THE EFFECTS OF POLICE VIOLENCE ON INNER-CITY STUDENTS*

Desmond Ang

Nearly 1,000 officer-involved killings occur each year in the United States. This article documents the large, racially disparate effects of these events on the educational and psychological well-being of Los Angeles public high school students. 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. *JEL* Codes: H75, I24, J15, K42.

I. Introduction

A central role of the state is to ensure public safety. As means of achieving this, U.S. law enforcement officers are afforded broad discretion over the use of force, and roughly 1,000 individuals are killed by police each year. In addition to protecting civilians from imminent harm, these incidents may help deter future criminal activity.

At the same time, the four largest urban riots in recent U.S. history were all triggered by acts of police violence (DiPasquale and Glaeser 1998).[1] Experiences with aggressive

*I gratefully acknowledge financial support from the National Science Foundation. I thank the editor, Lawrence Katz, and anonymous referees as well as Jim Andreoni, Kate Antonovics, Julian Betts, Prashant Bharadwaj, Peter Blair, Michael Callen, Jeffrey Clemens, Gordon Dahl, David Deming, Mitch Downey, Itzik Fadlon, Karthik Muralidharan, Andrei Shleifer, Charlie Sprenger, Uyen Tran, and seminar participants at UC San Diego, UC Riverside, University of Arizona, Dartmouth College, University of Chicago, Harris, University of Illinois-Chicago, Tulane University, the National Bureau of Economic Research, and Harvard University for valuable feedback. I am indebted to Kathy Hayes, Cynthia Lim, Pia Escudero and Joshua Klarin of Los Angeles Unified School District for assistance providing the data and to Michael Garcia for excellent research assistance. Any errors are my own.

1. These include the 1965 Watts riots, the 1980 Miami riots, the 1992 Los Angeles 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.

The Quarterly Journal of Economics (2021), 115–168. doi:10.1093/qje/qjaa027.
Advance Access publication on September 9, 2020.

policing have been linked to unfavorable attitudes toward law enforcement, particularly among racial minorities, whose lifetime odds of being killed by police are as high as 1 in 1,000 (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, Napolitano, and Keating 2007), and institutional distrust (Bobo and Thompson 2006; Kirk and Papachristos 2011).

Nonetheless, there exists little causal evidence of the social effects 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, Papachristos, and Kirk 2016) or the lethal shooting of Michael Brown in Ferguson (Gershenson and Hayes 2018).[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 effects on attitudes or interactions with law enforcement and are unable to shed light on broader economic implications.

This article seeks to document the short- and long-run consequences of police killings on the educational and psychological well-being of inner-city youth. I focus on high school students, because teenagers face crucial educational decisions and

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

3. Similarly, White, Weisburd, and Wire (2018) examine the impact of the Freddie Gray killing on perceptions of procedural justice. The policy implications of those findings are discussed by Lacoe and Stein (2018).

because studies suggest that even vicarious police contact during adolescence may be influential in shaping long-run beliefs and institutional trust (Winfree and Griffith 1977; Leiber, Nalla, and Farnworth 1998; Hurst and Frank 2000; Tyler, Fagan, and Geller 2014).[4]

To estimate these effects, I combine two highly detailed and novel data sets. The first contains incident-level data on the universe of officer-involved killings in Los Angeles County, California, from 2002 to 2016. The second contains home addresses and individual-level panel data for all high school students enrolled in the Los Angeles Unified School District, the second-largest public school system in the nation. By geocoding the exact location of the 627 incidents and over 700,000 home addresses, I can calculate each student's precise geographic proximity to police violence. Leveraging a dynamic difference-in-differences design, I 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 Los Angeles 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

4. Juveniles also experience far more frequent police interactions than do other populations (Snyder, Sickmund, and Poe-Yamagata 1996).

magnitude, effects remain statistically and economically significant for students exposed in the 10th and 11th grades.

These results likely understate the educational effect of police killings for several reasons. First, the article's estimation strategy relies on hyperlocal variation and differences out spillovers beyond 0.50 miles. Although examination of daily attendance data suggests that the immediate effect of police killings is limited to this distance, longer-run impacts may be more geographically diffuse. Second, the article focuses on impacts on high school achievement from exposure to police killings during high school. Thus, it does not capture any effects on younger children exposed in earlier grades or older individuals exposed in postsecondary school. Nonetheless, my estimates provide important insight into the negative consequences of police violence during critical adolescent years.

In unpacking the results, I document stark heterogeneity across race of the student and of the deceased. The effects are driven entirely by black and Hispanic students in response to police killings of other underrepresented minorities. I find no significant effect on white or Asian students. I also find no significant effect for police killings of white or Asian individuals. These differences cannot be explained by other contextual factors correlated with race, such as neighborhood characteristics, media coverage, or other student or deceased 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 people generate negative spillovers that are roughly twice as large as killings of people armed with a gun or other weapon. This difference is statistically significant and unattenuated when accounting for other observable individual, 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

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, the 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, Whyde, and Langton 2018).

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 article 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 Los Angeles caused three students of color to drop out of high school. As fatal shootings make up less than 0.1% of all police use of force encounters (Davis, Whyde, and Langton 2018), this is likely a lower bound of the total social costs of aggressive policing. Although estimating the effects of less extreme uses of force is complicated 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 2019).[6]

Second, this article complements a growing body of research demonstrating how perceived discrimination may lead to "self-fulfilling prophecies" in education (Carlana 2019), labor markets (Glover, Pallais, and Pariente 2017), and health care (Alsan and Wanamaker 2018).[7] While empirical evidence of racial bias is mixed (Nix et al. 2017; Fryer 2019; Johnson et al. 2019; Knox, Lowe, and Mummolo 2020; Knox and Mummolo 2020), the vast majority of blacks and Hispanics in the United States believe that police discriminate in use of force (Pew Research Center 2016, 2019; AP-NORC 2015; Dawson, Brown, and Jackson 2019).[8]

6. As Fryer (2019, 1212) 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.

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

Although more work is needed, the pattern of results suggests that the educational spillovers of officer-involved killings may be driven partly by perceptions of injustice surrounding these events.

Third, this article builds on 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 2018).[9] However, in contrast to other forms of violence, the explicit purpose of law enforcement is to improve public outcomes, and the directional effect of aggressive policing is ex ante far more ambiguous. Thus, my findings serve not just as an exercise in quantifying the costs of violence but as important inputs for pressing policy discussions around police oversight and officer use of force.

Finally, this article provides further insight into the link between neighborhoods and economic mobility (Katz, Kling, and Liebman 2001; Chetty, Hendren, and Katz 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 article is organized as follows: Section II describes the background and data, Section III discusses the identification strategy and provides evidence of its validity, 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

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 2009).

comparing the effects of police killings to criminal homicides, Section VI examines long-run effects on educational attainment, and Section VII concludes. All appendix tables and figures mentioned in the text are included in the Online Appendix.

II. Background and Data

A natural setting for this research is Los Angeles, California. The area bore witness to two of the most high-profile acts of police violence in U.S. history. In 1965, riots erupted in the Watts neighborhood after a physical altercation between California Highway Patrol officers and Marquette Frye, a 21-year-old African American male stopped for reckless driving. The civil unrest lasted for six days and resulted in 34 deaths and more than 3,000 arrests. A similar outbreak occurred in 1992 following the acquittal of Los Angeles Police Department officers involved in the beating of Rodney King, a 26-year-old black man. The resulting riots were the costliest in U.S. history, leading to $1 billion in property damage, 63 deaths, and more than 12,000 arrests.

Today, Los Angeles experiences more police killings than any other county in the nation. From 2002 to 2016, 627 officer-involved fatalities occurred. On a per capita basis, L.A. residents are more than twice as likely to be killed by law enforcement than those living in New York or Chicago (Swaine and Laughland 2015). To investigate the impact of these events, I leverage two novel data sets described below. Additional information about the data is included in the Online Appendix.

II.A. Police Killings Data

Incident-level data on police killings come from the *Los Angeles Times* Homicide Database, 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 and from the newspaper's own investigation. For each incident, the *L.A. Times* records the name, age, and race of the deceased and the exact address and date of the event. In total, the data contain 627 incidents from July 2002 to June 2016.

I supplement this data with contextual details drawn from Los Angeles County district attorney incident reports. Each report includes a detailed description of the event based on forensic and investigative evidence and officer and witness testimonies.

Reports also provide a legal analysis of officer actions. A sample report is included in the Online Appendix. 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 deceased and, if so, what type. In cases where a gun was found, I also captured whether the deceased had fired his weapon at officers or civilians during the police encounter or immediately before (e.g., in cases where police were dispatched for an active shooter).

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, on December 12, 2010, a woman called 911 to report that a man with a gun was sitting in her apartment stairwell. LAPD officers arrived and shot the man, Douglas Zerby, 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, on May 26, 2014, patrol officers attempted to stop Noel Aguilar for riding a bicycle on the sidewalk. Rather than complying, Aguilar 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, Stroshine, and Frank 2001; Braga et al. 2014).

