Student Exposure to Proactive Policing:

Heterogeneous Effects of Los Angeles Gang Injunctions

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Abstract

While more police have been found to reduce crime, growing evidence indicates that policies to increase police presence and authority place a heavy burden on targeted communities. How that burden is borne by children remains an open question. In this paper, I estimate the causal effects of a proactive policing program in Los Angeles on the education outcomes of children in kindergarten to grade eight. I leverage the staggered and plausibly exogenous timing of civil gang injunctions and a panel of student administrative data to estimate the effects of new gang injunctions on academic and behavioural outcomes using an event study approach. I document substantial heterogeneity in the effects of the policy, which enhanced police authority to arrest suspected gang members inside 'safety zones'. Four years after gang-injunction implementation, female English-learner students – likely first- or second-generation immigrants – see academic gains of 0.14σ in math and 0.12σ in English test scores compared to never exposed students. In stark contrast, male non-English-learner students suffer declines of 0.11σ in math and 0.13σ in English test scores, along with sharp increases in suspensions. Reductions in reported crime, which reduce Hispanic female victimization, are a key mechanism driving the gains for female English-learner students, based on a variance decomposition exercise. Overall, although the targeted policing strategy was effective in reducing crime and improving academic progress for many, the analysis draws attention to collateral damage inflicted on some at-risk children that ongoing and future policing initiatives could seek to mitigate.

1 Introduction

Recent instances of negative police-civilian interactions, most notably the killing of George Floyd by a Minneapolis police officer, have spurred waves of protests and social unrest, leading to a broad reconsideration of the role of police and the tactics they employ (Levin, 2021). Although it is well-established that more police reduce crime (Chalfin & McCrary, 2017), there is growing evidence that this comes at the cost of over-policing minority communities. Such over-policing has been documented in the form of more intrusive and sometimes violent police stops (Bandes et al., 2019; Fryer Jr, 2019), arrests for low-level offenses (Chalfin et al., 2020), and exposure to discriminatory practices (Goncalves & Mello, 2021). For children, reducing community violence tends to have direct benefits to their education (Laurito et al., 2019; Sharkey et al., 2014) and emotional well-being (Fowler et al., 2009; Sharkey, 2018; Sharkey et al., 2012). However, there may be collateral effects of policing among specific minority groups if over-policing leads to stress, erodes trust and threatens beliefs about police legitimacy.[1] These negative consequences might be disproportionately born by marginalized groups if they are the target of police enforcement efforts, potentially exacerbating early childhood inequalities. Yet, the distributional consequences and trade-offs associated with such policing efforts are poorly understood.

This paper aims to improve our understanding of the consequences of these policing practices by examining the effects of a geographically targeted proactive policing strategy in Los Angeles on the education outcomes of young students. Proactive policing, a popular method to deter crime by focusing officer resources on high-risk places and people, exemplifies the tension between reducing crime and doing so in a way that does not lower the quality of life in high-crime communities. In Los Angeles, this strategy was deployed in the form of civil gang injunctions, which were designed to be highly targeted at offenders though reportedly fell short of this in practice (Muniz, 2015; Queally, 2017). Gang injunctions are civil restraining orders prohibiting certain individuals or groups from engaging in both legal and illegal activity that is considered gang-related. These restrictions are applied in a designated area known as a 'safety zone', inside which police have the power to arrest suspected gang members for offences as minor as appearing in public together. Between 1987 and 2013, the Los Angeles City Attorney, working with the Los Angeles Police Department (LAPD), successfully obtained 46 permanent injunction orders covering over a fifth of the city.

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[1] Surveys of youth have linked both direct and vicarious contact with police to emotional distress and education withdrawal (Geller & Fagan, 2019; Gottlieb & Wilson, 2019). At one extreme, police killings of civilians have been shown to reduce attendance and graduation rates of high school students living nearby (Ang, 2021). Further, economic models of crime show that policing that is perceived to be biased could increase criminal behavior since the relative benefit of acting legally is perceived to be lower (Owens, 2020). If young people perceive that they will someday be unduly targeted by police because of how they look, they may respond by drawing closer to gangs and reducing investments in schooling.

In this study, I empirically examine how newly instituted gang injunctions affected the educational outcomes of elementary and middle school students in a large public school district. Using a rich panel of student administrative data, I estimate the causal effect of civil gang injunctions on academic performance and non-cognitive behavior of students attending schools in these targeted areas of Los Angeles. I map safety zones to school catchment areas, where students both live and go to school, and define treatment with a newly implemented gang injunction as a discontinuous jump in safety zone-to-catchment area overlap.[2] I then leverage the panel nature of the data to make within-student comparisons in outcomes over time using an event study approach with student fixed effects.[3] There are likely to be differences across demographic groups in the abatement of crime-associated harms, who comes into contact with police more under gang injunctions, and who is likely to update their perceptions of police bias or the relative returns to schooling. Given the distribution of treatment effects should be expected to vary, I estimate heterogeneous treatment effects across gender, ethnicity and English-learner status. Furthermore, to probe how student effects relate to the impact of the policy on crime, I combine student records with counts of total reported crimes by mapping LAPD crime reporting districts to neighborhoods around schools. I use these data to estimate the effects of safety zones on student exposure to crime, employing a similar event study model at the school-level.

I find that new injunctions increased student test scores on average by 0.05σ in math and 0.02σ in English after three years. These modest improvements in students' average academic achievement coincide with the finding that gang injunctions decreased violent crime inside catchment areas by 11% after four years, largely driven by a decline in aggravated assaults. Importantly, these average effects mask substantial heterogeneity. Four years after gang-injunction implementation, female English-learner students – likely first- or second-generation immigrants – see academic gains of 0.14σ in math and 0.12σ in English test scores compared to never exposed students. In stark contrast, male non-English-learner students suffer declines of 0.11σ in math and 0.13σ in English test scores, accompanied by a sharp increase in suspensions. Students across most demographic groups see a short-run decline in absences, though this dissipates after four years.

Results from a variance decomposition exercise suggests that the decline in crime correlates both

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[2] I improve on approaches that rely solely on point locations of schools or home addresses to map geographically-applied policing treatments to students. School catchment areas provide a realistic mapping of the spaces children inhabit, capturing exposure to policing tactics around their homes, schools and in between.

[3] Following suggestions from the recent econometrics literature on bias in two-way fixed effects models in staggered event studies, the control group includes only never treated units and I truncate relative treatment periods outside of a four-year window pre- and post-treatment (Sun & Abraham, 2021). Also, as heterogeneous treatment effects across treatment cohorts are plausible in this setting, I employ Sun and Abraham (2021)'s interaction-weighted estimator as a robustness check. I employ Freyaldenhoven et al. (2019)'s suggested test for pre-existing trends which, throughout my analysis, consistently fails to reject the hypothesis that coefficient estimates in the pre-treatment period are jointly zero.

with better scores for female immigrant students and worse scores for male native-born students. An analysis of crime data shows that victimization of females declines substantially after gang injunctions relative to unaffected areas, particularly for Hispanic women, suggesting that victimization or fear of it is an important channel for improving immigrant female test scores. In contrast, apprehensions of teenage Hispanic boys by police increase sharply, as do adult arrest rates. Thus, young Hispanic boys become more likely to witness interactions between people who resemble them demographically and the police. Taken together, the results imply that gang injunctions were positive for students in general, but the negative consequences experienced by many reflect a greater burden for groups who are at the center of police targeting.

This study contributes to the existing literature in several ways. First, I credibly estimate the causal effects on childhood human capital of a commonly employed policing strategy: proactive policing. The policy I study deploys both place-based and person-based targeting, two tools that a comprehensive report on proactive methods concludes are effective at reducing crime (Weisburd et al., 2019). Prior work quantifying the impact of policing strategies on education outcomes has examined one place-based and one person-based strategy used by the New York Police Department. Legewie and Fagan (2019) show that 'Operation Impact', a hot-spot policing program that rapidly saturated areas experiencing crime increases with officers, reduced test scores of African American boys aged 13 to 15. Bacher-Hicks and de la Campa (2020) document deleterious effects of 'Stop, Question and Frisk' on graduation and college enrollment for Black teenagers, both male and female. Gang injunctions present an opportunity for examining proactive policing that is both person- and place-based, was not selected on time-varying trends in crime, and was more targeted towards gang-involved individuals – a strategy recently shown to be highly effective in reducing violence (Chalfin et al., 2021). This quasi-experimental setting, along with a panel of student outcomes, allows for careful estimation of the dynamic effects of policing on human capital.

Second, I conduct a thorough investigation of treatment effect heterogeneity and the mechanisms driving disparate results across students. I uncover new evidence regarding the unique importance of gender and immigration status in determining the relationship between human capital and policing. Mapping localized crime to schools, I confirm findings from earlier research that gang injunctions reduce crime (Grogger, 2002; Ridgeway et al., 2019). I conduct a mediation analysis to directly link mechanisms to student treatment effects, examining the roles of both violent and non-violent crime, absences, suspensions, and resorting of teachers on value-added.Intuitively, this exercise decomposes the variance in student test scores into variation from mediating variables, which is driven by the policy

change, and a remaining unexplained component that operates directly through the policy. The results indicate that the same declines in crime that benefit female immigrant students can also be harmful to male native-born students, likely due to the means by which they are achieved.

Third, I contribute to a burgeoning literature quantifying the welfare tradeoffs of policing beyond the direct effects on crimes and those convicted of them. In related work, Owens et al., 2020 estimate the cost of gang injunctions in southern California using the capitalization of new injunctions into housing prices. They argue that homes inside safety zones become less valuable due to a loss in civil liberties. Related to education, recent studies focused on high school students quantify the adverse effects of local police killings of civilians (Ang, 2021; Gershenson & Hayes, 2017). Prior empirical research into the consequences for children of law enforcement more broadly has examined the use of school police in middle and high schools (Gottfredson et al., 2020; Owens, 2017; Sorensen et al., 2021; Weisburst, 2019; Zhang, 2019), and the effects of parental incarceration on children (Arteaga, 2021; Bhuller et al., 2018; Billings, n.d.; Dobbie et al., 2018; Norris et al., 2021). Bandes et al. (2019) provide a comprehensive review of social science research documenting adverse mental health outcomes from negative police encounters, and several papers use survey evidence to explore these relationships for youth (Geller & Fagan, 2019; Gottlieb & Wilson, 2019). The present study highlights the far-reaching and varied effects that policing can have. Considering their age, children in my sample are very unlikely to interact with police directly. Yet, the benefits and costs of proactive policing to this group, who are at critical stages in their development, are substantial.

Overall, I find evidence consistent with prior work that policing targeting gangs is an effective strategy for reducing violence, and less violence benefits children on net. This contrasts with studies on New York City programs, which do not uncover substantial achievement gains for any group but document harmful effects for African American teens (Bacher-Hicks & de la Campa, 2020; Legewie & Fagan, 2019). Yet, as in those settings, this policy did not come without a cost, and I document the link between those targeted by gang injunctions and those who suffer academically. Ongoing and future policing initiatives could seek to mitigate these harms through several avenues that merit further study, which I discuss in the final section.

The remainder of the paper is organized as follows. Section 2 provides a detailed background of civil gang injunctions and their implementation in Los Angeles. Section 3 outlines a conceptual framework for understanding the effect of safety zones on the human capital of students and how these might vary. Section 4 describes the data and Section 5 explains the empirical methodology and identification assumptions. Section 6 presents the main results and Section 7 examines treatment

effect mechanisms. Section 8 lays out the robustness of the results to empirical choices and alternative explanations. Section 9 concludes.

2   Los Angeles Gang Injunctions

Civil gang injunctions (CGIs) are civil restraining orders filed against a group of defendants that prohibit them from engaging in certain activities inside an area known as a safety zone. Thus, they are targeted at specific individuals (suspected gang members), actions (gang-related activities) and geographic areas (safety zones). The first injunction of this form was filed by the City Attorney of Los Angeles in 1987 against the Playboy Gangster Crips, and since then 46 injunctions targeting 79 different gangs were granted in Los Angeles, the latest in 2013 (Los Angeles City Attorney, 2021).

To obtain an order granting a CGI, City Attorneys must establish to the court that the gang's conduct creates a public nuisance and provide evidence of the involvement of listed defendants (Maxson et al., 2005). Actions prohibited under CGIs include both legal and illegal acts that the court has recognized as being gang-related. Though not all CGIs impose the same sanctions, there is substantial commonality. The most significant restrictions to otherwise legal behaviour include orders not to associate in public with other gang members and nighttime curfews. Other common clauses prohibit the wearing of certain gang-affiliated clothing, drinking or possessing alcohol, or carrying items associated with "lookouts", such as flashlights, radios, or pagers. Occasionally, CGIs include provisions to prevent youth recruitment, such as by ordering defendants to stay more than 1000 feet from certain schools unless necessary for attendance. Stipulations against illegal activity often include the use, possession or sale of drugs, criminal harassment or extortion, graffiti or vandalism, and illegal weapons possession.[4] Violating the order is a misdemeanor offense carrying fines of up to $1000 and imprisonment of up to 6 months (Maxson et al., 2005). If the violation involved illegal activity, these misdemeanor penalties would be applied in addition to those arising from the felony.

The CGIs list one or more gangs as defendants on the order and may list individual members of the gangs or a certain number of unspecified "Does". For example, one injunction lists the defendant as "MARA SALVATRUCHA aka MS, an unincorporated association; DOES 1 through 500, inclusive".[5] This practice gives police the discretion to add defendants to the injunction after it has been permanently granted, thereby broadening the order as desired (O'Deane, 2012). After an initial complaint is filed

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[4]The full text and maps of all permanent injunction orders is available at https://www.lacityattorney.org/gang-division, as well as additional information about gang injunctions policing in LA.
[5]https://filedn.com/lOJqn8isbUNJvUBnJTlV5OS/MARA%20INJUNCTION.pdf

and before a permanent injunction is granted, defendants must be notified of the action taken against them. In cases where no individual defendants are listed, City Attorneys and police officers identify representatives of the gang to serve with the complaints and assume that they will communicate their contents to other members (O'Deane, 2012).

Despite a relatively lower number of individual defendants listed on CGIs, over 50,000 people were recorded as LA gang members in the California Gang Database, known as CalGang, in 2003 (Los Angeles Police Department, 2003).[6] Unlike with CGIs, inclusion in the CalGang database was not subject to any legal review process or requirements to give notice to suspected gang members, even if they were minors.[7] It is possible that this provided a useful resource for quickly adding individuals to the gang injunction when expedient. As suggestive evidence to this effect, a 2011 audit found low rates of police compliance with procedures for serving CGI orders to defendants, particularly for getting prior legal approval for service and for notifying the guardians of minors served (Office of the Inspector General, 2012).

All approved CGIs in Los Angeles were eventually granted permanent status, typically shortly after the initial complaint was filed, and maintained fixed geographic boundaries designating the safety zones. Figure 1 displays a map of the safety zones, shaded according the year of implementation. By 2013, these areas covered about 22% of Los Angeles (Ridgeway et al., 2019). However, as part of a then-ongoing lawsuit, a federal court in 2018 barred Los Angeles from enforcing its existing CGIs, ruling that it was likely to find due process was not available to defendants to challenge the orders. The case was eventually settled in 2020 resulting in a prohibition on LA from enforcing CGIs until defendants had an opportunity to challenge them in court, and requiring their expiration after five years (Queally, 2020).

3   Conceptual Framework

I outline a conceptual model to frame the pathways through which proactive policing may influence education outcomes. I employ a simple model of human capital development over the life cycle, and relate this to both the decision to engage in illegal activity at a later age, as well as allowing for adverse effects of local crime and policing activity on cognitive development.

Children are endowed with an initial stock of human capital, h_{i0}, which evolves each year according to a linear technology that takes as inputs prior human capital, individual (or family) investments, θ_{it},

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[6]More recent figures are not reported, but 2003 is relevant as it is the beginning of my sample period.

[7]This changed in 2014, when a new law dictated the parents of juveniles must be notified upon their inclusion in CalGang (Winton, 2016).

and neighborhood conditions including levels of crime, C_{st}, and arrests, R_{st}^d, as follows:

$$h_{it} = \beta_1 h_{i,t-1} + \beta_2 \theta_{it} + \beta_3 h_{i,t-1}\theta_{it} - \beta_4^d C_{st} - \beta_5 R_{st}^d + \eta_{it} \tag{1}$$

where i indexes an individual, t the year, s the school or neighborhood, d a demographic grouping, and η_{it} is an idiosyncratic shock.[8] Individual investments can be thought of as student effort, or family investments into a child's cognitive development, which have positive returns, $\beta_2 > 0$. The technology exhibits dynamic complementarities in investments, that is $\beta_3 > 0$, and I assume that human capital depreciates over time, $0 < \beta_1 < 1$.

Neighborhood conditions of interest include the crime rate, C_{st}, and the arrest rate pertaining to an individual's demographic group, R_{st}^d. Neighborhood crime enters the equation negatively as research has consistently shown that exposure to direct or community violence has deleterious effects on student achievement, likely due to disturbances in executive function caused by stress (Sharkey, 2018). In Los Angeles, girls are more likely to be victimized than boys and women experience more violent victimization than men.[9] These diverging experiences motivate the parameter β_4^d to vary by demographic group, as the benefits of lower crime rates are likely to accrue more to potential victims. Arrest of a family member or close relation could also create cognitive disruptions for children, though little research has studied this.[10] Los Angeles' gang injunctions targeted predominantly Hispanic and African American men. Moreover, prior literature has established that while participation in criminal activity is typically higher among these groups, this is not the case if they are immigrants (Butcher & Piehl, 2007).[11] The degree to which youth are exposed to arrest-related family disruptions will depend on who participates in crime, and the rate at which police arrest both those who are involved in crime and those who are not.

Each period students and their families choose to make investments in human capital, θ_{it}, at some cost $c(\theta_{it})$, with the expectation that these will pay off in adulthood through wages $w(h_{iT})$. However, adults may choose instead to obtain earnings through criminal activity, y, which requires no human

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[8]I abstract from school inputs in this model as I do not expect them to change in response to the proactive policing policy in the time frame examined. In the model, students live in the neighborhood of their assigned school and do not relocate. In the empirical analysis, I establish that there is no significant relocation in response to the policy, and restricting to students attending their assigned neighborhood school has little bearing on the results.

[9]Author's calculations using data described in Section 4.4.

[10]An emerging literature on parental incarceration has found evidence that it is beneficial to children since, on average, incarcerated family members were contributing negatively to the child's home environment (Arteaga, 2021; Norris et al., 2021). On the other hand, research focused on the arrest of a family members finds that this is harmful to academic outcomes (Billings, n.d.).

[11]It is important to note that true participation in crime is not observed and can only be estimated by victims' reports, arrests, and incarceration, all of which are imperfect measures likely to reflect human biases (Owens, 2020).

capital but increases the likelihood of being arrested. I build on Owens (2020) in modelling police behavior and the relative payoffs of legal and illegal earnings. Police rely to some degree on demographic targeting, arresting those committing crime at rate p_a^d, but occasionally arresting the innocent with probability p_A^d. Being arrested when acting illegally and legally comes with associated costs, f_a and f_A. The resulting optimization problem faced by individuals in a given period is thus:

$$\max_{C, \bar{\theta}_{it}} \quad C_i \Big[(1 - p_a^d) U(y) + p_a^d U(y - f_a) \Big] + (1 - C_i) \Big[(1 - p_A^d) U(w_i) + p_A^d U(w_i - f_A) \Big] \tag{2}$$

where C_i is a binary variable indicating criminal activity in adulthood, $\bar{\theta}_{it}$ is the time path of investments from the current period t to the terminal (adult) period, T, and expected wages are net of the costs of these ongoing investments, $w_i = w(h_{iT}(h_{it}, \bar{\theta}_{it}, C_{st}, R_{st}^d)) - c(\bar{\theta}_{it})$.

Police are more effective when there is a high rate of foiling crimes for any demographic group, as captured by p_a^d. Gang injunctions grant police enhanced powers to arrest certain groups of people in a specific area, with the goal of increasing this rate of success. However, if this comes with an increase in the arrest of innocent individuals, p_A^d, then it is possible that the relative cost of crime actually decreases. Even a perception that police are biased and arrest innocent individuals based on their gender, race, or ethnicity, could lower the expected return to investing in education for individuals belonging to that demographic.

For adults engaged in criminal activity, changes in police behavior that increase p_a^d and p_A^d may create a deterrent effect, if crime becomes less attractive, and an incapacitation effect, if more people are arrested.[12] These channels influence student learning through their effects on human capital. The level of crime in a student's neighborhood is the number of people who choose to commit crimes, less those arrested. The level of family disruption, R_{st}^d, is determined by total arrests of both innocent and guilty individuals in one's demographic group. As a consequence of dynamic complementarities in the human capital technology, favorable changes in these variables will increase the return on future education investments. Thus, we should expect to see persistent improvements in achievement over time if proactive policing creates lower crime rates, and worsening achievement from family disruptions.

