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Aggressive Policing and the Educational Performance of Minority Youth

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Abstract

An increasing number of minority youth experience contact with the criminal justice system. But how does the expansion of police presence in poor urban communities affect educational outcomes? Previous research points at multiple mechanisms with opposing effects. This article presents the first causal evidence of the impact of aggressive policing on minority youths' educational performance. Under Operation Impact, the New York Police Department (NYPD) saturated high-crime areas with additional police officers with the mission to engage in aggressive, order-maintenance policing. To estimate the effect of this policing program, we use administrative data from more than 250,000 adolescents age 9 to 15 and a difference-in-differences approach based on variation in the timing of police surges across neighborhoods. We find that exposure to police surges significantly reduced test scores for African American boys, consistent with their greater exposure to policing. The size of the effect increases with age, but there is no discernible effect for African American girls and Hispanic students. Aggressive policing can thus lower educational performance for some minority groups. These findings provide evidence that the consequences of policing extend into key domains of social life, with implications for the educational trajectories of minority youth and social inequality more broadly.

Introduction

Over the last three decades, cities across the United States have adopted strategies known as proactive or broken windows policing with a focus on strict enforcement of low-level crimes and extensive use of pedestrian stops (Fagan et al. 2016; Greene 2000; Kohler-Hausmann 2013; Kubrin et al. 2010; Weisburd and Majmundar 2018).[2] As a consequence of these changes in the strategies and tactics of street policing, an increasing number of minority youth have involuntary contact with the criminal justice system (Brame et al. 2011; Hagan, Shedd, and Payne 2005). By age 18, national data suggests that between 15.9% and 26.8% of the population was arrested at least once (Brame et al. 2011). In a recent representative survey of 15-year old urban youth, 39% of African-American compared to 23% of white boys report that they were stopped by the police at least once (Geller 2018). In New York City, the police conducted more than 4 million pedestrian stops between 2004 and 2012, with more than half concentrated among persons less than 25 years of age (Fagan et al. 2010). Similar practices exist in major cities across the country (Weisburd and Majmundar, 2018).

While investments in policing including some forms of proactive policing are credited with reductions in crime, we know less about the social costs of policing (Weisburd and Majmundar 2018). What are the consequences of the increasing presence of police in minority communities for the educational performance of minority youth? Previous research points to multiple mechanisms with opposing effects. First, the effect of policing may be positive by reducing the level of neighborhood crime and violence, which in turn increases school performance. Second, aggressive, broken window policing may have negative effects by undermining trust in authorities including schools and teachers, and by leading to withdrawal and system avoidance. High rates of police direct or indirect contacts may also create stress and other health and emotional responses that undermine cognitive performance. Despite the increasing exposure of minority youth to the police and contradictory previous research, there is no convincing causal evidence about the effects of proactive policing on the educational performance of minority youth.

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[2] The terms order-maintenance, zero-tolerance, quality-of-life, proactive and broken-window policing are used somewhat interchangeably. They generally refer to a style of policing that strictly enforces low-level crimes, targets minor forms of disorderly behavior and proactively engages citizens through pedestrian stops and searches to prevent crime.

To address this question, we focus on New York Police Department's (NYPD) Operation Impact; a policing program in New York City that substantially increased the intensity of broken window policing in selected neighborhoods. Our design exploits the staggered implementation of Operation Impact, which quickly increased the number of police officers in high crime areas designated as impact zones at different points in time (Golden and Almo 2004). Starting in January 2004, the NYPD deployed around 1,500 recent police academy graduates to impact zones with the mission to engage in aggressive order maintenance policing targeted at disorderly behaviors through strict enforcement of low-level crimes and extensive use of pedestrian stops. The high concentration of officers in impact zones produced a substantial increase in policing activity and a modest decrease in violent crime. Between 2004 and 2012, the NYPD continuously adopted the program over 15 phases by expanding, moving, removing or adding impact zones roughly every six months. Over the duration of the program, 18.3% of African-American, 14.6% of Hispanic and 0.7% of White elementary and middle public school students in New York City were exposed to impact zones at least once.

To estimate the effect of Operation Impact on educational performance, we link information on impact zones with administrative data from the New York City Department of Education (NYCDOE) on public school students from the school year 2003/2004 to 2011/12. We use a Difference-in-Difference (DD) approach that exploits the longitudinal structure of the data and variation in the timing of police surges across neighborhoods (Meyer 1995). Focusing on students' residential context, our approach compares changes in test-scores before, during and after Operation Impact for areas affected by the intervention to the same difference for areas that are designated as impact zones at a different point in time. The analysis conditions on the level of prior crime as the most important criteria for the selection of impact zones.

The findings show that Operation Impact lowered the educational performance of African-American boys, with implications for child development, economic mobility and racial inequality. The effect size varies by race, gender and age. It is substantial for African-American boys aged 13 to 15, and small and statistically insignificant for other groups with need for further research on the consequences of policing for Hispanic and white students. Additional analyses provide first evidence on the underlying mechanisms but are also limited by the lack of data on student health. They show that Operation Impact reduced crime providing evidence for a positive

channel through lower crime rates; and they show that Operation Impact reduced school attendance indicating that system avoidance is a possible mechanism. Considering the significant racial disparities in police contact (Fagan et al. 2010; Hagan et al. 2005; Legewie 2016), these findings suggest that aggressive policing strategies and tactics can lower educational performance and perpetuate racial inequalities in educational outcomes. They reveal the social costs of policing with consequences that extend into key domains of social life.

Policing, Crime and Educational Outcomes

Over the last decades, law enforcement underwent a fundamental transition away from a focus on felony offenses towards proactive or broken-window policing and the relentless targeting of minor forms of disorderly behavior (Kohler-Hausmann 2013; Stuart 2016; Weisburd and Majmundar 2018). As a consequence of these strategic and tactical changes, an increasing number of minority youth experience contact with the police and later stages of the criminal justice system (Brame et al. 2011; Hagan, Shedd, and Payne 2005). Both individual and neighborhood-level police exposure are highly stratified by class and race (Fagan et al. 2010).

A growing literature examines the consequences of the criminal justice system for social inequality and stratification including children's educational outcomes (Pager 2003; Pettit and Western 2004; Wakefield and Uggen 2010). Among other conclusions, important research links parental incarceration to the social, cognitive, behavioral, and health development of children (Haskins 2014; Haskins and Jacobsen 2017; Wakefield and Uggen 2010). Haskins (2014), for example, focuses on the effect of paternal incarceration on school readiness. She finds that boys with incarcerated fathers have substantially worse non-cognitive skills at school entry with implications for educational trajectories.

However, this literature is limited in three important ways. First, most research on the link between the criminal justice system and child development focuses on parental incarceration even though law enforcement and policing are a central and the most visible part of the criminal justice system. Second, while important research has examined the consequences of police stops and arrests for mental health (Geller et al. 2014; Sugie and Turney 2017), system avoidance (Brayne 2014), political participation (Lerman and Weaver 2014) and other outcomes, it largely ignores the potential consequences of policing for youth (for exceptions see Ang 2018;

Fagan and Tyler 2005; Kirk and Sampson 2013). Juveniles are of particular interest for understanding the link between the criminal justice system and social inequality. Compared to adults, adolescents are more likely to be in contact with the police (Leiber, Nalla, and Farnworth 1998), and this contact is particularly consequential for youth (Hurst and Frank 2000). These childhood and adolescent experiences have the potential to shape long-term socioeconomic outcomes (Heckman 2006). Third, previous research almost exclusively focuses on the consequences of direct contact with the criminal justice system in terms of incarceration, arrests, convictions and sometimes police stops. However, ethnographic research powerfully demonstrates that the consequences of policing are not confined to those that are stopped and arrested by the police but extend to entire communities (Anderson 1999; Goffman 2014; Rios 2011; Shedd 2015).

This article addresses these limitations and examines the effect of neighborhood-level exposure to aggressive, broken-window policing on the educational outcomes of minority youth. Understanding how policing strategies and tactics influence child development is essential to advance our knowledge about the link between the criminal justice system and social inequality.

Aggressive Policing and the Educational Performance of Minority Youth

Theoretical models of neighborhood effects and policing point at different mechanisms that predict opposing effects of policing on education performance. First, policing can reduce crime rates (Braga, Welsh, and Schnell 2015; MacDonald, Fagan, and Geller 2016), and thereby improve the educational performance of students (Sharkey 2018a). Indeed, there is strong evidence that violent crimes reverberate across communities with implications that extend beyond the victims. Recent ethnographic studies on urban poverty document how violence forces kids to navigate their environment in very different ways with consequences that are subtle but often change their developmental trajectory (Anderson 1990; Harding 2010). David Harding's study of adolescent boys in Boston suggests that crime and violence is an important mechanism that explains neighborhood effects and influences life chances. Research on the heterogeneity of neighborhood effects in the Moving to Opportunity experiment similarly points to violent crimes as a key mechanism for neighborhood effects (Burdick-Will et al. 2011). In related work, Patrick Sharkey and colleagues show that exposure to violent crimes in your immediate surrounding induces stress and lowers test-scores (Sharkey 2010; Sharkey et al. 2012, 2014). This research indicates

that violent crimes in high-poverty neighborhoods can influence a child's cognitive development, school performance, mental health and long-term physical health. It suggests that effective policing promises to reduce neighborhood inequalities creating opportunities for children in high-crime areas.

Indeed, there is evidence that certain policing strategies reduce crime. Systematic reviews of experimental and quasi-experimental studies suggest a modest effect of policing disorder on crime with the strongest reduction following interventions that focused on community and problem-solving policing to change social and physical disorder conditions (Braga, Papachristos, and Hureau 2014; Braga et al. 2015:568; Weisburd and Majmundar 2018). MacDonald et al (2016) use a difference-in-difference approach to examine the effect of Operation Impact – the policing program analyzed in this study – on crime in New York City. They find that the surge in police officers partly contributed to the reduction in crime but show that pedestrian stops did not play an important role. Accordingly, the reduction in crime related to police surges might improve the educational prospects of children in high crime areas by reducing the developmentally disruptive effects of crime.

Second, aggressive policing might *negatively* influence educational outcomes based on both direct and indirect confrontations with the police. Direct police contact such as pedestrian stops, police harassment or arrests can erode trust in state institutions, lead to system avoidance and induce stress or other health problems, which in turn reduce educational performance. Previous research has documented many of these mechanisms. Based on extensive fieldwork in the late 1970s and 1980s, Elijah Anderson's *Streetwise* chronicles the perils faced by black youth of constant police presence including stops, harassment, and even arrests whether or not they had committed a crime (Anderson 1990 Cha. 7). Direct interactions with the police and the criminal justice system are an important source of legal cynicism defined as a "a cultural frame in which people perceive the law as illegitimate, unresponsive, and ill equipped to ensure public safety" particularly when these interactions are perceived as unjust and discriminatory (Kirk and Papachristos 2011:1199). Evidence from survey research supports this idea and shows that the number of police stops young men see or experience is related to a diminished sense of police legitimacy and legal cynicism (Tyler, Fagan, and Geller 2014). The concept of *system avoidance* suggests that police contact has implications for other domains as well including education. Brayne (2014) describes *system avoidance* as "the practice of individuals avoiding institutions

that keep formal records" (Brayne 2014:368) including medical, financial, labor market, and educational institutions. She uses survey data to show that individuals who have been stopped, arrested, convicted or incarcerated are subsequently less likely to engage with surveilling institutions but continue to participate in civic or religious institutions at the same rate. In New York City and many other places, school safety agents are widespread and part of the police force so that withdrawal from school is a plausible mechanism.

Police contact can also erode children's educational performance through negative health consequences related to stress, fear, trauma and anxiety (Geller et al. 2014; Golembeski and Fullilove 2005; Sugie and Turney 2017). Police encounters are often harsh, entail racial/ethnic degradation and in many cases the use of police force (Brunson 2007; Brunson and Weitzer 2009; Legewie 2016; Legewie and Fagan 2016). They can trigger adverse health effects such as stress, fear, anxiety or even depressive symptoms (Brunson 2007; Kaplan, Liu, and Kaplan 2005; Link and Phelan 2001), which reduce cognitive and educational performance (Kaplan et al. 2005; Lupien et al. 2007; Sugie and Turney 2017). Sugie and Turney (2017) explicate this argument based on the stress process paradigm. They argue that criminal justice contact is an important stressor that increases mental health problems and is particularly consequential for racial/ethnic minorities in disadvantaged settings. Others link the health consequences of police contact to perceptions of injustice arguing that the consequences are more pronounced when individuals fear future encounters or believe that they are unfairly stopped and questioned because of their race or ethnicity (Anderson 2013; Sawyer et al. 2012). These elevated levels of stress, fear, trauma and anxiety related to police contact undermine cognitive functions and are a potential mechanisms that leads to reduced educational performance (Osofsky et al. 2004; Sharkey 2010).

But the consequences are not confined to direct contact with the police. Indeed, indirect or vicarious exposure through friends, family and the community at large plays an important role in research on policing and neighborhood disadvantage (Anderson 1990; Bourgois 2003; Goffman 2014; Rios 2011; Shedd 2015; Stuart 2016; Venkatesh 2002). In *Unequal City*, Carla Shedd (2015) showcases how Chicago's youth navigate law enforcement in their neighborhoods. She shows how police procedures like stop-and-frisk shape adolescents' worldviews, foster distrust of authorities and induce feelings of powerlessness among youth who experience mistreatment either directly or indirectly through friends. In *On the Run*, Alice Goffman (2014) reports how the experience of (mis)treatment by police shapes not only those that are stopped and arrested

by the police but also the lives of families in America's poor, black neighborhoods. She details the intricate techniques used by African-American youth to evade the authorities, how their social life is shaped by police presence and how the influence extends beyond crime suspects to entire communities. Along similar lines, high-profile cases of police violence reinforce legal cynicism among African-American residents and lower citizen crime reporting across entire neighborhoods (Desmond, Papachristos, and Kirk 2016). From this perspective, legal cynicism is not only shaped by direct confrontations with the police but also by neighborhood variation in police practices (Kirk and Papachristos 2011). Potential health effects are similarly not confined to those that are stopped and arrested by the police. Indeed, Sewell and Jefferson (2016) suggest that living in neighborhoods with a higher rate of invasive police stops is associated with worse health (see also Sewell, Jefferson, and Lee 2016). They suggest that general anxiety and fear simply based on seeing police officers or observing police intrusions on neighbors, friends and family members explain part of this effect. Other research documents how police killings have spillover effects on the mental health of black Americans (Ang 2018; Bor et al. 2018). While many of these findings are limited by the cross-sectional nature of the data, they suggest that health consequences related to stress, fear, trauma and anxiety might affect entire communities and not only those that are in contact with the police and the criminal justice system.