Table I, Panel A provides a summary of the police killings data. Fifty-two percent of deceased individuals 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

10. Race categories are mutually exclusive.

TABLE I
SUMMARY STATISTICS

Panel A: Police killings				Panel B: Students				
	All	Black/ Hispanic	White/ Asian		All	⩽0.5 mi.	>0.5 mi. Area	>0.5 mi. Nonarea
Deceased 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

TABLE I
(CONTINUED)

	Panel A: Police killings			Panel B: Students			
						>0.5 mi.	
	All	Black/ Hispanic	White/ Asian		All	⩽0.5 mi.	Area	Nonarea
	Weapon type							
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 full police killings data and separately for killings of blacks and Hispanics and killings of whites and Asians. Unless otherwise noted, mean values are reported. Newspaper mentions come from a search of each incident by the name of the deceased in six local newspapers (the *Los Angeles Times*, the *Los Angeles Daily News*, *Pasadena Star News*, *San Gabriel Valley Tribune*, *Torrance Daily Breeze*, and *Whittier Daily News*). *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. *Weapon type* is only available for incidents for which I was able to obtain contextual information from District Attorney reports or other sources (556 out of 627 incidents). *Unarmed* refers to killings of individuals who did not have a weapon, *Gun* refers to individuals with firearms (including BB guns and replicas), and *Knife* refers to individuals with any other type of weapon. *Fired (if gun)* is the share of gun-wielding individuals who fired their weapon. Panel B provides summary statistics for the student sample, disaggregated by those who lived near/far from a killing during their LAUSD 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 (*Nonarea*). *Proficient (8th)* is an indicator for whether the student's average eighth-grade California Standards 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.

at death was 32 years. Only 10% of individuals were of school age (i.e., 19 or younger), and none were actively enrolled LAUSD students.

Consistent with national statistics, 54% of those killed 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 individuals who actively fired at officers and civilians (22% of all individuals; 41% of gun-wielding individuals).

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 the *Los Angeles Times*, 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 2 of the 627 incidents generated levels of media coverage anywhere near that of recent nationally reported killings.[12]

Disaggregating these contextual factors by race, I find that black and Hispanic individuals killed by police were younger on average than white and Asian individuals (31 versus 38 years old, respectively) and more likely to possess a firearm (58% versus 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 DA pursued criminal charges against police in only 1 case: the killing of Francisco Garcia in February 2016.[13] This is consistent with national statistics, which find that criminal charges were filed against police in less than 0.5% of all officer-involved shootings.

11. I searched for each incident by the name of the deceased in six local newspapers (the *Los Angeles Times*, the *Los Angeles Daily News*, *Pasadena Star News*, *San Gabriel Valley Tribune*, *Torrance Daily Breeze*, and *Whittier Daily News*). Combined, the papers circulate roughly one million copies each day in the Los Angeles area.

12. The killings of Kendrec McDade on March 24, 2012, and Ezell Ford on August 11, 2014, 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 December 2018, after the end of the sample period.

II.B. Student Data

The LAUSD administrative data contain 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 data set contains 712,954 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 eighth-grade standardized tests. The data also contains each student's last reported home address while enrolled at LAUSD.[14]

The data set includes a host of short- and long-run measures of academic achievement. Outcomes are only observed for grades 9 through 12. Semester GPA 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 noncumulative, semester GPAs. Daily attendance for every student is available from the 2009–2010 school year onward. Each student-date observation contains the number of scheduled classes for which a student was absent that day. This information is used to 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 (GED or CHSPE) or a Special Education Certificate of Completion" from LAUSD.[16] I am unable to distinguish between diploma types. Information on whether students enrolled in postsecondary schooling is available for those who graduated from LAUSD between 2009 and 2014 and

14. Because the data do 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 are 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 data set does not contain information on any years of schooling or diplomas that a student obtained at high schools outside of LAUSD. However, it does contain "leave codes" for students who transferred out of LAUSD before graduating, which allows me to test for differential attrition.

comes from the National Student Clearinghouse, which provides enrollment information for institutions serving over 98% of all postsecondary students in the country.

The data also contain two sources of information regarding student mental health. From the 2004 school year onward, 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 accommodations. These data are 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 the district's annual School Experience Survey (SES) 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]

Table I, Panel B provides summary statistics for the student data. The district is composed primarily of underrepresented minorities. Eighty-six percent 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 less than 10% with college-educated parents. Roughly 40% of students demonstrated basic or higher levels of proficiency on eighth-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 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 constitute 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

17. Responses are answered along a Likert scale ranging from 1 to 5. While the survey is not mandatory, it is typically administered during school hours leading to response rates above 75%.

18. LAUSD demographics differ from those of the county as a whole, which is composed 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.

and household backgrounds as treated students and are slightly less likely to be proficient or to have college-educated parents. This similarity is an important feature of the research design that helps bolster internal validity, particularly when comparing longer-run outcomes.

As a graphical overview of the data, Online Appendix Figure A.I maps LAUSD boundaries as well as the location of every student residence and police killing in the data set. Although Hispanic students are distributed across the county, others are more segregated. Black students reside primarily in urban centers such as central and south Los Angeles, whereas white and Asian students are located in more affluent areas in the northwest. Notably, there are few neighborhoods that never experienced a police killing during the sample period. Furthermore, while more heavily concentrated in certain areas, killings of each race group appear throughout the district.

III. Empirical Strategy

III.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 Huntington Park, where police shootings are relatively prevalent, and students from Northridge, where they are not, could be confounded by correlated neighborhood characteristics. Furthermore, if changes in local poverty, crime, or other unobserved factors predict police killings, biases could remain even when including student fixed effects in panel analysis.

To 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 probably exposed to similar local conditions, except for the killing itself.

The plausibility of this strategy is boosted 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 Los Angeles each year, compared with fewer than 50 officer-involved killings. Furthermore, many police killings were entirely unaccompanied by violent crime. Roughly 20% of incidents involved unarmed people, approximately the same share as those that involved armed individuals 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 likely exogenous.

The second factor in support of my empirical strategy is the underreported 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.

1. 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.), 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 the 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 comprises the pooled set of observations within two weeks of each incident, where distance and relative time are redefined in each window.[20]

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 postkilling gradients converge at around 0.50 miles and are roughly parallel from there outward. These results are quite consistent with Chetty et al. (2018, 28), who find that "a

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

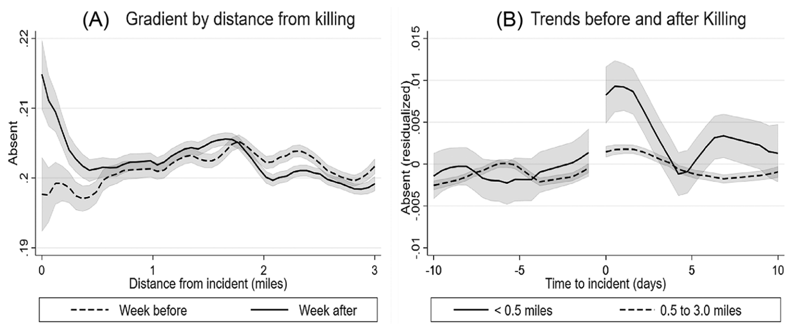

FIGURE I

Effects on Absenteeism

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 sample consists of the pooled set of student-date observations within 10 days and three miles of each police killing, where distance and relative time are redefined within each window. Analysis is restricted to killings from the 2009-2010 academic 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 at least one class on a given day.

child's immediate surroundings—within about half a mile—are responsible for almost all of the association between children's outcomes and neighborhood characteristics."

Figure I, Panel B 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 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 to a police killing, absenteeism is virtually identical in level and trend between the 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 more than 600 incidents spread across 15 years and thousands of square miles, this seems unlikely.

III.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 and 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 composed of students who lived within 0.50 miles of a police killing that occurred during their LAUSD high school career (i.e., grades 9–12). On average, this captures 303 students per incident. Roughly 20% of the sample is ever-treated based on this definition. The control group consists of students whose nearest police killing during their LAUSD high school career was between 0.50 and 3 miles away from their home.[22] As I 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 estimate the following base equation on the student panel data:

$$y_{i,t} = \delta_i + \lambda_{n,t} + \omega_{c,t} + \sum_{\tau=-7}^{7} \beta_\tau Shoot_\tau + \epsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ represents the semester GPA of individual i at semester t. δ_i are individual fixed effects, and $\lambda_{n,t}$ are

21. As I demonstrate in 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.

22. Students who were exposed to a police killing within 3 miles of their home either before the 9th grade or after the 12th grade, but not during high school, are not included in this sample.

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.[23] $Shoot_\tau$ are relative time to treatment indicators, which are set to 1 for treatment students if time t is τ semesters from treatment.[24] Because a typical student would only attend high school for up to eight semesters, τ ranges from -7 to 7 and the omitted period is $\tau = -1$.[25] For the 15% of treatment students who were exposed to multiple killings during high school, treatment is defined by the earliest nearby killing.[26] The coefficients of interest (β_τ) then represent the average change between time τ and the last semester 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, Duflo, and Mullainathan (2004), standard errors are clustered by ZIP code, allowing for correlation of errors over time within each of the sample's 219 ZIP codes.[27]

1. 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.[28]

23. For example, ninth graders in the 2010–2011 school year.

24. Killings from January to June are mapped to the spring semester, and those from July to December are mapped to the fall semester.

25. Students who repeat a grade may appear in more than eight semesters. To account for this, $Shoot_{-7}$ and $Shoot_7$ are set to 1 in cases where $\tau < -7$ and $\tau > 7$, respectively.

