Can Precision Policing Reduce Gun Violence? Evidence from "Gang Takedowns" in New York City*

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Abstract

During the last decade, while national homicide rates have remained flat, New York City has experienced a second great crime decline, with gun violence declining by more than 50 percent since 2011. In this paper, we investigate one potential explanation for this dramatic and unexpected improvement in public safety — the New York Police Department's rapid shift from a policy of mass enforcement, characterized by the intensive use of street stops and field interrogations, to a more surgical form of "precision policing", in which law enforcement focuses resources on a small number of individuals who are thought to be the primary drivers of violence. In anticipation of a 2013 federal court ruling in which the NYPD's stop and frisk practices were found to be unconstitutional, the number of official street stops made by NYPD officers declined from a high of 680,000 in 2011 to just 12,000 five years later. During the same period, the NYPD dramatically increased its investment in a different tactic — targeted enforcement actions against criminal gangs, often centered around the City's public housing communities. In this paper, we study New York City's campaign of "gang takedowns" in which suspected members of criminal gangs were arrested in highly coordinated raids and prosecuted on conspiracy charges. We show that gun violence in and around public housing communities fell by approximately 30 percent in the first year after a gang takedown. Our estimates imply that these coordinated gang takedowns explain nearly one quarter of the decline in gun violence in New York City's public housing communities over the last eight years.

Keywords: police, gang enforcement, crime

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1 Introduction

A large literature in both economics and criminology considers the impact of police resources on crime.[1] There is now a strong consensus that the number of police officers (Marvell and Moody, 1996; Evans and Owens, 2007; Chalfin and McCrary, 2018; Mello, 2019; Weisburst, 2019) combined with their presence and visibility (Sherman and Weisburd, 1995; Di Tella and Schargrodsky, 2004; Braga and Bond, 2008; Klick and Tabarrok, 2005; MacDonald et al., 2016; Weisburd, 2016) reduces crime — at least to a modest degree — and that these impacts are unlikely to be fully explained by increased incapacitation of offenders (Durlauf and Nagin, 2011; Nagin, 2013; Owens, 2013; Kaplan and Chalfin, 2019).[2] These findings — which offer optimism about the effectiveness and scalability of investments in police manpower — characterize the aftermath of a seismic shift in American policing, from an era in which police were primarily reactive, responding to calls for service while spending most of their time in a patrol car to an era in which police officers are expected to play an active role in preventing crimes.[3] Based on research which shows that visible police presence in hot spots leads to crime reductions without displac-

[1]This literature features prominently in a number of recent review articles on the topic including those of Nagin (2013) in *Crime & Justice* and Chalfin and McCrary (2017) in the *Journal of Economic Literature*. Other recent reviews of the police-crime literature include those of Lee et al. (2016) who review the literature on police manpower and Braga et al. (2014) which is specific to the practice of hot spots policing.

[2]There is likewise at least speculative evidence that "problem-oriented policing" approaches in which police officers systematically leverage a more expansive set of crime prevention strategies can lead to additional benefits to public safety (Braga et al., 1999; Weisburd et al., 2010).

[3]This dramatic shift in policing philosophy is famously evangelized in James Q. Wilson and George Kelling's seminal public essay, *Broken Windows*, published in 1982 in *The Atlantic Monthly* which argues that public safety cannot be effectively maintained unless police officers are actively involved in addressing physical and social disorder and enforcing community norms. The form of policing described by Wilson and Kelling is often referred to as "community policing" and represents a return to the philosophy of some of the first professional police forces in the United States (Greene, 2000). This philosophy later shifted as urban planners, under the umbrella of a broader social reform movement, sought to control widespread corruption by police officers by limiting their discretion and reducing their interaction with the communities they served (Walker, 1977; Alpert and Dunham, 1998). Beat cops who had, in the past, walked the street and who were empowered to use their judgment to solve community problems were replaced by officers in patrol cars whose focus was on responding to calls for service, a less proactive but more controllable form of policing (Greene, 2000). In the aftermath of the social unrest of the 1960s and the involvement of police departments in dispersing protesters, police departments shifted focus even further towards the three "Rs": rapid response, random patrol, and reactive investigation (Bratton and Anderson, 2018).

ing crimes to adjacent areas (Weisburd, 2005; Braga et al., 2014) and that today's misdemeanor arrests prevent tomorrow's felony crimes (Kelling and Sousa, 2001; Corman and Mocan, 2005; Messner et al., 2007), police departments have, over the last forty years, doubled down on the philosophy of broken windows policing, deploying large numbers of police officers to high-crime areas, often with explicit instructions to engage in large numbers of street stops and field interrogations (MacDonald et al., 2016).[4]

In recent years, public officials and members of the public at large have expressed concern that policing strategies which involve the intensive use of directed patrol and field interrogations may create high collateral costs for disadvantaged communities (Weitzer et al., 2008; Butler, 2014; Bandes et al., 2019), a problem that Bratton and Anderson (2018) refer to as the "great divide" in American policing. Likewise, recent scholarship has documented that mass enforcement policies have widened the net of the criminal justice system (Hagan and Dinovitzer, 1999; Howell, 2009; Kohler-Hausmann, 2018), thus leading to an increase in discriminatory practices which have had disproportionate impacts on minority communities (Gelman et al., 2007; Goel et al., 2016; Goncalves et al., 2017). While prior literature tends to find that the practice of concentrating police in high crime areas reduces crime, there is little evidence that such a practice has led to a decrease in fear or an increase in subjective well-being in high-crime communities (Rosenbaum, 2006; Ratcliffe et al., 2015; Kochel and Weisburd, 2017; Weisburd et al., 2011). Owing perhaps to increased media coverage of police shootings and political movements like *Black Lives Matter*, by 2015, public support for police had fallen to its lowest levels in twenty years despite the dramatic decline in crime since the 1990s.[5]

In response to concerns about the collateral consequences of mass enforcement, one of the most

[4] The conclusion that crime declines in New York City during the 1990s were fueled by an increase in police activity as proxied by misdemeanor arrests has been called into question persuasively in a critique by Harcourt and Ludwig (2006). The idea that misdemeanor crimes have a causal effect on next period's felony crimes has likewise been called into question by Caetano and Maheshri (2018). We cite the literature above in recognition that the findings were persuasive even if there continues to be active debate about the empirical findings.

[5] http://www.gallup.com/poll/183704/confidence-police-lowest-years.aspx

salient changes in American policing in recent years is the shift to a more surgical form of "precision policing" (Taylor et al., 2011; Bratton and Anderson, 2018) in which law enforcement focuses available resources on a small number of individuals who are thought to be the primary drivers of violence. Made possible by the same data-mining technologies that have been used to target law enforcement to high-crime locations (Ratcliffe, 2004; Johnson et al., 2008; Braga et al., 2014), targeting enforcement to high-risk individuals is a strategy that has been employed by a number of large law enforcement agencies during the last decade. However nowhere has the shift from mass enforcement to precision policing been more prominent than in New York City.

During the period from 2002-2011, the number of official street stops made by NYPD officers each year increased by nearly an order of magnitude from 97,000 to 680,000. In the aftermath of federal litigation which declared these and other related practices to be unconstitutional, the number of stops made by NYPD officers each year declined to just 12,000 five years later.[6] Around the same time, the NYPD, in partnership with the City's five district attorney's offices and, in some cases, federal prosecutors in New York's southern and eastern districts, began to invest more heavily in a precision policing strategy to confront serious crime and violence. In particular, there was a sustained increase in targeted enforcement actions against criminal gangs, often centered around the City's public housing communities (Howell, 2015). These "gang takedowns," as they often referenced colloquially, refer mainly to coordinated raids in which members of criminal gangs were arrested for specific felony crimes or, commonly, for conspiracy charges related to their alleged membership in a criminal organization that is under investigation. Many of the charges relate to the sale of drugs but there are also numerous charges for violent crimes. Indeed, a stated goal of the takedowns is to focus on

[6]These statistics are tabulated from official "stop, question and frisk" records made public by the NYPD. Naturally there are concerns about the accuracy of the data. We note that even if just 20 percent of the street stops made by police officers in 2016 were documented in the data, the decline in street stops relative to 2011 would still be on the order of 90 percent.

individuals who are the drivers of violence, with a particular emphasis on gun violence (Shea, 2018).

The shift from mass enforcement to precision policing in NYC coincides with an underappreciated feature of the City's crime landscape — that the great crime decline of the 1990s has given way to a second great crime decline during the 2011-2016 period, a period in which the City's shooting and homicide rates had declined by approximately 50 percent. This rapid and dramatic decline in gun violence remains largely unexplained and, critically, stands in stark contrast with sharp increases in gun violence in Chicago and several other large cities which, like NYC, dramatically cut back on their use of mass street stops. While prior research has supposed that the recent drop in gun violence in NYC may itself be the direct result of the winding down of mass enforcement (Sullivan and O'Keeffe, 2017), policymakers — including former NYC Police Commissioner James O'Neill — have implicated the NYPD's aggressive anti-gang initiatives as a driver of NYC's exceptionalism (Bruinius, 2018).[7]

This research evaluates the effectiveness of the City's targeted gang raids in reducing crime in and around NYC's public housing developments. While NYC is not the only city to engage in coordinated enforcement actions against criminal gangs, NYC's recent experience is unique in that these takedowns have been implemented alongside a wholesale reduction in mass enforcement activity by police. As such, this research offers an empirical test of the efficacy of gang takedowns in lieu of far less surgical order maintenance policing. Likewise, while related research has studied a variety of deterrence-focused strategies which empower law enforcement to disrupt gang activity, this is the first research to consider the impact of coordinated gang takedowns, a policy lever which is focused disproportionately on incapacitation.

We begin by showing that while gang takedowns tend to occur in some of the city's most violent

[7]Popular media reports have noted that the City's drop in shootings has been driven by an especially large decline in gang-related violence, noting that "scores of gang takedowns .... resulting in about 900 arrests, took violent people off the streets and made it more costly to engage in gang-related crime" (Mueller and Baker, 2018).

communities, their precise timing does not appear to be related to prior crime or enforcement trends. Next, using a differences-in-differences strategy paired with an event study framework, we show that shootings in public housing communities fell by approximately one third in the aftermath of a gang takedown and that the reduction in shootings persists — albeit to a smaller degree — for up to 18 months after a gang takedown occurs. Overall, we estimate that gang takedowns explain nearly one quarter of the decline in gun violence in and around New York City's public housing communities during the post-2011 period. In contrast, we find that other forms of violence — robberies and non-gun assaults — are far less responsive to the gang takedowns. We do not find evidence that the takedowns are mitigated by the displacement of crime to adjacent areas or lead to a subsequent increase in street stops or low-level arrests in these communities. Notably, this research studies the marginal impact of a gang takedown above and beyond other contemporaneous law enforcement innovations that may have an independent effect on community violence. As such, these estimates may represent a lower bound on the public safety value of the NYPD's shift to precision policing more generally.

2 Prior Literature

Precision policing is a broad umbrella under which a number of potential policing strategies might be classified. We begin by noting that while "hot spots" policing has, in practice, often been paired with a policy of mass enforcement (MacDonald et al., 2016), the increased concentration of police resources at identifiable hot spots is itself a form of precision policing.[8] Recognizing that an outsize share of crimes cluster at a small number of predictable places (Sherman et al., 1989; Braga and Clarke, 2014; Weisburd, 2015; Bernasco and Steenbeek, 2017), municipal police departments have, in recent years, hired increasing numbers of crime analysts to map administrative crime data and have established COMPSTAT pro-

[8] The increasing use of hot spots policing has been made possible by the increasing availability of large administrative datasets and, critically, the ability to analyze them. Leveraging "big data," police resources have been focused more intensively and visibly on crime hot spots.

grams in order to provide managerial support for data-driven crime reduction strategies, often centered on criminogenic places (Walsh, 2001; Weisburd et al., 2003). Recent reviews of the literature on hot spots policing, which includes a number of well-executed randomized experiments (Sherman and Weisburd, 1995; Braga and Bond, 2008; Groff et al., 2015), finds that these efforts have generally been successful in reducing crime without displacing crime to adjacent areas (Braga, 2005; Braga et al., 2014).

To date, many of the most surgical forms of precision policing have involved targeted enforcement against criminal gangs. Targeted gang enforcement has a long tradition in American policing and indeed is one of the chief motivations for the creation of the Federal Bureau of Investigation in 1933. In the modern era, policymakers have experimented with the provision of both "carrots" and "sticks" in developing precision responses to gang violence. Carrots include offers of social services and other types of programmatic assistance including educational programming (Cook et al., 2015; Heller et al., 2017) and employment opportunities (Heller, 2014; Gelber et al., 2016; Davis and Heller, 2017) for high-risk youth. Sticks include highly-targeted gang enforcement programs which are sometimes paired with the explicit threat of such enforcement. Among the many stick-based approaches have been the use of civil "gang injunctions," a place-based policy that is intended to widen the scope for local law enforcement to crack down on alleged gang members (Grogger, 2002; Hennigan and Sloane, 2013; Muniz, 2014; Ridgeway et al., 2019), truancy and curfew enforcement (Fritsch et al., 1999; Carr and Doleac, 2018) and the monitoring of suspected gang members, using electronic surveillance (Deuchar, 2012; DeMichele, 2014) and social media (Behrman, 2015; Balasuriya et al., 2016; Patton et al., 2017).

Perhaps the most transformational policy in the area of gang enforcement in the last three decades is "pulling levers" strategies, most famously exemplified by *Operation Ceasefire* (Kennedy et al., 2001) and the long lineage of similar interventions that have been inspired by its success in Boston (Braga et al., 2001, 2014). While the execution of these strategies varies from jurisdiction to jurisdiction, the underlying principle is to enlist various criminal justice system actors to "pull every lever" in confronting gang violence. Pulling levers programs combine an enhanced law enforcement focus on gang activity, particularly around illegal firearm markets, with activities that are designed to promote "focused deterrence." Examples of focused deterrence-based strategies include explicitly prioritizing enforcement resources on the most violent gangs and reaching out directly to gang members either individually or via group call-ins. There is now a myriad of quasi-experimental studies that test the efficacy of pulling levers-inspired strategies in a number of U.S. cities including Baltimore (Webster et al., 2013), Chicago (Papachristos et al., 2007), Cincinnati (Engel et al., 2013), High Point, NC (Corsaro et al., 2012), Indianapolis (McGarrell et al., 2006), Newark (Boyle et al., 2010), New Orleans (McVey et al., 2014), Phoenix (Fox et al., 2015), Richmond, VA (Raphael and Ludwig, 2003; Rosenfeld et al., 2005) and Rockford, IL (Corsaro et al., 2013) among others. Overall, the evidence tends to support the efficacy of these programs in the United States (Braga and Weisburd, 2012) though it is worth noting that the explicit targeting of criminal gangs may have been far less successful in countries with weaker political institutions (Ríos, 2013; Dell, 2015; Espinosa and Rubin, 2015).[9]

Precision policing practices that are focused on specific individuals have begun to proliferate in recent years. However, scholarship has been scant. One exception is research on the Chicago Police Department which, in partnership with researchers at the Illinois Institute of Technology, created a "strategic subjects list" of Chicago residents selected on the basis of the risk of being shooting-involved in the future (Saunders et al., 2016).[10] This research found little evidence that the interventions

[9] More recently, a number of jurisdictions have created programs that employ "gang interrupters," typically individuals who are former gang members and are therefore credible messengers in an effort to reduce retaliatory violence. This strategy, marketed under the *Cure Violence* umbrella constitutes an approach to gang violence that is more public health-oriented than law enforcement-oriented (Butts et al., 2015; Slutkin et al., 2015). The Cure Violence program has been evaluated New York City, yielding promising albeit early results (Picard-Fritsche and Cerniglia, 2013; Butts et al., 2015).

[10] Person-focused enforcement strategies have raised a number of legal questions which legal scholars are only beginning to untangle — see e.g., Tucek (2018).

applied to individuals named on the strategic subjects list — namely home visits and an offer of social services — led to enhanced public safety. However, the researchers have raised questions about the extent to which these interventions were actually deployed in practice.

The gang takedowns we study are most similar to stick-based approaches such as gang-injunctions insofar as both approaches are intended to provide law enforcement with greater leeway to deal with the unique challenges posed by criminal gangs. However, while gang injunctions are a civil law-based tool that are intended to empower street-level officers to disrupt gang activity such as graffiti or groups of suspected gang members "hanging out" in a particular area, NYC's gang takedowns are more specifically designed to immediately incapacitate gang members. Moreover, the takedowns are not directed by patrol officers. Instead they are the result of months and sometimes years-long investigations by detectives in cooperation with NYC's district attorneys offices. The underlying theory of change then is less behavioral and more mechanical with authorities relying on the idea that the number of individuals in a community who are willing to engage in gang-based violence is finite and that this population is not easily replaced (Blumstein, 1993; Piquero and Blumstein, 2007), at least in the short-run, when suspected gang members are incapacitated. A second feature of NYC's campaign of gang takedowns is worth nothing. While prior enforcement strategies such as gang injunctions and pulling levers (and even previous campaigns of gang takedowns in NYC and elsewhere) have largely been implemented alongside mass enforcement strategies, NYC's gang takedowns have been implemented in the aftermath of a court-ordered end to the NYPD's mass enforcement policy that predominated during the 1990-2010 period. As such, they have taken place in the absence of a policy of mass enforcement as part of a regime shift in policing in NYC during this time period.

3 Institutional Background

3.1 New York City's Second Great Crime Decline

The dramatic decline in crime during the 1990s in the United States and other industrialized countries has been a topic of considerable discussion among social scientists (Lafree, 2000; Blumstein et al., 2006; Baumer and Wolff, 2014).[11] While the causes of the "Great Crime Decline" have yet to be fully explained, scholars have proposed a number of candidate factors including larger police forces (Owens, 2013; Chalfin and McCrary, 2018; Mello, 2019; Weisburst, 2019), increased use of incarceration (Levitt, 1996; Blumstein and Rosenfeld, 2008; Zimring, 2008), the receding crack epidemic (Fryer Jr et al., 2013), increases in immigration (Sampson, 2008), lead paint eradication (Reyes, 2007; Billings and Schnepel, 2018) and the mass availability of safe and legal abortion (Donohue III and Levitt, 2001).

In New York City, which experienced a steep rise in its crime rate from 1960 to 1990, violent crime declined to an especially large degree — far larger than the national decline and larger than among any other large U.S. city (Zimring, 2011; Weisburd et al., 2014). These trends are explored in **Figure 1** which plots the number of murders per 100,000 individuals for New York City as well as the United States as a whole for the 1960-2018 period.[12] NYC's murder rate peaked at just over 30 murders per 100,000 residents in 1990. By 2000, the rate had declined by 73 percent to 8.4 murders per 100,000 individuals. By 2018, New York City, whose homicide rate had been 60 percent higher than the national average even after the great crime decline of the 1990s, had a homicide rate that was one third *lower* than the national average.

