Does Stop and Frisk deter crime? Evidence from the Aftermath of *Floyd v. City of New York*

Nathaniel Barlow*

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Abstract

One of the highest priorities of any government is protecting its citizens. Police serve a vital role in this by deterring crime using a variety of tactics. *The Floyd v. City of New York* (2013) decision created an exogenous shock to the amount of stop and frisk interactions initiated by the NYPD. Using the timing of the decision, I estimate the causal effect of stop and frisk on crime complaint. I find inconclusive results: small point estimates with wide confidence intervals. My findings contradict claims of the extreme effectiveness of stop and frisk, but leave open the possibility of stop and frisk having marginal deterrent effects.

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I Introduction

Crime deterrence theory centers on the idea that rational individuals must make a choice whether to commit a crime or not. Gary Becker (1968) birthed the utility maximization model of crime and paved the way for further models such as Ehrlich (1973) and Lee and McCrary (2009). One of the principal considerations all these models include is the likelihood that the individual will be caught. The individual's utility of crime is inversely related to her chances of repercussion. Police presence and tactics serve as the fundamental vehicle for affecting the individual's chances of being caught.

The main issue researchers face when moving from models to empirics with crime is simultaneity bias. Areas of high crime are likely to deploy more police officers and utilize stricter tactics. This would lead to finding an erroneous positive effect of police on crime. To correct this problem, researchers have looked to quasi-experimental designs starting with Levitt (1997), who used the timing of mayoral and gubernatorial elections as instruments. However, Justin McCrary (2002) later found an error in Levitt's paper thereby removing the statistical significance. Nevertheless, Levitt (1997) laid a sturdy foundation for further research. For example, Di Tella and Schargrodsky (2004) and Klick and Tabarrok (2005) bolstered the existing literature with their robust analyses of the effect of exogenous shocks to police presence in response to terrorist attacks.

In this article, I estimate the causal effect of the police procedure of stop and frisk in New York City. I continue in the quasi-experimental tradition and exploit the timing of the court decision for Floyd v. City of New York (2013). On August 12, 2013, a federal judge found the New York City Police Department guilty of racially biased and unconstitutional stops. What followed was a precipitous decline in the number of stops initiated by the NYPD, from 532,911 in 2012 to 191,851 in 2013. The presiding judge, Shira Scheindlin, repeatedly stated that the case was "not about the effectiveness of stop and frisk in deterring or combatting crime." This coupled with the substantial decline in stops immediately after the decision make it an appropriate natural experiment. The mechanism by which stop and frisk theoretically deter crime is very similar to that of police presence. If the individual perceives the increased activity of the police by way of stop and frisk, she may forego criminal activity due to higher risk of being caught. However, even if the individual does not perceive any change in likelihood of consequence, she may just be stopped prior to committing a crime and either be arrested or sufficiently deterred by the experience. Though given the high-profile nature of the case and the significant reduction in stops, it is likely that the majority of New York City residents were aware of some degree of change to stop and frisk procedure.

I collected information on every stop recorded by the NYPD between 2003 and 2015 as well as precinct level crime complaints for New York City. I then estimated the effect of stop and frisk on crime complaints. My results show negligible effects of stop and frisk on all forms of crime, from major felonies to misdemeanors. To my knowledge, there have been no previous quasi-experimental studies on the deterrent effects of stop and frisk, though Rosenfeld and Fornango (2014) estimated the effects without an instrument; the literature surrounding stop and frisk focuses on the issue of racial bias (Gelman, Fagan, Kiss 2007). However, a recent study from Chandrasekher (2016) analyzes the police slowdown in New York City in 1997 and connects it to broken windows theory. She finds a moderate increase in minor crimes and a small increase in serious crimes resulting from the slowdown. Her paper is also noteworthy given that she analyzes the effect of an exogenous decrease rather than an increase like so much of the existing literature. On the other hand, Rozema and Schanzenbach (2016) assess the signal strength of civilian allegations on police misconduct in Chicago, linking to the civil complaint feature of the Floyd decision. They find a strong signal from civilian allegations both in terms of litigation and in-house departmental allegations. I believe my estimates can bridge a gap between crime deterrence and citizen complaints.

The remainder of the paper is organized as follows. Section II describes the data sources used in this article. Section III describes the methodology of stop and frisk and details of the Floyd v. City of New York case. Section IV details the econometric framework and my results and economic interpretation. Section V concludes.

