The Effects of Local Police Surges on Crime and Arrests in New York City

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Since 2005, New York City’s crime rate has steadily declined below historically low levels not seen since the early 1960s. Operation Impact, implemented in 2003, has been cited as one of the leading causal factors for the city’s low crime rate. Operation Impact involved a large deployment of police officers to designated high crime areas who were instructed to implement stop and frisk and other proactive policing tactics that were central features of the city’s crime control strategy. The claims of success from Operation Impact have not been subject to rigorous examination. We rely on difference-in-difference regressions and stepped wedge designs to estimate the effect of Operation Impact on reported crimes and arrests. We find that Operation Impact reduced burglaries but increased overall amounts of reported crime. Most of the crime increase is a result of weapons, drugs, and other offenses whose counts are generated by police arrest activity. The increase in monthly stops by police as a result of Operation Impact is associated with a small reduction in crime. However, the number of stops per averted crime suggests that most stops occur with little crime reduction benefit. Across several tests, results indicate that Operation Impact had mixed success as a crime reduction strategy. The police tactics of Operation Impact also raise fundamental questions about police saturation of neighborhoods and aggressive stop practices that give rise to equal protection and due process concerns.

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1. Introduction

There has been a contentious debate on the contributions of the police to the crime reductions of the past two decades. Some scholars have argued that the decline in crime across cities with different policing regimes suggests that there may be broader social factors associated with declining crime, such as changes in demographics, access to guns, and drug markets. Others have suggested that different policing strategies have contributed to declining crime in cities in unique ways (Frydl & Skogan, 2004). Included among these policing strategies is a “new policing” model that can be characterized by three essential features (Heymann, 2000). First, police managers developed real-time policing metrics and other analytics to determine optimal police deployment (Braga & Weisburd, 2010). Second, local police commanders are held accountable for crime trends in their respective areas, creating a sense of urgency to respond quickly and intensively to emerging patterns (Bratton & Knobler, 1998). Failure to lower crime rates could lead to public shaming in meetings of police executives and possibly demotion and re-assignment. Third, proactive and aggressive police tactics are encouraged as a means of disrupting criminal activities. The use of street stops or field interrogations is seen as a critical tool to disrupt criminal activity. Increasing arrests for minor misdemeanors is also encouraged (Livingston, 1997; Maple & Mitchell, 2010; Taylor, 2001). These elements have been widely adopted across the U.S. in a number of cities. The debate on the contributions of “new policing” to crime reductions has been fierce.

The debate has been fiercest in New York City (NYC). Several of the features of the “new policing” model were first implemented in NYC in the early 1990s as a package of institutional reforms. The city had the steepest crime decline in the nation during this
decade (Fagan, Zimring and Kim, 1998; Joanes, 1999; Karmen, 2006; Zimring, 2006). The decline in crime continued at a slower rate through 2010, and recent data shows that crime continues to be at record lows in NYC (Zimring, 2011; NYPD, 2015).

Several policing factors have been cited to explain the two-decade long crime decline, both in NYC and elsewhere: the expansion of the size of police forces (Levitt, 2004; Evans & Owens, 2007; Chalfin & McCrary, 2014), stronger accountability measures and internal oversight of police command staff (Bratton & Knobler, 1998), more aggressive enforcement of laws against misdemeanor crimes (Corman & Mocan, 2005; Rosenfeld et al., 2007; Kelling & Sousa, 2001) and drug offenses (Messner et al., 2007), the adoption of crime analytics and its use in the targeting of police deployment (Weisburd et al., 2002; Zimring, 2006, 2011), and increased use of stop, question, and frisk tactics (Smith & Purtell, 2007).

Alternate explanations for the crime decline in NYC include: the improvement in the housing market and gentrification (Schwartz et al., 2003), changes in drug markets and distribution methods (Zimring, 2011), increased immigrant concentration in poor and high-crime neighborhoods (Davies & Fagan, 2012), and a shift in the crime-prone age population (Karmen, 2006).

Some have argued that the claim of a greater city-wide crime decline in NYC compared to other large cities is a computational artifact. Once one adjusts for the timing of the crime decline, the overall level of crime, and the per capita size of the police in NYC, the city’s comparative advantage either disappears or is sharply diminished (Joanes, 1999; Greene, 1999; Karmen, 2006; Rosenfeld et al., 2005; Harcourt & Ludwig, 2006; Harcourt, 1998).
Sorting out these competing claims requires careful attention to research designs that can examine variation in police strength and tactics. The empirical research on the crime drop during the past two decades in NYC has relied primarily on panel studies assessing changes in police tactics and crime over large administrative units, such as boroughs (Zimring, 2011) or precincts (Rosenfeld & Fornango, 2007; Greenberg, 2014). Precincts are internally heterogeneous in demographics, population density, housing configurations, and crime. Estimates based on these administrative units are average effects that mask important heterogeneity in crime and police deployment. In NYC many police tactics are often geographically focused on smaller spatial units, such as street blocks or public housing units.

More generally, the most rigorously designed studies examining the effects of police deployment on crime focus on short-term exogenous changes in police strength due to terror events, terror warnings, “crackdowns,” or field experiments. We have little knowledge about the long-term effects of extra police deployed to high crime areas. In the context of NYC it has been argued that a sustained deployment model of extra police to high crime city blocks has been responsible for lasting effects on crime (Zimring, 2011).

In this paper, we examine the effect of Operation Impact, a signature program of the NYPD for over a decade and a prototypical application of the “new policing” model of police deployment based on crime metrics. Operation Impact was developed in a small number of test sites in 2003 and implemented citywide beginning in 2004. Under Operation Impact, specific “impact zones” were identified based on analyses of crime patterns within the boundaries of public housing sites or city blocks. The boundaries for
impact zones were assessed and revised in approximately two cycles each year, in consultation with crime analysts and captains in police precincts in the months before each academy graduation cycle (Golden & Almo, 2004; Smith & Purcell, 2007). By 2006, Operation Impact’s sixth cycle defined 21 areas as impact zones and deployed a total of 1,372 uniformed officers. In addition to the tactical features of Operation Impact and its focus on proactive policing, its deployment approach reflects the contemporary police practice of “hot spots” policing (Skogan & Frydl, 2004; Braga & Weisburd, 2010). A major strength of this paper is that we have detailed administrative data on the exact locations of these areas and the timing of when they became impact zones. We can use these to identify the magnitude of the effect of police deployment on crime. Our application is also more general than other studies that focus on short-term changes in police deployment on crime, as we are able to assess the effects for all of NYC over a ten-year time period.

Until now the crime control effects of Operation Impact have not been analyzed with a rigorous research design. The only published report on Operation Impact used overall precinct crime rates to assess the program’s effects (Smith & Purcell, 2007). That study did not account for either the deployments of officers to specific impact zone blocks or the localized crime control effects in those areas. Again, the estimates in that study were average effects within the precincts. If impact zones, for example, simply displace crime to nearby blocks, an analysis of precincts would fail to detect such displacement.

Also, given the nature of how impact zones are selected, there is the obvious concern of regression to the mean. After all, if crime analysts and captains pick the
blocks within precincts with the highest levels of crime, then it is possible that the overall reduction in crime in these blocks may be due to the act of selecting areas just before crime would have dropped naturally. Finally, given the impact zones are likely distinct from other parts of the city in several ways (e.g., implemented in areas of elevated crime and concentrated poverty), it makes sense to compare what happens to crime in all areas that eventually become impact zone areas and to use designs that can estimate the effects of the policy in those distinct areas.