To summarize, there are three main pathways through which implementing a proactive policing policy may affect academic outcomes. If the policing strategy lowers crime, whether through deterrence or incapacitation, students should benefit through safer environments and lower victimization. The degree of benefit likely correlates with gender due to differences in victimization. On the other hand,

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[12]It is worth noting that the incapacitation effect alone is sufficient to result in a decline in crime rates, even if crime does not become less attractive.

high rates of arrests of adults in your demographic group increases the likelihood of family disruptions that impede cognitive development. While these disruptions should impact male and female children similarly, exposure may vary by ethnicity and immigration status. Lastly, ineffective policing or perceptions of police bias reduce the expected returns to education through relatively high p_A^d. If students perceive that earnings from crime are more profitable than legal earnings, the model predicts they will cease investing in education, and human capital will gradually depreciate.

4   Data

4.1   Student Administrative Data

I use a rich administrative panel dataset of students attending schools in a public district from academic year (AY) 2002-03 to 2016-17. The sample includes the universe of non-charter public schools in the district, which educates students from primary through high school levels. The data comprise socio-demographic characteristics of students, including gender, ethnicity, free- or reduced-price lunch status, and parental education; details of student transcripts; records of absences and suspensions; and standardized test scores from annual tests, disaggregated by subject. Though I do not observe student addresses, the data record both the school they attend as well as the school they are assigned to attend based on their residence.[13] This allows for ascertaining some degree of student mobility and demographic characteristics at the neighbourhood level.

I focus my analysis on students in primary school grades, that is, kindergarten to grade 8. I use standardized test scores in math and English to examine cognitive effects of the policy, and the number of days absent and suspended to investigate behavioral effects. The California Standards Test (CST) was a grade-specific test taken in every grade from 2 to 11. Above grade 7, the math test a student takes becomes specific to the math course in which they are enrolled and courses begin to vary in content and difficulty. To avoid issues with endogenous selection into streams, I only use math scores up to grade 7. California standardized testing was put on hold in AY 2013-14 and a new testing regime replaced the CST in AY 2014-15. I restrict my analysis to years in which CST scores are available, 2002-03 to 2012-13, to ensure comparability and minimize measurement error.

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[13]The data contain students' home ZIP codes but they are constant within student, suggesting that they are not updated when a student moves. Though this is helpful for pinning down where students live, it will be inaccurate for mobile students.

4.2   Test Score Standardization

I interpret the CST score, scaled to be $Y_{igst} \in [150, 600]$, as a linear transformation of human capital, test-coaching, and idiosyncratic disturbances.[14] Test coaching captures elements of teaching that do not improve human capital but improve test scores, such as teaching a formulaic process to answering common types of test questions that doesn't rely on conceptual understanding. Let K_t be a measure of test coaching such that $K_t \geq K_{t-1} \forall t$. As more sample test material becomes available over time and schools are pressured to improve test scores, coaching should increase. The effect of coaching on realized scores may vary across grades due to differences in test construction. Using ρ to denote the scaling parameter and ω_{igst} measurement error for student i in grade g, school s and year t, the functional form of test scores is:

$$Y_{igst} = \rho + h_{it} + \gamma_g K_t + \omega_{igst} \tag{3}$$

In order to best isolate the human capital component of the test score measure, h_{it}, a transformation of this variable is necessary. Standardizing on grade-year within the sample would yield the following:

$$y_{igst} = \frac{Y_{igst} - \overline{Y}_{gt}}{\sigma_{gt}} = \frac{h_{it} - \mathbb{E}[h_{it} \mid g, t]}{\sigma_{gt}} + \frac{\omega_{igst} - \mathbb{E}[\omega_{igst} \mid g, t]}{\sigma_{gt}}$$

where $\sigma_{gt} = \sqrt{\mathbb{E}[Y_{igst}^2 \mid g, t] - \mathbb{E}[Y_{igst} \mid g, t]^2}$. Since $\mathbb{E}[\rho \mid g, t] = \rho$ and $\mathbb{E}[K_t \mid g, t] = K_t$, this standardization purges the scaling and coaching terms from the test score measure.[15] A drawback of this approach is that it forces a static distribution of human capital within the district over a period when investments may change substantially due to the treatment. An alternative that preserves treatment-driven variation is to use *state-wide* moments from the CST to standardize individual scores. That is, I replace $\overline{Y}_{gt}$ with the mean CST score in all of California in grade g year t, $\overline{Y}_{gt}^C$, and σ_{gt} with the corresponding standard deviation, σ_{gt}^C. This is my preferred approach to standardizing as it relates individual scores to a broader population of test takers that are less likely to be affected by policy changes in the district of interest. It achieves the same goal of reducing extraneous terms from equation (3) as using within-district moments.

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[14]I interpret a gradual rightward shift over time in the test score distribution as evidence of teaching to the test. This is plausible as the testing regime was somewhat new at the beginning of my sample period, and more test practice materials became available over time.

[15]This is equivalent to including grade-by-year fixed effects in an estimating equation and expressing the resulting estimates in units of grade-year standard deviations.

4.3   School Boundaries and Treatment Definition

I link CGIs and schools geographically using map data of safety zones and school catchment areas. Catchment areas, or school attendance boundaries, assign students to schools based on their home address. All students have the right to attend their residentially assigned public school, though some may opt to attend other schools in the district through a variety of school choice programs.[16] As most students both live and attend schools within these boundaries, they provide a useful geographic delineation of a child's neighborhood and school environment.

Shapefiles of all Los Angeles safety zones and information on their attributes were generously shared by Ridgeway et al. (2019) who compiled them through public records requests filed with the LA City Attorney along with independent mapping. Shapefiles of annual school attendance boundaries were obtained from the district through a public records request. I define school exposure to CGIs using the geographic overlap between safety zones and catchment areas. If at least 75% of a school's catchment area is overlapped by an active safety zone, I consider the school treated by the policy. A small subset of schools in the sample offer district-wide specialized programming and therefore do not have defined attendance boundaries. These represent 6% of the sample of elementary schools and 2% of students. I map these schools to safety zones using point locations instead of attendance boundaries.[17]

Table 1 summarizes variables of interest across treated and control units for the sample of schools and students. Safety zones were not applied at random and as a result the schools they overlap and students therein differ along several dimensions from others in the district. While the empirical design will only make within-school or within-student comparisons, the sample characteristics are informative. The sample as a whole is largely made up of elementary schools which enroll children in kindergarten to grade 5. This is expected as middle schools generally pull from a larger population and are thus fewer in number. A substantial majority of students are of Hispanic ethnicity and over half are at some point classified as English learners. Treated students are disproportionately Hispanic, 82% relative to 67% of never treated students. This is offset by fewer White and Asian students, while the share of Black students is similar at just above 10%. Treated students are also comprised of a greater share of English learners, 66% relative to 45%, and free- or reduced-price lunch eligible students, at 96% relative to 81%. Similarly, treated students are less likely to have a parent with a college degree. Relative to the rest of California, students in the district score slightly worse on standardized tests, as indicated

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[16]These include charter schools, which are not included in the sample, as well as magnet schools and traditional schools accessed through transfer programs.

[17]Maps of school attendance boundaries shaded according to treatment year are provided in Figure A1. Schools without defined boundaries are plotted as points instead of polygons.

by negative mean scores which have been standardized relative to state-wide averages. Though treated students score lower on standardized test scores relative to never treated students, they are recorded as having similar rates of being absent or suspended. They are somewhat more likely to attend their residentially-assigned school and less likely to leave the district prior to grade 8 graduation.

4.4   Crime Data

The Los Angeles Police Department (LAPD) publicly reports quarterly crime totals by major crime category and reporting district. Seven types of crimes are included: aggravated assault, burglary/theft from a vehicle, burglary, grand theft auto, grand theft person, homicide, and robbery. I use data from Ridgeway et al. (2019) who digitized these paper reports from 1988 to 2014.[18] The LAPD are responsible for policing the city of Los Angeles alone, with other cities in LA county (e.g. Compton) either having independent police forces or contracting the policing services of the LA County Sheriff's Department. Safety zones are contained within the LA city limits and CGIs are enforced by the LAPD. According to shape files, 73% of school catchment areas in my sample are covered by LAPD reporting districts. Appendix Figure A2 displays a map of LAPD crime reporting districts (RDs) in the left panel, and 2014 elementary school catchment areas with RDs overlaid in red in the right panel.

I estimate a school's exposure to localized crime using an area-weighted average of crimes in the RDs overlapping the school's catchment area. For the small number of schools without a traditional attendance boundary, I use the crime totals in the RD in which the school is located. I aggregate quarterly crime totals to correspond to the academic year (July-June). Schools are included in the crime exposure dataset if at least 80% of their catchment area is covered by LAPD reporting districts. This includes all schools in the treated sample and over two thirds of schools in the control sample.

More detailed data on crime reports and arrests in the city of Los Angeles are available beginning in 2010. The incident-level crime reports are precisely geo-coded and include the date and time of the occurrence and report; the age, gender and race of the victim; over 100 different categorizations of crimes; and whether the incident led to the arrest of an adult or juvenile. Data on arrests are similarly geo-coded and time-stamped, but also list demographic details of arrestees. The crime reports and arrests data are separate samples which do not perfectly correspond. The arrests data comprise a broader set of police interactions which result in apprehensions, and the crime reports contain many crimes that do not result in an arrest and remain unsolved. The arrests data also have additional categories of offenses not included in the crime reports, such as narcotics and parole violations. Though

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[18]Accessed from https://github.com/gregridgeway/LAPDcrimedata

both datasets do not overlap well with my sample period, they are useful for further contextualizing the effects of gang injunctions on the more aggregated crime data as well as student outcomes. In the absence of student home addresses, I again use crimes reported within school catchment areas as a proxy for student crime exposure.

Table 2 reports summary statistics across treated and control schools of crime reports from the aggregated data for 2003 to 2013, as well as from the geo-coded datasets for 2010. Rates depict the number of reported crimes or arrests per 100 students enrolled in the school. The table depicts a higher rate of violent crimes in treated areas, though the geo-coded data from 2010 depict fewer overall crimes in these areas due to lower rates of property crime. Women and girls are more likely to be victims than men and boys in treated areas, particularly for violent crimes. About one in ten crimes results in the arrest of an adult throughout the city. About 80% of those arrested are male, and arrests are more common in treated areas for violent crimes and in untreated areas for non-violent crimes, reflecting differences in crime rates.

5   Empirical Methodology

I use an event study design to estimate the effects of new CGIs on student outcomes, leveraging the panel nature of the data and the staggered and plausibly exogenous timing of new safety zones. Though my main specification of interest is a student-level event study, I first present results from analogous school-level regressions to establish effects of the policy on local reported crime and school-averaged achievement.

5.1   School Event Study

I employ school-level regressions both to provide context to student-level estimates, through analysis of crime and achievement effects, as well as later in the paper to investigate school composition changes and student sorting. The school-level analysis has the added benefit of offering a treatment unit that is highly balanced in the panel, and which is fixed in space. This stands in contrast to the student-level dataset, which is a rotating panel as students matriculate and graduate, and where students are potentially mobile within the district and beyond. Thus, aggregating to the school level is informative in capturing broader average treatment effects and addressing potential selection concerns arising from students moving across schools or out of the sample.

Treatment occurs when a school's catchment area becomes at least 75% overlapped by a safety

zone. For schools with no attendance boundary, treatment occurs upon implementation of the safety zone inside which the school is located. I exclude treatment events where the school had previously been exposed to treatment in 33% or more of their school catchment area, but had not exceeded the 75% threshold. This serves to capture large, discontinuous increases in exposure to safety zones, and removes more gradual treatments where many students were already affected in the pre-treatment period.[19]

Let IY_s be the year a safety zone was implemented around school s and $D_{s,t}^k = \mathbb{1}\{t - IY_s = k\}$ indicate treatment of school s, k years relative to academic year t. I estimate the following school event study:

$$y_{st} = \sum_{k=-4}^{-2} \beta_k D_{s,t}^k + \sum_{k=0}^{4} \beta_k D_{s,t}^k + \alpha_s + \pi_t + \epsilon_{st} \tag{4}$$

where y_{st} is the mean outcome in school s in academic year t, α_s is a school fixed effect, π_t is academic year fixed effects and standard errors, ϵ_{st}, are clustered at the school level. I omit the relative treatment indicator for the year before treatment, $D_{s,t}^{-1}$, using this as the base year for comparison. The parameters of interest β_k capture the average within-school change in the outcome y_{st} from the year before treatment, $k = -1$, to k years after treatment relative to never treated schools in the same years.

The β_k parameters estimate the dynamic average treatment effects on the treated under three assumptions. The first is the parallel trends assumption, which asserts that differences in the potential outcome between periods are the same in the absence of treatment across all treatment cohorts and the never-treated. The second requires that there are no treatment effects prior to the initiation of treatment, also known as the no anticipation assumption. To state this mathematically, I borrow notation from Sun and Abraham (2021) that will easily generalize to the student event study in the following section. I denote year c as the treatment cohort, where $c = \infty$ for never treated units. Let $y_{i,t}^c$ represent the potential outcome of unit i (schools, or in the preceding section, students) in period t under treatment condition c. The assumptions are as follows:

(1) (Parallel Trends) $\quad \forall p \neq t, \mathbb{E}[y_{i,t}^\infty - y_{i,p}^\infty \mid IY_i = C]$ *is the same for all* $c \in supp(IY_i)$

(2) (No Anticipation) $\quad \mathbb{E}[y_{i,c+\ell}^c - y_{i,c+\ell}^\infty \mid IY_i = c] = 0$ *for all* $c \in supp(IY_i)$ *and all* $\ell < 0$

In estimating the event study for the effects of CGIs on reported crime, the parallel trends assumption

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[19]In Section 8, I show that results are largely similar using cutoffs other than 75% and 33%.

requires that safety zones are not selected on pre-existing trends in crime. This would be violated if CGIs are used as a tool to respond to increasing levels of crime in a neighborhood. Given the burden of evidence required for obtaining a permanent injunction order against a gang, and the highly fixed nature of safety zones, I argue that it is unlikely that CGIs were used for this purpose. Significant police work was needed to establish the gangs presence, membership, and activities in an area (O'Deane, 2012). Moreover, once a safety zone's boundaries were determined, they could not be adjusted to follow new crime spikes. Thus, CGIs are not well-suited to addressing recent and potentially transitory crime trends. The anticipation assumption would be threatened if crime-involved individuals were aware of coming injunctions and pre-emptively changed their behavior. Injunction records suggest this was typically not the case, as defendants rarely became involved in the legal process and permanent orders were often granted within months of the initial request. Empirical evidence in support of satisfying these assumptions are β_k estimates that are close to zero for all $k < 0$. Throughout my analysis, I present estimates of these parameters and report the p-value from a joint test of $\beta_k = 0 \,\forall\, k < 0$, as suggested by Freyaldenhoven et al. (2019).

Building on recent research on the validity of two-way fixed effects estimates from models with staggered treatment designs, Sun and Abraham (2021) show that a third condition for obtaining unbiased estimates of β_k is treatment effect homogeneity across treatment cohorts. In my setting, treatment effect heterogeneity is plausible as earlier safety zones may have targeted more crime-prone and economically disadvantaged areas and implementation may have varied over time. Sun and Abraham highlight several empirical adjustments that reduce bias arising from potential heterogeneity, which I implement. First, I use never treated units as a control group, not eventually treated or early treated units. Second, I truncate treated units outside of four years pre- and post-treatment, alleviating issues associated with censoring. As a robustness check, I estimate the event study using Sun and Abraham's reweighting procedure to adjust for treatment effect heterogeneity.

5.2 Student Event Study

The student-level event study design uses a similar regression framework but focuses only on students who were enrolled in treated schools when safety zones were first implemented. Let C_i be the first year student i attended school inside a safety zone. The treatment indicators for student i are absorbing and defined as $D_{i,t}^k = \mathbb{1}\{t - IY_s = k; C_i = IY_s\}$. The condition $C_i = IY_s$ restricts the treated students to those whose initial treatment coincided with the school's initial treatment. This allows me to focus on the effect on students of *newly* implemented safety zones, and to avoid the triggering of treatment

by a change of school attendance, which likely correlates with other unobserved influences.

The student event study with two-way fixed effects is:

$$y_{igst} = \sum_{k=-4}^{-2} \beta_k D_{i,t}^k + \sum_{k=0}^{4} \beta_k D_{i,t}^k + \alpha_i + \pi_t + \delta_e t + \epsilon_{igst} \tag{5}$$

where y_{igst} is the outcome of student i in grade g school s in academic year t, α_i is the student fixed effect, and $\delta_e t$ are ethnicity-specific time trends.[20] Standard errors, ϵ_{igst}, are two-way clustered at the student and school level. The parameters of interest, β_k, measure the average within-student change in y_{igst} from the year before treatment to k years after treatment, for treated students relative to never treated students in the same years. Since test scores are constructed as a relative measure within grade, it is not necessary to include grade fixed effects in regressions with test score as the dependent variable. However, I add these controls for behavioral outcomes, as absences and suspensions are strongly positively correlated with grade.

The event study assumptions of parallel trends, no anticipation and homogeneous treatment effects apply similarly here. The parallel trends assumption that safety zones are not selected on pre-existing trends in student outcomes could be violated if students select into or out of treated schools based on their academic trajectory. I show empirical evidence that statistically significant pre-trends are not observed in the data, and rule out confounding effects of endogenous student mobility in Section 8. As described above, I do not expect high salience of impending CGIs and a resulting anticipatory response by students or their families.[21] As in the school-level event study, I present coefficient estimates from four years leading up to treatment and p-values from joint tests of mutually zero point estimates in support of these assumptions. I again use only never-treated students in the control group, truncate to four pre- and post-treatment periods, and present robustness checks in Section 8 using Sun and Abraham's interaction-weighted treatment effects estimator.

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[20] The data report six categories of student ethnicity: American Indian, Asian or Filipino, Black, Hispanic, Pacific Islander, and White. Early exploration of the data indicated statistically significant sample-wide trends in test scores across these groups. To increase precision and avoid omitted variable bias driven by a correlation between ethnicity and student treatment, I control for this trend in all student-level regressions.

[21] Although the final gang injunctions implemented in 2013 were subject to significant community scrutiny and comment (Muniz, 2015), I have been unable to uncover any reports of similar public discourse regarding safety zones prior to this. As these were introduced in the final year of my sample, they would be used only to estimate pre-treatment coefficient estimates, which are on average statistically indistinguishable from zero.

6 Results

6.1 Crime Effects

I present empirical estimates of equation 4 in Figure 2, where the dependent variable in Figure 2a is annual violent crimes reported per thousand students, and in Figure 2b is annual non-violent crimes reported per thousand students. Violent crimes include aggravated assaults, robberies and homicides, and non-violent crimes include burglaries and thefts from vehicles, general burglary, grand theft auto and grand theft person.[22] The plots display point estimates and 95% confidence intervals for each $\hat{\beta}_k$ and indicate the reference period $k = -1$ as a point on the x-axis. I find that there are no statistically significant pre-trends in either violent or non-violent crime, and fail to reject joint tests for both outcomes that all pre-treatment coefficients are zero. Consistent with prior literature, I find that violent crimes declined by about 11% after four years as a result of LA's gang injunctions, relative to an average of 146 crimes reported per thousand students in treated schools the year before treatment. As in Ridgeway et al. (2019), most of this decline is driven by aggravated assaults, which fell nearly 30% after four years. Non-violent crimes were also reduced, by about 7%, but effects are only significant in the fourth year following treatment.

These measures of crime naturally contain some measurement error as crime reports were aggregated to LAPD reporting districts which did not perfectly overlap school attendance boundaries. As an additional source of evidence, I plot a more precise measure of total crime for schools treated in 2009 against never treated schools. These data are only available in the post-treatment period, 2010 to 2019, but allow me to map geo-located crime incidents to the school boundaries in which they occurred. After residualizing the data on common year effects, Figure 3 plots the average within-school percent change in total reported crimes, weighted by student enrollment. The figure shows crime declined by about 10% from 2010 to 2012 for those schools treated in 2009, which is consistent with the causal event study estimates. Appendix Figure A4 shows that these results are similar for both violent and non-violent crimes.

Using schools as the unit of analysis, I confirm the finding that CGIs were effective at reducing crime. Whether these improvements in reported crime translated to better education outcomes is the question of primary interest to this research, to which I now turn.

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[22] Appendix Figure A3 reports results for all categories of crime individually.

6.2 Achievement Effects

I first document average treatment effects at the school level. Figure 4a and Figure 4b display point estimates and 95% confidence intervals from the event study for school-averaged math and English scores, respectively. Recall that the year before treatment, $k = -1$, is omitted from the regression as the baseline period against which other relative treatment years are compared. For both math and English, estimates in the pre-treatment period are statistically indistinguishable from zero, providing support for the key identifying assumptions of parallel trends and no anticipation. In both regressions, I fail to reject that the pre-treatment coefficients are jointly zero. Treatment effect estimates following the change in policing are positive and increase gradually over time. Three years after the initiation of safety zones, students in treated schools see 0.09σ higher scores in math and 0.05σ higher scores in English tests relative to those in never treated schools.

These estimates indicate that average school achievement improved as a result of gang injunctions, possibly due to their negative effect on crime. To quantify individual learning, I estimate the empirical model for students that compares each child to their own earlier achievement level. Results from this student-level event study specification are presented in Figure 5a and Figure 5b. Though qualitatively similar to the school estimates, the student event study coefficients are smaller in magnitude. Treated students improve scores by 0.05σ in math and 0.02σ in English three years after exposure to CGIs, the latter estimate being statistically significant only at the 10% level.[23] Coefficient estimates in the pre-treatment period are again statistically indistinguishable from zero both individually and jointly. Though point estimates appear to trend upwards somewhat for math, the sample size becomes limited three and four years prior to treatment due to larger early treatment cohorts.