II Data

The NYPD has made recent records of stop and frisk encounters available to the public. I make use of all the available data, an unbalanced panel stretching from January of 2003 to December of 2015. A total of 5,047,350 stops across all 76 precincts are recorded in the 13-year span. The data comes from UF-250 forms filled out by police officers after they have made a stop. The officers make note of a variety of variables on the form including precinct, race of the individual stopped, suspected crime, whether the individual was frisked, etc. It is worth noting that there is inherent bias found in the UF-250 form; only the police officer's side of the story is necessarily told. This limits the use of this dataset, especially the subjective measures such as the suspected crime. For my main analysis, I only employ discrete counts of stops per precinct though the other variables may prove useful in further analyses. I also collected data on crime complaints at the precinct level. Annual crime complaints totals are available for all 76 precincts from 2000 to 2015, though I only use the data from 2003 on. Each precinct's annual report contains totals for individual crimes as well aggregates for three crime classifications: major felony, non-major felony, and misdemeanor. Due to the wide variance between precincts and very small totals for certain crimes, such as murder, I focus my analysis on these three aggregates.

Figure 1 depicts the time series for stops and crime complaints aggregated over precincts from 2003 to 2015. The vertical line corresponds to the timing of Floyd v. City of New York (2013). From this figure, no discernible relationship between stops and crime complaints is revealed. It is worth noting that graphics of this style have been used by various media sources as proof that stop and frisk is not effective though they are of course insufficient. Figure 2 plots stops against crime complaints for all years and precincts; each circle represents one precinct for one year. A noisy, but distinguishable positive relationship is revealed supporting the assumption that more stops are carried out in precincts with higher crime rates.

III Stop and Frisk and *Floyd v. City of New York*

New York City has seen a considerable decline in crime in the last 30 years, like much of the United States. One of the most popular theories about the cause of this decline is stricter police practices, especially in New York City. Rudy Giuliani was mayor for most of the 1990s and a main goal of his administration was a crackdown on crime through the channel of a "zero tolerance" policy. Giuliani brought in William Bratton in as Police Commissioner who overhauled the NYPD, making use of CompStat, a statistical tool as well as methodology for fighting crime, as well as the controversial "broken windows" theory. In this section I discuss the institutional and historical background of stop and frisk as well as the core details of the Floyd v. City of New York case.

III.A Compstat

Compstat at its core is not linked to any particular ideology of crime. It prioritizes data-based policing, tracking what crimes are committed and where. It is also intended to promote accountability and organized leadership. The program was the result of Bratton and his lieutenants' desire to prove that police could affect crime (Weiburd et al 2003). However, Compstat also brought with it large department meetings where precinct statistics were projected on a screen and police chiefs were questioned about their methods. This has the obvious potential to create perverse incentives to either lie about statistics or conduct poor police work. If an officer needs to meet a strict, or even implied quota for arrests, he may feel pressured to make an unsubstantiated arrest. Indeed, this issue was brought up in the Floyd case. Focus on quantitative results may affect qualitative results perhaps reducing police effect on crime. Weiburd et al (2003) caution that Compstat has become "shorthand" for all the issues found in the post-Bratton NYPD. Bratton enacted a top-down reconstruction of the department and thus it is difficult to ascertain the effects of Compstat itself on the overall trend of the NYPD.

III.B Broken Windows

Corman and Mocan (2002) note that at a press conference on February 4, 1998 Giuliani asserted that broken windows theory had become "an integral part of [New York City] law enforcement strategy." The theory featured prominently in Giuliani and Bratton's crime regime, but it predates their initiative. Broken windows theory comes from an article entitled Broken Windows: The police and neighborhood safety written by George Kelling and James Wilson (1982). The name comes from the idea that if a broken window is not repaired, the rest of the windows in the building will soon follow suit. The broken window sends a signal that the neighborhood is untended and disorderly, leading to more disorder. While Kelling and Wilson state that ensuing crime is not "inevitable," they strongly suggest crime, even serious crime, follows the disorder and that combatting low-level crimes deters all crime. They also posit that the disorder of a broken window leads to community decay. Police officers on foot, rather than in a vehicle, engage with the community and have a closer connection to its inhabitants. The community is more united and more resistant to crime. Residents feel more comfortable informing the police of potential wrongdoing and feel safer, even if crime rates have not declined. The article often prioritizes residents' perception of safety over their actual safety (as measured by crime rates). Clearly individuals' expectations and perceptions are nontrivial, however given scarce police resources, actual crime decline must be the focus of the police.