There is much to learn about police effects on crime from an analysis of the effects of Operation Impact. This program was implemented as a police deployment model to suppress crime in designated crime “hot spots.” The dosage of deployment was also large, providing an ideal test for whether police saturation can lead to lasting crime reductions in high-crime areas. The policing model itself included the twin elements of “new policing:” arrests for misdemeanor crime combined with a high allocation of officers engaged in field interrogations or stops, questions, and frisks based on Fourth Amendment standards of “reasonable suspicion” (Meares, 2015; Bellin, 2014). The majority of the officers assigned to impact zones were fresh academy graduates, creating the added research advantage of debiasing their actions from prior experiences with the areas they were assigned to.

In this paper, then, we estimate the effects of Operation Impact on crime, and in turn, assess an essential prong of “new policing” on crime. We resolve issues of measurement of crime and policing as well as selection of specific places for intensive policing, by focusing on data aggregated to the census block level within designated impact zones. We measure crimes, arrests, and police stops that occur within the
boundaries of impact zone blocks compared to adjacent blocks and also to other sections of NYC. We also rely on the timing of impact zones as a strategy to estimate their impact on outcomes. Specifically, we provide careful comparisons of outcomes by examining pre-post trends in crime and arrests in impact zones compared to other locations in the same precinct. We further refine our analysis and examine pre-post trends in crime and arrests only for those blocks that eventually become impact zones. Finally, we provide several tests of robustness that include event controls for the timing of impact zones and their selection.

This paper begins with a brief discussion of literature on the effect of police deployment on crime, and specifically the crime drop in New York, followed by a more detailed description of how the NYPD implemented Operation Impact. Subsequent sections discuss the data and methods of our analysis, the results, and our conclusions.

2. Background

The Effect of Police on Crime

A rich research literature shows that crime is responsive to several dimensions of policing, including police force size, deployment, specific tactics, and crime. Three threads in this line of research inform the current study.

First, several studies suggest that more police matter, independent of other factors. In New York, the flooding of high crime impact zones with rookie police officers produced a local surge in the presence of police and the likelihood that they would encounter crime or criminal suspects. Surges of this sort are consistent with several studies on the total size of the police force. Levitt (2002) uses the hiring of firemen as an
instrument for police hiring, and finds that additional police officers significantly decrease rates of both violent and property crimes. Other studies using federal grant funding as an instrument for police hiring have corroborated these results (Zhao et al., 2002; Evans & Owens, 2007). Evans and Owens (2007), for example, provide the strongest evidence of the effects of increased presence of police. They examined the relationship between crime and police hiring in cities with over 10,000 residents before and after receiving federal grants to hire additional police officers. They report that hiring police officers is associated with reductions in crime of 2% to 5% in both their reduced-form and instrumental variable specifications. The effect size of these estimates may seem small, but one should keep in mind that they represent average effects on crime for entire cities, not just the effect of the extra police in the specific neighborhoods in which they are deployed. In cities with thousands of felonies, these effect sizes are both statistically and practically significant. Even without considering the hiring or crime reduction mechanisms, Chalfin and McCrary (2014) report that more police per capita at the city-level reduces crime, independent of the tactics that police use.

Second, studies also suggest that police deployment and tactics affect crime. Quasi-experimental studies provide strong evidence that targeted police deployment reduces crime. Cohen and Ludwig (2002) test the effect of police deployment by exploiting variation in patrol intensity by day of week (Wednesday through Saturday) in two areas of Pittsburgh that received a 25% to 50% increase in police patrol over short

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1 The well-cited exception to this general observation is the Kansas Police Experiment (Kelling et al., 1974), which randomly assigned three levels of police patrol to 15 beats (five in each group were assigned either no patrol; usual patrol; or two to three times the usual patrol). Kelling et al. (1974) found that additional police on routine patrol had no observable effect on crime. But as the authors note, the study examined the effects of routine patrol and not police manpower or visibility during those patrols (see Sherman & Weisburd, 1995).
periods of time. Relying on the timing of the patrol intervention and dosage differences across days of the week, Cohen and Ludwig use a difference-in-difference-in-differences design to estimate that days with intensive patrol experienced 34% and 71% decreases in average daily gun shots fired and assault-related gun injuries.

Several studies have also used changes in police deployment due to concerns about terrorism to estimate the effects of police density on crime. Klick and Tabarrok (2005), for example, use daily changes in the deployment of police officers in Washington, DC due to changes in the terror alert level and find that increased police presence is associated with a 6.6% drop in total crime. Even stronger effects were observed in the Capitol Hill District where the highest numbers of police officers are deployed during terror alerts.

Di Tella and Schargrodsky (2004) used a similar quasi-experimental design but focused on street blocks rather than days of the week. Following a terrorist attack on a Jewish center in Buenos Aires in 1994, the police deployed additional patrols to areas of the city with Jewish institutions. Assuming that the resulting geographic allocation of patrols was exogenous to other types of crime, Di Tella and Schargrodsky compared the rate of car thefts before and after the attack in areas with and without Jewish institutions. After the attack, car thefts dropped by 75% on street blocks with a Jewish institution relative to street blocks without a Jewish institution. They found no effect on car thefts just one or two blocks away from Jewish institutions, suggesting that the deterrent effect of police deployment was highly localized.

Draca et al. (2011) more recently examined the effects of police deployments on crime by comparing changes in crime before and after the July 7, 2005 terrorist bombing
in London. After the bombing, police deployed a substantial number of supplementary police officers to central London. The rest of London did not receive an increase in officers. Draca et al. were also able to examine changes in crime rates after the extra deployment to central London was lifted, in effect, testing for the presence of residual deterrence or decay in deterrence over time. Their difference-in-differences design shows that a 10% increase in police deployment reduced crime in central London by 3 to 4% but that crime rates returned to pre-intervention levels after the deployment was lifted. Berk and MacDonald (2010) use a similar quasi-experimental design to test the effects of a police crackdown in downtown Los Angeles by comparing crime rates before and after the additional deployment, relative to adjacent police divisions. They observe a 30% to 39% relative reduction in crime in areas associated with the extra police officers.

In addition to the quasi-experimental literature on police deployment, there is also a body of field experiments in criminology that tests the effect of deployment to high crime “hot spots.” Sherman and Weisburd (1995), for example, identified the 110 highest crime blocks in Minneapolis, MN and randomly assigned 55 of these blocks to receive a “crackdown-back off” deployment pattern, in which police cars spent as much as one extra hour per shift in each of the treatment areas. The study found that treatment areas experienced a 6% to 13% relative reduction in crime compared to control areas, with the largest effects observed for disorder crimes. Other field experiments also have found that targeted police patrols in high crime areas reduce crime and disorder (Weisburd & Green, 1995; Braga et al., 1999; Braga & Bond, 2008; Ratcliffe et al., 2011). However, it is important to emphasize that these studies examine a special case of the effect of police deployment on crime in localized areas, as they focus on changes in
deployment for only short durations of time. They do not rule out the possibility that police deployment displaces crime to adjacent areas. There is little empirical evidence that supports the displacement hypothesis (Weisburd et al., 2006), but it remains a potential threat to causal inference for relatively small-scale field experiments.

Research suggests that tactics police employ matter. Police scholars have long recognized that the number of police officers employed in a jurisdiction—whether a count or a per capita rate—may be less important than the specific crime prevention activities they conduct (Sherman, 1995; Wilson & Boland, 1978; Sampson & Cohen, 1988; MacDonald, 2002; Weisburd & Braga, 2010, Kubrin et al., 2010). In general, research on the effect of police activities in subsections of a city provides more persuasive evidence of the deterrent effects of police on crime, suggesting an interaction between tactics and spatially targeted deployment. This makes intuitive sense, as research has demonstrated in a number of cities that fewer than 10% of city street blocks generate more than half of reported crimes and that most crimes occur during specific days of the week (Sherman, Gartin, & Buerger, 1998; Weisburd et al., 2006).