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

27. As shown in the Online Appendix, results are robust to different methods of calculating standard errors, such as clustering by school or census tract and multiway clustering by ZIP code and time (Cameron, Gelbach, and Miller 2011).

28. Although data on homicides are available for the entire sample, information for arrests and nonhomicide crimes is only available from 2010 onward.

These results are shown in Online Appendix Figure A.II. 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 posttreatment estimates are negative. Thus, if local crime and student performance are negatively correlated, potential biases would drive treatment estimates for GPA upward (i.e., toward zero). Similarly, all posttreatment 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 article, 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.[29]

2. 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 examine in Section VI. Of greater concern may be students who relocate within the county while remaining enrolled at LAUSD. Because the data only contain 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 suggest that any measurement error is uncorrelated with treatment and would simply bias my estimates toward zero. Eighty-six percent of individuals living

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

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 = .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 relocate following exposure.[30]

IV. Main Results

IV.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.

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% level. Pretreatment estimates are also jointly insignificant ($F = 0.69$, $p = .655$). 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, GPA 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, std. dev. = 1). Effects then gradually dissipate, reaching insignificance five semesters after exposure.

A number of factors may explain the pattern of effects. First, treatment effects may vary within student. That is, exposed students may recover over time or be tracked into less rigorous classes, mitigating the negative GPA effects. Alternatively, as I explore in Section VI, affected students may drop out of school entirely, causing treatment estimates to mechanically converge to

30. Although this does not rule out the existence of other forms of nonclassical measurement error, the data suggest that intracounty migration is unlikely to be a serious confound. In Online Appendix Figure A.III, I find limited evidence of increased intra-LAUSD transfers among schools that experienced police killings in their catchment zones, as would be expected if shootings caused students to move to safer neighborhoods.

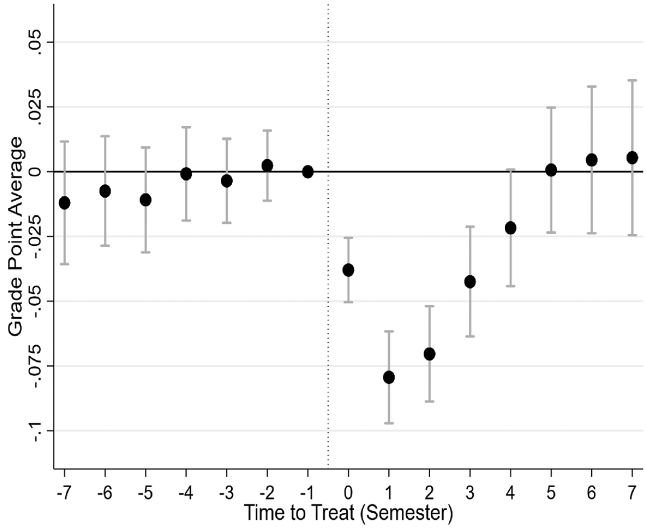

FIGURE II

Effects on GPA

The figure shows DD coefficients and 95% confidence intervals from estimating equation (1) on semester GPA (mean = 2.08). Standard errors are clustered by ZIP code. Treatment is defined as students living within 0.50 miles of a police killing during high school. Control students are those whose nearest killing during high school was between 0.50 and 3 miles away. Sample includes student-semester panel data for students enrolled in LAUSD high schools from the 2002–2003 academic year to the 2015–2016 academic year. The dotted vertical line represents the time of treatment.

zero as relative time increases. Finally, the treatment dynamics could also be driven by heterogeneous effects across grade of treatment. For example, while $\tau = 0$ is estimated across all treatment students, $\tau = 7$ is only identified off of students who were still enrolled seven semesters after treatment (i.e., primarily students exposed in the ninth grade).[31] These compositional differences also help explain the wider confidence intervals at higher absolute values of τ.

To place the treatment estimates in context, the mean post-treatment estimate of -0.030 std. dev. is larger in absolute magnitude than the average impact of randomized interventions providing student incentives (0.024 std. dev.), low-dosage tutoring

31. The Online Appendix includes analysis examining differential effects by grade of treatment.

(0.015 std. dev.), and school choice/vouchers (0.024 std. dev.) found in the literature (Fryer 2017). Alternatively, the observed effects predict a 1.3 percentage point decrease in the graduation rate, suggesting that changes in achievement may have significant consequences for long-run educational attainment.[32]

Online Appendix Figure A.IV 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 miles. Results are highly stable as the control group shrinks from students living within three miles of a killing to those living within one or two 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.

In Panel B, I vary the treatment bandwidth, defining exposure at 0.25, 0.375, and 0.50 miles. In all cases, the control group comprises students living between 0.50 and 3 miles from an incident. Again, I find little evidence of differential pretrends 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 underreported 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 deceased or directly witnessed the violence may be more negatively affected.

Though I cannot fully disentangle these two channels, Online Appendix Figure A.V compares average treatment effects for police killings that received media coverage and those that did not. I

32. To generate this prediction, I estimate naive regressions of high school graduation on semester GPA at grades 9, 10, 11, and 12, controlling for cohort and student demographics. I then multiply the average of these estimates (weighted by the share of students in each grade) by the estimated short-run treatment effect (β_1).

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% 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 deceased, suggesting that informal networks or personal affiliation may be a more salient mediating channel.

The remainder of Online Appendix Figure A.V 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.

2. Robustness. Table II, Panel A demonstrates robustness to a host of alternative specifications. Column (1) presents my preferred specification using a simple posttreatment 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 onward (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 L.A. 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.

The Online Appendix contains additional robustness checks and analysis. Table A.I shows results using alternative

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 × 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 × 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. The table shows results from estimating equation (1), replacing time to treatment indicators with a single posttreatment dummy. Treatment is defined as students living within 0.50 miles of a police killing during high school. Control students are those whose nearest killing during high school was between 0.50 and 3 miles away. Panel A examines noncumulative, semester GPA (mean = 2.08). Sample includes student-semester panel data for students enrolled in LAUSD high schools from the 2002–2003 academic year to the 2015–2016 academic year. Panel B examines emotional disturbance per 1,000 students (mean = 5) and is restricted to the 2003–2004 school year onward, the period for which emotional disturbance information is available. Column (1) presents my base specification controlling for time trends at the census block group level. 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 onward). 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 Los Angeles into a grid. Observation numbers change across alternative neighborhood specifications due to singletons being dropped. Column (6) excludes treatment students who were exposed to multiple police killings. Column (7) excludes students who entered LAUSD in the 10th to 12th grades.

calculations of standard errors (i.e., multiway 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 article's primary estimates pool across students exposed at different grades, Figure A.VI 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 in 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 posttreatment dummy. Online Appendix Figure A.VII 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.

Recent econometric literature has raised concerns about the possibility of negative weights in two-period DD estimators when treatment timing is staggered and there exists heterogeneity in treatment effects within-unit over time or between groups of units treated at different times (Goodman-Bacon 2018; de Chaisemartin and d'Haultfoeuille 2020). The latter case may also contaminate leads and lags in event studies where all treated observations are pooled together across groups (Sun and Abraham 2020). Per Callaway and Sant'Anna (2019), I interrogate this concern by estimating separate event studies for each treatment cohort (defined by the semester a student was first exposed to a police killing). I then average leads and lags across treatment cohort, weighted by the number of students in each treatment cohort-semester. This procedure ensures nonnegative weights and may better illuminate dynamic treatment effects. The results are shown in Online Appendix Figure A.VIII and reveal a similar pattern as my main estimates, with large, negative posttreatment estimates that converge toward zero in later periods.

Finally, I test the sensitivity of my estimates to violations of the parallel trends assumption. Following procedures described by Rambachan and Roth (2019), I compare 95% confidence intervals obtained from my primary DD model against those obtained after allowing for per period deviations from a linear trend of up to an arbitrary amount, M. Online Appendix Figure A.IX displays sensitivity plots under $0 \leqslant M \leqslant 0.02$ for $0 \leqslant \tau \leqslant 3$, the first four posttreatment periods.

IV.B. *Psychological Well-Being*

I 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." Although there is no single cause of emotional disturbance, its symptomatology and incidence are strongly linked with posttraumatic stress disorder (Mueser and Taub 2008). Figure III displays results from estimating equation (1) on incidence of ED under my preferred specification.

I find little evidence of differential pretrends between treatment and control students (F-test of joint significance: $F = 1.15$, $p = .334$). 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 a demonstration of robustness, Table II, Panel B shows similar effects under alternative specifications. Online Appendix Figure A.X displays results after accounting for staggered treatment timing per Callaway and Sant'Anna (2019), and Online Appendix Figure A.XI tests robustness to parallel trends violations per Rambachan and Roth (2019).

