ESSAYS ON THE ECONOMICS OF LAW ENFORCEMENT

by

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To time: you may have won this round, but I'm not finished with you.

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CONTENTS

Contents iii

List of Tables v

List of Figures vi

Abstract viii

1 Spatial Tests for Racial Bias in the NYPD's *Stop, Question & Frisk* Program 1

1.1 *Motivation* 1

1.2 *Contribution Beyond the Existing Literature* 4

1.3 *Model* 8

1.4 *Testable Implications of the Model* 15

1.5 *Findings* 17

1.6 *Conclusion* 36

2 A Hedonic Analysis of *Stop & Frisk's* Amenity Value 42

2.1 *Introduction* 42

2.2 *Current Understanding* 44

2.3 *Model & Data* 50

2.4 *Results* 57

2.5 *Conclusion* 63

3 Evaluating the Economic Impact of California's Proposition 47 67

3.1 *Introduction* 67

3.2 *Related Policies* 70

3.3 *Current Understanding* 73

3.4 *California Crime Trends* 78

3.5 *Conclusion* 85

A Appendix to Chapter One 87

B Appendix to Chapter Two 97

C Appendix to Chapter Three 115

Bibliography 124

LIST OF TABLES

1.1 Distribution of Sampling Rates 2
1.2 Guilt Frequencies are the observed rate of contraband recovery by race from 2006-2012 21
1.3 Probit Regression (Y = Contraband Recovery) w & w/o Precinct Controls 24
1.4 Bi-variate Probit Model Parameter Estimates 25
1.5 Estimates of differences in Δ within precincts 32
1.6 Regression Discontinuity Specifications with Running Control 35

3.1 Total Cost of Crime Estimates (2014 Dollars) 83

A.1 Stop, Frisk & Question Events by Race 87
A.2 Stop, Frisk & Question Events by Borough 87
A.3 Probit Regression (Y = Arrest) w & w/o Precinct Controls 88
A.4 Linear Regressions with Fixed Effects 88
A.5 Probit Regression of Contraband Recovery on Race 89
A.6 Variables Interacted with Race Indicator 89

B.1 Selected Manhattan Neighborhood Boundries 106
B.2 Distribution of Twelve Month Stop Totals by Borough 107
B.3 Description of Select Covariates 108
B.4 OLS Regression of Sale Price on Quadratic Police Activity 109
B.5 Linear Regression of Sale Prices on Police Stops (1,000 Foot Buffer) . . . 110
B.6 Linear Regression of Sale Prices on Police Stops (500 Foot Buffer) 111
B.7 Borough Level Calculations with Racial Controls 112
B.8 Linear Regression of Rental Unit Prices on Police Stops with Neighborhood Effects 113
B.9 Linear Regression of Commercial Property Sale Prices on Police Stops 114

C.1 Tangible Monetary Losses from Property Crime (Pre & Post Proposition 47) 115

LIST OF FIGURES

1.1 Annual SQF totals & the City of New York's payments to cover judgments and settlements related to claims against the NYPD. Source: (Liu, 2012) 4

1.2 Diagram of a typical traffic stop 5

1.3 Diagram of a proactive policing regime 6

1.4 Normal-form representation of model payoffs. 13

1.5 Box-and-whisker plots of 95% confidence intervals around 76 precinct-level estimates of Δ. Each plot represents seven annual measurements from 2006-2012 in a given precinct, with precincts presented in ascending order along the horizontal axis. 22

1.6 Hit Rates for Group Stops, by Race 26

1.7 Ten precincts with the largest racial disparities in stop rates (dark blue) and the precincts they share a border with (light blue) 30

1.8 Precinct borders drawn with 500, 750 & 1,000 foot buffers around them 30

1.9 Formal citizen complaints regarding NYPD actions 2008-2012. Source: (Chu, 2013) 38

1.10 2012 CCRB Stop & Frisk Complainants by Race vs. NYC Demographics 40

3.1 Charged Crimes in California Effected by Proposition 47 69

3.2 Expected Recidivism of Released State Prisoners 75

3.3 Statewide Crime (Annual Totals) 79

3.4 Residual Plot of Observed Monthly Crime Totals Against the Pre-Proposition 47 Linear Trend 80

A.1 Total Encounters in Each Precinct 90

A.2 Reasons given for SQF stops of white & black suspects (2006-2012) . . . 91

A.3 *Extensive form representation of model payoffs.* 92

A.4 NYPD Officer Ethnicity by Rank (2012) 93

A.5 Color represents the probability that each census block is an outlier in the Getis-Ord Gi* distribution of precinct stop intensities. Light blue represents 90% probability, dark blue represents 99% probability. The height of each feature is a scaled measure of the associated z-score. . . . 94
A.6 Distribution of Δ 95
A.7 Percentages reflect portion of parent node associated with the child along that edge. Numbers in parentheses reflect the contraband recoveries associated with each of the four possible partial histories (for instance, Frisked,Search). 96

B.1 New York City Community Districts 97
B.2 New York City Neighborhoods 98
B.3 Racial Distribution of Residents (NYC) 99
B.4 Racial Distribution of Residents (Upper Manhattan) 100
B.5 Hot-Spot Analysis using $\mathtt{Getis-Ord\ Gi*}$ Statistic 101
B.6 Distribution of Crime in East Manhattan (Q12015) 102
B.7 Distribution of Crime in Chelsea (Q12015) 103
B.8 Important Local Amenities 104
B.9 Important Local Amenities 105

C.1 'Strikers' in California Prisons 116
C.2 San Francisco Property Crime (Monthly Totals) 117
C.3 San Francisco Larceny (Monthly Totals) 118
C.4 San Francisco Motor Vehicle Theft (Monthly Totals) 118
C.5 San Francisco Burglary (Monthly Totals) 119
C.6 San Jose Property Crime (Monthly Totals) 120
C.7 San Diego Property Crime Levels (Monthly Totals) 121
C.8 Los Angeles Property Crime (Annual Totals) 122
C.9 Los Angeles Property Crime (Monthly Totals) 123

ABSTRACT

Chapter 1: Spatial Tests for Racial Bias in the NYPD's *Stop, Question & Frisk* Program

This paper introduces a model that allows investigators to determine whether police officers exhibit taste based racial bias when selecting suspects for interdiction, as well as whether suspect responses to police interdiction vary by race. Using data provided by the New York Police Department I estimate that African-American suspects are less likely than their white counterparts to be found in possession of contraband when searched by police. This finding is robust to alternative modeling assumptions and is consistent with racially biased policing within my model.

Chapter 2: A Hedonic Analysis of *Stop & Frisk's* Amenity Value

This paper measures the value households place on street-level intensive policing practices. It utilizes a large, spatially detailed data set that includes more than one hundred thousand real property sales and four million police-citizen encounters in New York City from 2006-2012. A hedonic analysis of this data shows that the New York Police Department's practice of Stop, Question Frisk policing was likely seen as a neighborhood disamenity by home buyers. Using finely partitioned geographical areas to control for variation in omitted variables and spatial statistics to precisely describe location relative to surrounding amenities and disamenities, I find that properties exposed to more intense Stop Frisk activity sold for significantly lower prices.

Chapter 3: Evaluating the Economic Impact of Proposition 47

Proposition 47 was a California ballot initiative that reclassifies many felony crimes as misdemeanors if they are non-serious, non-sexual, and non-violent. Passed by voters in November, 2014, the law immediately caused the retroactive release of more than 3,000 prisoners who had been convicted of felonies that would be misdemeanors under the new law. This chapter offers evidence related to the costs and benefits associated with the release of these prisoners, as well as the diversion of an expected 40,000 more offenders each year who will no longer face

felony charges. Preliminary estimates reveal the economic benefits outweigh the costs nearly 3-to-1.

1 SPATIAL TESTS FOR RACIAL BIAS IN THE NYPD'S *STOP, QUESTION & FRISK* PROGRAM

1.1 Motivation

An overwhelming amount of data, from police departments nationwide, unequivocally indicates that black males are the demographic group most likely to interact with police. For instance, this paper utilizes data collected by the New York Police Department (NYPD) to track officers' interactions with pedestrians as part of that organization's *Stop, Question & Frisk* (SQF) campaign. Under the SQF policing regime officers are assigned to patrol potential crime hot-spots on foot. Their dual goals are to promote safety and reduce crime by stopping anyone they have a 'reasonable suspicion' has committed, is committing, or will soon commit a crime. Statistics compiled by the NYPD in Table A.1 show that over 50% of the SQF stops recorded between 2006 and 2012 involve black suspects, despite that group making up only a quarter of the metropolitan population. In 2011 alone, 168,126 SQF encounters involved black men between the ages 14-24, a striking number when you take into account that only 158,406 black men ages 14-24 reside in the city.[1]

Table 1.1 gives a more detailed description of the racial composition of NYPD activities. That table calculates the relative stop and search rates of blacks and whites in NYC over the period 2006-2012. It shows that even at the quartile of precincts that display the highest degree of racial equity in stop targets, when compared to their relative population shares African-Americans are still stopped seven times more often than whites and are searched eight times more often. These statistics indicate that police not only stop blacks more intensively than whites (relative to their local population shares), but also that the disparity persists throughout the city and is not merely a symptom of black neighborhoods being policed more heavily. On

[1] Data supporting these figures come from NYPD's SQF database and the decennial census

average, the number of African-Americans stopped each year is equivalent to 22% of the black population in each precinct, compared to a rate of less than 2% for the white population. In several of the most heavily policed precincts, total stops of blacks men [annually] exceeded the black male population in that area.[2]

Table 1.1: Distribution of Sampling Rates

	Stops by Race / Population by Race			Searches by Race / Population by Race		
Quartile	Black	White	**Blk/Wht**	Black	White	**Blk/Wht**
25%	.0756	.0098	**7.674**	.0071	.0006	**7.73**
50%	.1496	.0126	**10.73**	.0118	.0010	**11.69**
75%	.2359	.0194	**18.16**	.0223	.0016	**21.23**
Mean	.2221	.0154	**16.28**	.0195	.0013	**18.08**
Std. Dev.	.4030	.0141		.0315	.0015	

The 25% group represents the upper bound for the least intensively stopped/searched quarter of precincts, 75%

The existing mass of literature on the subject of racially biased policing has sought to determine whether disparities like those just mentioned are symptomatic of relatively innocuous statistical discrimination or are instead the result of more nefarious taste-based preferences exhibited by police. Several papers, most notably Knowles, Persico & Todd (2001), have shown statistical bias to be consistent with effective policing by officers whose objective function is to maximize the number of guilty individuals apprehended.[3] On the other hand, a multitude of studies dating to Becker (1957) imply that the same is not true when taste-based preferences uncorrelated with guilt enter an officer's objective function.

This paper mirrors previous work in the field by examining if and how taste-based preferences may affect police officers' selection of suspects while also ex-

[2] New York was divided into 76 police precincts until 2013, when it was expanded to 77 precincts. This was due to the addition of the 121^{st} precinct on Staten Island, which absorbed parts of both the 120^{th} and 122^{nd} precincts

[3] Knowles, John, Persico, Nicola, and Todd, Petra (2001). *Racial bias in motor vehicle searches: theory and evidence*. Journal of Political Economy 109(1): 203 - 229. This paper will be referenced as KPT hereafter.

panding upon previous work by investigating the impact intensive and potentially discriminatory policing has on the behavior of suspects stopped by police. This extension is motivated by the fact that over the past decade (2003 - 2013) police recorded SQF encounters with nearly 5 million New Yorkers, 4.4 million of whom they later deemed innocent of any crime. More specifically and relevant to this paper, NYPD data shows that from 2006-2012 officers initiated 996,444 stops based on the suspicion that the target was illegally carrying a weapon. Those stops predominantly interdicted young black men; a total of 605,121 blacks were subsequently searched looking for weapons and police successfully recovered weapons 2.31% of the time. The obvious benefit of those searches were the nearly fourteen thousand weapons taken off the street. The costs are more difficult to quantify without a way to directly measure the collective psyche of the more than half a million black men who were erroneously detained and informed that police suspected them of being an armed threat. Even without regard to race, one could still very reasonably expect some detriment to the relationship between citizens and their police force when so many innocent individuals are being targeted. Data supporting this expectation show that SQF stops and civil rights claims against the City of New York jointly peaked in 2011, when the city paid $84 million to settle 3,080 citizen claims. Figure 1.1 more clearly illustrates the joint rise in SQF encounters and legal claims against the NYPD over the past decade.

Qualitatively it is easy to understand how racially biased policing adversely effects minority groups. It is more difficult to quantify the costs society incurs as a result. Durlauf (2006) responds to the question of whether racial profiling significantly impacts the group subjected to profiling by writing that there is a “clear violation of fairness that occurs in that innocent blacks are stopped and searched more frequently than innocent whites." At the same time, he points out that “there is no body of evidence that quantifies how racial profiling affects levels of racial stigma or how racial stigma affects African Americans. This of course does not mean that the costs should be assumed to be negligible."

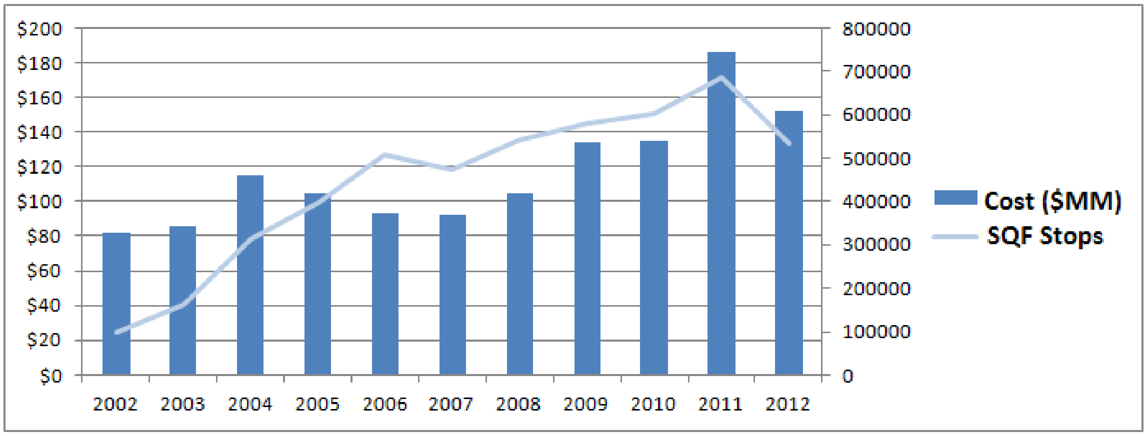


Figure 1.1: Annual SQF totals & the City of New York's payments to cover judgments and settlements related to claims against the NYPD. Source: (Liu, 2012)

1.2 Contribution Beyond the Existing Literature

The contemporary literature on taste-based discrimination in policing owes a great deal to the work of KPT. Those authors sought to determine whether or not taste-based racial bias existed in the search decisions made by police on a particular stretch of I-95 running through Maryland. To motivate their results the authors introduced an innovative model that jointly endogenized suspect guilt probability as a function of police search intensity and police search intensity as a function of suspect guilt probability. The joint endogeneity generated discontinuous best-response functions that allowed researchers to to proxy average search success rates, a statistic they could measure using data, for marginal search success rates, which are unobserved by the econometrician. Theory supposes that police are searching optimally when the marginal benefit of searching individuals equates across types, therefore KPT's contribution was creating a framework within which researchers could compare average search success rates across searched groups to determine if officers exhibited bias, evidence of which would be average guilt rates that differed along some observable attribute.

KPT's approach was somewhat limited in its power to detect police bias, I propose, because the interaction those researchers were modeling described a highway stop. When police stop a motorist on the highway, what ensues is a three-part interaction with a single decision problem for the police officer. A highway stop is first precipitated by some traffic offense that leads a police officer to stop the offending vehicle. Once the vehicle stops, the officer observes characteristics of the driver and engages in a short interrogation. Finally, based upon the information the officer has gathered, he or she decides whether to search the driver.

Grogger & Ridgeway (2006) offer evidence confirming that race, as well as other driver observables, are not immediately available to officers prior to a traffic stop. As such, the decision an officer makes to search a suspect's vehicle coincides with the officer learning (with certainty) the race of the individual they suspect of criminal activity.

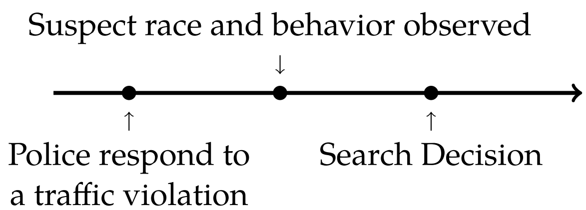


Figure 1.2: Diagram of a typical traffic stop

The obvious concern is that what might appear in the data as a bias against drivers of a particular race is actually a bias associated with some attribute or behavior highly correlated with race. While the distinction is nuanced, I demonstrate in this paper that it is important.

My work to further illuminate the factors affecting the unobserved interaction between police and suspects follows others such as Antonovics & Knight (2004). They were among the first to include determinants of a police search beyond the suspect's race, matching officer race to suspect race for every traffic stop made by officers of the Boston Police Department for two years. They point out that in the KPT model, results should not vary by officer race. However when they do allow heterogeneous officer behavior by race they find that officers are more likely to search a suspect if that individual's race differs from their own.

A similar exercise was undertaken by Anwar & Fang (2006). Using data from the Florida Highway Patrol, Anwar & Fang attempt to replicate the results of Antonovics & Knight while using a slightly altered model. Their contention was that because the data from the Boston Police Department comes from local neighborhoods, officers may elicit different amount of information from suspects belonging to different racial groups. If a black police officer has more intimate knowledge of the local black community, this information as well as the suspect's response to an officer of his own race could introduce statistical bias that would then be misinterpreted as a taste-based preference.[4] While both of these studies have added valuable insights into the effect of officer race on police interactions, those insights are still based on the necessary modeling assumption that suspect responses are either homogeneous or perfectly correlated with guilt. Without one, or both, of these assumptions, the studies just mentioned would been unable to uniquely identify officer bias. This paper's focus on proactive policing makes it the first to uniquely identify the roles race and suspect response play in these interactions.

This contribution is due in large part to changes in contemporary police tactics. Within the past decade several large police departments, notably New York, Philadelphia and Los Angeles, have adopted proactive pedestrian policing programs.

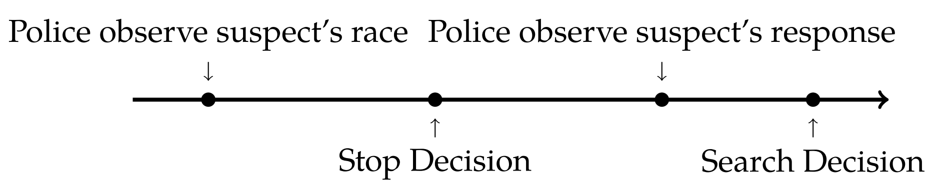


Figure 1.3: Diagram of a proactive policing regime

[4] This references an idea first introduced by Hernandez-Murillo & Knowles (2004)

Proactive policing equates to a four-stage interaction framework between police and citizens. In the first stage of the interaction police patrol on foot, observing citizens' race, appearance and behavior. In the second stage police stop citizens whose observable attributes generate a reasonable suspicion that they are involved in criminal activity. In the third stage, police question these citizens [who may now interchangably be referred to as suspects] and receive responses. Finally, officers decide whether to search suspects. Under this regime officers gain a second decision problem and therefore the revelation of race and suspect response are separated in time. I will show that, within certain limits, this allows researchers to separately identify the effect suspect responses have on police officers' decisions to conduct searches.

Data limitations have been another cause of the paucity of contemporary empirical work exploring the role of racial bias and suspect responses in policing. These barriers were a combination of technological contraints and police departments' general hesitancy to disseminate this information publicly. Widespread adoption of GPS technology in law-enforcement in the past decade has now begun to generate geographically-detailed bulk data, allowing researchers to link valuable information such as the demographic composition of the community immediately surrounding the stop location and even the number of stops occurring at a particular location in a given time period that could not previously be aggregated on the scale necessary for serious analysis. Once collected, those records had generally not been shared publicly and have only recently entered the academic arena, largely due to consent decrees with the U.S. Justice Department and judicial actions in cases alleging civil rights violations. Individual-level stop data from the NYPD was unavailable prior to 2008, when U.S. District Judge Shira Scheindlin required the NYPD to make public all stop-and-frisk data from 1998 to present as part of an agreement reached in the case *Floyd et al. v. City of New York*.[5] Now available,

[5] This followed Justice Marylin G. Diamond of New York State Supreme Court, who had asked the NYPD to release publicly its electronic database of stops from 2006 - 2007 in response to a lawsuit filed by the New York Civil Liberties Union. Andrew G. Celli Jr., a lawyer for a group of

I show in this paper exactly how precise stop location data provides contextual information that is pivotal in identifying suspect responses.

1.3 Model

Players and Payoffs

The model is based on interactions between a continuum of police officers and citizens within distinct geographical areas (precincts).

Police Officers

Police are modeled as homogeneous agents and act non-cooperatively within one of N autonomous precincts. Each officer comprises an insignificant part of the police force within a precinct so as to have a negligible marginal impact on the aggregate stop and search rates recognized by suspects. Each police officer seeks to maximize the number of criminals possessing contraband they stop/search.

Citizens

Citizens, who are also referred to as suspects at various points in this paper, are modeled as risk-neutral utility maximizers of racial composition $r \in \{W, B\}$, signifying that one is either white and black. In addition to race, individual citizens are also a combination of the following attributes:

- o, characteristics observable to both the econometrician and the police officer
- u_1, unobservable to the econometrician but observable to the police

academics seeking the access to the released data, said of the ruling: “This is the first time that data will be made available to scholars on an independent and autonomous basis to determine whether racial profiling is occurring.âŁž (Hauser, 2008)

- u_2, unobservable to the econometrician and only observable to police after a suspect has been stopped and questioned [6]

Each citizen maximizes utility over a choice of whether or not to carry contraband. If individuals choose to carry contraband there is an additional choice between the type of contraband to carry. This model generalizes two types of contraband: Type I contraband, which can be uncovered with complete certainty whenever a suspect in possession is stopped, and Type II contraband, which cannot be detected during a stop but will be uncovered 100% of the time a suspect in possession of it is searched. Thus, to recover Type II contraband an officer must first stop a suspect, question them, then subsequently search the suspect; an act which recovers contraband with absolute certainty when it is present. The contraband carrying decision is a function of the associated costs and benefits intrinsic to each type of contraband, all of which are race and characteristic specific as well as strictly positive.

In this model contraband is defined to be a perishable, single-use good so both the costs and benefits of carrying are determinate according to the following functions:

- $\alpha(o, u, r)$ is the benefit of carrying Type II contraband
- $\beta(o, u, r)$ the cost of being caught carrying Type II contraband
- $\omega(o, u, r)$ is the benefit of carrying Type I contraband
- $\phi(o, u, r)$ is the cost of being caught carrying Type I contraband

Officer's Choice

Police attempt to maximize contraband recoveries by choosing which types of citizens they will stop and which they will subsequently search. Officers may only search suspects within the precinct to which the officer is assigned.

---

[6] Whenever both u_1 and u_2 are arguments of a function I will use $u = (u_1, u_2)$ to denote the pair.

Police Officers' Utility Police are solving the following two-period utility maximization problem:

$$\max_{\substack{1_{stop} \\ 1_{search}}} \{W(o, u_1, r) + 1_{stop} 1_{search} W'(o, u_1, r)\} \tag{1.1}$$

$$W(o, u_1, r) = 1_{stop}[P(I|o, u_1, r) - C_r \lambda_r] \tag{1.2}$$

$$W'(o, u_1, r) = \int P(search|stop)[P(II|o, u, r) - C_r] df(u_2|o, u_1, r) \tag{1.3}$$

$$1_x = \begin{cases} 1 & \text{if x occurs} \\ 0 & \text{otherwise} \end{cases}$$

Police gain a benefit (normalized to 1) when they recover either type of contraband. Thus, the payoff to stopping a suspect is the probability that the officer recovers contraband; specifically $P(II|o, u, r)$ is the probability that a citizen of type r with characteristics $\{o, u\}$ is carrying Type II contraband and $P(I|o, u_1, r)$ is the analog for those carrying Type I contraband.[7] Both stops and searches have associated costs; here $C_r \in (0, 1)$ is the cost of searching a person of race r. Empirically, C_r is the cost of establishing 'probable cause' for a search. This is the highest burden that must be met by police and is required for any search. A lower standard of evidence, 'reasonable suspicion' is sufficient to stop a suspect but not to search. The difference between these evidentiary standards may also depend upon race and is reflected by the the parameter $\lambda_r \in (0, 1)$.

It is important to note that officers, even those who exhibit taste-based racial preferences, do not gain utility from stopping or searching suspects directly. As can be seen in the officers' objective function, stops and searches only benefit officers when they yield contraband. Representing bias through differential search costs,

[7] Guilt can either come from Type I or Type II contraband possession, but not both. The object $P(I|o, u_1, r)$ should be read as "the probability of Type I contraband possession given an individual's observable characteristics, unobservable characteristics (u_1) and race".

rather than a positive utility term for searching particular groups in the officers' objective function, is a standard modeling assumption in the literature. In the case of racially biased policing, the efficient outcome would be for police to stop suspects whenever the likely benefit exceeds the cost. If, however, the costs of stopping or searching a suspect vary according to race, then we would expect to see some groups being pursued by police more aggressively than others. As I present the rest of the model it will become clear how this result can be applied as a test for taste-based officer preferences.

Citizen's Choice

As officers decide which citizens to stop and which suspects to search the choices made by individual police officers will aggregate into the rates $\gamma_{stop}(o, u_1, r)$ and $\gamma_{search}(o, u, r)$. For clarity, $\gamma_{stop}(o, u_1, r)$ is the rate at which police stop members of group r with observable characteristics o and unobservable characteristics (to the econometrician) u_1. Similarly, $\gamma_{search}(o, u, r)$ is the aggregate rate at which police officers choose to search individuals of race r, observable characteristics o and unobservable characteristics u where $u = (u_1, u_2)$. For economy, these objects will be referred to as γ_{stop} and γ_{search} unless other notation is necessary for expositional purposes.

Citizens' Utility Given costs, benefits, the rate at which they are stopped and the rate at which they are subsequently searched, citizens will expect to see the following payoffs when choosing to carry the illegal items previously described:

$$\mathbb{E}[\mathbb{I}] = [1 - \gamma_{stop}]\omega(o, u, r) - \phi(o, u, r)\gamma_{stop} \tag{1.4}$$

$$\mathbb{E}[\mathbb{II}] = \alpha(o, u, r) - (\alpha(o, u, r) + \beta(o, u, r))\gamma_{search}\gamma_{stop} \tag{1.5}$$

Since the outside option of not carrying contraband yields no benefit, citizens will choose to carry whenever either of the above two equations are greater than or equal to zero. If both provide non-negative expected profits, agents choose the action which results in the larger expected payoff.

Timeline

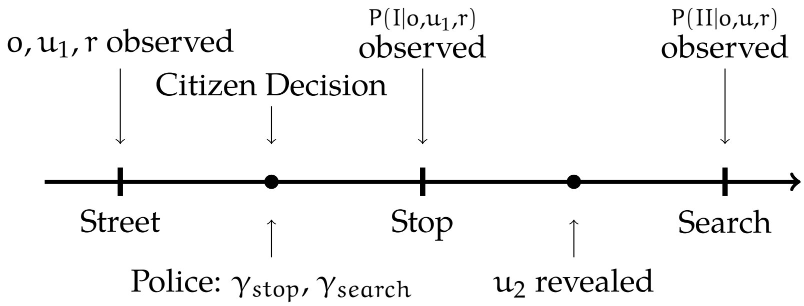
T=0 Citizens know their own attributes as well as the distributions of (o,u,r). They also know the costs and benefits associated with carrying contraband as given by the functions ω, ϕ, α and β. Police learn the distributions of attributes and learn the values of ω, ϕ, α and β up to the distribution of unobservable attributes.

1^{st} Police and citizens simultaneously act. Police choose the rates at which to stop and search individuals of a particular race and characteristic profile (γ_{stop} and γ_{search}) while citizens decide whether to carry contraband of Type I, Type II or possibly not at all.

2^{nd} Those who are stopped by police reveal whether or not they are in possession of Type I contraband ($P(I|o, u_1, r)$). This will reveal the frequency of guilt for each racial group upon the occurrence of a police stop, denoted as $D(r)$.

3^{rd} Individuals reveal to police the unobservable (to the econometrician) characteristic u_2. These are their responses to questioning during the law enforcement stop. Based on the γ_{search} previously arrived at, individuals will either be searched or released from custody.

4^{th} Finally, after a search, the guilt or innocence of an individual thought to be carrying Type II contraband is revealed ($P(II|o, u, r)$). Collectively this will be tabulated as $G(r)$, the frequency of guilt among searched suspects of race r.



Player Interaction

Plotting suspect decisions across rows and police actions across columns the payoff matrix associated with this model appears as follows:

	$\gamma_{stop} = 0$	$(\gamma_{stop} = 1, \gamma_{search} = 0)$	$(\gamma_{stop} = 1, \gamma_{search} = 1)$
Type I	$(\omega(o, u, r), 0)$	$(-\phi(o, u, r)\ , 1 - C_r\lambda_r$	$(-\phi(o, u, r)\ , 1 - C_r\lambda_r)$
Type II	$(\alpha(o, u, r), 0)$	$(\alpha(o, u, r), -C_r\lambda_r)$	$(-\beta(o, u, r), 1 - C_r\lambda_r - C_r)$
No Carry	(0,0)	$(0, -C_r\lambda_r)$	$(0, -C_r\lambda_r - C_r)$

Figure 1.4: Normal-form representation of model payoffs.

Equilibrium

Ignoring probability one events, the model has a unique (interior) perfect bayesian nash equilibrium. That equilibrium is fully characterized by a contraband carrying decision, as well as stop and search rates, for any and all partitions of the population delineated by some combination of the parameters o, u_1, u_2, and r.

By ignoring probability one events, we do not consider the degenerate example of police who stop every black citizen or certain groups who never/always carry contraband regardless of how intensively they are stopped by police. The purpose of this model is to describe the dynamic relationship between police and suspects, consequently there is no compelling reason to discuss an equilibrium that includes actions that occur with certainty as it does not depend upon any strategic interaction.

The model offered here can and will be modified in future work. Applications that would warrant relaxing and/or altering the modeling assumptions include situations where officers' costs also include possible disciplinary actions or the liklihood of being sued and where those costs vary by race. These alternate assumptions are given considerable treatment in the conclusion of this paper.

