The Effects of Police Violence on Inner-City Students

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Abstract

Nearly a thousand officer-involved killings occur each year in the United States. This paper documents the large, racially-disparate impacts of these events on the educational and psychological well-being of of public high school students in a large, urban school district. Exploiting hyperlocal variation in how close students live to a killing, I find that exposure to police violence leads to persistent decreases in GPA, increased incidence of emotional disturbance and lower rates of high school completion and college enrollment. These effects are driven entirely by black and Hispanic students in response to police killings of other minorities and are largest for incidents involving unarmed individuals.

I   Introduction

A central role of the state is to ensure public safety (Atkinson and Stiglitz, 2015). As means of achieving this, American law enforcement officers are afforded broad discretion over the use of force, and roughly a thousand individuals are killed by police each year. In addition to protecting civilians from imminent harm, these incidents may help to deter future criminal activity (Becker, 1968).

At the same time, the four largest urban riots in recent American history were all triggered by acts of police violence (DiPasquale and Glaeser, 1998).[1] Experiences with aggressive policing have been linked to unfavorable attitudes towards law enforcement, particularly among racial minorities, whose lifetime odds of being killed by police are as high as one in a thousand (Skolnick and Fyfe, 1993; Weitzer and Tuch, 2004; Brunson and Miller, 2005).[2] These attitudes are, in turn, correlated with fear (Hale, 1996; Renauer, 2007; Boyd, 2018), perceived discrimination (Brunson, 2007; Carr et al., 2007) and institutional distrust (Bobo and Thompson, 2006; Kirk and Papachristos, 2011).

Nonetheless, there exists little causal evidence of the social impacts of police use of force on local communities. Correlational analysis of police violence and neighborhood health is confounded by the fact that use of force is more likely to occur in disadvantaged areas, where homicide and poverty rates are high (Kania and Mackey, 1977; Jacobs, 1998). Researchers have attempted to address this issue by exploiting the timing of high-profile incidents: for example, the police beatings of Rodney King in Los Angeles (Sigelman et al., 1997) and Frank Jude in Milwaukee (Desmond et al., 2016) or the lethal shooting of Michael Brown in Ferguson (Gershenson and Hayes, 2017).[3] However, such landmark events were often tipping points for larger social movements, like widespread riots in Los Angeles and Black Lives Matter in Ferguson. Thus, their case studies may not be representative of the vast majority of police killings that go unreported in the media and provide limited insight into the day-to-day effects of use of force on nearby civilians. Furthermore, most existing studies examine impacts on attitudes or interactions with law enforcement and are unable to shed light on broader economic implications.

This paper seeks to document the short and long-run consequences of police killings on the

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[1]These include: the 1965 Watts riots, the 1980 Miami riots, the 1992 Rodney King riots and the 2013 Ferguson riots. Police violence has also triggered large protests in other contexts. For example, in 2014, the use of tear gas against students in Hong Kong sparked protests that blockaded roadways for several months.

[2]Edwards et al. (2019) estimate that roughly one in 1,000 black men and one in 2,000 Hispanic men will be killed by police over their life course, relative to one in 3,000 white men and one in 7,500 Asian men. Among 25- to 29-year-old males, police violence is the sixth leading cause of death, behind accidents, suicides, other homicides, heart disease and cancer.

[3]Similarly, White et al. (2018) examine the impact of the Freddie Gray killing on perceptions of procedural justice. The policy implications of those findings are discussed by Lacoe et al. (2018).

educational and psychological well-being of inner-city youth. I focus on high school students, both because teenagers face crucial educational decisions and because studies suggest that even vicarious police contact during adolescence may be influential in shaping long-run beliefs and institutional trust (Winfree Jr and Griffith, 1977; Leiber et al., 1998; Hurst and Frank, 2000; Tyler et al., 2014).[4]

To estimate these effects, I combine two highly detailed and novel datasets. The first contains home addresses and individual-level panel data for all high school students enrolled from 2002 to 2016 in a large urban school district in the Southwest (the "District"). The second contains incident-level information on the universe of officer-involved killings in the surrounding county (the "County"). By geo-coding the exact location of the 627 incidents and over 700,000 home addresses, I am able to calculate each student's precise geographic proximity to police violence. Leveraging a dynamic difference-in-differences design, I then exploit hyperlocal variation in the location and timing of police killings to compare changes in well-being among students who lived very close to a killing to students from the same neighborhood who lived slightly farther away.

I find that acts of police violence have negative spillovers across a range of outcomes. In the days immediately after a police killing, absenteeism spikes among nearby students. Effects are largest for students who lived closest to the event and dissipate beyond 0.50 miles. This is consistent with the highly localized nature of police killings, nearly 80% of which went unmentioned in local newspapers.

In the medium-run, students living within half a mile of a police killing experience decreases in GPA as large as 0.08 standard deviations that persist for several semesters. That these effects stem from exposure to a single officer-involved killing and that each killing affects more than 300 students, on average, suggests the potentially traumatizing impact of police violence. As corroboration, I find that exposed students are 15% more likely to be classified with emotional disturbance – a chronic learning disability associated with PTSD and depression – and twice as likely to report feeling unsafe in their neighborhoods the following year.

In the long-run, students exposed to officer-involved killings in the 9th grade are roughly 3.5% less likely to graduate from high school and 2.5% less likely to enroll in college. Though smaller in magnitude, effects remain statistically and economically significant for students exposed in the 10th and 11th grades.

In unpacking the results, I document stark heterogeneity across race, both of the student and of the person killed. The effects are driven entirely by black and Hispanic students in

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[4]Juveniles also experience far more frequent police interactions than other populations (Snyder et al., 1996).

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response to police killings of other underrepresented minorities. I find no significant impact on white or Asian students. I also find no significant impact for police killings of white or Asian suspects. These differences cannot be explained by other contextual factors correlated with race, such as neighborhood characteristics, media coverage or other suspect and student observables. However, the pattern of effects is consistent with large racial differences in concerns about use of force and police legitimacy.[5]

To further explore mechanisms, I exploit hand-coded contextual information drawn from District Attorney incident reports and other sources. I find that police killings of unarmed individuals generate negative spillovers that are roughly twice as large as killings of individuals armed with a gun or other weapon. This difference is statistically significant and unattenuated when accounting for other observable suspect, neighborhood and contextual factors. These findings suggest that student responses to police killings may be a function not simply of violence or gunfire *per se* but also of the perceived "reasonableness" of officer actions. Consistent with this, I find that the marginal effects of criminal homicides are only half as large as those of police killings. Furthermore, unlike with police violence, the effects do not vary with the race of the person killed. While students are only affected by police killings of blacks and Hispanics, they respond similarly to criminal homicides of whites and minorities.

This paper makes four main contributions. First, it documents the large externalities that police violence may have on local communities. My findings suggest that, on average, each officer-involved killing in the County caused three students of color to drop out of high school. As fatal shootings comprise less than one-tenth of one percent of all police use of force encounters (Davis et al., 2018), this is likely a lower bound of the total social costs of aggressive policing. While estimating the effects of less extreme uses of force is complicated both by measurement error and by their relative prevalence, research suggests that these interactions are also highly salient to local residents (Brunson and Miller, 2005; Brunson, 2007; Legewie and Fagan, 2019) and are perhaps more likely to be exercised in a racially-biased manner (Fryer Jr, 2019).[6]

Second, this paper complements a growing body of research demonstrating how perceived

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[5] A 2015 survey found that 75% of black respondents and over 50% of Hispanic respondents felt police violence against the public is a very or extremely serious issue, while only 20% of whites reported the same (AP-NORC, 2015). Similarly, Bureau of Justice Statistics show that even conditional on experiencing force, minorities are significantly more likely than whites to believe that police actions were excessive or improper (Davis et al., 2018).

[6] As Fryer Jr (2019) states, "data on lower level uses of force" are "virtually non-existent." Causal identification is further complicated by the fact that routine tactics like stop-and-frisk are often explicitly determined by policing objectives and thus more likely to be endogenous with changes in neighborhood conditions and law enforcement strategy.

discrimination may lead to "self-fulfilling prophecies" in education (Carlana, 2019), labor markets (Glover et al., 2017) and health care (Alsan and Wanamaker, 2018).[7]. While empirical evidence of racial bias is mixed (Fryer Jr, 2019; Nix et al., 2017; Knox and Mummolo, 2019; Johnson et al., 2019), the vast majority of blacks and Hispanics in America believe that police discriminate in use of force (Pew Research Center, 2016, 2019; Dawson et al., 1998; AP-NORC, 2015).[8] Though more work is needed, the pattern of results suggests that the educational spillovers of officer-involved killings may be driven in part by perceptions of injustice surrounding these events.

Third, this paper builds upon existing research measuring the short-run impacts of criminal violence on children (Sharkey, 2010; Sharkey et al., 2012, 2014; Beland and Kim, 2016; Rossin-Slater et al., 2019; Carrell and Hoekstra, 2010; Monteiro and Rocha, 2017; Gershenson and Tekin, 2017).[9] However, in contrast to other forms of violence, the explicit purpose of law enforcement is to improve public outcomes and the directional impact of aggressive policing is *ex ante* far more ambiguous. Thus, my findings serve not simply as an exercise in quantifying the costs of violence but rather as important inputs for pressing policy discussions around police oversight and officer use of force.

Finally, this paper provides further insight into the link between neighborhoods and economic mobility (Katz et al., 2001; Chetty et al., 2016). Chetty et al. (2020) find that intergenerational mobility differs dramatically between blacks and whites, even for children from the same neighborhood and socioeconomic background. Consistent with research by Derenoncourt (2018) documenting a negative correlation between police presence and black upward mobility in Great Migration destinations, my results suggest that law enforcement may play an important role in explaining this racial disparity. This is not only because minorities are more likely than whites to experience police contact but also because, conditional on contact, minorities may be more negatively affected by those interactions. Understanding these effects and disentangling them from correlated factors like crime and poverty is critical to the development of policies aimed at addressing persistent racial gaps across a wide range of domains.

The remainder of this paper is organized as follows: Section II describes the background and data, Section III discusses the identification strategy and provides evidence of its validity,

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[7]It also relates to work by Chetty et al. (2020), who find that implicit bias measures and Google searches of the "n" word strongly predict racial disparities in income mobility, and by Charles and Guryan (2008), who find that General Social Survey measures of prejudice are correlated with black-white wage gaps in a state.

[8]For example, in a 2015 national survey, 85% of black respondents and 63% of Hispanic respondents reported believing that police are more likely to use force against a black person. Similar shares reported believing that police "deal more roughly with members of minority groups."

[9]Other work examines the impact of violence on other margins, like wages (Aizer, 2007).

Section IV presents primary estimation results for academic achievement and psychological well-being, Section V explores mechanisms by estimating differential effects by race and incident context and by comparing the effects of police killings to criminal homicides, Section VI examines long-run effects on educational attainment, and Section VII concludes.

II   Data

A   Police Killings Data

Incident-level data on police killings come from a publicly available database compiled by a local newspaper, which chronicles all deaths in the County committed by a "human hand." Whether an officer was responsible for the death is based on information from the coroner and police agencies as well as from the newspaper's own investigation. For each incident, database records the name, age and race of the deceased as well as the exact address and date of the event. In total, the data contains 627 incidents from July 2002 to June 2016.

I supplement this data with contextual details drawn from District Attorney incident reports. Each report includes a detailed description of the event based on forensic and investigative evidence as well as officer and witness testimonies. Reports also provide a legal analysis of officer actions. DA reports are not available for incidents that occurred prior to 2004 or that are still under investigation. For killings without DA reports, I searched for incident details from police reports and other sources.

Of the 627 sample incidents, I was able to obtain contextual information for 556 killings: 513 from DA reports and 43 from other sources. In each case, I read and hand-coded reports to capture whether a weapon was recovered from the suspect and if so, what type. In cases where a gun was found, I additionally captured whether the suspect had fired their weapon at officers or civilians during the police encounter or immediately before (for example, in cases where police were dispatched for an active shooter).

It is worth noting that these measures provide an admittedly incomplete picture of the surrounding events, which often involve imperfect information and split-second decisions. In many cases, police actions were predicated on faulty or misreported information. For example, in 2010, a woman called 911 to report that a man with a gun was sitting in her apartment stairwell. Officers arrived and shot the man, but he was actually holding a water hose nozzle. Similar situations arose when police were confronted by individuals armed with firearms that turned out to be replicas. In other cases, killings were precipitated by seemingly innocuous encounters that escalated unexpectedly. For instance, in 2014, patrol officers attempted to stop a man for riding a bicycle on the sidewalk. Rather than complying, the

man grabbed an officer's gun and was shot by the officer's partner. Nonetheless, information about weapon type and discharge has the benefit of being objectively verifiable and can be found in all available incident reports. These details are also directly factored into legal assessments of police actions as well as community perceptions of the "reasonableness" of force (Brandl et al., 2001; Braga et al., 2014).

Panel A of Table I provides a summary of the police killings data. 52% of suspects were Hispanic, 26% were black, 19% were white and 3% were Asian.[10] Relative to their county population shares, black (Hispanic) individuals are roughly 4 (1.6) times more likely to be killed by police than whites, who are in turn 3 times more likely to be killed than Asians. The vast majority of individuals (97%) were male. The average age at death was 32 years old. Only 10% of individuals were of school age (i.e., 19 or younger) and none were actively-enrolled District students.

Consistent with national statistics, 54% of suspects were armed with a firearm (including BB guns and replicas), while another 29% were armed with some other type of weapon. This includes items like knives and pipes as well as cases in which the individual attempted to hit someone with a vehicle. The remaining individuals, nearly 20% of the sample, were completely unarmed. This is similar to the share of suspects who actively fired at officers and civilians (22% of all suspects; 41% of gun-wielding suspects).

Notably, the vast majority of incidents received little or no media coverage. Only 22% of sample killings were ever mentioned in any of six local newspapers (including one of the largest newspapers in the country) and only 13% were mentioned within 30 days of the event.[11] Conditional on being reported in a newspaper, the median number of articles is two. Only two of the 627 incidents generated levels of media coverage anywhere near that of recent nationally-reported killings.[12]

Examining contextual factors separately by race, black and Hispanic suspects were younger on average than white and Asian suspects (31 vs. 38 years old, respectively) and more likely to possess a firearm (58% vs. 36%). However, rates of media coverage are identical between groups (22%), as are the median number of mentions, conditional on coverage.

Regardless of demographics or circumstance, involved officers were rarely prosecuted. Of the 627 incidents, the District Attorney pursued criminal charges against police in only one case.[13] This is consistent with national statistics, which find that criminal charges were filed

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[10]Race categories are mutually exclusive.

[11]I searched for each incident by suspect name in six local newspapers. Combined, the papers circulate roughly 1 million copies each day in the County and surrounding area.

[12]Those killings were each cited in more than 200 articles. All other killings received fewer than 30 mentions.

[13]Charges were not pressed in that instance until after the end of the sample period.

against police in fewer than half a percent of all officer-involved shootings.