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

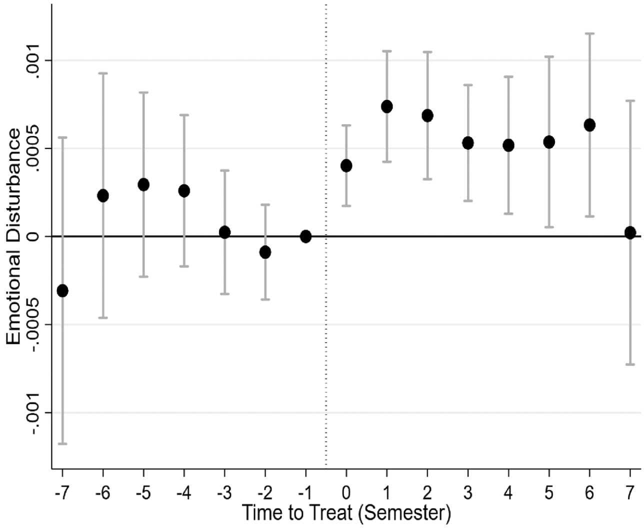

FIGURE III

Effects on Emotional Disturbance

The figure shows DD coefficients and 95% confidence intervals from estimation of equation (1) on an indicator for emotional disturbance (mean = 0.005). Standard errors are clustered by ZIP code. Treatment is defined as students living within 0.50 miles of a police killing during high school. Control students are those whose nearest killing during high school was between 0.50 and 3 miles away. The sample includes student-semester panel data for students enrolled in LAUSD high schools from the 2003–2004 academic year to the 2015–2016 academic year. The dotted vertical line represents the time of treatment.

triennial reevaluations for students diagnosed shortly after exposure.

Although 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, in Online Appendix Table A.II, I show 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 LAUSD's SES, suggests that exposure to police violence does affect 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 health among black men in the same state.[33]

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 effects of police violence.[34] 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 15 times the diagnosed rate among LAUSD 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. forthcoming), 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, Steer, and Brown 1996; Carter et al. 2006).[35]

V. Mechanisms

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

33. 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 antidepressant use increases following local school shootings.

34. Students are only classified as ED after (i) prereferral interventions have failed, (ii) referral to LAUSD Special Education, and (iii) 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.

35. ED is also associated with limited attention spans (McInerney, Kane, and Pelavin 1992) and impaired cognitive functioning (Yehuda et al. 2004).

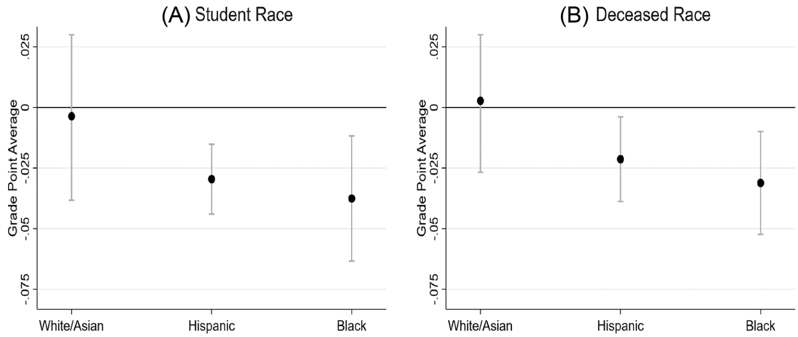

FIGURE IV

Effects on GPA by Race

The figure shows DD coefficients and 95% confidence intervals from estimation of equation (1) on semester GPA (mean = 2.08), replacing time to treatment indicators with a posttreatment dummy. Treatment is defined as students living within 0.50 miles of a police killing during high school. Control students are those whose nearest killing during high school was between 0.50 and 3 miles away. Sample includes student-semester panel data for students enrolled in LAUSD high schools from the 2002–2003 academic year to the 2015–2016 academic year. Standard errors are 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 deceased race subsample.

V.A. Racial Differences

I first explore differential responses by student race. I estimate equation (1) on GPA, separately for each race subsample. For the sake of power, I pool white and Asian students together. Figure IV, Panel A displays treatment coefficients for a simple posttreatment dummy.

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 0 (−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 eighth-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), eighth-grade CST score (by pentile), cohort (within three years), and school. To maximize power, I randomly select up to eight black/Hispanic students for each white/Asian student and weight observations by one over the number of matches to maintain sample balance on match characteristics. Online Appendix Table A.III provides a descriptive comparison of the matched and unmatched samples and estimation results for each. Notably, estimated effects for the original black/Hispanic sample are quite similar to those for the reweighted black/Hispanic sample (−0.031 points versus −0.029 points). This suggests that differences in family background, prior academic achievement, school, and cohort explain very little of the racial gap in 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 (2018), 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 (Leiber, Nalla, and Farnworth 1998; Weitzer and Tuch 2002).

A similar pattern emerges when examining heterogeneity by race of the deceased. As shown in Figure IV, Panel B, killings of black and Hispanic individuals 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.[36] The treatment estimate for killings of whites and Asians is essentially 0 (0.003 points).

In interpreting these results, it is important to note that race is obviously not randomly assigned. Thus, although 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 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

36. Given that Asians make up only 3% of the police killings sample, I again pool those individuals with whites.

was more likely to have been from the neighborhood or known in the community.

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

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

where $\boldsymbol{X_i}$ is a vector of controls that may be correlated with the race of the person killed. Controls are interacted with posttreatment 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 the deceased's 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 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, nonwhite 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 deceased's home and 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.[37] Finally, I control for age and gender of the

37. Because I do not have information on a deceased's exact home address, I am unable to link individuals killed by police to the anonymized schooling data (i.e., to identify former students). Instead, residence was inferred from the DA

TABLE III
EFFECTS ON GPA BY RACE OF THE DECEASED

	All students				Black/Hispanic students			
Avg. treatment effect	(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
$p(\beta_{BH} = \beta_{WA})$	.132	.131	.131	.134	.142	.184	.184	.179
Area characteristics	—	Y	Y	Y	—	Y	Y	Y
Media, residence	—	—	Y	Y	—	—	Y	Y
Deceased 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. The table shows average treatment effects for black/Hispanic and white/Asian killings from estimating equation (2) on semester GPA (mean = 2.08). Treatment defined as students living within 0.50 miles of a police killing during high school. Control students are those whose nearest killing during high school was between 0.50 and 3 miles away. Sample includes student-semester panel data for students enrolled in LAUSD high schools from the 2002–2003 academic year to the 2015–2016 academic year. Treatment effects computed at the sample median of each area, incident, and individual factor. Area characteristics include population density, average income, homicide rate, and percent nonwhite 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 deceased's home (median = 0). Individual demographics include age (median = 33) and gender (median = male) of the deceased. Left side examines all students, right side restricts analysis to black and Hispanic students.

deceased in column (4) to account for the fact that blacks and Hispanics killed by police were younger on average than whites and Asians. 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 individual factors.

Comparing across the four specifications, results mirror those found in Figure IV 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. Although 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.

Table III, columns (5)–(8), 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 deceased 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 Online Appendix provides additional corroborating evidence by examining the relationship between student-deceased similarity and effect sizes.[38]

incident reports and is a dummy variable set to 1 if the report mentioned that the shooting occurred in or directly outside the deceased's home. Of the 556 incidents with contextual information, 119 were identified as occurring near the deceased's home.

38. Specifically, Online Appendix Figure A.XII 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 effect on academic performance. For white and Asian students, however,

Taken together, the results highlight the salience of the race of the person killed 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; Brooks 1999; Pew Research Center 2019).

V.B. Weapon Type

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. To unbundle these contextual details and explore how responses may depend on the threat posed by the person killed, I estimate heterogeneous effects based on the type of weapon the deceased possessed.

Figure V 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 posttreatment by weapon interactions. The sample is restricted to the 556 incidents for which I was able to obtain contextual details.

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% level ($p = .047$ for unarmed versus knife killings; $p = .050$ for unarmed versus gun killings). As shown in Online Appendix Table A.IV, column (2), these differences are also largely unattenuated when accounting for differential treatment effects by neighborhood characteristics, media coverage, and the demographics and residence of the deceased. 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 deceased threat, I disaggregate killings of gun-wielding individuals by whether the

I find no statistically significant effect in all cases and no clear pattern with respect to deceased similarity.

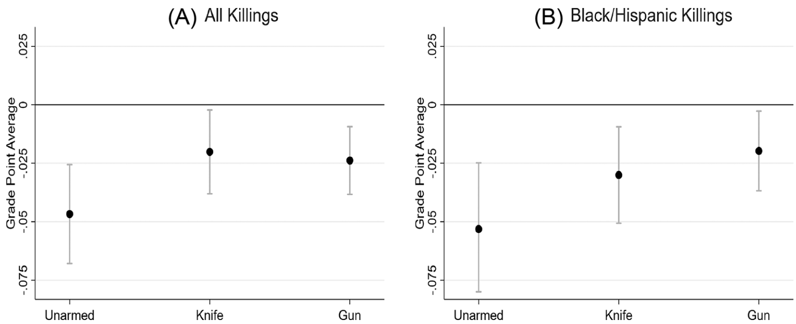

FIGURE V

Effects on GPA by Weapon Type

The figure shows DD coefficients and 95% confidence intervals from estimating equation (2) on semester GPA (mean = 2.08), replacing the posttreatment by race interactions with posttreatment by weapon interactions. Standard errors are clustered by ZIP code. Treatment is defined as students living within 0.50 miles of a police killing during high school. Control students are those whose nearest killing during high school was between 0.50 and 3 miles away. Sample includes student-semester panel data for students enrolled in LAUSD high schools from the 2002–2003 academic year to the 2015–2016 academic year. *Unarmed* refers to killings of individuals who did not possess a weapon. *Gun* refers to killings of individuals who possessed a gun. *Knife* refers to all other killings (i.e., individuals who possessed a weapon other than a gun). Panel A includes all killings with contextual information (556 out of 627 killings). Panel B restricts to killings of blacks and Hispanics with contextual information. Full estimation results are shown in Online Appendix Table A.IV, columns (1) and (5).

person fired his weapon. As shown in Online Appendix A.IV, columns (3) and (4), the effects for killings of gun-wielding individuals are primarily driven by incidents involving individuals who did not fire at others (−0.028 points). Despite taking up a similar share of the sample, treatment estimates for killings of individuals who shot at officers or civilians are 40% smaller and statistically insignificant.