Tactics, however, vary extensively across policing regimes but now fall generally under the broad label of “proactive policing.” Wilson and Boland (1978) emphasized misdemeanor arrests as part of “proactive” or aggressive policing, including intensive traffic enforcement and arrests for drunk driving and disorderly conduct. Cohen and Ludwig (2003) showed reductions in gun violence in areas where police in Pittsburgh increased patrols by 25 to 50% during high crime hours, employing suspicion-based police stops to search suspects for weapons. Research has also shown that cities where police make more arrests for drunk driving and disorderly conduct have lower robbery
rates (Sampson & Cohen, 1988; MacDonald, 2002; Kubrin et al., 2010). Operation Impact in NYC applied a similar strategy, although the suspicion-based stops were more broadly targeted at signals of both serious and misdemeanor crimes. The use of widespread suspicion-based pedestrian and vehicle stops has become an important feature of contemporary policing, despite its constitutional problems (Meares, 2015). A recent field experiment in Philadelphia compared routine patrol in high crime areas with problem-solving approaches and offender-focused policing aimed at high risk persons, finding that the offender-focused approach was the most effective in reducing violent crime (Groff et al., 2015).

Accordingly, while there is no consensus on which tactics are essential to “proactive policing,” there is evidence that deployment and targeting strategies matter. The tactics vary but tend to emphasize the anticipation of criminal activity, directing action to those places or persons, and a commitment to systematic criminal enforcement of minor crimes (see Kelling & Coles, 1996 for illustrations). Tactics such as investigative stops (stop and frisk or Terry stops), aggressive enforcement to quality of life violations and misdemeanor crimes, and formal arrest for high-discretion crimes (e.g., public drinking and disorderly conduct) are all are common features of “proactive policing” (Livingston, 1997). All of these features were incorporated in the Operation Impact program in NYC.

Still, there are important gaps in this literature. Although two decades of empirical research provide evidence that police deployment produces crime reduction benefits, there is conflicting evidence about the effects of specific tactics. The current empirical literature shows that police activities – once deployed – are largely
idiosyncratic, complicating comparative analyses (see Ratcliffe et al., 2011 and Groff et al., 2015 for an exception). Studies tend to be short-term: the research focuses on rare spikes in police deployment due to terror events or police crackdowns in response to acute crime epidemics and are confined to short periods of less than two years. These designs cannot address the effect of sustained increases in deployment or upticks in specific forms of police activity over a longer interval when crime rates vary as do the surrounding environmental contexts. Research on police crackdowns, for example, suggests that their deterrent effect would dissipate over time if they became a permanent fixture of police department tactics (Sherman, 1990). Also, it is possible that crime reductions associated with short-term terrorism-related deployment may derive from increased officer vigilance or alertness, a condition that is artifactual to imminent threats and that would subside or stabilize over longer periods of time. It is unlikely this increased level of vigilance would be maintained for long periods of time, absent further risk of terrorist events, and even under those conditions, heightened alert may not be sustainable for individual officers. Thus, relatively little is known about the sustained effect of normal police deployments over long periods of time and even less about the interaction between deployment and tactics in high crime areas.

New York’s Crime Decline

New York City is perhaps the most celebrated case of the crime decline in the 1990s, and it is also the most closely examined. The fact of the crime decline has been substantiated by comparisons of official NYPD crime data with analyses of city-specific national crime victimization survey (NCVS) data for New York (Langan & Durose, 2004). The magnitude and pace of the decline was further substantiated by comparisons
of homicide statistics reported by police and reports from the New York City Medical Examiner (i.e., coroner’s reports) (Zimring, 2011). Sustained low levels of crime have become a reality of life in all the City’s boroughs into the 2000s. Yet, the causes of the crime decline and the role of policing specifically have been contentiously debated throughout this era.

Innovations in policing in New York, scholars have argued, were responsible for the crime drop in that city during the 1990s (Kelling & Bratton, 1998; Bratton & Knobler, 1998) and the sustained low levels of crime since 2000 (Dickey, 2009; Zimring, 2011). Perhaps the best-known thesis attributed much of the decline to the NYPD’s movement to its place-based policing tactics. Zimring (2011) carefully reviews all the main correlates of crime in NYC during the first decade of 2000 and concludes that only the changes in police tactics toward “hot spot” policing explains how NYC became one of America’s safest cities. Yet, similar to many other scholars, Zimring cannot and does not attribute the decline to any specific police tactic. Nor does he carefully distinguish between the infrastructural design of its crime analysis metrics and the institutional reforms in police management and accountability. For Zimring, policing is a vector of strategic (focusing on weapons), tactical (aggressive or proactive policing), and institutional innovations (the use of real-time crime metrics to deploy and manage police resources).

We begin our story in 2004, shortly after the citywide implementation of Operation Impact. Although crime declines in New York were steeper from 1994-2001 than in the next decade, crime rates continued to go down after 2000, as the general crime
The crime decline in serious felonies is evident from a review of NYPD crime statistics. Between 2004 and 2012, for example, murder and robbery declined by 3.3% annually for a total drop of 30%. Burglaries similarly dropped by 4.5% annually for a total drop of 41.2%. By contrast, felony assaults stayed relatively flat.

- Figure 1 about here-

Felony offenses related to weapons, drugs, and assault also experienced annual reductions on the order of 1.4% for a total drop of 13.2%; whereas misdemeanor charges for similar offenses increased annually by 1.4% for a total increase of 16.5%.

- Figure 2 about here-

Figure 2 shows that reported felony crimes decreased annually by 3.3% for a total drop of 30% between 2004 and 2012, whereas reported misdemeanor offenses increased by 8.9%. In short, there appear to be sustained drops in reported serious felonies with a rise in misdemeanor offenses.

What accounts for the sustained reductions in serious felonies? Earlier, we mentioned prior research that focused on misdemeanor arrests (Rosenfeld & Fornango, 2007; Greenberg, 2011), drug arrests (Corman & Mocan, 2005), and the general strategy of place-based tactics related to police stops (Weisburd et al., 2014). None of these offer an identification strategy beyond time-limited cross-sectional panel designs, nor do they sort out the endogeneity of crime, police stops, and arrests in the smaller “hot spots” where police deployments and tactics were concentrated. In this paper, we consider the

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2 To some degree, the post-2001 crime decline may be seen as a long-term secular trend following an epidemic of violence in the 1985-93 era. The effects of policing that Zimring (2011) and others have identified may in fact be marginal effects compared to the longer-term trend.
role of Operation Impact, a feature of policing in New York that integrated the dimensions of deployment and tactics in a unique program of street stops.

3. Operation Impact: Impact Zones

In the early 2000s, the NYPD began a major change in its deployment practices by implementing the concept of an impact zone – a high crime area designated to receive extra police fresh out of the police academy. In January 2003, the NYPD deployed roughly two-thirds of its police academy graduates—about 1,500 new police officers—to impact zones, as part of Operation Impact (Golden & Almo, 2004). In impact zones, academy graduates were encouraged to use Terry stop rules to temporarily detain individuals in these areas and perhaps frisk and search them. Officers were also encouraged to aggressively enforce misdemeanor laws (Golden & Almo, 2004).

To identify areas as impact zones, local precinct commanders and borough commanders nominated “hot spots” within their precincts that they thought would benefit from additional targeted resources. Using street-level crime data presented on maps, NYPD crime analysts produced reports on each area and recommended ways of refining the targeted areas. After discussions among local commanders and headquarters analysts, the Police Commissioner initially selected 24 neighborhoods with the highest rates of crime, including shootings, in which to focus extra police officers to reduce crime (Golden & Almo, 2004).