Influence of Policing by Race, Gender and Age

The various processes may play out differently depending on the race, gender and age of the child. First, direct exposure to police varies substantially across these groups. African-American boys experience disproportionate contact with police (Geller 2018; Goel, Rao, and Shroff 2016). Police stops and arrest rates are substantially lower for white and to a smaller extent Hispanic students but also for African-American girls. Data from our research site in New York City show that rates of police stops and arrests rise through adolescence as students get older with stark racial and gender disparities (Figures and Legewie 2019). These disparities in direct exposure to police by race, gender and age have implications for the effect of increased police activity on student outcomes.

Second, previous research suggests that, conditional on being stopped, students experience different types of interactions with the police depending on their race and gender (Geller 2018). Brunson and Weitzer (2009) describe stark differences in police conduct during encounters

with White, Black and mixed race groups of teenage boys. Violence, sexual humiliation, and threats characterize stops of young African American males, while police were more deferential to young White males (Brunson and Weitzer 2009). Young men report routinely being treated as crime suspects during everyday stops, often with violence by police, while young women report more concern from police about their potential victimization (Brunson and Miller 2006). Other research shows racial and gender differences in police use of force (Legewie 2016).

Third, indirect or vicarious police contact likely depends on the race, gender and age of students as well. Vicarious exposure is based on hearing about experiences with the police from family or friends, or simply witnessing police stops, arrests or other police activity. The differences in the frequency and type of police stops and arrests by race, gender and age have direct implications for vicarious exposure through friends considering the racial, gender and age segregation of friendship networks (Moody 2001). Increased police exposure as student grow older coincides with the age at which adolescents increasingly engage with neighborhood peers and adults as a central part of their social life (Darling and Steinberg 1997). As a result, older African-American and to a smaller extend Hispanic boys are more likely to hear about experiences with and distrust of the police from somebody they know even when they are never confronted with the police or the criminal justice system themselves. Witnessing police stops, arrests or other police activity is similarly related to the race, gender and age of students because of variations in neighborhood-level police activity and time spent outside. Fagan et al (2010) document stark differences in neighborhood-level police activity by race and class, which translate to differences in vicarious police exposure.

These differences suggest that the various processes of erosion in trust, withdrawal, stress and anxiety operate in different ways depending on the level of direct and indirect exposure to police. We expect that the negative effect increases with age and is particularly pronounced for minority boys who fear future encounters or believe that they and their friends are unfairly stopped and questioned because of their race or ethnicity. On the other hand, the impact of crime might be similarly amplified for minority boys (Harding 2010).

NYPD's Operation Impact

In 2004, the NYPD launched Operation Impact, a tactic designed to maximize police investigative stops in areas designated as "Impact Zones" (Golden and Almo 2004). Operation Impact was a

second-generation enforcement tactic that replaced the Street Crime Unit, or SCU. SCU was created in 1994 and expanded citywide in 1997 (White and Fradella 2016). SCU officers roamed the city and conducted intensive stop activity under the NYPD stop and frisk program, targeting small "high crime areas" identified through a combination of police intelligence and data analytics.

Under Operation Impact, these activities were rationalized through crime analysis to focus on specific locations or "hot spots," as well as days of the week and times of day when criminal activity was highest. Impact zones ranged in size from very small areas, such as residential buildings or public housing sites, to areas as large as entire precincts. Impact zones were located in areas that were predominantly populated by Black and Latino New Yorkers. Over the years, the NYPD continuously adopted the program over 15 consecutive phases by expanding, moving, removing or adding impact zones roughly every six months (see Figure A1 for the rollout of impact zones over time). Between 2004 and 2012, 75 of the 76 New York City Police precincts had one or more Impact Zones (MacDonald et al. 2016). On average, areas remained designated as impact zones for 12.3 months but the duration ranged from 5.3 months to 7.5 years. Additional officers beyond regular precinct deployments were assigned to impact zones. From the outset, roughly two-thirds of the graduating classes from the Police Academy were assigned to Impact Zones (Golden and Almo 2004) while the overall number of sworn officers declined slightly between 2003 and 2013. These rookie patrolmen and patrolwomen were encouraged by supervisors to conduct high volumes of investigative stops. In addition to suspicion-based stops, officers were encouraged to make arrests for low-level offenses or issue warrants for minor non-criminal infractions (such as open containers of alcohol), and conducted other stops as pretexts to search for persons with outstanding warrants (Barrett 1998).

The high concentration of officers in impact zones produced a substantial increase in policing activity that quickly returned to the previous level after areas were removed from Operation Impact (see Appendix for a detailed analysis on the effect of Operation Impact on police activity). The number of pedestrian stops increased sharply by 33.2%. Arrests for low-level offenses similarly rose by 11.0% for misdemeanors and by 29.7% for violations whereas felony arrests remained largely the same. This increase in police activity was uneven by race. The number of pedestrian stops increased by 35.1% for African-Americans, 25.2% for Hispanics and 22.2% for whites with a similar pattern for misdemeanors and violation arrests.

Data and Methods

Our analyses rely on two sources of information. The first is administrative school district records from the New York City Department of Education (NYCDOE) assembled by the Research Alliance for New York City Schools. The database consists of administrative student-level records for all public school students in New York City in grade K – 8 from the school year 2003/2004 to 2011/12. The records include the school and grade identifier for the fall and spring terms, a limited number of standard demographic characteristics such as race/ethnicity, gender, date of birth, their eligibility for free lunch as a measure of parental socioeconomic background, limited English learner status as a measure of immigrant status, yearly test score measures for language and math in grade 3 through 8, students' residential neighborhood, and, starting in 2007, survey data from the NYC Learning Environment Survey.

The second data source includes pedestrian stops, crime complaints, arrests and information on Operation Impact from the New York Police Department (NYPD). *Pedestrian stops* are based on the "Stop, Question and Frisk" program and include records on 4.6 million time- and geocoded police stops of pedestrians in New York City between 2004 and 2012.[3] Stops are recorded by the officer on the "Stop, Question and Frisk Report Worksheet" (UF-250 form). Each record includes information on the exact timing, geographical location, the circumstances that led to the stop, details about the stopped person, the suspected crime, and the events during the stop itself such an arrest or the use of physical force by the police officer.[4] The *incident-level arrest data* include 3.3 million arrests in New York City between 2004 and 2012 with information on the date and time, the geocoded location, the offense charge, and the race, age, and gender of the arrested person. Offense charges were coded as violent felony, property felony, other felony, misdemeanor or violation. *Crime complaints* include 4.8 million geo-coded, incident-level felony, misdemeanor, and violation crimes reported to the NYPD between 2004 to 2012. The format of the data is similar to the arrest data. It includes information on the date and time, the geocoded

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[3] Stop, Question and Frisk (SQF) operations are regulated under both federal (Terry v. Ohio) and state (People v. DeBour) standards. SQF operations allow police officers who reasonably suspect that a person has committed, is committing, or is about to commit a felony or a Penal Law misdemeanor to stop and question that person. Frisks are permitted if the officer suspects the presence of a weapon or if the officer suspects he or she is in danger of physical injury. Searches are permitted if the officer has reasonable suspicion to believe that a crime has taken place. About 5% of stops resulted in arrest during this period, and another 5% resulted in the issuance of a citation for a non-criminal violation.

[4] Beginning in January 2006, New York City started using a citywide records management system to collect SQF data. Earlier records are not geocoded and instead include the address or street intersection of the stop. We geocoded these records using ArcGIS.

location, the offense type including violent felony, property felony, misdemeanor or violation, and (if available) the suspect's race, age, and gender. Finally, our information from the NYPD include digital boundary maps from the program *Operation Impact* (shapefiles) showing the exact geographic location and shape of impact zones over phase III to XVII together with information on the timing of the different phases. The NYPD was unable to provide comparable information for phase I and II in 2003. This initial period was smaller in scale and we ignore it in our analysis.

Estimation Strategy

Estimating the effect of policing on educational performance is challenging considering that police activity is closely linked to crime and other neighborhood characteristics. Indeed, the selection of impact zones was based on a two step-process (Golden and Almo 2004). First, police commanders nominated high crime areas within their precinct. Second, nominated areas were then discussed with officials and analysts at the police headquarter to make the final selection. According to the NYPD, this selection process was based on crime patterns and history. As a result, impact zones differ from other areas in many confounding ways such as crime or poverty rates that might also influence educational outcomes. This nonrandom selection of impact zones makes it difficult for typical observational studies to estimate the causal effect of Operation Impact and policing more broadly.

To overcome this challenge, we use student-level data and a Difference-in-Difference (DD) approach (Angrist and Pischke 2008; Legewie 2012; Meyer 1995) with additional control variables for prior crime and in some models student fixed-effect terms. This approach exploits the longitudinal nature of the data and variation in the timing of police surges across neighborhoods together with some of the same data on crime used by the NYPD in the selection of impact zones. It focuses on student's residential context and relies on the fact that Operation Impact was rolled out over 15 phases by expanding, moving, removing or adding impact zones roughly every six months. We restrict the sample to areas that were designated as impact zones at some point over the almost 10 years duration of the program to ensure a comparison between similar neighborhoods.[5] As a result, our approach compares changes in test scores before, during and

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[5] We also considered two alternative sample definitions that would imply different control groups. First, we considered no sample restrictions so that the trend in treatment areas would be compared to all other areas. Second, we considered restricting the sample to all areas with a high crime rate prior to the introduction of Operation Impact. This approach would use high crime areas that are not designated as impact zones as the control trend. However,

after Operation Impact for students in areas affected by the intervention to the same difference for students in areas that are designated as impact zones at a different point in time. The models control for prior crime defined as the number of violent and property crimes in the six months before the selection of impact zones. This neighborhood-level, pre-treatment control variable is important because the selection of impact zones was largely based on crime rates. The measures are based on the same crime data used by the NYPD and focus on the period during which decisions about changes to impact zones were made. They are temporally prior to the treatment ensuring that they are unaffected by the treatment itself.[6] In addition, the DD model adjusts for all time-constant differences across neighborhoods. It adjusts for stable differences in crime, policing, and test performance (but not important changes), as well as crime history, population characteristics such as the poverty rate and population size (but not population change) and housing structure including the presence of public housing. Although crime declined significantly in New York City, the differences across neighborhoods remained relatively stable. The neighborhood fixed-effect term adjusts for these stable neighborhood differences such as historic crime patterns that might still be important for contemporary perceptions of neighborhoods. While officially the selection of impact zones was solely based on crime patterns and history, population characteristics and housing structure are potentially relevant factors as well so that the neighborhood fixed effect term mitigates confounding bias.

Formally, we estimate two regression models separately by race and gender with clustered standard errors on the neighborhood level to address potential serial correlation problems.[7] The first model is a group-level difference-in-difference model without any covariates on the student level (aside from the dependent variable)[8]:

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both alternative definitions of the control group do not perform well in sensitivity analysis (i.e. the trend in test scores already diverges before areas were designated as impact zones).

[6] Controlling for lagged crime might be endogenous in later periods of Operation Impact. To address this concern, the appendix also presents results without prior crime as a control variable.

[7] Accounting for serial correlation is important in many Difference-in-Difference settings to ensure that the resulting standard errors are consistent (Bertrand, Duflo, and Mullainathan 2004). A simple approach to address this problem in settings with a large number of groups are standard errors clustered on the group or in our context neighborhood level instead of the group-by-year level (Angrist and Pischke 2008:237). We adopt this approach for the two Difference-in-Difference models described here. In general, the results are consistent across different methods of calculating standard errors.

[8] All models are estimated using a generalization of the within (fixed-effect) estimator for multiple high dimensional categorical variables using the lfe package in R (Gaure 2013). In particular, we use the within transformation for the neighborhood and grade-by-year fixed-effect terms and in some models for the student fixed-effect term.

$$y_{ijtg} = \pi_j + \eta_{tg} + \delta_1 D_{jt} + \boldsymbol{\beta_2 U}_{jt} + \varepsilon_{ijtg} \tag{1}$$

The second is a student-level DD estimator that adds a student fixed-effect term α_i and time-specific, student-level control variables $\boldsymbol{\beta_1 X}_{it}$

$$y_{ijtg} = \alpha_i + \pi_j + \eta_{tg} + \delta_1 D_{jt} + \boldsymbol{\beta_1 X}_{it} + \boldsymbol{\beta_2 U}_{jt} + \varepsilon_{ijtg} \tag{2}$$

where the dependent variables are English Language Arts (ELA) and mathematics test scores for student i, in neighborhood j, schoolyear t and grade g. The treatment variable D_{jt} is on the neighborhood-year level and measures the number of days a student lived in an impact zone during the school year scaled to one year. The corresponding coefficient δ_1 estimates the effect of Operation Impact D_{jt}. To obtain age-specific estimates, we either extend the model with a series of interaction terms $\delta_2 D_{jt} Age10_{it} + \cdots + \delta_7 D_{jt} Age15_{it}$ (main results) or run separate regressions for specific age groups (some additional analysis). In addition to the treatment indicator for Operation Impact, these models include a stable neighborhood effect π_j that controls for mean differences in test scores across neighborhoods, and a grade-by-year effect η_{tg} that captures test-scores differences across years and grades that are constant across all students such as the characteristics of a particular test. The student-level DD estimator in equation (2) also includes a student fixed-effect term α_i. The term adjusts for the selection of students into impact zones based on stable, observed and unobserved student characteristics. The individual-level fixed effect term means that all estimates are based on within-student variation over the years and reflect changes relative to the individual-level mean. The additional specification safeguards our analyses against other types of bias, reaffirms our findings based on different specifications and assumptions, and improves the precision of the estimates. Both models include the same time-varying covariates on the neighborhood-level $\boldsymbol{\beta_2 U}_{jt}$ for the number of violent and property crimes in the six months before the selection of impact zones, and the student-level DD estimator in equation (2) includes individual-level covariates $\boldsymbol{\beta_1 X}_{it}$ for free or reduced lunch as a measure of parental background and English learner status. The within students' variation in both variables is small but might capture important changes in family income and improvements in English

ability for non-native speakers. The appendix presents results from additional specifications including a model with a school-fixed effect term, a neighborhood-specific, linear time trend $\gamma_j year$, additional control variables for the prior level of police activity and a model without controls for prior crime (see Appendix for details).