6.3 Treatment Effect Heterogeneity

While these average treatment effects are notable, they could be masking significant heterogeneity if not all students are affected by policing activity and lower crime in the same way. As outlined in section 3, we might expect varying effects across gender, due to differences in victimization and crime rates, ethnicity, as police may rely on racialized criteria to identify suspected gang members for arrest, and immigrant status, on account of differences along this dimension in criminal activity and attitudes

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[23]In considering the discrepancy between the school and student treatment effect sizes, it is important to note how restrictions on the treatment group vary across the two models. In student regressions, student composition is fixed and I restrict to those who were enrolled at the school's initial treatment time in order to isolate a child's response to treatment. At the school level, I make no such restriction as I endeavor to quantify whole-school changes in achievement. In section 8, I examine changes in who attends treated schools and whether this contributes to the different effects.

towards police documented in the literature. If there are indeed dynamic complementarities in human capital production, students treated at an earlier age will be more sensitive to these crime and policing changes. An examination of heterogeneous treatment effects across fixed characteristics of students is therefore of key importance to this study. To investigate varying treatment effects across student characteristics, I estimate equation 5 interacting the relative treatment indicators, $D_{i,t}^k$, with binary measures of fixed student characteristics. For these heterogeneous specifications, I pool the 3- and 4-year treatment leads as the sample size in these relative periods becomes quite small upon sample segmentation.

Gender

First, since underage girls are more often victims of crime than underage boys, and women are more likely to be victims of violent crime than men, it may be the case that female students see greater achievement gains as a result of the crime reduction benefits of the policy. Moreover, men are both more likely to participate in crime and to be listed on gang injunctions, so channels relating to policing behavior may be more relevant for that group. Figure 6 and Figure 7 present event study plots for female students in panel (a) and male students in panel (b), for math and ELA scores, respectively. The results indicate that female students gain substantially more from the policy than male students in both subjects. After four years, female students improve test scores by 0.10σ in math and 0.08σ in English, whereas male student test scores decrease somewhat by 0.03σ in both math and English. The policy effectively closes the gender gap in math after two years.

English Learners

An important characteristic of my sample to consider is the high number of English learner (EL) students. Los Angeles has a large immigrant population, and EL status is an accurate proxy measure for first- or second-generation immigrant status (National Academies of Sciences & Medicine, 2017). Immigrant communities are likely to have unique relationships with law enforcement that are informed by factors in the immigration decision, cultural background and concerns around potential deportation. In the school setting, these students face the additional burden of learning the language of instruction. I define EL students to be those who were classified, through required testing, as English learners at any point in the sample period. Figure 6 and Figure 7 present event study plots for EL students in panel (c) and non-EL students in panel (d), for math and ELA scores, respectively. They show that EL students, like girls, benefit significantly more from the institution of gang injunctions than non-EL students. EL students improve scores 0.07σ in math and 0.06σ in English four years following treatment,

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whereas non-EL students lower scores by 0.05σ in math and 0.08σ in English relative to the year before treatment. Hispanic students make up the majority of both of these groups, and effects for Hispanic students within either category largely reflect the total group effect.[24]

Ethnicity and Poverty

I also make note of heterogeneity by student ethnicity and poverty status. Examining heterogeneity across these dimensions is of interest in order to understand the distributional consequences of the policy, which predominantly targeted Hispanic and Black men in high-poverty neighborhoods. However, I am limited by the high concentration in the treatment sample of Hispanic and high-poverty students. This is especially pronounced in the measure of poverty, eligibility for a free- or reduced-price lunch, which describes 96% of treated students, and is thus omitted.[25] Figure 6 and Figure 7 present event study estimates for math and English, respectively, by the largest ethnic groups, in order: Hispanic (panel (e)), Black (panel (f)), Asian/Filipino (panel (g)), and White (panel (h)). The results for Hispanic students largely reflect the average treatment effects in the full sample. I uncover no statistically significant effects for White or Asian students, though statistical power is limited. Black students, who make up just over 10% of the treated sample, improve in math by 0.09σ after three years but not in English. Exceedingly few Black-identifying students are English learners, but supplemental analysis uncovers that achievement gains are driven by female students.

Age at Initial Treatment

Considering the robust literature on critical ages of human capital development (Heckman & Mosso, 2014), whether younger students are more affected by interventions is a natural question. Estimating heterogeneity by age of first treatment is challenging using the event study approach, as students are observed in the panel for at most six years for math tests and seven years for English tests. For example, fixing the age at first treatment to 11 would leave only one to two years post-treatment from which to estimate effects. For this reason I employ a more general difference-in-difference analysis for age heterogeneity that pools pre-treatment years and post-treatment years and truncates past four years on either side of the treatment window. I split students into those who were treated under age 10, and those treated at age 10 and older. Results are presented in Panel A of Table 3 where the 'post' coefficient captures the average difference between students' pre-treatment and post-treatment test

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[24]Isolating the effects for other ethnicities interacted with EL status, such as Black students, is challenging due to the small sample size of non-Hispanic populations.

[25]An alternative measure is an indicator for whether the parents' highest education level was less than a high school diploma. However, the non-response rate for this variable is high. These results are available upon request, and resemble those of English-learner students (a highly correlated measure) but with point estimates that are smaller in magnitude.

scores, compared to never treated students in the same years, conditional on being treated under or over age 10. The estimates suggest that being exposed to safety zones at younger ages is effective at boosting math achievement, but not English achievement, which benefits from being treated at age 10 and over. This result is intuitive for math, where we might expect large dynamic complementarities as a result of the highly cumulative nature of math skills, however the logic behind this opposing result for English is less clear. Interacting the binary treatment-age measure with EL status, I show in Panel B that this is driven by EL students. This could be explained by greater English proficiency at older ages for sometime-English learners, allowing them to fully realize the potential gains from the policy.

Interactions between Gender and English Learners

I further explore heterogeneity across interactions of gender and EL status. Figure 8 plots event study coefficients across gender and English learners, for math in the left column and English in the right column. This additional partition of students is highly informative. I find that four years after gang-injunction implementation, female sometime-English learner students improve math scores by 0.14σ and English scores by 0.12σ compared to never exposed students. In stark contrast, male non-English-learner students suffer declines of 0.11σ in math and 0.13σ in English test scores. Other groups show no statistically significant treatment effects. These results suggest the benefits and costs of the policy were quite concentrated among certain students. The remainder of this section explores how these effects correspond to behavioral changes, before turning to mechanisms more broadly.

6.4 Behavioral Effects

I present event study estimates of the effects of gang injunctions on suspensions and absences to investigate whether student behavioral responses to the policing intervention are consistent with academic responses. Here I focus on the heterogeneous treatment effects of interest informed by the the prior section. Figure 9 plots coefficient estimates using the number of days suspended as the regressor, for the four groups interacting gender and English learner status. The results correlate strongly to those for achievement. Female English-learner students see a reduction in the number of days suspended, whereas male non-English learners are suspended more days as a result of treatment. This variable should be interpreted with some caution. The low incidence of suspensions makes results sensitive to outliers and as a result estimates for male students fail the no pre-trends test. Nonetheless, the consistent pattern of results is highly suggestive that either student behavior or school disciplinary practices were affected by gang injunctions.

[Figure 10](#) presents analogous results for the number of days absent. Unlike for other outcomes of interest, the effects on absences are quite homogeneous across groups. All, with the exception of male English-learners, see absences decline – or, put another way, attendance improve.[26] In the following section, I will investigate the linkage between these behavioral and achievement outcomes, and consider potential explanations for the lack of variation in attendance effects.

7 Mechanisms

Understanding why some children are better off and others are worse off under this policing strategy is necessary for recommending improvements to proactive methods or channels for mitigating their harms. In this section, I explore treatment effects pathways, both overall and for groups of interest who experience large changes in achievement. I begin by using mediation analysis to decompose the treatment effect into variation driven by channels that are measurable in the data and a remaining unexplained component. I proceed to explore additional datasets on crime and arrests that do not overlap well with my sample period but are informative for understanding how gang injunctions are enforced and affect victimization across groups. Finally, to explore potential hypotheses for remaining unexplained heterogeneity for immigrants, I consider facts established by the social science literature and their implications with respect to the conceptual model.

7.1 Mediation Analysis

To better explain the varying treatment effects of gang injunctions found in the main analysis, I first leverage the rich information contained in the data on students, their schools and their local environment. The available data allow for an examination of three channels of interest: student behavior, local crime, and changing school inputs. Prior research suggests there is a negative causal effect on achievement of both absences (Liu et al., [2021](#)) and suspensions (Pope & Zuo, [2020](#)). Though student absences and suspensions are outcomes of interest in themselves, the degree to which they correlate with treatment effects for achievement help give context to the academic dynamics. Of course, it is expected that less crime is positive for academic outcomes, as existing evidence is clear that the stress of localized violence has negative consequences for mental health and cognition (Laurito et al., [2019](#)).

Lastly, if school neighborhoods become more safe this could improve the school's ability to attract high-quality teachers. To probe this potential channel, I estimate teacher value added (TVA) using a

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[26]Note that absences do not include days missed due to out-of-school suspensions.

set of baseline years, 2003 to 2005. Value added is estimated using the standard fixed-effects approach with empirical Bayes shrinkage.[27] The vector of controls for residualizing test scores includes a cubic polynomial of lagged test scores; a set of individual covariates including gender, ethnicity, parental education, free- or reduced-priced lunch eligibility, English learner status, and age; and school-grade averages of both lagged scores and individual characteristics. I do not use the full sample period for this estimation, as I require a TVA measure that does not internalize any treatment effects realized later in the sample period.[28] I map the resulting value added measures for each teacher in math and English onto those who remain in the later sample of teachers from 2006 to 2013. The measure of (baseline-estimated) teacher quality is the mean TVA in either math or English at the school level. Any specifications involving these measures will control for the share of teachers for which TVA estimates are unavailable in the relevant subject.

I follow Heckman et al. (2013) linear model of causal mediation analysis and adapt it to the event-study specification. Let Y_d be the potential outcome given treatment status d, where d captures relative treatment periods and $d = \infty$ for never treated units. The set of measured mediator variables, $\theta_{\mathbf{d}}^{\mathbf{p}}$, is indexed by $j \in J_p$, and the set of unmeasured mediators, $\theta_{\mathbf{d}}^{\mathbf{u}}$, is indexed $j \in J \setminus J_p$. A vector of pre-treatment variables, $\mathbf{X}$, provides conditional independence of the treatment from the error term. Y_d is expressed as a linear combination of measured and unmeasured inputs, the control vector and an error term.

$$Y_d = \kappa_d + \sum_{j \in J_p} \alpha_d^j \theta_d^j + \sum_{j \in J \setminus J_p} \alpha_d^j \theta_d^j + X\beta_d + \tilde{\epsilon}_d, \qquad d \in \{-4, ..., 4, \infty\}$$
$$= \tau_d + \sum_{j \in J_p} \alpha_d^j \theta_d^j + X\beta_d + \epsilon_d$$

The second line rearranges this equation to group unmeasured mediators with the intercept term $\tau_d = \kappa_d + \sum_{j \in J \setminus J_p} \alpha_d^j E(\theta_d^j)$ and an error term $\epsilon_d = \tilde{\epsilon}_d + \sum_{j \in J \setminus J_p} \alpha_d^j (\theta_d^j - E(\theta_d^j))$. If the following assumptions hold:

(i) $\theta_{\mathbf{d}}^{\mathbf{p}} \perp\!\!\!\perp \theta_{\mathbf{d}}^{\mathbf{u}}$

(ii) $\alpha_d^j = \alpha_\infty^j \forall j \in J_p, d \in \{-4, ..., 4\}$

(iii) $\beta_d = \beta_\infty \forall d \in \{-4, ..., 4\}$

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[27]The approach is equivalent to (Chetty et al., 2014a) without the allowance for drift due to the limited time period.
[28]I also restrict estimation to teachers teaching grades 3 to 5. Grade 3 is the first year for which lagged scores are available, and I am not able to link teachers to students past grade 5.

the treatment effect can be decomposed as

$$E(Y_d - Y_\infty) = \underbrace{(\tau_d - \tau_\infty)}_{\text{direct effect}} + \underbrace{\sum_{j \in J_p} \alpha^j E(\theta_d^j - \theta_\infty^j)}_{\text{indirect effect through } \theta} \tag{6}$$

and Heckman and Pinto provide a two step procedure for estimating these causal parameters. In practice, these assumptions are challenging to meet. The first assumption asserts that measured and unmeasured mediators are independent. This is unlikely in my setting, as unmeasured aspects of policing could be correlated with changes in crime and also would help explain the heterogeneous effects observed. Because of measurement limitations, these unobserved policing influences will get loaded on to the crime variables, insofar as they are correlated. The other assumptions of constant counterfactual relationships between the outcome and both the mediators (ii) and the covariates (iii) are more plausibly satisfied. Nonetheless, I will interpret results of this analysis as suggestive evidence of potential channels, and not causally as Heckman and Pinto do.

Estimation proceeds in two steps. First, I estimate the relationship between the each of the mediating variables and the treatment, using the appropriate control vector given the dependent variable. Then, I estimate the event study holding fixed all mediator variables and any controls used in the first step.

$$\theta_{it}^j = \sum_{k=-4}^{-2} \mu_{1k} D_{i,t}^k + \sum_{k=0}^{4} \mu_{1k} D_{i,t}^k + \lambda_i + \pi_t + \epsilon_{it} \tag{7}$$

$$y_{it} = \sum_j \gamma_j \theta_{it}^j + \sum_{k=-4}^{-2} \mu_{2k} D_{i,t}^k + \sum_{k=0}^{4} \mu_{2k} D_{i,t}^k + \lambda_i + \pi_t + \epsilon_{it} \tag{8}$$

Heckman et al. show that under the assumptions, $\hat{\gamma}_j * \hat{\mu}_{1k}$ estimate the indirect effects of the observable mediators.

I present the resulting breakdown of the treatment effect into indirect effects operating through violent crime, non-violent crime, absences, suspensions, math TVA, and English TVA. The remaining unexplained component is the direct effect, and includes unmeasured mediators that are uncorrelated with those that are measured. The results of the mediation analysis are first presented in Figure 11 at the school level, since crime and TVA are measured by school. The y-axis denotes the relative treatment periods, and I focus interpretation on the third and fourth year post-treatment where coefficient are largest and most statistically significant. Violent crime is consistently associated with average test score improvements, explaining 15 to 20% of the variation driving treatment effects 3 and 4 years

post-treatment, with non-violent crime explaining some of the small gains in English scores. Lower absences also correlate with achievement gains, though this are imprecisely estimated and in the third year following treatment the average treatment effect on absences is positive and insignificant.[29]

Next, I focus on student groups of interest, presenting results at the student level in Figure 12 for female English learners in panels (a) and (b) and for male non-English learners in panels (c) and (d). The results suggest that variation in the mediators explain around 20% of female-EL achievement gains, but only around 10% of the losses for male non-EL students. For the girls, receiving higher value-added math teachers was an important contributor which may explain greater math than English gains. It is unclear why similar improvements in English TVA were not realized, but prior work has shown math teaching to be more consequential (Chetty et al., 2014b). Increasing suspensions correlate strongly with with achievement effects for male students, in line with the prior literature.

Violent crime is the strongest explanatory variable for both groups of students, though counter-intuitively more crime is correlated with better academic outcomes for male non-EL students. With no plausible reason why boys under the age of 15 would benefit academically from more neighborhood violence, a likely explanation is that they were affected by the means through which the decrease in violence was achieved – that is, unmeasured features of the policing approach. To understand why lower crime was good for female EL students and bad for male non-EL students, a more detailed understanding of who was victimized by crime and how police activity changed is required. For this, I turn to more detailed crime and arrest data.

7.2 Victimization

As described in Section 4, I obtain publicly available geo-coded data on reported crime reports from 2010 to 2019 and map these incidents to school attendance boundaries. These data include details of crimes and their disposition as well as victim characteristics. I compare schools that were treated in 2009 to never treated schools, after demeaning within year.[30] I compare schools to themselves in 2010, first calculating the percent change in a given outcome from 2010 to all other years, and then averaging this value across schools, weighted by the number of enrolled students. Figure 13a depicts large decreases in female victimization within treated areas relative to never treated areas and Figure 13c shows that this relationship is even clearer for violent crimes. For male victimization, the trend is negative but

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[29]I follow Heckman et al. (2013) in setting small and statistically insignificant mediating effects of the opposite sign of the total effect to zero (see their Figures 6 and 7).

[30]There are some schools treated in 2011 and 2013, but these sample sizes are very low, making 2009 the most instructive treatment cohort.

more subtle. These patterns hold when looking at Hispanic victims (Appendix Figure A5) as well as underage victims (Appendix Figure A6). Altogether, theses results point to greater benefits of lower crime for female relative to male students due to the lower threat of victimization. However, it is yet to be understood why some male students suffer under the policy.

7.3 Arresting Behavior

A natural question when considering why some students fared worse under this policy is: who was targeted? Though I focus on outcomes for students who are mostly too young to have direct contact with police, ethnographic and survey-based studies have shown that even vicarious contact, through relatives or community contacts, can impact the mental health and perceptions of young people (Geller & Fagan, 2019; Gottlieb & Wilson, 2019). Although there is little reason to expect the degree of secondary contact to differ by gender, the effects on stress and perception of police could vary if the primary contact is male-dominated. If youth perceive the police to be more biased against them as a result of CGI enforcement activity, this could lower the expected relative returns to schooling, as shown in the conceptual model in section 3.

I highlight several patterns observed in arrests data. First, the share of crime reports that result in arrests jumped after 2010 inside the catchment areas of schools treated in 2009, as shown in Figure 14a. Second, Figure 14b depicts a spike in arrests for pre-delinquency crimes, such as truancy or curfew violations, for 2009-treated schools in 2011 and 2012, before dropping again in 2013.[31] This increase in truancy arrests could be particularly motivating households, who bear the cost of citations (A. Jennings, 2012), to improve the attendance of all children. Figure A7 displays the group from which most of this increase comes – Hispanic male teenagers – and shows that females in the same group were not apprehended at higher rates.[32] Altogether, these patterns indicate that Hispanic children become more exposed to vicarious police contact, and that contact is more likely to come through males.

7.4 Immigrant Contact with and Perceptions of Police

In the case of first- or second-generation immigrants, research has shown that this group is less likely to commit crimes (Bersani, 2014; W. Jennings et al., 2013), and therefore should be less prone to increasing police contact. Piquero et al. (2016) examine surveys of youth who are first-generation immigrants,

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[31]The majority of these apprehensions conclude with a "counsel and release".

[32]Though a small number of apprehensions occur for children 12 to 14, apprehensions under age 11 are exceedingly rare and appear to be miscoded. Given this low rate of childhood criminal involvement, I do not consider a hypothesis of gangs lowering the age of recruitment in response to injunctions as plausible.

second-generation immigrants and native-born to better understand this phenomenon. They show that first- and second-generation immigrants perceive higher social costs to crime and believe more strongly in the legitimacy of the US legal system. First generation youth also tend to have less cynical beliefs about the legal system. The authors present theories where immigrants are culturally different in their attitudes toward offending and the law, or self-select into immigration due to more positive initial beliefs about the American justice system relative to their home country. Legal socialization over time may explain more cynical attitudes of second-generation immigrant youths.

This suggests that English learner youth may be insulated against negative perceptions of the police as biased, as well as from vicarious police contact. A similar process of belief updating may also occur for expectations of the relative returns to schooling. With visibly less crime, particularly against Hispanic women, immigrant parents may especially revise their beliefs about potential schooling and earnings outcomes for their daughters. With less cynicism and stronger trust in the legal system, this group may be more responsive to these changes than native-born parents.

8 Robustness

In this section, I rule out several confounding factors from biasing the main results and test the result sensitivity to empirical modelling choices.

8.1 School Composition Effects

Within a school the panel of students rotates as new students matriculate and older students graduate, and students may be mobile across schools for other reasons. Since a given school has an evolving student body, treatment effect estimates may reflect changes in student sorting. To probe this, Figure A8 uses the school event study framework to examine changes in the demographic characteristics of students attending schools in the safety zones. The plots illustrate that treated schools saw a decreasing share of free- or reduced-price lunch eligible students and an increasing share of Hispanic students offset by fewer Black students. These changes amount to about 14 fewer free- or reduced-price lunch students relative to an average of 925 the year before treatment, 14 more Hispanic students relative to a pre-treatment average of 845, and 11 fewer Black students relative to a pre-treatment average of 111. Enrollment declines of about 5% after three years are statistically significant only at the 10% level. No such enrollment declines are observed for the tested students, and results are robust to controlling for enrollment.