Since 2003, impact zones have evolved. By 2006, impact zones were present in 30 precincts. Some impact zones are time-limited, some appear to be semi-permanent, and some vary by time of day and day of week. Seventy-five of the 76 precincts in NYC
had an impact zone on at least a few blocks between 2004 and 2012. However, the share of impact zones was devoted largely to high crime precincts where a majority of residents are Black and Latino. The precincts with the largest concentration of impact zones, for example, include East Harlem (23rd), Harlem (32nd), South and West Bronx (40th, 44th, 46th, and 52nd), and Brooklyn (70th, 75th, and 79th). Figure 3 shows a map of the location of impact zones and their rollout over time.

-Figure 3 about here-

Saturation of officers in impact zones produces higher stop rates per block (and per 100 crimes) than in other places in the NYC. Figure 4 shows the trend in stop rates by impact zones and the rest of the city; it is clear that stops rose proportionately more in impact zones than other parts of NYC. This figure underscores that impact zones both deployed more officers and generated more stops.

-Figure 4 about here-

In this study, we examine the crime reduction effects of one feature of the NYPD’s place-based policing strategy of Operation Impact, and we focus our analysis on the central feature of the police tactics in the designated impact zones: stops, questions, and frisks (SQF). Pedestrian stops, or SQF activities, were the core tactic of officers deployed in Impact Zones (Smith & Purtell, 2007; Fagan, 2012b; NYPD, 2004). Beginning in 1994, the NYPD engaged in a deliberate program of stopping and frisking individuals throughout the city, focusing on certain areas where crime, minority populations, and poverty were concentrated (Fagan & Davies, 2000; Gelman et al., 2007). Although the purpose of these stops was to detect the presence of guns or other
weapons, and in turn reduce violent crime, others saw the tactic as a method to deter crime in general and violent crime in particular (Spitzer, 1999; Meares, 2015; Fagan & Geller, 2015).

Between 2003 and 2012, the police in NYC conducted over 4.5 million stop, question, and frisks. Because such stops were so central to the policing strategy in NYC, and because the ramping up of stops starting in the 1990s was so temporally close to the crime decline, the relationship between the NYPD's program and the city's astonishing decline in crime was a focal point in the national debate about the strategy. Not only was this tactic folded into the fierce debate about the role of policing in producing the city’s crime decline, it was also the focus of at least four federal civil rights lawsuits alleging both 4th Amendment search violation and 14th Amendment equal protection claims.3

The constitutional claims tied to this practice are important in two ways. Adhering to the limits of Fourth Amendment rights would place limitations on how and when stops could be conducted, even if that meant some sacrifice in the efficiency of stops to detect or deter criminal activity. Constitutional protections place a limitation or ceiling on the extent to which the SQF program could be implemented. Not only would legality be the benchmark for managing SQF encounters, but it also depends on the supply of suspicious behavior that justifies a stop. In effect, if SQF were doing its job, it would reduce the supply of both suspects and suspicious behavior, curtailing the number of stops. Eventually, some equipoise would be reached, allowing for identification of the effects of SQF and the threshold at which those effects would be reached.

3 Floyd, Davis, Ligon, Stinson. Floyd and Ligon were settled at trial in favor of the plaintiffs, citing Fourth and Fourteenth Amendment violation. Davis was settled between the parties, and the remedies for that case were placed under the Order for the Floyd and Ligon cases. Stinson is still being litigated.
The second way that the constitutional claims matter is connected to the first. Many of the civil rights cases that regulate how street stops can be conducted call for a balance of rights (to be free from unjustified or biased searches) with the effectiveness of an activity in reducing crime. A program of stops that produces little in the way of crime control benefits and routinely violating citizens’ rights fails that balancing test (David Floyd et al. v City of New York, 2013). We conceptualize SQFs in impact zones as part of the Operation Impact program because these activities were targeted to high-crime blocks and managed as a policing strategy.4

The only existing analysis of the crime reduction benefits of Operation Impact is an unpublished working paper that used monthly precinct-level crime data as the unit of analysis and a random effects regression model to examine crime rates over time in precincts where impact zones had been in effect (Smith & Purtell, 2007). We improve on this previous analysis of impact zones in five ways. First, we consider the effects of impact zones on crime by taking advantage of geocoded incident-level data on police reported crimes, arrests, and stops in NYC between 2004 and 2012. This allows us to indicate the location of crimes, arrests, and stops at the block-level. We can then determine whether or not each outcome occurred in an impact zone block, to allow for a granular analysis of block-level fixed effects.

Second, we use the timing of each impact zone to identify when an observation is occurring during an impact zone period. Thus, our model uses the timing of impact zones as the primary identification strategy and can control for time stable differences between

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4 See Meares (2015) for a detailed discussion of the difference between a wholesale program of stops and a retail distribution of stops tailored to individual circumstances. See Fagan and Geller (2015) for an analysis of the patterns of justification for SQF that confirms its nature as a program of connected activities aimed at a community.
blocks where impact zones are implemented and those that never experience an impact zone. Third, we compare the changes in crime and arrest patterns in blocks in neighboring areas to match areas with closely comparable risk factors for crime. Fourth, we compare changes in crime and arrests for only those blocks that ever experience an impact zone to address concerns that the selection of impact zones may bias estimates of their impact. Finally, we construct a counterfactual comparison of impact zone blocks to blocks that never adopted impact zones but could have been eligible due to comparable pre-existing crime rates.

4. Data and Methods

We aggregate incident-level data to generate monthly counts of stops, crimes and arrests by census block groups. Observational data on these incidents were recorded by the NYPD from 2004 to 2012, a time span which encompasses the full implementation of Operation Impact following its 2003 phase-in. Addresses in incident data were coded to the nearest block for crimes reported to the police, arrests conducted by the police, and stops made by the police. Data were then aggregated to the census block group by month in each year.₅

We use the total counts of reported crime and arrests, as well as separate measures for robbery, assault, burglary, weapons, misdemeanor offenses (e.g., criminal mischief, fraud, gambling, loitering, petty theft, and larceny), other felonies (e.g., escape 3 and forgery), drugs (e.g., dangerous drugs), property (e.g., grand larceny, burglary, and

₅ Census block groups consist of clusters of contiguous blocks in the same census tract. These are the smallest unit of geography that the Census uses to calculate population estimates (https://www.census.gov/geo/reference/gtc/gtc_bg.html, Accessed January 23, 2015). In New York, the block group often corresponds to a city block (see http://www.urbanresearchmaps.org/plurality/blockmaps.htm).
burglary tools), and violent felonies (e.g., homicide, rape, robbery, arson, serious assault, and kidnapping). We also include measures for whether the census block is located in an impact zone – relying on digital maps supplied by the NYPD.

Finally, we include in which of the 75 police precincts each block is located. Precincts are important because these are the units at which police managers deploy and supervise officers, including officers assigned to impact zones. We don’t calculate rates of crime per population because such rates will be distortedly high in business areas of NYC, such as Times Square or Wall Street, that have daytime populations that far exceed their residential population (see Fagan, 2010). Since the majority of stops take place after 8 PM (NYPD, various years), controls for daytime versus nighttime population were not used. We include block-level fixed effects in our analyses to control for time stable differences between the populations in each area of the city.

Identifying the effect of Operation Impact on reported crimes is complicated by the fact that NYPD self-selects in which areas to create an impact zone. To address the potential selection bias, we estimated the effect of impact zones on crimes and arrests using a longitudinal analysis of block-level crime data. In our first specification, we compared changes in crime and arrests (Y) on blocks before and after the implementation of impact zones to other blocks in the same precinct and time period. We rely on the timing of impact zone adoption as our identification strategy. We assume that shifts in the number of crimes or arrests in a given block month-year, is a function of the timing of IZ implementation and other unmeasured time stable factors, according to the following form:

$$ Y_{ipt} = \mu + \beta IZ_{ipt} + \lambda_{pt} + e_{ipt} $$
In equation 1, \( i \) denotes the block \((n=6,274)\) in which the crime or arrest occurs, \( p \) the precinct in which it is located, \( t \) the month-year \((=2004-01,\ldots,2012-12)\) of observation. For this model, we include fixed effect parameters for each block precinct-year \((\lambda)\) to control for yearly secular trends that are common to all blocks in the same precinct. This provides an estimate of the effect of impact zones on changes in crimes and arrests on blocks before and after they start compared to other blocks in the same precinct at the month-year. In these specifications, we include only blocks with at least 50 violent crimes total between 2004 and 2012 (5.5 per year) to reduce the number of incidental parameters.

