are provided as marker labels. Juvenile crime probability is defined as a school having at least one arrest of a person less than 18 years old reported in the NYPD city-wide arrest data within 0.1km of a school. School violence index is calculated as the number of violent and disruptive incidences per 100 enrolled. All estimates exclude the 2020/21 academic year and condition on school-level characteristics and school and year fixed effects. $^{***}p < 0.001, ^{**}p < 0.01, ^{*}p < 0.05$ and 95% confidence intervals are provided.

Figure 6: Effects on Youth Crime Surrounding Middle and High Schools

(a) By Demographics



(b) By Offense and Legal Consequence



Notes. This figure presents estimates of the change pre- to post-policy in the probability of arrest or changes in school violence index between over- and under-arresters. Coefficient estimates are provided as marker labels. All estimates provided are for subgroups of juveniles. Juvenile crime probability is defined as a school having at least one arrest of a person less than 18 years old reported in the NYPD city-wide arrest data within 0.1km of a school. All estimates exclude the 2020/21 academic year and condition on school-level characteristics and school and year fixed effects. Panel B's offense types are allocated based on Appendix Table A1. $^{***}p < 0.001, ^{**}p < 0.01, ^{*}p < 0.05$ and 95% confidence intervals are provided.

Table 1: Summary Statistics by Treatment Status

	(1) Control	(2) Treated	(3) Std. Mean Diff.
Panel A: Student-level variables			
Attendance Rate	0.894	0.888	0.037
Math Z-Scores	-0.011	0.014	-0.025
	(1.01)	(1.01)	
ELA Z-Scores	-0.003	0.010	-0.01
	(1.01)	(1.01)	
Suspension Probability	0.04	0.04	0.003
Male	0.510	0.515	-0.009
Asian	0.171	0.211	-0.211
Black	0.239	0.220	0.046
Hispanic	0.439	0.383	0.113
White	0.131	0.166	-0.098
Other Race	0.020	0.020	0.001
IEP-SPED Status Probability	0.18	0.20	-.004
Mean Enrollment Duration (years)	1.232	1.305	-0.060
	(1.203)	(1.208)	
Attendance Observations	1,733,186	1,303,714	3,036,900
Math Test Score Observations	534,219	236,391	770,610
ELA Test Score Observations	581,968	262,443	844,411
Suspended Students Observations	1,268,601	948,790	2,217,391
Number of Schools	515	203	718
Panel B: School-level variables			
Arrest rate per 100	0.08	0.21	-0.36
	(0.29)	(0.41)	
Mitigation rate per 100	0.74	1.82	-0.36
	(1.82)	(3.84)	
School Violence Index	0.98	1.00	-0.01
	(1.22)	(1.29)	
Any Crime:			
< 18	0.15	0.27	-0.31
18-24	0.21	0.28	-0.18
25-44	0.29	0.36	-0.14
45-64	0.18	0.22	-0.09
65+	0.03	0.02	0.04
Observations	3,494	1,499	4,993

Notes: Column 1 reports summary statistics for under-arrester middle and high schools, while Column 2 reports statistics for over-arrester middle and high schools. Panel A reports student-level summary statistics, and Panel B reports school-level incident rates. Demographic characteristics are representative of the most expansive sample (attendance). Both samples span the 2016/17 through 2023/24 academic years. The 2020/21 academic year is excluded from all calculations, test scores are unavailable for the 2019/20 academic year due to COVID-19, and suspension and infraction information excludes the 2022/23 and 2023/24 academic years.

Table 2: Academic Outcomes

	Primary	>0 Arrests	Terciles	Persistent Over-Arrester
Panel A: Attendance				
Full Sample	0.0049*	0.0066***	0.0060*	-0.0002
	(0.0019)	(0.0018)	(0.0027)	(0.0021)
Black Students	0.0059*	0.0063*	0.0040	0.0034
	(0.0027)	(0.0027)	(0.0036)	(0.0032)
White Students	0.0027	0.0040	0.0055	-0.0016
	(0.0029)	(0.0027)	(0.0040)	(0.0026)
Hispanic Students	0.0047	0.0065**	0.0046	-0.0025
	(0.0025)	(0.0024)	(0.0032)	(0.0027)
Male Students	0.0056**	0.0077***	0.0062*	0.0005
	(0.0021)	(0.0020)	(0.0030)	(0.0023)
Students in Poverty	0.0039	0.0054**	0.0056	-0.0007
	(0.0021)	(0.0020)	(0.0029)	(0.0022)
Students with Disabilities	0.0080**	0.0095***	0.0069	0.0019
	(0.0028)	(0.0027)	(0.0037)	(0.0030)
Observations	3,022,260	3,023,433	2,023,932	2,316,448
Panel B: Math Test Scores				
Math (s.d.'s): Average	-0.0309	-0.0291	-0.1010***	-0.0010
	(0.0262)	(0.0252)	(0.0292)	(0.0190)
Black Students	-0.0125	-0.0263	-0.0685	0.0079
	(0.0309)	(0.0314)	(0.0368)	(0.0236)
White Students	-0.0280	-0.0316	-0.1145**	0.0121
	(0.0408)	(0.0386)	(0.0436)	(0.0337)
Hispanic Students	-0.0380	-0.0360	-0.1258***	0.0047
	(0.0299)	(0.0295)	(0.0342)	(0.0218)
Male Students	-0.0355	-0.0334	-0.1002***	-0.0067
	(0.0261)	(0.0253)	(0.0290)	(0.0194)
Students in Poverty	-0.0202	-0.0137	-0.0664*	0.0116
	(0.0248)	(0.0240)	(0.0286)	(0.0187)
Students with Disabilities	-0.0193	-0.0154	-0.0754**	0.0183
	(0.0201)	(0.0197)	(0.0282)	(0.0193)
Observations	768,767	768,767	413,795	545,110
Panel C: ELA Test Scores				
ELA (s.d.'s)	-0.0104	0.0027	-0.1000***	-0.0286
	(0.0266)	(0.0248)	(0.0270)	(0.0173)
Black Students	-0.0191	-0.0248	-0.0673*	-0.0181
	(0.0234)	(0.0231)	(0.0284)	(0.0191)
White Students	-0.0035	0.0162	-0.1474*	-0.0259
	(0.0408)	(0.0397)	(0.0618)	(0.0333)
Hispanic Students	-0.0210	-0.0094	-0.1010***	-0.0064
	(0.0226)	(0.0229)	(0.0267)	(0.0189)
Male Students	-0.0063	0.0046	-0.0987***	-0.0311
	(0.0271)	(0.0254)	(0.0276)	(0.0196)
Students in Poverty	-0.0085	0.0016	-0.0604*	-0.0172
	(0.0224)	(0.0214)	(0.0239)	(0.0161)
Students with Disabilities	-0.0035	0.0082	-0.0640*	0.0028
	(0.0186)	(0.0181)	(0.0252)	(0.0164)
Observations	843,386	843,386	454,815	590,500