The main shortcoming of broken windows theory is its lack of empirical backing. The article relies heavily on anecdotal accounts and very limited psychological experiments. Considering Becker's model of crime deterrence (1968), the way that broken windows could work is that an individual may perceive his likelihood of being caught for a serious crime has increased due to a crackdown on low-level crimes. This is similar to how stop and frisk may deter crime in Becker's model; individuals see increased stops as increasing their likelihood of being caught for all crime. Corman and Mocan (2002) test broken windows theory by analyzing the effect of misdemeanor arrests on other crimes. They find evidence that misdemeanor arrests decrease motor vehicle and robberies, but have no effect on other crimes. Thus there is some evidence of spillover from one crime to the next, but little to suggest serious crimes are held in check by fixing broken windows.

III.C *Floyd v. City of New York* (2012)

Floyd v City of New York (2013) was a landmark case dealing directly with stop and frisk. It came as the climax of decades of tension and legal action in response to perceived racial bias in NYPD tactics. The plaintiffs claimed that the way in which the NYPD utilized stop and frisk infringed on their constitutional rights of both the Fourth and Fourteenth Amendments: stops were not based on probable cause (Fourth Amendment) and violated "equal protection" under the law (Fourteenth Amendment). The use of stop and frisk has been confirmed as constitutional several times, most notably the first time in Terry v. Ohio (1968) when the Supreme Court decided that reasonable suspicion not probable cause was necessary to validate a stop. The plaintiffs in the Floyd case did not dispute the constitutionality of stop and frisk, but rather the way it was carried out by the NYPD.

The presiding federal judge ruled in favor of the plaintiffs based on a varied set of evidences, which was mostly anecdotal, but had some empirical elements. Jeffrey Fagan provided expert testimony for the plaintiffs; he performed various statistical analyses of stop and frisk particularly in relation to race demographics and success rates. He found that 6% of stops resulted in arrest, 6% resulted in summons, and 1.5% of frisks found weapons. He also found that 52% of the persons stopped were black, 31% were Hispanic, and 10% were white compared to New York City's overall demographic of 23% black, 29% Hispanic, and 33% white. While Fagan's analysis uncovers a myriad of unsettling statistics, his regressions analyzing racial bias are slightly misinterpreted. He uses relevant controls like police patrol force and socioeconomic status, but he does not implement any quasi-experimental design and thus cannot avoid simultaneity bias. Given that the results were used as evidence of a breach of the Fourteenth Amendment, I can assume a causal inference was made, however the regressions provide insufficient proof (at least econometrically) for said inference.

Perhaps the most relevant facet of the case to this article is the claims of the decline in quality of stops in relation to the rapid increase in quantity of stops. Judge Scheindlin mentions in her decision that, "Between 2004 and 2009, as the number of stops per year soared from 314,000 to 576,000, the percentage of UF-250s on which the officer failed to state a specific suspected crime rose from 1% to 36%." In that same period, the most oft checked reasons for a stop were "Furtive Movements" and "High Crime Area," both of which are vague and hardly constituting "reasonable suspicion." From witness testimony and NYPD records, Judge Scheindlin found evidence that amount of UF-250 completed by a unit is a measure of performance and that commanders were questioned about this measure at Compstat meetings. If the number of stops performed was inflated either artificially or by a reduction in quality, the effect on crime may have been diluted. Individuals may assume diminishing marginal likelihood of being caught and thus not respond to increases in stop activity.

IV Effect of Stop and Frisk on Crime

My goal is to identify the causal effect of stops on crime complaints utilizing the timing of the Floyd v. City of New York (2013) case as an instrument. I present regression estimates for the effect of stops on crime and stop elasticity of crime for the three categories of crimes: major felonies, non-major felonies, and misdemeanors. I then present estimates using data from 2010 onward as a robustness check. Finally I discuss the economic interpretation of my results.