Another possible extension describes a situation where groups face differential likelihoods of apprehension by police. In the model I discuss here such contingencies are ignored in favor of expositional clarity.

Characterizing the Equilibrium

Intuitively, if any identifiable group of suspects *always* carried Type II contraband, police would *always* wish to search those individuals, however that cannot be an equilibrium since no group that will be searched with absolute certainty would ever find it rational to carry Type II contraband. Exercising similar lines of logic and looking at Figure 1.4 will show that neither pure strategies by police (always search, never search) nor by citizens (always carry contraband, never carry contraband) can be a part of an equilibrium.

Solving recursively from the second stage without γ^*_{stop} pinned down, we can see that individuals will be indifferent between carrying Type II contraband and carrying none at all when police search according to γ^*_{search} derived by setting the expected payoff of carrying Type II contraband equal to zero:

$$
\begin{aligned}
0 &= \alpha - (\alpha + \beta)\gamma_{\text{search}}\gamma^*_{\text{stop}} \\
\implies \gamma^*_{\text{search}} &= \frac{\alpha}{(\alpha + \beta)\gamma^*_{\text{stop}}}
\end{aligned}
\tag{1.6}
$$

Moving backwards to the initial stage of interaction, the stop rate that yields citizens indifferent between carrying Type I contraband and not carrying contraband is calculated via:

$$[1-\gamma_{stop}]\omega(o,u,r)-\gamma_{stop}\phi(o,u,r)=0$$

$$\implies \gamma^{*}_{stop}=\frac{\omega(o,u,r)}{\omega(o,u,r)+\phi(o,u,r)} \tag{1.7}$$

So long as there is a cost and a benefit associated with flagrantly carrying contraband ($\omega(o,u,r)>0$ and $\phi(o,u,r)>0$) then police must randomize stops according to condition (1.7) in any equilibrium.

Meanwhile, for police to be willing to randomize searches the expected benefit to searching any group (who is searched with positive probability at equilibrium) must be the same across all groups. Explicitly, this means the following must hold:

$$P^{*}(I|o,u_1,r)=C_r\lambda_r \tag{1.8}$$

$$P^{*}(II|o,u,r)=C_r \tag{1.9}$$

Notice that because the *ex ante* expected payoff from a search is necessarily zero, officers need only weigh the costs and benefits associated with pursuing Type I contraband in the first stage of interaction. Together equations (1.6)-(1.9) characterize the equilibrium for this model.

1.4 Testable Implications of the Model

In the case of SQF, C_r represents the cost or 'burden' of establishing *probable cause* that a crime has been committed, is in the process of being committed, or is about to be committed by someone of race r.[8] If this burden of proof is the same irrespective of race, that indicates officers do not harbor taste-based preferences. If, however,

[8] Probable cause is defined by *Ballentine's Law Dictionary* as "a reasonable amount of suspicion, supported by circumstances sufficiently strong to justify a prudent and cautious person's belief that certain facts are probably true"

officers find the cost of searching otherwise identical individuals who differ only along dimension r to be unequal, this indicates a taste-based racial preference on behalf of the officer. Such a scenario defines **primary racial bias** in this model:

- **Police exhibit primary racial bias if $C_r \neq C_{r'}$ for any r, r'.**

This definition is in contrast to statistical racial bias, which occurs if $\gamma_{search}(o, u, r)$ is not equal to $\gamma_{search}(o, u, r')$.[9] Given equations (1.6) and (1.7), the probability that any individual who is searched will be found in possession of contraband is given by $G(r)$. This is the frequency of guilt conditional on search and is formulated as:

$$G(r) = \int P(II|o, u, r) \frac{\gamma_{search}(o, u_1, u_2, r) f(u_2|o, u_1, r)}{\int \gamma_{search}(t, v, w, z) f(w|t, v, z) dt} du_2 \tag{1.10}$$

While the frequency that an individual is stopped and is immediately found to be carrying contraband is given by:

$$D(r) = \int P(I|o, u_1, r) \frac{\gamma_{stop}(o, u_1, r) f(u_1|o, r)}{\int \gamma_{stop}(v, w, t) f(w|v, t) dw} du_1 \tag{1.11}$$

If one were to assume that police were racially unbiased it would be that case that $C_r = C_{r'} = C$, allowing C to replace $P(II|o, u, r)$ in (1.10), which reveals that in an equilibrium where police harbor no racial bias:

$$G(r) = C = G(r') \tag{1.12}$$

This equality provides a testable proposition with which to identify racial bias:

Proposition 1: **Police Officers harbor a primary racial bias if $\frac{G(r)}{G(r')} \neq 1$.**

Clearly, officers would exhibit taste-based racial bias if different races were judged against unequal standards of guilt when police decided which citizens to search.

---

[9] These definitions are parallel to those offered by KPT.

Assuming this model has been correctly specified, *Proposition 1* provides a testable condition analogous to that offered by KPT. However, in this model, there exist two sources of potential bias; not only can search costs vary by race, but so too can stop costs. Consider first the totally unbiased equilibrium where reasonable suspicion and probable cause mean the same thing to officers regardless of suspect race. In that case, $C_r = C_{r'} = C$ and $\lambda_r = \lambda_{r'} = \lambda$. Under these assumptions, equation (1.12) still holds, but in addition the following will also be true:

$$D(r) = C\lambda = D(r') \tag{1.13}$$

This result can then be used to form the test statistic Δ:

$$\Delta = \frac{D(r)}{D(r')} - \frac{G(r)}{G(r')} \tag{1.14}$$

It may be the case that officers recognize the same costs regardless of race when considering the evidence necessary to establish probable cause, but that establishing only a reasonable suspicion may be relatively cheaper for some group. In this situation, which I term **situational racial bias**, we would expect to find $\Delta \neq 0$.

The converse may also be true. One could a case where an officer exhibits primary racial bias but that the officer applies the law in a uniform manner. That is to say, the evidentiary cost difference between probable cause and reasonable suspicion is invariant across races. In such a situation, we would find both $\frac{D(r)}{Dr'} \neq 1$ and $\frac{G(r)}{Gr'} \neq 1$, while still expecting to find $\Delta = 0$.

Proposition 2: **Police Officers exhibit a situational racial bias if** $\frac{D(r)}{D(r')} - \frac{G(r)}{G(r')} \neq 0$

1.5 Findings

To evaluate these three propositions empirically I use data sourced directly from the New York City DataMine. This portal provides data from SQF stops conducted from 2003 through 2013, though I restrict my sample to 2006-2012 due to a lack of geographic detail in the omitted years. The unit of observation is an individual stop, which is recorded by officers on a department standard UF-250 form. For each stopped individual under the aegis of the SQF program it is expected that one UF-250 will be submitted. The complete record of these forms amounts to 3,898,844 distinct police encounters, including over one hundred variables for each unique encounter. I further limit my sample to include only stops of black or white suspects. Finally, I throw out any records with incomplete geocodes or obviously innaccurate geospatial information, leaving a total of 2,361,590 data points.[10]

A possible data complication arises from the fact that UF-250 forms do not identify when contraband was recovered, only if it is recovered. It is most likely that officers find contraband and then arrest the criminal who possessed it. It is also possible that police arrest a suspect for some unrelated crime and recover contraband while taking the suspect into custody (which may require a search independant of any suspicion that the individual possessed such items). Figure A.7 indicates that the majority of stops progress first with a frisk (roughly 53% of stops) and then, if the evidence supports it, a suspect may be subsequently searched (roughly 15% of stops involving a frisk, 8% of all stops).

Frisks are an important tool for establishing the probable cause necessary to initiate a search. Due to the fact that frisks are essentially costless conditional on a stop one would expect nearly all searches to be preceded by a frisk. In fact, I observe that vast majority of searches (95%) occur only after a frisk and less than .5% of stops include a search not accompanied by a frisk. Nonetheless, these searches still account 3,005 contraband recoveries, 6% of total recoveries, and have a much higher success rate (26% of searches not preceded by a frisk recover contraband) than searches not paired with a frisk. This suggests that when a suspect is searched but not frisked it is likely the suspect was searched while being taken into custody and contraband was recovered as a result of the arrest as opposed to the inverse.

[10] An example would be if an officer reported that the stop occurred in precinct X but the associated geographic coordinates place the encounter in precinct Y.

I address the endogenous relationship between arrests and contraband recoveries by including arrests as an additional dependent variable in a bivariate probit specification.[11]

To match each stop with the demographics of the neighborhood it occurred within, I use data from the 2010 Census. That file provides population information at the census block level, as well as a rich set of demographic variables at the census tract level which I then aggregate up to the police precinct level using a geospacial mapping program. Several authors, notably Gelman, Fagan & Kiss, have pointed out that census population counts likely do not accurately reflect the racial composition of suspects observed by police in a given location. To account for daily migration those authors employed census journey files to study police bias in those selected for stop. I agree that the racial mix of the residential population may differ from the population available for police interdiction, however I believe the 2010 census estimates are the best alternative here for several reasons. The primary reason is that the commuter-adjusted population estimates offered by the 2006-2010 American Community Survey (ACS) are meant to reflect inter-boro commutes and are thus too widely aggregated to be useful in this study, which analyzes precinct-level estimates. [12] A second is that individuals commuting to work or visiting the city as a tourist are unlikely criminal suspects; the 2006-2010 ACS shows that Manhattan sees a large population influx across all races during daylight hours, however the majority of police stops in that borough occur at night.[13] At the very least, it is unclear whether we would expect criminals to follow commuting patterns of non-criminal workers or will instead populate a criminal contraflow lane heading towards temporarily unattended property. For these reasons I avoid using the Census Bureau's journey to work commuting estimates.

[11] See Table 1.4

[12] For instance, Manhattan is comprised of more 20 police precincts. See Figure A.1 to see the mapping of precincts to boroughs. The prevelence of SQF stops in each precinct varies greatly within boroughs.

[13] 61.15% of stops in lower Manhattan and 63.75 of stops city-wide occurred between 6 p.m. and 6 a.m.

Tests for Primary and Situational Bias

I begin investigation under the assumption that police exhibit no bias whatsoever. This provides two null hypotheses:

- Police exhibit no primary racial bias

$$H_0 : G(B) = G(W) \tag{1.15}$$

- Police exhibit no situational racial bias

$$H_0 : \Delta = \frac{D(r)}{D(r')} - \frac{G(r)}{G(r')} = 0 \tag{1.16}$$

Hit rates $(D(B), D(W), G(B), G(W))$ are calculated annually at the precinct level. The NYPD is divided into 76 precincts and I utilize seven years of data from 2006 - 2012, so this comprises a total of 532 data points. The hit-rate analysis in Table 1.2 measures the average hit rates, conditional on race and whether a search was conducted. It indicates that police find contraband on black citizens they stop nearly 23% less often than they do on whites. When officers search suspects they find contraband on black citizens 21% less often when compared with whites.

Noting that the difference $G(W) - G(B) = .0376$ is highly significant, the null hypothesis that police exhibit no primary racial bias can be rejected. The test statistic Δ is found to have an expected value (.0158) not significantly different from zero. This indicates we cannot reject the null hypothesis that officers display no situational bias across all precincts. Precincts, however, are modeled as autonomous regions, so given that the population of officers in each is unique it is appropriate to look for patterns of between-precinct variation in Δ.

Figure 1.5 is a collection of box and whisker plots representing the sample distribution of Δ in each NYPD precinct. The box represents the interquartile range for the annual observations of Δ in each precinct and the whiskers mark adjacent

Table 1.2: Guilt Frequencies are the observed rate of contraband recovery by race from 2006-2012

Hit Rate	Mean	Std Err	95% Confidence	
$D(B)$	.0187	.0004	.0179	.0196
$D(W)$	.0244	.0008	.0278	.0259
Difference	.0056***	.0006	.0044	.0069
$G(B)$	.1396	.0031	.1335	.1456
$G(W)$	.1772	.0051	.1673	.1871
Difference	.0376***	.0046	.0285	.0466
Δ	.0158	.0219	-.0271	.0587

values.[14] Dots represent outliers in the sampling distribution of Δ. Notice that 69 of the 76 plots have observations of Δ on both sides of zero and 64 have zero within their adjacency interval. Figure 1.5 shows the high variablity associated with small samples of white suspects in many precincts/years, but does not suggest that heterogeneity across precincts is masking the systemic deviations in Δ that would result from localized patterns of situational bias.

For a more descriptive analysis I run probit regressions of contraband recoveries on race, including relevant covariates. Several specifications were run and the main results are presented in Table 1.3. Those estimates were all generated by a probit model of the same general form (with indicies suppressed for notational clarity):

$$Contraband = \beta_1 X_1 + \beta_2 X_2 + \beta_{12}(X_1 X_2) + \mu_T X + \epsilon \tag{1.17}$$

Here X is a vector of binary and categorical regressors.[15] It is important to note the

[14]The lower adjacent value satisfies the inequality $x_i \geqslant L$ and $x_{i-1} < L$, where L is defined as $x_{25} - \frac{3}{2}(x_{75} - x_{25})$. The upper adjacent value satisfies the inequality $x_i \leqslant U$ and $x_{i+1} > U$, where U is defined as $x_{75} + \frac{3}{2}(x_{75} - x_{25})$.

[15]These include: age, gender, location, local demographics (racial composition of the surrounding

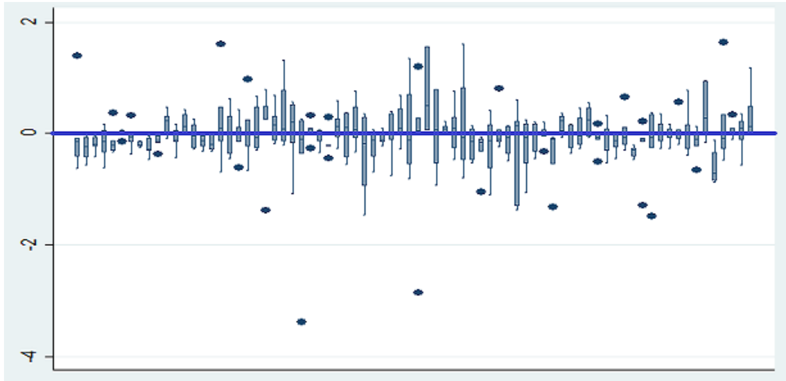


Figure 1.5: Box-and-whisker plots of 95% confidence intervals around 76 precinct-level estimates of Δ. Each plot represents seven annual measurements from 2006-2012 in a given precinct, with precincts presented in ascending order along the horizontal axis.

non-linear model presented here $X_1 = \mathtt{Black}$ and $X_2 = \mathtt{Searched}$. The coefficent on their interaction term, β_{12}, does not directly map to the change in the relative probability of recovering contraband due to situational bias applied by police. The reason for this is the test statistic used in this study, Δ, is a difference in ratios, while β_{12} measures differences in levels.

Thus, β_{12} cannot directly measure Δ, however $\beta_{12} \neq 0$ necessarily implies that the marginal effect of $\Delta \neq 0$. Specifically, $\beta_{12} \neq 0$ implies:

census-block (CB), mean income level of the surrounding census-tract, population density (CB), precinct-level stop-intensity (by race), change in the precinct-level number of stops from previous year), year and precinct dummies, as well as contextual descriptors (time of day, whether the officers identified themselves, number of suspects, period of observation, reason for stop, suspected crime, whether a frisk/search occurred, whether force was used, whether furtive movements were observed, whether the officer was in uniform and whether or not a frisk or search occured, whether additional suspects were present during the stop, and the difference in the precinct's total number of stops relative to the previous year)

$$\Phi(\beta_1 + \beta_2 + \beta_{12} + X\mu_T) - \Phi(\beta_1 + X\mu_T) - \Phi(\beta_2 + X\mu_T) + \Phi(X\mu_T) \neq 0 \quad (1.18)$$

Equation (1.18) measures the marginal effect of the interaction term for two binary regressors; it is the non-linear analog for a linear difference in difference regression coefficient. While values of β_{12} not equal to zero indicate situational bias is present, one cannot infer the degree of bias from the marginal effect associated with that coefficient.

The first line in Table 1.3 shows the coefficient associated with being black and the second line is the associated marginal effect. This is the marginal change in the probability of carrying either Type I or Type II contraband for black suspects (relative to whites). Being black is associated with lower relative probability of an officer recovering contraband in a stop with estimates that range between 0.8 and 1.3 percentage points lower relative to what the probability would be had the same citizen been white. The estimated unconditional probability of contraband recovery when facing a white suspect is roughly 3.5%, so being black is associated with a relative decrease in the probability of recovering contraband of between 23% - 33%. These estimates in line with the estimates in Table 1.2 and are just slightly larger than the linear estimates in Table A.4, which range from 17% - 26% and include the same covariates. Finally, looking at Table 1.4 I still find that even if jointly estimated with the probability that an arrest is made, estimates for the black guilt differential ($\frac{8}{10}$%) are roughly in line with the other estimates and indicate there is good reason to believe arrests are not confounding the results I present in Table 1.3.

The results under the alternative model where police pursue arrests rather than contraband I find results very similar to Coviello & Persico. Table A.3 shows that in specifications without precinct dummies the marginal effect of being black is negative and significant, with a marginal effect of .66 percentage points. Once precinct dummies are added to the model the sign flips to positive and we see that being black significantly increases the probability of arrest with an associated

Table 1.3: Probit Regression (Y = Contraband Recovery) w & w/o Precinct Controls

	Without Precinct Dummies				With Precinct Dummies			
	Radio		No Radio		Radio		No Radio	
Regressor	β	S.E.	β	S.E.	β	S.E.	β	S.E.
Black	-.2068***	.0068	-.2442***	.0088	-.2625***	.0079	-.2862***	.0101
$\frac{\partial Contraband}{\partial Black}$	-.0096	.0003	-.0116	.0004	-.0121	.0004	-.0134	.0005
Search	1.480***	.0099	1.602***	.0131	1.498***	.1012	1.607***	.0134
$\frac{\partial Contraband}{\partial Search}$	.0687	.0005	.0758	.0006	.0688	.0005	.0750	.0006
Black × Search	-.0025	.0108	-.0552	.0141	-.0110	.0109	-.0532***	.0144
$\frac{\partial Contraband}{\partial Black*Search}$	-.0001	.0005	-.0026	.0006	-.0110	.0109	-.0025	.0006
Stop Ratio	-.0007***	.0052	-.0013***	.0002	-.0028***	.0005	-.0029***	.0007
$\frac{\partial Contraband}{\partial StopRatio}$	-.0001	.0005	-.0001	.0000	-.0001	.0000	-.0001	.0000
n	2,361,590		1,471,268		2,361,590		1,471,268	
McFadden's R^2	.2732		.2911		.2824		.3017	

This regression includes two specifications and two populations. The two specifications are with and without precinct dummies. The two sample populations are one with includes all 2,361,590 stops of black & white suspects from 2006-2012 and another which excludes radio runs, on-going investigations and reported crimes (n = 1,471,268). Radio runs are stops which include any police radio traffic requesting or reporting service. Covariates omitted from the table include F.E. dummies, year dummies, time of day, an age dummy, a dummy for whether the stop took place inside or outside and a dummy for whether the suspect was frisked. The variable "Stop Ratio" refers to the black-to-white stop ratio.[16]

marginal effect of .18 percentage points.

I also explore how race and racial inequality in who is stopped interact to test the validity of my model's structure and assumptions. Looking at the racial composition of those stopped by police, I run a probit regression similar to (1.17). The difference being that I omit precinct fixed-effects and instead include dummies indicating whether a stop occurred in one of the 19 most equitable precincts (smallest values of the relative black-to-white stop ratio), one of the 38 most equitable precincts, one of the 19 least equitable precincts (largest values of the relative black-to-white stop ratio), as well as the interaction of each of these terms with the indicator variable for whether a suspect is black. I report each first-order interaction coefficient generated by this model in Table A.6.

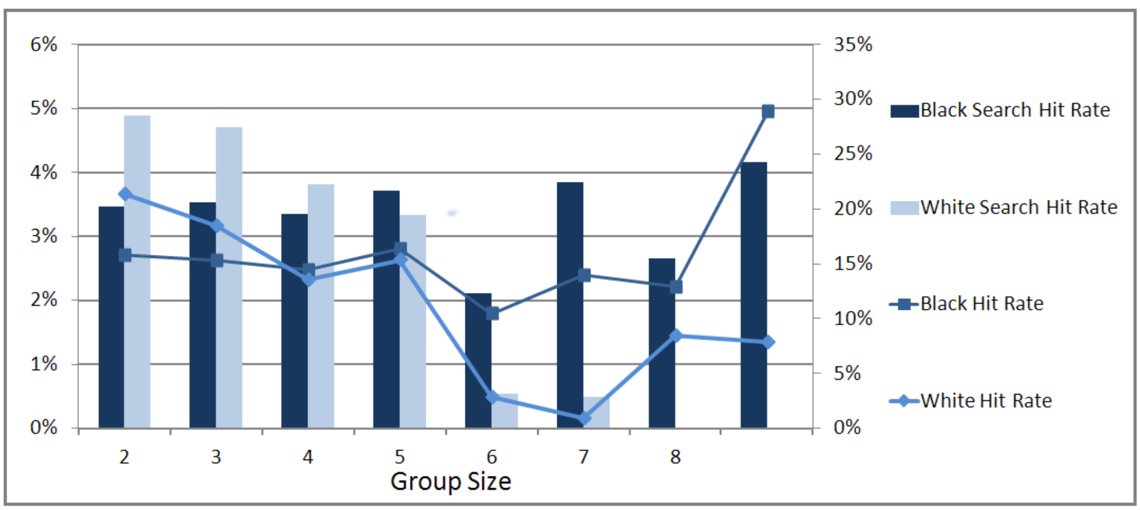
Table 1.4: Bi-variate Probit Model Parameter Estimates

	Y = Arrest Made		Y = Contraband Recovered	
Regressor	β	Std. Error	β	Std. Error
Black	-.0340***	.0075	-.2690***	.0086
$\frac{\partial Contraband}{\partial Black}$	-.0022	.0005		
Search	1.899***	.0121	1.604***	.0128
$\frac{\partial Contraband}{\partial Search}$	.4118	.0044		
Black × Search	-.0730***	.0129	-.0433***	.0139
$\frac{\partial Contraband}{\partial Black*Search}$	.0043	.0007		
ρ	.7389***	.0021		

This regression includes 1,471,268 stops of black & white suspects from 2006-2012 that do not include stops precipitated by an on-going investigation, victim report or included a radio run. Stops missing geographic identifiers were omitted. Covariates not reported include time of day, an age dummy, a dummy for whether the stop took place inside or outside and a dummy for whether the suspect was frisked

I find that relatively high (compared to whites) rates of sampling by law enforcement leads to lower probabilities of recovering contraband among black suspects. The marginal effect of being black and stopped in a precinct in the top half of black to white stop ratios the relative probability of guilt is significantly lower (Approximately 4% lower at the time of stop, 9% lower upon a search). Using upper and lower quartiles strengthens these results and support the central empirical finding of this paper; in areas that are policed most intensively, blacks respond to police interdiction in a way such that their relative guilt rates are much lower (relative to whites) than would be expected in an unbiased equilibrium. I also included interactions between whether the suspect was at least 25 years of age at the time of the stop, whether the suspect was part of a group stopped by police simultaneously, whether furtive movements were observed and whether the suspect was black. Table A.6 shows that older black men are significantly more likely to be guilty of carrying contraband (14% more likely relative to baseline), however the effect of being black and part of a group stop does not appear to significantly effect the probability of contraband recovery. Groups are of particular interest and I further investigate how the interaction between race and whether the individual was stopped as part of a group impacts the probability of recovering contraband in Table A.5. To do this, I first restrict my sample to the 443,827 UF-250 forms upon which the responding officer reported multiple suspects. I then grouped forms by location, date and time so that all stops occurring within 30 minutes of one another at the same location, on the same day, represent a single stop of N suspects. If any contraband is recovered during this stop, regardless of the amount or number of guilty suspects, I consider that encounter successful from the officers' standpoint. Regressing contraband recoveries on interaction term relating race and group size, as well as race, group size and whether a search occurred, I find small groups of black suspects are significantly less likely to be found in possession of contraband. Furthermore, the average marginal effect on contraband recovery for groups of two or three black suspects who are searched is significantly lower relative to the baseline.

Figure 1.6: Hit Rates for Group Stops, by Race



The left axis measures unconditional hit rates while the right axis measures hit rates conditional on a search occurring. A stop/search is considered a 'hit' if any member of the group is found in possession of contraband. Data comes from all stops including black or white suspects where reporting officers noted 'multiple suspects' on the UF-250 report accompanying the stop.

This is strikingly different than the results observed in the unrestricted sample, as the triple-interaction between group size, an indicator for suspect race and an binary search variable suggests that a small groups of black suspects are positively associated with police initiating fruitless searches. Figure 1.6 shows that this phenomenon is restricted to small groups of 3 or fewer suspects. A potential explanation may be that innocent black suspects respond more aggressively to police interrogation relative to white suspects when stopped among peers. The data shows that a pair of black suspects stopped together are much less likely to be in possession of contraband than a pair of white suspects, or put another way, relatively more innocent black pairs are stopped. One explanation may be that black suspects perceive this disparity to be racially motivated and take exception. If that, in turn, generates a disharmonious interrogation we would expect exactly these results: relatively more innocent black suspects being searched. So far only intuition supports the story that relatively intensive sampling leads to discord between police and members of the group intensively sampled, a hypothesis that will be tested more vigorously in the next section.[17]

Exploiting Spatial Variation

The previous section revealed that the test statistic Δ did not vary significantly from zero across all precincts and years. That result was shown to be inconsistent with officers' applying situational bias. That result, while somewhat illuminating in terms of officer bias, says nothing about suspect responses. In fact, the result that $\frac{D(r)}{D(r')} - \frac{G(r)}{G(r')} = 0$ when officers display no situational racial bias would in fact

[17] "The inclination to violence springs from the circumstances of life among the ghetto poor–the lack of jobs that pay a living wage, the stigma of race, the fallout from rampant drug use and drug trafficking, and the resulting alienation and lack of hope for the future....Simply living in such an environment places young people at special risk of falling victim to aggressive behavior....This is because the street culture has evolved what may be called a code of the streets, which amounts to a set of informal rules governing interpersonal public behavior, including violence. The rules prescribe both a proper comportment and a proper way to respond if challenged." - Anderson, Elijah. "The Code of the Streets". *The Atlantic Monthly*, Vol. 273, Issue 5, May 1994.

be expected regardless of how suspects respond to police interrogation. This is because all uncertainty over suspect responses has been endogenized in the officers' benefit function. Citizens make their contraband carrying decision prior to being stopped and have no opportunity to re-optimize once a stop is initiated. Similarly, officers already know the conditional distribution of possible responses when they initiate the stop, so they will simply adhere to a predetermined cut-off rule in the second stage. Rationality prevents the model from becoming fully dynamic.

To see this, simply assume suspect responses vary by race: $f(u_2|o, u_1, r) \neq f(u_2|o, u_1, r')$. So long as the conditional distribution of u_2 is known by officers and they incorporate this information into their conditional expectation of the suspect's ultimate guilt the only factor that can prevent guilt rates from equalizing at equilibrium is the officer's bias. One will always observe an equilibrium where a suspect's probability of being searched depends completely on his probability of guilt and officer bias. The probability of guilt cannot be altered by a suspect's response to interrogation since any information that response contains, by construction, is correlated with guilt.

In this section I propose a similar test for variation in δ within precincts. The purpose of imposing spatial constraints within precincts is to create conditions under which variation in Δ can be associated most certainly with suspect behavior. I offer a modification of the officers' second stage objective function such that the cost of searching decreases by some strictly increasing function $H(u_2(r))$:[18]

$$W'(o, u, r) = \int P(search|stop)[P(II|o, u, r) + (\frac{1}{H(u_2(r))})C_r]df(u_2|o, u_1, r)$$

In the event that $f(u_2|o, u_1, r) = f(u_2|o, u_1, r')$, this change will have no impact on the model. However if races respond differently to interrogation, *Proposition 2* will no longer be valid. This is because it is now possible to observe a situation

[18] Until this point, u_2 has been referenced as a parameter rather than a function. The change in notation here is merely for expositional purposes. I will use this notation only where u_2 enters another function as an argument.

where $\Delta \neq 0$ despite $\frac{\lambda_r}{\lambda_{r'}} = 1$. This is a pivotally important result, as it shows within-precinct variation in Δ necessarily implies variation in $f(u_2|o, u_1, r)$ across sampled groups.

Proposition 3: **For any partition** $a, -a \in$ **Precinct** A**;** $\Delta_a \neq \Delta_{-a}$ **necessarily implies** $\frac{f(u_2|o,u_1,r)_a}{f(u_2|o,u_1,r')_a} \neq \frac{f(u_2|o,u_1,r)_{-a}}{f(u_2|o,u_1,r')_{-a}}$

This result is somewhat limited in its scope. Proposition 3 says that if we take a precinct, draw a line through it, and compare all of the police-suspect interactions on one side of the line to those on the other side, we will be able to determine whether suspects are responding similarly to police interrogation. The reason is simple: within a precinct all of the officers are assumed to be homogenous. All officers exhibit the same degree of primary and situational bias. Ignoring the homogeneity assumption, the next most obvious Achilles's heel of this approach is that $\frac{f(u_2|o,u_1,r)_a}{f(u_2|o,u_1,r')_a} \neq \frac{f(u_2|o,u_1,r)_{-a}}{f(u_2|o,u_1,r')_{-a}}$ implies some groups will become inframarginal at the equilibrium. This is because officers will always prefer to search suspects with higher values of u_2, so given the inequity of the conditional distributions of u_2 across races, the average productivity of a search will no longer reflect the marginal productivity for these groups. We therefore cannot simultaneously say anything about racial bias on the part of police officers when comparing samples within precincts where suspect groups have been found to respond differently to police interrogation.

Proposition 3 implies that the variation in the distribution of suspect types within precincts can be used to identify suspect responses independently of officer bias. For instance, imagine a particular precinct borders affluent white neighborhoods on every side but one, where that border separates the precinct from a poor black neighborhood. If no reason exists to believe the officers assigned to this border are any different than the officers posted to the other borders, we would still expect those officers to potentially face a population of citizens with distinctive norms and attitudes towards police. Below I will show how comparing the differences in relative guilt rates within a very close proximity to precinct borders may help us better understand the unobserved interaction between police and suspects.