Online Appendix Table A.IV, columns (5)–(8), and Figure V, Panel B, replicate 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 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% level in nearly all cases. This is consistent with the fact that blacks and Hispanics killed by police 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 individuals who shot at others. In fact, those events have no statistically significant effect 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. 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, Hall, and Perry 2016).

V.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.[39] Specifically, I estimate:

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

39. From 2002 to 2016, L.A. County experienced nearly 9,500 gun-related homicides. Among the sample's four-year high school students, 80% were exposed to at least 1 gun-related homicide, with students experiencing an average of 4.5 such incidents during their high school careers.

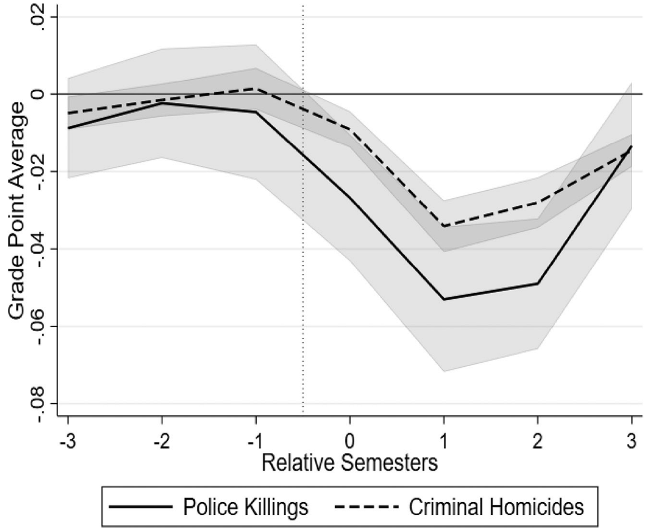

FIGURE VI

Effects on GPA of Police and Criminal Killings

The figure shows DD coefficients and 95% confidence intervals from estimating equation (3) on semester GPA (mean = 2.08). Includes time-varying controls for the number of reported crimes and arrests at the block level. Exposure to police and criminal killings is defined as living within 0.50 miles of the incident location. The sample includes student-semester panel data from the 2010 calendar year onward, the period for which crime data are available. Standard errors are clustered by ZIP code.

where $Police_\tau$ and $NonPolice_\tau$ are the number of police and nonpolice 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, $\boldsymbol{X_{b,t}}$.[40] 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.

Results are displayed in Figure VI. 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

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

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 2018).[41]

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% level for $0 \leqslant \tau \leqslant 2$.[42] As shown in Online Appendix Table A.V, I also find similar relative magnitudes for police and nonpolice killings when examining daily absenteeism, where the temporal granularity of the data helps 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 that 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.

To explore this, Online Appendix Table A.VI estimates heterogeneous effects of criminal homicides by race. In particular, I estimate a simplified model version of equation (3) regressing semester GPA on the number of police and criminal killings of whites/Asians and blacks/Hispanics a student was exposed to in the current and prior semester. Consistent with the racially disparate effects demonstrated earlier, police killings of blacks and Hispanics have large, negative effects 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). I find similar effects after restricting the sample to black and Hispanic students with

41. 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 2018) note a strong negative relationship with student test scores.

42. That is, comparing $\beta_\tau = \gamma_\tau$ yields $p = .032$ at $\tau = 0$, $p = .040$ at $\tau = 1$, and $p = .007$ at $\tau = 2$.

larger average impacts for police killings than nonpolice killings and distinct racial patterns within each type of event. Although 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 of how much gunfire was present or the fact that someone died. Put differently, police killings are not simply a more extreme form of violence than criminal homicides.[43] Rather, there exist meaningful qualitative differences in how students respond to these types of events.

VI. Long-Run Impacts

VI.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, because 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.

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 $\leqslant 12$ 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

43. As further evidence, Online Appendix Table A.VII 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, Gordon, and Klein 1985). Whether a nonpolice killing was gang-related was determined from incident descriptions provided by the *Los Angeles Times* 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.

began 9th grade at LAUSD. 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 data set 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.[44] As an example, consider a student who entered the ninth 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 data set: at expected grades 11 and 15 as treatment, and at grade 13 as control.[45]

The benefit of this construction is that it enables me to explicitly test for parallel "pretrends" 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:

$$(4)\quad y_{i,g} = \delta_{n,c} + \sum_{\tau=9}^{16} \beta_\tau Shoot_{i,g} \times Grade_\tau + \lambda Shoot_{i,g} + \boldsymbol{X_i \gamma} + \epsilon_{i,g}.$$

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 changes over time between cohorts in a block group. Because I cannot include individual fixed effects, I instead

44. 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.

45. This is similar to the framework employed by Cellini, Ferreira, and Rothstein (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 posttreatment period of one election and the pretreatment period of a different election.

control for a vector of demographic covariates, $\boldsymbol{X_i}$, including a student's school, race, sex, poverty status, household language, parental education, and eighth-grade proficiency. To account for level differences in attainment between treatment and control observations, $Shoot_{i,g}$ is an 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.

VI.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 Figure VII, Panel A. 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 ($F = 0.72$, $p = .541$), supporting parallel trends in achievement between older cohorts of students in treatment and control areas.

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

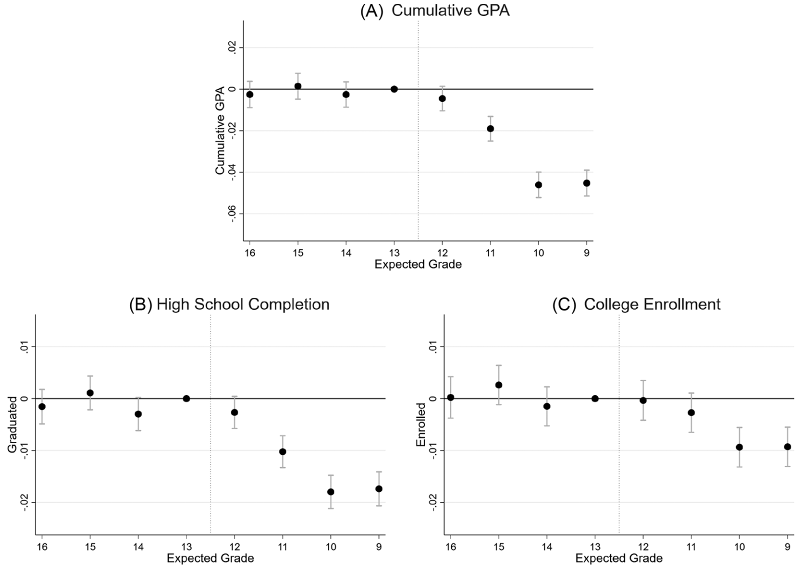

FIGURE VII

Effects on Educational Attainment

The figures show DD coefficients and 95% confidence intervals from estimation of equation (4) on final cumulative GPA (mean = 1.89), an indicator variable for whether the student received a high school diploma or equivalent from LAUSD (mean = 0.50) and an indicator for whether a student enrolled in a postsecondary degree program within the calendar year after their expected graduation date (mean = 0.33). The sample contains the pooled cross-section of students living within three miles of a killing from expected grades 9 through 16, where expected grade is determined by the year students began 9th grade at LAUSD. Student i may appear at multiple expected grades g, but only once per expected grade, with treatment status for observation (i, g) determined by the nearest killing in that grade (i.e., $\leqslant$0.50 miles). The sample is limited to entering ninth graders with expected graduation dates between 2006 to 2016 for the GPA and HS graduation analysis and to those with expected graduation dates between 2009 and 2014 for the college enrollment analysis. Includes demographic controls for a student's school, race, sex, free lunch status, household language, parental education, and eighth-grade proficiency. Standard errors are clustered by student.

1 if the student received a diploma or equivalent from LAUSD. In support of parallel trends, treatment estimates for expected grades $>$12 are all insignificant at the 5% 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 the graduation rate. Estimates are similar in magnitude among students exposed in the 10th grade

(1.8 percentage points), but decline by roughly half for those in the 11th grade (1.0 percentage point) and approach 0 for those exposed in the 12th grade (0.3 percentage points). As mentioned in Section IV, these estimates are in range of those expected from the semester GPA analysis, which predict a roughly 1.5 percentage point decrease in graduation rate.

Panel C examines effects on college enrollment. Similar to Billings, Deming, and Rockoff (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 are 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 0 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 ninth-grade treatment estimates correspond to a 3.4% decrease in graduation rate (mean of 50%) and a 2.7% decrease in postsecondary enrollment rate (mean of 32.6%).

