The Effect of Order-Maintenance Policing on Serious and Non-Serious Crime: Evidence from a Quasi-Experiment

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January 29, 2010

Abstract

This paper presents quasi-experimental evidence on the effect of order-maintenance policing on both serious and non-serious crime. In 1997, the New York City Police Department engaged in a nine-month ticket-writing slowdown in response to a long and contentious wage contract negotiation. As a result of this labor action, parking, traffic, and criminal summonses fell by 51, 42, and 23 percent respectively. Drawing on several different data sources, I use this exogenous variation in summons-writing to empirically test whether serious and non-serious crimes are deterred by low-level, order maintenance policing. I find that, citywide, complaints for both serious and non-serious crimes increased during the slowdown period. However, a borough-by-borough comparison of crime rates during the slowdown period reveals that the patrol boroughs that participated the most in the ticket-writing slowdown experienced statistically indistinguishable increases in crime when compared to the boroughs that participated the least. These results suggest that order-maintenance policing does not have an effect on either serious crime or on non-serious crime.

*I would like to thank David Card, Patrick Kline, Alex Mas, and Steve Raphael for many helpful conversations. I would also like to thank Carlos Dobkin and Rick Brooks for early guidance on the project. I am grateful for comments from seminar participants at Arizona State University Law School, Bryn Mawr College, John Jay College of Criminal Justice, Northwestern Law School, University of California-Berkeley, University of Maryland, and Vassar College, and conference participants at the 2009 Conference on Empirical Legal Studies. Lieutenant Scarazzini of the Office of Management Analysis and Planning provided the NYPD crime and summonses data. Louis Pereira of the NYC Department of Finance provided the data on NYC parking summonses. Jens Ludwig very generously shared his data on NYPD manpower. All errors are my own.

1 Introduction

Order-maintenance policing, the practice of aggressively policing community disorder to deter serious crime, is increasingly becoming a mainstay in major metropolitan police departments. While the strategy received its first major implementation in 1993 in Chicago, it has since been adopted by police departments in other large metropolitan areas such as New York City (1994), Los Angeles (2002), Boston (2006) and Denver(2006).[1] Police departments are not the only group that seem to favor the strategy; order-maintenance or broken windows policing has proven to be popular with community members, primarily because of its focus on cleaning up neighborhoods and reducing levels of community disorder.[2]

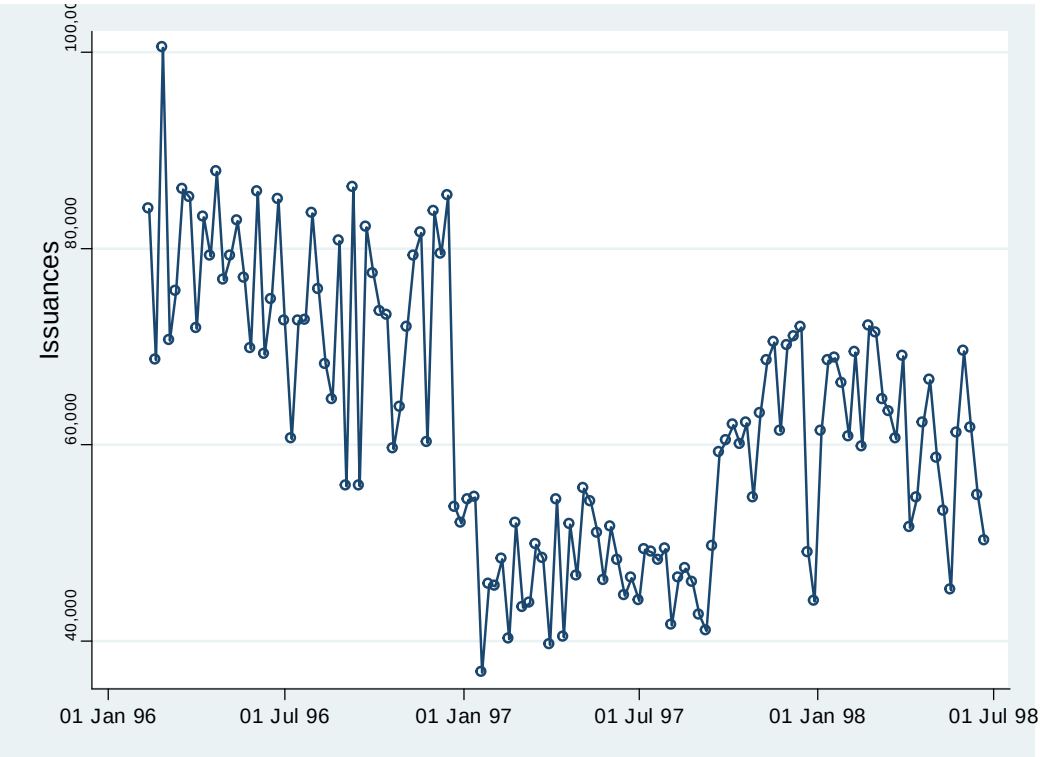
Despite the support of order-maintenance policing by law enforcement agencies and the public, there has been a lot of disagreement within the academic community as to whether or not "fixing" broken windows actually deters serious crime. There have been a flurry of papers from both sides of the debate claiming to either empirically prove or disprove the hypothesis that "cleaning up the streets" reduces serious crime. While the weight of the current evidence tends to be leaning towards the conclusion that order-maintenance policing does not deter serious crime, the literature is far from a universal consensus on the topic. At least part of the reason for the continuing debate on the subject is that, to date, there has been no experimental or quasi-experimental study of the order-maintenance policing question.[3],[4]

[1] The dates for Chicago and New York come from *Illusion of Order: The False Promises of Broken Windows Policing* by Bernard Harcourt. Denver: http://www.denvergov.org/NewsReleases/tabid/390355/newsid470102/1291/Hanover-Justice-Group-Launches-Crime-Reduction-Demonstration-Projects-in-Denver/Default.aspx. Boston: http://www.boston.com/news/globe/ideas/articles/2006/02/19/th_cracks_in_broken_windows/.
Los Angeles: Opinion editorial by Bernard Harcourt, http://articles.latimes.com/2006/apr/20/opinion/oe-harcourt20.

[2] See Skogan (2008) in showing that people tend to be more worried about the day-to-day disorder in their neighborhoods than they are about crime. Thacher (2004) makes the related point that the academic challenges to the broken windows policing hypothesis "have not yet discredited order maintenance policing with policy makers or the public" (p.386). But Kubrin (2008) points out that the execution of the policing strategy is critical; specifically, mandated crackdowns on community disorder are likely to be met with resistance from community members.

[3] Note, however, that Harcourt and Ludwig (2006) provided the first quasi-experimental evidence on the issue of whether neighborhood disorder affects an individual's propensity to commit crime.

[4] Braga and Bond (2008) was just brought to the author's attention as an interesting article that uses experimental methods to study the effects of problem-oriented policing on crime in hot spots. Future drafts of this paper will incorporate the Braga and Bond study into the literature review.



Introduction Figure: NYPD Parking Summonses, Weekly Data

In this paper, I use a quasi-experimental research design to examine the effect of order-maintenance policing on both serious crime and non-serious crime (or disorder).[5] As such, this paper represents the first such effort. Specifically, my research design uses exogenous variation in the policing of traffic-related offenses in New York City to identify the order-maintenance policing effect. In January 1997, the police officers of the New York City Police Department (NYPD) engaged in a nine-month ticket-writing slowdown to protest what they believed to be unfair wage negotiations. This labor action led to a sharp and dramatic decrease in ticket-issuance productivity for all types of summonses. (The Introduction Figure above shows the effect of the labor action on NYPD parking summonses.) Though the pointed goal of the slowdown was to exert financial pressure on the city to offer a more favorable wage contract, I investigate the secondary effect (if any) on serious crime and non-serious crime of this exogenous negative shock to traffic-related order-maintenance policing. I find that while all categories of crime were elevated during the slowdown period, there

[5] In what follows, I equate non-serious crime with disorder and thus use the terms interchangeably.

is no evidence to support the claim that the crime increases were related to the decline in traffic enforcement. These results imply that traffic-related order-maintenance policing, as measured by the level of hazardous traffic summonses, has neither an effect on serious crime nor on non-serious crime.[6]

This study proceeds as follows. Section 2 reviews the current literature on the order-maintenance policing debate. Section 3 introduces the various data sources used in the analyses. Section 4 provides both a historical and empirical documentation of the labor action. Section 5 presents the analysis of the order-maintenance policing effect and Section 6 concludes.

2 Review of the Literature on Order-Maintenance Policing

The central thesis of James Q. Wilson and George Kelling's 1982 article on order-maintenance policing is that serious crime can be deterred by policing disorder or non-serious crime. According to Wilson and Kelling, the aggressive enforcement of the most minor of laws signals a general willingness by community members and local police officers to defend the community against all offenses, especially those of a more serious nature.

While there are several articles that aim to generally test the theory that order-maintenance policing deters serious crime, probably the most widely cited are those that have aimed to test the theory within the particular context of New York City.[7] New York City provides a particularly intriguing case study of the broken windows policing theory because the initiation of the NYPD's order-maintenance policing strategy in the early 1990s, the centerpiece of its law enforcement policy under Mayor Rudolph Giuliani, was highly coincident with the city's record-breaking crime declines. From 1990 to 1999, violent crime fell by 56% and property crime fell by 65% in New York City; as a point of comparison, the national violent and property crime rates fell by 28% and 26%, respectively, during the same time period.[8] A first-order question for interested social scientists, then, has been whether a causal relationship exists between the two factors. Not surprisingly, the

[6] Hazardous traffic summonses are a subcategory of general traffic summonses. See Section 3.1.3. for more details on hazardous traffic summonses.

[7] See Harcourt and Ludwig (2006) for a review of articles that test the broken windows policing theory outside of the New York City context.

[8] Corman and Mocan (2005).

main challenge in answering this question has been attempting to control for other factors that were also at play during the order-maintenance policing policy change and that could possibly be confounding the effect. In what follows I outline the three main articles that lie at the heart of the New York City order-maintenance policing debate and also, I feel, best represent the general body of current empirical knowledge on order-maintenance policing.

In arguably the best effort up to that point, George Kelling and William Sousa, in a 2001 article, set out to settle the debate of whether order maintenance policing deters serious *violent* crime by assembling a data set of not only precinct-level crime but also borough-level proxies for the factors most likely to be at play in confounding the broken windows effect: the crack epidemic, the unemployment rate, and demographic changes. Using a hierarchical linear model, they report a strong correlation between misdemeanor arrests (their measure of order-maintenance policing) and violent crime.[9,10] Specifically, they find that the NYPD precincts that had the highest average levels of misdemeanor arrests during their sample period (1989-1998) also had the largest declines in violent crime.

In a 2006 replication and response paper, Bernard Harcourt and Jens Ludwig correctly point out that there is no attempt in Kelling and Sousa's model to control for the possible effects of mean reversion on precinct violent crime rates. In the mid- to late 1990s, many cities were experiencing crime declines, even those without order-maintenance policing strategies in place. According to a study by Ludwig and another author, Steven Raphael, the predominant reason for the large crime declines in these other cities was mean reversion—that the crack cocaine-induced, large crime increases of the mid- to late 1980s were followed by large crime declines in those same cities ten years later as crime trends returned to their mean levels. In the particular case of New York City, Harcourt and Ludwig document that the order-maintenance policing strategy was instituted

[9] It is not clear why Kelling and Sousa chose misdemeanor arrests instead of criminal summonses as their measure of order-maintenance policing. Though they point out that violation summonses (a particular type of criminal summonses) correlate well with misdemeanor arrests, they do not explain why they ultimately picked misdemeanor arrests to proxy order-maintenance policing. (See Kelling and Sousa's footnote number 32).

[10] As pointed out by Harcourt and Ludwig, Kelling and Sousa do not report their estimating equations in their paper, making it somewhat difficult to follow their analysis and interpret their reported coefficients. Harcourt and Ludwig, in their replication, write down their best guess of Kelling and Sousa's estimating equations. I use these equations in my evaluation of Kelling and Sousa's work.

most aggressively in those NYPD precincts that one would have expected to have the largest crime declines anyway because of mean reversion. In particular, Harcourt and Ludwig show that the precincts with the highest initial levels of misdemeanor arrests in 1989 (the start of the sample period) and the largest increases in misdemeanor arrests over the course of the sample period (1989-1998), were also the ones with: (1) the largest initial levels of violent crime in 1989, (2) the largest increases in violent crime from 1984 to 1989 and, (3) because of mean reversion, the largest crime declines from 1989 to 1998. According to Harcourt and Ludwig's analysis, the correlation between average precinct levels of misdemeanor arrests and precinct changes in violent crime is reduced by two-thirds once mean-reversion is accounted for by including the initial 1989 level of violent crime and the 1984-1989 change in violent crime in the regression. The correlation continues to decrease and ultimately becomes statistically insignificant once other key control variables are included.

Hope Corman and Naci Mocan (2005) also analyze the effect of misdemeanor arrests on New York City violent crime but they do so using city-level, monthly time series data (1974-1999), as opposed to the precinct-level, annual data used in the two studies discussed above. In their analysis, Corman and Mocan find a negative and statistically significant correlation between misdemeanor arrests and robbery and motor vehicle theft crime. However, as is also pointed out by Harcourt and Ludwig (2006), it is difficult to say that this relationship is causal because Corman and Mocan provide no control city in their analysis–undoubtedly a challenge when studying a city as unique as New York. Though Corman and Mocan include a rich set of controls in their analysis, in the absence of any quasi-experimental variation in misdemeanor arrests or comparison of New York City's crime trends to another city's crime trends, it is difficult to definitively attribute the robbery and motor vehicle theft effects to variations in misdemeanor arrests.

In a further attempt to improve upon the research designs of Kelling and Sousa (2001) and Corman and Mocan (2005), Harcourt and Ludwig (2006) use experimental variation from the Moving to Opportunity (MTO) demonstration to answer the related but distinct question of whether the mere presence of disorder increases both serious crime and non-serious crime. Harcourt and Ludwig argue that since it is unclear whether increases in misdemeanor arrests actually decrease disorder,

their MTO study provides a more basic, and perhaps more relevant, test of the order-maintenance policing theory by asking whether order, the ultimate goal of order-maintenance policing, is related to crime. As a consequence of the MTO demonstration, members of the treatment group moved to neighborhoods that had lower levels of physical and social disorder than their counterparts in the control group who did not move. In their study, Harcourt and Ludwig find that members of the treatment group as a whole were not significantly less likely to participate in criminal activities than members of the control group. And, for certain subgroups, such as men, moving to a new, more affluent neighborhood actually increased criminal activity.[11] While these findings have interesting implications, it is difficult to attribute these crime effects (or non-effects) exclusively to disorder alone. As the authors readily admit, the new neighborhoods had less disorder but, because of the gentrification, also presented new crime opportunities. Thus, it is unclear how much of the treatment dose was just order alone.