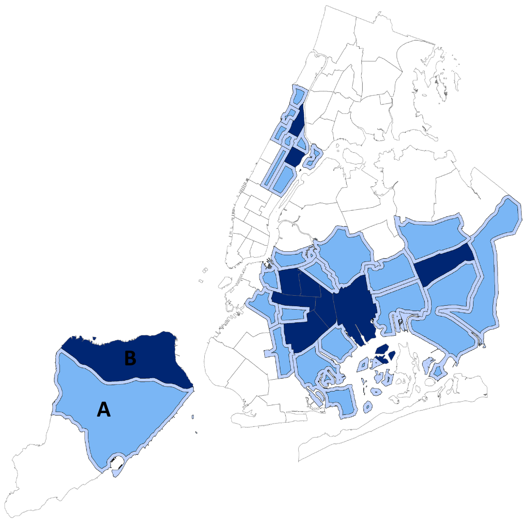


Figure 1.7: Ten precincts with the largest racial disparities in stop rates (dark blue) and the precincts they share a border with (light blue)

Figure 1.8: Precinct borders drawn with 500, 750 & 1,000 foot buffers around them

To visually motivate this approach, consider precinct A, which appears in Figure 1.7 as one of the areas shaded light blue. Precinct A shares a border with another precinct, B, which has been identified as among the ten most inequitable in the city in terms of the racial composition of those stopped by police.[19] While we assume officers are homogenous and monolithic within all precincts, we have made no such assumption about the citizens they will police. Though police are confined to the precinct which they have been assigned, citizens can cross precinct borders at will. If we believe citizens exposed to inequitable treatment by police, as is the case in precinct B, respond to police differently than citizens treated more equitably, as is the case in precinct A, then we would be interested in determining whether interactions between police and citizens differ with proximity to the border between

[19] This means the precinct has one of the ten highest values of the ratio $\frac{\frac{\text{Black Stops}}{\text{Black Residents}}}{\frac{\text{White Stops}}{\text{White Residents}}}$

the two precincts. After all, the officers policing the border area in precinct A are assumed to be no different than the officers in the rest of the precinct, however we would expect those officers are most likely to interact with citizens from precinct B. Consequently, any variation in the test statistic Δ between these two regions would indicate differential suspect responses to police interdiction. This can be shown algebraically:[20]

$$\Delta^T - \Delta^C = 0 \tag{1.19}$$

$$\implies \left[\frac{\lambda_B^T C_B^T}{\lambda_W^T C_W^T} - \frac{\frac{1}{H^T(u_2(B))} C_B^T}{\frac{1}{H^T(u_2(W))} C_W^T}\right] - \left[\frac{\lambda_B^C C_B^C}{\lambda_W^C C_W^C} - \frac{\frac{1}{H^C(u_2(B))} C_B^C}{\frac{1}{H^C(u_2(W))} C_W^C}\right] = 0 \tag{1.20}$$

$$\implies \left[\frac{\lambda_B^T C_B^T}{\lambda_W^T C_W^T} - \frac{\lambda_B^C C_B^C}{\lambda_W^C C_W^C}\right] = \frac{\frac{1}{H^C(u_2(B))} C_B^C}{\frac{1}{H^C(u_2(W))} C_W^C} - \frac{\frac{1}{H^T(u_2(B))} C_B^T}{\frac{1}{H^T(u_2(W))} C_W^T} \tag{1.21}$$

$$\implies \frac{\frac{1}{H^T(u_2(B))} C_B^T}{\frac{1}{H^T(u_2(W))} C_W^T} = \frac{\frac{1}{H^C(u_2(B))} C_B^C}{\frac{1}{H^C(u_2(W))} C_W^C} \tag{1.22}$$

$$\implies \frac{H^T(u_2(W))}{H^T(u_2(B))} = \frac{H^C(u_2(W))}{H^C(u_2(B))} \tag{1.23}$$

To test for differential suspect responses I have identified the ten precincts where blacks are stopped most intensively each year from 2006-2012. These will be known as the treator precincts. I then identify all the precincts the group of treator precincts border, but which are not also in the treator group. This is the group of treated precincts.[21] Within each treated precinct I refer to stops that occur within a fixed distance from the border to the treator precinct as the treatment group and all other

[20] Superscript ^{T} can distinguish any generic precinct partition; here it specifically denotes statistics compiled from the area in a *treated* precinct immediately adjacent to the border with a *treator* precinct. Superscript ^{C} is affixed to statistics compiled over the remainder of the *treated* precinct. Our uniform bias assumption within precincts necessarily requires $C_r^T = C_r^C$ and $\lambda_r^T = \lambda_r^C$

[21] Treator precincts are the dark shaded regions in Figure 1.7. Treated precincts are lightly shaded in that figure.

stops in the precinct comprise the control group.

Table 1.5: Estimates of differences in Δ within precincts

	Sample	Obs	Mean	Std Err	95% Conf		Pool
350 Feet	Control	208	.1218	.0489	.0254	.2182	.0667
	Treatment	197	.0143	.0389	-.0624	.0911	
	Difference		.1075***				
600 Feet	Control	260	.1285	.0462	.0374	.2196	.0561
	Treatment	258	-.0158	.0422	-.0990	.0673	
	Difference		.1443***				
850 Feet	Control	300	.1345	.0497	.0368	.2322	.0557
	Treatment	300	-.0244	.0493	-.1214	.0725	
	Difference		.1589***				

Estimates come from *treated* precincts bordering the 10 precincts (*treator* precincts) where blacks are most intensively stopped. Treatement groups are the collection of observations occurring within the specified distance from the border with the treator precinct. Significant effects were observed for treatment groups extending to 1,500 feet from a precinct border.

The results in Table 1.5 indicate significant disparity in the test statistic Δ within precincts 'treated' by their proximity to precincts where blacks are stopped most intensively. Higher values of $H(u_2(r))$ decrease search costs and therefore indicate a higher liklihood of a search occurring. Finding that $\Delta^C > \Delta^T$ implies $\frac{H^C(u_2|(W))}{H^C(u_2(B))} > \frac{H^T(u_2(W))}{H^T(u_2(B))}$, indicating that in the treatment areas innocent blacks are responding to police in a way that foments searches. This corresponds to searches on blacks being even less effective at discovering contraband in locations closest to the border of the treator precincts. I find that police recover contraband from black suspects within 1,000 feet of a treator precinct at a rate that is only 61% of what searching a white suspect yields.

While it appears this decrease in relative productivity when searching black suspects near areas where blacks are stopped intensively is driven by suspect responses, these results alone cannot eliminate officer bias as a potential source of the search success disparity. To affirmatively say suspect responses differ it is also necessary that officers do not display significant spatial heterogeneity in their application of situational bias.

Recall that in the previous section I offered a test for situational bias that assumed officers were unconstrained by suspect responses. That is to say, even if a suspect displayed a very high u_2 value, the officer could not be induced to stop the suspect unless the officer knew that value signaled guilt. That specification implicitly treated suspect response heterogeneity as irrelevant, however the modeling assumptions in this section allow for the possibility that officers are induced to search individuals of a sufficiently high u_2 value regardless of the correlation u_2 shares with guilt. At the same time, if we believe officer assignment within a precinct is correlated with officer bias, we might also explain the results in Table 1.5 as evidence that the most biased officers gravitate towards areas with larger black populations.[22]

It should be clear that without officer level data it one cannot dispositively rule out officer hetrogeneity as a source of disparity in Δ. While evidence can be offered to suggest that with precinct officer behavior is similar based upon across precinct measures of similarity it may still be the case that statistics estimated at the precinct level mask officer level hetrogeneity. Nonetheless, one way to assess, albeit crudely, whether spatial variation in officer bias is a potential cause of these success disparities involves shrinking the geographical area of interest. The potential identification problem generated by heterogeneous suspect responses can be neutralized by reducing the data set to just those stops that occur near a precinct border. In New York, police precincts are almost entirely divided along public streets. Residents on one side of a particular street might be residents of generic precinct A while their neighbor across the street is a resident of generic precinct B. It would take a heroic assumption to assume that suspects encountered within a few blocks of either side of a precinct border differ remarkably, however the police officers they encounter when crossing the invisible line separating precincts A and B certainly will. If Δ doesn't differ significantly across precincts when constraining the sample to only include stops occurring within a few blocks of a border, that

[22] All treator precincts are in the highest quartile of the black precinct population distribution

is strong evidence that officers are generally uniform in their bias.[23] Testing for differences in policing across borders is conducted using a regression discontinuity (RD) design. Each precinct border represents a two-dimensional discontinuity in the geospatial coordinate system and is represented by the parameter ϕ_p in the following regression formultion :

$$C_{bgp} = \alpha + \omega \mathtt{treatment}_g + X'_{bg}\beta + f(\mathtt{mercator}_g) + \phi_p + \epsilon_{pbg} \tag{1.24}$$

Here C_{bgp} is the outcome variable of interest for census block b in group g along precinct border p. Both primary and situational bias are considered, so C_{bgp} will represent $\frac{D(B)}{D(W)}$ and Δ, respectively. $\mathtt{Treatment}_g$ is an indicator variable assigning treatment status to census blocks on the side of a precinct border associated with a larger black population. X'_{bg} is a vector of covariates and $f(\mathtt{mercator})$ is the running-variable polynomial. The preferred form is a two dimensional specification using a quadratic polynomial formed from the x-y coordinate associated with the centroid of each census block. Table 1.6 also presents estimates associated with the $\mathtt{mercator}$ variable representing the linear distance from each census block centroid to the closest precinct border segment. Finally, ϕ_p is the set of precinct border fixed effects. Significance of the coefficient ω would be consitent with officer hetrogeneity across precincts conditional on the assumption that, aside from $\mathtt{Treatment}_g$, determinants of C_{bgp} don't 'jump' along the precinct border.

The distance of a New York City block varies from borough to borough, but generally averages about 200 feet. This variation led me to conduct my analysis at

[23] A study commissioned by the NYPD and conducted by Greg Ridgeway in 2007 used officer-level data and showed that officers are generally similar in their observed racial preference when selecting suspects for a stop. That study was unique in that the NYPD solicited the research and provided data uniquely identifying the officer involved in each encounter. Looking at 3,034 officers who made at least 50 SQF stops (a group that comprises well over 50% of SQF stops), the author found that only 10 officers significantly overstopped blacks relative to their peers. This finding indicates we should expect officers to display a high degree of uniformity in their selection criteria.

Table 1.6: Regression Discontinuity Specifications with Running Control

	Δ				$\frac{D(B)}{D(W)}$			
	250 ft	500ft	1000ft	1500ft	250ft	500ft	1000ft	15000ft
			X,Y Coordinate Cubic Polynomial					
Border (ω)	.0685	.0697	-.0186	-.0004	-.0319	-.0338	.0111	-.0874
Std. Error	(.0851)	(.0830)	(.0553)	(.0565)	(.0620)	(.0597)	(.0668)	(.0407)
R^2	.1561	.1347	.1828	.1628	.1973	.1693	.1276	.1149
			Cubic Polynomial of Distance to Precinct Border					
Border (ω)	.0621	.0728	-.0175	.0006	-.0423	-.0397	.0075	-.0923
Std. Error	(.0893)	(.0834)	(.0553)	(.0566)	(.0621)	(.0585)	(.0659)	(.0408)
R^2	.1611	.1339	.1846	.1625	.1971	.1689	.1287	.1147
Clusters	131	146	168	179	131	146	168	179
n	954	1,235	1,846	2,333	954	1,235	1,846	2,333

Observations are made at the census block level where the distance from the precinct border is measured from each census block's centroid. I report robust standard errors, clustered at the precinct border level. The top row in columns 2-9 reports the cut-off distance from the precinct border, such that in column 4 I only use observations from census-blocks with a centroid that falls within 1,000 feet of the nearest precinct border. Columns 2-5 report estimates of the coefficient associated with the spatial discontinuity generated by precinct borders (ω from the linear regression outlined in (1.24)) where the dependent variable is Δ. Columns 6-9 report estimates with $\frac{D(B)}{D(W)}$ as the dependent variable. The cubic polynomial in the first specification is a combination of the x,y coordinates in the NAD83 datum (New York Long Island FIPS 3104): $x^3 + y^3 + x^2y + y^2x + x^2 + y^2 + xy + x + y$. I also used a quadratic polynomial without significant results at any distance. The cubic polynomial of distance to the precinct border is uses Euclidean distance from a census-block's centroid to map the RD setup into one dimension ($\mathbb{R}^+$).

distance intervals from 250 - 1,500 feet that would clearly delineate blocks. Those figures appear in Table 1.6.

At no distance was the coefficient on the border dummy significant, implying officers exhibit no variation in their application of the law from precinct to precinct. This supports both of the main findings from the previous section: officers appear to apply the law without situational bias, but with a primary bias towards apprehending blacks that appears to pervade all corners of the city. It also supports the finding that suspect responses differ by race.

1.6 Conclusion

The data this paper studies appears to exhibit at least two prominent features. First, the relative probability of recovering contraband in a search of a black suspect is significantly lower than the relative probability of the same outcome immediately following a stop by police. Given the model posited in this paper, the implication is that following a stop, black males are searched too often relative to their white counterparts. The second is that this effect is largest where blacks are stopped most intensively. In areas where officers more disproportionately stop, question and frisk black citizens, the marginal effect of being black on the probability of being found in possession of contraband is significantly negative.

The most compelling explanation of this phenomenon can be found in the data's geographical variation. I find a significant divergence between stop and search success rates even in locations where officer bias appears uniform. This suggests when responding to police after being stopped, black suspects in heavily policed areas affect a liklihood of being searched higher than what observable criteria should dictate. This seems to indicate that there are diminishing returns to proactive policing. While proponents may contend there is a large, positive deterrent externality generated by a program like SQF, this study identifies misapprehension as a negative externality generated by intensive policing. In a finding so immediate empirical support seems uneccessary; the data indicate that where police target a relatively high proportion of innocent citizens it becomes more difficult to uncover crime in subsequent interactions.

Several issues threaten the validity and implications of the aforementioned results. The first, and most pertinent with respect to the practical applications my findings could inform, is that contraband recovery may not be the primary objective of police. In fact, City officials have implied that the motivation behind SQF activities is crime deterrence. New York Mayor Michael Bloomberg has stated publicly that, "By making it 'too hot to carry,' the N.Y.P.D. is preventing guns from being carried on our streets".[24] While this may be an effective tactic, it is also an extra-legal tactic and therefore I assert that focused deterrence should not be included in a rational model of police behavior. Figure A.1 shows that the stop intensity follows areas with the highest concentrations of public housing and historical crime rates. Further evidence to support the idea that SQF stops are meant to deter, not interdict, crime comes from Figure A.2, which shows that the most tenuous reasons for stopping an individual are also by far the most cited by officers.

Judge Shira A. Scheindlin of the Federal District Court in Manhattan appears to agree with this position. One of her early rulings in the *Floyd et al v City of New York* case involved her refusal to hear the City's attempt "to justify stops on the basis of their deterrent impact, regardless of their legality". That is the key point that should be elucidated; if police make a legal stop, and in doing so deter other potential criminals from committing crimes in the future, then deterrence is merely a positive externality generated by a legal police tactic targeting criminals. On the other hand, if police make a stop without the proper legal justification merely for the purpose of deterring future crimes, that is an illegal violation of citizens' constitutional rights. The City of New York has agreed to comply with Judge Scheindlin's ruling, making it clear that future research should continue to treat the deterrent effect of a police stop as an externality rather than an explicit benefit in the police officers' objective function. Nonetheless, if police in New York City accounted for the deterrent effect of their stop decisions from 2006-2012, the model and results posited in this paper are largely invalidated.

A second issue that merits discussion is the possibility that officers recognize potential costs outside of the time and effort required to interdict a suspect. For instance, citizen complaints to the Civilian Complaint Review Board (CCRB) can tarnish an officers' reputation and impede promotion. Though potential litigation costs associated with stops that violate citizen's constitutional rights are not directly

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[24] Buettner, Russ. (August 17, 2012) Judge Bars Testimony by Expert in Frisk Suit. *The New York Times*. A16.

paid by officers, they are most certainly recognized by Precinct commanders and may impact policing strategy. One might imagine a situation wherein police find searching innocent whites generates relatively more complaints and litigation compared to searching innocent blacks. What appears to be racial bias in my model could very well be attributed to risk aversion in a model where police take these costs into account. While I accept that it is possible to construct a model that attributes $\frac{D(r)}{D(r')} \neq \frac{G(r)}{G(r')}$ to 'defensive policing', I argue that neither of the above two arguments are supported by the data.

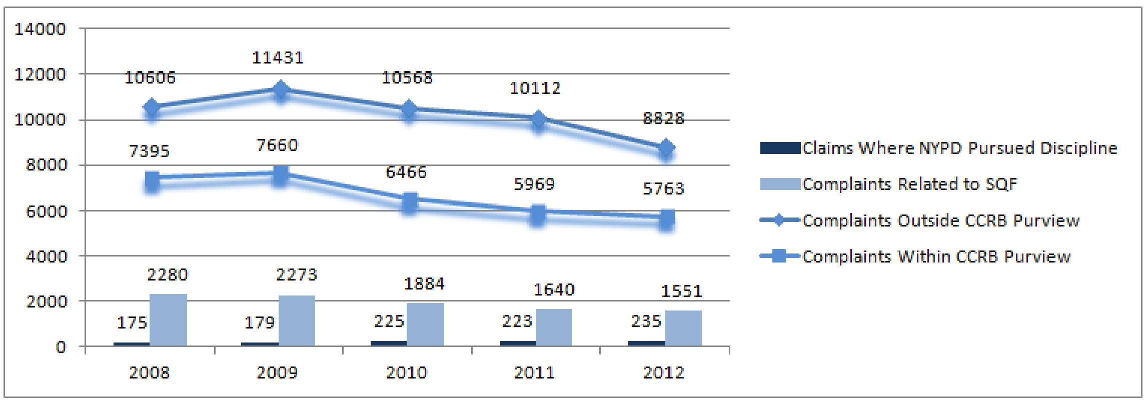

Figure 1.9: Formal citizen complaints regarding NYPD actions 2008-2012. Source: (Chu, 2013)

Of the 14,591 complaints submitted to the CCRB in 2012, only 235 were substantiated and forwarded to the NYPD for disciplinary action. It is unclear what portion of those 235 complaints actually resulted in officer sanction, but given that fewer than 2% of complaints result in any disciplinary action at all it is doubtful officers seriously consider complaints when making policing decisions.

Furthermore, the idea that differential litigation costs would explain situationally specific racial bias is tenuous given that the litigation costs of inequitable searches are not a burden of the individual officer (nor do they come from the NYPD's budget, for that matter). Nor is there data suggesting whites are more likely to sue officers for illegitimate searches than any other race. In fact, Figure 8 summarizes the racial distribution of complaints to the CCRB. That figure shows that, relative to their population share and their share of SQF stops, blacks are actually the most likely racial group to pursue claims against police.

Another possible source of bias in my estimates comes from the data itself. SQF reports are "required" whenever a stop takes place, however filing the report does include discretion on the part of the officer involved. To this end, several people familiar with the program have questioned the validity of the number of stops the data reports. Jeffrey Fagan suggests that SQF reports have become a proxy for officer productivity and thus there is an incentive for officers to chronicle encounters that do not actually merit record. Jonathan Timoney (former Philadelphia Police Commissioner and Miami Police Department Chief), on the other hand, worries that the actual amount of contraband recovered as a result of SQF stops is vastly under-counted.[25] His contention is that officers in the field will choose to fill out SQF reports for stops that end without incident or for petty offenses, but that for larger contraband recoveries and higher profile arrests that arise from SQF stops officers may fill out an arrest report but omit the UF-250. Consequently, if blacks are more likely to be arrested than whites, such a scenario could generate the results reported in this study even when no racial bias exists.

Finally, it is necessary to note that matching the data to the model is not a perfect one to one mapping. Two notable issues arise in using the SQF database to test my model. The first is that there is no way to tell, given the data, whether contraband was recovered before or after a search, only whether contraband was recovered at all. The second is that it is possible that at least some stops occurred simply to screen for wanted individuals. The problem this poses is that a portion of the searches undertaken by police will not be fomented by probable cause that the person is carrying contraband, but will instead be required by police policy. When police make an arrest and take a suspect into custody they are required to search the individual for the safety of themselves and other officers. This means that if police

[25] "I can almost guarantee you there are plenty of [uncounted] arrests that came as a result of a stop and frisk." (Trone, 2010)

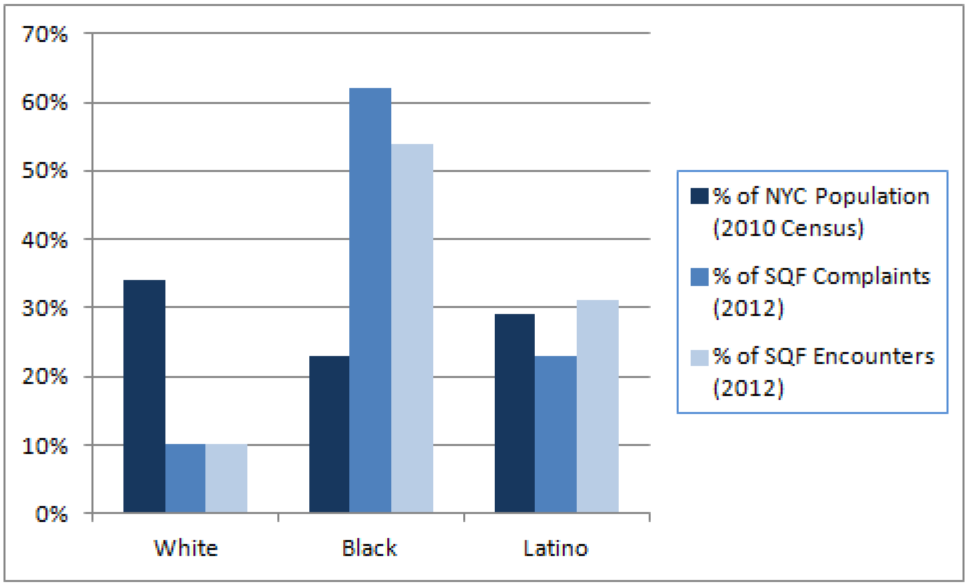

Figure 1.10: 2012 CCRB Stop & Frisk Complainants by Race vs. NYC Demographics

recover contraband prior to a search, and this contraband leads to an arrest, the individual will be searched and it will not be possible to tell definitively whether the recovery was a result of the stop or the search. Additionally, those individuals who are stopped and found to have outstanding warrants will be searched when taken into custody, regardless of whether or not police believe they are in possession of contraband. This likely has a limited effect on my work since I compare ratios rather than levels of contraband. Regardless, I control for both of these possibilities by including a dummy variable for whether an arrest was made in the regressions reported in Tables 1.3 & 1.4, both of which provide estimates supporting the central findings of this study.

Like the crimes police themselves investigate, economists who attempt to study bias face the difficult task of gathering information about events that are unobserved and relationships that are not fully identified. Like police, the best economists can do is collect as much evidence as possible and pass their work along to another party for further investigation. This paper extended the work of several other authors by allowing for two-stage tests of police decisions for taste-based preferences, as well as a test for changes in expected guilt frequencies that at the very least implicates New York City police in searching a disproportionately large number of innocent black citizens.

2 A HEDONIC ANALYSIS OF *STOP & FRISK'S* AMENITY VALUE

2.1 Introduction

National crime rates have been falling for more than two decades now and New York City is one of the most ubiquitous examples of the ebbing criminal tide. Nonetheless, both property crime and violent crime remain major urban problems in New York City and elsewhere. In 2014, New York City experienced 328 murders and more than $350,000,000 were lost by victims in more than 170,000 property crimes. Since the late 1990s the New York Police Department (NYPD) has sought to minimize crime through proactive pedestrian policing tactics. The most recent iteration of these strategies is an initiative called **Stop, Question & Frisk** (colloquially known as *Stop & Frisk*), which involves patrolling historically high-crime neighborhoods on foot looking for suspicious individuals to stop and, when deemed necessary, frisk.

Proponents of this approach say it is an effective deterrent to weapons possession and that it has played a major role in decreasing New York City's murder rate, which has fallen by more than half since the year 2000 and reached historic lows in 2013 and 2014.[1] Whether or not *Stop & Frisk* is causally related to the broad crime decline New York City has experienced is still an unsettled research question. Other cities across the nation have seen similarly declining crime rates without major pedestrian policing initiatives.

The failure to draw a direct link between *Stop & Frisk* policing and the crime decline has made the practice a controversial approach to urban law enforcement. In New York City, roughly nine out of ten individuals who are stopped are never arrested, issued a ticket, or charged with a crime. Thus, police have detained presumably innocent citizens more than four million times since 2002. Opponents

[1] The New York Police Department did not keep consistent records until 1963.

of the practice argue that this is a gross violation of individuals' fourth amendment rights, which promise:

> *The right of the people to be secure in their persons, houses, papers, and effects, against unreasonable searches and seizures, shall not be violated, and no Warrants shall issue, but upon probable cause, supported by Oath or affirmation, and particularly describing the place to be searched, and the persons or things to be seized.*[2]

In the absence of strong evidence one way or the other gauging the tactic's efficacy in reducing crime, the most intuitive way to evaluate *Stop & Frisk* is to estimate the public's valuation of the service. NYPD leadership has, in the past, argued that citizens view these stops as an amenity, preventing crime in the areas they are most heavily employed. Some citizens in these areas argue just the opposite, alleging that these stops are unnecessary and degrading, viewing them as a significant disamenity. This paper will undertake a hedonic analysis aimed at providing evidence to inform that debate and the concomitant policy questions.

Hedonic analyses are very common in applied research estimating the value citizens place on public safety. Typically, this will involve regressing home sales on crime rates, which can be problematic for several reasons that will be more thoroughly outlined in later sections. Those issues notwithstanding, if one wants to determine the optimal state provision of public safety, it would seem incumbent to evaluate the mechanism by which crime would be interdicted. Until now few studies have directly valued the design and allocation of law enforcement services. This paper is the first to estimate New York City home buyers' value for *Stop & Frisk* activity specifically, but also the hedonic prices associated with policing more generally.

[2] "The Constitution of the United States," Amendment 4.

2.2 Current Understanding

Griliches (1961) first introduced and Rosen (1974) later formalized a hedonic framework still cited frequently today. It explains how regression analysis can be used to measure the value of a composite product's specific attributes. This technique is common across broad areas of economics that utilize price indicies, but is a particularly common technique in applied real-estate studies. Research on a range of topics, from pollution to education to taxes and even crime, uses hedonic analyses to estimate consumers' value for each based on their capitalization into property values.

A great deal of social science research has used the hedonic framework, definitively and unsurprisingly, to show that people value safety. This has traditionally been done by estimating how its absence (criminal activity) impacted households' willingness to pay to live in a given area while controlling for other relevant covariates. Research attempting to estimate the amenity value of public safety using implicit price models dates at least as far back as Thaler (1978). In that paper he analyzed 398 single-family home sales in Rochester, NY and found that a one standard deviation increase in the gross number of property crimes per capita decreased nearby home prices by roughly 3%. To put his findings into perspective, a four standard deviation increase in property crimes per capita, which would have been the equivalent of moving from the lowest crime neighborhood to the highest crime neighborhood in Rochester, only had about half as large an effect on property values as living in close proximity to the Rochester airport. Other researchers have continued to find similar positive, though modest, results.

Studies such as Cohen (1990) sought to improve upon previous research that used locally aggregated crime data to estimate the effect of crime on housing prices. Not knowing which crimes citizens would be responsive to and recognizing the partial collinearity between many types of crime, researchers used not only aggregated geographies (crimes within a police precinct, along a certain police beat, or inside a given census tract) but also aggregated crime statistics (indices that count many types of crime but do not weight them by their appropriate social cost). This degree of aggregation limited the inference researchers were able to draw from such studies. Cohen (1995) was among the first to weight crimes by their seriousness and estimated cost to victims, but even this approach requires an out-of-sample application of cost estimates onto a localized population which may have very distinct preferences.

While the consensus that people value public safety appears to be unchallenged, other researchers believe that the relationship between public safety and home values may actually be more complicated. Case & Mayer (1996) offered evidence that increased property crimes could be associated with more expensive homes. Lynch & Rasmussen (2001) were able to associate this finding with more active reporting and better potential targets. Turner et al., (2002), Harris, (1999), along with Galster and Godfrey (2005) find that race, unrelated to reported crime, may be used by prospective home buyers as a proxy for neighborhood safety and quality. The implication of which being that public safety and the perception of safety by the public are not necessarily one in the same.

Another issue recognized by Deaton and Hoehn (2004) that requires explicit treatment in any hedonic analysis of property value is the fact that disamenities, like crime and urban blight, as well as amenities, like hot tubs and pools, are often bundled together and econometrically indistiguishable from one another. Sometimes goods can themselves be a mixture of amenities and disamenities (subways are a convenient way to get around a city but are also noisy to live near). From the distance of aggregated statistics even attributes that are not perfectly collinear can appear to be when measured using the wrong unit of observation.

This paper builds on the work of these and other researchers who have previously endeavored to estimate the demand for crime reduction by examining an existing program that purports to do just that: *Stop & Frisk*. As alluded to previously, attempting to determine the hedonic price associated with crime reduction is difficult. Criminals are loathe to reveal their location, so associating crimes to a sufficiently specific geographic location is difficult for many crimes. Where crimes occur and where they are ultimately reported do not necessarily coincide, especially for quality of life crimes. Similarly, many types of crime share a high degree of correlation, resulting in either omitted variable bias or high variability in estimates depending on the researchers' approach (Hellman & Naroff (1979)).

An important issue related to hedonic regressions of home prices on crime is application. The economics literature is flush with estimates of citizen's willingness to pay for crime reductions. The criminology literature features of large body of work evaluating the effectiveness of different approaches to crime reduction. Without a benevolent social planner to facilitate a transfer, the public must employ the police to achieve this end, so it seems a primary question ought to be whether or not citizens are valuing law enforcement's approach to crime reduction in the first place. In the 2015 fiscal year the NYPD expects to employ 35,122 uniformed officers and spend $1.4 billion on patrol officers alone. These costs are non-negligible and valuing the perceived efficacy (and value) of such a large investment in public safety is an important research topic that can only be answered by bridging the gap in the literature between what citizens are willing to pay for public safety and what services police provide.