Notes: *Notes:* DID estimates for academic outcomes. Each column presents a separate regression model. Column 1 uses the primary dichotomized over-arrest measure; Column 2 an indicator for having at least one arrest in 2018/19; Column 3 compares the top to the bottom tercile of residualized arrest rates; Column 4 compares schools over-arresting in all pre-policy years to schools under-arresting in all pre-policy years. School and academic-year fixed effects are included throughout, with student demographic fixed effects added to full-sample estimates. Standard errors clustered at the school level are in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 3: Primary Results: Infractions

	(1) Primary	(2) >0 Arrests	(3) Terciles	(4) Persistent
Panel A: All Infractions				
Full Sample	0.0105	0.0079	0.0244***	0.0066
	(0.0055)	(0.0051)	(0.0061)	(0.0053)
Black Students	0.0135	0.0119	0.0246**	0.0090
	(0.0076)	(0.0074)	(0.0087)	(0.0110)
White Students	0.0009	-0.0026	0.0159	-0.0078
	(0.0072)	(0.0070)	(0.0092)	(0.0088)
Hispanic Students	0.0136	0.0111	0.0218**	0.0088
	(0.0079)	(0.0071)	(0.0079)	(0.0063)
Male Students	0.0104	0.0071	0.0266***	0.0072
	(0.0061)	(0.0058)	(0.0069)	(0.0064)
Students in Poverty	0.0115*	0.0096	0.0212***	0.0087
	(0.0058)	(0.0053)	(0.0063)	(0.0055)
Students with Disabilities	0.0094	0.0066	0.0233**	0.0076
	(0.0072)	(0.0069)	(0.0081)	(0.0075)
Observations	2,252,641	2,253,822	1,520,540	1,761,686
Panel B: Level 1 Infractions				
Full Sample	0.0066	0.0046	0.0113**	0.0053
	(0.0037)	(0.0035)	(0.0042)	(0.0036)
Black Students	0.0122	0.0091	0.0180**	0.0096
	(0.0062)	(0.0059)	(0.0067)	(0.0056)
White Students	0.0034	0.0011	0.0046	-0.0022
	(0.0053)	(0.0051)	(0.0063)	(0.0058)
Hispanic Students	0.0069	0.0057	0.0086	0.0052
	(0.0046)	(0.0042)	(0.0047)	(0.0048)
Male Students	0.0078	0.0051	0.0128**	0.0066
	(0.0042)	(0.0039)	(0.0047)	(0.0042)
Students in Poverty	0.0076	0.0056	0.0107*	0.0066
	(0.0040)	(0.0036)	(0.0043)	(0.0039)
Students with Disabilities	0.0094	0.0088	0.0148*	0.0057
	(0.0055)	(0.0052)	(0.0060)	(0.0054)
Observations	2,252,641	2,253,822	1,520,540	1,761,686
Panel C: Level 2 Infractions				
Full Sample	0.0060	0.0045	0.0082*	-0.0026
	(0.0035)	(0.0032)	(0.0035)	(0.0017)
Black Students	0.0042	0.0029	0.0059	-0.0014
	(0.0027)	(0.0026)	(0.0031)	(0.0028)
White Students	0.0024	0.0026	0.0098*	-0.0055
	(0.0030)	(0.0029)	(0.0038)	(0.0032)
Hispanic Students	0.0095	0.0067	0.0102	-0.0040
	(0.0064)	(0.0056)	(0.0058)	(0.0022)
Male Students	0.0068	0.0052	0.0088*	-0.0031
	(0.0036)	(0.0032)	(0.0036)	(0.0021)
Students in Poverty	0.0063	0.0049	0.0076*	-0.0026
	(0.0039)	(0.0034)	(0.0037)	(0.0018)
Students with Disabilities	0.0075	0.0047	0.0092*	-0.0056
	(0.0041)	(0.0038)	(0.0043)	(0.0031)
Observations	2,252,641	2,253,822	1,520,540	1,761,686