The present paper thus continues in the spirit of Harcourt and Ludwig's MTO study but redirects the focus of the debate back on order-maintenance *policing*. I agree with Harcourt and Ludwig that the effect of disorder on serious crime is an important issue that deserves scientific exploration, especially given that disorder-reduction is the hypothesized mechanism through which order-maintenance policing is supposed to work. However, the reality of the current policing climate is that order-maintenance-type strategies are increasingly being adopted by U.S. law enforcement agencies at an incredibly fast pace. As such, determining the effects of order-maintenance policing is still an important policy issue.

An obvious question for the current study of order-maintenance policing is whether the traffic-related disorder that is targeted by traffic summonses hinders and offends quality of life to the same degree that the more traditional "quality of life offenses" such as public drunkenness and public urination do. I assert that traffic-related disorder, especially in a city as traffic-congested as New York City, is equally if not more disruptive to quality of life. Double and illegal parking obstruct the flow of business traffic in commercial districts and, more dangerously, impede the ability of emergency responders to reach disaster sites quickly and efficiently. Reckless driving,

[11] For women, the treatment effect went in the opposite direction.

speeding, and red light-running disrupt community order by making it hazardous for pedestrians to cross streets and for children to play on city sidewalks. As an example, during 1997, the year of the slowdown, pedestrian fatalities were up 24% from the previous year– a reversal of a previous downward trend in the data. This is suggestive, though not conclusive, evidence that community disorder was severely compromised by the reduction in traffic-related order-maintenance policing.[12] In light of the highly disruptive and potentially hazardous nature of traffic-related disorder, traffic summonses are arguably a critical type of order-maintenance policing, especially within the context of a traffic-congested city like New York.

Through the use of a quasi-experimental research design, this paper contributes to the order-maintenance policing literature in two ways. First, this paper provides new evidence on the effect of order-maintenance policing on serious crime which, as is clear from the discussion above, has been the central focus of the academic debate thus far. Second, this paper addresses the first-order deterrence question of whether order-maintenance policing actually deters disorder– a question that, to my knowledge, has not yet been explored. If order-maintenance policing is to be advanced as an effective law enforcement policy, it must, at a minimum, achieve this basic objective.

3 Data

Several data sets are utilized for the analyses in this paper. These data can generally be divided into three groups: summons data, crime data, and manpower and population data. This section provides a brief description of each data set as well as its role in the analysis.

3.1 Summons Data

3.1.1 New York City Department of Finance Parking Summons Data

I have monthly parking summons data from the New York City Department of Finance for fiscal years 1989 to 2007 and weekly data for fiscal years 1996 to 2007. This data is available for all

[12] *The New York Daily News*, "Stepping Out on Deadly Streets: Pedestrian Fatalities Rose 24% Last Year," David Saltonstall, 2/8/1998. Available at http://www.nydailynews.com/archives/news/1998/02/08/1998-02-08_stepping_out_on_deadly_stree.html. Future work will attempt to determine the precise effect of the slowdown on traffic-related disorder when data become available.

parking summons-issuing agencies in New York City including the four largest issuers: the NYPD, the Traffic Department, the Parking Department and the Sanitation Department. This data is used to empirically verify that the 1997 drop in NYPD parking summonses was in fact due to the slowdown and not to other factors.

3.1.2 NYPD General Summons Data

I have citywide monthly summons data from 1995 to 1998 for the NYPD, broken down by type of summons. The three basic types of summonses are parking summonses, summonses for traffic infractions, and criminal summonses (also known as quality-of-life or c-summonses). Criminal summonses can be broken down into summonses for violations and summonses for misdemeanors. Misdemeanor criminal summonses are further sub-divided into summonses for traffic misdemeanors and summonses for other misdemeanors. This data is used to determine the differential impact of the slowdown on various types of summonses at the city-level.

3.1.3 NYPD Hazardous Traffic Summons Data

In addition to the data on parking, traffic, and criminal summonses, I also have data on a particular subset of traffic infraction and traffic misdemeanor summonses called hazardous traffic summonses. This monthly data is available from 1996 to 1998. Hazardous traffic violations are those traffic offenses that the NYPD has determined to be particularly hazardous to public safety; they are thus tracked separately by patrol borough over time.[13] The main advantage of this data for the current project is that it is available both at the city-level and the borough-level whereas the parking and general traffic summons data are only available at the city-level. This enables me to measure the level of slowdown participation in hazardous traffic summonses by borough. I then use this inter-borough variation to identify the effect of the slowdown on serious and non-serious crime at the borough-level.

[13] The 75 precincts of the NYPD are grouped into eight patrol boroughs. They are Manhattan South, Manhattan North, the Bronx, Brooklyn South, Brooklyn North, Queens South, Queens North and Staten Island.

3.2 NYPD Crime Complaints Data

Monthly crime complaint data are available at the precinct-level for the period January 1989 to September 2002. This amazingly rich data set would, in theory, allow an in-depth analysis of the crime-slowdown relationship at the precinct level. However, because the key slowdown treatment variable—hazardous summonses—is only available at the borough-level, I aggregate the crime complaint data up to the borough level as well for analysis. I have complaint data for each of the seven serious felony crimes: murder, rape, robbery, aggravated assault, burglary, larceny, and motor vehicle theft.[14] I also have data on total non-serious felony complaints[15], total misdemeanor complaints and total violation complaints. This data set is used to construct the main crime measures used in the study. For the purposes of this paper, I define "serious crime" to be the seven serious felony crimes collectively. I define "non-serious crime" (which I alternatively refer to as disorder) to be crime which falls into any one of the other crime categories: non-serious felonies, misdemeanors, or violations.

3.3 NYPD Manpower and City Population Data

Jens Ludwig was very generous in sharing his data on NYPD precinct manpower and New York City precinct-specific population.[16] Both of these variables are available on an annual basis for the period 1990 to 2000. I use the manpower data in the usual way to control for the effects of changes in patrol force size on both summons productivity and crime. I use the population figures to create per capita crime measures.

[14] These categories are similar to, but not exactly the equivalent of, the FBI's seven index crimes because these crimes are defined by New York state penal codes.

[15] The category of non-serious felonies includes the following crimes: Possession of Stolen Property, Theft-Fraud, Forgery, Arson, Prostitution and related Offenses, Sex Crimes (Sodomy, Sexual Abuse, etc.), Dangerous Drugs, Dangerous Weapons, Intoxicated/Impaired Driving, Child Abandonment, Criminal Mischief and related Offenses, Gambling, Abortion, Kidnapping and related Offenses, Other State Laws (ABC, Tax Laws, etc.), and Misc. Laws (Menacing, Reckless Endangerment, Coercion, Criminal Contempt, Criminal Trespass, Conspiracy, Perjury, Riot, Criminal Facilitation, Bribery, etc.).

[16] Actually, Ludwig shared the entire analysis data set from his 2006 paper with Bernard Harcourt. However, I only utilize the manpower and city population data in this analysis.

4 Documenting the Slowdown

4.1 Historical Documentation

This section gives background information on the contract negotiations and labor dispute that gave rise to the 1997 slowdown.[17]

After ending their previous round of bargaining with a disappointing wage contract, the Patrolmen's Benevolent Association (PBA), the union that represents New York City police officers, began the 1995 round of bargaining with a new tactic: they sought to change the bargaining system altogether by moving the venue of their contract disputes from the city's Office of Collective Bargaining (OCB) to the state's Public Employment Relations Board (PERB). Under the then-current system, if contract negotiations with the mayor reached an impasse, the arbitration process was overseen by the OCB, whose standard for arbitration decisions was that police pay raises be in parity with the raises given to other municipal workers. In comparison, the PERB panel's standard would require NYPD pay raises to be based on achieving pay parity with other police departments across the state, including those in the surrounding suburbs whose salaries were typically much higher. Under such a system of rules, the PBA believed that they would finally be able to gain pay increases that were higher than those typically available through pattern bargaining.[18]

Changing the venue of PBA arbitrations from OCB to PERB required a change of state labor law and the PBA lobbied the state legislature heavily to get such legislation passed. The stakes were high because then-Mayor Rudolph Giuliani had already been successful at pressuring the city's two largest unions to agree to a particularly unfavorable contract that required two years of wage freezes.[19] This contract set the pattern for the 1995 round of bargaining and would ultimately be the police officers' fate if their legislative battle failed.

On February 12, 1996, the PBA was successful in getting the arbitration bill signed and passed

[17] This section draws liberally from Chapter 2 ("Frustration, Anger and Retaliation: The Costs of Holdouts: Evidence from Ticket-Writing Slowdowns"). Please see that paper for a more detailed historical discussion of the NYPD labor disputes.

[18] *The New York Times*, "Court Voids Law Aiding the Police in Wage Disputes: Big Victory for Giuliani," Vivian S. Toy, 4/11/1996, p. A1. Article explains how PERB compares with OCB. Available at http://proquest.umi.com.

[19] DC37 (major civilian union) and the Teamsters. *Newsday*, "2nd Union Oks City Wage Pact," William Murphy, 2/14/1996. Available at http://infoweb.newsbank.com.

into law.[20] However, Mayor Giuliani challenged the law and after two lengthy court battles, the New York State Court of Appeals (the state's highest court) declared the law void in a decision issued on December 19, 1996.[21] The defeat was devastating to the PBA for two reasons: (1) their officers had been working without a contract for more than one year and (2) without the backing of the PERB panel, they would most likely be forced to accept the 2-year wage freeze contract pattern that most other municipal unions had already agreed to. With no other pressure tactics available to them, the police began a ticket-writing slowdown on December 21, 1996.[22] One month later, while the slowdown was still underway, the PBA formally rejected the Mayor's outstanding contract offer, declared an impasse, and petitioned the OCB to set up an arbitration panel.[23]

The scope of the 1997 slowdown was extensive with officers engaging in rallies and picketing. Officers who refused to participate in the job action were harassed at work.[24] By the end of March, police issuances were so low that target practices and guns training had to be suspended so that the supervisors leading those trainings could be deployed for summons writing.[25]

On September 4th, 1997, the PBA contract was announced via arbitration. The terms of the contract were exactly those rejected by the PBA in January and adopted by most other unions more than one year before.[26] Two weeks after the contract announcement, the slowdown began to ease. By the third week, the slowdown was officially over.

4.2 Empirical Documentation: Graphical Analysis

This section presents a graphical illustration of the slowdown's effect on NYPD parking summonses. Figure 2A graphs the 1997 slowdown period using weekly parking summons data for the NYPD

[20] *The New York Times*, "Mayor Sues Over State Law on Police Contract Disputes," David Stout, 3/1/1996, p. B5. Available at http://proquest.umi.com.

[21] *The New York Times*, "Law to Help Police Win Big Pay Raises Is Voided: Ruling Helps Giuliani's Effort to Limit Costs," James Dao, 12/20/1996, p. B3. Available at http://proquest.umi.com.

[22] *The New York Times*, "New York Acts on Slowdown on Ticketing," Robert D. McFadden, 1/28/1997, p. A1. Available at http://proquest.umi.com.

[23] *The New York Times*, "Police Reject Contract Offer by New York," David Firestone, 1/22/1997, p. B1. Available at http://proquest.umi.com.

[24] *Newsday*, "Blue with Rage: Secret War in the Station Houses Splits the Finest," Dan Morrison, 1/25/1997, p. A05. Available at http://infoweb.newsbank.com.

[25] *Newsday*, "Guns at Rest: Cop Brass Stalled Arms Training to Help in Slowdown," Dan Janison, 3/6/1997, p. A06. Available at http://infoweb.newsbank.com.

[26] *Newsday*, "PBA Delegates Call Emergency Meeting," Dan Morrison, 9/6/1997, p. A18. Available at http://infoweb.newsbank.com.

and zooming-in to the fiscal years right before and right after the labor action. By presenting the data at the weekly level, I am able to precisely illustrate the exact correspondence between the slowdown and the dates of the major events that prompted and ultimately ended the job action. The most salient precipitating event of the 1997 slowdown was unquestionably the New York State Court of Appeals' December 19th ruling that declared the arbitration law void. The court's ruling was issued on a Thursday and, as we can see from the graph, the slowdown started in the following week with a 37% drop off in weekly issuances. Weekly issuances stabilized for about 4 weeks thereafter. Then, on January 21st, Mayor Giuliani, reissued to the PBA his previous contract offer which called for a two-year wage freeze. During that same week, police weekly issuances fell off even further to 36,792, the lowest level reached during any point in the entire slowdown period. Weekly issuances stayed depressed for 31 more weeks. In the 32nd week, the police contract was announced via arbitration. By the 35th week, the slowdown was over.

Figure 2B graphs monthly parking summons issuances for the four largest parking summons issuers in the city of New York: the NYPD, the Traffic Department, the Parking Department and the Sanitation Department. Together these agencies issued over 95% of the city's parking tickets in 1997. The monthly time series tells the same story as the weekly time series: police parking summons issuances fell off sharply when the slowdown began and stayed low until September 1997 when the new contract was signed. Importantly, none of the other agencies exhibited similar drops in issuances during the slowdown. This fact reinforces the validity of my research assumption that the slowdown, and not some citywide factor, caused the drop in NYPD parking summons issuances.[27]

Having documented the correspondence between the historical time line of the labor action and the decline in police parking summonses, I now move on to the main analysis of the paper that uses slowdown-induced changes in traffic summonses to identify the effect of order-maintenance policing on crime.

[27] In my paper "Frustration, Anger, and Retaliation: The Costs of Holdouts: Evidence from the NYPD," I use the parking summonses data from the four city agencies formally in a difference-in-differences strategy to empirically verify that the slowdown, and not other factors, caused the 1997 drop in police parking summonses. The difference-in-differences analysis implies that the slowdown caused a 51% reduction in police parking summonses for the nine months of the slowdown. Please see this companion paper for the complete difference-in-differences analysis.

5 The Effect of Order-Maintenance Policing on Serious and Non-Serious Crime

5.1 Sample Selection

As is clear from the discussion of the data in Section 3, I have various sources of data that cover multiple time periods. Though the crime data runs from January 1989 to September 2002, the crime analysis sample runs from September 1996 to December 2000. Three factors dictated the choice of this time frame. I outline them below.

First, the onset of Mayor Giuliani's order-maintenance policing strategy drastically changed the composition of law enforcement activities in 1994. As Figure 1 illustrates, arrests for misdemeanors and summonses for violations increased so drastically that the pre-1994 data is not comparable. Since the goal of the analysis is to evaluate how *slowdown-driven* changes in summonses, and not *Giuliani-driven* changes in summonses, affected crime, I limit the analysis to the post-1994 era.

Second, the manpower data ends in 2000. Because I need to be able to control for precinct and borough size in the analysis, I end the analysis sample in 2000.

The above two constraints lead us to a *de facto* sample of 1994 to 2000. The last remaining factor that affects the sample selection process is an anomaly in the data that begins in April 1995 and ends sharply in August 1996. Specifically, crime complaints for non-serious felonies, violations and misdemeanors had a temporary but dramatic break from trend.[28] As Figure 3 illustrates, citywide complaints for misdemeanors and non-serious felonies increased and complaints for violations decreased beginning in April 1995. In August 1995, the change that began in April 1995 accelerated. Interestingly, this change in the citywide data ends sharply in August 1996.