The obvious hurdle that must be overcome when trying to value police activity using home sales is that police are not randomly distributed in space. Police interactions with suspected criminals are, by definition, almost exclusively determined by the geographic distribution of crime. This means that a police stop is a composite good that is part amenity (police interdicting criminals) and part disamenity (there is potentially a criminal at that location). Typical identification strategies employed in the literature would fail to capture the effect of police holding crime constant, but would instead measure home buyers' value of policing and crime contemporaneously.

One way to identify home buyers' value of police activity without the unwanted influence of variation in crime rates relies on finding a very finely partitioned geographic area, such that the average citizen could not delineate significant spatial differences in criminal activity across the space but wherein police activity varied.

It is infeasible for any home buyer to gain perfect knowledge of the distribution of crime over the set of all available properties. Instead, we know that home buyers tend to gain general knowledge of average crime rates and other variables of interest aggregated at the **neighborhood** level. As both Taylor (1988) and Dubin & Goodman (1982) point out, neighborhoods vary in the degree to which they are identifiable, some boasting naturally occurring physical boundaries, and commonly recognized names, while others may reference a social group within a large population exhibiting few physical properties. Conceptually, neighborhoods are more than a geographical extent, they are also "social constructions named and bounded differently by numerous and diverse individuals" (Lee, Oropesa, and Kanan, 1994: 252). They also have a time component. As neighborhoods decay (or develop) over time, changes in the social, economic, cultural and even criminal characteristics within a neighborhood will eventually lead to new neighborhoods being formed once a clear distinction arises (or conversely subsides). The borders of a neighborhood, like the composition of the people and the structures that live inside them, vary with time.

Neighborhood Effects

The aggregation of crime statistics, combined with the impossible psychic cost associated with gaining knowledge of the exact location of every crime, prevent prospective buyers from being able to differentiate crime victimization hazard within a sufficiently small area. There is some scale at which spatial differences in victimization hazard are imperceptible. Exactly what level of observation that is may be up for debate, but in this paper I will define it to be the neighborhood level due to the practicality and intuitive appeal.

Douglas Elliman, New York City's largest real estate brokerage, indexes exactly 35 Manhattan neighborhoods. They use these titles to reference location because a neighborhood is the smallest geographically distinct unit that can be identified by the composition and behaviors of its inhabitants (Nicotera (2007)). Upon meeting prospective home buyers for the first time, brokers ask their clients which neighborhoods they would like to be shown homes in. The implicit assumption is that the neighborhood selected reveals the buyers' preferences over the attributes that define the neighborhood with the closest degree of specificity possible. My identification strategy uses buyers' perception of a neighborhood's level of public safety to control for crime levels. It is therefore necessary to establish a working definition of what a neighborhood is in order to facilitate a discussion of why I believe this is the correct unit of analysis. For the purposes of this study neighborhoods are geographical areas that possess the following properties:

- **Boundaries**: There must be clear physical demarcations along which the neighborhood is defined
- **Name**: The neighborhood must have a commonly used name that is referenced openly and without derision
- **Shared Public Facilities**: There must be some cooperative efforts to build and maintain common property within the neighborhood
- **Social/Cultural Connections**: Individuals share common social, economic and cultural characteristics, such as race, religion, ethnicity, relative income, profession, language, age, and daily routines.

In this paper I will use the neighborhood definitions offered by the New York City Department of Planning. Looking at Figure B.4, which shows the racial distribution of residents along the 96th/97th street border that separates East Harlem from the Upper East Side, we see that neighborhood distinction is clear and stark. Throughout the city, Figure B.3 shows how clearly one can delineate neighborhood borders just by studying the racial distribution of residents. Similar distinctions can be made across other economic and cultural dimensions. The ability to easily recognize and differentiate neighborhoods may be due, in part, to their names being broadly used even outside the city: Harlem, Bedford-Stuyvesant, Wall Street. Many of these names have become well-associated emblems of the social and cultural characteristics these neighborhoods manifest. This makes them the natural level at which to control for inter-neighborhood variation in crime rates. They also represent the finest partitioning of New York City that differentiates spatially adjacent partitions according to the criteria referenced above.

Data used in this study will come from 267 neighborhoods distributed across New York City. The finest governmental partition is a Community District, of which there are only 59 throughout all five boroughs. These are referenced in Figure B.1, while Figure B.2 shows the neighborhoods that collectively comprise several lower Manhattan Community Districts. Table B.1 lists the physical demarcations associated with a handful of representative neighborhoods.

The assumption that home buyers perceive a uniform public safety risk throughout a neighborhood does not rule out the possibility that 'hot-spots' may exist within neighborhoods. A hot-spot is an outlier in the crime-level distribution. The very definition of a hot-spot requires there to be an abnormal amount of [in this case] crime at a particular location. Though it is a named entity co-located with the neighborhood, a hot-spot is not a part of the neighborhood.

Hot-spots are aberrations, so if any exist they are assumed to be known to all home buyers and the intra-neighborhood variation in crime levels they generate can be controlled for using spatial statistics that measure the distance to subway stations and housing projects. In a finding not unique to New York City, a great deal of crimes are reported in housing projects and at subway stations. This phenomenon is so pronounced in New York City that the NYPD is using a grant from the U.S. Department of Justice to hire Rutgers University Professors Joel Caplan and Leslie Kennedy to explain its causes.

Most crime rate variation in New York City is inter-neighborhood, with very few pronounced crime-level gradients existing within neighborhoods. This can be established formally, calculating a Getis-Ord Gi* statistic for thousands of locations city wide and testing for outliers in its distribution. However a visual inspection of the NYPD's crime maps is all that is necessary to establish that no distinct crime pattern exists that is not explained by subway and housing project locations. Figure B.6 and Figure B.7 are representative of the patterns that persist city wide.[3]

2.3 Model & Data

As much as this paper's contribution is the insight of its author, significant credit must also be given to technological advances that have changed applied economic research dramatically since Thaler (1978). The differences between that paper and this one are stark in terms of the amount of data (398 homes vs. 113,007 properties) and the spatial precision (his study variables were adjacent land uses and disamenities visible from the property vs. distance measured in inches to relevant local amenities/disamenities), yet in most respects this paper is just a novel application of a similar research design only made possible by technological advances.

In this section I will offer a brief introduction to basic hedonic theory, explain how my estimation strategy relates to this theory, and finally I will describe the

[3] A $\mathtt{Getis-Ord\ Gi*}$ statistic was calculated for each census tract and a hot-spot analysis was conducted. The results appear in Figure B.5 and reveal no adjacent red and blue tracts. Most variation was between neighborhoods and boroughs, as indicated in Table B.2 which shows that properties from the sample group that were located in Manhattan and Brooklyn had much higher police stop activity levels in the twelve months preceding their sale.

data that will be used to estimate home buyers' valuation of *Stop & Frisk* policing in their neighborhood.

Hedonic Model

The hedonic approach is based on the premise that the utility an individual derives from a product, or in this case, the utility a household derives from their home, is a function of the characteristics of the home. These characteristics include spatial characteristics, such that a property a would be understood to be a bundle indexed by a vector of physical attributes, i, as well as its spatial relationship to local amenities and disamenities, l; $a_{i,l}$. Households may also choose to spend their money on things other than housing, which is referenced by a composite good c_l.

Without agent-level hetrogeneity, the quasi-concave utility function $u()$ relates household satisfaction with a quantity of the composite good and the housing bundle:

$$u = u(c_l, a_{i,l}) \tag{2.1}$$

Assuming no friction (costless relocation), we would expect that at equilibrium each household gains the same utility at any location given an exogenous income M and the bid-rent function $B()$. The bid-rent function, or 'bid-price' curve, as first articulated in Alonso (1964), relates the upper bound of the set of prices a given household is willing to pay for the bundle $a_{i,l}$. Thus, we know any equilibrium must satisfy the following condition:

$$u^* = \nu(M - B(a_{i,l})) \tag{2.2}$$

Households will choose the bundle of attributes $a_{i,l}$ to solve:

$$\max_{a_{i,l}} \; u(c_l, a_{i,l}) \tag{2.3}$$

$$\text{s.t. } M \geqslant P(a_{i,l}) + c_l \tag{2.4}$$

In (2.4), $P(a_{i,l})$ is the price function relating the cost of $a_{i,l}$ to the household. The first order conditions yield:

$$\frac{\partial u}{\partial a_{i,l}} + \lambda P'(a_{i,l}) = 0 \tag{2.5}$$

$$\frac{\partial u}{\partial c_l} + \lambda = 0 \tag{2.6}$$

$$\implies \frac{\frac{\partial u}{\partial a_{i,l}}}{\frac{\partial u}{\partial c_l}} = P'(a_{i,l}) \forall i \tag{2.7}$$

Here, $P'(a_{i,l}) = \frac{\partial P}{\partial a_{i,l}}$ is the partial derivative of attribute i with respect to P, measuring the incremental contribution of that characteristic to the property's total value - the hedonic price. For simplicity of interpretation I will use the notation $P_i = P'(a_{i,l})$ following Sheppard (1997). Further combining this result with the implicit differentiation of the indirect utility function in (2.2) shows that the utility maximizing housing choice is achieved when:

$$P_i = \frac{\partial B}{\partial a_{i,l}} \tag{2.8}$$

This result says that the hedonic price is the marginal contribution of attribute i to the bid-rent function.

Estimation

To estimate the marginal contribution of a police stop to the bid-rent function I regress the sale price, P of property i in neighborhood j in year t onto a vector of attributes, x'_{ijt} describing the lot, the improvements, spatial proximity to local amenities, and mean characteristics of police activity near the property, as well as the total number of *Stop & Frisk* encounters, $Stops_{ijt}$ that occurred within a radial buffer (multiple distances are estimated) around the property's location in the twelve months that preceded the sale. Also included are neighborhood/year fixed effects for 267 distinct city-defined neighborhoods throughout the five boroughs. These control for local crime levels, among other things, without the attendant collinearity with police stops that would arise by including property specific crime counts.

The baseline model is estimated via:

$$P_{ijt} = \beta_0 + \beta_1 Stops_{ijt} + x'_{ijt}\Phi + \delta_{jt} + \epsilon_{ijt}$$

I also evaluate a quadratic specification:

$$P_{ijt} = \beta_0 + \beta_1 Stops_{ijt} + \beta_2 Stops^2_{ijt} + x'_{ijt}\Phi + \delta_{jt} + \epsilon_{ijt}$$

The fixed effects, δ_{jt}, in this model are time by location interactions that uniquely identify neighborhoods annually. Although neighborhood borders in this study are time-invariant, neighborhood perception is not, so it is necessary to treat these as distinct groups of observations. Controlling for inter-neighborhood variation in stop activity will allow me to identify the hedonic price of police stops using intra-neighborhood differences in stop intensity unrelated to buyers' perception of crime-levels.

Data

All of the data used in this study are publicly available via the NYC Data Portal, including the two primary files. The first chronicles all police stops that were predicated on *Stop & Frisk* patrol directives. This file is an excellent data set (compared to reported crimes) from an econometric standpoint because officers engaged in *Stop & Frisk* policing are required to keep detailed records of their whereabouts and actions. The data provided by the NYPD include 4,033,817 stops from 2006-2014, all of which include geospatial coordinates. In August of 2013 a Federal Judge ruled that the NYPD had displayed a pattern of behavior that violated suspects Fourth and Fourteenth Amendment rights in carrying out *Stop & Frisk* directives and consequently ordered the department to discontinue the practice. While some stops occurred following the ruling in 2013 (186,562) and 2014 (44,137) the practice was significantly different from the 2008-2012 period when over a half a million individuals were stopped annually. As such, I limit the primary analysis to sales that occurred between January 1, 2007 and December 31, 2012 so that only stops that occurred between January 1, 2006 and December 31, 2012 are referenced. This amounts to 3,803,118 stops.

Property sale data comes from the New York City Department of Finance and includes 1,230,300 recorded transactions from 2003-2015. I am only interested in actual sales, not property transfers ($0 sale prices reflect a transfer of ownership rather than a sale), which eliminated 374,819 observations. Also omitted were sales that occurred prior to January 1, 2007 or after December 31, 2012, along with observations with questionably low sale prices. Here, 'questionably low' refers to residential units that sold for less than $75,000 and commercial properties that sold for less than $25,000. I further reduced the sample to exclude sales where the property was less 200 (gross) square feet in size as these are unlikely to house permanent occupants. These reductions ultimately removed all but exactly 153,110 sales, comprising my primary sample group.

Each observation is geographically detailed by its tax block. A GIS shapefile provided by the New York City Department of Planning was used to join each property sale to the centroid of the tax block it fell within to get a fairly precise geospatial location for each transaction.[4] Each sale is compared to NYPD records of all *Stop & Frisk* encounters that occurred within a fixed radius of the property's location. For instance, a December 31, 2007 sale would be indexed by the attributes of all *Stop & Frisk* encounters from January 1, 2007 to December 31, 2007 that occurred within fixed radius of the property's location. Attributes of these stops are then tabulated and appended to the observation they reference. They include: total number of police stops, percentage of suspects frisked, percentage of suspects searched, percentage of suspects issued a summons, percentage of suspects arrested, percentage of suspects who are Black or Latino, gun recovery percentage, contraband (drugs) recovery percentage.

Deciding which attributes are economically relevant to the buyer is vitally important to any hedonic analysis, particularly if one is interested in extrapolating predictions to agents outside the sample group. Many other studies utilize samples purchased from real estate companies that contain richly detailed information about each property including number of bedrooms, bathrooms, size of rooms, even brands and ages of appliances. In this study the only property characteristics utilized are: age of the structure, gross square footage, land square footage, and the total number of units.

Despite the lack of detail related to the structural attributes of the property, this study is still able to describe a comparable amount of price variation when contrasted with studies using more richly detailed data. This is likely attributable to the desirability of the New York City real-estate market and it should not be surprising that a great deal of the variation in prices is spatially determined. Fig-

[4] There are 28,838 tax blocks in New York City - 2,263 of which did not have a property sale (residential or commercial) associated with them.

ure B.8 identifies the GIS shapefiles from the Metropolitan Transit Authority, New York City Housing Authority and Department of Planning I utilized to append each observation to include spatial statistics measuring: distance to the nearest subway station, subway lines in close proximity, subway stations accessible from the location without transfer, distance to the nearest housing project, housing project size, housing project population, and the property's school district. Given this set of attributes, none of the structural characteristics (outside of gross square footage) markedly improve the model's ability to explain the price variation in the data.

This should not be surprising, given that New York City is home to one of the most competitive housing markets in the nation. In 2014, the median rent was second highest in the nation. According to the Case-Shiller Condominium Price Index New York has seen prices appreciate 240% over the past two decades, leaving only about 25% of homes affordable to the middle class. With low vacancy rates, many consumers are buying square footage and location, not fixtures. In that regard, this may imply that New York home buyers view the physical housing stock as relatively homogeneous.

The focus of this paper is on residential sales, so the primary specification excludes all non-residential building classes. I further omit any sales with four or more residential units since these properties are less likely to be solely owner-occupied. Two alternative samples are also considered. One utilizes only multi-unit rental properties and is designed to see how responsive the number of units are to crime and/or policing. The other is restricted to non-retail commercial properties. Comparing results from these analyses should inform my theory of how property is valued by different types of buyers with implicitly varying preferences over time and attributes.

2.4 Results

To justify my use of linear estimators I first estimated a regression of sale price on a set of regressors that included quadratic police stops to determine if using a non-linear form appeared warranted in the subsequent analyses. The full set of regressors is described in Table B.3, however most insignificant coefficients will be omitted from the results unless they are referenced in the analysis.

In Table B.4 the number of police stops are a twelve month total in a 1,000 foot radius surrounding each property. The estimate of the coefficient on linear stops is negative and highly significant while the coefficient on the quadratic term is very small, positive, and not significant. As one would expect, square footage was positive and highly significant. As of 2013, SteetEasy reports that the average price per square foot in the 75th precinct, where most stops occurred, was about $173. Throughout the city's neighborhoods average prices ranged from $110 in Woodlawn (Bronx) to $3,393 south of Central Park. Given the minimum stop requirement it is logical that the coefficient would tend towards the lower end of this range. Access to the subway is seen as an amenity given the negative and significant coefficient, while proximity to public housing structures are seen as a disamenity given that home buyers pay an average of $6.83 per additional foot their home is separated from a NYC Housing Authority (NYCHA) site. Year built is also highly significant and indicates that newer structures are more highly valued. Given the arduous approval process in New York and strict zoning requirements, most new developments are only economically viable if they are fully or predominantly "luxury" accommodations - hence buyers' willingness to pay a premium, although the estimated coefficient appears small if driven by the mechanism just described. The regression line appears strikingly linear and the inflection point lays at 4,323 stops, this is beyond 99% of the observed values and indicates police stops can be assumed to effect sale prices linearly.

When including all sales, a full set of demographic controls at the census tract level, and a full set of regressors describing typical *Stop & Frisk* results, and omitting the quadratic stop term regression, I find very similar results. Table B.5 contains estimated coefficients for variables measuring the fraction of *Stop & Frisks* that result in: arrest, summons, a gun recovery, a contraband recovery, or a non-white suspect being targeted. The coefficient on stops is negative and given that the city-wide average sale price was $575,070, a one standard deviation increase in the number of police stops (336) represents a 3.2% average sales price decrease - strikingly similar to the magnitude found by Thaler (1978) looking at a one standard deviation increase in total crimes.

Other variables show expected signs and significance: larger properties are more valuable, older structures are less valuable, people pay more to be closer to subway stations and less to be near housing projects. Notably, while the coefficient estimate on the distance between a property and the nearest housing project is positive, it is not significant. It appears households respond more to the size of the housing project, measured by its population, as compared to the proximity; the results indicate they are willing to pay $3.78 for a one resident reduction.

What is most surprising is that few of the coefficients on measures of stop efficacy were significant, including omitted coefficients for the portion of stops where summonses that were issued. Properties surrounded by stops that more often resulted in frisks, or more often targeted Black or Latino suspects, sold for relatively less. Higher contraband recovery rates had a significantly positive relationship with sale price. One implication of this finding is that if there is any amenity value to these stops it is in their deterrent value, and their ability to take dope smokers off the corner, rather than their ability to apprehend criminals. If this were to be interpreted terms of the disamenity value, it would seem to indicate buyers don't care whether the stops are productive. Since I can only control for the racial characteristics of the resident population, the negative coefficient estimate for the portion of stops that involved Black or Latino suspects could indicate that homebuyers' were willing to pay less for properties in areas with a more visible minority population. Still, the average effect of a one standard deviation in the portion of stops involving Black or Latino suspects would only impact prices by about 1%.

Across the city's five boroughs the effect police stops have on real property sale prices varies substantially, as does the prevalence of the NYPD's community policing tactics. Table B.7 reports only the estimated coefficients for police stops (in a 1,000 foot buffer) from a series of borough specific regressions identical to that which produced Table B.5. Note that a one standard deviation increase in *Stop & Frisk* activity ranges from an average of just one-tenth of one percent in the Manhattan and the Bronx to 2.4% in Brooklyn, and only estimates for Brooklyn and Queens were significant at the 1% level.[5]

Th analysis so far has omitted several classes of property which themselves have informative characteristics that can be used to determine residents' value of *Stop & Frisk* policing. Rental properties are particularly informative because they typically lack an attribute exhibited by the residential units already considered: owner-occupancy. A residential property is both a store of value and a consump-

[5] Descriptive statistics for the *Stop & Frisk* count distribution, broken down by borough, can be found in Table B.2

tion good. A rental, on the other hand, is an asset that should generate income to cover the cost of ownership in order to offset the opportunity cost of the investment. This creates a unique hazard, not faced by owner-occupants, stemming from possible “vacancy loss". Should the property prove undesirable to lessees, the property owner would potentially earn a negative return on the asset (property).

Intuitively, we would expect a lessee to value property characteristics differently than an owner-occupant given their (presumably) shorter time horizon. One notable limitation of the approach I have chosen is that it assumes citizens are only concerned with the current purchase decision. Bishop and Murphy (2011) use a dynamic model of the home purchase decision to show that in the presence of decreasing crime, myopic hedonic models can underestimate the true aversion agents have towards living near criminal acts. We can then think of lessees as making a dynamic decision, deciding where to rent today, as well as estimating the probability they will want to move when their lease ends. While a dynamic model for this estimation would be ideal for modeling lessees' preferences, it is unfeasible given this paper's identification strategy (a dynamic model would require time-invariant neighborhoods).

Without specifically modeling home buyer preferences, or those of a lessee, we can still recognize that renters are more sensitive to the current attributes of a property. Renters can, and often do, pick up and move. According to the 2007-2011 American Community Survey, 11.4% of New Yorkers report living in a different house than they did they year before (mostly renters). This is below the national average of 15%, but it is worth noting that the New York City Housing Authority, which administers city subsidized housing, has 635,000 residents who average a 20 year tenure in their current home.

An owner-occupant is more likely than a renter to see *Stop & Frisk* activity as an amenity, believing the stops may signal a commitment by law enforcement to rid the area of crime, which we know to be a characteristic valued by prospective buyers.

This is especially true because home-owners in New York City are predominantly White, the racial group least likely to be stopped by police. A lessee, on the other hand, is more likely to view the stops as a disamenity - particularly if the lessee is part of a demographic group likely to be stopped. As it happens, Blacks and Latinos have the lowest homeownership rate in the city and are the racial group most likely to be stopped by police. Given this information, it would not be surprising to see police stops exhibit a larger negative effect on the sales price of rental properties as opposed to owner-occupied residential properties.[6]

In Table B.8 I limit my sample to properties with a building code classification starting with either “C" or “D" according to the NYC Department of Buildings. These are typically sales of more than one unit and in the data I observe sales of as many as 8,270 units in a single transaction. After completing the data cleaning procedures previously explained the rental market consists of 11,320 sales for which all the variables included in the estimation of Table˜tab:main are available. Of these sales, only 126 occurred on Staten Island. That is also not surprising, as it is consistent with home ownership rates in New York City. The citywide homeownership rate was 32.5% in 2014, well below the national average of 64.5% in 2014. Staten Island had the highest owner-occupancy rate at 64.6%. Queens (44.3%), Brooklyn (29%), Manhattan (24.8%), and the Bronx (21.2%) have lower rates. The average sale price of a rental property in this sample is $3.3 million, ranging from $75,000 (my minimum constraint) to $443 million. The average number of *Stop & Frisk* events associated with a sale is 603, with a maximum of 4,660 and a standard deviation of 583 stops. Brooklyn accounted for the largest portion of the sample group (roughly 43% of the sales) and Staten Island only 1%.

The results that appear in Table B.8 result from a regression that includes all

[6] There is nothing unique to the building classifications considered in Table B.5 that precludes those properties from being rented out. Given the types of buildings, however, and limited number of units in each building, the probability of owner-occupancy is undoubtedly higher in that sample compared to this sample.

of the same variables as were included in Table B.5's estimation, however some insignificant coefficients are not reported. The estimates are consistent with my expectation that buyers of rental properties would display a greater aversion to *Stop & Frisk* activity. Here, a one standard deviation increase in the number of police stops is associated with a 5.4% decrease in sale price - roughly three times as large as was observed in the residential sample. The implications of this finding will be considered in the conclusion.

Finally, I consider the sales of three types of non-retail commercial properties: garages, warehouses and factories. This sample includes observations with a sale price of more than $25,000, including 1,030 warehouse sales, 810 garages, and 645 factory sales. More than 90% of these sales were located in Queens, Brooklyn, and the Bronx. The results from this sample's estimation appear in Table B.9. Here, I utilized all the same variables as the estimations of Table B.5 and Table B.8, though some insignificant coefficients are not reported.

When only this sample of non-retail commercial property sales are considered the coefficient on the number of police stops is large and positive, though it is not significant at any standard confidence level. All of the other covariates are of the expected sign, however it is slightly informative that neither the distance to the nearest subway station nor the distance to the nearest public housing facility are significant. The business uses of these properties are likely unrelated to pedestrian traffic generally and public transit commuting patterns specifically. For many of these properties, the business license of the owner and city regulations regulating possible uses are the greatest predictors of value. Thus, it is not unexpected that parcel size and number of units are the only significant variables.

2.5 Conclusion

Results indicate a negative correlation exists between the prevalence of *Stop & Frisk* activity and real property sale prices. This relationship is especially pronounced in rental properties and less evident in single-family and two-family residential sales. This is intuitive, as we would expect homeowners to gain a greater amenitiy value and a lower disamenity value (though, on balance, their behavior indicates the disamenity value is still the greater of the two) from the prevalence of nearby police stops holding crime constant. One the one hand, homeowners stand to gain financially if neighborhood crime levels decrease in the future. On the other hand, homeowners are less likely to be criminals due to their greater financial means and subsequently we would expect they are stopped less often by police.

According to a recent study by the Consumer Finance Protection Bureau (CFPB), home ownership and access to mortgage finance both diverge along predictable racial patterns. As of 2013, over 41% of Whites and Asians in New York lived in owner-occupied housing. This contrasts with just 26% of Black and 16% of Latino residents. Data from the Home Mortgage Disclosure Act reveals one cause of this disparity. Despite making up more than half of New York City's population, Blacks and Hispanics make up just 11% of the mortgage application pool. [7] Once considered, roughly a third of Black and Latino applicants are subsequently denied mortgages. Lacking access to credit, Blacks and Latinos subsequently become less than one-tenth of the home-buyer pool, further exacerbating the racial divide in owner-occupied housing. [8]

This provides one possible explanation for the fact that buyers of residential properties derive a smaller disamenity value (or gain a greater amenity value) from

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[7] Brevoort, K., Grimm, P., & Kambara, M. (2015) "Data Point: Credit Invisibles" *Consumer Finance Protection Bureau* Office of Research: May, 2015

[8] National Association of Realtors (2014) "Home Buyer and Seller Generational Trends" *Research Division*: March, 2014.

police stops compared to purchasers of rental properties, whose prospective renters are more likely to be police targets. While homeowners are more likely to be White, renters tend to be Black or Latino. At the same time, Blacks and Latinos (renters) are far more likely to be stopped by police relative to Whites (homeowners). From 2002-2014 well over 85% of stops involved Black or Latino suspects, mostly young males, despite comprising less than 50% of the metropolitan population. Whites, the racial group most likely to own their residence, make up one-third of New York City's population, but just 10% of police stops from 2002-2014.

While it seems intuitive that home buyers may not want to live in a place where police habitually stop and frisk mostly innocent people, another mechanism that could also be explored is that the total number of police stops proxy for an omitted variable: the number of suspicious people near a home. This does not seem likely. Police justify their choice of stop targets based on what they claim is a "reasonable suspicion" aroused by the appearance and behavior of suspects. While police may have believed the individuals they stopped were engaged in criminal activity it should again be noted that nearly nine out of ten times police found no evidence of wrong-doing. If anything, the total number of police stops is correlated with higher numbers of non-criminal pedestrians in an area (only residential population, not total transient population, was controlled for in the analysis).

From the econometric standpoint, the most distinguishable characteristics that defined those who were detained by police were that they tended to be young, male and either Black or Latino. Consequently, it is arguably more likely that the total number of police stops proxy for homebuyers' racial preferences related to these groups.

> "Because of the concentration of many social problems in neighborhoods with relatively large black populations (Massey 1995; Massey and Denton 1993; Peterson and Krivo 1993), selecting a "good" environment usually means choosing a predominantly white neighborhood." (Harris

(1999):464)

It has been well documented that “many white households, rightly or wrongly (and even perhaps, with some regret), associate predominantly black neighborhoods with diminished neighborhood quality and resilience" (Ellen (2000:47)). I control for the racial composition of residents at the census tract level, so this could serve as a good explanation for the negative (and significant) coefficient estimate associated with the “Black (%)" variable that measures the racial composition of the census tract the property lies within. Combined with the finding that a higher proportion of stops of persons of color was associated with significantly lower sale prices this indicates the stops could be creating a visible awareness of a location’s racial composition that discomforts some home buyer’s.

Another possible explanation for my findings of a negative relationship between police stops and real property sale prices is an omitted variable that spatially associates both police and lower quality housing. An example of this would be the NYPD’s Clean Halls program, also known as the Trespass Affidavit Program (TAP), which allowed private property owners (typically apartment buildings) to invite officers to patrol inside their building. If owners are unwilling to pay for private guards or maintain their property’s security, it is also conceivable they are unwilling to pay for general upkeep as well. Participation in the Clean Halls program could generate *Stop & Frisk* activity and also signify a property in disrepair. Started in 1991, the program was conducted quietly for years and the NYPD has repeatedly denied requests to produce a list of buildings participating. According to The Atlantic, “there are 3,895 Clean Halls participants in Manhattan, and nearly every building in the Bronx is enrolled" while the Manhattan District Attorney’s office claims that as of 2015 “more than 3,200 buildings are enrolled in TAP". Given the apparently widespread adoption of this program, and lack of information about enrolled buildings, it is impossible to speculate about its possible effects.

The main finding in this analysis, that New York City home buyers view *Stop &*

Frisk policing as a disamenity is not surprising. Though a Federal judge ordered the NYPD to review the manner in which it was practicing *Stop & Frisk* policing, nothing in [her] ruling explicitly precluded officers from continuing to stop New York City residents as they have for the past two decades. Instead, it was NYPD leadership that decided to rapidly curtail the program. Perhaps the strongest piece of evidence in support of my findings is the fact that there was little public outcry when they did.

3 EVALUATING THE ECONOMIC IMPACT OF CALIFORNIA'S PROPOSITION 47

3.1 Introduction

The United States of America is home to some of the most severe criminal consequences in the world, as evidenced by the stylized fact that the nation currently boasts one quarter of the world's prisoner population despite comprising just five percent of its total population. The growing prisoner population, as well as the populations of ex-prisoners and convicted felons, extract significant social and fiscal costs. In 2010 it was estimated that more than 2.7 million children in the U.S. have an incarcerated parent, including one in nine African-American children (Pettit and Western, 2010). Children with incarcerated parents are more likely to show poor academic performance, exhibit behavior problems, and are more likely to eventually be incarcerated themselves (Trice, 1997; Parke and Clarke-Stewart, 2001). Meanwhile, the corrections industry employs nearly as many workers as the automotive industry in the U.S. and costs taxpayers an estimated $80 billion annually. This paper will evaluate the economic cases for and against a law recently adopted in California to mitigate these burdens.

In 2011 California appeared to reach a tipping point. First, the U.S. Supreme Court in *Brown v. Plata* ordered California to reduce its prison population due to overcrowding so severe it reached the level of "cruel and unusual".[1] The *Plata* decision also brought a great deal of media attention to the fact that Governor Jerry Brown's 2012-2013 budget earmarked $1 billion more for prisons than it did for higher education.