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	(1) Primary	(2) >0 Arrests	(3) Terciles	(4) Persistent
Panel D: Level 3 Infractions				
Full Sample	0.0029	0.0018	0.0123***	0.0017
	(0.0023)	(0.0023)	(0.0032)	(0.0032)
Black Students	0.0055	0.0044	0.0123*	-0.0006
	(0.0043)	(0.0043)	(0.0050)	(0.0077)
White Students	-0.0037	-0.0055	0.0110*	-0.0012
	(0.0033)	(0.0034)	(0.0056)	(0.0046)
Hispanic Students	0.0036	0.0031	0.0097**	0.0042
	(0.0025)	(0.0025)	(0.0034)	(0.0032)
Male Students	0.0041	0.0027	0.0159***	0.0037
	(0.0032)	(0.0032)	(0.0042)	(0.0042)
Students in Poverty	0.0032	0.0026	0.0102**	0.0026
	(0.0024)	(0.0024)	(0.0033)	(0.0033)
Students with Disabilities	0.0046	0.0042	0.0160**	0.0060
	(0.0040)	(0.0041)	(0.0054)	(0.0051)
Observations	2,252,641	2,253,822	1,520,540	1,761,686
Panel E: Level 4 Infractions				
Full Sample	0.0013	0.0012	0.0081**	0.0037
	(0.0018)	(0.0018)	(0.0025)	(0.0026)
Black Students	0.0020	0.0019	0.0079	0.0050
	(0.0037)	(0.0036)	(0.0043)	(0.0056)
White Students	-0.0030	-0.0032	0.0027	-0.0037
	(0.0025)	(0.0025)	(0.0040)	(0.0041)
Hispanic Students	0.0015	0.0021	0.0051	0.0047
	(0.0022)	(0.0021)	(0.0028)	(0.0030)
Male Students	-0.0002	0.0002	0.0086**	0.0021
	(0.0023)	(0.0023)	(0.0030)	(0.0032)
Students in Poverty	0.0016	0.0018	0.0064*	0.0042
	(0.0019)	(0.0019)	(0.0026)	(0.0028)
Students with Disabilities	-0.0003	-0.0007	0.0063	0.0043
	(0.0033)	(0.0032)	(0.0041)	(0.0045)
Observations	2,252,641	2,253,822	1,520,540	1,761,686
Panel F: Level 5 Infractions				
Full Sample	0.0009	0.0010	0.0008	0.0020
	(0.0006)	(0.0006)	(0.0009)	(0.0011)
Black Students	0.0005	0.0022	0.0009	0.0031
	(0.0015)	(0.0015)	(0.0018)	(0.0026)
White Students	0.0016*	0.0015*	0.0042**	-0.0004
	(0.0006)	(0.0006)	(0.0015)	(0.0014)
Hispanic Students	0.0011	0.0008	0.0012	0.0020
	(0.0007)	(0.0007)	(0.0010)	(0.0012)
Male Students	0.0010	0.0011	0.0014	0.0014
	(0.0008)	(0.0008)	(0.0012)	(0.0013)
Students in Poverty	0.0006	0.0008	0.0002	0.0021
	(0.0007)	(0.0007)	(0.0010)	(0.0013)
Students with Disabilities	0.0004	0.0005	-0.0000	0.0009
	(0.0012)	(0.0012)	(0.0017)	(0.0019)
Observations	2,252,641	2,253,822	1,520,540	1,761,686

Notes: DID estimates for in-school infraction-related outcomes. Each column presents a separate regression model. Column 1 uses the primary dichotomized over-arrest measure; Column 2 an indicator for having at least one arrest in 2018/19; Column 3 compares the top to the bottom tercile of residualized arrest rates; Column 4 compares schools over-arresting in all pre-policy years to schools under-arresting in all pre-policy years. School and academic-year fixed effects are included throughout, with student demographic fixed effects added to full-sample estimates. Standard errors clustered at the school level are in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table 4: Primary Results: Suspensions

	(1) Primary	(2) >0 Arrests	(3) Terciles	(4) Persistent
Panel A: All Suspensions				
Full Sample	0.0014	0.0016	0.0098***	0.0051*
	(0.0016)	(0.0016)	(0.0024)	(0.0021)
Black Students	-0.0000	-0.0002	0.0060	0.0080
	(0.0034)	(0.0033)	(0.0041)	(0.0046)
White Students	-0.0012	-0.0019	0.0069*	0.0011
	(0.0019)	(0.0019)	(0.0035)	(0.0030)
Hispanic Students	0.0021	0.0026	0.0067**	0.0053*
	(0.0020)	(0.0020)	(0.0026)	(0.0024)
Male Students	0.0016	0.0015	0.0110***	0.0045
	(0.0021)	(0.0021)	(0.0029)	(0.0026)
Students in Poverty	0.0015	0.0022	0.0081**	0.0066**
	(0.0018)	(0.0018)	(0.0025)	(0.0024)
Students with Disabilities	0.0011	0.0010	0.0078	0.0092*
	(0.0031)	(0.0031)	(0.0040)	(0.0042)
Observations	2,252,641	2,253,822	1,520,540	1,761,686
Panel B: Mean Number of Days Suspended				
Full Sample	-0.1236	-0.1455	0.0118	-0.3562
	(0.2568)	(0.2439)	(0.2736)	(0.3177)
Black Students	-0.3339	-0.1554	-0.2079	0.0845
	(0.3427)	(0.3295)	(0.3559)	(0.4672)
White Students	0.1318	-0.3595	1.6271*	-2.2923*
	(0.4549)	(0.4387)	(0.6666)	(0.9440)
Hispanic Students	-0.1050	-0.1509	0.0058	-0.7077
	(0.3177)	(0.3062)	(0.3489)	(0.3791)
Male Students	-0.2381	-0.3051	-0.1268	-0.6003
	(0.3378)	(0.3153)	(0.3530)	(0.4380)
Students in Poverty	-0.0845	-0.0666	0.0318	-0.1004
	(0.2600)	(0.2483)	(0.2767)	(0.3265)
Students with Disabilities	-0.5467	-0.4580	-0.4829	-0.0694
	(0.3130)	(0.3010)	(0.3463)	(0.4256)
Observations	90,125	90,174	68,189	74,081
Panel C: Level 3 Suspensions				
Full Sample	0.0010	0.0007	0.0053***	0.0019
	(0.0009)	(0.0009)	(0.0012)	(0.0011)
Black Students	-0.0008	-0.0013	0.0028	0.0017
	(0.0018)	(0.0018)	(0.0021)	(0.0022)
White Students	-0.0004	-0.0012	0.0025	0.0025
	(0.0012)	(0.0012)	(0.0020)	(0.0016)
Hispanic Students	0.0020	0.0017	0.0042**	0.0027
	(0.0011)	(0.0011)	(0.0014)	(0.0015)
Male Students	0.0014	0.0011	0.0067***	0.0025
	(0.0013)	(0.0013)	(0.0017)	(0.0015)
Students in Poverty	0.0011	0.0012	0.0046***	0.0027*
	(0.0010)	(0.0010)	(0.0013)	(0.0012)
Students with Disabilities	0.0009	0.0008	0.0057*	0.0052*
	(0.0018)	(0.0018)	(0.0023)	(0.0025)
Observations	2,252,641	2,253,822	1,520,540	1,761,686
Panel D: Level 4 Suspensions				