B Student Data

The District administrative data contains individual-level records for all high school students ever enrolled in the District from the 2002-2003 to 2015-2016 academic years. In total, the dataset contains over 700,000 unique students. All student information is anonymized. For each student, I have detailed demographic information including the student's race, gender, date of birth, parental education, home language, free/subsidized lunch status and proficiency on 8th grade standardized tests. The data also contains each student's last reported home address while enrolled in the District.[14]

The dataset includes a host of short and long-run measures of academic achievement. Semester grade point average is calculated from student transcript data. I code letter grades to numerical scores according to a 4.0 scale. I then average grades in math, science, English and social sciences – the subjects used to determine graduation eligibility – by student-semester to produce non-cumulative, semester grade point averages. Daily attendance for every student is available from the 2009-2010 school year onwards. Each student-date observation contains the number of scheduled classes for which a student was absent that day. This information is used construct a binary indicator for whether a student was absent for any class on a given day (Whitney and Liu, 2017).[15]

The primary measures of educational attainment are high school graduation and college enrollment. Graduation is defined as receiving "a high school diploma or equivalent" from the District.[16] I am unable to distinguish between diploma types. Information on whether students enrolled in post-secondary schooling is available for those that graduated from the District between 2009 and 2014 and comes from the National Student Clearinghouse, which provides enrollment information for institutions serving over 98% of all post-secondary students in the country.

The data also contains two sources of information regarding student mental health. From the 2004 school year onwards, I observe the date students were designated by the District as "emotionally disturbed," a federally certified learning disability that "cannot be explained by intellectual, sensory or health factors" and that qualifies for special education accom-

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[14]Because the data does not track previous addresses, I do not observe if a student moved within the District. However, as I discuss in Section III, this is unlikely to be a serious source of bias.

[15]Because attendance data is sometimes missing for some classes but not others within a given student-date, using any absent classes requires less imputation. However, results are robust to coding absenteeism based on all classes on a given date.

[16]The dataset does not contain information on any years of schooling or diplomas that a student obtained at high schools outside of the District. However, it does contain "leave codes" for students who transferred out of the District before graduating, which allows me to test for differential attrition.

modations. This data is used to create student panel data indicating whether a student was classified as "emotionally disturbed" in a given semester. The second source contains student-level responses from a District-wide survey for the 2014-2015 and 2015-2016 academic years. Of particular interest to this study, the survey includes three questions examining feelings of school and neighborhood safety.[17]

Panel B of Table I provides summary statistics for the student data. The District is comprised primarily of underrepresented minorities. 86% of students identify as either black or Hispanic, while only 14% are white or Asian.[18] The majority of students come from disadvantaged households, with 69% qualifying for free or subsidized lunch and fewer than 10% with college-educated parents. Roughly 40% of students demonstrated basic or higher levels of proficiency on 8th grade standardized tests.

Relative to the full sample, students who lived within 0.50 miles of an incident during high school (i.e., the treatment group) are more likely to be Hispanic and to qualify for free lunch, and less likely to speak English at home or to have college-educated parents. However, these students look quite similar, on average, to students in the same Census block groups but more than 0.50 miles away, who comprise the effective control group in my analysis.[19] As shown in the "Area" column of Table I, control students in treated neighborhoods come from similar racial and household backgrounds as treated students, and are in fact, slightly less likely to be proficient or to have college-educated parents. This similarity is an important feature of the research design that helps to bolster internal validity, particularly when comparing longer-run outcomes.

III   Empirical Strategy

A   Exposure to Police Killings

The primary obstacle to identification is that police killings are not random and may be more likely to occur in disadvantaged neighborhoods where poverty and crime are high. Thus, a cross-sectional comparison of students from parts of the County where police shootings are relatively prevalent and students from parts of the County where they are not could be confounded by correlated neighborhood characteristics. Furthermore, if changes in local

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[17]Responses are answered along a Likert scale ranging from one to five. While the survey is not mandatory, it is typically administered during school hours leading to response rates above 75%.

[18]Demographics differ from those of the county as a whole, which is comprised of approximately 48% Hispanics, 9% blacks, 28% non-Hispanic whites and 14% Asian.

[19]As my preferred estimating equation includes Census block group-semester fixed effects, causal identification comes from comparing treatment and control students in the same Census block group, which average roughly one square mile in area.

poverty, crime or other unobserved factors predict police killings, biases could remain even when including student fixed effects in panel analysis.

The address this, I exploit hyperlocal variation in exposure to killings *within* neighborhoods. In essence, identifying variation comes from comparing changes over time among students who lived very close to a police killing to students who lived slightly farther away but in the same neighborhood. Thus, the two groups come from similar backgrounds and were likely exposed to similar local conditions, except for the killing itself.

The plausibility of strategy is aided by two factors. The first is that police killings are quite rare and difficult to predict. Over 300,000 arrests and nearly 60,000 violent crimes occur in the County each year, compared to fewer than 50 officer-involved killings. Furthermore, many police killings were entirely unaccompanied by violent crime. Roughly 20% of incidents involved unarmed individuals, approximately the same share as those involving armed suspects who fired at others. Thus, while underlying neighborhood conditions may lead certain areas to experience more crime or to be more heavily policed, the exact timing and location of officer-involved shootings within those neighborhoods is plausibly exogeneous.

The second factor in support of my empirical strategy is the under-reported nature of police violence. In contrast to the handful of incidents that attracted national attention in recent years, the vast majority of police killings received no media coverage. Thus, spatial proximity is likely to be highly correlated with even learning about the existence of a police killing. This provides meaningful treatment heterogeneity within neighborhoods.

Graphical Evidence

If students are affected by police killings, one might expect to see changes in school attendance in the days following these events. If awareness of police killings is limited to local communities or if the effects are otherwise correlated with geographic proximity (due to social networks, visceral effects of witnessing the incident, etc.), then these changes should dissipate with distance from the incident.

To test this, Figure I examines the raw absenteeism data. Panel A depicts the absenteeism gradient of distance, separately for week before police killings and the week after (including the incident date). Specifically, I estimate local polynomial regressions of daily absenteeism on the distance between a student's home and the incident location. The estimation sample is comprised of the pooled set of observations within two weeks of each incident, where distance and relative time are re-defined within each window.[20]

[Figure I about here.]

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[20]This analysis is restricted to killings from the 2009-2010 school year onward, the period for which daily attendance data is available.

The week prior to a killing, the gradient is relatively flat. That is, attendance patterns for students who lived very close to where the event would occur are quite similar to those who lived farther away. However, in the week after a police killing, absenteeism spikes among nearby students. This uptick is largest for those who lived closest to the incident and fades with distance. The pre- and post-killing gradients converge at around 0.50 miles and are roughly parallel from there outward. These results are quite consistent with Chetty et al. (2018), who find that "a child's immediate surroundings – within about half a mile – are responsible for almost all of the association between children's outcomes and neighborhood characteristics."

Panel B of Figure I then depicts an event study of absenteeism, separately for students who lived nearby (within 0.50 miles) and students who lived farther away (between 0.50 miles and 3.0 miles). I estimate local polynomial regressions of absenteeism (residualized by calendar date) on the number of days before and after each event. In the days leading up a police killing, absenteeism is virtually identical both in level and trend between the two groups. In the immediate aftermath of these events, absenteeism increases sharply among nearby students but remains smooth among those farther away.

Taken together, the two figures highlight the hyperlocal nature of exposure, suggesting that students are affected by police killings that occur within 0.50 miles of their homes, and that students living farther away may serve as a valid control for this group.[21] They also support the exogeneity of police killings. For these changes to be driven by unobserved factors, one would have to believe that those confounds coincided with the exact dates and locations of the police killings. Given that the full sample includes over 600 incidents spread across fifteen years and thousands of square miles, this seems unlikely.

B   Estimating Equation

To estimate effects on my primary measure of student performance – semester GPA – I exploit the same spatial and temporal variation using a flexible difference-in-differences (DD) framework. This model allows me to include individual fixed effects to account for level differences between students as well as neighborhood-time fixed effects to control for unobserved area trends or shocks, which may be of greater concern when examining outcomes that are measured less frequently and over longer time horizons than daily attendance.

Drawing on the graphical evidence, the treatment group is comprised of students who lived within 0.50 miles of any police killing that occurred during their District high school career. On average, this captures 303 students per incident. Roughly 20% of the sample is

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[21] As I will demonstrate in the Section IV, the flatness of the distant gradient also suggests that estimation results are insensitive to the choice of control bandwidth beyond 0.50 miles.

ever-treated based on this definition. The control group consists of students whose nearest police killing during their District tenure was between 0.50 miles and 3 miles away from their home. As I will demonstrate later, estimates are insensitive to alternative definitions of the control group, but increase in magnitude as the treatment bandwidth narrows to students living closest to a killing.

I then estimate the following base equation on the student panel data:

$$(1) \qquad y_{i,t} = \delta_i + \lambda_{n,t} + \omega_{c,t} + \sum_{\tau \neq -1} \beta_\tau Shoot_\tau + \epsilon_{i,t},$$

where $y_{i,t}$ represents semester GPA of individual i at semester t. δ_i are individual fixed effects and $\lambda_{n,t}$ are neighborhood-semester fixed effects. In my primary specification, neighborhood is defined by Census block group, which measure roughly one square mile in area. $\omega_{c,t}$ are cohort-year fixed effects, which account for grade inflation as students progress through high school. $Shoot_\tau$ are relative time to treatment indicators, which are set to 1 for treatment students if time t is τ periods from treatment.[22] For the 15% of treatment students who were exposed to multiple killings during their high school tenure, treatment is defined by the earliest nearby killing.[23] The coefficients of interest (β_τ) then represent the average change between time τ and the last period before treatment among students exposed to police violence relative to that same change over time among unexposed students in the same neighborhood. Drawing on Bertrand et al. (2004), standard errors are clustered by zip code, allowing for correlation of errors over time within each of the sample's 219 zip codes.[24]

Crime and Policing

A primary threat to identification is that unobserved changes in local crime or policing activity may explain both the presence of police shootings and changes in academic performance. However, because I am able to account for time trends at the neighborhood-level, any potential biases would have to be hyperlocal, differentially affecting students in the same Census block group. To test this, I use a block-level analogue of Equation 1 to examine whether Census blocks that experienced police killings also saw differential changes in homicides, crimes or arrests in the prior or following semesters.[25]

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[22]Killings from January to June are mapped to the spring semester, while those from July to December are mapped to the fall semester.

[23]In robustness analysis, I also drop students exposed to multiple killings and find similar results.

[24]As shown in the Appendix, results are robust to different methods of calculating standard errors, such as clustering by school or Census tract and multi-way clustering by zip code and time (Cameron et al., 2012).

[25]While data on homicides is available for the entire sample, information for arrests and non-homicide crimes is only available from 2010 onwards.

These results are shown in Appendix Figure A.I. In each case, I find little evidence of differential trends prior to police shootings. This supports the plausible exogeneity of police killings, after conditioning on block group-time. Following acts of police violence, I also find little evidence of differential changes in crimes or arrests between the streets where those incidents occurred and other areas in the same neighborhood. Point estimates for reported crimes never exceed 0.31 in magnitude, less than 10% of the sample mean (3.16 reported crimes per block-semester). Furthermore, six of the eight post-treatment estimates are negative. Thus, if local crime and student performance are negatively correlated, potential biases would drive treatment estimates for GPA upwards (i.e., towards zero). Similarly, all post-treatment estimates for homicides and arrests are insignificant and more than half are negative in sign.

This does not mean that police violence has no impact on crime. It is possible that the deterrence effects of police shootings are not localized to the specific blocks in which they occur, but are instead distributed throughout an entire precinct or city. These changes would then be absorbed by the neighborhood-time fixed effects in the difference-in-differences model. While a thorough investigation of the relationship between police use of force and crime is outside the scope of this paper, these findings reinforce the exogeneity of police killings and demonstrate that differential shocks in local crime or policing activity are unlikely to bias my treatment estimates.[26]

Selective Migration

Another potential threat is selective migration, as exposure to police violence may cause treated students to relocate or drop out of school. The latter is an outcome of interest in its own right, which I will examine directly in Section VI. Of greater concern are students who relocate within the county while remaining enrolled at the District. Because the data only contains a student's most recent address, students who were exposed to violence at their previous addresses may be incorrectly marked as control, or vice versa.

However, 2006-2010 ACS data suggests that any measurement error is uncorrelated with treatment and would simply bias my estimates towards zero. 86.6% of individuals living in Census block groups where a police shooting occurred reported residing at the same house one year prior, virtually identical to the 86.8% tenure rate among those living in block groups that did not experience a shooting ($p = 0.628$). Even if measurement error was correlated with treatment, the inclusion of student fixed effects would account for any level biases that might arise due to migration – such as if high-achieving students were more likely to re-locate

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[26]As corroboration, results in Section IV show that my primary treatment estimates are robust to directly controlling for homicides, crime and arrests.

following exposure.[27]

IV   Main Results

A   Academic Performance

I first examine the effects of exposure to police killings on academic performance by estimating Equation 1 on semester GPA. The omitted period is the last semester prior to treatment. Estimates are displayed in Figure II.

[Figure II about here.]

Prior to shootings, I find little evidence of differential group trends. For $\tau < 0$, all treatment coefficients are less than 0.012 points in magnitude and never reach statistical significance, even at the 10 percent level. Pre-treatment estimates are also jointly insignificant ($F = 0.69, p = 0.655$). This is consistent with the exogeneity of police killings, which are rare events that are not preceded by observable changes in local crime or policing activity.

Following shootings, grade point average decreases significantly among students living nearby. GPA declines by 0.04 points in the semester of the shooting and by between 0.07 and 0.08 points in the following two semesters (GPA mean=2.08, SD=1). Effects then gradually dissipate, reaching insignificance five semesters after exposure. As I will discuss in Section VI, this does not mean that there are no long-run effects of exposure. If police violence causes affected students to drop out, treatment estimates on semester GPA would mechanically converge to zero as relative time increases.[28]

To place these effects in context, the mean post-treatment estimate of -0.030 SD is larger in absolute magnitude than the average impact of randomized interventions providing student incentives (0.024 SD), low-dosage tutoring (0.015 SD) and school choice/vouchers (0.024 SD) found in the literature (Fryer Jr, 2017). Alternatively, the observed effects predict a roughly 1.5 percentage point decrease in graduation rate, suggesting that changes in achievement may have significant consequences for long-run educational attainment.

Figure III presents results from estimation using alternative definitions of treatment and control groups. In Panel A, I vary the control bandwidth, holding fixed treatment at 0.50

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[27]While this does not rule out the existence of other forms of non-classical measurement error, the data suggests that intra-county migration is unlikely to be a serious confound. In Appendix Figure A.II, I find limited evidence of increased intra-District transfers among schools that experienced police killings in their catchment zones, as would be expected if shootings caused students to move to safer neighborhoods.

[28]Additionally, if affected students are tracked into less rigorous classes, grades could rise even if academic performance or aptitude remains depressed.

miles. Results are highly stable as the control group shrinks from students living within 3 miles of a killing to those living within 1 or 2 miles from an incident. This is consistent with the absenteeism figures, which found relatively flat gradients of distance in student attendance beyond 0.50 miles, and demonstrates robustness to the choice of control group.

[Figure III about here.]

In Panel B, I instead vary the treatment bandwidth, defining exposure at 0.25, 0.375 and 0.50 miles. In all cases, the control group is comprised of students living between 0.50 and 3 miles from an incident. Again, I find little evidence of differential pre-trends and significant decreases in GPA coinciding with exposure to police killings. However, comparing results across models, magnitudes increase monotonically as the treatment bandwidth is tightened. Estimates for the semester after treatment rise from 0.08 points when exposure is defined at one-half mile, to 0.11 points at three-eighths of a mile and 0.16 points at one-quarter mile.

This is again consistent with the absenteeism figures and suggests that students living closest to police killings are most detrimentally affected. In light of the under-reported nature of these events, one explanation for the localized effects may be differences in information. That is, individuals living more than a few blocks from a killing may be completely unaware of its existence. It is also possible that even among students that knew about an incident, those that personally knew the suspect or directly witnessed the violence may be more negatively impacted.