We later extend these models with two lead and lag terms $\delta_{t\pm x} D_{j,t\pm x}$ that estimate changes in test scores before areas were designated as impact zones and after they were removed from the program (Angrist and Pischke 2008:237). The lead and lag terms are equal to one only in the relevant year. For example, for students in a neighborhood that is designated as an impact zone from July 2006 to July 2008, the lead term $D_{j,t+2}$ is coded as one for the 2004/2005 school year and the lead term $D_{j,t+1}$ for the 2005/2006 school year. The treatment indicator D_{jt} is coded as one for the school year 2006/2007 and 2007/2008 because students are exposed to Operation Impact for the entire school year. Finally, the lagged terms $D_{j,t-1}$ and $D_{j,t-2}$ are coded as one in the 2008/2009 and 2009/2010 school year respectively. The variables are defined at the neighborhood level and assigned to students based on their current neighborhood even if they are not part of the sample in pervious or future years. This approach assumes that students did not move but ensures that the sample size is sufficient to support the analysis. This specification allows us to estimate the effect of Operation Impact before (lead), during (treatment indicator) and after (lag) areas are designated as impact zones.

The core assumption of our Difference-in-Difference approach is that in the absence of Operation Impact and conditional on prior crime changes in test-scores of students exposed to the police surge would have been the same as changes in test scores of students in control areas (common trend assumption). The results section further discusses the plausibility of our approach and presents additional evidence to support the credibility of our design.

Examining the underlying mechanisms
As a second step of our analysis, we examine some of the underlying mechanisms that might explain the effect of Operation Impact on educational outcomes. These analyses focus on changes in crime, school-related attitudes and school attendance. The measures are more proximate causes of educational performance related to our theoretical argument about a positive effect based on crime reduction, and a negative effect based on trust in schools, and system avoidance.

First, we explore the possibility of a positive effect through the reduction of neighborhood crime and violence, which in turn increases school performance. The analysis is based on a similar Difference-in-Difference approach as our main analysis for student outcomes but uses data on the neighborhood-quarter level so that each observation (row) represents a specific neighborhood j in quarter q; where quarter ranges from Q1 in 2004 to Q4 in 2012 (36 quarters in total). The dependent variables are the number of violent and property crimes in neighborhood j and quarter q. The treatment indicator is coded as one when neighborhood j and quarter q are part of Operation Impact. Additional variables include four lead terms and four lag terms to estimate changes in crime before areas are designated as impact zones and after they are removed from the program (Angrist and Pischke 2008:237). We restrict the sample to neighborhoods that are designated as impact zones at some point over the duration of the program and areas with at least one student. This sample restriction ensures that the analysis focuses on the same areas as the main analysis discussed above. To model the number of violent and crime incidents, we use negative binomial regressions, which are a common approach in research on crime (Osgood 2000). The models assess the causal impact of Operation Impact on crime by comparing changes in crime before, during and after Operation Impact for areas affected by the intervention to the same difference for areas that are designated as impact zones at a different point in time. The Appendix includes a detailed discussion of this approach.

Second, we estimate the effect of Operation Impact on school-related attitudes, and school attendance as possible negative channels based on trust in schools and teachers, and system avoidance. For this purpose, we turn to the NYC Learning Environment Survey and administrative data on school attendance to estimate the effect of Operation Impact on school-related attitudes and attendance. Formally, we estimate the Difference-in-Difference models described in equation (1) and (2) using school-related attitudes and attendance rate as outcome variables (see variable descriptions below). The models include two lead and lag terms for the treatment indicator that allow us to examine changes in attitudes and attendance before, during and after areas were designated as impact zones. The analysis for school-related attitudes restricts the sample to grade 6-8 and the years 2007-2012 because the NYC Learning Environment Survey did not collect data for earlier periods and other grade levels.

Coding of Variables

The main *outcome variables* are test scores from the NYS English Language Arts (ELA) and Mathematics Test taken by students every spring in grades 3 through 8. The statewide tests are mandated by the No Child Left Behind law and developed by McGraw-Hill and Pearson in 2012. They are high-stakes exams administered in the spring term. All public-school students who are not excused for medical reasons or because of severe disabilities are required to take the ELA and mathematics exams. The ELA exam assesses three learning standards: information and understanding, literary response and expression, critical analysis and evaluation. It includes reading and listening sections as well as a short editing task with multiple-choice items and short-response answers. Depending on the grade level, the mathematics test consists of questions on number sense and operations, algebra, geometry, measurement, and statistics. The test scores are measured on a common scale using item response theory. To adjust for variations in the test across years and grades, we standardize the ELA and mathematics test scores to have mean zero and standard deviation one by year and grade across the entire New York City sample.

The *treatment indicator* is defined on the neighborhood-school year level and measures exposure to Operation Impact during the school year. It is a continuous variable that is defined as the number of days an area was part of Operation Impact during the school year scaled to one year so that it ranges from 0 (not exposed) to 1 (exposed for the entire school year). We link this neighborhood-level indicator to student records based on geocoded student addresses for the spring term of each school year.[9] The definition focuses on current exposure. Students who were exposed in the past either because they moved or because their residential neighborhood was removed from the program are coded as "not exposed". In additional analysis, we extend our model with lagged terms to estimate the effect of previous exposure and assess the temporal duration of the effect.

For the purpose of this study, *neighborhoods* are created by splitting 76 police precincts into 1,257 distinct areas with at least one student based on the boundaries of impact zones. As a comparison, there are 2,166 census tracts in New York City. Splitting police precincts by impact zones ensures that areas are aligned with impact zones as the central level of the intervention

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[9] The geocoding of student addresses was done by the Research Alliance for New York City School Research, which provided us with access to neighborhood identifiers and x- and y-coordinates with random noise to ensure privacy. Geographic information is missing for the 2004 school year (October 2003 and June 2004). These data are unrecoverable by the NYCDOE. To address this problem, we replace the missing 2004 data with the closest available information from either June 2003 or October 2004.

studied in this article. The modified police precincts provide a closer approximation to Operation Impact, policing and crime compared to other geographic units such as census tracts.

The *control variables* include student and neighborhood-level covariates. On the student-level, we control for student age, an indicator for free or reduced lunch and English learner status.[10] Most analyses are conducted separately by race and gender. On the neighborhood-level, we control for the number of violent and property crime in the six months before the selection of impact zones. Prior crime is an essential covariate because officially the selection of impact zones was solely based on crime patterns and history.

Additional analyses focus on crime, school-related attitudes and school attendance as possible mechanisms. The three measures are defined in the following way. First, we estimate the impact of Operation Impact on the number of *violent* and *property crimes* in neighborhood j during quarter q. The level of analysis and crime measures are distinct from the control variables discussed above. The measures are aggregated from incident-level crime data reported by the NYPD. While police-reported crime data is limited in many ways (Lynch and Addington 2006), the measures afford an important opportunity to examine changes in crime related to Operation Impact as a possible mechanism. Second, we estimate the effect of Operation Impact on *school-related attitudes and trust* using survey data from the NYC Learning Environment Survey.[11] The student questionnaire from the NYC Learning Environment Survey includes a range of questions on students' attitudes toward their school, teachers and other school staff. We use exploratory maximum likelihood factor analysis to construct the measure "positive school attitudes and trust" from five items. The five questions are measured on a four-point Likert scale ranging either from strongly disagree to strongly agree or from uncomfortable to comfortable. They include: "I feel welcome in my school" (factor loading 0.61), "Discipline in my school is fair" (factor loading 0.52), "My teachers inspire me to learn" (factor loading 0.60), "The adults at my school look out for me" (factor loading 0.64) and "The adults at my school help me understand what I need to do

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[10] Free or reduced lunch and English learner status are the two variables with the highest proportion of missing cases in our analytic sample. Many of these cases are for single years with complete student information for other years. We impute missing information for free or reduced lunch and English learner status with the lagged or lead value if it is missing.

[11] In 2007, the NYCDOE started to conduct the *NYC Learning Environment Survey*, which is a yearly student, parent and teacher surveys in grades 6-12. The survey focuses on the learning environment at each school and covers four categories used for reporting as part of the yearly school-level progress report. The four areas are Academic Expectations, Communication, Engagement, and Safety & Respect. The NYCDOE asks all parents and students in grades 6-12, and all teachers to participate in the survey. The response rate among students was 65% in 2007 and increased to 82% in 2012.

to succeed in school" (factor loading 0.62). The variables cover different aspect of school-related attitudes and trust. Our results suggest that the five items belong to the same factor.[12] The factor score from the exploratory maximum likelihood factor analysis can be understood as an index for positive attitudes and trust towards school and teachers. Third, we use *school attendance* to measure system avoidance based on administrative data from the NYCDOE. Our measure is defined as the attendance rate for a specific school year using the days on the roll as the denominator. We scale the variable from 0% to 100%. On average, the attendance rate is 92% with a standard deviation of 7.6.

Sample and Summary Statistics

We restricted student data in several ways to obtain our primary analysis sample of 285,439 students who were exposed to Operation Impact with over 827,922 student-year observations. First, we restrict our sample to African-American and Hispanic students because the sample size of White and Asian students living in impact zones is too small to support our analysis.[13] Second, we exclude students who did not participate in the yearly state test in grade 3-8. Third, we limit our analysis to students aged 9 – 15. The number of cases for younger and older students is insufficient to obtain age-specific estimates. Fourth, as part of our estimation strategy, we restrict our sample to areas that were designated as impact zones at least once. Finally, we exclude 2.6% of observations with missing data on any of the relevant variables.

Table A1 reports student-level summary statistics. It compares all students aged 9 to 15 who participated in the yearly state exam with our analytic sample restricted to areas that were part of Operation Impact at some point in time. The proportion of white students is far lower among students in impact zones whereas the share of African-American and Hispanic students is higher compared to the general student population. Students in impact zones are more likely to receive free or reduced lunch and score lower on the English Language Art and Mathematic

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[12] Only one of the factors has an eigenvalue above 1 (Kaiser criterion), all the variables have factor loadings above 0.5 (with most above 0.6), and common metrics such as the RMSEA index (0.056) and the Tucker Lewis Index (0.995) are below/above the standard thresholds.

[13] Appendix Table A2-A3 nonetheless show the results for white students. Some of the point estimates are comparable in size to the main findings reported in the article. However, the standard errors are extremely large, the estimates are inconsistent across different model specifications, there is no systematic pattern across age and gender (as expected by theory and other findings), and the sensitivity analysis indicate that the common trend assumption is violated for the sample of white students. Together, these findings challenge the validity of the estimates for white students presumably because of the number of white students exposed to impact zones is too small.

tests. Students in impact zones live in neighborhoods with a higher poverty rate, a smaller proportion of white residents, a higher share of minority groups, and substantially higher crime rates.

Results

We begin with two regression models that estimate the effect of Operation Impact on ELA test scores for African-American boys. Figure 1 presents the results and Appendix Table A2 and A3 show the corresponding regression tables. The findings are striking. African-American boys aged 9 and 10 are unaffected by the police surge with small and statistically insignificant effect estimates. As they grow older, however, African-American boys begin to experience a negative effect of living in areas that are designated as impact zones and subject to increased police activity. At age 12, this effect is modest in size and statistically significant for the Difference-in-Difference model with and without student fixed effects. For 13 to 15-year-old students, the effect is substantial. For 15-year-old students, exposure to impact zones for one school year decreased ELA test scores by -0.136 in the Difference-in-Difference model without student fixed effects and by -0.150 in our preferred model with student fixed effects. These estimates refer to the relative increase in the intensity of police activity related to Operation Impact of around 30% in pedestrian stops and arrests for low-level crimes. The size of this effect corresponds to a fifth of the black-white test score gap (0.72 standard deviations in our sample). It is similar in size to some of the most popular programs designed to increase educational achievement. For example, it is comparable to the lower end of some estimates for increasing teacher quality by 1 standard deviation (0.15–0.20 for reading test scores as reported by Rivkin, Hanushek, and Kain 2005; Rockoff 2004). While the effect is limited to specific demographic groups, these findings reveal a substantial impact of aggressive, order-maintenance policing on educational outcomes. They suggest that policing programs such as Operation Impact can wipe out the potential benefits of other costly interventions. The findings are consistent across a number of additional model specifications presented in Table A4. These specifications include a model with a school-fixed effect term, a neighborhood-specific, linear time trend, additional control variables for the prior level of police activity before the selection of impact zones, and a model without controls for prior crime (see Appendix for details).

In the next step of our analysis, we extend these findings by providing a sense of the temporal dynamics of the relation between Operation Impact and ELA test scores. To explore these dynamics, we estimate the same difference-in-difference models with additional leads and lags for the treatment

indicator, running from two years before to two years after a neighborhood was part of Operation Impact. We restrict the analysis to African-American boys aged 13 to 15 years as the group most affected by Operation Impact. Figure 2 presents the results. The estimates show no effect in the two years before Operation Impact. The effect size is small and statistically insignificant for both model specifications. Following the introduction of Operation Impact, however, test scores decreased substantially among African-American boys exposed to the policing program. The effect refers to the entire duration of the policing program in a particular area ranging from about half a year to 7.5 years. This negative effect dissipates over a two-year period after the end of the program but remains statistically significant. This temporal pattern is consistent with a causal interpretation of our results.

Figure 3 compares the results for African-American boys to African-American girls and Hispanic students by age. The findings show no discernible effect for these groups. The effect sizes are small for all and statistically insignificant for most of the estimated effects across age and student subgroups. There is no systematic pattern related to age and gender.[14] The difference in the effect size for African-American boys compared to both Hispanic boys and African-American girls is statistically significant at the 0.01 level for the ages 12, 13, 14 and 15. Figure A2 presents the corresponding results with leads and lags for 13-15-year-old students by race and gender. The results reaffirm the finding that the negative effect of Operation Impact is confined to older African-American boys. While some of the estimates are statistically significant, the point estimates are generally small, inconsistent across the two model specifications and show no clear temporal pattern related to Operation Impact.