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	(1) Primary	(2) >0 Arrests	(3) Terciles	(4) Persistent
Full Sample	0.0004	0.0009	0.0062***	0.0027
	(0.0012)	(0.0011)	(0.0017)	(0.0015)
Black Students	-0.0003	-0.0006	0.0037	0.0047
	(0.0026)	(0.0025)	(0.0030)	(0.0030)
White Students	-0.0014	-0.0014	0.0038	-0.0013
	(0.0013)	(0.0013)	(0.0025)	(0.0022)
Hispanic Students	0.0005	0.0015	0.0035	0.0025
	(0.0014)	(0.0014)	(0.0019)	(0.0018)
Male Students	0.0003	0.0007	0.0064**	0.0020
	(0.0015)	(0.0015)	(0.0021)	(0.0019)
Students in Poverty	0.0007	0.0014	0.0054**	0.0034*
	(0.0013)	(0.0012)	(0.0018)	(0.0017)
Students with Disabilities	0.0006	0.0011	0.0052	0.0049
	(0.0023)	(0.0023)	(0.0030)	(0.0032)
Observations	2,252,641	2,253,822	1,520,540	1,761,686
Panel E: Level 5 Suspensions				
Full Sample	0.0004	0.0007	0.0012	0.0016
	(0.0005)	(0.0005)	(0.0008)	(0.0008)
Black Students	0.0000	0.0012	0.0009	0.0033
	(0.0013)	(0.0012)	(0.0016)	(0.0020)
White Students	0.0010*	0.0008	0.0033**	-0.0007
	(0.0005)	(0.0005)	(0.0013)	(0.0011)
Hispanic Students	0.0005	0.0006	0.0012	0.0012
	(0.0006)	(0.0006)	(0.0008)	(0.0008)
Male Students	0.0008	0.0011	0.0019	0.0014
	(0.0007)	(0.0006)	(0.0010)	(0.0010)
Students in Poverty	0.0002	0.0006	0.0007	0.0018
	(0.0006)	(0.0006)	(0.0009)	(0.0009)
Students with Disabilities	-0.0001	0.0003	0.0004	0.0013
	(0.0011)	(0.0010)	(0.0015)	(0.0016)
Observations	2,252,641	2,253,822	1,520,540	1,761,686

Notes: DID estimates for in-school suspension-related outcomes. Each column presents a separate regression model. Column 1 uses the primary dichotomized over-arrest measure; Column 2 an indicator for having at least one arrest in 2018/19; Column 3 compares the top to the bottom tercile of residualized arrest rates; Column 4 compares schools over-arresting in all pre-policy years to schools under-arresting in all pre-policy years. School and academic-year fixed effects are included throughout, with student demographic fixed effects added to full-sample estimates. Standard errors clustered at the school level are in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table 5: Placebo: "Over-Mitigator" Schools

	Primary	Black	White	Hispanic	Male	Poverty	SWD
Panel A: Attendance							
Full Sample	0.0012	0.0000	0.0003	0.0028	0.0017	0.0005	0.0024
	(0.0020)	(0.0028)	(0.0031)	(0.0027)	(0.0022)	(0.0021)	(0.0030)
Panel B: Test Scores							
Math (s.d.'s)	0.0296	0.0504	0.0357	0.0264	0.0364	0.0220	0.0352
	(0.0229)	(0.0281)	(0.0352)	(0.0266)	(0.0226)	(0.0210)	(0.0187)
ELA (s.d.'s)	0.0167	0.0218	0.0161	0.0180	0.0148	0.0043	0.0035
	(0.0259)	(0.0218)	(0.0474)	(0.0221)	(0.0259)	(0.0207)	(0.0187)
Panel C: Infractions							
Any Infraction	0.0120*	0.0151	0.0108	0.0159*	0.0129*	0.0152**	0.0117
	(0.0054)	(0.0078)	(0.0081)	(0.0067)	(0.0064)	(0.0056)	(0.0073)
Level 1	0.0090*	0.0099	0.0102	0.0110*	0.0098*	0.0106*	0.0119*
	(0.0041)	(0.0061)	(0.0061)	(0.0050)	(0.0046)	(0.0044)	(0.0059)
Level 2	0.0010	0.0012	0.0040	0.0012	0.0014	0.0009	0.0015
	(0.0027)	(0.0027)	(0.0032)	(0.0040)	(0.0029)	(0.0029)	(0.0039)
Level 3	0.0046	0.0069	0.0018	0.0060*	0.0053	0.0063*	0.0076
	(0.0025)	(0.0046)	(0.0036)	(0.0026)	(0.0036)	(0.0026)	(0.0043)
Level 4	0.0027	0.0029	-0.0005	0.0057*	0.0031	0.0045*	0.0007
	(0.0020)	(0.0039)	(0.0027)	(0.0023)	(0.0025)	(0.0021)	(0.0034)
Level 5	0.0011	0.0017	-0.0000	0.0011	0.0004	0.0014	0.0001
	(0.0007)	(0.0016)	(0.0007)	(0.0007)	(0.0008)	(0.0008)	(0.0013)
Panel D: Suspensions							
Any Suspension	0.0042*	0.0070	0.0020	0.0065**	0.0042	0.0059**	0.0071*
	(0.0019)	(0.0036)	(0.0023)	(0.0021)	(0.0024)	(0.0020)	(0.0035)
Average Days Suspended	-0.7507**	-0.8852*	-0.2831	-0.6761*	-0.9997**	-0.7769**	-1.0284**
	(0.2805)	(0.3554)	(0.5452)	(0.3377)	(0.3792)	(0.2712)	(0.3362)
Level 3	0.0018	0.0036	0.0010	0.0026*	0.0021	0.0025*	0.0037
	(0.0010)	(0.0019)	(0.0015)	(0.0011)	(0.0014)	(0.0010)	(0.0019)
Level 4	0.0019	0.0030	0.0010	0.0040**	0.0023	0.0031*	0.0043
	(0.0013)	(0.0028)	(0.0015)	(0.0015)	(0.0017)	(0.0014)	(0.0026)
Level 5	0.0011	0.0023	0.0002	0.0008	0.0009	0.0014*	0.0010
	(0.0006)	(0.0014)	(0.0005)	(0.0006)	(0.0008)	(0.0007)	(0.0012)
Observations	2252641	541821	329163	923046	1156115	1655785	418621
Clusters	718	715	714	718	711	718	718