Though I cannot fully disentangle these two channels, Appendix Figure A.III compares average treatment effects for police killings that received media coverage and those that did not. I find nearly identical point estimates in each case, suggesting that more widely-known incidents do not necessarily have larger educational spillovers among local residents. Given that only 15 percent of media-covered incidents were mentioned in more than five newspaper articles, one explanation for the similar effects is that my measure of media coverage is only weakly correlated with information dissemination. However, as I discuss in Section V, effect sizes do increase with the demographic similarity of students and suspects, suggesting that informal networks or personal affiliation may be a more salient mediating channel.

The remainder of Figure A.III contains other heterogeneity analysis. I recover larger treatment estimates for male students as well as for students with less educated parents or lower 8th grade test scores, suggesting that lower-achieving and more disadvantaged students may be most affected by exposure to police killings. It is also possible that these differential impacts are driven in part by racial heterogeneity, which I will explore in detail in Section V.

Robustness

Panel A of Table II demonstrates robustness to a host of alternative specifications. Column 1 presents my preferred specification using a simple post-treatment dummy. To address possible biases due to local crime, Column 2 adds controls for the number of criminal homicides in a Census block-semester. In Column 3, I additionally add time-varying controls for the number of arrests and reported crimes in a block, restricting the sample to 2010 onwards (i.e., the period when crime and arrests data are available). To test robustness to alternative definitions of neighborhood, Column 4 replaces the semester by Census block group fixed effects with semester by Census tract fixed effects (there are roughly 2.6 block groups per tract). Column 5 instead controls for neighborhood time trends using arbitrary square-mile units obtained from dividing the County into a grid. To demonstrate that the effects are not driven by multiply-treated students, Column 6 drops the 15% of treatment students that were exposed to more than one police killing. To address potential differential migration *into* the sample, Column 7 drops students that first entered the District in the 10th to 12th grades. In all cases, I recover similar average treatment effects on student GPA of around -0.20 to -0.30 points.

[Table II about here.]

The Appendix contains additional robustness checks and analysis. Table A.I shows results using alternative calculations of standard errors (i.e., multi-way clustering with zip code and year and clustering by school catchment or tract). In all cases, I recover similar results with insignificant estimates prior to treatment and highly significant estimates in the semesters following police killings. As the paper's primary estimates pool across students exposed at different grades, Figure A.IV replicates the analysis separately for students exposed in the 9th, 10th, 11th and 12th grades and finds that exposure to police violence leads to decreased GPA across each subsample.

To test whether the documented effects are specific to the timing and location of the sample incidents, I run a series of permutation tests. In each regression, I first randomize the location and date of 627 placebo killings within the sample area and period. Treatment and control groups are generated as before and average treatment effects are estimated using Equation 1 and a single post-treatment dummy. Figure A.V presents a histogram of the coefficient of interest for each of 250 tests. The red vertical line benchmarks the estimated coefficient using the true sample. Of the 250 placebo regressions, only four produce estimates greater in absolute value than the true estimate of -0.027 points.

B Psychological Well-Being

I next explore effects on psychological well-being using data on clinical diagnoses of emotional disturbance. Emotional disturbance (ED) is a federally certified disability defined as a "general pervasive mood of unhappiness or depression," "a tendency to develop physical symptoms or fears," or "an inability to learn," which "cannot be explained by intellectual, sensory, or health factors." While there is no single cause of emotional disturbance, its symptomatology and incidence are strongly linked with post-traumatic stress disorder (Mueser and Taub, 2008). Figure IV displays results from estimation of Equation 1 on incidence of ED under my preferred specification.

[Figure IV about here.]

I find little evidence of differential pre-trends between treatment and control students (F-test of joint significance: $F = 1.15, p = 0.334$). However, students exposed to police violence are significantly more likely to be classified as emotionally disturbed in the following semesters. Though the treatment estimates are small, ranging from 0.04 to 0.07 percentage points, they are highly significant and represent a 15% increase over the mean (0.5% of sample students are classified with ED in a given year). As demonstration of robustness, Panel B of Table II shows similar effects under alternative specifications.

Changes in emotional disturbance are also highly persistent with little drop-off several semesters after exposure. This is likely due to two factors. First, emotional disturbance and psychological trauma are chronic conditions and often last for several years after the inciting incident (Friedman et al., 1996; Famularo et al., 1996). Second, ED designations are sticky. While designations are reviewed by the District each year, comprehensive re-evaluations are only required every three years. Thus, the drop-off in effect observed seven semesters after treatment coincides precisely with the timing of triennial re-evaluations for students diagnosed shortly after exposure.

While these results are consistent with the possible traumatizing effects of police violence, they could also be driven by changes in school reporting or detection of ED rather than actual incidence of it. However, as shown in Appendix Table A.II, I find that exposure to police killings also leads to changes in self-reported feelings of safety. In particular, nearby students are twice as likely to report feeling unsafe outside of school the year after a killing. This analysis, which draws on responses from the District's annual survey, suggests that exposure to police violence does impact students' underlying psychological well-being. It also provides causal evidence in support of recent work by Bor et al. (2018), who examine cross-sectional survey data and find that police killings of blacks are linked to lower self-reported mental

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health among black men living in the same state.[29]

Given that students are not regularly screened for ED and designations are only made after an intensive referral process, these estimates likely represent a lower bound of the true psychological impacts of police violence.[30] Epidemiological studies estimate that between 8% and 12% of all adolescents suffer from some form of emotional disturbance (U.S. Department of Education, 1993) — more than fifteen times the diagnosed rate among District students.

The results also provide important insight into the observed effects on academic performance. Consistent with recent work demonstrating that violence affects cortisol levels (Heissel et al., 2018) and that cortisol predicts test performance (Heissel et al., 2018), my findings suggest that decreases in GPA may be driven in part by psychological trauma. However, in addition to maintaining worse grades than their peers (Wagner, 1995), students with ED are 50% less likely to graduate and significantly more likely to suffer from low self-esteem and feelings of worthlessness, suggesting that the long-run effects of police violence may extend beyond in-class performance (Beck et al., 1996; Carter et al., 2006).[31]

V   Mechanisms

To better understand the mechanisms behind these effects, I exploit rich heterogeneity in the data. Given large racial differences in attitudes towards law enforcement as well as significant variation in the police killings themselves, I explore heterogeneous effects by race and incident context. I then directly compare the effects of police use of force to those of criminal homicides.

A   Racial Differences

I first explore differential responses by race. I estimate Equation 1 on GPA, separately for each race subsample. For sake of power, I pool white and Asian students together. Panel A of Figure V displays treatment coefficients for a simple post-treatment dummy.

[Figure V about here.]

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[29]Similarly, work by Moya (2018) and Callen et al. (2014) demonstrates that exposure to violence more generally may lead to changes in risk aversion. Rossin-Slater et al. (2019) find that youth anti-depressant use increases following local school shootings.

[30]Students are only classified as ED after 1) pre-referral interventions have failed, 2) referral to Special Education and 3) a comprehensive meeting between the student's parent, teachers and school psychologist. This process can be quite costly to the District, as students with ED often receive their own classrooms and are sometimes transferred to private schools or residential facilities at the District's expense.

[31]Emotional disturbance is also associated with limited attention spans (McInerney et al., 1992) and impaired cognitive functioning (Yehuda et al., 2004)

As shown, I find stark differences in effects by student race. Black and Hispanic students are significantly affected by police killings and experience average GPA decreases of 0.038 and 0.030 points, respectively. However, exposure to police killings has no impact on white and Asian students with a treatment coefficient of essentially zero (-0.003 points).

One possible explanation for the differing effects by student race is that black and Hispanic students may come from more disadvantaged backgrounds. Given earlier evidence of heterogeneous effects by parental education and 8th grade achievement, those same factors could potentially account for the results found here.

To test this, I create a new sample of black and Hispanic students that matches the distribution of the white and Asian students. I match the former set of students to the latter based on free lunch qualification, parental education (HS degree, less than HS, more than HS), 8th grade standardized test score (by pentile), cohort (within 3 years) and school. To maximize power, I randomly select up to 8 black or Hispanic student per each white or Asian student and weight observations by one over the number of matches to maintain sample balance on match characteristics. Table A.III provides a descriptive comparison of the matched and unmatched samples as well as estimation results for each. Notably, estimated effects for the original minority sample are quite similar to those for the re-weighted minority sample (-0.031 points vs. -0.029 points) and both are far larger than the zero estimate for the white sample. This suggests that differences in family background, prior academic achievement, school and cohort explain very little of the gap in minority and non-minority responses to police killings.

These results provide evidence of the disproportionate burden police violence may have on underrepresented minorities, even conditioning on exposure. This is consistent with work by Gershenson and Hayes (2017), who examine the 2013 Ferguson riots and find that test score decreases were largest in majority-black schools. It is also consistent with a host of research demonstrating that race is the single strongest predictor of perceptions of law enforcement (Taylor et al., 2001). Even controlling for other factors, blacks and Hispanics are significantly more likely to believe that police use of force is excessive or unjustified (Weitzer and Tuch, 2002; Leiber et al., 1998).

A similar pattern emerges when examining heterogeneity by suspect race. As shown in Panel B of Figure V, killings of black and Hispanic suspects have significant spillovers on academic achievement (-0.031 points and -0.021 points, respectively). This is not true of incidents involving white or Asian fatalities.[32] The treatment estimate for killings of whites and Asians is essentially zero (0.003 points).

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[32]Given that Asians comprise only 3% of the police killings sample, I again pool those individuals with whites.

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In interpreting these results it is important to note that suspect race is obviously not randomly assigned. Thus, while police killings of blacks and Hispanics exert demonstrably larger effects than killings of whites and Asians, these differences could be driven by factors correlated with suspect race rather than race itself. For example, it is possible that the former are particularly harmful because they occur in more disadvantaged areas or because the person killed was more likely to have been from the neighborhood or known in the community.

Thus, to better understand the salience of suspect race, I introduce flexible controls allowing for differential treatment effects along a range of neighborhood, incident and suspect characteristics. In particular, I estimate the following equation on the full sample:

(2)
$$y_{i,t} = \delta_i + \lambda_{n,t} + \omega_{c,t} + \beta_{BH} Post \times Shoot \times BlackHispanic + \beta_{WA} Post \times Shoot \times WhiteAsian$$
$$+ Post \times Shoot \times \boldsymbol{X_i} \boldsymbol{\gamma} + \epsilon_{i,t},$$

where $\boldsymbol{X_i}$ is a vector of controls that may be correlated with suspect race. Controls are interacted with post-treatment indicators to absorb variation in treatment effects associated with those factors. The inclusion of these controls means that β_{BH} and β_{WA} no longer represent the average treatment effects of black/Hispanic and white/Asian killings, respectively. Instead, estimated treatment effects are obtained from a linear combination of β_{BH}, β_{WA} and $\boldsymbol{\gamma}$. Nonetheless, the difference between β_{BH} and β_{WA} is informative of the remaining variation in treatment effects attributable to suspect race and provides insight into the relevant counterfactual: all else equal, how would students have responded if the person killed was of a different race?

[Table III about here.]

Table III displays estimated treatment effects from estimation of Equation 2 under various specifications. Column 1 shows results from my base specification without any controls. Consistent with the subsample analysis, I find large and significant estimates for black/Hispanic killings and small, insignificant estimates for white/Asian killings. To account for the possibility that killings in more disadvantaged neighborhoods produce larger spillovers, Column 2 controls for population density, non-white population share, homicide rate and average income in a student's Census block group. Column 3 further accounts for informational differences that may exist between black/Hispanic and white/Asian killings. In particular, I control for whether the incident occurred near the suspect's home and for whether it was mentioned in a local newspaper, as students may be more affected by killings that involved

someone they personally knew or that were more visible.[33] Finally, I control for suspect age and gender in Column 4 to account for the fact that black/Hispanic suspects were younger on average. In each specification, treatment effects for black/Hispanic and white/Asian killings are estimated at the sample median of each of the respective neighborhood, incident and suspect factors.

Comparing across the four specifications, results mirror those found in Figure V with significant, negative treatment effects for black/Hispanic killings of around 0.030 points and insignificant, near-zero estimates for white/Asian killings that never rise above 0.008 points in magnitude. While I cannot reject the null that the two estimates are equal due to a lack of power, their relative magnitudes remain virtually constant across the four models. Thus, other observable contextual factors cannot explain the large disparities in how students respond to killings of whites/Asians and blacks/Hispanics.

Columns 5 through 8 of Table III replicate the analysis restricting the sample to black and Hispanic students. I again recover significant, negative estimates for killings of blacks and Hispanics and insignificant, near-zero estimates for killings of whites and Asians. This suggests that the differential effects by suspect race are not simply mirroring the heterogeneous effects by student race. That is, if (in the extreme case) students were only exposed to own-race killings, higher sensitivity to police violence among black and Hispanic students would mechanically lead to larger average effects for black and Hispanic killings. Instead, my findings suggest a more nuanced story about race-match: conditional on exposure, black and Hispanic students respond differently to police violence depending on the race of the person killed. The Appendix provides additional corroborating evidence by examining the relationship between student-suspect similarity and effect sizes.[34]

Taken together, the results highlight the salience of suspect race in community responses to police violence. Consistent with a host of survey and ethnographic research showing that a majority of Americans believe that police treat minorities less fairly than whites, I find suggestive evidence that police killings of blacks and Hispanics are more damaging than observably similar killings of whites and Asians (Bayley and Mendelsohn, 1969; Dawson

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[33]Because I do not have information on a suspect's exact home address and am unable to link suspects to the anonymized schooling data (i.e., to identify former students), suspect residence was instead inferred from the DA incident reports and is a dummy variable set to one if the report mentioned that the shooting occurred in or directly outside the suspect's residence. Of the 556 incidents with contextual information, 119 were identified as occurring near the suspect's home.

[34]Specifically, Figure A.VI shows that treatment effects move monotonically with the demographic similarity of the person killed. For black and Hispanic students, exposure to police killings of individuals that looked like them (i.e., of the same gender, race and approximate age) leads to large decreases in GPA of nearly 0.10 points, while killings of dissimilar individuals have no negative impact on academic performance. For white and Asian students, however, I find no statistically significant effect in all cases and no clear pattern with respect to suspect similarity.

et al., 1998; Brooks, 1999; Pew Research Center, 2019).

B   Suspect Threat

The incident reports highlight the wide range of circumstances surrounding police use of force, from killings of individuals who actively shot at others to killings of individuals who were completely unarmed. In order to un-bundle these contextual details and explore how responses may depend on the threat posed by the suspect, I estimate heterogeneous effects based on the type of weapon the suspect possessed.

Figure VI compares average treatment effects for police killings of unarmed individuals (17% of the sample) to those for incidents involving individuals armed with a gun (54%) or other weapon (29%). Results come from estimation of a modified version of Equation 2 with separate post-treatment by weapon interactions. The sample is restricted to the 556 incidents for which I was able to obtain contextual details.

[Figure VI about here]

I find significant, negative effects for each type of killing. However, the point estimate for police killings of unarmed individuals (-0.047 points) is roughly twice as large as that for killings of individuals armed with a knife (-0.020) or a gun (-0.024). Differences between the first and last two estimates are statistically significant at the 5 percent level ($p = 0.047$ for unarmed vs. knife killings; $p = 0.050$ for unarmed vs. gun killings). As shown in Column 2 of Table IV, these differences are also largely unattenuated when accounting for differential treatment effects by neighborhood characteristics, media coverage, and suspect demographics and residence. This suggests that other informational and situational factors cannot explain the large disparity in responses to armed and unarmed killings.