The specification of the estimated effects of impact zones on crime and arrests is extended to include controls for the two months prior to and after their implementation. We introduce lag and lead indicators \((T)\) which indicate whether a block group in a precinct had or will have an impact zone in place for \( T = -2, -1, 0, 1, 2 \) months.\(^6\) If impact zones have only short-run effects, this specification should capture those effects directly. If the timing of impact zone formation is influenced by short-term movements in crime, then the lag and lead coefficients in this formulation should control for that form of endogeneity.

\[
Y_{ipt} = \mu + \beta*IZ_{ipt} + \lambda_{pt} + \theta T_t + e_{ipt} \tag{2}
\]

Given that the NYPD selected in which areas to adopt impact zones, selection bias is a natural concern. We rely on a stepped wedge design (Hussey & Hughes, 2007) as our second main specification that compares changes in crime and arrests after the introduction of impact zones for only those blocks that eventually adopted them. The

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\(^6\) We use this number range to have a balanced panel in our regressions.
identification here assumes that the timing of adoption is random, after controlling for secular trends.

\[ Y_{ipt} = \mu + \beta*I_{Zi} + \gamma_i + \delta_t + e_{ipt} \]

This model includes fixed effect parameters for each block (\(\gamma\)) and month-year (\(\delta\)) to control for time stable differences in the average amount of crimes or arrests in each block and secular trends that are common to all blocks that eventually adopt an impact zone.

Our primary specifications in equations 1 to 3 provide only estimates of the effect of impact zone presence, and do not identify the mechanism by which the Operation Impact influences crime or arrest rates. Impact zones differ in size, location, and the number of officers deployed to them.\(^7\) We include a measure of stop, question, and frisk activity (SQF) per month to estimate the variation in the “dose” of proactive policing in impact zones. Because larger impact zones may generate more SQF activities, this measure effectively captures the dose of officers in the impact zone.

\[ Y_{ipt} = \mu + \beta*I_{Zi} + \beta*PC_{ipt} + \beta*I_{Zi}*PC_{ipt} + \beta NPC_{ipt} + \beta*I_{Zi}*NPC_{ipt} + \lambda_{ipt} + e_{ipt} \]

This model compares stops that were based on indicators of a strict definition of probable cause (denoted PC) compared to other reasons that are insufficient to satisfy standards for probable cause (denoted NPC). We identified three indicators of probable cause consistent with analyses of state and federal case law, and which courts have said are sufficient on their own to justify a police stop (Fagan, 2012a; Fagan & Geller, 2015):

1. actions indicative of engaging in drug transaction;
2. actions indicative of violent

\(^7\) While academy graduates were assigned to impact zones, assessing the exact number of officers in an impact zone is difficult for two primary reasons. First, officers were assigned to impact zones but other officers may also be assigned from the precinct to work in impact zones. Second, over time the impact zone assignments became more flexible as a share of academy graduates would be assigned to incident response teams that would rotate around different impact zones in the same NYC borough.
crimes; or (3) “casing” victim or location. NPC factors were subjective and promiscuously invoked criteria whose definitions provide a perceptual space for ambiguity and reliability concerns in measurement. These include, for example, (1) furtive movements, (2) fits descriptions, (3) carrying objects in plain view, (4) suspicious bulge, or (5) evasive actions (Fagan, 2012a; Fagan & Geller, 2015). All models are estimated as Poisson regressions with standard errors clustered on block groups to allow for dispersion and dependence within blocks (Berk and MacDonald 2008).

5. Results

The results from the regressions for model 1 are displayed in Table 1. The top rows present the difference-in-differences specifications (models 1 and 2) that compare the changes in crime on blocks before and after an impact zone is formed compared to other blocks in the same precinct and time period. The bottom rows present the specification that includes the estimates for only those areas that eventually become impact zones (model 3).

The results show a consistent positive effect of impact zones on crime. Model 1 implies that impact zones increase the expected monthly count of crime by 27% (i.e., \(e^{0.246} = 1.27\)). Weapons offenses, which often are an arrest-generated crime,\(^8\) contribute to the largest share of the crime increase, at a 64% expected increase in the count. The increase in weapons offenses reflects primarily the seizure of knives from suspects. Gun possession arrests and gun seizures from street stops remained rare in NYC throughout this period (Fagan, 2012a), fewer than 1% of 4.5 million stops resulted in seizure of any

\(^8\) When an officer stops a suspect and finds a weapon, the officer records the stop but more likely, will generate an arrest report and a corresponding crime report. In other words, the crime would not have been known had the arrest not taken place. Even if the officer stopped a person on suspicion of drugs and then recovered a knife, the officer would generate both an arrest report for the knife and a crime report indicating that a weapons offense had been reported (albeit by the arresting officer).
type of firearm. Overall, violent crimes increased by a predicted 26% after the formation of an impact zone. Drugs and other felony crimes also rose significantly.

--- Table 1 about here ---

In contrast, burglaries dropped significantly after an impact zone was formed, with an expected 33.5% reduction. Burglary arrests also increased significantly after impact zones formed, relative to other blocks in the same precincts during the same time of year. These two sets of results suggest some crime suppression effects of impact zones on burglaries are likely driven by heightened probability of arrests for these offenses. The total property crime counts aren’t impacted.

Model 2 includes four indicators for the two months before and after the start of the impact zone. The coefficients are slightly smaller in each outcome, suggesting that the implementation of impact zone timing was only partially determined by the crime rates in the preceding months. But the general pattern of findings is largely the same and shows that overall weapons and violent crimes increase after impact zones are formed, while burglaries drop significantly.

These results suggest that Operation Impact increased crime in part by increasing reported crimes related to assaults, robberies, and weapons. With the exception of weapons offenses, these results do not appear to be driven by arrest-related crime reports as arrest outcomes for robbery and assaults do not change significantly after the formation of impact zones. Instead, reporting of serious crime to the police seems to have followed the startup of impact zone activity, even if those reports are distinct from other categories of arrest-generated crime reports.
Model 3 shows the results from the analysis of only those areas that eventually become impact zones. Here the results show significantly smaller effects of Operation Impact on increasing total crime, on the order of 4%. Calculating the marginal effect over an average block for a 6-month time period implies a total increase of 1.41 crimes. In contrast to the difference-in-differences models, we see that overall counts of robberies, assaults, burglaries, and murder significantly drop after an impact zone is formed compared to other areas that eventually adopted them. The effect on robbery is estimated to be a 4% monthly reduction, or the equivalent of about .13 robberies over six months. The largest estimated crime reduction for impact zone formation is for burglaries, reducing the expected count by 14.1% or about .25 burglaries over the course of six months. Burglary arrests also increase significantly in this specification, suggesting that the formation of impact zones had a consequence on arrests for burglary for any area that eventually adopted an impact zone.

Table 2 shows the results from model 4 that estimate the dose of police stops for probable cause (PC) and non-probable cause (NPC) occurring per month after the formation of impact zones compared to other blocks in the same precinct year. Across all models, the results (not shown) indicate that both types of stops are more frequent where crimes are higher.