[11] In the United States, per capita index crimes in 2000 were nearly 30 percent lower than they had been in 1990 and, by 2010, the index crime rate had fallen by another 10 percent. The decline in homicide was even greater with murders declining by 40 percent during the 1990s and by another 10 percent during the decade spanning 2000-2010.

[12] We focus on murders as they are particularly well-documented (Loftin and McDowall, 2010) and, as such, are a reliable means to compare crimes over time.

The latter point underscores a critical but underappreciated feature of the great crime decline in New York City — rather than ending in 2000, NYC's crime rate, in particular, for serious crimes involving firearms, has continued to decline to the present day. Indeed, NYC's homicide rate fell by nearly 50 percent during the 2010-2018 period, while national homicide rates were flat. Put differently, relative to national trends, NYC's homicide decline during the 2010-2018 period is 60 percent larger than it was during the 1990s. A visual schematic of NYC's second great crime decline is provided in **Figure 2** which plots the number of murder victims (Panel A) and shooting victims (Panel B) in New York City during the 2007-2018 period. While the number of shootings and homicides were fairly constant between 2007 and 2011, both figures declined precipitously after 2011; by 2018, the City's number of homicide and shooting victims had declined by 45 percent and 53 percent, respectively. [13]

The second great crime decline in New York City, to date, is mostly undocumented in the academic literature and remains largely unexplained though gentrification has been suggested as a contributing factor (Barton, 2016).[14] That said, the rapidity of the decline in gun violence over a very short time period suggests that demographic changes are unlikely to explain the near 50 percent decline in gun violence that is concentrated in these years. Likewise, there were no changes in police manpower during this period (see **Appendix Figure 3**) nor were cohorts born eighteen years earlier exposed to an abrupt change in abortion policy or exposure to lead paint.

[13] It is likewise instructive to compare NYC to other large cities. In **Appendix Figure 1** we plot log murders per 100,000 population for NYC against the thirty-two other U.S. cities with populations exceeding 500,000 during the 2007-2018 period. NYC's exceptionalism is evident in the figure — during the post-2011 period, while NYC experienced a large decline in its murder rate, murders were increasing in other large U.S. cities. Note that NYC's exceptionalism is largely confined to homicide — for other index crimes, NYC closely tracks trends in other large cities (see **Appendix Figure 2**). We note that, for rape, the increase observed in NYC in 2013 is an artifact of the change in the FBI's definition of rape of which NYC was an early adopter.

[14] There is support for the idea that gentrification can lead to important crime declines in the extant literature (Papachristos et al., 2011; Autor et al., 2014; MacDonald and Stokes, 2020).

3.2 The Rise and Fall of Mass Enforcement in New York City

During the period from 2002-2011, the number of official Terry stops made by NYPD officers increased by nearly an order of magnitude from 97,000 to 680,000. This dramatic increase in street-level enforcement by the NYPD was policy-driven and coincided with the administration of Police Commissioner Raymond Kelly as well as with the department's signature proactive policing program, *Operation Impact*, a broad-based initiative that directed additional police personnel to seventy "impact zones" throughout the city and encouraged especially intensive enforcement in these areas (MacDonald et al., 2016; Bratton and Anderson, 2018).

The dramatic increase in enforcement activity eventually led to federal civil rights litigation, in the form of a class action lawsuit, initially filed in 2008. This litigation, *Floyd v. City of New York*, was named after David Floyd, an African-American NYC resident who was stopped and searched by police officers who claimed that they suspected him of committing a burglary.[15] Floyd, along with several co-plaintiffs, filed a civil action against the City of New York alleging that the defendants designed and implemented a policy, practice, and/or custom of unconstitutional stops and frisks by the New York Police Department on the basis of race and/or national origin.[16] On August 31, 2011, the United States District Court for the Southern District of New York denied the City's motion for summary judgment of the case.[17] In August 2013, two years after failing to dismiss the case, Judge Shira A. Scheindlin ruled that the NYPD's policy and practice of mass enforcement through field interrogations had violated the Fourth Amendment by conducting unreasonable searches and the Fourteenth

[15] Case citation: 959 F. Supp. 2d 540 (2013).

[16] Another notable lawsuit was filed in *Ligon vs. City of New York*. This lawsuit challenged the constitutionality of Operation "Clean Halls" in which NYPD officers would direct routine patrols in the hallways of private buildings.

[17] The Honorable Shira A. Scheindlin, writing for the court, noted that, "there is a triable issue of fact as to whether the NYPD leadership has been deliberately indifferent to the need to train, monitor, supervise, and discipline its officers adequately in order to prevent a widespread pattern of suspicionless and race-based stops."

Amendment by systematically conducting stops and frisks in a racially discriminatory manner.

We plot the number of documented street stops in NYC during the 2007-2018 period in Panel A of **Figure 3**. As the *Floyd* case made its way through the court system, the number of street stops in NYC continued to increase, from 540,000 in 2008 to 685,000 in 2011. However, the number of street stops declined dramatically after Judge Scheindlin's 2011 ruling which allowed the *Floyd* litigation to move forward, falling particularly quickly between 2012 and 2013 when the number of stops declined from 532,000 to 192,000. After the August 2013 federal court ruling in which NYPD's policy and practice was found to be unconstitutional, street stops declined by more than an order of magnitude from 192,000 in 2013 to 12,000 three years later. While it has been suggested that some of the decline in the official numbers is due to a decrease in the practice of documenting street stops among NYPD officers, the decline in stop activity is so large that even if only 20 percent of stops in the post-*Floyd* period are documented, there still would have been an order of magnitude decline in the number of street stops by NYPD officers during the 2011-2016 period. The post-2011 period constitutes a sea change in policing in NYC and has been documented extensively in popular media reports from the *New York Times*[18], the *Wall Street Journal*[19] and the *Washington Post*[20], among other outlets.

The *Floyd* ruling was roundly criticized by Police Commissioner Raymond Kelly and then Mayor Michael Bloomberg who referred to the decision as "dangerous."[21]. Yet fears that the decision would have a negative impact on public safety were never realized. Instead, as noted in a prominent op-ed in *The National Review* entitled "We Were Wrong About Stop-and-Frisk," the City experienced

[18] https://www.nytimes.com/2017/05/30/nyregion/nypd-stop-and-frisk.html

[19] https://www.wsj.com/articles/in-new-york-city-police-stops-and-crime-are-both-down-1449875165

[20] https://www.washingtonpost.com/news/wonk/wp/2014/08/21/12-years-of-data-from-new-york-city-suggest-stop-and-frisk-wasnt-that-effective/

[21] See https://www.wsj.com/articles/mayor-calls-stopandfrisk-ruling-dangerous-1376357978

a dramatic *decline* in gun violence, even as police officers were deprived of what was thought to be a key deterrent in their arsenal — the street stop (Sullivan and O'Keeffe, 2017; Smith, 2018). A remaining question is how this came to be. While offering a complete explanation for NYC's decline in gun violence is beyond the scope of this paper, we note that as the NYPD began to wind down its policy of mass street stops, it began to invest further in another, qualitatively different type of enforcement action — the "gang takedown."

3.3 Precision Policing in the Post-*Floyd* Era

In 2012, the NYPD initiated two transformational policies which signaled a major change in how it dealt with issues of gang and youth violence. First, the department implemented *Operation Crew Cut*, a policy which doubled the number of officers in the department's gang unit from 150 to 300 in order to provide additional resources to promote the department's heightened focus on criminal gangs. Second, Commissioner Raymond Kelly announced the renewal of the department's "Criminal Group Database," a unified list of alleged gang members throughout the City. As noted by Howell (2015) and Trujillo and Vitale (2019) among others, Kelly made it clear that this new operation was intended to target not only established criminal enterprises (e.g., national gangs such as the Bloods or the Crips) but also crews — "loosely affiliated groups of teens" who often "identify themselves by the blocks where they live and are responsible for much of the violence in public housing."[22] By March 2018, more than 17,000 New Yorkers had been added to the NYPD's gang database.[23]

The explicit goal of the NYPD's shift in strategy was to address gang and crew violence by building large-scale conspiracy cases implicating not only individuals who may have perpetrated an underlying

[22] As noted by Howell (2015), the narrative that crews of young people are responsible for a large percentage of shootings in New York City was first advanced by Police Commissioner Raymond Kelly in October of 2012, when he announced Operation Crew Cut.

[23] Of those added, over 98 percent were identified as either Black or Hispanic (Trujillo and Vitale, 2019) and 8 percent were below the age of 18 (Shea, 2018).

crime but also a host of alleged criminal associates who are suspected to have been involved with associated criminal activity (Trujillo and Vitale, 2019).[24] Thus, while Operation Crew Cut and the re-creation of the department's gang database are the vehicle through which the department shifted from a policy of mass enforcement to precision policing, "gang takedowns" were, in practice, the fuel that powered the department's shift in policy.[25] "Gang takedowns" is, of course, not a technical or legal term — instead this is a colloquial expression used in media reports and among members of the law enforcement establishment to describe highly-coordinated and targeted raids on alleged gang members, often centered around the city's public housing communities. The two largest gang takedowns (taking place June 4, 2014 in Manhattan and April 27, 2016 in the Bronx) led to arrests of 103 and 120 individuals, respectively. However, hundreds of smaller takedowns have also occurred over the last decade. While there is no publicly-available description for how gangs were selected for a takedown, public commentary by a number of NYPD officials indicates that gangs are targeted on the basis of their perceived participation in violence (Shea, 2018).[26]

The NYPD's focus on gang takedowns coincides temporally with federal litigation related to the department's stop, question and frisk practices. In Figure 3, Panel B we plot the number of gang takedowns in New York City during the 2007-2018 period, as proxied by clusters of two or more conspiracy arrests in the same location and on the same day. Relative to the 2007-2010 period, the number of gang

[24] In order to build conspiracy cases, investigators have exploited a new tool in gang enforcement: the social media postings of suspected gang members. Leveraging digital connections to build criminal conspiracy cases against whole groups, NYPD investigators along with their partners in the City's five district attorney's offices, built wide-ranging criminal cases that might have otherwise take years of painstaking undercover work to prepare. Facebook, officers like to say now, is the most reliable informer (Goldstein and Goodman, 2018).

[25] There have been other department-wide operational shifts as well. In particular, in 2016, the NYPD's detective bureau absorbed many of the NYPD's alternative investigative units, a move that was intended to reduce the siloing of information. Our identification strategy nets out the effect of department-wide changes in gang enforcement by focusing on variation in the precise timing of gang takedowns.

[26] As discussed in Section 6.1, we find no evidence of strategic timing of takedowns in response to crime trends. This is likely because (1) many communities had levels of violence that met the criteria for takedowns at the start of the policy regime and (2) many months elapse between when a location is selected for a takedown and the takedown itself.

takedowns expanded rapidly after 2011 just as the department was engaging in dramatic reductions in the number of street stops made by NYPD officers (Figure 3, Panel A). While there is no way to establish conclusively that Operation Crew Cut and the re-creation of the department's gang database were intended as a substitute for mass enforcement policies that were the subject of federal litigation, this narrative has gained considerable traction among NYC-based advocates and legal scholars with some referring to the dual policies as "Stop and Frisk 2.0" (Speri, 2018). An explicit link between the policies has likewise been articulated in numerous media reports, beginning with a 2013 article in *The New York Times* entitled "Frisking Tactic Yields to a Focus on Youth Gangs" (Goldstein and Goodman, 2018).

4 Data

To evaluate the effects of gang takedowns, we use administrative records on crime, arrests and Terry stops which are available from NYC's Open Data website.[27] These records provide incident level data on each crime and arrest that occurred in NYC during the sample period, including the date, time, precise location, and top criminal charge pertaining to the incident. Following Chalfin et al. (2019), we geocode each incident to public housing communities by drawing a 500 foot (two-block) buffer around the shapefile of each housing development. We focus on public housing communities for two reasons. First, while these communities are home to approximately 7 percent of New Yorkers, they received nearly one third of the new gang enforcement activity. Second, gang takedowns in areas without public housing often span many different NYC neighborhoods. Accordingly, any resulting crime reductions are likely to be geographically diffuse. Given that the gang takedowns in public housing targeted gangs that operate in a specific area, we are better able to identify the effect of enforcement activity in these areas.

[27]The URL is: `https://opendata.cityofnewyork.us/`. We use the NYPD Arrest Data (Historic), NYPD Arrest Data (Year to Date), NYPD Complaint Data (Historic), NYPD Complaint Data (Year To Date), and The Stop, Question and Frisk datasets.

4.1 Gang Takedowns

We identify gang takedowns in the data using clusters of arrests for the crime of "conspiracy" which are uncommon in the data and strongly associated with gang takedowns.[28] In our main models, we focus on clusters of two or more conspiracy arrests within a 500 square foot area of a given public housing development. Using administrative data on geo-located arrests, we identify 455 probable gang takedowns that occurred city-wide since 2011. Of these, 109 occurred primarily within 500 feet of one of the city's 340 public housing developments. These data are summarized in **Table 1** which provides a sense for the overall scale of the takedowns as well as their geographic dispersion. Takedowns have occurred in all five boroughs of NYC though the Bronx and Manhattan, which comprise 36 percent of the city's population, account for 66 percent of the city's takedowns. Four of the City's seventy-seven police precincts (the 5th, 42th, 44th, and 84th), which comprise roughly 6 percent of the City's population, account for 48 percent of the gang takedowns city-wide.

The 455 identified takedowns were associated with 3,009 conspiracy arrest charges, nearly 30 percent of which occurred in and around the City's public housing developments. **Table 2** provides a description of the demographic composition of those arrested under conspiracy charges. The statistics in the table correspond closely with anecdotal descriptions found in the popular media which suggest that the arrestees are young minority men. Among those arrested, approximately half are below the age of 25 and 6 percent are below the age of 18. Among takedowns in and around public housing communities, these figures are 44 percent and 8 percent, respectively. More than 90 percent of the arrestees are male and the arrestees are largely residents of color. Non-Hispanic blacks who comprised approximately 25 percent of the City's 2011 population comprise 54 percent of the

[28] While the NYPD uses a "hierarchy rule" in assigning incidents to top-coded charges, we note that our focus on clusters of conspiracy charges is intended to identify the signature of a gang takedown rather than to reliability count the number of arrests that occur during takedowns.

arrestees. Hispanics (of any race) who were approximately 28 percent of the City's 2011 population comprise 38 percent of the arrestees.[29] Non-Hispanic whites who made up nearly 44 percent of the City's population comprise 6 percent of takedown arrestees.

4.2 Analytic Dataset

Given the strong nexus between the gang takedowns and the city's public housing communities, we generate an analytic dataset by collapsing the data to the housing development-by-week level for the 468 weeks that occur between January 2011 and December 2019, noting whether a given week occurs before, during or after a gang takedown.[30]

In all subsequent analyses we focus on the subset of 73 NYCHA developments that experienced at least one gang takedown and exclude from our analytic dataset the developments that do not experience a gang takedown during the study period. We do so because the never treated developments and the ever treated developments experience markedly different crime trends. In the spirit of the differences-in-differences decomposition of Goodman-Bacon (2018), we divide the City's public housing developments into three groups: 1) those that are never treated (i.e., that never experienced a gang takedown), 2) those that are treated early in the study period and 3) those that are treated late in the study period.[31] In **Figure 4**, using only the pre-treatment periods for the ever treated developments, we plot weekly shootings (residualized to remove development fixed effects) over time separately for the three groups and draw a trend line through each of the series. As is apparent from the graph, the early and late treated development follow trends that are very close to parallel making the ever

[29] NYPD arrest data identifies arrestees as either "White Hispanic" (25 percent) or "Black Hispanic" (13 percent). We have aggregated these two categories to conform with a global Hispanic category.

[30] We do not include pre-2011 data for takedowns occurring before 2011 due to the abrupt shift in the policy regime with respect to street stops. However, estimates remain extremely similar when pre-2011 data are used.

[31] We use $t = 250$ in November 2015 which is in the middle of our data as our cutoff to select late versus early adopters. We do not formally provide a Goodman-Bacon decomposition as using a two-year bandwidth in analysis means that our panel data are not strongly balanced. However, the analysis provided in Figure 4 is conceptually analogous to the formal decomposition.

treated developments a good comparison group for one another. The never treated developments, on the other hand, experience substantially more negative time trends. Accordingly, they are a poorly chosen counterfactual for the treated developments and we exclude them from the data.

Some of these 73 housing developments experienced more than a single takedown during the study period. Our primary analysis defines the post-intervention period as the period after the development's initial gang takedown. To ensure that results are not being driven by gang enforcement that occurs after the initial takedown, we perform an auxiliary analysis in which we limit our analysis to developments that experienced only a single takedown. We also estimate results for all takedowns in the data; results are substantively similar. In order to measure the effect of the initial gang takedown on community crime, in our main analysis, we focus on a two-year bandwidth around the takedown. Robustness to bandwidth selection is addressed in Section 6.2.

To confirm that these clusters of conspiracy arrests pertain to gang takedowns, we performed a directed media search, identifying whether any news articles reference a gang takedown on the date and location of the conspiracy arrests. We were able to find news articles on 53 of the 73 gang takedowns that are the primary subject of this research. In a robustness check, we re-estimate our models focusing on the subset of the data that we can validate in this way in addition to a battery of additional robustness checks. Beyond this, we note that to the extent that there are remaining errors in our gang takedown measure, so long as those errors are uncorrelated with crime, our estimates will simply be attenuated towards zero and, as such, will be conservative (Hyslop and Imbens, 2001).[32]

Summary statistics for our administrative data are reported in **Table 3** which reports the mean and standard deviation as well as the minimum and maximum values for our five outcome variables

[32]To the extent that our definition of a takedown omits small gang takedowns with either zero conspiracy charges or a single top-coded conspiracy charge, our results will be biased towards zero and will be conservative. During the directed media search we found two presumed takedown events that appear to be associated with fraud charges rather than being gang-related; our results are robust to omitting these events.

as well as for measures of police enforcement which we use to test mechanisms. Statistics are presented separately for the pre- and post-initial takedown periods. Overall, there are approximately 0.01 homicides, 0.04 shootings and 0.5 assaults per week in and around each of the public housing developments in our sample. Weekly homicides and shootings declined by 12 percent and 16 percent from the pre-takedown to the post-takedown periods. With respect to enforcement, there is evidence of sizable declines in the number of arrests for misdemeanor marijuana possession as well as the number of Terry stops. While this could, in part, be a response to the gang takedowns it is also consistent with the large secular decline in mass enforcement in NYC during this time period. In subsequent regression estimates, we net out secular variation using time fixed effects.