My best guess for what caused this anomaly in the crime data is the 1995 merger of the NYPD with the NYC transit police in April 1995 and with the NYC housing police in May 1995.[29,30]

[28] This anomaly also affects some of the categories of summonses, again with a sharp change in April 1995. See Appendix Table A1 for an analysis of how the anomaly affected all the categories of crime and summonses.

[29] "Housing Police Folded into Citywide Force," New York Times, May 1, 1995 and http://en.wikipedia.org/wiki/New_York_City_Transit_Police.

[30] Note that the reason for the sharp acceleration in the anomaly in August 1995 is that there is some variation in the timing of the anomaly by borough. Some boroughs broke from trend in April 1995 at exactly the time of the merger, with a small acceleration in May 1995 during the second merger. By August 1995, all boroughs had broken

However, it is currently unclear why the merger would have generated these particular data patterns. One possible explanation is that a temporary or erroneous re-categorization of the crime categories during this transitional period in the NYPD caused the data anomaly for this period, though my efforts to confirm this with the NYPD have not been successful.[31] Still, re-categorization may be a likely explanation of the data anomaly for two reasons. First, the increase in misdemeanor and non-serious felony complaints somewhat offsets the violation complaints decline, though not completely. Appendix Table A1 gives the estimates of the effect of the anomaly on the four categories of crime individually and on total crime. As the table indicates, even though violations fell by an average of 71 crimes per 100,000 people, per month, misdemeanors and non-serious felonies increased by an average of 33 and 27 crimes per 100,000 people, per month, respectively. Thus, the total shift in the sum of these crimes was a decline of about 10, a statistically insignificant change. Second, the data anomaly ends sharply in August 1996, as abruptly as it began, which may be more indicative of a return to a prior method of coding rather than a change in actual crime patterns.

Whatever the cause of the anomaly, the relevant factor for the present paper is that it seems to have stopped abruptly in August 1996. As such, I begin my crime sample in September 1996 and end it in December 2000. For the summons analyses (section 5.2), where my samples are much smaller (either January 1996 to December 1998 for the hazardous summonses or January 1995 to December 1998 for all other summonses), I use all of the anomaly period data but include a dummy variable to control for its effect.[32] Thus, in all of the subsequent analyses, I have four months of non-anomaly pre-slowdown data, September 1996 to December 1996.

from trend, and in August 1996 all boroughs simultaneously returned to trend in a remarkable fashion. Figure 3 displays the aggregate citywide effect of the anomaly. However, borough-level figures are available from the author upon request.

[31] According to the NYPD's OMAP, no documentation of how the merger affected the cataloging of the department's official criminal statistics exists. (Conversation with Lt. Scarazinni 9/08).

[32] For the non-hazardous summons regression sample, I start the sample in June 1995 (right after the start of the second merger) instead of in January 1995. For the hazardous summons regression sample, I start the sample in January 1996. This allows all regressions to have the same pre-slowdown period.

5.2 Effect of the Slowdown on Parking Summonses, General Traffic Summonses, Hazardous Traffic Summonses, and Criminal Summonses

The labor action had a significant impact on police summons-writing productivity. In this section, I estimate the size of this impact, both in the aggregate and by type of summons. To provide context, Table 1 presents descriptive statistics for the different types of summonses written, city-wide, from January 1995 to December 1998. From Panel A, column (2), we see that parking summonses represent the majority of summonses written (71%). Summonses for traffic infractions and violations represent 22% and 5% of all summonses written, respectively. Summonses for traffic misdemeanors and other misdemeanors represent just 1% of total summonses each. Because hazardous traffic summonses are a subset of all traffic infraction and traffic misdemeanor summonses, I present them separately in Panel B of Table 1. Hazardous traffic summonses represent about 30% of all traffic summonses. They are also the most expensive of all traffic summonses, with fines ranging from 150% to 300% of the cost of an average traffic fine.[33] (Appendix Table A2 lists all hazardous summonses along with their relative share in total hazardous summonses.)

To give a sense of how ticket productivity changed over the sample period, Columns (3) through (7) show the average number of summonses written per officer, by type, for each year of the sample period. All traffic-related summonses (parking, traffic, and hazardous traffic) show a sharp decline in 1997, the year of the slowdown.[34] Though this is suggestive of a negative impact of the labor action on ticket productivity, equations 1 and 2 test for this formally in a regression setting by controlling for the effects of trends and seasonal variation. The regression specifications are given below.

[33] Traffic fine schedule retrieved from the New York State Traffic Violation's Bureau Call Center, recorded message, 11/12/08. Available at http://www.nysdmv.com/dmvfaqs.htm#tickets.

[34] Because the slowdown started in January 1997 and lasted for nine months, the twelve-month average figures given in column (6) provide a good proxy for the average ticket productivity during the slowdown period itself.

$$\ln(Summons_{m,t}) = \beta_0 + \beta_m^S \; slowdown_t + \beta_m^A \; after \; slowdown_t + \beta_m^{AN} \; anomaly_t \quad (1)$$
$$+ \delta_m + \gamma_m \; time_t + \rho \; month_t + \epsilon_{m,t}$$

$$\ln(Hazardous \; Summons_t) = \alpha_0 + \alpha^S \; slowdown_t + \alpha^A \; after \; slowdown_t + \alpha^M \; anomaly_t \quad (2)$$
$$+ \gamma \; time_t + \rho \; month_t + \epsilon_t$$

Equation 1 is estimated using a panel data set of all types of summonses except hazardous traffic summonses. In equation 1, m indexes summons type (parking, traffic infraction, traffic misdemeanor, other misdemeanor, and violation) and t indexes time where the monthly sample runs from June 1995 to December 1998. $Summons_{m,t}$ is thus the number of type m summonses issued citywide by the NYPD patrol force in month t. *Slowdown* is a dummy variable equal to 1 during the slowdown period (defined as January 1997 to September 1997) and 0 otherwise. *After slowdown* is a dummy variable equal to 1 during the period after the slowdown until the end of the sample and 0 otherwise. *Anomaly* is a dummy variable equal to 1 for the period June 1995 to August 1996 and 0 otherwise. $\beta_m^S * 100$ are the summons-specific slowdown effects that measure the average percent change in type m summonses during the slowdown period, relative to the four month pre-period. δ_m and γ_m are the summons-type fixed effects and linear time trends, respectively. ρ are the coefficients on the seasonal dummies. Equation 2, which is estimated for hazardous summonses only, also includes a linear time trend (γ) and seasonal dummies (ρ). In equation 2, α^S*100 is the slowdown effect for hazardous summonses. The sample for equation 2 is a monthly sample that runs from January 1996 to December 1998. The estimation of equations 1 and 2 (as well as all other regression equations in this paper) proceeds via ordinary least squares with Newey-West heteroskedasticity and auto-correlation consistent (HAC) standard errors. I use the default maximum lag length of $\frac{3}{4}\left[T_i^{\frac{1}{3}}\right]$.

Table 2 has the results from these regressions. During the slowdown, total summons-writing for

the patrol force dropped off by about 46%.[35] However, the size of this effect varied by summons type. As we can see from Table 2, hazardous summonses fell the most during the slowdown (75%), perhaps a consequence of their being the most expensive of all traffic summonses. Parking summonses fell by 51%, summonses for traffic infractions fell by 42% and summonses for traffic misdemeanors, other misdemeanors and violations fell by 37, 20, and 19 percent, respectively, though the estimate for violation summonses is not statistically significant.

The main analysis (Section 5.3) uses hazardous traffic summonses as the measure of traffic-related order-maintenance policing, primarily because it is the only traffic-related summons data that is available at the borough-level. However, the fact that hazardous summonses declined by the largest percentage during the slowdown and the fact that hazardous traffic offenses are the most egregious and dangerous of all traffic-related offenses suggests that the study is capturing the effect of a particularly relevant type of order-maintenance law enforcement. In the next sections, I present the analysis of the effect of hazardous summonses, as instrumented by the slowdown, on crime.

5.3 Effect of the Slowdown on Crime

5.3.1 First Stage

To begin, I present the results of the first stage: the effect of the slowdown on hazardous summonses. For the first stage, I use a shortened sample of the hazardous summonses data that drops the anomaly period; thus, my sample period is now 9/1996–12/1998 rather than 1/1996–12/1998. I make this sample adjustment so that the data on my treatment variable (hazardous summonses) and on my outcome variable (crime) can be analyzed for the same time period in a traditional instrumental variables analysis. Unlike the city-level regressions from the previous section, the first-stage regression is run at the borough-level. As a result, I can now include a general set of time dummies in my analysis (year X quarter fixed effects) to control for any non-slowdown related, citywide changes in summons-writing that could be occurring during the slowdown period and thereby biasing the first stage results. To be sure, including this rather exhaustive set of

[35] Note that the effect of the slowdown on total summonses (46%) is not given in Table 2.

time dummies also purges my analysis of the valid, slowdown-related time variation in summons-writing. However, the advantage to this more rigorous identification strategy is that my estimates are robust to any indiosyncratic citywide fluctuations in summonses that affect all boroughs. My instruments, then, are a set of interaction terms between a dummy variable for the slowdown time period and seven distinct patrol borough indicators.[36] Thus, my instrumental variables estimate of the effect of hazardous summonses on crime is ultimately identified off of the relative rates of slowdown participation in the patrol boroughs, rather than off of the time variation in hazardous summonses during the slowdown period. My base specification for the first stage effect is below in equation 3. The dependent variable is the number of hazardous summonses given out in patrol borough i in month t, in logs. The α_i^S are the seven borough-specific slowdown effects. The λ_t are year X quarter fixed effects. $Manpower_{i,t}$ is the number of patrol officers in borough i in month t.[37] The δ_i are the borough fixed effects, the π_i are the borough-specific linear time trends, and the ψ_i are the borough-specific quadratic time trends. In an alternative specification, I exclude the quadratic time trends.

$$\begin{aligned} \ln(Hazardous\ Summons_{i,t}) &= \alpha_0 + \alpha_i^S\ slowdown_t + \lambda_t \\ &\quad +\gamma\ manpower_{i,t} + \delta_i + \pi_i\ time_t + \psi_i\ time^2 + \epsilon_{i,t} \end{aligned} \tag{3}$$

In order to get a sense of how the inclusion of the year X quarter general time dummies affects the results, I also present the version of equation 3 where the borough-slowdown interaction terms are excluded and the year X quarter time dummies are replaced by two time dummies: one for the slowdown period ($slowdown_t$) and one for the period after the slowdown ($after\ slowdown_t$). Using time variation only, this equation (equation 4) measures the average borough-level change in hazardous summonses during the slowdown period.[38]

[36] There are eight boroughs in total but one of the interactions must be dropped to avoid perfect collinearity.
[37] Note that the manpower data is only available annually, not monthly.
[38] This is the borough-level, panel data version of equation 2.

$$\ln(Hazardous\ Summons_{i,t}) = \alpha_0 + \alpha^S\ slowdown_t + \alpha^A\ after\ slowdown_t + \rho\ month_t \quad (4)$$
$$+\gamma\ manpower_{i,t} + \delta_i + \pi_i\ time_t + \psi_i\ time_t^2 + \epsilon_{i,t}$$

Table 3 has the results for equations 3 and 4. Columns 1 and 2 of table 3 give the results from equation 4 when borough-specific linear trends and borough-specific linear plus quadratic trends are used, respectively. As we can see, during the slowdown period, average borough-level hazardous summonses productivity fell by about 60%. Columns 3 and 4 present the regression results from equation 3, the first stage regressions where the instruments are the set of interactions between the slowdown dummy variable and the patrol borough dummy variables; column 3 includes linear controls only while column 4 includes both linear and quadratic trends. Taken together with the results from columns 1 and 2, these results show that while there was a large and statistically significant drop in average, borough-level hazardous summons writing during the slowdown period, there was not a great deal of variation *between* borough in terms of the relative strength of the participation. Roughly speaking, the boroughs can be split into three groups with regards to slowdown participation: the low-level participator (Queens North), the mid-level participators (Queens South and Staten Island) and the high-level participators (all the rest). T-tests between pairs of coefficients confirm that the participation in Queens North is statistically distinguishable from almost all of the other boroughs (with no p values larger than 0.10). In addition, t-tests were conducted to compare the average slowdown response across the three aforementioned participation levels. The null of equivalence of average participation levels across the three groups is rejected at the 7% significance level for all three tests (high versus low, medium versus low, and high versus medium). This means my results should be interpreted with an awareness of the fact that even though seven boroughs are available for identification purposes (already a small number), there are really only three distinct levels of slowdown participation. All in all, there is some but not a tremendous amount of identifying variation present in the first stage.

One additional factor to consider in evaluating the first-stage results is the effect on the results

of including the borough-specific quadratic trends. In comparing columns 3 and 4, it becomes clear that adding in the quadratic trends decreases the slowdown effect sizes in absolute value. (This occurs both because the coefficients on the slowdown (year X quarter) time dummies become less negative and because the coefficients on the borough-specific slowdown coefficients become more positive.) While the quadratic trends are jointly significant (p=0.01), their inclusion may be leading to an overfitting of the data and thus an underestimation of the slowdown coefficient sizes. In figures 4 through 11, I graph the raw hazardous summonses data for each of the eight boroughs. As is clear from the figures, the hazardous summonses data is quite sparse; for each borough there are only 35 data points available, and after dropping the anomaly period, there are only 26 data points per borough. After taking out the variation due to each borough's linear time trend, the predominant source of remaining variation is the U-shaped slowdown variation. When the quadratic trends are added in, some of this slowdown variation gets soaked up by the U-shaped functional form of the quadratic trends, so the individual borough coefficients are smaller in absolute value. Since my identification comes from the relative ordering of the boroughs in terms of their slowdown participation, the deflation of the coefficient sizes is not, in and of itself, a major concern. And, in fact, the relative ordering of the boroughs stays exactly the same with one exception: Brooklyn North goes from being the second highest participator (linear trend specification) to the second lowest participator (quadratic trend specification)–a shift from the highest participation group to the mid-level participation group. Given that only seven boroughs (or three groups of boroughs) are available for identification, small adjustments to this ordering are important. Thus, in the reduced form and instrumental variables analyses that follow, I present all results both with and without quadratic trends in order to determine the effect, if any, on the results of this small shift in the relative ordering of slowdown participation.