Given the fairly steady pre-treatment demographic trends, it appears safety zones caused some resorting of students along demographic dimensions. Using the available student-level data, I determine whether these sorting changes are driving the academic treatment effects. Regressing my outcomes of interest on a rich non-parametric model of individual student characteristics, I obtain residuals from this regression which are purged of demographic-driven variation. The model independent variables are fully interacted gender, English-learner status, free- or reduced-price lunch status, six categories of student ethnicity, and five categories of parental education.[33] These residuals in hand, I aggregate to the school-level and estimate the event study once again. The results of this exercise are presented in Figure A9a and Figure A9b. Despite the decrease in high-poverty students, positive estimates of treatment effects are robust to controlling for this and other student variables, at 0.08σ for math scores and 0.05σ for ELA scores after three years.

While these findings are supportive of school treatment effects not being driven by demographic composition, they could be masking sorting on underlying ability that is not captured by socio-economic characteristics. The best measure of early cognitive ability available is standardized test scores, which are not taken until grade 2 and which are a key outcome of interest. A better candidate for investigating selection on underlying ability would be measured earlier in the students' schooling and therefore less likely to be influenced by treatment and more likely to capture selection dynamics. The variable I select to fulfill these criteria is the grade point average (GPA) of students in grade 1. GPA is more subjective than test scores and is not responsive to the treatment.[34] Yet, grade 1 GPA predicts about 55% of the variation in grade 2 test scores, suggesting it is a good proxy for aptitude. I categorize students into four quartiles of baseline ability using this measure.[35] Since baseline ability is fixed for a student, changes in the share of students belonging to each group reflect changes in the student body. Figure A10 plots the treatment effects on these baseline ability shares. Contrary to a story of upward bias, the highest ability group share decreases post-treatment, replaced by increases in the second quartile ability group. Thus, a story of positive selection on underlying ability is not supported.

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[33]The measure of parental education is not reported for a large portion of observations, and I include this non-response as a category. In general, I avoid using this measure in favor of free- or reduced-price lunch status as a proxy for socio-economic status.

[34]This statement is based on estimating both the student- and school-level event studies using GPA as the dependent variable.

[35]Where grade 1 GPA is unavailable I use kindergarten GPA, or grade 2 GPA, in that order. The resulting measure is non-missing for 80% of the sample, and rates of missing data are uncorrelated with treatment.

8.2 Effects of New School Construction

New school construction occurs both inside and outside safety zones. A capital investment push in Los Angeles during this period relieved overcrowding in many schools, particularly middle and high schools, and Lafortune and Schönholzer (forthcoming) show that this improved academic outcomes for students who accessed new schools. Figure A11 summarizes new school openings in my sample by treatment status in each year. As a robustness check, I replicate my main results excluding schools that opened in the past 4 years. Students who did not attend new schools may still benefit from reduced overcrowding when schools open nearby. I estimate the influence of this spillover effect by controlling for the share of peers a student lost from their prior year's cohort to a new school opening this year.[36] Results are presented in Figure A12. Though this increases noise in my estimates, the coefficients of interest are highly stable in both the main results and findings of heterogeneous effects. These results and those at the school level are also robust to controlling for school enrollment and test enrollment.

8.3 Student Sorting through Endogenous Mobility

One potential source of bias in treatment effect estimates is that injunctions may induce student mobility across schools, whether within or outside the sample district. For example, Owens et al., 2020 find that while gang injunctions had no effect on home sales volumes, they did see an increase in female and black homeowners moving into the safety zones. If the implementation of safety zones has a push or pull effect on residential or school choice, a student's relocation could directly impact their academic performance as well as change the peer composition of immobile students. I determine whether this movement is taking place by estimating the event study at the school level on mobility outcomes. To abstract from changes in mobility that arise from new school openings, I follow the approach of the prior section of dropping newly opened schools and controlling for the share of students who leave to attend them.

Figure A13 displays the resulting estimates of the effects of school safety zone exposure on the rate of exits to other district schools, and exits outside of the district. To better isolate endogenous changes in school choice, I do not consider it an exit if a student left their school after completing the highest grade. Exits occur between the end of the current school year and the beginning of the following one. There is a suggestive increase in the share of students changing schools in the first two years of the safety zone, but these are not statistically significant. In Figure A14, I further confirm that treated students do not select out of the sample at higher rates across baseline GPA quartiles,

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[36]Results are also similar if I include three lags of this measure.

and Figure A15 shows that switching schools within the sample is also not discernible across gender-EL groups. Nonetheless, I estimate how treatment effects vary for students who enroll in schools outside safety zones post-treatment relative to those who stay. Figure A16 and Figure A17 plot these diverging post-treatment estimates for math and English, respectively. In both specifications, students who move to schools outside the safety zones experience a negative and transitory effect on test scores, recovering to their baseline achievement after one to two years. I am unable to disentangle the influence of the policing policy and that of the relocation disruption for mobile students. Though gang injunctions did not appear to drive relocation decisions on average, the choice to stay or leave treated schools may have been influenced by the change in policing for some. Isolating students who stay enrolled inside the safety zones results in larger and more statistically significant treatment effect estimates for both math and English scores.

8.4 Sensitivity to Treatment Thresholds

I approximate student exposure to gang injunction policing by calculating the share of a school catchment area that is overlapped by an operating safety zone. I define a treatment event as a large one-year increase in this overlap share, where the prior year share could not be above 33% and the current year share must be at least 75%. This upper threshold ensures a substantial share of students are exposed to the policy and the lower threshold isolates those which experienced a significant policy change, and were not already partially treated. In Table B1 and Table B2 I demonstrate that the results are not sensitive to varying the upper threshold between 50% and 85% for either math or English results, respectively. Though the magnitude of effects increases slightly for higher thresholds, this is to be expected as higher thresholds indicate a greater share of students in the school catchment area are exposed to treatment. Conversely, Table B3 and Table B4, illustrating robustness to varying the lower treatment threshold, show that including treated units that experienced higher pre-treatment exposure implies smaller magnitudes of the treatment effect.

8.5 Correcting for Cohort-Specific Treatment Effect Heterogeneity

Recent econometric studies on staggered difference-in-difference designs have shown that treatment effect estimates are biased unless cohorts treated at different times have homogeneous treatment effects (Athey & Imbens, 2021; Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021). Sun and Abraham (2021) provide a clear depiction of this issue in the context of event study approaches, and outline an estimator that resolves this bias by calculating the appropriate weights on treatment cohorts. Treatment

effect heterogeneity across cohorts is plausible in the context of gang injunctions. Though I observe no systematic differences across earlier versus later treated areas, it is likely that there was variance in how precincts enforced injunctions and their relationships with the community. I use Sun and Abraham's Stata-written command for implementing their event study correction.[37] Results are presented in Figure A18. The coefficient estimates are largely similar as in the main results, though pre-trends appear to be more of a concern in the math specification. One drawback of my empirical analysis is that many safety zones are implemented early in my sample period (2005 safety zones were quite large), and for these I am unable to estimate pre-treatment coefficients beyond a two-year window. Thus, I am somewhat underpowered in estimating these longer-run pre-treatment trends. As a result, I interpret the average treatment effects with caution, but am encouraged by relatively flat coefficients in the heterogeneous effects analysis.

9   Conclusion

This paper empirically estimates the causal effects of a proactive policing program, civil gang injunctions, on academic and behavioral outcomes of students under the age of 15. I find that new gang injunctions increase student test scores on average in both math and English, and uncover substantial heterogeneity along the dimensions of gender and a proxy measure for immigration. The results are robust to adjusting the empirical design, accounting for new schools and mobile students, and adjusting for across-cohort heterogeneity in treatment effects. I show that reductions in violent crime are a key mechanism for improving the scores of female immigrant students, but contribute negatively to the scores of native-born male students, whose increased suspensions appear to contribute to these adverse effects. An examination of crime and arrest reports suggests that diverging treatment effects arise due to greater declines in the victimization of women, and increases in the perception of bias for young boys, who see those who look like them being arrested at higher rates. Evidence from research in criminology supports hypotheses of lower contact with police and a lower proclivity to perceive bias as explanations for better outcomes among immigrants.

This study illustrates the far reaching implications of crime reduction strategies involving police. Despite the targeting of this policy toward gang-involved individuals, it's implementation was damaging to a sizeable group of young children who are seldom involved in crime themselves. This compliments

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[37]Due to a very large sample size and computing limitations, I am limited in employing this methodology. I am unable to include control variables, and instead must residualize the dependent variable on any controls aside from fixed effects. I do not have access to sufficient computing power to estimate heterogeneous treatment effects using this estimator.

recent results highlighting the burdens of targeted policing on the community at large (Chalfin et al., 2020), even when this strategy is successful at reducing crime. Moreover, these effects are likely to continue to accumulate in the longer term, as the research subjects are young students at critical stages of development.

The benefits of reducing crime for young female immigrant students should not be overlooked. This group starts out grade 2 with the lowest math scores and the second to lowest English scores, and they typically do not catch up to native English speakers in time for high school. However, it is important to consider the students who are not made better off, and how the costs to them might be mitigated. One potential strategy is improved targeting. If perceptions of police bias were indeed increased, then it may be the case that police were casting too wide a net and relying on racial profiling to identify gang members, as has been alleged by many who were targeted (Muniz, 2015; Queally, 2017). Newer, more data-driven tools for targeting violent and frequent offenders, branded as "precision" policing, have shown promise in reducing crime (Chalfin et al., 2021) but raise new concerns around encroaching surveillance and biased algorithms (Brayne, 2017; Puente, 2019). Future work reviewing the efficacy of these policies should incorporate analyses of their effects on children.

A popular proposal of the 'defund the police' movement is to shift budgeting resources away from traditional policing and toward alternative crime intervention strategies. In that vein, recent work shows that a community monitoring initiative around schools in Chicago was effective at improving attendance, though they do not report on achievement outcomes (Gonzalez & Komisarow, 2020). Counteracting investments may also help to reverse the damage of police tactics. In Los Angeles, a program to increase intervention services for gang-involved youth was expanded in 2007 and 2008. Future research should explore whether these service areas mitigated the negative outcomes of gang injunctions for native-born boys attending schools inside safety zones.

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Figures

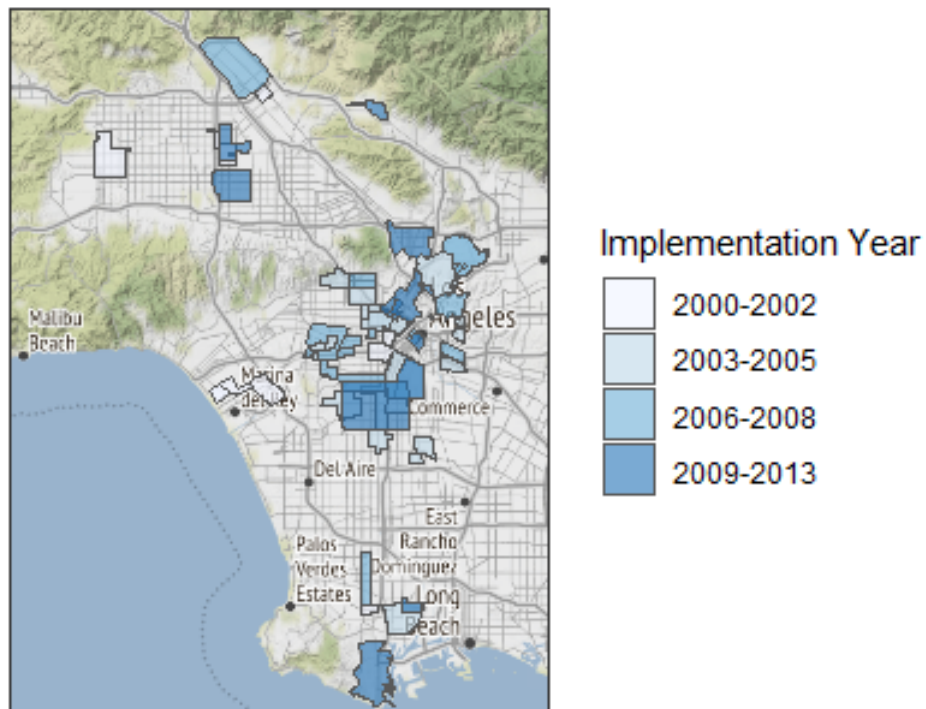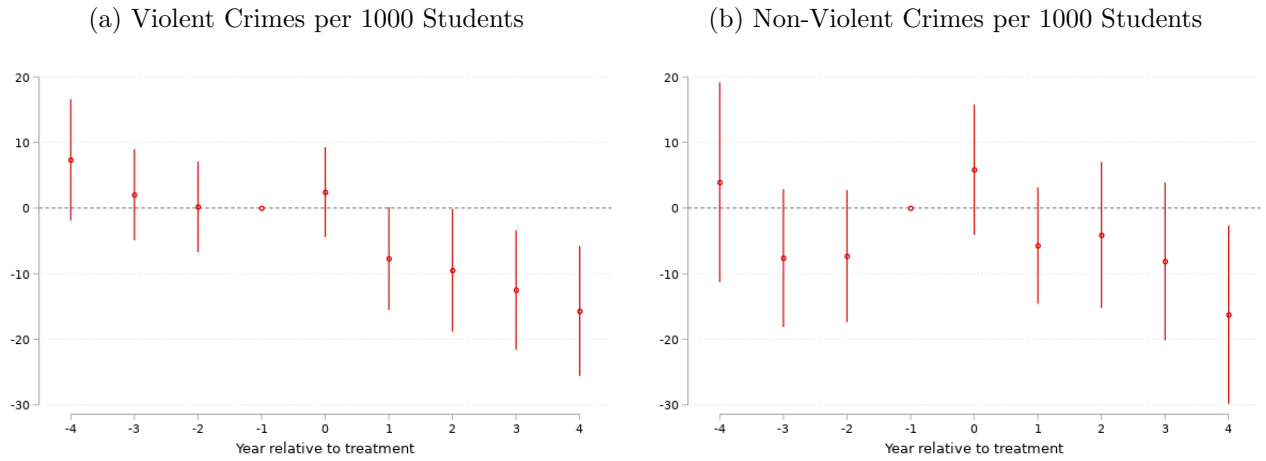Figure 1: Map of Safety Zones by Implementation Year

Figure 2: Effect of Safety Zone on Crime Reported inside School Boundaries

(a) Violent Crimes per 1000 Students
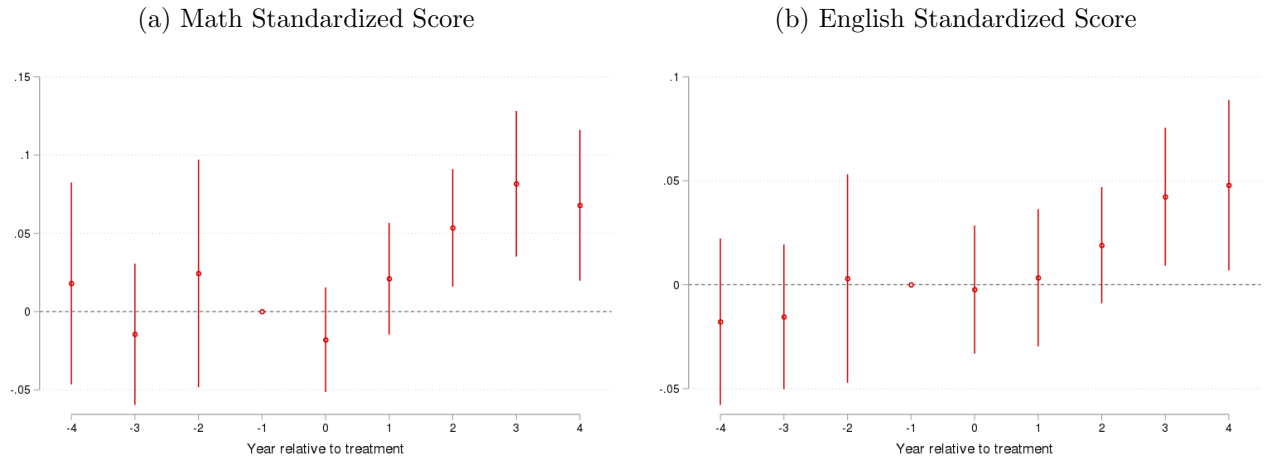
(b) Non-Violent Crimes per 1000 Students

Notes: Plots display point estimates and 95% confidence intervals of the school-level event study in Equation 4 for crime. The year before treatment, $k = -1$ is omitted as a baseline. Crime counts are scaled by school enrollment to account for neighborhood population, and total enrollment is used as analytic weights in regressions. Standard errors are clustered at the school-level.

Figure 3: Average Percent Change in Total Crime Relative to 2010
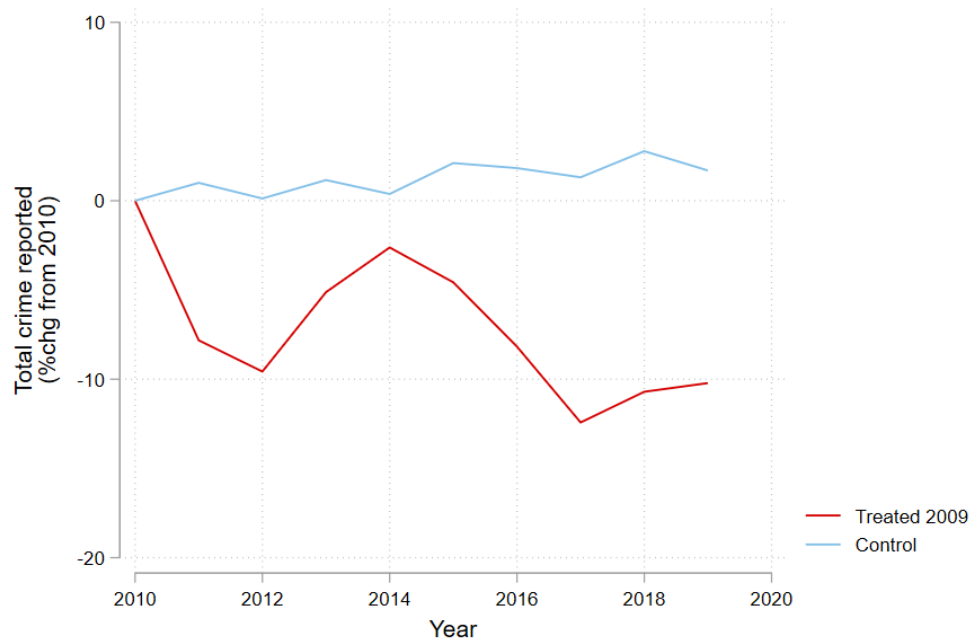
Notes: Percent change in crime is calculated by comparing each school to itself in 2010. Crime relative to 2010 is then purged of common year effects by de-meaning within year for the full sample, weighted by total enrollment. Plots display the average across schools of this measure, again weighting by total enrollment.

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Figure 4: Effects of Safety Zones on School Mean Test Scores

(a) Math Standardized Score   (b) English Standardized Score

Notes: Plots display point estimates and 95% confidence intervals of the school-level event study in Equation 4 for test scores. The year before treatment, $k = -1$ is omitted as a baseline. Test scores are normalized within subject-grade-year using statewide scores, and then averaged for each school and year. Standard errors are clustered at the school-level.

Figure 5: Effects of Safety Zones on Student Test Scores
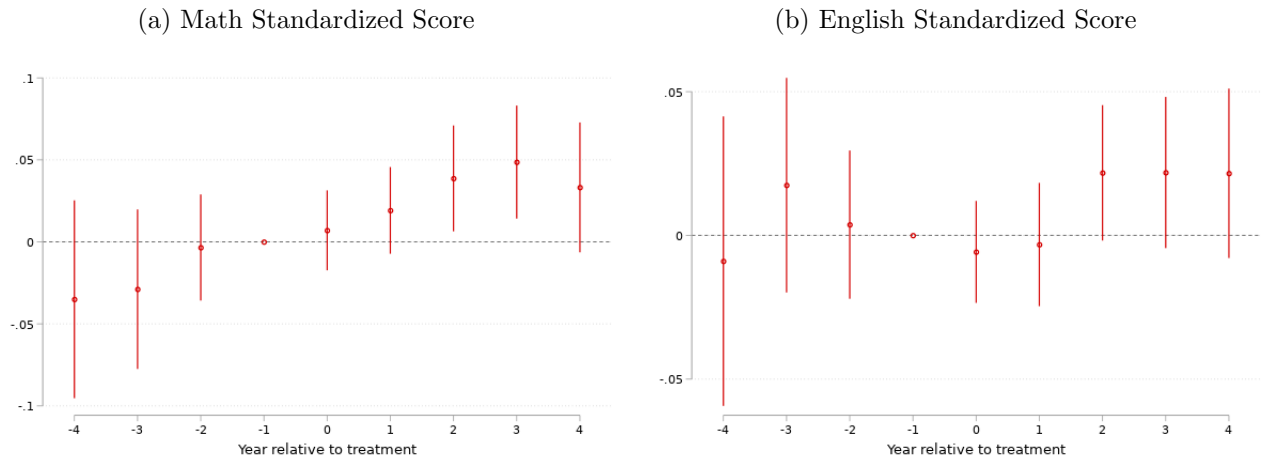
(a) Math Standardized Score   (b) English Standardized Score

Notes: Plots display point estimates and 95% confidence intervals of the student-level event study in Equation 5 for test scores. The year before treatment, $k = -1$ is omitted as a baseline. Test scores are normalized within subject-grade-year using statewide scores. Standard errors are two-way clustered at the student- and school-level.