5 Econometric Models

We study the effect of gang takedowns using a differences-in-differences strategy augmented with an event study which we use to control for pre-intervention crime trends and to better understand the temporal dynamics of the effect of gang takedowns on public safety.[33] Our primary analysis employs a differences-in-differences estimator which evaluates whether average weekly crimes changed after a gang takedown, net of NYCHA development fixed effects which account for time-invariant heterogeneity among housing developments and time fixed effects which account for citywide time trends which could be driven by other factors such as additional changes in policing or economic or social factors like gentrification. We focus on the 73 out of 340 public housing developments that experienced at least one gang takedown during the sample period. As such, all developments in our data experience the treatment, only at different times. Our estimator is therefore a "two-group timing only" estimator in the language

[33] The econometric properties of event studies have long been understood. For reviews of the history and development of the methodology, see: Binder (1998) and Kothari and Warner (2007). Event studies have the further attractive quality of being more reliable in estimating treatment effects than standard two-way differences-in-differences models when treatment effects vary over time (Abraham and Sun, 2018; Goodman-Bacon, 2018).

of the differences-in-differences de-composition of Goodman-Bacon (2018). By focusing on this subgroup, we enhance the comparability of the developments in our data and simplify the substantive interpretation of our differences-in-differences estimates. As noted in Section 4, we focus on each development's initial gang takedown and restrict the sample to the two years before and after each initial takedown, though, as we demonstrate in Sections 6.2 and 6.4, results are robust to relaxing this constraint.

In order to estimate treatment effects, we use a standard two-way fixed effects design. In particular, we estimate a Poisson regression model in which the count of crime, $Y_i \sim$ Poisson(γ_i), is regressed on a binary indicator for whether a time period occurs after a development's initial gang takedown, conditional on development and time fixed effects:

$$log(\gamma_{it}) = \alpha + \phi TAKEDOWN_{it} + \beta POST_{it} + \lambda_i + \delta_t \quad (1)$$

In (1), $TAKEDOWN_{it}$ is an indicator for whether a takedown occurred in a housing development i in week t and $POST_{it}$ is an indicator for whether a given week occurs after the housing development i's initial gang takedown.[34] Accordingly, β is the treatment effect of interest and e^{β} is the incidence rate ratio (IRR). The model also conditions on λ_i which represents housing development fixed effects and δ_t which represents week-year fixed effects (a dummy variable for each unique time period in the dataset). In all models, standard errors are clustered at the housing development level to account for both heteroskedasticity and arbitrary serial correlation in the error terms for observations in the same geographic unit measured at different time periods (Bertrand et al., 2004).

In auxiliary models, we change how we control for time trends in order to demonstrate that

[34] We regress out the week of the takedown itself because this week receives a great deal of law enforcement attention and including it potentially compromises our ability to learn about the enduring effect of the takedowns after the arrests have been made. In practice, results do not change when this week is included as a post-takedown week.

estimates are not dependent on the choice of a standard two-way fixed effects model. First, we augment (1) with a vector of development-specific linear time trends which allow each development to have a different slope in the model. Next, instead of using week-year fixed effects, we instead parameterize the model to control flexibly for time using month fixed effects and a cubic time trend. Finally, we replace the week-year fixed effects with interacted borough-week-year fixed effects which account for differential crime trends in each of New York City's five boroughs.

In order to further explore the temporal dynamics of the effect of the gang takedowns, we run two additional models. First, we generate an estimate of the instantaneous effect of the gang takedowns on crime using a regression discontinuity approach in which weeks relative to the initial gang takedown is the running variable. In order to do this, we collapse the data to a single cross-section at the week level and test whether the average number of crimes per development changes just after a gang takedown. We present this analysis graphically but generate estimates using the following equation which we estimate via least squares:

$$Y_i = \alpha + \beta POST_i + \phi(X_i - c) + \tau(X_i - c)POST_i + \varepsilon_i \tag{2}$$

In (2), Y_i is the average number of weekly crimes at NYCHA developments in our sample relative to the week of a gang takedown, (X_i-c) is the number of weeks relative the gang takedown and $POST_i$ is an indicator variable for whether or not a given week is prior to or after the gang takedown. The coefficient on $POST_i$, β identifies the causal effect of the gang takedown on crime local to the threshold in the running variable.

Next, in order to more formally trace the temporal dynamics of crime in relation to the initial gang takedown, we estimate an event study. In this model, the count of crime, $Y_{it} \sim$ Poisson(γ_{it}), is regressed on a vector of time period dummies which capture the time-path of the treatment effect, conditional on a cubic function in time.[35]

$$log(\gamma_{it}) = \left[\sum_k \beta_k POST_{it}^k\right] + f(t) \tag{3}$$

In (3), $POST_{it}^k$ is an indicator for whether housing development i is treated at time t and k indexes one of several subsets of time periods. In practice, we estimate this using six-month time periods such that there are $K = 8$ time windows in our four-year treatment period. The β_k parameters measure the impact of gang takedowns both before and after they are executed. As we include a constant and set the coefficient β_0 equal to zero, all estimated coefficients are relative to the period immediately prior to the initial gang takedown. These β_k terms serve two purposes. First, using the β_k terms in the pre-intervention period allows us to test for parallel trends, the key assumption of the differences-in-differences methodology, in the pre-intervention period. Second, the β_k terms in the post-intervention period allow us to characterize the extent to which the effect of the gang takedowns persist over time.

6 Results

6.1 Main Results

We begin our discussion of the results by presenting differences-in-differences estimates derived from equation (1). These estimates are presented in **Table 4**. In the table, we present coefficients and clustered standard errors (in parentheses) along with the IRR minus 1 (in square brackets) for four violent crime categories - homicides, shootings, assaults and robberies - as well as aggregated property crimes. The table presents estimates for four models. Model 1 is the standard two-way differences-in-differences estimator that conditions on NYCHA development and week-year fixed

[35] In practice, estimates are extraordinarily similar conditioning on exact week fixed effects.

effects. Model 2 adds a NYCHA development-specific linear time trend and Model 3 allows the week-year fixed effects to vary by borough. Model 4 drops the week-year fixed effects and instead models time using month dummies and a cubic time trend.

We begin with homicides. Since homicides are rare in the data, the estimates are somewhat imprecise. Nevertheless, across the four models there is evidence that homicides decline appreciably — by between 50 and 61 percent — in the aftermath of a gang takedown. While only the estimate in Model 3 is significant at conventional levels, the other three estimates are all significant at $p < 0.1$. Next, we turn to shootings which are more common in the data than homicides. In the two year period after a takedown, shootings decline by between 30 and 36 percent. Results for Models 1, 2 and 4 are significant at conventional levels; the estimate for Model 3 which conditions on borough-week-year time trends is less precise but is consistent with the other results. While the data we use represent shootings known to law enforcement which might be measured with systematic error if gang takedowns change patterns of crime reporting (Shepherd and Sivarajasingam, 2005), we note that the pattern of results is very similar to that of homicides which are presumably measured very well.[36] With respect to assaults, the coefficients in all four models are negative and suggest a small reduction in violence though only one of the estimates is significant at conventional levels. Finally, there is no clear evidence that crimes with a pecuniary motive — robberies or property crimes — decline after a gang takedown.

Next, we turn to our event study which is presented graphically in **Figure 5**. The figure plots the estimated incident rate ratios for shootings (Panel A) and violent crimes (the aggregate of shootings assaults and robberies) (Panel B) along with their 95 percent confidence intervals for each six-month window relative to the initial gang takedown, using the pre-intervention period as the leave-out group. We focus on six-month bins rather than a more granular time window in order to smooth

[36] Shotpsotter data are not publicly available in NYC.

out the inevitable noise in analyzing rare events such as shootings. However, in order to present a more granular view of the data, we also present a Lowess smoother through the weekly pre-period data (**Appendix Figure 4**).

Two broad trends are apparent. First, there is no evidence that violence was either rising or falling significantly prior to the execution of a development's initial gang takedown.[37] We confirm this formally using an F-test on the joint significance of the pre-intervention terms. This result is important insofar as strategic timing of the takedowns in response to violent crime trends would violate the key assumption of our differences-in-differences analysis. Though the analysis for shootings may be a little underpowered, the standard errors for overall violence are sufficiently small to rule out appreciable pre-trends.[38] While the lack of pre-trends might appear surprising given that law enforcement officials have strong incentives to target the intervention to communities in which violence is rising, we offer two explanations. First, there are many communities in which violence has been a persistent issue for a number of years. As such, the pool of communities in which intervention is potentially needed is large relative to available resources. Second, gang takedowns generally occur only after many months of investigation. As such, even if law enforcement officials intend to target the communities in which violence is growing most rapidly, the precise timing of the takedowns is likely to be arbitrary.

Second, turning to the post-intervention trends, we estimate that shootings fell by approximately 35 percent in the six-month period directly after a gang takedown, by approximately 25 percent in the next two six-month period and by approximately 30 percent in the following six-month period. While the

[37] We also note, in Appendix Figure 9, that there are no pre-trends with respect to street stops and arrests for marijuana possession.

[38] We also test whether the timing of a development's initial takedown is related to its overall level of violence by collapsing, for the pre-intervention period, weekly shootings to the housing development level and regressing weekly shootings on the takedown week. The raw data are plotted along with a quadratic best fit curve in **Appendix Figure 5**. For both shootings and community violence there is no clear pattern in the data. Likewise, when we drop the earliest and latest adopters from the data, estimates are unchanged.

standard errors are wider in the event study analysis, there is evidence of a sustained decline in gun violence for as long as twenty-four months after a takedown. We focus on six-month windows as limited statistical power means that smaller time windows lead to very noisy estimates. Nevertheless, to provide a sense for what is happening to shootings in the immediate aftermath of a gang takedown, we turn to our regression discontinuity estimates (see equation 2) which we present graphically in **Figure 6**. The figure plots the average number of weekly shootings in and around the NYCHA developments in our sample along with a best fit line and the associated 95 percent confidence interval. Immediately after a gang takedown, weekly shootings decline by 0.012 relative to a pre-period mean of 0.038 shootings per week. Thus, we estimate that shootings decline by approximately 32 percent in the immediate aftermath of a gang takedown ($p< 0.1$). Taken together, the results suggest that there is a large decline in gun violence in and around public housing developments that begins immediately after a gang takedown.

6.2 Robustness

A review of Table 4 suggests that shootings decline significantly in the aftermath of a gang takedown. In this section we subject this result to further scrutiny. We begin by establishing that discretionary arrest activity, in general, does not lead to a decline in gun violence by regressing weekly shootings on the first through fourth lags of arrests for marijuana possession (a proxy for proactive policing), using equation (1) which conditions on NYCHA development and week-year fixed effects. Two of the four coefficients are positive and two are negative; none of the four coefficients is significant. An F-test likewise fails to reject the joint significance of the lagged regressors. Next, we perform a simulation exercise in which we randomly re-assign each development's gang takedown week 2,000 times and compare our actual estimate to the 2,000 placebo estimates.[39] This analysis is presented in **Figure 7** which carries out

[39]This follows a recommendation in Young (2019) which shows that 2,000 repetitions should be sufficient to reliably recover correctly sized tests.

this placebo exercise for our sample of 73 developments which experienced a gang takedown (Panel A) as well as the sub-sample of never treated housing developments (Panel B). In both cases, our actual estimate (-33 percent) is highly unusual relative to the distribution of the placebo estimates.

We also test whether the estimated treatment effect is sensitive to a variety of choices we made in analyzing the data. **Figure 8** plots estimated treatment effects along with an associated 95 percent confidence interval for a series of alternative models. We begin with our preferred estimate which we reported in column (1) of Table 4. In alternate models 2 and 3, we change our definition of a gang takedown, defining it instead as weeks when three or more conspiracy arrests occurred (model 2) and using a sub-sample of takedowns that can be verified using a directed media search (model 3). Next, we test whether the estimates are sensitive to our decision to focus on the initial gang takedown, while controlling for subsequent takedowns. In model 4, we re-estimate treatment effects focusing on the subset of developments that experienced only a single takedown during the study period. Model 5 retains data for all developments in the sample but only up until a development's second gang takedown and Model 6 estimates the effect of all gang takedowns rather than the initial takedown. Next, we test several additional sample restrictions. In model 7, we drop housing developments that were selected to participate the City's "Mayor's Action Plan for Community Safety," a program that saturated fifteen NYCHA developments with a suite of infrastructure upgrades and enhanced social services during the study period.[40] In model 8, we control for the timing of the NYPD's neighborhood policing program, a novel community policing program that was intended to reduce crime.[41] In model 9, we retain only balanced panels — that is, developments for which we have two full years of pre- and post-intervention

[40]The MAPSTAT housing developments are: Butler, Castle Hill (I and II), Patterson, Polo Grounds, St. Nicholas, Wagner, Boulevard, Brownsville, Bushwick, Ingersol, Red Hook (East and West), Tompkins, Van Dyke (I and II), Queensbridge (I and II) and Stapleton. See: `https://www1.nyc.gov/assets/operations/downloads/pdf/mmr2016/mayors_action_plan_for_neighborhood_safety.pdf`.

[41]See: `https://www1.nyc.gov/site/nypd/bureaus/patrol/neighborhood-coordination-officers.page`.

data. Finally, though we prefer a Poisson model for several reasons, in model 10, estimate treatment effects using a negative binomial regression model.[42] While precision varies from model to model, we see little evidence that treatment effects are sensitive to choices made in our analysis.[43]

6.3 Displacement of Crimes

In studying any place-based intervention that might have an impact on public safety, a critical question is whether the intervention has reduced crime or has merely displaced it to other areas in a city (Reppetto, 1976; Cornish and Clarke, 1987; Eck, 1993; Guerette and Bowers, 2009). While both crime reduction and displacement are interesting from a scientific perspective, an intervention that merely shifts crime from one location to another is far less attractive to a policymaker than one which leads to a genuine improvement in public safety.[44] The conventional approach to studying displacement is to examine whether an intervention leads to a rise in crime in adjacent areas.[45] On the other hand, if an

[42]Sometimes crime counts are modeled using negative binomial regression models due to concerns about overdispersion in the data. For several reasons, we prefer Poisson regression in this context. First, tests for overdispersion do not distinguish between overdispersion and misspecification — see Berk and MacDonald (2008) and Blackburn (2015). Consequently, it is *a priori* unclear when overdispersion actually exists and is therefore an issue. Second, Poisson regression with robust standard errors is a more robust estimator than negative binomial regression when the distribution is not, in fact, distributed according to the negative binomial (Wooldridge, 2002). Finally, negative binomial regression yields inconsistent estimates when fixed effects are used in a model (Lancaster, 2000). This is not an issue for Poisson regression (Allison and Waterman, 2002). As our models do include fixed effects, the Poisson regression model is a more appropriate choice.

[43]We also demonstrate that treatment effects are robust to several additional choices. First, we show that our estimates do not depend on any one highly leveraged housing development. This is shown in **Appendix Figure 6** which plots the estimated treatment effect, re-running the model dropping each housing development one at a time. Second, we show that estimates do not change dramatically when add additional years to the dataset. In **Appendix Figure 7**, we estimate effects excluding takedowns that occurred in a given year in order to test whether effects are not driven by a small subset of the data. The estimates remain very similar. Finally, **Appendix Figure 8** shows the sensitivity of the estimates to our choice of a two-year bandwidth around the initial gang takedown. Standard errors shrink as the bandwidth increases but estimates are not sensitive to bandwidth choice.

[44]Because of its central importance in interpreting empirical estimates, testing for displacement has received a great deal of attention in experimental and quasi-experimental studies of hot spots policing (Sherman and Weisburd, 1995; Braga and Bond, 2008; Braga et al., 2014; Groff et al., 2015; Blattman et al., 2017) disorder reduction (Braga and Bond, 2008; Branas et al., 2011; MacDonald et al., 2016; Branas et al., 2018), closed circuit television cameras (Waples et al., 2009; Welsh and Farrington, 2009; Piza et al., 2014, 2015) and other place-based interventions (Grogger, 2002; Ridgeway et al., 2019).

[45]Measuring crime displacement is challenging for a number of reasons, chief among them that it is unclear *a priori* where crime might go upon being displaced. Will crime merely be pushed "around the corner" (Blattman et al., 2017) or will it migrate to some more distal area which shares one or more key characteristics with the treated area? Given the difficulty of exhaustively testing for all forms of displacement, the norm in the empirical literature is to focus on adjacent areas (Guerette and Bowers, 2009).

intervention causes crime to *fall* in adjacent areas, then there is thought to be evidence of "diffusion of benefits," which captures the idea that even untreated locations might benefit from the general perception that an intervention is in use (Clarke and Weisburd, 1994; Guerette and Bowers, 2009).

We study displacement in two ways. First, we test whether crime is changing in the 500 square foot region around our treated areas — that is, the area that is between 500 and 1,000 feet from a treated public housing development. Second, for each public housing development in our sample, we identify the three closest never-treated housing developments and test for changes in crime after a gang takedown in the closest one, two and three developments. Results are presented in **Table 5**. Consistent with research on gang injunctions (Ridgeway et al., 2019), we do not detect evidence of crime displacement using either heuristic. If anything, for shootings, coefficients are negative indicating that there may have been a diffusion of benefits to neighboring places. This would not be unexpected as gang takedowns, while centralized near housing developments, sometimes include arrests that take place in other locations. A second implication of this analysis is that to the extent that gang takedowns might deter gang activity in nearby locations, Table 5 provides suggestive albeit speculative evidence on the degree to which the intervention deters rather than simply incapacitates violence. To the extent that the gang takedowns facilitate deterrence, the deterrence effects do not appear to be large, at least in adjacent areas.