However, the primary focus of this analysis is whether the timing of the impact zone formation and stop activity is correlated with a shift in crime rates. For ease of interpretation, Table 2 only shows the results from the coefficients for the interactions of stop types and impact zone formation.
The findings imply that increases in probable cause SQFs, after the formation of impact zones, are associated with reductions in crimes across the board. The estimates, however, suggest statistically significant results with little practical importance. The effect size for probable cause stops translates into a return of about three fewer crimes for each increment of 100 PC stops per month after the formation of an impact zone.\(^9\) There were 2.78 probable cause stops in an average month for an impact zone block after its formation. This indicates that a ten-fold increase in the number of probable cause stops made after an impact zone is formed is necessary to avert more than one crime. By contrast, the increase in stops not based on probable cause after the formation of impact zones has no association with reductions in crimes. Not only are such stops unproductive with respect to crime control, but they also are constitutionally problematic.

These estimates are largely the same across all specifications, showing that stop activity was not particularly beneficial for any single crime category. Non-probable cause stops appear to have no material effect on crime, but they are 50\% more common on average in impact zones. The results are largely the same for arrests, showing that stops for probable cause reasons related to crime casing, engaging in violent crimes, or drug transactions are related to fewer arrests; whereas non-probable cause reasons have largely no association.

**Robustness Checks**

We conducted several robustness tests to examine whether the estimates we report are sensitive to the selection of impact zones and their timing.

\[^9\] \[\exp(-.01*100)*5.05]-[\exp(-.01*0)*5.05]=-3.19\]
First, to address whether the estimates are at least partially attributable to autocorrelation in the timing of impact zones and the secular trends in NYC on crime and arrests, we used a permutation test that randomly reassigned the timing of impact zone blocks 1,000 times and re-estimated models 1 to 3. If we use the conventional estimates for the treatment effect and the standard error, we find that our test statistics are never exceeded in each model. For the basic difference-in-differences (model 1), the largest estimated effect in 1,000 shuffled impact zone timings is an increase of .0105 (standard error of .004) compared to the actual estimated value of .246 (standard error .032) in Table 1.

We observed similar results of sensitivity tests for models 2 and 3. For model 2, that included event history leads and lags, the largest estimated effect from 1,000 shuffled impact zone timings is .012 (standard error .004) compared to the actual estimated effect of .212 (standard error .030). For model 3, that only estimates effects for only those areas that eventually become impact zones, the largest estimated effect is .015 (standard error .005) compared to an actual estimated effect of .043 (standard .013). In all cases the findings indicate that the estimates shown in Table 1 are not caused by autocorrelation in the timing of impact zones.

Next, we re-estimated model 1 by removing all blocks that were initially part of Operation Impact Era 3 (January-June 2004), since we have no prior crime or arrest data for these areas. These results are shown in Table 3 and indicate total crime and arrests increase significantly when blocks that were part of the first impact zone era are excluded. The largest increase appears to be related to weapons crimes and arrests. Consistent with the model estimated on all the data, we see that burglaries drop significantly with the
formation of an impact zone on a block, compared to other blocks in the same precincts during the same time of year. The overall pattern suggests that results are not sensitive to the inclusion of impact zone era 3, the baseline era when potential active offenders might have been caught off guard by the new surge in police.

– Table 3 about here –

Third, we tested for the effects of selection bias resulting from the fact that that blocks that become impact zones are all high crime areas. We estimated a propensity score model that equalizes the blocks that receive impact zones and compares blocks that had similar crime trajectories. We first estimate the average count of crime in each block, conditional on the year, using a zero inflated Poisson regression group-based trajectory model (Jones & Nagin, 2013). Each block then receives a probability of membership in any one of three yearly crime trajectory groups, reflecting high, medium, or low counts of crime (see Appendix A). We then estimate a logistic regression model of the probability that a block ever becomes an impact zone, conditional on crime trajectory group probabilities. The predicted probabilities from this model are then converted into propensity score weights (Haviland et al., 2007). Blocks that become impact zones receive weights equal to 1; whereas comparison blocks receive an inverse probability weight (IPW) equal to \( p/1-p \), according to their propensity score. These IPW values give greater weight to blocks for comparisons that have comparable pre-existing crime trajectories. We then re-estimate model 1 using the propensity scores as weights. This approach effectively estimates a difference-in-difference model but places more weight on blocks with comparable crime trajectories.
The results shown on the bottom rows of Table 3 indicate that total crime increases with the formation of an impact zone compared to other blocks in the same precincts and years with similar crime trajectories. Most of the increase in crime appears to be driven by weapons and other arrest related offenses. Overall, violent crimes also increase significantly in impact zone blocks compared to blocks in the same precinct with similar crime trajectories. Consistent with all models, burglaries drop significantly with the formation of an impact zone.

Finally, we wanted to see how sensitive the results were to the date of impact zone formation to check against the possibility of an “Ashenfelter dip” (Ashenfelter & Card, 1985). Blocks may enter the impact zone because they recently had a spike in crime, such that mean reversion would lead to upward biased estimates in the difference-in-differences models 1 and 2. To assess this potential problem, we re-estimated model 1 for crimes and arrests reassigning the timing of impact zones to the two months before the actual start date and the two months after.

– Table 4 about here –

The results shown in Table 4 for the two months before suggest that impact zones have 32 to 50% smaller effect in increasing offenses and arrests compared to the actual start date. For the two months after, the results suggest that impact zones had a 37 to 40% smaller effect in increasing offenses and arrests. The one outcome that appears to be sensitive to an Ashenfelter dip is robbery. Here we see that the effect is larger for the two months before than the date of assignment, and it is smaller and statistically insignificant in the two months after. However, these findings are not surprising given that the event history model that included two month lead and lag values (model 2) also found null
effects for robbery. Taken together the results suggest no clear pattern of mean reversion in any crime or arrest other than robbery.

6. Discussion

The U.S. Supreme Court in *Terry v. Ohio* ruled the police based on their experience and training had the power to stop, question, and frisk when they had reasonable suspicion that a crime was “afoot” – crime had just occurred, was in process, or was about to take place.\(^\text{10}\) Officers were required to form “reasonable suspicion” based on specific, articulable, and individualized factors that were observable. As practiced in New York in the decade in which we analyzed the data, SQF appears to have transformed from a practice of individuals officers to a policy-based program of police interventions that, although practiced citywide, was concentrated in specific high crime areas (Meares, 2015). The intensity of the SQF under Operation Impact, with their specific spatial borders in high crime areas created the conditions for a rigorous test of this program of both targeted deployment and proactive policing. This test also presents an opportunity to test the claims of both public officials and legal scholars that targeted SQFs were likely the primary driver of steadily declining crime rates over the past two decades in NYC (Zimring, 2011; Weisburd et al., 2014).

In this paper we provide new robust evidence of the NYPD’s main crime suppression deployment model under Operation Impact that rolled out over a ten-year time period. The results suggest a complicated set of effects that present both good and bad news for concentrated police deployment and SQF as a crime reduction strategy in

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\(^\text{10}\) *Terry v. Ohio*, 392 U.S. 1, 31 (1968) (holding that “[T]he detention must be based upon reasonable suspicion that criminal activity is afoot. Reasonable suspicion is simply specific, articulable facts combined with the rational inferences from these facts, and taken in light of an officer’s training and experience, that leads an officer to believe criminal activity is occurring or has just occurred.”)
high crime areas. We find consistent and compelling evidence that the formation of impact zones significantly reduced burglary offenses. The data, however, do not distinguish a clear mechanism of this effect. The increase in probable cause related SQFs after the formation of impact zones do have the strongest association with reduced burglary reports and increased burglary arrests, suggesting that physical presence of more police and enhanced apprehension likely generated a deterrent effect. If officers were attuned to the signs and indicia of crime, as stated policy suggested (Spitzer, 1999; Fagan et al., 2010), then the suppression of property crimes by their mere presence is a welcome byproduct of the surge of officers assigned to those areas. For a comparison of only those areas that eventually adopt impact zones it also appears they have some impact on robberies and assaults, thought the effects are relatively small and suggest a 4% reduction.