To further investigate the salience of suspect threat, I disaggregate killings of gun-wielding suspects by whether the individual fired his weapon. As shown in Columns 3 and 4 of Table IV, the effects for killings of gun-wielding suspects are primarily driven by incidents involving individuals who did *not* fire at others (-0.028 points). Despite comprising a similar share of the sample, treatment estimates for killings of individuals who shot at officers or civilians are 40% smaller and statistically insignificant.

[Table IV about here]

Columns 5 through 8 of Table IV and Panel B of Figure VI replicates the analysis, restricting the sample to incidents involving black and Hispanic fatalities. I again find significantly larger effects for police killings of unarmed individuals (-0.053 points) than for

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killings of individuals armed with guns (-0.020 points). However, across specification, the weapon gradient becomes steeper when restricting to killings of blacks and Hispanics. The difference between treatment estimates for unarmed and gun-armed killings is roughly 50% larger than in the full sample and significant at the 5 percent level in nearly all cases. This is consistent with the fact that blacks and Hispanics suspects were less likely to be unarmed than those of other race groups as well as earlier evidence showing that police killings of whites/Asians have smaller effects than observably similar killings of blacks/Hispanics.

Taken together, the results suggest that the effects of police violence are unlikely to be driven by those incidents with the most gunfire or the deadliest shootouts. If they were, one would expect the largest spillovers to come from killings of suspects who had shot at others. In fact, those events have no statistically significant impact on nearby students. Instead, I find that the most damaging events are police killings of unarmed individuals, those who may have been the least likely to pose a threat to the community or to be engaged in a violent crime at the time of the incident.

In this light, the findings suggest that students may be responding to the perceived reasonableness or legitimacy of officer actions as much as to the use of force itself. Given that virtually all sample killings were legally justified, it is important to note that the differential effects by weapon type are not reflective of differences in the actual legality of police behavior. However, as reflected by nationwide protests over the police killings of Michael Brown and George Floyd, community perceptions of "reasonableness" often depend on contextual factors similar to those assessed here, with police violence against unarmed minorities drawing particular concern (Hall et al., 2016).

C   Comparing Police and Criminal Violence

The previous results suggest that a simple model of violent exposure cannot fully explain the observed effects of police killings on student achievement. However, to further investigate, I directly compare the impacts of police violence to those of other gun-related homicides.

Given the frequency of the latter, I employ a modified event study model to compare the short-run effects of police and criminal gun-related killings.[35] Specifically, I estimate:

$$(3) \qquad y_{i,t} = \delta_i + \lambda_{n,t} + \omega_{c,t} + \sum_{\tau=-3}^{3} \beta_\tau Police_\tau + \sum_{\tau=-3}^{3} \gamma_\tau NonPolice_\tau + \boldsymbol{X_{b,t}}\boldsymbol{\gamma} + \epsilon_{i,t},$$

[35]From 2002 to 2016, the County experienced over 9,000 gun-related homicides. Among the sample's four-year high school students, 80% were exposed to at least one gun-related homicide, with students experiencing an average of 4.5 such incidents during their high school careers.

where $Police_\tau$ and $NonPolice_\tau$ are the number of police and non-police killings that a student was exposed to in semester $t - \tau$. Because exposure to violent crime may be correlated with incidence of other crimes or policing activity, I also include time-varying controls for arrests and reported crimes at the Census block-level, $\boldsymbol{X_{b,t}}$.[36] This model is similar to my main difference-in-differences approach in that it exploits temporal and spatial variation in exposure to violence, accounting for level differences between students and time-varying differences across neighborhoods.

[Figure VII about here]

Results are displayed in Figure VII. I find significant negative effects of violence on student achievement. Exposure to a single criminal homicide leads to decreases in GPA lasting three semesters. This is consistent with a host of recent studies showing that exposure to violent crime is associated with reduced academic performance (Burdick-Will et al., 2011; Burdick-Will, 2013; Sharkey et al., 2014; Gershenson and Tekin, 2017).[37]

However, at its peak, the effect of criminal homicides is only 60% as large as that for police killings. These estimates are statistically distinct from each other at the 5 percent-level for $0 \leq \tau \leq 2$.[38] As shown in Table A.IV, I also find similar relative magnitudes for police and non-police killings when examining daily absenteeism, where the temporal granularity of the data helps to precisely identify the very short-run effects of each event. Combined, the results suggest that the marginal impacts of police killings on education are nearly twice as large as those of criminal homicides.

This does not mean police killings are more damaging than criminal homicides, in aggregate. Given the relative frequency of criminal homicides, the opposite is likely true. It is also possible that the marginal effects for police killings are larger precisely because there are fewer of them, and that prior exposure has inured students to criminal homicides. However, the fact that the marginal effects differ suggests that students may view police killings and criminal homicides as unique phenomena and that different mechanisms might drive their responses to each.

[Table V about here]

To explore this, Table V estimates heterogeneous effects of criminal homicides by race. Columns 1 and 2 first replicate my event study findings using a simplified model examining

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[36]As these data are only available from 2010 onwards, the sample is restricted to that period. Results are similar when excluding the crime controls and including the entire sample period.

[37]While Burdick-Will (2013) finds that violence has little effect on grades, that study and others (Burdick-Will et al., 2011; Sharkey et al., 2014; Gershenson and Tekin, 2017) note a strong negative relationship with student test scores.

[38]That is, comparing $\beta_\tau = \gamma_\tau$ yields $p = 0.032$ at $\tau = 0$, $p = 0.040$ at $\tau = 1$ and $p = 0.007$ at $\tau = 2$.

exposure in the current and prior semester. As before, I find that police killings have a significantly larger impact on GPA (-0.031 points) than criminal homicides (-0.018 points). This difference remains even when including controls for local crimes and arrests.

In Columns 3 and 4, I separate police and criminal killings based on the race of the person killed. Consistent with the racially-disparate effects demonstrated earlier, police killings of blacks and Hispanics have large, negative impacts on student achievement (-0.034 points), while police killings of whites and Asians have no economically or statistically significant effect (-0.004). In contrast, criminal homicides of whites/Asians and blacks/Hispanics are associated with nearly identical decreases in grade point average (-0.016 and -0.018 points, respectively). Columns 5 through 8 demonstrate similar results when restricting the sample to black and Hispanic students. Again, I find larger average impacts for police killings than non-police killings and distinct racial patterns within each type of event. While students are only affected by police killings if they involve black or Hispanic fatalities, they are equally affected by criminal homicides regardless of the race of the person killed.

These findings provide further evidence that student responses to officer-involved killings are not merely a function how much gunfire was present or the fact that someone died. Put differently, police killings do not appear to be simply a more extreme form of violence than criminal homicides.[39] Rather, there exist meaningful qualitative differences in how students respond to these types of events.

VI Long-Run Impacts

A Identification

The estimated effects on academic achievement and mental health suggest that exposure to police killings may have significant long-run ramifications. However, I am unable to estimate Equation 1 when examining educational attainment, as individual fixed effects would fully absorb variation in outcomes, which are measured once per student at the end of their high school careers. Instead, I exploit variation in exposure to police violence between different cohorts of students from the same neighborhood. That is, I compare older students who had already left high school at the time of a killing to younger students who were still in school.

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[39] As further evidence, Table A.V finds that police killings generate larger effects even relative to gang-related homicides, which are more likely to occur in public areas, to involve multiple participants, and to result in bystander fatalities than other criminal homicides (Maxson et al., 1985). Whether a non-police killing was gang-related was determined from incident descriptions from the homicide database. Specifically, if the description contained the words "gang-related" or if either the suspects or the victims were described as having a gang affiliation or suspected gang affiliation, the incident was marked as gang-related.

To understand the relevant sample of observations, first consider a single police killing. Using cross-sectional data, the first difference in a DD model would compare graduation rates of students in expected grades ≤ 12 living nearby (within 0.50 miles) to graduation rates of nearby students in expected grades > 12, where expected grade is determined by the year a student began 9th grade. To account for trends in graduation rates over time, the second difference would capture the between-cohort change in attainment among students who lived farther away from the killing (i.e. between 0.50 and 3 miles).

Extending this logic to multiple killings, I identify the sample of students in expected grades 9 through 16 around each incident and pool these samples together. For students who experienced multiple killings, the same student would appear at each respective grade in the pooled data. However, duplicates are removed such that a given student may only appear once per expected grade. Thus, observations in the final dataset are uniquely identified by student, i, and expected grade, g, with treatment status for observation (i, g) determined by the student's distance to the nearest killing in that expected grade.[40] As an example, consider a student who entered the 9th grade in fall 2007 and experienced a killing 0.20 miles away in fall 2009, a killing 1.5 miles away in fall 2011, and two killings in fall 2013, one 0.20 miles away and one 1.5 miles away. The student would appear three times in the final dataset: at expected grades 11 and 15 as treatment, and at grade 13 as control.[41]

The benefit of this construction is that it enables me to explicitly test for parallel "pre-trends" in the cross-sectional data without otherwise having to condition the sample. This is done by estimating the following event study model on the pooled data:

$$(4) \qquad y_{i,g} = \delta_{n,c} + \sum_{\tau \neq 13} \beta_\tau Shoot_{i,g} \times Grade_\tau + \lambda Shoot_{i,g} + \boldsymbol{X_i}\boldsymbol{\gamma} + \epsilon_{i,g}.$$

Here, $y_{i,g}$ corresponds to the long-run educational attainment of student i of expected grade g. $\delta_{n,c}$ are neighborhood-cohort fixed effects accounting for a changes over time between cohorts in a block group. Because I cannot include individual fixed effects, I instead control for a vector of demographic covariates, $\boldsymbol{X_i}$, including a student's school, race, sex, poverty status, household language, parental education and 8th grade proficiency. To account for level differences in attainment between treatment and control observations, $Shoot_{i,g}$ is an

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[40]In robustness analysis, I restrict the treatment sample to students who were only treated once. Alternatively, I expand the sample to allow students to appear as both treatment and control in the same expected grade. I find similar results in all cases.

[41]This is similar to the framework employed by Cellini et al. (2010), who employ a regression discontinuity design around school bond referenda. Because school districts may have multiple elections in close succession, a single district-time observation is duplicated and appears in both the post-treatment period of one election and the pre-treatment period of a different election.

indicator set to 1 if observation (i, g) is in the treatment group. The coefficients of interest (β_τ) are on the interaction between the treatment indicator and a set of expected grade indicators $Grade_\tau$. As with a standard DD model, they represent the average difference in attainment between students exposed in expected grade g and students exposed in the omitted period (expected grade 13), relative to that same difference among control students. Standard errors are clustered by student to account for dependence arising from the use of multiple i observations in the sample. Results are robust to two-way clustering with cohort and to clustering at the area-level.

B   Educational Attainment

To validate the long-run empirical strategy against the student fixed effects model, I first estimate Equation 4 on final cumulative GPA. The sample is restricted to entering 9th graders with expected graduation dates from spring 2006 to spring 2016 (i.e., those students whose expected 9th to 12th grade years fall entirely within the sample period.) Results are displayed in Panel A of Figure VIII. In reading the figure, note that higher expected grades correspond to older cohorts, whose final GPA was already determined at the time of the killing. Treatment coefficients for these cohorts are near zero and jointly insignificant $(F = 0.72, p = 0.541)$, supporting parallel trends in achievement between older cohorts of students in treatment and control areas.

[Figure VIII about here.]

However, among students in lower expected grades, I find significant differences in long-run achievement associated with exposure to police violence. Notably, the average treatment estimate on cumulative GPA (0.029 points) is nearly identical to the average estimate on semester GPA (0.027 points) from the student fixed effects model in Table II. Though comparing across the two models is not a straightforward exercise, these findings nonetheless provide important validation of the long-run identification strategy, which produces estimates broadly consistent in direction and magnitude with the earlier analysis.

Turning to my primary attainment outcomes, Panel B presents results for high school completion, an indicator set to 1 if the student received a diploma or equivalent from the District. In support of parallel trends, treatment estimates for expected grades > 12 are all insignificant at the 5 percent level. However, students exposed in lower expected grades are significantly less likely to complete high school. Exposure in the 9th grade predicts a 1.7 percentage point decrease in graduation rate. Estimates are similar in magnitude among students exposed in the 10th grade (1.8 p.p.), but decline by roughly half for those in the

11th grade (1.0 p.p.) and approach zero for those exposed in the 12th grade (0.3 p.p.). As mentioned in Section IV, these estimates are in range of those expected from the semester GPA analysis, which predict a roughly 1.5 p.p. decrease in graduation rate.

Panel C examines effects on college enrollment. Similar to Billings et al. (2013), college enrollment is defined as whether a student attended college within the calendar year after their expected high school graduation. The sample is restricted to students in the 2009 to 2014 cohorts (i.e., those for whom NSC data is available). As shown, I find effects qualitatively similar to those for high school completion. Exposure to police violence is associated with significant decreases in college enrollment among 9th and 10th graders of 0.09 percentage points. Estimates then converge to zero for students in higher expected grades.

That effects decrease with expected grade is consistent with work in psychology suggesting that student resilience to neighborhood violence increases with age (Luthar, 1991; Hacker et al., 2006). These dynamics can also be explained more mechanically. As expected grade increases, the share of possible compliers decreases, both because the subset of individuals that remain enrolled shrinks and because the remaining individuals are likely less marginal than earlier dropouts. Nonetheless, the results point to the significant economic impact that police killings can have on younger high school students. The 9th grade treatment estimates correspond to a 3.4% decrease in graduation rate (mean of 50%) and a 2.7% decrease in post-secondary enrollment rate (mean of 32.6%).

[Figure IX about here.]

Figure IX unpacks these effects by student race. For each student race subsample, I estimate a simplified version of Equation 4 replacing the full set of expected grade by treatment interactions with a single post-treatment dummy (i.e., set to 1 for treatment observations in expected grade ≤ 12). Similar to the heterogeneous effects on semester GPA, a stark racial pattern emerges. Across the three outcomes, I find significant, negative effects of police violence on the educational attainment of black and Hispanic students. However, white and Asian students are unaffected by exposure to police killings, with insignificant, near zero estimates in all cases.

Taken together, the results indicate that police killings may have large long-run effects on local communities. This provides causal evidence supporting the link between adverse childhood experiences and educational attainment found in the literature (Harris, 1983; Broberg et al., 2005; Porche et al., 2011).[42] However, police violence differs from many other

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[42]For example, Porche et al. (2011) find that individuals who reported being in a car crash or natural disaster before age 16 were 50% more likely to have dropped out of high school.

forms of trauma in one important dimension. The costs of officer-involved killings are borne entirely by black and Hispanic youth and may serve to exacerbate existing racial disparities in human capital accumulation.

Robustness

Table VI presents a series of robustness checks on the long-run analysis. Column 1 displays my base specification using a single post-treatment dummy. Columns 2 and 3 test alternative bandwidths, restricting the treatment group to students within 0.25 miles and the control group to students between 0.50 and 2 miles, respectively. Columns 4 and 5 replace the cohort by Census block group fixed effects with cohort by Census tract and cohort by square-mile grid units, respectively. Column 6 expands the sample to allow students to appear as both treatment and control in a given expected grade (i.e., if the student lived within 0.50 miles of a killing and between 0.50 and 3 miles of a different killing in that grade). Column 7 instead restricts the sample by excluding students who were treated more than once from expected grades 9 through 16.