5.3.2 Reduced Form Effect

I now present my reduced form results: the effect of the slowdown on crime. As with the first stage analysis above, I begin by contrasting the average borough-level response of crime to the slowdown with the borough-specific responses of crime to the slowdown relative to the average. Equation 5

includes the interactions between the slowdown dummy variable and the patrol borough dummies as well as the rich set of year X quarter time dummies. As with the first-stage, I estimate equation 5 both with and without quadratic trends. Equation 6 includes instead just the two time dummies: one for the slowdown period and one for the period after the slowdown. The coefficient on the slowdown dummy tells us the average, borough-level percent increase in crime during the slowdown period.

$$\ln(Crime_{i,t}) = \beta_0 + \beta_i^S \; slowdown_t + \lambda_t + \gamma \; manpower_{i,t} + \delta_i + \pi_i \; time_t + \psi_i \; time_t^2 + \epsilon_{i,t} \tag{5}$$

$$\ln(Crime_{i,t}) = \beta_0 + \beta^S \; slowdown_t + \beta^A \; after \; slowdown_t + \rho \; month_t + \gamma \; manpower_{i,t} + \delta_i + \pi_i \; time_t + \psi_i \; time_t^2 + \epsilon_{i,t} \tag{6}$$

As before, i indexes the eight patrol boroughs and t indexes time where the post-anomaly, monthly sample runs from September 1996 to December 2000.[39] The dependent variable is the number of crime complaints per 100,000 people, per borough, per month, in logs. The crime categories analyzed with this regression specification are total serious felonies, total non-serious felonies, total misdemeanors and total violations. All covariates are as before.

Table 4 presents the results from specification 6. I find that during the nine months of the slowdown period, all categories of crime increased but to different degrees. Specifically, serious felonies increased by 7%, non-serious felonies increased by 17%, misdemeanors increased by 10% and violations increased by 9%. Thus, both the level of serious crime (as proxied by serious felony complaints) and disorder (as proxied by non-serious felony, misdemeanor, and violation complaints) were elevated citywide during the drop in summons law enforcement.

Table 5 presents the borough-specific crime slowdown effects from equation 5; columns 1 through 4 have linear trends only while columns 5 through 8 additionally have quadratic trends. Unlike

[39] Note that in the subsequent IV analysis (section 5.3.3), one of the samples used runs from 9/1996 to 12/1998 so that it is exactly the same length as the shortened hazardous summonses sample.

the first stage results, the reduced form results are far less sensitive to the exclusion of quadratic trends, though this is at least in part due to the fact that the crime sample includes two additional years of monthly data (1999 and 2000). From this table we see that, regardless of the modelling choice, all boroughs experienced roughly similar increases in complaints for misdemeanors and violations during the slowdown period. However, for serious and non-serious felonies, the increases in crime complaints in Staten Island far outpaced the increases in the other boroughs by at least 6 percentage points. For example, in the model with quadratic trends, Staten Island experienced an 11% relative increase in serious felony complaints while the other boroughs experienced relative increases that ranged from -1% to 5%. Similarly, for non-serious felonies, Staten Island's relative increase was 15% while the other boroughs had relative increases that ranged from -6% to 3%.

The dramatic relative increases in Staten Island's serious and non-serious felonies during the slowdown period are somewhat surprising given that it is the smallest of the eight patrol boroughs along all important criminal justice dimensions. As I show in Appendix Table A3, the other seven patrol boroughs include at least 8 precincts each. These boroughs cover an average of 1,076,185 people with 2,337 officers. Moreover, on average, in these boroughs, 260 serious felonies and 60 non-serious felonies are reported each month per 100,000 people per borough. Staten Island, by comparison, has only 3 precincts. As a patrol borough, Staten Island has only 424,682 people and 641 officers. In addition, the serious and non-serious felony complaint rates in Staten Island are much lower than the average in the other boroughs; only 117 serious felonies and 46 non-serious felonies are reported in Staten Island each month per 100,000 people. Given the large degree to which Staten Island's felony increases during the slowdown deviated from the average borough felony increases during the slowdown, it is possible that Staten Island's increases were partly endogenous to other factors that might have been in play in Staten Island at the time, though the current analysis cannot yet confirm this with any external evidence.[40]

[40] I do note, however, that this is not the first borough-level study of the NYPD to find a large deviation between the results for Staten Island and the results for the other patrol boroughs. In his 2006 RAND study of the NYPD's Stop and Frisk practices, Greg Ridgeway points out that while the other patrol boroughs showed similarly small levels of racial discrimination along several dimensions of post-stop outcomes (ie- frisk, search, arrest, use of force), Staten Island stood apart from the other boroughs in that it had consistently large and statistically significant levels of racial discrimination in post-stop outcomes. To be clear, however, the RAND study suggests a difference between Staten Island and the other boroughs in terms of law enforcement practices only whereas my analysis is finds a difference in terms of both law enforcement practices (extremely low slowdown participation) and crime rates (extremely high

On the whole, the above results show that during the slowdown period, crime increased citywide for all categories, with very little distinction between the boroughs in terms of differential crime responses. The one exception to this is Staten Island where the serious felony and non-serious felony crime increases were at least 6 percentage points higher than in any other borough. In summary, thus far my results reveal only a moderate amount of inter-borough variation in the first stage and even less in the reduced form. Together, these results foreshadow a zero-result finding for the instrumental variables analysis. I present the formal instrumental variables estimates below.

5.3.3 Instrumental Variables Estimates

As is mentioned in the discussion of the first stage results, the data on my treatment variable (hazardous summonses) and my outcome variable (crime) are not available for the same time period. Since a traditional, one-sample IV analysis requires that the data on both the treatment and outcome variable be available for the same sample period, I take a two pronged approach to my IV analysis. First, I do a traditional, one-sample IV analysis for the shortened time period for which both variables are available (9/1996 – 12/1998). Second, I do a two-sample IV analysis (Angrist and Krueger, 1992) where I use the short sample (9/1996 – 12/1998) in the first stage hazardous summonses regression and then I use the long sample (9/1996 – 12/2000) in the second stage crime regression. This allows me to exploit all of the variation in my crime data which ultimately aids in increasing the precision of my IV estimates. Operationally, the two-sample IV procedure is carried out by using the first stage coefficients from the borough-slowdown interaction terms to generate a new regressor that is equal to zero in the non-slowdown months and equal to each borough's predicted relative slowdown participation rate in the slowdown months. This generated regressor is then used in the second stage and its coefficient is the two-sample IV estimate of the effect of hazardous summonses on crime. The standard errors on this coefficient are corrected to account for the fact that the generated regressor is measured with sampling error using the Murphy-Topel correction (Murphy and Topel, 1985). The equations for both the one-sample and two-sample IV procedures are given below.

felony complaints).

The standard **one-sample IV analysis** is implemented using the system of equations given in equations 7 and 8.[41] All variables are as defined above. Note that some specifications use linear trends only while others use both linear and quadratic trends.

$$\begin{aligned} \ln(Crime_{i,t}) &= \beta_0 + \beta^{IV}\ \ln(Hazardous\ Summons_{i,t}) + \lambda_t \\ &\quad +\gamma\ manpower_{i,t} + \delta_i + \pi_i\ time_t + \psi_i\ time_t^2 + \epsilon_{i,t} \end{aligned} \tag{7}$$

$$\begin{aligned} \ln(Hazardous\ Summons_{i,t}) &= \alpha_0 + \alpha_i^S\ slowdown_t + \lambda_t \\ &\quad +\gamma\ manpower_{i,t} + \delta_i + \pi_i\ time_t + \psi_i\ time_t^2 + \epsilon_{i,t} \end{aligned} \tag{8}$$

The **two-sample IV analysis** is run using the following equation:

$$\ln(Crime_{i,t}) = \beta_0 + \beta_i^S\ HS_{i,t} + \lambda_t + \gamma\ manpower_{i,t} + \delta_i + \pi_i\ time_t + \psi_i\ time_t^2 + \epsilon_{i,t} \tag{9}$$

$$\text{where } HS_{i,t} = \begin{cases} 0 & \text{for non-slowdown months} \\ \alpha_i^S & \text{for slowdown months, (from eqn 7)} \end{cases}$$

Results from the IV analysis are presented in tables 6 and 7; table 6 has the results where only borough-specific linear trends are included and table 7 has the results where both linear and quadratic borough-specific controls are included. From these tables we can see that none of the categories of crime show any statistically significant relationship with hazardous summonses.

Graphs of the first stage coefficients against the reduced-form coefficients illuminate the zero-result finding. Note that in the graphs the negative of the first stage coefficients are used so that more positive numbers indicate stronger slowdown participation. Figures 12 and 13 give the graphs of the serious felony and non-serious felony crime coefficients when linear controls only are used

[41] Note that equation 8 is just the first stage regression given previously as equation 3 in section 5.3.1.

and figures 16 and 17 give the graphs when both linear and quadratic controls are used. As we can see, regardless of the model specification, the crime response in Staten Island during the slowdown period is an outlier when compared to the crime responses in the other boroughs. This accounts for some of the zero-result finding. However, even when the Staten Island data is omitted from the IV regressions, I still find no statistically significant linear relationship between relative slowdown strength and relative crime response. Figures 14 and 15 give the graphs of the misdemeanor and violation coefficients when linear trends only are used; figures 18 and 19 give the graphs when quadratic controls are added. In these graphs Staten Island is no longer an outlier. Despite this, there is still no clear linear relationship between the data points.

6 Conclusion

This paper presents quasi-experimental evidence on the effect of order-maintenance policing on both serious and non-serious crime (or disorder). In 1997, the officers of the NYPD engaged in a nine-month ticket-writing slowdown that caused their ticket-writing productivity to drop by 51% for parking summonses and 42% for moving violation summonses. The drop off in hazardous traffic summonses, the summonses given out for the most dangerous of all traffic offenses, was even more severe (75%). By exploiting this slowdown-induced variation in ticket-writing behavior, I attempt to identify the effect of traffic-related order-maintenance policing on both serious crime (defined as serious felonies) and disorder (defined as non-serious felonies, misdemeanors, and violations). I find that while both serious crime and disorder were elevated during the slowdown period, neither of these increases seems to be related to the decline in traffic enforcement. This zero-result is consistent with the existing broken-windows literature which has consistently found no effect of order-maintenance policing on serious crime. And, as the first paper to investigate the effect of order-maintenance policing on disorder, my zero-result finding draws into question the current law enforcement trend towards wider adoption of order-maintenance policing strategies. However, these results are presented with the caveat that only a moderate amount of inter-borough variation exists in terms of slowdown participation; at best, there are three distinguishable levels of slowdown

participation amongst the eight boroughs. All in all, the current evidence suggests that order-maintenance policing does not have an effect on either serious crime or disorder.

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Table 1. Average Monthly Summonses Productivity, NYPD Patrol Force
Standard Deviation in Brackets

Panel A: General Summonses[1]

	(1) Total	(2) Percent	(3) Per Officer (All Years)	(4) Per Officer (1995)	(5) Per Officer (1996)	(6) Per Officer (1997)	(7) Per Officer (1998)
Parking and Traffic Summonses							
Parking	238,878.20	71	13.93	15.38	16.55	10.71	12.71
	[44,659.96]		[2.60]	[1.50]	[0.94]	[1.49]	[0.99]
Traffic Infractions	74,638.80	22	4.35	4.89	5.11	3.43	3.86
	[15,398.55]		[0.89]	[0.68]	[0.55]	[0.38]	[0.60]
Criminal Summonses							
Traffic Misdemeanors	3,484.50	1	0.20	0.22	0.24	0.17	0.17
	[731.18]		[0.04]	(0.02]	[0.03]	[0.02]	[0.03]
Other Misdemeanors	1,980.74	1	0.12	0.08	0.11	0.12	0.16
	[565.98]		[0.03]	[0.02]	[0.02]	[0.02]	[0.02]
Violations	17,579.35	5	1.03	0.72	0.94	1.14	1.35
	[5,509.61]		[0.33]	[0.21]	[0.19]	[0.36]	[0.14]
Total Summonses	336,561.60	100	19.63	21.29	22.94	15.58	18.25
	[57,210.82]		[3.34]	[1.29]	[1.35]	[1.96]	[1.53]
N	46		46	12	12	11	11

Panel B: Hazardous Traffic Summonses[2]

Hazardous Traffic Summonses	22,225.77		1.31		1.84	0.89	1.17
	[7,386.89]		[0.44]		[0.23]	[0.13]	[0.18]
Share of General Traffic Summonses	.28						
N	35		35		12	11	12

Notes:
1. The sample period for all non-hazardous summons data is January 1995 to December 1998, though data are missing for December 1997 and January 1998.
2. The sample period for the hazardous summons data is January 1996 to December 1998, though data is missing for December 1997.

Table 2. Effect of the Slowdown on Different Types of Summonses

Type of Summons	Dependent Variable is Ln(Summonses)	Dependent Variable is Ln(Summonses Per Officer)
Panel A: General Summonses (results from Equation (1))		
Parking	-0.51***	-0.57***
	(0.06)	(0.06)
Traffic Infractions	-0.42***	-0.48***
	(0.06)	(0.06)
Criminal		
Traffic Misdemeanors	-0.37***	-0.42***
	(0.08)	(0.08)
Other Misdemeanors	-0.20**	-0.26***
	(0.07)	(0.07)
Violations	-0.19	-0.24
	(0.21)	(0.21)
Panel B: Hazardous Summonses (results from Equation (2))		
Hazardous Traffic	-0.75***	-0.80***
	(0.08)	(0.08)

Notes:
1. The sample for the results presented in panel 1 includes 41 observations per summons type, for a total of 205 observations. For each summons type, the data are monthly for the period June 1995 to December 1998, though data are missing for December 1997 and January 1998.
2. The sample for the results presented in panel 2 is a sample of hazardous traffic summonses only. The data are monthly for the period January 1996 to December 1998, though data are missing for December 1997. The sample size is 35.
3. The table presents OLS regression coefficients with Newey-West HAC standard errors. The default maximum lag length is used.
4.*** denotes statistical significance at the .1% level. ** denotes statistical significance at the 1% level.

Table 3. First Stage: Dependent Variable is Ln(Hazardous Summonses)

	(1)	(2)	(3)	(4)
Slowdown	-0.6048***	-0.6394***		
	(0.0738)	(0.0789)		
After Slowdown	-0.2759**	-0.3494**		
	(0.1038)	(0.1082)		
Slowdown X Borough 2 (MN)			0.1194	0.2009*
			(0.0680)	(0.0830)
Slowdown X Borough 3 (BX)			0.1263	0.1334
			(0.0946)	(0.0832)
Slowdown X Borough 4 (BS)			-0.0216	0.0496
			(0.0857)	(0.0862)
Slowdown X Borough 5 (BN)			0.1192	0.3130**
			(0.1260)	(0.0989)
Slowdown X Borough 6 (QS)			0.2875*	0.2750*
			(0.1432)	(0.1374)
Slowdown X Borough 7 (QN)			0.4727***	0.5254***
			(0.0912)	(0.0871)
Slowdown X Borough 8 (SI)			0.2534**	0.2880*
			(0.0820)	(0.1228)
Average of Quarter X Year Fixed Effects During Slowdown Period			-0.4827***	-0.3306**
			(0.1293)	(0.1425)
N	216	216	216	216
Constant	9.8259***	5.9229	14.0900**	21.0331
	(2.6520)	(7.9613)	(4.3035)	(11.3559)
R2	0.8891	0.8997	0.9168	0.9257
Borough-Specific Linear Trends	Y	Y	Y	Y
Borough-Specific Quadratic Trends		Y		Y
Quarter X Year Fixed Effects			Y	Y

Notes:

1.The sample used for these results includes borough-level monthly data. The sample period for the results is September 1996 to December 1998 but data for December 1997 are missing. All eight boroughs are included in each regression. The sample size for each column is thus 216.