However, Operation Impact also appears to significantly raise reported crime rates in city blocks by increasing the number of weapons, drugs, and other felony related offenses. The formation of impact zones also leads significantly more SQFs by the police. The increase in weapons and drug offenses are artifacts of arrests for those two crime categories – these two crime types in particular are arrest-generated offenses. This particular result is a positive outcome given the animating logic of SQF generally and Operation Impact in particular to remove weapons from the streets (Spitzer, 1999; Dickey, 2008).

Together, the results suggest that Operation Impact was not a major contributor to crime reductions in NYC. Moreover, the scale of deployment and the level of stop activity suggest that this program may have been more productive if the focus were placed on more limited use of proactive stops that are more directly related to criminal
activity. These findings are important for they suggest that more police activity and deployment to high crime areas can reduce criminal activity when it is tied to effective police tactics.

The fact that every specification shows a significant increase in total crime, weapons, misdemeanor, and the general other felonies categories after the formation of impact zones does not necessarily mean that these crimes actually increased, as it is clear that arrests also increased for these offenses. Crime reporting is also sensitive to police contact, so when more police are present there is a greater likelihood that victims will come into contact with them and report an offense. An increase in reporting of crime may be good news for Operation Impact, if this means, as has been shown in other studies, an increased presence of police in a neighborhood with positive interactions can enhance legitimacy and cooperation of citizens with the police (Tyler & Huo, 2002; Tyler & Sunshine, 2003; Tyler & Fagan, 2008; Reisig & Lloyd, 2009; Jackson et al., 2012; Tyler, Fagan & Geller, 2014). Despite problems in the conduct of stops under the SQF practices reported by residents in high crime areas exposed to this tactic (Geller, Fagan, Tyler & Link, 2014; Tyler, Fagan & Geller, 2014), the net legitimacy effect from the increased police presence in high crime areas such as impact zones apparently can produce a positive by product of increased crime reporting.

The results also show that when SQFs increase after the formation of impact zones and are made on “actions indicative” of ‘casing a victim or a place’, ‘engaging in a drug transaction, or ‘engaging in violent crimes’ they appear to have some crime suppression benefit. But, the bulk of SQFs made in impact zones are based on more subjective criteria of suspicion (e.g., furtive movement, suspicious bulge) that appear to
have no crime reduction benefit. The legality of these types of SQFs was a contentious issue that led to four separate civil rights actions in federal courts. NYC justified that some civil rights tradeoffs were necessary as SQF led to public safety benefits (Meares, 2015; Floyd, 2013; Ligon, 2013; Davis, 2014). While the federal court ultimately ruled that this tradeoff was not relevant to Fourth Amendment protections against unreasonable search and seizure by the police, the results from this analysis suggest that outside of legal rulings there is little empirical evidence to suggest that when SQFs are made on only general suspicion they appear to have no crime reduction benefit in impact zones.

Crime reduction is no doubt an important policy goal of police deployment and SQFs. There appears to have been some benefits of Operation Impact in reducing burglary, increasing arrests for weapons and drugs, and an increasing the reporting crimes to the police. Deploying extra police to high crime areas and asking them to be vigilant appears to have some crime benefits, but only when the vigilance is based on articulable behaviors of crimes occurring. Police interventions of the sort undertaken by Operation Impact should pay careful attention that increased vigilance does not come at the cost of extra intrusion on local residents that has no crime reduction benefit.
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*Terry v. Ohio*, 392 U.S. 1 (1968)
Figure 1: NYC Crime Reports for Murder & Robbery, Burglary, and Felony Assault 2004-2012
Figure 2: NYC Crime Reports for All Misdemeanors and Felonies 2004-2012
Figure 3: Rollout of Impact Zones 2004-2012
Figure 4: Stops in Impact Zones and Other Areas
**Table 1 – Effect of Impact Zones on Crimes and Arrests**

| Crime | Total | Robbery | Assault | Burglary | Weapons | Misd. | Other Felony | Drugs | Property | Violent Felony |
|-------|-------|---------|---------|----------|---------|-------|-------------|-------|----------|---------------|
| **Model 1** | | | | | | | | | | |
| **IZ** | 0.246* | 0.151* | 0.080* | -0.408* | 0.496* | 0.097* | 0.776* | 0.083* | 0.015 | 0.235* |
| **N=843,556** | (0.032) | (0.030) | (0.028) | (0.036) | (0.048) | (0.036) | (0.052) | (0.032) | (0.041) | (0.028) |
| **Mean** | 5.487 | 0.305 | 0.192 | 0.225 | 0.124 | 2.247 | 0.057 | 0.381 | 0.850 | 1.092 |
| **Model 2** | | | | | | | | | | |
| **IZ** | 0.212* | 0.121* | 0.077* | -0.371* | 0.449* | 0.078* | 0.682* | 0.066* | -0.003* | 0.213* |
| **N=843,556** | (0.030) | (0.029) | (0.027) | (0.035) | (0.045) | (0.034) | (0.053) | (0.034) | (0.039) | (0.028) |
| **Mean** | 4.910 | 0.318 | 0.220 | 0.149 | 0.172 | 1.708 | 0.072 | 0.577 | 0.542 | 1.166 |
| **Arrests** | Total | Robbery | Assault | Burglary | Weapons | Misd. | Other Felony | Drugs | Property | Violent Felony |
| **Model 1** | | | | | | | | | | |
| **IZ** | 0.403* | 0.003 | -0.012 | 0.376* | 0.271* | 0.282* | 0.490* | -0.082* | 1.123* | 0.026 |
| **N=843,556** | (0.047) | (0.062) | (0.062) | (0.187) | (0.047) | (0.075) | (0.051) | (0.029) | (0.112) | (0.050) |
| **Mean** | 3.508 | 0.100 | 0.071 | 0.021 | 0.147 | 0.265 | 0.080 | 1.030 | 0.525 | 0.344 |
| **Model 2** | | | | | | | | | | |
| **IZ** | 0.354* | 0.013 | 0.003 | 0.327* | 0.251* | 0.225* | 0.413* | -0.088* | 1.019* | 0.028 |
| **N=319,776** | (0.043) | (0.058) | (0.066) | (0.177) | (0.045) | (0.073) | (0.051) | (0.031) | (0.104) | (0.049) |
| **Mean** | 3.508 | 0.100 | 0.071 | 0.021 | 0.147 | 0.265 | 0.080 | 1.030 | 0.525 | 0.344 |
| **Model 3** | | | | | | | | | | |
| **Ever IZ** | 0.308* | 0.040 | 0.066 | 0.571* | 0.262* | 0.276* | 0.212* | -0.082* | 0.742* | 0.115 |
| **N=122,266** | (0.045) | (0.077) | (0.078) | (0.190) | (0.048) | (0.095) | (0.076) | (0.026) | (0.113) | (0.064) |
| **Mean** | 4.031 | 0.091 | 0.066 | 0.014 | 0.176 | 0.257 | 0.096 | 1.283 | 0.772 | 0.299 |

*p<.01; **p<.05; Full sample represents 847,928 census block month-years.