Figure VIII 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 posttreatment dummy (i.e., set to 1 for treatment observations in expected grade $\leqslant$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, I find no significant impact for white and Asian students, with point estimates near zero 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

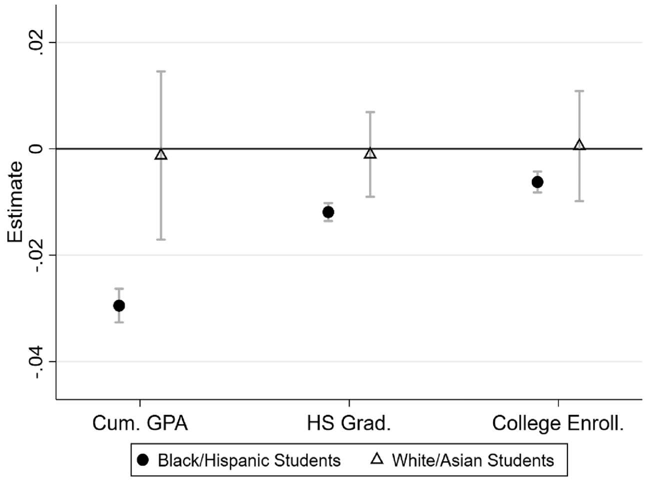

FIGURE VIII

Effects on Educational Attainment by Race

The figure shows DD coefficients and 95% confidence intervals from estimation of a modified version of equation (4), replacing the full set of expected grade at treatment interactions with a simple posttreatment dummy set to 1 for treated observations in expected grade $\leqslant$12. Outcomes are final cumulative GPA (mean = 1.89), an indicator variable for whether the student received a high school diploma or equivalent from LAUSD (mean = 0.50) and an indicator for whether the student enrolled in a postsecondary degree program within the calendar year after their expected graduation date (mean = 0.33). The sample contains the pooled cross-section of students living within three miles of a killing from expected grades 9 through 16, where expected grade is determined by the year students began 9th grade at LAUSD. Student i may appear at multiple expected grades g, but only once per expected grade, with treatment status for observation (i, g) determined by the nearest killing in that grade (i.e., $\leqslant$0.50 miles). The sample is limited to entering ninth graders with expected graduation dates between 2006 to 2016 for the GPA and HS graduation analysis and to students with expected graduation dates between 2009 and 2014 for the college enrollment analysis. Includes demographic controls for a student's school, race, sex, free lunch status, household language, parental education, and eighth-grade proficiency. Standard errors are clustered by student. Black circles represent estimation on black and Hispanic students. Triangles represent estimation on white and Asian students.

(Harris 1983; Broberg, Dyregrov, and Lilled 2005; Porche et al. 2011).[46] However, police violence differs from many other forms of trauma in one important dimension. The costs of officer-involved killings are borne entirely by black and Hispanic youth

46. 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.

and may exacerbate existing racial disparities in human capital accumulation.

1. Robustness. Table IV presents a series of robustness checks on the long-run analysis. Column (1) displays my base specification using a single posttreatment 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.

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 models, with exposure in expected grades $\leqslant$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.

Online Appendix Table A.VIII demonstrates robustness to alternative calculations of standard errors (i.e., multiway clustering by student and cohort and clustering by ZIP code or census tract). In all cases, treatment coefficients for expected grades $\geqslant$12 are insignificant, while those for expected grades <12 are highly significant. The Online Appendix also provides evidence that the long-run effects are not driven by differential attrition (i.e., students transferring out of LAUSD).[47] In particular, Online Appendix Figure A.XIII decomposes the effect on high school graduation by estimating equation (4) on an indicator for whether a student transferred out of LAUSD 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

47. The reason this may be a concern is that I do not observe whether students who transferred out of LAUSD went on to graduate from other school districts.

TABLE IV

EFFECTS ON EDUCATIONAL ATTAINMENT

	Base	Alt. bandwidth		Alt. neighborhood		Alt. sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: DV = cumulative GPA							
Treat × 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 × 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 × 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

TABLE IV
(CONTINUED)

	Base	Alt. bandwidth		Alt. neighborhood		Alt. sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Neighborhood	Blk grp	Blk grp	Blk grp	Tract	Grid	Blk grp	Blk grp
Treatment	<0.50 mi	<0.25 mi	<0.50 mi	<0.50 mi	<0.50 mi	<0.50 mi	<0.50 mi
Control	0.50–3 mi	0.50–3 mi	0.50–2 mi	0.50–3 mi	0.50–3 mi	0.50–3 mi	0.50–3 mi
Sample	—	—	—	—	—	Allow std-grade duplicates	Exclude multi-treaters

Notes. The table shows results from estimation of a modified version of equation (4), replacing the full set of expected grade at treatment interactions with a simple posttreatment dummy set to 1 for treated observations in expected grade $\leqslant 12$. The sample contains the pooled cross-section of students living within 3 miles of a killing from expected grades 9 through 16, where expected grade is determined by the year students began 9th grade at LAUSD. Student i may appear at multiple expected grades g, but only once per expected grade, with treatment status for observation (i, g) determined by the nearest killing in that grade (i.e., $\leqslant 0.50$ miles). Sample is limited to entering ninth graders with expected graduation dates between 2006 to 2016 for the GPA and HS graduation analysis and to those with expected graduation dates between 2009 and 2014 for the college enrollment analysis. Cumulative GPA is a student's final cumulative GPA on exiting LAUSD (mean = 1.89). Graduated is an indicator set to 1 if a student received a diploma, GED, or special education certificate of completion from LAUSD (mean = 0.50). College enrollment is an indicator for whether a student enrolled in college within the calendar year after their expected high school graduation date (0.33). 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. Column (1) presents my base specification controlling for time trends at the census block group level. Column (2) restricts the treatment group to students living within 0.25 miles of a 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 Los Angeles into a grid. Observation numbers change across alternative neighborhood specifications due to singletons being dropped. 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.

completion come almost entirely from dropouts. Treatment estimates for the two are nearly mirror images. I find no significant effect of exposure to police killings on transfers.[48]

VII. Conclusion

In recent years, officer-involved killings have generated significant controversy and raised important questions about police oversight and lethal use of force. This article provides the first causal evidence of the impact of police killings on nearby students. I find that exposure to police violence leads to significant decreases in the educational achievement and attainment of black and Hispanic students. These effects are largest following police killings of unarmed minorities and differ meaningfully from those of criminal homicides, which produce smaller spillovers that do not vary with the race of the deceased.

Taken together, my results indicate that police violence may exacerbate racial inequality in education. The long-run estimates suggest that officer-involved killings caused nearly 2,000 underrepresented minorities to drop out of Los Angeles schools over the sample period. Given that I focus on hyperlocal effects among high schoolers and do not account for effects on younger children or for other returns to schooling (Lochner and Moretti 2004), this figure likely underestimates the total educational costs of police killings. Nonetheless, the direction and magnitude of these effects are necessary considerations in policy debates about limits to officer use of force.

More broadly, this article's findings emphasize the need to critically examine the appropriate role of law enforcement in local communities. While researchers have found that increased police presence may reduce crime (Evans and Owens 2007; Blattman et al. 2017; Chalfin and McCrary 2018; Mello 2019), far less attention has been paid to the impact of law enforcement on other dimensions of community well-being. Rather than bolstering public safety, my results suggest that some police encounters may traumatize local residents and erode feelings of security. These findings and the racially disparate effects on education call to mind long-standing concerns among minorities of "a double-standard of

48. Treatment estimates on graduation in expected grades 9 and 10 are −0.017 and −0.018 points, respectively. Estimates for dropouts are 0.016 and 0.016 points, while those for transfers are 0.001 and 0.002 points.

justice and protection" (Kerner Commission 1968). Greater interrogation of these concerns and a more nuanced understanding of the specific factors driving them is vital to determining the optimal levels and responsibilities of law enforcement. Given the intensity of policing in many minority neighborhoods, this type of examination may hold important ramifications for the long-run outcomes of marginalized populations.

Harvard University

Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

Code replicating the tables and figures in this article can be found in Ang (2020), in the Harvard Dataverse doi: 10.7910/DVN/WYGB43.

References

Aizer, Anna, "Neighborhood Violence and Urban Youth," in *The Problems of Disadvantaged Youth: An Economic Perspective*, Jonathan Gruber, ed. (Chicago: University of Chicago Press, 2009), 275.

Alsan, Marcella, and Marianne Wanamaker, "Tuskegee and the Health of Black Men," *Quarterly Journal of Economics*, 133 (2018), 407–455.

Ang, Desmond, "Replication Data for: 'The Effects of Police Violence on Inner-City Students'," 2020, Harvard Dataverse, doi:10.7910/DVN/WYGB43.

AP-NORC, "Law Enforcement and Violence: The Divide between Black and White Americans," Technical report, 2015.

Bayley, David H., and Harold Mendelsohn, *Minorities and the Police: Confrontation in America* (New York: Free Press, 1969).

Beck, Aaron T., Robert A. Steer, and Gregory K. Brown, *Manual for the Beck Depression Inventory*, 2nd ed. (San Antonio: Psychological Press, 1996).

Beland, Louis-Philippe, and Dongwoo Kim, "The Effect of High School Shootings on Schools and Student Performance," *Educational Evaluation and Policy Analysis*, 38 (2016), 113–126.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan, "How Much Should We Trust Differences-in-Differences Estimates?," *Quarterly Journal of Economics*, 119 (2004), 249–275.

Billings, Stephen B., David J. Deming, and Jonah Rockoff, "School Segregation, Educational Attainment, and Crime: Evidence from the End of Busing in Charlotte-Mecklenburg," *Quarterly Journal of Economics*, 129 (2013), 435–476.