2. All specifications include manpower and borough fixed effects.

3. The table presents OLS regression coefficients with Newey-West HAC standard errors. The default maximum lag length is used.

4.*** denotes statistical significance at the .1% level. ** denotes statistical significance at the 1% level.

* denotes statistical significance at the 5% level.

Table 4. Reduced Form (from Equation 6):
Dependent Variable is Ln(Crime Complaints Per 100,000 People)

	(1) Serious Felonies	(2) Non-Serious Felonies	(3) Misdemeanors	(4) Violations
Slowdown	0.0692***	0.1704***	0.1004***	0.0887***
	(0.0190)	(0.0346)	(0.0176)	(0.0146)
N	408	408	408	408
Constant	8.1390***	6.3284***	12.7759***	10.3517***
	(0.8561)	(1.7313)	(0.7330)	(0.8326)
R2	0.9851	0.9567	0.9871	0.9663

Notes:
1.The sample used for these results includes borough-level, monthly data. The sample period for the results in September 1996 to December 2000 but data for December 1997 are missing. The sample size for each column is thus 408.
2. All specifications include manpower, borough fixed effects, borough-specific linear trends, borough-specific quadratic trends, month fixed effects.
3. The table presents OLS regression coefficients with Newey-West HAC standard errors. The default maximum lag length is used.
4.*** denotes statistical significance at the .1% level. ** denotes statistical significance at the 1% level.
* denotes statistical significance at the 5% level.

Table 5. Reduced Form (from Equation 5):
Dependent Variable is Ln(Crime Complaints Per 100,000 People)

	(1) Serious Felonies	(2) Non-Serious Felonies	(3) Misdemeanors	(4) Violations	(5) Serious Felonies	(6) Non-Serious Felonies	(7) Misdemeanors	(8) Violations
Slowdown X Boro 2 (MN)	0.0262	0.0265	0.0247	-0.0574	0.0257	0.0245	0.0346	-0.0354
	(0.0272)	(0.0513)	(0.0297)	(0.0509)	(0.0230)	(0.0434)	(0.0262)	(0.0282)
Slowdown X Boro 3 (BX)	0.0391	0.0043	0.0086	-0.0771	0.0254	-0.0330	0.0289	-0.0266
	(0.0229)	(0.0553)	(0.0247)	(0.0475)	(0.0210)	(0.0460)	(0.0204)	(0.0269)
Slowdown X Boro 4 (BS)	0.0020	0.0103	-0.0509	-0.0642	0.0075	0.0074	-0.0280	-0.0139
	(0.0226)	(0.0571)	(0.0266)	(0.0514)	(0.0157)	(0.0496)	(0.0234)	(0.0301)
Slowdown X Boro 5 (BN)	0.0485	0.0693	-0.0147	-0.0517	0.0505*	0.0329	0.0292	0.0048
	(0.0284)	(0.0510)	(0.0344)	(0.0495)	(0.0246)	(0.0440)	(0.0248)	(0.0307)
Slowdown X Boro 6 (QS)	0.0239	0.0572	-0.0036	-0.0831	0.0201	0.0116	0.0292	-0.0343
	(0.0255)	(0.0631)	(0.0285)	(0.0538)	(0.0215)	(0.0548)	(0.0267)	(0.0345)
Slowdown X Boro 7 (QN)	-0.0090	-0.0458	-0.0480	-0.0465	-0.0082	-0.0561	-0.0267	-0.0016
	(0.0224)	(0.0506)	(0.0284)	(0.0513)	(0.0182)	(0.0473)	(0.0268)	(0.0361)
Slowdown X Boro 8 (SI)	0.1291***	0.1531**	-0.0290	-0.0558	0.1062***	0.1479**	-0.0214	-0.0261
	(0.0342)	(0.0577)	(0.0325)	(0.0506)	(0.0247)	(0.0543)	(0.0269)	(0.0289)
Average of Quarter X Year Fixed Effects During Slowdown Period	0.0087	0.3097***	0.0429	0.0206	0.0076	0.2921**	0.0925	-0.0362
	(0.0415)	(0.0717)	(0.0467)	(0.0667)	(0.0501)	(0.0979)	(0.0605)	(0.0748)
N	408	408	408	408	408	408	408	408
Constant	7.8607***	8.5039***	8.1873***	4.3667***	7.2444***	5.4104	12.8125***	7.3718**
	(0.5745)	(0.8630)	(0.5621)	(0.6546)	(2.0072)	(3.1260)	(2.0330)	(2.7154)
R2	0.9793	0.9549	0.9758	0.9291	0.9803	0.9566	0.9774	0.9344
Borough-Specific Linear Trends	Y	Y	Y	Y	Y	Y	Y	Y
Borough-Specific Quadratic Trends					Y	Y	Y	Y
Quarter X Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y

Notes:
1.The sample used for these results includes borough-level, monthly data. The sample period for the results in September 1996 to December 2000 but data for December 1997 are missing. The sample size for each column is thus 408.
2. All specifications include manpower and borough fixed effects.
3. The table presents OLS regression coefficients with Newey-West HAC standard errors. The default maximum lag length is used.
4.*** denotes statistical significance at the .1% level. ** denotes statistical significance at the 1% level. * denotes statistical significance at the 5% level.

Table 6. Instrumental Variables Estimates (Linear Trends Only)
Dependent Variable is Ln(Crime Complaints Per 100,000 People)

	(1) Serious Felonies	(2) Non-Serious Felonies	(3) Misdemeanors	(4) Violations	(5) Serious Felonies	(6) Non-Serious Felonies	(7) Misdemeanors	(8) Violations
LN(Hazardous Summonses)	-0.0198	-0.0353	-0.0237	0.0231	0.0257	-0.0100	-0.0458	-0.0422
	(0.0503)	(0.0959)	(0.0580)	(0.0592)	(0.0678)	(0.0900)	(0.0603)	(0.0736)
Model	IV	IV	IV	IV	OLS	OLS	OLS	OLS
Sample	9/96 – 12/98	9/96 – 12/98	9/96 – 12/98	9/96 – 12/98	9/96 – 12/00	9/96 – 12/00	9/96 – 12/00	9/96 – 12/00
Standard Errors	Newey-West	Newey-West	Newey-West	Newey-West	Murphy-Topel	Murphy-Topel	Murphy-Topel	Murphy-Topel
N	216	216	216	216	408	408	408	408
Constant	7.6214***	9.4126***	10.0816***	6.3288***	7.8987***	8.5957***	8.1782***	4.5531***
	(0.8496)	(2.1735)	(0.9707)	(1.1135)	(0.5443)	(0.7243)	(0.4801)	(0.5885)
R2	0.9808	0.9573	0.9802	0.9487	0.9784	0.9528	0.9753	0.9283

Notes:

1.The sample used for these results includes borough-level, monthly data. The sample period for the results in columns (1) – (4) is September 1996 to December 1998 but data for December 1997 are missing; thus the sample size is 216. The sample period for the results in columns (5) – (8) is September 1996 to December 2000 but data for December 1997 are missing; thus the sample size is 408.

2. All specifications include manpower, borough fixed effects, borough-specific linear trends and quarter X year fixed effects.

3. The model used in columns (1) – (4) is IV with Newey-West HAC standard errors where the default maximum lag length is used. The model used in columns (5) – (8) is OLS with standard errors constructed using the Murphy-Topel method.

4.*** denotes statistical significance at the .1% level. ** denotes statistical significance at the 1% level. * denotes statistical significance at the 5% level.

Table 7. Instrumental Variables Estimates (Linear Trends and Quadratic Trends)
Dependent Variable is Ln(Crime Complaints Per 100,000 People)

	(1) Serious Felonies	(2) Non-Serious Felonies	(3) Misdemeanors	(4) Violations	(5) Serious Felonies	(6) Non-Serious Felonies	(7) Misdemeanors	(8) Violations
LN(Hazardous Summonses)	-0.0538	-0.0062	0.0288	0.0926	0.0310	-0.0099	-0.0222	0.0107
	(0.0556)	(0.0945)	(0.0489)	(0.0602)	(0.0663)	(0.0891)	(0.0584)	(0.0708)
Model	IV	IV	IV	IV	OLS	OLS	OLS	OLS
Sample	9/96 – 12/98	9/96 – 12/98	9/96 – 12/98	9/96 – 12/98	9/96 – 12/00	9/96 – 12/00	9/96 – 12/00	9/96 – 12/00
Standard Errors	Newey-West	Newey-West	Newey-West	Newey-West	Murphy-Topel	Murphy-Topel	Murphy-Topel	Murphy-Topel
N	216	216	216	216	408	408	408	408
Constant	11.7200**	-6.0544	9.3979*	7.6814	7.2758***	5.9655*	12.2964***	7.5199**
	(3.8635)	(8.1960)	(4.0618)	(5.4813)	(2.1329)	(2.8771)	(1.8809)	(2.2854)
R2	0.9809	0.9626	0.9820	0.9496	0.9798	0.9546	0.9769	0.9341

Notes:

1.The sample used for these results includes borough-level, monthly data. The sample period for the results in columns (1) – (4) is September 1996 to December 1998 but data for December 1997 are missing; thus the sample size is 216. The sample period for the results in columns (5) – (8) is September 1996 to December 2000 but data for December 1997 are missing; thus the sample size is 408.

2. All specifications include manpower, borough fixed effects, borough-specific linear trends, borough-specific quadratic trends and quarter X year fixed effects.

3. The model used in columns (1) – (4) is IV with Newey-West HAC standard errors where the default maximum lag length is used. The model used in columns (5) – (8) is OLS with standard errors constructed using the Murphy-Topel method.

4.*** denotes statistical significance at the .1% level. ** denotes statistical significance at the 1% level. * denotes statistical significance at the 5% level.

Appendix Table A1. Effect of Anomaly on Crime Complaints and Summonses

	Crime Complaints per 100K people				
	Total Offenses	Serious Felonies	Non-Serious Felonies	Misdemeanors	Violations
Anomaly	-9.7902	0.8653	26.6503***	33.3852***	-70.6910***
	(8.0297)	(3.4603)	(3.3844)	(7.6391)	(6.5266)
	Ln(Crime Complaints per 100K people)				
	Total Offenses	Serious Felonies	Non-Serious Felonies	Misdemeanors	Violations
Anomaly	0.0010	0.0180	0.3655***	0.0914***	-1.0873***
	(0.0079)	(0.0102)	(0.0397)	(0.0155)	(0.1054)
N	568	568	568	568	568

	Summonses				
	Parking	Traffic Infraction	Traffic Misdemeanor	Other Misdemeanor	Violation
Anomaly	-0.0607	-0.0719	-0.1193	-0.1540	-0.1176
	(0.0591)	(0.0726)	(0.1162)	(0.1434)	(0.1320)

Notes for the top two panels (Crime): Data is at the borough monthly level for 1994-2000 though data is missing for December 1997. Controls include the slowdown dummy, the after slowdown dummy, manpower, borough fixed effects, borough-specific seasonal dummies, and borough-specific linear and quadratic time trends.

Notes for the bottom panel (Summonses): Data is at the city monthly level for June 1995 to December 1998. The data is a panel that includes 41 observations for each of the 5 summons types. Data is missing for December 1997 and January 1998. The anomaly coefficients come from a single panel-data regression where the anomaly dummy is interacted with dummies for summons-type. Other controls in the regression are city seasonal dummies, summons-specific fixed effects, summons-specific linear trends, summons-specific slowdown effects and summons-specific after slowdown effects. The sample size is 205.

Appendix Table A2. Average Number of Hazardous Summonses Issued Per Month,
Patrol Force, 1996-1998

	Average [Std. Dev.]	Proportion [Std. Dev.]
Intox/Impair,	197.23	0.01
Driving	[21.87]	[0.00]
Ability Impair.	6.54	0.00
Alcohol	[2.05]	[0.00]
Reckless Driving	48.40	0.00
	[47.54]	[0.00]
Backing Unsafely	100.06	0.00
	[32.01]	[0.00]
Defective Brakes	316.74	0.02
	[246.27]	[0.02]
Follow. Too Closely	31.54	0.00
	[16.70]	[0.00]
Improper Passing	193.66	0.01
	[74.03]	[0.00]
Fail to Keep Right	48.43	0.00
	[32.43]	[0.00]
Fail to Give Ped R-	50.23	0.00
O-W	[34.20]	[0.00]
Fail to Give Veh R-	82.2	0.00
O-W	[32.64]	[0.00]
Disobey Sign	2,859.66	0.13
	[950.75]	[0.02]
Speeding	1488.17	0.08
	[636.37]	[0.04]
Fail to Stop Signal	9,074.11	0.39
	[4,667.50]	[0.08]
Improper Turn	1,374.31	0.07
	[884.15]	[0.05]
Unlicensed Operator	6,354.49	0.29
	[2,210.04]	[0.05]
Total	22,225.77	1.00
	[7,386.89]	

Notes:
1. The sample period for the hazardous summons data is January 1996 to December 1998, though data is missing for December 1997. Thus, for each cell in the table, N=35.

Appendix Table A3. Patrol Borough Summary Statistics, 1996-2000

	Number of Precincts	Average Population	Average Manpower	Average Serious Felony Crime Complaints per 100,000 People	Average Non-Serious Felony Crime Complaints per 100,000 People
Manhattan South	10	558,526	2,274	523	83
Manhattan North	11	1,007,964	2,598	190	68
Bronx	12	1,307,150	2,965	232	77
Brooklyn South	13	1,587,086	2,644	200	38
Brooklyn North	10	868,948	2,496	259	78
Queens South	8	922,896	1,717	222	47
Queens North	8	1,280,728	1,664	191	28
Staten Island	3	424,682	641	117	46

Note: These data are monthly, borough-level observations. 60 observations are used per borough for each cell in the table.