6.4 Extensions

6.4.1 Do gang takedowns lead to a sustained increase in enforcement?

In Section 6.1, we demonstrated that gun violence declines by between 30-60 percent in the aftermath of a gang takedown and that this effect persists for at least a year and perhaps for as long as two years. We now consider whether these reductions in crime are due to the effects of the gang takedown itself or a general increase in police presence and enforcement activity that may follow in the aftermath of a takedown. We explore this hypothesis in **Table 6** by considering whether a range of indicators of police activity — including arrests and Terry stops — change after a gang takedown. With respect to arrests, we consider total arrests as well as arrests for the sale of drugs, drug possession and, specifically, misdemeanor marijuana possession. These types of drug arrests are a reasonable proxy for police activity as such arrests are generally made as a result of officer surveillance rather than originating from a citizen 911 call. Referring to Table 6, we see little evidence that total arrests or arrests for the sale of drugs change after a gang takedown. With respect to arrests for drug possession including misdemeanor marijuana possession, if anything, there is evidence of a modest decline in such arrests after a gang takedown though the results are not significant at conventional levels. With respect to Terry stops, there is likewise little evidence that these are changing in the aftermath of gang takedowns. We note that these results are not an artifact of changes in policing in NYC generally as all models continue to condition on week-year (as well as NYCHA development) fixed effects. Likewise, the results are not explained by the NYPD's neighborhood policing program as effects persist when we control for the program's timing. [46]

6.4.2 Treatment Effect Heterogeneity

Next, we explore the extent to which there is heterogeneity in the effect of the takedowns. Recognizing that each NYC borough has a different district attorney's office, we begin by estimating models separately by borough for Manhattan, Brooklyn and the Bronx, the three NYC boroughs which account for 92 percent of the takedowns. These results are presented in **Appendix Figure 10**. Models are underpowered to detect effects at the borough level though we note that the largest estimate is for Brooklyn and the smallest estimate is for the Bronx.

[46] In **Appendix Figure 9**, focusing on misdemeanor marijuana arrests and Terry stops, we present results for the event study. These figures also show little evidence that there were pre-trends in discretionary police activities. There is some evidence that, if anything, Terry stops declined in the immediate aftermath of a takedown.

We next consider whether there is a dose-response relationship between shootings and the size of the gang takedown. We begin by showing what happened to violence — in this case, the aggregate of assaults and shootings — in and around Grant and Manhattanville Houses, two nearby Manhattan housing developments which experienced "the largest gang raid in NYC history"[47] As is evident from **Appendix Figure 11**, there was a sizeable decline in violence in these housing developments in the aftermath of the gang takedown. To investigate this more systematically, in **Figure 9** we plot estimated treatment effects for different subsets of our housing developments based on the size of the initial gang takedown. There is evidence that larger takedowns lead to larger treatment effects — for instance, when we focus on the n=21 developments with an initial takedown that consisted of at least five conspiracy arrests, the estimated treatment effect is approximately 50 percent ($p < 0.01$).

7 Discussion

This research leverages a natural experiment brought about by the NYPD's remarkable shift from a regime of mass enforcement centered around frequent street stops and field interrogations to a more surgical regime of precision policing in which the department concentrated its focus on a small number of suspected criminal organizations that are thought to be the driver of an outsize share of the City's gun violence. The signature policy of the new regime was a series of targeted gang enforcement actions conducted by the police in cooperation with the City's five district attorney's offices. Since 2011, there have been more than 600 of these "gang takedowns" across the city, nearly one third of which were centered around the City's public housing communities. As the gang takedowns have increased in size and scope, the number of official Terry stops made by NYPD officers declined by approximately 98 percent. Using data on gang takedowns in and around seventy-three of the City's public housing communities

[47] See `https://www.dnainfo.com/new-york/20140605/west-harlem/harlem-gang-takedown-is-largest-city-history-officials-say/`. We aggregate shootings and assaults here in order to increase the density of the crime data.

which experienced at least one takedown during the study period, we document evidence that gun violence declines by approximately one-third in the aftermath of a takedown. Remarkably, these impacts appear to last for at least one year and for perhaps as long as two years despite the fact that there is little evidence of any sort of sustained law enforcement presence after the initial gang takedown. We do not detect evidence of crime displacement to adjacent areas or to nearby public housing communities.

Prior research has investigated the substantial decline in gun violence in NYC during the last decade and has concluded that the public safety dividend of the last few years is a direct result of the federal court ruling which curtailed the NYPD's ability to engage in the intensive use of street stops and field interrogations (Sullivan and O'Keeffe, 2017). The conclusion of this research — that curtailing proactive policing can reduce major crime — is provocative but fails to account for the shift in the policy regime during this period from one of mass enforcement to one of precision policing.

How many shootings and homicides are abated by the takedowns? Our estimates suggest that the number of weekly shootings per development in the 73 housing developments in our sample declines by one third — from approximately 0.045 per week to 0.03 per week — in the one-year period after a gang takedown. Across 73 developments, this suggests that the takedowns abated slightly more than one shooting per week each year. During the 2011-2018 period, shootings per year in NYC declined by 50 percent from 1,509 to 754. Over this seven year period, the cumulative decline in shootings relative to NYC's 2011 rate was 3,237. In public housing communities, shootings declined by 52 percent, from 653 to 313, a cumulative decline of 1,582 during the seven year period. This, in turn, suggests that the gang takedowns may explain approximately 22 percent (352/1,582) of the cumulative decline in shootings in and around public housing in NYC during the 2011-2018 period and perhaps 11 percent (352/3,237) of the decline in shootings citywide. With respect to homicide, the estimates were noisier and were only significant at the $p < 0.1$ level. Nevertheless, taking the point estimates at face value suggests that the gang takedowns abate approximately 17 homicides per year which suggests that the takedowns may explain at least 10 percent of the City's homicide decline after 2011.

This research adds an important data point to the growing scholarly literature on gang enforcement which includes research on gang injunctions, truancy, curfew enforcement and "pulling levers" strategies which include a wide variety of interventions which have been inspired by *Operation Ceasefire* and programs like *Cure Violence*. Relative to these studies, which center on deterrence-focused strategies which empower law enforcement to disrupt gang activity, this research is the first to consider the impact of a gang takedowns strategy which focuses predominantly on incapacitation. We also present a test of the efficacy of the shift from a regime of mass enforcement to a precision policing regime at scale. We note that these findings describe the marginal impact of precision enforcement across time and space — to the extent that the regime shift has affected crime throughout the City, our estimates may, in fact, be conservative estimates of the impact of a precision policing regime more generally.

Gun violence imposes enormous costs on the most disadvantaged communities in the United States (Lee et al., 2014). Beyond the direct impacts of violence itself which are considerable (Ludwig and Cook, 2001), the long-term effects of gun violence include impacts on children's cognition and educational outcomes (Sharkey, 2010; Sharkey et al., 2014; Sharkey, 2018) as well as health and well-being (Breslau et al., 1999; Cook and Ludwig, 2002). Sadly, this burden may compound itself generation after generation, becoming an engine for the intergenerational transmission of violence (Ehrensaft et al., 2003). The findings we report in this paper suggest that more surgical policing tactics are a promising avenue through which law enforcement can abate the most socially costly types of crimes while limiting the extent to which mass enforcement widens the net of the criminal justice system for communities of color. Notably, unlike gang injunctions which empower street level officers to interact more extensively with suspected gang members, the gang takedowns we study do not change the mandate of the "street level bureaucrats" (Lipsky, 1971). As such, to the extent that this approach can be exported to other cities, this research suggests a potential pathway through which law enforcement agencies can address the dual plagues of under- and over-policing in poor minority communities (Leovy, 2015).

Several points of caution are worth noting. First, while the effects persist for up to two years after an initial gang takedown, the violence reductions we observe do not continue in perpetuity. The data thus suggest that gang takedowns offer a means of temporarily relieving the symptoms of the disease of gun violence rather than offering a cure. Second, we note that the effects do not tend to spill over appreciably to nearby locations, thus raising the prospect that deterrence effects may potentially be small relative to the incapacitation value of the intervention. That said, the precise mechanisms through which the gang takedowns improve public safety remain unresolved. Third, advocates and legal scholars have raised a number of due process and fairness concerns about gang takedowns and have suggested that while gang takedowns have been offered as a less invasive solution to previous stop and frisk practices, they may continue to create collateral damage for poor minority communities. This is an especially important consideration in light of recent research by Owens et al. (2019) which finds that gang injunctions, a related intervention, tend to reduce home values in a community despite having an effect on crime. Finally, while our findings broadly conform to those of Ridgeway et al. (2019) which studies the impact of gang injunctions, they contrast with those of Saunders et al. (2016) who studied Chicago's use of a "strategic subjects list" as a means of targeting enforcement resources in a person-focused policing strategy. While the interventions employed in Chicago do not appear to include targeted gang takedowns, the contrasting evidence reminds us that even when law enforcement is able to effectively identify high-risk individuals, an effective precision policing regime requires a robust mechanism to either incapacitate or deter potential offenders.

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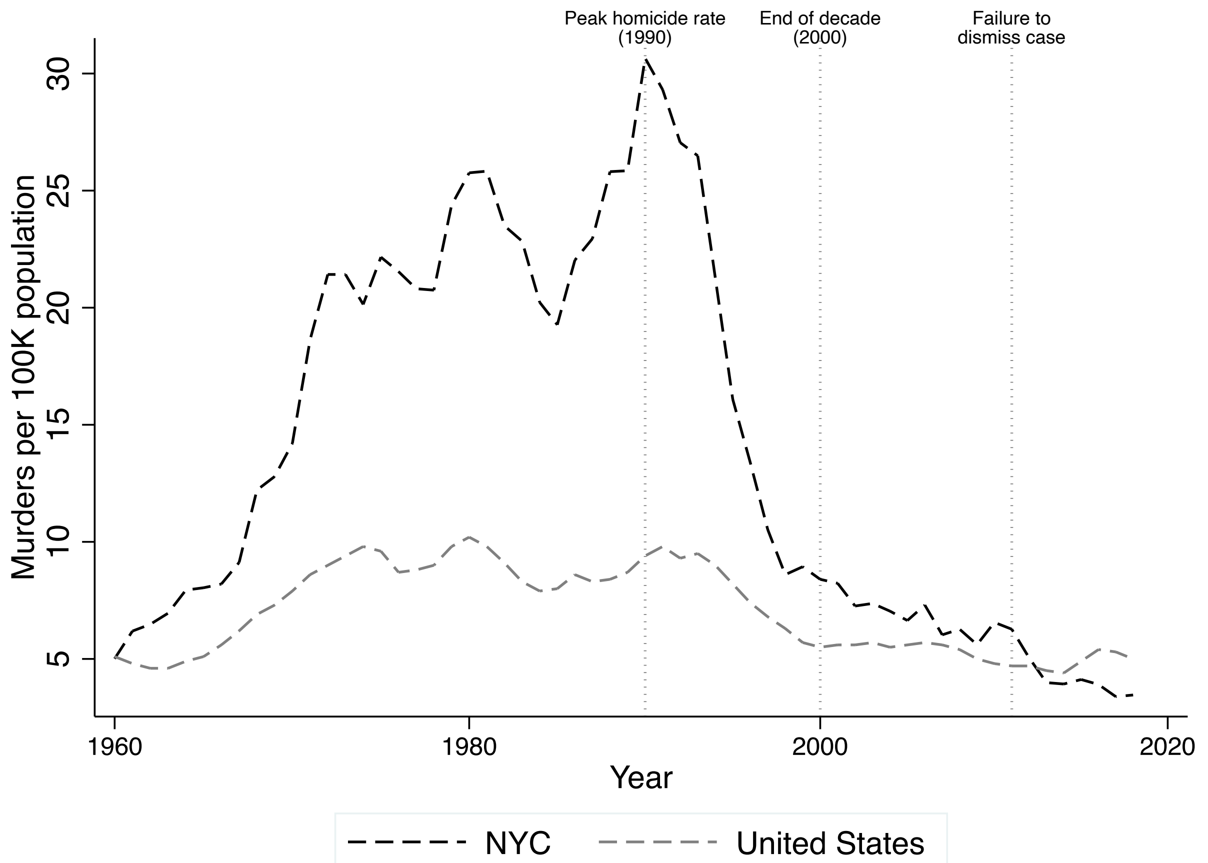
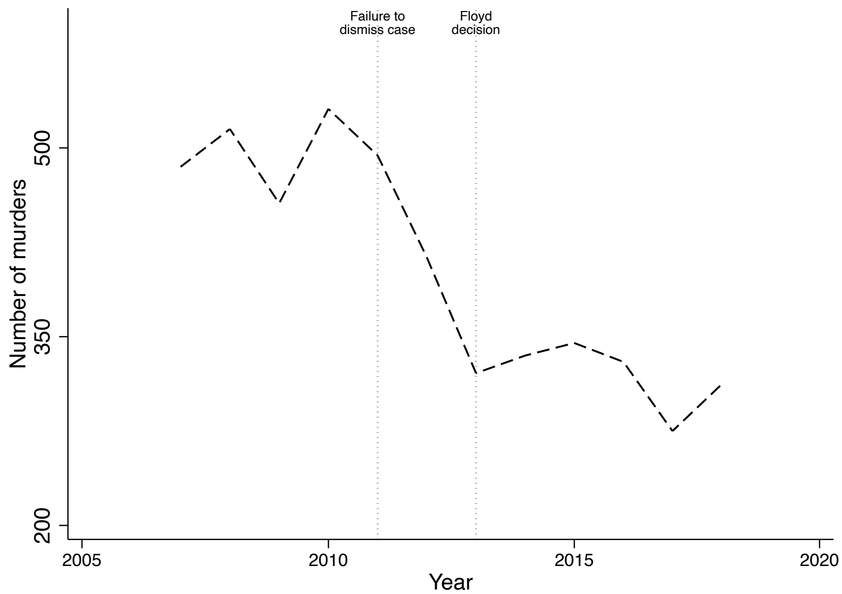
Figure 1: Trends in Murders per 100,000 Population, New York City vs. United States (1960-2018)



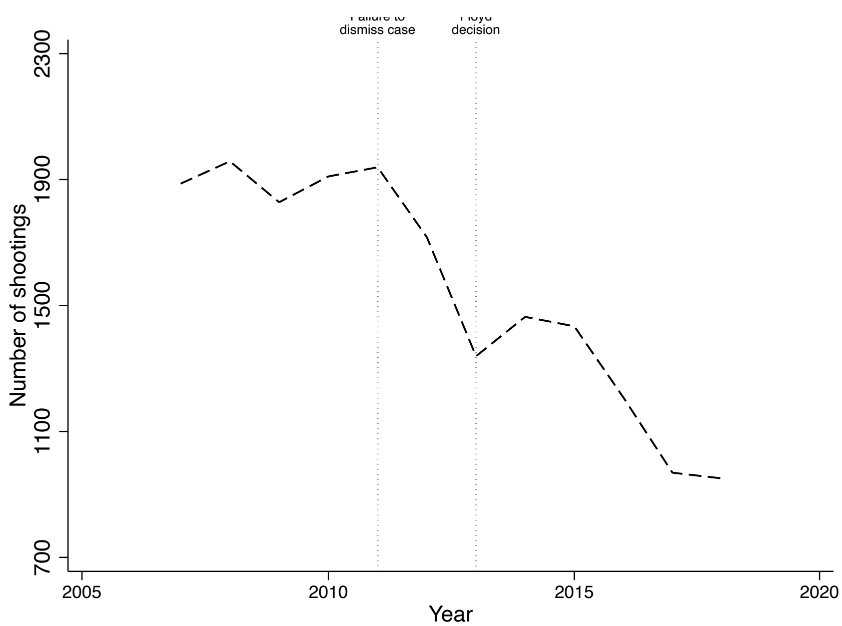
Note: Figure plots the number murders per 100,000 population in New York City and in the United States from 1960 until 2018 according to the Federal Bureau of Investigation's Uniform Crime Reporting program. Data were accessed from Jacob Kaplan's "Crime Data Tool" at the following URL: `https://jacobdkaplan.com/`. In the figure, there are vertical lines corresponding to 1990, the year in which NYC's homicide rate peaked and 2000, the end of the decade as well as 2011 which is the beginning of our sample period.

Figure 2: Trends in Gun Violence, New York City (2007-2018)

Panel B: Murders



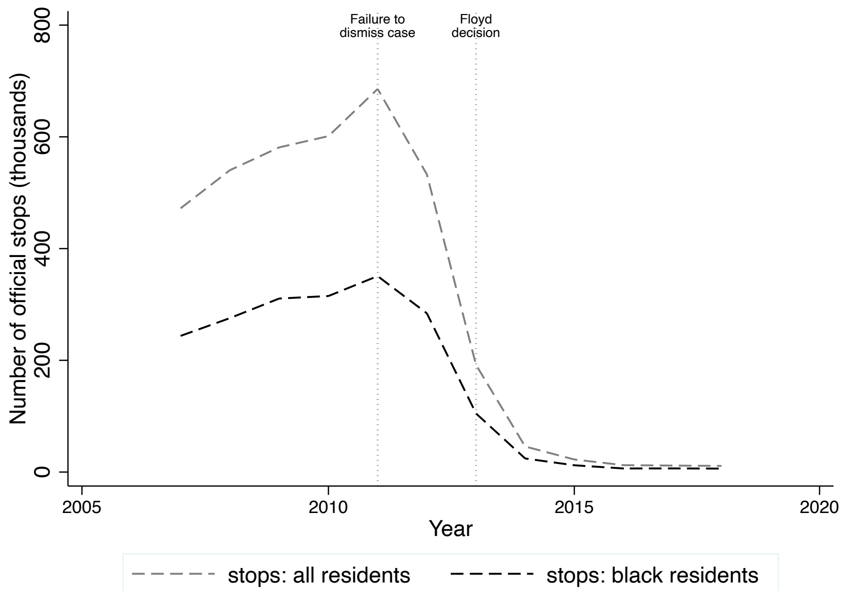
Panel B: Shootings



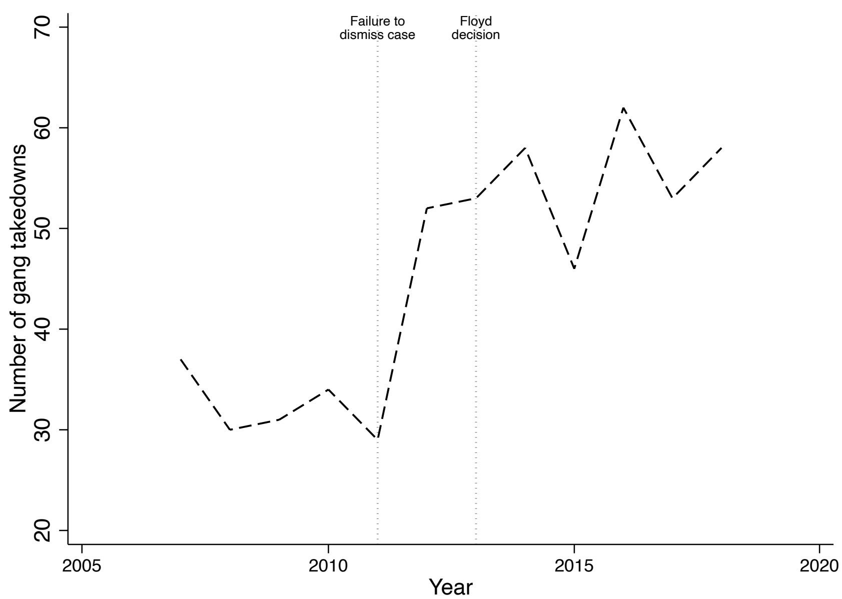
Note: Panel A plots the total number of murder victims in NYC from 2007-2018. Panel B plots the number of shooting victims. In both graphs, there are vertical lines corresponding to 2011, the year that a federal court failed to dismiss the *Floyd* case in a preliminary hearing and 2013, the year the *Floyd* case was decided. Data were obtained from the New York Police Department's Annual Crime and Enforcement Reports, accessed using the following URL: `https://www1.nyc.gov/site/nypd/stats/reports-analysis/crime-enf.page`.