Appendix Figure A3 presents the results for mathematic scores as the outcome variable. They show the same pattern for African-American boys with small or no effect for the younger age groups but an increasing effect size for older students. The effect size is somewhat smaller. In particular, exposure to impact zones for one school year decreased math test scores of 15-year old African-American boys by -0.127 in the group-level Difference-in-Difference model and by -0.075 in the student-level estimator with student fixed effects. The results for mathematics also show no evidence of an effect on African-American girls and Hispanic students. These findings for ELA and mathematic test scores indicate that Operation Impact negatively affected the education performance of African-Americans boys but not other groups.

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[14] The lack of any impact on Hispanic boys is surprising particularly for older students. However, Operation Impact increased the number of pedestrian stops by 35.1% for African-Americans compared to 25.2% for Hispanics with a similar pattern for misdemeanor and violation arrests (see Appendix for details). Accordingly, Hispanic students experienced Operation Impact at least somewhat differently compared to African-American students.

Plausibility of Design and Violent Crime as an Alternative Explanation

The core assumption of our Difference-in-Difference approach is that in the absence of Operation Impact and conditional on prior crime changes in test-scores of students exposed to the police surge would have been the same as changes in test scores of students in control areas (common trend assumption). The main threats to this assumption are time-specific, neighborhood-level factors that are related to the selection of impact zones and student outcomes but not captured with our measures for pre-treatment crime. We assess the plausibility of the common trend assumption by examining the pre-treatment trends in test scores. In particular, the analysis presented in Figure 2 uses leads and lags for the treatment indicator, running from two years before to two years after a neighborhood was part of Operation Impact. The estimates show no effect in the two years before Operation Impact. The effect size is small and statistically insignificant for both model specifications. This finding indicates that the trend in test scores is similar before areas were designated as impact zones. Accordingly, the main findings are not driven by neighborhood changes that are prior to Operation Impact such as increasing crime rates prior to the selection of impact zones. This finding is consistent with a causal interpretation of our results and supports the common trend assumption. In addition, we conduct a placebo analysis based on pre-treatment (lagged) test scores as the outcome variable (Athey and Imbens 2017). Pre-treatment test scores are known not to be affected by the treatment so that the true effect is zero. If, however, our estimates for the placebo analysis are not close to zero, our identification strategy would be implausible. The results are presented in Figure A4. They indicate that there is no discernible effect of impact zones on lagged test-scores for African-American boys or any of the other groups. The estimates are small and, in most cases, far from statistically significance. This finding increases the plausibility of our estimation strategy and suggests that our design can isolate the causal effect of impact zones on test scores.

The sensitivity analyses are encouraging but crime remains the most plausible alternative explanation for the effect of Operation Impact on African-American boys. While our models control for prior crime, the measures might not capture the full extent of police intelligence on particular areas and fail to adjust for other selection processes. They do not, for example, address the concern that NYPD selectively introduced Operation Impact in predominantly African-American areas with increasing crime rates. However, additional analyses contradict alternative explanations based on crime as a confounder.

First, the race, gender and age specific pattern in the effect of Operation Impact is closely aligned with exposure to policing. Figure 4 and Appendix Figure A5 show the stop and arrest rates respectively for each group. Similar to the size of the impact zone effect, the rate of police stops increases with age and is substantially lower for girls and Hispanics.[15] The effect of crime, however, is relatively constant across groups. In particular, we compare the pattern for the effect of Operation Impact with similar results for the effect of violent crime in the residential environment. The analysis uses the same model specification described above but replaces the main independent variable with the (standardized) number of violent felony crimes one month before the date of the standardized exam.[16] The findings are presented in Figure 5. In contrast to the results for policing, the estimates do not show a clear pattern by race, age and gender. Instead, the estimates are consistently negative and statistically significant for most groups. This finding suggests that neighborhood crime affects all students in similar ways. It indicates that the distinct race, gender, and age-specific pattern for the effect of Operation Impact is not driven by underlying crime rates. The group-specific patterns of the effects of Operation Impact compared to crime together with the pattern for police stops and arrests provide further support for the validity of our estimation strategy and discounts crime as an alternative explanation.

Second, local crime *declined* after the introduction of Operation Impact (see Figure 6 discussed below). Accordingly, exposure to Operation Impact coincided with a period during which the level of crime was lower compared to control areas. It remains possible that increased level of crime before areas were designated as impact zones exerts a lagged effect on student outcomes despite the contemporaneous decline in crime. However, it is implausible considering substantial research on the short-term effect of exposure to crime and violence (Sharkey 2010; Sharkey et al. 2014). Accordingly, the decreased level of crime in impact zones contradicts the idea that crime is driving the negative effect of Operation Impact.

Understanding the Effect of Operation Impact

To better understand the mechanisms that explain the effect of Operation Impact on educational outcomes, we examine changes in crime, school-related attitudes and school attendance. These

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[15] Also note that the negative effect is not confined to groups of students who are regularly stopped and arrested by the police. Indeed, the stop and arrest rate of African-American boys aged 12 and even 13 is relatively low; yet they begin to experience a negative effect of Operation Impact.

[16] The analysis also omits the pre-impact zone crime measures from the analysis.

measures are more proximate causes of educational performance related to our theoretical argument about a positive effect based on crime reduction, and a negative effect based on trust in schools and teachers, and system avoidance. Data limitations do not allow us to examine health-related mechanisms such as stress, fear and anxiety.

Violent and Property Crime

First, we explore the possibility of a positive effect through the reduction of neighborhood crime and violence, which in turn increases school performance. For this purpose, we examine changes in violent and property crime before, during and after Operation Impact (for a similar analysis see MacDonald et al. 2016). The analysis is based on a similar Difference-in-Difference approach as our main analysis for student outcomes but uses data on the neighborhood-quarter level and negative binomial regressions to model the number of crime incidents (Osgood 2000). The dependent variables are the number of police-reported violent and property crimes in neighborhood j and quarter q. Police-reported crime data is limited in important ways including potential changes in citizen reporting of crime related to Operation Impact and increased crime reporting as the result of additional police officers deployed to impact zones. The analysis nonetheless offers important insights in the changes of reported crime before, during and after Operation Impact. Figure 6 presents the results for the effect of Operation Impact on violent and property crimes together with estimated leads and lags, running from four quarters before to four quarters after a neighborhood was part of Operation Impact. The estimates show that the number of violent crimes was almost 10% higher in the two quarters before areas were designated as impact zones compared to control areas. This finding confirms that the NYPD selected impact zones based on the level of crime in the period leading up to the selection of impact zones. Our main analysis adjusts for this selection process by controlling for the level of crime in the six months before the selection of impact zones. After the implementation of Operation Impact in a particular area with the corresponding sharp increase in police activity in terms police stops and arrests for low-level offenses, the number of violent crimes decreased to about 5% below the level in control areas. This effect refers to the entire duration of the policing program in a particular area, which ranged from about 2 quarters to 7.5 years and is on average about one year. The reduction in crime, however, dissipates quickly after areas are removed from Operation Impact with violent crimes returning to the same level as in control areas in the second quarter after an

area was removed from Operation Impact. In contrast to violent crimes, property crimes were largely unaffected by Operation Impact and remained at the same level as control areas before, during and after the program.

These findings indicate that Operation Impact reduced violent crime although the effect was limited to the duration of the program. Together with substantial evidence that violent crime in the residential environment has a negative impact on cognitive development, school performance, mental health and long-term physical health (for an overview see Sharkey 2018a), these findings provide evidence for a potential positive channel. They suggest that Operation Impact might improve the educational prospects of children in high crime areas by reducing developmentally disruptive violent crime in the local environment. However, the main analysis clearly indicates that any positive effect through the reduction in crime is far exceeded by the negative consequences of aggressive, broken-window policing.

School-Related Attitudes and School Attendance

Second, we estimate the effect of Operation Impact on school-related attitudes and school attendance as possible negative channels. Broken-window policing programs such as Operation Impact might influence educational outcomes by undermining trust in schools and teachers, or by leading to system avoidance and withdrawal from institutions of social control such as schools. The analysis is based on the same Difference-in-Difference approach described above but uses a measure for positive attitudes towards school, and attendance rate as outcome variables.

Figure 7 reports the results for African-American boys aged 13-15 years. The findings for school-related attitudes show no evidence that Operation Impact influenced school-related attitudes. The effect estimates are small, statistically insignificant and the direction of the point estimates is inconsistent with our theoretical expectation. This result indicates that the negative consequences of Operation Impact are *not* driven by changes in school-related attitudes. However, Figure 7 provides evidence for the effect of Operation Impact on school attendance. The results show no effect before areas were designated as impact zones but a modest decrease after the introduction of Operation Impact, which flattens out in the years after the program ends. Figure A6 shows the same results by race and gender indicating that the reduced school attendance is confined to African-American boys. This pattern is consistent with a causal interpretation

of the results. The size of the effect indicates that Operation Impact reduced the attendance rate of African-American boys but not other groups by about 0.46 to 0.84 depending on the estimate, which correspondents to about 0.1 standard deviations or about 1.35 school days in a 180 days school year. Previous research consistently shows that lower attendance is related to performance on standardized tests, dropout and other educational outcomes (for research on NYC see Durán-Narucki 2008). While the effect size is modest or even small, the finding indicates that system avoidance is a possible mechanism by which Operation Impact reduced test scores for African-American boys.

Together, these analyses present first evidence about three possible mechanisms that might explain the effect of Operation Impact on educational outcomes. They show that Operation Impact decreased neighborhood crime, which might improve educational outcomes, did not influence school-related attitudes, and had a negative effect on school attendance. These findings indicate that system avoidance might partly explain the overall negative effect for African-American boys. The lack of health-related measures, however, prevents us from examining stress, fear and anxiety as plausible explanations of the negative effect.

Conclusion

In response to rising crime rates, police departments around the country implemented aggressive policing strategies and tactics often inspired by the broken windows theory of crime popularized by Wilson and Kelling (1982). Under Mayor Rudy Giuliani, the New York Police Department – the nation's largest municipal police force – pioneered these reforms in the early 1990s. While investments in policing including some forms of proactive policing are credited with reductions in crime, systematic research and empirical evidence on the social costs of policing is scarce (Weisburd and Majmundar 2018). We know little about the potential negative consequences of aggressive, broken-window policing for generations of African-American and Latino youth. The increasing exposure of minority youth to the police, however, makes it pivotal to understand the social influence of policing on individuals, neighborhoods, and communities at large.

This article focuses on the consequences of aggressive, broken-window policing for the educational performance of minority youth. The theoretical argument suggests that aggressive policing can either exacerbate racial inequality in educational attainment by disproportionately

targeting youth of color in high crime neighborhoods; or it can indirectly reduce educational inequality if it reduces violence and crime in risky communities. Exploiting the staggered implementation of Operation Impact in New York City and a Difference-in-Difference approach, we present the first causal evidence suggesting that this widely applied police model, which emphasizes extensive police contact at low levels of suspicious behavior, can lower the educational performance of African-American boys with implications for child development and racial inequality. The effect sizes vary by the race, gender and age of the student. It is substantial for African-American boys ages 13 to 15 and small and/or statistically insignificant for other groups. These race, ethnicity and gender gaps require further research on the consequences of policing for female, Hispanic and white students.

The findings advance our understanding about the role of the criminal justice system for youth development and racial/ethnic inequality. They complement recent research on how different forms of criminal justice contact such as arrest, conviction and incarceration influence mental and physical health, employment and other important outcomes. Most research on the link between the criminal justice system and child development focuses on parental incarceration even though law enforcement and policing are a central and the most visible part of the criminal justice system. Indeed, police are the face of the state and criminal justice system with a educative function for many youths especially minority youths in the most intensively policed neighborhoods (Justice and Meares 2014; Soss and Weaver 2017). The findings document how direct and indirect contacts with police can have spillover effects to the behaviors in other contexts of state authority and control. The focus on neighborhood-level exposure is particularly important. It shows that the consequences of the criminal justice system are not confined to those that are incarcerated, arrested or even stopped by the police and instead highlights that the consequences extend to entire communities. Considering the significant racial disparities in both individual and neighborhood-level police exposure (Fagan et al. 2010; Hagan et al. 2005; Legewie 2016), the findings suggest that aggressive policing strategies and tactics may perpetuate racial inequalities in educational outcomes. They provide evidence that the consequences of policing extend into key domains of social life, with implications for the educational trajectories of minority youth and social inequality more broadly.

These findings should encourage police reformers, policy makers and researchers to consider the broader implications and social costs of policing strategies and tactics. While investments in policing including some forms of proactive policing are credited with reductions in crime that might benefit students in high crime areas (Sharkey 2018b), the findings from this study and emerging evidence from other research indicate that aggressive policing influences a range of different outcomes and might harm students as well. Understanding these social costs of programs like Operation Impact is important for the design and implementation of policing programs that attempt to reduce crime and mitigate any negative consequences for minority youth. Today, the effectiveness of policing programs is regularly assessed based on crime rates. While our research focuses on New York City and Operation Impact as a prominent broken-window policing program, the findings inform new ways to assess the "effectiveness" of policing practices that are relevant for all policing programs. By combining large-scale administrative or "big" data from different government agencies with a rigorous research design, our work lays out an agenda for how to measure these long-term social consequences of policing across key domains of social life (Law and Legewie 2018). It suggests that a better understanding and regular assessment of the social consequences of policing should play a key role in the evaluation of police programs and police accountability.

Our research also points at a general set of processes that highlight how frequent and negative interactions with authority figures and the experience of discrimination can undermine educational and potentially other outcomes. We focus on students' residential context and ignore exposure to policing and other forms of social control in other contexts such as schools, non-residential neighborhoods or shopping malls. Indeed, many students attend school in a different neighborhood and/or spent significant time in other areas. Increasing surveillance and social control in schools or other settings might have similar adverse consequences and shape student's responses in important ways. From this perspective, our work lays out an agenda for how to test for these mechanisms. It encourages other researchers to examine the social costs of social control and surveillance across different settings.