Note: The data represent average monthly crime and arrests counts on NYC blocks over the period 2004–2012. Total crime includes the sum of all 52 different crime categories consistently recorded. Total arrest includes the sum of all 65 arrest categories consistently recorded. Standard errors are reported in parentheses and clustered at the census block level. Arrests missing 5 months of data in 2012.
Table 2 – Effect of Impact Zone Stops on Crime and Arrests

| Variables          | Crime       | Standard error | t-stat | Arrest      | Standard error | t-stat |
|--------------------|-------------|----------------|--------|-------------|----------------|--------|
| Total: IZ*PC       | -0.010*     | 0.002          | -4.825 | -0.006*     | 0.003          | -1.950 |
| Total: IZ*NPC      | 0.002       | 0.002          | 1.259  | -0.003      | 0.003          | -1.038 |
| Robbery: IZ*PC     | -0.009*     | 0.002          | -3.803 | -0.007**    | 0.004          | -1.916 |
| Robbery: IZ*NPC    | -0.001      | 0.002          | -0.397 | 0.007       | 0.003          | -0.038 |
| Assault: IZ*PC     | -0.008*     | 0.002          | -3.617 | 0.000       | 0.003          | -0.038 |
| Assault: IZ*NPC    | -0.002      | 0.002          | -0.840 | -0.009*     | 0.003          | -2.896 |
| Burglary: IZ*PC    | -0.015*     | 0.003          | -4.386 | -0.024**    | 0.013          | -1.927 |
| Burglary: IZ*NPC   | 0.005       | 0.002          | 2.071  | 0.001       | 0.006          | 0.156  |
| Weapons: IZ*PC     | -0.009*     | 0.003          | -3.082 | -0.008*     | 0.003          | -2.590 |
| Weapons: IZ*NPC    | 0.000       | 0.002          | 0.134  | 0.000       | 0.003          | 0.046  |
| Misdemeanor: IZ*PC| -0.011*     | 0.003          | -3.456 | -0.005      | 0.004          | -1.495 |
| Misdemeanor: IZ*NPC| 0.004     | 0.002          | 2.070  | -0.003      | 0.003          | -1.132 |
| Other Felony: IZ*PC| -0.015*    | 0.003          | -5.173 | -0.014*     | 0.003          | -4.253 |
| Other Felony: IZ*NPC| 0.000    | 0.003          | -0.108 | -0.001      | 0.004          | -0.203 |
| Drugs: IZ*PC       | -0.013*     | 0.003          | -4.123 | -0.005*     | 0.002          | -2.617 |
| Drugs: IZ*NPC      | 0.000       | 0.002          | 0.033  | -0.002      | 0.002          | -1.168 |
| Property: IZ*PC    | -0.011*     | 0.003          | -3.974 | -0.014*     | 0.004          | -3.197 |
| Property: IZ*NPC   | 0.002       | 0.002          | 1.292  | -0.004      | 0.005          | -0.747 |
| Violent: IZ*PC     | -0.009*     | 0.002          | -4.650 | -0.001      | 0.002          | -0.241 |
| Violent: IZ*NPC    | 0.001       | 0.002          | 0.351  | -0.006*     | 0.003          | -2.036 |

*p<.01; **p<=.05
Note: Standard errors are clustered at the census block level. Arrests missing 5 months of data in 2012. All models include parameters for impact zone, probable cause, and non-probable cause stops.
|                  | Total | Robbery | Assault | Burglary | Weapons | Misd. | Other Felony | Drugs | Property Felony | Violent Felony |
|------------------|-------|---------|---------|----------|---------|-------|--------------|-------|----------------|----------------|
| **Crime**        | 0.247*| 0.151*  | 0.081*  | -0.409*  | 0.496*  | 0.098*| 0.777*       | 0.084*| 0.015*         | -0.141*        |
|                  | (0.033)| (0.030) | (0.028) | (0.037)  | (0.049) | (0.037)| (0.053)      | (0.033)| (0.042)        | (0.028)        |
| **N=**           | 843,556|         |         |          |         |       |              |       |                |                |
| **Arrests**      | 0.264*| -0.046  | -0.005  | -0.198   | 0.245*  | 0.156 | 0.425*       | -0.127*| 0.813*         | -0.004         |
|                  | (0.037)| (0.089) | (0.087) | (0.170)  | (0.054) | (0.119)| (0.065)      | (0.030)| (0.096)        | (0.073)        |
| **N=**           | 319,776|         |         |          |         |       |              |       |                |                |
| **Crime Propensity Group** | 0.062*| -0.002  | -0.023* | -0.430*  | 0.345*  | -0.015*| 0.618*       | 0.035*| -0.102*        | 0.047*         |
|                  | (0.005)| (0.011) | (0.011) | (0.013)  | (0.012) | (0.006)| (0.021)      | (0.009)| (0.0085)       | (0.006)        |
|                  | 537,321|         |         |          |         |       |              |       |                |                |

*p<.01; **p<=.05;

Note: Crime = Blocks never part of Impact Zone 3 controlling for precinct-month fixed effects with at least 50 violent crimes. Arrests = Blocks never part of Impact Zone 3 controlling for precinct-month fixed effects. Crime Propensity Group = Blocks after Impact Zone formation compared to blocks with similar predicted crime trajectories. Standard errors are reported in parentheses and clustered at the census block level.
|                | Total | Robbery | Assault | Burglary | Weapons | Misd. | Other Felony | Drugs | Property | Violent |
|----------------|-------|---------|---------|----------|---------|-------|--------------|-------|----------|---------|
| **Crime**      |       |         |         |          |         |       |              |       |          |         |
| 2 months before| 0.170*| 0.179*  | 0.048   | -0.262*  | 0.267*  | 0.074*| 0.471*       | 0.042 | 0.054    | 0.161*  |
|                | 0.026 | 0.035   | 0.032   | 0.042    | 0.047   | 0.028 | 0.054        | 0.025 | 0.033    | 0.026   |
| 2 months after | 0.148*| -0.040  | -0.023  | -0.376*  | 0.345*  | 0.052 | 0.566*       | 0.112*| -0.049   | 0.076*  |
|                | 0.026 | 0.036   | 0.028   | 0.045    | 0.046   | 0.028 | 0.052        | 0.026 | 0.037    | 0.025   |
| **N=**         | 843,556|
| **Arrest**     |       |         |         |          |         |       |              |       |          |         |
| 2 months before| 0.211*| 0.015   | 0.030   | 0.204    | 0.058   | 0.195*| 0.299*       | -0.029| 0.671*   | 0.053   |
|                | (0.040)| (0.069) | (0.086) | (0.160)  | (0.051) | (0.059)| (0.070)      | (0.023)| (0.089)  | (0.047) |
| 2 months after | 0.276*| -0.100  | -0.016  | 0.326    | 0.242*  | 0.277*| 0.359*       | -0.018| 0.689*   | -0.012  |
|                | (0.037)| (0.092) | (0.067) | (0.170)  | (0.060) | (0.072)| (0.061)      | (0.022)| (0.077)  | 0.047   |
| **N=**         | 341,962|

*p<.01; **p<=.05;
Appendix A: Difference Before and After Weighting on the Probability of Trajectory Groups

| Group  | Average Crimes | Impact Zone Blocks | Comparison Blocks Un-weighted | Comparison Blocks Weighted | $D_x$ | $D_{sm}$ |
|--------|----------------|--------------------|-------------------------------|---------------------------|-------|---------|
| Group 1| 3.02           | .774 (.0007)       | .723 (.0005)                  | .774 (.0004)              | .061  | .0008   |
| Group 2| 11.41          | .211 (.006)        | .251 (.0004)                  | .212 (.0004)              | .050  | .0009   |
| Group 3| 37.94          | .013 (.0002)       | .024 (.0001)                  | .013 (.0001)              | .036  | .000    |

N= 268,674  579,254  268,647

Note: Standard errors in parentheses.
n=effective sample size due to weighting on propensity score.
$D_x$ represents the mean-standardized difference before weighting on propensity score
$D_{sm}$ represents the mean-standardized difference after weighting on propensity score.