Figure 3: Trends in Police Enforcement, New York City (2007-2018)

Panel A: Police Stops

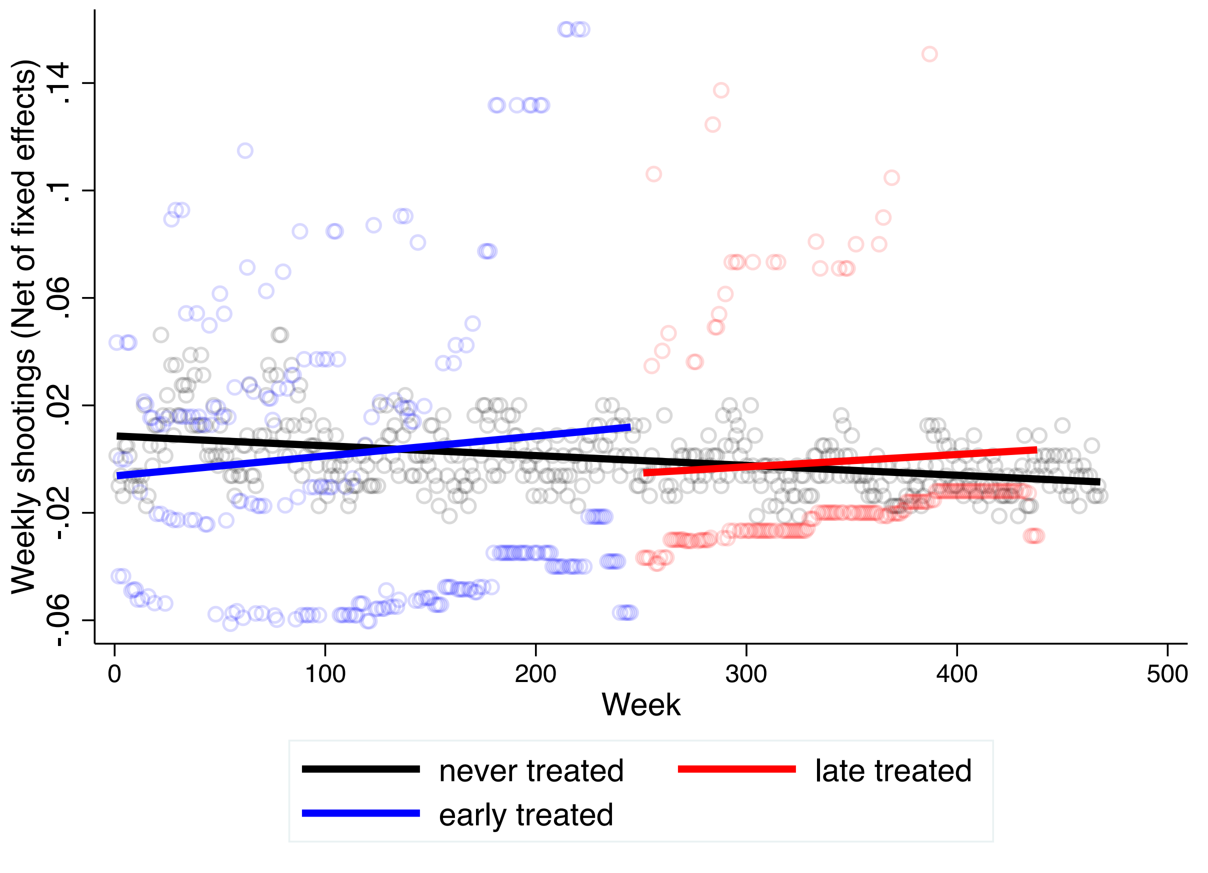


Panel B: Gang Takedowns



Note: Panel A plots the total number of documented Terry stops by NYPD officers (gray line) and stops of black residents (black line) from 2007-2018. Panel B plots the number of gang takedowns as proxied by two or more conspiracy arrests in a given NYCHA development-week. In both graphs, there are vertical lines corresponding to 2011, the year that a federal court failed to dismiss the *Floyd* case in a preliminary hearing and 2013, the year the *Floyd* case was decided.

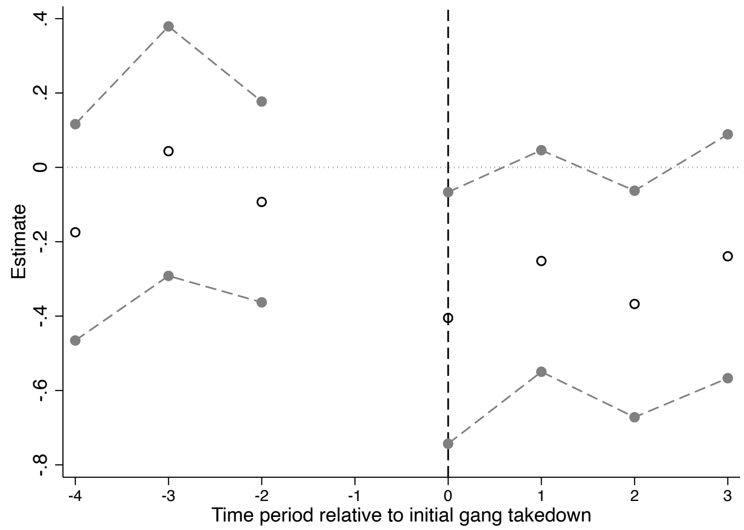
Figure 4: Test of Parallel Trends: Never Treated, Early Treated and Late Treated Developments (Shootings)



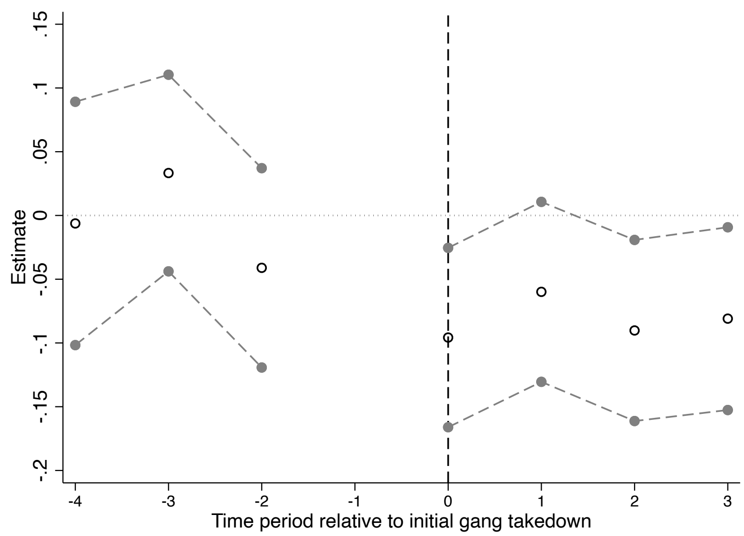
Note: Figure plots the number of weekly shootings in the pre-treatment period, residualized to remove NYCHA development fixed effects, separately for three groups of developments: 1) never treated developments, 2) developments whose initial gang takedown occurs early in the data (i.e., after week 250) and 3) developments whose initial gang takedown occurs late in the data (i.e., after week 250). For each group, a trend line is drawn through the data. The never treated group is plotted using the black line, the late treated group is plotted using the red line and the early treated group is plotted using the blue line. Consistent with subsequent models, the ever treated groups are plotted using the time periods that lie within a bandwidth of two years of the time of a development's initial gang takedown.

Figure 5: Event Study Analysis

Panel A: Shootings

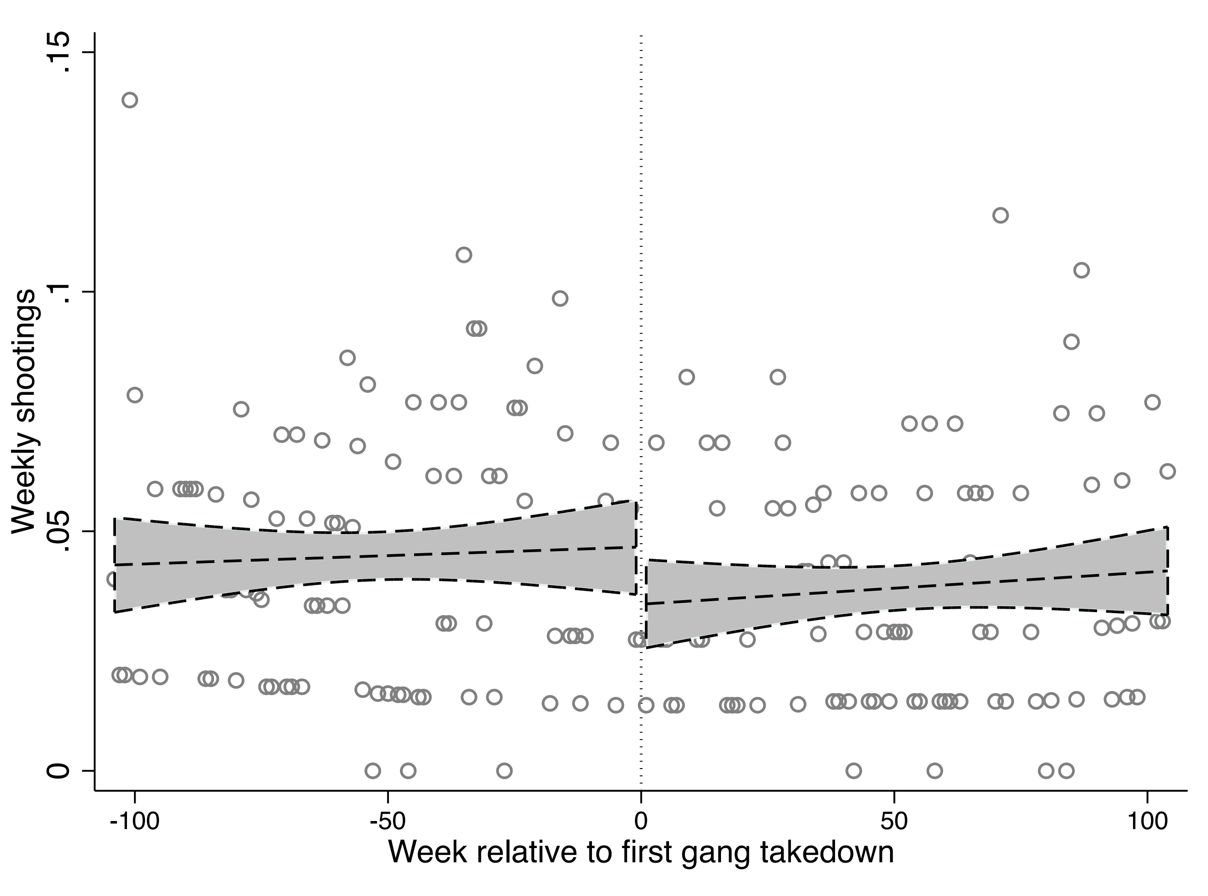


Panel B: Violent Crimes



Note: Figure plots coefficients from the event study regression described in Equation (3) for weekly shootings (Panel A) and weekly violent crimes (Panel B). Each estimate represents a six-month bin, relative to a NYCHA development's initial gang takedown. For each six-month bin, the point estimate is the coefficient from a Poisson regression of the number of weekly crime on the event study terms, conditional on a cubic function in time. Estimates are relative to the first time period prior to the intervention. Standard errors are clustered at the NYCHA development to account for heteroskedasticity as well as arbitrary serial correlation.

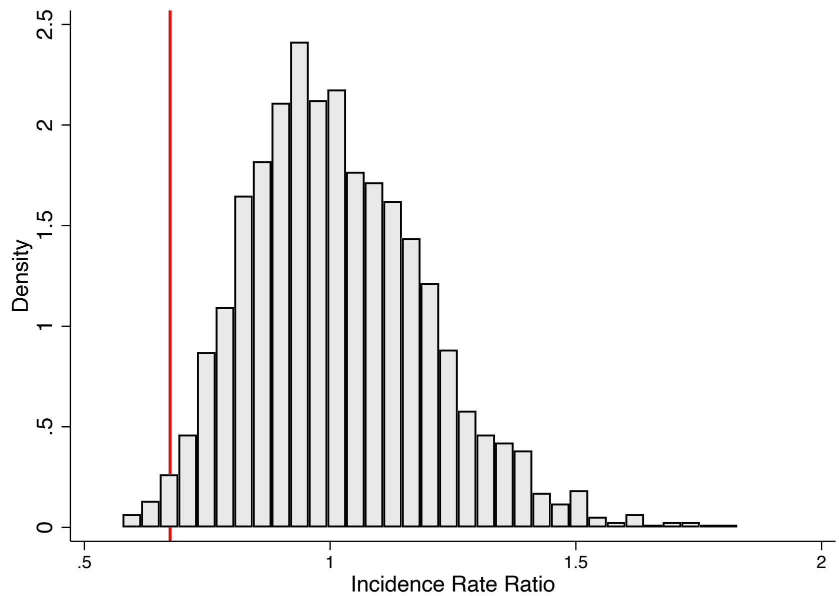
Figure 6: Regression Discontinuity Estimate of the Treatment Effect (Shootings)



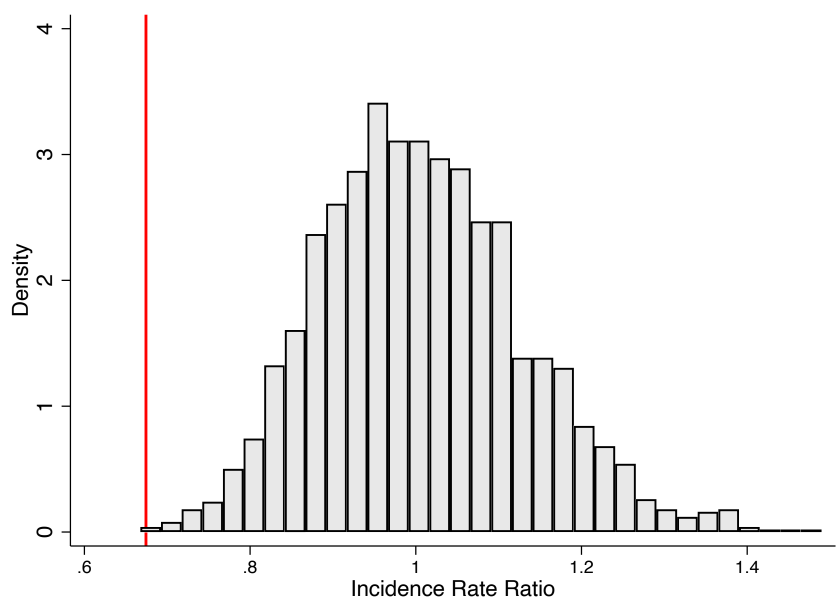
Note: Figure plots the average number of weekly shootings (y-axis) in and around NYCHA developments against time relative to a NYCHA development's first gang takedown (x-axis). The raw data are plotted using the gray open circles. A best fit line and the associated 95 percent confidence interval are drawn through the data separately for the pre- and post-takedown periods. The regression discontinuity estimate at the threshold in the running variable is -0.012 shootings per week relative to a pre-period mean of 0.045 ($p = 0.07$).

Figure 7: Actual Versus Placebo Treatment Effects (Shootings)