However, we acknowledge several limitations. First, our data only allow us to test some of the underlying mechanisms. We are unable to examine health effects related to stress, fear, trauma and anxiety, and our attitudinal measures are only a first step. Future research should

address this limitation and examine how neighborhood level exposure to policing and the experience of police discrimination is related to child development and particularly the mental and physical health of youth. Second, we evaluate the effect of aggressive policing based on New York City and Operation Impact alone. The advantage of our design is that it overcomes several of the challenges in isolating the causal effect of policing. As in any study, external validity is not ensured. The policing strategies and tactics at the core of Operation Impact, however, are fairly representative of police reforms in major cities across the country (Fagan et al. 2016; Greene 2000; Weisburd and Majmundar 2018). In addition, several studies suggest that adolescents in suburban areas experience policing in much the same way as do urban teenagers (Beck 2017; Boyles 2015; Brunson and Weitzer 2009). There is less research on rural policing, where police contact is less frequent and crime rates may be lower, and where policing may be more embedded in the community compared to urban areas (Christensen and Crank 2001). Similarly, the setting in other countries with different racial histories, policing strategies, and educational systems is distinct limiting the potential to generalize the findings. These similarities and differences are not sufficient to draw conclusions, but the findings warrant future research on how crime and policing shape youth development in non-urban contexts and across the world.

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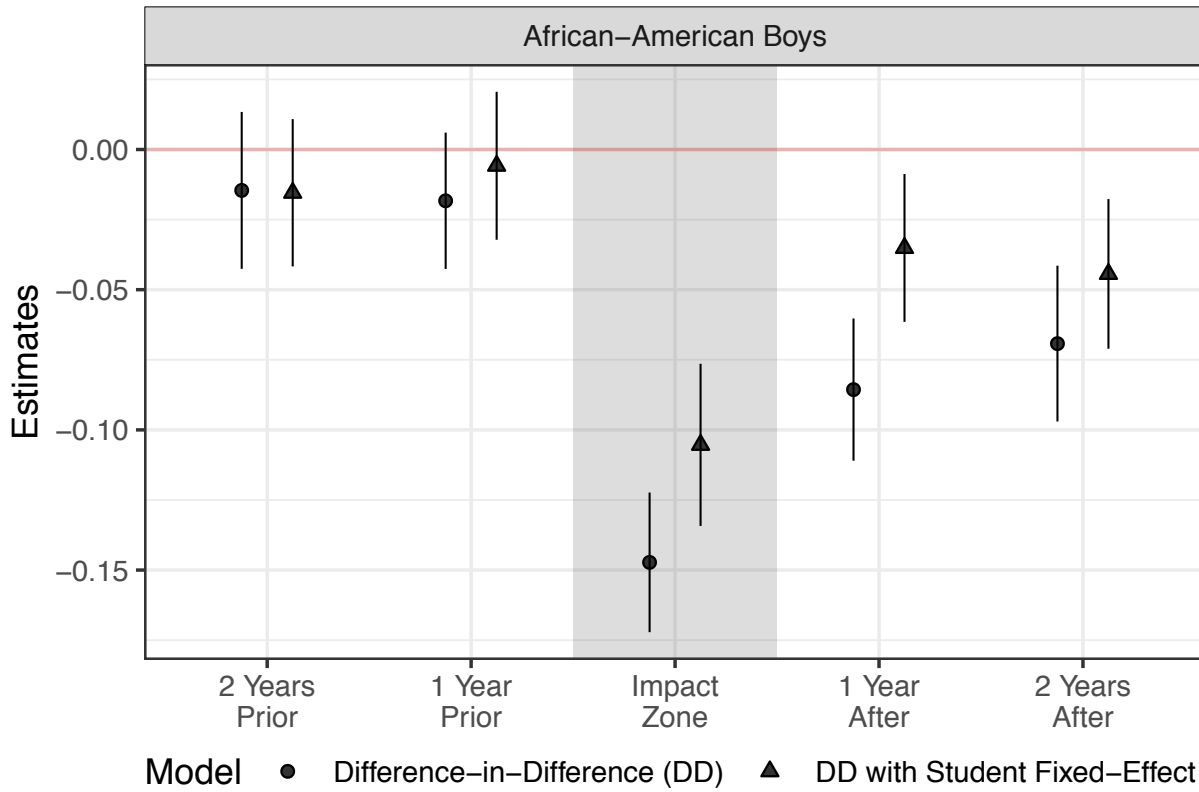
Tables and Figures

Figure 1. Effect of Operation Impact on English Language Arts (ELA) test scores by age for African-American Boys.
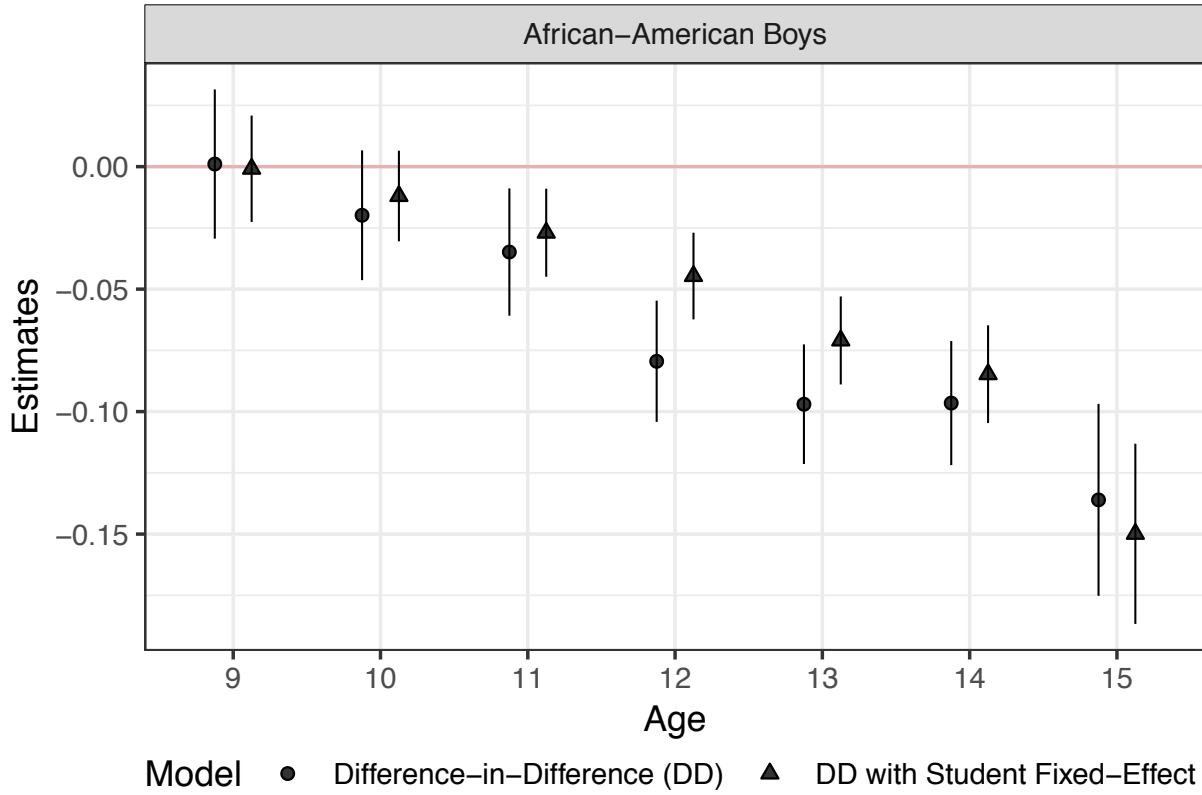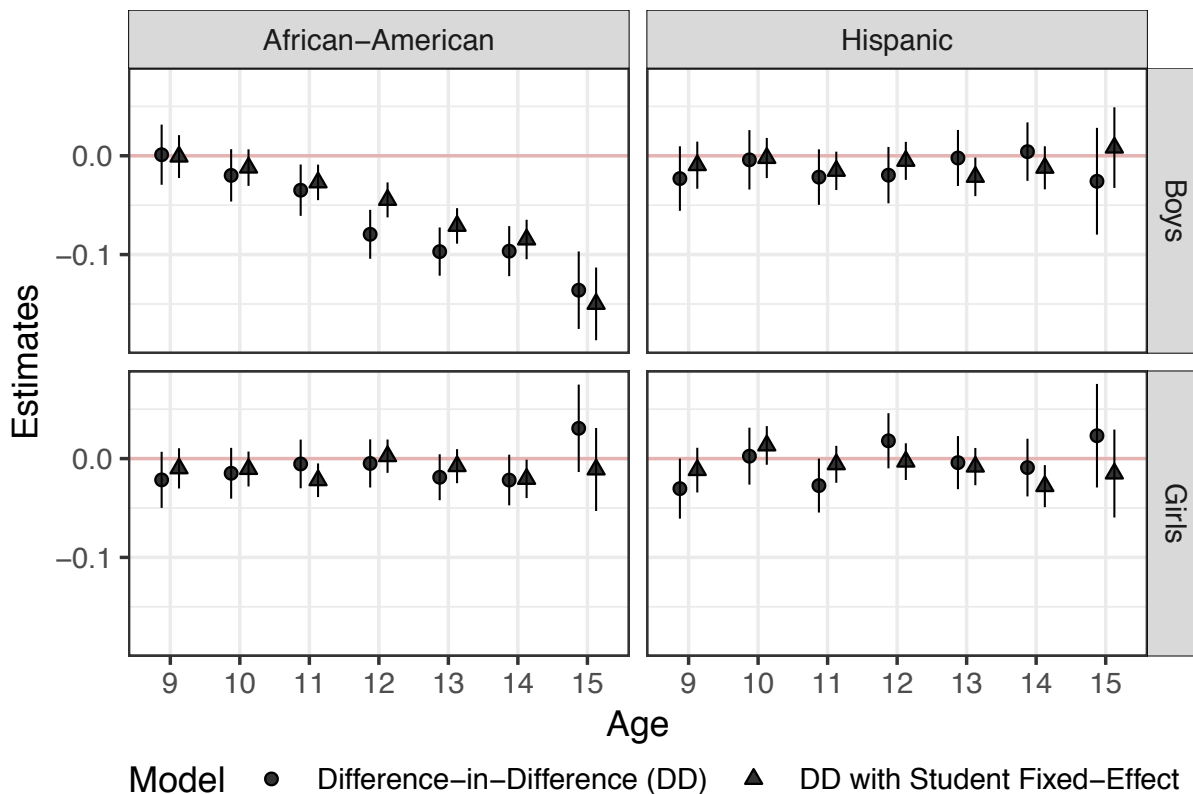

Note: Circles with lines represent point estimates with 95% confidence intervals. The sample is restricted to African-American boys (N= 195,743). The full regression results are presented in Table A2 and A3.

Figure 2. Effect of Operation Impact on English Language Arts (ELA) test scores for years before, during, and after Operation Impact, African-American Boys aged 13-15.
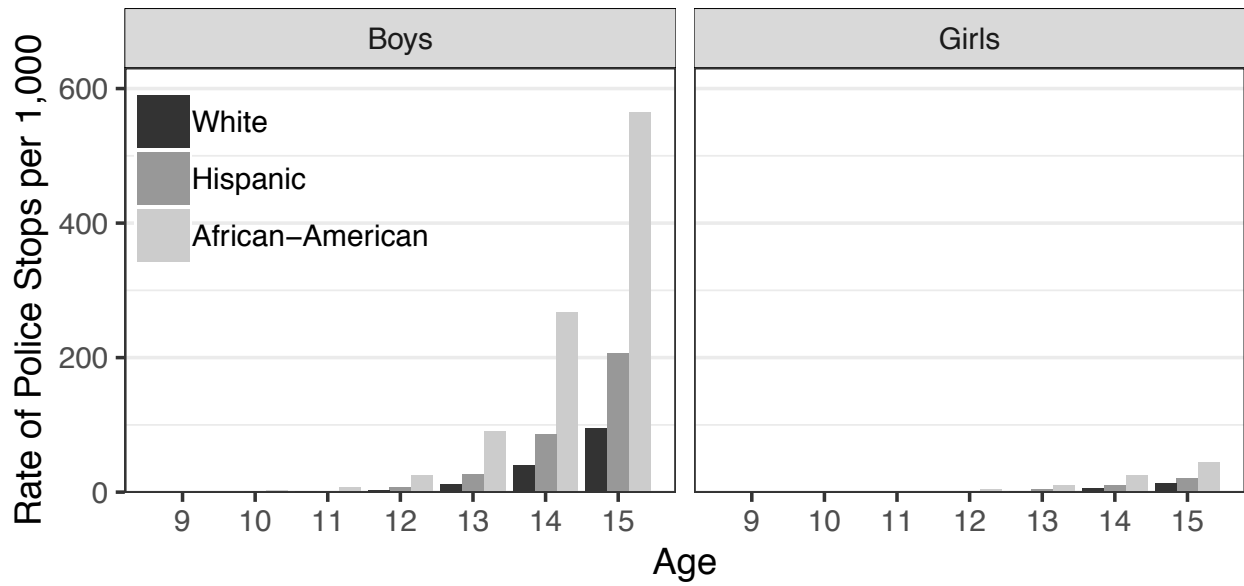

Note: Circles with lines represent point estimates with 95% confidence intervals. The sample is restricted to African-American boys aged 13-15 years (N=71,124).

Figure 3. Effect of Operation Impact on English Language Arts (ELA) test scores by race, gender and age.

Note: Circles with lines represent point estimates with 95% confidence intervals. The sample size is 195,743 for African-American boys, 210,566 for African-American girls, 200,830 for Hispanic boys, and 208,244 for Hispanic girls. Full regression tables are presented in Table A2 and A3.

Figure 4. Average yearly rate of police stops per 1,000 residents by race, gender and age, 2004 – 2012.

Note: Population data is from the 2010 Decennial census, Summary File 1, Table PCT012. The stop rate is averaged over the years so that it reflects the average rate of police steps per 1,000 residents between 2004 and 2012.