IV.A Results

Table 1 compiles the estimates for a variety of regressions including the following fully specified instrumental variable (IV) regression:

$$STOP_{it} = \gamma POST_i + X_i'\alpha + \epsilon_{it} \tag{1}$$

$$CRIMECOMPLAINT_{it} = \beta STOP_{it} + X_i'\delta + \eta_{it} \tag{2}$$

Where $STOP_{it}$ denotes number of stop and frisk interactions recorded in precinct i in year t, $POST_i$ denotes an indicator for year t being 2013 or later, X_i' denotes a vector of control variables that includes up to a quadratic polynomial in year as well as a full set of precinct dummies. The coefficient β is my main statistic of interest; it represents the mean effect of an increase of one stop on crime complaints.

Each regression incudes precinct effects, while each column increases the order of the year polynomial by one starting with zero in Column 1. Columns 4 through 6 mimic the specifications of the previous columns, but with the added IV design. As is anticipated, Column 1 shows significant positive coefficients for misdemeanors and non-major felonies, but precinct effects alone move the point estimate negative for major felonies. The fully specified model is represented in Column 6 and the results are inconsistent across different crimes. Stops are estimated at insignificantly reducing major felonies with a coefficient of -.0079 and standard error of .0066. The coefficients for both misdemeanors and non-major felonies are significant, but have different signs, at .0562 and -.0216 respectively.

Table 2 reports the same regressions as Table 1, but using the annual growth rates of stops and crime complaints instead discrete counts, thus the reported coefficients are stop elasticities of crime. The reported elasticities in Column 1 are all positive for the simple OLS regressions, but the elasticity for major felonies is not significant. Moving to the fully specified model in Column 6, none of the coefficients are significantly negative. The estimate for major felonies is significantly positive at .0811, while the estimates for misdemeanors and non-major felonies are both insignificant, -.0018 and .0127.

IV.B Robustness

As the time series of Figure 1.B and 1.C reveal, major and non-major felonies decline significantly before plateauing after 2010. One possible concern is that the POST instrument is just an indicator for after the decline, not after the Floyd case. Tables 3 and 4 report the estimates from a robustness check I conducted repeating the regressions from Tables 1 and 2, but only using data from 2010 and later. None of the estimates for frequencies in Column 6 of Table 3 are significantly different from zero: .0022 for major felonies, .0040 for misdemeanors, and -.0044 for non-major felonies. Furthermore, compared to their counterparts in Table 1, the estimates are all substantially smaller in absolute magnitude. The estimates for elasticities found in Column 6 of Table 4 are all very similar to the corresponding estimates in Table 2. The estimated elasticities for major felonies, misdemeanors, and non-major felonies are .0780, 0034, and .0210 respectively.

IV.C Economic Interpretation

In a recent review of the crime deterrence literature, Chalfin and McCrary (2017) estimate the elasticity with respect to police manpower of violent crime at -.4 and property crime at -.2. Taking my elasticity estimates from Tables 2 and 4 and shrinking them to the lower bound of the 95% confidence interval returns .0595/.0486 for major felonies, -.0230/-.0387 for misdemeanors, and -.0273/-.0266 for non-major felonies. These elasticities are much smaller in absolute magnitude than similar estimates for police manpower. However, the estimates for police manpower cover all activities of police and thus it is reasonable to think stop and frisk represents just a fraction of the deterrence achieved by police. The wide range of the confidence intervals for nearly all the point estimates allow for positive and negative coefficients. It is not clear if or to what degree stop and frisk deters crime.

The estimates for major felonies are surprisingly more positive in the fully specified model than in the basic OLS regression. This is perhaps a spurious result and could be a result of incorrectly specified standard errors. Clustered standard errors could be larger than the ones specified in the tables, however preliminary tests I performed produced even smaller standard errors. What is clear is that there needs to be further research to fully identify the efficacy of stop and frisk.

V Conclusion

Stop and frisk has garnered national attention due to the deep controversy as well as a few mentions by well-known public figures. President Trump advocated for nationwide stop and frisk in an interview stating, "We did it in New York, it worked incredibly well." On a more local level, in response to regulation of stop and frisk the president of the New York City Patrolmen's Benevolent Association said it would "ultimately accelerate the increase in crime and disorder." In this paper, I used the timing of Floyd v. City of New York (2013) as an instrument to estimate the deterrent effects of stop and frisk in New York City. Given the precipitous decline in stops following the court decision confirms the salience of the instrument and it is reasonable to assume independence since it is unlikely that the decision affected crime complaints. I estimate small insignificant deterrent effects of stop and frisk on crime complaints. However wide confidence intervals allow for positive and negative effects. Given my estimates it is entirely possible that stop and frisk deters crime.