[Table VI about here.]

Across specifications and outcomes, I find significant decreases in attainment associated with exposure to police violence. Magnitudes increase modestly when excluding multiple-treaters and when narrowing the treatment bandwidth, consistent with larger effects for closer students. Otherwise, estimates are relatively stable across model, with exposure in expected grades ≤ 12 associated with average decreases in cumulative GPA of roughly 0.03 points, in graduation rate of 1 percentage point and in college enrollment of around 0.6 percentage points.

Appendix Table A.VI demonstrates robustness to alternative calculations of standard errors (i.e., multi-way clustering by student and cohort and clustering by zip code or Census tract). In all cases, treatment coefficients for expected grades ≥ 12 are insignificant, while those for expected grades < 12 are highly significant. The Appendix also provides evidence that the long-run effects are not driven by differential attrition (i.e., students transferring out of the District).[43] In particular, Figure A.VII decomposes the effect on high school graduation by estimating Equation 4 on an indicator for whether a student transferred out of the District and, separately, on an indicator for whether a student dropped out altogether (i.e., did not graduate and did not transfer). The effects on high school completion come

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[43]The reason this may be concern is that I do not observe whether students who transferred out of the District went on to graduate from other school districts.

almost entirely from drop-outs. Treatment estimates for the two are near mirror images. I find no significant effect of exposure to police killings on transfers.[44]

VII   Conclusion

This study provides causal evidence of the deleterious effects of police violence on the academic and psychological well-being of black and Hispanic high school students. The findings suggest that police violence may have important ramifications for racial equity in education. Extrapolating from my estimates suggests that officer-involved killings caused nearly 2,000 black and Hispanic students to drop out of school during the sample period. This does not include any impacts on younger children nor does it consider other costs associated with lost schooling, such as increased crime (Lochner and Moretti, 2004).

These findings point to the particular salience of law enforcement in minority communities. Officer-involved killings are tail events and rarely appear in the media. That they exert lasting effects on schoolchildren points to the potential impact that police may have on the long-term health of neighborhoods, more generally. As the first line of defense and one of the most visible arms of government, law enforcement agencies are a vital part of local communities and may play a critical role in promoting public safety and fostering institutional trust. Better understanding these effects may have important ramifications not only for the design of optimal law enforcement policies, but also for the long-run outcomes of marginalized populations.

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[44]Treatment estimates on graduation in expected grades 9 and 10 are -0.017 and -0.018 points, respectively. Estimates for drop-outs are 0.016 and 0.016 points, while those for transfers are 0.001 and 0.002 points.

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Table I: Summary Statistics

Panel A: Police Killings				*Panel B: Students*				
		Black/	White/				>.5mi.	
	All	Hispanic	Asian		All	≤.5 mi.	Area	Non-Area
Suspect Demographics				*Student Demographics*				
Black	0.26	0.33	0.00	Black	0.12	0.11	0.12	0.12
Hispanic	0.52	0.67	0.00	Hispanic	0.74	0.82	0.80	0.70
White	0.19	0.00	0.83	White	0.08	0.03	0.03	0.10
Asian	0.03	0.00	0.14	Asian	0.06	0.04	0.04	0.08
Male	0.97	0.97	0.96	Male	0.50	0.50	0.49	0.50
Age	32.3	30.6	38.0	Proficient (8th)	0.43	0.40	0.35	0.46
Newspaper Mentions				*Household Characteristics*				
Any	0.22	0.22	0.21	Free lunch	0.69	0.77	0.72	0.66
Total	1.48	1.66	0.88	English lang.	0.29	0.23	0.25	0.32
Median (if any)	2.00	2.00	2.00	College+	0.08	0.06	0.05	0.09
Suspect Weapon								
Unarmed	0.17	0.17	0.20					
Knife	0.29	0.25	0.44					
Gun	0.54	0.58	0.36					
Fired (if gun)	0.41	0.42	0.33					
Incidents	627	486	141	Students	712,954	141,628	133,758	437,568

Notes: Panel A provides summary statistics for the police killings data, separately for killings of minorities (blacks and Hispanics) and killings of individuals of other races (whites and Asians). Unless otherwise noted, mean values reported. Newspaper mentions come from a search of each incident by suspect name in six local newspapers including one nationally-distributed paper. Any is an indicator for whether the incident was mentioned in any article, Total is the number of articles mentioning the incident. Median is the median number of articles in each race category, conditional on being mentioned. Suspect weapon is only available for incidents for which I was able to obtain contextual information from District Attorney reports and other sources (556 out of 627 incidents). Unarmed refers to suspects that did not have a weapon, gun refers to suspects with firearms (including BB guns and replicas), knife refers to suspects with any other type of weapon. Fired is the share of gun-wielding suspects that discharged their weapon.

Panel B provides summary statistics for the student sample, disaggregated by those who lived near/far from a killing during their District tenure. Students whose home address was more than 0.50 miles from a killing are further grouped based on whether they lived in a Census block group where at least one other student in their cohort lived within 0.50 miles of a killing ("Area") or in a Census block group where no other students in their cohort lived within 0.50 miles of a killing ("Non-Area"). Proficient is an indicator for whether the student's average 8th grade state standardized test scores were at a "basic" or higher level of proficiency. Free lunch is an indicator for free/subsidized lunch qualification, English language is an indicator for students from English speaking households, College+ is an indicator for whether a student's parent has a college degree or higher.

Table II: Effects on GPA and Emotional Disturbance

	Base	Alt. Controls		Alt. Neighborhood		Alt. Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: DV = Grade Point Average						
Treat x Post	-0.027***	-0.027***	-0.029***	-0.019***	-0.029***	-0.021***	-0.029***
	(0.006)	(0.006)	(0.010)	(0.005)	(0.007)	(0.006)	(0.007)
Obs.	4,166,188	4,166,188	1,815,131	4,173,300	4,157,829	4,005,642	3,778,162
	Panel B: DV = Emotional Disturbance (per 1,000 students)						
Treat x Post	0.470***	0.470***	0.637***	0.382***	0.428***	0.481***	0.469***
	(0.127)	(0.127)	(0.216)	(0.115)	(0.125)	(0.148)	(0.124)
Obs.	4,029,073	4,029,073	1,876,183	4,029,436	4,028,739	3,867,867	3,768,180
Neighborhood	Blk grp	Blk grp	Blk grp	Tract	Grid	Blk grp	Blk grp
Homicides	-	Y	Y	Y	Y	Y	Y
Crime, Arrests	-	-	Y	-	-	-	-
Exclude	-	-	< 2010	-	-	Multi-treaters	New 10-12 graders

Notes: Table shows DD coefficients and 95 percent confidence intervals from estimation of Equation 1, replacing time to treatment indicators with a post-treatment dummy. Panel A examines non-cumulative, semester GPA. Panel B examines emotional disturbance per 1,000 students. Information on emotional disturbance is only available from the 2003-2004 school year onwards. Column 1 presents my base specification. Column 2 introduces controls for criminal homicides in a block-semester. Column 3 adds controls for the number of crimes and arrests in a block-semester (this information is only available from 2010 onwards). Column 4 controls for neighborhood-semester effects at the Census Tract-level, as opposed to Census block group-level (there are roughly 2.6 block groups per tract). Column 5 instead controls for neighborhood using arbitrary square mile units derived from dividing the County into a grid. Column 6 excludes treatment students that were exposed to multiple police killings. Column 7 excludes students that entered the District in the 10th to 12th grades.

Table III: Effects on GPA by Suspect Race

Avg. Treatment Effect	All Students				Black/Hispanic Students			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black/Hispanic Killing	-0.028***	-0.031***	-0.030***	-0.030***	-0.031***	-0.034***	-0.033***	-0.033***
	(0.007)	(0.007)	(0.006)	(0.006)	(0.008)	(0.007)	(0.007)	(0.007)
White/Asian Killing	-0.005	-0.008	-0.007	-0.007	-0.005	-0.011	-0.010	-0.010
	(0.012)	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.015)	(0.015)
$\beta_{BH} - \beta_{WA}$	-0.023	-0.023	-0.023	-0.023	-0.026	-0.023	-0.023	-0.023
$p(\beta_{BH} = \beta_{WA})$	0.132	0.131	0.131	0.134	0.142	0.184	0.184	0.179
Area Characteristics	-	Y	Y	Y	-	Y	Y	Y
Media, Residence	-	-	Y	Y	-	-	Y	Y
Suspect Demo.	-	-	-	Y	-	-	-	Y
Observations	4,166,168	4,166,168	4,166,168	4,166,168	3,590,169	3,590,169	3,590,169	3,590,169
R-squared	0.695	0.695	0.695	0.695	0.677	0.677	0.677	0.677

Notes: Average treatment effects for minority and white killings from estimation of Equation 2 displayed. Treatment effects computed at sample median of each area, incident and suspect factor. Area characteristics include population density, average income, homicide rate and percent non-white in a student's block group. Media coverage is an indicator for whether the incident was reported in local newspapers (median = 0). Residence is an indicator for whether the incident occurred in or directly outside of the suspect's home (median = 0). Suspect demographics include age (median = 33) and gender (median = male). Left panel examines all students, right panel restricts analysis to black and Hispanic students.

Table IV: Effects on GPA by Suspect Threat

	All Killings				Black/Hispanic Killings			
Avg. Treatment Effect	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unarmed	-0.047***	-0.043***	-0.047***	-0.043***	-0.053***	-0.054***	-0.053***	-0.054***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.014)	(0.014)	(0.014)	(0.014)
Knife	-0.020**	-0.022**	-0.020**	-0.021*	-0.030***	-0.033***	-0.030***	-0.032***
	(0.009)	(0.011)	(0.009)	(0.011)	(0.010)	(0.012)	(0.010)	(0.012)
Gun	-0.024***	-0.023***	-	-	-0.020**	-0.023***	-	-
	(0.007)	(0.006)	-	-	(0.009)	(0.008)	-	-
Gun, not fired			-0.028***	-0.027***			-0.023**	-0.026***
			(0.009)	(0.008)			(0.011)	(0.010)
Gun, fired			-0.017	-0.017			-0.016	-0.018
			(0.012)	(0.012)			(0.013)	(0.013)
$\beta_{none} - \beta_{gun/fired}$	-0.023	-0.020	-0.030	-0.026	-0.033	-0.031	-0.037	-0.036
$p(\beta_{none} = \beta_{gun/fired})$	0.050	0.098	0.056	0.114	0.023	0.025	0.045	0.052
Area/Media/Suspect Ctrls	-	Y	-	Y	-	Y	-	Y
Observations	4,068,357	4,068,357	4,068,357	4,068,357	3,963,677	3,963,677	3,963,677	3,963,677
R-squared	0.694	0.694	0.694	0.694	0.692	0.692	0.692	0.692

Notes: Average treatment effects for killings of unarmed suspects (18%), suspects armed with a weapon other than a gun (29%), and suspects armed with a gun (53%) from estimation of Equation 2 with separate post-treatment by weapon type interactions displayed. Fired/not fired refers to gun-wielding suspects who did/did not shoot at officers or civilians. Treatment effects computed at sample median of each neighborhood, incident and suspect characteristic. Neighborhood characteristics include population density, average income, homicide rate and percent non-white in a student's block group. Media coverage is an indicator for whether the incident was reported in local newspapers (median = 0). Residence is an indicator for whether the incident occurred in or directly outside of the suspect's home (median = 0). Suspect demographics include age (median = 33) and gender (median = male). Left panel includes all killings with contextual information, right panel restricts to killings of blacks and Hispanics.

Table V: Comparing GPA Effects of Police and Criminal Violence

|	All Students				Black/Hispanic Students			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Police Killings								
Any	-0.031***	-0.029***	-	-	-0.033***	-0.031***	-	-
	(0.006)	(0.006)	-	-	(0.006)	(0.006)	-	-
Black/Hispanic			-0.034***	-0.032***			-0.037***	-0.035***
			(0.006)	(0.006)			(0.006)	(0.006)
White/Asian			-0.005	-0.004			0.000	0.002
			(0.015)	(0.015)			(0.016)	(0.016)
Non-Police Killings								
Any	-0.018***	-0.016***	-	-	-0.018***	-0.016***	-	-
	(0.002)	(0.002)	-	-	(0.002)	(0.002)	-	-
Black/Hispanic			-0.018***	-0.016***			-0.018***	-0.016***
			(0.002)	(0.002)			(0.002)	(0.002)
White/Asian			-0.016***	-0.013**			-0.016**	-0.013**
			(0.006)	(0.006)			(0.006)	(0.006)
$p(\beta_P = \beta_N)$	0.030	0.027	-	-	0.012	0.010	-	-
$p(\beta_{P,BH} = \beta_{P,WA})$			0.082	0.088			0.036	0.038
$p(\beta_{N,BH} = \beta_{N,WA})$			0.727	0.631			0.788	0.669
Crime, Arrests	-	Y	-	Y	-	Y	-	Y
Obs.	1,922,635	1,922,635	1,922,635	1,922,635	1,653,541	1,653,541	1,653,541	1,653,541
R-sq.	0.712	0.712	0.712	0.712	0.696	0.696	0.696	0.696

Notes: Coefficients from estimation of modified version of Equation 3 on semester grade point average, replacing the full set of leads and lags with the number of police and non-police killings of each type that occurred within 0.50 miles of a student's home in the current and previous semester. Crime controls include the number of reported crimes and arrests that occurred in the student's Census block in the current and previous semester. Standard errors clustered by zip code. Left panel examines all students, right panel restricts analysis to black and Hispanic students.

Table VI: Effects on Educational Attainment

	Base	Alt. Bandwidth		Alt. Neighborhood		Alt. Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)

Panel A: DV = Cumulative GPA

Treat x Grade≤12	-0.028***	-0.034***	-0.022***	-0.028***	-0.029***	-0.030***	-0.034***
	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Obs.	3,052,158	3,009,826	2,256,623	3,052,310	3,051,204	3,284,564	2,666,509

Panel B: DV = Graduated HS

Treat x Grade≤12	-0.011***	-0.014***	-0.009***	-0.010***	-0.012***	-0.012***	-0.014***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Obs.	3,219,062	3,175,495	2,381,580	3,219,206	3,218,091	3,466,890	2,805,025

Panel C: DV = College Enrollment

Treat x Grade≤12	-0.006***	-0.010***	-0.005***	-0.006***	-0.006***	-0.006***	-0.007***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Obs.	1,826,985	1,801,498	1,354,303	1,827,044	1,826,484	1,963,684	1,588,165

|							
Neighborhood	Blk grp	Blk grp	Blk grp	Tract	Grid	Blk grp	Blk grp
Treatment	< .50 mi	< .25 mi	< .50 mi	< .50 mi	< .50 mi	< .50 mi	< .50 mi
Control	.50-3 mi	.50-3 mi	.50-2 mi	.50-3 mi	.50-3 mi	.50-3 mi	.50-3 mi
Sample	-	-	-	-	-	Allow std-grade duplicates	Exclude multi-treaters

Notes: Coefficients and standard errors from estimation of modified version of Equation 4, replacing the full set of expected grade at treatment interactions with a simple post-treatment dummy set to 1 for treated observations in expected grade ≤ 12. Standard errors clustered by student. Cumulative GPA is a student's final cumulative GPA upon exiting the District. Graduated is an indicator set to 1 if a student received a diploma, GED or special education certificate of completion from the District. College enrollment is an indicator for whether a student enrolled in college within the calendar year after their expected high school graduation date. Transcript data is missing for roughly 5% of students in the school registration data. Results are robust to dropping these students from the graduation and college enrollment analysis. College enrollment data is only available for students in the 2009 to 2014 cohorts. Column 1 presents my base specification. Column 2 restricts the treatment group to students living within 0.25 miles of killing in an expected grade. Column 3 restricts the control group to students living between 0.50 and 2 miles from a killing. Column 4 controls for neighborhood-cohort effects at the Census Tract-level, as opposed to Census block group-level. Column 5 instead controls for neighborhood-cohort using arbitrary square mile units derived from dividing the County into a grid. Column 6 allows (i, g) duplicates if a student was in the treatment group for one killing and the control group for another killing in the same expected grade. Column 7 excludes treatment students who were exposed to multiple killings from expected grades 9 through 16.