Notes: DID estimates using the "over-mitigation" placebo treatment. Each column presents a separate regression model for the indicated subgroup. School and academic-year fixed effects are included throughout. Standard errors clustered at the school level are in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 6: Balance Test

	Full Sample		**Test Score Sample**	
	Main	Tercile	Main	Tercile
Black Students	-0.0022	0.0092*	0.0006	0.0165*
	(0.0030)	(0.0041)	(0.0047)	(0.0064)
White Students	-0.0040	-0.0119**	-0.0050	-0.0235**
	(0.0044)	(0.0039)	(0.0099)	(0.0086)
Hispanic Students	0.0030	-0.0042	0.0117	0.0045
	(0.0039)	(0.0049)	(0.0079)	(0.0080)
Male Students	-0.0074**	-0.0102**	0.0040	0.0012
	(0.0024)	(0.0033)	(0.0040)	(0.0050)
Students in Poverty	-0.0035	-0.0084	0.0064	0.0049
	(0.0055)	(0.0057)	(0.0117)	(0.0111)
Students with Disabilities	-0.0023	-0.0020	-0.0031	0.0013
	(0.0021)	(0.0027)	(0.0041)	(0.0054)
Observations	3,017,469	2,009,394	872,800	467,499
Clusters	718	479	337	196

Notes: DID demographic estimates for over-arrester schools in the post-MOU period. Columns 1 and 2 provide estimates for all eligible students in each subgroup using the primary and tercile treatment measures, respectively, while Columns 3 and 4 restrict to eligible students from each subgroup who have observed test scores. Standard errors clustered at the school level are in parentheses. *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

Table 7: Primary Results: Crime

	Primary	>0 Arrests	Terciles	Persistent
Panel A: Age				
< 18	-0.0688**	-0.0746***	-0.0493*	-0.0590*
	(0.0212)	(0.0207)	(0.0237)	(0.0250)
18-24	-0.0445*	-0.0330	-0.0451	-0.0037
	(0.0223)	(0.0218)	(0.0240)	(0.0244)
25-44	-0.0340	-0.0476**	-0.0306	0.0035
	(0.0183)	(0.0182)	(0.0203)	(0.0216)
45-64	-0.0235	-0.0076	-0.0098	0.0101
	(0.0205)	(0.0201)	(0.0212)	(0.0242)
65+	-0.0071	-0.0064	-0.0154	0.0105
	(0.0097)	(0.0095)	(0.0113)	(0.0119)
Panel B: Race				
Black	-0.0800***	-0.0669***	-0.0330	-0.0675**
	(0.0205)	(0.0197)	(0.0225)	(0.0229)
Hispanic	-0.0558**	-0.0644***	-0.0534**	-0.0552**
	(0.0179)	(0.0175)	(0.0199)	(0.0210)
White	-0.0297**	-0.0343**	-0.0297**	-0.0311**
	(0.0113)	(0.0108)	(0.0099)	(0.0112)
Panel C: Sex				
Female	-0.0743***	-0.0855***	-0.0621**	-0.0494*
	(0.0181)	(0.0176)	(0.0189)	(0.0214)
Male	-0.0347	-0.0432*	-0.0136	-0.0659*
	(0.0218)	(0.0214)	(0.0245)	(0.0261)
Panel D: Offense				
Drugs	-0.0306*	-0.0376**	-0.0263	-0.0121
	(0.0135)	(0.0133)	(0.0142)	(0.0139)
Other	-0.0165	-0.0132	-0.0174*	-0.0074
	(0.0088)	(0.0086)	(0.0085)	(0.0105)
Public Order	-0.0362*	-0.0326*	-0.0327*	-0.0420*
	(0.0158)	(0.0152)	(0.0162)	(0.0203)
Violent	-0.0818***	-0.0841***	-0.0459	-0.0578*
	(0.0216)	(0.0209)	(0.0237)	(0.0232)
Panel E: Legal Category				
Felony	-0.0294	-0.0324	-0.0196	-0.0434
	(0.0179)	(0.0178)	(0.0194)	(0.0231)
Misdemeanor	-0.0940***	-0.0977***	-0.0706**	-0.1070***
	(0.0239)	(0.0231)	(0.0267)	(0.0242)
Violation	-0.0103*	-0.0092*	-0.0075	-0.0083
	(0.0044)	(0.0041)	(0.0041)	(0.0043)
Observations	4993	5003	3326	4248