Police departments have limited time and manpower, which mandates efficient practices. My results are mixed and suggest that stop and frisk may or may not have a deterrent effect on crime. New York City may have experienced diminished marginal stop effectiveness due to a quality decline or just oversaturation, which could explain my results. It is worth nothing that the cost to the individual of a stop is also non-trivial. The individual is inconvenienced and possibly experiences some degree of humiliation. In light of this and the decision of the Floyd case, it is even more important that effectiveness of police practices be measured and evaluated. I have presented reasonable estimates of the causal effect of stop and frisk on crime, but further research is needed to hone in on estimates and to evaluate the effectiveness of similar police procedures in other cities.

Tables and Figures

Table 1: The Effect of Stops on Crime 2003 - 2015 (Frequencies)

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Major felonies	-0.0061^{**}	-0.0152^{***}	-0.0029	0.0392^{***}	-0.0230^{***}	-0.0079
	(0.0024)	(0.0018)	(0.0024)	(0.0047)	(0.0025)	(0.0066)
Misdemeanors	0.0720^{***}	0.0704^{***}	0.0562^{***}	0.0866^{***}	0.0780^{***}	0.0562^{***}
	(0.0045)	(0.0046)	(0.0064)	(0.0075)	(0.0066)	(0.0176)
Non-major felonies	0.0075^{***}	0.0048^{***}	-0.0003	0.0206^{***}	0.0024	-0.0216^{***}
	(0.0014)	(0.0013)	(0.0018)	(0.0024)	(0.0019)	(0.0054)
Precinct effects	✓	✓	✓	✓	✓	✓
Order of year polynomial	0	1	2	0	1	2
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 2: The Effect of Stops on Crime 2003 - 2015 (Growth Rates)

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Major felonies	0.0028	0.0348^{***}	0.0371^{***}	-0.0021	0.0523^{***}	0.0811^{***}
	(0.0039)	(0.0054)	(0.0056)	(0.0049)	(0.0103)	(0.0110)
Misdemeanors	0.0289^{***}	0.0182^{***}	0.0086	0.0436^{***}	0.0557^{***}	-0.0018
	(0.0039)	(0.0056)	(0.0057)	(0.0050)	(0.0110)	(0.0108)
Non-major felonies	0.0190^{**}	-0.0058	0.0157	-0.0037	-0.1064^{***}	0.0127
	(0.0074)	(0.0107)	(0.0107)	(0.0095)	(0.0214)	(0.0204)
Precinct effects	✓	✓	✓	✓	✓	✓
Order of year polynomial	0	1	2	0	1	2
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 3: The Effect of Stops on Crime 2003 - 2010 (Frequencies)

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Major felonies	0.0017	0.0011	−0.0002	0.0020	0.0022	0.0022
	(0.0013)	(0.0020)	(0.0019)	(0.0016)	(0.0051)	(0.0048)
Misdemeanors	0.0887***	0.0739***	0.0723***	0.0842***	0.0040	0.0040
	(0.0051)	(0.0080)	(0.0080)	(0.0063)	(0.0220)	(0.0218)
Non-major felonies	0.0041***	0.0033**	0.0035**	0.0033***	−0.0044	−0.0044
	(0.0010)	(0.0016)	(0.0016)	(0.0013)	(0.0041)	(0.0041)
Precinct effects	✓	✓	✓	✓	✓	✓
Order of year polynomial	0	1	2	0	1	2
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 4: The Effect of Stops on Crime 2010 - 2015 (Growth Rates)

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Major felonies	0.0298***	0.0306***	0.0399***	0.0402***	0.0780***	0.0780***
	(0.0055)	(0.0076)	(0.0078)	(0.0072)	(0.0156)	(0.0150)
Misdemeanors	0.0238***	−0.0023	−0.0032	0.0427***	0.0034	0.0034
	(0.0054)	(0.0071)	(0.0075)	(0.0071)	(0.0140)	(0.0140)
Non-major felonies	−0.0189**	0.0068	0.0129	−0.0329***	0.0210	0.0210
	(0.0092)	(0.0124)	(0.0130)	(0.0119)	(0.0244)	(0.0243)
Precinct effects	✓	✓	✓	✓	✓	✓
Order of year polynomial	0	1	2	0	1	2
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 5: First Stage Results