Figure 5. Effect of violent crime on English Language Arts (ELA) test scores by race, gender and age.
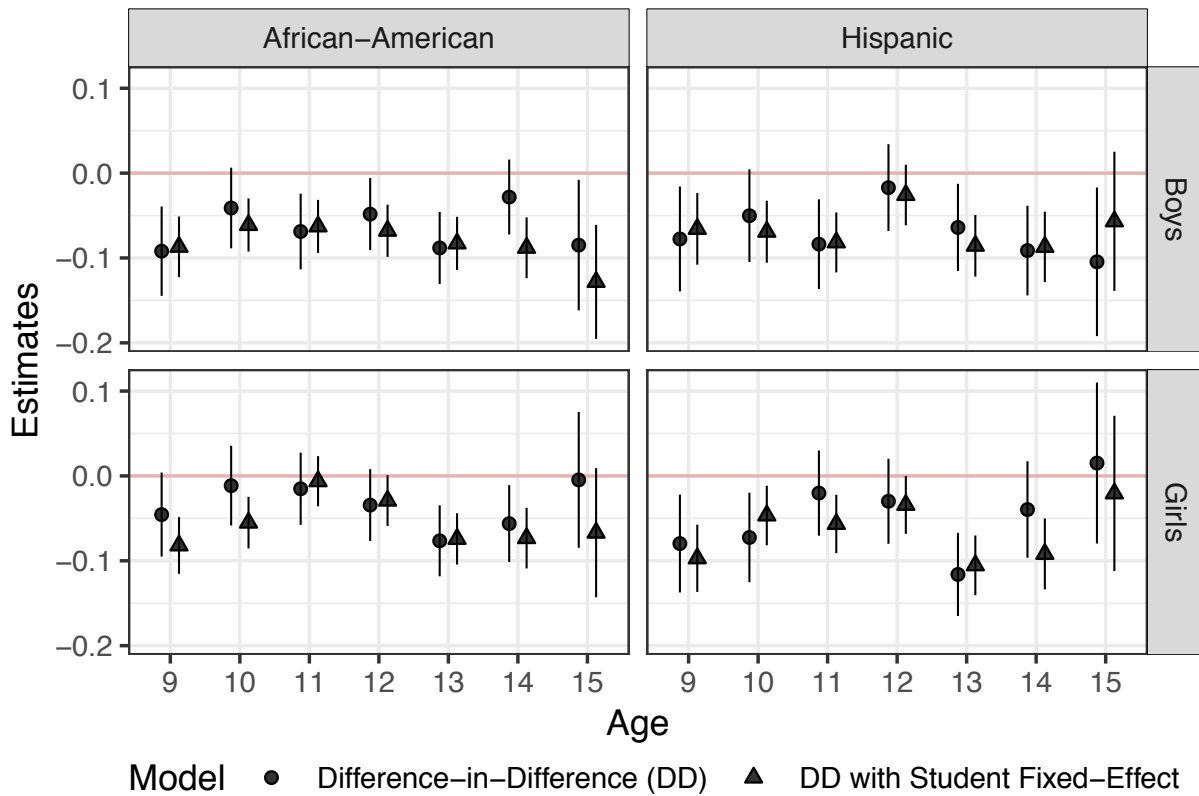
Note: Circles with lines represent point estimates with 95% confidence intervals. The sample size is 195,743 for African-American boys, 210,566 for African-American girls, 200,830 for Hispanic boys, and 208,244 for Hispanic girls.

Figure 6. Potential mechanism: Effect of Operation Impact on violent and property crimes.
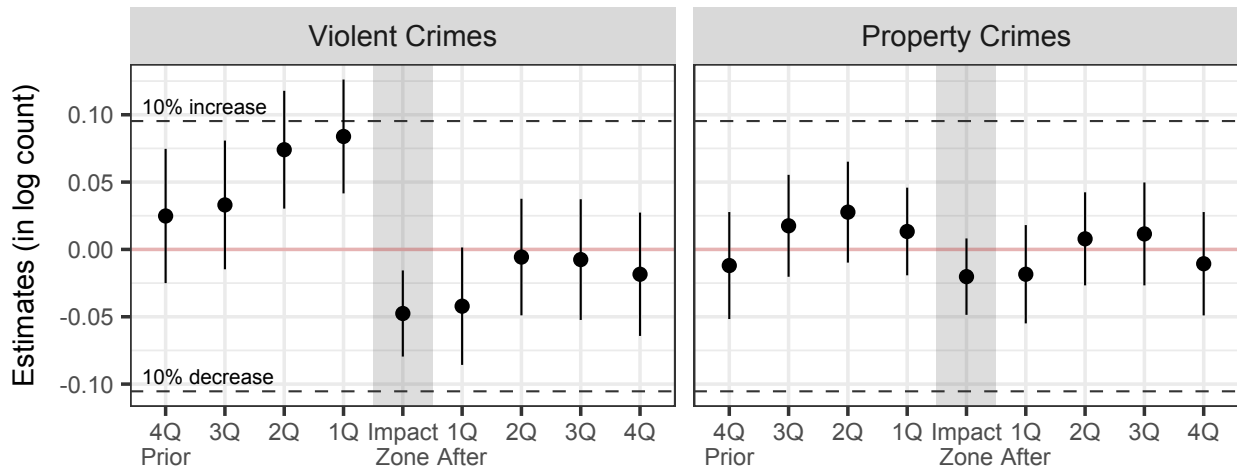
Note: Circles with lines represent point estimates with 95% confidence intervals from negative-binominal regression models based on a Difference-in-Difference framework. The analyses are based on the neighborhood-quarter level with a sample size of 42,552 for 1,182 neighborhoods times 36 quarters ranging from Q1 2004 to Q4 2012.

Figure 7. Potential Mechanism: Effect of Operation Impact on Attitudes towards School and Attendance Rate, African-American Boys aged 13-15.
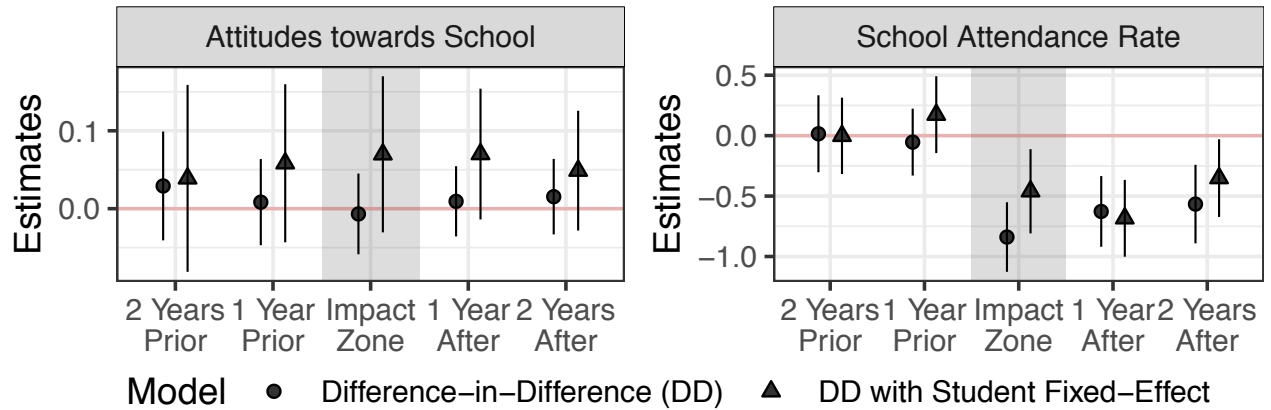
Note: Circles with lines represent point estimates with 95% confidence intervals from Difference-in-Difference models. The sample is restricted to African-American boys aged 13-15 years. The analysis for school-related attitudes uses data from 2007-2012 (N=28,050) and the analysis for attendance rate data from 2004-2012 (N=70,616).

Appendix

Effect of Operation Impact on Police Activity and Crime

To understand changes in police activity and crime before, during and after Operation Impact, we estimate the effect of Operation Impact on police activity and crime. The analysis is based on the neighborhood-quarter level so that each observation (row) represents a specific neighborhood j in quarter q; where quarter ranges from Q1 in 2004 to Q4 in 2012 (36 quarters in total). We restrict the sample to neighborhoods that are designated as impact zones at some point over the duration of the program and areas with at least one student. This sample restriction ensures that the analysis focuses on the same areas as the main analysis. We use six separate dependent variables to measure police activity and crime. These variables are the number of police stops, felony, misdemeanor and violation arrests, and the number of violent and property crimes in neighborhood j and quarter q. Our estimation strategy is based on a Difference-in-Difference approach (Angrist and Pischke 2008; Meyer 1995) and exploits the fact that Operation Impact was implemented in different neighborhoods at different points in time. It assesses the causal impact of Operation Impact by comparing changes in police activity and crime before, during and after Operation Impact for areas affected by the intervention to the same difference for areas that are designated as impact zones at a different point in time. To model the number of police stops, arrests and crime incidents, we use negative binomial regressions, which are a common approach in research on crime (Osgood 2000). Negative binomial regressions are appropriate for count data confined to positive integers and allow for over-dispersion across neighborhoods (Long and Freese 2005 Cha. 8). The models use clustered standard errors on the neighborhood level to address potential serial correlation problems (see footnote 7). The models are specified as

$$\lambda_{jq} = exp\big(\delta_j + \vartheta_q + \gamma_j time + \theta_{q+4}D_{j,q+4} + \cdots + \theta D_{j,q} + \cdots + \theta_{q-4}D_{j,q-4} + \varepsilon_{jq}\big) \quad (3)$$

where the dependent variables λ_{jq} are our six separate count variables for police activity and crime in neighborhood j and quarter q. The two fixed effect terms capture constant neighborhood effects δ_j and quarter-specific effects that are constant across all neighborhoods ϑ_q such as general changes in the NYPD's stop, question and frisk program, arrests or the level of crime.

$\gamma_j time$ refers to a neighborhood-specific, linear time trend where *time* indicates the quarter and ranges from 1 to 36. In the absence of impact zones, police activity and crime are determined by the sum of these two effects and the neighborhood-specific time trend.

The treatment indicator $D_{j,q}$ is coded as one when neighborhood j and quarter q are part of Operation Impact.[17] The corresponding coefficient θ estimates changes in police activity and crime related to Operation Impact. In addition, we include four lead terms $D_{j,q+x}$ and four lag terms $D_{j,q-x}$ to estimate changes in police-activity before areas are designated as impact zones and after they are removed from the program (Angrist and Pischke 2008:237). These indicator variables are equal to one only in the relevant quarter. For example, imagine a neighborhood that is designated as an impact zone from Q1 2006 to Q2 2007. For this area, the lead term $D_{j,q+4}$ is coded as one in Q1 2005, $D_{j,q+3}$ in Q2 etc. The treatment indicator $D_{j,q}$ is coded as one for all quarters between Q1 2006 to Q2 2007, and finally the lagged terms $D_{j,q-1}$ to $D_{j,q-4}$ are coded as one for Q3 2007 to Q2 2008 respectively. This specification allows us to track changed in police activity and crime before (lead), during (treatment indicator) and after (lag) areas are designated as impact zones.

Results

The main text of the manuscript presents the results on the effect of Operation Impact on crime in Figure 6. Here we present the results for changes in police activity before, during and after Operation Impact. Figure A7 shows the estimates for the effect of Operation Impact on police stops, felony, misdemeanor and violation arrests together with estimated leads and lags, running from four quarters before to four quarters after a neighborhood was part of Operation Impact. The estimates show no or little changes in police stops and different types of arrests in the four quarters (one year) before Operation Impact was introduced indicating that the level of police activity was the same in treatment and control areas in the month leading up to the introduction of Operation Impact. However, police activity increased sharply after the introduction of Operation Impact. The number of pedestrian stops increased by as much as 33.2% for the duration of

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[17] In most cases, impact zones were introduced around the beginning of the 1st or 3rd quarter so that quarters fall (almost) entirely in or outside of impact zones. For the remaining cases, we code an area as "impact zone" if the area was part of Operation Impact for at least half of the quarter.

the program.[18] Arrests for low-level offenses similarly increased substantially by 10% for misdemeanors and by 26% for violations whereas felony arrests remained largely the same. In Table A5, we further break down these changes by race. The results show that the increase in police activity related to Operation Impact is substantially larger for African-Americans than Hispanics and whites. In particular, the number of pedestrian stops increased by 35.1% for African-Americans, 25.2% for Hispanics and 22.2% for whites. The pattern for misdemeanors and violation arrests is similar with a substantially higher increase for African-Americans compared to the other groups. Finally, Figure A7 shows that the sharp increase in police activity quickly subsided after areas were removed from Operation Impact returning to the same level as control areas. These findings show that Operation Impact produced a sharp increase in policing activity particularly in terms of pedestrian stops and arrests for low-level offenses – key characteristics of proactive, broken-window policing.

Supplementary Analysis based on Alternative Model Specifications

Appendix Table A4 presents the results from supplementary analysis based on alternative model specifications that reaffirm our main findings. The analyses extend the Difference-in-Difference model with student fixed-effect terms described in equation (2) and are restricted to African-American boys. The first column repeats the results discussed in the main text and presented in Figure 1 and Table A3 (left column for African-American boys). The second column extends this model with a school fixed-effect term. This term controls for all observed and unobserved school characteristics such as school-based exposure to law enforcement activity. Column three reports the results from a model with a neighborhood-specific, linear time trend $\gamma_j year$. This specification accounts for time trends that differ between neighborhoods that are designated as impact zones at different points in time prior to the treatment (Morgan and Winship 2015; Vaisey and Miles 2014). Column four reports the results from a model with additional, neighborhood-level control variables for the level police activity in the six months prior to the treatment. The variables are the number of pedestrian stops, felony arrests and misdemeanor arrests. Similar to prior crime, they are pre-treatment measures of the level of police activity in a specific area. Finally, column five shows the results for a student-fixed effect model without controls for prior crime.

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[18] This interpretation is based on the incident rate ratios defined as the exponentiated coefficient.

This specification addresses concerns about the endogeneity of the prior crime measures in later years.

The results from these additional model specifications reaffirm the main findings discussed in the article. They show the same pattern with small and statistically insignificant effect estimates for African-American boys aged 9 to 11 but a substantial, negative effect for older students.