While the state legislature has made some notable reforms to California's crimi-

[1] Brown v. Plata, 131 S. Ct. 1910, 1923 (2011).

nal justice system in the past few years, voters recognized that the state was still spending too much to warehouse non-violent, non-serious, and non-sexual offenders who posed a minimal risk to society. In a display of the direct democracy California is well-known for, voters passed Proposition 47 on November 4, 2014. Known as the "Safe Neighborhoods and Schools Act", Proposition 47 re-classified several crimes as misdemeanors that could previously be charged as felonies.[2]

- **Grand Theft**: Theft of property worth $950 or less is now a misdemeanor. Previously, the theft of certain types of property could be charged as grand theft. Before Proposition 47 grand theft charges were also possible if the defendant had previously committed certain theft-related crimes, however this has been amended.
- **Shoplifting**: When the property is worth $950 or less it is a misdemeanor.
- **Receiving Stolen Property**: Receiving stolen property worth $950 or less is now a misdemeanor.
- **Writing Bad Checks**: It is now a misdemeanor to write a bad check unless the check is worth more than $950 or the offender had previously committed three forgery related crimes.
- **Check Forgery**: Forging a check worth $950 or less is now a misdemeanor, unless the offender commits identity theft in connection with the forgery.
- **Drug Possession**: In amounts consistent with personal consumption there crimes are misdemeanors.

The impact of the ballot initiative was both large and immediate. Statewide, some 2,700 prisoners who had been incarcerated for crimes re-classified under Proposition 47 gained retroactive release from prison. Jails have seen an even greater outflow of former offenders. Los Angeles, the nation's largest jailer, has released more than 3,200 inmates who had been incarcerated for crimes re-classified

[2] CAL. PENAL CODE Â§ 1170.18 (West 2015).

under Proposition 47. San Diego County, the second largest jailer in the state, released 130 inmates immediately due to the initiative and is taking in about 1,000 fewer each month as a result of the new laws.

Figure 3.1: Charged Crimes in California Effected by Proposition 47

Felony Category, 2012	Charges filed	Total convictions for offenses affected by Proposition 47	Estimated convictions affected by Proposition 47	Convictions affected by Proposition 47 as percent of charges filed
Burglary	41,678	14,110	9,727	23.3%
Theft	32,014	8,003	5,517	17.2%
Forgery	6,092	169	117	1.9%
Narcotic	32,631	11,748	8,099	24.8%
Marijuana	11,020	706	487	4.4%
Dangerous Drugs	65,355	23,282	16,050	24.6%
Total	**188,790**	**58,018***	**40,000**	**21.2%**

Reproduced from Males et al., 2014. Sources: Legislative Analyst's Office (2014); CJSC (2013)

It is clear that a great many convicted criminals are being released due to these statutory changes. The conventional understanding of released prisoners is that many will recidivate (be incarcerated again for a new offense within three to five years). Nationally, we know that roughly two-thirds of former prisoners will recidivate, regardless of what their prior offense was. The inmates who were released due to the passage of Proposition 47 were necessarily convicted of either a drug or property crime. According to the most recent figures from the Bureau of Justice Statistics (BJS) more than 82% of property offenders and nearly 77% of drug offenders released from state prisons are rearrested within five years of going free.[3]

Waiting five years to measure the full impact of Proposition 47 would be impractical, but also unnecessary: one-third of those who will be rearrested within five years commit their new crime within six months of release. Moreover, the batch release of prisoners is just one aspect of the initiative's broader policy shift, much of which has not yet been felt. According to the California Legislative Analyst's Office (LAO), an additional 40,000 individuals are expected to be convicted of crimes specified by Proposition 47 in 2015. Many of these offenders will be diverted from

[3] Durose, Matthew R., Cooper, Alexia D, and Snyder, Howard N. "Recidivism of Prisoners Released in 30 States in 2005: Patterns from 2005 to 2010" *Bureau of Justice Statistics*; April, 2014.

prisons and jails, potentially saving counties hundreds of millions of dollars in forgone incarceration costs. At the same time, these individuals will have greater opportunity to commit additional crimes given the less severe sanctions they are now slated to receive.

Understanding the magnitude of the immediate behavioral response to Proposition 47's statutory changes is a vitally important task. The policy shifts mandated by this initiative have not been fully formed or implemented at the time of this draft. Much of the cost savings generated by prisoner reductions are slated to fund addiction and rehabilitation support services in the near future. An understanding of how the 40,000+ individuals impacted by the law changes are responding to them could help to ensure these funds are allocated most efficiently.

Using data from the state's four largest local law enforcement organizations, this paper aims to discover what impact Proposition 47 has had on crime in California since its enactment in November of 2014, and what effects it is likely to have in the coming months and years.

3.2 Related Policies

Since the mid-1990s experts across the country have been warning of an impending criminal justice system crisis, and California was specifically identified as a potential catastrophe following the nationwide adoption of so-called "Three Strikes and You're Out" laws. These laws originated in Washington in 1993 and by 1995 23 states and the federal government had adopted similar legislation. These laws were meant to target the worst of the worst offenders, at a time when crime rates were at or near historical highs. For some context, last year New York City had a population of nearly 8.4 million residents and the annual city-wide homicide total stood at 328 victims. In 1993, the year Washington State passed Initiative 593 ushering in the "Three Strikes" era of criminal justice, New York City had a population of 7.3

million and there were 1,960 murders in the city.

Stories of repeat offenders committing heinous acts convinced citizens that some criminals could not be reformed or deterred from committing future crimes with the threat of what were then typical sentences. Many felt a new approach was necessary to deal with this type of problem criminal. On November 8, 1994, 72% of California voters decided Initiative 184, California's "Three Strikes" law, provided the necessary and prudent tools to fix the state's crime problem. These tools turned out to be among the harshest sentencing requirements in the nation.

For felons with one prior conviction for a serious or violent offense, the law mandated a sentence enhancement. These prisoners would be called "second strikers" and their sentences would typically run twice as long as would have been awarded to another offender who did not have the prior serious/violent felony on their record. Should a second striker subsequently commit another felony, regardless of the nature (serious or otherwise) they would receive a life sentence with a mandatory minimum of 25 years that had to be served before the [now] "third striker" could be considered for parole. In addition, Initiative 184 allowed any prior serious offense to count as a strike, no matter how far in one's past. This language differed from what was adopted by the majority of other states and led to nearly 40% of "third strikers" in California being handed life sentences for non-violent offenses (see Figure C.1.

Immediately following its adoption, two things happened: crime rates continued to drop precipitously (as they had been doing for several years prior), and prisons started to become overcrowded. Less than four years after the state adopted its three strike statute California prisons were home to 36,043 second strikers and 4,468 third strikers.[4] As early as 1999 California's LAO predicted the state would have a 70,000 prison bed shortage by 2006. This turned out to be strikingly accurate,

[4] California Department of Corrections, Data Analysis Unit Report, August 16, 1998.

as California's prison population crested in 2006-2007 when approximately 173,000 prisoners were housed throughout the California Department of Corrections and Rehabilitation (CDCR). At the time that figure represented a little more than double the system's design capacity.

With many facilities running at 200% of capacity or higher, California prisons became criminogenic cesspools. Recidivism rates increased more than 8% for released felons between 2000-2006, with roughly two-thirds of prisoners released for the first time returning within three years. As the prison system seemed to breed career criminals, California's three-strikes law was keeping tens of thousands of offenders locked up with no hope for imminent release - even if their most recent offense was non-violent.

Despite the dire space constraints, it wasn't until the U.S. Supreme Court's decision in *Brown v. Plata* that California began to unwind its prison population.[5] In that decision, the court ruled 5-4 that California's prison overcrowding amounted to a violation of prisoners' Eighth Amendment rights against cruel and unusual punishment. The court ordered California to bring prison populations down to 137.5 percent of design capacity within two years. To meet this requirement, California lawmakers passed several measures collectively known as "realignment" after the title of the centerpiece legislation, AB109, the "Public Safety Realignment Act". That bill shifted the responsibility of administering criminal sanctions for inmates convicted of non-serious, non-violent and non-sexual offenses to local law enforcement jurisdictions. These criminals will no longer be sent to the state's prisons, but will now serve their sentences in county jails. Realignment reduced the prison population drastically in 2011-2012, simultaneously increasing the jail population by approximately one inmate per every three released from prison.

While some portions of AB109 were expected to generate significant cost sav-

[5] Brown v. Plata, 131 S. Ct. 1910, 1923 (2011).

ings, the main goal of the bill was to realign criminal justice incentives within the hierarchical structure of state government. For years, local authorities had arrested and convicted relatively low-risk offenders and sent them off to be warehoused by the state. This occurred in spite of scant evidence that prison was an effective tool to lower crime levels (Chen and Shapiro, 2007; Lofstrom and Raphael, 2013; Zapryanova, 2014). One study by Stolzenberg & D'Alessio (1997), looking at California and Initiative 184 specifically, found little evidence that stiffer penalties played any role in California's crime decline.

By shifting the responsibility for carrying out sanctions to local authorities, AB109 created incentives for local law enforcement leadership and taxpayers to re-think the way they were dealing with petty crime. This proved to be a catalyst that resulted in several voter-led ballot initiatives aimed at further decreasing the total incarcerated population in California; Proposition 47 is unquestionably the most aggressive among them.

3.3 Current Understanding

A specific literature on Proposition 47 does not exist, and the related literature covering prisoner releases is only just now emerging. However there is a rapidly growing literature related to California's broader realignment agenda. Lofstrom and Raphael (2013) were the first to estimate the relationship between realignment (AB109) and California's property crime rates. Those authors found evidence that reductions in incarceration attributable to realignment policies could be associated with nearly 27,000 additional auto thefts annually and 95,000 more property crimes. This effect was specifically linked to a diminished incapacitation effect among former criminals who were committing crimes that otherwise would not have occurred had they remained incapacitated by a (longer) prison sentence.

Proposition 47 is similar to AB109, but goes several steps further. Whereas

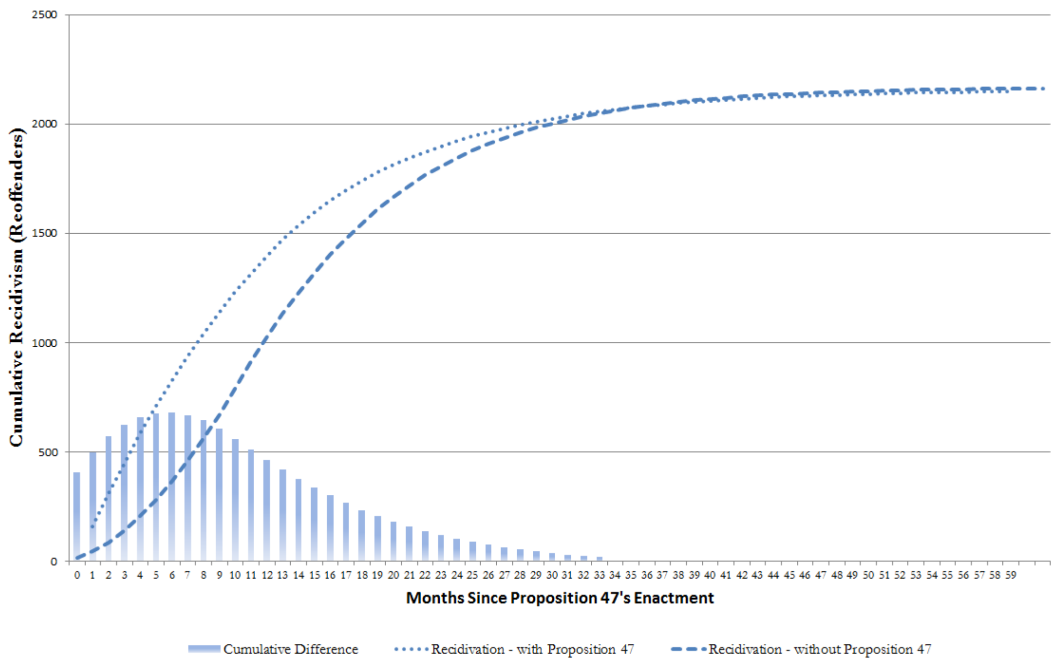
AB109 was prospective, applying only to future crimes, Proposition 47 is retrospective, allowing current inmates to request their cases be reclassified. Even ex-prisoners who had long since served their sentences are eligible to have their felony records changed to misdemeanors. In addition, AB109 did not alter sanctions, it just changed the venue in which they were carried out. Proposition 47, on the other hand, re-classified several felonies that were previously punishable with prison time as misdemeanors which can often be addressed with probation or other community supervision. AB109 would therefore be expected to predominantly effect crime rates by altering the incapacitation effect related to a prison sentence. Proposition 47 would be expected to exhibit the same incapacitation, but would also be expected to exhibit a behavioral effect related to marginal criminals taking advantage of the newly lowered costs of being apprehended.

Recidivism

Proposition 47 has caused the retroactive release of more than 2,700 convicted criminals who were serving prison sentences for drug or property crimes. A 2014 study conducted by the BJS (Durose et al., 2014) followed a cohort of released prisoners in 30 states from 2005-2010 and found that roughly 82 percent of property criminals will re-offend (and be re-arrested) within five years of release. For those released after serving a sentence for a drug crime the five year recidivism rate was nearly 77 percent. This indicates that Proposition 47 released prisoners who are likely to re-offend.

This is not necessarily bad news for Californians, as there is no evidence these prisoners' risk of recidivating is increased (or decreased) by their early release. It does, however, mean that they will generally commit the crimes that will earn a re-arrest sooner than they would have under the previous release regime. In the absence of accurate information about the amount of time each of the 2,700 released prisoners would have otherwise spent incarcerated, let us assume they would have been released uniformly over a twelve month period from December 2014-December 2015. By shifting those release dates to coincide in or around December, 2014, Proposition 47 shifted their criminal careers and the temporal incidence of their crimes.

Figure 3.2: Expected Recidivism of Released State Prisoners



Using the recidivism rates offered by the BJS study, which included inmates from California, Figure 3.2 shows that we should expect a higher number of former offenders to commit new crimes that send them back to jail over the first year following Proposition 47âĽ™s passage. Within the first six months following Proposition 47's enactment it is estimated that an additional 600 ex-prisoners will re-offend compared to the number that wold have been expected had they followed the counter factual release schedule.

This period will then be followed by a lower relative rate of recidivism in subsequent years (this is represented by the flatter slope of the recidivism function in Figure 3.2 until the trajectories once again coincide. The retroactive release of prisoners shifted recidivism so that we can expect more crimes in the near term and fewer in the longer term, but overall the total effect is ambiguous in the absence of information about the individual offenders and the stage/trajectory of their criminal careers.

The number of crimes we ought to expect to see occurring now is also unclear. By June/July of 2015 we should have expected 600 more ex-prisoners to have re-offended and been caught. In California the clearance rates on property crimes vary somewhat, but generally we can expect that between 18 and 30 percent of property crimes are solved according to the FBI's most recent data. Assuming that the release of 2,700 prisoners is large enough to be representative of the criminal talent distributed throughout that population, this means the average criminal is expected to be apprehended somewhere around his/her sixth criminal act. According to the BJS, a little better than half of drug/property offenders recidivate with property crimes, so rudimentary calculations indicate one may expect, based on experience with other groups, something in the ballpark of 1,800 additional property crimes attributable to these ex-offenders.

Behavioral Effects

In addition to creating more opportunity for ex-offenders to commit new crimes, Proposition 47 may also influence incentives faced by all potential offenders. By reducing the sanctions a potential criminal would face if apprehended it is possible that more Californians will find a positive expected benefit from committing property crimes.

Prior research has show that in order to generate a significant effect it is not just necessary that Proposition 47 lowered the sanctions a potential criminal would face, but also that the potential criminal is able to perceive that change (Durlauf and Nagin, 2011). Helland and Tabarrok (2007) tested whether criminals accurately perceive changes in sanction regimes by comparing the records of individuals convicted of a second strike to a group of criminals charged with a second strike but who ended up being convicted of a less serious offense. Apparently the second strike scared more than a few straight, reducing that groups' felony re-arrest rates by 20 percent. No evidence has yet been offered with regard to the salience of Proposition 47's re-classifications among the criminal population.

Both the potential behavioral effect and incapacitation effect are theorized to be criminogenic in the case of Proposition 47, so uncovering a structural break in the property crime time-series immediately following the retroactive release of eligible prisoners would provide strong evidence that the initiative has effected crime levels. Unfortunately, there is not enough data currently available for statistical testing at this time.

3.4 California Crime Trends

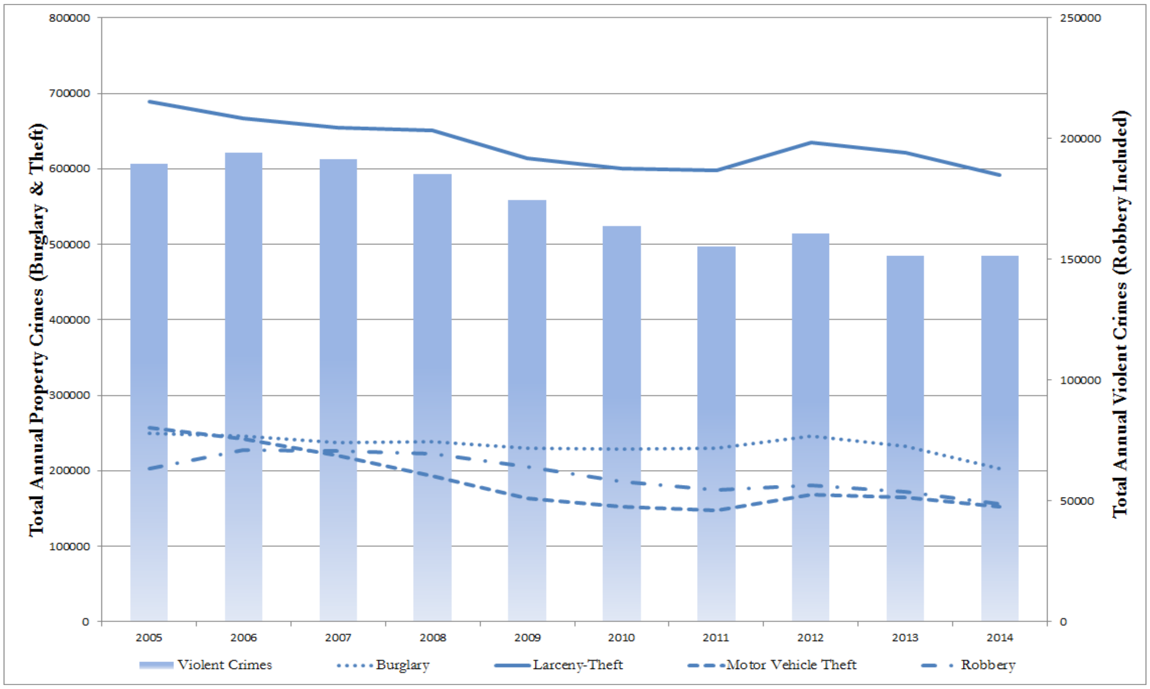
As with any significant reform, those potentially impacted will want to know whether it is going to make them better or worse off. In the case of Proposition 47, this requires determining whether property crime rates have increased subsequent to its adoption. The law only impacts sanctions for non-violent offenders, so naturally one would expect burglary, larceny and auto theft to be the most responsive to the policy shift. A definitive answer to the question of whether or not property crime levels have been significantly impacted by Proposition 47 will require additional crime level observations to be recorded. With only a few months of observations from only a handful of local police jurisdictions, it is nearly impossible to say that any observed change represents a new crime rate trajectory for California.

While it is almost impossible to say what specific impact Proposition 47 has had, it is more feasible to attempt to rule out impacts it has not had. Intuitively, one might realistically expect that releasing thousands of convicted offenders back into the community could generate an immediate impact on crime rates. The following section will evaluate the likelihood that Proposition 47 has caused property crime levels to increase significantly. Data provided by California's four largest metro areas and their respective law enforcement agencies indicate that recent property crime levels are not markedly different from those that were observed prior to November 4, 2014.[6]

California, like most of the U.S., is currently enjoying some historically low crime levels. As Figure 3.3 shows, both violent crime and property crime have fallen state-wide over the past decade. As would be expected, this ebb in violent

[6] Data analyzed in this section is generated by the following local law enforcement agencies: Los Angeles Police Department (LAPD), San Jose Police Department (SJPD), Carlsbad Police Department, Chula Vista Police Department, Coronado Police Department, El Cajon Police Department, Escondido Police Department, La Mesa Police Department, National City Police Department, Oceanside Police Department, San Diego Harbor Police Department, San Diego Police Department (SDPD), and the San Francisco Police Department (SFPD))

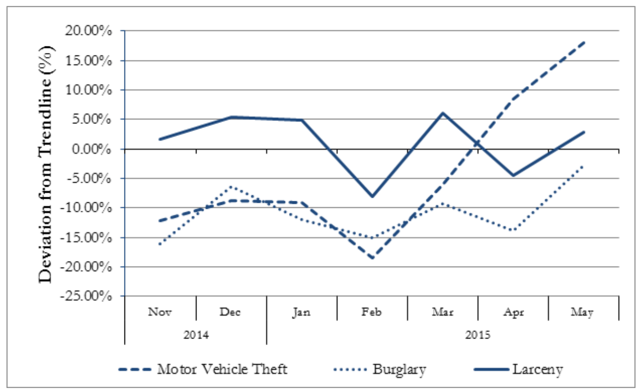
Figure 3.3: Statewide Crime (Annual Totals)



crime allowed state leadership to shift priorities away from relatively unproductive uses (prisons) to more productive ends (schools). Should the tide quickly turn, many of California's recent reforms may well go with it.

One pocket of humanity bucking the broader state-wide crime decline is San Francisco. This can be seen visually in Figure C.2, which displays San Francisco's monthly property crime totals. These time-series show that monthly totals for larceny, burglary, and auto theft were higher in May (2015) compared to their levels immediately prior to Proposition 47 taking effect in October (2014). Obviously this does not mean that Proposition 47 caused the level of crime to increase, and the full time-series confirms this to be an unlikely scenario. Located in the appendix, Figures C.3, C.4, and C.5 show that all three types of property crime were increasing long before Proposition 47 was even authored. Furthermore, in all three cases a positive linear time trend well approximates the change crime levels. Figure 3.4 is a plot of the residuals that result from comparing post-Proposition 47 observed values to the pre-Proposition 47 trend line. It offers little evidence to support the conclusion that there has been a structural change in any of the series' trends.

Figure 3.4: Residual Plot of Observed Monthly Crime Totals Against the Pre-Proposition 47 Linear Trend



Elsewhere in the Bay Area, San Jose has not seen the same upward trend in property crime that San Francisco has experienced over the past four years. Despite the two citiesâĽ™ close geographic proximity, Figure C.6 shows monthly property crime totals in San Jose have recently been roughly at or below the levels they were at in January 2013. Furthermore, of the three property crimes tracked, only larceny is (slightly) above the level it was at in November, 2014.

In Southern California crime rates seem to have almost universally increased following Proposition 47's passage into law. In Los Angeles, the LAPD reports that as of August 8, 2015 year-to-date totals of rape, robbery, assault, burglary, auto theft, and larceny are all larger than they were at this point in 2014. In San Diego County, residents are in a similar situation. Figure C.7 shows that property crimes in San Diego are up relative to Proposition 47's reforms taking effect. All of these statistics are factually correct, but contextually misleading.

In both Los Angeles and San Diego, the first half of 2014 marked the nadir for crime rates in both cities' modern history. In San Diego, the major crimes index hit a 35 year low in 2014. The burglary rate fell to it's lowest level since 1959 and automotive theft totals were lower than they had been at any point in time since 1974. In L.A., 2014 marked the eleventh consecutive year that the city's major crime index fell relative to the previous year.

Looking further back in Los Angeles' property crimes time-series, Figure C.9 clearly shows that the first quarter of 2014 saw a dramatic decrease in all types of crime. Compared to the full history of data collected by the LAPD current crime rates are right in line with the recent levels Angelenos are used to, and much lower than they have historically enjoyed, as evidenced in Figure C.8.

In the few jurisdictions that have released 2015 crime statistics there does not appear to be strong evidence that the policy changes associated with Proposition 47 have led to significantly more property crime. If true, this would be surprising and would also be in conflict with a large literature that has consistently found lower sanctions and increased street time for ex-offenders have significant positive effects on crime. This is especially true in light of Lofstrom & Raphael's (2013) findings related to realignment. On the other hand, there is good reason to believe that a more detailed counter factual analysis would uncover just these effects, which are perhaps being masked by other trends, such as the state's falling unemployment rate. Such an undertaking is outside of the scope of this study, the purpose of which is to quantify the immediate economic costs and benefits associated with Proposition 47's implementation.

Projected Economic Impact

Costs

Roughly nine million persons, or 23% of California's total population, fall under the jurisdiction of the law enforcement agencies providing data for this analysis. Operating on the simplifying assumption that these four geographic areas are representative of the state's population, Table C.1 can be used to quantify the monetary losses associated with the additional crime observed subsequent to Proposition 47's enactment. This table reports the average count of monthly burglaries, larcenies, robberies and auto thefts for a six-month period before Proposition 47 was passed (May-October 2014) and another six month period following it's adoption December 2014 - May 2015). Using the average monetary loss associated with each crime, the table also calculates the average monthly losses attributable to property crimes in each of the four locations listed before, and after, Proposition 47 became law.[7] Values enclosed in parentheses indicate that the average monthly count of crime increased following the change in the statutes and those figures represent the additional monies lost by the respective community.

Property crime increases that have occurred subsequent to Proposition 47's enactment have an average monthly cost of $934,605.67 across these four areas. If one assumes these communities are representative, then extrapolating out to the state level yields an estimate of approximately $49 million in tangible property is being lost annually to increased property crime.

This is only a partial accounting of the economic costs associated with crime. In addition to the value of the property that is taken there are psychological costs, health risks, diminished quality of life, lost productivity and psychic costs associated with the heightened vigilance necessary when crime levels rise. These are the intangible costs of crime and they tend to be much larger than the monetary value of the property lost. In Table 3.1 several estimates of these costs are offered for each type of offense. The table then uses the average change in monthly totals for each crime subsequent to Proposition 47 taking effect (see Table C.1) to calculate a range of estimates of the total economic cost to Californians of living with increased property crime. These figures are once again generated under the assumption that

[7] Tangible monetary loss values used here are taken from the Table 23 of: United States Department of Justice, Federal Bureau of Investigation. *Crime in the United States, 2013*; September 2014

Table 3.1: Total Cost of Crime Estimates (2014 Dollars)

Crime		Low Estimate	Moderate Estimate	High Estimate
Robbery	Total Loss	$28,331[2]	$26,649[3]	$46,394[1]
	Implicit Intangible Loss	$27,142	$25,460	$45,160
Burglary	Total Loss	$3,268[2]	$5,793[3]	$7,086[1]
	Implicit Intangible Loss	$908	$3,433	$4,726
Larceny	Total Loss	$2,960[4]	$3,244[3]	$3,873[1]
	Implicit Intangible Loss	$1,681	$1,965	$2,594
Auto Theft	Total Loss	$10,527[3]	$11,812[1]	$21,146[4]
	Implicit Intangible Loss	$4,458	$5,743	$15,077
Monetary Losses in California (Monthly)		$11,760,890	$12,911,075	$22,016,787
Monetary Losses in California (Annual)		$141,130,680	$154,932,900	$264,201,444
Estimated Benefits to California(Annual)[5]		$500,000,000	$750,000,000	$1,000,000,000
Net Benefit (Annual)		**$358,869,320**	**$595,067,100**	**$735,798,556**

[1]McCollister, French & Fang (2010), [2]Miller, Cohen & Wiersma (1996), [3]Cohen & Piquero (2009), [4]Roman (2009), [5]Durose et al. (2014)

the changes observed in San Francisco, Los Angeles, San Diego County, and San Jose are representative of the entire state.

Benefits

The changes induced by Proposition 47 have many benefits for Californians, the majority of which are intangible. Families that would have previously been seperated by a parent's incarceration will now remain intact, employers will see less turnover as fewer employees are lost to the criminal justice system, and a number of workers will not see their human capital atrophy and depreciate as they spend all day confined to a cell. There are also quantifiable benefits, most of which come in the form of cost-savings to taxpayers. The California LAO projects the state government will save between $100 million to $300 million each year, beginning next fiscal year, thanks to a lower state prison population. This is on top of an additional savings of between $400 million - $700 million that is expected to be generated at the county and municipal level.[8]. These have yet to be realized, but given estimates that the average cost of housing an inmate runs $47,421 annually, the savings from the retroactive release of 2,700 prisoners will save $128 million on an annualized basis alone.[9]

The bottom of Table 3.1 shows three estimates of the net benefits California is expected to realize each year as a result of Proposition 47's reforms. Even taking into account the intangible costs of crime, it appears that based on early (and limited) data describing the criminal response to the new law the state should still expect to see significant economic benefits. While these net benefits to the state are large, that fact is surely of little consolation to a recent property crime victim. Good mechanisms do not currently exist that use cost savings to make victims whole. Furthermore, the geographic distribution of crime may be such that the incidence of benefits do not fall on the same parties upon which the initiative's cost burden is borne. We also don't know how the law has impacted vigilance. While fair amount of research has investigated criminals salience of changes in sanction regimes, the author of this paper does not know of any research measuring the salience of changes in sanction regimes among potential victims. If it is in fact the case that Californians spent several hundred million dollars on personal security equipment ahead of Proposition 47's adoption, that would not only be consistent with the data, but would also significantly alter the conclusions reached in the next section.

[8] Legislative AnalystâĽ™s Office (LAO). (2014). "Proposition 47: Criminal Sentences. Misdemeanor Penalties. Initiative Statute" At: `http://www.lao.ca.gov/ballot/2014/prop-47-110414.pdf`

[9] Henrichson and Delaney (2012)

3.5 Conclusion

Even with the potential for an increase in crime in the short-term, the projected savings that Proposition 47 will generate appear to far outweigh the possible costs. Estimates, which include both the tangible and intangible economic costs associated with increased criminal activity, indicate California should see a net economic benefit of between $360 million and $730 million annually. These benefits are not necessarily evenly distributed and it is very conceivable that some parties will be made worse off as a result of these policy shifts. Furthermore, these estimates have not been subjected to rigorous statistical testing and do not take into account other events co-occurring, such as the falling unemployment rate. Any number of factors could be masking a behavioral response to the diminished criminal sanctions introduced by this initiative, and as a result it is impossible say with any certainty whether property crime will remain at current levels.