Panel A: Shootings

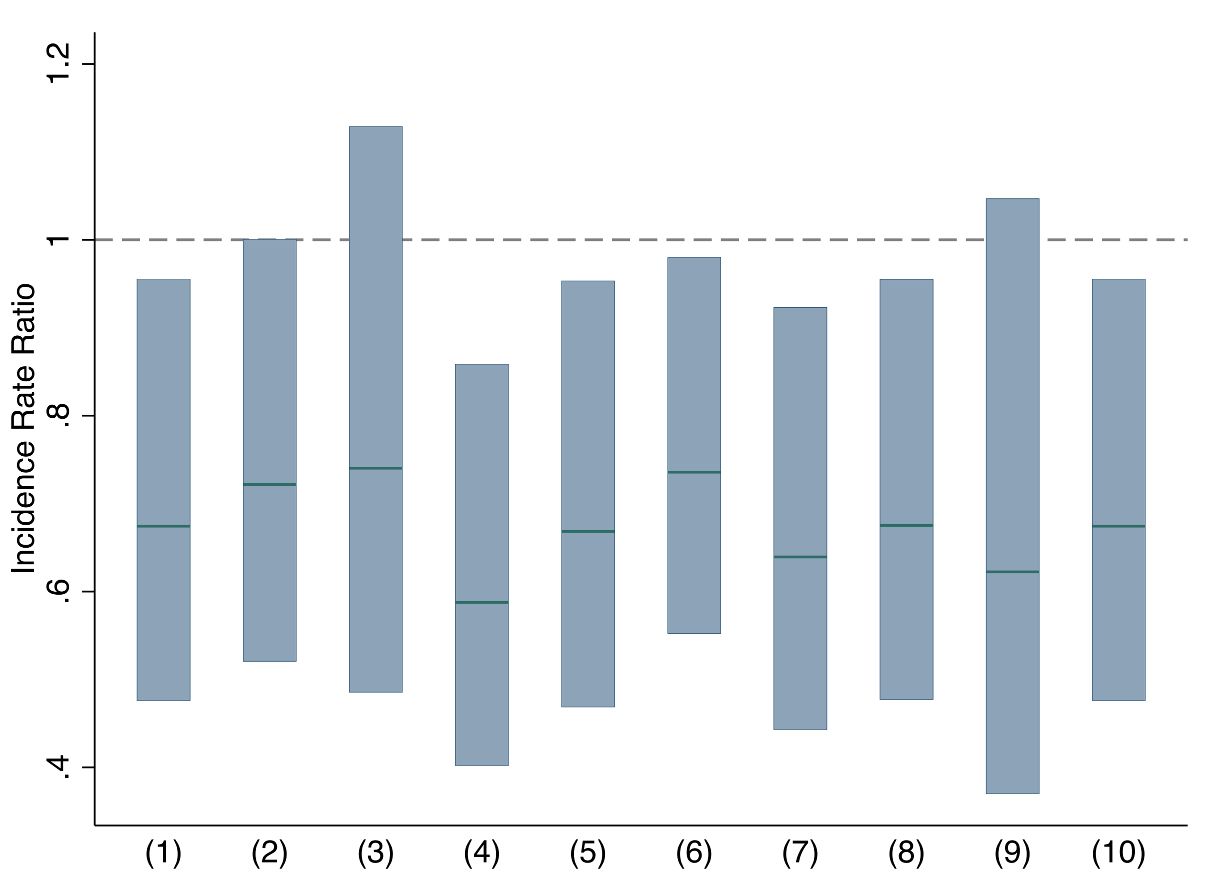


Panel B: Violent Crimes



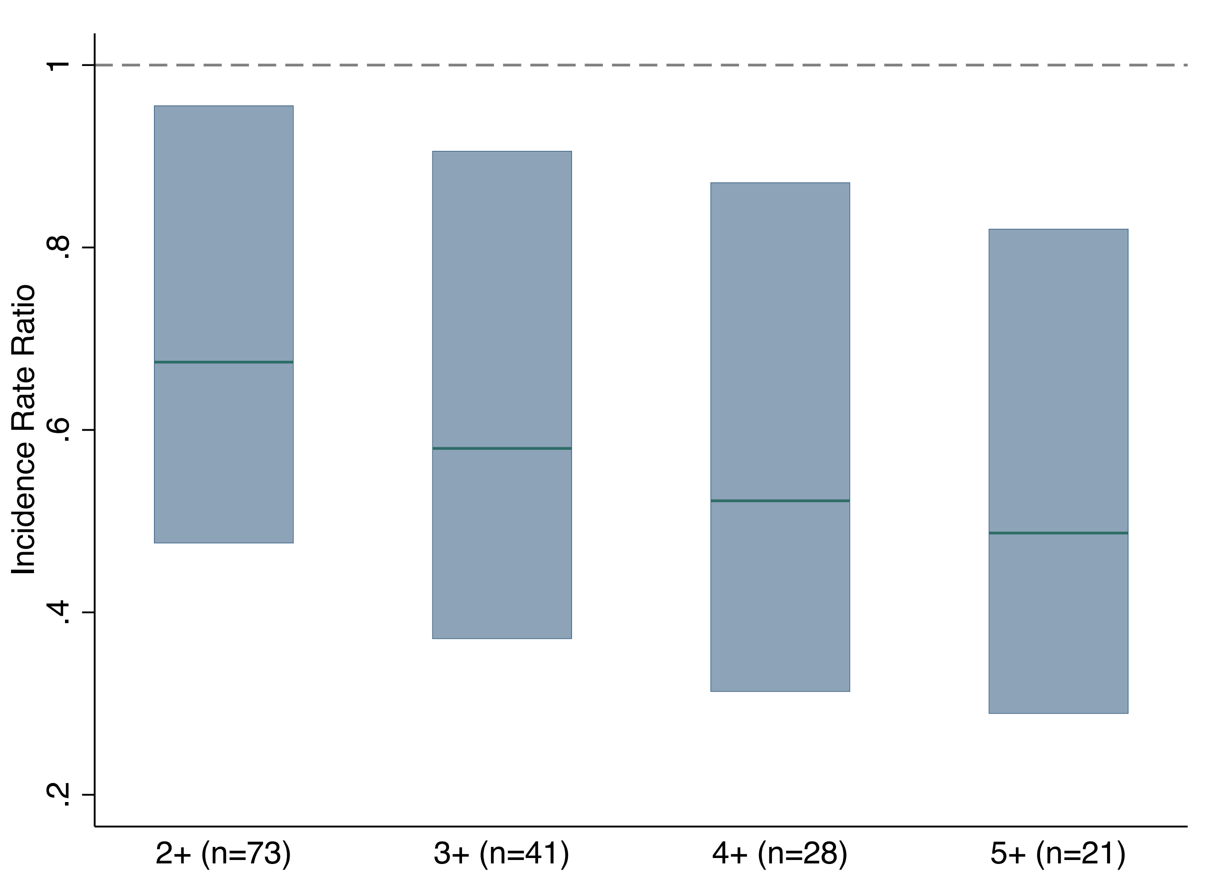
Note: Figures plot the density of placebo regression estimates from a simulation exercise in which we re-randomize (2,000 times) the week of a gang takedown to each NYCHA development and run a Poisson regression of the count of weekly shootings on the placebo treatment indicator, the placebo post-treatment indicator and NYCHA development and week-year fixed effects. In Panel A, we study shootings; in Panel B we study all violent crimes. The actual estimate reported in column 1 of Table 4 is represented by the solid red line; the histogram provides the empirical distribution of placebo treatment effects.

Figure 8: Robustness of Estimated Treatment Effects (Shootings)



Note: Figure plots estimates from a series of Poisson regressions in which we regress the count of weekly shootings on the treatment indicator, the post-treatment indicator and NYCHA development and week-year fixed effects. We begin with our preferred estimate (Model 1) which was reported in column (1) of Table 3. Models 2 and 3 employ alternative definitions of a gang takedown — Model 2 uses three conspiracy arrests to define and takedown and Model 3 uses only the subset of takedowns that can be verified via a directed media search. Models 4, 5 and 6 test robustness to our decision to focus on a development's initial gang takedown. Model 4 excludes developments which experienced more than one gang takedown during the sample period, Model 5 retains data for all developments in the sample but only up until a development's second gang takedown and Model 6 estimates the effect of all gang takedowns rather than the initial takedown. Model 7 excludes the City's 15 MAPSTAT developments and Model 8 adds a control variable for whether a development lies within a precinct which has implemented the NYPD's signature neighborhood policing program at a given point in time. Model 9 focuses on the subset of developments for which we have weakly balanced panels — that is, two full years of pre- and post-intervention data. Model 10 uses negative binomial instead of Poisson regression. Confidence intervals are asymmetric as estimates are incident rate ratios.

Figure 9: Estimated Treatment Effect by Size of Gang Takedown (Shootings)



Note: Figure plots estimates from a series of Poisson regressions in which we regress the count of weekly shootings on the treatment indicator, the post-treatment indicator and NYCHA development and week-year fixed effects. The first bar represents our preferred estimate which was reported in column (1) of Table 3 and includes all seventy-three developments that experienced at least one gang takedown, as defined by two or more conspiracy arrests. The remaining models use the subset of the data for which the initial gang takedown consisted of at least 3, 4 or 5 arrests. Confidence intervals are asymmetric as estimates are incident rate ratios.

Table 1: Gang Takedowns by Borough and Nexus to Public Housing

Borough	2010 Population	All takedowns	Public housing takedowns
Bronx	1,385,108 (17%)	153 (34%)	33 (30%)
Brooklyn	2,504,700 (31%)	102 (22%)	30 (28%)
Manhattan	1,585,873 (19%)	145 (32%)	41 (38%)
Queens	2,203,722 (27%)	43 (9%)	4 (4%)
Staten Island	468,730 (6%)	12 (3%)	1 (1%)

Note: Arrest data were obtained from NYC's Open Data site. Takedowns refer to incidents in which at least two conspiracy arrests were made in the same location.

Table 2: Demographic Composition of Conspiracy Arrestees

Variable	All	Public housing
Age =		
< 18	191 (6%)	66 (8%)
18-24	1,254 (42%)	362 (44%)
25-44	1,269 (42%)	321 (39%)
45-64	277 (9%)	63 (8%)
> 64	18 (1%)	3 (< 1%)
Gender =		
Male	2,733 (91%)	739 (91%)
Female	276 (9%)	76 (9%)
Race =		
White	175 (6%)	44 (5%)
Black	1,611 (54%)	514 (63%)
Hispanic	1,143 (38%)	251 (31%)
Other/Unknown	80 (3%)	6 (1%)

Note: Arrest data were obtained from NYC's Open Data site.

Table 3: Summary Statistics

	Before Initial Takedown				After Initial Takedown			
	Mean	S.D.	Min.	Max.	Mean	S.D.	Min.	Max.
Homicides	0.010	0.109	0	3	0.008	0.088	0	2
Shootings	0.045	0.214	0	3	0.038	0.196	0	3
Assaults	0.510	0.825	0	6	0.527	0.83	0	8
Robberies	0.388	0.662	0	5	0.355	0.648	0	7
Property crimes	1.822	1.925	0	23	1.996	2.155	0	23
Total arrests	10.285	9.522	0	69	9.898	9.843	0	91
Drug sale arrests	0.603	1.203	0	15	0.513	1.081	0	13
Drug possession arrests	1.148	1.708	0	27	0.761	1.325	0	16
Marijuana possession arrests	1.067	1.653	0	27	0.684	1.251	0	16
Terry stops	10.402	16.91	0	219	4.307	10.318	0	136

Note: Table reports the mean and standard deviation as well as the minimum and maximum values of ten outcome variables. The top panel reports summary statistics for our crime variables; the bottom panel reports summary statistics for our measures of police proactivity. For each variable, we report separate summary statistics for the pre- and post-intervention time periods.

Table 4: Main Estimates

	(1)	(2)	(3)	(4)
Homicides	-0.794*	-0.842*	-0.936**	-0.687*
	(0.419)	(0.457)	(0.468)	(0.361)
	[-55%]	[-57%]	[-61%]	[-50%]
Shootings	-0.394**	-0.350*	-0.439**	-0.366**
	(0.178)	(0.202)	(0.171)	(0.166)
	[-33%]	[-30%]	[-36%]	[-31%]
Assaults	-0.078	-0.066	-0.099**	-0.068
	(0.049)	(0.048)	(0.050)	(0.052)
	[-8%]	[-6%]	[-9%]	[-7%]
Robberies	-0.009	0.004	-0.052	-0.001
	(0.067)	(0.066)	(0.068)	(0.056)
	[-1%]	[0%]	[7%]	[6%]
Property Crimes	0.034	0.052	0.026	0.041
	(0.036)	(0.039)	(0.034)	(0.038)
	[3%]	[5%]	[3%]	[4%]
NYCHA effects	X	X	X	X
Week-year effects	X	X	X	
NYCHA linear trend		X		
Borough-week-year effects			X	
Cubic time trend				X

Note: Estimates are from Poisson regressions of the weekly count of crime on an indicator variable for a gang takedown as well as an indicator for the post-gang takedown period. Models are run using a bandwidth of up to two years around each takedown. All models condition on NYCHA development fixed effects but each model accounts differently for time trends. Model 1 conditions on week-year fixed effects and Model 2 adds a NYCHA development-specific linear time trend. Model 3 conditions replaces the week-year fixed effects with interacted borough-week-year fixed effects. Model 4 instead conditions on a cubic time trend. In all models, standard errors are clustered at the NYCHA development level to account for arbitrary serial correlation in the regressors. In each cell, the first row is the raw coefficient. Below this we present the clustered standard error (in parentheses) and the percentage change obtained by subtracting 1 from the estimated IRR and multiplying by 100 (in square brackets). Significance: * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$.

Table 5: Displacement of Crimes

	(1)	(2)	(3)	(4)
Homicides	-0.551	0.047	0.013	0.008
	(0.492)	(0.173)	(0.135)	(0.127)
	[-42%]	[5%]	[1%]	[1%]
Shootings	-0.275	-0.319***	-0.123	-0.031
	(0.241)	(0.132)	(0.095)	(0.090)
	[-24%]	[-27%]	[-12%]	[-3%]
Assaults	0.011	-0.063	-0.043	-0.017
	(0.074)	(0.037)	(0.027)	(0.025)
	[1%]	[-6%]	[-4%]	[-2%]
Robberies	0.057	-0.012	-0.022	-0.026
	(0.088)	(0.045)	(0.030)	(0.030)
	[6%]	[-1%]	[-2%]	[-3%]
Property Crimes	0.004	0.015	0.026	0.004
	(0.039)	(0.038)	(0.022)	(0.021)
	[0%]	[2%]	[3%]	[0%]
NYCHA effects	X	X	X	X
Week-year effects	X	X	X	X

Note: Estimates are from Poisson regressions of the weekly count of crime in a particular off-development region on an indicator variable for a gang takedown as well as an indicator for the post-gang takedown period. Models are run using a bandwidth of up to two years around each takedown. All models condition on NYCHA development and week fixed effects. Model 1 counts weekly crimes in the area that is between 500 and 1,000 feet away from the treated housing development. Models 2, 3 and 4 count crimes in the nearest 1, 2 and 3 untreated developments. In all models, standard errors are clustered at the NYCHA development level to account for arbitrary serial correlation in the regressors. In each cell, the first row is the raw coefficient. Below this we present the clustered standard error (in parentheses) and the percentage change obtained by subtracting 1 from the estimated IRR and multiplying by 100 (in square brackets). Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

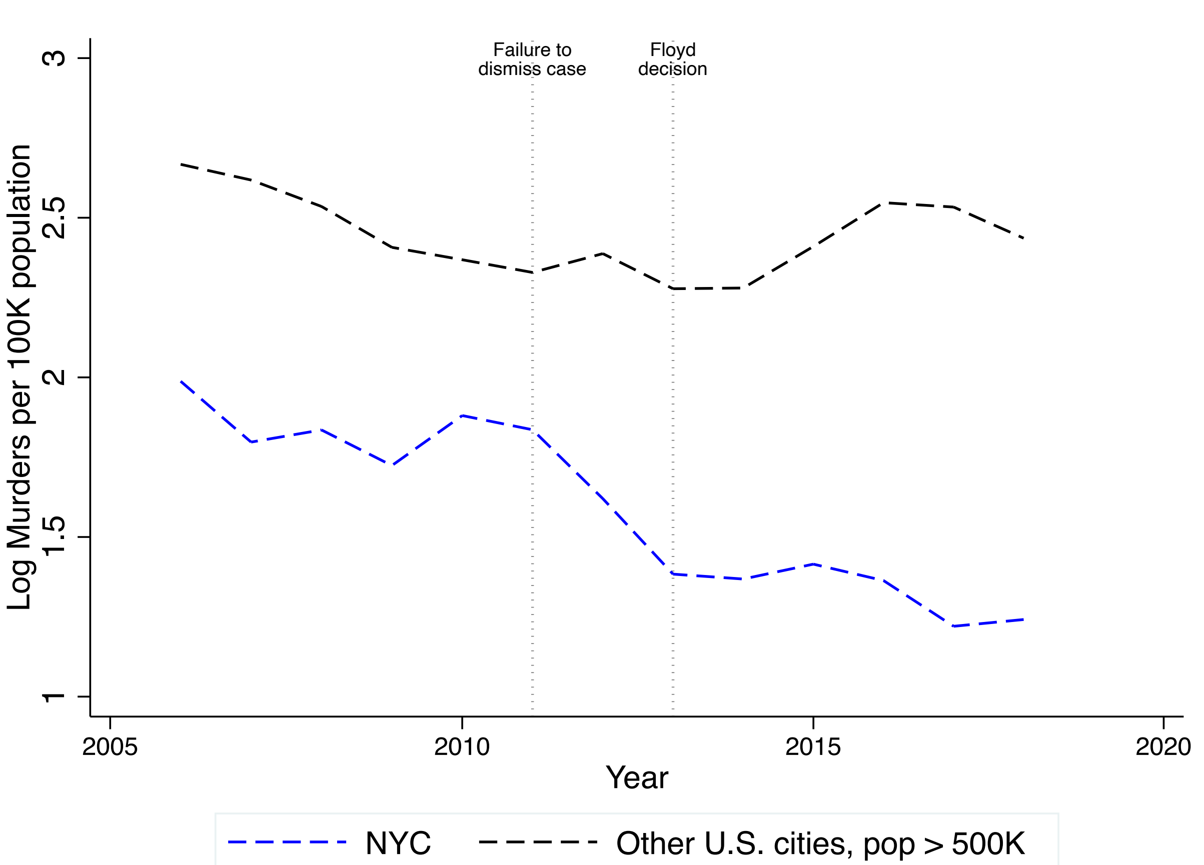
Table 6: Estimates: Enforcement Activity

	(1)	(2)	(3)	(4)
Total Arrests	-0.006	-0.013	0.006	-0.010
	(0.031)	(0.030)	(0.031)	(0.030)
	[1%]	[-1%]	[1%]	[-1%]
Drug Sale Arrests	-0.024	-0.053	-0.019	-0.047
	(0.084)	(0.087)	(0.079)	(0.086)
	[-2%]	[-5%]	[-2%]	[-5%]
Drug Possession Arrests	-0.105*	-0.094	-0.110*	-0.090
	(0.056)	(0.060)	(0.061)	(0.060)
	[-10%]	[-9%]	[-10%]	[-9%]
Misdemeanor Marijuana Arrests	-0.119*	-0.106*	-0.114*	-0.099
	(0.063)	(0.063)	(0.067)	(0.063)
	[-11%]	[-10%]	[-11%]	[-9%]
Terry stops	-0.048	0.038	-0.060	-0.081
	(0.092)	(0.123)	(0.084)	(0.083)
	[-5%]	[4%]	[-6%]	[-8%]
NYCHA effects	X	X	X	X
Week-year effects	X	X	X	
NYCHA linear trend		X		
Borough-week-year effects			X	
Cubic time trend				X

Note: Estimates are from Poisson regressions of the weekly count of enforcement activities on an indicator variable for a gang takedown as well as an indicator for the post-gang takedown period. Models are run using a bandwidth of up to two years around each takedown. All models condition on NYCHA development fixed effects but each model accounts differently for time trends. Model 1 conditions on week-year fixed effects and Model 2 adds a NYCHA development-specific linear time trend. Model 3 conditions replaces the week-year fixed effects with interacted borough-week-year fixed effects. Model 4 instead conditions on a cubic time trend. In all models, standard errors are clustered at the NYCHA development level to account for arbitrary serial correlation in the regressors. In each cell, the first row is the raw coefficient. Below this we present the clustered standard error (in parentheses) and the percentage change obtained by subtracting 1 from the estimated IRR and multiplying by 100 (in square brackets). Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