Tables A1 to A5

Table A1. Sample characteristics

	All Students	Students in Impact Zones
Students	835,531	285,439
Student-Year Observations	2,725,235	827,922
Avg. Observation per Student	3.26	2.9
Avg. Length of Exposure (in years)	0.26	0.77
Demographics		
Percent Female	50.2%	50.9%
Percent White	13.6%	1.6%
Percent African-American	38.7%	48.5%
Percent Hispanic	47.7%	49.9%
Percent Free/Reduced Lunch	96.4%	97.9%
Schooling		
Percent English Learner	11.3%	13.5%
English Language Art Score	661.2	653.6
Mathematics Score	674.8	666.4
Neighborhood Characteristics		
Percent Below Poverty Line	24.7%	35.0%
Percent White	22.8%	4.9%
Percent African-American	30.3%	44.4%
Percent Hispanic	33.5%	44.7%
Percent Asian	9.9%	3.3%
Violent Crime Rate	16.2	22.9
Property Crime Rate	9.8	10.0
Misdemeanors Crime Rate	28.7	33.3
Violations Crime Rate	8.5	10.8

Note: The sample is defined as all African-American, Hispanic or White students between 2004 and 2012 who are 9 to 15 years old, participated in the yearly state exam and were exposed to Operation Impact at least once. All characteristics are averaged over years and across students. Neighborhood characteristics are on the census tract level. Crime rates are per 1,000 residents.

Table A2. Difference in Difference model for the effect of Operation Impact on English Language Arts (ELA) test scores by race and gender.

	African-American		Hispanic		White	
	Boys	Girls	Boys	Girls	Boys	Girls
Operation Impact (OI)	0.001	-0.022	-0.023	-0.030*	0.144	0.054
	(0.02)	(0.01)	(0.02)	(0.02)	(0.13)	(0.12)
OI x Age 10	-0.021	0.007	0.019	0.033	-0.324	-0.075
	(0.02)	(0.02)	(0.02)	(0.02)	(0.17)	(0.17)
OI x Age 11	-0.036	0.016	0.001	0.003	-0.261	-0.054
	(0.02)	(0.02)	(0.02)	(0.02)	(0.18)	(0.17)
OI x Age 12	-0.081***	0.017	0.003	0.048*	-0.062	-0.142
	(0.02)	(0.02)	(0.02)	(0.02)	(0.17)	(0.19)
OI x Age 13	-0.098***	0.003	0.021	0.026	-0.145	-0.115
	(0.02)	(0.02)	(0.02)	(0.02)	(0.17)	(0.18)
OI x Age 14	-0.098***	0.000	0.027	0.021	-0.192	-0.160
	(0.02)	(0.02)	(0.02)	(0.02)	(0.18)	(0.19)
OI x Age 15	-0.137***	0.052	-0.003	0.054	-0.049	0.070
	(0.03)	(0.03)	(0.03)	(0.03)	(0.27)	(0.37)
Prior Violent Crime	0.000	0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Prior Property Crime	0.000*	0.000*	0.000**	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Neighborhood FE	✓	✓	✓	✓	✓	✓
Grade-by-year FE	✓	✓	✓	✓	✓	✓
Student FE						
Observations	195,743	210,566	200,830	208,244	6,657	5,882

*P < 0.05, **P < 0.01, ***P < 0.001

Table A3. Difference in Difference model with student fixed-effect term for the effect of impact zones on English Language Arts (ELA) test scores by race and gender.

	African-American		Hispanic		White	
	Boys	Girls	Boys	Girls	Boys	Girls
Operation Impact (OI)	-0.001	-0.010	-0.010	-0.012	-0.033	0.238*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.11)	(0.12)
OI x Age 10	-0.011	-0.001	0.007	0.025	0.022	-0.316*
	(0.01)	(0.01)	(0.02)	(0.02)	(0.14)	(0.15)
OI x Age 11	-0.026	-0.012	-0.006	0.006	0.182	-0.478**
	(0.01)	(0.01)	(0.02)	(0.02)	(0.14)	(0.15)
OI x Age 12	-0.044**	0.012	0.004	0.009	-0.033	-0.405**
	(0.01)	(0.01)	(0.02)	(0.02)	(0.14)	(0.16)
OI x Age 13	-0.070***	0.002	-0.012	0.004	0.061	-0.310
	(0.01)	(0.01)	(0.02)	(0.02)	(0.15)	(0.16)
OI x Age 14	-0.084***	-0.011	-0.003	-0.016	-0.156	-0.196
	(0.02)	(0.01)	(0.02)	(0.02)	(0.15)	(0.17)
OI x Age 15	-0.149***	-0.001	0.018	-0.003	0.147	-1.060
	(0.02)	(0.02)	(0.02)	(0.03)	(0.29)	(0.58)
Free Lunch Status	0.016	-0.016	0.030**	0.042***	0.074	-0.065
	(0.01)	(0.01)	(0.01)	(0.01)	(0.07)	(0.12)
English Learner Status	-0.124***	-0.149***	-0.068***	-0.062***	-0.175	-0.071
	(0.02)	(0.02)	(0.01)	(0.01)	(0.09)	(0.13)
Prior Violent Crime	0.000	0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Prior Property Crime	0.000	0.000*	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Neighborhood FE	✓	✓	✓	✓	✓	✓
Grade-by-year FE	✓	✓	✓	✓	✓	✓
Student FE	✓	✓	✓	✓	✓	✓
Observations	195,743	210,566	200,830	208,244	6,657	5,882

*P < 0.05, **P < 0.01, ***P < 0.001

Table A4. Supplementary analysis for the effect of Operation Impact on English Language Arts (ELA) test scores for African-American Boys

	DD model with student FE	...and school FE term	...and area-specific time trend	...and prior policing controls	...without prior crime
Operation Impact (OI)	-0.001	0.000	0.000	0.002	-0.001
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
OI x Age 10	-0.011	-0.011	-0.012	-0.012	-0.010
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
OI x Age 11	-0.026	-0.027	-0.027	-0.028	-0.024
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
OI x Age 12	-0.044**	-0.043**	-0.044**	-0.045**	-0.042**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
OI x Age 13	-0.070***	-0.068***	-0.071***	-0.071***	-0.069***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
OI x Age 14	-0.084***	-0.081***	-0.083***	-0.085***	-0.082***
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
OI x Age 15	-0.149***	-0.142***	-0.148***	-0.151***	-0.146***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Free Lunch Status	0.016	0.016	0.016	0.016	0.015
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
English Learner Status	-0.124***	-0.123***	-0.126***	-0.125***	-0.125***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Prior Violent Crime	0.000	0.000	0.000	0.000*	
	(0.00)	(0.00)	(0.00)	(0.00)	
Prior Property Crime	0.000	0.000	0.000	0.000	
	(0.00)	(0.00)	(0.00)	(0.00)	
Prior Police Stops				0.000***	
				(0.00)	
Prior Felony Arrests				0.000	
				(0.00)	
Prior Misd. Arrests				0.000	
				(0.00)	
Neighborhood FE	✓	✓	✓	✓	✓
Grade-by-year FE	✓	✓	✓	✓	✓
Student FE	✓	✓	✓	✓	✓
School FE		✓			
Neighborhood trend			✓		
Observations	195,743	195,743	195,743	195,743	195,743

*P < 0.05, **P < 0.01, ***P < 0.001

Table A5. Effect of Operation Impact on police activity by race.

		All	African-American	Hispanic	White
			Race		
Pedestrian Stops	Coefficient	0.29***	0.30***	0.23***	0.20***
	Std. Error	(0.03)	(0.03)	(0.03)	(0.05)
	% Change	33.2%	35.1%	25.2%	22.2%
Felony Arrests	Coefficient	0.02	0.01	0.01	0.13*
	Std. Error	(0.02)	(0.02)	(0.02)	(0.06)
	% Change	2.0%	1.2%	1.2%	13.9%
Misdemeanor Arrests	Coefficient	0.10***	0.11***	0.09***	0.10*
	Std. Error	(0.02)	(0.02)	(0.02)	(0.04)
	% Change	11.0%	12.2%	9.8%	10.2%
Violation Arrests	Coefficient	0.26***	0.29***	0.21***	0.23**
	Std. Error	(0.04)	(0.05)	(0.05)	(0.08)
	% Change	29.7%	33.4%	22.9%	25.4%

Note: The table reports the treatment effect $\theta D_{j,q}$ described in equation (3) and omits other regression coefficients. The estimates are based on negative-binominal regression models using a Difference-in-Difference framework and data on the neighborhood-quarter level. The sample size is 42,552 for 1,182 neighborhoods times 36 quarters ranging from Q1 2004 to Q4 2012.
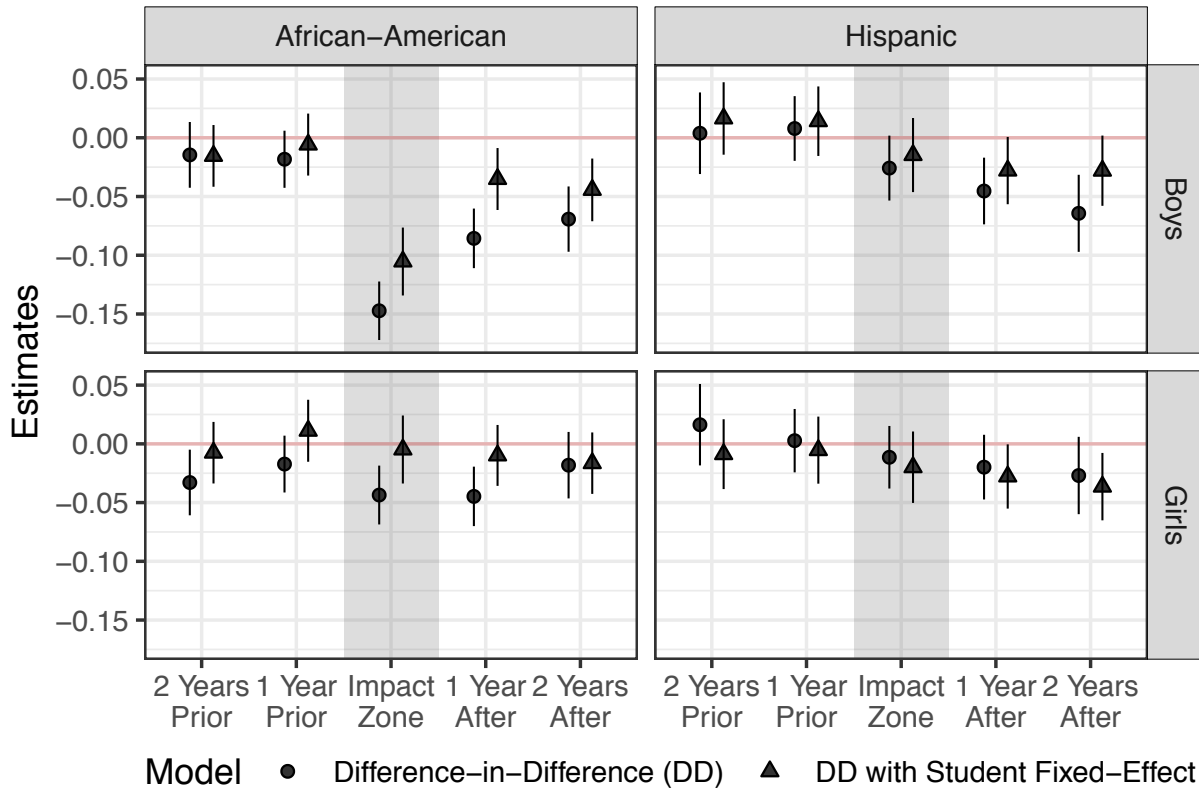*P < 0.05, **P < 0.01, ***P < 0.001

Figures A1 to A7

Figure A1. Rollout of NYPD Operation Impact over time.

Figure A2. Effect of Operation Impact on English Language Arts (ELA) test scores for years be-
fore, during, and after Operation Impact by race and gender for 13-15-year-old students.
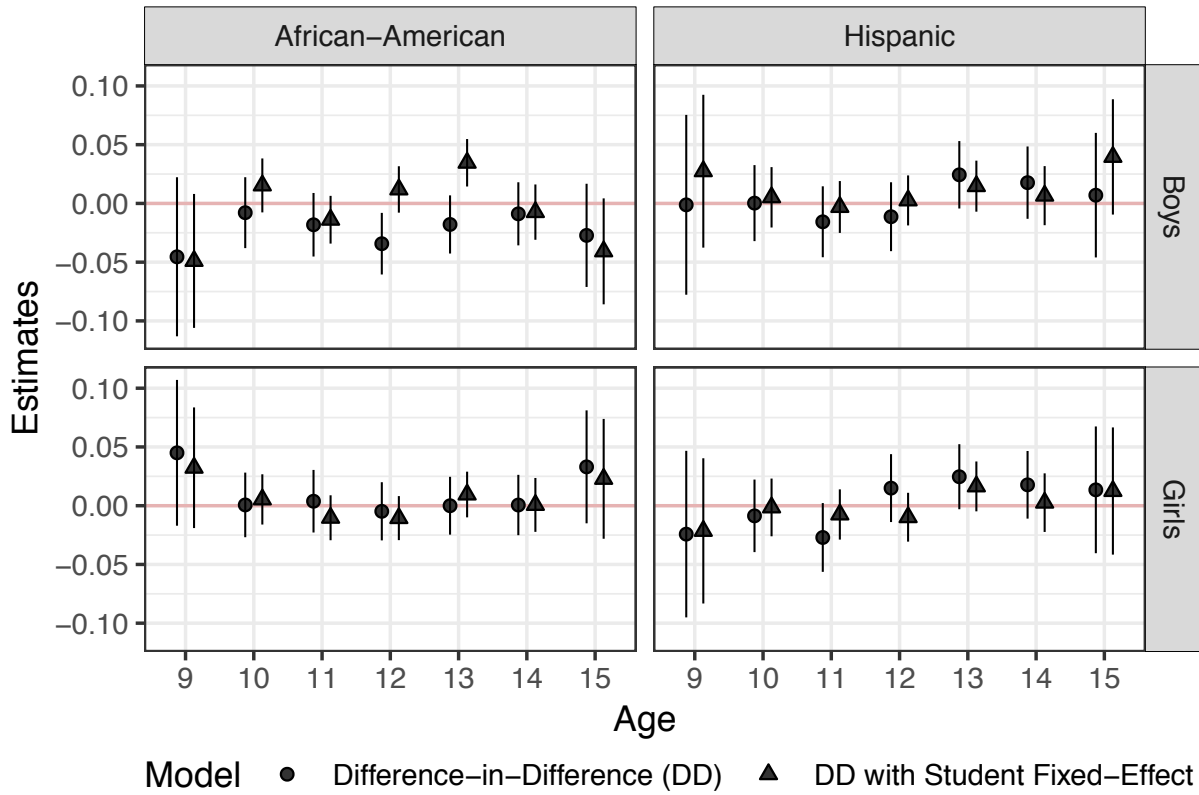

Note: Circles with lines represent point estimates with 95% confidence intervals. The sample is restricted to 13-15-
year-old students. The number of cases is 71,124 for African-American boys, 73,635 for African-American girls,
72,505 for Hispanic boys, 72,984 for Hispanic girls.