Figure 1: Misdemeanors and Violations

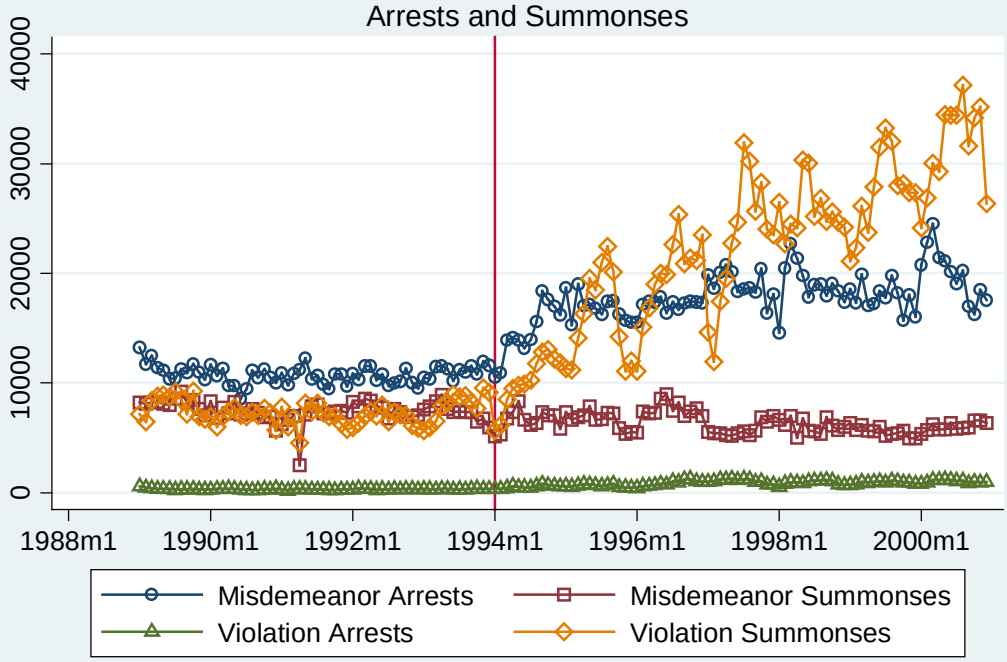


Figure 2A: NYPD Parking Summonses

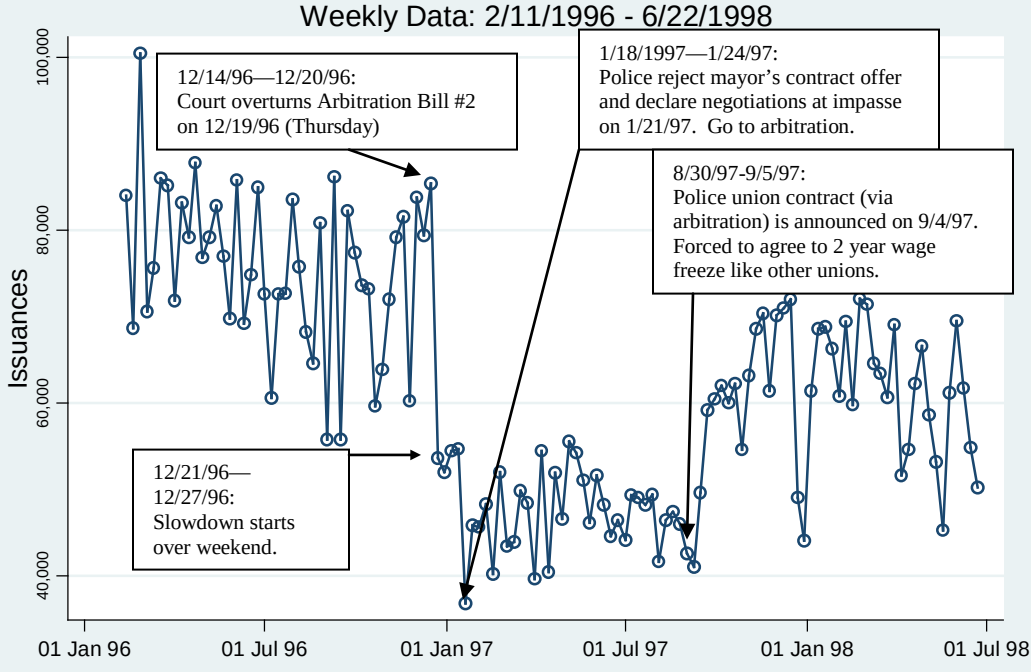


Figure 2B: Parking Summonses: Top 4 Issuing Agencies

Monthly Data: FYs 1994-95 - 2000-01

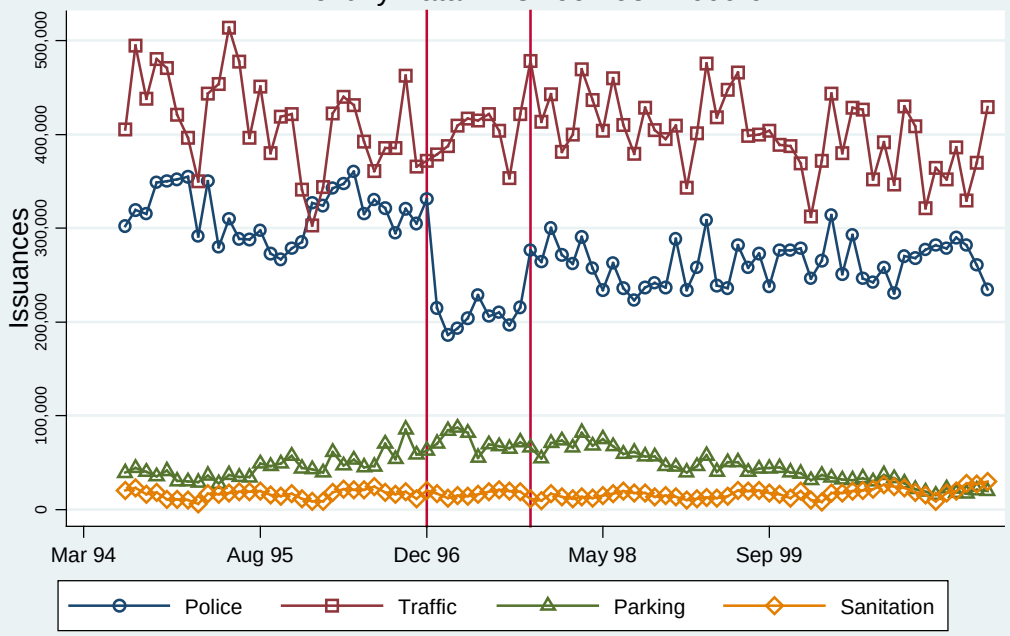


Figure 3: Data Anomaly in Crime Complaints

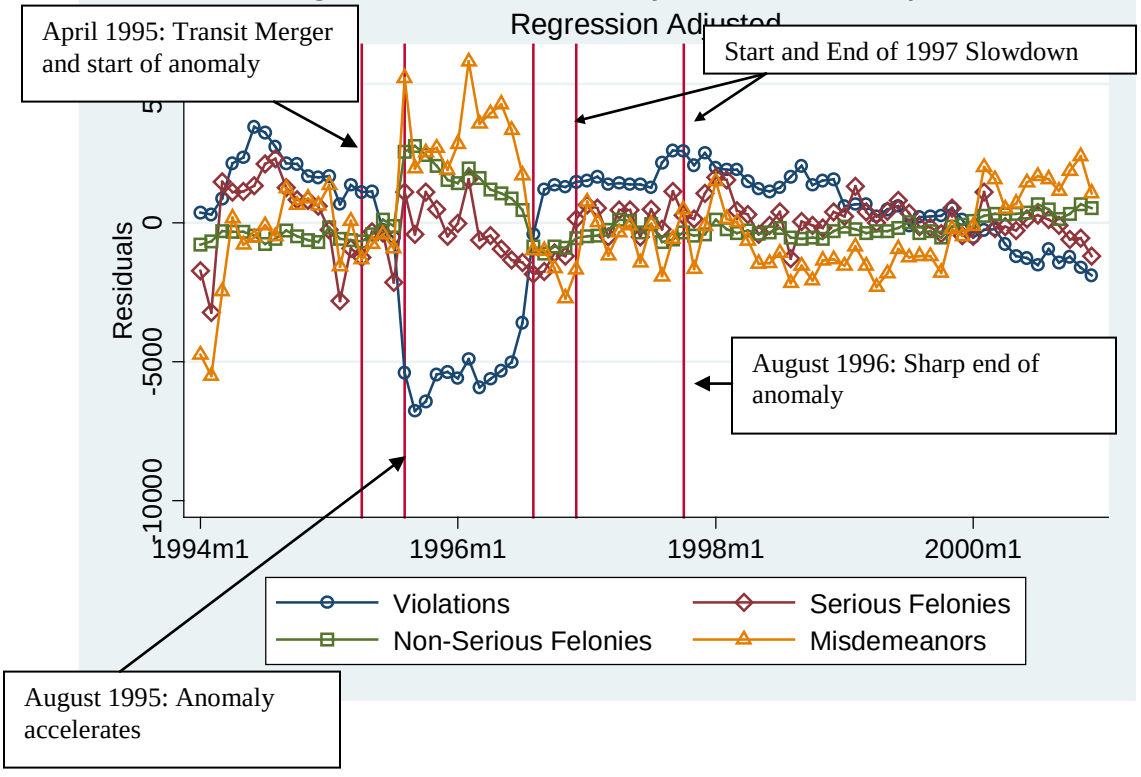


Figure 4: Borough 1- Manhattan South

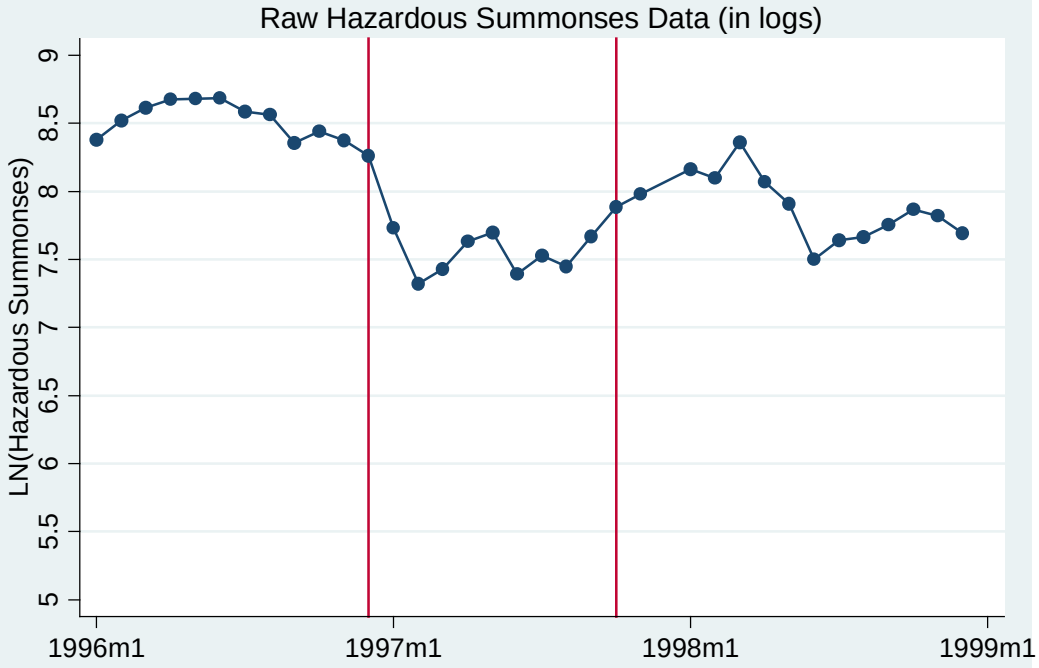


Figure 5: Borough 2- Manhattan North

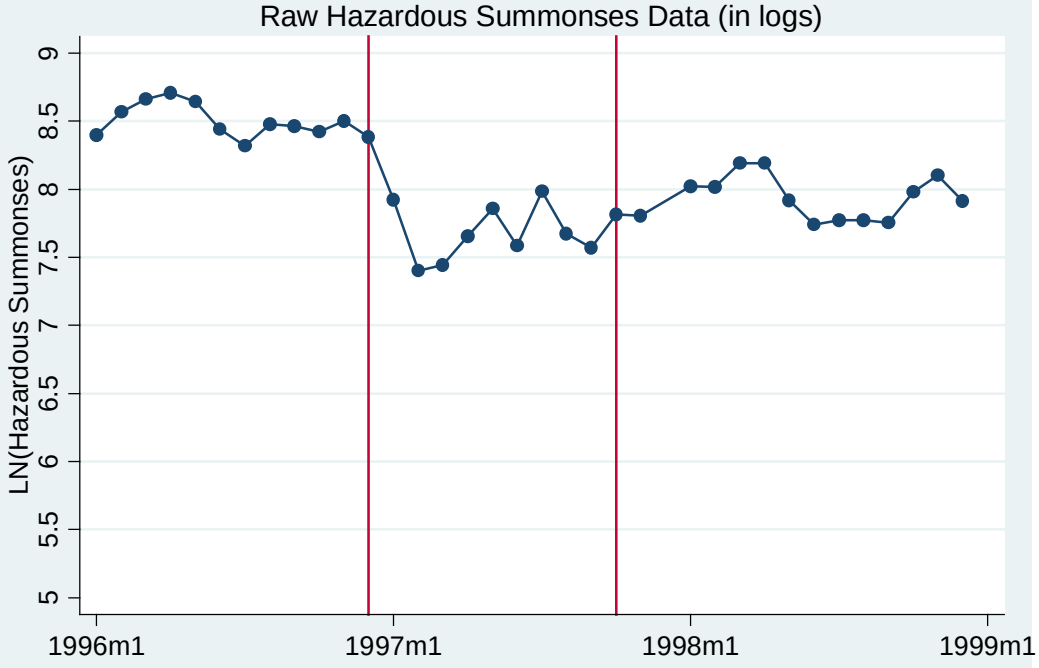


Figure 6: Borough 3- Bronx

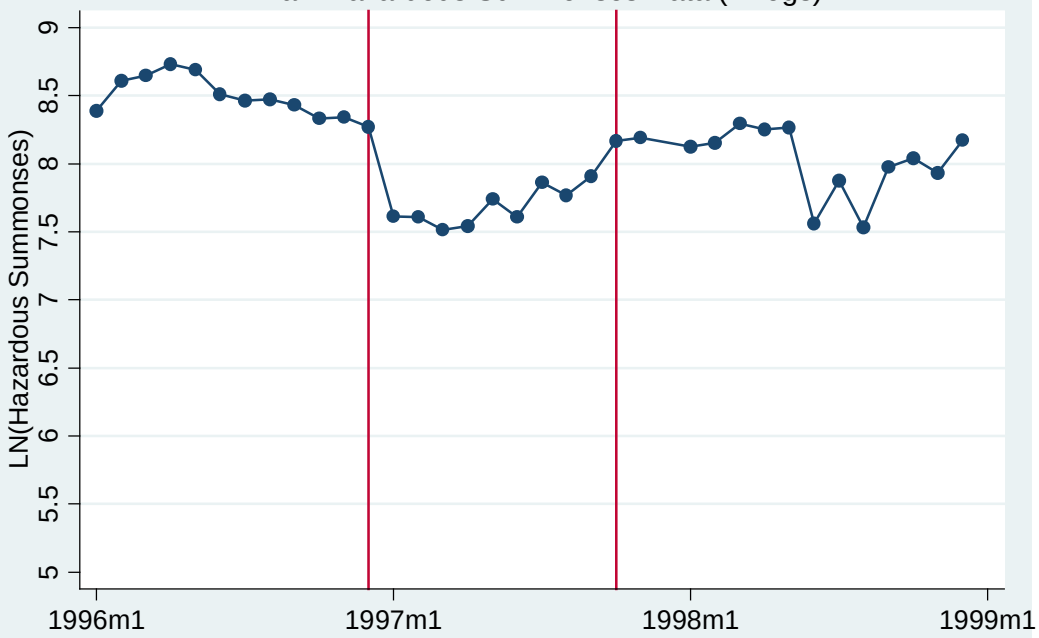


Figure 7: Borough 4- Brooklyn South

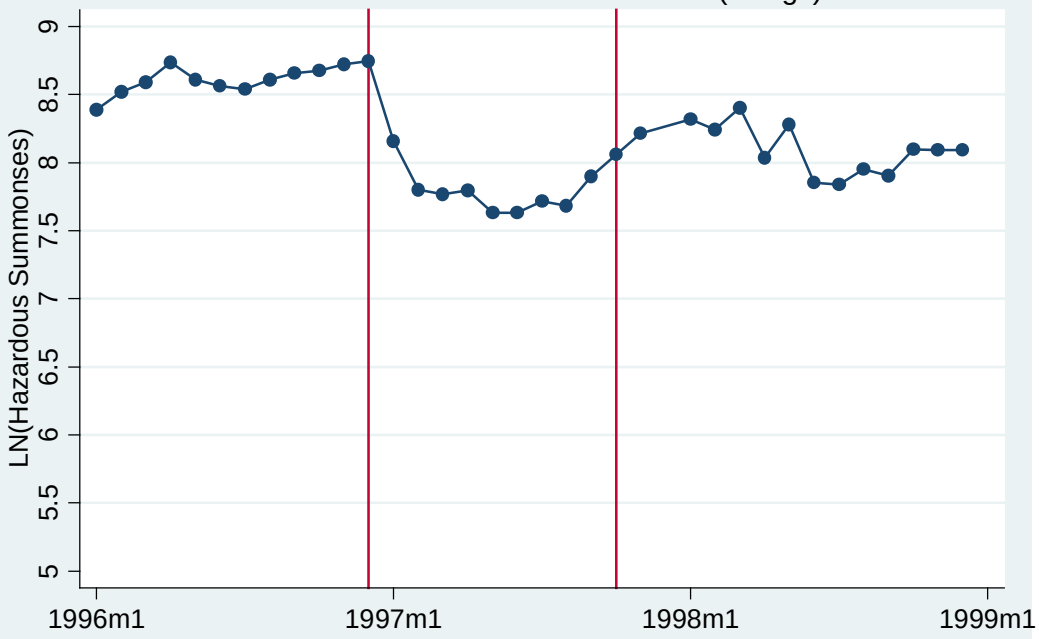


Figure 8: Borough 5- Brooklyn North

Raw Hazardous Summonses Data (in logs)