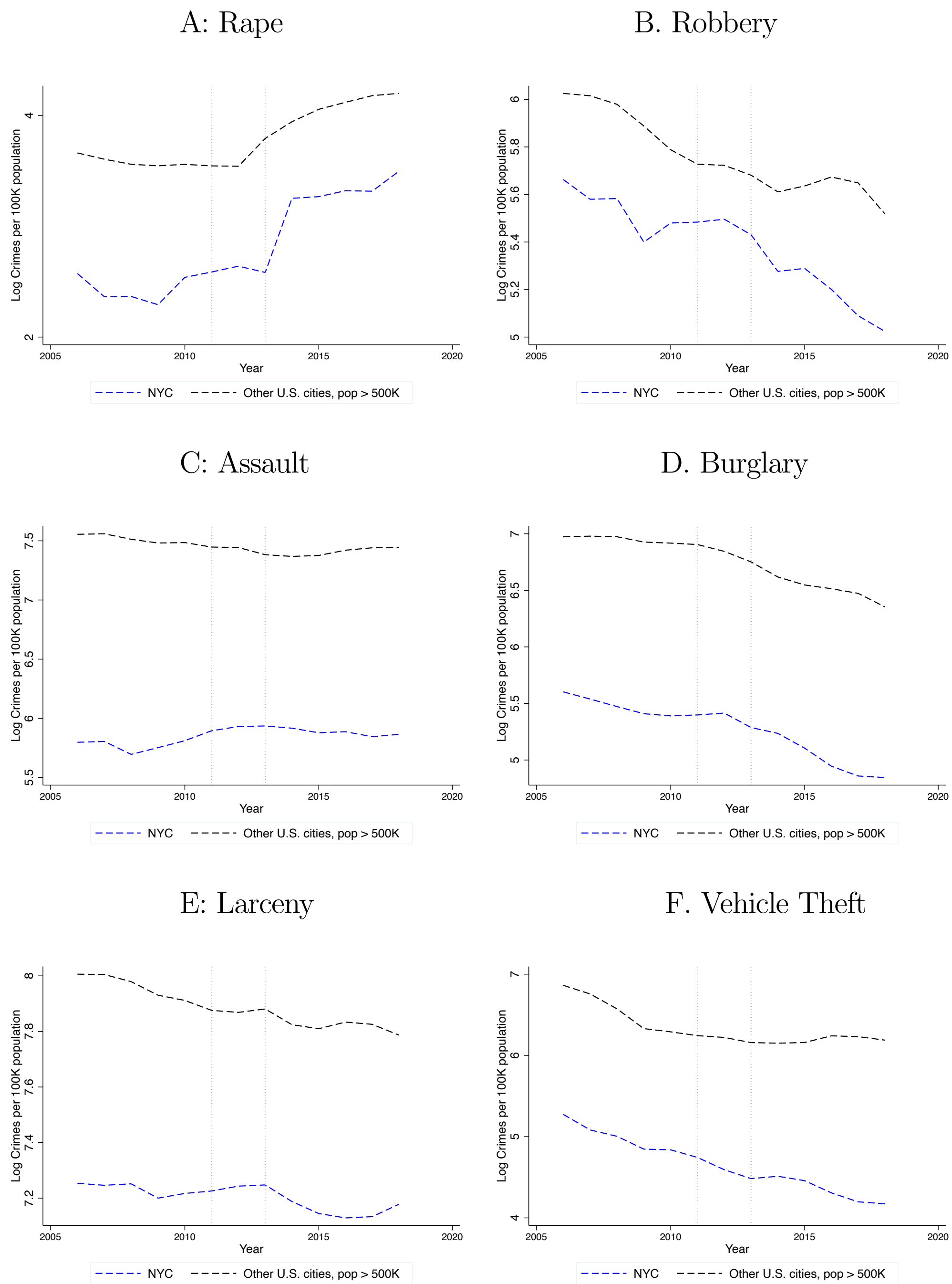
Online Appendix Material

Appendix Figure 1: Log Murders per 100,000 Population, New York City vs. Other U.S. Cities (2006-2018)



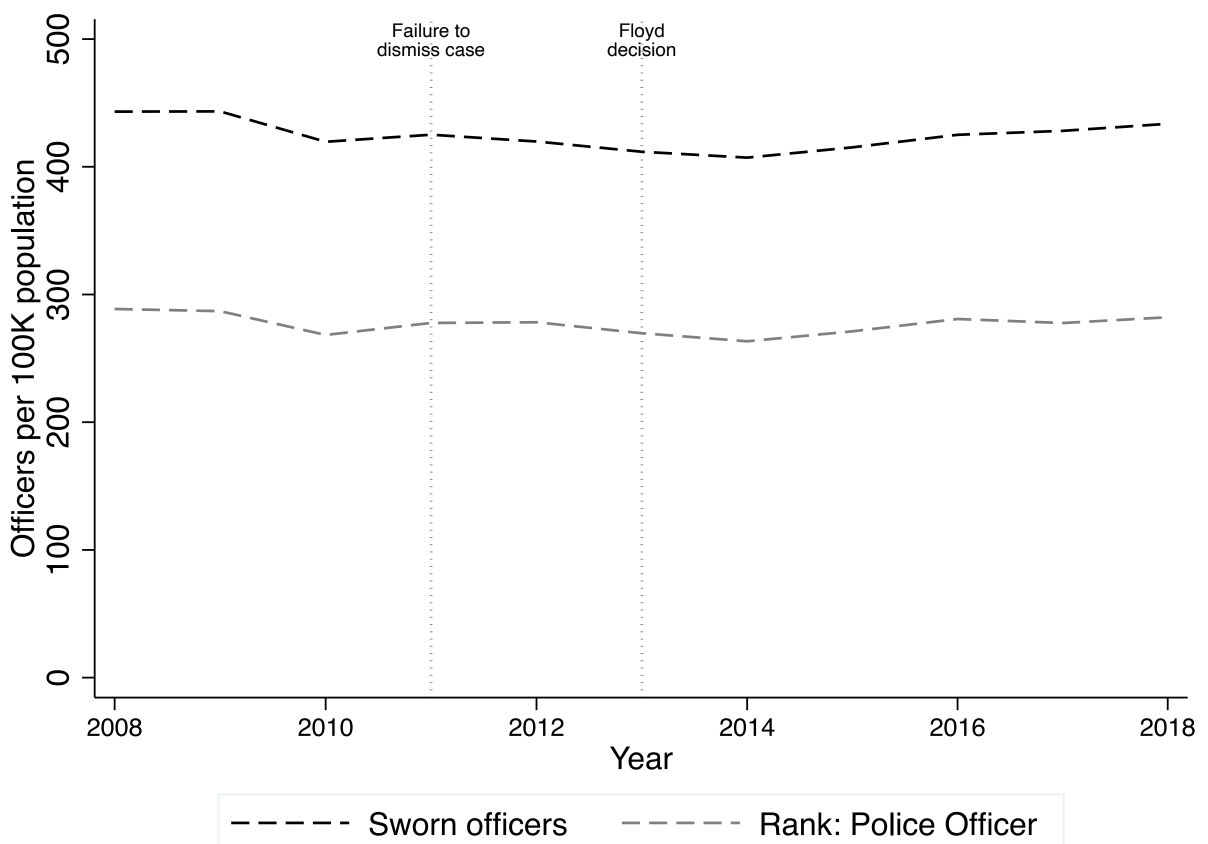
Note: Figure plots log murders per 100,000 population in New York City and in other U.S. cities with populations exceeding 500,000 in each year from 2006 until 2018. Data were accessed from Jacob Kaplan's "Crime Data Tool" at the following URL: `https://jacobdkaplan.com/`. In the figure, there are vertical lines corresponding to 2011, the year that a federal court failed to dismiss the *Floyd* case in a preliminary hearing and 2013, the year the *Floyd* case was decided.

Appendix Figure 2: Log Crimes per 100,000 Population, New York City vs. Other U.S. Cities (2006-2018)



Note: Figure plots log crimes per 100,000 population in New York City and in other U.S. cities with populations exceeding 500,000 in each year from 2006 until 2018. Data were accessed from Jacob Kaplan's "Crime Data Tool" at the following URL: `https://jacobdkaplan.com/`. In the figure, there are vertical lines corresponding to 2011, the year that a federal court failed to dismiss the *Floyd* case in a preliminary hearing and 2013, the year the *Floyd* case was decided.

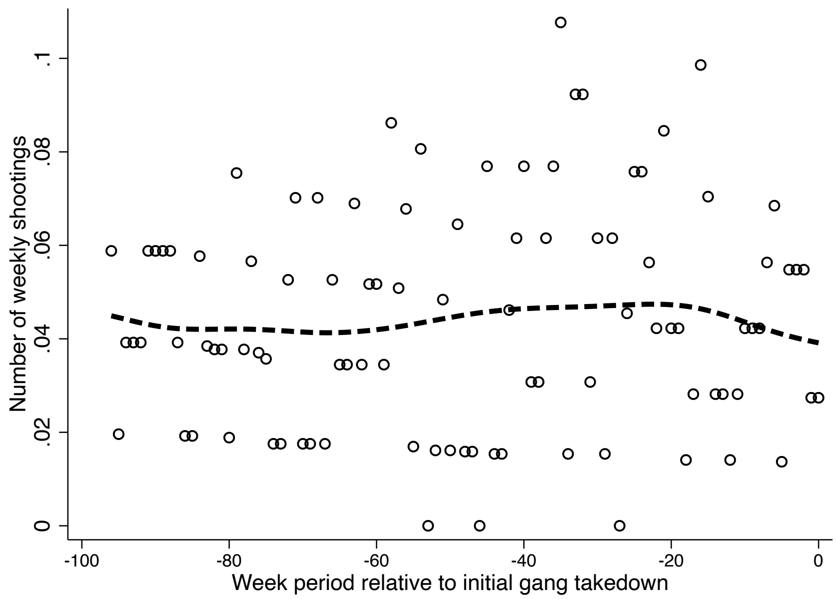
Appendix Figure 3: Trends in Sworn Police Officers per 100,000 Population, New York City (2008-2018)



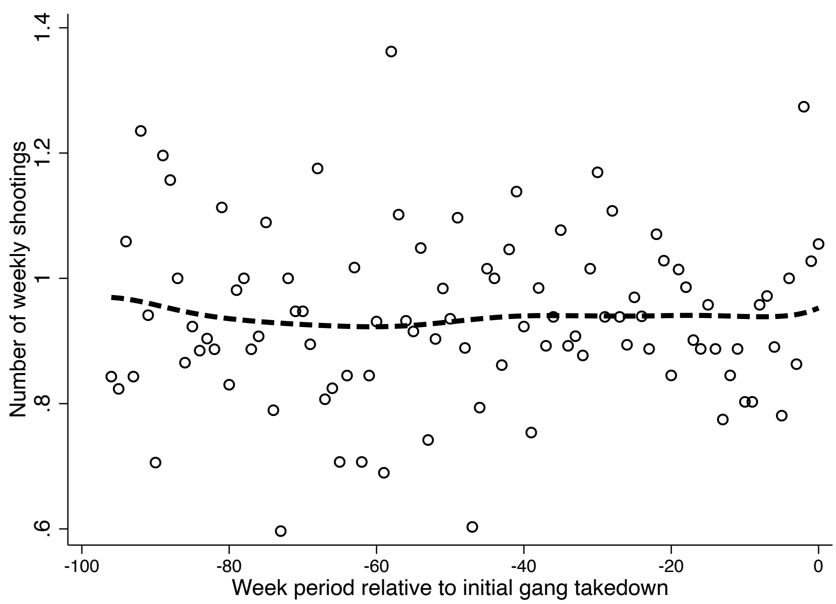
Note: Figure plots the number sworn police officers per 100,000 population in New York City and in the United States from 1960 until 2018. Data were obtained from the New York Police Department's Annual Crime and Enforcement Reports, accessed using the following URL: `https://www1.nyc.gov/site/nypd/stats/reports-analysis/crime-enf.page`. In the figure, there are vertical lines corresponding to 2011, the year that a federal court failed to dismiss the *Floyd* case in a preliminary hearing and 2013, the year the *Floyd* case was decided.

Appendix Figure 4: Analysis of Pre-Treatment Trends (Weekly Shootings)

Panel A: Shootings



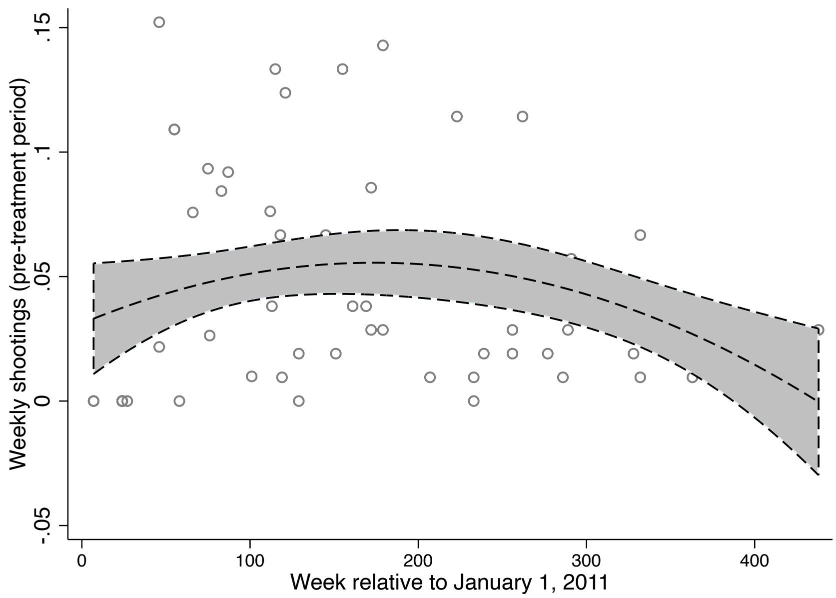
Panel B: Violent Crimes



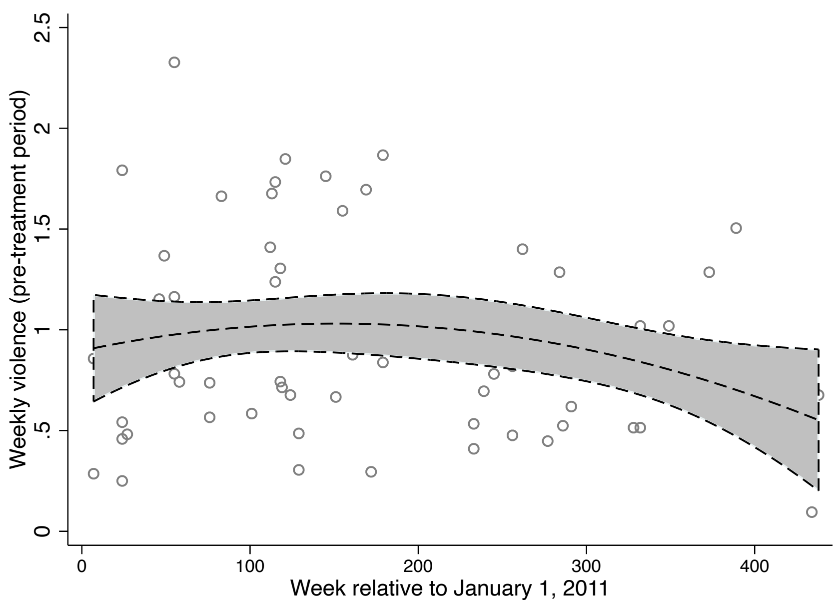
Note: Figure plots the number of weekly shootings (Panel A) and violent crimes (Panel B) by week relative to a development's first gang takedown. A Lowess smoother is drawn through the data.