Blattman, Christopher, Donald Green, Daniel Ortega, and Santiago Tobón, "Place-Based Interventions At Scale: The Direct and Spillover Effects of Policing and City Services on Crime," NBER Technical report, 2017.

Bobo, Lawrence D., and Victor Thompson, “Unfair by Design: The War on Drugs, Race, and the Legitimacy of the Criminal Justice System,” *Social Research: An International Quarterly*, 73 (2006), 445–472.
Bor, Jacob, Atheendar S. Venkataramani, David R. Williams, and Alexander C. Tsai, “Police Killings and Their Spillover Effects on the Mental Health of Black Americans: A Population-based, Quasi-experimental Study,” *Lancet*, 392 (2018), 302–310.
Boyd, Rhea W., “Police Violence and the Built Harm of Structural Racism,” *Lancet*, 392 (2018), 258–259.
Braga, Anthony A., Christopher Winship, Tom R. Tyler, Jeffrey Fagan, and Tracey L. Meares, “The Salience of Social Contextual Factors in Appraisals of Police Interactions with Citizens: A Randomized Factorial Experiment,” *Journal of Quantitative Criminology*, 30 (2014), 599–627.
Brandl, Steven G., Meghan S. Stroshine, and James Frank, “Who Are the Complaint-Prone Officers?: An Examination of the Relationship between Police Officers’ Attributes, Arrest Activity, Assignment, and Citizens’ Complaints about Excessive Force,” *Journal of Criminal Justice*, 29 (2001), 521–529.
Broberg, Anders G., Atle Dyregrov, and Lars Lilled, “The Göteborg Discotheque Fire: Posttraumatic Stress, and School Adjustment as Reported by the Primary Victims 18 Months Later,” *Journal of Child Psychology and Psychiatry*, 46 (2005), 1279–1286.
Brooks, Richard R. W., “Fear and Fairness in the City: Criminal Enforcement and Perceptions of Fairness in Minority Communities,” *Southern California Law Review*, 73 (1999), 1219.
Brunson, Rod K., “Police Don’t Like Black People: African-American Young Men’s Accumulated Police Experiences,” *Criminology & Public Policy*, 6 (2007), 71–101.
Brunson, Rod K., and Jody Miller, “Young Black Men and Urban Policing in the United States,” *British Journal of Criminology*, 46 (2005), 613–640.
Burdick-Will, Julia, “School Violent Crime and Academic Achievement in Chicago,” *Sociology of Education*, 86 (2013), 343–361.
Burdick-Will, Julia, Jens Ludwig, Stephen W. Raudenbush, Robert J. Sampson, Lisa Sanbonmatsu, and Patrick Sharkey, “Converging Evidence for Neighborhood Effects on Children’s Test Scores: An Experimental, Quasi-experimental, and Observational Comparison,” *Whither Opportunity?: Rising Inequality, Schools, and Children’s Life Chances* (New York: Russell Sage Foundation, 2011), 255–276.
Callaway, Brantly, and Pedro H. C. Sant’Anna, “Difference-in-Differences with Multiple Time Periods,” SSRN 3148250 (2019).
Callen, Michael, Mohammad Isaqzadeh, James D. Long, and Charles Sprenger, “Violence and Risk Preference: Experimental Evidence from Afghanistan,” *American Economic Review*, 104 (2014), 123–148.
Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, “Robust Inference with Multiway Clustering,” *Journal of Business & Economic Statistics*, 29 (2011), 238–249.
Carlana, Michela, “Implicit Stereotypes: Evidence from Teachers’ Gender Bias,” *Quarterly Journal of Economics*, 134 (2019), 1163–1224.
Carr, Patrick J., Laura Napolitano, and Jessica Keating, “We Never Call the Cops and Here Is Why: A Qualitative Examination of Legal Cynicism in Three Philadelphia Neighborhoods,” *Criminology*, 45 (2007), 445–480.
Carrell, Scott E., and Mark L. Hoekstra, “Externalities in the Classroom: How Children Exposed to Domestic Violence Affect Everyone’s Kids,” *American Economic Journal: Applied Economics*, 2 (2010), 211–228.
Carter, Erik W., Kathleen L. Lane, Melinda R. Pierson, and Barbara Glaeser, “Self-determination Skills and Opportunities of Transition-Age Youth with Emotional Disturbance and Learning Disabilities,” *Exceptional Children*, 72 (2006), 333–346.

Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein, "The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design," *Quarterly Journal of Economics*, 125 (2010), 215–261.
Chalfin, Aaron, and Justin McCrary, "Are US Cities Underpoliced? Theory and Evidence," *Review of Economics and Statistics*, 100 (2018), 167–186.
Charles, Kerwin Kofi, and Jonathan Guryan, "Prejudice and Wages: An Empirical Assessment of Becker's the Economics of Discrimination," *Journal of Political Economy*, 116 (2008), 773–809.
Chetty, Raj, John N. Friedman, Nathaniel Hendren, Maggie R. Jones, and Sonya R. Porter, "The Opportunity Atlas: Mapping the Childhood Roots of Social Mobility," NBER Technical report, 2018.
Chetty, Raj, Nathaniel Hendren, Maggie R. Jones, and Sonya R. Porter, "Race and Economic Opportunity in the United States: An Intergenerational Perspective," *Quarterly Journal of Economics*, 135 (2020), 711–783.
Chetty, Raj, Nathaniel Hendren, and Lawrence F. Katz, "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment," *American Economic Review*, 106 (2016), 855–902.
Davis, Elizabeth, Anthony Whyde, and Lynn Langton, "Contacts between Police and the Public, 2015," Bureau of Justice Statistics, US Department of Justice, 2018.
Dawson, Michael C., Ronald E. L. Brown, and James S. Jackson, "National Black Politics Study, [United States], 1993," Inter-university Consortium for Political and Social Research [distributor], 2019-06-06. https://doi.org/10.3886/ICPSR02018.v3.
de Chaisemartin, Clément, and Xavier d'Haultfoeuille, "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects," *American Economic Review*, 110 (2020), 2964–2996.
Derenoncourt, Ellora, "Can You Move to Opportunity? Evidence from the Great Migration," Technical report, Harvard University, 2018.
Desmond, Matthew, Andrew V. Papachristos, and David S. Kirk, "Police Violence and Citizen Crime Reporting in the Black Community," *American Sociological Review*, 81 (2016), 857–876.
DiPasquale, Denise, and Edward L. Glaeser, "The Los Angeles Riot and the Economics of Urban Unrest," *Journal of Urban Economics*, 43 (1998), 52–78.
Edwards, Frank, Hedwig Lee, and Michael Esposito, "Risk of Being Killed by Police Use of Force in the United States by Age, Race–Ethnicity, and Sex," *Proceedings of the National Academy of Sciences*, 116 (2019), 16793–16798.
Evans, William N., and Emily G. Owens, "COPS and Crime," *Journal of Public Economics*, 91 (2007), 181–201.
Famularo, Richard, Terence Fenton, Marilyn Augustyn, and Barry Zuckerman, "Persistence of Pediatric Post Traumatic Stress Disorder after 2 Years," *Child Abuse & Neglect*, 20 (1996), 1245–1248.
Fan, Jianqing, and Irene Gijbels, *Local Polynomial Modelling and Its Applications* (Boca Raton, FL: CRC Press, 1996).
Friedman, Robert M., Judith W. Katz-Leavy, Ronald W. Manderscheid, and Diane L. Sondheimer, "Prevalence of Serious Emotional Disturbance in Children and Adolescents," *Mental Health, United States*, 996 (1996), 71–89.
Fryer, Roland G. Jr., "The Production of Human Capital in Developed Countries: Evidence from 196 Randomized Field Experiments," In "Handbook of Economic Field Experiments" (Elsevier, 2017). 95–322.
———, "An Empirical Analysis of Racial Differences in Police Use of Force," *Journal of Political Economy*, 127 (2019), 1210–1261.
Gershenson, Seth, and Michael S. Hayes, "Police Shootings, Civic Unrest and Student Achievement: Evidence from Ferguson," *Journal of Economic Geography*, 18 (2018), 663–685.
Gershenson, Seth, and Erdal Tekin, "The Effect of Community Traumatic Events on Student Achievement: Evidence from the Beltway Sniper Attacks," *Education Finance and Policy*, 13 (2018), 513–544.