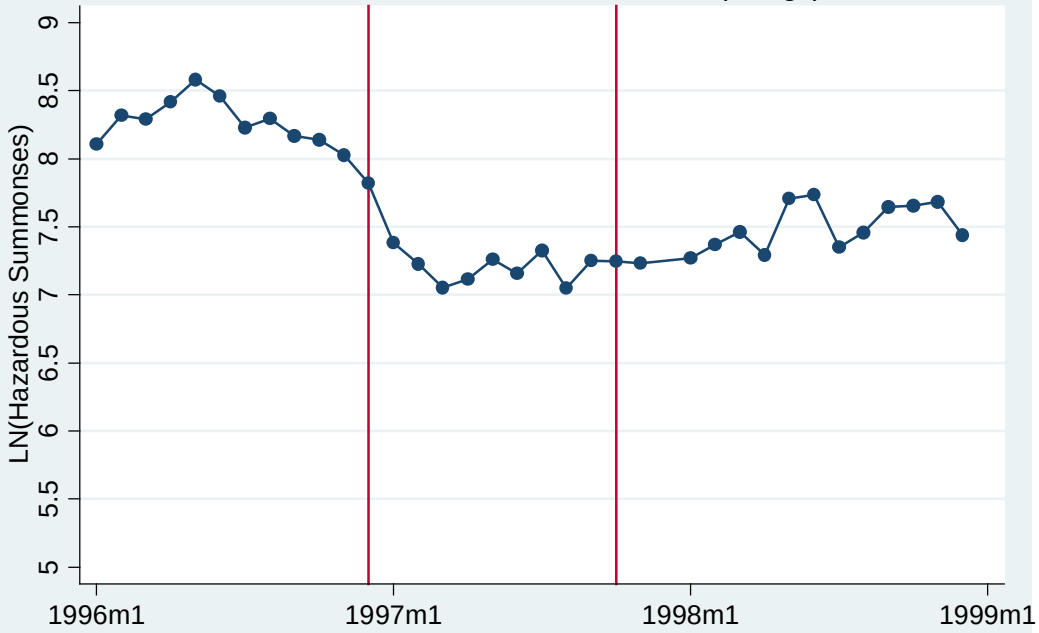
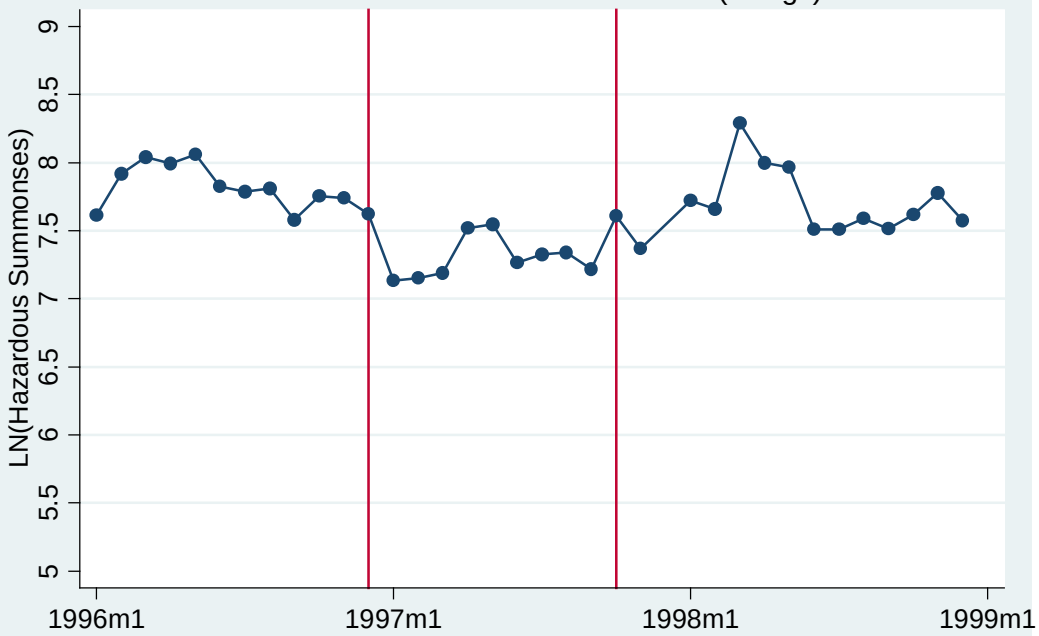


Figure 9: Borough 6- Queens South

Raw Hazardous Summonses Data (in logs)



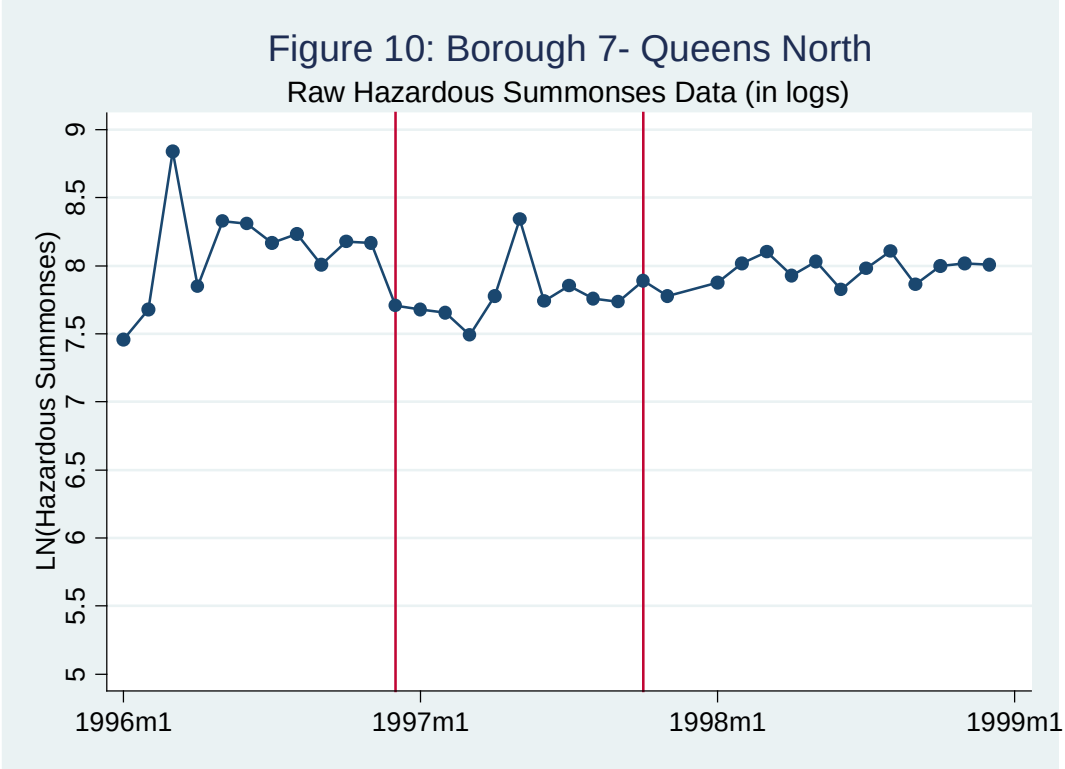


Figure 10: Borough 7- Queens North
Raw Hazardous Summonses Data (in logs)
LN(Hazardous Summonses)
5
5.5
6
6.5
7
7.5
8
8.5
9
1996m1
1997m1
1998m1
1999m1

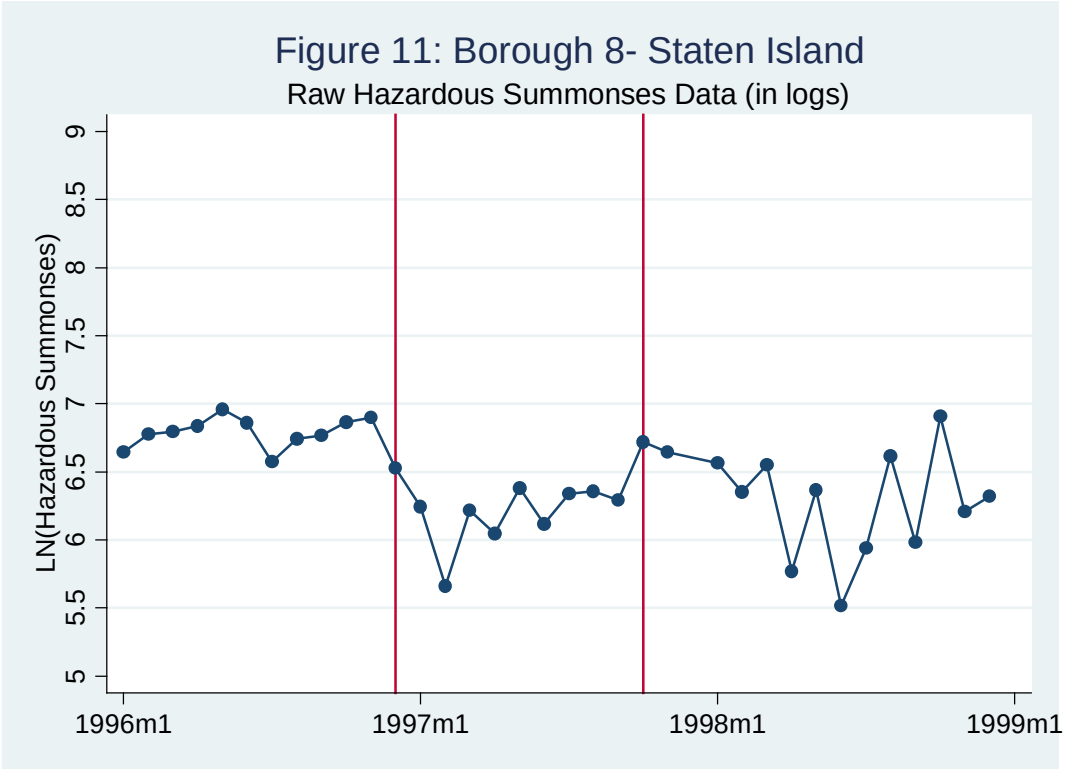
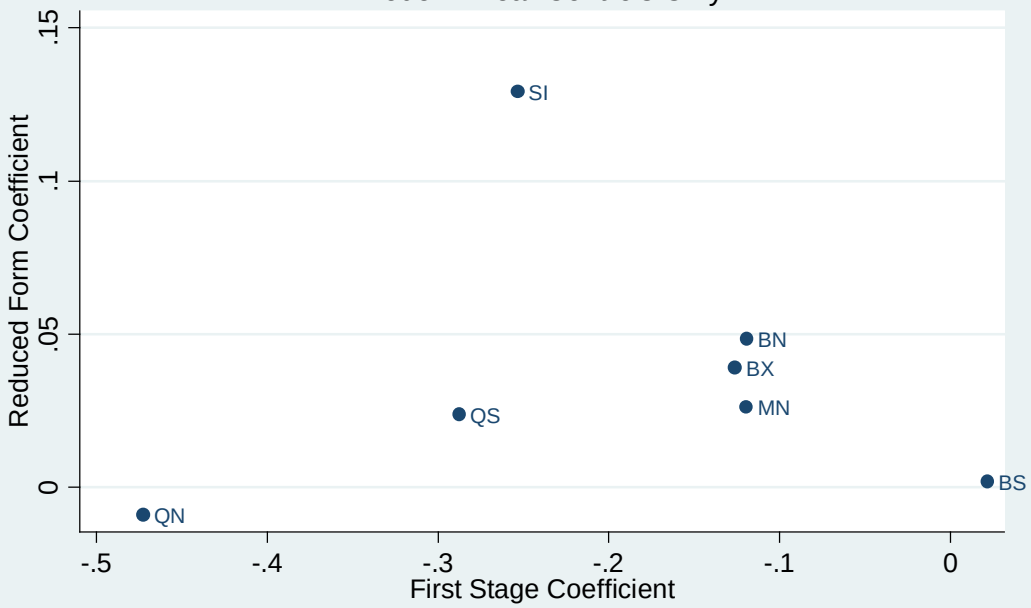


Figure 11: Borough 8- Staten Island
Raw Hazardous Summonses Data (in logs)
LN(Hazardous Summonses)
5
5.5
6
6.5
7
7.5
8
8.5
9
1996m1
1997m1
1998m1
1999m1

Figure 12: Serious Felonies IV Effect

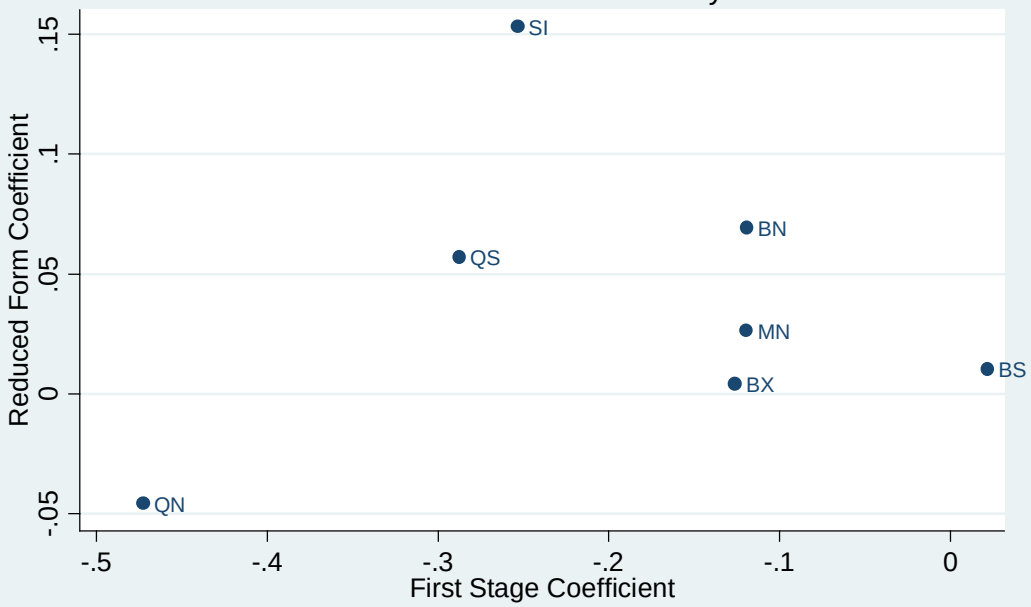
Model: Linear Controls Only



Note: The negative of the first stage coefficient is used for graphing.