These are just projections at this point, so the economic surplus ultimately realized may be substantially larger or substantially smaller than what was calculated above. The goal of this ballot initiative was to re-direct funding and low-level criminals away from California's expensive prison system into more effective and less costly rehabilitation services that can return productive citizens to society. The exact amount of those savings will be reported by the Department of Finance on July 31 of 2016, at which point the state hopes to reinvest those dollars by funding grants to support K-12 students in public schools. The state has also earmarked some of the cost savings to provide mental health services, substance abuse treatment, diversion programs, and compensation for victims. These are all important tools and have their own associated economic benefits. Should investments in these programs prove successful, the estimates presented above will surely fall well below realized values. On the other hand, if the state is unable to redirect savings to these programs, which law enforcement leaders agree are necessary to support Proposition 47's policy changes, it is doubtful any of the economic benefits outlined above will materialize.

A APPENDIX TO CHAPTER ONE

Table A.1: Stop, Frisk & Question Events by Race

RACE	2006	2007	2008	2009	2010	2011	2012	Total
White	53,500	52,887	57,650	53,601	54,810	61,805	50,366	384,619
	10.56%	11.21%	10.74%	9.28%	9.18%	9.09%	9.52%	9.86%
Black	267,466	243,766	275,588	310,611	315,083	350,743	284,229	2,047,486
	52.81%	51.69%	51.32%	53.76%	52.79%	51.58%	53.71%	52.52%
Indian	1,863	1,762	2,031	2,271	2,579	2,897	2,257	15,660
	0.37%	0.37%	0.38%	0.39%	0.43%	0.43%	0.43%	0.40%
Black Hisp	28,608	27,831	32,513	35,207	38,689	48,438	35,772	247,058
	5.65%	5.90%	6.05%	6.09%	6.48%	7.12%	6.76%	6.34%
White Hisp	119,254	114,037	135,962	144,848	150,637	175,302	129,368	969,408
	23.55%	24.18%	25.32%	25.07%	25.24%	25.78%	24.45%	24.86%
Asian	13,477	13,150	16,491	16,845	19,732	23,932	17,058	120,685
	2.66%	2.79%	3.07%	2.92%	3.31%	3.52%	3.22%	3.10%
Other	22,321	18,202	16,730	14,346	15,360	16,867	10,102	113,928
	4.41%	3.86%	3.12%	2.48%	2.57%	2.48%	1.91%	2.92%
Total	506,489	471,635	536,965	577,729	596,890	679,984	529,152	3,898,844

Table A.2: Stop, Frisk & Question Events by Borough

Borough	Black Population	White Population	Stop Rate	Frisk Rate	Search Rate	Arrest Rate	Contraband † Recovery Rate
Staten Island	55,014	350,679	4.6%	41.5%	7.2%	5%	2.5%
	11.74%	74.81%					
Manhattan	267,365	948,073	5.8%	49.6%	9.9%	8.2%	3.7%
	17.07%	60.55%					
Queens	471,634	949,686	4.3%	55.6%	9.8%	6.8%	2.9%
	20.93%	42.15%					
Bronx	537,967	428,377	5.1%	65.4%	9.9%	6.4%	2.7%
	38.91%	30.98%					
Brooklyn	896,165	1,120,587	7%	51.2%	6.5%	4.1%	2.1%
	35.78%	44.74%					

† Contraband Recovery Rate = $\frac{ContrabandRecovered}{TotalStops}$

Table A.3: Probit Regression (Y = Arrest) w & w/o Precinct Controls

	With Precinct Dummies				Without Precinct Dummies			
	Radio		No Radio		Radio		No Radio	
Regressor	β	S.E.	β	S.E.	β	S.E.	β	S.E.
Black	.0230***	.0063	.0296***	.0086	-.0256***	.0055	-.0299***	.0075
$\frac{\partial Arrest}{\partial Black}$	.0018	.0005	-.0049	.0006	-.0066***	.0003	-.048	.0005
Search	1.920***	.0090	1.915***	.0125	1.920***	.0089	1.910***	.0123
$\frac{\partial Arrest}{\partial Search}$	.4152	.0032	.3780	.0016	.3837	.0012	.3901	.0016
Black × Search	-.1405	.0096	-.0882	.0133	-.1391***	.0095	-.0830***	.0130
$\frac{\partial Arrest}{\partial Black*Search}$	-.0088	.0024	-.0451	.0044	-.0646	.0030	-.0442	.0042
Stop Ratio	-.0001**	.0004	-.0007	.0005	.0009***	.0001	-.0029***	.0007
$\frac{\partial Arrest}{\partial StopRatio}$	-.0001	.0000	-.0000	.0000	.0001	.0000	-.0000	.0000
n	2,361,590		1,471,268		2,361,590		1,471,268	
McFadden's R^2	.3322		.3344		.3205		.3190	

This regression includes two specifications and two populations. The two specifications are with and without precinct dummies. The two sample populations are one with includes all 2,361,590 stops of black & white suspects from 2006-2012 and another which excludes radio runs, on-going investigations and reported crimes (n = 1,471,268). Radio runs are stops which include any police radio traffic requesting or reporting service. Covariates omitted from the table include F.E. dummies, year dummies, time of day, an age dummy, a dummy for whether the stop took place inside or outside and a dummy for whether the suspect was frisked. The variable "Stop Ratio" refers to $\frac{\frac{BlackStops}{BlackPopulation}}{\frac{WhiteStops}{WhitePopulation}}$

Table A.4: Linear Regressions with Fixed Effects

	Year F.E.		Precinct F.E.		Year × Precinct F.E.	
Regressor	β	Std. Error	β	Std. Error	β	Std. Error
Black	-.0058***	.0003	-.0089***	.0003	-.0089***	.0003
Search	.2602***	.0009	.2601***	.0009	.2601***	.0009
Black × Search	-.0600***	.0010	-.0600***	.0010	-.0601***	.0010
Constant	.0008**	.0004	.0050***	.0013	-.0016	.0039

This regression includes all 2,361,590 stops of black & white suspects from 2006-2012. Only stops missing geographic identifiers were omitted. Covariates omitted include F.E. dummies, time of day, an age dummy, a dummy for whether the stop took place inside or outside and a dummy for whether the suspect was frisked

Table A.5: Probit Regression of Contraband Recovery on Race

Number of obs = 443,827
Psuedo R^2 = 0.2697

	Searched		Black		Black*Searched	
Group Size	A.M.E.	S.E.	A.M.E.	S.E.	A.M.E.	S.E.
Alone	.1750***	.0036	-.0114***	.0013	-.0723***	.0101
2	.1798***	.0031	-.0131***	.0011	-.0895***	.0086
3	.1807***	.0048	-.0126***	.0017	-.0772***	.0147
4	.1650***	0069	-.0067**	.0039	-.0312	.0218
5	.1735***	.0108	-.0078***	.0039	.0117	.0325
6	.0744***	.0108	.0093***	.0033	.0786***	.0274

Only stops involving multiple suspects are considered in this table. If police fail to recover any contraband during a stop/search the dependent variable is coded as 0, it is coded as 1 otherwise. Columns 2,4, and 6 present the average marginal effects of searching, whether the suspect is black, and the interaction of those two binary indicators (respectively) over the size of the group stopped. Standard errors are adjusted for 76 Precinct clusters

Table A.6: Variables Interacted with Race Indicator

	A.M.E.	S.E.
Bottom 25% (Most Equitable)	.001982***	(.00046)
Top 50% (Median)	-.00135***	(.00037)
Top 25% (Least Equitable)	-.00216***	(.00040)
Overall	-.00609***	(.00034)
Age > 25	.0055***	(.0002)
Multiple Suspects	.0003	(.0003)
Furtive Movements	-.0017***	(.0006)
R^2: .2827		
n: 2,361,593		

Average marginal effects associated with an interaction term for each listed regressor and a dummy denoting whether the suspect is black. Covariates omitted include all individual factor variables in the interaction effects and F.E. dummies on time/location. Standard errors are clustered by time and group (groups are created by quartiles of the sampling distribution of black-to-white stop ratios).

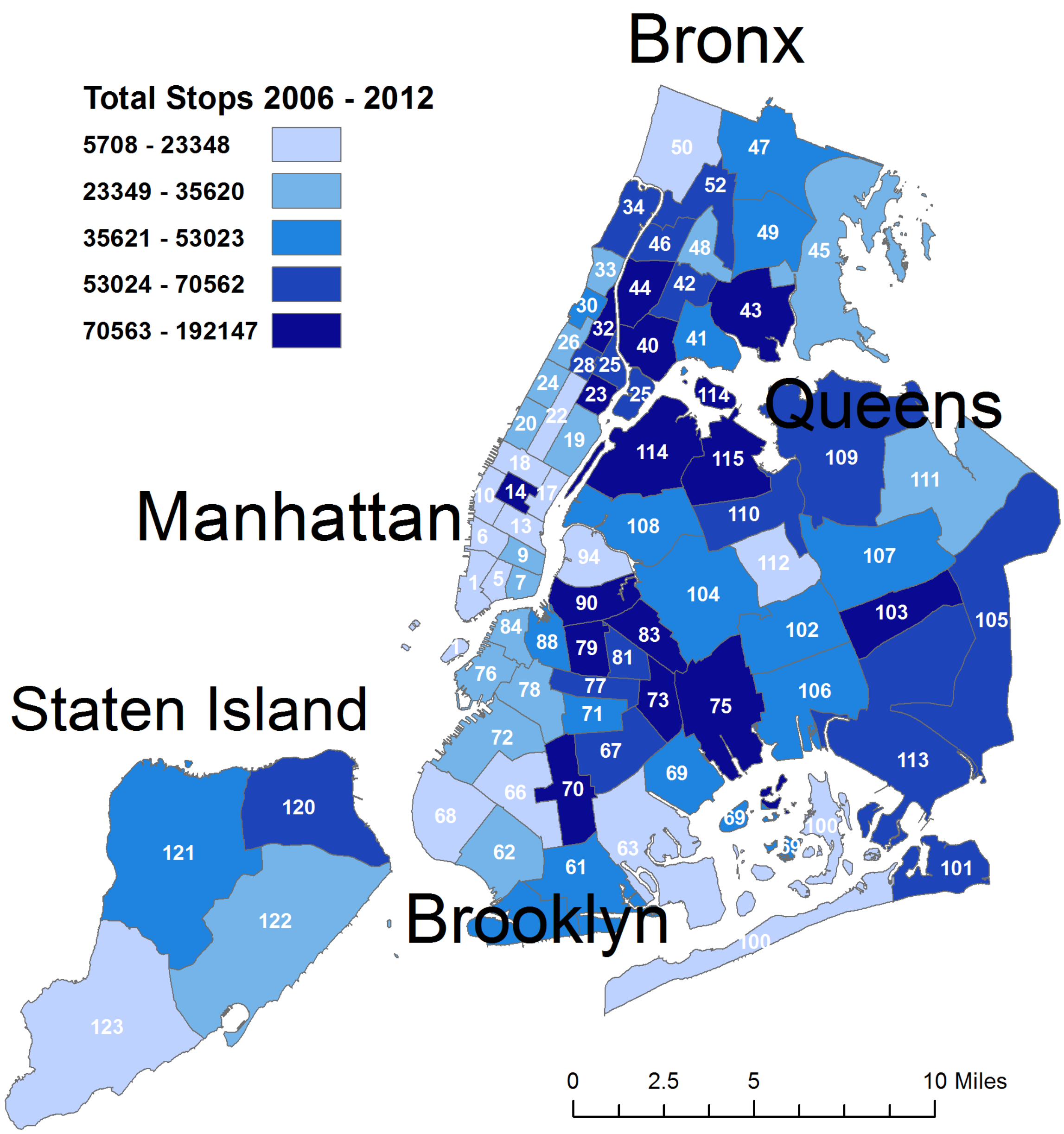


Figure A.1: Total Encounters in Each Precinct

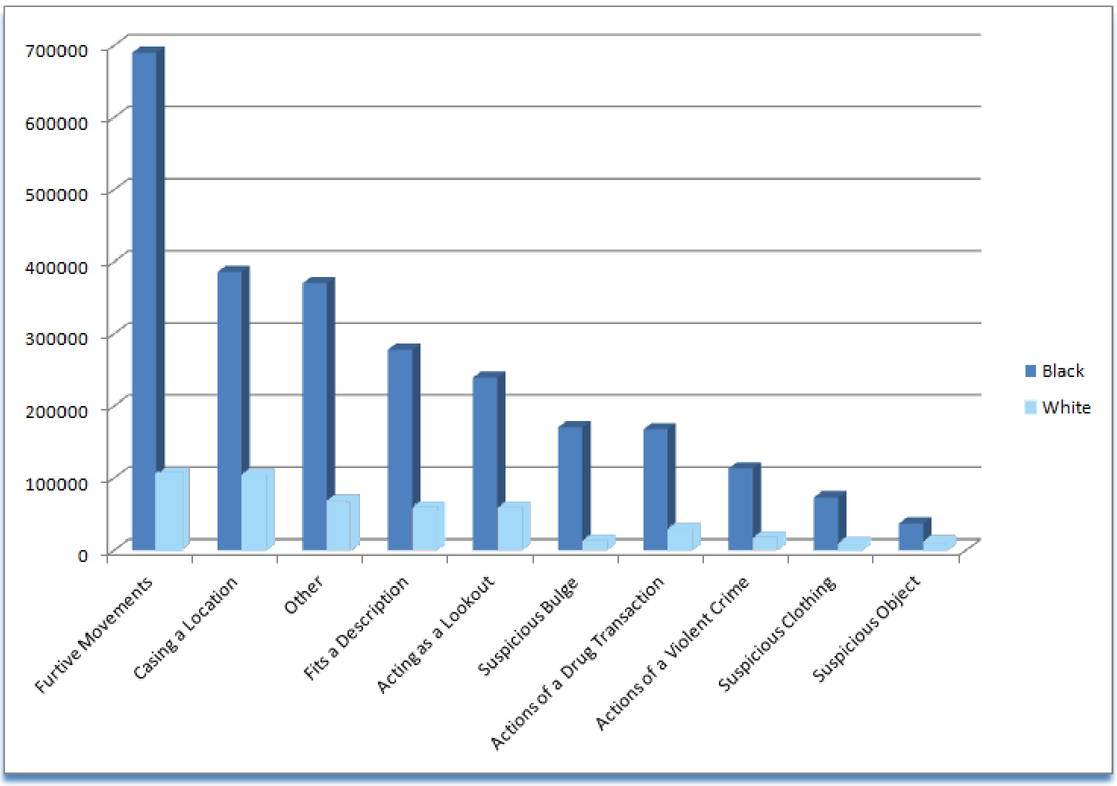


Figure A.2: Reasons given for SQF stops of white & black suspects (2006-2012)

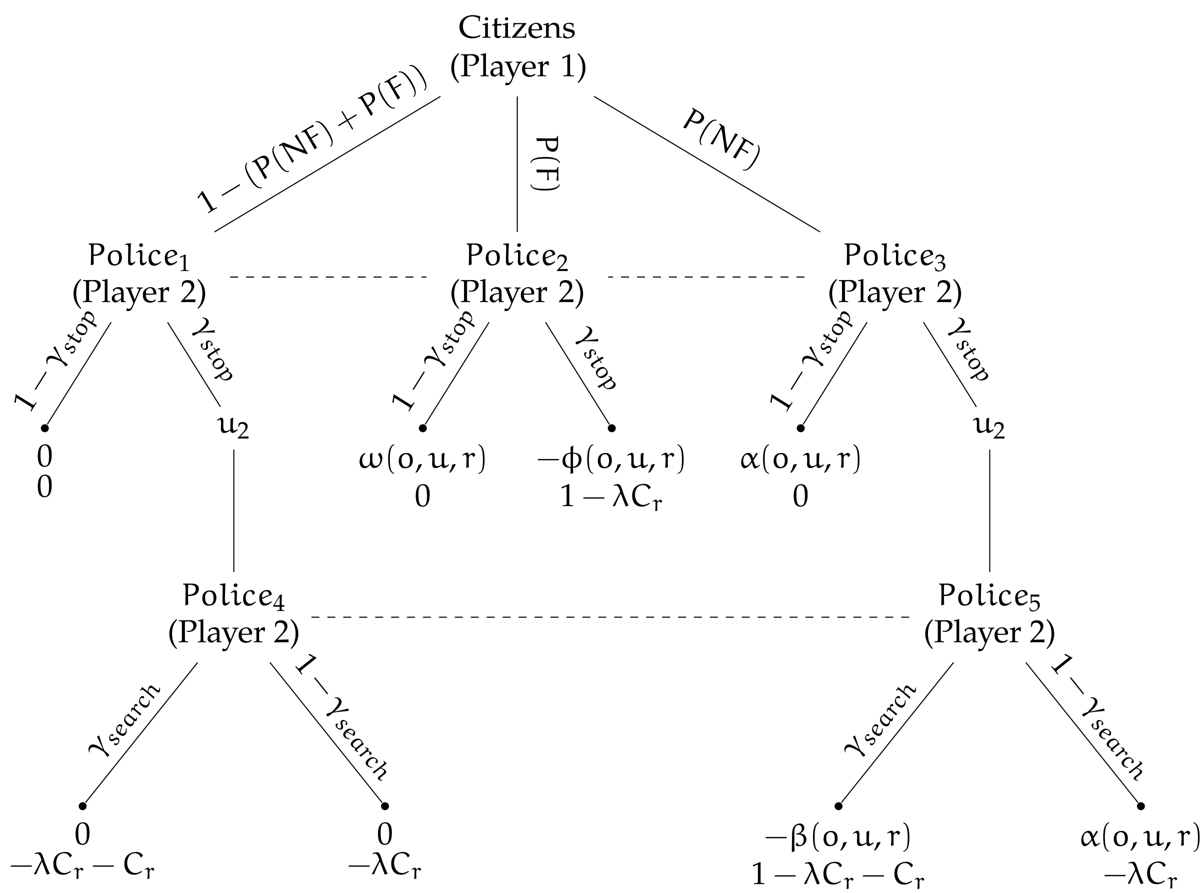


Figure A.3: *Extensive form representation of model payoffs.*

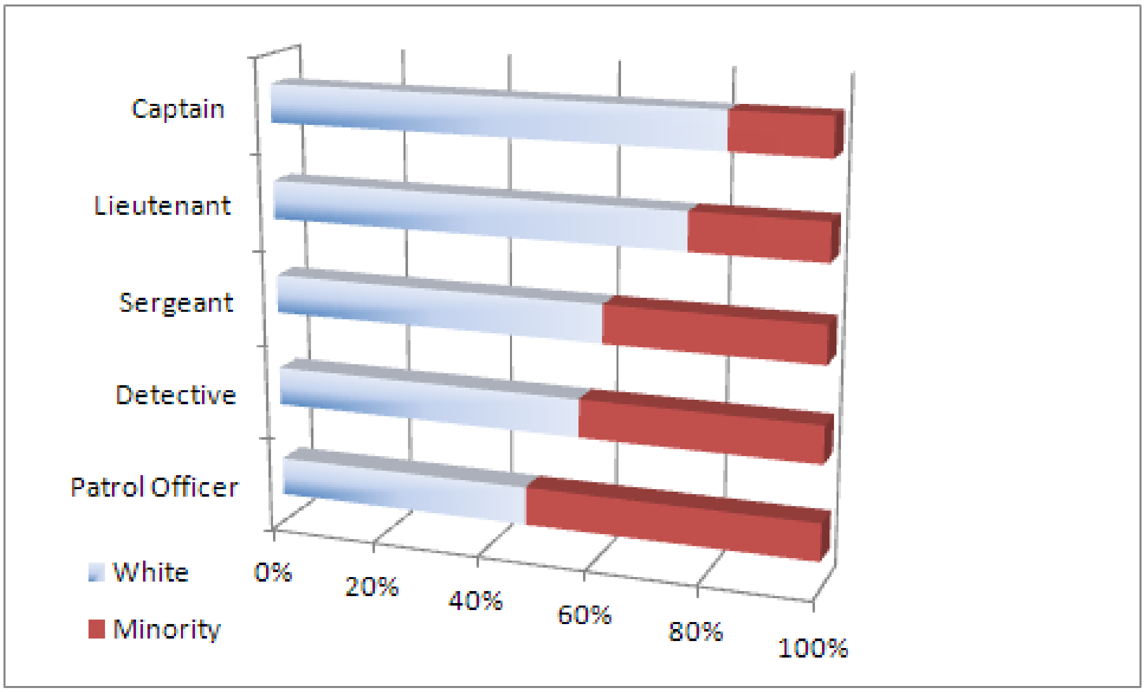


Figure A.4: NYPD Officer Ethnicity by Rank (2012)

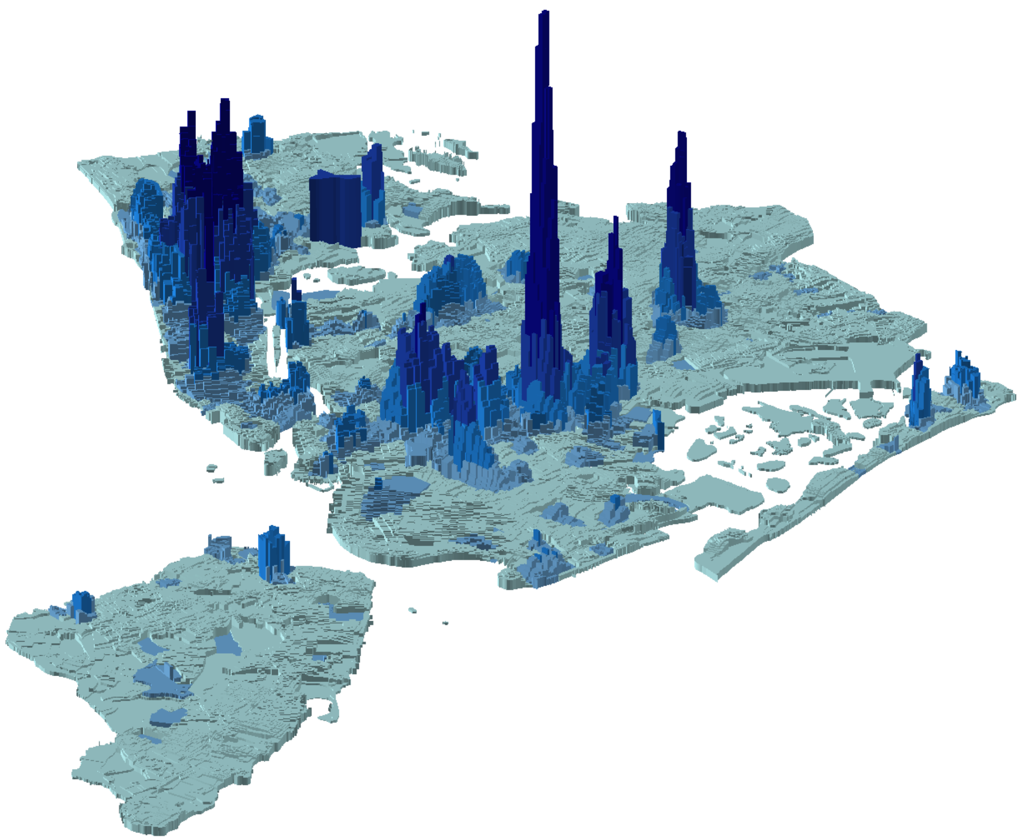


Figure A.5: Color represents the probability that each census block is an outlier in the Getis-Ord Gi* distribution of precinct stop intensities. Light blue represents 90% probability, dark blue represents 99% probability. The height of each feature is a scaled measure of the associated z-score.

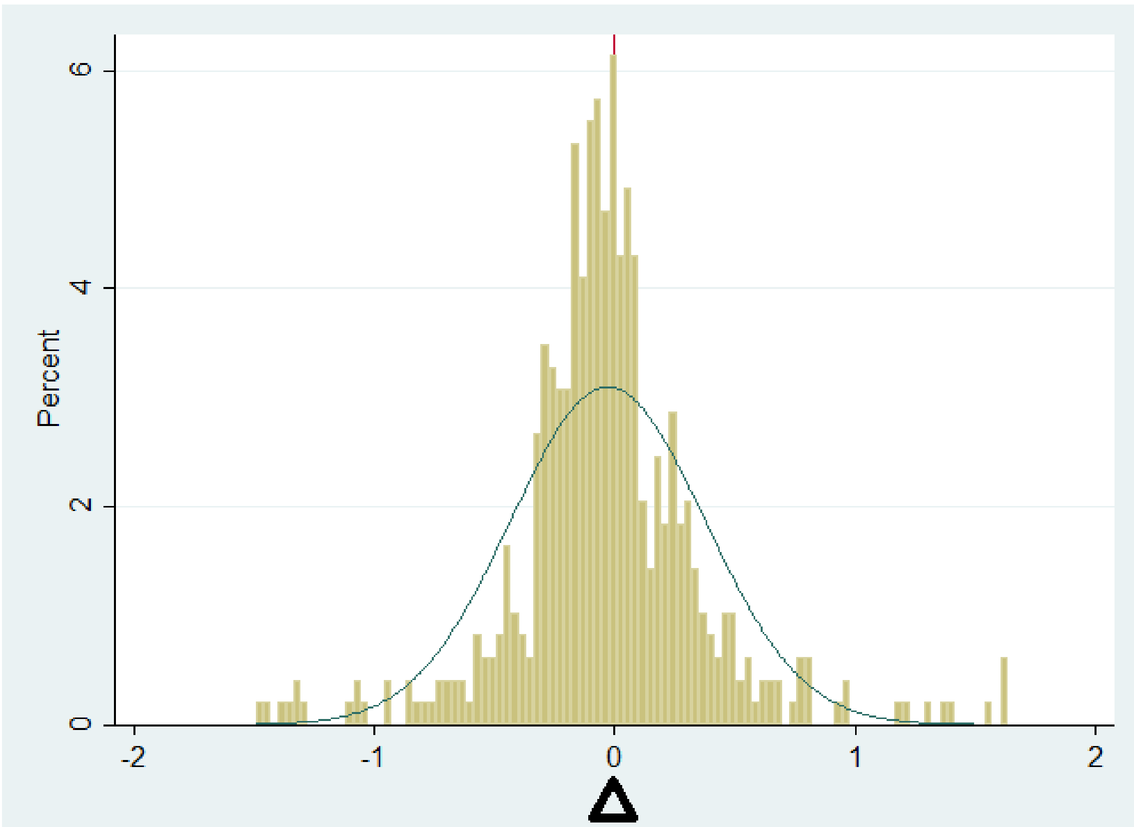


Figure A.6: Distribution of Δ

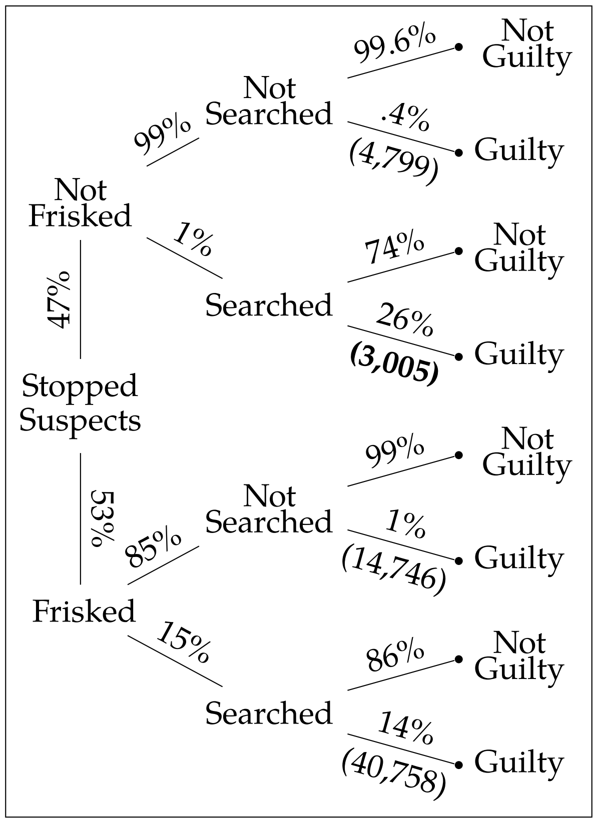


Figure A.7: Percentages reflect portion of parent node associated with the child along that edge. Numbers in parentheses reflect the contraband recoveries associated with each of the four possible partial histories (for instance, Frisked,Search).