Figure I: Effects on Absenteeism
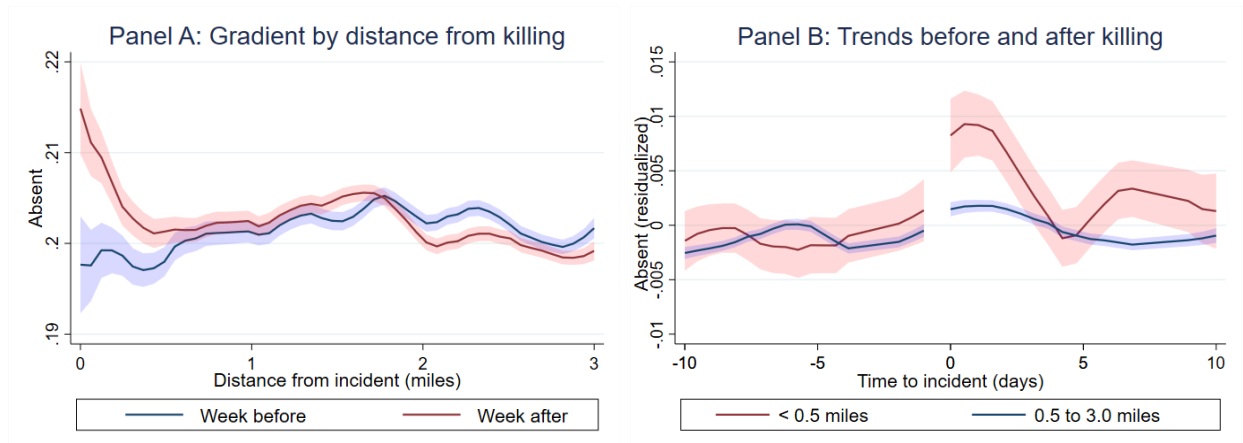
Notes: Panel A depicts local polynomial regressions of daily absenteeism on distance from police killings (bandwidth = 0.075 miles), separately for the week before and the week after (inclusive of the incident date). Panel B depicts local polynomial regressions of daily absenteeism (residualized by calendar date) on days before/after police killings (bandwidth = 1 day), separately for students who lived within 0.5 miles and students who lived between 0.5 and 3 miles of these events. The estimation samples consist of the pooled set of observations within each event window, where distance and relative time are re-defined within each window. Analysis is restricted to killings from the 2009-2010 school year onward, the period for which daily attendance data is available. Per Fan and Gijbels (1996), standard errors are calculated using pilot bandwidths equal to 1.5 times the kernel bandwidths. Shaded areas represent 95% confidence intervals. Absent is a binary indicator for whether a student missed any class on a given day.
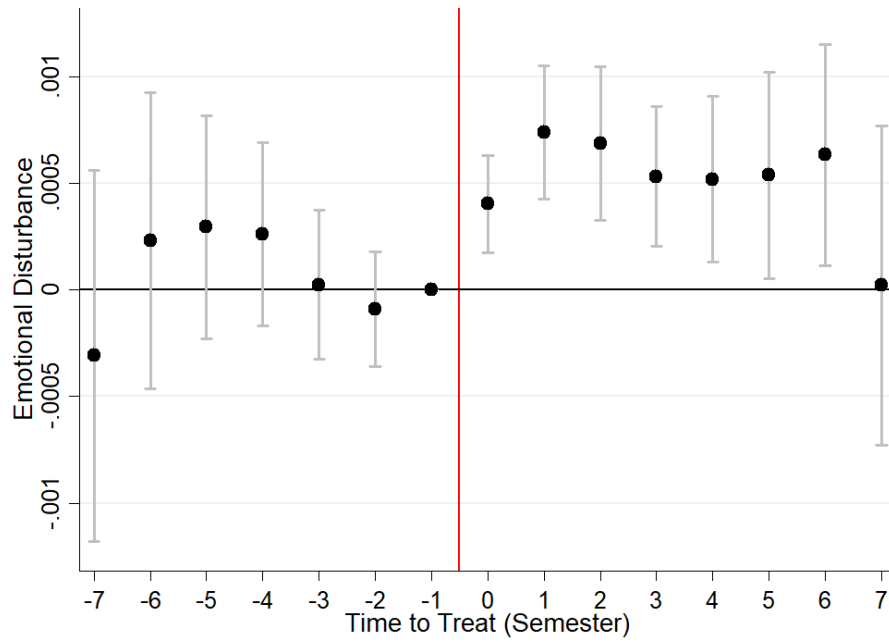
Figure II: Effects on GPA
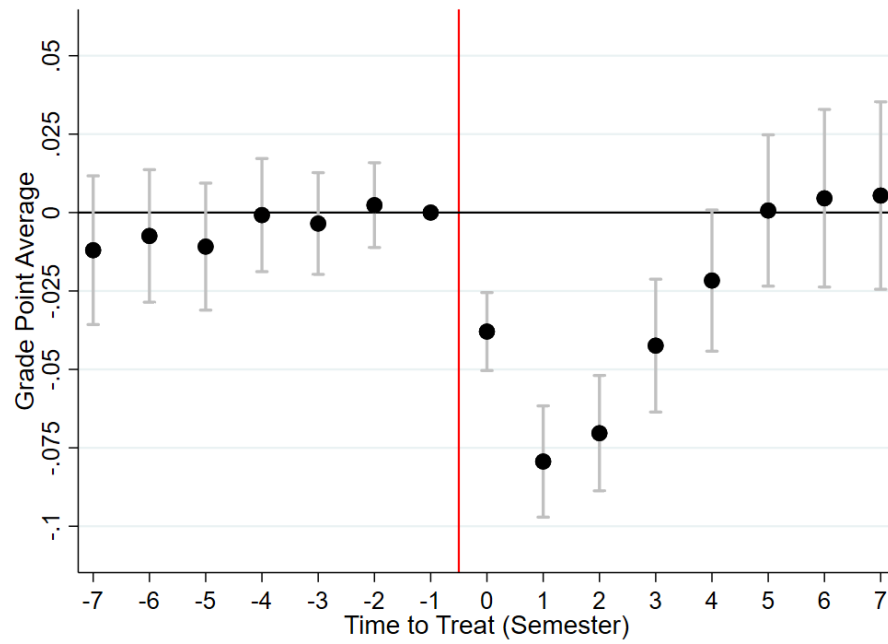
Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 1 on semester grade point average. Standard errors clustered by zip code. Treatment defined as students living within 0.50 miles of an incident. Red vertical line represents time of treatment.

## Figure III: Effects on GPA: Alternative Specifications
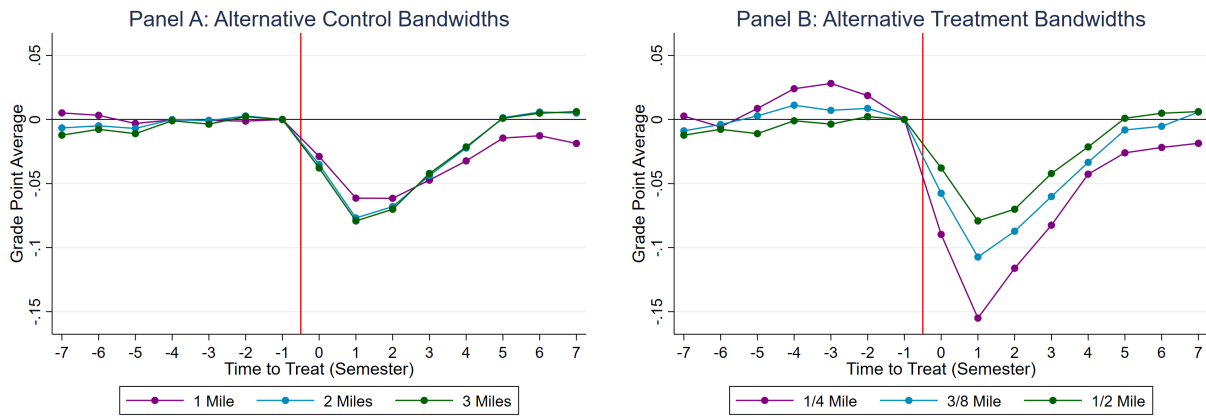
Notes: Graphs show DD coefficients from estimation of Equation 1 on semester grade point average under alternative treatment and control bandwidths. Standard errors clustered by zip code. In Panel A, the control group varies to include students living between 0.50 miles and 1 mile away, between 0.50 miles and 2 miles away and between 0.50 miles and 3 miles away of a killing. In all cases, the treatment group includes students living within 0.50 miles of a killing. In Panel B, the treatment group varies to include students living within 0.25 miles, within 0.375 miles and within 0.50 miles of a killing. In all cases, the control group includes students living between 0.50 and 3 miles of a killing.

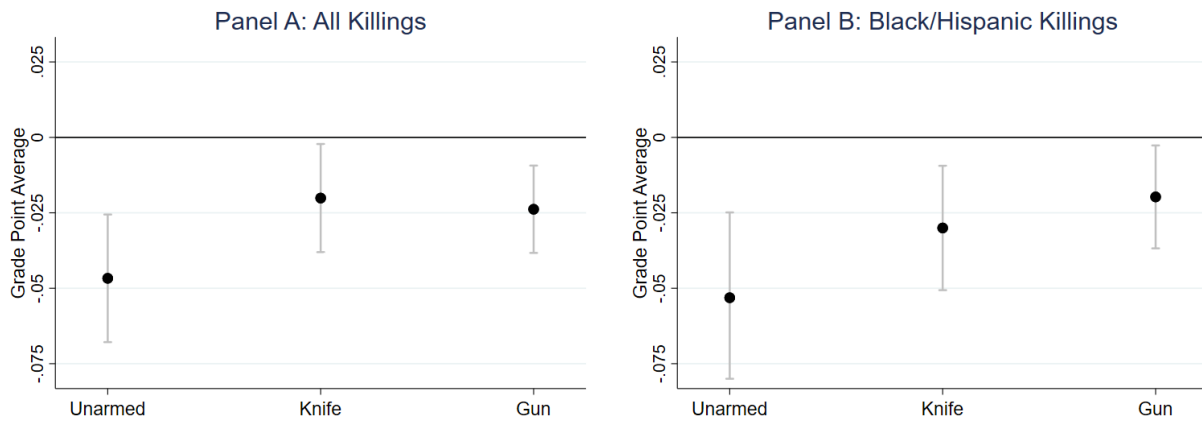Figure IV: Effects on Emotional Disturbance

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 1 on an indicator for emotional disturbance. Standard errors clustered by zip code. Treatment defined as students living within 0.50 miles of an incident. Red vertical line represents time of treatment.
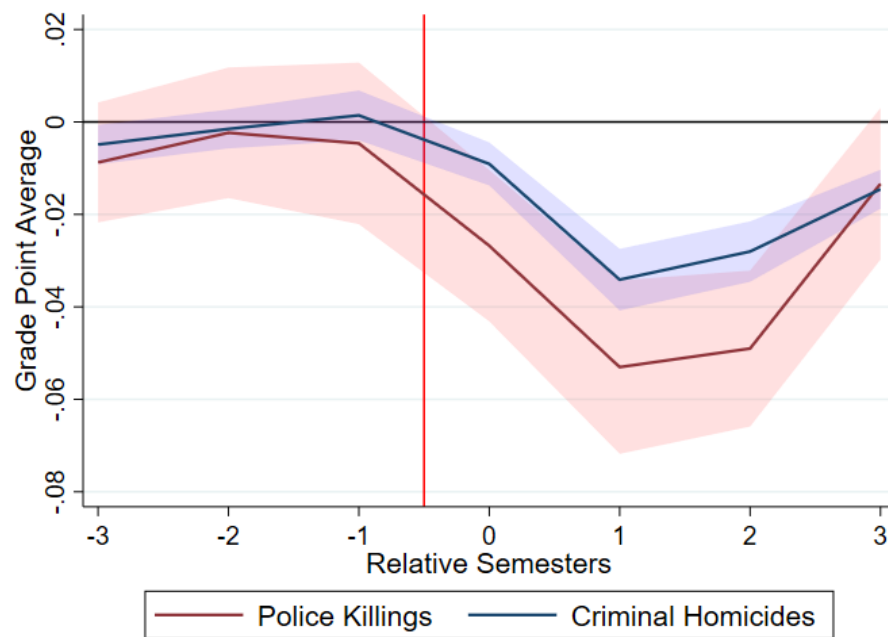
Figure V: Effects on GPA by Race
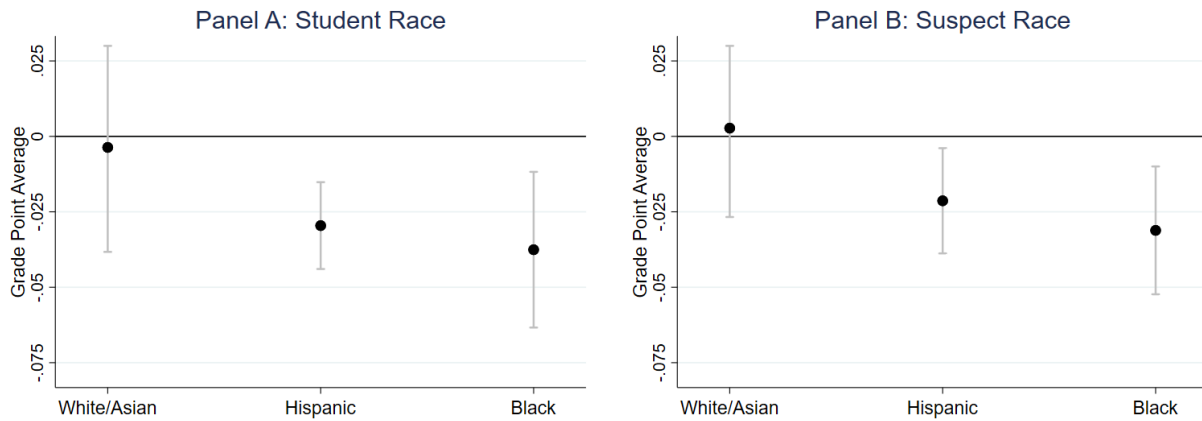
Notes: DD coefficients and 95 percent confidence intervals from estimation of Equation 1 on semester grade point average displayed, replacing time to treatment indicators with a post-treatment dummy. Standard errors clustered by zip code. Panel A estimates effects separately for each student race subsample (i.e, blacks, Hispanics and the pooled sample of whites and Asians). Panel B estimates effects separately for each suspect race subsample.

Figure VI: Effects on GPA by Suspect Weapon

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2 on semester grade point average, replacing the post-treatment by race interactions with post-treatment by weapon interactions. Standard errors clustered by zip code. Treatment defined as students living within 0.50 miles of an incident. Left panel includes all killings with contextual information, right panel restricts to killings of blacks and Hispanics with contextual information. Full estimation results are shown in Table IV, Columns 1 and 5.