	Stop growth		Stop	
	(1)	(2)	(3)	(4)
POST	-1.251^{***}	-1.228^{***}	$-4{,}923.253^{***}$	$-5{,}185.139^{***}$
	(0.068)	(0.032)	(683.538)	(329.438)
year	-56.872^{***}		$462{,}315.800^{***}$	
	(7.382)		(63,568.270)	
year squared	0.014^{***}		-115.004^{***}	
	(0.002)		(15.831)	
Constant	$57{,}166.570^{***}$	0.133^{***}	$-464{,}618{,}201.000^{***}$	$6{,}311.613^{***}$
	(7,412.645)	(0.016)	(63,811,619.000)	(158.337)
Observations	911	911	987	987
R^2	0.676	0.613	0.316	0.201
Adjusted R^2	0.675	0.612	0.314	0.200
Residual Std. Error	0.388	0.423	4,038.684	4,362.184
F Statistic	629.913^{***}	$1{,}437.403^{***}$	151.706^{***}	247.726^{***}

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Figure 1: Trends in Stop and Frisk and Crime

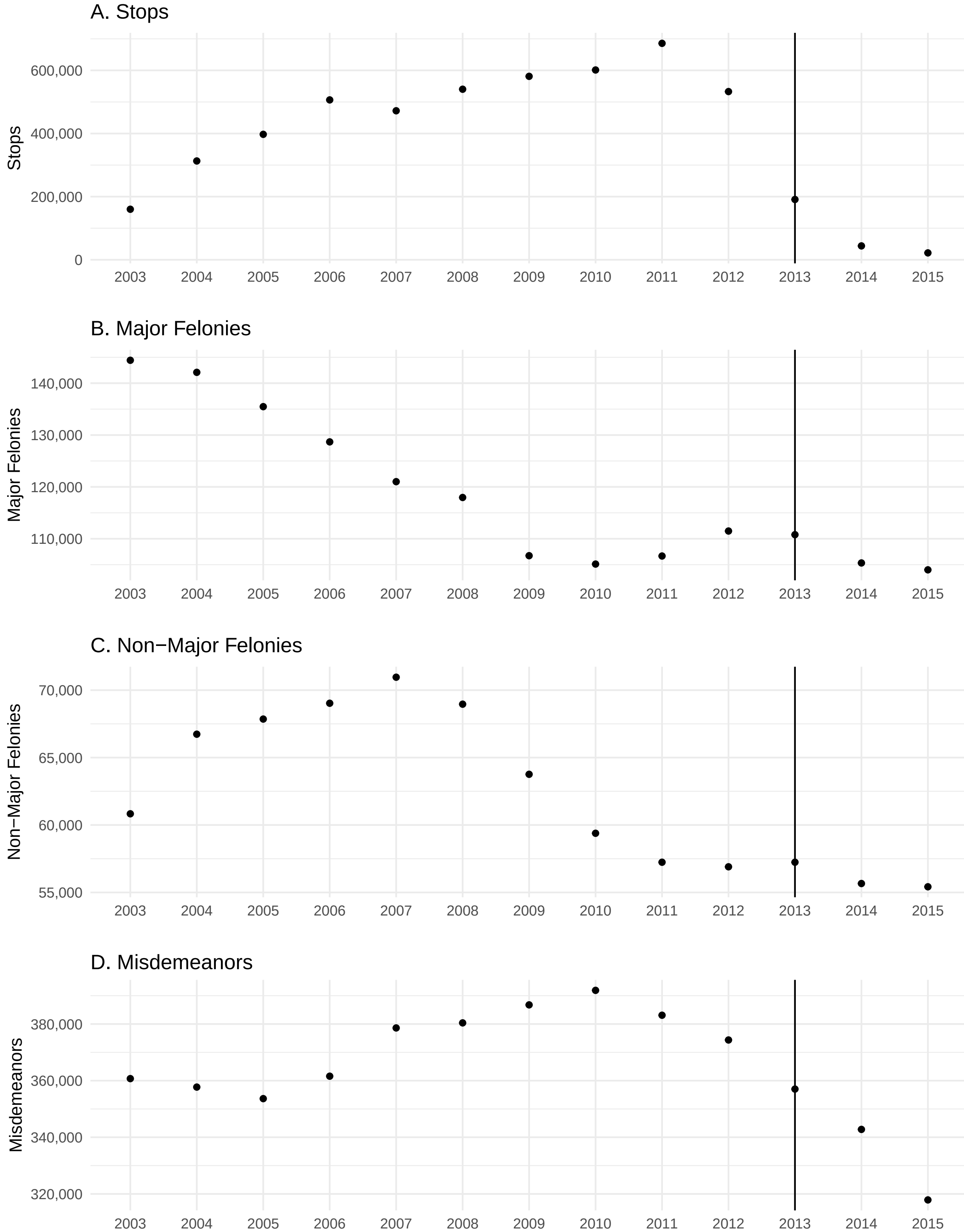
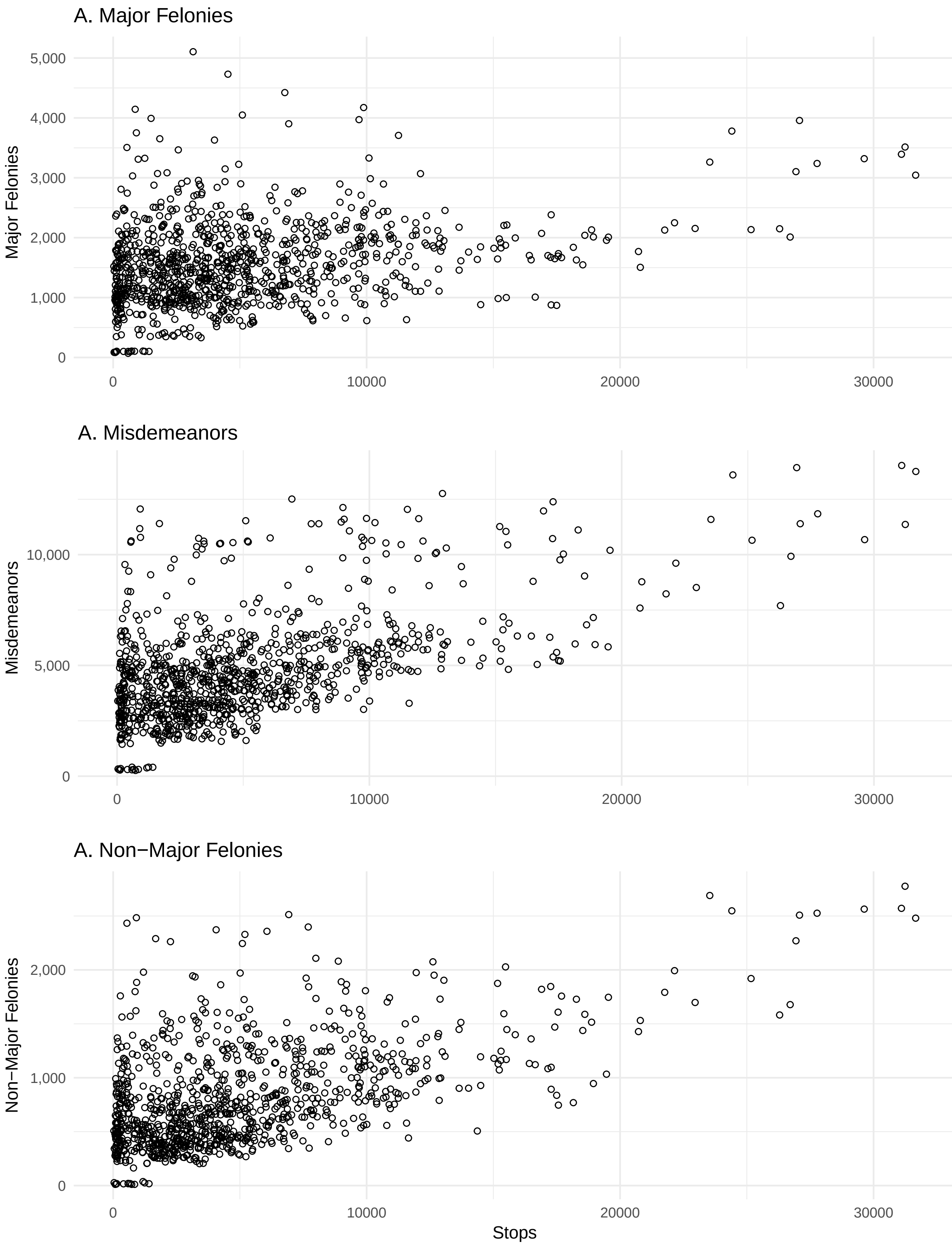


Figure 2: Crime Complaints Against Stops



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