B APPENDIX TO CHAPTER TWO

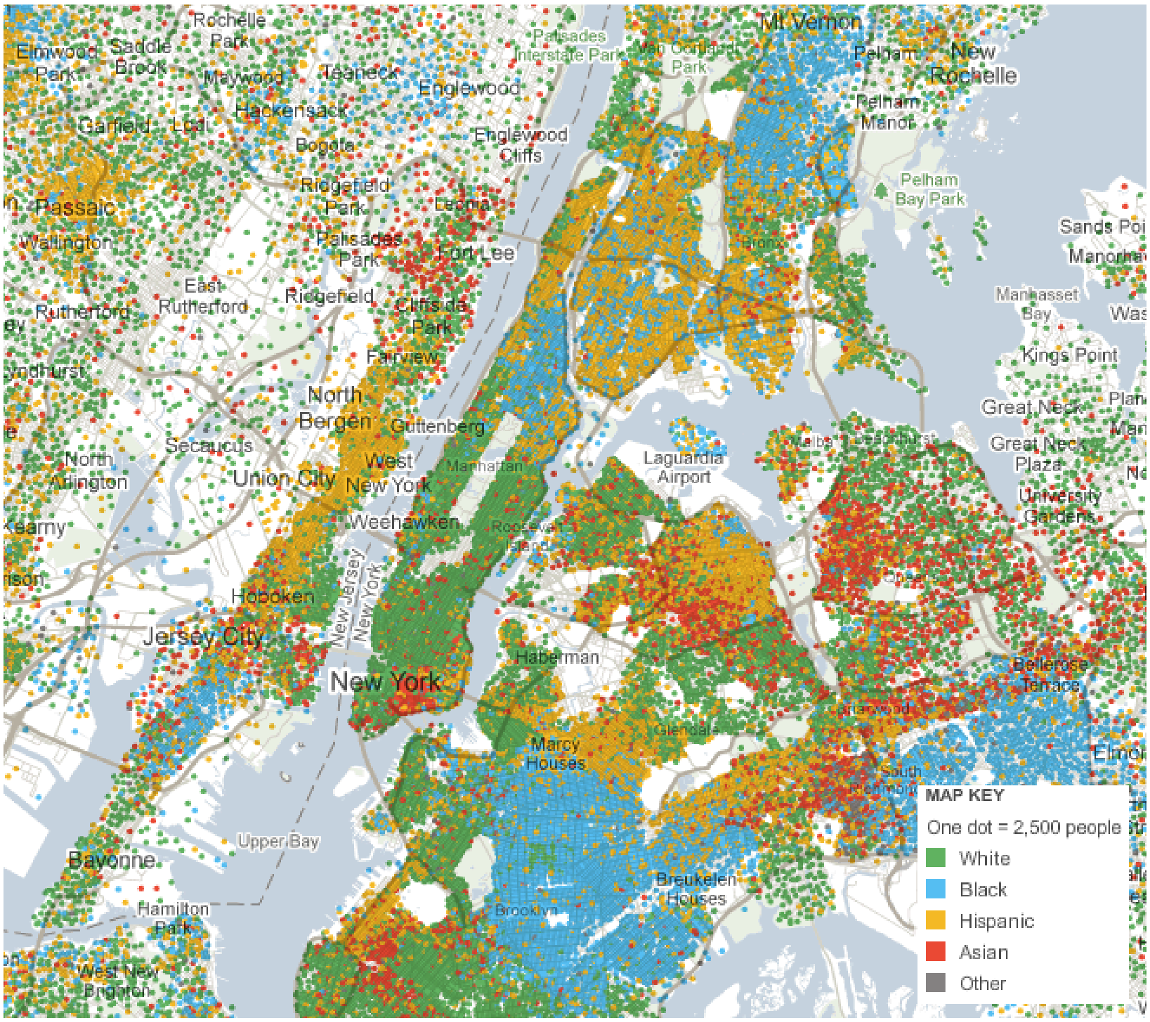
Figure B.1: New York City Community Districts

Source: NYC Department of City Planning

Figure B.2: New York City Neighborhoods

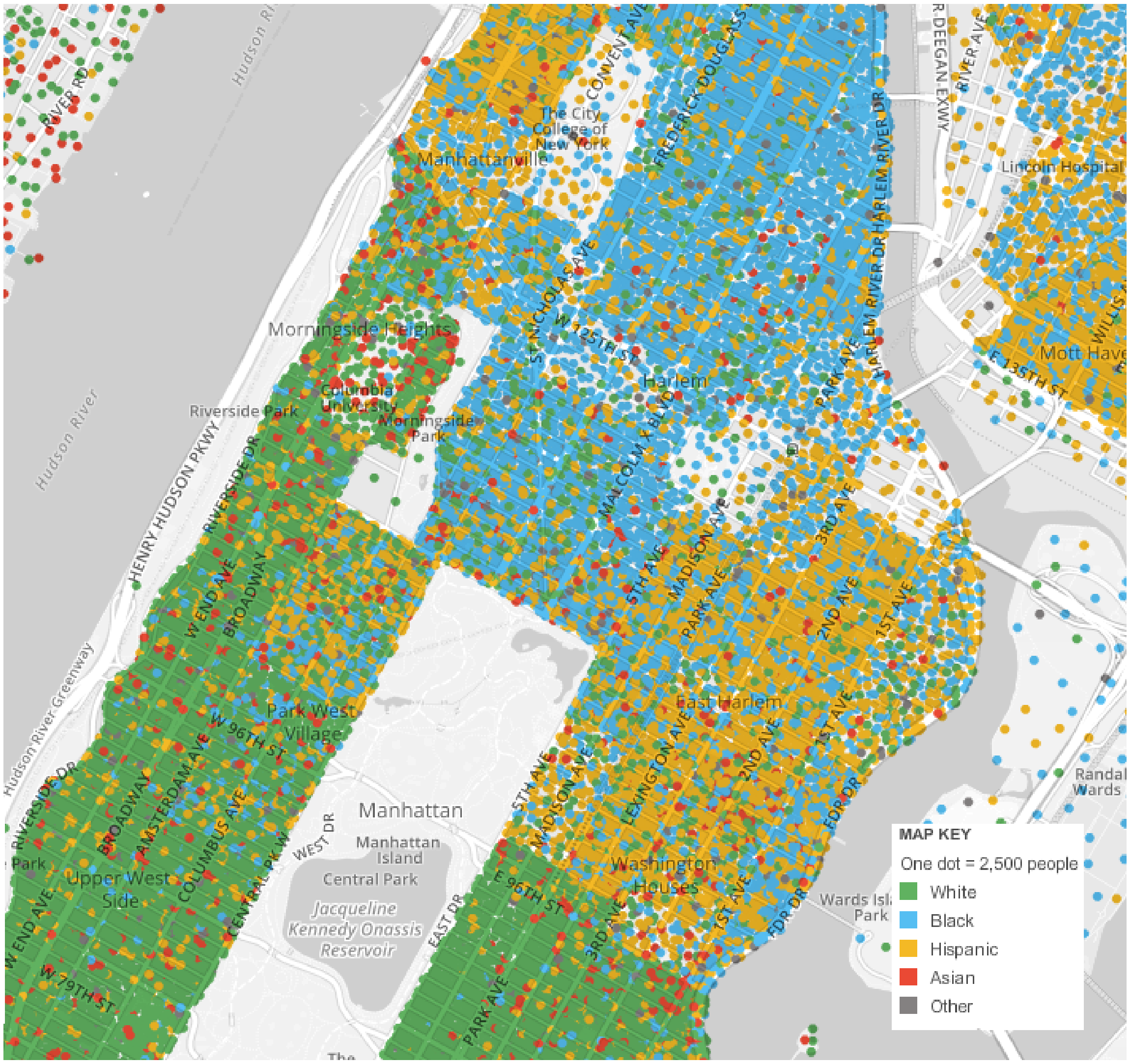
Source: NYC Department of City Planning

Figure B.3: Racial Distribution of Residents (NYC)



Based on 2005-2009 ACS. Image courtesy of the New York Times.

Figure B.4: Racial Distribution of Residents (Upper Manhattan)



Based on 2005-2009 ACS. Image courtesy of the New York Times.

Figure B.5: Hot-Spot Analysis using $Getis - Ord\ Gi*$ Statistic

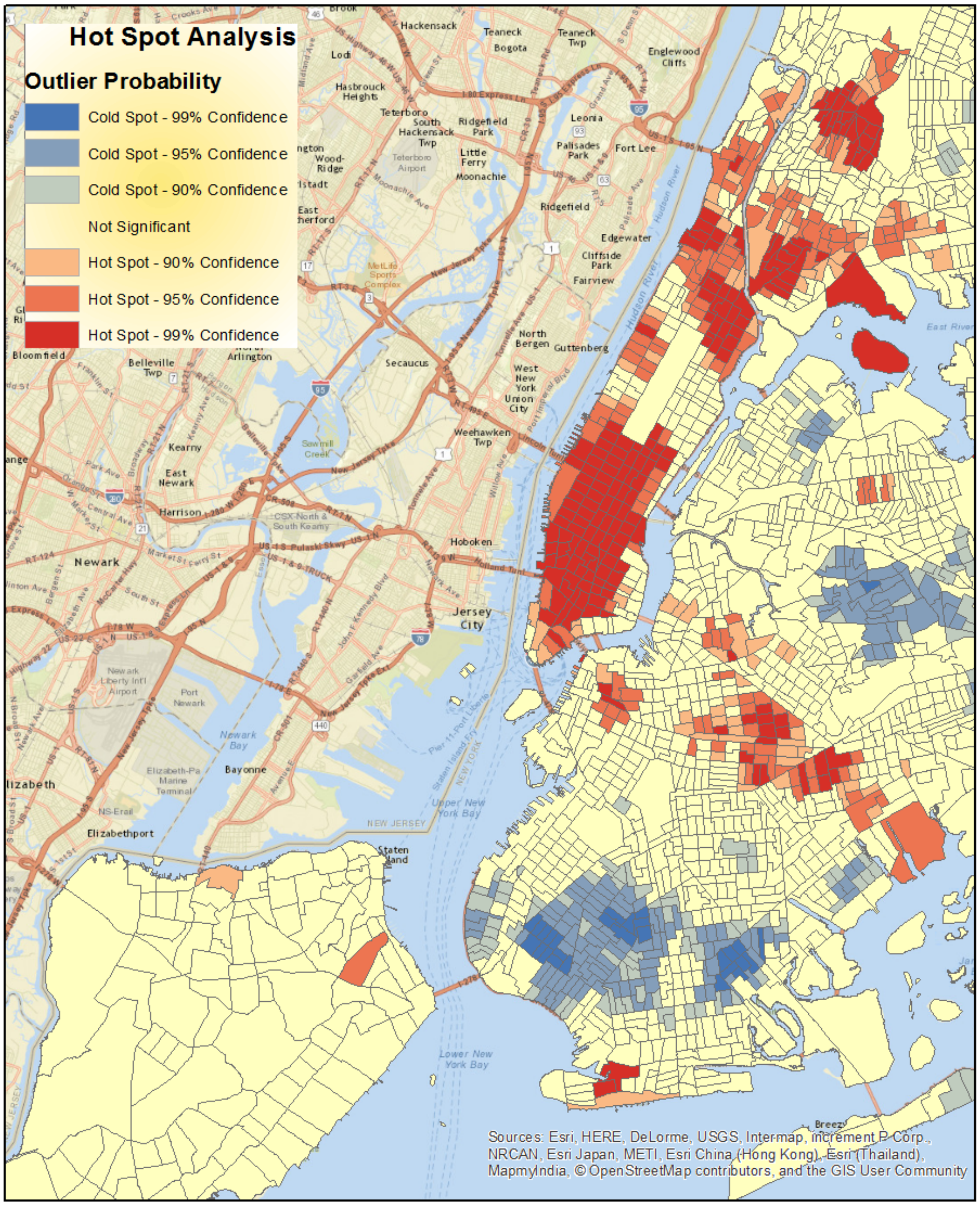
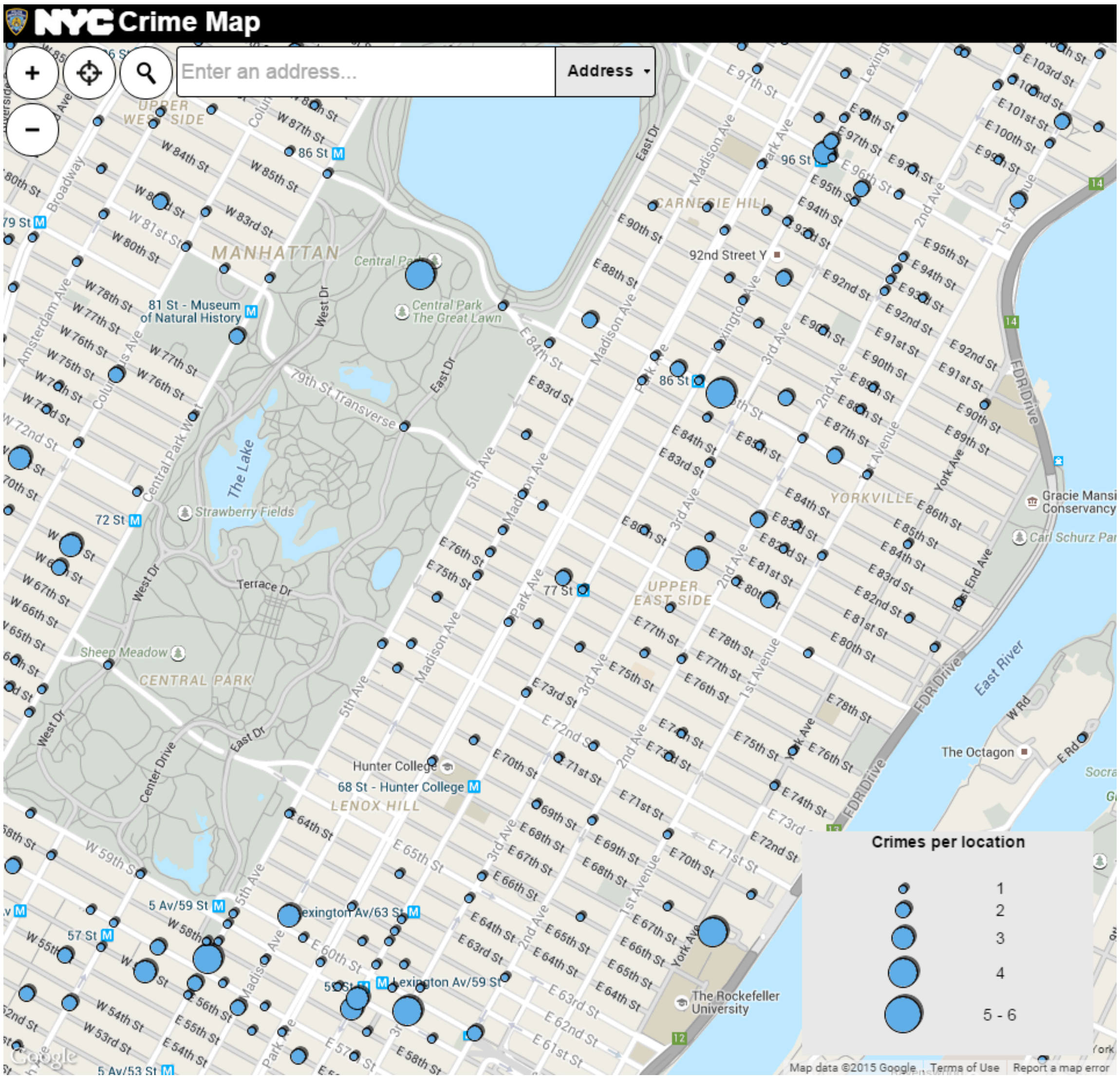
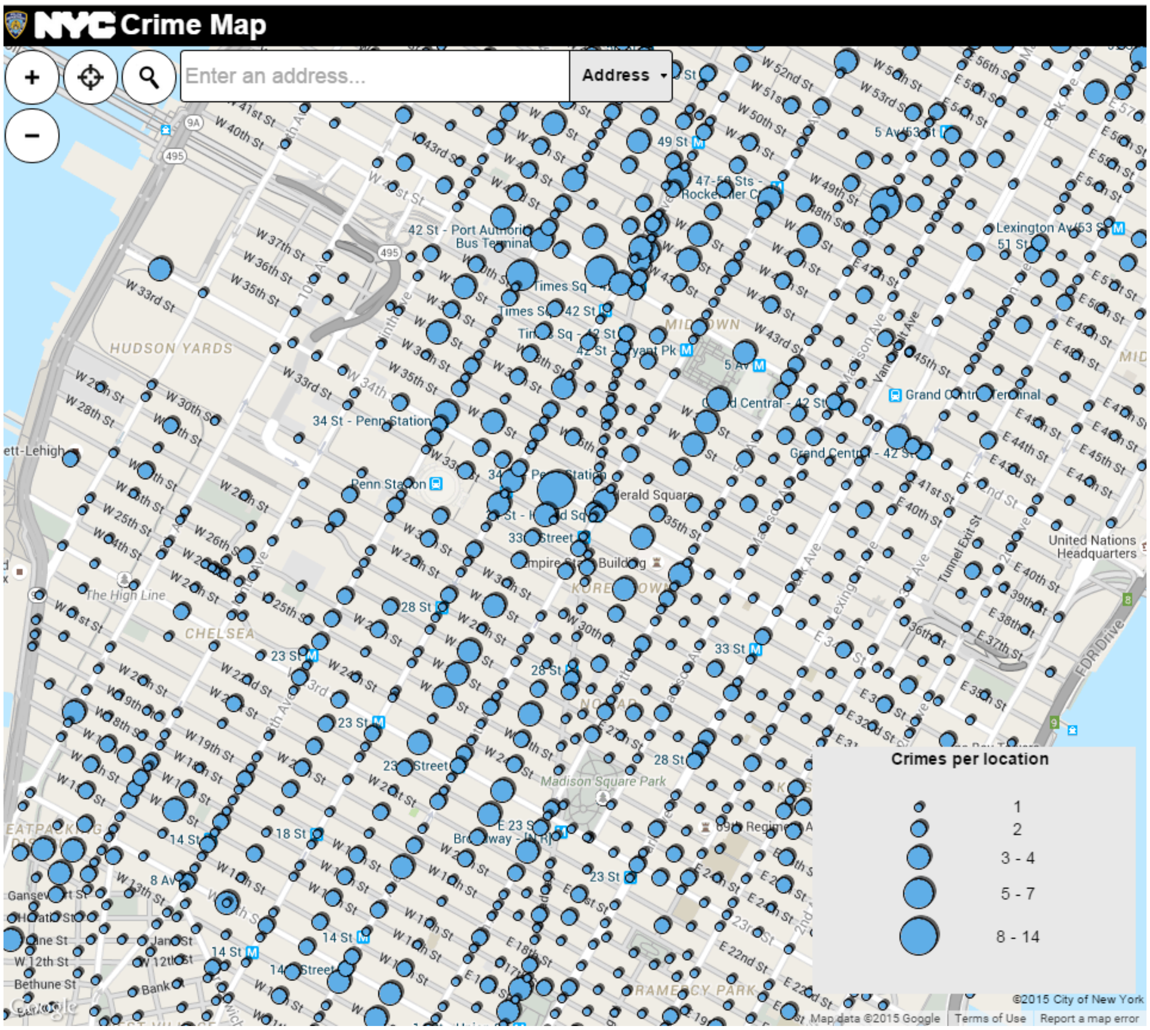


Figure B.6: Distribution of Crime in East Manhattan (Q12015)



Source: New York Police Department

Figure B.7: Distribution of Crime in Chelsea (Q12015)



Source: New York Police Department

Figure B.8: Important Local Amenities

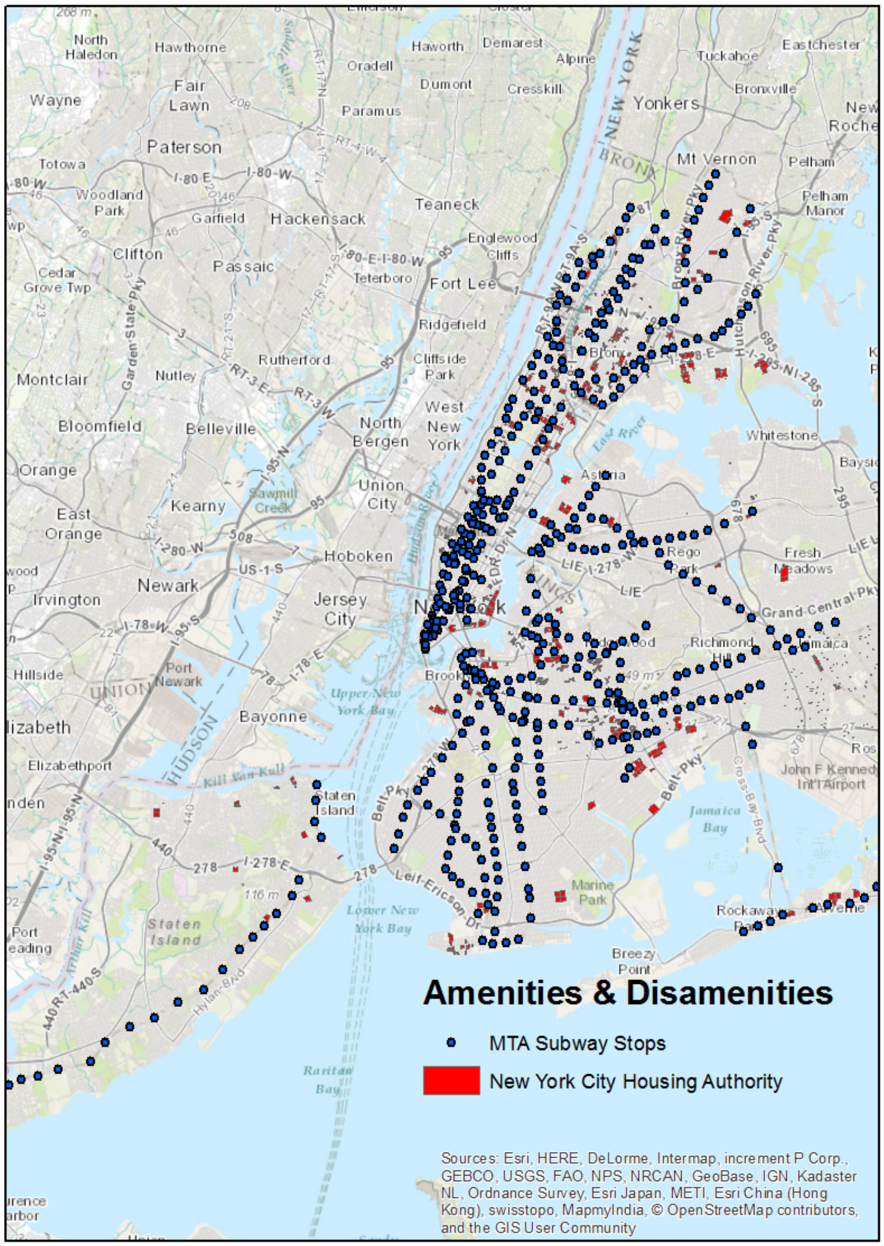


Figure B.9: Important Local Amenities

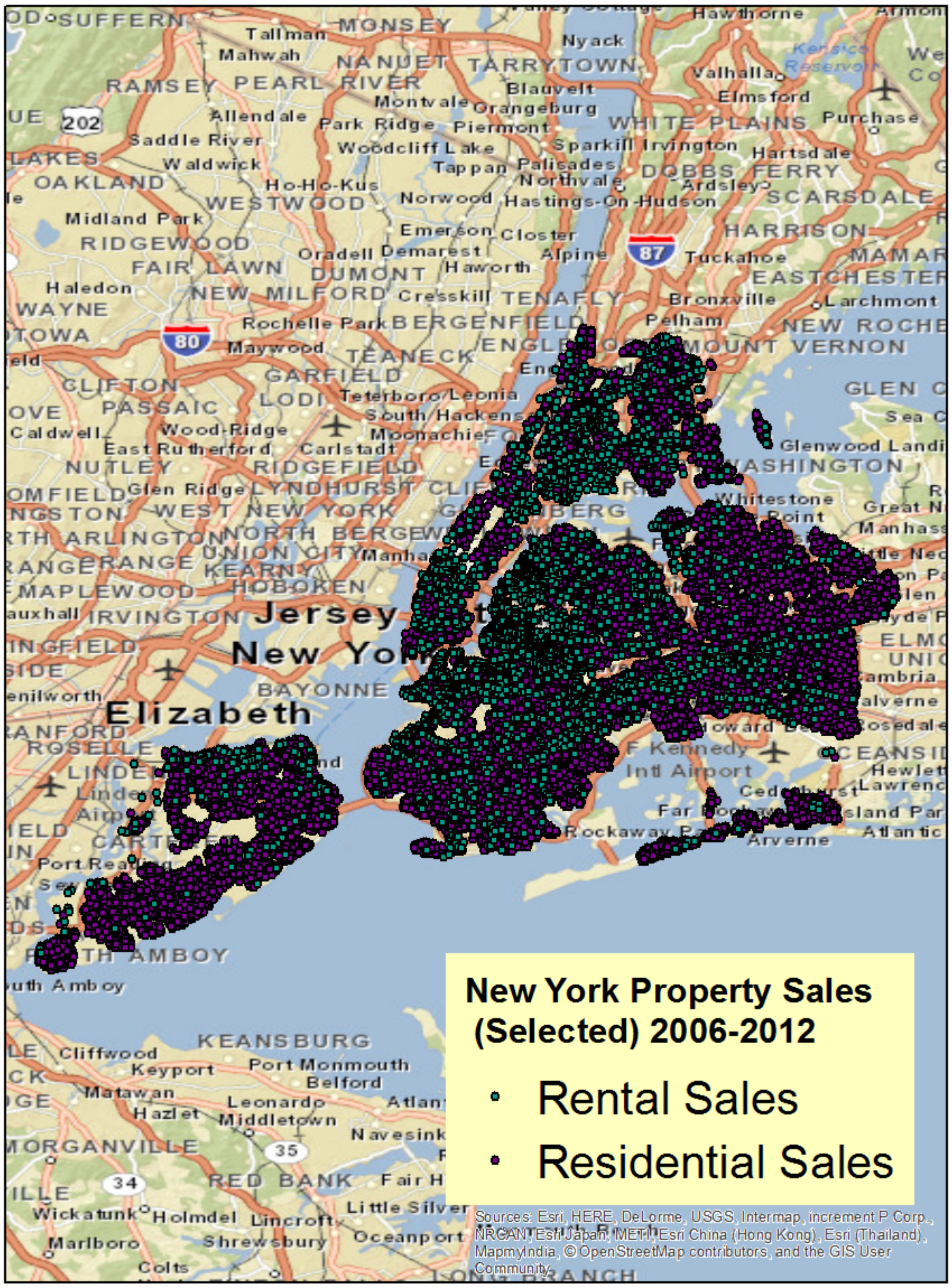


Table B.1: Selected Manhattan Neighborhood Boundries

Name of the neighborhood	Limits south to north and east to west
Midtown	34th to 59th Streets
Columbus Circle	59th Street and 8th Avenue
Sutton Place	53rd to 59th Streets; 1st Avenue to Sutton Place
Rockefeller Center	49th to 51st Streets; 5th to 6th Avenues
Diamond District	47th Street from 5th to 6th Avenues
Theater District	42nd to 53rd Streets; 6th to 8th Avenues
Turtle Bay	42nd to 53rd Streets; East River to Lexington Avenue
Midtown East	42nd to 59th Streets; East River to 5th Avenue
Midtown	40th to 59th Streets; 3rd to 9th Avenues
Tudor City	40th to 43rd Streets; 1st to 2nd Avenues
Little Brazil	46th Street from 5th to 6th Avenues
Times Square	39th to 52nd Streets; 7th to 9th Avenues
Hudson Yards	28th to 43rd Streets; 7th Avenue to the Hudson River
Midtown West	34th to 59th Streets; 5th Avenue to the Hudson River
Hell's Kitchen, Clinton	34th to 57th Streets; 8th to the Hudson River
Garment District	34th to 42nd Streets and 5th to 9th Avenues
Herald Square	34th Street and 6th Avenue
Koreatown	31st to 36th Streets; 5th to 6th Avenues
Murray Hill	34th to 40th Streets; 3rd to Madison Avenues
Tenderloin	23rd Street to 42nd Streets; 5th to 7th Avenues
Madison Square	23rd to 26th Streets; 5th Avenue to Broadway

Source: Wikipedia

Table B.2: Distribution of Twelve Month Stop Totals by Borough

		Number of Stops				
Quartile	Buffer Distance	Manhattan	Bronx	Brooklyn	Queens	Staten Island
25%	1,000 Feet	259	41	78	42	17
	500 Feet	44	8	16	9	3
	250 Feet	7	0	1	0	0
50%	1,000 Feet	505	120	180	93	33
	500 Feet	103	23	38	21	6
	250 Feet	15	2	3	1	0
75%	1,000 Feet	1034	345	490	208	68
	500 Feet	210	74	97	48	14
	250 Feet	32	8	9	4	2
Mean	1,000 Feet	713	270	358	167	82
	500 Feet	163	62	84	42	18
	250 Feet	30	10	9	5	3
Std. Dev.	1,000 Feet	618	378	453	219	167
	500 Feet	182	100	129	65	59
	250 Feet	45	24	24	16	15
Max	1,000 Feet	4038	3919	8398	2911	2645
	500 Feet	1,560	1,165	1,854	1,317	1,102
	250 Feet	514	442	1,005	888	465
N	-	1,169	10,990	31,554	51,904	18,411

Table B.3: Description of Select Covariates

Variable	Obs	Mean	Std. Dev.	Min	Max
Stop & Frisks (250 feet)	113,007	6.7538	21.1590	0	1005
Stop & Frisks (500 feet)	113,007	55.5545	97.9929	0	1854
Stop & Frisks (1,000 feet)	113,007	221.7108	336.3902	0	8398
Land ft^2	113,007	2,968.41	2,121.008	250	116130
Structure ft^2	113,007	2,094.413	930.6061	216	30758
Year Built	113,007	1945.8	31.5848	1900	2013
Sale Price	113,007	575,070	846,569.9	75000	50000000
Residential Units	113,007	1.6356	0.6752	1	3
Distance to Subway (feet)	113,007	5,609.6840	5,323.56	1.9165	26,779.77
Distance to NYCHA (feet)	113,007	5,816.4990	7,294.09	0	53,482.2
NYCHA Population	113,007	1,363.0890	1,487.9490	0	5274
Census Tract Population	113,007	3,974.5460	1,977.671	1	26,588
Census Tract Median Age	113,007	36.9224	5.4310	13.4	76
Census Tract Pop. > 16	113,007	3,167.6290	1,585.383	1	22,727
Census Tract Pop. > 65	113,007	490.3589	328.5849	0	6,287
Census Tract % White	113,007	42.9493	31.5131	0	100
Census Tract % Black	113,007	29.4120	33.7003	0	100
Census Tract % Asian	113,007	13.4086	16.0681	0	88.1
Census Tract Avg. Family Size	113007	3.4292	0.3586	0	6.09
Arrest (%)	113,007	0.0608	0.0569	0	1
Summons (%)	113,007	0.0556	0.0554	0	1
Frisked (%)	113,007	0.5136	0.171	0	1
Searched (%)	113,007	0.0864	0.0640	0	1
Contraband Found (%)	113,007	0.0202	0.0320	0	1
Gun Found (%)	113,007	0.0092	0.0168	0	0.5
Black or Latino Suspect (%)	113,007	0.6788	0.2855	0	1

Other dummy regressors include subway lines accessible from the nearest station, school districts, and specific housing projects

Table B.4: OLS Regression of Sale Price on Quadratic Police Activity

Number of obs = 43,508
Adjusted R^2 = 0.7319

Sale Price	Coefficient	Robust Standard Error	t
Stops	-.51.87	12.96	-4
$\mathtt{Stops}^2$	.006	.004	1.46
Structure $\mathtt{ft}^2$	172.23	3.51	49.11
Land $\mathtt{ft}^2$	19.99	2.62	7.64
Distance to Subway (feet)	-8.91	2.27	-3.92
Dist to NYCHA (feet)	6.83	2.16	3.17
# of Units	-105.33	40.54	-2.60
Year Built	173.06	60.31	2.87
Constant	-188387.3	539086	-0.35