Figure VII: Effects on GPA of Police and Criminal Killings

Notes: Graph shows DD coefficients from estimation of Equation 3 on semester grade point average. Standard errors clustered by zip code. Includes time-varying controls for the number of reported crimes and arrests at the block-level. Exposure to police and criminal killings defined as living within 0.50 miles of the incident location. Shaded areas represent 95% confidence intervals.

Figure VIII: Effects on Educational Attainment

Panel A: Cumulative GPA

Panel B: High School Completion

Panel C: College Enrollment
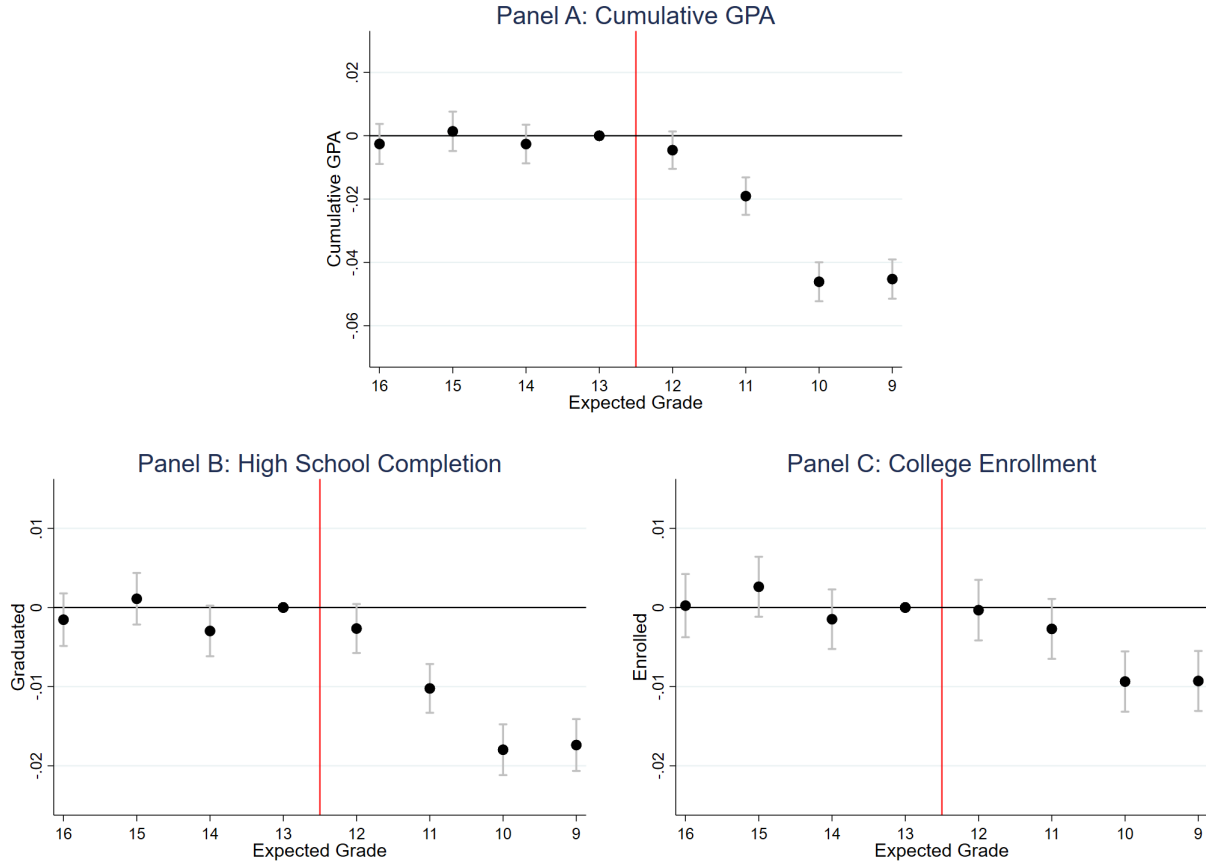
Notes: Figures plot DD coefficients and 95 percent confidence intervals from estimation of Equation 4 on final cumulative GPA, an indicator variable for whether the student completed high school in the District (diploma, GED or special education certificate) and an indicator for whether a student enrolled in a post-secondary degree program within the calendar year after their expected graduation date. Standard errors clustered by student. Includes demographic controls. Treatment defined as students living within 0.50 miles of a killing in a given expected grade, where expected grade is determined by the year students began 9th grade in the District.

Figure IX: Effects on Educational Attainment by Race
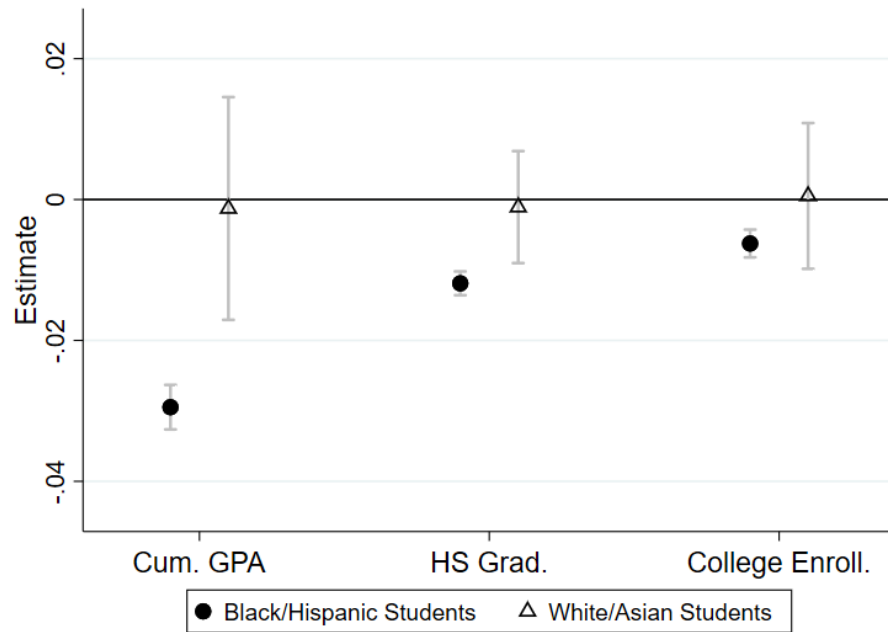
Notes: Figure plots DD coefficients and 95 percent confidence intervals from estimation of modified version of Equation 4, replacing the full set of expected grade at treatment interactions with a simple post-treatment dummy set to 1 for treated observations in expected grade ≤ 12. Standard errors clustered by student. Includes demographic controls. Black circles represent estimation on black and Hispanic students. Triangles represent estimation on white and Asian students.

Appendix A: Supplementary figures and tables noted in text

Table A.I: Effects on GPA: Alternative Standard Errors

Treat x Rel. Time	Coef.	Standard Errors			
		cluster zip (1)	cluster zip, year (2)	cluster catchment (3)	cluster tract (4)
-7	-0.012	(0.012)	(0.012)	(0.015)	(0.011)
-6	-0.008	(0.011)	(0.010)	(0.012)	(0.009)
-5	-0.011	(0.010)	(0.011)	(0.010)	(0.008)
-4	-0.001	(0.009)	(0.009)	(0.010)	(0.007)
-3	-0.004	(0.008)	(0.010)	(0.008)	(0.007)
-2	0.002	(0.007)	(0.009)	(0.007)	(0.006)
-1	-	-	-	-	-
0	-0.038	(0.006)***	(0.008)***	(0.007)***	(0.006)***
1	-0.079	(0.009)***	(0.011)***	(0.010)***	(0.007)***
2	-0.070	(0.009)***	(0.010)***	(0.010)***	(0.008)***
3	-0.042	(0.011)***	(0.015)**	(0.012)***	(0.008)***
4	-0.021	(0.011)*	(0.014)	(0.013)	(0.009)**
5	0.001	(0.012)	(0.014)	(0.012)	(0.010)
6	0.005	(0.014)	(0.018)	(0.013)	(0.011)
7	0.006	(0.015)	(0.020)	(0.013)	(0.013)

Notes: Standard errors calculated with various methodologies in parentheses. Coefficients and zip code-clustered standard errors (shown in Column 1) are derived from main estimation results displayed in Figure II.

Table A.II: Effects on Perceptions of Safety

Question (scale 1-5, higher is safer)	Score (raw)		Score (=1)	
	Mean	Treat x Post	Mean	Treat x Post
How safe do you feel in the neighborhood around the school?	3.68	-0.137** (0.054)	0.038	0.043*** (0.011)
How safe do you feel when you are at school?	3.74	-0.053 (0.056)	0.037	0.015 (0.009)
I feel safe in my school	3.57	-0.048 (0.055)	0.035	0.010 (0.009)
Combined (avg score; min score)	3.66	-0.092** (0.042)	0.075	0.035** (0.015)
Observations		91,358		

Notes: DD coefficients from estimation of Equation 1 on student survey responses, replacing time to treatment indicators with a post-treatment dummy. Standard errors clustered by zip code. Left column examines raw scores for each question, where higher values correspond to feeling more safe. Right column examines an indicator for each question, which is set to 1 if the raw score equaled 1 (least safe). The final row combines all three questions into an average safety score (left column) and an unsafe indicator (right column), based on whether students answered 1 for any of the three questions. Standard errors clustered by zip code. Sample is limited to students in grades 9 through 11 in 2014-2015 academic year who had not been exposed to police violence prior to the first survey wave and treatment is defined as those living within 0.50 miles of a shooting that occurred between the 2015 and 2016 survey administrations. Results robust to including previously treated students.

Table A.III: Matching Minority and Non-Minority Students

Panel A: Summary Statistics			
	White/Asian	Black/Hispanic	
	(Actual)	(Actual)	(Matched)
Household			
Poverty	0.47	0.78	0.43
English	0.54	0.30	0.47
8th Grade Achievement			
Proficient	0.45	0.33	0.45
Avg. Score	372	313	363
Parental Education			
HS+	0.39	0.22	0.40
College+	0.23	0.04	0.17

Panel B. Effects on GPA			
	White/Asian	Black/Hispanic	
	(Actual)	(Actual)	(Matched)
Treat x Post	-0.003	-0.031**	-0.029**
	(0.018)	(0.007)	(0.013)
Obs.	548,315	3,590,169	4,800,724

Notes: Panel A shows summary statistics for the actual sample of minority (i.e., black and Hispanic) and non-minority (i.e., white and Asian) students as well as for the matched sample of minority students. Up to ten minority students are matched to each non-minority based on free lunch status, pentiles of 8th grade standardized test scores, parental education (less than HS, HS, more than HS), cohort (within 3 years) and school. Panel B shows average effects on GPA from estimation of Equation 1 on GPA for each sub-sample. Observations in the matched minority sample are weighted by one over the number of matched minorities to each non-minority to maintain balance on matched characteristics between Columns 1 and 3.

Table A.IV: Effects on Absenteeism of Police and Criminal Killings

Post x Treat x	(1)	(2)	(3)	(4)
Police	0.006**	0.005**	0.007**	0.007*
	(0.003)	(0.003)	(0.003)	(0.004)
Non-Police	0.003***	0.002***	0.003***	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
$\beta_p - \beta_n$	0.003	0.003	0.004	0.005
$p(\beta_p = \beta_n)$	0.244	0.305	0.255	0.269
Sample	All	Restricted	All	Restricted
Neighborhood	Tract	Tract	Blk Group	Blk Group
Obs.	38,762,819	20,337,840	38,694,704	20,311,523
R-sq.	0.257	0.255	0.267	0.265

Notes: DD coefficients from estimation of Equation 1 on absenteeism, replacing replacing time to treatment indicators with interactions between type of violence and a post-treatment dummy. Standard errors clustered by zip code. Treatment defined as students living within 0.50 miles of an incident. Sample includes ten-day windows around each incident, with treatment re-defined in each window. Restricted sample limits the analysis to Census tracts that experienced both police and non-police killings. Neighborhood refers to the geographic level at which semester effects are controlled.

Table A.V: Comparing GPA Effects of Police and Gang-Related Killings

|	All Students				Black/Hispanic Students			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Police Killings								
Any	-0.031***	-0.029***	-0.031***	-0.029***	-0.033***	-0.031***	-0.033***	-0.031***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Non-Police Killings								
Any	-0.018***	-0.016***	-	-	-0.018***	-0.016***	-	-
	(0.002)	(0.002)	-	-	(0.002)	(0.002)	-	-
Gang-Related			-0.020***	-0.018***			-0.020***	-0.018***
			(0.005)	(0.005)			(0.005)	(0.005)
Not Gang-Related			-0.018***	-0.016***			-0.018***	-0.015***
			(0.002)	(0.002)			(0.002)	(0.002)
$p(\beta_P = \beta_N)$	0.030	0.027	-	-	0.012	0.010	-	-
$p(\beta_P = \beta_{N_G})$			0.140	0.133			0.070	0.063
$p(\beta_P = \beta_{N_N})$			0.026	0.022			0.011	0.009
Crime, Arrests	-	Y	-	Y	-	Y	-	Y
Obs.	1,922,635	1,922,635	1,922,635	1,922,635	1,653,541	1,653,541	1,653,541	1,653,541
R-sq.	0.712	0.712	0.712	0.712	0.696	0.696	0.696	0.696

Notes: Coefficients from estimation of modified version of Equation 3 on semester grade point average, replacing the full set of leads and lags with the number of police and non-police killings of each type that occurred within 0.50 miles of a student's home in the current and previous semester. Standard errors clustered by zip code. Crime controls include the number of reported crimes and arrests that occurred in the student's Census block in the current and previous semester. Whether a non-police killing was gang-related was determined from incident descriptions provided by the newspaper database. Specifically, if the description contained the words "gang-related" or if either the suspects or the victims were described as having a gang affiliation or suspected gang affiliation, the incident was marked as gang-related. Left panel examines all students, right panel restricts analysis to black and Hispanic students.

Table A.VI: Effects on Cumulative GPA: Alternative Standard Errors

		Standard Errors			
Treat x		cluster std	cluster std, cohort	cluster zip	cluster tract
Grade	Coef.	(1)	(2)	(3)	(4)
9	-0.045	(0.003)***	(0.004)***	(0.004)***	(0.004)***
10	-0.046	(0.003)***	(0.003)***	(0.005)***	(0.004)***
11	-0.019	(0.003)***	(0.002)***	(0.004)***	(0.003)***
12	-0.005	(0.003)	-0.004	(0.002)*	(0.003)
13	-	-	-	-	-
14	-0.003	(0.003)	(0.004)	(0.003)	(0.003)
15	0.001	(0.003)	(0.003)	(0.003)	(0.003)
16	-0.003	(0.003)	(0.003)	(0.003)	(0.003)

Notes: Standard errors calculated with various methodologies in parentheses. Coefficients and student-clustered standard errors (shown in Column 1) are derived from main estimation results displayed in Panel A of Figure VIII.