Glover, Dylan, Amanda Pallais, and William Pariente, "Discrimination as a Self-fulfilling Prophecy: Evidence from French Grocery Stores," *Quarterly Journal of Economics*, 132 (2017), 1219–1260
Goodman-Bacon, Andrew, "Difference-in-Differences with Variation in Treatment Timing," NBER Technical report, 2018.
Hacker, Karen A., Shakira F. Suglia, Lise E. Fried, Nancy Rappaport, and Howard Cabral, "Developmental Differences in Risk Factors for Suicide Attempts between Ninth and Eleventh Graders," *Suicide and Life-Threatening Behavior*, 36 (2006), 154–166.
Hale, Chris, "Fear of Crime: A Review of the Literature," *International Review of Victimology*, 4 (1996), 79–150.
Hall, Alison V., Erika V. Hall, and Jamie L. Perry, "Black and Blue: Exploring Racial Bias and Law Enforcement in the Killings of Unarmed Black Male Civilians," *American Psychologist*, 71 (2016), 175.
Harris, Linda Hall, "Role of Trauma in the Lives of High School Dropouts," *Children & Schools*, 5 (1983), 77–88.
Heissel, Jennifer A., Emma K. Adam, Jennifer L. Doleac, David N. Figlio, and Jonathan Meer, "Testing, Stress, and Performance: How Students Respond Physiologically to High-Stakes Testing," Education Finance and Policy, (forthcoming).
Heissel, Jennifer A., Patrick T. Sharkey, Gerard Torrats-Espinosa, Kathryn Grant, and Emma K. Adam, "Violence and Vigilance: The Acute Effects of Community Violent Crime on Sleep and Cortisol," *Child Development*, 89 (2018), e323–e331.
Hurst, Yolander G., and James Frank, "How Kids View Cops the Nature of Juvenile Attitudes toward the Police," *Journal of Criminal Justice*, 28 (2000), 189–202.
Jacobs, David, "The Determinants of Deadly Force: A Structural Analysis of Police Violence," *American Journal of Sociology*, 103 (1998), 837–862.
Johnson, David J., Trevor Tress, Nicole Burkel, Carley Taylor, and Joseph Cesario, "Officer Characteristics and Racial Disparities in Fatal Officer-Involved Shootings," *Proceedings of the National Academy of Sciences*, 116 (2019), 15877–15882.
Kania, Richard R. E., and Wade C. Mackey, "Police Violence as a Function of Community Characteristics," *Criminology*, 15 (1977), 27–48.
Katz, Lawrence F., Jeffrey R. Kling, and Jeffrey B. Liebman, "Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment," *Quarterly Journal of Economics*, 116 (2001), 607–654.
Kerner Commission, *Report of the National Advisory Commission on Civil Disorders* (Washington, DC: US Government Printing Office, 1968).
Kirk, David S., and Andrew V. Papachristos, "Cultural Mechanisms and the Persistence of Neighborhood Violence," *American Journal of Sociology*, 116 (2011), 1190–1233.
Knox, Dean, Will Lowe, and Jonathan Mummolo, "Administrative Records Mask Racially Biased Policing," *American Political Science Review*, 114 (2020), 619–637.
Knox, Dean, and Jonathan Mummolo, "Making Inferences about Racial Disparities in Police Violence," *Proceedings of the National Academy of Sciences*, 117 (2020), 1261–1262.
Lacoe, Johanna, and Jillian Stein, "Exploring the Policy Implications of High-Profile Police Violence," *Criminology & Public Policy*, 17 (2018), 859–863.
Legewie, Joscha, and Jeffrey Fagan, "Aggressive Policing and the Educational Performance of Minority Youth," *American Sociological Review*, 84 (2019), 220–247.
Leiber, Michael J., Mahesh K. Nalla, and Margaret Farnworth, "Explaining Juveniles' Attitudes toward the Police," *Justice Quarterly*, 15 (1998), 151–174.
Lochner, Lance, and Enrico Moretti, "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports," *American Economic Review*, 94 (2004), 155–189.

Luthar, Suniya S., "Vulnerability and Resilience: A Study of High-Risk Adolescents," *Child Development*, 62 (1991), 600–616.
Maxson, Cheryl L., Margaret A. Gordon, and Malcolm W. Klein, "Differences between Gang and Nongang Homicides," *Criminology*, 23 (1985), 209–222.
McInerney, Maura, Michael Kane, and Sol Pelavin, "Services to Children with Serious Emotional Disturbance," Washington, DC: Office of Planning and Budget Evaluation, US Department of Education (1992).
Mello, Steven, "More COPS, Less Crime," *Journal of Public Economics*, 172 (2019), 174–200.
Monteiro, Joana, and Rudi Rocha, "Drug Battles and School Achievement: Evidence from Rio De Janeiro's Favelas," *Review of Economics and Statistics*, 99 (2017), 213–228.
Moya, Andrés, "Violence, Psychological Trauma, and Risk Attitudes: Evidence from Victims of Violence in Colombia," *Journal of Development Economics*, 131 (2018), 15–27.
Mueser, Kim T., and Jonas Taub, "Trauma and PTSD among Adolescents with Severe Emotional Disorders Involved in Multiple Service Systems," *Psychiatric Services*, 59 (2008), 627–634.
Nix, Justin, Bradley A. Campbell, Edward H. Byers, and Geoffrey P. Alpert, "A Bird's Eye View of Civilians Killed by Police in 2015: Further Evidence of Implicit Bias," *Criminology & Public Policy*, 16 (2017), 309–340.
Pew Research Center, "On Views of Race and Inequality, Blacks and Whites Are Worlds Apart," Technical report, 2016.
———, "Race in America," Technical report, 2019.
Porche, Michelle V., Lisa R. Fortuna, Julia Lin, and Margarita Alegria, "Childhood Trauma and Psychiatric Disorders as Correlates of School Dropout in a National Sample of Young Adults," *Child Development*, 82 (2011), 982–998.
Rambachan, Ashesh, and Jonathan Roth, "An Honest Approach To Parallel Trends," Harvard University working paper, 2019.
Renauer, Brian C., "Reducing Fear of Crime: Citizen, Police, or Government Responsibility?" *Police Quarterly*, 10 (2007), 41–62.
Rossin-Slater, Maya, Molly Schnell, Hannes Schwandt, Sam Trejo, and Lindsey Uniat, "Local Exposure to School Shootings and Youth Antidepressant Use," NBER Technical report, 2019.
Sharkey, Patrick, "The Acute Effect of Local Homicides on Children's Cognitive Performance," *Proceedings of the National Academy of Sciences*, 107 (2010), 11733–11738.
Sharkey, Patrick, Amy Ellen Schwartz, Ingrid Gould Ellen, and Johanna Lacoe, "High Stakes in the Classroom, High Stakes on the Street: The Effects of Community Violence on Student's Standardized Test Performance," *Sociological Science*, 1 (2014), 199–220.
Sharkey, Patrick T., Nicole Tirado-Strayer, Andrew V. Papachristos, and C. Cybele Raver, "The Effect of Local Violence on Children's Attention and Impulse Control," *American Journal of Public Health*, 102 (2012), 2287–2293.
Sigelman, Lee, Susan Welch, Timothy Bledsoe, and Michael Combs, "Police Brutality and Public Perceptions of Racial Discrimination: A Tale of Two Beatings," *Political Research Quarterly*, 50 (1997), 777–791.
Skolnick, Jerome H., and James J. Fyfe, *Above the Law: Police and the Excessive Use of Force* (New York: Free Press, 1993).
Snyder, Howard N., Melissa Sickmund, and Eileen Poe-Yamagata, *Juvenile Offenders and Victims: 1996 Update on Violence* (Washington, DC: US Department of Justice, Office of Justice Programs, 1996).
Sun, Liyang, and Sarah Abraham, "Estimating Dynamic Treatment Effects in Event Studies With Heterogeneous Treatment Effects," MIT working paper (2020).
Swaine, Joe, and Oliver Laughland, "The County: The Story of America's Deadliest Police," *The Guardian* (Dec 1 2015), https://www.theguardian.com/us-news/2015/dec/01/the-county-kern-county-deadliest-police-killings.

Taylor, Terrance J., Kelly B. Turner, Finn-Aage Esbensen, and L. Thomas Winfree, "Coppin' an Attitude: Attitudinal Differences among Juveniles toward Police," *Journal of Criminal Justice*, 29 (2001), 295–305.

Tyler, Tom R., Jeffrey Fagan, and Amanda Geller, "Street Stops and Police Legitimacy: Teachable Moments in Young Urban Men's Legal Socialization," *Journal of Empirical Legal Studies*, 11 (2014), 751–785.

U.S. Department of Education, "To Assure the Free Appropriate Public Education of All Children with Disabilities: Fifteenth Annual Report to Congress on the Implementation of the Individuals with Disabilities Education Act," 1993.

Wagner, Mary M., "Outcomes for Youths with Serious Emotional Disturbance in Secondary School and Early Adulthood," *Future of Children*, 5 (1995), 90–112.

Weitzer, Ronald, and Steven A. Tuch, "Perceptions of Racial Profiling: Race, Class, and Personal Experience," *Criminology*, 40 (2002), 435–456.

———, "Race and Perceptions of Police Misconduct," *Social Problems*, 51 (2004), 305–325.

White, Clair, David Weisburd, and Sean Wire, "Examining the Impact of the Freddie Gray Unrest on Perceptions of the Police," *Criminology & Public Policy*, 17 (2018), 829–858.

Whitney, Camille R., and Jing Liu, "What We're Missing: A Descriptive Analysis of Part-Day Absenteeism in Secondary School," *AERA Open*, 3 (2017), 2332858417703660.

Winfree, L. Thomas Jr., and C. T. Griffith, "Adolescent Attitudes towards the Police: A Survey of High School Students," in *Juvenile Delinquency: Little Brothers Grow Up* (Beverly Hills, CA: Sage, 1977), 1–14.

Yehuda, Rachel, Sarah L. Halligan, Julia A. Golier, Robert Grossman, and Linda M. Bierer, "Effects of Trauma Exposure on the Cortisol Response to Dexamethasone Administration in PTSD and Major Depressive Disorder," *Psychoneuroendocrinology*, 29 (2004), 389–404.