Figure 6: Heterogeneous Treatment Effects on Math Standardized Tests

(a) Female

(b) Male

Pretrends p-value: 0.14

Pretrends p-value: 0.69

(c) English Learners

(d) Non-English Learners

Pretrends p-value: 0.47

Pretrends p-value: 0.18

(e) Hispanic

(f) Black

Pretrends p-value: 0.52

Pretrends p-value: 0.60

(g) Asian/Filipino

(h) White

Pretrends p-value: 0.12

Pretrends p-value: 0.28

Notes: Plots display point estimates and 95% confidence intervals of the student-level event study in Equation 5, where relative treatment indicators $D_{i,t}^k$ are interacted with indicators for student characteristics. The year before treatment, $k = -1$ is omitted as a baseline. The dependent variable is math test scores normalized within subject-grade-year using statewide scores. Standard errors are two-way clustered at the student- and school-level.

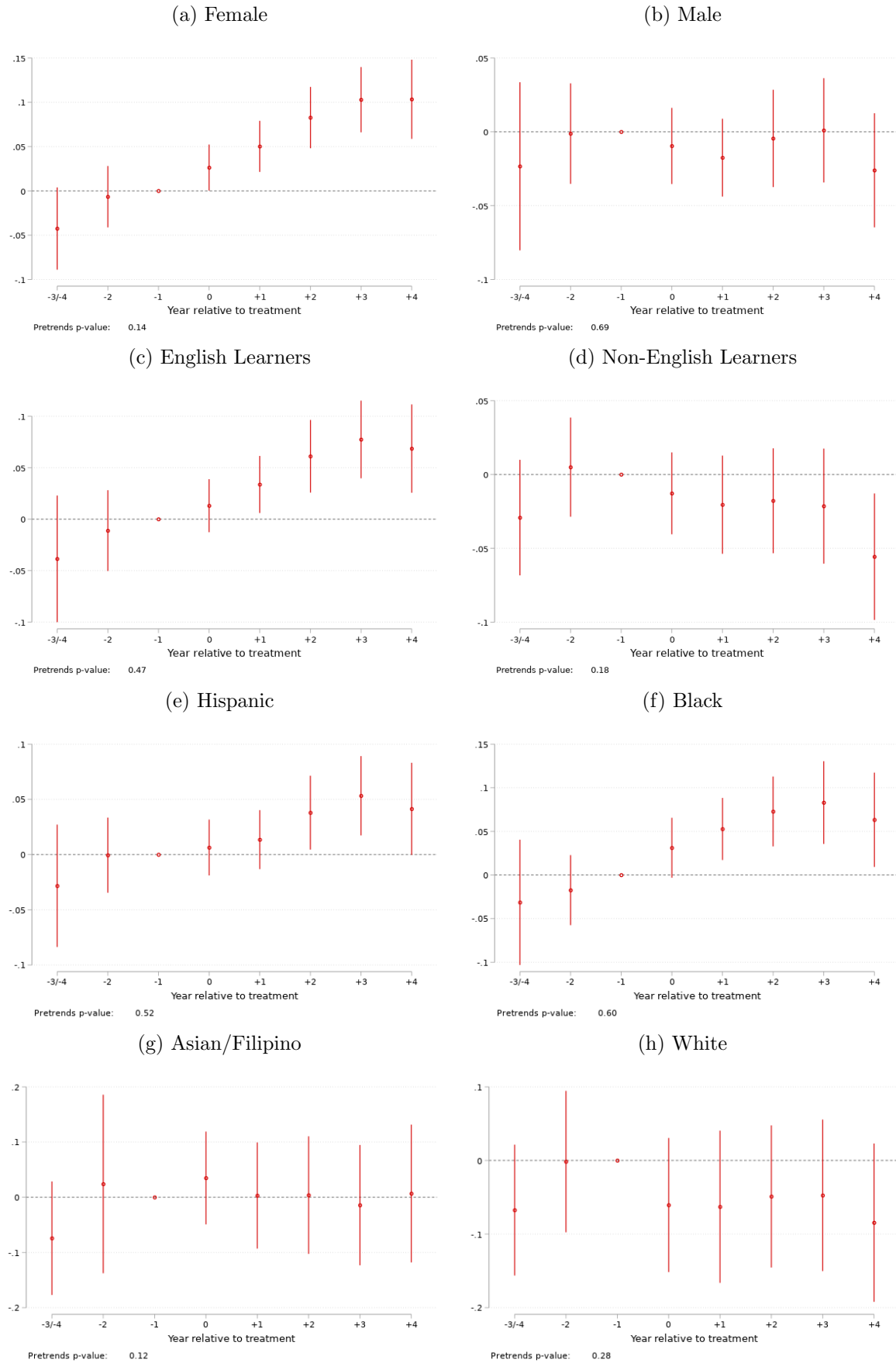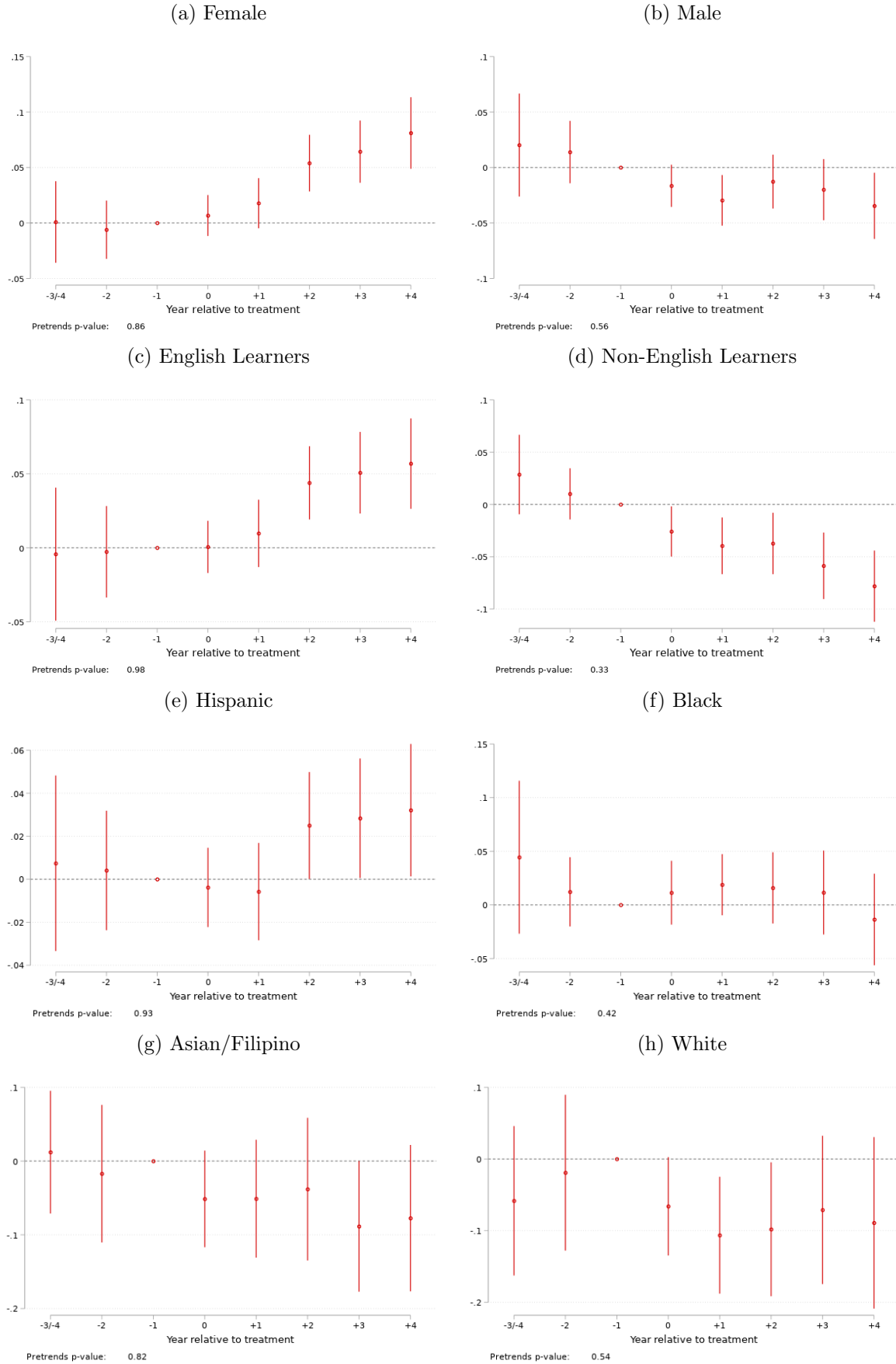Figure 7: Heterogeneous Treatment Effects on English Standardized Tests

(a) Female

(b) Male

(c) English Learners

(d) Non-English Learners

(e) Hispanic

(f) Black

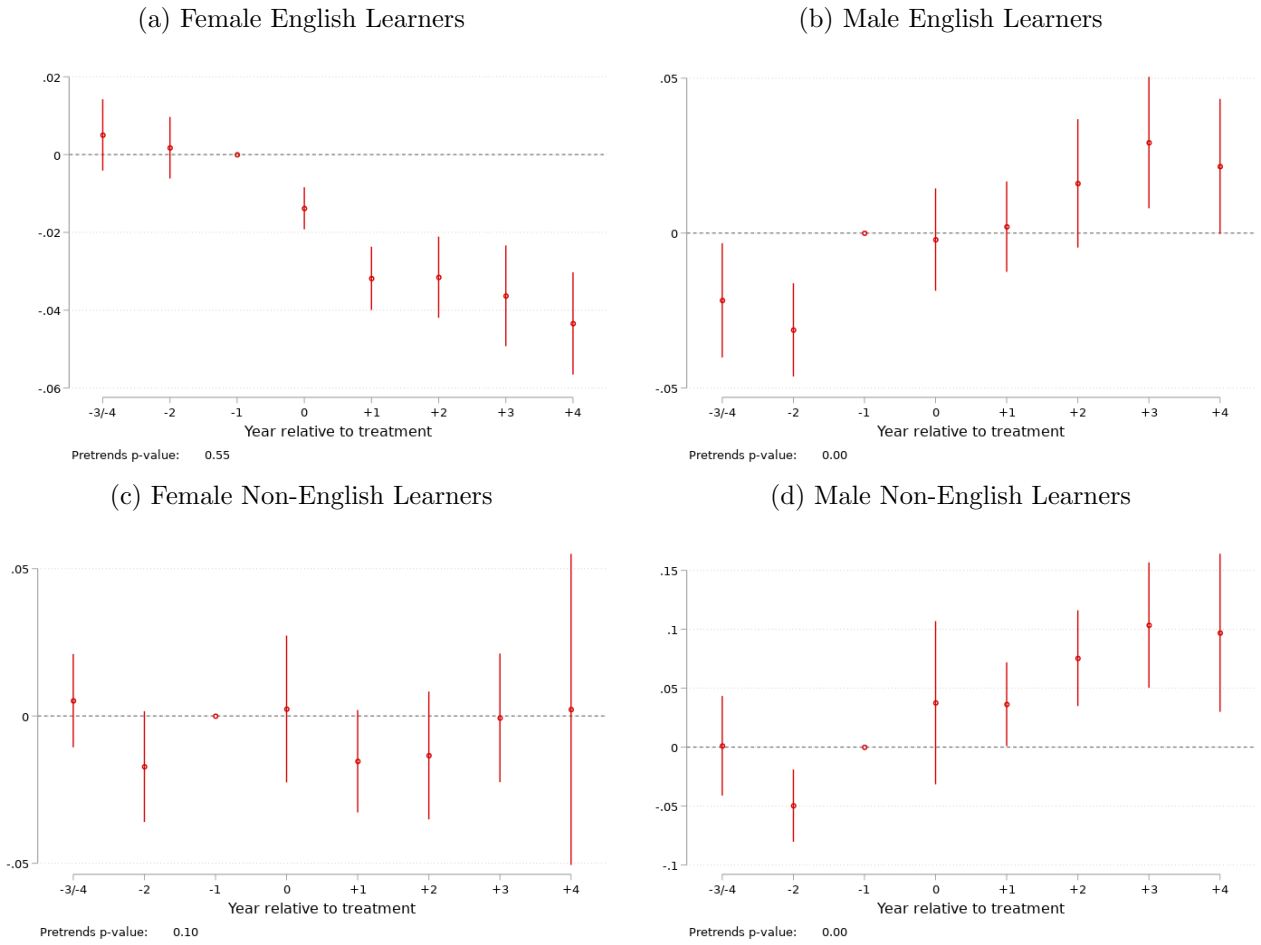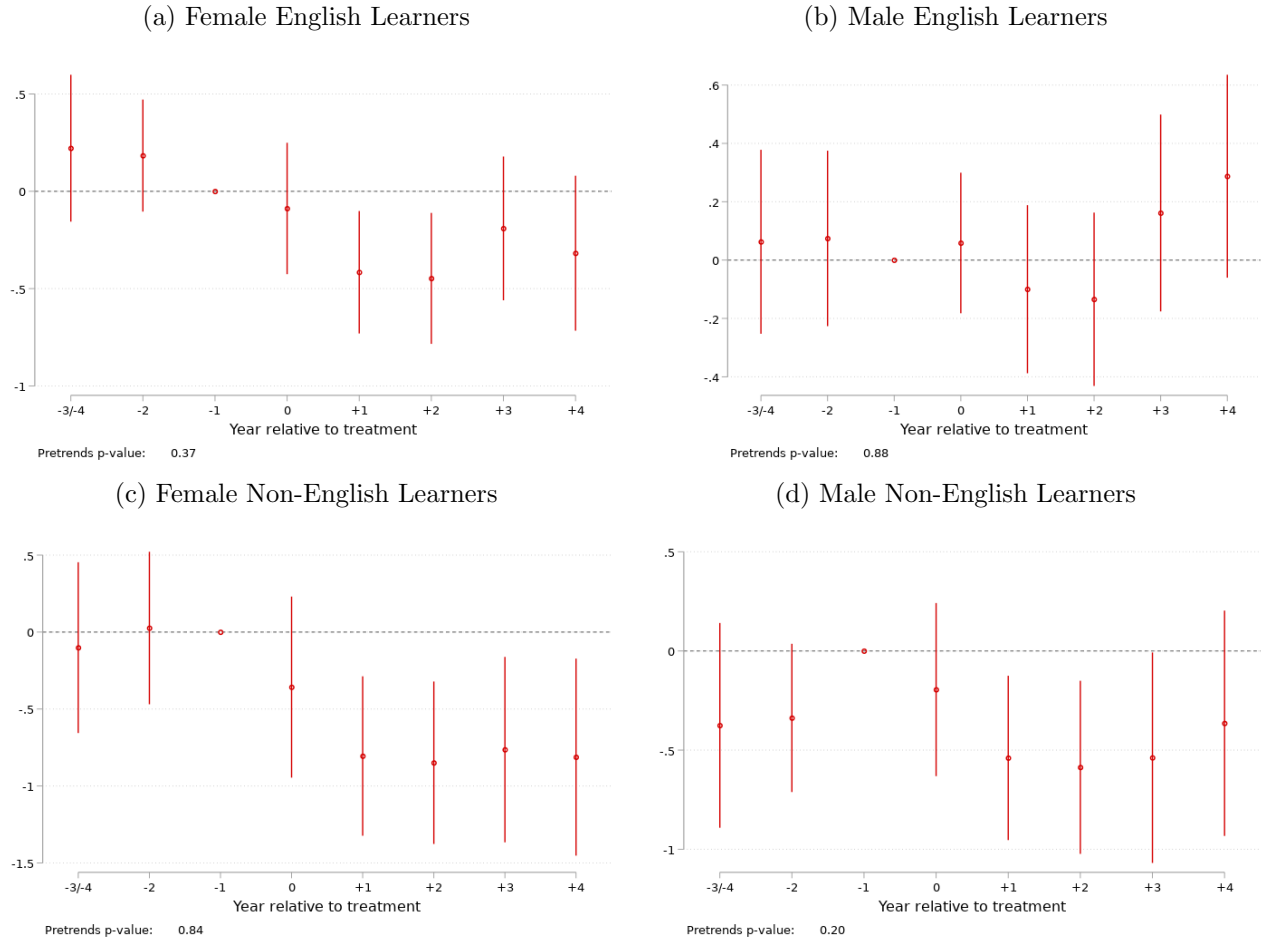(g) Asian/Filipino

(h) White

Notes: Plots display point estimates and 95% confidence intervals of the student-level event study in Equation 5, where relative treatment indicators $D_{i,t}^k$ are interacted with indicators for student characteristics. The year before treatment, $k = -1$ is omitted as a baseline. The dependent variable is English test scores normalized within subject-grade-year using statewide scores. Standard errors are two-way clustered at the student- and school-level.

Figure 8: Effects on Standardized Test Scores, by Gender and English Learner Status

(a) Female English Learners - Math

(b) Female English Learners - ELA

(c) Male English Learners - Math

(d) Male English Learners - ELA

(e) Female Non-English Learners - Math

(f) Female Non-English Learners - ELA

(g) Male Non-English Learners - Math

(h) Male Non-English Learners - ELA

Notes: Plots display point estimates and 95% confidence intervals of the student-level event study in Equation 5, where relative treatment indicators $D_{i,t}^k$ are interacted with indicators for gender and English learner status. The year before treatment, $k = -1$ is omitted as a baseline. Math test scores are normalized within subject-grade-year using statewide scores. Standard errors are two-way clustered at the student- and school-level.

Figure 9: Behavioral Effects - Days Suspended, by Gender x English Learners

(a) Female English Learners

Pretrends p-value: 0.55

(b) Male English Learners

Pretrends p-value: 0.00

(c) Female Non-English Learners

Pretrends p-value: 0.10

(d) Male Non-English Learners

Pretrends p-value: 0.00

Notes: Plots display point estimates and 95% confidence intervals of the student-level event study in Equation 5, where relative treatment indicators $D_{i,t}^k$ are interacted with indicators for gender and English learner status. The year before treatment, $k = -1$ is omitted as a baseline. The regression includes grade fixed-effects to control for increasing suspension rates in higher grades. Standard errors are two-way clustered at the student- and school-level.