Figure A.I: Effects on Crimes, Homicides and Arrests
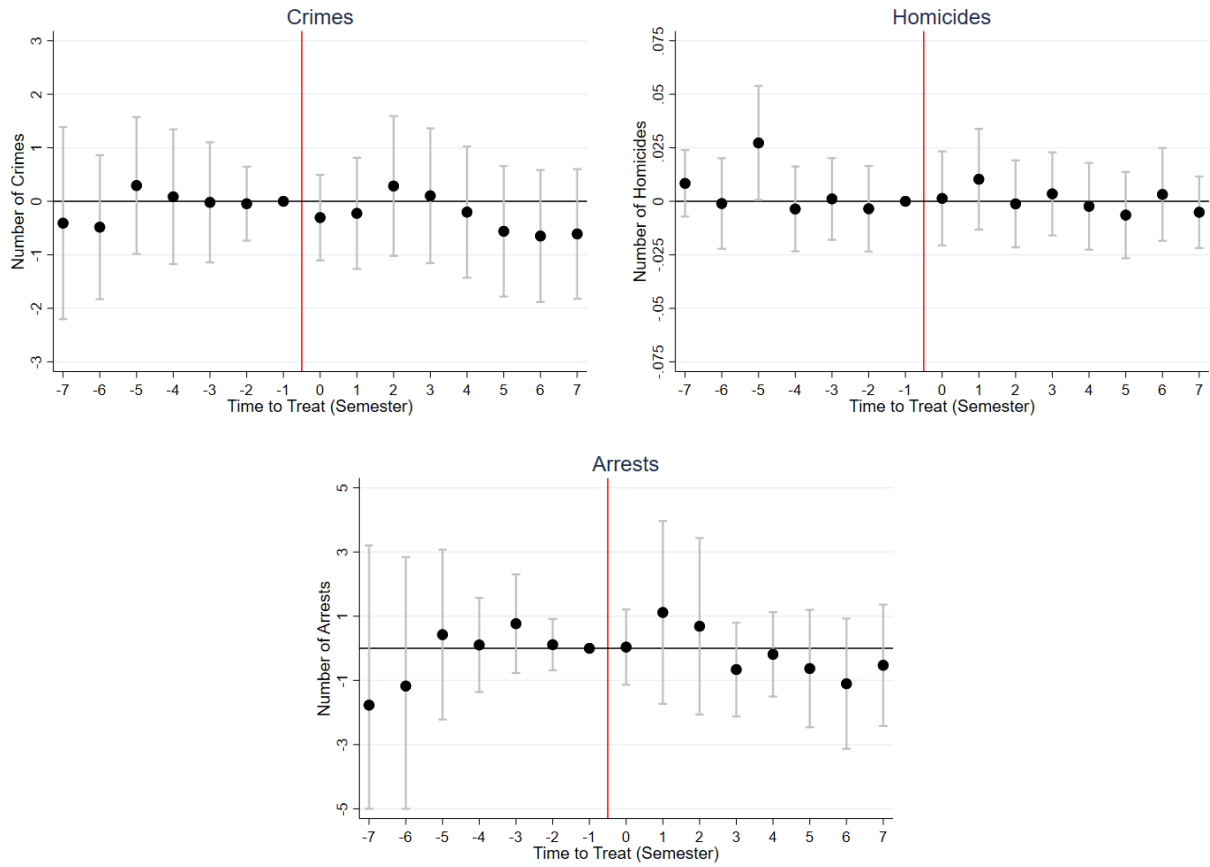
Notes: Graph shows DD coefficients from block-level estimation of Equation 1 on number of reported crimes, homicides and arrests displayed. Unit of observation is the Census block-semester and treatment is defined as blocks that experienced police killings. Standard errors clustered by zip code.

Figure A.II: Effects on Intra-District Transfers
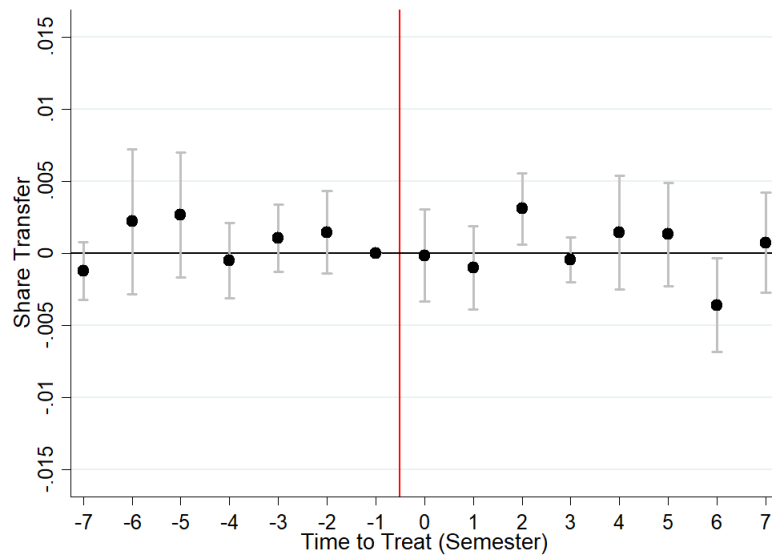
Notes: Graph shows DD coefficients from school-level estimation of Equation 1 on the share of enrolled students that transferred to other District schools in the following semester. Unit of observation is the school and treatment is defined as school catchment areas that experienced police shootings. Includes school board zone-semester fixed effects.
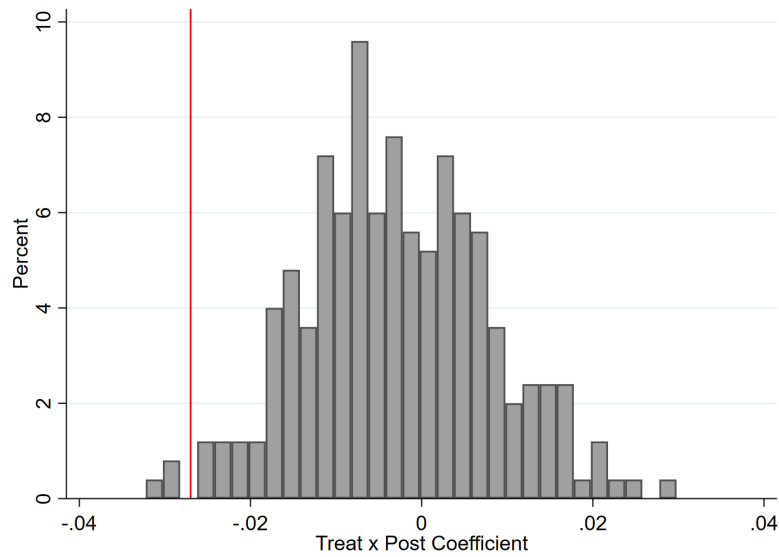
Figure A.III: Effects on GPA: Heterogeneity Analysis
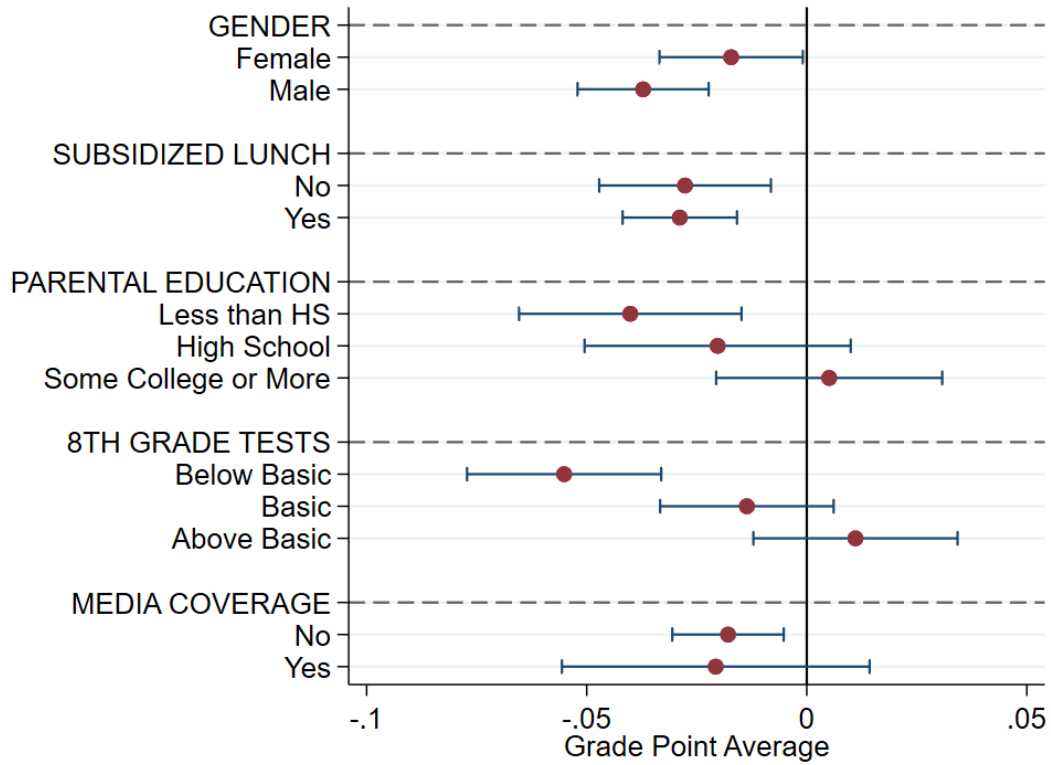
Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 1 on semester grade point average, replacing time to treatment indicators with a post-treatment dummy. Each row corresponds to a separate regression on that particular subsample. 8th grade proficiency is determined by a student's average score on statewide 8th grade standards tests. Scores range from 150 to 600 and, per the state's rubric, are coded as "Below Basic" if less than 300 and "Above Basic" if more than 350. Standard errors clustered by zip code. Treatment defined as students living within 0.50 miles of an incident.

Figure A.IV: Effects on GPA by Grade of Treatment
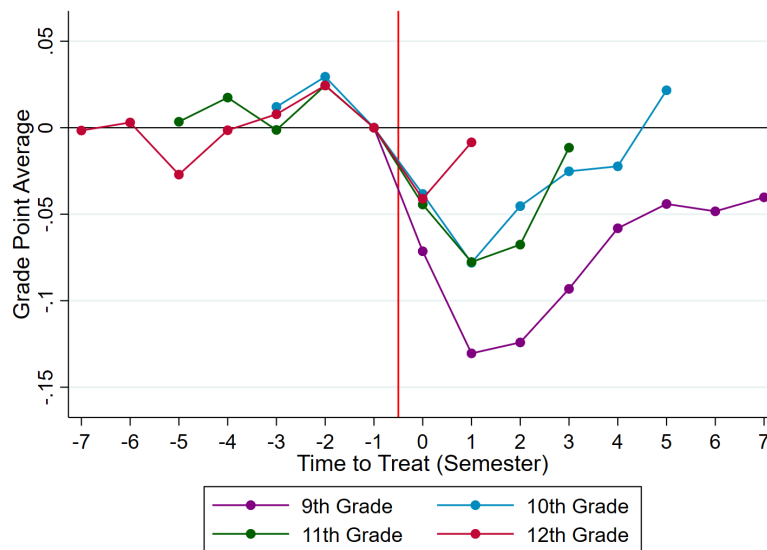
Notes: Graph shows DD coefficients from estimation of Equation 1 on semester grade point average, separately for students who were treated in the 9th grade, 10th grade, and so on. Standard errors clustered by zip code. Red vertical line represents time of treatment. Treatment defined as those living within 0.50 miles of an incident.

Figure A.V: Permutation Tests on GPA

Notes: Figure shows a histogram of the Treat x Post coefficient from estimation of 250 placebo regressions on GPA using a simplified version of Equation 1. In each regression, I randomize the timing and location of 627 placebo shootings and re-define treatment based on proximity to the placebo events. The vertical red line represents the DD coefficient using the true treatment events as reported in Column 1 of Table II.

Figure A.VI: Effects on GPA by Student-Suspect Similarity
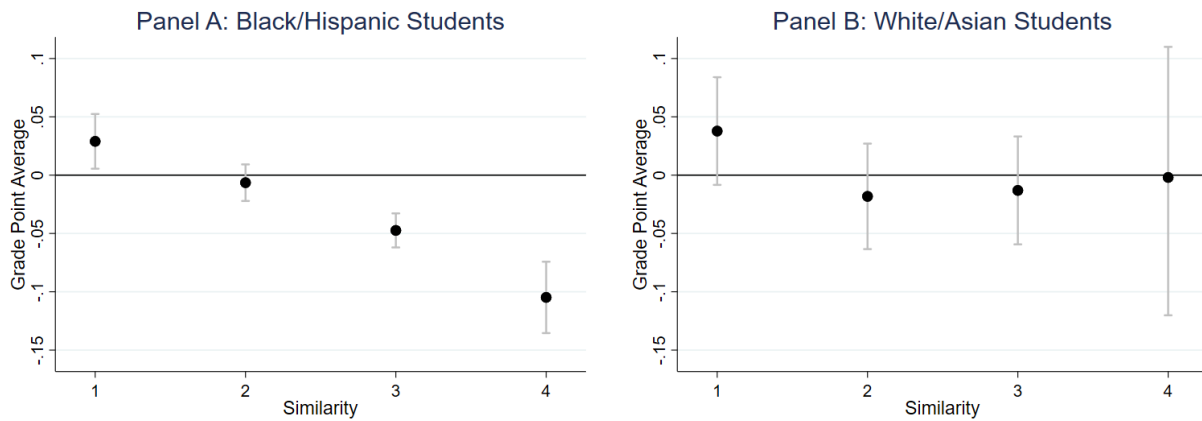
Notes: Figures show DD coefficients and 95 percent confidence intervals from estimation of Equation 1 on semester grade point average, replacing time to treatment indicators with interactions between a student-suspect similarity index and a post-treatment dummy. Similarity increments by 1 if the exposed student and suspect are of the same gender (male or female), ethnicity (black, Hispanic, white or Asian) or age group (suspect was under 25). Panel A restricts analysis to black and Hispanic students. Panel B restricts analysis to white and Asian students.

Figure A.VII: Effects on HS Graduation: Dropouts vs. Transfers
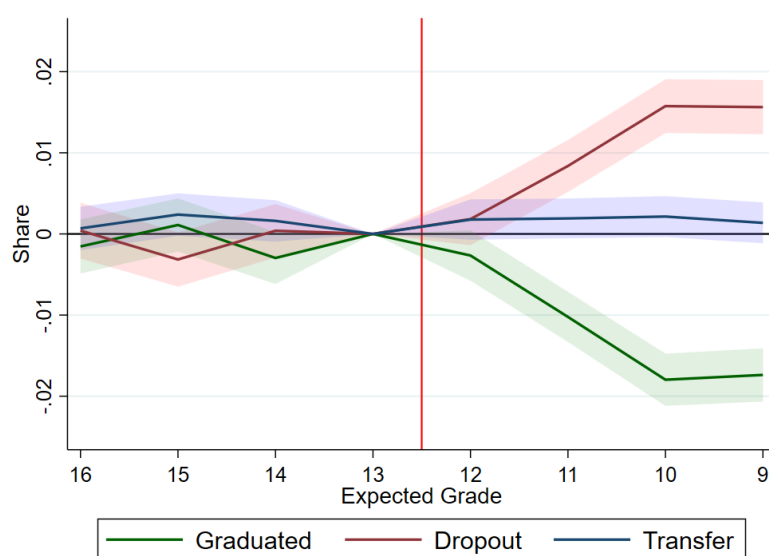
Notes: Graph shows results from estimation of Equation 4 on three separate outcomes: whether a student graduated from the District, whether a student transferred out of the District, and whether a student dropped out (i.e., did not graduate and did not transfer). Standard errors clustered by student. Includes demographic controls. Treatment defined as students living within 0.50 miles of a killing in a given expected grade, where expected grade is determined by the year students began 9th grade at the District. Shaded areas represent 95% confidence intervals.