Notes: DID estimates for NYPD arrests within 0.1km of a school campus. Panels B-E estimate effects only for individuals <18 years of age. Each column presents a separate regression model specification. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 8: Crime Results Placebo and Falsification

	>0.1km	Non-school days	Over-mitigation
Panel A: Age			
< 18	-0.0327	-0.0400*	0.0125
	(0.0211)	(0.0190)	(0.0213)
18-24	-0.0119	-0.0046	0.0040
	(0.0117)	(0.0217)	(0.0214)
25-44	-0.0065	0.0010	-0.0256
	(0.0061)	(0.0190)	(0.0191)
45-64	-0.0001	-0.0251	-0.0000
	(0.0114)	(0.0219)	(0.0203)
65+	-0.0022	-0.0265*	0.0142
	(0.0196)	(0.0106)	(0.0099)
Panel B: Race			
Black	-0.0473*	-0.0117	-0.0024
	(0.0206)	(0.0155)	(0.0191)
Hispanic	-0.0406	-0.0231	0.0269
	(0.0218)	(0.0138)	(0.0164)
White	-0.0019	-0.0012	-0.0147
	(0.0145)	(0.0050)	(0.0093)
Panel C: Sex			
Female	-0.0381	0.0025	0.0104
	(0.0207)	(0.0107)	(0.0167)
Male	-0.0325	-0.0427*	0.0056
	(0.0222)	(0.0191)	(0.0211)
Panel D: Offense			
Drugs	-0.0519**	-0.0027	-0.0142
	(0.0180)	(0.0075)	(0.0123)
Other	-0.0041	-0.0059	0.0208**
	(0.0128)	(0.0046)	(0.0076)
Public Order	-0.0224	-0.0108	-0.0197
	(0.0172)	(0.0094)	(0.0148)
Violent	-0.0330	-0.0095	-0.0034
	(0.0216)	(0.0132)	(0.0209)
Panel E: Legal Category			
Felony	-0.0301	-0.0086	0.0096
	(0.0205)	(0.0156)	(0.0175)
Misdemeanor	-0.0453	-0.0269	-0.0114
	(0.0250)	(0.0157)	(0.0223)
Violation	-0.0068	-0.0002	0.0023
	(0.0062)	(0.0019)	(0.0030)
Observations	4993	4993	4993
Clusters	718	718	718

Notes: DID estimates for NYPD arrests. Column 1 presents estimates for arrests occurring outside of a 0.1km radius of a school campus, while Columns 2 and 3 present estimates for arrests within 0.1km of a school campus. Panels B-E estimate effects only for individuals <18 years of age. Each column presents a separate regression model specification. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

A Appendix

Table A1: Arrest Data Coding

NYPD Arrest Label	Arrest Category
Moving Infractions	Criminal Traffic
Other Traffic Infraction	Criminal Traffic
Vehicle And Traffic Laws	Criminal Traffic
Alcoholic Beverage Control Law	Drugs
Cannabis Related Offenses	Drugs
Dangerous Drugs	Drugs
Intoxicated/Impaired Driving	Drugs
Administrative Code	Other
For Other Authorities	Other
Miscellaneous Penal Law	Other
NYS Laws-Unclassified Felony	Other
Offenses Related To Children	Other
Other State Laws	Other
Other State Laws (Non Penal Law)	Other
Arson	Property
Burglar's Tools	Property
Burglary	Property
Criminal Mischief & Related Offenses	Property
Criminal Trespass	Property
Forgery	Property
Frauds	Property
Grand Larceny	Property
Grand Larceny Of Motor Vehicle	Property
Offenses Involving Fraud	Property
Other Offenses Related To Theft	Property
Petit Larceny	Property
Possession Of Stolen Property	Property
Theft Of Services	Property
Theft-Fraud	Property
Unauthorized Use Of A Vehicle	Property
Agriculture & Mrkts Law-Unclassified	Public Order
Anticipatory Offenses	Public Order
Dangerous Weapons	Public Order
Disorderly Conduct	Public Order

Continued on next page

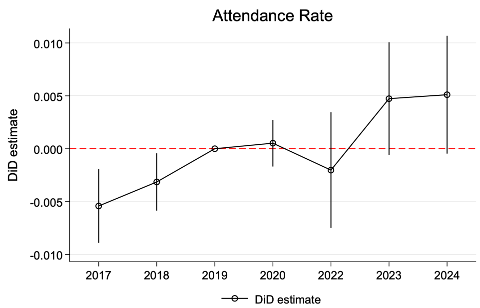
Table A1: Arrest Data Coding (Continued)

NYPD Arrest Label	**Arrest Category**
Escape 3	Public Order
F.C.A. P.I.N.O.S.	Public Order
Fraudulent Accosting	Public Order
Gambling	Public Order
Jostling	Public Order
Loitering	Public Order
Loitering/Gambling	Public Order
Off. Agnst Pub Ord Sensibility	Public Order
Offenses Against Public Administration	Public Order
Offenses Against Public Safety	Public Order
Prostitution & Related Offenses	Public Order
Unlawful Poss. Weap. On School Grounds	Public Order
Assault 3 & Related Offenses	Violent
Child Abandonment/Non Support	Violent
Endan Welfare Incomp	Violent
Felony Assault	Violent
Forcible Touching	Violent
Harassment 2	Violent
Homicide-Negligent,Unclassified	Violent
Homicide-Negligent-Vehicle	Violent
Kidnapping & Related Offenses	Violent
Murder & Non-Negl. Manslaughter	Violent
Offenses Against The Person	Violent
Rape	Violent
Robbery	Violent
Sex Crimes	Violent