Number of stops must exceed 100

Table B.5: Linear Regression of Sale Prices on Police Stops (1,000 Foot Buffer)

Number of obs = 113,007				R^2 (within) = .1926
Number of groups = 1,350				R^2 (between) = .5713
$\rho = .9369$				R^2 (overall) = .6335
	β	Std.Err.	t	$P > \|t\|$
Number of Police Stops	-39.97471	6.864964	-5.82	0.000
Total Units	-52572.31	2813.564	-18.69	0.000
Land ft^2	17.53237	.8195966	21.39	0.000
Gross ft^2	178.1455	2.19887	81.02	0.000
Year Built	100.451	58.15034	1.73	0.084
Distance to Subway (feet)	-2.384221	.9332766	-2.55	0.011
Distance to NYCHA (feet)	1.064294	.8597527	1.24	0.216
NYCHA Population	3.892363	1.522101	2.56	0.011
Median Age (Tract)	3968.508	619.685	6.40	0.000
Total Population (Tract)	-7.854514	1.919115	-4.09	0.000
Pop. > 65 (Tract)	21.94592	12.22024	1.80	0.073
Percentage Black (Tract)	-578.9679	139.0052	-4.17	0.000
Average Family Size (Tract)	18977.25	6861.197	2.77	0.006
Arrest (%)	-61205.12	38182.3	-1.60	0.109
Frisk (%)	-47618.41	12943.28	-3.68	0.000
Search (%)	1414.772	34790.48	0.04	0.968
Contraband Recovery (%)	89532.06	52813.41	1.70	0.090
Gun Recovery (%)	-44209.88	91615.11	-0.48	0.629
Black or Latino (%)	-23897.98	13423.22	-1.78	0.075
Constant	-73753.95	121962.1	-0.60	0.545

Table B.6: Linear Regression of Sale Prices on Police Stops (500 Foot Buffer)

Number of obs = 113,007 R^2 (within) = .2254
Number of groups = 1,350 R^2 (between) = .5739
$\rho = .9369$ R^2 (overall) = .6380

	β	Std.Err.	t	$P > \|t\|$
Number of Police Stops	-91.2395	20.0737	-4.55	0.000
Total Units	-52697.95	2813.947	-18.73	0.000
Land ft^2	17.5595	.8197	21.42	0.000
Gross ft^2	178.1101	2.199	81.00	0.000
Year Built	100.7817	58.1631	1.73	0.083
Distance to Subway (feet)	-2.2077	.9321	-2.37	0.018
Distance to NYCHA (feet)	1.1608	.8595	1.35	0.177
NYCHA Population	3.7757	1.5222	2.48	0.013
Median Age (Tract)	4065.274	619.2165	6.57	0.000
Total Population (Tract)	-8.3291	1.9150	-4.35	0.000
Pop. $>$ 65 (Tract)	23.4773	12.2140	1.92	0.055
Percentage Black (Tract)	-584.7175	139.0122	-4.21	0.000
Average Family Size (Tract)	19217.91	6861.311	2.80	0.005
Arrest (%)	-57663.91	38172.69	-1.51	0.131
Frisk (%)	-46200.89	12941.1	-3.57	0.000
Search (%)	3380.979	34788.61	0.10	0.923
Contraband Recovery (%)	90171.5	52816.48	1.71	0.088
Gun Recovery (%)	-42107.41	91619.26	-0.46	0.646
Black or Latino (%)	-27932.12	13380.81	-2.09	0.037
Constant	-79628.92	121969.9	-0.65	0.514

Table B.7: Borough Level Calculations with Racial Controls

	β	Std. Dev	Avg Sale Price	% Change
Manhattan	-85.28	618.17	$5,148,268	-.01%
Bronx	-12.48	377.89	$439,088	-.01%
Brooklyn	-33.16***	453.5	$634,009	-2.4%
Queens	-10.33***	218.56	$511,127	-0.4%
Staten Island	-39.60	167.43	$445,121	-1.5%

***Significant at 99% confidence level

Table B.8: Linear Regression of Rental Unit Prices on Police Stops with Neighborhood Effects

Number of obs = 11,320				R^2 (within) = 0.4202
Number of groups = 943				R^2 (between) = 0.5923
$\rho = .6196$				R^3 (overall) = .5968

	β	Std.Err.	t	$P > \|t\|$
Number of Police Stops	-306.33	177.92	-1.72	0.085
Distance to NYCHA (feet)	56.71	61.20	0.93	0.354
Distance to Subway (feet)	-7.101	99.82	-0.07	0.943
Public Housing Pop.	44.56	78.42	0.57	0.570
Residential Units	8476.44	1506.36	5.63	0.000
Commercial Units	160268.1	59156.61	2.71	0.007
Year Built	16957.59	3884.56	4.37	0.000
Gross ft^2	228.86	3.70	61.93	0.000
Land ft^2	-328.65	12.95	-25.37	0.000
Population > 65	108.10	11.45	9.44	0.000
Total Population	-23.17	1.85	-12.50	0.000
Arrest Made (%)	-62107.75	36920.86	-1.68	0.093
Gun Recovered (%)	-53075.05	102360.4	-0.52	0.604
Constant	4190894	6894715	.061	0.543

Table B.9: Linear Regression of Commercial Property Sale Prices on Police Stops

Number of obs = 2,638	R^2 (within) = 0.2821
Number of groups = 687	R^2 (between) = 0.3983
$\rho = .7037$	R^2 (overall) = 0.6950

	β	Std.Err.	t	$P > \|t\|$
Number of Police Stops	903.71	718.60	1.26	0.21
Commercial Units	62908.14	16409.04	3.83	0.00
Land ft^2	22.097	8.95	2.47	0.01
Gross ft^2	96.42	5.72	16.86	0.00
Year Built	6053.54	9596.00	0.63	0.53
Distance to Subway (feet)	-195.87	233.66	-0.84	0.40
Distance to NYCHA (feet)	269.66	182.50	1.48	0.14
Housing Population	-148.77	287.86	-0.52	0.61
Frisked (%)	-66035.58	13908.24	-4.75	0.000
Constant	-9452867	1.86e+07	-0.51	0.612

C APPENDIX TO CHAPTER THREE

Table C.1: Tangible Monetary Losses from Property Crime (Pre & Post Proposition 47)

Crime (Avg. Loss)		**Pre-Proposition 47** Avg. Monthly Totals		**Post-Proposition 47** Avg. Monthly Totals		
	City	Incidents	Cost	Incidents	Cost	Pre-Post
Burglary ($2,322)	Los Angeles	1,186.83	$2,755,827	1,285.5	$2,984,931	($229,104)
	San Diego	873	$2,027,106	834	$1,936,548	$90,558
	San Francisco	486	$1,128,492	506	$1,174,932	($46,440)
	San Jose	414.5	$962,469	430	$998,460	($35,991)
						($220,977)
	City	Incidents	Cost	Incidents	Cost	Pre-Post
Larceny ($1,259)	Los Angeles	4,317.5	$5,435,732.5	3,639.83	$4,582,550.17	$853,182.33
	San Diego	1,377	$1,733,643	1,494	$1,880,946	($147,303)
	San Francisco	3,218	$4,051,462	3,597.5	$4,529,252.5	($477,790.5)
	San Jose	960	$1,208,640	1,120.17	$1,410,289.83	($201,649.83)
						$26,439
	City	Incidents	Cost	Incidents	Cost	Pre-Post
Robbery ($1,170)	Los Angeles	593.17	$694,005	602.17	$704,535	($10,530)
	San Diego	221.5	$259,155	220.67	$258,180	$975
	San Francisco	288.33	$337,350	326.5	$382,005	($44,655)
	San Jose	88.17	$103,155	93.33	$109,200	($6,045)
						($60,255)
	City	Incidents	Cost	Incidents	Cost	Pre-Post
Motor Vehicle Theft ($5,972)	Los Angeles	1,116.67	$6,668,733.33	1,232.67	$7,361,485.33	($692,752)
	San Diego	761.5	$4,547,678	752.5	$4,493,930	$53,748
	San Francisco	637	$3,804,164	648.5	$3,872,842	($68,678)
	San Jose	621	$3,708,612	616.33	$3,680,742.67	$27,869.33
						($679,812.67)
	Total	16,723	$41,499,167.33	16,931.5	$42,616,537.83	($934,605.67)

Calculations in this table rely on average cost of crime values supplied by the FBI's *Cost of Crime in the United States, 2013*. This is the actual value of property taken. Property crime counts represent monthly averages for the six months preceding Proposition 47's adoption and the six months following. The totals are monthly, so the total value of additional crime above pre-Proposition 47 levels equates to a property loss equivalent to $11,215,268.04 annually. As mentioned elsewhere, values for Los Angeles' larceny totals in 2015 do not correspond to figures released in bi-weekly COMPSTAT reports. This issue will be addressed in future drafts, however because of the low tangible cost associated with each larceny, updated numbers are unlikely to significantly change this analysis.

Figure C.1: ‘Strikers’ in California Prisons

Reproduced from the California Legislative Analyst’s Office

Figure C.2: San Francisco Property Crime (Monthly Totals)

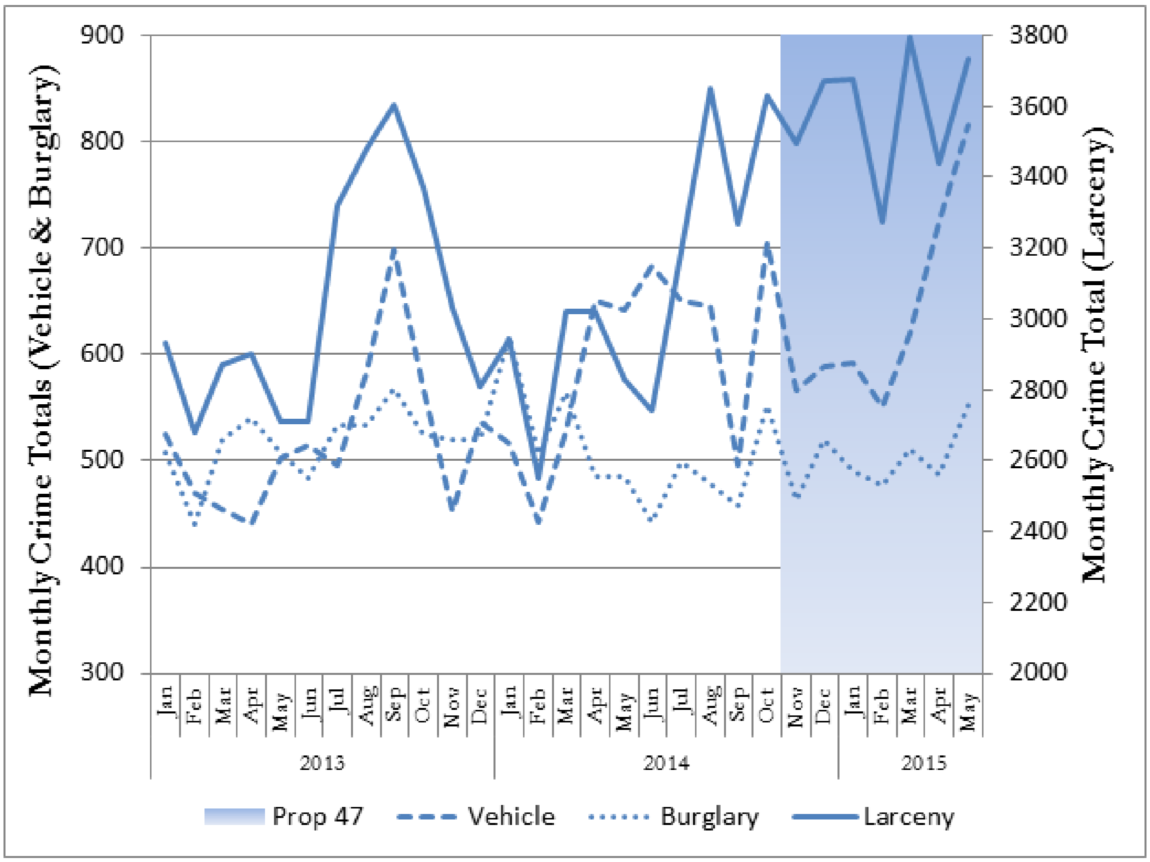


Figure C.3: San Francisco Larceny (Monthly Totals)

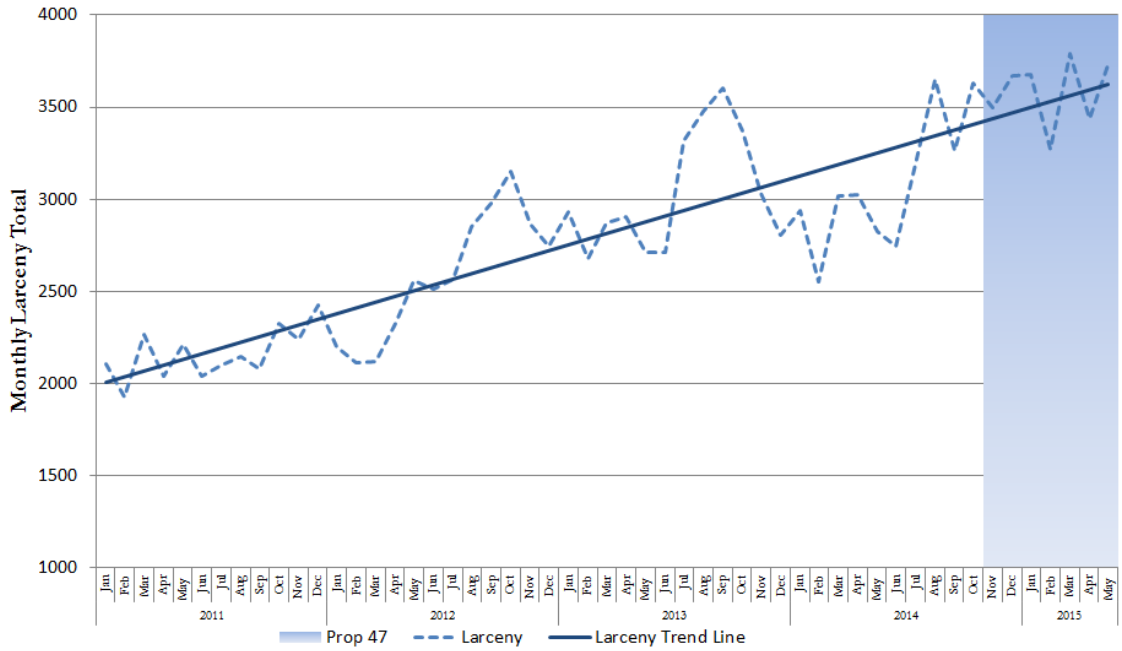


Figure C.4: San Francisco Motor Vehicle Theft (Monthly Totals)

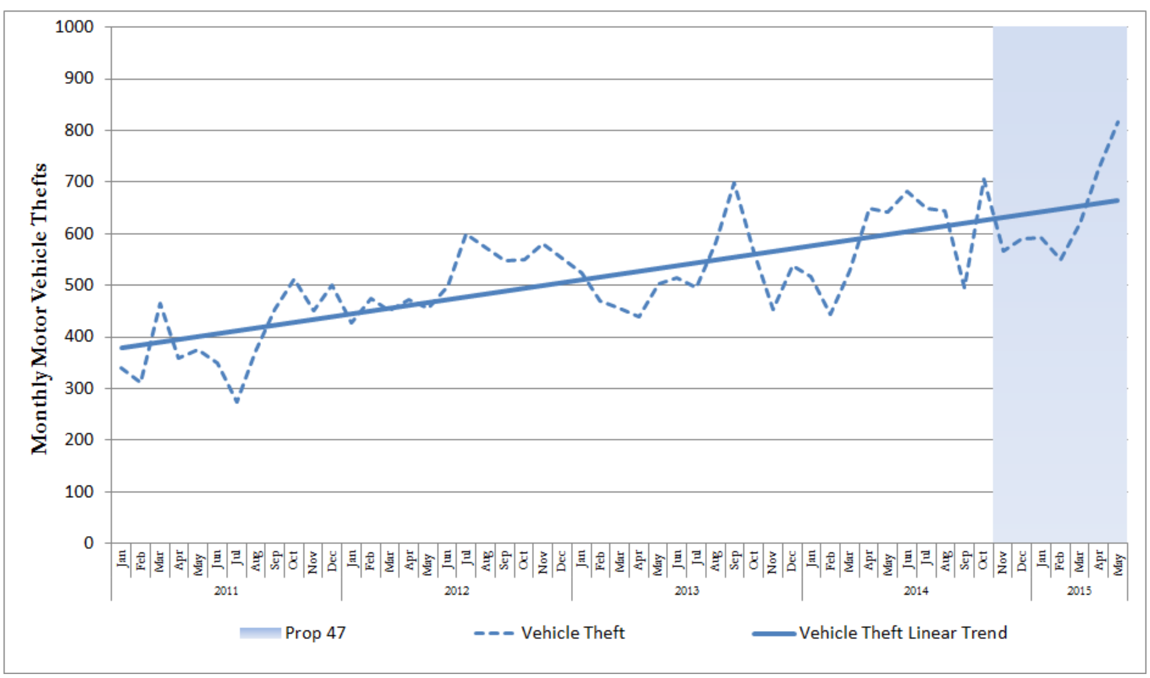


Figure C.5: San Francisco Burglary (Monthly Totals)

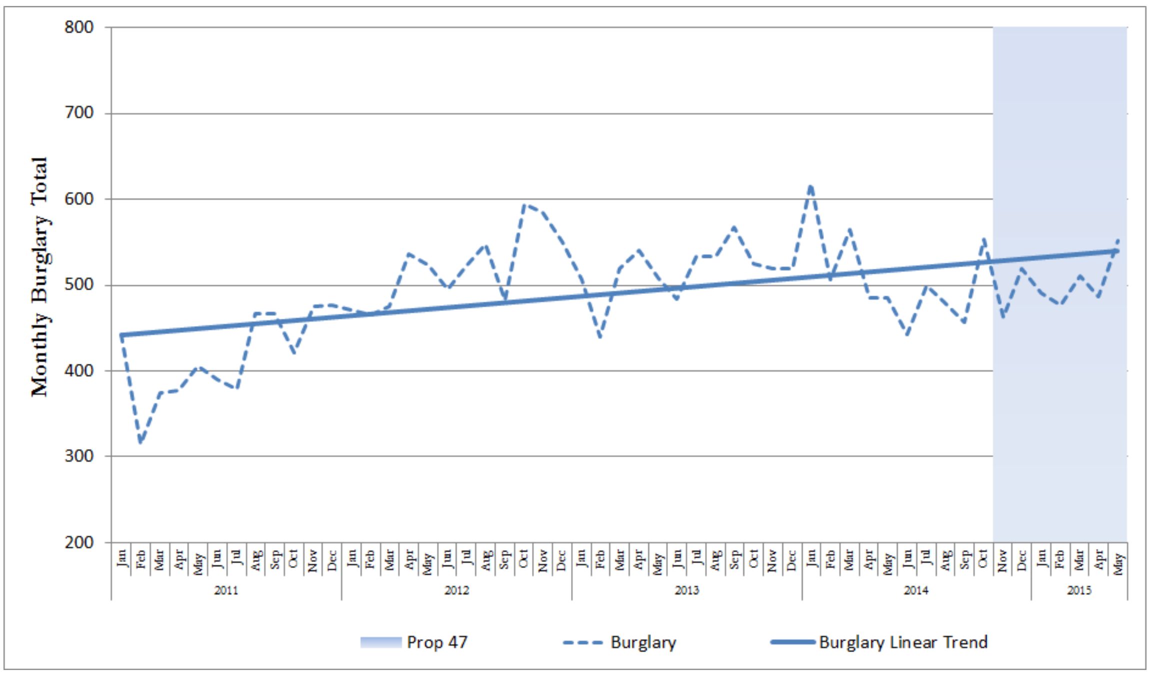


Figure C.6: San Jose Property Crime (Monthly Totals)

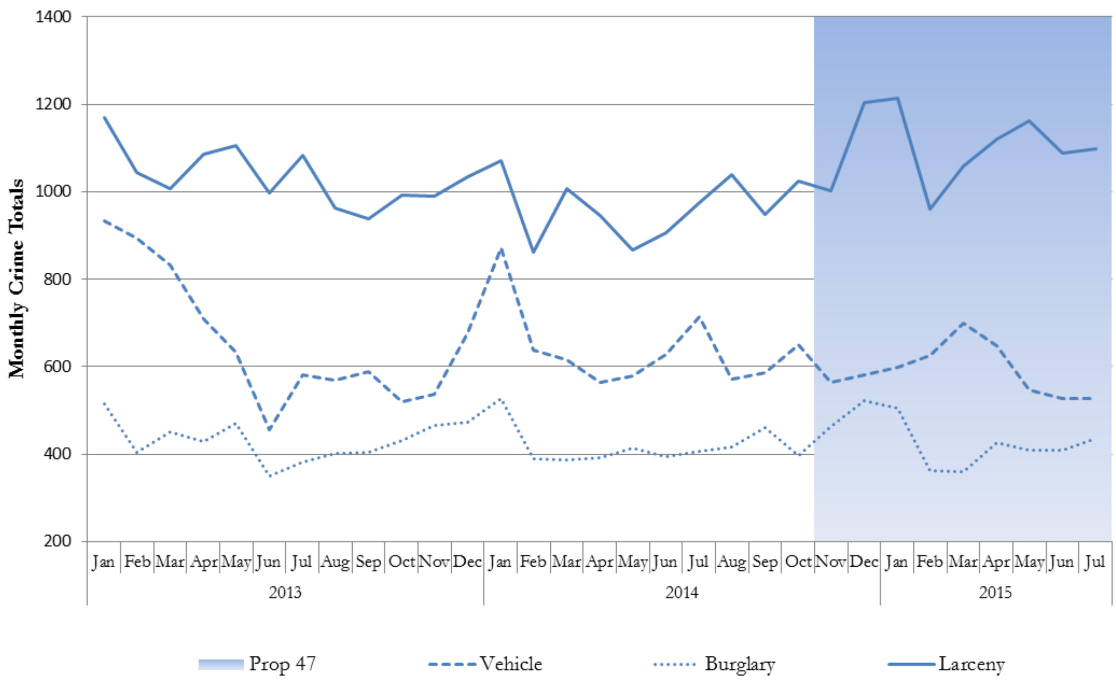


Figure C.7: San Diego Property Crime Levels (Monthly Totals)

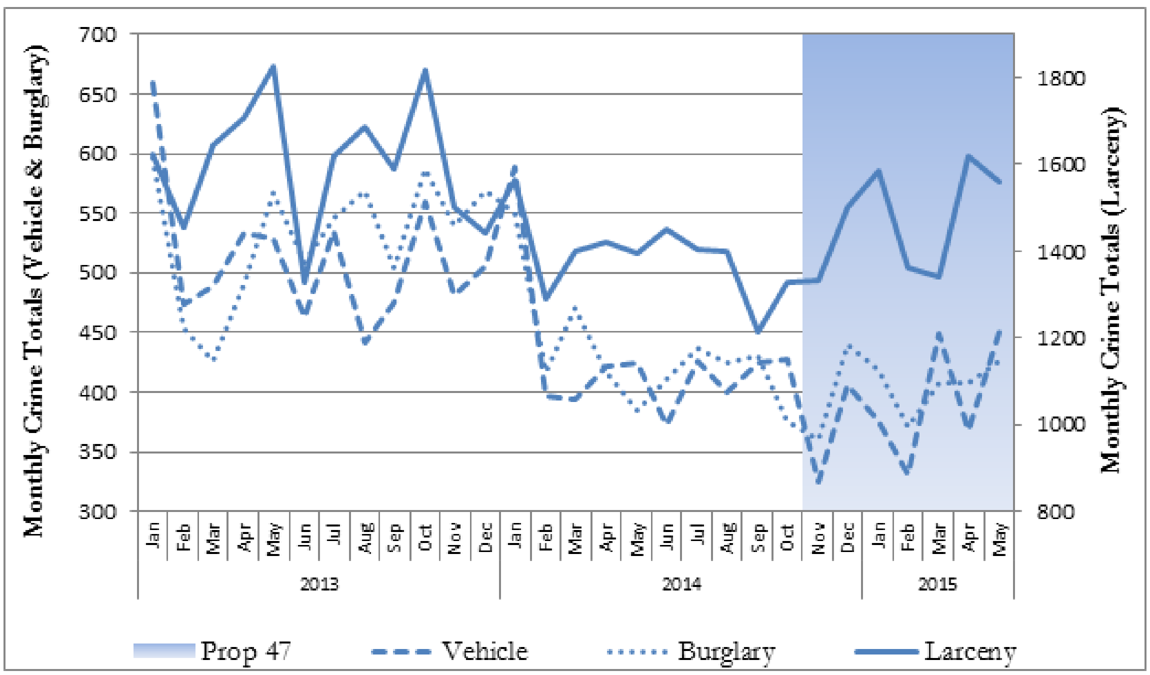


Figure C.8: Los Angeles Property Crime (Annual Totals)

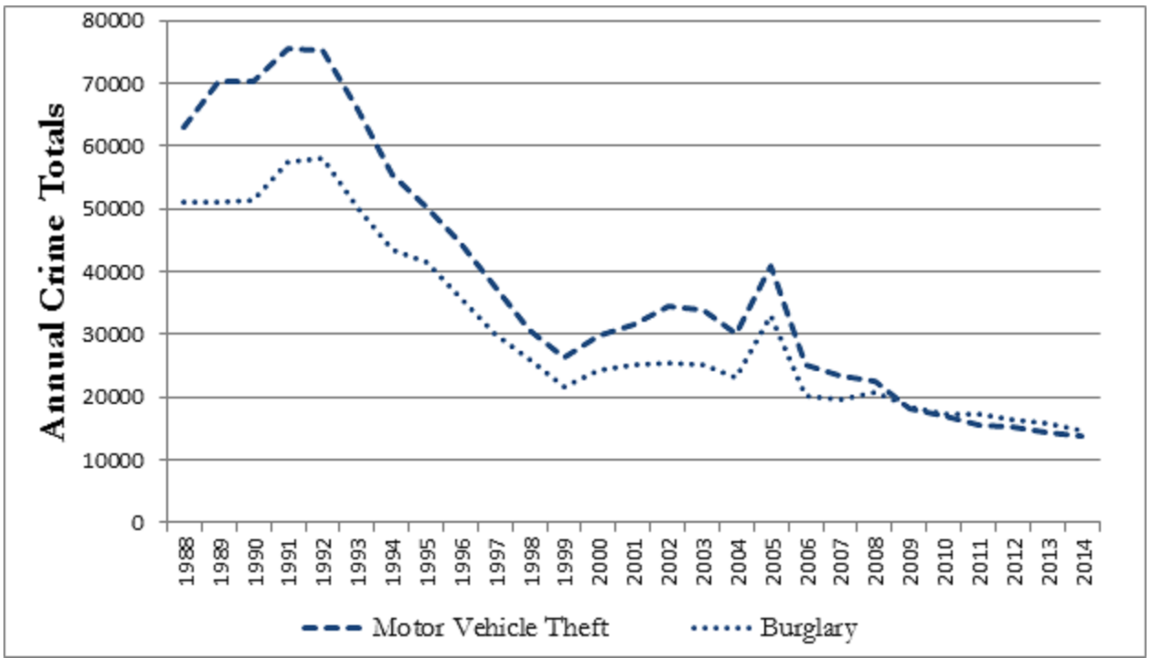
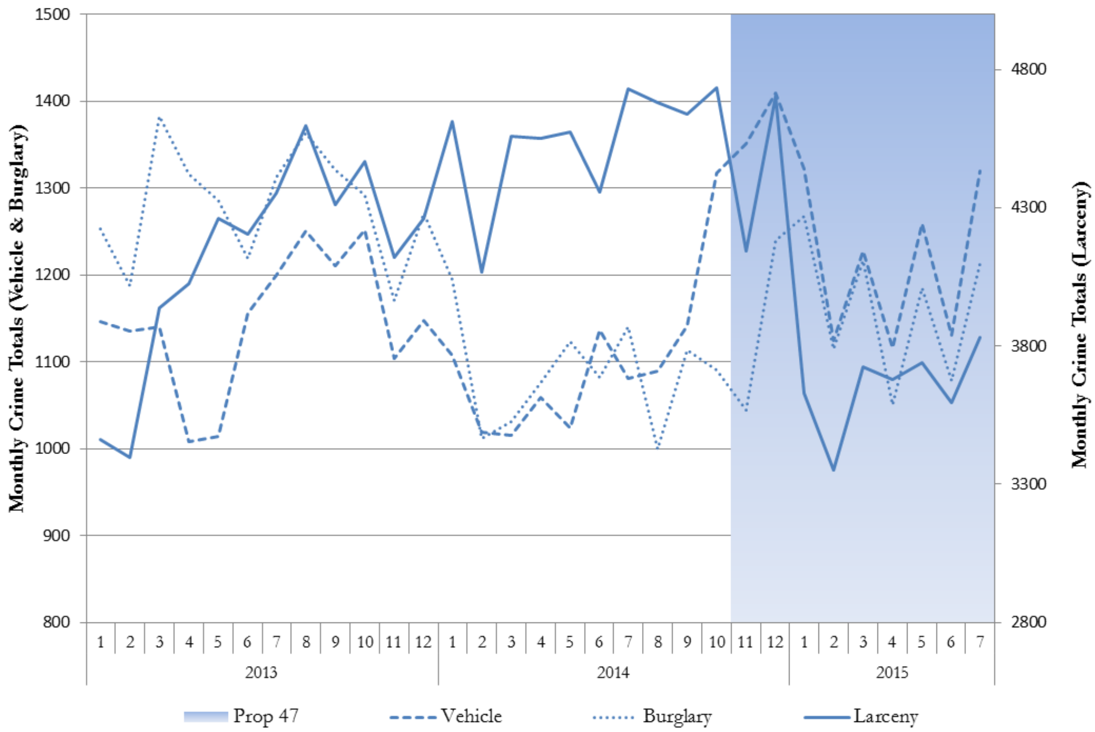


Figure C.9: Los Angeles Property Crime (Monthly Totals)



Data used to generate this figure are directly sourced from the LAPD's crime data ftp. Values describing monthly larceny totals are much lower in 2015 as compared to 2014 and do not match up with bi-weekly COMPSTAT reports issues by the LAPD's media office. This disparity will be addressed in future drafts.

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