Figure 10: Behavioral Effects - Days Absent, by Gender x English Learners

(a) Female English Learners

(b) Male English Learners

(c) Female Non-English Learners

(d) Male Non-English Learners

Notes: Plots display point estimates and 95% confidence intervals of the student-level event study in Equation 5, where relative treatment indicators $D_{i,t}^k$ are interacted with indicators for gender and English learner status. The year before treatment, $k = -1$ is omitted as a baseline. The regression includes grade fixed-effects to control for increasing absences in higher grades. Standard errors are two-way clustered at the student- and school-level.
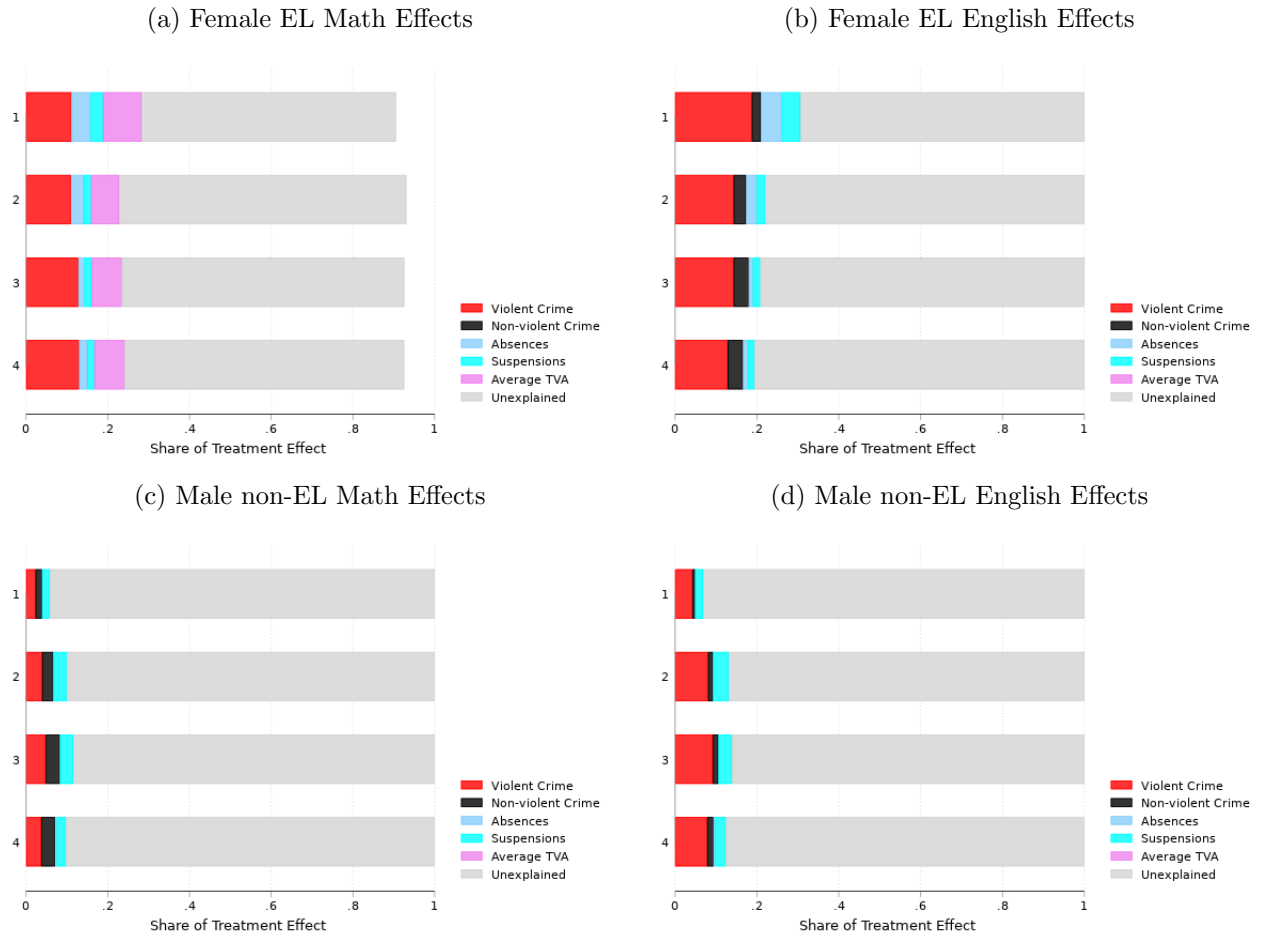
Figure 11: Mediation Results - School Average Effects

(a) Math Effects

(b) English Effects
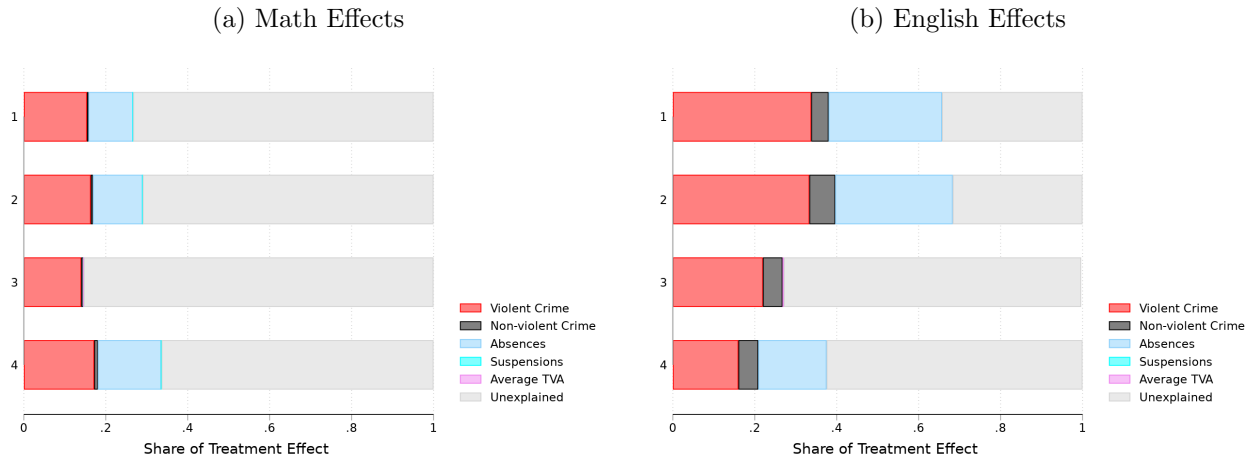
Notes: A bar displays the share of the total effect that is explained by each school-level mediator, θ_j, and a remainder. That is $\hat{\gamma}_j * \hat{\mu}_{1k}$ from equations 7 and 8, and the direct or unexplained component, $\hat{\mu}_{2k}$. This breakdown is given for each post-treatment period $k \in \{1, ..., 4\}$ for school-level treatment effects on math scores in the left panel and English scores in the right panel.

Figure 12: Mediation Results - Student Subgroup Effects

(a) Female EL Math Effects

(b) Female EL English Effects

(c) Male non-EL Math Effects

(d) Male non-EL English Effects

Notes: A bar displays the share of the total effect that is explained by each student-level mediator, θ_j, and a remainder. That is $\hat{\gamma}_j * \hat{\mu}_{1k}$ from equations 7 and 8, and the direct or unexplained component, $\hat{\mu}_{2k}$. This breakdown is given for each post-treatment period $k \in \{1, ..., 4\}$ for math scores in the left panels and English scores in the right panels.

Figure 13: Descriptive Evidence of Changes in Victimization by Gender

(a) Female Victims of Crime

(b) Male Victims of Crime

(c) Female Victims of Violent Crime

(d) Male Victims of Violent Crime

Notes: Percent change in crime victimization is calculated by comparing each school to itself in 2010. Victimization relative to 2010 is then purged of common year effects by de-meaning within year for the full sample, weighted by total enrollment. Plots display the average across schools of this measure, again weighting by total enrollment.

Figure 14: Descriptive Evidence of Changes in Arrests

(a) Overall Arrest Rate   (b) Pre-Delinquency Arrests
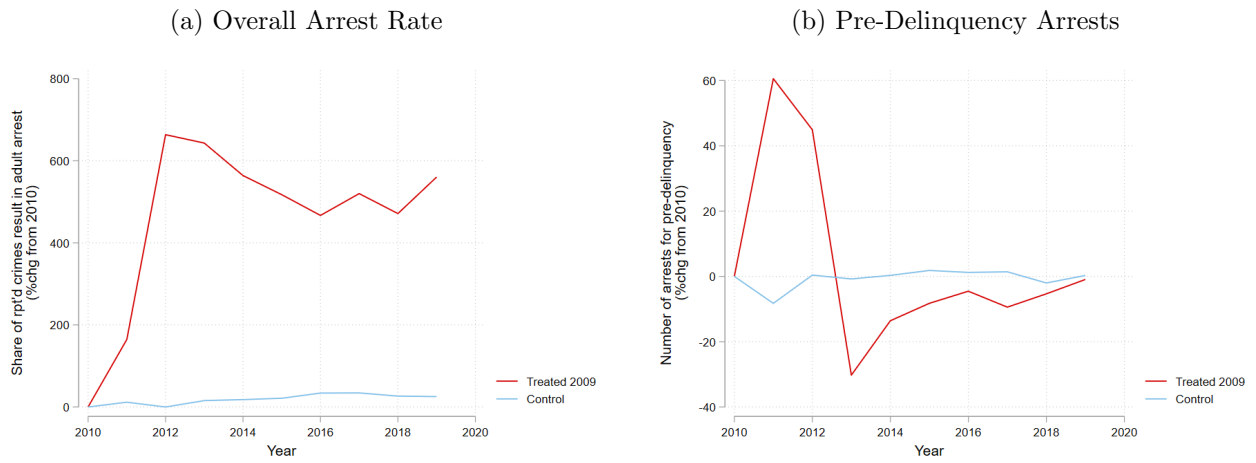
Notes: Percent change in arrests relative to total crimes is calculated by comparing each school to itself in 2010. Arrest rates relative to 2010 are then purged of common year effects by de-meaning within year for the full sample, weighted by total enrollment. Plots display the average across schools of this measure, again weighting by total enrollment.

Tables

Table 1: Education Data Summary by Treatment Status

	Never Treated		Treated	
	mean	(st. dev.)	mean	(st. dev.)
Panel A: Schools				
Share Elementary Schools	0.72		0.76	
Share Middle Schools	0.20		0.21	
Share Combination Schools	0.09		0.03	
Observations	574		170	
Panel B: School-Years				
Total School Enrollment	817	(571)	712	(404)
Tested Enrollment (Math)	502	(353)	434	(280)
Tested Enrollment (ELA)	595	(543)	448	(306)
Observations	5,006		1,158	
Panel C: Students				
Hispanic	0.67		0.82	
Black	0.12		0.11	
White	0.13		0.02	
Asian/Filipino	0.07		0.04	
Less than high school	0.18		0.26	
High school	0.27		0.28	
College or higher	0.15		0.07	
Not reported	0.40		0.39	
Ever Free- or Reduced-Price Lunch Eligible	0.81		0.96	
Ever Classified as English Learner	0.45		0.66	
Observations	913,312		88,494	
Panel D: Student-Years				
Math Standardized Test Score	-0.09	(0.99)	-0.32	(0.93)
ELA Standardized Test Score	-0.17	(0.97)	-0.44	(0.88)
Total Days Absent	8.12	(10.48)	8.04	(10.62)
Total Days Suspended	0.10	(0.63)	0.09	(0.59)
Attend Residential Assigned School	0.79		0.83	
Change District Schools Pre-Graduation	0.07		0.08	
Leave the District	0.11		0.07	
Observations	3,459,842		500,167	

Notes: Treated schools have an attendance boundary that is at least 75% overlapped by a safety zone at some point between 2003 and 2013, and which had not earlier been more than a third covered by a safety zone. Never treated schools have an attendance boundary that never exceeds 75% overlap by a safety zone. Treated students are those who were enrolled in a treated school the first year the school became over 75% overlapped with a safety zone. Never treated students are never enrolled in schools at the time that they were over 75% covered with a safety zone.

Table 2: Crime Data Summary by Treatment Status

	Never Treated Schools		Treated Schools	
	mean	(st. dev.)	mean	(st. dev.)
Panel A: Aggregated Crime Reports (2003 to 2013)				
Total Violent Crimes (per 000 students)	40.01	(46.28)	73.26	(70.87)
Aggravated Assaults	19.89	(20.97)	39.81	(28.24)
Robberies	15.39	(14.38)	31.13	(19.05)
Homicides	0.50	(0.73)	1.12	(0.93)
Total Non-Violent Crimes (per 000 students)	123.27	(104.09)	133.07	(118.08)
Burglaries	26.90	(13.74)	28.97	(13.20)
Thefts from Persons	1.51	(1.68)	2.91	(2.33)
Burglary/Thefts from Vehicle	42.47	(21.77)	44.65	(20.02)
Vehicle Thefts	31.15	(22.97)	46.29	(22.80)
Observations	3,383		1,264	
Panel B: Geo-Coded Crime Reports (2010)				
Total crime reported (per 000 students)	1353.91	(1153.12)	1099.83	(847.68)
Number of Female Victims	570.21	(454.54)	501.48	(398.60)
Number of Male Victims	657.46	(628.23)	479.54	(380.19)
Female, Underage Victims	41.49	(30.30)	49.88	(37.17)
Male, Underage Victims	34.00	(25.35)	41.52	(33.79)
Violent crimes reported (per 000 students)	373.81	(384.25)	414.41	(336.17)
Violent Crime - Female Victims	187.30	(169.07)	226.72	(187.32)
Violent Crime - Male Victims	186.21	(221.56)	187.39	(153.67)
Non-violent crimes reported (per 000 students)	980.10	(816.42)	685.42	(529.22)
Share of rpt'd crimes result in adult arrest	0.08	(0.05)	0.10	(0.05)
Share of rpt'd crimes result in juvenile arrest	0.01	(0.01)	0.01	(0.01)
Observations	285		103	
Panel C: Geo-Coded Arrest Reports (2010)				
Total number of arrests (per 000 students)	1084.67	(1968.11)	807.97	(780.36)
Share of arrestees are male	0.79	(0.07)	0.81	(0.05)
Number of arrests for violent crimes (per 000 students)	88.36	(107.83)	92.62	(87.24)
Number of arrests for pre-delinquency (per 000 students)	21.64	(26.17)	21.17	(33.98)
Number of arrestees <18 years old (per 000 students)	119.20	(118.46)	122.24	(119.56)
Number of arrestees 18 to 24 years old (per 000 students)	269.54	(529.57)	190.19	(187.93)
Observations	285		103	

Notes: Treated schools have an attendance boundary that is at least 75% overlapped by a safety zone at some point between 2003 and 2013, and which had not earlier been more than a third covered by a safety zone. Never treated schools have an attendance boundary that never exceeds 75% overlap by a safety zone. All crime counts are scaled to be per 1000 students enrolled in the school.

Table 3: Treatment Effects by Age at Initial Treatment

	Math (1)	English (2)
Panel A: Full Sample		
post x Treated under age 10	0.048***	-0.011
	(0.015)	(0.012)
post x Treated age 10+	-0.015	0.021*
	(0.014)	(0.011)
Panel B: English Learner Students		
post x Treated under age 10	0.067***	0.010
	(0.016)	(0.013)
post x Treated age 10+	-0.003	0.036***
	(0.016)	(0.011)
Panel B: Non-English Learner Students		
post x Treated under age 10	-0.001	-0.070***
	(0.018)	(0.017)
post x Treated age 10+	-0.044**	-0.011
	(0.017)	(0.013)

* p<0.10, ** p<0.05, *** p<0.01

Table display regression results from a differences-in-differences version of equation 5, grouping post-treatment indicators into a single regressor, 'post'. The treatment measure, 'post' is interacted with a dummy variable for whether a student was first treated when they were under age 10. Panel A presents the full sample estimates for the dependent variable standardized math test scores in column (1) and standardized English test scores in column (2). Panels B and C present the same estimates for English learner and non-English learner students, respectively. Standard errors are two-way clustered at the student and school level are reported in parentheses.

A   Appendix Figures

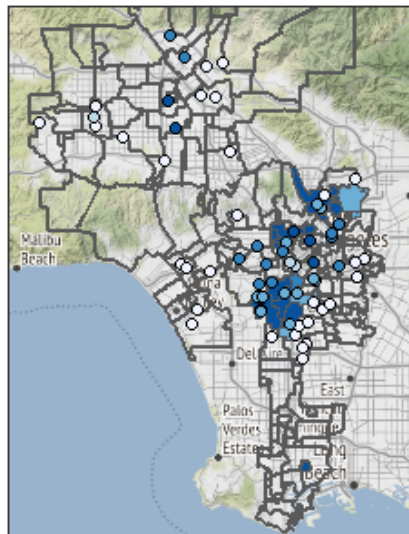Figure A1: Elementary and Middle School Boundaries, Shaded by Treatment Year

(a) Elementary Schools
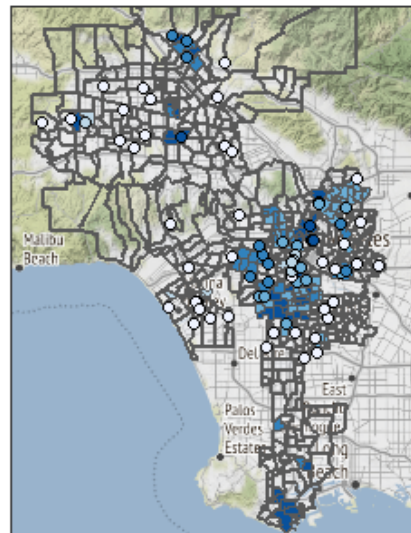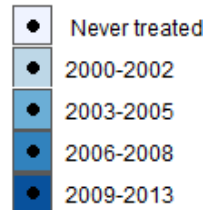
(b) Middle Schools


Notes: School attendance boundaries are outlined in gray, and shaded in blue according to the year they became at least 75% overlapped by a safety zone. Schools without attendance boundaries are plotted as points, and treatment status is based on their point overlap with safety zones.

Figure A2: Reporting Districts and School Boundaries

(a) LAPD Reporting Districts
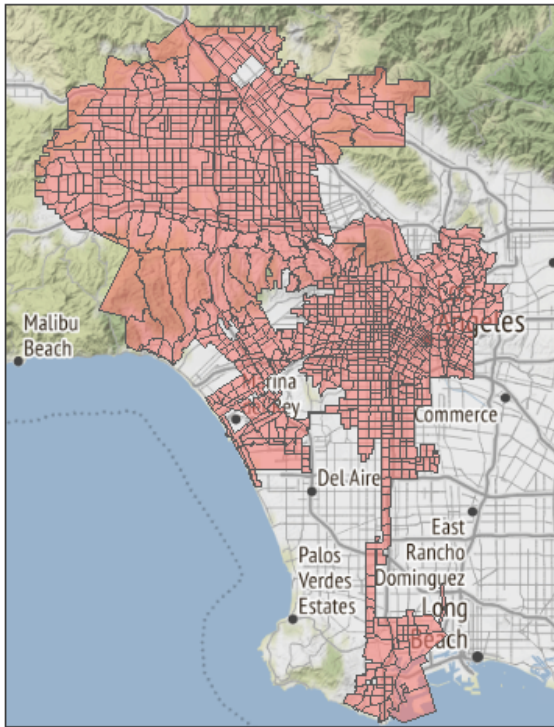
(b) With Attendance Boundaries in Black
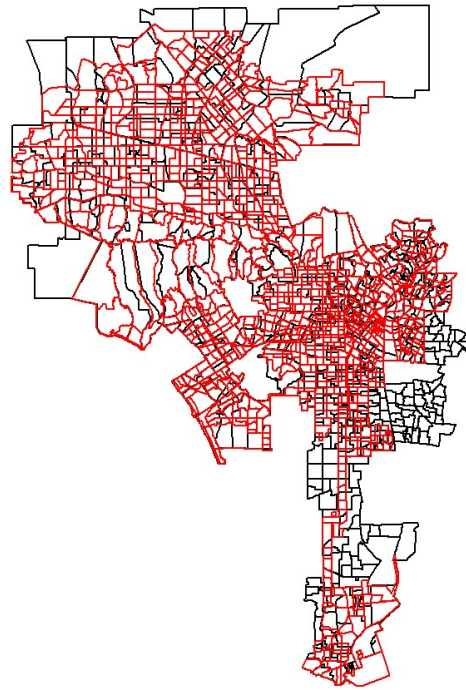
Figure A3: Effects of Safety Zones on All Crime Types
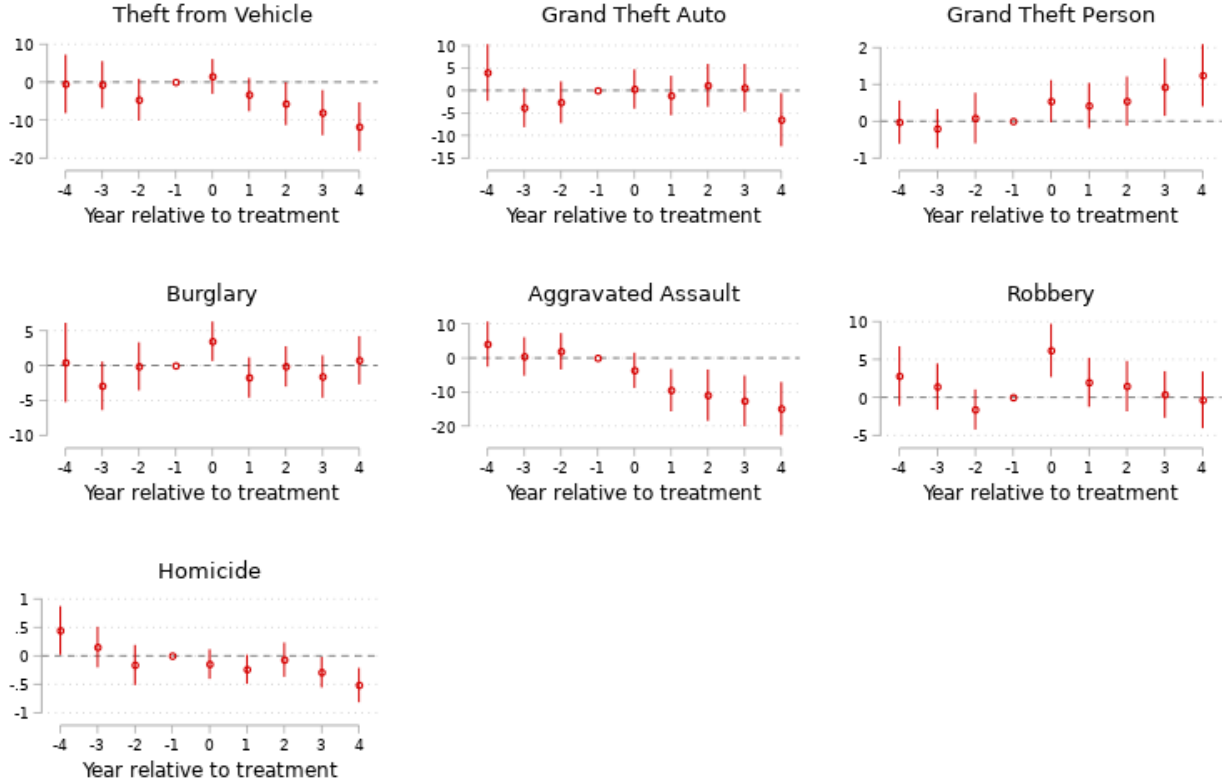
Notes: Plots display point estimates and 95% confidence intervals of the school-level event study in Equation 4 for crime. The year before treatment, $k = -1$ is omitted as a baseline. Crime counts are scaled by school enrollment to account for neighborhood population, and total enrollment is used as analytic weights in regressions. Standard errors are clustered at the school-level.