B Event Study Appendix

Figure B1: Event studies: Academic Outcomes

(a)



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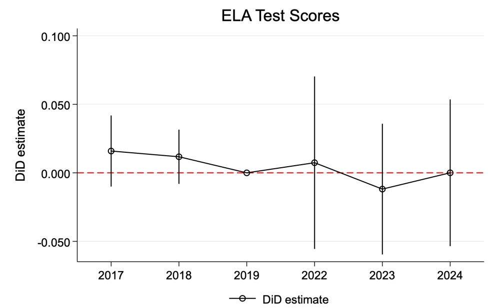

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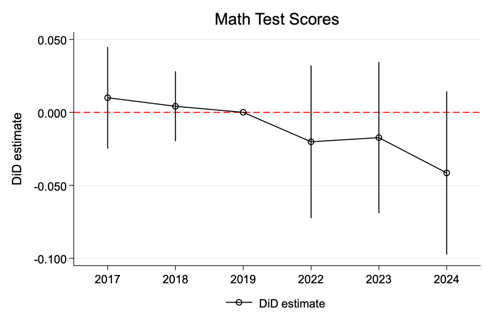
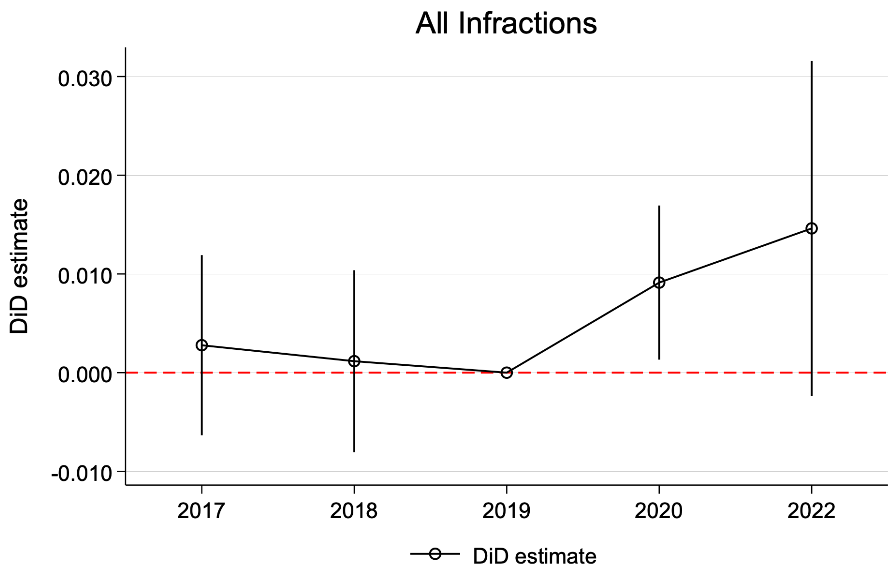


Notes. All estimates exclude the 2020/21 academic year and condition on school-level characteristics and school and year fixed effects. 95% confidence intervals are provided.

Figure B2: Event studies: Infraction and Suspension

(a)



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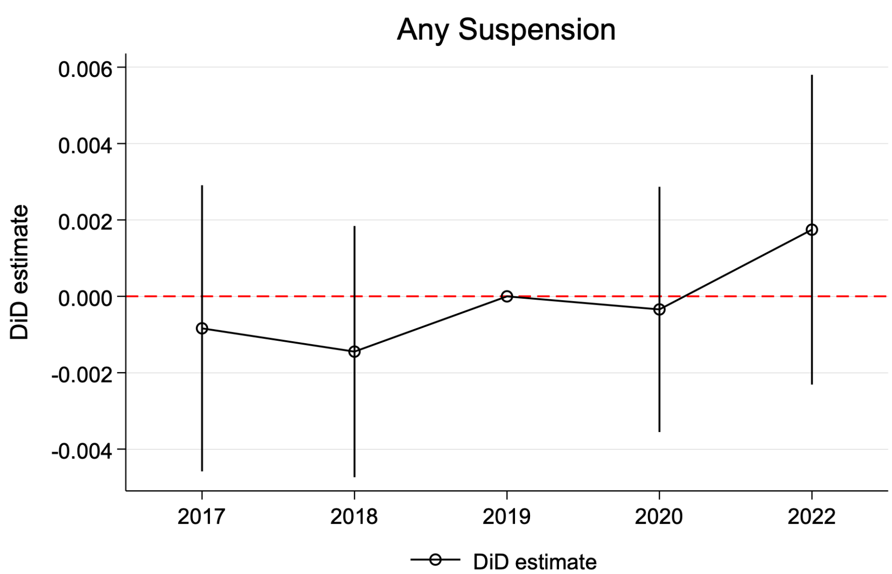
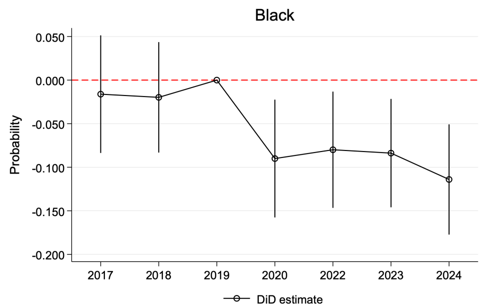


Notes. All estimates exclude the 2020/21 academic year and condition on school-level characteristics and school and year fixed effects. 95% confidence intervals are provided.