Figure 13: Non-Serious Felonies IV Effect

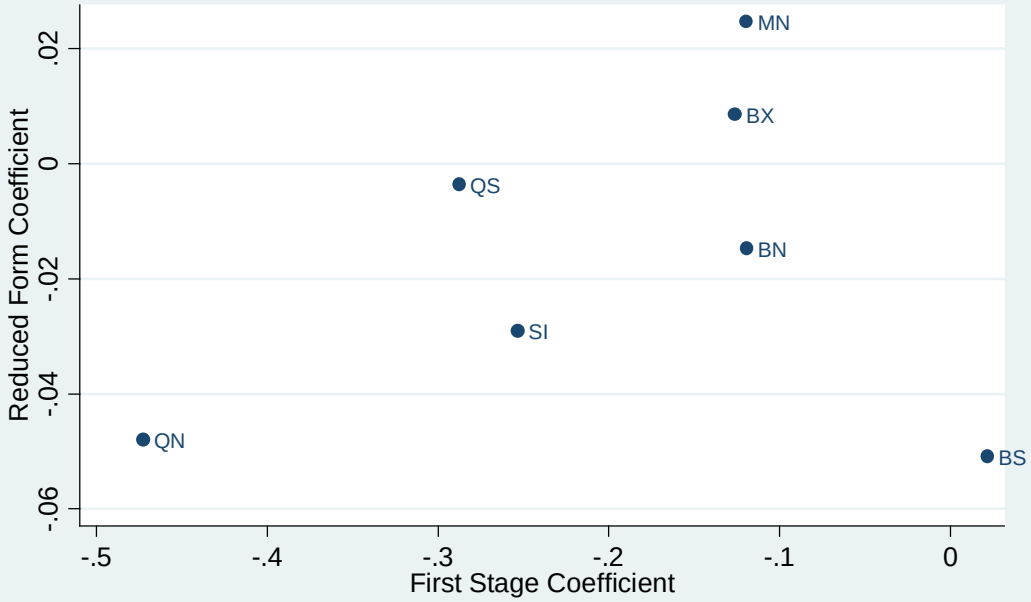
Model: Linear Controls Only



Note: The negative of the first stage coefficient is used for graphing.

Figure 14: Misdemeanors IV Effect

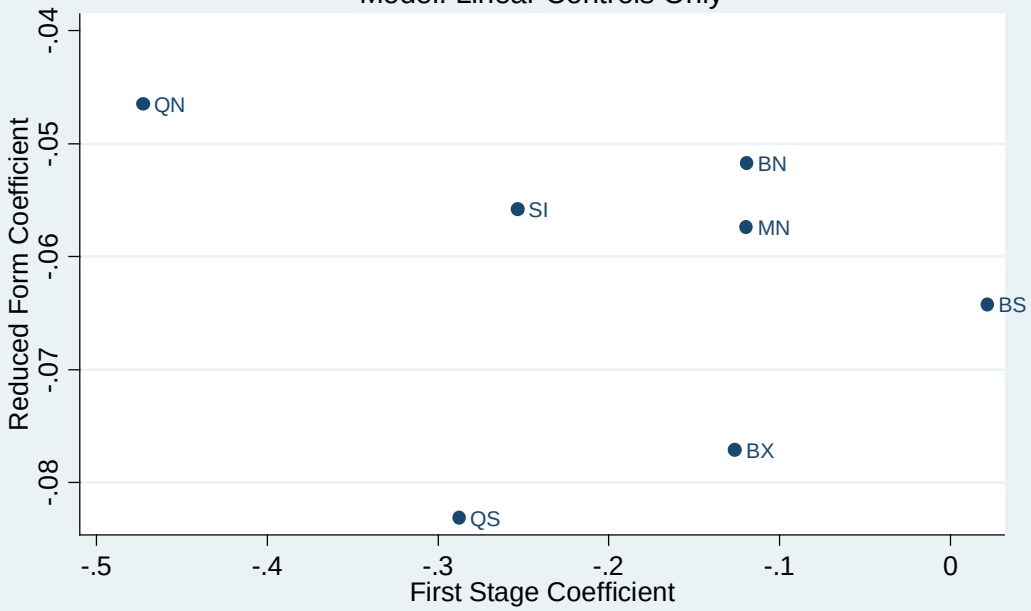
Model: Linear Controls Only



Note: The negative of the first stage coefficient is used for graphing.

Figure 15: Violations IV Effect

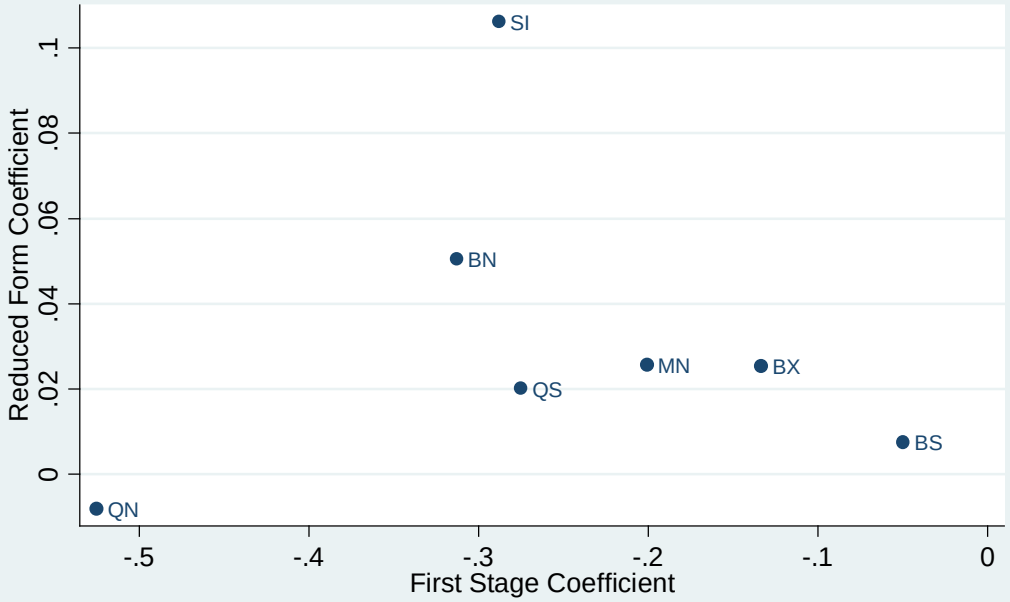
Model: Linear Controls Only



Note: The negative of the first stage coefficient is used for graphing.

Figure 16: Serious Felonies IV Effect

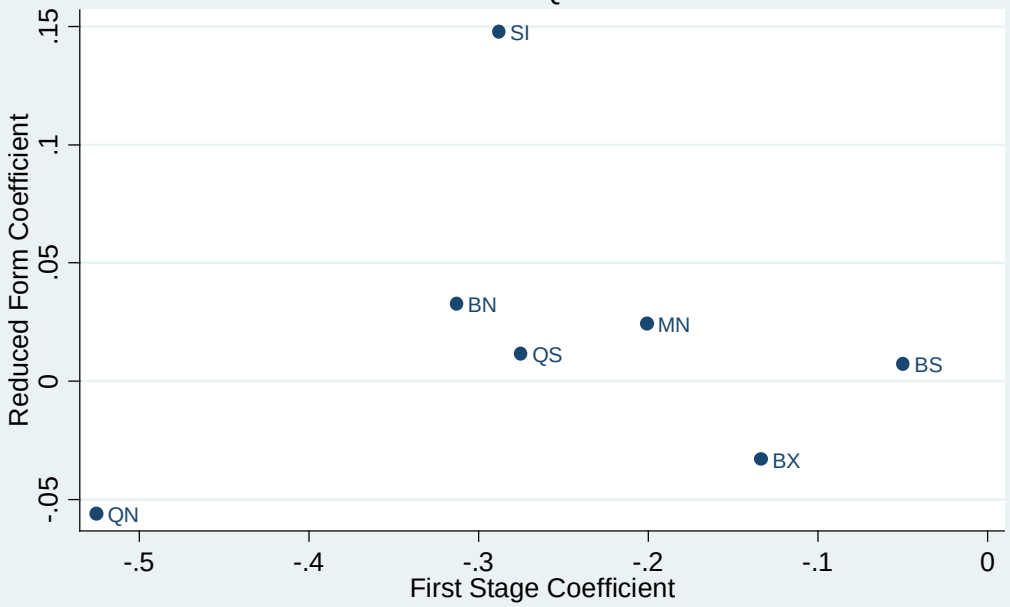
Model: Linear and Quadratic Controls



Note: The negative of the first stage coefficient is used for graphing.

Figure 17: Non-Serious Felonies IV Effect

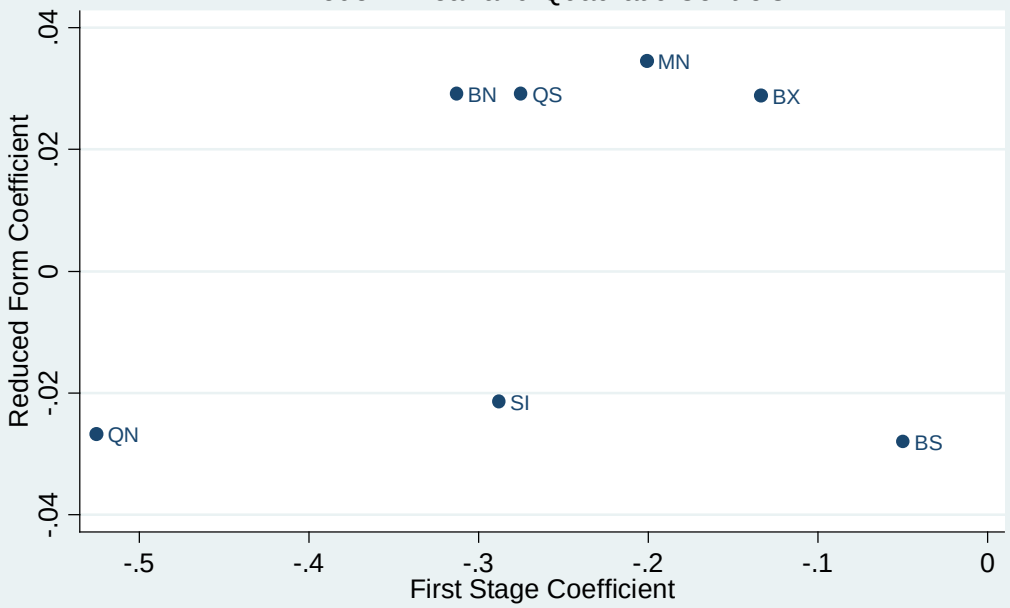
Model: Linear and Quadratic Controls



Note: The negative of the first stage coefficient is used for graphing.

Figure 18: Misdemeanors IV Effect

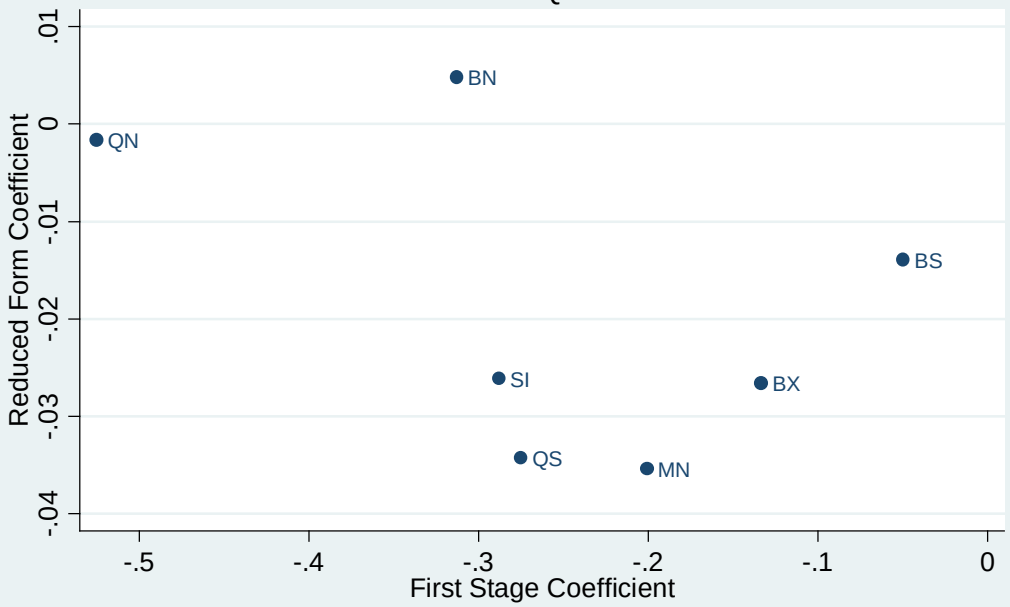
Model: Linear and Quadratic Controls



Note: The negative of the first stage coefficient is used for graphing.

Figure 19: Violations IV Effect

Model: Linear and Quadratic Controls



Note: The negative of the first stage coefficient is used for graphing.