Figure A4: Descriptive Evidence of Safety Zone Effect on Crime
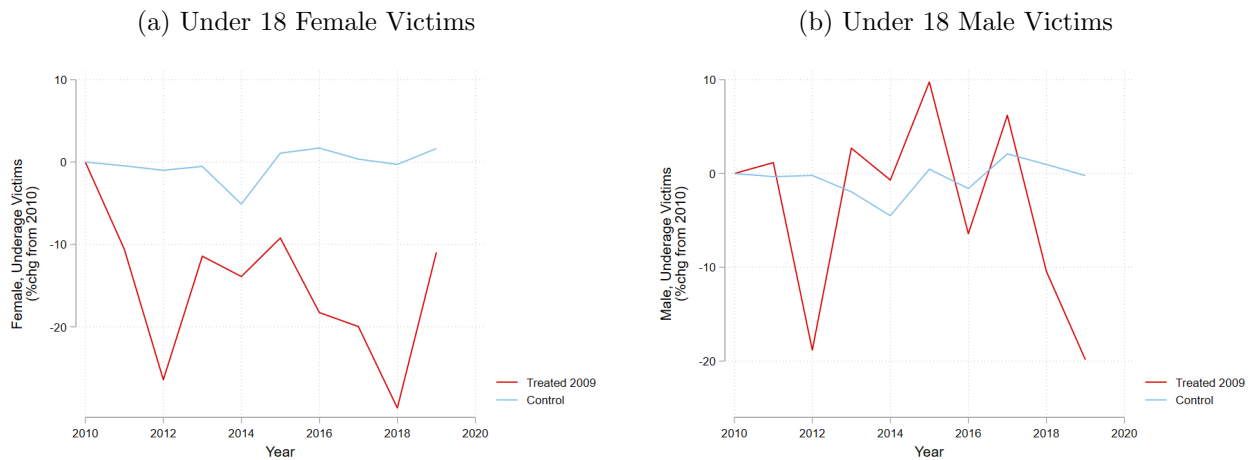
(a) Violent Crimes
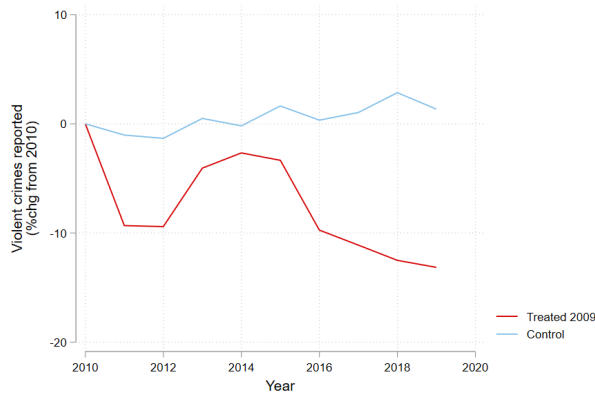
(b) Non-Violent Property Crimes
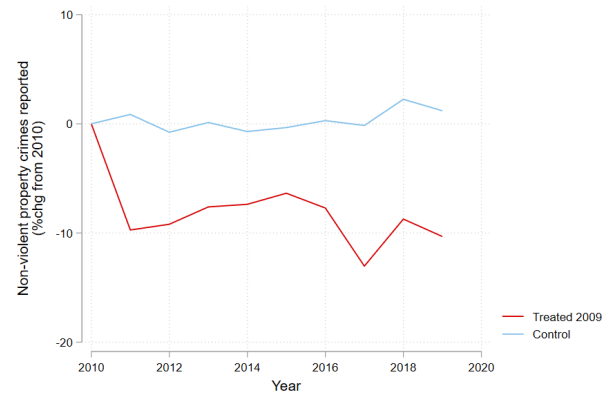
Notes: Percent change in crime is calculated by comparing each school to itself in 2010. Crime relative to 2010 is then purged of common year effects by de-meaning within year for the full sample, weighted by total enrollment. Plots display the average across schools of this measure, again weighting by total enrollment.

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Figure A5: Descriptive Evidence of Changes in Hispanic Victimization by Gender

(a) Female Hispanic Victims

(b) Male Hispanic Victims
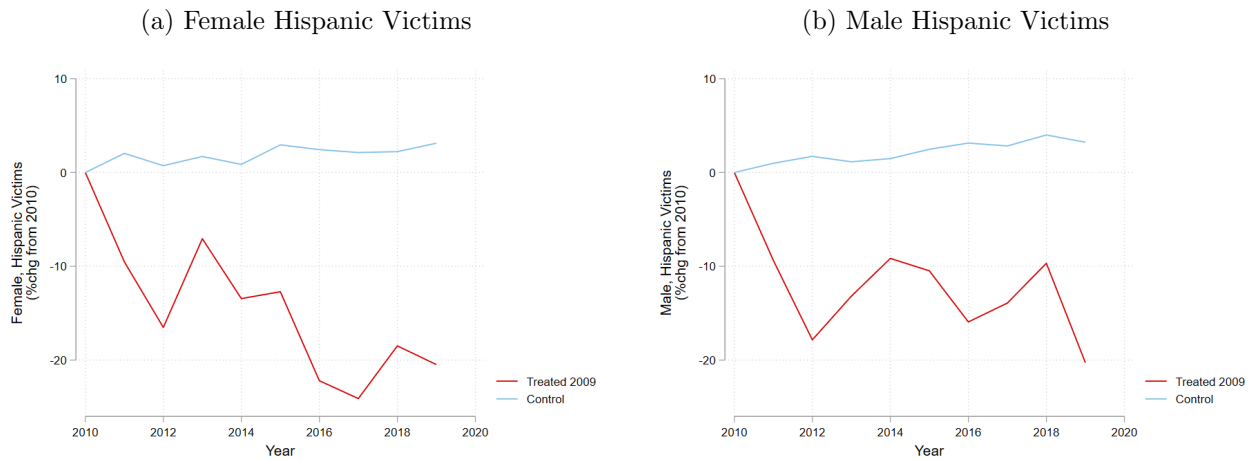
Notes: Percent change in Hispanic victimization is calculated by comparing each school to itself in 2010. Hispanic victimization relative to 2010 is then purged of common year effects by de-meaning within year for the full sample, weighted by total enrollment. Plots display the average across schools of this measure, again weighting by total enrollment.

Figure A6: Descriptive Evidence of Changes in Victimization of Minors by Gender

(a) Under 18 Female Victims

(b) Under 18 Male Victims

Notes: Percent change in underage victimization is calculated by comparing each school to itself in 2010. Underage victimization relative to 2010 is then purged of common year effects by de-meaning within year for the full sample, weighted by total enrollment. Plots display the average across schools of this measure, again weighting by total enrollment.

Figure A7: Descriptive Evidence of Changes in Arrests of Hispanic Minors by Gender

(a) Under 18 Hispanic Female Arrests

(b) Under 18 Hispanic Male Arrests
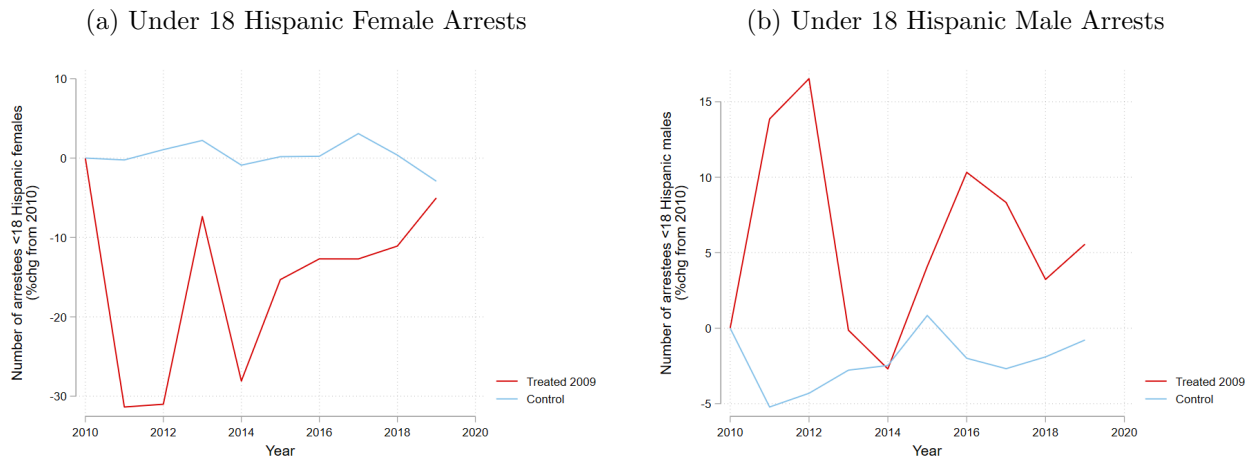
Notes: Percent change in underage Hispanic arrests are calculated by comparing each school to itself in 2010. Underage Hispanic arrests relative to 2010 is then purged of common year effects by de-meaning within year for the full sample, weighted by total enrollment. Plots display the average across schools of this measure, again weighting by total enrollment.

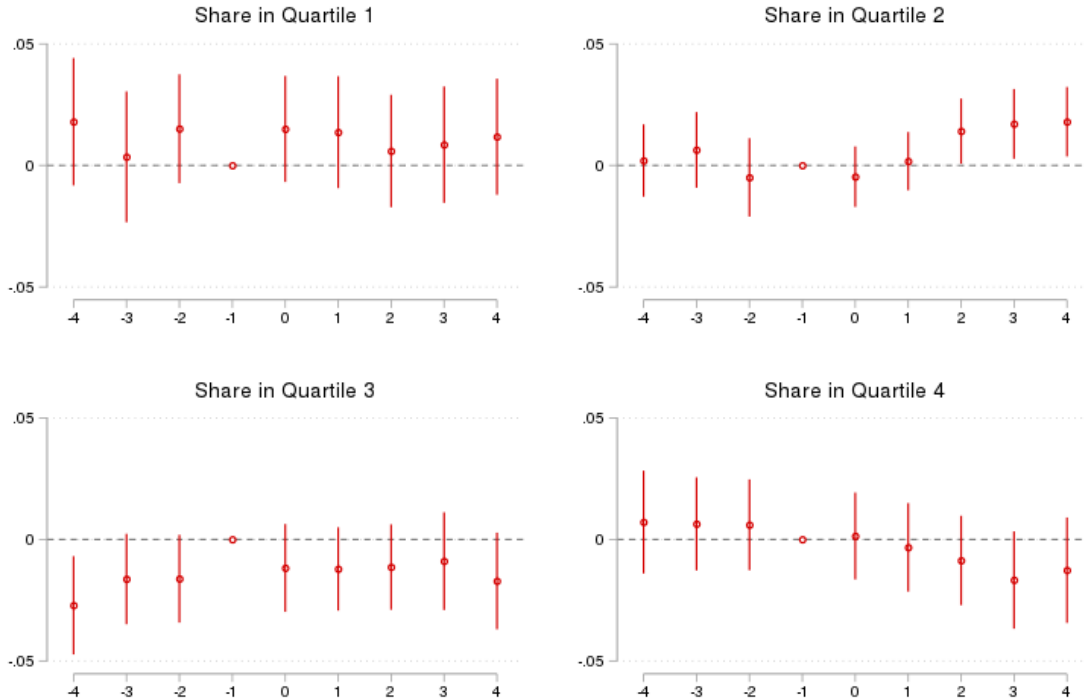Figure A8: Relative Demographic Changes in Safety Zone Schools
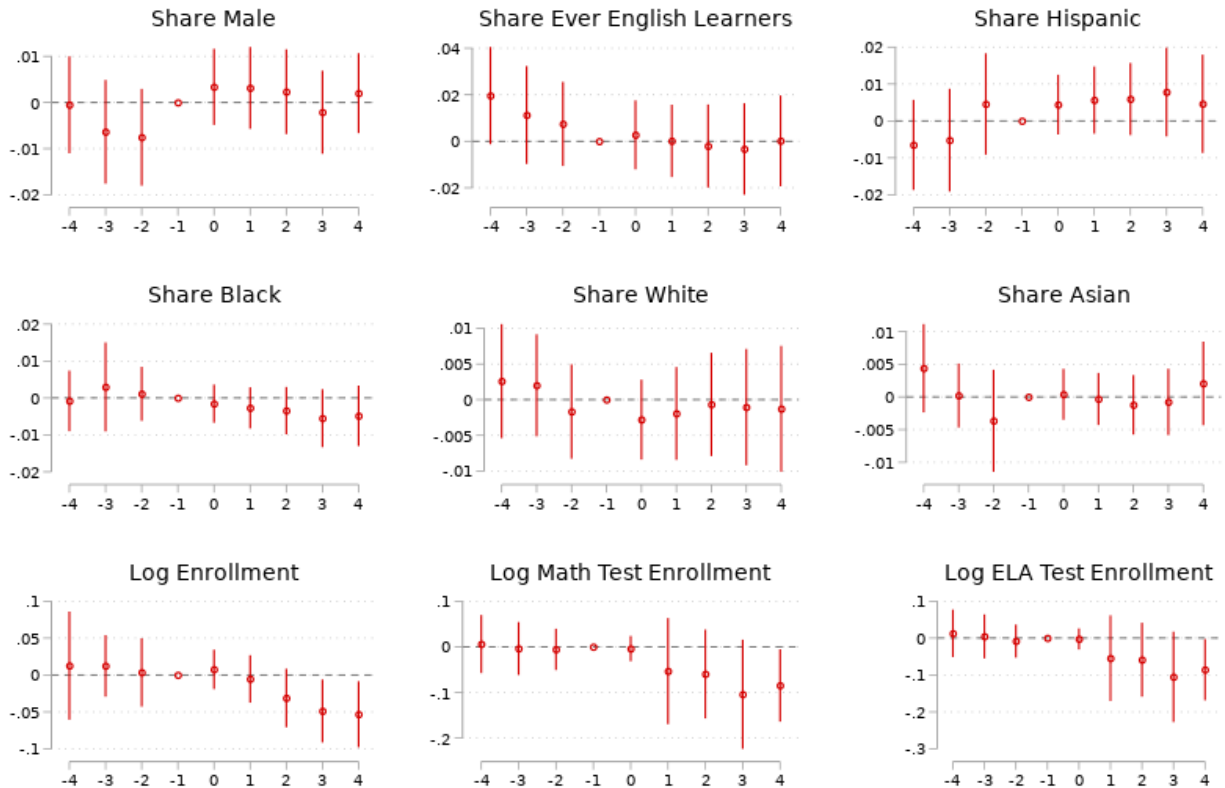
Notes: Coefficients and 95% confidence intervals are obtained by estimating the school-level event study in equation 4, regressing relative treatment indicators on school shares of gender, English learners, ethnicity groups, and the log of total enrollment as well as tested enrollment in Math and English. I drop schools that newly opened inside a safety zone as there is no pre-treatment period against which to compare changes.

Figure A9: Effect of Safety Zones on School Residualized Test Scores
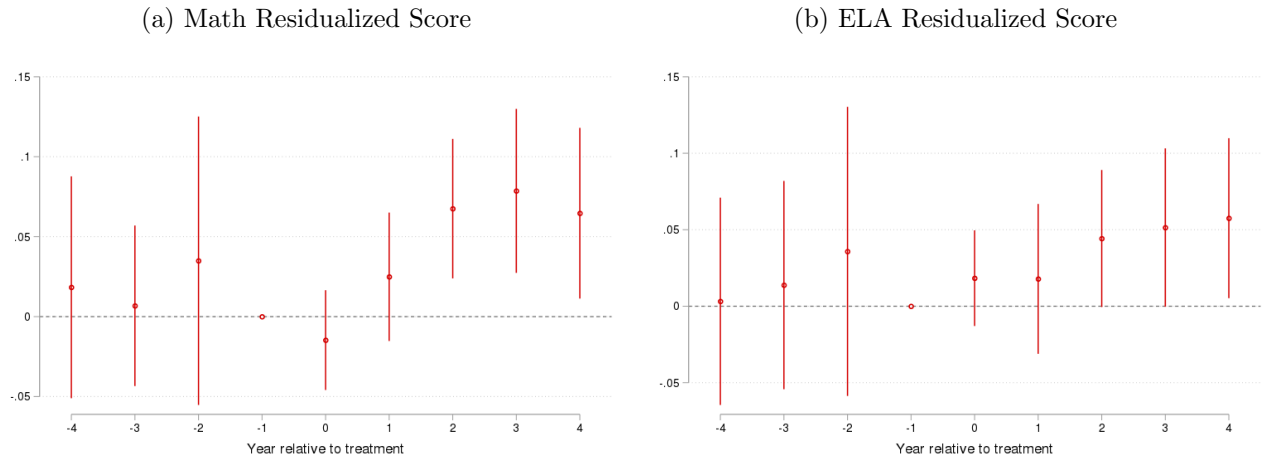
(a) Math Residualized Score

(b) ELA Residualized Score

Notes: Plots display point estimates and 95% confidence intervals of the school-level event study in Equation 4. The dependent variable is the school-averaged test score residualized on a non-parametric model interacting gender, English learner status, free- or reduced-price lunch status, student ethnicity, and highest parental education. The year before treatment, $k = -1$ is omitted as a baseline. Standard errors are clustered at the school level.

Figure A10: Effect on Shares of Baseline GPA Quartiles

Notes: Coefficients and 95% confidence intervals are obtained by estimating the school-level event study in equation 4, regressing relative treatment indicators on the share of students in each quartile of baseline grade point average (GPA). Baseline GPA is estimated using grade 1 (or kindergarten, or grade 2 where missing) GPA. The year before treatment, $k = -1$ is omitted as a baseline. Standard errors are clustered at the school level.

Figure A11: Summary of New School Openings

Notes: Categorization into future safety zone and existing safe zone are based on the treatment status at the time the school opened.

Figure A12: Student Treatment Effects Dropping New Schools

(a) Math Standardized Scores   (b) English Standardized Scores

Notes: Plots display point estimates and 95% confidence intervals of the student-level event study in Equation 5 for test scores, dropping schools that opened in the past 4 years and controlling for the share of a student's cohort last year that enrolled in a new school this year. The year before treatment, $k = -1$ is omitted as a baseline. Test scores are normalized within subject-grade-year using statewide scores. Standard errors are two-way clustered at the student- and school-level.

Figure A13: Effects of Safety Zones on Mobility Across Schools

(a) Share Change Schools Before Terminal Grade
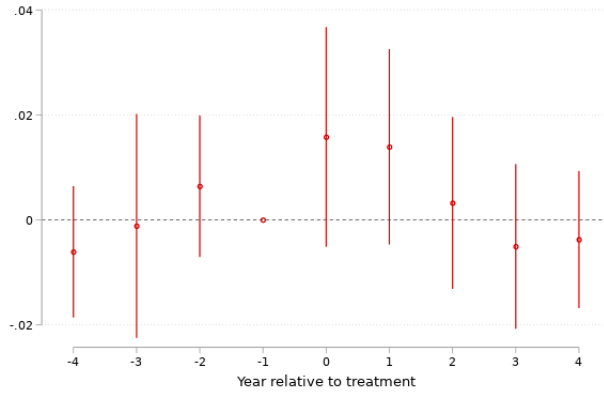
(b) Share Exit Sample
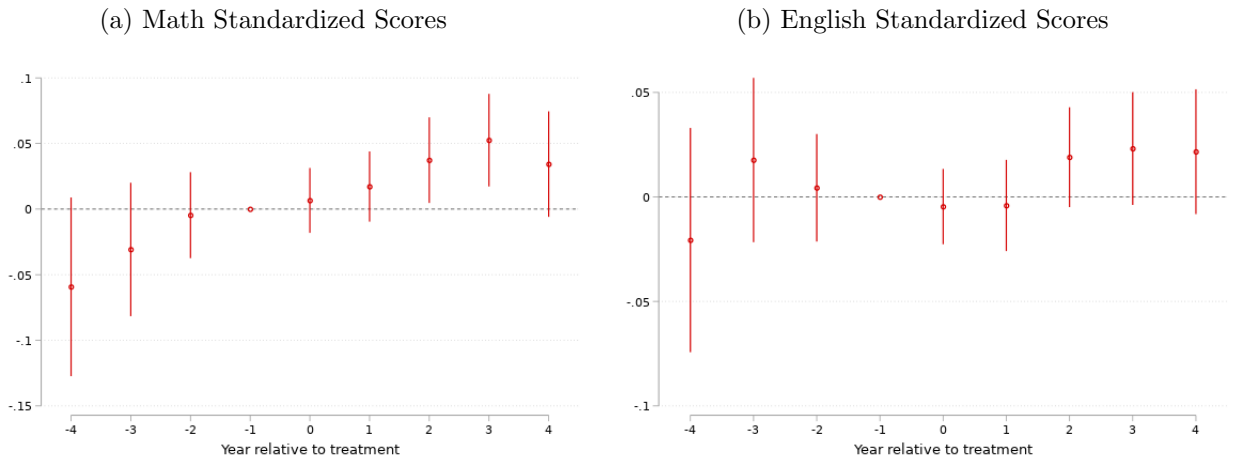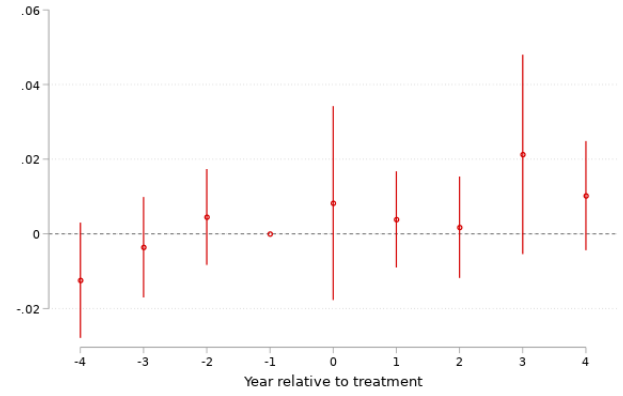

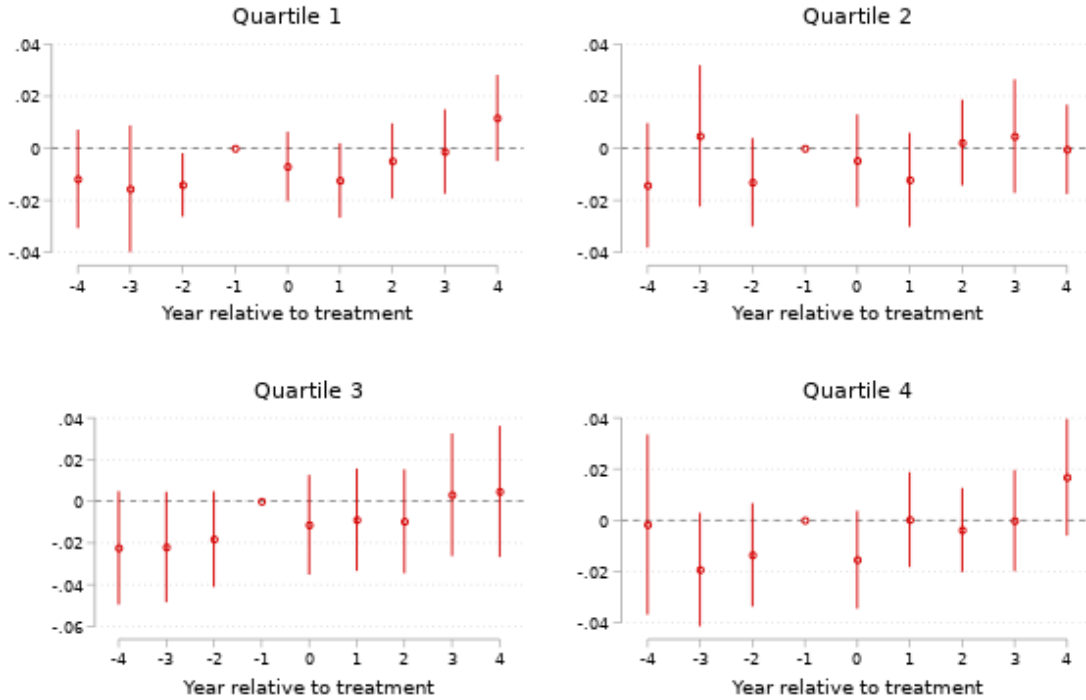Notes: Coefficients and 95% confidence intervals are obtained by estimating the school-level event study in equation 4. The dependent variable in the left panel is the share of students who leave the school at the end of the year for another school in the district. In the right panel, it is the share of students who disappear from the sample at the end of the year. The year before treatment, $k = -1$ is omitted as a baseline. Standard errors are clustered at the school level.