Figure A3. Effect of Operation Impact on Mathematics test scores by race, gender and age.
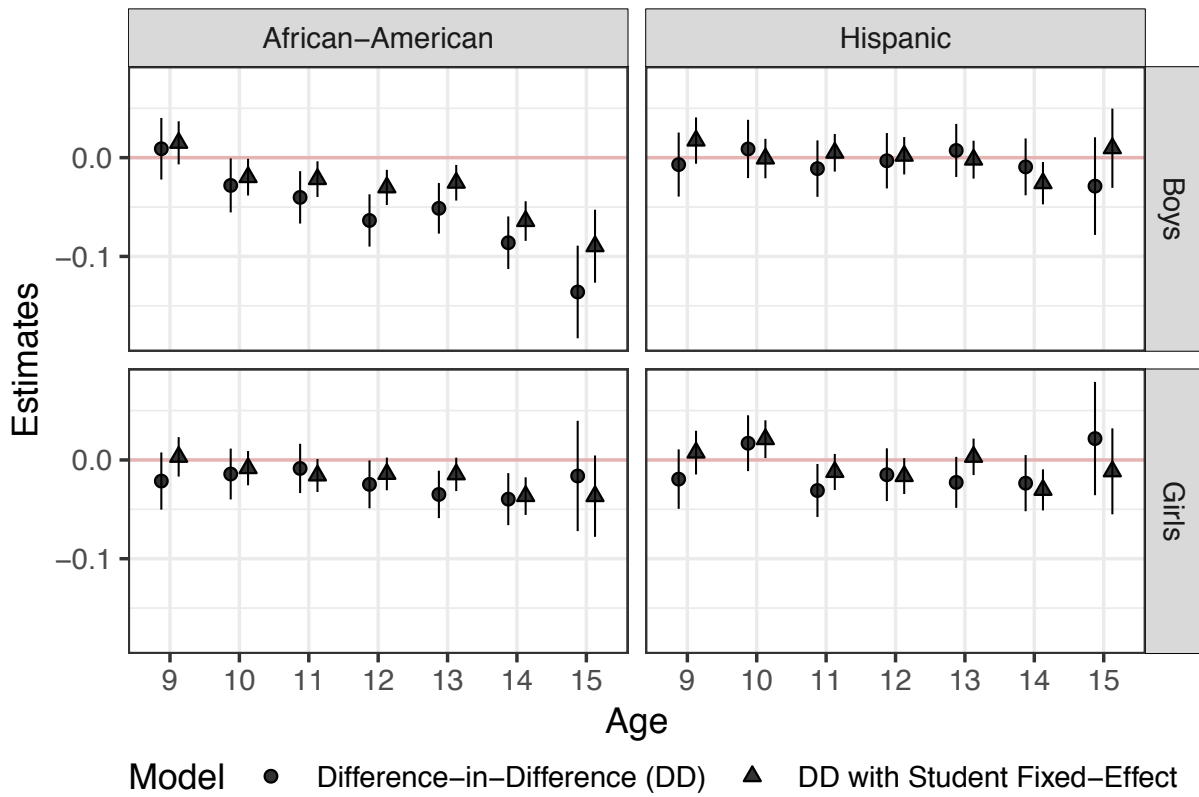
Note: Circles with lines represent point estimates with 95% confidence intervals. The sample size is 195,743 for African-American boys, 210,566 for African-American girls, 200,830 for Hispanic boys, and 208,244 for Hispanic girls.
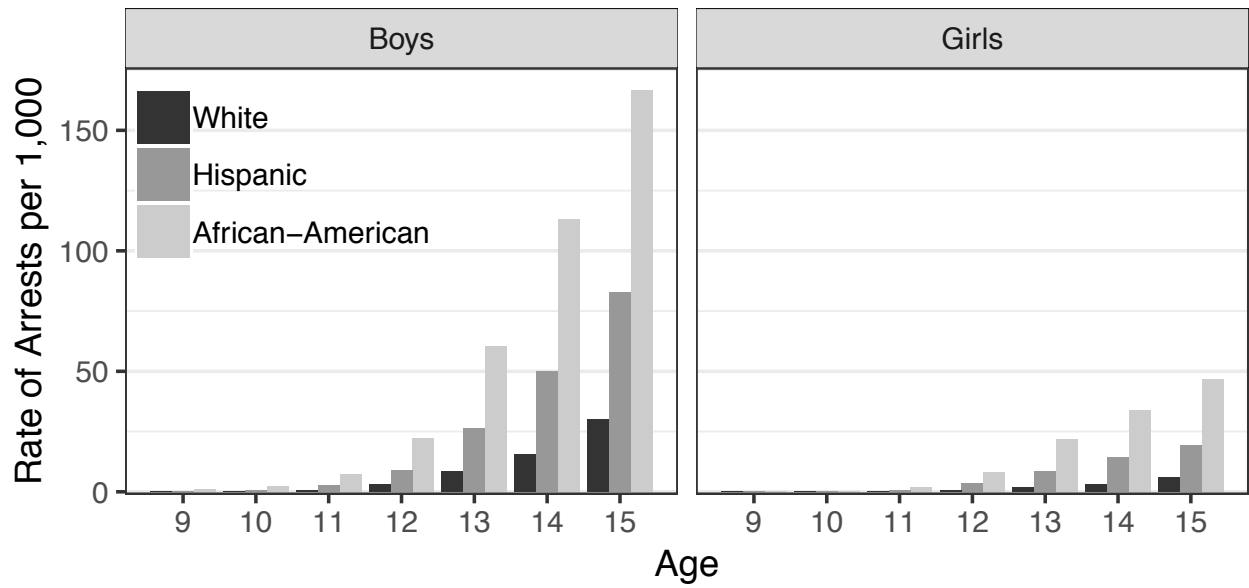
Figure A4. Placebo Analysis based on Lagged Outcome Variable.

Note: Circles with lines represent point estimates with 95% confidence intervals. The sample size is 139,318 for African-American boys, 152,334 for African-American girls, 140,625 for Hispanic boys, and 147,358 for Hispanic girls.

Figure A5. Average yearly rate of arrests per 1,000 residents by race, age, and gender, 2004 – 2012.
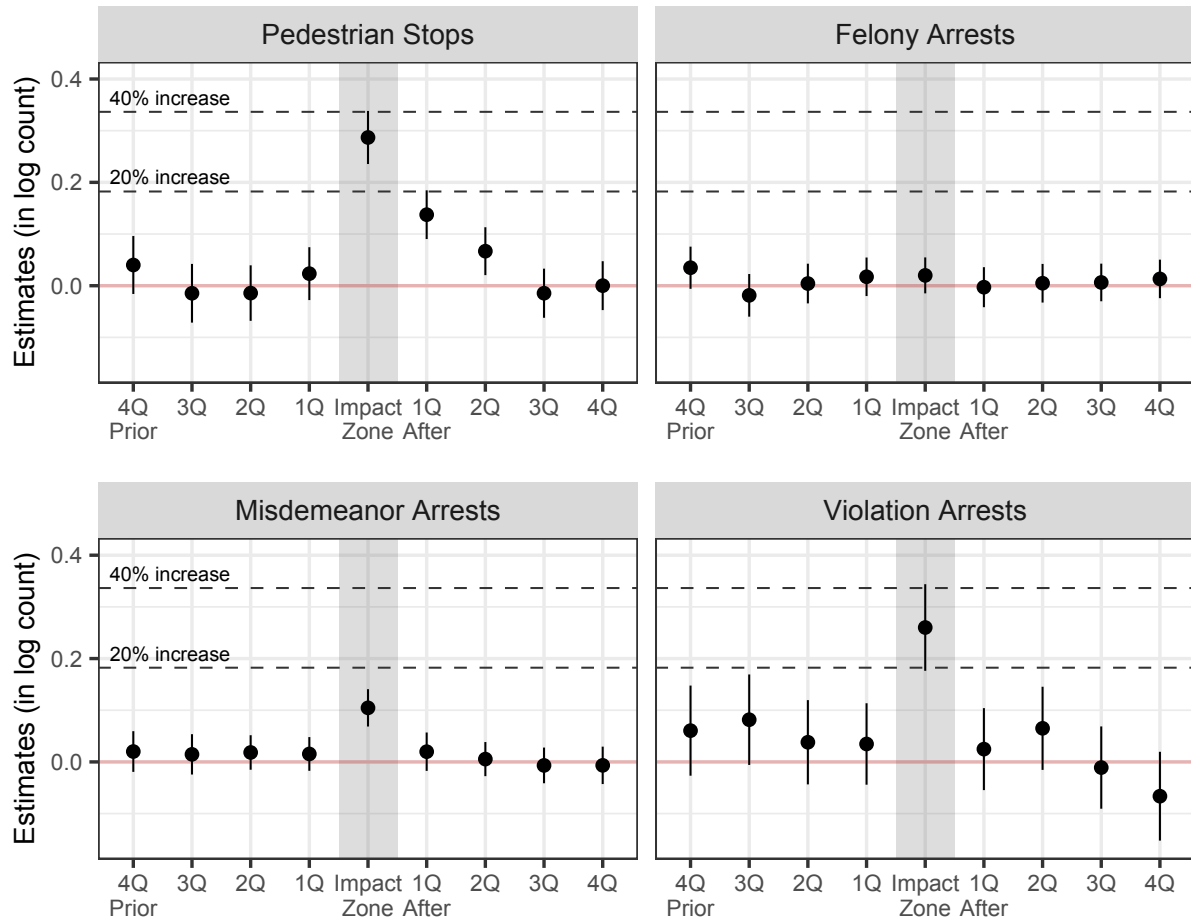

Note: Population data is from the 2010 Decennial census, Summary File 1, Table PCT012. The arrest rate is averaged over the years so that it reflects the average rate of police steps per 1,000 residents between 2004 and 2012.

Figure A6. Effect of Operation Impact on School Attendance Rate by race and gender, 13-15-year old students.
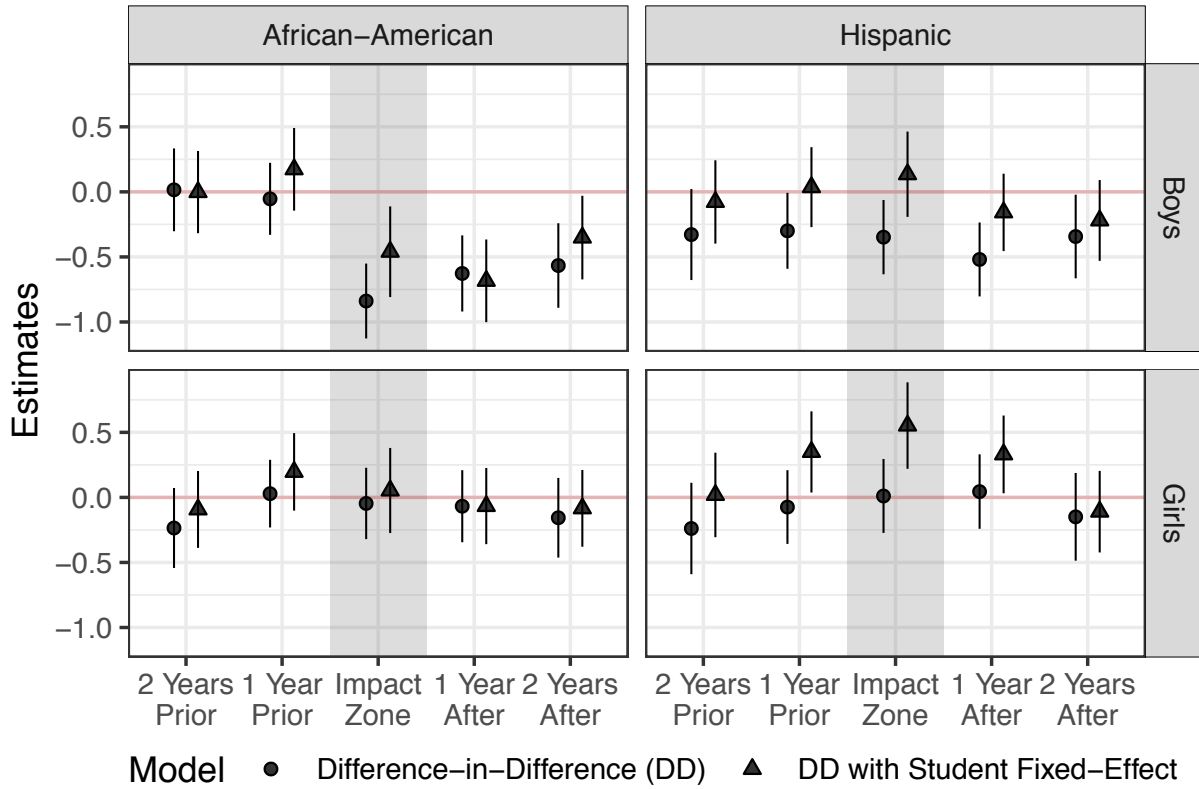
Note: Circles with lines represent point estimates with 95% confidence intervals. The sample is restricted to 13-15-year-old students. The number of cases is 70,616 for African-American boys, 73,168 for African-American girls, 72,277 for Hispanic boys, 72,756 for Hispanic girls.

Figure A7. Effect of Operation Impact on Police Activity.

Note: Circles with lines represent point estimates with 95% confidence intervals from negative-binominal regression models based on a Difference-in-Difference framework. The analyses are based on the neighborhood-quarter level with a sample size of 42,552 for 1,182 neighborhoods times 36 quarters ranging from Q1 2004 to Q4 2012.