Appendix Figure 5: Exogeneity of the Timing of the Initial Gang Takedown

Panel A: Shootings

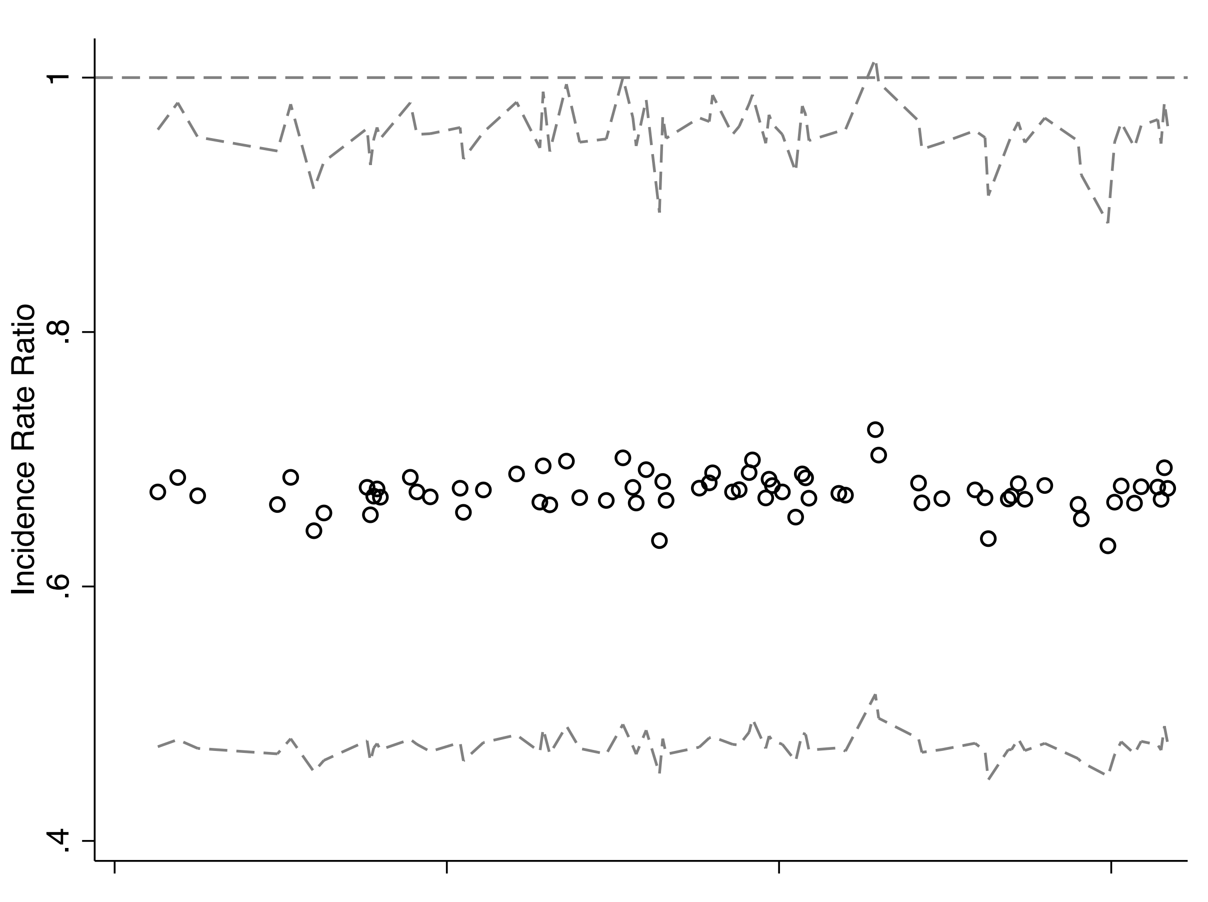


Panel B: Violent Crimes



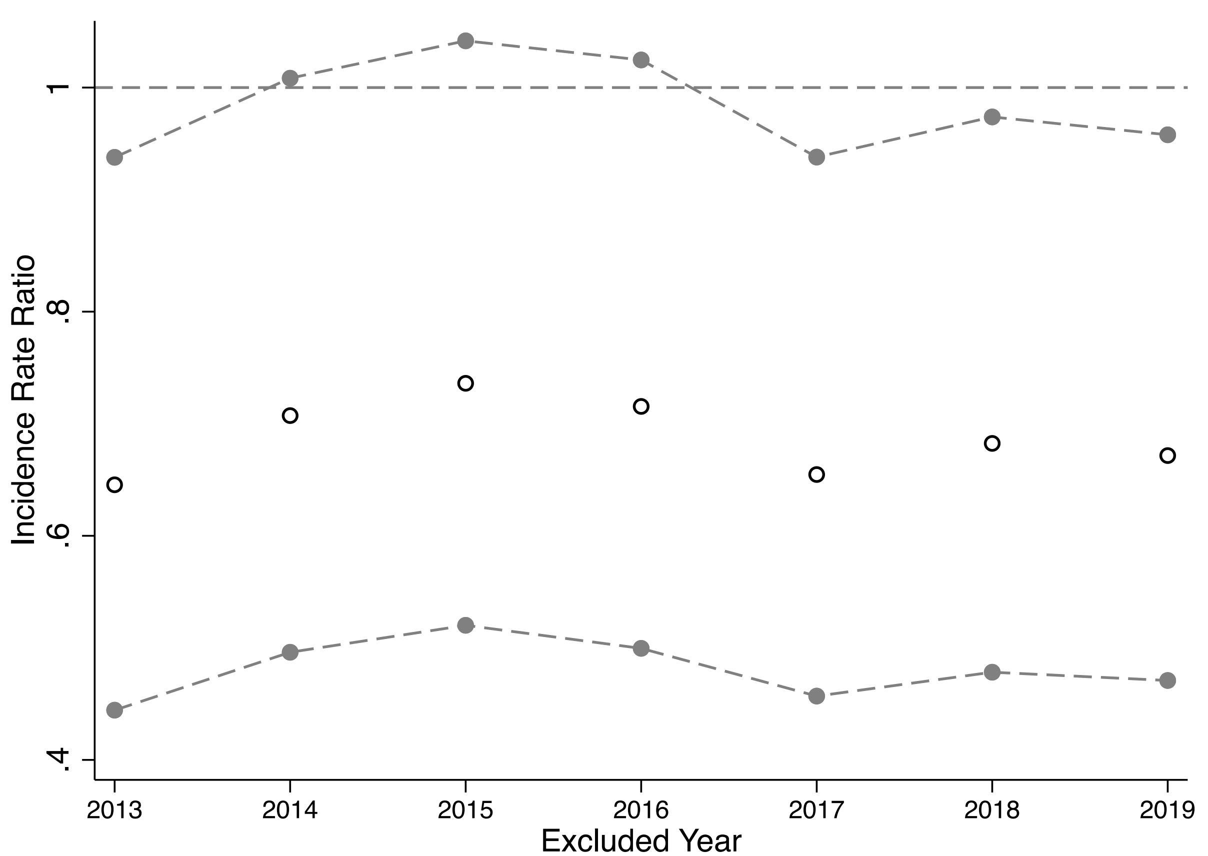
Note: Figure plots the number of weekly shootings (Panel A) and weekly violence (Panel B) against the timing of a housing development's initial gang takedown. A quadratic best fit curve is drawn through the data. Data are plotted only for each development's pre-treatment period.

Appendix Figure 6: Sensitivity of Estimated Treatment Effects to the Removal of a Given Housing Development (Shootings)



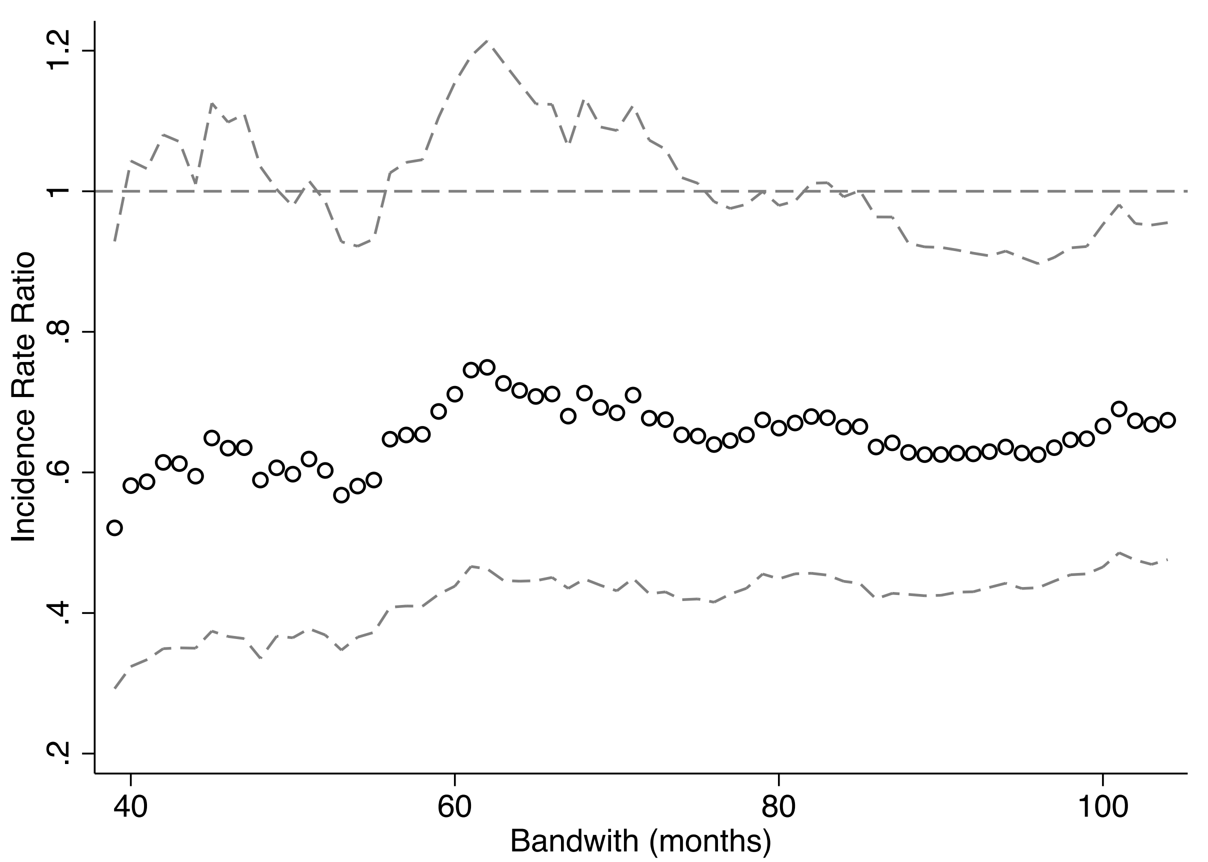
Note: Figure plots regression estimates (y-axis) for a series of Poisson regressions in which we regress the count of weekly shootings on the treatment indicator and NYCHA development and week-year fixed effects. In each regression, a different housing development is omitted from the data. Coefficients are plotted using the black solid line; 95 percent confidence intervals are plotted using the dashed gray lines.

Appendix Figure 7: Sensitivity of Estimated Treatment Effects to the Treatment Period (Shootings)



Note: Figure plots regression estimates (y-axis) for a series of Poisson regressions in which we regress the count of weekly shootings on the treatment indicator and NYCHA development and week-year fixed effects. Each model is run excluding gang takedowns that occur in a given year. Coefficients are plotted using the black solid line; 95 percent confidence intervals are plotted using the dashed gray lines.

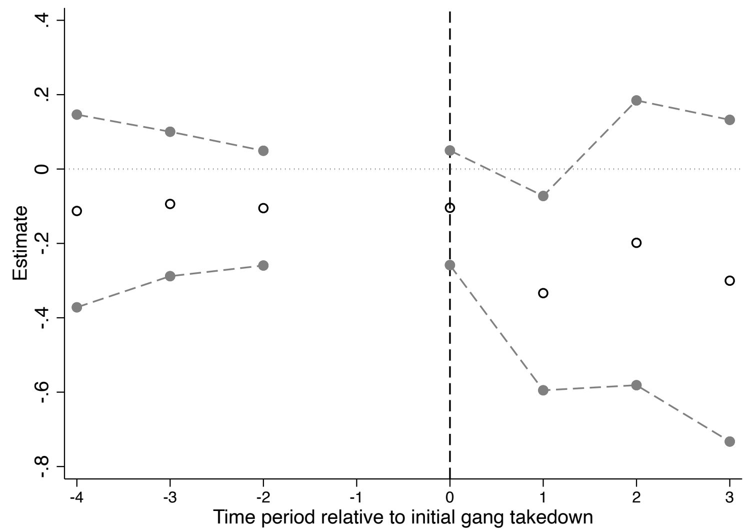
Appendix Figure 8: Bandwidth Sensitivity (Shootings)



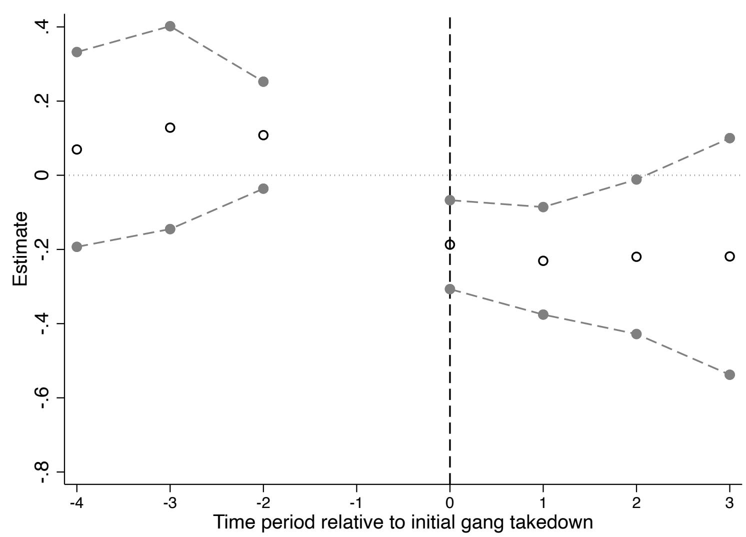
Note: Figure plots incident rate ratios for a series of Poisson regressions in which we regress the count of weekly shootings on the treatment indicator and NYCHA development and week-year fixed effects. In each regression, a different bandwidth is employed. Coefficients are plotted using the black solid line; 95 percent confidence intervals are plotted using the dashed gray lines.

Appendix Figure 9: Event Study Analysis (Discretionary Police Enforcement)

Panel A: Marijuana Possession Arrests

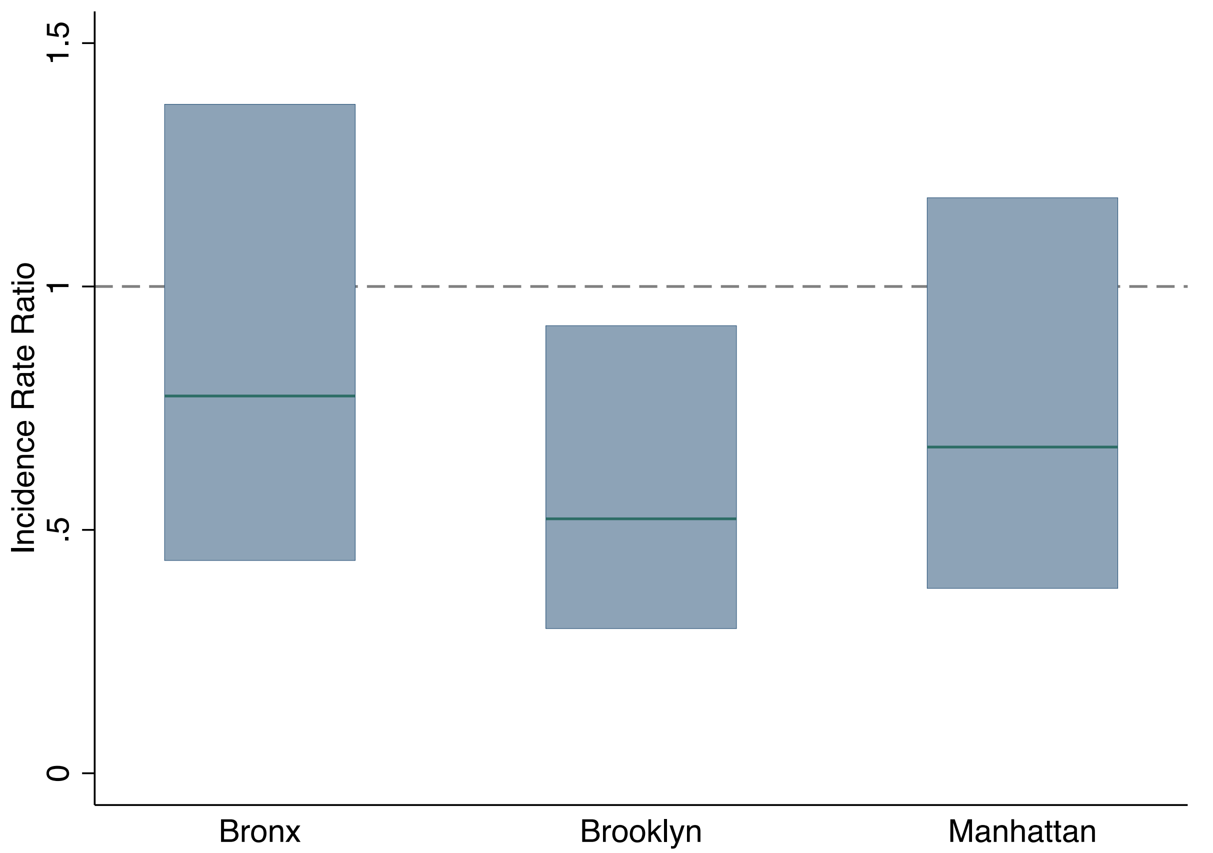


Panel B: Terry Stops



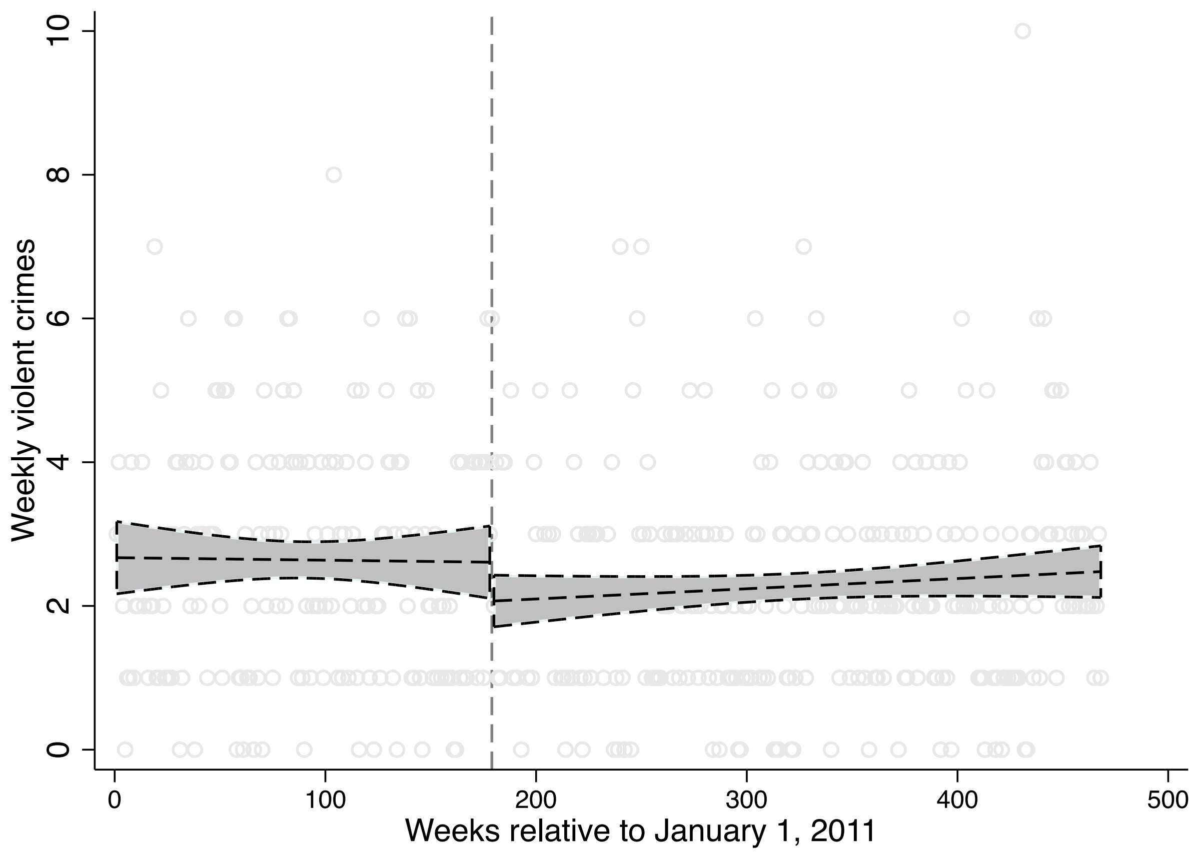
Note: Figures plot coefficients from the event study regression described in Equation (3). Each estimate represents a six-month bin, relative to a NYCHA development's first gang takedown. For each six-month bin, the point estimate is the coefficient from a Poisson regression of the number of weekly marijuana possession arrests (Panel A) or weekly Terry stops (Panel B) on the event study terms, conditional on NYCHA development and week fixed effects. Standard errors are clustered at the NYCHA development to account for heteroskedasticity as well as arbitrary serial correlation.

Appendix Figure 10: Estimates by Borough (Shootings)



Note: Figure plots incident rate ratios and 95 percent confidence intervals from Poisson regressions in which we regress the count of weekly shootings on the treatment indicator and NYCHA development and week-year fixed effects. The model is estimated separately for Manhattan, Brooklyn and the Bronx, the three boroughs with a sufficient number of gang takedowns.

Appendix Figure 11: Effect of the "Largest Gang Takedown in NYC History," June 2014



Note: Figure plots the weekly number of violent crimes in Grant and Manhattanville Houses, two public housing developments in Manhattan which, on June 5th, 2014, experienced the largest gang takedown in NYC history — see `https://www.dnainfo.com/new-york/20140605/west-harlem/harlem-gang-takedown-is-largest-city-history-officials-say/`. The raw data are plotted using the gray open circles. A best fit line and the associated 95 percent confidence interval are drawn through the data separately for the pre- and post-takedown periods. The horizontal reference line at week 179 represents the time of the gang takedown.