Figure A14: Share Exit Sample by Baseline GPA Quartile

Notes: Coefficients and 95% confidence intervals are obtained by estimating the school-level event study in equation 4 for groups of students based on their grade 1 grade point average. The dependent variable is the share of students who disappear from the sample at the end of the year. The year before treatment, $k = -1$ is omitted as a baseline. Standard errors are clustered at the school level.

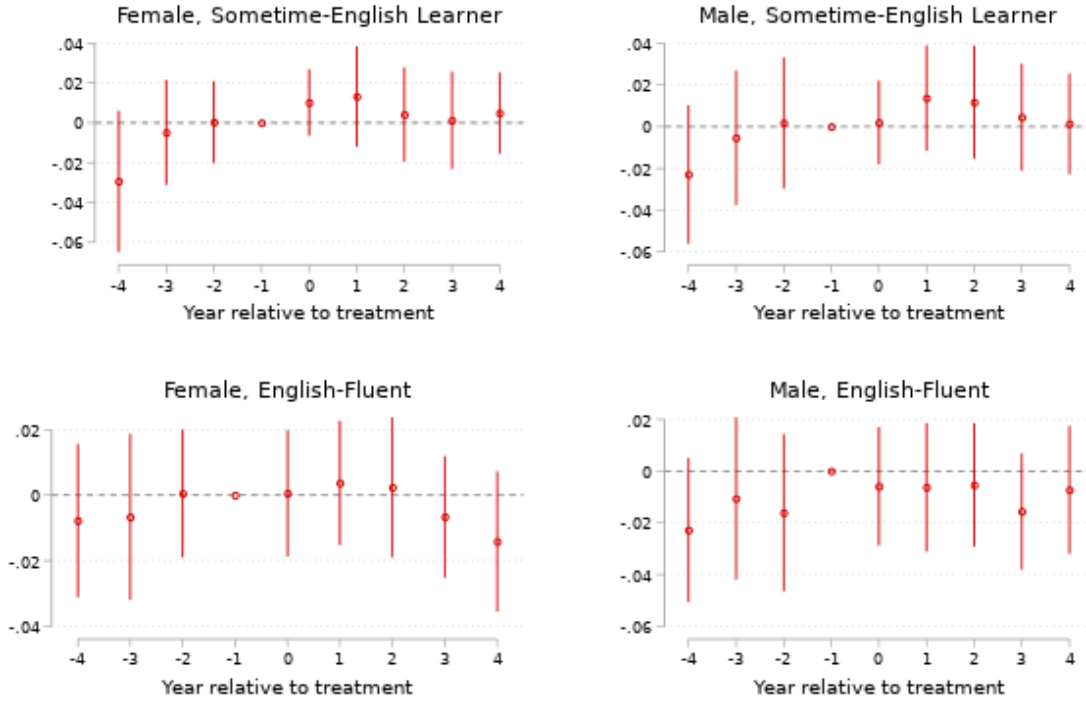Figure A15: Share Exit Sample by Gender x English Learner Status

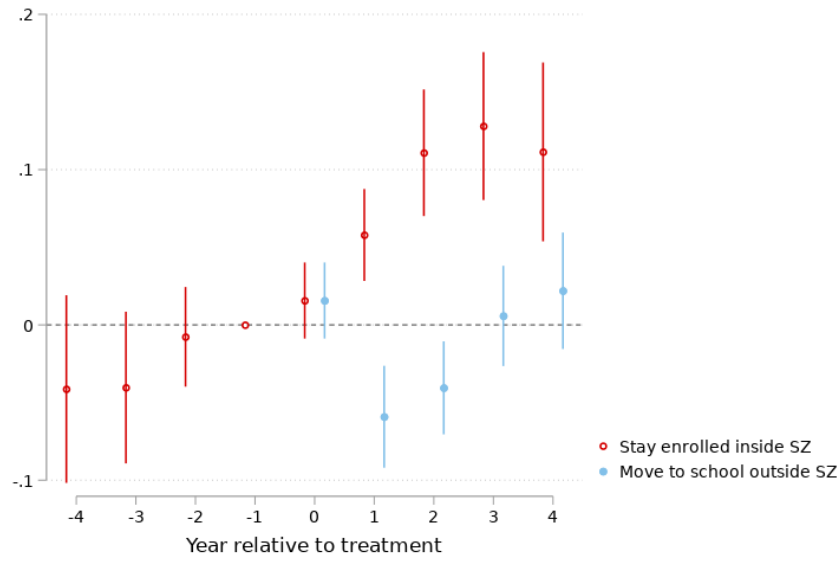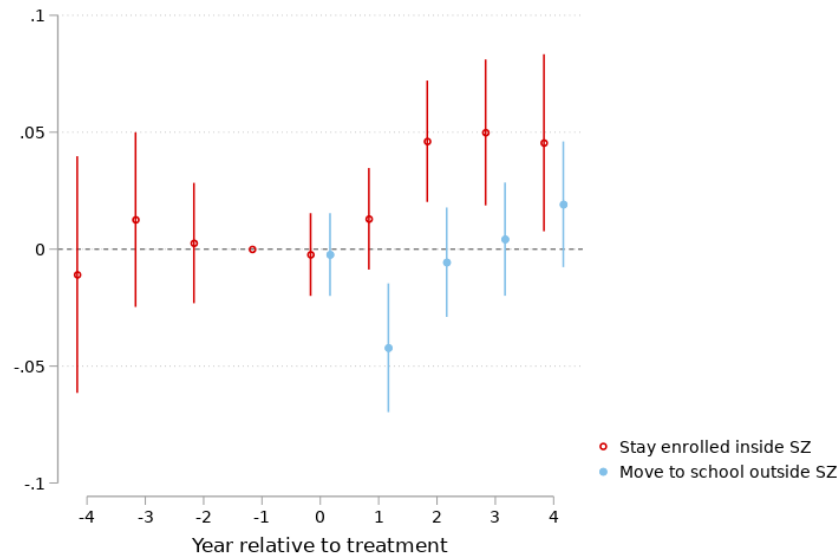
Notes: Coefficients and 95% confidence intervals are obtained by estimating the school-level event study in equation 4 for students grouped by the gender and English learner status. The dependent variable is the share of students who disappear from the sample at the end of the year. The year before treatment, $k = -1$ is omitted as a baseline. Standard errors are clustered at the school level.

Figure A16: Student Effects on Math by Safety Zone Exit

Notes: Plots display point estimates and 95% confidence intervals of the student-level event study in Equation 5 for math test scores, where relative treatment indicators $D_{i,t}^k$ are interacted with an indicator for whether the student remains enrolled in a school inside a safety zone. The year before treatment, $k = -1$ is omitted as a baseline. Test scores normalized within subject-grade-year using statewide scores. Standard errors are two-way clustered at the student- and school-level.

Figure A17: Student Effects on English by Safety Zone Exit

Notes: Plots display point estimates and 95% confidence intervals of the student-level event study in Equation 5 for English test scores, where relative treatment indicators $D_{i,t}^k$ are interacted with an indicator for whether the student remains enrolled in a school inside a safety zone. The year before treatment, $k = -1$ is omitted as a baseline. Test scores normalized within subject-grade-year using statewide scores. Standard errors are two-way clustered at the student- and school-level.

Figure A18: Main Results using Interaction-Weighted Estimator

(a) Math

(b) English

Notes: Plots display point estimates and 95% confidence intervals of the student-level event study in Equation 5 using Sun and Abraham's interaction weighted estimator to correct for bias from heterogeneous treatment effects. The year before treatment, $k = -1$ is omitted as a baseline. Test scores normalized within subject-grade-year using statewide scores. Standard errors are two-way clustered at the student- and school-level.
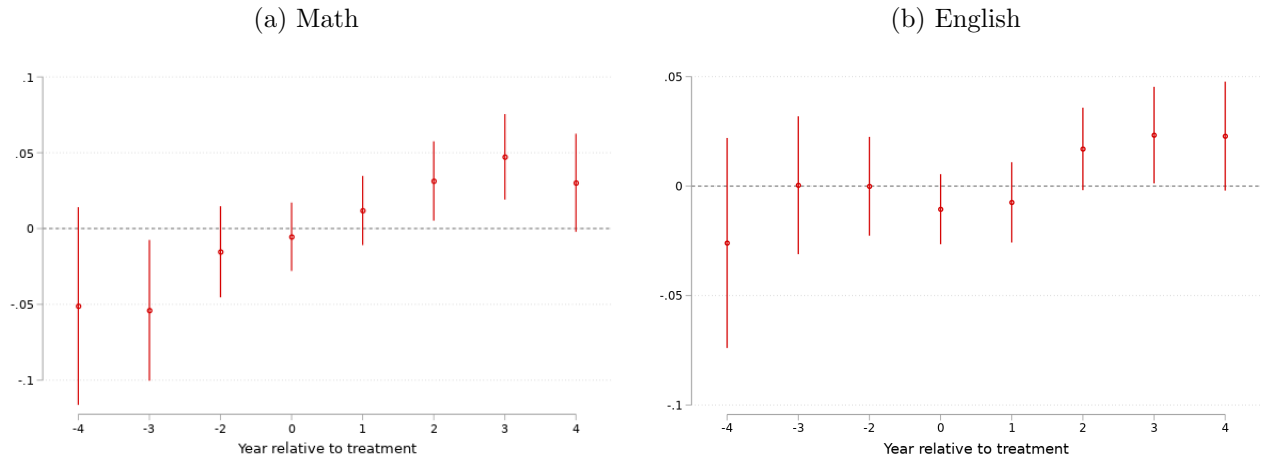
B Appendix Tables

Table B1: Math Effects Varying Overlap Thresholds for Defining Treatment

	50% Overlap (1)	67% Overlap (2)	75% Overlap (3)	80% Overlap (4)	85% Overlap (5)
-4	0.024	-0.058*	-0.039	-0.037	-0.040
	(0.019)	(0.032)	(0.031)	(0.031)	(0.032)
-3	-0.005	-0.021	-0.031	-0.031	-0.039
	(0.016)	(0.022)	(0.025)	(0.025)	(0.026)
-2	0.001	0.008	-0.004	-0.004	-0.017
	(0.011)	(0.013)	(0.016)	(0.017)	(0.018)
-1	.	.	.	.	.
	(.)	(.)	(.)	(.)	(.)
0	0.000	-0.002	0.007	0.009	0.000
	(0.010)	(0.012)	(0.012)	(0.013)	(0.014)
1	0.010	0.012	0.020	0.022*	0.017
	(0.013)	(0.014)	(0.013)	(0.013)	(0.015)
2	0.022	0.040**	0.041**	0.044***	0.042**
	(0.016)	(0.017)	(0.016)	(0.017)	(0.018)
3	0.035*	0.052***	0.052***	0.056***	0.058***
	(0.018)	(0.019)	(0.018)	(0.018)	(0.020)
4	0.015	0.038*	0.037*	0.040*	0.040*
	(0.021)	(0.022)	(0.020)	(0.021)	(0.023)
Constant	-0.091***	-0.095***	-0.108***	-0.109***	-0.108***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
R-squared	0.756	0.756	0.756	0.756	0.755
N	2,089,600	2,164,335	2,258,772	2,264,597	2,263,024
Treated N	454,500	324,485	289,558	284,069	244,828

* p<0.10, ** p<0.05, *** p<0.01

Notes: Table displays point estimates of the student-level event study in Equation 5 for math test scores, varying the overlap threshold for defining school treatment. Column (3) displays results from my preferred specification, wherein schools are treated when their attendance boundary becomes over 75% overlapped by a safety zone. Columns (1) and (2) use a lower threshold and columns (4) and (5) a higher threshold. The year before treatment, $k = -1$ is omitted as a baseline. Test scores are normalized within subject-grade-year using statewide scores. Standard errors are two-way clustered at the student- and school-level.

Table B2: English Effects Varying Overlap Thresholds for Defining Treatment

	50% Overlap (1)	67% Overlap (2)	75% Overlap (3)	80% Overlap (4)	85% Overlap (5)
-4	0.014	-0.030	-0.010	-0.006	-0.020
	(0.013)	(0.019)	(0.026)	(0.026)	(0.026)
-3	0.007	0.008	0.016	0.018	0.002
	(0.011)	(0.016)	(0.019)	(0.019)	(0.019)
-2	0.004	0.002	0.004	0.006	-0.003
	(0.008)	(0.010)	(0.013)	(0.013)	(0.014)
-1	.	.	.	.	.
	(.)	(.)	(.)	(.)	(.)
0	0.001	-0.003	-0.005	-0.002	-0.008
	(0.007)	(0.008)	(0.009)	(0.009)	(0.010)
1	0.002	0.001	-0.003	-0.001	-0.009
	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)
2	0.019*	0.028**	0.023*	0.024**	0.017
	(0.010)	(0.012)	(0.012)	(0.012)	(0.012)
3	0.019	0.029**	0.023*	0.027**	0.021
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)
4	0.016	0.028*	0.023	0.026*	0.019
	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)
Constant	-0.173***	-0.179***	-0.193***	-0.193***	-0.193***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
R-squared	0.814	0.813	0.813	0.813	0.812
N	2,465,420	2,562,548	2,677,073	2,682,541	2,684,447
Treated N	503,229	352,836	310,912	303,443	260,758

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Notes: Table displays point estimates of the student-level event study in Equation 5 for English test scores, varying the overlap threshold for defining school treatment. Column (3) displays results from my preferred specification, wherein schools are treated when their attendance boundary becomes over 75% overlapped by a safety zone. Columns (1) and (2) use a lower threshold and columns (4) and (5) a higher threshold. The year before treatment, $k = -1$ is omitted as a baseline. Test scores are normalized within subject-grade-year using statewide scores. Standard errors are two-way clustered at the student- and school-level.

Table B3: Math Effects Varying Lower Thresholds for Defining Treatment

	All treated	25% Overlap	Drop Treated that Earlier Surpassed Drop Treated that Earlier Surpassed 33% Overlap	50% Overlap	67% Overlap
	(1)	(2)	(3)	(4)	(5)
-4	-0.017	-0.039	-0.039	-0.032	-0.008
	(0.024)	(0.034)	(0.031)	(0.030)	(0.027)
-3	-0.019	-0.028	-0.031	-0.027	-0.016
	(0.020)	(0.026)	(0.025)	(0.023)	(0.020)
-2	-0.002	-0.004	-0.004	0.000	0.005
	(0.012)	(0.016)	(0.016)	(0.014)	(0.012)
-1	0.000	0.000	0.000	0.000	0.000
	(.)	(0.000)	(.)	(.)	(0.000)
0	0.016	0.001	0.007	0.010	0.015
	(0.011)	(0.013)	(0.012)	(0.012)	(0.011)
1	0.024*	0.013	0.020	0.023*	0.022*
	(0.012)	(0.014)	(0.013)	(0.013)	(0.013)
2	0.040***	0.033*	0.041**	0.040**	0.037**
	(0.015)	(0.018)	(0.016)	(0.016)	(0.015)
3	0.045***	0.045**	0.052***	0.049***	0.042**
	(0.016)	(0.019)	(0.018)	(0.017)	(0.017)
4	0.028	0.028	0.037*	0.033*	0.025
	(0.020)	(0.021)	(0.020)	(0.020)	(0.020)
Constant	-0.117***	-0.104***	-0.108***	-0.111***	-0.115***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
R-squared	0.755	0.756	0.756	0.756	0.755
N	2,343,823	2,237,365	2,258,772	2,288,978	2,322,797
Treated N	569,293	267,729	289,558	320,611	355,276

* p<0.10, ** p<0.05, *** p<0.01

Notes: Table displays point estimates of the student-level event study in Equation 5 for math test scores, varying the lower overlap threshold for defining school treatment. Column (3) displays results from my preferred specification, wherein schools that earlier surpassed 33% overlap by a safety zone before reaching the 75% threshold are dropped from the treated sample. Columns (1) makes no such exclusion, column (2) uses a lower threshold, and columns (4) and (5) use a higher threshold. The year before treatment, $k = -1$ is omitted as a baseline. Test scores are normalized within subject-grade-year using statewide scores. Standard errors are two-way clustered at the student- and school-level.

Table B4: English Effects Varying Lower Thresholds for Defining Treatment

| | All treated | Drop Treated that Earlier Surpassed | | | |
| | | 25% Overlap | 33% Overlap | 50% Overlap | 67% Overlap |
	(1)	(2)	(3)	(4)	(5)
-4	-0.019	-0.006	-0.010	-0.011	-0.009
	(0.015)	(0.030)	(0.026)	(0.024)	(0.017)
-3	-0.003	0.018	0.016	0.018	0.004
	(0.014)	(0.020)	(0.019)	(0.017)	(0.015)
-2	-0.000	-0.001	0.004	-0.003	0.003
	(0.009)	(0.012)	(0.013)	(0.011)	(0.010)
-1	.	.	.	.	.0
	(.)	(.)	(.)	(.)	(.)
0	0.004	-0.008	-0.005	-0.001	0.003
	(0.007)	(0.010)	(0.009)	(0.009)	(0.008)
1	0.005	-0.005	-0.003	0.002	0.004
	(0.010)	(0.012)	(0.011)	(0.011)	(0.010)
2	0.022**	0.020	0.023*	0.026**	0.023**
	(0.011)	(0.013)	(0.012)	(0.012)	(0.011)
3	0.021*	0.018	0.023*	0.027**	0.023*
	(0.012)	(0.014)	(0.013)	(0.014)	(0.013)
4	0.020	0.019	0.023	0.026*	0.020
	(0.014)	(0.016)	(0.015)	(0.015)	(0.015)
Constant	-0.203***	-0.189***	-0.193***	-0.196***	-0.201***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
R-squared	0.812	0.813	0.813	0.812	0.812
N	2,774,317	2,653,384	2,677,073	2,711,824	2,750,178
Treated N	569,293	286,947	310,912	346,236	385,493

* p<0.10, ** p<0.05, *** p<0.01

Notes: Table displays point estimates of the student-level event study in Equation 5 for English test scores, varying the lower overlap threshold for defining school treatment. Column (3) displays results from my preferred specification, wherein schools that earlier surpassed 33% overlap by a safety zone before reaching the 75% threshold are dropped from the treated sample. Columns (1) makes no such exclusion, column (2) uses a lower threshold, and columns (4) and (5) use a higher threshold. The year before treatment, $k = -1$ is omitted as a baseline. Test scores are normalized within subject-grade-year using statewide scores. Standard errors are two-way clustered at the student- and school-level.