Figure B3: Event studies: Crime Probability by Race

(a)



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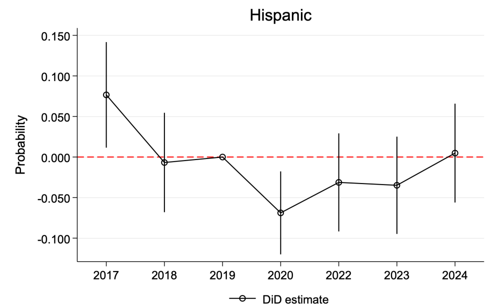

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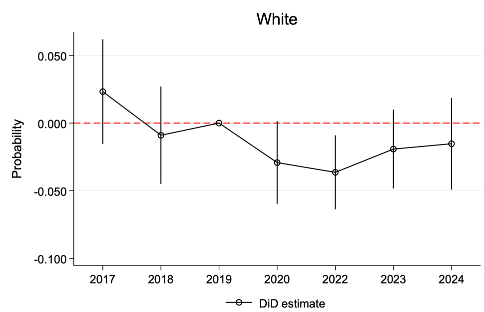
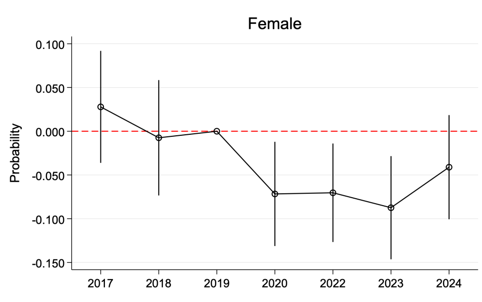


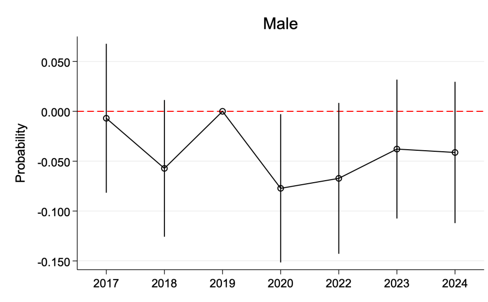
Notes. All estimates exclude the 2020/21 academic year and condition on school-level characteristics and school and year fixed effects. 95% confidence intervals are provided.

Figure B4: Event studies: Crime Probability by Sex

(a)



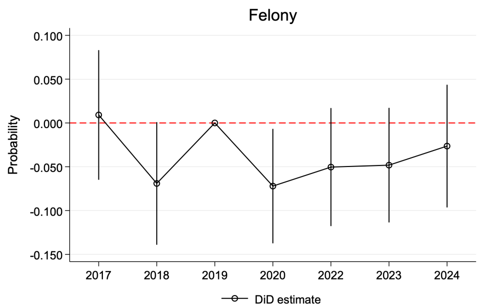
(b)



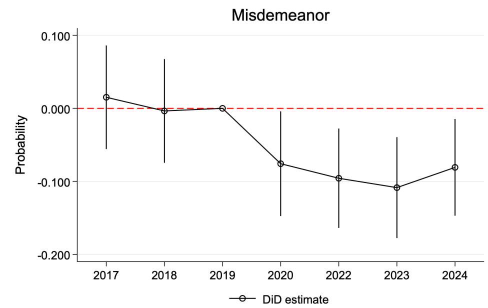
Notes. All estimates exclude the 2020/21 academic year and condition on school-level characteristics and school and year fixed effects. 95% confidence intervals are provided.

Figure B5: Event studies: Crime Probability by Legal Category

(a)



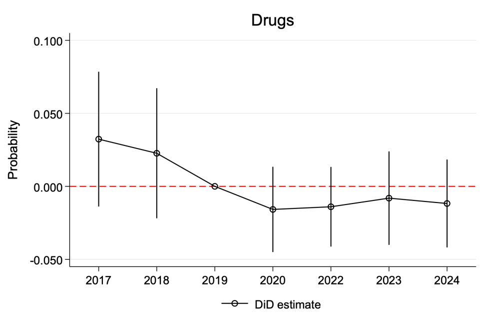
(b)



Notes. All estimates exclude the 2020/21 academic year and condition on school-level characteristics and school and year fixed effects. 95% confidence intervals are provided.

Figure B6: Event studies: Crime Probability by Offense Type

(a)



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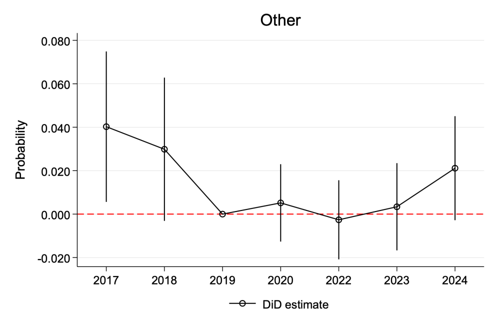

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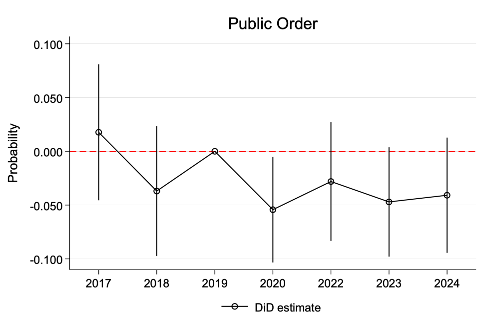
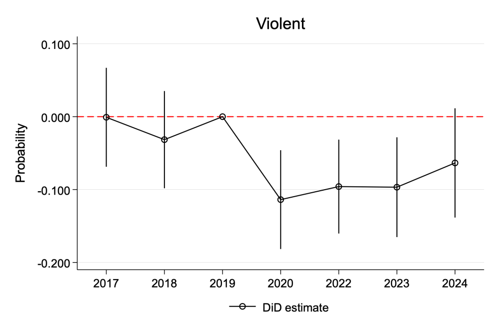


(d)



Notes. All estimates exclude the 2020/21 academic year and condition on school-level characteristics and school and year fixed effects. 95% confidence intervals are provided.